Sleep Medicine Reviews 40 (2018) 160–169

Contents lists available at ScienceDirect

Sleep Medicine Reviews

journal homepage: www.elsevier.com/locate/smrv

CLINICAL REVIEW

A systematic review and meta-analysis of cognitive and behavioral interventions to improve sleep health in adults without sleep disorders

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ARTICLE INFO

Article history:
Received 25 August 2017
Received in revised form
5 December 2017
Accepted 5 December 2017
Available online 29 December 2017

Keywords:
Sleep health
Sleep quality
Adults
Behavior change
Meta-analysis

SUMMARY

Many adults without a diagnosed sleep disorder report poor sleep health, which is defined by dissatisfactory levels of sleep duration, sleep quality, or the timing of sleep. No previous review has summarized and described interventions targeting poor sleep health in this population. This meta-analysis aimed to quantify the efficacy of behavioral and cognitive sleep interventions in adults with poor sleep health, who do not have a sleep disorder. Electronic databases (Medline, Embase, PsycInfo, Cinahl) were searched with restrictions for age (18–64 y) and English language full-text, resulting in 18,009 records being screened and 592 full-texts being assessed. Eleven studies met inclusion criteria, seven of which reported a measure of overall sleep health (Pittsburgh sleep quality index [PSQI]). Following appraisal for risk of bias, extracted data were meta-analyzed using random-effects models. Meta-analyses showed interventions had a medium effect on sleep quality (Hedge's $g = -0.54$, [95% confidence interval (CI)] −0.90 to −0.19, $p < 0.01$). Baseline sleep health was the only significant effect moderator ($p = 0.01$). The most frequently used intervention components were stress management and relaxation practice, stimulus control, sleep hygiene, and exercise. Interventions targeting cognitive and behavioral self-regulation improve sleep quality in adults without clinical sleep disorder.

Introduction

Healthy sleep plays a key role in the maintenance of good health and wellbeing and is recognized as an important behavior to improve public health [1]. Good sleep health consists of multiple indicators, such as adequate duration, timing, efficiency, and a level of satisfaction with sleep that leaves a person feeling alert and functional throughout the day [2]. Indicators of poor sleep health include a sleep duration of fewer or more hours than the recommended seven to nine hours per night [3] and dissatisfactory sleep quality. Sleep hygiene recommendations that are aimed at promoting sleep health [4] also frequently address inconsistencies in sleep timing (fluctuating bed and wake times). Although non-pharmacological treatment for clinical sleep disorders such as Cognitive Behavioral Therapy for Insomnia (CBT-I) [5], recommends consistent wake times, some variability in bed times is encouraged based on prioritizing feelings of tiredness as a requirement for sleep onset and maintenance [6,7], with overall timing consistency being more of a secondary or long-term goal. Nevertheless, in non-clinical populations, the effect of regular bed and wake times on sleep health is unknown. The aforementioned indicators are associated with a host of non-communicable diseases including cardiovascular disease, type-two diabetes, obesity, and poor mental health [2]. Adults who report inadequate sleep duration and/or poor sleep quality hence are at high risk for morbidity and early mortality [8].

A large proportion of the global population does not meet guidelines for optimal sleep duration, sleeping either less than seven or more than nine hours per night [9,10]. The evidence regarding temporal changes in the prevalence of inadequate sleep duration is inconsistent [10]. However, it is possible there has been

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https://doi.org/10.1016/j.smrv.2017.12.003

Abbreviations

BCT	behavior change technique
BMI	body mass index
CALO-RE	Coventry, Aberdeen and London – refined taxonomy
CBT-I	cognitive behavioral therapy for insomnia
CG	control group
CI	confidence interval
CT	control group
GP	general practitioner
IG	intervention group
M	mean
MOS-SLP9	medical outcomes study sleep problem index-II
OSA	obstructive sleep apnea
PRISMA	preferred reporting items for systematic reviews and meta-analyses
PSQI	Pittsburgh sleep quality index
SD/SE	standard deviation/standard error
RCT/CT	randomized controlled trial/controlled trial

a concomitant increase in poor sleep health, due to reductions in the quality of sleep or shifts in the timing of sleep [11]. Indeed, poor quality sleep is reported by more than a quarter of the adult population [12]. This prevalence is greater than any of that associated with clinical sleep disorders such as chronic insomnia at 6%–15% [13], restless legs syndrome at 2%–8% [14], and sleep apnea at 3%–7% [15]. To improve sleep health at the population level, it is important to promote sleep as a modifiable health behavior and provide access to effective solutions [16].

Given that traditional practitioner-delivered treatments to improve sleep health cannot meet treatment demands for those with clinical sleep disorders [17], it is unlikely that resources are sufficient for those without diagnoses. Technology- or web-based interventions therefore may be useful in providing the necessary reach to effectively improve various indicators of sleep health [18,19].

Individuals who are in need to improve their sleep health may benefit from cognitive and behavioral interventions, as the underlying causes for poor sleep health often relate to factors at the cognitive or behavioral level [20]. These interventions include components such as mindfulness, relaxation training, and *sleep hygiene* [4],[1] all of which improve sleep quality and can be made accessible in ways (i.e., technology-based delivery) that do not require a trained facilitator [17]. Numerous systematic reviews have summarized the efficacy of non-pharmacological interventions in populations reporting a diagnosed sleep disorder or meeting diagnostic criteria for insomnia [21–24] with more recent publications also including web-based interventions [25,26]. Most of these reviews report a large pooled effect for sleep quality outcomes following treatment for insomnia. The authors are unaware of any prior reviews that have specifically examined the efficacy of sleep interventions in individuals who report poor sleep health, but do not have a clinical sleep disorder, or compared treatment efficacy based on the presence or absence of a clinical sleep disorder. Pharmacological interventions including prescription medications have also demonstrated high levels of effectiveness, but are not always superior to non-pharmacological treatment [27] and do not present a long-term solution [26]. Further, the evidence on use of over-the-counter sleep aids in non-clinical populations is limited and remains inconclusive [28]. Therefore, a synthesis with a focus on the efficacy of non-pharmacological interventions in populations that report poor sleep health, but do not have a sleep disorder is much needed.

It is important to describe intervention features of interventions conducted in the non-clinical population as prior reviews of clinical populations have identified that intervention efficacy varies by the type and number of intervention components used [25]. Furthermore, describing the different components of an intervention can advance the understanding of how intervention content is delivered to participants and why a component is effective in changing behavior [29]. A useful way to describe these features is the use of behavior change taxonomies [30]. However, no literature to date has described how BCTs are implemented in sleep interventions or to what extent they drive changes in sleep health; and whether the frequencies at which BCTs are implemented differ by intervention components. Furthermore, it is to be clarified, if the efficacy of sleep interventions differs by mode of delivery and a study duration similar to that reported in other health behavior trials [25,31].

The aims of this systematic review with meta-analysis were to 1) synthesize the evidence from peer-reviewed published studies on cognitive and/or behavioral sleep interventions in adults without a sleep disorder, 2) describe intervention components by use of behavior change techniques, and 3) examine if intervention efficacy is moderated by number and type of intervention components, mode of delivery, study duration, and participant characteristics (age and baseline sleep).

Methods

The search strategy, selection criteria, data extraction, study quality assessment, and statistical analyses described below were defined *a priori*. The conduct and reporting of this review was guided by PRISMA guidelines [32] and prospectively registered (PROSPERO: CRD42015029642).

Selection of studies

Electronic database searches were conducted in December 2015 using comprehensive search strings (see Table S1) in MEDLINE, Embase, PsycINFO, and CINAHL. Search strings were devised from the following term sets: 1) *sleep*, 2) *intervention*, and 3) *study type*. Record retrieval was limited to age groups between 18 and 64 y, and English language full-text. Searches covered the periods from database inception to December 2015 and weekly search alerts were set up to identify any records that were indexed while the review was underway (date of last search alert considered for review: 28/10/2016). The reviewers' existing libraries complemented the electronic database search. Any study protocols identified as part of the electronic database searches were retained and one reviewer (BM) then manually searched for any publications of related study outcomes (BM). In addition, the WHO International Clinical Trials Registry Platform (ICTRP) and the Cochrane Central Registry of Controlled Trials (CENTRAL) were searched for potentially relevant records. One reviewer (BM) also screened the titles of all studies listed in relevant reviews that were either known to the authors, or identified through electronic database searches. Abstracts of references were only screened, if the eligibility criteria specified in the review were too

[1] Sleep hygiene refers to a set of recommended behaviors a person can engage in throughout the day or before bedtime to promote good sleep. This includes abstinence from caffeine, alcohol, and nicotine late in the day, the practice of relaxation, regular exercise, regular sleep/wake times, modifying the environment (e.g., reduce impact of noise/light), no daytime napping, and minimal use of light-emitting devices (e.g., smartphones). Sleep hygiene differs from sleep knowledge, in that it has an instructional nature, whereas sleep knowledge in this context refers to any broader information highlighting the importance of good sleep health.

ambiguous (i.e., if it was not clear, whether the review included only studies conducted in an insomniac population).

Inclusion criteria

Studies were eligible, if they were full reports of experimental studies that had a control condition (i.e., no-intervention/waitlist control group, treatment as usual) with both or all groups reporting poor sleep health at baseline. Interventions were limited to those aiming to improve sleep health using one or several cognitive and/or behavioral components. A cognitive/behavioral component included any of the following: sleep knowledge or education; single components usually found as part of CBT-I (stimulus control, sleep restriction, relaxation, cognitive restructuring, and sleep hygiene); single components usually found as part of sleep hygiene education; stress management techniques; mind-body approaches (i.e., mindfulness-based practice, breathing), and sleep diaries or logs. Studies also had to state that it was their aim (or one of several study aims) to improve sleep health. Subjective and/or objective measurements of any parameter relating to sleep health had to be reported including baseline and immediate post-test or change scores (M, SD, SE, etc.) for all groups. Table S2 presents a detailed list of exclusion criteria and reasons for applying these criteria. Briefly, studies were excluded, if participants were not aged 18–64 y, had a chronic disease, mental health condition, or sleep disorder, were institutionalized, were shift workers, had a BMI >35, were normal sleepers, or if all intervention arms received pharmacological treatment.

Study screening

Records were exported to EndNote X7 and de-duplicated using automated and manual procedures. Irrelevant records were screened out by two reviewers (BM, LW) based on titles and abstracts. Following full-text retrieval for those retained after screening, both reviewers (BM, LW) independently assessed each record against inclusion criteria and a third reviewer (MJD) provided mediation, if no decision could be made.

Data extraction

Both reviewers (BM, LW) independently extracted and coded data of interest using a set of pilot-tested coding sheets. Data were extracted in five general categories: *study design* (study type, sample size, study duration, follow-ups, attrition), *sample characteristics* (age, BMI, gender, chronic disease, baseline sleep), *intervention components* (e.g., relaxation, sleep hygiene, immediate), *mode of delivery* (face-to-face or remote delivery, frequency and duration of contact), and *intervention outcomes* (sleep measures including M, SD/SE at all time points). Where necessary, authors of eligible studies were contacted to request missing information. Intervention components were identified based on a list of common components of CBT-I [17] and individual sleep hygiene behaviors [4]. Behavior change techniques (BCTs) were extracted using a 40-item taxonomy of BCTs [33]. The presence or absence of a BCT was coded independently by each reviewer (BM, MJD) and specifically in relation to each intervention component. Coding outcomes were then compared and discrepancies noted to determine a *kappa* statistic for inter-rater agreement, with greater *kappa* values corresponding to greater strength of agreement [34].

Risk of bias

Risk of bias was assessed by two reviewers (BM, MJD) using an adaptation of an existing checklist [35]. Item number 13 was omitted from the scoring procedure, as it lacks applicability in a non-clinical context (see Table S3). Fewer scores across a total of 26 items indicate lower study quality, due to poor reporting, low external validity, low internal validity relating to risk of bias, confounding, or insufficient power [35].

Data synthesis

Extracted data were analyzed using *Comprehensive Meta-Analysis* (CMA, Version 3; Biostat, Englewood, NJ). Means and standard deviations from pre-test and immediate post-test measurements were used to calculate change scores per group in each study for subsequent analysis in the meta-analysis. Confidence intervals and standard errors were converted to standard deviations, where necessary. If a study had more than one intervention arm that was to be included in the meta-analysis, the sample size of the shared control group was divided by the number of included intervention arms to avoid participants being counted multiple times [36]. Due to the broad spectrum of intervention components, an analysis using random-effects models was deemed appropriate *a priori*.

Mean effects throughout are reported as Hedge's *g* as a result of including studies with small sample sizes. The magnitude of effects is interpreted using the criteria small (0.2), medium (0.5), and large (0.80) as defined by Cohen [37]. Pooled effect sizes were deemed statistically significant at $p < 0.05$. In addition to computed estimates of between-study variance (Tau^2), *Q*-statistics and *I*-statistics are reported to determine the level of heterogeneity in the aggregate data. I^2 values under 25% are interpreted as low heterogeneity, values of 50–75%, and above 75% indicate moderate and high study heterogeneity, respectively [38].

Analyses for risk of publication bias were carried out using Rosenthal's *classic fail-safe N* [39], if mean effect estimates were statistically significant. Greater *fail-safe N* values are interpreted as lower concern for risk of publication bias and refer to the number of studies with a zero mean effect that are needed before the pooled effect would no longer be statistically significant ($p > 0.05$). A tolerance level (criterion value) for the robustness of results was calculated by multiplying the number of effects (m) pooled in the analysis by five and adding 10. In addition, funnel plots were inspected for symmetry, followed by Duval and Tweedie's Trim and Fill analyses [40], which re-calculates the pooled effect size after adjusting for potential bias (i.e., small studies with large effect sizes).

Due to eligibility of only a small number of studies, the previously screened records (n = 7) reporting results from studies with an active comparator condition, which otherwise met inclusion criteria were considered for a separate meta-analysis of the PSQI total score. However, this was conducted merely to examine the potential superiority of cognitive and behavioral interventions relative to minimal interventions or other types of active control groups. Pooling effect sizes from these studies with the primary mean effect from studies with a no-intervention or waitlist control group would have caused substantial blurring of the mean effect [41] and increased heterogeneity, which in turn would have limited the conclusions to be drawn from these findings.

Subgroup analyses

Subgroup analyses were performed to examine potential moderator effects on the overall sleep health outcome measure (PSQI only). Moderator analyses were only conducted for the PSQI total score only if a minimum of two studies per subgroup were available. The *a priori* dichotomized moderators were 1) number and type of intervention components, 2) intervention duration, 3) mode of delivery, and 4) sample characteristics (age, baseline sleep)

and use of BCTs. A previous meta-analysis of comparable sleep interventions [25] showed that intervention effects can be influenced by the duration of an intervention and whether a relaxation component was included or not. The number of intervention components was initially specified as a moderator, because there is little consensus as to how the use of individual components of CBT-I impacts on treatment efficacy. In addition, although inconclusive, there is some evidence that indicates interventions to improve sleep are more effective, if baseline sleep is worse [42]. Internet-delivered CBT-I and face-to-face CBT-I have been found to be equally effective in clinical populations [43,44]. It is unclear how mode of delivery influences intervention efficacy in non-clinical populations, thus mode of delivery was examined as a moderator. Age was examined as a further moderator, since it is suggested that sleep problems are more frequent in older individuals [45] and previous meta-analyses also having reported a less pronounced intervention effect in older adults [46]. Although planned *a priori*, moderator analyses for use of BCTs were not performed due to inadequate reporting of use and extensive variation between studies.

For all moderator analyses, the results of mixed-effects models are reported and include effect size predictions per covariate, as well as Q-values, and *p*-values. Moderator effects were deemed statistically significant at $p < 0.05$.

Results

Record selection

The study selection and the reasons for exclusion of studies are detailed in Fig. S1. A total of 27,883 records were retrieved from database searches (Medline: 12,443; Embase: 11,330; PsycInfo: 2250; Cinahl: 1860) and 13 studies that were known to the authors through previous studies or cited in closely related literature were also considered for screening. In four instances, additional data were requested from authors for the purpose of inclusion in meta-analyses. However, none of these requests were fulfilled in due time (six months from date of initial correspondence). In summary, eleven studies (m) were selected for synthesis and meta-analyzed.

Inter-rater agreement for record screening procedures based on Cohen's *kappa* [47] was almost perfect ($\kappa = 0.97$, $p < 0.01$), corresponding to disagreement on 40 out of 18,009 screened titles and abstracts between the two reviewers (BM, LW), which was resolved by discussion. A substantial level of agreement was reached for the 592 records that were assessed for eligibility ($\kappa = 0.78$, $p < 0.001$), with discrepancies in judgment for six studies, which also were resolved by discussion.

Description of included studies

The eleven studies [48–58] were conducted in seven different countries (China, Denmark, Germany, Japan, Taiwan, UK, USA) and full-text reports were published in English language between 1984 [56] and 2017 [57]. There was a high degree of diversity in sample sizes, intervention components, study duration, and mode of delivery (see Table 1).

Description of participants

Results from a total of 1082 participants were available for analyses and sample sizes ranged from n = 19 [49] to n = 391 [55] (M = 98; SD = 104; Median = 84; IQR = 36–107). Participant mean age across studies ranged from 19.47 (SD = 2.73) [48] to 58.42 (SD = 2.75) [49] years with a weighted average of 33.98 (SD = 12.34). Table 1 provides further details of participant characteristics. Participants of all study arms identified as poor sleepers with a weighted mean PSQI score of 7.67 (SD = 2.26) at baseline. In line with cut-off values for self-report measures other than the PSQI, participants in the remaining studies also classed as poor sleepers.

Description of interventions

The selected studies provided 24 study arms and 11 eligible intervention arms. One three-arm RCT [50] provided only one eligible intervention arm and another three-arm RCT [53] collapsed its two intervention arms for analyses and therefore was treated as a two-arm trial.

Interventions had a mean duration of five weeks (Range 2–10 wk), with repeat contact once per week in four out of the nine face-to-face studies (all of which used mind-body approaches [51–53,55]), a one-off session in two studies (sleep hygiene used in both [48,54]), daily contact in one study (relaxation training [56]), twice weekly contact in one study (comprehensive sleep management [52]), and three sessions per week in one study (aerobic exercise [49]). Two studies (online cognitive-behavioral program and mindfulness course [57,58]) were delivered entirely remotely and therefore did not involve any face-to-face contact. Seven studies [48,50–55] provided additional materials (i.e., booklets, audiotapes). Instructor-led group practice was complemented by structured home-based practice using complementary materials in five studies [48,50,51,54,55], whereas two studies [52,53] advised optional home-based practice. Both online programs [57,58] had a structured modular format, combining educational and instructional content.

The most frequently used cognitive and behavioral intervention components used to target changes in sleep were *stress management/relaxation* (m = 7); *meditation* (m = 4); *controlled breathing* (m = 4); and stimulus *control* (m = 4). The frequencies at which other components were used are listed in Table S4. With up to 13 components per trial, studies reported using an average of four intervention components.

Each intervention component was coded individually for use of behavior change techniques (see Table S4). Agreement between the two reviewers (BM, MJD) when coding each component against the 40 BCTs was almost perfect ($\kappa = 0.93$, $p < 0.01$). BCT use per component per study ranged from one to 16. The most frequently used BCTs across components were *providing instructions on how to perform the behavior* (k = 41), *providing information on where and when to perform the behavior* (k = 33), and *action planning* (k = 24).

Description of outcomes

Eight studies [48–50,52–55,57] measured sleep quality using the Pittsburgh sleep quality index (PSQI) [59]. Three of these [48,50,55] reported a total PSQI score and all seven component scores and one study [52] used a single-component PSQI measure (subjective sleep quality). Table 1 details the instruments used to assess sleep outcomes in all included studies. The change scores used for meta-analyses are presented in Table S5.

Attrition, adherence, and acceptability

Three studies [49–51] reported no loss to follow-up in either of their groups. Average loss to follow-up was 16% in intervention groups and 12% in control groups. Five studies that required home-based practice of intervention components reported program compliance using either participant diaries or website logs. However, it was generally unclear which components and what

Table 1
Summary table of characteristics reported in the included studies.

Study	n[a] (IG/CG)	Study design[b]	Format	Study duration[c]	Participants	Outcome measure
Brown et al. [48]	56/66	repeated-measures design	face-to-face; group-based education	6	US American students; M_{age} = 19.47 male and female (50:72)	PSQI (total and component scores)
Cai et al. [49]	10/9	controlled pre-post design	face-to-face; group-based instruction	10	Taiwanese postmenopausal women; M_{age} = 58.42	PSQI (total score)
Gao et al. [50]	42/42	2-arm RCT	face-to-face, brochure; group-based education	4	Chinese University students; M_{age} = 20.49; male and female (27:57)	PSQI (total and component scores)
Greeson et al. [51]	45/45	2-arm RCT with a waitlist CG	face-to-face; group-based instruction; home-based practice	4	US American students; M_{age} = 25.4; male and female (31:59)	MOS-SLP9
Hahn et al. [52]	48/47	2-group trial with a waitlist CG	face-to-face; group-based workshops	2	German employees (from various organizations); M_{age} = 44.6; male and female (42:53)	PSQI single-item score (sleep quality)
Jensen et al. [53]	48/24	3-arm RCT with a TAU CG[d]	face-to-face; group-based instruction plus materials (print, web, audiotape)	9	Danish volunteers of the general public recruited through GP practices; M_{age} = 42.24; male and female 25:47	PSQI (total score)
Kakinuma et al. [54]	214/177	2-arm CT with a waitlist CG	face-to-face and E-mail; group-based education	4	Japanese IT company workers; M_{age} = 33.8; male and female (316:75)	PSQI (total score)
Klatt et al. [55]	22/20	2-arm RCT with a waitlist CG	face-to-face; group-based instruction	6	US American working adults; M_{age} = 43.41; male and female (11:34)	PSQI (total and component scores)
Murphy [56]	11/8	3-arm RCT with a waitlist CG[e]	face-to-face; group-based instruction	2	US American highway maintenance workers; M_{age} = 42[f]	Sleep Quality (Sleep Behavior Scale)
Querstret et al. [57],[g]	60/58	2-arm RCT with a waitlist CG	online course (incl. video instructions)	6	British employees from various organizations; M_{age} = 40.68; male and female (23:95)	PSQI (total score)
Suzuki et al. [58]	12/18	2-arm RCT with a waitlist CG	online program (incl. website, E-mail, SMS)	2	Japanese workers; M_{age} = 39.6; male and female (25:16)	CSQI (total score)

Abbreviations: MOS-SLP9, Medical outcomes study sleep problem index-II; PSQI, Pittsburgh sleep quality index; CSQI, Current sleep quality index (over the last seven days).
Note.
[a] n analyzed per group, where IG = Intervention group and CG = Control group.
[b] As reported by the authors, where RCT = randomized controlled trial, CT = controlled trial and TAU = treatment as usual.
[c] Study duration in weeks.
[d] The two intervention groups in this study were collapsed for analyses.
[e] This study provided only one eligible study arm.
[f] Gender not reported.
[g] This study was available online in full-text at the time of screening; however, it was not indexed within any of the electronic databases until 2017.

proportion of instructed contents were taken up in the home-setting. None of the studies (m = 11) reported any adverse events.

Post-treatment efficacy

The PSQI total score (overall sleep quality) was based on pooled data from seven studies [48–50,53–55,57], and the multi-component score was based on pooled data from the nine studies [48–51,53–55,57,58] that reported scores from the PSQI total, the CSQI, and the MOS SLP-9 scale (Table 2).

Sensitivity analyses were conducted on both of the above listed primary outcomes by removing those studies that had the main aim to reduce stress [51,53,55,57] and a moderator analysis used to test differences between studies that had the primary aim to improve sleep [48–50,54,58] and those that aimed to improve sleep and other secondary health indicators through reductions in stress [51,53,55,57].

Secondary outcomes included all PSQI component scores. One analysis was conducted using outcome data (m = 4) for *subjective sleep quality* from the three studies [48,50,55] reporting all PSQI component scores and from one study [52], which used this item as a single measure to assess changes in sleep. Six separate analyses were carried out on the remaining PSQI component scores (m = 3 per analysis) for 1) *sleep onset latency*, 2) *sleep duration*, 3) *sleep efficiency*, 4) *sleep disturbance*, 5) *sleep medication use*, and 6) *daytime dysfunction*. Lastly, another meta-analysis of combined outcome measures was used to pool all single-component sleep quality scores (m = 5; using the subjective sleep quality score from the PSQI from four studies and the sleep quality rating used by Murphy, 1984 [56]). Analyses of both, pooled effects and moderator effects were standardized by change scores. Effect directions were kept negative, due to a reduction in PSQI scores corresponding to improved sleep quality [59].

Changes in PSQI total scores (m = 7) following intervention (see Fig. S2; Table 2) resulted in a medium effect for changes in overall sleep quality ($g = -0.54$, [95% CI] −0.89 to −0.19, $p < 0.01$) and a high level of heterogeneity ($Q = 30.1$, $I^2 = 80.0$, $p < 0.01$). Sensitivity analysis removing the three stress management studies increased the effect size ($g = -0.70$, [95% CI] −1.31 to −0.09, $p = 0.02$), but a moderator analysis confirmed that there was no statistically significant difference ($Q = 0.68$, [95% CI] −0.72 to −0.22, $p = 0.41$ between studies with the primary aim to improve sleep and those that measured changes in sleep following a stress reduction program.

The seven studies [60–66] with active comparator conditions provided a total of ten effect sizes with control groups receiving a range of reduced or minimal intervention content (e.g., sleep diaries, sleep hygiene education or basic health education). A summary table describing these studies and a forest plot showing the pooled effect are provided as supplemental material (Table S6). The pooled effect from this meta-analysis based on a random-effects model was small, yet in favor of the intervention groups relative to the active control groups ($g = -0.25$, [−0.39 to −0.10], $p < 0.01$).

Baseline sleep health was the only pre-specified moderator that was significant ($Q = 30.1$, $p = 0.01$) (Table 3). Those reporting poorer sleep health at baseline (PSQI total score ≥8) resulted in larger point estimates (−1.03, [95% CI] −1.65 to −0.41, $p < 0.01$) compared with studies reporting better sleep health (PSQI <8: −0.20, 95% −0.36 to −0.04, $p = 0.01$).

Table 2
Summary of effect sizes, study heterogeneity, and publication bias per outcome.

	Effect sizes				Heterogeneity				Publication bias	
	m	Hedge's *g*	CI	*p*-value	Q (df)	*p*	I^2	Tau^2	N^1	Criterion
Primary outcomes										
Overall sleep quality										
PSQI total	7	−0.54	−0.90; −0.19	<0.01	30.1 (6)	<0.01	80.0	0.17	65	45
Overall sleep quality										
Combined measures	9	−0.52	−0.80; −0.24	<0.01	30.4 (8)	<0.01	73.7	0.12	100	65
Secondary outcomes										
Subjective sleep quality										
PSQI component	4	−0.21	−0.43; −0.02	0.05	3.1 (3)	0.37	3.8	<0.01	0	30
Subjective sleep quality										
Combined measures	5	−0.22	−0.42; −0.01	0.04	3.4 (4)	0.49	0.0	<0.01	0	35
Sleep duration										
PSQI component	3	−0.32	−0.57; −0.07	0.01	0.9 (2)	0.65	0.0	<0.01	2	25
Sleep onset latency										
PSQI component	3	−0.44	−0.94; 0.05	0.08	7.1 (2)	0.03	71.7	0.14		
Sleep efficiency										
PSQI component	3	−0.28	−0.62; 0.06	0.11	3.5 (2)	0.18	42.1	0.04		
Sleep disturbance										
PSQI component	3	−0.22	−0.47; 0.03	0.09	0.8 (2)	0.67	0.0	<0.01		
Sleep medication use										
PSQI component	3	−0.15	−0.40; 0.10	0.25	1.1 (2)	0.59	0.0	<0.01		
Daytime dysfunction										
PSQI component	3	−0.67	−1.85; 0.51	0.27	36.4 (2)	<0.01	94.5	1.03		

Abbreviations: CI, confidence interval; PSQI, Pittsburgh sleep quality index.
Note. [1]*N* refers to the number of studies with a zero mean effect needed for *p* to be >0.05, based on Rosenthal's *fail-safe N* test (computed for statistically significant mean effects only). Standard deviations for change scores were imputed where necessary [36] and conservative pre-post correlations of $r = 0.5$ were used throughout. Effect directions for scores based on the CSQI, the MOS SLP-9 and the single-item sleep quality rating were reversed for consistency.

Table 3
Summary of outcomes testing moderator effects on overall sleep quality (PSQI total score).

Subgroups	m	Point estimates	Q	95% CI	P^1
Number of components					
Overall	7	−0.49	0.18	−0.77; −0.22	0.67
Less than four	4	−0.47		−0.76; −0.17	<0.01
Four or more	3	−0.63		−1.31; 0.06	0.07
Mean participant age					
Overall	7	−0.50	0.18	−0.77; −0.22	0.67
18–35	3	−0.63		−1.31; 0.06	0.07
36–64	4	−0.47		−0.76; −0.17	<0.01
Baseline sleep quality					
Overall	7	−0.25	6.57	−0.40; −0.10	0.01
Less than eight	4	−0.20		−0.36; −0.04	0.01
Eight or more	3	−1.03		−1.65; −0.41	<0.01
Primary study aim					
Overall	7	−0.47	0.68	−0.72; −0.22	0.41
To improve sleep	4	−0.70		−1.31; −0.09	0.02
To reduce stress	3	−0.42		−0.70; −0.15	<0.01

Abbreviation: CI, confidence interval.
Note. [1]Testing the hypothesis of a difference between subgroups using mixed-effects models (significant at $p < 0.05$).

Analysis of overall sleep quality from nine studies (see Fig. S3; Table 2) revealed a high level of study heterogeneity (Q = 32.2, $I^2 = 75.1$, $p < 0.01$). Pooling the effects of these sleep interventions yielded a medium-sized effect ($g = -0.52$, [95% CI] −0.80 to −0.24, $p < 0.01$). Removing the four studies that focused on stress management in this analysis also resulted in a slightly larger effect size ($g = -0.65$, [95% CI] −1.18 to −0.13, $p = 0.01$), but a moderator analysis that stratified effect sizes by primary study aims confirmed there was no statistically significant difference (Q = 0.46, [95% CI] −0.69 to −0.28, $p = 0.50$) between the two subgroups (sleep improvement versus stress management).

Pooling data from the four studies reporting subjective sleep quality resulted in low heterogeneity (Q = 3.1; $I^2 = 3.8$, $p = 0.37$) and a small effect size ($g = -0.21$, [95% CI] −0.43 to −0.002, $p = 0.05$). The addition of an additional study with a single-component sleep quality measure reduced study heterogeneity to zero (Q = 3.4, $I^2 = 0.0$ $p = 0.49$) and also resulted in a small, yet statistically significant effect ($g = -0.22$, [95% CI] −0.42 to −0.01, $p = 0.04$).

Changes in the PSQI component score for sleep duration (m = 3) showed a small to medium pooled effect ($g = -0.32$, [95% CI] −0.57 to −0.07, $p = 0.01$) and low heterogeneity (Q = 0.8; $I = 0.0$, $p = 0.66$). None of the meta-analyses conducted on the remaining outcomes (sleep onset latency, sleep efficiency, sleep disturbance, sleep med use, and daytime dysfunction) showed statistically significant changes. See Table 2.

Efficacy at follow-up

Only three of the included studies reported results from follow-up assessments, which took place after three weeks [58], 12 wk [53] and after 12 and 24 wk [57], respectively. Group means for sleep quality continued to improve following discontinuation of the intervention in all of these studies, but follow-up data were not pooled due to insufficient numbers of studies per outcome measure.

Clinical significance

In the context of chronic insomnia, cut-off criteria for treatment response and remission of sleep problems specify a 3-point change in PSQI total scores and a post-test score of less than five, respectively [67]. None of the studies that were meta-analyzed however, yielded a post-test PSQI total score under five. Only one study [53] reported a mean score below five in favor of the intervention group (IG 4.96 ± 2.93 compared with CG 6.63 ± 3.16), but this was measured at the 12-week follow-up. Although the majority of samples had a mean baseline sleep duration 'between 6 and 7 h', which shifted towards '7 h or more' after the intervention (reduced scores indicate longer sleep duration), a longer than recommended

sleep duration cannot be determined based on the scoring of this PSQI sub-component [3]. Measuring change in any of the PSQI component scores, in fact is problematic as response scores range only from zero to three.

Risk of bias

Following independent full-text assessment for risk of bias, the two reviewers agreed on 240 out of 260 scores ($\kappa = 0.86$, $p < 0.01$). Disagreements were resolved through discussion under consideration of the *a priori* consolidated criteria for each item. Table S7 shows that study quality varied substantially.

Publication bias

For both primary outcomes (PSQI total and PSQI total combined with other sleep health measures), the Rosenthal's *classic fail-safe N* was high with 65 and 100 studies needed to bring the *p*-value above 0.05. See Table 3. A Trim and Fill analysis of the pooled effect for the PSQI total score did not identify any outliers and the reported effect size therefore remained unchanged. Trim and Fill analysis of the combined PSQI total score resulted in one study being imputed to the left of the mean, which caused the pooled estimate to increase ($g = -0.59$ [95% CI] -0.90, -0.28). Funnel plots illustrating these findings are provided in the supplement Figs. S4 and S5.

Discussion

This systematic review with meta-analysis is the first to quantify the efficacy of cognitive and behavioral interventions to improve sleep health in adults without a clinical sleep disorder. Meta-analyses showed that cognitive and behavioral interventions have small effects on subjective sleep quality and sleep duration as individual parameters of sleep health. Improvements in overall sleep health were of medium size and appeared robust when comparing results based on PSQI total scores only ($g = -0.54$) and those on combined multi-component sleep health scores ($g = -0.52$). Moderator effects revealed that larger effects are observed in studies where sleep health baseline sleep health was worse. The moderator analysis comparing studies that had the primary aim to improve sleep relative to studies that sought to improve sleep as a secondary outcome to stress reduction was not significant. This may be due to studies with the primary aim to improve sleep also including a stress reduction component, despite not detailing that as a main aim.

Subjective sleep quality and sleep duration were the only two parameters of sleep health that improved significantly following cognitive and/or behavioral intervention. This may have been a function of the objectives most studies had and the extent to which changes in the various parameters of sleep health were tangible for participants. A small effect ($g = -0.32$) associated with improved sleep duration observed in this synthesis is similar to the magnitude of change ($d = 0.22$) observed in interventions targeting insomnia patients [46,68]. A direct comparison between these estimates is difficult, since CBT-I interventions in insomnia populations [18] commonly include sleep restriction. Sleep restriction is an intervention component that was not identified in any of the included studies, which may be due to the relatively short duration of studies in the current review.

Larger effects have been observed in systematic reviews of cognitive and behavioral sleep interventions for the treatment of clinical insomnia [46,69]. This may be due to a larger potential magnitude of change for populations with a clinical sleep disorder relative to non-clinical populations, which was reflected in the moderator analysis on baseline sleep.

Similarly, the lack of change observed for other components of sleep health (e.g., sleep onset latency) may have been due to assessment issues, as the use of self-report measures for these parameters is known to be subject to recall bias [70]. No study used an objective measure of sleep (e.g., polysomnography, accelerometers) despite the growing use of accelerometers for the assessment of sleep in epidemiology research and interventions research [71]. Using a combination of both accelerometer and continuous self-report measures (e.g., sleep diaries) may assist to overcome this, while still catering for the issue that accelerometer-based methods are not capable of assessing the perceived restorative effects of sleep.

The small effect that was found for studies with an active control group again, did not lend itself for comparison or incorporation with the primary effect estimate for studies that did not have active control groups. Particular caution should be applied when interpreting the pooled estimate for these studies, although all of the studies in this meta-analysis employed an active control group, they varied greatly in what was included as the active control, which introduced an undesirable level of heterogeneity. Overcoming this would have required moderator analyses to be conducted by type of comparator (e.g., non-sleep specific, minimal sleep intervention), for which too few studies were available. This finding however, does provide some support for the superiority of the cognitive and behavioral interventions that were tested in these studies.

Effect moderators

Due to the low number of studies that were available for synthesis, any subgroup analyses conducted in this review are exploratory in nature and therefore warrant cautious interpretation. The only statistically significant effect moderator for PSQI score was baseline sleep health. Although mean changes were significant in both subgroups, a greater effect was seen in those with poorer sleep health at baseline (PSQI > 8). Whilst likely a result of the low number of studies per subgroup, this observation is partially explained by the smaller margin of improvement that can be achieved with individuals who have less severe sleep difficulties [42] (i.e., ceiling effects).

Although hypothesized *a priori*, mode of delivery, study duration and the inclusion of a relaxation component were not assessed as effect moderators, due to insufficient effect sizes available per subgroup. In a clinical context, however, there is some evidence for the comparable efficacy of face-to-face versus remote modes of delivery [43], thus future studies using remote intervention delivery in non-clinical populations are warranted. Furthermore, examining the efficacy of longer interventions in non-clinical populations may be worthwhile, given prior reviews of CBT-I in insomniac populations demonstrated that longer lasting studies yielded larger effects [25].

The use of behavior change techniques

The overall reporting of BCTs was generally inadequate, which made it impossible to incorporate this factor in the quantitative synthesis. Patterns for reported use of BCTs were relatively consistent between studies and many were based on utilizing information and instructions relating to the behavior (e.g., sleep hygiene). A greater number of BCTs were used in studies with cognitive and mind-body components (e.g., mindfulness), whereas fewer BCTs were reported in studies using mainly behavioral components. This was particularly true for exercise and food intake in relation to sleep, where generic advice on the importance of these behaviors was provided, but no further implementation plans

were given to participants. This may reduce the likelihood that participants change physical activity behaviors and obtain the benefits that physical activity has on sleep [46,62].

The behavior change techniques most commonly used in interventions for other health behaviors, for example goal-setting, self-monitoring, and feedback [72], were less prominent in the sleep interventions examined in this review. A certain degree of concern when implementing strategies that have the potential to exacerbate sleep problems may exist, if not delivered appropriately and with due guidance. Self-monitoring for example, involves a strong observational focus on practicing the behavior in question and encourages individuals to build a sense of enhanced self-efficacy when evaluating behavioral progress against goals or expectations over time [73]. In some participants, this may lead to unintended outcomes (i.e., delayed sleep onset due to feelings of frustration), caused by undue effort assigned to trying to sleep, which is a common driver of chronic insomnia. Given the evidence that goal-setting, self-monitoring, and feedback are consistently associated with improvements in other health behaviors, it would be useful to examine the efficacy of these techniques to improve sleep health. This needs to be implemented in a way that is cognizant of these issues.

Implications of findings from this review

This review focused on populations reporting poor sleep in the absence of a diagnosed sleep disorder and demonstrated that cognitive and behavioral interventions are effective at improving sleep quality and duration. This has important implications given the large number of adults who report poor quality sleep, but do not have a sleep disorder. It was beyond the scope the review to comment on how these improvements influence the risk of developing future sleep disorders. Despite the magnitude of observed effects, intervention efficacy needs to be enhanced, given no study reported PSQI scores under five at post-treatment. Reported attrition was low; however, study durations were relatively short and it is unknown, if dropout would increase in studies of longer duration, given the evidence from other health behavior interventions [74]. The majority of participants were full-time students or employees, who were in relatively good health, hence why the efficacy of similar interventions in other populations is unknown.

Limitations

It remains unclear to what extent individuals with poor sleep health are truly distinct from those who would meet diagnostic criteria for insomnia. It may be that included participants simply had an undiagnosed sleep disorder. However, PSQI mean scores in chronic insomniacs are usually much higher (>10 [75]) compared with those observed in included studies and may be indicative of a different population group. The coding of BCT was constrained by a lack of detailed reporting in the studies, thus future reports are encouraged to improve reporting of intervention strategies used to operationalize the intervention.

In studies including multiple sleep hygiene recommendations, it was not possible to determine adherence to different recommendations and the difficulty in assessing adherence is exacerbated by the fact that not all sleep hygiene recommendations apply to all participants. It is undetermined, if the exclusion of studies where effect sizes could not be calculated (due to missing data) affected the overall findings in this review; however, all of these studies reported improvements in one or several parameters of sleep following intervention. Some level of bias may have been introduced by only including published studies and studies published in English language, however, the impact of this is likely to be minor [76]. Statistical tests for publication bias showed the findings in this review are robust for the primary outcomes. This potential limitation was further reduced by searching trial registries for studies that were yet to be published.

Directions for future research

Future interventions are encouraged to combine educational approaches with BCTs to help provide participants with the tools necessary to drive behavior change. There is a need for future studies to better utilize the potential of individual intervention components and extend the choice of self-regulation strategies beyond the ones fostering implementation intentions (i.e., action planning) [77]. Some components identified in this review are components commonly found in CBT-I interventions, suggesting these components also are effective in non-clinical populations. However, there is a need to test the efficacy of these interventions in more diverse populations to better understand the mechanisms that drive changes in sleep health. Further, given many of the included trials still included some face-to-face aspect, it will be useful to further examine the efficacy of interventions that use a mode of delivery capable of broad reach to address the high prevalence of poor sleep health.

Conclusion

This systematic review with meta-analysis showed that interventions involving activities that de-stress mind and body significantly improve sleep quality in adults without clinical sleep disorders. Although producing robust effects of medium magnitude on overall sleep health, interventions using cognitive and behavioral components show room for improvement, as the exact mechanisms by which sleep health is restored to normal (PSQI <5) remain to be investigated. Additional investigations into broad-reaching interventions that promote self-regulatory strategies with the aim to improve sleep health are much needed and the present review supports the efficacy thereof.

Practice points

1) Previous systematic reviews of sleep interventions have focused exclusively on individuals with clinical insomnia.
2) The high prevalence of individuals reporting poor sleep health, but no diagnosed sleep disorder calls for a summary of interventions that are effective in this subgroup.
3) Pooled estimates with medium effect size (Hedge's $g = -0.54$) were achieved for changes in overall sleep health (PSQI total score) at the immediate post-test following self-regulatory strategies to de-stress mind and body.
4) Participants also improved subjective sleep quality and sleep duration
5) Effects were greater in those with poorer sleep health at baseline.
6) Sleep continued to improve at the follow-up, but very little data were available for an evaluation of long-term behavior maintenance.

Research agenda

1) Larger, more diverse samples are required.
2) Studies with longer study duration including follow-up assessments may provide insights into long-term changes in sleep health.
3) Comprehensive guidance is needed to facilitate the implementation of behavior-specific knowledge for participants.

Conflicts of interest

The authors of this review have no conflicts of interests to declare.

Funding and Acknowledgments

This research was supported in part by a grant from the National Heart Foundation of Australia (100029) awarded to Mitch J Duncan and a grant from the Australian Research Council (FT140100399) awarded to David R Lubans.

Appendix A. Supplementary data

Supplementary data related to this article can be found at https://doi.org/10.1016/j.smrv.2017.12.003.

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