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# Health Effects of Psychological Interventions for Worry and Rumination: A Meta-Analysis.

Dane McCarrick[1*]

Andrew Prestwich[1]

Arianna Prudenzi[1]

Daryl B. O'Connor[1]

[1]School of Psychology, University of Leeds, Leeds, LS2 9JT, UK

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**Health Psychology**

Running Head*:* Perseverative Cognition on Health

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*Corresponding author at: School of Psychology, University of Leeds, Leeds, LS2 9JT, UK

Email: D.J.McCarrick@leeds.ac.uk

## Abstract

*Objective*

Evidence suggests that perseverative cognition (PC), the cognitive representation of past stressful events (rumination) or feared future events (worry), mediates the relationship between stress and physical disease. However, the experimental evidence testing methods to influence PC and the subsequent relationship with health outcomes has not been synthesised. Therefore, the current review addressed these gaps.

*Methods*

Studies randomly assigning participants to treatment and control groups, measuring PC and a physical and/or behavioural health outcome after exposure to a non-pharmacological intervention, were included in a systematic review. Key terms were searched in Medline, PsycInfo and CINAHL databases. Of the screened studies ($k$ = 10,703), 36 met the eligibility criteria.

*Results*

Random-effects meta-analyses revealed the interventions, relative to comparison groups, on average produced medium-sized effects on rumination ($g$ = -.58), small-to-medium sized effects on worry ($g$ = -.41) and health behaviours ($g$ = .31), and small-sized effects on physical health outcomes ($g$ = .23). Effect sizes for PC were positively associated with effect sizes for health behaviours (following outlier removal). Effect sizes for PC were significantly larger when interventions were delivered by healthcare professionals than when delivered via all other methods. No specific intervention type (when directly compared against other types) was associated with larger effect sizes for PC.

*Conclusions*

Psychological interventions can influence PC. Medium-sized effect sizes for PC correspond with small, but positive associations with health behaviours.


*Keywords*: Perseverative cognition, Worry, Rumination, Health outcomes, Meta-analysis.

Psychological stress has consistently been linked to negative health outcomes, with recent figures suggesting stress-related health care costs an estimated $300 billion per annum (American Institute for Stress, 2020). Indeed, the impact of psychological stress, that is, when the appraisal processes attached to a threat or experience exceeds an individual's perceived coping ability, has long been implicated in a variety of health and illness outcomes (e.g. neurotic symptoms, House et al., 1979; organ damage, Plante, 2002; cardiovascular disorders, Lundberg, 2005; migraines, Schoonman., 2007; diabetes, Öhman, Bergdahl, Nyberg & Nilsson, 2007; for a review see O'Connor, Thayer & Vedhara, in press). Whether directly through autonomic and neuroendocrine responses or indirectly, via changes[1] in health behaviours (Christiansen, Larsen & Lasgaard, 2016, Jones & Bright, 2007, O'Connor, Thayer & Vedhara, in press), adverse health outcomes have been noted to be of direct consequence to stress, even when the stressor is no longer present (Brosschot et al., 2006). In particular, perseverative cognition (PC) has been identified as an important mechanism that may help explain how stressful events and encounters increase the risk of ill-health and poor wellbeing. PC is thus defined as any type of stress-related, negative, repetitive thought and encompasses thoughts about feared future events (worry) and thoughts and negative feelings about distressing past experiences (rumination).

In the original perseverative cognition hypothesis (PC hypothesis), Brosschot et al. (2006) suggested that stressful thoughts activate the body's stress response in the same way as stressors in the physical environment and serve to prolong the hypothalamic-pituitary-adrenal-axis stress response. Since then, several key reviews have shown that PC is associated with a range of physiological health outcomes; including higher blood pressure and heart rate, lower heart rate variability, as well increased cardiovascular activity, reduced secretion of antibody productions, blunted cortisol response and increased levels of somatization (for reviews, see Ottaviani et al., 2018; Verkuil, Brosschot, Gebhardt & Thayer, 2010).

Aside from evidence connecting PC with physical health, emerging work suggests PC can influence a variety of health behaviours including sleep, diet and alcohol

consumption (Clancy, Prestwich, Caperon & O'Connor, 2016; Cropley et al., 2012; Frone, 2015). Importantly, these negative health behaviours are related with illness (Suris & Parera, 2005), disease and morbidity rates (Burke et al., 2007), in both adults and children cross-culturally (for review, see Mackenbach, 2014). Notably, in a meta-analytic review of health behaviours across 19 studies, Clancy et al. (2016) showed that higher levels of PC were associated with significantly more health risk behaviours. In particular, these authors found that PC was associated with greater substance use, unhealthy eating and smoking. Taken together, these findings provided evidence for an extended PC hypothesis, such that there may be scope for an additional route to pathogenic disease via poorer health behaviours.

However, the evidence base discussed thus far for the impact of PC on both health behaviours and physical health outcomes is mostly based on correlational methodologies. Reliance on this type of evidence has a number of issues as: (a) it does not account for the likelihood that negative health-outcomes may trigger variations in measures of PC and/or vice-versa; (b) it overlooks consistency biases that may inflate the strength of the relationship between stress and health outcomes, as shown in previous work (see, Arkin, Gabrenya, Appelman & Cochran, 1979; Renner, Laux, Schütz & Tedeschi, 2004); and (c) it disregards statistical considerations around the important role(s) of confounding variables on the PC and health outcome relationship; meaning the impact of a third variable, or 'spuriousness', is often not accounted for in analyses (see, Kenny, 1979; Mauro, 1990). An alternative, more valid way to strive towards understanding causality would be to observe studies whereby an experimental manipulation brings about statistically significant differences in PC between intervention and control arms after exposure to some level of intervention, while observing the same between group differences with subsequent measures of health. This approach can be considered superior to correlational tests as: (a) standardized differences between intervention arms within measures of PC (particularly when assessed early) are attributable to an experimental manipulation and thus are not based on deviations in health accrued later; and (b) random assignment of participants to condition help to account for the influence of extraneous variables and potential biases.

A number of techniques have been used in an attempt to influence PC (e.g. mindfulness, Garland, 2011; relaxation, Andersson et al., 2012; action planning, Versluis, Verkuil, Spinhoven & Brosschot, 2018), however, these are small in number and there are few, if any, that observe health consequences. Querstret and Cropley (2013) represent the only available review exploring how PC might be reduced via psychological interventions. Across nineteen studies, comprising both face-to-face and internet-delivery formats, interventions in which participants were encouraged to detach themselves from emotional responses to PC and adopt more concrete or re-constructive ways of thinking, were reported as most promising. However, few studies in the Querstret and Cropley review were explicitly designed to target PC, it only includes studies between 2002 and 2012; and, most importantly, it did not consider the impact of changing PC on health outcomes. An up-to-date evaluation of current studies which provides a quantitative estimate of the effectiveness of interventions for reducing PC, while also accounting for moderating factors and health consequences, is thus timely and warranted.

*The present review*

Evidence for the PC-health outcome relationship has tended to be based on correlational evidence (for reviews, see Ottaviani et al., 2016; Clancy et al., 2016) and a review has not been conducted to identify the best approaches to reduce PC in a health context that captures the consequences of changing PC on health behaviours and physical health outcomes. Thus, using the available experimental literature, in this review we examined whether: PC can be influenced by interventions (Objective 1a); and, if so, which intervention or study characteristics, following exposure to intervention content, produce larger effect sizes for PC (Objective 1b); interventions that target PC also impact health outcomes (Objective 2a); and, if so, which intervention or study characteristics, at post-intervention, produce larger effect sizes for health (Objective 2b); larger effect sizes for PC are also associated with larger, but positive, effect sizes for health outcomes at post-intervention (Objective 3). Across these objectives, PC was considered at three levels (worry, rumination and both PC types combined) and health outcomes were explored across

two levels (health behaviours, physical health outcomes). Sleep (the most popular health outcome) and a composite measure for both types of health outcomes (behaviours and physical health combined, health *overall*) were also considered but these findings are reported in the online supplementary material (OSM).

## Method

This review was pre-registered with PROSPERO (CRD42019119381) and is available on the Open Science Framework (see, https://bit.ly/35X81xi).

### *Eligibility Criteria*

To be eligible, studies had to: (1) involve the random assignment of participants to a treatment group that received a psychological intervention targeted at PC or to a control group who received either a control intervention or no intervention, (2) include a measure of perseverative cognition (worry and/or rumination) after exposure to an intervention, (3) contain measures of either physical health outcomes and/or health behaviours, at follow up (to reflect the PC hypothesis). Studies were excluded if: (1) they had a non-human (animal) sample, (2) they were an existing review/meta-analysis, (3) if any aspect of the intervention was pharmacological (i.e. to test the effects of a drug), or (4) participants were specifically recruited on the basis of a learning disabilities/intellectual disorders (e.g., cerebral palsy, autism, epilepsy) severe alcohol and/or substance dependency (i.e., based on author classifications as per standardized measures), or severe psychiatric disorders (e.g., schizophrenia, bipolar disorder, depression with psychotic symptoms, psychosis, serious suicidal thoughts). However, because Generalised Anxiety Disorder (GAD) has several temporal and theoretical properties relating to PC (e.g., repetitive negative thinking, constant worrying), studies whose participants had a diagnosis of GAD ($N = 2$) were included; so long as they did not have other severe comorbid mental health disorders akin to those described above. Studies comprising participants with sleep disturbances (i.e., insomnia, $N = 4$) were also included, as we were interested in the effects of PC on parameters of sleep.

Pharmacological based interventions were not included for two main reasons. First, such interventions are very different to the psychological therapies included in this review as

they trigger change at the neuroendocrinological level that are out of the control of the participant; i.e. taking a pill/tablet is not comparable to offering people a strategy to control their worry. Whereas, all the studies within our inclusion criteria offered participants a conscious opportunity to tackle their PC. Second, the participants included in pharmacological studies typically derive from samples which have several co-morbid issues that may interfere with the PC-health outcome relationship.

*Search Strategy*

Three databases were searched to maximize search sensitivity (see Montori et al., 2005): PsycINFO (1806 – present) and Medline (1806 – present) via OVID, and CINAHL (1960-present) using EBSCO. The search was last conducted on the 23rd November 2019 with search terms relating to perseverative cognition, and randomized interventions. Perseverative cognition search terms were adapted from Clancy et al. (2016). Specifically, "negative and (thought or thinking)" was removed to enhance specificity; "perseverati" with "cogniti" was replaced with "perseverative and (thought* or thinking or cognition*)". The Eady et al. (2008) RCT filter (random*.tw) was employed as a single term to capture the best optimisation of sensitivity and specificity, complimented with the term (intervention*.tw) to enhance sensitivity. Further, to maximise sensitivity (at the expense of specificity), search terms were not generated for health outcomes. The search was limited by the English language and human studies but not by year (see, OSM 1). Titles, abstracts, and full-text screening were completed by the first author. The third author independently screened the titles and abstracts using a subset of 1070 studies (20% of total) (Cohen's kappa = .91). Any discrepancies were discussed and resolved. Any study identified as potentially eligible at the abstract screening stage was progressed to full-text screening. The first author then independently assessed all full-texts with 40% of full-texts independently double-screened by the third author (Cohen's kappa = .98). Discrepancies were then discussed and verbally agreed upon between both authors. Across the sets of double-screened studies, the secondary coder did not identify any eligible studies missed by the primary coder.

*Data Extraction & Data Coding*

The subsequent data were extracted and coded for each study: lead author name, publication year, country, study design (RCT or cluster RCT), measurement points (in days) for PC and health outcomes, type of PC (worry or rumination), measurement of PC and health outcomes (i.e. self-report vs non-self-report), health outcome type (behavioural or physical), participant characteristics: age, percentage female, GAD diagnosis, sleep disturbance, and number of participants included in analysis and attrition (across the entire study). We recognise health outcomes is a broad term, though for the purposes of this review, we defined health behaviours a-priori as an action(s) to maintain, attain, or regain good health and to prevent illness (Conner & Norman, 2005) and physical health outcomes as any marker indicative of, or which would impede, impact or constrain routine physiological functioning (e.g., neurological, circulatory, endocrinological, immune, digestive, muscular systems) (Corbin, Pangrazi & Franks, 2000).

The following main intervention types were extracted: pain management, PC action plans (i.e., planning interventions to help better manage PC), stress management (i.e., broad ranging therapies concerned with eliminating stress), mindfulness and relaxation (i.e., refocusing on the present moment), psychological detachment (i.e., 'switching off' from situations, such as work, that trigger negative affect), Cognitive Behavioural Therapies (CBT) and Acceptance and Commitment Therapies (ACT) (i.e., challenging unhelpful thoughts and engendering self-help strategies) and expressive writing (i.e., disclosing one's deepest thoughts and feelings). Other features of the intervention: duration (in days), number of sessions, weeks delivered across, delivery format (group or individual), mode of delivery (health-care professional, self-administered, trained facilitator) and if the intervention was delivered online or delivered in-person was also assessed. Study setting was also evaluated. Studies were classified as medical if they took place within a hospital or health-care environment, educational if within a school, or academic if they took place within a university or research unit.

Study quality and risk of bias were assessed using all items from Cochrane's Risk of Bias tool (Higgins et al., 2011), including selective outcome reporting and extra bias sources.

Other important methodological or statistical features (e.g., using validated measures, reporting of satisfactory levels of internal consistency, baseline differences between groups) and if studies incorporated intention-to-treat analysis (ITT) were also considered. We approached data extraction in two phases to minimise the possibility of coding errors. The first phase was piloted on 10% of the studies in a 'training phase". For this piloted 10%, the coding for all measures was checked by a second reviewer. Inter-rater agreement levels were classified as near-perfect for items relating to health outcomes and PC (Cohen's kappa = .75 - .1) and often perfect for items relating to risk of bias and other study characterises (i.e., population, attrition, design, measure timing) (all kappas >.92; Landis & Koch, 1977). Second, we operated a 'validation phase' whereby data for all studies was first extracted by a primary coder before an extra 20% of studies were independently assessed by a second coder. For this phase, agreement between coders was near perfect across all study items (Cohens kappa = .97 - .1). In all cases, if either coder was in any doubt, the study authors were contacted for additional clarification before making deciding upon eligibility.

*Data Synthesis*

Effect sizes were calculated based on means and standard deviations and, when not available ($k = 6$), using other statistics reported (i.e. $F$ and $p$ values). Effect sizes were calculated for *PC overall* (worry, rumination and measures of perseverative thinking combined), for worry and rumination separately, for health behaviours and physical health outcomes separately, as well as for sleep as it was the most common health outcome (77.3% of studies) (note, we view sleep as a health behaviour as it is an action that is under volitional control). Results pertaining to health outcomes *overall* (i.e., physical health and behaviours combined) are reported in OSM 1 and 2. Standard errors were adjusted to account for clustering in relevant studies ($k = 3$) (see Higgins, Deeks, & Altman, 2008). Hedges' $g$ was used as the main effect-size measure (see OSM 2 for Hartung-Knapp-Sidik-Jonkman method) as it provides an unbiased estimate of effects (Hedges & Olkin, 1985).

When more than one intervention group was present ($k = 5$), there were four cases where we selected the arm which authors stated, or hypothesised, would outperform the

other arms. However, as this was not made clear in one study (Topper et al., 2017), to avoid including the same participants more than once within the meta-analysis (to avoid unit-of-analysis error) and because the primary aim of this review was to identify the most effective methods of influencing PC, the intervention that generated the largest effect on PC was selected. For the selection of comparator groups there was just one study whereby there was more than one comparison group present (i.e., 'waitlist' vs. 'standard control'; Versluis et al., 2018). In this case, the 'standard control' was selected for our analyses because: a) authors hypothesized that the 'standard control' would be more likely to reduce PC than the 'waitlist' and, b) because the 'standard control' in this particular study contained all the features of an attention-placebo control (i.e., an intervention that mimics the theoretically inactive elements, but not the active elements) which are regarded as highly valid control groups (Popp & Schneider, 2015).

Effect sizes were calculated using the first measure of PC following exposure to an intervention and the final measure of health reported in each study. We used this approach because the temporal relationship that was of primary interest was from PC to health rather than vice-versa and because the impact of interventions on PC was more likely to be detected at this initial time point (i.e., after intervention exposure), rather than in later follow-ups (i.e., in a number of weeks/months). We did not consider baseline scores within the calculation of study effect sizes because data was not always available for baseline assessments across the included studies and none of the studies reported pre-post correlations on the dependent variable which are used in the calculation of these effect sizes. Given concerns regarding additional heterogeneity with baseline scores being reported for some studies but not others, and the need to estimate correlations, effect sizes were based only on post-intervention scores. In cases where there were multiple measures of the same construct (e.g. two questionnaires for worry, total sleep time and sleep onset latency) the effect sizes were calculated and then averaged using a random effects model. All analyses were exclusively between conditions (treatment vs control) and none were within conditions.

STATA (version 13) was used to conduct random-effects meta-analyses (to produce effect size estimates for the effect of interventions on influencing PC (objective 1a) and impacting health outcomes (objective 2a). STATA was also used for sub-group analysis and meta-regressions; to assess whether the presence or absence of specific study or intervention characteristics were associated with: larger effect sizes for PC (objective 1b) and for health outcomes (objective 2b), as well as the association between larger effect sizes for PC and effect sizes for health outcomes at post-intervention (Objective 3). For this latter objective, the 'Metafor' package (Viechtbauer, 2010) in *R* was used to conduct permutation test(s) with 10,000 random interactions to test the robustness of effects. The package was also used to test for potential influential cases and/or outliers (using the 'influence' function) (in addition to visual plot inspections) in the relevant sensitivity analyses. All meta-regressions were univariate, except to test for confounding between two significant moderators (these exceptions can be found in OSM 2, section B).

A range of additional analyses were conducted to: (a) check data met the statistical assumptions associated with regression such as multivariate normality, low multicollinearity, lack of auto-correlation and homoscedasticity; (b) identify potential confounds that may have affected the conclusions and consider the results when the behavioural and physical health outcomes were combined as an *overall* health index; (c) assess the possible impact of two studies for which we had concerns regarding the measures of behaviour; assess the robustness of the findings when focused only on studies (d) measuring PC *immediately* post-intervention and then health at a *later* point in time and (e) measured sleep; (f) check for small-study bias; (g) assess, when an alternative study arm was available (i.e., two treatment arms/different control types), if our approach to arm selection significantly altered study effect sizes for both PC and health; h) control for the possibility that baseline between group differences influenced effect sizes; i) detect if clinical heterogeneity influenced effect sizes. The results of these ten sets of additional analyses are reported in OSM 2.

## Results

Studies considered for inclusion in the review are displayed in Figure 1. Thirty-six studies met the inclusion/exclusion criteria. Nineteen studies included measures of worry (52.7%), 9 included measures of rumination (25%) and 11 measured perseverative thinking (a composite measure of worry and rumination) (30.5%). Of these studies, two included measures of both worry and rumination (Ebert et al., 2015; Thiart, Ebert & Riper, 2015) and one study (Topper et al., 2017) included measures of worry, rumination and perseverative thinking. Regarding health outcomes, 21 studies (58.3%) included measures of physical health and health behaviours, and, of these, 6 studies included measures of both a health behaviour and physical health outcome (6%). Of all health behaviours, sleep was the most common ($k$ = 17, 77.3%) and, of all physical health outcomes, pain ($k$ = 3, 14.3%) was the most common.

**INSERT FIGURE 1 HERE**

*Study Characteristics*

The characteristics of included studies are summarized in OSM 1, Table 1. All studies were RCTs (3 cluster-trials, 33 non-cluster trials). Twenty-one studies (58.3%) obtained participants from academic research settings, seven (19.4%) sourced participants from educational environments (i.e., schools) and 8 (22.2%) drew participants from medical settings (e.g. hospitals; clinics). Nine (25%) utilised a student sample and, on average, 70.4% of participants were female. Thirty-one studies (86.1%) recruited adults (aged 18 or over) and 5 (13.8%) obtained samples of school children. Studies were conducted across 9 countries, though the most common were the USA ($k$ = 9, 25%), Netherlands ($k$ = 8, 22.2%) and Germany ($k$ = 7, 19.4%). The mean age of all participants ($n$ = 5098) was 36.52 years ($SD$ = 14.32) and the average number of participants in each study, across all studies, was 142 ($SD$ = 53.88). Two studies (5.5%) recruited their participants on the basis of a GAD diagnosis and a further four (11.1%) studies had participants which reported sleep disturbance (i.e., insomnia).

On average, content was provided across 8 days (*SD* = 4.27), with intervention groups receiving content on more days (*M* = 9.2, SD = 3.81) than the comparison groups (*M* = 7.14, SD = 3.11). The mean time-point at which post-intervention measures were collected (from initial exposure to intervention content) was 49 days (*SD* = 52.49) for PC, 99 days for physical health outcomes (SD = 103.06) and 143 for health behaviours (SD = 130.38) (*M* = 118*, SD* = 115.59 for health outcomes *overall*). All of the interventions that were delivered in an in-person (*k* = 21, 58.3%) used printed materials, and employed a variety of delivery formats (i.e., self-administered, self-administered with support, healthcare professionals). Fifteen studies (41.66%) were hosted using an online platform (i.e., computer, mobile phone or tablet based). The most popular mode of delivery was interventions that were self-administered, with participants set a task to complete (e.g., to postpone worry) by experimenters in their own time (*k* = 16, 44.4%), followed by self-administration with support (i.e. from the experimenter) (k = 8, 22.2%). Less popular were interventions delivered with a trained facilitator (i.e. a mindfulness coach) (*k* = 6, 16.6%), or by a health-care professional (i.e. a nurse practitioner) (*k* = 4, 11.1%). Of these, three studies (8.3%) also used the telephone, two studies used mail (5.55%) and one study adopted a video to deliver part of the intervention (3.6%). The interventions tested were broadly defined as: (1) cognitive behavioural/acceptance and commitment therapies (*k* = 10, 27.7%), (2) PC action plans (*k* = 9, 25%), (3) mindfulness and relaxation (*k* = 7, 19.4%), (4) stress management (*k* = 4, 11.1%), (5) psychological detachment (*k* = 2, 5.5%), (6) expressive writing (*k* = 2, 5.5%), and (7) pain management (*k* = 2, 5.5%). While these categories do not capture the granular level nuances between interventions, they do represent the core therapy used.

In general, studies were unclear or at high risk of bias. Although only 4 studies (11.1%) failed to report a valid method of randomization, 21 (58.3%) did not report a method of allocation concealment, 29 (80.6%) did not report adequate steps to blind the experimenter or data analyst and 34 (94.4%) did not report adequate methods to blind participants. Over 60% of studies (*k* = 20, 61.1%) did not claim contamination prevention between groups and did not consider using ITT analysis, though only one study (3.6%) used

measures of PC that were not internally reliable. The majority of studies contained information on informed consent ($k$ = 32, 88.8%). Attrition rates were moderate (22.9%, *SD* = 16.63), and did not significantly influence PC effect sizes ($p$ =.381). A summary of the risk of bias for each study is available via OSM 3 & 4. Despite instances of high risk of bias across the included studies, each risk of bias item did not moderate the effects of the interventions on PC ($p$ =.076 to .981; median = $p$ = .432).

*Objective 1a: Can PC (worry and rumination) be influenced by interventions?*

Levels of PC were lower in the intervention group versus the comparison group at follow-up. The interventions produced, on average, a near medium-sized effect on PC, $g$ = -0.42, 95% *CI* = -0.51 to -0.33 ($k$ = 36, see Figure 2), albeit the effect sizes were heterogeneous across studies, $I^2$ = 59.3%; $Q(35)$ = 87.17 $p$ < .001. A similar-sized, and heterogeneous effect, $I^2$ = 47.9%; Q(18) = 34.56 $p$ = .011, emerged when the analyses were repeated specifically for worry, $g$ = -0.41, 95% *CI* = -0.51 to -0.30 ($k$ = 19, see OSM 1, Figure 1). Interventions produced a medium-sized effect on rumination, $g$ = -0.58, 95% CI = -0.84 to -0.32 ($k$ = 8, see OSM 1, Figure 2), with the effect sizes again heterogeneous, $I^2$ = 66.9%; Q(7) = 21.14 $p$ = .004.

**INSERT FIGURE 2 HERE**

*Objective 1b: Study characteristics associated with greater effect sizes for PC.*

All but two of the seven intervention types (pain management and expressive writing) produced significant effect sizes for PC. However, meta-regressions indicated that none of the intervention types produced larger effects than the other interventions combined (see OSM 1, Tables 2 & 3). Effect sizes were significantly larger, suggesting more effectiveness, when interventions were delivered by healthcare professionals, $B$ = 0.39, *S.E.* = 0.18, *CI* = -0.77 − -.009, $p$ = .045, versus when they were not delivered by healthcare professionals. No other moderators influenced PC effect sizes across all PC related analyses.

Three intervention types, (PC action planning, psychological detachment and CBT) produced significant effect sizes for worry, though subsequent meta-regressions revealed none of these intervention types outperformed one another. Effect sizes were, however,

significantly larger in studies comprising of a student sample, *B* = -0.35, *S.E.* = 0.14, *CI* = -0.65 − -0.05, *p* = .024, than in those which did not. Worry effect sizes were not influenced by any other moderators across all other worry related analyses.

Four intervention types (mindfulness, psychological detachment, CBT and pain management) produced significant post-intervention differences in rumination between the intervention and comparison conditions (see Table 2, OSM 1), though subsequent meta-regressions revealed none of these intervention types outperformed one another. These effects were not influenced by any moderators.

*Objective 2a: Can interventions targeting PC also impact health outcomes?*

The interventions targeting PC, on average, led to a small-to-medium, and heterogeneous $I^2$ = 51.8%; *Q*(20) = 41.50 *p* = .003, effect for health behaviours, *g* = 0.31, 95% *CI* 0.21 to 0.42 (*k* = 21, see Figure 3). A similar-sized, but non-significant and homogeneous $I^2$ = 24.7%; *Q*(20) = 26.57 *p* =.148, effect, *g* = 0.23, 95% *CI* = 0.15 to 0.31, was detected for physical health outcomes (*k* = 21, see Figure 4).

**INSERT FIGURE 3 & 4 HERE**

*Objective 2b: Study characteristics associated with larger effect sizes for health behaviours and physical health.*

A range of study characteristics were significantly associated with effect sizes for both health behaviours and physical health outcomes. These are reported in full within OSM 1 (see, Table 2 - 4) and OSM 2 (see, section B); where we also consider the impact of confounding. In brief, all intervention types had a significant, positive effect on health behaviours with the exception of pain management strategies. However, the effect sizes in studies testing psychological detachment style interventions, *B* = 0.33, *S.E.* = 0.16, *CI* = -.007 − 0.67, *p* = .05, and PC action plans, *B* = 0.37, *S.E.* = 0.14, *CI* = 0.08 − 0.66, *p* = .016, produced significantly larger effect sizes than studies not testing this intervention type for health behaviours. In addition, effect sizes were significantly larger when interventions were self-administered, *B* = 0.26, *S.E.* = 0.09, *CI* = 0.07 − 0.45, *p* = .01, delivered at an individual level rather than group-level, *B* = -0.25, *S.E.* = 0.11, *CI* = -0.49 − 0.006, *p* = .045, and when

health behaviours were assessed closer to the conclusion of an intervention, $B$ = -0.001, $S.E.$ = .0003, $CI$ = -.002 − -.0003, $p$ = .01 ($k$ = 21) (see OSM 2, section B for further consideration).

While no particular intervention type was related to significantly larger effect sizes for physical health outcomes, interventions were at their most effective when delivered in educational, $B$ = 0.19, $S.E.$ = 0.07, $CI$ = 0.48 − 0.32, $p$ = .01, and academic settings, $B$ = -0.17, $S.E.$ = 0.08, $CI$ = -0.35 − 0.06, $p$ = .043, as opposed to delivered in medical settings, $B$ = 0.009, $S.E.$ = 0.10, $CI$ = -0.19 − 0.21, $p$ = .919.

*Objective 3: Are larger effect sizes for PC associated with positive effect sizes for health outcomes?*

Initially, effect sizes for PC were unrelated to effect sizes for health behaviours $B$ = -0.21, $S.E.$ = 0.15, $CI$ = -0.54 − 0.12, $p$ = .212 ($k$ = 21). However, after the removal of a multivariate influential case (Magnan et al., 2014), medium-sized effects for PC, $g$ = -.43, were associated with a small, but positive, $g$ = .27, effect for health behaviours, $B$ = -0.28, $S.E.$ = 0.10, $CI$ = -0.50 − -0.07, $p$ = .012. Importantly, this effect was upheld in subsequent permutation tests with 10,000 random computations, $B$ = -0.28, $S.E.$ = 0.24, $CI$ = -0.75 − 0.19, $p$ = .019. Marginal associations between both worry and health behaviour, as well as between rumination and health behaviour were also revealed (see OSM 2, section B).

Effect sizes for PC were unrelated to effect sizes for physical health, $B$ = -0.18, $S.E.$ = 0.16, $CI$ = -0.52 − 0.15, $p$ = .264 ($k$ = 21), even after the removal of an influential case (Digdon & Koble, 2011), $B$ = -0.18, $S.E.$ = 0.10, $CI$ = -0.52 − 0.15, $p$ = .261. There were no significant associations between specific effect sizes for either worry or rumination and physical health outcomes (see Table 1).

## Discussion

The findings of this systematic review and meta-analysis revealed that interventions produce medium-sized effect sizes for worry and rumination and that these correspond to small, but positive, effect sizes for health behaviours (and small-medium positive effect sizes for sleep, see OSM 2). Interventions did not, however, produce significant differences for

physical health outcomes. Interventions produced significantly larger effect sizes for PC when interventions were delivered by healthcare professionals compared to all other alternative methods, and despite no intervention type producing larger effect sizes for PC (when directly compared against other types), there was evidence that studies incorporating psychological detachment style and PC action planning interventions generated significantly larger effect sizes for health behaviours.

This review provides the first meta-analytic evidence that a range of psychological interventions can be used to influence PC. Consistent with a previous narrative review (see, Querstret & Cropley, 2013), a broad variety of interventions encouraging participants to challenge their thinking style, or to disengage from the emotional response brought on by worry or rumination, can significantly decrease PC. Larger effect sizes were observed for rumination ($g$ = .58, $k$ = 8) than for worry, but worry was represented by far more studies and therefore subject to a wider variety of intervention types ($g$ = .41, $k$ = 19) and, promisingly, the majority of studies used the same well-validated measures (i.e., PSWQ; RRS) for these constructs. Further, the Querstret and Cropley review promoted the utility of CBT and mindfulness approaches, which was in line with our moderation analyses highlighting both approaches as useful strategies to mitigate against PC. Interestingly, however, in the current meta-analysis, no particular intervention type produced significantly larger PC effect sizes, but this is likely attributable to considerable heterogeneity belonging to the specific intervention content adopted by the studies. Therefore, despite the need for future research to understand the mechanisms of action in more detail, these findings show that these brief, inexpensive, and often self-administered interventions represent a useful safeguard against the harmful consequences brought on by worry and/or rumination.

The theoretical significance of the current findings are twofold as: a) they represent the first synthesis of experimental studies testing Brosschot et al.'s (2006) original PC hypothesis; and b) they document fresh evidence for the extension of the PC hypothesis to one that includes health behaviours, given that effect sizes for PC (following intervention) are positively associated with health behaviours, but not physical health outcomes. The original

PC hypothesis proposed that worry, rumination and related thought processes mediate the relationship between stress and disease as, when stressors are perseverated upon in thought, the damaging physiological activation associated with stress is also protracted, thus increasing susceptibility to stress-related ill-health (see, Brosschot et al., 2006, O'Connor et al., 2013). Therefore, the absence of effects for physical health outcomes in this review does not support the original PC hypothesis, though a number of contextual factors relating to this meta-analysis may account for these findings. First, the intervention content was delivered over a relatively short period (*M* = ~ 8 days) and very few of the studies reviewed here set out to improve physical health, with almost all studies listing their physical health outcome as a secondary measure (i.e., with the exception of the pain management studies). Second, as many interventions targeted determinants of behaviour, it would follow that they are more likely to produce larger effect sizes for health behaviours than in physical health outcomes; highlighting that the null effect observed for physical health may not be a reflection of PC failing to mediate the link between stress and physical disease, but rather that the intervention content was misaligned to significantly impact physical health. Third, it is notable that there was significantly more heterogeneity among physical health outcomes than for health behaviours, indicating that the observed intervention effects for physical health contained greater differences and more 'noise' among the data and, fourth, health behaviours were largely represented by a number sleep studies which yielded significant effects. It must therefore be noted that while the currently available evidence does not support the original PC hypothesis, such a conclusion may change; given the relationship between PC and physical health is theoretically viable, the effects were in the predicted direction, and potentially confounded by the aforementioned factors. Combined with the fact that previous published work drawing comparisons between PC and physical health is sparse, we are not ruling out that the effects for physical health outcomes may have been different with a greater number of studies and with interventions which more carefully targeted this particular facet of health. This does, however, highlight the need for future research to design carefully controlled studies with robust intervention arms to explicitly

investigate further the relationship between PC and subsequent improvements (or otherwise) in physical health outcomes.

However, the current findings do support the recent extension of the PC hypothesis to include health behaviours as an additional pathway to disease (see, Clancy et al., 2016; 2020). These findings are an important milestone for the extended PC hypothesis, and for the stress literature more generally as they show, for the first time across a range of studies, that effect sizes for PC following randomised experimental manipulations (taken, on average, 41 days after intervention exposure) are positively associated with health behaviours (taken, on average, at 143 days post-intervention). Further to what has been previously revealed in correlational tests by Clancy and colleagues – who first showed that the effects for health behaviours were most strongly associated with rumination (Clancy et al., 2016), before a second meta-analysis demonstrated that both types of PC were robustly associated with poorer sleep (Clancy et al., 2020) – here, using experimental evidence, we show that a more negative health behaviour profile (and sleep in particular) are related to larger effect sizes for the maladaptive characteristics of *both* worry and rumination. This is not only theoretically important, as this finding supports the view that worry and rumination, though separate and related constructs, are likely underpinned by related cognitive processes (as the same intervention content yielded the similar treatment effects), but also affords further clarity to healthcare professionals and other interventionists to help make more informed treatment choices in the knowledge that both constructs are sensitive to similar interventions. Therefore, given the prominence of PC in the aetiology of illness and disease, the interventions included in this review can be used to attenuate the impact of both worry and rumination on health behaviours.

Promisingly, the findings for PC were not exclusive to a particular population (age or gender), setting or participant format (group vs. individual), and did not vary across duration of delivery or the number of sessions (single session vs. multi-session); suggesting that similar results could be achieved through brief and long interventions as well as single and multi-session interventions. Effect sizes also did not vary for PC across time possibly

indicating the interventions might have a longer term impact on PC. However, despite our best efforts to identify and control for confounding, it is not possible to remove all sources and it must be remembered that the number of studies reviewed here was relatively small especially when accounting for potential confounds in multivariate analyses. Equally, although all but pain management and expressive writing intervention types yielded significant effect sizes for PC, no intervention type was found to outperform another by producing significantly larger effect sizes. However, significantly greater differences between intervention and comparator groups for health behaviours and sleep, were attributable to psychological detachment style interventions (see, OSM 2) and, for health behaviours in particular, PC action planning interventions were more effective than interventions not utilising this approach. Interpreting and understanding the impact of these interventions is a challenging task that is influenced by a range of moderators and factors that are difficult to explain. It is interesting, however, that the two most successful interventions yielding larger health behaviour effect sizes (psychological detachment & PC action planning) do share one common feature in that both place emphasis on the appraisal of metacognitions that urge the participant to discover internal goals and use environmental cues to either 'switch-off' or 'offset' their intrusive thoughts (e.g., Brosschot & van der Doef, 2006; Ebert et al., 2015).

A number of potential moderators were identified which may be helpful in identifying means to maximise intervention effects. For example, larger effect sizes for PC were found when interventions were delivered by healthcare professionals (for all results, see OSM 1 & 2). Overall, however, these findings are consistent with recent observations suggesting a range of study characteristics, beyond behaviour change techniques, can influence the magnitude of change in health contexts (Prestwich, Kenworthy & Conner, 2017) and thus should be carefully considered within prospective interventions targeting similar or related mechanisms of influence.

Surprisingly, few studies in this meta-analysis explicitly targeted rumination, which is notable given its long-standing role in the aetiology of adverse mental health conditions (see, Kraft, 2019; Mezulis, Priess & Hyde, 2011; O'Connor, O'Connor & Marshall, 2007; Nolen-

Hoeksema, 2000; Pugach, Campbell & Wisco, 2020; Thomsen et al., 2004). As a result, power issues were present in some of the rumination related analyses and should be therefore interpreted with caution (Cochrane, 2020). Indeed, an insufficient number of studies did not allow for a thorough exploration of the specific facets of rumination (e.g., positive vs. negative rumination, brooding vs. self-reflection, relationships with catastrophic thinking) that may be more likely to mediate the relationship between stress and ill health. Therefore, while the studies in this review are important and highlight the impact of rumination on subsequent health-related outcomes and behaviours, we strongly advocate future work exploring rumination.

We recognise that there are a number of limitations of the current meta-analysis. First, as with any meta-analysis, the effect sizes reported only represent estimates of the true effects. Second, the majority of measures for both PC and health outcomes were based on self-report methods. Although some work does exist documenting the impact of PC on objective measures of health (e.g., Teisman et al., 2014 & Versluis et al., 2018), this review highlights the pressing requirement for future interventions to incorporate more objective measures of health within their designs. Third, formal tests of mediation are required to further examine whether PC mediates the effects of interventions upon health behaviours. Fourth, studies were generally at unclear or high risk of bias (See, OSM 3 & 4). Although synthesising evidence across studies noted to have different sources of bias can be problematic, the risk of bias factors did not significantly moderate the effectiveness of any of the interventions on PC or health variables. Equally, it was reassuring that small study or publication bias had no impact on any study effect sizes. Fifth, although they did not meaningfully influence the main objectives there was some evidence for confounding across the assessed moderators (see OSM 2) and, sixth, this meta-analysis did not address all sources of heterogeneity contributing towards effect sizes despite testing a range of moderators. Future research is thus required to understand the mechanisms of action relating to the types of intervention content most likely to produce larger PC effects.

In conclusion, this systematic review and meta-analysis reveals interventions can produce medium-sized effect sizes for worry and rumination and that these correspond to small, but positive, effect sizes for health behaviours (and small-medium effect sizes for sleep) but not physical health. This casts new light on the original PC hypothesis and offers fresh support for its extension, placing greater emphasis on the role of health behaviours as an important mediating factor in the relationship between stress and disease.

**Conflict of interest**

The authors declare no conflict of interest.

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912 *Figure 1. PRISMA diagram for included studies.*

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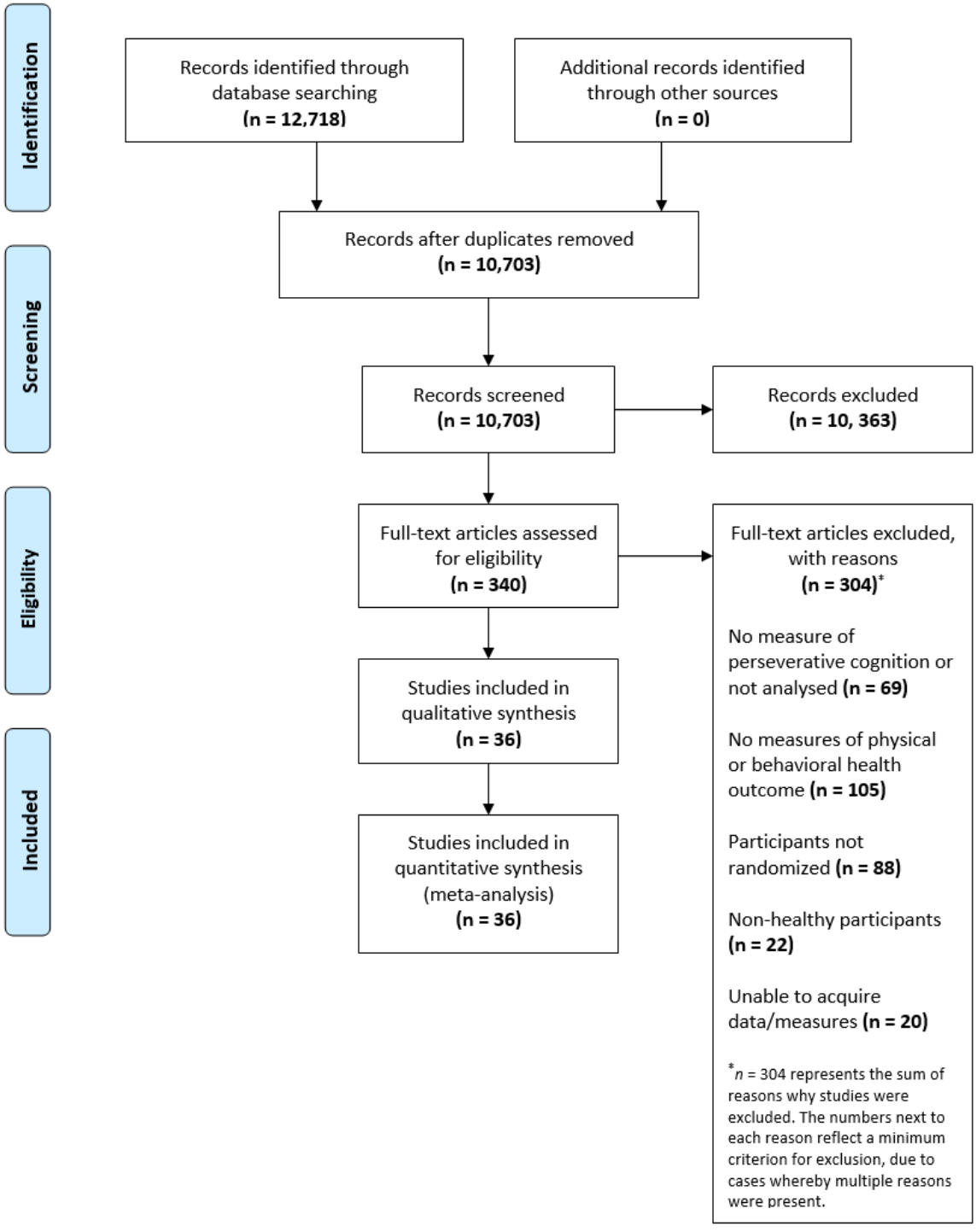

914 *Figure 2. Forest plot for PC.*

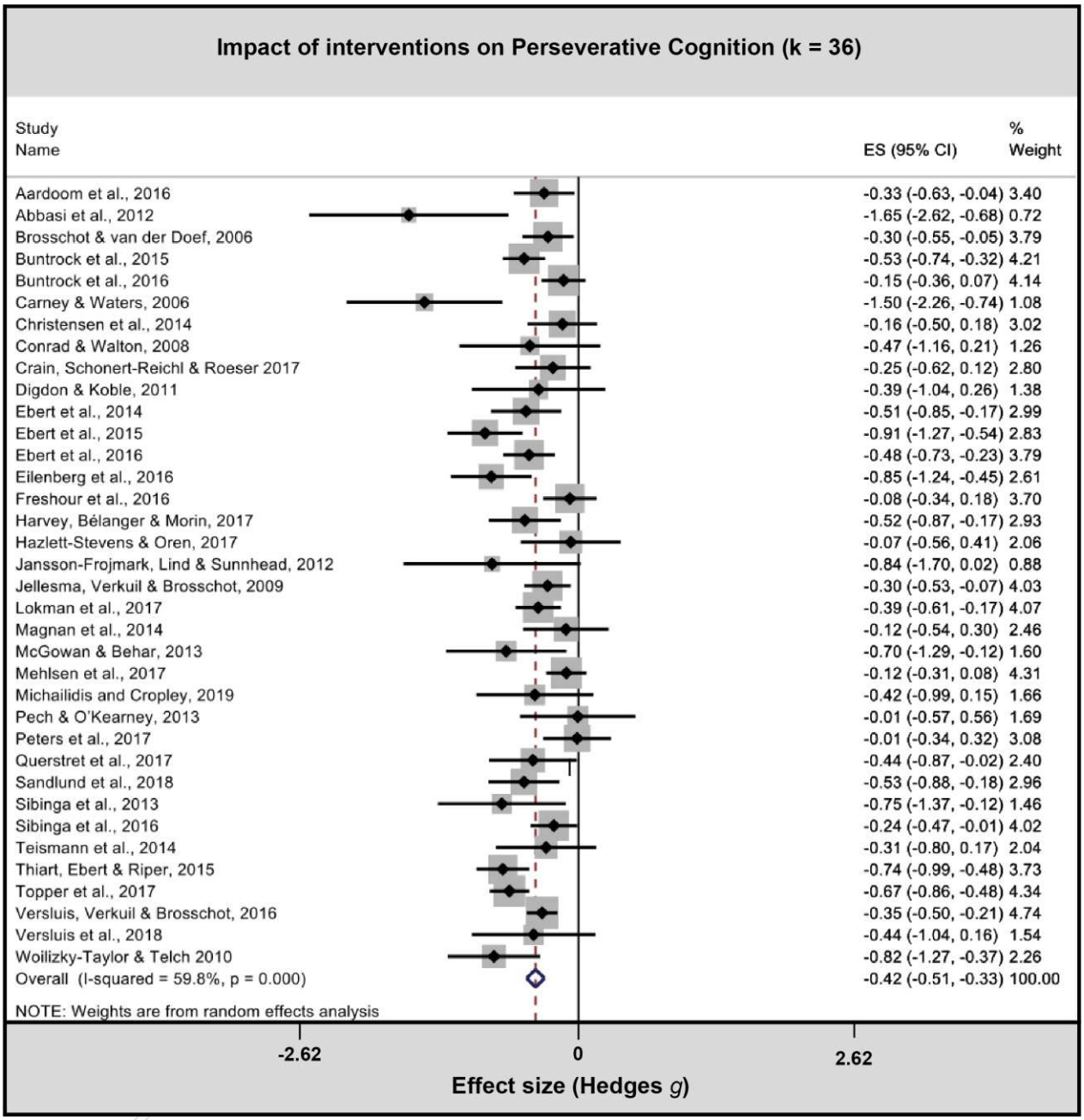


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*Figure 3. Forest plot for Health Behaviours.*

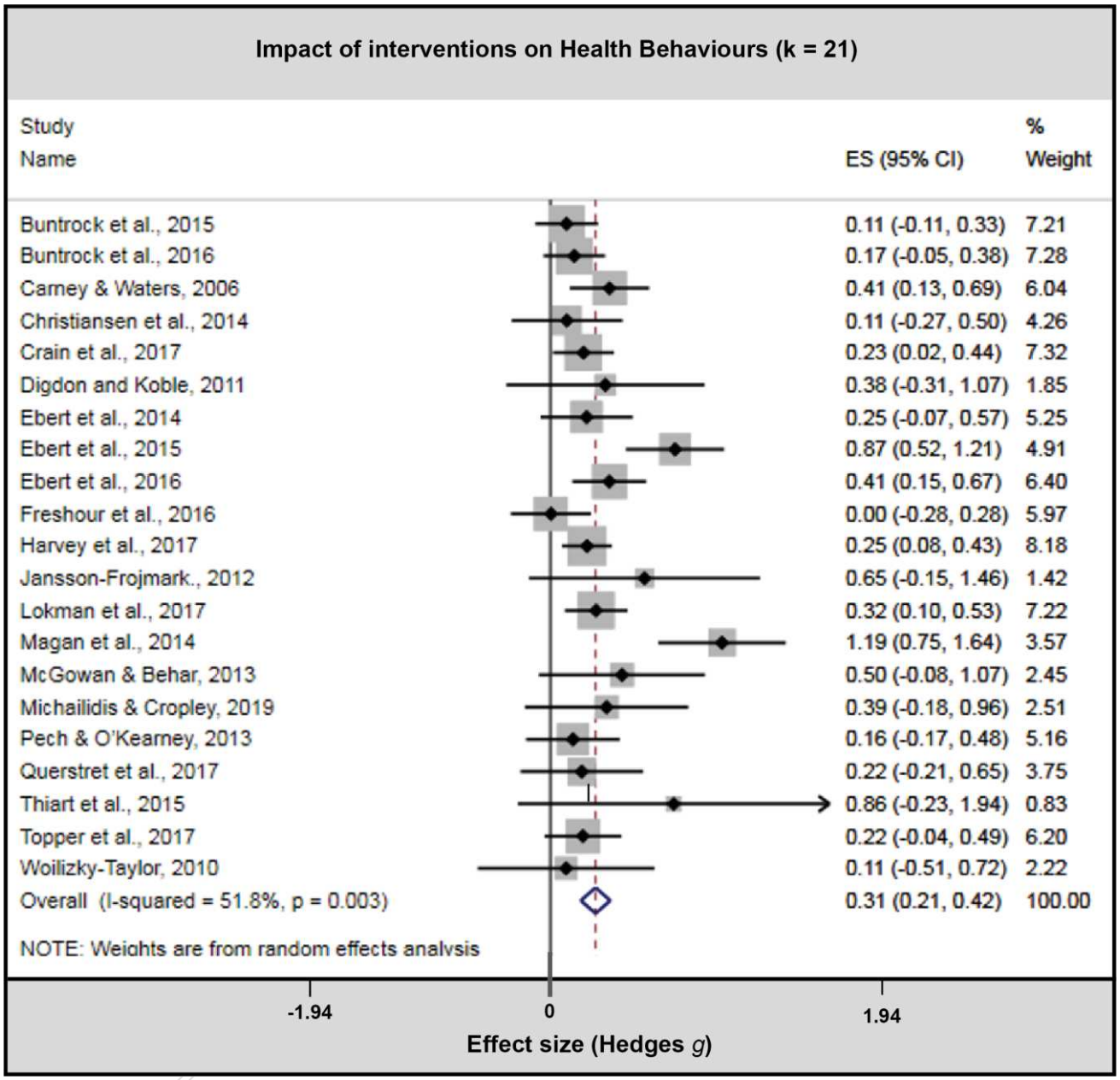

*Figure 4. Forest plot for Physical Health.*

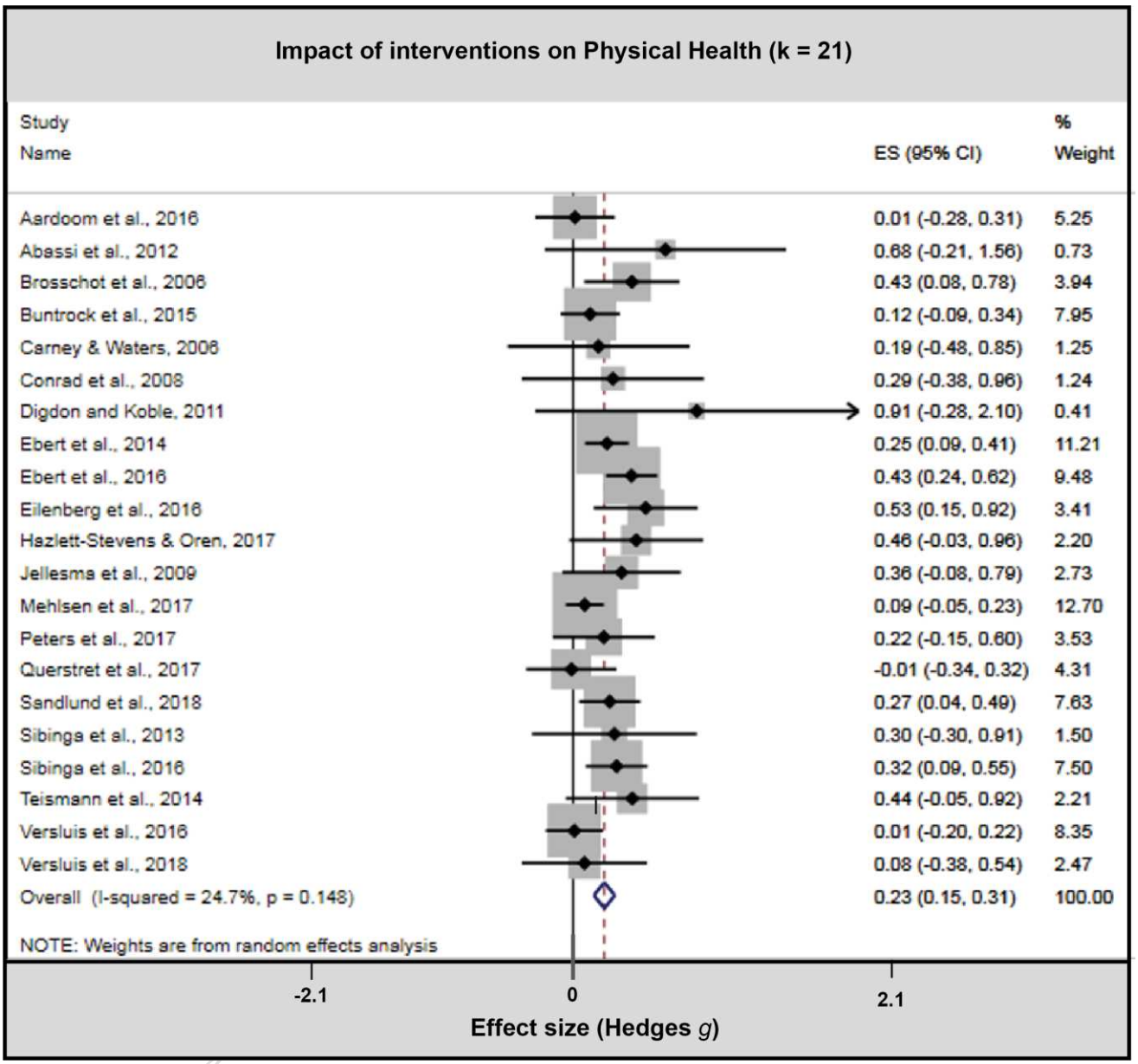

*Table 1. Associations between PC effect sizes and health outcome effect sizes.*

| Predictor | Outcome | Studies | *k* | Statistic | |
|---|---|---|---|---|---|
| | | | | *B* | *S.E* |
| PC | Health behaviours | Full | 36 | -.21 | .15 |
| | | Exc.outliers | 35 | -.28* | .10 |
| PC | Physical health | Full | 36 | -.18 | .16 |
| | | Exc.outliers | 35 | -.18 | .10 |
| PC | Sleep | Full | 17 | -.29* | .10 |
| | | Exc.outliers | 16 | -.19* | .11 |
| Worry | Health behaviours | Full | 14 | -.45† | .21 |
| Worry | Physical health | Full | 9 | -.35 | .61 |
| | | Exc.outliers | 8 | -.67 | .53 |
| Worry | Sleep | Full | 10 | -.76 | .28 |
| | | Exc.outliers | 9 | -.94** | .23 |
| Rumination | Health behaviours | Full | 5 | -.71† | .27 |
| Rumination | Physical health | Full | 4 | -27 | .36 |
| Rumination | Sleep | Full | 5 | -62 | .34 |

*Note:* $*p < .05$; $**p < .01$; ***; † = $p>.05$ -.08; PC = perseverative cognition; Exc. = exclude.

*The following pages comprise of Online Supplementary Material (OSM).*

## OSM 1: Information on Included Studies

*Table 1.* Overview of included studies (*k* = 36)

| Lead Author, year | Design | Location & Setting | Intervention features (*n* treatment sessions/ delivery across weeks. | PC & HO Measurement points (days after intervention exposure) | Type of PC (& measure) | Type of health outcome (& measure) | Participant characteristics | Pps included in analysis (*k*) & mean age (& *SD*) | % Female | Attrition (across entire study) |
|---|---|---|---|---|---|---|---|---|---|---|
| **Aardoom et al., 2016** | Randomized controlled trial | Netherlands, Educational | Stress management (8/8): Online based psychoeducation intervention. | PC: 56<br>HO: 91 | Perseverative thinking (PTQ) | Binge eating (EDE-Q). | Opportunity sample of adults with dietary concerns. | *k* = 178, *M* = 24.2 (*SD* = 7.7) | 98.9% | 63.3% |
| **Abbasi et al., 2012** | Randomized controlled trial | Israel, Medical | Pain management (7/7): In person spouse-assisted programme to alleviate back pain. | PC: 49<br>HO: 365 | Health Rumination (PCS - rumination subscale) | Physical pain (TSK; RDQ; VAS (1-10) of pain intensity for the week) | Referred to the GP with<br>lower back pain of greater than 6 months duration. | *k* = 21, *M* = 45 (*SD* = 10) | 87.88% | 10% |
| **Brosschot et al., 2006** | Cluster randomized controlled trial | Netherlands, Educational | PC action plans (6/<1): In person Diary based worry postponement. | PC: 7<br>HO: 7 | Worry (PSWQ & tally of daily worry). | Physical health complaints (SCH) | Volunteer sample of final grade high school students from 25 different schools. | *k* = 171, *M* = 16.7 *(range:* 15 – 19) | 81.4% | 29% |
| **Buntrock et al., 2015** | Randomized controlled trail | Germany, Academic | CBT (6/3): Online CBT to prevent relapse into depression | PC: 42<br>HO: 183 | Worry (PSWQ) | Insomnia severity (ISI) & functional impairment (SF-12v1) | Volunteer sample adults with minor depression. | *K* = 366, *M, =* 45 *(SD* = 11.9*)* | 73.9% | 19.9% |
| **Buntrock et al., 2016** | Randomized controlled trail | Germany, Academic | CBT (6/<1): Online CBT to prevent relapse into depression | PC:40<br>HO:365 | Worry (PSWQ) | Insomnia severity (ISI) | Volunteer sample of adults with minor depression. | *k* = 336, *M* = 45 *(SD* = 11.9*)* | 73.9% | Not reported. |
| **Carney & Waters, 2006** | Randomized controlled trail | USA, Academic | PC action plans (6/4): In person experimental | PC: 7 | Worry (PSWQ; WDQ; PSAS- | Sleep (SOL, TST, TWT). | University students with the presence of | *k* = 33, *M* = 20.97 (*SD* = 3) | 78.78% | 3.1% |

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| | | | pre-sleep constructive worry intervention. | HO: 7 | worry subscale). | | 3 or more nights per week of sleep onset difficulty. | | | |
| **Christiansen et al., 2014** | Randomized controlled trial | Australia, Academic | CBT (10/10): Online CBT programme to reduce anxiety. | PC: 77<br>HO: 183 | Worry (PSWQ) | Alcohol dependence (AUDIT) | GP referred with elevated anxiety. | *k* = 133, *M* = 25.7 (*SD* = 3.1) | 82.9% | 35% |
| **Conrad et al., 2008** | Randomized controlled trial | America, Medical | Mindfulness & Relaxation (12/12): In person applied relaxation to reduce worry. | PC: 7<br>HO: 7 | Worry (PSWQ) | Somatization (CSAI, somatic subscale) | Self-enrolled individuals with GAD. | *k* = 33, *M* = 44.6 (*SD* = 12.8) | 59% | 38% |
| **Crain et al., 2017** | Randomized controlled trial | Canada/USA; Academic | Mindfulness & Relaxation (11/8): In person group based mindfulness sessions. | PC: 91<br>HO: 152 | Job rumination (2 Likert scales, from teacher stress scale) | Sleep (Likert scales on sleep quality, sleep quantity & daytime sleepiness). | Self-enrolling public school teachers. | *k* = 113, *M* = 46.9 (*SD* = 9.2) | 89% | Not reported |
| **Digdon & Koble, 2011** | Randomized controlled trial | Canada, Academic | PC action plans (7/<1): Online constructive worry sessions to help with pre-sleep worry. | PC: 7<br>HO: 7 | Worry (daily sleep log; PSAS, worry subscale) | Sleep (SQS; sleep onset latency, sleep quantity and sleep quality) & somatic complaints (PSAS, somatic subscale). | Self-enrolled undergraduate students with pre-sleep worries. | *k* = 22, *M* = 23.22 (*SD* = 6.11) | 78.05% | 51.2% |
| **Ebert et al., 2014** | Randomized controlled trial | Germany, Educational | Stress management (5/7): Online based, virtual instructor lead, problem solving therapy. | PC: 49<br>HO: 183 | Worry (PSWQ) | Burnout (MBI-D) & physical health (SF-12-PCS subscale). | School teachers with minor depression. | *k* = 150, *M* = 47.1 (*SD* = 8.2) | 83.3% | 15.3% |

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| **Ebert et al., 2015** | Randomized controlled trial | Germany, Educational | Detachment (6/8): Online based recovery training on work related stress. | PC: 56<br>HO: 56 | Worry (PSWQ-PW) & work related rumination (CI, rumination subscale) | Sleep (PSQI, ISI, SSI & GSI). | School teachers experiencing poor sleep and low levels of detachment from work. | *k* = 100, *M* = 48.5 (*SD* = 9.9) | 74.2% | 31.17% |
| **Ebert et al., 2016** | Randomized controlled trial | Germany, Academic | Detachment (7/7): Online based, e-coach led, work detachment stress-management sessions. | PC: 49<br>HO: 183 | Worry (PSWQ-PW) | Sleep (ISI) & burnout (MBI· emotional exhaustion subscale) & physical health complaints (SF-12) | General population with elevated symptoms of stress. | *k* = 249, *M* = 42.9 (SD = 9.8) | 85.9% | 50.8% |
| **Eilenberg et al., 2016** | Randomized controlled trial | Denmark, Medical | CBT (9/9): In person ACT to help with health anxiety | PC: 304<br>HO: 304 | Illness worry (IWS) | Somatic symptoms (90-item Symptom Checklist & SCL - somatization subscale). | Opportunity sample of patients with health anxiety. | *k* = 107, *M* = 36.23 (*SD* = 8.75*)* | 67% | 6% |
| **Freshour et al., 2016** | Randomized controlled trial | USA, Medical | CBT (10/24): In person therapist led CBT reduce anxiety. | PC: 70<br>HO: 365 | Worry (PSWQ) | Patient health (PHQ-8) | Later-life individuals with GAD. | *k* = 224, *M* = 66.83 (*SD* = 6.38) | 54.57% | 12.5% |
| **Harvey et al., 2017** | Randomized controlled trial | USA, Medical | CBT (8/8): In person CBT for chronic insomnia. | PC: 56<br>HO:183 | Pre-sleep worry (APSQ) | Insomnia severity (ISI) and sleep diary (BTv, RTv,<br>TIB). | Self-referred individuals with moderate insomnia. | *k* = 128, *M* = 47.4 (*SD* = 12.6) | 62.23% | 7.5% |
| **Hazlett-Stevens & Oren, 2017** | Randomized controlled trial | USA, Academic | Mindfulness & relaxation (10/10): In person reflection and | PC: 70<br>HO: 70 | Worry (PSWQ) | Physical health (WHOQOLBREF, physical subscale). | Self-enrolled students seeking stress reduction. | *k* = 68, *M* = 22.1 (*SD* = 4.7) | 75% | 26.1% |

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| | | | mindfulness workshops. | | | | | | | |
| **Jansson-Frojmark et al., 2012** | Randomized controlled trial | Sweden, Academic | PC action plans (4/4): In person worry construction and behavioural therapy to aid with sleep. | PC: 7<br>HO: 14 | Pre-sleep worry (APSQ) | Insomnia severity (ISI) | Self-enrolled individuals with primary insomnia from local care centres. | *k* = 21, *M* = 56.5 (*SD* = 12.7) | 52.5% | 9.1% |
| **Jellesma et al., 2009** | Cluster randomized controlled trial | Netherlands, Educational | PC action plans (7<1): In person worry postponement to stop night time worriers.. | PC: 7<br>HO: 7 | Perseverative thoughts (CERQ-K, nightly tally) | Somatic complaints (SCL) | Children from grades 7 and 8 from seven primary schools. | *k* = 227, *M* = 11.4 (*SD* = .70) | 56.83% | 15.4% |
| **Lokman et al., 2017** | Randomized controlled trial | Netherlands, Educational | CBT (7/4): Online CBT self-help to improve sleep and wellbeing. | PC: 91.25<br>HO:91.25 | Worry (PSWQ) | Sleep quality (JSEQ) | Self-enrolled individuals with mild depressive symptoms. | *k* = 237, *M* = 43 (S*D* = 12.93) | 75.7% | 54.4% |
| **Magan et al., 2014** | Randomized controlled trial | USA, Academic | PC action plans (14/2): Online constructive plans on smoking-related consequences, negative thoughts and worry prevention. | PC: 14<br>HO: 14 | Worry (PSWQ, 2 Likert items on smoking worry) | Smoking addiction (FTND-R, and mean number of cigarettes smoked per week at baseline, compared to post-intervention) | Volunteer sample of university students who smoke on a daily basis. | *k* = 117, *M* = 29.6 (*SD* = 12.9) | 44.4% | Not reported. |
| **McGowan & Behar, 2013** | Randomized controlled trial | USA, Academic | PC action plans (14/2): In person focused worry postponement to reduce anxiety. | PC: 14<br>HO: 14 | Worry (PSWQ) | Insomnia severity (ISI) | Volunteer sample of university students/ are high trait worriers | *k* = 46, *M* = 19.9 (*SD* = 3.8) | 82.6% | 16.9% |

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| **Mehlsen et al., 2017** | Randomized controlled trial | Denmark, Medical | Pain management (6/6): In person, therapist led, chronic pain self-management programme to improve wellbeing. | PC: 63<br>HO: 152 | Illness worries (Whiteley-7) | Physical health symptoms (SCL) & bodily pain (RDQ, a 1-100 pain intensity VAS). | Individuals with chronic pain for longer than 3 months from 75 different hospitals. | *k* = 399, *M* = 54 (*SD* = 13.05) | 72% | 8% |
| **Michailidis and Cropley, 2019** | Randomized controlled trial | England, Academic | Expressive writing (3/<1): In person self-guided, expressive writing to reduce work-related rumination. | PC: 31<br>HO: 91 | Work-related rumination (WRRQ) | Sleep quality (ISI) | Full-time adult employees working in the UK from a wide range of occupations | *k* = 47, *M* = 34.22 (*SD* = 11.39) | 50% | 49% |
| **Pech & O'Kearney, 2013** | Randomized controlled trial | Australia, Academic | Stress management (5/6): In person problem solving therapy to reduce stress and improve sleep quality. | PC: 7<br>HO: 70 | Worry (PSWQ) | Sleep quality (PSQI) and insomnia severity (ISI). | Individuals with primary insomnia for longer than 3 months. | *k* = 47, *M* = 39.21 (*Range:* 18-60) | 62.8% | 14.9% |
| **Peters et al., 2017** | Randomized controlled trial | Netherlands/Belgium, Medical | CBT (8/8): Online CBT to reduce pain and intrusive thoughts. | PC: 65<br>HO: 65 | Perseverative thinking (PTQ) | Bodily pain (Likert 1-10 rating of pain intensity) | Volunteer sample of adults who had experienced musculoskeletal pain for longer than 3 months | *k* = 162, *M* = 48.6 (*SD* = 12) | 85% | 25.4% |
| **Querstret et al., 2017** | Randomized controlled trial | England, Educational | Mindfulness (10/4): Online instructor-led, mindfulness to | PC: 28<br>HO: 183 | Work-related rumination (WRRQ) | Sleep quality (PSQI) & | Self-enrolling working adults with elevated levels of work- | *k* = 87, *M* = 40.68 (*SD* = 10.45) | 80.5% | 25% |

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| | | | reduce work-related rumination/fatigue. | | | work-related fatigue (OFER, 2 subscales for chronic fatigue & acute fatigue) | related rumination | | | |
| **Sabinga et al., 2013** | Randomized controlled trial | USA, Educational | Mindfulness (12/12): In person, instructor led, mindfulness based stress reduction to improve sleep and reduce negative physical health. | PC: 84<br>HO: 84 | Rumination (AMR, mindfulness inventory, rumination subscale) | Sleep quality (nightly sleep diary, and via ACTigraph 24 h/day during the 1-week). | Self-enrolling 7th and 8th grade boys at urban middle school. | *k* = 41, M = 12.5 (*range* 11–14) | 0% (all male) | 2.38% |
| **Sabinga et al., 2016** | Cluster randomized controlled trial | USA, Educational | Mindfulness (12/12): In person, instructor led, mindfulness based stress reduction to improve physical health and reduce rumination. | PC: 84<br>HO: 84 | Rumination (CRSQ, rumination subscale) | Somatization symptoms (SCL) | Volunteer sample of 5th to 8th grade students in two public schools. | *k* = 300, *M* = 12 (unclear) | 50.7% | Unclear: between 25.2% and 27.2% |
| **Sandlund et al., 2018** | Randomized controlled trial | Sweden, Medical | CBT (6/10): In person, nurse-led CBT to improve daytime symptomology of insomnia. | PC: 70<br>HO: 70 | Pre-sleep worry (1-100 VAS) | Sleep quality (USI, ISI) | Volunteer Individuals with primary insomnia. | *k* = 132, *M* = 54 (*SD* = 16) | 72.7% | 20% |
| **Teismann et al., 2014** | Randomized controlled trial | Germany, Academic | Expressive writing (3/<1): In person, diary based, self-guided positive writing about | PC: 3<br>HO: 3 | Perseverative thinking (PTQ) | Cortisol awakening response (CAR) | Volunteer sample of general population. | *k* = 64, *M* = 29.1 (*SD* = 8.42) | 62.5% | 0% (4 sets of missing data were excluded) |

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| | | | personal life goals | | | | | | | |
| **Thiart et al., 2015** | Randomized controlled trial | Germany, Academic | CBT (6/8): Online, mixed intervention based on CBT principles to improve wellbeing and sleep quality. | PC: 56<br>HO: 182 | Worry (PSWQ) & work-related rumination (IS, cognitive irritation subscale) | Insomnia severity (ISI) & recuperation in sleep (SF-AR) | Volunteer sample of school teachers with sleep complaints. | *k* = 118, M = 48 (*SD* = 9.9) | 74.2% | 7.2% |
| **Topper et al., 2017** | Randomized controlled trial | Netherlands, Academic | CBT (6/6): Online, group based, CBT to prevent anxiety and depression | PC: 56<br>HO: 365 | Worry (PSWQ), rumination (RRS) & perseverative thinking (PTQ) | Alcohol consumption (QDS) & dietary screening (EDI-2-BU) | Self-enrolled high school children from final three grades in 13 schools. | *k* = 150, *M* = 17.43 (*SD* = 2.09) | 83.7% | 17% |
| **Versluis et al., 2016** | Randomized controlled trial | Netherlands, Academic | PC action plans (6/<1): Online, worry postponement to reduce health complaints. | PC: 6<br>HO 6 | Worry (nightly diary for duration and frequency) | Subjective health complaints (SHC) | Volunteer sample of general population. | *k* = 351, *M* = 36.36 (*SD* = 12.97) | 84.76% | 64% |
| **Versluis et al., 2018** | Randomized controlled trial | Netherlands, Academic | PC action plans (26/4): Smartphone-based, self-guided, worry-reduction training for stress reduction and emotion regulation. | PC: 14<br>HO: 27 | Worry (PSWQ & nightly diary recording of: duration, frequency, severity) | Cardiac activity (ambulatory measured continuously for the three test days via an ekgMove sensor). | Volunteer sample of adults who reported elevated levels of work-based stress | *k* = 79, *M* = 43.60 (*SD* = 11.39) | 74% | 8% |
| **Woilizky-Taylor et al., 2010** | Randomized controlled trial | USA, Academic | Mindfulness and relaxation (12/4): In person, pulsed audio-photic | PC: 12<br>HO: 12 | Worry (PSWQ & AQW) | General health (visits to health centres in the past semester). | Self-enrolled sample of university students concerned about assessments. | *k* = 41,(not reported, undergraduate university students) | 75.2% | 40.7% |

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| | stimulation for relaxation to reduce worry. | | |

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978 *Table 2.* Sub-group analyses between intervention types and PC and health outcome
979 variables.

| Intervention type© | Outcome | Test Statistic | | |
|---|---|---|---|---|
| | | *Hedges g* | *Z* | *p* |
| Pain management | PC (*k* = 2) | -.807 | 1.06 | .290 |
| | Worry (*k* = 0) | - | - | - |
| | Rumination (*k* = 1) | -1.65 | 3.34 | .001** |
| | HO (*k* = 2) | 0.213 | 0.87 | .382 |
| | HB (*k* = 0) | - | - | - |
| | PHO (*k* = 2) | 0.283 | 0.87 | 0.382 |
| | Sleep (*k* = 0) | - | - | - |
| PC action plans | PC (*k* = 9) | -0.396 | 4.89 | .001*** |
| | Worry (*k* = 5) | -0.360 | 5.86 | .001*** |
| | Rumination (*k* = 0) | - | - | - |
| | HO (*k* = 9) | 0.422 | 3.41 | .001** |
| | HB (*k* = 4) | 0.635 | 3.59 | .001*** |
| | PHO (*k* = 6) | 0.203 | 2.01 | .044* |
| | Sleep (*k* = 4) | 0.440 | 3.84 | .001** |
| Stress management | PC (*k* = 4) | -0.264 | 2.78 | .005** |
| | Worry (*k* = 3) | -0.242 | 1.74 | .081 |
| | Rumination (*k* = 0) | - | - | - |
| | HO (*k* = 4) | 0.190 | 3.56 | .001** |
| | HB (*k* = 3) | 0.184 | 2.31 | .021* |
| | PHO (*k* = 2) | 0.165 | 1.46 | .145 |
| | Sleep (*k* = 2) | 0.163 | 1.78 | .075 |
| Mindfulness/relaxation | PC (*k* = 7) | -0.382 | 3.94 | .001*** |
| | Worry (*k* = 3) | -0.462 | 1.89 | .059 |
| | Rumination (*k* = 4) | -0.310 | 3.59 | .001*** |
| | HO (*k* = 7) | 0.246 | 4.33 | .001*** |
| | HB (*k* = 3) | 0.217 | 2.34 | .019* |
| | PHO (*k* = 5) | 0.252 | 3.02 | .003** |
| | Sleep (*k* = 3) | 0.214 | 2.60 | .009** |
| Psychological detachment | PC (*k* = 2) | -0.673 | 3.15 | .002** |
| | Worry (*k* = 2) | -0.552 | 4.90 | .001*** |
| | Rumination (*k* = 1) | -1.100 | 5.16 | .001*** |
| | HO (*k* = 2 ) | 0.617 | 2.81 | .005** |
| | HB (*k* = 2) | 0.623 | 2.73 | .006** |
| | PHO (*k* = 1) | 0.429 | 4.47 | .001*** |
| | Sleep (*k* = 2) | 0.623 | 2.73 | .006** |
| CBT/ACT | PC (*k* = 10) | -0.450 | 5.39 | .001*** |
| | Worry (*k* = 6) | -0.432 | 4.09 | .001*** |
| | Rumination (*k* = 1) | -0.594 | 3.51 | .001*** |
| | HO (*k* = 10) | 0.216 | 6.31 | .001*** |
| | HB (*k* = 7) | 0.202 | 4.16 | .001*** |
| | PHO (*k* = 4) | 0.245 | 3.29 | .001** |
| | Sleep (*k* = 5) | 0.201 | 3.02 | .003** |
| Expressive writing | PC (*k* = 2) | -0.361 | 1.91 | .056 |
| | Worry (*k* = 0) | - | - | - |

| | | | |
|---|---|---|---|
| Rumination (*k* = 1) | -0.424 | 4.37 | .145 |
| HO (*k* = 2) | 0.416 | 2.20 | .028* |
| HB (*k* = 1) | 0.390 | 1.34 | .179 |
| PHO (*k* = 1) | 0.435 | 1.74 | .082 |
| Sleep (*k* = 1) | 0.390 | 1.34 | .179 |

Note: *$p$ =.05; **$p$<.05; ***$p$<.001; ◎ = the categorical predictors for these analyses are set as 1 (type present) and 0 (type not present);CBT/ACT = cognitive behavioural/acceptance and commitment style therapies; PC = perseverative cognition; HO = health outcomes (overall); HB = health behaviours; PHO = physical health outcomes; *Hedges g* statistic = effect size estimate; *Z* statistic = the distribution under the null hypothesis that can be approximated by a normal distribution (accompanied by a significance test, p).

1003 *Table 3.* Associations between intervention types and study outcome effect sizes.

| Intervention type© | Outcome | Test Statistic | | | Heterogeneity |
|---|---|---|---|---|---|
| | | *B* | *SE* | *p* | $I^2$ |
| Pain management (k = 2) | PC (*k* = 36) | .084 | .22 | .71 | 58.61 |
| | Worry (*k* = 19) | - | - | - | - |
| | Rumination (*k* = 8) | -1.14 | .56 | .09 | 60.98 |
| | HO (*k* = 36) | -.145 | .15 | .35 | 45.58 |
| | HB (*k* = 21) | - | - | - | - |
| | PHO (*k* = 21) | -.125 | .12 | .32 | 18.12 |
| | Sleep (*k* = 17) | - | - | - | - |
| PC action plans (k = 9) | PC (*k* = 36) | -.004 | .12 | .97 | 60.81 |
| | Worry (*k* = 19) | .029 | .13 | .83 | 50.38 |
| | Rumination (*k* = 8) | - | - | - | - |
| | HO (*k* = 36) | .123 | .09 | .19 | 48.50 |
| | HB (*k* = 21) | .366 | .14 | .02** | 39.12 |
| | PHO (*k* = 21) | -.047 | .11 | .66 | 26.78 |
| | Sleep (*k* = 17) | .188 | .15 | .26 | 31.60 |
| Stress management (k = 4) | PC (*k* = 36) | .171 | .15 | .25 | 59.27 |
| | Worry (*k* = 19) | .199 | .15 | .19 | 44.29 |
| | Rumination (*k* = 8) | - | - | - | - |
| | HO (*k* = 36) | -.135 | .11 | .19 | 48.44 |
| | HB (*k* = 21) | -.156 | .15 | .32 | 52.05 |
| | PHO (*k* = 21) | -.082 | .12 | .51 | 28.17 |
| | Sleep (*k* = 17) | -.144 | .14 | .31 | 34.30 |
| Mindfulness/relaxation (k = 7) | PC (*k* = 36) | .027 | .13 | .84 | 60.86 |
| | Worry (*k* = 19) | -.066 | .19 | .74 | 50.28 |
| | Rumination (*k* = 8) | .456 | .22 | .09 | 44.42 |
| | HO (*k* = 36) | -.034 | .10 | .73 | 49.59 |
| | HB (*k* = 21) | -.132 | .17 | .45 | 53.56 |
| | PHO (*k* = 21) | .012 | .11 | .86 | 27.90 |
| | Sleep (*k* = 17) | -.094 | .13 | .47 | 37.14 |
| Psychological detachment (k = 2) | PC (*k* = 36) | -.263 | .19 | .18 | 58.63 |
| | Worry (*k* = 19) | -.188 | .17 | .29 | 47.01 |
| | Rumination (*k* = 8) | -.652 | .33 | .09 | 47.92 |
| | HO (*k* = 36) | .304 | .18 | .01** | 36.63 |
| | HB (*k* = 21) | .332 | .16 | .05* | 42.19 |
| | PHO (*k* = 21) | .225 | .13 | .09 | 9.83 |
| | Sleep (*k* = 17) | .346 | .11 | .01*** | 3.21 |
| CBT/ACT (k = 10) | PC (*k* = 36) | -.051 | .11 | .63 | 58.35 |
| | Worry (*k* = 19) | -.031 | .11 | .79 | 49.97 |
| | Rumination (*k* = 8) | -.0002 | .44 | .99 | 70.81 |
| | HO (*k* = 36) | -.071 | .08 | .38 | 48.78 |
| | HB (*k* = 21) | -.195 | .11 | .09 | 47.57 |
| | PHO (*k* = 21) | .029 | .11 | .78 | 28.05 |
| | Sleep (*k* = 17) | -.134 | .10 | .19 | 31.73 |
| Expressive writing (*k* = 2) | PC (*k* = 36) | -.056 | .26 | .83 | 60.98 |
| | Worry (*k* = 19) | - | - | - | - |

| | | | | |
|---|---|---|---|---|
| Rumination (*k* = 8) | .19 | .50 | .71 | 71.57 |
| HO (*k* = 36) | .14 | .23 | .53 | 49.1 |
| HB (*k* = 21) | .075 | .37 | .84 | 54.06 |
| PHO (*k* = 21) | .208 | .27 | .45 | 26.32 |
| Sleep (*k* = 17) | .108 | .33 | .74 | 38.08 |

Note: **p* =.05; ***p*<.05; ****p*<.001; ◎ = the categorical predictors for these analyses are set as 1 (type present) and 0 (type not present);CBT/ACT = cognitive behavioural/acceptance and commitment style therapies; PC = perseverative cognition; HO = health outcomes (overall); HB = health behaviours; PHO = physical health outcomes; *B* statistic = standardized beta (accompanied by standard error, *S.E* and significance test, p); $I^2$ statistic = percentage of residual variation due to heterogeneity.

*Table 4.* Association between effect sizes and study characteristics

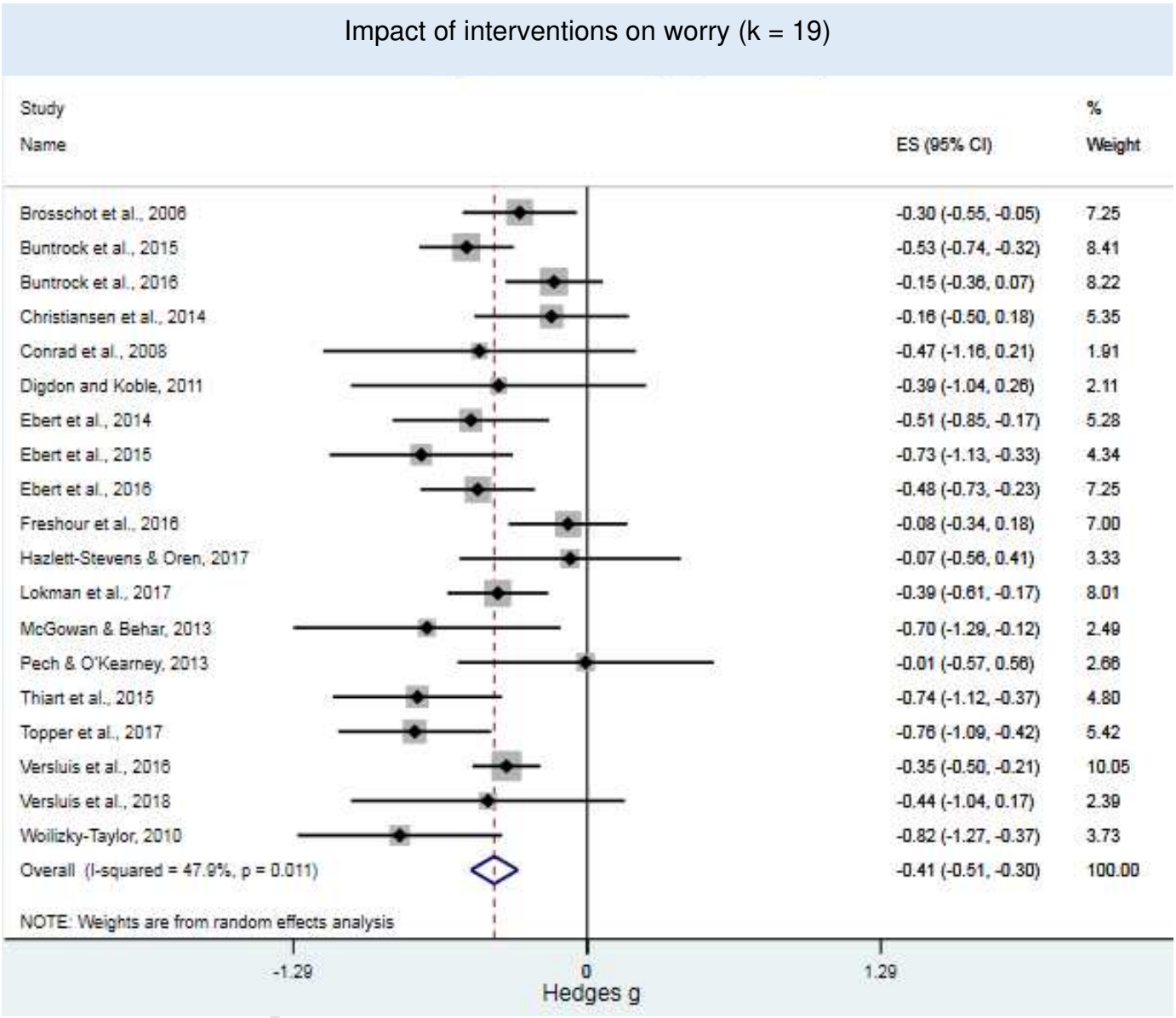
| Outcome | Predictor© | Test Statistic | |
|---|---|---|---|
| | | *B* | *S.E.* |
| PC | Age | -.003 | .003 |
| (*k* = 36) | Sleep disturbance | -.26 | .09 |
| | GAD participants | -.21 | .18 |
| | % of participants female | -.0002 | .002 |
| | Adult's vs children | -.005 | .13 |
| | Measure time-point | -.0008 | .001 |
| | Number of sessions | -.15 | .01 |
| | ITT analyses | -.19 | .02 |
| | Mode of delivery | | |
| | Health-care professional | -.39* | .18 |
| | Self-administered | -.02 | .10 |
| | Self-administered with support | .08 | .11 |
| | Trained facilitator | .10 | .12 |
| | Intervention setting | | |
| | Medical | .06 | .12 |
| | Educational | -.04 | .12 |
| | Academic | -.01 | .10 |
| | Hosted online vs In person | -.06 | .10 |
| | Active vs non-active control | .05 | .10 |
| | Individual vs group delivery | -.002 | .11 |
| | Student sample | -.14 | .12 |
| | Attrition | .002 | .003 |
| Worry | Age | .002 | .004 |
| (*k* = 19) | Sleep disturbance | -.23 | .16 |
| | GAD participants | -.14 | .11 |
| | % of participants female | -.01 | .06 |
| | Adult's vs children | .10 | .17 |
| | Measure time-point | .0007 | .002 |
| | Number of sessions | -.0002 | .15 |
| | ITT analyses | -.09 | .11 |
| | Mode of delivery | | |
| | Health-care professional | — | — |
| | Self-administered | -.02 | .10 |
| | Self-administered with support | .15 | .12 |
| | Trained facilitator | .02 | .16 |
| | Intervention setting | | |
| | Medical | .27 | .18 |
| | Educational | -.09 | .13 |
| | Academic | -.02 | .12 |
| | Hosted online vs In person | -.14 | .12 |
| | Active vs non-active control | -.16 | .11 |
| | Individual vs group delivery | .07 | .12 |
| | Student sample | -.35* | .14 |
| | Attrition | .07 | .12 |
| Rumination | Age | .003 | .01 |
| (*k* = 8) | Sleep disturbance | -.12 | .02 |
| | GAD participants | — | — |

| | | | | |
|---|---|---|---|---|
| | | % of participants female | -.0009 | .01 |
| | | Adult's vs children | -.18 | .30 |
| | | Measure time-point | .006 | .006 |
| | | Number of sessions | .05 | .04 |
| | | ITT analyses | -.30 | .33 |
| | | Mode of delivery | | |
| | | Health-care professional | -1.14† | .56 |
| | | Self-administered | -.32 | .33 |
| | | Self-administered with support | .17 | .46 |
| | | Trained facilitator | .38 | .26 |
| | | Intervention setting | | |
| | | Medical | -1.14† | .56 |
| | | Educational | -.10 | .32 |
| | | Academic | .34 | .28 |
| | | Hosted online vs In person | -.23 | .29 |
| | | Active vs non-active control | .26 | .30 |
| | | Individual vs group delivery | -.08 | .31 |
| | | Student sample | .18 | .30 |
| | | Attrition | .009 | .001 |
| | | | | |
| | HO (*k* = 36) | Age | -.002 | .002 |
| | | Sleep disturbance | -.14 | .17 |
| | | GAD participants | -.12 | .09 |
| | | % of participants female | -.002 | .002 |
| | | Adult's vs children | -.02 | .10 |
| | | Measure time-point | -.0005 | .0003 |
| | | Number of sessions | -.0007 | .01 |
| | | ITT analyses | -.19 | .08 |
| | | Mode of delivery | | |
| | | Health-care professional | .12 | .15 |
| | | Self-administered | .18* | .07 |
| | | Self-administered with support | -.14† | .08 |
| | | Trained facilitator | -.11 | .08 |
| | | Intervention setting | | |
| | | Medical | -.08 | .08 |
| | | Educational | .14 | .08 |
| | | Academic | -.05 | .07 |
| | | Hosted online vs In person | .001 | .07 |
| | | Active vs non-active control | -.02 | .07 |
| | | Individual vs group delivery | -.16 | .07 |
| | | Student sample | .07 | .09 |
| | | Attrition | -.002 | .002 |
| | | | | |
| | HB (*k* = 21) | Age | -.004 | .004 |
| | | Sleep disturbance | -.13 | .11 |
| | | GAD participants | -.11 | .12 |
| | | % of participants female | -.005 | .004 |
| | | Adult's vs children | .10 | .25 |
| | | Measure time-point | -.001** | .0003 |
| | | Number of sessions | .02 | .02 |
| | | ITT analyses | .05 | .12 |
| | | Mode of delivery | | |
| | | Health-care professional | — | — |
| | | Self-administered | .26* | .09 |
| | | Self-administered with support | -.17 | .12 |

| | | | |
|---|---|---|---|
| | Trained facilitator | -.15 | .14 |
| | Intervention setting | | |
| | Medical | -.20 | .18 |
| | Educational | .22 | .15 |
| | Academic | -.02 | .13 |
| | Hosted online vs In person | .10 | .12 |
| | Active vs non-active control | -.08 | .12 |
| | Individual vs group delivery | -.25* | .12 |
| | Student sample | -.19 | .13 |
| | Attrition | .002 | .004 |
| | | | |
| PHO (*k* = 21) | Age | -.002 | .003 |
| | Sleep disturbance | .009 | .006 |
| | GAD participants | .002 | .003 |
| | % participants female | -.001 | .001 |
| | Adult's vs children | -.15 | .11 |
| | Measure time-point | .0003 | .005 |
| | Number of sessions | -.002 | .001 |
| | ITT analyses | -.02 | .001 |
| | Mode of delivery | | |
| | Health-care professional | .16 | .13 |
| | Self-administered | .09 | .09 |
| | Self-administered with support | -.14 | .09 |
| | Trained facilitator | -.05 | .10 |
| | Intervention setting | | |
| | Medical | .01 | .10 |
| | Educational | .19** | .07 |
| | Academic | -.17* | .08 |
| | Hosted online vs In person | -.13 | .09 |
| | Active vs non-active control | .04 | .09 |
| | Individual vs group delivery | .002 | .09 |
| | Student sample | -.01 | .12 |
| | Attrition | -.004 | .002 |

*Note:* *$p < .05$; **$p < .01$; *** $p < .001$; † = $p > .05$-.09; — = dropped due to collinearity issues; ◎= the categorical predictors for these analyses are set as 1 (feature present) and 0 (feature not present); PC = perseverative cognition; HO: health outcomes (health behaviours and physical health outcomes combined); HB: health behaviours; PHO: physical health outcomes, Clin vs non-clin: whether participants derived of a clinical or . background; M time-point: point in time at which measures were taken; N sessions: number of sessions participants were exposed too; ITT analyses: whether the results influenced intention-to-treat analysis

1192 *Figure 1.* Worry forest plot.

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1194

*Figure 2*. Rumination forest plot.

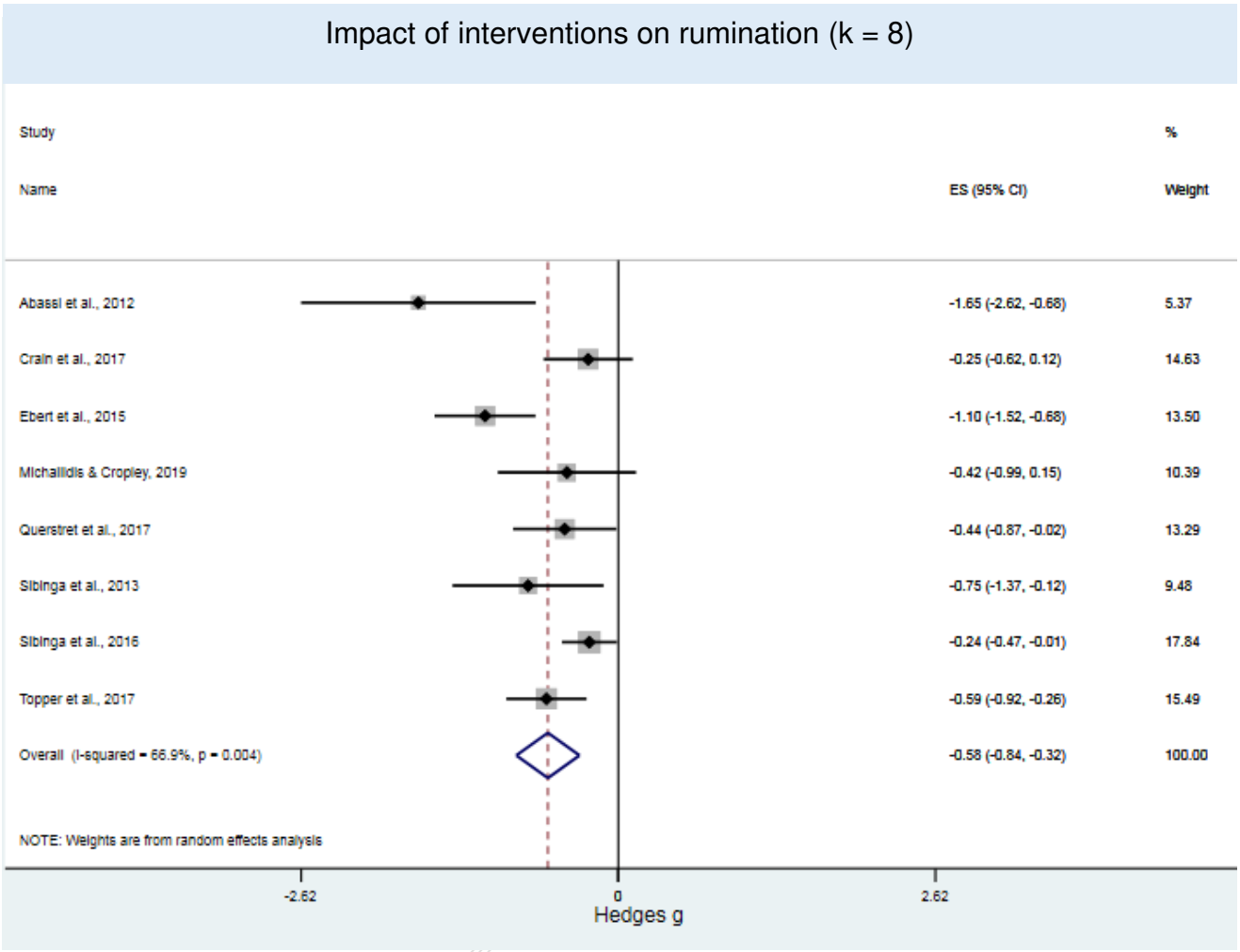


*Figure 3.* Funnel plot for health outcomes.

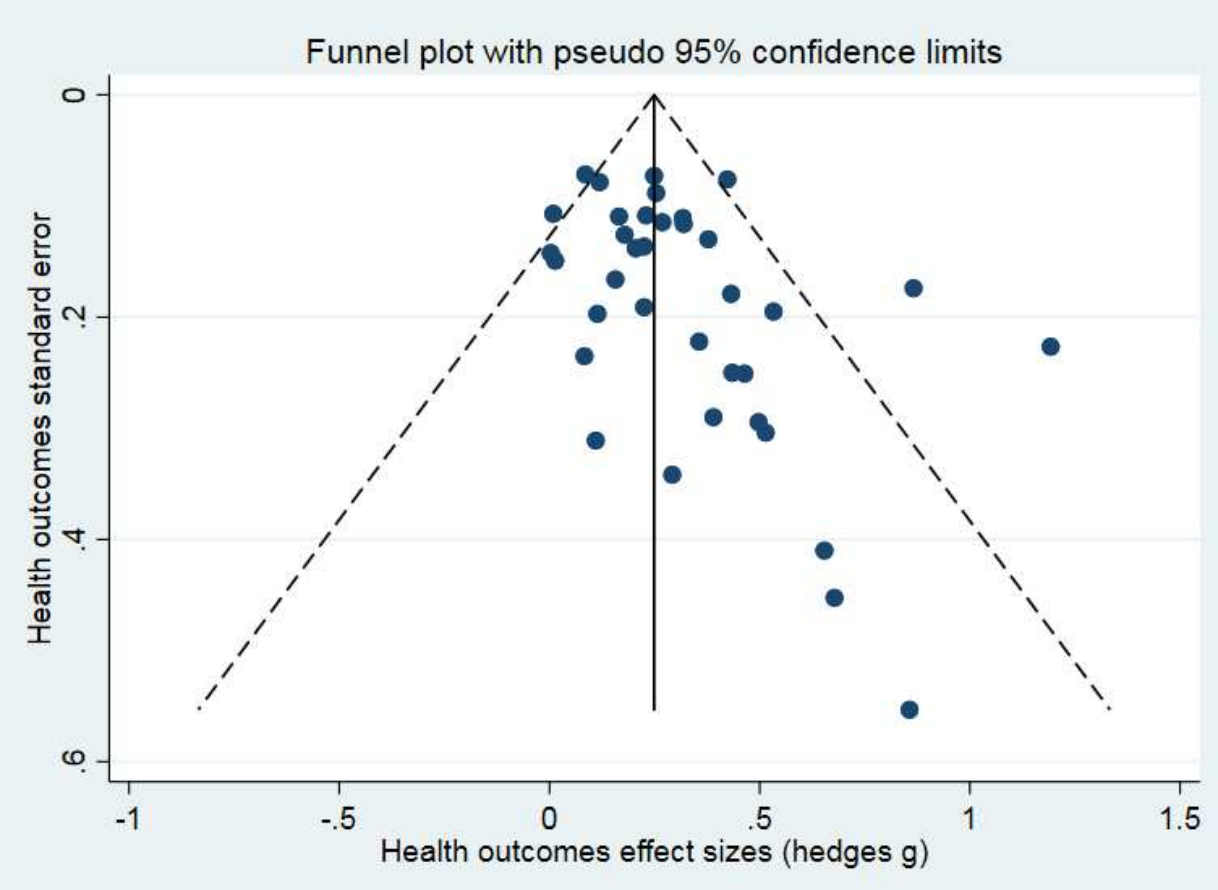
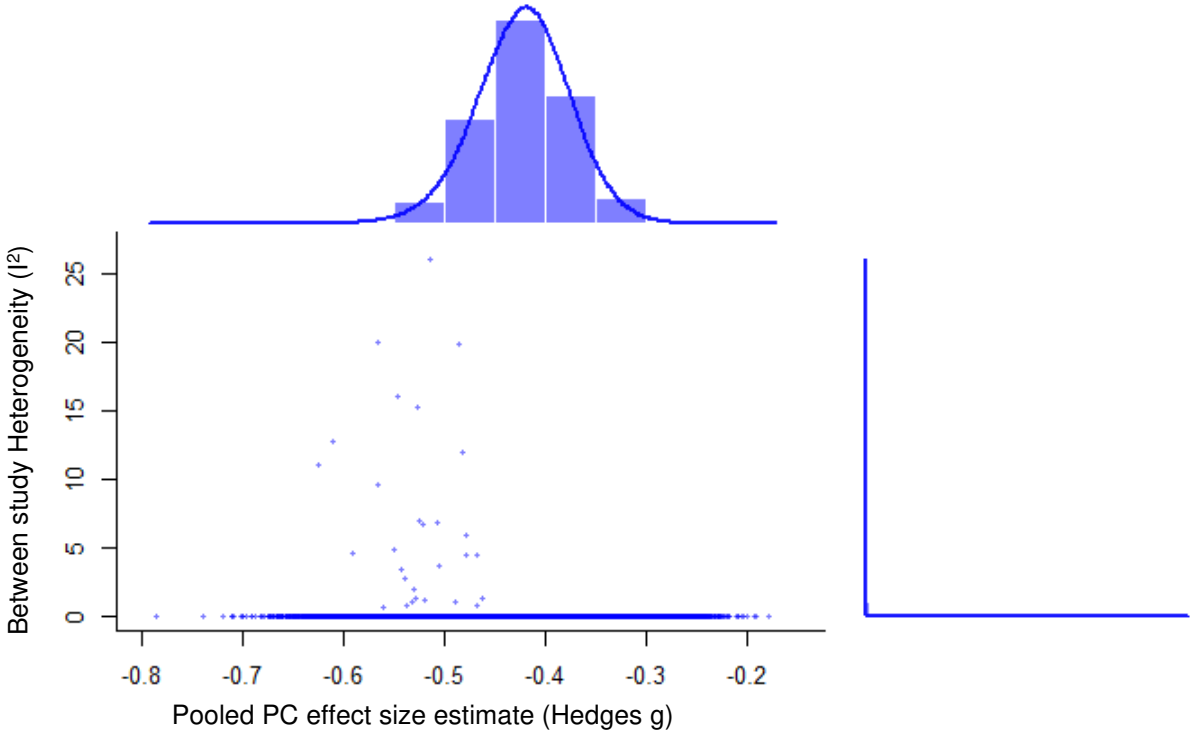


*Figure 4*. Graphical Display of Heterogeneity (GOSH) plot with PC effect sizes as a function of between-study heterogeneity across all studies ($k = 36$).



*Search Terms*

| | |
|---|---|
| 1 | Rumination.mp. |
| 2 | (Ruminat* and (thought* or thinking)).mp. |
| 3 | (perseverative and (thought* or thinking or cognition*)).mp. |
| 4 | (Repetitive and (thought* or thinking)).mp. |
| 5 | (Intrusive and (thought* or thinking)).mp |
| 6 | worr*.mp. |
| 7 | (Stress* and (thought* or thinking)).mp |
| 8 | (Self referential and (thought* or thinking)).mp. |
| 9 | brooding.mp. |
| 10 | reflection.mp. |
| 11 | (obsessive and (thought* or thinking)).mp |
| 12 | unconscious stress*.mp. |
| 13 | implicit stress*.mp. |
| 14 | anticipat* stress*.mp. |
| 15 | cognitive intrusion*.mp. |
| 16 | 1 or 2 or 3 or 4 or 5 or 6 or 7 or 8 or 9 or 11 or 12 or 13 or 14 or 15 |
| 17 | intervention*.tw. |
| 18 | random*.tw. |
| 19 | 17 or 18 |
| 20 | 16 and 19 |
| 21 | limit 20 to (English language and human) |

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## OSM 2: Supplementary Results Section: Additional exploratory analyses and robustness checks

A range of additional analyses were conducted to: (a) check data met the statistical assumptions associated with regression such as multivariate normality, low multicollinearity, lack of auto-correlation and homoscedasticity; (b) identify potential confounds that may have affected the conclusions and consider the results when the behavioural and physical health outcomes were combined as an *overall* health index; (c) assess the possible impact of two studies for which we had concerns regarding the measures of behaviour; assess the robustness of the findings when focused only on studies (d) measuring PC *immediately* post-intervention and then health at a *later* point in time and (e) measured sleep; (f) check for small-study bias; (g) assess, when an alternative study arm was available (i.e., two treatment arms/different control types), if our approach to arm selection significantly altered study effect sizes for both PC and health; h) control for the possibility that baseline between group differences influenced effect sizes; i) detect if clinical heterogeneity influenced effect sizes.

### ***A. Statistical assumptions***

Visual inspection (i.e. radial & QQ plots) and formal tests (i.e. Cook's distance, DFBETAS) were conducted to ensure data met the statistical assumptions associated with regression such as multivariate normality, low multicollinearity, lack of auto-correlation and homoscedasticity. To identify potential patterns of effect sizes and heterogeneity in our data Graphic Display of Heterogeneity (GOSH) plots (Olkin, Dahabreh, and Trikalinos 2012) were computed. This function fits the same random effects meta-analysis model to all possible subsets of included studies meaning not only $K^{-1}$ models are fitted, but all $2^{k-1}$ possible study combinations. Further, as an extra safeguard against detecting false-positives the Hartung-Knapp-Sidik-Jonkman (HKSJ, see Hartung & Knapp, 2001a) method was used to calculate effect sizes across all primary analyses when between study heterogeneity was statistically significant (in addition to Hedges' *g*).

Throughout, all appropriate statistical assumptions and graphical checks were met across these tests and no assumptions were found to be violated. The GOSH plot analysis revealed that although heterogeneity was high, the calculated effect sizes for PC represent a consistent distribution across all possible random sub-sets of the studies in this review with no significant sub-clusters present in the data (see OSM 1, Figure 4). Furthermore, when the HKSJ method was used to calculate effect sizes due to significant heterogeneity within the analyses the effects from all primary analyses (using Hedges' g) were upheld (a summary is available from the lead author upon request).

### ***B. Confounding assessments***

To identify potential confounds that may have affected the conclusions, chi-square analyses and Pearson's correlations were conducted to examine whether pairs of significant moderators co-occurred. When significant moderators co-occurred, they were entered simultaneously as predictors in multivariate meta-regressions to determine whether or not any predictor explained significant unique variance in effect size outcomes. For clarity, and to understand the context in which these tests were run, all analyses that aimed to identify potential confounds between study variables are reported in the appropriate 'objective' sub-section below.

**Objective 1b:**

*Study characteristics associated with greater effect sizes for PC.*

In the main report, one study characteristic was associated with larger effect sizes for PC: studies testing interventions delivered by healthcare professionals generated larger effect sizes than studies testing interventions not delivered by healthcare professionals, $B = 0.39$, $S.E. = 0.18$, $CI = -0.77 - -.009$, $p = .045$. As no other moderator significantly predicted PC (see OSM1, Table 4), no further analyses were conducted.

**Objective 2a**

*Can interventions targeting PC also impact health outcomes?*

In the main report, we report the effect of the interventions targeting PC on health behaviour and physical health outcomes separately. Here, and wherever the term *overall* is used, we

report the effect of these interventions on a combined outcome (health behaviours + physical health outcomes – health *overall*).

The interventions produced, on average, small, but significant and heterogeneous $I^2$ = 48.1%; $Q(35) = 67.45$ $p < .001$, effect sizes for health outcomes overall $g = 0.28$, 95% CI = 0.21 to 0.34 ($k = 36$).

**Objective 2b**

*Study characteristics associated with larger effect sizes for Health Overall.*

As above, we repeated the analyses that were conducted separately for health behaviour and physical health outcomes and reported in the main text such that we test the association between study characteristics and for health overall.

These analyses revealed that all intervention types had a significant positive effect on health overall with the exception of pain management strategies. The effect sizes in studies testing psychological detachment style interventions were larger than in studies testing different types of interventions, for health overall, $B = 0.30$, *S.E.* = 0.18, *CI* = 0.07 − 0.54, $p = .014$. For health overall, interventions were significantly more effective at yielding larger effect sizes in studies where content was self-administered $B = 0.18$, *S.E.* = 0.07, *CI* = -0.04 − 0.31, $p = .013$, as opposed to those in which content was delivered by a health-care professional, $B = 0.12$, *S.E.* = 0.15, *CI* = -0.18 − 0.42, $p = .415$, or a trained facilitator, $B$ = -0.11, *S.E.* = 0.08, *CI* = -0.28 − 0.06, $p = .196$.

*Study characteristics associated with larger effect sizes for Health Behaviours.*

Further to the main report of: Effect sizes were significantly larger when interventions were self-administered $B = 0.26$, *S.E.* = 0.09, *CI* = 0.07 − 0.45, $p = .01$, delivered at an individual level rather than group-level, $B = -0.25$, *S.E.* = 0.11, *CI* = -0.49 − 0.006, $p = .045$, and when health behaviours were assessed closer to the conclusion of an intervention $B = -0.001$, *S.E.* = .0003, *CI* = -.002 − -.0003, $p = .01$ ($k = 21$). Given these moderators co-occurred, we ran further analyses to test for confounding. Accordingly, self-administered interventions tended to be delivered to individuals, $\chi^2$ (1) = 11.08, $p < .001$, and self-administered interventions tended to have shorter follow-ups, $r = -.60$, $p < .001$. In subsequent multivariate meta-

regressions to account for these potential confounds, self-administered interventions marginally predicted health behaviour effect sizes when controlling for group/individual delivery format, *B* = 0.20, *S.E.* = 0.10, *CI* = -0.02 − 0.43, *p* = .05, but not after controlling for time-point, *B* = 0.16, *S.E.* = 0.13, *CI* = -0.12 − 0.42, *p* = .237. Neither group/individual delivery format, *B* = 0.002, *S.E.* = 0.09, *CI* = -0.18 − 0.19, *p* = .979 or measure time point, *B* = -0.0006, *S.E.* = 0.0005, *CI* = -0.0018 − 0.0005, *p* = .24, explained unique variance in health behaviour effect sizes, thus suggesting some evidence of confounding.

*Study characteristics associated with larger effect sizes for Physical Health Outcomes*

Further to the main report of: while no particular intervention type was related to significantly larger effect sizes for physical health outcomes, interventions were at their most effective when delivered in educational, *B* = 0.19, *S.E.* = 0.07, *CI* = 0.48 − 0.32, p = .01, and academic settings, *B* = -0.17, S.E. = 0.08, *CI* = -0.35 − 0.06, *p* = .043, as opposed to delivered in medical settings, *B* = 0.009, *S.E.* = 0.10, *CI* = -0.19 − 0.21, *p* = .919. We did not, however, conduct further tests to detect confounding as it was not theoretically possible for a study to be conducted in more than one setting and because no other moderators co-occurred.

**Objective 3:**

*Are larger effect sizes for PC associated with larger, but positive, effect sizes for health overall?*

There was a non-significant trend regarding the association between PC effect sizes health outcomes overall *effect* sizes, *B* = -0.21, *S.E.* = 0.11, *CI* = -0.43 − 0.02, *p* = .067 (*k* = 36, see Table 1). However, following the removal of two studies identified as multivariate influential cases (Magnan et al., 2014 & Thiart et al., 2015), medium-sized effects for PC, *g* = .41, were associated with small, but positive, *g* = .25, effect sizes for health overall, *B* = -0.25, *S.E.* = 0.09, *CI* = -0.44 − -0.07, *p* = .008 (*k* = 34). This effect was upheld in subsequent permutation tests with 10,000 random computations, *B* = -0.36, *S.E.* = 0.21, *CI* = -0.78 − 0.05, *p* = .038. Larger effect sizes for worry, *B* = -0.46, *S.E.* = 0.21, *CI* = -0.92 − 0.09, *p* = .054 (*k* = 14), and rumination, *B* = -0.71, *S.E.* = 0.27, *CI* = -1.58 − 0.15, *p* = .062 (*k* = 5), specifically, were

marginally associated with larger effects for health behaviours, with a $g$ = .41 for worry corresponding with a $g$ = .27 in health behaviours, and a $g$ = .56 in rumination corresponding with a $g$ = .38 in health behaviours.

Effect sizes for worry, $B$ = -0.38, *S.E.* = 0.22, *CI* = -0.83 − 0.08, $p$ = .091 ($k$ = 19), and rumination ($k$ = 8), $B$ = -0.43, *S.E.* = 0.21, *CI* = -0.94 − 0.07, $p$ = .081, were not significantly associated with effect sizes for health *overall*.

### ***C. Sensitivity analyses for two studies using proxy measures for health behaviours.***

Given two of the included studies interested in health behaviour (Christiansen et al., 2014; Aardoom et al., 2016) used measures (AUDIT & EDE-Q, respectively) incorporating items relevant to both health behaviours *and* determinants of health behaviours within a single index (i.e., proxy measures, while all other related studies only included behavioural items), we removed these two studies in an additional sensitivity analysis to ensure this feature did not influence any of the conclusions.

The findings reported in the main manuscript were upheld. The interventions, on average, led to a small-to-medium, and heterogeneous $I^2$ = 48.8%; $Q$(33) = 64.39 $p$ < .001, effect sizes for health behaviours, $g$ = 0.29, 95% *CI* 0.22 to 0.36 ($k$ = 34) and effects for PC were only marginally associated with positive effect sizes for health behaviours, $B$ = -0.20, *S.E.* = 0.10, *CI* = -0.40 − 0.007, $p$ = .058. Thus, suggesting the inclusion of these two studies had no meaningful impact on the study objectives relating to health behaviours.

### ***D. Accounting for the potential impact of reverse causality between PC and health.***

To minimize the potential impact of reverse causality between PC and health (i.e. intervention content first influencing health before being captured within measures of PC), studies measuring PC *immediately* post-intervention and then health at a *later* point in time, were subject to additional tests. This sub-set of studies ($k$ = 18, 50%) were subject to a separate meta-regression examining if effect sizes for PC were positively, and significantly, associated with effect sizes for health outcomes (overall), to control for this possibility. As an

extra precaution, PC effect sizes for these 18 studies were also directly compared via a Welches *t*-test to the remainder of studies which simultaneously measured PC *and* health either immediately post-intervention ($k$ = 15, 41%), or within follow-up measures ($k$ = 3, 9%), to detect if they significantly differed depending on the point in time in which they were collected post-intervention. Note, we did not run this separately for health behaviours and physical health outcomes due to power concerns.

The sub-group meta-regression comprising studies measuring PC *immediately* post-intervention, and health outcomes (overall) later ($k$ = 18, 50%), revealed effect sizes for PC significantly predicted more positive health effect sizes, $B$ = -0.36, *S.E.* = 0.15, *CI* = -0.67 − -0.04, $p$ = .031, denoting lower levels of PC in the intervention condition versus the control. Furthermore, a Welches two-sample *t*-test comparing this sub-set of studies to those which measured PC *and* health at the same point in time ($k$ = 18), indicated that PC effect sizes did not significantly differ between the two sub-sets of studies as a function of time, $t$ (36) = -.31, $p$ = .371. Deviations in PC that occurred following the delivery of an intervention package are thus unlikely to have been driven by effects for health outcomes and do not differ across the period of time in which all post-intervention measures were collected.

### ***E. Analyses relating to Sleep***

Additional analyses were conducted for the most common health outcome (sleep, $k$ = 17). The interventions produced, on average, small-medium and non-heterogeneous $I^2$ = 8.1%; $Q$(16) = 4.49 $p$ = .997, effect sizes for sleep, $g$ = 0.30, 95% CI = 0.11 to 0.49 ($k$ = 17). Effect for PC, $B$ = -0.21, *S.E.* = 0.11, *CI* = -0.52 − 0.04, $p$ = .022, and worry specifically, $B$ = -0.76, *S.E.* = 0.28, *CI* = -1.41 − -0.11, $p$ = .027, but not rumination, $B$ = -0.62, *S.E.* = 0.34, *CI* = -.1.72 − -0.47, $p$ = .167, were positively associated with parameters of sleep (i.e., total-sleep-time/sleep-onset-latency) (see, Table 1). In addition, the effect sizes in studies testing psychological detachment style interventions were larger than in studies testing different types of interventions for sleep, $B$ = 0.35, *S.E.* = 0.11, *CI* = .109 − .583, $p$ < .001.

Studies which included a measure of sleep, versus those which did not, were entered as an additional moderator to assess if larger effect sizes were associated with this intervention

feature. However, there was no evidence to suggest studies which measured sleep yielder larger effect sizes on behaviour when compared to all other studies, *B* = -0.10, *S.E.* = 0.09, *CI* = -.30 − .10, *p* = .309 (*k* = 36). Furthermore, psychological detachment interventions generated significantly larger effect sizes for studies testing this type of intervention within measures of sleep versus those studies testing other types of intervention, *B* = 0.35, *S.E.* = 0.11, *CI* = .109 − .583, *p* < .001.

### ***F. Testing for small study bias***

Small-study bias, whereby larger effect sizes tend to be reported within smaller sample sizes, was examined using Egger's test. Duval and Tweedie's (2000) trim and fill analysis was conducted to estimate the impact of publication bias on PC and health outcome effect sizes.

Egger's regression coefficient was non-significant for PC (*p* = .087) but was significant for health outcomes (overall) (*p* = .022) suggesting small study bias for the latter. Thus, Duval and Tweedie's (2000) trim and fill analysis imputed nine additional effect sizes for the effect of the interventions on health outcomes (overall) (see OSM 1, Figure 3), generating an overall effect size of *g* = .22 (95%*CI* = 0.13 − 0.30). Consequently, the effect of the interventions on PC and health outcomes remained significant after controlling for small-study bias.

### ***G. Potential impact of studies with multiple study arms.***

We took extra steps to control for potential selection-bias when an alternative study arm was available (e.g., Topper et al., 2017; internet vs. group-based therapy). There were six studies whereby more than one study arm was available to choose from as the 'treatment' arm. For the 5 intervention arms, we first prioritized the intervention arm authors hypothesized to produce greatest effect sizes in PC (this was the case for 4/5 of studies). In one case when this was not reported, as we were interested in the most effective methods at influencing PC, we chose the arm which yielded the largest effect size in PC. Only 1 study required us to make a choice between comparator arms. In this one instance (Versluis et al., 2018), we followed the conservative approach of selecting the attention-placebo control, as it is well

known that effect sizes of interventions compared with no-treatment control groups are greater than effect sizes of interventions compared to attention-placebo control groups (Lipsey & Wilson, 1993).

To control for the potential inflation of effect-sizes we ran a further sensitivity analysis; first, via a series of meta-regressions with the feature of 'more than one intervention arm present' set as the predictor and study effect sizes for PC, health behaviours and physical health outcomes as the DV. Importantly, effect sizes for PC, *B* = 0.18, *S.E.* = 0.12, *CI* = -0.05 − 0.42, *p* = .124, health behaviours, *B* = - 0.10, *S.E.* = 0.09, *CI* = -0.21 − -0.82, *p* = .309, and physical health outcomes, *B* = - 0.07, *S.E.* = 0.16, *CI* = -0.31 − -0.71, *p* = .317, were unrelated to the number of intervention arms a study employed. Second, we conducted sensitivity analyses in which these studies that included more than one 'treatment arm' were removed from the meta-analyses. The impact on the conclusions was negligible for PC: *g* = - 0.42, 95% *CI* = -0.51 to -0.21; health behaviours: *g* = 0.32, 95% *CI* 0.24 - 0.49, and physical health outcomes: *g* = 0.22, 95% *CI* = 0.22 - 0.31. Crucially, these analyses shows our handling of intervention arms did not bias this reviews conclusions.

### ***H. Assessing potential impact of baseline differences between study arms***

To control for the possibility that baseline differences between study conditions influenced effect sizes, we carried out two further tests. Seven studies reported significant baseline differences in PC and two studies reported significant baseline differences in health outcomes. First, a univariate meta-regression, with reported vs. non-reported baseline differences set as the predictor and effect sizes for PC, health behaviours and physical health outcomes, respectively set as the DV, was carried out. Study effect sizes for PC, *B* = 0.11, *S.E.* = 0.23, *CI* = -0.12 − 0.32, *p* = .271, health behaviours, *B* = - 0.12, *S.E.* = 0.14, *CI* = -0.52 − 0.24, *p* = .159, and physical health outcomes, *B* = -0.09, *S.E.* = 0.19, *CI* = -0.49 − 0.19, *p* = .347, were unrelated to the presence of baseline differences among studies. Second, we conducted sensitivity analyses in which these studies that reported baseline differences on specific measures were removed from the meta-analyses. The impact on the

conclusions was minimal (PC: *g* = - 0.41, 95% *CI* = -0.54 to -0.25; health behaviours: *g* = 0.30, 95% *CI* 0.19 - 0.40; physical health outcomes: *g* = 0.22, 95% *CI* = 0.12 - 0.29). Therefore, we can be fairly confident that any degree of baseline between-condition difference did not meaningfully impact any of our analyses which rest upon this assumption.

***I. Examining the potential impact of clinical differences in participant characteristics.***

To control for the possibility that clinical baseline heterogeneity between studies which either contained GAD participants (Conrad et al., 2008 & Freshour et al., 2016; *N* = 2) or pertained participants with sleep disturbance (Sandlund et al., 2018; Pech & O'Kearney, 2013; Jansson-Frojmark et al., 2012; Harvey et al., 2017; N = 4) affected the conclusions, we carried out three further analyses.

First, two sets of univariate meta-regressions (i.e., one for each sample type), with sample type (GAD sample: yes/no; sample with sleep disturbances: yes/no) set as the predictor and effect sizes for PC, health behaviours and physical health outcomes, respectively set as the DV, was carried out. Study effect sizes for PC (*B: -.26, S.E = .09, p = .204),* health behaviours *(B: -.13, S.E = .11, p = .112*) and physical health outcomes (*B: .002, S.E = .003, p = .62*) were not significantly impacted by GAD samples, and the same was true for those studies containing participants with sleep disturbances for PC: (*B: -.21, S.E = .18, p = .174),* health behaviours *(B: -.13, S.E = .11, p = .403),* and physical health outcomes *(B: .009, S.E = .003, p = .405*).

Second, we conducted two sensitivity analyses in which the studies that had GAD participants, or those with 'clinical' sleep disturbances, were removed from the respective meta-analyses. The impact on the conclusions was minimal (PC: *g* = - 0.41, 95% *CI* = -0.57 to -0.29; health behaviours: *g* = 0.29, 95% *CI* 0.17 - 0.39; physical health outcomes: *g* = 0.21, 95% *CI* = 0.15 - 0.31) when removing the 2 studies including GAD participants, and similar effects were found when (separately) removing the 4 studies comprising participants with sleep disturbances (PC: *g* = - 0.40, 95% *CI* = -0.54 to -0.33; health behaviours: *g* = 0.30, 95% *CI* 0.20 - 0.38; physical health outcomes: *g* = 0.23, 95% *CI* = 0.13 - 0.30).

Therefore, we can be fairly confident that any degree of baseline between-condition difference did not meaningfully impact any of our conclusions.

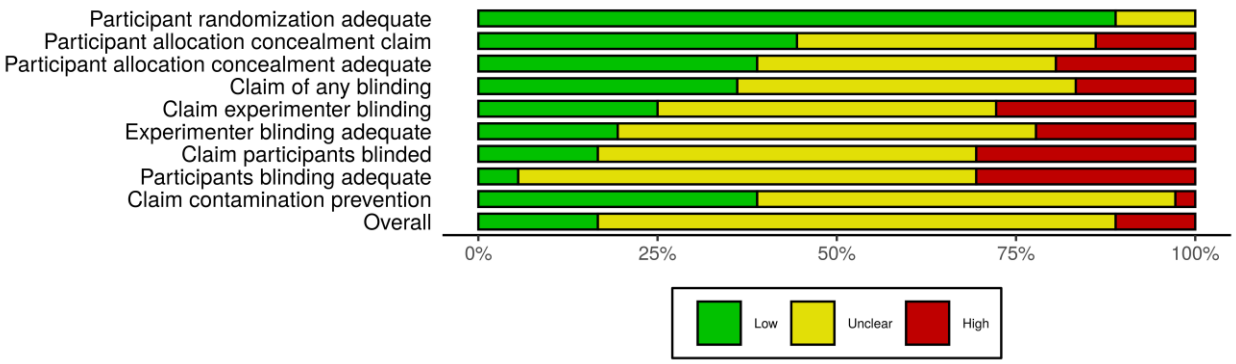
Third, to assess if these samples had any impact on objective 3 (i.e., the association between PC and health) we re-analysed with the 6 studies (2 GAD studies & 4 sleep studies) removed; along with any influential cases relevant to either outcome removed to be consistent with the main report. The impact of removing these 6 studies (and one influential case, Magnan et al., 2014) on the findings was minimal. Medium-sized effects for PC, $g$ = -.39, were still associated with a small, but positive, $g$ = .24, effect for health behaviours, $B$ = -0.22, *S.E.* = 0.13, *CI* = -0.47 − -0.11, $p$ = .028. A similar trend was present with physical health outcomes when compared to our original analysis. Effect sizes for PC were still unrelated to effect sizes for physical health $B$ = -0.15, *S.E.* = 0.21, *CI* = -0.58 − 0.17, $p$ = .328, when removing these 6 studies (and an influential case, Digdon & Koble, 2011), $B$ = -0.16, *S.E.* = 0.08, *CI* = -0.56 − 0.19, $p$ = .292. As such, combined, these three sets of analyses show that we can be fairly certain that while sample characteristics are important to consider, they had very little bearing on the findings of this particular meta-analysis.

1679 ***OSM 3: Risk of Bias Traffic Light Plot***

1680


Risk of bias domains
D1
D2
D3
D4
D5
D6
D7
D8
D9
Overall
Study
Aardoom 2016
Abassi 2012
Brosschot 2016
Buntrock 2015
Buntrock 2016
Carney and Waters 2006
Christiansen 2014
Conrad 2008
Crain 2017
Digdon and Koble2011
Ebert 2014
Ebert 2015
Ebert 2016
Eilenberg 2016
Freshour 2016
Harvey 2017
Hazlett-Stevens and Oren 2017
Jansson-Frojmark 2012
Jellesma 2009
Lokman 2017
Magan 2014
McGowan and Behar 2013
Mehlsen 2017
Michailidis and Cropley2019
Pech and Okearney 2013
Peters 2017
Querstret 2017
Sandlund 2018
Sibinga 2013
Sibinga 2016
Teismann 2014
Thiart 2015
Topper 2017
Versluis 2016
Versluis 2018
Woilizky-Taylor 2010
D1: Participant randomization adequate
D2: Participant allocation concealment claim
D3: Participant allocation concealment adequate
D4: Claim of any blinding
D5: Claim experimenter blinding
D6: Experimenter blinding adequate
D7: Claim participants blinded
D8: Participants blinding adequate
D9: Claim contamination prevention
Judgement
High
Unclear
Low

**OSM 4: Weighted Risk of Bias Plot**


Participant randomization adequate
Participant allocation concealment claim
Participant allocation concealment adequate
Claim of any blinding
Claim experimenter blinding
Experimenter blinding adequate
Claim participants blinded
Participants blinding adequate
Claim contamination prevention
Overall
0%
25%
50%
75%
100%
Low
Unclear
High

1713