

Final Evaluation Report: Do Responsible Fatherhood Programs Work? A Comprehensive Meta-analytic Study



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Introduction

Responsible fatherhood (RF) 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 or nonresident fathers (e.g., the Administration for Children and Families has spent an estimated \$700 million from 2006 to 2018 on funding responsible fatherhood programs, allocating \$50 to \$75 million a year), 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). Unfortunately, work addressing responsible fatherhood programs tends to fly under the academic and policy radars (Holmes, Brotherson, & Roy, 2012; Holmes, Cowan, Cowan, & Hawkins, 2013). The time is right to synthesize for researchers, practitioners, and policy makers this body of work that aims to increase fathers' positive involvement with their children.

The current paper 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, coparenting 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. Nearly one-sixth of the over 70 million fathers in the U.S. has a nonresident child. Furthermore, half of all children in the U.S. are expected to live with a single parent at some time in their life (Livingston, 2014) and 84 percent of these children will have a nonresident father. 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).



While a generation of research has shown that fathers make important contributions to child development (for a review, see Sarkadi, Kristiansson, Oberklaid, & Bremberg, 2008), the same research has established significant 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, 2011), poorer mental health (Culpin, Heron, Araya, Melotti, & Joinson, 2013), 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 is causal (McLanahan, Tach, & Schneider, 2013), and others have presented evidence that there is intergenerational continuity (Pouget, Serbin, Stack, Ledingham, & Schwartzman, 2012).

The high 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 fathers and children through these transitions in residence because increasing nonresident father involvement can improve child outcomes.

Framework for Effective Programs

Programs for fathers became prolific in the 1980s (e.g., Dachman, Alessi, Vrazo, Fuqua, & Kerr, 1986; Levant & Doyle, 1983; Vadasy, Fewell, Greenberg, Dermond, & Meyer, 1986), with the objective of fostering more "responsible" fathering behaviors in men. These behaviors fall in three broad categories: economic support, father involvement/parenting, and coparenting (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 (Administration for Children and Families, 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).

Father involvement and parenting programs teach men to be engaged and nurturing with their children, providing 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 (Administration for Children and Families, 2009). The main objective of these programs, however, is to increase the quality of the time men spend with their children, as solely increasing quantity produces no positive effect on children (Amato & Gilbreth, 1999).

The last category consists of programs that enhance coparenting. The nature of coparenting 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, to take inventory of interpersonal strengths and weaknesses, how to communicate more effectively, and how to control aggressive behavior (Administration for Children and Families, 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 coparenting 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 coparenting relationship, communication, and social support (Fagan & Kaufman, 2015a).

Review of Existing Programs, Evaluations, and Meta-analyses

Despite the numerous programs that have been developed and implemented over the decades, very few rigorous evaluations have been done to test their effectiveness. Holmes et al. (2010) conducted the only meta-analysis that has been conducted on fatherhood programs, and its focus was on resident fathers. Overall, fathering programs had a small to moderate effect size. Broken into individual outcomes, most of the effects were small to moderate (father involvement, fathering attitudes, coparenting, and child behaviors), though the father-child relationship had a large effect size. Notably, the meta-analysis included only 16 studies that fit the criteria for a fathering program and that included codable statistics on program outcomes. Very little is known of the effectiveness of all other fathering programs that have been administered in the last decade.

Additionally, most of the programs in the Holmes et al. analysis were conducted among married, white, middle- to upper-income families; very few included cohabiting, unmarried, or nonresident fathers. This is particularly problematic given the fact that the focus of these programs has recently shifted to at-risk families, such as low-income, divorced, or separated families, as they experience many more pressures and challenges than more advantaged families (McLanahan, Garfinkel, Mincy, & Donahue, 2010), exhibiting more problems and needing help in more areas (see Holmes et al., 2010; Kaminski, Valle, Filene, & Boyle, 2008).



In summary, the purpose of the current paper is to highlight the results of a comprehensive meta-analysis of evaluation studies of responsible fatherhood programs 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, coparenting 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, African American, unmarried, involvement, intervention, treatment, parenting, child support, divorce, evaluation, assessment, coparenting, 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 Administration for Children and Families; 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 reports. From this list, we identified 270 primary research reports evaluating fathering programs targeting unmarried, never married, low-income fathers for closer examination.

Inclusion Criteria

In this section, we describe the inclusion and exclusion criteria for our meta-analysis in detail. We also refer readers to [Figure 1](#) for a summary of this process. Our initial search yielded 270 potential reports, but only 28 met our inclusion criteria. Two of these reports used the same sample, so they were collapsed into one study ($k = 27$); five other reports employed multiple independent treatment groups, so these were coded as independent studies ($k = 7$). Thus, of the 28 reports, 34 independent studies were identified for coding.



Sample characteristics

We focused on programs for nonresident or low-income fathers. Samples in the included studies were composed primarily of unmarried fathers who did not reside with their children or who were cohabiting with their child's mother. We note here that even though programs may target nonresident fathers, nonresident fathers may still reside with some but not all of their children.

Based on our sample criteria, we excluded 25 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 different and diverse 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 non-incarcerated 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 non-incarcerated 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, & Dawe (2018). By excluding programs focused on reentering and incarcerated fathers, we reduced our search by another 49 studies.

We also excluded studies of programs focused on divorced fathers ($k = 17$). This was a more difficult decision, but our rationale is 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 co-resident with their children. Moreover, many of these divorcing fathers likely have a stronger connection to and longer resident relationship to their children compared to the unmarried fathers targeted by many fatherhood programs. Finally, some studies of economically disadvantaged fathers included both married and unmarried fathers in their samples. Though our target population was unmarried fathers, we chose to keep studies in the meta-analysis if fewer than 35 percent of the sample were 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 ($k = 37$). 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. Still, 37 articles were removed from the analysis due to the lack of reporting crucial statistical information. Because of this, 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 experiment 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 12 experimental and 12 quasi-experimental studies in the analyses.



We also included 10 one-group/pre-post-test studies in our analyses. These non-experimental 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 post-test variables needed to calculate the effect sizes precisely are rarely reported. We estimated pre-post correlations of .50 for these studies, as suggested by Shwartz and his colleagues (2006).

Because qualitative evaluations did not provide us with codable statistics for meta-analysis, we excluded 40 qualitative evaluations from our initial search.

Publication status

Although there were some studies included in our analysis that 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

Because clinical interventions target a specific clinical population, and because they 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. While coding studies, however, we discovered that no clinical interventions 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, a peer support group, or may combine case management with other educational interventions. We included such studies in our meta-analysis.

Language

We found only one study that was not translated into English; this was excluded from our analyses.

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 benefited from the program). Our search yielded 23 such studies that were eliminated from our meta-analysis because they did not focus on the outcomes applicable to our meta-analysis. (For more information on the outcomes examined in this analysis, see the "Outcomes" section below.)

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 two meta-analytically trained graduate students and one undergraduate student who assisted with the collection of the articles. The evaluation of these articles was done in stages, with inclusion criteria identified first before moderators were chosen and coded and statistical information on outcomes for effect size calculations was recorded.

Outcomes

We attempted to code for a wide range of outcomes of interest, 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 regards to positive parenting), coparenting (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). Only one study evaluated program effects on child outcomes (Fagan & Iglesias, 1999), so we cannot include an aggregated report here.

Computing and Reporting Effect Sizes

We used Comprehensive Meta Analysis III (CMA) to calculate post-intervention 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. Very few studies reported any follow-up effects, so these effect sizes are based on immediate post-treatment 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, and k expresses the number of coded studies that contribute to the overall effect size.



Results

Preliminary Analyses

We identified and coded 27 reports containing a total of 34 studies. Of these, 24 employed a control/treatment design, and 10 employed a one-group/pre-post design. Prior to estimating the effects, we conducted preliminary analyses to detect any differences in effects based on the study design (e.g., quasi-experimental versus 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 versus those that did not). In addition, we were concerned that results from one government-funded study—Parents and Children Together, or P.A.C.T., Evaluation Study, with four independent sites totaling more than 5,000 fathers—would heavily weight the overall effect size for all the programs. So, we tested whether the effects for these P.A.C.T. sites were significantly different from the effect sizes for the rest of the studies.

We found no significant difference between the overall effect sizes for experimental-design studies versus quasi-experimental studies ($Q = .10$, ns ; $d_{exp} = .09$, $p < .01$, $k = 12$; $d_{quasi} = .11$, $p = .05$, $k = 12$). We also found no significant difference based on reporting of marital status ($Q = .03$, ns ; $d_{reported} = .11$, $p = .04$, $k = 12$; $d_{not} = .10$, $p < .01$, $k = 12$). 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 ($Q = 13.04, p < .01; d_{journal} = .23, p < .01, k = 13; d_{report} = .03, p = .09, k = 7; d_{diss/thesis} = -.04, p = .78, k = 4$). Finally, although we found that studies that were part of P.A.C.T. had lower effect sizes compared to those that were not part of P.A.C.T, the difference was not statistically significant ($Q = 1.25, ns; d_{PACT} = .071, p = .06, k = 4; d_{not} = .14, p < .01, k = 20$).

We focus the overall results section on the effects of the more rigorous control/treatment designs (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, non-resident 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 10 one-group/pre-post studies producing similar effect sizes to the 24 studies that employed control-group designs.

Aggregate Effects

When all five outcome categories (e.g., coparenting, child support, father economic well-being, father involvement, and parenting) were aggregated into one common measure of program success, programs targeting non-resident, low-income fathers had an overall significant effect ($d = .097, p < .001, k = 24$). (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 the recently reported effects of relationship education efforts in a similar low-income, at-risk population (Arnold & Beelmann, 2018). This effect may be interpreted to mean that, on average, fathers who participated in responsible fatherhood interventions scored about 5 percent higher on the outcomes measured than those fathers in the control group.

Effects on Specific Outcomes

Because the aggregate effect size reported above 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 ($k = 19$) than other outcomes. The average effect size for parenting was small but statistically significant ($d = .111, p < .01, k = 19$). 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 coparenting, and the effect size was also statistically significant but small, although it was slightly larger than the effects of the interventions on the other outcomes ($d = .147, p < .05, k = 14$). 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. And 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 attempt 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 ($k = 24$), and this analysis did suggest potential missing-study bias with a smaller adjusted effect size ($d = .043$). 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 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, coparenting 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. As stated in our results section, when aggregating all effects of the control-group design studies, these programs had a significant, small effect size. However, when exploring the effects more specifically, we found that only father involvement, parenting, and coparenting were significantly impacted. The strongest effect size was in coparenting skills. This was particularly encouraging as the coparenting relationship is one of the most important predictors of nonresident father involvement (Carlson, McLanahan, & Brooks-Gunn, 2008; Fagan & Palkovitz, 2011; McHale & Coates, 2014). Unfortunately, these programs did not significantly impact father employment/economic prospects, and father payment of child support. Since 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 coparenting, it is possible that our findings also reflect differences due to measurement. Fathers may overestimate in their self-reports, while 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 programs and more evaluations of programs that target fathers' employment, economic well-being, and formal or informal payment of child support.

Advancing the Field

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. And in the last 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 (since 2006), 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, 2018).

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.



Statistical reporting

There were many studies that 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, accompanying 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 non-trivial, 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

None of the evaluations we analyzed reported on child outcomes. Considering the fact that the primary rationale for father involvement programs is to positively influence the lives of children, this was disappointing. While the research literature does suggest that features of father involvement, parenting, coparenting, 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 impacts their children's well-being.



Father-only reporting

All of the studies included in our analyses were based on father reports. 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, Day, & Harper, 2014). Regardless, the need for multiple reporters and observational measures of father involvement is evident in this body of work. Meta-analytic studies in the relationship education field 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 are an important element of program effectiveness. Future studies in this area need to do the hard 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 location of the program (e.g., inside versus outside the U.S.), the child development stage, the number of children, multi-partner fertility, and other barriers to father involvement such as incarceration history and employment history (when not included as an outcome being assessed).

In conclusion, though RF programs for low-income, unmarried, nonresident fathers have increased in number and scope, and we have identified a small but significant impact of these programs, we still have more work to do to evaluate our efforts and to increase the impact of these programs. We hope this meta-analytic review will spur and inform more work in this important area.



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*indicates a study included in the meta-analysis.

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Table 1. Effect Sizes for Control-Group Studies

	<i>k</i>	<i>ES</i>	<i>p</i> -value	Adj. <i>ES</i> (trimmed studies)	<i>Q</i> -value	<i>p</i> -value	<i>I</i> ²	Prediction Lower Limit	Interval Upper Limit
Pre-Check: Marital Status									
Reported	12	.110	.036		.028	.868			
Not	12	.100	.004						
Pre-Check: Study Design									
Experimental	12	.091	.004		.103	.748			
Quasi-Experimental	12	.113	.059						
Pre-Check: Report Type									
Journal1	13	.233	< .001		13.040	.001			
Report	7	.029	.088						
Dissertation/Thesis	4	-.037	.777						
Pre-Check: PACT vs. Non									
PACT	4	.071	.064		1.247	.264			
Non-PACT	20	.136	.002						
Overall Program Impact (Aggregated Outcomes)	24	.097	.001	.043 (8)	32.203	.096	28.578	-0.048	0.242
- Co-parenting	14	.147	.033	(0)	36.131	.001	64.020	-0.287	0.581
- Child Support	8	.054	.128	(0)	11.860	.105	40.977	-0.123	0.231
- Employment	6	.030	.103	.026 (2)	2.094	< .001	.000	-0.028	0.088
- Father Involvement	15	.114	.039	(0)	34.777	.002	59.743	-0.221	0.449
- Parenting	19	.111	.003	(0)	21.580	.251	16.589	-0.044	0.266

Note. = Effect size for journals are significantly larger than effect size for reports and dissertations/theses (*k* = 11, *d* = .028, *p* = .099), *Q* = 12.788, *p* < .001.

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

	<i>k</i>	<i>ES</i>	<i>p</i> -value	Adj. <i>ES</i> (trimmed studies)	<i>Q</i> -value	<i>p</i> -value	<i>I</i> ²	Prediction Lower Limit	Interval Upper Limit
Pre-Check: Marital Status									
Reported	7	.030	.647		1.193	.275	--		
Not	3	.193	.147				--		
Pre-Check: Study Design									
Peer Reviewed Journal	5	.172	.006				--		
Report	5	-.028	.768		3.053	.081	--		
Dissertation/Thesis	0	--	--				--		
Overall Program Impact (Aggregated Outcomes)	10	.077	.173	(0)	76.730	< .001	88.271	c	0.521
- Co-parenting	1	.078	.442		.000	1.000	.000		
- Child Support	7	.060	.397	(0)	65.736	< .001	90.873	-0.297	.417
- Employment	0	--	--		--	--	--		
- Father Involvement	1	.516	< .001		.000	1	.000		
- Parenting	3	.069	.123	.057 (1)	.577	.749	.000	-1.644	1.958



Figure 1: PRISMA flowchart describing identification and selection of studies for inclusion in the meta-analysis adapted from Moher et al. (2009)

