

# Does Marriage and Relationship Education Work? A Meta-Analytic Study

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In this meta-analytic study, the authors examined the efficacy of marriage and relationship education (MRE) on 2 common outcomes: relationship quality and communication skills. A thorough search produced 86 codable reports that yielded 117 studies and more than 500 effect sizes. The effect sizes for relationship quality for experimental studies ranged from  $d = .30$  to  $.36$ , while the communication skills effect sizes ranged from  $d = .43$  to  $.45$ . Quasi-experimental studies generated smaller effect sizes, but these appeared to be due to pretest group differences. Moderate-dosage programs produced larger effect sizes than did low-dosage programs. For communication skills, published studies had larger effects than those of unpublished studies at follow-up; there were no publication differences for relationship quality. There was no evidence of a gender difference. Unfortunately, a lack of racial/ethnic and economic diversity in the samples prevented reliable conclusions about the effectiveness of MRE for disadvantaged couples, a crucial deficit in the body of research. In addition, intervention outcomes important to policy makers, such as relationship stability and aggression, rarely have been addressed.

*Keywords:* family policy, marriage and relationship education, meta-analysis, program evaluation

The science of prevention of human problems continues to grow and show promise (Flay et al., 2005; Rishel, 2007). In addition to the prevention of individual mental health problems, prevention efforts also include educational interventions to help romantic couples form and sustain healthy marriages and relationships. Marriage and relationship education (MRE) consists of two general components. The primary emphasis has been on developing better communication and problem-solving skills that are core to healthy, stable relationships, such as diminishing criticism and contempt and improving listening skills (Gottman & Silver, 1999). Couples learn about the importance of these skills and usually practice them with some instructor guidance. A second component of MRE is didactic presentation of information that correlates with marital quality, such as aligning expectations and managing finances. Couples learn about and discuss these issues and often make specific plans for dealing with them more effectively. Often within this component are discussions about important virtues related to relationship quality, such as commitment and forgiveness (Fincham, Stanley, & Beach, 2007). While some MRE programs emphasize one component to the exclusion of the other, most combine the two, and most of these give more emphasis to communication skills training. While many couple therapists also provide MRE services, MRE is distinct from couple therapy. MRE does not provide intensive, one-on-one work between participants and professionals on specific personal problems, as therapy does.

MRE provides “upstream” educational interventions to groups of couples and individuals before problems become too serious and entrenched (J. H. Larson, 2004).

Over the last decade, MRE has grown beyond programs offered by private professional and lay practitioners to become a tool of public policy. For example, U.S. federal policy makers recently have supported MRE as a way to help couples—especially lower-income couples—form and sustain healthy marriages as an additional tool to reduce poverty and increase children’s well-being (Administration for Children and Families, 2007; Dion & Hawkins, 2008). In 2006, federal legislation allocated \$500 million over 5 years to support promising MRE programs and initiatives targeted primarily at lower-income couples. (See <http://www.acf.hhs.gov/programs/ofa/hmabstracts/index.htm> for a listing of funded programs.) In addition, a growing number of states have also allocated significant public funds to support MRE efforts (Ooms, Bouchet, & Parke, 2004). For instance, Texas has dedicated more than \$10 million a year to support MRE; Utah has dedicated \$750,000 a year. With greater public support for MRE, however, comes greater public scrutiny (Huston & Melz, 2004).

Scholars have conducted many evaluation studies of various MRE programs over the past three decades (Halford, 2004; Halford, Markman, Kline, & Stanley, 2003). Previous meta-analytic reviews of MRE research have generally shown it is effective in increasing relationship quality and communication skills (Butler & Wampler, 1999; Carroll & Doherty, 2003; Giblin, Sprenkle, & Sheehan, 1985; Hahlweg & Markman, 1988; Hight, 2000; Reardon-Anderson, Stagner, Macomber, & Murray, 2005). However, these studies have been limited in their conclusions. The first meta-analysis of MRE is more than 25 years old (Giblin et al., 1985). The most recent meta-analysis (Reardon-Anderson et al., 2005) did not include quasi-experimental studies, studies that may be more representative of MRE as it is practiced under normal field conditions (Shadish, Matt, Navarro, & Phillips, 2000). Two studies reviewed only a narrow band of the marriage education

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spectrum—premarital education (Carroll & Doherty, 2003; Hahlweg & Markman, 1988). Another focused only on one specific program—Couples Communication (Butler & Wampler, 1999). Two meta-analyses did not distinguish between therapy and educational interventions for couples (i.e., Giblin et al., 1985; Reardon-Anderson et al., 2005). One meta-analysis was an unpublished dissertation (Hight, 2000) that did not differentiate between relationship quality and communication skills outcomes, although it was the only meta-analysis that gave significant attention to unpublished studies. Moreover, moderator variables important to practitioners and policy makers, such as gender differences, ethnic/racial diversity, and economic diversity of participants, have not been investigated extensively.

Our meta-analytic study addresses these limitations. Our primary aim is to address the following question: Does the overall evidence suggest that MRE can help couples form and sustain healthy relationships? Specifically, we evaluate the efficacy of MRE for relationship quality and communication skills at both immediate postassessment and follow-up assessment. We also explore several important methodological, sample, and intervention variables that may moderate the effects of MRE.

## Method

### *Selection and Inclusion Criteria*

*Psychoeducational intervention.* In the current meta-analysis, all studies assessed the effects of a psychoeducational intervention that included improving couple relationships or communication skills as a goal. Therapeutic interventions were excluded to provide a clear picture of the effects of psychoeducational intervention. Therapeutic interventions generally have stronger effects than do psychoeducational interventions (Shadish & Baldwin, 2003). Thus, we excluded studies that had set curricula but were delivered by a therapist to a couple as well as programs that were essentially group therapy (e.g., Worthington et al., 1997). Studies that focused on improving sexual functioning were excluded (e.g., Cooper & Stoltenberg, 1987).

*Reporting of outcome data.* We included studies that reported sufficient information to calculate effect sizes for the specified outcomes. When studies did not report sufficient information to calculate effect sizes, we contacted the authors where possible for more information and used methods for “rehabilitating” studies outlined by Lipsey and Wilson (2001). Six studies (5%) were dropped because we could not calculate an effect size.

*Outcome measures.* We coded measures of relationship quality that assessed various aspects of relationships such as areas of agreement–disagreement and conflict, time together, and areas of satisfaction–dissatisfaction. Some measures simply asked about overall relationship satisfaction. We included these measures as a subset of the broader construct of relationship quality. Most studies ( $k = 112$ ) used standardized measures, such as the Dyadic Adjustment Scale (Spanier, 1976) or the Marital Adjustment Test (Locke & Wallace, 1959). Communication skills were reported in numerous ways, including global assessments, positive and negative communication, positive problem solving, and negative problem solving, with both self-report and observational measures employed. We combined all these measures into a single, communication outcome indicating a global intervention effect on communication skills.

We examined both immediate postassessments and follow-up assessments, reporting these separately to explore deterioration (or gain) over time. Timing of follow-up for experimental studies ranged from 1 to 60 months; 3- and 6-month follow-ups were most common. Timing of follow-up for quasi-experimental studies ranged from 1 to 36 months; again, 3- and 6-month follow-ups were most common. When multiple follow-up assessments were available, we chose the assessment closest to 12 months. Only a handful ( $k = 7$ ) of studies employed follow-up assessments greater than 12 months. For instance, Schulz, Cowan, and Cowan (2006) evaluated the effects of their transition to parenthood MRE intervention at 6-, 18-, 42-, and 60-months postpartum. Although we coded the follow-up closest to 12 months to allow for more deterioration (or gain) of effects, note that most studies had only one follow-up and that assessment usually occurred between 3 and 6 months, not at a more distal 12 months.

*Methodological design.* Our primary interest is the efficacy of MRE, which is addressed by effect sizes representing the difference between intervention and no intervention. Thus, we included only studies that used control groups. This means we did not include a number of “horse race” studies comparing one intervention with another. Some studies were conducted with classic no-intervention control groups ( $k = 38$ ), but most used “waitlist” control groups ( $k = 73$ ). We chose to examine both experimental and quasi-experimental studies because quasi-experimental studies may be more representative of MRE under normal field conditions (Shadish et al., 2000). Experimental studies compared groups randomly assigned to an MRE-treatment or a control group; quasi-experimental studies included a no-treatment control group, but random assignment was not assured. (A full list of MRE studies reviewed but not included in this study, including treatment A versus treatment B studies, one-group/pre-post design studies, uncodable studies, and studies with duplicate data, is available on request.)

*Publication status.* We searched extensively for both published and unpublished studies so that we could address publication bias directly. Studies that are not published may be systematically different than published studies, including differences in the intervention effect size. Indeed, meta-analyses that ignore unpublished studies likely overstate the true effect size (e.g., Vevea & Woods, 2005). More than 60% of the studies in this meta-analysis were unpublished reports, primarily dissertations. Clinical graduate students conducted the large majority of these unpublished dissertation studies. Some developed their own intervention programs, but most employed well-known programs, such as Couples Communication. The studies generally were well designed but usually suffered from lack of statistical power due to small sample sizes. We suspect that the studies were unpublished primarily due to a lack of statistical power to produce significant results combined with authorship by graduate students who may have been headed toward clinical rather than academic positions.

*Foreign language studies.* While we did not conduct an exhaustive search for studies published in languages other than English, our search uncovered a handful of reports ( $k = 4$ ) published in other languages (i.e., German, Dutch, Afrikaans). When this occurred, we employed translators to help us code in order to include these studies in our meta-analysis. While program participants in these studies came from non-English-speaking countries with different cultures, they were predominantly White, middle

class, and educated, similar to samples of U.S. studies, and the programs they received were programs common in the U.S. studies. Moreover, language-inclusive meta-analyses generally provide more precise estimates (Moher et al., 1999).

### *Search Procedure*

We searched for MRE research conducted over the last three decades (since 1975), when the pace of work in this field began to pick up, through 2006, when substantial federal funding first targeted support for MRE. First, we reviewed 502 studies identified by a search conducted by the Urban Institute for their meta-analysis of MRE (Reardon-Anderson et al., 2005). Second, we searched bibliographies from other meta-analyses and literature reviews. Third, we searched PsycINFO for more recent work (since the Urban Institute search in 2003). Fourth, we searched *Dissertation Abstracts International* for unpublished work. Finally, we made extensive efforts over the course of 2 years at national conferences and through e-mail to contact researchers and practitioners to find unpublished (and in-press) reports. These search procedures produced 86 codable reports containing 117 independent studies.

### *Variable Coding*

We created a 55-item codebook to systematically code various moderators relevant to the effect of MRE. Due to space limitations, this study will employ only a handful of those moderators, such as publication status, sample characteristics (i.e., race/ethnicity, relationship distress, gender), and program intensity. (A copy of the codebook is available on request.)

Our coding team for this large collection of studies consisted of four individuals: two PhD researchers, a trained MS student, and a trained undergraduate student. One coder (MS student) was the “anchor,” coding every study. One of the other three individuals was the second coder. After separately coding, the two coders compared answers. When there were discrepancies, coders discussed their rationale and sought further clarification from the study text until they reached agreement. In cases where the two coders were unable to come to a consensus, the coders discussed the differences with the first author until a consensus was reached. Thus, we did not compute inter-coder reliability; rather, we used coder discrepancies as a stimulus for deeper investigation into the study to ascertain the correct coding.

### *MRE Participants Summary*

Samples in the 117 studies consisted mostly of White, middle-class, married couples in general enrichment programs who were not experiencing significant relationship distress. Only 7 studies had more than 25% racial/ethnic diversity in their samples; only 4 of these 7 studies had samples that were predominantly non-White. Similarly, only 2 studies had primarily low-income samples; another handful of studies had samples with at least some low-income couples. (Almost of all these studies came from unpublished dissertations.) There were no reports of homosexual couples in any of these studies. In terms of relationship status, the study samples consisted overwhelmingly of married couples; the number of unmarried, cohabiting couples, when reported, was negligible in

enrichment studies. (In programs targeting engaged couples there likely were more cohabiting couples, but this information was seldom provided.) In terms of life-course timing, 3 studies targeted single high-school students, 16 targeted engaged or seriously dating couples, and 10 targeted couples at the transition to parenthood. The remaining 75% of studies were general marriage enrichment programs (although these samples sometimes included a few engaged or cohabiting couples). There was more variation for relationship length (when reported); the average relationship length was 0–2 years for 18 studies, 3–5 years for 18 studies, 6–10 years for 32 studies, 11–15 years for 30 studies, and 16–20 years for 11 studies. Only about half ( $k = 61$ ) of the studies reported the relationship distress level of the samples. From these reports, there appear to be negligible numbers of distressed couples in the samples of most studies. Eight studies reported that 50%–89% of couples in the samples were distressed; 2 studies reported that 90%–100% of couples in the samples were distressed.

### *Computation and Reporting of Effect Sizes*

The effect size statistic employed is the standardized mean group difference. We adjusted each effect size by using Hedges’s (1981) correction for small sample bias. All effect sizes were weighted by the inverse variance (squared standard error) and averaged to create the overall effect size. We employed random effects estimates, as opposed to fixed effects. The random effects model allows for the possibility that differences in effect sizes from study to study are associated not only with participant-level sampling error but also with variations such as study and intervention methods (Lipsey & Wilson, 2001). In addition, the random effects model allows researchers to generalize beyond the studies included in the meta-analysis (Hedges & Vevea, 1998). We aggregated effect sizes to the study level because many studies included multiple outcomes. We used Biostat’s Comprehensive Meta Analysis II to perform these calculations.

For technical and conceptual reasons, it was wise to conduct analyses separately for experimental and quasi-experimental studies (Lipsey & Wilson, 2001). Often meta-analysts will include only experimental studies in their analyses because they provide the best evidence of efficacy. However, this also has the potential side effect of excluding significant numbers of studies that also may yield valuable information. In essence, we provide a “benchmark” by analyzing experimental studies first. Then, as suggested by Shadish and Ragsdale (1996), we compare these results with those from quasi-experimental studies. Moreover, rather than combining immediate postassessments and later follow-up assessments, we computed effect sizes separately by time to examine potential deterioration (or gain) in effects.

By analyzing the data in these ways, we encountered the challenge of dealing with a set of effect sizes rather than a single estimate. That is, we generated a set of four effect sizes for each outcome: 2 (design: experimental/quasi-experimental)  $\times$  2 (time points: postassessment/follow-up). In addition, we wanted to make a more direct test of deterioration (or gain) of effects. The most direct test of effect size stability from postassessment to follow-up requires limiting our analyses only to those studies that included both an immediate postassessment and a follow-up assessment. Some studies contributed effect sizes only at postassessment with no follow-up, some had no immediate postassessment but did have

a follow-up, and some studies had both. The first set of analyses described above compares postassessment and follow-up effects across studies, confounding real differences between postassessment and follow-up effects with potential between-study differences. Within-study comparisons that examine only those studies that have both a postassessment and a follow-up do not have this problem. Our overall challenge, then, was to interpret the pattern of effect sizes, as well as individual effects.

## Results

### Relationship Quality

*Experimental studies.* As seen in Table 1, at the immediate postassessment, the effects of MRE on relationship quality for experimental studies was  $d_{ex} = .361$  ( $p < .001$ ); and at follow-up,  $d_{ex} = .306$  ( $p < .05$ ). When limited to studies with both a postassessment and follow-up, a similar picture emerges of the magnitude and maintenance of MRE on relationship quality, although the effects generally do not reach conventional levels of significance due primarily to small numbers of studies. The postassessment effect size for experimental studies was  $d_{ex} = .244$  ( $p < .10$ ). The follow-up assessment effect size was  $d_{ex} = .277$  ( $p < .10$ ).

*Quasi-experimental studies comparison.* Using the effect sizes for experimental studies as benchmarks, we compared them with effect sizes for quasi-experimental studies. These effect sizes were generally smaller than those for experimental studies:  $d_{qe} = .150$  (*ns*) at postassessment;  $d_{qe} = .195$  ( $p < .05$ ) at follow-up. When limited to studies that included both a postassessment and a follow-up, the effect sizes were  $d_{qe} = .286$  ( $p < .10$ ) at postassessment;  $d_{qe} = .218$  ( $p < .10$ ) at follow-up. Although experimental effect sizes generally were somewhat larger than quasi-experimental effect sizes, the differences between the two research designs were not significant, although there was a trend for the difference at postassessment ( $Q = 2.98$ ,  $p < .10$ ).

In sum, the MRE program effects on relationship quality were modest but generally significant—ranging from .24 to .36—in experimental studies. In quasi-experimental studies, the effects were smaller, but not statistically so—ranging from .15 to .29. Moreover, immediate postassessment program effects did not diminish significantly at follow-up assessments.

### Communication Skills

*Experimental studies.* Table 1 also displays our analyses for communication skills. At postassessment, the effects of MRE on communication skills for experimental studies was  $d_{ex} = .435$  ( $p < .001$ ); and at follow-up,  $d_{ex} = .448$  ( $p < .01$ ). When limited to studies that included both a postassessment and a follow-up, a relatively similar picture emerges. The postassessment effect size was  $d_{ex} = .539$  ( $p < .01$ ). The follow-up effect size, however, suggested some deterioration:  $d_{ex} = .366$  ( $p < .10$ ).

*Quasi-experimental studies comparison.* Using the effect sizes for experimental studies as benchmarks, we compared them with effect sizes for quasi-experimental studies. Again, the effect sizes for communication skills were generally smaller than those for experimental studies:  $d_{qe} = .229$  ( $p < .01$ ) at postassessment;  $d_{qe} = .143$  (*ns*) at follow-up. When limited to studies that included both a postassessment and a follow-up, the effect size was  $d_{qe} = .290$  ( $p < .05$ ) at postassessment; and  $d_{qe} = .170$  (*ns*) at follow-up. Experimental effect sizes were significantly (or near significantly) larger than those for quasi-experimental studies (postassessment  $Q = 3.86$ ,  $p < .05$ ; follow-up  $Q = 2.84$ ,  $p < .10$ ) when examining all studies. When examining only studies with both postassessments and follow-up, however, the differences between research design groups were not significant.

Overall, we conclude that MRE program effects on communication skills were modest but significant—ranging from .36 to .54—for experimental studies. In quasi-experimental studies, the effects were smaller—ranging from .14 to .29. Moreover, communication skills effects generally did not deteriorate significantly

Table 1  
Effect Sizes of MRE on Relationship Quality and Communication Skills, by Study Design

Outcome/time	Experimental studies			Quasi-experimental studies			Design/ group difference $Q(df=1)$
	<i>k</i>	<i>d</i>	95% CI	<i>k</i>	<i>D</i>	95% CI	
Relationship quality							
Postassessment	46	.361****	.189, .533	48	.150 ( <i>ns</i> )	-.018, .317	2.98*
Follow-up	22	.306**	.069, .544	34	.195**	.003, .338	0.51 ( <i>ns</i> )
Post + follow-up studies							
Postassessment	17	.244*	-.064, .551	29	.286**	.051, .522	0.00 ( <i>ns</i> )
Follow-up	17	.277*	-.014, .568	29	.218*	-.003, .439	0.19 ( <i>ns</i> )
Communication skills							
Postassessment	37	.435****	.280, .589	48	.229****	.095, .364	3.86**
Follow-up	18	.448***	.170, .727	29	.143 ( <i>ns</i> )	-.077, .364	2.84**
Post + follow-up studies							
Postassessment	13	.539***	.199, .879	26	.290**	.053, .528	1.38 ( <i>ns</i> )
Follow-up	13	.366*	-.020, .753	26	.170 ( <i>ns</i> )	-.094, .435	0.67 ( <i>ns</i> )

Note. Post + follow-up studies refers to those studies that included both an immediate postassessment and a follow-up assessment. MRE = marriage and relationship education; CI = confidence interval.

\*  $p < .10$ . \*\*  $p < .05$ . \*\*\*  $p < .01$ . \*\*\*\*  $p < .001$ .

over time, although a clearer test with only studies that included both a postassessment and follow-up suggested some modest deterioration for experimental studies. Again, quasi-experimental studies generally produced smaller effects, but at follow-up the differences were not significant. Next, we explored possible reasons for the smaller effect sizes in quasi-experimental studies.

### *Exploring Smaller Effects in Quasi-Experimental Studies*

We found that quasi-experimental studies generally produced smaller effect sizes for both relationship quality and communication skills. Perhaps this is explained partially by the possibility that individuals with greater relationship needs self-selected into psychoeducational treatment (see Shadish et al., 2000) in these studies. Thus, even if their trajectories of change are positive relative to those of control-group participants, differences between treatment and control groups at postassessment may be small. We tested for significant group differences at pretest for quasi-experimental studies, as suggested by Shadish et al. (2000). In randomized studies, group differences at pretest should be zero, but in nonrandomized studies, this assumption may not hold. This was true in our case; we found that the treatment group was significantly lower than the control group at pretest for both relationship quality ( $d_{qe} = -.153, p < .05$ ) and communication skills ( $d_{qe} = -.178, p < .01$ ). In addition, we examined effect sizes for quasi-experimental studies based on pre-to-post change scores that took account of potential pretest differences. Computing effect sizes based on change scores presented computational challenges because information on the correlation between baseline and post-assessments was seldom provided. Noting this imprecision, effect sizes for quasi-experimental studies based on change scores produced effect sizes quite similar to those for experimental studies (and the effect sizes for experimental studies predictably did not change much; analyses are available on request.) Accordingly, pretest differences between treatment and control groups appear to have reduced the postassessment effect sizes difference for quasi-experimental studies.

### *Moderators of Effect Size Outcomes*

We explored seven variables that could moderate the effect size distributions described so far. (Due to space constraints, we do not include a complete table of these analyses, but a full table is available on request.)

*Racial/ethnic, economic diversity, and relationship distress.* We explored differences for sample racial/ethnic and economic diversity but found no significant differences for either outcome. However, as we described earlier, the serious lack of sample diversity in MRE evaluation work prevented fair tests of differences. Similarly, we found no evidence of differences by level of sample relationship distress, but the lack of distressed couples in the studies also prevented a fair test.

We did examine qualitatively the few studies that evaluated programs with more racially diverse samples ( $k = 4$ ) and more economically disadvantaged samples ( $k = 2$ ). For instance, Vijayalakshmi (1997) found significant effects of MRE on Indian American couples, and Wu (1999) found MRE to be effective in improving the relationships of Chinese American couples. Moitinho (2000) also found that MRE was able to improve significantly

Hispanic couples' scores on two dimensions of marital quality. Burnham (1984) found that MRE improved the relationships of low-income couples. In addition, a recently conducted but still unpublished randomized trial has suggested that MRE may help strengthen relationships for lower-income couples (Cowan, Cowan, Pruett, & Pruett, 2007). Cowan et al. (2007) randomized nearly 300 new-parent couples to receive either a 16-week couple intervention, a 16-week father involvement intervention, or a no-intervention control. The couple-intervention group did not decline significantly in relationship satisfaction, but the control and fathering-intervention groups did. Both treatment groups were significantly higher in their levels of father involvement and lower in their levels of conflict about children than was the control group. There were better child outcomes observed for both treatment groups, as well. These effects generally did not differ by ethnicity, income, relationship status, or distress.

*Publication status.* The large number of unpublished studies in our meta-analysis allowed us to test directly for publication bias. In our primary analyses with experimental studies, in one of four comparisons, published studies produced a significantly larger effect size (for communication skills at follow-up,  $Q = 5.08, p < .05$ ; published studies,  $k = 10, d_{ex} = .695, p < .001$ ; unpublished studies,  $k = 8, d_{ex} = .026, ns$ ). Comparison analyses with quasi-experimental studies yielded no significant differences between published and unpublished studies. Although publication bias does not appear to be a concern for relationship quality, the significantly larger effect for communication skills at postassessment suggests that the inclusion of unpublished studies is needed for an unbiased estimate of effects for this outcome.

*Timing of study.* For experimental studies assessing relationship quality, we found no evidence that there were significant differences in postassessment effect sizes over time for studies conducted in the approximately three-decade time period of our study (1975–1985; 1986–1995; 1996–2006;  $Q = 0.42, ns$ ). For experimental studies assessing communication skills, the earliest studies seemed to produce stronger effects compared with those of the later studies (1975–1985 studies,  $k = 13, d_{ex} = .581, p < .001$ ; 1996–2006 studies,  $k = 10, d_{ex} = .296, p < .05$ ), but this difference did not reach statistical significance ( $Q = 1.74, ns$ ).

*Gender.* In our primary analyses with experimental studies, there were no significant differences between women's and men's effect sizes, for relationship quality at postassessment (for women,  $k = 14, d_{ex} = .170, p < .10$ ; for men,  $k = 14, d_{ex} = .198, p < .05$ ;  $Q = 0.04, ns$ ), for relationship quality follow-up (for women,  $k = 12, d_{ex} = .173, ns$ ; for men,  $k = 12, d_{ex} = .219, ns$ ;  $Q = 0.04, ns$ ), for communication skills at postassessment (for women,  $k = 15, d_{ex} = .259, p < .01$ ; for men,  $k = 15, d_{ex} = .234, p < .05$ ;  $Q = 0.04, ns$ ), or for communication skills at follow-up (for women,  $k = 9, d_{ex} = .447, p < .01$ ; for men,  $k = 9, d_{ex} = .440, p < .05$ ;  $Q = 0.00, ns$ ). Similarly, comparison analyses with quasi-experimental studies yielded no differences. Hence, we find no evidence that MRE produces differential effects for women and men.

*Program intensity.* There was substantial variation in the length (in hours) of MRE programs, although most fell into a moderate-dosage category. We compared studies of low-dosage programs (1–8 hr) with studies of moderate-dosage programs (9–20 hr); the number of high-dosage programs (21+ hr) was too small ( $k = 9$ ) to yield reliable comparisons. Here the pattern was

clear. In all comparisons for experimental studies, including both relationship quality and communication skills, studies of moderate-dosage programs had substantially larger effect sizes than did low-dosage programs, and most of these differences were statistically significant (or indicated a statistical trend): for experimental studies, relationship quality at postassessment (low-dosage,  $k = 16$ ,  $d_{ex} = .179$ , *ns*; moderate-dosage,  $k = 27$ ,  $d_{ex} = .468$ ,  $p < .001$ ;  $Q = 3.24$ ,  $p < .10$ ) and at follow-up (low-dosage,  $k = 8$ ,  $d_{ex} = .115$ , *ns*; moderate-dosage,  $k = 11$ ,  $d_{ex} = .520$ ,  $p < .01$ ;  $Q = 1.86$ , *ns*); and for communication skills at postassessment (low-dosage,  $k = 15$ ,  $d_{ex} = .212$ ,  $p < .01$ ; moderate-dosage,  $k = 21$ ,  $d_{ex} = .557$ ,  $p < .001$ ;  $Q = 4.68$ ,  $p < .05$ ) and at follow-up (low-dosage,  $k = 6$ ,  $d_{ex} = -.184$ , *ns*; moderate-dosage,  $k = 11$ ,  $d_{ex} = .699$ ,  $p < .001$ ;  $Q = 7.62$ ,  $p < .01$ ). This same pattern held true for quasi-experimental studies. A survey of the effects associated with the small number of high-dosage programs, however, did not suggest that the most intensive programs yield even larger effect size estimates. Indeed, for quasi-experimental studies, effect sizes for high-dosage programs were generally negative. We speculate that these high-dosage studies attracted more distressed couples, and the more distressed couples were able to self-select into the treatment groups, thus creating the negative differences at postassessment. The highest dosage MRE program, PAIRS, attracts many distressed couples (DeMaria, 2005).

## Discussion

In this meta-analytic study, we coded 86 reports yielding 117 studies that produced more than 500 effect sizes in order to investigate the efficacy of MRE, which is now being used as a public policy tool intended to help couples form and sustain healthy relationships. Before discussing some limitations in this body of work, we review our findings and discuss their possible implications.

### Overall

Our primary analyses, which focused on experimental studies that clearly address efficacy, demonstrated that MRE produces significant, moderate effect sizes on two different outcomes that were commonly examined in MRE studies. For relationship quality, those effects ranged from .24 to .36. For communication skills, the effects were somewhat larger, ranging from .36 to .54. Moreover, when follow-up assessments were employed and evaluated, there was not much evidence of diminishing effects, a finding consistent with psychotherapy research on follow-up assessments (Nicholson & Berman, 1983). However, the most common follow-ups were at 3 or 6 months. Only a handful of studies included follow-up assessments at 12 months or longer. We surveyed these studies to explore whether longer-term follow-ups showed significant effects. Three studies found similar, significant effects with 2–5 year follow-up assessments (i.e., Hahlweg, Markman, Thurmaier, Engl, & Eckert, 1998; Markman, Floyd, Stanley, & Storaasli, 1988, for relationship quality; Schulz, Cowan, & Cowan, 2006), but two other studies did not (e.g., Markman et al., 1988, for communication skills; Van Widenfelt, Hosman, Schaap, & van der Staak, 1996). Inasmuch as the ultimate goal of MRE is to enhance long-term relationship quality and stability, we should be cautious about asserting long-term effects until a sufficient body of studies

is available to address this question. Two current, large-scale demonstration and evaluation studies (see Dion & Hawkins, 2008) will follow participants over 4–5 years and yield insight into the important question of duration of effects.

While not as strong as therapeutic interventions for couples (Baucom, Hahlweg, & Kuschel, 2003; Shadish & Baldwin, 2003), MRE is in the range of effects for other valuable prevention programs. Lipsey and Wilson (1993, see Table 1) reported the effect sizes of a number of prevention programs: parent effectiveness training,  $d = .33$  ( $k = 26$ ); maternal sensitivity to newborns programs,  $d = .44$  ( $k = 20$ ); adolescent pregnancy prevention programs,  $d = .35$  ( $k = 14$ ); alcohol and drug abuse prevention programs,  $d = .30$  ( $k = 98$ ); and stress management programs,  $d = .75$  ( $k = 18$ ). Thus, it seems reasonable that federal and state policy makers are interested in exploring whether greater availability of MRE services can help more couples form and sustain healthy marriages. Long-term funding of MRE services, however, should be informed by the results of research being conducted now on more disadvantaged couples.

### Disadvantaged Couples

Unfortunately, the research on the effects of MRE with couples from diverse racial/ethnic and economic backgrounds is sparse, making it impossible to draw definitive conclusions about MRE's efficacy for diverse groups. This is a crucial issue because publicly funded programs are being directed primarily at more disadvantaged groups that face greater risks for relationship problems (Ooms & Wilson, 2004). While we reviewed some emerging evidence that MRE can work for disadvantaged couples (Cowan et al., 2007), more work is clearly needed. Fortunately, both small- and large-scale longitudinal studies with curriculum adapted for disadvantaged populations are now being conducted (Dion & Hawkins, 2008; see Dion, 2005, for a description of these program adaptations). Similarly, these future studies are likely to contain substantial numbers of couples experiencing more relationship distress than is typical in MRE studies to date. A few studies suggested that distressed couples can benefit from MRE (Kaiser, Hahlweg, Fem-Wolfsdorf, & Groth, 1998). Halford, Sanders, and Behrens (2001) found that couples at higher risk for divorce benefited more from MRE than did lower risk couples, but future work will provide a better test of this possibility. In 2–5 years we will be in a better position to address the question of the efficacy of MRE programs for more diverse, disadvantaged, and distressed participants. In addition, programs targeting the distinctive needs of remarried couples are rare but increasing (Adler-Baeder & Higginbotham, 2004). More research about these more complex marriages is needed, as nearly half of marriages in the United States now involve at least one partner who was previously married (Bramlett & Mosher, 2001).

### Communication Skills Versus Relationship Quality

Overall, this meta-analysis generated somewhat larger effect sizes for communication skills than for relationship quality. We identify three possible explanations for this finding. First, more than two-thirds of the programs in these studies had a primary focus on communication skills training (another 20% had a secondary focus on it), and researchers typically directly measured

participants' demonstration of these specific communication skills. Thus, it is not surprising that communication skills would be most affected by the interventions. Second, most of the relationship quality measures were self-reports whereas many of the communication skills assessments were observational. Observational measures of communication skills typically yield higher effect sizes (Blanchard, Hawkins, & Fawcett, 2007). While couples may be able to display for researchers various communication behaviors learned in MRE, couples may not yet recognize or otherwise attend to positive changes in their overall relationship. In fact, there is some evidence that increases in communication skills can have a negative effect on relationship quality, at least in the short run, presumably because more relationship problems are being attended to but perhaps not fully resolved (Dindia & Timmerman, 2003). Third, observational methods are subject to reactivity (Heyman, 2001); couples may demonstrate recently learned skills for researchers under observation but not use them in natural settings. Thus, observational methods may overestimate the effects of MRE. If this is the case, then the smaller effect sizes for relationship quality may be better indicators of the true MRE effect size than those for communication skills.

### *Research Design*

We conducted separate analyses for studies with experimental and quasi-experimental designs, benchmarking quasi-experimental studies against experimental studies. This allowed us to analyze the most comprehensive set of studies to date. Other meta-analytic studies have found substantial effect size differences between different designs, although the direction of bias is not consistent (Lipsey & Wilson, 1993; Shadish & Ragsdale, 1996). We found that quasi-experimental studies generated smaller effect sizes than did experimental studies (though not necessarily significantly different). Post hoc analyses, however, suggested that quasi-experimental effect sizes were likely underestimated because pretest treatment-group scores were significantly lower on the outcome measures compared with control-group scores.

We draw two implications from these findings. First, the significant, modest effect sizes generated from the experimental studies provide some assurance that MRE effects are more than selection effects. That is, when randomization procedures are employed, group differences still emerge. Second, the artificial demands of true randomization may not be essential to every MRE program evaluation. Quasi-experimental studies, when they take account of potential pretest group differences, appear to yield similar effects. This should be welcome news to field practitioners who seldom have the resources or the circumstances to conduct evaluations with randomized control groups. Practitioners and evaluators also should be aware that when randomization procedures are not used, then somewhat more distressed couples may self-select into the treatment group.

### *Other Moderators*

Meta-analysts worry whether published studies, which are easier to find than unpublished studies, overestimate true effects. The problem of publication bias is especially salient in areas of study where sample sizes are generally small (Begg, 1994), which is the case for MRE studies. While numerous techniques have been

developed to estimate publication bias indirectly (Begg, 1994), we were able to examine this possibility directly because of our extensive search for unpublished studies that yielded a large number of these studies for our data set. Our analyses uncovered no evidence that published studies upwardly bias effect sizes of MRE on relationship quality. However, there was some evidence that reliance only on published studies may modestly overestimate effects for communication skills, at least at follow-up.

We were able to test for gender differences, but we found no evidence of effect size differences between women and men. This is good news to MRE practitioners who worry that men are less enthused than women about their programs and may benefit less from interventions. Perhaps MRE practitioners could use this information to help with recruiting more men.

In addition, we examined program intensity or dosage as a moderator of effects. We found that moderate-dosage programs—between 9 hr and 20 hr of instructional time—produced significantly stronger effects than did programs with less instructional time. Moderate dosages of MRE may be necessary to generate desired effects. That is, there may be a threshold of temporal commitment needed to create more substantial change, as some research suggests (Rishel, 2007). However, program intensity may be confounded with program content; more didactic programs generally are shorter than skills-training programs. On the other hand, high-dosage programs (21+ hr) did not appear to produce even stronger effects, although there were too few of these studies to produce reliable conclusions. Our findings seem to correspond to findings from a large, cross-sectional survey of participation in premarital education (Stanley, Amato, Johnson, & Markman, 2006). These researchers found no further positive effect on marital satisfaction after about 20 hr of premarital instruction. They also found an effect of premarital education on decreasing conflict, but this effect diminished after about 10 hr of instruction. The modal dosage we found in our meta-analysis was about 12 hr. While more analysis of dosage is needed, taken with the Stanley et al. (2006) findings, our findings suggest that moderate dosages may be about the right intensity, at least for middle-class, nondistressed couples.

### *Further Critique: Limited Outcomes*

MRE researchers have studied a limited range of outcomes, namely variables that primarily address relationship quality and communication skills. Moore and her colleagues (Moore et al., 2004) have suggested a multi-dimensional definition for healthy relationships that is being used to guide the federal Healthy Marriage Initiative. Many of those dimensions of a healthy relationship are understudied as outcomes in MRE research. For instance, only a handful of studies have examined indicators of marital stability or divorce propensity. This is a crucial outcome, because the stability of the relationship has important consequences beyond its quality (Amato, Booth, Johnson, & Rogers, 2007). Similarly, few MRE studies include measures of relationship aggression. This is a crucial outcome relevant to the quality of the couple relationship and the well-being of children in that relationship. It is also an important concern for policy makers supporting MRE with public funds (Roberts, 2006). Finally, intervention effects on important relationship virtues, such as commitment, sacrifice, and forgiveness (Fincham et al., 2007; Fowers, 2000) are seldom reported in

the research. Yet many MRE curricula address these virtues because they are important elements of healthy relationships. If relationship virtues can be strengthened, then MRE researchers should give these outcomes more attention. The hegemonic focus on relationship quality and communication skills is curious given that these are only two of the many known predictors of divorce (Amato et al., 2007; Karney & Bradbury, 1995). Finally, MRE studies have not directly linked adult relationship changes to child outcomes. From a policy perspective, MRE effects on adults will be most valuable when they are linked to children's well-being. Accordingly, we recommend that future MRE researchers regularly assess measures of relationship stability, aggression, and virtues, and consider including child outcome measures, as well.

### *Further Critique: Design Challenges*

MRE researchers have not attended to the potential fuzziness inherent in simple "treatment" and "no-treatment" designs. For instance, many MRE studies employ waitlist control groups that enroll couples who volunteer for intervention but are told they need to wait, sometimes for as long as a year, before beginning treatment. Given that many couples volunteer because they want help, negative effects could emerge during the wait period. In addition, treatment couples anticipating intervention likely have expectations that may initiate positive change even before treatment begins, what psychotherapy researchers term "pretreatment change" (Weiner-Davis, de Shazer, & Gingerich, 1987). For some couples, the initial choice simply to focus on their relationship, regardless of the treatment specifics, may create meaningful, positive change. Treatment versus no-treatment comparisons do not address these confounds. Psychotherapy researchers have attempted to address these issues (e.g., Wampold, Minami, Tierney, Baskin, & Bhati, 2005), but these confounds have not been given serious attention by MRE evaluation researchers. Little MRE research has explicitly studied the mechanisms of change, or the "active ingredients," in MRE, as well as the moderators and mediators of change. Perhaps this meta-analysis provides enough support for the general effectiveness of MRE so that future research can now concentrate on understanding *how* change occurs. This kind of understanding would lead to the design of even stronger interventions.

### *Further Meta-Analytic Research*

Finally, we reflect on further meta-analytic work that would be valuable. We have examined undifferentiated outcomes in this study: relationship quality and communication skills. In fact, however, these undifferentiated constructs also deserve a more fine-grained examination. For instance, although we examined the effect sizes of MRE programs on any aspect of communication, treating communication as a global construct may have obscured important distinctions (S. Wilson & Sabee, 2003), including observed versus self-report measures, and positive and negative communication constructs. Similarly, relationship satisfaction and relationship quality can be seen as distinct constructs (Amato et al., 2007). Some scholars argue that marital quality is not unidimensional; rather, positive and negative evaluations of the relationship are distinct (though related) dimensions and should be measured as such (Bradbury, Fincham, & Beach, 2000). Future meta-analytic

studies should test whether different approaches for measuring relationship quality yield different effect size estimates. In addition, because our focus was on the efficacy of MRE, we excluded from our analyses a large number of evaluation studies employing one-group/pre-post designs, or comparing one treatment with another treatment (38 reports yielding 67 codable studies; see for instance, Halford et al., 2001). Yet many of these studies were well conceptualized and reported results that could shed further light on the practice of MRE if they can yield data appropriate for meta-analysis. Future meta-analysis should consider how to make better use of these studies.

### Conclusion

MRE recently has gone beyond private psychoeducational programs to become a tool of social policy to help couples form and sustain healthy relationships. The results of our meta-analysis demonstrate the efficacy of MRE for White, middle-class couples; MRE produces modest but reliable effects comparable with those of other psychoeducational interventions of interest to policy makers. However, the question of efficacy for more diverse and disadvantaged samples remains an important area for research that will inform practitioners and policy makers. Moreover, having demonstrated the efficacy of MRE, at least with White, middle-class samples, the challenge now for practitioners and evaluation researchers is to develop even better interventions that produce stronger effects for relationship stability and quality.

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## Call for Nominations

The Publications and Communications (P&C) Board of the American Psychological Association has opened nominations for the editorships of **Developmental Psychology**, **Journal of Consulting and Clinical Psychology**, and **Psychological Review** for the years 2011–2016. Cynthia García Coll, PhD, Annette M. La Greca, PhD, and Keith Rayner, PhD, respectively, are the incumbent editors.

Candidates should be members of APA and should be available to start receiving manuscripts in early 2010 to prepare for issues published in 2011. Please note that the P&C Board encourages participation by members of underrepresented groups in the publication process and would particularly welcome such nominees. Self-nominations are also encouraged.

Search chairs have been appointed as follows:

- **Developmental Psychology**, Peter A. Ornstein, PhD, and Valerie Reyna, PhD
- **Journal of Consulting and Clinical Psychology**, Norman Abeles, PhD
- **Psychological Review**, David C. Funder, PhD, and Leah L. Light, PhD

Candidates should be nominated by accessing APA's EditorQuest site on the Web. Using your Web browser, go to <http://editorquest.apa.org>. On the Home menu on the left, find "Guests." Next, click on the link "Submit a Nomination," enter your nominee's information, and click "Submit."

Prepared statements of one page or less in support of a nominee can also be submitted by e-mail to Emmet Tesfaye, P&C Board Search Liaison, at [etesfaye@apa.org](mailto:etesfaye@apa.org).

Deadline for accepting nominations is January 10, 2009, when reviews will begin.