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Prevention of depression in youth: A qualitative review and future suggestions

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Abstract

Since 1990, significant efforts have been made towards developing interventions to prevent depression in youth. Meta-analyses of preventive interventions have consistently yielded small but significant effect sizes in the short-term prevention of depression. However, the maintenance of intervention effects over extended follow-ups ranging from 6 months to 3 years has not been consistently demonstrated. In this qualitative review, significant methodological issues that continue to be of concern are discussed. Illustrative studies are described to highlight the accomplishments and limitations of interventions to date. Particular areas in need of attention include the implementation of booster sessions, use of appropriate statistical analyses, examination of multiple outcome variables, augmentation of protective factors, and exploration of mediators and moderators of intervention effects. Future directions for the field of depression prevention are outlined.

In 1994, a commission on the prevention of mental disorders convened by the Institute of Medicine (IOM) concluded its landmark evaluation by stating, “(t)here could be no wiser investment in our country than a commitment to foster the prevention of mental disorders and the promotion of mental health through rigorous research with the highest of methodological standards” (Mrazek & Haggerty, 1994, p. 483). Among mental disorders, major depression is startlingly prevalent, with nearly 1 in 6 Americans experiencing an episode during the course of his or her lifetime (Kessler et al., 2003). Furthermore, in the year 2000 unipolar depression was rated as the fourth leading cause of disease burden worldwide (Üstün, Ayuso-Mateos, Chatterji, Mathers, & Murray, 2004). Interventions that are theoretically informed, efficacious, and cost-effective would stand to benefit millions of Americans who are affected by depression.

Both developmental and practical considerations make child and adolescent samples particularly appropriate for preventive interventions. From a practical sense, the use of schools for recruiting and implementing an intervention allows for a streamlined process of conducting research. In addition, recent research has suggested that ages 13–14 may be a key developmental period for preventive interventions for depression (Evans et al., 2005). Two large-scale studies of youth reported lifetime prevalence rates for major depressive disorder (MDD) of over 20% by age 18, suggesting that preventive measures need to be taken prior to age 18 (Hankin et al., 1998; Lewinsohn, Hops, Roberts, Seeley & Andrews, 1993). Hankin et al. reported a marked increase in diagnosed depressive disorders between ages 15 and 18 for both boys and girls in a New Zealand birth cohort. Further, two other studies reported substantial increases in levels of depressive symptoms beginning at around age 13, particularly

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For a brief history on classification, see pp.19–22 of that report; also Evans et al., (2005).

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in girls (Ge, Lorenz, Conger, Elder, & Simons, 1994; Silberg et al., 1999). Thus, targeting interventions to youth prior to a period of significantly heightened risk will maximize the opportunity to reduce the incidence of major depression.

This review begins by defining prevention and different types of interventions. Next, a summary of quantitative reviews is provided. Third, rationale for the present qualitative review is explicated. Fourth, some exemplary studies are examined in detail. Fifth, several methodological issues are discussed. Finally, the paper will conclude with recommendations for future research on the prevention of depression.

Prevention Defined

The standard definitions currently used in the field of prevention are derived from the IOM's 1994 report.¹ Prevention refers to "interventions that occur before the initial onset of a disorder" (p.23, Mrazek & Haggerty, 1994). Although this definition is straightforward, it is important to keep in mind three significant considerations. First, "disorder" implies a discrete, categorical outcome, e.g., major depressive episode. However, as will be reviewed below, a significant body of prevention research has used depressive symptom levels as outcomes, and there are both practical and theoretically justifiable reasons for doing so. Second, the word "before" distinguishes prevention from treatment and maintenance interventions. Any intervention subsequent to a diagnosis of depression cannot be considered prevention. A third consideration pertains to the use of "initial onset." Although preventing relapse and recurrence of major depression are valuable enterprises, by definition, prevention seeks to halt the first onset of disorder. This distinction between first onset and recurrence is a useful principle for guiding research as there may be different psychosocial risk factors or processes implicated in first versus later episodes (e.g., Lewinsohn, Allen, Seeley, & Gotlib, 1999; Monroe & Harkness, 2005).

Three major categories of target populations were outlined in the IOM report (Mrazek & Haggerty, 1994, pp. 24–26). First, *universal* prevention refers to an intervention that is administered to the general population. That is, no particular subgroup is selected to receive the procedure. Second, *selective* interventions are targeted at subpopulations that are at elevated risk for a mental disorder. This risk factor may or may not be causally related to the outcome and may be based in biological (e.g., family history of affective disorder), cognitive (e.g., negative inferential style), or psychosocial (e.g., recent stressful life events) domains (Mrazek & Haggerty, 1994). Third, *indicated* interventions are targeted at participants who are already showing some signs, symptoms, or biological markers of disorder, but who do not yet meet diagnostic criteria. In some cases, the term "targeted" may be used to refer to selective and/or indicated interventions, reflecting that these procedures are only given to a subset of the population. Further, selected and indicated approaches are not mutually exclusive. For instance, Clarke et al. (2001) selected adolescents who had a parent with a current or recent history of depression, and focused their intervention on those adolescents with a subsyndromal level of depressive symptoms.

A focus on malleable causal risk (and protective) factors is crucial for successful intervention (Rapee, 2002; Reiss & Price, 1996). Distinctions among correlates, markers, and causal risk factors were outlined by Kazdin, Kraemer, Kessler, Kupfer, and Offord (1997). A *correlate* refers to a concurrent relationship between two variables without evidence of temporal precedence or causality. Kazdin et al. (1997) described a *marker* as having "intermediate status": it is a proven risk factor, but its causal role in the outcome has not been established. A *causal risk factor* is responsible, at least in part, for bringing about a particular outcome. In a related vein, protective factors are associated with a decrease in the probability of a negative outcome or an increase in the likelihood of a favorable outcome (Kazdin et al., 1997). In the

context of the present review, protective factors are defined as reducing the probability of a poor outcome (depression).

Quantitative Findings

Several quantitative reviews examining prevention and youth depression outcomes have recently been conducted (e.g., Horowitz & Garber, 2006; Jane-Llopis, Hosman, Jenkins, & Anderson, 2003). Most reviews of youth interventions have focused on depressive symptoms as the outcome variable because of the relative dearth of studies examining diagnostic outcomes (cf. Merry, McDowell, Hetrick, Bir, & Muller, 2004). Generally, intervention effects have been small to moderate in size during the intervention with somewhat larger effects demonstrated in targeted than universal interventions; effects across follow-up have varied across different meta-analyses. Jane-Llopis et al. (2003) reported on 25 trials of youth interventions and found a mean effect size of .21 for children and .19 for adolescents on post-intervention depressive symptoms. Merry, McDowell, Hetrick, et al. (2004) examined 21 randomized controlled studies, although only 13 reported data that could be used for pooling in their meta-analysis. For symptom-level outcomes at post-intervention, they reported standardized mean differences between intervention and no intervention groups of $-.26$ and $-.21$ for targeted (selected and/or indicated) and universal interventions, respectively. Notably, the 95% confidence intervals for universal interventions overlapped with 0, leading the authors to suggest that overall these universal interventions have not been shown to be effective. Merry, McDowell, Hetrick, et al. also summarized results over various follow-up intervals after prevention (e.g., 6 months, 12 months) and did not find support for intervention effects of depressive symptoms relative to the comparison group at any time interval.

Horowitz and Garber (2006) published the most recent meta-analysis of the effects of prevention on depressive symptoms in youth samples (up to age 20). The authors reviewed 30 randomized controlled studies, several of which were not included in the other meta-analyses. Horowitz and Garber reported that selective interventions yielded the largest effect ($d = .30$) at post-intervention. In contrast to the findings by Jane-Llopis et al. (2003) (which were aggregated across age groups), the effect size for universal studies ($d = .12$) was significantly smaller than for selective studies and the difference between universal and indicated interventions ($d = .23$) approached statistical significance. At follow-up both classes of targeted interventions ($d = .34$ -selected, $d = .31$ -indicated) were superior to universal interventions ($d = .02$). These findings suggest that universal interventions yield inferior results in the short and long-term as compared to targeted interventions.

Merry, McDowell, Hetrick et al. (2004) reviewed 5 studies that reported on diagnoses of depression, and found a significant standardized mean difference of $-.10$ at post-intervention. Both universal and targeted interventions were associated with lower incidence of depression. Significantly lower incidence of MDD at 1-year follow-up (but not at 3 or 6-month follow-up) was found in the intervention groups (vs. no treatment control participants) for targeted, but not universal, interventions.

Across these reviews, interventions have generally shown small but positive short-term effects on symptoms and diagnoses, particularly for targeted interventions. Short-term refers to post-intervention differences between the control and treatment groups. Results over the course of follow-up have not consistently demonstrated significant preventive effects.

The Present Review

As is apparent from the meta-analyses reviewed above, there is significant heterogeneity in effect sizes across studies (Horowitz & Garber, 2006; Jane-Llopis et al., 2003). Whereas one valuable contribution of meta-analyses is to synthesize conclusions across a wide range of

studies (and examine effect sizes, rather than statistical significance levels), a qualitative review can highlight nuances of interventions that may contribute to the heterogeneity in findings. Certainly, some moderating variables are examined in meta-analyses. Notwithstanding, a more fine-grained dissection of the strengths and weaknesses of particular studies may illuminate foci for future interventions.

Further, the present review examines a wide range of issues relevant to prevention research. A few of these recommendations have been echoed elsewhere (Horowitz & Garber, 2006). Notwithstanding, a fuller discussion of the status of the field in terms of methodology is included herein. A summary of the exploration of moderators and mediators of prevention outcome is provided. Sampling considerations and the use of appropriate statistical analyses and outcome measures are addressed. Finally, substantial recommendations are made for future research including increasing the focus on protective factors, examining the role of anxiety in the prevention of depression, and modifying current research programs.

Illustrative studies

The illustrative research programs discussed below represent a subset of the various prevention programs in the literature. They were selected based on their methodological rigor and their implementation in multiple investigations (see also Evans et al., 2005). Their solid empirical foundation makes the studies particularly appropriate for illuminating the strengths and weaknesses of youth prevention programs for depression. Space considerations do not allow for a close inspection of every program geared towards preventing depression in youth. However, the discrepancies among the results of these selected research programs are useful for exploring potential reasons for heterogeneity across the literature. Examples of each category of intervention are described, and where appropriate, compared. At the end of the section, two additional research programs that have been extensively investigated are also discussed.

Universal Interventions

Problem Solving for Life—The first set of studies reviewed here implemented universal interventions in large Australian samples. Spence, Sheffield, and Donovan (2003,2005) followed-up a sample of 1500 8th grade students over the course of 4 years. Eight schools were in the intervention condition and eight additional schools received no intervention. The schools were matched on size of enrollment and public versus private status (for 14 of 16 schools). All students with current major depression at the study's outset were excluded from analyses. The Problem Solving for Life (PSFL) intervention was disseminated by teachers in eight weekly sessions conducted in 45–50 minute meetings during school. Thus, this trial was essentially an effectiveness trial in which non-mental health professionals were the conductors of the intervention. The foci of the intervention were cognitive restructuring and problem solving skills training. The study was remarkable for its sample size, use of diagnoses, exploration of multiple outcomes, and substantial length of follow-up. The authors divided the sample into high and low-risk, mostly based on Beck Depression Inventory (BDI; Beck, Rush, Shaw, & Emery, 1979) scores at pre-intervention ($BDI \geq 13$). At post-intervention, the authors reported a significant group x intervention interaction (but not a significant main effect for group): high-risk students demonstrated a larger decrease in depressive symptoms than the monitoring control group; the low-risk students in the intervention condition also had a decrease in symptom levels whereas the control group increased in depressive symptoms. Further, there were greater improvements in problem-solving outcomes for those receiving the intervention than for control participants across both levels of initial risk.

However, at 12-month follow-up no significant differences on depressive symptoms or diagnoses were found between intervention and no intervention conditions (Spence et al.,

2003). Spence et al. (2005) extended these results and reported that across 2, 3, and 4-year follow-up, intervention group was not a significant predictor of changes in depressive symptoms, attributional style, social functioning, or problem solving skills. Diagnostic survival analyses were available only for initially high-risk students and yielded non-significant differences between the intervention and control groups. Given these results the authors concluded that universal interventions may not be optimal for the prevention of depression in youth. Certain factors potentially limiting the effect size of the intervention in this study included: the large group format, using only eight 45–50 minute sessions, teacher implementation, and significant attrition (about 30% dropout by 12-month follow-up). Jane-Llopis et al.'s meta-analysis (2003) reported larger effects for interventions that had more than eight sessions, met for 60–90 minutes, and involved a health care professional. Notwithstanding, Spence et al. (2005) cited other work (Shatté, 1996) that did not share several of these potential shortcomings but still reached similar conclusions at extended follow-up. Finally, changes in attributional style and problem solving were not demonstrated across follow-up, thus calling into question whether this intervention focusing on cognitive restructuring and problem solving operated via expected mechanisms.

Sheffield, Spence, et al. (2006) reported similar findings in their investigation of nearly 2,500 9th grade students. One set of comparisons (other findings will be discussed below) was between the participants receiving the universal intervention described above and a control condition receiving no intervention. Hierarchical linear modeling across post-intervention, 3-month, and 15-month follow-up revealed no significant intervention effect on depression symptom levels, hopelessness, negative automatic thoughts, problem solving, anxiety symptoms, or social functioning. Of note, many of these variables improved over the course of study across both conditions.

Resourceful Adolescent Program—In contrast, the work by Shochet and colleagues has yielded statistically significant findings across follow-up. The strength of this group's work using the Resourceful Adolescent Program (RAP) lies in the convergence of positive findings across three studies (Merry, McDowell, Wild, Bur & Cunliffe, 2004; Shochet et al., 2001; Shochet & Ham, 2004). An initial universal intervention trial conducted in Australia with 9th graders yielded high recruitment (88% consented; 20% attrition at 10 month follow-up), and consisted of 11 weeks of small group (8–12 students) treatment in 40–50 minute sessions conducted by psychologists or psychology graduate students (Shochet et al., 2001). The foci of these interventions were cognitive restructuring, problem solving, stress management, and accessing social support. Two of the three self-report depression measures were significantly lower in the treatment group than in the control group at post-intervention and at 10-month follow-up. Moreover, using symptom cutoffs to establish groups (clinical, subclinical, and healthy ranges), the RAP intervention yielded lower rates of students in the clinical range at follow-up. However, a major problem with this study was that the control and intervention groups were assigned nonrandomly: cohort 1 served as the control group, and the following year cohort 2 was in the intervention group.

In another study using RAP, Merry, McDowell, Wild, et al. (2004) conducted a randomized “placebo-controlled” trial with 9th and 10th grade students (N = 392) in New Zealand. Whereas some students received the RAP-Kiwi program, other students engaged in an 11-week program including fun activities that were thought to be pleasurable but not actively prophylactic against depression. Post-intervention effects were significant, with the intervention group reporting significantly fewer symptoms. Area under the curve analyses were significant for one of two measures of depression through 18-month follow-up. As will be discussed later, that approach, by itself, is problematic because using an omnibus test may obscure nonsignificant differences between groups at particular follow-up intervals, and may be driven by the large differences at post-treatment.

Details from an ongoing RAP study were described in Shochet and Ham (2004). The ongoing effectiveness trial of RAP involves over 2500 8th grade students in Australia from 12 schools. Shochet and Ham (2004) reported that the intervention was associated with lower levels of depressive symptoms at post-intervention and 12-month follow-up than a control condition.

Unfortunately, no statistical analyses are reported in Shochet and Ham (2004), so no conclusions can be drawn about the size of the effects in that study; given the large sample size, even a very small effect could reach statistical significance. Further, the RAP studies have not reported on diagnostic outcomes. Finally, it is notable that not all results using the RAP intervention have been positive. For example, Harnett and Dadds (2004) explored the effectiveness of the RAP intervention in a universal prevention study in a sample of girls (N = 212) ages 12–16. Whereas the facilitators in Shochet et al. (2001) were psychologists who received eight hours of orientation and ongoing supervision after every session, facilitators in the Harnett and Dadds (2004) study were largely teachers who received six hours of training and no ongoing supervision. Across the 1 and 3-year follow-ups, the girls who had received the RAP intervention did not differ significantly on measures of depressive symptoms from girls who had received no intervention. Thus, the effectiveness of the RAP program under “real world” conditions and training is still to be established.

Comparison of RAP and PSFL

What might account for the significant longitudinal results for RAP but not for the PSFL intervention? One possible difference is the dosage of the intervention: Shochet et al.’s RAP intervention consists of 11 instead of only 8 sessions. However, Horowitz and Garber (2006) did not report a significant moderating effect of intervention length on effect size. Furthermore, Sheffield et al. (2006) included a condition in which students with elevated symptoms were given the universal intervention followed by an additional eight 90-minute long small group sessions in the indicated intervention. Thus, the dosage of intervention was higher than in the RAP studies, and yet group differences were not significant across outcome measures. A second possibility is that the interventions are different and therefore differentially effective. Although it is not totally clear from the published reports how the content of the programs may differ, stress management and support seeking skills may be additional benefits of the RAP program. Further, whereas it appears that the Problem Solving For Life intervention does not result in lasting changes in cognitive style or problem-solving that could buffer against depression, it may be that the RAP program does inculcate cognitive, interpersonal, and problem-solving skills. Unfortunately, the work by Shochet and colleagues has not examined putative mediators, so it is difficult to assess what components of the RAP program may be particularly helpful. Future study on the RAP intervention is needed to clarify how it is achieving success when an ostensibly similar intervention is not as well as whether the effects of the RAP intervention extend to prevention of the occurrence of depressive disorders.

Targeted Interventions

Studies of children of depressed parents—Beardslee and colleagues (e.g., Beardslee, Gladstone, Wright, & Cooper, 2003; Beardslee et al., 1997) have implemented a promising selective intervention focusing on the offspring of parents with mood disorders. A substantial body of research has demonstrated increased prevalence of major depression in the offspring of depressed parents (e.g., Weissman, Warner, Wickramaratne, Moreau, & Olfson, 1997). Implicated in the transmission of depression are the influences of genetics, marital conflict, and parenting (Goodman & Gotlib, 1999). Further, Beardslee and colleagues have integrated prior work that found that adolescents’ understanding of their parent’s illness and absence of guilt for the parent’s illness were two factors associated with high levels of adaptive functioning and the absence of current psychiatric disorder in the adolescents of depressed parents (Beardslee & Podorefsky, 1988). This combination of selecting a well-established risk factor

(parental depression), examining the mechanisms by which it may operate, and attempting to augment protective factors is a good model for other prevention research.

In the most recent prevention study published by this group, Beardslee et al. (2003) followed-up 116 families with children ages 8–15 for 2.5 years. Their work has compared two active interventions: one in a group lecture format conducted by the principal investigator over two sessions with parents only, and the other a 6–11 session clinician-led psychoeducational intervention with individual families geared at decreasing children's self-blame and encouraging parents to facilitate their child's resilience via pursuit of extra-familial relationships and interests. At the 2.5-year follow-up, substantial changes were documented in parental behaviors and attitudes towards the children (e.g., increased communication with the children, decreased parental guilt, increased understanding of their children's experience), and these changes had significantly increased from the 1.5 to 2.5-year follow-up. Further, those changes were associated with children's increased understanding of the parent's illness. The number of changes in parental behavior was greater in the clinician-led family intervention. Importantly, self-reported child internalizing symptoms decreased over time in both conditions (from baseline to 2.5-year follow-up) suggesting that the changes in parental behavior led to improvements in children's behavior (see discussion below on mediation).

Unfortunately, there is a number of limitations to this study. Foremost, the absence of a placebo or waitlist control leaves uncertain any conclusions about the magnitude of the protective effects of this intervention. Earlier work by this group (e.g., Beardslee et al., 1997) also compared the same two active conditions used in the 2003 study. Inclusion of a control group could greatly enhance the value of their results by permitting an estimation of the effect size of the two interventions. Second, although Beardslee et al. (2003) reported on the prevalence of depressive disorders at follow-up interviews, they did not use disorder as a dependent variable to explore whether changes in parental behavior were associated with decreased rates of depressive disorders. Third, the authors did not report comparisons on most outcomes between pre-intervention, post-intervention, and the two follow-up periods; these analyses would clarify the trajectory of changes in parental behavior, children's understanding of parental illness, and children's symptoms over time. So, although the study's selective recruitment, excellent retention, and theoretically informed design are real strengths, significant limitations exist in the study's design and analytical framework.

Studies of adolescents with elevated depressive symptoms—Some of the most promising results to date have come from the work of Clarke and colleagues (Clarke et al., 1995;2001). Using a cognitive intervention consisting of fifteen 45–60 minute small group sessions, Clarke and colleagues are one of the few prevention research teams to report results on diagnostic outcomes. Their indicated intervention focused on the identification and challenging of negative thoughts. Clarke et al. (1995) randomly assigned 150 9th and 10th grade students who scored highly on a depression symptom screening measure, but did not meet criteria for current MDD or dysthymia, to intervention or usual care conditions. Clarke et al. (1995) found a significant reduction in the one-year incidence of MDD and dysthymia as compared to the control condition (25.7% vs. 14.5%). Interestingly, there were not significant differences in depressive symptoms or global functioning during follow-up. These seemingly conflicting results (positive findings for diagnoses but not for symptoms) reinforce the need for assessing multiple outcome measures. Further, as discussed below, these results highlight potential differences between cross-sectional (symptoms at a particular time point) and longitudinal (survival analyses using diagnostic outcomes across follow-up) analyses.

Using a combined selected and indicated intervention, Clarke et al. (2001) recruited participants from a large HMO sample. As a first step, they identified parents who had current or recent depression and/or dysthymia and had an adolescent aged 13–18 years old. Then, based

on a diagnostic interview, the offspring were divided into current depression, subsyndromal depression, or resilient (no significant depressive symptoms) subgroups. The group of 94 subsyndromal adolescents was randomized into intervention or usual care conditions. Risk for depression onset was significantly lower in the intervention group at 12-month (8% versus 24.7%) and 18-month follow-up and approached statistical significance at 24-month follow-up.² Notably, there was a significant delay in the onset of depression in the intervention participants who ultimately did meet diagnostic criteria, which may be a beneficial outcome itself (Muñoz, Mrazek, & Haggerty, 1996). There are two limitations to these studies. First, one-third of the sample in the earlier study and two-thirds of the latter sample of youth met criteria for prior depression, and results are not presented separately for first onset versus recurrent cases. Secondly, possible mediational mechanisms were not explored, and so further work is needed to examine how this intervention works.

Combined indicated and universal intervention—The study by Sheffield et al. (2006) discussed above also compared the effects of an indicated intervention (for high-symptom students only), a universal intervention, a sequential program consisting of the universal intervention followed by the indicated intervention, and a no intervention control condition. The facilitators of the indicated intervention were mental health professionals and this intervention entailed eight 90-minute small group meeting of 8–10 high-risk adolescents. The intervention focused on cognitive restructuring, problem-solving skills, interpersonal skills, and self-reward. No significant intervention effects were found among the four groups across one measure of depressive symptoms, hopelessness, problem solving, or social functioning. Regarding diagnostic outcomes, there were no significant differences between conditions on incidence of major depressive disorder or dysthymia over the 18 month study period: universal alone (18.1%), indicated alone (21.4%), universal + indicated (17.8%), and control (20.4%).

Comparison of targeted studies—What can be concluded about targeted interventions from the disparate results between Clarke and colleagues and Sheffield et al. (2006)? It does not appear that the length of the interventions accounts for this difference. Although the dosage of intervention is higher in the Clarke et al. (1995;2001) studies as compared to the indicated alone condition in Sheffield et al. (2006), the combined intervention in the latter study consisted of 16 meetings totaling about 18 hours. Further, similar to Clarke and colleagues, the indicated intervention used by Sheffield et al. was facilitated by mental health professionals. The sample size in Sheffield et al. (2006) was larger across follow-up ($N \geq 100$ per group) and had adequate power to detect moderate effects. One possible explanation for the differences in outcome between studies is that, simply, *not all “cognitive-behavioral” interventions are equally effective*. The methodologically strong studies by Spence and colleagues indicated that their particular intervention did not result in significant effects on depression outcomes nor on the mediating variables it was designed to alter: cognitive style and problem solving (Sheffield et al. 2006). In contrast, across two studies, Clarke and colleagues have demonstrated that their intervention is associated with significant decreases in the incidence of depressive disorder for up to 18 months after intervention. The programs do appear to diverge in their foci. Clark et al.’s Coping with Stress intervention focuses exclusively on cognitive restructuring, whereas the interventions used by Spence and colleagues focus on problem solving skills (Spence et al. 2003) and also interpersonal skills (Sheffield et al., 2006). It may be that the problem solving skills focused on in PSFL are not prophylactic and dilute the program’s focus on cognitive skills, which may be more effective. Alternatively, in light of the results described in the next

²The authors reported comparisons between groups using survival and chi-square analyses. Exact prevalence rates at the 18 and 24-month follow-up intervals were not reported.

section, interpersonal skills may be a useful intervention component, but the indicated program implemented in Sheffield et al. (2006) may not adequately instill those skills into the students.

Penn Prevention Program

Several studies have investigated the efficacy and effectiveness of a prevention program that has been referred to as the Penn Prevention Program (e.g., Jaycox, Reivich, Gillham, & Seligman, 1994; Gillham, Reivich, Jaycox, & Seligman, 1995), Penn Optimism Program (Shatté, 1996), or Penn Resiliency Program (PRP; Gillham, Hamilton, Freres, Patton, & Gallop, 2006; Gillham et al., in press). The Penn Program typically consists of 10–12 sessions each lasting 90 minutes and has two primary foci (e.g., Gillham et al., in press; Shatté, 1996). The first several sessions focus on cognitive-behavioral concepts and tools: e.g., education about pessimistic explanatory style, cognitive restructuring, and decatastrophizing. The second major component of the program involves social problem-solving skills: e.g., brainstorming, assertiveness, and decision-making.

A major initial study of the Penn Prevention Program investigated the efficacy of the intervention in 143 children in the 5th or 6th grade (Gillham & Reivich, 1999; Gillham et al., 1995; Jaycox et al., 1994). The intervention group was comprised of three different intervention conditions: cognitive training skills, social problem-solving skills, and a combined treatment. This study would be classified as a selected and/or indicated intervention, with students selected based on their combined score on a measure of depressive symptoms and their perception of parental conflict. The intervention groups were recruited from a single school district whereas students in the no-intervention control condition were from a different school district.

No significant differences were reported among the three active intervention conditions, and so the authors combined them for analyses. The intervention group had significantly lower levels of depressive symptoms than the control group at post-intervention as well as at 6, 18, and 24-month follow-ups (Gillham et al., 1995; Jaycox et al., 1994). Importantly, students in the intervention condition were less likely to report moderate to severe levels of symptoms at 12, 18, and 24-month follow-up. However, these results were not maintained at 2.5 and 3-year follow-up assessments (Gillham & Reivich, 1999). This important first study has several limitations: low recruitment rate, non-randomized assignment, and the absence of diagnostic outcomes.

PRP was used as an indicated intervention in Gillham et al. (2006). The intervention was administered by mental health professionals from an HMO to a sample of 11 and 12-year old participants who were randomized to either PRP or treatment as usual. Analyses of the full sample (N = 271) revealed non-significant differences between conditions on depressive symptoms or depressive disorders across 2-year follow-up. More fine-grained analyses yielded two significant findings. Girls, but not boys, who received the intervention had lower symptoms over follow-up as compared to the control condition, with differences approaching significance at 6-month follow-up and reaching significance at 12-month follow-up. Further, PRP significantly prevented depression, anxiety, and adjustment disorders with anxious and/or depressed mood (when analyzed collectively) among initially high symptom participants.

A recent study of the Penn Resiliency Program compared the program to another active intervention as well as to a control group (Gillham et al., in press; see also Shatté, 1996). Over 200 students were randomly assigned to one of three groups: PRP, Penn Enhancement Program (PEP), or the control group. The PEP program focuses on typical adolescent stressors (e.g., peer pressure, family conflict) and is largely discussion and activity based (Gillham et al., in press; Shatté, 1996). Some intervention groups were led by teachers or school counselors, whereas others were led by psychology graduate students unaffiliated with the research team. This randomized control trial was intended to be an indicated intervention but wound up being

quasi-universal, because of low recruitment rates. That is, all students who received parental consent were invited to participate in the study, regardless of symptom levels. Analyses of the full sample over the 3-year follow-up demonstrated that PRP did not significantly reduce depressive symptoms relative to PEP or control conditions. Interestingly, PRP was significantly effective in two of the three schools. At those two schools, PRP was significantly better than PEP at 24, 30, and 36-month follow-ups, and significantly more effective than the control condition at 12, 18, 24, and 30-month follow-ups. Similarly, students in the PRP intervention had lower overall rates of *elevated* and tended to have lower rates of *clinical-level* symptoms relative to controls (though not so relative to PEP). In contrast, at the third school, PRP was not significantly better than the control group, and was significantly outperformed by the PEP intervention. The authors were unable to determine the source of these discrepant results.

Several observations can be put forth regarding the Penn Resiliency Program, some of which have been clearly articulated by that research team (e.g., Gillham et al., in press). First, several studies have demonstrated at least small-to-moderate effects for this program on depressive symptoms at 1 (Shatté, 1996), 2 (Gillham et al., 1995), and 2.5 years post-intervention (Gillham et al., in press). Second, the largest effect sizes have been documented in studies implemented by the Penn research team (e.g., Gillham et al., 1995). Results have been smaller or inconsistent when the group leaders were not members of the research team (e.g., Gillham et al., in press) or in studies conducted by different investigators (e.g., Roberts, Kane, Bishop, Mathews, and Thomson, 2004). This discrepancy between efficacy and effectiveness is an important hurdle for this and other research teams to continue to address.

Third, few PRP studies have examined diagnostic outcomes, and results have been somewhat weak to date (e.g., Gillham et al., 2006). As is discussed below, the absence of diagnostic outcomes is a limitation of most research programs. Fourth, the results from Shatté (1996) and Gillham et al. (in press) call into question the superiority of the *specific* techniques implemented in PRP, and invite the possible explanation that nonspecific therapeutic factors largely account for the prevention effects. Few depression prevention studies in youth have compared two active interventions, and future investigations of this sort will be fruitful for illuminating the aspects of an intervention that are most strongly related to positive outcomes.

Finally, the rigorous training provided to group leaders may play an important role in the success of preventive interventions. Several of the studies that have documented sustained results have had either substantial pre-intervention training (40 hours-Clarke et al., 1995; 30 hours-Gillham et al., in press) or training plus ongoing supervision (25 hours-Shochet et al., 2001). In contrast, several studies that have not found significant preventive results have included lesser levels of training (e.g., 6 hours-Spence et al., 2005; one day- Barrett, Farrell, Ollendick, & Dadds, 2006; Sheffield et al., 2006). Extensive training requirements may make the large-scale implementation of an intervention more complicated. However, careful training and supervision of group leaders may be an important factor in treatment efficacy, and is an area that merits further attention.

FRIENDS program

The FRIENDS program was adapted from protocols designed to treat anxiety disorders in youth (for details see Barrett & Turner, 2001). Across a series of universal trials, Barrett and colleagues (e.g., Barrett & Turner, 2001; Lowry-Webster, Barrett, & Lock, 2003; Barrett et al., 2006) have examined the effects of the FRIENDS interventions on symptoms of both anxiety and depression. This universal intervention focuses on coping strategies for dealing with anxiety and distress, and includes tools such as cognitive restructuring, relaxation, and exposures with the help of parents (e.g., Barrett & Turner, 2001). Lowry-Webster et al. (2003) described the program as consisting of 10 weekly 75-minute long sessions with students,

three sessions with parents, and two booster sessions at 1 and 3 months following completion of the program.

The results for the FRIENDS intervention have been less consistent regarding depressive outcomes than for anxiety outcomes. Positive results were found by Lowry-Webster et al. (2003) who followed-up a sample of 594 students in grades 5–7. The investigators reported that the full intervention group had lower depression symptom scores than the control group at both post-treatment and 12-month follow-up assessments, and effects were larger for those students with initially elevated levels of anxiety symptoms. The universal effects appeared to be largely due to maintenance of baseline differences in depression scores between the control and intervention groups. However, in the smaller sample of initially highly anxious participants, baseline differences in depressive symptoms between conditions were not an issue, and the results were consistent with an intervention effect on depressive symptoms.

In contrast, a very similar study conducted by Barrett and Turner (2001) using the FRIENDS program did not find any significant reduction in depressive symptoms at post-treatment in their intervention groups. In fact, the lone significant effect involved an increase in symptom levels in the teacher-led intervention, which contrasted with decreases in symptoms in both the psychologist-led groups and the control group. Barrett et al., (2006) examined the long-term effects of the FRIENDS intervention in a sample of 6th and 9th grade students. The investigators reported no significant effects on levels of depressive symptoms across the 12, 24, and 36-month follow-up assessments. An earlier report on this sample by Lock and Barrett (2003) reported significantly lower depressive symptoms at 12-month follow-up for the intervention group, although Barrett et al. (2006) noted that those prior analyses had not accounted for initial group differences in the dependent measures. Interestingly, Barrett et al. found that there were fewer students with elevated anxiety and/or depressive symptoms (“high-risk”) at 36-month follow-up in the intervention condition relative to the control condition. However, the composition of the high-risk group, which included elevated anxiety symptoms, makes it difficult to determine the particular preventive effects on depression.

The FRIENDS program has wisely acknowledged the substantial overlap of depression and anxiety (e.g., Clark & Watson, 1991) and has demonstrated positive effects on anxiety symptoms (Barrett & Turner, 2001) and disorders (Lowry-Webster et al., 2003, cf. Lock & Barrett, 2003). However, although the major foci of the program ostensibly overlap with those of other depression prevention programs, the FRIENDS intervention has not demonstrated consistent nor long-term effectiveness in preventing depressive outcomes. Further, only two of the FRIENDS studies reported information on diagnostic outcomes for a subset of its sample, and no statistically significant reduction in depressive disorder was apparent (Lock & Barrett, 2003; Lowry-Webster et al., 2003). Interestingly, some of the most promising results are found in the significant reduction in depressive symptoms by participants who had high levels of initial anxiety (Lowry-Webster et al., 2003). It may be fruitful for the FRIENDS investigation to shift from universal to indicated approaches to examine whether the intervention may be particularly useful for students with elevated anxiety or depressive symptoms.

Issues in Prevention Research

Booster Sessions

So how long can the effects of prevention programs last? Several studies that have explored multiyear outcomes (e.g., Clarke et al., 2001; Gillham & Reivich, 1999; Spence et al., 2005) have generally found waning effects of an intervention over time. However, some other studies have yielded relatively sustained results. As mentioned above, Beardslee et al. (2003) reported sustained changes in parental behaviors and attitudes towards their children and lower levels of internalizing symptoms in the children resulting from their selective intervention. Their

program included periodic check-ins by phone or meeting every 6–9 months during 2.5-year follow-up. Likewise, Seligman, Schulman, DeRubeis, and Hollon (1999) reported on a randomized controlled trial of a selected intervention in a college student sample ($N = 231$). The intervention entailed eight weekly small group workshops lasting 2 hours each. Seligman et al. reported a significant reduction in cases of moderate depressive symptomatology over a 3-year follow-up. Notably, there were four post-intervention individualized sessions given to review information covered from the intervention.

The findings cited above are consistent with the notion that psychosocial interventions are not inoculations that are given once and then offer protection either indefinitely or even for many years. As astutely noted by Greenberg, Domitrovich, and Bumbarger (2001), short-term interventions may yield short-term effects, whereas ongoing/multi-year programs are more likely to have longer-term effects. Several authors have called for the inclusion of booster sessions in interventions, yet they have been implemented infrequently (e.g., Chaplin et al., 2006; Gillham et al., 2006; Merry, McDowell, Hetrick et al. 2004).

Nevertheless, the use of booster sessions is still a complex issue. Clearly, additional sessions weigh into the cost-benefit analysis of whether an intervention makes sense to implement. Also, further study is needed on the ideal timing of booster sessions (Gillham et al., 2000) and how to administer these sessions (e.g., single sessions periodically spaced out versus multiple sessions in close temporal proximity). Further, whereas booster sessions have not consistently resulted in sustained prevention effects (e.g., Barrett et al., 2006; Spence et al., 2003, 2005), longitudinal studies that compare an intervention with booster sessions to an intervention without booster sessions would help clarify their impact.

Sampling considerations

Offord, Kraemer, Kazdin, Jensen, and Harrington (1998) offered a substantive review regarding trade offs between universal interventions, targeted interventions, and clinical treatment. Only a few of their excellent points are reviewed here, and the interested reader is referred to their original paper for a deeper discussion of sampling issues.

Universal Interventions—Two of the major benefits of universal intervention are low stigma and the potential for widespread impact. That is, whereas targeted interventions necessarily involve selecting a subsample of the population at risk, all participants stand to benefit from universal intervention.

However, a universal approach also has significant problems, three of which will be discussed here. First, the issue of power is a considerable one (Cuijpers, 2003). Given the relatively low base rate of depression in an unselected sample, and the small effect sizes of interventions, universal interventions require enormous sample sizes (see Cuijpers, 2003 for a fuller discussion). In this vein, there is a substantial cost to widespread implementation of a general intervention, given to a sample who is mostly at low-risk, and most of whom will not develop depressive psychopathology. Finally, there is a significant challenge in selecting an appropriate intervention given that there are many different risk factors associated with depression that could potentially be addressed (e.g., family history, life stress, poverty, hopelessness, neuroticism) (Evans et al., 2005; Mrazek & Haggerty, 1994).

Targeted Interventions—The more promising longitudinal results, particularly for diagnostic outcomes, have been found in studies of targeted interventions (see review above). Indicated intervention is certainly a valuable means of prevention given the extensive literature on the potency of depressive symptoms as a predictor of future disorder. For example, Pine, Cohen, Cohen, and Brook (1999) reported increased incidence of depressive episodes at 2 and 9-year follow-up for youth (ages 9–18) who had elevated symptoms at the initial interview

(see also Fergusson, Horwood, Ridder, & Beautrais, 2005). Furthermore, statistical power is substantially elevated, particularly in indicated prevention where some subclinical psychopathology is already demonstrated (see Cuijpers, 2003).

However, there are also disadvantages to targeted intervention. First, for selected intervention, only a fraction of the population who will ultimately experience depression will be identified by a particular risk variable because of the multitude of known risk factors (Cuijpers, 2003). Similarly, only a fraction of those who have a specific risk factor will experience the disorder. In order to increase power, one might recruit participants who have multiple risk factors, thus greatly increasing the incidence of negative outcomes (e.g., Rutter, 2000). However, that increase in incidence comes at the cost of recruiting an even more specific sample, and may limit the generalizability of results. Further, there are practical consequences to using a targeted design. For instance, in the combined selected/indicated sample used by Clarke et al. (2001), the 94 youth were derived from an initial sample of nearly 3000 parents and 3400 youth.

Other potential challenges inherent to targeted interventions are that risk status may be unstable for certain variables and determining what constitutes elevated/at-risk status may be somewhat difficult (Offord et al., 1998). For example, regarding risk status, Seligman et al. (1999) found that 50% of their sample who scored in the most pessimistic quartile of attributional style at one pre-workshop assessment scored above the bottom quartile cutoff score at a second pre-workshop assessment. Thus, the reliability of an “at-risk” classification based on a single assessment may only be modest in magnitude for some variables. Second, Offord et al. underscored that there may be little difference in risk between those scoring just below the cutoff and those scoring just above the threshold. So, there is certainly some arbitrariness invoked in any cutoff decision.

General Considerations—To guide the decision on what type of prevention and/or whether to even implement prevention, Offord et al. (1998) outlined several variables to consider when evaluating preventive interventions: the cost of screening, sensitivity and specificity of the screening measure, the cost of the preventive intervention, the effectiveness of that intervention, the cost of treatment, and the base rates for the disorder during the targeted period. Future research should pay particular attention to these considerations when evaluating the practicality and cost-effectiveness of implementing a particular intervention, when comparing competing interventions, and when considering whether to intervene preventively at all.

Concerns about Statistical Analyses and Outcome Measures

Several issues need to be addressed regarding the outcomes reported in intervention studies. The first involves the definition of “prevention.” Gillham et al. (2000) drew a key distinction between prevention of an outcome “through a time of elevated risk” and treatment effects. More specifically, the goal of prevention is to reduce the incidence of diagnosed depression in the group receiving an intervention or, in studies without diagnostic outcomes, to prevent increases in levels of depression that occur in the untreated population over time. This prevention effect should extend beyond the post-intervention assessment and into follow-up.³ In contrast, a decrease from baseline levels of symptoms in the treatment group during the course of intervention that is not replicated in the control group is better described as *treatment* (Gillham et al., 2000). Horowitz and Garber (2006) reported that only 4 of the 30 studies they reviewed demonstrated an actual prevention effect wherein the control group experienced increases in symptoms that the intervention group did not. This distinction of prevention versus treatment should be carefully addressed by future studies in the literature.

³Gillham et al. (2000) do alter this definition if an intervention is ongoing for an extended period. If a preventive intervention lasted several years, for example, then a lower incidence of depressive disorders in the intervention group relative to the control group could be called prevention because expected increases in depression rates were diminished.

A second issue involves whether to report symptom or diagnostic level outcomes, or both. Although the IOM report placed a premium on diagnostic outcomes (Mrazek & Haggerty, 1994), there are valid justifications for using symptom-based outcomes as well. First, as cited above, depressive symptoms are a potent predictor of future depressive disorder. Interestingly, Pickles et al. (2001) found that depressive symptoms at time 1 were a stronger predictor of symptoms, depression diagnosis, and depression plus impairment at 1.5-year follow-up than were time 1 diagnostic status or level of impairment. Thus, if symptom levels remain lower following the intervention, there may be a future gain in terms of reduced incidence of depressive disorder. Second, symptom levels themselves are associated with dysfunction (e.g., Lewinsohn, Solomon, Seeley, & Zeiss, 2000). Across three different aged community samples, Lewinsohn et al. (2000) found that mild/moderate levels of depressive symptoms were associated with significantly greater psychosocial impairment than were low levels of symptoms. Third, particularly in universal interventions or during short-term follow-up, one's ability to detect effects may be greater using symptom-based outcomes because they are dimensional measures. Nevertheless, diagnostic level analyses should also be conducted in prevention trials.

A third issue is whether omnibus statistical tests alone are appropriate for studies using multiple assessments. Particularly in cases where follow-up assessments are spread far apart, interpretations of omnibus tests are problematic in the absence of planned or post-hoc cross-sectional comparisons. For example, Merry, McDowell, Wild et al. (2004) reported a significant difference favoring the intervention using area under the curve analyses over the post-intervention and 18 month follow-up periods. Based on their graphs however, it appears that at two if not all three follow-up assessments (6, 12, and 18 months) the error bars overlap between conditions. Thus, although it is correct to say that the intervention was associated with lower levels of depressive symptoms on average, it does not appear accurate to conclude that a statistically significant difference in symptomatology was found between the intervention and control groups at all or even most follow-up assessments. As an example of the use of statistics advocated here, Clarke et al. (2001) reported diagnostic results using both survival analyses and chi-square cross-sectional analyses (see also Gillham, Reivich, Jaycox, & Seligman, 1995, using symptom-level outcome). The use of both continuous and cross-sectional analyses obviates misinterpretation that could occur if a pronounced effect occurs early on but is not sustained.

A fourth consideration is raised by Kazdin (2000), who noted that there is more to measuring outcome than just assessing symptoms and diagnoses. Certainly, measures of depression are essential dependent variables to be investigated. Nevertheless, additional outcome measures such as social functioning, grades, family interactions, and service usage also merit exploration. For example, Clarke et al. (2001) reported significant intervention effects on global functioning. By collecting measures of functioning beyond just indices of depression, future studies may expand our understanding of the scope of the impact of preventive interventions.

Fifth, as more studies report diagnostic data (e.g., Arnarson & Craighead, 2005) it is vital that participants either are screened out based on history of depression, or that analyses are conducted that separate first onsets of depression from recurrences. As noted above the work by Clarke et al. (1995,2001) must be viewed in light of the fact that their sample contained a substantial number of participants with a history of depression and no analyses are presented that allow one to conclude that the first onset of depression was prevented. Importantly, there may be differences in the psychosocial processes implicated in the prediction of initial onsets versus recurrences of depression (e.g., Lewinsohn et al., 1999; Monroe & Harkness, 2005). As noted above, preventing recurrences of depression is a valuable enterprise, but largely falls under the rubric of maintenance intervention according to the IOM report (Mrazek & Haggerty, 1994).

Recent work by Arnarson and Craighead (2005) has begun to explicitly address the issue of preventing the first onset of adolescent depression. Only one-third of students (279 out of 866) met eligibility criteria for inclusion in this targeted intervention study: scoring between the 75th and 90th percentiles on a depression symptom measure and/or scoring above the 75th percentile on a measure of negative attributional style. Further, less than one-half of eligible students agreed to the diagnostic interview, and only 70% of those receiving the diagnostic interview were eligible for the study and wanted to participate (N = 86). Using a chi-square analysis of intent-to-treat, participants in the intervention condition had significantly fewer cases of major depression/dysthymia during the 1-year follow-up period than did a control group receiving no intervention. Limitations of this investigation included substantial attrition, recruitment issues, and the need for a longer follow-up period (the authors reported they are seeking to replicate their results with a larger sample). However, the study provides a much-needed demonstration of prevention effects of first onsets of depression.

Examination of Moderation Effects

An essential step in prevention research and application is to develop adequate knowledge of the factors that moderate and mediate the impact of interventions on depression outcomes (Gillham, Shatté, & Reivich, 2001). Moderator variables are those that affect the size and/or direction of the relationship between the predictor and dependent variables, whereas mediators explain either wholly or in part how the predictor affects the dependent variable (Baron & Kenny, 1985; Holmbeck, 1997). Deeper understanding of the circumstances under which interventions are optimally helpful as well as how they work are crucial to moving the field forward. Unfortunately, few studies have investigated potential moderators of preventive depression interventions (Gillham et al., 2001).

Initial symptom level—Several studies have conducted analyses separately for high versus low symptomatic participants (e.g., Gillham et al., 2006; Jaycox et al., 1994; Spence et al., 2003, 2005). For example, across two different follow-ups of the same sample, Jaycox et al. (1994) and Gillham et al. (1995) reported somewhat inconsistent findings regarding high versus low symptom participants. Whereas intervention effects were stronger for initially high symptom participants at 6-month follow-up, only the low symptom group has significantly fewer symptoms than comparison controls at the 18-month follow-up analyses. Regarding the 6-month follow-up findings, Jaycox et al. speculated that floor effects, lack of motivation, and the inability to practice the skills might have worked against positive findings in the lower symptom group. Notably, symptom levels in the initially low-symptom participants (both in the control and intervention conditions) increased throughout the study, and perhaps at 18 months that increase in symptoms gave the low symptom intervention participants the motivation and ability to use their skills. Exploration of symptom level moderation is useful in determining whether low symptom participants benefit from an intervention (weighing in on the decision to use universal interventions) and whether the timeline for effects may differ amongst participants with high versus low symptom levels at the outset.

Gender—Another moderator that has been examined is gender. To date, moderation analyses of gender have yielded conflicting results (Gillham et al. 2001; Horowitz & Garber, 2006; Merry, McDowell, Hetrick, et al., 2004). An earlier review by Gillham et al. (2001) summarized findings across several studies in which various permutations of outcomes have occurred: significant effects for boys only; increased symptoms in boys and decreased symptoms in girls; and nonsignificant gender differences. By contrast, in their meta-analysis, Merry, McDowell, Hetrick et al. (2004) came to a somewhat different conclusion. In the eight studies they included that reported symptom results by gender, intervention was associated with significantly decreased symptoms at post intervention for boys but not for girls (cf. Gillham et al., 2006). However, for the six studies that used diagnostic outcomes, significant

post-intervention effects were found for girls but not for boys. Across both sets of analyses, there were no significant gender differences during follow-up periods. These findings by Merry, McDowell, Hetrick, et al. (2004) should be viewed in light of the following considerations: only a small number of studies were reviewed and the effect sizes for both boys' diagnostic and girls' symptom outcomes at post-intervention approached statistical significance.

Finally, Horowitz and Garber (2006) operationalized sex as the percentage of females in each particular sample, and reported a significantly larger effect on post-intervention symptoms for samples with a higher percentage of female participants. However, this effect was not significant when two studies with college student samples were eliminated, and no significant effect for sex was found at follow-up. The approach of Horowitz and Garber differs from the comparison of pooled effect sizes for male versus female participants used by Merry, McDowell, Hetrick et al. (2004), and may account in part for the differences in findings, as may differences in the studies included in each review. Further work is needed to clarify the impact of gender on intervention as a whole as well as on specific interventions.

It is also important to examine the implications of gender on the design of preventive interventions. Chaplin et al. (2006) compared the effects of the Penn Resiliency Program on girls who were in all female groups, co-ed groups, or in the control condition. At post-intervention, girls in both intervention conditions showed a significant reduction in depressive symptoms, but girls in the unisex groups had higher attendance and significantly greater reductions in hopelessness. A related consideration, particularly in universal or indicated interventions where individuals are not selected because of the presence of any particular risk variables, involves population-wide gender differences in certain risk factors. For example, females tend to show greater cognitive style vulnerability than males (see Hankin & Abramson, 2001). If an intervention is geared towards cognitive restructuring and changing attributional style, then a two-way treatment X gender interaction may be significant because more of the female participants received an intervention relevant to their risk factor. Although male participants at cognitive risk may also derive benefit from the intervention, gender differences on risk factors could impact findings from universal and indicated interventions.

The influence of other demographic variables such as race, ethnicity, and socioeconomic standing merit further exploration. As Gillham et al. (2001) noted, the majority of prevention studies have been conducted with middle and upper class Caucasian samples. Fortunately, several research teams have begun to develop and investigate interventions for culturally and economically diverse populations (e.g., Cardemil, Kim, Pinedo, & Miller, 2005; Yu & Seligman, 2002). These steps towards bridging the prevention gaps are to be applauded and continued.

Examination of Mediation Effects

A dozen years ago, Coie et al. (1993) called for attention to mediators in prevention research. Sadly, to date only a small minority of studies have reported mediational analyses and few positive results have been reported. Several studies that documented significant prevention effects (Arnarson & Craighead, 2005; Clarke et al., 1995, 2001; Shochet et al., 2001) did not test for the mechanisms by which their interventions were effective. Yet, the benefits of such an approach are great because one could potentially target the key components of the intervention and maximize their benefit.

In some cases where putative mechanisms have been tested, the proposed mediators did not reach statistical significance. For example, in Beardslee et al. (2003), changes in parental behavior and attitudes were not significant predictors of changes in youth internalizing problems. Whereas a significant relationship between the putative mediator and the outcome

is essential for establishing mediation (see Baron & Kenny, 1985), the changes in parental behavior cannot be interpreted as the mechanism through which the intervention affected the level of children's internalizing symptoms. In the report by Gillham et al. (1995) the authors reported that changes in attributional style appeared to mediate the effects of intervention on depressive symptoms. However, a later analysis by Gillham and Reivich (1999) reviewed the 3-year study as a whole and noted that significant symptom changes occurred at post-intervention and 6-month follow-up whereas significant changes in attributional style were not present until the 12-month follow-up. An assumption of mediational analysis using regression is that the dependent variable not cause the mediator, a possibility that cannot be ruled out in this study (Baron & Kenny, 1985).

On the other hand, in the study of college students conducted by Seligman et al. (1999), the authors presented some positive findings for mediation. They reported that changes in attributional style, hopelessness, and dysfunctional attitudes from pre-intervention to post-intervention mediated the effect of the intervention on depressive symptoms averaged across post-intervention and follow-up. One methodological caveat should be noted: by including both the follow-up and post-workshop measures of the dependent variable (depressive symptoms), the relationship between the mediators (measured post-workshop) and the outcome was not truly prospective. Notwithstanding, Seligman et al. used a theoretically informed choice of mediators, had ample follow-up, and found positive results. Future studies should include mediational analysis in order to better understand the key mechanisms through which a particular intervention is operating.

Protective Factors

Throughout this review particular attention has been paid to identifying and changing risk factors for depression. However, increasing protective factors is also an important component of intervention research and implementation (Coie et al., 1993; Greenberg et al., 2001; Mrazek & Haggerty, 1994; Reiss & Price, 1996). Unfortunately, few studies have examined changes in protective factors (Gillham et al., 2000). Both the IOM report and Coie et al. (1993) described individual and environmental factors related to resilience. Among the nonspecific protective factors that the IOM report mentioned were easy temperament, high intelligence, and a close relationship with a parent or other adult (Mrazek & Haggerty, 1994). As was the case regarding risk factors, it is vitally important that prevention research focus not merely on the descriptive characteristics of resilient individuals, but also on malleable protective factors through which intervention can actually affect outcome (e.g., parental-child relationship, presence of significant adult role models, investment in activities outside the home; Mrazek & Haggerty, 1994).

Southwick, Vythilingam, and Charney (2005) presented a valuable summary of factors contributing to stress resilience against depression. Given the impact of stress on the onset of depression (see Hammen, 2005), increasing these protective factors may play a useful primary or adjunctive role in preventive interventions. Southwick et al. (2005) listed the following factors as promoting resilience to stress based on extant research: positive emotionality, optimism, humor, adaptive attributional style, ability to reframe stressors, acceptance, spirituality, altruism, social support, presence of role models, active coping style, and exercise.

Optimism and attributional style appear to be what many interventions targeting cognitive risk factors are addressing and are thus incorporated into risk reduction approaches. Positive emotionality, to the degree that it refers to a trait tendency to have positive emotions, may be difficult to change, whereas interventions focusing on eliciting positive affect via pleasurable or masterful activities (e.g., exercise) may yield beneficial results (see also Dimidjian et al., 2006). Regarding humor, the authors do not specify to what extent it can be taught or altered. Ascertainment of role models and spirituality may not be appropriate either logistically or

practically for a school or other secular setting (e.g., job center). However, there are likely to be environments in which role models or spiritual guidance are available.

Some of the more practical resiliency factors to be modified by an intervention include active coping style, acceptance, and ability to reframe stressors (akin to attributional style). For instance, acceptance is likely to be important in interventions of youth whose parents experience depression. Regarding active coping, some programs targeted at resolving specific problems have yielded promising results in adult samples (e.g., Day, Kane, & Roberts, 2003; Vinokur et al., 2000). It may be that these kinds of practical, goal-focused approaches to problem solving (e.g., developing action plans to make specific life changes, job search skills) are more effective than are general problem-solving skills (e.g., Spence et al., 2003). Future work in this promising area is needed in youth samples.

Preventing Anxiety: The Back Road

A body of work has demonstrated a relationship between anxiety symptoms and disorders and future depression (e.g., Cole, Peeke, Martin, Truglio, & Seroczynski, 1998; Lewinsohn et al. 1994; Pine, Cohen, Gurley, Brook, & Ma, 1998). For example, in a 3-year prospective study of a sample of elementary school students, Cole et al. (1998) found that the level of anxiety symptoms at one assessment (t_i) were significant predictors of depressive symptoms at follow-up assessment 6 months later (t_{i+1}), even after controlling for the prior level of depression. In a large sample of community adolescents, Lewinsohn et al. (1994) reported that a history of anxiety disorder prior to the initial assessment was associated with increased risk for a major depressive episode during the one-year follow-up.

Thus, if anxiety is conceptualized as a risk factor for depression,⁴ interventions for anxiety might yield subsequent benefits for depression. As reviewed above, Lowry-Webster, Barrett, and Dadds (2003) as well as Lock and Barrett (2003) reported favorable results for their FRIENDS anxiety interventions with respect to preventing depressive symptoms. Although the effects of this intervention have been inconsistent to date, further work should be conducted to examine the conditions under which the FRIENDS intervention might be most beneficial in alleviating depressive symptoms.

Of note also are some findings that depression interventions may have effects on anxiety. For example, in the selected intervention of college students conducted by Seligman et al. (1999), the investigators reported decreased incidence for *DSM-III-R* (American Psychiatric Association, 1987) generalized anxiety disorder (GAD) in their intervention group relative to controls and this effect was more pronounced in men than women.⁵ In addition, the intervention group reported significantly lower levels of anxiety symptoms on 1 of 2 anxiety measures at post-intervention, 12, and 18-month follow-up, although not at 6, 24, or 30, or 36-month follow-up. Using the Penn Prevention Program in a rural Australian sample of 7th grade students ($N=189$), Roberts, Kane, Thomson, Bishop, and Hart (2003) reported no significant group effects for depression symptom levels, but found significantly lower anxiety symptoms in the intervention group at both post-intervention ($d = .26$) and 6 month follow-up ($d = .24$) (see also Roberts et al., 2004 for 18 and 30-month follow-up assessments of this sample).

So, what can be concluded regarding the relationship between prevention, anxiety, and depression? As noted above, some interventions ostensibly tailored to addressing one set of

⁴Alternatively, one could state this as anxiety being an indicator of the potential presence of risk factors that predispose to both anxiety and depression.

⁵The authors used a somewhat liberal cutoff on their diagnostic interview (Longitudinal Interval Follow-up Evaluation; Keller et al., 1987). Seligman et al. (1999) used a cutoff rating of 2, denoting moderate GAD, due to the low incidence of definite GAD (a rating of 3).

symptoms have led to effects on the other class of symptoms. These generalized effects are an added benefit to any intervention and support the notion that multiple outcome measures should be used in prevention trials. What is somewhat perplexing is that in the study by Roberts et al. (2003), the ancillary effects appeared to be at least as strong as the intended effects, and it will be useful for further work in this area to explore why this occurs. In light of the substantial relationship between anxiety and depression, focusing on nonspecific risk factors that may be common to both anxiety and depression has the potential to yield significant results (Dozois, Dobson, & Westra, 2004; Durlak, 1998).

Conclusion and Future Directions

The literature on prevention of depression, particularly in youth samples, has grown significantly and resulted in a substantial accumulation of positive results. Some recent studies have implemented theoretically informed interventions, explored both symptom and diagnostic outcomes, used follow-up periods beyond 6 months, and examined the effectiveness of interventions in the community.

However, there is continued need for studies of high methodological quality with interpretable results. As noted above, methodological issues pervade even some of the best studies: e.g., lack of random assignment in Jaycox et al. (1994) and Shochet et al., (2001); lack of separate analyses for recurrence versus first onset MDD in Clarke et al. (1995; 2001); no control group in Beardslee et al. (2003). Thus, the development of efficacious preventive interventions is still in its early stages. As outlined in this review, future research should investigate diagnostic outcomes, address protective factors, and explore potentially mediating and moderating variables of intervention (see also Evans et al., 2005; Gillham et al., 2000). It will be especially important to establish how particular interventions work so that the active ingredients can be harnessed. Further, the effects of an intervention on different genders, ages, and cultures will be vital in the decision making of how to disseminate helpful interventions (see also Evans et al., 2005, for recommendations).

Regarding the identification of risk factors, prevention research should attempt to capitalize on important breakthroughs in behavioral genetics research. For example, Caspi and colleagues (2003) reported a significant interaction between life stress and a serotonin transporter polymorphism in the prediction of depressive symptoms and also found a strong trend for this interaction in the prediction of depressive episodes. These findings were replicated and extended by Kendler, Kuhn, Vittum, Prescott, and Riley (2005) who also found a significant interaction between this polymorphism and life stress in predicting depression. Assessment of these genetic risk factors may be a valuable asset to identifying at-risk participants in future prevention efforts.

Finally, the effects of depression interventions on anxiety and the effects of anxiety prevention efforts on depression merit further exploration. As reviewed above, Gillham and colleagues (2006) reported that in early adolescents with initially elevated levels of depressive symptoms, PRP resulted in decreased incidence across the 2-year follow-up of depressive, anxiety, and adjustment disorders (with anxiety and/or depressed mood) when the disorders were analyzed as a group. Future studies may benefit from the inclusion of symptom measures that assess both nonspecific and specific symptoms of depression and anxiety (e.g., the Mood and Anxiety Symptom Questionnaire; Watson & Clark, 1991). The key issue would be to address whether interventions designed to prevent depression are improving levels of general negative affect, low positive affect, or both.

The cost-effectiveness of preventive interventions should also be analyzed. Lynch et al. (2005) reported the first study of this sort in an adolescent population using the sample from

Clarke et al. (2001). The authors found that the intervention group had a higher number of depression free days during the 12-month follow-up, the total cost of all medical care (including the cost of intervention) was not significantly higher in the intervention group than in the control group, and the intervention met conventional guidelines for cost-efficiency (e.g., < \$50,000 per quality adjusted life-year). Further studies of cost-effectiveness are needed.

In conclusion, there will never be a preventive vaccine for depression. Indeed, some consider that at least mild depression may have some adaptive value (e.g., Nesse, 2000). However, we have reason to be optimistic that the incidence of depression can be reduced and the time to onset delayed. Intervention research has grown rapidly in the past several years and some promising avenues have been illuminated. Nevertheless, substantial work is needed: replication and extension of previous findings, exploration of the mechanisms through which interventions are operating, and integration of both individual and environmental risk and protective factors into preventive protocols. We are certainly “not there yet”, but we are on the right track.

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