ADDITIONAL ARTICLES

A RIGOROUS QUASI-EXPERIMENTAL DESIGN TO EVALUATE THE CAUSAL EFFECT OF A MANDATORY DIVORCE EDUCATION PROGRAM

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All couples with minor children who filed for divorce within a specific 6-week period ($N = 191$ couples) in one jurisdiction were ordered to attend a divorce education program. The control group included about 20 couples randomly selected from each of six 6-week intervals before and six 6-week intervals after the treatment interval ($N = 243$ couples). Archival records were searched for variables such as legal and residential custody award, visitation percentage, and relitigation. The impact of the program was assessed by evaluating, for each variable, whether the data for program interval departed from the straight (regression) line drawn through all the control group intervals. Only the visitation time award significantly differed: 27.75% for treatment couples and 22.46% for control couples. Analyses show that the father's attendance at the program primarily accounts for the difference.

Key Points for the Family Court Community:

- There are considerable methodological weaknesses in most of the existing evaluations of divorcing parent education programs.
- Stronger, more scientifically rigorous—and thus persuasive—designs are possible in court settings, such as the regression discontinuity quasi-experimental design we feature here.
- Archival records, such as various court filings, are a rich and relatively untapped source of data.
- Being mandated to attend a single 2-hour divorcing parent education class caused an increase in the visitation time award in divorce decrees.
- There is a disconnect between being mandated by a judge to attend a program and actual attendance.

Keywords: *Divorce Education*; *Experimental Design*; *Parenting Program*; *Program Evaluation*; *Quasi-Experiment*; *Regression Discontinuity*; *Relitigation*; *Visitation*

INTRODUCTION

Between 40% and 50% of marriages in the United States end in divorce (Cherlin, 2010). The potential negative effects of divorce on minor children, including adjustment problems such as childhood depression, conduct problems, and academic difficulties, have been well-reviewed elsewhere (Amato, 2000; Clarke-Stewart & Brentano, 2006). Because the divorce process itself can be fraught with potential conflict and stressful changes (Braver & Lamb, 2012), parents should benefit from programs that prepare them for coping with factors such as the child's adjustment and age-appropriate communication about the divorce, as well as the potential economic hardship, new involvement patterns, and parenting skills required of both the custodial and noncustodial parents (Braver, Hipke, Ellman, & Sandler, 2004; Braver, Shapiro, & Goodman, 2006).

In response to that belief, divorcing parent education programs (DPEs) that address issues like these emerged in the 1970s (Pollet & Lombreglia, 2008; Salem, Sandler, & Wolchik, 2013) and experienced a rapid uptick in growth in the 1990s (Braver, Salem, Pearson, & DeLusé, 1996; Geasler

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& Blaisure, 1999; Salem et al., 2013). The most recent survey reports that 46 states had court-affiliated parent education programs in the late 2000s (Pollet & Lombreglia, 2008) in the hopes of reducing negative outcomes from divorce for both parents and children. Despite the increase in the number of parent education programs, few methodologically rigorous evaluations of such programs have been conducted (Salem et al., 2013). A recent meta-analysis of evaluations of parent education programs reported positive effects on a wide range of outcomes, but acknowledged that the quality of methodology in the studies in the meta-analysis was poor (Fackrell, Hawkins, & Kay, 2011). There is a consistent call for more rigorous program evaluation of DPEs to substantiate claims that they help stem the potential deficits that can accrue to divorcing parents or their children (Fackrell, et al., 2011; Pollet & Lombreglia, 2008; Salem et al., 2013). We next briefly review DPEs, the quality of evaluation, and methodological concerns, then present the results of a DPE program evaluation that utilizes a rigorous quasi-experimental design that allows strong causal conclusions about program effects.

DPE: AN OVERVIEW

A wide variety of DPEs have been developed. For example, programs range from mailing an informational booklet to parents when they file for divorce (Arbuthnot, Poole, & Gordon, 1996) to in-depth 8- to 11-week programs available later in the divorce adjustment process, including both group and private sessions with ample opportunity for skill building (Braver, Griffin, & Cookston, 2005; Wolchik et al., 1993; Wolchik et al., 2000). The bulk of the programs, however, offer one to two sessions of 2–4 hours in length, and if more than one session is involved the sessions typically occur no more than a week apart (Braver et al., 1996; Fackrell et al., 2011).

The content of DPEs is determined in part by the length of the program and in part by the various stakeholders connected to the programs. Depending on the program's emphasis, its content may include material for parent-focused goals, child-focused goals, and/or court-focused goals. Parent-focused goals include, for example, improving parents' adjustment to divorce through normalizing the event, improving communication and parenting skills, and reducing conflict. Child-focused goals might include increasing the parents' awareness of how divorce affects children, how to keep children out of the middle of parental conflict, the importance of visitation and child support, how to otherwise create a safe environment for children, and in turn helping to prevent or reduce internalizing and externalizing child behavior problems. Court-focused goals involve helping parents understand court procedures and options, resolving custody and visitation issues, and reducing litigation. Salem et al. (2013) propose considering programs through a public health framework, whereby Level 1 programs are primarily informational and generally brief; Level 2 programs aim to enhance skills and last longer, over multiple sessions; and Level 3 programs which include even more experiential skill-building activities and are targeted to specific subgroups of parents, for instance higher conflict parents.

In addition to the variety in the length and content of DPEs, there is also variation in how parents come to be included as participants in a program (Pollet & Lombreglia, 2008). For instance, there are programs where attendance is completely voluntary (e.g., Wolchik et al., 2000), and these are typically run by outside agencies or institutions that are not connected with domestic relations courts. Other programs are clearly connected with the courts, being either operated by the court itself or by outside providers under contract from the court (Braver et al., 1996; Geasler & Blaisure, 1999). Of these programs, many are available on a voluntary attendance basis, but most operate in jurisdictions where the courts mandate attendance for all divorcing parents with minor children, or for those who are conflicted over child-related issues. Indeed, most DPEs are court related. Whitehurst, O'Keefe, and Wilson (2008) describe a study where some parents were mandated into two DPEs based on their needs; first, parents attended a 2-hour program and then, if the presiding judge deemed that the couple was "keeping their conflict" alive, they were ordered into another 2-hour per week program that lasted 6 weeks.

The courts being involved in the support and referral of parents to divorce education programs is natural. During the stress of a divorce, many families can benefit from additional information about, and support for, what is happening in what is often an emotionally and legally complicated process (Sigal, Sandler, Wolchik, & Braver, 2011). In addition to the potential personal ameliorative effects of parent education, success in these programs is hoped to lighten the burgeoning load on the family courts by both initially solving key issues outside of the courtroom and reducing the return to court for adjustments to an original decree or for problems with child support compliance (Salem et al., 2013; Schepard, 2004). While it is well established that high levels of interparental conflict impact children negatively, there has not been a great deal of research on how DPEs relate to the relitigation load on the courts (Goodman, Bonds, Sandler, & Braver, 2004). Fackrell et al. (2011), in their meta-analysis of DPEs, found only six control group studies and only two pre–post design studies that considered relitigation. They posit that the dearth may be due to the increased time required to measure such variables, which in turn requires greater patience and resources. The present study includes this variable.

Although the aims and importance of divorce education are rarely questioned, whether to make such education mandatory or voluntary is often the subject of discussion. Some critics contend that mandatory attendance is unnecessary in uncontested cases, and that the resources necessary to service so many people are not negligible, while others point out that mandatory attendance raises legal issues about the power of the court in general and, in particular, what sanctions will be levied against noncompliant parents (Salem et al., 2013; Schepard, 2004). Among the participants, the parents ordered to attend a class frequently report that they initially resented "being forced" to go, but upon completion, the vast majority have shifted to a more favorable position, feeling that the experience was worthwhile (Braver et al., 1996). Experts in the field believe that mandating attendance, or at the very least having a "strong recommendation" from a judge, is important because it is otherwise unlikely parents will attend (Geasler & Blaisure, 1999; Pollet & Lombreglia, 2008). Although there are philosophical and jurisdictional differences, the trend would appear to be moving toward broad mandatory attendance, with at least some categories of cases being mandated with certainty.

Having established that court-related programs are becoming more common, it is reasonable to question "How (well) do they work?" Given the wide variety of program styles and content, a simple answer is not evident (Fackrell et al., 2011; Sigal et al., 2011). One might assume that the content and length of programs will greatly impact what kind of results might be expected. The strength of very brief programs, for instance, may be in raising parental awareness of issues they can have personal impact on, and in motivating them to seek further education or assistance (DeLusé, Braver, & Sandler, 1995; Kramer & Washo, 1993; Salem et al., 2013). Conversely, longer programs may have more impact in skill development, as would brief programs that intentionally focus on one or two specific skills as opposed to covering a wide range of information (Kramer & Washo, 1993; Kramer & Kowal, 1998). The question of how effective the many different DPEs are, however, remains an open question to many because relatively little extensive and/or rigorous evaluation has been conducted. An Association of Family and Conciliation Courts survey (1994) found that while a majority of programs did some sort of immediate "consumer satisfaction" exit survey with parents, only 19% did a longer term follow-up and only 5% compared program outcomes with some kind of control group. Echoing the broad survey, Geasler and Blaisure (1995), focusing on court-related programs in Michigan alone, found that only 2 of 40 programs made formal evaluation efforts, while 24 conducted variants of a consumer satisfaction survey. While consumer satisfaction is important, more in-depth evaluations that probe whether the DPE is actually causing change in the parents offer better insight. A recent meta-analysis of empirical studies of court-affiliated DPEs found only a handful of studies that met the authors' criteria for effective evaluation such that their report rests on 19 studies with a control group, and nine that included a one-group/pre–post design (Fackrell et al., 2011). Thus, there is considerable room for improvement in the evaluations performed. In seeking to determine effectiveness, the evaluators must keep in mind both what the goals of the key stakeholders are, and what is reasonable to expect given the scope of the program both in terms of content and time length (Braver, Smith, & DeLusé, 1997). Beyond that, the nature of the research design also relates to the quality of the results.

RESEARCH DESIGNS FOR EVALUATING COURT-RELATED PROGRAMS

In our primer on methodological considerations for such evaluations (Braver, Smith, & DeLusé, 1997), as virtually all expositions do (Campbell & Stanley, 1963; Shadish, Cook, & Campbell, 2002), we distinguish three categories of research designs: preexperimental, experimental, and quasi-experimental. Most evaluations of DPEs in the literature used the first category, which is generally considered to permit such a small degree of conclusiveness that it is of "almost no scientific value" (Campbell & Stanley, 1963, p. 6). The second category, (true) experimental designs, require control group(s) and random assignment. While these much stronger ("gold standard") designs flourish in many other domains (e.g., medicine, mental health, programs addressing criminality), in the context of DPEs there is only one published example (Whitehurst et al., 2008), primarily because courts are reputed to be inimical to assigning some parties to the real group and others to a control, entirely on the basis of random chance. However, the third category, quasi-experimental designs, though they have rarely been employed, have much to recommend them scientifically and are well suited to evaluating DPEs. Among this category, two designs that are considered especially strong (Cook & Campbell, 1979; West, Biesanz, & Pitts, 2000) are the interrupted time series and the regression discontinuity (RD) design. (The two are virtually indistinguishable for our purposes.) In this design, there is a prolonged baseline set of periods during which outcomes are repeatedly assessed; then the experimental program is introduced and outcomes again assessed; if possible, outcomes continue to also be assessed for many equivalent intervals after the program ceases. Then, one examines whether a "discontinuity" (a "jump" or "blip") occurs at the exact time of the program initiation on the outcomes under study. Regression statistics are available to assess whether the departure from the general trend is greater than would be expected by chance, that is, statistically significant. The "internal validity" of such a design (i.e., the ability to make unbiased causal inferences as to program effects) is considered virtually as high as in experimental designs if its assumptions are met (Shadish et al., 2002).

THE PRESENT STUDY

The above review of literature demonstrated that: (1) there are considerable methodological weaknesses in the bulk of the existing evaluations of court-related DPEs and (2) there is a paucity of research on postdivorce and systems level court-related outcomes in connection with the conduct of these programs. The present study expands the court-related divorce education literature by evaluating the impact of a mandatory DPE in Maricopa County (the Maricopa Experimental Parenting Program [MEPP]) on postdivorce legal outcomes, including the occurrence of relitigation, using a rigorous RD quasi-experimental design.

METHOD

PARTICIPANTS AND SAMPLE ACQUISITION

A research partnership was established with the Maricopa County Superior Court of Arizona. The presiding judge agreed to order each of approximately 200 couples (~400 parents) with minor children who petitioned for divorce within a specified 6-week interval (and only that interval) to attend the MEPP class.[1] Of these, 191 were in fact so ordered and are henceforth referred to as the treatment group. The control group consisted of about 20 families with minor children drawn randomly from those petitioning in each of the six consecutive 6-week intervals before and six consecutive 6-week intervals after the filing dates for the treatment group. In other words, 243 families (about 20 from each of the 12 6-week intervals) were in the control group and, when combined with the 191 in the treatment group, yielded a total sample size of 434 families.

PROCEDURE

MEPP

The parents in the treatment group came to one of eight available sessions, each with 2 hours of program content. Each MEPP session was presented by two highly experienced personnel from conciliation services, a male and a female. The program was a typical one, in that it presented similar content material and structure (parent as passive listener instead of active participant) as the majority of programs of equivalent length currently operating in this country (Braver et al., 1996; Geasler & Blaisure, 1999). Specifically, most of the content was aimed at the overarching theme of positive, nonconflictual ongoing contact with the nonresidential parent.

Data Collection

The outcome data for this study are archival: the divorce decrees and any parenting plans in the files (which are public information) were photocopied for coding. The file was also examined from petition date to 18 months postdecree for frequency and type of litigation. Information on seven divorce outcome variables, such as the custody configuration and the legal planned visitation schedule were coded from the divorce decree and/or parenting plan. (Note that we refer here to the de jure arrangements, which can be distinguished from the de facto or actual visitation schedule followed by the couple.) Six coders, blind to condition and hypotheses, were trained to code the variables from the data sources. Kappa statistics were calculated and all were above .95, indicating that excellent intercoder reliability was achieved.

THE SEVEN DIVORCE OUTCOME (DEPENDENT) VARIABLES THAT WERE CODED

Custody and Visitation

Three variables were coded from the divorce decrees and/or parenting plan: physical custody, legal custody, and the visitation percentage (i.e., the percent of time the children were scheduled to be in the care of the noncustodial or nonresidential parent). Both the physical and legal custody variables were coded into two categories: 1 = mother named sole custodian and 2 = father named sole custodian, joint custody, or split custody[2] (i.e., father has some custodial responsibility).[3] Visitation percentage was coded by examining the parenting plans and divorce decrees for the access/visitation verbiage or schedules and then summing the access in accord with the Maricopa County Guidelines in effect at the time. Specifically, according to these guidelines, an overnight or 12 consecutive hours was counted as one day, more than 4 but less than 12 hours was counted as a half day, and 4 hours or less was counted as a quarter day. The total days were then added and then divided by 365 to arrive at the visitation percentage. Take, for example, a case where the visitation award specified “every other weekend from 3:00 p.m. Friday to 8:00 p.m. Sunday, and one weekday evening per week from after school until 6:30 p.m.” For the weekends, we credited 2 days (through Sunday at 3:00 pm), plus .5 day (for the remainder of Sunday) for 26 weeks (= 65 days). For the weekday visit, we added .25 days for 52 weeks (= 13 days). This sums to a total of 78 days and a 21 percent visitation percentage (78/365). This 21% was also the value recorded when the records stated “reasonable visitation according to court guidelines” or variants thereof. Summer or other vacation weeks were coded and added in when they were specified, with care taken not to double count visitation. On occasion, a visitation schedule would say, “one or two visits a week.” In cases like these, two visits were coded to reflect the opportunity for access.

Relitigation

Four types of relitigation actions were noted: requests to (1) enforce child support, (2) enforce visitation, (3) modify visitation, and (4) modify custody. The occurrence of relitigation was so scarce

(41 total instances from the entire sample of completed divorces; no more than 6% for any request type) that the relitigation variable given to the family was dichotomized such that 0 = no relitigation requests and 1 = at least one such request.

Divorce Status, Divorce Length, and Total Legal Activity

By divorce status, we mean whether the divorce was finalized. We treated it dichotomously such that a zero was assigned if the divorce had completed, and a one was assigned if the divorce had been dismissed.[4] The divorce length variable (i.e., how many days or months it took for the divorce to be completed, for those who did so) was calculated by subtracting the divorce petition date from the divorce decree date. Finally, total legal activity was a count of the number of legal documents recorded in the file from the petition date to the 18-month postdecree date.[5]

OVERVIEW OF ANALYSIS

As noted in West, Beisanz, and Pitts (2000), the RD design is analyzed using a multiple regression equation.[6] As adapted to our case, one attempts to predict a divorce outcome for each couple from both the petition date interval and the treatment condition:

$$Y = B_0 + B_1A + B_2G + e$$

where Y is the value of one of the divorce outcome dependent variables, A is the "assignment" variable (in our case, within which 6-week interval the couple filed their divorce petition, from 1 to 13), and G is a dichotomous "dummy" variable indicating which Group the couple was in (where G = 0 for the control group, i.e., represented by intervals 1–6 and 8–13; and G = 1 for the treatment group, the group of parents ordered to attend the MEPP, i.e., interval 7) and e is random error. The B coefficients (and the e) are to be estimated from the data, where B_0 is a constant, B_1 is the regression coefficient predicting the divorce outcome from the interval of the petition date, and B_2 is the regression coefficient predicting the divorce outcome from the group. If and only if the MEPP had an effect on the outcome, there should be a discontinuity in (or noticeable departure from) the regression line (B_1 represents the general slope or trend of that line) at precisely the interval where the MEPP was delivered. Thus, if there is a treatment effect, B_2 should be significantly different than 0.[7]

RESULTS

SAMPLE CHARACTERISTICS

Of the total sample of 434 families, the majority (n = 347, 80%) completed their divorce process in the 18-month postdecree litigation period, while a minority (n = 65, 15%) allowed their divorce petition to be dismissed. The remaining 5% of cases was not included in the final analysis because their divorce took so long to resolve that the 18-month postdecree period was not completed by the end of the study period (n = 7), there were no minor children by decree date (n = 5), or there was divorce-related activity that was initiated prior to the petition interval date that defined the sample (n = 10). Thus, the final sample size for analysis purposes was 412 families (234 families in the control group, 178 families in the treatment group) representing both completed divorces and dismissals.

The couples in the total sample had an average of 1.7 children (47% had only one child, 36% had two children, 12% had three children, 4% had four children, and 1% had five children), with an average age of 8.3 years,[8] ranging from 2 months to 18.5 years old. Within the completed divorces, there were 314 male children and 295 female children. The monthly child support award averaged $413 ($SD$ = $329), with median = $372. In 49% of the sample, neither parent had a lawyer; in 22%, mothers alone had lawyers; in 11%, fathers alone had lawyers; and in only 18% did both parents have an attorney.

We turn next to the remaining divorce outcome variables. With respect to physical custody, 78% had sole mother custody, while in the remaining 22%, the father had some custodial responsibility. For legal custody, those two percentages were 47% and 53%, respectively. The visitation percentage specified in the decree ranged from 0% to 62%, with a mean percentage of 25% (*SD* = 14%); the modal visitation percentage was 21%. The divorce length variable ranged from 2 months to 3 years with mean divorce length of 8 months (*SD* = 6.6). Eighty percent of couples had no relitigation. Total legal activity, the count of the number of legal documents in the file, ranged from 5 to 137 entries, with a mean of 25 (*SD* = 20).

HYPOTHESIS TESTS: INTENT TO TREAT (ITT)[9]

The most conservative analyses to determine presence or absence of an effect of a program is termed the ITT approach. This approach addresses whether there is an impact for being ordered to attend the MEPP class, regardless of whether or not either or both parents actually attended (Shadish & Cook, 2009; Newell, 1992). If the couple was sent an order by the court to attend, then that family was counted as being in the treatment group for purposes of ITT.

Table 1 displays the regression coefficients for these primary analyses.[10] The table indicates that B_2, Group, produced a significant discontinuity (i.e., "jump" or "blip") for (and only for) visitation percentage. In concrete terms, the treatment group had a mean visitation percentage of 27.75%, whereas the control group had a mean visitation percentage of 22.46%. The 5.29% difference translates into 23.5% more visitation time (5.29% ÷ 22.46%) awarded to the nonresidential parent. Figure 1 presents the results graphically for visitation percentage. Note the trend (or regression) line has a slight (and not statistically significant) decline over time. Because all sample statistics are subject to sampling error, it is helpful to present the 95% confidence interval as well, represented here by the shaded area. We can be 95% confident that the true (i.e., population) regression line fits somewhere within it. Note finally that the seventh interval (the one within which every couple was ordered to the MEPP) is noticeably above that area indicating, in this case, a significant treatment effect.

None of the other divorce outcome variables had a significant B_2, implying no detectable effect or discontinuity of being ordered to the MEPP. Thus, being mandated to a DPE class did not, in our data, impact custody (of either variety), divorce status, divorce length, relitigation, or total activity. (For that matter, on *no* divorce outcome was there an effect of Date Interval, B_1, implying no discernable upward or downward trend over the 13 6-week intervals of the study).

Table 1
Regression Coefficients and (Standard Errors) for the ITT Analysis

Predictors	*Dependent Variables*						
	Physical Custody[†c]	*Legal Custody*[†a]	*Visitation Percentage*[a]	*Divorce Status*[†b]	*Divorce Length*[c]	*Relitigation*[†c]	*Total Activity*[b]
Date Interval (B_1)	.0525	.0216	−.2210	−.0363	.0214	−.0524	−.3992
	(.0461)	(.0375)	(.2499)	(.0449)	(.1222)	(.0563)	(.3346)
Condition (B_2)	.1171	.1666	5.2859*	−.2253	−.5940	−.2251	.6619
	(.2622)	(.2172)	(1.450)	(.2776)	(.7063)	(.3416)	(1.980)

[†] indicates coefficients obtained by logistic regression.
[a]n = 346; [b]n = 412; [c]n = 347.
*p < .001.

PARENTAL ATTENDANCE

It was mentioned above that the ITT approach is viewed as a conservative estimate of the intervention's effect. This is so because it waters down whatever effect the program actually has on

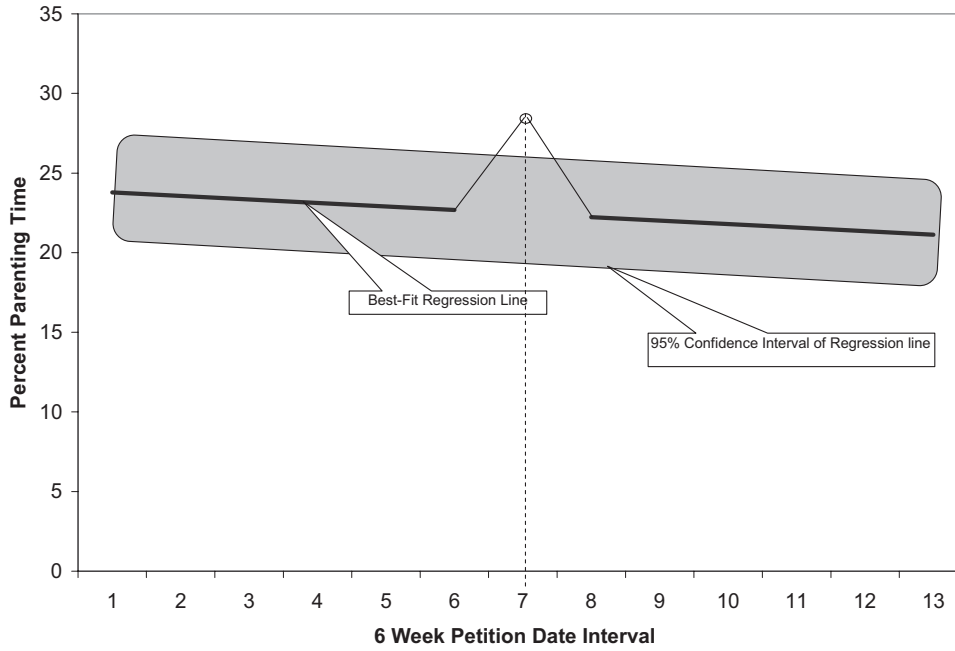

Figure 1 Findings for visitation percentage. Because all sample statistics are subject to sampling error, we show both the sample's single line of best fit and the 95% confidence interval (we can be 95% confident that the true population regression line fits somewhere within it). If there were no effect of the MEPP then the values for all 13 intervals would fall on the same regression line. The spike at interval 7, however, is outside the shaded area, indicating the significant treatment effect of the families who received the MEPP.

Table 2
Attendance (Percentages) Amongst the 178 Treatment Group Couples

			Did Dad Attend?			*Total*
			No	*Yes*	*No, excused*	
Did Mom Attend?	No	*N*	33	20	0	53
		% of Total	18.5%	11.2%	0.0%	29.8%
	Yes	N	25	94	2	121
		% of Total	14.0%	52.8%	1.1%	68.0%
	No, excused	N	1	1	2	4
		% of Total	0.6%	0.6%	1.1%	2.2%
Total		*N*	59	115	4	178
		% of Total	33.1%	64.6%	2.2%	100.0%

Note: Excused parents formally acknowledged the order but received permission to not attend for various reasons. For our purposes, excused parents were recoded simply as not attending.

those who actually attend, with the "no effect" it must have had on all those who did *not* attend. Recall that, while the judge ordered every couple in the seventh interval into the class, far from every parent so ordered complied and attended. Table 2 shows how many parents in each family in the treatment group interval actually attended. As can be seen, in only 52.8% of families ordered did both parents actually attend.

Table 3
Average Visitation Percentages for Treatment Group, Broken Down by Actual Parental Attendance at the DPE

		Did Dad Attend?		*Average*
		No	*Yes*	
Did Mom Attend?	No	22.75	27.6	25.17
	Yes	21.32	30.15	25.74
	Average	22.03	28.88	

Table 3 reports the average visitation percentage for the treatment group (only, not the control group), broken down by the actual attendance. Whether the mother attended made little difference, on average. When the mother did not attend, on average, the visitation percentage was 25.17. It increased only trivially (and nonsignificantly, $F(1, 343) = .067$, $p = .80$) to 25.74, on average, when she *did* attend. The story was quite different concerning the father's attendance. Visitation percentage increased significantly, $F(1, 343) = 9.99$, $p < .01$, from 22.03 to 28.88, when the father attended. When neither attended, the visitation was 22.75, virtually that of the control group. While it appears from these average visitation percentages that mothers' attendance amplified the effect of fathers' (the average visitation percentage increased from 27.60 to its highest value, 30.15, when both attended) this increase did not attain statistical significance, interaction $F(1,343) = .85$, $p = .36$.

DISCUSSION

The key finding of this study is that being ordered to attend this mandatory DPE resulted in a significantly higher percentage of visitation access being awarded to the nonresidential parent, which in this sample was the father 78% of the time. Specifically, the control group had an average visitation percentage of 22.46%, whereas the treatment group had a 5.29% increase to an average visitation percentage of 27.75%; the difference represents 23.5% more visitation time awarded to nonresidential parents. This finding resulted from using the ITT approach, in which couples *ordered* to attend were counted as in the treatment group whether or not they actually attended. Because, in over 20% of the treatment group couples, neither parent ultimately did attend, eliminating these couples suggests an even stronger effect would be in evidence.

This finding is unique in that, of the court-related program evaluations reviewed for this paper, none examined actual custody awards and only two studies began to broach the issue of parenting time or visitation in a parent self-report or predictive manner. Specifically, Buehler et al. (1992) found a trend toward an increase in length of visitation when they asked parents "How long are the visitation periods usually?" Similarly, Arbuthnot and Gordon (1996) found that treatment group parents report being more willing to have the child spend more days with the other parent in the next year than did controls. These evaluations, however, were hampered by weaker research designs, self-selection biases, and relatively small samples. It is encouraging that the present study confirms and extends the strength of visitation related findings.

Investigating the separate impact of each parent's attendance sheds more light on the process involved in the visitation award finding, revealing that it is the father's attendance (not the mother's) that relates to the increase in visitation percentage. Why might this tendency have occurred? There are several possible interpretations. First, the emphasis of the particular DPE evaluated in this study was to sensitize parents to the importance of encouraging ongoing, nonconflictual contact with the nonresidential parent and emphasized the father's importance in the child's life. Thus, the father's attendance may have persuaded him to seek greater access to his children. Indeed, one could wonder,

and test in a future study that varies the content of the course, if realizing that the court views fathers as being as important as mothers, fathers may simply ask for more time with the child(ren). Alternatively, it may not be the *substance* of what a father learns at the program that is responsible for the result; it may be the fact that it is the court that is doing the teaching that might be critical. Thus, a father's attendance may have empowered the father by offering him evidence, as it were; perhaps quoting the "authority" of the program to counter any opposition a mother may have to him seeking access. An additional possibility targets not the course itself but who attends it. It is possible that families where fathers choose to attend are simply the kind of families that agree to a greater visitation percentage. Or, perhaps the mother who knows a father has attended may see him as being a more dedicated father who deserves more time with the child(ren). The data available from this investigation do not allow us to choose between these and other possibilities concerning the mechanism by which this effect occurred, but the fact remains that the program had a significant effect in visitation percentage awarded.

Of course, there may be some debate about whether increasing the parenting time award is a good or a bad thing. While some disagree, most observers appear to feel that, at least in cases that do not involve domestic violence or excessive conflict,[11] the child having more time with the nonresidential parent is beneficial to the child's well-being. To be sure, there is no way to ascertain from this investigation whether the parenting time awarded matches the parenting time in actual practice. Thus, a higher percentage of time being legally awarded only means the opportunity is there.

In terms of the other dependent variables, when using the preferred ITT approach, the MEPP neither impacted the length of time until reaching a divorce nor the total amount of legal activity. Likewise, this investigation revealed no effects on relitigation. In fact, there was a low base rate of relitigation within the sample. Taken together, the above null findings, in view of our rigorous research design and very specific operationalization, if generalizable to other, perhaps longer DPEs, are perhaps as meaningful to the field as having found differences. Although the MEPP did not appear to reduce the burden on the legal system, as some have hoped, there is also no evidence to suggest that it increases the burden on the court, either. Finally, "some have claimed that once exposed to the information dispensed routinely in divorce programs, divorcing spouses will be more realistic about the harm, difficulties, and disadvantages of divorce, and this will cause the more frivolous or impulsive cases to reconsider and reconcile" (Braver, Smith, & DeLusé, 1997, p. 14). In this study, however, we found no evidence that the MEPP program attendance influenced whether or not a divorce was terminated.

This investigation is the only mandatory DPE evaluation that utilized the actual divorce decrees and parenting plans as a source of data. These documents seem to hold promise as a resource for other evaluators as there is no missing data, at least not in the present study. With the exception of relitigation, no other evaluation has specifically examined whether these programs influence choice of custody or amount of de jure parenting time, or any of the other legal variables described above. Notwithstanding, the study could have been improved by the addition of numerous other variables such as child adjustment, parent adjustment, conflict, coparenting, and actual (de facto) contact between the child(ren) and the nonresidential parent. Clearly what parents are ordered to do and what actually happens might be two different things, but examining archivally what the parents agreed to, or were ordered to do, in court still offers value in understanding an impact of DPEs. Notably, the collection of these variables requires surveying, preferably *both* parents and possibly the children, which, in turn, requires substantial resources and requires technical acumen to deal properly with nonresponse rates, missing data, and self-report biases. Given the problems with those, combined with the rigor of the present study, one could argue that the results of this study are *more* generalizable in some respects.

Additional limits on one's ability to generalize from the current findings, besides the exact content of the course and the specific outcome variables explored, should be mentioned. One of these concerns aspects of the sample. We had only a limited amount of demographic data available in the archival data so we could not explore differential effects across ethnicity, education level, and religious preference of parents. Future researchers should attempt to collect these data through other means.

An interesting and somewhat unanticipated finding was the disconnect between being mandated by the judge to attend the DPE and actual attendance. Omitting those formally excused from attending, only about two-thirds of those ordered actually attended. This should alert courts to the necessity of verifying compliance with their orders generally and, specifically, with the order to attend the DPE. Many jurisdictions (including ours, after this study was completed) have put into place mechanisms to assure and verify compliance and otherwise to impose sanctions. Substantial noncompliance also raises research questions about differences between the compliers and noncompliers which then results in complexities about how properly to evaluate, in our case giving rise to the ITT approach.

As mentioned above, a common problem with evaluation of court programs is the lack of the use of any kind of control group. The field would be improved by moving toward designs where strong control groups are employed to help answer the all-important "compared to what?" question. The gold standard for most evaluators is random assignment, but this is often precluded in court settings. We have presented here the RD design, a quasi-experimental design regarded as a strong and rigorous alternative to randomization, that can readily be employed in court settings, where assigning by need, merit, or chronological order of entry (as in the current study) is feasible. The *sine qua non* of the RD design is that the mechanism for assignment to condition is both quantifiable and rigidly adhered to. In our case, the 13 petition date intervals were indeed both quantifiable and adhered to rigidly. (It was also beneficial, but not necessary, that the seventh treatment interval was followed by several additional control intervals.) When these conditions—and certain others, such as noncurvilinearity—are met, the RD design permits strong inferences about the causal impact of the Treatment virtually as strong as for true experiments. We strongly recommend that courts consider such designs (if not random assignment) in the evaluations of its various endeavors. The current (RD) study could serve as a model for other jurisdictions aiming to more rigorously evaluate their programs, but who must do so without a randomized design.

NOTES

1. The language of the order was of the judge's own devise, but was typical of the orders to attend DPEs found around the country. It had no language specifying the purpose or goal of the DPE.

2. Joint custody means both parents share custody of any and all children, split custody means that in families with two or more children the custody of one or more children is awarded to one parent while the custody of the remaining children are awarded to the other parent.

3. Initially, the three categories coded 2 were kept distinct, but there were ultimately too few in each of them, so they were combined.

4. A dismissal can occur because a deadline for filing paperwork has passed unmet or because the couple reconciled. In either event, the legal divorce is not completed. It cannot be assumed that the failure to file paperwork means the couple has reconciled; it is possible that the marriage is still broken. For example, lack of funds to pursue the divorce, lack of understanding for the legal divorce process, familial or religious pressure to retain the legal union, or fear of conflict etc., could undermine completion of legal closure.

5. These documents include everything from filing an address change or filing sundry required paperwork to writing a letter to the judge or requesting relitigation. This variable was also calculated for dismissed cases from petition date to whenever the dismissal was filed. There were 19 cases where couples filed for divorce, then permitted the divorce to be dismissed only to later initiate a new petition filing for divorce again. In cases like these, total legal activity was counted from the first petition through 18 months postdecree of whenever the final decree was granted. Likewise, divorce length was counted from when the first divorce petition was filed and counted through any and all petitions filed by the couple until the divorce was finally granted.

6. One uses a multiple logistic regression method (Pampel, 2000) when the dependent variable is dichotomous, as was the case for both two custody variables, the relitigation variable, and the divorce status variable.

7. It might at first appear that simply aggregating all the control subjects (in the weeks before and the weeks after those of the treatment group) and comparing their average to that of the treatment group might accomplish the same purpose. However, if there are systematic increases or decreases during these periods, and if the pre- and post control times have different trends, or if the treatment group is not exactly in the middle interval, such a procedure could lead to misleading and spurious conclusions.

8. The ages of children were calculated as of their parent's divorce decree date.

9. Prior to testing via the ITT approach, the data were examined for features that could have created a spurious detection of a treatment effect, most importantly the assumption of noncurvilinearity. To test for significant curvilinearity, a deficiency

matrix (Q; see Algina & Swaminathan, 1979) was constructed that tested for any departure from linearity once the middle interval (the treatment interval) was removed. For only one variable, divorce length, was any indication of curvilinearity detected, $F(10, 334) = 1.913$, $p = .04$. In other words, for (and only for) divorce length, there appeared to be some departures from linearity unlikely to be chance. Thus, procedures to take into account curvilinearity were followed (West et al., 2000; Shadish et al., 2002; Trochim, 1990) for analyses involving divorce length. This also involved entering as predictors in the regression equation all polynomial trend orders up to and including the trend order where the curvilinearity was detected. Those are the coefficients for divorce length presented and tested in Table 1.

10. The standard errors of the regression coefficients are also presented, in parentheses. Standard errors provide an indication of how much the coefficient might vary from its value if the entire population, not merely a sample, had been obtained. If the coefficient were to be divided by its respective standard error, one would obtain the t-statistic, the value that evaluates the significance. As a rough rule of thumb, t-statistics of 2.0 or more are deemed significant. If so, this is noted with the *.

11. Protections and/or attendance waivers for parents alleging domestic violence are now common for DPEs (Frazee, 2005; Fuhrmann, McGill, & O'Connell, 1999; Lutz & Gady, 2004).

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