Postdeployment Battlemind Training for the U.K. Armed Forces: A Cluster Randomized Controlled Trial

Kathleen Mulligan[1], Nicola T. Fear[1], Norman Jones[1], Helen Alvarez[1], Lisa Hull[2], Ulrike Naumann[3], Simon Wessely[2], Neil Greenberg[1*]

1. Academic Centre for Defence Mental Health, King's College London, London, United Kingdom
2. King's Centre for Military Health Research, King's College London
3. Department of Biostatistics & Computing, Institute of Psychiatry, King's College London

*Corresponding author

ABSTRACT

Objective: Combat exposure can increase the risk of subsequent psychological ill-health in armed forces (AF) personnel. A U.S. postdeployment psycho-educational intervention, Battlemind, showed a beneficial effect on mental health in U.S. military personnel exposed to high combat levels. We evaluated the effectiveness of an anglicized version of postdeployment Battlemind.

Method: Battlemind was adapted for the United Kingdom. The main amendments were to sections about carrying weapons, driving, and alcohol misuse. The anglicized Battlemind was compared with the U.K. standard postdeployment brief in a cluster randomized controlled trial. At baseline, 2,443 U.K. AF personnel returning from Afghanistan via Cyprus completed questionnaires about their combat experiences and mental health. Of these, 1,616 (66%) completed 6-month follow-up questionnaires. We used the Posttraumatic Stress Disorder Checklist (PCL–C) to measure probable posttraumatic stress disorder and the General Health Questionnaire (GHQ–12) to measure common mental disorders. Secondary outcomes included alcohol misuse, assessed with the Alcohol Use Disorders Identification Test (AUDIT), and binge drinking. Mixed-effects models were used to account for possible cluster effects.

Results: We did not find a difference in mental health or overall AUDIT score. Those who received Battlemind versus the standard brief were less likely to report binge drinking, although the effect size was small (adjusted odds ratio = 0.73, 95% CI [0.58, 0.92]).

Conclusions: The anglicized Battlemind did not improve mental health but had a modest impact on the reporting of binge drinking. Alcohol misuse is problematic in military populations; therefore, an intervention that reduces binge drinking may be helpful.

Keywords: armed forces, combat, mental health, postdeployment, psycho-education

Introduction

Research among armed forces (AF) personnel has consistently found that combat exposure increases the risk of subsequent psychological ill-health (Castro, 2009; Hoge et al., 2004; Iversen et al., 2008). A recent survey of the U.K. AF (Fear et al., 2010) found that amongst regular personnel, those in combat roles were more likely than those in support roles to report probable posttraumatic stress disorder (PTSD), and regular personnel who had deployed to Iraq or Afghanistan were more likely to report alcohol misuse than personnel who had not deployed on these operations. Reserve personnel who deployed were more likely to report probable PTSD than reservists who did not deploy.

Many AF provide psycho-educational interventions for personnel returning from deployment to help mitigate possible adverse psychological consequences (Adler et al., 2008). These interventions typically include information about common responses to trauma, self-help techniques, where to get help if necessary, and making the psychological transition from the operational theater to home. However, few of these interventions have been evaluated using robust research methodology (i.e., using a randomized controlled trial [RCT]; Mulligan, Fear, Jones, Wessely, & Greenberg, 2011).

Since 2006, it has been U.K. AF policy to deliver a psycho-educational brief to personnel returning from deployment. The brief uses a traditional educational approach, delivering advice in lecture format, and consists of two parts: (a) provision of stress management information, usually delivered by a military mental health practitioner, and (b) information about the homecoming transition, often delivered by a military chaplain or welfare officer. The stress management information provided covers common postdeployment reactions experienced, possible problem indicators, some self-help techniques (e.g., advice to reestablish routines, talk to trusted people, and avoid "self-medicating" with alcohol), advice on seeking help, and myths around mental health. The homecoming information covers advice about renewing relationships, reasonable expectations on returning home, and managing the transition. The central message is that life will have continued in their absence so personnel should be patient and take a relaxed view of reestablishing their role(s) and relationships with friends and family. The briefs were standardized in 2008 but have not been evaluated in an RCT.

Battlemind is a training program developed by the U.S. Army (Adler, Bliese, McGurk, Hoge, & Castro, 2009; Adler, Castro, & McGurk, 2009). Several Battlemind packages have subsequently been developed for delivery at different stages (e.g., pre- or post-deployment) or to different personnel (e.g., soldiers, leaders, spouses). Battlemind and the U.K. standard brief contain similar content, including topics such as common postdeployment reactions and self-help. However, the interventions differ in their approaches. In Battlemind, group training interventions are designed to be interactive, and participants are encouraged to contribute their experiences, whereas the U.K. standard briefs are more didactic. Battlemind draws on positive psychology (Seligman & Csikszentmihalyi, 2000) and cognitive behavioral techniques to reframe difficulties that personnel may encounter. Each letter of the acronym *Battlemind* stands for a different strength or skill that personnel would have relied on during deployment. Battlemind aims to help participants recognize the cognitive and behavioral strategies that helped them to be effective during deployment and discusses how these individuals can adapt these skills to prevent problems arising in the transition to the home environment. For example, the "B" in Battlemind stands for *buddies*, and the intervention discusses how personnel establish strong relationships with their operational colleagues and that when they return home they may feel that only these colleagues can truly understand what they have been through. Battlemind shows how this may lead to withdrawal from family and friends and discusses how the success personnel demonstrated in building strong relationships during deployment can be applied to the home environment.

A U.S. cluster RCT compared postdeployment Battlemind training with stress education and psychological debriefing in the U.S. Army, where it was delivered to small groups, ranging in size from 18 to 45 individuals, or to large groups, ranging in size from 126 to 225 individuals. The U.S. study did not find an overall main effect of Battlemind training; however, in troops who had experienced high levels of combat, those who received Battlemind training reported significantly fewer symptoms of PTSD, depression, and sleep problems than those who had received the standard postdeployment stress brief (Adler, Bliese, et al., 2009). The study did not find a difference between Battlemind training delivered in small versus large groups.

We examined whether an anglicized form of postdeployment Battlemind training would be of benefit for U.K. AF personnel. Differences between the U.S. and U.K. AF in terms of the prevalence of mental ill-health and also cultural and linguistic differences indicate that it would not have been prudent to simply implement Battlemind with U.K. Forces without a robust evaluation. For example, rates of probable PTSD, as assessed with the Posttraumatic Stress Disorder Checklist, Civilian Version (PCL–C; Weathers, Litz, Herman, Huska, & Keane, 1994), exceed 15% in U.S. personnel (Thomas et al., 2010), compared with 4% in the U.K. AF (Fear et al., 2010). In contrast, rates of alcohol misuse are higher in U.K. AF personnel. Fear et al. (2007) reported that 65% of U.K. AF personnel scored 8 or more on the Alcohol Use Disorders Identification Test (AUDIT; Babor, Higgins-Biddle, Saunders, & Monteiro, 2001), representing drinking at "hazardous levels"; this compares with 33% reported in a recent study of U.S. personnel (Mattiko, Rae Olmsted, Brown, & Bray, 2011).

The current study compared an anglicized postdeployment Battlemind training intervention with "standard care" (i.e., the standard stress and homecoming briefs) in a cluster randomized controlled trial (RCT). We opted to evaluate Battlemind delivered in groups of up to company size (a military unit of approximately 100 personnel). U.K. AF postdeployment briefs are normally delivered in large groups; therefore, delivery in larger groups would give the study greater external validity. This is comparable to "large group Battlemind" in the U.S. study.

The primary study hypothesis was that participants who received Battlemind would report significantly fewer symptoms of PTSD and common mental disorders than those who received the standard brief. We also hypothesized that participants in the Battle-mind arm would report fewer symptoms of depression, less alcohol misuse, and fewer stigmatizing beliefs about mental health and associated care than those who received the standard brief. Our secondary hypothesis was that, as in the U.S. study, there would be a moderating effect of combat exposure, where those who reported the highest levels of combat exposure would gain the most benefit from Battlemind.

Method

Design

The study was a two-arm cluster RCT. Cluster randomization was appropriate because military personnel work closely together in organized units; therefore, randomization by individual would have increased the risk of contamination between study arms. The unit of randomization was the company, a military unit of approximately 100 personnel. Some units, for example, logistic support squadrons, transit through the decompression facility in groups of much smaller numbers, and where this was the case, the group was the unit of randomization. Data on cluster sizes are shown in Figure 1.

Participants

Study participants were members of the U.K. AF—the Royal Navy, including the Royal Marines; the British Army; and the Royal Air Force—all of whom were returning home from deployment in Afghanistan via a postoperational decompression facility in Cyprus (Jones, Burdett, Wessely, & Greenberg, 2011). We aimed to recruit personnel exposed to potentially traumatic combat events whilst deployed; therefore, personnel from units known not to have been deployed outside the main base headquarters were excluded. As Battlemind emphasizes unit cohesion and the roles of leaders and peers in enabling help seeking (Adler, Bliese, et al., 2009), we also sought to recruit only "formed units," that is, units who normally work together in peacetime and also deploy together on operations. Thus, individuals who came through the decompression facility separately from their deployment unit were not recruited.

The required sample size was based on detecting a difference of 5 points between the study arms on the primary study outcome,

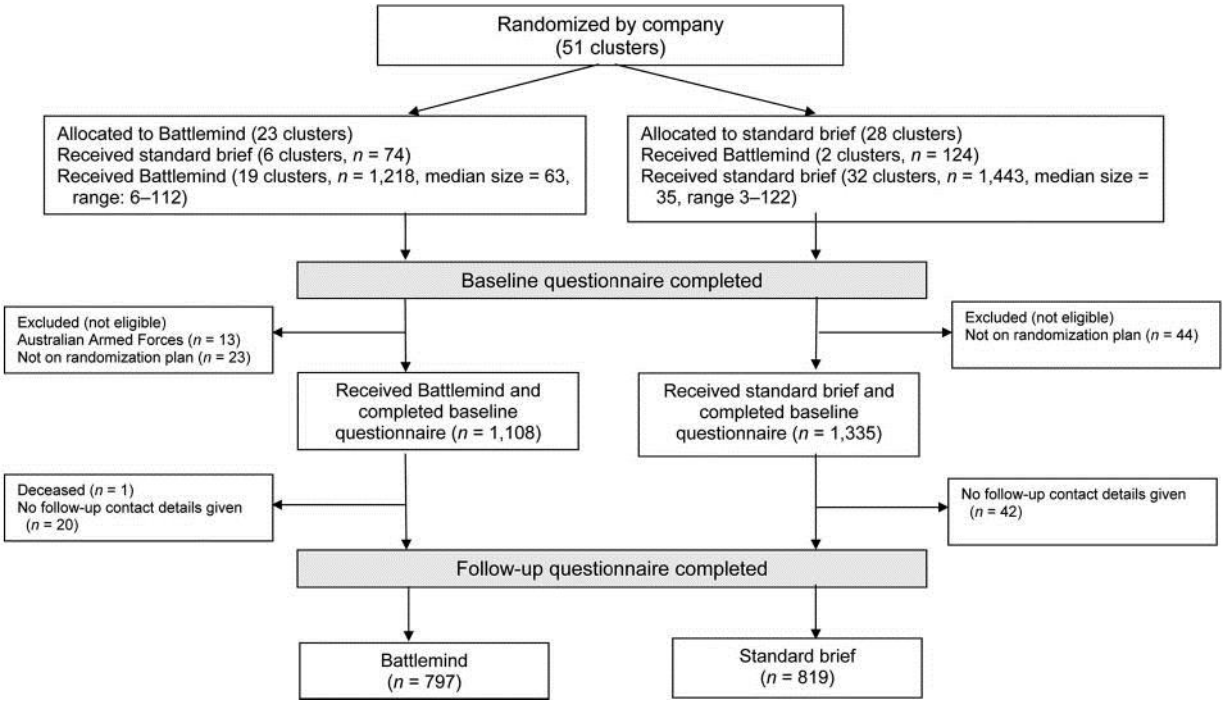

Figure 1. Flowchart of participants through randomization, intervention, and follow-up.

symptoms of PTSD measured with the PCL–C (see below for details of outcome measures). As randomization was at the company level, we took into account the correlation among companies and individuals. In the absence of a validated intraclass correlation coefficient, we explored a range of plausible values based on reports given in the published literature (Campbell, Thomson, Ramsay, MacLennan, & Grimshaw, 2004; Donner, Birkett, & Buck, 1981; Roy, Bhaumik, Aryal, & Gibbons, 2007). Consequently, the sample size required was modeled on the basis of intraclass correlations between companies of .3 and individuals of .01. To achieve a power of 80%, with a two-sided significance level of .05, we required seven companies in each arm, assuming a company size of 100, to detect a difference of 5 points on the PCL–C score.

Allowing for a loss to follow-up of 30%, we needed to recruit a total of 20 companies, 10 per study arm, or approximately 2,000 participants in total.

Randomization and Masking

Randomization was performed by a member of the research team who was blind to study arm (NTF) and was not involved in recruitment or data collection. A list of companies returning via Cyprus was received in advance by the study team; the list did not contain details of cluster composition. After removing the ineligible companies, this list was then randomized using a simple randomization process using the statistical software package Stata (Version 10).

It was not possible to conceal random group allocation sequence from the researchers who performed recruitment, as for practical reasons they had to be able to advise the military decompression team of their requirements for the briefings in advance of the units arriving at the facility. However, the researchers did not meet with the prospective study participants prior to recruitment.

Procedures

Ethical approval. This study received approval from the U.K.'s Ministry of Defence Research Ethics Committee and the King's College Hospital Research Ethics Committee. All participants gave written informed consent.

Adapting Battlemind training for the United Kingdom. The Battlemind materials include a PowerPoint presentation with facilitator notes, video clips to help illustrate some of the training points and promote discussion, and an information sheet that reinforces the information contained in the training. We wished to ensure that U.K. Battlemind remained as close to the U.S. version as possible but also that it was appropriate to U.K. culture, language, and context. To assist with the adaptation, we asked 28 U.K. AF personnel to attend a presentation of Battlemind by the U.S. team and to provide detailed feedback on any amendments that they considered necessary. This resulted in some minor changes to the U.S. format, for example, changes to colloquial terms such as "buddy" and "combat zone" in the U.S. version to "mate" and "on deployment," respectively, in the U.K. version. One of the issues dealt with in U.S. Battlemind training (the "L" of the Battlemind acronym) concerns the carrying of loaded weapons once back in the home environment. This item was considered less relevant to the United Kingdom, where gun laws are more stringent, and was deleted. We replaced it with the subject of alcohol misuse, which is addressed in the U.S. Battlemind as a subsidiary topic rather than as a formal part of the Battlemind acronym. We also received feedback that the description of combat driving was not an accurate representation of how U.K. AF personnel drive on deployment; therefore, this section was modified in line with recommendations received from U.K. AF combat driving instructors. The Battlemind videos were remade using volunteer U.K. actors from the U.K. AF working to modified U.S. scripts. We also produced a booklet based on the U.S. Battlemind information sheet. The final U.K. presentation was shown to a member of the U.S. Battlemind team, who was satisfied with the amendments made.

Members of the U.K. AF were trained to deliver Battlemind by a military psychiatrist and military mental health nurse (authors NG and NJ). A member of the U.S. Battlemind team assisted with some of the training sessions. A military mental health nurse piloted the delivery of Battle-mind to a group of approximately 30 AF personnel prior to commencement of the trial. This resulted in some minor amendments to the presentation.

Recruitment and assessment. Recruitment of personnel who had been deployed to Afghanistan on Operation HERRICK 9 (HERRICK is the codename for U.K. military operations in Afghanistan since 2002) was conducted at the U.K. postdeployment decompression facility in Cyprus during March–April 2009. When U.K. AF personnel leave Afghanistan, they make a stop in Cyprus for a period of decompression that lasts about 36 hr in order to allow personnel who fought together to begin to unwind together (Jones et al., 2011). During decompression, troops receive a stress and homecoming brief, described above. The trial involved comparing Battlemind with the standard briefs conducted during decompression.

Personnel transiting through the decompression facility were randomized to receive either Battlemind or the standard brief. A member of the research team explained the study to potential participants, who were asked to give consent before completing a baseline questionnaire (see below). They then received the appropriate brief and afterward were asked to complete a short feedback questionnaire rating utility and relevance. Questionnaire completion took approximately 10 min. The study team was allocated 1 hr for the entire procedure—brief, consent, questionnaire, and feedback. At the end of the hour, all personnel were scheduled to have a meal and attend a social event including a free bar; this ensured that we kept to time.

Four to 6 months later, participants were asked to complete a follow-up questionnaire. Where there were a large number of research participants in the same military unit, the research team visited the unit to distribute and collect the follow-up questionnaires; the remaining participants were sent a postal questionnaire. An electronic version of the questionnaire was also available and was e-mailed to personnel who had supplied an e-mail address. Nonrespondents received an average of two further questionnaire mailings. Where telephone numbers were available for nonrespondents, we called them to encourage them to return the follow-up questionnaire. All participants who completed both the baseline and follow-up questionnaires were entered into a prize draw for the chance to win one of 10 cash prizes, ranging from £50 to £500 (approximately U.S. $80 to U.S. $800).

A flowchart of participants through the trial is shown in Figure 1.

Intervention delivery. Battlemind was delivered to groups consisting of a single formed unit of up to company size. The intervention lasted approximately 45 min. The standard brief was delivered in groups that could be drawn from one or more units, depending on how many different units randomized to this arm of the study were transiting through the decompression facility on a particular day. Combining units for the standard brief reflects existing policy. The brief lasted approximately 35 min.

The interventions were delivered by a team of 12 facilitators— three Royal Navy community mental health nurses, two Royal Navy chaplains, two commissioned officers (one Army, one Royal Marines), and five noncommissioned officers (one Army and four Royal Marines)—who received training in delivery of Battlemind and the standard brief from military members of the study team. All facilitators delivered both the standard brief and Battlemind, apart from the chaplains, who delivered only the homecoming section of the standard brief. To help ensure intervention fidelity (Bellg et al., 2004), both Battlemind and the standard briefs were observed by a member of the research team. The observer completed a checklist to rate whether the facilitator had covered each phase of the brief. Facilitators were given feedback on deviations from the intended format. It was not possible to conduct a more in-depth assessment of intervention fidelity by taping the briefs as, given the interactive nature of the Battlemind training, this would have required unanimous consent of all group participants; it was not considered feasible to request this consent from such large groups.

Measures

Table 1 Assessment Measures and Time Points

Variable	Baseline	Follow-up
Demographic information		
Age	✓	
Gender	✓	
Military service		
Service (RN, RM, Army, or RAF)	✓	✓
Engagement type (regular or reserve)	✓	
Rank	✓	✓
Length of military service	✓	
No. of operational tours in past 5 years	✓	
Deployment experience		
Combat exposure	✓	
Well-being		
Common mental disorder (GHQ–12)	✓	✓
Symptoms of PTSD (PCL–C)	✓	✓
Depression (PHQ–9)		✓
Sleep problems		✓
Risk behaviors		
Alcohol consumption (AUDIT)		✓
Help seeking		
Stigma	✓	✓
Feedback on briefing	✓	✓

Note. AUDIT = Alcohol Use Disorders Identification Test; GHQ–12 = General Health Questionnaire; PCL–C = Posttraumatic Stress Disorder Checklist, Civilian Version; PHQ–9 = Patient Health Questionnaire; PTSD = posttraumatic stress disorder; RAF = Royal Air Force; RM = Royal Marines; RN = Royal Navy.

Table 1 shows the variables that were assessed at baseline and follow-up. We did not assess all outcomes at both time points due to time constraints at the baseline assessment and, as personnel are not permitted to drink alcohol during deployment, the alcohol questions relating to current drinking behavior were not asked at baseline.

The primary outcomes were symptoms of posttraumatic stress and common mental disorders. The 17-item Posttraumatic Stress Disorder Checklist, Civilian Version (PCL–C; Weathers et al., 1994), was used to assess symptoms of PTSD. The PCL–C has a possible range of 17 to 85, and a score of 50 or more is considered indicative of probable PTSD. It has been validated in U.S. military samples (Bliese et al., 2008) and has also been used in U.K. military surveys (Fear et al., 2010; Hotopf et al., 2006). Internal consistency for this measure was .92 at baseline and .94 at follow-up.

Symptoms of common mental disorders were assessed with the 12-item General Health Questionnaire (GHQ–12; Goldberg & Williams, 1988), a validated screening tool (Goldberg et al., 1997) that has been previously used with military personnel (Bridger, Brasher, Dew, & Kilminster, 2008; Fear et al., 2010; McKenzie et al., 2004). Each of the 12 questions is rated on a 4-point scale, with the responses being scored 0 or 1, giving a possible total score ranging from 0 to 12. Those who score 4 or more using this scoring method are considered possible "cases." Internal consistency for this measure was .80 at baseline and .90 at follow-up.

Secondary outcomes were depression, sleep quality, alcohol misuse, and stigmatizing beliefs regarding seeking help for or having a mental health problem. Depression was measured at follow-up with the Patient Health Questionnaire (PHQ–9; Spitzer, Kroenke, Williams, & the Patient Health Questionnaire Primary Care Study Group, 1999), a nine-item measure that can be used to give a continuous score of depressive symptoms, ranging from 0 to 27, or categorical scores for the presence of "major depressive disorder" or "other depressive disorder." It has been used previously with U.K. military samples (Iversen et al., 2009). Internal consistency for this measure was .88.

Sleep quality was assessed at follow-up with three items adapted from the U.S. Battlemind study (Adler, Bliese, et al., 2009): "How satisfied/dissatisfied are you with your current sleep pattern?" rated from *very satisfied* to *very dissatisfied*; "To what extent do you consider your sleep problem to interfere with your daily functioning (e.g., daytime fatigue, ability to function at work/daily chores, concentration, memory, mood etc.?)," rated from *not at all* to *very much*; plus the item from the PCL–C that asked about trouble falling or staying asleep, rated from *not at all* to *extremely*. All were scored on a 5-point Likert scale, giving a total score ranging from 3 to 15. Internal consistency for this measure at follow-up was .84. To account for sleep quality at baseline, we used the single item from the PCL–C in the analysis.

Alcohol use was measured with the World Health Organization's Alcohol Use Disorders Identification Test (AUDIT; Babor et al., 2001). The AUDIT is a 10-item measure that has a possible range of 0 to 40. Internal consistency for this measure at follow-up was .80. The AUDIT question about binge drinking asks how often respondents drink six or more units of alcohol on one occasion. A large proportion of U.K. military personnel have been classified as binge drinkers using this item (Fear et al., 2007), so to increase specificity, an additional question concerning "binge drinking" was included; this question asked, "How often do you have 12 or more units [of alcohol] on one occasion?" The response options are *never*, *less than monthly*, *monthly*, *weekly*, and *daily/almost daily*. A binge drinker was defined as someone who responded *weekly* or *daily/almost daily* to this question.

Stigma was assessed with an eight-item scale adapted from a measure used in U.S. military research (Hoge et al., 2004), including the U.S. Battlemind study (Adler, Bliese, et al., 2009). Respondents are asked to rate how their beliefs about having a mental health problem might affect their decision to seek help. The beliefs were that "It would be too embarrassing," "It would harm my career," "My leaders/bosses might treat me differently," "I would be seen as weak by those who are important to me," "I don't know where to get

help," "My visit would not remain confidential," "There would be difficulty getting time off work for treatment," and "I would think less of a team member if I knew he/she was receiving mental health treatment." Questions were scored on a 5-point Likert scale, giving a possible range of 8 to 40, with a higher score reflecting more stigmatizing beliefs about mental health. Internal consistency for this measure was .88 at baseline and .87 at follow-up.

Combat exposure was assessed at baseline with a 14-item measure (scale range 0–14) adapted from the U.S. Combat Experiences Scale (Hoge et al., 2004), which asked about exposure to potentially traumatic combat events, for example, exposure to an improvised explosive device (IED) or coming under small arms fire. The measure has been used in previous research with U.K. AF personnel (Fear et al., 2010). Internal consistency for this measure was .88.

The feedback questionnaire that participants completed immediately after conclusion of the brief consisted of three questions: "How satisfied are you with the briefing?", "How useful did you find the briefing?", and "How relevant is the briefing for personnel returning from deployment?" At follow-up, an additional item asked participants to rate to what extent "the brief has helped me to deal with coming home from operations." Internal consistency for this measure was .88 at baseline and .91 at follow-up.

Statistical Analysis

Analyses were conducted in Stata 10.1 and R 2.11. Results were deemed to be statistically significant when two-sided $p < .05$. Baseline characteristics of the Battlemind and standard brief study arms were compared using chi-square tests for categorical variables and either *t* tests or Mann-Whitney U tests for continuous variables, depending on data distribution. Baseline predictors of nonresponse at follow-up were examined using binary logistic regression analyses. Baseline differences between the study arms and variables that predicted nonresponse were then controlled for in the main analyses of study outcomes.

Differences in study outcomes between Battlemind and the standard brief were analyzed using mixed-effects models to take account of possible cluster effects (Hayes & Moulton, 2009; RabeHesketh & Skrondal, 2008). A two-level analysis was used, with individual nested within company. The individual was the unit of analysis, and company was entered as a random effect. For each outcome, follow-up data were regressed on study arm, corresponding baseline data where applicable, plus variables that differed between the study arms at baseline and predictors of nonresponse. The type of model used differed according to the distribution of the outcome variable in question. For binary outcome variables (caseness on the GHQ–12, PHQ–9, and binge drinking), we performed mixed-effects logistic regression models. For continuous variables that were approximately normally distributed (stigma and AUDIT total score), we used mixed-effects linear regression models, and in the case of the PCL–C and sleep scores, which were highly positively skewed and could not be transformed to a normal distribution, we used mixed-effects negative binomial regression and mixed-effects Poisson regression analyses, respectively. The statistical software package Stata 10.1 was used for the majority of the analyses presented, with R 2.11 being used for the negative binomial regression analyses.

To examine whether the intervention effect varied at different levels of combat exposure, as in the U.S. Battlemind study, we used orthogonal polynomials to create linear and quadratic interaction terms for study arm by combat exposure and added the interaction terms to the regression analyses reported above. We used orthogonal polynomials because compared with similar methods they provide better numerical accuracy for highly collinear variables and covariates are not dropped in the regression due to high colinearity (El Attar, 2006).

One hundred and ninety-eight participants received the alternative intervention from the one they were randomized to receive. This occurred when the demands of the decompression facility did not allow a sufficient number of briefs to be delivered on the day and groups had to be combined. As our primary interest is in the comparative efficacy of the interventions, the analysis presented here is based on the intervention that participants received. However, an intention-to-treat analysis was also performed to compare the groups as randomized. In addition, as a further check for any possible bias, we repeated our analyses but without these 198 participants.

We performed multiple imputation to examine whether loss to follow-up led to detectable bias in the sample of those who completed the follow-up in relation to the full study sample. Multiple imputation was performed using the ICE command in Stata and 35 data sets were imputed. We repeated the mixed-effects analyses for GHQ–12, AUDIT, binge drinking, and depression using the imputed data sets and compared these findings with the analyses of those who completed at both time points. We were unable to impute data sets for PCL–C and sleep outcomes because of the highly skewed distribution of the data.

The outcomes of the intention-to-treat analyses and analyses using the imputed data are not tabulated in this article.

Results

Study Sample

The final sample was 2,443. The study arms differed at baseline on gender, engagement type, service, and rank but not in terms of their mental health (as measured by the GHQ–12 and PCL–C; see Table 2).

The follow-up questionnaire was completed by 1,616 participants (66.1%). Participants who completed the follow-up were more likely than noncompleters to be older (odds ratio [OR] = 1.05, 95% CI [1.03, 1.06]), in the Battlemind arm of the study (OR = 1.61, 95% CI [1.36, 1.92]), in the Army rather than the Royal Marines (OR = 0.62, 95% CI [0.52, 0.74]) or the Royal Navy (OR = 0.56, 95% CI [0.33, 0.94]), and of noncommissioned officer (OR = 2.09, 95% CI [1.61, 2.71]) or officer rank (OR = 3.42, 95% CI [2.36, 4.97]) than junior rank. Time from baseline to receipt of a completed follow-up questionnaire was longer in the standard brief (M = 201 days, SD = 52) than in the Battlemind arm (M = 189 days, SD = 52). Importantly, there were no differences between completers and noncompleters in baseline mental health measures (GHQ–12 and PCL–C) or in their baseline ratings of the brief they received.

Feedback on Briefs

Feedback was mostly favorable, and there were no differences between the study arms in how participants rated the briefs (see Table 3). Ratings of satisfaction, usefulness, and relevance were lower at follow-up but also did not differ between the study arms. Helpfulness ratings at follow-up were higher in the Battlemind arm, but this difference was no longer significant in the adjusted analysis.

Main Study Outcomes

Table 4 shows per-protocol analyses with study completers. The mixed-effects models (see Table 4) found no effect of study arm on PCL–C, GHQ–12, depression, sleep, or stigma. The effect on the total AUDIT score was of borderline significance, and an effect of study arm was found on the reporting of binge drinking, with those who received Battlemind less likely to be classified as binge drinkers than those who received the standard brief.

The Impact of Combat Exposure

Participants reported a median of 7 (interquartile range [IQR] = 7) on the combat exposure scale. Higher combat exposure was associated

with higher scores at follow-up on the PCL–C (Spearman's p = .24, $p < .0001$) and AUDIT (Spearman's p = .16, $p < .0001$) but not with sleep score or GHQ–12 caseness. When level of combat exposure was controlled for (data not shown but available from the authors), the mixed-effects model findings for PCL–C, GHQ–12, depression, and stigma did not change; however, in the case of the AUDIT total score and sleep score, those in the Battlemind arm scored significantly better than those in the standard brief. We did not find any significant interaction effects for study arm by combat exposure.

Lost to Follow-Up Analysis

We reran the mixed-effects models using the imputed data. In this analysis, study arm became statistically significant in predicting total AUDIT score, with those who received Battlemind scoring lower than those who received the standard brief (mean difference = –0.78, 95% CI [–1.50, –0.07], $p < .05$). There remained a significant effect of Battlemind on binge drinking. Caseness on the GHQ–12 remained nonsignificant in the imputed data. These data were not tabulated but are available from the authors.

Intention-to-Treat Analysis

As some participants did not receive the brief to which they had been randomized, we repeated the main analyses, comparing the groups as per randomization on the outcomes of GHQ–12, PCL–C, depression, stigma, sleep, AUDIT score, and binge drinking. This analysis also found a significant positive effect of Battlemind on binge drinking. There remained no effect on PCL–C, GHQ–12 caseness, depression, sleep, or total AUDIT score. These data were also not tabulated but are available from the authors.

When analyses were repeated, excluding the 198 participants who did not receive the brief to which they had been randomized, the findings did not change.

Table 2. *Comparison of Sample Characteristics at Baseline*

Variable	Full study sample			Sample who completed follow-up		
	Battlemind (*n* = 1,108)	Standard brief (*n* = 1,335)	Test statistic	Battlemind (*n* = 797)	Standard brief (*n* = 819)	Test statistic
Gender, *n* (%)						
Male	1,095 (98.9)	1,306 (97.8)	$X^2(1) = 4.34^{*}$	789 (99.0)	798 (97.4)	$X^2(1) = 5.58^{*}$
Female	12 (1.1)	29 (2.20)		8 (1.0)	21 (2.6)	
Age in years, *n* (%)						
<25	485 (43.9)	594 (44.5)	$X^2(4) = 3.71$	327 (41.1)	316 (38.6)	$X^2(4) = 3.05$
25–29	291 (26.3)	381 (28.6)		215 (27.0)	244 (29.8)	
30–34	149 (13.5)	155 (11.6)		114 (14.3)	108 (13.2)	
35–39	120 (10.9)	129 (9.7)		95 (11.9)	95 (11.6)	
-40	60 (5.4)	75 (5.6)		45 (5.7)	56 (6.8)	
Engagement type, *n* (%)						
Regular	1,055 (96.9)	1,244 (94.4)	$X^2(1) = 8.64^{**}$	763 (97.1)	769 (94.7)	$X^2(1) = 5.66^{*}$
Reserve	34 (3.1)	74 (5.6)		23 (2.9)	43 (5.3)	
Service, *n* (%)						
Royal Navy	13 (1.2)	49 (3.7)	$X^2(2) = 83.09^{**}$"	12 (1.5)	24 (2.9)	$X^2(2) = 63.85^{**}$"
Army	710 (64.2)	622 (46.7)		545 (68.4)	403 (49.2)	
Royal Marines	379 (34.3)	662 (49.7)		237 (29.7)	392 (47.9)	
Royal Air Force	4 (0.4)	0 (0.0)		3 (0.4)	0 (0.0)	
Rank, *n* (%)						
Junior rank	798 (72.2)	1,037 (78.0)	$X^2(2) = 11.24^{**}$	546 (68.5)	589 (72.0)	$X^2(2) = 2.38$
SNCO	191 (17.3)	180 (13.5)		149 (18.7)	137 (16.7)	
Commissioned officer	117 (10.6)	112 (8.4)		102 (12.8)	92 (11.2)	
Length of military service in months, *Mdn* (*IQR*)	66 (34–129)	65 (39–119)	$Z = -0.29$	67 (36–144)	72 (42–138)	$Z = -1.53$
No. of tours in past 5 years (including HERRICK 9)						
1	474 (43.5)	561 (42.7)	$X^2(3) = 1.16$	349 (44.0)	329 (40.1)	$X^2(3) = 3.52$
2	358 (32.9)	442 (33.6)		246 (31.0)	288 (35.1)	
3	172 (15.8)	220 (16.7)		139 (17.5)	141 (17.2)	
-4	85 (7.8)	91 (6.9)		60 (7.6)	62 (7.6)	
Weeks in theater, *M* (*SD*)	24.9 (5.6)	24.7 (4.9)	$t(2148.18) = -0.88$	25.0 (5.5)	24.8 (4.8)	$t(1518.9) = -0.99$
Combat exposure score, *M* (*SD*)	6.60 (4.12)	6.87 (4.08)	$t(2428) = 1.64$	6.51 (4.15)	6.55 (4.04)	$t(1607) = 0.19$
Psychological distress (GHQ–12)						
Case, *n* (%)	169 (15.4)	198 (14.9)	$X^2(1) = 0.12$	121 (15.4)	126 (15.5)	$X^2(1) = 0.00$
PTSD (PCL–C)						
Case, *n* (%)	32 (2.9)	26 (2.0)	$X^2(1) = 2.40$	20 (2.5)	16 (2.0)	$X^2(1) = 0.60$
Continuous score, *Mdn* (*IQR*)	21 (18–26)	20 (17–26)	$Z = -0.06$	21 (18–26)	20 (17–26)	$Z = 0.39$

Note. Numbers may not total to 2,443 at baseline or 1,616 at follow-up due to missing data. GHQ–12 = General Health Questionnaire-12; *IQR* = interquartile range; PCL–C = Posttraumatic Stress Disorder Checklist, Civilian Version; PTSD = posttraumatic stress disorder; SNCO = senior noncommissioned officer.

[a] Analysis excludes Royal Air Force.

* $p < .05$. ** $p < .01$. *** $p < .0001$.

Table 3

Participant Feedback on Battlemind and Standard Briefs

	Baseline			Follow-up		
Question	Battlemind	Standard brief	Test statistic and *p* value[a]	Battlemind	Standard brief	Test statistic and *p* value[a]
How satisfied are you with the briefing?	886 (84.2)	1,072 (85.2)	*Z* = —0.23, *p* = .82	590 (75.1)	591 (73.4)	*Z* = —0.22, *p* = .83
How useful did you find the briefing?	791 (75.4)	950 (75.8)	*Z* = —0.30, *p* = .76	540 (68.7)	529 (65.7)	*Z* = —0.93, *p* = .35
How relevant is/was the briefing for personnel returning from deployment?	876 (83.6)	1,042 (83.1)	*Z* = —1.68, *p* = .09	584 (74.4)	584 (72.5)	*Z* = —0.09, *p* = .93
The brief has helped me to deal with coming home from operations	n/a	n/a	n/a	413 (52.9)	381 (47.3)	*Z* = —2.10, *p* = .04[b]

Note. Number and percentage of participants who responded *somewhat* or *very much* are shown. Numbers may not total to 2,443 at baseline or 1,616 at follow-up due to missing data. n/a = not applicable.

[a] Mann-Whitney U tests were performed using the continuous scale. [b] No longer statistically significant after adjusting for variables that differed between the groups at baseline (gender; engagement type, i.e., regular or reserve; service; and rank) and variables that predicted noncompletion of follow-up (age, study arm, service, rank).

Discussion

This study compared the efficacy of postdeployment Battlemind training with the standard stress and homecoming briefs among U.K. AF personnel. There were three key findings. First, when compared with the standard brief, Battlemind had no impact positively or negatively on participants' reporting of mental health or stigma. Second, Battlemind had a small effect on the reporting of binge drinking. Finally, U.K. AF personnel did not show a preference for Battlemind or the standard brief.

This study, like the U.S. Battlemind study, did not include a no-treatment control. Whilst the U.S. study found Battlemind to be superior to a standard briefing in terms of mental health outcomes in personnel reporting high levels of combat exposure, our results show that it did not perform any better than a standard brief whatever the level of combat exposure. There are a number of possible reasons why we did not replicate the findings of the U.S. Battlemind study in its impact on mental health and stigma. The standard brief "control" conditions differed between the two studies, so we cannot be sure that the U.K. and U.S. standard briefs were of similar efficacy. A study by Greenberg, Langston, Fear, Jones, and Wessely (2009) found that Royal Navy personnel who reported having received a stress brief that they found useful were less likely to be classified as having probable PTSD than those who had not received the brief. However, those who did not find the brief useful were no more or less likely to have probable PTSD than those who had not received a brief at all. In the current study, both briefs received high satisfaction ratings. The U.K. standard brief had been standardized in 2008, and it may be the case that it was also able to successfully address the issues dealt with in Battlemind. For example, both briefs deal with issues around recognition of mental health problems and how to get help.

Another possible explanation for our findings is the difference in mental ill-health between U.K. and U.S. service personnel (Hoge et al., 2004; Hotopf et al., 2006). For example, at baseline, mean scores on the PCL–C were higher in the U.S. sample than in this study. U.S. mean PCL–C scores at baseline were 32.6 (A. B. Adler, personal communication, June 23, 2010) compared with 23.6 (*SD* = 8.6) in our sample; therefore, the likelihood of gaining a significant improvement was lower. In contrast, alcohol misuse, including binge drinking, is high among U.K. AF, and therefore the possibility of significant improvement may be greater. The U.S. Battlemind study (Adler, Bliese, et al., 2009) did not report data on alcohol misuse, and so we are unable to compare the samples on this outcome.

U.K. troops may not benefit as much as U.S. personnel from an intervention focused on the problems of returning home because of differences in organization of the U.S. and U.K. militaries. In the United Kingdom, although personnel may be transferred to a different unit, most remain in their regiment or under command of their parent unit. The sense of working in a cohesive unit, which has been found to be an important protective factor for mental health (Iversen et al., 2008), may be greater than in the United States where there is greater use of Reserve Forces (including the National Guard) and also less emphasis on the "military family" element inherent in the U.K. AF regimental system. U.K. troops deploy for shorter periods than the U.S. troops, with other research showing the link between longer deployments and poorer mental health (Rona et al., 2007). The shorter U.K. deployments may lead to fewer problems in the transition home, and, therefore, there may be less impact of an intervention dealing with transition problems.

Unlike the U.S. study, we did not find an interaction effect between combat exposure and study arm. The U.S. study did not examine why this interaction occurred but suggested that those most at risk have most to gain from early intervention. Both studies recruited participants from deployments who had experienced high operational tempo, but the longer U.S. deployments may result in their sample having a higher exposure to traumatic events. We are unable to make a direct comparison of combat exposure in the two samples.

It is possible that we did not replicate the U.S. findings on mental health due to differences in the way Battlemind was delivered in the different studies. However, we collaborated with members of the U.S. Battlemind team in adapting the intervention to the U.K. context and in training AF personnel to deliver the intervention. We also observed the sessions to ensure the fidelity of the interventions. The positive effect found on reporting of binge drinking also suggests that the messages of Battlemind were effectively communicated. Therefore, differences in delivery are unlikely to explain the differences in our findings.

Our finding that Battlemind had an effect on the reporting of binge drinking is of particular interest given that levels of alcohol misuse are high among the U.K. AF (Fear et al., 2007, 2010). The issue of using alcohol to deal with problems is addressed in both U.K. briefs, but it is dealt with in slightly more depth in U.K. Battlemind than the standard brief. Both briefs advise against "self-medicating" with alcohol, but the anglicized Battlemind also discusses the link between alcohol and aggression and its possible consequences. Battlemind also encourages personnel to recognize alcohol misuse by a colleague as a sign of possible problems and to offer support. It is a possibility that any effect on binge drinking could result from the changes that were made to the anglicized Battlemind regarding alcohol, rather than to Battlemind as a whole. The subject of alcohol misuse was included as part of the Battlemind acronym rather than an additional topic at the end of the presentation; however, overall the changes made in "translating" Battlemind were not substantial.

The effect on binge drinking was small and must be considered in light of the effect on the total AUDIT score, which was of borderline significance. However, considering that alcohol dependence is rare in the U.K. military and binge drinking is highly prevalent, our findings are quite relevant to the target population. Our findings for binge drinking should also be treated with some caution, given that we examined several outcomes, thus increasing our likelihood of chance findings. But if the finding for binge drinking is valid, it is consistent with the findings of a Cochrane Review of brief alcohol interventions in primary care (Kaner et al., 2007), which has shown that it is possible for a low intensity intervention to produce a reduction in alcohol intake. The interventions in the Cochrane Review were varied, including one or more of motivational interviewing, cognitive behavioral therapy, self-completed action plans, leaflets, drinking diaries, written personalized feedback, follow-up telephone counseling, and exercises to complete at home. One recommendation of the review was that future trials should focus on delineating the most effective components of interventions.

Limitations

Our findings must be interpreted in light of some study limitations. We did not include a "no treatment" control arm; therefore, our finding of no difference between the study arms on mental health and stigma does not inform us as to whether the effect of both briefs on these outcomes was positive, negative, or neutral. The U.K. standard brief has not undergone prior empirical evaluation in comparison to a no treatment control, so its effectiveness has not been established. Postdeployment briefs are mandated for the U.K. AF; therefore, it was not possible to remove "usual care" for ethical reasons.

Battlemind was of longer duration than the standard brief, so we cannot be sure that any differences were not attributable simply to a subject being given more time rather than to the way in which topics were addressed.

Assessment of intervention fidelity was limited in that we were unable to record sessions. Reliability of our assessment could have been strengthened by using more than one observer and checking interrater reliability; however, we did not have sufficient research personnel at the decompression facility to enable this.

We did not assess all outcomes at both time points, so we cannot be certain that any difference seen at follow-up was not also present at baseline. This was a necessary restriction on the baseline assessment, as we were not able and did not wish to burden personnel who were on their first day out of Afghanistan with a time-consuming assessment. A maximum time of 1 hr was allocated for completion of the questionnaires and delivery of the briefs to ensure that all decompression activities could be completed. It is a limitation of the study that we were unable to control for predeployment alcohol consumption. U.K. personnel do not drink alcohol while deployed (a prohibition that is rigorously enforced); therefore, assessing this behavior at baseline was not necessary. However, our findings may have been strengthened if we had asked about predeployment drinking at baseline. We did control for all variables on which the study arms differed at baseline, that is, gender, engagement type, service, and rank. The analyses of alcohol scores controlled for variables that are known to be associated with higher alcohol intake, such as male gender and younger age (Fear et al., 2007); therefore, we consider it reasonable to conclude that the difference found was likely to be a result of the intervention.

Some participants did not receive the brief to which they had been randomized. This was an inevitable consequence of undertaking research in a challenging environment. Military requirements and demands took priority, which sometimes necessitated study changes; for instance, some personnel randomized to receive a condition on a particular date did not arrive in Cyprus and had to be reallocated to an alternative intervention because of space and time demands. When the analyses were repeated on an intentionto-treat basis, study outcomes were not altered.

The follow-up response rate of 66% compares favorably with other studies of postdeployment briefs in the AF (Mulligan et al., 2011) and with the U.S. Battlemind study response rate of 46% (Adler, Bliese, et al., 2009).

In spite of randomization, there were significant differences between the study arms at baseline. Given that randomization was done at company level, it is more difficult to achieve an equal distribution of demographic variables, and we did adjust for these baseline differences in all our analyses. The use of a stratified randomization may have helped to avoid uneven baseline distribution; however, this was not possible, as we did not have sufficient advance information about the companies on which to stratify.

We obtained a differential response rate between the Battlemind and standard brief arms of the study, which is difficult to explain. We did not advise participants that they were receiving the “old” or “new” brief. It may be that those who received Battlemind recognized that it was a novel approach and were more motivated to respond at follow-up; however, it may be just a chance finding. Data analyses using the imputed data indicate that this difference in follow-up rates did not bias our findings.

We recruited mainly Army and Royal Marine personnel to the study, which is to be expected, as they form the majority of personnel on deployment in Afghanistan and the bulk of those who were returning via the decompression facility in Cyprus at the time of study recruitment. In the U.S. study (Adler, Bliese, et al., 2009), Battlemind was evaluated only in Army personnel, so care should be taken before generalizing to the other services for both studies. The difference between our findings and those of the U.S. Battle-mind study does, however, suggest that other countries that may be considering using Battlemind should also conduct their own evaluation.

Conclusions

Whilst an anglicized version of postdeployment Battlemind did not improve mental health for U.K. AF personnel compared with the standard postdeployment brief, it did have a modest impact on reporting of binge drinking. Given that alcohol misuse is problematic in the U.K. AF, an intervention that reduces binge drinking (one of the behaviors associated with alcohol misuse) may be helpful.

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Table 4. Mixed-Effects Model Results

	PCL-C total score		GHQ-12 caseness		Depression caseness (major or other depression)		Sleep, total score		Stigma, total score			AUDIT, total score			Binge drinking, caseness	
Variable	Coef	SE	Adj. OR	95% CI	Adj. OR	95% CI	Adj. IRR	95% CI	Coef.	95% CI	SE	Coef.	95% CI	SE	Adj. OR	95% CI
Baseline score	0.02	0.00***	6.89	[4.96, 9.56]***	n/a		1.23	[1.21,1.26]***	0.50	[0.46, 0.55]***	0.02	n/a		n/a	n/a	
Study arm																
Standard brief			1.00		1.00		1.00								1.00	
Battlemind	-0.00	0.02	0.84	[0.57, 1.23]	1.12	[0.71, 1.77]	0.95	[0.90, 1.01]	-0.41	[-1.00, 0.19]	0.30	-0.73	[-1.45, -0.001]	0.37	0.73	[0.58, 0.92]**

Note. Data adjusted for baseline score (where applicable) age, gender, service, rank, engagement type, months to follow-up, and study arm. Analyses exclude Royal Air Force because of small numbers. Adj. = adjusted; AUDIT = Alcohol Use Disorders Identification Test; Coef = coefficient; CI = confidence interval; GHQ–12 = General Health Questionnaire; IRR = incidence rate ratio; n/a = not applicable; OR = odds ratio; PCL–C = Posttraumatic Stress Disorder Checklist, Civilian Version; SNCO = senior noncommissioned officer.
~~ $p < .01$. ~~~ $p < .001$.