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PREP for Strong Bonds: A review of outcomes from a randomized clinical trial

Elizabeth Allen,

Psychology, University of Colorado Denver

Scott Stanley,

Psychology, University of Denver

Galena Rhoades, and

Psychology, University of Denver

Howard Markman

Psychology, University of Denver

Abstract

To help address the relationship needs of service members, there have been a number of programs offered within active duty and veteran contexts. One program, offered within the Strong Bonds portfolio delivered by Army Chaplains, is PREP for Strong Bonds (PREP = the Prevention and Relationship Education Program). PREP has a number of empirically based and tested variants. This article reviews the disseminated research regarding results from a large randomized clinical trial designed to test the effectiveness of PREP for Strong Bonds. From a sample of 662 Army couples drawn from two sites, outcome papers have focused on different subsamples, marital outcomes, follow up time points, and moderators. Reviewing these disseminated outcomes, we conclude that PREP for Strong Bonds has significant divorce reduction effects at one site; these divorce effects were found at both one and two years post intervention, and were moderated by factors such as minority status, economic strain, and cohabitation history of the couple. In terms of marital quality outcomes, some modest overall effects were found pre to post intervention, but there were no overall marital quality outcome effects two years post intervention. However, marital quality outcomes are significantly moderated by infidelity and cohabitation history, with couples reporting these risk factors showing greater positive marital quality outcomes. These results to date are discussed in terms of clinical and research implications as well as directions for future work, such as examining longer term preventative effects.

As well detailed by other contributions to this special issue, military couples and families face a number of stressors, particularly in the context of recent extended and dangerous military commitments in Iraq and Afghanistan. For military families, stressors include extended and repeated separations, risk of injury and death during deployment, high work

Correspondence concerning this article should be addressed to: Elizabeth Allen, Department of Psychology, University of Colorado Denver, Denver, CO 80217., elizabeth.allen@ucdenver.edu.

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demands even when not deployed, and frequent moves (see review by Link & Palinkas, 2013). Deployment has been identified as one of the primary stressors for military spouses (Dimiceli et al., 2010) and stress regarding deployment is related to higher levels of marital conflict (Allen, Rhoades, Stanley, & Markman, 2011). When service members incur injuries or mental health problems such as PTSD, these issues may also be associated with higher levels of marital distress, marital conflict, and intent to divorce (Allen, Rhoades, Stanley, & Markman, 2010; Foran, Wright, & Wood, 2013; MacDermid Wadsworth, 2010). In addition, selection factors associated with risk (e.g., younger age at marriage for military compared to civilian couples) may further contribute to marital distress and instability for Army couples (Hogan & Seifert, 2010, Karney & Crown, 2007).

Given the need to support military and veteran couples and families in the context of these risks and stressors, a number of programs have been developed for or implemented with this population. Examples include brief expressive writing interventions for military couples (Baddeley & Pennebaker, 2011), a trauma focused skills based prevention program for military families entitled Families Overcoming Under Stress (FOCUS; Beardslee et al., 2011), and Strength at Home (Taft et al., 2014) which aims to prevent intimate partner violence among military couples. Moreover, couples based approaches for specific mental health issues are often offered within active duty or veteran facilities; these approaches include conjoint therapy for PTSD (Monson & Fredman, 2012) and Behavioral Couple Therapy for alcohol (O'Farrell & Fals-Stewart, 2000; see review by Sayers, Glynn, & McCutcheon (2014) of VA family centered care). MacDermid Wadsworth et al. (2013) reviewed a number of these interventions and the importance of couples and family based support, prevention, and intervention services within the military.

One such set of services is the Strong Bonds Program in the Army. Strong Bonds is a system of relationship education programs offered by Army Chaplains, with specific programs for unmarried soldiers, military couples, and military families, including programs on deployment and reintegration (www.strongbonds.org). One Strong Bonds program is an adaptation of the Prevention and Relationship Education Program (PREP; Markman, Stanley, & Blumberg, 2010). PREP is a couples based psychoeducational prevention program. It is typically delivered to groups of couples in a workshop format, with a focus on communication and conflict management, problem solving, the preservation of fun and friendship, and relationship expectations and commitment. Relationship education is distinct from therapy. For example, relationship education is often offered in groups and partners are usually not asked not to reveal personal information to the group (see Halford & Snyder, 2012; Markman 2014; Markman & Rhoades, 2012 for details). Overall, relationship education interventions have shown positive effects in improving marital quality or maintaining high relationship satisfaction (Blanchard, Hawkins, Baldwin, & Fawcett, 2009; Halford & Bodenmann, 2013; Hawkins, Blanchard, Baldwin, & Fawcett, 2008). Additionally, there have been a number of specific studies showing positive effects of PREP and variations of PREP (Markman & Rhoades, 2012), resulting in PREP being classified as an efficacious program (Institute of Medicine, 1994; Jakubowski, Milne, Brunner, & Miller, 2004) that is listed in the SAMHSA National Registry of Evidence-based Programs and Practices.

Recognizing the need for support services for Army couples, the U.S. Army Chaplain Corps began to systematically train chaplains in PREP in the 1990s. Over time, PREP was adapted for use with Army couples (see Markman, 2014 for an overview of PREP's approach to dissemination). Relationship education is also being used broadly across the military (see Bakhurt, Lowe, McGuire, Halford & Markman, 2014). With increased utilization of the adaptation of PREP for the Army, in 2001 the U.S. Army Chief of Chaplains office funded a study on the effectiveness of a version of PREP that was being used at that time by Army chaplains with Army couples (called "Building Strong and Ready Families"). This study found positive pre-post effects on a range of marital variables, such as relationship satisfaction, communication, and confidence in the marriage to survive over time (Stanley et al., 2005). These positive effects held for husbands and wives, racially and ethnically diverse couples, and couples of different income. However, this study lacked a control group, making it difficult to isolate intervention effects from confounding variables such as time and measurement effects. Further, outcomes were measured only at pre, post, and one month follow up, which precluded any assessment of longer term prevention or intervention effects. Moreover, the intervention itself continued to evolve based on emerging empirical findings in the field regarding factors important to couple functioning and with greater improvement of materials (including unique images and examples pertinent for Army couples, more focus on stress management and emotional support, etc.). Thus, a larger randomized clinical trial was undertaken to evaluate a version of PREP adapted for the Strong Bonds program, called PREP for Strong Bonds (which was later called Fearless Marriage and will soon be called PREP 8.0). The randomized clinical trial of this program began in 2007, with 662 couples enrolled across two different Army forts. The central purpose of this trial is to examine the short and long-term effects of PREP for Strong Bonds on marital quality and divorce. *The focus of the current review is to describe the intervention, the overall research design, and findings from this trial that have been disseminated to date.*

Description of the Intervention

PREP for Strong Bonds is a psychoeducational workshop delivered to groups of couples by trained Army chaplain leaders. It is designed to prevent or ameliorate marital distress and divorce by helping couples lessen risk (e.g., negative communication patterns) and increase protective (e.g., positive interactions) factors empirically related to marital outcomes.

PREP continues to evolve given new empirical findings, applications to specific contexts and populations, and pedagogical improvements (for example, use of distinctive modules, animations, and lecture doodles). The version of PREP evaluated in this research consists of two parts: a one-day training on post followed by a weekend retreat at a hotel off post, with a total of 14.4 hours of planned content. In PREP, the inclusion of skills coaches in addition to program leaders is optional. In this trial, there were not individuals specifically designated as skills coaches; however, chaplains were trained to coach and were encouraged to circulate during exercises to support couples' practice and there was often more than one chaplain leader per group. The intervention consisted of 16 lessons or modules: (1) background on the program, (2) how to express partner appreciation, (3) recognizing communication danger signs, (4) the need for and how to take a time out, (5) speaker listener communication skills,

(6) relaxation and affect regulation skills, (7) the need for fun and friendship, (8) recognizing underlying issues/events and problem solving communication skills, (9) personality differences and implications for the relationship (10) providing partner support, (11) identifying negative interpretations, (12) relationship maintenance during deployment, (13) understanding and sharing love and support (14) reviewing core beliefs, expectations, and future decisions in the relationship, (15) forgiveness, and (16) commitment. Most lessons included a presentation portion from the leader, illustrative audio or video clips, and a couple exercise to consolidate the lesson.

Although there is likely to be variability in how chaplains actually implement the program outside of this research trial, such as omitting some lessons or adding additional content, for the randomized clinical trial we strove for adherence to this version of PREP being used within Strong Bonds. In addition to the standard training all Army Chaplains receive in PREP, the research team provided supplementary training regarding this version of PREP for Strong Bonds for most of the chaplains providing the intervention for this study. All chaplains leading the study intervention received manualized intervention materials (i.e., a detailed, scripted leader manual, PowerPoint slides, couple manuals, exercises, and video/audio components) (Stanley, Markman, Jenkins, & Blumberg, 2006). Chaplains were asked not to significantly deviate from the manual either in terms of omitting material or adding extraneous material. In fact, in order to be able to code fidelity to the intervention, chaplains were instructed to record all portions of the intervention. Not all portions were recorded, sometimes due to technical difficulties with the equipment and sometimes due to chaplain oversight. We obtained adequate audiorecording of 20 out of the 27 iterations of the intervention to code chaplain fidelity to the lesson material from 1 (Very poor) to 5 (Excellent). The inter-rater correlation for coding fidelity was .88, and the average fidelity rating across sites was 4.10.

Research Design

Approval for all procedures was obtained from our university IRB as well as the U.S. Army IRB (Dwight David Eisenhower Army Medical Center). To be eligible for the study, couples had to be married, age 18 or over, fluent in English, with at least one spouse in active duty with the Army. Couples could not have already participated in PREP, and they had to express willingness to be randomly assigned to intervention or to an untreated control group, which can be considered treatment as usual.

Recruitment was conducted on two separate Army forts via brochures, media stories, posters, and referrals from chaplains. The study was presented as long term research, where couples would be asked to complete multiple assessments (each taking approximately 1 hour) over the course of the next four years, with compensation for the assessments ranging from \$50 to \$90 per person per assessment (compensation increasing over time). Couples enrolled by directly contacting study staff, who conducted the eligibility screenings with the couples.

Prior to the intervention, couples completed baseline (pre) questionnaires under the supervision of study staff to ensure confidentiality (e.g., spouses could not see each other's

responses). After the couple completed pre assessment, they were randomly assigned to the intervention group or the control group. In total, 343 couples were assigned to intervention and 319 were assigned to the control group. Across the two sites, there were 27 iterations of the program to accommodate multiple groups of couples. These groups of couples are referred to as “cohorts”. After a couples’ cohort completed the intervention, couples completed post questionnaires, again under the supervision of study staff.

Approximately every six months after the intervention, each participant was emailed a unique personal link to an online survey that could be completed from anywhere in the world, and even during deployment. This was the typical mode of data collection for each follow up, although individuals could opt for a mailed, hard copy questionnaire if they preferred. Although follow up assessments with these participants continue (now once per year), the existing disseminated outcome research reviewed here includes outcomes for up to two years post intervention.

As noted, we evaluated the intervention at two forts (i.e., Sites 1 and 2). There were a number of differences between sites. Site 1 included 478 couples and Site 2 included 184 couples. At Site 1, intervention couples received the training in classes comprised of other couples from their unit, with their unit chaplains leading the intervention. This is the most typical way these services are offered in the U.S. Army, and it is the main approach strategically reinforced from the Chief of Chaplains office in the U. S. Army. Site 1 also consisted of major war-fighting units which were preparing for deployment for the OIF surge at the time of the intervention.

Although we had completed the intervention phase with a large sample at Site 1, we were concerned about the high operational tempo at Site 1 and how it might impact the implementation of the intervention. For example, the high operational tempo at Site 1 caused some disruptions in the delivery of the intervention, including rescheduling of some iterations due to conflicts with training calendar. Our contacts in the Army Chief of Chaplains office provided the opportunity to also conduct a smaller test at a second site where couples were not as exposed to the stress of ramping up for deployment, and where leaders were drawn from a group of highly-trained family life chaplains who delivered the intervention in classes composed of couples from across the installation. Hence, Site 2 couples were not trained by their own unit chaplains, nor trained in groups with other couples from their units, meaning that the Site 1 procedures are more typical of the way such services are delivered in the Army than Site 2. As detailed in Allen et al. (2011b), couples at Site 2 were married longer, were older, had higher income and husband rank, and had lower rates of recent deployment compared to couples at Site 1 (all significance levels $<.01$). This is consistent with the nature of the couples available for participation in the study at the two sites. That is, Site 2 is a major training installation, where many of the junior enlisted are there for training and are not allowed to bring their spouses. Those available for participation were mostly drill sergeants, instructors, soldiers in senior regiments, and senior enlisted soldiers or officers in training programs where spouses can accompany them. Thus, these individuals are typically more senior and involved in training operations. These differences may be markers for less contextual stress for the couple and possible concomitant risk. However, whereas couples at Site 2 differed from couples at Site 1 in

these contextual and sociodemographic variables, they did not differ from couples at Site 1 on baseline levels of the measured relationship constructs (with the exception of husbands at Site 2 reporting significantly higher levels of baseline forgiveness).

The intervention phase was completed at Site 1 in 2007 and at Site 2 in 2008. The findings to be reviewed here include a range of marital outcomes for these couples, and moderators of these outcomes, analyzed and presented in five papers. In accordance with the goals and available data for each individual paper, there has been variability regarding the actual sample size used for analyses, based on factors such as (1) whether both sites were included in the analyses or only Site 1, (2) the time point evaluated, ranging from immediately post intervention to two years after intervention, (3) whether only participants who were still married at a given time point were evaluated or whether more participants could be evaluated. See Table 1 for a summary of the studies reviewed here, including their respective sample sizes and response rates.

As required by the Consolidated Standards of Reporting Trials (CONSORT), each of these papers has included a CONSORT table, which is a general flow chart of interested and enrolled participants through phases of the study. However, the variability of specific subsamples and timepoints used per study has resulted in some differences in the CONSORT table between papers. For the purposes of this review, we use the data reported on in Stanley et al. (2014) that had the broadest set of parameters (i.e., both sites, two year outcomes, virtually all couples evaluated on divorce outcomes) to develop the flow chart shown in Figure 1. As seen in Figure 1, there are a large number (698) couples in the “other” category for non-participation. The reason for most couples to be in this category is reported scheduling problems. Before enrollment, we attempted to confirm that the couple would be able to attend the intervention if so assigned, and 361 couples told us that they would be unable to fit the available intervention to their schedules. Before enrollment at Site 2, couples were asked to get an approval sheet from the unit staff sergeant confirming the release of the soldier to attend intervention on a given date if assigned. An additional 241 couples from Site 2 simply did not follow up with study staff or did not return this approval sheet; reasons for this lack of follow up may include scheduling difficulties, disinterest, or other reasons. We had an additional 29 couples who did not show up for their pre assessment, 32 couples who would not have been able to attend a given iteration because it was full, and the remainder of couples coded by study staff as “miscellaneous” reasons for nonparticipation.

Our analyses use rigorous intent to treat procedures (i.e., analyzing based on randomly assigned group regardless of whether the couples actually attended the intervention). As shown in Figure 1, 52 couples who were assigned to PREP did not attend any portion of it (with the remainder of assigned couples receiving at least some of the intervention). Of these couples, 42 husbands and 41 wives provided data at the post assessment regarding barriers to participation. The most common cited reasons were difficulty with childcare (36% of husbands and 37% of wives endorsed this) and difficulty getting time off from job or unit responsibilities (e.g., 33% of husbands endorsed unit responsibility conflicts). Other, infrequently endorsed, reasons included illness of self or family member, difficulties with travel, or disinterest in the intervention. At Site 1, 60% attended all, 23% attended part (e.g.,

either attending the first day of training on a duty day or attending the weekend retreat, but not both), and 17% attended none of the intervention. At Site 2, 54% attended all, 35% attended part, and 11% attended none of the intervention. Two couples assigned to the control group attended the intervention at Site 1.

Data on marital status at one and two years follow up have been remarkably complete, as study staff were able to ascertain marital status by (1) surveys or (2) personal contact with at least one member of the couple for most couples who had not completed surveys. As seen in Figure 1, we were able to categorize 660 of the original 662 couples into divorced or married two years after the intervention. Although response rates have been generally high, there is more missing data on marital quality outcomes at various time points due to participants either not responding to the invitations to complete the survey or because of divorce (which means marital quality was not assessed). Table 1 provides response rates per study reviewed here.

Participants

The overall sample included 662 Army couples. At the baseline (pre) assessment, husbands averaged 28.5 years of age ($SD = 5.9$); wives 27.7 ($SD = 6.2$). Seventy-one percent of wives were white non-Hispanic, 11% Hispanic, 10% African American, 1.8% Native American/Alaska Native, 0.9% Hawaiian or Pacific Islander, 0.8% Asian, and 4.1% endorsed mixed race/ethnicity. Sixty-nine percent of husbands were white non-Hispanic, 12% Hispanic, 11% African American, 1.4% Native American/Alaska Native, 1.1% Asian, 0.6% Hawaiian or Pacific Islander, and 4.6% endorsed mixed race/ethnicity. Almost all husbands (97.3%) were Active Duty Army (2.4% were in the study due to the wife being Active Duty Army), whereas almost all wives (91.6%) were civilian spouses of Active Duty Army males (8.1% of wives were Active Duty Army or Reserves). Husbands' median income (endorsed by 31.2% of men) was between \$30,000 and \$39,999 a year, while wives' median income (endorsed by 67.6% of women) was under \$10,000 a year. High school or an equivalency degree was the median highest degree (67% of husbands and 53% of wives). At the pre-assessment, couples had been married an average of 4.93 years, and 74% reported at least one child living with them at least part-time.

Core Outcome Measures

As seen in Table 1, our outcome evaluations have focused on divorce and measures of marital functioning. Below we describe the measures used for these outcomes. Short forms were used when possible to minimize participant burden. All measures described below demonstrated adequate relevant psychometrics in the current sample (e.g., internal consistency, logical convergence among constructs). Additional details regarding any measure listed below are available in the reviewed papers or from the first author. As part of the larger study, we have assessed several other constructs including: child and family functioning (e.g., parenting alliance), mental health functioning (e.g., PTSD, global distress), deployment variables (e.g., communication during deployment), and Army specific variables (e.g., use of services, exposure to combat). Analyses with these and other variables have been included in our basic science papers emerging from these data (e.g., Bergmann et

al., 2014; Renshaw et al., 2014; Allen et al., 2011a), but have not yet been evaluated as program outcomes or moderators/predictors of program effects.

Participants' evaluation of program—For couples who attended the intervention, husbands and wives were asked to evaluate the program with a number of questions regarding positive program impact, program enjoyment, program helpfulness, overall satisfaction, whether they would recommend the program to a friend, and leader quality.

Divorce—Divorce status was assessed using the following question in the standard survey: “Have you and/or your spouse filed for divorce or obtained a divorce?” Couples who answered “no” were coded as not divorced. Additional divorce outcomes (from couples who did not complete follow up surveys) were obtained via the participants informing study staff of their status.

Divorce proneness—Three items were adapted from the short form of the Marital Instability Index (MII; Booth, Johnson, & Edwards, 1983) to measure divorce proneness. These items assess thoughts that one's marriage may be “in trouble”, consideration of separation or divorce, and couple-level suggestion of divorce. Participants rated these items on a 1 to 7 scale, and responses were averaged to create a total score.

Marital satisfaction—The Kansas Marital Satisfaction Scale (KMS; Schumm et al., 1986) was used to assess marital satisfaction. This is a brief (3-item) scale assessing satisfaction with the marriage, the partner as a spouse, and the relationship with spouse. This scale has strong reliability and validity (Schumm et al.) and provides a pure global satisfaction rating without including other aspects of relationship functioning, which has been considered a potential problem with more omnibus measures of relationship adjustment (Fincham & Bradbury, 1987).

Communication skills—From the larger Communication Skills Test (Saiz & Jenkins, 1995), ten items were employed to measure the type of communication skills taught in PREP and other similar relationship education programs, while avoiding the specific jargon of PREP (e.g., “speaker-listener skills”). Example statements include “When discussing issues, I allow my spouse to finish talking before I respond,” and “When our discussions begin to get out of hand, we agree to stop them and talk later.” Studies support the reliability and validity of this measure (Stanley et al., 2001; Stanley et al., 2005).

Confidence—Five items from the Confidence Scale (Stanley, Hoyer & Trathen, 1994) were selected to assess participants' level of confidence in their marital strength and stability. Example items are “I believe we can handle whatever conflicts will arise in the future” and “I am very confident when I think of our future together.” The larger scale has shown good evidence of reliability and validity (e.g., Kline et al., 2004; Whitton, Rhoades, Stanley, & Markman 2008); these items are representative of the larger scale.

Positive bonding—The Positive Bonding Scale was adapted from the Couple Activities Scale (Markman, 2000). It consists of 9 questions assessing the friendship, intimacy, fun, felt support, and sensual/sexual relationship of the couple. Example questions include “We

regularly have conversations where we just talk as good friends,” “We have a satisfying sensual or sexual relationship,” “I feel emotionally supported by my partner,” and “We regularly make time for fun activities together as a couple.” Stanley, Whitton, Kline, and Markman (2006) report logical convergence of the parent scale with other indices of individual and marital functioning.

Dedication—Based on the Dedication Scale from the multidimensional Commitment Inventory (Stanley & Markman, 1992), five items reflecting couple identity, long term view, and priority of the relationship were selected. Example items are: “My relationship with my spouse is more important to me than almost anything else in my life.” And “I want this relationship to stay strong no matter what rough times we may encounter.” Studies support the reliability and validity of variations of this scale (e.g., Owen et al., 2011).

Satisfaction with sacrifice—To assess this construct, we utilized three items from the Satisfaction with Sacrifice Scale, which is also part of the Commitment Inventory (Stanley & Markman, 1992). Items reflect positive feelings and satisfaction derived from sacrificing for the spouse (e.g., “It makes me feel good to sacrifice for my spouse”). The parent measure converges as expected with other relationship constructs; for example, predicting future marital adjustment and mediating the relationship between male dedication levels and future marital adjustment (Stanley, Whitton, Low, Clements & Markman, 2006).

Forgiveness—4 items from the 6 item Marital Forgiveness Scale (Fincham & Beach, 2002) were used. We used 2 of the 3 positive items (When my partner wrongs me, I just accept their humanness, flaws and failures; I am quick to forgive my partner) and 2 of the 3 negative items (I think about how to even the score when my partner wrongs me; If my partner treats me unjustly, I think of ways to make them regret what they did.)

Negative communication—The 4-item version of the Communication Danger Signs Scale (Stanley & Markman, 1997) was used to assess problematic communication patterns. Items reflect escalation (“Little arguments escalate into ugly fights with accusations, criticisms, name calling, or bringing up past hurts”), invalidation (“My spouse criticizes or belittles my opinions, feelings, or desires.”), negative interpretation (“My spouse seems to view my words or actions more negatively than I mean them to be.”), and withdrawal (“When we argue, one of us withdraws...that is, does not want to talk about it anymore, or leaves the scene.”). Forms of this measure have demonstrated convergence with other theoretically related constructs and predicted changes subsequent to communication skill interventions (e.g., Stanley et al., 2005).

Summary of Disseminated Outcome Results

Satisfaction with the Program

Overall, participants generally rated the program well, with average high ratings of the program as impactful, helpful, and enjoyable, and average high satisfaction with the program. At Site 1, the overall rating across these dimensions was 6.12 on a 7 point scale where higher scores indicate more positive impact. The average leader quality rating was

4.13 based on a scale from 1 (needs improvement) to 5 (excellent). At Site 2, average program ratings were 6.05 and average leader quality rating was 4.31.

Overview of Outcome Analyses

Our evaluation of the program has spanned basic pre-post change, longer term (out to two years) outcomes of divorce and marital quality, and moderators of these effects. The specific analytical approach has differed per paper, but always involves: (1) rigorous intent-to-treat procedures (Hollis & Campbell, 1999) where couples are analyzed according to the group they were assigned to, regardless of whether they actually did or did not receive the intervention. This method preserves the random assignment of the groups, thereby avoiding selection effects that can impact the interpretation of studies where participants choose or do not choose to participate in an intervention. (2) methods that account for dependency among variables such as time, gender (i.e., husbands and wives), and cohort (i.e., couples assigned to the same iteration of the intervention). These approaches have ranged from repeated measures analyses (e.g., Allen et al., 2011b) to four level hierarchical linear modeling (e.g., Allen et al., 2012).

Pre Post Change

Allen et al. (2011b) modeled basic pre to post change for the intervention group compared to the control group for eight different outcomes. Results were presented separately for Sites 1 and 2. At Site 1, significant intervention effects were found for half of the variables analyzed: communication skills, confidence, positive bonding, and satisfaction with sacrifice. There was also a trend toward significance for marital satisfaction. Specifically, for one significant effect (communication skills), the PREP for Strong Bonds group improved more from pre to post relative to the control group. For other significant effects (confidence, positive bonding, satisfaction with sacrifice), the PREP for Strong Bonds group improved just slightly or maintained levels of the construct while the control group declined from pre to post. At Site 1, the effect sizes were modest for those outcomes showing significance, ranging from .11 to .20 (Cohen's *d*). There were no significant impacts (or trends) for forgiveness, dedication, and negative communication.

At Site 2, significant intervention effects were more limited. Four of the eight outcomes analyzed had effect sizes commensurate with significant change at Site 1, but with the smaller sample size at Site 2, only communication skills change was significant. Couples assigned to the intervention increased significantly more in communication skills relative to control couples ($d = .27$). There was a trend for forgiveness, in which couples assigned to the intervention increased while control couples declined slightly. There were no significant impacts (or trends) for the other outcomes assessed.

Thus, we found modest evidence for pre post improvement or maintenance of quality on some outcomes. There were no instances in which the intervention appeared to be related to worsening functioning over time relative to control couples. It is important to note that PREP for Strong Bonds is a universal preventive intervention program, and all interested married couples could enroll regardless of current marital distress. Whereas there was some variability for our participants in marital functioning at the pre (baseline) assessment, in

general couples reported high initial levels of marital quality (e.g., $M = 5.67$, $SD = 1.27$ on the marital satisfaction scale and $M = 6.57$, $SD = .68$) on the dedication scale; both of these scales can range from a low of 1 to a high of 7). Thus, these results are consistent with what would be predicted in general for such a sample receiving relationship education (see Halford & Bodenmann, 2013), as the overall high marital quality at baseline may have produced ceiling effects that can affect the ability to detect pre to post change. Recall that some of the intervention effects at Site 1 related more to preserving initial levels of some aspects of marital adjustment (rather than increasing levels of adjustment) while the control group worsened, which may be consistent with the notion that the intervention provided support for these couples during a stressful time (e.g., high operational tempo and impending deployment).

Longer Term Divorce and Marital Quality Outcomes

Two papers from this study have assessed longer term outcomes: Stanley et al. (2010) assessed divorce for Site 1 at one year post intervention, whereas Stanley et al. (2014) assessed divorce and marital quality outcomes for both sites at two years post intervention. At Site 1, one year after intervention, 19 couples had divorced. The PREP for Strong Bonds group had a significantly lower divorce rate ($5/246 = 2.03\%$) than the control group ($14/226 = 6.20\%$).

One year later (thus, two years post intervention), there were 54 divorces at Site 1 (11.3% divorce rate) and 10 divorces at Site 2 (a 5.4% divorce rate); the difference between sites in overall divorce rates was significant. Because of the small number of divorces at Site 2, Stanley et al. (2014) elected to exclude this site from analyses examining divorce as an outcome. That is, the number of divorces at Site 2 was too small to identify stable, meaningful effects, and too small to test for moderation by site (see p. 488 in Stanley et al., 2014). Using multilevel or hierarchical modeling (HLM) and HLM 7.0 (Raudenbush, Bryk, Fai, Congdon, & du Toit, 2011), which allowed couples to be nested within cohort, and the Bernoulli routine for dichotomous outcomes, we found that, at Site 1, those assigned to intervention had lower odds of divorce than those assigned to the control group ($p < .01$). Specifically, 8.1% (20/248) of the intervention group divorced compared to 14.9% (34/228) of the control group.

Thus, at Site 1, there were significant divorce reduction effects for couples assigned to the intervention group compared to those assigned to the control group, both at one year and two years post intervention. Stanley et al. (2014) noted that the overall findings supported the divorce reduction impact for the intervention group when collapsing across both sites, but it was clear that this finding was entirely driven by Site 1. Although we noted earlier that there were not any differences at pre by site in terms of marital quality, the differences in demographics and military duties between the sites are consistent with Site 2 being comprised of couples at relatively lower risk. The fact that the divorce rate at Site 2 is less than half of that at Site 1 two years post intervention supports this possibility. As noted earlier, Site 2 couples were more established and service members there were not involved to the same degree as those at Site 1 in major combat operations. Hence, the finding of significant impact on divorce rates for Site 1 but not Site 2 is consistent with other findings

in the field showing that groups of married couples at higher risk tend to benefit the most from such interventions (e.g., Halford et al., 2001; Halford & Bodenmann, 2013; Petch et al., 2012).

Stanley et al. (2014) also analyzed changes in marital quality over time following the intervention, using a four-level HLM model. Time-varying characteristics of individuals (e.g., marital satisfaction) were modeled at level 1, time-invariant individual characteristics (e.g., preintervention marital quality) were modeled at level 2, couple characteristics associated with risk (e.g., economic strain) were model at level, 3 and cohort was modeled at level 4. Pre-intervention marital quality scores were controlled for by including them at level 2. Four measures of marital quality were examined in separate models: communication skills, marital satisfaction, positive bonding, and divorce proneness. No differences were found for these aspects of marital quality at the 2 year follow up for intervention couples as compared to control couples. That, is intervention effects were not apparent for marital quality two years after the intervention.

Thus, across these three papers, in terms of basic outcomes, there were modest intervention effects for some (but not all) pre to post marital quality outcomes; but by two years post intervention these effects are no longer apparent. However, there is a significant impact of PREP for Strong Bonds on divorce at one and two years post intervention. Why would the intervention appear to reduce divorce but not improve or sustain marital quality compared to control couples during the same follow-up period? Methodologically, prevention researchers in this field have discussed the possibility that strong stability impacts could inhibit the ability of evaluators to detect relationship quality differences because of differential loss to follow-up for relationship quality outcomes; i.e., more couples with lower relationship quality may leave the control group over time, biasing the comparison of that group with the intervention group (e.g., Halford et al., 2001; Markman et al., 1988; Stanley, 2001). In fact, this issue has been discussed in detail with regard to findings in this field by one of the major, independent evaluation teams (Mathematica) that has been involved in similar evaluation efforts for the federal government (McConnell, Stuart, & Devaney, 2008). While there are ways to try to counter the potential bias caused by strong stability impacts, the options are ultimately limited by the fact that there is no entirely compelling method for estimating marital quality for couples who are divorced. Nevertheless, we attempted to control for this potential differential selection with additional analyses which tested divorce at two years out as a moderator of relationship quality outcomes, and found no such moderation. Whereas we are following this sample to assess longer-term outcomes (i.e., beyond two years), the evidence so far does not suggest that lower rates of divorce in the intervention group are the result of less happy couples being more likely to remain together post intervention.

Given the external stressors such as deployment that couples in the military may experience, small initial impacts (such as those found pre to post) during times of higher stress may function as protection, and may help some couples get through challenging times when they may have otherwise not remained together. Further, some have shown evidence of larger effects over time in comparison to small, initial effects from family focused interventions (e.g., Cowan, Cowan, & Barry, 2011; Halford & Bodenmann, 2013; Schulz, Cowan, &

Cowan, 2006). Nevertheless, there is not much evidence in this sample of relationship quality impacts that can explain the divorce reduction effect, nor do we have evidence of impacts on relationship quality being larger over time, raising the possibility of there being other non-specific or unmeasured effects (see Hawkins et al., 2012). As typical in other fields, we believe that it is important to try to identify mechanisms of effect (e.g., Wadsworth & Markman, 2012), but it is challenging to demonstrate such mechanisms (e.g., Schilling, Baucom, Burnett, Allen, & Ragland, 2003; Stanley, Rhoades, Olmos-Gallo, & Markman, 2007).

Moderators of Effects

Whereas mechanisms of impact are very difficult to examine, particularly in preventative interventions with little baseline distress, we have examined a number of moderators of treatment effect. Identification of moderators of impact is particularly useful for identifying types of couples who might benefit most from the intervention (Wadsworth & Markman, 2012). In general, prior studies of couples education programs have found that couples with higher risk (e.g., parental divorce, parental aggression) show greater improvements following the intervention relative to couples without such risk factors (see review by Halford & Bodenmann, 2013). Our evaluation of moderators similarly focuses on variables that may be markers of risk. We also consider each of our moderation analyses to address a particular question in the field.

Minority status and economic strain as moderators of divorce effects—One primary question that has been raised in the literature is whether couples education programs, mostly developed with non-minority, middle class samples, are appropriate for lower income and ethnic minority couples (Johnson, 2012). Thus, Stanley et al. (2014) evaluated whether the two year outcomes were moderated by income, a sense of economic strain, and minority status. None of these factors moderated the marital quality outcomes; however, the divorce outcomes for Site 1 were moderated by minority status and sense of economic strain (but not by income). Couples in the intervention group in which one or both partners were racial or ethnic minorities experienced even larger reductions in the likelihood of divorce than non-minority (White) couples. Specifically, for minority couples, the divorce rate in the control group was 15.7% but only 3.8% in the intervention group (more than a four-fold difference). These rates compare to a 13.3% divorce rate in the control group and a 10.6% divorce rate in the intervention group for non-minority couples. Moreover, there was a trend for couples in the intervention group who reported higher levels of stress about their financial situation to have a larger divorce reduction effect. In subsequent analyses (not reported in Stanley et al., 2014 but available from the author), there was also a three way interaction of minority status, economic strain, and intervention. A series of analyses to probe the interaction suggested that minority couples were less likely to divorce if assigned to intervention (vs. control) regardless of economic strain, and that couples in which both partners were White were less likely to divorce if assigned to intervention if they had high levels of economic strain. Non-minority couples with lower economic strain did not show divorce reduction effects from being assigned to the intervention.

Overall, this pattern suggests that minority couples or White couples who experience economic strain benefit more from the intervention, rather than less. Couples who were neither minority nor experiencing economic strain did not show divorce reduction compared to couples who endorse at least one (or both) of these factors. Actual income did not moderate findings. As all participants were Army couples with baseline supports such as income, housing, and medical benefits, the present sample is not considered to be in poverty, and thus there may be floor effects for income. Nevertheless, our sample of Army couples is, on average, at the lower end of the income scale compared to most couples involved in relationship education studies.

History of infidelity as moderator of marital quality outcomes—Allen et al.

(2012) examined if PREP for Strong Bonds would be ineffective or even contraindicated for couples with a history of infidelity. Couples therapists have also identified infidelity as one of the most difficult issues to treat in marital therapy (Whisman, Dixon, & Johnson, 1997), and therapies designed to treat infidelity are geared to help couples through intense, even traumatic, reactions many experience related to infidelity (Gordon, Baucom, & Snyder, 2004). Because marriage education “does not provide intensive, one-on-one work between participants and professionals on specific personal problems, as therapy does” (Hawkins, Blanchard, Baldwin, & Fawcett, 2008, p. 723), it was possible that PREP for Strong Bonds would not show positive effects for couples with a history of infidelity.

Across both sites, 23.4% of couples reported a history of infidelity, a rate commensurate with general rates of infidelity in the U.S. (Allen et al., 2005). These couples were significantly more maritally distressed at the baseline (pre-intervention) assessment than couples who did not report a history of infidelity. Differential intervention effects were tested for couples with and without infidelity out to one year post intervention. A four level HLM model analyzing changes in marital satisfaction, accounting for time, gender, and cohort, revealed a significant time by infidelity by group (intervention vs. control) interaction effect, suggesting that couples with a history of infidelity benefitted more from the intervention than couples without such history. Specifically, in the group assigned to PREP for Strong Bonds, couples with infidelity showed greater gains on marital satisfaction relative to couples without infidelity from pre to post ($d = .40$) and from pre to one year follow up ($d = .38$). In contrast, within the control group, infidelity couples did not significantly improve more than couples without infidelity: Although there was a trend from pre to post for control group infidelity couples to improve more than control group non-infidelity couples ($d = .16$), this effect was non-existent one year after intervention ($d = .04$).

Whereas infidelity couples assigned to PREP for Strong Bonds experienced greater gains, they do not “catch up” to couples without infidelity assigned to PREP for Strong Bonds with regard to marital satisfaction. Specifically, at the baseline assessment, intervention couples with infidelity were significantly less satisfied relative to intervention couples without infidelity ($d = .73$). Although infidelity couples assigned to intervention were still more distressed than non-infidelity couples assigned to intervention at post ($d = .34$) and one year follow up ($d = .29$), their relatively greater gains mean that the difference between infidelity couples and non-infidelity couples’ satisfaction ratings narrowed by just over half after intervention.

A similar pattern of findings was shown for changes in communication skills, although the initial time by infidelity by group interaction effect was just a trend. Overall, infidelity couples assigned to intervention showed relatively more improvement in communication skills than couples without a history of infidelity assigned to intervention or infidelity couples assigned to control. Intervention couples with infidelity “caught up” to intervention couples without infidelity, showing an absence of significantly different levels of communication skills after intervention.

Thus, the results suggest that couples with a history of infidelity, on average, benefit from the intervention, and there is no need to screen out such couples. However, we did not expressly recruit for a distressed sample or a sample that identified infidelity as a current problem in the marriage; in fact, as noted earlier, the sample on average was generally satisfied with their marriage at the baseline assessment. Thus, it may be that couples who report that infidelity is currently a distressing issue or who have high levels of volatility related to this issue (see Gordon et al. (2004) stages of reaction to infidelity) need more intensive, one on one therapeutic support. Perhaps PREP for Strong Bonds could serve as a gateway to additional support, but we have not analyzed whether participation in PREP for Strong Bonds was associated with seeking additional marital therapy for couples with infidelity or other correlates of distress.

History of pre-commitment cohabitation as moderator of marital quality and divorce outcomes—Another risk factor for divorce and marital distress is premarital cohabitation (for meta-analyses, see Jose, O’Leary, & Moyer, 2010), particularly when cohabitation occurs before a commitment to marriage (Kline et al., 2004; Manning & Cohen, 2012; Rhoades, Stanley, & Markman, 2009; Stanley, Rhoades, Amato, Markman, & Johnson, 2010). Inertia theory (Stanley, Rhoades, & Markman, 2006) suggests that cohabitation makes it harder to end a relationship and therefore increases the likelihood that a poorer quality and less committed relationship may progress into marriage.

Rhoades, Stanley, Markman & Allen (2014) examined the degree to which PREP for Strong Bonds would mitigate the risk for divorce and marital distress for our sample of Army couples who reported pre-commitment cohabitation. Over half (58.6%) of the sample reported pre-commitment cohabitation; specifically, that they lived together before marriage or had not made a specific commitment to marry before living together. As predicted, couples with pre-commitment cohabitation had significantly lower marital satisfaction and dedication at the baseline (pre-intervention) assessment. However, contrary to our expectation, couples with pre-commitment cohabitation did not have significantly worse communication than couples without pre-commitment cohabitation. As in the tests of infidelity history as a moderator, four level HLM models, controlling for pre-intervention levels of marital quality, were used to examine the possible differential effects of the intervention over the next two years for couples with and without pre-commitment cohabitation.

Findings suggested that PREP for Strong Bonds could mitigate problems associated with pre-commitment cohabitation. For those in the control group, pre-commitment cohabitation was associated with lower estimated average dedication across the two years after

intervention ($p < .01$). In contrast, there was no difference in dedication based on cohabitation history for those in the PREP group ($p = .80$). Additional analyses in the control group showed a trend for dedication declining more for those with pre-commitment cohabitation compared to those without pre-commitment cohabitation ($p = .06$), but there was no such difference for those in the PREP group ($p = .46$).

Marital satisfaction showed basically the same pattern of effects. Again, for those in the control group, pre-commitment cohabitation was associated with lower estimated average marital satisfaction in the post-intervention assessment ($p < .05$), but there was no such difference for those in the PREP group ($p = .67$). Further tests showed that, for those in the control group, marital satisfaction declined more across the two years following the post-intervention assessment for those with pre-commitment cohabitation compared to those without pre-commitment cohabitation ($p < .01$), but there was no such difference for those in the PREP group ($p = .72$).

These differential effects were also found for divorce two years post intervention. In the control group, significantly more (14.6%) of those who cohabited before a mutual commitment to marry filed for divorce compared to 7.5% of those who did not have pre-commitment cohabitation. In contrast, for the PREP group, there was not a significant difference in divorce by pre-commitment cohabitation history, with 7.4% of those with pre-commitment cohabitation having filed for divorce compared to 7.8% of those without pre-commitment cohabitation.

Thus, overall, it appeared that being assigned to PREP for Strong Bonds mitigated the risks of lower satisfaction and lower dedication associated with pre-commitment cohabitation, as well as reduced the risk of divorce for those with pre-commitment cohabitation.

Summary of moderation findings—The moderators reviewed above (minority status, economic strain, history of infidelity, pre-commitment cohabitation) would conventionally be considered markers of risk for poor marital outcomes. This assumption is usually empirically borne out in our data, such as the fact that a history of infidelity or pre-commitment cohabitation was associated with poorer relationship functioning. However, minority status of the couple did not predict greater average divorce risk; thus, minority status may not be a risk factor in our sample in terms of divorce. There is evidence that risks for divorce are substantially lower for minority couples within the military compared to those not in the military (Teachman & Tedrow, 2008), likely because of there being less discrimination and greater economic and social supports.

Overall, we generally find that couples with a risk factor show greater gains from or protective effects from the intervention. Further, the fact that there is evidence of a substantial divorce reduction effect only at the larger site, where couples were younger, lower ranking, and on a higher war footing, is also consistent with this general interpretation of greater benefit for married couples at higher risk. Thus, although some have wondered if programs like PREP for Strong Bonds would be ineffectual or contraindicated for certain couples (e.g., minority couples; Johnson, 2012), our evidence indicates the opposite.

Discussion

Taken as a whole, we show significant overall divorce effects at Site 1 (the site with adequate power for divorce analyses) at one and two years out, where couples assigned to intervention show significantly lower rates of divorce compared to couples assigned to control. At the same time, we have found little evidence for overall intervention effects on marital quality across the first two years following the intervention. However, when separately evaluating couples with risk factors of infidelity or pre-commitment cohabitation, we see significantly greater positive outcomes on aspects of marital quality out to one year (as evaluated for infidelity) or two years (as evaluated for pre-commitment cohabitation). Moreover, the divorce reduction effect is particularly clear for minority couples, couples with economic strain, and/or couples with pre-commitment cohabitation. Thus, general conclusions about the effectiveness of the intervention depend somewhat upon the outcome measured (specific aspects of marital quality, divorce) and/or upon specific characteristics of the couples (e.g., cohabitation history).

These findings regarding moderators raise the question of whether interventions like PREP for Strong Bonds should be offered universally or only target certain “at risk” populations. The version of PREP for Strong Bonds evaluated here is resource intensive in some ways (e.g., a weekend retreat) but the delivery of the intervention is efficient, compared to conjoint therapy, in that it is delivered to groups of couples at one time by a trained leader. Selective prevention (i.e., pushing the intervention only toward those identified as “high risk”) within the military could have a number of pitfalls, such as: many couples would not self-identify as being high in “risk” and would not pursue the intervention, screening processes with subsequent assignment (or not) to intervention could be intrusive and stigmatizing, all important relevant risk factors have yet to be identified, and there would be many false positives and false negatives in regards to who is deemed to need the intervention. Consider the implications of findings in this project, and other recent evaluations in non-military samples, where there is also evidence that minority couples may benefit most from the services (see discussion in Stanley et al., 2014). It seems obviously unacceptable to plan for a program that invites only couples where one or both partners are members of a minority group to attend. Furthermore, there are other planned benefits of providing such services universally within the chaplain corps, including the building of supportive relationships between chaplains and military families. Halford and Bodenmann (2013) and Markman (2014) review this issue for the field of couple education in general and discuss a range of options including adaptive content and stepped approaches. Of note, there are less resource intense versions of PREP, such as ePREP, which is now delivered online (see <http://www.lovetakeslearning.com>) and has demonstrated efficacy (e.g., Braithwaite & Fincham, 2014), but these approaches have not been examined in a military or higher risk population. Further, such approaches do not offer the benefits to a service unit like the chaplain corps.

As noted, the results reviewed in the current paper have been found using the most conservative and rigorous model of analysis (e.g., intent to treat) for testing the intervention that has been disseminated in a naturalistic setting. Some impacts are particularly clear and large (reduction in divorce) and some are small and in evidence only for shorter-term

outcomes. As we discussed earlier, it is entirely possible that the former impacts affect the ability to detect or demonstrate the latter. In addition, it seems to us that divorce impact findings are rarer in this field and more noteworthy (see Stanley et al, 2014), and are based on an outcome less subject to various biases than self-reported relationship quality. Self-report measures also often show smaller effects of relationship education relative to observational assessments (Blanchard et al., 2009; Fawcett et al., 2010). Thus, there may be some methodological factors which have limited the measured intervention impacts on relationship quality.

Whereas the divorce impacts are important, the lack of long-term findings for overall changes in marital quality could also suggest that PREP for Strong Bonds, and similar interventions, could show improved effects with additional intervention components such as booster trainings or brief booster emails or phone calls. For example, in tests of ePREP, Braithwaite and Fincham (2009; 2011; 2014) sent participants weekly emails (for six to seven weeks) reminding participants about program content and the importance of using what they had learned. In terms of military couples, it may be that additional content focusing particularly on the issues frequently faced by military couples (e.g., frequent moves, trauma) would be a useful enhancement, informed by interventions such as FOCUS and Battlemind (Adler, Bliese, McGurk, Hoge, & Castro, 2011). It could also be that additional critical moderators, such as timing of PREP for Strong Bonds delivery (e.g., pre deployment vs. post deployment), will be identified and inform ways to maximize the effect of the intervention. Given that the goals of preventive interventions are by definition long-term, we will also continue to evaluate outcomes over a greater time frame; our current data collection represents seven years (Site 1) and six years (Site 2) post intervention. Moreover, we will continue to evaluate other critical outcomes, such as effects on mental health, and parenting and child outcomes. Thus, future analyses from this ongoing research trial will investigate further areas of impact and important moderators and predictors of such impact.

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Figure 1.
Consolidated Standards of Reporting Trials

Table 1

PREP for Strong Bonds Randomized Clinical Trial Outcome Studies

	Sites Evaluated	Time Points Evaluated	Response Rate (n = # of couples; N = # of individuals)	Outcomes Evaluated	Moderators of Outcome Evaluated
Allen, Stanley, Rhoades, Markman & Loew (2011)	Site 1 and Site 2	Pre, Post	97% (n = 644)	Marital Satisfaction, Communication Skills, Confidence, Positive Bonding, Forgiveness, Dedication, Satisfaction with Sacrifice, Negative Communication	Results evaluated separately for Site 1 and Site 2. Gender included as moderator.
Stanley, Allen, Markman, Rhoades & Prentice (2010)	Site 1	One year follow up	99% (n = 472)	Divorce	Deployment after intervention
Stanley, Rhoades, Loew, Allen, Carter, Osborne, Prentice, & Markman (2014)	Site 1 and Site 2	Two year follow up	Marital Quality 99% (N = 1312) Marital Status ~100% (n = 660)	Divorce, Communication Skills, Marital Satisfaction, Positive Bonding, Divorce Proneness	Ethnic minority status, economic strain, income
Allen, Stanley, Rhoades, Loew, & Markman (2012)	Site 1 and Site 2	Pre, Post, One year follow up	Post 97% (n = 640) One year follow up 84% (n = 557)	Marital Satisfaction, Communication Skills	History of Infidelity
Rhoades, Stanley, Allen & Markman (2014)	Site 1 and Site 2	Pre, post, four follow ups (six months, one year, 18 months, two years)	Marital Quality 99% (N = 1312) Marital Status ~100% (n = 660)	Marital Satisfaction, Dedication, Negative Communication, Divorce	Premarital cohabitation