Fatherhood Research & Practice Network

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EXECUTIVE SUMMARY

Framing the Future of Responsible Fatherhood Evaluation Research for the Fatherhood Research and Practice Network

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The Fatherhood Research and Practice Network (FRPN) convened a workgroup of researcher and practitioner experts in the field of fatherhood and father involvement to develop a research agenda that defines the state of the current research in this area, identifies extant gaps in this research, and generates suggestions for future research. The goal is for the workgroup's ideas to inform subsequent funding by the FRPN for evaluation and research projects that seek to enhance our understanding of the important role fathers play in the lives of their children and how programs and policies can strengthen low-income fathers' positive involvement with their children.

One of the overwhelming conclusions of the current status of research is that fathers matter. Previous research clearly finds that the extent to which fathers participate in childrearing carries significant implications for the welfare of a child. This includes research that links father involvement to the child's cognitive development, educational achievement, self-esteem, and pro-social behavior. Fathers who live with their children tend to have positive influences on child wellbeing, but the research findings on nonresident father involvement is considerably more mixed. One reason for the mixed findings is that the quality of the mother-father relationship, whether the parents are still romantically involved, and other factors affect father involvement. Another issue is that many disadvantaged fathers are often simultaneously a resident father in one household and a nonresident father to another household. Nonetheless, research affirms that most fathers, regardless whether they live with the child, have a desire to be involved in their children's lives.

Programs targeting unmarried fathers have traditionally prioritized increasing a father's financial contributions toward his children by increasing employment and, in turn, child support payments. More recent initiatives and funding streams acknowledge the broader role that fathers play in their children's lives. Programs that serve low-income and primarily nonresidential fathers now provide services aimed to increase and improve fathers' amount and quality of involvement with their children.

What is known about the effectiveness of these services is limited. Exhaustive research cataloguing evaluations of fatherhood programs targeting low-income fathers found only eight rigorous studies (i.e., research designs that included a control or comparison group) that provided services aimed to increase and improve fathers' involvement. Most of these studies showed positive impacts but were conducted in a variety of settings. Nonetheless, five obvious gaps exist in the extant research.

1. **There is a lack of research that shows a definitive, causal link between participating in the program and changes in fathers' behavior and children's outcomes.** In general, little is known about the mechanisms within the program curriculum or policies that lead to changes in outcomes, and even less is known about the effect of these programs on a broader array of outcomes that measure the quality of father-child interaction. Since some of the expected outcomes are in the long-run, they cannot be measured in a short-term study. To address this limitation, effective programs need to link the outcomes of the programs to proximate changes in child outcomes.

2. **There is also a lack of evidence as to whether even our more effective programs can translate these benefits across a range of populations of fathers.** For example, will the program outcomes be similar for fathers who are immigrants, incarcerated, have children across multiple households, or who do not live in their child's household? The FRPN may have the opportunity to fund extensions of current evaluations to determine if the programs have similar results across multiple populations and environmental contexts.

3. **There is a need to address concerns about the impact of increased parenting time for low-income fathers in never-married households on parental relationships, family safety, and child wellbeing.** A growing number of initiatives promote the establishment of parenting time for nonresidential parents who were never married to the other parent and ordered to pay child support for child of the never-married parents. We lack research that will inform these initiatives particularly with regard to safety. The field also needs to test methods of engaging and serving custodial parents to elicit their participation in creating parenting plans. In addition, there is a need to research the effectiveness of varying forums (e.g., courts or fatherhood programs), timeframes (e.g., when paternity is established or a parent files a motion for visitation), and formats (e.g., self-help forms or mediation).

4. **There is a need to address several program implementation issues, including recruitment, retention, dosage, fidelity, and scalability.** Most programs have suffered from low participation and high attrition on the part of fathers. To this same point, is there a certain "dosage" or length of time in a program that is necessary to affect change? Can programs be implemented with fidelity and scaled up? The issues of recruitment, retention, and dosage may be important "first steps" in advancing the research agenda.

5. **We need to develop a theoretical framework on par with (but perhaps different from or integrated with) the theoretical model linking mothering to child wellbeing.** The theoretical framework for mothers is typically applied to all populations of mothers, regardless of socioeconomic status, race/ethnicity, or immigrant status, and it generally assumes that the mother lives with child. For fathers, we may need a separate theoretical framework for resident and nonresident fathers, and possibly for various subpopulations (e.g. fathers with children across multiple households, incarcerated fathers, and immigrant fathers). Importantly, we need to rigorously test the causal pathways of any theoretical model that is developed.

Several specific research questions emerge from these research gaps. Do fatherhood programs produce positive changes in father engagement? Which of the core components of a fatherhood program are most effective? What is inside the "black box"? Is it the dosage, approach, service delivery strategy, curriculum? Are some program components more important than others? Do fatherhood programs lead

to changes in fathers' skills, knowledge, attitudes, and behaviors? Are programs associated with changes in father-child contact, the quality of their involvement, their knowledge of parenting and child development, their behavior and the childrearing activities they engage in, and/or their support of the other parent and their co-parenting? What level of program "dosage" is necessary to get impacts on father engagement and child wellbeing outcomes and do the effects "stick" or do they fade out over time? How do father engagement components of a program dovetail with other program components dealing with economic security and/or co-parenting? What child support and visitation policies and programs best promote, and safely allow, never-married, nonresident fathers to be involved with their children? How does program effectiveness differ for fathers with different socioeconomic and life circumstances? How do programs more successfully recruit and retain low-income, nonresident, and never-married fathers into the programs and can we infuse the program elements into our existing policy structures (e.g. schools, prisons, child support system) and communities? Do the programs affect child wellbeing; that is, if child outcomes cannot be measured in the short-term, what proximate outcomes can be linked to subsequent child outcomes? Relatedly, how does father engagement influence child wellbeing? What aspects of engagement are most important for child wellbeing? Does this differ for fathers with different socioeconomic and life circumstances such as race/ethnicity, immigrant status, age, and fathers who have been absent due to incarceration, military service, or abandonment? Fathers who have children across multiple households?

In all, we want to build the field of fatherhood research and identify effective programs that improve father engagement and support, and ultimately child wellbeing. To achieve this, researchers and practitioners will need to work together closely to build a stronger understanding of what the goals of the programs should be and how to best measure the program outcomes. We must also make better efforts to collect data from our fathers who are the hardest to serve. With more theoretical rigor and better data, the research of the FRPN can help facilitate this process.