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Do Responsible Fatherhood Programs Work? A Comprehensive Meta-Analytic Study

Objective: To investigate the effectiveness of fatherhood programs targeting unmarried, low-income, nonresident fathers.

Background: Programs for unmarried, nonresident, and low-income fathers increased in number and scope over the past decade. Programs for fathers have typically targeted five broad areas: positive father involvement, parenting, co-parenting, employment, and child support payment.

Method: We conducted a systematic search for published and unpublished evaluations of fathering programs targeting unmarried, never married, and low-income fathers. We identified 25 reports with 30 independent studies. Of these, 21 employed a control-treatment design, and nine employed a one-group/pre-post design.

Results: These programs produce small but statistically significant effects ($d = .099$, $p < .01$). We found that only father involvement ($d = .114$, $p < .05$), parenting ($d = .110$, $p < .01$), and co-parenting ($d = .167$, $p < .05$) were significantly affected; the strongest effect size was in

co-parenting. Unfortunately, these programs did not significantly influence father employment and economic well-being, nor did they significantly impact father payment of child support.

Conclusion: Although programs for low-income, unmarried, nonresident fathers have a small statistically significant effect, evaluation work may increase the impact of these programs.

Implications.: There is a continued need for evaluation focused on unmarried, nonresident, low-income fathers. There is also need for improved statistical reporting, reports of attrition, assessment of child outcomes, observational measures of outcomes, and better assessment of moderators, such as father age, program location, child developmental stage, multipartner fertility, and other barriers to father involvement.

Responsible fatherhood programs for unmarried and nonresident fathers have increased in number and scope over the past decade, spurred by greater scholarly attention to the risk factors associated with family instability (Amato, 2005; Cherlin, 2010), increased federal funding for programs for unmarried

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or nonresident fathers, and rigorous evaluation studies of some programs (e.g., Fagan, 2008; Fagan, Cherson, Brown, & Vecere, 2015; Fagan & Stevenson, 2002; Florsheim et al., 2012; Zaveri, Baumgartner, Dion, & Clary, 2015). An important reason for the growth of responsible fatherhood programs has been the Administration for Children and Families (ACF) responsible fatherhood initiative. Although use of the term *responsible fatherhood* has been debated (see Doherty Kouneski, & Erickson, 1998; Doherty, Kouneski, & Erickson, 2000; Walker & McGraw, 2000), this term also names a specific federal policy initiative. This initiative, which has extended the work of nonprofit organizations, including community, state, private secular, and faith-based efforts, to strengthen fathers' connections to their children, has allocated \$50 to \$75 million a year from 2006 to 2018 to support fatherhood programs for a total of about \$725 million (for more details, see Pearson, 2018; Tollestrup, 2018). For some reason, however, work addressing the impact of these programs on fathers and children tends to be understudied by academics (Holmes, Brotherson, & Roy, 2012; Holmes, Cowan, Cowan, & Hawkins, 2013). Despite the significant numbers being served by these federally funded programs (Hawkins & Simpson, 2015) and some rigorous evaluation studies (Avellar, Covington, Moore, Patnaik, & Wu, 2018), the ACF responsible fatherhood initiative has not received the scrutiny (or criticism) that the parallel ACF Healthy Marriage and Relationships Education initiative has received (e.g., Johnson, 2012; Randles, 2017). The time seems right, then, to synthesize for researchers, practitioners, and policy makers this emerging body of applied family science research that aims to increase fathers' positive involvement with their children.

This article highlights the results of a comprehensive meta-analysis of responsible fatherhood program evaluation studies targeted primarily to unmarried, low-income, nonresident fathers. The overall research question is: How effective are responsible fatherhood programs at increasing unmarried, low-income, nonresident fathers' positive father involvement, parenting, co-parenting behavior, employment, economic prospects, and child support payments?

THE PREVALENCE AND IMPORTANCE OF NONRESIDENT FATHERS

Nonresidential fatherhood is a growing phenomenon in the United States. According to the U.S. Census Bureau, 23% of children (17 million) lived in father-absent homes in 2017 (U.S. Census Bureau, 2017). Demographers estimate that half of all children in the United States are expected to live with a single parent at some time in their life (Livingston, 2014), and 84% of these children will have a nonresident father (Lippold, 2017). Nonresident and unmarried fathers are significantly less likely to stay involved with their children when their romantic relationships dissolve (Castillo, Welch, & Sarver, 2011; Osborne, Manning, & Smock, 2007).

A generation of research has shown that fathers make important contributions to child development (for reviews, see Sarkadi, Kristiansson, Oberklaid, & Bremberg, 2008; Yogman & Garfield, 2016), and the same research has established potential threats to child development when fathers are absent from their children's lives. For example, father absence has been correlated with significantly higher levels of child poverty (U.S. Census Bureau, 2017), poorer mental health (Culpin, Heron, Araya, Melotti, & Joinson, 2013; Elam, Sandler, Wolchik, & Tein, 2016), higher levels of delinquency (Kofler-Westergren, Klopff, & Mitterauer, 2010), higher levels of drug use (Hoffmann, 2002), and more risky sexual behavior (Ellis, Schlomer, Tilley, & Butler, 2012). Some have provided evidence that this association between father absence and negative outcomes may be causal (McLanahan, Tach, & Schneider, 2013), and others have presented evidence that there is intergenerational continuity (Pouget, Serbin, Stack, Ledingham, & Schwartzman, 2012). Others have also provided evidence that family structure, such as nonresidential fatherhood, matters less for child outcomes than the quality of the relationship between the father and his child or access to the economic and social resources children need (for review, see Lamb, 2012). Fathers themselves say their barriers include access to their children, poor co-parenting skills with the mothers of their children, and the need to both find and secure employment (Holcomb et al., 2015; Osborne, Dillon, Craver, & Hovey, 2016; Tollestrup, 2018). Thus, researchers and policymakers have been invested in understanding how to buffer the potentially negative effects of

father absence while bolstering the potentially protective processes that help children, fathers, and mothers thrive (Tollestrup, 2018).

The prevalence of nonresident fatherhood in children's lives, the negative outcomes that result when fathers lose connections with their children, and the positive outcomes that result when fathers are able to stay connected suggest that scholars should be invested in understanding how to support nonresident fathers' desires and efforts to benefit the lives of their children.

FRAMEWORK FOR EFFECTIVE PROGRAMS

Programs for fathers emerged in the 1980s (e.g., Dachman, Alessi, Vrazo, Fuqua, & Kerr, 1986; Levant & Doyle, 1983; Vadasy, Fewell, Greenberg, Dermond, & Meyer, 1986), with the objective of supporting "responsible" fathering behaviors in men, especially nonresident and married fathers. A wide variety of programs serving fathers in an array of different family circumstances were developed and implemented by clinical social workers, faith leaders, and other helping professionals concerned generally with promoting fathers' positive involvement in the lives of their children (Tollestrup, 2018). Most programs were housed in community social service agencies, nonprofit human service organizations, or faith-based institutions. Programs generally included group instructional sessions and case management services helping men to connect with resources to mitigate barriers to involvement with their children. Since 2006, federal dollars from the ACF have been allocated via competitive grants to support responsible fatherhood programs (\$50 million–\$75 million a year; <https://www.acf.hhs.gov/ofa/resource/healthy-marriage-grantees>). Nonprofit organizations such as the National Fatherhood Initiative (<http://www.fatherhood.org>) emerged to facilitate program administrations' efforts to help fathers and their families. Similarly, a government-funded National Fatherhood Clearinghouse now exists to support organizations that are providing services to fathers (<https://www.fatherhood.gov/home>).

Although the focus of responsible fatherhood programs, their targeted populations, and the discourse in specific programs is not without criticism and controversy (for full discussion of these issues, see Randles, 2013, 2020), these programs explicitly focus on parenting

behaviors that fall in three broad categories: economic support, father involvement/parenting, and co-parenting/healthy relationships (Fagan & Kaufman, 2015a). Economic support programs typically involve teaching skills to gain employment or find a better job, to be more fiscally responsible, or to increase child support payments (ACF, 2009). Measurable outcomes include increases in employment rates, income, child support order establishment, and the payment of formal and informal child support (e.g., Pearson et al., 2003).

A second category of father involvement and parenting programs teaches men to be engaged and nurturing with their children and provides the parenting skills to do so. Outcomes in these programs are more diverse, including parent competence, parenting satisfaction, parenting stress, self-esteem, engagement with children, and father–child contact (ACF, 2009). The main objective of these programs, however, is to increase the quality of the time men spend with their children because solely increasing quantity produces no positive effect on children (Amato & Gilbreth, 1999).

The last category consists of programs that enhance co-parenting. The nature of co-parenting programs depends on the status of the father's relationship with his child's mother. Married or cohabiting fathers learn skills to strengthen their relationship, take inventory of interpersonal strengths and weaknesses, communicate more effectively, and control aggressive behavior (ACF, 2009). Programs for nonresident fathers teach many of the same skills as resident programs, with the focus on improving the relationship with the mother. The co-parenting relationship is the priority because it is among the largest predictors of nonresident fathers' involvement with their children (Carlson, McLanahan, & Brooks-Gunn, 2008; Fagan & Palkovitz, 2011; McHale & Coates, 2014). Specific outcomes measured in these programs include relationship satisfaction, strength of co-parenting relationship, communication, and social support (Fagan & Kaufman, 2015).

REVIEW OF EXISTING PROGRAMS, EVALUATIONS, AND META-ANALYSES

Despite the numerous programs that have been developed and implemented over the decades, rigorous evaluations of these programs have lagged behind. Scholars made this observation

nearly two decades ago (Hawkins & Fagan, 2001); unfortunately, progress has been slow. Holmes, Galovan, Yoshida, and Hawkins (2010) conducted the only meta-analysis on fatherhood programs, with a focus on resident fathers. Overall, these fathering programs had a small to moderate effect size. Broken into individual outcomes, the effects were also small to moderate (e.g., father involvement, fathering attitudes, co-parenting, and child behaviors), although the father-child relationship outcome had a large effect size. Notably, the meta-analysis included only 16 studies that fit the criteria and included codable statistics on program outcomes. Little is known of the effectiveness of all other programs administered in the past decade.

Additionally, most of the programs in the Holmes et al. (2010) analysis were conducted among married, middle- to upper-income families; few included cohabiting, unmarried, or non-resident fathers. This is particularly problematic given that fathering programs have grown to include a strong focus on at-risk families, such as low-income, divorced, or separated families because at-risk families experience many more pressures and challenges than more advantaged families (McLanahan, Garfinkel, Mincy, & Donahue, 2010), with at-risk families exhibit more problems and need help in more areas (see Holmes et al., 2010; Kaminski, Valle, Filene, & Boyle, 2008).

In summary, the purpose of the current study was to further understanding of fathering programs to highlight the available evidence for the effectiveness of responsible fatherhood programs targeted primarily to unmarried, low-income, nonresident fathers in a comprehensive meta-analysis of evaluation studies. With more than a decade now of significant federal funding for these kinds of programs, it seems timely to ask: How effective are responsible fatherhood programs at increasing unmarried, low-income, nonresident fathers' positive father involvement, parenting, co-parenting behavior, employment, economic prospects, and child support payments?

METHOD

Search Procedure

To conduct our search for programs for unmarried, low-income, nonresident, fathers, we used search terms including *responsible fatherhood*, *nonresident father program*, *low-income*,

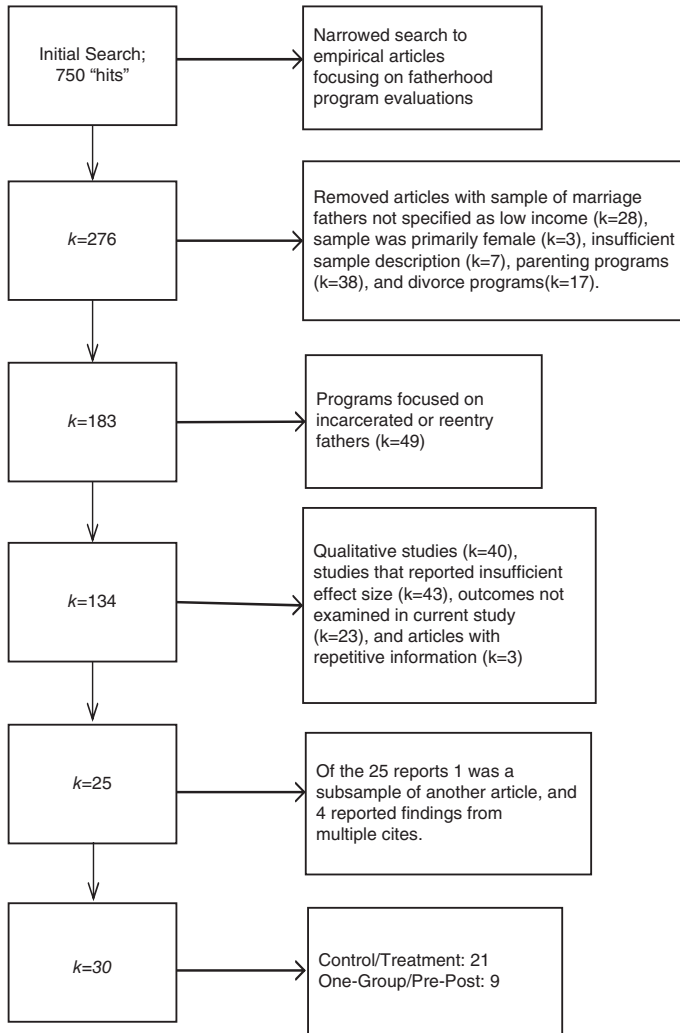
noncustodial, *cohabiting*, *unmarried*, *involvement*, *intervention*, *treatment*, *parenting*, *child support*, *divorce*, *evaluation*, *assessment*, *co-parenting*, *home visitation*, and *employment*. We employed Academic Search Premier, PsycINFO, PsycARTICLES, Psychology and Behavioral Sciences Collection, Social Sciences Abstracts, and ProQuest Dissertation and Theses Databases in order to identify academic articles. We also examined websites of organizations that support responsible fatherhood programs to identify potential evaluation reports not published in academic outlets, such as the ACF; Office of Planning, Research and Evaluation (specifically within ACF); Fatherhood Research and Practice Network; National Fatherhood Initiative; Nurturing Father; the Manpower Demonstration Research Corporation (MDRC); the Institute for Research and Poverty; Issue Lab; the U.S. Department of Health and Human Services; Urban Institute; Mathematica; the National Responsible Fatherhood Clearinghouse; and Fathers and Families Coalition of America. We combed through reference sections of articles and reports for other studies that we may have missed in these searches. This search process returned 750 research articles and reports for additional examination. From this list, we identified 276 primary research reports evaluating fathering programs targeting unmarried, never married, low-income fathers for closer examination.

Inclusion Criteria

Our initial search yielded 276 potential reports, but only 25 met our inclusion criteria. Two of these reports used the same sample, so they were collapsed into one study; five other reports employed multiple independent treatment groups, so these were coded as independent studies, adding five studies ($k = 7$). Thus, of the 25 reports, 30 independent studies were identified for coding ($k = 30$). Figure 1 summarizes this process explained in more detail.

Sample characteristics. We focused on programs for nonresident, low-income fathers. Samples were composed primarily of unmarried fathers who did not reside with their children or who were cohabiting with their child's mother. We note that although programs may target nonresident fathers, nonresident fathers may still reside with some but not all of their children.

FIGURE 1. PRISMA FLOWCHART DESCRIBING IDENTIFICATION AND SELECTION OF STUDIES FOR INCLUSION IN THE META-ANALYSIS(SEE MOHER, LIBERATI, TETZLAFF, ALTMAN, & THE PRISMA GROUP (2009)). *k* = EXPRESSES THE NUMBER OF CODED STUDIES CONTRIBUTING TO THE OVERALL EFFECT SIZE.



On the basis of our sample criteria, we excluded 28 studies where the sample included married fathers and the fathers’ income status was not specified, three studies where the sample was primarily female, 38 studies where the authors did not distinguish between the sex of participants when reporting outcomes (thus we could not distinguish fathers from mothers), and seven studies that did not describe the sample in sufficient detail. When plausible, we contacted study authors for additional clarifying information or additional data.

Other potential studies included samples with fathers who were never married, divorced, reentering, and incarcerated. We excluded studies of programs focused on incarcerated fathers because these fathers often have little to no direct contact with their children (Roy, 2005) and because the outcomes measured were distinct from studies of programs with nonincarcerated fathers. Similarly, studies of fatherhood reentry programs were excluded because often they are conducted while men are still imprisoned. The way fathers from these programs

interact with their children can be very different from nonincarcerated fathers. For those interested in this particular population, we note a recent meta-analysis of programs for incarcerated mothers and fathers by Armstrong, Eggins, Reid, Harnett, and Dawe (2018). By eliminating programs focused on reentering and incarcerated fathers, we excluded another 49 studies.

We also excluded studies of programs focused on divorced fathers ($k = 17$). This was a more difficult decision, but our rationale was as follows. First, a meta-analysis of divorcing parent programs has already been published looking at parenting outcomes (Fackrell, Hawkins, & Kay, 2010). Second, many divorcing parent programs are conducted before the divorce is finalized and most fathers are still married to the mothers and coresident with their children. Moreover, many of these divorcing fathers likely have a stronger connection to and longer resident relationship with their children compared with the unmarried fathers targeted by many fatherhood programs. Some studies of economically disadvantaged fathers included both married and unmarried fathers in their samples. Although our target population was unmarried fathers, we chose to keep studies in the meta-analysis if less than 35% of the sample was married.

Reporting data. All quantitative studies that reported sufficient data for calculating effect sizes on targeted outcomes were included in the study. Some studies did not report sufficient data to calculate effect sizes such as means, standard deviations, or sample size of the control and treatment groups ($k = 37$). (Forty studies reported only qualitative data, so they could not be included.) When plausible, we contacted study authors for additional data. In some instances, we were able to use a few statistical techniques to calculate the effect size from other information provided in the study or could make reasonable guesses. Nonetheless, 37 articles were removed from the analysis due to the lack of reporting crucial statistical information. Therefore, we conducted missing-study bias analyses (reported later).

Study design. We included experimental, quasi-experimental, and one-group/pre–post study designs in our analysis. The primary difference between experimental and quasi-experimental designs is that while both have control groups, experimental designs randomly assign subjects

to groups and quasi-experimental designs do not (e.g., assign preexisting groups to treatment and control, assign early volunteers to treatment group, and later volunteers to control group). There were 11 experimental and 10 quasi-experimental studies in the analyses.

We also included nine one-group/pretest–posttest studies in our analyses. These nonexperimental studies may expand our understanding of the impact of fatherhood programs on nonresident, low-income, unmarried fathers. Supplemental analyses with one-group/pre–post designs were conducted separately from studies with control-group designs, as recommended by Lipsey and Wilson (2001). As is often an issue with using one-group/pre–post designs, the correlations between pre- and posttest variables needed to calculate the effect sizes precisely were rarely reported. We estimated pre–post correlations of .50 for these studies, as suggested by Schwartz and his colleagues (2006).

Publication status. Although there were studies included in our analysis were not specifically published in peer-reviewed journals, all of the studies had gone through some kind of reviewing process before they were published. As a result, instead of coding for published and unpublished studies, we coded for (a) studies published in peer-reviewed journals, (b) formal reports (e.g., government reports), and (c) dissertations and theses.

Type of program. Clinical interventions involve one-on-one sessions with a licensed mental health practitioner, but because these programs typically have stronger effect sizes than education programs (Shadish & Baldwin, 2003), we initially decided to exclude any studies that described the interventions as clinical rather than educational. Educational programs give participants knowledge (e.g., the positive effects of warm, supportive parenting) and skills (e.g., time-out to deal with an upset child) but do not focus on a single individual, help them with a specific problem they might be facing, or provide in-depth analysis of the reasons they might be experiencing the problem. While searching for and coding studies, however, we discovered that no clinical intervention studies came up in our search. We did, however, discover an important group of fatherhood programs that include case management. In such studies, fathers may be connected to a case manager

or a peer support group, or they case management may be combined with other educational interventions. However, case managers usually are not trained mental health practitioners. We included studies of these programs in our meta-analysis.

Language. We found only one study that was not in English, which was excluded.

Reported outcomes. Not all father program evaluations that were examined reported outcomes that were applicable to the current meta-analysis. These outcomes included health items, peer mentoring, and program satisfaction (i.e., simple yes-or-no questions asking fathers if they felt they had benefited from the program). Our search yielded 23 such studies that were excluded from our meta-analysis because they did not focus on family well-being outcomes that were central to our research question and that are commonly targeted in responsible father programs. (For more information on the outcomes examined in this analysis, see “Outcomes” later in the article.)

Variable Coding

We created a 28-item codebook similar to that used by Holmes et al. (2010) in their meta-analysis of father-involvement programs focused specifically on resident fathers. Included in the codebook were moderators related to the study (e.g., study design), sample (i.e., income), publication status, and the relationship of the father with both the mother and target child. Our research team included four meta-analytically trained individuals (two PhD faculty and two graduate students) and one undergraduate student who assisted with the collection of the articles. The evaluation of these studies was done in stages, with inclusion criteria identified first before moderators were chosen and statistical information to compute effect size was recorded.

We attempted to code for a wide range of outcomes focused on family well-being, ultimately aggregating outcomes into five categories for both conceptual and statistical-power reasons: father involvement (e.g., any interaction the father had with his child), parenting (e.g., skills developed in regard to positive parenting), co-parenting (e.g., cooperation with the mother, and father–mother relationship quality), father

employment and economic well-being (e.g., administrative data tracking quarterly wages, employment status, and increase in paid work hours), and payment of child support (e.g., formal and informal payments, administrative data on arrears, and payment of arrears). Three of our studies contained child outcomes (Caldwell et al., 2014; Caldwell, Rafferty, Reischl, De Loney, & Brooks, 2010; Fagan & Iglesias, 1999), one of which was a subsample study. Each study reported different outcomes, so we cannot include an aggregated report here. Further, as mentioned earlier, some studies targeted outcomes not focused on family well-being; we did not code for these outcomes.

Computing and Reporting Effect Sizes

We used Comprehensive Meta-Analysis III to calculate postintervention standardized mean difference effect sizes for the experimental and quasi-experimental studies. Standardized mean change score effect sizes were computed separately for one-group/pre–post studies. Few studies reported any follow-up effects, so these effect sizes are based on immediate posttreatment differences. We reported random effects model effect sizes that allow for the possibility that differences in effects between programs result from both sampling error and differences in intervention and study methods (Lipsey & Wilson, 2001). To help all readers understand the reporting of meta-analytic effects, d expresses the magnitude of the effect size comparing one group to another (or one group before and after intervention), and k expresses the number of coded studies that contribute to the overall effect size. On the basis of an analysis of more than 300 meta-analytic studies of psychological, behavioral, or educational interventions, Lipsey and Wilson (2001) labeled effect sizes $\leq .30$ as small, effect sizes $\geq .67$ as large, and medium effect sizes are between those two points. The Q statistic (the weighted sum of squares of all effects about the grand mean) tests the between-subgroups portion of the variance and is analogous to a t test that examines whether the means of two subgroups are statistically different (Borenstein, Hedges, Higgins, & Rothstein, 2011). In our preliminary analyses, we employ the Q statistic to test whether a number of

methodological moderators alter the true effect sizes.

RESULTS

Preliminary Analyses

We identified and coded 25 reports containing a total of 30 studies. Of these, 21 employed a control–treatment design, and nine employed a one-group/pre–post design. Before estimating the effects, we conducted preliminary analyses to detect any differences in effects based on the study design (e.g., quasi-experimental vs. experimental design), report type (e.g., journal article, public report, or dissertation/thesis), and reporting of marital status (e.g., those that included 35% or fewer married low-income fathers in their sample vs. those that did not). In addition, we were concerned that results from one government-funded study—Parents and Children Together, or PACT, Evaluation Study, with four independent sites totaling more than 5,000 fathers—would heavily weight the overall effect size for all the programs. We therefore tested whether the effects for these PACT sites were significantly different from the effect sizes for the rest of the studies.

The difference between the overall effect sizes for experimental design studies versus quasi-experimental studies was not quite significant ($Q = .28$, *ns*; $d_{\text{exp}} = .09$, $p < .01$, $k = 11$; $d_{\text{quasi}} = .13$, $p = .09$, $k = 10$). Thus, we lumped these control-group studies together. We also found no significant difference based on reporting of marital status ($Q = .51$, *ns*; $d_{\text{reported}} = .13$, $p = .04$, $k = 10$; $d_{\text{not}} = .21$, $p = .01$, $k = 7$). We did, however, discover that the effect size for studies published in journals was significantly higher than for studies in public reports or dissertations/theses ($Q = 15.09$, $p < .001$; $d_{\text{journal}} = .28$, $p < .001$, $k = 10$; $d_{\text{report}} = .03$, $p = .09$, $k = 7$; $d_{\text{diss/thesis}} = -.04$, $p = .78$, $k = 4$). This is a common situation but still one worth noting. Finally, although we found that studies that were part of PACT had somewhat lower effect sizes compared with those that were not part of PACT, the difference was not statistically significant ($Q = 1.45$, *ns*; $d_{\text{PACT}} = .071$, $p = .06$, $k = 4$; $d_{\text{not}} = .15$, $p < .01$, $k = 17$).

We focus the overall Results section on the effects of the more rigorous control-group design studies (see Table 1) but also direct readers to Table 2, where the effects of the

one-group/pre–post designs are reported. Some meta-analysts choose not to report the results of one-group/pre–post designs because they do not account for potential biases and confounds, but we wanted to acknowledge in our meta-analysis the broad range of evaluation work focused on low-income, nonresident fathers, and we think that these supplemental analyses also can shed light on responsible fatherhood programs. In general, however, these analyses painted a similar picture, with the 9 one-group/pre–post studies producing similar effect sizes to the 21 studies that employed control-group designs.

Aggregate Effects

When all five outcome categories (e.g., co-parenting, child support, father economic well-being, father involvement, and parenting) were aggregated into one common measure indicating program impact, programs targeting nonresident, low-income fathers had an overall significant effect ($d = .099$, $p < .01$, $k = 21$). (To facilitate aggregation, all outcomes were coded such that positive numbers indicated greater intervention success.) This effect size is considered small (Card, 2015; Lipsey & Wilson, 2001) but is comparable to reported effects of relationship education efforts in similar low-income, at-risk populations (Arnold & Beelmann, 2019; Hawkins & Erickson, 2015). This effect may be interpreted to mean that about 54% of fathers, on who participated in responsible fatherhood interventions scored above the control-group median.

Effects on Specific Outcomes

Because the aggregate effect size provides only a general sense of how effective these responsible fatherhood interventions were at meeting their target goals, we also calculated effects for each of the five specific outcomes. More studies measured parenting ($k = 16$) than other outcomes. The average effect size for parenting was small but statistically significant ($d = .111$, $p < .01$, $k = 16$). The next most commonly assessed outcome was father involvement. Again, this effect was small but statistically significant ($d = .114$, $p < .05$, $k = 15$). The third most commonly assessed outcome was co-parenting, and the effect size also was statistically significant but small, although it was slightly larger than the effects of the interventions on the other

Table 1. Effect Sizes for Control-Group Studies

	<i>k</i>	ES	<i>p</i> value	Adj. ES (trimmed studies)		<i>I</i> ²	Prediction Interval	
				<i>Q</i> value	<i>p</i> value		lower limit	upper limit
Precheck: marital status								
Reported	10	.132	.041	.512	.474			
Not	7	.205	.010					
Precheck: study design								
Experimental	11	.088	.006	.279	.597			
Quasi-experimental	10	.132	.086					
Precheck: report type								
Journal ¹	10	.280	<.001	15.506	<.001			
Report	7	.029	.088					
Dissertation/thesis	4	-.037	.777					
Precheck: PACT vs. non-PACT								
PACT	4	.071	.064	1.450	.229			
Non-PACT	17	.147	.004					
Overall program impact (aggregated outcomes)								
Co-parenting	12	.167	.039	.044 (6)	30.466	.063	34.353	-0.065 0.242
Child support	8	.054	.128	(0)	11.860	.105	69.003	-0.339 0.581
Employment	6	.030	.103	.026 (2)	2.094	<.001	40.977	-0.123 0.231
Father involvement	15	.114	.039	(0)	34.777	.002	.000	-0.028 0.088
Parenting	16	.110	.009	(0)	19.687	.184	59.743	-0.221 0.449
							23.808	-0.080 0.300

Note. Effect size (ES) for journals is significantly larger than effect size for reports and dissertations/theses (difference between journals and reports $Q = 15.09, p < .001$; difference between journals and dissertations/theses $Q = 4.731, p = .030$). Adj. = adjusted.

Table 2. Effect Sizes for One-Group/Pre-Post Studies

	<i>k</i>	ES	<i>p</i> value	Adj. ES (trimmed studies)		<i>I</i> ²	Prediction Interval	
				<i>Q</i> value	<i>p</i> value		lower limit	upper limit
Precheck: marital status								
Reported	6	.020	.793	1.257	.262	—		
Not	3	.193	.147			—		
Precheck: report type								
Peer-reviewed journal	4	.198	.010	3.375	.066	—		
Report	5	-.028	.768			—		
Dissertation/thesis	0	—	—			—		
Overall program impact (aggregated outcomes)								
Co-parenting	1	.078	.442	—	.000	1.000	.000	
Child support	7	.060	.397	.111 (0)	65.736	<.001	89.538	-0.346 0.500
Employment	0	—	—	—	—	—	—	
Father involvement	1	.516	<.001	—	.000	1	.000	
Parenting	2	.061	.334	—	.577	.749	.000	

Note. Adj. = adjusted; ES = effect size.

outcomes ($d = .167, p < .05, k = 12$). These effects can be interpreted to mean that between 54% and 56% of treatment-group fathers scored above the control-group median. Finally, the

least examined outcomes were the effects of the interventions on a father’s child support payment and a father’s employment and economic prospects. However, these effects were not

significant (child support: $d = .054$, *ns*, $k = 8$; employment/prospects: $d = .030$, *ns*, $k = 6$).

Follow-Up Analyses

Meta-analytic researchers can never be sure that they have found all relevant studies for their meta-analysis. In our case, we know that 37 potential studies were excluded from our analyses because they did not provide sufficient data from which to calculate an effect size. Accordingly, we conducted follow-up analyses that attempted to detect missing study bias that can inflate overall effect sizes. A Duval and Tweedie (2000) trim-and-fill analysis for each of the outcomes examined did not find much evidence of significant bias, so adjusted effect sizes were virtually unchanged. Statistical power to detect bias in these analyses, however, was limited. Greater statistical power was available for the aggregate program effect size ($k = 21$), and this analysis did suggest potential missing study bias with a smaller adjusted effect size ($d = .044$). Accordingly, even the modest unadjusted aggregate effect size ($d = .097$) may be inflated. These analyses clearly highlight the need for more evaluation work in this field. We were not able to conduct moderator analyses to explore how program features might explain some of the heterogeneity of effect sizes despite Q test for heterogeneity statistics that indicated the presence of substantial systematic variation. This was because many reports did not provide information to be able to code program features and because the small number of studies did not provide adequate statistical power to examine the program moderators that we were able to code.

DISCUSSION

The purpose of this meta-analysis was to better understand how effective responsible fatherhood educational programs are at increasing low-income, unmarried, nonresident fathers' positive father involvement, parenting, co-parenting behavior, employment and economic prospects, and child support payments. We came away with two major findings. First, based on the current data available to us using control-group designs, these programs produce small but statistically significant effects for the populations they serve. However, when exploring the effects more specifically, we found

that only father involvement, parenting, and co-parenting were significantly affected. The strongest effect size was in co-parenting skills. This was particularly encouraging because the co-parenting relationship is one of the most important predictors of nonresident father involvement (Carlson et al., 2008; Fagan & Palkovitz, 2011; McHale & Coates, 2014). Nonresident fathers regularly report concerns about their relationship with the mother of their children, even reporting that she is one of the strongest barriers to their continued involvement with their children. It is encouraging that programs are making a difference in nonresident, low-income, unmarried fathers' co-parenting skills.

Unfortunately, these programs did not significantly influence father employment and economic prospects or fathers' payment of child support. Because a father's child support payments and employment outcomes (such as quarterly wage reports from employers) were typically measured using more objective assessments than the self-report data assessing father involvement, parenting, and co-parenting, it is possible that our findings also reflect differences due to measurement. Fathers may overestimate in their self-reports, whereas more objective reports are less likely to be inflated. Measurement concerns aside, fathers' economic contributions also are important to child well-being (Amato & Gilbreth, 1999). We hope to see more evidence that programs targeting fathers' employment, economic well-being, and formal or informal payment of child support can succeed.

A second general conclusion from our meta-analysis is that there is a continued need for evaluation of these fatherhood programs, especially work focused on unmarried nonresident low-income fathers. Evaluation work in this field lags behind a significant amount of basic research on fathers and also lags behind other types of evaluation work in this field. A parallel field of family life education interventions to help couples form and sustain healthy relationships now has more than 300 evaluation studies. In the past decade, those studies have more carefully attended to evaluating the effectiveness of relationship education (RE) programs with disadvantaged, at-risk individuals and couples (more than 50 studies now). We do not have an adequate explanation for why responsible fatherhood programs have not

generated more frequent evaluative attention, especially compared to RE programs. Given the family studies field's intense interest in diverse family forms and in family instability and the fact that federal funding for responsible fatherhood programs has been nearly equivalent to funding for RE programs, we should be farther along in evaluating the effectiveness of these programs than we are at present. It is possible that targeting couples, instead of targeting low-income, never married, nonresident fathers, allows relationship education programs to garner better attendance. When RE programs focus on low-income couples, their results are comparable to the results we report here (Arnold & Beelman, 2019; Hawkins & Erickson, 2015).

In addition to the quantity of work in this field, there are needed improvements in the quality of evaluation research. We focus the rest of our discussion on key areas for improvement.

Advancing Research in the Field

Statistical reporting. Many studies did not report adequate numbers from which we could compute effect sizes. Of those that did report some kind of statistical information, a large portion of the effect sizes had to be computed using p values and sample sizes (and sometimes these figures were not reported precisely). Where possible, future evaluations should always report means and standard deviations and group sample sizes. For example, one would report the sample size for the control group and also report the sample size for any treatment groups that are part of the evaluation. These means, standard deviations, and sample sizes would be reported for each time point in the evaluation as well. This statistical reporting is not only for the benefit of future meta-analyses but also for clarity in interpretation for other researchers interested in this literature. In addition, many studies reported outcomes in percentages, which may seem to provide ease of understanding, but unfortunately, they can be difficult to convert into effect sizes. In addition to reporting raw percentages, we encourage researchers to report risk differences between groups with appropriate measures of variance so that effect sizes can be calculated.

We discovered a large number of qualitative evaluation studies. A careful review of qualitative studies can add richness to

our understanding of what occurs in programs and how they may produce change. Mixed-method studies, however, will contribute even more understanding (and can be included in meta-analytic reviews). More mixed-method studies will advance the field.

Attrition. It was also fairly rare for the attrition rates before, during, or after the programs to be reported. Attrition is a frequent occurrence in any intervention program. There is the possibility that some systematic reason is responsible for fathers not completing a program, suggesting that the program may be consistently failing to meet the needs of a specific subsample of nonresident fathers. When attrition rates are nontrivial, a program can appear to be more effective than it actually is. When attrition rates are reported, a meta-analyst can at least decide whether rates are too high to accept the outcomes or can code for high attrition and attempt to analyze its potential effects on overall outcomes.

Child outcomes. Only three of the evaluations we analyzed reported on child outcomes, and one of these three was a subsample of a larger evaluation. The outcomes were divergent enough that we could not aggregate them across studies for the purposes of this meta-analysis (e.g., one study assessed child's tendency toward violence and delinquency, another assessed child academic readiness, and a third assessed child emotion regulation and prosocial skills). Considering that the primary rationale for father involvement programs is to positively influence the lives of children, this was disappointing. Although the research literature does suggest that features of father involvement, parenting, co-parenting, and economic provisions are correlated with child outcomes, we need to include child outcomes in our evaluations to better understand whether our outreach to fathers directly affects their children's well-being.

Father-only reporting. As we reviewed the studies that met our inclusion criteria, we discovered that all of the studies were based on father reports. We recognize that focusing specifically on fathers in one's program necessitates the inclusion of fathers' reports about their experiences in the program. However, fathers are known to overestimate their involvement with children (Dyer, Day, & Harper, 2014), and it is

possible that fathers may exhibit social desirability in reporting higher program outcomes. Mothers' reports, on the other hand, are typically more accurate in reporting involvement and child outcomes (Dyer et al., 2014). The need for multiple reporters and observational measures of father involvement is evident in this body of work. Ideally, moving forward, program evaluators will incorporate additional sources of assessment. For example, observational measures may be a good assessment tool. Meta-analytic studies have shown that observational measures can find evidence of higher effect sizes (Blanchard, Hawkins, Baldwin, & Fawcett, 2009).

Longer term effects. Few studies followed fathers for significant periods of time after completing the interventions to examine whether program effects deteriorated or grew over time. Demonstrating longer term effects is an important element of program effectiveness. Future studies in this area need to do the hard (and expensive) work of following fathers over time.

Moderators. As this evaluation work continues to grow, we hope to see moderation analyses in subsequent meta-analyses. Some important moderators to consider in future work may include program length, the presence of follow-up assessments, the number of follow-up assessments, the length of follow-up assessments, attrition rates during the program, attrition rates after the program, the age of fathers, the child's developmental stage, the number of children, multipartner fertility, and other barriers to father involvement such as incarceration history and employment history (when not included as an outcome being assessed).

In a meta-analysis, researchers hope to assess current program effectiveness, as well as provide information for fathers over time and across populations. Listing details of these components that contribute to program success in one's evaluation report will afford those invested in these programs to learn more about what truly makes an effective program work—in other words, it allows us to make distinctions between programs that produce larger or longer lasting effects than those that produce smaller or short-term effects.

In conclusion, responsible fatherhood programs for low-income, unmarried, nonresident

fathers have increased in number and scope over the past four decades and are now supported by significant public and private funding. Yet research documenting the effectiveness of these programs lags behind. In this meta-analytic study, we have surveyed the limited number of program evaluation studies in this area and identified a small but statistically significant impact of these programs. Clearly, there is more work to do to improve our efforts, increase the impact of these programs, and document their effectiveness. We hope this meta-analytic review will spur and inform more work in this important area.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Table S1. *List of Organizations and Websites that Support Fatherhood Programs.*

Table S2. *Program Name, Length, and Type by Article.*