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Review

Meta-analytic review of dissonance-based eating disorder prevention programs: Intervention, participant, and facilitator features that predict larger effects

Eric Stice*, C. Nathan Marti, Heather Shaw, Paul Rohde

Oregon Research Institute, United States

HIGHLIGHTS

- Conducted meta-analytic review of dissonance-based eating disorder prevention programs.
- We identified 56 trials evaluating 68 prevention programs (7808 participants).
- Average effect sizes (*d*) relative to minimal and alternative control were .39 and .20.
- Effects were larger given more activities/sessions, higher risk status, leader training/supervision.
- Future directions for dissonance-based prevention programs are provided.

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ABSTRACT

Many trials have provided support for dissonance-based eating disorder prevention programs. This meta-analytic review characterized the average intervention effects and tested whether various intervention, participant, and facilitator features correlated with larger effects to guide implementation of optimally effective versions of this program. We identified 56 trials that evaluated 68 dissonance-based eating disorder prevention programs (7808 participants). Average intervention effect sizes (*d*) relative to minimal intervention control conditions and credible alternative interventions (respectively) were 0.57 and 0.31 for thin-ideal internalization, 0.42 and 0.18 for body dissatisfaction, 0.37 and 0.17 for dieting, 0.29 and 0.21 for negative affect, and 0.31 and 0.13 for eating disorder symptoms. As hypothesized, effects were larger for interventions with more dissonance-inducing activities, more group sessions, and larger group sizes, as well as when delivered in-person versus on-line, sessions were recorded, participation was voluntary, body dissatisfaction was required, participants were mid-adolescents or adults (versus older adolescence), there were more ethnic minority participants, groups were led by clinicians versus researchers and at least two facilitators, and when facilitators received more training and supervision. Unexpectedly from a dissonance-induction perspective, effects were larger when participants were compensated. Results offer directions for maximizing the benefits of implementation efforts with dissonance-based eating disorder prevention programs, and may hold lessons for preventing other public health problems with dissonance-based interventions.

Representative data from the USA and Australia suggest that 13% of females experience a threshold or subthreshold DSM-IV eating disorder by young adulthood, which are characterized by chronicity, distress, functional impairment, and risk for future obesity, depression, suicide, and mortality (Arcelus, Mitchell, Wales, & Nielsen, 2011; Stice, Marti, & Rohde, 2013). Indeed, eating disorders show stronger relations to mortality, suicide attempts, and functional impairment than other psychiatric disorders, including schizophrenia, major depression, and bipolar disorder (Crow & Smiley, 2010) and are the second leading cause of mental health disability for adolescent girls and young women (Begg et al., 2007). Yet 80% of individuals with eating disorders do not receive treatment (Swanson, Crow, le Grange, Swendsen, & Merikangas, 2011) and extant treatments only result in lasting remission from binge eating and compensatory behaviors for 47% of patients on average (Hay, 2013). Thus, a public health priority is to develop and broadly implement effective eating disorder prevention programs. Prevention

* Corresponding author at: Oregon Research Institute, 1776 Millrace Drive, Eugene, OR 97403, United States.
E-mail address: estice@ori.org (E. Stice).

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programs may be particularly effective for eating disorders because the peak risk period for onset occurs around the ages of 16 to 19 (Hudson, Hiripi, Pope, & Kessler, 2007; Stice, Marti, & Rohde, 2013), suggesting that if efficacious prevention programs were broadly implemented during adolescence, they could reduce the population prevalence of eating disorders.

1. Empirical support for dissonance-based eating disorder prevention programs

The fact that several malleable sociocultural, attitudinal, and behavioral risk factors for eating disorders have been identified suggests that it should be viable to prevent these disorders (e.g., pursuit of the thin beauty ideal, body dissatisfaction, overeating; Stice, Gau, Rohde, & Shaw, 2017). Dozens of eating disorder prevention programs have been developed and evaluated, but only three have reduced eating disorder symptom composite measures (Atkinson & Wade, 2016; Stice, Marti, Spoor, Presnell, & Shaw, 2008; Stice, Rohde, Shaw, & Marti, 2013) and only three have reduced future eating disorder onset (Martinsen et al., 2014; Stice, Marti, Spoor, et al., 2008, Stice, Rohde, Shaw, & Gau, 2017a,Stice, Rohde, Shaw, & Gau, 2017b; Stice, Rohde, Shaw, & Marti, 2013). The most empirical support has emerged for dissonance-based eating disorder prevention programs (commonly referred to as the *Body Project*), which has included efficacy trials, effectiveness trials, comparative trials, and tests of the intervention theory conducted by multiple research teams (reviewed subsequently). In the *Body Project* young women voluntarily critique the thin beauty ideal in verbal, written, and behavioral exercises, which theoretically generates dissonance that prompts reduced pursuit of this unrealistic ideal because people align their attitudes with their publically displayed behaviors (Stice, Mazotti, Weibel, & Agras, 2000). Festinger (1957) proposed people are motivated to maintain consistency between their behaviors and attitudes, and that when an individual engages in a behavior that is inconsistent with an attitude, they experience psychological discomfort that causes them to align their attitudes with their behavior. For instance, in the *Body Project* participants verbally generate costs associated with pursuing the thin ideal in response to Socratic questions, complete role-plays in which they talk facilitators out of pursuing this ideal, write a letter to a younger self on how to avoid body image concerns, and engage in acts of body activism that challenge this ideal (intervention script provided at: www.bodyprojectsupport.org).

According to the dual pathway model (Stice, Chase, Stormer, & Appel, 2001), reduced subscription to the thin ideal should decrease body dissatisfaction, unhealthy weight control behaviors, negative affect, eating disorder symptoms, and risk for future eating disorder onset. In support, elevated pursuit of the thin ideal, body dissatisfaction, dieting, and negative affect predict futures onset of any eating disorder (Ghaderi & Scott, 2001; Jacobi et al., 2011; McKnight, 2003; Rohde, Stice, & Marti, 2015; Santonastaso, Friederici, & Favaro, 1999). Research has also provided support for the thesis that pursuit of the thin ideal typically emerges before body dissatisfaction, which typically emerges before dieting and negative affect, which typically emerges before eating disorders, for those who eventually show onset of an eating disorder (Stice & Van Ryzin, 2019).

Efficacy trials have found that the *Body Project* produced greater reductions in eating disorder risk factors (thin-ideal internalization, body dissatisfaction, dieting, negative affect), eating disorder symptoms, and functional impairment in adolescent girls and young women with body image concerns relative to assessment-only control conditions, and often relative to alternative interventions, with several effects persisting through 3-year follow-up (Stice et al., 2000; Stice et al., 2001; Stice, Marti, Spoor, et al., 2008; Stice, Shaw, Burton, & Wade, 2006; Stice, Trost, & Chase, 2003). It is important to note that although the *Body Project* produced significantly greater reductions in outcomes than the *Healthy Weight* eating disorder prevention program from pretest to posttest, the latter program produced greater reductions in eating disorder risk factors and symptoms than control conditions over longer-term follow-up (Stice, Marti, Spoor, et al., 2008). That trial also found that future onset of threshold/subthreshold eating disorders over 3-year follow-up was 6% in *Body Project* participants and 6.5% in *Healthy Weight* participants, versus 15% in the assessment-only condition, representing a statistically significant 60% reduction in future ED onset for both programs. Efficacy trials conducted by independent teams also found that the *Body Project* produced greater reductions in risk factors and eating disorder symptoms versus assessment-only control conditions (Halliwell & Diedrichs, 2014; Matusek, Wendt, & Wiseman, 2004; Mitchell, Mazzeo, Rausch, & Cooke, 2007) and a media advocacy prevention program (Becker, Smith, & Ciao, 2005), though not all of these trials targeted young women with body image concerns. Further, the *Body Project* reduced eating disorder symptoms and electrocardiogram (ECG)-assessed cardiac risk indices among women with subclinical and clinical eating disorders (Green et al., 2016), which is important because cardiac problems are a leading cause of mortality among such individuals. Thus, the *Body Project* has shown efficacy when implemented universally to young women who were not screened for body image concerns, when implemented selectively to young women with body image concerns, and when implemented in an indicated fashion to young women with subclinical eating pathology.

Effectiveness trials have confirmed that the *Body Project* produces similar effects, relative to educational brochure and educational video control conditions, when high school and college counselors deliver the intervention under ecologically valid conditions, with several effects persisting through 3-year follow-up (Stice, Butryn, Rohde, Shaw, & Marti, 2013; Stice, Durant, Rohde, & Shaw, 2014; Stice, Stice, Rohde, Butryn, Shaw, & Marti, 2015; Stice, Rohde, Durant, & Shaw, 2012; Stice, Rohde, Gau, & Shaw, 2009; Stice, Rohde, Shaw, & Gau, 2011). However, this program did not reduce future eating disorder onset over 3-year follow-up in these effectiveness trials, potentially because the facilitators implemented the *Body Project* only a few times compared to facilitators in efficacy trials. Effectiveness trials also found that when undergraduate peer educators deliver the *Body Project* to other college students it produces larger reductions in risk factors and eating disorder symptoms than educational brochure and educational video controls (Stice, Rohde, Durant, Shaw, & Wade, 2013; Stice, Rohde, et al., 2017a,Stice, Rohde, et al., 2017b), the *Healthy Weight* prevention program (Becker et al., 2010), and the Internet-delivered *eBody Project* prevention program (Stice, Rohde, et al., 2017a,Stice, Rohde, et al., 2017b). However, other effectiveness trials did not find that it significantly outperformed the *Healthy Weight* prevention program or a media advocacy prevention program (Becker, Bull, Schaumberg, Cauble, & Franco, 2008; Becker, McDaniel, Bull, Powell, & McIntyre, 2012; Becker, Smith, & Ciao, 2006). Undergraduate-led *Body Project* groups also produced greater reductions in risk factors in high school students than assessment-only controls (Halliwell, Jarman, McNamara, Risdon, & Jankowski, 2015). An effectiveness trial that provided more intensive training and supervision found that young women assigned to peer-led *Body Project* groups showed a significant 74% reduction in eating disorder onset over 7-month follow-up relative to young women assigned to the *eBody Project* and a (nonsignificant) 64% reduction in eating disorder onset relative to an educational video control condition (2.2% versus 8.4% and 6.1% respectively; Stice, Rohde, et al., 2017a,Stice, Rohde, et al., 2017b). By 3-year follow-up, participants assigned to peer-led *Body Project* groups showed a statistically significant 56% reduction in eating disorder onset than participants assigned to the educational video control condition and a statistically significant 53% reduction in eating disorder onset than participants assigned to clinician-led *Body Project* groups (7% versus 16% and 15% respectively). The Internet-delivered *eBody Project* has likewise produced significantly greater reductions in risk factors and eating disorder symptoms than educational brochure and educational video controls, with some effects persisting through the final 2-year follow-up (Stice, Durant, et al., 2014; Stice, Rohde, Durant, & Shaw, 2012). Thus, at least

some effects persisted through 3-year follow-up in each of the four trials that collected data over that duration, and at least some effects persisted through 2-year follow-up in one trial that collected data over that duration.

In support of the intervention theory, reductions in thin-ideal internalization mediated the effects of the *Body Project* on reduction in eating disorder symptoms (Seidel, Presnell, & Rosenfield, 2009; Stice, Presnell, Gau, & Shaw, 2007). Consistent with the notion that dissonance induction contributes to intervention effects, completing versions of this intervention designed to maximize dissonance versus versions designed to minimize dissonance, but with the same general intervention content, produced greater symptom reductions (Green, Scott, Diyankova, Gasser, & Pederson, 2005; McMillan, Stice, & Rohde, 2011). Although those trials imply that both session content and non-specific factors contribute to intervention effects, participants who completed the Internet delivered *eBody Project,* which involves no interactions with facilitators or other participants, showed greater reductions in eating disorder risk factors and symptoms than educational video controls, suggesting that effects are not solely due to non-specific factors (Stice, Durant, et al., 2014; Stice, Rohde, et al., 2017a, 2017b; Stice, Rohde, Durant, & Shaw, 2012). And as noted, the *Body Project* has produced stronger reductions in outcomes than other group-based prevention programs, implying that effects are not solely due to non-specific factors inherent to group-based interventions. Further, the *Body Project* produced larger effects for participants with initial elevations in thin-ideal internalization, consistent with the thesis that they should experience the greatest dissonance (Stice, Marti, Shaw, & O'Neil, 2008). Completing the *Body Project* also offset the risk conveyed by the most potent eating disorder risk factor in one trial - denial of the costs of pursuing the thin ideal (Stice, Rohde, Gau, & Shaw, 2012): participants with this risk factor who completed the *Body Project* showed an eating disorder incidence of 0% over 3-year follow-up, versus 18% for those who completed two alternative interventions and 50% for assessment-only controls. Completing the *Body Project* also eliminated the adverse effects of exposure to thin models on preadolescent girls (Halliwell & Diedrichs, 2014). Further, participants who completed the *Body Project* showed a larger pre-to-post reduction in fMRI-assessed reward region (caudate) response to thin models compared to educational brochure controls, providing objective evidence that participants may not perceive the thin ideal as a desirable goal after completing the *Body Project* (Stice, Yokum, & Waters, 2015). The evidence that this intervention affects objective biological outcomes (neural response to thin models and cardiac functioning) is reassuring because these outcomes are immune to the demand characteristics inherent in randomized trials that can contribute to greater reductions in self-reported outcomes for participants in intervention versus control conditions.

The fact that the *Body Project* has produced effects that replicate across independent trials is noteworthy because many published findings do not replicate (Ioannidis, Munafò, Fusar-Poli, Nosek, & David, 2014). It is the only eating disorder prevention program that has produced effects that have been independently replicated. A recent meta-analytic review found that dissonance programs produced the largest reductions in eating disorder symptoms versus any other type of selective or indicated prevention program, with an average $d = 1.06$ when compared against assessment-only control conditions and an average $d = 0.30$ when compared against non-specific control conditions (Watson et al., 2016).

2. Meta-analytic review of dissonance-based eating disorder prevention program trials

A benefit of meta-analytic reviews is that they involve effect sizes based on observed changes across groups over time and report average effect sizes. The focus on a standardized effect size metric, such as change in standard deviation (*SD*) units (*d*), facilitates comparisons across studies that used different measures. Cohen (1988) defined $d = 0.20$, 0.50, and 0.80 as small, medium, and large effects, respectively. In contrast, narrative reviews focus on statistical significance, which is potentially problematic because sample size plays a major role in determining whether an effect is significant. In trials with small samples, larger effects may not be statistically significant, whereas in trials with very large samples, even trivial effects can be statistically significant. Thus, the first aim of this report is to characterize the average intervention effects of the *Body Project* on four primary eating disorder risk factors (thin-ideal internalization, body dissatisfaction, dieting, and negative affect) and on eating disorder symptoms.

A second benefit of meta-analytic reviews is that they permit an examination of moderators that correlate with effect size magnitude. This is key because there has been variation in effects, with some trials indicating that dissonance prevention programs produce large effects (e.g., Becker et al., 2010; Brown & Keel, 2015; Stice et al., 2006; Stice, Rohde, Durant, et al., 2013) and others suggesting that they produce small effects (e.g., Smith & Petrie, 2008; Wade, George, & Atkinson, 2009; Wiseman, Sunday, Bortolotti, & Halmi, 2004; Wolfe, 1992). Because it appeared that the trials that generated large effects differed systematically from those that generated small effects in terms of intervention, participant, and facilitator features, we tested hypotheses regarding factors that might correlate with larger effects. Thus, the second aim is to test whether certain intervention, participant, and facilitator features correlate with larger effects on eating disorder risk factors and symptoms. The fact that trials have evaluated various dissonance eating disorder prevention programs affords a unique opportunity to investigate finer details regarding the effects of these features. Improved understanding of features associated with larger effects should inform implementation of optimally effective versions of this prevention program, which is timely because organizations are increasingly implementing dissonance eating disorder prevention programs. Given pressures to deliver versions of this prevention program that are as short or inexpensive as possible, it is crucial to determine whether delivering versions of the intervention with fewer sessions, fewer dissonance-induction activities, using abbreviated training, omitting supervision, or working with undergraduate peer educators to deliver the intervention is associated with smaller effects. An understanding of features that correlate with larger effects may also inform implementation of prevention programs for other mental health and health problems.

3. Intervention features that may moderate intervention effect sizes

We investigated several variables hypothesized to moderate intervention effects based on theory, prior findings, and clinical experience. We focused on the following intervention features: (1) number of dissonance-inducing activities, (2) number of group sessions, (3) average number of group participants, (4) whether the intervention was delivered on-line or in-person, and (5) whether sessions were video-recorded. We hypothesized effects would be larger in trials in which participants engaged in more dissonance-inducing activities during or between sessions, as this should maximize dissonance-induction. We coded whether interventions included 12 dissonance-induction activities: (1) verbally agreeing to voluntarily participate in group discussions, (2) critiquing the thin ideal in a group discussion, (3) arguing facilitators out of pursuing the thin ideal in role-plays, (4) writing a letter to a younger girl about costs of the thin-ideal, (5) recording positive personal physical features while looking in a mirror, (6) writing a "rewind response" letter to someone in the past who led you to pursue the thin ideal; (7) engaging in body activism (e.g., like placing a love-your-body poster in the women's bathroom), (8) generating verbal "quick comebacks" to thin-ideal statements (9) engaging in a behavioral challenge to refute body image concerns, and (10) writing a letter to one's younger self about why she shouldn't pursue the thin-ideal; (11) discussing the benefits of the group; (12) committing to a self-

affirmation exercise (e.g., like keeping a journal of all of the positive things your body does). Theoretically, each of these activities generates dissonance regarding pursuit of the thin ideal, which should contribute to larger effects. Establishing a dose-response relation is a critical test of the intervention theory for a prevention program.

We hypothesized that interventions delivered over more sessions would produce larger effects because completing more home exercises should produce greater dissonance, based on the evidence that when participants put more effort into a goal, they show greater attitudinal shift towards valuing that goal (Cooper & Axsom, 1982). In addition, having time to reflect on the intervention content between sessions may aid in the consolidation of learning, which may contribute to larger effects. Further, it is also possible that meeting multiple times may contribute to a stronger development of group support, which *Body Project* participants state is one of the most valuable elements of this intervention (Shaw, Rohde, & Stice, 2016).

We hypothesized that intervention effects would be larger when the prevention program is delivered in larger groups. Theoretically, larger groups should maximize public accountability, which is key to dissonance-induction (Festinger & Carlsmith, 1959). Experiments have found that composing and giving a speech or writing an essay about a perspective produces increased endorsement of that perspective if participants think that the speech or essay will be heard or viewed by others, but produces little attitudinal change otherwise (Aronson, Fried, & Stone, 1991). The fact that there is typically no attitudinal change in these studies when participants believe that the speech or essay will not be heard or viewed by others illustrates that public accountability is critical for dissonance-induction. To our knowledge, no study has tested for a linear relation between group size and degree of dissonance-induced attitudinal change in dissonance-based group-delivered interventions.

In addition, we hypothesized intervention effects would be significantly larger if the dissonance eating disorder prevention programs was delivered in-person versus on-line. This is because in-person delivery should maximize public accountability, which has been shown to maximize dissonance induction and consequent attitudinal change (Festinger & Carlsmith, 1959).

Further, we hypothesized the intervention effects would be significantly larger when sessions were video-recorded. This is because recording sessions should increase perceived accountability, which should theoretically contribute to larger effects.

4. Participant features that may moderate intervention effect sizes

We focused on the following participant features: (1) whether participants were required to have body image concerns, (2) whether participants were mandated to complete the prevention program, (3) whether participants were compensated, (4) mean age of the sample, and (5) the percentage of the sample that was European-American. We tested the hypothesis that the effects of dissonance eating disorder prevention programs would be stronger for samples in which participants were required to have body image concerns versus samples without this requirement because meta-analytic reviews have found that effects were stronger for selective prevention programs that target samples at elevated risk for eating disorders versus universal prevention programs that target unselected populations (e.g., Stice, Shaw, & Marti, 2007). Body dissatisfaction is one of the most potent predictors of future escalation in symptoms and onset of eating disorders (e.g., Johnson & Wardle, 2005; Stice, Gau, et al., 2017). However, it is unclear whether this pattern of findings emerged because all prevention programs are more effective for populations at risk for eating disorders due to the fact that they are more motivated to engage in the intervention and have more room for improvement, or because the prevention programs that have been evaluated in selective trials are more effective than those evaluated in universal trials. As the present meta-analysis focused on a more homogenous set of prevention programs, it may better discern whether the risk status of participants is associated with larger effects, holding intervention content more constant. We hypothesize that dissonance-based eating disorder prevention programs will produce larger effects in trials that focused on participants with versus without body dissatisfaction because they have more motivation to engage in the intervention activities and more room for improvement in the outcomes.

We hypothesized that effects for the dissonance eating disorder prevention programs would be larger if participants volunteered to complete the intervention versus being mandated to do so. Many universities require that students complete interventions to prevent alcohol abuse and sexual victimization before they are permitted to register for classes, illustrating that some schools mandate participation in prevention programs. Further, some sororities have mandated that pledges complete dissonance-based eating disorder prevention programs (Becker & Stice, 2008). Research indicates that when participants *voluntarily* engage in a counter-attitudinal behavior (e.g., writing an essay that supports a perspective they do not initially endorse) they subsequently show attitudinal shift towards this new perspective, but that this does not occur if they are *required* to write the counter-attitudinal essay (Baumeister & Tice, 1984). Theoretically, engaging in a counter-attitudinal behavior prompts people to adjust their attitudes to conform to their behavior only if they feel that they engaged in that behavior voluntarily. It is important to note that in the studies in which individuals were mandated to complete a dissonance-based eating disorder prevention program the individuals were given the option of deciding not to provide data for the evaluation (i.e., participation in the research study was voluntary).

For similar theoretical reasons, we hypothesized that dissonance eating disorder prevention programs would produce weaker effects if individuals were compensated for participating in the study. Experiments indicate that when participants engage in a behavior that is inconsistent with their attitude, they only change that attitude if they have been paid a small amount of money rather than a large amount (Cohen, 1962), presumably because in the latter case they attribute their inconsistent behavior to the large payment. Participant compensation has ranged from a low of no financial or other type of compensation to payment of up to $165 USA dollars for completing seven assessments over a 4-year follow-up (i.e., participants were typically compensated for completing assessment, rather than the prevention program).

We hypothesized that intervention effects would be larger for trials with older versus younger participants because meta-analytic reviews have found that eating disorder prevention programs produced larger effects for older adolescents and young adults ~~versus younger adolescents~~ than for participants aged 9–14; Stice, Shaw, & Marti, 2007). Participants in trials of dissonance-based eating disorder prevention programs have ranged from 11 to 64, which includes trials that evaluated versions adapted for middle school girls (Halliwell & Diedrichs, 2014; Rohde et al., 2014). It is unclear whether this is because most eating disorder prevention programs are more effective if delivered immediately before or during the peak period of risk for eating disorder onset (ages 15–19; Stice, Rohde, Durant, et al., 2013) or because the interventions that have been evaluated with older participants are more effective than those evaluated with younger participants. Our meta-analysis should be able to differentiate between these two possibilities because the prevention programs included herein are more similar than those included in past meta-analytic reviews of eating disorder prevention trials. We hypothesized that intervention effects would be significantly smaller for early adolescents versus older adolescents and adults.

We investigated the effects of the ethnic composition of the sample because some data suggested that on average European-Americans subscribe more strongly to the thin beauty ideal than certain ethnic minority groups (Warren, Gleaves, Cepeda-Benito, Fernandez, & Rodriguez, 2005) and the basis of dissonance eating disorder

prevention programs is to critique this ideal. Thus, we tested the hypothesis that dissonance eating disorder prevention programs would produce larger effects in trials with higher percentages of European-Americans. However, a more recent study with a larger sample found that European-American young women did not report significantly higher endorsement of the thin beauty ideal than did Hispanic, Asian American, of African American young women (Cheng, Perko, Fuller-Marashi, Gau, & Stice, 2018).

5. Facilitator features that may moderate intervention effect sizes

We focused on the following facilitator features: (1) whether group facilitators were researchers, clinicians, or peer leaders, (2) the number of facilitators who led the groups, (3) the number of training hours provided to facilitators, and (4) whether facilitators received supervision. We hypothesized that intervention effects would be larger for groups facilitated by research clinicians (including the investigators and their graduate students) versus any type of clinician who naturally provide services to the population (e.g., to high school or college students) or undergraduate peer leaders based on a prior meta-analytic review (Stice, Shaw, & Marti, 2007). One trial found that effect sizes were larger for clinicians versus peer-leaders (Stice, Rohde, Durant, et al., 2013), though this did not occur after doubling the training time for peer-leaders versus clinicians (Stice, Rohde, et al., 2017a,Stice, Rohde, et al., 2017b).

We hypothesized that effects would be smaller for groups led by a single facilitator versus more facilitators. Theoretically, multiple facilitators are better positioned to manage the session activities and monitor whether participants are fully engaged in the discussions.

Finally, we hypothesized that effects would be larger when facilitators received more training and supervision. Greater initial training and supervision should maximize intervention fidelity and therapist competence, which should theoretically translate into larger effects. It should be noted that we had to focus on the amount of training and supervision, rather than intervention fidelity and therapist competence ratings, because the latter variables were not routinely reported.

6. Methods

6.1. Sample of studies

A database search to retrieve published articles was performed on *PsychInfo, MedLine, Dissertation Abstracts, and Cumulative Index to Nursing and Allied Health* for the years January 1980 – October 2018 using the following keywords: cognitive, dissonance, prevention, preventive, intervention, body dissatisfaction, body acceptance, and eating disorder. We also examined the reference sections of identified articles and reviews. With regard to unpublished studies, we asked established prevention researchers (e.g., Drs. Becker and Halliwell) if they had or knew of any results from unpublished trials. Further, we contacted investigators (e.g., Ms. Daníelsdóttir) who had requested the intervention script for the *Body Project* from Dr. Stice. We included randomized and non-randomized controlled trials that allowed estimation of between-condition effect sizes relative to either assessment-only/waitlist control conditions or alternative interventions. We also included studies that used a single-group repeated measures design without a comparison condition if it was possible to calculate adjusted within-condition effect sizes. We would have included studies written in other languages, but did not locate any published or unpublished studies in languages other than English.

6.2. Statistical methods

6.2.1. Effect size estimation

An effect size in the metric of mean difference in standard deviation (*SD*) units (equivalent to Cohen's *d*) was derived for each study. Effect sizes were computed using the R metafor package (Viechtbauer, 2010) which implemented recommendations from Morris (2008) for computing effect sizes for meta-analyses that combine studies from repeated measures, independent-groups, and independent-group repeated measures designs. Effect sizes were adjusted with a small sample bias-correction function (Hedges, 1982) because effects from small samples are more likely to be biased. Effect sizes and sampling variance estimates were computed in a raw-score *SD* metric, based on raw scores, as opposed to change scores. Sampling variances for effect sizes based on repeated measures require a pretest-posttest correlation that is typically unreported. Thus, for all repeated measures studies we used an estimate of the pretest-posttest correlation that was based on raw data from two studies conducted by the authors (Stice, Rohde et al., 2015). Studies with a single-group repeated measures design ($n = 10$; see Appendix Table 1) potentially overestimate effect size because the effect does not contain an adjustment for longitudinal change that would have occurred in the control group. We followed Becker (1988), in which single-group repeated measures effect sizes are adjusted by first conducting a meta-analysis using only control groups, then subtracting the average control group change from single-group repeated measures effect sizes. For studies with multiple follow-ups, we used only the assessment that most closely followed the intervention, because all trials collected data at posttest (only 5 of the 56 identified trials collected follow-up data over 2- to 3-year follow-up). Further, because intervention effects are typically strongest at posttest, focusing on pretest to posttest effects would afford the greatest sensitivity to detecting moderators that predict variation in effect sizes. When effect sizes could not be derived from the report, this information was requested from the authors and was obtained in all but one case. *SD*s were obtained from the standard error when only the latter was provided ($n = 5$; Appendix Table 1).

6.2.2. Selection of outcomes

Trials in this meta-analysis evaluated interventions designed to reduce eating disorder risk factors and symptoms. We identified five outcomes commonly reported in these trials: eating disorder symptoms and four risk factors (thin-ideal internalization, body dissatisfaction, dieting, negative affect), the latter four variables have been found to predict future onset of eating disorders in multiple independent prospective studies (e.g., Allen, Byrne, Oddy, & Crosby, 2013; Stice, Gau, et al., 2017), but are not diagnostic symptoms of eating disorders per DSM-IV or V. Effect sizes were reverse coded if a decrease in the outcome indicated improvement so that positive effect sizes reflect improvement in the outcome.

6.2.3. Intervention group contrasts

Effect sizes were computed for dissonance prevention programs versus assessment-only/educational brochure control conditions and for dissonance interventions versus credible alternative comparison interventions, which included group interventions and educational video conditions. This is because effect sizes are typically larger when an intervention is compared to a minimal intervention control condition versus when compared to a credible alternative comparison intervention, presumably because expectancies, demand characteristics, and non-specific effects inherent to any credible intervention contribute to greater reductions in the outcomes. Assessment-only and educational brochure control conditions were both coded as minimal-intervention control conditions for the analyses, as both tended to be associated with similar change in outcomes. Trials with neither of the above comparison conditions were treated as within-group repeated measures studies and were combined with effect sizes from trials that used minimum intervention control conditions because the effect sizes from the uncontrolled trials were adjusted for the typical reduction in minimal intervention control conditions (see above). We compared effect sizes for the dissonance interventions relative to each of these two classes of comparison conditions to assess the impact of type of comparison

Table 1
Operationalization and descriptive statistics for moderators.

Moderator	Coding description and criteria	Descriptive statistics
Intervention features		
Number of dissonance inducing activities	Continuous variable reflecting the number of dissonance-inducing activities completed.	*M* = 8.74, *SD* = 3.54 *Range* = (0, 12)
Number of intervention sessions	Continuous variable reflecting the total number of sessions.	*M* = 3.31, *SD* = 1.71 *Range* = (1, 12)
Average group size	Continuous variable reflecting the number of participants in intervention groups.	*M* = 9.75, *SD* = 6.15 *Range* = (1, 31)
Online intervention	Dichotomous variable reflecting whether intervention was delivered online 1 = *Yes*; 0 = *No*	*Yes* = 5; *No* = 63
Sessions video or audio recorded	Dichotomous variable reflecting whether sessions were video-recorded 1 = *Yes*; 0 = *No*	*Yes* = 29; *No* = 39
Participants compensated financially or otherwise (e.g., course credit) for study participation	Dichotomous variable reflecting whether participants were compensated. 1 = *Yes*; 0 = *No*	*Yes* = 38; *No* = 29
Participant Features		
Body image concerns required	Dichotomous variable reflecting whether participants were required to have body image concerns 1 = *Yes*; 0 = *No*	*Yes* = 43; *No* = 25
Mandatory participation	Dichotomous variable reflecting whether participation was mandatory 1 = Yes; 0 = No	Yes = 10; No = 58
Mean age of sample	Continuous variable reflecting mean age of participants at baseline	*M* = 19.60, *SD* = 4.10 Range = (12.07, 43.20)
Percentage of European-American participants	Continuous variable reflecting percentage of the sample that was European-American.	*M* = 74.26, *SD* = 16.64 Range = (33, 100)
Facilitator features		
Facilitator type	Categorical variable reflecting whether facilitator was a researcher, clinician, or peer	Researcher = 27; Clinician = 15; Peer = 20
Number of facilitators	Continuous variable reflecting the number of facilitators who led groups	*M* = 1.90, *SD* = 0.85 *Range* = (0, 4)
Number of training hours	Continuous variable reflecting the number of training hours	*M* = 6.00, *SD* = 4.21 *Range* = (0, 16)
Facilitators supervised	Dichotomous variable reflecting whether facilitators were supervised 1 = *Yes*; 0 = *No*	*Yes* = 43; *No* = 18

condition on effect sizes in preliminary analyses. Because results confirmed that dissonance prevention programs produced larger effect when compared to minimal intervention control conditions versus when compared to credible alternative comparison conditions we examined moderators separately for these two comparison conditions.

6.2.4. Operationalization and coding of effect size moderators

Table 1 lists the operational definition of each moderator. The reliability of moderator coding was assessed by examining agreement between the third and fourth authors. Cohen's κ were computed for categorical moderators and intraclass correlations (ICC) were computed for continuous moderators. In a randomly selected subset of 10 studies included in this meta-analytic review, the mean κ = 0.84 and the mean ICC = 0.95 for the moderators. Thus, inter-rater reliability was high for moderator coding.

6.2.5. Moderator models

We investigated moderators in a mixed-effects regression model in which effect size heterogeneity is represented through an additional error term that allows for variance in the true effect size and study characteristics were treated as fixed effect moderators. We implemented multilevel meta-analysis models to analyze moderators of effect sizes using the meta3 function from the metaSEM R package (Cheung, 2015b). Multilevel meta-analysis is an application of variance-known multilevel models in which effect sizes are nested in higher-level units, such as papers, and the variance of the effect sizes is treated as known based on standard formulas for effect sizes variance, but estimation for all other model parameters is conducted in a multilevel model framework (Hox, 2010), which is ideal for situations in which there are multiple effect sizes within a study (Maas, Hox, & Lensvelt-Mulders, 2004). Many studies had more than two conditions that could be used to compute effect sizes, resulting in multiple effect sizes per study. Prior to examining moderators, effect size heterogeneity was estimated using the *Q* statistic (Cochran, 1954), which tests the null hypotheses that effect size variance is zero and can be applied to both the mixed-effects and multilevel models (Cheung, 2015a).

We fit a multilevel regression model for each moderator. In moderator models in which random effects were not estimable (i.e., near zero values) or optimization did not succeed, the models were refit as mixed-effect models using the meta function from the metaSEM R package (Cheung, 2015b). Facilitator type contained three levels (clinician, researcher, peer) and we thus dummy-coded for researcher and peer, using clinicians as the reference group. Continuous variables were standardized in a *z* score format prior to fitting models to facilitate interpretation of the coefficients which effectively placed continuous moderators on the same scale (i.e., *M* = 0, *SD* = 1). For continuous moderators we tested for both linear and quadratic effects; in the event of a significant quadratic effect, we report the linear and quadratic terms. For models that did not exhibit a significant quadratic effect, we report only the linear model. Most quadratic models exhibited an inverted-U (i.e., effect sizes increased to an inflection point and then began to decrease). We computed fitted values for 2 SDs above and below the means to determine the point where the maximum effect size was achieved. In the few instances in which a U-shaped quadratic effect was observed, we note this pattern and report the value of the moderator with the smallest effect. When we observed significant moderator effects, we probed the effects using model-based marginal means (Bauer & Curran, 2005). For categorical moderators, we estimated marginal effects for each level of the moderator; for continuous, mean-centered moderators, we estimated marginal effects at 1 *SD* above and below the mean.

7. Results

The literature search identified 59 published or unpublished reports from which we extracted data for 71 dissonance eating disorder prevention programs. Appendix Table 1 provides an overview of each study and the results. We excluded Wiseman et al. (2004) because we

Table 2
Effect sizes (d) for dissonance-based eating disorder prevention programs versus controls and uncontrolled repeated measures designs.

Study	Thin-Ideal Internalization	Body Dissatisfaction	Dieting	Negative Affect	Eating Disorder Symptoms
Dissonance-based programs versus controls and uncontrolled repeated measures designs					
Amaral, Stice, and Ferreira (2018)	1.95	1.00	1.09	0.68	0.30
Atkinson & Wade (2015) (dissonance v. classes as usual)	−0.01	0.18	0.24	0.19	0.44
Atkinson and Wade (2016) (dissonance v. control)	0.50	0.38	0.60	0.34	0.72
Becker et al. (2005) (dissonance v. waitlist)	0.26	0.33	0.21	–	0.33
Brown, et al. (2017)	0.47	–	–	–	0.31
Brown and Keel (2015) (dissonance v. waitlist)	0.73	0.77	0.58	–	0.55
Casasnovas et al. (2018)	0.16	0.19	−0.14	0.24	−0.09
Chithambo & Huey (2016) (dissonance v. no intervention)	0.94	0.24	0.29	0.46	0.15
Ciao et al. (2015) (peer-led dissonance v. waitlist)	0.96	0.47	0.22	–	0.43
Corning, et al. (2010) (intervention v. control)	0.16	0.24	–	–	–
Cruwys, Haslam, Fox, & McMahon (2015) (no comparison group)	1.55	0.71	1.49	–	–
Danielsdottir et al., (2012) (study 1) (body project intervention v. waitlist control)	0.76	0.19	0.23	–	0.39
Danielsdottir et al., (2012) (study 2) (no comparison group)	0.37	0.82	0.10	–	0.19
Green et al., (2016) (dissonance v. control)	–	–	–	–	1.02
Green, Kroska, and Herrick (2018)	0.21	0.40	–	0.20	0.64
Green et al. (2005) (high dissonance) (high level dissonance v. control)	–	–	–	–	0.13
Green et al. (2005) (low dissonance) (low level dissonance v. control)	–	–	–	–	−0.25
Green et al. (2017)	0.59	0.92	–	–	1.02
Greif, Becker, & Hildebrandt (2015) (no comparison group)	0.91	0.23	–	0.17	0.39
Grossman, et al. (2018)	0.75	0.20	–	–	–
Halliwell & Diedrichs (2013) (intervention v. control)	0.49	0.40	0.14	–	–
Halliwell et al. (2015) (dissonance v. control)	0.12	0.32	–	–	–
Jilka (2002) (cognitive dissonance v. no treatment)	1.03	0.48	0.14	–	0.26
Jilka (2002) (cognitive dissonance v. psychoeducational control)	1.31	0.68	−0.01	–	0.44
Kilpela et al. (2016) (female-only) (Female only dissonance v. Waitlist)	0.12	0.81	–	0.36	0.56
Kilpela et al. (2016) (mixed sex) (Mixed sex dissonance v. Waitlist)	0.47	0.56	–	0.28	0.60
Kilpela et al. (2015) (2-session dissonance) (no comparison group)	0.55	0.48	0.36	0.25	0.22
Kilpela et al. (2015) (4-session dissonance) (no comparison group)	0.24	0.07	0.14	0.11	0.17
Kilpela et al., 2014 (no comparison group)	0.81	0.24	–	0.25	0.19
Krishna (2011, Honors Thesis) (dissonance v. control)	0.87	–	–	–	0.72
Machin (2008, Dissertation) (cognitive dissonance v. control)	0.31	0.17	−0.04	–	0.83
Matusek et al. (2004) (dissonance v. control)	0.69	0.18	–	0.25	0.43
McMillan et al. (2011) (high dissonance) (high dissonance v. control)	1.21	0.64	0.70	0.29	0.47
McMillan et al. (2011) (low dissonance) (low dissonance v. control)	0.87	0.67	0.55	0.17	0.10
Mitchell et al. (2007) (dissonance v. control)	0.15	0.33	−0.01	0.43	0.72
Pennesi and Wade (2017) (dissonance v. control)	–	–	–	−0.29	0.33
Perez, Becker, and Ramirez (2010) (no comparison group)	0.53	0.30	0.15	–	0.13
Presnell, Seidel, and Madeley (2008) (dissonance only v. assessment only control) Ramirez, Perez, and Taylor (2012)	0.18	0.34	0.45	0.38	0.07
(Prevention Group v. Comparison Group)	0.25	0.60	–	0.03	0.14
Roehrig, Thompson, Brannick, and van den Berg (2006) (no comparison group)	0.46	0.33	0.54	0.80	0.63
Rohde et al. (2014) (study 1) (Body Project v. Control)	0.32	0.81	0.44	0.15	0.22
Rohde et al. (2014) (study 2) (Body Project v. Control)	0.23	0.36	0.59	0.55	0.84
Serdar, 2006 Group dissonance (Group dissonance v. Control)	0.11	0.08	0.23	0.26	0.26
Serdar, 2006 Internet dissonance (Internet dissonance v. Control)	0.03	0.17	0.06	0.14	0.16
Smith and Petrie (2008) (cognitive dissonance v. control)	0.47	−0.25	–	0.30	0.20
Stice, Butryn, Rohde, et al. (2013) (Dissonance v. Control)	0.74	0.61	0.50	0.41	0.38
Stice et al. (2006) (dissonance v. assessment only)	0.87	0.61	0.43	0.39	0.30
Stice et al. (2009) (dissonance v. Brochure controls)	0.50	0.43	0.30	0.21	0.30
Stice et al. (2000) (dissonance v. waitlist)	0.49	0.46	0.22	0.76	0.50
Stice, Durant, et al. (2014) (Body Project) (Body Project v. Brochure controls)	0.75	0.87	0.50	0.42	0.49
Stice, Durant, et al. (2014) (eBody) (eBody Project v. Brochure controls)	0.72	0.84	0.60	0.61	0.39
Stice, Rohde, Durant, et al. (2013) (clinician-led groups Study 1) (Clinician-led intervention v. Brochure controls)	1.06	0.90	0.77	0.57	0.52
Stice, Rohde, Durant, et al. (2013) (peer-led groups Study 1) (Peer-led intervention v. Brochure controls)	0.81	0.61	0.43	0.26	0.33
Stice, Rohde, Durant, et al. (2013) (peer-led groups Study 2) (Peer-led intervention v. Delayed controls)	1.06	0.85	0.86	0.64	0.38
Stice et al. (2003) (dissonance v. waitlist)	0.52	0.34	0.27	−0.23	0.31
Trost (2006) (HIP intervention v. waitlist)	0.24	0.15	0.12	0.14	0.16
Van Diest & Perez, 2013 (no comparison group)	0.73	0.37	–	–	0.25
Wade et al. (2009) (cognitive dissonance v. control)	–	0.37	–	–	–
Wolfe (1992, Dissertation) (high dissonance v. minimal dissonance)	−0.12	−0.04	–	–	–
Dissonance-based programs versus alternative interventions					
Atkinson and Wade (2016) (dissonance v. mindfulness)	−0.08	0.14	0.07	0.00	0.31
Atkinson and Wade (2016) (dissonance v. mindfulness)	−0.95	−0.58	−0.11	−0.45	−0.11
Becker et al. (2008) (dissonance v. media advocacy)	0.22	0.02	0.13	–	0.11
Becker et al. (2012) (athlete-modified dissonance v. athlete-modified healthy weight)	0.18	0.17	0.15	0.22	0.12

(*continued on next page*)

Table 2 (*continued*)

Study	Thin-Ideal Internalization	Body Dissatisfaction	Dieting	Negative Affect	Eating Disorder Symptoms
Becker et al. (2006) (dissonance v. media psychoeducation)	0.16	0.11	0.13	–	0.18
Becker et al. (2006) (dissonance v. media advocacy)	0.40	−0.07	0.11	–	0.01
Becker et al. (2010) (dissonance v. modified healthy weight)	0.44	0.25	0.18	0.32	0.33
Chithambo and Huey (2017) (internet dissonance v. internet CB)	0.37	−0.20	−0.19	0.14	−0.31
Krysanski (2005, Dissertation) (dissonance v. psychoeducation control)	0.28	−0.20	0.04	−0.07	−0.11
Linville et al. (2015) (dissonance v. video control)	0.52	0.24	0.43	0.45	0.34
Machin (2008, Dissertation) (cognitive dissonance v. healthy weight)	0.47	0.42	−0.09	–	−0.56
Matusek et al. (2004) (dissonance v. healthy weight)	0.16	−0.02	–	0.29	0.12
Mitchell et al. (2007) (dissonance v. yoga)	0.01	0.40	−0.13	0.14	0.24
Pennesi and Wade (2017) (dissonance v. imagery rescripting)	–	–	–	0.02	−0.70
Smith and Petrie (2008) (cognitive dissonance v. healthy weight)	0.54	0.58	–	0.51	0.27
Stice et al. (2001) (dissonance v. early version of the healthy weight)	0.62	0.31	0.16	0.09	0.04
Stice et al., (2006) (dissonance v. expressive writing)	0.73	0.71	0.42	0.40	0.41
Stice et al., (2006) (dissonance v. healthy weight)	0.39	0.31	0.38	0.19	0.16
Stice, Durant, et al. (2014) (Body Project) (Body Project v. Video controls)	0.54	0.50	0.35	0.34	0.35
Stice, Durant, et al. (2014) (eBody) (eBody Project v. Video controls)	0.52	0.37	0.40	0.43	0.14
Stice, Rohde, et al. (2017a),Stice, Rohde, et al., 2017b), (eBody Project v. Video controls)	0.41	0.27	0.32	0.20	0.29
Stice, Rohde, et al. (2017a),Stice, Rohde, et al., 2017b), Peer (Body Project v. Video controls)	0.67	0.64	0.49	0.34	0.28
Stice, Rohde, et al. (2017a),Stice, Rohde, et al., 2017b), Clinician (Body Project v. Video controls)	0.74	0.60	0.57	0.37	0.43
Stice et al. (2003) (dissonance v. healthy weight)	0.03	0.10	0.16	−0.09	−0.14
Wade et al. (2009) (cognitive dissonance v. acceptance)	–	−0.01	–	–	–
Wade et al. (2009) (cognitive dissonance v. distraction)	–	−0.21	–	–	–
Wade et al. (2009) (cognitive dissonance v. ruminative attention control)	–	0.30	–	–	–

could not obtain means and *SD*s. We excluded Magnuson (2012), which evaluated a single intervention that contained dissonance-based activities as well as cognitive behavioral and media literacy activities, because it was impossible to determine whether it was the dissonance-induction content that produced intervention effects. We excluded Ciao (2012), study 1 due to its small sample size ($n = 9$); Study 2 from Ciao, Latner, Brown, Ebneter, and Becker (2015) was included. This resulted in a total of 56 papers that evaluated 68 dissonance eating disorder prevention programs (total $N = 7808$ participants) that were included. There was an average of 1.26 unique condition contrasts per dissonance programs, resulting in 86 possible effect sizes per outcome. Observed effect sizes for dissonance interventions versus control conditions and versus alternative interventions are reported in the upper and lower portions of Table 2, respectively.

7.1. Average effect size and effect size heterogeneity

Average effect sizes were significantly different from zero for all five examined outcomes: thin-ideal internalization ($d = 0.50$, $z = 9.20$, $p < .001$), body dissatisfaction ($d = 0.36$, $z = 9.63$, $p < .001$), dieting ($d = 0.31$, $z = 5.97$, $p < .001$), negative affect ($d = 0.25$, $z = 8.46$, $p < .001$), and eating disorder symptoms ($d = 0.26$, $z = 9.26$, $p < .001$). There was significant heterogeneity in effect sizes for thin-ideal internalization (Q [76] = 358.51, $p < .001$), body dissatisfaction (Q [78] = 231.84, $p < .001$), dieting (Q [58] = 272.08, $p < .001$), negative affect (Q [55] = 89.53, $p = .002$), and eating disorder symptoms (Q [75] = 133.16, $p < .001$).

The magnitude of effect sizes for dissonance interventions versus control conditions and pre-post only studies was larger than those for dissonance interventions versus alternative interventions for thin-ideal internalization ($d = 0.28$, $z = 3.66$, $p < .001$), body dissatisfaction ($d = 0.23$, $z = 3.69$, $p < .001$), dieting ($d = 0.19$, $z = 3.03$, $p = .002$), and eating disorder symptoms ($d = 0.18$, $z = 3.05$, $p = .002$) but not for negative affect ($d = 0.08$, $z = 1.55$, $p = .121$). Thus, we conducted separate analyses for the average effect sizes and moderators of effect size magnitude for dissonance prevention programs versus control conditions and for dissonance prevention programs versus alternative interventions.

Average dissonance eating disorder prevention programs versus control condition effect sizes were significantly different from zero for all outcomes: thin-ideal internalization ($d = 0.57$, $z = 9.83$, $p < .001$), body dissatisfaction ($d = 0.42$, $z = 10.78$, $p < .001$), dieting ($d = 0.37$, $z = 5.88$, $p < .001$), negative affect ($d = 0.29$, $z = 8.65$, $p < .001$), and eating disorder symptoms ($d = 0.31$, $z = 9.89$, $p < .001$). Similarly, average dissonance eating disorder prevention programs versus alternative intervention condition effect sizes were significantly different from zero, though smaller in magnitude, for thin-ideal internalization ($d = 0.31$, $z = 4.48$, $p < .001$), body dissatisfaction ($d = 0.18$, $z = 2.90$, $p = .004$), dieting ($d = 0.17$, $z = 3.12$, $p = .002$), negative affect ($d = 0.21$, $z = 5.35$, $p < .001$), and eating disorder symptoms ($d = 0.13$, $z = 2.31$, $p = .021$).

7.2. Moderators of eating disorder prevention program effects

7.2.1. Thin-ideal internalization

Table 3 reports moderators of effects for dissonance prevention programs versus control conditions. Three moderators were significant. Number of facilitators was a significant predictor of effect sizes, wherein effect sizes were larger for interventions with more facilitators ($z = 2.04$, $p = .041$); simple slope estimates of effect sizes were $d = 0.46$ at 1 *SD* below the mean (1.0) and $d = 0.73$ at 1 *SD* above the mean (2.8). Number of training hours exhibited a non-significant positive linear effect ($z = 1.06$, $p = .288$) and a significant negative quadratic effect ($z = -2.55$, $p = .011$); in the inverted U function, effects sizes peaked at 7.3 h of training. Effect sizes were significantly larger when facilitators were supervised ($d = 0.68$) than when they were not ($d = 0.37$) ($z = 2.52$, $p = .012$).

Among dissonance intervention versus alternative intervention contrasts, three moderators were significant (Table 4). Group size exhibited a non-significant linear effect ($z = 0.27$, $p = .790$) and a significant negative quadratic effect ($z = -2.05$, $p = .040$); effects sizes peaked at group sizes of 10.4. Effect sizes were significantly larger when participants were compensated financially or otherwise ($d = 0.51$) than when they were not ($d = 0.12$) ($z = 3.86$, $p < .001$). Effect sizes were significantly larger when facilitators were supervised ($d = 0.39$) than when they were not ($d = 0.01$) ($z = 2.44$, $p = .015$).

Table 3
Moderator models for effect sizes from dissonance-based eating disorder prevention programs versus control conditions.

Moderator	Thin-ideal internalization	Body dissatisfaction	Dieting	Negative affect	Eating disorder symptoms
Unconditional means model	0.57***	0.42***	0.37***	0.29***	0.31***
Number of dissonance-inducing activities	0.06	0.12***	0.13	0.08*	0.05
Number of intervention sessions					
Linear	−0.09	0.05	−0.01	0.13*	0.07
Quadratic		−0.03*		−0.06*	
Average group size					
Linear	−0.02	0.00	−0.09	0.00	−0.02
Quadratic				−0.12*	
Delivered online	−0.13	−0.03	−0.12	0.02	−0.05
Sessions video-recorded	0.10	0.12	0.18	0.10	0.03
Body image concerns required	0.22	0.16*	0.25	0.13*	0.13*
Mandatory participation	−0.11	−0.21*	−0.33	−0.09	−0.14*
Compensated financially or otherwise	0.01	0.13	0.09	0.03	0.07
Mean age of sample	−0.01	−0.02	−0.01	0.00	0.03
Percentage of European-American participants					
Linear	−0.06	−0.05	−0.13*	−0.06*	−0.08**
Quadratic			−0.09*		−0.06**
Type of facilitator (ref = clinician)					
Peer	0.07	−0.10	−0.05	−0.09	−0.01
Researcher	−0.06	−0.14	−0.03	−0.02	0.12
Number of facilitators	0.14*	0.06	0.04	0.04	0.03
Number of training hours					
Linear	0.06	0.04	−0.02	0.00	−0.01
Quadratic	−0.11*				
Facilitators supervised	0.31*	0.21*	0.22	0.10	0.07

7.2.2. Body dissatisfaction

Among dissonance intervention versus control condition contrasts, five moderators were significant predictors of effect size. Number of dissonance-inducing activities was a significant predictor of effect sizes, wherein effects were larger for studies with a greater number of dissonance-inducing activities ($z = 3.37$, $p < .001$); effect sizes were $d = 0.30$ at 1 *SD* below the mean (5.2) and $d = 0.54$ at 1 *SD* above the mean (12.3). It should be noted that sometimes the value of the moderator at 1 *SD* above or below the mean was slightly outside the observed range of the moderator (extrapolation), which can occur if the moderator has a skewed distribution (Hays, 1994). Intervention duration exhibited a non-significant positive linear effect ($z = 0.98$, $p = .328$) and a significant negative quadratic effect ($z = -2.11$, $p = .035$); effects sizes peaked at 4.9 intervention sessions. Effect sizes were significantly larger when body image concerns were required ($d = 0.50$) than when they were not ($d = 0.34$) ($z = 2.25$, $p = .025$).

Table 4
Moderator models for effect sizes from dissonance-based eating disorder prevention programs versus alternative interventions.

Moderator	Thin-Ideal Internalization	Body Dissatisfaction	Dieting	Negative Affect	Eating Disorder Symptoms
Unconditional means model	0.31***	0.18**	0.17**	0.21***	0.13*
Number of dissonance-inducing activities	0.13	0.14*	0.24***	0.07	0.19***
Number of intervention sessions					
Linear	0.09	0.17*	0.00	0.08	0.07
Quadratic					−0.17*
Average group size					
Linear	0.04	−0.09	−0.11	0.00	0.25**
Quadratic	−0.36*			−0.16*	−0.21*
Delivered online	−0.11	−0.26*	−0.08	−0.01	−0.10
Sessions video-recorded	0.20	0.24*	0.28**	0.16*	0.23
Body image concerns required	0.21	0.16	0.07	0.07	−0.08
Mandatory participation	−0.15	−0.12	−0.06	−0.12	0.09
Compensated financially or otherwise	0.39***	0.29**	0.27**	0.16	−0.02
Mean age of sample					
Linear	0.18	0.02	0.06	0.06	−0.00
Quadratic			0.29*		0.39**
Percentage of European-American participants					
Linear	−0.09	−0.12	−0.06	−0.01	0.10
Quadratic		−0.12**	−0.10*		
Type of facilitator (ref = clinician)					
Peer	−0.11	−0.09	−0.25	−0.07	−0.18
Researcher	−0.28	−0.13	−0.32**	−0.25*	−0.21*
Number of facilitators					
Linear	0.01	0.05	0.08*	0.06	0.12***
Quadratic		−0.09**	−0.08***		−0.08***
Number of training hours					
Linear	0.09	−0.10	−0.07	0.08	−0.10
Quadratic		−0.42**	−0.35***		−0.40***
Facilitators supervised	0.38*	0.37*	0.32**	0.23**	0.26

Effect sizes were significantly larger for studies with voluntary participation ($d = 0.47$) versus mandatory participation ($d = 0.26$) ($z = -2.42$, $p = .015$). Effect sizes were significantly larger when facilitators were supervised ($d = 0.48$) than when they were not ($d = 0.28$) ($z = 2.47$, $p = .013$).

Among dissonance intervention versus alternative intervention contrasts, nine moderators were significant. Number of dissonance-inducing activities exhibited a significant positive linear effect ($z = 2.50$, $p = .012$); effect sizes were $d = 0.06$ at 1 *SD* below the mean (5.2) and $d = 0.34$ at 1 *SD* above the mean (12.3). Total intervention duration was a significant predictor of effect sizes, wherein effects were larger for studies that evaluated interventions of longer durations ($z = 2.03$, $p = .042$); effect sizes were $d = 0.05$ at 1 *SD* below the mean (1.6) and $d = 0.38$ at 1 *SD* above the mean (5.0). Effect sizes were significantly smaller for online interventions ($d = -0.06$) than in-person interventions ($d = 0.21$) ($z = -2.15$, $p = .031$). Effect sizes were significantly larger when sessions were video-recorded ($d = 0.39$) than when they were not ($d = 0.14$) ($z = 2.06$, $p = .039$). Effect sizes were significantly larger when participants were compensated ($d = 0.38$) than when they were not ($d = 0.09$) ($z = 3.07$, $p = .002$). Percentage of European-American participants exhibited a non-significant negative linear effect ($z = -1.85$, $p = .065$) and a significant negative quadratic effect ($z = -2.82$, $p = .005$); effects sizes were largest at 65.9% European-American. Number of facilitators exhibited a non-significant positive linear effect ($z = 1.10$, $p = .271$) and a significant negative quadratic effect ($z = -3.01$, $p = .003$); effects sizes were largest at 2.2 facilitators. Number of training hours exhibited a non-significant negative linear effect ($z = -0.98$ $p = .333$) and a significant negative quadratic effect ($z = -2.85$, $p = .004$); effects sizes peaked at 5.6 h of training. Effect sizes were significantly larger when facilitators were supervised ($d = 0.27$) than when they were not ($d = -0.10$) ($z = 2.46$, $p = .014$).

7.2.3. Dieting

Among dissonance intervention versus control condition contrasts, only percentage of European-American participants was a significant predictor of effect size. The percentage of European-American participants exhibited a significant negative linear effect ($z = -2.40$, $p = .016$) and a significant negative quadratic effect ($z = -2.27$, $p = .023$); effects sizes were largest at 62.6% European-American.

Among dissonance intervention versus alternative intervention contrasts, eight moderators were significant. Number of dissonance-inducing activities was a significant predictor of effect sizes, wherein effects were larger for studies with a greater number of dissonance-inducing activities ($z = 3.71$, $p < .001$); effect sizes were $d = -0.10$ at 1 *SD* below the mean (5.2) and $d = 0.37$ at 1 *SD* above the mean (12.3). Effect sizes were significantly larger when sessions were video-recorded ($d = 0.44$) than when they were not ($d = 0.16$) ($z = 2.99$, $p = .003$). Effect sizes were significantly larger when participants were compensated ($d = 0.37$) than when they were not ($d = 0.10$) ($z = 3.28$, $p = .001$). Mean age of sample exhibited a non-significant positive linear effect ($z = 0.62$, $p = .533$) and a significant positive quadratic effect ($z = 2.01$, $p = .045$) that resulted in a U-shaped curve with the minimal effect size at age 19.2. The percentage of European-American participants exhibited a non-significant negative linear effect ($z = -0.82$, $p = .410$) and a significant negative quadratic effect ($z = -2.39$, $p = .017$); effects sizes were largest at 69.3% European-American. Effect sizes were significantly larger for clinician-led groups ($d = 0.49$) versus researcher-led groups ($d = 0.17$; $z = -2.72$, $p = .007$); peer-led groups ($d = 0.24$) did not differ from clinician-led groups ($z = -1.94$, $p = .053$). Number of facilitators exhibited a significant positive linear effect ($z = 2.22$, $p = .026$) and a significant negative quadratic effect ($z = -3.71$, $p < .001$); effects sizes peaked at 2.3 facilitators. Number of training hours exhibited a non-significant negative linear effect ($z = -0.92$, $p = .357$) and a significant negative quadratic effect ($z = -3.33$, $p < .001$); effects sizes peaked at 5.6 h of training. Effect sizes were significantly larger when facilitators were supervised ($d = 0.26$) than when they were not ($d = -0.06$) ($z = 2.60$, $p = .009$).

7.2.4. Negative affect

Among dissonance intervention versus control condition contrasts, five moderators were significant. Number of dissonance-inducing activities was a significant predictor of effect sizes, wherein effects were larger for studies with a greater number of dissonance-inducing activities ($z = 2.15$, $p = .031$); effect sizes were $d = 0.18$ at 1 *SD* below the mean (5.2) and $d = 0.35$ at 1 *SD* above the mean (12.3). Intervention duration exhibited a significant positive linear effect ($z = 2.60$, $p = .009$) and a significant negative quadratic effect ($z = -2.11$, $p = .035$); effects sizes peaked at 5.0 intervention sessions. Group size exhibited a non-significant negative linear effect ($z = -0.04$, $p = .970$) and a significant negative quadratic effect ($z = -2.13$, $p = .033$); effects sizes peaked at group sizes of 9.8. Effect sizes were significantly larger when body image concerns were required ($d = 0.34$) than when they were not ($d = 0.20$) ($z = 2.19$, $p = .029$). The percentage of European-American participants was a significant moderator of effect sizes, wherein effects were lower in interventions with a greater percentage of European-American participants ($z = -2.26$, $p = .024$); effect sizes were $d = 0.31$ at 1 *SD* below the mean (57.6) and $d = 0.20$ at 1 *SD* above the mean (90.9).

Among dissonance intervention versus alternative intervention contrasts, four moderators were significant. Group size exhibited a non-significant linear effect ($z = 0.01$, $p = .994$) and a significant negative quadratic effect ($z = -2.32$, $p = .020$); effects sizes peaked at group sizes of 9.8. Effect sizes were significantly larger for video-recorded studies ($d = 0.33$) versus non-recorded studies ($d = 0.17$) ($z = 2.15$, $p = .032$). Effect sizes were significantly larger for clinician-led groups ($d = 0.37$) versus researcher-led groups ($d = 0.30$; $z = -2.54$, $p = .011$); peer-led groups ($d = 0.12$) did not differ from clinician-led groups ($z = -0.60$, $p = .548$). Effect sizes were significantly larger when facilitators were supervised ($d = 0.27$) than when they were not ($d = 0.04$) ($z = 2.64$, $p = .008$).

7.2.5. Eating disorder symptoms

Among dissonance intervention versus control condition contrasts, there were three significant moderators. Effect sizes were significantly larger when body image concerns were required ($d = 0.36$) than when they were not ($d = 0.23$) ($z = 2.42$, $p = .016$). Effect sizes were significantly smaller when interventions were mandated ($d = 0.20$) than when they were not ($d = 0.34$) ($z = -2.53$, $p = .011$). Percentage of European-American participants exhibited a significant negative linear effect ($z = -3.03$, $p = .002$) and a significant negative quadratic effect ($z = -2.61$, $p = .009$); effects were largest at 62.6% European-American.

Among dissonance intervention versus alternative intervention contrasts, seven moderators were significant. Number of dissonance-inducing activities was a significant predictor of effect sizes, wherein effects were larger for studies with more dissonance-inducing activities ($z = 3.92$, $p < .001$); effect sizes were $d = -0.09$ at 1 *SD* below the mean (5.2) and $d = 0.29$ at 1 *SD* above the mean (12.3). Intervention duration exhibited a non-significant positive linear effect ($z = 0.89$, $p = .375$) and a significant negative quadratic effect ($z = -2.27$, $p = .023$); effects sizes peaked at 3.7 intervention sessions. Group size exhibited a significant positive linear effect ($z = 2.86$, $p = .004$) and a significant negative quadratic effect ($z = -2.51$, $p = .012$); effects sizes peaked at group sizes of 13.4. Mean age of sample exhibited a non-significant negative linear effect ($z = -0.05$, $p = .964$) and a significant positive quadratic effect ($z = 2.76$, $p = .006$) that resulted in a U-shaped curve with the minimal effect size at age 19.6. Effect sizes were significantly larger for clinician-led groups ($d = 0.38$) versus researcher-led groups ($d = 0.17$; $z = -2.03$, $p = .043$); peer-led groups ($d = 0.19$) did not differ from clinician-led groups ($z = -1.68$, $p = .093$). Number of facilitators exhibited a significant positive linear

effect ($z = 3.44$, $p < .001$) and a significant negative quadratic effect ($z = -4.14$, $p < .001$); effects sizes peaked at 2.5 facilitators. Number of training hours exhibited a non-significant negative linear effect ($z = -0.92$, $p = .357$) and a significant negative quadratic effect ($z = -3.33$, $p < .001$); effects sizes peaked at 5.6 h.

8. Discussion

8.1. Summary of average effect sizes

We identified 56 published and unpublished trials from which we extracted 68 dissonance-based eating disorder prevention programs involving data from 7808 participants. As hypothesized, dissonance-based prevention programs produced significantly larger reductions in thin ideal internalization ($d = 0.57$), body dissatisfaction ($d = 0.42$), dieting ($d = 0.37$), negative affect ($d = 0.29$), and eating disorder symptoms ($d = 0.31$) in contrast to minimal-intervention control conditions, the most common type of control condition in the eating disorder prevention literature. These effects are medium to small effects. Results establish that dissonance-based prevention programs produced larger reductions in these outcomes than occur in response to the passage of time (regression to the mean) or from measurement of the outcomes, which is often associated with reductions in pathological outcomes. These analyses averaged across all dissonance-based eating disorder prevention programs, which included some that did not produce significant reductions in outcomes (see below), suggesting that these average effect sizes are a conservative estimate of efficacy. Nevertheless, the magnitude of these average effects is noteworthy because the interventions were brief and because prevention programs and treatments for these outcomes often produce only small effects relative to minimal-intervention comparison conditions (Corrieri et al., 2014; Scott-Sheldon, Carey, Elliott, Garey, & Carey, 2014; Turk et al., 2009).

Also as hypothesized, dissonance-based eating disorder prevention programs produced significantly larger reductions in thin-ideal internalization ($d = 0.31$), body dissatisfaction ($d = 0.18$), dieting ($d = 0.17$), negative affect ($d = 0.21$) and eating disorder symptoms ($d = 0.13$) in contrast to alternative interventions. These findings establish that these interventions also produce larger reductions in the outcomes than occur in response to expectancies and demand characteristics that operate in any credible active intervention condition, as well as non-specific factors that operate in any group-based intervention in which participants interact with a facilitator and other group members (e.g., emotional support). Although the effects from these more stringent comparisons are smaller, we think it vital to show that a particular intervention produces superior effects than alternative credible interventions because this establishes that the effects are not solely due to demand characteristics, expectances, and non-specific effects. These findings are encouraging because no other eating disorder prevention program has produced significantly greater reductions than active credible alternative interventions. Again, it is important to note that some of the alternative interventions are efficacious (e.g., the *Healthy Weight* eating disorder and obesity prevention program; Stice et al., 2006, Stice, Marti, Spoor, et al., 2008; Stice, Rohde, Shaw, & Marti, 2012; Stice, Rohde, Shaw, & Marti, 2013). When interpreting these average effect sizes, it is important to note that the trials in this meta-analysis included females and males, participants who ranged in age from preadolescent to later adulthood, and a wide range of ethnic and racial groups, including studies conducted in countries other than the USA (e.g., Brazil).

One noteworthy pattern in the findings was that the effects were stronger for thin-ideal internalization than for body dissatisfaction, dieting, negative affect, and eating disorder symptoms compared to both types of comparison conditions. This might be interpreted as providing additional support for the intervention theory for dissonance-based eating disorder prevention programs because the intervention activities center on exploring the costs of pursuing the thin ideal.

Effect sizes shown in Table 2 reveal that the trials that observed the largest effects ($d > 0.70$) for eating disorder symptoms, arguably the most critical outcome, emerged for the prevention programs evaluated by Atkinson and Wade (2016), Green et al. (2016), Green et al. (2017), Krishna (2011), Machin (2008), Mitchell et al. (2007), and Rohde et al., 2014) relative to minimal intervention control conditions. Given that the studies by Machin, Mitchell, and Green, and Rohde evaluated interventions that added unique exercises to the standard *Body Project* the pattern of findings suggest that these versions of dissonance-based eating disorder prevention programs might be good candidates for implementation. For instance, Green and associates added a focus of the costs of social comparisons, particularly via social media, to the standard *Body Project*, and Rohde and associates expanded the *Body Project* from 4 to 6 sessions, adding novel interactive exercises to increase engagement (e.g., groups produce a video news report on the costs of the thin beauty ideal). Adding additional dissonance-induction activities theoretically contributed to greater reductions in thin-ideal internalization, body dissatisfaction, and eating disorder symptoms, which is substantiated by the evidence discussed below that dissonance-based prevention programs that contain more dissonance-induction activities and more sessions were associated with larger intervention effects.

Some trials found very small effects for dissonance-based eating disorder prevention programs relative to control conditions (e.g., Green et al., 2005; McMillan et al., 2011; Serdar et al., 2014; Wolfe, 1992). One possible explanation for the small effect sizes for the high-dissonance prevention program evaluated by Green et al. (2005) is that it is the only trial that used a posttest-assessment design, which is not as sensitive as the pretest-posttest repeated-measures design in detecting intervention effects. The small effect sizes for the low-dissonance prevention programs evaluated by Green et al. (2005) and McMillan et al. (2011) would be expected because those interventions were expressly designed to minimize dissonance-induction, a key mechanism of effect for dissonance-based prevention programs. The most likely explanation for the small effects from Serdar et al. (2014) is that it evaluated a simple Internet-based intervention in which participants typed responses to written versions of the Socratic questions posed by facilitators to participants in *Body Project* groups, which would not be expected to induce much dissonance because of the low public accountability and level of effort required. In contrast, in the more effective *eBody Project* participants create personalized profile pages with pictures of the participant and post their anti-thin-ideal letters and videos on the Internet to increase accountability and complete additional dissonance-induction exercises to increase the level of required effort. The most likely explanation for the small effects from Wolfe (1992) is that the intervention evaluated in that study did not contain 8 out of the 9 dissonance-induction activities contained in the *Body Project*. As such, the small effects in the Green, McMillan, Serdar and Wolfe trials can be interpreted as providing additional support for the intervention theory of dissonance-based prevention programs.

Seven out of 27 trials did not find that dissonance-based prevention programs produced significantly larger reductions in outcomes that alternative interventions (Atkinson & Wade, 2016; Becker et al., 2006; Becker et al., 2008; Pennesi & Wade, 2017; Stice et al., 2001; Stice et al., 2003; Wade et al., 2009). Atkinson and Wade (2016) found that the *Body Project* did not produce significantly larger reductions in outcomes compared to a mindfulness-based eating disorder prevention program, suggesting that it would be worthwhile for future trials to evaluate such prevention program. Stice et al. (2001, 2003) found that the *Body Project* did not produce superior effects than the *Healthy Weight* eating disorder prevention program, likely because the latter has been found to produce reductions in eating disorder risk factors, eating disorder symptoms, and future eating disorder onset (Stice et al., 2006; Stice, Marti, Spoor, et al., 2008; Stice, Rohde, Durant, et al., 2013; Stice, Rohde, Gau, & Shaw, 2012). Becker et al. (2006, 2008) found that the dissonance-based eating disorder prevention program they evaluated

did not produce markedly larger reductions in eating disorder symptoms than a prevention program promoting media literacy and media advocacy, suggesting that the latter may also be efficacious. In addition, the fact that Becker and associates evaluated a streamlined version of the *Body Project* that omitted several dissonance-induction activities may also have attenuated effects, based on the evidence from moderator analyses that prevention programs with more dissonance-induction activities and more intervention sessions produce larger effects (discussed below). Pennesi and Wade (2017) found that a very brief dissonance-based prevention program did not produce larger reductions in eating disorder symptoms relative to an imagery rescripting prevention program and Wade et al. (2009) found that a very brief dissonance-based prevention program did not produce larger reductions in body dissatisfaction than a brief acceptance-based intervention or a distraction-based intervention, most likely because the dissonance-based interventions were extremely brief and did not contain most of the dissonance-induction activities used in other dissonance-based prevention programs.

8.2. Moderators of dissonance-based eating disorder prevention programs

The second aim of this meta-analysis was to test whether certain intervention, participant, and facilitator features correlate with larger effects on eating disorder risk factors and symptoms, which should be useful in guiding implementation of the most effective version of this prevention program to the optimal population and in the optimal manner by facilitators. Before interpreting the moderating effects, it is important to acknowledge that we conducted a large number of inferential tests. The fact that 34% of the moderation tests were statistically significant is reassuring because only 5% would have been expected to be significant based on chance alone. We considered using a more conservative significance threshold, however given that fewer than 60 studies contributed effect sizes to most moderator analyses, we were more concerned about false-negative findings than false-positive findings. Nonetheless, given the possibility of chance findings, it is probably best to prioritize features that moderated effects for at least two outcomes, as we have more confidence that those effects are reliable.

Interestingly, moderator analyses appeared more sensitive when the effects of dissonance-based eating disorder prevention programs were compared to active alternative interventions (43% of the moderator effects were significant) versus minimal-intervention control conditions (25% of the moderator effects were significant). This suggests that there was less “noise” in the data when dissonance-based interventions are compared to alternative interventions, potentially because alternative interventions better equate conditions on expectancy effects and demand characteristics inherent to randomized trials. We also think that the analyses comparing dissonance-based prevention programs to alternative interventions is more important because research suggests that assessment-only conditions are typically not associated with clinically meaningful reductions in outcomes and the results from the comparative trials should be more informative to clinicians attempting to decide which eating disorder prevention program to implement.

With regard to intervention features, we hypothesized that effects would be larger in trials in which participants engaged in a greater number of dissonance-inducing activities, as this should maximize dissonance-induction. This feature significantly moderated the effects on four of the five outcomes (body dissatisfaction, dieting, negative affect, and eating disorder symptoms), making it one of our most consistent effect size moderators. These results suggest that it would be optimal to implement versions of the *Body Project* that contain all of the dissonance-induction activities contained in the 4-session *Body Project*, rather than versions that omit some of these activities. Indeed, these moderator findings imply that adding addition in-session or home practice dissonance-induction exercises, such as was done for the 6-session version of the *Body Project* that has been implemented in the USA, might produce the largest reductions in outcomes and is worth evaluating further.

We had also hypothesized that dissonance-based prevention programs with more sessions would produce larger effects because they typically contain more in-session and between-session dissonance-inducing activities, which should produce more dissonance due to the increased level of effort. Further, we theorized that the increased opportunity to reflect on intervention content between sessions might promote improved consolidation of learning that contributes to larger effects. In addition, meeting multiple times as a group may contribute to a stronger development of social support, which *Body Project* participants state is one of the most valuable elements of this intervention. Effect sizes were significantly larger for two out of the five outcomes (body dissatisfaction and negative affect) for dissonance-based eating disorder prevention programs with more versus fewer sessions, with two quadratic effects suggesting that the optimal effects occur when there are 4 to 5 sessions. Results suggest that dissonance-based eating disorder prevention programs that have at least 4 sessions should be implemented if the goal is to maximize intervention effects. The evidence that programs with more dissonance-induction activities and more sessions produce larger effects than those with fewer activities and sessions provide evidence of a dose-response relation for this prevention program, which is a key test of the intervention theory for a prevention program.

We had hypothesized that intervention effects would be larger for dissonance-based eating disorder prevention programs implemented in larger versus smaller groups, as the former would theoretically increase public accountability that increases dissonance-induction. This moderator was associated with a significantly larger effect for three of five the outcomes (thin-ideal internalization, negative affect and eating disorder symptoms). The quadratic relations suggested that the largest effect size for this outcome emerged when groups contained 10 participants. This is consistent with practice guidelines for effective group psychotherapy, which recommend 7–10 participants per group (American Group Psychotherapy Association, 2007; Bernard et al., 2008).

In addition, we had hypothesized the group-based dissonance-based eating disorder prevention programs would produce larger effects than versions implemented on-line, reasoning that the former would promote greater public accountability and consequent dissonance-induction. However, group versus on-line delivery only moderated the effects of one of the five outcomes (body dissatisfaction), suggesting that this moderator may have limited impact. It is important to note that there was substantial heterogeneity of the effects for on-line delivery of dissonance-based eating disorder prevention programs. One involved only one of the 12 dissonance-induction activities from the *Body Project* (Chithambo & Huey, 2017) and another evaluated a simple Internet-based intervention in which participants typed responses to written versions of the Socratic questions posed by facilitators to participants in *Body Project* groups (Serdar et al., 2014). In contrast, we evaluated the Internet-delivered *eBody Project* that conserved each of the dissonance-induction activities from the group-based *Body Project* and included posting of writing and video home exercises on the Internet to promote public accountability. The former two programs produced a much smaller average effect than the latter program ($d = 0.04$ versus 0.43).

With regard to the last intervention feature we examined, we had hypothesized that dissonance-based eating disorder prevention programs would produce larger effects if the sessions were video-recorded because it should theoretically increase public accountability and contribute to larger effects. As expected, intervention effects were significantly larger when dissonance-based eating disorder prevention groups were video-recorded versus not video-recorded for three of the five outcomes (body dissatisfaction, dieting, and negative affect). It is also possible that videotaping sessions improves intervention fidelity because facilitators have greater accountability to supervisors, assuming the recordings are reviewed on at least a random basis, which

could also contribute to larger effects. These results imply that if interventionists are interested in maximizing intervention effects, the group-based sessions should be video-recorded, and perhaps also reviewed by supervisors.

We investigated five participant features hypothesized to moderate the effects of dissonance-based eating disorder prevention programs. Results provided support for the hypothesis that intervention effects of dissonance-based eating disorder prevention programs would be larger if implemented to body dissatisfied young women at high risk for eating disorders; this moderator affected three of the five outcomes (body dissatisfaction, negative affect, and eating disorder symptoms). Results converge with prior meta-analytic reviews that found that a broad array of eating disorder prevention programs produce larger effects when delivered to high-risk versus unselected participants (Stice, Shaw, & Marti, 2007) and with evidence that several universal prevention programs were more effective for subgroups of high-risk participants than for the full sample (e.g., Buddeberg-Fischer, Klaghofer, Gnam, & Buddeberg, 1998; Stewart, Carter, Drinkwater, Hainsworth, & Fairburn, 2001; Weiss & Wertheim, 2005). Collectively these data imply that the larger effects may occur because young women with body image concerns are more motivated to engage in the intervention, find the material more pertinent to their lives, and have more room for improvement because of their elevated risk. Results suggest that larger effects will typically occur for dissonance-based eating disorder prevention programs if they are implemented in a selective fashion to high-risk individuals. It is important to acknowledge that some participants who enroll in body acceptance eating disorder prevention programs due to body image concerns also have threshold or subthreshold eating disorders (Stice, Rohde, et al., 2017a,Stice, Rohde, et al., 2017b), which blurs the line between selected and indicated prevention. Further, it is possible that effects for dissonance-based eating disorder prevention programs will be even larger if they are implemented in an indicated fashion to young women with subclinical eating pathology versus when implemented universally to unselected populations, but only two trials evaluated a dissonance-based eating disorder prevention program when delivered in an indicated fashion (Green et al., 2016; Green, Kroska, & Herrick, 2018), preventing us from comparing the effects sizes from indicated versus selective and universal dissonance-based prevention programs. An added benefit of this approach is that the intervention can be described as a body acceptance class, which facilitates recruitment. Focusing only on high-risk individuals would make dissemination more manageable and less expensive because there are fewer high-risk individuals. However, one potential downside of indicated prevention programs is that they miss low-risk individuals who may still show onset of eating disorders.

Results provided support for the hypothesis that intervention effects would be larger if participants completed the dissonance-based eating disorder prevention programs on a voluntary versus mandated basis, though this moderator was significant for only two outcomes (body dissatisfaction and eating disorder symptoms). Theoretically, engaging in dissonance-induction activities on a voluntary basis results in greater dissonance-induction (Baumeister & Tice, 1984). These findings suggest it might be optimal to implement dissonance-based eating disorder prevention programs on a voluntary basis. However, several trials have observed significant reductions in risk factors and eating disorder symptoms when the dissonance-based prevention programs are mandated (e.g., Becker et al., 2010), suggesting that implementing the prevention programs on a mandated basis can benefit participants.

Counter to our hypothesis based on dissonance theory that intervention effects would be smaller when participants are compensated for participating in the prevention trial (typically for completing assessments) versus when they are not, moderator analyses revealed that effects were significantly *stronger* when participants were compensated for three of the five outcomes (thin-ideal internalization, body dissatisfaction, and dieting). It is possible that effects were larger when participants were compensated because they were more likely to attend more intervention sessions, which could explain the larger effects. This null finding might be interpreted as suggesting that external justification for engaging in counter-attitudinal activities does not reduce dissonance-induction, as suggested by much more tightly controlled trials (e.g., Cohen, 1962). However, we would argue that experiments that manipulate external justification for engaging in counter-attitudinal activities permit firmer inferences. For instance, it is possible that trials that compensated participants tended to require that participants have body image concerns, which might have contributed to the observed moderating effects. With regard to implementation implications, findings suggest that compensating participants for attending dissonance-based eating disorder prevention programs does not attenuate effects, and indeed that it might be optimal to do so if the goal is to maximize intervention effects.

We hypothesized that intervention effects for dissonance-based eating disorder prevention programs would be larger for older versus younger participants, based on findings from a meta-analytic review of a broader range of eating disorder prevention programs (Stice, Presnell, et al., 2007). There were significant moderating effects for two of the five outcomes (dieting and eating disorder symptoms). The significant quadratic effects for these outcomes suggested that the effects were smallest for 19-year old participants, but larger for mid-adolescent females and young-adult women. Results imply that it might be optimal to implement dissonance-based eating disorder prevention programs to mid-adolescent females and with adult women versus older-adolescent females. However, the fact that randomized trials have found that dissonance-based eating disorder prevention programs have produced significant reductions in eating disorder risk factors and symptoms for female middle school students (Halliwell et al., 2015; Rohde et al., 2014), high-school students (Stice et al., 2009, 2011), college students (Becker et al., 2010; Stice, Rohde, Durant, et al., 2013), and young adult women (Rohde et al., 2017) implies that even though effects may be smaller for 19 year olds, they are still clinically meaningful.

Results indicated that dissonance-based eating disorder prevention programs produced significantly larger reductions four of the five outcomes (body dissatisfaction, dieting, negative affect, and eating disorder symptoms) in trials that involved samples with a lower percentage of European-Americans. The main effects indicated that intervention effects tended to be larger for samples that were more ethnically diverse, though there were some quadratic effects that suggested that effects were greatest when 30%-40 of the sample were from ethnic minority groups. These findings are consistent with a prior studies that found that a dissonance-based eating disorder prevention program produced non-significantly larger reductions in eating disorder risk factors and symptoms for Asian-American, Hispanic, and African-American versus European-American participants (Rodriguez, Marchand, Ng, & Stice, 2008; Stice, Marti, & Cheng, 2014). Moreover, more recent findings from a large data set revealed that European-American young women did not report significantly higher endorsement of the thin beauty ideal than did Hispanic, Asian American, of African American young women (Cheng et al., 2018); indeed, Asian American young women reported significantly greater thin-ideal internalization than both European-American and African-American young women. Results from that large ethnic differences study and the present study suggest that pursuit of the thin beauty ideal may be a trans-ethnicity risk factor for eating disorders, which may explain why dissonance-based eating disorder prevention programs work well with multiple ethnic groups. Thus, results imply that it may not be necessary to develop eating disorder prevention programs that are tailored to specific ethnic groups, which would complicate implementation. Dissonance-based interventions, due to their use of Socratic questioning and greater engagement of participant involvement for change, may adapt more naturally to female appearance ideals associated with various cultures compared to interventions that are more focused on presenting factual information or are more proscriptive in their model of change. This suggests that dissonance-based prevention and treatment

interventions for other applied outcomes may likewise be effective for a wide variety of racial and ethnic groups.

We investigated four facilitator factors hypothesized to predict larger intervention effects for dissonance-based eating disorder prevention programs. Results provided no support for the hypothesis that clinician-led groups would produce larger intervention effects than peer educator-led groups. This finding dovetails with results from the only randomized trial to compare the effectiveness of clinician-led to peer-led *Body Project* groups (Stice, Rohde, et al., 2017a,Stice, Rohde, et al., 2017b). Collectively, these findings suggest that it should be equally valuable to implement dissonance-based eating disorder prevention programs with peer educators or clinicians. Of course, given that there are typically many more peer-educators at colleges than clinicians, and peer-educators often deliver interventions as part of their coursework (though some are paid), they represent a very cost-effective method of broad implementation of interventions (Becker et al., 2008). Results provided evidence that intervention effects were significantly larger for three of the five outcomes for clinician-led groups versus researcher-led groups (dieting, negative affect, and eating disorder symptoms). The finding that clinicians produced stronger effects than closely trained and supervised research therapists is encouraging for broader implementation of these programs. Clinicians may have produced superior effects because they have more experience than research clinicians, consistent with one study found that clinicians reported an average of 8 years of experience working with young adults in a professional mental health role, all had experience delivering prevention programs, and 89% had experience conducting group interventions (Stice, Rohde, Durant, et al., 2013).

We tested the hypothesis that dissonance-based eating disorder prevention programs delivered by more facilitators would produce larger intervention effects, based on the assumption that a greater number of facilitators would be able to conduct more effective sessions and adhere to the curriculum more closely. There was evidence that effects were significantly larger when more versus fewer individuals facilitated dissonance-based eating disorder prevention programs for four of the five outcomes (thin-ideal internalization, body dissatisfaction, dieting, and eating disorder symptoms), making this one of the moderators that received the greatest empirical support. However, there were quadratic effects for three of these outcomes that suggested that implementation with 2–3 facilitators might be ideal for producing maximal effects. The implementation implication is that it would be ideal to have 2–3 individuals facilitate dissonance-based eating disorder prevention programs.

We also evaluated the hypothesis that facilitators who received more hours of training in implementing dissonance-based eating disorder prevention programs would produce larger effects than those who received less training. This moderator produced significant effects for four of the five outcomes (thin-ideal internalization, body dissatisfaction, dieting, and eating disorder symptoms), suggesting it is also one of the most critical moderators. There was a quadratic component to this effect for all outcomes, which indicated that the largest effects for these outcomes occurred when training took 5.5 to 7.2 h, suggesting that 6–7 h might be optimal for implementation efforts.

Finally, we tested the hypothesis that providing supervision to facilitators implementing dissonance-based eating disorder prevention programs would be associated with larger intervention effects versus when supervision was not provided. Results indicated that providing supervision was associated with significantly larger effects for four of the five outcomes (thin-ideal internalization, body dissatisfaction, dieting, and negative affect), making this another of the most critical moderators identified in this meta-analysis. Theoretically, supervision based on video-recorded groups allows careful monitoring of both fidelity and competence after the initial training has been completed and provides an opportunity for corrective feedback when indicated, though other types of supervision are also beneficial. Results provide very strong evidence that providing supervision will optimize intervention effects from dissonance-based eating disorder prevention programs.

8.3. Caveats of moderator analyses

First, evidence that a moderator is associated with intervention effect sizes does not establish a causal relation. It will be necessary to experimentally manipulate the moderators in a trial to permit firmer causal inferences regarding the effects of the moderators. Second, many of the conclusions regarding moderators of intervention effects are based on theoretical considerations rather than on direct empirical evidence. For instance, although we posit that selective programs delivered to youth with body image concerns produce large effects because the distress that may characterize high-risk samples leads them to engage more effectively in the program content, most trials did not systematically report attendance, homework completion, or session engagement and participation, which would be useful for confirming this speculation. Third, it is possible that some of the moderator effects are due to chance because the moderator analyses involved numerous inferential tests. However, 34% of the moderator effects were statistically significant, versus the 5% that would be expected based on chance alone. The fact that the number of moderator effects was over 7 times the number that would be expected based on chance suggests that most of the effects are reliable. Moreover, the fact that many of the moderator findings converge with findings from lab-based experiments on factors that maximize dissonance-induction and with results from prior meta-analytic reviews increases the confidence that can be placed in the findings.

9. Conclusions and directions for future research

In sum, this meta-analytic review found that dissonance-based eating disorder prevention programs produced significant reductions in each of the four examined risk factors and in eating disorder symptoms that correspond to small to medium effect sizes relative to control conditions. The average effects for dissonance-based eating disorder prevention programs relative to alternative interventions were smaller, but the most commonly evaluated alternative intervention has itself been shown to reduce eating disorder risk factors and symptoms relative to control conditions and alternative interventions and to reduce eating disorder onset in multiple trials, which suggests this may be a high standard. As noted, no other eating disorder prevention program has been found to produce significantly larger intervention effects than alternative interventions.

The moderator findings suggest that effects should be largest if interventionists implement versions of the dissonance-based eating disorder prevention program that include all dissonance-induction activities contained in the *Body Project* and use the 4- or 6-session versions of the prevention program. Effects should likewise be largest when dissonance-based eating disorder prevention programs are delivered to groups of approximately 10 young women and when sessions are video-recorded, as both factors increase accountability, which increases dissonance-induction. Effects should also be larger when participants complete the prevention programs voluntarily, have body image concerns, and are compensated. Moderator analyses also suggested that effects should be larger if dissonance-based eating disorder prevention programs are implemented to adolescent girls and young adult women, and to more ethnically diverse groups. In addition, intervention effects should be larger when clinicians or peer educators implement groups, there are 2–3 facilitators, and when facilitators received between 6 and 7 h of training and, perhaps most importantly, are supervised.

In terms of future research directions, it would be useful to conduct randomized experiments that manipulate the enumerated moderators to confirm that they are causally related to larger intervention effects. It might be most logical to start with the moderators that received the strongest support in this meta-analysis, which include the number of dissonance-based activities in the prevention program, the number of

facilitators, the number of training hours, and whether supervision is provided. For instance, one trial directly compared the effects of the *Body Project* when implemented by clinicians versus peer educators, providing evidence that peer-led groups produced significantly greater reductions in future onset of eating disorders over 3-year follow-up compared to clinician-led groups (+ +Stice, Rohde, et al., 2017a,Stice, Rohde, et al., 2017b). It would be particularly useful to have experimental support for the hypothesis that combining these moderators should produce larger intervention effects. It would also be helpful if future trials of dissonance-based eating disorder prevention programs systematically report average attendance of the intervention sessions, homework completion, intervention fidelity, therapist competence, and cohesion of groups, as this would allow an investigation of the effects of these variables on outcomes and potentially elucidate the mechanism of effect for the moderators. It might be also be useful to identify the types of young women who do not respond well to dissonance-based eating disorder prevention programs (i.e., who subsequently develop eating disorders despite completing this type of prevention program) so that alternative interventions can be developed for these individuals. Although the evidence-base for dissonance-based eating disorder prevention programs is encouraging, we think it vital to develop additional prevention programs that address distinct risk factors for eating disorders (e.g., psychosocial impairment and negative affect) if we are to realize the goal of reducing the population prevalence of eating disorders, which represent a major mental health problem confronting young women. More broadly, results suggest that it would be potentially useful to develop dissonance-based prevention programs for other health and mental health problems, such as depression and suicidality, substance misuse, and obesity, as dissonance-induction appears to represent a robust method of reducing attitudinal and behavioral risk factors for public health problems.

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Appendix A. Supplementary data

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