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Effects of worry postponement on daily worry and sleep: a randomised controlled trial

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ABSTRACT

Objective: Previous studies have shown that perseverative, worrisome thoughts are prospectively related to poor sleep outcomes and that worry postponement interventions may be effective in reducing worry. However, their effectiveness for improving sleep outcomes is unknown and they have not been tested over a period longer than 7 days. The current study investigated the effects of a worry postponement intervention, alongside a worry postponement + planning intervention (augmented condition) against active and non-active control conditions on daily worry and sleep outcomes.

Methods and Measures: A four-armed (online) randomised controlled trial (RCT) was conducted using an interval-contingent design, where self-report measures of worry each night (duration & frequency for that day) and sleep (sleep onset latency; number of awakenings, sleep quality) each morning (for the previous night) were collected within 186 participants (M^{age} = 30.34; SD=7.89) across 14 days.

Results: Participants in the augmented arm reported significantly lower worry duration (by ~15 min), relative to the standard worry postponement arm alone. However, the intervention arms did not produce significant improvements in any of the sleep outcomes, relative to the control groups.

Conclusion: Creating specific 'if-then' plans for when and how to engage in a worry postponement can produce favourable outcomes for worry reduction; however, future studies are needed to unpick how to best translate these effects into positive outcomes for sleep.

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Introduction

Data from the United Kingdom suggest an alarming 74% of adolescents have reported feeling stressed, worried, or anxious in recent years, to the extent that they have felt overwhelmed or unable to cope (Mental Health Foundation, 2018). Worry, defined as a 'chain of thoughts and images, that are negatively affect-laden and relatively uncontrollable' (Borkovec et al., 1983, p. 10), has long been studied as a cardinal

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aetiological element in several psychopathological conditions and as an aspect of everyday life (e.g. Hawkes et al., 2009; Meyer, 1908). For example, worry is a key feature in generalised anxiety disorder (GAD), post-traumatic stress disorder (PTSD), and depressive disorders (Borkovec, Robinson, et al., 1983; Chelminski & Zimmerman, 2003). Importantly, there is also empirical evidence showing that worry is a significant determinant of heightened physiological activity and neuroendocrine dysregulation (Brosschot et al., 2006; O'Connor, Thayer & Vedhara, 2021; for review see, Ottaviani, 2018) and, more recently, studies have linked worry to the enactment of poorer health behaviours, including adopting a less balanced diet, poorer sleep, job strain, and increased alcohol consumption (Clancy et al., 2022; Cropley et al., 2012; Eschle & McCarrick, 2021; Frone, 2015; McCarrick et al., 2024). It is well established that these negative health behaviours are related to illness (Suris & Parera, 2005), disease, and morbidity rates (Burke & Hutchins, 2007) in both adults and children globally (for review, see Mackenbach et al., 2014).

A conceptual model now exists—the Perseverative Cognition Hypothesis – which portrays how worry can have a detrimental impact upon health. Central to this hypothesis is the notion that perseverative cognition (PC), the cognitive representation of past stressful events (rumination) or feared future events (worry), is a key mediator through which psychosocial stress leads to negative health outcomes. PC is thought to prolong the physiological activation beyond the presence of a direct stressor, and that this prolongation of the stress response can lead to health problems (see, Brosschot et al., 2005). This is a concern, given that prolonged physiological activity carries severe health risks; for example, prolonged increases in heart rate has been shown to be predictive of coronary heart disease and cardiovascular death (Fisher & Newman, 2013; Kubzansky et al., 1997; Palatini & Julius, 1997). Further, Ottaviani et al. (2018) reviewed compelling neuroimaging data suggesting that heart rate variability (HRV) reduction from pre- to post-induction of PC is associated with both structural and functional brain abnormalities. PC was shown to lead to impaired prefrontal inhibitory control over subcortical structures (e.g. diminished prefrontal amygdala functional connectivity). More recent meta-analyses provide further support. In a review of 24 studies, examining 55 effect-sizes on the relationship between PC and HRV, small to medium sized associations were reported, thus further documenting how PC (through worry and rumination) can both generate psychophysiological stress before and after exposure to stressors (Lim et al., 2024). As such, there is growing and obvious need for interventions that aim to prevent the onset and experience of PC due to its function as a transdiagnostic risk factor for both psychological and somatic health.

A recent meta-analysis of 36 randomised controlled trials that aimed to reduce PC, while subsequently measuring health outcomes, revealed that psychological interventions (relative to control groups), on average, produced medium-sized effects on rumination ($g = -0.58$),[1] small-to-medium sized effects on worry ($g = -0.41$) and health behaviours ($g = .31$), and small-sized effects on physical health outcomes ($g = .23$) (McCarrick et al., 2021). Crucially, these findings suggest PC can be influenced by briefly delivered psychological (non-pharmacological) interventions and that these treatment methods also hold beneficial downstream health effects. Of the interventions reviewed the most common was 'PC action plans', an approach focusing on

proactively planning to deal with PC before encountering it; importantly, 'PC action plans' were responsible for significant reductions in the frequency of worry ($g = -0.40$). While there was some diversity in the practical implementation of this technique, the most prominent approach, coined 'worry postponement' (for example, see Brosschot & Van Der Doef, 2006), aims to confine the negative consequences of stress—attenuated through worry—within a restricted time period reserved for such negative thoughts, rather than them being freely dispersed throughout the day (see, Brosschot et al., 2005). In turn, it is argued that the negative physiological and neuroendocrine dysregulation, as well as the unhealthier behaviours that are associated to worry, will be equally confined to the same window of time (i.e. the 20–30 min worry window). These brief, inexpensive, psychological interventions serve as a tool to control PC, particularly in contexts where more conventual psychological therapies cannot be implemented due to resource or time restrictions.

Despite the reported effectiveness of worry postponement interventions, there are, however, ostensible issues that must not be overlooked. First, from a perspective of temporal validity, worry postponement interventions have not been tested over a period of more than 7 days, which is significant given worry episodes in those clinically affected are known to be temporally sensitive (Mackintosh et al., 2021). Second, there are methodological limitations related to measurement strategies adopted in previous studies; change scores (i.e. baseline > follow-up) do not account for daily within-person variation in worry (and health behaviours). Third, there are issues related to adherence to protocol and to high attrition rates (e.g. Jellesma et al., 2009; Versluis et al., 2016). Each of these issues are important for the prospective design and implementation of interventions containing 'postponement' features that are built on this evidence base. Therefore, as worry postponement techniques have shown some promise in previous studies, further exploration of this method, combined with novel ways to boost adherence over an extended timeframe (e.g. techniques that include methods such as planning) with a more precise methodological lens (e.g. utilising daily-diary designs), are both timely and warranted.

One solution, to boost intervention adherence, may be represented by a promising behaviour change technique called implementation intentions (Gollwitzer, 1993). Implementation intentions involve individuals identifying appropriate cues for action (e.g. time of day, particular TV program) and response (intended action) and linking them in the form of an if-then plan (If I encounter X then I will do Y). A robust body of evidence shows that implementation intentions are an effective strategy for bridging the well-established gap between intentions and behaviour (i.e. the intention-behaviour gap; see Godin et al., 2005 for a review). Moreover, implementation intentions have been shown to be an effective approach to overcome worry-related self-regulatory problems. For example, in social anxiety, implementation intentions have been shown to remove attentional biases towards social threat words and to lead to rapid disengagement from threatening stimuli (Webb et al., 2010). Therefore, this strategy could be paired with worry postponement to increase the likelihood that individuals engage in worry postponement. By enhancing the likelihood of engaging in worry postponement, there is greater likelihood that worry postponement would reduce worry and, in turn, outcomes associated with worry.

To our knowledge, implementation intentions and methods of worry postponement are yet to be combined within a randomised controlled trial. Therefore, given worry is a central facet of the PC Hypothesis, and considering the health-risks it poses, there is a need for innovative interventions that aim to reduce worry that may also improve health behaviours. Sleep, in particular, is an important health behaviour due to its role regulating physiological repair and recuperation processes (Buysse, 2014; Carskadon & Dement, 2005). Indeed, people who sleep less are more likely to report a poorer quality of life (Groeger et al., 2004), are at higher risk of cardiovascular disorders (Broström et al., 2004) and ongoing sleep deficiency has been associated to kidney disease, high blood pressure, diabetes, and stroke (Stenholm et al., 2010, 2011). The properties of worry make it a key mediator between stress and sleep quality (Brosschot et al., 2007), principally this is because of worry's ability to prolong the psychological experience of stress (i.e. with 'what-if' self-assertions) and due to the frequently reported presence of worrying before bedtime (c.f., Pillai & Drake, 2015). Consequently, a direct attempt to attenuate worry may indirectly also positively impact upon sleep and sleep quality.

Therefore, in this study, we tested the relative effectiveness of two worry postponement interventions: an 'augmented' worry postponement intervention (i.e. including implementation intentions), and a 'standard' worry postponement arm, compared to two control arms (active & non-active control) at reducing (state-level) worry (duration & frequency) and improving sleep parameters over a 14-day period in an (online) randomised controlled trial. We tested the following hypotheses:

> The intervention arms, combined, will outperform both of the control conditions; such that participants in the augmented and standard worry postponement arms will report, on average, significantly lower levels of worry (Hypothesis 1A; H1A, relative to an active-control), and significantly improved sleep outcomes (Hypothesis 1B; H1B, relative to control groups combined).

The augmented worry postponement arm will outperform the standard worry postponement arm; such that participants in the augmented condition will report, on average, significantly lower levels of worry (Hypothesis 2A; H2A), and significantly improved sleep outcomes (Hypothesis 2B; H2B).

Method

Design

A four-armed (online) randomised controlled trial (RCT), conducted between September 2021 and December 2021, was used to compare standard and 'augmented' worry postponement interventions (i.e. a standard worry postponement arm and an augmented worry postponement arm, including implementation intentions) to two control arms (active and non-active control). We chose to include two control arms as, in typical worry postponement trials, control groups typically have an 'active' role, in that participants are either asked to complete worry diaries or itemised questionnaires. Given this task has been shown to trigger further worry (see, Carney & Waters, 2006), we also included a non-active control group in view of having a 'pure' control condition, whose participants were solely required to complete daily measures of sleep.

An interval-contingent design was utilised, where self-report measures of worry each night (duration & frequency for that day) and sleep (onset latency; number of awakenings, quality) each morning (for the previous night) were collected across the trial period. End-of-day/following morning diaries, rather than event contingent diaries were used to reduce participant burden and to help maintain adherence to the diary protocol (Broderick & Stone, 2006; Tennen et al., 2006). The study was preregistered on AsPredicted (No.79783).

The survey was hosted on Qualtrics (2022). In addition, for the sleep measures only (across all arms), text messages were sent to participants on each morning of the study prompting their response. The timing of these texts was pre-determined based on the question '*what time do you usually wake up?*'; the texts were then scheduled within 30 min of this waking time. Text prompts were not used for the worry measures as this would violate the hypotheses testing the adherence aspect of the 'augmented' worry arm.

Participants and procedure

Participants were recruited *via* an online purposeful sample using email distribution, social media and adverts on university webpages. A power calculation (in G*Power version 3.1, Faul et al., 2009) revealed 252 participants were required (in total, post attrition, across groups; 63 in each arm) to detect a moderate change (effect size of $f = .21$ (based on the association between worry and sleep; see, McCarrick et al., 2021)) at post-treatment based on a power of .80 in a two-tailed test with alpha set at .05. This calculation was based on comparisons between all 4 arms of the intervention *via* a one-way omnibus analysis of variance.[2]

The interventions were evaluated in adults (18 & older) who answered 'yes' to both of the following screening questions: (i) 'Do you find yourself worrying a lot?; (ii) 'Does your worry keep you awake at night?'. These questions were used in view of recruiting individuals who concurrently experienced issues with worry and sleep (see, Digdon & Koble, 2011, for previous validation).

Participants who took sleep medication were not excluded from the study but were requested to keep their medication constant during the study period (e.g. Querstret & Cropley, 2013). Participants who presented with either chronic medical conditions (e.g. cardiovascular disease, musculoskeletal disorders, neurological disorders), who reported medically diagnosed insomnia, sleep paralysis or chronic sleep apnoea, or who have a new-born baby, were excluded from the study as these have been shown to prevent or negatively impact sleep (Parish, 2009; Van den Bulck, 2004). Participants were required to own a mobile phone at the time of the study to participate.

All eligible participants were first directed to Qualtrics (2022) where, following providing informed consent, they were enrolled onto the study. Qualtrics (2022) 'Randomizer' performed the randomisation process, randomly allocating participants into one of the four arms (1:1:1:1) at the 'block' level, in order to reduce potential selection bias and to ensure methodologically adequate allocation concealment; as such, the researchers and participants were blinded to allocation. Specifically, participants were not informed of their group, nor were they aware that other arms exist,

but instead were told they were taking part in a study investigating the general sleep quality of adults for the purposes of designing a prospective intervention to improve sleep. Participants were told that they would be required to provide responses every day for the next 14 days (on measures specific to their study arm). At this stage, participants in the active arms were asked to make note of a specific hyperlink that would take them to their worry diary and save it in the notes section of their phone. When completing the diary, participants were asked to provide a consistent and unique codeword for which they could be identified. Upon completion of the study, participants were compensated with a £5 gift voucher, were informed of the aims of the study and provided with documents detailing how they can utilise the worry postponement techniques. Leaking of study information was highly unlikely given this study was hosted online and participants never met one another or knew who else was participating in the trial. The precise contents of each arm and the procedural differences are described below. The study was approved by institutional ethical procedures (PSYC-310).

Intervention arms

Worry postponement (WP)

This arm of the intervention aimed to replicate the classic worry postponement intervention put forward by Brosschot et al. (2006). Participants were thus provided the same instructions afforded to those in the original Brosschot study of postponing their worrying every time they realise they are doing so to a special 20–30min period in the early evening (4–5 h before sleep). As such, participants were first provided with a description of what worrying is (e.g. 'Worrying involves thinking about a subject that has or can have *negative* consequences for *yourself*, and for which there is *no, or not yet, a solution*. Worrying often, but not always, consists of a *chain* of negative thoughts, about the same or different topics') and then instructed: *'every time you realise that you are worrying, terminate them right away, and 'postpone' them to this special period that you may reserve for worrying'.* Participants received no further instruction for the timing or content of this special period.

Augmented worry postponement (AWP)

The augmented arm adopted the same central feature of worry postponement as described above, but as specific goal-orientated plans have been consistently shown to promote greater automated goal-directed responses (see Gollwitzer & Sheeran, 2006), implementation intentions (i.e. an 'if-then' plan) were utilised within this group as a supplement to the standard worry postponement approach. Accordingly, the first aspect of instructions to participants were identical to the worry postponement arm. They were instructed: : *'every time you realise that you are worrying, terminate them right away, and "postpone" them to this special period'.* However, unlike the worry postponement arm, participants were then told to form a specific plan framed to aid their adherence in executing this instruction.

Next, participants were asked to highlight an activity they routinely engage around 4–5 h before bed and to use the period to trigger the onset of their special

period of worry reflection. This took the form of an if-then plan (i.e. IF X happens, THEN I will do Y). As an initial aid, participants were provided examples such as: IF I begin to worry, THEN I will begin my worry window shortly after: '*watching 9 pm news', 'putting the kids in bed', 'eating supper'* or '*reading my favourite book'* (to identify scenarios that they frequently engage in). Thus, the THEN aspect of the plan served to trigger engagement in the special worry period. Participants were also asked if this period/activity would be appropriate for every night of the week and, in instances where participants report otherwise, an alternative plan was requested. As it served as the trigger for the initiation of a participants worry window, it was important that the THEN aspect of the plan was an activity or experience that was as close to habitual to the participant as possible, Once agreed, participants were instructed to complete the following checklist to ensure their plans meet the criteria for an implementation intention: (a) does your plan(s) contain the words IF, THEN and I?; (b) does your plan(s) identify ample situations for you to do 30 min of worry reflection every night of the week?; (c) does your plan(s) identify how you will undertake worry postponement in the situations identified in your plans? Participant must have answered yes to all responses to proceed with the study. Table 1 comprises example IF THEN plans made by participants in this study.

Active control (A-C)

The active control arm aimed to replicate the control group from the original Brosschot et al. (2006) study to provide a neutral reference point for the direct comparison(s) between the relative efficacy of the augmented arm compared to, and combined with, the standard worry postponement arm found in the original Brosschot et al. (2006) study. In this group, participants were asked to register their worry (in their

Table 1. Examples of Implementation Intentions set by participants in the AWP arm.

Activity	Implementation Intention (IF THEN plan)
Brushing Teeth	'If I find myself worrying, then I will pause and save it for my worry window after brushing my teeth'
Folding Laundry	'If I worry, then I will save it for my worry window after folding up my laundry'.
Drinking evening tea	'If and when I begin to worry, then I will postpone thinking about it until I sit down with my cup of tea during my worry period tonight'.
Reading a book	'If I worry, then I will postpone thinking about my worries until I have finished my evening reading later today.
Feeding a pet	'If I find myself worrying, then I will postpone any focus on this worry until I have fed the dog its evening meal'.
Exercise	'If I worry or begin feeling anxious, then I will not think about it again until I have finished evening stretches tonight'.
Housework	'If I find myself worrying or start stressing out, then I will pause and save it for my worry window after I have done the post-dinner pot washing'.
Lighting candles	'If I feel any worrisome thoughts or emotions, then I will only engage in thinking about them after I have lit my evening candles post-dinner'.
Checking weather forecast	'If I notice myself worrying, then I will postpone these worries until I have checked the evening weather forecast later this evening'.
Listening to music	'If I worry, then I will save it for my worry window following my evening music session'.
Making lunch for the next day	'If I find myself worrying or feel any type of negative thought, then I will pause and save it for my worry period which I will engage in after making my lunch (for the next day) later this evening'
Watching the news	'If I worry or feel anxious, then I will not think about it until after the 6 pm news'.

online diary) on each night of the study. They were instructed: *'On each night of this study, please provide an estimate on the total number and total frequency of your worries today via your worry diary'.* These participants were not asked to postpone their worries and no implementation intentions were made.

Non-Active control (NA-C)

Completing a nightly worry diary may confound the accuracy of worry recall, or worse, result in further worries (for example, see Szabó & Lovibond, 2006). Therefore, a fourth arm was incorporated in view of providing a 'pure' control. In this group, participants were only asked to provide an estimate of their sleep parameters on each morning of the study *via* text message. These participants were not asked to postpone their worries and no implementation intentions were made. As a way of masking the intervention, participants were instructed *'on each morning of this study, please provide as close to as possible your best estimate of your previous night's sleep quality'* and were asked to complete the battery of sleep questions outlined below.

Measures

Worry

An online worry diary was used in the three active arms (i.e. augmented worry postponement, standard worry postponement, active-control) to record the total duration and frequency of worry episodes experienced (retrospectively) that day. The questions asked to participants were as follows: '*How many times did you find yourself worrying today?*' and '*How long did these periods of worry, on average, last for?*'. Responses were requested in minutes. The worry diary used in this study was replicated from the original Brosschot et al. (2006) study and has since been validated in more recent studies both in terms of its content validity and sensitivity to change (e.g. Clancy, O'Connor & Prestwich, 2020; Fisher et al., 2017; Narmandakh et al., 2021).

Sleep

The following items were extracted from the Consensus Sleep Diary (Carney et al., 2012), Time in Bed: '*At what time did you get into bed last night?*; Sleep Onset Latency (SOL): *How long do you think it took you to fall asleep?*; Number of awakenings (NA): '*How many times did you wake up during the night?;* Sleep Quality (SQ): '*On a scale of 1–5, how would you rate the overall quality of last night's sleep?'* (1 = very poor to 5 = very good). This instrument has been used widely, and has been extensively validated in a variety of sleep trials and recent research (e.g. Dietch & Taylor, 2021; Jungquist et al., 2015) and has robust psychometric properties (see, Maich, Lachowski & Carney, 2018; O'Connor et al., 2025). These components of sleep were selected as a recent meta-analysis found they were reliably associated with worry (Clancy et al., 2020).

Statistical analysis

Chi-squared tests and analysis of variance (ANOVA) were used to test for differences between baseline characteristics (Table 2) and ANOVAs to test for differences between

Table 2. Means (SD) of baseline characteristics across study conditions.

	AWP ($n = 37$)	WP ($n = 39$)	A-C ($n = 42$)	NA-C ($n = 66$)	Test, p, η^2 or φ
Age (SD)	30.43 (7.77)	29.97 (7.72)	30.95 (8.26)	30.03 (7.81)	$F_{(3, 180)} = 0.15$. $p = .931$, $\eta^2 = .002$
% Female	65%	62%	62%	63%	χ^2 (1) 0.06, $p = .801$, φ = .01
BMI (SD)	23.16 (9.49)	24.27 (9.10)	24.22 (10.01)	24.18 (10.98)	$F_{(3, 180)} = 0.11$. $p = .955$, $\eta^2 = .002$
% from UK and Ireland	70%	71.%	73%	70%	χ^2 (1) 0.03, $p = .858$, φ = .01
% White Ethnicity	82%	81%	81%	82%	χ^2 (1) 0.003, $p = .956$, φ = .003
Average units of alcohol consumed per week	19.21	18.39	19.06	18.89	χ^2 (1) 0.014, $p = .906$, φ = .013
% of current smokers	21%	19%	20%	17%	χ^2 (1) 0.02, $p = .891$, φ = .01

Note: WP = worry postponement; AWP = augmented worry postponement; A-C = active control; NA-C = non-active control. Z = percentage contract coefficient based on NA-C as the base. Effect sizes are η^2 and φ.

Table 3. Means (SD) of (level 1) daily study variables across the study period ($n = 184$).

	Total sample	AWP ($n = 37$)	WP ($n = 39$)	A-C ($n = 42$)	NA-C ($n = 66$)	Test, p, η^2	Post-Hoc
Worry Duration	40.40 (34.42)	29.82 (27.04)	47.53 (38.32)	43.13 (33.35)	N/A	$F_{(2, 115)} = 2.76$, $p = .059$, $\eta^2 = .048$	
Worry Frequency	18.60 (13.35)	16.40 (12.68)	23.83 (15.09)	13.40 (7.10)	N/A	$F_{(2, 115)} = 8.042$, $p < .001$, $\eta^2 = .12$	WP > AWP AWP = A-C WP > A-C
Sleep Onset Latency	25.88 (31.03)	35.03 (34.19)	32.10 (26.89)	34.98 (29.18)	39.23 (32.88)	$F_{(3, 180)} = 0.47$. $p = .700$, .008	
No. of awakenings	6.73 (2.01)	6.61 (2.12)	6.83 (2.08)	6.82 (1.91)	6.70 (1.96)	$F_{(3, 180)} = 0.11$. $p = .951$, $\eta^2 = .002$	
Sleep Quality	3.27 (1.07)	3.27 (1.11)	3.39 (1.07)	3.18 (1.01)	3.27 (1.04)	$F_{(3, 180)} = 0.27$. $p = .847$, $\eta^2 = .004$	

Note: WP = worry postponement; AWP = augmented worry postponement; WP = worry postponement; A-C = active control; NA-C = non-active control; N/A as not measured in this arm. These values reflect the winsorized data. Effect sizes are η^2.

daily study variables (Table 3). The primary outcome was variability in worry duration and frequency across the 14-day study period; parameters of sleep (SOL; SQ; SD) were assessed as a secondary outcome. Responses were analysed in Stata 18 and SPSS28. These data were analysed using generalised estimating equations (GEE) models to account for within subject component (times) and the between component (study arm). Moreover, we used GEE as we were interested in the average effect of our predictors (i.e. experimental conditions) on our outcomes and because we were analysing correlated, repeated measures data. Frequency data (number of times reporting worry or waking) negative binomial model were estimated and for continuous data (sleep onset latency and worry variables) OLS models. The intervention arms were dummy coded (i.e. each reference group coded as 0). Outliers were dealt with in several ways according to the variable properties. For sleep onset latency, the data ranged from 4 min to just under 9 h for one participant, then 7 h for another and then 6 h, with a mean of 39 min. This distribution was positively skewed (*Z* for skew = 4.68/0.054) with a long tail. In line with the pre-registration, as the extremely high values were very likely implausible, symmetrical 90th percentile winsorization was applied (see Wilcox, 2005). Sleep quality was marginally negatively skewed (*Z* for skew = −0.24/0.054). For the frequency of daily worry, the range of data was from 0 to 100. For daily worry duration, the data ranged from 0 to 240 with a modal value of 20 min analysed using negative binomial regression. These are all plausible values. The minimum number of days completed was 1, the maximum was 14 and the median was six. Eighty-six participants completed all 14 days. As long as a minimum of 3 days of data were available,

we included all of the participant data in the main analyses. This is because recent Monte-Carlo studies show that more reliable estimates are obtained if all data is included (Arend & Schäfer, 2019)[3]. Moreover, as an additional check, we ran all the analyses using data from participants who completed 7 days as well as 3 years. The results did not significantly differ. We also explored whether there were group differences in completion rates and found that the completion rate was low for the active control compared to the intervention arms (all p values <.001), so more people than you would expect by chance stayed in the study longer than 3 days or 7 days if they are in the active arms. Therefore, the intervention keeps more people actively engaged in the worry interventions than then active control. In addition, for the main analyses relating to each of the hypotheses, age and gender were entered into the models as confounders. Finally, exploratory multilevel mediation analysis was conducted using MPlus to test indirect effects of any significant intervention condition (X) on sleep outcome (Y) through relevant worry variable/s (M).

Results

Descriptive statistics

A total of 265 participants were enrolled onto the study, completed baseline measurements, were (block) randomised to condition, and provided their informed consent; fulfilling the power requirements set out by the a-priori calculation. However, 79 participants were excluded from the study by not providing a minimum of 3 days data (for worry variables) across the 14-day worry period (WP: $n = 28$; AWP: $n = 31$; A-C: $n = 11$; NA-C: $n = 9$). Consequently, the final sample comprised 186 participants (WP: $n = 39$; AWP: $n = 37$; A-C: $n = 42$; NA-C: $n = 66$) who were aged, on average, 30.34 years (SD= 7.89) (63% female; 71.24% from United Kingdom & Ireland; 81.52% of white ethnicity; mean BMI = 23.95 (SD = 9.89)). Table 2 displays the baseline participant characteristics across the four study arms and shows there were no significant differences between the conditions. Table 3 shows the means and SDs for the study variables across the study period, and Table 1 presents the types of implementation intentions made by study participants in the augmented worry postponement arm.

Hypothesis 1: The intervention arms, combined, will produce significantly lower worry scores, relative to the active–control (Hypothesis 1A) and produce significantly superior scores on sleep outcomes, relative to control groups combined (Hypothesis 1B).

Worry (Hypothesis 1 A)

The augmented worry postponement and standard worry postponement arms (combined) did not significantly differ, relative to the active-control, on worry duration or on worry frequency and Neither age nor gender significantly impacted these findings (see Table 1, Panel B & D).

Sleep (Hypothesis 1B)

The augmented worry postponement and standard worry postponement arms (combined) did not engender any significant improvements in the sleep variables, relative

to the control groups (combined), in terms of sleep quality, sleep onset latency, or number of awakenings frequency. either age nor gender significantly influenced the findings for sleep quality or number of awakenings frequency. There was no significant impact of age on the findings for sleep onset latency, however a significant effect for gender was present (see Table 4, Panel B, D & F).

Hypothesis 2: The augmented worry postponement arm will outperform the standard worry postponement arm; such that participants in the augmented condition will score, on average, significantly lower on worry (Hypothesis 2A), and significantly superior on sleep outcomes (Hypothesis 2B).

Worry (Hypothesis 2 A)

The augmented worry postponement arm yielded significantly lower worry duration across the study period compared to the standard worry postponement arm. The regression coefficient reflects an average reduction in worry duration of just over 14 min per day (reported by participants in the augmented worry arm), relative to the standard worry postponement arm. These findings did not significantly change when accounting for age or gender (see Table 5, Panel A).

For hypothesis 2 A, for worry frequency, the standard worry postponement arm significantly increased the frequency of worries across the study period, relative to the augmented worry postponement arm (by ~ half a worry episode). Neither age nor gender significantly impacted these findings (see Table 5, Panel C).

Sleep (Hypothesis 2B)

The augmented worry postponement arm did not yield any significant improvements, relative to the standard worry postponement arm, in terms of sleep quality, sleep onset latency, or number of awakenings frequency. Neither age nor gender significantly impacted the findings for sleep quality, number of awakenings frequency. There was no significant impact of age on the findings for sleep onset latency, however a significant effect for gender was present (see Table 4, Panel A, C & E).

Exploratory multilevel mediation analysis

Multilevel mediation analysis was conducted to test the indirect effects of the augmented worry postponement arm versus the standard worry postponement arm (X) on each of the sleep outcomes (Y) through worry duration (M). The results of these analyses found no evidence for mediation on any of the outcomes (all p values > 0.15).

Discussion

The aim of the present study was to test the relative effectiveness of two worry postponement interventions; an 'augmented' worry postponement arm, featuring implementation intentions, and a 'standard' worry postponement intervention, compared to an active-control arm at reducing worry (duration & frequency) and improving

Table 4. Panel analysis for the effects of interventions on sleep variables.

		Sleep Quality **Panel A**		
	B (SE)	*p*	95% CI Lower	95% CI Upper
Intervention Arm				
WP	−0.06 (0.12)	.28	−0.11	0.38
AWP	0.043 (0.13)	.73	0.38	0.29
Demography				
Gender	−0.07 (0.09)	.45	−0.25	0.11
Age	0.01 (0.01)	.99	−0.01	0.01
Constant	3.26 (3.26)	<.001***	2.90	3.68
n Groups (*n*), M-days	184 (2089), 11.4			
		Panel B		
Intervention Arm				
Combined Arms	0.11 (0.09)	.22	−0.07	0.29
Demography				
Gender	−0.07 (0.09)	.45	−0.25	−0.25
Age	−0.01 (0.01)	.98	−0.01	0.01
Constant	3.24 (0.19)	<.001***	2.87	3.61
n Groups (*n*), M-days	184 (2089), 11.4			
		Sleep Onset Latency **Panel C**		
Intervention Arm				
WP	−7.01 (4.27)	.10	−15.38	1.37
AWP	−2.892 (4.332)	.504	−11.38	5.60
Demography				
Gender	7.27 (3.24)	.02	0.92	13.62
Age	0.16 (0.20)	.43	−0.23	0.55
Constant	30.47 (6.66)	<.001***	17.42	43.52
n Groups (*n*), R^2	184 (2046), 11.1			
		Panel D		
Intervention Arm				
Combined Arms	−3.54 (3.18)	.27	−9.79	2.69
Demography				
Gender	7.52 (3.25)	.021	1.14	13.9
Age	0.15 (0.20)	.45	−0.24	0.54
Constant	29.01 (6.55)	<.001***	16.25	41.92
n Groups (*n*), R^2	184 (2044), 11.2			
		Number of awakenings **Panel E**		
Intervention Arm				
WP	0.02 (0.13)	.85	−0.23	0.28
AWP	−0.03 (0.13)	.83	−0.29	0.23
Demography				
Gender	−0.01 (0.10)	.92	−0.20	−0.20
Age	−0.01 (0.01)	.95	0.95	0.01
Constant	1.93 (0.20)	<.001***	1.53	2.32
n Groups (*n*), R^2	184 (2089), 11.4			
		Panel F		
Intervention Arm				
Combined Arms	−0.10 (0.10)	.94	−0.19	0.18
Demography				
Gender	−0.01 (0.10)	.92	−0.20	0.18
Age	−0.01 (0.01)	.95	−0.01	0.01
Constant	1.93 (0.20)	<.001***	1.55	2.32
n Groups (*n*), R^2	184 (2089), 11.4			

Note. *$p < .05$, **$p < .01$ ***$p < .001$; For panels B, D & F, the active and non-active control (combined) is the reference arm, for panels A, C & E the interventions are tested against each other, Gender (0 = Men, 1 = Women). WP = worry postponement; AWP = augmented worry postponement. Confidence intervals are 95% upper and lower.

Table 5. Panel analysis for effects of interventions on worry variables.

	Worry			
	Duration **Panel A**			
	B (SE)	*p*	95% CI Lower	95% CI Upper
Intervention Arm				
WP	6.39 (7.32)	.38	−7.96	20.75
AWP	−14.83 (7.57)	.05*	−29.72	−0.05
Demography				
Gender	10.40 (6.08)	.09	−1.52	22.31
Age	0.28 (0.40)	.43	−0.42	0.99
Constant	32.50 (12.72)	.01*	7.57	57. 44
n Groups (*n*), M-days	91 (849), 9.3			
	Panel B			
Intervention Arm				
Combined Arms	−3.39 (7.01)	.63	−17.11	10.33
Demography				
Gender	10.58 (6.43)	.10	−2.03	23.18
Age	0.26 (0.38)	.67	−0.49	0.98
Constant	33.36 (13.43)	.01*	7.03	59.67
n Groups (*n*), M-days	91 (849), 9.3			
	Frequency **Panel C**			
Intervention Arm				
WP	0.63 (0.22)	.01**	0.19	1.07
AWP	0.20 (0.230)	.39	−0.25	0.65
Demography				
Gender	−0.01 (0.18)	.96	−0.37	0.35
Age	0.01 (0.01)	.81	−0.019	0.02
Constant	2.51 (0.39)	<.001***	1.751	3.276
n Groups (*n*), R^2	89 (846), 9.5			
	Panel D			
Intervention Arm				
Combined Arms	0.45 (0.206)	.03*	0.04	0.85
Demography				
Gender	−0.01 (0.18)	.96	−0.36	0.34
Age	0.01 (0.01)	.91	−0.02	0.02
Constant	2.55	<.001***	1.77	3.33
n Groups (*n*), R^2	89 (846), 9.5			

Note. *$p < .05$, **$p < .01$ ***$p < .001$; Active control is the reference arm, Gender (0 = Men, 1 = Women); WP = worry postponement; AWP = augmented worry postponement; A-C = active control; NA-C = non-active control.

sleep parameters (relative to active & non-active control groups) over a 14-day period in an (online) randomised controlled trial. Hypothesis 1 (A & B) received no support, as participants in the intervention arms (combined) did not experience any significant reductions in worry (H1A) (relative to the active control) or any improvements in sleep (H1B) (relative to control groups combined). The findings for hypothesis 2 A were more complex, whereas hypothesis 2B received no support. Specifically, participants in the augmented worry postponement arm experienced significantly lower worry duration (by ~15 min) across the study period, relative to the standard worry postponement arm (H2A). Furthermore, when compared to the augmented worry postponement arm, the standard worry postponement arm significantly increased the frequency of worry (by ~ half a worry episode) (H2A). Neither of the intervention arms, augmented or standard worry postponement, produced significant improvements in any of the sleep parameters (H2B). Accordingly, these preliminary set of findings shed

new light on the value of implementation intentions at enhancing traditional approaches to the management of worry, while also highlighting the growing requirement for future avenues of research to test intervention methods that not only impact worry but also engender positive changes in health behaviours, such as sleep.

Psychological interventions aiming to reduce the adverse effects of worry on health behaviours have been sparsely researched until recent years. Of the varied and pervasive health consequences attributable to worry (for review, see Ottaviani, 2018), research concerning its negative effects on health behaviours has only come to light in recent years (see, Clancy et al., 2016; McCarrick, Prestwich & O'Connor, 2024) and interventions directly targeting worry to, in turn, improve sleep are rare. A meta-analysis by McCarrick et al. (2021) found PC can be influenced by psychological interventions and that, in line with the PC Hypothesis (Brosschot et al., 2005), they may yield beneficial effects on health behaviours (e.g. sleep outcomes), with the most effective strategy being 'PC action plans'. However, several issues were raised with the technique relating to adherence barriers, limited testing periods (i.e. studies restricted to 7 days), in addition to methodological limitations underpinned by study designs that do not account for daily within-person variation in worry and that overtly rely on change scores (i.e. baseline > follow-up assessments; see McCarrick et al., 2021). Therefore, the present finding, that shows beneficial treatment effects for the augmented worry postponement arm, extends previous knowledge in three key ways: (i) *via* complementing a standalone worry postponement intervention with implementation intentions in an attempt to boost adherence, (ii) expanding the testing period to 14 days to assess robustness, and (iii) adopting a multi-level daily diary design to capture not only how worry varies within-participants, but also how these variations relate to sleep outcomes measured the previous evening.

Despite the efficacy of the augmented arm at reducing the duration of worry, it must also be discussed as to why the standalone worry postponement arm increased worry frequency. Interestingly, this finding is somewhat consistent to what other authors have reported in relation to worry frequency. For example, Brosschot and van der Doef (2006) found postponers reported lower worry duration than controls (152 min, relative to 222 min), however this effect did not significantly extend to worry frequency (23.7 mins versus 30.0 mins). Similarly, Versluis et al. (2016) reported no difference between postponers and controls at reducing worry frequency in an online trial (null effects were also present for worry duration) (see also, Mobach et al., 2019, for similar conclusions). One possibility for these findings, consistent with the present study, is that the worry postponement intervention is successful at inspiring people to think about their worry differently but is less successful at stopping worries from accumulating, which is reflected in the significant increase in worry frequency (in the WP arm), here. It seems plausible that the instruction to postpone a worry may prevent experiencing the one that is presently being experienced, however be less useful in preventing worry recurrence or the onset of new worries.

Another finding from the present study was the unanimous null effects of the interventions on sleep outcomes. Sleep onset latency, number of awakenings and subjective sleep quality were unaffected by both the interventions combined (H1B), and when tested in isolation of one another (H2B). While the true cause for these findings is not clear, one possible explanation is that while the properties of worry

make it a key mediator between stress and sleep outcomes (Brosschot et al., 2007), the change in worry was not large enough to impact sleep. Despite being contrary to the hypotheses, this is hardly surprising as dedicated sleep interventions show mixed efficacy (for meta-analysis, see Griggs et al., 2020) and effect sizes for mediation models including the indirect effects of worry on sleep are generally small (e.g. Pillai & Drake, 2015). Another explanation may be sought from the measurement approach used in this study. Sleep data was self-reported and obtained *via* text message each morning of the study, posing two complications. First, descriptive analysis of this data shows people took, on average, 28.21 min (SD = 12.14) to provide an estimation of their sleep parameters from the previous night, raising questions over the accuracy of recall. Second, while self-report methods of sleep estimation were used due to logistical implications in this study. They suffer from varying degrees of reporting error (see, Manconi et al., 2010) and have been linked to other demand characteristics such as over estimation of an idealised sleep schedule (see, Short et al., 2013) mainly because recall relates to timeframes confined to unconscious thought. It is thus a possibility that the nuances of individualised sleeping patterns were not accurately captured by the measures used in this study. Accordingly, future research should not only adopt more multifaceted intervention methods that target specific aspects of worry that may be sensitive to sleep outcomes (e.g. temporal relations and bi-directionality between worry & sleep; also see O'Connor & Rogerson, 2024), but that they do so using objective measures of sleep associated to superior ecological validity (e.g. such as Actigraphy; see, Martin & Hakim, 2011). Future research should also explore in more precise detail which aspects of worry are associated with which components of sleep and sleep quality.

The exploratory multilevel mediation analysis was conducted to test the indirect effects of any significant intervention condition on sleep outcomes through relevant worry variables, yielding non-significant results. Further research is required to build upon these findings, particularly those that consider the role of other moderating variables known to be key motivators of behaviour. Indeed, self-regulation, goal intentions, habits, and self-efficacy have all been shown to significantly improve goal action (Prestwich et al., 2015), which is significant for interventions featuring planning components. Future interventions should therefore measure the relative weighting of such variables in terms of their impact on the successful enactment of worry postponement windows, as well as their role in other indirect outcomes such as sleep.

The present study was not without its limitations. First, as noted above, this study relied entirely on online self-reported measures for all the variables examined, raising questions over measurement accuracy. Second, the analysis would have benefitted from the addition of covariates known to impact sleep, such as body mass index, smoking, alcohol use, and utilisation of sleep aids (e.g. sleep masks). Future studies should therefore aim to replicate these findings while also accounting for such variables in the analysis to consider their impact on PC and sleep. Third, additional sleep outcomes, such as total minutes awake after sleep onset (WASO), or sleep duration, should be assessed in future work to detect if the findings in this paper extend to other important aspects of sleep. Fourth, there is a chance that our screening criteria ('Do you find yourself worrying a

lot?'; 'Does your worry keep you awake at night?') were not sensitive enough to identify individuals whose sleep might benefit from the intervention. Fifth, there is some possibility the results here are under-powered, as despite recruiting the required participants to meet the a-priori power calculation ($n = 252$) the final sample (post-attrition, post study) consisted of 186 participants. Sixth, despite randomisation being completed in random blocks at a ratio of 1:1:1:1, attrition meant that there was unequal participants in each group, raising some concerns over internal validity (see, Hey & Kimmelman, 2014); although attrition patterns were consistent with expectations (wherein the more participants were asked to do, relatively across groups, the higher the drop out rate). Seventh, the interventions in this study were self-administered and delivered online, meaning future studies are required to ascertain their replicability when delivered using offline methods and when facilitated by health-care professionals or in group-based settings. However, despite these shortcomings, this study is one of the few RCTs available to test the impact of a psychological intervention on both mental and behavioural health outcomes (i.e. worry and sleep). It is also the first to test a worry postponement intervention augmented with implementation intentions, across a period greater than 7-days, and *via* a daily-diary multi-level design. Some of the concerns regarding statistical underpowering can also be alleviated. A post-hoc sensitivity analysis, with alpha = .05, power 80% and the study sample of 186 participants, indicated the minimum effect size to which the study was sensitive was $f = .24$ (3, 182, critical $f = 2.65$; equivalent to $d = .48$) which only slightly exceeded the original anticipated effect size of $f = .21$. Moreover, other daily-diary studies (i.e. not the pre-post designs in McCarrick et al., 2021) have reported effect sizes between worry and sleep that have exceeded the minimum effect size to which our study was sensitive (Weise et al., 2013). Though, these aforementioned limitations aside, the significant reductions in worry duration in the augmented arm has unearthed the promising nature of this brief, cost-effective, and easily implementable new intervention technique to tackle worry.

In sum, the present study considered, for the first time, the relative effectiveness of two worry postponement interventions; an 'augmented' worry postponement arm, featuring implementation intentions, and a 'standard' worry postponement intervention, compared to two control arms (active & non-active control) at reducing worry and improving sleep parameters. Participants in the augmented worry postponement arm experienced significantly lower worry duration (by ~15 min) across the study period, relative to the standard worry postponement arm. However, when compared to the augmented worry postponement arm, the standard worry postponement arm significantly increased the frequency of worry (by half a worry episode). Neither of the intervention arms, augmented or standard worry postponement, had any impact on sleep. Accordingly, these new set of findings shed new light on the value of implementation intentions at enhancing traditional approaches to worry postponement, while also opening up new avenues of research to test intervention methods that not only impact worry but engender equally positive changes in health behaviours, such as sleep. These findings are important as they represent a positive step forward in providing individuals with new tools to manage worry and potentially promoting better sleep.

Notes

1. *g* refers to the effect size measure Hedges g (Hedges, 1982).
2. A post-hoc sensitivity analysis, with alpha = .05, power 80% and the study sample of 186 participants, indicated the minimum effect size to which the study was sensitive was f = .24 (3, 182, critical f = 2.65; equivalent to d = .48) (see, Bloom, 1995) which only slightly exceeded the original anticipated effect size of f = .21. Moreover, other daily-diary studies (i.e. not the pre-post designs in McCarrick et al., 2021) have reported effect sizes between worry and sleep that have exceeded the minimum effect size to which our study was sensitive (Weise et al., 2013).
3. The analyses were replicated on a non-winsorized, complete, dataset (i.e. with participants who provided <3 days data) and the results did not significantly differ.

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