Assessing the Impact of Community Marriage Policies® on County Divorce Rates*

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Community marriage initiatives (CMIs) are designed to strengthen marriage and increase marital stability by addressing relevant laws, policies, and cultural factors. We examined a specific CMI designed to lower divorce rates by establishing a shared public commitment among clergy to strengthen marriage. A mixed-effects general linear model was used to determine whether changes in divorce rates over time were different before than after in 122 sites. Results indicate that divorce rates declined more rapidly following adoption, and this decline was larger than that observed in comparison counties. This difference in declines translates into a 2% difference annually in favor of CMI counties. Implications for measuring the effectiveness of CMIs are addressed.

Researchers, therapists, policymakers, and clergy have studied and discussed marriage and divorce for decades, resulting in a general consensus around two basic themes. First, divorce increases the likelihood of negative medical, legal, financial, social, physical, and mental health consequences for both parents and their children (Doherty et al., 2002; Stanley 2001). Although there are examples where divorced individuals are better off than when they were married, married couples and their children, on average, do better on nearly every front than those who are divorced (Doherty et al.; Waite & Gallagher, 2000). Second, communities and societies have lower crime and poverty levels when the proportion of intact families is higher (Doherty et al.).

Growing concern about the negative consequences of divorce is at least partly responsible for a recent interest in the connection between family structure and social policy. Evidence of this interest is seen in the increase in the number of private, political, and legal movements to strengthen marriage at a community level (Parke & Ooms, 2002). Doherty and Anderson (2004) relate the historical development of such efforts to strengthen marriage and describe the various forces behind the recent emergence of a broad cultural trend among multiple constituencies to work together to "revive the institution of marriage" (p. 425). The term community marriage initiative has been adopted to describe these movements (see Burg, 2004; Doherty & Anderson). Examples of such movements include Marriage Savers (described below), Families Northwest (http://www.familiesnorthwest.org), First Things First (http:/www.firstthings.org), and the Oklahoma Marriage Initiative (http://www.okmarriage.org). Some federal support for community marriage initiatives (CMIs) through the Community Healthy Marriage Initiative was initiated through the Department of Health and Human Services Administration for Children and Families. Further support has been proposed in a bill that features a $120 million annual appropriation, with up to $120 million more to match state funds for similar community marriage efforts being considered by the U.S. Senate in 2004 (Talent, 2003). Although some may consider marriage outside the purview of government responsibility, it is increasingly common for government and private agencies to combine efforts to promote healthy marriages.

Issues Related to Testing Community Marriage Initiatives

As the number of community-level approaches being implemented grows, the need to identify effective strategies also increases. Most research in the area of strengthening marriage focuses on the effects of interventions offered to individual couples (Bray & Jouriles, 1995; Fagan, Patterson, & Rector, 2002). However, community-level interventions to strengthen marriage have yet to be rigorously tested.

Several issues complicate the ability to isolate the effects of CMIs. CMIs most often develop without control over the quality or timing of implementation (e.g., random assignment to groups is usually impossible). Good information about implementation often is not available to incorporate into quasi-experimental designs. CMIs develop within a context of numerous factors that directly and indirectly affect marriage and divorce, such as economic trends, population shifts, cultural and religious factors, varying judicial practices, and legislative changes. Finally, divorce rate declines have been common in the United States over the last 10–15 years. The national divorce rate (the sum total of all divorces in the United States per 1,000 in the population) declined 14.9% between 1990 and 2001. Not only is the national divorce rate declining, but also 63% of U.S. counties in our database showed a decline (Institute for Research and Evaluation, 2003, based on data from state and county sources and the National Center for Health Statistics, 1995). Thus, it is unclear whether these changes in aggregate-level divorce statistics are the result of specific attempts to influence divorce rates or are simply due to more general factors (e.g., lower marriage rates, higher cohabitation rates, and demographic changes).

*This project was supported by Grant 2001-DD-BX-0079, awarded by the Bureau of Justice Assistance, U.S. Department of Justice. The Bureau of Justice Statistics is a component of the Office of Justice Programs, which also includes the Bureau of Justice Statistics, the National Institute of Justice, the Office of Juvenile Justice and Delinquency Prevention, and the Office for Victims of Crimes. The National Fatherhood Initiative secured and administered the grant. Points of view or opinions in this document are those of the authors and do not represent the views of the Department of Justice. We also acknowledge and thank James Burg, Mike Fishman, Norval Glenn, Wade Horn, George Julnes, and Lenny Sternbach for helpful comments on earlier drafts of this article.

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Key Words: divorce, initiative, marriage, marriage initiatives, policy.

(*Family Relations*, 2004, 53, 495–503)

Taken together, these issues require careful attention to several design and analysis features in our evaluation. We employed a quasi-experimental design, described later, that tests the effects of a specific CMI using divorce rate changes at the county level. For information about assessing the effects of CMIs at the individual and aggregated county levels, see Birch, Weed, and Olsen (2004).

Program History and Description

One of the earliest CMIs was the Marriage Savers program (see Parke & Ooms, 2002), launched in 1986 by founder Mike McManus with a group of concerned faith community leaders in Modesto, California. The premise was that, because a large majority of marriages occur in church settings (86% according to Hart, 2003), religious leaders could do more to strengthen marriage in their congregations through better preparation and marriage education in their communities.

The main emphasis of Marriage Savers has been to facilitate the adoption of what are known as Community Marriage Policies®. Most CMPs involve local clergy developing a policy in which they pledge, publicly and in writing, to implement at least five components to revitalize marriage (see McManus, 2003).

1. Require rigorous marriage preparation of at least 4 months, during which couples take a premarital inventory and discuss the identified relational issues with trained mentor couples who also teach couple communication skills.
2. Renew existing marriages with an annual enrichment retreat.
3. Restore troubled marriages by training couples—whose marriages at one time nearly failed—to mentor couples currently in crisis.
4. Reconcile the separated with a course conducted with a same-gender support partner.
5. Revive stepfamilies by creating stepfamily support groups for parents in remarriages with children.

In each of these components, couples in healthy marriages are enlisted to be mentor couples to help others at critical stages of marriage. By January 2004, the clergy of 183 cities and towns in 40 states had adopted a CMP with the goal of reducing divorce rates among those married in area churches. Most of these were created after 1996, when McManus formed the nonprofit organization Marriage Savers, which helps clergy organize CMPs. Numerous others have become involved through local congregational efforts.

The program is described in more detail in *Marriage Savers* (McManus, 1995) and in a manual, *How to Create a Marriage Saviors Congregation* (McManus, 2003). The latter was designed to help congregations implement both a community-level and a congregational-level program. On-site training for clergy who sign the policy and for mentor couples also is offered. The program founders have trained over 3,000 mentor couples in the past 10 years, and these couples in turn have trained others in their communities.

The theoretical framework for CMPs is based more on the developer's personal experiences and insights than on formal theory or research from the social sciences. It is probably more accurate to call it a model than a theory, although some aspects of the model are consistent with Social Learning Theory (Bandura, 1997). In essence, the model brings together several marriage-strengthening components, adds mentor couples, and promotes the package at the community-level through congregational leaders who agree to cooperate in strengthening marriage. Although the model prescribes the use of some components that have been shown to be research based (e.g., premarital inventories; see Larson, Newell, Topham, & Nichols, 2002), other components have not been evaluated (e.g., adding mentor couples to the various components). Whether this package of components affects divorce rates at the community level remains unknown.

Research Hypothesis

The main question we addressed here is whether divorce rates change differently in communities having CMPs compared with those without CMPs. We hypothesized that the divorce rate decline following CMP adoption will be greater in these communities than in matched non-CMP communities during the corresponding time period. This hypothesis was tested in the context of already declining divorce rates and with statistical controls for key predictors of aggregate divorce rates.

Method

Sample Characteristics

As noted, 183 U.S. cities have established a CMP. Because 55 signed policies at a date later than the most recent year for which divorce rate information was available (2001 in most cases), and 14 could not be included because of data limitations (described later), our sample was limited to 114 policies. This comprised 122 counties (a few large cities span more than one county).

CMP counties in the sample ranged in population from 9,031 to 2,265,208 (U.S. Census Bureau, 2000). The Midwest contained the greatest number of sites (46 sites, or 38% of the sample), the South had 28 (23%), the West had 23 (19%), the Southwest had 13 (11%), and the East had 12 (10%).

Description of Data

This evaluation relied on county-level data, because most of the required data are reported at that level. We assumed that the ratio of CMP city population to the county population was sufficiently high to ensure adequate program saturation. If this assumption is not true, finding significant effects will be less likely, and observed effects may underestimate actual policy effects.

Source information. Table 1 contains a list of variables used in the study and their sources. Specifically, county-level data on marriage and divorce compiled by the National Center for Health Statistics were used for the years 1980 to 1988, as reported by the U.S. Census Bureau (2002). Because county-level data for divorce were not available from this source after 1988, data from 1989 to 2001 were obtained directly from state and county sources. Some states provide this data online in electronic format, others provide data in the form of printed reports, and still other states have stopped collecting and or reporting it, in part because the federal government no longer requires or funds such data collection (National Center for Health Statistics, 1995).

After obtaining data for all 128 existing policies, 14 were excluded because of one or more of the following problems: (a) data points were missing (e.g., the first year after the signing of

Table 1
Demographic Data and Sources

Variable	Years Available	Source
Number of divorces	1980 to 2001	IRE
Number of marriages	1980 to 2001	IRE
Resident population	1980 to 2001	U.S. Census Bureau
City population	1980 to 2001	U.S. Census Bureau
Population of females > 15 years	1990 to 1999	U.S. Census Bureau
Number of females	1990, 1999	U.S. Census Bureau
Urban population	1990, 1999	U.S. Census Bureau
Population living below poverty	1989, 1999	U.S. Census Bureau
Median income	1989, 2000	U.S. Census Bureau
Median age	1990, 2000	U.S. Census Bureau
Number of religious congregations	1990, 2000	Glenmary
Number of members of religious congregations	1990, 2000	Glenmary
Number of religious affiliates	1990, 2000	Glenmary
Percent Catholics	1990, 2000	Glenmary

Note: IRE = Institute for Research and Evaluation, Salt Lake City, UT. U.S. Census Bureau = records, reports, and documents held by the Census Bureau, such as USA Counties, City and County Data Book, Current Populations Reports, and others. Glenmary = Glenmary Research Center, Nashville, TN (see Jones et al., 2002).

the CMP, all postintervention years, and so on) and were not available from any source; (b) when county sources were accessed to attempt to obtain missing data, county and state methods were not consistent across all years (e.g., notations were for filed versus finalized divorces); and (c) in some cases, data from county sources were too anomalous to be reliable (e.g., a divorce rate of 23.5 in one year when rates were typically around 4.0).

Demographic information, such as population, income, age, and urbanity, came from various U.S. Census Bureau reports. Information on churches and church membership was obtained from the Religious Congregations and Membership Survey (Jones et al., 2002), a standard source for these data since 1952. These data were used to assess how religious variables were related to divorce and to help establish a measure of the proportion of clergy signing the policy. Some of these demographic and contextual variables were reported annually, and others were collected at or near decennial census years.

We also attempted to collect data about the implementation of the policy (e.g., how well it was organized, how many people were involved), but the original CMP organizers were difficult to reach and often were no longer involved. Those who could be reached had inadequate records for this purpose. The latter difficulty was exacerbated because those surveyed had difficulty identifying the nature and extent of the activities engaged in by large numbers of pastors over a several-year period. Finally, because of the low response rate to the mail and telephone data collection attempts, a measure of the number of clergy who actually signed each policy was only available for a minority of the policies. Therefore, the data obtained were not adequate to further test the relationship between implementation levels and program results.

Data transformations. Several key variables for the analyses were developed from the sources noted. The divorce rate was calculated as the number of divorces in the county for the given year, multiplied by 1,000 and divided by the population, yielding a commonly used crude divorce rate per 1,000 in the population. Unfortunately, a more refined measure of the divorce rate, such as the number of divorces per marriageable female, was not available past 1999 at the county level.

To construct the variable that represented the period relative to when the CMP was signed, data on the divorce rate for each year from 1980 to 2001 were arranged in two columns: the year and the divorce rate for that year. This resulted in 22 rows of data for each policy. To test whether divorce rates changed simply as a function of time, the year variable could have been used. Because we were interested in seeing whether the change in divorce rates over time was different before and after the policy, a separate time variable was necessary because not all policies were signed the same year. Thus, the year that the policy was signed was coded as 1, and all other years were coded in increments of 1. For example, the year prior to signing was coded as 0, the fifth year prior as −4, and the third year after as 3.

A second variable (After) was created to distinguish the data observed prior to the signing of the policy (0) from that data observed following the signing of the policy (1). Another variable, CMP, was used to distinguish policy counties from comparison counties and was coded 0 for comparison counties and 1 for CMP counties.

Comparison County Data

Rationale for using comparison data. In the context of the previously cited decline in divorce rate at the national level, simple declines in the divorce rate in program counties do not constitute evidence of a policy effect. To more accurately portray the effect of the policy in the context of these declines, it was necessary to demonstrate that the change in the rate of decline in divorce rates after the intervention is greater than the corresponding change among a group of comparable counties without CMPs. Thus, using a well-matched comparison group helped to address potential selection bias because factors affecting marriage and divorce should be similar in the two groups (Shadish, Cook, & Campbell, 2002).

Selection of comparison counties. After reviewing the methods described by Shadish et al. (2002) for choosing matched comparison groups, we took three steps. First, the group of potential matches for each CMP county was limited to those within the same state. This increased the potential similarity of the matches on factors such as legal procedures for divorce and county-level divorce data collection practices. Second, we chose comparison counties that were as similar as possible to the CMP counties in terms of preexisting divorce rates. To accomplish this, we computed the difference between CMP and potential matched county divorce rates for each of the 5 years prior to the policy, squared that value, and then summed the squared values. This distance measure between CMP counties and all potential comparison counties within the same state (also known as the Euclidean distance; SPSS, 1999) provided a measure of similarity between the CMP county and each potential match. Although we considered matching on propensity scores (Rosenbaum, 2002), these data did not meet the criteria of reliably distinguishing between CMP counties and non-CMP counties in a logistic regression model.

In choosing comparison counties based on the preintervention divorce rates, we matched counties on the slope of the divorce rate over time prior to signing rather than the level of divorce rates. This choice was made because the hypothesis tested dealt with the change over time in divorce rates rather than change in the absolute level.

Finally, population was used as a second matching variable, and Euclidean distance measures were computed using population density. After comparing each potential match on divorce rates and population density, the county closest to the CMP county on both measures was chosen as the comparison county.

Table 2
*Results of Regression Analysis Results of Aggregate Predictors of Divorce Rates (*N = *1,824)*

Predictor	*b*	*SE*	β	*p*
Intercept	4.05	1.93	–	.04
Marriage rate	.08	.01	.17	.00
Median age	.03	.01	.08	.00
Median income	−.92	.41	−.05	.02
Percent adherents	−.39	.21	−.04	.06
Percent Catholic	−4.80	.28	−.40	.00
Percent female	7.06	.36	.12	.00
Percent urban	1.42	.14	.25	.00

Note: $R^2 = .30$, Adjusted $R^2 = .29$.

Matching on the dependent variable resulted in a set of comparison counties with similar preexisting divorce rate patterns as the CMP counties. The average divorce rate for the year prior to the CMP signing for each group of counties was not significantly different (4.79 for comparison group and 4.84 for CMP counties, $p = .79$), nor was the trend of the divorce rates prior to signing (−.095 divorces per 1,000 per year in comparison counties, −.084 in CMP counties). This increased our confidence that observed differences in postdivorce rate patterns would be a function of the intervention rather than preexisting differences between CMP counties and their matches.

We recognize that counties with similar divorce rates and trends may still differ in other important ways. To further control for differences between CMP and comparison counties, a predictive model of aggregate divorce rates at the county level was developed using data from all U.S. counties. Variables that were positively related to divorce rates were the marriage rate, percent of county population living in urban areas, median age, and percent of the population who are female. Variables negatively related to divorce rates were percent of population who are Catholic, median family income, and percent of population who are adherents of a religion. Table 2 contains information on the results of a multiple regression analysis. The inclusion of the Percent Catholic variable was based on its relationship to divorce rates generally and was not chosen for any reason specific to the CMP program because CMPs are intended for religious leaders of all faiths.

Differences between CMP and comparison counties on the predictors of divorce were examined next. Significant differences were found between the two groups in terms of the percent of the population living in urban areas, median age, and median family income. Table 3 displays the means and *t*-test results. Specifically, the inequality in terms of the percent of the population living in urban areas (mean percent was 76% in CMP counties compared with 59% in comparison counties) was attributable in part to several policy counties being located in metropolitan areas. This made it difficult to find counties within the same state that were closely matched on the divorce rate trend prior to the intervention and that also were similar in population and or urbanity. Notwithstanding the differences found, we did not change the composition of the comparison group, because there was no strong evidence that these differences were enough to account for differences in the change in divorce rates. However, as additional insurance, all of the predictors identified in the regression model were entered as covariates in the final analyses.

Design and Analysis

A pooled interrupted time series design was employed (Sayrs, 1989), using a full piecewise mixed-effects linear model (Hox, 2002; Raudenbush & Bryk, 2001) to determine whether the slope of divorce rates after the intervention differed from the slope before the intervention. The mixed effects model accounts for nonindependence of repeated observations on the same counties and differing numbers of observations available prior to and following the intervention. A two-level model was used to test the independent effects of several variables (Time, After, CMP) and their interactions. The model also controlled for the random differences that exist in the level of divorce rate between each county in the sample.

Examining the results of a mixed effects model—including fixed effects terms for Time, After, CMP, and all two- and three-way interactions—tested the hypothesis. The three-way interaction term (Time X After X CMP) tested whether Time X After or the change in the rate of decline from preintervention to postintervention, is different in the CMP counties compared with other counties. If Time X After X CMP is negative and statistically significant, the hypothesis holds. Having tested the model in this way, we then entered the identified predictors of divorce into the equation as covariates to see whether the results persisted. The marriage rate for 2000 was included as the covariate instead of the annual marriage rate to maintain consistency among the covariates (none of the other variables was available annually).

The Time X After and Time X After X CMP interactions were tested on data from 5 years prior to the intervention and up to 7 years after the intervention. The 7-year cutoff was chosen because data from policies beyond 7 years comprised only 1.5% of the total data, and truncating the data at any fewer than 7 years resulted in losing 10%–18% of the data for each year under this

Table 3
Comparison of CMP and Comparison County on Demographic Characteristics

		CMP County		Comparison County		
Variable	*N*	*M*	*SD*	*M*	*SD*	*t* value
Marriage rate	99	8.03	2.30	8.30	2.43	0.83
Median age	108	34.70	3.40	36.90	3.70	4.56**
Median family income	108	$50,061.00	$10,549.00	$47,065.00	$10,515.00	−2.10*
Percent adherents	109	.50	.13	.49	.16	−.17
Percent Catholic	105	.17	.11	.17	.14	−.47
Percent female	108	.40	.02	.40	.02	.65
Percent urban	109	.76	.20	.59	.28	−5.14**

*$p \leq .05$. **$p \leq .001$.

cutoff. It also should be noted that not all policies had the full 7 years of postintervention data available. The proportion of policies with given numbers of available years was distributed fairly evenly across years, with between 11% and 18% of the policies at any given year.

Results

The first test of our hypothesis used the two-level mixed effects model we specified. The result was that divorce rates in CMP counties declined by .084 per 1,000 per year prior to signing, and .144 per year after (a difference of −.060), whereas comparison counties declined by .095 prior to and .060 after the signing year (a difference of +.035). Testing the difference in these slope changes while controlling for demographic predictors of divorce identified above, the Time X After X CMP beta was −.095, $p = .010$; see Table 4. Figure 1 illustrates the expected values for CMP and comparison counties based on the equations yielded in this analysis.

Additional Models Tested

There are various analytical models that can be used to test this hypothesis. For example, in addition to controlling for random differences in the intercept values for different counties, we could have elected to control for random differences in the slope of the divorce rate over time prior to signing. A reviewer also suggested that the model would be more precise if it accounted for random differences between CMP/comparison county pairs and for random differences between counties. Thus, an additional three-level model was tested using these features: (a) fixed effects for Time, After, CMP, and all combinations of the interaction terms; (b) a variable to control for differences between all counties in the level of the divorce rate, both CMP and comparison counties (as in the original two-level model); and (c) a variable to control for divorce rate level and slope differences between pairs of matched CMP/comparison counties. The random Time effect was only included at the matched pair level, because we would not expect random differences in this effect when comparing CMP with comparison counties because we matched on that

Table 4
Estimates of Differences in Divorce Rates Between CMP and Comparison Counties Controlling for Demographic Predictors of Divorce

Parameter	β	*SE*	*T*	*p*
Intercept	23.50	4.55	5.16	.00
Marriage rate	.14	.03	4.57	.00
Percent urban	1.26	.36	3.56	.00
Percent Catholic	−4.86	.56	−8.60	.00
Median age	.03	.02	.22	.22
Median family income	−3.71	.95	−3.92	.00
Percent female	−8.92	3.40	−2.62	.01
Time	−.10	.02	−5.12	.00
After	−.02	.07	−.29	.77
CMP	.06	.16	.38	.70
Time X CMP	.01	.03	.26	.80
After X CMP	.16	.10	1.56	.12
Time X CMP	.04	.03	1.64	.10
Time X After X CMP	−.10	.04	−2.70	.01

Note: A few policies (13) were limited to just one year of data following the policy implementation. Calculation of the Time X After X CMP term is not affected by these policies because of this data limitation. Thus, the significant result (β = −.10) was identical with or without these policies.

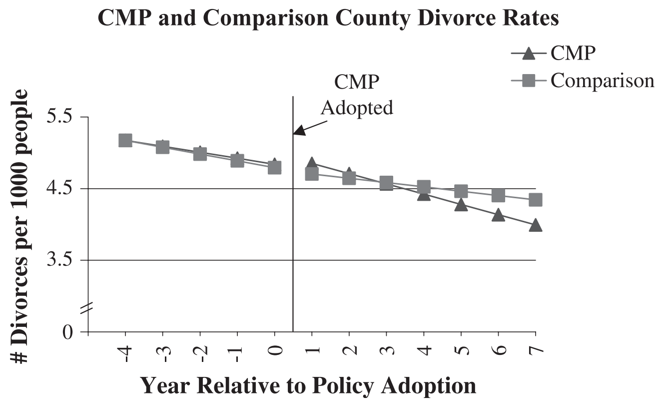

Figure 1. Graph of expected values based on original mixed model.

exact effect. These results were similar to that of the previous model with the Time X After X CMP effect being $\beta = -.10$, $p = .001$.

The CMP counties were matched with comparison counties on the basis of the preintervention slope of the divorce rate over time. This resulted in similar overall trend and level of the divorce rate prior to the CMP signing (see Figure 1). However, because differences between CMP and comparison counties in preintervention trends might still contribute spuriously to the overall program effect, an additional model was tested. This model added a random Time effect at the individual county level, resulting in a Time X After X CMP effect of $\beta = -.08$, $p = .009$. The variance attributable to the random effects entered into the model was larger at the county pair level than for individual counties within pairs, reflecting the effect of the matching procedure. This was especially true for the intercepts (1.52 versus .05) and for the time slopes (.01 versus .00).

We also employed each of the three models using a connected piecewise (or spline) model (Darlington, 1990). This model restricts the After and CMP X After effects to zero. The purpose of this additional test was to compare the connected model to the full piecewise model in terms of the fit of the data to the model. If they fit equally well, the intercept shift of the full models may be seen as anomalous to a few cases and not reflecting the overall pattern of data. We found no differences in the fit of these models compared with the earlier models. The effect for differential slope change also was similar (see Table 5). The spline model provides a more precise and parsimonious account of the program effects. The intercept shift (After X CMP effect; see Table 4) in the original model does not appear to characterize the overall pattern of data.

To further establish the appropriateness of the estimated models, we examined plots of the divorce rate means over time for counties with the same number of postintervention years of data (from 2 to 7 years following the policy signing). The pattern of means for CMP and comparison counties exhibited consistent downward trends and no evidence of nonlinearity, supporting the selection of the estimation models.

One concern related to the decreasing number of policies that have succeeding numbers of postintervention data available is that the overall effect might be attributable only to policies with more data available. No relationship was found between how many years of postintervention data were available and the likelihood of having favorable results. This conclusion was based on

Table 5
Estimated Effects Using Six Different Mixed Models

Model	# Levels with Random Slopes[a]	Program Effect[b]	*p*	AIC[c]	BIC[d]
Full piecewise	Both	−.08	.01	4016.4	4089.7
	Pair only	−.10	.00	4072.4	4140.1
	Neither (one random level)	−.10	.01	4640.8	4652.1
Connected piecewise	Both	−.07	.02	4016.6	4078.6
	Pair only	−.09	.00	4074.0	4130.4
	Neither (one random level)	−.09	.01	4638.8	4650.0

Note: The three-level models were tested twice, with the first having random slope terms at both the matched pair and the subject levels, and the second having a random slope term only at the matched pair level. The third version of each of the two models was the first model reported in the results section and has two levels: fixed effects for Time, After, CMP, and interaction terms; and a random intercept term at the county level only. [a]Time X After X CMP *beta* values [b]AIC = Akaike's Information Criterion; smaller relative values indicate that data fit the model better. [c]BIC = Schwarz's Bayesian Criterion; smaller relative values indicate that data fit the model better.

the fact that the beta estimates for groups of counties having the same number of postintervention data points were not significantly different from one another (Time X After X CMP X Years of data $\beta = -.016$, $p = .51$).

Table 5 contains parameter estimates, *p* values, and model fit information for the different models tested. The results produced by these different analytical models were quite similar. Although the fit of the data to the models was progressively better the more complex they were, using these different models did not produce more insight into the pattern than did the original model. This narrow range of results using several different estimation models increases our confidence that our findings are stable.

Practical Significance

Based on the results of the mixed models, supplementary calculations were carried out to illustrate the practical significance of the findings. The mixed effects equation was used to estimate the divorce rate for each year after the signing for both CMP counties and comparison counties. Recall that not all policies had the same number of postintervention data points available, with about half having < 4 and half having 5 – 7.

The first calculation translated the beta coefficient directly into an annual percentage change in divorce rates. This value is unaffected by the decreasing number of cases with data at each postintervention year. Prior to signing, CMP counties declined by an estimated 1.4% per year compared with 2.3% following the signing, a difference of −.9%. Comparison counties declined by 2.1% prior to the signing year compared with only 1.0% after (a difference of +1.1%). The difference in these two slope changes is −2.0% (i.e., the CMP counties change in the rate of decline was 2% lower than the change in comparison counties).

Table 6 contains the results of the second calculation, which provide the estimated number of divorces per 1,000 for the year prior to signing, the estimated divorce rate 4 and 7 years after signing, and the corresponding percentage changes in the divorce rates for the three full piecewise models. Although illustrative of policy effects, these results should be interpreted cautiously because fewer policies have data available for each succeeding year.

Based on the predicted divorce rate values yielded by the mixed model, we calculated how many fewer divorces occurred than would have had the trend in the CMP counties not accelerated greater than in the comparison counties. We found that the number of divorces that actually occurred differed depending on which of the six models was used, but averaged about 30,000 fewer than the 700,000 expected without the intervention. Because clergy and community leaders have now created 183 CMPs, that number could be 40,000 to 50,000.

Table 6
Estimates of Changes in Divorce Rates 4 Years and 7 Years After Signing; Full Piecewise Models Only

Random Slopes	County	Year Prior to Signing	4 Years After	Percent Change	7 Years After	Percent Change
Both	CMP	4.91	4.50	−8.4	4.19	−14.8
	Comparison	4.79	4.61	−3.8	4.48	−6.6
	Difference			−4.7	0.29	−8.2
Pair	CMP	4.84	4.47	−7.6	4.09	−15.4
	Comparison	4.79	4.58	−4.4	4.47	−6.6
	Difference			−3.2	0.38	−8.8
Neither	CMP	4.84	4.42	−8.6	3.99	−17.5
	Comparison	4.79	4.52	−5.6	4.34	−9.4
	Difference			−3.0	0.35	−8.1

Discussion

Our purpose was to determine whether county divorce rates change as a function of the adoption of a Community Marriage Policy. Our findings indicate that divorce rates appear to decline more rapidly following the signing of a policy than would be predicted by the passage of time alone. Whereas CMP counties evidenced acceleration in the divorce rate decline after the policy signing (from a decline of .084 divorces per 1,000 per year before to .144 after signing), comparison counties showed no such acceleration (decline of .095 before to .060 after). This effect translates to a modest but statistically significant decline in divorce rates.

The results reported here are important not because of their magnitude (which was modest), but because they are present. In reality, finding a significant program effect is surprising when the context of the program implementation is considered; volunteers implement the program, there is high turnover among those doing so, there is wide variation in the intensity of the program implementation, there is often a low proportion of signed congregations in the context of the larger county population, and this largely city-level intervention is only testable using the county statistics in which their results are embedded.

Further, given the many factors that could affect divorce rate declines over the last decade, to test the effects of any specific intervention to reduce them is a challenge. We attempted to account for and control these various factors while isolating CMP effects. These factors include those that predict divorce but are not readily amenable to intervention, such as urbanity, economics and the composition of the population. In addition, multiple and simultaneous efforts—such as state and federal level policies and practices, deliberate marriage-strengthening efforts (e.g., community marriage initiatives), activities of marriage educators and therapists, and concerned individuals and organizations with the power and influence to make a difference—complicate the isolation of the specific effects of this program.

We have considered several alternative explanations for the results, many of which were addressed by the design of the study (i.e., multiple communities, staggered years in which the policies were signed and inclusion of well-matched comparison counties). For example, it is possible that some other major event occurred to cause the observed changes. However, because policies were signed across multiple years and matched with comparison counties in the same state, it is unlikely that multiple events near the time of signing were responsible for the effects. One threat to the validity of the results that is not addressed by the design of the study is matching bias. A second threat is that communities with CMPs are different from comparison counties in ways that would affect the divorce rate independent of the intervention (i.e., selection bias).

Potential for Bias in Matching Procedure

A method used to control for matching bias was the inclusion of known demographic predictors of divorce as covariates in the analyses. Even though comparison counties were chosen on the basis of preexisting divorce rate trends, the analysis also controlled for differences in divorce that could be influenced by these predictor variables. Further, it could be argued that there was bias in our matching procedure that led to the selection of comparison counties that were different on other variables related to divorce, and not examined as part of the matching procedure. Those of most relevance were changes in marriage and cohabitation rates.

Marriage rates. Marriage rates are an important test of matching bias because of the obvious logical connection between marriage rates and divorce rates. If we found that marriage rates among CMP counties prior to policy signing were changing differently than rates in comparison counties, this might constitute at least a partial explanation for the observed results in the divorce rates. We conducted three tests to explore this possibility.

First, we examined whether there were differences in marriage rates between CMP and comparison counties. The average marriage rate in CMP counties was not different from the average rate in comparison counties for any given year from 1980 to 2001. Second, we tested marriage rates as a nonequivalent dependent variable (Shadish et al., 2002). The three-level mixed-effects model with random slope terms at the matched-pair level was chosen to examine differences in the trend and level of the marriage rates across the 5 years prior to the signing. Rather than report results of all three models (two-level, three-level with random Time effect at one level, three-level with random Time effects at two levels), we report only the midrange estimate. No differences were found in either the trend or level of preintervention marriage rates (Time X CMP for preperiod only $\beta = -.06$, $p = .11$; intercept for CMP counties = 8.43, comparison counties = 8.65, $p = .37$). There was no change in the rate of decline in marriage rates pre/post in either the CMP counties (marriage rate changes over time increased by a nonsignificant .03 divorces per 1,000 per year; Time X After $\beta = +.03$, $p = .29$) or in the comparison counties (.02 divorces per 1,000 per year reduction; Time X After $\beta = -.02$, $p = .58$). The Time X After X CMP effect also was not significant ($\beta = .05$, $p = .29$).

Second, when the marriage rate is measured and controlled for at only one point in time, as in previous analyses, it controls for differences in the level of the marriage rate but not the change over time. Controlling for the marriage rate as a time series covariate in a mixed model provides a more sensitive test. Results revealed that the program effect persisted when time series marriage rates (centered at the mean) were controlled for (Time X After X CMP effect = $-.10$, $p = .002$). This indicates that differences in the change in marriage rates over time between CMP and comparison counties do not explain the observed effect on divorce rates.

Finally, testing marriage rates as a factor rather than a covariate in the model allowed a test of whether the marriage rate, either by itself or in interaction with other design variables, might result in a reduction in the program effect. We tested a mixed model with Time, After, CMP, Marriage Rate, and all interaction terms predicting divorce rates. Presumably, if the Time X After X CMP term remained significant, it would indicate that divorce rates are changing differently in CMP counties even when controlling for changes in marriage over time and differential change in marriage rates between CMP and comparison counties. Results of this test indicate that the Time X After X CMP effect remains significant when controlling for differential change in marriage rates ($\beta = -.11$, $p = .001$). Because the Time X After effect was not found to vary as a function of marriage rates (as indicated by a weak and nonsignificant Time X After X Marriage Rate effect, $\beta = .00$, $p = .81$), it is unlikely that differential marriage rates are responsible for observed differences in divorce rates.

In summary, we concluded that marriage rates did not differ significantly between CMP and comparison counties, and that controlling for marriage rates did not reduce the program effects on divorce rates.

Cohabitation rates. Because research at the individual level has shown that marriages preceded by cohabitation are more likely to end in divorce (Bumpass & Sweet, 1995), it is possible that changes in aggregate-level cohabitation rates might be linked to changes in divorce rates. If there were a significant difference in the rates of cohabitation in CMP counties compared with the comparison counties, this difference might account for some of the differences in divorce rates.

Cohabitation rates for 1990 in CMP counties averaged 18.7, compared with 14.1 in comparison counties, $t(242) = 3.53$, $p = .01$. In 2000, the rates were 16.2 and 16.1 in CMP and comparison counties, respectively, $t(242) = .25$, $p = .80$. Apparently, cohabitation rates decreased significantly from 1990 to 2000 in CMP counties, $t(242) = 2.74$, $p = .007$, and increased significantly in comparison counties, $t(242) = 2.96$, $p = .004$. However, when this differential change in cohabitation from 1990 to 2000 was controlled in mixed models, Time X After X CMP remained significant. Various problems with cohabitation data (e.g., only available at census years, inconsistent methods of measuring from 1990 to 2000) make strong inferences unwarranted. However, these results suggest that cohabitation differences do not account for the difference between CMP and comparison counties.

Selection Bias

An important competing explanation for the divorce rate decline differences between CMP and comparison counties is selection bias. Because CMP counties chose to adopt the program, self-selection is operating to some degree. The program effect might be a function of differences in the marriage and divorce culture of the recipients of the program. It also could be due to differences in motivation, interest, or preexisting efforts of the clergy who organized the policy. These differences might effect the change even without a CMP. The extreme cases would be that either all or none of the difference was related to selection bias, with the reality probably somewhere in between. Therefore, we assessed the degree to which selection factors might account for the differences observed.

First, selection could affect the results if CMP counties were significantly different from comparison counties in terms of the marriage culture among program recipients. The first argument against this point is that even if faith community leadership were unique and self-selecting into the program, the actual program couples from which the divorce statistics are generated were not self-selecting. Therefore, it is unlikely that any systematic difference between recipients of the program and couples living in comparison counties accounted for the results. To support this assertion, we tested whether marriage and divorce rate trends prior to the intervention were different in CMP counties and comparison counties. Both marriage rate and divorce rate trends were found to be similar prior to signing. Further, there were no significant differences in the trend or level of marriage rates before and after the signing year for either group of counties. That the marriage rate decline held steady following signing reduces the likelihood that selection bias produced a sample of counties where divorce rate declines would have accelerated after the signing, even without the intervention.

Second, the clergy who signed the CMPs may have already been interested in strengthening marriage and were poised to do so, with or without a CMP. Interviews with several CMP organizers revealed that many had no plans to address marriage when they heard of a CMP, others had interest but no concrete direction, and some had a movement in place and the CMP became part of that movement (Institute for Research and Evaluation, 2002). Thus, although there are probably some CMPs that would have had favorable results without one, it is unlikely that all of the observed effect can be attributed to this group alone.

Further, even if pastors had a preexisting interest due to their concern about county divorce rates, the level and pattern of divorce statistics prior to intervention in the CMP counties were similar to the national pattern, and even more so to the comparison counties. In other words, there is nothing unique about the divorce rates in CMP counties prior to the signing of the policies, such that the pastors had more cause for concern than those in the comparison counties. Thus, although selection bias may be operating to some degree, we have no strong reason to believe that it had a significant effect on our results.

Conclusions

Notwithstanding the contextual factors that made finding a program effect unlikely, divorce rates in CMP counties were found to decline faster than those in closely matched comparison counties. Although the possibility of selection bias exists, we addressed this possibility. The CMP and comparison counties were similar in important ways, and we believe that the modest results can be tied to deliberate efforts to strengthen marriage. Apparently, the institution of marriage is amenable to community-based support efforts.

Implications

Individuals and organizations concerned with marriage have long hoped for the day when widespread public education initiatives could be established to help couples find needed assistance before it is too late (Burg, 2004; Doherty & Anderson, 2004). The main implication of our study is that such efforts to change the culture of marriage and divorce at the community level have potential. Estimates of the reductions here are that divorce rates decrease by more than 2% per year more in CMP counties than in comparison counties. This effect was observed even though policies were probably not equally well implemented, and not all community members were reached by the policies. It may be that if similar efforts were undertaken more systematically, reached a majority of the population, and used the best practices available, divorce rates might be substantially lowered.

We found some evidence that counties with higher percentages of religious adherents and average or higher than average marriage rates have larger effects. However, it is difficult to tie this directly to the program because of limited program implementation data. Communities in which there is higher concern and a demonstrated history of collaborating to address community problems likely will have better results than those communities devoid of these features.

The main limitation of our study was the lack of program implementation data to describe the actual program operation and establish a dose–response relationship to show that as implementation strength increases, results improve. Finding a program effect in spite of this lack of implementation data was quite surprising. Finding an effect without a good measure of program implementation may actually indicate that some well-implemented policies have had much stronger results than were reflected in our analyses. Nonetheless, implementation data would strengthen the causal argument, and thus should be a primary goal of future research.

The potential for selection bias also deserves further examination. Our estimates of effects on the divorce rate are based on the presumption that the effect is being caused by a deliberate effort to strengthen marriage, and not just a reflection of nonsystematic changes in divorce rates.

Another limitation was the overall data quality and completeness of the aggregate data at all levels: county, state, and national. Although we have reconstructed a heretofore unavailable database, we hope that an adequately funded government effort with sufficient quality control mechanisms in place could do a more thorough job and maintain it over time. Finally, that data were only available at the county level for a policy originating and largely operating at a city level may have underestimated the effects.

Recommendations

More rigorous testing of community marriage initiatives, will require improvement in the quality, completeness, and consistency of data. In addition, it will be necessary to study these movements in more depth to see what components are most important and what methods are most effective for saturating a community with new information and for effecting change in existing practices. This will require collecting data from community members and leaders. Further, complete and accurate data regarding program implementation levels are necessary, so dose-response relationships can be tested, and studies incorporating random assignment are desirable (although difficult at the county level). Predictive models of divorce that include more time series data (such as those available in economic survey data) also would help to model changes in divorce rates over time, resulting in more precise choice of covariates. More details and recommendations for evaluating these types of movements can be found elsewhere (Birch et al., 2004).

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