



Moderators of two indicated cognitive-behavioral depression prevention approaches for adolescents in a school-based effectiveness trial



Frédéric N. Brière^{a,b,*}, Paul Rohde^c, Heather Shaw^c, Eric Stice^c

^a Université de Montréal, 90 Vincent d'Indy Avenue, H2V 2S9 Québec, Canada

^b School Environment Research Group, 7101 ave du Parc, H3N 1X7 Québec, Canada

^c Oregon Research Institute, 1776 Millrace Dr., 97403 Oregon, USA

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ABSTRACT

Objective: Our aim was to identify moderators of the effects of a cognitive behavioral group-based prevention program (CB group) and CB bibliotherapy, relative to an educational brochure control condition and to one another, in a school-based effectiveness randomized controlled prevention trial.

Method: 378 adolescents (*M* age = 15.5, 68% female) with elevated depressive symptoms were randomized in one of three conditions and were assessed at pretest, posttest, and 6-month follow-up. We tested the moderating effect of three individual (baseline depressive symptoms, negative attributional style, substance use), three environmental (negative life events, parental support, peer support), and two sociodemographic (sex, age) characteristics.

Results: Baseline depressive symptoms interacted with condition and time. Decomposition indicated that elevated baseline depressive symptoms amplified the effect of CB bibliotherapy at posttest (but not 6-month follow-up) relative to the control condition, but did not modify the effect of CB group relative to the control condition or relative to bibliotherapy. Specifically, CB bibliotherapy resulted in lower posttest depressive symptoms than the control condition in individuals with elevated, but not average or low baseline symptoms. We found no interaction effect for other putative moderators.

Conclusions: Our findings suggest that bibliotherapy is effective only in participants who have elevated depressive symptoms at baseline. The fact that no study variable moderated the effects of CB group, which had a significant main effect in reducing depressive symptoms relative to the control condition, suggests that this indicated prevention intervention is effective for a wide range of adolescents.

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Major depressive disorder (MDD) in adolescents is a common and recurrent condition that predicts several long-term psychosocial impairments, including academic underachievement, early pregnancy, and dysfunctional interpersonal relationships and work performance (Lewinsohn, Rohde, Seeley, Klein, & Gotlib, 2003a). Several prevention interventions have been developed to reduce the risk for adolescent MDD. Selective and indicated cognitive-behavioral (CB) depression prevention programs have been found

to be efficacious, but overall have shown moderate (and sometimes inconsistent) effects that typically do not persist over long-term follow-up (Horowitz & Garber, 2006; Stice, Shaw, Bohon, Marti, & Rohde, 2009).

To date, relatively few studies have investigated factors that moderate the effects of depression prevention programs for adolescents and adults. Understanding intervention moderators is important for several reasons: (a) it helps to identify individuals most likely to benefit from the intervention, which should guide inclusion criteria; (b) it can determine individuals unlikely to benefit from the prevention program, for whom alternative interventions are needed; and (c) it helps to refine intervention theory for the programs by revealing variables predictive of greatest efficacy (Kazdin & Weisz, 1998; Lockman, 2001). On the other hand, knowing that a prevention program is similarly effective for a wide variety of individuals would imply that it can be easily and broadly disseminated. Limited information also exists

Abbreviations: CB, cognitive-behavioral; MDD, major depressive disorder; CES-D, Center for Epidemiologic Studies-Depression; K-SADS, schedule for affective disorders and schizophrenia for school-age children; ICC, intra-class correlation; ACSQ, adolescent cognitive style questionnaire; NRI, Network of Relationship Inventory; SD, standard deviation.

* Corresponding author. Université de Montréal, C.P. 6128, succursale Centre-ville, Montréal, Québec H3C 3J7, Canada. Tel.: +1 514 452 0332.

E-mail address: frederic.nault-briere@umontreal.ca (F.N. Brière).

regarding prevention interventions delivered in real-life contexts as opposed to tightly-controlled research settings (Rohde, Stice, Shaw, & Brière, 2013). Testing interventions in natural environments as part of effectiveness trials is an essential step to wide-scale dissemination, and schools have been recognized as a key setting for delivering these types of prevention programs with young people (Weist et al., 2003).

The present study examines moderators of the effects of two indicated CB depression prevention interventions, a brief group intervention and an individual bibliotherapy intervention, relative to an educational brochure control condition, evaluated in the context of a school-based effectiveness trial (Rohde et al., 2013). This effectiveness trial followed an earlier efficacy trial (Stice, Rohde, Seeley, & Gau, 2008) and investigated whether there would be similar preventive effects of CB group and bibliotherapy when intervention recruitment and delivery were conducted by school personnel rather than research staff. Results of the acute phase showed that CB group was successful at reducing the risk of MDD onset by 6-months follow-up and mean depressive symptom severity at posttest compared to brochure control, but that bibliotherapy did not affect either outcome (Rohde et al., 2013). The present report extends these earlier findings by examining moderators of CB group and bibliotherapy effects relative to brochure control, as well as relative to one another. Other novel contributions of this study beyond the effectiveness design include the exploration of moderators for CB group vs. CB bibliotherapy, in addition to comparisons of CB interventions vs. minimal control, and the examination of moderation effects at two post-intervention timepoints.

We examined three individual risk factors as putative moderators. Based on findings from other CB programs (Jaycox, Reivich, Gillham, & Seligman, 1994) and two meta-analytic reviews (Horowitz & Garber, 2006; Stice et al., 2009), we hypothesized that effects of the two CB interventions would be larger for adolescents with higher baseline depressive symptoms. Elevated symptoms can provide greater motivation for change and facilitate the learning of intervention skills as they can be applied to address current symptoms. Second, we hypothesized that participants entering a CB intervention with a more negative attributional style would most benefit from the intervention relative to controls, given that the two CB approaches directly target such cognitions. Negative attributional style has been found to moderate program outcomes in adolescent depression treatment studies (e.g., Curry et al., 2006). A recent report that used Classification Tree Analysis found that attributional style was the strongest risk factor of depression onset and revealed that CB bibliotherapy produced significantly stronger reductions in depressive symptoms for youth with a negative attributional style relative to participants in CB group, supportive expressive therapy, or an assessment-only control condition (Rohde, Stice, & Gau, 2012), suggesting a moderation-type effect. Third, we hypothesized that substance use could reduce program effects by maintaining or exacerbating depressive symptoms (Brook, Brook, Zhang, Cohen, & Whiteman, 2002; Rohde, Lewinsohn, Kahler, Seeley, & Brown, 2001) or by reducing participant motivation to engage in the intervention. Consistent with this hypothesis, substance use reduced the effects of CB group on depressive symptoms relative to brochure control in our previous efficacy trial (Gau, Stice, Rohde, & Seeley, 2012).

We also examined three environmental risk factors as putative moderators. We hypothesized that CB interventions would be less effective at reducing depressive symptoms in youth who experience more negative life events. Major negative life circumstances constitute a major risk factor of depression (Lewinsohn et al., 1994) and significantly mitigated the effects of a CB group prevention program in our efficacy trial (Gau et al., 2012), possibly because

negative life events are difficult to alter directly in a prevention program. We also hypothesized that social support deficits would reduce the effects of the interventions. Low support might limit the capacity of participants to benefit from some components of the program (e.g., increase social pleasant activities) and could reflect social skills deficits that inhibit participation and rapport with other group members and the facilitators, especially given that social skills training was not part of the short CB program. Parental and peer support did not have a moderating effect in our efficacy trial when the two were considered jointly (Gau et al., 2012). In the present study, we examined perceived parent and peer support separately because the two types of support are theoretically distinct, have been shown to have different predictive effects (Lewinsohn et al., 1994; Stice, Ragan, & Randall, 2004), and program components may relate specifically to one type of support but not the other (e.g., CB group may directly address deficits in peer support because groups are conducted with peers, but may have a limited impact on parent support). To our knowledge, no study has explored the moderating effects of parental and peer support separately in a CB depression prevention trial.

Finally, we examined two sociodemographic factors as potential moderators. We hypothesized that CB effects would be larger for females than males, based on previous meta-analytic results (Horowitz & Garber, 2006; Stice et al., 2009) and because female adolescents report greater depressive symptoms and higher rates of MDD than male adolescents (Hankin et al., 1998; Lewinsohn et al., 1994), which can make it easier to detect prevention effects. We also hypothesized that CB interventions would produce larger effects for older compared to younger adolescents because the acquisition of cognitive-behavioral skills may become easier as adolescents mature cognitively. Previous trials tended to show better CB program effects in older adolescents (Horowitz & Garber, 2006; Stice et al., 2009).

Our examination of intervention moderators was devised to shed light on several important issues (Bloom & Michalopoulos, 2013). One issue is whether interventions are more effective in some subgroups than other. In the case of CB bibliotherapy, we were particularly interested in finding subgroups that may benefit from the intervention despite the fact we found no effect in the full sample (Rohde et al., 2013). In the case of CB group prevention, we were interested in finding subgroups that were more or less responsive to the intervention to complement our understanding of its general effectiveness and to inform inclusion and exclusion criteria. Alternatively, finding few or no moderators for CB group relative to the control condition would suggest that the main effect for this intervention applies to a wide variety of individuals, which would imply that this depression prevention program could be widely and easily disseminated.

Method

Participants

Participants were 378 high school students aged 13 to 19 ($M = 15.5$; $SD = 1.2$; 68% female). Participants were 72% Caucasian, 6% Hispanic, 2% Asian-American, 1% African-American, 1% Native American, and 18% other or mixed heritage. Most participants had at least one parent who had some college education (26% some college; 22% college graduate; 13% graduate degree). The sample was generally representative of the county from which it was sampled in terms of race/ethnicity and parental education (Rohde et al., 2013). About a third of participants (37%) had received treatment for mental health problems in the year before the study. Consistent with the logic of an effectiveness trial, these participants were not excluded from the trial.

Procedures

Participants were recruited from five high schools representing the majority of area high schools. Consistent with the aims of effectiveness research, school staff recruited participants. School-wide direct mailings and recruitment posters invited potential students to take part in a trial evaluating interventions aimed at helping adolescents reduce sadness and promoting emotional well-being. Letters included a one-page screener of depressive symptoms using the Center for Epidemiologic Studies–Depression Scale (CES-D; Radloff, 1977) with a simplified 2-point response choice (i.e. rarely/a little vs. occasionally/most of the time). Students were encouraged to participate if they endorsed two or more items. We did not collect information on the screener as it was completed before participants provided informed consent.

A total of 421 potential participants completed a pretest assessment with a research assistant. Of this group, 43 participants were excluded due to a current MDD diagnosis or acute suicidal ideation (these participants were encouraged to seek treatment and were told that the prevention programs evaluated in this trial were not sufficient). No other exclusion criteria were used to maximize ecological validity. The 378 eligible participants were randomly assigned within blocks created by gender and school in one of three conditions: (1) CB group ($n = 126$), (2) CB bibliotherapy ($n = 128$), or (3) educational brochure control ($n = 124$). Participants completed a survey and diagnostic interview at pretest, posttest, and 6-month follow-up. Participants received \$25 for completing each assessment. Assessors were research assistants who were blind to condition, had at least a BA in psychology, received approximately 40 h of training in conducting semi-structured interviews, and were required to show a minimum κ agreement of .80 with expert raters before starting data collection. Assessments were conducted at schools. The Institutional Review Board of the Oregon Research Institute approved this study.

Intervention groups

CB group depression prevention. The CB group program consisted of 6 weekly 1-h sessions in single-gender groups of 5–9 participants ($M = 6.0$). Groups were conducted at schools. The intervention was analogous to the one evaluated in our previous efficacy research (Stice et al., 2008), but utilized a more scripted intervention manual. Each session had a portion devoted to thought identification/recording and cognitive restructuring (e.g., generating more accurate and positive counter-thoughts to replace inaccurate and negative thoughts using strategies such as “What’s the Alternative?” or “Where’s the Evidence?”) and a portion devoted to increased involvement in pleasant activities (e.g., generating a personal list of fun activities, especially physical and social, and rewards for engaging in these activities). Motivational enhancement, behavioral techniques, and group activities were used to support the acquisition of skills. Sessions were facilitated by two school personnel (e.g., school counselors or nurses; $n = 13$, 12 women), except for 3 groups led by solo facilitators due to staff availability. A brief (10–15 min) individual make-up session was conducted when participants missed a session. Facilitator training included reading relevant articles and attending a one-day workshop that included a review of the intervention components and practice delivery of the sessions. To assess implementation and provide supervision to facilitators, sessions were audiotaped and 50% were randomly selected for review by Drs. Rohde and Shaw. Tapes were rated based on 10-point rating scales of facilitator competence and adherence. Mean adherence was 7.0 ($SD = .7$) and mean competence was 7.1 ($SD = .7$), indicating that all key concepts of the various session sections were presented with good or very

good therapist competence. Only 1.4% of items were rated as “minimal adherence” and 1.9% as “minimal competence” (Rohde et al., 2013).

CB bibliotherapy. CB bibliotherapy consisted of being encouraged to read the book *Feeling Good: the New Mood Therapy* (Burns, 1980). This book teaches relevant and practical CB techniques for reducing negative mood and covers topics such as understanding feelings of sadness, building self-esteem, overcoming guilt and helplessness, and coping with stress. This book is written at a high-school reading level. Participants were told, “This book has been shown to be helpful to some individuals who are feeling sad or depressed. This copy is yours to keep, so feel free to write or highlight in it as you read. We encourage you to use this as a self-help resource.” We asked the school personnel to make two brief scripted reminder phone calls to CB bibliotherapy participants encouraging them to read the book and complete exercises contained in the book (e.g., BDI, quiz on CBT skills, 5-column cognitive restructuring technique). Participants were asked at follow-up assessments how much of the book they had read, but we did not track usage of material contained in the book.

Brochure-only control condition. The educational brochure control condition consisted of giving participants an NIMH brochure (“Let’s Talk About Depression” NIH Pub. 01-4162). This brochure describes MDD, recommends treatment for depressed youth, and provides information about local treatment options. We selected this control condition because it corresponded to what most local schools were providing to prevent depression.

Measures

Depressive symptoms. Depressive symptoms were assessed at pretest and posttest using sixteen items adapted from the Schedule for Affective Disorders and Schizophrenia for School-Age Children (K-SADS; Puig-Antich & Chambers, 1983). Trained research assistants conducted interviews. Participants indicated the peak severity of each symptom over the past 12 months at pretest or since the last interview at posttest using a 4-point response format (1 = *not at all* to 4 = *severe symptoms*). Ratings of 3 and 4 were indicative of a diagnostic level symptom. Items were averaged to form a continuous measure reflecting the severity of depressive symptoms. This measure has shown good internal consistency ($\alpha = .68-.84$), predictive validity, and inter-item correlation ($ICC = .99$) (Stice, Rohde, Gau, & Wade, 2010).

Substance use. Substance use was measured using a 10-item scale (Stice, Barrera, & Chassin, 1998). Participants were asked to report the frequency of past 6-month intake of beer/wine/wine coolers and hard liquor, frequency of heavy drinking (5 or more drinks in a row), frequency of times drunk, and frequency of marijuana, stimulants, downers, inhalants, and hallucinogen use. Items were rated on a 6-point response scale ranging from *never* to 3–7 *times a week*. This scale has shown adequate internal consistency at pretest ($\alpha = .79$), 1-year test-retest reliability ($r = .72$), and predictive validity for substance abuse symptoms (Stice et al., 2010).

Negative life events. Negative life events were assessed by asking participants to report the occurrence of 14 negative life events during the past year (Lewinsohn et al., 1994). This scale has demonstrated internal consistency ($\alpha = .78$; Lewinsohn et al., 1994) and predictive validity (Monroe, Rohde, Seeley, & Lewinsohn, 1999). Pilot testing ($N = 30$) revealed high 1-week test-retest reliability ($r = .90$).

Negative attributional style. We assessed negative attributional style using the Adolescent Cognitive Style Questionnaire (ACSQ; Hankin & Abramson, 2002). The ACSQ consists of 12 hypothetical negative event scenarios relevant to adolescents. Participants were presented with hypothetical negative events and

were asked to rate (a) the degree to which the cause of the negative event is internal, stable, and global (negative inferences for causal attributions), (b) the likelihood that further negative consequences will result from the negative event (negative inferences for consequences), and (c) the degree to which the negative event signifies that the person is fundamentally flawed (negative inference for self). Hankin and Abramson (2002) reported high internal consistency for the ACSQ ($\alpha = .95$). This 12-item short form of the ACSQ has shown high internal consistency ($\alpha = .82$) and 1-week test-retest reliability ($r = .89$), and correlates with BDI scores ($r = .36$). Internal consistency in the present study was $\alpha = .85$.

Perceived peer support/perceived parental support. Perceived peer support and perceived parental support were obtained using separate six-item scales derived from the Network of Relationships Inventory (NRI; Furman & Buhrmester, 1985; Stice et al., 2004). This questionnaire assessed various aspects of relationships (companionship, guidance, intimacy, etc.) adolescents have with parents and peers. This version of the NRI has shown high internal consistency ($\alpha = .88$), test-retest reliability ($r = .69$), and predictive validity for future increases in depressive symptoms (Burton, Stice, & Seeley, 2004; Furman, 1996; Stice & Bearman, 2001). Internal consistency for the peer and parental subscales was $\alpha = .88$ and $\alpha = .89$ respectively.

Data analysis

To evaluate moderation, we used repeated measures analyses of covariance (ANCOVA) using outcomes at post-test and follow-up as the dependent variable and baseline scores as the covariate. We examined omnibus F-tests ($\alpha = .05$) for Moderator \times Condition \times Time, as well as Moderator \times Condition interactions. Continuous moderators were centered in all analyses (Aiken & West, 1991). Compared to evaluating a large number of contrasts between conditions at specific time points (e.g., CB group vs. control condition at posttest), this omnibus strategy reduced the number of potential tests (14 omnibus interactions instead of 72 pairwise contrasts) and family-wise error rate, and consequently risk of type I error. We did not apply additional correction strategies to avoid overcompensating and reducing statistical power (Bloom & Michalopoulos, 2013).

Given a significant omnibus interaction, we first identified the specific groups (CB group vs. control; bibliotherapy vs. control; CB group vs. bibliotherapy) to which the moderation effect applied by testing post hoc interactions involving pairwise contrasts between the three conditions, paired with a Bonferroni correction to adjust for non-orthogonality (p values = $.05/3 = .017$). We then evaluated simple slopes at low, average, and elevated values of the moderator (1 standard deviation below average [$-1SD$], average, 1 standard deviation above average [$+1SD$]) to compare the relevant conditions (Aiken & West, 1991). We also plotted mean values of depressive symptoms for all conditions based on these moderator values ($-1SD$, average, $+1SD$). We report effect sizes as Cohen's d (Cohen, 1988), which we obtained by dividing the difference in estimated marginal means by the pooled standard deviation. Using this metric, intervention effects can be interpreted as small $\geq .20$, medium $\geq .50$, and large $\geq .80$ (Cohen, 1988). We used the following coding in analyses with sex as a predictor or moderator: 1 = male; 0 = female.

Attrition for diagnostic interview data was 5% at posttest and 13% at 6-month follow-up, with no differential attrition across conditions or for any of baseline study variables. To maximize statistical power and reduce potential missing data biases, we used multiple imputation (Graham, 2009). The "fully conditional specification" in SPSS 19.0 was used to produce 20 imputed datasets. The imputation model included all study variables, including interaction terms. Parameters and statistical tests were averaged over the 20 datasets using Rubin's rules (Rubin, 1987). We conducted

Table 1
Means, standard deviations, and correlations for main study variables.

	Mean (SD) or No (%) ^a	Correlations ^b						
		1	2	3	4	5	6	7
1. Pretest depressive symptoms	1.40 (.38)							
2. Sex (1 = male)	122 (32)	-.03						
3. Age	15.52 (1.22)	.08	-.04					
4. Negative life events	4.05 (2.84)	.14	.05	.01				
5. Substance use	.31 (.56)	.13	-.02	.15	.28			
6. Negative attributional style	3.47 (1.10)	.32	-.04	.10	.15	.16		
7. Perceived peer support	3.88 (.87)	-.13	-.10	-.08	-.07	.04	-.08	
8. Perceived parental support	3.65 (.94)	-.29	.05	.03	-.15	-.18	-.33	.40

Note: Correlations in bold are statistically significant at the $\alpha = p < .05$ level.

^a Based on the unimputed dataset.

^b Correlations are averages across 20 imputed datasets.

analyses using SPSS MIXED (and SAS proc MIXED and proc MIANALYZE to obtain omnibus F-tests specifically). A key advantage of MIXED procedures was its ability to test different variance-covariance structures and select the best-fitting structure to accurately estimate fixed effects. In preliminary analyses, we compared various variance-covariance structures (compound symmetry or unstructured, with or without heterogeneity between the three conditions). Based on considerations of parsimony and fit on the AIC criterion, we modeled ANCOVAs with heterogeneous variances over time, but homogeneity between conditions.

Results

Preliminary analyses

Table 1 presents the means, SDs, and correlations for study variables. Baseline depressive symptoms correlated with all variables, except sex. Participants from the three intervention conditions did not differ on any of the study variables at pretest.

We examined the main effects of CB group and bibliotherapy at posttest and 6-month follow-up in a previous report (Rohde et al., 2013). Analyses showed that CB group resulted in lower depressive symptoms than brochure control at posttest ($p = .03$, $d = .29$) but not 6-month follow-up, and marginally lower symptoms than CB bibliotherapy at posttest ($p = .06$, $d = .27$), but not at 6-month follow-up. We extend these findings by examining potential moderators of effects at posttest and 6-month follow-up.

Moderation analyses

Results of Moderator \times Condition and Moderator \times Condition \times Time interaction effects on depressive symptoms at posttest and 6-month follow-up are presented in Table 2. We found a significant baseline depressive symptoms \times condition \times time interaction (Fig. 1). To probe this interaction, we first examined specific post hoc interactions involving pairwise contrasts between the three conditions. This examination indicated that baseline depressive symptoms and time moderated the effect of bibliotherapy relative to the control condition ($t = 2.46$, $p = .014$), but not CB group relative to the control condition ($t = .29$, $p = .77$) or bibliotherapy relative to CB group ($t = 2.10$, $p = .04$).¹ We next

¹ Simple slopes for this marginal effect suggested that CB group resulted in lower posttest symptoms than bibliotherapy in participants with low baseline symptoms ($B = -.18$, $p = .004$, $d = .59$) but not average ($B = -.08$, $p = .08$, $d = .26$) or elevated baseline symptoms ($B = .02$, $p = .36$, $d = .07$).

Table 2
Moderator × Condition × Time and Moderator × Condition effects on depressive symptoms at posttest and 6-month follow-up.

	Depressive symptoms (posttest, 6-month follow-up)			
	Moderator × condition × time		Moderator × condition	
	F	p	F	p
Pretest depressive symptoms	3.69	.03	—	—
Age	1.98	.14	2.27	.10
Sex	.43	.65	.91	.40
Negative attributional style	.95	.39	1.82	.16
Substance use	.33	.72	1.98	.14
Negative life events	.08	.92	1.19	.30
Perceived peer support	.18	.84	2.03	.13
Perceived parental support	.26	.77	.23	.80

tested simple slopes to compare bibliotherapy and the control condition at low (−1SD), average, and elevated (+1SD) values of baseline depressive symptoms at posttest and 6-month follow-up. At posttest, bibliotherapy resulted in lower depressive symptoms than the control condition in participants with elevated (+1SD) baseline symptoms ($B = -.02, p = .08, d = .07$), but not average or low (−1SD) baseline symptoms ($B = .10, p = .13, d = .31$). At 6-month follow-up, bibliotherapy did not differ from the control condition at either low, average, or elevated baseline symptoms.

As seen in Table 2, we did not find significant interaction effects with Condition or Condition × Time for age, sex, negative attributional style, substance use, negative life events, peer support, and parental support. We therefore did not examine *post hoc* contrasts for these moderators.

Finally, we conducted ancillary analyses to test the robustness of findings. First, to evaluate the impact of missing data corrections on study results, we conducted sensitivity analyses comparing unimputed and imputed results. Results between the two sets of analyses did not differ in the pattern of significant results. Second, we re-analyzed the moderating effect of baseline depressive symptoms adjusting for all other interaction terms, as well as the fixed effect of school to control for the nested study design (models with random effects for school did not converge, presumably due to the small number of schools). We found a similar interaction between baseline depressive symptoms, condition, and time ($F(2, 2768) = 4.81, p = .01$). Using a robust “sandwich” estimator as an alternative to adjust for clustering also did not modify results. Third, we tested whether our results were robust to various aspects

of model specification (e.g., combinations of variance-covariance matrices). Results did not substantively vary with modeling strategy.

Discussion

The aim of this study was to identify factors that moderate the effects of two CB depression prevention interventions, a brief group program (CB group) and an individual minimal contact book intervention (bibliotherapy), relative to an educational brochure control and to one another, in a school-based effectiveness randomized trial. We examined three individual (baseline symptoms, negative attributional style, substance use), three environmental (negative life events, parent support, peer support), and two sociodemographic (sex, age) putative moderators. Our design allowed us to extend previous literature by 1) testing moderators in the “real-life” context of an effectiveness trial, 2) comparing two validated interventions together, as well as with a minimal treatment-as-usual condition (i.e. educational brochure control), and 3) examining effects over two post-intervention timepoints (posttest, 6-month follow-up).

We found a single moderation effect involving baseline depressive symptoms, intervention condition, and time. Probing of this interaction revealed that baseline depressive symptoms moderated the effect of bibliotherapy relative to the control condition, but did not moderate the effect of CB group relative to either the control condition or bibliotherapy. Consistent with our hypothesis, CB bibliotherapy resulted in lower depressive symptoms

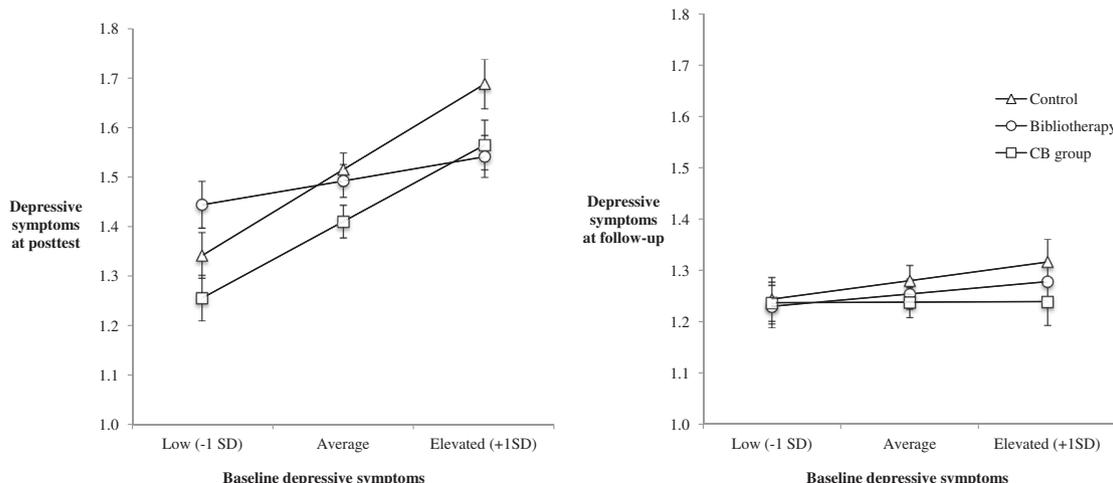


Fig. 1. Mean depressive symptoms at posttest and 6-month follow-up in CB group, bibliotherapy, and control participants with low, average, and elevated baseline symptoms.

at posttest than the control condition in participants with elevated (but not low or average) baseline depressive symptoms. An interpretation of this finding is that bibliotherapy requires elevated levels of current symptoms to be effective because active symptoms generate greater impetus for change and facilitate the concrete application of intervention skills. Alternatively, a statistical explanation is that elevated baseline symptoms may have facilitated the detection of symptom improvements in CB bibliotherapy participants by limiting “floor effects.” Importantly, this result may help to explain why CB bibliotherapy successfully reduced depressive symptoms in the efficacy trial (Stice et al., 2008), but not this effectiveness trial. The fact that participants in the present trial were on average less depressed than those in the prior efficacy trial (Rohde et al., 2013) may have contributed to attenuating the main effect of bibliotherapy, as this intervention appears to be beneficial specifically in adolescents who have elevated baseline symptoms. Based on the K-SADS symptom scale used in this study (1 = not at all, 2 = mild, 3 = moderate, 4 = severe), bibliotherapy appears to require an average baseline symptom severity of at least mild (or close to mild) to be effective.

An interesting novel contribution of this study was to examine the timing and maintenance of the moderating effect of baseline depressive symptoms. The moderating effect of baseline symptoms on the effect of bibliotherapy (relative to the control condition) was found at posttest, but was not maintained at 6-month follow-up. This mirrors the pattern for the main effect of CB group prevention on reductions in depressive symptoms, which was also found at posttest, but not 6-month follow-up (Rohde et al., 2013). Our earlier report revealed that CB group appeared to prevent the escalation of depressive symptoms that occurred in brochure controls from pretest to posttest, but that depressive symptoms tended to decline in participants from all three conditions from posttest to 6-month follow-up. Although statistical “floor effects” related to limited magnitude and variance in follow-up depressive scores may have contributed to both findings, a more substantive explanation may lie in the episodic nature of depression. CB interventions may accelerate recovery from periods of subclinical depressive symptoms (elevated specifically in the case of bibliotherapy), but may not have discernible effects in the long run given that most cases of elevated symptoms are likely to remit spontaneously over a few months (Birmaher et al., 2007).

We did not find evidence of moderating effects for any of the seven other factors that we investigated. The lack of support for our hypotheses was unexpected in the case of moderators that have been identified in previous meta-analyses (i.e. sex, age) (Horowitz & Garber, 2006; Stice et al., 2009) and our efficacy trial (i.e. substance use, negative life events) (Gau et al., 2012), although replicating findings is a recognized challenge of moderator research (Supplee, Kelly, MacKinnon, & Barofsky, 2013). Several factors may explain why our findings differ from previous studies. First, our study focused on a single trial rather than multiple trials, which may have restricted the variance of some moderators. Second, results from single trials are impacted by sample selection factors and interventions that are unique to that particular study. Consistent with this second suggestion, results from other single trials regarding sex have also been inconsistent, with studies reporting stronger effects in females (Gillham, Hamilton, Freres, Patton, & Gallop, 2006; Jaycox et al., 1994; Petersen, Leffert, Graham, Alwin, & Ding, 1997), stronger effects in males (Clarke, Hawkins, Murphy, & Sheeber, 1993; Seligman, Schulman, DeRubeis, & Hollon, 1999) or no moderation by sex (Gillham, Shatté, & Reivich, 2001; Gillham et al., 2012). Third, and perhaps most importantly, the sample of this effectiveness trial was characterized by relatively low levels of initial depressive symptoms (Rohde et al., 2013). This complicates comparisons with previous selective and indicated prevention

trials and may have precluded the detection of interaction effects, notably because of the restricted range of putative moderators (Farrell, Henry, & Bettencourt, 2013).

Notwithstanding differences with previous studies, the fact that we found no moderation effect for CB group is noteworthy. One key question of moderator research concerns the scope of effects for an intervention, i.e. whether effects are dispersed across different types of sample members as opposed to concentrated within a homogeneous subgroup (Bloom & Michalopoulos, 2013). The pattern of findings in the present study suggests that the preventive effects of CB group found in our acute phase analyses (Rohde et al., 2013) may pertain to a relatively wide range of individual, environmental, and sociodemographic participant characteristics at baseline. Presumably, this could reflect the fact that CB group facilitators were able to adapt the intervention to each participant, the program content was presented in way that is relevant to many adolescents or the group format provided nonspecific therapeutic factors. Given that the sample evaluated in this effectiveness was self-selected and free of the stringent selection criteria used to ensure homogeneity in efficacy trials, our findings add to the notion that brief CB group prevention interventions may be recommended to a broad range of at-risk adolescents in typical school contexts, which would facilitate dissemination.

Several limitations of this study should be acknowledged. First, given the low incidence of MDD, we focused on depressive symptoms reductions as opposed to onset of major depression. It is unclear whether our results would generalize to MDD prevention. Second, all examined moderators were obtained via self-reported questionnaires and participants may not have accurately reported all variables due to self-presentation biases. For instance, self-reported negative life events may result in over-reporting compared to interview-based approaches (Lewinsohn, Rohde, & Gau, 2003b). Third, we did not have enough minority participants to examine whether race or ethnicity moderated intervention effects, although a prior report from a depression prevention efficacy trial (conducted in a larger, more ethnically diverse metropolitan area) found similar effects for Asian American, Latino, and European American adolescents (Marchand, Ng, Rohde, & Stice, 2010). Fourth, we were unable to account for the nested design using a school random effect. However, considering that randomization was executed independently of school clusters and that adjusting for a fixed effect of school and a “sandwich” estimator did not modify the significance of moderating effects, clustering can be assumed have a limited influence on estimates of moderating effects (Proschan & Follmann, 2008). Lastly, we conducted multiple moderation tests, which raises the possibility that some of the findings were due to chance (Farrell et al., 2013; Supplee et al., 2013). Our approach to limit the number of potential tests was to examine omnibus moderation tests rather than specific comparisons between conditions and time points, but not to apply additional alpha corrections to avoid over-compensating and reducing statistical power. This approach was based on recent guidelines (Bloom & Michalopoulos, 2013) and was chosen to strike a reasonable balance between risks of Type I and II errors. Further, several elements support the robustness of our interaction finding: 1) the interaction between baseline depressive symptoms, condition, and time remained even after adjusting for all other simultaneous covariates and interactions effects; 2) listwise and multiple imputation results were substantively identical; and most importantly, 3) this interaction effect was consistent with our initial expectation and previous results (Bloom & Michalopoulos, 2013; Farrell et al., 2013; Supplee et al., 2013). Importantly, we should also emphasize that our failure to identify moderators of brief CB group prevention does not necessarily mean that moderators do not exist. For instance, specific moderation effects may have been

undetected by our omnibus testing strategy and moderators that were not examined in this study may have an important contribution.

Additional studies are still required to determine whether any moderators exert consistent and replicable effects across trials evaluating adolescent depression prevention interventions. More effectiveness, as opposed to efficacy, trials may result in samples that have more diverse baseline features, facilitating efforts to detect the presence or absence of clinically important moderators. Future research may also explore how adolescent preference for, or interest in, particular approaches moderate the effects of prevention programs. Designs that compare multiple active conditions, like the one used here, would be most appropriate to achieve this aim.

Several clinical implications may be derived from our findings. Consistent with the objectives of personalized medicine (Cuijpers, 2009) and the increased attention devoted to the use of evidence to inform policy decision-making (Supplee et al., 2013), this study – paired with our previous study reporting results of acute phase main effects (Rohde et al., 2013) – provides information that may contribute to tailoring prevention strategies based on participant characteristics at screening. With regards to bibliotherapy, our findings suggest that reading a CB prevention book generally does not outperform a simple educational brochure procedure in a typical school context (Rohde et al., 2013), but that it potentially has added benefits for the subgroup of adolescents who have higher pre-existing depressive symptom levels. However, given that the effect of bibliotherapy in the full sample was not significant, this finding should probably be qualified as exploratory pending confirmatory replication (Bloom & Michalopoulos, 2013). With regard to the CB group prevention intervention, our acute phase study demonstrated that our brief group program significantly prevented an increase in depressive symptoms at posttest that occurred for those in educational brochure control condition (Rohde et al., 2013). The present study complements this result by showing that the preventive effect of this program does not appear to be moderated by the individual, environmental, and socio-demographic characteristics that were examined at baseline. Continued follow-up and research with this sample will explore the degree to which our brief CB prevention program represents a viable (and potentially cost-effective) avenue to reducing depression in at-risk students when conducted in typical school contexts.

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