

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



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This document was prepared for the Fatherhood Research and Practice Network (FRPN) under grant #90PR0006 from the US Department of Health and Human Services, Office of Planning, Research, and Evaluation (OPRE) to Temple University and the Center for Policy Research, Denver, Colorado. The points of view expressed in this document are those of the authors and do not represent the official views of OPRE.

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 fathers (e.g., The Administration for Children and Families has spent an estimated \$700 million from 2006 to 2018, allocating \$50 to \$75 million a year toward funding responsible fatherhood programs), and rigorous evaluation studies of such programs (e.g., Fagan, 2008; Fagan, Cherson, Brown, & Vecere, 2015; Fagan & Stevenson, 2002; Florsheim et al., 2012; Zaveri, Baumgartner, Dion, & Clary, 2015).

Programs for fathers have typically targeted three broad areas: economic support, father involvement/parenting, and coparenting (Fagan & Kaufman, 2015). Economic support programs 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, Thoennes, Davis, Venohr, Price, & Griffith, 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 nature of coparenting programs depends on the status of the father's relationship with his child's mother. Programs for nonresident fathers focus on improving the relationship with the mother, and teach skills to strengthen the coparenting relationship, how 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; Fagan & Kaufman, 2015). 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).



Despite the numerous programs that have been developed and implemented, few rigorous evaluations have tested their effectiveness. The time is right to synthesize this body of work for practitioners, policymakers, and researchers. This summary report highlights the results of a comprehensive meta-analysis of responsible fatherhood program evaluation studies targeted primarily to unmarried, low-income, nonresident fathers. We asked: How effective are responsible fatherhood educational programs at increasing unmarried, low-income, nonresident fathers' positive father involvement, parenting, coparenting behavior, employment, economic prospects, and child support payments?

Method

To conduct our search, we used academic databases including those reporting dissertations and theses, sought out potential evaluation reports not published in academic outlets, and combed through reference sections of articles and reports for other studies that we may have missed. This search process returned 750 research reports. From this list, we identified 270 primary research reports evaluating fathering programs targeting unmarried, never married, and low-income fathers. Despite identifying 270 potential reports in our initial search, only 28 were appropriate to be included in our meta-analysis. All details regarding our inclusion and exclusion criteria are available in the full report, but based on our inclusion criteria, we excluded studies where married fathers were not specified as low income ($k = 25$), parenting programs that did not specify between the outcomes by participant sex ($k = 41$), programs with insufficient sample description ($k = 7$), programs targeting reentry and incarcerated fathers ($k = 49$), programs targeting divorced fathers ($k = 17$), qualitative studies ($k = 40$), studies that reported insufficient effect size information ($k = 37$), studies listing outcomes not examined in this meta-analysis ($k = 23$), or articles with repetitive information ($k = 3$).

Of the 28 reports that met our inclusion criteria, two 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. Of these, 24 employed a control/treatment design, and 10 employed a one-group/pre-post design.

We coded for a wide range of outcomes, ultimately aggregating outcomes into five categories: father involvement (e.g., any interaction the father had with the 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, 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.

Results

We identified and coded 28 reports containing a total of 34 studies (for a list of all studies included, please see the full report). Of these, 24 employed a control/treatment design, and 10 employed a one-group/pre-post design. 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. All results can be located in the full report. Per standard practice in meta-analysis, this summary report focuses on the effects for the more rigorous control/treatment designs. When all five outcomes (e.g. coparenting, child support,



father economic well-being, father involvement, and parenting) were combined into one common measure of program success, programs targeting nonresident, low-income fathers had an overall significant positive effect ($d = .097, p < .001$). Thus, we conclude that these programs produce small but statistically significant effects for the populations they serve. Though the effects were small, they were comparable to 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 after controlling for pre-test scores than those fathers in the control group.

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 than any others (19 total studies). The average effect size for parenting was small and positive but statistically significant ($d = .111, p < .01$). The next most commonly assessed outcome was father involvement (15 total studies). Again, this effect was small and positive but statistically significant ($d = .114, p < .05$). The third most commonly assessed outcome was coparenting (14 total studies), and the effect size was also statistically significant and positive, and slightly larger than the effects of the interventions on the other outcomes ($d = .147, p < .05$). Finally, the least examined outcomes were a father's child support payment (eight total studies) and a father's employment and economic prospects (six total studies). Unfortunately, these programs did not significantly impact father employment and economic well-being, nor did they significantly impact father payment of child support (child support: $d = .054, ns$; employment/prospects: $d = .030, ns$). 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 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

It is clear to us that there is a continued need for evaluation of programs, especially work focused on unmarried, nonresident, low-income fathers. Unfortunately, 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.

In addition, there are needed improvements in the quality of evaluation research. For example, we found a need for improved statistical reporting, and a need for more mixed-methods studies. Where possible, future evaluations should report means, standard deviations, and group sample sizes at each time point. 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. Further, all of the studies included in our analyses that assessed father involvement, parenting, and coparenting outcomes 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). The need for multiple reporters and observational measures of father involvement is evident in this body of work. As we move forward in this work, it may be encouraging to note that 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) in some cases.

We also found a greater need for reports of attrition, assessment of child outcomes, and observational measures of outcomes. Finally, few studies followed fathers for significant periods of time after completing the interventions to examine whether program effects deteriorated (or grew) over time.

In conclusion, we are encouraged that the most rigorous evaluations of these programs demonstrate statistically significant improvements for low-income, unmarried, nonresident fathers. But 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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