

Dissonance and Healthy Weight Eating Disorder Prevention Programs: Long-Term Effects From a Randomized Efficacy Trial

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Adolescent girls with body dissatisfaction ($N = 481$, $SD = 1.4$) were randomized to a dissonance-based thin-ideal internalization reduction program, healthy weight control program, expressive writing control condition, or assessment-only control condition. Dissonance participants showed significantly greater decreases in thin-ideal internalization, body dissatisfaction, negative affect, eating disorder symptoms, and psychosocial impairment and lower risk for eating pathology onset through 2- to 3-year follow-up than did assessment-only controls. Dissonance participants showed greater decreases in thin-ideal internalization, body dissatisfaction, and psychosocial impairment than did expressive writing controls. Healthy weight participants showed greater decreases in thin-ideal internalization, body dissatisfaction, negative affect, eating disorder symptoms, and psychosocial impairment; less increases in weight; and lower risk for eating pathology and obesity onset through 2- to 3-year follow-up than did assessment-only controls. Healthy weight participants showed greater decreases in thin-ideal internalization and weight than did expressive writing controls. Dissonance participants showed a 60% reduction in risk for eating pathology onset, and healthy weight participants showed a 61% reduction in risk for eating pathology onset and a 55% reduction in risk for obesity onset relative to assessment-only controls through 3-year follow-up, implying that the effects are clinically important and enduring.

Keywords: prevention, body dissatisfaction, eating disorders, obesity

Eating disorders are marked by medical complications, functional impairment, and mortality (Wilson, Becker, & Heffernan, 2003). Even subthreshold eating pathology produces impairment, distress, and increased risk for future onset of obesity, depression, substance abuse, health problems, and mortality (Stice & Peterson, 2007). In addition, over half of individuals who present for treatment do not satisfy criteria for a full-threshold eating disorder, but rather show subthreshold pathology (Stice & Peterson, 2007). Because most people with eating disturbances do not seek treatment, and because the 50% recovery rate observed over 5-year follow-up for those who receive the treatment of choice (cognitive behavioral therapy; Fairburn et al., 1995) is virtually identical to the recovery rate that occurs over a 5-year follow-up without treatment (Fairburn, Cooper, Doll, Norman, & O'Connor, 2000),

numerous eating disorder prevention programs have been developed. Our group has investigated two programs for young women (ages 13–29) in a series of four controlled efficacy trials that are subsequently described. The goal of the present report is to investigate the long-term effects of these two programs from the final and largest of these trials (Stice, Shaw, Burton, & Wade, 2006).

The first program is a 3-hr *dissonance intervention*, wherein young women with body image concerns who have internalized the thin ideal voluntarily engage in verbal, written, and behavioral exercises in which they critique this ideal (e.g., voluntarily write essays and conduct role plays that are counterattitudinal). Theoretically these activities result in psychological discomfort that motivates them to reduce internalization of the thin ideal, which decreases body dissatisfaction, dieting, negative affect, and eating disorder symptoms. The second program is a 3-hr *healthy weight intervention* that promotes lasting healthy improvements to dietary intake and exercise as a way of achieving body satisfaction among young women with body image concerns, which putatively decreases risk for unhealthy weight control behaviors that typify eating disorders. In this program participants learn about the determinants of weight and implement a lifestyle change plan involving gradual healthy improvements to their diet and activity level. As these programs target young women with body image concerns who did not meet current criteria for anorexia nervosa, bulimia nervosa, or binge eating disorder, they represent a blend of selective prevention programs, which target at-risk but asymptomatic individuals, and indicated prevention programs, which target

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individuals with subsyndromal symptoms of a disorder (Munoz, Mrazek, & Haggerty, 1996).

In our first trial, young women assigned to the dissonance intervention showed significantly greater decreases in thin-ideal internalization, body dissatisfaction, negative affect, and eating disorder symptoms than did waitlist controls from pretest to posttest (Stice, Mazotti, Weibel, & Agras, 2000). All effects, with the exception of negative affect, remained significant at 1-month follow-up.

Our second trial (Stice, Chase, Stormer, & Appel, 2001) compared the dissonance intervention with an initial version of the healthy weight intervention, which was intended to serve as a placebo control condition because past trials of psychoeducational programs with components promoting healthy weight control behaviors had not produced significant effects on eating pathology (e.g., Killen et al., 1993). Young women randomized to the dissonance intervention showed significantly greater reductions in thin-ideal internalization and body dissatisfaction than did those assigned to the healthy weight intervention through 1-month follow-up, providing some evidence that the former intervention produced effects superior to those of an alternative intervention. However, participants in both conditions showed reductions in dieting, negative affect, and eating disorder symptoms. The reductions in these outcomes in the healthy weight condition implied that we inadvertently developed an efficacious intervention, as most prevention programs evaluated previously had not reduced eating disorder symptoms.

Because the effects for the healthy weight intervention were difficult to interpret, we conducted a third trial, in which young women were randomized to the dissonance program, the initial healthy weight program, or a waitlist control condition (Stice, Trost, & Chase, 2003). Relative to controls, participants in both interventions showed significantly greater reductions in thin-ideal internalization, negative affect, and eating disorder symptoms at posttest, 3-month follow-up, and 6-month follow-up. The effects of the two interventions were similar across outcome measures.

Next, we enhanced the healthy weight intervention by integrating behavioral weight control techniques and persuasion principles (e.g., foot-in-the-door approach and motivational enhancement activities) and began a large efficacy trial in which young women with body image concerns were randomized to the dissonance intervention, healthy weight intervention, expressive writing control condition, or assessment-only control condition. A report on the short-term effects found that, relative to assessment-only controls, dissonance participants showed greater reductions in thin-ideal internalization, body dissatisfaction, dieting, negative affect, and eating disorder symptoms at posttest and 6-month follow-up, and in thin-ideal internalization, dieting, and eating disorder symptoms at 1-year follow-up (Stice, Shaw, Burton, & Wade, 2006). Compared with expressive writing controls, dissonance participants showed greater reductions in thin-ideal internalization, body dissatisfaction, dieting, negative affect, and eating disorder symptoms at posttest; in body dissatisfaction, dieting, negative affect, and eating disorder symptoms at 6-month follow-up; and in dieting at 1-year follow-up. Relative to healthy weight participants, dissonance participants showed greater reductions in thin-ideal internalization, body dissatisfaction, dieting, negative affect, and eating disorder symptoms at posttest, and in negative affect at 6-month and 1-year follow-ups. Compared with assessment-only controls,

healthy weight participants showed greater reductions in thin-ideal internalization, body dissatisfaction, and negative affect at posttest; in thin-ideal internalization, body dissatisfaction, dieting, and eating disorder symptoms at 6-month follow-up; and in thin-ideal internalization, dieting, and eating disorder symptoms at 1-year follow-up. Relative to expressive writing controls, healthy weight participants showed greater reductions in thin-ideal internalization, body dissatisfaction, negative affects, and eating disorder symptoms at posttest; in body dissatisfaction and eating disorder symptoms by 6-month follow-up; and in thin-ideal internalization at 1-year follow-up. Healthy weight participants showed lower risk for eating disorder symptom onset by 1-year follow-up than did participants in the other three conditions. Dissonance and healthy weight participants were at lower risk for obesity onset by 1-year follow-up than were expressive writing and assessment-only controls.

A separate report examined mediators hypothesized to account for the intervention effects of the two interventions in the large efficacy trial (Stice, Presnell, Gau, & Shaw, 2007). The dissonance intervention produced significant reductions in outcomes (body dissatisfaction, dieting, negative affect, eating disorder symptoms) and the mediator (thin-ideal internalization). In addition, change in the mediator predicted, and usually occurred before, change in outcomes, and intervention effects became significantly weaker when change in the mediator was controlled. The healthy weight intervention produced significant reductions in outcomes (body dissatisfaction, negative affect, eating disorder symptoms) and increases in the mediators (healthy eating and physical activity), and change in mediators occurred before change in outcomes. Yet, change in the mediator rarely predicted change in outcomes, and the intervention effects did not become significantly weaker when change in the mediators was controlled. The dissonance intervention had much weaker effects on changes in healthy eating and physical activity than did the healthy weight intervention, and the healthy weight intervention had much weaker effects on thin-ideal internalization than did the dissonance intervention, providing evidence for the specificity of the mediators. Thus, results provided at least partial support for the hypothesized mediators of both interventions.

Five independent labs have provided evidence that the dissonance program produces greater reductions in eating disorder symptoms and risk factors relative to assessment-only control conditions and that it significantly outperformed two alternative interventions (Becker, Smith, & Ciao, 2006; Green, Scott, Diyanakova, Gasser, & Pederson, 2005; Matussek, Wendt, & Wiseman, 2004; Mitchell, Mazzeo, Rausch, & Cooke, 2007; Roehrig, Thompson, Brannick, & van den Berg, 2006). Matussek and colleagues (2004) found that a streamlined version of the healthy weight intervention reduced thin-ideal internalization and eating disorder symptoms. Although not all of the effects replicated, these findings are noteworthy because, to our knowledge, the effects for other eating disorder prevention programs have not been independently replicated.

In sum, these trials have provided evidence that the dissonance and healthy weight interventions have produced significantly greater reductions in risk factors, in eating disorder symptoms, and in risk for obesity onset relative to assessment-only control groups. They have also provided some evidence that these interventions produced significantly stronger effects for many of these outcomes

than did alternative interventions, though these latter effects tend not to persist as long. These results are encouraging given that a recent meta-analytic review (Stice, Shaw, & Marti, 2007) indicated that only 15 of the 66 eating disorder prevention programs (23%) that have been evaluated in controlled trials produced significant reductions in current or future eating disorder symptoms and only 3 of the 66 programs (5%) produced intervention effects for eating pathology that have persisted over at least a 1-year follow-up. In addition to the dissonance and healthy weight interventions, Neumark-Sztainer, Butler, and Palti (1995) found that a 10-hr universal intervention that presented information on healthy weight control, body image, eating disorders and their causes, and skills to resist social pressure to adolescent girls produced significant improvements in eating disorder symptoms at 1-month follow-up and binge eating at 6- and 24-month follow-ups relative to assessment-only controls.

The fact that few prevention programs have included long-term follow-ups reveals that little is known about how long prevention program effects persist. Once effective programs are identified, the next step is to test whether they produce enduring changes. Thus, this report tested whether the effects for the dissonance and healthy weight programs that were observed in our large efficacy trial (Stice, Shaw, Burton, & Wade, 2006) persist through 2- and 3-year follow-ups. We addressed methodological limitations of past trials by using an active control intervention, blinded semistructured diagnostic interviews, a large ethnically diverse sample, and ecologically meaningful outcomes (e.g., risk for obesity onset). We hypothesized that participants in the dissonance and healthy weight interventions would show greater reductions in risk factors (thin-ideal internalization, body dissatisfaction, negative affect), eating disorder symptoms, and psychosocial impairment, and lower risk for eating pathology and obesity onset than would expressive writing and assessment-only controls. Because recent findings indicate that dietary restraint scales are not valid measures of objectively measured short-term or longer-term caloric intake (Bathalon et al., 2000; Stice, Fisher, & Lowe, 2004; Sysko, Walsh, Schebendach, & Wilson, 2005), this outcome was omitted. We focused on adolescent girls between the ages of 14 and 19 because they are at much greater risk for eating disorders than are adolescent boys and the peak period of risk occurs during late adolescence (Lewinsohn, Striegel-Moore, & Seeley, 2000; Wilson et al., 2003).

Method

Participants and Procedure

Participants were 481 adolescent girls (M age = 17.0 years, $SD = 1.4$) with a mean body mass index ($BMI = \text{kg}/\text{m}^2$) of 23.2 ($SD = 4.4$). The sample was 10% Asian/Pacific Islander, 6% Black, 19% Hispanic, 58% Caucasian, and 7% who specified other or mixed racial heritage. Parental education ranged from high school graduate or less (17%) to graduate/professional degree (28%), with a mode of college graduate (31%). The sample was reasonably representative of the populations from which we sampled in terms of BMI ($M = 22.8$, $SD = 5.0$), ethnicity (7% Black, 18% Hispanic, 65% Caucasian), and parental education (34% high school graduate or less; 25% some college; 26% college graduate; 15% graduate degree).

From 2001 to 2003 participants were recruited from high schools and a university (52% and 48% of the sample, respectively) using direct mailings, flyers, and leaflets inviting young women “between the ages of 14 and 19 to participate in a research project evaluating interventions aimed at helping young women to accept their bodies.” At high schools, mailings were sent home to all female students, and flyers were posted in women’s bathrooms. At the university, flyers were posted on bulletin boards, and leaflets were distributed to women outside large introductory classes. Informed written consent was obtained from all participants (and their parents if they were minors) through the mail before data collection. For inclusion, students had to answer affirmatively when asked, “Do you have body image concerns?” during a phone screen. We used self-selection for this prevention trial and did not require a specific response on a body dissatisfaction screen because we wanted to simplify recruitment so that this intervention could be easily disseminated. This approach attracted at-risk young women with average body dissatisfaction and thin-ideal internalization scores that were significantly higher than the average scores from a normative sample (Stice, Shaw, Burton, & Wade, 2006), although 17% of the sample were below the mean normative body dissatisfaction score and 9% were below the mean normative thin-ideal internalization score. The sole exclusion criterion was meeting criteria for *DSM-IV* anorexia nervosa, bulimia nervosa, or binge eating disorder at pretest. The 24 individuals who met criteria for these disorders were encouraged to seek treatment, given referrals, and told that the prevention programs were not appropriate for them. An additional 17 students were excluded, 12 because they did not report body image concerns and 5 because they were too old (see Figure 1 for participant flow-chart).

Participants were randomized to the dissonance intervention, healthy weight intervention, expressive writing control intervention, or assessment-only control condition (using a four-sided die). The dissonance and healthy weight interventions consisted of three weekly 1-hr group sessions with 6–10 participants. With the exception of the initial delivery of each intervention, which was facilitated by Eric Stice for training purposes, one graduate student conducted all of the dissonance groups and another conducted all of the healthy weight groups. Scripted treatment manuals (Stice & Presnell, 2007) were developed. Facilitator training involved three steps: (1) facilitators directly observed Eric Stice deliver the intervention, (2) facilitators then delivered the intervention in the presence of Eric Stice and received supervision, and (3) Eric Stice reviewed audiotapes of the facilitator delivering the intervention alone and provided supervision until he decided that each component of the interventions was delivered with fidelity. Female undergraduates served as cofacilitators for both interventions because clinical experience suggests it is beneficial to have a cofacilitator who is similar in age to participants. Cofacilitators were responsible for distributing handouts, collecting homework, and writing on whiteboards. Cofacilitator training involved two steps: (1) cofacilitators first observed the delivery of the intervention as an ostensive participant, and (2) they then received supervision from the facilitator regarding their performance as a cofacilitator in groups. The expressive writing condition consisted of three weekly 45-min individual writing sessions. We selected this comparison group because we wanted to control for the effects of demand characteristics and expectancies but not nonspecific group factors

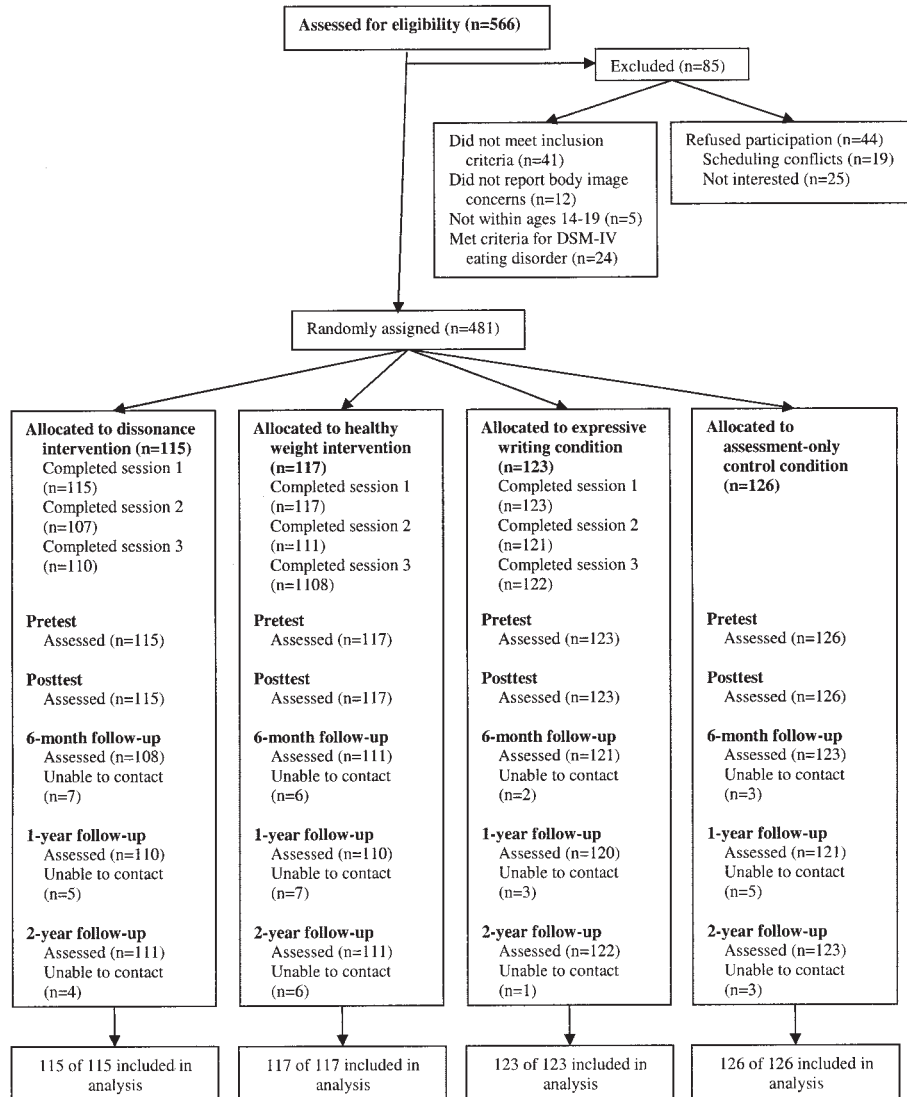


Figure 1. Participant flowchart.

(e.g., social support from group members), as we believe the latter contribute to the beneficial effects of group-based prevention programs such as the dissonance and healthy weight programs.

Participants provided interview and survey data at pretest and posttest, as well as at 6-month, 1-year, 2-year, and 3-year follow-ups after posttest. Female assessors—who had a B.A., M.A., or Ph.D. in psychology—were blinded to the condition of participants. Assessors attended 24 hr of training, wherein they received instruction in interview skills, reviewed diagnostic criteria for eating disorders, observed simulated interviews, and role-played interviews as well as annual training workshops. They had to demonstrate interrater agreement ($\kappa > .80$) with expert raters using 12 tape-recorded interviews conducted with individuals with and without eating disorders before collecting data. They also had to maintain test–retest kappa values of .80 or greater for a randomly selected 5% of the interviews that were reconducted by the same assessor and to maintain interrater kappa values of .80 or greater

for a randomly selected 5% of the interviews that were reconducted with independent assessors during the study. Assessments were conducted on an individual basis at schools, project offices, or participants' homes. To retain participants in this longitudinal study we paid them \$25 for completing each assessment, offered a \$25 bonus for completing all assessments, collected updated participant addresses and contact information data every 6 months, collected the names and phone numbers of three individuals who would “always know how to contact you,” and mailed newsletters about the project, holiday cards, and birthday cards. The University of Texas Institutional Review Board approved this project.

Dissonance intervention. In this intervention, participants voluntarily engaged in verbal, written, and behavioral exercises in which they critiqued the thin-ideal ideal. These exercises were conducted in sessions and in homework activities. For example, they wrote a counterattitudinal essay about the costs associated with pursuit of the thin ideal and engaged in a counterattitudinal

role play in which they attempted to dissuade facilitators from pursuing the thin ideal.

Healthy weight intervention. In this intervention, participants were encouraged to make gradual healthy and lasting changes to their diet and physical activity, to balance their energy needs with their energy intake and thereby achieve a healthier weight and body satisfaction. With support from the facilitator and group members, they initiated an individual lifestyle change plan to reduce intake of fat and sugar and to increase exercise using behavioral modification principles. Food and exercise diaries were used to identify behaviors to target in this lifestyle modification and to monitor change. Motivational enhancement activities were used to promote motivation for behavior change.

Expressive writing control intervention. In this condition, which is based on the work of Pennebaker (1997), participants wrote about emotionally significant topics in three individual weekly 45-min sessions. They were told that research indicates that body dissatisfaction is linked to emotional issues and that expressive writing helps resolve these issues. Sample topics included relationships or goals. They were told that their work would not be read and were asked to write continuously for the duration of the session about an emotionally important topic.

Assessment-only control condition. Participants in this condition received no intervention but were referred to treatment if they met criteria for anorexia nervosa, bulimia nervosa, or binge eating disorder at any of the follow-up assessments (as were participants in all conditions).

Measures

Thin-ideal internalization. The Ideal-Body Stereotype Scale—Revised assessed thin-ideal internalization (Stice, Shaw, Burton, & Wade, 2006). Items used a response format ranging from 1 = *strongly disagree* to 5 = *strongly agree*. Items were averaged for this scale and those described below (scale range: 1–5). This scale has shown internal consistency ($\alpha = .91$), test-retest reliability ($r = .80$), and predictive validity for bulimic symptom onset (Stice, Shaw, Burton, & Wade, 2006).

Body dissatisfaction. Eight items from the Satisfaction and Dissatisfaction With Body Parts Scale (Berscheid, Walster, & Bohrnstedt, 1973) assessed satisfaction with body parts that are often of concern to women (e.g., stomach, thighs, and hips). Response options range from 1 = *extremely satisfied* to 6 = *extremely dissatisfied* (scale range: 1–6). This scale has shown internal consistency ($\alpha = .94$), 3-week test-retest reliability ($r = .90$), and predictive validity for bulimic symptom onset (Stice, Shaw, Burton, & Wade, 2006).

Negative affect. Negative affect was assessed with the Sadness, Guilt, and Fear/Anxiety subscales from the Positive Affect and Negative Affect Scale—Revised (PANAS-X; Watson & Clark, 1992). Participants reported the extent to which they had felt various negative emotional states on scales ranging from 1 = *very slightly or not at all* to 5 = *extremely* (scale range: 1–5). This scale has shown internal consistency ($\alpha = .95$), 3-week test-retest reliability ($r = .78$), convergent validity, and predictive validity for bulimic symptom onset (Stice, Shaw, Burton, & Wade, 2006).

Eating pathology. The Eating Disorder Diagnostic Interview, a semistructured interview that was adapted from the Eating Disorder Examination (Fairburn et al., 1995), assessed *DSM-IV* eating

disorder symptoms. Items assessing the symptoms in the past month were summed to create an overall eating disorder symptom composite for each assessment (observed scale range: 0–87), as done previously (Presnell & Stice, 2003; Stice, Burton, & Shaw, 2004). A square root transformation was used to normalize this composite.

We also tested whether the interventions reduced risk for onset of *DSM-IV* anorexia nervosa, bulimia nervosa, and binge eating disorder, as well as subthreshold variants of these disorders among those free of these conditions at pretest. For a subthreshold anorexia nervosa diagnosis we required participants to (a) have a BMI of between 90% and 85% of that expected for age and gender (as opposed to less than 85% of that expected for age and gender for a full-threshold diagnosis), (b) report a definite fear of weight gain, and (c) report that weight and shape was definitely an aspect of self-evaluation. For subthreshold bulimia nervosa we required participants to report at least six uncontrollable binge eating episodes and six compensatory behavior episodes over a 3-month period (an average of twice monthly for each, as opposed to twice weekly for a full-threshold diagnosis) and to report that weight and shape was definitely an aspect of self-evaluation. For subthreshold binge eating disorder we required that participants report (a) at least 12 uncontrollable binge eating episodes/days over a 6-month period (as opposed to 48 for a full-threshold diagnosis), (b) fewer than six compensatory behavior episodes, (c) marked distress about binge eating, and (d) binge eating that was characterized by three or more of the following: rapid eating; eating until uncomfortably full; eating large amounts when not physically hungry; and eating alone because of feeling embarrassed, disgusted, depressed, or guilty after overeating. Such subthreshold eating disorders would receive a diagnosis of eating disorder not otherwise specified according to *DSM-IV* criteria.

The symptom composite showed internal consistency ($\alpha = .92$), 1-week test-retest reliability ($r = .90$), sensitivity to detecting intervention effects, and predictive validity for future onset of depression in past studies (Presnell & Stice, 2003; Stice, Burton, & Shaw, 2004). In the current trial, the symptom composite showed internal consistency ($\alpha = .86$ at pretest) and 1-month test-retest reliability for assessment-only controls ($r = .81$). To assess the test-retest reliability for eating disorder diagnoses for this adapted interview, each of the assessors who interviewed a randomly selected subset of 137 participants for this study and another ongoing study (Stice, Burton, & Shaw, 2004) reinterviewed the same participants within a 1-week period, resulting in high test-retest reliability for threshold and subthreshold diagnoses of anorexia nervosa, bulimia nervosa, and binge eating disorder ($\kappa = .96$). To assess the interrater agreement for these threshold and subthreshold eating disorder diagnoses, blinded assessors interviewed a randomly selected subset of 149 participants who had been interviewed by different assessors for this study and the other ongoing study, resulting in high interrater agreement ($\kappa = .86$).

Obesity. BMI was used to reflect height-adjusted weight (Pietrobelli et al., 1998), to accommodate for the fact that many participants increased in height during the 3-year follow-up. After removal of shoes and coats, height was measured to the nearest millimeter using stadiometers, and weight was assessed to the nearest 0.1 kg using digital scales. Two measures of each were obtained and averaged in an effort to ensure reliable measurement. BMI correlates with direct measures of body fat such as dual

energy X-ray absorptiometry ($r = .80-.90$) and health measures such as blood pressure, adverse lipoprotein profiles, and diabetes mellitus (Pietrobelli et al., 1998). Following Barlow and Dietz (1998), adolescent obesity was defined using the 95th centiles of body mass index for age and gender based on nationally representative data from the National Health and Nutrition Examination Survey I. We used age-adjusted obesity cutoff points to accommodate the fact that participants were still growing during the 3-year follow-up.

Psychosocial impairment. Items from the Social Adjustment Scale (Weissman & Bothwell, 1976) assessed psychosocial functioning in the family, peer group, school, and work spheres. Response formats ranged from 1 = *never* to 5 = *always* (scale range: 1–5). The original scale has shown convergent validity with clinician and collateral ratings ($mean r = .72$), discriminates between controls and psychiatric patients, and is sensitive to treatment effects (Weissman & Bothwell, 1976). The adapted version of this scale has shown internal consistency ($\alpha = .77$), 1-week test–retest reliability ($r = .83$), and sensitivity to detecting intervention effects (Burton & Stice, 2006; Stice, Shaw, Burton, & Wade, 2006).

Data Analytic Plan

Preliminary analyses tested whether the groups differed at pretest on all study variables, to determine whether randomization failed to create equivalent groups on all variables and whether it was necessary to control for any variables that were confounded with condition. Analyses also tested for differences in ancillary care, session attendance, and homework completion across conditions (to assess potential confounds); examined intervention fidelity; tested for attrition biases; and tested for equivalence in intervention credibility. Finally, analyses investigated the clinical significance of eating pathology, as operationalized in this report.

To assess intervention effects for the continuous outcomes at 2- and 3-year follow-ups, we conducted mixed models analyses (in SAS PROC MIXED) that included all five waves of postintervention data as dependent variables. Independent variables included condition, wave, Condition \times Wave interaction, pretest level of the outcome, pretest of the Outcome \times Wave interaction, posttest to 1-year slope of the outcome, and Posttest to 1-Year Slope \times Wave interaction. The posttest to 1-year slope was derived from random coefficients models that regressed each outcome on time measured in months since treatment and generated individuals' slope parameters, which thus represents the direction and magnitude of change in the 1st year after the intervention. There were five contrasts that tested for the hypothesized effects at both 2- and 3-year follow-ups in each model: dissonance versus assessment only, dissonance versus expressive writing, healthy weight versus assessment only, healthy weight versus expressive writing, and dissonance versus healthy weight. These were examined with estimated marginal means and standard errors that were adjusted for the covariates in the model. We controlled for baseline versions of the outcomes and for change in the outcomes from posttest to 1-year follow-up in an effort to ensure that differences in the marginal means at 2- and 3-year follow-ups were a result of the interventions rather than these covariates. For the continuous outcomes, we report standardized mean difference effect size (Cohen's d) based on the estimated marginal means, which thus

represent the mean differences adjusted for the effects of the covariates. Cohen's (1988) criteria for small ($d = 0.2$), medium ($d = 0.5$), and large ($d = 0.8$) effects were used.

The two dichotomous outcomes—risk for onset of threshold or subthreshold eating pathology and onset of obesity through 3-year follow-up—were modeled with Cox proportional hazard models (in SAS PHREG) because these models were designed to test for differential risk for dichotomous outcomes across conditions as well as to handle right-censored data of the type we collected in this trial. Participants who met criteria for full- or subthreshold anorexia nervosa, binge eating disorder, or binge eating disorder or obesity at pretest were excluded from analysis (respectively) because the focus was on onset that occurred after the interventions. The same five contrasts that were examined for continuous variables were also examined in the proportional hazard models. Testing these five contrasts required three models in order to dummy-code the contrasts of interest. The first model contained assessment-only, dissonance, and healthy weight participants with dummy variables for dissonance and healthy weight; the second model contained expressive writing, dissonance, and healthy weight participants, with dummy variables for dissonance and healthy weight; and the third model contained dissonance and healthy weight participants, with a dummy variable for dissonance. For the dichotomous outcomes we report the hazard ratio (HR) effect size, which reflects the increase in risk for onset in one condition versus the other condition over the 3-year follow-up (it is conceptually similar to an odds ratio). On the basis of a conversion of Cohen's d to an odds ratio (Lipsey & Wilson, 2001), HRs of 1.44, 2.48, and 4.28 represent small, medium, and large effects, respectively.

Results

Preliminary Analyses

Analyses verified that the groups did not differ significantly in terms of demographic factors, any of the study variables at pretest, or ancillary treatment (e.g., psychotherapy, pharmacotherapy) for any type of psychiatric disturbance (range: 15%–26%), suggesting that none of these factors were potential confounds that needed to be included as covariates. We also confirmed that the results did not differ when we controlled for whether participants received any ancillary psychiatric treatment during the 3-year follow-up; all significant effects remained significant.

Participants rated how much they expected to benefit from the three interventions at the end of the first session (response options were 1 = *not at all*, 2 = *somewhat*, 3 = *moderately*, 4 = *a lot*, and 5 = *extremely*). The dissonance condition ($M = 3.4$, $SD = 0.86$) did not differ significantly from the healthy weight condition ($M = 3.6$, $SD = 0.89$) or the expressive writing condition ($M = 3.1$, $SD = 0.91$) in perceived credibility, but the expressive writing condition was perceived as significantly less likely to be beneficial than the healthy weight intervention ($p < .05$, $r = .26$).

Among dissonance participants, 91% attended all three sessions, 6% attended only two sessions, and 3% attended only one session. Parallel attendance rates were 91%, 6%, and 3% for the healthy weight intervention and 98%, 1%, and 1% for the expressive writing condition. Session attendance did not differ across conditions. The proportion of participants who completed the homework

assignments was not significantly different in the dissonance (80%) versus the healthy weight (87%) condition.

A random sample of sessions (10%) were audiotaped and coded by research assistants who were blinded to intervention condition to determine whether the facilitators covered the main exercises listed in the relevant treatment manual and did not cover the main exercises listed in the other treatment manual (each session contains four to six main exercises). Results indicated that group leaders covered 100% of the main exercises from the relevant manual and included 0% of exercises from the other manual. We considered this approach sufficient to assess fidelity because we used highly scripted manuals for these brief interventions and because the two interventions were based on very distinct conceptual rationales.

The 5% of participants who did not complete the assessments through the 3-year follow-up did not differ from the remaining 95% of participants on any demographic factors or study variables at pretest, and attrition did not differ across conditions, suggesting that attrition was not systematic. Nonetheless, we used expectation maximization in a single imputation to fill in missing data points using the SPSS Missing Value module with available data for all the variables examined in this report. This data set was used in all analyses reported herein. Given the minimal amount of missing data, this approach was deemed preferable to a more complex multiple imputation approach that involves considerably more inferential tests (Schafer & Graham, 2002).

Over the course of the 3-year follow-up, 3 participants showed subthreshold anorexia nervosa onset, 1 showed bulimia nervosa onset, 23 showed subthreshold bulimia nervosa onset, 1 showed binge eating disorder onset, and 12 showed subthreshold binge eating disorder onset. Those who showed onset of anorexic pathology had a mean BMI of 17.2 ($SD = 0.69$). Those who showed onset of bulimic pathology had a mean number of binge episodes of 75 ($SD = 90.7$) and a mean number of compensatory behaviors of 30 ($SD = 21.3$) over the 3-year follow-up. Those who showed onset of binge eating pathology had a mean number of binge episodes of 83 ($SD = 91.0$) over the 3-year follow-up. The 36 participants who experienced onset of eating pathology during the follow-up reported significantly greater mental health care utilization at 2-year, $t(958) = -3.78, p < .0001$, and 3-year, $t(958) =$

$-3.36, p = .0004$, follow-ups and significantly greater psychosocial impairment at 2-year, $t(958) = -3.99, p < .0001$, and 3-year, $t(958) = -5.31, p < .0001$, follow-ups than did participants who remained free of an eating disorder. In addition, the 36 individuals who experienced onset of eating pathology during the 3-year follow-up reported significantly more functional impairment, $t(67) = 1.60, p = .03$, and mental health care utilization, $t(67) = 2.33, p = .006$, during this episode than was the case before they showed onset. The participants who showed onset of eating pathology reported seeing mental health providers an average of 11 times over the follow-up (range: 0–63; only 30% reported not seeing a provider). Over the 3-year follow-up, a total of 56 participants (14%) showed onset of obesity.

Intervention Effects for Continuous Outcomes

Table 1 reports the d effect sizes for analyses testing for differences in marginal means at 2- and 3-year follow-ups controlling for covariates, which included pretest values of the outcome and effects at posttest, 6-month follow-up, and 1-year follow-up. Table 2 provides means and SD s across conditions at pretest and 2- and 3-year follow-ups. Relative to assessment-only controls, dissonance participants showed significantly lower thin-ideal internalization, body dissatisfaction, negative affect, bulimic symptoms, and psychosocial impairment at 2-year follow-up and significantly lower body dissatisfaction, negative affect, and psychosocial impairment at 3-year follow-up. Relative to expressive writing controls, dissonance participants showed significantly lower thin-ideal internalization and body dissatisfaction at 2-year follow-up and significantly lower body dissatisfaction and psychosocial impairment at 3-year follow-up. Dissonance participants also showed significantly greater reductions in psychosocial impairment than did healthy weight participants by 3-year follow-up.

Compared with assessment-only controls, healthy weight participants showed significantly lower thin-ideal internalization, body dissatisfaction, and negative affect at 2-year follow-up and significantly lower thin-ideal internalization, body dissatisfaction, negative affect, and bulimic symptoms as well as significantly less increases in BMI at 3-year follow-up. Relative to expressive writing controls, healthy weight participants showed significantly

Table 1

Effect Sizes (d) and Significance Levels for the Tests of Marginal Means for the Continuous Outcomes at 2- and 3-Year Follow-ups

Variable	Thin-ideal internalization	Body dissatisfaction	Negative affect	Bulimic symptoms	Body mass index	Psychosocial impairment
Pretest to 2-year follow-up						
Dissonance vs. assessment only	.35***	.28***	.16*	.19*	-.00	.20**
Dissonance vs. expressive writing	.20*	.18*	.01	.15	.00	.08
Dissonance vs. healthy weight	-.01	.11	-.03	.17	-.02	.08
Healthy weight vs. assessment only	.37***	.16*	.19*	.06	.01	.13
Healthy weight vs. expressive writing	.22*	.05	.04	-.00	.02	-.00
Pretest to 3-year follow-up						
Dissonance vs. assessment only	.13	.43***	.17*	.07	-.01	.19**
Dissonance vs. expressive writing	.10	.32***	.01	-.04	.02	.17*
Dissonance vs. healthy weight	-.10	.14	.01	-.09	-.11*	.18*
Healthy weight vs. assessment only	.23*	.28*	.16*	.17*	.08*	.03
Healthy weight vs. expressive writing	.23*	.17	.00	.05	.10**	-.02

* $p < .05$. ** $p < .01$. *** $p < .001$.

Table 2
Means (SD) for Outcomes From the Four Conditions and Results From the Within-Condition Pairwise Contrasts

Variable	Pretest	2-yr follow-up	3-yr follow-up
	<i>M (SD)</i>	<i>M (SD)</i>	<i>M (SD)</i>
Thin-ideal internalization			
Dissonance	3.76 (0.49)	3.53 (0.52)	3.59 (0.50)
Healthy weight	3.72 (0.53)	3.43 (0.65)	3.44 (0.65)
Expressive writing	3.75 (0.53)	3.59 (0.61)	3.58 (0.49)
Assessment only	3.72 (0.50)	3.66 (0.55)	3.61 (0.61)
Body dissatisfaction			
Dissonance	3.57 (0.81)	3.10 (0.76)	3.06 (0.87)
Healthy weight	3.47 (0.81)	3.10 (0.92)	3.08 (0.89)
Expressive writing	3.44 (0.82)	3.10 (0.87)	3.17 (0.79)
Assessment only	3.53 (0.88)	3.29 (0.94)	3.36 (0.84)
Negative affect			
Dissonance	2.22 (0.88)	1.96 (0.84)	1.92 (0.76)
Healthy weight	2.11 (0.81)	1.88 (0.83)	1.87 (0.79)
Expressive writing	2.15 (0.94)	1.85 (0.82)	1.83 (0.73)
Assessment only	2.22 (0.92)	2.04 (0.84)	2.02 (0.86)
Bulimic symptoms			
Dissonance	17.42 (16.49)	10.05 (10.48)	11.09 (17.99)
Healthy weight	14.67 (12.85)	9.61 (14.27)	8.62 (8.87)
Expressive writing	14.08 (11.50)	9.76 (9.64)	8.76 (9.60)
Assessment only	15.55 (15.08)	12.38 (12.64)	10.85 (14.34)
Body mass index			
Dissonance	23.04 (3.83)	23.36 (4.08)	23.60 (4.95)
Healthy weight	23.09 (4.12)	23.39 (4.54)	23.24 (4.32)
Expressive writing	23.02 (4.37)	23.46 (4.69)	23.78 (5.50)
Assessment only	23.69 (4.80)	24.17 (4.96)	24.37 (4.99)
Psychosocial impairment			
Dissonance	2.61 (0.49)	2.38 (0.40)	2.32 (0.43)
Healthy weight	2.54 (0.40)	2.36 (0.38)	2.35 (0.40)
Expressive writing	2.59 (0.46)	2.37 (0.36)	2.36 (0.37)
Assessment only	2.59 (0.44)	2.45 (0.43)	2.40 (0.42)

Note. The untransformed versions of the bulimic symptom measures are presented in the table.

lower thin-ideal internalization at 2-year follow-up and significantly lower thin-ideal internalization and less increases in BMI at 3-year follow-up. Healthy weight participants also showed significantly less increases in BMI than did dissonance participants by 3-year follow-up.

Intervention Effects for Dichotomous Outcomes

Prior to analysis, the proportional hazard assumption was assessed using the supremum test (Lin, Wei, & Ying, 1993). The survival functions for bulimia nervosa onset are graphed in Figure 2a, and the survival functions for obesity onset are graphed in Figure 2b. Dissonance participants showed significantly lower risk of eating pathology onset over the 3-year follow-up relative to assessment-only controls (Wald = 4.01 $p = .022$, HR = 2.50). Healthy weight participants showed significantly lower risk for eating pathology onset than did assessment-only controls (Wald = 4.39, $p = .018$, HR = 2.75) and significantly lower risk of obesity onset than did assessment-only controls (Wald = 4.05, $p = .024$; HR = 2.27) over the 3-year follow-up.

Summary of Intervention Effects

Relative to the assessment-only control intervention, the dissonance intervention produced significantly greater reductions in

three risk factors (thin-ideal internalization, body dissatisfaction, and negative affect), initial eating disorder symptoms, and psychosocial impairment by 2- to 3-year follow-up, as well as reduced risk for onset of eating pathology through 3-year follow-up. Dissonance participants also showed greater decreases in thin-ideal internalization, body dissatisfaction, and psychosocial impairment than did expressive writing controls. Relative to the assessment-only control intervention, the healthy weight intervention produced greater reductions in three risk factors (thin-ideal internalization, body dissatisfaction, and negative affect) and initial eating disorder symptoms by 2- to 3-year follow-up, as well as reduced risk for future weight gain and onset of eating pathology and obesity onset through 3-year follow-up. Healthy weight participants showed greater decreases in thin-ideal internalization and body mass than did expressive writing controls. There was also evidence that the healthy weight intervention produced significantly less weight gain than did the dissonance intervention by 3-year follow-up and that the dissonance intervention produced larger improvements in psychosocial impairment than did the healthy weight intervention. The significant effects for the continuous outcomes were small to medium in magnitude (mean $d = 0.21$). The significant effects for the dichotomous outcomes were medium in magnitude (mean HR = 2.51). The effects for the remaining hypotheses were not statistically significant.

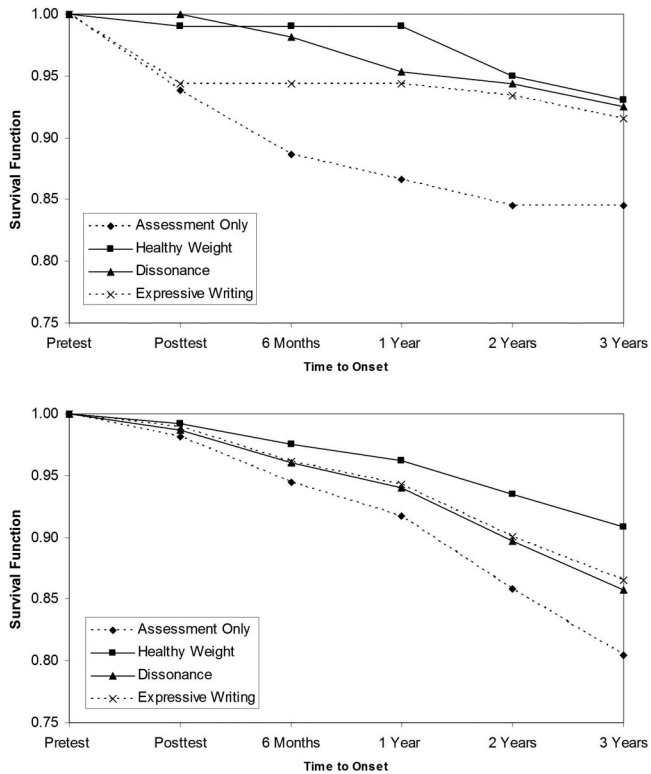


Figure 2. Survival function by condition for eating disorder onset (a) and obesity onset (b).

Discussion

Most importantly, participants in the dissonance intervention showed a significantly lower risk for onset of clinically significant eating pathology relative to assessment-only controls (6% versus 15%), which amounts to a 60% reduction in the number of expected cases that would have theoretically emerged in this high-risk sample in the absence of the intervention. These results suggest that for every 100 young women who complete this intervention, approximately 9 fewer should show onset of eating pathology. It was also noteworthy that healthy weight participants showed a statistically significant 61% reduction in risk for future onset of eating pathology relative to assessment-only controls, which suggests that for every 100 young women who complete the program, approximately 9 fewer should show onset of this eating disturbance. An examination of the rates of onset for the three different eating disorders revealed that the effects were primarily driven by differential risk for onset of bulimic pathology, which was 3% for dissonance participants, 5% for healthy weight participants, 6% for expressive writing controls, and 12% for assessment-only controls (suggesting that the dissonance intervention produced a 75% reduction in risk for bulimic pathology onset relative to the assessment-only control intervention). Although it will be vital to replicate these effects in an independent trial, these findings are encouraging because to our knowledge, no other eating disorder prevention program has been shown to reduce risk for future onset of clinically significant eating pathology. In addition, these appear to be the first eating disorder prevention pro-

grams to produce effects that persist through 3-year follow-up, though we acknowledge that most prevention programs that have produced effects for eating pathology have not been evaluated in trials with a long-term follow-up; thus it is possible that other programs may produce long-term effects.

Of equal importance was the evidence that participants in the healthy weight intervention showed a 55% reduction in risk for obesity onset and less increases in BMI relative to assessment-only controls over the 3-year follow-up, because a recent meta-analytic review suggested that no obesity prevention program has produced effects that persist this long (Stice, Shaw, & Marti, 2006). Again, however, because most obesity prevention trials have not included long-term follow-ups, some of the weight-gain prevention effects that have been observed from pretest to posttest in other trials may persist over longer-term follow-up. The fact that the two prevention programs evaluated in this trial have produced significant intervention effects for both eating disorder symptoms and weight gain (Stice, Shaw, Burton, & Wade, 2006) is noteworthy because few programs have produced effects for both of these important public health problems (Stice, Shaw, & Marti, 2007). Prevention programs that produce effects for more than one physical or psychiatric problem have greater public health utility and cost effectiveness than do those that produce effects for only one problem.

Interestingly, although the effects that reflect reductions in initial symptoms and risk factors were greatest at earlier follow-ups, the prophylactic effects were stronger at the latter follow-ups (e.g., reduced risk for eating pathology onset). This pattern of findings probably emerged because the incidence of eating pathology and obesity were greatest at the latter follow-ups. These results imply that it is important to conduct longer-term follow-up in prevention trials to fully characterize the effects of prevention programs. The fact that the average follow-up in eating disorder prevention trials is only 4 months (Stice, Shaw, & Marti, 2007) suggests this is an important limitation to address.

Another encouraging aspect of the findings is that results provided further evidence that each intervention significantly outperformed an active alternative intervention. The dissonance program produced significantly stronger effects on thin-ideal internalization, body dissatisfaction, and psychosocial impairment than did the expressive writing intervention; the healthy weight program produced significantly stronger effects on thin-ideal internalization and increases in BMI than did the expressive writing control program. There was even evidence that the healthy weight intervention was significantly more effective than the dissonance intervention at slowing the rate of weight gain, and that the dissonance intervention was significantly more effective than the healthy weight intervention in reducing functional impairment.

It was also striking that this high-risk sample of young women with self-identified body image concerns showed such high rates of onset of eating pathology (15%) and obesity (17%) in the assessment-only condition over the 3-year follow-up. The fact that new cases of eating pathology continued to emerge at 3-year follow-up (at which point participants had a mean age of 20 years) implies that the peak period of risk for onset of eating pathology extends into young adulthood for this high-risk population, in contrast to findings from studies of normative samples (e.g., Lewinsohn et al., 2000). This also implies that it would be important to extend the follow-up periods of future eating disorder

prevention trials to capture this peak period of risk. In addition, these results underscore the importance of delivering a prevention program to this high-risk population.

However, there were also discouraging aspects of the results. First, many of the effects that were observed at posttest did not persist through follow-up. For instance, for the continuous outcomes 73% of the effects were significant at posttest, but only 47% were significant at 3-year follow-up. Although this might not be surprising given that these interventions were only 3 hr in duration (plus the time to complete the homework), this pattern of findings suggests that a priority for future trials will be to investigate methods of bolstering the persistence of intervention effects. Second, the effects that were observed were somewhat more likely to emerge when the interventions were compared with the assessment-only control condition rather than with the expressive writing placebo control condition. However, even the current treatments of choice for certain disorders—such as cognitive-behavior therapies for bulimia nervosa, binge eating disorder, and depression—typically do not significantly outperform alternative interventions over long-term follow-ups, although they often do in the short term (Brent et al., 1997; Fairburn et al., 1995; Willfley et al., 2002). Because these same preventive and treatment interventions produce greater reductions than are observed in assessment-only control groups, this pattern of findings may suggest that if individuals engage in *any* type of intervention, they may be more willing to try alternative methods of resolving the problem behavior (though there was no evidence of differential ancillary treatment across conditions in our trial) or that expectancies contribute more to long-term than short-term effects. Third, although important prophylactic effects emerged, they were limited in number. This pattern of findings, and the more general evidence that prevention programs rarely produce prophylactic effects (Horowitz & Garber, 2006; Stice, Shaw, & Marti, 2006; Stice, Shaw, & Marti, 2007), suggests that another important priority for future prevention trials will be to determine how to better reduce risk for future onset and escalation in eating disorder symptoms.

The findings from this report and the previous report on the short-term effects of these interventions (Stice, Shaw, Burton, & Wade, 2006) suggest that these two programs have somewhat different strengths. Relative to healthy weight participants, dissonance participants showed significantly greater reductions in thin-ideal internalization, body dissatisfaction, dieting, negative affect, and eating disorder symptoms at posttest; in negative affect at 6-month and 1-year follow-ups; and in psychosocial impairment by 3-year follow-up. This is noteworthy because few eating disorder prevention programs have been shown to outperform structurally equivalent alternative prevention programs, though other trials have provided evidence that the dissonance intervention outperformed alternative prevention programs (Becker et al., 2006; Green et al., 2005). In contrast, relative to dissonance participants, healthy weight participants showed significantly greater reduction in risk for obesity onset by 1-year follow-up and significantly less weight gain through 3-year follow-up. There were no other instances wherein either intervention produced significantly stronger effects than did the other intervention. Overall, this pattern of findings suggests that the dissonance intervention produces stronger effects for eating pathology and risk factors, whereas the healthy weight intervention produces stronger weight gain prevention effects.

It is noteworthy that both interventions produced effects for eating pathology and weight gain that persisted over long-term follow-up, because the two programs have such different content. One potential explanation is that they both make use of strategic self-presentation (Cialdini & Goldstein, 2004) to promote a reduction in maladaptive attitudes and behaviors; in the dissonance program participants engage in activities in which they voluntarily and publicly criticize the thin ideal to reduce thin-ideal internalization, and in the healthy weight program participants publicly commit to making healthy improvements to their dietary intake and physical activity to promote lasting healthy lifestyle changes. Theoretically, people who make public commitments to change behavior are more likely to enact these changes because of the increased accountability and a desire to be consistent in their actions. Interestingly, the one other prevention program that produced effects for eating pathology that persisted through long-term follow-up (Neumark-Sztainer et al., 1995) also used role-play exercises involving strategic self-presentation (refusing pressure to be thin). In addition, both interventions make use of motivational enhancement techniques (Miller, 1983); in the dissonance program participants discuss the costs of pursuing the thin ideal, and in the healthy weight intervention participants discuss the costs of obesity and the benefits of maintaining a healthy weight. Theoretically, these exercises increase the likelihood that participants will engage in the program and make changes to their maladaptive attitudes and behaviors because the exercises emphasize individual responsibility and promote internal attribution for attitudinal and behavioral change. In line with this interpretation, dismantling studies indicate that the dissonance-induction procedures used in the dissonance program contribute to the observed effects of this intervention (Green et al., 2005; Roehrig et al., 2006). In addition, our meta-analytic review of eating disorder prevention programs (Stice, Shaw, & Marti, 2007) suggested that other prevention programs that have not produced intervention effects for eating pathology that persist through follow-up typically have not included strategic self-presentation and motivational enhancement exercises. Future research should consider ways of increasing the use of strategic self-presentation and motivational enhancement components to these and other prevention programs, in an effort to produce larger and more persistent intervention effects.

Within this context, it should be noted that the healthy weight intervention also uses the foot-in-the-door technique (Cialdini & Goldstein, 2004), wherein a small request for behavior change that is easy to make is followed by a request for a related larger and more difficult behavior change. Theoretically, people are more inclined to acquiesce to the larger request when it is made after completion of the smaller request because they feel motivated to maintain behavioral consistency. Specifically, we ask participants to make minor behavioral changes regarding diet and activity level in the first session, but then in subsequent sessions request additional and more substantial health behavior changes. It might be fruitful to explore using this persuasion principle in eating disorder prevention programs to enhance the effects.

Because this trial required contrasts between four conditions, involved multiple follow-up points, and included a broad array of outcomes, it was necessary to perform numerous inferential tests to investigate all the hypotheses and fully characterize the intervention effects. Thus, there is a possibility that some of the findings reported here are false positive findings (Type I errors). Although

it is important to consider this issue, two considerations suggest that the findings are valid. First, the number of significant effects reported here (40%) is eight times higher than the 5% that would be expected based on chance. Second, the fact that these two interventions produced effects for many of these outcomes across three studies that we conducted (e.g., Stice et al., 2000, 2003) and at least 10 trials that have been conducted by seven independent research groups (e.g., Becker et al., 2006; Green et al., 2005; Matussek et al., 2004; Mitchell et al., 2007; Roehrig et al., 2006) leads us to believe that these effects are not simply false positive findings. Nonetheless, results should be interpreted with some caution.

Limitations

Although we improved upon many prior prevention trials by using random assignment, an active control condition, blinded diagnostic interviews, a large and ethnically diverse sample, and ecologically valid outcomes, the current study had several limitations. First, we relied on self-report data, with the exception of the direct measures of height and weight, which introduces the possibility of reporter bias. It might be useful for future prevention trials to collect multiple-informant data and objective biological data. Second, we used a simple approach to assess intervention fidelity; future trials that compare multiple interventions should employ skilled clinicians as raters and should assess the finer details of these interventions. Third, these blended selective-targeted prevention programs were delivered to a relatively small portion of high-risk adolescent girls from the larger population from which we sampled (e.g., typically about 7% of female students in a given school). Future trials should explore the approach of targeting multiple high-risk populations (e.g., those with elevated body dissatisfaction, dieting, or negative affect), as this may increase the reach of selective prevention programs. Within this context, it is important to note that prior trials have suggested that the dissonance intervention produces effects for unselected populations (e.g., Becker et al., 2006; Green et al., 2005).

Implications for Prevention and Future Research

The present findings imply it will be vital to determine ways to enhance the magnitude and duration of the effects of these two prevention programs. Future trials should test whether effects would be improved by increasing the number and duration of intervention sessions or using in-person, mail-based, or Internet-based booster sessions; adjunctive interventions (e.g., an Internet-based support group); or an intervention component targeting parents. We also recommend evaluating whether an increased use of persuasion principles from social and clinical psychology—including strategic self-presentation, motivational enhancement exercises, and the foot-in-the-door approach—represents a method of enhancing effects of the interventions. The findings from Green and colleagues (2005) suggest that making the in-session and home exercises more effortful, increasing accountability (e.g., by videotaping sessions), and increasing the perception that participation is voluntary may increase the effects for the dissonance program, as these are processes that increase dissonance induction. In addition, future research should investigate whether the dissonance and healthy weight interventions can be integrated to pro-

duce even stronger effects. Researchers should also consider adapting components from other eating disorder prevention programs that produced promising intervention effects (e.g., Neumark-Sztainer et al., 1995).

More generally, it will be important for future prevention trials to use longer-term follow-ups to better understand the persistence of intervention effects, as most trials have not included long-term follow-ups. Furthermore, additional efforts should be devoted to designing interventions that affect multiple physical and mental health problems because this would improve the public health impact of prevention efforts. One promising approach might be to target general risk factors that predict onset of several pathological conditions, rather than risk factors that are specific to only one condition. It might also be useful to test whether prevention programs that effectively reduce a pathological behavior (e.g., binge eating) also reduce other public health problems arising from that behavior (e.g., obesity and depression). Furthermore, it will be vital to conduct effectiveness trials to determine whether these interventions will produce effects in the real world (e.g., among all high schools in a district) when delivered by endogenous providers (e.g., school counselors). To date, virtually all prevention programs have been evaluated in efficacy trials that test whether the interventions are effective when delivered under optimal conditions by highly trained and supervised experts. Finally, researchers should initiate dissemination studies to (a) determine how best to train large numbers of providers, (b) determine whether these providers can deliver the interventions with fidelity and competence, and (c) identify barriers to the successful implementation of these interventions on a large-scale basis. We believe that additional programmatic and rigorous research will bring us closer to realizing the goal of reducing the overall prevalence of eating disorders through prevention.

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