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

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Running Head: Perseverative Cognition on Health

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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.

62 Psychological stress has consistently been linked to negative health outcomes, with
63 recent figures suggesting stress-related health care costs an estimated \$300 billion per
64 annum (American Institute for Stress, 2020). Indeed, the impact of psychological stress, that
65 is, when the appraisal processes attached to a threat or experience exceeds an individual's
66 perceived coping ability, has long been implicated in a variety of health and illness outcomes
67 (e.g. neurotic symptoms, House et al., 1979; organ damage, Plante, 2002; cardiovascular
68 disorders, Lundberg, 2005; migraines, Schoonman., 2007; diabetes, Öhman, Bergdahl,
69 Nyberg & Nilsson, 2007; for a review see O'Connor, Thayer & Vedhara, in press). Whether
70 directly through autonomic and neuroendocrine responses or indirectly, via changes¹ in
71 health behaviours (Christiansen, Larsen & Lasgaard, 2016, Jones & Bright, 2007, O'Connor,
72 Thayer & Vedhara, in press), adverse health outcomes have been noted to be of direct
73 consequence to stress, even when the stressor is no longer present (Brosschot et al., 2006).
74 In particular, perseverative cognition (PC) has been identified as an important mechanism
75 that may help explain how stressful events and encounters increase the risk of ill-health and
76 poor wellbeing. PC is thus defined as any type of stress-related, negative, repetitive thought
77 and encompasses thoughts about feared future events (worry) and thoughts and negative
78 feelings about distressing past experiences (rumination).

79 In the original perseverative cognition hypothesis (PC hypothesis), Brosschot et al.
80 (2006) suggested that stressful thoughts activate the body's stress response in the same
81 way as stressors in the physical environment and serve to prolong the hypothalamic-
82 pituitary-adrenal-axis stress response. Since then, several key reviews have shown that PC
83 is associated with a range of physiological health outcomes; including higher blood pressure
84 and heart rate, lower heart rate variability, as well increased cardiovascular activity, reduced
85 secretion of antibody productions, blunted cortisol response and increased levels of
86 somatization (for reviews, see Ottaviani et al., 2018; Verkuil, Brosschot, Gebhardt & Thayer,
87 2010).

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

90 consumption (Clancy, Prestwich, Caperon & O'Connor, 2016; Cropley et al., 2012; Frone,
91 2015). Importantly, these negative health behaviours are related with illness (Suris & Parera,
92 2005), disease and morbidity rates (Burke et al., 2007), in both adults and children cross-
93 culturally (for review, see Mackenbach, 2014). Notably, in a meta-analytic review of health
94 behaviours across 19 studies, Clancy et al. (2016) showed that higher levels of PC were
95 associated with significantly more health risk behaviours. In particular, these authors found
96 that PC was associated with greater substance use, unhealthy eating and smoking. Taken
97 together, these findings provided evidence for an extended PC hypothesis, such that there
98 may be scope for an additional route to pathogenic disease via poorer health behaviours.

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

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

132 *The present review*

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

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

150 **Method**

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

153 *Eligibility Criteria*

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

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

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

180 *Search Strategy*

181 Three databases were searched to maximize search sensitivity (see Montori et al.,
182 2005): PsycINFO (1806 – present) and Medline (1806 – present) via OVID, and CINAHL
183 (1960-present) using EBSCO. The search was last conducted on the 23rd November 2019
184 with search terms relating to perseverative cognition, and randomized interventions.
185 Perseverative cognition search terms were adapted from Clancy et al. (2016). Specifically,
186 “negative and (thought or thinking)” was removed to enhance specificity; “perseverati” with
187 “cogniti” was replaced with “perseverative and (thought* or thinking or cognition*)”. The Eady
188 et al. (2008) RCT filter (random*.tw) was employed as a single term to capture the best
189 optimisation of sensitivity and specificity, complimented with the term (intervention*.tw) to
190 enhance sensitivity. Further, to maximise sensitivity (at the expense of specificity), search
191 terms were not generated for health outcomes. The search was limited by the English
192 language and human studies but not by year (see, OSM 1). Titles, abstracts, and full-text
193 screening were completed by the first author. The third author independently screened the
194 titles and abstracts using a subset of 1070 studies (20% of total) (Cohen’s kappa = .91). Any
195 discrepancies were discussed and resolved. Any study identified as potentially eligible at the
196 abstract screening stage was progressed to full-text screening. The first author then
197 independently assessed all full-texts with 40% of full-texts independently double-screened by
198 the third author (Cohen’s kappa = .98). Discrepancies were then discussed and verbally
199 agreed upon between both authors. Across the sets of double-screened studies, the
200 secondary coder did not identify any eligible studies missed by the primary coder.

201 *Data Extraction & Data Coding*

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

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

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

230 Other important methodological or statistical features (e.g., using validated measures,
231 reporting of satisfactory levels of internal consistency, baseline differences between groups)
232 and if studies incorporated intention-to-treat analysis (ITT) were also considered. We
233 approached data extraction in two phases to minimise the possibility of coding errors. The
234 first phase was piloted on 10% of the studies in a 'training phase'. For this piloted 10%, the
235 coding for all measures was checked by a second reviewer. Inter-rater agreement levels
236 were classified as near-perfect for items relating to health outcomes and PC (Cohen's kappa
237 = .75 - .1) and often perfect for items relating to risk of bias and other study characterises
238 (i.e., population, attrition, design, measure timing) (all kappas >.92; Landis & Koch, 1977).
239 Second, we operated a 'validation phase' whereby data for all studies was first extracted by
240 a primary coder before an extra 20% of studies were independently assessed by a second
241 coder. For this phase, agreement between coders was near perfect across all study items
242 (Cohens kappa = .97 - .1). In all cases, if either coder was in any doubt, the study authors
243 were contacted for additional clarification before making deciding upon eligibility.

244 *Data Synthesis*

245 Effect sizes were calculated based on means and standard deviations and, when not
246 available ($k = 6$), using other statistics reported (i.e. F and p values). Effect sizes were
247 calculated for *PC overall* (worry, rumination and measures of perseverative thinking
248 combined), for worry and rumination separately, for health behaviours and physical health
249 outcomes separately, as well as for sleep as it was the most common health outcome
250 (77.3% of studies) (note, we view sleep as a health behaviour as it is an action that is under
251 volitional control). Results pertaining to health outcomes *overall* (i.e., physical health and
252 behaviours combined) are reported in OSM 1 and 2. Standard errors were adjusted to
253 account for clustering in relevant studies ($k = 3$) (see Higgins, Deeks, & Altman, 2008).
254 Hedges' g was used as the main effect-size measure (see OSM 2 for Hartung-Knapp-Sidik-
255 Jonkman method) as it provides an unbiased estimate of effects (Hedges & Olkin, 1985).

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

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

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

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

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

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Results

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INSERT FIGURE 1 HERE

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Study Characteristics

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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).

341 On average, content was provided across 8 days ($SD = 4.27$), with intervention
342 groups receiving content on more days ($M = 9.2$, $SD = 3.81$) than the comparison groups (M
343 $= 7.14$, $SD = 3.11$). The mean time-point at which post-intervention measures were collected
344 (from initial exposure to intervention content) was 49 days ($SD = 52.49$) for PC, 99 days for
345 physical health outcomes ($SD = 103.06$) and 143 for health behaviours ($SD = 130.38$) ($M =$
346 118 , $SD = 115.59$ for health outcomes *overall*). All of the interventions that were delivered in
347 an in-person ($k = 21$, 58.3%) used printed materials, and employed a variety of delivery
348 formats (i.e., self-administered, self-administered with support, healthcare professionals).
349 Fifteen studies (41.66%) were hosted using an online platform (i.e., computer, mobile phone
350 or tablet based). The most popular mode of delivery was interventions that were self-
351 administered, with participants set a task to complete (e.g., to postpone worry) by
352 experimenters in their own time ($k = 16$, 44.4%), followed by self-administration with support
353 (i.e. from the experimenter) ($k = 8$, 22.2%). Less popular were interventions delivered with a
354 trained facilitator (i.e. a mindfulness coach) ($k = 6$, 16.6%), or by a health-care professional
355 (i.e. a nurse practitioner) ($k = 4$, 11.1%). Of these, three studies (8.3%) also used the
356 telephone, two studies used mail (5.55%) and one study adopted a video to deliver part of
357 the intervention (3.6%). The interventions tested were broadly defined as: (1) cognitive
358 behavioural/acceptance and commitment therapies ($k = 10$, 27.7%), (2) PC action plans ($k =$
359 9 , 25%), (3) mindfulness and relaxation ($k = 7$, 19.4%), (4) stress management ($k = 4$,
360 11.1%), (5) psychological detachment ($k = 2$, 5.5%), (6) expressive writing ($k = 2$, 5.5%), and
361 (7) pain management ($k = 2$, 5.5%). While these categories do not capture the granular level
362 nuances between interventions, they do represent the core therapy used.

363 In general, studies were unclear or at high risk of bias. Although only 4 studies
364 (11.1%) failed to report a valid method of randomization, 21 (58.3%) did not report a method
365 of allocation concealment, 29 (80.6%) did not report adequate steps to blind the
366 experimenter or data analyst and 34 (94.4%) did not report adequate methods to blind
367 participants. Over 60% of studies ($k = 20$, 61.1%) did not claim contamination prevention
368 between groups and did not consider using ITT analysis, though only one study (3.6%) used

369 measures of PC that were not internally reliable. The majority of studies contained
 370 information on informed consent ($k = 32$, 88.8%). Attrition rates were moderate (22.9%, SD
 371 = 16.63), and did not significantly influence PC effect sizes ($p = .381$). A summary of the risk
 372 of bias for each study is available via OSM 3 & 4. Despite instances of high risk of bias
 373 across the included studies, each risk of bias item did not moderate the effects of the
 374 interventions on PC ($p = .076$ to $.981$; median = $p = .432$).

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

376 Levels of PC were lower in the intervention group versus the comparison group at
 377 follow-up. The interventions produced, on average, a near medium-sized effect on PC, $g = -$
 378 0.42, 95% $CI = -0.51$ to -0.33 ($k = 36$, see Figure 2), albeit the effect sizes were
 379 heterogeneous across studies, $I^2 = 59.3\%$; $Q(35) = 87.17$ $p < .001$. A similar-sized, and
 380 heterogeneous effect, $I^2 = 47.9\%$; $Q(18) = 34.56$ $p = .011$, emerged when the analyses were
 381 repeated specifically for worry, $g = -0.41$, 95% $CI = -0.51$ to -0.30 ($k = 19$, see OSM 1, Figure
 382 1). Interventions produced a medium-sized effect on rumination, $g = -0.58$, 95% $CI = -0.84$ to
 383 -0.32 ($k = 8$, see OSM 1, Figure 2), with the effect sizes again heterogeneous, $I^2 = 66.9\%$;
 384 $Q(7) = 21.14$ $p = .004$.

385 **INSERT FIGURE 2 HERE**

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

387 All but two of the seven intervention types (pain management and expressive writing)
 388 produced significant effect sizes for PC. However, meta-regressions indicated that none of
 389 the intervention types produced larger effects than the other interventions combined (see
 390 OSM 1, Tables 2 & 3). Effect sizes were significantly larger, suggesting more effectiveness,
 391 when interventions were delivered by healthcare professionals, $B = 0.39$, $S.E. = 0.18$, $CI = -$
 392 $0.77 - .009$, $p = .045$, versus when they were not delivered by healthcare professionals. No
 393 other moderators influenced PC effect sizes across all PC related analyses.

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

397 significantly larger in studies comprising of a student sample, $B = -0.35$, $S.E. = 0.14$, $CI = -$
 398 $0.65 - -0.05$, $p = .024$, than in those which did not. Worry effect sizes were not influenced by
 399 any other moderators across all other worry related analyses.

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

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

406 The interventions targeting PC, on average, led to a small-to-medium, and
 407 heterogeneous $I^2 = 51.8\%$; $Q(20) = 41.50$ $p = .003$, effect for health behaviours, $g = 0.31$,
 408 $95\% CI 0.21$ to 0.42 ($k = 21$, see Figure 3). A similar-sized, but non-significant and
 409 homogeneous $I^2 = 24.7\%$; $Q(20) = 26.57$ $p = .148$, effect, $g = 0.23$, $95\% CI = 0.15$ to 0.31 ,
 410 was detected for physical health outcomes ($k = 21$, see Figure 4).

411 **INSERT FIGURE 3 & 4 HERE**

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

414 A range of study characteristics were significantly associated with effect sizes for
 415 both health behaviours and physical health outcomes. These are reported in full within OSM
 416 1 (see, Table 2 - 4) and OSM 2 (see, section B); where we also consider the impact of
 417 confounding. In brief, all intervention types had a significant, positive effect on health
 418 behaviours with the exception of pain management strategies. However, the effect sizes in
 419 studies testing psychological detachment style interventions, $B = 0.33$, $S.E. = 0.16$, $CI = -$
 420 $.007 - 0.67$, $p = .05$, and PC action plans, $B = 0.37$, $S.E. = 0.14$, $CI = 0.08 - 0.66$, $p = .016$,
 421 produced significantly larger effect sizes than studies not testing this intervention type for
 422 health behaviours. In addition, effect sizes were significantly larger when interventions were
 423 self-administered, $B = 0.26$, $S.E. = 0.09$, $CI = 0.07 - 0.45$, $p = .01$, delivered at an individual
 424 level rather than group-level, $B = -0.25$, $S.E. = 0.11$, $CI = -0.49 - 0.006$, $p = .045$, and when

425 health behaviours were assessed closer to the conclusion of an intervention, $B = -0.001$,
 426 $S.E. = .0003$, $CI = -.002 - -.0003$, $p = .01$ ($k = 21$) (see OSM 2, section B for further
 427 consideration).

428 While no particular intervention type was related to significantly larger effect sizes for
 429 physical health outcomes, interventions were at their most effective when delivered in
 430 educational, $B = 0.19$, $S.E. = 0.07$, $CI = 0.48 - 0.32$, $p = .01$, and academic settings, $B = -$
 431 0.17 , $S.E. = 0.08$, $CI = -0.35 - 0.06$, $p = .043$, as opposed to delivered in medical settings, $B = -$
 432 0.009 , $S.E. = 0.10$, $CI = -0.19 - 0.21$, $p = .919$.

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

435 Initially, effect sizes for PC were unrelated to effect sizes for health behaviours $B = -$
 436 0.21 , $S.E. = 0.15$, $CI = -0.54 - 0.12$, $p = .212$ ($k = 21$). However, after the removal of a
 437 multivariate influential case (Magnan et al., 2014), medium-sized effects for PC, $g = -.43$,
 438 were associated with a small, but positive, $g = .27$, effect for health behaviours, $B = -0.28$,
 439 $S.E. = 0.10$, $CI = -0.50 - -0.07$, $p = .012$. Importantly, this effect was upheld in subsequent
 440 permutation tests with 10,000 random computations, $B = -0.28$, $S.E. = 0.24$, $CI = -0.75 -$
 441 0.19 , $p = .019$. Marginal associations between both worry and health behaviour, as well as
 442 between rumination and health behaviour were also revealed (see OSM 2, section B).

443 Effect sizes for PC were unrelated to effect sizes for physical health, $B = -0.18$, $S.E.$
 444 $= 0.16$, $CI = -0.52 - 0.15$, $p = .264$ ($k = 21$), even after the removal of an influential case
 445 (Digdon & Koble, 2011), $B = -0.18$, $S.E. = 0.10$, $CI = -0.52 - 0.15$, $p = .261$. There were no
 446 significant associations between specific effect sizes for either worry or rumination and
 447 physical health outcomes (see Table 1).

448 Discussion

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

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

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

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

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

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

511 However, the current findings do support the recent extension of the PC hypothesis
512 to include health behaviours as an additional pathway to disease (see, Clancy et al., 2016;
513 2020). These findings are an important milestone for the extended PC hypothesis, and for
514 the stress literature more generally as they show, for the first time across a range of studies,
515 that effect sizes for PC following randomised experimental manipulations (taken, on
516 average, 41 days after intervention exposure) are positively associated with health
517 behaviours (taken, on average, at 143 days post-intervention). Further to what has been
518 previously revealed in correlational tests by Clancy and colleagues – who first showed that
519 the effects for health behaviours were most strongly associated with rumination (Clancy et
520 al., 2016), before a second meta-analysis demonstrated that both types of PC were robustly
521 associated with poorer sleep (Clancy et al., 2020) – here, using experimental evidence, we
522 show that a more negative health behaviour profile (and sleep in particular) are related to
523 larger effect sizes for the maladaptive characteristics of *both* worry and rumination. This is
524 not only theoretically important, as this finding supports the view that worry and rumination,
525 though separate and related constructs, are likely underpinned by related cognitive
526 processes (as the same intervention content yielded the similar treatment effects), but also
527 affords further clarity to healthcare professionals and other interventionists to help make
528 more informed treatment choices in the knowledge that both constructs are sensitive to
529 similar interventions. Therefore, given the prominence of PC in the aetiology of illness and
530 disease, the interventions included in this review can be used to attenuate the impact of both
531 worry and rumination on health behaviours.

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

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

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

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

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

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

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

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599 The authors declare no conflict of interest.

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604 **References**

605 (References marked by an asterisk are studies included in the meta-analysis).
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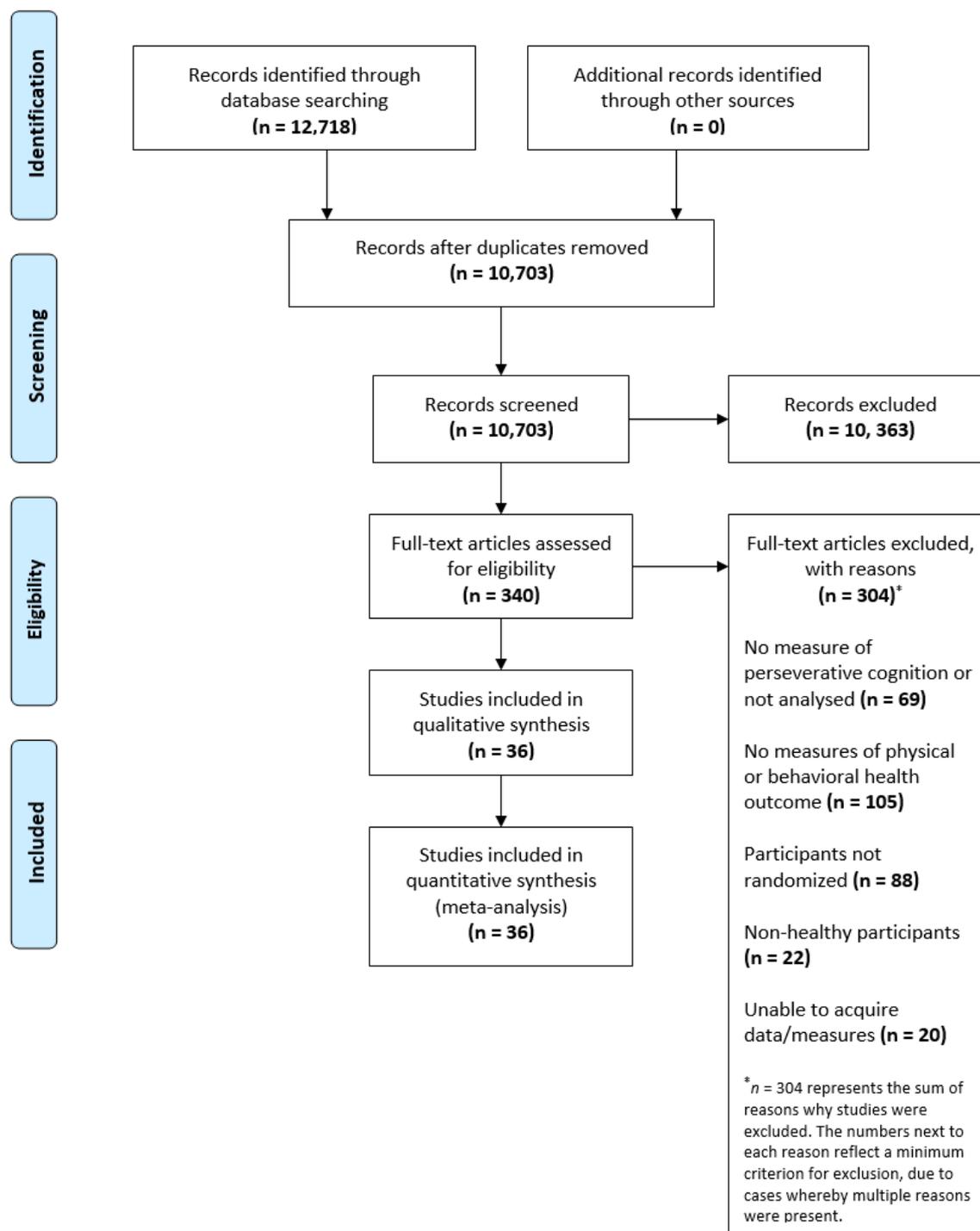
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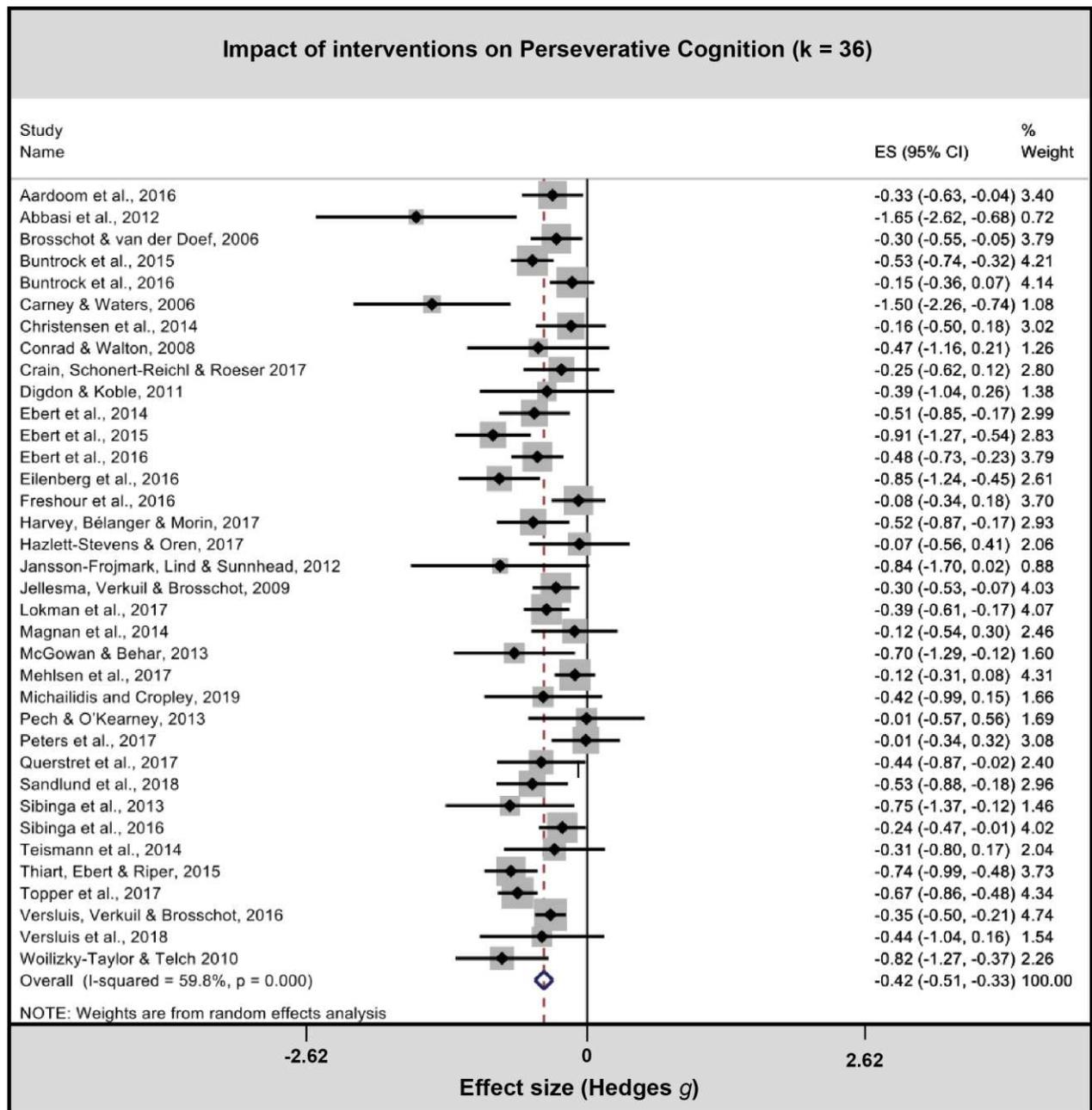
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912 *Figure 1. PRISMA diagram for included studies.*

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914 *Figure 2. Forest plot for PC.*



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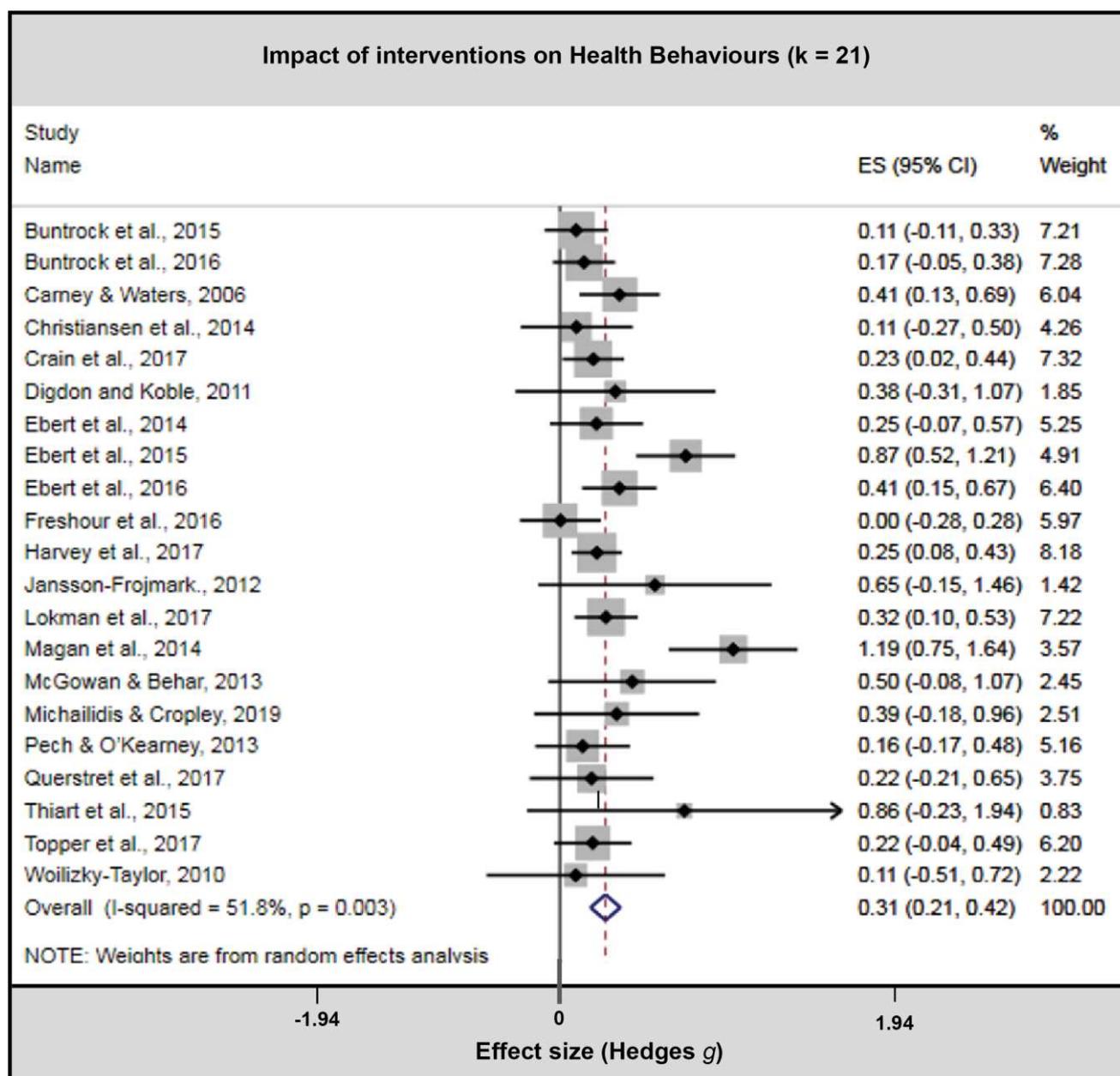
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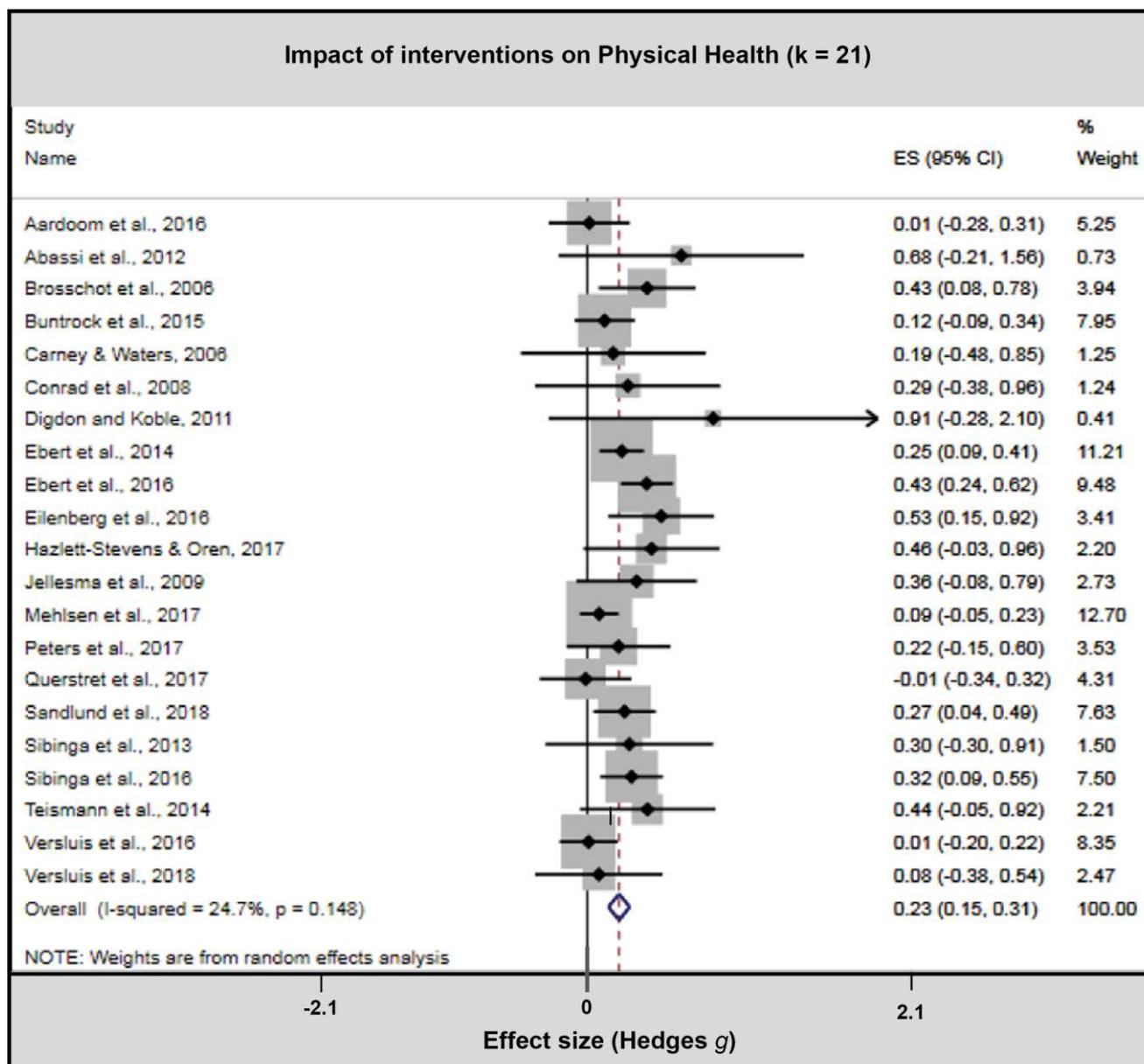
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923 *Figure 3. Forest plot for Health Behaviours.*



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933 *Figure 4. Forest plot for Physical Health.*



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944 *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

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

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

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Table 1. Overview of included studies ($k = 36$)**OSM 1: Information on Included Studies**

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 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 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 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 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 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 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-worry	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	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 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 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 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 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 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 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-coached, work detachment stress-management sessions.	PC: 49 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 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 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 HO: 183	Pre-sleep worry (APSQ)	Insomnia severity (ISI) and sleep diary (BTv, RTv, 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 mindfulness	PC: 70 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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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 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 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 HO:91.25	Worry (PSWQ)	Sleep quality (JSEQ)	Self-enrolled individuals with mild depressive symptoms.	$k = 237, M = 43 (SD = 12.93)$	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 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 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%
Mehlsen et al., 2017	Randomized controlled trial	Denmark, Medical	Pain management (6/6): In person,	PC: 63	Illness worries (Whiteley-7)	Physical health symptoms (SCL) & bodily	Individuals with chronic pain for longer than 3	$k = 399, M = 54 (SD = 13.05)$	72%	8%

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			therapist led, chronic pain self-management programme to improve wellbeing.	HO: 152		pain (RDQ, a 1-100 pain intensity VAS).	months from 75 different hospitals.			
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 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 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 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 reduce work-related rumination/fatig	PC: 28 HO: 183	Work-related rumination (WRRQ)	Sleep quality (PSQI) & work-related fatigue (OFER, 2 subscales for chronic fatigue	Self-enrolling working adults with elevated levels of work-related rumination	$k = 87, M = 40.68 (SD = 10.45)$	80.5%	25%

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			ue.			& acute fatigue)				
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 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 HO: 84	Rumination (CRSQ, rumination subscale)	Somatization symptoms (SCL)	Volunteer sample of 5 th to 8 th 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 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 personal life goals	PC: 3 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)
Thiart et al., 2015	Randomized controlled trial	Germany, Academic	CBT (6/8): Online, mixed intervention	PC: 56	Worry (PSWQ) & work-related rumination (IS,	Insomnia severity (ISI) & recuperation in	Volunteer sample of school teachers	$k = 118, M = 48$ ($SD = 9.9$)	74.2%	7.2%

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			based on CBT principles to improve wellbeing and sleep quality.	HO: 182	cognitive irritation subscale)	sleep (SF-AR)	with sleep complaints.			
Topper et al., 2017	Randomized controlled trial	Netherlands, Academic	CBT (6/6): Online, group based, CBT to prevent anxiety and depression	PC: 56 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 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 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-photoc stimulation for relaxation to reduce worry.	PC: 12 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%

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

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

980 Note: * $p = .05$; ** $p < .05$; *** $p < .001$; © = the categorical predictors for these analyses are set
 981 as 1 (type present) and 0 (type not present); CBT/ACT = cognitive behavioural/acceptance
 982 and commitment style therapies; PC = perseverative cognition; HO = health outcomes
 983 (overall); HB = health behaviours; PHO = physical health outcomes; *Hedges g* statistic =
 984 effect size estimate; *Z* statistic = the distribution under the null hypothesis that can be
 985 approximated by a normal distribution (accompanied by a significance test, p).
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1003 *Table 3. Associations between intervention types and study outcome effect sizes.*

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

1004 Note: * $p = .05$; ** $p < .05$; *** $p < .001$; © = the categorical predictors for these analyses are set
 1005 as 1 (type present) and 0 (type not present); CBT/ACT = cognitive behavioural/acceptance
 1006 and commitment style therapies; PC = perseverative cognition; HO = health outcomes
 1007 (overall); HB = health behaviours; PHO = physical health outcomes; B statistic =
 1008 standardized beta (accompanied by standard error, $S.E$ and significance test, p); f^2 statistic =
 1009 percentage of residual variation due to heterogeneity.
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1027 *Table 4. Association between effect sizes and study characteristics*

1028	1029	1030	1031 Test Statistic	
1032	1033	1034	1035 <i>B</i>	1036 <i>S.E.</i>
1037	1038	1039	1040	1041
1042	1043	1044	1045	1046
1047	1048	1049	1050	1051
1052	1053	1054	1055	1056
1057	1058	1059	1060	1061
1062	1063	1064	1065	1066
1067	1068	1069	1070	1071
1072	1073	1074	1075	1076
1077	1078	1079	1080	1081
1082	1083	1084	1085	1086
1087	1088	1089	1090	1091
1092	1093	1094	1095	1096
1097	1098	1099	1100	1101
1102	1103	1104	1105	1106
1107	1108	1109	1110	1111
1112	1113	1114	1115	1116
1117	1118	1119	1120	1121
1122	1123	1124	1125	1126
1127	1128	1129	1130	1131
1132	1133	1134	1135	1136
1137	1138	1139	1140	1141
1142	1143	1144	1145	1146
1147	1148	1149	1150	1151
1152	1153	1154	1155	1156
1157	1158	1159	1160	1161
1162	1163	1164	1165	1166
1167	1168	1169	1170	1171
1172	1173	1174	1175	1176
1177	1178	1179	1180	1181
1182	1183	1184	1185	1186
1187	1188	1189	1190	1191
1192	1193	1194	1195	1196
1197	1198	1199	1200	1201
1202	1203	1204	1205	1206
1207	1208	1209	1210	1211
1212	1213	1214	1215	1216
1217	1218	1219	1220	1221
1222	1223	1224	1225	1226
1227	1228	1229	1230	1231
1232	1233	1234	1235	1236
1237	1238	1239	1240	1241
1242	1243	1244	1245	1246
1247	1248	1249	1250	1251
1252	1253	1254	1255	1256
1257	1258	1259	1260	1261
1262	1263	1264	1265	1266
1267	1268	1269	1270	1271
1272	1273	1274	1275	1276
1277	1278	1279	1280	1281
1282	1283	1284	1285	1286
1287	1288	1289	1290	1291
1292	1293	1294	1295	1296
1297	1298	1299	1300	1301
1302	1303	1304	1305	1306
1307	1308	1309	1310	1311
1312	1313	1314	1315	1316
1317	1318	1319	1320	1321
1322	1323	1324	1325	1326
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1332	1333	1334	1335	1336
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1357	1358	1359	1360	1361
1362	1363	1364	1365	1366
1367	1368	1369	1370	1371
1372	1373	1374	1375	1376
1377	1378	1379	1380	1381
1382	1383	1384	1385	1386
1387	1388	1389	1390	1391
1392	1393	1394	1395	1396
1397	1398	1399	1400	1401
1402	1403	1404	1405	1406
1407	1408	1409	1410	1411
1412	1413	1414	1415	1416
1417	1418	1419	1420	1421
1422	1423	1424	1425	1426
1427	1428	1429	1430	1431
1432	1433	1434	1435	1436
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1442	1443	1444	1445	1446
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1452	1453	1454	1455	1456
1457	1458	1459	1460	1461
1462	1463	1464	1465	1466
1467	1468	1469	1470	1471
1472	1473	1474	1475	1476
1477	1478	1479	1480	1481
1482	1483	1484	1485	1486
1487	1488	1489	1490	1491
1492	1493	1494	1495	1496
1497	1498	1499	1500	1501
1502	1503	1504	1505	1506
1507	1508	1509	1510	1511
1512	1513	1514	1515	1516
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1582	1583	1584	1585	1586
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1592	1593	1594	1595	1596
1597	1598	1599	1600	1601
1602	1603	1604	1605	1606
1607	1608	1609	1610	1611
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1672	1673	1674	1675	1676
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1742	1743	1744	1745	1746
1747	1748	1749	1750	1751
1752	1753	1754	1755	1756
1757	1758	1759	1760	1761
1762	1763	1764	1765	1766
1767	1768	1769	1770	1771
1772	1773	1774	1775	1776
1777	1778	1779	1780	1781
1782	1783	1784	1785	1786
1787	1788	1789	1790	1791
1792	1793	1794	1795	1796
1797	1798	1799	1800	1801
1802	1803	1804	1805	1806
1807	1808	1809	1810	1811
1812	1813	1814	1815	1816
1817	1818	1819	1820	1821
1822	1823	1824	1825	1826
1827	1828	1829	1830	1831
1832	1833	1834	1835	1836
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1842	1843	1844	1845	1846
1847	1848	1849	1850	1851
1852	1853	1854	1855	1856
1857	1858	1859	1860	1861
1862	1863	1864	1865	1866
1867	1868	1869	1870	1871
1872	1873	1874	1875	1876
1877	1878	1879	1880	1881
1882	1883	1884	1885	1886
1887	1888	1889	1890	1891
1892	1893	1894	1895	1896
1897	1898	1899	1900	1901
1902	1903	1904	1905	1906
1907	1908	1909	1910	1911
1912	1913	1914	1915	1916
1917	1918	1919	1920	1921
1922	1923	1924	1925	1926
1927	1928	1929	1930	1931
1932	1933	1934	1935	1936
1937	1938	1939	1940	1941
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1997	1998	1999	2000	2001
2002	2003	2004	2005	2006
2007	2008	2009	2010	2011
2012	2013	2014	2015	2016
2017	2018	2019	2020	2021
2022	2023	2024	2025	2026
2027	2028	2029	2030	2031
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2037	2038	2039	2040	2041
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2072	2073	2074	2075	2076
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2082	2083	2084	2085	2086
2087	2088	2089	2090	2091
2092	2093	2094	2095	2096
2097	2098	2099	2100	21

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1082		% of participants female	-.0009	.01
1083		Adult's vs children	-.18	.30
1084		Measure time-point	.006	.006
1085		Number of sessions	.05	.04
1086		ITT analyses	-.30	.33
1087		Mode of delivery		
1088		Health-care professional	-1.14 [†]	.56
1089		Self-administered	-.32	.33
1090		Self-administered with support	.17	.46
1091		Trained facilitator	.38	.26
1092		Intervention setting		
1093		Medical	-1.14 [†]	.56
1094		Educational	-.10	.32
1095		Academic	.34	.28
1096		Hosted online vs In person	-.23	.29
1097		Active vs non-active control	.26	.30
1098		Individual vs group delivery	-.08	.31
1099		Student sample	.18	.30
1100		Attrition	.009	.001
1101				
1102	HO (<i>k</i> = 36)	Age	-.002	.002
1103		Sleep disturbance	-.14	.17
1104		GAD participants	-.12	.09
1105		% of participants female	-.002	.002
1106		Adult's vs children	-.02	.10
1107		Measure time-point	-.0005	.0003
1108		Number of sessions	-.0007	.01
1109		ITT analyses	-.19	.08
1110		Mode of delivery		
1111		Health-care professional	.12	.15
1112		Self-administered	.18*	.07
1113		Self-administered with support	-.14 [†]	.08
1114		Trained facilitator	-.11	.08
1115		Intervention setting		
1116		Medical	-.08	.08
1117		Educational	.14	.08
1118		Academic	-.05	.07
1119		Hosted online vs In person	.001	.07
1120		Active vs non-active control	-.02	.07
1121		Individual vs group delivery	-.16	.07
1122		Student sample	.07	.09
1123		Attrition	-.002	.002
1124				
1125	HB (<i>k</i> = 21)	Age	-.004	.004
1126		Sleep disturbance	-.13	.11
1127		GAD participants	-.11	.12
1128		% of participants female	-.005	.004
1129		Adult's vs children	.10	.25
1130		Measure time-point	-.001**	.0003
1131		Number of sessions	.02	.02
1132		ITT analyses	.05	.12
1133		Mode of delivery		
1134		Health-care professional	—	—
1135		Self-administered	.26*	.09
1136		Self-administered with support	-.17	.12

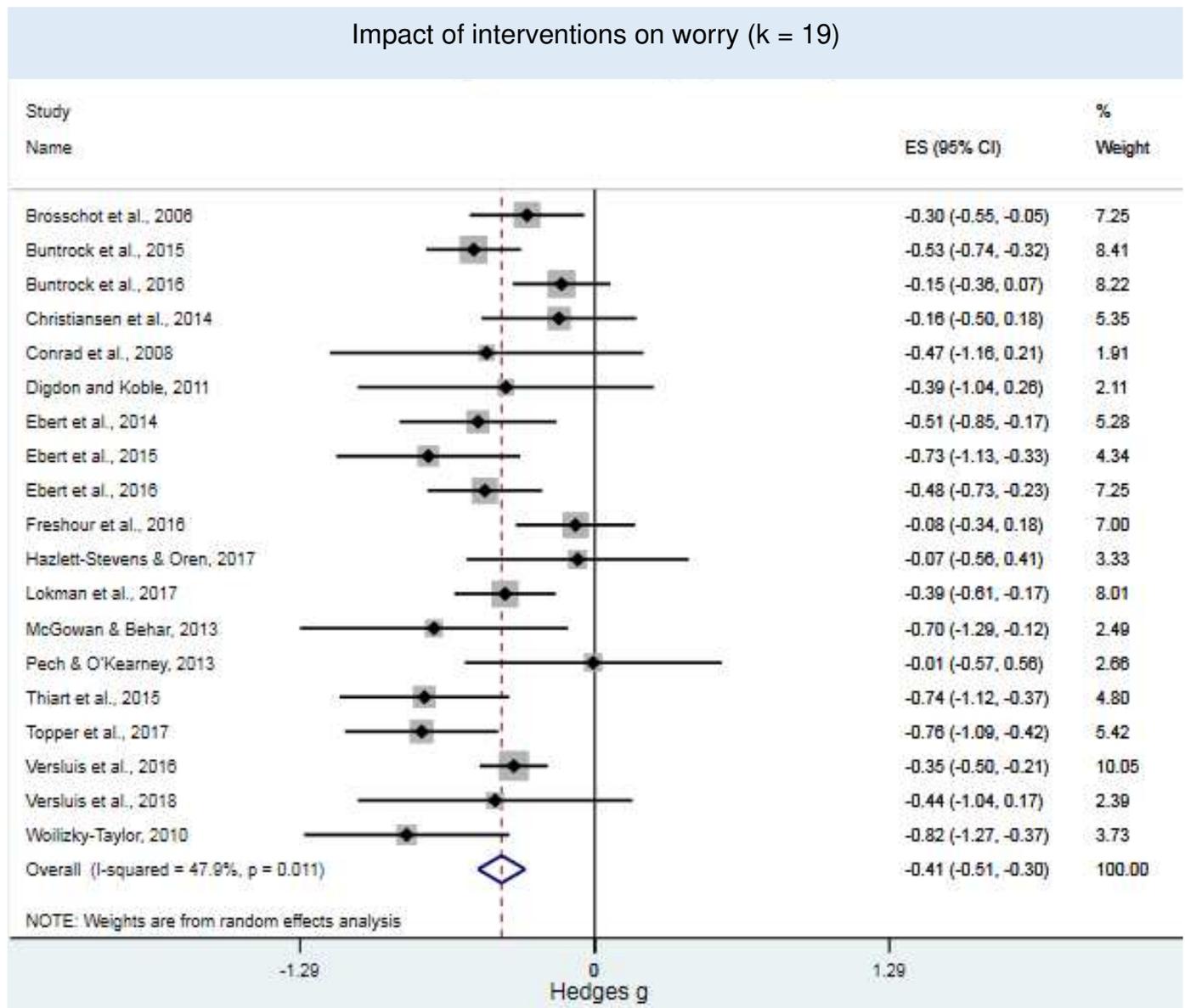
PERSEVERATIVE COGNITION ON HEALTH

1137		Trained facilitator	-.15	.14
1138		Intervention setting		
1139		Medical	-.20	.18
1140		Educational	.22	.15
1141		Academic	-.02	.13
1142		Hosted online vs In person	.10	.12
1143		Active vs non-active control	-.08	.12
1144		Individual vs group delivery	-.25*	.12
1145		Student sample	-.19	.13
1146		Attrition	.002	.004
1147				
1148	PHO ($k = 21$)	Age	-.002	.003
1149		Sleep disturbance	.009	.006
1150		GAD participants	.002	.003
1151		% participants female	-.001	.001
1152		Adult's vs children	-.15	.11
1153		Measure time-point	.0003	.005
1154		Number of sessions	-.002	.001
1155		ITT analyses	-.02	.001
1156		Mode of delivery		
1157		Health-care professional	.16	.13
1158		Self-administered	.09	.09
1159		Self-administered with support	-.14	.09
1160		Trained facilitator	-.05	.10
1161		Intervention setting		
1162		Medical	.01	.10
1163		Educational	.19**	.07
1164		Academic	-.17*	.08
1165		Hosted online vs In person	-.13	.09
1166		Active vs non-active control	.04	.09
1167		Individual vs group delivery	.002	.09
1168		Student sample	-.01	.12
1169		Attrition	-.004	.002
1170				

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

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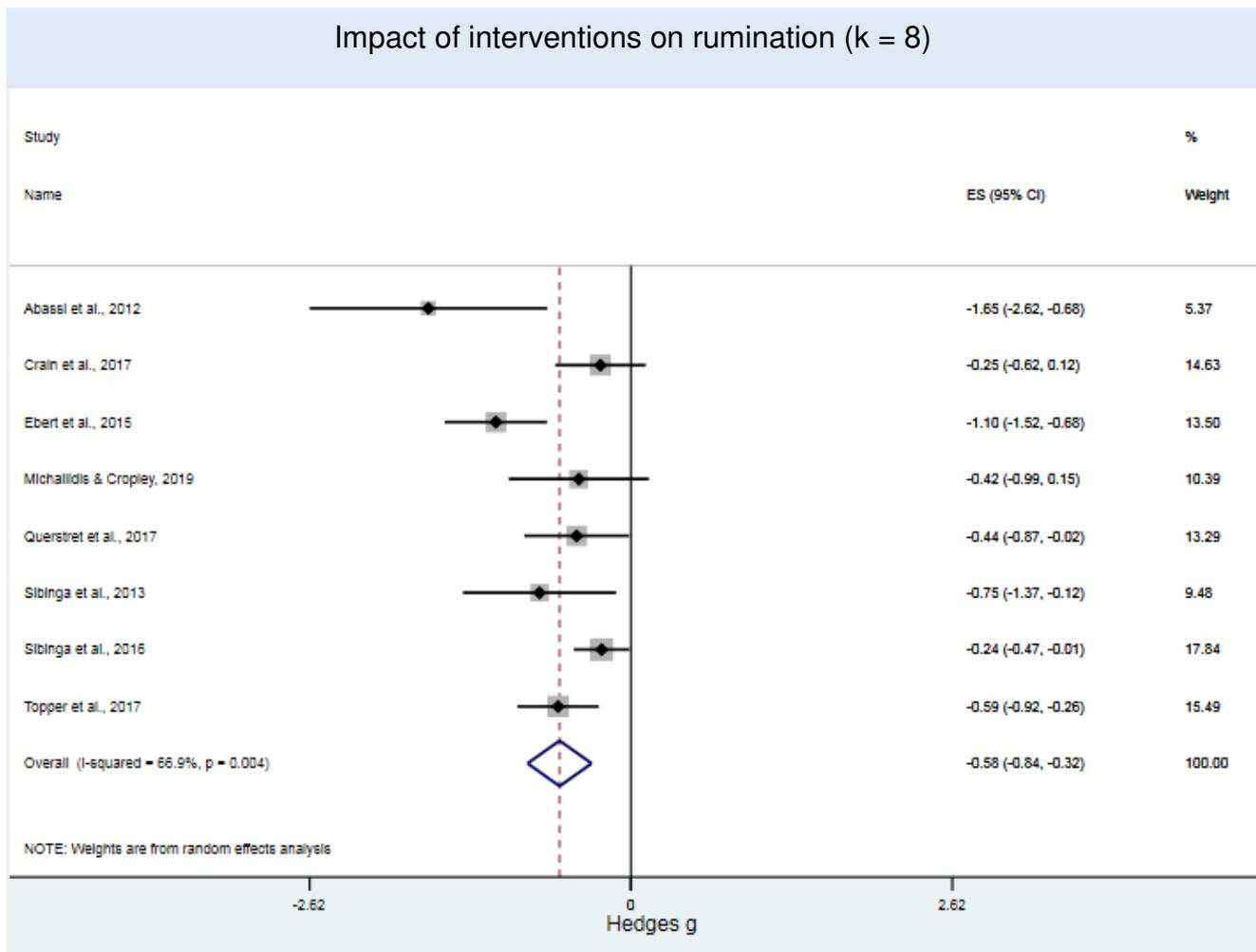
1192 *Figure 1. Worry forest plot.*



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1195 *Figure 2.* Rumination forest plot.



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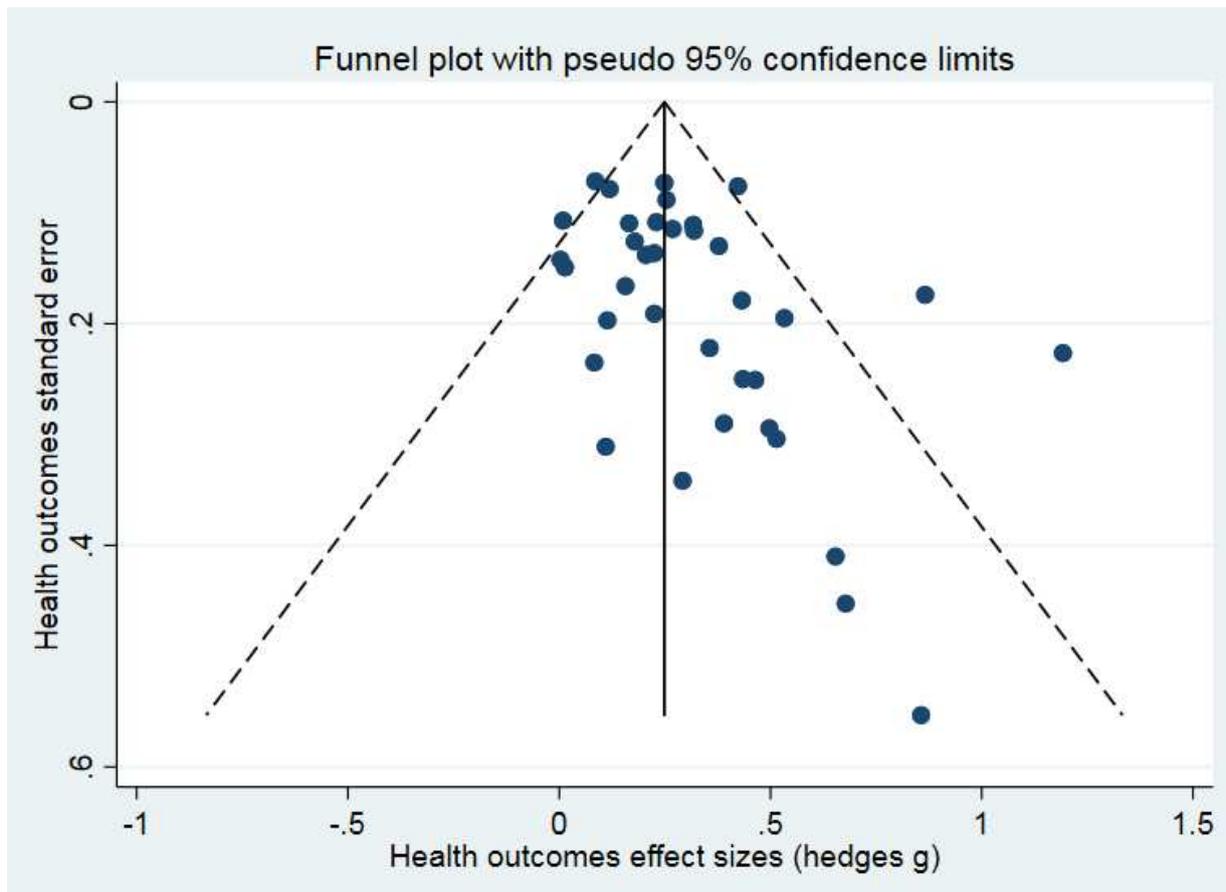
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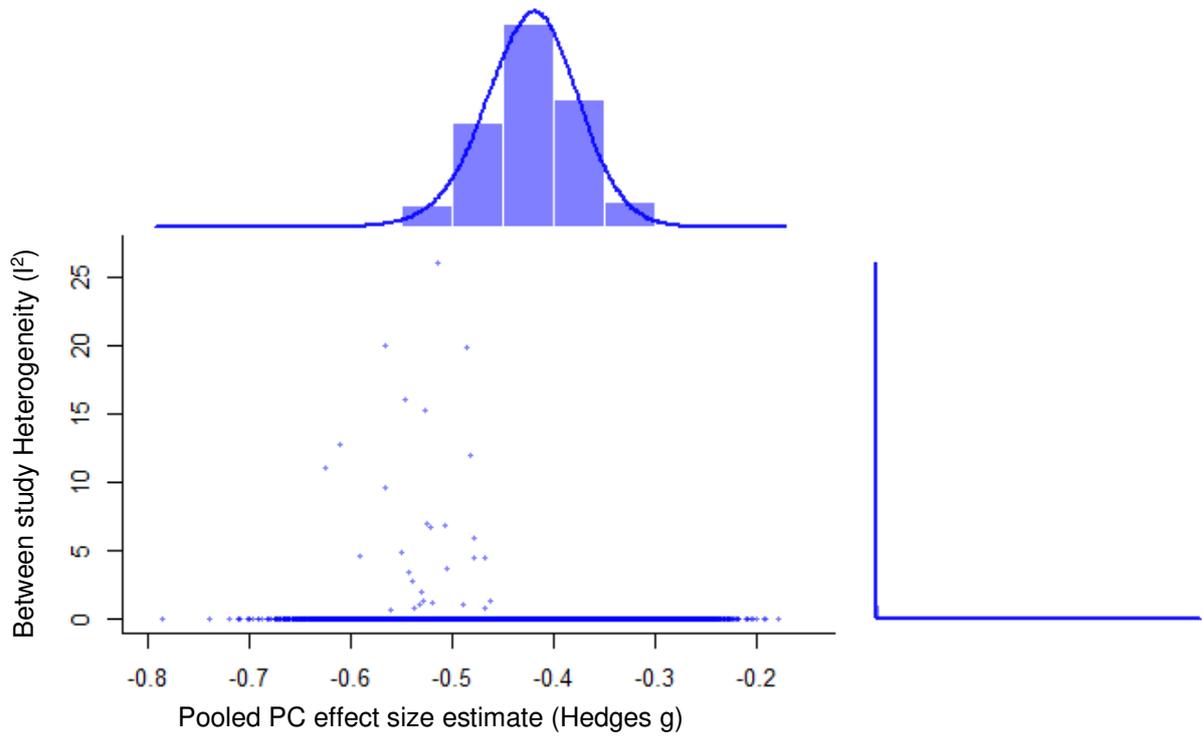
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1205 *Figure 3. Funnel plot for health outcomes.*



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1222 *Figure 4.* Graphical Display of Heterogeneity (GOSH) plot with PC effect sizes as a function
1223 of between-study heterogeneity across all studies ($k = 36$).



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1242 *Search Terms*

- 1243 1 Ruminat*.mp.
- 1244 2 (Ruminat* and (thought* or thinking)).mp.
- 1245 3 (perseverative and (thought* or thinking or cognition*)).mp.
- 1246 4 (Repetitive and (thought* or thinking)).mp.
- 1247 5 (Intrusive and (thought* or thinking)).mp
- 1248 6 worr*.mp.
- 1249 7 (Stress* and (thought* or thinking)).mp
- 1250 8 (Self referential and (thought* or thinking)).mp.
- 1251 9 brooding.mp.
- 1252 10 reflection.mp.
- 1253 11 (obsessive and (thought* or thinking)).mp
- 1254 12 unconscious stress*.mp.
- 1255 13 implicit stress*.mp.
- 1256 14 anticipat* stress*.mp.
- 1257 15 cognitive intrusion*.mp.
- 1258 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
- 1259 17 intervention*.tw.
- 1260 18 random*.tw.
- 1261 19 17 or 18
- 1262 20 16 and 19
- 1263 21 limit 20 to (English language and human)
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1426 Throughout, all appropriate statistical assumptions and graphical checks were met across
1427 these tests and no assumptions were found to be violated. The GOSH plot analysis revealed
1428 that although heterogeneity was high, the calculated effect sizes for PC represent a
1429 consistent distribution across all possible random sub-sets of the studies in this review with
1430 no significant sub-clusters present in the data (see OSM 1, Figure 4). Furthermore, when
1431 the HKSJ method was used to calculate effect sizes due to significant heterogeneity within
1432 the analyses the effects from all primary analyses (using Hedges' g) were upheld (a
1433 summary is available from the lead author upon request).

1434 **B. Confounding assessments**

1435 To identify potential confounds that may have affected the conclusions, chi-square analyses
1436 and Pearson's correlations were conducted to examine whether pairs of significant
1437 moderators co-occurred. When significant moderators co-occurred, they were entered
1438 simultaneously as predictors in multivariate meta-regressions to determine whether or not
1439 any predictor explained significant unique variance in effect size outcomes. For clarity, and
1440 to understand the context in which these tests were run, all analyses that aimed to identify
1441 potential confounds between study variables are reported in the appropriate 'objective' sub-
1442 section below.

1443 **Objective 1b:**

1444 *Study characteristics associated with greater effect sizes for PC.*

1445 In the main report, one study characteristic was associated with larger effect sizes for PC:
1446 studies testing interventions delivered by healthcare professionals generated larger effect
1447 sizes than studies testing interventions not delivered by healthcare professionals, $B = 0.39$,
1448 $S.E. = 0.18$, $CI = -0.77 - -.009$, $p = .045$. As no other moderator significantly predicted PC
1449 (see OSM1, Table 4), no further analyses were conducted.

1450 **Objective 2a**

1451 *Can interventions targeting PC also impact health outcomes?*

1452 In the main report, we report the effect of the interventions targeting PC on health behaviour
1453 and physical health outcomes separately. Here, and wherever the term *overall* is used, we

1454 report the effect of these interventions on a combined outcome (health behaviours + physical
1455 health outcomes – health *overall*).

1456 The interventions produced, on average, small, but significant and heterogeneous $I^2 =$
1457 48.1%; $Q(35) = 67.45$ $p < .001$, effect sizes for health outcomes overall $g = 0.28$, 95% CI =
1458 0.21 to 0.34 ($k = 36$).

1459 **Objective 2b**

1460 Study characteristics associated with larger effect sizes for Health Overall.

1461 As above, we repeated the analyses that were conducted separately for health behaviour
1462 and physical health outcomes and reported in the main text such that we test the association
1463 between study characteristics and for health overall.

1464 These analyses revealed that all intervention types had a significant positive effect on health
1465 overall with the exception of pain management strategies. The effect sizes in studies testing
1466 psychological detachment style interventions were larger than in studies testing different
1467 types of interventions, for health overall, $B = 0.30$, $S.E. = 0.18$, $CI = 0.07 - 0.54$, $p = .014$.

1468 For health overall, interventions were significantly more effective at yielding larger effect
1469 sizes in studies where content was self-administered $B = 0.18$, $S.E. = 0.07$, $CI = -0.04 -$
1470 0.31 , $p = .013$, as opposed to those in which content was delivered by a health-care
1471 professional, $B = 0.12$, $S.E. = 0.15$, $CI = -0.18 - 0.42$, $p = .415$, or a trained facilitator, $B = -$
1472 0.11 , $S.E. = 0.08$, $CI = -0.28 - 0.06$, $p = .196$.

1473 Study characteristics associated with larger effect sizes for Health Behaviours.

1474 Further to the main report of: Effect sizes were significantly larger when interventions were
1475 self-administered $B = 0.26$, $S.E. = 0.09$, $CI = 0.07 - 0.45$, $p = .01$, delivered at an individual
1476 level rather than group-level, $B = -0.25$, $S.E. = 0.11$, $CI = -0.49 - 0.006$, $p = .045$, and when
1477 health behaviours were assessed closer to the conclusion of an intervention $B = -0.001$, $S.E.$
1478 $= .0003$, $CI = -.002 - -.0003$, $p = .01$ ($k = 21$). Given these moderators co-occurred, we ran
1479 further analyses to test for confounding. Accordingly, self-administered interventions tended
1480 to be delivered to individuals, $\chi^2(1) = 11.08$, $p < .001$, and self-administered interventions
1481 tended to have shorter follow-ups, $r = -.60$, $p < .001$. In subsequent multivariate meta-

1482 regressions to account for these potential confounds, self-administered interventions
 1483 marginally predicted health behaviour effect sizes when controlling for group/individual
 1484 delivery format, $B = 0.20$, $S.E. = 0.10$, $CI = -0.02 - 0.43$, $p = .05$, but not after controlling for
 1485 time-point, $B = 0.16$, $S.E. = 0.13$, $CI = -0.12 - 0.42$, $p = .237$. Neither group/individual
 1486 delivery format, $B = 0.002$, $S.E. = 0.09$, $CI = -0.18 - 0.19$, $p = .979$ or measure time point, B
 1487 $= -0.0006$, $S.E. = 0.0005$, $CI = -0.0018 - 0.0005$, $p = .24$, explained unique variance in
 1488 health behaviour effect sizes, thus suggesting some evidence of confounding.

1489 *Study characteristics associated with larger effect sizes for Physical Health Outcomes*

1490 Further to the main report of: while no particular intervention type was related to significantly
 1491 larger effect sizes for physical health outcomes, interventions were at their most effective
 1492 when delivered in educational, $B = 0.19$, $S.E. = 0.07$, $CI = 0.48 - 0.32$, $p = .01$, and
 1493 academic settings, $B = -0.17$, $S.E. = 0.08$, $CI = -0.35 - 0.06$, $p = .043$, as opposed to
 1494 delivered in medical settings, $B = 0.009$, $S.E. = 0.10$, $CI = -0.19 - 0.21$, $p = .919$. We did not,
 1495 however, conduct further tests to detect confounding as it was not theoretically possible for a
 1496 study to be conducted in more than one setting and because no other moderators co-
 1497 occurred.

1498 **Objective 3:**

1499 *Are larger effect sizes for PC associated with larger, but positive, effect sizes for health*
 1500 *overall?*

1501 There was a non-significant trend regarding the association between PC effect sizes health
 1502 outcomes overall *effect sizes*, $B = -0.21$, $S.E. = 0.11$, $CI = -0.43 - 0.02$, $p = .067$ ($k = 36$, see
 1503 Table 1). However, following the removal of two studies identified as multivariate influential
 1504 cases (Magnan et al., 2014 & Thiar et al., 2015), medium-sized effects for PC, $g = .41$, were
 1505 associated with small, but positive, $g = .25$, effect sizes for health overall, $B = -0.25$, $S.E. =$
 1506 0.09 , $CI = -0.44 - -0.07$, $p = .008$ ($k = 34$). This effect was upheld in subsequent permutation
 1507 tests with 10,000 random computations, $B = -0.36$, $S.E. = 0.21$, $CI = -0.78 - 0.05$, $p = .038$.
 1508 Larger effect sizes for worry, $B = -0.46$, $S.E. = 0.21$, $CI = -0.92 - 0.09$, $p = .054$ ($k = 14$), and
 1509 rumination, $B = -0.71$, $S.E. = 0.27$, $CI = -1.58 - 0.15$, $p = .062$ ($k = 5$), specifically, were

1510 marginally associated with larger effects for health behaviours, with a $g = .41$ for worry
 1511 corresponding with a $g = .27$ in health behaviours, and a $g = .56$ in rumination corresponding
 1512 with a $g = .38$ in health behaviours.

1513 Effect sizes for worry, $B = -0.38$, $S.E. = 0.22$, $CI = -0.83 - 0.08$, $p = .091$ ($k = 19$), and
 1514 rumination ($k = 8$), $B = -0.43$, $S.E. = 0.21$, $CI = -0.94 - 0.07$, $p = .081$, were not significantly
 1515 associated with effect sizes for health *overall*.

1516 **C. Sensitivity analyses for two studies using proxy measures for health**
 1517 **behaviours.**

1518 Given two of the included studies interested in health behaviour (Christiansen et al., 2014;
 1519 Aardoom et al., 2016) used measures (AUDIT & EDE-Q, respectively) incorporating items
 1520 relevant to both health behaviours *and* determinants of health behaviours within a single
 1521 index (i.e., proxy measures, while all other related studies only included behavioural items),
 1522 we removed these two studies in an additional sensitivity analysis to ensure this feature did
 1523 not influence any of the conclusions.

1524 The findings reported in the main manuscript were upheld. The interventions, on average,
 1525 led to a small-to-medium, and heterogeneous $I^2 = 48.8\%$; $Q(33) = 64.39$ $p < .001$, effect
 1526 sizes for health behaviours, $g = 0.29$, 95% CI 0.22 to 0.36 ($k = 34$) and effects for PC were
 1527 only marginally associated with positive effect sizes for health behaviours, $B = -0.20$, $S.E. =$
 1528 0.10 , $CI = -0.40 - 0.007$, $p = .058$. Thus, suggesting the inclusion of these two studies had
 1529 no meaningful impact on the study objectives relating to health behaviours.

1530 **D. Accounting for the potential impact of reverse causality between PC and**
 1531 **health.**

1532 To minimize the potential impact of reverse causality between PC and health (i.e.
 1533 intervention content first influencing health before being captured within measures of PC),
 1534 studies measuring PC *immediately* post-intervention and then health at a *later* point in time,
 1535 were subject to additional tests. This sub-set of studies ($k = 18$, 50%) were subject to a
 1536 separate meta-regression examining if effect sizes for PC were positively, and significantly,
 1537 associated with effect sizes for health outcomes (overall), to control for this possibility. As an

1538 extra precaution, PC effect sizes for these 18 studies were also directly compared via a
 1539 Welches *t*-test to the remainder of studies which simultaneously measured PC *and* health
 1540 either immediately post-intervention ($k = 15$, 41%), or within follow-up measures ($k = 3$, 9%),
 1541 to detect if they significantly differed depending on the point in time in which they were
 1542 collected post-intervention. Note, we did not run this separately for health behaviours and
 1543 physical health outcomes due to power concerns.

1544 The sub-group meta-regression comprising studies measuring PC *immediately* post-
 1545 intervention, and health outcomes (overall) later ($k = 18$, 50%), revealed effect sizes for PC
 1546 significantly predicted more positive health effect sizes, $B = -0.36$, $S.E. = 0.15$, $CI = -0.67 - -$
 1547 0.04 , $p = .031$, denoting lower levels of PC in the intervention condition versus the control.
 1548 Furthermore, a Welches two-sample *t*-test comparing this sub-set of studies to those which
 1549 measured PC *and* health at the same point in time ($k = 18$), indicated that PC effect sizes did
 1550 not significantly differ between the two sub-sets of studies as a function of time, $t(36) = -.31$,
 1551 $p = .371$. Deviations in PC that occurred following the delivery of an intervention package are
 1552 thus unlikely to have been driven by effects for health outcomes and do not differ across the
 1553 period of time in which all post-intervention measures were collected.

1554 **E. Analyses relating to Sleep**

1555 Additional analyses were conducted for the most common health outcome (sleep, $k = 17$).
 1556 The interventions produced, on average, small-medium and non-heterogeneous $I^2 = 8.1\%$;
 1557 $Q(16) = 4.49$ $p = .997$, effect sizes for sleep, $g = 0.30$, 95% CI = 0.11 to 0.49 ($k = 17$). Effect
 1558 for PC, $B = -0.21$, $S.E. = 0.11$, $CI = -0.52 - 0.04$, $p = .022$, and worry specifically, $B = -0.76$,
 1559 $S.E. = 0.28$, $CI = -1.41 - -0.11$, $p = .027$, but not rumination, $B = -0.62$, $S.E. = 0.34$, $CI = -$
 1560 $.172 - -0.47$, $p = .167$, were positively associated with parameters of sleep (i.e., total-sleep-
 1561 time/sleep-onset-latency) (see, Table 1). In addition, the effect sizes in studies testing
 1562 psychological detachment style interventions were larger than in studies testing different
 1563 types of interventions for sleep, $B = 0.35$, $S.E. = 0.11$, $CI = .109 - .583$, $p < .001$.
 1564 Studies which included a measure of sleep, versus those which did not, were entered as an
 1565 additional moderator to assess if larger effect sizes were associated with this intervention

1566 feature. However, there was no evidence to suggest studies which measured sleep yielded
1567 larger effect sizes on behaviour when compared to all other studies, $B = -0.10$, $S.E. = 0.09$,
1568 $CI = -.30 - .10$, $p = .309$ ($k = 36$). Furthermore, psychological detachment interventions
1569 generated significantly larger effect sizes for studies testing this type of intervention within
1570 measures of sleep versus those studies testing other types of intervention, $B = 0.35$, $S.E. =$
1571 0.11 , $CI = .109 - .583$, $p < .001$.

1572 **F. Testing for small study bias**

1573 Small-study bias, whereby larger effect sizes tend to be reported within smaller sample
1574 sizes, was examined using Egger's test. Duval and Tweedie's (2000) trim and fill analysis
1575 was conducted to estimate the impact of publication bias on PC and health outcome effect
1576 sizes.

1577 Egger's regression coefficient was non-significant for PC ($p = .087$) but was significant for
1578 health outcomes (overall) ($p = .022$) suggesting small study bias for the latter. Thus, Duval
1579 and Tweedie's (2000) trim and fill analysis imputed nine additional effect sizes for the effect
1580 of the interventions on health outcomes (overall) (see OSM 1, Figure 3), generating an
1581 overall effect size of $g = .22$ ($95\%CI = 0.13 - 0.30$). Consequently, the effect of the
1582 interventions on PC and health outcomes remained significant after controlling for small-
1583 study bias.

1584 **G. Potential impact of studies with multiple study arms.**

1585 We took extra steps to control for potential selection-bias when an alternative study arm was
1586 available (e.g., Topper et al., 2017; internet vs. group-based therapy). There were six studies
1587 whereby more than one study arm was available to choose from as the 'treatment' arm. For
1588 the 5 intervention arms, we first prioritized the intervention arm authors hypothesized to
1589 produce greatest effect sizes in PC (this was the case for 4/5 of studies). In one case when
1590 this was not reported, as we were interested in the most effective methods at influencing PC,
1591 we chose the arm which yielded the largest effect size in PC. Only 1 study required us to
1592 make a choice between comparator arms. In this one instance (Versluis et al., 2018), we
1593 followed the conservative approach of selecting the attention-placebo control, as it is well

1594 known that effect sizes of interventions compared with no-treatment control groups are
1595 greater than effect sizes of interventions compared to attention-placebo control groups
1596 (Lipsey & Wilson, 1993).

1597 To control for the potential inflation of effect-sizes we ran a further sensitivity
1598 analysis; first, via a series of meta-regressions with the feature of 'more than one
1599 intervention arm present' set as the predictor and study effect sizes for PC, health
1600 behaviours and physical health outcomes as the DV. Importantly, effect sizes for PC, $B =$
1601 0.18 , $S.E. = 0.12$, $CI = -0.05 - 0.42$, $p = .124$, health behaviours, $B = -0.10$, $S.E. = 0.09$, CI
1602 $= -0.21 - -0.82$, $p = .309$, and physical health outcomes, $B = -0.07$, $S.E. = 0.16$, $CI = -0.31 -$
1603 -0.71 , $p = .317$, were unrelated to the number of intervention arms a study employed.
1604 Second, we conducted sensitivity analyses in which these studies that included more than
1605 one 'treatment arm' were removed from the meta-analyses. The impact on the conclusions
1606 was negligible for PC: $g = -0.42$, 95% $CI = -0.51$ to -0.21 ; health behaviours: $g = 0.32$, 95%
1607 $CI 0.24 - 0.49$, and physical health outcomes: $g = 0.22$, 95% $CI = 0.22 - 0.31$. Crucially,
1608 these analyses shows our handling of intervention arms did not bias this reviews
1609 conclusions.

1610 **H. Assessing potential impact of baseline differences between study arms**

1611 To control for the possibility that baseline differences between study conditions influenced
1612 effect sizes, we carried out two further tests. Seven studies reported significant baseline
1613 differences in PC and two studies reported significant baseline differences in health
1614 outcomes. First, a univariate meta-regression, with reported vs. non-reported baseline
1615 differences set as the predictor and effect sizes for PC, health behaviours and physical
1616 health outcomes, respectively set as the DV, was carried out. Study effect sizes for PC, $B =$
1617 0.11 , $S.E. = 0.23$, $CI = -0.12 - 0.32$, $p = .271$, health behaviours, $B = -0.12$, $S.E. = 0.14$, CI
1618 $= -0.52 - 0.24$, $p = .159$, and physical health outcomes, $B = -0.09$, $S.E. = 0.19$, $CI = -0.49 -$
1619 0.19 , $p = .347$, were unrelated to the presence of baseline differences among studies.
1620 Second, we conducted sensitivity analyses in which these studies that reported baseline
1621 differences on specific measures were removed from the meta-analyses. The impact on the

1622 conclusions was minimal (PC: $g = -0.41$, 95% $CI = -0.54$ to -0.25 ; health behaviours: $g =$
1623 0.30 , 95% $CI = 0.19 - 0.40$; physical health outcomes: $g = 0.22$, 95% $CI = 0.12 - 0.29$).

1624 Therefore, we can be fairly confident that any degree of baseline between-condition
1625 difference did not meaningfully impact any of our analyses which rest upon this assumption.

1626 **I. Examining the potential impact of clinical differences in participant**
1627 **characteristics.**

1628 To control for the possibility that clinical baseline heterogeneity between studies which either
1629 contained GAD participants (Conrad et al., 2008 & Freshour et al., 2016; $N = 2$) or pertained
1630 participants with sleep disturbance (Sandlund et al., 2018; Pech & O'Kearney, 2013;
1631 Jansson-Frojmark et al., 2012; Harvey et al., 2017; $N = 4$) affected the conclusions, we
1632 carried out three further analyses.

1633 First, two sets of univariate meta-regressions (i.e., one for each sample type), with sample
1634 type (GAD sample: yes/no; sample with sleep disturbances: yes/no) set as the predictor and
1635 effect sizes for PC, health behaviours and physical health outcomes, respectively set as the
1636 DV, was carried out. Study effect sizes for PC ($B: -.26$, $S.E = .09$, $p = .204$), health
1637 behaviours ($B: -.13$, $S.E = .11$, $p = .112$) and physical health outcomes ($B: .002$, $S.E = .003$,
1638 $p = .62$) were not significantly impacted by GAD samples, and the same was true for those
1639 studies containing participants with sleep disturbances for PC: ($B: -.21$, $S.E = .18$, $p = .174$),
1640 health behaviours ($B: -.13$, $S.E = .11$, $p = .403$), and physical health outcomes ($B: .009$, $S.E$
1641 $= .003$, $p = .405$).

1642 Second, we conducted two sensitivity analyses in which the studies that had GAD
1643 participants, or those with 'clinical' sleep disturbances, were removed from the respective
1644 meta-analyses. The impact on the conclusions was minimal (PC: $g = -0.41$, 95% $CI = -0.57$
1645 to -0.29 ; health behaviours: $g = 0.29$, 95% $CI = 0.17 - 0.39$; physical health outcomes: $g =$
1646 0.21 , 95% $CI = 0.15 - 0.31$) when removing the 2 studies including GAD participants, and
1647 similar effects were found when (separately) removing the 4 studies comprising participants
1648 with sleep disturbances (PC: $g = -0.40$, 95% $CI = -0.54$ to -0.33 ; health behaviours: $g =$
1649 0.30 , 95% $CI = 0.20 - 0.38$; physical health outcomes: $g = 0.23$, 95% $CI = 0.13 - 0.30$).

1650 Therefore, we can be fairly confident that any degree of baseline between-condition
1651 difference did not meaningfully impact any of our conclusions.

1652 Third, to assess if these samples had any impact on objective 3 (i.e., the association
1653 between PC and health) we re-analysed with the 6 studies (2 GAD studies & 4 sleep
1654 studies) removed; along with any influential cases relevant to either outcome removed to be
1655 consistent with the main report. The impact of removing these 6 studies (and one influential
1656 case, Magnan et al., 2014) on the findings was minimal. Medium-sized effects for PC, $g =$
1657 $.39$, were still associated with a small, but positive, $g = .24$, effect for health behaviours, $B =$
1658 0.22 , $S.E. = 0.13$, $CI = -0.47 - -0.11$, $p = .028$. A similar trend was present with physical
1659 health outcomes when compared to our original analysis. Effect sizes for PC were still
1660 unrelated to effect sizes for physical health $B = -0.15$, $S.E. = 0.21$, $CI = -0.58 - 0.17$, $p =$
1661 $.328$, when removing these 6 studies (and an influential case, Digdon & Koble, 2011), $B =$
1662 0.16 , $S.E. = 0.08$, $CI = -0.56 - 0.19$, $p = .292$. As such, combined, these three sets of
1663 analyses show that we can be fairly certain that while sample characteristics are important to
1664 consider, they had very little bearing on the findings of this particular meta-analysis.

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OSM 3: Risk of Bias Traffic Light Plot

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Study	Risk of bias domains									Overall
	D1	D2	D3	D4	D5	D6	D7	D8	D9	
Aardoom 2016	+	+	+	-	X	-	-	-	-	-
Abassi 2012	+	X	X	+	X	X	+	-	-	X
Brosschot 2016	+	X	X	-	X	-	-	-	+	-
Buntrock 2015	+	+	+	+	+	+	X	X	+	+
Buntrock 2016	+	+	+	+	+	+	X	X	+	+
Carney and Waters 2006	+	-	-	+	+	+	-	-	X	-
Christiansen 2014	+	-	-	-	-	-	X	X	-	-
Conrad 2008	+	-	-	-	-	-	+	-	+	-
Crain 2017	-	-	-	-	-	-	-	-	-	-
Digdon and Koble2011	+	+	+	X	X	X	X	X	+	X
Ebert 2014	+	-	-	-	-	-	-	-	+	-
Ebert 2015	+	-	-	-	-	-	-	-	-	-
Ebert 2016	+	+	-	+	+	+	X	X	-	-
Ellenberg 2016	+	X	X	+	+	+	X	X	-	-
Freshour 2016	+	+	+	+	+	-	-	-	-	+
Harvey 2017	+	+	+	+	+	+	-	-	+	+
Hazlett-Stevens and Oren 2017	+	X	X	-	-	-	-	-	+	-
Jansson-Frojmark 2012	+	-	-	-	-	-	-	-	-	-
Jellesma 2009	-	+	+	-	-	-	-	-	+	-
Lokman 2017	+	+	+	X	X	X	X	X	-	X
Magan 2014	-	-	-	-	-	-	-	-	-	-
McGowan and Behar 2013	+	-	-	+	-	-	+	-	-	-
Mehlsen 2017	+	+	+	X	X	X	X	X	-	-
Michailidis and Cropley2019	+	+	+	+	+	+	+	+	+	+
Pech and Oksarney 2013	+	+	+	-	-	-	-	-	+	-
Peters 2017	-	-	X	-	-	-	-	-	-	-
Querstret 2017	+	+	+	+	X	X	+	+	+	+
Sandlund 2018	+	+	+	X	X	X	X	X	+	-
Sibinga 2013	+	+	-	+	+	-	+	-	-	-
Sibinga 2016	+	-	-	+	-	-	-	-	-	-
Telsmann 2014	+	-	-	-	-	-	-	-	-	-
Thiart 2015	+	X	X	X	X	X	X	X	+	X
Topper 2017	+	-	X	-	-	-	-	-	-	-
Versluis 2016	+	-	-	-	-	-	-	-	-	-
Versluis 2018	+	+	+	X	X	X	X	X	-	-
Wollizky-Taylor 2010	+	-	-	-	-	-	-	-	-	-

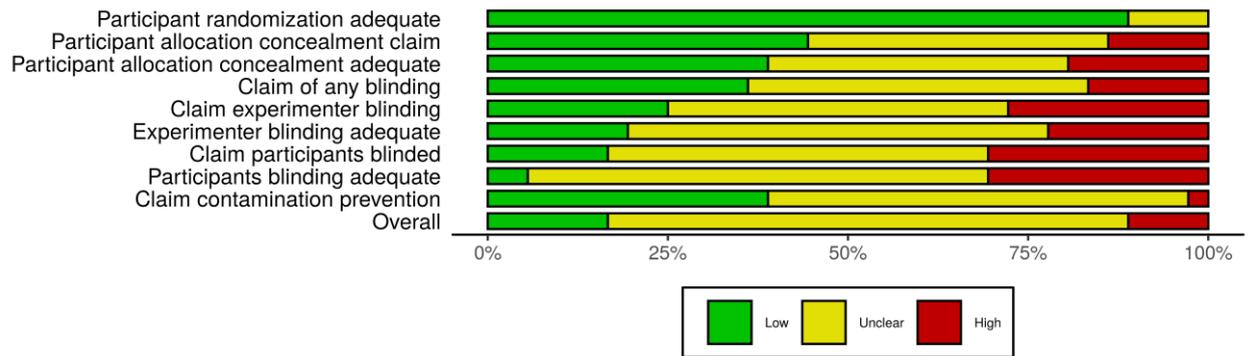
D1: Participant randomization adequate
 D2: Participant allocation concealment claim
 D3: Participant allocation concealment adequate
 D4: Claim of any blinding
 D5: Claim experimenter blinding
 D6: Experimenter blinding adequate
 D7: Claim participants blinded
 D8: Participants blinding adequate
 D9: Claim contamination prevention

Judgement
 High
 Unclear
 Low

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OSM 4: Weighted Risk of Bias Plot

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Pre-print