

Investigating the Effects of Marriage and Relationship Education on Couples' Communication Skills: A Meta-Analytic Study

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This meta-analysis probes into previous research substantiating the positive effects of marriage and relationship education (MRE) on couples' communication skills. We reviewed 97 MRE research reports that yielded 143 distinct evaluation studies. We found modest evidence that MRE functions both as a universal prevention and as a selective or indicated prevention. That is, MRE demonstrates program effects at longer term follow-ups for well-functioning couples and at postassessment and shorter term follow-ups for more distressed couples. In addition, we found that observational assessments produced larger effects than self-report assessments, although interpreting the meaning of this difference is difficult.

Keywords: marriage/relationship education, couples' communication, meta-analysis

Since 1975, more than 100 published and unpublished studies have evaluated the effect of marriage and relationship education (MRE) on couples' communication. Previous meta-analytic studies have provided evidence that MRE is generally effective in improving couples' relationships (Butler & Wampler, 1999; Carroll & Doherty, 2003; Hight, 2000; Reardon-Anderson, Stagner, Macomber, & Murray, 2005). For instance, the most recent and comprehensive meta-analysis of MRE (Hawkins, Blanchard, Baldwin, & Fawcett, 2008) found that MRE produces significant positive effects on participants' communication skills ($d = .36$ to $.54$) and relationship quality ($d = .24$ to $.36$) at both immediate postassessment and follow-up. That study also found no gender differences in effects and only limited evidence of publication bias. Lower dosage programs produced weaker effects than moderate-dosage programs.

Notwithstanding the current evidence of the general efficacy of MRE, challenging questions and issues about MRE remain to be explored. For instance, although some previous meta-analyses have examined both immediate and longer term follow-up effects (Butler & Wampler, 1999; Hawkins et al., 2008), meta-analysts have not investigated whether MRE acts as universal prevention, which prevents relationship problems by strengthening well-functioning couples' relationship skills, or as selective or indicated prevention (Mrazek & Haggerty, 1994), which helps cou-

ples at risk for distress or those experiencing nonclinical levels of distress enhance their relationships. In addition, the question of how assessment method (i.e., self-report versus observed measures) impacts effect sizes has not been addressed adequately. The purpose of this meta-analysis is to examine the effects of MRE more carefully and critically. Because MRE is now a tool of federal and state policy receiving significant public funding, it requires even greater scrutiny in evaluation (Halford, Markman, & Stanley, 2008).

Universal Prevention Versus Selective or Indicated Prevention

The goal of MRE is to help couples form and sustain healthy, stable marriages and relationships. Most MRE programs evaluated to date appear to be oriented toward universal prevention. That is, the interventions are targeted to couples not experiencing significant distress, and their objective is to prevent future relationship problems by strengthening couples' relationship skills. However, using immediate, postprogram outcome measures to test universal prevention efficacy is inadequate (Halford et al., 2008). Although other meta-analytic work focusing on parenting interventions (Nowak & Heinrichs, 2008) found little evidence that follow-up timing influenced effect sizes, we thought it prudent to limit a test of universal prevention to studies with follow-up assessments longer than 6 months. We conduct this more rigorous test to examine if MRE can properly be termed effective universal prevention.

Previous research has found that couples at greater risk for future relationship problems are underrepresented in MRE (Halford, O'Donnell, Lizzio, & Wilson, 2006); yet distressed couples do participate in MRE (DeMaria, 2005). Furthermore, some evaluated MRE interventions appear to be oriented toward relationship-skill enhancement, either

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selective prevention (i.e., for those at greater risk for relationship problems) or indicated prevention (i.e., for those with elevated but nonclinical relationship problems). In fact, a great deal of work in the field is now targeting more at-risk couples (Dion & Hawkins, 2008). In this investigation, we explore whether couples at more risk or more distress display a gain in communication skills, thus clarifying whether MRE can be termed effective selective or indicated prevention. This has not been examined in previous meta-analytic studies of MRE.

Self-Report Versus Observational Assessment

Couple communication has been assessed using either efficient self- (or partner-) report questionnaires or with more labor-intensive observational measures. The method of assessment may be a significant moderator of MRE effects. Butler and Wampler (1999) reviewed seven evaluation studies of the Couple Communication program and found that effect sizes for observational measures of communication (post $d = .95$; follow-up $d = .69$) were significantly larger than for self-report measures (post $d = .21$; follow-up $d = .08$). Similarly, Hahlweg and Markman (1988) reviewed seven cognitive-behavioral premarital intervention programs that employed both observational and self-report measures and also found stronger effects for observational ($d = 1.51$) versus self-report measures ($d = .52$). Accordingly, they recommended the use of observational measures when evaluating MRE programs because the proximal goals of the programs are to modify couples' communication patterns, and ceiling effects may limit the changes that can be demonstrated by self-report measures. These meta-analyses, however, reviewed only a few MRE studies. Examining differences between self-report and observational measures should be done with all available MRE evaluation studies, which we do in this study.

Nevertheless, interpreting the potential differences between self-report and observed measures of couple communication is a complex task because these different methods may not assess the same construct. Self-report communication questionnaires generally consist of items that ask individuals to reflect in a general way about their communication patterns as a couple. For example, one of the most commonly used self-report questionnaires in evaluation studies of MRE programs is the Marital Communication Inventory (MCI; Bienvenu, 1970), which includes items such as, "Does your spouse insult you when he (she) gets angry with you?" (p. 28). As Lorenz and his colleagues (Lorenz, Melby, Conger, & Xu, 2007) pointed out, these kinds of self-reports then, are context and issue nonspecific and tap into "more heterogeneous and amorphous circumstances" (p. 506), which may reflect participants' "efforts to average their behaviors over recent weeks, or to recall the more salient of these behaviors, and in either case, may reflect efforts to weigh their behaviors to account for the life-long meanings couples attach to their behaviors" (p. 507). Self-report questionnaire data "may reflect a mixture of events and interactions that are recalled more or less vividly and then imbued with meanings that emerged over

the life of the relationship" (Lorenz et al., 2007, p. 500). In short, self-report measures likely tap into individuals' general schema about overall couple interaction patterns.

In contrast, observational measures of communication behaviors may tap into a somewhat different construct. For instance, one of the most common coding systems used in the assessment of couples' communication is the Marital Interaction Coding Scheme (MICS; Hops, Wills, Patterson, & Weiss, 1972). As is done in most other observational coding systems, the MICS requires couples to be videotaped while engaging in a 10-min conversation about a problem they have identified in their relationships (Floyd, O'Farrell, & Goldberg, 1987). The behavior displayed in a particular conversation likely includes both typical behaviors and behaviors that are idiosyncratic to the specific context (Lorenz et al., 2007). Moreover, the researchers, not the participants, interpret the meaning of the behaviors. Furthermore, the context for this behavior is constructed and observed by researchers, and studies have suggested that couples are less negative in laboratory settings than they are in other settings (Heyman, 2001). Although specific observations seem to be modestly correlated with couples' overall communication patterns (Kelly, Fincham, & Beach, 2003), still the generalizability of any context-specific interaction may be limited.

In short, observational measures of communication are highly contextualized whereas self-report measures likely reflect general, personal schema. A general schema about interaction patterns assessed by self-report measures may involve perceptions and interpretations that are more resistant to short-term change, whereas observed measures of specific communication behavior about a specific issue is more contextual and subject to greater change. Thus, outcome evaluations employing observational measures are likely to produce larger effect sizes.

The gap between observational and questionnaire assessments also may be related to issues of social desirability and perception bias (Lorenz et al., 2007). Observational data are collected by well-trained coders who are strictly following measurement protocols (Lorenz et al., 2007). Thus, researchers are typically coding the communication behaviors taught in their interventions (often by coders also familiar with the content of the intervention program), and the demonstration of these behaviors may reflect participants' desire to impress researchers by demonstrating their new techniques (Heyman, 2001). Self-report questionnaire data could be prone to similar bias if individuals report their knowledge of learned techniques rather than reflecting on how well they are implementing them (Heyman, 2001). However, because self-report measures likely tap into a more general schema about overall communication patterns, perhaps this bias is less of an issue for self-reports than it is for observational measures.

It should not be surprising, then, to find significant differences between observational and self-report questionnaire data. One way to minimize this difference, of course, would be to have couples report on a specific conversation about a specific problem rather than reflect on their overall communication patterns. Indeed, when this is done, obser-

vational and questionnaire data are more highly correlated than past research suggests (Lorenz et al., 2007). When the context of observational and questionnaire data is shared, they correlate as strongly as any combination of correlations among questionnaire reports (Lorenz et al., 2007). Unfortunately, to date in MRE research, there are no evaluation studies that use context-specific self-reports of couple communication, nor are there any studies that obtain a more representative sample of observed interactions across diverse contexts. Accordingly, our study can only highlight any systematic differences between self-report and observational methods in MRE outcome evaluation research; it cannot yet empirically explain the reasons for any differences found.

Observational measures do not necessarily yield larger effects than self-report measures, however. Although comparisons of self-report and observational MRE evaluation data to date have not reflected this pattern (e.g., Butler & Wampler, 1999; Hahlweg & Markman, 1988), self-report measures may actually be more sensitive when researchers are assessing low-incidence behaviors. Nowak and Heinrichs (2008) conducted a meta-analysis of the Triple-P parenting program and found that the intervention effects were smaller when assessed with observational measures than with other kinds of reports. They speculated that low-prevalence (child) behaviors are unlikely to be captured in brief observational measures. Instead, these infrequent behaviors may be better reported by parents who observe a child's behavior across a greater span of time and circumstances. Hence, if low-incidence communication behaviors are being assessed in MRE evaluation research, then self-report measures may be better indicators of skill acquisition.

The current meta-analysis will examine the following two questions: (a) Are MRE programs capable of acting both as a universal prevention by preventing deterioration of communication skills over time in well-functioning couples, and as a selective or indicated prevention by enhancing skills both short- and long-term for more distressed couples? (b) Do self-reports and observed assessments of communication skills yield similar or different estimates of the effects of MRE programs? We will answer these questions by conducting separate analyses for experimental, quasi-experimental, and one-group/pre-post studies. To test MRE as universal prevention, we will limit analyses to studies with at least 7-month follow-ups. To test MRE as selective or indicated prevention, we will limit analyses to studies that had samples with a substantial proportion of couples reporting preprogram relationship distress.

Method

This investigation is part of a comprehensive meta-analysis of MRE evaluation research conducted from 1975 through 2006 (see Hawkins et al., 2008). Of the 124 codable reports identified by this larger meta-analysis, the current meta-analysis limits its analyses to 97 reports (e.g., journal articles, dissertations) of research evaluating the effect of MRE on couples' communication. However, rather than reporting the general effects of MRE on communication,

these analyses provide in-depth information on the moderating effects of extended follow-up assessment, sample distress, and assessment method. Some of these reports used distinct samples to examine more than one intervention, and these distinct samples were each considered as separate studies (Lipsey & Wilson, 2001). Thus, the current meta-analysis represents the results of 143 distinct studies of the effects of MRE on couples' communication.

However, the number of studies reported for a particular statistic will be much smaller than this number for a number of reasons. First, we conduct analyses separately for experimental ($k = 41$), quasi-experimental ($k = 50$), and one-group/pre-post studies ($k = 52$; see Selection and Inclusion Criteria: Study design). The number of studies used in a particular set of analyses will also depend on the number of studies available for a given timing or method of assessment. There were only five experimental, two quasi-experimental, and seven one-group/pre-post studies that reported follow-ups beyond 6 months. There were only four experimental and three one-group/pre-post studies that had samples with a significant proportion of distressed couples. (Quasi-experimental studies, on average, had samples reporting lower satisfaction than average, indicating the presence of more distressed couples.)

Literature Search

We identified studies by reviewing the reference lists of previous meta-analyses and the search list from a recent MRE meta-analysis (i.e., Reardon-Anderson et al., 2005). We also conducted searches with PsychInfo and Dissertation Abstracts International. Also, we contacted many researchers and practitioners over a 2-year period to find unpublished reports. Although we did not do a specific search for studies in non-English languages, we did come across five such studies in our search. We employed translators to help us with the coding of these studies.

Selection and Inclusion Criteria

Psychoeducational couple intervention. All studies assessed the effects of a psychoeducational intervention that attempted to improve couple relationships. Three studies evaluated communication skills taught to high school students but not to couples in relationships; we excluded these studies from our analyses (Adler-Baeder, Kerpelman, Schramm, Higginbotham, & Paulk, 2007; Gardner & Boellaard, 2006; Gardner, Giese, & Parrot, 2004). Therapeutic interventions were excluded to provide a clear picture of the effects of psychoeducational intervention. This is an important conceptual and methodological consideration because therapeutic interventions generally have stronger effects than psychoeducational interventions (Shadish & Baldwin, 2003).

Reporting of outcome data. For inclusion in the current meta-analysis, studies had to report effects using quantitative methods that could produce an effect size. Some quantitative studies did not report the data necessary to calculate an effect size, such as group size, means, or standard devi-

ations. However, using various techniques, we succeeded in “rehabilitating” a limited number of these by following recommendations outlined in Lipsey and Wilson (2001). Yet, six published and five unpublished studies were excluded from analyses because rehabilitation or contacting the author for more information was not possible.

Study design. We included both experimental and quasi-experimental evaluation studies. Experimental studies included a randomized no-treatment control group, whereas quasi-experimental studies included a nonrandomized, no-treatment control group. A no-treatment control group was defined as a group that did not receive an intervention comparable to mainstream MRE in content or intensity. When control groups administered interventions comparable in time or intensity to mainstream MRE, they were coded as treatment groups. (In this case, what may have been designed and reported in a research article or dissertation as one experimental study was coded as multiple, one-group pre-post studies; e.g., Halford, Sanders, & Behrens, 2001.) Wait-list control groups were coded as no-treatment control groups as long as appropriate pre-intervention data were collected. This was done to make use of all available MRE evaluation information and to standardize the comparison between treatment and no-treatment. For technical and conceptual reasons (such as self-selection into treatment groups), analyses were conducted and reported separately for each design group (see Lipsey & Wilson, 2001; Shadish, Matt, Navarro, & Phillips, 2000).

In addition, to get as complete a picture of MRE intervention as possible, we included studies employing one-group pre-post designs in our investigation as a supplement to analyses of controlled studies. Included in the one-group pre-post studies are a number of reports that compared one MRE intervention to another with two independent samples. In these cases, we coded each treatment separately as a one-group pre-post study. The one-group pre-post studies generally were conducted with otherwise sound methods, so ignoring them would have excluded an important body of evaluation work and potentially limited our understanding of the practice of MRE, especially if these studies are more representative of MRE in field (as opposed to laboratory) conditions, a distinct possibility. (One third of these studies were conducted in community settings, such as churches, where obtaining a no-treatment comparison group is more difficult, see Laurenceau, Stanley, Olmos-Gallo, Baucom, & Markmam, 2004).

We acknowledge, however, potential problems associated with using one-group pre-post studies. First, a precise effect size calculation requires the correlation between the pre-post assessments, information that was seldom reported. Often in these circumstances, meta-analysts reasonably estimate the correlation to be .50 (Schwartz et al., 2006), which we did in our study. In a meta-analysis of parenting education interventions, Nowak and Heinrich (2008) reconstructed a reasonable pre-post correlation from other statistical information in reports and found an average correlation of .54. In sensitivity analyses (not reported here),

we found that using estimates of the correlations from .40 to .60 did not substantially change the effects.

A second concern is that it is inappropriate to mix one-group pre-post or change-score effects sizes with group-difference effect sizes both because they are calculated with different formulae and because there may be unknown systematic differences between these different designs (Lipsey & Wilson, 2001). Accordingly, we report effect sizes from one-group pre-post studies separate from those with experimental and quasi-experimental studies, and do so as a supplement to group-difference analyses to see if they produce similar patterns of findings. We are interested in the pattern of findings rather than the precise estimate of the one-group pre-post effect sizes because, as mentioned above, there could be systematic differences that bias these effects, although the direction of that bias can go either way (Nowak & Heinrichs, 2008).

Publication status. Both published and unpublished studies were included to directly address publication bias. Baldwin and Shadish (2006) found that meta-analyses that ignore unpublished studies overstate the true effect size by an estimated 5% to 20%. Of the 97 reports analyzed in this study, 42 were published reports and 55 were unpublished reports, most of which were doctoral dissertations. The fact that there were more unpublished than published reports of MRE programs diminishes the specter of publication bias, and in analyses reported elsewhere (Hawkins et al., 2008), there was only minor evidence of publication bias. Still, including unpublished studies should yield a less biased estimate of the true effects of MRE.

Timing of assessment. Earlier meta-analyses found that communication skills may deteriorate at follow-up (Butler & Wampler, 1999; Reardon-Anderson et al., 2005), although the most recent, comprehensive meta-analysis found little deterioration (Hawkins et al., 2008). Conducting analyses on extended follow-up assessments allowed us to address the question of whether communication training sustained effects beyond immediate postassessments or short-term follow-ups. For these analyses, we included only assessments that occurred at least 7 months after the conclusion of the intervention. This benchmark is consistent with the standards of evidence published by the Society for Prevention Research, which specifies that to establish program efficacy for outcomes that may deteriorate over time, there must be at least one study with a follow-up assessment greater than or equal to 6 months (Flay et al., 2005).

Level of relationship distress. To examine whether MRE functions as a selective or indicated prevention, we were interested in identifying the relative distress level of the samples in the studies included in our meta-analysis. Overall, only 79 out of 143 studies (55%) reported the distress level of their samples. To determine whether individuals were distressed, most researchers measured whether pretreatment relationship quality scores were below established cut-offs on standardized instruments, such as the Marital Adjustment Test (e.g., Durana, 1996b). For experimental and one-group pre-post studies that reported this information (53%), we were interested in analyzing those studies in which a significant proportion of the sample was

distressed, operationalized as 26% to 100%, (7 of 49 studies). For quasi-experimental studies, the treatment groups were significantly lower than control groups at pretest for both relationship quality ($d_{qe} = -.153, p < .05$) and communication skills ($d_{qe} = -.178, p < .01$). Accordingly, we were able to use these studies as a proxy for somewhat more distressed couples.

Variable Coding

A 55-item codebook was created to code moderators relevant to the effects of MRE. Many of the codes reflected dimensions of MRE identified in the Comprehensive Framework for Marriage Education (Hawkins, Carroll, Doherty, & Willoughby, 2004; e.g., program dosage). There were also a number of codes relating to the study (e.g., publication type), sample (e.g., ethnicity), and assessment methods (e.g., assessment timing). Two trained coders coded every study. The first author coded every study. Three other individuals served as the second coder. After separately coding, the two coders compared answers. When there were discrepancies, coders sought further clarification from the study text until they reached agreement. In cases in which the two coders were unable to come to a consensus, the coders discussed the differences with the second author until a consensus was reached. Thus, we did not compute intercoder reliability; rather we used coder discrepancies as a stimulus for deeper investigation into the study to ascertain objectively the correct code.

Computation of Effect Size

We computed effect sizes with Comprehensive Meta Analysis II (CMA; Biostat, 2006). Standardized mean group differences were calculated for experimental and quasi-experimental studies. The standardized mean change score was computed for one-group pre-post studies. Each effect size was weighted by the inverse variance (squared standard error) to account for the precision of the effect size estimates. Hedges's (1981) correction for small sample size bias was used because many studies had small sample sizes. We report the random effects results, which are generally a more conservative estimate of effects. Meta-analytic experts now recommend random effects estimates as standard practice (Shadish & Baldwin, 2003). As mentioned previously, we conducted analyses separately for each study-design group.

Results

Descriptive Data

The majority of studies (73%) delivered programs targeting married (rather than premarital) couples, and most (62%) provided a moderate dosage of 9 to 20 hr of formal training (median length = 12.3 hr), although not all those hours necessarily were devoted exclusively to communication and problem-solving skills training. Couples Communication (Miller, Nunnally, & Wackman, 1975) and interventions based on the Prevention and Relationship Enhancement Program

(Markman & Floyd, 1980) were the most frequently evaluated programs. The mean year of publication for experimental and one-group pre-post studies was about 1993, whereas for quasi-experimental studies it was about 1987.

Can MRE Programs Prevent Future Communication Problems?

In previous analyses (Hawkins et al., 2008), we found that the experimental-design communication skills effect size associated with short-term follow-up assessments ranged from $d_{ex} = .366$ to $.428$. However, only a handful of these studies conducted follow-up assessments past 6 months postintervention that would allow for more potential deterioration. For the five experimental studies that conducted follow-up assessments at 7 months or longer, the effect size was $d_{ex} = .588$ ($p < .01$). For the two quasi-experimental studies that conducted follow-up assessments at 7 months or longer, the follow-up effect size was $d_{qe} = .492$ (*ns*). For the seven one-group pre-post design studies with follow-up assessments at 7 months or longer, the follow-up effect size was $d_{pp} = .321$ ($p < .001$). Even with the small number of studies with follow-up assessments greater than 6 months, there is some evidence that MRE programs can prevent deterioration of communication skills for nondistressed couples.

Can MRE Programs Enhance Communication Skills for More Distressed Couples?

We explored whether MRE programs can enhance communication skills for somewhat distressed couples. As mentioned earlier, many MRE programs are taken mostly by couples functioning well within the nondistressed range of relationship quality, and some researchers even actively screened distressed couples from their samples. A small number of MRE program studies in our meta-analysis, however, do have somewhat distressed couples in their samples. Note that many studies (45%) did not report sample-distress levels, which limited our ability to explore this moderator of MRE effects.

Experimental studies. Four experimental studies had samples with a substantial proportion of couples (26% to 100%) reporting preprogram relationship distress. The post-assessment communication effect size for these four studies was $d_{ex} = .567$ ($p < .001$). At short-term follow-up, the effect size for these four studies was $d_{ex} = .646$ ($p < .001$). This finding is based on a small number of studies and should be interpreted cautiously, but it provides modest evidence that MRE programs can enhance immediate communication skills for somewhat distressed couples, at least in the short term. Note, however, that our analyses could underestimate or overestimate the effect size of programs oriented to somewhat more distressed couples because there were still many nondistressed couples in some of these programs.

Quasi-experimental studies. Another way we examined whether MRE programs can enhance relationship skills for somewhat more distressed couples was to examine effects

for quasi-experimental studies. In quasi-experimental studies, individuals with greater needs may be able to, and often do, self-select into treatment rather than control groups (Shadish et al., 2000). Thus, even if the trajectories of change are positive for treatment-group participants relative to control-group participants, differences between treatment and control groups at postassessments still may be small. We tested for significant group differences for pretest scores in quasi-experimental studies, as suggested by Shadish et al. These analyses revealed that, indeed, overall, treatment groups were significantly lower than control groups at pretest for both relationship quality ($d_{qe} = -.153, p < .05$) and communication skills ($d_{qe} = -.178, p < .01$). That is, quasi-experimental studies, overall, had somewhat more distressed couples and poorer communicators in treatment groups than in control groups at pre-assessment. (This was not true for experimental studies, as would be expected when effective randomization procedures are employed.) Thus, investigating quasi-experimental studies indirectly allows us to explore the effects of MRE programs that employ somewhat more distressed samples.

When we estimated effect sizes for quasi-experimental studies using the standardized mean difference effect size, the effects were likely underestimated because they did not take account of initial group differences due to non-random assignment to groups (postassessment communication $d_{qe} = .227, p < .01, k = 45$; follow-up $d_{qe} = .146, ns, k = 29$). To deal with this challenge, we examined effect sizes for quasi-experimental studies based on postassessment group differences of pre-to-post change scores that account for potential pretest group differences. As discussed in the Method section, computing effect sizes based on change scores presented computational challenges, however, because information on the correlation between baseline and postassessments was seldom provided. As we did for computation of the one-group pre-post effect size, we used an estimated correlation of .50 in our analyses. Noting this imprecision, change score effect sizes for the treatment group were: $d_{qe} = .356, p < .001$, at postassessment; and $d_{qe} = .379, p < .001$, at follow-up. Thus, examining quasi-experimental studies that tended to have slightly more distressed treatment-group couples, we found that MRE programs can produce modest, positive gains in communication skills that are maintained at short-term follow-up assessments.

One-group pre-post studies. Among nonexperimental, pre-post design studies, there were three studies that had a significant proportion (26% to 100%) of their samples in the distressed range. For these three studies, the postassessment effect size was $d_{pp} = 1.567, p < .001$. (Unfortunately, there was only one study with a follow-up effect size.) As with the results for experimental studies, these results must be interpreted with caution because they are based on a very small number of studies and the samples of the studies also include nondistressed couples.

Do Self-Report Measure Effect Sizes Differ From Observational Measure Effect Sizes?

We conducted analyses to test for differences between self-report and observational measures of communication skills outcomes. Observational measures yielded significantly larger effects than self-report measures. For experimental studies at postassessment, $d_{ex} = .173 (ns)$ for self-report measures ($k = 25$), but $d_{ex} = .849 (p < .001)$ for observational measures ($k = 19$). The difference between these effects was significant ($Q = 19.58, p < .001$). There was little deterioration of either effect size at short-term follow-up: $d_{ex} = .144 (ns)$ for self-report measures and $d_{ex} = .831 (p < .001)$ for observational measures. The difference between these effect sizes also was significant ($Q = 9.13, 1, p < .01$).

The results for quasi-experimental studies were similar to the results for experimental studies. At postassessment, $d_{qe} = .057 (ns)$ for self-report measures ($k = 36$) and $d_{qe} = .651 (p < .001)$ for observational measures ($k = 23$), a significant difference ($Q = 20.7, p < .001$). At short-term follow-up, $d_{qe} = .046 (ns)$ for self-report ($k = 21$) and $d_{qe} = .433 (p < .01)$ for observational ($k = 14$). This difference between the follow-up effect sizes, however, was not quite significant ($Q = 3.71, p = .054$).

For one-group pre-post studies, at immediate postassessment, we found no significant difference between self-report measures ($d_{pp} = .633, p < .001, k = 28$) and observational assessments ($d_{pp} = .720, p < .001, k = 20; Q = .89, ns$). However, at short-term follow-up assessment, effect sizes from observational measures were significantly larger than self-report measure effects (for self-report measures, $d_{pp} = .357, p < .001, k = 11$; for observational measures, $d_{pp} = .607, p < .001, k = 16; Q = 9.18, p < .01$).

Discussion

The purpose of this meta-analysis was to examine more critically the effects of MRE. Specifically, we sought greater clarity for the following two questions: (a) Are MRE programs capable of acting both as a universal prevention of deteriorating communication skills over time in well-functioning couples and also as a selective or indicated prevention by enhancing skills over both short- and long-term for more distressed couples? (b) Do self-reports and observed assessments of communication skills yield similar or different estimates of the effects of MRE programs? Answers to questions such as these are an important step in justifying any claims to the effectiveness of MRE, especially in the context of its current public funding.

Our analyses provide modest evidence that MRE functions effectively as a universal prevention; well-functioning couples improved or maintained learned communication skills compared to control-group couples (or improved relative to themselves in the case of one-group pre-post design studies) even when we limited analyses to studies with follow-up assessments greater than 6 months. Again, however, caution is needed because, unfortunately, the number of studies with longer-term follow-up assessments was

small. A strong test of universal prevention efficacy would be to push the temporal horizon for skill maintenance even further, to 2 to 3 years, which only a couple of small-sample studies have done. Fortunately, major studies with large samples followed for 3 or more years are in the professional pipeline.

In addition, examining a small set of MRE evaluation studies that included couples experiencing some relationship distress, we found modest evidence that MRE can function as a selective or indicated prevention by producing significant program effects for communication skills. This is encouraging news to policy makers who now are funding MRE services targeted to lower income couples with greater relationship needs and communication challenges. Caution is needed, however, because these findings are based on a small set of studies to date. Fortunately, researchers are currently conducting more studies with large and diverse samples of lower income and more distressed couples, so these results soon will be available (Halford et al., 2008). In addition, there is growing evidence in the related area of parent training programs that distressed families gain even more than nondistressed families from these kinds of prevention programs (Nowak & Heinrichs, 2008).

Our modest findings that MRE programs can enhance communication skills among couples with distress and relationship challenges are reinforced by some recent published studies not included in our study. Cummings, Faircloth, Mitchell, Cummings, and Schermerhorn (2008) evaluated the effects of an MRE program on a sample in which nearly half of couples were distressed and found short- and long-term reductions in marital conflict and improvements in marital quality. In addition, a recently conducted but still unpublished randomized trial (Cowan, Cowan, Pruett, & Pruett, 2007) suggested that MRE can produce positive relationship outcomes for lower income, ethnically diverse couples in California (67% were below the poverty line).

MRE, then, appears to be capable of functioning as universal, selective, and indicated prevention. This is encouraging because, as Weissberg, Kumpfer, and Seligman (2003) noted, there is a dilemma in the prevention field of how to balance the need for universal prevention programs provided to all interested individuals with the need for selective or indicated prevention programs designed to serve more at-risk or distressed individuals. Rishel (2007) urged prevention scholars "to implement and evaluate comprehensive prevention programs that aim at promoting and enhancing . . . mental health while still targeting those most at risk" (p. 161). There is mounting evidence that comprehensive prevention programs can be effective at both these goals (Nowak & Heinrichs, 2008; Weissberg et al., 2003).

Our second research question addressed how MRE effects are assessed; that is, whether self-report and observational measures of communication skills produce similar results. In most comparisons, observational measures produced significantly larger effect sizes than self-report measures. Also, it is interesting to note that in every case, effect sizes for observational data were significant, yet self-report

effect sizes were significant only for one-group pre-post studies.

Interpreting these results is challenging. As we noted earlier, observational and self-report measures may assess different constructs. Observational measures may better indicate whether couples have been able to incorporate specific communication skills and techniques into their behavioral repertoire, at least as they discuss a specific issue within a specific context. Thus, observational measures may be more sensitive to whether program participants learned a targeted skill. However, behavioral observations may be less effective at assessing whether learned communication behaviors are generalized across the diverse contexts and situations couples experience in their everyday lives to produce a personal schema of effective interaction. Self-report measures, on the other hand, may be better at tapping into a personal schema of how well couples are able to interact, communicate, and resolve problems in daily life.

One interpretation of our results is that MRE appears to be capable of changing couples' communication behavior in a specific context, but couples may not be generalizing that behavior to interaction in their relationships. Research has found that couples show less negativity in observed contexts than in naturalistic settings, so the large effect sizes produced for observed assessments may reflect reactivity effects (Heyman, 2001). This could be an issue particularly where researchers select only those codes from a coding system that capture the skills taught in their MRE interventions (e.g., Adam & Gringas, 1982). The fact that we found no evidence of deterioration of effect sizes from postassessments to short-term follow-up assessments for observed measures makes inflation due to reactivity effects less likely, but it is still possible. Of course, one possibility is that the communication and problem-solving skills emphasized in MRE are not relevant to day-to-day couple interactions. Indeed, some researchers have criticized the utility of some of these skills, such as active listening, noting that couples appear not to make good use of them in everyday life (Gottman, Coan, Carrere, & Swanson, 1998; Spitzberg, 2003). In contrast, another possibility is that skills such as active listening may act as important, albeit temporary, "scaffolding" that provides couples with external support while they develop more internal mechanisms for enhancing their relationships (Stanley, Bradbury, & Markman, 2000).

Our meta-analysis cannot determine which of these explanations for the difference between self-report and observational measures is valid. We need data from context-specific self-report measures and observational methods that sample a much broader range of everyday circumstances, issues, and times to see if context-specific behaviors are generalized. For example, one team of University of California at Los Angeles researchers (Ochs, Graesch, Mittmann, Bradbury, & Repetti, 2006) videorecorded family interactions in participants' homes, cars, and other settings on multiple occasions and times to investigate family cohesion. MRE evaluation researchers could invest in the use of video ethnography to observe family members in naturalistic settings, sampling several distinct time periods to capture everyday conversations and interactions.

Until we have new data reflecting some of these methods and measures, we cannot confidently interpret the meaning of the different outcomes produced by self-report and observational methods. Thus, for now it seems wise for MRE researchers to invest in collecting both self-report and observational assessments rather than to assume one measure is superior. Both assessment methods may have merit to MRE researchers and practitioners. Practitioners need to know if couples can learn complex new skills, and observational methods may be unbiased indicators of an ability to reproduce learned communication behaviors in a specific context. Practitioners hope that skills learned in their programs are implemented across a wide spectrum of communication challenges in everyday family life, not just in their laboratories. Self-report measures may represent a fair test of whether this goal has been achieved.

Past program-evaluation studies, as well as systematic meta-analyses, have asked the question, "Does MRE work?" There is mounting evidence that, in general, it does. This meta-analysis sought greater detail and depth to answer this question. We found modest evidence that MRE works not only as a universal prevention but also as a selective or indicated intervention. We also found that couples are capable of displaying learned communication behavior to trained researchers in specific problem-solving exercises. Less clear, however, is whether couples incorporate those skills effectively into their everyday communication challenges. Future research needs to be designed to clarify this important question.

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