Title
The Effect of Digital Interventions on Sleep and Exploring the Role of Self-Efficacy

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THE EFFECT OF DIGITAL INTERVENTIONS ON SLEEP AND EXPLORING THE ROLE OF SELF-EFFICACY: A META-ANALYSIS

A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy

In

Psychological Sciences

by

Amber Carmen Arroyo

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2022

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This dissertation is dedicated to my parents.

Los amo con toda mi alma.

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Arroyo, A. C., Winter, S. D., Depaoli, S., Zawadzki, M. J. (2021). Illuminating differences in the psychological predictors of academic performance for first- and continuing-generation students. *Journal of Educational and Psychological Research, 3*(2): 234-246.

Tiemensma, J., Depaoli, S., Winter, S. D., Felt, J. M., Rus, H. M., **Arroyo**, A. C. (2018). The performance of the IES-R for Latinos and non-Latinos: Assessing measurement invariance. *PLoS ONE, 13*(4): e0195229.

ABSTRACT

Dissertation Title: The Effect of Digital Interventions on Sleep and Exploring the Role of Self-Efficacy: A Meta-Analysis
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Background: The World Health Organization has officially recognized inadequate sleep as a public health issue, yet 30% of Americans do not meet the minimum requirement for sufficient sleep. Digital interventions delivered through websites and smartphone apps are an increasingly prevalent tool to address inadequate sleep, although there is mixed evidence on their efficacy.

Objective: This study aimed to answer the following: (1) Are digital interventions aimed at promoting sleep efficacious in improving sleep outcomes? (2) Is the efficacy of digital interventions on sleep outcomes moderated by sleep dimension, hygiene, or measurement, along with other study characteristics? (3a) Is the efficacy of digital interventions on sleep outcomes mediated by self-efficacy? (3b) Do digital interventions using self-efficacy behavior change techniques (BCTs) lead to changes in sleep outcomes?

Method: A systematic review and meta-analysis following PRISMA guidelines examined articles on randomized controlled trials for sleep-promoting digital interventions retrieved from three scientific databases. Intervention effect sizes (Cohen's *d*), method of sleep measurement (self-report or electronic), and self-efficacy BCTs (the eight BCTs identified by the Human Behavior Change Project as 'linked' to changes in self-efficacy) were extracted from all studies. The average bias-corrected effect of digital interventions on sleep outcomes was computed using multi-level meta-analysis (RQ1). Effects of key moderators including method of sleep measurement (RQ2) and the number of self-efficacy BCTs used in an intervention (RQ3) were tested using meta-regression.

Results: Forty samples met eligibility criteria. Digital interventions had a moderate-to-large effect size on sleep outcomes, Cohen's $d = 0.670$, SE = 0.103, k = 193, $t(192)=6.519$, $p < .001$, 95% CI [0.467, 0.872]. Sleep dimension, method of measurement, mode of intervention delivery, and intervention focus significantly moderated the main effect, while sleep hygiene, number of self-efficacy BCTs, funding source, name of digital intervention program, intervention length, health condition at baseline, and comparison group, did not. We were unable to test if the construct self-efficacy mediated the main effect due to insufficient reporting of data necessary to run the analysis.

Conclusions: The current study contributes to a growing body of research finding that digital health interventions are an effective tool to improve a range of health behaviors, including sleep. We found evidence that the way sleep is defined and measured can significantly affect the reported efficacy of a digital intervention on sleep, and implications and future directions for all moderators are discussed.

INTRODUCTION AND RATIONALE

Thirty percent of Americans do not meet the minimum requirement for sufficient sleep (7-9 hours; Liu et al., 2016; National Sleep Foundation, 2020). Inadequate sleep costs the United States economy up to $411 billion annually and is implicated in the occurrence and development of several chronic diseases including cardiovascular disease, diabetes, obesity, and cancer (Jackson, 2015). The World Health Organization and the National Institutes of Medicine have officially recognized inadequate sleep as a public health issue (Altevogt & Colten, 2006). Digital interventions (i.e., interventions delivered through websites or smartphone applications/apps) have a growing body of research to support their potential to improve sleep outcomes (Chu et al., 2018; Horsch et al., 2017; Luik et al., 2017; Pulantara et al., 2018). A recent systematic review concluded that the majority of studies examining fully-automated mHealth app interventions reported a positive impact on sleep outcomes (Arroyo & Zawadzki, 2022). However, to date there is no meta-analytic synthesis of digital sleep interventions to examine their overall effect size and potential moderators of their efficacy. Likewise, a systematic understanding of how an intervention exerts its effects on sleep (i.e., its mediator) has not been conducted for digital sleep interventions. The purpose of this study is to systematically review and meta-analyze digital sleep interventions to determine their efficacy with sleep outcomes, and to examine if self-efficacy is implicated as a mediator through which they are effective.

Sleep Outcomes

To determine the efficacy of digital interventions for sleep, it is first important to operationalize sleep. It is not simple to define sleep because it is a multifaceted construct composed of different dimensions, methods of measurement, and behaviors, all of which go into the current review's definition of sleep outcomes. An influential model of sleep outcomes proposes that there are five dimensions (Buysse, 2014): sleep quality (satisfaction with sleep), sleep duration (total amount of sleep over a 24 hour period), sleep continuity (ease of falling asleep and staying asleep), sleep timing (placement of sleep in a 24 hour period), and sleepiness (ability to maintain wakefulness). Complicating the story more, these dimensions can either be measured through self-report methods (i.e., subjective appraisal of how one is sleeping) or electronic methods of measurement (i.e., an electronic device-driven observation of sleep parameters). Research suggests dimensions produce non-redundant information as dimensions do not always change together after an intervention (Oginska & Pokorski, 2006), and method of sleep measurement can sometimes differentially predict treatment efficacy for sleep (Lauderdale et al., 2008). For example, one digital intervention improved sleep quality but sleep duration remained the same (Murawski et al., 2019), another study found no correlation between participants' self-report and electronic measures of sleep duration (Rotenberg et al., 2000), and a review noted multiple reports of individuals with objectively (electronically) normal sleep but clinically significant self-report insomnia (Edinger et al., 2000). Similarly, there have been several other studies with inconsistent findings of how (or whether) the sleep dimensions and methods of sleep measurement correlate with one another (Argyropoulos et al., 2003; Armitage et al., 1997; Edinger et al., 2000; Lauderdale et al., 2008; Rotenberg et al., 2000).

Another operationalization of sleep outcomes is sleep hygiene, which is a set of environmental and behavioral recommendations to promote sleep (Irish et al., 2015). These practices include sleep scheduling and timing, eating/drinking behaviors that influence sleep, arousal-related behaviors close to bedtime, the use of bed for activities other than sleep, and the comfort of one's sleep environment (American Academy of Sleep Medicine, 2005; Gellis & Lichstein, 2009; Yang et al., 2010). Sleep hygiene is often treated as an outcome in intervention research and implemented as a component in cognitive behavioral therapy for insomnia due to its strong and consistent predictive relationship with sleep outcomes (American Academy of Sleep Medicine, 2005; Mead & Irish, 2019; Yang et al., 2010). There is even an overlap between the sleep hygiene behavior of maintaining a consistent sleep schedule and the sleep timing dimension of sleep. However, similar to the inconsistency among other operationalizations of sleep, the association between sleep hygiene and other sleep outcomes is not perfect. For instance, following the sleep hygiene behavior recommendation to avoid caffeine 6 hours before bedtime will not guarantee 7-9 hours of sleep duration that night. For these reasons, while examining the effect of digital interventions on sleep overall – referred to from here on as "sleep outcomes" – it is important to also examine sleep outcomes by dimension, method of measurement, and sleep hygiene.

Digital Interventions for Sleep

Beyond the operationalization of sleep outcomes, digital health interventions are becoming increasingly popular to address widespread health problems, including poor sleep outcomes (Pagoto & Bennett, 2013; World Health Organization, 2011). Given the vast ownership of smartphones, laptops, or desktop computers across the United States (Ryan, 2018), there is an emerging consensus that technology can be harnessed to implement health behavior change at scale. Digital interventions address many of the barriers present in traditional in-person interventions. For example, a key benefit of digital interventions is that they enable the delivery of an intervention's content to people in everyday life where it is needed most, instead of being restricted to a doctor's or interventionist's office where the recently learned content is usually not applicable, especially with sleep. Further demonstrating the promise of digital interventions, a position paper released from the American Academy of Sleep Medicine recognized telemedicine as a tool to narrow the widening gap between sleep provider access and patient demand (Singh et al., 2015; Zia & Fields, 2016).

Despite digital interventions' potential benefits, the efficacy of digital interventions in promoting sleep outcomes is unclear. There are reports that such interventions can be counter-productive by causing orthosomnia, an individual's unhealthy obsession with achieving perfect sleep (Baron et al., 2017). Considering the sleep crisis, the increased use of digital interventions, and the potential harm of digital interventions for sleep outcomes, it is critical to determine the overall effect of digital interventions on sleep outcomes. It is possible that digital interventions are not uniformly harmful or helpful for sleep and that their success lies in the content of the digital intervention.

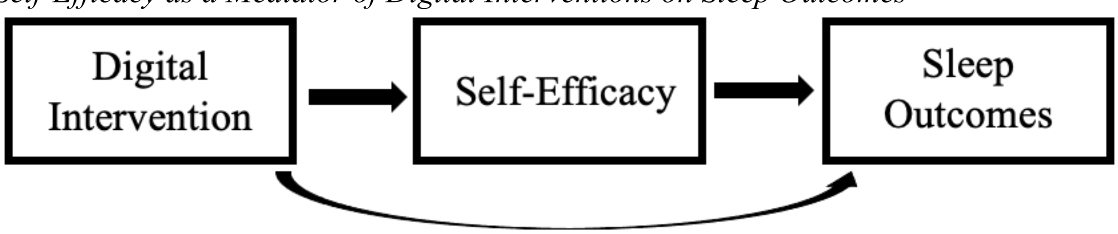
Self-Efficacy and Digital Interventions for Sleep

Interventions informed by theories of behavior change tend to have greater efficacy than those that are not (Glanz & Bishop, 2010). It has been noted that most sleep interventions would be improved by incorporating health behavior theories in their design (Mead & Irish, 2019). This could be because theories provide a framework for the development of interventions that increase efficiency and alignment to the process likely to drive or determine behavior. A theory particularly relevant to sleep outcomes is social cognitive theory (Bandura, 1986) due to its core construct of self-efficacy (Bandura, 1986; Luszczynska & Schwarzer, 2020). The theory posits that self-efficacy is a modifiable factor to reliably predict behavior, and that higher levels of self-efficacy will lead to improvements in behavior change (Bandura, 1986; Luszczynska & Schwarzer, 2020). Self-efficacy is an individual's perceived personal capacity or agency to successfully perform a behavior (Bandura, 1986, 1997; Warner & French, 2020). Higher levels of self-efficacy are associated with increased effort, persistence, and adherence to intervention treatment (Bandura, 1986, 1997; Luszczynska & Schwarzer, 2020). In relation to self-efficacy operationalization, there can be general self-efficacy (generalized across a range of contexts), sleep outcome self-efficacy (specific to obtaining sleep outcomes), sleep hygiene self-efficacy (specific to performing sleep hygiene behaviors), and non-sleep specific self-efficacy (all other self-efficacy that does not fit into the three previous categories). Most of the self-efficacy research has been done with general self-efficacy, and thus will be assumed unless otherwise noted.

Self-efficacy is prominent in several other theories that have been applied to behavior change, such as theory of planned behavior (Ajzen, 1985), health action processes approach (Schwarzer, 1992), protection motivation theory (Rogers, 1975), revised health belief model (Janz & Becker, 1984; Rosenstock, 1974), and the transtheoretical model (Prochaska & Velicer, 1997). The repeated implication of self-efficacy across many theories of behavior change highlights the field's perception of its important role in effective behavior change interventions. These leading theories would all posit that one of the primary pathways interventions use to exert their effect on sleep outcomes is through changes in the psychological construct self-efficacy (Figure 1). As indicated by the bottom arrow, we also acknowledge there may be other pathways beyond the scope of this study that could be important.

Figure 1

Self-Efficacy as a Mediator of Digital Interventions on Sleep Outcomes



Self-efficacy is important for sleep outcomes because high levels of self-efficacy can help individuals invest the considerable time and effort needed to improve one's sleep (Bouchard et al., 2003). For an individual to successfully improve their sleep, they must first believe they can be effective at improving their sleep (Rutledge et al., 2013). Low self-efficacy is associated with more sleep disturbances (Schlarb et al., 2012) and higher incidence of insomnia and associated symptoms (Bihlmaier & Schlarb, 2016).

There is growing recognition from researchers that self-efficacy plays an important role in the development, maintenance, and behavioral treatment of insomnia (Bouchard et al., 2003; Lacks, 1987; Morin, 1993). It has also been argued that self-efficacy should be more closely examined in the prevention and therapy of sleep disorders (Schlarb et al., 2012).

Although there are many psychological constructs that could be important to examine in the context of sleep, there is a growing body of research to support the importance of self-efficacy over other constructs in sleep interventions. For instance, a study by Knowlden et al. (2012) examined the role of the constructs self-efficacy, attitudes, and social norms (all in the context of sleep) and found self-efficacy was the only direct predictor of sleep behavior. Self-efficacy also significantly predicted total sleep time, and individuals who obtained adequate sleep (7-9 hours; National Sleep Foundation, 2020) reported significantly higher levels of sleep self-efficacy than those who did not (Knowlden et al., 2012). This is consistent with other studies that found individuals with insomnia had significantly lower levels of self-efficacy compared to those without insomnia (Bihlmaier & Schlarb, 2016; Schlarb et al., 2012). Low self-efficacy has been implicated as a risk factor for insomnia (Schlarb et al., 2015) and high self-efficacy is a suggested protective factor for sleep problems (Schlarb et al., 2012). Self-efficacy is associated with various sleep parameters (i.e., bedtime resistance, sleep onset delay, sleep duration, sleep anxiety, nighttime awakenings, parasomnias, and sleep disturbance scores; Bihlmaier & Schlarb, 2016), and general, sleep outcome, and sleep hygiene self-efficacy have been shown to positively predict sleep across diverse samples (i.e., American, Chinese, Australian, Iranian, German; Bihlmaier & Schlarb, 2016; Hamilton et al., 2020; Knowlden et al., 2012; Lao et al., 2016; Strong et al., 2018; Zhang et al., 2020).

Self-Efficacy Behavior Change Techniques

Despite all the evidence for self-efficacy and sleep, it can prove difficult to determine self-efficacy's role in sleep improvement due to lack of construct reporting. This problem is not unique to the sleep literature as single studies rarely examine the mediator through which an intervention was effective (Aklin et al., 2020; Carey et al., 2019; Davidson & Scholz, 2020; Hagger et al., 2020; Suls et al., 2020). This means that many studies may lack a direct measure of self-efficacy. Yet, given most interventions are designed with specific components to change the target behavior, it may be possible to overcome this limitation by using the behavior change techniques (BCTs) associated with changes in self-efficacy as a proxy for self-efficacy.

BCTs are the irreducible active ingredients of all interventions (Michie et al., 2013). Extensive work has been conducted to identify links between BCTs and the psychological constructs they manipulate in order to ultimately change behavior. Seminal work by the Human Behavior Change Project (Carey et al., 2019; Connell et al., 2019; Human Behaviour-Change Project, n.d.; Johnston et al., 2020; Michie et al., 2017) brought together a panel of international behavior change experts to examine hundreds of articles on behavior change interventions and ultimately create a database linking BCTs to the psychosocial constructs they manipulate. From this database, there are eight primary BCTs linked to modifying levels of self-efficacy to change behavior (Table 1). The presence of the BCTs with established links to self-efficacy could therefore be used to infer the psychological construct self-efficacy even when it is not explicitly tested by the intervention. Figure 2 provides a visual representation of the self-efficacy BCTs predicting changes in the construct self-efficacy.

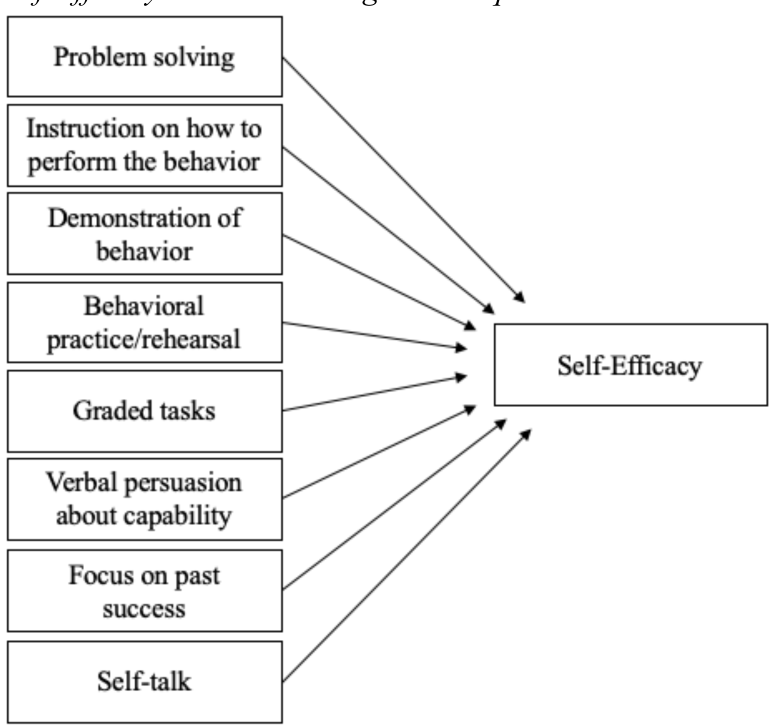
Table 1
Behavior Change Techniques Linked to Self-Efficacy

Technique	**Definition** (Michie et al., 2013)	**Evidence with sleep and self-efficacy**	**How the BCT may increase self-efficacy for sleep**
Problem Solving	Prompt an individual to assess factors influencing the behavior and create strategies to overcome barriers or increase facilitators of the behavior.	Incorporating problem solving in sleep interventions has demonstrated success (Bogdanov et al., 2017; Schlarb et al., 2017). In one sleep intervention participants reported that opportunities to problem solve was one of the most helpful components of the intervention and increased their confidence (Tse & Hall, 2008).	Having an individual identify barriers to sleep (e.g., too much noise outside) and finding solutions to overcome those barriers (e.g., purchase ear plugs) may increase perceived preparedness and confidence to overcome barriers in achieving sleep (i.e., coping self-efficacy).
Instruction on How to Perform the Behavior	Agree or advise on how to perform the behavior. Includes skills training.	Sleep education was rated the most helpful component of one sleep intervention (Spadola et al., 2020). Providing instruction has led to significant increases in self-efficacy and task persistence (Schunk, 1984) and has facilitated content mastery and self-efficacy beliefs (Brannick et al., 2005).	Providing instruction on how to perform behavior increases knowledge on how to perform the behavior (e.g., sleep hygiene education) and subsequently increases confidence in one's ability to successfully perform the behavior.
Demonstration of Behavior	Observable sample of performance of the behavior is provided to the person directly or indirectly. Includes modeling.	Sleep studies have used modeling of the behavior for patient adjustment and use of sleep technology (Malow et al., 2014; Rains, 1995), and sleep safe practices have also been learned through demonstration of behavior (Rowe et al., 2016).	Observing a similar other successfully perform the behavior (e.g., consistently get 7-9 hours of sleep) can increase one's confidence that they can also achieve this behavior.

		People can learn new skills from observing others (Rosenthal & Bandura, 1978; Rosenthal & Zimmerman, 2014) and the belief that one has learned new skills raises self-efficacy (Schunk, 1984; Schunk & Hanson, 1985).	
Behavioral Practice/Rehearsal	Prompt practice or rehearsal of the behavior in a context or at a time when it may not be necessary, to increase habit or skill.	Rehearsal of an imagery technique for chronic nightmare suffers significantly improved sleep quality compared to controls (Krakow et al., 1995). Opportunities to rehearse and practice behaviors engenders mastery experiences, the most powerful source of self-efficacy (Bandura, 1997).	Practicing the behavior (e.g., guided breathing exercises) increases one's confidence, ability, and/or skill in performing the behavior later (e.g., mindful breathing while falling asleep).
Graded Tasks	Set easy-to-perform tasks, increasing difficulty, but achievable, until the behavior is performed.	A clinical trial found individuals assigned to the gradual sleep extension treatment had longer sleep duration compared to the control condition (Dewald-Kaufmann et al., 2013). Success or mastery of tasks can increase self-efficacy (Gist & Mitchell, 1992).	Graded tasks for sleep (e.g., gradually setting a bedtime earlier and earlier) may alter self-efficacy through increasing confidence to perform the behavior gradually in an achievable/realistic manner.
Verbal Persuasion about Capability	Tell the person they can successfully perform the behavior, asserting that they can succeed, and arguing against self-doubts.	One study found that their verbal persuasion text message intervention motivated participants toward good sleep hygiene practices (Gipson et al., 2019). Verbal persuasion is a principal method of increasing levels of self-efficacy (Gist & Mitchell, 1992).	Verbal persuasion about capability could increase levels of self-efficacy for sleep through assertion of capability from a respected other.
Focus on Past Success	Recommend thinking about or	Focusing on past success through self-monitoring	Highlighting successful performance of the

	listing previous successes at performing the behavior (or parts of it).	improved sleep quality and sleep hygiene (Mairs & Mullan, 2015). A review of three popular commercial apps for sleep and physical activity found they all provided feedback to allow the user to focus on their previous success with changing behavior (Duncan et al., 2017).	behavior in the past can increase confidence that the behavior can be performed successfully again.
Self-Talk	Prompt positive self-talk (aloud or silently) before and during the behavior.	Positive self-talk has successfully been included in cognitive therapy for insomnia (Hendricks et al., 2014). Self-talk is a form of verbal persuasion, one of the core sources of self-efficacy (Bandura, 1997).	Encouragement that one can successfully perform the behavior can increase confidence in one's ability to perform the behavior.

Figure 2
Self-Efficacy Behavior Change Techniques



The Present Study

Despite the rapid uptake in digital interventions to improve sleep globally (World Health Organization, 2011) and the growing public health issue of inadequate sleep (Altevogt & Colten, 2006), a meta-analytic review has never been conducted to examine if digital interventions are an efficacious approach to sleep improvement. One systematic review did examine the design engineering and implementation of mHealth apps for sleep disturbances, but did not focus on apps to *intervene* on sleep (Aji et al., 2021). Rather, the review included apps that only measured and tracked sleep (but did not attempt to intervene), and included papers that had no quantitative evaluation of sleep. Further, the review only had one paper with an adequately powered randomized controlled trial (RCT), whereas the present study will exclusively review RCTs of digital interventions with measured sleep outcomes. A recent systematic review by Arroyo & Zawadzki (2022) identified a systematic difference in the BCTs used for fully-automated mHealth apps for sleep, but was unable to determine whether the presence of BCTs was associated with better or worse sleep outcomes. Another review found internet-delivered cognitive-behavioral therapy for insomnia were an efficacious and viable option to treat insomnia (Zachariae et al., 2016). However, the review did not include mobile apps, which provide distinct features from websites, including portability, processing on the device, notifications and alters to the user, and remote location services (Turner-McGrievy et al., 2017). Further, the review was limited to articles published before 2015, and there have since been major advances in technology and subsequently digital health (Galov, 2020; Healthcare Information and Management Systems, 2021). Thus, it is essential to conduct a systematic review and meta-analysis analysis of digital interventions (both websites and mobile apps) to determine their effect size on sleep outcomes as of 2021.

In addition to testing whether or not digital sleep interventions are efficacious in improving sleep outcomes, it is also critical to know *when* or *under what conditions* they are efficacious. For the reasons described earlier, it is important to examine sleep dimension, sleep hygiene, and sleep method of measurement as moderators of the effect of digital interventions on sleep outcomes. There are also other candidate moderators that may be important to examine that are typically assessed in meta-analyses (e.g., sample characteristics, intervention characteristics). In testing whether digital interventions are efficacious in improving sleep outcomes, it is also critical to know *why* they are efficacious. This study also tests whether the effects of digital interventions on sleep outcomes is mediated by self-efficacy (Rhodes et al., 2020; Sheeran et al., 2020). This will provide cumulative evidence for one potential mediator of digitally delivered behavioral interventions on sleep outcomes. Further, the BCTs associated with changes in self-efficacy will also be examined for their role in the mechanism of action for digital interventions on sleep outcomes.

The current meta-analysis aimed to answer the following research questions (RQs):

RQ1: Are digital interventions aimed at promoting sleep efficacious in improving sleep outcomes?

RQ2: Is the efficacy of digital interventions on sleep outcomes moderated by sleep dimension, sleep hygiene, sleep measurement, intervention length, sample characteristics, or intervention characteristics?

RQ3a: Is the efficacy of digital interventions on sleep outcomes mediated by self-efficacy?
RQ3b: Do digital interventions using self-efficacy BCTs lead to changes in sleep outcomes?

METHOD

Study methods and analysis plan were pre-registered with Prospero [CRD42021269066]. We conducted a series of searches of three databases on May 12th 2021: PubMed, PsycINFO, and Web of Science. These databases were chosen due to their comprehensive coverage of research literature and because they enable researchers to specify and formulate queries and reliably reproduce searches compared to other systems (see Gusenbauer & Haddaway, 2019). The following search string (represented here in the format for PubMed) was formatted and used to search the title and abstract fields of each database: [sleep* OR insomnia*] AND [intervention OR randomize* control* trial* OR cognitive behavi* therapy OR lifestyle change OR behavi* modification OR behavi* change OR prevent* medicine OR prevent* health] AND [smartphone* OR phone* OR mHealth OR eHealth OR telehealth OR app OR mobile OR digital OR iPhone* OR Android* OR internet OR website OR webpage OR web OR tablet]. An asterisk next to a term denotes truncation (search all terms that have this root). A filter for RCTs was also applied.

Article Screening Procedure

Article screening was conducted using Rayyan software (Ouzzani et al., 2016). Five categories of criteria were applied in the article screening procedure. The first category of inclusion criteria required articles to have an adult sample (participant age range 18-64 years old) and for the articles to be published in English. The adult sample specification was included to fit the scope of the current paper given differences in clinical guidelines of healthy sleep for adults (ages 18-64; 7-9 hours) compared to teenagers (ages 14-17; 8-10 hours) and older adults (ages 65+; 7-8 hours; National Sleep Foundation, 2020). If mean age range was not reported by the study, then the mean age of the sample was used to determine eligibility.

The second category required all articles to use a (parallel or factorial design) RCT study design. The RCT design is widely considered the gold standard of intervention research and is recommended by the Cochrane Collaboration (Higgins et al., 2021). According to the Oxford Levels of Evidence scale, RCTs have the highest possible level of evidence compared to other study designs and can be used to make causal inferences (OCEBM Levels of Evidence Working Group, 2011).

The third category of criteria required all articles to provide their own data. This excluded review papers, commentaries, protocol-only, and secondary data analyses on data that were already coded in a previous article. The fourth category required that sleep was a measured variable (i.e., self-report or electronic) and that sleep was targeted/manipulated by the digital intervention (i.e., sleep was an outcome variable in which the intervention was the predictor). This category excluded studies using a doctor's or partner's assessment of how the participant slept.

The last category contained criteria that required the evaluated intervention to be a digital intervention. Digital interventions could be delivered through websites, smartphone applications, or both (multimodal). All interventions had to be delivered through a digital program in the user's natural environment, which excluded one-time delivery of a digitally program at a doctor's office or researcher's lab. The criteria also excluded interventions evaluating an in-person intervention with a supplemental digital component.

Data Collection

Interrater Reliability

After the final set of included articles were identified, there was an initial coding calibration period between the primary coder ACA and a trained researcher. A random 10% (n = 5) of articles were coded independently by each coder, disagreements were resolved through discussion, and when needed the code book was evaluated, clarified, and revised. Because perfect agreement was not met after coding the five articles, an additional four articles were coded during the calibration period to ensure agreement between coders before moving on to independently code the remaining articles.

After the calibration period, all articles were independently coded by author ACA. To ensure the reliability of this coding, a random selection of 10 articles was independently coded by a second coder. Interrater reliability (IRR) scores were then calculated separately for the coding of sleep outcomes, self-efficacy, and BCTs, and adequate reliability in coding was operationalized as $k > 0.8$ for each category. Adequate IRR was met for all three of the coding categories: BCTs (k = .94), self-efficacy (k = 1.0), sleep outcomes (k = .89). For BCT and sleep outcome coding, there was no pattern to coding incongruence (e.g., systematic disagreement of over- or under-inclusion from either coder). Therefore, the primary coder independently coded the remaining articles.

Sleep Outcomes

Sleep outcomes consisted of all dimensions and measures of sleep health. To answer RQ1 and RQ3, all measures of sleep outcomes and their effect sizes were extracted from articles. When multiple reports of the same sleep outcome measure were provided within a study (e.g., intended global score and select sub-scales), only the intended global score was recorded. This was to avoid over-representation of values reported separately but derived from the same measure. To try and capture the diversity within sleep outcomes, the sleep data extracted were also categorized by sleep dimension, sleep hygiene, and method of sleep measurement (RQ2). The definitions for sleep hygiene and for each of the five dimension of sleep are provided in Table 2, along with the method of measurement commonly used to assess them. Measurement method was categorized by self-report (subjective appraisal of how one is sleeping, commonly assessed with retrospective questionnaires and sleep diaries), or electronic (observation of sleep parameters, often assessed with behavioral or physiological technology). More information on sleep outcome operationalization is provided in Appendix A.

Table 2

Definitions and Measures of Sleep Outcomes

Sleep Construct	Definition (Buysse, 2014)	Self-report Measures	Electronic Measures
Sleep Dimension			
Sleep quality	One’s satisfaction with their sleep, or the subjective assessment of	Sleep Diary, Current Sleep Quality Index,	N/A

	their sleep as 'good' or 'poor'.	Pittsburg Sleep Quality Index, Bergen Insomnia Scale	
Sleep duration	The total amount of sleep obtained in a 24-hour period.	Sleep Diary, Pittsburg Sleep Quality Index	Polysomnography (PSG), wearables, actigraphy, WatchPat
Sleep continuity	Sometimes referred to as sleep efficiency, is defined as the ease of falling asleep and returning to sleep, and includes constructs such as wake after sleep onset, sleep onset latency, and number of awakenings.	Sleep Diary, Jenkins Sleep Questionnaire, Bergen Insomnia Scale, Sleep Condition Indicator	PSG, wearables, actigraphy, WatchPat
Sleep timing	The placement of sleep within the day, and incudes shift work and chronotype (i.e., innate preference to sleep or be awake at certain times of day).	Sleep Diary, Sleep Timing Questionnaire, Sleep Hygiene Index	PSG, wearables, actigraphy, WatchPat
Sleepiness	Sometimes referred to as alertness, is one's ability to maintain wakefulness throughout the day.	Epworth Sleepiness Scale, Glasgow Sleep Impact Index, Sleep Condition Indicator	N/A
Sleep Hygiene			
	All measures of sleep scheduling and timing, eating/drinking behaviors that influence sleep, activating or arousing activities close to bedtime, activities in bed other than sleep or sex, maintaining a comfortable sleep environment.	Sleep Hygiene Index, Sleep Hygiene Behavior, Sleep Timing Questionnaire	PSG, wearables, actigraphy, WatchPat

Self-Efficacy

The Psychological Construct Self-Efficacy. To assess RQ3a, all measures of self-efficacy and control-related constructs were extracted from articles. The current study's operationalization of self-efficacy focused on the content of constructs, rather than how a construct was labeled. This is in line with recommendations for researchers to include conceptually similar constructs in syntheses of the literature to ensure reviews are complete and do not overlook entire bodies of research due to different construct labeling (Hagger, 2014). When a construct with content matching self-efficacy (an individual's perceived personal capacity or agency to successfully perform a behavior) was identified, the measure of self-efficacy and its effect size were extracted from the article.

Self-efficacy measures were also categorized by the type of self-efficacy it was measuring: general self-efficacy, sleep outcome self-efficacy, sleep hygiene self-efficacy, or non-sleep specific self-efficacy. General self-efficacy does not specify a behavior or context, and measures can sometimes include "I can solve most problems if I invest the necessary effort" (Schwarzer & Jerusalem, 1995). Sleep outcome self-efficacy assesses levels of self-efficacy specific to sleep outcomes such as perceived capacity to sleep at least seven hours in a night. Sleep hygiene self-efficacy assesses levels of self-efficacy specific to sleep hygiene behaviors such as an individual's perceived capacity to maintain a consistent sleep schedule or implement a caffeine curfew. Non-sleep specific self-efficacy was a catch-all for all other self-efficacy measures that did not fit into the three previously mentioned categories, and includes task-specific or other behavior-specific self-efficacy that was unrelated to sleep (e.g., physical activity, chronic disease management). More information on the construct self-efficacy is provided in Appendix B.

Self-Efficacy Behavior Change Techniques. To assess RQ3b, the BCTs identified as linked to self-efficacy by the Human Behavior Change Project (Carey et al., 2019; Connell et al., 2019; Johnston et al., 2020) were coded in all articles. The eight self-efficacy BCTs are: problem solving, instruction on how to perform the behavior, demonstration of behavior, behavioral practice/rehearsal, graded tasks, verbal persuasion about capability, focus on past success, self-talk. These eight BCTs are from a taxonomy of 93 possible BCTs defined by Michie et al (2013). The primary and secondary coder completed the web-based BCTTv1 training through the official website (bct-taxonomy.com) before coding the articles. The taxonomy's coding manual definitions, examples (BCTTv1, Online Supplemental Material 1), and the online training materials (UCL Centre for Behaviour Change, 2014) were used to code the eight BCTs. Supplemental materials and supporting articles explicitly referenced in an intervention were followed to code all BCTs used in the digital intervention for sleep. Binary coding was conducted for the presence or absence of each BCT in a digital intervention for sleep. Using the binary coding, a count variable was also created to represent the total number of self-efficacy BCTs used in an intervention. More information on self-efficacy BCTs is provided in Appendix C.

Candidate Moderators

The following variables were also examined as moderators of the main effect of digital interventions on sleep outcomes (RQ2).

Intervention Characteristics. The characteristics of each study's digital intervention were coded and reported. Intervention characteristics included the funding source of the digital intervention's development and testing (e.g., industry/private sector, academia, government, non-profit), the mode of intervention delivered through a website or smartphone app (or both), and the name of the digital intervention program (e.g., Sleepio, iRest, etc.). To avoid issues with collinearity, different versions of the same intervention program (i.e., Balanced for Sleep & Physical Activity; Balanced for Sleep, Physical Activity & Diet) were collapsed into one program name for the purpose of moderation analysis. When there were multiple eligible digital interventions reported in the same study both were included and treated as separate intervention samples.

The focus of the digital intervention was also coded. Specifically, if the intervention's sole focus was on sleep or if the focus was on both sleep and physical activity equally. This was done to examine if targeting both sleep and physical activity in the same intervention may have a synergistic effect and potentially lead to greater changes in sleep outcomes compared to interventions targeting sleep alone. If an intervention focused on sleep and another health outcome equally, it was coded but not included in this specific analysis as sleep and physical activity were the foci. Similarly, if an intervention focused on both sleep, physical activity, and another health outcome equally (i.e., diet), it was reported in study characteristics but for the purposes of analysis was coded as sleep and physical activity.

Intervention length was also coded as a potential moderator and was operationalized as the number of days the digital intervention was provided to the participant. If this was not reported by a study, then (in order of priority) the average time it took for participants to complete the digital intervention or the length of time the program was designed to be administered to participants was recorded as intervention length. Before being used in data analysis, length of intervention was grouped by similar durations in increments of five weeks: 34 days or less (<5 weeks), 35-63 days (5-9 weeks), 70-98 days (10-14 weeks), 99 days or more (>14 weeks). This was done to reduce noise from the variability between subtle differences in intervention lengths. These groupings were also made to test if there was a non-linear relationship between intervention length and the effect of digital interventions on sleep outcomes (i.e., that each additional day does not produce a concordant additional unit of effect).

Sample Characteristics. The health condition of each sample at baseline was coded and tested as a moderator (i.e., participants with clinical insomnia, chronic health condition, pregnant, or non-clinical sample). These conditions were coded to account for potential confounding effects of health conditions known to influence sleep outcomes. Clinical insomnia was determined using standard cut-offs for insomnia scales. Chronic health conditions encompassed both mental and physical health conditions known to effect sleep including (but not limited to) posttraumatic stress disorder, depression, drug dependency disorder, cancer, and migraines. Pregnancy was also included as a variable given reports that individuals sleep significantly worsens during the course of pregnancy (Hedman et al., 2002). For statistical analyses, the pregnancy variable was merged with the chronic health condition variable to account for small sample size of the former group, and was deemed acceptable given both conditions are known to have an adverse effect on sleep. If a sample had both clinical insomnia and another chronic health condition (e.g., epilepsy), it was reported in sample characteristics but was coded as insomnia for moderation analysis as this was more closely in line with the aims of our review. Non-clinical samples were defined as participants who did not fit into any of the above categories. Although not tested as a moderator, the sample's race/ethnicity, age, gender, and the country that the sample was recruited from was coded and reported for all samples.

Comparison Groups. The type of comparison group in each study was coded and tested as a moderator of the overall effect. When there were multiple comparison groups within a single study (e.g., both a waitlist control group and a minimal intervention group), the most standard control group (i.e., waitlist control) was chosen to get the clearest view of the effect of digital interventions on sleep. Comparison groups consisted of one of five types: none (no treatment given to comparison group), treatment as usual (comparison group got standard treatment, or were not restricted from continuing or starting any new treatments), minimal (not a stand-alone treatment for insomnia; may include sleep hygiene education, basic information about sleep, etc.), attention-matched control (requires equal attention and effort as the treatment group but does not have a known effect on sleep or health outcomes), and non-sleep treatment (the comparison group received an intervention that did not target sleep but could have had an indirect effect on sleep, such as a physical activity intervention).

To reduce statistical noise, before being used in analyses the comparison groups were combined by their expected effect on sleep. Comparison groups receiving no treatment, treatment as usual, or attention-matched control were combined because these comparison groups do not specifically target sleep and therefore were not expected to have a direct effect on sleep outcomes. Treatment as usual could mean no restriction to existing or new treatment (and therefore included participants receiving no treatment at all), while attention-matched controls are designed to have no effect on the outcome of interest (i.e., sleep). Comparison groups receiving a minimal treatment for sleep or a non-sleep treatment (i.e., treatment that may have indirect effects on sleep) were combined given they are not standalone treatments for insomnia but may still have subtle effects on sleep outcomes.

Extracting Effect Sizes

The effect size data for the effect of digital interventions on sleep outcomes were extracted for both the intervention and comparison group at baseline and posttest. The mean, standard deviation, and sample size data were used to calculate the raw intervention effect size – Cohen's *d* – for each study using standardized formulae (Borenstein et al., 2005). Authors of studies without sufficient effect size data were contacted for the necessary information to calculate effect sizes.

Some studies reported more than one effect size for the effect of their intervention on sleep outcomes (e.g., a study used a self-report sleep diary measuring sleep duration and sleep quality, and also used a wearable to capture sleep duration). In these cases, each effect size was included as multiple effects from the same study. If a study reported effect sizes from two or more digital interventions that were independent samples (e.g., two digital interventions with two separate samples reported in one publication), they were treated as separate studies.

Analysis Plan

Multilevel Meta-Analytic Model

All data were analyzed using the metafor package (Viechtbauer, 2010) in R (R Core Team, 2021). To test the efficacy of digital interventions on sleep outcomes (RQ1), we used a multilevel random effects meta-analytic model to account for studies using multiple within-study measures of sleep outcomes. In such cases, these effect sizes cannot be treated as independent samples, since they are nested within the same study. Accounting for this bias in multiple effect sizes from the same study is necessary to meet model assumptions of independence between effect sizes. Using a multilevel model separates the variance both within- and between studies, and accounts for both sources of variance in the analysis.

Meta-Regression Models

Meta-regression models were used to conduct all moderation analyses (RQ2 and RQ3b). For RQ2, this includes examining if sleep dimension, sleep hygiene, or sleep method of measurement moderated the effect of digital interventions on sleep outcomes. This RQ also examines the candidate moderators: funding source, mode of intervention delivery, name of program, dual- or single- intervention focus, intervention length, health condition at baseline, and comparison group.

RQ3b examined if the presence of self-efficacy BCTs moderated the effect of digital interventions on sleep outcomes. This question was tested in two ways: as a minimal effect and as a linear effect. The minimal (presence) effect compared interventions using 0-1 self-efficacy BCTs to interventions using 2+ BCTs. The categories of 0-1 BCTs compared to 2+ BCTs were informed by both data and logic. Given only one intervention ($k = 4$) used 0 BCTs, the decision was made to combine interventions using 0 and 1 self-efficacy BCTs. This judgement was deemed appropriate because while for some individuals one BCT may result in changes in self-efficacy, there is also the risk that for some individuals the one BCT will not be compatible for their needs. Having at least two self-efficacy BCTs increases the chances that at least one of the BCTs will be compatible for an individual, and thus can increase their levels of self-efficacy. In addition to the presence effect, RQ3b was also tested as a linear effect. Specifically, the linear (continuous) effect examined self-efficacy BCTs as a continuous moderator (possible range 0-8 self-efficacy BCTs). This second test aimed to determine if more self-efficacy BCTs would result in more changes to the construct self-efficacy.

Meta-Analytic Structural Equation Model

We also aimed to test whether the effects of digital interventions would be mediated by self-efficacy across the included studies (RQ3a). To test this mediation, we planned to use the meta-analytic effect size data for the effect of digital interventions targeting self-efficacy on sleep outcomes and concurrent measures of self-efficacy, and of the association between self-efficacy and sleep outcomes. A meta-analytic structural equation model (MASEM) can be estimated using the meta-SEM package in R, which reproduces the direct effect of digital interventions on sleep outcomes and the indirect effect through measures of self-efficacy. This analysis is confined only to studies using digital interventions that measured self-efficacy or a conceptually similar construct. To examine mediation, three effect sizes are needed: the effect of digital interventions on sleep outcomes, the effect of digital interventions on measures of self-efficacy, and the effect of self-efficacy measures on sleep outcomes. While it is not necessary for each study to report data on all three effects, it is required that sufficient data are available across all included studies so that all three ‘cells’ in the matrix of effect sizes are complete.

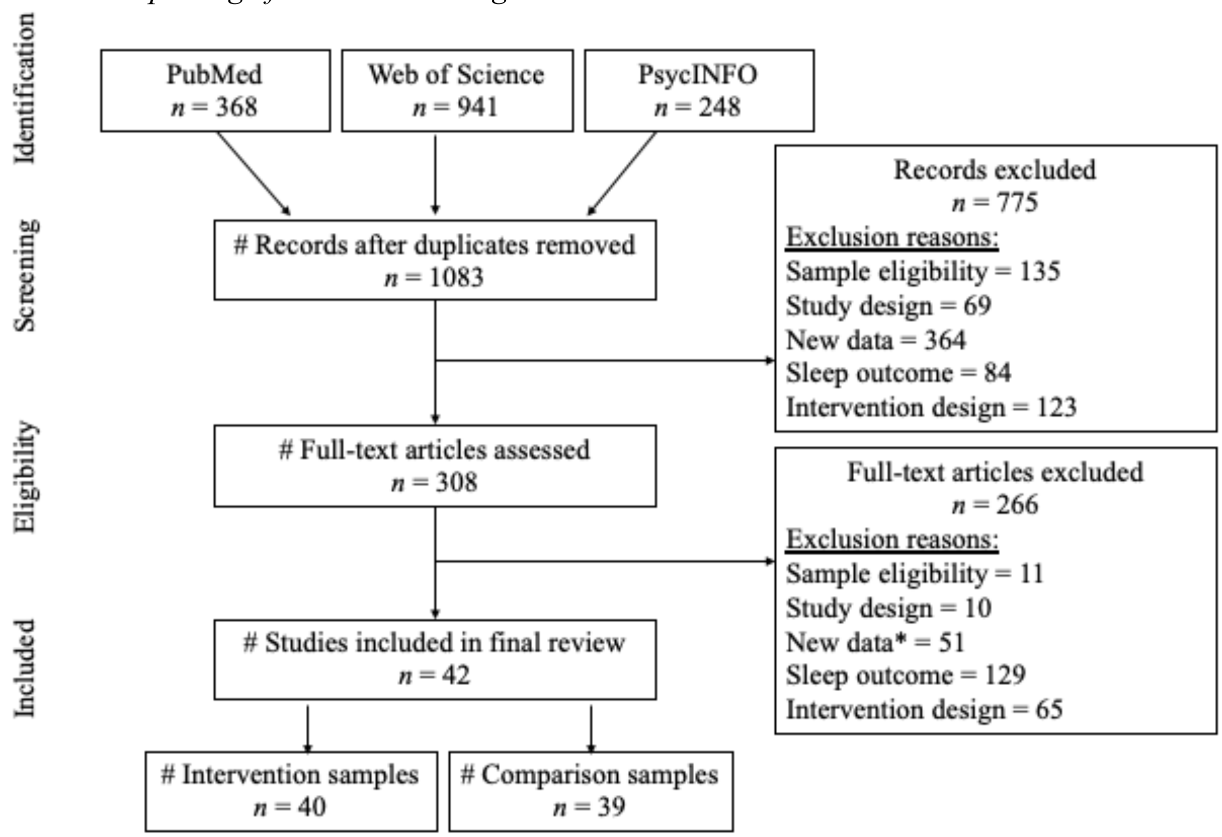
RESULTS

Literature Search

A total of 1,083 non-duplicate articles were identified from the database search of PubMed, Web of Science, and PsycINFO conducted on 5/12/2021. Of these, 775 were excluded in the title/abstract screening. Of the 308 articles that underwent full-text review, 266 were excluded, resulting in 42 eligible articles in the current meta-analysis. Three articles provided additional data for samples already included, and one article had two eligible digital intervention samples, resulting in $k = 40$ samples. Figure 3 presents the article screening decisions in PRISMA format (Moher et al., 2009).

Figure 3

PRISMA Reporting of Article Screening Decisions



*two articles met eligibility criteria but authors could not be reached to provide data to calculate effect sizes, and therefore were excluded due to lack of new data.

Overview of Included Studies

Intervention Characteristics

Of the 40 eligible digital intervention samples, twenty-four (60.00%) were delivered through a web browser, eleven (27.50%) were delivered through a smartphone app, and five (12.50%) were delivered through both a web browser and a smartphone app. Thirty-three (82.50%) of the digital interventions focused solely on sleep improvement, six (15.00%) focused on sleep and physical activity equally, and one (2.50%) focused on sleep and alcohol equally.

The average intervention duration[1] across the 40 samples was about 10 weeks (*M* = 71.55 days, *SD* = 73.69, Range = 14-490 days). Twenty-one (52.50%) of the interventions were between 5-9 weeks/35-63 days, followed by twelve (30.00%) interventions between 10-14 weeks/70-98 days, then five (12.50%) interventions less than 5 weeks/34 days or less, and two interventions (5.00%) more than 14 weeks/99 days or more.

Across the 40 intervention samples, there were 20 digital intervention programs used, and 14 of them were only used in one sample. The other programs appeared multiple times across samples: Sleepio (k = 11 samples), SHUTi (k = 8 samples), Balanced: Sleep & Physical activity (k = 2), Balanced: Sleep & Physical Activity & Diet (k = 2), GetOn (k = 2). The program Sleepio was funded jointly by government and industry (National Health Service in UK and Big Health Ltd), SHUTi was funded by government and academic partnership (University of Virginia, National Institute of Mental Health), Balanced was funded by a non-profit (National Heart Foundation of Australia), and GetOn was funded by the European Union.

Looking at the 20 programs overall, 15 (75%) had no industry funding while five (25%) were partially or fully funded by industry. Specifically, the majority of programs were funded fully by an academic source (n = 6, 30%), followed by fully government (n = 3, 15%), non-profit (n = 3, 15%), government and industry jointly (n = 3, 15%), then fully industry (n = 2, 10%), the European Union (n = 2, 10%), and lastly government and academia jointly (n = 1, 5%). Examining funding by effect sizes represented across studies presented a different distribution, with government and academic partnerships producing the most effect sizes (k = 56, 29.01%) followed by government and industry partnerships (k = 50, 25.90%), fully academic (k = 27, 13.98%), government (k = 22, 11.11%), non-profit (k = 17, 8.81%), industry (k = 13, 7.10%), and the European Union (k = 8, 4.15%).

Sample Characteristics

The majority of intervention samples were from English-speaking counties: USA (k = 12), Australia (k = 9), and UK or England (k = 5). There were two samples recruited from each of the following: Iran, Japan, the Netherlands, Norway, International/worldwide. The remaining countries only had one sample represented in the current study: Denmark, Hong Kong, Ireland. The mean age of all samples was 41.13 (*SD* = 10.51), and there were more female participants (67.87%) compared to male (32.10%). Twenty-two of the 40 samples did not report race/ethnicity demographics of their sample. Of the 18 samples that did report race/ethnicity information, almost all (n = 17) had a sample majority of self-identified White/Caucasian participants, 9 of which had over 90% of their sample being White/Caucasian.

[1] Thirty of the intervention lengths were taken from the length of access to a digital intervention, nine were taken from the intended/designed length of the intervention, and one was taken from the average length it took for participants to complete the intervention.

The most common health condition of samples at baseline was insomnia only (k = 17), followed by non-clinical (k = 16), both insomnia and a chronic condition (k = 3), both insomnia and pregnancy (k = 2), chronic condition only (k = 1), and pregnancy only (k = 1).

Comparison Groups

Of the 39 comparison groups, twenty-four[2] (61.54%) were no treatment or treatment as usual, twelve (30.77%) were a minimal treatment (e.g., sleep hygiene education, patient education), and three (7.69%) were attention-matched digital programs with no sleep information (i.e., online puzzles, HealthWatch placebo app, MyFitnessPal app focused on diet and physical activity tracking and advice).

Behavior Change Techniques

The average number of self-efficacy BCTs used across digital intervention programs was 2.70 (SD = 1.53), and ranged from 0 to 5 self-efficacy BCTs (out of 8 possible), with the use of two self-efficacy BCTs being the most common across interventions. As reported in Table 3, the most frequently used self-efficacy BCT across digital intervention programs for sleep (n = 20) were instruction on how to perform the behavior, problem solving, and behavioral practice/rehearsal. Conversely, the BCT self-talk was only used once, while verbal persuasion about capability was never used.

Table 3

Frequency of BCTs used in Digital Intervention Programs (n = 20).

BCT	Programs using this BCT n^a (%)
Instruction on how to perform the behavior	18 (90%)
Problem solving	13 (65%)
Behavioral practice/rehearsal	11 (55%)
Focus on past success	5 (25%)
Graded tasks	3 (15%)
Demonstration of the behavior	3 (15%)
Self-talk	1 (5%)
Verbal persuasion about capability	0 (0%)

[a] Total number of programs = 20

Results of Quantitative Analyses

Identification: Outliers

Prior to analyses, the dataset was checked for outliers as operationalized as z values > |0.329| (Tabachnick & Fidell, 2013). Two outlying effect sizes were identified in the dataset (d = 7.37, z = 4.76; d = 6.10, z = 3.85), both originating from the same study, although there were other effect sizes in the same study that were not flagged as outliers. Therefore, the results presented below represent the dataset without the two outlying effect sizes.

Research Question 1: Efficacy of Digital Interventions on Sleep Outcomes

[2] Twenty were no-treatment control, and four were treatment as usual (or no restriction to other treatment).

RQ1 tested how effective sleep-promoting digital interventions were at improving sleep outcomes. Digital interventions had a moderate-to-large effect size on sleep outcomes, Cohen's $d = 0.670$, SE = 0.103, k = 193, $t(192)=6.519$, $p < .001$, 95% CI [0.467, 0.872]. The variance between effect sizes within studies (level two) was estimated at 0.208, and between studies (level three) was 0.333. Examining model fit of a full model compared to an adjusted model revealed significantly more variability in effect sizes at both levels than would be expected from sampling variance alone ($p < .001$). This provided justification to examine moderator variables that may account for the variance within- and between-studies.

Research Question 2: Moderators of Digital Interventions on Sleep Outcomes

RQ2 aimed to understand if the efficacy of digital interventions on sleep outcomes was moderated by sleep dimension, sleep hygiene, sleep method of measurement, intervention characteristics, or sample characteristics.

Sleep Dimension. Of the 193 effect sizes, sleep quality had the greatest representation ($k = 71$), followed by sleep continuity ($k = 56$), sleepiness ($k = 18$), sleep duration ($k = 15$), and sleep timing ($k = 6$). The remaining 27 effect sizes were coded as 0 for all sleep dimensions because they did not measure a discrete dimension of sleep (e.g., measured multiple dimensions or didn't clearly measure any dimension). The dimension of sleep significantly moderated the effect of digital interventions on sleep outcomes, $F(5, 187) = 3.971$, $p = .002$. Compared to sleep outcomes that did not capture a discrete dimension of sleep ($b = 0.785$, $SE = 0.108$, $p < .001$), sleep duration ($b = -0.572$, $SE = 0.144$, $p < .001$) and sleep timing ($b = -0.526$, $SE = 0.265$, $p = .048$) were significant moderators of the effect, while sleep quality ($b = -0.243$, $SE = 0.176$, $p = .167$), sleep continuity ($b = -0.175$, $SE = 0.095$, $p = .066$), and sleepiness ($b = -0.156$, $SE = 0.156$, $p = .321$) were not statistically significant. This equated to a mean effect of 0.213 for sleep duration, 0.259 for sleep timing, 0.542 for sleep quality, 0.61 for sleep continuity, and 0.629 for sleepiness. After adding sleep dimension into the model as a moderator, there was a significant amount of heterogeneity remaining in the model, $QE(187) = 1756.830$, $p < .001$, suggesting additional moderators remained.

Sleep Hygiene vs Sleep Outcomes. We also aimed to distinguish sleep hygiene ($k = 37$) from all other sleep outcomes ($k = 156$). The omnibus test of moderation was not statistically significant, $F(1, 191) = 2.756$, $p = .099$, indicating that compared to all other sleep outcomes ($b = 0.697$, $SE = 0.103$), sleep hygiene did not significantly moderate the main effect ($b = -0.167$, $SE = 0.101$).

Sleep Method of Measurement. The test of moderation by sleep method of measurement was significant, $F(1, 191) = 3.963$, $p = .048$, indicating effects were significantly larger when measured with self-report ($b = 0.367$, $SE = 0.184$, $p = .048$) compared to electronic methods ($b = 0.329$, $SE = 0.198$, $p = .099$). The mean effect of sleep outcomes measured with self-report methods was 0.696 ($k = 177$), and for electronically measured sleep was 0.329 ($k = 16$). There was a significant amount of residual heterogeneity remaining in the model after accounting for method of sleep measurement, $QE(191) = 1917.058$, $p < .001$.

Intervention Characteristics. The intervention characteristics of funding source, mode of intervention delivery, intervention name, intervention focus, and intervention length were also examined as potential moderators of the effect of digital interventions on sleep outcomes.

Funding Source. To examine if intervention funding was a moderator, two moderation analyses were run, first with two variables (no industry funding, partial or full industry funding), and second with five variables (academia, industry, government or non-profit or European Union, government and industry partnership, government and academic partnership). The first moderation analysis was not statistically significant, $F(1, 191) = 0.807$, $p = .370$, indicating that compared to digital interventions with no industry funding ($b = 0.601$, $SE = 0.123$, $p < .001$, $k = 130$), digital interventions with (partial or full) industry funding did not significantly moderate the main effect ($b = 0.195$, $SE = 0.217$, $p = .370$, $k = 63$). Similar to the first analysis, the second moderation analysis was not significant $F(4, 188) = 0.925$, $p = .450$, indicating that compared to interventions fully funded by an academic source ($b = 0.799$, $SE = 0.264$, $p = .003$), interventions funded fully by industry ($b = -0.316$, $SE = 0.530$, $p = .552$), fully by a government, a non-profit, or the European Union ($b = -0.420$, $SE = 0.327$, $p = .201$), funded by a government and industry partnership ($b = 0.050$, $SE = 0.324$, $p = .878$), or funded by a government and academic partnership ($b = -0.042$, $SE = 0.346$, $p = .904$) did not moderate the main effect.

Mode of Intervention Delivery. To examine if mode of delivery was a moderator, two moderation analyses were run, first with two variables (app, website), and then with three variables (app, website, both app and website). To conduct the first test comparing apps to websites, the effect sizes for interventions using both methods of delivery together were excluded from the dataset, resulting in 181 effect sizes (previously 193) and 35 samples (previously 40). The second test used the entire dataset (193 effect sizes, 40 samples) to compare the effect sizes of interventions delivered through apps, websites, or both apps and websites equally, with the latter being used as the reference category.

The first analysis (app, website) showed a significant moderation effect for mode of delivery, $F(1, 179) = 7.937$, $p = .005$, indicating effects were significantly smaller when the intervention was delivered through an app ($b = -0.476$, $SE = 0.169$, $p = .005$) compared to a website ($b = 0.774$, $SE = 0.094$, $p < .001$). Specifically, the mean effect of interventions delivered through a website was 0.774 ($k = 131$), while the mean effect for apps was 0.268 ($k = 50$). The second analysis showed that mode of intervention delivery (app, website, or both) significantly moderated the overall effect of digital interventions on sleep outcomes, $F(2, 190) = 4.600$, $p = .011$. Compared to interventions using both an app and website equally as their method of intervention delivery ($b = 1.195$, $SE = 0.274$, $p < .001$), interventions delivered with an app only had a significantly lower mean effect on sleep outcomes ($b = -0.931$, $SE = 0.328$, $p = .005$), while the mean effect of interventions delivered with a website only was not significantly different ($b = -0.447$, $SE = 0.300$, $p = .138$). After adding intervention mode of delivery to the model, a significant amount of unexplained variance remained, $QE(190) = 1938.454$, $p < .001$.

Name of Digital Intervention Program. We also examined if the name of the digital intervention program moderated the overall effect of digital interventions on sleep outcomes. Specifically, we tested five categorical moderators, one for each of the four digital intervention programs that appeared more than once across samples (i.e., Sleepio, SHUTi, Balanced, GetOn) compared to all other digital intervention programs that only appeared once (serving as the reference group). The type of digital intervention program did not significantly moderate the effect of digital interventions on sleep, $F(4, 188) = 1.094$, $p = .361$, indicating that neither Sleepio ($b = 0.381$, $SE = 0.263$, $p = .149$, $k = 44$), SHUTi ($b = 0.217$, $SE = 0.284$, $p = .445$, $k = 44$), Balanced ($b = -0.298$, $SE = 0.352$, $p = .399$, $k = 15$), or GetOn ($b = 0.270$, $SE = 0.490$, $p = .582$, $k = 71$) were significantly different from the digital intervention programs that only appeared once across studies ($b = 0.540$, $SE = 0.174$, $p = .002$).

Intervention Focus. Next, we examined if the efficacy of digital interventions focusing on both physical activity and sleep significantly differed from digital interventions focusing on sleep alone. To run this analysis, the one study that did not fit into either category (i.e., focused on sleep and alcohol abstinence equally) was excluded from the dataset in order to understand the difference in effect sizes taken from studies focusing on sleep only compared to studies focusing equally on physical activity and sleep. This brought the number of effect sizes used in this particular moderation analysis from 193 to 189, and the number of studies from 40 to 39.

Intervention focus was a significant moderator of the overall effect of digital interventions on sleep outcomes, $F(187) = 4.775$, $p = .030$, indicating the effects of digital interventions focused on both physical activity and sleep were significantly lower ($b = -0.611$, $SE = 0.280$, $p = .030$) than digital interventions focusing on sleep only ($b = 0.780$, $SE = 0.106$, $p < .001$). The mean estimate of digital interventions focusing on sleep and physical activity was 0.169 ($k = 19$) and for sleep only was 0.780 ($k = 170$). There was a significant amount of residual heterogeneity remaining in the model after accounting for intervention focus, $QE(187) = 1930.585$, $p < .001$.

Intervention length. Length of digital intervention delivery was not a significant moderator of the effect of digital interventions on sleep outcomes, $F(3, 189) = 1.323$, $p = .268$. This indicated that compared to interventions with a duration of less than five weeks (< 34 days; $b = 0.229$, $SE = 0.294$, $p = .437$, $k = 17$), interventions with a duration of 5-9 weeks (35-63 days; $b = 0.594$, $SE = 0.324$, $p = .068$, $k = 142$), 10-14 weeks (70-98 days; $b = 0.353$, $SE = 0.356$, $p = .322$, $k = 26$), or longer than 14 weeks (≥ 99 days; $b = 0.219$, $SE = 0.531$, $p = .413$, $k = 8$) did not significantly moderate the main effect.

Sample Characteristics. The sample characteristics of health condition at baseline and comparison group were also examined as potential moderators of the effect of digital interventions on sleep outcomes.

Health Condition at Baseline. To examine if the health condition of a sample at baseline moderated the effect of digital interventions on sleep outcomes, effect sizes derived from samples with insomnia ($n = 22$) and samples with a chronic health condition ($n = 2$) were compared to samples with no clinical condition ($n = 26$). The health condition of the sample at baseline did not significantly moderate the effect of digital interventions on sleep outcomes, $F(2, 190) = 0.837$, $p = .434$, indicating effects were not significantly different for samples with insomnia ($b = 0.225$, $SE = 0.217$, $p = .302$, $k = 130$) or samples with a chronic health condition ($b = -0.220$, $SE = 0.477$, $p = .646$, $k = 14$) compared to samples with no clinical health condition ($b = 0.553$, $SE = 0.169$, $p < .001$, $k = 49$).

Comparison Group. Of the 193 effect sizes, 136 were computed with a comparison group expected to have no effect on sleep (i.e., treatment as usual, no treatment), and the remaining 57 were computed with a comparison group expected to have a minimal effect on sleep (i.e., sleep hygiene education, patient education). Comparison group was not a significant moderator of the effect of digital interventions on sleep outcomes, $F(1, 191) = 3.022$, $p = .084$, indicating effect sizes computed with a comparison group expected to have no effect on sleep ($b = 0.791$, $SE = 0.122$, $p < .001$) was not significantly different from effect sizes computed with a comparison group expected to have a minimal effect on sleep ($b = -0.372$, $SE = 0.214$, $p = .084$).

Research Question 3a: Mediation of the Construct Self-efficacy

RQ3a aimed to understand if the efficacy of digital interventions on sleep outcomes was mediated by self-efficacy. Due to insufficient reporting of data necessary to run these analyses, we were unable to test if the construct self-efficacy mediated the main effect for the five studies with measures of the construct self-efficacy. None of the studies reported the zero-order correlations necessary to run such an analysis. Of the five studies with a measure of self-efficacy, two of the studies' authors could not be reached for previous data requests. Thus, we were unable to determine the role of the construct self-efficacy in sleep-promoting digital interventions.

Using the information provided from articles, we were able to calculate effect sizes of the digital intervention on the construct self-efficacy. Across the five studies, there were 11 measures of self-efficacy: five for sleep hygiene self-efficacy, four for a non-sleep specific self-efficacy, and two for sleep outcome self-efficacy. The average effect size (d) across the 11 self-efficacy measures was 0.508, (*range* = -0.143 to 1.724), suggesting a medium effect of digital interventions on self-efficacy.

Research Question 3b: Self-Efficacy Behavior Change Techniques

RQ3b aimed to understand if digital interventions using self-efficacy BCTs led to changes in sleep outcomes. This question was tested as both a minimal effect using a binary variable and as a linear effect using a continuous variable. The moderation analysis testing a minimal effect of self-efficacy BCTs was not significant, $F(1, 191) = 0.922$, $p = .338$, indicating that compared to digital interventions using 0-1 BCTs ($b = 0.403$, $SE = 0.297$, $p = .176$, $k = 18$) interventions using more than one self-efficacy BCT did not moderate the main effect ($b = 0.304$, $SE = 0.316$, $p = .338$, $k = 175$). Consistent with results for the minimal effect, examining RQ3b as a continuous moderator showed the number of self-efficacy BCTs was not a significant moderator of the effect of digital interventions on sleep outcomes, $F(1, 191) = 2.009$, $p = .158$.

DISCUSSION

The current systematic review and meta-analysis aimed to determine the effect of digital interventions on sleep outcomes and to identify moderators of this effect. We found strong evidence that digital interventions are effective at improving sleep outcomes (RQ1), and our results contribute to a growing body of research suggesting digital interventions are an effective tool to improve a range of health behaviors, including physical activity (Fanning et al., 2012), diet (Berry et al., 2021), and medication adherence (Mistry et al., 2015). Our results counter reports that digital interventions are ineffective or even harmful to sleep outcomes (i.e., orthosomnia; Baron et al., 2017). Overall, these findings support the efforts by both academic and private institutions to develop sleep-promoting digital interventions and our result should encourage future growth and development to continue to improve the efficacy of these interventions.

Operationalization of Sleep as a Moderator

We hypothesized that the operationalization of sleep (by dimension, sleep hygiene, or method of measurement) may partly account for the previous reports of adverse effects. Seeing as there was a significant amount of unexplained variance both within and between studies, a series of moderators were examined to determine if they could explain the variance in effect sizes (RQ2).

Sleep Dimension

Sleep dimension significantly moderated the effect of digital interventions on sleep outcomes. Compared to sleep outcomes that did not capture any specific dimension of sleep, sleep duration and sleep timing had a significantly smaller effect. Sleep duration is the only dimension of sleep with recommendations from the American Medical Association (7-9 hours; American Medical Association, 2020) and is commonly promoted among physicians and public health campaigns. It is possible that individuals are already aware of recommendations for sleep duration, and therefore there was less room for improvement with this dimension of sleep. However, this explanation would not account for why sleep timing also demonstrated a significantly smaller effect.

The finding about sleep timing was surprising given a key benefit of delivering sleep interventions digitally is their ability to be at home with the user in their everyday life and provide prompts/reminders for sleep behaviors like sleep timing. It is possible the digital interventions included in this review did not regularly include the BCT prompts/reminders, and that the inclusion of these BCTs would improve sleep timing behaviors. An important future direction is for researchers to justify why a particular dimension of sleep was examined over others, and why/how their intervention was expected to change the particular dimension(s) of sleep they chose to measure. This requires both better reporting from authors and also the development of new standardized measures that are representative of all five dimensions of sleep.

Sleep Hygiene vs Sleep Outcomes

We also distinguished sleep hygiene from all sleep outcomes in a test of moderation. There was no significant difference in the values of effect sizes representing sleep hygiene behaviors compared to other sleep outcomes, indicating that both hygiene and outcomes can be targeted effectively by digital interventions. Although highly correlated with one another (American Academy of Sleep Medicine, 2005; Irish et al., 2015; Yang et al., 2010), sleep hygiene often precedes sleep outcomes in the sequence of behavior change. For instance, the sleep hygiene behavior to avoid the consumption of stimulating substances before bed (i.e., caffeine, nicotine) increases the ease of falling asleep (sleep latency) and total sleep time (Jaehne et al., 2009; Roehrs & Roth, 2008). Given this cascading of effects, future research may benefit from examining sleep hygiene as a mediator of the effect of digital interventions on sleep outcomes. This would allow interventionists to identify the most immediate and effective target for sleep-promoting digital interventions, and to develop programs that utilize behavior change techniques that directly influence the mediator.

Sleep Method of Measurement

Studies using self-report measures of sleep demonstrated a significantly larger effect size compared to studies using electronically measured sleep outcomes. This finding corroborates reports from single-studies that method of sleep measurement can produce different results for intervention efficacy (Lauderdale et al., 2008). The result also prompts questions as to why this difference exists, with one possibility being response bias. There is a substantial amount of time and effort that participants dedicate to engage in these digital interventions, so it is possible that participants, consciously or unconsciously, are inflating the benefit of the intervention on self-report measures of sleep because they want their efforts to pay off. Another possibility for this moderation could be shortcomings in the development and testing of digital interventions. Less than 10% of the sleep outcome effect sizes reported across studies were electronically measured. If the majority of digital intervention research and development has been done with self-reported sleep measures, then it stands to reason that digital intervention content has been optimized to improve self-reported sleep. Considering self-report and electronic measures of sleep do not always change together (Lauderdale et al., 2008), this could be an indication that they require different ingredients and intervention components to improve each of them.

Implications of Sleep Measurement for Future Research. It is important for future research to pay equal attention to both self-report and electronically measured sleep outcomes. There is a notable underrepresentation of electronically measured sleep across peer-reviewed studies, which could result in the loss of critical information about overall sleep health and how to improve it. Researchers may be mistakenly assuming digital interventions that improve self-reported sleep outcomes will also improve their seemingly objective (electronically measured) counterparts. However, this assumption can be troublesome as both self-report and electronically measured sleep outcomes have unique associations with health outcomes (Buysse, 2014), and as this meta-analysis suggested, do not benefit equally from digital interventions.

One implication of our meta-analysis is the identification of a mismatch between the research and testing of digital interventions conducted in academia compared to the private sector. The vast majority (if not all) commercially available digital interventions for sleep (e.g., Fitbit, Noom, Oura ring, Apple health) use only electronically measured sleep to track progress and provide feedback to users, and an estimated 86.3 million users (Insider Tech, n.d.) use a commercially available digital health app at least once per month. Yet, electronically measured sleep made up less than 10% of the outcomes used in the peer-reviewed literature. This mismatch may be a reflection of different goals for research in academia and industry. In general, academics are quite often interested in understanding whether an intervention works and why. On the other hand, industry is most often interested in whether users will purchase and use a product; although the efficacy of the product may be a factor in this decision, it is not the only factor. This difference in priorities for academic and industry research and development partly explains why self-report measures are rarely used industry. Self-report measures are time consuming for users, so it makes sense that industry is designing products to give the most perceived benefit with the least perceived effort. Contrarily, research in academia often pays users for their participation and can therefore rationalize the use of (sometimes) burdensome self-report measures in their intervention research. Concerns over the accuracy of electronic measures of sleep (Ameen et al., 2019) may also partly explain why electronic sleep measures have been underrepresented by academics in peer-reviewed research, as academia's (supposed) priority is to gain the most accurate picture of intervention efficacy. Alternatively, an opposing explanation may be that the pressure on academic research to demonstrate that their intervention "worked" could result in issues with reporting bias (van der Steen et al., 2019), in which electronic measures are demonstrating null or adverse effects of digital interventions and therefore are not being reported or published in peer-reviewed studies.

Sample and Intervention Characteristics as Moderators

In addition to sleep outcome operationalization, RQ2 also tested sample and intervention characteristics as potential moderators of the main effect. We did not find that digital intervention efficacy was moderated by the source of funding, nor did we find a significant difference in effect sizes between interventions with and without industry funding. Interestingly, the amount of time a digital intervention was delivered did not significantly change the efficacy of the program for sleep outcomes. This finding could suggest a ceiling effect at about 5 weeks, and thus interventions lasting longer than 5 weeks may not lead to significantly more improvements to sleep outcomes. This could be due to several reasons including participant fatigue after engaging in an intervention for several weeks, or the depletion of new intervention material to deliver to the user. Another moderator examined was comparison group, and we found the comparison group used when calculating effect sizes did not moderate the overall effect. We also found that sleep-promoting digital interventions are as effective for samples with insomnia and other chronic health conditions as they are for non-clinical samples.

Given the reciprocal relationship sleep and physical activity have with one another (Kline, 2014; Rayward et al., 2018), we aimed to understand if the efficacy of digital interventions focusing on both physical activity and sleep significantly differed from those focusing on sleep alone. We found digital interventions focused only on sleep improvement significantly out-performed digital interventions focusing on both physical activity and sleep improvement. This is in line with the self-control strength model which posits improving just one health behavior requires significant effort, and thus the effort spent to improve multiple health behaviors at the same time would result in ego (energy) depletion and limit one's ability to improve either behavior adequately (Baumeister, 1998; Baumeister et al., 2000). Some of the most popular industry-developed health apps focus on multiple health behaviors including physical activity and sleep (e.g., Fitbit, Apple Health), but an implication of our research is that this model may not be as effective as targeting one behavior at a time. Further, as suggested by Arroyo & Zawadzki (2021) there are some BCTs that are more suitable for targeting specific health outcomes, and thus an intervention targeting multiple health outcomes may require several different BCTs (which could overwhelm the user), or have a suboptimal set of BCTs for each health outcome. Future research is needed to confirm our finding and then optimize future program development, such as prompts at onboarding to choose a health behavior, specifying a goal, and upon reaching that goal, choosing the next health behavior to work on.

Another moderator examined was mode of intervention delivery. Digital interventions delivered through mobile apps had a much smaller effect on sleep compared to websites. This finding could suggest interventions delivered through websites are more effective than apps, or could also be the result of availability of technology. It is possible that the intervention programs that have been around longest – thus having the most updates and improvements to their program – started on websites and continue to be delivered through this platform. Future research will benefit from more studies testing app-delivered digital interventions to understand whether there are truly differences in the efficacy of app- and website-delivered digital interventions for sleep, or whether our finding was a result of unbalanced sample size or the availability of technology. A greater number of app-delivered sleep interventions will also increase the generalizability of peer-reviewed research to real-world contexts given most commercially available digital interventions are delivered through apps, and not websites.

Self-Efficacy as a Moderator

RQ3 examined if self-efficacy played a role in the effect of digital interventions on sleep outcomes. Specifically, RQ3a examined the role of the construct self-efficacy, and RQ3b examined the role of BCTs known to manipulate levels of self-efficacy. As will be discussed more in the limitations section, we were unable to test if the construct self-efficacy mediated the main effect of digital interventions on sleep outcomes due to insufficient data. Nevertheless, we did find a medium effect size for digital interventions on levels of self-efficacy, suggesting there were changes made to the construct self-efficacy as a result of exposure to sleep-promoting digital interventions. However, it remains unknown whether self-efficacy mediates the effects on sleep outcomes and is an important area for future research.

While we were unable to use a direct measure of self-efficacy, RQ3b tried to overcome this limitation by using the BCTs known to manipulate levels of self-efficacy as a proxy for the construct. We did not find a significant moderation effect for the total number of self-efficacy BCTs used in sleep-promoting digital interventions. One possible explanation could be that just one self-efficacy BCT has the potential to evoke enough self-efficacy to effect sleep outcomes. Due to limitations with the availability of construct self-efficacy data, we had to assume that more self-efficacy BCTs would evoke more improvements to the construct self-efficacy

Another explanation for this finding could be that there are other psychological constructs and BCTs involved in the mechanism of action for sleep-promoting digital interventions. For example, a study by Peach and colleagues (2018) found positive attitudes about sleep were directly associated with longer sleep duration. Attitudes toward a health behavior (i.e., positive or negative evaluation towards a target behavior) are important because favorable attitudes can raise motivation to engage in the behavior (Ajzen & Schmidt, 2020; Bamberg & Schmidt, 2001). Attitudes is a construct that appears across multiple theories of behavior change [e.g., expectancy value theory (Fishbein, 1967), theory of reasoned action (Fishbein & Ajzen, 2009) and planned behavior (Ajzen, 1985, 1991)], and sometimes appears alongside self-efficacy.

Limitations & Future Directions

While there were several strengths to our study, including a comprehensive review of sleep-promoting digital interventions and moderators of their effect, it also had some limitations. We did not include a variable to capture socioeconomic status (SES) due to either inconsistent or infrequent measurement in studies, so the representation of different SES populations in our sample is unknown. A systematic review of studies found digital interventions like wearables and apps were not as effective in increasing physical activity for people in low socioeconomic status groups (Western et al., 2021) compared to high SES groups, and in fact did not find any evidence that digital physical activity interventions were effective for low SES groups. It is necessary for future research examine groups at all levels of the SES ladder, particularly those at the lower end and underrepresented groups, to ensure the development of digital interventions are equitable.

We were unable to examine if the psychological construct self-efficacy mediated the effect of digital interventions on sleep outcomes due to an insufficient number of studies measuring the construct self-efficacy. While there were five (three eligible) samples that did include a measure of self-efficacy, none reported the zero order correlations necessary to run a full mediation model. Moreover, the data was insufficient to running the analyses and producing a reliable result. In line with recommendations by Sharpe & Poets (2020), meta-analyses have tremendous influence over future research in one's field, and thus the highest level of scientific methodology should be applied to any conclusions. Future research should include measures of mechanisms of action, such as self-efficacy, to understand its role in the effect of digital interventions on sleep outcomes, and whether other psychological constructs are contributing to sleep improvements.

While the use of self-efficacy BCTs as a proxy for the psychological construct self-efficacy was our best option given the lack of reporting of psychological constructs, it has several limitations. Notably, these BCTs may also be influencing other psychological constructs, not just self-efficacy, although the extensive research by the Human Behaviour Change Project indicates self-efficacy is the construct it has the largest and most reliable effect on (Carey et al., 2019; Connell et al., 2019; Johnston et al., 2020; Michie et al., 2017). We also did not have a measure of frequency of implementation of self-efficacy BCTs (which would require a hands-on review design), and thus we had to make the assumption that the presence of a self-efficacy BCT meant it was implemented at an adequate frequency to evoke improvements to the construct self-efficacy. The frequency of implementation required for a BCT to evoke significant change to the psychological construct its targeting is also unknown, and is another future direction of our research. It is possible that customized BCTs are necessary for equitable sleep improvement, and that the most efficacious BCT for an individual may vary from moment-to-moment (JITAIs; Nahum-Shani et al., 2018). Answering these questions would require more micro-randomized trials (Klasnja et al., 2015) and optimization trials (e.g., MOST Trials; Collins et al., 2007) followed by meta-analyses.

CONCLUSION

A major takeaway from this review is that as a field, we need to do a better job specifying which BCTs we implement in an intervention, which psychological construct we think it will change, and how changes to that construct predict changes in the health outcome we are targeting. Two studies are model examples of how this should be done. Majd et al. (2020) and Murawski et al. (2019) provided tables with an overview of their intervention strategies (BCTs), where each BCT appeared in the content of their intervention, and the psychological construct they thought it would affect to ultimately improve sleep outcomes. This type of specification and informed planning of intervention components should be a pre-requisite for future intervention development and testing so that we can not only understand if an intervention is effective, but also why it is effective. This will allow the field to understand what content is most effective for an intervention, why an intervention may stop being effective, and which individuals might benefit most for specific intervention components.

In conclusion, the current meta-analysis aimed to understand the effect of digital interventions on sleep outcomes and identify moderators of this effect. We found digital interventions are an effective tool at improving sleep outcomes, and identified several moderators of the effect including method of sleep measurement, sleep dimension, mode of digital intervention delivery, and intervention focus. The identification of these moderators highlights future directions for digital intervention research to examine why these moderators exist, and if they vary by individual or environment. Equally important was the identification of variables that did not moderate the effect, including the distinction between sleep hygiene behaviors from other sleep outcomes, intervention length, or clinical status of the sample at baseline. We also identified a disconnect between academia in industry. Namely, the representation (or lack thereof) of industry-funded digital interventions and the characteristics of these interventions points to an over-arching issue of the real-world implications of research conducted in the peer-reviewed literature. More research is needed for the new and rapidly developing field of digital interventions, and there needs to be greater representation of electronically measured sleep outcomes and app-delivered interventions.

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