Context matters for the effectiveness of video games for young people's mental health

Aniek Wols

Behavioural Science Institute

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Prof. dr. Anna Lichtwarck-Aschoff (Rijksuniversiteit Groningen)
Prof. dr. Isabela Granic

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Prof. dr. Tibor Bosse
Prof. dr. Maaike H. Nauta (Rijksuniversiteit Groningen)
Prof. dr. Rutger C.M.E. Engels (Erasmus Universiteit Rotterdam)

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Chapter 1

General introduction

Anxiety and depression are the most prevalent mental health problems in youth. Their associated significant negative outcomes and economic consequences for society, call for effective prevention programs. Video games for mental health have been proposed as an alternative delivery approach and potential solution to tackle social and practical access barriers as well as limitations regarding engagement related to conventional prevention programs. The field of games for mental health has been rapidly developing, and research on both applied and casual games has grown extensively; however, a comprehensive overview of the field is lacking. Therefore, Part 1 of the current thesis aims to provide an overview of the field. Furthermore, research on games for mental health has largely overlooked the influence of nonspecific factors such as expectations, motivation to change, and mindsets on game uptake, engagement and mental health outcomes. The studies described in Part 2 and 3 of this dissertation aim to address this significant gap in the literature.

MENTAL HEALTH PROBLEMS IN YOUTH

Anxiety and depression rank among the most common mental health issues experienced by youth (Costello et al., 2003; Global Burden of Disease Collaborative Network, 2020; D. Knopf et al., 2008; Merikangas et al., 2010; Polanczyk et al., 2015). Although strongly related to each other and showing high comorbidity rates (Axelson & Birmaher, 2001; J. R. Cohen et al., 2014; Cummings et al., 2014), symptoms of anxiety and depression pertain to different constructs and represent different disorders with distinct features (Hale III et al., 2009; Seligman & Ollendick, 1998). Anxiety is a natural, common feeling of nervousness in response to dangerous, stressful or unfamiliar situations (American Psychiatric Association, 2013). One's reaction to these situations may be physiological (e.g., palpitations, sweating, tense muscles), behavioural (e.g., avoidance of feared objects, stressful situations or people), and cognitive (e.g., increased attention, worrying) (American Psychiatric Association, 2013). While anxiety is a useful and adaptive emotional response to stress, facilitating avoidance of danger or coping with challenging situations, it becomes maladaptive when intense levels of anxiety persist and interfere with social or occupational functioning (Beesdo et al., 2009; Davis III, 2009; National Library of Medicine). Subsequently, an anxiety disorder may develop, which is characterised by intense feelings of anxiety, excessive worry about future events or activities, apprehensive expectations, increased attentional bias towards threat-related information, selective memory processing, and distorted judgements of risk (American Psychiatric Association, 2013; Gelder et al., 2005; National Library of Medicine, 2016; World Health Organization, 2010).

Core symptoms of depression, on the other hand, include a depressed mood (e.g., feeling empty or hopeless, persistent unhappiness) and loss of interest or pleasure in most activities (American Psychiatric Association, 2013; Thapar et al., 2012; World Health Organization, 2010). Additional symptoms may include sleep disturbances, change in eating patterns and subsequent change in weight, loss of energy, feeling worthless or guilty, diminished ability to concentrate or make decisions, thinking or moving slower or showing restless behaviour, and suicidal ideation (American Psychiatric Association, 2013). People with depression experience significant distress and impairment in social or occupational functioning (American Psychiatric Association, 2013; Jaycox et al., 2009; Verboom et al., 2014).

Unlike depression, anxiety disorders usually have their onset in childhood. They are one of the earliest form of mental health problems to emerge in childhood and the most prevalent disorders in children (Bandelow & Michaelis, 2015; Palitz & Kendall, 2020). Already at a young age, approximately 20% of children are diagnosed with an anxiety disorder (Beesdo et al., 2009; Chavira et al., 2004; Kroes et al., 2001). Moreover, up to 49% of children are impaired by subclinical levels of anxiety symptoms (Muris et al., 2000a).

During adolescence, anxiety symptoms continue to rise (Grant, 2013; Roza et al., 2003), with 25% of adolescents being diagnosed with an anxiety disorder in the past 12 months (Kessler et al., 2012) and up to 32% experiencing subclinical anxiety symptoms (Balázs et al., 2013). Depression onset sharply peaks during adolescence (Kessler et al., 2012; Merikangas et al., 2010; Roza et al., 2003). Prevalence rates of 10% of adolescents being depressed have been reported for large international and US samples (Balázs et al., 2013; Kessler et al., 2012). Furthermore, almost a third of young adolescents report subclinical depressive symptoms (Balázs et al., 2013). Moreover, remarkable differences between boys and girls emerge during adolescence and throughout (young) adulthood. Females are approximately twice as likely as males to experience subclinical levels of anxiety and depressive symptoms, and the same holds for clinical diagnoses of anxiety and depression (Balázs et al., 2013; Hankin et al., 2007; Kessler et al., 2005; Twenge & Nolen-Hoeksema, 2002).

Even into young adulthood, anxiety and depression remain the most prevalent mental health problems. Up to 11.7% and 15.6% of young adults, respectively, report to have been diagnosed with an anxiety disorder or major depressive episode in the past year (De Graaf et al., 2012; Kessler & Walters, 1998). By the time they reach early adulthood, a quarter of young adults have experienced anxiety and/or depression throughout their lives (Copeland et al., 2014; Kessler et al., 2005; Kessler & Walters, 1998). Most concerning is that mental health problems among older adolescents and young adults have been increasing over the past decades (Centraal Bureau voor de Statistiek, 2018; Collishaw et al., 2010; Schoemaker et al., 2019), as well as more recently due to the Covid-19 pandemic (Al Omari et al., 2020; A. Knopf, 2020; Rauschenberg et al., 2021; Salari et al., 2020).

Anxiety and depression have been associated with detrimental short- and long-term consequences for the individual (Balázs et al., 2013; World Health Organization, 2001), such as academic underachievement (Owens et al., 2012; Woodward & Fergusson, 2001), problems in social and family functioning (Hoglund & Chisholm, 2014; Woodward & Fergusson, 2001), increased risk for substance (ab)use and dependence (Merikangas et al., 1998; Woodward & Fergusson, 2001), suicidal behaviour (Bolton et al., 2008; Glied & Pine, 2002), other psychopathology (Lavigne et al., 2015; Priddis et al., 2014), unemployment (Fergusson et al., 2001), and early parenthood (Fergusson & Woodward, 2002). These negative outcomes have not only been associated with clinical anxiety and depressive disorders, but also subclinical levels of anxiety and depressive symptoms cause significant distress and impair youth's functioning in multiple domains (Balázs et al., 2013; Roza et al., 2003). Moreover, subclinical anxiety and depressive symptoms put individuals at significant risk for later development of full-blown anxiety and depressive disorders (Aalto-Setälä et al., 2002; Copeland et al., 2014; Lewinsohn et al., 2000; D. S. Pine et al., 1999). Besides individual consequences, (subclinical) anxiety and depression also have societal consequences. They are the leading cause of disability among youth (GBD Mental Disorders Collaborators, 2022; Gore et al., 2011; World Health Organization, 2021) and impose an enormous burden on health care costs (Bodden et al., 2008) and other (indirect) societal costs, such as absenteeism at work, sick leave and incapacity for work (Meijer et al., 2006; Smit et al., 2006).

It is known that untreated symptoms persist into adulthood, and that recurrence and chronic courses of anxiety and depression are common (Asselmann & Beesdo-Baum, 2015; Kovacs et al., 2016; Reef et al., 2009), continuing to affect youth's daily life and increasing risk for further health issues (Balázs et al., 2013; Essau et al., 2002). Given the high rates of anxiety and depressive symptoms in youth, as well as their severe impact on individuals' daily and future functioning and economic consequences, it is of critical importance to prevent these symptoms from exacerbating in to full-blown disorders.

CURRENT PREVENTION PROGRAMS

Prevention programs can be classified into three types, namely universal, selective, and indicated prevention (Garber & Weersing, 2010; Munoz et al., 2010). All three levels of prevention aim to decrease symptoms and/or the likelihood that disorders develop by reducing risk factors and increasing resilience (McGorry et al., 2011). *Universal* prevention programs target the entire population regardless of individuals' risk status or symptoms, but some broader target groups may be distinguished. For anxiety and depression specifically, these broader target groups may be: women, young people, the elderly, ethnic minorities, people with low socio-economic status or little social support (Meijer et al., 2006). *Selective* prevention programs target specific subgroups of individuals who are known to have an increased risk of developing a disorder. Risk factors for anxiety and depression include having a parent with psychopathology, having a chronic disease, problems in social contacts (Meijer et al., 2006) or an avoidant coping style, or showing behavioural inhibition (Rapee, 2002). *Indicated* prevention programs are delivered to individuals who already show symptoms of anxiety or depression, which do not (yet) meet the diagnostic criteria of the disorder. This type of prevention is about early detection and intervention to prevent symptoms from getting worse (Meijer et al., 2006). Research indicates that selective and indicated (i.e., targeted) prevention programs generally have larger effect sizes and sustained effects over longer time periods compared to universal approaches in preventing and reducing symptoms of anxiety and depression in youth (Horowitz & Garber, 2006; Merry, 2007; Stice et al., 2009; Stockings et al., 2016; Teubert & Pinquart, 2011).

Prevention programs for anxiety and depression often target transdiagnostic mechanisms related to both disorders (Butler et al., 2006; Schaeuffele et al., 2021). Anxiety and depression tend to have a high rate of comorbidity; not only the disorders often coexist, but also symptoms of anxiety and depression are highly correlated (Axelson & Birmaher, 2001; Balázs et al., 2013; J. R. Cohen et al., 2014; Cummings et al., 2014). Furthermore, overlapping risk factors and similar underlying mechanisms play a role in the onset, exacerbation and maintenance of the disorders. For example, it is known that parental psychopathology, negative parenting behaviour and parenting stress increase the risk for youth to develop mental health problems (Bijl et al., 2002; Connell & Goodman, 2002; Fox et al., 2010; Kane & Garber, 2004; Knappe et al., 2009; Lieb et al., 2002; Merikangas et al., 1998; Needham, 2008). Additionally, females, youth with (chronic) stress or those who have been through traumatic events have a higher risk as well (Chaplin et al., 2009;

De Graaf et al., 2010; Garber et al., 2002; Ge et al., 1994; Larson & Ham, 1993; Meijer et al., 2006). Regarding underlying mechanisms, both anxiety and depression are characterised by cognitive biases such as negative automatic thoughts, maladaptive beliefs and behaviours, negative affect, elevated distress and dysfunctional emotions (Calvete et al., 2013; Chu & Harrison, 2007; Cole et al., 2008; Dozois et al., 2009; Ehring & Watkins, 2008; Farchione et al., 2012; Harvey et al., 2004; McEvoy et al., 2013; Muris et al., 2005; Trosper et al., 2012). Moreover, clinical reliability in distinguishing anxiety and depression from one another has proven to be low (T. A. Brown et al., 2001). Taking this into account, as well as the fact that anxiety and depression share similar underlying mechanisms and cognitive processes, prevention efforts often focus on transdiagnostic mechanisms underlying both anxiety and depression, which potentially also lead to a larger benefit of prevention programs (Chu et al., 2015; Dozois et al., 2009).

Most prevention programs for anxiety and depression include elements of cognitive behavioural therapy (CBT) (Butler et al., 2006; Kendall, 2011; Mychailyszyn et al., 2012), which aims to address and target the underlying mechanisms associated with anxiety and depression (Calvete et al., 2013; Chu & Harrison, 2007; Cole et al., 2008). CBT is designed to focus on the interplay between thoughts, feelings, and behaviours, and to practice adaptive responses to difficult events (Compton et al., 2004). Individuals learn to identify, challenge and replace dysfunctional or negative thoughts with more adaptive thoughts and behaviours (Beck, 2005; Nolen-Hoeksema, 2001). CBT-based prevention programs typically include various therapeutic techniques, such as problem-solving skills, cognitive restructuring, family communication skills training, exposure therapy, pleasant activity scheduling, and behavioural activation (Compton et al., 2004; Kendall, 2011). These techniques help individuals develop effective coping skills and challenge negative thinking patterns, with the goal to improve negative affect, decrease distress and increase cognitive coping, subsequently reducing symptoms and the risk of developing an anxiety or depressive disorder (Calvete et al., 2013; Chu & Harrison, 2007; Cole et al., 2008; Sander & McCarty, 2005).

LIMITATIONS AND BARRIERS OF CONVENTIONAL PREVENTION PROGRAMS

Although CBT-based prevention programs are widely used and several meta-analyses have found them to be effective in reducing symptoms and preventing the onset of disorders in some cases, the overall effectiveness of these programs is generally considered to be small to moderate (Beardslee et al., 2013; Christensen et al., 2010; Fisak et al., 2011; Merry, Hetrick et al., 2012; Mychailyszyn et al., 2012; Rasing et al., 2017; Stockings et al., 2016). Additionally, conventional prevention programs face several social and practical access barriers that hinder the implementation, accessibility, and effectiveness of these programs. First, perceived stigma around mental health can discourage youth from seeking or participating in prevention programs (Clement et al., 2015; Gulliver et al., 2010; Mukolo & Heflinger, 2011; Salloum et al., 2016). Furthermore, access and resources may be limited depending on, for instance, geographical location, socioeconomic status, healthcare and ethnic disparities, and shortage of mental health professionals and long waiting lists (Bijl et al., 2003; Collins et al., 2004; Kataoka et al., 2002; Wells et al., 2001). Additionally, high costs are an important barrier that impedes youth from seeking help, especially those who need it the most (Collins et al., 2004; Salloum et al., 2016).

Moreover, limitations and challenges have been identified in the delivery of CBT-based prevention programs. It is important to note that these limitations specifically relate to how CBT is delivered, rather than the underlying therapeutic principles of CBT itself (Granic et al., 2014; Kazdin, 2011). CBT programs largely rely on a didactic approach, which might not be appealing, engaging and motivating for youth (Crenshaw, 2008; De Haan et al., 2013; Weisz & Kazdin, 2010; World Health Organization, 2012). Another limitation in many CBT approaches is the considerable disparity between what youth learn and know, and what they actually do in their daily lives (Eichstedt et al., 2014). Recognising this gap between knowledge and behaviour, CBT programs often incorporate activities such as role-playing, problem-solving exercises, and homework assignments (Kendall, 2011), but these activities are often disconnected from authentic emotional experiences and lack real-life context hampering the transfer of learned skills to other contexts and youths' everyday lives (Granic et al., 2014). Finally, the aforementioned social barriers (e.g., stigma), practical barriers (e.g., high costs) and the unappealing didactic approach may contribute to the large percentage of premature dropout of mental healthcare that has been reported (De Haan et al., 2013; Salloum et al., 2016). Taken together, these limitations and barriers of conventional prevention programs highlight the need for alternative delivery approaches that are engaging, motivating, accessible, and cost-effective (P. J. Jones et al., 2019; Kazdin, 2019; Liverpool et al., 2020; Weisz et al., 2019).

VIDEO GAMES AS AN ALTERNATIVE TO CONVENTIONAL PREVENTION PROGRAMS

Video games have shown promise in serving as a potential alternative approach, and present a potential solution to tackle the aforementioned limitations (Granic et al., 2014; Kazdin, 2015). Video games offer various advantages when compared to traditional mental health interventions, including prevention programs for anxiety and depression. First, reflected in their ubiquitous use among youth, video games provide a non-threatening and non-stigmatising medium for youth to engage with mental health interventions (Lenhart et al., 2008; McFarlane et al., 2002; McGonigal, 2011; NPD Group, 2011). This can help reduce the perceived stigma associated with seeking help or participating in traditional forms of interventions (Granic et al., 2014). Additionally, the interactive and immersive nature of games is appealing and can enhance engagement and motivation, making it more likely that youth will actively (continue to) participate in the intervention (Fleming et al., 2017; Granic et al., 2014; Kazdin, 2015). Secondly, games allow youth to engage in simulated (emotion eliciting) scenarios: through gameplay youth can practice newly learned skills or strategies repeatedly in a safe and controlled environment, promoting learning and skill transfer to real-life situations (Buday, 2015; Fleming et al., 2017; Granic et al., 2014). Furthermore, games can be designed to be adaptable and personalised, allowing for individualised learning experiences based on users' needs, preferences, and skills levels. This customisation can enhance the relevance and effectiveness of the intervention (Bakkes et al., 2012; Bakkes et al., 2014; Barnes & Prescott, 2018). Another benefit of video games is that they can easily be distributed and used on various ubiquitous devices such as smartphones, tablets and gaming consoles, making them accessible to a larger population including individuals in remote or underserved areas (Entertainment Software Association, 2017; Fleming et al., 2023; Granic et al., 2014). This potential for scalability offers a cost-effective alternative to traditional approaches, such as in-person therapy or training sessions. Once developed, games can be distributed with relatively low cost and effort (Eichenberg & Schott, 2017; Granic et al., 2014; Kazdin, 2015; Lau et al., 2017). In summary, video games could potentially overcome the shortcomings of conventional programs, improving appeal, reach, uptake and accessibility, and ultimately effectiveness.

VIDEO GAMES FOR MENTAL HEALTH

Given the aforementioned benefits of video games in comparison to traditional prevention programs, it is unsurprising that in recent years there has been a growing interest among mental health practitioners and researchers in utilising *applied* video games as a means of promoting mental health (Fleming et al., 2017; Fleming et al., 2023; Granic et al., 2014; Kazdin, 2015; Lau et al., 2017; Townsend et al., 2022). Furthermore, the effects of *casual* games (i.e., entertainment or commercially available, off-the-shelf games) have been increasingly examined in the field of mental health, because they are affordable and already widely available for the general population and provide repeated training for target behaviours (Ceranoglu, 2010; Colder Carras et al., 2018; Granic et al., 2014; Steadman et al., 2014).

Applied games are digital interventions that employ game design elements in an effort to making interventions more enjoyable, motivating and engaging (Schmidt et al., 2015). Their primary aim is to educate or motivate users, and to train or promote behaviour change, other than pure entertainment purposes (Michael & Chen, 2005; Stapleton, 2004; Stokes, 2005; Vajawat et al., 2021). Two categories of applied games can be distinguished, namely 'gamified' interventions and 'serious games' (Fleming et al., 2017; Schmidt et al., 2015; Vajawat et al., 2021). Gamification refers to adding game elements such as points and rewards, increasing difficulty levels and narratives to interventions (M. Brown et al., 2016; Cheng et al., 2019; Cugelman, 2013; H. J. Park & Bae, 2014; Seaborn & Fels, 2015), without necessarily focussing on playfulness and fun (Fleming et al., 2017). Serious games, on the other hand, are full game experiences: they are designed to be immersive and entertaining as well as incorporating evidence-based therapeutic techniques (Eichenberg & Schott, 2017; Fleming et al., 2014; Zayeni et al., 2020). Concerning mental health, the potential and promising effects of applied games have been shown in several reviews and meta-analyses focussing on a variety of mental health problems (David et al., 2020; Dewhirst et al., 2022; Eichenberg & Schott, 2017; Fleming et al., 2017; Halldorsson et al., 2021; Johnson et al., 2016; Lau et al., 2017; Shah et al., 2018), but also specifically for anxiety (Barnes & Prescott, 2018) and depression (Dias et al., 2018; Fleming et al., 2014; Li et al., 2014; Rasing, Stikkelbroek, & Bodden, 2020).

Casual games, on the other hand, are built for player enjoyment and recreational purposes, with no consideration of their therapeutic potential (Deterding, 2015; Fullerton, 2014). While casual games are not explicitly designed as such, they may have a positive impact on mental health. Players may feel better after playing casual games and skills that may relate to improved mental health can also be learned (Ferguson & Olson, 2013; Olson, 2010; R. Pine, Sutcliffe et al., 2020). Casual games can also trigger positive emotions (McGonigal, 2011; Osmanovic & Pecchioni, 2016; Ryan et al., 2006) which, in turn, aid individuals in expanding their momentary thought-action repertoires and personal resources (Fredrickson, 2001; Quinn et al., 2012). There may also be benefits from games that trigger intense (negative) emotions, which may allow the player to practice coping strategies in a safe environment (Granic et al., 2014). In addition, playing casual games may provide distraction from problems and worries, showing beneficial effects on mood (Bowman & Tamborini, 2012; Colder Carras et al., 2018; Pallavicini et al., 2021). Finally, casual games may evoke the experience of flow, intrinsic motivation and basic need satisfaction, which have been linked to mental health benefits as well (Nakamura & Csikszentmihalyi, 2009; Ryan et al., 2008; Ryan et al., 2006; Sherry, 2004). Indeed, there is growing evidence for the potential of a wide range of casual games to improve emotion regulation (Pallavicini et al., 2018; Villani et al., 2018), general well-being (Halbrook et al., 2019; C. Jones et al., 2014), and alleviate mental health problems, such as depression, anxiety, and stress (Kowal et al., 2021; Lee et al., 2021; Li et al., 2016; Pallavicini et al., 2021; R. Pine et al., 2020; Russoniello et al., 2009) and posttraumatic stress symptoms (Holmes et al., 2009; E.L. James et al., 2015).

In conclusion, both applied and casual games have been researched as a means to address the limitations of traditional intervention programs and to increase reach, uptake and accessibility, appeal and effectiveness of prevention and intervention efforts for mental health. In the past decades, research on games for mental health has grown extensively and the field has been rapidly developing. A comprehensive overview of the field, however, is lacking. Therefore, in Part 1 of this dissertation, we performed a systematic review to provide a state-of-the-art overview of the field.

PREDICTORS OF EFFECTIVENESS: NONSPECIFIC FACTORS IN APPLIED GAMES

As previously discussed, traditional evidence-based programs in the field of developmental psychopathology are mostly based on CBT, which includes evidence-based therapeutic techniques (Kendall, 2011). These *'specific'* therapeutic techniques are derived from theories that explain the underlying processes involved in the development and persistence of mental health disorders (e.g., relaxation and exposure training for anxiety; Kendall, 2011). In the development of applied games for mental health, the primary focus is on translating these specific therapeutic techniques into game elements and mechanisms (Eichenberg & Schott, 2017; Fleming et al., 2017). In research on applied games, the main objective is to examine the game's overall effectiveness, and to a lesser extent, how specific techniques incorporated contribute to changes in mental health (for exceptions, see De Vries et al., 2015; Dovis et al., 2015; Van Houdt et al., 2019). The assumption underlying this research is, however, that the observed improvements in mental health can be attributed to the specific CBT techniques incorporated in the game. A substantial body of evidence, however, consistently indicates that *nonspecific* factors – factors not specific to any particular psychotherapeutic approach – significantly contribute to positive intervention outcomes (Colloca, 2018a, 2018b; Grencavage & Norcross, 1990; Ilardi & Craighead, 1994; Thiruchselvam et al., 2019; Wampold, 2015) as well as process-related variables such as engagement, adherence, alliance quality, effort and invested time in the treatment (Boettcher et al., 2013; Boot et al., 2013; Constantino et al., 2011; Greenberg et al., 2006; Meyer et al., 2002; Westra et al., 2007). In fact, specific factors explain relatively little and account for only a small percentage of the variance in outcome measures, whereas nonspecific factors play a more important role in treatment efficacy and improving mental health outcomes (Ahn & Wampold, 2001; Lambert, 2005, 2011). Examples of the most important and most researched nonspecific factors include the client-therapist relationship (Krupnick et al., 2006; Norcross, 2002), expectations for improvement and placebo effects (Asay & Lambert, 1999; Constantino et al., 2018; Crum & Phillips, 2015; Greenberg et al., 2006; Kazdin, 1979; Thiruchselvam et al., 2019), and client-related variables such as hope (Ilardi & Craighead, 1994), mindset or implicit theories of beliefs about the malleability of personal attributes (Crum & Phillips, 2015; Tamir et al., 2007), and motivation or readiness to change (Dozois et al., 2004; Norcross et al., 2011; Prochaska & Norcross, 2001; Taylor et al., 2012).

Given the central role of nonspecific factors in nearly all psychological interventions (Lambert, 2005), it is likely that nonspecific factors, at least in part, drive mental health improvements in well-designed applied games as well. After all, applied games are often part of a treatment context, are delivered in a specific way, and individuals may have different expectations and beliefs about the effectiveness of applied games and different motivations to install or follow an applied game program. Findings from two randomised controlled trials (RCTs) performed by our own Games for Emotional and Mental Health (GEMH) lab further highlight the need to examine nonspecific factors in applied games. These two RCTs investigated the effectiveness of two applied games, MindLight and Dojo, specifically designed to reduce anxiety symptoms, and compared the effects to commercial control games (Scholten et al., 2016; Schoneveld et al., 2016). Results from both trials showed equal improvements in anxiety symptoms for both the intervention and the control group. Although the applied games explicitly incorporated evidence-based techniques for anxiety and the two control games did not, equal reductions in anxiety symptoms were found. This suggests that nonspecific factors such as expectations and motivation to change may have contributed to these findings.

To date, limited attention has been given to nonspecific factors in the literature on applied games and their potential effects remain largely unknown (Enck et al., 2017; Torous & Firth, 2016). In order to optimise the effectiveness of applied games to their full potential, it is crucial to examine and harness the benefits of nonspecific factors (Enck et al., 2013). It is hypothesised that nonspecific factors positively influence mental health outcomes directly, as well as through engagement. Previous research on conventional therapy has shown that nonspecific factors contribute to both positive intervention outcomes and process-related variables such as engagement, adherence, effort and invested time in the treatment (Boettcher et al., 2013; Boot et al., 2013; Colloca, 2018b; Constantino et al., 2018; Greenberg et al., 2006; Wampold, 2015; Westra et al., 2007). In turn, research has found that these variables related to engagement predict positive intervention outcomes (Becker et al., 2015; Lindsey et al., 2019; C. M. Yeager & Benight, 2022). The variables studied in the current dissertation are explained in more detail below.

NONSPECIFIC FACTORS, ENGAGEMENT AND POSITIVE OUTCOMES

As briefly mentioned earlier, one of the most significant nonspecific factors to consider in applied games is individuals' expectations for improvement (Asay & Lambert, 1999; Lambert, 2005). Applied games are usually introduced with a clear aim to promote (mental) health, which naturally induces expectations for improvement. Previous research has demonstrated that expectations drive a large majority of (conventional) intervention effects (Greenberg et al., 2006), but also particularly so in experimental game design studies (Boot et al., 2013). Research on conventional programmes has also shown that individuals with higher expectations for improvement invested more time and effort in, for example, an unguided internet-based self-help programme for social anxiety (Boettcher et al., 2013). Thus, expectations for improvement influence intervention outcomes as well as engagement with conventional programs.

It is unknown whether similar processes may be at hand in applied games, but the findings of the two earlier mentioned RCTs performed by our lab suggest so. More specifically, expectations for improvement may explain the equal improvements in anxiety that were found for both the intervention and control games (Scholten et al., 2016; Schoneveld et al., 2016). In our RCTs, we made sure that youth's expectations for both the applied and (casual) control game were equal, prior to the random assignment. However, in doing so, youth in both groups had similar expectations of effective anxiety reduction, which might have resulted in the equal improvements we observed in both the intervention and control group. Therefore, it is important to examine the effect of expectations on engagement and positive intervention outcomes in applied games, such that we understand to what extent specific therapeutic techniques and nonspecific factors each explain positive outcomes and whether enhancing expectations prior to gameplay would be fruitful.

Motivation to change may be a second variable that could explain the equal improvements in anxiety we found in our RCTs , and a nonspecific factor that generally may be relevant to take into account in research on applied games. Motivation to change refers to an individual's readiness and willingness to change the symptoms or challenges they are facing (Prochaska & DiClemente, 1982). It has been recognised as one of the key predictors of (conventional) treatment outcomes, as well as a significant factor influencing variables associated with engagement (e.g., treatment engagement, adherence, and dropout; Brogan et al., 1999; Derisley & Reynolds, 2000; Lewis et al., 2012; Lewis et al., 2009; Norcross et al., 2011; Taylor et al., 2012). Individuals lacking motivation or not yet prepared to address their symptoms are less likely to show improvement post-treatment, and tend to invest less time or engage less actively in the treatment process. Although games are naturally appealing and considered intrinsically motivating and engaging (e.g., Granic et al., 2014), based on previous research we expect that individuals' motivation to change may still influence actual engagement and their perseverance in the face of failure, and subsequent mental health improvements. Therefore, it is important to examine motivation to change in applied games.

Symptom severity may be another important nonspecific factor to consider. Although previous research on conventional programs has not yielded conclusive results, there is research suggesting that higher symptom severity or symptoms at baseline are related to more professional help-seeking and greater symptom decreases after intervention (Merikangas et al., 2011; M.

I. Oliver et al., 2005; Sawyer et al., 2012; Van Starrenburg et al., 2017). Yet, little research has examined the influence of symptom severity on the engagement and positive outcomes of applied games.

While certain nonspecific factors may have independent effects in applied games, they may also interact with one another. For instance, individuals' motivation to change may be related to the severity of symptoms experienced (Dozois et al., 2004). Additionally, the influence of expectations on engagement and positive intervention outcomes may be moderated by motivation to change and symptoms severity (Buday, 2015; M. B. Oliver & Krakowiak, 2009).

Furthermore, it has been hypothesised that nonspecific factors interact with specific factors in the prediction of positive intervention outcomes (Boot et al., 2013; Greenberg et al., 2006; Kazdin, 2005; Messer & Wampold, 2002). For example, an individual more motivated to change or experiencing more severe symptoms may engage more with the (specific) therapeutic techniques in the game, and subsequently show larger improvements in mental health (Buday, 2015; Dean et al., 2016; M. B. Oliver & Krakowiak, 2009). Conversely, it is possible that individuals experiencing more severe symptoms exhibit lower levels of engagement with the therapeutic techniques embedded in the game, as it confronts them with their problems (Poppelaars, Lichtwarck-Aschoff et al., 2018). It is important to devote attention to these interaction effects as insights into the complexity of factors at work may give critical information about individual differences in intervention outcomes. By utilising knowledge about both specific and nonspecific factors, applied games for mental health can be optimised.

PROMOTING AND DELIVERING GAMES FOR MENTAL HEALTH: INTERVENTION REACH AND UPTAKE

As outlined earlier, video games offer a significant advantage when it comes to their implementation potential (Granic et al., 2014; Kazdin, 2015; Lau et al., 2017). On the one hand, effective applied and casual games can be used in a clinical setting to complement traditional intervention approaches, reinforcing therapeutic techniques, providing additional support or enhancing engagement between sessions (e.g., Beaumont et al., 2021; Ducharme et al., 2021; Schuurmans et al., 2018). They can also be utilised as replacements for (school-based) prevention programs, offering interactive and engaging interventions to address mental health concerns (e.g., Schoneveld et al., 2018). On the other hand, video games also have the capacity to be distributed and utilised outside the clinical context; they may be offered as (applied) stand-alone and/or a freely accessible (commercial) game. For individuals with mild symptoms, who may not require intensive therapy but could benefit from self-guided interventions, video games may provide support, education, and self-help resources. That way, mental health games can help individuals manage and improve their mental health in a convenient manner. Outside the traditional healthcare setting, however, individuals' motivation to pick and play a video game to improve their mental health becomes an important condition for the exploit of the implementation potential of video games. Therefore, it is essential to consider and address the factors that enhance motivation to engage with games for mental health.

One factor potentially influencing the reach and impact of video games is the way in which games for mental health are presented and promoted. How these games are marketed, framed, and communicated can influence their acceptance and engagement. One approach may be promoting mental health games as proven tool for enhancing mental health. This approach involves highlighting the evidence-based benefits of the games. By emphasizing the positive outcomes and scientific validity of mental health games, individuals may be more inclined to engage with them, recognising the potential value they can offer to mental health (M. B. Oliver & Krakowiak, 2009). While some individuals may be motivated by the potential health benefits, others may experience limited autonomy and feelings of resistance in response to an explicitly promoted mental health aim (Brehm, 1966; Dillard & Shen, 2005; A. S. Richards & Banas, 2015). Another approach would therefore be to position mental health games as regular entertainment games rather than explicitly highlighting their therapeutic nature. This 'stealth' approach involves presenting the mental health game as enjoyable and engaging entertainment experiences and may motivate individuals to engage with the game without feeling stigmatised or triggering resistance. Understanding messaging effects can inform the design and promotion of mental health games to maximise engagement and motivation.

Moreover, personal motivational traits of youth are likely to influence their receptiveness and the degree to which explicit messages about mental health resonate with them. For example, the desire to improve one's mental health may motivate individuals to pick and play games explicitly promoted for their mental health benefits. As outlined earlier, motivation to change has been found to be an important predictor of help-seeking, adherence, drop-out and treatment engagement (Brogan et al., 1999; Derisley & Reynolds, 2000; Norcross et al., 2011; Taylor et al., 2012). Likewise, symptom severity may be another factor influencing game choice and gameplay. Individuals experiencing more (severe) symptoms may feel motivated by the personal relevance of a game promoted for its mental health benefits, such that they want to play the game compared to individuals with less (severe) symptoms. On the other hand, however, these individuals may avoid such games as they may think that it confronts them with their problems. In research on conventional programs, symptom severity has been associated with more help-seeking (Merikangas et al., 2011; M. I. Oliver et al., 2005; Sawyer et al., 2012; Van Starrenburg et al., 2017) as well as lower help-seeking tendencies (Chin et al., 2015; Sawyer et al., 2012), suggesting that evidence is inconclusive at this moment, and it remains unknown how symptom severity may impact game selection and engagement with mental health games.

Additionally, individuals' implicit theory or *mindsets* may play a role in the likelihood of selecting and playing a mental health game. Individuals can have mindsets about every personal attribute such as intelligence (Dweck, 2017c), personality (D. S. Yeager & Dweck, 2012), emotions (Tamir et al., 2007), and the nature of stress (Crum et al., 2013). In the literature, a distinction is made between two types of mindsets: a *growth* mindset (incremental theory) and a *fixed* mindset (entity theory). When adopting a growth mindset regarding a particular attribute, one believes that the attribute can be altered through dedicated effort, experience, and assistance from others (Dweck, 2013; Tamir et al., 2007). Individuals with a fixed mindset believe that a specific attribute is not (or less) malleable and cannot be controlled. The concept of mindsets, as proposed by Dweck (2013), revolves around the notion that mindsets shape individuals' goals, action tendencies, beliefs about effort, and responses to setbacks (Dweck, 2017a, 2017c). As a result, mindsets emerge as significant motivational factors influencing behaviour (Burnette et al., 2013; Dweck, 2017a), potentially impacting help-seeking tendencies and treatment engagement (Burnette et al., 2019; Schroder et al., 2015). Furthermore, having a growth mindset has been linked to better mental health and well-being (Miu & Yeager, 2015; Romero et al., 2014; Schleider et al., 2015; Schroder et al., 2015; Zeng et al., 2016). Thus, given their relevance for behaviour and motivation, mindsets may influence the likelihood of selecting and playing a mental health game.

Mindsets themselves, however, are malleable as well. Past studies have demonstrated that mindsets can change as a result of relatively brief interventions, such as watching short video clips, reading an article or performing a short writing exercise (e.g., Crum et al., 2013; Dweck, 2008; Jamieson et al., 2018; Miu & Yeager, 2015; D. S. Yeager et al., 2014). Of relevance in the current context is that games are known to be able to effectively motivate players to persist in playing, even in the face of failures. Games serve as a platform for cultivating perseverance, which involves exerting continuous effort towards achieving a goal despite encountering difficulties or setbacks (Malone & Lepper, 2021). Individuals persevering in the face of failure are more likely to have a growth mindset (Dweck, 2017b), suggesting that games may boost one's growth mindset (Weerdmeester et al., 2020). Therefore, in this dissertation, we also examined the influence of gameplay on changes in one's mindset.

THE CURRENT THESIS

Overall, this dissertation aims to examine the potential of applied and casual games as a possible alternative delivery approach of traditional intervention programs for mental health, and to investigate the effects of nonspecific (motivational) factors on game selection, game experiences, engagement, and mental health outcomes. This dissertation is divided into three parts.

Part 1 of my dissertation aims to provide a state-of-the-art overview of the field. In **Chapter 2**, we performed a systematic review of randomised controlled studies that have assessed digital games for improving mental health in children, adolescents and young adults and examined the effectiveness of both applied and casual games. Thus far, previous reviews and meta-analyses focused on either applied or casual games exclusively, on very specific mental health domains, on clinical populations only, and/or they did not include multi-modal interventions (i.e., using a digital game in addition to other therapy components). In order to gain a more comprehensive understanding of the impact of games on mental health outcomes and to examine when games are most effective, we evaluated applied and casual games simultaneously, included and distinguished between both clinical and healthy populations, and compared the effectiveness of these games across a variety of mental health domains, including internalising, externalising, neurodevelopmental, psychotic and personality-related outcomes. Moreover, we examined methodological characteristics to explore current research trends and whether nonspecific factors have been taken into account.

To maximise the effectiveness of applied games to their fullest potential, it is essential to explore and leverage the advantages offered by nonspecific factors. Literature on applied games has largely overlooked nonspecific factors, and their impacts on engagement and mental health outcomes in this context are unknown. To address this significant gap in the literature, **Part 2** of my dissertation aims to contribute to our understanding of the effect of specific and nonspecific factors on engagement and mental health outcomes in an applied game. We start off with **Chapter 3**, in which we examined to what extent engagement with the therapeutic techniques in an applied game for anxiety prevention (i.e., MindLight) predicts mental health improvements. More specifically, we examined whether baseline anxiety symptoms were related to in-game play behaviours as well as whether changes in in-game play behaviours predicted changes in anxiety symptoms three months after playing the game.

The study in **Chapter 4** aimed to further unravel whether MindLight also has beneficial effects on other mental health outcomes associated with anxiety symptoms (i.e., internalising problems, externalising problems and self-efficacy), given their overlap in symptoms and transdiagnostic mechanisms. Additionally, we examined who benefitted most from the game by assessing several possible nonspecific variables as predictors of changes in anxiety symptoms, namely baseline anxiety symptoms, maternal mental health problems, and self-efficacy. In **Chapter 5**, we experimentally manipulated expectations for improvement and examined the effect on engagement while playing MindLight, as well as the moderating role of symptom severity and motivation to change. Additionally, changes in state anxiety and arousal were examined.

Despite the notable implementation benefits of games, there is a lack of research examining the uptake of and engagement with mental health games among individuals with mental health symptoms (Fleming et al., 2018; Fleming et al., 2016). Understanding how motivational factors influence selection and engagement with mental health games is crucial as it will enable us to customise and tailor the promotion of mental health games, maximising their uptake and sustained use. Therefore, in **Part 3** of my dissertation, we aimed to examine how messaging and several (nonspecific) motivational factors influence choice for a mental health game, and subsequent game experiences and engagement.

In **Chapter 6**, we examined how messaging affects game choice, perceived attractiveness and fun of the game, as well as subsequent engagement and experiences of gameplay. We further explored how severity and type of mental health symptoms influence game choice, game experiences and engagement. To better understand the influence of personal motivational traits, we examined how motivation to change and one's mindsets influence game choice and engagement in **Chapter 7**. We also examined whether mindsets change after playing a game promoted as mental health game. To conclude my dissertation, **Chapter 8** presents a summary and general discussion of the main findings, including limitations and implications for future research and implementation.

Chapter 2

Effectiveness of applied and casual games for young people's mental health: A systematic review of randomised controlled trials

Based on:
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ABSTRACT

Many youth experience mental health problems and digital games hold potential as mental health interventions. This systematic review provides an overview of randomised controlled studies assessing the effectiveness of digital applied and casual games for improving mental health in youth aged 6–24 years. A systematic search of PsycINFO, Web of Science and Pubmed yielded 145 eligible studies. Studies on (sub)clinical participant samples (*n* = 75) most often focused on attention-deficit/hyperactivity disorder (ADHD), autism and anxiety. Applied games were found most effective for improving social skills, verbal memory and anxiety, whereas casual games were found most effective for improving depression, anxiety and ADHD. Studies involving healthy youth (*n* = 70) were grouped into papers examining anxiety in medical settings, momentary effects on positive and negative affect, and papers employing a longitudinal design measuring mental health trait outcomes. Promising results were found for the use of games as distraction tools in medical settings, and for applied and casual games for improving momentary affect. Overall, our findings demonstrate the potential of digital games for improving mental health. Implications and recommendations for future research are discussed, such as developing evaluation guidelines, clearly defining applied games, harmonising outcome measures, including positive outcomes, and examining nonspecific factors that may influence symptom improvement as well.

Many children, adolescents and young adults (herein 'youths') experience mental health problems such as anxiety, depression, self-injury, autism spectrum disorder (ASD), attention-deficit/hyperactivity disorder (ADHD), conduct disorder and schizophrenia (Costello et al., 2003; Global Burden of Disease Collaborative Network, 2020; D. Knopf et al., 2008; Merikangas et al., 2010; Polanczyk et al., 2015). In recent decades, the prevalence rates of mental health problems have been on the rise (Collishaw, 2015; Olfson et al., 2015), making them a significant contributor to the overall burden of disease among young people (GBD Mental Disorders Collaborators, 2022; Gore et al., 2011; World Health Organization, 2021). Despite extensive research into psychological therapies for mental health, multiple meta-analyses show modest effect sizes at best, indicating that only 63% of youth experience improvements following treatment (Mychailyszyn et al., 2012; Stockings et al., 2016; Weisz et al., 2017). This suggests that the effectiveness of current mental health interventions has reached its limit (P. J. Jones et al., 2019; Weisz et al., 2019). At the same time, considerable unmet needs remain, with the vast majority of youth in need of care not receiving or accessing treatment due to barriers related to high costs of interventions, shortage of mental health specialists, and stigma associated with mental health (Bijl et al., 2003; Collins et al., 2004; Granic et al., 2014; Kataoka et al., 2002; D. Knopf et al., 2008; Mukolo et al., 2010; Park-Lee et al., 2016; Salloum et al., 2016; Wells et al., 2002). Moreover, a substantial proportion of youth receiving treatment discontinue prematurely because current therapies (those based on cognitive-behavioural principles) are not engaging and motivating for youth (De Haan et al., 2013; Granic et al., 2014; Kazdin, 1996).

These findings underscore the urgent need for a new approach in designing, delivering and implementing mental health interventions (P. J. Jones et al., 2019; Kazdin, 2019; Liverpool et al., 2020; Weisz et al., 2019). As indicated by systematic reviews and meta-analyses, digital health interventions hold enormous potential to improve effectiveness, reach, uptake and accessibility, and appeal (Fairburn & Patel, 2017; Hollis et al., 2017; Li et al., 2014; Torous et al., 2021). More specifically, applied games have been proposed as a viable alternative for delivering interventions, owing to their natural appeal, intrinsic motivational characteristics, cost-effectiveness, and scalability (Fleming et al., 2017; Fleming et al., 2023; Granic et al., 2014; Kazdin, 2015; Lau et al., 2017).

Applied Games

Applied games are digital interventions that employ game design elements in an effort to make interventions more enjoyable, motivating and engaging (Schmidt et al., 2015). As opposed to pure entertainment purposes, their primary aim is to educate or motivate users, and to train or promote behaviour change (Michael & Chen, 2005; Stapleton, 2004; Stokes, 2005; Vajawat et al., 2021). The potential and promising effects of applied games for mental health have been shown in several reviews and meta-analyses focussing on multiple or broad mental health domains (Ahmed et al., 2023; David et al., 2020; Dewhirst et al., 2022; Eichenberg & Schott, 2017; Fleming et al., 2017; Halldorsson et al., 2021; Johnson et al., 2016; Lau et al., 2017; Shah et al., 2018; Townsend et al., 2022), or specific mental health conditions such as depression (Dias et al., 2018; Fleming et al., 2014; Li et al., 2014; Rasing et al., 2020), anxiety (Barnes & Prescott, 2018), ADHD (Penuelas-Calvo et al., 2020), and schizophrenia (Macedo et al., 2015). In general, these reviews and meta-analyses have shown small to moderate significant effects on improvement of symptoms, favouring applied games over no intervention or active control conditions.

Applied games include both 'gamification' and 'serious games' (Fleming et al., 2017; Schmidt et al., 2015; Vajawat et al., 2021). Gamification is commonly defined as the use of gaming features in non-game contexts (Cheng, 2020; Deterding et al., 2011; Fleming et al., 2020). Game elements that are often added to interventions include the use of an objective and rules, feedback (e.g., obtaining points, rewards or achievement badges), increasing difficulty levels, competition, customizable avatars/characters and narratives (Brown et al., 2016; Cheng et al., 2019; Cugelman, 2013; Park & Bae, 2014; Seaborn & Fels, 2015). These elements, however, do not necessarily focus on playfulness and fun (Fleming et al., 2017). On the other hand, serious games are experiences that seek to leverage the structure and function of play: they are designed to be immersive and entertaining as well as incorporating evidence-based therapeutic techniques (Eichenberg & Schott, 2017; Fleming et al., 2014; Zayeni et al., 2020). Although gamification and serious games may seem easily distinguishable, various definitions exist and the terms have been used interchangeably in the literature (Cheng et al., 2019; Eichenberg & Schott, 2017; Fleming et al., 2017; Lau et al., 2017; Seaborn & Fels, 2015; Vajawat et al., 2021). Therefore, the current review did not distinguish between gamified and serious game interventions.

Casual Games

The effects of regular entertainment games or commercially available, off-the-shelf games (herein 'casual games') on mental health have also been examined. These games are affordable and widely available for the general population and provide repeated training for target behaviours (Ceranoglu, 2010; Colder Carras et al., 2018; Granic et al., 2014; Steadman et al., 2014). Casual games are built for player enjoyment and recreational purposes (Deterding, 2015; Fullerton, 2014), with no consideration of their therapeutic potential. While casual games are not explicitly designed to improve mental health, players may feel better after playing them and skills that may relate to improved mental health can also be learned (Ferguson & Olson, 2013; Olson, 2010; R. Pine, Sutcliffe, et al., 2020). Casual games can also trigger positive emotions (McGonigal, 2011; Osmanovic & Pecchioni, 2016; Ryan et al., 2006) which, in turn, aid individuals in expanding their momentary thought-action repertoires and personal resources (Fredrickson, 2001; Quinn et al., 2012). There may also be benefits from games that trigger intense (negative) emotions, which may allow the player to practice coping strategies in a safe environment (Granic et al., 2014). In addition, playing casual games may provide distraction from problems and worries, showing beneficial effects on mood (Bowman & Tamborini, 2012). Finally, casual games may evoke the experience of flow, intrinsic motivation and basic need satisfaction, which have been linked to mental health benefits as well (Nakamura & Csikszentmihalyi, 2009; Ryan et al., 2008; Ryan et al., 2006; Sherry, 2004). Indeed, there is growing evidence for the potential of a wide range of casual games to improve emotion regulation (Villani et al., 2018) and alleviate mental health problems, such as depression, anxiety, and stress (Kowal et al., 2021; Lee et al., 2021; Li et al., 2016; Pallavicini et al., 2021), posttraumatic stress symptoms (Holmes et al., 2009; E. L. James et al., 2015), autism (Fang et al., 2019; Lima et al., 2020), and schizophrenia (Suenderhauf et al., 2016).

Nonspecific Effects in Games

As outlined earlier, applied games are developed to train or promote behaviour change. Specific therapeutic techniques are designed as training elements in these games and these techniques are assumed to be responsible for improvements in mental health outcomes, though these links are rarely investigated explicitly (for an exception, see Wols et al., 2018). At the same time, casual games do not incorporate any therapeutic techniques in particular, and yet playing casual games may have mental health benefits. These findings suggest that factors other than the incorporated therapeutic techniques may contribute to improvements in mental health.

In the clinical literature, positive intervention outcomes are, in part, explained by nonspecific factors. These are elements that are not specific to any psychotherapeutic school but nevertheless contribute to mental health improvements (e.g. Grencavage & Norcross, 1990; Ilardi & Craighead, 1994).

A consistent and large body of evidence in the therapy literature shows that nonspecific factors play an important role in treatment efficacy and improve mental health outcomes, and even more so than specific therapeutic techniques that are based on theories about mental health disorder onset and maintenance (Ahn & Wampold, 2001; Lambert, 2005, 2011; Messer & Wampold, 2002; Wampold, 2001). Examples of the most important and most researched nonspecific factors include the client-therapist relationship (Krupnick et al., 2006; Norcross, 2002), expectations for improvement (Asay & Lambert, 1999; Crum & Phillips, 2015; Kazdin, 1979), hope (Ilardi & Craighead, 1994), mindset or implicit theories of beliefs about the malleability of personal attributes (Crum & Phillips, 2015; Dweck, 2017b; Tamir et al., 2007), and motivation or readiness to change (Dozois et al., 2004; Lewis et al., 2009; Norcross et al., 2011; Prochaska & Norcross, 2001; Taylor et al., 2012).

Although nonspecific factors play a significant role in treatment outcomes, research on games for mental health has largely overlooked these factors (Enck et al., 2017; Torous & Firth, 2016). Considering the central role of nonspecific factors in nearly all psychological interventions (Lambert, 2005), it is likely that these factors, at least in part, contribute to the improvements in mental health observed in applied games as well. Interestingly, two recent randomised controlled trials each investigating the effectiveness of an applied game relative to a casual game, showed equal improvements in anxiety symptoms for both the intervention and control group (Scholten et al., 2016; Schoneveld et al., 2016). This suggests that both specific and nonspecific factors played a role in the applied games. Therefore, it is crucial to examine whether positive intervention outcomes are caused by the specific therapeutic techniques incorporated in the game or by nonspecific factors.

Contributions and Aims of the Current Review

Systematically reviewing the effectiveness of both applied and casual games for mental health as well as current research trends would contribute to moving this rapidly growing field forward. Although several reviews and meta-analyses have already been conducted on this topic, these previous studies focused on either applied or casual games exclusively (e.g., Cheng et al., 2019; Dewhirst et al., 2022; Eichenberg & Schott, 2017; Lau et al., 2017; Shah et al., 2018), on specific mental health domains (e.g., Barnes & Prescott, 2018; Fang et al., 2019; Fleming et al., 2017; Macedo et al., 2015; Pallavicini et al., 2021; Penuelas-Calvo et al., 2020; Silva et al., 2021; Strahler Rivero et al., 2015; Suenderhauf et al., 2016; Thabrew et al., 2018; Wang et al., 2021), on clinical populations only (e.g., Dewhirst et al., 2022; Eichenberg & Schott, 2017; Horne-Moyer et al., 2014), and/or they did not include multi-modal interventions (e.g., David et al., 2020) and/or positive outcomes such as happiness and relaxation. For a more comprehensive understanding of the impact of games on mental health outcomes, the effectiveness of applied and casual games should be examined simultaneously, across mental health domains, and for both clinical and healthy populations.

The aim of the current review was to provide an overview of randomised controlled studies that have assessed digital games for improving mental health in children, adolescents and young adults and to examine the effectiveness of both applied and casual games. Mental health was defined as including internalising, externalising, neurodevelopmental, psychotic or personality-related outcomes, symptoms or disorders. Specifically, we examined the range of mental health issues that were targeted, specified the populations (clinical or healthy population), and linked this information to the types of games (applied and casual) that were used, and their subsequent effectiveness. In addition, we set out to explore how this type of research is conducted by examining methodological characteristics such as the specific design, type of control group(s), use of 'multi-modal interventions' (i.e., using a digital game in addition to other therapy components), the frequency and duration of sessions, whether nonspecific factors had been taken into account, as well as risk of bias in the individual studies.

METHODS

We followed the guidelines of the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) in conducting the search and writing up the results (Page, McKenzie et al., 2021; Page, Moher et al., 2021). The protocol for the current systematic review was not pre-registered.

Information Sources and Search

To identify relevant studies, PsycINFO, Web of Science, and Pubmed were searched on October 21, 2020 (updated searches were run on October 29, 2021 and July 22, 2023), using keywords related to games and a randomised controlled trial (RCT) design. In general, the search string was: (applied game* OR applied video game* OR applied videogame* OR serious game* OR serious video game* OR serious videogame* OR game* OR video game* OR videogame*) AND (randomi*ed control* trial OR rct). No restrictions were made with regard to the publication date of possible articles and the full databases were thus searched. For all three databases, the search was limited to the English language and, if possible, to relevant age ranges. See Table A.1 (Appendix) for the specifics per database. References were imported into Endnote and duplicates were removed. Missing abstracts were either manually retrieved or the author(s) were approached.

Eligibility Criteria

Studies were included if they fulfilled the following criteria:

1. *Population*: The study was conducted among children or youth, which was defined as a mean age between 6 and 24 years old (Sawyer et al., 2018). When age was not explicitly mentioned in the abstract, the study was initially included during the screening phase such that age could be further checked while reading the full-texts or could be verified with the study authors. Studies were also included when participants in only one trial-arm were within the right age-range (and participants in the other condition were slightly outside the age-range). Abstracts that referred to the participant sample with terms such as 'senior', 'elderly', 'old age', or 'older adults' were excluded. Studies were also excluded when the mean age was below 6 or over 24 years of age, when no age was mentioned, and when the study was not about humans.
2. *Intervention*: The effect of playing a digital game was studied, either in the experimental or control arm of the study. Studies using a digital game in addition to treatment as usual or other therapy components ('multi-modal interventions') and studies including multiple digital games in one trial arm were also included. Studies including board-, card- or gambling games, playground or physical sports games, therapeutic (role-playing) games, and simulations were excluded, as well as studies in which the game was used as an assessment or manipulation tool (e.g., economic game or games to induce stress). During the initial screening process, we erred on the side of over-inclusion and studies were initially included when the title and/or abstract referred to trainings or interventions that were described as computerised, mobile, internet- or web-based, game-based, game-like, gamified, digitalised, or using e-health, game-elements or virtual reality applications. We then doubled-checked the full-texts to make sure studies met inclusion criteria.
3. *Comparison*: No criteria were set with regard to the specific comparison group.

4. *Outcome*: Changes in mental health outcomes were studied and/or a (sub)clinical population was used (i.e., participants were either currently diagnosed with a mental health disorder, experienced elevated mental health symptoms, or had a past diagnosis). Mental health was defined as including internalising (e.g., anxiety, depression, mood, stress), externalising (e.g., aggression, delinquency, conduct-related, oppositional defiant, disruptive), neurodevelopmental (e.g., autism, ADHD), psychotic or personality-related outcomes, symptoms or disorders. Studies were also included when positive mental health outcomes were measured (e.g., happiness, relaxation) or when physiological markers were measured as an outcome (e.g., heartrate, cortisol). Populations and outcomes related to, for instance, addictive or eating disorders, dementia, and stroke were excluded.
5. *Study design*: The study employed an RCT design that adhered to the following three components (Bhide et al., 2018): use of only one single population, eligible participants have an equal chance to be allocated to each condition (i.e., (pseudo) random allocation), and all participants are treated the same with the single and only exception of the actual intervention or control condition (i.e., equal conduct). Both fully randomised and pseudo-randomised controlled trials were included, as well as papers that reported on the same dataset (e.g., primary and secondary outcome papers), on aggregated RCT results or results from one RCT-arm (e.g., secondary analyses). Studies that did not include a comparison condition (e.g., all participants played the same game) were excluded. Editorials, letters to the editor, book chapters, unpublished reports, studies that included no original data (e.g., reviews), study protocols and RCT-studies that did not report on the effectiveness of the intervention were also excluded.

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Review Process and Procedure

Screening of the titles and abstracts was done by two independent reviewers (i.e., the first author and either a research assistant or the second author) and a record was kept regarding the reasons for exclusion, checking all four eligibility criteria. After every ± 250 references a reliability meeting was done, in which disagreements about in- or exclusion were resolved. For the original search, the mean percent agreement (kappa statistic) for eligibility was 95.8% (k = 0.76), with the range over all seven reliability meetings being 95.1–96.4% agreement and k being between 0.73 and 0.78. Due to slight refinement of the eligibility criterion for games (i.e., excluding non-digital games and games used as an assessment or manipulation tool), a second screening round was performed (percent agreement for eligibility 95.9% and k = 0.87). For the first updated search, one reliability meeting was held and the percent agreement for eligibility was 94.1% (k = 0.78). For the second updated search, two reliability meetings were held with percent agreement for eligibility being 92.9% (k = 0.62) and 90.9% (k = 0.61), respectively.

For the papers identified during screening, full-texts were retrieved through Endnote's automatic search, a manual search or by contacting the author(s). In- and exclusion criteria were checked again, in the following order: digital game, mental health, RCT design, and age. As soon as one criterion was not met, the paper was excluded and further criteria were not checked (i.e., an exclusion on 'RCT design' means that the study met the inclusion criteria for digital game and mental health, but not for RCT design, and that age was not further checked). If necessary and available, additional information (e.g., publicly available video of the game, website of the research group) was consulted to determine whether the game met our inclusion criterion. Disagreements on in- and exclusion were discussed and resolved by the two independent reviewers. For the original search and the updated searches, the percent agreement (kappa statistic) for eligibility of the full-texts were 92.5% (k = 0.85), 90.9% (k = 0.82) and 88.7% (k = 0.77), respectively.

To make sure that no relevant papers were missed, a snowballing approach was used in which titles and abstracts were screened from the reference lists of the final included papers and relevant reviews that were identified during the screening phase. In addition, for RCT protocol papers that were identified during the original search in October 2020 and updated search in October 2021, it was checked (in March 2021 and August 2023) whether outcomes had been published in the meantime. References were screened by one of the reviewers and if a paper was deemed possibly relevant, the full-text was retrieved and the in- and exclusion criteria were checked by the same reviewer. If the reviewer thought the reference would match all inclusion criteria, the second reviewer confirmed this (or not) and the article was included (or not).

Assessment of Risk of Bias in Individual Studies

Risk of bias in the included studies was assessed using the revised Cochrane risk of bias tool for randomised trials (RoB 2.0; Higgins et al., 2016; Sterne et al., 2019). The RoB 2.0 addresses five domains of bias: 1) bias arising from the randomisation process; 2) bias due to deviations from the 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. Additional guidance was employed for cluster-randomised trials (Eldridge et al., 2021), adding an extra domain 1b examining bias arising from how individual participants were identified and recruited within clusters. For crossover trials, we utilised data solely from the first period of the trial (i.e., before participants crossed over to the other intervention; see analytic procedure outlined below). In this scenario, the trial represents a parallel group trial (Higgins, Eldridge, & Li, 2022). Therefore, the main RoB 2.0 tool for individually-randomised parallel-group trials was used for crossover trials. In close contact with the first author, a research assistant and the second author applied the tool to each included study and recorded supporting information and justifications for judgements of risk of bias for each domain (i.e., low risk of bias; some concerns; high risk of bias).

Data Extraction and Synthesis

A standardised data extraction Excel-sheet was developed, piloted and adapted by the first author and a research assistant, and used to extract the following information from the included papers: 1) background information and 2) statistics. Extraction of background information was done by the first author and included study objective(s), country of research, target group, type of design, total number of participants randomised, gender and age of the sample, number of intervention arms, characteristics of the interventions (such as name, type, content, and information on number, duration, and frequency of sessions), assessment time points (e.g., pre, post, follow-up), variables measured and key findings. Possibly relevant outcome variables were selected by the first author and for each time point, where relevant and available, the following statistics were extracted: mean, standard deviation or standard error, median, (inter)quartiles (range), sample size, change scores, and within-group effect sizes for the separate intervention arms, as well as results from statistical analyses (e.g., *t*-tests, F-tests) and between-group effect sizes (i.e., Cohen's *d*, *r*, eta square, (un)standardised regression coefficients, and nonparametric equivalents), and notes on the used statistical analyses. As much as possible, intention-to-treat data were extracted. For the second updated search only the directly relevant variables and statistics were extracted. Extraction of the statistics was done by the first author, second author and a research assistant, and independently checked by one of the others, respectively. Weekly meetings with the first author were held to resolve discrepancies and to ensure reliable extraction of future data. When information regarding background information or statistics was insufficient, unclear or missing, inquiries were sent to the corresponding author through e-mail and/or ResearchGate. If needed, reminders were sent, a more recent e-mail address was sought or other study authors were contacted. In case age, gender or mean values and standard deviations were provided for subgroups of participants, these were combined into a single value (Higgins, Li et al., 2022).

Based on the target group and outcome measures, included papers were grouped into 1) studies including a (sub)clinical participant sample, further subdivided per problem behaviour/pathology, and 2) studies including a (mentally) healthy participant sample but in which mental health variables were measured, further subdivided into anxiety in medical settings, momentary effects on positive and negative affect, and studies using a longitudinal design measuring mental health traits. For each subsample target group, a summary in terms of age, gender, study design, country of research, and types of games and interventions being investigated will be provided in the Results section, followed by the calculated effect sizes of the included papers (see analytic procedure outlined below). For the purpose of this review, the specific intervention arms were coded as applied game, casual game, active condition, or passive condition. If an intervention arm included, for instance, both an applied game and treatment as usual or other therapy components, this intervention arm was coded as applied game. For the studies looking at anxiety in medical settings, intervention arms were coded as game distraction or standard care. At the end of the Results section, we will reflect on some methodological characteristics of the included papers (i.e., risk of bias, use of multi-modal interventions, intervention duration, and nonspecific factors).

Analytic Procedure

Because of the broad scope of the review, we intentionally included several types of games (i.e., serious games, gamified interventions, casual games, whether or not combined with treatment as usual or other treatment components) and a wide range of mental health outcomes and target groups. Consequently, the literature search revealed heterogeneity of target groups, outcome measures, interventions being investigated and control conditions being employed, hence a meta-analysis could not be undertaken. In addition, a meta-analysis would result in unit-of-analysis problems if cluster-randomised trials, crossover trials, and multiple intervention comparisons or multiple outcomes of the same trial were to contribute to the meta-analytic synthesis. We do, however, present effect sizes in separate forest plots per target group, grouping studies in subpanels based on the types of interventions that were compared within a study, with the intention to aid interpretation of individual study results and to (informally) explore differences but also overall patterns across studies (McKenzie & Brennan, 2022). Although no meta-analytic summary diamond will be provided in the forest plots and effect sizes will not be weighed by sample size, we deemed this method superior to a narrative review that describes results study by study.

Standardised mean differences (i.e., Cohen's *d* effect sizes with their 95% confidence intervals) at the post-test measurement were calculated by dividing the difference between two means by the pooled standard deviation. The calculation of the pooled standard deviation was adjusted with weights for the sample sizes (Lenhard & Lenhard, 2016; calculator 2). The rationale and choice for specific outcome measures used in the current review are further discussed below in the Results sections of the subsequent subgroups of the (sub)clinical and healthy participant samples. If higher values on the outcome measurement indicated better performance or improvements, the mean value was multiplied by –1 such that a positive effect size indicated a beneficial effect of the experimental group compared to the control group. Effect sizes in the follow-up period were not calculated because not all studies included a follow-up measurement and the follow-up periods differed greatly among the included studies. Effect sizes of 0.2 indicate a small effect, 0.5 a moderate effect, and 0.8 a large effect (Cohen, 1988).

If studies reported standard errors (SE) instead of standard deviations (SD), these were transformed with the following formula: $SD = \mathrm{SE} * \sqrt{n}$ (Barde & Barde, 2012; Higgins, Li et al., 2022). For studies that reported medians and (inter)quartiles (range), skewness was checked prior to transformation (Shi et al., 2020). If data were not skewed, medians and (inter)quartiles (range) were transformed to means and standard deviations (Luo et al., 2018; Wan et al., 2014).

For crossover trials, effect sizes were based on data from the first period of the trial, essentially representing a parallel group comparison. In crossover trials, each participant is randomised to an ordering of interventions and thus receives all interventions in sequence. These types of trials are suitable for evaluating interventions with a temporary effect and when used in the treatment of stable conditions (Higgins, Eldridge et al., 2022). Because skills learned in treatment may not 'wash-out' before participants enter the second phase of the trial (i.e., a carry-over effect is very likely), we deemed it inappropriate to calculate effect sizes on data combined over both periods.

Cluster-randomised trials involve randomising groups of participants (e.g., school classes) to different interventions. A key implication of a cluster design is that participants within a cluster often tend to respond in a similar manner, challenging the assumption of data independence. Statistical analyses should therefore account for the clustering, to prevent artificially narrow confidence intervals and false-positive conclusions (Higgins, Eldridge et al., 2022). When calculating the effect size for cluster-randomised trials, we therefore reduced the sample size to its 'effective sample size' (Rao & Scott, 1992), by dividing the original sample size of each trial arm by a quantity called the 'design effect'. The design effect was calculated with the following formula: 1 + (*M* – 1) * ICC, where *M* is the average size of each cluster and ICC is the intracluster/intraclass correlation coefficient describing the 'similarity' of individuals within the same cluster (Eldridge et al., 2009). The average cluster size was directly extracted, or calculated based on the number of clusters/groups randomised to each intervention arm and the total number of participants in the study. The ICC was directly extracted from the paper or requested from the authors, or an external estimate obtained from another study examining the same or a similar outcome was used (Higgins, Eldridge et al., 2022). Note that in the calculation of the effect size, only the sample size was reduced; means and standard deviations remained unchanged.

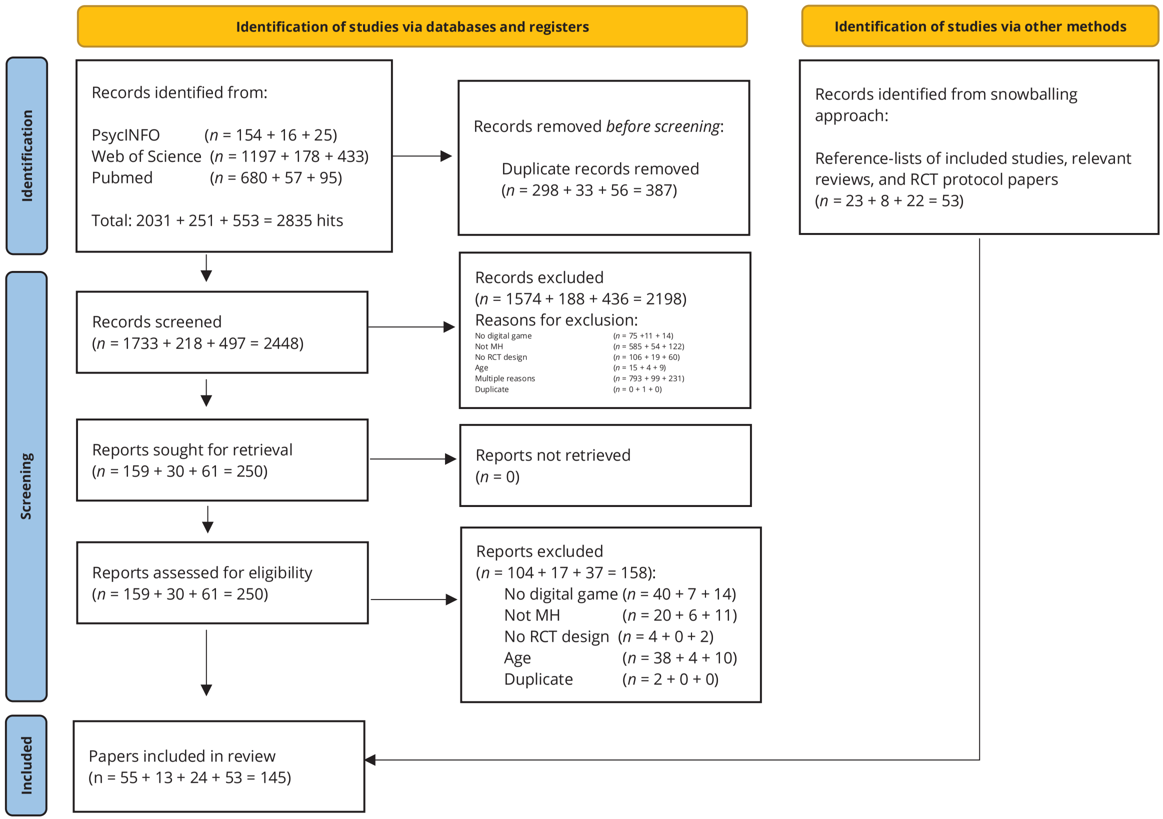
To verify the results, we compared the magnitude, significance and direction of all computed effect sizes with the findings presented in the original paper (McKenzie et al., 2022). Cohen's *d* effect sizes and 95% confidence intervals will be presented in forest plots that were made in Excel (Lajeunesse, 2021). For consistency within and across plots, the unadjusted effect size for cluster-randomised trials will be included in the plot and the adjusted effect size will be reported in a note. If the original paper reported Cohen's *d* as well, this will be included as a note to the plot. It should be noted, however, that these values may deviate due to the specific calculation method followed. All data included in the forest plots are stored on a repository (Radboud Data Repository, https://doi.org/10.34973/t6df-m766).

RESULTS

The literature search resulted in 2835 hits. After removing 387 duplicates, the title and abstract of 2448 studies were screened. During the screening phase, 250 papers were initially included. After reading the full-texts of these papers, 92 were included. An additional 53 papers were identified through snowballing. In total, the current review includes 145 papers. See Figure A.1 for the flow diagram (Page, McKenzie et al., 2021). Endnote libraries with screened, included and excluded papers are available upon request.

Based on the target group and outcome measures, included papers were grouped into two main categories: papers including a (sub)clinical participant sample (n = 75) and papers including a (mentally) healthy participant sample but in which mental health variables were measured (*n* = 70). The papers including a (sub)clinical participant sample were further subdivided per problem behaviour/pathology: ADHD and attentional problems (*n* = 22), autism and social skill challenges (*n* = 17), anxiety (*n* = 17), depression (*n* = 7) and self-injury (*n* = 1), psychosis (*n* = 5), externalising problems (*n* = 3), and other clinical populations (*n* = 4). The papers including a healthy participant sample were further grouped into papers examining anxiety in medical settings (*n* = 24), momentary effects on positive and/or negative affect or emotions (*n* = 14), and papers employing a longitudinal design measuring mental health trait outcomes (*n* = 32).

Figure A.1 Flow diagram of the systematic review



Note. Numbers refer to the original and updated searches, respectively. MH = mental health, RCT = randomised controlled trial.

Clinical Populations

ADHD and Attentional Problems

Twenty studies (described in 22 papers) included participants with ADHD, ADHD symptoms, and/or (neurodevelopmental conditions characterised by) attentional problems. Participants' mean age ranged from 7 to 15 years, and the proportion of male participants varied from 52.2% to 100%. All studies employed a regular RCT design, with the exception of one study (reported by Bul et al., 2018; and Bul et al., 2016) which used a crossover design. See Table A.2 (Appendix) for the characteristics and findings of these studies.

Most studies examined (in)attention as (primary) outcome variable, hence this was taken as (first) outcome of interest. Effect sizes for attention were calculated on data from 14 papers. Attention measured by means of a task was preferred, with (in)attentional problems reported by a clinician (Qian et al., 2018) or parents (Dovis et al., 2015; Shalev et al., 2007; Van der Oord et al., 2014) was considered when task-based measures were not available. Eight papers were not included in the forest plot, because they did not measure attention (Barkın et al., 2023; Benzing & Schmidt, 2019; Bul et al., 2018; Bul et al., 2016; Prins et al., 2011), were part of a study that was already included (van Houdt et al., 2021), or data could not be obtained (García-Baos et al., 2019; Kollins et al., 2020). Nine papers included two intervention arms, comparing the applied game to an active condition (Bikic et al., 2018; Smith et al., 2020), to (a) casual game(s) (Medina et al., 2021; Weerdmeester et al., 2016), to a passive condition (Qian et al., 2018; Van der Oord et al., 2014), or comparing an active condition to casual game(s) (Bikic et al., 2017; Ji et al., 2023; Shalev et al., 2007). Five papers included three intervention arms. The studies of Rodrigo-Yanguas et al. (2023), Steiner et al. (2011) and Van Houdt et al. (2019) included an applied game, active condition and passive condition and therefore each contributed two effect sizes to the plot. Dovis et al. (2015) investigated a full-active, partially-active and full-placebo condition of the applied game. The difference between the full-active and partially-active condition was that working memory was not trained in the partially-active condition. As working memory is not an outcome of interest in the current review, only an effect size comparing the full-active condition to the full-placebo condition was included in the plot. The study by Tullo et al. (2018) included an active condition, a casual game and a TAU condition. However, as the TAU condition did not focus on attention, only an effect size comparing the active condition to the casual game was included in the plot.

As shown in Figure A.4 (Appendix), the six effect sizes comparing an applied game to an active condition ranged from –0.15 to 1.34. Notably, Steiner et al. (2011) reported a significant effect favouring the applied game, while Rodrigo-Yanguas et al. (2023) indicated a significant effect favouring the active condition. Both effect sizes comparing an applied game to a casual game were nonsignificant (–0.01 and 0.12, respectively). The four studies comparing an applied game to a passive condition showed effect sizes of –0.30 to 0.86. Van der Oord et al. (2014) and Qian et al. (2018) reported significant effects favouring the applied game. The four effect sizes comparing an active condition to (a) casual game(s) ranged from –0.04 to 0.53, with only the study of Tullo et al. (2018) showing a significant effect in favour of the active condition.

Given that the majority of papers also measured ADHD symptoms, this was taken as the second outcome of interest. Effect sizes for ADHD symptoms were calculated on data from 12 papers. Data from a questionnaire comprising all ADHD symptoms (i.e., measuring attention, hyperactivity, impulsivity) were preferred, but if not available, a subscale measuring both hyperactivity and impulsivity was used (Dovis et al., 2015; Shalev et al., 2007; Van der Oord et al., 2014). In addition, parent-reported symptoms were used as much as possible as literature shows this is most reliable (Achenbach et al., 1987; Riley, 2004). Only Weerdmeester et al. (2016) measured ADHD symptoms through teacher-reports. Ten papers were not included in the plot, as they did not measure ADHD symptoms (Barkın et al., 2023; Bul et al., 2018; Bul et al., 2016; Ji et al., 2023; Prins et al., 2011; Qian et al., 2018; Tullo et al., 2018), were part of a study that was already included (van Houdt et al., 2019), or data could not be obtained (García-Baos et al., 2019; Kollins et al., 2020). Eight papers included two intervention arms, comparing the applied game to an active condition (Bikic et al., 2018; Smith et al., 2020), to a casual game (Medina et al., 2021; Weerdmeester et al., 2016), or to a passive condition (Van der Oord et al., 2014), comparing (a) casual game(s) to an active condition (Bikic et al., 2017; Shalev et al., 2007) or passive condition (Benzing & Schmidt, 2019). Four papers included three intervention arms. For Rodrigo-Yanguas et al. (2023), Steiner et al. (2011), and Van Houdt et al. (2021) two effect sizes were calculated and for Dovis et al. (2015) one effect size (see the reasoning described in the previous paragraph).

Figure A.5 (Appendix) shows that the six effect sizes comparing an applied game to an active condition ranged from –0.58 to 0.29, with no studies showing a significant effect size. The two effect sizes comparing an applied game to a casual game were both nonsignificant (–0.27 and 0.23, respectively). The four effect sizes comparing an applied game to a passive condition were all nonsignificant and ranged from 0.29 to 0.58. The two effect sizes comparing an active condition to (a) casual game(s) were both nonsignificant (–0.29 and 0.56, respectively). Benzing and Schmidt (2019) showed a nonsignificant effect size of 0.12 when comparing a casual game to a passive condition.

Autism and Social Skill Challenges

Seventeen papers included participants with (high functioning) ASD, ASD and face processing deficits/difficulty in maintaining eye contact/deficient eye gaze processing, ASD and attending autism-specific classes, and participants with social skill deficits or social communication impairments. Participants' mean age ranged from 6 to 13 years, and the proportion of male participants varied from 59.6% to 95.0%. Thirteen papers used a regular RCT design, two papers used cluster randomisation (Beaumont et al., 2015; Murphy et al., 2021), one paper used a crossover design (Beaumont & Sofronoff, 2008), and one paper used both cluster randomisation and a crossover design (Einfeld et al., 2018). See Table A.3 (Appendix) for the characteristics and findings of these studies.

Most papers measured social skills as an outcome variable. Effect sizes for social skills were calculated on data from eight papers. These papers measured social skills through parent-report, with the exception of Sanchez et al. (2017) who included a self-report scale and Murphy et al. (2021) which included a teacher-report scale. The remaining nine papers were not included in the plot because they did not measure social skills as outcome variable (Alvares et al., 2019; De Vries et al., 2015; Dickinson & Place, 2014; Fridenson-Hayo et al., 2017; Kirst et al., 2022; Milajerdi et al., 2021; Sanchez et al., 2014; Sosnowski et al., 2022; Tanaka et al., 2010). The papers included in the plot all employed two intervention arms, comparing the applied game to an active condition (Beaumont et al., 2015; Beaumont et al., 2021; Einfeld et al., 2018; Griffin et al., 2021; Murphy et al., 2021) or to a passive condition (Beaumont & Sofronoff, 2008; Faja et al., 2021; Sanchez et al., 2017). Although both intervention arms in Beaumont et al. (2015) included the applied game, the structured version was categorised as an applied game and the unstructured version as the active condition for the purpose of this review, because Beaumont et al. (2015) originally hypothesised that both conditions would show improvements in social skills with the structured version showing greater gains.

Figure A.6 (Appendix) shows that the five effect sizes comparing an applied game to an active condition ranged from –0.60 to 1.00. Only the study of Beaumont et al. (2021) reported a significant effect favouring the applied game. The three effect sizes comparing the applied game to a passive condition ranged from 0.19 to 1.41, with Beaumont and Sofronoff (2008) and Sanchez et al. (2017) showing significant effects in favour of the applied game.

Anxiety

Fourteen studies (described in 17 papers) included participants with an anxiety disorder, elevated (trait) anxiety symptoms or fear of spiders, of which three studies included participants who additionally had an ASD diagnosis (Wijnhoven et al., 2020), showed elevated levels of externalising problems (Schuurmans et al., 2018) or low mood (McCashin et al., 2022). The studies of Schuurmans and colleagues (Schuurmans, Nijhof, Popma, et al., 2021; Schuurmans, Nijhof, Scholte, et al., 2021; Schuurmans et al., 2020) included participants with clinical levels of post-traumatic symptoms. Participants' mean age ranged from 9 to 22 years, and the proportion of male participants varied from 11.8% to 83.8%. All studies used a regular RCT design. See Table A.4 (Appendix) for the characteristics and findings of these studies.

Given that almost all studies measured anxiety symptoms, this was selected as the primary outcome variable. Effect sizes were calculated on data from thirteen papers. Self-reported anxiety symptoms were used as much as possible, with the exception of McCashin et al. (2022) for which parent-reported anxiety was used. As three papers originated from the same study (Schoneveld et al., 2018), two of them were not included in the forest plot (Schoneveld et al., 2020 reported on secondary outcomes; Wols et al., 2018 focused on effects of one intervention arm). Two other papers were not included in the plot, because anxiety was not measured (Schuurmans, Nijhof, Scholte, et al., 2021) and all intervention arms included applied games (Schuurmans et al., 2020). Eleven papers included two intervention arms, comparing the applied game to an active condition (Beidel et al., 2021; Schoneveld et al., 2018; Schuurmans, Nijhof, Popma, et al., 2021; Schuurmans et al., 2018; Tsui et al., 2021), to a casual game (Haberkamp et al., 2021; Scholten et al., 2016; Schoneveld et al., 2016; Wijnhoven et al., 2020), or to a passive condition (Knox et al., 2011; McCashin et al., 2022). One paper included three intervention arms, namely an applied game, an active condition and a condition including casual games, contributing three effect sizes to the plot (Khanna & Kendall, 2010). In the study of Dennis and O'Toole (2014), four intervention arms were employed. Participants engaged with an applied or placebo version of the game, for either 25 or 45 minutes. For the purpose of this review, we compare the 45-minute gameplay conditions of the applied and placebo version of the game.

As shown in Figure A.7 (Appendix), the seven effect sizes comparing an applied game to an active condition ranged from 0.00 to 1.77. Notably, Beidel et al. (2021) found a significant effect favouring the applied game. The five effect sizes comparing an applied game to casual game(s) ranged from 0.11 to 0.46, with only Schoneveld et al. (2016) finding a significant effect in favour of the applied game. The two studies comparing an applied game to a passive condition found effect sizes of 1.31 and 0.25, respectively, with only Knox et al. (2011) finding a significant effect in favour of the applied game. Finally, the study of Khanna and Kendall (2010) showed a nonsignificant effect size of 0.22 when comparing the active condition to casual games.

Depression and Self-Injury

Six studies (described in seven papers) included participants at risk of developing a depressive disorder, seeking help for it, or experiencing (elevated) depressive or related symptoms. Participants' mean age ranged from 12 to 17 years, and the proportion of male participants varied from 0% to 58.8%. All studies used a regular RCT design, with the exception of the study of Poppelaars and colleagues (reported in Poppelaars et al., 2014; and Poppelaars et al., 2016) in which cluster randomisation was used and the study of Bohr et al. (2023) using both cluster randomisation and a crossover design. See Table A.5 (Appendix) for the characteristics and findings of these studies.

All studies measured self-reported depressive symptoms, hence this was taken as an outcome variable. Effect sizes were calculated on data from six papers. As two papers originated from the same study (Poppelaars et al., 2016), one of them was not included in the forest plot (Poppelaars et al., 2014 focused on player motivation). Four papers included two intervention arms, comparing the applied game to an active condition (Merry, Stasiak, et al., 2012; Stasiak et al., 2014), or to a passive condition (Bohr et al., 2023; Fleming et al., 2012). One paper included three intervention arms, namely two casual games and a passive condition (Poppelaars et al., 2021). As one of the casual games was hypothesised to include mechanisms that could reduce depressive symptoms, this game (i.e., Journey) was coded as applied game for the purpose of this review. This paper contributed two effect sizes to the plot. One paper included four intervention arms, namely an applied game, an active condition, a combined condition (applied game + active condition), and a passive condition (Poppelaars et al., 2016). For the purpose of this review, only the two effect sizes comparing the applied game to the active condition and to the passive condition were included in the plot.

As seen in Figure A.8 (Appendix), the three effect sizes comparing an applied game to an active condition ranged from 0.00 to 0.23. Only the study of Merry, Stasiak, et al. (2012) reported a significant effect in favour of the applied game. The comparison between the 'applied' game Journey and a casual game revealed a nonsignificant effect size of 0.07 in the study of Poppelaars et al.

(2021). Finally, the four effect sizes comparing an applied game to a passive condition ranged from –0.01 to 0.62, all being nonsignificant.

Three studies (described in one paper) focused on self-injury among participants with a recent history of (non-)suicidal self-injury (Franklin et al., 2016). Participants' mean age ranged from 22 to 24 years and the proportion of male participants in the studies ranged from 19.3% to 41.1%. All studies used a regular RCT design and were conducted online. As this was the only paper focussing on self-injury, no effect sizes were calculated and no forest plot was made. See Table A.6 in the Appendix.

Psychosis

Four studies (described in five papers) involved participants within five years of psychosis onset, at high risk for a psychosis, or had psychosis. Participants' mean age ranged from 15 to 21 years, and the proportion of male participants varied from 50.6% to 74.4%. All studies used a regular RCT design. See Table A.7 (Appendix) for the characteristics and findings of these studies.

All studies were interested in assessing the effects on cognition, utilising various subtests to measure cognitive functioning. Because Loewy et al. (2016) designated verbal memory as primary outcome, this concept was also taken from the other papers to compute effect sizes, facilitating cross-study comparisons. Effect sizes were calculated on data from four papers. As the paper from Urben et al. (2012) originated from the same study as Holzer et al. (2014) and only reports on the outcomes at follow-up, this paper was not included in the forest plot. The four papers included in the forest plot all include two intervention arms, comparing an applied game (Fisher et al., 2015; Loewy et al., 2016; Piskulic et al., 2015) or an active condition (Holzer et al., 2014) to casual games.

As shown in Figure A.9 (Appendix), the three effect sizes comparing an applied game to casual games ranged from –0.02 to 0.48. Notably, Fisher et al. (2015) and Loewy et al. (2016) reported significant effects favouring the applied game. Regarding the comparison of an active condition to casual games, the study of Holzer et al. (2014) showed a nonsignificant effect size of –0.24.

Externalising Problems

Three papers included participants with externalising problems, namely children with (symptoms of) foetal alcohol syndrome having problems with disruptive behaviours (Coles et al., 2015), youth with anger problems (Ducharme et al., 2021) and youth with both clinically elevated levels of anxiety and externalising problems (Schuurmans et al., 2018). The latter paper was also assigned to the anxiety category mentioned earlier. Participants' mean age ranged from 7 to 14 years, and the proportion of male participants varied from 60.0% to 83.8%. All studies used a regular RCT design. See Table A.8 (Appendix) for the characteristics and findings of these studies.

Effect sizes were calculated on parent-reported disruptive behaviours (Coles et al., 2015) and mentor-reported externalising problems (Schuurmans et al., 2018). Data could not be obtained for the study of Ducharme et al. (2021). The study of Coles et al. (2015) included three intervention arms, comparing two intervention programs (both including a game and therapy sessions) to a passive condition. The two intervention programs were the same with the exception that the material presented by the game content differed. Since the aim of the study was to evaluate the intervention that trained metacognitive skills, this specific intervention was categorised as the applied game and the other intervention program as the active condition for the purpose of this review. The study of Coles et al. (2015) therefore contributed two effect sizes to the plot. The study of Schuurmans et al. (2018) included two intervention arms, comparing an applied game to an active condition.

As can be seen in Figure A.10 (Appendix), the two effect sizes comparing an applied game to an active condition were –0.60 and 0.09, both nonsignificant. The comparison of an applied game to a passive condition showed a nonsignificant effect size of 0.32.

Other Clinical Populations

Four papers included a clinical population not previously addressed. These papers focused on youth with developmental delays or disabilities, those at high risk of psychopathology, and those with elevated symptoms of psychopathology. Participants' mean age ranged from 5 to 18 years, and the proportion of male participants varied from 25.0% to 59.0%. Three studies used a regular RCT design, while one study used a crossover design (R. L. Hsieh et al., 2016). Because the target groups and outcomes measured differed between these four papers, no effect sizes were calculated. See Table A.9 (Appendix) for the characteristics and findings of these studies.

Healthy Populations

Anxiety in Medical Settings

Twenty-five studies (described in 24 papers) included participants who underwent different medical procedures (e.g., venipunctures, surgeries, cast removals or dental visits). Participants' mean age ranged from 6 to 15 years, and the proportion of male participants varied from 38.7% to 87.4%. All studies used a regular RCT design, with the exception of the study of Kjeldgaard Pedersen et al. (2023) using a crossover design. See Table A.10 (Appendix) for the characteristics and findings of these studies.

Twenty-one studies examined the effects of (both applied and casual) games as a distraction tool to reduce anxiety during the procedure. Nine of those studies used a virtual reality game (Chan et al., 2019, study 1 and 2; Dunn et al., 2019; Gold & Mahrer, 2018; Jivraj et al., 2020; Jung et al., 2021; Kjeldgaard Pedersen et al., 2023; Osmanlliu et al., 2021; Schlechter et al,. 2021) and ten of those studies allowed participants to select a game of their choice (Burns-Nader et al., 2017; Dwairej et al., 2020; Inan & Inal, 2019; Ko et al., 2016; Marechal et al., 2017; Pande et al., 2020; Patel et al., 2006; Sahin & Karkiner, 2022; Sakızcı Uyar et al., 2021; Stewart et al., 2019). In the study of Kumari et al. (2021), participants played a virtual reality game of their choice. Nilsson et al. (2013) also used a game as distraction, however, all participants were given the same game. In three studies, an applied game was provided prior to dental treatment or surgery and the effect on anxiety or pre-operative worries was examined (Elicherla et al., 2019; Fernandes et al., 2015; Matthyssens et al., 2020). Finally, the study of Kassam-Adams et al. (2016) examined the effects of an applied game to prevent posttraumatic stress symptoms in children following medical events.

Effect sizes were calculated for the group of studies that used games as a distraction tool to reduce anxiety during the procedure. Because all games were used as a distraction tool, the distinction between applied and casual games was deemed irrelevant. For the purpose of this review, the effect of game distraction was compared to standard care. Data from 15 studies were used. For five studies, data could not be obtained (Dunn et al., 2019; Gold & Mahrer, 2018; Jivraj et al., 2020; Kjeldgaard Pedersen et al., 2023; Schlechter et al., 2021) and one study did not include a standard care comparison group (Kumari et al., 2021). The primary variable of interest for the current review was self-reported anxiety during the procedure or during mask induction/anaesthesia. For both studies of Chan et al. (2019) and the study of Pande et al. (2020) no measurement during the procedure was available. Therefore, effect sizes were calculated on the post-test measurement. For seven studies no self-reported anxiety was available, hence (clinician) observed anxiety was taken as outcome (Burns-Nader et al., 2017; Dwairej et al., 2020; Jung et al., 2021; Patel et al., 2006; Sahin & Karkiner, 2022; Sakızcı Uyar et al., 2021; Stewart et al., 2019). Ko et al. (2016) measured participants' heartrate as an indication of anxiety. Finally, Nilsson et al. (2013) measured distress during the procedure and anxiety after the procedure. In this case, distress during the procedure was taken as outcome variable. Eight studies included two intervention arms comparing the effect of game distraction to standard care (Burns-Nader et al., 2017; Chan et al., 2019, study 1 and 2; Dwairej et al., 2020; Jung et al., 2021; Osmanlliu et al., 2021) or medication (Marechal et al., 2017; Stewart et al., 2019). Five studies included three intervention arms, additionally adding a condition in which participants watched a video of their choice (Ko et al., 2016), were given lollipops (Nilsson et al., 2013), had their parents present (Patel et al., 2006), were not given any anxiolytic (Sahin & Karkiner, 2022), or watched an informative cartoon about anaesthesia (Sakızcı Uyar et al., 2021). Two studies included four intervention arms, with Inan and Inal (2019) comparing (1) game distraction to (2) watching a cartoon, (3) verbal distraction by parents, and (4) regular procedure without distraction. In Pande et al. (2020), the effect of (1) game distraction was compared to (2) audio distraction, (3) audiovisual distraction in virtual reality (watching a cartoon), and (4) the Tell-Show-Do techniques. For the purpose of this review, medication use (Marechal et al., 2017; Patel et al., 2006; Sahin & Karkiner, 2022; Sakızcı Uyar et al., 2021; Stewart et al., 2019) and the Tell-Show-Do technique (Pande et al., 2020) were coded as standard care as well.

As shown in Figure A.11 (Appendix), effect sizes for the 15 studies ranged from –1.48 to 0.59, with seven studies showing a significant effect size in favour of game distraction (Burns-Nader et al., 2017; Chan et al., 2019, study 2; Dwairej et al., 2020; Inan & Inal, 2019; Jung et al., 2021; Patel et al., 2006; Stewart et al., 2019) and Sahin and Karkiner (2022) showing a significant effect size in favour of standard care.

Positive and Negative Affect

Thirteen studies (described in 14 papers) examined momentary effects of games in a (mentally) healthy sample. All studies included conditions in which participants played a game for one session, with the exception of the study of Andrade and colleagues (reported in Andrade et al., 2019; and Andrade et al., 2020) in which three gameplay sessions within one week took place. After gameplay, state positive or negative affect, or current emotions were measured. Seven studies measured both positive and negative affect/emotions. Participants' mean age ranged from 9 to 23 years, with the majority of studies focusing on adolescents and university students. The proportion of male participants varied from 22.0 to 100%. The studies of Alloway and Carpenter (2021), Branton et al. (2014), Gheller et al. (2019) and Douris et al. (2012) used a crossover design, the study of Andrade and colleagues (2019;

2020) used cluster randomisation, and Poppelaars, Lichtwarck-Aschoff, et al. (2018) and Russell and Newton (2008) randomly allocated individuals in pairs to the conditions. All other studies used a regular RCT design. See Table A.11 (Appendix) for the characteristics and findings of these studies.

Effect sizes were calculated for positive affect/emotions and negative affect/emotions separately. Effect sizes for positive affect/emotions were calculated on data from three papers, with self-reported positive affect (Poppelaars, Lichtwarck-Aschoff, et al., 2018; Russell & Newton, 2008) or vigour (Andrade et al., 2019) as the chosen outcome measures. For four studies data could not be obtained (Alloway & Carpenter, 2021; Branton et al., 2014; Douris et al., 2012; Gheller et al., 2019) and one paper was not included in the forest plot as it used the same data as an already included paper (Andrade et al., 2020). Regarding the papers included in the forest plot, Andrade et al. (2019) compared an active condition to casual (exer)games. For Poppelaars, Lichtwarck-Aschoff, et al. (2018), the condition in which participants received a mental-health focused introduction message was coded as active condition and the condition in which participants received an entertainment-focused introduction message was coded as casual game. In Russell and Newton (2008), three intervention conditions were employed: the conditions of regular bicycle ergometer exercise, interactive video game bicycle ergometer exercise and video game-only were coded as active condition, casual game+ and casual game, respectively, contributing two effect sizes to the plot.

As can be seen in Figure A.12 (Appendix), the three effect sizes comparing an active condition to casual game(s) ranged from –0.06 to –0.50. Only Andrade et al. (2019) showed a significant effect favouring the casual exergames. The comparison of the interactive video game bicycle ergometer exercise (casual game+) with the video game-only condition (casual game) showed a nonsignificant effect size of 0.24.

For negative affect/emotions, effect sizes were calculated on data from nine papers. Outcome measures used were self-reported negative affect (Matheson et al., 2021; Poppelaars, Lichtwarck-Aschoff, et al., 2018; Russell & Newton, 2008), anger (Andrade et al., 2019), hostile feelings/state hostility (Ferguson & Rueda, 2010; Ferguson et al., 2016; Valadez & Ferguson, 2012), and current stress (Hunter et al., 2019), or heartrate in response to a mental arithmetic task (Goodie & Larkin, 2001). Data from five studies were not included in the plot, as data could not be obtained (Alloway & Carpenter, 2021; Branton et al., 2014; Douris et al., 2012; Gheller et al., 2019) or the paper used the same data as an already included paper (Andrade et al., 2020). Regarding the specific intervention arms, the same notes as outlined above apply to

Poppelaars, Lichtwarck-Aschoff, et al. (2018) and Russell and Newton (2008). Ferguson and Rueda (2010) employed four intervention arms: an antisocial violent game, a prosocial violent game, a nonviolent game, and a no-game control condition. For the purpose of this review, we simplified the comparisons and thus the antisocial violent game was compared to the nonviolent game and the passive condition, respectively, contributing two effect sizes to the plot. In the study of Valadez and Ferguson (2012), six intervention arms were employed. Participants played a casual game (i.e., Red Dead Redemption) in violent or nonviolent mode, or a nonviolent casual game (i.e., FIFA), for either 15 or 45 minutes. For the purpose of this review, we compare the 45-minutes gameplay of the violent game condition of Red Dead Redemption to the nonviolent game FIFA. In the study of Matheson et al. (2021), a body image playable was compared to two active conditions, namely an environmental conservation playable and body image social networking posts. Effect sizes for both comparisons were calculated, contributing two effect sizes to the plot. Finally, Hunter et al. (2019) compared an applied game to an active condition and passive condition, contributing two effect sizes to the plot.

As shown in Figure A.13a (Appendix), the three effect sizes comparing an applied game to an active condition ranged from –0.14 to 0.17, with Matheson et al. (2021) showing a significant effect favouring the applied game. Goodie and Larkin (2001) showed an effect size of 1.07, significantly favouring the applied game over a casual game. Hunter et al. (2019) showed a significant effect size of 1.95 when comparing the applied game to a passive condition. In addition, Figure A.13b (Appendix) shows that the three effect sizes comparing a casual game to an active condition ranged from –0.27 to –0.08 and were all nonsignificant. Four effect sizes comparing a casual game (+ or violent) to another (nonviolent) casual game condition ranged from –0.15 to 0.49, with only the study of Russell and Newton (2008) showing a significant effect in favour of the casual game+ over another casual game. Lastly, Ferguson and Rueda (2010) presented a nonsignificant effect size of 0.39 in the comparison of a violent casual game to a passive condition.

Mental Health Traits

Twenty-six studies (described in 32 papers) included a (mentally) healthy participant sample and used a longitudinal design in which participants engaged with the intervention over a longer period of time. Consequently, outcome measures mainly reflect trait outcomes of mental health rather than momentary states. Based on common outcome measures across studies, the included papers were grouped into studies focussing on well-being, internalising symptoms, and general psychological difficulties for the purpose of this review. Some papers fit into more than one group.

Well-Being

Eleven studies (described in 13 papers) measured well-being. Participants' mean age ranged from 12 to 22 years, and the proportion of male participants varied from 0% to 100%. Eight studies used a regular RCT design, while three used cluster randomisation (C. Y. Hsieh & Chen, 2019; Kuosmanen et al., 2017; Sun et al., 2022). See Table A.12 (Appendix) for the characteristics and findings of these studies.

Effect sizes were calculated on data from eight papers. Outcome measures used were self-reported happiness (Nguyen et al., 2018), positive emotions (David et al., 2019b), quality of life (Kato et al., 2008; Schakel et al., 2020; Staiano et al., 2018), and well-being (Kuosmanen et al., 2017; Sun et al., 2022; Walsh et al., 2019). Data from six papers were not included in the plot, because the paper used the same data as an already included paper (David et al., 2019a; David & Fodor, 2022), focused on effects of one intervention arm (David & Magurean, 2022; David, Magurean, et al., 2022) or data could not be obtained (C. Y. Hsieh & Chen, 2019; Ruiz-Ariza et al., 2018). Of the papers included in the forest plot, two compared an applied game to a casual game (Kato et al., 2008; Walsh et al., 2019), three compared an applied game to a passive condition (Kuosmanen et al., 2017; Schakel et al., 2020; Sun et al., 2022), and two compared casual game(s) to a passive condition (Nguyen et al., 2018; Staiano et al., 2018). David et al. (2019b) compared an applied game to both an active and a passive condition and therefore two effect sizes were calculated. Finally, Kato et al. (2008) used different measures on minors and adults to measure quality of life and is therefore included twice in the plot.

The forest plot comprises ten effect sizes (see Figure A.14 in Appendix). The effect size for the study by David et al. (2019b), comparing an applied game to active condition, was 0.18 and nonsignificant. The three effect sizes comparing an applied game to casual game ranged from 0.17 to 0.57, with only Walsh et al. (2019) showing a significant effect size in favour of the applied game. The four effect sizes comparing an applied game to a passive condition ranged from 0.09 to 0.35 and were all nonsignificant. The two effect sizes comparing (a) casual game(s) to a passive condition were –0.13 and 0.18, both nonsignificant.

Internalising Symptoms

Nineteen studies (described in 25 papers) measured internalising symptoms. Participants' mean age ranged from 8 to 23 years, and the proportion of male participants varied from 0% to 100%. Ten studies used a regular RCT design (Cioffi & Lubetzky, 2023; David et al., 2019a, 2019b; David & Fodor, 2022, 2023; David & Magurean, 2022; David, Magurean, et al., 2022; David et al., 2021; David, Stroian, et al., 2022; Egan et al., 2021; Kato et al., 2008; Mannweiler et al., 2023; Schakel et al., 2020; Walsh et al., 2019; Wu et al., 2022; Yu et al., 2023), five studies used cluster randomisation (Axford et al., 2020; Kuosmanen et al., 2017; Perry et al., 2017; Shum et al., 2019; Tuijnman et al., 2022), one study employed a crossover design (Abbott et al., 2014), and one study combined cluster randomisation with a crossover design (Mack et al., 2020). Yunus et al. (2020) and Maden et al. (2022) used a regular RCT design but the intervention group underwent the exergame activity in (random) pairs. See Table A.12 (Appendix) for the characteristics and findings of these studies.

Effect sizes were calculated on data from 15 papers. Outcome measures used were caregiver-reported internalising symptoms (Mannweiler et al., 2023), self-reported depressive mood (disorder) or symptoms (David et al., 2019b; Egan et al., 2021; Kuosmanen et al., 2017; Perry et al., 2017; Wu et al., 2022; Yunus et al., 2020), emotional symptoms (Axford et al., 2020 (teacher-reported); David & Fodor, 2023), anxiety (Cioffi & Lubetzky, 2023; Maden et al., 2022; Shum et al., 2019), mental health (Yu et al., 2023), perceived stress (Kato et al., 2008), and negative affect[1] (Schakel et al., 2020). Data from nine papers were not included in the plot, because the paper used the same data as an already included paper (David et al., 2019a; David & Fodor, 2022), focused on effects of one intervention arm (David & Magurean, 2022; David, Magurean, et al., 2022) or the outcome measure did not match with the outcome measure used by the majority of papers (Abbott et al., 2014; David et al., 2021; David, Stroian, et al., 2022; Mack et al., 2020). In the study of Tuijnman et al. (2022), the outcome measure was comparable to the papers included in the plot, but the aim of the intervention was not to reduce depressive symptoms and depressive symptoms were only measured to examine adverse effects. Therefore, data from this paper was not included in the forest plot. Regarding the papers included in the plot, three studies compared the applied game to an active condition (Egan et al., 2021; Mannweiler et al., 2023; Perry et al., 2017), one to a casual game (Kato et al., 2008) and five studies compared an applied game

1 Similar to the studies examining momentary effects, Schakel et al. (2020) measured negative affect. The study was categorised under internalising symptoms, however, because it employed a longitudinal design.

to a passive condition (Axford et al., 2020; David & Fodor, 2023; Kuosmanen et al., 2017; Schakel et al., 2020; Shum et al., 2019). The studies of Wu et al. (2022), Yu et al. (2023) and Yunus et al. (2020) compared (a) casual game(s) to a passive condition. Three studies compared an applied (David et al., 2019b; Maden et al., 2022) or casual game (Cioffi & Lubetzky, 2023), to both an active and passive condition and these studies therefore contributed two effect sizes to the plot.

Figure A.15 (Appendix) shows that the five effect sizes comparing the applied game to an active condition ranged from 0.01 to 0.45, with David et al. (2019b) and Perry et al. (2017) showing significant effects favouring the applied game. When adjusting for clustering, however, the effect size found in Perry et al. (2017) became nonsignificant. Kato et al. (2008) showed a significant effect size of –0.36 favouring the casual game over the applied game. The seven effect sizes comparing an applied game to a passive condition ranged from –0.08 to 0.66, with three studies finding a significant effect in favour of the applied game (David et al., 2019b; David & Fodor, 2023; Kuosmanen et al., 2017). However, when the effect size was adjusted for clustering, the effect size found in Kuosmanen et al. (2017) became nonsignificant. Cioffi and Lubetzky (2023) found a nonsignificant effect size of 0.59 when comparing a casual game to an active condition. The four effect sizes comparing a casual game to a passive condition ranged from –0.05 to 0.36 and were all nonsignificant.

General Psychological Difficulties

Six studies (described in seven papers) examined general psychological difficulties. Participants' mean age ranged from 8 to 12 years, and the proportion of male participants varied from 0% to 77.8%. Three studies used a regular RCT design (David et al., 2019b; David & Fodor, 2022, 2023; Mannweiler et al., 2023), one study employed a crossover design (Hammond et al., 2014), and two studies used cluster randomisation (Axford et al., 2020; Valenzuela et al., 2022). See Table A.12 (Appendix) for the characteristics and findings of these studies.

Effect sizes were calculated on data from six papers. Outcome measures used to calculate the effect sizes on were self-reported (David et al., 2019b; David & Fodor, 2023; Valenzuela et al., 2022) and teacher-reported total level of psychological difficulties (Axford et al., 2020), and parent-reported social behaviour and emotional/behaviour problems (Hammond et al., 2014; Mannweiler et al., 2023). Data from David and Fodor (2022) was not used as relevant data was already extracted from David et al. (2019b). Regarding the papers included in the plot, one study compared the applied game to an active condition (Mannweiler et al., 2023), two compared an applied game to a passive condition (Axford et al., 2020; David & Fodor, 2023), and one study compared an active condition to a casual game (Hammond et al., 2014). Two studies compared an applied game to both an active and passive condition, and contributed two effect sizes to the plot (David et al., 2019b; Valenzuela et al., 2022).

As seen in Figure A.16 (Appendix), the three effect sizes comparing the applied game to an active condition ranged from –0.10 to 0.33, with only the study of Valenzuela et al. (2022) showing a significant effect favouring the active condition. When adjusting for clustering, however, this effect became nonsignificant. The four effect sizes comparing the applied game to a passive condition ranged from –0.06 to 0.36: Axford et al. (2020) and David and Fodor (2023) found a significant effect in favour of the applied game. When adjusting for clustering, however, the effect size in Axford et al. (2020) became nonsignificant. Finally, the study of Hammond et al. (2014) showed a nonsignificant effect size of 0.13 when comparing the casual games to an active condition.

Methodological Characteristics

Risk of Bias

Figures A.2 and A.3 (Appendix) present both the domain-level and overall risk-of-bias judgements for the included papers. Data from the underlined papers were used to calculate effect sizes and included in the forest plots. Out of the 145 papers included in this review, most demonstrated either low or moderate overall risk of bias (75 and 57 papers, respectively), while 13 were deemed to have a high overall risk of bias. Around a sixth of the included papers showed moderate to high risk of bias due to missing outcome data, bias in measurement of the outcome and/or bias in selection of the reported result.

Multi-modal Interventions

Across both populations, several studies examined the effect of multi-modal interventions, combining in one intervention arm: an applied game and treatment as usual/standard program (Bikic et al., 2018; Bul et al., 2018; Bul et al., 2016; Dickinson & Place, 2014; Fisher et al., 2015; Gold & Mahrer, 2018; R. L. Hsieh et al., 2016; Loewy et al., 2016; Mannweiler et al., 2023; Osmanlliu et al., 2021; Schuurmans, Nijhof, Popma, et al., 2021; Schuurmans, Nijhof, Scholte, et al., 2021; Schuurmans et al., 2018; Wijnhoven et al., 2020), an applied game and other intervention program/therapy components (Axford et al., 2020; Beaumont et al., 2015; Beaumont & Sofronoff, 2008; Beaumont et al., 2021;

Beidel et al., 2021; Coles et al., 2015; Ducharme et al., 2021; Egan et al., 2021; Einfeld et al., 2018; Faja et al., 2021; Fridenson-Hayo et al., 2017; Khanna & Kendall, 2010; Kirst et al., 2022; Knox et al., 2011; Matheson et al., 2021; Matthyssens et al., 2020; Patel et al., 2006; Poppelaars et al., 2014; Poppelaars et al., 2016; Russell & Newton, 2008; Schakel et al., 2020; Shum et al., 2019; Smith et al., 2020; Sun et al., 2022; Tuijnman et al., 2022; Valenzuela et al., 2022), an applied game and casual game (Kato et al., 2008), or casual games and an active condition/treatment as usual (Dwairej et al., 2020; Fisher et al., 2015; Jivraj et al., 2020; Khanna & Kendall, 2010; Loewy et al., 2016; Wijnhoven et al., 2020).

Intervention Duration

Almost all of the studies that examined (sub)clinical populations included multiple sessions over multiple weeks. Only the studies of Alvares et al. (2019) and Dennis and O'Toole (2014) included participants who played one session, and in the study of Haberkamp et al. (2021) participants played the game twice a day for seven days. In studies examining anxiety in medical settings, nearly all interventions included one gameplay session which took place shortly prior and/or during the procedure. Exceptions were the study of Kassam-Adams et al. (2016) in which participants played the game as many times as they wished over a one-month period and the study of Matthyssens et al. (2020) in which participants played the game one week preoperatively. For studies measuring positive and negative affect or emotions all interventions included one gameplay session, with the exception of the study of Andrade and colleagues (2019; 2020) in which participants followed three sessions within a one-week period. The majority of interventions in studies examining mental health traits in a healthy population included multiple sessions or playing at participants' own convenience over multiple weeks. Exceptions were the study of Tuijnman et al. (2022) in which the full program was delivered within one week and the studies of Mack et al. (2020) and Nguyen et al. (2018) in which one session per week for two weeks took place.

Nonspecific Factors

Results showed that nonspecific factors were not systematically measured and examined in the included studies. Only a couple of studies controlled for or measured factors such as expectations (Scholten et al., 2016; Schoneveld et al., 2018; Schoneveld et al., 2016; Wijnhoven et al., 2020), perceived helpfulness or self-reported value of the training (Bikic et al., 2017; Kuosmanen et al., 2017), therapeutic alliance (Khanna & Kendall, 2010; McCashin et al.,

2022) or motivation for treatment (Holzer et al., 2014; Kirst et al., 2022). Most studies examined whether nonspecific factors were equal between intervention arms, which does not give insight into how nonspecific factors relate to improvements. Only two studies examined the association between nonspecific factors and changes in mental health. Wijnhoven et al. (2020) found that children in the control condition (playing a casual game) showed a greater decrease in anxiety symptoms when they had higher treatment expectancies. Holzer et al. (2014) showed that higher motivation was significantly associated with greater improvements in attention but not with visuospatial abilities and delayed memory in youth with (high risk of) psychosis.

DISCUSSION

The aim of the current review was to provide an overview of randomised controlled studies that have assessed the effectiveness of applied and casual games for improving mental health in youth. Specifically, we summarized studies in terms of the mental health issues that were targeted, the populations, what types of games were examined, and their effectiveness. We also examined several methodological characteristics.

Clinical Populations

In total, 145 papers were included in our review of which 75 targeted a (sub) clinical sample. With regard to areas of mental health, most studies focused on ADHD and attentional problems, autism and social skill challenges, and anxiety, followed by depression, psychosis, and externalising problems, respectively. The current findings are in line with previous reviews in terms of the number of studies that examined games in a specific area of mental health (e.g., Cheng et al., 2019; Dewhirst et al., 2022; Ferrari et al., 2022; Shah et al., 2018; Zayeni et al., 2020).

The reasons why applied games are more often used as intervention in certain clinical populations compared to others remain unclear. Possible benefits of applied games may be more pronounced in some clinical populations than others. For example, conventional (talk) therapies may be challenging for youth with autism and ADHD because of their cognitive and social impairments (Silver & Oakes, 2001; Weiss et al., 2008). As a result, applied games have been developed as alternative interventions for these populations. Furthermore, the growing reliance on medication in the treatment of ADHD raises concerns, prompting a call for nonmedical alternatives

(American Academy of Pediactrics, 2011; Graham et al., 2011; Health Council of the Netherlands, 2014). Additionally, games may increase engagement and motivation for therapy in youths with ADHD (Bioulac et al., 2014; Prins et al., 2011). For youth with anxiety, current gold standard therapy (i.e., cognitive behavioural therapy) often involves decontextualised exercises that may not fully represent the authentic emotional and physical experiences of anxious youth (Granic et al., 2014). On the other hand, for depression and psychosis, applied games may be found less suitable as they are unable to detect and deal with crisis situations such as suicide or self-harm (Jiménez-Murcia et al., 2009; Shah et al., 2018). Moreover, for mental health problems such as depression, the pivotal therapeutic ingredient may involve the interpersonal connection with a therapist, rather than playing an applied game by yourself (Sucala et al., 2012). Thus, specific benefits for specific clinical populations may have been differentially related to the effectiveness of applied games.

Across all mental health domains, the majority of studies examined the effects of an applied game, especially in clinical populations (Zayeni et al., 2020). The exception was studies on psychosis, which all included a casual game condition rather than an applied game. Interestingly, only one study in a (sub)clinical population with depressive symptoms examined the effects of two casual games (Poppelaars et al., 2021).

Regarding the type of control groups, results showed a wide variety of control and comparison groups that was used to establish the effectiveness of the digital games. These included active and passive conditions, as well as other game conditions (both casual and applied). Depending on the comparison group, however, effectiveness varied. Games may look stronger in comparison to passive conditions, but active conditions are more scientifically rigorous and will provide the most convincing evidence for the effectiveness of a game as intervention tool (Boot et al., 2013; Connolly et al., 2012). Comparing the effectiveness of applied games directly to that of casual games also has its value. Given that casual games do not incorporate any specific therapeutic techniques but still may have beneficial effects on mental health (e.g., Fang et al., 2019; Kowal et al., 2021; Pallavicini et al., 2021; Suenderhauf et al., 2016), a direct comparison between applied and casual games sheds light on the (additional) effect of the therapeutic techniques incorporated in the applied game. Alternatively, if a casual game is equally effective as an applied game or active (treatment as usual) condition, it would be highly valuable to further examine cost-effectiveness as casual games are already easily accessible and scalable through mainstream media channels and game platforms.

The current review highlights the potential of applied games. Overall, our findings showed that applied games are more effective than passive conditions (shown by 5 studies), equally effective as active conditions (shown by 19 studies), and sometimes even more effective than active conditions (shown by 4 studies). Many comparisons between applied games and active conditions were nonsignificant, meaning that the games were equally effective as conventional, often face-to-face therapies. Most often these were therapies that were strongly rooted in theory, had decades of support for their effectiveness, and served as clinical gold standards. The fact that a video game could compete and show comparable impacts on mental health outcomes is remarkable given these comparison contexts, and suggests that cost-effectiveness may be a topic for further research. It should be noted, however, that a couple of studies indicated that the applied game was not more effective than a passive control condition (Coles et al., 2015; Faja et al., 2021; McCashin et al., 2022), especially when examining ADHD (Rodrigo-Yanguas et al., 2023; Steiner et al., 2011; Van der Oord et al., 2014; Van Houdt et al., 2019; Van Houdt et al., 2021) and depressive symptoms (Bohr et al., 2023; Fleming et al., 2012; Poppelaars et al., 2021; Poppelaars et al., 2016). Considering that both ADHD and depression are highly heterogeneous syndromes with numerous biological and neurological correlates (e.g., Cassano & Fava, 2002; Fried & Nesse, 2015; Olbert et al., 2014; Sonuga-Barke, 2003; Willcutt et al., 2005), it may be that the games were only effective for a small number of youth. Alternatively, it may also be the case that a higher dose of interaction with the game mechanics is needed for ADHD and depressive symptoms to improve (Bohr et al., 2023; Poppelaars et al., 2021; Van der Oord et al., 2014; Van Houdt et al., 2021). In sum, the current review indicates that applied games used in (sub)clinical populations seem most promising and effective for improving social skills, verbal memory in psychosis and anxiety symptoms. These findings are in line with reviews showing that (serious and entertainment) games are effective in improving social skills in autism spectrum disorder (Silva et al., 2021) and cognitive difficulties in psychosis (Cella et al., 2020; Rus-Calafell et al., 2018), and research on traditional therapy showing the largest effects for anxiety (e.g., Weisz et al., 2017).

For casual games, a promising picture emerged showing that they were as effective as both applied games and active conditions in the majority of studies that included a casual game condition. These findings suggest that further research into cost-effectiveness and working mechanisms would be valuable. The current review showed that casual games seem most promising and effective for improving depressive symptoms, anxiety symptoms and ADHD symptoms, respectively. This is especially promising for people with ADHD and depressive symptoms, because the aforementioned findings on applied games for these mental health domains were less positive, and traditional therapy for depression does not show large effect sizes (Weisz et al., 2017; Weisz et al., 2006). The potential of using casual games in clinical practice has been highlighted by previous reviews (e.g., Silva et al., 2021) and our findings provide further support.

Healthy Populations

Out of the 145 papers in this review, 70 included a healthy sample. Approximately one-third examined anxiety in medical settings, a fifth examined momentary effects on positive and/or negative affect or emotions, and almost half of the papers employed a longitudinal design measuring mental health trait outcomes (mainly internalising symptoms and/or well-being). For the papers focussing on anxiety in medical settings, most of them used (mainly casual) games as distraction tools and compared them to standard care. Our findings showed that games were equally, or more, effective as standard care in reducing anxiety. Although this group of papers included a diverse set of samples (e.g., youth undergoing hydrotherapy, wound dressing or venipuncture, outpatient surgery, general anaesthesia, dental or cast room procedures) and some forms of standard care included distractions while others explicitly did not, the findings are in line with previous promising reviews and meta-analyses noting the potential of games as distractions (e.g., Dwairej et al., 2018; Gates et al., 2020; Suleiman Martos et al., 2022).

Regarding momentary effects of games on positive and/or negative affect or emotions, the majority of studies examined the effects of (a) casual game(s). These were compared to an active or passive condition, or to (an)other casual game(s). All studies measured negative affect or emotions, and only a subsample of those also measured positive affect or emotions. In line with previous studies (Bowman & Tamborini, 2012; McGonigal, 2011; Osmanovic & Pecchioni, 2016; Ryan et al., 2006), for positive affect and emotions, we found that casual games were as effective, or more, than active conditions. These results, however, should be interpreted cautiously as effect sizes were only calculated on three studies. For negative affect and emotions, our results showed that the applied games examined were as effective as an active condition (Hunter et al., 2019; Matheson et al., 2021), and, in some instances, more effective than an active condition (Matheson et al., 2021), a casual game (Goodie & Larkin, 2001), and a passive condition (Hunter et al., 2019). For the examined casual games, we found them to be as effective as an active condition

(Andrade et al., 2019; Poppelaars, Lichtwarck-Aschoff, et al., 2018; Russell & Newton, 2008) or passive condition (Ferguson & Rueda, 2010), with also no differences between violent and nonviolent casual games (Ferguson & Rueda, 2010; Ferguson et al., 2016; Russell & Newton, 2008; Valadez & Ferguson, 2012). Only in the study of Russell and Newton (2008), an interactive videogame bicycle exercise was more effective than a video game-only condition in reducing negative affect.

In studies employing a longitudinal design measuring mental health traits, almost two-thirds focused on applied games, while more than a third focused on casual games. The majority of these studies compared the applied or casual game to a passive condition. With regard to mental health domains, well-being and internalising symptoms were most often researched. For well-being, mixed evidence was found with no clear pattern. For internalising symptoms, no clear patterns were found for the effects of both applied and casual games. Six other studies employing a longitudinal design measured general psychological difficulties, but here as well mixed evidence was found (Axford et al., 2020; David et al., 2019b; David & Fodor, 2023; Hammond et al., 2014; Mannweiler et al., 2023; Valenzuela et al., 2022). Overall, these mixed findings resonate with conventional prevention research, showing smaller effect sizes for universal prevention (i.e., in a healthy population) compared to selective and indicated prevention (i.e., in a clinical population; Horowitz & Garber, 2006; Stockings et al., 2016). Future research on digital games in healthy populations may want to focus more on positive outcomes, thereby fostering resilience and well-being rather than preventing negative outcomes (Adachi & Willoughby, 2013; C. Jones et al., 2014; Kelly, 2020).

Multi-modal Interventions

For the purpose of the current review, the specific intervention arms were coded as applied game, casual game, active condition, or passive condition. We noticed, however, several studies that examined the effect of multi-modal interventions in which a digital game and other therapy components were offered simultaneously. Interestingly, most multi-modal interventions were used within (sub)clinical populations. Using games as an adjunct to therapy is common practice in clinical settings (Horne-Moyer et al., 2014). The downside of researching experimental conditions that include a multi-modal intervention and the way in which they were coded in the current review is that it is not possible to single out the (additional) effect of the game (or the standard intervention components for that matter). Additional work is needed to unravel the relative effectiveness of games and to examine how conventional interventions can be optimally enhanced through game add-ons.

Nonspecific Factors

The current review found that nonspecific factors are not systematically measured and examined in randomised controlled studies on games for mental health. Only a couple of studies controlled for or measured expectations, variables related to hope, and motivation for treatment, of which just two studies explicitly examined the association between nonspecific factors and changes in mental health (Holzer et al., 2014; Wijnhoven et al., 2020). A more substantial number of studies measured variables related to adherence, engagement, time of playing or number of completed sessions, and several more measured variables related to satisfaction, enjoyment, likability or experiences with the game. These concepts can be seen as rather indirect measures of nonspecific factors such as motivation and hope, but more direct and concrete measures are needed. Additionally, due to the large heterogeneity of target groups, outcome measures, and intervention and control conditions employed, a meta-analysis and subsequent moderator analyses on these (indirect) measures could not be performed. Overall, future research may be enriched by examining nonspecific factors, utilising standardised measurements for a more comprehensive understanding.

Strengths and Limitations of the Current Review

Particular strengths of the current review are the inclusion of a wide range of mental health areas, both applied and casual games in healthy and (sub) clinical populations, and the inclusion of positive mental health outcomes. No particular keywords related to mental health were used and thus a relatively ‘open’ search was performed, leading to results that were not restricted to specific mental health problems or clinical populations. Additionally, because most mental health problems diagnosed in adulthood begin in adolescence and three-quarters of lifetime diagnosable mental health disorders have started by the age of 24 (Kessler et al., 2005; World Health Organization, 2023), it seemed important to us to use an expanded and more inclusive definition of adolescence, including participants up to age of 24 years (Sawyer et al., 2018). These inclusion criteria were unique to our study and yielded important insights about the promise of games in this age group.

Several limitations of the current review should also be acknowledged. The first limitation relates to our analytic procedure. By calculating effect sizes based on post-data only, control variables or covariates were not taken into account, which may have overestimated our effects. Moreover, the heterogeneity among included studies prevented the examination of effect sizes in the follow-up period. In order to draw conclusions about long-term effects of digital games on mental health, future research should include follow-up measurements. Furthermore, our method did not account for differences in the relative sample sizes of the studies (McKenzie & Brennan, 2022) and together with having the same study contributing multiple effect sizes this may have biased the results. Our aim, however, was to provide a complete overview of research that has evaluated applied and casual games for mental health improvement in youth and to examine current research trends by reviewing methodological characteristics such as type of control group(s) and the use of multi-modal interventions. Making decisions about which comparison or outcome variable from a specific study should be presented, would have undermined our aim. The strength of the current review is that it gives insight into the frequency of certain control groups and the relative effectiveness of the games to these control groups, which is crucial information to move the field forward. At the same time, we acknowledge that the breadth of the current review did not allow us to provide an in-depth examination of the effect of applied and casual games on specific mental health outcomes and this is a trade-off that future research should take into account.

The second limitation of the current review is related to the reliability of the findings. Although the risk-of-bias judgements showed that the majority of included papers had a low or moderate overall risk of bias, the overall strength of the evidence is limited by the number of studies with (comparable) measurable outcomes per mental health area and the available data that could be obtained. For some mental health domains, the number of studies on which effect sizes could be calculated was small, limiting the conclusions that could be drawn from those analyses. Future research should attempt to harmonise outcome measures and to provide all necessary data for systematic reviews and meta-analyses in the paper itself or make them easily accessible through an online repository.

Finally, the current review specifically included studies that employed a randomised controlled design with pre and post measurements and, as a result, studies evaluating digital games through a different design may have been overlooked. Randomised controlled designs, however, follow robust methodological procedures and are considered to provide the highest quality of evidence. Additionally, by far most games currently available have no experimental evidence (Donker et al., 2013) and Bakker et al. (2016) recommend that randomised controlled trials are required to validate future games. Thus, by including only studies employing a randomised controlled design, the current review provides the highest quality of evidence for the effectiveness of games for mental health (Akobeng, 2005).

Recommendations for Future Research

The first recommendation is to reach agreement on clear definitional boundaries of ‘applied game’, ‘gamification’ and ‘serious game’. While conducting the review, we encountered grey areas between the definitions asking ourselves, for instance, when a (gamified) intervention could be called a ‘game’, and whether there would be a distinction between game elements that could be seen as more basic or more complex and sophisticated, being more indicative of gamified or serious game interventions, respectively. Clear definitions would not only aid coding of interventions, but also the interpretation of (possibly mixed) results. Not being able to distinguish between gamified and serious game interventions has limited the conclusions of our review, because one would expect that the quality or level of sophistication of a digital game could matter for its effectiveness (Scholten & Granic, 2019). Gamification, involving the addition of game elements to an intervention, is often done to transform activities perceived to be demotivating into more enjoyable experiences (Turan et al., 2016). While it does not necessarily focus on playfulness and fun (Fleming et al., 2017), it aims to ensure engagement with a task that is less motivating or not entertaining in the first place. Youth today, however, are digital natives (Prensky, 2012), use interactive media and technology daily and have grown accustomed to interacting with highly engaging, sophisticated and immersive contexts (Knowles et al., 2014; The Lancet Child Adolescent Health, 2018). Gamified interventions may not be as attractive, interactive, flexible and responsive to individual needs as youth nowadays would expect and prefer (Liverpool et al., 2020) and, thus, they may prove less effective in improving mental health (Scholten & Granic, 2019). Instead, digital health interventions that stimulate intrinsic motivation to play, offer choice and autonomy over one’s gameplay and that are simply fun (i.e., one would continue playing outside the therapeutic or trial environment), may be more effective in improving mental health (Granic et al., 2014; Scholten & Granic, 2019). Consequently, gamification may be most suitable to test mechanisms of change and basic game mechanics that may be fruitful to iterate on. In contrast, well-designed immersive and entertaining environments that retain youths’ attention and foster replayability might be more suitable to improve mental health for the long term.

On a related note, it is important to mention that our findings showed that for all clinical populations except those with externalising problems, either applied or casual games showed promising effects. It remains unclear why certain clinical populations benefitted more from applied games and others more from casual games. It might be that the casual games were more engaging and intrinsically motivating than the applied games, providing (better) disruption of negative automatic thoughts and enhancing positive emotions in youth with anxiety and depressive symptoms, and increased motivation for therapy in youth with ADHD. Due to the fact that we were not able to distinguish between gamified and serious game interventions, however, further explanations remain speculative. Given the promising results of games on a wide range of mental health outcomes, more research is needed that looks into the working mechanisms of these games.

A second recommendation to improve methodological rigor and to advance research in this field, is to establish standards for evaluation and type of control groups (cf. Boot et al., 2013; Connolly et al., 2012; Zayeni et al., 2020). As this review showed, a wide variety of control and comparison groups has been investigated to establish the effectiveness of digital games. These groups consist of active and passive conditions, multi-modal interventions, and other game conditions including those in which multiple games were provided that participants could choose from. As outlined earlier, effectiveness may vary depending on the comparison group, and although the specific effect of the game remains unknown in multi-modal interventions, they are valuable control conditions as they are more ecologically valid (Spielmans et al., 2010; Weisz et al., 2013). Additionally, conditions providing participants with free choice from a set of games may instil intrinsic motivation and autonomy, increasing engagement and the likelihood for positive intervention outcomes (Granic et al., 2014; Scholten & Granic, 2019). Proper standards for effectiveness evaluation should be established, taking into account additional benefits of certain control groups. In addition, future research may want to take a closer look at the relative effectiveness of different types of control conditions.

Finally, our review uncovered a great deal of variability in outcomes and variables typically measured in studies using games for mental health interventions. The lack of harmonised and/or comparable outcome measures in some areas of mental health limited the conclusions that could be drawn from this. Future research should aim to use standard sets of outcome measures, particularly for nonspecific factors. Furthermore, relatively few studies measured positive outcomes, such as positive affect and well-being. Both the promising effects on positive affect as well as the mixed evidence on well-being that the current review found suggest that there might be interesting and nuanced results from future research with games if a larger range of positive outcomes were to be assessed in the future.

Implications and Concluding Statement

The current review shows that a wide variety of digital games has been investigated for a broad range of mental health symptoms across both clinical and healthy populations. Unsurprisingly, studies on clinical populations mainly examined applied games and studies on healthy populations mainly investigated the effects of casual games. Overall, promising results were found for both type of games, demonstrating the potential of digital health interventions to improve mental health in youth. Nevertheless, research in the field could benefit from clear definitions for applied games, guidelines on evaluation standards, and harmonisation of outcome measures. Future research should further focus on positive outcomes and systematically examine nonspecific factors that may influence symptom improvement as well.

APPENDIX

Table A.1 Search terms and limits per database

Database	Search terms / string	Limits
PsycINFO	(randomi*ed control* trial OR rct).mp OR Exp Randomized Controlled Trials AND (applied game* OR applied video game* OR applied videogame* OR serious game* OR serious video game* OR serious videogame* OR game* OR video game* OR videogame*).mp	English language School age (6-12 years) Adolescence (13-17 years) Young adulthood (18-29 years)
Web of Science	TOPIC: ("randomi*ed control* trial") OR TOPIC: (rct) AND TOPIC: ("applied game*" OR "applied video game*" OR "applied videogame*" OR "serious game*" OR "serious video game*" OR "serious videogame*" OR "game*" OR "video game*" OR "videogame*")	English language
Pubmed	("randomized control trial") OR ("randomized controlled trial") OR ("randomised control trial") OR ("randomised controlled trial") OR ("rct") AND ("applied game") OR ("applied video game") OR ("applied videogame") OR ("serious game") OR ("serious video game") OR ("serious videogame") OR ("game") OR ("video game") OR ("videogame")	English language Child (6-12 years) Adolescent (13-17 years) Young adult (19-24 years)

Table A.2 Characteristics of the studies focussing on ADHD and attentional problems

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Barkin et al. 2023	Turkey	Children with ADHD	176	NA	9.68 (1.02)	7-12 years	1. Therapist guided game based intervention group (TGGIP). 2. Self-oriented game-based program group (SGBP). Both groups played the commercially available mobile game Brawl Stars.	Both conditions: Two 45-minute sessions per week, for 8 weeks. Intervention sessions were conducted remotely.	Pre and post.	Motor skills, game performance: number of games played, number of times won and lost.	The game was effective in increasing motor skills. The TGGIP group was found to be more effective compared to SGBO in improving motor skills.
Benzing & Schmidt, 2019	Switzerland	Children diagnosed with ADHD	51	82.4	10.43 (1.37)	8-12 years	1. Exergaming with 'Shape UP' (**casual game**). 2. Waitlist control group (**passive condition**).	1. Participants trained three times a week for at least 30 minutes, for 8 weeks.	Pre and **post**.	Executive functions by means of a task (inhibition, switching, updating), ADHD symptoms (hyperactivity, inattentiveness, **combined**), general psychopathology, motor abilities, training duration/adherence, valence after each session, heartrate, physical exertion, cognitive engagement, enjoyment.	Compared to the control group, the exergaming group showed improvements in inhibition and switching, general psychopathology and motor abilities.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Bikic et al. 2017	Denmark	Adoles-cents with ADHD	18	76.5	15.6 (0.99)	14-17 years	1. Computer-ised cognitive exercises from the Sci-entific Brain Training (SBT) program (**active condi-tion**). 2. Tetris (**casual game**).	Both conditions: Play for 30 minutes a day, 5 days a week, for 7 weeks. Including weekly phone calls to discuss compli-ance and possible problems.	Pre and **post**.	Visual memory, executive functions, (visual) **sus-tained attention**, working memory, **ADHD symp-toms**; perceived interest and self-reported value of the training, adherence.	No significant differences between conditions on cog-nitive and ADHD-symptom measures after interven-tion. SBT showed a significant pre-post intra-group ben-eficial effect on sustained attention and Tetris a significant positive pre-post intra-group effect on spatial working memory.
Bikic et al. 2018	Denmark	Children with ADHD	70	84.3	9.95 (1.70)	6-13 years	1. Cognitive computer games from the ACTIVATE program (**ap-plied game**) + TAU. 2. TAU (**active condition**).	Both conditions received TAU. TAU consisted of clinical assessment and treatment (psycho-education, parent training, advising the parent and school, medi-cation). In addition, the intervention group was encouraged to use the computer program ACTIVATE for six 40-minute sessions per week, for 8 weeks.	Pre, **post**, 12 and 24-weeks FU.	**Sustained attention**, spatial working memory, reaction time, spatial plan-ning, attention switching, executive functioning, response inhibition; exec-utive functioning, **ADHD symptoms**, functional impairment; age and ADHD subtype (moderator); com-pliance, time of playing, game progress	No significant beneficial effects of ACTIVATE on primary or secondary outcome measures (sus-tained attention, executive functioning and ADHD symptoms). ACTIVATE did had an effect on one exploratory measure, the accuracy in planning, which was main-tained at both FUs.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Bul et al. 2016**	The Neth-erlands and Belgium	Children with ADHD	170	80.6	9.85 (1.26)	8-12 years	1. Plan-It Commander + TAU. 2. TAU cross-over group.	1. Participants received the game intervention in addition to TAU for the first 10 weeks and then received TAU for the next 10 weeks. Participants were instructed to play for a maximum of 65 minutes approx. 3 times per week. 2. Participants re-ceived TAU for the first 10 weeks and crossed over to the game intervention in addition to TAU for the subsequent 10 weeks.	Pre, half-way the trial, and 10-weeks FU.	Time management skills, skills in time perception and organisation (i.e., time-oriented behaviour), executive functioning (i.e., planning and organising skills, working memory), social skills (i.e., coop-eration, responsibility, assertiveness, self-control), self-efficacy; satisfaction with the game.	After 10 weeks, partic-ipants in group 1 com-pared to group 2 showed greater improvements in time management skills, responsibility and working memory. These effects were maintained or further improved in at the 10-week FU for group 1. Participants in group 2, improved on comparable domains of daily life func-tioning over time. In both groups, total social skills improved over time.
Bul et al. 2018**	The Neth-erlands and Belgium	Children with ADHD	170	80.6	9.85 (1.26)	8-12 years	1. Plan-It Commander + TAU. 2. TAU cross-over group	1. Participants received the game intervention in addition to TAU for the first 10 weeks and then received TAU for the next 10 weeks. Participants were instructed to play for a maximum of 65 minutes approx. 3 times per week. 2. Participants re-ceived TAU for the first 10 weeks and crossed over to the game intervention in addition to TAU for the subsequent 10 weeks.	Pre and halfway the trial (i.e., after 10 weeks).	Time management skills, planning/organising skills, cooperation skills; gender, age, intellectual level of functioning, medication use, computer experience, ADHD subtype, severity of inattention problems, severity of hyperactivity/ impulsivity problems, comorbid ODD and CD (moderators).	Girls were most likely to show greater improve-ments in planning/organ-ising skills as compared to the total group of participants. Among boys, those with lower baseline levels of hyperactivity and higher levels of CD symptoms showed more improve-ments in planning/organis-ing skills when they played the game intervention as compared to the total group of participants. No effects of age, IQ, medication use, game experience and ADHD diagnosis were found on the examined outcome measures.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Dovis et al. 2015	The Nether-lands	Children with ADHD – combined type	89	79.8	10.47 (1.33)	8-12 years	1. Full-active condition of Braingame Brian (BGB); training visu-ospatial work-ing memory, inhibition, cognitive flexibility (**ap-plied game**). 2. Partially-ac-tive condition of BGB; train-ing inhibition, cognitive flexibility. 3. Full-place-bo condition of BGB; training tasks were in pla-cebo-mode (**active condi-tion**).	All conditions con-sisted of 25 training sessions of 35-50 minutes, over a 5-week period, in-cluding an external reward system (re-ceiving game-relat-ed stickers, reward ribbons and medals for completing ses-sions) and weekly coaching calls to monitor progress, motivation and compliance, and to solve technical and game-related problems.	Pre, **post** and 3-months FU.	Inhibition, interference control, visuospatial short-term/working memory, verbal short-term/working memory, cognitive flexibil-ity, non-verbal reasoning ability, **inattention, ADHD symptoms**, executive functioning, sensitivity to punishment and reward, general problem/disrup-tive behaviours, quality of life, impact of problem behaviour at home and in public situations; oppo-sitional defiant disorder (covariate); compliance, im-provement during training sessions.	After training, only children in the full-active condition showed improvement on visuospatial short-term memory and working memory. Children in the full-active- and the partially-active conditions improved in inhibitory performance and interference control. Almost all measures showed main time-effects and no interactions with treatment-condition were found, suggesting that transfer to untrained exec-utive functions was mostly nonspecific.
García-Baos et al. 2019	Spain	Children diagnosed with ADHD	28	64.3	11.05 (2.54)	8-15 years	1. Play REC-OGNeyes with eye-tracker, using eyes as game controller. 2. Play REC-OGNeyes with a mouse.	Both conditions: Three 30-minute sessions per week, for 3 weeks.	Pre and post.	Two attention assess-ments measuring several parameters to support the diagnosis of clinical ADHD (probability/severity of ADHD, hyperactivity/ impulsivity indexes, perfor-mance, gaze fixations) and reading difficulties (per-formance, dyslexia index, gaze fixations); comorbid-ity with dyslexia and age (moderators); usability and enjoyability.	Participants from the eye-tracker group improved in impulsivity, reaction time, and fixation gaze control. No changes were found in the mouse-control group.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Ji et al. 2023	Korea	Children with mild to moderate ADHD	42*c	86.7	8.93 (1.52)	8-12 years	1. Exergaming (**casual game**). 2. Bicycle exercise group (**active control**).	Both conditions: Three 50-minute sessions per week, for 4 weeks.	Pre and **post**.	Sustained attention, response control, and event-related potentials were measured during the Go/No-go task; three dimensions of attention were measured by means of a task: selective attention, self-control and **sustained attention**.	Both groups showed increased selective attention, sustained attention and self-control after the intervention, as well as reduced response time on the Go/No-go task. Larger N2 amplitudes were found for the exergaming group compared to the bicycle exercise group.
Kollins et al. 2020	USA	Children with ADHD and cognitive deficits in the attention domain	348	71.3	9.65 (1.30)	8-12 years	1. AKL-T01. 2. Digital word game.	During the intervention period, participants were instructed to use AKL-T01 for 5 sessions a day (total time on task about 25 minutes), 5 days per week, for 4 weeks or the control for 25 minutes per day, 5 days per week, for 4 weeks.	Pre and post.	Attention, inhibitory control, functional impairment, ADHD symptoms, overall improvement, executive functioning; site, baseline attention, age, sex, and stimulant medication (moderators); safety, tolerability (i.e., adverse events), compliance, experience, satisfaction, perceived benefits.	Performance on the main attention measure, as well as additional attention-related measures (i.e., reaction time and response variability) and parent-reported attention significantly improved in the AKL-T01 group compared to the control group. ADHD symptoms and functional impairment significantly improved, similarly in both groups.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Medina et al. 2021	Spain	Children diagnosed with combined-type ADHD	40[*d]	86.2	9.45 (1.27)	8-11 years	1. Fourteen KAD_SCL_01 games (**applied games**). 2. Three commercial video games (**casual game**).	Both conditions: Three 15- to 20-minute sessions per week, for 12 weeks.	Pre and **post**.	Inhibitory control, MEG recording, **attention** and executive functioning by means of a performance task, auditory attention, cognitive flexibility, verbal fluency, card classification, inhibition, verbal memory, visuospatial attention, cognitive functioning, spatial working memory, executive functioning questionnaire (inhibition, shifting, working memory, behavioural and global scales), ADHD symptoms (hyperactivity, attention deficit, behavioural disorder, **global**).	The intervention improved inhibitory control performance, visuospatial working memory performance, and in the cognitive flexibility, working memory, and behaviour and general executive functioning behavioural clinical indexes.
Prins et al. 2011	The Netherlands	Children with ADHD (symptoms)	52[*a]	82.4	9.47 (1.08)	7-12 years	1. Visuospatial working memory training in gaming format. 2. Regular visuospatial working memory training not in gaming format.	Both conditions: Three weekly 30-to-35-minute sessions. After 15 minutes of training, participants could choose during the second half of each session to continue using the training or to stop and read magazines at any time.	Pre and post.	Visuospatial short-term memory and working memory; motivation, game evaluation, time spent not using the training, training performance.	At post, children using the game version of the working memory training showed better working memory, better training performance and greater motivation than children using the regular working memory training.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Qian et al. 2018	Singa-pore	Children with ADHD, either com-bined or inattentive subtypes	66[*d]	100	9.17 (1.42)	6-12 years	1. Brain-com-puter-inter-face-based attention intervention (**applied game**). 2. Non-inter-vention group (**passive condition**).	1. Three 30-minute BCI-based training sessions per week, for 8 weeks.	Pre and **post**.	**Inattention**, internalising problems, brain functional connectivity, brain network topology; age, scanner type (covariates).	Compared to the control group, the intervention group showed greater reduction of inattention symptoms accompanied with differential brain net-work reorganisations after training. Reduced local functional processing in the interven-tion group was associated with less inattentive and internalising problems.
Rodri-go-Yan-guas et al. 2023	Spain	Adoles-cents and young adults with ADHD	104	68.3	14.38 (2.26)	12-22 years	1. The Secret Trail of Moon (TSTM), VR chess-based game (**ap-plied game**). 2. Electronic therapeutic chess (TS; **active condi-tion**). 3. Control group (**passive condition**).	1. Twelve weekly 25-minute ses-sions. 2. Twelve weekly 40-minute ses-sions. 3. The control group participants were called by phone every week, but received no intervention.	Pre and **post**.	Executive functioning questionnaire, ADHD and executive functions questionnaire, emotional intelligence, **ADHD rating scales**, performance task measuring aspects of **attention** and executive functioning, side effects (TSTM group only).	The TSTM group showed improvements in measures of emotional regulation, inattention, and school context. The TC group showed improvements in measures of emotional control, emotional regulation, and inattention. The TSTM and TC groups did not improve executive function symptoms, but they improved in ADHD symptomatology related to emotional regulation.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Shalev et al. 2007	Israel	Children with ADHD	36	83.3	9.14 (NA)	6-13 years	1. Computerised progressive attentional training (CPAT; **active condition**). 2. Playing various commercial computer games, and paper and pencil activities (**casual games**).	Both conditions: Two 1-hour sessions per week over an 8-week period.	Pre and **post**.	Academic performance (i.e., passage copying, math exercises, reading comprehension), **inattentive symptoms**, **hyperactive-impulsive symptoms**.	Participants in the CPAT group showed significant improvements in passage copying, reading comprehension and a reduction of inattentive symptoms. No significant improvements were observed in the control group.
Smith et al. 2020	USA and China	Children with (sub-threshold) ADHD	92	70.0	7.40 (1.10)	5-9 years	1. Integrated brain, body, and social (IBBS) intervention, consisting of computerised cognitive remediation training, physical exercises, and a behaviour management strategy (**applied game**). 2. TAU, including, but not limited to, psychosocial and/or psychopharmacological interventions for ADHD (**active condition**).	1. Forty-five to sixty sessions, delivered 3-4 times per week, 1.5 to 2 hours a day for 15 weeks. 2. Children in the TAU were offered the IBBS intervention following endpoint assessments.	Pre and **post**.	**ADHD symptoms**, overall improvement, verbal learning and memory, spatial working memory and spatial storage manipulation, **sustained attention** and response inhibition***; treatment compliers (subsample analysis), age, medication status, ADHD subtype, treatment site (covariates); compliance.	No significant treatment effects were found on ADHD symptoms and overall improvement scores. The IBBS group showed improvement on verbal working memory, but the result did not survive correction for multiple group comparisons. No other treatment effects were found on any of the remaining neurocognitive outcome measures.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Steiner et al. 2011	USA	Children with ADHD	41	52.2	12.4 (0.90)	NA	1. Neurofeedback (EEG) training (NF) while playing a simple computer game (**applied game**). 2. Cognitive retraining in a standard computer format (SCF), exercises from the BrainTrain software (**active condition**). 3. Waitlist control (**passive condition**).	1-2. Two 45-minute sessions per week, for 4 months. 3. After the final post-intervention assessment, children in the waitlist control group were invited to complete a course of NF or SCF.	Pre and **post**.	Hyperactivity, (in)attention, **ADHD index**, executive functioning, response control functioning, **attention******; satisfaction, observed behaviour during intervention.	Parents of children in the NF condition reported improvements in hyperactivity, (in)attention and ADHD index. Parents of children in the SCF condition reported improvements in (in) attention, ADHD index, and executive functioning. Teachers and students did not consistently report significant change in ADHD symptoms.
Tullo et al. 2018	Canada	Children and adolescents diagnosed with neuro-developmental conditions that are characterised by attentional difficulties*****	129	71.3	13.23 (2.11)	6-18 years	1. 3D Multiple Object-Tracker paradigm, NeuroTracker (**active condition**). 2. The '2048' game, a visual strategy, math-based puzzle game (**casual game**). 3. Treatment as usual.******	1-2. Fifteen 7-minute training sessions over a five-week period.	Pre and **post**.	**Attention**; diagnosis, IQ and age (moderators); progression (i.e., average score) per session.	Only participants in the NeuroTracker group significantly improved in attention. Attention performance did not improve in the other two groups.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Van der Oord et al. 2014	The Nether-lands	Children with ADHD	43	82.5*[b]	9.79 (1.04)	8-12 years	1. Braingame Brian, training visuospatial working memory, inhibitions and cognitive flexibility (**applied game**). 2. Waitlist control (**passive condition**).	1. Twenty-five 40-minute sessions over a 5-week period. 2. After 6 weeks of waiting, participants in the waitlist group also participated in the treatment.	Pre, **post** and 9-weeks FU.	Executive functioning, disruptive behaviours: **inattention**, **hyperactivity/impulsivity**, symptoms related to oppositional deviant disorder and conduct disorder; treatment with methylphenidate (subsample analysis); training performance during the sessions, adherence and dosage.	Children in the training group showed more improvement than those in the waitlist group on executive functioning and parent-rated inattention and hyperactivity / impulsivity symptoms. Effects were maintained at FU.
Van Houdt Et al. 2019	The Nether-lands	Children born very preterm and/or with extremely low birth weight with parent reported attention problems	85	57.7	10.24 (1.18)	8-12 years	1. Braingame Brian, executive functioning training (**applied game**). 2. Placebo training; Braingame Brian without specific training elements (**active condition**). 3. Waitlist control (**passive condition**).	1-2. Twenty-five 30–45-minute sessions over a 6-week period. 3. Children in the waitlist condition did not play Braingame Brian and were instructed to perform the same activities in the waiting period as they normally do.	Pre, **post** and 5-months FU.	Attentional functioning (efficiency of the alerting, orienting and **executive attention** network), behavioural and emotional functioning (i.e., emotional problems, hyperactivity, conduct problems, peer problems, prosocial behaviour), self-perceived competence; time spent gaming outside school-hours (covariate), age (moderator); improvement during training sessions.	No significant differences over time on any of the outcome measures between the three groups. Efficiency of the alerting and executive network, self-perceived competence and teacher-reported hyperactivity symptoms improved over time for participants in all three groups. Correcting for time spent gaming outside school-hours, the analyses showed larger improvements of efficiency of the alerting network for participants in the placebo training arm than those in the EF training arm, and improvements of emotional problems over time for participants in all three groups.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Van Houdt et al. 2021	The Nether-lands	Children born very preterm and/or with extremely low birth weight with parent reported attention problems	85	57.7	10.24 (1.18)	8-12 years	1. Braingame Brian, executive functioning training (**applied game**). 2. Placebo training; Braingame Brian without specific training elements (**active condition**). 3. Waitlist control (**passive condition**).	1-2. Twenty-five 30–45-minute sessions over a 6-week period. 3. Children in the waitlist condition did not play Braingame Brian and were instructed to perform the same activities in the waiting period as they normally do.	Pre, **post** and 5-months FU.	ADHD symptoms (i.e., attention, hyperactivity/ impulsivity, **total score**), executive functioning, verbal working memory, visuospatial working memory, inhibition, cognitive flexibility, academic performance (i.e., arithmetic and technical reading).	No significant differences in improvement over time between the three groups on parent- and teacher ratings of attention and executive functioning, computerised executive functioning tasks and academic performance. Teacher-reported working memory and overall executive functioning, visuospatial working memory, inhibition, and academic performance improved over time for participants in all three groups.
Weerd-meester et al. 2016	The Nether-lands	Children with an ADHD diagnosis or elevated ADHD symptoms	73	79.5	9.77 (1.74)	6-13 years	1. Adventurous Dreaming Highflying Dragon, targeting inattention, impulsivity, hyperactivity and motor deficiency (**applied game**). 2. Angry Birds Trilogy (**casual game**).	1-2. Six 15-minute sessions, over the course of 3 weeks.	Pre and post.	**ADHD symptoms**, **sustained attention**, impulsivity, fine and gross motor skills; engagement, satisfaction (i.e., game evaluation).	Marginally significant group differences in the improvement of teacher-reported ADHD symptoms, with the intervention group showing greater improvement in comparison to the control group. Both groups showed equal improvement in fine motor skills, no change was found in gross motor skills. A deterioration in sustained attention was found for both groups and the intervention group showed a greater increase in impulsivity than the control group.

Notes. Intervention arms and variables measured in bold are included in the forest plot. All studies used a regular randomised controlled trial (RCT) design, unless otherwise stated. ADHD = attention-deficit/hyperactivity disorder; ASD = autism spectrum disorder; CD = conduct disorder; EEG = electroencephalography; FU = follow-up; MEG = magnetoencephalography; NA = not available and/or could not be obtained; ODD = oppositional defiant disorder; TAU = treatment as usual.
* Gender and/or mean age are reported for included/analysed participants (and not for the total randomised participants), with [a] n =51, [b] n=40 (gender only), [c] n = 30, [d] n = 29.
** Crossover RCT.
*** Data available for US sample only.
**** Attention was not measured for the waitlist control group and therefore this paper is included once in the attention plot and twice in the symptoms plot.
***** 31.8% ASD, 32.6% ADHD, 14.0% Intellectual Disability, 10.9% Language Disorder, 2.3% Specific Learning Disorder, 8.5% Other Neurodevelopmental Disorder.

****** As TAU did not focus on attention, this group was not put in the plot.

Table A.3 Characteristics of the studies focusing on autism and social skill challenges

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Alvares et al. 2019	Australia	Children diagnosed with ASD	66	84.8	8.45 (2.21)	5-12 years	1. Frankie and Friends, social attention training game. 2. Control/ sham version of Frankie and Friends.	1-2. One 15-minute session.	Pre and post.	Social attention orienting, social attention maintenance; social and communication difficulties, comorbid ADHD (moderators); in-game training data.	Compared to the control group, the training group showed a significant increase in one of the social attention orienting measures. No training effect was observed on attentional maintenance to social stimuli.
Beaumont et al. 2015***	Australia	Children with high-functioning ASD	69	92.8	9.54 (1.57)	7-12 years	1. Structured version of the (modified) Secret Agent Society (SAS) program; manualized intervention delivered by facilitators, including the SAS computer game (**applied game**) and group sessions. 2. Unstructured SAS program, provided by facilitators "as they saw fit", including the SAS computer game and free use of several resources (**active condition**).	1. Ten 90-minute (or twenty-five 45-minute) group sessions over a 10-week period. 2. Computer game pack materials were freely used over a 10-week period.	Pre, **post** and 6-weeks FU.	Social competence, **emotion regulation and social skills**, child anxiety, anxiety and anger management strategies, child internalising and externalising behaviours, teacher's efficacy in dealing with problem behaviours; treatment fidelity, qualitative program feedback.	Both program variants led to improvements in social competence, emotion regulation and social skills, emotion management strategies, and problem behaviours at school and home. In general, the structured intervention showed superior treatment outcomes. Results were maintained at 6-weeks FU.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Beaumont & Sofronoff, 2008**	Australia	Children with Asperger syndrome	49	89.8	9.72 (1.22)	7-11 years	1. Junior Detective Training Program (JDTP)/ Secret Agent Society (SAS) Program, consisting of the JD computer game (**applied game**), small group therapy sessions, parent training sessions and teacher handouts. 2. Waitlist control (**passive condition**).	1. Eight sessions within a 7-week period. 2. Once the treatment group completed the intervention, the program was offered to the waitlist group.	Pre, **post**, 6-weeks and 5-months FU.	Social competence, **social skills**, emotion recognition, anxiety and anger management strategies.	Compared to the waitlist group, participants in the treatment group showed greater improvements in social competence and social skills, and were better able to suggest appropriate emotion management strategies at post-test. Parent-rated social competence and social skills were maintained at the FU's. Emotion recognition improved in both groups over the course of the program.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Beaumont et al. 2021	Australia	Children with autism spectrum disorder	70	85.7	9.89 (1.37)	7-12 years	1. (adapted) Secret Agent Society program, consisting of the SAS game (**applied game**), one therapist-led parenting webinar, visual support cards, parent training slides, therapist-led weekly online video coaching sessions, and program delivery guide. 2. Caregiver-supported cognitive skills training; Central Intelligence Agency (CIA) control condition, consisting of a similarly structured program and online cognitive activities, but with no social or emotional skill training components (**active condition**).	1-2. one 2.5-hour webinar, weekly online coaching sessions (max. 30 minutes), rest of the program (10 sessions) done at home in own time, all over a period of 10 weeks. All CIA participants were offered the SAS intervention six weeks after the completion of their CIA program.	Pre, **post** and 6-weeks FU.	**Social skills** (**near transfer**), social skills (far transfer), child anxiety, disruptive behaviour; program satisfaction, program fidelity and adherence.	Participants following the SAS program improved more than participants following the CIA program on social skills and problem behaviours. Improvements were maintained at 6-week FU. Parent-reported anxiety of children in both groups reduced over time.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
De Vries et al. 2015	The Nether-lands	Children with autism spectrum disorder	121	89.3	10.53 (1.35)	8-12 years	1. Braingame Brian, work-ing memory training version. 2. Braingame Brian, flexi-bility training version. 3. Mock-train-ing version of Braingame Brian.	1-3. Twenty-five 45-minute sessions over a 6-week period (5 sessions each week and the training period included one spare week).	Pre, post and 6-weeks FU.	Visual-spatial working memory and cognitive flexibility were both mea-sured twice, either resem-bling or being different from the training tasks (representing near and far transfer effects, respec-tively); other far-transfer measures: inhibition, sustained attention, ex-ecutive functioning (total, shift, working memory scores), social behaviour, ADHD symptoms, and quality of life; amount of parental support, number of completed sessions.	Children in all condi-tions improved in work-ing memory, cognitive flexibility, sustained at-tention and parent-rat-ed executive function-ing, social behaviour, ADHD symptoms and quality of life. There were no signifi-cant differential inter-vention effects.
Dickin-son & Place, 2014	England (UK)	Children with autism	100	79.0	9.88 (1.87)	5-15 years	1. Nintendo Wii with 'Mario and Sonics at Olympics' + standard school physi-cal education programme. 2. Control group re-ceiving only the standard school physi-cal education programme.	1. Three 15-minute sessions per week, for one academic year, in addition to the regular curriculum consisting of two 30–45-minute physical exercise lessons per week. 2. Two 30–45-minute physical exercise lessons per week.	Pre and post.	BMI, aerobic fitness, speed/agility and aerobic capacity, explosive leg strength, abdominal strength and endurance, hip flexibility.	At the end of one year, the intervention group showed significantly greater improvements on all tests other than flexibility when compared to the control group.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Einfeld et al. 2018****	Australia	Children diagnosed with ASD and attending autism-specific classes	84	89.3	10.71 (1.48)	8-14 years	1. Secret Agent Society (SAS) program, consisting of the SAS game (**applied game**), group sessions, between-sessions practice tasks, parent information sessions, and handout for teachers. Participants kept attending the autism-specific classes (i.e., TAU). 2. TAU (**active condition**).	1. Nine 90-minute child sessions, four 2-hour parent sessions, weekly teacher tip sheets, delivered over a 10-13-week period. Child booster sessions and parent phone calls 3 and 6 months after the end of the weekly sessions. 2. Participants received the standard curriculum for the autism-specific classes. After the TAU-period, participants were given the SAS program.	Pre, **post** and 20-weeks FU.	Social competence, **emotion regulation and social skills**, anxiety and anger management strategies; treatment fidelity.	Following participation in the SAS program, participants showed improved parent-rated social competence, parent-rated emotion regulation and social skills, and anxiety and anger management strategies. Gains were maintained at 12-month FU. Improvements in teacher-rated social competence and teacher-rated emotion regulation and social skills became evident at 12-month FU.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Faja et al. 2021	USA	Children with autism spectrum disorder	70	90.0	9.13 (1.35)	7-11 years	1. Playing computer games that emphasized set-shifting, inhibition and spatial working memory (**applied game**), psychoeducation and coaching EF strategies. 2. Waitlist control (**passive condition**).	1. Up to 10 one-hour long sessions in which children played the training games for 30-40 minutes and received coaching.	Pre and **post**.	Neural measures of EF, inhibition, shifting ability, interference suppression, executive functioning, verbal working memory, decision-making, social cognition, social problem solving, restricted and repetitive symptoms, **social skills**; feasibility, acceptability, qualitative responses about child's improvement.	Relative to the waitlist group, participants in the training group showed a change in neural response and reduced symptoms of repetitive behaviour. No changes in inhibition, shifting ability, interference suppression, parent-reported EF, social cognition and functioning, verbal working memory, and decision-making were found.
Fridenson-Hayo et al. 2017	Israel and Sweden *****	Children with high functioning autism spectrum conditions	Israel: 43*[a] Sweden: 40*[b]	92.1 and 86.1	7.47 (1.25) and 7.11 (0.97)	6-9 years	1. Emotiplay serious game with written parent-child activity guide. 2. Waitlist control group, receiving treatment as usual.	1. Two hours per week, for a period of 8 to 12 weeks.	Pre and post.	Emotion recognition, autism symptoms, social adaptive functioning,	Participants using the serious game improved significantly more than controls on all emotion recognition measures. In Israel, parents of the serious game group reported reduced autism symptoms.
Griffin et al. 2021	USA	Adolescents with a diagnosis of autism spectrum disorder and deficient eye gaze processing	42*[c]	82.5	13.73 (2.70)	10-28 years	1. Social Games for Adolescents with Autism (SAGA) serious game training (**applied game**). 2. Standard care control group (**active condition**).	1. Participants played the game on their own computer at home for three 30-minute sessions per week, for 10 weeks. 2. Participants received treatment as usual in the community.	Pre and **post**.	Feasibility outcomes (attrition/retention rate, adverse events, total gameplay, time engaged in eye gaze tasks), usability/game experience, eye gaze task performance, **social communication skills**, problematic behaviours, social functioning deficits; game time (subgroup analyses).	The intervention group showed increased sensitivity to human eye gaze cues, whereas the control group did not. Increases in sensitivity to human eye gaze cues were associated with improvements in social skills.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Kirst et al. 2022	Germany and Austria	Children with autism spectrum disorder	82	84.1	7.86 (1.47)	5-10 years	1. parent-assisted serious game Zirkus Empathico. 2. parent-assisted computerised training using games targeting non-social skills/knowledge.	1. Six weeks, with a minimum intensity of 100 minutes of training per week, to be done in a minimum of two single training sessions. Modules II/III were complemented with an additional of 40 minutes per week. Module V was required to be used for a minimum of 10 minutes per day. 2. Six weeks, with 100 minutes per week in the context of a minimum of two session per week, complemented with 10 minutes per day for transfer exercises.	Pre, post and 3-months FU.	Empathy, emotion recognition accuracy, emotional awareness, emotion regulation, callous-unemotional traits, general autism symptomatology, well-being, treatment goal achievement, treatment satisfaction (motivation and enjoyment)	Intervention effects were observed at post-test for empathy and emotion recognition. Moderate effects were found on emotional awareness, emotion regulation, and autism social symptomatology. Parents reported treatment goal attainment and positive training transfer.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Milajerdi et al. 2021	Iran	Children with autism spectrum disorder	60	95.0	8.18 (1.50)	6-10 years	1. SPARK. 2. Kinect, tennis game. 3. Control group.	1-2. Children participated in a 14-hour intervention (35 min. p/session; 3 sessions p/week), over a period of 8 weeks. Typically, each session was conducted individually. On a few occasions, two children participated at the same time. 3. Participants received treatment as usual and did not participate in any structured physical activity program during the study period.	Pre and post.	Fine and gross motor skills, executive functions (concept formation, set shifting, mental flexibility).	The SPARK group significantly improved in aiming and catching skills compared to the other two groups. For executive functions, the Kinect group showed more correct responses than the other two groups. Conceptual responses and perseverative errors improved in all groups.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Murphy et al. 2021***	United Kingdom	Children with social commu-ni-cation impair-ments	50	74.0	6.25 (0.79)	4-7 years	1. E-PLAYS intervention (**applied game**). 2. TAU (**active condition**).	1. Twelve weekly 30-minute sessions; eight sessions were played with a teaching assistant and four with a classmate. 2. Usual practice by the speech and language therapist, one-to-one or in a small group.	Pre, **post** and 3-months FU.	Pragmatic language skills, collaborative and communicative skills, **communication difficulties,** behavioural and emotional problems and prosocial behaviour, quality of life, overall language ability; resources used by parents, duration and number of sessions, process evaluation.	Results show that a full-scale RCT appears feasible and warranted to assess the effectiveness of E-PLAYS. Because the study assessed the feasibility of conducting of a full-scale trial, no statistical analyses were undertaken for the outcome measures and only descriptive data were presented. These data suggest that both groups showed improved pragmatic language skills, collaborative and communicative skills and prosocial behaviour over time. Other outcome measures were more mixed.
Sanchez et al. 2014	USA	Children with social skills deficits	36	73.1	NA	8-12 years	1. Playing Adventures aboard the S.S. GRIN, completing episode quizzes and minigames. 2. Waitlist control.	1. Episodes were released weekly for 4 weeks and children had one week to complete each episode, totalling approximately 90 minutes of gameplay. 2. Participants in the waitlist control group got access to the game after the post-test measurement.	Pre, weekly evaluation survey, and post.	Psychosocial distress, behavioural and emotional strength; weekly evaluation survey (usability and likability).	Compared to the wait-list group, participants playing the Adventures game showed a decrease in psychosocial distress and increase in behavioural and emotional strength at post-test.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Sanchez et al. 2017	USA	Children with social skills deficits	69	59.6	8.90 (1.21)	7-11 years	1. Adventures aboard the S.S. GRIN (**applied game**). 2. Waitlist control (**passive condition**).	1. Episodes were released weekly for 9 weeks and children had one week to complete each episode. 2. Participants in the waitlist control group got access to the game after the post-test measurement.	Pre and **post**.	**Social literacy (i.e., social skills knowledge),** social self-efficacy, social anxiety, satisfaction with peer relationships, bullying victimization, bullying perpetration; age, gender (moderators).	Compared to the waitlist control group, participants who played Adventures improved significantly more in social literacy, social anxiety, satisfaction with peer relationships, and bullying victimization.
Sosnowski et al. 2022	USA	Children and adolescents with autism spectrum disorder and difficulty in maintaining eye contact with others or difficulty under-stan-ding others' facial expressions	54	87.0	8.56 (2.84)	4-14 years	1. Lookware^{tm}, a video game-based digital therapeutic. 2. Control video game group without the intervention's therapeutic exercises.	1-2. Participants engaged in three to five 15-minute sessions per week, over a period of 6 weeks.	Pre, mid-treatment (after 3 weeks), and post.	Emotion recognition, feasibility, acceptability and overall evaluation.	Children in the intervention condition showed significant improvements in emotion recognition from pre- to post-test compared to children in the control group. The game was demonstrated to be feasible and acceptable.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Tanaka et al. 2010	USA	Children, adolescents and young adults diagnosed with autism spectrum disorder and with face processing deficits	117*[d]	78.5	10.92 (3.76)	6-12 years	1. Face training with the Let's Face It! Computer-based intervention, comprised of interactive computer games. 2. Waitlist control following TAU.	1. Participants were instructed to play for at least 100 minutes per week; 20 hours over a two- to four-month period. 2. Participants on the waitlist underwent treatment as usual for a comparable period of time.	Pre and post.	Facial identity skills (recognition, matching, perceptual sensitivity, memory), object processing skills (ability to discriminate, memory); age, IQ, autism symptoms, total time on intervention (moderators); treatment compliance.	Relative to the control group, participants in the face training group showed improvements in their face recognition skills (i.e., recognition of eye and mouth face features).

Notes. Intervention arms and variables measured in bold are included in the forest plot. All studies used a regular randomised controlled trial (RCT) design, unless otherwise stated. ADHD = attention-deficit/hyperactivity disorder; ASD = autism spectrum disorder; BMI = body mass index; EF = executive function; FU = follow-up; NA = not available and/or could not be obtained; TAU = treatment as usual.
* Gender and mean age are reported for included/analysed participants (and not for the total randomised participants), with [a] n = 38, [b] n = 36, [c] n = 40, [d] n = 79.
** Crossover RCT.
*** Cluster RCT.
**** Cluster & crossover RCT.
***** Research was conducted in the UK, Israel and Sweden. Only the studies in Israel and Sweden were randomised controlled trials and are therefore included in the table.

2

Table A.4 Characteristics of the studies focussing on anxiety

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Beidel et al. 2021	USA	Children with a primary diagnosis of social anxiety disorder	42	38.1	9.55 (1.84)	7-12 years	1. Social effectiveness therapy for children (SET-C; **active condition**). 2. Pegasys-VR^{tm}; artificially intelligent, web-based application (**applied game**).	1. Twenty-four sessions for a total of 48 hours; two sessions per week. 2. Twenty-four sessions for a total of 36 hours; two sessions per week.	Pre and **post**.	**Social fears (social phobia and anxiety)**, competencies, behavioural problems/ diagnoses, internalising, externalising and total problems symptomology, overall functioning, severity of social anxiety disorder, social skills.	Both programs were equally efficacious in decreasing anxiety and improving social skill in social encounters. Sixty-three percent of children treated with SET-C and 60% treated with Pegasys-VR^{tm} no longer met diagnostic criteria for social anxiety disorder at posttreatment.
Dennis et al. 2014	USA	Highly trait-anxious adults	78	29.5	21.26 (5.70)	17-50 years	1. Short attention-bias modification training (ABMT). 2. Short placebo training. 3. Long ABMT training (**applied game**). 4. Long placebo training (**active condition**).	1-2. One session with 25 minutes of gameplay and two 10-minute breaks. 3-4. One session with 45 minutes of gameplay with brief breaks given as needed.	Pre and **post**.	**State anxiety**, attentional bias, stress reactivity (anxious behaviour and mood were measured before and after a social-evaluative-threat task and a lack-of-control task; both stress tasks were performed after the training); threat-bias, state anxiety and negative mood, age (covariates).	The ABMT reduced subjective anxiety and observed stress reactivity. Only the long ABMT reduced threat bias.
Haberkamp et al. 2021	Germany	Spider-fearful adults	68	11.8	22.73 (3.18)	18-35 years	1. Spider App (**applied game**). 2. Bubble Shooter (**casual game**).	1-2. Both conditions played the game twice a day for approximately 12 minutes, for 7 days.	Pre, **post** and 1-week FU, as well as before and after each session (emotional states only for the Spider App group).	Avoidance behaviour, **spider fear**, depressive symptoms, psychological stress, emotional states (anxiety, arousal, disgust).	Compared to the control group, the intervention group showed less avoidance behaviour and lower anxiety of spiders. No differences between groups on depression or psychological distress were found. Emotional states of anxiety, disgust and arousal after playing the game decreased from day to day.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Khanna & Kendall, 2010	USA	Children with a principal anxiety disorder	49	67.4	10.10 (1.60)	7-13 years	1. Camp Cope-A-Lot (CCAL; **applied game**) 2. Individual CBT (ICBT; **active condition**) 3. Computer-assisted education, support, and attention condition (CESA; **casual games** + active condition)	1. Twelve 35-minute sessions, one per ; first 6 sessions are individual, remaining 6 sessions are therapist guided. Including 2 parent sessions. 2. Twelve 50-minute sessions over 12 weeks, including 2 parent sessions. 3. Twelve 50-minute sessions; 30 minutes of psychoeducation and support, 20 minutes of casual videogames.	Pre, **post**, and 3-months FU.	**Anxiety symptoms**, general functioning, depression; age and gender (moderators); therapeutic alliance, adherence and flexibility; program satisfaction.	At , CCAL and ICBT children showed significantly greater change in anxiety than CESA children. Gains were maintained at follow-up.
Knox et al. 2011	USA	Children and adolescents with anxiety symptoms or a diagnosed anxiety disorder	30*[a]	62.5	12.88 (2.42)	9-17 years	1. Game-based biofeedback (**applied game**) 2. Waitlist control (**passive condition**)	1. Eight sessions, including biofeedback relaxation training, psychoeducation, identification of triggers and signs of anxiety, and in vivo practice. 2. Biofeedback was offered after the completion of the study.	Pre and **post**.	**Anxiety symptoms**, depressive symptoms, trait anxiety.	Significant reduction of anxiety in the intervention group.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
McCashin et al. 2022	Ireland	Children with clinically significant levels of internalising problems (low mood and anxiety)	122	58.2	9.94 (1.26)	8-12 years	1. Pesky gNATs technology-assisted CBT intervention (**applied game**). 2. Waitlist control group (**passive condition**).	1. One orientation/assessment session and seven 45-minute levels, one per week. The game was played alongside the therapist in a collaborative fashion.	Pre, **post** and 3-months FU.	Internalising symptoms, externalising problems, total problem score, with subscales: depressive problems, **anxiety problems**, somatic problems, attention deficit problems, oppositional defiant problems, conduct problems; within-game outcomes (game group only): well-being, therapeutic alliance, anxiety and depression, helpfulness and experiences of the game.	Both intervention and waitlist control groups experienced significantly positive change across all timepoints. No between-group differences were found on primary or secondary outcomes. Reliable clinical indices showed that at post-test more participants from the intervention group went from clinical to non-clinical levels of internalising problems compared to the waitlist control group. Children and parents reported high levels of acceptability, helpfulness and positive change.
Scholten et al. 2016	The Netherlands	Adolescents with subclinical levels of anxiety	138	34.6	13.27 (0.88)	11-15 years	1. Dojo (**applied game**). 2. Rayman 2: The great escape (**casual game**).	Both conditions: two one-hour gameplay sessions per week, for 3 weeks.	Pre, **post**, and 3-months FU.	**Total** and personalised **anxiety symptoms**; age and gender (moderators); game expectations.	Equal significant improvements in anxiety symptoms in both conditions at FU.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Schon-eveld et al. 2018	The Nether-lands	Children with elevated levels of anxiety symptoms	174	40.8	9.97 (1.16)	7-12 years	1. MindLight (**applied game**). 2. CBT (**active condition**).	1. Six one-hour gameplay ses-sions, one per week. 2. Eight sessions (9 hours), one per week; first two sessions lasted 1.5 hours, the last 6 sessions lasted 1 hour. Parents re-ceived information about child prog-ress and general information about the program.	Pre, **post**, 3 and 6-months FU.	**Anxiety symptoms**, personalised anxiety symptoms; gender, age, weekly game time and expectations (moderators); game and program evalua-tions.	Significant decrease in child- and parent-reported anxiety symptoms over time. Magnitude of improve-ment was the same for MindLight and CBT.
Schon-eveld et al. 2016	The Nether-lands	Children with elevated levels of anxiety symptoms	136	45.2	9.95 (1.33)	8-13 years	1. MindLight **(applied game).** 2. Max and the Magic Marker **(casual game).**	Both conditions: 5 one-hour game-play sessions, twice a week.	Pre, **post**, and 3-months FU.	**Total anxiety symp-toms**, personalised anxiety symptoms; age and gender (moderators); game expectations, game evaluations.	Significant reduction in child- and parent-reported anxiety by the 3-month FU, but magnitude of im-provements did not differ between conditions.
Schon-eveld et al. 2020	The Nether-lands	Children with elevated levels of anxiety symptoms	174	40.8	9.97 (1.16)	7-12 years	1. MindLight. 2. CBT.	1. Six one-hour gameplay ses-sions, one per week. 2. Eight sessions (9 hours), one per week; first two sessions lasted 1.5 hours, the last 6 sessions lasted 1 hour. Parents re-ceived information about child prog-ress and general information about the program.	Pre, post, 3 and 6-months FU.	Anxiety symptoms, self-efficacy, internal-ising and externalising problems; baseline anxiety symptoms, baseline self-effica-cy, maternal mental health (moderators).	Significant reduction in internalising and exter-nalising problems and an increase in self-efficacy, sustained up to 6 months. The magnitude of change did not differ between intervention groups. Baseline anxiety levels, maternal mental health problems and self-effica-cy did not influence the change of anxiety over time.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Schuurmans et al. 2018	The Netherlands	Youth with clinically elevated levels of both anxiety and externalising problems	41*[b]	83.8	14.00 (1.88)	NA	1. Dojo (**applied game**) + TAU. 2. TAU (**active condition**).	1. Eight 30-minute gameplay sessions, twice a week for four weeks. 2. Treatment as recommended by participants' clinicians.	Pre, **post**, and 4-months FU.	**Anxiety symptoms**, externalising problems; intellectual disability (moderator); user evaluation.	Youths who played Dojo, compared with youths in the control condition, showed reductions in self-reported anxiety and externalising problems at post-test, and mentor-reported anxiety at both post-test and FU.
Schuurmans et al. 2020	The Nethelands	Youth with clinical levels of post-traumatic symptoms	15	60.0	14.46 (2.40)	10-18 years	1. Muse. 2. DayDream. 3. Wild Divine.	All three interventions make use of game-based meditation-based relaxation techniques and either neurofeedback or biofeedback. All conditions consisted of twelve 15-minute gameplay sessions (twice a week, for six consecutive weeks).	Pre, during each intervention session (physiology only), post, and 1-month FU.	Autonomic nervous system parameters, respiratory sinus arrhythmia parameters, user evaluations, post-traumatic symptoms, depression, anxiety, stress, aggression.	Physiological stress regulation was improved during the meditation sessions of all three interventions. Participants in the Muse condition showed reliable improvements in post-traumatic symptoms, stress and anxiety. Inconsistent progression was found for participants who play DayDream or Wild Divine. Overall, improvements in experienced stress were most robust across all conditions.
Schuurmans, Nijhof, Popma et al. 2021	The Nethelands	Adolescents with clinical levels of post-traumatic symptoms	77	59.7	15.12 (2.29)	10-18 years	1. Muse, game-based and neurofeedback meditation intervention in addition to TAU (**applied game**). 2. Treatment as usual (**active condition**).	1. Two individual 15-20 minutes gameplay sessions a week, for 6 consecutive weeks. 2. TAU consisted of evidence-based treatments that did not specifically target post-traumatic symptoms.	Pre, **post** and 2-months FU.	Post-traumatic symptoms, stress, **anxiety**, depression, aggression, intrinsic motivation; age, gender, IQ, type of residential care, medication use, concomitant therapy (covariates), gender (moderator).	Significantly greater improvements at post-test for post-traumatic symptoms, stress, anxiety, depression and aggression were found in the Muse group compared to the control group. No differences between the groups were found at FU.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Schuurmans, Nijhof, Scholte et al. 2021	The Netherlands	Adolescents with clinical levels of post-traumatic symptoms	77	59.7	15.25 (1.79)	10-18 years	1. Muse, game-based and neurofeedback meditation intervention in addition to TAU. 2. Treatment as usual.	1. Two individual 15-20 minutes gameplay sessions a week, for 6 consecutive weeks. 2. TAU consisted of evidence-based treatments that did not specifically target post-traumatic symptoms.	Pre, post and 2-months FU.	Basal autonomic nervous system measures and parasympathetic nervous system activity (SNS and PNS), basal hypothalamic-pituitary-adrenal (HPA) axis activity (hair cortisol), neurobiological reactivity to acute stress during a social stress task (autonomic nervous system parameters and saliva cortisol); concomitant interventions, smoking, alcohol and caffeine use, sporting behaviour, eating behaviour medication use, comorbid psychological and physiological disorders, hormonal contraception, menstrual cycle phase, sleep, season, time of testing, storage time, age, hair treatment, hair washing (covariates).	The Muse group exhibited lower basal activity for the SNS and increased HPA reactivity to acute stress. No differences between groups were found on SNS and HPA axis activity during rest and on SNS and PNS reactivity to acute stress.
Tsui et al. 2021	Canada	Youth with anxiety symptoms	116*[c]	33.3	13.50 (1.58)	8-15 years	1. MindLight (**applied game**). 2. online CBT (**active condition**).	Both conditions: five one-hour sessions within a 3-week period.	Pre, **post**, and 3-months FU.	**Anxiety symptoms,** state anxiety after stress task, psychophysiological arousal during stress tasks (heartrate); age (moderator).	Anxiety significantly decreased over time in both conditions. Youth in the MindLight condition showed greater pre-to-post reductions in anxiety symptoms.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Wijnhoven et al. 2020	The Nether-lands	Children with an autism spectrum disorder and (sub) clinical levels of anxiety symptoms	122*[d]	77.1	11.10 (2.07)	8-16 years	1. MindLight (**applied game**) + TAU. 2. Triple Town (**casual game**) + TAU.	Both conditions: Game sessions of one hour per week, for 6 weeks. Children received TAU parallel with MindLight or Triple Town. TAU did not focus on anxiety symptoms and therapists were instructed to not focus on anxiety symptoms in TAU.	Pre, **post**, and 3-months FU.	**Anxiety symptoms**, depressive symptoms, problematic social functioning, internal-ising and externalising behaviour problems, anxiety disorders, treatment adherence; gender, age, type of TAU, intervention location, treatment expectancies, total IQ, ethnicity and parental marital status (moder-ators).	The course of child- and parent-rated anxiety symptoms and anxiety remission rates showed that anxiety in both condi-tions decreased to anxiety symptoms beneath (or equal to) the subclinical level at 3-months FU. The decrease in parent-rat-ed anxiety symptoms was significantly larger in the experimental condition.
Wols et al. 2018	The Nether-lands	Children with elevated levels of anxiety	43**	46.5	9.94 (1.14)	8-12 years	1. MindLight.	1. Six one-hour gameplay ses-sions, one per week.	Pre, post, and 3-months FU.	Anxiety symptoms, in-game play behaviours.	Changes in in-game play behaviours representing therapeutic exposure techniques predicted improvements in anxiety symptoms 3 months later (when children had not played the game for 3 months).

Notes. Intervention arms and variables measured in bold are included in the forest plot. All studies used a regular randomised controlled trial (RCT) design. CBT = cognitive behavioural therapy; FU = follow-up; NA = not available and/or could not be obtained; TAU = treatment as usual.
* Gender and mean age are reported for included/analysed participants (and not for the total randomised participants), with [a] n=24, [b] n=37, [c] n=117 eligible participants, [d] n=109.
** Randomisation not applicable as the study used participants from one trial arm of a previous study. Number of participants, gender and mean age refer to the included partici-pants in the current study.

Table A.5 Characteristics of the studies focussing on depression

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assessments	Variables measured	Findings relevant for current review
Bohr et al. 2023****	Canada	Youth exhibiting low mood, negative affect, depressive presentation, or significant levels of stress	50	46.0	15.18 (NA)	13-18 years	1. SPARX (**applied game**). 2. Waitlist control group (**passive condition**).	1. Youth were asked to play the game once a week, for 7 weeks. 2. Youth waited for 7 weeks and then began to play SPARX.	Pre and **post** the intervention of both groups.	**Depressive symptoms**, hopelessness, resilience, cognitive emotion regulation.	Following the SPARX intervention, youth felt less hopeless and engaged in less self-blame, rumination and catastrophizing. No decrease in depressive symptoms or increase in resilience was found.
Fleming et al. 2012	New Zealand	Adolescents (at risk of being) excluded from mainstream education with possible through almost certain depressive disorder	49*[a]	56.3	14.90 (0.79)	13-16 years	1. SPARX (**applied game**). 2. Waitlist control (**passive condition**).	1. Seven modules completed over 5 weeks (1-2 modules per week). 2. After 5 weeks of waiting, participants in the delayed treatment group got the intervention.	Pre, **post**, and 5-weeks FU.	**Depressive symptoms**, quality of life enjoyment and satisfaction, anxiety symptoms, hopelessness, locus of control; adherence, adverse events.	The SPARX group demonstrated improvements in depression scores over the waitlist control group. Gains were maintained at FU.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Merry, Stasiak et al. 2012	New Zealand	Adolescents seeking help for their depressive symptoms	187	34.2	15.56 (1.60)	12-19 years	1. SPARX (**applied game**). 2. TAU (**active condition**).	1. Seven modules delivered over a period of 4 to 7 weeks. 2. Comprising primarily face to face counselling delivered by trained counsellors and clinical psychologist.	Pre, **post**, and 3-months FU.	**Depressive symptoms**, clinical depression, quality of life enjoyment and satisfaction, hopelessness, anxiety symptoms, overall improvement; age, gender, ethnicity, setting, baseline depression and type of TAU (moderators); safety data, adherence rates, satisfaction with and enjoyment of the intervention. *	Both groups showed a reduction in depressive symptoms at post-test. There were no differences in response rates between the groups on the observer rated scale; self-reported depressive symptoms improved more in the SPARX group compared to TAU. Remission rates were significantly higher in the SPARX group than in the TAU group. Improvements were maintained at FU.
Poppelaars et al. 2021	The Netherlands	Youth with elevated depressive symptoms	244	33.6	17.11 (1.76)	15-20 years	1. Journey (**applied game****). 2. Flower (**casual game**). 3. Passive condition (**passive condition**).	1-2. Participants were given 4 weeks to complete the game; no further restrictions were given to encourage naturalistic gameplay. 3. Participants continued their normal routine and only completed questionnaires. They were offered Journey and/or Flower after the last questionnaire.	Pre, **post**, 6 and 12-months FU.	**Depressive symptoms**; age, rejection sensitivity, narrative identity, hope and optimism, coping strategies (moderators); game engagement and experience.	All participants decreased in depressive symptoms. Results showed no beneficial effects of playing Journey on youth's change in depressive symptoms above and beyond the Flower and passive control groups.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Pop-pelaars et al. 2014***	The Nether-lands	Adoles-cents with elevated depressive symptoms	208	0.0	12.83 (0.76) *****	11-16 years	1. SPARX . 2. Op Volle Kracht (OVK). 3. SPARX + OVK. 4. Monitoring condition.	1. Seven levels, 20-40 min. p/ level, one level p/ week. 2. Eight one-hour lessons in groups of 10-15 girls, one per week. 3. SPARX game-play combined with 8 sessions of OVK. 4. Weekly depres-sion question-naires during the intervention period. Par-tic-ipants could participate in the intervention of their choice after the final FU assessment.	Pre, post, 3, 6 and 12-months FU.	Depressive symp-toms, autonomous and controlled motivations.	No differences in autonomous and controlled motiva-tion were found between conditions at any time point. However, OVK was negatively associated with autono-mous motivation during the program, while SPARX and the OVK and SPARX combined were associated negatively with controlled motivation during the programs. Additionally, autonomous motivation and controlled motivation at the start of the interventions and controlled motivation half-way through the interventions was found to positively influence long-term depressive symptoms (at one-year FU).

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Pop-pelaars et al. 2016***	The Nether-lands	Adoles-cents with elevated depressive symptoms	208	0.0	13.35 (0.71)	11-16 years	1. SPARX (**applied game**). 2. Op Volle Kracht (OVK; **active condition**). 3. SPARX + OVK. 4. Monitoring condition (**passive condition**).	1. Seven levels, 20-40 min. p/level, one level p/week. 2. Eight one-hour lessons in groups of 10-15 girls, one per week. 3. SPARX gameplay combined with 8 sessions of OVK. 4. Weekly depression questionnaires during the intervention period. Participants could participate in the intervention of their choice after the final FU assessment.	Pre, weekly during intervention period, **post**, 3, 6 and 12-months FU.	**Depressive symptoms**, suicidal ideation, evaluation and satisfaction with the program(s).	Depressive symptoms decreased significantly in all conditions, with no difference in depressive symptoms between conditions.
Stasiak et al. 2014	New Zealand	Depressed adolescents who had referred themselves for help with low mood to school counsellors	34	58.8	15.2 (1.50)	13-18 years	1. The Journey (**applied game**). 2. Control attention placebo program with psycho-educational content (computerised psychoeducation; CPE; **active condition**).	Both conditions: Seven 25-30 minutes modules, accompanied by a paper guidebook. Participants were asked to complete the program one module at a time in 4 to 10 weeks.	Pre, **post**, and 1-month FU.	**Depressive symptoms**, quality of life, coping strategies; age and gender (moderators), completion rates and satisfaction ratings, treatment blinding.	Adolescents treated with the cCBT version of The Journey showed greater observer-rated depressive symptoms improvement than those in the control attention placebo group. Improvements were maintained at 1-months FU. Self-reported depressive symptoms reduced from baseline to post in both groups, but the difference did not reach statistical significance.

Notes. Intervention arms and variables measured in bold are included in the forest plot. All studies used a regular randomised controlled trial (RCT) design, unless otherwise stated. (c)CBT = (computerised) cognitive behavioural therapy; FU = follow-up; NA = not available and/or could not be obtained; TAU = treatment as usual.
* Gender and mean age are reported for included/analysed participants (and not for the total randomised participants), with [a] n=32.
** Journey is a casual (commercial) game but was categorised as applied game for the purpose of this review as the game includes hypothesised mechanisms that could reduce depressive symptoms.
*** Cluster RCT.
**** Cluster & crossover RCT
***** Due to a round-off difference the mean age slightly differs from the 2016 page.

Table A.6 Characteristics of the studies focussing on self-Injury

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Franklin et al. 2016 – study 1	Online study, majority from USA	Individuals with a recent history of frequent nonsuicidal self-injury	114	19.3	23.02 (5.47)	18-NA years	1. Active version of the TEC app. 2. Control version of TEC.	Both conditions: Participants were allowed to access the TEC app as much as they desired over a one-month period.	Pre, weekly during intervention period, and post.	Self-injurious thoughts and behaviours, dysregulated behaviours, implicit aversion to nonsuicidal self-injury, death/suicide and the self; emotion reactivity (covariate), psychological distress (covariate); dosage/retention rates.	Active TEC had a positive impact on most self-injurious thoughts and behaviours. The active version of TEC, compared with the control version of TEC, significantly reduced self-cutting, overall nonsuicidal self-injury, and suicide plans. Among those who played active TEC at least once, there were fewer suicidal behaviours in the active group.
Franklin et al. 2016 – study 2	Online study, majority from USA	Individuals with a recent history of frequent nonsuicidal self-injury	131	26.0	22.91 (4.99)	18-NA years	1. Alternated version of active TEC. 2. Control version of TEC.	Both conditions: Participants were allowed to access the TEC app as much as they desired over a one-month period.	Pre, weekly during intervention period, and post.	Self-injurious thoughts and behaviours, dysregulated behaviours, implicit aversion to nonsuicidal self-injury, death/suicide and the self; emotion reactivity (covariate), psychological distress (covariate); dosage/retention rates.	The study partially replicated some of the findings from study 1. The active group showed significant reductions in self-cutting episodes that increased as active TEC dosage increased, and fewer suicidal behaviours.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Franklin et al. 2016 – study 3	Online study, majority form USA	Individuals with at least one suicidal behaviour within the past year	163	41.1	24.50 (6.61)	18-NA years	1. Active version of TEC with suicide-related targets. 2. Control version of TEC.	Both conditions: Participants were allowed to access the TEC app as much as they desired over a one-month period. In addition, they were allowed to continued access during the second month of the study to explore TEC use patterns and effects over a longer period of time.	Pre, weekly during intervention period, and post.	Self-injurious thoughts and behaviours, dysregulated behaviours, implicit aversion to nonsuicidal self-injury, death/suicide and the self; emotion reactivity (covariate), psychological distress (covariate); dosage/ retention rates.	The active group showed significant reductions for most self-injurious thoughts and behaviours except suicide ideation. Nonsuicidal self-injury effects were no longer significant after controlling for covariates.

Notes. All studies used a regular randomised controlled trial (RCT) design. NA = not available and/or could not be obtained. TEC = Therapeutic Evaluative Conditioning.

Table A.7 Characteristics of the studies focussing on psychosis

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Fisher et al. 2015	USA	Adolescents and young adults who were within 5 years of psychosis onset	121*[a]	74.4	21.22 (3.33)	16-30 years	1. Computerised cognitive training of auditory processing and verbal learning; training exercises from Posit Science Corporation (**applied game**) + TAU. 2. Commercial computer games (**casual games**) + TAU.	2. Participants rotated through a series of 16 commercially available computer games Both conditions: Participants were asked to train/play for 40 hours in total (one hour per day, 5 days per week, for 8 weeks). In addition, participants were contacted 1–2 times per week to discuss progress, and after every 10 sessions a 'check-in' appointment with coaching was provided. While in the trial, participants received treatment by outside providers or clinic personnel not involved in the study.	Pre and **post**.	Global cognition, speed of processing, working memory, verbal learning, **verbal memory**, visual learning, visual memory, problem solving, symptoms and functioning, global functioning; baseline reward anticipation (moderator); enjoyment, target engagement, adherence.	Compared with those playing commercial computer games, participants in the auditory training group showed significant improvements in global cognition, verbal memory, and problem solving. Both groups showed a significant decrease in symptoms and no change in functional outcome measures.
Holzer et al. 2014	Switzerland	Adolescents with psychosis or at high risk of psychosis	32	56.3	15.53 (1.33)	13-17 years	1. Computer-assisted cognitive remediation (CACR); selection of tasks from Captain's Log R software (**active condition**). 2. Computer games (**casual games**).	1. Sixteen 45-minute individual sessions, with a frequency of two sessions per week for 8 weeks, guided by a research psychologist. 2. A set of various videogames that require attention and visuo-motor skills was offered to patients, with two half-hour sessions weekly for 8 weeks.	Pre, **post** and 4-months FU (i.e., 6 months post-baseline)**.	Cognitive functioning: immediate memory, visuospatial/constructional, language, attention, **delayed memory**; symptoms, psychosocial functioning; compliance, motivation and engagement in treatment, time spent training/playing.	Improvement in visuospatial abilities was significantly greater in the CACR group than in the computer games group. Other cognitive functions, psychotic symptoms and psychosocial functioning improved significantly, but at similar rates, in the two groups.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Loewy et al. 2016	USA	Adoles-cents and young adults at clinical high risk for psy-chosis	83	50.6	18.15 (3.75)	12-30 years	1. Computer-ised cognitive training of auditory processing and verbal learn-ing; training exercises from Posit Science Corporation (**applied game**) + TAU. 2. Commercial computer games (**casual games**) + TAU.	2. Participants rotated through a series of 16 commercially available computer games Both conditions: Partici-pants were asked to train/play for 40 hours in total (one hour per day, 5 days per week, for 8 weeks). In addition, participants were contacted 1–2 times per week to discuss progress, and after every 10 sessions a 'check-in' in-person appointment with coaching was pro-vided. While in the trial, participants received treatment by outside providers or clinic per-sonnel not involved in the study.	Pre and **post**.	Global cognition, speed of processing, working memory, verbal learning, **verbal memory**, visual learning and memory, problem solving, symptoms, functioning; attrition, hours of training completed, training evaluation.	Participants in the audito-ry training group showed a significant improve-ment in verbal memory compared to participants in the computer games group. Positive and Total symp-toms improved in both groups over time.
Piskulic et al. 2015	Canada	Individu-als at clin-ical high risk of psychosis	43*[b]	65.6	18.75 (4.92)	14-35 years	1. Brain Fitness Program (**ap-plied game**). 2. Commercial computer games (**casual games**).	2. Participants in the con-trol group rotated through a series of 16 commercially available computer games, playing 4-5 games per training day. Both conditions: Partici-pants were expected to train/play for forty hours in total (one hour per day, 4 days per week, over a period of 10-12 weeks).	Pre, **post**, and 6-months FU (i.e., 9 months post-base-line).	Cognition**:** speed of processing, atten-tion/vigilance, work-ing memory, **verbal learning**, visual learning, reasoning and problem solving; global functioning; training activity/ progress, attrition.	No differences in cog-nition between the two groups at any timepoint. For the experimental group, however, there was a trend towards improvement in speed of processing between baseline and FU, and between post and FU. A significant improvement in social cognition was found between baseline and FU. In the commercial game group, there were sig-nificant improvements in working memory between post and FU.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Urben et al. 2012	Switzer-land	Adoles-cents with psychosis or at high risk for psychosis	32*[c]	63.6	15.55 (1.30)	13-17 years	1. Comput-er-assisted cognitive reme-diation (CACR); selection of tasks from Captain's Log R software. 2. Computer games.	1. Sixteen 45-minute individual sessions, with a frequency of two sessions per week for 8 weeks, guided by a research psychologist. 2. A set of 13 various videogames that require attention and visuo-motor skills was offered to patients, with two half-hour sessions weekly for 8 weeks.	Pre, post*** and 4-months FU (i.e., 6 months post-base-line).	Cognitive measures: processing speed, working memory, episodic (long-term) memory, executive functioning (inhibi-tion and initiation), reasoning and plan-ning abilities, symp-toms; time spent training/playing.	Significant improvements in inhibition and reason-ing, in the CACR group. No improvements in cognitive abilities in the control group. Symptom severity decreased significantly in the control group and marginally in the CACR group.

Notes: Intervention arms and variables measured in bold are included in the forest plot. All studies used a regular randomised controlled trial (RCT) design. FU = follow-up; NA = not available and/or could not be obtained; TAU = treatment as usual.
* Gender and mean age are reported for included/analysed participants (and not for the total randomised participants), with [a] n=86, [b] n=32, [c] n=22.
** Results reported in Urben, 2012.
*** Results reported in Holzer et al., 2014.

Table A.8 Characteristics of the studies focussing on externalising problems

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Coles et al. 2015	USA	Children with (partial) foetal alcohol syndrome or significant levels of alcohol-related physical features; majority of children had problems with disruptive behaviours	30	60.0	7.10 (1.42)	5-9 years	1. GoFAR program (**applied game** + active condition). 2. FACELAND (**active condition**). 3. No-intervention control group (**passive condition**)	1-2. Both programs consisted of three components: A) computer game for the child, B) therapy sessions for the parent, and C) behaviour analog therapy (BAT) sessions for parent and child. A and B were presented simultaneously in five 1-hour sessions over a 5-week period, followed by 5 weekly sessions of C. The programs only differed in the material presented to children by the game content: metacognitive FAR strategy (Focus and plan, Act, and Reflect) and learning to recognise facial expressions, respectively.	Pre, mid-treatment, and **post**.	**Disruptive behaviour**; interval between pre and post, child's age and intellectual status (covariates); time spent playing the game, enjoyment of gameplay, child's attention to the game; enthusiasm for gameplay, number of prompts required from observer, mastery of targeted concepts (game learning), adherence to treatment procedures (for BAT sessions).	Disruptive behaviour reduced in the GoFAR group after the first components, game play and parent training (i.e., at mid-treatment) and remained stable up to post-treatment. For the FACELAND group, disruptive behaviour reduced after completing the BAT sessions (i.e., at post). No changes were found in the no-intervention control group. Results suggest that the GoFAR game supported positive behaviour change after an initial 5 sessions, and that exposure to the FAR technique as implemented through the BAT sessions was effective in reducing disruptive behaviours in the FACELAND group and maintaining gains in the GoFAR group.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Ducharme et al. 2021	USA	Children and adolescents with clinically significant anger problems	40	72.5	12.75 (2.20)	10-17 years	1. Anger Control Therapy (ACT; empirically supported, manualised CBT), augmented with the RAGE-Control video game. 2. ACT augmented with a sham version of the RAGE-Control video game.	Both conditions: 10 weekly one-hour sessions of ACT with a parent check-in at the end of each session. Participants spent 15 minutes playing either the real of sham version of RAGE-Control after each session.	Pre and post.	Angry feelings, aggression, disruptive behaviours, severity of psychopathology, overall improvement, heart rate.	Similar reductions in self-reported anger were reported in both conditions. ACT with RAGE-Control led to larger improvements in aggression, disruptive behaviours and severity of psychopathology relative to sham.
Schuurmans et al. 2018	The Netherlands	Youth with clinically elevated levels of both anxiety and externalising problems	41*[a]	83.8	14.00 (1.88)	NA	1. Dojo (**applied game**) + TAU 2. TAU (**active condition**)	1. Eight 30-minute gameplay sessions, twice a week for four weeks. 2. Treatment as recommended by participants' clinicians.	Pre, **post**, and 4-months FU.	Anxiety symptoms, **externalising problems**; intellectual disability (moderator); user evaluation.	Youths who played Dojo, compared with youths in the control condition, showed reductions in self-reported anxiety and externalising problems at post-test, and mentor-reported anxiety at both post-test and FU.

Notes: Intervention arms and variables measured in bold are included in the forest plot. All studies used a regular randomised controlled trial (RCT) design. CBT = cognitive behavioural therapy; FU = follow-up; NA = not available and/or could not be obtained; TAU = treatment as usual.
* Gender and mean age are reported for included/analysed participants (and not for the total randomised participants), with [a] n= 37.

Table A.9 Characteristics of the studies focussing on other clinical populations

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Craig et al. 2016	USA	Children***	59[d]	59.0	9.65 (1.27)	7-11 years	1. Social skills intervention game 'Zoo U'. 2. Waitlist control group.	1. Participants completed the game over a 10-week period. 2. Participants did not have access to the game until the study trial period was over.	Pre and post.	Social skills (subscales: impulse control, communication, cooperation, social initiation, empathy, emotion regulation, assertiveness skills, internalising and externalising behaviour problems), self-efficacy, loneliness and dissatisfaction with peer relationships, social literacy.	The intervention group showed enhanced social skills in the areas of impulse control, emotion regulation, and social initiation, as well as more adaptive social behaviour compared to the control group. Participants in the intervention group also reported improvements in their social self-efficacy, social satisfaction, and social literacy compared to the control group. Parents of children in the intervention group reported an increase in internalising problem behaviours at post-test.
Hsieh et al. 2016**	Taiwan	Children diagnosed with developmental delays in the domains of gross or fine motor function, speech and language, cognition, social or emotional function, personal interaction, and/or sensory integration, and attending traditional rehabilitation treatment*	157[a]	31.3[b]	5.87 (2.09)	3-12 years	1. Several sports video games (selected from 24 games) + TAU. 2. Traditional rehabilitation treatment only.	1. Eight 30-minute sessions over a four-week period (i.e., two sessions per week), in addition to traditional rehabilitation treatment. 2. After four weeks of TAU, participants were offered the gaming intervention.	Pre, halfway the trial (i.e., after 4 weeks), and post.	Children's health-related quality of life (subscales: physical functioning and psychosocial health), functional performance (i.e., upper extremity and physical function, transfer and basic mobility, sports and physical function, global functioning), family functioning, parents' satisfaction with child's care, parents' quality of life.	Physical functioning improved in both groups during the intervention periods. No significant improvements were observed for psychosocial health, functional performance, family functioning, parents' satisfaction with child's care and parents' quality of life.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Mewton et al. 2020	Austra-lia	Youth with high risk for development of a mental illness based on elevated levels of personality risk factors	228	25.0[c]	18.55 (2.50)	16-24 years	1. Executive functioning training; game-like computerised tasks provided by Lumosity. 2. Control cognitive training focused on other cognitive abilities; game-like computerised tasks provided by Lumosity.	1-2. Twenty-five 40-minute sessions (one session per day, five days per week, over five weeks).	Pre, post, 3-, 6- and 12-months FU.	Overall psychopathology / problem behaviours, day-to-day functioning, executive functioning (working memory, task shifting, inhibitory control), risky drinking; training compliance.	Regardless of intervention group, psychopathology increased and day-to-day functioning decreased over the 12-months FU. Participants in the intervention group improved more than those in the control group on working memory (although the effect was not statistically significant after adjusting for multiple comparisons).
Nekar et al. 2023	Korea	Children with a developmental disability diagnosis or exhibiting some of its symptoms****	36	58.3	12.43 (1.23)	6-16 years	1. Cooperative exergames group. 2. Competitive exergames group. 3. Solitary exergames group.	All conditions: Two 30-minute session per week, for 8 weeks. Two exergames were used in each group.	Pre and post.	Cognition (orientation, memory, attention, visual perception, language, and high-level cognition), anxiety symptoms.	All groups showed improvements in memory, attention and visual perception. Only the cooperative exergames group showed significant improvements in language and all anxiety subscales. The competitive exergame group showed improvements in social phobia and the solitary exergames group showed improvements in physical injury and general anxiety.

Notes. Intervention arms and variables measured in bold are included in the forest plot. All studies used a regular randomised controlled trial (RCT) design, unless otherwise stated. FU = follow-up; NA = not available and/or could not be obtained; TAU = treatment as usual. [a] Gender and mean age are reported for 147 included/analysed participants (and not for the total randomised participants). [b] Number of males and females reported in the concerned paper is inconsistent. The current table includes the value from the body of the concerned paper. [c] Remaining participants were 74.6% female, and 0.4% (n=1) other gender. [d] Gender and mean age are reported for 47 included/analysed participants (and not for the total randomised participants).

* Participating children had the following diagnoses: 21.1% ADHD, 8.2% ASD, 14.3% Cerebral palsy, 13.6% Mental retardation, 35.4% unclassified.

** Crossover RCT.

*** Some of the participating children exhibited clinical levels of social problems (19%), externalising problems (20%) and/or internalising problems (27%).

**** Participating children had the following diagnoses: 58.3% ASD, 13.9% ADHD, 13.9% Obsessive-compulsive disorder, 13.9% Intellectual disorder.

Table A.10 Characteristics of the studies focussing on anxiety in medical settings

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Burns-Nader et al. 2017	USA	Children undergoing hydrotherapy for the treatment of burns	30	63.3	7.47 (2.54)	4-12 years	1. Child life specialist-supported distraction using a computer tablet with interactive games (**game distraction**). 2. Standard care, including psychosocial support by a child life specialist but no form of distraction (**standard care**).	1. The computer tablet was used during the hydrotherapy procedure.	Pre, **during**, and post hydrotherapy.	**Anxiety**, pain, length of procedure.	Participants in the tablet distraction group displayed less anxiety during the procedure compared to those in the standard care group and returned to baseline after the procedure while participants in the control group displayed higher anxiety post-procedure.
Chan et al. 2019 – trial 1 (emergency department)	Australia	Children in the emergency department requiring intravenous cannulation or venipuncture	123	54.5	8.06 (2.42)	4-11 years	1. Virtual reality distraction (**game distraction**). 2. Standard care, including age-appropriate distraction, such as child-life therapy, toys, books and electronic devices (**standard care**).	1. The participant interacted with the virtual environment during the procedure.	Pre and **post** needle procedure.	Pain, **anxiety**, distress, need for restraint, number of needle attempts and success, child's withdrawal of their arm, need for procedural sedation; procedural data, adverse events; age (moderator).	In comparison with standard care, virtual reality reduced pain, anxiety and distress.
Chan et al. 2019 – trial 2 (outpatient pathology)	Australia	Children in outpatient pathology requiring venipuncture	131*[a]	57.4	7.80 (2.33)	4-11 years	1. Virtual reality distraction (**game distraction**). 2. Standard care, including age-appropriate distraction, such as child-life therapy, toys, books and electronic devices (**standard care**).	1. The participant interacted with the virtual environment during the procedure.	Pre and **post** needle procedure.	Pain, **anxiety**, distress, need for restraint, number of needle attempts and success, child's withdrawal of their arm, need for procedural sedation; procedural data, adverse events; age (moderator).	In comparison with standard care, virtual reality reduced pain, anxiety and distress.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Dunn et al. 2019	USA	Children with haemophilia with clinically indicated intravenous procedure (laboratory draw or factor infusion)	25	83.0*[b]	13.00 (NA) **	6-18 years	1. Virtual reality distraction. 2. Standard care, including any technique routinely available that did not include VR (i.e., smart devices, bubbles, videos).	1. Games were played during the IV procedure.	Pre and post the IV procedure.	Anxiety (only at pre-test), perceived effectiveness for anxiety and pain (only at post-test); discomfort, infection, VR sickness, barriers to feasibility (technical issues, duration of the procedure), engagement, VR usability and likeability; age, being on prophylaxis (moderators).	Scores regarding the impact of VR on pain and anxiety were favourable. Participants in both groups reported a positive influence of distraction on procedural anxiety and pain.
Dwairej et al. 2020	Jordan	Children undergoing day-case surgery and receiving mask induction or general anaesthesia	136*j	45.3	6.55 (1.79)	5-11 years	1. Videogame distraction and anaesthesia mask exposure and shaping intervention (**game distraction**). 2. Usual care (**standard care**).	1. After 20-minutes of the mask exposure and shaping intervention, children played a videogame of their choice. 2. Participants received the routine unit's care.	Baseline, after the exposure/ shaping or usual care intervention, at the time of transfer to the operation room, and **during** anaesthesia induction.	**Anxiety**, anaesthesia induction compliance, emergence delirium.	Children in the intervention group reported lower anxiety at postintervention, the time of transfer to the operation room and during anaesthesia induction compared to the control group. Additionally, fewer children in the intervention group showed poorer anaesthesia induction compliance.
Elicherla et al. 2019	India	Children at their first dental visit, receiving prophylactic cleaning	50	60.0	NA	7-11 years	1. Little Lovely Dentist, mobile app. 2. Tell-Show-Do technique (TSD).	1-2. One session, prior to dental treatment.	Pre, during (heartrate only), and post the dental procedure.	Anxiety.	Heartrate decreased over time for participants in the dental app group, but not for those in the TSD group. Self-reported anxiety decreased for participants in both groups.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables mea-sured	Findings relevant for current review
Fernandes et al. 2015	Portugal	Children who underwent a minor outpatient surgery	90	76.7	10.20 (1.54)	8-12 years	1. 'An Adventure at the Hospital'; educational multimedia intervention including illustrations of hospital procedures and stages, and interactive game activities. 2. Entertainment video games; playing a video game of their choice (Super Mario, Angry Birds or FIFA/PES). 3. No intervention group.	1-2. One session of 15 minutes.	Pre and post the multimedia or entertainment gaming intervention (but still pre-surgery).	Pre-operative worries (subscales: global, hospitalisation, medical procedures, illness), valence, arousal, heartrate, blood pressure; child's temperament, parental anxiety, age, previous hospitalisation (moderators).	Compared to participants in the gaming and no intervention groups, participants receiving the educational multimedia intervention reported lower levels of worries about hospitalisation, medical procedures, illness, and negative consequences. Participants in all groups showed an increase in positive affect, decrease of arousal, and a decrease in blood pressure levels.
Gold & Mahrer, 2018	USA	Children and adolescent pediatric patients undergoing blood draw	146*[c]	49.7	15.43 (3.13)	10-21 years	1. Virtual reality + standard care. 2. Standard care, involving a brief interaction with the phlebotomist and each patient room has a television playing a cartoon movie at a low volume.	1. Participants received standard of care and interacted with the VR game a few minutes before, during and following the blood draw (approximately 5 minutes in total).	Pre and post (asking about pain and anxiety during the procedure).	Pain intensity, affective pain, **anxiety**; VR goggle-type, age, gender, number of previous blood draws, anxiety sensitivity (moderators); immersion in the game, simulator sickness, satisfaction.	Compared with standard care, VR reduced acute procedural pain and anxiety, and participants reported better affect.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables mea-sured	Findings relevant for current review
Inan & Inal, 2019	Turkey	Children referred to the phlebotomy unit for blood sampling	180	49.4	7.77 (1.28)	6-10 years	1. Playing a video game of their choice (**game distraction**). 2. Watching a cartoon movie of their choice. 3. Distraction through verbal interaction with their parents. 4. Regular procedure without any distraction (**standard care**).	1-3. Videogame playing / watching cartoon movie / parental distraction started three minutes before the beginning of the blood-drawing and lasted all through the procedure (4 to 8 minutes in total).	Pre and post (asking about anxiety **during** the procedure).	**Anxiety**, pain.	Results showed significant differences in anxiety and pain between the groups, with the lowest level of anxiety and pain reported in the videogame group. The scores observed in the cartoon and parent distraction groups were lower than in the regular procedure group.
Jivraj et al. 2020	Canada	Children and adolescents undergoing cast removal	90	66.7	10.25 (3.35)	4-18 years	1. Standard care and use of the VR game SnowThrow. 2. Standard of care.	1. Participants started playing once they were in the procedure room and continued playing up until completion of the procedure. 2. Participants received standard care and were allowed to play the game for 5 minutes after the procedure.	Pre, during and post.	Tendency to worry/trait anxiety, state anxiety, nausea, future VR use, areas for improvement; trait anxiety, age, VR experience, first visit (covariates).	Intraprocedural and post-procedural anxiety were lower in the VR group.
Jung et al. 2021	USA	Children scheduled for an elective surgery requiring general anaesthesia	71	48.6	8.00 (2.30)	5-12 years	1. VR game (**game distraction**). 2. Non-VR control group (**standard care**).	1. Participants played the game during induction in the operation room. 2. Participants received standard medical care without any audiovisual devices.	Baseline (in preoperative holding area), on entering the operating room, and **during** induction of general anaesthesia.	**Anxiety**, parental anxiety, paediatric induction compliance, parental and patient satisfaction.	The change in anxiety from baseline to time of induction was lower in the VR group compared to the control group. Participants in the VR group were less anxious at room entry and at induction compared to the control group. No differences were found on the other outcomes.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Kassam-Adams et al. 2016	USA	Children admitted to the hospital for treatment of an acute medical event that occurred within the past 2 weeks which the child perceived as potentially traumatic	72	54.2	9.80 (1.39)	8-12 years	1. Coping Coach intervention. 2. Waitlist group.	1. Three levels/ modules (approximately 20 minutes each). Participants were encouraged to complete the activities over a period of one month, and to replay the game as many times as they wished. 2. Participants in the waitlist group received the intervention after the 12-week FU.	Pre, post, 12- and 18- weeks FU.	PTSS, health-related quality of life, post-traumatic cognitions, coping strategies (subscale: positive cognitive restructuring, support seeking, avoidance coping); risk for persistent PTSS, age, gender, ethnicity, low family income, prior trauma exposure (moderators); compliance, help-seeking and services used after the index medical event.	Coping Coach participants showed decreases in PTSS severity from baseline to 6 weeks and baseline to 12 weeks, while the waitlist group was stable from baseline to 6 weeks and showed a slight increase from baseline to 12 weeks. A small intervention effect was found when waitlist participants initiated the intervention after 12 weeks.
Kjeldgaard Pedersen et al. 2023***	Denmark	Children recruited in an orthopaedic outpatient clinic	74*k	54.2	10.20 (NA)	6-14 years	1. VR game 2. VR video 3. 2D video 4. Control group; small talk with the investigators	All interventions lasted 4 minutes. Participants went through all 4 interventions in randomised sequence.	Pre and post each intervention session.	Pressure pain threshold, heart rate, anxiety, level of pain, demographic gaming behaviour and experiences.	The VR game and VR video groups showed increased pain threshold and decreased anxiety levels. 2D video had a small significant effect on pain threshold bud did not relief anxiety. The control group did not show a significant effect on outcome measures.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Ko et al. 2016	USA	Children and adolescents who undergoing a typical cast room procedure	146	57.5	8.84 (4.42)	1-18 years	1. No iPad group / standard of care (**standard care**). 2. iPad with video of their choice. 3. iPad with game of their choice (**game distraction**).	2-3. Participants started watching/ playing upon their transition to the cast room and were allowed to use the iPad throughout the procedure.	Baseline (in waiting room), pre-procedure (upon entering cast room), **during**, and post procedure.	**Heartrate**; age, type of procedure (moderators); cooperation of participants.	Compared to the no-iPad group, there was a trend towards decreased heartrate in the video group and a significant increase in the game group prior to the procedure. There were no significant decreases in heartrate within any of the group between baseline and during the procedure. When comparing the change in heartrate from baseline to pre-procedure, there was a significant decrease in the video group compared to the no-iPad group.
Kumari et al. 2021	India	Children requiring local anaesthesia for various dental procedures	200	50.5	8.61 (1.87)	6-12 years	1. Immersive VR; play videogame of their choice. 2. Non-immersive VR; watching a cartoon movie of their choice.	Participants were given a few minutes to get accustomed to the VR headset/ controller prior to the beginning of the treatment. Then, topical anaesthetic gel was applied (1 minute), followed by injectable local anaesthesia (1-1.5 minute). Immediately after the local anaesthesia injection, the VR intervention(s) were stopped.	Pre and post the local anaesthesia injection.	Anxiety, pain.	Post-operatively, anxiety and pain scores were lower in the immersive VR group as compared to the non-immersive VR group.

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Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables mea-sured	Findings relevant for current review
Marechal et al. 2017	France	Children undergoing general anaesthesia for various ambulatory minor surgical procedures	118*[d]	70.4	6.75 (1.85)	4-10 years	1. Midazolam group (**standard care**). 2. Computer tablet-based age-appropriate games of their choice (**game distraction**).	1. Participants received midazolam upon arrival to the ambulatory ward (i.e., 20-30 minutes before anaesthesia). 2. Participants received the tablet 20 minutes before anaesthesia and were encouraged to play until loss of consciousness. Children were allowed to resume playing as soon as they were fully awake again.	Baseline (at arrival surgical ward), pre (at separation from parents, just before transfer from waiting area to surgical room), **during** mask induction, and post (when transferred from post-anaesthesia care unit to ambulatory surgery ward).	**Anxiety**, parental anxiety, children's postoperative behaviour changes at home (measured 1 day, 1 week and 2 weeks after the surgery); satisfaction.	No significant differences in anxiety between the midazolam and game groups at the time of mask induction, nor at the time of parent separation. Over the four measurements, mean level of anxiety was significantly lower in the game group compared with the midazolam group, but the mean score did not change differentially between the two groups.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Matthys-sens et al. 2020	Belgium	Children scheduled for ambulatory surgery	98*[e]	68.1	7.32 (2.00)	5-11 years	1. CliniPup, including a parent e-learning module and digital scoring tools. 2. Digital computer game without educational information, and digital scoring tools. 3. Standard care, including oral information on the planned surgical intervention.	1-2. Children played the game in the week prior to surgery. Further information on duration and/or frequency NA.	Before and after playing the game (both measured at home, one week pre-operatively), at hospital admission (still pre-operatively), postoperatively (before discharge), one week post-operatively, and one month postoperatively (post-operative behaviour only).	Anxiety of the child, pain, parental anxiety, post hospitalization behaviour of the child; surgery type (covariate).	Participants playing the CliniPup game showed a reduction in anxiety from before to after playing the game. Their anxiety was also lower compared to participants in the standard care group. After playing the game, participants in the digital game group reported lower anxiety scores than the standard care group, although the anxiety score was higher than their own anxiety score measured before playing the game.
Nilsson et al. 2013	Sweden	Children undergoing wound dressings	61*[f]	70.0	7.47 (1.92)	5-12 years	1. Use of lollipops. 2. Playing Tux Racer (**game distraction**). 3. Care as usual without any specific distraction techniques added to the care (**standard care**).	1-2. Participants started to lick the lollipops/ began to play the game approximately 3 to 5 min. before the wound care and continued to do so throughout the session (mean duration of session was 25 min.). 3. Mean duration of procedure was 20 minutes.	Pre (at arrival on the ward), **during** the wound dressing procedure (collected retro-spectively), and post.	Pain intensity, **distress**, anxiety.	Playing the game reduced the observed pain intensity and self-reported distress during the wound dressing procedure compared with the other groups.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables mea-sured	Findings relevant for current review
Osmanlliu et al. 2021	Canada	Children requiring an intra-venous procedure (IV inser-tion or ve-ni-punc-ture)	63*[g]	38.7	11.70 (2.99)	7-17 years	1. Standard care + virtual reality game Dreamland (**game distrac-tion**). 2. Standard care, including parental presence, topical anaesthetic and distraction by parent or health-care provider (**standard care**).	1. Three minutes familiarisation period prior to the IV procedure + during the proce-dure itself.	Pre, **during**, post proce-dure, and 24h after procedure (pain memory only)	Pain intensity, **anxiety**, proce-dural distress, memory of pain; recruit-ment feasibility, acceptability/sat-isfaction survey; adverse events/ side effects, completion of VR game.	No significant difference in pain score during the procedure, but recall of pain 24h after the procedure was lower in the virtual reality group. Anxiety and distress during the procedure were lower in the virtual reality group.
Pande et al. 2020	India	Children with negative behaviour, requiring resto-ration of carious teeth	60	50.0	NA	5-8 years	1. Tell-Show-Do technique (TSD; **standard care**). 2. Audio distrac-tion. 3. Audiovisual dis-traction (cartoon) in VR. 4. Mobile phone game distraction (**game distrac-tion**).	1. One session, prior to dental treatment. 2. Listening to their favourite story or music during dental treatment. 3. Watching their favourite cartoon using VR glasses during dental treatment. 4. Playing their favourite mobile game during dental treatment.	Pre and **post** proce-dure.	**Anxiety**, systolic and diastolic blood pressure, pulse rate.	Significant decreases in both physiological and non-physiological measures were observed in all groups. Compared to TSD, all groups showed a stronger reduc-tion in systolic and diastolic blood pressure, pulse rate and anxiety. The strongest decreases were found in the audiovi-sual distraction (VR) group, followed by mobile phone game distraction and audio distraction.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables mea-sured	Findings relevant for current review
Patel et al. 2006	USA	Children under-going general anaesthe-sia for am-bulatory surgery	112	61.6	6.84 (0.43)	4-12 years	1. Parent presence + video game of their choice (**game distraction**). 2. Parent presence + midazolam (**standard care**). 3. Parent presence only.	1-2. Participants started playing the video game or were medicated in the holding area. At least 20 minutes after the (start of the) intervention, they were escorted into the operating room. Participants assigned to the video game group were allowed to play through the introduction of the anaesthesia mask.	Pre and **during** anaesthe-sia, and 7-10 days after surgery (postop-erative behaviour only).	**Anxiety**, parent satisfaction, postoperative behaviour changes; age and gender (moder-ators).	The midazolam and parent presence only groups showed an increase in anxiety from baseline to induction of anaesthesia, while participants in the video game group showed a decrease in anxiety.
Sahin & Karkiner, 2022	Turkey	Children scheduled for their first elec-tive sur-gery for phimosis, inguinal hernia, hydrocele, unde-scended testis, hypospa-dias, and under-going general anaesthe-sia	300*ǀ	87.4	6.57 (1.95)	4-10 years	1. Midazolam group (**standard care**). 2. Computer tablet-based age-appropriate games of their choice (**game distraction**). 3. Control group.	1. Approximately 30 minutes before mask induction, participants re-ceived Midazolam. 2. Participants played games for 20 minutes and continued to play while leaving their families to go to the operating room. They were encouraged to play with the tablet until they lost conscious-ness during mask induction. 3. Participants were not given any anxiolytic.	Pre and post inter-vention, during separation from fami-lies, **during mask induc-tion**, one week after discharge (postop-erative behaviour only).	**Anxiety**, sepa-ration anxiety, recovery time from anaesthe-sia, postopera-tive behaviour changes; anxiety (moderator/co-variate).	Significant differences in anxiety scores after the in-tervention and percentages of decrease from baseline were found. The highest anxiety scores and lowest percent decreas-es were found in the control group, and Midazolam was more effective than playing games. Significantly more children separated easily from their parents in the Midazolam group, as well as accepted the masks more readily.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Sakizci Uyar et al. 2021	Turkey	Children undergoing ambulatory surgery	138*[h]	44.0	6.60 (1.03)	5-8 years	1. Premedication with midazolam (**standard care**). 2. Watching an informative cartoon about anaesthesia. 3. Playing video games of their choice (**game distraction**).	Participants received the intervention in the pre-operative holding area. Twenty minutes after the intervention, they were transported to the operation room. Participants in the video game group were allowed to play until loss of consciousness.	Before and 20 minutes after the intervention (both pre-operatively), while entering the operating room, and **during mask induction**.	**Anxiety**, cooperation during mask induction.	From before to after the intervention, the video game group showed a reduction in anxiety compared to with the midazolam and cartoon group. After the intervention and while entering the operating room, the game group showed less anxiety than the cartoon group. At mask induction there were no differences between the groups.
Schlechter et al. 2021	USA	Children and adolescents who required IV placement as part of their emergency department care	116*[i]	47.8	11.00 (4.19)	4-17 years	1. Virtual reality game distraction. 2. Standard of care, including the provision of standard distraction techniques such as reading a book or iPad use.	1. Participants played the VR game while venipuncture was performed, for as long as it takes to place the IV. 2. Venipuncture was performed per standard of care.	Pre and post IV placement.	Success on first attempt of IV placement, anxiety, number of IV attempts, median time to successful IV placement, change in pain level, removal of the VR headset, adverse events.	Changes in anxiety scores were similar between the groups.
Stewart et al. 2019	USA	Children undergoing outpatient surgery	102	57.8	6.85 (2.44)	4-12 years	1. Tablet-based interactive distraction (**game distraction**). 2. Oral midazolam control group (**standard care**).	1. Participants started playing one minute before parental separation and continued playing through mask induction. 2. Midazolam was given 15 to 45 minutes before separation.	Baseline, at parental separation, at **mask induction**, and 7 and 14 days postoperatively (behavioural changes only).	**Anxiety**, emergence delirium, caregiver satisfaction at the time of separation, length of stay in post anaesthesia care unit (PACU), postoperative behavioural changes.	Participants in the intervention group were less anxious at parental separation and mask induction than children in the control group, and demonstrated lower emergence delirium and were discharged earlier.

Notes. Intervention arms and variables measured in bold are included in the forest plot. All studies used a regular randomised controlled trial (RCT) design. FU = follow-up; IV = intravenous; NA = not available and/or could not be obtained; PTSS = post-traumatic stress symptoms; VR = virtual reality.

* Gender and mean age are reported for included/analysed participants (and not for the total randomised participants), with [a] n= 129, [b] n=24 (gender only), [c] n=143, [d] n=115, [e] n=72, [f] n=60, [g] n=62, [h] n=134, [i] n=115, [j] n = 128, [k] n = 72, [l] n = 286.
** Median age.
*** Crossover RCT.

Table A.11 Characteristics of the studies focussing on positive affect and negative affect and/or emotions

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables mea-sured	Findings relevant for current review
Alloway & Carpenter, 2021**	USA	University students	59	22.0	23.33 (3.70)	18-35 years	1. Playing Pokémon Go outside. 2. Walking outside without phone.	1-2. One hour.	Pre and post.	Positive and negative affect**,** verbal and visuospatial working memory, sustained attention, empathy.	After playing Pokémon Go participants showed a decrease in negative affect, but not in the control condition.
Andrade et al. 2019***	Brazil	Healthy children	187*[a]	42.1	9.41 (0.48)	7-11 years	1. Playing exergames during PE classes, Just Dance 2015 (**casual game**). 2. Regular school PE classes (**active condition**).	1-2. Three 40-minutes sessions within 1 week.	Pre and **post**.	Mood (psychological states: depression, **anger**, mental confusion, and psychosomatic states: fatigue, tension, **vigour**).	Playing exergames increased vigour and fatigue.
Andrade et al. 2020***	Brazil	Healthy children	187*[a]	42.1	9.41 (0.48)	7-11 years	1. Playing exergames during PE classes, Just Dance 2015. 2. Regular school PE classes.	1-2. Three 40-minutes sessions within 1 week.	Pre and post.	Mood (psychological states: depression, anger, mental confusion, and psychosomatic states: fatigue, tension, vigour), self-esteem, gender (moderator).	Boys playing exergames showed higher tension compared to the PE group. Girls playing exergames showed higher levels of vigour than those from the PE group. In the exergame group, boys had higher levels of anger than girls. In the regular PE group, boys had lower scores for mental confusion than girls.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables mea-sured	Findings relevant for current review
Branton et al. 2014**	Canada	Normal weight boys	19	100	12.00 (0.50)	9-14 years	1. Video game playing, Angry Birds. 2. Nonscreen resting control condition; engaged in conversation with other participants and research volunteers.	1-2. Thirty minutes.	Baseline, 15 and 30 minutes before and immediately after lunch. Note: procedure included a standardized breakfast, glucose or control preload, video game playing or nonscreen resting control, and an ad libitum pizza lunch.	Subjective emotions (aggression, anger, excitement, disappointment, happiness, upset, frustration), subjective appetite sensations, food intake; sweetness and pleasantness of the preloads, pleasantness of the pizza lunch, enjoyment of videogame playing.	Frustration and aggression scores increased after video game playing.
Douris et al. 2012**	USA	Healthy sedentary young adults	21	42.9	23.20 (1.80)	18-25 years	1. Nintendo Wii Fit "Free Run" program. 2. Brisk walking exercise on treadmill.	1. One 30-min. session. Participants exercised at a self-selected pace. 2. One 30-min. session, walking on a treadmill set at 3.5 miles per hour. Participants followed both exercise conditions in counterbalanced order. Sessions were separated by 1 week.	Pre, during and post each session.	Heart rate, rate pressure product, respiratory rate, perceived exertion, positive well-being, psychological distress, fatigue.	Mean maximum heart rate achieved when exercising with Wii Fit and perceived exertion was significantly greater than for the brisk walking group. Positive well-being decreased from pre- to postexercise for Wii Fit participants while it increased for participants exercising on the treadmill.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Ferguson & Rueda, 2010	USA	Young adults	103	60.2	23.60 (5.82)	18-54 years	1. Hitman: Blood Money (anti-social violent; **casual game**). 2. Call of Duty 2 (prosocial violent). 3. Madden 2007 (nonviolent; **casual game**). 4. No-game control (**passive condition**).	1-3. After a frustration task, participants played one session of 45 minutes. 4. After the frustration task, participants were given a cover story and then allowed a 45-minutes 'cooldown' period in which they were not given any directed activity.	Pre and **post**.	**Hostile feelings**, depressive symptoms, aggressive behaviour (measured only at post-test); game evaluation.	Video game play had no effect on aggressive behaviour. Real-life violent video game-playing history was predictive of decreased hostile feelings and decreased depression following the frustration task.
Ferguson et al. 2016	USA	Youth	43	67.4	15.37 (2.38)	12-18 years	1. Tomb Raider 2013 (violent; **casual game**). 2. FIFA (nonviolent; **casual game**).	1-2. One session of 45 minutes.	Pre and **post**.	**State hostility**, current stress/anxiety; gender and trait aggression (moderators).	Hostility levels neither decreased nor increased following violent game play. Only for females, stress levels increased when playing the violent game.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Gheller et al. 2019**	Canada	Boys with overweight or obesity	22	100	11.90 (1.60)	9-14 years	1. Video game playing, Angry Birds. 2. Nonscreen resting control condition; engaged in conversation with other participants and research volunteers.	1-2. Thirty minutes.	Baseline, 15 and 30 min. before and immediately after lunch. Note: procedure included a standardized breakfast, glucose or control preload, video game playing or nonscreen resting control, and an ad libitum pizza lunch.	Subjective emotions (aggression, anger, excitement, disappointment, happiness, upset, frustration), subjective appetite, thirst, food intake, water intake, pleasantness of the test meal, sweetness and pleasantness of the preload, enjoyment of video game playing.	Frustration was the only emotion that increased following video game play.
Goodie & Larkin, 2001	USA	Healthy males	46*[b]	100	22.44 (4.57)	NA	1. Heartrate feedback training group, while playing the Sno-Cat videogame (**applied game).** 2. Control group, not receiving heartrate feedback while playing the Sno-Cat videogame (**casual game**).	1-2. Five 6-minutes training blocks with a 1-minute rest period between each block.	Pre and **post**.	**Heartrate**, blood pressure, hemodynamic parameters (stroke volume, cardiac output, total peripheral resistance), both **in response to a mental arithmetic task** and to the video game; game playing performance.	At post training, the feedback training group showed lower heart rate, systolic blood pressure, stroke volume and total peripheral resistance responses to the videogame compared to the control group. There was no evidence that the acquired skills generalised to the mental arithmetic task.

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Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Hunter et al. 2019	USA	Under-graduate psychology students	140	22.9	20.28 (2.68)	18-26 years	1. Heartrate variability biofeedback game Breather (**applied game**). 2. Condition in which phone was present but not used (**active condition**). 3. Condition without phone (**passive condition**).	1-3. Participants underwent a laboratory stressor and were randomly assigned to recover in one of three ways during a five-minute session.	Pre, post and **20 minutes after the laboratory stressor**.	**Current stress,** physiological stress (salivary alpha amylase and salivary cortisol).	Participants in the heartrate variability biofeedback game conditions had lower levels of salivary alpha amylase during recovery than participants in the other conditions. There were no differences between conditions for salivary cortisol levels or self-reported stress.
Matheson et al. 2021	United King-dom	Adolescents	6576	51.6	13.47[*c] (0.50)	13-14 years	1. Body image playable, 'Self-Esteem Squad', combined with a 'call to action' pledge (**applied game**). 2. Environmental conservation playable, combined with a 'call to action' pledge (**active condition**). 3. Body image social media posts, combined with a 'call to action' pledge (**active condition**).	1-3. One-hundred fifty seconds.	Pre and **post intervention**, and after experimental media exposure task (which followed after the intervention).	State-based body satisfaction, **negative affect**, prosocial behavioural intent; gender, body esteem, internalisation of appearance ideals, and media literacy (moderators); intervention acceptability, manipulation check.	Relative to the environmental conservation playable, both body image micro-interventions improved state body satisfaction and decreased negative affect. All three conditions elicited online prosocial behaviours in a majority of participants, although the social media posts condition was most effective. No condition buffered against the negative effects associated with viewing idealised media images.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables mea-sured	Findings relevant for current review
Poppelaars, Lichtwarck-Aschoff et al. 2018****	The Netherlands	Undergraduate students	146*[d]	28.7	20.21 (1.74)	17-26 years	1. Playing a co-operative commercial video game (i.e., WAY) in pairs following an entertainment-focused introduction message (**casual game**). 2. Playing WAY in pairs following a mental health-focused introduction message (**active condition**).	1-2. Participants finished the game in approximately 40 minutes.	Pre and **post**.	Mood, arousal, **positive and negative affect**; intrinsic motivation, psychological need satisfaction, depressive symptoms (moderator).	Participants experienced more positive mood and positive affect after gameplay, regardless of condition. No changes in negative affect and arousal were found.
Russell & Newton, 2008****	USA	College students	168	46.4	21.51 (5.31)	18-58	1. Regular bicycle ergometer exercise at moderate intensity (**active condition**). 2. Interactive video game bicycle ergometer exercise at moderate intensity (**casual game**). 3. Video game-only control condition, without exercise (**casual game**).	1-3. Thirty minutes	Pre, **post** and 10-minute FU.	**Positive and negative affect**, concentration, social desirability/ defensiveness, actual and perceived exertion, heartrate.	Participants in either exercise condition had higher positive mood at 10-minutes post activity compared to the video game-only control participants. In addition, participants in the video game-only group had higher post-activity negative affect than either exercise group.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Valadez & Ferguson, 2012	USA	University students	100	51.0	19.90 (3.92)	18-45 years	1. Red Dead Redemption (violent condition; **casual game**). 2. Red Dead Redemption (non-violent condition). 3. FIFA (nonviolent; **casual game**).	1-3. After a frustration task, participants played the game for either 15 or **45 minutes**, resulting in 6 experimental conditions in total.	Pre and **post** game-play.	**Hostile feelings**, depressive symptoms, visuospatial cognition.	Results showed that neither randomised video game play nor time spent playing a video game had any effect on depression, hostility or visuospatial cognition.

Notes. Intervention arms and variables measured in bold are included in the forest plot. All studies used a regular randomised controlled trial (RCT) design, unless otherwise stated.
FU = follow-up; NA = not available and/or could not be obtained; PE = physical exercise.
* Gender and mean age are reported for included/analysed participants (and not for the total randomised participants), with [a] n=140, [b] n=25, [c] n=4531 (age only), [d] n=129.
** Crossover RCT.
*** Cluster RCT.
**** Regular RCT design, but participants played in pairs.

Table A.12 Characteristics of the studies focussing on well-being, internalising symptoms, and/or general psychological difficulties

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Abbott et al. 2014**	Australia	Children	74*[a]	50.0	11.30 (0.80)	10-12 years	1. No games, removal of all electronic game devices from the home. 2. Traditional nonviolent games of their choice. 3. Active-input nonviolent games of their choice.	1-3. Participants were randomised to a counterbalanced order of the three conditions, each lasting 8 weeks.	Pre and after each condition.	Self-esteem (global self-worth, social acceptance, scholastic, athletic, physical and behavioural competence), electronic game use anxiety; enjoyment of physical activity.	Compared with home access to traditional electronic games, neither removal of all electronic games nor replacement with active-input games resulted in any significant change in self-esteem, enjoyment of physical activity or anxiety related to electronic games.
Axford et al. 2020***	UK	Children	3480*[h]	44.7	8.85 (1.15)	7-11 years	1. KiVa program (**applied game**). 2. Waitlist control group (**passive condition**).	1. Ten 90-minute lessons to be delivered monthly over a full academic year. 2. Control schools were asked to continue with their usual practices in line with their bullying policy. The KiVa program was implemented in the next academic year.	Pre and **post** (after 12 months).	Victimization, bullying perpetration, social and emotional well-being (subscales: **emotional symptoms**, conduct problems, peer relationship problems, prosocial behaviour, **total difficulties score** and impact score), school absenteeism, fidelity measures, program implementation; school size, free school meals eligibility, gender, age, special education needs, free school meals status (control variables).	No significant differences were found on any of the variables.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assessments	Variables measured	Findings relevant for current review
Cioffi & Lubetzky, 2023	USA	Healthy adolescents	42	31.0	15.73 (1.16)	14-18 years	1. Oculus Rift BOXVR game (**casual game**). 2. Boxing with a guided workout video (**active condition**). 3. Non-intervention control group (**passive condition**).	1 and 2. Ten-minute exercise sessions, five times a week for three weeks. 3. The non-intervention control group did not participate in any exercise in relation to the study.	Pre, weekly during intervention period (including **post** measurement), and 1-week FU.	Executive functioning, **anxiety**, stress, enjoyment and engagement, heartrate.	The BOXVR group showed a reduction in stress and improvements in one part of the executive functioning measure at the weekly assessments and FU. All groups showed improvements in another part of the executive functioning measure. At the end of the study, the BOXVR group reported lower stress levels than the guided video group, and better executive functioning than the control group. While the control group showed a reduction in anxiety, the groups were not different in anxiety at the end of the study. The BOXVR group reported significantly greater enjoyment after each exercise session than the guided video group.
David et al. 2019a	Romania	Healthy children and adolescents	165*[b]	35.9	12.90 (2.06)	10-16 years	1. REThink. 2. Rational Emotive Behaviour Education (REBE) condition. 3. Waitlist control group.	1. Seven 50-minute online modules, completed during one month. 2. Seven group meetings in class format, completed during one month. 3. Waitlist participants receive the REThink intervention after the 6-month FU.	Pre, mid-treatment (after 2 weeks), and post.	Emotional symptoms, depressive mood; irrational cognitions, intolerance of rules and for work, and (five subscales of) negative and positive automatic thoughts (mediators); gender and age (moderators).	Irrational beliefs mediate the effect for the REThink intervention. A decrease in irrational beliefs contributed to a decrease in emotional symptoms and depressive mood. The efficacy of the intervention did not vary by age or gender, but younger and older adolescents responded differently on the measures assessing emotional symptoms and depressive mood.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
David et al. 2019b	Roma-nia	Healthy children and ado-lescents	165*[b]	35.9	12.90 (2.06)	10-16 years	1. RETHink (**applied game**). 2. Rational Emotive Behaviour Education (REBE) condi-tion (**active condition**). 3. Waitlist control group (**passive condition**).	1. Seven 50-minute online modules, completed during one month. 2. Seven group meetings in class format, completed during one month. 3. Waitlist partici-pants received the RETHink inter-vention after the 6-month FU.	Pre and **post**.	Emotional symptoms, conduct problems, hyperactivity/inatten-tion, peer problems, **total level of psycho-logical difficulties**, prosocial behaviour, emotional control, emotional self-aware-ness**, depressive mood**, attention, fear, inhibitory control, intensity of functional and dysfunctional negative emotions and **positive emo-tions**; satisfaction.	The RETHink intervention had a significant impact on emo-tional symptoms. The REBE and waitlist groups did not show significant changes and no differences between groups were found at any timepoint. The RETHink intervention also reduced depressive mood, compared to the waitlist group. Furthermore, the RE-Think intervention increased emotional self-awareness and emotional control. Both the RETHink and REBE groups showed increased attention.
David & Fodor, 2022	Roma-nia	Healthy children and ado-lescents	165*[i]	41.4	12.84 (1.97)	10-16 years	1. RETHink. 2. Rational Emotive Be-haviour Edu-cation (REBE) condition. 3. Waitlist control group.	1. Seven 50-minute online modules, completed during one month. 2. Seven group meetings in class format, completed during one month. 3. Waitlist partici-pants receive the RETHink inter-vention after the 6-month FU.	Pre, mid-treat-ment, post and 6-months FU.	Emotional symptoms, total level of psycho-logical difficulties, conduct problems, hyperactivity, peer re-lationship problems, prosocial behaviour, emotion regulation (emotional control and self-awareness), depressive mood, temperamental focused attention, fear, inhibitory control, , functional and dysfunctional emotions and positive emotions; parental psychological control, (insecure) parental attachment and childhood trauma (moderators).	The RETHink intervention had a durable impact on children's mental health and emotion regulation skills. Childhood trauma, parental psychological control and parent attachment moder-ated the maintenance of the improvements.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
David & Fodor, 2023	Romania	Children with a self-reported maltreatment history	294	47.6	10.03 (1.28)	8-12 years	1. RETHink (**applied game**). 2. Care as usual (**passive condition**).	1. Participants played one level each week, for a total of seven weeks. 2. Participants only took part in the assessment phases during the trial.	Pre and **post**.	**Emotional symptoms**, **total level of psychological difficulties**, conduct problems, hyperactivity, peer relationship problems, prosocial behaviour, emotion regulation abilities (emotional control and self-awareness), emotion regulation strategies, irrational cognitions; severity of maltreatment and security of parental attachment (moderators).	Participants in the RETHink group showed significantly lower levels of emotional problems, mental health difficulties, use of maladaptive emotion-regulation strategies, and irrational cognitions at post-test compared to the care as usual group. Severity of maltreatment and parent attachment moderated the effects.
David & Magurean, 2022	Romania	Healthy children and adolescents ******	54[*j]	25.0	13.00 (2.06)	10-16 years	1. RETHink.	1. Seven 50-minute online modules, completed during one month. The game included an attentional bias training level based on the visual search paradigm.	Pre, mid-treatment, and post.	Attentional bias, emotional symptoms, conduct problems, hyperactivity/ inattention, peer relationship problems, prosocial behaviour, functional negative emotions, dysfunctional negative emotions, positive emotions.	Increases in attentional bias towards positive faces from the intermediary assessment to the post-test assessment were associated with improvements in participants' conduct problems, hyperactivity, and peer relationship problems.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
David, Ma-gurean et al. 2022	Roma-nia	Healthy children and ado-lescents ******	54*j	25.0	13.00 (2.05)	10-16 years	1. RETHink.	1. Seven 50-minute online modules, completed during one month.	Pre, mid-treat-ment, and post.	Game performance (per level and delta changes between two consecutive play sessions), emotional symptoms, conduct problems, hyperac-tivity/inattention, peer relationship problems, prosocial behaviour, depressive mood, attention, fear, inhibitory control, emotion regulation (emotional control, emotional self-aware-ness, situational responsiveness), irra-tional cognitions (low frustration tolerance for work, low frus-tration tolerance for rules, total irrational-ity score), functional and dysfunctional negative emotions, positive emotions, problem solving.	In-game performance at some levels consistently reflect im-provements in psychological functioning (i.e., total mental health, tolerance for rules, positive emotions, emotional control), while in-game perfor-mance at other levels are less associated with outcomes.
David et al. 2021	Roma-nia	Healthy children and ado-lescents	165*k	41.8	12.94 (2.05)	10-17 years	1. RETHink. 2. Rational Emotive Be-haviour Edu-cation (REBE) condition. 3. Waitlist control group.	1. Seven 50-minute online modules, completed during one month. 2. Seven group meetings in class format, completed during one month. 3. Waitlist partici-pants receive the RETHink inter-vention after the 6-month FU.	Pre and post-in-tervention partic-ipants under-went an impromp-tu speech task.	Subjective state anxiety and brain activity (frontal alpha asymmetry) was mea-sured before, at an-ticipation, during and after the impromptu speech task.	In comparison to the waitlist and REBE group, the RETHink game was effective in reducing biological reactivity, but not for modulating subjective anxiety. State anxiety decreased from pre to postintervention in all groups, specifically for the speech and recovery phases.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
David, Stroian et al. 2022	Romania	Healthy children and adolescents	165*I	41.4	12.84 (1.97)	10-16 years	1. REThink. 2. Rational Emotive Behaviour Education (REBE) condition. 3. Waitlist control group.	1. Seven 50-minute online modules, completed during one month. 2. Seven group meetings in class format, completed during one month. 3. Waitlist participants receive the REThink intervention after the 6-month FU.	Pre post-intervention and at 6-months FU participants underwent an impromptu speech task.	Subjective state anxiety and biological stress reactivity was measured before, at anticipation, during and after the impromptu speech task.	Group did not differ at follow-up in terms of subjective state anxiety variations before, at anticipation, during and after the impromptu speech task. Furthermore, an increase in left asymmetry for the REThink group was found at post-test and FU compared to pre-intervention levels. The same trend emerged for the waitlist group, but not for the REBE group.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Egan et al. 2021	USA (online study)	Sexual and/or gender minority youth	240	36.7[p]	15.77 (1.11)	14-18 years	1. 'Singularities', web-accessible computer role-playing game (**applied game**), including a list of resources. 2. Control group receiving only a list of resources (**active condition**).	1. Participants received instructions for downloading and installing the game, and could (re)play the game at their own convenience for as long as they wished.	Pre, **after 4** and 8 weeks.	Several feasibility assessments and benchmarks for success, help-seeking intentions, help-seeking self-efficacy, help-seeking behaviours, coping skill use (problem solving and passive avoidant coping), coping flexibility (evaluation and adaptive coping), knowledge and use of web-based resources, bullying victimisation, loneliness, anxiety, **depressive symptoms**, suicidality (ideation, plan, attempt), substance use, internalised gender minority and sexual stigma; game play data, several measures focussing on implementation procedure, game demand, game acceptability; game play intensity; control variables).	The intervention was feasible and acceptable. Intervention participants showed larger reductions in binge drinking frequency, marijuana use frequency and cyberbullying victimisation, and increased knowledge and use of online resources, compared to the control group.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Hammond et al. 2014**	United King-dom	Children with move-ment difficulties and/or a Devel-op-mental Coordi-nation Disorder (DCD)	20*[c]	77.8	8.97 (1.34)	7-10 years	1. Wii Fit intervention, choice of nine games (**casual games**). 2. Treatment as usual, school-run Jump Ahead intervention (**active con-dition**).	1. Ten minutes, three times a week, for four weeks. 2. One hour per week, for four weeks. Two and a half months after the end of phase 1, the groups participat-ed in the alterna-tive intervention for the following four weeks.	Pre, **post** and 18-weeks FU.	Motor proficiency, self-perceived ability and satisfaction with motor tasks, **social behaviour and emo-tional/behavioural problems**.	Significant gains were seen in motor proficiency, self-per-ceived ability and satisfaction with motor tasks, and emo-tional well-being for many, but not all children.
Hsieh & Chen, 2019***	Taiwan	Primary students	144	48.8*[d]	12.58 (1.03) [r]	11-13 years	1. Pokémon Go. 2. Control group not using Pokémon Go.	1. Ten weeks, on average playing 40 minutes per day.	Pre and post.	Emotional intelligence (factors: well-being, self-control, emo-tionality, sociability), memory, selective attention, concentra-tion, creative imag-ination; adherence, amount of physical activity, satisfaction; age, sex, family edu-cation level, number of computers in the home, daily study time, and access to internet (confound-ers).	Compared to the control group, the Pokémon Go group showed increases in selective attention, concentration, creative imagination, emo-tionality and sociability, but not in memory, well-being or self-control.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Kato et al. 2008	USA, Canada, and Australia	Adolescents and young adults with an initial or relapse diagnosis of a malignancy, currently undergoing treatment and expected to continue treatment for at least four months	375*[e]	67.7	13-14 year: 35.5%. 15-16 year: 30.7%. 17-18 year: 21.3%. 19-29 year: 12.7%.	13-29 years	1. Re-Mission (**applied game**) + control commercial game. 2. Commercial game, Indiana Jones and the Emperor's Tomb (**casual game**).	1-2. Participants were asked to play the game(s) for at least 1 hour per week, during the 3-months study period.	Pre, halfway (after 1 months), and **post** (i.e., three months after baseline).	Adherence to treatment, self-efficacy to manage cancer and its treatment, knowledge about cancer, **quality of life (minors) / functional status (adults)**, locus of control, **perceived stress**; gender, ethnicity, country of residence, and game-play adherence (moderators); adverse events, attrition.	Quality of life and perceived stress were not significantly altered by the intervention.
Kuosmanen et al. 2017***	Ireland	Adolescents attending an alternative education programme[q]	146	46.6	17.60 (1.24)	15-20 years	1. SPARX-R, CBT-based self-help intervention (**applied game**). 2. No-intervention control (**passive condition**).	1. Seven levels/ modules, each taking 20-30 minutes to complete. Every week, one module was completed.	Pre and **post**.	**Depressive symptoms**, generalised anxiety symptoms, **gen. mental well-being**, coping styles (avoidant, support seeking, problem solving), emotion regulation (cognitive reappraisal, expressive suppression); program acceptability, perceived helpfulness, frequency of practicing techniques.	Compared to the control group, the SPARX-R group showed a decrease in expressive suppression. No significant effects on other outcomes were found.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Mack et al. 2020****	Germa-ny	Children	82	52.4	9.65 (3.42)	9-12 years	1. Kids Obesity Prevention program, including a serious game. 2. Brochure, control group.	1. Two 45-minutes sessions over a two-week period. 2. The brochure was handed out at the beginning of the study phase. The control group received the intervention after the intervention group completed the program.	Pre, post, and 4-weeks FU.	Knowledge about nutrition and stress coping (total score, food pyramid score, dietary energy density score, stress score), dietary intake, physical activity, media consumption; knowledge of dietary energy density (covariate); game acceptance, emotions during gameplay, in-game data assessing the application of the dietary energy density principle.	Knowledge about nutrition and stress coping increased from pre to post, and knowledge gains were maintained at FU.
Maden et al. 2022	Turkey	University students with gaming disorder and a sedentary lifestyle	44	100	22.71 (1.92)	18-28 years	1. Virtual reality-based (exer-game) training in random pairs (**applied game**). 2. Aerobic (treadmill) training (**active condition**). 3. Control/ no-intervention group (**passive condition**).	1-2. Three 30-minutes training sessions over a period of six weeks. 3. Participants in the control group continued their habitual activities of daily living, with no intervention applied.	Pre and **post**.	Online gaming disorder severity, physical activity, daily sitting time, physical fitness, predicted maximum oxygen uptake, **anxiety**.	Virtual reality-based training and aerobic training both reduced the severity of gaming disorder, gaming time and sedentary time and increased levels of physical activity compared to the control group. Additionally, the virtual reality-based training group showed greater improvements in physical fitness than the aerobic training group. Reduced anxiety levels were found after the virtual reality-based training and the aerobic training.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Mannweiler et al. 2023	USA	Children recruited from a low-cost community summer camp	72	0.0	9.08 (1.40)	7-12 years	1. Mightier, heart rate biofeedback-based videogame (**applied game**). 2. Participation in summer camp program activities as usual (**active condition**).	All participants engaged in bi-weekly social and emotional learning groups. Children in the game group further engaged in one 30-minute session each week for 6 weeks. Children in the control group received no additional services.	Pre and **post**.	**Internalising symptoms**, externalising behaviour, **total problem behaviour**, (adaptive) emotion regulation, emotional dysregulation, parental stress.	Participants in the intervention group exhibited greater levels of adaptive emotion regulation and lower levels of internalising symptoms and externalising behaviour after the intervention. Caregivers of participants in the game group also reported significantly less parenting-related stress after the intervention. Both groups demonstrated significant decreases in emotional dysregulation.
Nguyen et al. 2018	Taiwan	College students	337	42.7	22.00 (2.13)	20-40 years	1. Playing exergames from the Your Shape: Fitness Evolved software (**casual games**). 2. Control group (**passive condition**).	1. Participants were asked to play 1 of 10 exergame programs for 30 minutes once a week for two weeks. 2. Participants completed a questionnaire and came back the following week to complete another questionnaire.	Pre and **post**.	**Happiness**; age, gender, optimism and weight control (moderators)	Playing exergames helped to maintain happiness levels and prevented them from decreasing.
Perry et al. 2017***	Australia	Final year secondary students	540	36.9	16.70 (0.51)	16-18 years	1. SPARX-R (**applied game**). 2. Control intervention attention-matched control condition (lifestyle; **active condition**)	Both conditions were delivered weekly in 7 modules, each taking approximately 20 to 30 minutes to complete. The intervention was completed over a 5 to 7 weeks period.	Pre, **post**, 6- and 18-months after baseline (FU).	**Depressive symptoms**, anxiety symptoms, suicidal ideation, personal and perceived stigma toward depression, academic performance; gender, streaming status, living situation (control variables).	Compared to the control condition, participants in the intervention group showed reduced depressive symptoms at post-test and the 6-months FU. Anxiety symptoms reduced for both groups from baseline to post-test. No group differences were found on the secondary outcomes.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Ruiz-Ariza et al. 2018	Spain	Adoles-cents attending summer school	253*m	50.5	13.32 (1.07)	12-15 years	1. Pokémon Go. 2. Control group.	1. Participants used Pokémon Go during 8 weeks. 2. Participants did not use Pokémon Go.	Pre and post.	Memory, selective attention, concentration, mathematical calculation, linguistic reasoning, emotional intelligence (well-being, self-control, emotionality, sociability), in-game data (adherence, amount of physical activity, acquired level, points, Pokémon captured, distance travelled, daily game time), satisfaction; age, sex, BMI maternal educational level, number of computers at home, physical activity (covariates).	Participants who played Pokémon Go increased their selective attention, concentration and sociability compared to the control group. Other variables did not show differences between groups.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Schakel et al. 2020	The Nether-lands	Healthy males	69*[f]	100	22.71 (3.33)	18-35 years	1. Inter-net-based CBT with asyn-chronous therapist, combined with playing a serious game (ViaNova; **applied game**) and a telephone booster ses-sion 2 weeks after the intervention. 2. No intervention control group (**passive condition**).	1. Participants received the intervention for six weeks. 2. Participants did not receive any training.	Pre, **post, one day ******* and 4-weeks after the inter-ven-tion. Note: post-in-tervention partic-ipants were vaccinat-ed with bacillus Calmette-Guérin. One day later, they un-der-went psy-cho-phys-iological stress challenges (i.e., paced auditory serial addition task, cold pressor test, trier social stress test).	Physical and **mental quality of life,** positive and **negative affect,** well-being, vitality, bodily sensa-tions, sleep problems, several psychophys-iological endpoints, several immune endpoints.	The intervention did not affect vitality and quality of life, but sleep problems and bodily sensations were lower directly after the intervention compared with controls. Well-being was higher in the intervention group after the psychophysiological challeng-es compared with the control group.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Shum et al. 2019***	China	Children	459	NA	9.51 (0.69)	8-12 years	1. digital game combined with school-based teaching (**applied game**). 2. Control group (**passive condition**).	1. Eleven 20-minute digital game-based lessons and eight 25- to 60 minute classroom teaching lessons, over a period of 4 to 6 months. 2. The control group only completed the questionnaires.	Pre, **post** and 6-months FU.	**Anxiety symptoms**, mental health knowledge, positive and negative thinking, perspective-taking, self-esteem; completion-rate (subgroup analyses).	At post-test and FU, the intervention group showed significant improvements in mental health knowledge compared to the control group. For perspective-taking, the intervention group showed significant improvements at FU. The intervention was not effective in reducing anxiety and negative thinking.
Staiano et al. 2018	USA	Adolescent girls with overweight or obesity	41	0.0	16.00 (1.40)	14-18 years	1. Exergaming intervention, 'Just Dance' and 'Dance Central' games (**casual games**). 2. No-treatment control (**passive condition).**	1. Three weekly gaming sessions of 60 minutes, for 12 weeks. 2. Participants were asked to maintain their normal level of physical activity for 12 weeks and did not take part in the exergaming intervention.	Pre and **post**.	Height and weight, subjective health, peer support, **health-related quality of life**; enjoyment, evaluation of exergames.	There was no difference by condition for health-related quality of life.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Sun et al. 2022***	China	Secondary school students	390	53.8	13.10 (1.20)	11-17 years	1. Sharing, mind, and enjoyment (SME) smartphone app (**applied game**). 2. Waitlist control group (**passive condition**).	1. An experienced social worker gave an introductory workshop covering aspects of SME, SME in daily life, features of the app, anxious symptoms, and how to seek help for emotional disturbances. Thereafter, the app was used daily for 1 month. 2. The waitlist control group was offered the workshop and app after completing all assessments.	Baseline, pre (immediately after the workshop, but before app use), **post**, and 2-months FU.	Sharing, mind and enjoyment behaviours, subjective happiness, **well-being**, personal health and happiness, family health, happiness and harmony, self-perceived knowledge and understanding of anxious symptoms, satisfaction, subjective changes, difficulties in adherence, suggestions for improvement; age, gender, use of SME-related programs, baseline outcomes (covariates); students using the app with their parents (subgroup analyses).	No differences in SME behaviours were found between the intervention and control group at post and FU. The intervention group did report greater increase in the awareness of anxious symptoms at post and FU compared to the control group. At FU, students who used the app with their parents showed a greater increase in SME-related behaviours than the control group.
Tuijnman et al. 2021***	The Netherlands	Adolescents	285	54.6	13.43 (0.67)	12-15 years	1. Moving Stories program, comprising of an introduction lesson, an applied video game and a contact session with someone who has lived experience with a depressive disorder. 2. No-intervention control.	1. The full program was delivered within one week; gaming consisted of five 15-minutes sessions. 2. Participants in the control group only filled out questionnaires and teachers received information on depression and suicide.	Pre, post, 3- and 6-months FU.	Mental health literacy (symptom recognition, first aid intentions and skills/knowledge, first aid confidence, beliefs about help, help-seeking intentions), stigma (personal and perceived stigma, social distance), first aid behaviour, help-seeking behaviour, depressive symptoms (distal/ side effect measure); program evaluation, contamination check.	Compared with the control group, participants in the Moving Stories group improved after the program in personal stigma. Improvements were maintained at the 3-months FU. Participating in the Moving Stories program was not related to changes in depressive symptoms over time.

2

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Valenzuela et al. 2022***	Chile	Children in socioeconomically vulnerable schools	5923	53.8	10.30 (0.90)	10-12 years	1. Full KiVa program group, including the online game (**applied game**). 2. Partial KiVa program group, without the online game (**active condition**). 3. Control group (**passive condition**).	1. Thirteen 45-minute weekly lessons, with additionally five 45-minute weekly online game lessons. 2. Thirteen 45-minute weekly lessons. 3. Participants followed their regular school curriculum.	Pre and **post**.	Bullying victimization, perpetration and witnessing, **psychological difficulties**, psychological sense of school membership/belonging, academic performance; sex and age (covariates); school grade (moderator).	At post-test, the partial KiVa group had lower bullying victimization than the control group. No effect of the full KiVa group on bullying victimization compared to the control group and partial KiVa group was found. Compared to the control group, the partial KiVa group showed lower witnessing bullying. Participants in the full KiVa group had more psychological difficulties at post-test than the partial KiVa group.
Walsh et al. 2019	Canada	Undergraduate students	108*[g]	16.3	20.02 (2.53)	18-31 years	1. Wildflowers, mindfulness training app (**applied game**). 2. The '2048' game, cognitive training through puzzle game (**casual game**).	1-2. Three weeks, for at least 10 minutes per day.	Pre, during (state measures only) and **post**.	Subjective well-being (nine measures with in total 31 subscales were reduced to a three-factor solution with exploratory factor analysis: **acceptance**, awareness, openness), attentional control (alerting effect, orienting effect, conflict monitoring), interoceptive integration/attention; pre- and post-session ratings of mood, stress level and heart rate (in-app state measures).	Mindfulness training resulted in greater improvements in attentional control compared to cognitive training. Both groups showed increased acceptance and awareness from pre- to post intervention.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Wu et al. 2022	Taiwan	Healthy university students	83*°	57.5	23.20 (2.96)	20-36 years	1. Nintendo Switch Ring Fit Adventure exergame (**casual game**). 2. Control group (**passive condition**).	1. Participants were required to exercise three times per week for 30 minutes, for 4 weeks. 2. Participants maintained their regular exercise habits during the study period.	Baseline, pre and **post**.	Running time, heart condition during exercise (cardiac force index), sleep quality, **mood disorder**; gender (subgroup analyses); age, sex, height, weight, neck and waist circumference, smoking status, alcohol consumption, milk and vegetable and fruit intake, exercise habits, sleep quality and mood disorder (covariates measured at baseline).	Running time significantly improved in the intervention group and remained stable for the control group. Mood disorders increased in the intervention group. No differences between the groups were found on the cardiac force index and for sleep quality.
Yu et al. 2023	Taiwan	Healthy university students	117	42.7	23.70 (2.05)	20-24 years	1. Playing exergames from the Your Shape: Fitness Evolved software (**casual games**). 2. Control group (**passive condition**).	1. Participants were asked to play exergames three times a week for 30 minutes, for 12 weeks. 2. Participants did not play exergames during the study period.	Pre and **post**.	Quality of life (subscales: physical functioning, role-physical, bodily pain, general health, vitality, social functioning, role-emotional, **mental health**); exercise enthusiasm and weight control (moderators).	The intervention group showed increased quality of life in terms of physical functioning, role-physical, general health, and social functioning compared to the control group.

Paper	Country	Target group	N	% male	Age, *mean (SD)*	Age range	Intervention arms	Intervention characteristics	Assess-ments	Variables measured	Findings relevant for current review
Yunus et al. 2020	Malay-sia	Un-dergra-du-ate students	36	13.9	22.89 (1.06)	20-24 years	1. Playing ex-ergame(s) of their choice, in pairs, from Kinect Sports 1 and 2 (**casual game**). 2. Control group (**passive condition**).	1. Three 30-min-utes sessions per week, for six weeks. 2. Participants continued with their daily routine for 6 weeks.	Pre and **post**.	Sleep, **depressive symptoms**, anxiety symptoms, stress symptoms; questions related to feasibility.	Exergaming significantly im-proved sleep and anxiety and stress symptoms.

Notes. Intervention arms and variables measured in bold are included in the forest plot. All studies used a regular randomised controlled trial (RCT) design, unless otherwise stated. CBT = cognitive behavioural therapy; EEG = electroencephalography, FU = follow-up; NA = not available and/or could not be obtained.
*Gender and (mean) age are reported for included/analysed participants (and not for the total randomised participants), with [a] n=66, [b] n=142, [c] n=18, [d] n=123 (gender only), [e] n=371, [f] n=60, [g] n=86, [h] n = 3214, [i] n = 137, [j] n = 48, [k] n = 134, [l] n = 111, [m] n = 190, [o] n = 80.
[p] Remaining participants were 16.3% female and 47.1.% gender minority. [q] The study was set up as a universal intervention. All participants who provided consent were allowed to participate in the study. Participants who scored above the predefined cut-off point for moderate levels of depression or anxiety were allowed to carry on in the study but were also offered additional support. These students were not excluded from the analysis. Of the sample, 36% were identified as being at risk for depression, 24% had high levels of depression, and 34% scored above the cut off for generalised anxiety disorder.
** Crossover RCT.
*** Cluster RCT.
**** Cluster & crossover RCT.
***** For mental quality of life the post value was used to calculate the effect size, for negative affect the value one the day after post was used.
****** Randomisation not applicable as the study used participants from one trial arm of a previous study. Number of participants, gender and mean age refer to the included participants in the current study.

Figure A.2 Domain-level and overall risk-of-bias judgements for studies including a (sub)clinical participant sample

Domain of bias	D1	D1b	D2	D3	D4	D5	Overall
ADHD and attentional problems							
Barkin et al. 2023	+	NA	+	+	+	+	+
Benzing & Schmidt, 2019	+	NA	+	!	+	+	!
Bikic et al. 2017	+	NA	+	+	+	+	+
Bikic et al. 2018	+	NA	+	+	+	-	-
Bul et al. 2016	+	NA	+	-	+	+	-
Bul et al. 2018	+	NA	+	+	+	+	+
Dovis et al. 2015	+	NA	+	+	+	+	+
García-Baos et al. 2019	+	NA	+	!	+	+	!
Ji et al. 2023	+	NA	+	+	+	+	+
Kollins et al. 2020	+	NA	+	+	+	+	+
Medina et al. 2021	+	NA	+	+	+	+	+
Prins et al. 2011	+	NA	+	+	+	+	+
Qian et al. 2018	+	NA	+	+	+	+	+
Rodrigo-Yanguas et al. 2023	+	NA	+	+	+	+	+
Shalev et al. 2007	+	NA	+	+	+	!	!
Smith et al. 2020	+	NA	+	+	+	+	+
Steiner et al. 2011	+	NA	+	+	!	+	!
Tullo et al. 2018	!	NA	+	+	+	!	!
van der Oord et al. 2014	+	NA	+	+	!	+	!
van Houdt et al. 2019	+	NA	+	+	+	+	+
van Houdt et al. 2021	+	NA	+	+	+	+	+
Weerdmeester et al. 2016	+	NA	+	+	+	+	+

Domain of bias	D1	D1b	D2	D3	D4	D5	Overall
Autism and social skill challenges							
Alvares et al. 2019	+	NA	+	-	+	+	-
Beaumont et al. 2015	-	!	+	+	!	!	-
Beaumont & Sofronoff, 2008	+	NA	+	!	+	!	!
Beaumont et al. 2021	!	NA	+	+	+	+	+
de Vries et al. 2015	+	NA	+	!	+	+	!
Dickinson & Place, 2014	!	NA	+	!	+	!	!
Einfeld et al. 2018	!	+	+	+	+	+	!
Faja et al. 2021	!	NA	+	+	!	+	!
Fridenson-Hayo et al. 2017	+	NA	+	!	+	+	!
Griffin et al. 2021	+	NA	+	+	+	+	+
Kirst et al. 2022	+	NA	!	+	+	+	!
Milajerdi et al. 2021	!	NA	+	+	+	+	!
Murphy et al. 2021	+	+	+	+	+	+	+
Sanchez et al. 2014	+	NA	+	+	+	+	+
Sanchez et al. 2017	+	NA	+	+	+	+	+
Sosnowski et al. 2022	+	NA	+	+	+	+	+
Tanaka et al. 2010	!	NA	+	+	+	!	!

Domain of bias	D1	D1b	D2	D3	D4	D5	Overall
Anxiety							
Beidel et al. 2021	+	NA	+	+	+	+	+
Dennis et al. 2014	+	NA	+	!	+	+	!
Haberkamp et al. 2021	+	NA	+	+	+	+	+
Khanna & Kendall, 2010	+	NA	+	+	!	!	!
Knox et al. 2011	-	NA	+	+	+	!	-
McCashin et al. 2022	+	NA	+	+	+	+	+
Scholten et al. 2016	+	NA	!	+	+	!	!
Schoneveld et al. 2018	+	NA	+	+	!	+	!
Schoneveld et al. 2016	+	NA	+	+	+	!	+
Schoneveld et al. 2020	+	NA	+	+	+	!	+
Schuurmans et al. 2018	+	NA	+	-	!	!	-
Schuurmans et al. 2020	+	NA	+	+	+	+	+
Schuurmans, Nijhof, Popma et al. 2021	+	NA	+	+	+	+	+
Schuurmans, Nijhof, Scholte et al. 2021	+	NA	+	+	+	+	+
Tsui et al. 2021	+	NA	+	+	!	+	!
Wijnhoven et al. 2020	!	NA	+	!	+	+	!
Wols et al. 2018	+	NA	+	+	!	+	!

Figure A.2 – (continued)

Domain of bias	D1	D1b	D2	D3	D4	D5	Overall
Depression							
Bohr et al. 2021	+	!	+	+	+	!	!
Fleming et al. 2012	+	NA	+	-	!	+	-
Merry et al. 2012	+	NA	+	+	+	+	+
Poppelaars et al. 2021	+	NA	+	+	+	+	+
Poppelaars et al. 2014	+	+	+	+	!	+	+
Poppelaars et al. 2016	+	+	+	+	!	+	+
Stasiak et al. 2014	+	NA	+	+	+	+	+

Domain of bias	D1	D1b	D2	D3	D4	D5	Overall
Self-injury							
Franklin et al. 2016 - study 1	!	NA	+	!	+	+	!
Franklin et al. 2016 - study 2	!	NA	+	+	+	+	!
Franklin et al. 2016 - study 3	+	NA	+	+	+	+	+

Domain of bias	D1	D1b	D2	D3	D4	D5	Overall
Psychosis							
Fisher et al. 2015	+	NA	+	+	+	+	+
Holzer et al. 2014	+	NA	+	!	+	-	-
Loewy et al. 2016	+	NA	+	+	+	+	+
Piskulic et al. 2015	+	NA	+	!	+	+	!
Urben et al. 2012	!	NA	+	-	+	+	-

Domain of bias	D1	D1b	D2	D3	D4	D5	Overall
Externalising problems							
Coles et al. 2015	+	NA	!	+	!	+	!
Ducharme et al. 2021	+	NA	+	+	+	+	+
Schuurmans et al. 2018	+	NA	+	-	!	!	-

Domain of bias	D1	D1b	D2	D3	D4	D5	Overall
Other clinical populations							
Craig et al. 2016	+	NA	+	!	+	+	!
Hsieh et al. 2016	+	NA	+	+	+	+	+
Mewton et al. 2020	+	NA	+	+	+	+	+
Nekar et al. 2023	+	NA	+	+	+	+	+

Notes. Data of the underlined papers were used to calculate effect sizes and included in the forest plots. D1 = domain 1, randomisation process; D1b = domain 1b, for cluster-randomised trials only, timing of identification or recruitment of participants; D2 = domain 2, deviations from the intended interventions; D3 = missing outcome data; D4 = domain 4, measurement of the outcome; D5 = domain 5, selection of the reported result. + low risk. ! some concerns. - high risk. ADHD = attention-deficit/hyperactivity disorder, NA = not applicable.

Figure A.3 Domain-level and overall risk-of-bias judgements for studies including a healthy participant sample

Domain of bias	D1	D1b	D2	D3	D4	D5	Overall
Anxiety in medical settings							
Burns-Nader et al. 2017	+	NA	+	+	!	+	!
Chan et al. 2019	+	NA	!	+	!	+	!
Dunn et al. 2019	+	NA	!	+	!	!	!
Dwairej et al. 2020	+	NA	+	+	+	+	+
Elicherla et al. 2019	+	NA	+	+	+	+	+
Fernandes et al. 2015	+	NA	!	+	+	!	!
Gold & Mahrer, 2018	+	NA	!	+	+	+	!
Inan & Inal, 2019	+	NA	+	+	+	+	+
Jivraj et al. 2020	+	NA	+	+	!	+	!
Jung et al. 2021	+	NA	+	+	+	!	!
Kassam-Adams et al. 2016	!	NA	+	+	+	+	!
Kjeldgaard Pedersen et al. 2023	+	NA	+	+	+	+	+
Ko et al. 2016	+	NA	+	+	+	+	+
Kumari et al. 2021	+	NA	+	+	+	+	+
Marechal et al. 2017	+	NA	!	+	+	+	!
Matthyssens et al. 2020	+	NA	+	!	+	+	!
Nilsson et al. 2013	+	NA	+	+	+	+	+
Osmanlliu et al. 2021	+	NA	+	+	!	+	!
Pande et al. 2020	+	NA	+	+	+	!	!
Patel et al. 2006	!	NA	+	+	+	+	!
Sahin & Karkiner, 2022	+	NA	+	+	+	+	+
Sakizci Uyar et al. 2021	+	NA	+	+	!	+	!
Schlechter et al. 2021	+	NA	+	+	!	+	!
Stewart et al. 2019	+	NA	+	+	+	+	+

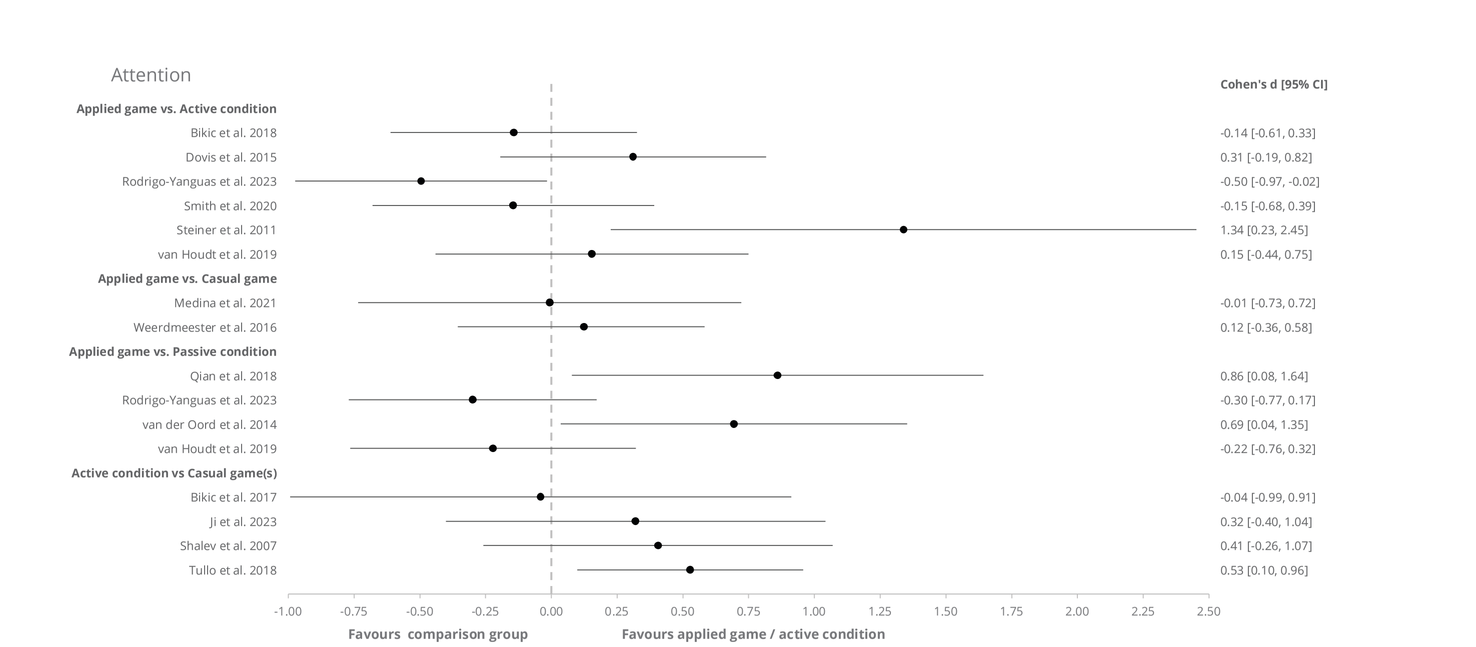
Domain of bias	D1	D1b	D2	D3	D4	D5	Overall
Positive and negative affect							
Alloway & Carpenter, 2021	!	NA	!	!	+	+	!
Andrade et al. 2019	-	+	+	+	+	+	-
Andrade et al. 2020	+	+	+	+	+	+	+
Branton et al. 2014	+	NA	+	+	+	+	+
Douris et al. 2012	+	NA	+	+	+	+	+
Ferguson & Rueda, 2010	+	NA	+	+	+	!	!
Ferguson et al. 2016	+	NA	+	!	+	+	!
Gheller et al. 2019	+	NA	+	+	+	+	+
Goodie & Larkin, 2001	+	NA	-	+	+	+	-
Hunter et al. 2019	+	NA	+	!	+	+	+
Matheson et al. 2021	+	NA	+	+	+	+	+
Poppelaars et al. 2018	+	NA	+	+	+	+	+
Russell & Newton, 2008	+	NA	+	+	+	!	!
Valadez & Ferguson, 2012	+	NA	+	+	+	!	!

(Figure A.3 – continued)

Domain of bias	D1	D1b	D2	D3	D4	D5	Overall
Mental health traits							
Abbott et al. 2014	+	NA	+	+	+	+	+
Axford et al. 2020	!	-	+	+	+	+	-
Cioffi & Lubetzky, 2023	+	NA	+	!	+	+	!
David et al. 2019a	+	NA	+	+	!	+	!
David et al. 2019b	+	NA	+	+	!	+	!
David & Fodor, 2022	+	NA	!	+	+	+	!
David & Fodor, 2023	+	NA	+	+	+	+	+
David & Magurean, 2022	+	NA	+	+	+	+	+
David, Magurean et al. 2022	+	NA	+	+	+	+	+
David et al. 2021	+	NA	+	+	+	+	+
David, Stroian et al. 2022	+	NA	+	+	+	+	+
Egan et al. 2021	+	NA	+	+	+	+	+
Hammond et al. 2014	+	NA	+	+	+	+	+
Hsieh & Chen, 2019	+	+	+	+	+	+	+
Kato et al. 2008	+	NA	+	+	+	+	+
Kuosmanen et al. 2017	+	+	+	+	+	+	+
Mack et al. 2020	+	+	+	+	+	+	+
Maden et al. 2022	+	NA	+	+	+	+	+
Mannweiler et al. 2023	+	NA	+	+	+	+	+
Nguyen et al. 2018	+	NA	+	+	+	+	+
Perry et al. 2017	+	!	+	+	+	+	!
Ruiz-Ariza et al. 2018	+	NA	+	!	+	+	!
Schakel et al. 2020	+	NA	+	+	+	+	+
Shum et al. 2019	-	NA	+	!	+	+	-
Staiano et al. 2018	+	NA	+	+	!	+	!
Sun et al. 2022	+	+	+	+	+	+	+
Tuijnman et al. 2021	!	+	+	+	+	+	!
Valenzuela et al. 2022	+	+	+	!	+	+	!
Walsh et al. 2019	+	NA	+	+	+	+	+
Wu et al. 2022	+	NA	+	+	+	+	+
Yu et al. 2023	+	NA	+	!	+	+	!
Yunus et al. 2020	+	NA	+	+	+	+	+

Notes. Data of the underlined papers were used to calculate effect sizes and included in the forest plots. D1 = domain 1, randomisation process; D1b = domain 1b, for cluster-randomised trials only, timing of identification or recruitment of participants; D2 = domain 2, deviations from the intended interventions; D3 = missing outcome data; D4 = domain 4, measurement of the outcome; D5 = domain 5, selection of the reported result. + low risk. ! some concerns. - high risk. NA = not applicable.

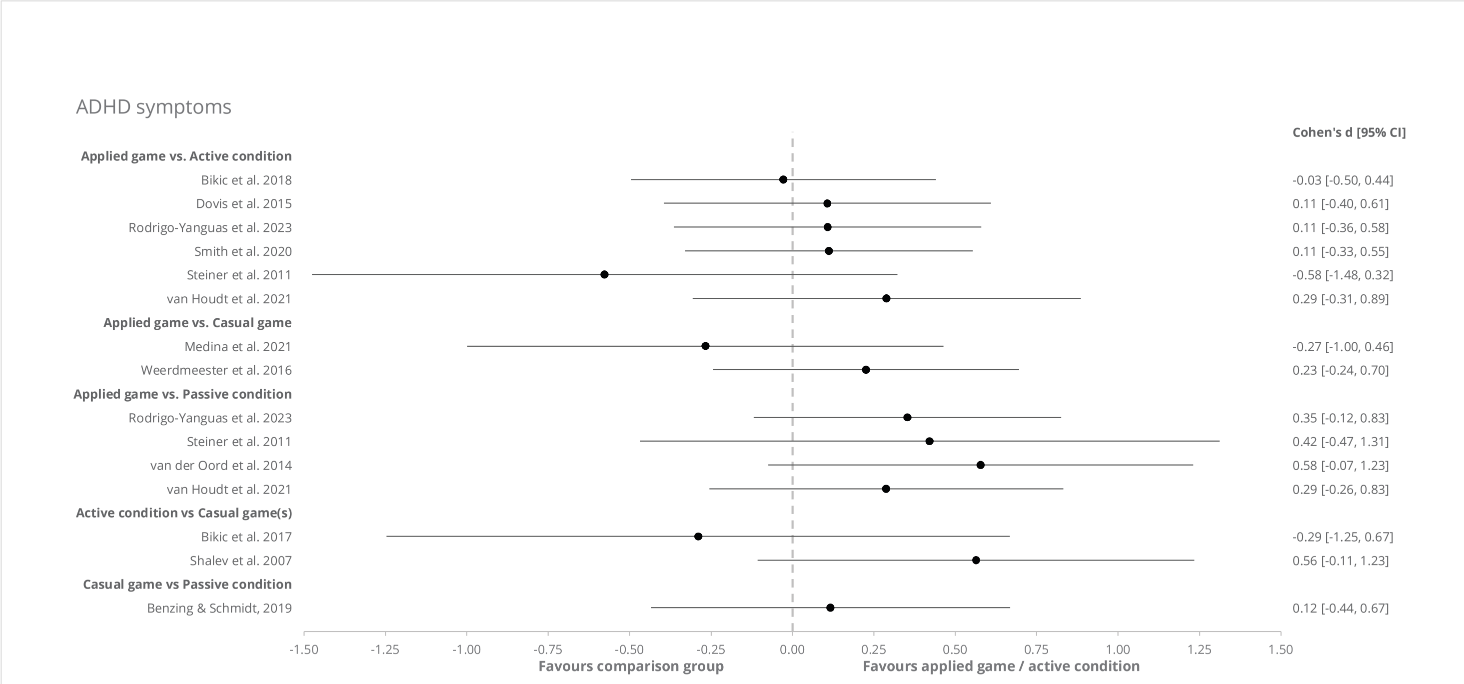
Figure A.4 Forest plot of standardised effect sizes of studies examining attention in youth with ADHD or attentional problems



Notes. For the study of Smith et al. (2020) only data for the US sample was available. Additionally, the authors report a Cohen's d of -0.46 (based on difference between change scores). Tullo et al. (2018) report a Cohen's d of 0.52, with 95% CI [0.09, 0.95] (based on standardised change scores). ADHD = attention-deficit/hyperactivity disorder, CI = confidence interval.

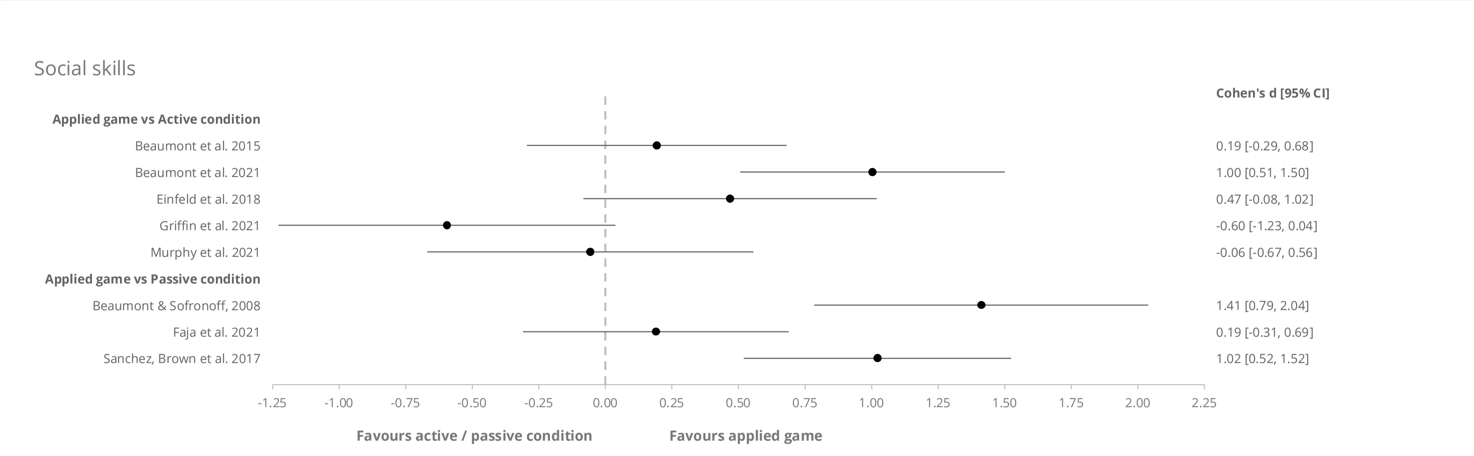
2

Figure A.5 Forest plot of standardised effect sizes of studies examining ADHD symptoms in youth with ADHD or attentional problems



Notes. Smith et al. (2020) report a Cohen's d of 0.21 (based on difference between change scores) and Benzing & Schmidt (2019) a Cohen's d of 0.32. ADHD = attention-deficit/hyperactivity disorder, CI = confidence interval.

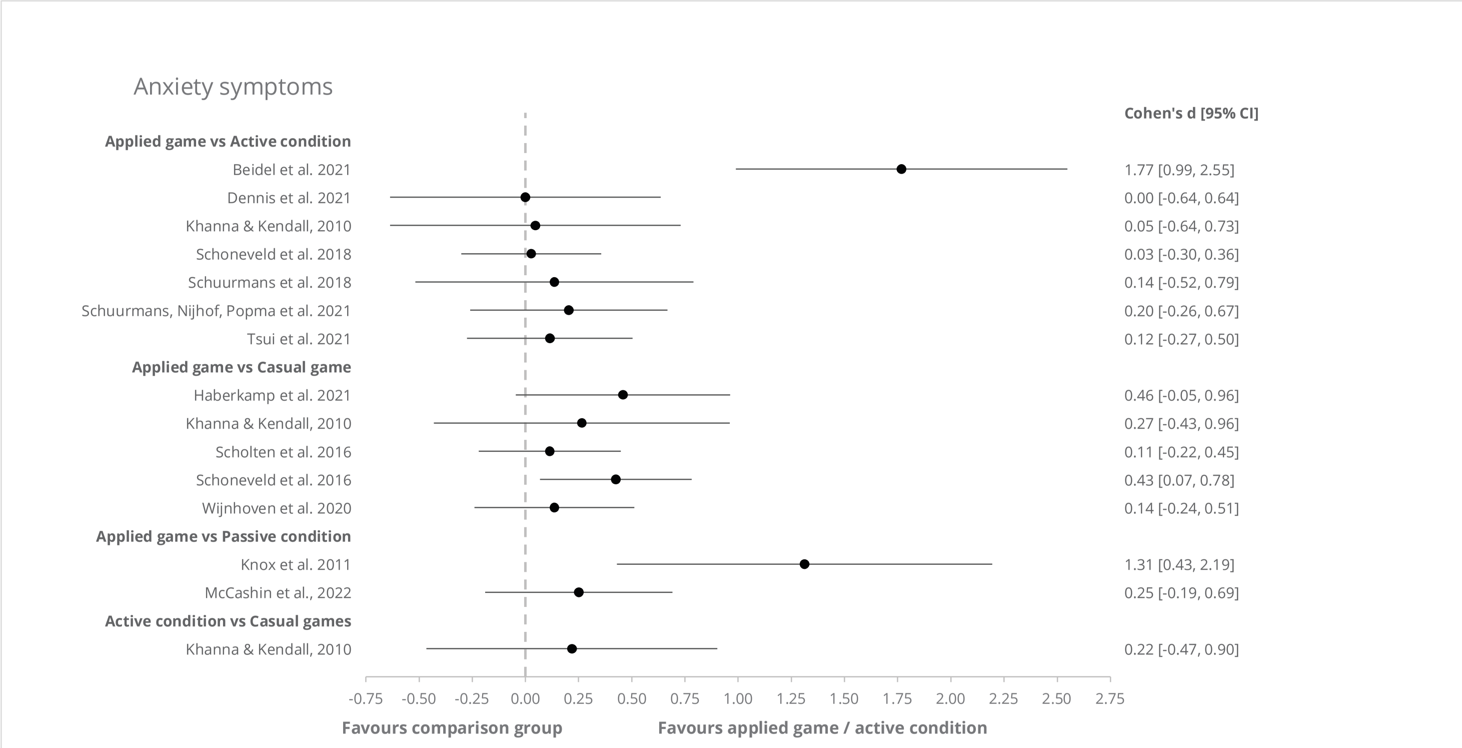
Figure A.6 Forest plot of standardised effect sizes of studies examining social skills in youth with autism or social skill challenges



Notes. The study of Einfeld et al. (2018) used cluster randomisation and a crossover design. The adjusted effect size was calculated based on an average cluster size of 5.60 and an ICC of 0.05 (i.e., design effect = 1.23), which gave a Cohen's d of 0.47, with 95% CI [-0.14, 1.08]. The study of Beaumont et al. (2015) used cluster randomisation. The adjusted effect size was calculated based on an average cluster size of 4.06 and an ICC of 0.05 (i.e., design effect = 1.15), which gave a Cohen's d of 0.19, with 95% CI [-0.33, 0.71]. The study of Murphy et al. (2021) used cluster randomisation as well. The adjusted effect size was calculated based on an average cluster size of 4.17 and an ICC of 0.05 (i.e., design effect = 1.16), which gave a Cohen's d of -0.06, with 95% CI [-0.72, 0.61]. Faja et al. (2021) report a Cohen's d of 0.19. CI = confidence interval, ICC = intracluster/intraclass correlation coefficient.

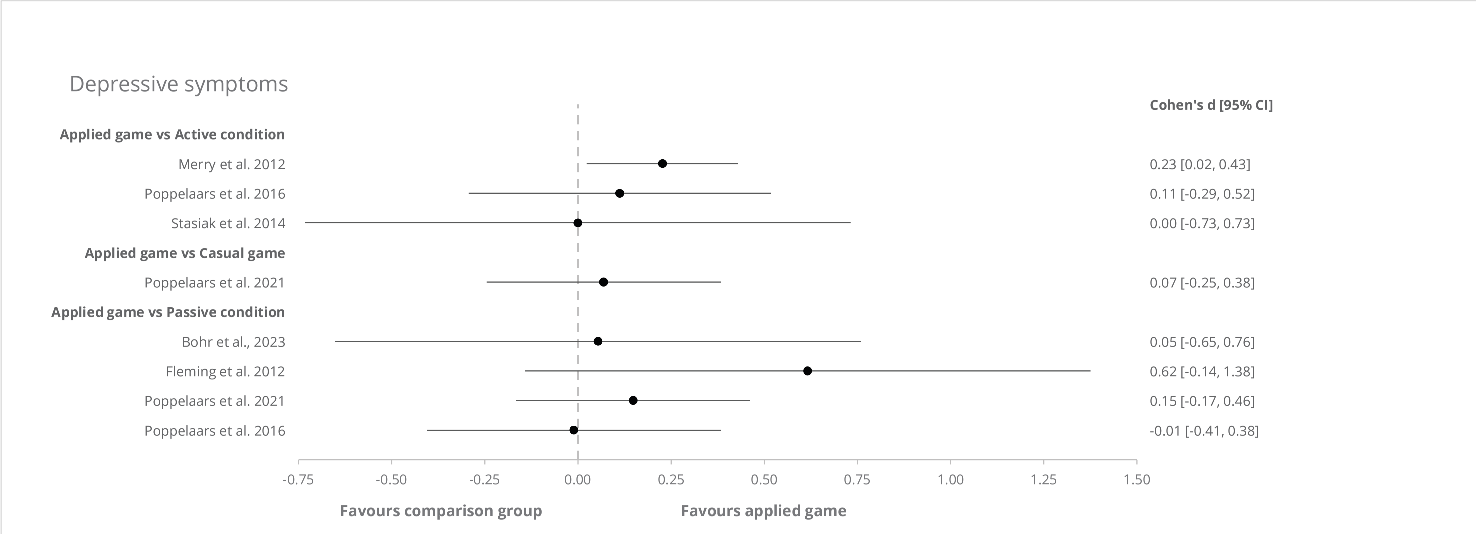
2

Figure A.7 Forest plot of standardised effect sizes of studies examining anxiety symptoms



Notes. Scholten et al. (2016) report a Cohen's d of 0.02 (controlled for clustering and baseline anxiety) and McCashin et al. (2022) a Cohen's d of 0.26, with 95% CI [0.01, 0.11]. CI = confidence interval.

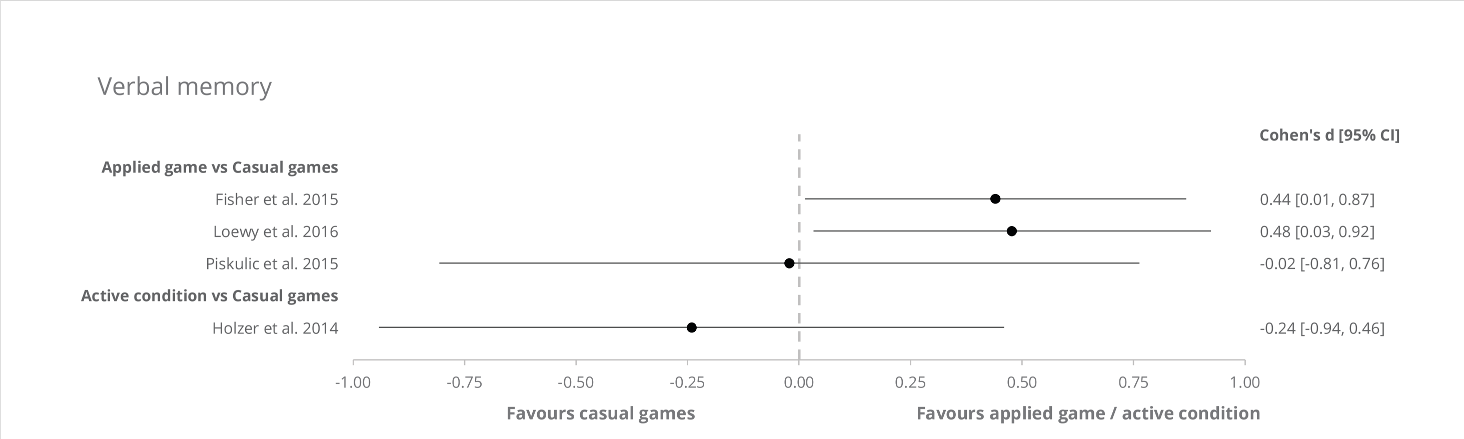
Figure A.8 Forest plot of standardised effect sizes of studies examining depressive symptoms



Notes. The studies of Bohr et al. (2023) and Poppelaars et al. (2016) used cluster randomisation. For Bohr et al. (2023), the adjusted effect size was calculated based on an average cluster size of 3.50 and an ICC of 0.01 (i.e., design effect = 1.03), which gave a Cohen's d of 0.05 with 95% CI [-0.66, 0.77]. For Poppelaars et al. (2016), the adjusted effect size was calculated based on an average cluster size of 13.42 and an ICC of 0.01 (i.e., design effect = 1.12), which gave for the applied game vs. active condition comparison a Cohen's d of 0.11, with 95% CI [-0.32, 0.54], and for the applied game vs. passive condition comparison a Cohen's d of -0.01, with 95% CI [-0.43, 0.40]. Stasiak et al. (2014) report a Cohen's d of 0.70. Fleming et al. (2012) report a Cohen's d of 0.77. CI = confidence interval, ICC = intracluster/intraclass correlation coefficient.

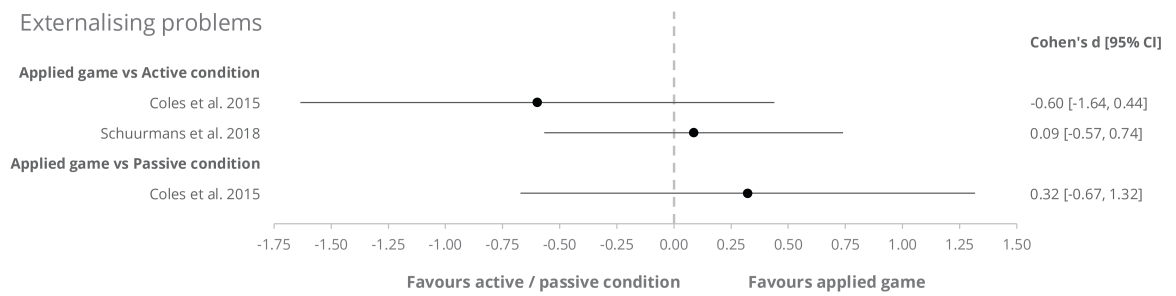
2

Figure A.9 Forest plot of standardised effect sizes of studies examining verbal memory in youth with (high risk of) psychosis



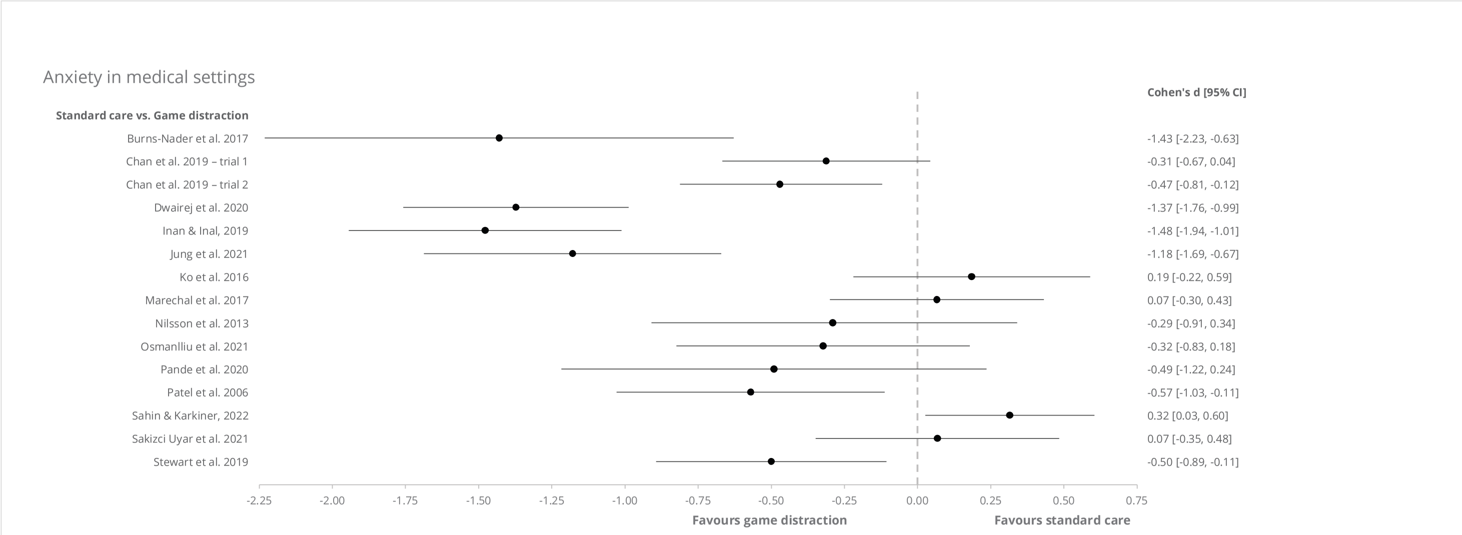
Notes. Fisher et al. (2015) report a Cohen's d of 0.69. Loewy et al. (2016) report a Cohen's d of 0.61, with 95% CI [0.15, 1.05]. CI = confidence interval.

Figure A.10 Forest plot of standardised effect sizes of studies examining externalising problems



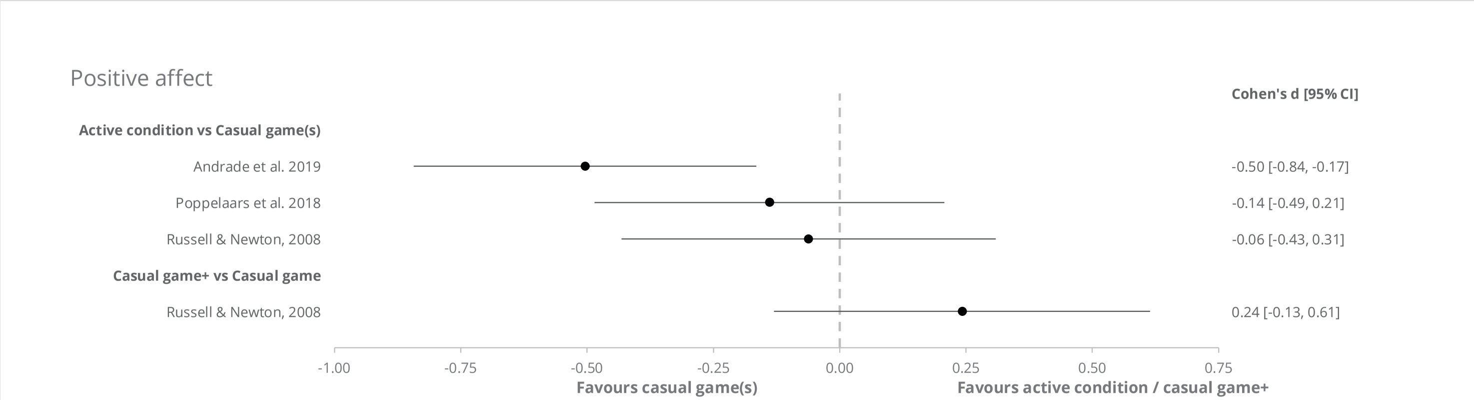
Notes. CI = confidence interval.

Figure A.11 Forest plot of standardised effect sizes of studies examining anxiety in medical settings



Notes. For the study of Nilsson et al. (2013) Cohen's d as reported by the authors was inserted in the forest plot, because the data were significantly skewed away from normality and the normal-based methods for data transformation could not be applied. Dwairej et al. (2020) report a Cohen's d of 1.37 and Stewart et al. (2019) report a Cohen's d of 0.50. CI = confidence interval.

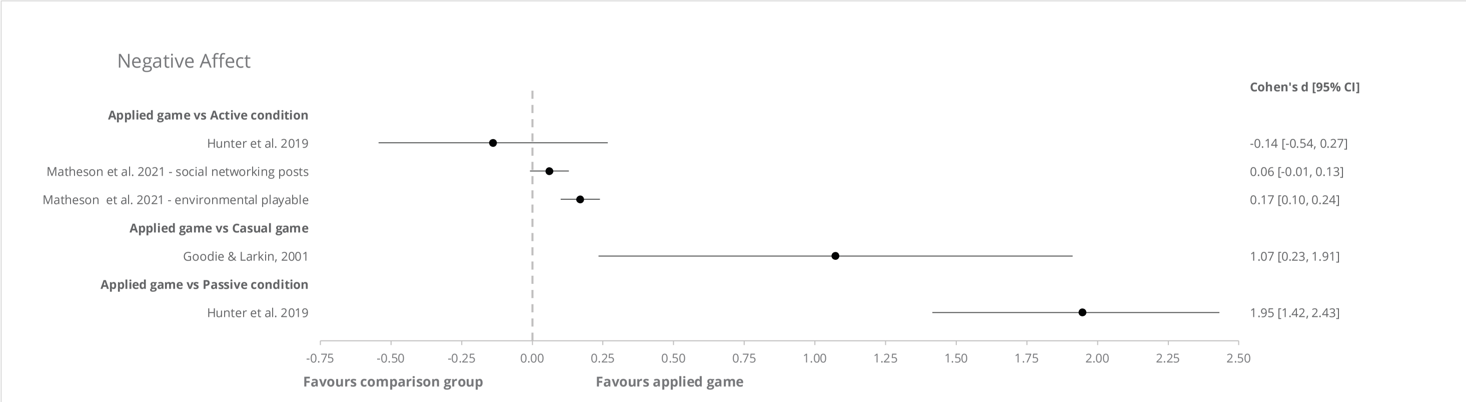
Figure A.12 Forest plot of standardised effect sizes of studies examining positive affect



Notes. In the study of Poppelaars, Lichtwarck-Aschoff, et al. (2018) participants played the game in pairs. The ICC, however, was zero for positive affect at post-test. Therefore, no adjusted effect size was calculated. In the study of Russell & Newton (2008), participants played as much as possible in pairs. However, based on the ICC found in Poppelaars, Lichtwarck-Aschoff, et al. (2018), no adjusted effect size was calculated. The study of Andrade et al. (2019) used cluster randomisation, but based on the ICC found in Poppelaars, Lichtwarck-Aschoff, et al. (2018) no adjusted effect size was calculated. CI = confidence interval, ICC = intracluster/intraclass correlation coefficient.

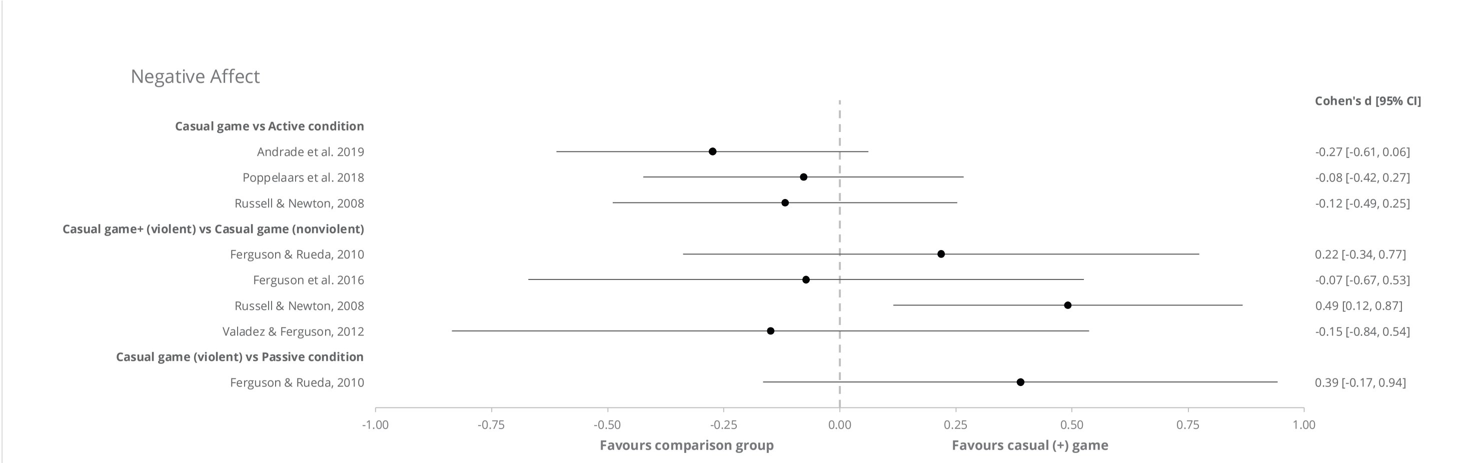
2

Figure A.13a Forest plot of standardised effect sizes of studies examining negative affect with applied games



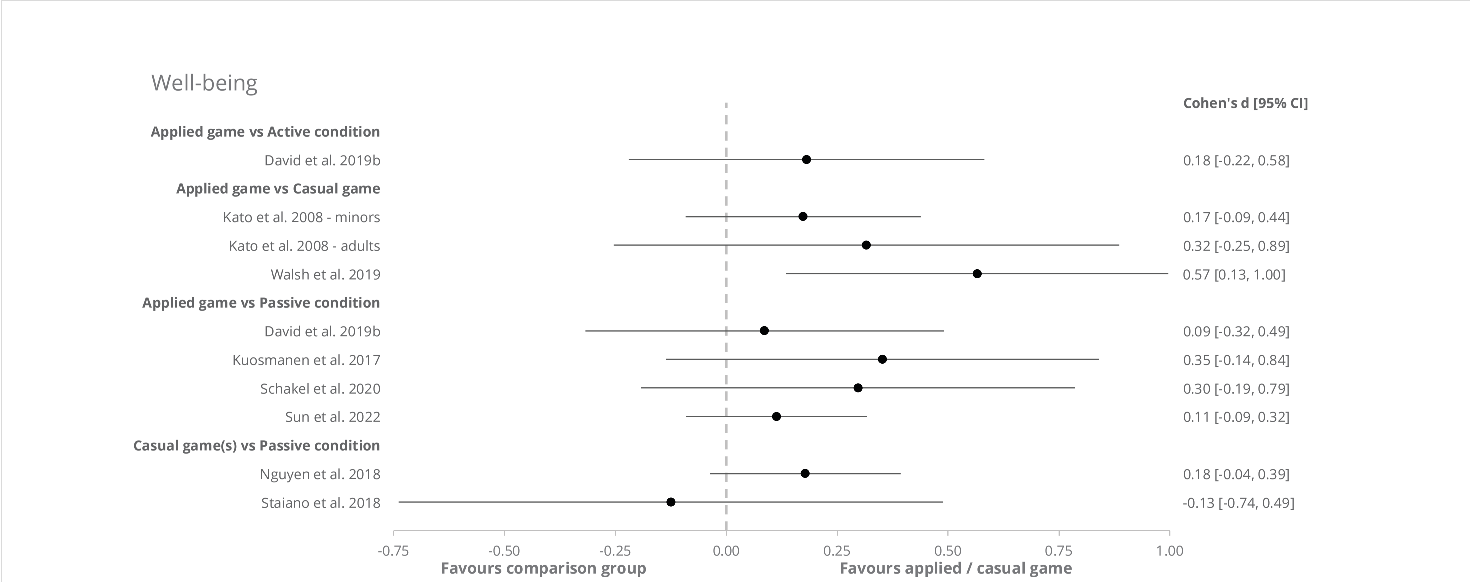
Notes. In the study of Matheson et al. (2021), a body image playable was compared to two active conditions, namely an environmental conservation playable and body image social networking posts. CI = confidence interval.

Figure A.13b Forest plot of standardised effect sizes of studies examining negative affect with casual games



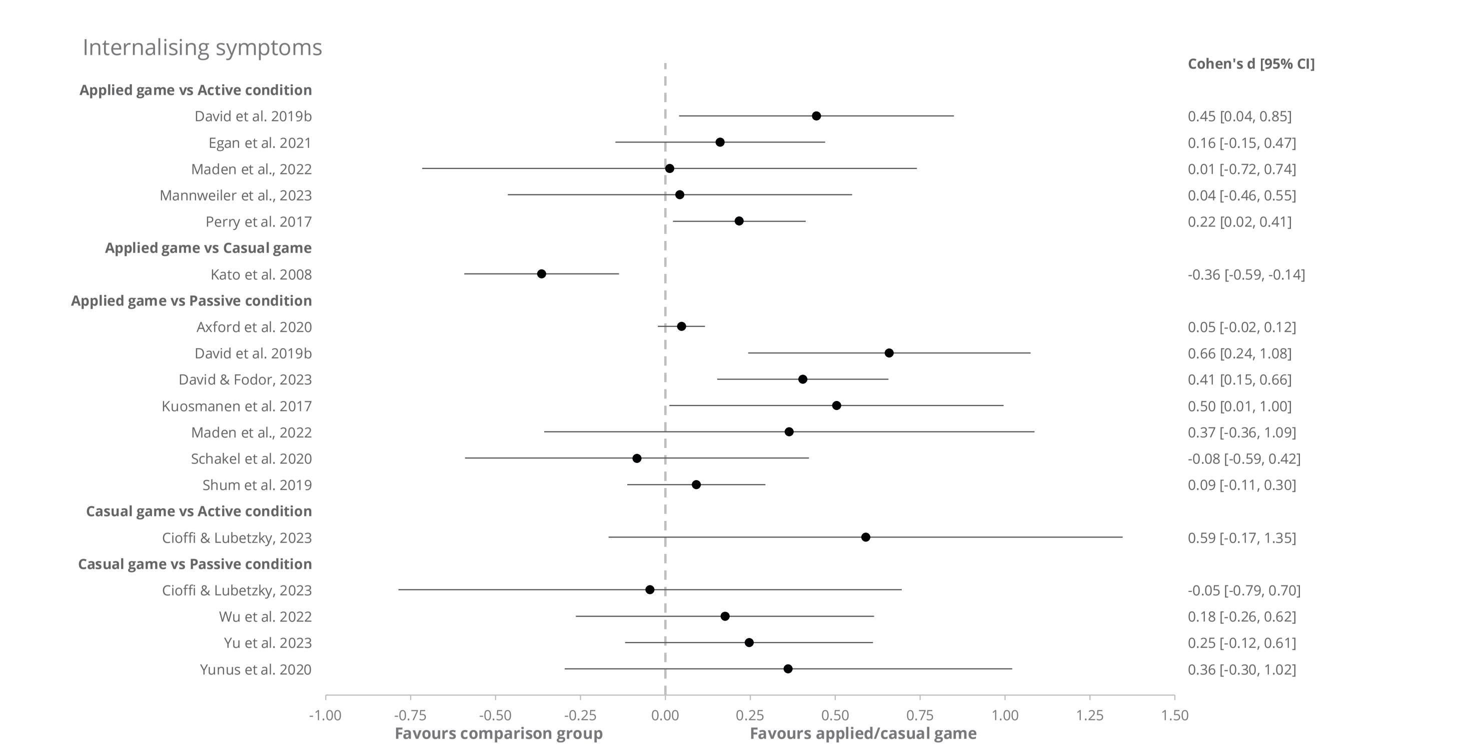
Notes. In the study of Poppelaars, Lichtwarck-Aschoff, et al. (2018) participants played the game in pairs. The average cluster size was 2.00 and the ICC for negative affect at post was 0.0018, resulting in a design effect of 1.00. The adjusted effect size was therefore the same as the unadjusted effect size reported in the forest plot. The same applied to the study of Russell & Newton (2008) in which participants played as much as possible in pairs. Based on the ICC found in Poppelaars, Lichtwarck-Aschoff, et al. (2018), the adjusted effect size was the same as the unadjusted effect size reported in the forest plot. The study of Andrade et al. (2019) used cluster randomisation. The adjusted effect size was calculated based on an average cluster size of 17.50 and an ICC of 0.0018 (i.e., design effect = 1.03), which gave a Cohen's d of -0.27, with 95% CI [-0.62, 0.07]. CI = confidence interval, ICC = intracluster/intraclass correlation coefficient.

Figure A.14 Forest plot of standardised effect sizes of studies examining well-being



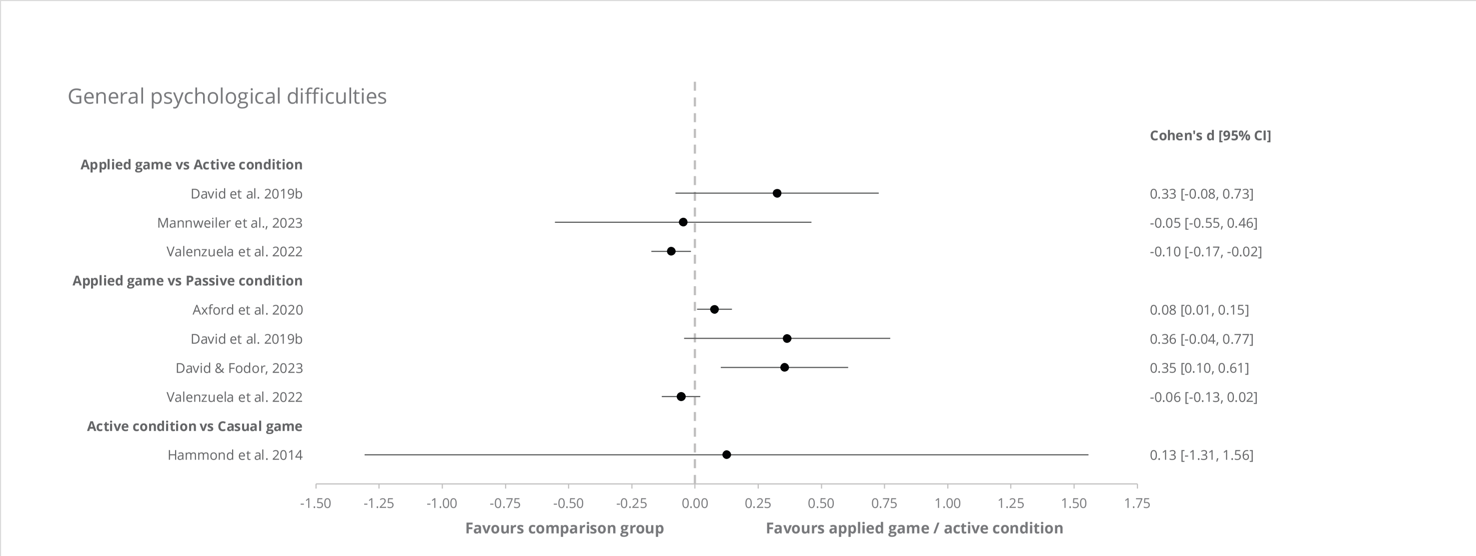
Notes. The study of Kuosmanen et al. (2017) used cluster randomisation. The adjusted effect size was calculated based on an average cluster size of 6.00 and an ICC of 0.02 (i.e., design effect = 1.10), which gave a Cohen's d of 0.35, with 95% CI [-0.16, 0.87]. The study of Sun et al. (2022) used cluster randomisation as well. Because the ICC was close to zero (resulting in a design effect of 1.00), the adjusted effect size was the same as the unadjusted effect size reported in the forest plot. In the study of Kato et al. (2008) different measures for minors and adults were used to measure quality of life and the study is therefore included twice in the forest plot. CI = confidence interval, ICC = intracluster/intraclass correlation coefficient.

Figure A.15 Forest plot of standardised effect sizes of studies examining internalising symptoms



Notes. David et al. (2019b) report a Cohen's d of 0.66 when comparing the applied game to the passive condition. David & Fodor (2023) report a Cohen's d of 0.42 and Perry et al. (2017) report a Cohen's d of 0.29, with 95% CI [0.09, 0.49]. The studies of Axford et al. (2020), Kuosmanen et al. (2017), Maden et al. (2022), Perry et al. (2017), Shum et al. (2019) and Yunus et al. (2020) used cluster randomisation. For Axford et al. (2020), the adjusted effect size was calculated based on an average cluster size of 153.05 and an ICC of 0.097 (i.e., design effect = 15.75), which gave a Cohen's d of 0.05, with 95% CI [-0.23, 0.32]. For Kuosmanen et al. (2017), the adjusted effect size was calculated based on an average cluster size of 6.00 and an ICC of 0.02 (i.e., design effect = 1.10), which gave a Cohen's d of 0.50, with 95% CI [-0.01, 1.02]. In contrast to the unadjusted effect size, the adjusted effect size was nonsignificant. For Maden et al. (2022), the adjusted effect size was calculated based on an average cluster size of 2.00 and an ICC of 0.02 (i.e., design effect = 1.02), which gave for the applied game vs. active condition comparison a Cohen's d of 0.01, with 95% CI [-0.72, 0.75], and for the applied game vs. passive condition comparison a Cohen's d of 0.37, with 95% CI [-0.36, 1.09]. For Perry et al. (2017), the adjusted effect size was calculated based on an average cluster size of 54.00 and an ICC of 0.017 (i.e., design effect = 1.90), which gave a Cohen's d of 0.22, with 95% CI [-0.05, 0.49]. In contrast to the unadjusted effect size, the adjusted effect size was nonsignificant. For Shum et al. (2019), the adjusted effect size was calculated based on an average cluster size of 114.75 and an ICC of 0.02 (i.e., design effect = 3.28), which gave a Cohen's d of 0.09, with 95% CI [-0.28, 0.46]. For Yunus et al. (2020), the adjusted effect size was calculated based on an average cluster size of 2 and an ICC of 0.02 (i.e., design effect = 1.02), which gave the same effect size as the unadjusted effect size reported in the forest plot. CI = confidence interval, ICC = intracluster/intraclass correlation coefficient.

Figure A.16 Forest plot of standardised effect sizes of studies examining general psychological difficulties



Notes. David & Fodor (2023) report a Cohen's d of 0.35. The studies of Axford et al. (2020) and Valenzuela et al. (2022) used cluster randomisation. For Axford et al. (2020), the adjusted effect size was calculated based on an average cluster size of 153.05 and an ICC of 0.097 (i.e., design effect = 15.75), which gave a Cohen's d of 0.08, with 95% CI [-0.20, 0.35]. In contrast to the unadjusted effect size, the adjusted effect size was nonsignificant. For Valenzuela et al. (2022), the adjusted effect size was calculated based on an average cluster size of 166.33 and an ICC of 0.02 (i.e., design effect = 4.31), which gave for the applied game vs. active condition comparison a Cohen's d of -0.10, with 95% CI [-0.26, 0.07], and for the applied game vs. passive condition comparison a Cohen's d of -0.06, with 95% CI [-0.21, 0.10]. In contrast to the unadjusted effect size for the applied game vs. active condition comparison, the adjusted effect size for this comparison was nonsignificant. Valenzuela et al. (2022) report a Cohen's d of 0.13 with 95% CI [0.01, 0.25] for the applied game vs. active condition comparison, and a Cohen's d of 0.11 with 95% CI [-0.01, 0.23] for the applied game vs. passive condition comparison. CI = confidence interval, ICC = intracluster/intraclass correlation coefficient.

Chapter 3

In-game play behaviours during an applied video game for anxiety prevention predict successful intervention outcomes

Based on:
Wols, A., Lichtwarck-Aschoff, A., Schoneveld, E. A., & Granic, I. (2018).
In-game play behaviours during an applied video game for anxiety prevention predict successful intervention outcomes. Journal of Psychopathology and Behavioral Assessment, 40(4), 655-668.
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ABSTRACT

Anxiety disorder is the most prevalent and frequently diagnosed disorder in youth, and associated with serious negative health outcomes. Our most effective prevention programs, however, have several limitations. These limitations can be addressed using game-based interventions. Results from two randomised controlled trials on the video game MindLight show improvements in anxiety that are maintained up to 6 months. The game was designed based on evidence-based therapeutic techniques; however, it is unclear if children's engagement with these techniques actually predict improvements in anxiety symptoms. An important advantage of game-based interventions is that they provide excellent opportunities to isolate therapeutic action mechanisms and test their impact on intervention outcomes. In the current study, on-screen videotaped output while playing MindLight was coded and analysed for forty-three 8 to 12-year old children with elevated levels of anxiety. Results showed that changes in in-game play behaviours representing therapeutic exposure techniques predicted improvements in anxiety symptoms 3 months later (when children had not played the game for 3 months). The current study is a first step towards identifying and validating game mechanics that can be used in new applied games to target anxiety symptoms or other psychopathologies with the same underlying deficits.

Anxiety disorders are the earliest form of psychopathology to emerge in childhood, the most prevalent and frequently diagnosed disorders in youth (Beesdo et al., 2009), and associated with adverse health outcomes (Brent et al., 1986; Essau, 2003, Kessler et al., 1996; Pine et al., 1998; Weissman et al., 1999; Woodward & Ferguson, 2001). Beyond the numbers for clinical diagnoses, sub-clinical levels of anxiety symptoms are estimated at 40% in children (Muris et al., 2000a), which increase the risk for full-blown anxiety disorders at older ages. Currently, programs based on cognitive behavioural therapy (CBT) have been shown to be most effective (Butler et al., 2006; Fisak et al., 2011; In-Albon & Schneider, 2007; Kendall, 2011; Mychailszyn et al., 2012). However, CBT outcomes are mixed and effect sizes are small to moderate (Fisak et al., 2011; Mychailszyn et al., 2012).

These disappointing outcomes of CBT-based indicated prevention programs might be related to limitations regarding the way in which interventions are delivered, rather than the therapeutic principles on which they are based (Granic et al., 2014; Kazdin, 2011). These limitations include a didactic-based approach that might not be engaging and motivating for some children (Gosch et al., 2006), few opportunities to practice newly-acquired knowledge, non-adherence of practitioners to the protocol (Eichstedt et al., 2014), and low accessibility and high costs of interventions (Collins et al., 2004; Farmer et al., 1999; Kataoka et al., 2002).

The aforementioned limitations can be addressed by using game-based interventions (see Granic et al., 2014 for full discussion; Rideout et al., 2010; Olson, 2010). On this basis, the video game MindLight (GainPlay Studio, 2014) was developed in order to prevent the escalation of anxiety in at-risk children. The game incorporates evidence-based techniques by translating these techniques into game mechanics. Game mechanics are the actions in the game that are designed for the player to repeat over and over; they are the vehicles by which certain skills are trained (see Table 1). Results from an indicated randomised controlled trial (RCT; Schoneveld et al., 2016) showed that after playing 5 sessions of MindLight, at-risk children showed significant reductions in anxiety symptoms by three-month follow-up. In addition, a second indicated RCT showed that playing MindLight for 6 sessions is as effective as a CBT-based indicated prevention program (i.e., Coping Cat; Van Starrenburg et al., 2017) in reducing anxiety symptoms from pre- to post-measurement, including the three- and six-month follow-up (Schoneveld et al., 2018)[1]. Finally, results from

1 The effect sizes for the MindLight group were 0.60 from pre- to post-measurement, 0.75 at three-month follow-up, 1.07 at six-month follow-up, comparable to meta-analytic results on CBT (Mychailyszyn et al., 2012) and Kendall's Coping Cat in particular (Lenz, 2015).

a third study showed that compared to online CBT-based psychoeducation, playing MindLight resulted in similar improvements in anxiety symptoms and state anxiety in response to a (social and cognitive) stressor (Tsui et al., 2021).

These results are promising, but it remains unclear whether the means by which children improve in anxiety symptoms are through the game mechanics that were explicitly designed into MindLight. Investigating such in-game processes is extremely valuable, not only because it gives insight into who will benefit most from the game, but also into which game mechanics are most important for anxiety symptom-reduction more generally (and those that are in need of further development). By testing the effect of the game mechanics in MindLight, the current study provides a first step towards building a toolbox of validated game mechanics. In turn, those validated game mechanics that are able to change causal processes associated with the development and maintenance of anxiety could be used in new applied games targeting anxiety symptoms or other psychopathologies with the same underlying deficits.

Table 1 Overview of the evidence-based principles that are translated into game mechanics in MindLight and the specific in-game play behaviours that are indicative of these mechanics

Evidence-based principle	Game mechanic	In-game play behaviours	
		Engaged	**Avoidant/safety**
Relaxation	Neurofeedback	Bright mindlight	No mindlight
Exposure	Approach fear events	Exploration Decloak/attack fear events Defeat Pick up coins	Turn on ceiling light Hide in chest Inactivity
Attention bias modification	Attention bias puzzles	Solve puzzle	

MindLight

MindLight is a video game for children 8 to 12 years of age, designed and developed in a cross-disciplinary collaboration among developmental psychologists, clinicians, game designers, and children themselves. In the game, the player is in grandma's dark and decrepit mansion and needs to save her from evil forces by chasing away or uncovering "fear events" and solving puzzles. The player plays through the game by using his/her "*mindlight*", a beam of light at the end of an antenna attached to the magical hat the avatar is wearing. The player needs this light in order to move through the dark game environment.

MindLight incorporates three evidence-based techniques based on cognitive-behavioural principles: relaxation through neurofeedback training (Price & Budzynski, 2009), exposure training (Feske & Chambless, 1995), and attention bias modification (Bar-Haim, 2010; Bar-Haim et al., 2011). These techniques have been repeatedly shown to address causal processes associated with the development and maintenance of anxiety such as avoidance and negative attention bias (Mathews & MacLeod, 2005; Weersing et al., 2012). In MindLight, the three techniques are translated into specific game mechanics: neurofeedback, approaching fear events, and attention bias modification puzzles (see Table 1), together aiming at teaching children how to cope with anxiety in a playful manner. Below we explain these techniques and game mechanics in more detail. More information can also be found in Appendix A.

The first technique incorporated in MindLight is relaxation training through the use of neurofeedback. In conventional neurofeedback training individuals are presented with real-time electroencephalogram (EEG) recordings from their brain and guided through relaxation exercises in order to keep their EEG waves consistent with identified proxies of relaxation (i.e., reduction in relative beta power and increase in relative alpha power; Price & Budzynski, 2009). In MindLight, the player wears an EEG headset with one (dry) sensor touching the forehead and one reference point located in the clip attached to the left earlobe. The headset detects EEG signals and converts them into a continuous data stream representing relaxation (Johnstone et al., 2012). This data stream is fed into the game and controls the amount of light that shines in the game. The brightness of players' *mindlight* is proportional to the strength of the real-time relaxation of the player. When the player becomes more relaxed the *mindlight* becomes brighter, providing more light in the game environment. This way, players get feedback on their level of relaxation. Thus, relaxation is trained through the neurofeedback game mechanic in MindLight (see Table 1).

The second technique incorporated in MindLight is exposure training. There are fear events (i.e., fearful obstacles) in the game and by shining one's *mindlight* on them they can be chased away or uncovered. Some fear events will then turn into a friendly kitten that follows the player and that reminds the player of past fears conquered. Other fear events turn into a benign object or animal and reward the player with a coin that is needed to unlock puzzles (see below). To play through the game, the fear events need to be approached. However, a player can also escape them (temporally) by hiding inside one of the chests or by turning on a ceiling light (both instances of safety behaviours). Thus, exposure training is done through the fear events in the game (see Table 1).

The third technique incorporated in MindLight is attention bias modification (ABM). Attentional biases characterized by a hyper attention towards potential threats play an important role in the development and maintenance of anxiety disorders (Muris, 2016; Muris & Field, 2008). With ABM-training, the attentional bias towards threats is retrained such that individuals pay more attention to positive stimuli rather than to negative stimuli (Bar-Haim, 2010; Bar-Haim et al., 2011). Conventional ABM-procedures have used a modified version of the dot-probe task or a visual search task and have been shown to reduce anxiety (at least in the short term; Bar-Haim, 2010; Bar-Haim et al., 2011; Hakamata et al., 2010). In MindLight, gaming elements have been added to the main principles of the dot-probe training. In the ABM puzzles, the player learns to focus on and attend to portraits of happy faces rather than threatening faces. Upon completion of the puzzle, the lights will turn back on in that particular room. Thus, ABM-training is done through the ABM-puzzles in MindLight (see Table 1).

In-game play behaviours

Although the game was designed to provide children with repeated training opportunities to learn emotion-regulation strategies, there is also variability in how much children actually practice these skills. This variability is largely a function of the design of the game, which allows children to play at their own pace and explore and progress through the game in a variety of ways that foster a sense of autonomy and fun. For example, some children may be too afraid to move through the game at first, and will hide in chests to avoid the fearful stimuli, while others might prefer to explore a great deal more from the outset. These play-pattern differences may lead to differences in the amount of opportunities to practice the relaxation skills or to encounter fear events further in the game.

There are several specific in-game play behaviours that are most relevant to the intervention goals of MindLight (see Table 1). These behaviours can be classified into two types: “engaged” and “avoidant/safety” in-game play behaviours. Clinical research has shown that avoidant and/or safety behaviours are important maintenance processes for anxiety symptoms (Clark, 1999; McManus et al., 2008; Salkovskis et al., 1999; Salkovskis et al., 1996), and that reducing avoidant and/or safety behaviours and increasing engagement with the treatment are predictive of better treatment outcomes (e.g., Glenn et al., 2013; Morgan & Raffle, 1999; Salkovskis et al., 1999).

While playing MindLight, children can show in-game play behaviours that indicate engagement versus avoidant/safety behaviour. To be successful at MindLight, children need to show engaged in-game play behaviours, which will lead to a higher "dosage", or more practice, with the game mechanics. Avoidant/safety in-game play behaviours limit that "dosage". Differences in the dosage may result in anxiety symptom-reduction for some children and no anxiety symptom-reduction for other children based on *how* they played the game.

Table 1 presents an overview of the in-game play behaviours categorised as "engaged" versus "avoidant/safety" and how these behaviours relate to the game mechanics and cognitive-behavioural principles that are incorporated in MindLight. In the "engaged" category, all behaviours represent experiences that support players' practice of relaxation, exposure, and modifying attention biases. For example, by exploring children are more exposed to fear events, which they can chase away or decloak and from which they can learn how to regulate their anxiety in the face of perceived threat. By solving puzzles they learn to focus more on the positive faces than on the negative ones. The "avoidant/safety" category represents behaviours that interfere with the intervention goals in MindLight. For example, hiding inside a chest or being inactive reduces exposure to fear events in the game. Turning on ceiling lights minimizes relaxation training, because under the ceiling light monsters are less likely to show up and the child needs to rely less on his or her own *mindlight* to brighten the environment.

Design and Hypotheses

In the current study, participants were children with elevated levels of anxiety that participated in a RCT to test the effect of 6 play-sessions of MindLight compared to CBT (Schoneveld et al., 2018). Only children from the MindLight group were included in the current study. Engaged and avoidant/safety in-game play behaviours were coded for the first and last play-sessions to examine 1) how pre-test anxiety scores were related to in-game play behaviours during the first play-session, and 2) whether changes in in-game play behaviours from the first to the last play-session predicted changes in anxiety symptoms at the three-month follow-up assessment. We hypothesised that a) higher pre-test anxiety scores would be related to less engaged, and more avoidant/safety, in-game play behaviours during the first play-session, and b) increases in engaged behaviours and decreases in avoidant/safety behaviours from the first to the last play-session would predict reductions in anxiety symptoms at the three-month follow-up assessment.

METHODS

Participants

Forty-three children (20 boys, 23 girls) with elevated levels of anxiety participated in the current study. Participants' age ranged from 8.17 to 12.65 years ($M = 9.94$, $SD = 1.14$) at pre-test. All children attended primary school in the east of the Netherlands and 97.7% were born in the Netherlands.

Procedure

The current study was part of a 2-armed indicated prevention RCT (Schoneveld et al., 2018) which has been approved by the ethics committee of the Faculty of Social Sciences. Within this RCT, the effect of MindLight on anxiety symptoms is compared to the effect of the CBT-program Coping Cat (Kendall & Hedtke, 2006; Van Starrenburg et al., 2017). Participants for this RCT were selected based on their score on the child-version of the Spence Children's Anxiety Scale (SCAS-C; Spence, 1998). Children were included with a total SCAS-score one standard deviation above the mean or when they scored one standard deviation above the mean on two subscales of the SCAS (not including the obsessive compulsive disorder subscale). Children that already received mental health care were excluded. Seven hundred and ninety-one children from eight primary schools filled out the screening. Two hundred and eight children met the inclusion criteria, of which 174 children agreed to participate in the study. Eighty-six children were randomly allocated to play MindLight. Seventy-seven children completed the MindLight-intervention.

Participants played MindLight (Version 1.0.1; 2014) for a total of approximately six hours. Gameplay was broken down into six one-hour sessions spread out over a six-week period. MindLight sessions took place under supervision of two research assistants at the children's own school after school hours. MindLight was played on 15.6-inch ASUS X552CL-SX033H laptops, using a MindWave headset (Version 1.1.23, Neurosky Inc., 2011; Johnstone et al., 2012) and a Xbox 360 controller. Children and their parents filled out several questionnaires including the Spence Children's Anxiety Scale (Spence, 1998) two weeks before and after playing MindLight, at three-month and six-month follow-up (when children had not played the game for three respectively six months).

Before coding the in-game data for the current study, only children that had completed both pre- and post-measurements of anxiety symptoms were included. In addition, only children with (almost) complete video data were selected: children with less than 4 recorded and/or attended play-sessions, with no recorded first and/or last play-session, with only a short video of the first session (i.e., 6 and 9 minutes), or with significant disturbances[2] during game-play were excluded. In total, video data of 43 children were coded and analysed. Children that were excluded did not significantly differ from the children that were included with respect to their sex, age and pre-test anxiety scores ($\chi^2(1, N=77) = 0.53$, $p = .466$; $t(75) = -0.60$, $p = .551$; and $t(75) = 0.94$, $p = .351$, respectively).

Measures

Anxiety symptoms

The Spence Children's Anxiety Scale (SCAS-C; Spence, 1997, 1998) was used to assess anxiety symptoms before and after playing MindLight. This scale consisted of 44 items where children are asked how often (i.e., never, sometimes, often, always; scored as 0-3) they experience symptoms of six DSM-IV defined anxiety disorders, namely separation anxiety disorder (6 items), social phobia (6 items), panic disorder and agoraphobia (9 items), physical injury fears (5 items), generalised anxiety disorder (6 items), and obsessive-compulsive disorder (6 items). Six items were positive filler items to reduce negative response bias. Mean anxiety scores were calculated over the 38 anxiety items and used in the analyses. The SCAS-C is a reliable and valid measure (Birmaher et al., 1997; Muris et al., 1998; Muris et al., 2000b; Reynolds & Richmond, 1978) and in the current study internal consistency was good; Cronbach's alpha = .89 for pre-test and .90 for post-test.

Coding of in-game play behaviours

During the MindLight sessions, the on-screen output was videotaped using Fraps (Version 3.5.99; 2015). This program records the exact output of the screen (i.e., what the children see on their screens while playing the game). On-screen video data was coded using observational codes with the Noldus Observer XT (Version 11.5; Noldus Information Technology, 2013). Real-time in-game play behaviours during the first and last play-session[3] were coded following the adapted version of the *MindLight Coding System* (i.e., *MCS-II*; based

2 Significant disturbances included: [1] trainer was wearing the MindWave instead of the child, [2] child had to change laptops, which led to the situation that the child had to start over with the game, [3] technical problems or no connection with the MindWave for more than 50% of a specific session.

3 When a child was absent during the last or the fifth session, video data from the fifth or the fourth session was coded, respectively.

on Sherman, 2015; see Appendix B for written guidelines of all codes) which has been developed to measure the frequency and duration of different in-game play behaviours. The *MCS-II* includes mutually exclusive codes for the: [1] type of engagement, [2] location in the game-environment, [3] presence of different kinds of fear events, and [4] brightness of the *mindlight*[4] (which could be “none”, “some”, or “total”). In the current study, codes of interest included four engaged in-game play behaviours and two avoidant/safety in-game play behaviours (see Table 1). Engaged behaviours were the *frequency* of defeat and the *duration* of exploration, attempts to decloak or attack fear events (“fear attempts”), and the brightness of the *mindlight*. Avoidant/safety behaviours were the *frequency* of ceiling light attempts, and the *duration* of hiding inside a chest. A detailed description of all codes can be found in Appendix B.

The first author trained research assistants in the use of Noldus Observer XT and the *MindLight Coding System-II*. Training took approximately 8 hours and training materials included video data from excluded cases. Coders were blind to the hypotheses. Weekly to bi-weekly recalibration training and reliability checks were conducted to monitor coding and minimize coder drift. Fifteen percent of the total amount of coded video data was independently coded by 2 or more randomly-selected coders. The average reliability was .89 kappa (range .49 – 1.00).

Strategy of Analyses

The frequency and duration of in-game play behaviours were transformed to frequencies per minute and proportions, respectively, to control for differences in duration of play-sessions. For the brightness of the *mindlight*, only the durations of the two extremes (“none” and “total”) were used in the analyses. Then, descriptive statistics of the study variables and Pearson correlations between 1) pre-test anxiety scores and in-game play behaviours during the first play-session, and 2) in-game play behaviours during the first play-session and the last play-session were examined. Second, two hierarchical regression analyses (one for engaged and one for avoidant/safety in-game play behaviours) were performed to examine whether changes in in-game play behaviours from the first to the last play-session (i.e., differences in frequencies/proportions calculated as last minus first play-session) were related to changes in anxiety symptoms three months later. The pre-test anxiety score was entered in the first step. The difference in frequency or proportion of the specific in-game play

4 Although the MindWave headset provided a more continuous measure for relaxation through the EEG data, this data could not be matched to the video data. Therefore, the brightness of the *mindlight* as an indication for relaxation was coded manually.

behaviours were entered as a set in the second step. The anxiety score at the three-month follow-up assessment was entered as dependent variable. Before running the regression models, statistical assumptions necessary for multiple regression (i.e., normal distribution of measurement error, homoscedasticity of the variances, linearity of the model, and multicollinearity) were tested and met. Nevertheless, bootstrapping (n = 5000) was used to ensure accurate and valid results (and taking into account the small sample size). One participant that was an outlier (z-score > |3|) on both pre-test and post-test anxiety was excluded from the analyses.

RESULTS

Descriptive Statistics

Means and standard deviations of the study variables are presented in Table 2. Contrary to what was expected, anxiety at pre-test was not significantly associated with the in-game play behaviours during the first play-session (see Table 3). In addition, age and sex were not significantly associated with anxiety at pre-test nor with the different in-game play behaviours during the first play-session.

Moderate to large correlations were found between several in-game play behaviours during the first play-session (see Table 3). Exploration was positively associated with total mindlight, and negatively associated with ceiling light attempts, hiding inside a chest, and fear attempts. Further, total mindlight was negatively associated with ceiling light attempts. Thus, a child that spent large amounts of time exploring the game environment showed longer periods of bright mindlight, fewer ceiling light attempts, and shorter periods of time hiding inside a chest and decloaking or attacking fear events. Longer periods of bright mindlight were associated with fewer ceiling light attempts.

Regarding the correlations between in-game play behaviours during the first and the last play-session, Table 4 shows that none of the in-game play behaviours were associated with one another over time except for the negative association between the proportion of exploration during the first play-session and the proportion of fear attempts during the last play-session. It might be that children that explored more during the first session defeated more fear events during the game, resulting in less fear attempts needed during the last session.

Table 2 Means and standard deviations (SDs) of the study variables

	Demographics	First play-session	Last play-session
Anxiety at pre-test	0.91 (0.31)		
Anxiety at three-month follow-up[a]	0.64 (0.37)		
Age	9.94 (1.15)		
Sex	.48 (.51)		
Engaged in-game play behaviours			
Mindlight - total		.53 (.10)	.46 (.13)
Exploration		.72 (.09)	.81 (.11)
Fear attempt		.38 (.16)	.25 (.20)
Defeat		0.05 (0.03)	0.07 (0.04)
Avoidant/safety in-game play behaviours			
Mindlight - none		.02 (.04)	.05 (.06)
Ceiling light attempt		0.20 (0.13)	0.07 (0.08)
Inside chest		.04 (.03)	.02 (.06)

Notes. n = 42, [a] n = 38. All in-game play behaviours are proportions, except for ‘defeat’ and ‘ceiling light attempt’ which are frequencies.

Table 3 Pearson correlations of anxiety at pre-test, age, sex, and in-game play behaviours during the first play-session

	1	2	3	4	5	6	7	8	9
Demographics									
1. Anxiety at pre-test									
2. Age	.25								
3. Sex	- .22	.01							
Engaged in-game play behaviours									
4. Mindlight - total	.12	- .04	- .08						
5. Exploration	- .03	.01	.08	.32*					
6. Fear attempt	- .14	.01	.12	- .02	- .42**				
7. Defeat	.15	- .05	.02	.18	.05	.03			
Avoidant/safety in-game play behaviours									
8. Mindlight - none	.11	.03	- .10	- .29[a]	- .14	- .28[b]	- .05		
9. Ceiling light attempt	.13	.06	.21	- .32*	- .46**	.01	.27[c]	.10	
10. Inside chest	.18	- .15	.06	- .08	- .54***	- .03	- .14	.07	.16

Notes. * $p < .05$, ** $p < .01$, *** $p < .001$, [a] $p = .067$, [b] $p = .070$, [c] $p = .090$. n = 42. All in-game play behaviours are proportions, except for 'defeat' and 'ceiling light attempt' which are frequencies. Sex was coded as 0 for girls and 1 for boys. Partial correlations testing age and sex as potential suppressor variables showed similar results.

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Table 4 Pearson correlations of in-game play behaviours during the first and last play-session

Last play-session	1	2	3	4	5	6	7
First play-session							
Engaged in-game play behaviours							
1. Mindlight - total	.05	.09	.01	.04	- .09	.04	.12
2. Exploration	- .22	.25	- .36*	- .07	- .09	.13	- .13
3. Fear attempt	.10	- .20	.17	.03	.06	.02	.12
4. Defeat	- .15	.15	.06	.12	.03	.03	- .19
Avoidant/safety in-game play behaviours							
5. Mindlight - none	.09	.11	- .06	.16	- .17	.12	- .13
6. Ceiling light attempt	- .05	- .05	- .14	.03	.18	- .07	.01
7. Inside chest	.20	- .07	.30[a]	.01	- .11	- .24	.18

Notes. * $p < .05$, ** $p < .01$, *** $p < .001$, [a] $p = .051$, n = 42. All in-game play behaviours are proportions, except for 'defeat' and 'ceiling light attempt' which are frequencies. Partial correlations testing age and sex as potential suppressor variables showed similar results.

In-Game Play Behaviours and Improvements in Anxiety 3 Months Later

Table 5 presents hierarchical regression analyses predicting anxiety at the three-month follow-up from the difference in in-game play behaviours from the first to the last play-session controlled for anxiety at pre-test. Model statistics represent the difference in the unique proportion of variance explained by the engaged and avoidant/safety in-game play behaviours respectively, beyond the proportion of variance explained by anxiety at pre-test. Anxiety at pre-test explained 44.4% of the variance in anxiety at the three-month follow-up.

We hypothesised that increases in engaged in-game play behaviours and decreases in safety/avoidant in-game play behaviours from the first to the last play-session would predict reduction in anxiety symptoms at the three-month follow-up. In line with these expectations, Table 5 shows that increases in exploration were significantly associated with lower anxiety scores three months later. For the avoidant/safety in-game play behaviours increases in ceiling light attempts and time spent hiding inside chests were significantly associated with higher anxiety scores three months later.

DISCUSSION

The video game *MindLight* translates evidence-based techniques into game mechanics that teach children how to cope with anxiety in a playful manner. The aim of the present study was to investigate whether children with elevated levels of anxiety improved in their anxiety levels through these game mechanics that were explicitly designed into *MindLight*. Based on the anxiety literature, two types of in-game play behaviours (i.e., “engaged” and “avoidant/safety”) that are most relevant to the intervention goals of *MindLight* were distinguished and coded during gameplay. First, contrary to what was expected, pre-test anxiety scores were not associated with the different in-game play behaviours during the first play-session. Second, in line with our predictions, increases in one of the in-game engaged behaviours and decreases in the two avoidant/ safety behaviours predicted lower anxiety scores at 3-months follow-up. Together, these findings suggest that mechanics related to exposure techniques predicted improvements in children’s anxiety symptoms.

Associations between Pre-test Anxiety Scores and In-Game Play Behaviours

Our finding that none of the in-game play behaviours, and the avoidant/ safety behaviours in particular, were associated with pre-test anxiety scores was unexpected. Because safety behaviours are an important maintenance process in anxiety disorders (Clark, 1999), it was expected that children higher in anxiety would try to create more light by turning on ceiling lights and to hide more inside chests, because these objects provide a way to reduce and/or avoid exposure to fear events. The current study is part of an indicated prevention trial in which selection was based on scoring one standard deviation above the mean and clinical cases that already received mental health care were excluded. Therefore, the current finding might be due to a restricted range in anxiety scores. It might also be that the first play-session was standardized in such a way (with a lot of cut-scenes explaining the game) that little room was left for children to show very different in-game play behaviours. Nevertheless, it is promising that irrespective of their pre-test anxiety scores, children started playing *MindLight* in a similar way because this strengthens the findings for the second hypothesis; changes in in-game play behaviours and the predicted improvements in anxiety symptoms are not due to associations between initial levels of anxiety and in-game play behaviours during the first session. Furthermore, the finding that none of the in-game play behaviours during the first session was associated with behaviours during the last session (except

Table 5 Hierarchical regression analyses predicting anxiety at the three-month follow-up from the differences in in-game play behaviours from the first to the last play-session controlled for anxiety at pre-test

Model	**Unstandardized estimate**	**Standardized estimate**	**Bootstrap 95% CI for B**		**Model Statistics**		
	B (*SE*)	β	Lower	Upper	ΔF	Error *df*	ΔR^2
Anxiety pre-test	0.82 (0.15)	.67***	.54	1.02	28.76	36	.44***
Engaged in-game play behaviours					2.99	32	.15*
Anxiety pre-test	0.90 (0.15)	.74***	0.60	1.17			
Difference in mindlight - total	- 0.29 (0.25)	- .13	- 0.82	0.35			
Difference in exploration	- 1.02 (0.38)	- .33**	- 1.80	- 0.37			
Difference in fear attempt	- 0.27 (0.19)	- .16	- 0.69	0.07			
Difference in defeat	1.22 (0.84)	.18	- 0.52	2.99			
Avoidant/safety in-game play behaviours					5.82	33	.19**
Anxiety pre-test	0.90 (0.13)	.74***	0.65	1.11			
Difference in mindlight - none	0.66 (0.48)	.15	0.02	2.89			
Difference in ceiling light attempt	0.68 (0.25)	.29*	0.21	1.23			
Difference in inside chest	1.51 (0.58)	.28*	0.23	3.02			

Notes. * $p < .05$, ** $p < .01$, *** $p < .001$, 5.000 bootstrap samples. Thirty-eight of the forty-two children completed the three-month follow-up assessment. All in-game play behaviours are proportions, except for 'defeat' and 'ceiling light attempt' which are frequencies.

one association) is promising, because this suggests that *MindLight* is able to change the way in which children continued playing the game after the first session, enhancing opportunities to change anxiety symptoms.

Additional findings showed negative associations between several engaged and avoidant/safety in-game play behaviours during the first play-session, which provide support for the theory-based distinction between these two types of behaviours and at the same time supports the contention that the *MindLight Coding System-II* is able to distinguish these conceptually different in-game play behaviours.

In-Game Play Behaviours Representing Exposure Predicted Improvements in Anxiety

Results showed that changes in in-game play behaviours representing therapeutic exposure techniques predicted improvements in anxiety symptoms three months later. Regarding the engaged in-game behaviours, exploring the fearful game-environment for longer periods of time predicted decreases in anxiety symptoms. The time spent on actual attempts to chase away these fearful stimuli did not predict changes in anxiety symptoms nor did the frequency of getting defeated by these fear events. Regarding the avoidant/safety in-game behaviours, more attempts to seek safety under a ceiling light and increases in time spent hiding inside chests (and therefore avoiding fearful stimuli) predicted higher anxiety symptoms three months later. These findings are in line with previous research on avoidant/safety behaviours during traditional therapy (Glenn et al., 2013; Hammond, 2005; Morgan & Riffle, 1999; Salkovskis et al., 1999; Price & Budzynski, 2009). However, the present study also extends this research by investigating these predictors in a game-based intervention. The current findings suggest that avoidant/safety behaviours play a similar role in an applied video game as in traditional face-to-face therapy.

Moreover, relaxation during game-play (indicated by no or total mindlight) did not predict changes in anxiety symptoms. It might be that relaxation during game-play is not as important for improvements in anxiety as the exposure game mechanic. Recent evidence (published after *MindLight* was developed) suggests that the contribution of relaxation training to anxiety improvements in youth appears to be limited, although it might be that a higher dose of relaxation is needed to improve anxiety symptoms (Peris et al., 2015).

Most importantly, the present study provides a unique contribution to the field by demonstrating that changes in the interaction with the game mechanics in *MindLight* predicted real-world improvements in anxiety symptoms at the three-month follow-up assessment (when children had not played the game for three months). These findings show that what children learned and practiced in the game led to actual changes in their real-life (self-reported) behaviours. Most CBT approaches are not able to close this gap (Granic et al., 2014; Kendall, 2011) and only a few game-based interventions have examined these effects (e.g., Girard et al., 2013). Moreover, by including a long-term follow-up, the present study is one of the few longitudinal studies in the field of applied video games (Girard et al., 2013; Granic et al., 2014; Primack et al., 2012).

Limitation and Suggestions for Future Research

There are a few limitations that should be mentioned when interpreting the findings and future research should attempt to overcome. The first limitation is that no in-game play behaviours representing the third technique (i.e., attention bias modification), that was initially incorporated in *MindLight*, were included in the current study. Because of how the game was designed, these behaviours were too dependent on the specific puzzle and the current location of the player and these locations varied widely across players, leaving comparisons across children unreliable. For example, a puzzle success on an easier puzzle would be different from a puzzle success on a more difficult puzzle, which requires more puzzle attempts and also the ability to distinguish different varieties of positive faces and negative faces. However, evidence for the effectiveness of attention bias modification in the literature is mixed. Some research has shown that attention bias modification is capable of reducing anxiety (Bar-Haim, 2010; Bar-Haim et al., 2011; Hakamata et al., 2010), whereas other recent studies – that were published after *MindLight* was developed - failed to show this (for an overview see Clarke et al., 2014; Cristea et al., 2015). Clarke and colleagues (2014) argue that the studies finding no effects of attention bias modification failed to manipulate attention and therefore found no effects on the outcome measures. Future research should first test whether the puzzles in *MindLight* actually are able to modify attentional bias before in-game puzzle-solving behaviours are examined in relation to changes in anxiety.

A second limitation is that despite the fact that inactivity has good face validity for representing avoidant/safety behaviour because it reduces exposure to fear events and literally limits engagement with the game mechanics in *MindLight*, the inactivity code in the *MindLight Coding System-II* is not able to distinguish between actively avoiding the game or being inactive because the child is waiting for a research assistant to answer a question. Relatedly, the technical problem code might have been confounded too, because this code was used – but not exclusively – when the game was paused. It is possible that some children paused the game to actively avoid fear events in the game. Future research should try to separate these different behaviours, such that codes become as clear as possible and might show relations to changes in anxiety symptoms.

Finally, the current study did not test mediators or moderators. It is possible that associations between (multiple) in-game play behaviours and changes in anxiety symptoms are mediated or moderated by other factors, such as game expectancies, game experience, motivation, and enjoyment, which are known to be important predictors of treatment outcome (Castonguay et al., 1996; Ferguson & Olson, 2013; Przybylski et al., 2010). Future research might want to examine these effects in a larger sample to get insight into why and for whom *MindLight* works best.

Practical Implications

Notwithstanding the aforementioned limitations, the current study has three important implications. The first implication applies to clinical research on anxiety. Results showed that in-game behaviours representing the exposure principle predicted improvements in anxiety. These findings underscore the importance of this technique in anxiety disorders and show that applied games can be used to reduce anxiety symptoms. Using game-based interventions contribute to the movement of tailoring and personalizing treatments (Chorpita & Weisz, 2009) by engaging and motivating children that might not like the rather didactic-based approach of traditional CBT-treatments (Gosch et al., 2006) and by providing rich practice opportunities for children that find it difficult to use newly-acquired knowledge in real-life situations (Granic et al., 2014).

The second implication relates to designs aimed at improving both gameplay experiences and intervention outcomes. Because results demonstrated that the *MindLight Coding System-II* is useful for distinguishing between engaged and avoidant/safety in-game play behaviours, the coding system could be integrated into the game itself. In-game play behaviours can then be tracked automatically and be used as a measure for determining children's progress. Measurements of in-game behaviours can be used to dynamically adjust the game to the player's actions, diverse needs and learning paces (Bakkes et al., 2012; Bakkes et al., 2014). For example, the game may provide more hiding spaces in the beginning of the game for more anxious children (cf. Milosevic & Radomsky, 2008; cf. Rachman et al., 2008), or provide more exposures to fear events when children are able to stay relaxed and calm in the face of less/easier fear events. Additionally, such measurements could tell clinicians and researchers when and where children are encountering difficulties in the game, and provide opportunities to help at an early stage. Finally, measuring in-game behaviours provide feedback for the players, clinicians, researchers and future game-based interventions, without the biases and stigma that are associated with self-reports.

The third implication applies to future research and the development of game-based interventions. Using observational codes, the current study provided a first step in testing the effect of the game mechanics in *MindLight*; the exposure to fear events is an important game mechanic that predicted anxiety symptoms three months later. Because of the modularity of game design, games hold the immense potential to test mechanisms of change with tightly controlled experiments. The next step for future research could be to experimentally manipulate the game mechanics in *MindLight* to test their causal impact on improvements in anxiety. Different versions of the game could be played with and without the game mechanics. Such experimentally controlled studies contribute to the development of a toolbox of validated game mechanics that could be used in new games targeting anxiety symptoms or other psychopathologies with the same underlying deficits.

APPENDIX A

Description of the Video Game MindLight

MindLight incorporates three evidence-based techniques (i.e., relaxation, exposure, attention bias modification) to teach children how to cope with anxiety in a playful manner (see Table 1). To trigger real feelings of anxiety and to practice regulating these feelings, the game is set in grandma's dark and decrepit mansion. Little Arty (the player) is left on the doorstep of grandma's mansion that has been taken over by evil forces. It is his task to save grandma who succumbed to shadows. In the house Arty finds a magical headset (Teru) and together they can save grandma. However, the player needs to overcome his fears by using his own mind. Teru teaches how to use this "mindlight", a beam of light at the end of an antenna attached to the magical hat which players need to play through the game.

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Arty's mindlight responds to the electroencephalogram (EEG) signals that the MindWave headset, a one-channel dry-sensor EEG headset the player is wearing, picks up. The headset detects and converts EEG signals (i.e., relative beta power and relative alpha power) into two continuous data streams representing relaxation and focused attention. These data streams are fed into the game (i.e., *neurofeedback training*) and control Arty's mindlight and "mindbeam". The strengths of the mindlight and mindbeam are proportional to the strength of the relaxation and focused attention signals, respectively. Being more relaxed and focused makes it possible to effectively engage with the objects and evidence-based game mechanics in the game. When the player becomes more relaxed the mindlight will become stronger, providing more light inside the game environment which makes it easier to get through the game. When exposed to fear events, children need their mindlight to chase away or "decloak" these monsters. Some fear events that are decloaked will turn into a friendly kitten that will remind the player of past fears conquered. More focused attention leads to a stronger mindbeam, which is needed to unlock hiding spaces, turn on ceiling lights and to solve attention bias modification puzzles in which the child learns and is rewarded for focusing on positive faces rather than on negative faces.

The goal in *MindLight* is to save grandma by completing all puzzles, which will turn the lights back on in the house. To solve a puzzle, coins have to be found first to unlock the puzzle. Every puzzle requires a predetermined amount of coins (i.e., from 3 up to 7 coins) that corresponds to the number of faces in the puzzle. The amount of coins reflects the difficulty of the puzzle; a puzzle with more coins requires more effort than a puzzle with less coins because in the puzzle with more coins the positive face has to be distinguished from more negative faces.

APPENDIX B

MindLight Coding System-II

Engagement Behaviour Codes

These codes describe the character's behaviour in the game.

Name	Code	Description
Teru Story	ts	Activate this code when Teru explains things to the player character while the picture is letterboxed. The code starts when the picture becomes letterboxed and ends when the picture returns to full screen again. This code is also activated when Arty discovers the magical hat. After Teru's story, activate the *Exploration* code before you use another code.
Defeat	d	Coded as point event. Defeat occurs when the health meter in the lower-left corner of the screen is reduced to zero heart icons. Granny will begin to hunt the character. If the character is caught by Granny, he will be incapacitated and sent back to the map room. The picture will fade out to black, and then fade in as the character revives in the map room. Activate the *Defeat* code as soon as the picture fades to black. In between, most of the times the *Exploration* code will be activated.
Inactive	in	The player character is classified as inactive if three conditions are met: (1) the character remains still and (2) there are no changes in camera angle (3) for a period of 6 seconds or more. Activate the code after these 6 seconds. NB. When the character is inside a chest, use the *Inside Chest* code. When the character is in front of a fear event, use the *Fear Attempt* code.
Exploration	ex	The player character is moving through the game-environment *or* the camera angle is being adjusted.
Ceiling Light Attempt	cla	The player character begins to project his mindbeam onto a ceiling light.
Ceiling Light Success	cls	Coded as point event. The ceiling light turns on and illuminates the surrounding environment.
Chest Attempt	cha	The player character begins to project his mindbeam onto a chest. If the player is successful, a cloud of smoke will appear and the character will be transported into the chest. Consequently, when the cloud of smoke appears, immediately activate the *Inside Chest* code. When the character leaves the chest, activate the *Exploration* code. If the player is unsuccessful, the mindbeam will not be projected onto the chest anymore. Activate the appropriate engagement code (this will be the *Exploration* code).
Inside Chest	ich	When the cloud of smoke appears, immediately activate the *Inside Chest* code. When the character leaves the chest, activate the *Exploration* code.
Pick up Coin	coi	Coded as point event. The player character picks up a coin. NB. Sometimes you don't see this (due to e.g., camera angle), but you will hear when a coin is picked up.
Decloak Cat	cat	Coded as point event. Sometimes when the player character decloaks a (green-eyed) fear event, a cat will appear. Code when the cat appears.

Name	Code	Description
Puzzle Activation	pac	Coded as point event. Use this code when the player character activates the blue cog-shaped platform of a puzzle.
Puzzle Attempt	pat	Start this code as soon as the mindbeam makes contact with the puzzle face. Stop the code and use the *Exploration* code when the mindbeam is no longer connected to the puzzle face.
Puzzle Success	psu	Coded as point event. When the attention bias modification task within a puzzle room is completed, the room will light up. Pleasant music is audible in these illuminated rooms. Activate the *Puzzle Success* code as soon as the room lights up. NB. Also use this code when the player solves the map room puzzle multiple times.
Fear Attempt	fat	The player character moves towards the fear event and/or stays in front of the fear event in order to let it disappear (this means that the player character doesn't necessarily need to be facing the fear event). Start the code at the right distance (i.e., when you see purple spots). NB. Sometimes the player might stay in front of a monster that cannot be beaten. Do activate this code at the same distance as you would do for the other monsters.
Fear Success	fsu	Coded as point event. The player character successfully decloaks the fear event.
Technical Problem	2	Use this code when the picture freezes, the game is paused, when the game is restarted or when there is no connection with the mindwave*. Start the code from the beginning that this technical problem appeared. * When the connection is at ¼ or ½.

Location Codes

These codes describe the character's location.

Name	Code	Description
Entry Hall	ent	When you just entered the house, you will be in this hallway. There are two doors at both ends of the hallway (the front door which is locked and the door to the bedroom).
Bedroom	bed	Arty's bedroom. There are, for instance, his bed, pictures on the wall, teddy bears and a box behind his bed.
Couch Room	cr	This is the room you enter after the bedroom. In this room there are two red couches and on the other side of the room there are three chairs. In this room the player character learns how to turn on a ceiling light.
Hall between Couch Room and Fear Room	hb	After the couch room you walk through this hallway to the fear room. There are no doors and only one ceiling light in this hallway.
Fear Practice Room	fp	In this room there is a blue round carpet and the player character learns to decloak a fear event. There is one door that is locked and the other door is already open.
Box Hall	bh	You enter this hallway by walking through the open door in the fear room. This hallway has a lot of boxes and one chest. The player character learns here to open a chest. This hallway leads to the map room or to the secret hallway (if you walk around the door).

Name	Code	Description
Secret Hall	sh	You can only enter this hallway by walking around the door between the fear room and the box hall. After a right turn the hallway will have five doors.
Map Room	mp	The map room contains a map of the game environment, which is located to the left of a door flanked by two puzzle faces. This room serves two general purposes: *recuperation* and *orientation*. The player is transported back to the map room upon defeat. Alternatively, the player may return to the map room voluntarily to orient themselves within the game space.
Box Room	box	In this room there are a lot of piled boxes and chairs. The puzzle in this room consists of three faces.
TV Room	tv	In this rooms there are a lot of seating areas and a TV that is very noisy. The puzzle in this room consists of four faces.
Ball Room	bal	In this room there are different long dining tables, flags and knights. The puzzle in this room consists of five faces.
Tree Room	tre	In this room there are different gardens with a lot of trees. The puzzle in this room consists of six faces.
Fear Room	fr	In this room there are several fear events and seven coins that need to be collected to activate the puzzle in the Final Room. Through this room it is possible to enter the Final Room (although it is also possible to enter the Final Room right away).
Final Room	fin	After turning on the lights in all the rooms, the Final Room is unlocked. This is the hardest room in the game. If the player is successful in finding all the coins and solving the puzzle in this room, all the lights in the mansion will turn on. The player character then receives a letter from his deceased grandpa with the instruction to bring this to Granny's Room in order to save her.
Granny's Room	gr	When the player character enters this room with the letter from grandpa, the picture will be letterboxed and shows the end of the story.
Small Hall	sma	Hallways are areas of the game that connect puzzle rooms. If there are no coin icons in the upper-right corner of the screen, then the character is in a hallway. There are no monsters in the small hallway.
Wide Hall	wid	Hallways are areas of the game that connect puzzle rooms. If there are no coin icons in the upper-right corner of the screen, then the character is in a hallway. There are monsters in the wide hallway.
Technical Problem	1	Use this code when the picture freezes, the game is paused, when the game is restarted or when there is no connection with the mindwave*. Start the code from the beginning that this technical problem appeared. *  When the connection is at ¼ or ½.

Fear Event Presence Codes

These codes describe the presence/absence of a fear event.

Name	Code	Description
Fear on Screen – NO	nf	Use this code when there are no fear events or monsters visible on the screen.
Fear on Screen - YES	yf	Use this code when there are fear events or monsters visible on the screen. Indicate under modifiers the type of fear: 1). Fear event (rumbling/shadowed objects) 2). Monster with yellow eyes 3). Monster with green eyes. NB. Sometimes you know the monsters are within the screen, but you cannot see them because of, for instance, the camera angle. Do activate this code in that case.
Technical Problem	4	Use this code when the picture freezes, the game is paused, when the game is restarted or when there is no connection with the mindwave*. Start the code from the beginning that this technical problem appeared. * When the connection is at ¼ or ½.

MindLight Codes

These codes describe the brightness of the mindlight. Given the variance in ambient lighting throughout the game space, both luminosity and body language should be taken into consideration to discern the mindlight codes. When, for instance, the brightness is difficult to see (due to a lot of light in the room or the camera angle), pay attention to the body posture and facial expression of the player character.

Name	Code	Description
Pre-Teru	P	Use this code when the player character hasn't picked up Teru yet. Start this code when the picture fades black between the overall story of the game and that you see Arty in the first hallway.
None	N	The environment around the player is almost to completely dark and/or the character's body language is suggestive of high state anxiety; he is frowning, slouching, and crossing his arms.
Some	S	The mindlight is extended to provide a small-to-moderate range of illumination. Furthermore, the character's posture is suggestive of a slightly anxious state; he is holding his arms close to his body and is beginning to slouch forward.
Total	T	The mindlight is extended to provide a large range of illumination. Furthermore, the character's posture exudes confidence; he is standing erect, smiling, and his arms are held out and to the side, causing his body to take on a triangular silhouette. Another important identifying feature of total mindlight activation is the sparkle particle effect (bright flashes of light) that is visible on the ground in front of the character.
Teru Occupied	O	During engagement in activities such as turning on ceiling lights, unlocking chests, and solving puzzles, the diffuse mindlight typically seen during exploration will be focused into a beam or not visible at all (for example, when walking around while making a puzzle, when inside a chest). Activate the *Teru Occupied* code following the onset of the foregoing activities.
No Connection	C	Use this code when there is no connection with the mindwave (green circle is half or less).
Technical Problem	3	Use this code when the picture freezes, the game is paused or when the game is restarted. Start the code from the beginning that this technical problem appeared.

Chapter 4

Mental health outcomes of an applied game for children with elevated anxiety symptoms: A randomised controlled non-inferiority trial

Based on:
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ABSTRACT

Anxiety disorders are the most prevalent mental health problems in childhood. Engaging, adequate, and appropriate prevention programs are needed. Applied games form a potential alternative delivery model and recent evidence suggests that they could be effective. The present randomised controlled non-inferiority trial investigated the beneficial effects of the applied game MindLight compared to cognitive behavioural therapy (CBT) on mental health outcomes associated with anxiety symptoms: internalising problems, externalising problems, and self-efficacy. In addition, we examined who benefitted most from both programs and analysed baseline levels of anxiety, maternal mental health problems, and self-efficacy as predictors of changes in anxiety symptoms. After being screened for elevated anxiety, 174 selected children (8–12-year-old) were randomised to play MindLight or to receive a prevention program based on CBT. Study variables were assessed before and after the intervention, and at 3- and 6-months follow-up. Intention-to-treat analyses showed a significant reduction in mother-reported internalising and externalising problems and an increase in self-efficacy. Importantly, the magnitude of change did not differ between intervention groups. Non-inferiority analyses showed that MindLight was as effective as CBT in affecting internalising problems and self-efficacy. However, CBT was more effective in decreasing externalising symptoms than MindLight. Furthermore, baseline anxiety levels, maternal mental health problems, and self-efficacy did not influence the change of anxiety symptoms over time. Applied games, specifically theory-based games such as MindLight, hold potential as effective interventions for not only targeting anxiety symptoms, but also more general mental health outcomes.

Approximately one in five children has an anxiety disorder (Beesdo et al., 2009) and even more children suffer from subclinical levels of anxiety, with prevalence rates up to 49% (Muris et al., 2000a). Compared to children with low levels of anxiety, these children perform worse at school (Owens et al., 2012), and have a lower general quality of life (Ramsawh & Chavira 2016). In addition, children with elevated levels of anxiety have more depressive feelings (Lavigne et al., 2015), more difficulties in relationships with their peers (Hoglund & Chisholm 2014), express more conduct problems (Priddis et al., 2014; Kidwell et al., 2016), and show lower levels of self-efficacy (Mathews et al., 2016; Niditch & Varela 2012; O'Neal & Cotten 2016) than children with low levels of anxiety. Left untreated, anxiety symptoms show a disabling and chronic course (Asselmann & Beesdo-Baum 2015). Therefore, effective, accessible and engaging prevention programs are needed that are implemented before full-blown anxiety disorders develop (World Health Organization 2012).

Decades of research has led to the development of several anxiety prevention programs (e.g., Van Starrenburg's adaptation of Kendall's Coping Cat, Barrett's FRIENDS for Life and Rapee's Cool Little Kids programs). However, conventional programs face several obstacles that hamper their implementation. Specifically, stigma (Salloum et al., 2016; Mukolo & Heflinger 2011) and program costs (Salloum et al., 2016) impede parents and children from seeking help. Furthermore, conventional programs are moderately effective as shown in various meta-analyses (Stockings et al., 2016; Mychailyszyn et al., 2012; Fisak et al., 2011; Teubert & Pinquart 2011) and drop-out rates are high (i.e., 28 up to 75%; De Haan et al., 2013), possibly because programs are not engaging, adequate, nor appropriate (World Health Organization 2012). These obstacles call for a reconsideration of current group-based and clinical expert-led delivery models of prevention programs (Kazdin, 2015). To overcome those barriers, applied games have recently been put forward as an alternative delivery model of therapeutic techniques used in prevention programs (Kazdin, 2015). In contrast to current services, games might be cheaper than therapists, easily accessible, engaging and not stigmatizing (Granic et al., 2014).

In the past, we have tested the effectiveness of the applied game MindLight in two randomised controlled indicated prevention trials (RCTs; Schoneveld et al., 2016, 2018). MindLight is an applied game designed for children with anxiety symptoms. The game uses several evidence-based techniques, informed by cognitive behavioural therapy (CBT): exposure (Kendall et al., 2005), attention bias modification (Bar-Haim et al., 2011) and neurofeedback (Price & Budzynski 2009). These techniques are embedded in a horror-themed survival game that trains children to cope with anxious feelings. First, anxiety symptoms reported by both children and parents (primary outcomes) were examined in one RCT (Schoneveld et al., 2016). It was found that MindLight was as effective as a commercial video game (i.e., Max and the Magic Marker, an award-winning exploratory puzzle game*)* in 8–12-year-olds with elevated levels of anxiety symptoms. Anxiety symptoms decreased after game play and up to 3 months later. The second RCT was designed to more rigorously examine the effectiveness of MindLight by comparing the applied game with the first-line treatment of choice for anxiety symptoms: CBT (A. C. James et al., 2015). Children who played MindLight showed the same decrease in anxiety symptoms as children who received CBT (Schoneveld et al., 2018). Importantly, the magnitude of improvement was equal across MindLight and both comparison groups (i.e., commercial game and CBT). In addition, children's age and gender did not moderate effectiveness in both RCTs.

In light of these initial positive effects of MindLight on anxiety symptoms, the next steps are to investigate whether MindLight also has beneficial effects on other outcomes associated with anxiety symptoms, and for whom MindLight might be most effective. Children who experience elevated levels of anxiety often also suffer from depressive feelings (Lavigne et al., 2015), have difficulties in relationships with their peers (Hoglund & Chisholm 2014), express more externalising symptoms such as conduct problems (Priddis et al., 2014; Kidwell et al., 2016), and are generally characterized by low levels of self-efficacy (Mathews et al., 2016; Niditch & Varela 2012; O'Neal and Cotten 2016). Given the debilitating effect of these problems on the lives and further development of these children, and the fact that they co-occur as well as contribute to further increases in anxiety, it seems important to investigate whether anxiety prevention programs also have a beneficial effect on those domains.

Previous studies assessing changes in internalising problems after anxiety prevention programs found that pre-school aged children with an anxiety disorder decreased in internalising behaviour problems from pre- to post-test after receiving an internet-based, therapist assisted, parent-focused, CBT program (Donovan & March 2014). In addition, Morgan et al. (2016) found that highly inhibited children between the age of 3 and 6 years improved significantly in emotional symptoms during an online version of the parenting group program Cool Little Kids. Last, a meta-analytic review showed that interventions targeting anxiety in youth showed significant effects on depressive symptoms for treatment and universal prevention programs, but not in targeted prevention programs (Garber et al., 2016). Thus, it seems that anxiety prevention programs are able to improve other internalising problems as well.

Whether anxiety prevention programs also have beneficial effects on externalising problems is unclear, but there are reasons to believe this may be so. Research on the comorbidity between anxiety and aggression could be informative. Two recent reviews about the often-found comorbidity between anxiety and aggression focus on attention control (Fraire & Ollendick 2013; Granic, 2014). Anxious children pay more attention to potential threats in their environment (i.e., attentional bias; Bar-Haim et al., 2011) and have less processing capacity left to focus and sustain attention on other stimuli (i.e., attentional control; Fraire & Ollendick 2013; Reinholdt-Dunne et al., 2013; Eysenck et al., 2007). This vigilant focus on the potential negative aspect of the environment might consume most of the available resources. As a result, anxious children may have difficulties inhibiting their impulses and act out and behave aggressively (Granic, 2014). Thus, when anxiety symptoms are decreasing as a result of the prevention program this might free up cognitive resources to better regulate impulses (Hadwin & Richards 2016) and consequently externalising problems might decrease.

Last, previous research has not focused on changes in self-efficacy following anxiety prevention programs, but it seems important to consider as well (Muris 2001; Muris et al., 2009). Self-efficacy has been theorized to play a key role in the aetiology and maintenance of anxiety and refers to the belief in one's ability to produce a desired behaviour (Bandura, 1997; Maric et al., 2011). More specifically, self-efficacy in the social domain (i.e., perceived capability to deal with social situations), academic domain (i.e., perceived capability to master academic affairs), and emotional domain (i.e., perceived capability to cope with negative emotions) seem relevant for the study of anxiety in youth (Muris, 2001). In the literature focusing on intervention and treatment, one study assessed self-efficacy in a CBT-program for school-refusing children and found improvements in children's self-efficacy for school situations (i.e., perceived ability to cope with anxiety-provoking situations at school; King et al., 1998). Another study found improvements in self-efficacy for anxiety management in youth with anxiety disorders following CBT (Suveg et al., 2009). These findings suggest that it may be important to examine changes in self-efficacy following anxiety prevention programs.

Next to investigating whether MindLight has beneficial effects on other outcomes associated with anxiety symptoms, another important question pertains to the idea that individuals respond differently to prevention programs. There may be important predictors of efficacy to consider. Past research in CBT-based anxiety prevention programs for children has identified several potential baseline predictors, such as baseline anxiety, maternal mental health problems, and self-efficacy. Research investigating the effect of anxiety symptoms at baseline on the response to a prevention/intervention program is inconclusive. One study found that higher levels of baseline anxiety were related to greater decreases in anxiety after an indicated CBT-based program (Van Starrenburg et al., 2017). However, another study found that children with clinical anxiety show a more limited response to pain-focused CBT than children with subclinical levels of anxiety (Cunningham et al., 2016), suggesting that anxiety symptoms at baseline adversely impact the intervention response.

Further, parents of children with mental health problems often have mental health issues themselves (Powdthavee & Vignoles 2008; Goodman et al., 2011). Research has shown that parental problems with mental health can be genetically transmitted (Lubke et al., 2016). Alternatively, parental problems can also impact children's mental well-being through distortions in parenting, for example harsh discipline (Gershoff, 2002) or a controlling parenting style (Chorpita et al., 1998). For anxiety specifically, more controlling parents diminish children's sense of personal control, thereby contributing to increases in anxiety (Chorpita et al., 1998). Last, parental modelling of anxious behaviours and cognitions may also contribute to children's anxiety (Fisak & Grills-Taquechel 2007).

Studies investigating the effect of maternal mental health problems on intervention effectiveness show inconsistent results. One study on the effect of maternal depression on posttraumatic stress treatment in children found that maternal depression was associated with increasing posttraumatic stress symptoms in children (Weems & Scheeringa 2013), especially for children with higher baseline levels of these symptoms (Nixon et al., 2012). However, another study showed that maternal psychopathology did not predict the effect of a depression and anxiety prevention program for adolescents with parents with mental health problems (Rasing et al., 2018).

There are no studies that have directly examined the role of self-efficacy on anxiety prevention effects. Therefore, it is unclear whether children with high or low levels of self-efficacy respond differently to anxiety prevention programs. A recent meta-analysis of 155 experimental trials found, however, that self-efficacy has a causal effect on health-related intentions and behaviour (Sheeran et al., 2016), indicating that when people believe that they can execute the relevant action, they are more likely to change their health intention and behaviour.

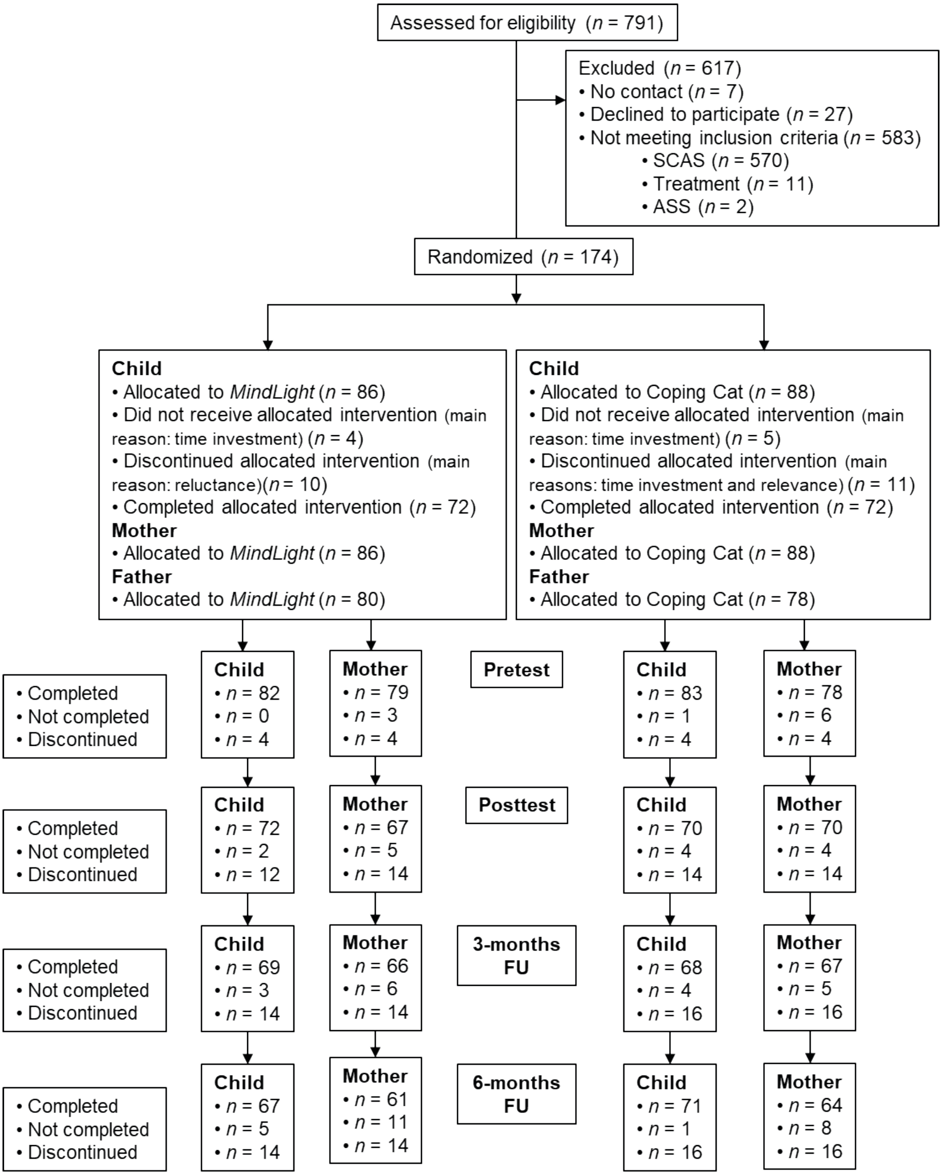
The current study is the second study out of a non-inferiority RCT comparing MindLight and CBT (Schoneveld et al., 2018) and describes its secondary outcome results. The first aim of the present study was to report the effect of MindLight on mental health outcomes of children with elevated levels of anxiety symptoms. Specifically, changes in internalising problems, externalising problems, and self-efficacy were investigated. Based on previous research, we hypothesised that MindLight and CBT would be equally effective in decreasing internalising and externalising problems, and in increasing self-efficacy. The second aim was to assess possible predictors of MindLight and CBT anxiety outcome: baseline anxiety symptoms, maternal mental health problems, and self-efficacy. We hypothesised that baseline anxiety would, and maternal mental health problems would not predict changes in anxiety symptoms. We tentatively hypothesised that higher baseline levels of self-efficacy would predict a larger decrease in anxiety symptoms.

METHODS

Participants

A total of 174 children were randomly assigned to MindLight ($n = 86$) or CBT ($n = 88$; see Figure 1 for a flowchart of participants throughout the trial). The target total sample size was 135 children. Details about the sample size calculation can be found in Schoneveld et al. (2018). At pre-test, children ranged from 7 to 12 years old ($M = 9.97$, $SD = 1.16$) and 59.2% were girls. Mothers were between 28 and 49 years old ($M = 41.13$, $SD = 3.67$) at pre-test, the majority being Dutch (87.9%). No differences were found between the MindLight and the CBT group on pre-test anxiety symptoms, age, sex, and weekly game time (i.e., number of hours spent playing video games per week; see Schoneveld et al., 2018).

Figure 1 Flowchart of participants through trial



Procedure

The study was designed as a randomised, multicenter non-inferiority study with two parallel intervention arms: MindLight and CBT. Between January and September 2015, all children with active parental consent in grades 3–6 from eight primary schools in the southeast part of the Netherlands ($N = 791$) were first screened on anxiety symptoms with the child version of the Spence Children's Anxiety Scale (SCAS; Spence, 1998). Screening took place in the classroom during school hours in the presence of one or two members of the research team. Parent reports were not included in the screening for practical reasons, but also because self-report questionnaires for anxiety are considered more reliable than parent reports (Hourigan et al., 2011; Lagattuta et al., 2012; Lahikainen et al., 2006). Children were eligible if either at least two SCAS subscales (excluding the obsessive compulsive disorder subscale) or the total SCAS score, was 1 *SD* or more above the mean found in a large normative sample (Muris et al., 2000a). Parents of the 221 (27.9%) eligible children were contacted by phone to assess exclusion criteria and invite them and their child(ren) to participate. Children were excluded if they already received anxiety treatment or if they were diagnosed with either obsessive-compulsive disorder, posttraumatic stress disorder or autism spectrum disorder. These children were excluded because obsessive-compulsive disorder and posttraumatic stress disorder are no longer considered an anxiety disorder in the DSM-V, and children with these disorders (including autism spectrum disorder) may benefit from a more specific or specialized treatment than the prevention programs in the current study (e.g., Barrett et al., 2008; Nauta & Scholing 2007). Parents of 174 (78.7%) children gave initial verbal consent; written informed consent was obtained from parents at pre-test, a week prior to the intervention. Children and their mothers then filled out the questionnaires at school or online respectively (i.e., pre-test). Next, children were randomised within school and younger/older age groups, and stratified by sex and grade to participate in the MindLight or CBT group (further details of the randomisation procedure are available elsewhere; see Schoneveld et al., 2018). Children and mothers were assessed again at completion of the intervention (post-test), and 3- and 6-months post-intervention completion (i.e., 3- and 6-months follow-up).

MindLight

MindLight is a 3D third-person neurofeedback video game designed by a multidisciplinary team of researchers from the PlayNice Institute and game designers from GainPlay Studio. The game starts with a little boy named Arty who is left at the doorstep of his grandmother's scary mansion. In his bedroom, he finds Teru, a magical glowing hat that faces him with the task of saving grandmother from the evil forces that have possessed her and the house. Teru teaches Arty (and the player) to change his state of mind and thereby overcome his fears. The player controls the movement of Arty using a Microsoft Xbox 360 controller and (s)he controls Teru's light via the Neurosky one-channel dry-sensor EEG headset (i.e., MindWave). Several evidence-based, theoretically grounded strategies for decreasing anxiety were translated into game mechanics: neurofeedback, exposure, and attention bias modification. The game mechanic associated with neurofeedback involves the amount of light that shines from the player character's (Teru's) magical hat. The EEG headset that the player wears measures the real-time relaxation of the player, which in turn controls the amount of light in the game environment. When the player becomes more relaxed, the light becomes brighter. The exposure game mechanic involves fear events (i.e., fearful obstacles) that need to be approached in order to play through the game. By shining one's light on the fear events they can be chased away or uncovered. Uncovered fear events will either turn into a friendly kitten or an animal/benign object which rewards the player with a coin that is needed to unlock the (attention bias modification) puzzles. In these puzzles, the player learns to focus on and attend to portraits of happy faces rather than threatening faces. To complete the puzzle, the player needs to shine one's light on the happy faces, which eventually will turn the light back on in that particular room. For a more elaborate description of MindLight, see previous papers on the applied game (Schoneveld et al., 2016, 2018; Wijnhoven et al., 2015; Wols et al., 2018).

Children played MindLight in 1-h sessions at school, after regular school hours, once a week, for 6 weeks. Children played the game individually but they were seated in a room with about five to ten other children. Children were seated one table away from each other and used earplugs to hear the game sound and to diminish distraction. Master's degree students gave instructions about MindLight and supervised the groups.

CBT

The CBT program used in the current study was an adaptation of Kendall's Coping Cat (Flannery-Schroeder et al., 2005; Van Starrenburg et al., 2017). It is one of the few CBT prevention programs for children with elevated anxiety symptoms that focuses on anxiety-specific symptoms, emphasizes exposure and is freely accessible (Van Starrenburg et al., 2017). Specifically, a shortened eight-session (9 h) Dutch version of the indicated prevention group-based version of Van Starrenburg et al., (2017) was given. In this program, children are taught both cognitive (i.e., cognitive restructuring) and behavioural techniques (i.e., relaxation training and exposure). Children received two 1.5-h sessions and six 1-h sessions at school, after regular school hours, once a week, for 8 weeks. Groups consisted of four to seven children and were led by two CBT trainers (Schoneveld et al., 2018). Parents were informed about the progress of their child halfway through the program and after the last session via e-mail.

Measures

Anxiety Symptoms

Children's anxiety symptoms were measured with the child (45 items) and mother (38 items) versions of the SCAS (Spence 1998). To reduce negative response bias, the child version includes seven positive filler items. All items were rated on a four-point scale: 0 = never, 1 = sometimes, 2 = often, 3 = always. Good convergent validity (Brown-Jacobsen et al., 2011; Muris et al., 2000b) and reliability (Whiteside & Brown 2008) are demonstrated for both the child and the mother version. In our sample, Cronbach's alphas were 0.90–0.93 for the child version and 0.80–0.84 for the mother version across all time points. Two outcome variables were computed: total anxiety child and total anxiety mother, which are the overall means (with the exception of filler items).

Self-Efficacy

Children's self-efficacy was measured with the self-report version of the Self-Efficacy Questionnaire for Children (SEQ-C; Muris, 2001). The 24 items were rated on a five-point scale (0 = very bad, 1 = pretty bad, 2 = not good, not bad, 3 = pretty good, 4 = very good) and represented three domains of self-efficacy: (1) social self-efficacy: perceived capability for assertiveness and peer relationships, (2) academic self-efficacy: perceived capability to fulfil academic expectations, to master academic subjects, and to manage one's own learning behaviour and (3) emotional self-efficacy: perceived capability to cope with negative emotions. The SEQ-C shows satisfactory internal consistency, reliability and validity (Muris, 2001). In our sample, Cronbach's alphas were between 0.73–0.84 for academic self-efficacy, between 0.64–0.75 for social self-efficacy, and between 0.79–0.88 for emotional self-efficacy across all time points. Three outcome variables were computed: social, academic and emotional self-efficacy.

Internalising and Externalising Problems

Children's internalising and externalising problems were measured with the mother version of the Strengths and Difficulties Questionnaire (SDQ; Goodman, 1997; Stone et al., 2010). The SDQ includes 25 items divided in three subscales: (1) internalising problems: emotional symptoms and peer relationship problems; (2) externalising problems: conduct problems and hyperactivity/inattention symptoms; (3) prosocial behaviour. We used this three subscales division, because it is more valid than the original five subscales division in a low-risk (i.e., without disorders) sample (Goodman et al., 2010). We only used the first two subscales in this study. All items were rated on a three-point scale: 0 = not true, 1 = somewhat true, 2 = certainly true. The two subscales showed good convergent and discriminant validity, and good internal reliability (Goodman et al., 2010). In our sample, Cronbach's alphas were between 0.72–0.75 for internalising problems and between 0.75–0.79 for externalising problems across all time points. Two outcome variables were computed: internalising problems and externalising problems.

Maternal Mental Health Problems: Depression, Anxiety and Stress

Maternal mental health problems were assessed with the 21 items version of the Depression Anxiety Stress Scales (DASS-21; Antony et al., 1998). The 21 items, all covering negative feelings, were rated on a four-point scale (0 = not at all, 1 = sometimes, 2 = often, 3 = usually) and represented three subscales: depression, anxiety, and stress. The subscales show good concurrent validity and reliability (Antony et al., 1998). In our sample, Cronbach's alphas were 0.89 for depression, 0.81 for anxiety and 0.87 for stress at pre-test. Three outcome predictor variables were computed: maternal depression, maternal anxiety and maternal stress.

Data Analyses

A *t*-test and a χ^2-test were performed in IBM SPSS Statistics 23 to assess whether randomisation was successful for sex and age. To test non-inferiority, we used a two-sided confidence interval (CI) approach. The idea behind this approach is that if the upper bound of the CI for the difference in mean change in secondary outcomes is below the margin of non-inferiority, MindLight is non-inferior to CBT. Based on a previous indicated anxiety prevention trial (Van Starrenburg et al., 2017), the margin of non-inferiority was set at 0.38 SEQ points for social, emotional and academic self-efficacy, at 1.11 SDQ points for internalising problems, and at 0.90 SDQ points for externalising symptoms. These differences correspond to 0.5 SD of the change in emotional self-efficacy (*M* = −0.28, *SD* = 0.76), internalising symptoms (*M* = 1.50, *SD* = 2.22), and externalising symptoms (*M* = 1.10, *SD* = 1.80) at post-test in children in the CBT condition of the Van Starrenburg et al. (2017) trial.

To further examine the effectiveness of MindLight on the secondary outcomes (i.e., social, emotional, and academic self-efficacy, internalising and externalising problems), Latent Growth Curve Modelling (LGCM) was performed using Mplus 7.2 (Muthén & Muthén 1998–2012). First, we estimated the initial model based on the four time points (i.e., pre-test, post-test, 3- and 6-months FU) without any predictors or control variables. Second, we tested whether condition predicted initial levels of outcomes (i.e., intercept) and/or rates of change in outcomes (i.e., slope). For our second aim, predictors (i.e., baseline anxiety symptoms, maternal mental health problems and self-efficacy) of the effectiveness of MindLight and CBT on anxiety symptoms were also assessed with LGCM. The effectiveness on anxiety symptoms was already reported in Schoneveld et al. (2018). For the current analyses, we started with the quadratic growth model of anxiety symptoms found in Schoneveld et al. (2018) and added the outcome predictors measured at pre-test (i.e., social, emotional, and academic self-efficacy) next to condition as predictors. In addition, we added maternal depression, anxiety and stress, and baseline anxiety levels as predictors. This was done separately for the model based on child-reported and mother-reported total anxiety symptoms.

To determine model fit, we used the Chi-square an *p* value, the Comparative Fit Index (CFI, critical value ≥ 0.95), Tucker Lewis Index (TLI, critical value ≥ 0.95) and the Root Mean Squared Estimate of Approximation (RMSEA, critical value ≤ 0.06) (Hu & Bentler 1999). The default maximum likelihood estimator was used. Attrition analyses were conducted but no systematic relationships were found between baseline covariates and missingness. The model was estimated using all available data.

RESULTS

Descriptive Statistics

Means and *SD*s of all study variables at all-time points separately for condition are shown in Table 1. Randomisation was successful. Details of the randomisation results are available elsewhere (see Schoneveld et al., 2018). In addition, no differences were found on the outcome predictors: baseline anxiety symptoms child report, baseline anxiety symptoms mother report, maternal depression, maternal anxiety, and maternal stress.

Bivariate correlations between secondary outcomes across time points are available in Table 2. The self-efficacy subscales were positively correlated at the same time and over time. Similarly, internalising and externalising symptoms were positively correlated within assessments and over time. In addition, Table 3 shows bivariate correlations of maternal mental health problems and anxiety symptoms. Child- and mother-reported anxiety were positively correlated at all time-points, except for child-reported anxiety at pre-test and mother-reported anxiety at 6-months follow-up. In addition, mother-reported anxiety was positively correlated with baseline maternal mental health problems at all time-points. Last, maternal depression, anxiety and stress were positively correlated at pre-test.

Table 1 Means, sums, standard deviations, minimum, maximum, skewness, and kurtosis of study variables separately for intervention programs and for time point

	MindLight						**CBT**					
Measure	***M / Sum***[a]	***SD***	**Min.**	**Max.**	**SK**	**KU**	***M / Sum***[a]	***SD***	**Min.**	**Max.**	**SK**	**KU**
	Pre-test											
Sex	50[b]	58.1[c]	-	-	-	-	53[b]	60.2[c]	-	-	-	-
Age	9.87	1.16	7.84	12.65	0.35	-0.62	10.07	1.16	7.85	12.80	0.33	-0.69
Anx. child	0.97	0.40	0.24	2.24	0.73	0.80	0.99	0.41	0.26	2.24	0.88	0.80
Anx. m.	0.51	0.26	0.03	1.20	0.54	0.05	0.50	0.19	0.16	1.05	0.66	0.13
Dep. mother	1.17	2.04	0.00	8.37	2.37	5.32	1.04	1.79	0.00	8.37	2.28	4.95
Anx. mother	0.54	1.12	0.00	5.47	2.69	7.68	0.57	0.99	0.00	5.47	2.37	7.31
Stress mother	3.13	3.09	0.00	12.51	0.91	0.29	3.10	3.00	0.00	12.51	1.08	1.08
Social SE	2.34	0.68	0.86	4.00	-0.11	-0.31	2.28	0.60	0.53	3.57	-0.12	0.15
Emotional SE	1.84	0.76	0.00	3.29	-0.18	-0.50	1.84	0.73	0.29	3.86	-0.22	0.01
Academic SE	2.43	0.74	0.43	4.00	-0.52	-0.07	2.28	0.63	1.00	3.71	-0.10	-0.40
Int. problems	4.58	3.27	0.00	13.00	0.70	-0.01	4.62	3.54	0.00	14.81	1.18	1.32
Ext. problems	5.42	3.47	0.00	15.00	0.35	-0.24	5.87	4.01	0.00	16.89	0.74	0.33
	Post-test											
Social SE	2.53	0.67	0.86	4.00	0.27	-0.22	2.49	0.66	0.57	4.00	-0.46	0.34
Emotional SE	2.10	0.73	0.43	4.00	0.58	0.42	2.18	0.76	0.00	4.00	-0.32	0.77
Academic SE	2.57	0.75	0.86	4.00	-0.15	-0.31	2.56	0.67	1.14	4.00	0.07	-0.40
Int. problems	3.48	3.18	0.00	12.00	1.04	0.13	3.70	3.16	0.00	13.14	0.87	0.11
Ext. problems	5.09	3.11	0.00	14.00	0.28	-0.33	4.79	3.33	0.00	15.08	1.12	1.72
	3-months follow-up											
Social SE	2.67	0.71	0.51	4.00	-0.57	0.54	2.58	0.66	0.51	4.00	-0.49	0.60
Emotional SE	2.25	0.86	0.57	4.00	0.12	-0.59	2.32	0.73	0.57	4.00	0.06	-0.41
Academic SE	2.72	0.78	0.43	4.00	-0.63	0.48	2.60	0.71	0.71	4.00	-0.31	-0.39
Int. problems	3.65	3.43	0.00	13.00	1.12	0.23	3.33	3.01	0.00	13.00	1.11	1.14

	MindLight						CBT					
Measure	***M / Sum***[a]	***SD***	**Min.**	**Max.**	**SK**	**KU**	***M / Sum***[a]	***SD***	**Min.**	**Max.**	**SK**	**KU**
Ext. problems	5.09	3.20	0.00	14.00	0.47	0.14	5.07	3.75	0.00	15.54	0.95	0.61
	6-months follow-up											
Social SE	2.73	0.67	1.29	4.00	-0.22	-0.33	2.63	0.58	1.43	4.00	0.18	-0.48
Emotional SE	2.37	0.86	0.29	4.00	-0.21	-0.24	2.33	0.79	0.14	4.00	-0.29	0.10
Academic SE	2.67	0.75	0.71	4.00	-0.15	-0.40	2.62	0.68	1.00	4.00	-0.10	-0.55
Int. problems	3.18	3.14	0.00	11.93	1.31	1.16	3.27	2.62	0.00	10.00	0.80	-0.27
Ext. problems	4.75	3.33	0.00	15.00	0.50	0.09	4.88	3.53	0.00	15.24	0.80	0.75

Notes. Min. = minimum; Max. = maximum; SK = skewness; KU = kurtosis; Anx. = anxiety; m. = mother; Dep. = depression; SE = self-efficacy; Int. = internalising; Ext. = externalising.
[a] Internalising and externalising problems are sum scores; [b] Not M, but n girls; [c] Not SD, but %.

Table 2 Bivariate correlations of secondary outcomes across time points

Measure	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**	**15**	**16**	**17**	**18**	**19**
Pre-test																			
1. SE soc.	-																		
2. SE emo.	**.51**	-																	
3. SE aca.	**.31**	**.39**	-																
4. Int.	**-.18**	-.14	-.06	-															
5. Ext.	-.10	-.09	**-.36**	**.48**	-														
Post-test																			
6. SE soc.	**.55**	**.38**	**.40**	-.09	-.03	-													
7. SE emo.	**.41**	**.52**	**.41**	-.09	-.06	**.60**	-												
8. SE aca.	**.33**	**.27**	**.69**	-.07	**-.22**	**.58**	**.56**	-											
9. Int.	**-.21**	**-.18**	-.15	**.72**	**.41**	**-.18**	**-.20**	-.13	-										
10. Ext.	-.06	-.12	**-.27**	**.46**	**.81**	.02	-.06	**-.22**	**.47**	-									
3-months FU																			
11. SE soc.	**.57**	**.36**	**.39**	-.14	.03	**.67**	**.47**	**.37**	**-.21**	.00	-								
12. SE emo.	**.32**	**.54**	**.35**	-.08	.16	**.55**	**.69**	**.47**	-.14	.04	**.65**	-							
13. SE aca.	**.28**	**.24**	**.64**	-.05	-.15	**.48**	**.51**	**.72**	-.10	**-.18**	**.60**	**.61**	-						
14. Int.	**-.18**	**-.20**	-.11	**-.75**	**.40**	-.11	-.10	-.08	**.76**	**.47**	**-.21**	**-.18**	-.06	-					
15. Ext.	-.07	-.11	**-.29**	**.41**	**.81**	.00	-.01	-.18	**.42**	**.84**	.01	.07	-.15	**.46**	-				
6-months FU																			
16. SE soc.	**.46**	**.23**	**.20**	-.11	.05	**.62**	**.39**	**.27**	-.13	.02	**.72**	**.53**	**.36**	**-.18**	-.07	-			
17. SE emo.	**.27**	**.41**	**.17**	-.03	.05	**.48**	**.59**	**.40**	-.08	.04	**.41**	**.70**	**.35**	-.09	.02	**.58**	-		
18. SE aca.	**.21**	.15	**.63**	.01	**-.18**	**.44**	**.40**	**.72**	-.04	-.17	**.44**	**.50**	**.73**	-.05	**-.18**	**.44**	**.52**	-	
19. Int.	-.17	**-.22**	-.04	**.57**	**.32**	-.07	-.07	.04	**.67**	**.40**	**-.20**	-.16	.01	**.76**	**.40**	**-.21**	.07	.00	-
20. Ext.	-.01	-.10	**-.28**	**.29**	**.76**	.00	.03	**-.21**	**.33**	**.77**	.02	.10	-.18	**.40**	**.85**	.08	.08	**-.22**	**.36**

Notes. Correlations in bold are significant with at least $p < .05$. SE soc. = social self-efficacy; SE emo. = emotional self-efficacy; SE aca. = academic self-efficacy; Int. = internalising problems; Ext. = externalising problems.

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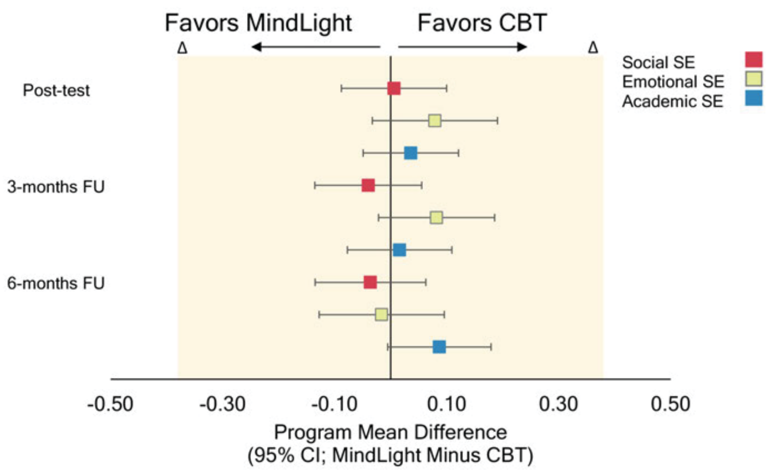
Table 3 Bivariate correlations of (other) predictors and anxiety symptoms across time points

Measure	1	2	3	4	5	6	7	8	9	10
Pre-test										
1. Anxiety (child report)	-									
2. Anxiety (mother report)	**.20**	-								
3. Depression mother	-.01	**.26**	-							
4. Anxiety mother	.05	**.22**	**.46**	-						
5. Stress mother	.01	**.31**	**.67**	**.46**	-					
Post-test										
6. Anxiety (child report)	**.61**	**.25**	.08	.10	-.01	-				
7. Anxiety (mother report)	**.18**	**.69**	**.18**	**.24**	**.23**	**.29**	-			
3-months FU										
8. Anxiety (child report)	**.50**	**.20**	.05	.04	-.03	**.72**	**.25**	-		
9. Anxiety (mother report)	**.25**	**.69**	**.19**	**.24**	**.27**	**.37**	**.79**	**.39**	-	
6-months FU										
10. Anxiety (child report)	**.44**	**.20**	-.04	.08	-.04	**.67**	**.19**	**.83**	**.33**	-
11. Anxiety (mother report)	.14	**.50**	.14	**.28**	**.22**	**.24**	**.66**	**.30**	**.76**	**.25**

Note. Correlations in bold are significant with at least $p < .05$.

Figure 2 Differences between programs in self-efficacy (SE), in relation to non-Inferiority



Improvement in Mental Health Outcomes over Time

Table 4 presents the change in secondary outcomes and 95% CIs for both intervention programs over the course of the study. It shows that non-inferiority of MindLight to CBT could be demonstrated at post-test, 3-months follow-up and 6-months follow-up for social self-efficacy, emotional self-efficacy, academic self-efficacy and internalising problems. For externalising problems, non-inferiority could only be shown at 3-months follow-up. At post-test and 6-months follow-up, the CI lay entirely to the right of zero, indicating significant differences in favour of CBT. The results are visualized in Figure 2 (self-efficacy), Figure 3 (internalising problems), and Figure 4 (externalising problems). Results from these analyses in the completers only sample were similar and are available in Appendix A.

To define the growth function that best reflected children's change in social, emotional and academic self-efficacy, internalising problems, and externalising problems, we first fitted a linear growth model with intercept (*i*) and linear slope (*s*) as latent variables for all secondary outcomes separately in the intention-to-treat (ITT) sample. Most model fit indices showed acceptable fit (Appendix B), especially given our small sample size (Chen et al., 2008). The intercept and linear slope component were significant for all secondary outcomes (Table 5). This indicated that (1) children differed in their initial levels of social, emotional and academic self-efficacy, and internalising and externalising problems, (2) that their levels of social, emotional and academic self-efficacy increased (positive slope B), and that their levels of internalising and externalising problems decreased (negative slope B) significantly over time. The absence of significant variances of the slope reflects the idea that most children in our sample did change in a similar manner. Second, we added condition in the linear growth function. As expected, we did not find any differences in initial levels and rates of change of the secondary outcomes between conditions (Appendix C). Figure 5a shows the increase in emotional self-efficacy and Figure 5b shows the decrease in internalising and externalising problems separate by condition. The patterns in the other self-efficacy models were similar to the one presented in panel a. Results from the LGCM of the secondary outcomes in the completers only sample were similar and are available in Appendix D.

Table 4 Change in secondary outcomes over the study (intention-to-treat sample)

Assessment	MindLight	CBT	Mean difference[a]	*SD*	95 % CI
Social self-efficacy					
Post-test – pre-test[b]	0.19	0.20	0.01	0.62	[-0.09, 0.10] [d]
3-months FU – pre-test[b]	0.33	0.29	-0.04	0.63	[-0.14, 0.06] [d]
6-months FU – pre-test[b]	0.38	0.34	-0.04	0.65	[-0.13, 0.06] [d]
n	82	83			
Emotional self-efficacy					
Post-test – pre-test [b]	0.26	0.34	0.08	0.73	[-0.03, 0.19] [d]
3-months FU – pre-test [b]	0.40	0.49	0.08	0.68	[-0.02, 0.19] [d]
6-months FU – pre-test [b]	0.54	0.52	-0.02	0.73	[-0.13, 0.10] [d]
n	82	83			
Academic self-efficacy					
Post-test – pre-test [b]	0.18	0.22	0.04	0.56	[-0.05, 0.12] [d]
3-months FU – pre-test [b]	0.31	0.33	0.02	0.61	[-0.08, 0.11] [d]
6-months FU – pre-test [b]	0.24	0.33	0.09	0.60	[-0.01, 0.18] [d]
n	79	78			
Internalising problems					
Post-test – pre-test [c]	-1.28	-1.03	-0.25	2.47	[-0.63, 0.13] [d]
3-months FU – pre-test [c]	-1.32	-1.46	0.15	2.37	[-0.22, 0.51] [d]
6-months FU – pre-test [c]	-1.47	-1.39	-0.08	2.81	[-0.51, 0.35] [d]
n	67	66			
Externalising problems					
Post-test – pre-test [c]	-0.49	-1.06	0.57	2.17	[0.24, 0.90] [e]
3-months FU – pre-test [c]	-0.62	-0.79	0.17	2.22	[-0.17, 0.51] [d]
6-months FU – pre-test [c]	-0.62	-1.02	0.40	2.43	[0.03, 0.77] [e]
n	67	66			

Notes. CI = confidence interval.
[a] A negative difference is a difference in favour of MindLight. [b] A positive score means an increase in self-efficacy. [c] A negative score means a decrease in problems. [d] The 95 % CI of the difference in change in secondary outcome lies entirely between the equivalence margins, indicating equivalence of MindLight and CBT. [e] The 95 % CI of the difference in change in secondary outcome lies entirely to the right of zero, indicating significant differences in favour of CBT.

Figure 3 Differences between programs in internalising symptoms, in relation to non-inferiority

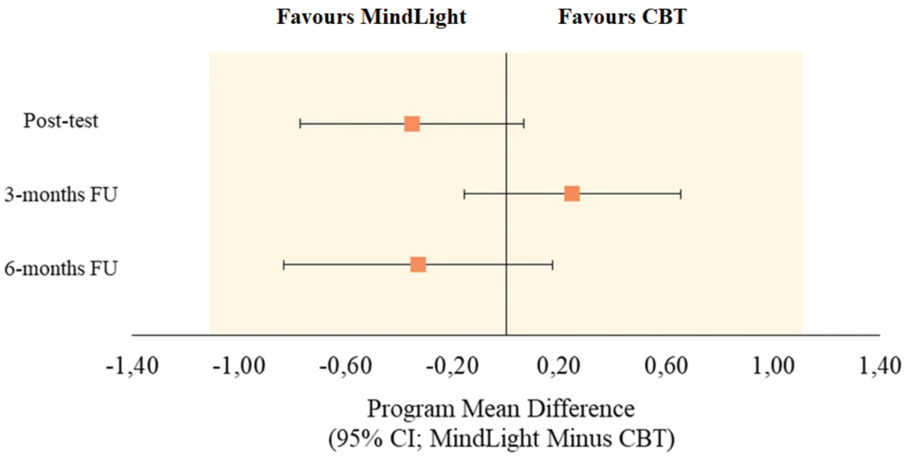


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Figure 4 Differences between programs in externalising symptoms, in relation to non-inferiority

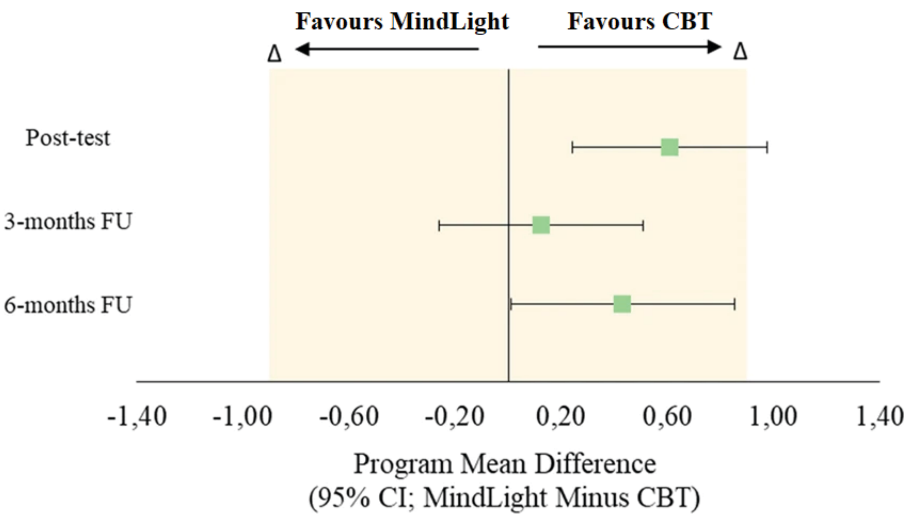
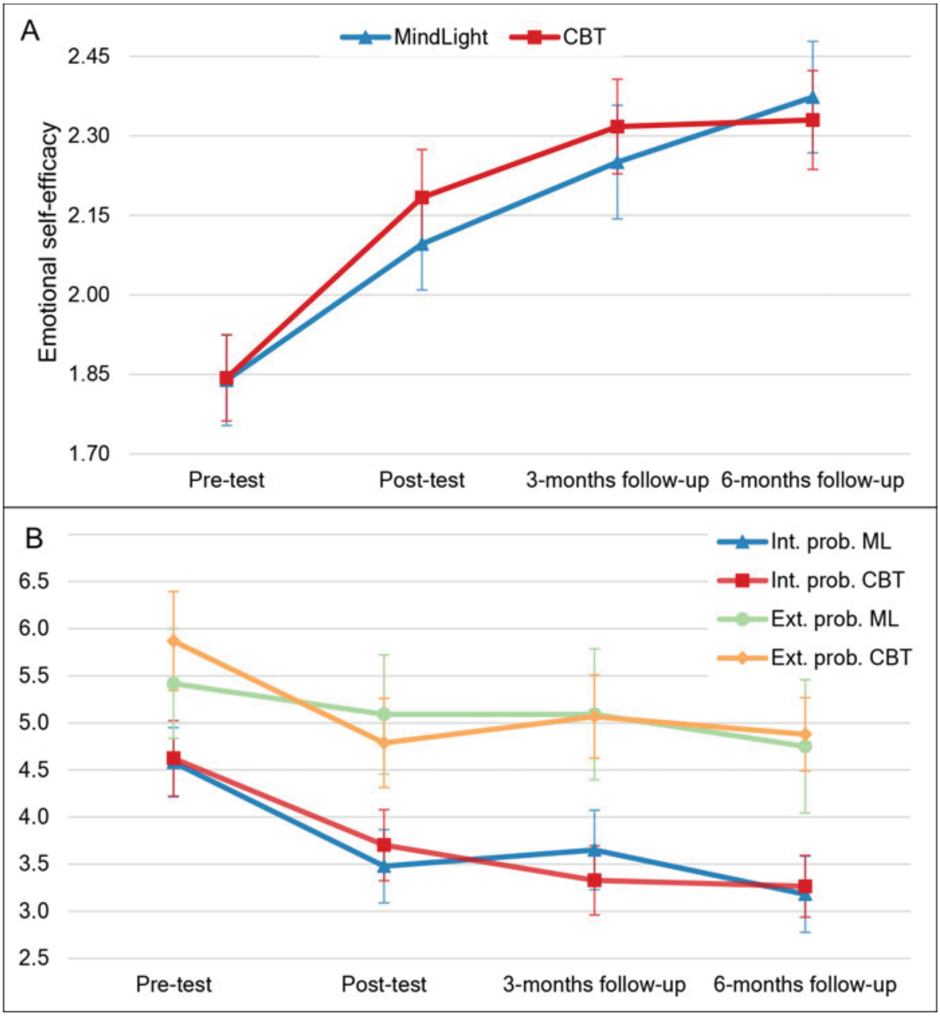


Figure 5 Emotional self-efficacy (in panel **a**), internalising problems (int. prob.; in panel **b**) and externalising problems (ext. prob.; in panel **b**) across time by program: ML (MindLight) and CBT.



Note: Error bars are standard errors.

For Whom are MindLight and CBT effective?

As described above, we started with the quadratic growth model of anxiety symptoms found in Schoneveld et al. (2018). This model showed that both child- and mother-reported anxiety symptoms decreased quadratically over time (i.e., the rate of the decrease slowed over time) in both the MindLight and CBT group. In the current study, we added social, emotional and academic self-efficacy, levels of maternal depression, anxiety and stress, and baseline anxiety separately as outcome predictors to the quadratic model. None of these predicted the linear nor the quadratic slope component for both child- and mother-reported anxiety symptoms (Table 6). However, social, emotional and academic self-efficacy, and mother-reported baseline anxiety were associated with the intercept of the quadratic growth model of child-reported anxiety. Specifically, children with higher levels of social, emotional and/or academic self-efficacy, and/or lower levels of mother-reported baseline anxiety showed lower initial levels of child-reported anxiety. In addition, levels of maternal depression, anxiety and stress predicted the intercept of the quadratic growth model of mother-reported anxiety: children from mothers with lower levels of depression, anxiety and stress, showed lower initial levels of mother-reported anxiety. Results from the LGCM of the outcome prediction analyses in the completers only sample are similar and available in Appendix E.

DISCUSSION

This study reported secondary outcomes and outcome predictors of effectiveness of a non-inferiority RCT comparing the applied game MindLight with a CBT-program (Coping Cat; Van Starrenburg et al., 2017) for childhood anxiety prevention. Findings were reported for post-intervention, 3- and 6-months follow-up using child- and mother-reports.

Improvement in Mental Health Outcomes

The first set of hypotheses were supported in that children in both intervention groups showed improvements in internalising and externalising problems, and self-efficacy. The CI approach showed affirmatively that MindLight was non-inferior to CBT over the course of the study for social self-efficacy, emotional self-efficacy, academic self-efficacy and internalising problems. CBT, however, showed a larger decrease in externalising problems at post-test and 6-months follow-up. Importantly, improvements in secondary outcomes were sustained up to 6 months after intervention completion. Thus, a relatively short intervention, delivered as a game or face-to-face CBT for 8 weeks, seems to have a significant and promising impact on more than the targeted anxiety.

The effects on internalising problems are consistent with prior findings that interventions targeting anxiety also effectively reduce internalising problems (Donovan & March 2014) and emotional symptoms (Morgan et al., 2016). The decrease in externalising problems corresponds with research showing that anxiety and externalising problems are strongly associated (Priddis et al., 2014). Last, self-efficacy increased in children participating in our study. This finding is in line with improvements in self-efficacy found in school-refusing children (King et al., 1998) and youth with anxiety disorders (Suveg et al., 2009) following a CBT-program, and now extends these results to applied games. The non-inferiority of MindLight to CBT on internalising symptoms and self-efficacy suggests that MindLight is as effective as CBT in improving these mental health outcomes. However, CBT was more effective in decreasing externalising symptoms than MindLight. An explanation could be the (minimal) involvement of parents in CBT, which is recommended in treatment guidelines for externalising problem behaviour (Buitelaar et al., 2013). In sum, these results may suggest that MindLight, an applied game, is as effective as a conventional expert-led group-based CBT prevention program in enhancing self-efficacy and internalising symptoms beyond reductions in anxiety.

For Whom are MindLight and CBT effective?

The second set of hypotheses were not supported: none of the mental health variables predicted interventions' effectiveness in preventing anxiety symptoms over time. First, we found no differences in the rate of change in anxiety symptoms for children with relatively lower or higher levels of baseline anxiety. This is in contrast to prior research that has demonstrated that anxiety symptoms at baseline could impact the response to CBT positively (Van Starrenburg et al., 2017) or adversely (Cunningham et al., 2016). Our results seem to suggest that the responsiveness to MindLight and CBT has little to do with the severity of presenting problems children began with. In addition, levels of children's self-efficacy did not predict interventions' effectiveness. A possible explanation is the rather low variance in self-efficacy scores in our sample. Most children rated themselves at the middle "not good, not bad" end of the scale, perhaps because we selected children from the general population. Other prevention studies (e.g., Tak et al., 2014) have also found rather low variance on self-efficacy. This restricted range precludes detecting effects.

Table 5 Linear growth model growth curve parameters for secondary outcomes, from pre-test to 6-months follow-up (intention-to-treat sample)

	Means						Variances					
	Intercept			Slope			Intercept			Slope		
	B	*SE*	*t*	B	*SE*	*t*	B	*SE*	*t*	B	*SE*	*t*
Social self-efficacy	2.37	0.05	47.37***	0.50	0.08	6.33***	0.26	0.05	5.28***	0.31	0.18	1.69
Emotional self-efficacy	1.92	0.06	33.65***	0.74	0.10	7.54***	0.31	0.06	4.87***	0.42	0.29	1.47
Academic self-efficacy	2.42	0.05	45.68***	0.40	0.08	5.39***	0.33	0.05	6.30***	0.12	0.17	0.72
Internalising problems	4.28	0.27	15.73***	-1.84	0.36	-5.11***	8.98	1.32	6.81***	6.28	3.63	1.73
Externalising problems	5.44	0.28	19.41***	-0.94	0.31	-3.08**	10.35	1.66	6.23***	2.13	2.26	0.95

Notes. SE = standard error.
*** $p < .001$; ** $p < .01$, two-tailed tests.

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Table 6 Linear regression predicting growth parameters of quadratic growth model of anxiety by mental health predictors at pre-test separate for child-reported anxiety and mother-reported anxiety (intention-to-treat sample)

	Outcome								
	Intercept anxiety			**Linear slope anxiety**			**Quadratic slope anxiety**		
Predictor	*B*	*SE*	*t*	*B*	*SE*	*t*	*B*	*SE*	*t*
Anxiety (child report)									
Baseline anxiety (mother report)	0.30	0.15	2.03*	0.19	0.58	0.33	-0.20	0.74	-0.28
Social self-efficacy	-0.17	0.05	-3.39**	0.01	0.25	0.04	0.13	0.31	0.41
Emotional self-efficacy	-0.23	0.04	-5.91***	-0.05	0.20	-0.25	0.27	0.26	1.07
Academic self-efficacy	-0.21	0.05	-4.24***	0.04	0.21	0.19	0.23	0.25	0.92
Depression mother	-0.01	0.02	-0.47	0.11	0.07	1.55	-0.17	0.09	-1.96
Anxiety mother	0.01	0.03	0.35	0.00	0.13	0.02	0.01	0.16	0.08
Stress mother	-0.00	0.01	-0.20	-0.00	0.05	-0.01	-0.01	0.06	-0.10
Anxiety (mother report)									
Baseline anxiety (child report)	0.08	0.05	1.62	0.11	0.18	0.64	-0.11	0.23	-0.49
Social self-efficacy	-0.02	0.03	-0.47	-0.09	0.10	-0.84	-0.04	0.13	-0.27
Emotional self-efficacy	-0.03	0.03	-1.07	-0.12	0.08	-1.48	0.09	0.11	0.83
Academic self-efficacy	-0.02	0.03	-0.61	-0.14	0.10	-1.37	0.18	0.13	1.33
Depression mother	0.03	0.01	2.86**	-0.06	0.03	-1.86	0.06	0.04	1.59
Anxiety mother	0.05	0.02	2.85**	-0.04	0.06	-0.60	0.03	0.08	0.35
Stress mother	0.02	0.01	3.96***	-0.04	0.02	-1.65	0.03	0.03	1.30

Notes. SE = standard error.
*** $p < .001$; ** $p < .01$; * $p < .05$, two-tailed tests.

Next to child factors, we also examined maternal mental health (i.e., maternal depressive, anxiety and stress symptoms) as possible outcome predictors. In line with our hypothesis, maternal mental health problems did not affect changes in children's anxiety symptoms over time. It is important to note, however, that the prevalence of maternal mental health problems was relatively low in our sample. Considering the important role of maternal mental well-being in children's development (Goodman et al., 2011; Powdthavee & Vignoles 2008) more research may be needed. For example, it may be interesting to select children for a prevention program based on whether their mother is highly stressed versus not and examine differential effects.

Although anxiety symptoms at baseline, self-efficacy and maternal mental health did not predict the change in anxiety symptoms, there were some interesting associations between these mental health variables and initial levels of anxiety symptoms that warrant further discussion. First, as expected, children that rated themselves as more anxious at baseline also reported lower levels of social, emotional and academic self-efficacy. Second, initial levels of child-reported anxiety were predicted by mother-reported children's anxiety at baseline. This result basically represents cross-informant agreement of baseline anxiety levels, which is generally significant but low (De Los Reyes et al., 2015). Last, higher initial levels of mother-reported children's anxiety were predicted by higher maternal depression, anxiety and stress symptoms. This finding suggests that mothers with mental health problems might have a biased perception of their child's emotional well-being (Briggs-Gowan et al., 1996). That is that mothers' rating of their children's anxiety is not so much a reflection of children's actual anxiety level but reflects their own struggles with depression, anxiety and/or stress. In sum, these results suggest that MindLight and CBT can enhance mental health despite difficulties children or their parents may have.

Strengths, Limitations and Future Research

The current study used a non-inferiority randomised controlled design to assess the effectiveness of an applied game against the standard of anxiety prevention (i.e., CBT). More specifically, we extended previous work about the effectiveness of MindLight on anxiety symptoms by assessing effects on secondary outcomes and potential outcome predictors, factors that are often neglected in previous research. In addition to the broader range of outcomes, we extended previous studies on anxiety prevention programs by including children's as well as maternal reports. The result that internalising and externalising problems decreased according to both children and mothers seems to imply that skills transferred from the program context to everyday life, where mothers were able to observe their children's behaviour. Last, we addressed a limitation of applied games research – a focus on short-term outcomes – by assessing children and mothers directly, 3 and 6 months after the program. These follow-ups give insight into the immediate, short-term but also longer-term effects of anxiety prevention programs, which informs implementation research. Programs with only short-term benefits might be less favourable than programs that show sustained beneficial effects.

Apart from these strengths, this study has several limitations that need to be addressed in the future. First, the internal consistency statistics for the social self-efficacy subscale were in a questionable range, which may have affected the results related to social self-efficacy. However, when the pattern of these results is compared with the results of the remaining types of self-efficacy, no differences appear. Second, the sample consisted of relatively well-functioning children. Whether the current results hold in a more distressed sample is a question for future research. Third, given the absence of a waitlist control group, passage of time could not be eliminated as an alternative explanation for the change in mental health outcomes.

In addition, future studies might want to investigate program effects on other important (secondary) outcomes. For example, as anxiety problems are related to worse academic performance (Nail et al., 2015), academic functioning indicators such as grades and problem solving skills could be relevant. Furthermore, assessing the effect of anxiety prevention programs on children's social skills might be another important avenue for future research. Social skills start to develop in childhood, are affected by anxiety issues, and at the same time are an important contributing factor in the maintenance and further aggravation of mental health problems (Clarke et al., 2015). Prior research has shown that increasing children's social skills helps to reduce their behavioural and emotional difficulties (Humphrey et al., 2010).

Next to investigating outcomes, future studies may want to extend the range of possible predictors of effectiveness. An important general predictor of program effectiveness is children's motivation to change. Children entering the program more motivated might show a larger decrease in anxiety symptoms than children who are less motivated (Dean et al., 2016). In addition, choice could be a moderator of program effectiveness. Given the positive effect of choice and autonomy on intrinsic motivation (Ryan & Deci 2000), children who choose themselves which program to attend might respond more positively than children who are randomly assigned.

Furthermore, the current study leaves open the question about mechanisms of change. Possible psychological mechanisms worth of investigation in future studies are increased active coping skills (Thorne et al., 2013) and decreased maladaptive cognitions (Hogendoorn et al., 2014). Ideally, these mediators should be assessed repeatedly over the course of the intervention, to investigate whether changes in these mediators contribute to decreases in anxiety (Selig & Preacher 2009). In addition, studying mediators of change in applied games versus conventional prevention programs (i.e., moderated mediation) could be an interesting future direction. MindLight focuses more on relaxation whereas CBT centers on changing cognitions. Thus, contrasting the relative contribution of changes in relaxation and cognitions in anxiety reduction will give insight into program specific mechanisms of change but will also contribute to a broader understanding of what is actually driving changes in anxiety.

Notwithstanding the remaining questions for future research, the findings of the current study and our previous work (Schoneveld et al., 2016, 2018) suggest that MindLight could be implemented as an indicated prevention program in schools to reduce anxiety symptoms, internalising and externalising problems, and to improve self-efficacy in children with elevated levels of anxiety, regardless of the potential mental health problems or struggles that children or their parents may have. An applied game as MindLight might facilitate cost-effectiveness because no clinicians or teachers need to be involved and there is no specific training required to guide children through the game. Adding MindLight to the list of other prevention programs that already take place in the school context may provide children an alternatively delivered prevention program at school that is potentially less stigmatizing, more accessible, and thereby increasing adherence and their motivation to participate in such programs. Future research on the implementation and cost-effectiveness of MindLight in the school context is recommended.

APPENDIX A

Pre-test and change in secondary outcomes over the study (completers only sample).

Assessment	**MindLight**	**CBT**	**Mean difference**[a]	***SD***	**95 % CI**
Social self-efficacy					
Pre-test	2.34	2.28			
Post-test – pre-test[b]	0.21	0.22	0.01	0.62	[-0.10, 0.11] [d]
3-months FU – pre-test[b]	0.31	0.31	-0.01	0.62	[-0.11, 0.10] [d]
6-months FU – pre-test[b]	0.39	0.34	-0.05	0.65	[-0.16, 0.06] [d]
n	82	83			
Emotional self-efficacy					
Pre-test	1.84	1.84			
Post-test – pre-test [b]	0.26	0.36	0.10	0.73	[-0.03, 0.22] [d]
3-months FU – pre-test [b]	0.41	0.50	0.09	0.73	[-0.03, 0.22] [d]
6-months FU – pre-test [b]	0.55	0.52	-0.03	0.84	[-0.17, 0.11] [d]
n	82	83			
Academic self-efficacy					
Pre-test	2.43	2.28			
Post-test – pre-test [b]	0.19	0.26	0.08	0.55	[-0.02, 0.17] [d]
3-months FU – pre-test [b]	0.31	0.34	0.03	0.61	[-0.07, 0.14] [d]
6-months FU – pre-test [b]	0.24	0.36	0.12	0.59	[0.01, 0.22] [e]
n	79	78			
Internalising problems					
Pre-test	4.58	4.62			
Post-test – pre-test [c]	-1.28	-0.93	-0.35	2.46	[-0.77, 0.07] [d]
3-months FU – pre-test [c]	-1.31	-1.56	0.25	2.34	[-0.16, 0.65] [d]
6-months FU – pre-test [c]	-1.68	-1.35	-0.33	2.83	[-0.83, 0.18] [d]
n	67	66			
Externalising problems					
Pre-test	5.42	5.87			
Post-test – pre-test [c]	-0.48	-1.09	0.61	2.17	[0.24, 0.97] [f]
3-months FU – pre-test [c]	-0.63	-0.76	0.12	2.23	[-0.26, 0.51] [d]
6-months FU – pre-test [c]	-0.63	-1.06	0.43	2.37	[0.01, 0.85] [f]
n	67	66			

Notes. CI = confidence interval.
[a] A negative difference is a difference in favour of MindLight. [b] A positive score means an increase in self-efficacy. [c] A negative score means a decrease in problems. [d] The 95 % CI of the difference in change in secondary outcome lies entirely between the equivalence margins, indicating equivalence of MindLight and CBT. [e] The 95 % CI of the difference in change in secondary outcome lies entirely to the left of zero, indicating significant differences in favour of MindLight. [f] The 95 % CI of the difference in change in secondary outcome lies entirely to the right of zero, indicating significant differences in favour of CBT.

APPENDIX B

Linear growth model fit indices for secondary outcomes, from pre-test to 6-months follow-up (intention-to-treat sample).

	χ^2 *(df)*	***p***	**RMSEA**	**CFI**	**TLI**
Social self-efficacy	11.29 (5)	.046	0.09	0.97	0.97
Emotional self-efficacy	13.73 (5)	.017	0.10	0.96	0.95
Academic self-efficacy	15.60 (5)	.008	0.11	0.96	0.95
Internalising problems	22.09 (5)	<.001	0.14	0.94	0.93
Externalising problems	12.61 (5)	.027	0.10	0.98	0.98

Notes: RMSEA = root mean square error of approximation; CFI = comparative fit index; TLI = Tucker-Lewis Index.

APPENDIX C

Linear model fit indices and linear regression predicting growth parameters of linear growth model of secondary outcomes by program, from pre-test to 6-months follow-up (Intention-to-Treat Sample).

						Outcome					
						Intercept			Linear slope		
	$\chi^2(df)$	*p*	RMSEA	CFI	TLI	B	*SE*	*t*	B	*SE*	*t*
Social self-efficacy	11.82 (7)	.107	0.07	0.98	0.97	0.04	0.098	0.36	0.06	0.151	0.42
Emotional self-efficacy	15.08 (7)	.035	0.08	0.96	0.95	-0.05	0.114	-0.43	0.05	0.198	0.23
Academic self-efficacy	16.33 (7)	.022	0.09	0.97	0.95	0.13	0.105	1.25	-0.11	0.141	-0.80
Internalising problems	23.89 (7)	.001	0.12	0.95	0.93	-0.05	0.534	-0.09	0.04	0.691	0.06
Externalising problems	15.66 (7)	.028	0.09	0.98	0.97	-0.09	0.557	-0.17	0.26	0.599	0.44

Notes. *** $p < .001$; ** $p < .01$, two-tailed tests. RMSEA = root mean square error of approximation; CFI = comparative fit index; TLI = Tucker-Lewis Index; SE = standard error.

APPENDIX D

Linear growth model growth curve parameters for secondary outcomes, from pre-test to 6-months follow-up (completers only sample).

	Means						Variances					
	Intercept			Slope			Intercept			Slope		
	B	*SE*	*t*	B	*SE*	*t*	B	*SE*	*t*	B	*SE*	*t*
Social self-efficacy	2.36	0.05	48.86***	0.50	0.08	6.44***	0.26	0.05	4.87***	0.33	0.19	1.80
Emotional self-efficacy	1.92	0.06	33.99***	0.73	0.10	7.07***	0.30	0.07	4.52***	0.49	0.33	1.48
Academic self-efficacy	2.41	0.05	45.68***	0.42	0.08	5.45***	0.34	0.05	6.42***	0.18	0.18	0.99
Internalising problems	4.36	0.27	16.06***	-2.00	0.35	-5.63***	8.96	1.37	6.54***	7.07	4.00	1.77
Externalising problems	5.43	0.28	19.10***	-1.00	0.31	-3.28**	10.35	1.75	5.90***	2.44	2.33	1.05

Notes. SE = standard error.
*** $p < .001$; ** $p < .01$, two-tailed tests.

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APPENDIX E

Linear regression predicting growth parameters of quadratic growth model of anxiety by mental health predictors at pre-test separate for child-reported anxiety and mother-reported anxiety (completers only sample).

	Outcome								
	Intercept anxiety			Linear slope anxiety			Quadratic slope anxiety		
Predictor	***B***	***SE***	***t***	***B***	***SE***	***t***	***B***	***SE***	***t***
Anxiety (child report)									
Baseline anxiety (mother report)	0.35	0.14	2.52*	0.01	0.54	0.01	0.09	0.67	0.13
Social self-efficacy	-0.17	0.05	-3.34**	0.08	0.26	0.31	0.07	0.32	0.22
Emotional self-efficacy	-0.23	0.04	-5.79***	-0.02	0.20	-0.08	0.23	0.26	0.88
Academic self-efficacy	-0.22	0.05	-4.47***	0.09	0.22	0.41	0.17	0.26	0.64
Depression mother	-0.00	0.02	-0.06	0.10	0.07	1.35	-0.16	0.08	-1.91
Anxiety mother	0.02	0.03	0.75	-0.01	0.13	-0.10	0.03	0.15	0.23
Stress mother	0.00	0.01	0.11	-0.01	0.05	-0.25	0.01	0.06	0.14
Anxiety (mother report)									
Baseline anxiety (child report)	0.09	0.05	1.78	0.06	0.18	0.32	-0.06	0.24	-0.23
Social self-efficacy	-0.02	0.03	-0.48	-0.06	0.11	-0.59	-0.08	0.15	-0.53
Emotional self-efficacy	-0.03	0.03	-1.03	-0.13	0.09	-1.48	0.11	0.12	0.89
Academic self-efficacy	-0.02	0.03	-0.79	-0.14	0.11	-1.25	0.19	0.16	1.19
Depression mother	0.03	0.01	2.57*	-0.05	0.03	-1.66	0.05	0.04	1.40
Anxiety mother	0.05	0.02	2.73**	-0.01	0.06	-0.21	0.01	0.07	0.09
Stress mother	0.02	0.01	3.64***	-0.03	0.02	-1.25	0.02	0.03	0.88

Notes. SE = standard error.

*** $p < .001$; ** $p < .01$; * $p < .05$, two-tailed tests.

Chapter 5

The effect of expectations on experiences and engagement with an applied game for mental health

Based on:
Wols, A., Hollenstein, T., Lichtwarck-Aschoff, A., & Granic, I. (2021).
The effect of expectations on experiences and engagement with an applied game for mental health. Games for Health Journal, 10(4), 207-219.
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ABSTRACT

Applied games are considered a promising approach to deliver mental health interventions. Nonspecific factors such as expectations and motivation may be crucial to optimize effectiveness yet have not been examined so far. The current study examined the effect of expectations for improvement on (1) experienced fun and positive affect, and (2) in-game play behaviours while playing MindLight, an applied game shown to reduce anxiety. The secondary aim was to examine the moderating role of symptom severity and motivation to change. Fifty-seven participants (47 females; 17–21 years old) preselected on anxiety symptoms viewed a trailer in which MindLight was promoted as either a mental health or an entertainment game. These trailers were used to induce different expectations in participants. Participants subsequently played the game for 60 minutes. Before playing, participants filled out questionnaires about their general anxiety symptoms, motivation to change, state anxiety, affect, and arousal. While playing, in-game behaviours and galvanic skin response (GSR) were recorded continuously. After playing, state anxiety, affect, and arousal were measured again as well as experienced fun. Results showed that state anxiety, arousal and GSR increased for participants in both trailer conditions. Expectations did not influence experienced fun and positive affect, nor in-game behaviours. In addition, no moderation effects of motivation to change and symptom severity were found. Thus, experiences and engagement with MindLight were not influenced by expectations, motivation to change, and symptom severity. For future research, it is recommended to examine individual differences in these effects, and long-term and more distal outcomes and processes.

There has been an increasing interest in the use of applied games to treat and prevent mental health problems (e.g., Granic et al., 2014; Lau et al., 2017). Due to their intrinsically motivating features and their high accessibility and potential for scalability, applied games are considered a promising and cost-effective approach to improve access to mental health care (Granic et al., 2014; Kazdin, 2015). The primary focus in the development of applied games has been on translating evidence-based *specific* therapeutic techniques into game mechanics (Eichenberg & Schott, 2017; Fleming et al., 2017). These specific therapeutic techniques are drawn from theories about the working mechanisms responsible for the onset and maintenance of mental health disorders (e.g., relaxation and exposure training in cognitive behavioural therapy; Kendall, 2011). The underlying assumption is that these specific techniques are responsible for the observed improvements in mental health. There is, however, a consistent and large body of evidence showing that *nonspecific* factors – factors not specific to any psychotherapeutic school, such as individuals' expectations and motivation to change – actually outweigh the role of specific techniques in explaining positive intervention outcomes (e.g., Ahn & Wampold, 2001; Lambert, 2005, 2011; Messer & Wampold, 2002; Wampold, 2001). Additionally, nonspecific factors are associated with patients' engagement in the therapeutic process such as invested time and effort (Boettcher et al., 2013; Dean et al., 2016; Greenberg et al., 2006; Meyer et al., 2002) and adherence to the treatment regimen (e.g., homework assignments; Constantino et al., 2011; Westra et al., 2007). So far, nonspecific factors have largely been neglected in the e-health literature (Enck et al., 2017; Torous & Firth, 2016) and their effects remain unknown. In order to optimize the effectiveness of applied games to its best potential, it is crucial to examine and harness the benefits of nonspecific factors (Enck et al., 2013).

Presumably, the most relevant nonspecific factor to examine in applied games is individuals' expectations for improvement (Schakel et al., 2018; Schwarz et al., 2016). Previous research has shown that expectations drive a large majority of intervention effects (Greenberg et al., 2006), in particular in experimental game design studies (Boot et al., 2013). Although commercial video games are usually promoted for their entertainment value, applied games are often introduced with an explicit (mental) health aim, which naturally induces expectations for improvement. It is unknown how expectations relate to players' experiences of a game and their engagement with it. Therefore, the primary aims of the current study were to examine the effect of expectations for improvement on 1)experienced fun and affect, and 2) in-game play behaviours while playing an applied game for mental health.

The effect of expectations on players' experiences and in-game engagement may be moderated by two additional nonspecific factors, namely motivation to change (i.e., individuals' willingness to change symptoms or problems they are experiencing; Prochaska & DiClemente, 1982) and symptom severity. An individual more motivated to change mental health symptoms and/or experiencing more (severe) symptoms may find an applied game with an explicit mental health aim more personally relevant, possibly leading to higher engagement with the therapeutic techniques and a more positive experience of the game (i.e., experienced fun and affect; Buday, 2015; Dean et al., 2016; Oliver & Krakowiak, 2009). On the other hand, however, it might be that individuals with more (severe) symptoms engage less with the therapeutic techniques as they may fear unwanted confrontation with their mental health problems (e.g., a game aimed at emotion and stress management may imply confrontations with negative emotions and stress for some individuals; Poppelaars, Lichtwarck-Aschoff, et al., 2018). The secondary aim of the current study was to examine the moderating role of motivation to change and symptom severity.

Design of the Present Study

In the current study, we used the applied game MindLight, designed to reduce anxiety symptoms among youth (GainPlay Studio, 2014; PlayNice Institute, 2014). Previous research has compared MindLight to a cognitive behavioural therapy (CBT) based indicated prevention program in children (7 to 12 years old) (Schoneveld et al., 2018) and to online CBT-based psychoeducation in adolescents (8 to 16 years old) (Tsui et al., 2021), finding evidence for its overall effectiveness. Previous research also suggests that both specific and nonspecific factors (expectations and motivation) play a role in MindLight (Schoneveld et al., 2016; Wols et al., 2018).

Expectations for MindLight were experimentally manipulated by showing participants a teaser trailer, in which MindLight was promoted as a mental health game or as a regular entertainment game (Boot et al., 2013; Kazdin, 2005). The primary outcomes were experienced fun, positive affect, and in-game play behaviours. Because MindLight has been specifically designed to induce anxiety in order to train youth to regulate this anxiety (Schoneveld et al., 2018; Schoneveld et al., 2016), we also examined changes in (self-reported) state-anxiety and arousal. The study design, hypotheses and analyses were preregistered on the Open Science Framework (OSF; https://osf.io/6gmwv; Wols et al., 2019) and deviations from the planned methodology are uploaded on OSF (see https://osf.io/j7mvu/). Exploratory analyses were performed on changes in galvanic skin response (GSR; i.e., the small changes in the amount of moisture or perspiration on the surface of the skin), to have an objective indicator of arousal complementing the self-reported measures.

METHODS

Participants

Participants were 57 psychology students, who were between 17 and 21 years old, primarily Caucasian, and indicated to be moderately experienced with playing video games (see Table 1 for descriptives). All participants were preselected on elevated levels of anxiety (see preregistration). The Queen's University Health Sciences & Affiliated Teaching Hospitals Research Ethics Board (HSREB) granted ethics approval for the current study (code number: 6019310 PSYC-187-16).

Procedure

Participants signed informed consent and filled out a questionnaire measuring demographics, anxiety symptoms, and motivation to change. After that, participants viewed a neutral video to measure their baseline GSR (Biopac Systems Inc., 2007) and completed a questionnaire measuring their state-anxiety, affect and arousal. Next, participants viewed a mental health or an entertainment trailer and played MindLight for 60 minutes on a 15.6 inch laptop. After having played the game, participants again filled out questions about their state-anxiety, affect, and arousal, as well as questions about their experiences with the game and questions related to the manipulation checks.

MindLight and Experimental Manipulation

MindLight is a 3D neurofeedback game designed to reduce anxiety symptoms among youth (GainPlay Studio, 2014; PlayNice Institute, 2014). In the game, Little Arty (the player) needs to save his grandma who succumbed to evil forces. He finds a magical hat that teaches him (and the player) how to use his “*mindlight*”, a beam of light coming from the antenna attached to the magical hat. The *mindlight* is controlled via the one-channel dry-sensor electroencephalogram (EEG) headset that the player wears (Neurosky Inc., 2011) and which responds to the real-time relaxation of the player (neurofeedback training; Price & Budzynski, 2009): when the player becomes more relaxed, the light becomes brighter providing more light in the game environment, and making it possible to chase away or uncover “fear events” (exposure training; Feske & Chambless, 1995) and effectively engage with the puzzles (attention

Table 1 Means, frequencies and standard deviations or percentages of the demographic and study variables for the total sample and for each experimental condition

			Experimental condition						
			Mental health trailer		Entertainment trailer				
Variable	**Mean / Frequency**	**(*SD*) / Percentage**	**Mean / Frequency**	**(*SD*) / Percentage**	**Mean / Frequency**	**(*SD*) / Percentage**	χ^2 / t [a]	***df***	***p***
Age	18.23	(0.63)	18.21	(0.77)	18.25	(0.44)	1.05[b]	54	.30
Gender							0.13[c]	1	.72
Female	47	82.5%	24	82.8%	23	82.1%			
Male	9	15.8%	4	13.8%	5	17.9%			
Non-binary	1	1.8%	1	3.4%	0	0.0%			
Race							0.06[d]	1	.81
White	44	77.2%	22	75.9%	22	78.6%			
Asian	8	14.0%	5	17.2%	3	10.7%			
Arabic	1	1.8%	0	0.0%	1	3.6%			
Multi-racial	3	5.3%	1	3.4%	2	7.1%			
Prefer not to say	1	1.8%	1	3.4%	0	0.0%			
Video game experience [e]	4.46	(2.70)	4.93	(2.70)	3.96	(2.66)	-1.36	55	.18
Motivation to change	3.91	(0.45)	3.91	(0.42)	3.92	(0.49)	0.01	55	.99
General anxiety symptoms	1.55	(0.67)	1.54	(0.65)	1.56	(0.70)	0.08	55	.94
Experienced fun	4.18	(2.12)	4.71	(2.21)	3.64	(1.91)	-1.94	55	.06
Affect									
Pre-test [g]	3.67	(0.61)	3.62	(0.72)	3.73	(0.48)	0.70	55	.49
Post-test	2.92	(0.90)	2.98	(1.02)	2.86	(0.76)	-0.53	55	.60
Arousal									
Pre-test	2.10	(0.82)	1.91	(0.85)	2.29	(0.76)	1.74	55	.09

	Experimental condition								
			Mental health trailer		Entertainment trailer				
Variable	**Mean / Frequency**	**(*SD*) / Percentage**	**Mean / Frequency**	**(*SD*) / Percentage**	**Mean / Frequency**	**(*SD*) / Percentage**	χ^2 / t [a]	***df***	***p***
Post-test	2.61	(1.16)	2.57	(1.21)	2.64	(1.13)	0.24	55	.81
State anxiety									
Pre-test	1.88	(0.47)	1.88	(0.57)	1.87	(0.34)	-0.13	45.57[f]	.90
Post-test	2.26	(0.59)	2.26	(0.60)	2.27	(0.58)	0.12	55	.90
Engaged in-game play behaviours									
Mindlight - total [g]	.01	(.01)	.01	(.01)	.01	(.01)	-0.86	55	.39
Exploration	.65	(.10)	.65	(.10)	.65	(.11)	0.08	55	.94
Fear attempt	.05	(.03)	.05	(.03)	.05	(.04)	0.47	55	.64
Avoidant/safety in-game play behaviours									
Mindlight - none [g]	.04	(.05)	.04	(.06)	.03	(.04)	-0.93	55	.36
Inactive [g]	.04	(.03)	.03	(.03)	.04	(.03)	0.70	55	.49
Ceiling light attempt	.24	(.08)	.24	(.08)	.23	(.07)	-0.46	55	.65
Inside chest [g]	.02	(.01)	.02	(.02)	.02	(.02)	1.15	55	.25
Overall GSR Percent Change [g]	32.92	(51.24)	41.50	(62.36)	25.25	(38.25)	-1.16	51	.25

Notes. df, degrees of freedom; GSR, galvanic skin response; SD, standard deviation.
[a] Bootstrapping the independent t-tests with n = 1000 samples showed similar results for all variables. [b] For the t-test, one outlier was removed. [c] The χ^2-test included males and females (one non-binary participant was removed); 2 cells (50%) had an expected count less than 5 (minimum expected count was 4.50). [d] The χ^2-test included the categories "white" vs. "other". [e] On a 10-point scale ranging from "0 = not at all experienced" to "10 = expert". [f] Levene's test was significant and equal variances could not be assumed. [g] One value was winsorized to ± 3.5 standard deviation from the mean.

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bias modification; Bar Haim, 2010; Bar Haim et al., 2011) and other objects (e.g., unlock hiding spaces and turn on ceiling lights, which both prevent that fear events will attack the player). For more information, see previous studies on MindLight (Schoneveld et al., 2018; Schoneveld et al., 2016; Wijnhoven et al., 2015; Wijnhoven et al., 2020; Wols et al., 2018).

Half of the participants (n = 29) viewed a teaser trailer in which MindLight was promoted as a mental health game (MH-condition; https://osf.io/zdqs5/), emphasizing the beneficial effects of the game on players' emotion regulation and stress reduction. The other half of the participants (n = 28) viewed a teaser trailer in which MindLight was promoted as a regular entertainment game (ENT-condition; https://osf.io/jf4ab/). Although the trailers differed in their specific message, both trailers included the same video footage and background music, and lasted for 1 minute and 11 seconds.

Materials

Experienced Fun

Participants answered 'How much did you like playing MindLight?' on a 10-point scale.

Affect and Arousal

Participants indicated on two manikin based scales (ranging from 1 to 5) how they felt at that moment (Self-Assessment Manikin; SAM) (Bradley & Lang, 1994). Each manikin is a graphical depiction of various points along the affect/arousal dimension. For affect, the SAM ranged from an unhappy and frowning manikin (1) to a happy and smiling manikin (5). For arousal, the SAM ranged from a sleepy and relaxed manikin (1) to a wide-eyed and excited manikin (5).

In-game Play Behaviours

While playing MindLight, on-screen output was recorded using the Open Broadcast Software (https://obsproject.com). In-game play behaviours were coded in Noldus (Noldus Information Technology, 2013) following an adapted version of the *MindLight Coding System* based on Wols et al. (2018) (see preregistration). Reliability was maintained above 75% agreement and .65 kappa using a frequency/sequence-based analysis, and above 80% agreement using a duration/sequence-based analysis. The in-game play behaviours can be divided into *engaged* behaviours that support players' practice of relaxation, exposure, and modifying attention biases, and *avoidant/safety* behaviours that interfere with the intervention goals of MindLight and reduce opportunities to practice. Codes of interest included three engaged and four avoidant/safety behaviours. The frequency and duration of the in-game play behaviours were transformed to frequencies per minute and proportions respectively (for more details see preregistration).

State Anxiety

The state scale of the State-Trait Anxiety Inventory (STAI-S) (Spielberger, 1989; Spielberger et al., 1983) consists of 20 items (α_{pre} = .90 and α_{post} = .92) and asks participants to indicate how they feel at this moment (e.g., “I am tense”; on a 4-point scale).

Galvanic Skin Response (GSR)

During the baseline task (i.e., viewing a neutral video) and while playing MindLight, GSR was recorded continuously with Biopac AcqKnowledge 4.2 software (Biopac Systems Inc., 2011) and MP150 amplifier (Biopac Systems Inc., 2007). GSR data files were trimmed to 120s and 3600s for the baseline task and gameplay, respectively, and cleaned and processed using the AcqKnowledge software. No smoothing was applied to the data, but a low pass filter was used to improve the signal quality of the entire waveform (fixed frequency was set at 0.5 Hz). An overall GSR mean value for baseline and gameplay was calculated, as well as GSR mean values for six 10-minute timebins (i.e., dividing the 60 minutes of gameplay into timebins of 10 minutes). Then, the GSR percent change from baseline was calculated for each participant to control for individual differences and to facilitate interpretation across participants (Boucsein, 2012; Eastabrook et al., 2013). For more details see OSF; https://osf.io/j7mvu/.

Motivation to Change

The contemplation subscale of the University of Rhode Island Change Assessment questionnaire (URICA; McConnaughy et al., 1983) consists of eight items (α = .73), measuring the extent to which participants are aware of their “problems” (as identified with the BAI, see below) and have the intention to change (e.g., “I think I might be ready for some self-improvement”; on a 5-point scale).

Symptom Severity

The Beck Anxiety Inventory (BAI; Beck et al., 1988; Beck & Steer, 1993) measures various symptoms of anxiety with 21 items (α = .94). Participants indicated the degree to which they were bothered by each symptom on a 4-point scale.

Trailer Manipulation Check

To examine whether the two trailers induced different expectations, participants answered two open questions: “What were your impressions of MindLight right after you watched the trailer?”, and “What did you expect from MindLight based on the trailer (before you played the game)?”. In addition, participants answered the following *Yes/No* question: “Did you notice that the message in the trailer were focused on [game enjoyment (ENT-condition)] *or* [on how MindLight could help people who feel stressed / anxious or have some mental health difficulties (MH-condition)]?”.

Statistical Analyses

First, trailer manipulation and randomisation were checked. Bootstrapped Pearson correlations between study variables are reported in Table 2. Bootstrapped paired *t*-tests were used to examine changes in affect, state-anxiety and arousal. The remaining preregistered research questions were examined within a (hierarchical) regression framework (controlled for high correlations; see preregistration) (IBM Corp, 2017). Univariate outliers were winsorized to ± 3.5 standard deviation from the mean before conducting the analyses. Because some study variables were not normally distributed, all regression models were bootstrapped with n = 1000 samples.

For GSR, univariate outliers were winsorized to ± 3.5 standard deviation from the mean, both for the overall GSR percent change value and the timebin values. The exploratory analyses for GSR included 1) a bootstrapped one-sample *t*-test to examine whether overall GSR percent change during gameplay was higher than zero, 2) bootstrapped regression analyses to examine differences between the experimental conditions and the interactions with motivation to change and anxiety symptoms on overall GSR percent change, and 3) a Repeated Measures ANOVA with the six 10-minute timebins to examine (polynomial) changes in GSR during gameplay, with experimental condition as between-subjects factor.

Table 2 Pearson correlations between the study variables

Variable [a]	**1.**	**2.**	**3.**	**4.**	**5.**	**6.**	**7.**	**8.**	**9.**	**10.**	**11.**	**12.**	**13.**	**14.**	**15.**	**16.**	**17.**
1. Age [b]	-																
2. Experienced fun	.14	-															
3. Affect pre-test [c]	.08	.07	-														
4. Affect post-test	.23	**.55**	.22	-													
Engaged in-game play behaviours																	
5. Mindlight – total [c]	.08	.19	.17	.08	-												
6. Exploration	.17	-.06	**-.30**	-.16	.04	-											
7. Fear attempt	-.07	.10	.19	.21	-.18	**-.31**	-										
Avoidant/safety in-game play behaviours																	
8. Mindlight – none [c]	**-.24**	.10	-.08	.17	**.31**	-.17	-.02	-									
9. Inactive [c]	-.11	**-.23**	.13	.03	.07	-.23	.03	**.28**	-								
10. Ceiling light attempt	-.15	-.08	.09	**.23**	-.12	-.16	-.06	.10	.01	-							
11. Inside chest [c]	.01	-.18	-.06	.04	-.15	-.02	-.16	-.16	-.17	-.11	-						
12. Motivation to change	**.24**	-.00	.13	-.06	.07	.05	-.11	.08	.10	-.02	-.04	-					
13. General anxiety symptoms	.03	.07	**-.32**	.09	**-.31**	.19	-.00	.16	.07	-.19	.02	.11	-				
14. State-anxiety pre-test	-.11	-.14	**-.68**	-.22	.06	**.39**	-.06	.06	-.19	-.15	.10	-.10	**.28**	-			
15. State-anxiety post-test	-.17	**-.36**	-.12	**-.58**	.15	.18	-.03	-.14	.03	**-.29**	.00	.14	.11	**.36**	-		
16. Arousal pre-test	-.04	.12	.06	-.00	.11	-.09	.09	-.00	-.16	-.06	.09	-.06	.06	.20	.07	-	
17. Arousal post-test	-.09	.03	-.02	-.07	.24	.01	-.02	-.10	-.21	-.02	.22	.07	.05	.08	**.46**	**.27**	-
18. Overall GSR percent change [c, d]	-.04[e]	-.04	.22	-.06	-.06	-.12	**.25**	**-.19**	.10	-.17	-.09	-.01	.02	-.12	.05	-.12	-.07

[a] All correlations were bootstrapped with n = 1000 samples. Correlations in bold have a 95% confidence interval that does not include zero. [b] n = 56 because one outlier was removed.
[c] One value was winsorized to ± 3.5 standard deviation from the mean. [d] n = 53. [e] n = 52.
GSR, galvanic skin response.

RESULTS

General Manipulation, Trailer Manipulation and Randomisation Check

Participants who were aware of the study aims (*n* = 0) and/or knew MindLight prior to the experiment (*n* = 1) were excluded from the analyses. The two trailers induced expectations as intended with our manipulation (see Table 3 and the pilot study in the preregistration; Wols et al., 2019). Descriptive statistics for the entire sample and for each experimental condition are provided in Table 1. Randomisation was successful indicating no differences between the experimental conditions on any study variables.

Table 3 Trailer Manipulation Check

	Experimental condition	
	Mental health trailer	**Entertainment trailer**
Expectations reported	62.1% mentioned mental health benefits of the game and/or that the game was a mental health game.	82.1% mentioned the entertainment value of the game and/or was positive about the game. 14.3% was negative about the game. For 1 participant it remained unclear whether (s)he was positive or negative about the game.
Trailer message awareness	93.1% noticed that the message in the trailer was focused on how MindLight could help people who feel stressed/anxious or have some mental health difficulties. 6.9% did not notice this.	50% noticed that the message in the trailer was focused on game enjoyment. 42.9% did not notice this. For 2 participants their answers were missing.

Experienced Fun and Change in Affect

Experienced fun did not significantly differ between the two trailer-conditions. The interactions between trailer-condition and motivation to change or anxiety symptoms did not have a significant effect on experienced fun (see Table 4). For positive affect, we found a significant decrease from pre- to post-test ($t(56) = 5.87$, $p = .001$; 95%CI mean difference [0.50;0.99]). Furthermore, affect at post-test did not significantly differ between the two trailer-conditions. The interactions between trailer-condition and motivation to change or anxiety symptoms did not have a significant effect on affect at post-test (see Table 4).

In-game Play Behaviours

Trailer-condition did not significantly predict any of the in-game play behaviours (see Table 5 and Table 6). The interaction between trailer-condition and motivation to change did not have a significant effect on any of the in-game play behaviours, with the exception of fear attempt (see Table 5). A significant positive effect of motivation to change on fear attempts was found in the MH-condition, b = 0.03, 95%CI [0.01;0.05], p = .025, and a significant negative effect was found in the ENT-condition, b = -0.03, 95%CI [-0.05;-0.00], p = .013. Finally, the interaction between trailer-condition and anxiety symptoms did not have a significant effect on any of the in-game play behaviours (see Table 5 and Table 6).

Change in State Anxiety and Arousal

Participants in both trailer-conditions reported increased state-anxiety (t(56) = -4.85, p = .001; 95%CI mean difference [-0.54;-0.25]), and increased arousal (t(56) = -3.13, p = .002; 95%CI mean difference [-0.82;-0.23]) after playing MindLight. There were no significant differences on state-anxiety and arousal at post-test between the two trailer-conditions (see Table 7). The interactions between trailer-condition and motivation to change or anxiety symptoms did not have a significant effect on state-anxiety and arousal at post-test (see Table 7).

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Exploratory Analyses

Overall GSR percent change during gameplay was significantly higher than zero with a mean difference of 32.92 (t(52) = 4.68, p = .002; 95%CI mean difference [19.77;47.54]). Trailer-condition did not significantly predict overall GSR percent change during gameplay, nor did the interactions between trailer-condition and motivation to change or anxiety symptoms (see Table 8).

For the Repeated Measures ANOVA, Mauchly's Test of Sphericity indicated that the assumption of sphericity had been violated, $\chi^2(14) = 117.92$, $p < .001$, and therefore a Greenhouse-Geisser (ε = .53) correction was used. There was no significant effect of time (F(2.67, 133.62) = 2.11, p = .109, η^2_p = .04) and no significant interaction effect between time and experimental condition (F(2.67, 133.62) = 0.16, p = 903. η^2_p = .00). See Table 9 for the GSR means per timebin and separately for experimental condition, including post-hoc (bootstrapped) independent t-tests to examine the differences between conditions per timebin (all non-significant).

Within-subjects polynomial contrasts, however, showed a significant quadratic trend for GSR (F(1, 50) = 6.67, p = .013, η^2_p = .12), and this trend did not differ between experimental conditions ($F(1, 50) = 0.08$, $p = .785$, $\eta^2_p = .00$). Paired samples *t*-tests were used to make post-hoc comparisons between the different timebins, and suggested that GSR increased during the first 40 minutes of gameplay and decreased after that (see Table 10).

Table 4 Hierarchical linear regression analyses predicting experienced fun and affect at post-test

		Dependent Variable			
		Experienced Fun		**Affect at post-test**	
		Unstandardized estimate *b* [95% CI]	**(*SE*)**	**Unstandardized estimate *b* [95% CI]**	**(*SE*)**
Step 1 [b]					
	Constant	0.39 [-1.05; 1.87]	(0.74)	2.93 [1.14; 4.53] **	(0.85)
	Affect at pre-test (control variable)			0.21 [-0.07; 0.53]	(0.15)
	Affect at post-test (control variable)	1.30 [0.83; 1.72] **	(0.23)		
	State anxiety post (control variable)			-0.65 [-1.00; -0.31] **	(0.18)
	Experienced fun (control variable)			0.16 [0.06; 0.27] **	(0.05)
Step 2 [c]					
	Constant	0.03 [-1.35; 1.65]	(0.76)	2.95 [1.13; 4.51] **	(0.85)
	Affect at pre-test (control variable)			0.21 [-0.07; 0.53]	(0.15)
	Affect at post-test (control variable)	1.26 [0.78; 1.67] **	(0.22)		
	State anxiety post (control variable)			-0.64 [-1.00; -0.29] **	(0.18)
	Experienced fun (control variable)			0.17 [0.06; 0.28] **	(0.06)
	Trailer-condition [a]	0.91 [0.05; 1.83]	(0.45)	-0.04 [-0.39; 0.31]	(0.18)
Step 3a [d]					
	Constant	-0.03 [-1.38; 1.54]	(0.75)	2.93 [1.06; 4.57] **	(0.90)
	Affect at pre-test (control variable)			0.20 [-0.07; 0,56]	(0.16)
	Affect at post-test (control variable)	1.28 [0.81; 1.69] **	(0.22)		
	State anxiety post (control variable)			-0.63 [-1.00; -0.27] **	(0.19)
	Experienced fun (control variable)			0.17 [0.06; 0.28] **	(0.06)
	Trailer-condition [a]	0.90 [-0.07; 1.87]	(0.46)	-0.04 [-0.42; 0.30]	(0.19)

		Dependent Variable			
		Experienced Fun		**Affect at post-test**	
		Unstandardized estimate *b* [95% CI]	**(*SE*)**	**Unstandardized estimate *b* [95% CI]**	**(*SE*)**
	Motivation to change	0.32 [-1.07; 1.82]	(0.73)	-0.07 [-0.66; 0.46]	(0.28)
	Interaction: motivation to change X trailer-condition	-0.45 [-2.65; 1.68]	(1.06)	0.10 [-0.72; 1.13]	(0.45)
Step 3b [e]					
	Constant	0.20 [-1.36; 1.84]	(0.84)	2.81 [1.09; 4.32] **	(0.83)
	Affect at pre-test (control variable)			0.28 [-0.05; 0.62]	(0.17)
	Affect at post-test (control variable)	1.21 [0.72; 1.69] **	(0.25)		
	State anxiety post (control)			-0.68 [-1.00; -0.27] **	(0.19)
	Experienced fun (control)			0.15 [0.05; 0.26] **	(0.05)
	Trailer-condition [a]	0.91 [-0.00; 1.81]	(0.48)	-0.01 [-0.35; 0.31]	(0.18)
	General anxiety symptoms	0.49 [-0.48; 1.51]	(0.50)	0.27 [-0.17; 0.58]	(0.19)
	Interaction: general anxiety symptoms X trailer-condition	-0.88 [-2.53; 0.61]	(0.80)	-0.07 [-0.56; 0.44]	(0.25)

Notes. * $p < .05$, ** $p < .01$, *** $p < .001$, 1.000 bootstrap samples. Steps 1 till 3a were performed within the same bootstrapped regression model.
[a] Trailer-condition was coded as 0 = entertainment trailer, 1 = mental health trailer. [b] $R^2 = .30$ and .49 for experienced fun and affect at post-test, respectively. [c] $R^2 = .35$ and .49 for experienced fun and affect at post-test, respectively. [d] $R^2 = .35$ and .49 for experienced fun and affect at post-test, respectively. [e] $R^2 = .37$ and .52 for experienced fun and affect at post-test, respectively.
CI, confidence interval; SE, standard error.

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Table 5 Hierarchical linear regression analyses predicting the engaged in-game play behaviours

		Dependent Variable					
		Mindlight total		**Exploration**		**Fear attempt**	
		Unstandardized estimate *b* [95% CI]	**(*SE*)**	**Unstandardized estimate *b* [95% CI]**	**(*SE*)**	**Unstandardized estimate *b* [95% CI]**	**(*SE*)**
Step 1 [b]							
	Constant	0.01 [0.01; 0.01] **	(0.00)	0.65 [0.61; 0.69] **	(0.02)	0.05 [0.04; 0.07] **	(0.01)
	Trailer-condition [a]	0.00 [-0.00; 0.01]	(0.00)	-0.00 [-0.06; 0.05]	(0.03)	-0.00 [-0.02; 0.01]	(0.01)
Step 2a [c]							
	Constant	0.01 [0.01; 0.01] **	(0.00)	0.65 [0.61; 0.69] **	(0.02)	0.05 [0.04; 0.07] **	(0.01)
	Trailer-condition [a]	0.00 [-0.00; 0.01]	(0.00)	-0.00 [-0.06; 0.05]	(0.03)	-0.00 [-0.02; 0.01]	(0.01)
	Motivation to change	0.01 [-0.01; 0.02]	(0.01)	0.05 [-0.03; 0.10]	(0.03)	-0.03 [-0.05; -0.00] *	(0.01)
	Interaction: motivation to change X trailer-condition	-0.01 [-0.02; 0.01]	(0.01)	-0.09 [-0.19; 0.04]	(0.06)	0.06 [0.02; 0.09] **	(0.02)
Step 2b [d]							
	Constant	0.01 [0.01; 0.01] **	(0.00)	0.65 [0.61; 0.69] **	(0.02)	0.05 [0.04; 0.07] **	(0.01)
	Trailer-condition [a]	0.00 [-0.00; 0.01]	(0.00)	-0.00 [-0.05; 0.05]	(0.03)	-0.00 [-0.02; 0.01]	(0.01)
	General anxiety symptoms	-0.01 [-0.01; 0.00]	(0.00)	0.01 [-0.05; 0.09]	(0.03)	0.00 [-0.03; 0.02]	(0.01)
	Interaction: general anxiety symptoms X trailer-condition	-0.00 [-0.01; 0.01]	(0.01)	0.04 [-0.05; 0.12]	(0.04)	0.00 [-0.02; 0.04]	(0.02)

Notes. * $p < .05$, ** $p < .01$, *** $p < .001$, 1.000 bootstrap samples. Steps 1 and 2a were performed within the same bootstrapped regression model.
[a] Trailer-condition was coded as 0 = entertainment trailer, 1 = mental health trailer.
[b] R^2 = .01, .00, and .00 for mindlight total, exploration, and fear attempt, respectively.
[c] R^2 = .04, .04, and .19 for mindlight total, exploration, and fear attempt, respectively.
[d] R^2 = .11, .06, and .00 for mindlight total, exploration, and fear attempt, respectively.
CI, confidence interval; SE, standard error.

Table 6 Hierarchical linear regression analyses predicting the avoidant/safety in-game play behaviours

		Dependent Variable							
		Mindlight none		**Inactive**		**Ceiling light attempt**		**Inside chest**	
		Unstandardized estimate *b* [95% CI]	***(SE)***	**Unstandardized estimate *b* [95% CI]**	***(SE)***	**Unstandardized estimate *b* [95% CI]**	***(SE)***	**Unstandardized estimate *b* [95% CI]**	***(SE)***
Step 1 [b]									
	Constant	0.03 [0.01; 0.05] **	(0.01)	0.04 [0.03; 0.05] **	(0.01)	0.23 [0.21; 0.27] **	(0.01)	0.02 [0.02; 0.03] **	(0.00)
	Trailer-condition [a]	0.01 [-0.01; 0.04]	(0.01)	-0.01 [-0.02; 0.01]	(0.01)	0.01 [-0.03; 0.05]	(0.02)	-0.01 [-0.01; 0.00]	(0.00)
Step 2a [c]									
	Constant	0.03 [0.02; 0.04] **	(0.01)	0.04 [0.03; 0.05] **	(0.01)	0.23 [0.21; 0.27] **	(0.01)	0.02 [0.02; 0.03] **	(0.00)
	Trailer-condition [a]	0.01 [-0.01; 0.04]	(0.01)	-0.01 [-0.02; 0.01]	(0.01)	0.01 [-0.03; 0.05]	(0.02)	-0.01 [-0.01; 0.00]	(0.00)
	Motivation to change	-0.02 [-0.06; 0.03]	(0.02)	-0.00 [-0.03; 0.03]	(0.02)	-0.03 [-0.09; 0.03]	(0.03)	-0.00 [-0.02; 0.01]	(0.01)
	Interaction: motivation to change X trailer-condition	0.06 [-0.00; 0.14]	(0.04)	0.02 [-0.02; 0.06]	(0.02)	0.06 [-0.01; 0.15]	(0.04)	0.00 [-0.02; 0.02]	(0.01)
Step 2b [d]									
	Constant	0.03 [0.02; 0.05] **	(0.01)	0.04 [0.03; 0.05] **	(0.01)	0.23 [0.21; 0.27] **	(0.01)	0.02 [0.02; 0.03] **	(0.00)
	Trailer-condition [a]	0.01 [-0.01; 0.04]	(0.01)	-0.01 [-0.02; 0.01]	(0.01)	0.01 [-0.03; 0.05]	(0.02)	-0.01 [-0.01; 0.00]	(0.00)
	General anxiety symptoms	0.01 [-0.01; 0.02]	(0.01)	0.00 [-0.02; 0.01]	(0.01)	-0.03 [-0.07; 0.00]	(0.02)	-0.00 [-0.01; 0.01]	(0.01)
	Interaction: general anxiety symptoms X trailer-condition	0.01 [-0.01; 0.05]	(0.02)	0.01 [-0.02; 0.03]	(0.01)	0.02 [-0.04; 0.07]	(0.03)	0.00 [-0.01; 0.01]	(0.01)

Note. * $p < .05$, ** $p < .01$, *** $p < .001$, 1.000 bootstrap samples. Steps 1 and 2a were performed within the same bootstrapped regression model.
[a] Trailer-condition was coded as 0 = entertainment trailer, 1 = mental health trailer.
[b] R^2 = .02, .01, .00, and .02 for mindlight none, inactive, ceiling light attempt, and inside chest, respectively.
[c] R^2 = .10, .04, .03, and .03 for mindlight none, inactive, ceiling light attempt, and inside chest, respectively.
[d] R^2 = .05, .02, .05, and .03 for mindlight none, inactive, ceiling light attempt, and inside chest, respectively.
CI, confidence interval; SE, standard error.

Table 7 Hierarchical linear regression analyses predicting arousal and state-anxiety at post-test

		Dependent Variable			
		Arousal at post-test		**State-anxiety at post-test**	
		Unstandardized estimate *b* [95% CI]	***(SE)***	**Unstandardized estimate *b* [95% CI]**	***(SE)***
Step 1 [b]					
	Constant	1.80 [1.00; 2.67] **	(0.41)	2.69 [1.90; 3.53] **	(0.40)
	Arousal at pre-test (control variable)	0.38 [0.02; 0.75] *	(0.18)		
	State-anxiety at pre-test (control variable)			0.31 [0.05; 0.58] *	(0.13)
	Affect at post-test (control variable)			-0.34 [-0.48; -0.21] **	(0.07)
Step 2 [c]					
	Constant	1.75 [0.76; 2.80] **	(0.53)	2.69 [1.90; 3.58] **	(0.41)
	Arousal at pre-test (control variable)	0.39 [0.01; 0.77] *	(0.19)		
	State-anxiety at pre-test (control variable)			0.31 [0.05; 0.58] *	(0.14)
	Affect at post-test (control variable)			-0.35 [-0.49; -0.21] **	(0.07)
	Trailer-condition [a]	0.07 [-0.56; 0.72]	(0.33)	0.02 [-0.23; 0.24]	(0.12)
Step 3a [d]					
	Constant	1.73 [0.70; 2.65] **	(0.51)	2.64 [1.87; 3.51] **	(0.40)
	Arousal at pre-test (control variable)	0.40 [0.03; 0.80] *	(0.19)		
	State-anxiety at pre-test (control variable)			0.31 [0.05; 0.58] *	(0.14)
	Affect at post-test (control variable)			-0.33 [-0.47; -0.19] **	(0.07)
	Trailer-condition [a]	0.08 [-0.53; 0.74]	(0.33)	0.02 [-0.23; 0.25]	(0.12)
	Motivation to change	0.33 [-0.87; 1.22]	(0.54)	0.27 [-0.18; 0.67]	(0.21)
	Interaction: motivation to change X trailer-condition	-0.25 [-1.61; 1.46]	(0.79)	-0.23 [-0.79; 0.39]	(0.29)
Step 3b [e]					
	Constant	1.58 [0.51; 2.57] **	(0.51)	2.81 [1.97; 3.72] **	(0.42)
	Arousal at pre-test (control variable)	0.47 [0.10; 0.86] *	(0.19)		
	State-anxiety at pre-test (control variable)			0.27 [-0.05; 0.58]	(0.15)
	Affect at post-test (control variable)			-0.37 [-0.50; -0.24] **	(0.07)

	Dependent Variable			
	Arousal at post-test		**State-anxiety at post-test**	
	Unstandardized estimate *b* [95% CI]	***(SE)***	**Unstandardized estimate *b* [95% CI]**	***(SE)***
Trailer-condition [a]	0.10 [-0.52; 0.68]	(0.31)	0.02 [-0.22; 0.27]	(0.12)
General anxiety symptoms	-0.34 [-1.17; 0.36]	(0.39)	0.17 [-0.24; 0.50]	(0.19)
Interaction: general anxiety symptoms X trailer-condition	0.80 [-0.20; 1.83]	(0.50)	-0.17 [-0.60; 0.30]	(0.23)

Notes. * $p < .05$, ** $p < .01$, *** $p < .001$, 1.000 bootstrap samples. Steps 1 till 3a were performed within the same bootstrapped regression model.
[a] Trailer-condition was coded as 0 = entertainment trailer, 1 = mental health trailer.
[b] $R^2 = .07$ and .39 for arousal at post-test and state-anxiety at post-test, respectively.
[c] $R^2 = .07$ and .39 for arousal at post-test and state-anxiety at post-test, respectively.
[d] $R^2 = .08$ and .42 for arousal at post-test and state-anxiety at post-test, respectively.
[e] $R^2 = .13$ and .41 for arousal at post-test and state-anxiety at post-test, respectively.
CI, confidence interval; SE, standard error.

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Table 8 Hierarchical linear regression analyses predicting overall GSR percent change

		Dependent Variable	
		Overall GSR percent change	
		Unstandardized estimate *b* [95% CI]	***(SE)***
Step 1 [b]			
	Constant	25.25 [11.67; 40.90] **	(7.32)
	Trailer-condition [a]	16.25 [-9.82; 47.91]	(14.63)
Step 2a [c]			
	Constant	25.26 [10.62; 40.91] **	(7.33)
	Trailer-condition [a]	16.56 [-9.53; 50.74]	(15.10)
	Motivation to change	- 6.34 [-41.29; 37.75]	(20.28)
	Interaction: motivation to change X trailer-condition	15.42 [-66.11; 87.64]	(38.67)
Step 2b [d]			
	Constant	25.15 [11.81; 39.61] **	(7.14)
	Trailer-condition [a]	16.02 [-12.61; 46.60]	(15.05)
	General anxiety symptoms	13.49 [-2.69; 30.47]	(8.03)
	Interaction: general anxiety symptoms X trailer-condition	-25.67 [-69.94; 32.48]	(25.67)

Notes. * p < .05, ** p < .01, *** p < .001, 1.000 bootstrap samples. Steps 1 and 2a were performed within the same bootstrapped regression model.
[a] Trailer-condition was coded as 0 = entertainment trailer, 1 = mental health trailer.
[b] R^2 = .03.
[c] R^2 = .03.
[d] R^2 = .06.
CI, confidence interval; SE, standard error; GSR, galvanic skin response.

Table 9 Mean GSR percent change for the six timebins for the total sample and for each experimental condition

		Experimental condition				
		Mental health trailer [a]	**Entertainment trailer [b]**			
Timebin	**Mean (*SD*) [95% CI[c]]**	**Mean (SD) [95% CI[c]]**	**Mean (SD) [95% CI[c]]**	***t* [d]**	***df***	***p***
1	26.45 (38.50) [15.73; 37.17]	34.64 (44.25) [19.02; 50.26]	19.43 (31.95) [6.08; 36.50]	-1.44	50	.16
2	32.90 (46.21) [20.03; 45.76]	40.98 (54.83) [22.10; 59.85]	25.97 (36.94) [10.72; 40.73]	-1.17	50	.25
3	34.16 (50.23) [20.18; 48.15]	41.54 (59.95) [20.94; 62.14]	27.84 (40.19) [14.37; 41.78]	-0.98	50	.33
4	35.71 (62.20) [18.40; 53.03]	45.81 (80.77) [20.35; 71.27]	27.06 (39.78) [15.94; 53.44]	-1.04	32.37[e]	.31
5	31.11 (58.29) [14.88; 47.33]	40.15 (71.51) [16.26; 64.03]	23.36 (43.90) [15.76; 49.34]	-1.04	50	.31
6	31.43 (57.21) [15.50; 47.36]	39.39 (67.97) [15.91; 62.88]	24.60 (46.29) [17.22; 46.80]	-0.93	50	.36

Notes. [a] n = 24.
[b] n = 28.
[c] Bootstrapped with n = 1000 samples.
[d] Bootstrapping the independent t-tests with n = 1000 samples showed similar results for all timebins.
[e] Levene's test was significant and equal variances could not be assumed.
SD, standard deviation, CI, confidence interval; df, degrees of freedom; GSR, galvanic skin response.

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Table 10 Paired samples t-tests for GSR between the different timebins

Pair		Mean difference (*SD*) [95% CI]	*t* [a]	*df*	p
Timebin 1	Timebin 2	-6.45 (17.92) [-11.44; -1.46]	-2.59	51	.01
	Timebin 3	-7.71 (21.64) [-13.74; -1.69]	-2.57	51	.01
	Timebin 4	-9.26 (31.67) [-18.08; -0.45]	-2.11	51	.04
	Timebin 5	-4.66 (27.38) [-12.28; 2.97]	-1.23	51	.23
	Timebin 6	-4.98 (27.87) [-12.74; 2.78]	-1.29	51	.20
Timebin 2	Timebin 3	-1.27 (12.64) [- 4.79; 2.25]	-0.72	51	.47
	Timebin 4	-2.82 (22.87) [-9.18; 3.55]	-0.89	51	.38
	Timebin 5	1.79 (23.84) [-4.84; 8.43]	0.54	51	.59
	Timebin 6	1.47 (23.19) [-4.99; 7.92]	0.46	51	.65
Timebin 3	Timebin 4	-1.55 (21.86) [-7.63; 4.53]	-0.51	51	.61
	Timebin 5	3.06 (22.66) [-3.25; 9.36]	0.97	51	.34
	Timebin 6	2.73 (22.01) [-3.39; 8.86]	0.90	51	.38
Timebin 4	Timebin 5	4.61 (19.02) [-0.69; 9.90]	1.75	51	.09
	Timebin 6	4.28 (21.04) [-1.58; 10.14]	1.47	51	.15
Timebin 5	Timebin 6	-0.32 (9.99) [-3.10; 2.46]	-0.23	51	.82

Notes. [a] Bootstrapping the paired samples t-tests with n = 1000 samples showed similar results for all comparisons.
SD, standard deviation, CI, confidence interval; df, degrees of freedom; GSR, galvanic skin response.

DISCUSSION

The current study examined the effect of participants' expectations for improvement (i.e., playing a mental health game or a regular entertainment game) on the following outcomes: 1) experienced fun and positive affect, and 2) in-game play behaviours while playing MindLight, an applied game shown to reduce anxiety symptoms in several randomised controlled trial (RCT) studies (Schoneveld et al., 2018; Schoneveld et al., 2016; Tsui et al., 2021; Wijnhoven et al., 2020). We also investigated changes in state-anxiety, arousal, and GSR. The secondary aim was to test the moderating role of motivation to change and symptom severity.

Our findings that expectations did not influence experienced fun and affect, are in line with previous research showing that young adults experiencing mental health symptoms found a commercial video game promoted as a mental health game similarly attractive and fun as the same game being promoted for its entertainment value (Poppelaars, Wols, et al., 2018). More importantly, players' game experiences and affect were not influenced by the mental health messaging (Poppelaars, Wols et al., 2018). We also found that expectations did not predict in-game behaviours and that participants in both trailer-conditions showed similar increases in state-anxiety, arousal and GSR. Although players can explore and progress through the game in a variety of ways, MindLight's design seems to ensure that players engage similarly with the game, regardless of their expectations about the game. Because engagement with the therapeutic techniques is necessary to be successful at the game, players who are unaware of the mental health aim still end up playing the game in a similar way as players who are aware of the mental health aim and may also benefit from it. Similarly, a previous study showed that initial anxiety levels were not associated with in-game play behaviours (Wols et al., 2018). Given the current findings, game designers may want to design applied games in such a way that players are encouraged to engage with the therapeutic techniques, regardless of their expectations about the game.

Regarding the secondary aim, we found no moderation of motivation to change and symptom severity, with the exception of one significant interaction between expectations and motivation to change on fear attempts. Given the small sample and multiple interactions that were tested, it could well be a chance finding and hence will not be further elaborated on. It might be that no moderation effects were found because individual differences have cancelled out some of the effects. For example, not all participants with equal levels of anxiety may have perceived the mental health message as personally relevant (Buday, 2015; Oliver & Krakowiak, 2009; Wols et al., 2019). In addition, expectations may not only be affected by an explicit mental health aim but may depend on other personal characteristics, such as gender, age, race, dispositional optimism, personality, treatment history, beliefs about and experiences with applied games (Enck et al., 2013; Oliver & Krakowiak, 2009; Wols et al., 2019; Zhou et al., 2019). Future research may want to examine the role of perceived personal relevance (De Haan et al., 2013; Oliver & Krakowiak, 2009) in combination with other individual differences. Future research may also investigate the role of non-specific factors on the long-term as well as more ecological valid contexts, such as voluntary choice for, prolonged engagement with and ongoing use of an applied game for mental health.

A limitation of the study is the modest sample size, only allowing detection of medium-sized effects. Secondly, participants were preselected on elevated levels of anxiety but there was no criterion regarding the time window between screening and participation in the lab, resulting in 54.4% of participants who did not meet the initial inclusion criterion anymore when they came to the lab. Finally, MindLight is an applied game in which the mental health aim is integrated in the story and cut-scenes of the game. Thus, for participants receiving the ENT-trailer it became clear while playing the game that it was aimed at reducing arousal and anxiety through relaxation, undermining their expectations that the game was a pure entertainment game.

Although MindLight has been developed for and tested for efficacy in a younger age group, we expected that the first year psychology students in the current study would still enjoy playing the game based on our previous experiences with an older age group (Tsui et al., 2021). In addition, recent reviews have shown that biofeedback interventions work for youth and young adults (Lantyer et al., 2013; Schoenberg & David, 2014), but may be more effective for young people when the feedback is integrated in an applied game, increasing their motivation and engagement (Weerdmeester et al., 2020). Because first year students often experience elevated levels of anxiety (Farrer et al., 2016; Stallman, 2010; Storrie et al., 2010), we considered MindLight an appropriate and relevant applied game for this specific age group and to test our research questions.

Notwithstanding the aforementioned limitations and remaining questions for future research, the current study integrated research on applied games with research on nonspecific factors and suggests that promoting an applied game as a mental health or entertainment game does not influence participants' experiences and engagement with the game, regardless of participants' motivation to change and symptom severity.

Chapter 6

Explicit mental health messaging promotes serious video game selection in youth with elevated mental health symptoms

Based on:
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ABSTRACT

Serious games aimed at promoting well-being in youth have promising effects and potential for far-reaching impact. Considering that most mental health disorders remain untreated in youth, therapeutic games may be most valuable when they are aimed at untreated youth with internalising symptoms. However, when targeting youth outside of a clinical setting, the first impression of therapeutic video games may determine whether and how a game is played. Thus, understanding the influence of messaging used in the promotion of therapeutic games on game choice and experience is critical. The current study examined two alternatives in promoting mental health games: one included explicit mental health messaging (e.g., learn to manage stress) and the other was a stealth promotion that did not mention mental health but highlighted the entertainment value. Young adults with mild to severe internalising mental health symptoms (i.e., depressive, anxiety, and stress symptoms) were shown two distinct trailer designs, with random assignment determining which design held which message. Participants (n = 129, M_{age} = 21.33, SD_{age} = 3.20), unaware that both trailers promoted the same commercial video game, were 3.71 times more likely to choose what they believed was the mental health game. Additionally, an unforeseen difference in the attractiveness of the two trailer designs resulted in participants being 5.65 times more likely to select the mental health game promoted in one trailer design over the other. Messaging did not influence game experience (i.e., gameplay duration, autonomy, competence, intrinsic motivation and affect). Exploratory analyses indicated that game experience, but not game choice, was influenced by symptom severity, symptom type and the interaction between symptom severity and messaging. The present study suggests that explicit mental health messages attract youth with mental health symptoms. Ultimately, youth may be empowered to seek out mental health games if they are promoted properly, allowing for far-reaching positive influences on well-being. Toward this aim, future research is needed on the game selection process, addressing underlying motivations, the balance between explicit health and entertainment messaging, and multiple interacting influences on game selection (e.g., promotion and peers).

Video games are immensely popular among youth (Lenhart et al., 2008; Entertainment Software Association, 2017). Consequently, the idea to capitalize on this popularity and to use games to teach youth skills is thriving (Wilkinson et al., 2008; Sardi et al., 2017; Dias et al., 2018). Serious games are designed to teach knowledge, skill or behaviour change and may be used to promote mental well-being in youth (Lau et al., 2017). Mental health games can be offered as tools to supplement standard therapy or as a replacement of school-based prevention programs (e.g., Fernández-Aranda et al., 2012; Schoneveld et al., 2018). However, the potential for impact may be far greater. Mental health games may be used to promote overall well-being in the general population, to offer light interventions to people with mild mental health symptoms or to reach individuals with clinical disorders who are not seeking professional help. Whether or not serious games can reach such diverse populations may in part be determined by how games are presented. Therefore, the aim of the current study was to test how messaging used to promote therapeutic games affects the game choice of youth and their experience of gameplay outside of a therapeutic context. Our focus in the current study was on youth as they are both avid consumers of video games (Entertainment Software Association, 2017) and particularly vulnerable to the development of psychological disorders (Merikangas et al., 2010).

The idea to capitalize on the popularity of video games stems from the attraction and engagement of video games. The potential that using games for therapeutic purposes holds has researchers and therapists excited for several reasons. Specifically, games may: (1) get youth motivated to learn skills; (2) attract and retain youth in therapy programs; (3) help youth persevere throughout therapy's strenuous process, similar to persisting in a difficult game level; (4) help youth realistically practice and (5) facilitate internalisation and generalisation of new skills (see e.g., Granic et al., 2014; Buday, 2015; Fleming et al., 2017). Although immensely challenging, creating games in which the therapeutic aim and engagement of game design enhance each other has some precedence. One example is MindLight, an effective anxiety reduction game that successfully evokes anxiety during gameplay, trains regulation of these anxious feelings, and engages children to the level that they would recommend the game to others as much as they would recommend a commercial game (Schoneveld et al., 2016, 2018). With this engagement potential of therapeutic games, it would be a shame to have mental health games only be played as part of a therapy protocol rather than reaching billions of people who enjoy commercial video games (Meeker, 2017).

While the first target population for serious games may be youth already in therapy, for whom serious games could supplement a therapist-led intervention, serious games can also be aimed at youth not (currently) in therapy. Specifically, a particularly interesting target population for serious games are youth with mild mental health issues, because: (1) their symptoms may be mild enough to be alleviated through an intervention outside of a clinical setting and (2) prevention research has repeatedly shown larger effect sizes for prevention targeted at risk groups compared to universal prevention (Horowitz & Garber, 2006; Stockings et al., 2016).

Moreover, people with acute mental health problems who nevertheless do not seek help is a third target population for serious games. A wide range of studies demonstrates that professional help-seeking is low in youth, with only about a quarter to a third of youth seeking help for diagnosable mood and anxiety disorders (Alonso et al., 2004; Merikangas et al., 2011). Perceived stigma is one important barrier that prevents youth from seeking help (Clement et al., 2015). Youth tend to feel self-conscious and embarrassed about seeking help from professionals and are concerned about confidentiality (Gulliver et al., 2010). Serious games may relieve the most pressing distress of these youth and/or aim to encourage youth to seek professional help. Thus, although serious games may target a range of populations, they may be most valuable when targeting youth with light mental health problems as well as those with severe mental health problems who are not inclined to seek professional help.

Given the promise of serious games for mental health targeted at youth not (currently) in therapy, factors that increase motivation to engage with these games need to be addressed. It may be that promoting a serious game as a proven tool to enhance well-being could be effective. However, youth may not always be inclined to do something because it is ‘good for them’ and even if they do, their motivation may be lower than for a regular game. Another option may be a stealth approach which promotes mental health games as regular entertainment games.

The value of each approach will depend on how youth value addressing mental health concerns. On the one hand, youth who are actually seeking interventions may be more attentive to games with explicit mental health aims than to games using a stealth entertainment promotion. Games that are explicitly promoted for their mental health aims may be easier to locate (e.g., online game stores or platforms have health and well-being sections). Also, youth who are not actively seeking interventions but have mild symptoms may still be more attentive to information about mental health improvement as it is personally relevant for them.

On the other hand, the stealth approach may be very promising considering stigma and the reluctance of youth to seek treatment for mental health issues (Barney et al., 2006; Gulliver et al., 2010; Merikangas et al., 2011). Whereas explicitly promoted serious games may stand apart from 'normal games,' serious games may blend in with the available commercial games if they avoid explicit health messages both in their promotion and in the content of the game (e.g., not using psychoeducation; Buday, 2015). Additionally, youth who are (initially) resistant to the idea of treatment may avoid anything related to their mental health issues including a mental health game (e.g., out of embarrassment), while a stealth game is less likely to trigger resistance.

The current study examined the impact of explicit mental health and stealth entertainment promotion on game choice and game experience in young adults with elevated mental health symptoms. Hypotheses about the relative impact of these two approaches may be formed based on several theoretical models focused on media choice.

These models suggest that media selection can be based on a person's needs, motivations (Katz et al., 1973), mood (Zillmann, 1988), mood deteriorating costs from media (Perse, 1998; Fahr & Bocking, 2009), long term benefits of media (Oliver, 2009), and a person's desire to maintain their autonomy (Brehm, 1966; Burgoon et al., 2002). All of these models may indicate that explicit messaging would turn youth off to a mental health game. For example, reactance theory and the escape model suggest that health messages would drive youth to an alternative game, either because they perceive the message to threaten their choice freedom (Brehm, 1966; Hornik et al., 2008; Richards & Banas, 2015; Richards et al., 2017) or because they expect the game to induce negative emotions (e.g., an aim of stress management may imply confrontations with stress; Perse, 1998; Fahr & Bocking, 2009). However, many of these same theories may also be used to explain why youth may be attracted to explicit mental health games provided that youth have an interest in improving their well-being. For example, a wider definition of needs and costs from media suggests that youth would play an explicit mental health game to gain insight into personal issues and current negative emotions may be tolerated for long-term benefits (Oliver, 2009).

Next to media selection theories, the motivational theory of self-determination may predict youth's responses to messaging depending on their intrinsic values. Intrinsic motivation (i.e., motivation stemming from the activity itself, e.g., the activity interests you) is theorized to be supported by three psychological needs being fulfilled: autonomy (experiencing the freedom to make your own decisions), competence (experiencing that you are able to be successful given your skills) and relatedness (experiencing a connection to others; Ryan & Deci, 2000). Self-determination theory is particularly interesting as need fulfilment and intrinsic motivation have been associated with better outcomes, including therapeutic outcomes (Ryan & Deci, 2000; Zuroff et al., 2007, 2017; Ryan et al., 2011). Moreover, Ryan et al. (2006) showed that experiencing autonomy and competence during gameplay is associated with game enjoyment, continued gameplay and better mood after gameplay. Initially, youth's motivations (e.g., improving their mental health) and experienced need fulfilment (e.g., feeling autonomy is limited by the mental health message) may influence game choice. Thus, self-determination theory may be another theory that can explain youth's game choice, although again it allows both hypothesis in favour of mental health messaging and in favour of entertainment messaging. Moreover, during gameplay intrinsic motivation and need fulfilment are elements of the game experience (i.e., experiencing low or high competence during gameplay) influenced by gameplay and potentially messaging. Therefore, intrinsic motivation and psychological needs may be vital when we try to understand the impact of messaging on game experience.

Thus, theoretically it is hard to predict how youth will react to serious games with an explicit or stealth promotional approach. Moreover, there is hardly any empirical evidence that can guide our hypotheses. In one closely related previous study, we examined how a mental health or entertainment trailer preceding a commercial video game influenced the experience of this game (Poppelaars, Lichtwarck-Aschoff, et al., 2018). This study showed that even though all participants played the same game, game experience was influenced by the trailer that participants viewed. Although intrinsic motivation and changes in affect were equal for participants regardless of the trailer message, participants who saw the mental health message experienced less autonomy in the game. Also, participants who reported more depressive symptoms and saw the mental health message experienced less competence. Additionally, participants with more depressive symptoms increased their positive affect after gameplay. This suggested that gameplay may at least temporarily improve the depressed mood of those at elevated risk for a depressive disorder. In contrast to daily life, however, participants had no influence on which game they played.

Therefore, the current study was designed to replicate the main findings as well as expand the scope of the previous study in three ways. First, to better approximate real-world media decisions, the current study allowed participants to *choose* between games promoted with a mental health or entertainment message. Second, as serious games may be most valuable if they target youth with some level of mental health problems, we selected participants with elevated mental health symptoms. Finally, the current study assessed stress and anxiety symptoms, in addition to depressive symptoms, to broaden the scope of our understanding and inform future prevention efforts.

Our primary aim was to test how entertainment versus mental health messaging influenced the choice and experience of a video game in young adults with elevated mental health symptoms. Participants chose a game to play after viewing two trailers, one for each type of messaging. We were able to directly link messaging to differences in choice and experience as both trailers portrayed the same game, unbeknownst to the participants. Following gameplay, game experience was assessed. This allowed us to examine the effect of entertainment and mental health messaging on eight dependent variables in the whole sample. We studied the effect of trailer message on three indicators of game appeal: (1) game choice; (2) perceived attractiveness and (3) perceived fun of the game. Additionally, the effect of game choice was examined on five prominent aspects of game experience, that is: (4) gameplay duration; (5) intrinsic motivation; (6) autonomy; (7) competence and (8) change in affect. Based on the theoretical literature and lack of previous empirical evidence, we made no predictions about how the messaging would influence game choice, preference for the games (i.e., perceived attractiveness and fun of the games from the trailers), the duration of gameplay nor about changes in affect. However, based on previous results (Poppelaars, Lichtwarck-Aschoff, et al., 2018), we expected gameplay after either trailer selection to result in similar levels of intrinsic motivation and competence and to improve affect equally. Furthermore, we hypothesised that participants would experience equal levels of autonomy because all participants selected the game they played.

The secondary aim of this study was to explore how severity and type of mental health symptoms influence game choice and experience. Thus, we explored if symptom severity or symptom type (i.e., depression, anxiety, and stress) moderated the effects of messaging on the same eight variables named above. Although there currently is no consensus on the relation between symptom severity and professional help-seeking, most evidence suggests that more severe symptoms are related to seeking more professional help (e.g., Oliver et al., 2005; Merikangas et al., 2011; Sawyer et al., 2012). Therefore, we hypothesised that youth with severe symptoms would select the mental health game more often. As the personal relevance of the mental health game is higher for those with severe mental health symptoms compared to those with less severe symptoms, we hypothesised that this choice is more intrinsically motivated and related to higher levels of autonomy. Furthermore, based on previous results (Poppelaars, Lichtwarck-Aschoff, et al., 2018), we expected elevated depressive symptoms to predict a greater increase in affect and to predict less competence in participants who select the mental health game over the entertainment game. For anxiety, stress and the remaining dependent variables for depressive symptoms and symptom severity, no hypotheses were formulated.

METHODS

Participants

In total 155 young adults (M_{age} = 21.48, SD_{age} = 3.36) participated in this study between March and November 2017. Participants were only included in the analyses if they were unaware of the study's aims and the fact that both trailers reflected the same game (*n* = 129).

Participants included in the analyses were between 18 and 31 years old, with a mean age of 21.33 years (*SD* = 3.20). The majority of the participants was female 73.6%. Almost all participants were enrolled in or had completed higher education (91.5%), while some were enrolled in or had completed a pre-university track (7.0%). Two participants had completed unsegregated secondary education (1.6%). The majority of participants currently enrolled in education were enrolled in a social science track (76.9%).

All participants were selected based on having at least mildly elevated mental health symptoms on at least one subscale of the Depression Anxiety Stress Scale (DASS-21; Lovibond & Lovibond, 1995). Participants showed mildly elevated symptoms on one (41.9%), two (27.9%) or three (30.2%) subscales. At least a mildly elevated score on depression, anxiety or stress were shown by, respectively, 64.3%, 67.4%, and 56.6% of participants. Of the sample 31.8% had severe or extremely severe scores on at least one DASS-21 subscale.

Although participants indicated that they were moderately positive about video games in general (M_{liking} = 4.50, *SD* = 1.75; on a 7-point scale with a higher score indicating a more positive attitude), 48.8% indicated not playing video games at all in an average week, with an additional 9.3% playing an hour or less per week. Almost a fifth of participants indicated playing video games more regularly (1–7 h a week; 18.6%) and almost a quarter of participants played more than 7 h a week (23.3%).

Procedure

Participants were recruited for the study on a university and higher vocational education campus in the Netherlands through flyers and the university's online research participation system. Young adults (*n* = 648), who provided informed consent, were invited to fill out a 15-min online screening questionnaire either voluntarily or for study credits. The online questionnaire was used to assess eligibility, as well as to gather information on demographics, video game behaviour and additional questionnaires that are not part of the current study. The inclusion criteria were: (1) Being 18 years or older; (2) Having at least a mildly elevated score on the DASS-21; (3) Being unfamiliar with the game Monument Valley, meaning that the participant had not seen or heard anything about the game; and (4) Being willing and able to sign informed consent. Those who met the inclusion criteria (*n* = 264) were invited to participate in the lab experiment within 2 weeks from completing the screening questionnaire (range 1–20 days, *M* = 8.15, *SD* = 4.52). Of the 155 young adults participating in the lab experiment 9.0% participated between 2 and 3 weeks from screening, due to holiday periods and unavoidable delays (e.g., a participant being ill). Both the screening and experiment questionnaires were available in Dutch and English. For standardized questionnaires with no official translation, the Dutch translations of Poppelaars, Lichtwarck-Aschoff, et al. (2018) were used (see article for further information).

During the lab experiment, participants were seated in a plain cubicle with a computer and a tablet. At the start of the experiment participants were given both verbal and written information on the study and gave informed consent. Participants were told that they would see two trailers of video games. The researcher requested the participants to choose the game they believed they would enjoy most, under the guise that the study's topic was gameplay of a game that participants may have played at home. To further encourage choosing the most appealing game, participants were informed that they had a chance to win their chosen game.

Next, participants received instructions on using the tablet. They were told they were free to play the chosen game as long as they liked, but that it was important that they could evaluate the game. In fact, although there was no minimum time limit, participants were told to continue with the questionnaires 50 min from the start of the experiment (approximately 40 min of gameplay), to ensure they did not exceed the 60 min set for the experiment.

Once the researcher left the cubicle, participants filled out an assessment of affect. Next, participants were shown, in a random order, two trailers of the video game Monument Valley. One trailer portrayed the game as an entertainment game and one trailer portrayed the game as beneficial for players' mental health. The trailers were designed to convince participants that two separate games were being promoted and the combination of trailer design and message was counterbalanced across participants. To achieve the latter, participants were randomised using a blocked randomisation to receive trailer A including the entertainment message and trailer B including the mental health message or to see the same trailers with the messages interchanged. After viewing the trailers, participants were asked to select the game they would like most and were reminded that they could win that same game. After choosing a game, participants rated the attractiveness and fun of each game based on the trailer, before they were instructed to play Monument Valley 1 (Ustwo Games, 2014a).

Following gameplay, participants completed questionnaires on their affect, intrinsic motivation, autonomy and competence and questions about the manipulation check, trailer message and questionnaires not included in the current study. Participants were provided with study credits or a gift certificate worth €10. Once testing was completed in November 2017, debriefing was done through email and 10 participants were randomly selected to receive a reimbursement for purchasing Monument Valley 1 and 2 (Ustwo Games, 2014a, 2017). This study was approved by the ethical committee of the Faculty of Social Sciences at Radboud University (ECSW2017-3001-461).

Monument Valley and Messaging

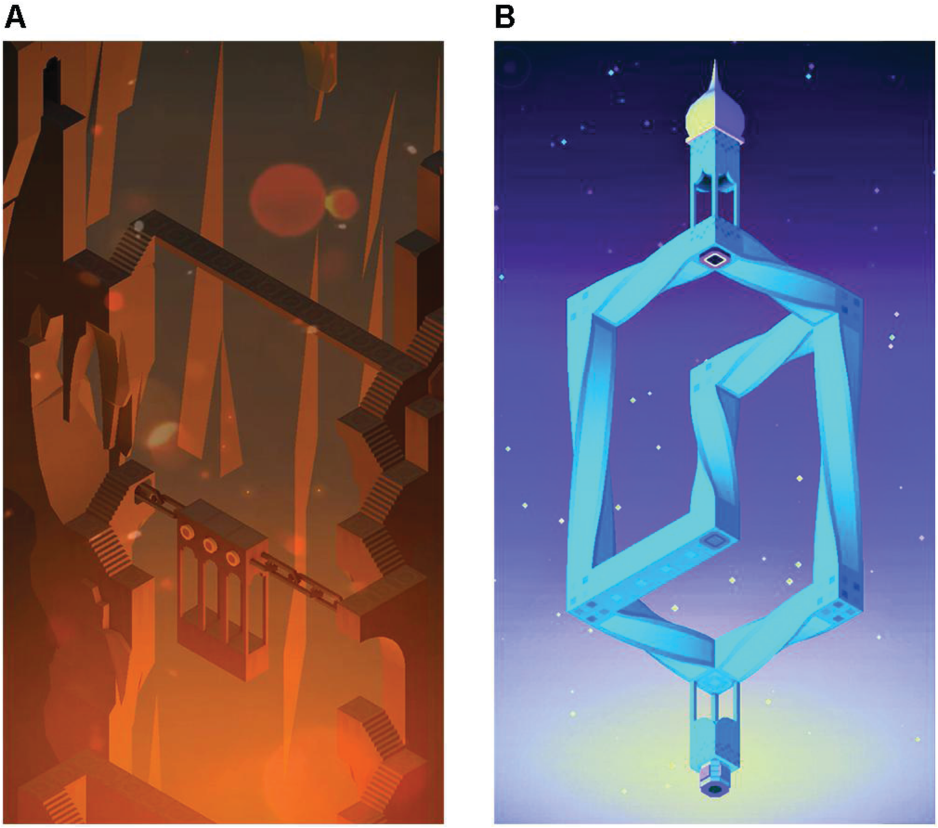
The original Monument Valley game released in 2014 (Ustwo Games, 2014a) is an award winning commercial puzzle game with optical illusions inspired by the art of M. C. Escher and can be played on smartphones and tablets (Figure 1). This game was designed to create an optimal balance between difficulty and pleasure, as well as to allow all players to be able to complete all levels (Ustwo Games, 2014b) making it accessible for participants with various levels of gameplay experience. Although the game was not designed with a therapeutic aim, players may believe that it was when it is presented that way, because of the relaxed atmosphere and the way the game illustrates problem solving, an adaptive technique for coping with stress and negative emotions (i.e., the player finds solutions for the game's challenges by literally looking at the challenge from several angles). Indeed, participant comments following gameplay indicated that the mental health claim was credible (e.g., one participant recommended the mental health game to other participants because 'it will relax all stressed out students').

Figure 1 Screenshots from the video game Monument Valley. **(A)** A building before the dark khaki section is rotated. The player's aim is to have the avatar in white reach the top of this building. **(B)** The same building after rotating the dark khaki section, allowing the player to find a new path upward. These images are reproduced from Monument Valley by Ustwo Games (2014a) with the permission of the copyright holder Ustwo Games.

A B

For this study, we created two distinguishable trailers of Monument Valley allowing us to attribute any differences in game choice or game experience to messaging while participants were unaware of this manipulation. Screenshots taken from the later levels and expansion levels of Monument Valley were used in the trailers to make it unlikely for participants to encounter these levels during the experiment. We differentiated the trailers on several aspects to create the impression that the trailers were portraying two separate games. Pilot studies were conducted to make sure that potential participants indeed believed that the two trailers advertised two different games. Information regarding pilot studies are available upon request. The trailers, which we will refer to as the *detailed* and the *abstract* trailer, were both approximately 1-min long and differed on the following aspects, respectively: (1) Showing game challenges that had a more *detailed* environment vs. game challenges that were *abstract* buildings floating in space (Figure 2); (2) Faster vs. slower music; (3) A warmer vs. a cooler colour palette; (4) AR BONNIE vs. Gloucester MT font for the trailer text; and (5) An editing style focused on slowly moving across the pictures vs. zooming in or out of the pictures.

Figure 2 Screenshots from the two trailers of Monument Valley. **(A)** A screenshot from the detailed trailer. **(B)** A screenshot from the abstract trailer. These images have been adapted from Monument Valley by Ustwo Games (2014a) with the permission of the copyright holder Ustwo Games.



During the experiment, the messaging in each trailer design was counterbalanced and trailers were shown in a random order. This allowed us to assess the effects of messaging while controlling for trailer design. Approximately half of the participants saw the detailed trailer as the mental health trailer (n = 66) and the abstract trailer as the entertainment trailer, while the other half (*n* = 63) saw the abstract trailer as the mental health trailer and the detailed trailer as the entertainment trailer. Both messages consisted of five short phrases. For both messages the first sentence introduced the game as appealing, as one would expect from a promotional trailer. The next four phrases focused on the mental health or the entertainment message (Table 1).

Table 1 Messages included in the mental health and entertainment trailers

Mental Health	Entertainment
Perfect for a single marathon playthrough	A game you must play
Learn to manage stress more efficiently	Think outside the box to solve intricate puzzles
Therapeutic insights for emotional mastery	9/10 Polygon 5/5 Touch Arcade
Both challenging and relaxing	Almost impossibly gorgeous
Recommended by games for mental health	iPad game of the year

Instruments

Mental Health Symptoms

The DASS-21 (Lovibond & Lovibond, 1995) was used both in the original English version as well as in the Dutch translation by De Beurs (2010). All 21-items were scored on a 4-point scale (*Did not apply to me at all* = 0, *Applied to me to some degree, or some of the time* = 1, *Applied to me to a considerable degree, or a good part of the time* = 2, *Applied to me very much, or most of the time* = 3) with seven items for each of the subscales: depressive symptoms (e.g., 'I felt that I had nothing to look forward to.'; α = 0.83), anxiety symptoms (e.g., 'I felt scared without any good reason.'; α = 0.70) and stress symptoms (e.g., 'I found it difficult to relax.'; α = 0.79). Participants rated to what degree each statement applied to them over the past week. The summed score for each subscale indicated mild symptoms at or above a score of 5 for depression, 4 for anxiety, and 8 for stress. Thus, when we refer to elevated symptoms, we are referring to symptoms at or above the mild cut-off. Additionally, in the analyses all participants who scored at or above the severe cutoff (11 for depression, 8 for anxiety, and 13 for stress) for one or more of the subscales were compared to participants who had no symptoms within the severe or extremely severe range. As symptoms of the different subscales have different cutoffs (e.g., a score of 8 can indicate mild stress or severe anxiety) a continuous sum score cannot be used to indicate overall mental health. Thus, when referring to severe symptoms, we are referring to a score at or above the severe cut-off on one or more DASS-21 subscales.

Affect

The Self-Assessment Manikin (SAM; Bradley & Lang, 1994) for affect was used to asses current affect before and after gameplay. Participants were asked to indicate, on a 5-point manikin based scale, the manikin that reflected how they felt at that moment and were given a list of adjectives to indicate the extreme negative (e.g., *unhappy, annoyed, bored* = 1) and extreme positive (e.g., *happy, satisfied, hopeful* = 5) points of the scale. Each manikin was a simple line drawing of a person with an emotional facial expression, with the neutral facial expression in the middle of the scale (a score of 3).

Game Choice and Trailer Preference

After viewing the trailers, participants chose one of the games, referred to as game A and game B and accompanied by screenshots to ensure that participants could correctly identify each game. Although all participants played Monument Valley, the analyses were done from the perspective of the participant and focus on their choice for a game promoted with mental health or entertainment messaging. Directly after game selection, participants rated both games based on the trailers on attractiveness and fun using two separate 10-point scales (1–10), with higher scores indicating more perceived attractiveness and perceived fun, respectively.

Gameplay Duration

Gameplay duration was measured using two methods. First, the online questionnaire page that was open while participants played Monument Valley contained an invisible timer. Second, the game tablets were equipped with the program Funamo Parental Control (Funamo; Funamo Inc., 2016), which recorded how long Monument Valley was open for each participant.

Both measures were used to create a gameplay duration measure, as some participants did not close the game when they continued with the questionnaire (resulting in an incorrect gameplay duration in Funamo) and others continued to the next page in the questionnaire before finishing gameplay (resulting in an incorrect gameplay duration on the questionnaire timer). As Funamo directly records how long the game is opened it was the preferred measure. For participants who had an incorrect gameplay duration in Funamo (n = 9), we used the gameplay duration from the questionnaire corrected for the average time it took to open and close the game as well as to read the instruction on the questionnaire page. For one participant, no gameplay duration could be calculated as both the Funamo and questionnaire measures were incorrect.

Additionally, it was decided that a standard gameplay duration would be given to all participants who were stopped during gameplay to complete the questionnaire (*n* = 25) and participants who exceeded that standard duration without being stopped (*n* = 2). This was done because the maximum amount of time participants could play during the experiment was constrained by the duration of the experiment explanation and the time it took participants to fill out all questions prior to gameplay. Thus, the gameplay duration for all these participants was set at the mean gameplay duration correctly recorded for participants who were stopped, that is 40.38 min.

Intrinsic Motivation

*I*ntrinsic motivation was measured with the interest/ enjoyment subscale from the Intrinsic Motivation Inventory (Ryan, 1982; McAuley et al., 1989). Participants responded to seven statements (e.g., 'This game was fun to do.') about their experience with Monument Valley using a 7-point scale (*Not at all true* = 1, *Somewhat true* = 4, *Very true* = 7). Two items needed to be recoded to create a mean score where higher scores indicated more intrinsic motivation (α = 0.89).

Autonomy and Competence

Using the Player Experience of Need Satisfaction questionnaire (PENS; Ryan et al., 2006; Immersyve, 2007), the psychological needs autonomy and competence were measured. Both needs were assessed with three items, for example 'The game lets you do interesting things.' for autonomy (α = 0.78) and for example 'I feel very capable and effective when playing.' for competence (α = 0.83). Items were rated on a 7-point scale (*Strongly disagree* = 1, *Strongly agree* = 7) and a mean was calculated for each subscale with higher scores indicating a stronger experience of autonomy and competence, respectively. The psychological need for relatedness was not measured as Monument Valley provides no opportunity to interact with other players.

Manipulation Check

At the end of the experiment participants were asked (1) what they believed the study's aim was, (2) which game they would recommend to the next participant and why (i.e., *Game A, because …*, *Game B, because …*, *or I do not have a preference, because …*, with the last option prompting some participants to indicate that they believed there was only one game) and (3) to explain the difference between the messaging of the two trailers, if they had noticed a difference. The answers to these questions were checked for (1) awareness of the study's manipulation using one game rather than two games; (2) awareness of the study's aim to relate the mental health message in one of the trailers to game choice and (3) awareness of the study's aim to relate players mental health symptoms to game choice. Participants who were aware of at least one, were excluded from the analyses (*n* = 26).

Trailer Message Awareness

After the manipulation check, participants answered two last questions to identify if they had noticed the mental health message. The first question was 'Did you notice that the message of one of the two trailers was primarily focused on game enjoyment, while the other trailer contained the message that it could help people who feel stressed or have some mental health difficulties?' (*Yes* or *No*). Finally, participants selected a screenshot from one of the trailers to answer the question 'Which of the two trailers do you believe contained the message that this video game can help people who feel stressed or have some mental health difficulties?'.

Statistical Analyses

All analyses were done in version 25 of SPSS (IBM Corp, 2017). First, randomisation was checked by comparing the two conditions on several descriptive variables, mental health symptoms and awareness of messaging using *t*-tests and Chi-square. Additionally, a Chi-square test was used to compare the selection of the trailer designs and dependent *t*-tests were used to test if perceived attractiveness and perceived fun differed per trailer design. Also, *t*-tests were done to test if the trailer design of the chosen game was related to gameplay duration, intrinsic motivation, autonomy, competence, affect. Additionally, a Repeated Measures ANOVA (RM-ANOVA) was used to relate changes in affect to the trailer design of the chosen game.

Turning to the main research aim, a logistic regression was performed predicting game choice using trailer design as a predictor to understand how the effects of trailer design and trailer message interact. Furthermore, *t*-tests were used to test if game choice was related to gameplay duration, intrinsic motivation, autonomy, competence, and affect before and after gameplay. Moreover, for perceived attractiveness and fun RM-ANOVAs were used to compare the scores for the mental health and the entertainment trailer provided by each participant. Another RM-ANOVA was used to relate changes in affect to game choice.

Finally, these analyses were repeated to address the second research aim to distinguish effects of mental health symptom severity and type of mental health symptoms. First, in logistic regressions game choice was predicted using trailer design, symptoms and the interaction between symptoms and trailer design. Next, one-way ANOVAs were used to relate symptoms and the interaction between symptoms and game choice to gameplay duration, intrinsic motivation, autonomy, competence, and affect before and after gameplay. Again, RM-ANOVAs were used for attractiveness, fun and affect. Thus, the analyses were repeated comparing those with and without severe symptoms, those with or without elevated depressive symptoms, those with or without elevated anxiety symptoms and those with or without elevated stress symptoms. For all significant interaction effects *post hoc* analyses were performed with a Bonferroni correction.

RESULTS

Descriptive Statistics

The descriptive statistics for the entire sample and per condition are provided in Table 2. Randomisation was successful as there were no differences between conditions on age, gender, birth country, general video game liking, weekly hours of video gameplay, depressive symptoms, anxiety symptoms, stress symptoms or severe symptoms. Participants played Monument Valley for a mean of 28.48 min (SD = 8.85), with a range of 12.77–40.38 min.

Moreover, we tested if the mental health message was clear in both trailer designs. Of the whole sample 88.4% indicated that they noticed that one of the trailers contained a mental health message and 82.2% was able to correctly identify this trailer. There was no difference between conditions in correctly identifying the trailer containing the mental health message (Table 2). Furthermore, there was no difference in awareness of the mental health message between those who decided to play the mental health game versus those who decided to play the entertainment game (81.8% vs. 82.7% correctly identified the mental health trailer, respectively; X^2 (1, n = 129) = 0.02, p = .90).

Table 2 Descriptives (means and standard deviations or percentages) for the total sample and per condition including chi-square tests and t-tests comparing conditions

	Total		**Detailed trailer with mental health**		**Abstract trailer with mental health (SD)**		χ^2 / *t*	*df*	*p*
	M / %	***(SD)***	***M / %***	***(SD)***	***M / %***	***(SD)***			
Age	21.33	(3.20)	21.56	(3.06)	21.10	(3.35)	0.83	127	.41
Gender							0.06	1	.81
Female	73.6%		72.7%		74.6%				
Male	26.4%		27.3%		25.4%				
Birth country							1.77	2	.41
Dutch	49.6%		53.0%		46.0%				
German	29.5%		24.2%		34.9%				
Other	20.9%		22.7%		19.0%				
Video game liking	4.50	(1.75)	4.55	(1.85)	4.46	(1.64)	0.28	127	.78
Weekly video game-play	4.25	(6.77)	4.83	(7.52)	3.63	(5.87)	1.01	122.20	.31
Depressive symptoms	6.02	(3.92)	6.30	(4.16)	5.71	(3.67)	0.85	127	.40
Anxiety symptoms	5.12	(3.41)	5.52	(3.75)	4.70	(3.00)	1.36	127	.18
Stress symptoms	8.24	(3.85)	8.35	(4.04)	8.13	(3.67)	0.33	127	.75
Severe symptoms							< 0.01	1	.99
Yes	31.8%		31.8%			31.7%			
No	68.2%		68.2%			68.3%			
Elevated depressive symptoms							0.29	1	.59
Yes	64.3%		62.1%			66.7%			
No	35.7%		37.9%			33.3%			
Elevated anxiety symptoms							0.88	1	.35
Yes	67.4%		71.2%			63.5%			
No	32.6%		28.8%			36.5%			
Elevated stress symp-toms							0.70	1	.40
Yes	56.6%		53.0%			60.3%			
No	43.4%		47.0%			39.7%			
Messaging identified							0.32	1	.57
Yes	82.2%		80.3%			84.1%			
No	17.8%		19.7%			15.9%			

Trailer Design

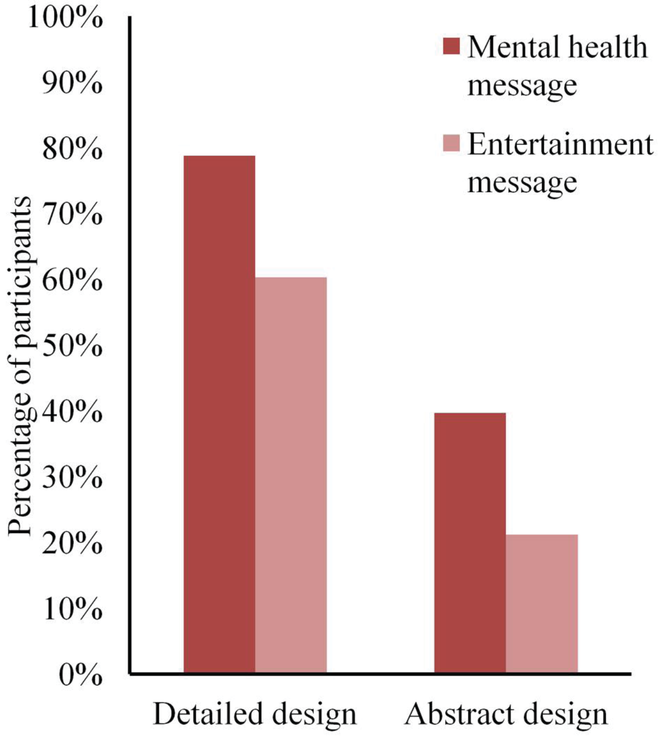
Before testing the effect of the trailer message, we tested if trailer designs were selected equally and if trailer design affected preference (Table 3). Participants were significantly more likely to select the detailed trailer than the abstract trailer. Similarly, participants perceived the game in the detailed trailer as more attractive and more fun than the abstract trailer. Therefore, trailer design was controlled for in all further analyses on game choice, perceived attractiveness and perceived fun.

Next, we tested whether the trailer design of the chosen game was related to game experience. We found that affect increased over time with a small effect size [$F(1, 124) = 6.45$, $p < .05$, $\eta^2_p = .05$], however, the trailer design of the chosen game was not related to change of affect over time [$F(1, 124) = 0.01$, $p = .92$, $\eta^2_p < .01$]. In addition, results show that trailer design was not related to affect before gameplay, affect after gameplay, duration of gameplay or the experience of intrinsic motivation, autonomy or competence (Table 3). Thus, participants who played Monument Valley after selecting the detailed trailer did not play longer and did not experience more intrinsic motivation, autonomy or competence than participants who played the game after selecting the abstract trailer.

Table 3 Descriptives (means and standard deviations or percentages) for the detailed and abstract trailers including chi-square tests and t-tests comparing trailers

	Detailed trailer		**Abstract trailer**		***χ²* / *t***	***df***	***p***
	***M* / %**	***(SD)***	***M* / %**	***(SD)***			
Game choice	69.8%		30.2%		20.16	1	< .001
Attractive	6.50	(1.60)	5.53	(1.83)	– 6.07	128	< .001
Fun	6.25	(1.62)	5.47	(1.81)	– 5.25	128	< .001
Affect before	3.73	(0.73)	3.67	(0.81)	– 0.46	127	.65
Affect after	3.89	(0.67)	3.87	(0.67)	– 0.14	124	.89
Duration play	28.32	(8.91)	28.86	(8.82)	0.32	126	.75
Motivation	5.25	(1.04)	5.38	(0.97)	0.67	127	.51
Autonomy	4.75	(1.20)	4.95	(1.03	0.89	127	.37
Competence	4.98	(1.11)	5.10	(1.19)	0.56	127	.58

Figure 3 The percentage of game selection



Notes: Participants were given the choice between (1) the detailed trailer with the mental health message (78.8%) and the abstract trailer with the entertainment message (21.2%) or (2) the detailed trailer with the entertainment message (60.3%) and the abstract trailer with the mental health message (39.7%). Despite the detailed design being preferred over the abstract design, both designs were chosen more often when presented with the mental health message.

Main Analyses

To address the first research aim, we first compared the effects of trailer message on game choice and preference. A logistic regression predicting game choice using the trailer design as a predictor showed that the odds of choosing the mental health game were 3.71 times higher than the odds of choosing the entertainment game [X^2 (1, n = 129) = 18.99, p < .001]. However, the odds of choosing the mental health message when it was portrayed in the detailed trailer were 5.65 times higher than the odds of choosing the mental health message when it was portrayed in the abstract trailer [X^2 (1, n = 129) = 19.09, p < .001]. The complete model was able to correctly predict 69.8% of the game choices [X^2 (1, n = 129) = 21.11, p < .001]. Figure 3 shows how the four combinations of trailer design and trailer message were chosen at different rates by the participants. Specifically, the figure shows both a favouring of the detailed trailer and the trailer with the mental health message.

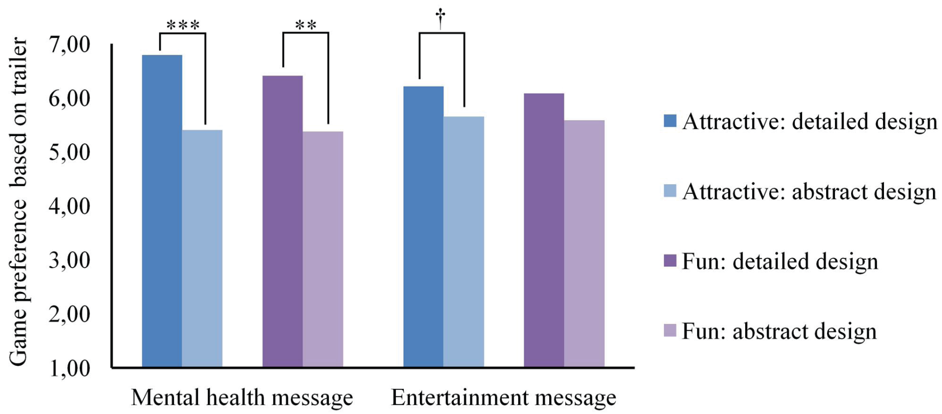
Although mental health messaging made it more likely for participants to choose a game, participants did not perceive the game promoted with the mental health message as more attractive or more fun than the game promoted with the entertainment message (Table 4). However, there was a significant interaction for perceived attractiveness of trailer message × trailer design [F(1, 127) = 36.60, p < .001, η^2_p = .22], indicating that there was a significant difference between the trailer designs in perceived attractiveness when they contained the mental health message [$M_{abstract}$ = 5.40, $SE_{abstract}$ = 0.21; $M_{detailed}$ = 6.79, $SE_{detailed}$ = 0.20; F(1, 127) = 22.89, p < .001, η^2_p = .15], but not when the trailer designs contained the entertainment message [$M_{abstract}$ = 5.65, $SE_{abstract}$ = 0.22; $M_{detailed}$ = 6.21, $SE_{detailed}$ = 0.22; F(1, 127) = 3.16, p = .08, η^2_p = .02]. Similarly, there was a significant interaction for perceived fun of trailer message × trailer design [F(1, 127) = 27.23, p < .001, η^2_p = .18], indicating that there was a significant difference between the trailer designs in perceived fun when they contained the mental health message [$M_{abstract}$ = 5.37, $SE_{abstract}$ = 0.21; $M_{detailed}$ = 6.41, $SE_{detailed}$ = 0.21; F(1, 127) = 12.51, p < .001, η^2_p = .09] and not when they contained the entertainment message [$M_{abstract}$ = 5.58, $SE_{abstract}$ = 0.22; $M_{detailed}$ = 6.08, $SE_{detailed}$ = 0.22; F(1, 127) = 2.64, p = .11, η^2_p = .02]. Thus, participants believed the game to be significantly more fun and attractive if the mental health message was included in the detailed trailer than if they received the mental health message in the abstract trailer. However, when trailers contained the entertainment message the abstract and the detailed trailer were perceived as equally fun and attractive (Figure 4).

Table 4 Descriptives (means and standard deviations or percentages) for the mental health and entertainment trailers including tests comparing trailers: chi-square, RM-ANOVA and t-tests

	Mental health trailer		**Entertainment trailer**		***χ^2 / t / F***	***df***	***p***
	M / %	***(SD)***	***M / %***	***(SD)***			
Game choice	59.7%		40.3%		4.85	1	< .05
Attractive	6.11	(1.79)	5.92	(1.79)	1.03	1, 127	.31
Fun	5.90	(1.75)	5.82	(1.77)	0.16	1, 127	.69
Affect before	3.78	(0.77)	3.62	(0.72)	1.22	127	.23
Affect after	3.95	(0.57)	3.79	(0.78)	1.25	88.28	.16
Duration play	28.74	(8.87)	28.10	(8.90)	0.40	126	.69
Motivation	5.31	(1.03)	5.26	(1.01)	0.27	127	.79
Autonomy	4.81	(1.15)	4.81	(1.16)	−0.02	127	.98
Competence	4.99	(1.16)	5.06	(1.10)	−0.38	127	.71

Note. The chi-square test for game choice was not controlled for trailer design.

Figure 4 Perceived attractiveness and fun of the game for each design and message combination



Notes. Significant effects are indicated as follows: †p < 0.10, p < 0.05, p < 0.01, and p < 0.001.

Furthermore, we tested if game choice predicted game experience (Table 4). *T*-tests showed no differences in gameplay duration, intrinsic motivation, autonomy and competence. Thus, playing a game promoted for entertainment or mental health did not change the duration of gameplay, nor how much intrinsic motivation, autonomy and competence players experienced.

Next, we tested change in affect. As shown previously there was a significant improvement in affect over time [$F(1, 124) = 7.18$, $p < .01, \eta^2_p = .06$], however, there was no effect of game choice on the change in affect [$F(1, 124) < 0.01$, $p = .98$, $\eta^2_p < .01$]. This indicates that participants experienced more positive affect after gameplay regardless of playing a game promoted for entertainment or mental health.

Exploratory Analyses

In order to address the second research aim, we explored the effects of mental health symptom severity and type on all dependent variables. For all groups, we first tried to predict game choice with a logistic regression using symptoms and the interaction between symptoms and trailer design. Next, the remaining dependent variables were predicted using symptoms and the interaction between symptoms and game choice as predictors. All descriptives and statistics can be found in Table 5, with the exception of the three-way interactions for fun and attractiveness and the RM-ANOVAs for change in affect.

Symptom Severity

For game choice, symptom severity was not found to be related either directly [X^2 (1, n = 129) = .86, p = .35] or in interaction with trailer design [X^2 (1, n = 129) = 0.51, p = .48]. Thus, participants without severe symptoms were equally likely to select the mental health game (58.0%) as participants with severe symptoms (63.4%).

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However, further analyses did show effects of symptom severity on competence and autonomy. A direct effect was found for competence, demonstrating that participants with severe symptoms experienced more competence in the game than participants without severe symptoms regardless of game choice. Additionally, both a direct and an interaction effect was found for autonomy. Together these effects show that for those who selected the entertainment game, no difference was found on autonomy for participants with or without severe symptoms [$F(1, 125) = 0.02$, $p = .90$, $\eta^2_p < .01$]. However, when participants selected the mental health game, participants with severe symptoms experienced more autonomy in the game than participants without severe symptoms [$F(1, 125) = 11.43$, $p < .001$, $\eta^2_p = .08$].

Moreover, we observed that affect was lower prior to, but not after, gameplay in participants with severe symptoms than in participants without severe symptoms. Further analyses showed an interaction of time × symptom severity [$F(1, 122) = 4.16$, $p < .05$, $\eta^2_p = .03$], but no three-way interaction: time × game choice × symptom severity [$F(1, 122) = 0.83$, $p = .36$, $\eta^2_p = .01$]. *Post hoc* tests suggested that there was an increase in affect for participants with severe symptoms [$F(1, 122) = 10.30$, $p < .01$, $\eta^2_p = .08$], but affect did not change for participants without severe symptoms [$F(1, 122) = 1.29$, $p = .26$, $\eta^2_p = .01$]. In sum, participants with more severe symptoms experienced less positive affect prior to gameplay, but showed an increase in positive affect, resulting in equal positive affect for participants with more and less severe symptoms after gameplay. Symptom severity had no further effects on the dependent variables.

Depressive Symptoms

Elevated depressive symptoms did not influence game choice either directly [X^2 (1, $n = 129$) = 2.69, $p = .10$] or in interaction with trailer design [X^2 (1, $n = 129$) = 1.22, $p = .27$], which indicates that there was no difference in the odds for participants with and without elevated depressive symptoms to select the mental health game (62.7% and 54.3%, respectively).

Also, depressive symptoms did not predict dependent variables except for affect. Elevated depressive symptoms were related to less positive affect both before and after gameplay. Further analyses showed that there was an interaction for time × elevated depressive symptoms on affect [$F(1, 122) = 4.40$, $p < .05$, $\eta^2_p = .04$], indicating that affect became more positive for participants with elevated depressive symptoms [$F(1, 122) = 12.45$, $p < .001$, $\eta^2_p = .09$], while affect did not change for participants without elevated depressive symptoms [$F(1, 122) = .01$, $p = 0.94$, $\eta^2_p < .01$]. There was no three-way interaction: time × game choice × elevated depressive symptoms [$F(1, 122) = 3.68$, $p = .06$, $\eta^2_p = .03$]. This effect replicates what we previously saw for severe symptoms.

Anxiety Symptoms

We found no direct effect of anxiety symptoms [X^2 (1, $n = 129$) = 0.46, $p = .50$], nor an interaction effect of anxiety symptoms × trailer design on game choice [X^2 (1, $n = 129$) = 0.34, $p = .56$]. Thus, anxiety symptoms did not predict how often the mental health trailer was selected (non-elevated symptoms: 59.5%; elevated symptoms: 59.8%).

Moreover, no main or interaction effects of anxiety symptoms with game choice were found on gameplay duration, intrinsic motivation, autonomy, competence, attractiveness, or affect before and after gameplay. However, there was a significant effect of anxiety symptoms on perceived fun of the trailers, showing that participants with elevated anxiety symptoms rated the games as less fun than participants without these symptoms based on the trailers. Further analyses did not show an interaction of time × anxiety symptoms [$F(1, 122) = 1.29$, $p = .26$, $\eta^2_p = .01$] or a three-way interaction of time

× game choice × anxiety symptoms [F(1, 122) = 0.62, p = .43, η^2_p = .01] on affect. Thus, anxiety symptoms did not influence the change in affect after gameplay.

Stress Symptoms

Finally, elevated stress symptoms did not influence the odds of selecting the mental health or the entertainment trailer directly [X^2 (1, n = 129) = 2.07, p = .15], nor in interaction with trailer design [X^2 (1, n = 129) = 3.25, p = .07]. Therefore, it appears participants with elevated stress symptoms were no more likely to select the mental health trailer than participants without elevated stress symptoms (58.9% and 60.7%, respectively).

Furthermore, no significant direct or interaction effects of stress symptoms were found on gameplay duration, intrinsic motivation, autonomy, competence, perceived attractiveness, fun, or affect before and after gameplay. Moreover, there was no interaction of time × stress symptoms [F(1, 122) < 0.01, p = .96, η^2_p < .01] nor a three-way interaction of time × game choice × stress symptoms [F(1, 122) = 0.28, p = .60, η^2_p < .01] for affect. Thus, participants with elevated stress symptoms did not react differently to the game than participants without elevated stress symptoms.

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Affect

As previous analyses showed that affect only improves in participants with severe symptoms and participants with elevated depressive symptoms, but not in other participants, we examined if severe depressive symptoms are driving the effect. Therefore, we performed RM-ANOVAs for change in affect using only severe symptoms within each type of mental health symptoms as a predictor. These analyses showed that only those with severe depressive symptoms increased in affect [F(1, 122) = 5.65, p < .05, η^2_p = .04], in contrast to those with severe anxiety [F(1, 122) = 0.82, p = .37, η^2_p = .01] or severe stress symptoms [F(1, 122) = 0.06, p = .82, η^2_p< .01]. This significant interaction is also reflected by the fact that participants with severe depressive symptoms only scored lower than participants without severe depressive symptoms before gameplay [M_{severe} = 3.30, SE_{severe} = 0.18; $M_{non\text{-}severe}$ = 3.76, $SE_{non\text{-}severe}$ = 0.07; F(1, 122) = 5.62, p < .05, η^2_p = .04] and not after gameplay [M_{severe} = 3.86, SE_{severe} = 0.17; $M_{non\text{-}severe}$ = 3.87, $SE_{non\text{-}severe}$ = 0.06; F(1, 122) < 0.01, p = .96, η^2_p < .01).

Table 5 Descriptives (means and standard deviations) and ANOVA results: main effect of symptoms and interaction with messaging

	Total					Mental health		Entertainment				
	Non-elevated *M (SD)*	**Elevated** *M (SD)*	***F***	***df***	η^2_p	**Non-elevated** *M (SD)*	**Elevated** *M (SD)*	**Non-elevated** *M (SD)*	**Elevated** *M (SD)*	***F***	***df***	η^2_p
Symptom severity												
Attractive	5.98 (1.54)	6.10 (1.28)	0.21	1, 125	< .01	6.17 (1.84)	5.98 (1.68)	5.78 (1.86)	6.22 (1.60)	**3.39†**	1, 125	.03
Fun	5.78 (1.56)	6.04 (1.35)	0.83	1, 127	< .01	5.89 (1.79)	5.93 (1.68)	5.67 (1.84)	6.15 (1.59)	1.99	1, 125	.02
Affect before	3.83 (0.72)	3.45 (0.78)	**6.66***	1, 122	.05	3.92 (0.67)	3.48 (0.87)	3.70 (0.74)	3.40 (0.63)	0.22	1, 122	< .01
Affect after	3.92 (0.71)	3.80 (0.56)	0.44	1, 122	< .01	4.04 (0.54)	3.76 (0.60)	3.76 (0.86)	3.87 (0.52)	2.28	1, 122	.02
Duration play	28.25 (9.19)	28.79 (7.93)	0.02	1, 124	< .01	28.21 (9.38)	29.60 (7.64)	28.31 (9.06)	28.05 (8.50)	0.44	1, 124	< .01
Motivation	5.19 (1.07)	5.52 (0.87)	**2.97†**	1, 125	.02	5.21 (1.06)	5.50 (0.96)	5.15 (1.09)	5.54 (0.70)	0.08	1, 125	< .01
Autonomy	4.63 (1.21)	5.20 (0.89)	**4.80***	1, 125	.04	4.50 (1.19)	5.41 (0.79)	4.80 (1.24)	4.84 (0.97)	**3.97***	1, 125	.03
Competence	4.87 (1.20)	5.34 (0.91)	**5.13***	1, 125	.04	4.84 (1.24))	5.27 (0.95)	4.90 (1.16)	5.47 (0.85)	0.10	1, 125	< .01
Depressive symptoms												
Attractive	6.12 (1.63)	5.96 (1.36)	0.23	1, 125	< .01	6.22 (1.92)	6.05 (1.72)	6.02 (1.97)	5.87 (1.69)	0.03	1, 125	< .01
Fun	5.77 (1.60)	5.91 (1.45)	0.31	1, 125	< .01	5.84 (1.83)	5.93 (1.72)	5.70 (1.91)	5.89 (1.70)	0.05	1, 125	< .01
Affect before	4.02 (0.61)	3.53 (0.76)	**16.43*****	1, 122	.12	4.00 (0.71)	3.65 (0.78)	4.05 (0.50)	3.32 (0.70)	2.04	1, 122	.02
Affect after	4.04 (0.60)	3.79 (0.69)	**4.47***	1, 122	.04	4.16 (0.47)	3.84 (0.59)	3.90 (0.70)	3.71 (0.82)	0.27	1, 122	< .01
Duration play	26.96 (9.03)	29.25 (8.58)	1.24	1, 124	.01	25.40 (8.54)	30.30 (8.54)	28.82 (9.45)	27.52 (8.50)	**3.65†**	1, 124	.03
Motivation	5.11 (1.16)	5.39 (0.92)	2.64	1, 125	.02	5.19 (1.20)	5.37 (0.95)	5.01 (1.14)	5.43 (0.88)	0.42	1, 125	< .01
Autonomy	4.63 (1.28)	4.91 (1.07)	1.44	1, 125	.01	4.52 (1.35)	4.95 (1.02)	4.76 (1.19)	4.85 (1.16)	0.63	1, 125	< .01
Competence	4.93 (1.17)	5.06 (1.12)	0.30	1, 125	< .01	4.81 (1.33)	5.07 (1.08)	5.08 (0.95)	5.05 (1.21)	0.44	1, 125	< .01
Anxiety symptoms												
Attractive	6.35 (1.18)	5.86 (1.56)	**3.73†**	1, 125	.03	6.48 (1.45)	5.93 (1.91)	6.21 (1.76)	5.78 (1.79)	0.70	1, 125	.01
Fun	6.30 (1.18)	5.65 (1.59)	**6.09***	1, 125	.05	6.36 (1.45)	5.68 (1.85)	6.24 (1.53)	5.62 (1.85)	0.38	1, 125	< .01

	Total					Mental health		Entertainment				
	Non-elevated M (SD)	Elevated M (SD)	F	df	η^2_p	Non-elevated M (SD)	Elevated M (SD)	Non-elevated M (SD)	Elevated M (SD)	F	df	η^2_p
Affect before	3.54 (0.81)	3.79 (0.71)	**2.93†**	1, 122	.02	3.58 (0.83)	3.86 (0.73)	3.47 (0.80)	3.69 (0.68)	0.05	1, 122	< .01
Affect after	3.80 (0.68)	3.92 (0.66)	0.47	1, 122	< .01	3.79 (0.59)	4.02 (0.55)	3.82 (0.81)	3.77 (0.77)	1.20	1, 122	.01
Duration play	30.82 (8.42)	27.26 (8.76)	**3.67†**	1, 124	.03	32.15 (7.52)	26.99 (8.94)	28.85 (9.49)	27.65 (8.61)	1.43	1, 124	.01
Motivation	5.44 (1.03)	5.22 (1.01)	1.12	1, 125	.01	5.51 (0.92)	5.21 (1.08)	5.34 (1.19)	5.22 (0.92)	0.24	1, 125	< .01
Autonomy	4.86 (1.15)	4.79 (1.15)	0.07	1, 125	< .01	4.88 (1.14)	4.78 (1.16)	4.82 (1.21)	4.81 (1.16)	0.04	1, 125	< .01
Competence	5.08 (1.25)	4.99 (1.08)	0.15	1, 125	< .01	5.07 (1.34)	4.95 (1.07)	5.10 (1.13)	5.05 (1.11)	0.02	1, 125	< .01
Stress symptoms												
Attractive	5.98 (1.37)	6.04 (1.53)	0.13	1, 125	< .01	6.21 (1.68)	6.03 (1.87)	5.75 (1.70)	6.05 (1.86)	1.16	1, 125	.01
Fun	5.74 (1.56)	5.95 (1.45)	0.77	1, 125	.01	5.91 (1.75)	5.89 (1.76)	5.57 (1.80)	6.01 (1.74)	1.39	1, 125	.01
Affect before	3.72 (0.71)	3.69 (0.78)	0.04	1, 122	< .01	3.78 (0.71)	3.76 (0.82)	3.64 (0.73)	3.60 (0.72)	< 0.01	1, 122	< .01
Affect after	3.91 (0.65)	3.86 (0.68)	0.08	1, 122	< .01	4.00 (0.51)	3.90 (0.62)	3.77 (0.81)	3.80 (0.76)	0.25	1, 122	< .01
Duration play	28.59 (9.24)	28.30 (8.48)	< 0.01	1, 124	< .01	29.47 (9.14)	28.08 (8.58)	27.25 (9.43)	28.63 (8.84)	0.74	1, 124	.01
Motivation	5.20 (1.10)	5.36 (0.95)	0.94	1, 125	.01	5.27 (1.20)	5.34 (0.89)	5.09 (0.93)	5.39 (1.05)	0.38	1, 125	< .01
Autonomy	4.66 (1.30)	4.93 (1.02)	1.49	1, 125	.01	4.64 (1.31)	4.95 (0.99)	4.70 (1.30)	4.90 (1.06)	0.06	1, 125	< .01
Competence	5.11 (1.11)	4.95 (1.15)	0.79	1, 125	.04	5.02 (1.27)	4.96 1.08)	5.24 (0.82)	4.93 (1.27)	0.37	1, 125	< .01

Notes. [a] df = (1, 125), [b] df = (1,124), [c] df = (1, 122), † p < .10, * p < .05, ** p < .01, *** p < .001, significant effects are printed in bold.

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DISCUSSION

The current study examines an important, rarely addressed, factor in the potential success of therapeutic games: how we present and promote therapeutic games. With the number of interactive media interventions for mental health growing rapidly, it is vital to understand how these interventions are going to be accepted outside of research and clinical settings.

Messaging and Game Choice

Our findings show that young adults with mild to severe mental health symptoms were almost four times more likely to select a game when it was explicitly promoted as beneficial for mental health compared to when it was promoted as entertaining. The most important conclusion we can draw from these results is that explicit mental health messaging did not deter young adults with mild to severe mental health symptoms from selecting a game, but in fact made the game more appealing to select. Overall, nearly 60% of participants selected the game they believed to be a mental health game. Given that regular professional help is sought by only approximately a quarter to a third of people with a diagnosable disorder (Alonso et al., 2004; Merikangas et al., 2011), it is encouraging that in a sample with elevated mental health symptoms three in five young adults showed interest in playing a game that purported to benefit their mental health.

Additionally, the results show that participants were almost six times more likely to select the game promoted with the mental health message in the detailed trailer as they were to select the mental health message in the abstract trailer. In our attempt to create two distinguishable trailers for the same video game we unintentionally created a version that was perceived as more attractive and fun. Although not manipulated as such, this incidental finding may suggest that both trailer messaging and trailer design are important factors in game choice. Accordingly, mental health games will likely be more successful if their trailer design is equally as appealing or even more appealing than the design of commercial videogames.

Furthermore, exploratory results show that symptom type did not predict game choice, suggesting that results are not specific to disorders and apply to internalising issues in general. Interestingly, the same is found for symptom severity. Thus, youth with more severe symptoms were not more likely to turn to a mental health game, while most other studies indicate that people with severe symptoms are more likely to seek professional help (Merikangas et al., 2011; Sawyer et al., 2012). However, our result is in line with other studies that have shown that severity of depressive symptoms had no influence on help-seeking (Merikangas et al., 2011; Chin et al., 2015). Yet, considering that severe depressive symptoms have been found to decrease informal help-seeking (Sawyer et al., 2012; Chin et al., 2015), another explanation for our finding may lie in the undefined nature of mental health games. Mental health games may be interpreted as formal, informal or a separate category of help. This may have resulted in participants reacting to the therapeutic video game differently based on their interpretation. However, it is also possible that for mental health games, severity counteracts existing differences in help-seeking between those with more and less severe symptoms by decreasing barriers for help (Granic et al., 2014).

Moreover, exploratory analyses showed that the attractiveness and fun young adults expected of the games based on the trailers was not predicted by symptom severity or symptom type with the exception of anxiety symptoms. Participants with elevated anxiety symptoms rated both the entertainment and the mental health game as less fun based on the trailers than participants without anxiety symptoms. This may indicate that individuals more prone to anxiety are apprehensive of video games. Alternatively, a third variable (e.g., intolerance of uncertainty), which influences both the perceived fun of video games and the level of anxiety symptoms, may explain why participants with elevated anxiety symptoms expected the games to be less fun irrespective of messaging.

Thus, besides the influence of the trailer design the analyses provided no indication of why messaging differentially influenced participants. As two in five participants chose the entertainment game over the mental health game it is meaningful to explore why they may have made this decision and what may enhance the success of mental health messaging. First, participants may have experienced reactance or perceived stigma in response to the mental health trailer (e.g., one participant indicated that the mental health game suggested that there was ‘something wrong with me’) and therefore may have chosen the entertainment game instead. Previous research has shown that reactance may be limited both by warning people ahead of the persuasive intent of a health message (Richards & Banas, 2015) as well as by restoring their sense of autonomy after the message (Bessarabova et al., 2013). Yet, stigma is a broader societal problem, and evoking a sense of stigma cannot easily be avoided within an explicit mental health message.

Alternatively, participants may have selected the entertainment game to improve their mood in line with the mood management model (Zillmann, 1988) or to avoid potential mood damaging effects of the mental health game in line with the escape model (Perse, 1998; Oliver, 2009). Both motivations appear plausible. One participant explained her choice for the entertainment game indicating 'If everybody likes it, it is very likely I will like it too' supporting a mood management perspective. Whereas another participant argued the mental health game 'looks frustrating and more complicated' supporting an escape perspective. This suggests that explicit mental health games emphasizing positive game experiences would be even more attractive.

Finally, self-determination theory (Ryan & Deci, 2000) suggests participants may have chosen the entertainment game as the mental health game was not in line with their intrinsic motivations (e.g., one participant explained 'I did not feel stressed or bad at the moment'). Though for some individuals beliefs about personal relevance may depend on their current state, suggesting that the mental health game may be selected at another point in time, others may never identify reducing stress as an intrinsic need. Thus, proclaiming to meet multiple needs may increase the chances of mental health games matching individual needs. Moreover, it will be valuable to further study motivations for mental health or entertainment game selection to inform strategies to attract even more young adults to mental health games.

Game Experience

Besides testing the influence of messaging on game choice, the current study also looks at how the selected message influenced game experience. Young adults played Monument Valley for approximately 28 min regardless of game choice, mental health symptom severity or type. Additionally, as hypothesised, young adults who selected the mental health game experienced similar intrinsic motivation, autonomy and competence compared to those who selected the entertainment trailer. In contrast, exploratory analyses showed that although young adults who selected the entertainment message experienced similar autonomy, participants with severe symptoms who selected the mental health message felt more autonomous compared to participants without severe symptoms, confirming our expectation. In a similar vein but contrary to our expectations, participants with severe symptoms felt more competent than participants without severe symptoms, regardless of whether they believed the game was an entertainment or mental health game.

Finally, as expected, the current study shows that overall participants improved in affect over time, regardless of the message they selected. However, exploratory results suggest that this effect was driven by participants with severe depressive symptoms rather than those with no to moderate depressive symptoms or other severe symptoms. Thus, although we had expected participants with elevated depressive symptoms to experience more positive affect, the game increased positive affect only for those youth who reported severe depressive symptoms.

The current results replicate several of the findings from a previous study in which a mostly healthy sample of young adults were exposed to either a mental health or an entertainment-focused introduction message (Poppelaars, Lichtwarck-Aschoff, et al., 2018). Both studies show that mental health messaging does not influence intrinsic motivation and affect. Both studies also show that positive affect increased for those with (severe) depressive symptoms, while there was no change in affect for those without (severe) depressive symptoms (i.e., using SAM). Perhaps most importantly, the current findings extend the previous one by indicating that these effects are particularly relevant for youth with severe depressive symptoms.

It is promising that those who are most affected by depressive symptoms show at least a short-term boost in positive affect. In general people with depressive symptoms report fewer positive experiences in their daily life (Peeters et al., 2003; Bylsma et al., 2011) and also have been shown to react with less positive affect to positive experiences in experimental settings (Bylsma et al., 2008). However, naturalistic studies show a mood-brightening effect in people with depressive symptoms, indicating that they may in fact be more sensitive to positive experiences in daily life and respond with more positive and less negative affect (Peeters et al., 2003; Bylsma et al., 2011). Thus, our results are in line with the mood-brightening effect in young adults with (severe) depressive symptoms. Temporary positive affect may partially explain why casual commercial games have been shown to reduce depressive symptoms (Russoniello et al., 2013). That is, according to the broaden-and-built theory, momentary positive emotions can create an upward spiral, broadening one's perspective thus allowing one to seek out positive experiences, leading to more opportunities for positive emotions (Fredrickson, 2001). This upward spiral could eventually reduce depressive symptoms as depressed mood is replaced by a more positive mood.

Just like for mood, messaging had no effect on competence. However, exploratory analyses indicated that participants with more severe symptoms experienced enhanced competence following gameplay. This may reflect that youth with severe symptoms selected either the entertainment or the mental health game based on their capacity to deal with their issues and felt competent in the selection they made. Moreover, Nezlek and Gable (2001) propose that mental health issues are associated with unstable self-worth which increases sensitivity to daily events that may reflect on self-worth.

As Monument Valley is designed for successful completion and triumphs are more visible (i.e., the avatar's path becomes visible) than failures, participants with more severe symptoms may experience more pronounced competence after playing Monument Valley compared to participants with less symptoms and more stable self-worth.

The current study revealed no negative effect of mental health messaging on autonomy. These results may indicate that the ability to actively choose a game with a mental health message reduces the potential reactance evoked by the messaging. In fact, reactance to persuasive messages can be limited by providing a text confirming the choice freedom of individuals (Bessarabova et al., 2013), and the actual behavioural choice provided here may be even more successful in reestablishing a sense of choice freedom. Alternatively, we may hypothesise that the participants who felt that the mental health message was controlling and who would have therefore experienced less autonomy in the game, selected the entertainment game and so avoided experiencing less autonomy. Additionally, both explanations could work in unison.

Moreover, exploratory analyses indicated that when the mental health game was selected, participants with severe mental health symptoms experienced more autonomy than participants without severe symptoms. Thus, suggesting that mental health messaging can stimulate a sense of autonomy. Possibly, participants with severe mental health symptoms felt the mental health aim to be especially in line with their own values and motivations, thus leading to an enhanced sense of autonomy when choosing this option. However, this did not result in the expected accompanying increase in intrinsic motivation.

Strengths, Limitations, and Future Directions

Besides focusing on a societally relevant research question, the current study has some additional strengths. First, our design is an important strength to highlight. We provided participants with a controlled choice between an explicit mental health and a stealth entertainment message that in reality promoted the same game. Participants were able to freely choose and play the game for as long as they liked (within the study's time-constraints) creating a genuine game experience and an objective measure of engagement, avoiding self-report biases that come with post-game questionnaires. Second, the counterbalanced presentation of the two trailer designs in which the messages were shown allowed us to separate the effects of messaging from the perceived attractiveness of the trailers. Third, we could relate any differences in game play experiences to messaging because all players played the same commercial game and because there was no contamination of any in-game mental health content, as would be the case with most existing therapeutic games. Finally, the current study included at-risk participants with elevated mental health symptoms, arguably the most relevant target audience for therapeutic games. Thus, our sample is likely representative of the audience who would be seeking such games and serious game designers with mental health targets would benefit from incorporating our messaging results.

Naturally, the study also has a number of limitations. The sample overrepresented highly educated young adults, females, and social science students due to our recruitment strategy. Highly educated social science students may be more open to and curious about innovative mental health interventions, which may have inflated the interest for the mental health game. Similarly, women may potentially be more attracted to mental health games than men, as a recent review indicates that gender influences gaming motives and behaviour (Veltri et al., 2014), which could have skewed our results. Thus, it is advisable for future research to include men and women equally, to target a more diverse group of young adults, as well as to target other age groups as children may react differently to messaging.

Another limitation to consider is the experimental setting in which participants selected and played the game. First, the length of the experiment limited gameplay to a single session under 45 min. Vital differences in gameplay patterns between mental health and entertainment games may only become visible when repeated gameplay is possible and sessions are not artificially limited. Therefore, future research is needed that observes naturalistic gameplay over a matter of weeks or even months.

Second, participants may still have selected a different game than they would have outside the lab due to several experimental factors. The choice within the experiment was limited to two games, rather than the reality of almost unlimited video game choice, in which one mental health game may not even attract enough attention to be considered as an option. Moreover, participants may have behaved in a socially desirable way when choosing a game, knowing that their choice was recorded. On the one hand, participants may have assumed that selecting the mental health game (i.e., an uncommon game type) was preferred by the experiment leader. On the other hand, participants may have felt that selecting the mental health game indicated needing a therapeutic intervention and therefore selected the more normative entertainment game. A final way the experimental setting may limit the generalisability of this study is the fact that the experimental environment may have encouraged participants to make a more thoughtful choice, potentially anticipating that they would have to explain their choice. In everyday situations, game choice may be a more instinctive choice (i.e., based on unconscious decision processes) as well as a more implicit choice (i.e., multiple games may be played and selecting game X does not rule out playing game Y).

Consequently, future research exploring more promotion channels for therapeutic games and the relative success of explicit and stealth messaging will be valuable. Youth can be recommended video games through a myriad of sources such as friends, blogs, forums, online video game stores (e.g., Steam), video game news and review sites and popular video game players who demonstrate games online (e.g., on YouTube or Twitch). Research on any of these sources of recommendations can help clarify if messaging may need to be adapted per situation. Additionally, alternative messaging may be explored. For example, messaging may combine entertainment and mental health messaging in various proportions or alternatively explicit messaging may describe causes of mental health issues rather than the effects of the game. A recent study shows that young adults believed a mental health app to be more useful and had higher intentions to use it if a prior message emphasized internal causes of depression (Khan & Peña, 2017).

Furthermore, in real-life youth may only play a video game after receiving recommendations from multiple sources (e.g., a friend, review site and popular gamer recommending the same game) and future research may study this complexity (see Konijn et al., 2013 for methods to use YouTube for research purposes). In order to enhance the generalisability of the research results further, researchers may provide more choice to youth. Certainly, if youth are presented with an entire webpage of video games, which may easily contain 50 video games, the chances of them selecting a single mental health game will be lower than the almost 60% found in the present study. However, given the fact that youth would not necessarily only play one game recommended on such a page, it will be critical to see if youth will consider mental health games in such a scenario and what aspects influence the likelihood for youth to play therapeutic games.

In addition to messaging, individual characteristics besides symptoms may also influence game choice and experience. The current study did not assess if participants were diagnosed with a mental health disorder nor if they themselves believed that they had a mental health disorder. Participants who are aware of a mental disorder may be more likely to select a mental health game as they feel that is meant for them in comparison to participants who do not identify their symptoms as a coherent mental issue. Additionally, game choice is likely affected by personal motivational factors, the individual's believes concerning the value of video games and their beliefs about mental health disorders. For example, someone who considers video games violent and a waste of time may be much less likely to find a mental health game credible than someone who believes video games can be educational and foster social connections. Similarly, the stigma that one experiences surrounding mental health or the extent to which one believes mental disorders cannot effectively be treated can limit the effectiveness of mental health messages. Thus, future research may examine which individual factors influence the choice between explicit and stealth messaging.

Finally, future research will need to study the effects of messaging on game effectiveness. Naturally, games that are not played cannot be effective. However, while maintaining the attractiveness of the game, the promotion may also aim to enhance effectiveness. Intrinsic motivation for playing a game may enhance its effectiveness. Furthermore, explicit mental health messaging promising health benefits may enhance the effectiveness of therapeutic games through the positive expectations of the player (Enck et al., 2013).

Conclusion

Therapeutic games, other serious games (e.g., educational games) and other self-administered interventions will have the biggest impact if they are widely available and can motivate people to seek out and stay engaged in these interventions. Thus, it is critical to consider how messaging in the promotion of therapeutic games can attract youth and support an immersive game experience. The current study indicates that once effective therapeutic games targeting youth outside of a clinical setting are developed, these games may indeed be attractive to youth, especially when promoted in an engaging trailer that includes an explicit mental health message.

This study has several implications for the design of games that target a wide range of mental health issues and the messaging used in these games. First, as youth with the most severe symptoms in our study were found to have a better game experience in terms of autonomy, competence, and affect, special attention needs to be paid to youth with mild to moderate symptoms. When designing therapeutic games for youth with mild to moderate symptoms, supporting autonomy and competence either in game design or in promotional messaging is especially important, given that these factors predict a more positive game experience (Ryan et al., 2006). Second, designers of therapeutic games may search for ways to improve mood for youth without severe depressive symptoms to enhance engagement. This is especially important if the game's effectiveness relies partially on short-term increases in mood.

Finally, this study indicates that elevated anxiety symptoms were related to expecting less fun from video games. Thus, it may valuable for those developing anxiety reducing therapeutic games to study if promoting these games in a different way makes them appear more fun and so allows youth to take the initiative to play therapeutic games.

Many research questions remain regarding the promotion of therapeutic games, but so far the results support the idea that explicitly marketing games as beneficial to mental health may not turn youth away from these games. Instead, they may provide youth with the opportunity to improve their well-being in an autonomous and engaging manner.

Chapter 7

The role of motivation to change and mindsets in a game promoted for mental health

Based on:
Wols, A., Poppelaars, M., Lichtwarck-Aschoff, A., & Granic, I. (2020).
The role of motivation to change and mindsets in a game promoted for mental health.
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ABSTRACT

Anxiety and depression are the most prevalent mental health disorders among young adults, leading to debilitating outcomes. Recently, there has been an increasing interest in the use of video games to prevent and treat mental health problems. Insight into the motivational factors that influence game selection and engagement will inform us on how to tailor the promotion of mental health games. The primary aims of the current study were to examine whether motivation to change, emotion mindset, and stress mindset influenced (a) the choice for, and (b) engagement with, a game promoted as a mental health game. The secondary aim was to examine whether emotion and stress mindsets changed after playing a game promoted as a mental health game. Young adults (n = 129; 95 women; 82.2% university students) with elevated mental health symptoms viewed two trailers, in which the same commercial video game was presented as a mental health game and as an entertainment game. Results showed that motivation to change and mindsets did not influence game choice, and participants who chose the mental health trailer played the game for a similar period of time as participants who chose the entertainment trailer, regardless of their motivation to change or mindsets. Additionally, after gameplay participants who chose the mental health trailer reported a decrease in the belief that the effects of stress are debilitating. These results suggest that video games aiming to improve mental health may benefit from promoting the game's mental health benefits.

Anxiety and depression are the most prevalent mental health disorders among young adults, with up to 11.7% and 15.6% of young adults reporting, respectively, an anxiety disorder or major depressive episode in the past year (De Graaf et al., 2012; Kessler & Walters, 1998). These mental health disorders lead to high societal costs, as well as debilitating and often devastating long-term outcomes for the individual (WHO, 2001). Recently, there has been a strong increase in the use of e-health applications to prevent and treat mental health problems, including – but not limited to – video games for mental health. Video games have been proposed as a viable alternative for delivering interventions, due to their natural appeal and intrinsic motivational characteristics (Fleming et al., 2017; Granic et al., 2014; Kazdin, 2015). Games may have the potential to engage individuals and keep them engaged for longer periods of time than conventional programmes. Additionally, video games can be a cost-effective alternative because of their potential for scalability, reaching individuals most in need with little cost and effort (for recent reviews see e.g., Eichenberg & Schott, 2017; Lau et al., 2017).

Despite these proposed advantages of mental health games, limited research has examined uptake and adherence among individuals with mental health symptoms (Fleming et al., 2018; Fleming et al., 2016). Additionally, we do not know how individuals with different motivations engage with mental health games (Fleming et al., 2017). Knowledge about whether and how motivational factors influence the selection of, and engagement with, mental health games will allow us to tailor the promotion of mental health games such that we can optimize uptake and adherence, eventually broadening the impact of mental health games on population mental health. The primary aims of the current study were to examine how motivational factors influence (a) the willingness to play, and (b) subsequent levels of engagement with, a game promoted as a mental health game, among young adults with mental health symptoms. The secondary aim was to examine whether mindsets change after playing a game promoted as a mental health game.

Motivation to Change

Motivation to change refers to individuals' willingness to change symptoms or problems they are experiencing. In the clinical literature, motivation to change has been identified as an important predictor for help-seeking, treatment engagement, adherence, dropout, and treatment outcomes (e.g., Beitman et al., 1994; Brogan et al., 1999; Dozois et al., 2004; Lewis et al., 2012; Lewis et al., 2009; Lichtenberg & Hummel, 2000; May et al., 2007; Norcross et al., 2011; Prochaska & Norcross, 2001; Taylor et al., 2012; Vogel et al., 1997 in

Greenstein et al., 1999). In the transtheoretical model (TTM) of behavioural change (Prochaska & DiClemente, 1982) motivation to change is conceptualized as a process that involves progression through five stages (Norcross et al., 2011): 1) precontemplation (not being aware of any problems, no intention to change behaviour), 2) contemplation (awareness about problems that need to be changed), 3) preparation (intention to seek help), 4) action (actively working on behavioural change), and 5) maintenance (preventing relapse and continuing to implement newly learned skills). The TTM proposes that clients' treatment adherence and engagement, as well as outcomes depend on the client's stage of motivation.

Previous research among university students with self-identified anxiety issues found that higher precontemplation scores were associated with reduced help-seeking and that higher contemplation, action and maintenance scores were associated with greater help-seeking (Dozois et al., 2004). In addition, research among psychotherapy patients has found that higher precontemplation, lower contemplation and lower action scores were associated with premature termination of therapy (Brogan et al., 1999; Derisley & Reynolds, 2000; Dozois et al., 2004; Smith et al., 1995). Based on this literature, we expected that motivation to change would also influence individuals' willingness to play, and engagement with, a game promoted as a mental health game.

Emotion and Stress Mindsets

Two other important motivational factors that might influence individuals' willingness to play, and engagement with, a game promoted as a mental health game are their mindsets about the malleability of emotions (i.e., emotion mindset) and the nature of stress (i.e., stress mindset). Individuals with a *fixed emotion mindset* believe that emotions are not changeable and cannot be controlled. Individuals with a *growth emotion mindset* believe that emotions are malleable and can be changed with effort, experience, and help from others (Dweck, 1999; Tamir et al., 2007). Regarding stress mindset, individuals with a *stress-is-enhancing* or *stress-is-debilitating mindset* experience stress as enhancing or debilitating, respectively, for health, performance and personal growth (Crum et al., 2013). The concept of mindsets is based on Dweck's (1999) framework in which it is proposed that mindsets – also called implicit theories – determine one's goals, action tendencies, beliefs about effort, and predict one's reaction to setbacks (Dweck, 2017a, 2017b). This makes mindsets an important motivational factor for behaviour (Burnette et al., 2013; Dweck, 2017a), presumably influencing help-seeking and treatment engagement (Burnette et al., 2019; Schroder et al., 2015).

Changing Mindsets

Because mindsets have been thought of as both trait-like and changeable (Dweck, 2008, 2017a) and day-to-day experiences may change one's mindset, the secondary aim of the current study was to examine whether emotion and stress mindsets *change* after playing a game promoted as a mental health game. Previous research has shown that mindsets can be changed through surprisingly brief interventions, such as watching short video clips, reading an article or performing a short writing exercise (Aronson et al., 2002; Chiu et al., 1997; Crum et al., 2013; Dweck, 2008; Miu & Yeager, 2015; Yeager et al., 2014). Other research however suggests that these interventions may not work for everyone and in fact show strongest effects for individuals who are at-risk or confronted with (mental health) challenges (Burnette et al., 2013; Dweck, 2011; Sisk et al., 2018; Yeager et al., 2019). It might be that selecting and playing a game that is promoted for its mental health benefits, changes one's emotion and/or stress mindset.

Hypotheses and Design

The primary aims of the current study were to examine how motivation to change, emotion mindset and stress mindset influenced (a) the choice for, and (b) engagement with, a game promoted as a mental health game. Based on the motivation to change literature, we hypothesised that individuals with lower precontemplation scores, and higher contemplation, action and maintenance scores would (a) be more likely to select and play a game promoted as a mental health game, and (b) would play the game for a longer period of time. Regarding emotion mindset, we hypothesised that individuals with a growth emotion mindset would (a) be more likely to select and play a game promoted as a mental health game than individuals with a fixed emotion mindset, and (b) would play the game for a longer period of time, because individuals with a growth emotion mindset believe that emotions can be changed, are willing to confront challenges, and persist to reach their goal. Because a game aimed at stress and/or emotion management may imply confrontations with stress, we further hypothesised that individuals with a stress-is-debilitating mindset would (a) be less likely to select and play a game promoted as a mental health game than individuals with a stress-is-enhancing mindset, and (b) would play the game for a shorter period of time. The secondary aim of the current study was to examine whether emotion and stress mindsets change after playing a game promoted as a mental health game. By making the mental health benefits of a game explicit, players are presumably more aware of changes in emotions and their stress level, which could have an influence on their mindset concerning the malleability of emotions and/or the nature of stress. Hence, we hypothesised that playing a game promoted as a mental health game would lead to changes in participants' emotion and stress mindsets as a result of the game experience.

In the current study, participants were young adults with elevated levels of mental health symptoms. Participants viewed two trailers, in which the same commercial video game was presented as a mental health game and as an entertainment game. Although participants believed they could choose between two different games, both trailers portrayed the same commercial video game, which allowed us to attribute differences in gameplay duration (as an indication of engagement) to the mental health message, while holding game content equal. The current study was part of a larger study on the impact of different messaging types on the choice and experience of mental health games. A previous publication on this dataset (Poppelaars, Wols, et al., 2018) showed that young adults with elevated mental health symptoms were 3.71 times more likely to select the game introduced as having mental health benefits than the game with the entertainment message.

METHOD

Participants

Participants were 155 young adults (M_{age} = 21.48, SD_{age} = 3.36) with elevated levels of mental health symptoms. Of these 155 participants, 26 were excluded from the analyses because a) they realized that the two trailers were about the same game and/or b) knew the (broader) aim of the study, leaving 129 participants (95 women) for data analyses. Excluded participants reported to be playing video games for more hours per week (M = 7.83, SD = 10.19) than included participants (M = 4.25, SD = 6.77; t(29.60) = -1.72, p = .096), presumably making it more likely for them to notice that the two trailers were about the same video game. Based on a-priori power analyses (power = 80%, α = 0.05, medium effect size), at least 128 participants were required for the analyses.

Participants included in the analyses were between 18 and 31 years old (M_{age} = 21.33, SD_{age} = 3.20) during screening and 49.6% of the participants were born in the Netherlands, 29.5% in Germany, 13.2% elsewhere in Europe, and 7.8% outside Europe. Most participants (82.2%) were currently enrolled in a university program, of which 56.0% studied psychology. Participants indicated to play video games for on average 4.25 hours (*SD* = 6.77) per week. Almost half of the participants (48.8%) did not play video games at all in an average week, 27.9% indicated playing video games up to 7 hours a week, and 23.3% played video games for more than 7 hours a week.

Procedure

Participants were recruited through the university's participant pool system, by handing out flyers at lectures, and by putting up posters/advertisements on campus and on Facebook. The current study consisted of an online screening questionnaire to assess eligibility and – if participants showed elevated mental health symptoms and were unfamiliar with Monument Valley – a subsequent experiment in the lab.

In the online screening, participants (*n* = 648) first read information about the study and gave active consent for the screening prior to filling in the questionnaires. Participants filled out questionnaires regarding demographics, mental health symptoms, motivation to change, mindsets, and video gaming habits. The screening also included filler items on academic performance to disguise the aim of the screening and to avoid demand characteristics. Participants were invited to the lab if they had elevated mental health symptoms (see measures below) and if they were not familiar with the game that was used in the experiment. The online screening took approximately 15 minutes and participants filled out the online screening on a voluntarily basis or in exchange for course credits.

Eligible participants (*n* = 264) were invited to come to the lab within two weeks after filling out the screening questionnaire (range 1 to 20 days, *M* = 8.15, *SD* = 4.52). This invitation was accepted by 155 participants who received information about the study set-up and signed informed consent for the experiment. During the experiment, participants first filled out two questions about their mood, and subsequently watched two trailers after which they had to choose one of the games. Although participants believed they had two game options to choose from, both trailers portrayed the same commercial video game (Monument Valley; Ustwo Games, 2014a). The two trailers promoted the game through different graphic design, musical elements, font choice, and editing style, as a mental health or entertainment game.

After participants made their choice, they rated the attractiveness and fun of each game based on the trailers and played Monument Valley on a tablet (a 10.1-inch Samsung Galaxy Tab 3, GT-P5210). Participants were free to decide how long they wanted to play the game. To ensure that participants did not exceed the 60 minutes time limit for the experiment we asked them to move onto the questionnaire about 50 minutes into the experiment (i.e., after approximately 40 minutes of gameplay) in case they had not done so.

After gameplay, participants filled out questions about, respectively, their mood, intrinsic motivation, autonomy and competence, evaluation of the game and whether they wanted to participate in a raffle to win the game, as well as questions regarding their mindsets, the manipulation check and the promotional message of the trailers. In exchange for their participation, participants received course credits or a €10 gift certificate. Full debriefing was done via email after completion of the study. The Ethics Committee Social Sciences of the university (code number: ECSW2017-3001-461a) approved the study protocol.

Experimental Manipulation: Monument Valley, Trailers and Messaging

Monument Valley is a commercial 3D puzzle game (Ustwo Games, 2014a) inspired by the optical illusions of M.C. Escher (see Figure 1). The game has received excellent reviews on gaming websites and has won several awards (e.g., the Apple Design Award in 2014 and the Apple iPad game of the year in 2014). The game's aim is to guide the protagonist, princess Ida, through mazes of impossible objects and optical illusions. Players progress through the game by rotating and manipulating the architecture and geometry of the game world (Ustwo Games, 2014b). The game consists of several levels that could be finished in approximately 1.5 hours.

We used Monument Valley because it is designed for players with various levels of game experience (Ustwo Games, 2014b). By balancing difficulty and enjoyment the developers aimed to create a game "that would excite the player, but never frustrate" (https://ustwo.com/work/monument-valley). Although the game was not designed with a therapeutic aim, we believe that presenting the game with a mental health message would be credible because of the relaxed atmosphere and the way in which the game illustrates problem solving.

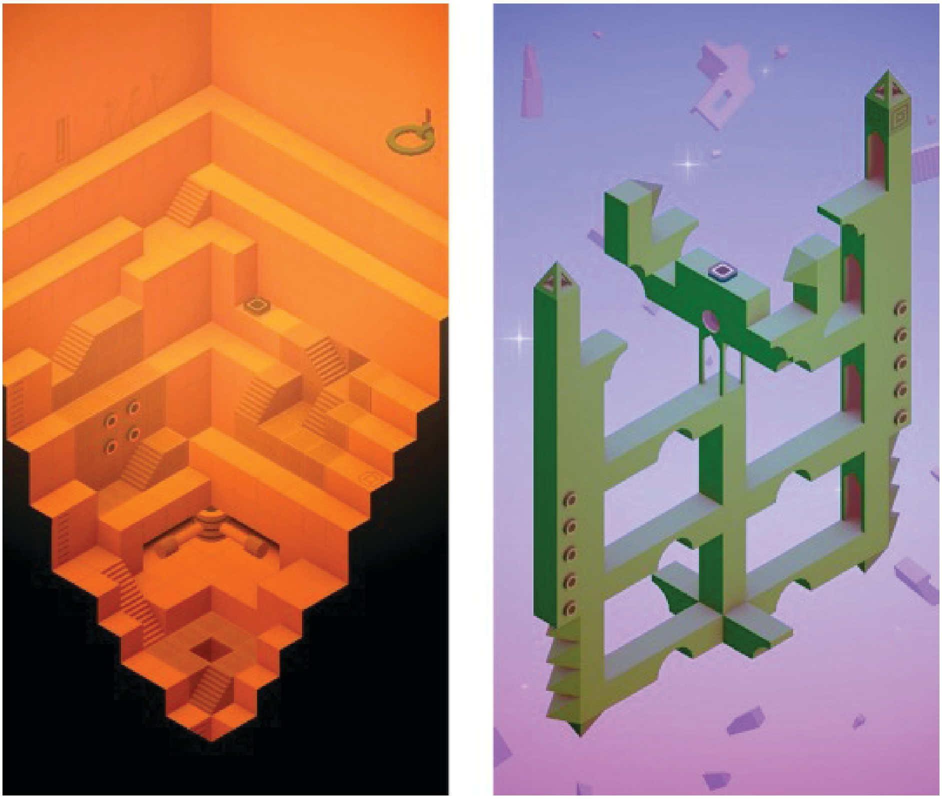
In order to attribute differences in gameplay duration to the mental health message while holding game content equal, two 1-minute trailers were created based on screenshots of Monument Valley. We will refer to these trailers as the *detailed* and the *abstract* trailer (see Figure 2). The trailers were different in several aspects to give the impression that the trailers portrayed two different games. The detailed trailer had screenshots of detailed graphical designs, faster music, AR BONNIE font for the trailer text, and an editing style focused on slowly moving across the pictures. The abstract trailer had screenshots of abstract graphical designs, slower music, Gloucester MT font for the trailer text, and an editing style focused on zooming in or out of the pictures. Based on a pilot study (*n* = 22) we slightly adapted the two trailers to further improve attractiveness and enhance their differences (for more details, see Poppelaars, Wols, et al., 2018).

These two 1-minute trailers were filled with two different messages: a mental health message or an entertainment message. Each message consisted of five sentences, with the first sentence in both messages introducing the game as appealing. The *mental health message* emphasized beneficial effects of the game on players' mental health and included the following sentences: "perfect for a single marathon playthrough", "learn to manage stress more efficiently", "therapeutic insights for emotional mastery", "both challenging and relaxing", and "recommended by games for mental health". The *entertainment message* emphasized the entertainment value of the game and that the game was highly acclaimed. The following sentences were included in the entertainment message: "a game you must play", "think outside the box to solve intricate puzzles", "9/10 Polygon 5/5 Touch Arcade", "almost impossibly gorgeous", and "iPad game of the year". By using two experimental conditions, we counterbalanced between participants which trailer design (i.e., the detailed or abstract trailer) contained the mental health message and which trailer design contained the entertainment message. Participants were randomly assigned to one of the two conditions. In condition 1, participants (*n* = 66) saw the detailed trailer with the mental health messages (see https://www.youtube.com/watch?v=N17yM7c2gu4) and the abstract trailer with the entertainment messages (see https://www.youtube.com/watch?v=eXm0_N_h02w). In condition 2, participants (n = 63) saw the abstract trailer with the mental health messages (see https://www.youtube.com/ watch?v=D5xOB3OFPxQ) and the detailed trailer with the entertainment messages (see https://www.youtube.com/watch?v=UX9fYmWwb24). In both conditions, the trailers were shown in random order.

Figure 1 Screenshots from the video game Monument Valley

Notes. The player's aim is to have the avatar in white reach the top of the building. On the left, the current path seems to be a dead end. On the right is the same building after rotating it counter clockwise. By rotating the building the visual illusion is created that the dead end connects to another path section, allowing the avatar to move on. These images are reproduced from Monument Valley by Ustwo Games (2014a) with the permission of the copyright holder Ustwo Games.

Figure 2 Screenshots from the two trailers of Monument Valley



Notes. On the left, a screenshot from the detailed trailer. On the right, a screenshot from the abstract trailer. These images have been adapted from Monument Valley by Ustwo Games (2014a) with the permission of the copyright holder Ustwo Games.

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Measures

Mental health symptoms

The Depression Anxiety Stress Scale (DASS-21; Lovibond & Lovibond, 1995; Dutch translation from De Beurs, 2010) was used during screening to assess mental health symptoms. This scale consists of three subscales with seven items each to measure depressive symptoms (e.g., "I felt that life was meaningless"; α = .83), anxiety symptoms (e.g., "I felt I was close to panic"; α = .70), and stress symptoms (e.g., "I found it hard to wind down"; α = .79). Participants answered all items on a 4-point Likert scale indicating to what degree each statement applied to them during the past week, with 0 = *did not apply to me at all*, 1 = *applied to me to some degree, or some of the time*, 2 = *applied to me to a considerable degree, or a good part of the time*, and 3 = *applied to me very much, or most of the time*. Participants were selected for the experimental part of the study based on having at least mildly elevated mental health symptoms on one or more of the subscales, indicated by a summed score of at least 5 for depression, at least 4 for anxiety, or at least 8 for stress (De Beurs, 2010).

Motivation to change

The University of Rhode Island Change Assessment (URICA; McConnaughy et al., 1983; Dutch translation from De Jonge et al., 2002) was used during screening to assess motivation to change. The questionnaire consists of four subscales with eight items each, measuring the four primary stages of change within the TTM: precontemplation (e.g., "As far as I am concerned, I don't have any problems that need changing"; α = .77), contemplation (e.g., "I have a problem and I really think I should work at it"; α = .80), action (e.g., "I am really working hard to change"; α = .88), and maintenance (e.g., "I am trying to prevent myself from having a relapse of my problem"; α = .85). The URICA measures only the four stages that have been supported by research among psychotherapy clients and therefore does not include the preparation stage as proposed in the TTM (Mcconnaughy et al., 1989; McConnaughy et al., 1983).

In its original form, eight items of the URICA refer to the specific treatment setting a patient is currently in. Because we administered the questionnaire in a community sample outside of a treatment context, we slightly adapted the wording of these items so that they would fit with both individuals currently considering treatment and individuals currently not considering treatment. For example, the item "Maybe this place will be able to help me" was changed into "Maybe someone or something will be able to help me". Participants indicated on a 5-point Likert scale to what extent they agreed with the statements, ranging from 1 (*strongly disagree*) to 5 (*strongly agree*). Only participants that indicated to have at least mildly elevated mental health symptoms filled out the URICA. Participants were instructed that when the statement referred to "their problem" they should answer in terms of the behaviours, feelings or situations they had identified in the previous questionnaire (i.e., the DASS-21). An average score for each subscale was calculated.

Emotion mindset

The Theory of Emotions Scale (Tamir et al., 2007) was used during screening (*pre-test*) and after gameplay (*post-test*) to assess implicit theories of emotions. The questionnaire was translated into Dutch using a forward-backward method; two researchers translated the questionnaire into Dutch and a third researcher translated it back. Any discrepancies were resolved through discussion. The scale consists of two items measuring the incremental dimension (e.g., "If they want to, people can change the emotions that they have") and two items measuring the entity dimension (e.g., "The truth is, people have very little control over their emotions"). Participants indicated to what extent they agreed with the statements on a 5-point Likert scale, ranging from 1 (*strongly disagree*) to 5 (*strongly agree*). The two entity items were reverse-scored and a mean score across all items was calculated (αpre-test = .72 and αpost-test = .79), such that higher scores indicated a growth mindset (i.e., believing that emotions are malleable).

Stress mindset

The Stress Mindset Measure-General (SSM-G; Crum et al., 2013) was used during screening (*pre-test*) and after gameplay (*post-test*) to assess beliefs about the enhancing and debilitating nature of stress. We translated the questionnaire into Dutch using a forward-backward method. The scale consists of four items measuring negative beliefs about stress (e.g., "Experiencing stress depletes my health and vitality") and four items measuring positive beliefs about stress (e.g., "Experiencing stress facilitates my learning and growth"). Participants indicated on a 5-point Likert scale to what extent they agreed with the statements, ranging from 0 (*strongly disagree*) to 4 (*strongly agree*). The four negative items were reversed and a mean score across all items was calculated (αpre-test = .78 and αpost-test = .84), such that higher scores indicated a stress-is-enhancing mindset.

Game choice

The variable game choice represents participants' choice with regard to the promotional messaging (i.e., mental health or entertainment) regardless of the specific trailer (i.e., detailed or abstract) that incorporated that message.

Gameplay duration

As an indication of engagement with the game, gameplay duration was measured using the Funamo Parental Control app (Funamo Inc, 2016) on the tablets, which tracked how long Monument Valley was open for each participant. In case gameplay duration measured by Funamo was incorrect (e.g., when participants did not close Monument Valley when they continued with the questionnaire), we used the time from an invisible timer on the questionnaire page which tracked when participants continued with the questionnaire. For one participant gameplay duration could not be obtained as both methods proved incorrect. In addition, a standard maximum gameplay duration (i.e., 40.38 minutes) was given to participants who were prompted by the researcher to move on to the questionnaire due to time constraints (n = 25) and to participants who exceeded that standard duration without being stopped (n = 2). For more details see Poppelaars, Wols, et al., 2018.

RESULTS

Descriptive Statistics

Table 1 presents the descriptive statistics for the total sample and for each experimental condition. Randomisation was successful as there were no differences between experimental conditions in age, gender, birth country, weekly video gameplay, mental health symptoms, motivation to change subscales, and emotion and stress mindset at pre-test. However, participants were more likely to choose the mental health message when it was portrayed in the detailed trailer (experimental condition 1) than when it was portrayed in the abstract trailer (experimental condition 2; for information and discussion see Poppelaars, Wols, et al., 2018). Therefore, we controlled for experimental condition in the analyses that predicted game choice and gameplay duration, and in the analyses examining the changes in emotion and stress mindsets.

Overall, 77 of the 129 participants (59.7%) selected the trailer with the mental health message. No significant differences in game choice were found between females and males (χ^2 (1) = 1.80, p = .18). On average, participants played Monument Valley for 28.48 minutes (SD = 8.85), with a range of 12.77 to 40.38 minutes. Table 2 presents means and standard deviations of the study variables by game choice. Participants choosing the mental health trailer and participants choosing the entertainment trailer did not differ in mental health symptoms, motivation to change subscales, emotion mindset at pre- and post-test, stress mindset at pre- and post-test, and gameplay duration.

Table 1 Means and standard deviations or percentages of the demographic and study variables for the total sample and for each experimental condition

			Condition						
			Detailed trailer with mental health messaging		**Abstract trailer with mental health messaging**				
Variable	**Mean / Percentage**	***(SD)***	**Mean / Percentage**	***(SD)***	**Mean / Percentage**	***(SD)***	χ^2 / *t*	***df***	***p***
Age	21.33	(3.20)	21.56	(3.06)	21.10	(3.35)	0.83	127	.41
Gender							0.06	1	.81
Female	73.6%		72.7%		74.6%				
Male	26.4%		27.3%		25.4%				
Birth Country							5.93[a]	3	.12
Netherlands	49.6%		53.0%		46.0%				
Germany	29.5%		24.2%		34.9%				
Elsewhere in Europe	13.2%		18.2%		7.9%				
Outside Europe	7.8%		4.5%		11.1%				
Weekly hours of video gameplay	4.25	(6.77)	4.83	(7.52)	3.63	(5.87)	1.01	122.20	.31
Depressive symptoms	6.02	(3.92)	6.30	(4.16)	5.71	(3.67)	0.85	127	.40
Anxiety symptoms	5.12	(3.41)	5.52	(3.75)	4.70	(3.00)	1.36	127	.18
Stress symptoms	8.24	(3.85)	8.35	(4.04)	8.13	(3.67)	0.33	127	.75
Motivation to change									
Precontemplation	2.21	(0.58)	2.29	(0.64)	2.12	(0.50)	1.65	121.79	.10
Contemplation	3.58	(0.58)	3.55	(0.63)	3.60	(0.53)	-0.52	127	.60
Action	3.21	(0.70)	3.11	(0.71)	3.32	(0.67)	-1.71	127	.09
Maintenance	3.06	(0.71)	3.03	(0.67)	3.08	(0.76)	-0.38	127	.71
Emotion mindset									
Pre-test	3.21	(0.77)	3.24	(0.83)	3.17	(0.71)	0.53	127	.60

			Condition						
			Detailed trailer with mental health messaging		**Abstract trailer with mental health messaging**				
Variable	**Mean / Percentage**	***(SD)***	**Mean / Percentage**	***(SD)***	**Mean / Percentage**	***(SD)***	**$\chi 2$ / *t***	***df***	***p***
Post-test	3.16	(0.78)	3.17	(0.82)	3.15	(0.74)	0.09	127	.93
Stress mindset									
Pre-test	1.55	(0.59)	1.62	(0.59)	1.48	(0.58)	1.39	127	.17
Post-test	1.59	(0.64)	1.66	(0.63)	1.51	(0.65)	1.36	127	.18
Game choice							20.49	1	< .001
Mental health	59.7%		78.8%		39.7%				
Entertainment	40.3%		21.2%		60.3%				
Gameplay duration in minutes	28.48	(8.85)	29.31	(9.16)	27.60	(8.50)	1.09	126	.28

Note. [a] one cell (12.5%) had an expected count less than 5.

Table 2 Means and standard deviations of the study variables per game choice

	Game choice						
Variable	**Mental health[a] *(SD)***		**Entertain-ment[b] (*SD*)**		***t***	***df***	***p***
Depressive symptoms	6.19	(3.94)	5.75	(3.92)	-0.63	127	.53
Anxiety symptoms	5.43	(3.88)	4.65	(2.54)	-1.37	126.92	.17
Stress symptoms	8.34	(3.96)	8.10	(3.71)	-0.35	127	.73
Motivation to change							
Precontemplation	2.20	(0.60)	2.21	(0.56)	0.44	127	.97
Contemplation	3.63	(0.56)	3.49	(0.61)	-1.33	127	.19
Action	3.19	(0.73)	3.24	(0.65)	0.39	127	.70
Maintenance	3.09	(0.69)	3.00	(0.75)	-0.69	127	.49
Emotion mindset							
Pre-test	3.19	(0.75)	3.24	(0.80)	0.34	127	.73
Post-test	3.17	(0.73)	3.15	(0.86)	-0.08	127	.93
Stress mindset							
Pre-test	1.54	(0.56)	1.57	(0.63)	0.25	127	.80
Post-test	1.68	(0.61)	1.45	(0.67)	-1.94	127	.06
Gameplay duration	28.74	(8.87)	28.10	(8.90)	-0.41	126	.69

Note. [a] n = 77. [b] n = 52.

Finally, Table 3 presents Pearson correlations between all study variables. Emotion mindset at pre-test and emotion mindset at post-test were strongly positively associated, as were stress mindset at pre- and post-test. Moderate to large positive associations were found between depressive symptoms, anxiety symptoms, and stress symptoms. Regarding motivation to change, moderate to large negative associations were found between precontemplation and the other three subscales. In addition, contemplation, action, and maintenance were strongly positively associated with one another. Depressive symptoms were moderately positively associated with contemplation and maintenance. Stress symptoms were weakly negatively associated with emotion mindset at pre-test and weakly positively associated with contemplation. Finally, emotion mindset at pre-test was weakly negatively associated with maintenance.

Table 3 Pearson correlations between the study variables

Variable	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
1. Depressive symptoms	-										
2. Anxiety symptoms	.24**	-									
3. Stress symptoms	.35***	.56***	-								
4. Precontemplation	- .14	.07	- .06	-							
5. Contemplation	.27**	- .05	.18*	- .52***	-						
6. Action	.09	- .04	.11	- .43***	.46***	-					
7. Maintenance	.33***	- .01	.17†	- .37***	.59***	.61***	-				
8. Emotion mindset pre-test	- .17†	- .13	- .18*	- .00	- .09	.03	- .19*	-			
9. Emotion mindset post-test	- .07	- .08	- .17†	.03	- .07	.06	- .13	.71***	-		
10. Stress mindset pre-test	.03	.06	- .01	.15	- .09	- .02	- .16†	- .01	.00	-	
11. Stress mindset post-test	- .10	.01	- .16†	.07	- .02	- .07	- .08	.03	.01	.62***	-
12. Gameplay duration	.03	- .10	.02	- .06	.04	- .02	.02	.07	.04	- .06	- .08

Notes. * $p < .05$. ** $p < .01$. *** $p < .001$. † $p < .10$.
Partial correlations controlling for gender showed similar results.

Predicting Game Choice and Gameplay Duration

To examine whether motivation to change, emotion mindset and stress mindset influenced the choice for a game promoted as a mental health game, two binary logistic regression analyses were performed to predict game choice. One analysis used the motivation to change subscales as independent variables and the other analysis used emotion and stress mindsets at pre-test as independent variables. Experimental condition was included as a control variable in both analyses. Statistical assumptions were tested and met. Contrary to expectations, motivation to change subscales, emotion mindset, and stress mindset did not significantly predict game choice (see Tables 4 and 5).

To examine whether motivation to change, emotion mindset and stress mindset influenced gameplay duration when choosing the mental health trailer, two linear regression analyses were performed. One analysis included the motivation to change subscales as centred independent variables and the other analysis included emotion and stress mindsets at pre-test as centred independent variables. In addition, both analyses included game choice and the interaction terms between the centred independent variables and game choice as predictors. Experimental condition was included as a control variable in both analyses. Statistical assumptions were tested and met. Motivation to change, emotion mindset, and stress mindset had no direct effect on gameplay duration. Contrary to expectations, the interactions between game choice and the motivation to change subscales, emotion mindset, or stress mindset also did not have a significant effect on gameplay duration (see Tables 6 and 7).

Table 4 Results from a binary logistic regression predicting game choice from motivation to change subscales

Predictor	**B**	***SE* B**	**Wald's $\chi 2$**	***df***	***p***	**e^B (odds ratio)**
Constant	-2.33	2.22	1.10	1	.29	0.10
Experimental condition[a]	1.78	0.41	18.46	1	< .001	5.92
Precontemplation	0.01	0.42	0.00	1	.99	1.01
Contemplation	0.66	0.47	1.91	1	.17	1.93
Action	-0.19	0.38	0.26	1	.61	0.82
Maintenance	0.05	0.39	0.02	1	.89	1.06
Test			**$\chi 2$**	***df***	***p***	
Omnibus test of model coefficients			24.30	5	< .001	
Goodness-of-fit test						
Hosmer & Lemeshow			4.75	8	.78	

Notes. Dependent variable 'game choice' was coded as 0 = entertainment, 1 = mental health.
-2 Log likelihood = 149.65. Cox and Snell R^2 = .17. Nagelkerke R^2 = .23. When gender was included as a control variable, similar results were found and gender was not a significant predictor.
[a] coded as 0 = detailed trailer with mental health messaging, 1 = abstract trailer with mental health messaging.

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Table 5 Results from a binary logistic regression predicting game choice from emotion and stress mindsets

Predictor	B	*SE* B	*Wald's* $\chi 2$	*df*	*p*	e^B (odds ratio)
Constant	0.52	0.99	0.28	1	.60	1.68
Experimental condition[a]	1.80	0.41	19.67	1	< .001	6.06
Emotion mindset pre-test	-0.16	0.26	0.37	1	.54	0.85
Stress mindset pre-test	-0.30	0.34	0.77	1	.38	0.74
Test			$\chi 2$	*df*	*p*	
Omnibus test of model coefficients			22.27	3	< .001	
Goodness-of-fit test						
Hosmer & Lemeshow			3.28	8	.92	

Notes. Dependent variable 'game choice' was coded as 0 = entertainment, 1 = mental health.
-2 Log likelihood = 151.69. Cox and Snell R^2 = .16. Nagelkerke R^2 = .21. When gender was included as a control variable, similar results were found and gender was not a significant predictor.
[a] coded as 0 = detailed trailer with mental health messaging, 1 = abstract trailer with mental health messaging.

Changes in Emotion and Stress Mindsets based on Game Choice

To examine whether emotion and stress mindsets changed when participants chose the mental health trailer (and not the entertainment trailer), two Repeated Measures ANOVA's were performed. One analysis included pre-test and post-test emotion mindset as a within-subjects factor. The other analysis included pre-test and post-test stress mindset as a within-subjects factor. In addition, both analyses included game choice as a between-subjects factor and experimental condition as a covariate. Statistical assumptions were tested and met[1].

Overall, emotion mindset did not change from pre- to post-test, $F(1, 126) = 1.68$, $p = .20$, $\eta^2_p = .01$. In addition, there was no significant interaction effect between game choice and the change in emotion mindset, $F(1, 126) = .73$, $p =$

1 Box's *M* test of equality of covariance matrices was significant in the Repeated Measures ANOVA for emotion mindset. Tabachnick and Fidell (2014) state that this test is highly sensitive and that only if the sample sizes are unequal and Box's *M* test is significant at $p < .001$, robustness is not guaranteed. Our sample sizes were unequal, but the *p*-value of the test was .03 suggesting the test is still robust. In addition, it has been suggested to use Pillai's criterion instead of Wilks' lambda to evaluate multivariate significance when covariance matrices are unequal (Olson, 1979). However, Pillai's criterion provided the same results as Wilks' lambda in our analysis. Therefore, we ignored the significance of the Box's *M* test.

.40, η^2_p = .01. Thus, regardless of participants' game choice, emotion mindset remained the same from pre- to post-test (see Table 2 for means)[2].

For stress mindset, no overall change from pre- to post-test was found, $F(1, 126) = .372$, $p = .54$, $\eta^2_p < .01$. However, there was a significant interaction effect between game choice and the change in stress mindset[3], $F(1, 126) = 7.86$, $p = .006$, $\eta^2_p = .06$. For participants who chose the mental health trailer, stress mindset significantly increased from 1.54 to 1.68, $t(76) = -2.15$, $p = .04$, indicating that participants became slightly less convinced about the debilitating nature of stress, becoming more neutral. For participants who chose the entertainment trailer, stress mindset did not change ($M_{\text{pre-test}} = 1.57$, $M_{\text{post-test}} = 1.45$; $t(51) = 1.65$, $p = .11$).

Table 6 Results from a linear regression predicting gameplay duration from motivation to change subscales

	Unstandardized estimate		**Standardized estimate**
Predictor	**B**	***(SE)***	**B**
Constant	29.72***	(1.87)	
Experimental condition[a]	- 2.30	(1.83)	- 0.13
Game choice	- 0.27	(1.81)	- 0.02
Precontemplation	- 6.08†	(3.17)	- 0.40
Contemplation	- 2.24	(3.03)	- 0.15
Action	- 2.60	(2.38)	- 0.20
Maintenance	0.78	(2.26)	0.06
Precontemplation * Game choice	7.15†	(3.76)	0.37
Contemplation * Game choice	3.39	(3.93)	0.17
Action * Game choice	3.54	(3.14)	0.22
Maintenance * Game choice	- 1.83	(3.17)	- 0.11
	Model statistics		
F	0.63		
Error *df*	117		
R^2	.05		

Notes. When gender was included as a control variable, similar results were found and gender was not a significant predictor. [a] coded as 0 = detailed trailer with mental health messaging, 1 = abstract trailer with mental health messaging. *** $p < .001$, † $p < .10$.

2 Similar results were found when gender was included as a covariate.

3 Similar results were found when gender was included as a covariate.

Table 7 Results from a linear regression predicting gameplay duration from emotion and stress mindsets

	Unstandardized estimate		Standardized estimate
Predictor	**B**	***(SE)***	**B**
Constant	29.50***	(1.79)	
Experimental condition[a]	- 1.95	(1.76)	- 0.11
Game choice	- 0.14	(1.76)	- 0.01
Emotion mindset pre-test	- 0.12	(1.58)	- 0.01
Stress mindset pre-test	1.43	(2.00)	0.10
Emotion mindset pre-test * Game choice	1.19	(2.10)	0.08
Stress mindset pre-test * Game choice	- 4.33	(2.71)	- 0.21
	Model statistics		
F	0.86		
Error *df*	121		
R^2	.04		

Notes. When gender was included as a control variable, similar results were found and gender was not a significant predictor. [a] coded as 0 = detailed trailer with mental health messaging, 1 = abstract trailer with mental health messaging. *** $p < .001$.

DISCUSSION

The present study examined how motivational factors influenced young adults' choice for, and engagement with, a game promoted as a mental health game. Contrary to what was expected, motivation to change, emotion mindset and stress mindset did not predict participants' willingness to choose a game promoted as a mental health game over a game promoted as an entertainment game. Additionally, when individuals with more motivation to change, a growth emotion mindset, or a stress-is-enhancing mindset did choose the game promoted as a mental health game, they did not play the game for a longer period of time. The secondary aim of the study was to examine whether participants' game choice led to changes in their emotion and stress mindsets. Regardless of participants' game choice, emotion mindset did not change. Interestingly, however, participants who chose the mental health trailer reported a change in their stress mindset after gameplay, showing a decline in their belief that the effects of stress are debilitating. No change in stress mindset was found for participants choosing the entertainment trailer.

Motivation to Change and Engagement with a Mental Health Game

Our findings that the motivation to change stages did not predict game choice nor influenced participants' engagement with the game promoted as a mental health game compared to the game promoted as an entertainment game, were unexpected. Although we conducted the study among young adults experiencing at least mild mental health symptoms, it seems that on average our participants were currently not considering treatment or looking for help, as indicated by scores around the mid-point for the action and maintenance subscales. The current sample seems to include individuals that are less motivated to change their symptoms compared to individuals already receiving counselling or admitted for treatment (e.g., Greenstein et al., 1999; Lichtenberg & Hummel, 2000). Nevertheless, this low motivation to change did not prevent participants from choosing to play a game promoted as a mental health game. In fact, 59.7% of the participants chose to play the game promoted as a mental health game – regardless of their motivation to change – and they played it for a similar period of time as participants choosing the game promoted as an entertainment game. This finding speaks to the promising potential of using games as mental health interventions, given that over two thirds of youth do not seek professional help for their mood and anxiety disorders (e.g., ESEMeD/MHEDEA investigators et al., 2004; Merikangas et al., 2011), which appears to be mainly because of the perceived stigma associated with professional help (Clement et al., 2015). The current findings support the notion that video games for mental health are a promising approach to reach individuals with mental health symptoms, even the ones that are not inclined to seek professional help.

Emotion and Stress Mindsets and Engagement with a Mental Health Game

Contrary to what was expected, our results showed that emotion and stress mindsets did not predict game choice nor did they influence participants' engagement with the game promoted as a mental health game compared to the game promoted as an entertainment game. We hypothesised that individuals with a fixed emotion/stress-is-debilitating mindset would be less likely to choose a game promoted as a mental health game than individuals with a growth emotion/stress-is-enhancing mindset, and that these individuals would play that game for a shorter period of time. However, there may have been different reasons for why individuals did or did not select the mental health trailer. For example, it might be that right before and/or during the experiment participants were not experiencing negative emotions that needed to be regulated, and therefore did not choose the mental health trailer (e.g., one participant indicated that they did not choose the mental health trailer because "[it] was advertised with [*sic*] being good for your mental health and stress. I did not fell [*sic*] stressed or bad at the moment..."). In addition, a game aimed at stress management may imply confrontations with stress for some individuals, but others might think that a mental health game is less stressful than a regular entertainment game. These opposing motivations may have cancelled each other out, explaining why we did not find an effect. Future research should examine how current negative affect and expectations about mental health games (e.g., stress-inducing/confrontations with stress versus stress-reducing) affect the willingness to play a game promoted as a mental health game.

Finally, it might be that emotion and stress mindsets are not directly linked to actions of help-seeking because of a moderating or mediating third variable, such as self-efficacy or stigma (see also De Castella et al., 2013; Gutentag et al., 2017). For example, some participants indicated that they did not choose the mental health trailer because "it looks frustrated [*sic*] and more complicated" and ". . . it implicates that there is something wrong with you". Future research may examine whether additional variables have an influence on participants' choice.

Change in Mindsets after Playing a Mental Health Game

Results showed that regardless of participants' game choice, emotion mindset did not change. Interestingly, however, stress mindset changed towards a more neutral mindset about the nature of stress (although scores were still on the debilitating side of the scale) for participants choosing the mental health trailer, but stayed the same for participants choosing the entertainment trailer. It remains unclear why participants' stress mindset changed after selecting and playing the game when it was promoted as a mental health game and not when it was promoted as an entertainment game. Because all participants played the *same* game for an *equal* amount of time, the findings strongly suggest that expectations primed through the trailers prior to gameplay were driving these effects. It might be that these prior expectations made participants more aware about the potential benefits of the video game and/or primed participants to focus on any slight stress and interpret that as manageable and less debilitating (cf. Colloca & Miller, 2011; Kirsch, 1997). Future research should examine how expectations of playing a mental health or entertainment game influences participants' in-game experiences and interpretations.

The magnitude of the observed changes in participants' stress mindset is similar to changes that Crum and colleagues (2013) reported in their first study on stress mindset. In their experiment, they exposed participants to three 3-minute videos presenting either the enhancing or debilitating effects of stress. A control group did not view any videos. Similar to our participants who chose the mental health trailer, participants in the enhancing condition became less convinced that stress is debilitating, although they still scored below the neutral point of the scale. Participants in the debilitating condition became more convinced about the debilitating nature of stress. Stress mindset did not change for participants in the control condition. The findings of the current study are not only similar, but also show that stress mindset can be changed by providing a mental health message before playing a short video game. The change in stress mindset may seem small, but is worth further exploration given that a stress-is-debilitating mindset has been linked to less adaptive physiological effects of stress, worse cognitive and affective outcomes, poorer (mental) health and well-being, and premature death (Crum et al., 2017; Keller et al., 2012; D. Park et al., 2018).

It may be important to consider the reasons why stress mindset changed while emotion mindset did not. Previous research has shown that mindsets are domain specific and only impact on factors within the same domain (Beer, 2002; Dweck et al., 1995; Lee et al., 2012; Schroder et al., 2015; Scott & Ghinea, 2014; Tamir et al., 2007). It might be that the promotional mental health message and/or the game itself spoke more to one's stress mindset than to one's emotion mindset. In fact, the messages in the mental health trailer emphasized stress management ("learn to manage stress more efficiently") and the relaxing nature of the game ("both challenging and relaxing") rather than the changeability of emotions. These mental health messages may have led participants to focus on any slight stress that they experienced during the puzzles. In turn, because the mental health messages focused on stress management, participants may have interpreted this stress as manageable, less debilitating, and actually beneficial in helping them to find the solution. For example, one participant said "I felt a bit stressed when lots of black birds appeared on the map, which was a positive and exciting kind of stress. This kept me more interested in the game and motivated me". Thus, the combination of the mental health messages and the gameplay experiences may have taught participants more about the enhancing nature of stress rather than the changeability of emotions.

Limitations and Future Directions

Several limitations should be mentioned when interpreting the findings. First, due to our recruitment strategy, the majority of the sample was highly educated and predominantly female. The current sample is therefore not representative of the Dutch young adult population that experiences mental health problems, which may have biased the results. In particular, university students may not have the same preferences and motivations regarding video games for mental health as the overall population might have. Secondly, the present study included participants with elevated mental health symptoms, but the current sample seemed to have restricted variability in motivation to change. When filling out the motivation to change questionnaire, participants were instructed that when the statements referred to “their problem”, they should answer in terms of the behaviours, feelings or situations they had identified in the previous questionnaire (i.e., the DASS-21 measuring depression, anxiety, and stress symptoms). It might be that participants did not experience “problems” that needed to be changed. As found in the current study, as well as in other studies (e.g., Dozois et al., 2004), more (severe) mental health symptoms are associated with more motivation to change. In addition, research has shown that more (severe) mental health symptoms are associated with more professional help-seeking (e.g., Merikangas et al., 2011; Oliver et al., 2005; Sawyer et al., 2012). Future research should examine how motivational factors influence engagement with a mental health game among a more representative sample experiencing more severe mental health symptoms.

Future research should investigate the influence of motivational factors on prolonged engagement and ongoing use of mental health video games, for example through a daily diary study and/or ecological momentary assessments. On the other hand, it might also be interesting to examine how ongoing use and in-game experiences influence motivational factors. It might be that positive in-game experiences (e.g., increased positive mood) motivate participants to pursue activities that may improve their mental health symptoms (e.g., seeking help). Furthermore, as the current study only examined one specific game, future research should investigate whether other game-related motivational factors such as genre and visual aspects of the game, play a role in the willingness to select and play a mental health game. Insights into the influence of these game-related factors on game choice and engagement may guide game design and game promotion, potentially reaching a larger group of individuals with mental health symptoms. Finally, the current study found that participants became more neutral about the debilitating nature of stress. Future research should examine how changes in stress mindset are related to mental health outcomes over time, as well as whether changes in mindset persist over time.

Notwithstanding the aforementioned limitations and remaining questions for future research, the current study has implications for the prevention of mental health symptoms and promotion of mental health. Results showed that pitching a game as beneficial for one's mental health does not deter individuals from playing it. In fact, 3 out of 5 participants preferred to play a game promoted as a mental health game over a game promoted as a regular entertainment game, regardless of their motivation to change and mindsets. These findings support the notion that video games can be a motivating and engaging approach to reach individuals with mental health symptoms, and that video games aiming to improve mental health can be promoted as a mental health game. In addition, the current study showed that pitching a game as beneficial for one's mental health can lead to changes in people's mindset about the effects and nature of stress. Considering the adverse effects of a stress-is-debilitating mindset for mental health and well-being, this finding is promising. Taken together, this study indicates that video games aiming to improve mental health may benefit from promoting the game for its mental health benefits.

Chapter 8

General discussion

The aim of the current thesis was to examine the potential of applied and casual games for improving mental health in youth, and to investigate the influence of nonspecific (motivational) factors on game selection, game experiences, engagement and mental health outcomes. Part 1 of my dissertation presented a state-of-the-art overview of the field through a systematic review that focused on randomised controlled studies assessing digital games as interventions to improve mental health in youth. The review encompassed an exploration of the effectiveness of both applied and casual games across diverse mental health domains, integrating both clinical and healthy populations. Additionally, current research trends within the field were examined. In Part 2, we investigated several specific and nonspecific factors, their impact on engagement with the game, and their association with mental health outcomes, utilising the applied game MindLight. The research outlined in Part 3 aimed to enhance our understanding of how (nonspecific) motivational factors influence the selection of mental health games, as well as subsequent game experiences and engagement with mental health games. The current concluding chapter of my dissertation presents a summary and general discussion of the main findings, followed by a reflection on the limitations within this dissertation and recommendations for future research. Additionally, implications of the findings for game design and implementation will be discussed.

INCREASING ENGAGEMENT AND EFFECTIVENESS THROUGH NONSPECIFIC FACTORS

In Chapters 3, 4 and 5, we examined specific and nonspecific predictors of engagement and effectiveness in MindLight, an applied game to alleviate youth anxiety. MindLight incorporates three evidence-based techniques from cognitive behaviour therapy (CBT), namely relaxation (Price & Budzynski, 2009), exposure (Feske & Chambless, 1995; Kendall et al., 2005), and attention bias modification (Bar Haim, 2010; Bar Haim et al., 2011). The game aims to teach youth how to cope with anxiety in a playful manner. Research from our own GEMH lab demonstrated MindLight's effectiveness in reducing anxiety, with effect sizes comparable to those reported in meta-analyses on CBT (Mychailyszyn et al., 2012). Additionally, our research suggested that nonspecific factors may play a role (Scholten et al., 2016; Schoneveld et al., 2016). Therefore, in the current dissertation, we conducted further research on specific and nonspecific factors in MindLight.

In Chapter 3, we investigated whether engagement with specific therapeutic techniques in MindLight predicted improvements in anxiety symptoms. Drawing from the anxiety literature, we identified two types of in-game play behaviours: engaged behaviours that facilitate a higher 'dosage' of therapeutic techniques and contribute to successful gameplay, and avoidant or safety behaviours that reduce opportunities to engage and practice with the therapeutic techniques. Clinical research emphasizes the significance of reducing avoidant and safety behaviours for better treatment outcomes (McManus et al., 2008; Morgan & Raffle, 1999; Salkovskis et al., 1999). We assessed in-game play behaviours during the initial and final play sessions, and examined how changes from the first to the last session predicted improvements in anxiety. Results revealed that changes in gameplay behaviours, particularly those representing therapeutic exposure techniques, predicted improvements in anxiety symptoms three months later. Specifically, children actively engaging with the game, as evidenced by increased exploration behaviour, reported lower anxiety levels. Conversely, children increasing their avoidance (i.e., hiding from the fear events) and safety behaviours (i.e., turning on lights in the game) reported higher anxiety levels after three months. Thus, more engagement with the specific therapeutic techniques contributed to effective anxiety reduction. Additionally, our analysis revealed that baseline anxiety was not associated with in-game play behaviours during the initial session. Children exhibiting high initial anxiety symptoms engaged with the game in a manner akin to children with milder symptoms. This demonstrates that heightened symptom severity does not impede game engagement.

In Chapter 4, we found that baseline anxiety levels, maternal mental health and children's self-efficacy did not influence the changes observed over time in both mother- and child-reported anxiety. These results show that nonspecific factors do not hamper nor increase the effectiveness of MindLight. Additionally, we examined whether MindLight exhibits beneficial effects on outcomes associated with anxiety symptoms. The results revealed that MindLight is equally effective as CBT in reducing mother-reported internalising problems and increasing child-reported self-efficacy. Additionally, both interventions led to decreases in mother-reported externalising problems. CBT, however, exhibited a larger reduction at post-test and the 6-month follow-up. These findings indicate that MindLight shows promise as an effective intervention not only for addressing symptoms of anxiety but also for improving more general mental health outcomes.

In the study outlined in Chapter 5, we investigated the impact of expectations for improvement on engagement during MindLight gameplay, and explored the moderating roles of symptom severity and motivation to change. To experimentally manipulate expectations, participants watched a teaser trailer promoting MindLight either as a mental health game or a regular entertainment game. Similar to the methodology of the study described in Chapter 3, on-screen output during MindLight play was recorded, and in-game play behaviours were coded. Results indicated that expectations did not predict participants' game experiences (i.e., experienced fun) nor engagement with the therapeutic techniques in MindLight. Additionally, neither symptom severity nor motivation to change predicted or moderated these outcomes. These findings show that nonspecific factors neither hinder nor enhance engagement with the therapeutic techniques in MindLight. From pre- to post-test, we found similar decreases in positive affect and similar increases in state anxiety and arousal between the experimental groups, which were not influenced by participants' symptom severity or motivation to change. These results provide further evidence that nonspecific factors do not influence engagement with MindLight.

In summary, the results of the studies described in Chapters 3, 4, and 5 indicate that nonspecific factors, such as baseline anxiety (symptom severity), maternal mental health, and children's self-efficacy did not serve as predictors for effectiveness. Examining engagement with therapeutic techniques in MindLight revealed that in-game behaviours supporting players' practice of exposure were associated with reduced anxiety symptoms three months later. Symptom severity, expectations for improvement and motivation to change did not influence game experiences and engagement. Our results contradict existing literature on conventional therapy showing that nonspecific factors significantly contribute to both positive intervention outcomes and engagement (Boettcher et al., 2013; Boot et al., 2013; Colloca, 2018a, 2018b; Constantino et al., 2018; Derisley & Reynolds, 2000; Dozois et al., 2004; Greenberg et al., 2006; Ilardi & Craighead, 1994; Lewis et al., 2009; Meyer et al., 2002; Norcross et al., 2011; Thiruchselvam et al., 2019; Wampold, 2015; Westra et al., 2007). A possible explanation for this may be that games are already naturally appealing and considered intrinsically motivating and engaging (Fleming et al., 2017; Granic et al., 2014; Kazdin, 2015), and because of that, motivation to change and expectations for improvement do not (further) contribute to engagement and subsequent mental health improvements in applied games as it does for conventional therapy. Research investigating the effects of symptom severity on the response to conventional programs is inconclusive. Previous research has shown that higher symptom severity is related to greater symptom decreases after intervention (Lorenzo-Luaces et al., 2020; Van Starrenburg et al., 2017), but also that it may adversely impact effectiveness (Cunningham et al., 2016; Kampman et al., 2008; Lorenzo-Luaces et al., 2020).

Taken together, the results of the studies described in Part 2 of this dissertation underscore the potential of theory-based applied games like MindLight as effective interventions. MindLight demonstrates efficacy not only in reducing explicitly targeted anxiety symptoms, but also in improving more general mental health outcomes associated with anxiety. Crucially, the observed improvements in mental health symptoms persisted for up to 6 months post-intervention, even without continued gameplay. Our findings further suggest that nonspecific factors are not that crucial to harness for applied games to be effective, and that both youth with high as well as low expectations, motivation to change and baseline symptoms benefit equally from applied games.

INTERVENTION REACH AND UPTAKE: MESSAGING AND (NONSPECIFIC) MOTIVATIONAL FACTORS

In Part 3 of this dissertation, we examined the uptake of and engagement with mental health games among individuals exhibiting symptoms of mild to severe internalising issues, encompassing depressive, anxiety and stress symptoms. In our experimental study, participants were presented with two trailers of video games. After viewing these trailers, participants were asked to choose the game they found most appealing and were allowed to play their chosen game as long as they wished. Unbeknownst to the participants, both trailers portrayed the same commercial video game, Monument Valley. To create the impression of two distinct games, we varied the designs of the trailers and incorporated different messages. One trailer presented the game as a mental health game, while the other portrayed it as an entertainment game. Following the viewing of the trailers, we measured participants' game choices based on the promotional message (i.e., mental health or entertainment).

In Chapter 6, we investigated the impact of messaging, symptom type and symptom severity on game choice, perceived attractiveness and fun. In Chapter 7 we further examined how motivation to change and mindsets influence game choice. The results indicated that participants were nearly four times more likely to select a game explicitly marketed as beneficial for mental health, as opposed to when solely promoted for entertainment. Unexpectedly, a significant difference in attractiveness emerged between the two trailer designs, specifically when they contained the mental health message.

This distinction was not observed when the trailer designs contained the entertainment message. Consequently, participants were more than five times more likely to select the mental health game promoted in one trailer design over the other. These findings compellingly show that explicit mental health messaging does not deter young adults exhibiting mental health symptoms, but, in fact, enhances appeal of such games, making them more attractive to these individuals. Moreover, the emergence of the unexpected difference in the attractiveness of the two trailer designs underscore the importance of aesthetic appeal in the choice for mental health games. Indeed, previous research has shown that youth consider engaging and visually appealing game designs crucial for mental health games to succeed (R. Pine, Sutcliffe, et al., 2020).

Surprisingly, (the generally low levels of) motivation to change, and emotion and stress mindsets did not significantly influence the choice to engage with a mental health game. We did find that individuals with heightened anxiety levels anticipated less fun from the games compared to those without such symptoms, however symptom type and severity did not predict actual game choice. These findings are inconsistent with previous research in conventional intervention contexts. That research showed that motivation to change, mindsets and symptom severity are important predictors of help-seeking and presumably openness to mental health interventions (Burnette et al., 2020; Lawrence et al., 2017; M. I. Oliver et al., 2005; Sawyer et al., 2012). A couple of studies, however, have also shown that severity of (depressive) symptoms does not influence help-seeking (Chin et al., 2015; Merikangas et al., 2011). Our findings are in line with these latter studies, indicating that nonspecific factors do not influence the likelihood of selecting a mental health game. Although youth with anxiety symptoms may initially expect less fun from a mental health game, the results demonstrated that mental health messaging can reach youth experiencing a broad spectrum of internalising issues.

In Chapters 6 and 7 we also examined the impact of game choice and nonspecific factors on engagement and experiences of gameplay. Participants' game choice for the mental health or entertainment promoted game did not influence gameplay duration or overall game experiences. Nor were motivation to change, and emotion and stress mindsets associated with gameplay duration. These results show that those choosing the mental health game exhibited comparable gameplay duration, intrinsic motivation, autonomy, and competence levels, as well as similar increases in affect compared to those selecting the entertainment game. Exploratory analyses, however, did reveal that participants with severe symptoms reported feeling more autonomous (while playing the mental health game) and more competent (in either game) compared to their counterparts without severe symptoms. These results suggest that providing individuals with the choice to select a mental health game may be particularly important for individuals experiencing severe mental health symptoms. With regard to specific mental health symptoms, we found that participants with severe depressive symptoms exhibited lower levels of affect before gameplay, which increased during gameplay. This positive affect change was not observed in participants without elevated depressive symptoms, resulting in comparable positive affect levels post-gameplay for both groups. These results are in line with the mood-brightening effect in individuals experiencing depressive symptoms, indicating their heightened sensitivity to positive experiences and propensity to respond with more positive and less negative emotions (Bylsma et al., 2011; Peeters et al., 2003). Together our findings are promising, demonstrating that mental health messaging has no adverse effects on engagement and game experiences. In fact, it appears to have positive effects on youth with severe (depressive) symptoms, suggesting the potential to use mental health games for prolonged and continued gameplay over time.

Finally, in Chapter 7 we also investigated whether mindsets change after playing a game promoted as mental health game. Irrespective of participants' chosen game, there was no significant change observed in emotion mindset from pre- to post-test. Similarly, there was no overall change in stress mindset. However, among participants who opted for the mental health trailer, a notable change in their stress mindset after gameplay was observed. This suggested that these participants became slightly more neutral in their perceptions of the nature of stress, reflecting a decline in their belief that the effects of stress are inherently debilitating. It thus seems that the mental health messaging effectively functioned as a stress mindset intervention. These results are in line with previous research showing that mindsets can be changed through surprisingly brief interventions, such as reading an article, performing a short writing exercise or playing a virtual reality biofeedback game (e.g., Dweck, 2008; Jamieson et al., 2018; Maarsingh et al., 2019; Miu & Yeager, 2015; D. S. Yeager et al., 2014). Our results are particularly similar to research from Crum and colleagues (2013), who showed that watching short videoclips with messages about either the enhancing or debilitating effects of stress significantly changed participants' stress mindset. Considering the established link between a stress-is-debilitating mindset and poorer mental health and well-being, further exploration into these findings is warranted (Crum et al., 2017; Keller et al., 2012; D. Park et al., 2018).

In summary, the results from Chapter 6 and 7 support the idea that explicitly promoting video games for mental health present a promising approach to reach individuals with mental health symptoms, even those less inclined to seek professional help. Video games, specifically designed to improve mental health, can be positioned and promoted as mental health games, leveraging their motivational and engaging potential to effectively reach and benefit individuals with mental health symptoms.

LIMITATIONS AND FUTURE SUGGESTIONS

When interpreting the results across the various chapters of this dissertation, it is crucial to acknowledge that certain conclusions are drawn from null findings. These conclusions pertain to the findings that nonspecific factors do not influence the effectiveness or engagement with MindLight (Chapters 4 and 5, respectively), nor individuals' choice for and engagement with a mental health game (Chapter 7). In this context, null findings signify instances where statistically non-significant results are interpreted as supporting the null hypothesis, indicating 'no difference' or 'no effect' (Tabachnick & Fidell, 2014). The justification for such interpretation rests on the premise that the study possesses sufficient statistical power to reject the null hypothesis when it is genuinely false (Leppink et al., 2017). It is essential to recognise that a lack of statistical power can lead to a Type II error, where the null hypothesis is not rejected even when it is not true. With low statistical power it remains unclear whether a non-significant result means that there is actually no effect (i.e., null hypothesis is true) or whether there is not enough power to reject the null hypothesis.

In psychology research, the commonly sought statistical power is around 0.80, balancing the need for accurate conclusions with the practical feasibility of the study (as striving for higher statistical power would mean that more participants are needed) (J. Cohen, 1992; Picho & Artino Jr, 2016). The sample sizes in the studies conducted in this dissertation were determined using a-priori power analyses, aiming for 80% power (i.e., 1-β), with a (conventional) significance level of 0.05, and the capacity to detect a medium effect size. This implies that there is an 80% probability of correctly rejecting the null hypothesis if it is false, instilling confidence in the results obtained in this dissertation (Lakens, 2022). Nevertheless, future research is needed to replicate our findings.

There is always the possibility (i.e., β), however, that our non-significant findings represent false negatives (Type II error), or that small effects or differences exist but were undetectable due to our sample size. Acknowledging this, we recognise that our a-priori power might be limited to detecting medium-sized effects, and thus, we may have erroneously concluded a lack of difference or effect. Nevertheless, the consideration of practical relevance is crucial here. Small effects, as noted by J. Cohen (1988), are challenging to detect and might not translate into real-world, noticeable, improvements. A small effect size should therefore be considered negligible (Hojat & Xu, 2004). Even if larger sample sizes were employed, potentially revealing small effects, their practical importance might still be minimal.

Despite this, however, our results do not provide insights into the *strength of evidence* supporting the null hypothesis (Rouder et al., 2009). Analytic approaches comparing the likelihood of findings under the null hypothesis to the likelihood under the alternative hypothesis are necessary. Bayesian analyses offer a solution by computing a likelihood ratio, indicating under which hypothesis – null or alternative – the observed finding is more likely to have occurred (Leppink et al., 2017; Rouder et al., 2009). This approach provides a spectrum of evidential strength, ranging from 'not worth more than a bare mention' to substantial, strong, very strong, and decisive evidence (Jeffreys, 1961). Thus, a comprehensive understanding of how strong the evidence for the current non-significant findings is, requires Bayesian hypothesis tests. Future research should consider incorporating these analyses to address the questions related to evidence favouring the null hypothesis.

A second limitation of the current dissertation is that none of the studies collectively explored all hypothesised associations simultaneously or provided a comprehensive overview of these associations. In the general introduction of this dissertation, we posited that nonspecific factors might exert a direct impact on intervention outcomes, as well as an indirect effect through variables related to engagement. Additionally, we theorised that these nonspecific factors might interact with each other and with specific factors in predicting both engagement and intervention outcomes. While each study in this dissertation delved into specific facets or sub-relations of the hypothesised associations between specific and nonspecific factors, engagement-related variables, and mental health outcomes, none comprehensively examined all these associations concurrently within the same experimental setup and population. Future research should aim to address this gap by providing a holistic understanding of the interplay between these various factors. It is important to note, however, that Type I errors become more likely when conducting multiple tests on the same data set (Tabachnick & Fidell, 2014). To be able to examine all associations within the same study, a stricter significance level (< 0.05) should be adopted to correct for multiple testing (e.g., Bonferroni adjustment; Streiner & Norman, 2011; Tabachnick & Fidell, 2014).

Furthermore, this dissertation encompasses studies involving children, adolescents and/or young adults. The inclusion of diverse age groups may hinder our ability to draw general conclusions about the impact of nonspecific factors as predictors of effectiveness. Nonspecific effects might manifest differently in children compared to adolescents and young adults. Research on placebo effects may inform us in this regard, as placebo effects can be considered as a nonspecific factor, theorised to arise from expectancies and Pavlovian conditioning (Enck et al., 2008; Peiris et al., 2018; Rief et al., 2011). Examining the limited available data on placebo effects in children and adolescents versus adults, systematic reviews and meta-analyses indicate variations in placebo responses across age groups. Notably, (younger) children exhibit higher placebo response rates than (older) adolescents and adults, meaning that there is a negative correlation between age and the size of the placebo response (Janiaud et al., 2017; Parellada et al., 2012; Weimer et al., 2013).

Explanations for the observed differences in placebo effects and the underlying mechanisms remain speculative (Weimer et al., 2013). Compared to adults, children may possess heightened associative learning capacities (i.e., Pavlovian conditioning and instrumental learning), potentially contributing to increased placebo responses (Weimer et al., 2013). Furthermore, children more frequently presume they are assigned to the intervention arm rather than the control condition in studies (Rothner et al., 2006), and parental expectations and behaviours may foster higher expectations in children (Gniß et al., 2020). Thus, although the mechanisms underlying placebo effects in children and adults are not fully understood, the negative correlation between age and the size of the placebo response is evident (Janiaud et al., 2017; Parellada et al., 2012; Weimer et al., 2013). Consequently, it can be hypothesised that nonspecific factors affecting mental health improvements in applied games may vary between children and young adults. This warrants further investigation in future research to better understand the nuances of these effects across different age groups.

In addition to age, exploring various other individual differences becomes crucial in understanding the effectiveness and eventual adoption of applied games once they enter the public domain. As these games become more widely available, factors such as gender, cultural differences, and socio-economic background merit attention. Although the studies in this dissertation did not explicitly address these effects, acknowledging their potential influence is paramount. Gender, for instance, introduces distinct motivational aspects, preferences and play behaviours between males and females (Phan et al., 2012; Romrell, 2014; Veltri et al., 2014). To gain a comprehensive understanding, future research should strive for including men and women equally and investigate the associations with gender more explicitly. Furthermore, cultural backgrounds and values can significantly shape attitudes towards certain types of games (Ando et al., 2019; Colwell & Kato, 2005), motivation to play (Kitami et al., 2011), expected outcomes and usage patterns (Wohn & Lee, 2013), play behaviours (Bialas et al., 2014), overall gaming experiences (Brückner, 2020; Brückner et al., 2019), but also differences in perception of mental health (problems) and openness to ask for help (Cauce et al., 2002; Guo et al., 2015; Masuda et al., 2009; Mojaverian et al., 2013; Sheikh & Furnham, 2000). It is imperative for future research to intentionally target diverse youth groups, shedding light on the implications of these cultural factors.

Beyond demographic differences, other predictors of efficacy and uptake should also be considered. For instance, examining how the perceived personal relevance of a mental health game and individuals' beliefs and experiences with applied games influence game choices and effectiveness might be a promising avenue for future research (Buday, 2015; Enck et al., 2013; M. B. Oliver & Krakowiak, 2009). It is also crucial to consider that, although 3 in 5 participants chose and played the game promoted as a mental health game, a significant portion did not. As participants explained during our study, various reasons may exist: some individuals might be more inclined to play a mental health game during times of actually experiencing negative emotions. Moreover, perceptions of a mental health game can differ; some may anticipate confronting stress or negative emotions, while others may expect the game to be less stressful than a regular entertainment game, influencing their choice. Additionally, experiencing elevated mental health symptoms might not automatically translate into perceiving (any) mental health game as personally relevant, either in the moment or at all. Future research should thus explore the reasons for (not) selecting the mental health game, possibly through qualitative interviews to uncover nuanced explanations. This exploration could provide valuable insights for devising strategies to attract an even broader audience of young adults to mental health games.

A final notable limitation of the studies presented in Chapters 5, 6, and 7 is the use of a single game play session. It is plausible that the effects of expectations and motivation to change might manifest more distinctly over prolonged engagement with an applied game for mental health. Critical distinctions in gameplay patterns between mental health and entertainment games may only emerge when gameplay is repeated without artificial session constraints. Therefore, future research should prioritize investigations into naturalistic gameplay, spanning weeks or even months, for instance by using passive data on real-world selection and retention. Moreover, it is crucial to note that the current studies only investigated two specific games, namely MindLight and Monument Valley. To build a more nuanced and comprehensive understanding of the factors influencing game selection and engagement, future research should explore whether other game-related motivational factors, such as genre and visual aspects of the game, play a role in individuals' willingness to select and engage with a mental health game.

Additionally, to enhance the generalisability of research findings concerning the promotion and delivery of mental health games, future studies should also consider broader contexts that capture the complexity of youth's daily media choices. It would need to be investigated whether individuals would still opt for and engage with a mental health game in the vast landscape of game or app stores, where mental health games coexist with numerous entertainment options. While the study described in Chapters 6 and 7 allowed participants to choose between two alternatives, resembling real-world decision-making more closely than prior research where participants had no choice (Poppelaars, Lichtwarck-Aschoff, et al., 2018), the reality is that in everyday settings, youth can select from a multitude of nearly unlimited video games. In such scenarios, the likelihood of choosing a mental health game on a single occasion might be lower than observed in our research. However, youths may opt for a mental health game on subsequent occasions. Multiple games can be played concurrently, and the choice of one game does not preclude the selection of another. Hence, future research needs to explore the real-life factors influencing youth's likelihood to engage with mental health games (e.g., their current emotional state, or recommendations made by peers) and examine how motivational factors impact sustained engagement and ongoing use of mental health games over time. Conducting daily diary studies, ecological momentary assessments or talk-aloud methods investigating choice and decision making could shed light on these dynamics and their relationship to effectiveness, informing strategies on how to make mental health games stand out to the public. Finally, mental health games could also be integrated and promoted through more traditional channels, such as mental health institutions and school counselling services. Future research should explore the most suitable method that increase the likelihood of youth choosing a mental health game. Importantly, these delivery forms need not be mutually exclusive.

IMPLICATIONS FOR GAME DESIGN

The research presented in this dissertation has implications for game design and development. Our systematic review in Chapter 2 revealed that many studies assessing the effectiveness of digital games incorporated multimodal interventions. In these cases, digital games, particularly applied games, were utilised alongside treatment as usual or other therapeutic components, and offered simultaneously within the same intervention program. While we were not able to evaluate the additional effect of the game on the overall intervention's effectiveness, indicating a need for future research to elucidate the relative effectiveness of games and explore how traditional interventions can be optimally enhanced through game add-ons, it is evident that multimodal interventions are common practice in clinical settings (Horne-Moyer et al., 2014) and may enhance generalisability of treatment effects (e.g., Wijnhoven et al., 2022). Considering this trend, future game development should anticipate that applied games will often complement therapy. Developers should contemplate implementing features relevant to therapists and practitioners.

For instance, integrating a coding system into the game and including a 'backend' interface for therapists to monitor observed behaviours could be beneficial. This would allow for the automatic tracking and coding of in-game play behaviours, offering therapists insights into (dis)engagement with the game and its therapeutic techniques (Bijkerk et al., 2023). It could also serve as a tool to assess and determine progress of clients, offering early-stage support and addressing difficulties or increasing motivation for engagement. Furthermore, these measurements of in-game play behaviours may be utilised to assess clients' behaviour in practical settings (i.e., the game environment), potentially combined with psychophysiological measures, avoiding the bias and stigma often associated with self-report measures (Caqueo-Urízar et al., 2022; Fadnes et al., 2009). This information is not only valuable for therapists but can also be used to provide feedback to players, maintaining motivation and engagement throughout the digital intervention. It can also inform researchers and game developers about the most effective game mechanics and mechanisms of change.

Moreover, these backend measurements can be employed to dynamically adjust the game to the player's actions, diverse needs and learning paces

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(Bakkes et al., 2012; Bakkes et al., 2014), thereby enhancing motivation and creating optimal learning environments. Drawing from our research described in Chapter 3, the game could, for example, offer more hiding spaces at the beginning of the game for more anxious children (Milosevic & Radomsky, 2008; Rachman et al., 2008) or provide additional exposures to fear events when children demonstrate calmness in the face of less challenging fear events. Research by, for instance, Alsem et al. (2023) has also shown that virtual reality scenarios in which therapists could dynamically adjust the game to children's needs (by manipulating the virtual situation itself or the actions of the virtual characters), were found to be more effective in reducing aggressive behaviour than care-as-usual and CBT with roleplay practices.

Secondly, game designers should consider the specific characteristics of their target group. Our results in Part 3 revealed that youth with varying types and severity of mental health problems may react differently to (promotional messages for applied) games. In Chapter 6, we observed that youth with severe internalising symptoms reported higher levels of autonomy, competence, and affect. Therefore, it is crucial to focus on fostering autonomy and competence, particularly for youth with mild to moderate symptoms, either through game design or promotional messaging, as these factors correlate with a more positive gaming experience (Ryan et al., 2006). Moreover, it may be advantageous to design games in ways that improve mood of youth without severe depressive symptoms, especially if the game's effectiveness relies partly on short-term mood enhancements (Olson, 2010; R. Pine, Fleming, et al., 2020; Russoniello et al., 2009). Lastly, for those developing applied games to reduce anxiety, explicitly promoting the game as enjoyable may be beneficial. Although our research highlighted that youth with elevated anxiety symptoms were as likely to select the mental health game as youth with other internalising symptoms (Chapter 6) and that they showed similar engagement patterns than youth with other or milder symptoms (Chapters 3, 5 and 7), we did find that youth with elevated levels of anxiety symptoms anticipated less enjoyment from video games. Considering this aspect can ensure that youth with elevated anxiety symptoms are more inclined to take the initiative to play applied games.

Finally, in Chapter 7, our research revealed a noteworthy change in participants' stress mindset, specifically after selecting and playing the game when it was promoted as a mental health game, as opposed to an entertainment game. We posited that expectations set through pre-game trailers, guiding youth to interpret stress as manageable, drove these effects. This interpretation, coupled with the unique gameplay experiences and design inherent to Monument Valley (i.e., balancing difficulty and enjoyment through its solvable yet challenging puzzles) presumably contributed to the observed change. Previous research has demonstrated that perceiving stressful situations as challenges, where personal resources are perceived to outweigh situational demands, correlates with improved stress reactivity, more adaptive responses to stressful situations, and better academic performance, compared to situations perceived as threats, where situational demands exceed personal resources (Jamieson et al., 2010; Jamieson, Mendes, et al., 2013; Jamieson, Nock, et al., 2013; D. S. Yeager et al., 2016). The specific messages conveyed in our promotional trailer, paired with Monument Valley's ability to portray stress as a manageable challenge, likely facilitated a shift in viewing stress as more controllable and less debilitating. Considering the positive impacts of a stress-is-enhancing mindset and challenge-based appraisals of stressful situations (Crum et al., 2013; Jamieson, Mendes, et al., 2013; Keller et al., 2012; D. Park et al., 2018), game designers should be mindful of how stress-inducing scenarios are incorporated in the game. It is essential that games offer challenges without becoming excessively difficult, and attention should be given to how the game is promoted and how difficult situations within the game are communicated to players. This approach may optimise the potential for fostering positive stress mindsets among players.

IMPLICATIONS FOR IMPLEMENTATION

The research presented in this thesis also yields several implications and recommendations for implementation. Drawing on our findings in Chapters 3 to 5, along with our prior work (Schoneveld et al., 2018; Schoneveld et al., 2016), MindLight has demonstrated efficacy as a digital intervention suitable for implementation as an indicated prevention program (e.g., in school settings). It has shown effectiveness in reducing anxiety symptoms, internalising and externalising problems, and improving self-efficacy. Notably, the game's strength regarding implementation lies in the fact that supervision during game sessions can be undertaken by non-expert staff, reducing the need for clinicians or teachers and enhancing the cost-effectiveness of MindLight in comparison to other indicated prevention programs, which often rely on expert-led models of delivery in order to be effective (D. Richards & Richardson, 2012; Scholten & Granic, 2019). Furthermore, our research indicates that the game's effectiveness is not contingent on the severity of presented anxiety problems, the level of youth's self-efficacy, or maternal mental health. Additionally, factors such as symptom severity, expectations for improvement, and motivation to change did not significantly influence game experiences and in-game play behaviours. These findings suggest that MindLight is a viable prevention program suitable for children exhibiting elevated anxiety symptoms.

In this thesis, we not only uncovered promising results for MindLight but also our systematic review in Chapter 2 highlighted the potential of a diverse range of both applied and casual games to enhance mental health among youth with (sub)clinical symptom levels. Particularly noteworthy is that many of these games proved as effective as traditional, expert-led clinical gold standard programs, even when utilised without the direct involvement of therapists. These findings offer a potential solution to the escalating mental health problems and the subsequent widening gap between the demand and supply of mental health services (Bijl et al., 2003; Collishaw, 2015; Olfson et al., 2015). This gap is exacerbated by the shortage of mental health professionals and long waiting lists (Van Den Broek et al., 2022; Vektis, 2022).

To address the growing demand for mental health services, effective applied games that do not necessitate expert supervision could be provided to youth while they are on the waiting list for mental health services. This approach may prevent demotivation among youth facing long waiting times. By offering access to applied games proven to be effective in reducing (elevated) mental health symptoms, it becomes possible to alleviate some of their mental health challenges early on and potentially shortening the overall duration of therapeutic services or its necessity at all. Additionally, engaging with a video game during this waiting period may serve as a non-threatening and non-stigmatising entry point for youth into their mental health service journey (McGonigal, 2011), further enhancing engagement and motivation and thereby the likelihood that youth will actively participate in subsequent mental health services (Fleming et al., 2017; Granic et al., 2014; Kazdin, 2015). While the implementation of games during the waitlist period holds promise, future research should examine the specific conditions under which this implementation is most fruitful and how it should be structured in practice. It is suggested that the evaluation of this approach is conducted in the exact setting where it will be deployed (Mohr et al., 2017).

While the advantages of video games lie in their ability to be distributed at low cost and with minimal effort, offering substantial scalability and cost-effectiveness (Granic et al., 2014; Kazdin, 2015), it is crucial not to underestimate the initial investment required. Designing and testing truly engaging and intrinsically motivating games require partnering up with professional game designers, as well as entail considerable monetary expenses and time commitments. Additionally, practical and financial implementation hurdles must be addressed, such as ensuring the availability and suitability of laptops for game installation and establishing help desk services to address questions regarding the practical use of the game (Barnes et al., 2023; Wykes & Brown, 2016). However, once these initial requirements are in place, providing games to youth on the waiting lists may ultimately prove to be a cost-effective solution compared to allowing untreated symptoms to persist over an extended period of time. Untreated symptoms can lead to prolonged treatment duration and an increased likelihood of persistent symptoms and chronic courses of mental health conditions (Aalto-Setälä et al., 2002; Copeland et al., 2014; Lewinsohn et al., 2000; D. S. Pine et al., 1999). Therefore, while the upfront investment may be substantial, the long-term benefits in terms of improved mental health outcomes and reduced treatment duration could outweigh these costs.

Given the considerable potential of games as standalone applications or freely accessible (commercial) games outside clinical settings, functioning as self-help tools for individuals with mild symptoms, we dedicated Part 3 of this thesis to exploring their implementation. Here, our focus was on the uptake of and engagement with a game promoted as a mental health tool among individuals experiencing mental health symptoms. Our aim was to gain insights into how to customise and tailor the promotion of mental health games, with the goal of maximising their uptake and sustained use. Our findings suggest that youth experiencing mild to severe internalising symptoms are receptive to game-based mental health interventions. A game promoted specifically as a mental health tool emerges as an appealing alternative for them, although an attractive trailer design appeared to be a crucial factor and promotional messages for mental health games should thus convey both the game's effectiveness as well as its engaging nature. These results support the notion that video games designed for mental health represent a promising approach to reaching individuals with mental health symptoms, including those who may not be inclined to seek professional help. The results are especially promising in light of previous research showing that only a quarter to a third of individuals with mental health symptoms seek regular professional help (Alonso, Angermeyer, & Lépine, 2004; Merikangas et al., 2011) due to barriers related to motivation or the believe that emotions cannot be regulated (Burnette et al., 2020; Lawrence et al., 2017). Therefore, promoting the mental health benefits of video games intended to improve mental health could enhance their effectiveness.

It is also essential to consider the use of casual games more generally for improving mental health. There is a growing acknowledgement of the significance of games in our lives – games have the power to evoke positive emotions and foster social connections (McGonigal, 2011). Encouragingly, our systematic review showed the potential of casual games for improving mental health, indicating that casual games demonstrated effectiveness comparable to applied games and active conditions in the majority of studies. This prompts further investigation into their effectiveness and underlying mechanisms, and suggests that they may also be used in clinical practice (e.g., Silva et al., 2021). Casual games are easily accessible and scalable through mainstream media channels and game platforms. Previous research has also shown that youth already play casual games to relieve stress and feel more relaxed, and that they find the idea of casual games focussing on mental health interesting and appealing (R. Pine, Sutcliffe, et al., 2020), suggesting that casual games may be a viable option to explore as well.

Finally, our findings support the notion that applied games can be effective for multiple mental health problems simultaneously, and that it is possible to target transdiagnostic mechanisms within applied and casual games. Our results in Chapter 4 demonstrated that MindLight, next to anxiety symptoms, has beneficial long-term effects on other mental health outcomes associated with anxiety symptoms (i.e., internalising problems, externalising problems, and self-efficacy). Furthermore, in our systematic review presented in Chapter 2, we identified several instances of applied games where effects were found in multiple mental health domains (e.g., on anxiety and depression, Knox et al., 2011; Merry, Stasiak, et al., 2012; Schuurmans, Nijhof, Popma, et al., 2021; and on both anxiety and externalising problems, Schuurmans et al., 2018). Also for casual games, we found promising results in a wide range of mental health domains, which may be attributed to their ability to counteract mechanisms defined as transdiagnostic, such as negative affect and rumination (Schaeuffele et al., 2021). A transdiagnostic prevention approach to anxiety and depression, or psychopathology in general (Fraire & Ollendick, 2013; Granic, 2014), may potentially result in a more significant benefit from prevention programs, including reduced overall treatment duration and cost-effectiveness, as well as facilitating the dissemination of evidence-based programs (Chu et al., 2015; Craske, 2012; Dozois et al., 2009; Weisz et al., 2014). Several reviews and meta-analyses have indeed shown the promise of transdiagnostic interventions (Schaeuffele et al., 2023; Schaeuffele et al., 2024). Another recent example from David and Fodor (2022) has been reported, in which they explicitly used a transdiagnostic framework to develop an applied game for emotional disorders in youth. To enhance the cost-effectiveness of games, which are already expensive to design, future research should prioritise incorporating a transdiagnostic approach in games used to improve mental health.

CONCLUSION

In the current thesis we examined the potential of applied and casual games for improving mental health in youth, and investigated the influence of nonspecific (motivational) factors on game selection, game experiences, engagement, and mental health outcomes. Our state-of-the-art overview of the field showed the potential of both applied and casual games to alleviate (sub)clinical symptoms of various mental health issues among youth. Noteworthy is that many of the digital games proved as effective as traditional, expert-led clinical gold standard programs. Our research on the applied game MindLight demonstrated its effectiveness in reducing anxiety symptoms, internalising and externalising problems, and improving self-efficacy. Nonspecific factors did not serve as predictors of effectiveness, nor did they influence game experiences and engagement. Our experimental study on (nonspecific) motivational factors compellingly showed that explicitly promoting a video game for mental health by means of an appealing trailer design is attractive for youth with mental health symptoms, even those less inclined to seek professional help. Nonspecific factors did not influence the likelihood of selecting a mental health game nor engagement with it. Still, we argue that more research is needed to assess the strength of evidence regarding nonspecific factors as well as to assess possible nuanced effects across different age groups, demographic and other individual differences. When examining motivational factors and the selection of mental health games, we recommend that future research also considers naturalistic gameplay and the complexity of the media choices made by youth on a daily basis. Taken together, the current thesis demonstrates that digital interventions are a viable alternative to traditional intervention approaches, offering a potential solution to the escalating mental health problems of youth, the growing demand for mental health services, and the social and practical access barriers related to conventional approaches. Our research further showed that video games designed to improve mental health can be positioned and promoted as such, leveraging their motivational and engaging potential to effectively reach and benefit individuals with mental health symptoms.

Appendices

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DUTCH SUMMARY (NEDERLANDSE SAMENVATTING)

Angst- en depressieve klachten zijn de meest voorkomende mentale gezondheidsproblemen bij jongeren. Tot wel een kwart van de jongeren ontwikkelt een angststoornis en/of depressieve stoornis tegen de tijd dat ze volwassen zijn. De symptomen en bijkomende negatieve gevolgen vragen om effectieve preventieprogramma's. Daarbij is het belangrijk dat preventieprogramma's aantrekkelijk, motiverend en toegankelijk zijn. Video games zouden mogelijk als veelbelovende interventie voor mentale gezondheidsproblemen ingezet kunnen worden.

De afgelopen decennia is er een groeiende belangstelling ontstaan voor het gebruik van *applied*, ofwel toegepaste video games als middel om de mentale gezondheid te bevorderen. Deze games zijn speciaal gemaakt om spelers specifieke kennis aan te leren en/of vaardigheden te laten oefenen. Ook de effecten van *casual* games op de mentale gezondheid worden steeds vaker onderzocht. Dit zijn vrij verkrijgbare commerciële games die ontwikkeld zijn voor recreatieve doeleinden en het plezier van de speler. Onderzoek laat de potentie van applied en casual video games voor mentale gezondheid zien, maar een uitgebreid overzicht van de huidige literatuur ontbreekt. Daarom was het eerste doel van dit proefschrift om een uitgebreid overzicht te geven van de effectiviteit van video games voor het verbeteren van de mentale gezondheid bij jongeren.

Een beperking in het vele onderzoek naar de effectiviteit van applied games is dat de invloed van *niet-specifieke factoren* vaak niet is onderzocht. Niet-specifieke factoren verwijzen hierbij naar aspecten die niet direct gerelateerd zijn aan de specifieke inhoud of technieken in de applied game zelf, maar aan bredere invloeden die kunnen bijdragen aan de effectiviteit van de applied game. Zo zouden bijvoorbeeld iemands verwachtingen ten aanzien van de effectiviteit van de game, de ernst van iemands klachten, de motivatie of bereidheid om mentale gezondheidsklachten te veranderen en iemand houding ten opzichte van emoties en stress van invloed kunnen zijn op hoe men interacteert met een video game en de uiteindelijke effectiviteit ervan. Daarom was het tweede doel van dit proefschrift om de effecten van niet-specifieke factoren in de applied game MindLight te onderzoeken.

Tot slot hebben we in dit proefschrift onderzoek gedaan naar het interventiebereik van video games voor mentale gezondheid. Video games bieden een aanzienlijk voordeel als het gaat om implementatie. Aan de ene kant kunnen effectieve applied en casual games gebruikt worden in een klinische setting als aanvulling op traditionele interventies, of als vervanging van bijvoorbeeld school-brede preventieprogramma's. Aan de andere kant kunnen video games ook buiten de klinische setting aangeboden en gebruikt worden, bijvoorbeeld als 'standalone' en/of vrij toegankelijk spel. Het is daarbij echter wel essentieel om te onderzoeken welke factoren bijdragen aan de motivatie om dergelijke games te spelen en of deze factoren van invloed zijn op spelervaringen en interactie met de games. In het derde deel van dit proefschrift hebben we daarom onderzocht of de manier waarop een video game voor mentale gezondheid wordt aangeboden van invloed is op de keuze en ervaringen met het spel, alsmede de invloed van de ernst en het type van iemands klachten, de motivatie of bereidheid om mentale gezondheidsklachten te veranderen en iemands houding ten opzichte van emoties en stress.

Onderzoeksresultaten

In Hoofdstuk 2 hebben we middels een systematische review uitgebreid inzicht gegeven in de effectiviteit van applied en casual games op de mentale gezondheid van jongeren. Onze resultaten laten zien dat applied games het meest effectief bleken te zijn voor het verbeteren van sociale vaardigheden, verbaal geheugen en angstsymptomen, terwijl casual games het meest effectief waren voor het verbeteren van depressie-, angst- en ADHD-symptomen. Daarnaast werden applied en casual games effectief bevonden voor het verminderen van angstklachten in medische settingen (bv. tijdens een operatie) en voor het verbeteren van zowel positieve als negatieve gemoedstoestanden. Samengevat tonen onze bevindingen uit Hoofdstuk 2 aan dat video games ingezet kunnen worden voor het verbeteren van de mentale gezondheid van jongeren.

In de Hoofdstukken 3, 4 en 5 hebben we specifieke en niet-specifieke voorspellers van betrokkenheid en effectiviteit onderzocht in MindLight. Dit is een applied game om angst bij jongeren te verminderen. Het spel heeft tot doel om jongeren op een speelse manier te leren hoe ze met angst om kunnen gaan. MindLight integreert drie technieken uit de cognitieve gedragstherapie, namelijk ontspanning, 'exposure' (d.w.z. blootstelling aan angstaanjagende situaties of objecten) en aandachtsbias modificatie. Deze laatste techniek heeft tot doel om de hyperaandacht op mogelijke bedreigingen te verminderen. In eerder onderzoek is de effectiviteit van MindLight op het verminderen van angstklachten al aangetoond.

In Hoofdstuk 3 vonden we dat speelgedrag dat spelers ondersteund bij het oefenen van blootstelling aan angstaanjagende situaties of objecten, leidde tot verminderde angstsymptomen drie maanden later. Niet-specifieke factoren zoals de ernst van de angstklachten, verwachtingen over de effectiviteit van

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MindLight en de motivatie om angstklachten te verminderen waren niet van invloed op het speelplezier en de manier waarop jongeren MindLight speelden (Hoofdstukken 3 en 5). De resultaten uit Hoofdstuk 4 laten verder zien dat de effectiviteit van MindLight niet werd beïnvloed door niet-specifieke factoren zoals de ernst van de angstklachten, de mentale gezondheid van de moeder en de zelfeffectiviteit van de kinderen. Zelfeffectiviteit verwijst daarbij naar het geloof van een individu in zijn of haar vermogen om specifieke taken uit te voeren of om bepaalde doelen te bereiken, met name in moeilijke situaties of bij het omgaan met uitdagingen. Tot slot vonden we dat MindLight even effectief is als cognitieve gedragstherapie in het verminderen van internaliserende en externaliserende problemen, en het vergroten van de zelfeffectiviteit.

Samengevat tonen de resultaten van de studies beschreven in Hoofdstukken 3, 4 en 5 aan dat MindLight een effectieve interventie is voor het verminderen van angstklachten, alsmede het verbeteren van meer algemene gezondheids-uitkomsten die verband houden met angst. Cruciaal daarbij is dat de waargenomen verbeteringen tot 6 maanden na de interventie aanhielden. Onze bevindingen suggereren verder dat niet-specifieke factoren geen invloed hebben op de effectiviteit van en interactie met de applied game, en dat zowel jongeren met hoge als lage verwachtingen, motivatie en angstsymptomen evenveel baat hebben bij MindLight.

In de Hoofdstukken 6 en 7 van dit proefschrift onderzochten we de keuze voor en de interactie met een (casual) game die werd gepromoot als spel voor het verbeteren van mentale gezondheid. Deelnemers aan ons onderzoek waren jongeren met milde tot ernstige depressieve, angst- en stresssymptomen. Zij kregen twee trailers te zien voor wat op het eerste gezicht twee verschillende video games leken te zijn. Echter, in werkelijkheid ging het om hetzelfde spel, waarbij de ene trailer het spel promootte als vermakelijk en leuk, terwijl de andere trailer het spel promootte als bevorderlijk voor de mentale gezondheid. Jongeren werd gevraagd om aan te geven welk spel zij wilden spelen.

De resultaten uit Hoofdstuk 6 tonen overtuigend aan dat een trailerboodschap waarin de positieve effecten van het spel op de mentale gezondheid worden uitgelicht, jongeren niet afschrikt maar juist de aantrekkelijkheid van een dergelijke video game vergroot. Echter, het bleek dat de aantrekkelijkheid van de trailer daarbij wel van belang is voor de keuze om een video game voor mentale gezondheid te gaan spelen. Ook verwachtten jongeren met verhoogde angstklachten vooraf minder plezier bij het spelen van het spel. Resultaten uit Hoofdstuk 7 lieten verder zien dat de motivatie of bereidheid om mentale gezondheidsklachten te veranderen en iemands houding ten opzichte van emoties en stress geen invloed hadden op de keuze om een spel voor mentale gezondheid te gaan spelen. Ook was de ernst en het type van iemands klachten niet van invloed op de spelkeuze (Hoofdstuk 6). We kunnen dus concluderen dat niet-specifieke factoren geen invloed hebben op de keuze om een video game voor mentale gezondheid te gaan spelen.

Wat betreft de interactie met het spel zelf ontdekten we dat niet-specifieke factoren geen invloed hebben op de speelduur of algehele spelervaring. Verkennende analyses in Hoofdstuk 6 toonden echter wel aan dat jongeren met ernstige symptomen zich autonomer en competenter voelden tijdens het spelen en dat jongeren met ernstige depressieve symptomen zich positiever voelden na het spelen. Deze resultaten suggereren dat het aanbieden van een spel gepromoot voor mentale gezondheid met name belangrijk kan zijn voor jongeren die ernstige mentale gezondheidsproblemen ervaren. Tot slot lieten de resultaten uit Hoofdstuk 7 zien dat jongeren neutraler tegenover de aard van stress stonden na het spelen van de game die gepromoot werd als spel voor het verbeteren van mentale gezondheid.

Samengevat zijn onze bevindingen in Hoofdstuk 6 en 7 veelbelovend en tonen ze aan dat expliciete berichten over de effecten van een game op de mentale gezondheid geen nadelige invloed hebben op de keuze om een dergelijke game te spelen en ervaringen met het spel zelf. Het expliciet promoten van video games voor mentale gezondheid is een veelbelovende benadering om jongeren met mentale gezondheidsproblemen te bereiken, zelfs degenen die minder geneigd zijn om professionele hulp te zoeken.

Implicaties en Conclusie

De bevindingen van dit proefschrift en ons al eerder uitgevoerde onderzoek laten zien dat MindLight kan worden ingezet als een preventieprogramma op scholen om angstklachten, internaliserende en externaliserende problemen te verminderen en de zelfeffectiviteit te verbeteren bij kinderen met verhoogde angstklachten. Dit kan ongeacht (de ernst van) de mentale gezondheids-problemen of moeilijkheden die kinderen of hun ouders ervaren. Het inzetten van een applied game zoals MindLight kan kostenbesparend zijn, omdat er geen therapeuten of leraren bij betrokken hoeven te zijn en er geen speciale training nodig is om de kinderen tijdens het spelen te begeleiden. MindLight zou ingezet kunnen worden als een preventieprogramma dat minder stigmatiserend en beter toegankelijk is, waardoor kinderen gemotiveerder zijn om deel te nemen aan een preventieprogramma.

Ook onze systematische review laat het potentieel van een divers scala aan applied en casual games zien om de mentale gezondheid bij jongeren te verbeteren. Bijzonder opmerkelijk daarbij is dat veel van de onderzochte

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spellen even effectief bleken te zijn als traditionele, door experts geleide interventies, zelfs wanneer de games gebruikt werden zonder directe betrokkenheid van therapeuten. Deze bevindingen impliceren dat effectief bevonden applied of casual games ook aangeboden zouden kunnen worden aan jongeren die op de (lange) wachtlijst staan voor behandeling van hun mentale gezondheidsklachten. Op deze manier is het mogelijk om mentale gezondheidsklachten al vroegtijdig te verlichten en mogelijk de totale duur van een behandeling te verkorten of deze helemaal overbodig te maken. Daarnaast kan het spelen van een video game tijdens de wachttijd de betrokkenheid en motivatie van jongeren verbeteren en daarmee de kans vergroten dat jongeren actief deelnemen aan de behandeling voor hun mentale gezondheidsklachten. Het aanbieden van video games aan jongeren op de wachtlijst zou daarmee een kosteneffectieve oplossing kunnen zijn, die tegelijkertijd de lange wachtlijsten voor een behandeling doen terugdringen.

Onze systematische review liet verder zien dat veel studies die de effectiviteit van video games onderzochten, gebruik maakten van zogenoemde multimodale interventies. Dit zijn interventies waarbij jongeren een video game speelden in aanvulling op de gebruikelijke behandeling of andere therapeutische componenten. Het is daarmee duidelijk dat multimodale interventies veelvuldig worden gebruikt in de klinische praktijk. Toekomstige game ontwikkelaars zouden dan ook rekening moeten houden met het feit dat applied games vaak in of naast een (traditionele) behandeling gebruikt worden. Ontwikkelaars zouden moeten overwegen om functies te implementeren die relevant zijn voor therapeuten. Het zou bijvoorbeeld nuttig kunnen zijn om een codeersysteem in het spel te integreren en een interface voor therapeuten om zo het gedrag van jongeren in het spel te kunnen monitoren. Dit kan therapeuten inzicht geven in hoe de jongeren interacteren met de therapeutische technieken in het spel alsmede om de voortgang bij te houden en vroegtijdige ondersteuning te kunnen bieden, bijvoorbeeld door het spel aan te passen aan de behoeften en het leertempo van de jongere. Daarnaast kan inzicht in het eigen speelgedrag ook relevant zijn voor jongeren om van te leren en hun motivatie te behouden tijdens de behandeling. Tot slot kan inzicht in het speelgedrag ook onderzoekers en game ontwikkelaars informeren over de meest effectieve elementen van het spel, om zo de effectiviteit verder te verbeteren.

Om de inzet van video games voor mentale gezondheid te maximaliseren is het ook van belang om rekening te houden met kenmerken van de specifieke doelgroep en hoe het spel wordt gepromoot. Een aantrekkelijke trailer blijkt cruciaal voor de keuze om een spel voor mentale gezondheid te gaan spelen.

Verder lieten onze resultaten zien dat jongeren met verschillende soorten en ernst van mentale gezondheidsproblemen anders reageren op (promotie-boodschappen voor) games voor mentale gezondheid. In de ontwikkeling van video games voor de mentale gezondheid is het belangrijk om te focussen op het bevorderen van autonomie en competentie tijdens het spelen, vooral voor jongeren met milde tot matige symptomen. Voor jongeren met angstklachten is het van belang om in de trailerboodschap nog explicieter stil te staan bij speelplezier. Gezien het bijna onbeperkte aantal video games waar jongeren dagelijks uit kunnen kiezen is toekomstig onderzoek nodig om de complexiteit van mediakeuzes van jongeren te onderzoeken alsmede hoe dit inspeelt op de langdurige betrokkenheid van jongeren bij een spel voor mentale gezondheid.

Samenvattend laten de resultaten van dit proefschrift zien dat applied en casual video games ingezet kunnen worden om de mentale gezondheid bij jongeren te verbeteren. Jongeren ervaren deze video games als een aantrekkelijk, motiverend en toegankelijk alternatief. Gezien het tekort aan behandelaren en de lange wachtlijsten in de mentale gezondheidszorg, zijn video games een welkome oplossing om jongeren te bereiken en hun mentale gezondheid op grote schaal te verbeteren.

RESEARCH DATA MANAGEMENT STATEMENT

The research in this dissertation was conducted in compliance with the General Data Protection Regulation (GDPR) and all applicable laws and ethical guidelines. When applicable, the Ethics Committee Social Sciences (ECSS) of Radboud University, Nijmegen, the Netherlands has given a positive advice to conduct these studies to the Dean of the Faculty, who formally approved the conduct of these studies. For the research reported in Chapter 5, ethics clearance was given by the Queen's University Health Sciences & Affiliated Teaching Hospitals Research Ethics Board (HSREB) in Kingston, Ontario, Canada. All participants provided active informed consent. Signed consent forms are stored in the central archive of Radboud University, or at Queen's University (Chapter 5 – main experiment). An overview of the ethics approval numbers can be found in Table 1. This table also includes information on funding that supported the research in this dissertation.

Table 1 Overview of ethics approval numbers and funding information

Chapter	Ethics approval number	Funding
1	N/A (general introduction)	Not applicable
2	N/A (systematic review)	Dutch Research Council (NWO; grant # 406-16-524, A. Wols); Behavioural Science Institute, Radboud University
3	EC2013-0410-139a1	Dutch Research Council (NWO; grant # 406-16-524, A. Wols)
4	EC2013-0410-139a1	Dutch Research Council (NWO; grant # 406-12-017 for E.A. Schoneveld, and grant # 406-16-524 for A. Wols)
5	Pilot trailer manipulation: ECSW2017-2808-530 Main experiment: HSREB 6019310 PSYC-187-16, August 9 and September 18, 2017	Dutch Research Council (NWO; grant # 406-16-524, A. Wols) Ontario Mental Health Foundation; Dutch Research Council (NWO; grant # 406-16-524, A. Wols)
6	ECSW2017-3001-461 and ECSW2017-3001-461a	Dutch Research Council (NWO; grant # 406-16-524, A. Wols); Behavioural Science Institute, Radboud University
7	ECSW2017-3001-461 and ECSW2017-3001-461a	Dutch Research Council (NWO; grant # 406-16-524, A. Wols); Behavioural Science Institute, Radboud University
8	N/A (general discussion)	Not applicable

To ensure confidentiality of participants' data, random individual subject codes were used. A pseudonymization key linked these random subject codes with identifiable personal information. Pseudonymization keys were stored separately from the research data in an encrypted or password secured file and only accessible to members of the project who needed access to it because of their role within the project. The key files were destroyed after data processing was completed.

Radboud University and the Behavioural Science Institute (BSI) have set strict conditions for the management of research data. All research data resulting from this dissertation were handled in accordance with the university's research data management policy (https://www.ru.nl/rdm) and the BSI's research data management protocol (https://www.radboudnet.nl/bsi/rdm). To increase the usefulness of the data collected, research data management was conducted according to the FAIR principles (Findable, Accessible, Interoperable, Reusable).

The full datasets and research documentation of the different studies presented in this thesis are stored on project network drives of the Radboud University. More specifically, data and documentation related to Chapters 2, 3, and 5 are stored at FSW-BSI-DP-PhD_Aniek_Wols.lnk. For Chapter 4, data and documentation are stored at FSW-BSI-DP-PhD_Elke_Schoneveld.lnk. For Chapters 6 and 7, data and documentation are stored at FSW-BSI-DP-Messaging_Study.lnk. The data were accessible to all members involved in the relevant project.

To enhance open science and transparent research practices, the research in Chapters 4 and 5 were (pre)registered (see https://onderzoekmetmensen.nl/en/trial/29118; https://osf.io/6gmwv and https://osf.io/j7mvu, respectively). Furthermore, most publication packages associated with the publications in this dissertation were registered in the Research Information Services (RIS) system of Radboud University and deposited in the Radboud Data Repository (RDR) as a Data Sharing Collection (DSC; https://data.ru.nl). Depending on the nature and context of the data, appropriate access levels and licenses were applied to the different publication packages. The publication package associated with Chapter 4 was stored by responsible researcher E.A. Schoneveld on her project network drive only, in accordance with research data management procedures that applied at that time. The publication package associated with Chapter 5 was not deposited in the Radboud Data Repository due to legal and ethical concerns, as this study was conducted at Queen's University in Kingston, Ontario, Canada.

Table 2 below details where the publication package for each chapter can be found. All data remain available for at least 10 years after termination of the studies.

Table 2 Overview of the publication packages in this dissertation

Chapter	Data Sharing Collection identifier	Access level	License	DOI
2	ru.bsi.dp_2024_aw_games_ mentalhealth_youth_dsc_228	Open access	CC-BY-4.0	10.34973/t6df-m766
3	ru.bsi.dp_2024_aw_ in-gameplaybehaviours_ mindlight_anxiety_dsc_536	Restricted access	RU-RA-DUA-1.0	10.34973/9a2e-jq89
4	N/A	N/A	N/A	N/A
5	Pilot trailer manipulation: ru.bsi.dp_2024_aw_ expectations_mindlight_ trailer_dsc_550 Main experiment: N/A	Open Access for Registered Users	RU-HD-SU-1.0	10.34973/b8fr-0822
6	ru.bsi.dp.2022.mp.messaging. impact_dsc_399	Open Access for Registered Users	RU-HD-1.1	10.34973/pcj8-bp23
7	ru.bsi.dp.2022.mp.messaging. impact_dsc_399	Open Access for Registered Users	RU-HD-1.1	10.34973/pcj8-bp23

A

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PUBLICATION LIST

Published manuscripts included in this dissertation

Wols, A., Pingel, M., Lichtwarck-Aschoff, A., & Granic, I. (2024). Effectiveness of applied and casual games for young people's mental health: A systematic review of randomised controlled studies. *Clinical Psychology Review, 108*, 102396. https://doi.org/10.1016/j.cpr.2024.10239

Wols, A., Lichtwarck-Aschoff, A., Schoneveld, E. A., & Granic, I. (2018). In-game play behaviours during an applied video game for anxiety prevention predict successful intervention outcomes. *Journal of Psychopathology and Behavioral Assessment, 40*(4), 655-668. https://doi.org/10.1007/s10862-018-9684-4

Schoneveld, E. A., **Wols, A**., Lichtwarck-Aschoff, A., Otten, R., & Granic, I. (2020). Mental health outcomes of an applied game for children with elevated anxiety symptoms: A randomized controlled non-inferiority trial. *Journal of Child and Family Studies, 29*(8), 2169-2185. https://doi.org/10.1007/s10826-020-01728-y

Wols, A., Hollenstein, T., Lichtwarck-Aschoff, A., & Granic, I. (2021). The effect of expectations on experiences and engagement with an applied game for mental health. *Games for Health Journal, 10*(4), 207-219. https://doi.org/10.1089/g4h.2020.0155

Poppelaars, M., **Wols., A**., Lichtwarck-Aschoff, A., & Granic, I. (2018). Explicit mental health messaging promotes serious video game selection in youth with elevated mental health symptoms. *Frontiers in Psychology, 9*, 384784. https://doi.org/10.3389/fpsyg.2018.01837

Wols, A., Poppelaars, M., Lichtwarck-Aschoff, A., & Granic, I. (2020). The role of motivation to change and mindsets in a game promoted for mental health. *Entertainment Computing, 35*, 100371. https://doi.org/10.1016/j.entcom.2020.100371

Other publications

Bossenbroek, R., **Wols, A**., Weerdmeester, J., Lichtwarck-Aschoff, A., Granic, I., & Van Rooij, M. M. (2020). Efficacy of a virtual reality biofeedback game (DEEP) to reduce anxiety and disruptive classroom behavior: single-case study. JMIR Mental Health, 7(3), e16066. https://doi.org/10.2196/16066

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Wols, A., Scholte, R. H. J., & Qualter, P. (2015). Prospective associations between loneliness and emotional intelligence. Journal of Adolescence, 39, 40-48. https://doi.org/10.1016/j.adolescence.2014.12.007

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ABOUT THE AUTHOR

Aniek Wols (1991) was born in Mijnsheerenland, the Netherlands. She obtained her bachelor's degree in Pedagogical and Educational Sciences at Radboud University in 2013 (cum laude). As part of the Honours program she was enrolled in during her bachelor studies, she spent three months abroad writing her bachelor thesis at the University of Central Lancashire in Preston, United Kingdom. After obtaining her bachelor's degree, Aniek wrote an extra-curricular master thesis. In September 2014, she started with the research master Behavioural Science at Radboud University and graduated in the summer of 2016. Subsequently, Aniek started her PhD project at the Developmental Psychopathology group at the Behavioural Science Institute (BSI) at Radboud University, which was funded by a Talent grant from the Dutch Research Council (NWO). During her PhD project, Aniek visited the Adolescent Dynamics Lab at Queen's University in Kingston (Ontario, Canada) to collaborate on a research project. Besides her research work, Aniek has taught several courses in the bachelor Pedagogical and Educational Sciences (2016-2020), supervised several master students writing their theses for the master Pedagogical Sciences (2018-2020) and gave guest lectures in bachelor and master courses. Since 2017, Aniek combined her PhD research with work for the Ethics Committee Social Sciences at Radboud University. She was the secretary of the ethics committee for five years (2018-2023). Since August 2023, Aniek is working as privacy officer for the Faculty of Social Sciences at Radboud University. She also followed additional training and successfully obtained her Certified Information Privacy Professional/Europe (CIPP/E) certificate in January 2024.

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IN DEI NOMINE FELICITER

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