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A feasibility and pilot additive randomised control trial of attachment security priming during behavioural activation

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Abstract

Background: There is some initial evidence that attachment security priming may be useful for promoting engagement in therapy and improving clinical outcomes.
Aims: This study sought to assess whether outcomes for behavioural activation delivered in routine care could be enhanced via the addition of attachment security priming.
Method: This was a pragmatic two-arm feasibility and pilot additive randomised control trial. Participants were recruited with depression deemed suitable for a behavioural activation intervention at Step 2 of a Talking Therapies for Anxiety and Depression service. Ten psychological wellbeing practitioners were trained in implementing attachment security priming. Study participants were randomised to either behavioural activation (BA) or BA plus an attachment prime. The diagrammatic prime was integrated into the depression workbook. Feasibility outcomes were training satisfaction, recruitment, willingness to participate and study attrition rates. Pilot outcomes were comparisons of clinical outcomes, attendance, drop-out and stepping-up rates.
Results: All practitioners recruited to the study, and training satisfaction was high. Of the 39 patients that were assessed for eligibility, 24 were randomised (61.53%) and there were no study drop-outs. No significant differences were found between the arms with regards to drop-out, attendance, stepping-up or clinical outcomes.
Conclusions: Further controlled research regarding the utility of attachment security priming is warranted in larger studies that utilise manipulation checks and monitor intervention adherence.

Keywords: Attachment; Behavioural activation; Improving Access to Psychological Therapies programme (IAPT)

Introduction

A defining feature of Talking Therapies for Anxiety and Depression services (previously known as Improving Access to Psychological Therapies, IAPT) in England is that they are commissioned to deliver National Institute for Clinical Excellence (NICE)-recommended interventions for anxiety and depression (Layard and Clark, 2014). Services are expected to organise and deliver their interventions using a stepped-care organisational model. This entails delivery of low-intensity (LI) interventions for mild to moderate anxiety and depression, with patients 'stepped-up' to high-intensity interventions according to need, responsivity and risk (Gyani *et al.*, 2013). Guided self-help (GSH) is a brief LI psychological intervention that uses psychoeducational self-help materials

supported by trained practitioners typically over six (30–35 minute) sessions (Shafran *et al.*, 2021). In Talking Therapies services, these low-intensity practitioners are called psychological well-being practitioners (PWPs; University College London, 2014). A recent meta-analysis of the IAPT evidence base (i.e. including all the LI effectiveness studies) found a significant and large pre–post treatment effect size for reductions in depression ($d = 0.87$, 95% CI: 0.78–0.96) and anxiety ($d = 0.88$, 95% CI: 0.79–0.97) and a moderate effect size ($d = 0.55$, 95% CI: 0.48–0.61) for improvements to work and social adjustment (Wakefield *et al.*, 2020). The meta-analysis highlighted the relative lack of LI evidence and the need to balance the generation of evidence across stepped care.

Behavioural activation (BA) is an LI intervention at Step 2 of Talking Therapies services suitable for patients presenting with mild to moderate depression. BA aims to increase engagement in activities that create and maintain positive mood, reduces depressogenic activities/avoidance, and helps to problem-solve barriers to activation (Dimidjian *et al.*, 2011). Kanter *et al.*'s (2010) review of the components of BA highlighted model-specific assessment techniques (i.e. activity monitoring and values assessment), model-specific treatment techniques (i.e. activity scheduling and reduction of avoidant behaviours) and a range of additional treatment techniques that increase the likelihood of completion of identified activation activities (i.e. social and non-social skills training, relaxation, contingency management and targeting of covert verbal behaviours). The effectiveness of these additional techniques has not been isolated and analysed.

In terms of the BA evidence base, systematic reviews and meta-analyses have consistently found that BA compares favourably with cognitive behavioural therapy (CBT) as a frontline intervention for depression (Cuijpers *et al.*, 2007; Ekers *et al.*, 2014; Sinohara *et al.*, 2013). Richards *et al.* (2016) found no difference in treatment outcomes between BA and CBT for depression, but with the BA being delivered at 21% less cost. When BA is delivered in groups then outcomes are superior to controls (Simmonds-Buckley *et al.*, 2019) – and again produced equivalent outcomes (typically measured on the Beck Depression Inventory-I and -II) when compared with active therapies (e.g. supportive psychotherapy, psychodynamic psychotherapy, non-directive psychotherapy, problem-solving and assertiveness training). There is also meta-analytic evidence for BA in terms of facilitating an increase in wellbeing (Mazzucchelli *et al.*, 2010).

Ekers *et al.* (2011) emphasised the cost-effectiveness, ease of training and the parsimonious nature of BA. The uncomplicated and pragmatic nature of BA signals its potential suitability for treatment augmentation, in the effort to improve outcomes. Whilst new therapies are developed all the time, efforts to augment extant therapies simultaneously get neglected (Portela *et al.*, 2015). Simmonds-Buckley *et al.* (2020) found that a theory-driven enhancement of BA intervention delivered at Step 3 of a Talking Therapies service (i.e. helping patients to utilise implementation intentions during homework) was associated with significant improvements in depression outcomes. Such treatment augmentation and associated evaluation has not been attempted with BA at Step 2.

The Talking Therapies for Anxiety and Depression programme has a recovery rate target of 50% on its three minimum dataset clinical outcome measures (Patient Health Questionnaire-9, Generalised Anxiety Disorder-7, and the Work and Social Adjustment Scale; Department of Health, 2008). The programme had a national recovery rate of 50.1% in 2022 (NHS Digital, 2022). However, the situation that approximately half of all IAPT patients do not reach recovery during treatment indicates that treatments and associated treatment outcomes can be improved (Simmonds-Buckley *et al.*, 2020). In the context of depression, poor outcome maintains hopelessness and suppresses the motivation for future help-seeking (Ten Have *et al.*, 2010).

Attachment theory explains how the quality of relationships with early caregivers ('attachment figures') is internalised into mental models of self and others known as *attachment styles* (Bowlby, 1969). Attachment styles are conceptualised along two continuous dimensions of insecurity: avoidance and anxiety. In the context of seeking psychological therapy, attachment security has

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been associated with an ability to effectively utilise emotional support (Mikulincer and Shaver, 2018), greater willingness to seek help and the capacity to form more positive relationships with therapists (Slade, 2008). Attachment security priming is a social-cognitive technique in which secure attachment schemas are made salient through a 'spreading activation' mechanism, triggering semantic and affective nodes that then create a sense of security comparable to the presence of a secure attachment figure (Baldwin *et al.*, 1993; Gillath *et al.*, 2008). Repeatedly priming a secure attachment style in those with an insecure attachment style should increase accessibility to secure internal working models (Carnelley and Rowe, 2007). Different techniques have been developed to prime attachment security (Gillath and Karantzas, 2019). Security priming tasks are delivered either subliminally (i.e. outside of conscious awareness) or supraliminally (i.e. within conscious awareness). This has involved exposing people to methods such as security-related words (i.e. words associated with secure attachment), the names of secure attachment figures, pictures representing attachment security and recalling memories associated with being loved/supported by attachment figures (Gillath and Karantzas, 2019). A recent meta-analysis (Gillath *et al.*, 2022) of $k = 120$ studies reported large positive effect sizes for attachment priming across affective, cognitive and behavioral outcomes.

Security priming has previously been shown to have potential in promoting engagement with therapy and improving outcomes. Millings *et al.* (2019) found that priming security led to more positive and less negative attitudes towards therapy, via the mechanism of cognitive openness. Rowe and Carnelley (2003) found that making a secure attachment style temporarily accessible via priming methods led to more positive interpersonal expectations, such as conferring greater trust in therapists. Furthermore, Carnelley and Rowe (2007) found that repeated security priming led to more positive self-views and relationship expectations and less (state) attachment anxiety. Security priming increases willingness to engage in mindfulness training (Rowe *et al.*, 2016). In a non-clinical sample, McGuire *et al.* (2018) found that individuals that were repeatedly exposed to a security prime for 2 weeks showed lower depression symptoms than those exposed to neutral primes. Moreover, in a clinical population of out-patients experiencing depression, those exposed to repeated security primes showed reduced symptoms of anxiety and depression (Carnelley *et al.*, 2018).

To conclude, whilst attachment security priming shows promise, it has not been previously tested in the context of LI interventions. Therefore, the current study used a feasibility and pilot additive trial design to evaluate whether attachment security priming is practicable and effective in a routine service setting such as Talking Therapies services. This is an appropriate first step before considering the possibility of conducting a full-scale randomised control trial (Craig *et al.*, 2008). Additive trials evaluate the efficacy of an intervention by starting with one component (or full treatment) and subsequently adding additional components, therefore indexing the contribution made by each component when systematically added (Papa and Follette, 2015). Eldridge *et al.* (2016) demarcated feasibility as answering questions about whether a larger study is possible (i.e. recruitment rates, etc.) and pilot aspect as pertaining to initial (i.e. and so under-powered) examinations of various indices of effectiveness. The feasibility outcomes in the current study were PWP satisfaction with the attachment priming training, the willingness of patients to participate, PWPs' willingness to recruit and the study attrition rates. The willingness to recruit outcome was included because of the acknowledged pressure of the PWP role in terms of carrying high clinical caseloads (Golden, 2011; University College London, 2014), as any future main trial would be dependent on PWPs being willing and able to recruit. The pilot outcomes were whether attachment security priming during BA differentially reduced drop-out, increased session attendance, reduced the stepping-up rate and enabled better clinical outcomes (i.e. on the IAPT minimum dataset measures). The origins of the attendance hypothesis were based on the awareness of the need to develop and experimentally evaluate low-cost, easy to implement interventions that reduce drop-out from clinical services (Oldham *et al.*, 2012).

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Method

Study design

The study was a pragmatic two-arm feasibility and pilot study, utilising an additive trial design (Papa and Follette, 2015). Participants were randomised to receive either treatment as usual BA (i.e. the BA group) or a version of BA enhanced with an embedded attachment security-priming task (i.e. the 'BA-prime' group). A coin toss by a member of the research team (S.K.) was used to allocate participants to one of the two study arms. Suresh's (2011) overview of randomisation techniques noted that the coin toss method was appropriate in small scale research. This was because it maintains randomness of participant assignment when simple randomisation (i.e. either/or) is all that is required. The participant information sheet explained that participants would be allocated to one of two interventions. On allocation, participants were not told which group they were allocated to, but rather offered a treatment space. It was not possible to blind PWPs to allocation, due to needing to know which treatment to deliver.

Context, participants and recruitment

Participants were recruited through an IAPT service in North Yorkshire covering a wide geographical area. At the time of the study, the service was before the shift to the name Talking Therapies for Anxiety and Depression. Finegan *et al.* (2018) demonstrated the negative impact of deprivation on psychological therapy outcomes for anxiety and depression and therefore due consideration of context is required. At local authority level, North Yorkshire is ranked 125th least deprived out of 152 upper tier local authorities on the Index of Multiple Deprivation (IMD). The area also has pockets of very high levels of deprivation (e.g. three areas in one town are within the most deprived 1% in England; Ministry of Housing, Communities & Local Government, 2015). Participants were eligible for the study if they were over the age of 18 and spoke fluent English (i.e. due to the need to be able to read and understand the information sheet to fully consent to taking part) and were presenting with depression suitable for a LI Step 2 treatment. Due to the feasibility and pilot nature of the current study, a formal power calculation was not required. Previously, sample sizes of between 12 and 50 have been recommended for such studies (Julious, 2005; Lancaster *et al.*, 2004; Sim and Lewis, 2012). Figure 1 summarises participant flow through enrolment and allocation stages and summarises attendance outcomes (i.e. number of sessions attended). The final sample consisted of 24 participants: 16 in the BA group and eight in the BA-prime group. All interventions were delivered over the telephone due to the study being conducted during the COVID-19 pandemic.

Study procedure

Depression had been initially identified by a General Practitioner (GP) in Primary Care. As per standard practice in the IAPT service then all referrals from GPs were initially screened on the telephone by a PWP. At this service screen, those patients who were deemed to be suitable for BA at Step 2 were asked if they would be interested in participating in the study. A script was developed to ensure that the offer was standardised across screening PWPs. When the person referred to the service verbally agreed, their name and address was then sent via secure email to research team, who then sent out the study information sheet and consent form in the post. If the patient consented to taking part, they were asked to return the consent form using a pre-paid envelope. Once the consent form was returned, participants were randomly allocated and the principal investigator at the study site informed the treating PWP which treatment to deliver.

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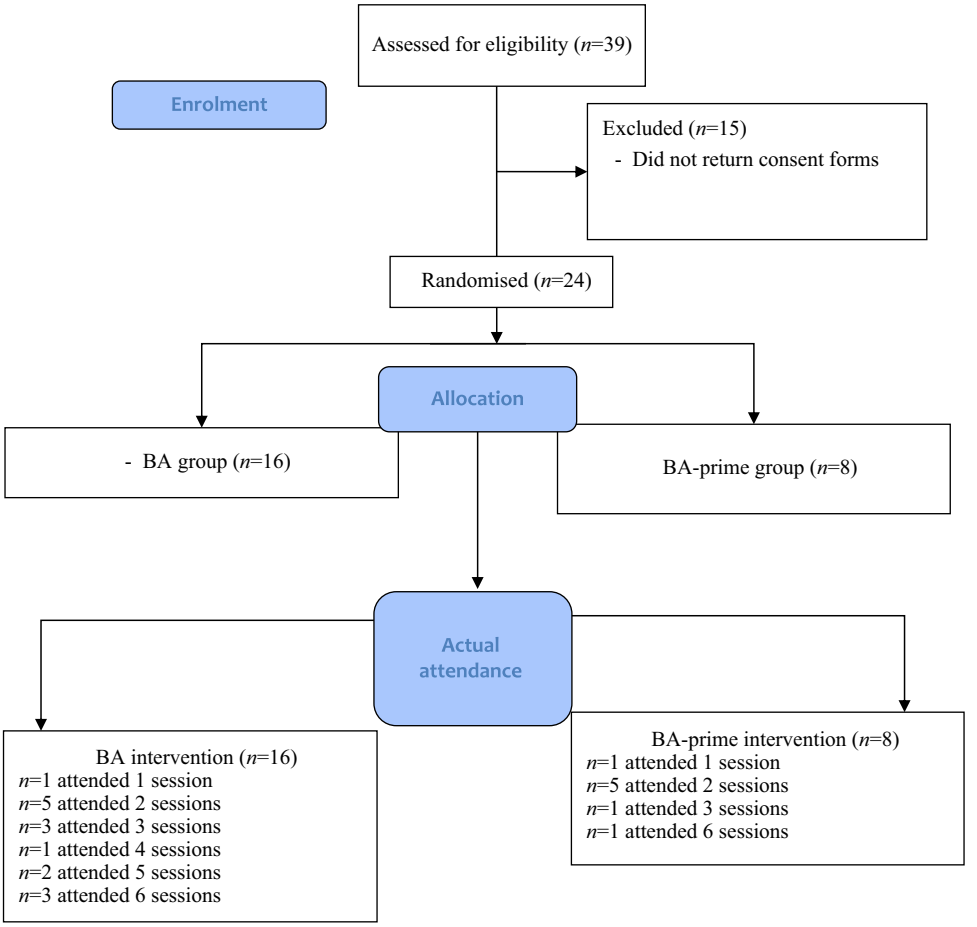

Figure 1. CONSORT summary flowchart. In the BA group, one additional participant received this intervention due to error in assignment from the BA-prime group.

Training

Three managers and ten PWPs attended a half-day training workshop on attachment theory and attachment security priming. This was delivered by the research team, one of whom is an expert within the attachment security-priming field (A.M.). The training covered an overview of attachment theory as well as information regarding the study including aims, design, recruitment, content/delivery of the attachment security prime, study measures and ethical issues. The training specified how to introduce and complete the prime. It was assumed that PWPs would be consistent in introducing the prime, as it was integrated into the depression workbook used in the BA sessions.

BA versus BA-prime treatments

Participants in both arms had a matched offer of six sessions (30–35 minutes per session) of weekly BA facilitated by a PWP. BA at Step 2 of IAPT services follows a structured workbook format. The workbook content for BA and BA-prime groups are detailed in the online Supplementary material. The core BA components across groups was therefore matched. Those in the BA-prime group completed a short additional security-priming task prior to each session

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(i.e. hypothesised to support attendance) and also prior to engaging in homework activation exercises (i.e. hypothesised to better enable activation). Time of day for the use of the attachment prime was not specified, as it was to be used prior to homework activities that could occur at any point in time. The prime was introduced to participants as a method that might help them engage in the activation exercises identified in sessions and specified as homework activities. Therefore, before attempting a piece of activation between sessions, those in the BA-prime group were encouraged to use the security attachment prime. This was introduced by PWPs as a means of feeling more relaxed and confident about engaging in the activation task.

The attachment security prime was embedded within the BA workbook, and this is available in the online Supplementary material. The brief nature of the diagrammatic attachment prime is in keeping with the brevity of LI interventions in IAPT and was based on the version developed by Rowe *et al.* (2017). The prime consisted a description of a secure relationship, based on Bartholomew and Horowitz (1991), space to write a/some names, and a diagram of concentric circles, with 'me' in the centre. Participants were asked to recall a person (or group of people) with whom they experienced a relationship matching the secure attachment description, then write the name/names on the space provided in the prime and finally reflect on the emotional connection felt to that person/group. Hence, placing a person/group close to the centre represented greater feelings of closeness to that person/group, than placing them further away. Participants listed up to six more people/groups with whom they felt an emotional connection with and plotted these names onto the prime in relation to emotional closeness (i.e. the closer they placed them to the innermost circle, the closer they experienced this person in relation to them). During the first session, the PWPs guided participants through the initial priming task to explain this fully, giving time and opportunity to ask any questions. PWPs were encouraged to check out with participants that they understood the prime and how to use it.

Feasibility and pilot outcomes: measures and definitions

Attendees at the training event completed a bespoke training evaluation questionnaire with items concerning theory, knowledge, recognition, satisfaction and confidence in usage of attachment priming. Answers were measured on a 1–10 Likert scale (1: not at all, to 10: extremely). The satisfaction measure is presented in the online Supplementary material. Willingness to engage in the research was measured by comparing the number of potential participants that were sent the information sheet with the number who consented to take part. Willingness to recruit was determined by comparing how many PWPs attended the training with how many recruited participants. Willingness to recruit was a feasibility outcome in this pragmatic study because the style of LI work means that PWPs typically carry high caseloads (University College London, 2014) and therefore may not have felt able to support research due to its additional burden. Number of participants dropping out of the study assessed the study attrition rates. Attendance was the number of sessions attended. Treatment drop-out was measured as the number of participants who dropped out of treatment after having started BA. Stepping-up rates were determined by the number of participants subsequently stepped-up to a Step 3 intervention following the LI treatment. Clinical outcome measures were collected at the service screen stage and then at every treatment session. The clinical outcome measures consisted of the three outcome measures forming the main spine of the IAPT minimum dataset (Clark *et al.*, 2018). Accordingly, the cut-offs and reliable change criteria used were all taken from the IAPT manual (National Collaborating Centre for Mental Health, 2023).

Depression

The PHQ-9 (Kroenke *et al.*, 2001) contains nine items monitoring the severity of depression. Total scores of 5, 10, 15 and 20 are taken as the cut-off points for mild, moderate, moderately severe and

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severe depression. The cut-off for PHQ-9 is a score ≥10 and a change score ≥6 is taken as a reliable change in depression. The PHQ-9 had excellent internal consistency when used with an adult primary care population (α = 0.89).

Anxiety

The GAD-7 (Spitzer *et al.*, 2006) is a 7-item scale used to measure severity of generalised anxiety. Total scores of 5, 10 and 15 are taken as the cut-off points for mild, moderate and severe anxiety, respectively. The cut-off for the GAD-7 is a score ≥8 and a change score of ≥4 is taken as reliable change. The GAD-7 has excellent internal consistency (α = .92).

Functioning

The Work and Social Adjustment Scale (WSAS; Mundt *et al.*, 2002) is a 5-item questionnaire used to measure impaired functioning. A WSAS score above 20 suggest moderately severe to severe impairment. The WSAS demonstrated excellent internal consistency in adult populations with mood or anxiety disorders (Cronbach's α = 0.70- 0.94; Mundt *et al.*, 2002).

Analysis

Due to the small sample size (Dwivedi *et al.*, 2017), non-parametric statistics were used in addition to a case-by-case analysis of reliable and clinically significant change. Demographic data were analysed non-parametrically using the Mann–Whitney *U*-test for continuous data and Fisher's exact test for categorical data. Drop-out, stepping-up, attendance comparisons were also analysed using the Mann–Whitney *U*-test and Fisher's exact test. The Mann–Whitney *U*-test was used to compare outcome measure scores (GAD-7, PHQ-9 and WSAS) between the arms at screening and post-treatment. Post-treatment scores were obtained for each participant based on the last set of outcome measures completed. A Wilcoxon signed-rank test compared pre- and post-scores within each arm on the clinical outcome measures. Cohen's *d* effect sizes and 95% confidence intervals (CI) were reported for all between-group (BA mean – BA-prime mean/pooled standard deviation (SD)) and within-group pre–post (pre-mean – post-mean/pre-SD) outcome comparisons. Individual pre–post PHQ-9 and GAD-7 outcomes in each arm were also examined and plotted to assess for reliable (pre–post treatment change scores; PHQ-9 ≥6 and GAD-7 ≥4) and clinical change (post-treatment scores; PHQ-9 ≤10 and GAD-7 ≤8) according to the metrics applied in Talking Therapies services (Jacobson and Traux, 1991; National Collaborating Centre for Mental Health, 2023). For a reliable recovery outcome to be recorded, then a participant had to fall below the clinical cut-off on both measures following treatment and demonstrate reliable change on the GAD-7 and/or PHQ-9 measures without a reliable deterioration on either measure.

Results

Feasibility outcomes

Training satisfaction results are reported in the online Supplementary material, with high mean training satisfaction scores (i.e. mean satisfaction ratings ranged from 6 to 9.75 out of 10). Of the ten PWPs who attended the training session (i.e. excluding the three managers not involved in recruitment), three left the service before recruitment started and seven recruited participants. Of the 39 patients that agreed to be sent the study information sheet, 24 (61.53%) returned consent forms. No participants dropped out of the study.

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Table 1. Participant demographics

Demographic variable	BA ($n = 16$)	BA-prime ($n = 8$)	Test statistic	Statistical significance
Gender (number female)	10	7		$p = .03$*
Mean age (*SD*)	48.19 (12.31)	35.5 (6.65)	$U = 25.5$	$p = .02$*
Ethnicity	15 White British 1 Mixed other	8 White British		$p = 1.00$
Participants with a long-term condition	5	3		$p = 1$
Provisional diagnosis	10 Moderate depressive episode 2 Mild depressive episode 2 Severe depressive episode without psychotic symptoms 2 Recurrent depressive disorder	6 Moderate depressive episode 2 Recurrent depressive disorder		$p = .45$
Number of veterans	2	0		$p = .53$
Number perinatal	1	1		$p = 1.00$
Employment status (number in each category)	13 Employed 1 Unemployed 1 Retired 1 Long-term sick	4 Employed 3 Unemployed 1 Long-term sick		$p = .12$
Number seeking employment support	4	1		$p = .63$
Civil status (number in each category)	8 Married 5 Single 1 Not reported 1 Separated 1 Divorced	5 Single 2 Married 1 Living with partner		$p = .47$
Psychotropic medication (number in each category)	14 Prescribed and taking 2 Not prescribed	4 Prescribed and taking 4 Not prescribed		$p = .13$

*Significant at $p < .05$ level.

Pilot outcomes

Table 1 reports participant demographics. This shows that significantly more female participants were randomised to the BA-prime arm and participants were significantly older within the BA group. Two participants attended all six BA sessions, and one participant attended all six sessions of the BA-prime version. There were no significant differences in session attendance, but BA participants attended more sessions on average ($M = 3.81$) than those in the BA-prime group ($M = 1.88$). When excluding participants who dropped out of treatment, the mean number of sessions attended in each group was higher. Table 2 reports the treatment drop-out, stepping-up and attendance rates. Within the BA group, 4/16 dropped out of treatment and 4/16 were stepped up. In the BA-prime group, 4/8 dropped out and 2/8 were stepped up. There were no significant differences between BA and BA-prime groups at the service screening on the PHQ-9 ($Z = -.03$, $p = .98$, between-groups $d = 0.05$ [95% CI −0.80 to 0.89]), GAD-7 ($Z = -.87$, $p = .38$, between-groups $d = 0.60$ [95% CI −0.27 to 1.46]) or the WSAS ($Z = -1.14$, $p = .26$, between-groups $d = -0.46$ [95% CI −1.32 to 0.40]). There were no significant differences between BA and BA-prime groups at post-intervention on the PHQ-9 ($Z = -.44$, $p = .67$, between-groups $d = 0.30$ [95% CI −0.55 to 1.15]), GAD-7 ($Z = -.23$, $p = .82$, between-groups $d = 0.12$ [95% CI −0.73 to 0.97]) and WSAS ($Z = -.67$, $p = .54$, between-groups $d = 0.28$ [95% CI −0.57 to 1.13]). Figure 2 presents a mean PHQ-9 plot for each arm per sessional time-point.

On the PHQ-9, 15/16 in the BA group at screening were above clinical cut-off, which had fallen to 9/16 at end of treatment (7/16 had shown clinical change), with 9/16 demonstrating reliable improvement in depression. At screening, 15/16 in the BA group were above clinical cut-off on the

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Table 2. Drop-out, stepping-up and attendance rates

Outcome	BA ($n=16$)	BA-prime ($n=8$)	Test statistic	Statistical significance
Mean number of sessions attended (overall)	3.81 (1.91)	1.88 (1.17)	$U=26.5$	$p=0.19$
Mean number of sessions attended (excluding those who dropped out)	4.17 (1.91)	2.25 (1.48)	$U=26.5$	$p=0.58$
Dropped out	4	4		$p=.68$
Stepped up	4	2		$p=1.00$
Mean number of sessions did not attend (*SD*)	0.19 (0.39)	0.25 (0.43)	$U=66$	$p=.76$
Mean number of sessions cancelled with >24 hours notice (*SD*)	0.58 (0.31)	0.25 (0.43)	$U=63$	$p=.98$
Number treatment changed within duration of Step 2 intervention	0	2		$p=.09$
Number diagnosis changed	1	1		$p=1$

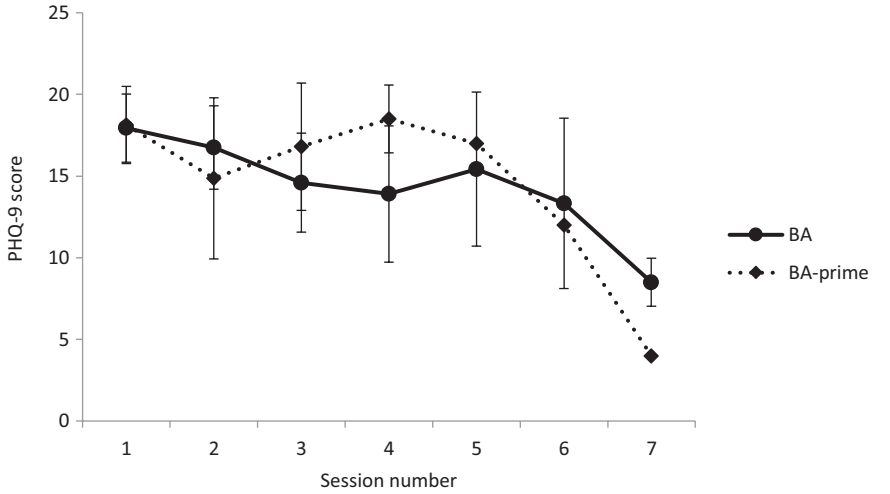

Figure 2. Sessional PHQ-9 (Patient Health Questionnaire-9) depression outcomes in BA and BA-prime groups. Error bars represent 95% confidence intervals (BA-prime sessions 5–7 based on $n=1$ so 95% CI unable to be calculated).

GAD-7 and this fell to 8/16 at end of treatment (8/16 had shown clinical change), with 7/16 demonstrating reliable improvement in anxiety (see the Jacobson plot in Fig. 3A). Overall, 6/16 BA participants demonstrated reliable change in scores on both the GAD-7 and PHQ-9 and 4/16 demonstrated reliable recovery. In the BA group, there was a significant reduction in PHQ-9 scores from screening to post-treatment ($Z=-.259$, $p=.01$, pre–post $d=1.14$ [95% CI −1.58 to −0.09]) and a significant reduction in WSAS scores ($Z=-.251$, $p=.01$, pre–post $d=1.04$ [95% CI −1.62 to −0.14]), but no significant difference in scores on the GAD-7 between screening and post-treatment ($Z=1.37$, $p=.17$, pre–post $d=0.53$ [95% CI −0.93 to 0.48]).

On the PHQ-9 at screening, all the participants in the BA-prime group (8/8) were above the clinical cut-off. At post-treatment, 2/8 participants fell below the clinical cut-off and 3/8 demonstrated reliable improvement in depression. On the GAD-7 at screening, 7/8 participants in the BA-prime group were above the clinical cut-off on the GAD-7. By the end of treatment, 4/8 participants in the BA-prime group then fell below clinical cut-off on the GAD-7 and 3/8 demonstrated reliable improvement (see the Jacobson plot in Fig. 3B). In terms of recovery rates, 1/7 BA-prime participant demonstrated reliable recovery. There was no significant difference between PHQ-9 scores in the BA-prime group from screening to post-treatment ($Z=-.93$,

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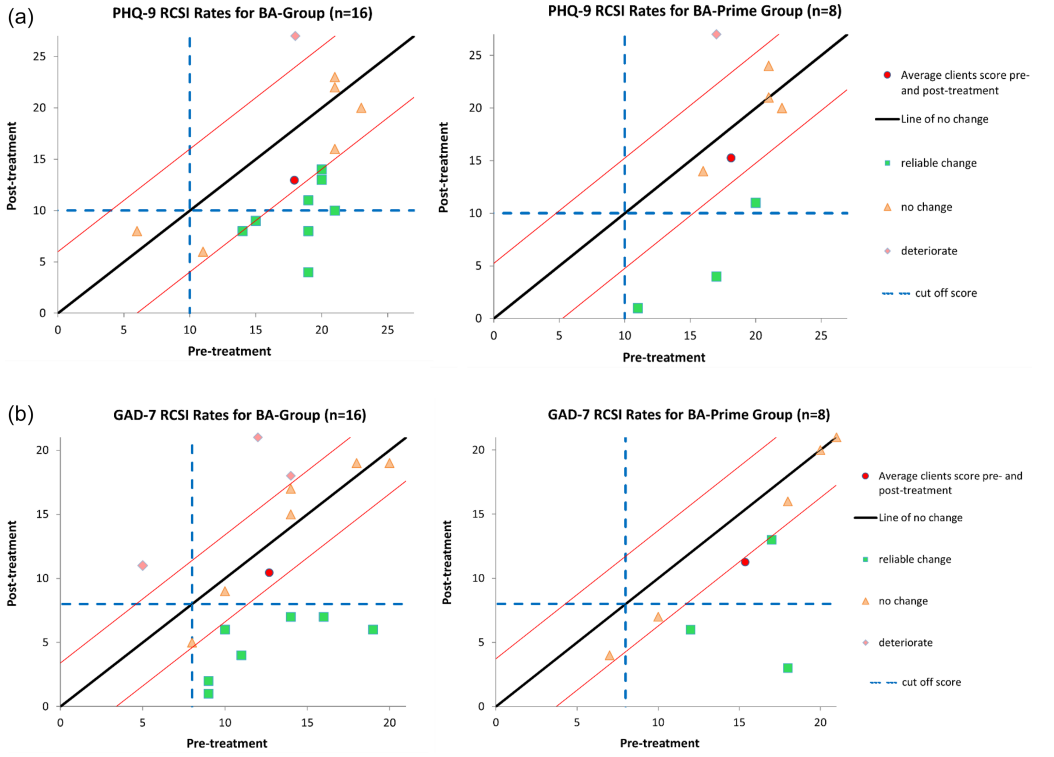

Figure 3. Reliable and clinically significant change rates (RCSI) for the behavioural activation (BA) and BA-prime groups according to (A) Patient Health Questionnaire-9 (PHQ-9) scores and (B) Generalised Anxiety Disorder-7 (GAD-7) scores. Two BA-group participants had identical pre (19) and post (8) PHQ-9 scores, so appear as one data point in the plot.

$p = .35$, pre–post $d = 0.79$ [95% CI −1.37 to 0.67]), or any significant difference in WSAS scores ($Z = -.14$, $p = .18$, pre–post $d = 0.48$ [95% CI −1.38 to 0.60]). There was a significant difference in scores on the GAD-7 between screening and post-treatment ($Z = -2.21$, $p = .03$, pre–post $d = 0.81$ [95% CI −1.66 to 0.38]).

Discussion

This study was motivated by trying to improve recovery rates for a routinely delivered LI intervention in Talking Therapies for Anxiety and Depression services (Gyani *et al.*, 2013). This was attempted via augmenting the established evidence-based intervention of BA with attachment priming and then evaluating outcomes in an additive trial design. The study had two specific research aims. Firstly, to determine the feasibility of conducting an additive trial in a Step 2 IAPT service to be better placed to consider the possibility of staging a larger scale study. Secondly, to use the trial design to conduct a preliminary and tentative comparison of outcomes, and so to contribute to the BA evidence base. There was a divergence between the feasibility and the pilot outcomes. The feasibility findings found the training to be satisfactory, with good willingness from patients and PWPs to participate and there being no attrition from the study. Utilising the IAPT minimum dataset meant that there was no additional measure administration burden and hence there was 100% outcome data collection. The brief diagrammatic attachment security-priming enhancement was easily integrated into the existing BA workbook. The feasibility findings overall would suggest that conducting a larger and fully powered study was practicably possible. However,

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the pilot results overall would suggest that low-intensity BA was not effectively augmented by the addition of attachment security priming.

No significant differences were found between the BA and BA-prime arms with regard to attendance, treatment drop-out or stepping-up rates. Post-treatment, there were no significant between-arm differences with regard to anxiety, depression, and impaired functioning. The small-to-medium between-group effect sizes may inform clinical practice in that opportunity cost of attachment priming in terms of time taken to prime in already very brief sessions may be too high. However, sample size issues limit the degree of reliability of differences found with regards to effectiveness. Szuhany and Otto (2020) used a similar design to test whether adding in exercise to BA improved outcomes and found no differences between their BA and BA+ groups and that all participants engaged in increased exercise over time.

The within-arm BA evaluation demonstrated a significant improvement in depression and impaired functioning scores from screening to post-treatment and 4/16 participants demonstrated reliable recovery. The BA-prime within-arm evaluation found a significant improvement in anxiety from screening to post-treatment only, with just 1/7 demonstrating reliable recovery. The findings suggest a higher recovery rate in the BA arm compared with those in the BA-prime arm, but in the context of small and unequal group sizes and some significant differences in terms of demographics. The reasons for the lack of an augmentation effect might be (1) participants did not actually complete the task despite it being integrated into the workbook, (2) possible lack of understanding of the priming task itself or (3) the mixing of theory, in that BA is a purely behavioural model, whereas attachment priming is a cognitive task underpinned by a cognitive theory and therefore this may have confused participants or PWPs.

This pragmatic study has been the first to explore the feasibility of conducting a trial comparing the effects of attachment security priming within an IAPT setting. The lack of any differences between the arms with regard to symptom reduction runs contrary to evidence using clinical (Carnelley *et al*., 2018; McGuire *et al*., 2018) and community (Sood *et al*., 2021) samples, where repeatedly priming attachment security has been found to improve symptoms. There are three key differences that might account for the disparity. Firstly, the comparison made here was with an active, effective treatment (BA), rather than a neutral prime group, as per the studies of McGuire *et al*. (2018) and Carnelley *et al*. (2018). Secondly, the nature of the security prime differed. This study used a relatively new diagrammatic prime, whereas Carnelley *et al*. (2018) utilised a written task (i.e. where participants listed up to 10 of their closest significant others, chose the relevant attachment style illustrating this relationship and rated how representative each relationship was of the chosen style) and McGuire *et al*. (2018) utilised a subliminal task, as well as a written supraliminal task. The attachment prime used in the current study was driven by the need to integrate it into the BA treatment protocol, as opposed to it being the intervention itself. Thirdly, Sood *et al*. (2021) employed a manipulation check and therefore this may have ensured that the attachment prime had an effect because there was more certainty that it was done.

There are several limitations of the present study, particularly with regard to the small sample size. The small sample size means that the study lacked power to detect a significant effect, and therefore, findings from statistical analyses cannot be interpreted with certainty and confidence. Small studies utilising conventional randomisation methods often result in unequal sample sizes and variations in baseline characteristics between control and intervention groups (Scott *et al*., 2002). Indeed, the current study showed significant differences between the arms with regard to age and gender, therefore introducing a potential confound and so making direct comparisons unreliable (Kang *et al*., 2008). The fact that there was only a single male in the attachment prime condition does raise questions concerning the generalisability of the findings, particularly as the average percentage of females is 60.2% across IAPT outcome studies (Wakefield *et al*., 2020). Suresh (2011) noted that using the coin toss randomisation technique in a small sample was more prone to generating unequal group sizes and that only large studies iron out these vagaries of coin toss chance. The artificial creation of equal arm sizes through, for example, permuted-block

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randomisation would have potentially limited the unpredictability of treatment allocation and so allowed bias to creep into the study (Matthews, Cook, Terada & Aloi, 2010). Block randomisation was therefore rejected as despite it enabling balanced group sizes, these groups would be potentially incomparable in terms of covariates (Suresh, 2011). Schulz and Grimes (2002) noted that pilot randomised trials do not strictly need to have equal sample sizes in their comparison arms. As the current study was a simple unrestricted trial, the unequal group sizes more represent the essence of coin toss random variation in a small sample. Some difference between the numbers in the comparison arms might be expected due to the randomisation method employed.

Another limitation of the present study is the lack of assurance with regard to adherence to the security-priming task itself and associated manipulation tests. Although PWPs were asked to complete the initial priming task with participants in the BA-prime group, this was not monitored during the study. Neither was it monitored that primes were completed prior to each session and to help with the homework activation efforts. There was therefore no assurance as to whether the priming task was completed initially and then regularly. Whether participants in the attachment priming group were able to report a secure attachment figure was not collected. The priming task could also have been introduced differently by different PWPS, so potentially impacting on outcomes. Some individuals included in the current sample met criteria for 'severe' depression and/or anxiety on the outcome measures at service screening. LI GSH interventions are designed for those with mild–moderate anxiety and depression (National Institute for Health and Care Excellence, 2011). Therefore, LI-BA may not have been the most appropriate intervention for some individuals included within the study sample, with lack of change from pre-to-post intervention potentially representing an issue with regard to inappropriate allocation to LI treatment. Lack of any follow-up data also precluded the investigation of the durability of the clinical outcomes. The study was not registered as a trial, and the reason for this is that the project was a student thesis, and the basic requirement was IRAS ethical approval, and this was achieved. Finally, the study did not assess the attachment style of participants. It has been shown previously that emotion regulation benefits of attachment primes are weaker for participants that score high on attachment avoidance (Selcuk *et al.*, 2012).

The current study indicates that implementing a larger sufficiently powered attachment priming trial within IAPT is feasible and this would enable a much more reliable evaluation of efficacy. Studies should also seek to compare the efficacy of different kinds of both supraliminal and subliminal security primes to ensure that the most effective, pragmatic, and efficient attachment prime is identified per presenting psychological disorder. The systematic review of $k = 20$ studies by Gillath and Karantzas (2019) highlighted that when supraliminal primes were administered with anxiously attached people via guided imagery or visualisation, then this was particularly effective in promoting positive outcomes. Therefore, the role of attachment priming in patients presenting with anxiety disorders is worthy of investigation and holds the promise of helping patients to better engage in services through being less anxious.

In terms of timing, the efficacy of priming either before or following sessions needs to be investigated in clinical groups, as there is some evidence that the efficacy of primes is affected by whether they are introduced prior to or following stress (Selcuk *et al.*, 2012). It may be the case that in clinical populations, more elaborate or longer primes may be needed to induce felt security. Rowe *et al.* (2020) found that repeated priming showed a cumulative positive effect over single episode priming and suggest that this should be considered in the design of future clinical studies.

The use of mobile technology apps to support attachment priming is worthy of investigation and Sood *et al.* (2021) have already demonstrated that is possible to administer attachment primes online. Future research needs to monitor adherence to attachment security priming activities to ensure that any effects detected can be reliably ascribed to attachment security priming tasks. When primes are delivered via apps, this enables more accurate recording of when priming and adherence occurs. It is acknowledged that other confounding variables could exert an unmeasured influence regardless of observed adherence levels to the priming task itself. Additionally, training

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should be routinely implemented, so those delivering the priming intervention are confident in its rationale and how to implement this. A larger trial would make better use of block randomisation. Assessing for trauma history in terms of being a potential moderator would also be useful in future studies.

In conclusion, this study indicates that it is feasible to conduct a larger study on the efficacy of attachment security priming as an BA augmentation with people presenting with depression in routine clinical service settings. The study contributes to the literature by adding to the small number of studies testing attachment priming in a clinical population, through using a new version of a prime and there being lessons learnt in terms of the need for manipulation checks in any research going forward. Feasibility outcomes were encouraging, but pilot outcomes less so. Findings should be treated as preliminary but promising. Future research needs to implement similar procedures in a larger and better conducted study (i.e. with adherence/manipulation checks and follow-up) to define the true effect of attachment security-priming as a treatment augmentation for existing GSH interventions.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/S1352465823000504

Data availability statement. The data that support the findings of this study are available from the corresponding author (S.K.) on reasonable request.

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