NORTHWESTERN UNIVERSITY


Balancing Work and Family with Less:

Employment and Welfare Decisions among Single Mothers of Young Children


A DISSERTATION


SUBMITTED TO THE GRADUATE SCHOOL IN PARTIAL FULFILLMENT OF THE

REQUIREMENTS


for the degree


DOCTOR OF PHILOSOPHY


Field of Human Development and Social Policy


By

Heather D. Hill


EVANSTON, ILLINOIS

December 2007

ABSTRACT

Balancing Work and Family with Less:

Employment and Welfare Decisions among Single Mothers of Young Children

Heather D. Hill

This dissertation presents the results of three studies examining the determinants and consequences of employment among single mothers with infants and toddlers. In the first study, I use nationally-representative survey data to identify the effect of age-of-youngest-child welfare-to-work exemptions on the employment of single mothers with no more than a high school degree. These exemption policies, which vary by state and calendar year, determine when welfare recipients must comply with program work requirements after having a child. I estimate that eligibility for an exemption decreases the probability of working, suggesting that the shortening in recent years of these state-level welfare exemptions has hastened, on average, single mothers' employment after a pregnancy.

The second study combines quantitative and qualitative data to explore the context of maternal work decisions following the birth of a child in cohabiting (unmarried) families. To systematically generate and test hypotheses that capture the complexity of return-to-work decisions, I use an inventive combination of Qualitative Comparative Analysis (QCA), narrative analysis of qualitative data, and conventional statistical analyses. These analyses indicate that mothers' employment prior to the birth is positively associated with employment within the first year after the birth. Also, for these unmarried couples, the influence of father's employment or income on maternal work decisions is less uniformly negative than theory would predict. This

chapter also highlights the promise and challenges of using "mixed methods" to examine complex social phenomena.

The final study estimates the effects of maternal employment on young children's cognitive and socioemotional development using data from five randomized experiments of welfare reform programs. In the estimation model, maternal employment is instrumented by random assignment to the treatment group, an exogenous predictor of maternal employment. While OLS estimates indicate that both level and stability of employment are positively related to children's socioemotional development, decreasing problem behavior, this result does not hold in instrumental-variable (IV) models. IV models do estimate large positive effects of both level and stability of employment on school achievement, but these effects decrease in size and lose significance in models controlling for income and welfare receipt.

ACKNOWLEDGEMENTS

A dissertation masquerades as an individual accomplishment, but it is truly a team effort. Throughout my life, I have been fortunate to be supported by a collection of family, friends, and mentors who nurtured and pushed me to this place. Professionally, I am most grateful to my advisor, Greg Duncan. To the extent that I am successful at producing careful and conscientious research, it is due to the model and training provided by Greg. To the extent that I fall short, I am always striving to reach the high standards that he sets for himself and for his students and colleagues. Greg also taught me the only econometric humor I know: "That's the way the stochastic cookie crumbles."

My dissertation committee members, Chris Taber and Lindsay Chase-Lansdale at Northwestern University and Paula England at Stanford University, could not have been more generous with their time, knowledge, and guidance throughout this process. Each of them shaped my interests and my approach to research in substantial and unique ways. My Ph.D. training and the development of my research program have also been influenced by the work and mentoring of Kathy Edin at Harvard University. In addition, I am forever in debt to three incredible researchers and mentors who introduced me to the study of poverty and welfare policy and encouraged me to pursue a Ph.D: Sheldon Danziger and Mary Corcoran at the University of Michigan and LaDonna Pavetti at Mathematica Policy Research, Inc.

Four fantastic women who began the Human Development and Social Policy Ph.D. program with me—Amy Claessens, Mimi Engel, Leah Doane, and Anita Zuberi—have been

exceptional friends and colleagues along the way. I thank them and my other HDSP friends for their laughter and sympathetic ears, as well as their expertise and scholarly advice.

A portion of this dissertation, described in Chapter Four, was conducted as part of the Next Generation project, led by the non-profit research firm MDRC. I have benefited greatly from the training and structure provided by the project, as well as the opportunity to work with a group of exceptional researchers at MDRC and other academic institutions. I am also grateful for the use of MDRC's laboriously collected data. Most importantly, I am thankful to Pamela Morris, from whom I learned immeasurably about research methods and the links between social policy and child well-being. The specific analyses presented in Chapter Four were also aided substantially by the ideas of Jessica Thornton at the University of Texas, Austin, as well as research assistance by Desiree Alderson and Francesca Longo at MDRC and Constance Lindsay at Northwestern University. Finally, other senior members of the Next Generation team, Lisa Gennetian, Aletha Huston, and Katherine Magnuson, provided thoughtful comments and suggestions on these analyses. Katherine also generously provided coding of state child care expenditures for Chapter Two.

The gratitude I feel for my family in this and all other life successes is beyond expression. My parents, Steve and Sandy Hill, have offered me unconditional validation and support at every twist and turn in my life, an exceptionally rare and generous quality of parenting. Equally important, they set high expectations for me and my sister, Tessa Hill, as students, citizens, and humans. Ironically, my little sister takes on most of life's great challenges before me, excels at them, and goes on to advise me. She is the professor, mother, partner, and sister that I strive to be myself.

Finally, in the midst of writing the proposal for this dissertation, I took a second look at a friend and fell in love with my soon-to-be husband, Scott Allard. He turned my life upside down in so many wonderful ways, but he also provided me with the stability and support to come through the dissertation-writing process with my sanity (mostly) intact. He managed to inspire me, nurture me, laugh at me, and bring me flowers at all the right moments. My work and my life are immeasurably better for his friendship and love. This dissertation is dedicated to him.

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CHAPTER ONE

INTRODUCTION

The dramatic influx of mothers into the paid labor force in the latter half of the 20th century was one of the most profound transformations in the history of American families. While most children in the early 1960s had only one working parent, a majority now live in families with two earners or a single working mother (Waite & Nielson, 2001). Labor force participation rates have increased most dramatically for married mothers, more than three-quarters of whom were employed in 1996 compared to just less than half in 1967 (Meyer & Rosenbaum, 2000). In the early 1990s, however, when work rates among married mothers had generally plateaued, labor force participation among single mothers began to climb precipitously, ultimately surpassing that of married mothers. A large part of this increase has been attributed to welfare reform and expansions of the Earned Income Tax Credit (EITC), changes that conditioned income assistance for poor families, primarily single mothers, on employment during a period of historically high demand for workers (Blank, 2002; Ellwood, 2000; Grogger, 2004b; Meyer & Rosenbaum, 2000).

Increases in labor force participation have been largest among single mothers with preschool-aged children. By 2002, data from the Current Population Survey (CPS) indicate that 58 percent of single mothers with children under age five worked for pay, up from 45 percent just 12 years earlier and six percentage points higher than their married counterparts. Such increases in work activity are consequential because infants and toddlers appear to be particularly sensitive to changes in family income and processes, both of which can be affected

by maternal employment (Duncan & Brooks-Gunn, 1997; V.C. McLoyd, 1998; Shonkoff & Phillips, 2000; Thompson, 2001). In addition, the challenges of balancing work and family responsibilities can be particularly acute for parents of very young children, who are not yet in school but need constant supervision and care. These challenges are only amplified for single mothers, who are disproportionately likely to be poor, raising children alone, and working in low-wage jobs without health insurance or other employee benefits (Knitzer, 2001; Lambert, 1999).

There is no shortage of research that is relevant to understanding the relationships between government policy, single mothers' work decisions, and the well-being of young children. There are, however, important substantive gaps and methodological limitations in our existing knowledge on these topics. On the one hand, there are observational studies of maternal employment and its effects on children. These studies suggest that employment in the first year of life has a small negative effect on later cognitive outcomes, which can be offset by positive effects of maternal employment in the second and third years of a child's life (Baydar & Brooks-Gunn, 1991; Brooks-Gunn, Han, & Waldfogel, 2002; Desai, Chase-Lansdale, & Michael, 1989; Waldfogel, Han, & Brooks-Gunn, 2002). However, these effects are often limited to white children or those in married-parent families and their generalizability to less advantaged populations is unclear (Moore & Driscoll, 1997; Smith, Brooks-Gunn, Klebanov, & Lee, 2000; Vandell & Ramanan, 1992). In addition, only recently have studies on this topic used econometric techniques to carefully control for both observed and unobserved differences between employed and unemployed mothers(e.g. Baum, 2003; e.g. J. L. Hill, Waldfogel, Brooks-Gunn, & Han, 2005; Ruhm, 2004). These approaches produce results of similar size and

direction to the OLS models, but can be more confidently viewed as estimates of causal relationships. In at least one case, the negative effects of maternal employment in the first year were still limited to children in married-parent families (J. L. Hill, Waldfogel, Brooks-Gunn, & Han, 2005), but this has not been explored systematically in the literature.

On the other hand, experimental and non-experimental studies of social policy changes in the 1990s indicate that welfare work requirements and other employment-focused policies increased employment among single mothers and had neutral or positive effects on children two to five years of age (Dan Bloom & Michalopoulos, 2001; Hamilton et al., 2001; Morris, Duncan, & Clark-Kauffman, 2005; Morris, Huston, Duncan, Crosby, & Bos, 2001). However, this literature is largely silent about policy changes that directly targeted parents of infants and toddlers. Most studies of welfare programs do not include measures of well-being for children under two, and the few that do find few if any effects, positive or negative, on maternal participation in experimental welfare programs on cognitive or behavioral development among children ages zero to two (H. D. Hill & Morris, 2007; Morris & Michalopoulos, 2003). Also, experimental programs almost always bundle services or policies, which limits their application to estimating the effects of specific components of welfare policy that were targeted at families with young children.

In this dissertation, I present results from three distinct studies of employment among single mothers with young children that seek to fill some of the gaps in the existing literature. Two of the studies concern maternal decision-making about employment after a birth. In one case, I estimate the effect of a specific welfare policy on employment rates and in the other I engage in exploratory analyses to better understand maternal employment decisions in the

context of the family and the labor market. The final study focuses on how young children are affected by maternal employment.

These studies use diverse data sources, including a nationally-representative survey, a set of experimental welfare programs, and combined survey and in-depth interview data from a study of new parents. The analytic methods are also uniquely varied. Two studies estimate regression models using techniques for addressing unobserved heterogeneity, called difference-in-differences and instrumental-variable estimation. The other is both substantively and methodologically exploratory, in that I combine conventional quantitative and qualitative methods with Qualitative Comparative Analysis (QCA), a sociological method well-suited to theory-building but not widely applied to studies of individual behavior or policy-relevant questions. I describe the studies in more detail below and Table 1.1 briefly summarizes the research question, data sources, and methods of each.

*The Studies*

Chapter Two describes the first empirical study of this dissertation. In this study, I used data from the June Fertility Supplement of the Current Population Survey (CPS) to estimate the effects of a specific welfare rule on employment rates of single mothers with young children. Federal law passed in 1996 conditioned eligibility for welfare benefits on employment or participation in work-related activities and gave states the discretion to make this requirement more or less stringent using a variety of policy and administrative options. One such option was to set the amount of time before welfare recipients were required to meet work requirements following the birth of a child, a policy called the age-of-youngest-child exemption. This change increased policy variation, but also radically decreased the average length of exemptions. In

2005, prior to welfare reforms, the vast majority of states had an exemption of 36 months. By 1998, exemptions ranged from 0 to 48 months, depending on the state, but ninety percent of states had an exemption of 12 months or shorter (Rowe & Roberts, 2004). Movement over time toward shorter exemptions became a fundamental component of federal and state efforts to encourage employment among single mothers and discourage long-term welfare receipt.

Table 1.1 Descriptions of Dissertation Studies

| Study | Research question | Data | Methods |
|---|---|---|---|
| 1 | Do exemptions from welfare work requirements affect employment rates among welfare-eligible mothers with young children? | Current Population Survey, June Fertility Supplement 1990-2000 | Difference-in-difference-in-differences |
| 2 | How do mother's human capital and family circumstances interact to determine the timing of employment after a birth? | Fragile Families and Child Well-Being study and Time, Love, and Cash among Couples with Children study | Qualitative Comparative Analysis, Case studies, & OLS regression |
| 3 | What effect does maternal employment in the first three years of life have on children's cognitive and socioemotional development? | Next Generation dataset of participants in MDRC welfare experiments | OLS & IV regression |

This variation across states and over time provides a "natural experiment," a situation in which policy changes provide an opportunity to observe two comparable groups that were affected differentially by a policy. I use a common statistical technique associated with natural experiments, the difference-in-difference-in-differences approach, to identify the employment effects of age-of-youngest-child exemptions. This approach takes advantage of variation across

time and states, and between the "treatment" group (defined as single mothers eligible for an exemption) and appropriate comparison groups (e.g. single mothers with older children, married mothers with infants and toddlers), to isolate the behavioral impacts of a specific policy net of individual difference and concurrent policy or economic changes.

The second study, described in Chapter Three, combines quantitative and qualitative data to explore the context of maternal work decisions following the birth of a child in cohabiting (unmarried) families. The analysis is based on data from the Fragile Families and Child Well-Being survey and an embedded qualitative study called Time, Love, and Cash among Couples with Children (TLC3). In order to systematically generate and test hypotheses, I use an inventive combination of small sample analysis using Qualitative Comparative Analysis (QCA) and conventional statistical analyses with a larger survey sample.

Qualitative Comparative Analysis is a technique developed by Charles Ragin (1987) for cross-case comparisons and causal inference that combines attributes of conventional qualitative and quantitative methods. Unlike regression techniques, QCA does not estimate the relationship between any single variable and an outcome. Instead, the method assumes "maximum causal complexity," by first identifying all logical combinations of the independent variables of interest and then minimizing those combinations using the laws of Boolean algebra (Ragin, 1987, , 2000). QCA is quite deliberately case- rather than variable-oriented, but unlike conventional case study approaches, it also tries to produce generalizable results using cross-case comparisons. I use narrative case study analysis to elucidate the QCA findings and develop hypotheses, which I then test using more conventional statistical methods and a larger sample from the Fragile Families study.

The final empirical chapter of the dissertation, Chapter Four, describes a study of the effects of early maternal employment on child development. I combine experimental data with an instrumental-variable estimation strategy to identify the effect of hours of maternal employment during infancy and toddlerhood on later child achievement. The data for this study is pooled from a set of randomized experiments of different approaches to welfare reform conducted by MDRC during the 1990s. As with many questions in social science, the key challenge to research on the effects of early maternal employment on children's cognitive and socio-emotional development is selection bias. There are many characteristics of mothers, families, and children that are associated both with the decision to work and child outcomes, and it is impossible to control for them all. This study, like many of the more recent contributions to the large body of research on this topic, uses econometric techniques to address this issue. I use random assignment to welfare reform programs, a source of exogenous variation in maternal employment, as an instrument for actual level and stability of employment.

*The Contributions of the Dissertation*

Together, these studies make three primary contributions to scholarship in this area. First, this dissertation focuses on families with children in infancy and toddlerhood, highly productive developmental periods in which physical, cognitive, and psycho-social capacities are changing quickly and meaningfully (McCall, 1981; Shonkoff & Phillips, 2000). In the study of poverty's effects on children, and interventions designed to mitigate those effects, early childhood is considered a period of "unique opportunity and vulnerability" (Thompson, 2001). Yet, studies of welfare reform have neglected this age group of children.

Second, each of the three studies is an ambitious methodological enterprise. The two quantitative studies in this dissertation tackle the substantial methodological challenges inherent to estimating policy and employment effects. While true social experiments in which individuals are randomly assigned to treatment and control groups are widely considered the "gold standard" of social science (McCall & Green, 2004; Rutter, 2005), they are not always practically or ethically possible. The challenge in these cases is to identify causal relationships that we can argue confidently are not biased by unmeasured differences between working and nonworking mothers, or between families affected by a policy and other families. In this dissertation, I use two of the most common approaches to controlling for unobserved heterogeneity in non-experimental data, instrumental variables and difference-in-difference-in-differences.

The mixed method study ventures into largely uncharted territory in the study of maternal employment and children by combining a set of methods not commonly considered compatible. The primary method, QCA, was developed by Charles Ragin (1987) in order to bridge the gap between rich qualitative description quantitative and qualitative paradigms. Consistent with current perspectives in human development, including family systems and ecological theories, the logic of QCA is that social science phenomena are generally caused by a combination of factors and, furthermore, that different combinations may produce the same outcome. However, few studies have used QCA to examine individual behaviors or current policy questions. While experiments and econometric techniques are the cornerstone of policy-relevant research, Ragin rightly points out that policy discussions are often about clusters of conditions rather than the independent effect of individual factors that regression methods estimate (Ragin, 2006).

Finally, the title of this dissertation "Balancing Work and Family with Less" signifies my hope that these studies can be viewed in the context of the larger discussion of how parents balance the responsibilities of work and family. This is a popular topic in both mainstream and academic circles, but one that is discussed primarily in reference to two-parent, middle- to upper-class families. For single and low-income parents, the universal challenges of competing demands and finite time are often exacerbated by the lack of a co-parent or sufficient resources. While it has not generally been conceived of in these terms, the social policy changes in the 1990s changed the expectations and choices related to balancing work and family for poor parents. This shift, in both philosophy and reality, was particularly abrupt for poor mothers with very young children for whom social programs had long offered financial support for stay-at-home parenting. A better understanding of both the determinants and consequences of single mothers' decisions to work should be central to the broader discussion of the work-family balance.

CHAPTER TWO

WELFARE-TO-WORK EXEMPTIONS AND MATERNAL EMPLOYMENT RATES:

DID WELFARE REFORM HASTEN SINGLE MOTHERS' RETURNS TO WORK

FOLLOWING THE BIRTH OF A CHILD?

Social policy reforms in the 1990s worked in tandem with historically low unemployment rates to dramatically increase labor force participation among single mothers. At the heart of these reforms was the reorganization of welfare cash assistance into a time-limited employment program for low-income parents. Federal law passed in 1996 conditioned eligibility for welfare benefits on employment or participation in work-related activities and gave states the discretion to make this requirement more or less stringent using a variety of policy and administrative options. One such option was to set the amount of time before welfare recipients were required to meet work requirements following the birth of a child, a policy called the age-of-youngest-child exemption. Movement over time toward shorter exemptions became a fundamental component of federal and state efforts to encourage employment among single mothers and discourage long-term welfare receipt. Yet, the effects of this policy have not been studied.

JOBS, the original work program associated with welfare cash assistance in the early 1990s, did not require employment for recipients with children less than 36 months of age. In the middle 1990s, several states received approval from the federal government to implement shorter exemption lengths and in 1996, this policy became a state, and in some cases county, option under the Temporary Assistance for Needy Families (TANF) program. This change increased policy variation, but also radically decreased the average length of exemptions. By 1998,

exemptions ranged from 0 to 48 months, depending on the state, but ninety percent of states had an exemption of 12 months or shorter (Rowe & Roberts, 2004).

This policy shift has received surprisingly little attention given its potential to affect children during a particularly important developmental stage. Infancy and toddlerhood are periods of fast-paced and substantive change in all domains of development, the results of which provide a foundation for later development (Shonkoff & Phillips, 2000). Early childhood, particularly infancy, is also a time when it is more difficult to find non-parental child care, both because a limited number of child care providers will take infants and because mothers are less likely to trust child care providers with very young children (Hofferth, 1992; Kirby, Ross, & Puffer, 2001). For women with fewer resources, the challenges of finding acceptable child care may be even greater, but the need for income from earnings is likely to be greater as well.

More so than older children, infants and toddlers appear susceptible to changes in the family environment, both positive and negative (Graber & Brooks-Gunn, 1996). For instance, studies find few consistent effects of maternal employment on children's development, with the exception of employment in the first year of life, which has small negative effects on children's cognitive outcomes in middle childhood (Baum, 2003; Berger, Hill, & Waldfogel, 2005; J. L. Hill, Waldfogel, Brooks-Gunn, & Han, 2005; Ruhm, 2004). There is also increasing evidence that early returns to work decrease the probability and shorten the length of breast-feeding, both of which have potentially negative consequences for children's physical and cognitive development (Haider, Jacknowitz, & Schoeni, 2003; Roe, Whittington, Fein, & Teisl, 1999).

The lack of debate about these policies is particularly striking when juxtaposed against the more vigorous public discussion of family leave benefits. The Family and Medical Leave

Act (FMLA) offers 12 weeks of unpaid leave with job protection, but has limited coverage, particularly for low-wage workers. To the extent that age-of-youngest-child exemptions allow mothers to stay at home with infants, the policy is essentially paid family leave for single mothers without job protection. Employer-based family leave policies have offsetting effects on maternal employment by increasing employment during the pregnancy and after the leave period, but decreasing maternal employment during the leave period (Baum, 2003; Berger & Waldfogel, 2004; Joesch, 1997). In contrast, exemptions from welfare work requirements are more likely to have unambiguously negative effects on mothers' employment before and after the birth. Exemptions may create incentives for women to quit work leading up to a birth because the policy is not employer-based and offers no job protection. Also, the option of receiving cash assistance without having to meet work requirements is likely to make it easier for mothers to remain unemployed for a longer period of time after the birth. The question addressed in this paper is whether these incentives are large enough to affect maternal decisions about work (and, if so, how large), particularly in the context of the stigma of welfare and time limits on welfare receipt.

The present study uses six years of data from the 1990-2000 June Fertility Supplements of the Current Population Survey (CPS) to estimate the effect of age-of-youngest-child exemptions from welfare work requirements on the employment rates of single mothers. The statistical approach I use takes advantage of policy variation over time and across states, as well as between a pseudo-treatment group—single mothers with no more than a high school diploma or GED—and an appropriate comparison group—married or high education mothers with

children of the same age—to isolate the behavioral impacts of exemptions from work requirements, net of individual differences and concurrent policy or economic changes.

*Background*

While women with preschool-aged children have always worked at lower rates than women with school-aged children, they have experienced the largest increases in employment rates over time. The percentage of women with children under age five who were employed increased by 36 percentage points between 1975 and 2002, compared to a 29 percentage point increase for mothers of older children (U.S. Department of Labor, 2005).[1] Recent studies have also highlighted a particularly steep increase in labor force participation among single mothers during the 1990s. Eighty-two percent of single mothers worked in 1996, compared with only 73 percent in 1988 (Meyer & Rosenbaum, 2000). Women with less education, no more than a high school diploma or GED, also entered the paid labor force at higher rates throughout the 90s, although they were still employed at lower rates than women with more education (author's calculations using the CPS).

The increase in labor force participation among single and low-education mothers in the late 1990s coincided with transformative reforms to many aspects of U.S. social policy. Policy changes that encouraged or mandated employment among welfare recipients began at the state level in 1992, and were implemented federally in 1996 with the passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA). As a consequence of federal welfare reform, government financing of child care subsidies also increased, from $2.1

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[1] These statistics refer to employment, not labor force participation.

million in 1997 to $7.4 million in 2000 (Adams & Rohacek, 2002). In addition, the Earned

Income Tax Credit (EITC)—which supplements the income of low income working parents—

was expanded repeatedly throughout the decade by increasing credit rates and broadening the

eligible population. The income taxes of a working single mother of two or more children

decreased by nearly $2,000 between 1984 and 1996, and most of the change occurred after 1993

(Meyer & Rosenbaum, 2000). By 2001, 11 states had refundable credit programs similar to those

offered at the federal level (N. Johnson, 2001).

Studies consistently find that these policy changes combined with historically low

unemployment rates to produce large increases in labor force participation among single mothers

(Ellwood, 2000; Fang & Keane, 2004; Grogger, 2004b). The specific effect of "welfare

reform"—the bundle of policy changes that made cash assistance both time-limited and tied to

work effort—on employment rates is estimated to be approximately 3 to 4 percent, which is

substantial but still lower than the estimated effects of the economy and EITC expansions (Fang

& Keane, 2004; Grogger, 2003, , 2004b; Grogger & Karoly, 2005; Meyer & Rosenbaum, 2001).

Reductions in unemployment are believed to account for 20 to 30 percent of the increase in

single mothers' employment rates between 1993 and 1999 (Grogger, 2003; O'Neill & Hill,

2001). There is consistent evidence that the largest share of the increase, about 60 percent, can be

attributed to expansions of the Earned Income Tax Credit (Blank, 2002; Fang & Keane, 2004;

Grogger, 2003; Meyer & Rosenbaum, 2000, , 2001).

The majority of evidence on the labor market effects of specific components of welfare

policy comes from experimental studies conducted prior to federal welfare reform. In these

studies, work requirements are consistently shown to increase employment rates among program

participants, although the effects vary in size considerably. Of the 11 sites of the National Evaluation of Welfare-to-Work Strategies, six had significant impacts on employment rates in the first year of the program, with effects varying in size from 4.7 to 16.5 percent (Grogger & Karoly, 2005; Hamilton et al., 2001). Two observational studies of components of welfare policy have results with particular relevance to families with infants and toddlers. Using CPS data, Grogger (2003) finds that time limits had particularly large positive effects on employment rates of mothers with young children. Haider et al. (2003) link more stringent welfare work requirements to reductions in breast-feeding rates with a measure of stringency that includes the length of age-of-youngest-child exemption length.

Child care subsidies, a related policy, are clearly related to employment rates. There is strong evidence that the price of childcare (which can be viewed as a tax on maternal earnings) is negatively associated with maternal employment, particularly for low-skilled and single women (Anderson & Levine, 1999; Baum, 2002; Wenjui Han & Waldfogel, 2001; Michalopoulos, Robins, & Garfinkel, 1992). Most studies find employment elasticities of -0.3 to -0.4, suggesting that a 10 percent increase in child care subsidies would increase maternal labor force participation by 3 to 4 percent (Gornick & Meyers, 2003). The substantial marginal effect of an increase in child care spending on single mothers' work effort has been estimated as equal to, or greater than, that of a comparable increase in EITC benefits (Bainbridge, Meyers, & Waldfogel, 2003).

While no study that this author knows of has tested the labor market effects of exemptions from welfare work requirements after a birth, some related areas of research on family leave policies may be informative. A growing set of studies show that leave policies

increase the probability that women will take leave after a birth and lengthen the average leave, but also increase the probability that a woman will return to her pre-birth employer and return to work within 12 weeks of the birth (Berger & Waldfogel, 2004; Glass & Riley, 1998; Hofferth, 1996; Joesch, 1997). Yet, government-mandated family leave differs from age-of-youngest-child exemption policies because it is most often unpaid, but guarantees job security during the period of the leave. In contrast, age-of-youngest-child exemptions provide paid leave without job protection, potentially creating an incentive for mothers to quit jobs leading up to the birth, particularly since they are unlikely to have employer-provided paid sick leave or vacation (J. S. Heymann & Earle, 1998). So, while family leave policies increase the likelihood that women will take leave (decrease work) but maintain employment, exemptions from welfare work requirements are more likely to decrease employment rates in the months surrounding a birth.

While it is likely that the direction of any effect of exemptions on maternal employment is negative, it is difficult to predict what the size of that effect will be. Among a set of experimental welfare reform programs focused on mandating employment, impacts on employment rates ranged from 0 to 15 percent (Grogger & Karoly, 2005). For the most part, non-experimental estimates of how the full array of welfare reforms passed in the late 1990s affected employment rates among single or low-education mothers hover around three percent. Several specific policy changes, including increasing welfare benefits by $1000 annually (Meyer & Rosenbaum, 2000) and decreasing child care subsidies by 10 percent (Gornick & Meyers, 2003), have been found to decrease employment rates in this population by about three percentage points. My expectation is that if there is an effect of exemptions on employment rates

of single, low-education mothers with young children it will be negative and less than five percent.

*Methods*

*Data*

      The primary source of data for this study is the June Fertility Supplement of the Current Population Survey (CPS). The CPS, the source of official government statistics on employment, is conducted once a month on a nationally-representative sample of approximately 48,000 households in the U.S. Standard monthly questions focus on employment status and characteristics (e.g. hours worked, occupation, wage) and demographics including race, sex, and marital status. The June CPS, which has been conducted on a semi-regular basis since 1971, adds questions for women of childbearing age regarding historical and planned fertility. In addition, all household members over the age of 15 are asked the standard monthly questions about employment in the week prior to the survey.

      For the purposes of this study, the principal advantage of using the June Fertility Supplement is that it collects both the birth *month and year* of the respondent's youngest child. Having birth month of the youngest child allows for much more exact calculation of child age and for division of the sample by child age in months. This is crucial because the vast majority of women in the U.S. return to work within the first year of a child's birth, and welfare work exemptions are determined by child age in months.

      My analyses used data from the six years between 1990 and 2000 in which the June Supplement of the CPS was fielded: 1990, 1992, 1994, 1995, 1998, 2000. This 10-year period is ideal for my purposes because it included years before and after state and federal welfare

reforms, which began in 1992 but were widely enacted in 1996 & 1997. The June supplement to the CPS was not conducted in 1996 and 1997, the years in which states implemented their TANF programs. Even if data had been collected in these years, however, there would have been good reason to exclude them from this analysis because work requirements and exemption policies were likely changing too quickly during this period to have clear effects on maternal work decisions.

*Sample*

I analyzed a stacked cross-sectional dataset of mother-level observations from all six years of data. The sample for this analysis is female respondents to the CPS who were 18-44 years of age and had a youngest child 0-60 months old. The CPS has a panel structure in which individuals are interviewed in four consecutive months, not interviewed for the next eight months, and then interviewed again for four consecutive months. In most years of this study, that structure is irrelevant because the June Supplement was not conducted in consecutive years. However, approximately fifty percent of sample members who were interviewed in 1994 were also interviewed in 1995. These years also coincide with a redesign of the CPS that prohibits matching observations across the surveys in 1994 and 1995 (Bureau of Labor Statistics). To maintain the independence of observations, I dropped the 1995 observations for sample members who were surveyed in both years.

I also dropped observations that were missing the age of the youngest child or the dependent variable (labor force participation in the week prior to the survey). In addition, in three states—California, Colorado, and New York—for which I am using county-level variation in exemption length, I dropped observations that were missing a county identifier. The resulting

sample numbers 42,091 individuals.  Table 2.1 presents sample sizes for the full sample and the

sub-groups described below.

Table 2.1 Case Counts by Welfare and Age-of-Youngest-Child Exemption Eligibility

| Year | Total | Proxy for Welfare Eligibility[a] | | Eligibility for Exemption[b] | |
| | | Single and Low-Education | Married or High-Education | Child age under limit | Child age over limit |
| --- | --- | --- | --- | --- | --- |
| 1990 | 8,939 | 1,638 | 7,301 | 6,087 | 2,852 |
| 1992 | 8,633 | 1,649 | 6,984 | 5,860 | 2,773 |
| 1994 | 7,981 | 1,493 | 6,488 | 5,026 | 2,955 |
| 1995 | 3,875 | 773 | 3,102 | 2,358 | 1,517 |
| 1998 | 6,353 | 1,220 | 5,133 | 1,326 | 5,027 |
| 2000 | 6,310 | 1,212 | 5,098 | 1,203 | 5,107 |
| Total | 42,091 | 7,985 | 34,106 | 21,860 | 20,231 |

Notes.
[a]"Single" defined as never married, divorced, or widowed. "Low-ed" defined as having no more than a high school diploma or GED.
[b]Eligibility for age-of-youngest-child exemption is determined by the age of the youngest child and the policy in a given state and year.

Previous studies of welfare policies have generally used either marital status or

educational attainment as a proxy for welfare eligibility (e.g. Hao & Cherlin, 2004; Meyer &

Rosenbaum, 2000, , 2001), and both characteristics are, in fact, strongly associated with welfare

receipt. The Department of Health and Human Services reports that in the period between

October 1999 and September 2000, 88 percent of TANF recipients were single (including never

married, widowed, separated, and divorced individuals) and 94 percent had no more than 12

years of schooling.

Instead of considering either single or low-education parents as the population eligible for

welfare, I used individuals who met both criteria (7,985 women in this sample). I argue this is a

better definition because it excludes single parents with higher education, a group with

substantially different employment and wage rates than single parents with low education. It also

excludes married individuals with low-education, which is appropriate given that the rules for two-parent welfare-to-work programs differ considerably and changed differently over time from standard welfare rules.

One concern with using these demographic characteristics as proxies for welfare eligibility is that welfare policies themselves may affect individuals' decisions about marriage, fertility, and education. The identification strategy of this study depends on the assumption that this is not the case, that being single and having no more than a high school diploma or GED is exogenous to the length of age-of-youngest-child exemptions. It is conceivable that longer age-of-youngest-child exemptions would provide an incentive for additional childbearing, although many states limit the total number of months or children for which the parent can be exempted. It is harder to imagine, however, how exemption policies would affect marital status. Perhaps of most concern is the possibility that longer exemption policies would increase educational attainment among welfare recipients by providing an income subsidy without the requirement of employment.

Existing research makes it difficult to say whether the assumption that exemption policies are not associated with marriage, childbearing, and educational outcomes is a reasonable one. Studies of how welfare policies and benefits affect marriage and fertility decisions have produced mixed results. Some find little or no effect (Acs, 1996; Gennetian & Knox, 2003; Hoyne, 1997; Theodore Joyce, Kaestner, & Korenman, 2002; Ryan, Manlove, & Hofferth, 2006), while others find evidence that lower benefits and stronger work incentives reduce divorce rates (Bitler, Gelbach, Hoynes, & Zavodny, 2004; Hu, 2003) and that higher base benefit levels lead single mothers to delay marriage and hasten a subsequent birth (Grogger & Bronars,

2001). The evidence is mixed even for family cap policies, which are designed to directly affect fertility decisions (Grogger & Bronars, 2001; Jagannathan, Camasso, & Killingsworth, 2004; Ted Joyce, Kaestner, Korenman, & Henshaw, 2004; Ryan, Manlove, & Hofferth, 2006). In the wake of federal welfare reform, declines in post-secondary enrollment among welfare recipients were attributed to work requirements (Jacobs & Winslow, 2003), but this author knows of no studies that estimate the causal relationship between work requirements and adult education.

*Measures*

Table 2.2 presents descriptive statistics for the variables included in this analysis averaged over the full study period. The means presented are population estimates weighted for CPS sample selection and non-response. A binary variable equal to one if the respondent was employed (working or not) prior to the survey is the dependent variable in this analysis. During the period of the study, 53 percent of women age 18-44 with children under the age of five were employed.

All models controlled for age of youngest child with a set of 60 dummy variables for one-month age groups, 0-59. Zero months of age was the omitted category. Other control variables included the respondent's race and age, as well as her lifetime number of births. Race is Black and race is other were included in the models; race is white was the omitted category. [2] Mother's age and number of births were both continuous variables. According to my definition of welfare eligibility, being single and having no more than a high school diploma, 20 percent of mothers 18-44 with a youngest child under 60 months were eligible.

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[2] I do not control for being ethnically Hispanic because the CPS did not collect these data in the June Supplements conducted in the early 1990s.

Table 2.2 Descriptive Statistics for Variables in Analysis

| Variable Name | Unit | Range | Mean[a] |
|---|---|---|---|
| Outcome of interest | | | |
| Employed in prior week | Yes/No | 0-1 | .53 |
| | | | |
| Independent variables of interest | | | |
| Single (never married, divorced, or widowed) | Yes/No | 0-1 | .30 |
| Low education (no more than HS diploma/GED) | Yes/No | 0-1 | .50 |
| Welfare target population (single and low-ed) | Yes/No | 0-1 | .20 |
| Eligible for age-of-youngest-child exemption | Yes/No | 0-1 | .38 |
| | | | |
| Mother's characteristics | | | |
| Age | Years | 18-44 | 29.67 |
| Number of babies | Integers | 1-18 | 2.10 |
| Age of youngest child | Months | 0-59.99 | 26.80 |
| Race | | | |
| White | Yes/No | 0-1 | .79 |
| Black | Yes/No | 0-1 | .15 |
| Other | Yes/No | 0-1 | .05 |
| | | | |
| State economic/policy conditions | | | |
| Length of age-of-youngest child exemption | Months | 0-48 | 22.78 |
| Full family sanction | Yes/No | 0-1 | .38 |
| Required TANF work participation rate for state | Fraction | 0-0.4 | .11 |
| Maximum benefit (AFDC/TANF + FS) for family of 4 | $1000 | .67-1.94 | 1.04 |
| $ disregarded of first $600 earned in 1st month of welfare receipt | $1000 | 0-.72 | .40 |
| Refundable state EITC | Yes/No | 0-1 | .12 |
| Spending on child care subsidies, per poor child under 13 | $1000 | 0-3.32 | .57 |
| Unemployment rate | Percentage | 2.2-11.5 | 5.14 |

Note.

[a]Means are population estimates weighted for CPS sample design and attrition.

I used existing primary and secondary sources to compile information on the length of the age-of-youngest-child exemption, as well as other relevant state policies. Appendix A details the sources and specific coding rules for each policy variable. Eligibility for the age-of-youngest-exemption is based on the age of the sample member's youngest child and the policy in the state-year in which she was surveyed. I coded exemption policies in months, ranging from 0-48; if the exemption policy was in weeks, I converted the measure to months (e.g. 13 weeks = 3.25 months). In three states, California, Colorado, and New York, age-of-youngest-child exemption

lengths were determined at the county-level. I used the best available information to code these policies at the county-level. Across the study period, 38 percent of the sample was defined as eligible for an exemption.

In order to identify the effect of state variation in age-of-youngest-child welfare-to-work exemptions, I also controlled for a set of state-level policies that changed differentially by state over the study period and may have altered the incentives to work among welfare-eligible mothers. I coded two dimensions of welfare work requirements: whether the state had a "full family sanction" policy, in which a family's cash assistance benefit can be reduced to zero if the adult recipient fails to comply with work requirements for a specified length of time, and the percentage of a state's welfare recipients required by the federal government to be in work activities. In addition, the models included measures of benefit levels and earned income disregards. Benefit levels were measured as the maximum combined AFDC/TANF and Food Stamps benefit for a family of four in each state year.[3] Earned income disregard policies were measured as the amount of the first $600 in earnings disregarded in benefit calculations in the first month of benefit receipt.[4] All monetary values were inflated to 2005 dollars and scaled in $1000 increments.

The models also included measures of two non-welfare policies that target the welfare population: child care subsidies and the state Earned Income Tax Credit programs. A continuous variable measured spending on child-care subsidies per poor child under 13 years of age in each

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[3] Benefit levels from 1990-1998 were taken from a publicly-available dataset compiled by Dr. Robert Moffitt at Johns Hopkins University (Moffitt 2002). Using his variable definition, I completed this information for 2000 and 2002 using the U.S. House of Representatives Ways and Means Committee's Green Book (2004).
[4] $600 is the approximate monthly earnings of a full-time minimum-wage worker.

state-year.[5] A dummy variable indicated whether there was a refundable state EITC in place in each state-year. Finally, as a control for local labor market conditions, I included yearly state and county unemployment rates from the Bureau of Labor Statistics.

*Sources of identification*

I used three primary sources of variation to identify the effect of age-of-youngest-child exemption policies on the work behaviors of single, low-education mothers with young children. State and time fixed-effects models controlled for any unobserved trends that are consistent across states and for time-invariant state characteristics. With this specification, the identifying assumption is that there is no interaction between time and selection into the treatment or control groups. In other words, if other changes to state policies or economic conditions over time were correlated with the policy of interest and the outcome measure, estimates of the effect of the policy of interest would be biased. I addressed this issue in two ways. First, I used a third "difference" between welfare-eligible and welfare-ineligible mothers with the same age children. Second, I included demographic controls and a set of state policies to reduce bias from between-group time-variant differences.

I estimated the effect of age-of-youngest-child exemptions on eligible mothers using logistic regression models. The probability of employment was estimated as a function of exemption eligibility, controlling for child age, time-variant state policies, and individual demographics. The basic model had the following specification:

(1) $Y_{ist} = a_{ist} + B_1 W_{ist} + B_2 E_{ist} + B_3 (W_{ist} * E_{ist}) + A_i B_a + X_i B_x + e_{ist,}$

where $i$ indexes individuals; $s$ states; and $t$ years. $Y_{ist}$ is a dichotomous variable equal to one if individual $i$ was working in the week prior to the survey in state $s$ and time $t$. $W_{ist}$ is an indicator variable equal to one if individual $i$ is both single and has no more than a high school diploma or GED. $E_{ist}$ is also a dichotomous variable indicating whether individual $i$ in state $s$ and year $t$ was eligible for an exemption from welfare work requirements based on the age of her youngest child. $A_i$ is a vector of dummy variables for age of youngest child in one-month age groups from 0-59 months. The vector $X_i$ contains demographic covariates for individual $i$ including age, number of babies ever born, and race. Finally, $e_{itj}$ represents any unobserved heterogeneity in the model.

The parameter of interest in this model is $B_3$, the coefficient on the interaction between eligibility for welfare and eligibility for exemption. The inclusion of the child age dummies (Ai) is essential to this estimation strategy because it ensures that estimated differences between the eligible and ineligible populations are not confounded with the positive relationship between maternal employment and child age. In this specification, while eligibility for exemption is related to child age, the effect of eligibility on employment rates is identified *within* child age groups.

Starting with model (1), I estimated four models with increasing levels of controls. The second model included state fixed-effects and in the third I added a set of dummy variables for year and year interacted with welfare eligibility. In the fourth and final model, I added a set of state policies that changed differentially by state over the study period, as well as unemployment rates. It is this model and conditions that most confidently estimates the causal effect of age-of-youngest-child exemptions, but it depends on the assumption that these state characteristics are

the same, on average, for the welfare-eligible and welfare-ineligible populations.[6] I tested the

sensitivity of the results to this assumption by estimating separate regressions for the welfare-

eligible and welfare-ineligible populations. In addition, I estimated the policy effect by child age

in year-age groups. In all models, I use Huber-White corrected standard errors to account for

non-independence between observations at the state level.

*Results*

*Policy variation*

Table 2.3 shows the length of age-of-youngest-child exemption policies by state and year

and Figure 2.1 summarizes the distribution of states across different length ranges. Until federal

welfare reform in 1996, most states were operating under the rules of the JOBS program, which

did not impose work requirements on recipients of cash assistance with children under 36 months

of age. A handful of states were granted waivers to this rule, the earliest of which were

implemented in 1993, allowing the states to require work of welfare recipients with younger

children. This was one of many efforts to reduce dependency on welfare and increase

employment rates among single mothers, which culminated in the federal welfare reforms of

1997. By 1994, Utah had eliminated the exemption altogether, while Iowa, New Jersey, and

Oregon had reduced the length of the exemption (to three months in Iowa and Oregon, and to

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[6] I used this specification, rather than two separate regressions, because the inclusion of the policy variables in model (1) is primarily aimed at controlling for unobserved and time variant changes that might be correlated with exemption length policies, rather than deriving point estimates for the policy variables themselves. This model also had the advantage of producing standard errors and the associated test statistics for the difference between the effect of the exemption policy on the targeted and non-targeted population. Given this specification, the coefficients Bp? should be interpreted as average effects of welfare policies across both the targeted and non-targeted populations.

twenty-four months in New Jersey). In 1995, on the brink of federal welfare reforms, Michigan also eliminated the exemption, Vermont reduced it to four months, and Indiana to three months.

Under the TANF program, which replaced AFDC/JOBS after federal welfare reforms, the length of the age-of-youngest-child exemption became a state option, greatly increasing the variation in this policy while simultaneously decreasing the average length of exemptions. In 1995, prior to federal welfare reform, most states had an exemption of 36 months. By 1998, exemption lengths ranged from 0 to 48 months, with 90 percent of states offering an exemption of 12 months or less. By 2000, only one state allowed exemptions to last more than 24 months and nearly 50 percent had exemptions of 6 months or less (Rowe & Roberts, 2004; Rowe & Russell, 2004).

Figure 2.2 offers another picture of the policy variation over time by graphing the percentage of single, low-education (welfare eligible) mothers that was eligible for an age-of-youngest-child exemption in each year. The percentage of women with a child under 12 months that was eligible for an exemption decreased from 100 percent to 65 percent between 1990 and 2000. However, it is the population of mothers with children between 12 and 35 months of age that saw the biggest change in policy over time. While 100 percent of these mothers would have been eligible for an exemption under AFDC/JOBS, only nine percent were eligible in 2000 under TANF.

Table 2.3  Lengths of JOBS/TANF Age-of-Youngest-Child Exemptions from Work Requirements by State: 1990-2000

| State | Length of exemption (child's age in months) | | | | | |
|---|---|---|---|---|---|---|
| | 1990 | 1992 | 1994 | 1995 | 1998 | 2000 |
| Alabama | 36 | 36 | 36 | 36 | 12 | 3 |
| Alaska | 36 | 36 | 36 | 36 | 12 | 12 |
| Arizona | 36 | 36 | 36 | 36 | 0 | 0 |
| Arkansas | 36 | 36 | 36 | 36 | 3 | 3 |
| California | 36 | 36 | 36 | 36 | 3[a] | 3[a] |
| Colorado | 36 | 36 | 36 | 36 | 0[a] | 0[a] |
| Connecticut | 36 | 36 | 36 | 36 | 12 | 12 |
| Delaware | 36 | 36 | 36 | 36 | 3.25 | 3.25 |
| D.C. | 36 | 36 | 36 | 36 | 12 | 12 |
| Florida | 36 | 36 | 36 | 36 | 3 | 3 |
| Georgia | 36 | 36 | 36 | 36 | 12 | 12 |
| Hawaii | 36 | 36 | 36 | 36 | 6 | 6 |
| Idaho | 36 | 36 | 36 | 36 | 0 | 0 |
| Illinois | 36 | 36 | 36 | 36 | 12 | 12 |
| Indiana | 36 | 36 | 36 | 3 | 3 | 3 |
| Iowa | 36 | 36 | 3 | 3 | 3 | 3 |
| Kansas | 36 | 36 | 36 | 36 | 12 | 12 |
| Kentucky | 36 | 36 | 36 | 36 | 12 | 12 |
| Louisiana | 36 | 36 | 36 | 36 | 12 | 12 |
| Maine | 36 | 36 | 36 | 36 | 12 | 12 |
| Maryland | 36 | 36 | 36 | 36 | 12 | 12 |
| Massachusetts | 36 | 36 | 36 | 36 | 24 | 24 |
| Michigan | 36 | 36 | 36 | 0 | 3 | 3 |
| Minnesota | 36 | 36 | 36 | 36 | 12 | 12 |
| Mississippi | 36 | 36 | 36 | 36 | 12 | 12 |
| Missouri | 36 | 36 | 36 | 36 | 12 | 12 |
| Montana | 36 | 36 | 36 | 36 | 0 | 0 |
| Nebraska | 36 | 36 | 36 | 36 | 3 | 3 |
| Nevada | 36 | 36 | 36 | 36 | 12 | 12 |
| New Hampshire | 36 | 36 | 36 | 36 | 36 | 24 |
| New Jersey | 36 | 36 | 24 | 24 | 3 | 3 |
| New Mexico | 36 | 36 | 36 | 36 | 12 | 12 |
| New York | 36 | 36 | 36 | 36 | 3[a] | 3[a] |
| North Carolina | 36 | 36 | 36 | 36 | 12 | 12 |
| North Dakota | 36 | 36 | 36 | 36 | 4 | 4 |
| Ohio | 36 | 36 | 36 | 36 | 12 | 12 |
| Oklahoma | 36 | 36 | 36 | 36 | 12 | 3 |
| Oregon | 36 | 36 | 3 | 3 | 3 | 3 |
| Pennsylvania | 36 | 36 | 36 | 36 | 12 | 12 |
| Rhode Island | 36 | 36 | 36 | 36 | 12 | 12 |
| South Carolina | 36 | 36 | 36 | 36 | 12 | 12 |
| South Dakota | 36 | 36 | 36 | 36 | 3 | 3 |
| Tennessee | 36 | 36 | 36 | 36 | 4 | 4 |
| Texas | 36 | 36 | 36 | 36 | 48 | 36 |
| Utah | 36 | 36 | 0 | 0 | 0 | 0 |
| Vermont | 36 | 36 | 36 | 4 | 18 | 18 |
| Virginia | 36 | 36 | 36 | 36 | 18 | 18 |
| Washington | 36 | 36 | 36 | 36 | 12 | 3 |
| West Virginia | 36 | 36 | 36 | 36 | 12 | 12 |
| Wisconsin | 36 | 36 | 36 | 36 | 3 | 3 |
| Wyoming | 36 | 36 | 36 | 36 | 3 | 3 |

Primary source.  Welfare Reform Databooks (Rowe 2000; Rowe, McManus, and Roberts 2004; Rowe and Roberts 2004; Rowe and Russell 2004).
Notes.

The policies shown here represent the best available information about the exemption policies in each state in June of a given year.
[a]Under state TANF rules, the length of exemption for mothers of young children is determined at the county level in California, Colorado, and New York.  It varies from 3-12 months in California and New York, and from 0-12 months in Colorado.

**Figure 2.1  Distribution of States by Length of Age-of-Youngest Child Exemption Policies (n=51): 1990-2000**

**Figure 2.2 Percentage of Welfare-Eligible Population  Eligible for an Age-of-Youngest-Child Exemption, by Year and Age of Youngest Child**
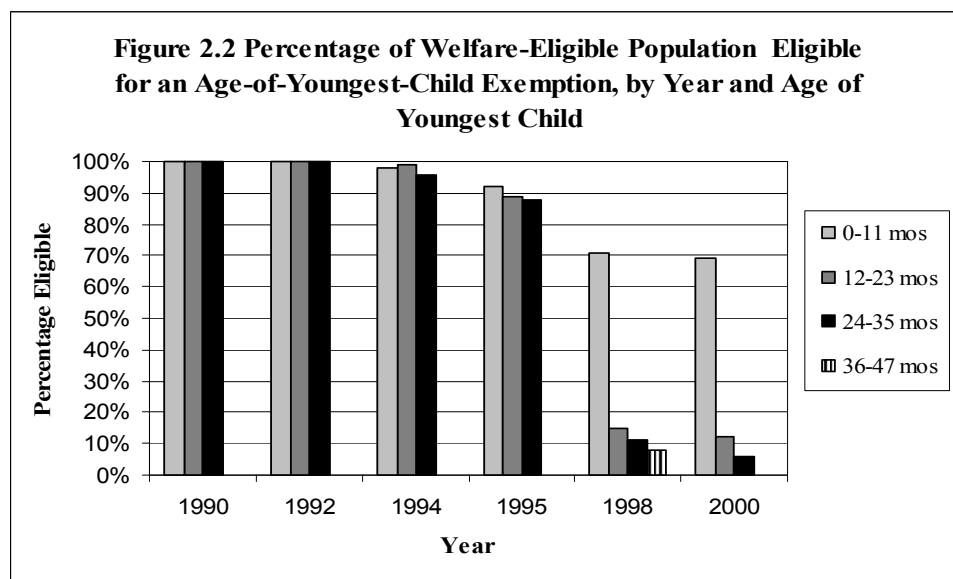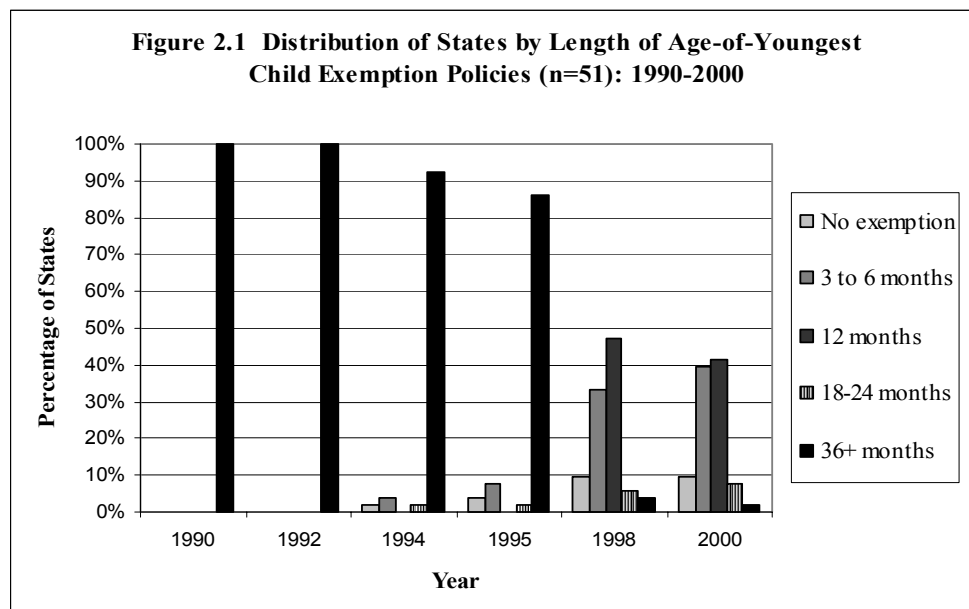
## Labor force participation rates

Trends in employment rates in the 1990s among welfare-eligible mothers with young children mirror those previously documented for single and low-education mothers generally (Figure 2.3). Among married or higher education women with one or more children under five years of age, employment rates remained constant through the decade. However, single women

and women with no more than a high school diploma or GED worked at substantially higher

rates by 2000 than they had in 1990. The increase is particularly steep for single mothers, only

45 percent of whom were working at the beginning of the decade. By 2000, that rate had

increased to 62 percent, surpassing the employment rate of married women with young children.

Employment rates among the welfare-eligible population also varied by eligibility for an

exemption. Figure 2.4 shows that employment rates were lower throughout the study period for

women eligible for an exemption than for those who were not eligible, within three age

categories. The largest difference, 16 percentage points, is between eligible and ineligible

mothers with a youngest child between 12 and 24 months, the group for whom exemption

policies changed the most. This is suggestive of a policy effect, but the differences could be the

result of a variety of state and year differences that are correlated with exemption policies and

employment rates.

**Figure 2.3 Employment Rates among Women with a Youngest Child Under 5 Years of Age by Welfare Eligibility: 1990-2000**

Source: Current Population Survey, June Fertility Supplement

Note. Estimated employment rates are weighted for sample selection and non-response.

**Figure 2.4 Employment Rates Among Welfare-Eligible Women by Eligibility for Age-of-Youngest Child Exemption and Age of Youngest Child**



Source: Current Population Survey, June Fertility Supplement

Note. Estimated employment rates are weighted for sample selection and non-response.

*Regression results*

In order to control for those differences, I estimated four logistic regression models predicting employment rates, each with an increasing number of control variables. Marginal effects and standard errors for the key independent variables are shown in Table 2.4. Appendix Table B.1 presents coefficients and standards errors for all of the variables in the final model. The models present a consistent picture of a large and significant effect of exemption policy length on the probability of working among the welfare target population. Recall that a policy effect of welfare-to-work exemptions on the welfare-eligible population is captured by the coefficient on the interaction between eligibility for welfare and eligibility for an exemption. This coefficient is negative and robust to all model specifications, providing evidence that eligibility for an exemption decreases the probability of employment for women in the welfare-eligible population, controlling for child age. Phrased differently, shorter exemption lengths increase employment among mothers with infants and toddlers.

Table 2.4 Marginal Effects of Welfare Eligibility and Age-of-Youngest-Child Exemption Eligibility on Employment Rates among Mothers of Young Children

| Independent Variables of Interest | Models | | | |
|---|---|---|---|---|
| | 1 | 2 | 3 | 4 |
| Eligible for exemption | -.050** | -.040** | -.010 | -.004 |
| | (.012) | (.009) | (.011) | (.010) |
| Eligible for welfare | -.038** | -.033* | -.077** | -.072** |
| | (.014) | (.013) | (.016) | (.016) |
| **Eligible for exemption X  Eligible for welfare** | **-.076**** | **-.078**** | **-.036*** | **-.039**** |
| | **(.015)** | **(.015)** | **(.014)** | **(.014)** |
| Demographic controls[a] | X | X | X | X |
| State-county fixed effects | | X | X | X |
| Year controls[b] | | | X | X |
| State policies and unemployment rates[c] | | | | X |
| Observations | 42,091 | 42,089 | 42,089 | 41,731 |
| Clusters | – | 97 | 97 | 96 |

Notes.

Sample includes all Female members of the CPS June Supplement who were 18-44 years old and had a youngest child under 60 months.
[a]Demographic control variables include mother's age, number of births, and race, as well as the age of youngest child represented by one-month dichotomous variables from 0-59 months.
[b]Year controls include dummies for each year and interactions between each year and welfare eligibility.
[c]State policies include whether the state has a full family sanction; the federally-required work rate for the state; the maximum AFDC/TANF and Food Stamps benefit for a family of four; the dollar amount of the first $600 in earnings in the 1st month of welfare receipt that is disregarded from the benefit calculation; whether the state has a refundable EITC; and spending on child care subsidies per poor child under 13 years of age.  When these controls are included, the Washington DC observations are dropped because of missing child care data.
+$p<.10$; *$p<.05$; **$p<.01$

The size and significance of the coefficient on "eligible for exemption" is also consistent

with a policy effect on the targeted population. This coefficient captures the effect of being

"eligible" for an exemption by virtue of child age for women who are not in the welfare-eligible

population. While this coefficient is significant in the first model, it decreases in size and is

insignificant once the model is estimated with year-fixed-effects.  The lack of significant effects

on welfare-ineligible mothers is what we would expect given that work decisions among these

mothers should not be affected by welfare policies. The fact that this effect disappears once

controls are included for years and interactions between year and welfare eligibility suggests that

the difference in the first regression model is not related to exemption policies, but rather to economic conditions that changed over time and covaried with exemption policies.

The magnitude of the estimated policy effect of exemptions decreases across the four models. In the first two models, the marginal effect of the interaction is approximately eight percentage points, but the magnitude of the coefficient is reduced by 50 percent once the model controls for the time trend in employment and specifically for differential time trends by welfare eligibility. In model 4, which includes unemployment rates and time variant state policies, the estimated marginal effect of the policy is approximately four percentage points ($p<.01$). It is interesting that the size of the coefficients on *eligible for welfare* and the interaction term reverse almost perfectly when the year controls are added. One explanation for this is that the year dummies are controlling for the changes over time toward not only shorter exemption lengths but stricter work requirements and other policies that might encourage employment.

In the main models, welfare policy effects are presumed to be the same, on average, for both welfare eligible and ineligible mothers. To test the sensitivity of these results to this assumption, I estimated the effects of age-of-youngest-child exemptions separately for welfare-eligible and welfare-ineligible populations (results not shown here). Given the smaller sample sizes used in these models, I restricted the controls to demographic characteristics and year controls. This test confirmed the results of the main models in this analysis. For the welfare-eligible population, eligibility for an exemption predicted employment rates that were four percentage points lower ($p<.10$). The coefficient for the welfare-ineligible population was half the size and insignificant.

*Discussion*

The question posed by this study was how much the length of age-of-youngest-child exemptions from welfare work requirements affects maternal work decisions among welfare recipients with young children. I estimated a substantial negative effect, four percentage points, of eligibility for an exemption on the probability of maternal employment. The magnitude of this effect is comparable to previously estimated effects of $1000 increase in annual welfare benefits (Meyer & Rosenbaum, 2000) or a 10 percent decline in child care subsidies (Gornick & Meyers, 2003). The trend toward shorter exemption lengths is effectively pulling some single mothers into the labor market sooner than they would have gone otherwise.

While many studies have examined the labor market effects of welfare policies, few have focused on parents of young children, and none have estimated an effect of this specific aspect of welfare policy. In addition, many studies of welfare reform either compare the time period before and after the year of state TANF implementation or between components of state policy that vary little across states or over time. As Blank (2007) noted, both are relatively weak identification strategies. This study improves on those approaches by identifying a policy effect using substantial variation over time and across states in the length of age-of-youngest-child exemption policies, as well as a comparison between single and low-education mothers with young children and married or higher-education mothers with children of the same ages.

This study adds to our understanding of how welfare policy affects families with young children, but it leaves unanswered the question of whether shortening age-of-youngest-child exemptions is "good" policy. Welfare-to-work exemptions can be viewed as a form of paid family leave, a benefit that few workers, and particularly few low-wage workers, have access to

in this country. Framed this way, the exemptions give single mothers the option of caring for very young children full-time, which many would argue is a laudable goal for public policy. Framed differently, however, this study provides evidence that the exemptions provide a disincentive to work among welfare-eligible mothers, making the policy clearly at odds with the current paradigm for U.S. social policy.

In addition, the evidence on how families and children might be affected by this policy is mixed. For low-income children, the effects of early maternal employment appear to be neutral, even in the first year of life, or positive if employment is associated with gains in income (Dearing et al. 2006; Kalil et al. 2001; Morris, Duncan & Clark-Kauffman 2005; Vandell & Ramanan 1992). Yet, most welfare-eligible women are employed in the low-skilled labor market—characterized by low pay, limited benefits, and few opportunities for advancement. This makes it unlikely that a move from welfare to work will increase family income without the help of generous earnings disregards or supplements. Given the choice between low-wage work and non-parental child care of unknown quality, low-income infants may be better served by having a stay-at-home mother. However, the longer-term effects could be negative if the time a mother spends away from the labor market ultimately decreases her employability and earning potential. Later in this dissertation, Chapter Four takes up directly the question of how young children of single mothers are affected by both the level and stability of maternal employment.

CHAPTER THREE

UNMARRIED MOTHERS' WORK AND WELFARE DECISIONS FOLLOWING THE

BIRTH OF A CHILD: MIXED METHOD CASE STUDIES OF NEW PARENTS

In the spring of 2000, Lila and Marisol were both single mothers living in Midwestern cities. They had each recently had a child and were cohabiting with the respective fathers of those children. One had a high school diploma, while the other was studying for the GED when she became pregnant. Both had worked for pay early in their pregnancies, but also had a history of receiving government assistance. Over the course of the next year, the decisions each woman made about caring for an infant, working outside the home, and receiving welfare were shaped by a confluence of factors including their attitudes about work and welfare, employability, and family circumstances, most critically the role of their cohabiting partner in supporting the family financially.

Lila, an African-American woman living in Chicago, was 27 when she had her third child, a daughter. She had stopped working at a market research firm six months before the birth and was relying on her partner, Robert, a part-time bus driver, to support her. Lila had received cash assistance when she was pregnant with her first child, but that was over four years ago. She still thought the welfare program served an important role: "…if you need it, then fine…," she said, "I just had a baby, so I'm not working…If he (Robert) was not around [and] I did not have help or anything…I would need it, right?" At the time of the birth, however, the burden of applying and meeting the administrative and participation requirements outweighed whatever financial benefit she might reap from returning to welfare. Robert reported that it was difficult

paying the bills on just his salary from part-time work, but that their situation improved when his position became full time shortly after the birth of their child.

Having worked consistently throughout her life, Lila was confident about her ability to find work without the help of job training programs. She planned to start looking for employment when the baby was around six months old. Robert was more ambivalent about Lila working outside the home because he believed that men were responsible for providing financially for the family. In addition, both parents expressed concern about finding a child care provider. "You can't trust [just] anybody with your baby," Robert said. Lila's stated preference was to wait until the baby could talk before putting her in group day care. Lila did return to work full time about six months after the birth and, while still ambivalent, Robert supported her because he thought the additional income would better their circumstances.

Like Lila, Marisol, a Hispanic woman living in Milwaukee, became employed in the first year of her child's life. The context for that decision could not have been more different, however. In the time surrounding the birth, Marisol's life was ruled by chaos. She made interstate moves several times, relied on a combination of family support and welfare to provide for her two children, and ultimately experienced the dissolution of her relationship with Damon, the father of her two children. She agreed with Lila that welfare should be used by mothers who are raising children without consistent help from a partner, a category she put herself in even when she was living with the father of her new baby.

Marisol's mother encouraged her to apply for Food Stamps and welfare when she became pregnant with her second child. During the pregnancy, she fulfilled welfare program requirements by taking GED classes. Damon was doing factory work, but he had a drug habit

that made his presence and financial contribution unpredictable. Consequently, Marisol was responsible for paying their bills with the cash assistance check she received each month. The state TANF policies allowed Marisol to receive cash assistance without working or attending school for three months following the birth of her son. She was well aware of this rule and was making plans to return to school when the period ended. By that time, however, she had left the state, found a minimum wage at a fast food restaurant, and was trying to determine what classes she could take in her new city. When Marisol's youngest child turned one, her relationship with Damon had ended, she'd become unemployed again, and was still depending on cash assistance.[7]

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In Chapter Two, I showed that a specific welfare policy, age-of-youngest-child exemptions, affected labor force participation rates among single mothers with young children. The strength of that analysis rested in using a natural experiment to identify the average effect of a single factor on maternal work decisions, net of all others. Yet, the examples of Lila and Marisol remind us that families' lives are complex and that maternal decisions about work and welfare are not made in isolation. While the employment "outcome" was quite similar for the two women—namely, becoming employed late in the first year after a birth—the paths they took to arrive at that outcome and the roles that individual factors, such as welfare, played in shaping those paths varied substantially. This view of maternal work decisions is consistent with ecological and family systems theories from the field of psychology, which posit that individual behavior is best understood in the context of family life, which is, in turn, best understood in the

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[7] The vignettes at the beginning of this chapter were written using data from two waves of the Fragile Families and Child Well-Being survey and two waves of in-depth interviews with the survey respondents conducted as part of the Time, Love, and Cash in Couples with Children study.

context of larger social and cultural contexts (Uri Bronfenbrenner, 1979; Urie Bronfenbrenner, 1986; Mistry, Biesanz, Taylor, Burchinal, & Cox, 2004).

It is difficult to capture this diversity of experiences in strictly quantitative or qualitative studies. Qualitative methods elucidate the details of individual cases and can generate theory based on comparisons of a small number of cases, but the results are rarely generalizable. In contrast, quantitative methods are generally used to compare thinner information on a large number of cases and, under the right circumstances, produce results that can be applied to a larger population and applied to policy decisions. However, the process of identifying average tendencies often obscures the diversity and complexity of individual cases.

Despite their origins in vastly different paradigms, quantitative and qualitative methods are increasingly viewed as complimentary precisely because of their opposing strengths and weaknesses. Program evaluation techniques have long paired outcome studies, which use survey or administrative data to estimate the quantitative effects of a program, with implementation studies based on qualitative interviews with staff and participants. Recently, several prominent studies of single-parents have been designed with ethnographic or observational components embedded in a large survey project.

The analysis presented in this chapter is based on data from one such effort, the Fragile Families and Child Well-Being survey (henceforth referred to as the Fragile Families study), which included an embedded qualitative study called Time, Love, and Cash among Couples with Children (TLC3).[8]   I combine data from both sources on the same families, as well several

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[8] For another example of a study designed with survey and ethnographic components, see Welfare, Children, & Families: A Three City Study at: http://web.jhu.edu/threecitystudy/index.html.

methodological approaches, in hopes of capturing both the complexity of individual cases and the patterns across cases that explain maternal employment in the first year of a child's life. In the central part of the analysis, I use survey and in-depth interview data on a small sample to conduct Qualitative Comparative Analysis (QCA), a case study method designed to relate a small set of theoretically-supported conditions to an outcome. My QCA analysis explores the interactions between measures of mother's human capital and family circumstances in determining whether a mother returns to work in the first year after a birth. I use narrative analysis of qualitative data from the transcripts of in-depth interviews with the same families to provide context and explanation for the patterns identified in the QCA. Finally, I explore methods for testing QCA-driven hypotheses using more conventional statistical methods and a larger sample from the Fragile Families study.

*Background*

In keeping with overall increases in female labor force participation, women are now more likely to work for pay during a pregnancy and to return more quickly to employment following a birth than ever before. The average American woman works full-time into the last month of her first pregnancy and takes some form of maternity leave (paid or unpaid) for no more than 4 months (J. O. Johnson & Downs, 2005). These overall trends mask important differences between married and unmarried women's work behaviors following the birth of a child, however. For instance, never married mothers are about 20 percentage points less likely to work during pregnancy than other mothers and if they do they are much less likely to take paid leave following the birth (J. O. Johnson & Downs, 2005). This is consistent with the fact that single mothers are about two-thirds less likely to have access to employer-provided paid vacation

or paid sick leave than married mothers (Friedman, 2001; Shonkoff & Phillips, 2000; Thompson, 2001). The considerable instability in the low-wage labor market may also prevent single mothers from taking unpaid leave with job protection through the FMLA, because its coverage is limited to employees who have worked for an eligible employer for 12 months or more.

When labor force participation rates increased for single mothers in the late 1990s, so did the timing of returns to work. Figure 3.1 compares trends in the timing of returns to work after a first birth among never married mothers between 1991 and 1999, Figure 3.2 charts returns of work during the same period for never married mothers who worked at some point during the pregnancy. In both cases, the proportion of women who did not work for pay in the 12 months after their first birth declined by approximately 12 percentage points during the decade. The largest increases in work participation were among women who became employed sometime between the 3rd and 11th month following a birth. These shifts are in contrast to trends for ever-married mothers, for whom the distribution of timing of returns to work after a first birth changed very little in the late 1990s (J. O. Johnson & Downs, 2005).

Why has the average timing of employment following the birth of a child shortened for never-married women? The previous chapter provided evidence that changes in welfare policies altered the incentives for single mothers to return to work sooner after a birth. Family leave policies have also been shown to affect maternal employment decisions in the year after a birth (Berger & Waldfogel, 2004; Glass & Riley, 1998; Hofferth, 1996; Joesch, 1997), but the extent to which these policies cover single mothers and affect their work decisions is unknown.

**Figure 3.1 Timing of Maternal Employment after First Birth for Never Married Mothers: Comparison of Middle to Late 1990s**
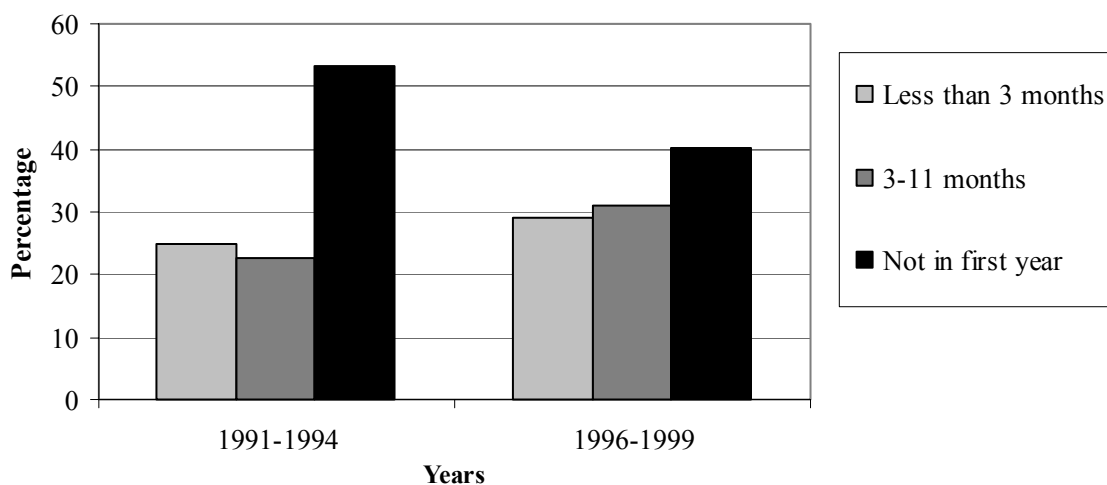


Source: Survey of Income and Program Participation (SIPP); Johnson & Downs 2005; Smith, Downs, O'Connell 2001.

**Figure 3.2 Timing of Returns to Work after First Birth for Never Married Mothers *who Worked During Pregnancy*: Comparison of Middle to Late 1990s**
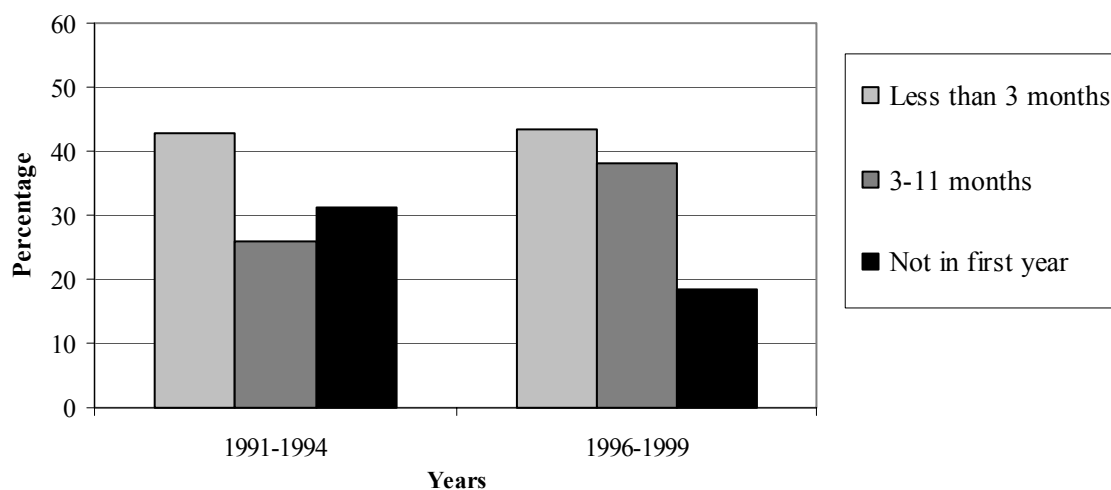


Source: Survey of Income and Program Participations (SIPP); Johnson & Downs 2005; Smith, Downs, O'Connell 2001.

In contrast, the positive labor market effects of child care subsidies are known to be largest among low-skilled and single women (Anderson & Levine, 1999; Baum, 2002; Wenjui Han & Waldfogel, 2001; Michalopoulos, Robins, & Garfinkel, 1992), but few studies have estimated their specific effects on the timing of maternal employment among new mothers. In one case, eligibility for a subsidy was found to increase the probability of employment in the first three months after a first birth, but not in the first 24 months (Leibowitz, Klerman, & Waite, 1992).

The present study focuses on the family circumstances related to when and if a mother seeks employment after having a child. Theory suggests that the decision to work is a calculation between the value of market and non-market time (Becker, 1991; Blau, Ferber, & Winkler, 2005; Leibowitz, Klerman, & Waite, 1992). There is evidence for two primary types of factors that alter that calculation: mother's human capital or labor force attachment, and aspects of family structure, such as marital status, number of children, and partner's income. Women with the greatest investments in human capital and higher wages are more likely to remain employed or leave the workplace only briefly around the birth of a child, net of spousal earnings, family structure, and other maternal characteristics (England, Garcia, & Richardson, 2004; Leibowitz, Klerman, & Waite, 1992; Tienda & Glass, 1985; Wenk & Garrett, 1992). The largest countervailing force is family income from sources other than maternal employment, including a partner's earnings, welfare assistance, or family support, all of which increase the value of nonmarket time(Leibowitz & Klerman, 1995).

Theoretically, marriage and childbearing are both thought to reinforce the benefits of gender specialization in the home and increase the costs to the family of having the mother work outside the home (Becker, 1991). Married women and mothers do have lower employment rates

than other women, but the direction and causality of these relationships has been difficult to determine. When estimation models control for family income, marriage no longer has a statistically significant effect on the probability of a mother being employment in the first year (Leibowitz, Klerman, & Waite, 1992). There is also evidence that the effects of childbearing on maternal employment are confounded by women's tastes and preferences. For instance, it is plausible that employed mothers have a preference for fewer children (Desai & Waite, 1991; Nakumura & Nakumura, 1994). Overtime, the negative effects of marriage and children on some women's labor force participation have decreased or disappeared, which is likely the result of both demographic and cultural changes as well as more sophisticated estimation models.

Most studies of maternal work decisions following a birth use economic modeling to predict the contribution of human capital and family structure factors and, implicitly or explicitly, to test the relative importance of these factors in explaining trends in labor force participation (Berger & Waldfogel, 2004; Klerman & Leibowitz, 1990, , 1999; Leibowitz & Klerman, 1995). Regression methods are well-suited to the task of identifying point estimates of the additive contributions of individual factors. However, there is a drawback to using this approach exclusively in order to understand maternal work decisions. Evidence from both quantitative and qualitative studies suggest that women's employment decisions are influenced by a large set of interrelated factors (Leach et al., 2006; Volling & Belsky, 1993; Zaslow, Rabinovich, & Suwalsky, 1991). Theoretically, this complexity is described well by what developmental psychologists call the "ecology of development." This perspective views individual behavior and child development as embedded in multiple, interacting contexts—from culture to family, schools, and peers—the importance of which may vary over a lifetime (Urie

Bronfenbrenner, 1986; McCall, 1981; Mistry, Biesanz, Taylor, Burchinal, & Cox, 2004).

Methodological approaches for understanding individuals in the context of multiple interacting

factors include cluster analysis and Q-Sort (Mandara, 2003) in psychology and Qualitative

Comparative Analysis in sociology (Ragin, 1987).

My approach uses QCA and analysis of in-depth qualitative data to generate a set of

hypotheses about maternal work decision after a birth and With a sample of 30 cases for which I

had both survey and qualitative data, I use QCA to generate hypotheses about multiple pathways

that lead to mothers working within the first year of their children's lives. Combining the QCA

results with detailed qualitative data about family circumstances around the time of the focal

child's birth, I then develop hypotheses about sets of conditions that lead to maternal

employment in the first year that would be testable in conventional regression models. Next, I

selected half of the Fragile Families sample randomly and ran a series of exploratory regressions

to test the hypotheses. Finally, using the other half of the Fragile Families sample, I ran a final

and "best" logistic regression model predicting maternal employment.

*Data*

The families described in this chapter are part of the Fragile Families and Child Well-

Being Study, as well as an embedded qualitative study, Time, Love and Cash among Couples

with Children (TLC3). The Fragile Families Study is a longitudinal survey of 3600 unmarried

and 1200 married new parents (couples) in 20 cities[9]; TLC3 is a longitudinal qualitative study of

75 Fragile Families' sample members. Fragile Families surveys were completed at the hospital at

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[9] When weighted, the Fragile Families data is representative of all new parents in cities with populations of 200,000 or more. For more information on the study, see: http://crcw.princeton.edu/fragilefamilies/index.asp.

the time of the birth and over the phone when the child was approximately one, three, and five

years old. Parents in the TLC3 study were interviewed in their homes together and separately,

shortly after the birth of the focal child and again when the child was approximately one, two,

and four years old. In this analysis, I use the first two waves of Fragile Families survey and the

TLC3 interviews, collected soon after birth and around the child's first birthday.

The Fragile Families and the TLC3 studies were designed to investigate relationship

dynamics, the factors that affect relationship stability and dissolution, and the implications of

relationship quality and outcomes for child well-being, particularly among unmarried parents. In

the course of both studies, information was also collected on both parents' employment status

and other sources of household income. The Fragile Families surveys asked sample members a

battery of close-ended questions on these topics. The TLC3 interviewers used an interview guide

with suggested topics and questions, but the two- to three-hour interviews were conversational

and guided as much by the respondent as the interviewer.

I used two samples for this study: a small case study sample of 30 families in both the

Fragile Families and TLC studies and a larger survey sample of 2,745 Fragile Families sample

members. In order to approximate a welfare-eligible population, I narrowed both samples to

unmarried couples in which the mother had no more than a high school diploma or GED and

some history of receiving help from means-tested government programs.[10] The case study

sample was selected from among TLC3 participants in order to have a sufficiently small sample

with both survey and interview data for the Qualitative Comparative Analysis. In this sample, I

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[10] AFDC/TANF, Food Stamps, WIC, or Medicaid.

excluded two cases because they did not complete the Fragile Families One-Year Follow-Up survey. The 30 cases in the first sample were excluded from the larger Fragile Families sample.

The demographic characteristics of both samples are presented in Table 3.1. The 30-case sample is generally representative of the larger sample. In both samples, the average ages of mothers and fathers in the study were 23 and 25, respectively. Only ten percent of the mothers in each sample had never worked two consecutive weeks by the time of the birth. Half had received TANF or Food Stamps in the previous 12 months. The samples were limited to mothers who had no more than a high school diploma or GED (to approximate a sample of welfare-eligible women), but approximately one-fifth of the fathers had some college education. Seventy to eighty percent of the women in this study worked at some point during the pregnancy.[11] In both samples, the vast majority of mothers said they expected to work in the year after the birth. [12]

Where there are differences between the two samples, they do not suggest that one is more advantaged than the other. The TLC3 sample had a higher proportion of high school graduates (or GED holders), 53 percent compared to 45 percent. Similarly, employment rates among the fathers in the week prior to the birth were 10 percentage points higher in the TLC3 sample than the Fragile Families sample. However, the TLC3 sample also had a higher mean number of children in the household and a larger proportion of racial minorities.

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[11] These rates are substantially higher than national estimates for never married mothers (J. O. Johnson & Downs, 2005).

[12] The QCA sample is divided roughly in thirds between the three cities of the TLC3 study; I do not have the geographic identifiers for the Fragile Families respondents who were not part of the TLC3 study.

*Methods*

I describe and analyze the complexity of maternal work decisions following the birth of a child using several methods. Using two waves of Fragile Families data and one wave of TLC3 interviews, I first reconstruct family circumstances in the first year after the birth of a child. This analysis combines both data sources to create three data points stretching from 3 months prior to the birth until up to 18 months following the birth, depending on the timing of the surveys and interviews. I describe patterns of maternal employment status and welfare receipt in the context of relationships status and paternal employment by assigning cases to categories of employment and welfare receipt commonly used in the literature. One goal of the descriptive analysis is to examine the extent to which knowing the cohabiting father's employment status alters the categorization of the mothers' circumstances.

For the central analysis of this study, I both combine survey and interview data and use multiple methods to examine human capital and family structure as determinants of the timing of maternal employment in the year after a birth. This analysis proceeded in four steps designed to systematically generate and test hypotheses. First, with the sample of 30 cases for which I had both survey and qualitative data, I used Qualitative Comparative Analysis to generate hypotheses about multiple pathways that lead to mothers working within the first year of their children's lives. Second, using the results from the QCA, as well as detailed qualitative data about family circumstances around the time of the focal child's birth, I developed hypotheses about sets of conditions that lead to maternal employment in the first year that would be testable in conventional regression models. Next, I selected half of the Fragile Families sample randomly

Table 3.1 Sample Characteristics at Time of Focal Child's Birth

| | QCA Sample (n=30) | | Regression Sample (n=2,715) | |
|---|---|---|---|---|
| | Number | Proportion/Mean | Number | Proportion/Mean |
| **Mother's characteristics** | | | | |
| Race | | | | |
|     Black | 16 | .53 | 1,512 | .57 |
|     White | 3 | .10 | 625 | .24 |
|     Other | 10 | .33 | 578 | .19 |
| Age | – | 22.93 | – | 23.32 |
| | – | (4.64) | – | (5.42) |
| Months since last worked 2 consecutive weeks | | | | |
|     3 or less | 11 | .37 | 1,138 | .44 |
|     4 to 9 | 13 | .43 | 661 | .26 |
|     10 or more | 3 | .10 | 516 | .20 |
|     Never | 3 | .10 | 264 | .10 |
| Expects to work in next year | 26 | .87 | 2,310 | .90 |
| Received TANF or Food Stamps in year before birth | 16 | .53 | 1,274 | .47 |
| Educational attainment | | | | |
|     Less than HS Diploma/GED | 14 | .47 | 1,488 | .55 |
|     HS Diploma/GED | 16 | .53 | 1,227 | .45 |
| **Father's characteristics** | | | | |
| Race | | | | |
|     Black | 14 | .47 | 1,148 | .58 |
|     White | 4 | .13 | 390 | .20 |
|     Other[a] | 12 | .40 | 434 | .22 |
| Hispanic | 14 | .47 | 627 | .31 |
| Age | – | 24.83 | – | 25.94 |
| | – | (4.51) | – | (6.90) |
| Worked in week prior to birth | 25 | .83 | 1,471 | .73 |
| Received SSI or UI in year before birth | 4 | .13 | 164 | .08 |
| Educational attainment | | | | |
|     Less than HS Diploma/GED | 15 | .50 | 925 | .46 |
|     HS Diploma/GED | 8 | .27 | 740 | .37 |
|     Some college | 7 | .23 | 256 | .13 |
|     College degree | 0 | 0 | 92 | .05 |
| **Household characteristics** | | | | |
| City of residence | | | | |
|     Chicago | 11 | .37 | – | – |
|     Milwaukee | 10 | .33 | – | – |
|     New York | 9 | .30 | – | – |
| Number of children (age<18) in household | – | 1.63 | – | 1.36 |
| | – | (1.45) | – | (1.32) |
| Number of preschool children (age<5) in household | – | .73 | – | .49 |
| | – | (.83) | – | (.70) |

Source: Fragile Families and Child Well-Being Study, Baseline and One-Year surveys.

Notes.

[b]Measured at the time of the Fragile Families One-Year Follow-Up Survey, which occurred no more than 18 months after the birth

and ran a series of exploratory regressions to test the hypotheses. Finally, using the other half of

the Fragile Families sample, I ran a final and "best" logistic regression model predicting

maternal employment. I describe the QCA and regression methods in more detail below.

Appendix Table B.1 provides details about the variables I used from each data sources, as well as

the timing of the interviews relative to the focal child's birth.

*QCA*

Qualitative Comparative Analysis is a technique developed by Charles Ragin (1987) for

cross-case comparisons and causal inference that combines attributes of conventional qualitative

and quantitative methods. Unlike regression techniques, QCA does not estimate the relationship

between any single variable and an outcome. Instead, the method assumes "maximum causal

complexity," by first identifying all logical combinations of the independent variables of interest

and then minimizing those combinations using the laws of Boolean algebra (Ragin, 1987, ,

2000). QCA is quite deliberately case- rather than variable-oriented, but unlike conventional case

study approaches, it also tries to produce generalizable results using cross-case comparisons.

QCA can answer both descriptive and explanatory questions, but it does so using a central

paradigm of qualitative research: that the diversity of experience is as informative as the average

tendencies or patterns across cases. It also seems particularly well suited to generating testable

hypotheses about phenomenon not easily reduced to simple models.

The logic of QCA is that social science phenomena are generally caused by a

combination of factors and, furthermore, that different combinations may produce the same

outcome.[13] While the method has been used primarily in the field of political sociology (Amenta & Halfmann, 2000; Brown & Boswell, 1995; Cress & Snow, 2000; Roscigno & Hodson, 2004), its conception of causality is consistent with a human development perspective. In both cases, the context of individual experiences or decisions are seen as fundamental in determining the consequent repercussions or effects (Urie Bronfenbrenner, 1986; Ragin, 2000, , 2006). While experimental and econometric methods are generally thought of as the keystones of policy-relevant research, Ragin argues that the configurational analysis like QCA, which can speak to categories of individual based on configurations of conditions, has some advantages in the context of policy discussions.  He points out that policy discussions rarely concern individual variables or levers that can change one aspect of family life, but instead focus on categories of people and the role of individual factors in different contexts (Ragin, 2006).

The theory and mechanics of QCA call for a limited set of hypothesized causal conditions, ideally selected with theoretical and/or empirical support (Rihoux, 2006). With each new condition, the complexity of the model and the interpretation of results are made exponentially larger. In addition, the goal of a QCA model is not to control for all factors that might affect the outcome, but rather to identify the interactions between a set of causally-related conditions selected based on theory and existing empirical evidence.

In the present study, I related four conditions (described in Table 3.2) to the timing of maternal employment following a birth. The conditions capture two factors that have been

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[13] This approach shares some logic and goals with typological techniques, including cluster analysis and Q-factor sort. In those methods, however, cases that are missing a single attribute of a set of attributes defining a type are still included in the type. In contrast, QCA considers cases qualitatively different if they differ on even one condition. In other words, the absence of a condition has the same logical value as the presence of a condition (Ragin, 2000).

theoretically and empirically linked to maternal employment: maternal human capital and family

circumstances. Each mother's human capital (or employability) was measured as whether the

mother was employed for two weeks or more during the three months prior to the birth.

Measures of the family context for a mother's decision to work included whether the father of

the child was employed in the week prior to the birth; whether there were children under the age

of five, in addition to the focal child, in the household; and whether the mother had received

TANF in the year prior to the birth.  In QCA, all conditions (independent variables) and

outcomes (dependent variables) are represented by binary variables, with a 1 indicating the

presence of a condition and a 0 indicating the absence of that condition.[14]

The outcome of interest is whether a mother worked in the first year (0-11 months) of the

focal child's life.  In the QCA analyses, I examine patterns of conditions associated with both the

presence and absence of this outcome.  This measure was derived mainly from a question in the

Fragile Families One-Year survey: How old was your child (in months or years) when you

returned to work? I checked the responses to this question against data collected in multiple

waves of the TLC3 study. In cases where the responses differed, I favored the information

provided in the more detailed TLC3 data.

The fs/QCA software[15] (Ragin, 2006) produces a list of all logical combinations of the

conditions and outcomes, called "configurations." Each row in the resulting "truth table" is a

unique configuration of the conditions, with the number of rows equal to $2^K$, in which K is the

number of conditions. Next, the laws of Boolean algebra are used to simplify the positive

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[14] Ragin has also developed a method called fuzzy set analysis that addresses partial membership in a group (Ragin, 2006).
[15] Available at: http://www.u.arizona.edu/~cragin/fsQCA/

configurations with the goal of identifying the shortest possible expression that covers all of the original configurations associated with the outcome. This last step is akin in purpose to conventional statistical methods in the sense that it seeks a parsimonious solution that captures cross-case patterns in the data.

Table 3.2 Measures used in Qualitative Comparative Analysis

| Variable | Definition |
| --- | --- |
| | |
| Outcomes | |
| work | Mother employed in first year after birth |
| nowork | Mother not employed in first year after birth |
| | |
| Conditions | |
| *Mother's human capital* | |
| | Mother worked 2 weeks or more in three months prior to |
| pregwork | birth |
| | |
| *Family circumstances* | |
| faemp | Father employed in week prior to birth |
| tanf | Mother received TANF in 12 months prior to birth |
| youngkids | Other children under 5 years of age in household |

Source: Fragile Families and Child Well-Being Study, Baseline and One-Year surveys.

An important aspect of the analysis process is that only cases that belong to a configuration associated with a positive outcome 100 percent of the time are included in the analysis. QCA excludes configurations associated with both positive and negative outcomes (contradictory cases) or configurations not actually observed in the data (hypothetical/remainder cases), assuming that in both cases the configurations would not be consistently associated with a positive outcome and therefore offer no additional information to the analysis. QCA has been criticized for its exclusion of contradictory cases because the technique does not make full use of the data. Hypothetical cases are analogous in some ways to missing data in statistical analyses. However, in the context of estimating average effects, missing data can be ignored if it is

believed to be missing at random. In the case of QCA, either missing cases are capturing the implausibility of certain configurations in the real world, or they are limiting the exploration of diversity.

Complementing the theory-building insights that might emerge from QCA results, I used qualitative data from multiple waves of TLC3 interviews to provide detailed personal narratives about sample members' employment and welfare decisions following the birth of the focal child. While QCA provides insight into patterns of interactions among causally-related conditions, more traditional narrative or case study analysis offers an opportunity to see how those interactions manifest themselves in families' lives. I use pseudonyms whenever I refer to the details of a case or quote a sample member.

*Regression analysis*

I tested hypotheses generated from the case comparisons using logistic regression models in which maternal employment, measured as having worked in the first 11 months of the focal child's life, was the dependent variable. Using a randomly-selected half of the sample, I estimated three types of exploratory models:

1. Additive models that included main effects for each of the four conditions. I estimated three of these models, one with only the four conditions, and two others with sets of control variables related to the QCA findings.

2. Models with main effects and specific interactions that the QCA results indicated were predictive.

Based on the results of these analyses, I developed a "best" model testing the hypotheses generated in the comparative case analysis and predicting maternal employment in the first year.

I tested this model on the remaining half of the sample with and without additional control variables.

The control variables used in the regression analyses include four measures of mother characteristics: mother's race represented by three dummy variable for race is white, Black, and other (white is the omitted category); mother being ethnically Hispanic; mother having a high school diploma or GED (as opposed to less education); and mother having traditional views of gender roles in the home. The final measure is created from a single agree/disagree question on the Fragile Families Baseline survey: Is it better if the husband earns the main living and the woman cares for the family? I also use a set of family-level controls, including number of adults in the household, whether the couple is cohabiting, and whether the mother says that someone will be able to help her with child care in the coming year.

*Results*

*Descriptive*

Table 3.3 provides additional information about family circumstances following the birth of the focal child. Depending on the couple, the data span the 9 to 18 months following the birth. On average, the first data point was collected immediately after the birth, the second when the child was 3 months, and the third when the child was 12 months old. The measures are not identical across the three time points. Time 1 data was collected in the baseline Fragile Families survey, conducted immediately after the child was born, but the questions about employment and TANF receipt ask about the past year (rather than current). In the TLC3 interview (Time 2) and the one-year follow-up of the Fragile Families survey (Time 3) questions are asked about current

Table 3.3 Family Circumstances Following the Birth of a Child (n=30)

| | Time 1 | | Time 2 | | Time 3 | |
|---|---|---|---|---|---|---|
| **Relationship status** | | | | | | |
| Dating | 4 | 13% | 0 | 0% | 1 | 3% |
| Cohabiting | 28 | 93% | 30 | 100% | 16 | 53% |
| Married | 0 | 0% | 0 | 0% | 3 | 10% |
| Broken-up | 0 | 0% | 0 | 0% | 10 | 33% |
| **Mother's employment** | | | | | | |
| Employed for two weeks or more in year prior to birth | 26 | 87% | – | – | – | – |
| Employed for two weeks or more in three months prior to birth | 11 | 37% | – | – | – | – |
| Employed in week prior to survey (birth) | – | – | 6 | 20% | 14 | 47% |
| **Mother's TANF receipt** | | | | | | |
| Received TANF in prior 12 months | 16 | 53% | – | – | – | – |
| Receiving TANF at time of survey | – | – | 10 | 33% | 12 | 40% |
| **Cohabiting partner's employment** | | | | | | |
| Number of mothers with cohabiting partners | 28 | 93% | 30 | 100% | 23 | 77% |
| Number/percentage employed in week prior to survey | 25 | 89% | 22 | 73% | 15 | 65% |
| Child age in months (mean) | 0 (0) | | 1-8 (3) | | 9-18 (12) | |
| Data source | Fragile Families Baseline | | TLC3 Baseline Couple | | Fragile Families One-Year | |
| Years | 2000 | | 2000-2001 | | 2001 | |

Source: Fragile Families and Child Well-Being Study, Baseline and One-Year surveys, and TLC3 Baseline couple interviews
Notes.
– indicates that the measure is not applicable or was not collected at a given time point.

employment and use of government assistance. Comparisons between the time points should be considered in the light of those differences.

All of the couples were romantically-involved at the time the focal child was born and when the TLC3 Couple interview was conducted between 1 and 3 months later. One-third of the sample had ended their romantic relationships by the time the Fragile Families One-Year survey was fielded. As we would expect, maternal employment rates decreased leading up to the birth and then increased over the next year. In the year before the birth, 87 percent of the mothers worked for pay during at least two weeks of the year. Shortly after the birth, only one-fifth were employed, but that fraction had increased to nearly half by the child's first birthday (on average).

Rates of TANF receipt also increased slightly in the year following the birth, from 33 percent shortly after the birth to 40 percent at Time 3.[16]

One aspect of maternal decisions to work following a birth that has not been explored sufficiently is the role of cohabiting partners. Beyond measuring how much "other income" available from partners, family, or government programs, research rarely places the work and welfare decisions of single mothers in the context of family circumstances. In this sample, nearly all of the mothers were cohabiting with the father of the focal child at the time the child was born (only 4 were not). By Time 2, all the couples were cohabiting, but by the end of the first year only half were. This is consistent with other studies documenting the volatility of the romantic relationships among unmarried parents (citations). The majority of cohabiting partners were employed in the week prior to the survey or interview, but the proportion does decline from nearly 90 percent to 65 percent over the course of the year.

Several studies of the welfare population have divided single mothers into categories based on work/welfare status, generically described as 1) work, no welfare, 2) welfare, no work, 3) work & welfare, and 4) no work or welfare (Dunifon, Kalil, & Danziger, 2003; Smith, Brooks-Gunn, Klebanov, & Lee, 2000). These categories have policy relevance because different combinations of welfare policies produce incentives for different behavior. For instance, states that disregard larger amounts of earnings from work from the calculation of case assistance benefits are implicitly encouraging category #3. Recently, women in the fourth category who report no work or welfare receipt have become a population of interest to researchers (Blank,

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[16] In this relatively small sample, this seven percentage point increase is the result of only two additional mothers receiving TANF.

2007). Several surveys of former welfare recipients have uncovered a small but growing proportion of low-income families who report no income from employment or government assistance (Brock et al., 2002; Loprest, 2002; Turner, 2006; Wood and Rangarajan, 2003).

In this sample, the distribution of families across the four categories differs substantially depending on whether families are categorized based on the mother's income sources or the couple's income sources. Figures 3.3 and 3.4 show the percentage of the sample in each income source category at the three time points described in Table 3.3. In Figure 3.3, sample members have been categorized by mother's employment status and welfare receipt; in Figure 3.4 both mother's and father's employment status, as well as mother's welfare receipt, are considered.

**Figure 3.3 Longitudinal Distribution of Sample Mothers across Work/Welfare Status Categories**
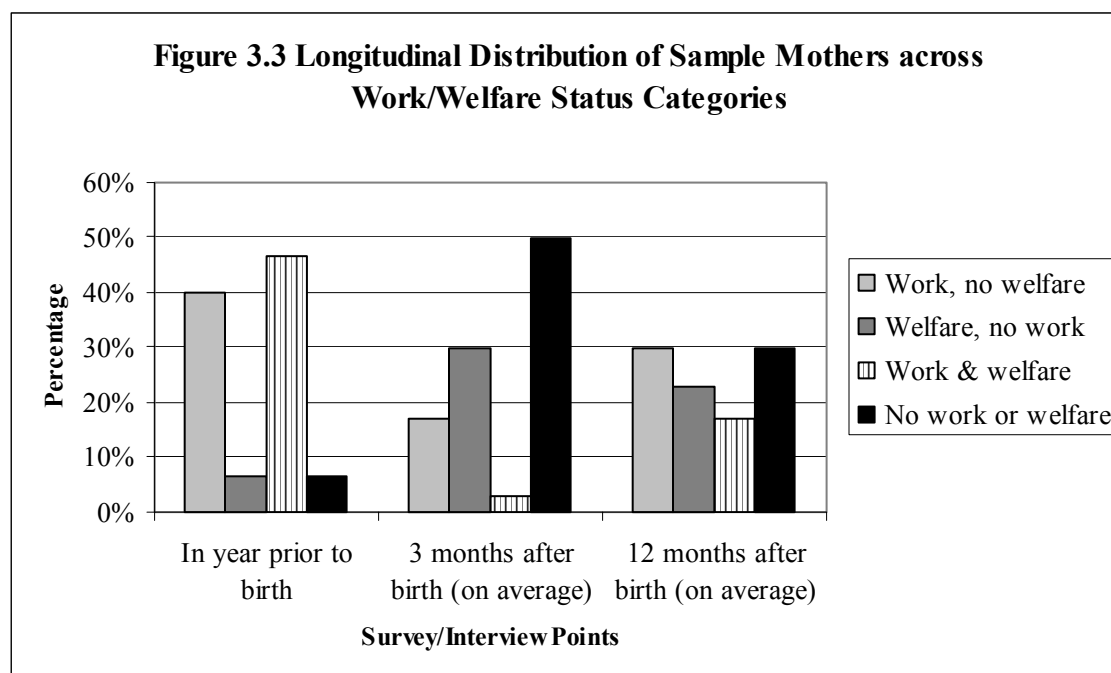


Of particular importance, these figures show that the proportion of the sample categorized as having no earnings from work decreases as time passes following a birth. At time point two, on average three months after the birth, nearly half the sample of mothers have no earnings from

work or welfare (Figure 3.3). However, two-thirds of those women were living with partners who were working. By the third time point, mothers fall into a relatively even distribution across the categories, but 30 percent still report no employment or welfare receipt. When father's employment is included, only 13 percent of families fall into the no work, no welfare category and 47 percent report earnings from work and no welfare receipt.

**Figure 3.4 Longitudinal Distribution of Sample *Couples* across Work/Welfare Status Categories**
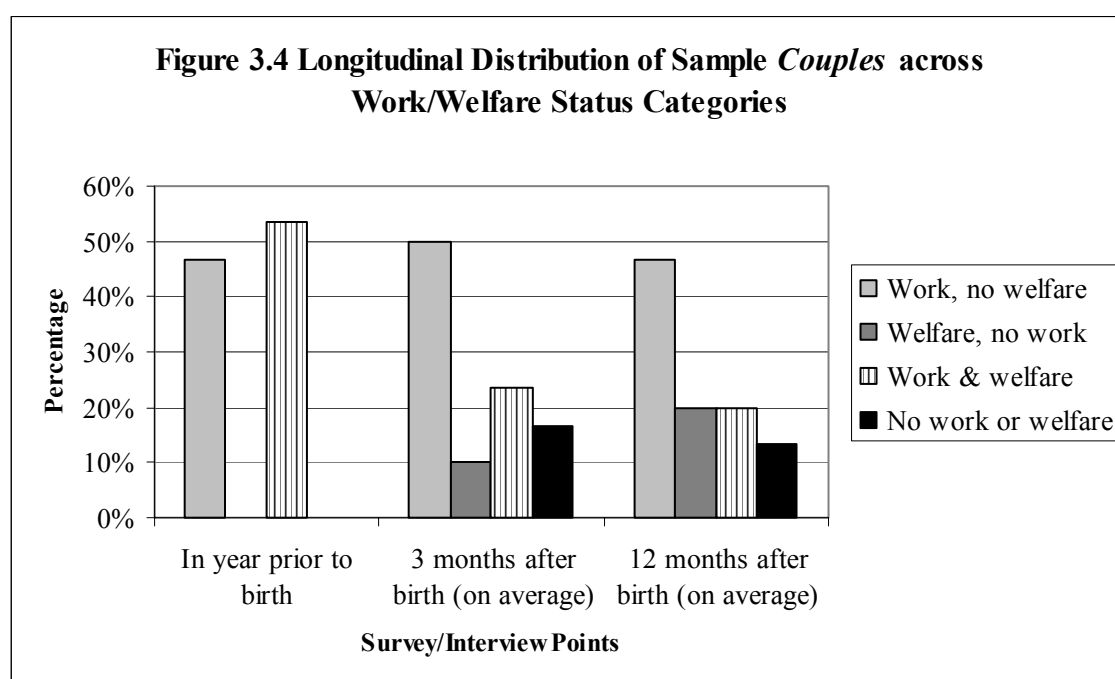


To focus further on the timing of maternal employment following a birth, Figure 3.5 graphs the proportion of the sample in each of three categories: worked in less than three months after the birth, worked between 3 and 11 months after the birth, and did not work in the first year. The graphs shows the distribution across these categories for the full sample of mothers and for two sub-groups, those who worked for two weeks or more in the last three months of their pregnancy (n=11) and those who did not (n=19).

Among all mothers in the sample, 43 percent did not return to work in the first year of the focal child's life. Twenty percent returned to work in less than three months after the birth and 37 percent began working sometime between the third and eleventh months. These proportions are roughly consistent with never married mothers nationally in the late 1990s (Figure 3.1).When the timing of work returns is compared across women who worked in the latter third of the pregnancy versus those who did not work late in the pregnancy, I find women who worked in the last three months of their pregnancies are more likely to return to employment shortly after a birth than women who did not work leading up to the birth. Of the 11 women who worked during the pregnancy, four (36 percent) returned to work within three months of having a child. This is compared to just two of 19 women (11 percent) who began working that soon after not having worked during the pregnancy.

**Figure 3.5 Timing of Maternal Employment Following a Birth in Analytic Sample as a Function of Employment Prior to the Birth**

Source. Fragile Families and Child Well-Being Study, One-Year Follow-Up Survey, conducted in 2001.
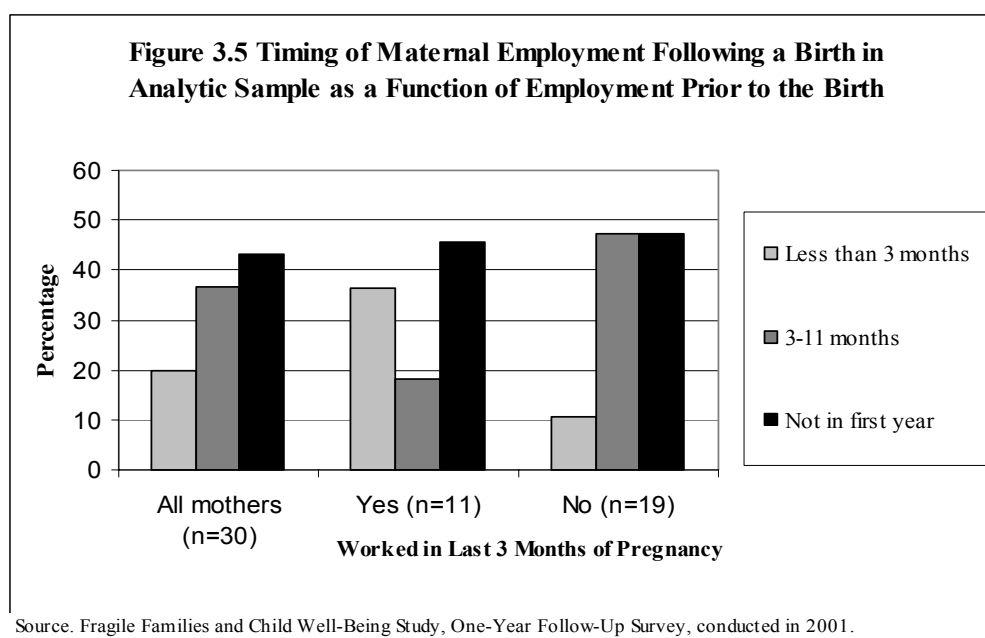
One final note about these tables: In the general population, the distinction between being unemployed (having quit a job or been fired) and being on leave, but still employed, is an

important determinant of the return to work following a birth (Klerman & Leibowitz, 1994). Paid or unpaid leave allows mothers to maintain employment while taking some time off to care for a newborn. The availability of this benefit has been shown increase the probability of leave taking and to decrease the time before a mother returns to work (Berger & Waldfogel, 2004). However, low-educated women are less likely to be employed in jobs that provide leave (Friedman, 2001; J. Heymann, Boynton-Jarrett, Carter, Bond, & Galinsky, 2002; J. S. Heymann & Earle, 1998). Consistent with this finding, only one mother in this sample describes taking leave rather than quitting a job when she became pregnant. For the remaining cases, these statistics reflect a return to employment, not work.

*QCA*

Consistent with these findings, the QCA analysis related four conditions—maternal work during the pregnancy, father's employment at the time of the birth, TANF receipt in the year before the birth, and whether the household included other children under 5 years of age—to the timing of maternal employment after a birth. Table 3.4 presents the absence or presence of each condition and outcome for each of the 30 cases included in this analysis. There is variation on all five of the conditions, but also some central tendencies. In most cases, the father had worked in the week prior to the birth (25 of 30) and the mother had not worked in the last three months of her pregnancy (19 of 30). The sample is split almost evenly on the measures of TANF in the year before birth and whether the household includes other children less than 5 years old.

The essential component of QCA, the "truth table," is presented in Table 3.5. The truth table displays all logical combinations of the four conditions and the number of cases represented by each configuration. In addition, it relates the conditions to the outcome variables by showing

the number and proportion of cases in each configuration for which the outcome variables was equal to one.[17] For instance, the first configuration is represented by eight cases, nearly two-thirds of which included mothers who were employed in the first year. Table 3.5 also reports the coverage of each configuration, which is the proportion of the total number of positive cases in the sample that represent this configuration.[18] About 30 percent of the 17 cases in which the mother returned to work in the first year had the combination of conditions in configuration number one.

In Table 3.6, the QCA results are presented as combinations of factors associated with a positive outcome. In the notation of Boolean algebra, a multiplication sign symbolizes the word AND, while a plus sign is the equivalent of the word OR. In addition, the conventional practice for QCA is to use capital letters to signify the presence of a condition and lowercase letters to represent the absence of a condition. For example, the first configuration for the outcome *employed in the first year*—TANF * pregwork * youngkids—indicates that the combination of a mother receiving TANF in the year prior to the birth, having not worked in the last three months of her pregnancy, and having no other children under five years in the household, is associated with a mother working during the first year. It is one of three separate configurations associated with this same outcome.

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[17] The proportion of positive cases is also called the "consistency" of the configuration. It is calculated as the number of positive cases in a configuration divided by the number of total cases in a configuration.
[18] Coverage is calculated as the number of positive cases in a configuration divided by the number of positive cases is the sample as a whole.

Table 3.4 Conditions and Outcomes for QCA Cases

| | Conditions | | | | Outcomes | |
|---|---|---|---|---|---|---|
| Case | Father worked in week prior to birth | Mother received TANF in year prior to birth | Mother worked in last 3 months of pregnancy | Other children ages 0-4 in household | Mother employed in first year | Mother not employed in first year |
| 1 | 1 | 0 | 0 | 0 | 0 | 1 |
| 2 | 1 | 0 | 0 | 0 | 1 | 0 |
| 3 | 1 | 1 | 0 | 1 | 1 | 0 |
| 4 | 1 | 1 | 0 | 1 | 1 | 0 |
| 5 | 1 | 0 | 1 | 0 | 1 | 0 |
| 6 | 1 | 1 | 0 | 1 | 0 | 1 |
| 7 | 1 | 1 | 0 | 1 | 0 | 1 |
| 8 | 0 | 1 | 0 | 1 | 1 | 0 |
| 9 | 1 | 1 | 0 | 1 | 0 | 1 |
| 10 | 1 | 1 | 0 | 1 | 1 | 0 |
| 11 | 0 | 1 | 0 | 1 | 0 | 1 |
| 12 | 1 | 0 | 0 | 0 | 1 | 0 |
| 13 | 1 | 0 | 0 | 0 | 1 | 0 |
| 14 | 1 | 1 | 0 | 0 | 1 | 0 |
| 15 | 0 | 1 | 0 | 0 | 1 | 0 |
| 16 | 1 | 0 | 1 | 0 | 1 | 0 |
| 17 | 1 | 0 | 1 | 1 | 1 | 0 |
| 18 | 1 | 1 | 0 | 1 | 1 | 0 |
| 19 | 1 | 1 | 1 | 1 | 1 | 0 |
| 20 | 1 | 0 | 1 | 1 | 0 | 1 |
| 21 | 1 | 1 | 0 | 1 | 1 | 0 |
| 22 | 1 | 1 | 1 | 0 | 0 | 1 |
| 23 | 1 | 0 | 1 | 0 | 1 | 0 |
| 24 | 0 | 1 | 1 | 1 | 0 | 1 |
| 25 | 1 | 0 | 1 | 1 | 1 | 0 |
| 26 | 1 | 0 | 0 | 1 | 0 | 1 |
| 27 | 0 | 0 | 1 | 0 | 0 | 1 |
| 28 | 1 | 1 | 1 | 0 | 1 | 0 |
| 29 | 1 | 0 | 0 | 0 | 0 | 1 |
| 30 | 1 | 0 | 0 | 1 | 0 | 1 |

Source: Fragile Families and Child Well-Being Study, Baseline and One-Year surveys.

Notes.

"0" indicates the absence of a condition; "1" indicates the presence of a condition.

One goal of this study was to use a small sample and the QCA method to examine the diversity of family circumstances surrounding the birth of a child. The results in Table 3.6 indicate that there are, in fact, multiple pathways to the same outcome and that mother's labor force attachment and family circumstances interact in complex ways. In order to identify a reduced number of conditions most strongly associated with the outcome, I make comparisons across cases as well. For the most part, there was not sufficient overlap between the conditions of individual cases in this study to develop reduced configurations; all but one of the final configurations included all four conditions. Put another way, all of the conditions in my model appear to be associated with the outcome.

The QCA process did not identify necessary conditions for either outcome; that is, no condition was present in all the configurations for either working or not working in the first year. There are some conditions present in two of the three configurations. For instance, father's employment in the week before the birth was present in two of the three configurations predicting maternal employment in the first year and TANF receipt in the year prior and having no other preschool children were both present in two of the three configurations predicting employment. The direction of the relationship with father's employment is counter to theoretical predictions that father's employment should reduce the opportunity costs of mothers remaining out of the labor force for longer. There is more ambiguity about the predictive effect of TANF receipt in the year prior to the birth. If women continue to receive TANF after having a child, they can do so for a period of time without fulfilling work requirements. However, the TANF rules in the three cities of this study require recipients to begin working within a year of the birth.

In the next sections, I explore these relationships and other configurations associated with the timing of maternal employment in more detail and relate them to specific cases in the sample. The in-depth information about family circumstances provided in the qualitative interviews offer some insight into the QCA results and potential conditions or interactions that my QCA analysis did not identify.

*Worked in the year after the birth.* About 80 percent of the mothers in this study said they expected to work in the first year after the focal child was born; around 60 percent of the case study sample actually did go to work in that year. According to the QCA analysis, maternal work in the first year after a birth is associated with three configurations of the conditions. The first grouping, TANF * pregwork * youngkids, is reduced from two configurations, each represented by one case in the data. The grouping covers 12 percent (2 of 17) of the positive cases in the data. The second configuration FAEMP * tanf * PREGWORK * youngkids was represented by three cases in the data, covering 18 percent of the total number of cases in which mothers worked in the first year. The final configuration is the presence of all four conditions—FAEMP * TANF * PREGWORK * YOUNGKIDS—which matches one case in the data.

I draw upon three cases to provide depth to the QCA solutions for maternal employment in the first year. I refer to the mothers and fathers in these cases as Linda and Lou, Keisha and Robert, and Barbara and Lonnie; each case represents one of the three configurations associated with a positive outcome. Linda and Lou, a white couple in their early 20s, were living together

Table 3.5  All Logical Configurations of Conditions

| | Conditions | | | | | Outcomes | | | | | |
| | | | | | | Employed in first year | | | Not employed in first year | | |
| Configuration | faemp | tanf | pregwork | youngkids | Number of cases | # of 1s | Proportion of 1s | Coverage of 1s | # of 1s | Proportion of 1s | Coverage of 1s |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Represented in the data | | | | | | | | | | | |
| 1 | 1 | 1 | 0 | 1 | 8 | 5 | .63 | .29 | 3 | .38 | .23 |
| 2 | 1 | 0 | 0 | 0 | 5 | 3 | .60 | .18 | 2 | .40 | .15 |
| 3 | 1 | 0 | 1 | 0 | 3 | 3 | 1.00 | .18 | 0 | .00 | .00 |
| 4 | 1 | 0 | 1 | 1 | 3 | 2 | .67 | .12 | 1 | .33 | .08 |
| 5 | 0 | 1 | 0 | 1 | 2 | 1 | .50 | .06 | 1 | .50 | .08 |
| 6 | 1 | 0 | 0 | 1 | 2 | 0 | .00 | .00 | 2 | 1.00 | .15 |
| 7 | 1 | 1 | 1 | 0 | 2 | 1 | .50 | .06 | 1 | .50 | .08 |
| 8 | 0 | 0 | 1 | 0 | 1 | 0 | .00 | .00 | 1 | 1.00 | .08 |
| 9 | 0 | 1 | 0 | 0 | 1 | 1 | 1.00 | .06 | 0 | .00 | .00 |
| 10 | 0 | 1 | 1 | 1 | 1 | 0 | .00 | .00 | 1 | 1.00 | .08 |
| 11 | 1 | 1 | 0 | 0 | 1 | 1 | 1.00 | .06 | 0 | .00 | .00 |
| 12 | 1 | 1 | 1 | 1 | 1 | 1 | 1.00 | .06 | 0 | .00 | .00 |
| Hypothetical (Remainders) | | | | | | | | | | | |
| 13 | 0 | 0 | 0 | 0 | 0 | ? | ? | ? | ? | ? | ? |
| 14 | 0 | 0 | 0 | 1 | 0 | ? | ? | ? | ? | ? | ? |
| 15 | 0 | 0 | 1 | 1 | 0 | ? | ? | ? | ? | ? | ? |
| 16 | 0 | 1 | 1 | 0 | 0 | ? | ? | ? | ? | ? | ? |

Source: Fragile Families and Child Well-Being Study, Baseline and One-Year surveys.

Notes.

"0" indicates the absence of a condition; "1" indicates the presence of a condition.

Table 3.6 Reduced Configurations and Variations in the Timing of Maternal Employment

| Outcomes | Groups of Conditions | Representative Cases | Coverage |
|---|---|---|---|
| Employed in first year | TANF * pregwork * youngkids+ | 14, 15 | .12 |
| | FAEMP * tanf * PREGWORK * youngkids+ | 5, 16, 23 | .18 |
| | FAEMP * TANF * PREGWORK * YOUNGKIDS | 19 | .06 |
| Not employed in first year | faemp * tanf * PREGWORK * youngkids+ | 27 | .08 |
| | FAEMP * tanf * pregwork * YOUNGKIDS+ | 26, 30 | .15 |
| | faemp * TANF * PREGWORK * YOUNGKIDS | 24 | .08 |

Notes.

Lower-case letters indicate the absence of a condition; Upper-case letters indicate the presence of a condition.

In Boolean notation, * is equivalant to the logical operator "and"; + is equivalant to the logical operator "or."

when they had their first child, a son. The couple's life circumstances leading up to the birth of their son met the conditions of the first configuration –TANF * pregwork * youngkids. At the time of the birth, neither parent was employed because Lou had recently been laid off from an electrician's job. Linda had worked early in her pregnancy as an at-home nurse for sick and disabled children, but the physical demands of the job became too great and she did not expect that the employer would keep her on if she took maternity leave. She said, "I don't think they wanted to give me maternity leave, either. I mean, I know that they couldn't SAY that." Although this issue did not come up explicitly in other cases, it raises the issue of whether employer investments in low-wage workers are too low to warrant providing leave.

Linda stated explicitly soon after the birth that she would find a new job with a different employer when her son was about three months old. She did exactly that, taking a position at a phone company. Around the same time, Lou found work at a bank and was looking for a second job to supplement their income. Linda described this as a particularly good time for the couple:

"…when I started going back to work…he was working at [the bank] and we were making it month-to-month and everything was fine, and we were able to save a little bit of money, but the moment I went to work full time, and everything after that, for those three or four months, we were doing really well."

The relative luxury of two full-time jobs ended quickly, when a few months later Lou was laid off when the bank went through a merger. After not receiving TANF for nearly a year, Linda reapplied for cash assistance during this time. Her use of TANF during times of need was consistent with both parents' views of the welfare system. Linda and Lou thought that work

requirements and time limits were reasonable restrictions to place on case assistance and that they helped reduce the abuses of the system.

Representing the second QCA configuration associated with maternal employment in the first year—FAEMP * tanf * PREGWORK * youngkids—are Barbara and Lonnie, a mixed-race couple also in their early 20s. Both parents were attached to the labor market around the time of the birth and worked during most of the following year, although not steadily in one position. At the time of the birth, Lonnie was working for a package delivery company and Barbara for an airline company. She took just three weeks off after having their son. Lonnie said,

> " I tried to get her to wait for a couple months, but she was ending one job and starting another, and they start at certain intervals throughout the year, and she didn't want to have to wait another four months to start."

Both parents emphasized their strong work ethics and disinterest in receiving government assistance. Barbara describes her attitude about work and welfare:

> "…when I turned fifteen, I basically started working. They put me in the office to do something…We work things out (financially). I mean, I don't know people that's on welfare, cause my mom was on it, but it's just something that I don't want to get into.

The couple goes so far as to criticize Barbara's sister for receiving assistance, despite the fact that she provides child care so that they can both work.

Less than four months after the birth, Lonnie lost his job. His driver's license had been suspended for speeding tickets, which made it difficult for him to find new work. At that point, Barbara was working full time in a job that provided health insurance for her and her child.

When the couple was interviewed again at 16 months, both were working full time, although at different jobs.

Finally, Keisha and David, an African-American couples who were 22 and 29, respectively, when interviewed shortly after the birth of their third child. Keisha and David represent the third configuration, the presence of all four conditions—FAEMP * TANF * PREGWORK * YOUNGKIDS. At the time they were living with their children, David's cousin, and her three sons, but hoping to move shortly to their own apartment. Keisha applied for cash assistance late in her pregnancy to facilitate her ability to stay home for a limited period of time with the baby, using TANF as a form of paid family leave. Keisha was familiar with specific TANF rules in her city, which allowed her to receive cash assistance for up to three months without meeting work requirements. These rules met her needs exactly. In her words,

> "They giving you a certain period of time to, you know, get yourself prepared,
> and do what you have to do…It's working. It's not like it was before where you
> just go collect that money, sit, or whatever. Now it's like helping better you, you
> know. Helping better you and your children. They're helping you find a job, you
> know."

In addition to providing some context for the QCA results, these case narratives highlight several important dimensions of maternal work decisions that were not immediately apparent from the QCA solutions. First, when interviewed right after the birth, all three women had a specific plan for when they would start looking for work and followed through on that plan despite having very different family circumstances. For instance, Linda returned to work after three months just as she had planned. This decision seemed almost irrespective of Lou's

movement in and out of employment, although her decision to combine work and welfare did appear to relate to how much Lou could contribute. Barbara knew she would return to her old employer soon after the birth because a new position was opening up for which she did not want to be passed over. Keisha was using TANF in a very deliberate and planful way, to support her for several months before she returned to work.

Second, in all three cases the mothers were relatively attached to the labor force and their pre-birth jobs suggests that they each had employable skills, in customer service and health care, for example. Linda is the only mother who did not work in the last three months of her pregnancy, but she had been working steadily and stopped because the job was physical trying for her and she did not expect she would be able to take time off after the birth and keep her job.

Third, although all three couples believed in individual responsibility, they had very different views of government assistance and those views matched their own behavior. Each of the three couples talks about TANF in the context of their work decisions even if only to say that they would never apply for it. In one case, the parents were explicitly against using welfare; in another, welfare was used when the father of the child lost his job; and in Keisha's case described above, she used welfare explicitly as a way to stay at home while her baby was very young.

Fourth and finally, the role of father's employment in these decisions is more complex than the QCA solutions suggest. Counter to theoretical predictions, when father's employment is related to maternal employment in the first year in the QCA analysis (it is not in one configuration), it is a positive association. That is, a father being employed in the week prior to the birth is positively associated with a mother working in the first year. Case narratives,

however, indicate that fathers' employment was not stable or consistent. Within the first year of the focal child's life, each of the fathers in these three couples was laid off at least once, and in one case a second time. The couples were cohabiting and each father was contributing to the family's resources, but these contributions were inconsistent enough that mothers seemed to pursue their own plan for returning to work and providing for the family, almost despite the father's moves in and out of employment.

*Did not work in first year after birth.* Not working in the first year after birth was also associated with three configurations, none of which were reduced from the original four conditions. The combination FAEMP * tanf * pregwork * YOUNGKIDS is represented by two positive cases; the other two combinations—(faemp * tanf * PREGWORK * youngkids) + (faemp * TANF * PREGWORK * YOUNGKIDS) are each represented by a single case.

The cases that exemplify these configurations are Teresa and Paulo, Octavia and Jesus, and Antonio and Maria. A Hispanic couple, Teresa and Paulo, represent the configuration faemp * tanf * PREGWORK * youngkids. At the time of the birth, the couple was having significant financial trouble. Paolo was a merchant marine who was having trouble finding employment since his work visa had expired. Because of these difficulties, Teresa was thinking about returning to work shortly after the birth, but she had concerns about placing their daughter in child care too soon. She said,

> "…I know she's too small right now to leave her with a babysitter and I don't
> want that. But when she's like around 4 months, then I, I'm thinking about going
> back to do something…I'm going to work and have my own money…I'm just

going to wait for a little while…until she's eating like solid food, so that way, you know, she doesn't miss me that much."

His unemployment only lasted a few months, but by the time he started working for Teresa's uncle in a construction job, they were behind on rent payments and could not make outgoing calls from their home phone.  As she said in the quote above, Teresa planned to return to work when her child was about four months but before she had that opportunity, she became pregnant with their second child. Teresa voiced some sadness that her plans for finding a job or returning to school would be delayed while she cared for the new baby. "I don't to want to [take care of kids] for the rest of my life," she said. "I want to do something too, you know? I've been tired of being home all day." Paolo was supportive of her eventually finding work but thought that she ought to be a patient and wait until a particularly good job was available.

Octavia and Jesus had similar concerns about child care as Teresa and Paul. The couple, also Hispanic, were living in a home that they owned with their other infant daughter together. Octavia had worked for pay after the birth of their first child.  After this birth, however, she expected to be a full-time homemaker for an extended period of time. Both parents agreed that they did not have a child care option that would feel comfortable with.

"…he (Jesus) can't stay home to take care of the kids, so that makes me to stay home, because his mother works and my mother works. Those are the only two people we trust to take care of the kids. I think, when they grow up, like when they're ready to go to either pre-K or a day care, maybe I will start going to school again."

Jesus explained the situation similarly, "I have no problem with her working…I don't like anybody I don't trust to take care of the kids. So, that's why basically she's not working."

By the time her son was four months old, Octavia was getting anxious to return to school and not spend all day caring for her children. "I spend 24 hours in here," she said, "so I kind of missed being outside." At the same time, she felt pressure from Jesus to begin contributing financially to the family. Nine months later, Octavia had started helping her mom clean apartments one day a week (this work was not coded as employment in the QCA analysis), but her plans of returning to school were still on hold because they did not have a babysitter they trusted.

Finally, Antonio and Maria, whose conditions prior to the birth were faemp * TANF * PREGWORK * YOUNGKIDS, are both Puerto Rican and the parents of twin daughters. The year after the birth of Antonio and Maria's twin daughters was volatile. The twins were their fourth and fifth children and they were also living with Antonio's cousin and her three children. Antonio was working as a manual laborer and Maria was receiving TANF while she cared for the babies. They are both highly critical of the work requirements imposed on welfare recipients, particularly of public jobs that they perceive as make work. Maria told her caseworker, "…send me for some training. Learning something, training, even go for your GED. Something like that, but not sweep the streets."

Their economic circumstances declined when Antonio lost his job, about two months after the twins were born. Maria had work experience and was interested in returning to school for a Certified Nurses Assistant degree, but Antonio had traditional views about gender roles and

preferred that Maria stay at home. He also felt that their lives were too unstable at the time for Maria to begin working. In his words,

"You know, right now, I feel that [Maria] should take care of first the things with the housing and apartment. Get everything straight…cause we have a lot of problem that we have to face…we have no gas right now. Two months without gas"

Similar to some of the mothers who returned to work within the first year, Maria was receiving TANF immediately after the birth of her twin daughters with Antonio so that she could stay home with the babies. The couple faced considerable financial problems during the first year, but Antonio felt strongly that it was Maria's job to stay at home with the children. They resented the fact that the TANF rules required that Maria work after the children were 12 months old, particularly because they had heard stories of the "make-work" positions that welfare recipients were filling. Her benefit amount was eventually cut because she refused to comply.

By the time the twins turned one-year-old, they were living with an Aunt in a nearby state because Antonio and Maria were having serious financial difficulties. Antonio was employed again but they were being evicted from their apartment. Maria was still a stay-at-home mother and was receiving TANF, but her benefit amount had been reduced because she did not comply with work requirements and was at risk of being taken away completely.

Couples fitting these configurations of conditions were similar in many ways to the couples in which the mothers returned to work within a year. Several of the mothers had worked in the three months prior to the birth and had expectations that they would return to work during the first year. Also, in all the cases, the fathers' employment was unsteady throughout the

pregnancy and the year following the birth. A few factors differentiate them from the positive cases: the lack of acceptable child care and the father having a traditional view of gender roles. Also, both parents in all three couples are ethnically Hispanic. The QCA analysis did not include race or ethnicity as a condition, partially because those characteristics are confounded with city of residence in this study. It is plausible that Hispanic couples are more likely to practice a traditional division of labor or to have other reasons to wait longer before the mother begins working after a birth. From these data and methods, I am not able to shed much light on that possibility.

*Regression results*

Based on a combination of the QCA results and more detailed comparison of specific cases, I developed several hypotheses that were testable in a conventional regression framework using the full Fragile Families sample that met the criteria for this study (n=2,715; Table 3.1). The hypotheses (listed below) relate both to the individual contribution of specific conditions controlling for all others and to interactions between conditions. They are informed by the QCA and narrative analyses in the previous section, but also by existing theory and research on maternal work decisions following a birth. The hypotheses are:

Hypothesis 1. Mother's employment during the pregnancy will positively predict employment in the first year.

Hypothesis 2. Father's employment in the week prior to the birth will be positively associated with employment in the first year, but father's steady employment in the year prior to the birth will be negatively associated with employment in the first year.

Hypothesis 3. The case studies suggest that two factors that were not included as conditions in the QCA, mothers' expectations about working and about having help with child care, will both be positively associated with maternal employment in the first year.

Hypothesis 4. The effect of TANF receipt in the year preceding the birth will be moderated by maternal attachment to the labor force and father's employment

To explore these hypotheses, I first calculated zero-order correlations between the dichotomous variable indicating whether the mother worked in the first year and each of the four conditions used in the QCA analyses. As shown in Table 3.7, only one condition, whether the mother worked for pay in the last three months of the pregnancy, is significantly correlated with work decisions after the birth. In addition, TANF receipt in the year prior to the birth is negatively associated with both father's and mother's employment, but positively associated with having other preschool children.

The direction and significance of these relationships remain the same in a multivariate regression framework. Column two in Table 3.8 presents logistic regression coefficients and standard errors in a model relating the four QCA conditions to the outcome. The direction of coefficients suggests a positive relationship between father's employment at the time of the birth and mother's employment in the year after. This is counter-intuitive but consistent with the QCA results. The coefficient is small and insignificant, however. The coefficients on TANF receipt and having other preschool children—negative and positive, respectively—are also insignificant. Supporting my first hypothesis, the only statistically significant relationship is that mother's work in the last three months of the pregnancy is associated with higher rates of

employment in the first year. Using a base probability of .73, the coefficient is translated into a marginal effect of 8 percentage points.

Table 3.7 Zero-Order Correlations between QCA Conditions and Maternal Employment in the First Year After Focal Child's Birth

|           | work   | faemp  | tanf   | youngkids | pregwork |
|-----------|--------|--------|--------|-----------|----------|
| work      | 1.00   |        |        |           |          |
| faemp     | .01    | 1.00   |        |           |          |
| tanf      | -.03   | -.12** | 1.00   |           |          |
| youngkids | .00    | .01    | .27**  | 1.00      |          |
| pregwork  | .09**  | .05    | -.15** | -.02      | 1.00     |

Notes
**p<.01

The next two columns show similar models but with increasing levels of controls at the mother- and family-level. In the first, I add a set of controls that were all measured by mother's report include mother's race, education, and expectations for employment. In the second, I add a measure of the stability of father's employment in the year prior to the birth. I define steady employment as working in 26 or more weeks. The measure is reported by the fathers and has a much higher proportion of missing observations. As I hypothesized, father's steady employment in the year prior to the birth is negatively associated with mother's employment in the first year, although the coefficient is imprecisely estimated. Overall, these controls make little difference in the estimates for the four primary conditions. The significant coefficient on *pregwork* remains significant and of the same size. There are only two control variables that are themselves related to the outcome, mother's education and the availability of child care assistance.

My third hypothesis was that mother's expectations about working and child care availability would be related positively to her employment in the first year. Neither of the coefficients on these variables is statistically significant. The effect of expecting to work is positive in the third model, but becomes negative once father's steady employment in the year prior to the birth is substituted for father's employment in the week prior to the birth. It is highly likely that mothers' expectations about working are correlated with the stability of fathers' employment, but it is not clear why the coefficient on mothers' expectations becomes negative.

To test the fourth hypotheses about moderating factors, I also estimated an interactive model. The model includes variables for father's employment interacted with mother's employment; mother's employment interacted with TANF; and a three-way interaction of those components. Those results are presented in column five of Table 3.8. None of the coefficients are significant. The direction of the first and the last (faemp X pregwork and faemp X pregwork X tanf) are negative, and the second (tanf X faemp) is positive.

I selected four conditions for the QCA analysis that I argued were supported by theoretical predictions and prior empirical work on the determinants of maternal employment. The pseudo R-squared in the regression models suggest that combined these variables account for less than one percent of the variance in maternal employment. This is surprising given that these conditions have been consistently linked to maternal work decisions in large quantitative studies. This suggests that either I selected conditions that were not as relevant as I presumed, or that the conditions were relevant but I measured them poorly, or some combination of both problems.

Table 3.8 Results of Logistic Regression Models Testing QCA-Derived Hypotheses

| Independent variables | Additive Models | | | Interactive Model | Final model |
|---|---|---|---|---|---|
| | 1 | 2 | 3 | | |
| **QCA conditions** | | | | | |
| **Father's employment** | | | | | |
| Worked in week prior to birth (faemp; used in QCA) | .008 (.134) | .034 (.148) | | -.120 (.210) | |
| Worked in 26 or more weeks of year prior to birth (alternative) | | | -0.126 (.209) | | |
| Mother recieved TANF in year prior to birth (tanf) | -.069 (.137) | -.010 (.148) | .114 (.188) | -.133 (.261) | -0.139 -0.215 |
| Other children in household less than 5 years old (youngkids) | .013 (.139) | -.051 (.151) | -.105 (.187) | -.052 (.150) | .178 (.137) |
| Mother worked 2 weeks or more in 3 months prior to birth (pregwork) | .395** (.136) | .341* (.146) | .440** (.184) | .461** (.198) | .374* (.186) |
| **Interactions of interest** | | | | | |
| tanf X pregwork | | | | -.123 (.363) | -.065 (.367) |
| tanf X faemp | | | | .364 (.319) | .216 (.229) |
| tanf X faemp X pregwork | | | | -.229 (.421) | -.168 (.417) |
| **Mother's baseline characteristics** | | | | | |
| **Race** | | | | | |
| White | | – – | – – | – – | – – |
| Black | | .152 (.176) | -.094 (.220) | .147 (.176) | |
| Other | | -.013 (.208) | -.318 (.258) | -.017 (.209) | |
| Has HS Diploma/GED | | .202 (.142) | .300+ (.178) | .192 (.143) | .132 (.136) |
| Expected to work in the year after the birth | | .079 (.237) | -.184 (.288) | .072 (.238) | |
| **Family baseline characteristics** | | | | | |
| Number of adults in the household | | -.003 (.068) | .029 (.087) | -.000 (.068) | -.010 (.061) |
| Mother says someone will help with childcare in next year | | .288 (.231) | .337 (.295) | .278 (.232) | |
| Mother is cohabiting with father of child | | -.048 (.150) | -.157 (.190) | -.040 (.150) | -.054 (.134) |
| Observations | 1,159 | 1,062 | 703 | 1,062 | 1,162 |
| Pseudo R-squared | .007 | .012 | .020 | .013 | .007 |
| Wald chi-squared | 9.44+ | 14.29 | 15.29 | 16.24 | 10.57 |

Notes
*p<.05; **p<.01
– indicates an omitted category

Finally, my analysis plan called for estimating a last, best model based on the results of all three analysis—QCA, case study, and regressions. I used the second randomly-selected half of the Fragile Families sample to do this and the results are presented in the final column of Table 3.8. The results of the regression analyses were not conclusive enough to provide real

guidance on the specification of this final model. I reduced the number of conditions and control variables to basic demographic characteristics and family structure. I did not include father's employment because the father-reported measure of steady work, while more consistent with theoretical predictions, had such a high proportion of missing cases. As with the other models, mother's employment in the three months prior to the birth is the only significant predictor of her employment after the birth. The present study was designed to identify patterns of interactions between different factors related to a mother's decision to work in the first year after the birth. Instead, the conventional statistical methods I used to test the hypotheses driven by the QCA results point to a single factor of paramount importance, mother's employment during the pregnancy.

*Discussion*

The purpose of this study was to better understand the complexity of maternal work decisions following a birth. I used an inventive combination of methods to both describe the context of individual decisions and to draw conclusions across cases about the determinants of maternal employment in the first year. My analytic strategy was to use QCA and narrative analysis of individual cases to identify hypotheses about individual factors and interactions between factors associated with maternal employment in the first year. I then conducted some exploratory regression analyses, testing both additive and interactive models with the QCA conditions only and with an additional set of controls.

My approach produced mixed results. The QCA analysis did identify multiple pathways to the same outcome, but there was so little overlap between cases predicting a positive outcome that the configurations of conditions were generally not informative. The analysis of qualitative

data for cases that exemplified the configurations fleshed out the QCA results, but also introduced more complexity and some relevant factors that the QCA analysis did not identify (largely because they were not part of my simplified a priori model of predictors of maternal employment). The clearest finding from the analyses is that maternal human capital or labor force attachment, as measured by employment in the last three months of the pregnancy, was consistently associated with returns to work within a year. This is consistent with national statistics (J. O. Johnson & Downs, 2005) and with several studies of the timing of maternal returns to work that controlled for pre-birth employment (Klerman & Leibowitz, 1999; Leibowitz, Klerman, & Waite, 1992).

These analyses also suggested that TANF receipt was a key factor in the couples' decisions about maternal employment, even if it was simply posed as an option they preferred not to take. Some mothers used TANF strategically or sporadically, while others used it as a way to be stay-at-home mothers and were frustrated that the system requires them to work. The role of father's employment was also not uniform. Spousal employment and income theoretically increases the probability of mothers staying home for longer. In this sample of unmarried and predominantly low-education parents, the fathers' employment was constantly in flux and that instability seemed to attenuate its influence on maternal employment decisions. Child care was not something I captured in the QCA, but based on the case studies, it was clearly important to the decisions of mothers who did not work in the first year. This too is consistent with a variety of qualitative and quantitative studies (Leach et al., 2006; Leibowitz, Klerman, & Waite, 1992).

The study was designed to identify aspects of the maternal decision to work that might not emerge in large-scale quantitative analyses. The role of cohabiting father's employment in determining the employment of new mothers is one of those factors. More theory-building and research should be done to understand whether and how the role is similar for married and unmarried couples. Also, several mothers in this study quit jobs during the pregnancy, often in the first few months. This is consistent with population statistics on work during pregnancy among never married moms, and in contrast to married mothers most of whom work up until the last month. As far as I know, differences in employment during pregnancy by marital status or education have not been explored. Perhaps they are the result of incentives created by the welfare system or characteristics of the jobs in which single mothers are employed. A methodological issue arises from this difference as well. If human capital or labor force attachment is measured using pre-birth employment, the construct being measured may differ substantially for single mothers than other mothers.

A methodological result from this exploratory study is that, in this context, the creation of a small number of binary conditions for the QCA did not actually capture the complexity in families' lives very well. It is possible that Ragin's fuzzy-set method (Ragin, 2000) would be more successful, in that it allow for degrees of membership in a given condition. However, I suspect that narrative analysis of qualitative data on individual cases is still the most promising method of describing complexity. Of course, while cross-cutting themes are commonly identified in purely qualitative studies, there is no systematic approach to identifying causal relationships with narrative analysis. Still, in this study, I am not sure that the QCA analysis

produced results that could convincingly be interpreted as causal relationships. It is difficult to know how much weight to give to results driven by one or two cases.

Despite the limitations of the QCA analysis, I did use the results to identify several testable hypotheses. The implementation of these tests in a conventional regression format was challenging, however. The additive model, which included the four conditions of the QCA analysis, did suggest that mother's labor force attachment during the pregnancy was highly predictive of employment in the first year, even after controlling for a set of maternal and family characteristics. This is consistent with evidence from other studies. Yet, the overall explanatory power of the models, as measured by the R-squareds, was extremely low. This does not match the theoretical or empirical predictions that these key conditions would explain substantial variation in maternal work decisions in the first year.

CHAPTER FOUR

MATERNAL EMPLOYMENT AND LOW-INCOME CHILDREN'S EARLY

DEVELOPMENT: USING WELFARE EXPERIMENTS TO ADDRESS THE

ENDOGENEITY OF WORK DECISIONS

The preceding chapters examined how policies, mother's human capital, and family

factors influence the employment decisions of single mothers with young children. The focus of

those studies on mothers with infants and toddlers was motivated by theoretical and empirical

evidence that children are most vulnerable to changes in family income and maternal

employment in the early years of their lives (McCall, 1981; Shonkoff & Phillips, 2000;

Thompson, 2001). This chapter examines directly how young children in low-income, single-

parent homes are affected by maternal employment.  I examine not only the amount that a

mother works early in her child's life, but also the stability of that employment, which is an

understudied but potentially critical aspect of maternal employment and family life.  In doing so,

the present study improves on most observational studies of maternal employment and child

well-being by using an instrumental-variable (IV) estimator to control for the many unobservable

differences between working and non-working mothers.

*Early Maternal Employment and Child Development in Low-Income Families*

The relationship between maternal employment and children's development is not a

simple one, particularly with regard to low-income families. There is accumulating and

increasingly robust evidence that maternal employment in the first year of a child's life is

associated with modest decreases in cognitive ability, generally measured during middle

childhood (Baum, 2003; Berger, Hill, & Waldfogel, 2005; Blau & Grossberg, 1992; J. L. Hill, Waldfogel, Brooks-Gunn, & Han, 2005; James-Burdumy, 2005; Ruhm, 2004). In contrast, maternal work after the first year and continuously in the first three years has been related to higher scores on reading and math tests (Blau & Grossberg, 1992; W. Han, Waldfogel, & Brooks-Gunn, 2001; James-Burdumy, 2005; Waldfogel, Han, & Brooks-Gunn, 2002). With regard to socioemotional development, several studies find that early maternal employment predicts increased problem behavior (Baydar & Brooks-Gunn, 1991; Belsky & Eggebeen, 1991), but more find no relationship whatsoever (e.g Chase-Lansdale et al., 2003; W. Han, Waldfogel, & Brooks-Gunn, 2001; Parcel & Menaghan, 1994).

In addition, family circumstances, such as mother's marital status and family income, appear to moderate the effects of maternal employment on children, such that the least advantaged children are the most likely to benefit from (or at least not be harmed by) their mothers working. The negative effects of maternal employment on infants are generally limited to or larger for more advantaged children—whites, those in two-parent homes, and those in higher income households (Baydar & Brooks-Gunn, 1991; Brooks-Gunn, Han, & Waldfogel, 2002; Desai, Chase-Lansdale, & Michael, 1989; Waldfogel, Han, & Brooks-Gunn, 2002). One observational study specifically linked early maternal employment to positive cognitive and socioemotional outcomes among low-income children (Vandell & Ramanan, 1992).

More recent evidence suggests that it may not be maternal employment, per se, that positively predicts the outcomes of low-income children, but rather maternal employment accompanied by income boosts, either from earnings or welfare receipt (Dearing, McCartney, & Taylor, 2006; Kalil, Dunifon, & Danziger, 2001; Morris, Duncan, & Clark-Kauffman, 2005).

For instance, the effects of experimental welfare programs on preschool children's school achievement were shown to be neutral if the program only increased maternal employment, but positive if it also increased family income (Morris, Duncan, & Clark-Kauffman, 2005). Similarly, in Moore & Driscoll (1997) maternal employment was associated with reductions in children's problem behavior, but only if the mother's hourly wage was above a certain level.

In samples of single-mothers or welfare recipients, it is maternal unemployment or job loss, and welfare reliance, which are more consistently associated with negative outcomes for children. Consecutive years of unemployment or reliance on income from welfare programs is correlated with worse mother-reported rating of child health (Secret & Peck-Heath, 2004), lower cognitive test scores, increased behavior problems (Kornberger, Fast, & Williamson, 2001; Smith, Brooks-Gunn, Klebanov, & Lee, 2000), and lower educational attainment (Ku & Plotnick, 2003).

There is also growing evidence that maternal job loss is detrimental to low-income, adolescent children's behavioral and school outcomes, particularly when it occurs repeatedly or regularly (R. C. Johnson, Kalil, & Dunifon, Forthcoming; Kalil & Ziol-Guest, 2005; Randolph, Rose, Fraser, & Orthner, 2004). The effects of transitions out of employment on low-income children under five years old are largely unknown, although the results of a study of maternal transitions in and out of welfare and employment suggests, counter to theoretical predictions, that preschool children may be more resilient to job losses than adolescents (Chase-Lansdale et al., 2003).

*The Mechanisms of Employment Effects on Children*

Considering the pathways through which maternal employment is thought to affect child well-being, it is sensible that the relationship might differ by family resources and structure. According to economic theory, the effects of maternal employment depend on whether increases in family income outweigh any corresponding reductions in parental time spent with children (Becker, 1991; Bergstrom, 1997; Desai, Chase-Lansdale, & Michael, 1989). However, incremental changes in income are proportionally larger for low-income families and appear to be more consequential for the development of poor children than for other children (Dearing, McCartney, & Taylor, 2006; Duncan, Brooks-Gunn, Yeung, & Smith, 1998; Mistry, Biesanz, Taylor, Burchinal, & Cox, 2004; Votruba-Drzal, 2006). Similarly, psychological theory suggests that the effects of maternal employment derive not only directly from changes in resources, but indirectly from changes in parental well-being and parenting behavior (Chase-Lansdale & Pittman, 2002; Conger et al., 1992; Conger, Ge, Elder, Lorenz, & Simons, 1994; V.C. McLoyd, 1990, , 1998; V. C. McLoyd, Jayaratne, Ceballo, & Borquez, 1994; Mistry, Biesanz, Taylor, Burchinal, & Cox, 2004), the size and direction of which might quite plausibly vary by family characteristics.

It is well-established that parental psychological well-being, and its effects on parenting practices, are a key pathway through which economic deprivation negatively affects children (Conger et al., 1992; Conger, Ge, Elder, Lorenz, & Simons, 1994; V.C. McLoyd, 1990, , 1998; V. C. McLoyd, Jayaratne, Ceballo, & Borquez, 1994; Mistry, Vandewater, Huston, & McLoyd, 2002). It follows that if maternal employment improves the economic well-being of the family, it may also lead to things like decreased stress and depression in parents, increased parental

warmth and more positive interactions between parent and child. However, low-income mothers are disproportionately employed in low-quality jobs— characterized by low-pay, limited benefits, and unstable or nonstandard hours (Dunifon, Kalil, & Bajracharya, 2005), which may not reduce economic deprivation and could be detrimental to parental well-being (Parcel & Menaghan, 1994).

Maternal employment also increases the relevance of non-parental child care settings, the effects of which on children have been shown to differ by the income level of the family. Studies have shown that low-income preschoolers benefit from high-quality and center-based child care situations, compared to low-quality and home-based (Burchinal et al., 2000; Network, 2000; Shonkoff & Phillips, 2000; Votruba-Drzal, Coley, & Chase-Lansdale, 2004). In general, welfare programs with positive child impacts had increased the use of child care and after-school programs (Morris, Huston, Duncan, Crosby, & Bos, 2001), but a separate study of low-income families found no significant relationship between welfare status and type, quality, or characteristics of child care (Coley, Li-Grining, & Chase-Lansdale, 2006).

*The Endogeneity Problem*

Nearly all studies on this topic are observational, making it difficult to control for all the differences between women who work and those who do not, many of which are not observable or measurable. A depressed mother may be less likely not only to work, but also to practice warm parenting, which can lead to poor child behavior. Or, the mother of a child who develops language skills later than average might wait longer to begin work because she thinks the child will do better in center-based child care once he/she is talking. If differences like these are unmeasured, then estimates of the effects of maternal employment on children will be biased.

The fundamental problem is that maternal employment, like most human behavior, is not easily (or ethically) manipulatable in a controlled setting. Neither is it conceivable that a study would identify and collect data on every possible confounding factor, which almost guarantees that estimates of the effects of maternal employment on children are, to some degree, biased by omitted variables (also referred to as "unobserved heterogeneity").

Recently, a set of studies of maternal employment and child well-being have tackled the issue of omitted variable bias directly using larger sets of controls and/or econometric techniques such as fixed-effects, instrumental-variables, and propensity score matching (Baum, 2003; Blau & Grossberg, 1992; J. L. Hill, Waldfogel, Brooks-Gunn, & Han, 2005; Ruhm, 2004). In different ways, these methods all approximate experiments by controlling for unmeasured as well as measured correlates. These studies strengthen the evidence that maternal employment in the first year of a child's life is associated with lower scores on cognitive tests in middle childhood. For instance, using propensity score matching to address selection bias, Berger et al. (2005) found that children of mothers who returned to work within 12 weeks of their birth exhibited more externalizing behavior, on average, at age four. To date, studies that carefully control for omitted variable bias have not addressed how the effects of maternal employment may differ for low-income children or how employment stability and job loss relate to child outcomes.

In the present study, I estimate the effects of level and stability of maternal employment on very young children in families receiving welfare cash assistance. The measures of child well-being are parent-reported assessments of cognitive and behavioral development collected when the children were two to seven years old. To effectively control for both measured and

unmeasured correlates of employment and child-well being, I estimate an instrumental-variable model, in which random assignment to the treatment group provides a source of exogenous variation in both the level and stability of maternal employment. Under certain assumptions, this approach produces unbiased estimates of the causal relationship between maternal employment and young children's development.

The variation in maternal employment provided by random assignment comes not just from the five different studies, but also from several of those studies testing multiple experiments in multiple sites. This approach, outlined in Gennetian et al. (Gennetian, Morris, Bos, & Bloom, 2005), has been used in similar studies of the effects of income and child care type on child development (Crosby, Dowsett, Gennetian, & Huston, Under review; Morris, Duncan, & Rodrigues, 2005).

*Methods*

*Data*

This study uses data from five random-assignment evaluations of welfare reform programs conducted by the research organization MDRC in the late 1990s. Table 4.1 lists the studies, sites, follow-up periods, and core program components. All of the evaluations began prior to the passage of the federal welfare reform legislation in 1996 as experimental demonstrations of approaches to increasing parental employment and reducing welfare receipt among single mothers. The programs were not identical to one another, but they included some combination of five core components. All of the studies encouraged work by conditioning welfare benefits on participation in employment-related activities, such as job searching, job training, and employment. These mandates were enforced through sanctions, which resulted in

reduced benefit levels in the event of non-participation. Connecticut Jobs First and Florida's Family Transition (FTP) program also set limits on the length of time parents could receive welfare benefits.

Table 4.1 Experimental Studies Included in Analysis

| Study | Sites | Follow-up Period(s) (in years) | Work Requirements | Generous Earnings Supplements | Expanded Child Care Assistance | Time Limits | Basic Education Requirements |
|---|---|---|---|---|---|---|---|
| | | | | Core Program Components | | | |
| Connecticut Jobs First (CT Jobs First) | New Haven, CT Manchester, CT | 3 | X | X | | X | |
| Florida's Family Transition Program (FTP) | Escambia County, FL | 4 | X | | X | X | |
| New Hope Project | Milwaukee, WI | 2 | X | X | X | | |
| Minnesota Family Investment Program (MFIP)[a] | Minnesota counties | 3 | X | X | X | | |
| National Evaluation of Welfare to Work Strategies (NEWWS)[b] | Atlanta, GA Grand Rapids, MI Riverside, CA | 2 | X | | | | X |

Notes.

[a]The MFIP study tested two experimental programs. The full MFIP program, which mandated employment and provide a set of financial incentives to work, including an earnings-supplement, was implemented in seven counties. The MFIP Incentives-Only program offered the financial incentives without work requirements and was implemented in four counties.

[b]The NEWWS study tested two experimental programs at each of the three sites. The Labor Force Attachment (LFA) program mandated employment, while the Human Capital Development (HCD) program required participation in basic education.

In addition to work requirements and time limits, three of the programs (CT Jobs First, New Hope, and MFIP) offered financial incentives for employment. Earnings supplements were provided either by reducing the implicit tax rate on earnings for families receiving welfare or by directly supplementing family income from sources outside the welfare system. Some programs also provided child care assistance beyond what parents would receive in the standard policy regime. FTP, MFIP, and New Hope made it easier for parents to purchase child care through

some combination of subsidies, direct payment to child care providers, promotion of formal child care, and access to child care resource and referral services.[19]

Two of the studies in this analysis tested two different programmatic approaches. For the MFIP evaluation, applicants were randomly assigned to the control group, a treatment group receiving work requirements and financial incentives, and a treatment group receiving just financial incentives. These programs are referred to as MFIP Full and MFIP Incentives-Only, respectively. Similarly, the National Evaluation of Welfare to Work Strategies (NEWWS) evaluation included two treatment groups, one that mandated immediate employment, called Labor Force Attachment (LFA), and the other that required participation in basic education classes, called Human Capital Development (HCD).

In each study, sample members were randomly assigned either to a program group that was subject to a new set of welfare rules or to a control group that received the standard benefits available to AFDC recipients and low-income families. In most studies, parents were applying for welfare or renewing eligibility when they were randomly assigned and receipt of welfare included participation in the study effort (i.e., random assignment and collection of administrative data).[20]

As part of the Next Generation Project[21], MDRC created a stacked child-level dataset containing observations from children who mothers participated in an MDRC welfare-reform evaluation.  For this study, I narrowed that sample to children 0 to 3 years of age at the time of

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[19] The New Hope Project also subsidized health insurance for the program participants.
[20] In the case of New Hope, all low-income parents within a geographic region were eligible to participate on a voluntary basis.
[21] For more information on the Next Generation Project: http://www.mdrc.org/project_8_10.html

random assignment to the experiments (n=5,297).[22] Table 4.2 presents the sample sizes for each

program tested by experimental status (treatment or control group) and child age. These case

counts include children age 0-3 years at the time of random assignment that had at least one non-

missing outcome measures (problem behavior, positive social behavior, or achievement).

*Measurement*

The data for this study come from multiple sources: an information form completed by

the participants prior to random assignment, interviews of participants by program staff, state

and federal administrative records, follow-up surveys of parents and teachers, as well as tests

administered directly to children. Even though parents could opt out of the survey effort

associated with each evaluation, response rates in all studies were between 71 and 90 percent (D.

Bloom et al., 2000; D. Bloom et al., 2002; Bos et al., 1999; Gennetian & Miller, 2000; Hamilton

et al., 2001; McGroder, Zaslow, Moore, & LeMenestrel, 2000; Morris & Michalopoulos, 2000).

Table 4.3 displays the descriptive statistics for these variables and they are described in more

detail below.

*Child outcomes.* This study estimated the effect of maternal employment on three

measures of cognitive and socio-emotional development. Children's school performance was

measured using parent or teacher reports, or test scores, with some studies including multiple

sources per child. The data was collected two to four years after random assignment, depending

on the study, and child age at the time of the assessment ranged from two to seven years old. All

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[22] In some cases, there are multiple children per family in the dataset. In analysis of children's achievement, there may be multiple observations per child because this outcome was measured from several sources.

Table 4.2  Case Counts by Study/Site/Program and Treatment Status

| Study/site/program | Total | Treatment group | Control group |
|---|---|---|---|
| CT Jobs First | 800 | 392 | 408 |
| FTP | 1,008 | 474 | 534 |
| New Hope | 602 | 300 | 302 |
| MFIP | | | |
|     Full program | 467 | 249 | 218 |
|     Incentives only | 347 | 183 | 164 |
| NEWWS[a] | | | |
|     Atlanta LFA | 570 | 244 | 326 |
|     Atlanta HCD | 661 | 335 | 326 |
|     Grand Rapids LFA | 358 | 174 | 184 |
|     Grand Rapids HCD | 344 | 160 | 184 |
|     Riverside LFA | 570 | 180 | 390 |
|     Riverside HCD | 244 | 244 | 254 |
| Total[b] | 5,297 | 2,935 | 2,362 |

Notes.
In studies that tested two programs (MFIP and NEWWS) there is generally a single control group to which both treatment groups are compared.  There are two exceptions.  At the NEWWS Riverside site, assignment to the HCD program required being designated as "in need" of basic education.  In this case, the comparison is made with the control group narrowed to those "in need" of basic education.  Also, the MFIP Full program was implemented in more sites than the MFIP Incentives-Only program, making the control group larger.
[a]LFA = labor force attachment; HCD = human capital development
[b]1st and 3rd columns do not sum to the total because control group members would be double-counted.

studies included parent reports of children's achievement on a single-item 5-point rating of how well the child was doing in school. Teacher reports of achievement (collected in three of the studies) were based on items from the Academic Subscale of the Social Skills Rating System (Gresham & Elliot, 1990). On this 10-item measure, the teacher compares the child's performance with that of other students in the same classroom on reading skill, math skill, intellectual functioning, motivation, oral communication, classroom behavior, and parental encouragement (internal consistency alpha = .94). The New Hope 5-year follow-up study also collected test scores from the Peabody Picture Vocabulary Test; a math skills test containing a subset of items from the Canadian Achievement Tests, Second Edition (CAT/2); and the Math and Reading scores from the Woodcock Johnson (WJ) tests of achievement. All are well-

validated, reliable tests of children's cognitive performance.

Children's socioemotional development was measured with parent reports of behavior. Parents reported children's problem behavior at follow-up using the Behavior Problems Index (a 28-item scale; Peterson & Zill, 1986). Both a total score ($\alpha$=.69-.92 depending on the study) and internally reliable subscales of internalizing ($\alpha$=.61-.82 depending on the study) and externalizing ($\alpha$=.81-.92 depending on the study) behavior were created. Mothers also completed either the full Positive Behavior Scale, a 25-item scale measuring socially approved behavior (Polit, 1996; $\alpha$=.90-.95 depending on the study), or an abbreviated version of seven items. Items on this scale assess the extent to which the child is helpful, thoughtful, compliant to adults, independent, and successful in interactions with peers.

To provide comparability in child outcomes across measures and studies, I standardized achievement and behavioral outcomes using study-specific, control-group standard deviations. This approach is comparable to "effect size" calculations commonly used in power analyses and the interpretation of program impacts (Cohen & Bianchi, 1999). Standardized outcomes were obtained with the following formula:

$$\widetilde{Y}_{ip} = \frac{Y_{ip} - \overline{Y}_p}{s_{ypc}}$$

where $\widetilde{Y}_{ip}$ is the standardized observation of outcome Y for child $i$ in study $p$, $Y_{ip}$ is the unstandardized observation of outcome Y for child $i$ in study $p$, $\overline{Y}_p$ is the full sample mean for measure Y in study $p$, and $s_{ypc}$ is the control-group standard deviation for the measure Y in study $p$. Control-group standard deviation are used because it is conceivable that the treatment affected the sample variance of outcome measures.

*Maternal employment status.* The independent variables of interest are measures of the level and stability of maternal employment based on Unemployment Insurance (UI) records of earnings over the follow-up period of the study. Depending on the study, length of follow-up varied from two to four years, or 8 to 16 quarters (Table 4.1). I constructed the employment variables relative to follow-up length to make them comparable across studies. Level of employment is measured as *fraction of quarters employed* during the follow-up, in which "employed" was defined as having any positive earnings in that quarter. Stability of employment is measured with two variables: *average length of employment spells* and *largest fraction of quarters employed consecutively*. I defined an employment spell as consecutive quarters of positive earnings and computed the largest fraction of quarters employed consecutively as the longest spell divided by the follow-up period. Unemployment is measured with the variable *average number of unemployment spells per year*, in which I defined an unemployment spell as one or more quarters without zero earnings.

There are several limitations to these measures. While administrative data on earnings are considered more accurate than self-reported earnings on surveys, there are some well-documented limitations to the coverage of UI records. They do not capture "off-the-books" work, legal or illegal, or self-employment. In addition, they only capture in-state earnings (citations). In addition, the three measures of stability are conditional on having been employed at some point during the follow-up period. When these measures are included in the regression models, the estimate is derived from a sub-sample of treatment and control group children whose mothers had positive earnings in at least one quarter. These variables are also censored on the right-side at the end of the follow-up period.

Table 4.3  Descriptive Statistics for Variables in Analysis

| | Mean | Standard Deviation | Minimum | Maximum | N |
|---|---|---|---|---|---|
| **Baseline characteristics** | | | | | |
| Age of child (years) | 2.98 | 0.83 | 0.1 | 3.99 | 5297 |
| Mother's number of children | 2.25 | 1.19 | 1 | 8 | 5259 |
| Age of mother's youngest child (years) | 2.42 | 1.21 | 0 | 18 | 5297 |
| Mother's race | | | | | |
| White | .32 | – | 0 | 1 | 5255 |
| Black | .51 | – | 0 | 1 | 5255 |
| Latino | .14 | – | 0 | 1 | 5255 |
| Other | .03 | – | 0 | 1 | 5255 |
| Mother's marital status | | | | | |
| Married | .02 | – | 0 | 1 | 5269 |
| Separated | .32 | – | 0 | 1 | 5269 |
| Never married | .66 | – | 0 | 1 | 5269 |
| Mother has a high school diploma | .59 | – | 0 | 1 | 5264 |
| Yearly income in the year prior to random assignment ($) | 1,893 | 4,568 | 0 | 60,000 | 5259 |
| Employed in year prior to random assignment | .43 | – | 0 | 1 | 5297 |
| Length of AFDC receipt prior to random assignment (years) | | | | | |
| 1-2 | .10 | – | 0 | 1 | 5245 |
| 3-4 | .19 | – | 0 | 1 | 5245 |
| 5 or more | .71 | – | 0 | 1 | 5245 |
| **Study characteristics** | | | | | |
| Follow-up length (years) | 3.57 | 1.18 | 1.8 | 6.8 | 5293 |
| **Follow-up measures of economic well-being** | | | | | |
| Fraction of quarters employed | .46 | 0.35 | 0 | 1 | 5297 |
| Average duration of employment spell (quarters) | 5.40 | 4.6 | 1 | 20 | 4296 |
| Largest fraction of quarters consecutively employed | .46 | 0.31 | 0.05 | 1 | 4296 |
| Average number of job losses per year | .49 | 0.31 | 0 | 2 | 5297 |
| Average yearly income ($) | 11,111 | 5,615 | 0 | 77,627 | 5297 |
| Fraction of quarters on welfare | .64 | 0.34 | 0 | 1 | 5297 |

Notes.

Child outcomes are not shown because the measures vary by study and have been standardized with mean=0 and standard deviation=1 for the purposes of this analysis.

T-tests of the difference in means by treatment/control status show few significant differences between the two groups. Latinos made up a larger percentage, and participants of other ethnicities a smaller percentage, of the control group, but the differences were small.  Control group members were also more likely to be working in the year prior to random assignment (difference=.03; p<.05). The child's mean age was one-tenth of a year higher in the treatment group (p<.01).

*Family income & welfare receipt.* In addition to maternal employment, the welfare

programs included in this analysis affected two other key aspects of family economic well-being,

family income and welfare receipt. I created variables to measure these outcomes based on state

administrative records of cash assistance, food stamp, and earnings supplement receipt, as well

as quarterly earnings data from UI records. For each quarter in the follow-up period, I calculated

quarterly parent income by summing all four sources. This income measure omits certain aspects

of family income, including public transfers from programs other than AFDC/TANF (e.g. SSI),

private transfers, and earnings from family members other than the sample member. All income

amounts were adjusted to 2001 dollars using the Consumer Price Index. For the purposes of this

analysis, I calculated average annual income and fraction of quarters of welfare receipt in the

years of follow-up.

*Control variables.* The models also include the following baseline parent and family

characteristics: years on AFDC (0-2, 2-5, 5 or more), family earnings in the year prior to random

assignment (in $1000 units), family earnings in the year prior to random assignment squared,

whether mother was employed in the year before random assignment (0 or 1), whether mother

has a high school diploma (0 or 1), mother's marital status (never married, married, separated),

number of children in the family, age of youngest child in the family, and mother's race (Black,

white, Latino, other). Study-level controls include dichotomous variables for each study (and for

sites in the case of two studies that were implemented in diverse regions) and a continuous

measure of follow-up length.

*Analysis Strategy*

Equation (1) shows the standard OLS regression predicting the effect of maternal employment on child outcomes, net of family and child characteristics.

(1) $Y_i = \beta_1^{OLS} E_i + \sum \gamma_k X_{ki} + \varepsilon_i$

where $Y_i$ is the outcome (e.g. achievement or problem behavior) of child i; $E_i$ is the labor force participation (measured in various ways) of child i's mother; and $X_{ki}$ is a vector of control variables (n=k) for family, parent, and child characteristics believed to be related to the development of child i.

The primary threat to the internal validity of model (1) is that employment is not randomly assigned; the decision to work is correlated with many family, parent, and child characteristics that are also correlated with children's cognitive and behavioral outcomes. OLS assumptions dictate that if all of these correlates are not included in the model, and consequently the independent variable of interest is correlated with the error term, than the estimate of $\beta_1^{OLS}$ will be biased. This problem is commonly referred to as omitted variable or unobserved heterogeneity bias.

In this study, in addition to estimating OLS regressions, I use a common econometric technique for addressing omitted variable bias, the instrumental-variable estimator. The logic of IV models is to identify a variable, called an "instrument," that is correlated with the problem variable (in this case, maternal employment) but uncorrelated with the error term. The problem variable is regressed on the instrument and the resulting fitted values are included in a model predicting the outcome of interest rather than actual values. The result is an estimate of the effect

of the problem variable based only on the exogenous variation in that variable associated with the instrument.

Taking advantage of the experimental design of these welfare studies, as well as their differential effects on maternal employment, I estimate the effects of maternal employment on child well-being using only the exogenous variation in maternal employment engendered by the programs. The exogenous variation in maternal employment provided by random assignment comes not just from the five different studies, but also from several of those studies testing multiple experiments in multiple sites.

This strategy estimates an equivalent of model (1) in two-stages. First, maternal employment is modeled as a function of assignment to the treatment group and a set of control variables. Next, child outcomes are estimated using the predicted values of employment the first stage equation. The models are as follows:

$$(2) \; E_i^* = \sum \gamma_k X_{ki} + \sum \chi_r T_{ri} + \sum \delta_n S_{ni} + \eta_i$$

$$(3) \; Y_i = \beta_1^{IV} E_i^* + \sum \gamma_k X_{ki} + \sum \delta_n S_{ni} + \varepsilon_i$$

In which $T_{ri}$ is vector of r indicator variables equal to one if the child's mother was assigned to the treatment group in a specific study/site/program. The 11 site-specific treatment dummies are CT Jobs First, FTP, and New Hope, MFIP Full, MFIP Incentives-Only, and LFA and HCD programs in the three NEWWS sites. $S_{ni}$ is a vector of four of the five dichotomous variables indicating membership in the a given study/site: CT Jobs First, FTP, New Hope, MFIP, NEWWS Atlanta, NEWWS Grand Rapids, and NEWWS Riverside. The same set of covariates $X_{ki}$ is included in both stages. In all models, heteroskedasticity-robust standard errors were used

to account for non-independence between observations of children within the same family and between multiple observations of the same child (White, 1980).

Given certain assumptions, equation (2) expunges the aspects of maternal employment related to the error term, leaving an exogenous measure to include in equation (3). The cost of specifying the model this way is that it inevitably produces larger standard errors than a simple OLS model (Murray, 2006). In addition, it in no way guarantees less bias in the IV estimates than was present in the OLS estimates (Angrist & Krueger, 2001). The extent to which IV estimates are biased depends largely on two conditions. First, the covariance of the instrument (random assignment to the treatment group) and the independent variable it is representing (maternal employment) must be greater than zero. The so-called "strength" of the instrument is measured by the size of the covariance between these two variables. A weak instrument can produce severely biased coefficient estimates, particularly for finite samples, as well as inaccurately small standard errors, which lead to type 1 errors (Murray, 2006).

In addition, the validity of the IV estimator depends on it being uncorrelated with the error term. This is referred to as the "exclusion restriction" and, unlike the first assumption, it cannot be observed empirically. Using random assignment as an instrument goes a lot way to meeting this condition. If implemented properly, random assignment ensures that there are no differences between the average baseline demographic characteristics of treatment and control group members. A factor such as spousal earnings, which could plausibly affect both maternal employment and child development, is effectively controlled for with the IV models, *provided that the programs themselves did not affect spousal earnings*. The two assumptions are related in

that the stronger the relationship between the instrument and the instrumented variable, the less sensitive the model is to violations of the exclusion restriction (Angrist, Imbens, & Rubin, 1996).

In this study, the exclusion restriction is convincingly met for many potential confounding factors, including child characteristics and parental well-being, which the welfare programs did not directly affect. However, there are several other mechanisms through which these programs affected families and children, each of which are correlated with employment, namely changes in welfare receipt, family income and child care. In addition to increasing employment, all of the programs were also designed to decrease welfare receipt. Several of the programs offered earnings supplements to treatment group members in order to boost family income and three of the programs subsidized child care for participants' children.

I tested the sensitivity of the IV estimates to the exclusion restriction in two ways. First, I estimated the effect of the fraction of quarters employed on child outcomes using an IV model similar to (3), in which multiple endogenous variables were instrumented by random assignment. The other variables I instrumented were average yearly income and average quarters receiving welfare over the follow-up period. Ideally, I would have also included child care in this model, but I did not have sufficiently consistent data across the studies to do so.

An alternative model specification addressed the potential for other pathways, including child care, somewhat less directly. In this case, I estimated the original IV models, with a single endogenous variable measuring either amount or consistency of employment, using only the six programs in the National Evaluation of Welfare to Work Strategies (NEWWS). There is sufficient variation in program impacts on maternal employment across the NEWWS sites and programs to estimate this model. In addition, NEWWS is a program that mandated employment

or basic education, but did not provided income supports or child care assistance. The components of NEWWS make the exclusion restriction more tenable, although the NEWWS program design introduces educational attainment as another pathway to program impacts.

The cost to both sensitivity tests—the multiple-instrumented variables model and the NEWWS-only model—is in the precision of the estimates. In the first case, the substitution of fitted values for actual values in multiple independent variables increases the standard errors for the estimates. In the second case, it is largely the decrease in sample size that leads to a similar problem. Nonetheless, these alternative specifications help examine the assumptions behind the IV estimates.

*Results*

*Program Impacts on Families and Children*

All five welfare programs included in this analysis mandated employment among participants. Yet, their effects on the level and stability of maternal employment among women with young children varied substantially. Table 4.4 displays differences in means for treatment and control group members with children 0-3 years of age on four measures of employment, as well as average yearly income and average quarters with welfare receipt. The program impacts are shown for the 11 programs that will be used as instruments in the second stage models. Each cell in the table presents results from a separate regression in which the outcome measure was regressed on a dummy for treatment/control group membership and a set of baseline covariates, included to increase the precision of the estimates.

Most of the programs (8 of 11) increased employment, measured as fraction of quarters employed during the follow-up period, although the magnitude of the effects varied from five

percent (marginally significant) to 16 percent. None of the programs had statistically significant negative impacts on amount of employment. These results are consistent with estimates of individual study impacts published in prior reports (D. Bloom et al., 2000; D. Bloom et al., 2002; Bos et al., 1999; Gennetian & Miller, 2000; Hamilton et al., 2001; McGroder, Zaslow, Moore, & LeMenestrel, 2000; Morris & Michalopoulos, 2000).

The three other employment measures capture the stability of employment over the follow-up period. They measure average duration of employment spells, the largest fraction of quarters consecutively employed (a measure of the longest employment spell relative to follow-up period length), and the average number of job losses per year of follow-up. The size and significance of program impacts on these three measures vary by study, as does the consistency of impacts across the three measures. The most consistent picture is from New Hope, which increased the average duration of employment spells by 1.67 quarters, increased the largest fraction of quarters employed by 12 percent, and decreased the number of job losses per year by a little over a tenth of spell. All of these impacts are statistically significant at the $p<.01$ level. No programs had significant negative impacts on the duration of employment spells, but MFIP Full did increase slightly the yearly number of employment spells (.069; $p<.05$).

The six NEWWS programs provide an interesting contrast. The LFA programs were designed to move participants into employment immediately, while the HCD programs emphasized providing basic education to participants so that they would ultimately find higher quality job. At the Atlanta and Grand Rapids sites, this difference in approach produces consistent differences in program impacts on employment. Namely, the LFA programs increased employment while the HCD programs did not. In Riverside, both programs produced relatively

large increases in employment among treatment group members, but only assignment to the HCD programs was associated with improvements in the stability of employment. The Riverside HCD program increased the duration of employment spells and had no effect on number of unemployment spells, while the LFA program increased the number of unemployment spells and had few effects on length of employment spells.

In addition to affecting the level and stability of employment among treatment group members, certain programs also increased family income. Of the four programs that offered generous earnings supplements—CT Jobs First, MFIP Full, MFIP Incentives-Only, and New Hope—just two were associated with increases in income among treatment group members. New Hope and MFIP Incentives-Only increased average yearly income by approximately $1500 and $1300, respectively. In addition, Riverside HCD, a program that did not provide financial incentives, increased the income of participants by over $1000 per year. While the mechanism for this impact in the Riverside HCD program can not be tested directly, it is plausible that they are related to program impacts on employment stability or to the programmatic approach of helping participants find higher-quality jobs.

Table 4.5 is similar to Table 4.4 except that the outcomes are related to child development rather than family economic circumstances. Most striking in these results is that the programs produced almost no statistically significant differences between treatment and control group children ages 0 to 3 in either the socio-emotional or cognitive realms. The one exception is NEWWS Grand Rapids HCD, which decreased the school achievement of children in the treatment group by more than one-quarter of a standard deviation.

Table 4.4 Adjusted Treatment-Control Differences (Program Impacts) on Measures of Maternal Employment, Income, and Welfare Receipt by Study/Site/Program, for Families with Children 0-3 Years Old at the Time of Random Assignment

| Study/site/program | Maternal employment | | | | Average yearly income ($1000) | Average quarters with welfare receipt |
|---|---|---|---|---|---|---|
| | Fraction of quarters employed | Average duration of employment spell (quarters) | Largest fraction of quarters consecutively employed | Average number of job losses per year | | |
| CT Jobs First | .058** | .738* | .056* | -.004 | .533 | .004 |
| | (.022) | (.293) | (.023) | (.022) | (.361) | (.021) |
| | [770] | [662] | [662] | [770] | [770] | [770] |
| FTP | .085** | 1.39** | .089** | -.011 | .404 | -.070** |
| | (.018) | (.291) | (.018) | (.018) | (.276) | (.018) |
| | [962] | [866] | [866] | [962] | [962] | [962] |
| New Hope | .108** | 1.67** | .123** | -.121** | 1.49** | -.031 |
| | (.023) | (.433) | (.025) | (.023) | (.437) | (.023) |
| | [597] | [570] | [570] | [597] | [597] | [597] |
| MFIP | | | | | | |
| Full | .082** | .098 | .014 | .069* | .507 | .073** |
| | (.028) | (.368) | (.030) | (.028) | (.529) | (.028) |
| | [457] | [354] | [354] | [457] | [457] | [457] |
| Incentives only | .033 | -.034 | -.005 | .030 | 1.31* | .062+ |
| | (.030) | (.391) | (.032) | (.029) | (.567) | (.032) |
| | [397] | [314] | [314] | [397] | [397] | [397] |
| NEWWS[a] | | | | | | |
| Atlanta LFA | .050+ | .780+ | .055* | -.003 | .452 | -.067** |
| | (.026) | (.408) | (.027) | (.022) | (.340) | (.025) |
| | [564] | [421] | [421] | [564] | [564] | [564] |
| Atlanta HCD | .023 | .295 | .009 | .010 | .023 | -.021 |
| | (.023) | (.366) | (.026) | (.002) | (.272) | (.022) |
| | [648] | [483] | [483] | [648] | [648] | [648] |
| Grand Rapids LFA | .133** | 1.26** | .092** | -.036 | .340 | -.101** |
| | (.030) | (.426) | (.030) | (.034) | (.432) | (.033) |
| | [658] | [317] | [317] | [358] | [358] | [358] |
| Grand Rapids HCD | .006 | .222 | .002 | .009 | -.306 | -.043 |
| | (.030) | (.389) | (.028) | (.037) | (.454) | (.031) |
| | [340] | [283] | [283] | [340] | [340] | [340] |
| Riverside LFA | .164** | .500 | .070* | .078** | .092 | -.103** |
| | (.028) | (.475) | (.032) | (.026) | (.481) | (.030) |
| | [570] | [325] | [325] | [570] | [570] | [570] |
| Riverside HCD | .122** | 1.48** | .103** | .013 | 1.174** | .010 |
| | (.023) | (.482) | (.033) | (.021) | (.342) | (.026) |
| | [495] | [252] | [252] | [495] | [492] | [495] |

Notes.

Standard errors in parentheses; sample sizes for each regression in brackets.

Program impacts on each outcome measure were estimated in separate regressions that included the following control variables: follow-up length; time on afdc prior to random assignment; earnings in the year prior to random assignment; employment in the year prior to unemployment; having a high school diploma; marital status; number of children; whether child is youngest in family; and mother's race. The regressions did not control for the other outcomes measures.

+p<.10; *p<.05; **p<.01 indicating statistical significance of treatment-control differences.

[a]LFA = labor force attachment; HCD = human capital development

Given the consistency of the signs for each child outcome across the different programs, Table 4.5. suggests that these seven programs—CT Jobs First, New Hope, MFIP Full, MFIP Incentives-Only, NEWWS Atlanta LFA, Grand Rapids LFA, and Riverside HCD—may have had generally positive effects on children, by decreasing problem behavior while increasing positive social behavior and school achievement.

The precision of the IV estimates depends on the extent to which there are patterns that relate program impacts on the employment measures to program impacts on child outcomes. Tables 4.4 and 4.5 provide this information, but it is easier to visualize with figures plotting these relationships. Figures 4.1a-d, 4.2a-d, and 4.3a-d display the relationships between program impacts on each measure of maternal employment and program impacts on child outcomes. Impacts on child problem behavior are presented in Figures 4.1a-d, positive social behavior in Figures 4.2a-d, and school achievement in Figures 4.3a-d.

As can be seen from these figures, there is little evidence of a pattern of program effects on maternal employment and children's behavior, regardless of the employment measure or whether the outcome is problem behavior or positive social behavior. The patterns look somewhat more promising for program impacts on achievement (Figures 4.3a-d). The direction of these relationship suggest a positive relationship between maternal employment, both level and stability) and child school achievement, and a corresponding negative relationships between maternal unemployment and child school achievement. Such findings are consistent with existing evidence that early maternal employment is more relevant for children's cognitive development than for socio-emotional development (Harvey, 1999; Waldfogel, 2000).

Table 4.5 Adjusted Treatment-Control Differences (Program Impacts) on Measures of Child Well-Being by Study/Site/Program, for Children 0-3 Years Old at the Time of Random Assignment

| | Child Outcomes | | |
|---|---|---|---|
| Study/site/program | Problem Behavior | Positive Social Behavior | Achievement |
| CT | -.021 | .022 | .066 |
| | (.087) | (.097) | (.068) |
| | [384] | [386] | [883] |
| FTP | .034 | -.142 | .048 |
| | (.084) | (.086) | (.066) |
| | [530] | [528] | [914] |
| New Hope | -.059 | .081 | .061 |
| | (.081) | (.084) | (.081) |
| | [542] | [553] | [1312] |
| MFIP Full program | -.135 | .061 | .009 |
| | (.139) | (.146) | (.086) |
| | [222] | [222] | [456] |
| MFIP Incentives-Only program | -.124 | 0.14 | .050 |
| | (.164) | (.149) | (.091) |
| | [194] | [194] | [395] |
| NEWWS[a] | | | |
|   Atlanta LFA | -.073 | .049 | .022 |
| | (.087) | (.095) | (.097) |
| | [512] | [518] | [888] |
|   Atlanta HCD | -.099 | -.007 | .074 |
| | (.076) | (.086) | (.089) |
| | [590] | [598] | [1012] |
|   Grand Rapids LFA | -.107 | .112 | .105 |
| | (.117) | (.102) | (.112) |
| | [335] | [340] | [631] |
|   Grand Rapids HCD | .107 | -.028 | -.255* |
| | (.135) | (.108) | (.129) |
| | [321] | [324] | [592] |
|   Riverside LFA | .018 | .014 | .006 |
| | (.089) | (.090) | (.108) |
| | [536] | [539] | [931] |
|   Riverside HCD | -.011 | .067 | .024 |
| | (.084) | (.092) | (.073) |
| | [455] | [458] | [799] |

Notes.

Standard errors in parentheses; sample sizes for each regression in brackets.

Program impacts on each outcome measure were estimated in separate regressions that included the following control variables: follow-up length; time on afdc prior to random assignment; earnings in the year prior to random assignment; employment in the year prior to unemployment; having a high school diploma; marital status; number of children; whether child is youngest in family; and mother's race. The regressions did not control for the other outcomes measures.

+p<.10; *p<.05; **p<.01 indicating statistical significance of treatment-control differences.

[a]LFA = labor force attachment; HCD = human capital development

In contrast to these graphs suggesting only weak association between programs that affected maternal employment and programs that affected child development, evidence of a relationship between family income and child outcomes is much stronger (this too is consistent with previous studies). Figures 4.4a-c display the relationships between program impacts on average yearly income and children's problem behavior, positive social behavior, and achievement. There are positive and relatively strong relationships in all three cases. Programs that increased family income also decreased children's problem behavior and increased both positive social behavior and school achievement.

*OLS and IV Regression Results*

Table 4.6 reports coefficients, standard errors, and sample sizes from separate OLS and IV models regressions of each measure of child development on each measure of employment. In all, there are a total of 24 regressions presented. All models included a set of family-level baseline characteristics as well as binary variables identifying programs and sites.
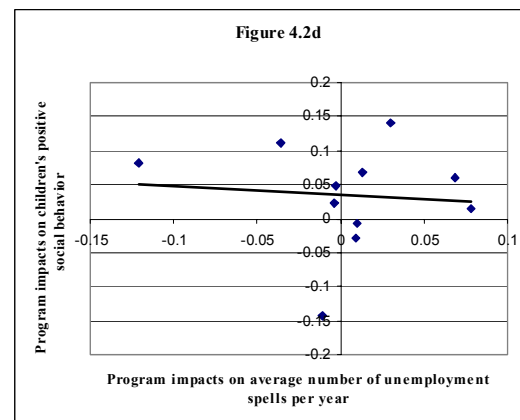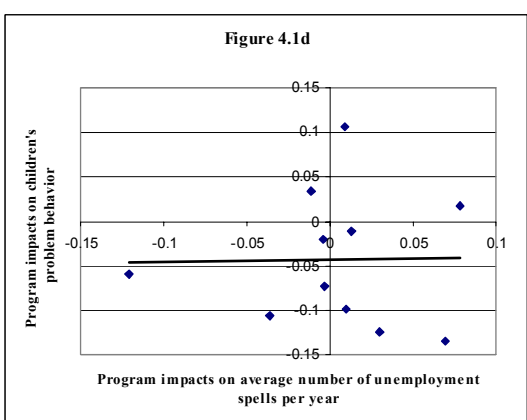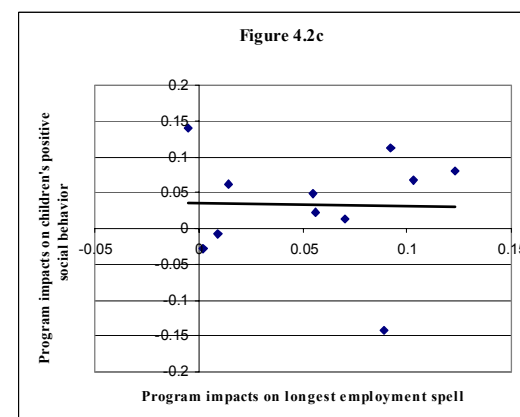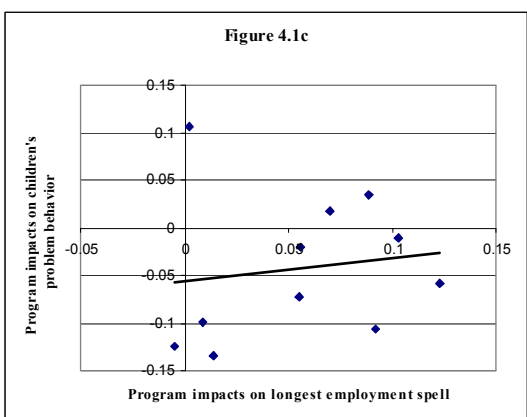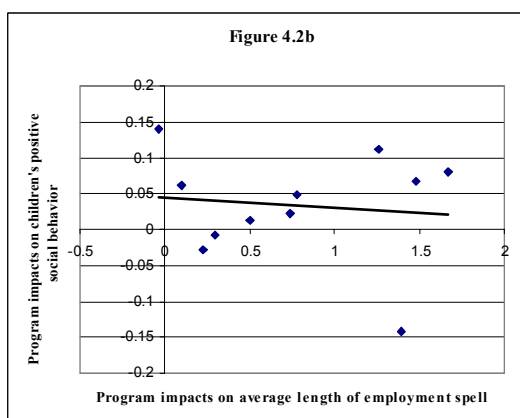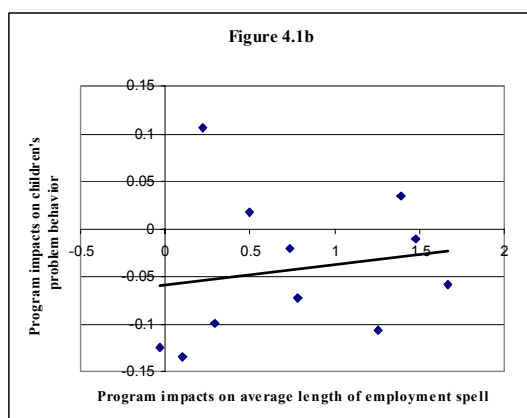
The OLS results[23] suggest that employment has positive effects on preschool-aged children's behavior. Both the amount and stability of employment predict lower problem behavior scores. The coefficients on fraction of quarters employed and largest fraction of quarters consecutively employed are relatively large, approximately .20 of a standard deviation on the problem behavior score, and significant at the p<.01 level. The middle columns of Table 4.6 indicate there are also two marginally significant coefficients linking maternal employment

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[23] These models are not dissimilar to those estimated in other studies of maternal employment and children's development, although the sample is restricted to low-income women, most of whom were receiving welfare before they entered these programs. This adds concerns about generalizability to the already substantial threats to internal validity posed by omitted variables in these models.

to increased positive behavior among children. Again, these suggest positive effects on children from both increased employment and increased stability of employment among mothers. The effect sizes are smaller, however, just .09 and .12 of a standard deviation, respectively.

The one marginally significant effect of maternal employment on child achievement reported in Table 4.6 has intuitive validity. An increase in the number of spells of unemployment is associated with a decrease in children's achievement of about one-tenth of a standard deviation. Overall, the OLS results suggest that maternal employment among low-income single mothers is associated with small positive effects on children ages 0 to 3 years, specifically on reducing the children's problem behavior. The extent to which these estimates are biased by omitted variables (or measurement error or simultaneity) cannot be known with accuracy, but it is presumably large given the long list of potential confounding factors.

Columns 3, 5, and 7 in Table 4.6 show the results of models in which random assignment to the welfare programs was used as an instrument for each measure of maternal employment. When looking at the size and direction of the coefficients, the IV estimates consistently are in the same direction and nearly always larger than the OLS estimates. For instance, the size of the effect of fraction of quarters employed on children's problem behavior doubles in size to .40 standard deviations. Despite these larger coefficients, the IV estimates often are too imprecisely estimated to be statistically significant. The standard errors of the IV coefficients are consistently 10 times larger than the standard errors of the OLS estimates, and commonly equal in size to the standard deviation of the instrumented variable.

**Figure 4.1a**



Impacts on children's problem behavior vs. Program impacts on fraction of quarters employed

**Figure 4.2a**



Program impacts on children's positive social behavior vs. Program impacts on fraction of quarters employed

**Figure 4.1b**



Program impacts on children's problem behavior vs. Program impacts on average length of employment spell

**Figure 4.2b**



Program impacts on children's positive social behavior vs. Program impacts on average length of employment spell

**Figure 4.1c**



Program impacts on children's problem behavior vs. Program impacts on longest employment spell

**Figure 4.2c**



Program impacts on children's positive social behavior vs. Program impacts on longest employment spell

**Figure 4.1d**



Program impacts on children's problem behavior vs. Program impacts on average number of unemployment spells per year

**Figure 4.2d**



Program impacts on children's positive social behavior vs. Program impacts on average number of unemployment spells per year

123



**Figure 4.3a**

**Figure 4.3c**

**Figure 4.3b**

**Figure 4.3d**

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**Figure 4.4a**

Program impacts on children's problem behavior

Program impacts on average yearly income

**Figure 4.4c**

Program impacts on children's school achievement

Program impacts on average yearly income

**Figure 4.4b**

Program impacts on children's positive social behavior

Program impacts on average yearly income

Table 4.6 OLS and IV Regression Estimates of Maternal Employment Effects on Measures of Child Well-Being, for Children 0-3 Years of Age

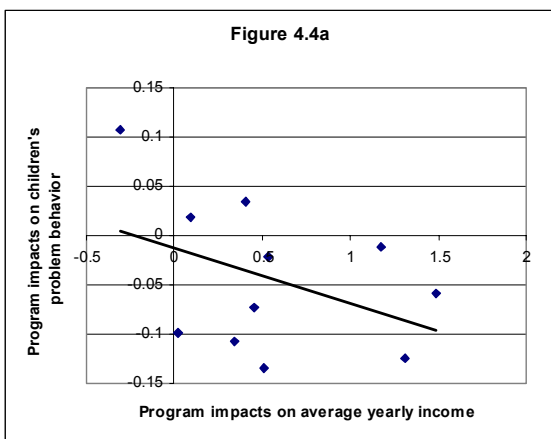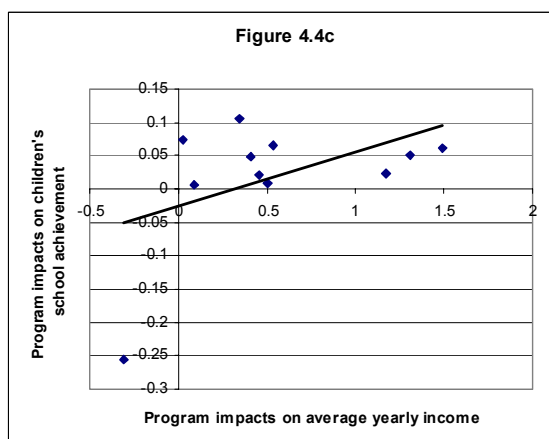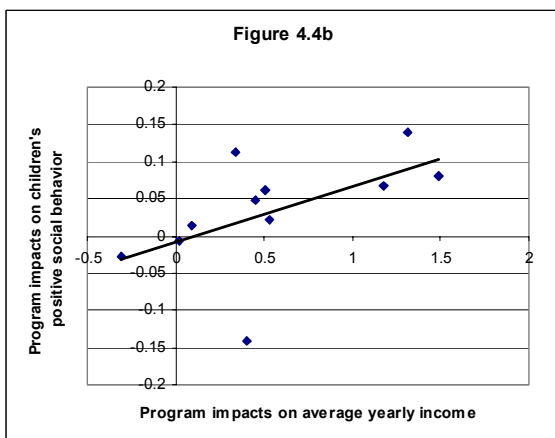| | Problem Behavior | | Positive Social Behavior | | Achievement | |
|---|---|---|---|---|---|---|
| | OLS | IV | OLS | IV | OLS | IV |
| **Model 1** | | | | | | |
| Fraction of quarters employed | -.208** | -.399 | .094+ | .355 | .065 | .836* |
| | (.055) | (.381) | (.057) | (.389) | (.053) | (.361) |
| | [3944] | [3944] | [3971] | [3971] | [4108] | [4108] |
| **Model 2** | | | | | | |
| Average length of employment spell | -.014** | -.031 | .006 | .023 | .004 | .079+ |
| | (.004) | (.035) | (.004) | (.038) | (.005) | (.045) |
| | [3202] | [3202] | [3221] | [3221] | [3264] | [3264] |
| **Model 3** | | | | | | |
| Largest fraction of quarters consecutively employed | -.212** | -.478 | .119+ | .505 | .064 | 1.105* |
| | (.060) | (.511) | (.064) | (.547) | (.063) | (.555) |
| | [3202 | [3202] | [3221] | [3221] | [3264] | [3264] |
| **Model 4** | | | | | | |
| Number of unemployment spells per year | .063 | .473 | -.046 | -.257 | -.105+ | -.389 |
| | (.056) | (.601) | (.057) | (.617) | (.053) | (.532) |
| | [3944] | [3944] | [3971] | [3971] | [4108] | [4108] |

Notes.

Standard errors in parentheses; sample sizes for each regression in brackets.

Control variables include: study/site; follow-up length; time on afdc prior to random assignment; earnings in the year prior to random assignment; employment in the year prior to unemployment; having a high school diploma; marital status; number of children; age of mother's youngest child; and mother's race.

$+p<.10$; $*p<.05$; $**p<.01$

The standard errors of IV estimates of employment effects on child achievement increase exponentially as well, but here the coefficients are large enough to become significant (whereas they were not in the OLS models). The final column of Table 4.6 suggests that a .01 increase in the fraction of quarters a mother is employed is associated with a .85 standard deviation increase in child achievement. The effect of a .01 increase in largest fraction of quarters consecutively employed is even larger—more than a standard deviation increase. These are unrealistically large coefficients.

A key assumption underlying the IV results in Table 4.6 is that maternal employment was the only pathway through which these programs could have affected children's development.

There is value in considering this hypothetically, but realistically we know that the programs operated through several other pathways, namely family income, welfare receipt, and child care. These studies do not contain information about child care use that would be appropriate to include in these models. Table 4.7, however, presents OLS and IV estimates from models that included measures of amount of employment (fraction of quarters, family income (average yearly), and welfare receipt (fraction of quarters). In the OLS models that control for income and welfare receipt, the amount, but not stability, of employment is still negatively related to problem behavior (associated with a decrease). Nearly all of the coefficients on income and welfare receipt are small and insignificant, although a larger fraction of quarters on welfare is associated with a .13 standard deviation decrease in children's achievement. As expected, the IV estimates with instrumented employment, income, and welfare receipt are even less precise than those shown in Table 4.6. The few significant IV estimates of employment effects on child achievement become insignificant in this model specification. While this approach is more plausibly meeting the exclusion restriction than my main models, the imprecision of the estimates makes the results largely uninformative.

A final approach I took to addressing selection bias was to estimate my original models using on the NEWWS evaluation. Three NEWWS sites, each with two different programs, are included in this analysis and there is substantial variation between the sites on measures of maternal employment, both level and stability (Table 4.4). These six programs offer a "cleaner" instrument in some ways because mandated work was the key component of NEWWS. The programs did not offer earnings supplement or other incentives that were likely to increase family income and they did offer expanded child care assistance. The argument for maternal

employment being the primary pathways through which the NEWWS programs affected children is much stronger than for the full set of programs.[24]

Table 4.8 presents OLS and IV results from the main set of models estimated with six NEWWS programs. The OLS results on behavior are similar to the multiple-endogenous variable models shown in Table 4.7. That is, fraction of quarters employed is still associated with decreases in problem behavior. In this case, the coefficient is one-quarter of standard deviation, which is larger than the equivalent coefficient in either the main models or the multiple-endogenous variable model. There are no other statistically significant OLS coefficients on problem or positive social behavior. In contrast, the OLS models using only the NEWWS sites estimate significant effects of all four measures of employment on child achievement. The largest (but marginally significant) coefficient is on the largest fraction of quarters consecutively employed, also about one-quarter of a standard deviation. As with the previous models, the IV coefficients are all insignificant, with standard errors as much as 10 times as large as the OLS estimates.

Existing evidence strongly supports the idea that the effects of maternal employment differ by child age. I examined age differences in the present study by estimating the OLS and IV models separately for three child age groups: 1-year-olds, 2-year-olds, and 3-year-olds (results not shown). The most consistently estimated effects of maternal employment are among children under 12 months of age. Unfortunately, I had too few children of this age to run the IV models. Samples sizes of the other age groups were sufficiently large to run both OLS and IV

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[24] Restricting the analysis to NEWWS does reduce the sample size considerably. In addition, NEWWS did not measure child outcome for children under two years of age, so the results presented in Table 5.8 are restricted to children who were two and three years old at the time of random assignment.

analyses, but were still relatively small and not always representative of all five studies in the analysis. As a consequence, the IV estimates by child age are even less precise than they are in the main models.

Table 4.7 OLS and IV Regression Estimates of Employment, Income, and Welfare Receipt on Measures of Child Well-Being, for Children 0-3 Years of Age

| | Problem Behavior | | Positive Social Behavior | | Achievement | |
|---|---|---|---|---|---|---|
| | OLS | IV | OLS | IV | OLS | IV |
| Fraction of Quarters Employed | -.164* | -.547 | .067 | .641 | .013 | .592 |
| | (.073) | (.586) | (.076) | (.584) | (.066) | (.621) |
| Average Yearly Income | -.002 | -.006 | .002 | -.009 | .002 | .049 |
| | (.004) | (.057) | (.004) | (.057) | (.004) | (.070) |
| Average Quarters of Welfare Receipt | .087 | -.381 | -.025 | .525 | -.134* | -.052 |
| | (.065) | (.607) | (.066) | (.598) | (.059) | (.668) |
| | [3944] | [3944] | [3971] | [3971] | [4108] | [4108] |

Notes.
Standard errors in parentheses; sample sizes for each regression in brackets.
Control variables include: study/site; follow-up length; time on afdc prior to random assignment; earnings in the year prior to random assignment; employment in the year prior to unemployment; having a high school diploma; marital status; number of children; age of mother's youngest child; and mother's race.
+p<.10; *p<.05; **p<.01

Despite these limitations, several findings emerge confirming the importance of child age as a moderator, but providing little insight into the nature of the employment by child age interaction. In the OLS models, the largest positive effects of level and stability of employment on child behavior (reductions in problem behavior) were for one-year-olds, the youngest children in the sample. Only one IV estimate of an effect on behavior was even marginally significant; that coefficient indicates that additional spells of unemployment during the follow-up period were related to increased problem behavior among one-year-olds. The direction and significance of this effect did not hold for older children. The IV estimates of effects on child achievement also suggest that child age moderates the effects of maternal employment, but in the opposite direction. In these models, the fraction of quarters employed and the average duration of

employment spells were both negatively associated with child school achievement among one-year-olds, while the same relationship was positive for two- and three-year-olds. Unfortunately, the imprecision of these estimates make it difficult to gauge their validity or relevance.

Table 4.8 OLS and IV Regression Estimates of Maternal Employment Effects on Measures of Child Well-Being, *Using Only the Six NEWWS Progams,* for Children 2-3 Years Old at Random Assignment

| | Problem Behavior | | Positive Social Behavior | | Achievement | |
|---|---|---|---|---|---|---|
| | OLS | IV | OLS | IV | OLS | IV |
| **Model 1** | | | | | | |
| Fraction of quarters employed | -.240** | -.088 | .107 | -.029 | .201* | .610 |
| | (.090) | (.515) | (.093) | (.479) | (.101) | (.540) |
| | [1204] | [1204] | [1202] | [1202] | [1260] | [1260] |
| **Model 2** | | | | | | |
| Average length of employment spell | -.015 | -.089 | .011 | .227 | .032+ | .246 |
| | (.015) | (.179) | (.016) | (.188) | (.017) | (.192) |
| | [763] | [763] | [761] | [761] | [793] | [763] |
| **Model 3** | | | | | | |
| Largest fraction of quarters consecutively employed | -.121 | -.666 | .107 | 1.708 | .244+ | 1.613 |
| | (.125) | (1.26) | (.129) | (1.281) | (.136) | (1.346) |
| | [763] | [763] | [761] | [761] | [793] | [763] |
| **Model 4** | | | | | | |
| Number of unemployment spells per year | -.039 | -.351 | -.019 | .444 | -.201* | .744 |
| | (.100) | (1.34) | (.090) | (1.269) | (.091) | (1.29) |
| | [1204] | [1204] | [1202] | [1202] | [1260] | [1260] |

Notes.

Standard errors in parentheses; sample sizes for each regression in brackets.

Control variables include: study/site; follow-up length; time on afdc prior to random assignment; earnings in the year prior to random assignment; employment in the year prior to unemployment; having a high school diploma; marital status; number of children; age of mother's youngest child; and mother's race.

+p<.10; *p<.05; **p<.01

## *Discussion*

The question asked in this study was how the level and stability of maternal employment affects the development of very young children, age 0 to 3 years, living in poor families. This is a well-researched topic, but most studies have been non-experimental and left questions about whether the estimates were biased by unobserved heterogeneity. The effects on low-income

children are particularly elusive, but there is some suggestion that they may be positive even at the youngest ages.

I estimated the effects of employment on young children using a sample of low-income families that participated in 11 different welfare programs. Assignment to the treatment group was a promising instrument for maternal employment because, if implemented properly, random assignment ensures there are no differences between the average demographic characteristics of treatment and control members at baseline. In addition, these were programs designed to increase maternal employment, but the specific programmatic approaches and the corresponding program impacts on employment differed substantially across the 11 programs.

OLS estimates suggest that both level and stability of employment are associated with lower levels of problem behavior, even while controlling for income and welfare receipt. Relative to the OLS estimates, the IV estimates are nearly always in the same direction and substantially larger. The IV models find no such relationship, but estimate significant and positive coefficients for both level and stability of employment on child achievement. However, when income and welfare receipt are included in the IV models, no coefficients on the employment measures remain significant. This is also true when I limit the analysis to a single evaluation in which employment is more clearly the main pathway for program impacts on children. Most importantly, the imprecision of the IV estimates makes if very difficult to interpret or draw conclusions from the results. The strength of the instrument, measured as the covariance between treatment assignment and the employment measures is reasonably good, but program impacts on employment is only weakly related to program impacts on children.

None of the effects on problem behavior remain significant, despite increasing in size, because the standard errors blow up to 10 times their size in the OLS models. It is interesting that in models with a single instrumented variable measuring either level or stability of employment, statistically significant effects are estimated on child achievement that were not found in the OLS models. A greater fraction of quarters employed at all or employed consecutively predict higher cognitive test scores. This is consistent with several studies finding positive relationships between maternal employment and *low-income children's* cognitive development.

There are several considerable limitations of this analysis that make it difficult to interpret or draw conclusions from these results. First, the IV estimator is only unbiased and consistent if it is uncorrelated with the error term. While random assignment convincingly controls for the many time invariant characteristics of mothers and families that might be correlated with both work decisions and child development, program participation had the potential to affect many aspects of family economic circumstances, including welfare receipt, income, and child care. In other words, it is difficult to argue that the exclusion restriction is being met for an instrument based on welfare programs. In addition, descriptive analysis of the patterns of program impacts on maternal employment vis-à-vis program impacts on child outcomes raised concerns that the relationship was too weak and would lead to imprecisely estimated IV coefficients.

I estimated two alternative models that more realistically complied with the exclusion restriction. In the first, I instrumented multiple pathways through which the welfare programs are known to have operated. I was unable to include an instrument for child care use because of

data limitations, but I estimated effect of fraction of quarters employed on all three measures of child development using random assignment as an instrument not only for employment, but also for income and welfare receipt. In the second, I restricted my analyses to the NEWWS programs, which did not offer earnings supplements or child care assistance, and therefore created a "cleaner" instrument.  In both cases, the standard errors of the IV estimates increase even further and there are no statistically significant coefficients on the measures of maternal employment, regardless of the child outcome. Whether the absence of significant effects is primarily due to a more realistic model specification that controls for income and welfare receipt or to the consequences of adding multiple endogenous variables for the precision of the estimates is difficult to say.

CHAPTER FIVE

CONCLUSION

Despite the considerable challenges to being both the primary financial provider and caretaker of an infant or toddler, single mothers with preschool-aged children are working at higher rates than ever before. Many of them do so in the context of poverty, low-wage employment, and absent or inconsistent support from the fathers of their children. In addition, single and married mothers are making work decisions in largely distinct policy environments, with contrasting incentives about employment in the early years of a child's life. For their part, single mothers are less likely to be covered by the FMLA, which guarantees workers job protection during a period of unpaid leave, than they are by welfare cash assistance and the Earned Income Tax Credit, which provide strong financial incentives for employment, often quite soon after a child is born.

What factors propel single mothers into employment in the years after the birth of a child? As important, what are the implications of this social phenomenon for young children? Two large bodies of literature explore these questions, but leave several gaps in our knowledge about maternal employment among single mothers with young children. Studies of the effects of maternal employment on young children have focused primarily on infants and toddlers, but have devoted less attention to the moderating effects of marital status, maternal education, or income on the relationship between maternal employment and child outcomes. In contrast, substantial research has examined the effects of welfare reform on the work outcomes of low-income single mothers and the consequences of maternal employment for families and children.

However, little is known about the effects of these programs on infants or toddlers, or about the effects of specific components of welfare reform policies targeted directly at parents of young children.

The goal of this dissertation was to conduct three studies that addressed these gaps with rigorous and diverse methodological approaches. Two of the studies examined maternal decision-making about employment after a birth, but with very different approaches and purposes. In one case, I estimated the effect of a specific welfare policy on employment rates of single mothers with no more than a high school diploma, controlling for all observed and unobserved characteristics of individuals, families, and their local economic and policy environments. Instead of isolating the effect of one factor, the second study attempted to capture maternal decisions about employment after the birth of a child more holistically. The final study considered how these decisions might relate to the development of infants and toddlers in single-parent families. I used a sample of mothers in experimental welfare programs to estimate how employment rates and the stability of employment affected their children's cognitive and behavioral development.

These studies used varied data sources, including a nationally-representative survey, a set of experimental welfare programs, and combined survey and in-depth interview data from a study of new parents. The analytic methods were also uniquely wide-ranging. Two studies estimated regression models using techniques for addressing unobserved heterogeneity, difference-in-difference and instrumental variable estimation. The other was both substantively and methodologically exploratory, in that I combined conventional quantitative and qualitative methods with Qualitative Comparative Analysis (QCA), a sociological method particularly well-

suited for theory-building, but not widely applied to studies of individual behavior or policy-relevant questions.

The two studies in this dissertation that examined the determinants of early maternal employment both speak to the importance of welfare policy rules in the lives of single mothers. In one case, I found a substantial effect of the shortening of age-of-youngest-child exemptions on employment rates among welfare-eligible mothers with young children. The size of this policy effect—eligibility for an exemption reduced the probability of working among welfare-eligible women with preschool-aged children by four percentage points—is comparable to previously estimated effects of substantial changes in the generosity of welfare benefits or child care subsidies. Qualitative evidence from the case studies in the second study indicate that TANF rules influence maternal work decisions, even if only as an option that is rejected by parents, but that this influence may vary qualitatively depending on both parents' work experience.

In the second study, I examined the individualized pathways that lead to maternal decisions about work, as well as several issues that cut across cases. Consistent with most studies of the timing of maternal employment, mother's employment prior to the birth was positively related with employment within the first year after the birth. Other important factors shaping maternal employment emerged from the QCA and qualitative analysis in this second chapter. For instance, there was evidence that fathers' employment may affect mothers' returns to work following a birth. Both theory and past empirical studies suggest that father's work and income should be negatively associated with mother's employment status and the timing of employment after a birth. In this study, however, father's work status in the week prior to the birth was positively and negatively associated with whether a mother returned to work within 12 months,

depending on the other conditions present in the mother's life. The qualitative case study descriptions helped explain this finding by uncovering significant instability in the employment status of the fathers in the sample. Instability is a feature of low-wage employment and of low-income families' lives more generally, but we know very little about how a partner's financial or job instability might influence mothers' decisions about work after a birth. In a regression model, I substituted a measure of the father having worked steadily in the year prior to the birth (26 or more weeks) rather than having worked in the week prior to the birth. The coefficient on this variable was negative, although insignificant.

Finally, in a study of the effects of maternal employment on young children's cognitive and behavioral development, OLS estimates indicated that both level and stability of employment were positively related to children's socio-emotional development, decreasing problem behavior among zero to three year olds. However, these effects did not hold in instrumental variable models partially because of imprecision in the coefficient estimates. The IV models did produce estimates of large positive effects of both level and stability of employment on school achievement, but these effects decreased in size and lost significance when other endogenous variables—namely income and welfare receipt—were also instrumented.

*Policy Implications*

In a democracy, policy-making is the product of public conversations and negotiations, explicit or not, about our values, perceptions of human behavior, and beliefs about the role of government. This was certainly the case with welfare reform in the 1990s, which tapped into strong negative public sentiment about welfare creating dependency and worsening rather than improving the problems associated with poverty. At approximately the same time, a vigorous

debate was in progress about how parents should balance the demands of work and family and what role government should play in facilitating that balance. While framed very differently, these two public debates pose the same fundamental questions about values and human behavior: Who should care for young children? How involved should employers or the government be in family life? How do we weigh the value of unregulated labor markets against the preeminence of families and children?

Importantly, these policy discussions about work and family had substantially divergent results. On one hand, welfare reform increased the incentives and mandates for single mothers to work, including shortening the period after a birth before welfare recipients had to meet work requirements. Policy changes related to welfare reform also increased the supports available to single mothers by expanding child care subsidies and connecting parents to services that could address barriers to employment, such as domestic violence or substance abuse. The policy message came through loud and clear: single mothers should be working, regardless of the age of their children. On the other hand, the primary policy result of the debate about balancing work and family was the Family and Medical Leave Act of 1993, which provided 12 weeks of unpaid family leave with job protection for many employees in the U.S. This policy reflected popular opinion that it was important for parents, particularly mothers, to care for very young children without the risk of losing their jobs.

The policy debates surrounding state and federal reforms to welfare programs often centered on whether maternal employment would benefit children in poor families. Some argued that employment could increase family income and maternal self-esteem, as well as add structure and organization to family life. Others countered that transitions from welfare to low-wage

employment was unlikely to improve family circumstances and that children might be harmed by decreases in parent-child time and placement in low-quality child care settings. Research in the ten years following welfare reform found little support for either argument. There is evidence of limited effects on children of certain ages in the context of certain programmatic approaches, but overall it can not be argued that child well-being was either greatly improved or worsened by welfare reform or the increase in maternal labor force participation.

In the more than ten years following both welfare reform and FMLA, employment rates among low-income mothers have increased dramatically. Maternal employment is happening more often and sooner after a birth than even a decade ago. Yet, public policy discussions have not shifted to address the consequences of this change. For instance, what modest debate surrounded reauthorization of welfare reform in 2006 remained focused on incentives for increasing work activity among low-income mothers. The issue was framed as one of continuing efforts to move welfare recipients off the government roles and toward self-sufficiency. What was not discussed was how realistic or appropriate these requirements were for parents with young children. To some extent, the discussion about welfare and work needs to be reframed as one about parents and young children and integrated in with the parallel discussions we, as a society, are having about balancing work and family.

One approach to reconciling our values about children, families, and work would be a paid family leave policy (common in most industrialized nations) with broad coverage of workers in all occupations and wage-levels. There is substantial evidence from multi-national studies that broader paid family leave laws would increase the probability that a mother or father stays home for some period time after a child is born, but would also increase continuous

employment over the pre- and post-partum periods. Leave coverage could be particularly beneficial to single mothers, many of whom work in jobs without paid sick or vacation leave, and their children. The financial support during the leave would protect children from the risks of poverty or income drops, but the incentive to maintain employment around the time of a birth could have positive benefits for family economic circumstances in the longer-term.

Policy discussions should also move beyond a focus on single mothers as lone parents and providers. As research shows, the romantic and familial relationships of women we label "single mothers" are varied and do not fit neatly into one category. Single mothers are most often involved with partners, in many cases the father of one or more of their children, and to varying degrees they are making decisions about work and welfare in the context of those partnership. If they are living with other family members, the partnerships may extent beyond romantic involvements. Moreover, while old welfare rules present during AFDC prohibited single mothers from living with a partner, TANF not only supports two-parent families, but has a specific goal of increasing the formation and stability of two-parent households.

Prior research indicates that, in many respects, cohabiting couples look a lot more like dating couples than they do married couples. Yet, they are living together and, to varying degrees, pooling resources in order to care for their children. The case studies presented in Chapter Three suggest that the reality of family processes in unmarried couples may be more unique from either dating or married couples than we thought. The fathers in those cases were present and contributing to family resources, but their employment status and income was not always dependable. Public policy has made small steps toward viewing low-wage male workers as members of families and worthy of support. The findings of this dissertation suggest that

more could be done to help unmarried fathers improve the quality and stability of their jobs, which would ultimately benefit single mothers and their children, and promote the values of family and self-sufficiency that were central to welfare reforms.

*Research Directions*

The results of these studies inspire several observations related to both the substance and methods of future research. First, our knowledge of single-parent families has increased substantially in recent years with welfare evaluations and large-scale surveys of unmarried parents, such as the Fragile Families and Well-Being study. There is still a lot to learn, however, about how unmarried parents make decisions about employment, welfare, and other financial matters separately or together. The relevance of theories of family process—including economic theories of specialization and bargaining, as well as psychological theories about family systems—to the circumstances of unmarried couples is rarely discussed or tested.

One example of a question worth pursuing is that the second study of this dissertation highlighted the fact that single mothers who were attached to the labor force when they become pregnant often quit their jobs early on in the pregnancy. This is consistent with national statistics showing that unmarried mothers are much less likely that married mother to work during a pregnancy. We know that employment during the pregnancy is predictive of earlier returns to work after the birth, but no study that this author knows of examines the determinants of employment during pregnancy, specifically among single mothers. It may be that policies, such as welfare and child care subsidies, are providing incentives for single mothers to quit working during pregnancy. We also know that single mothers are less likely to have paid vacation or sick leave through their employers, which may make voluntary job loss a necessity or at least more

attractive.  Another possibility is that single mothers, who are disproportionately poor, may have greater health difficulties during pregnancy or work in positions that are less tolerable of health limitations.

Methodologically, this dissertation argues for continued exploration into using multiple methods to examine social phenomenon.  As others have observed, research methods should be viewed less like a ladder and more like a tool box.  Certain methods, such as experiments or ethnographies, are not inherently better than others, but they are inherently better at answering certain questions. While qualitative methods elucidate the details of individual cases and can generate theory based on comparisons of a small number of cases, their results are rarely generalizable. In contrast, quantitative methods are generally used to compare thinner information on a large number of cases and, under the right circumstances, produce results that can be applied to a larger population and applied to policy decisions. However, the process of identifying average tendencies often obscures the diversity and complexity of individual cases. These opposing strengths and weaknesses make quantitative and qualitative methods both difficult to reconcile but also complimentary.

Mixed methods are a particularly promising approach to studying human behavior and family processes.  The logic of QCA, a method used in this dissertation that was designed to bridge quantitative and qualitative perspective, is that social science phenomena are generally caused by a combination of factors and, furthermore, that different combinations may produce the same outcome. This is consistent with current perspectives in human development including ecological and family systems theory.  In addition, while experiments and econometric techniques are the cornerstone of policy-relevant research, Ragin rightly points out that policy

discussions are just as often about clusters of conditions or the context of human lives rather than the independent effect of individual factors. However, too few studies have used QCA or other approaches to mixing quantitative and qualitative methods to examine individual behaviors or current policy questions.

The two quantitative studies also highlighted some challenges to identifying causal relationships between policy and individual behavior or individual behavior and its consequences.  For instance, only recently have studies of the effects of maternal employment on children controlled seriously for the many observable and unobservable differences between women who do and do not work.  Few of these more sophisticated models have been applied to the specific question of how young children of single parents fare when their mothers work.  The third study in this dissertation attempted to do that using an instrumental variable approach, but encountered, as many IV studies do, several challenges to the validity of the estimates. Future research will continue to grapple with finding strong instruments or developing other methods that can address rather than replace or increase sources of bias.

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APPENDIX A

DESCRIPTION OF SOURCES AND CODING OF STATE WELFARE POLICIES USED IN

CHAPTER TWO ANALYSES

Information on the timing and content of waivers to AFDC and TANF implementation came from reports compiled by the Department of Health and Human Services (Crouse, 1999; A. C. F. U.S. Department of Health and Human Services; A. S. P. E. U.S. Department of Health and Human Services, 1997), as well as papers by Greenberg & Savner (Greenberg & Savner, 1996a, , 1996b), and Schoeni & Blank (Schoeni & Blank, 2000). For policy changes following the implementation of TANF, I relied primarily on the Urban Institute's *Welfare Reform Databooks* (Rowe, 2000; Rowe, McManus, & Roberts, 2004; Rowe & Roberts, 2004; Rowe & Russell, 2004), which recorded most major areas of state welfare policy for the years 1999-2003. The Databooks are a particularly good source of information on state TANF policies because they were compiled using both state regulations and caseworker manuals. In addition, the vast majority of states verified the information in the Databooks before they were published. Other sources for TANF policies included the State Policy Documentation Project (www.spdp.org), Holcomb et al. (2001), Montgomery et al. (2002), Thompson et al. (2001) Maloy et al. (1998), and Gallagher et al. (1998).

In addition, several scholars were generous enough to share the fruits of their hard work to code state policies. Dr. Raquel Bernal at Northwestern University shared her coding of work requirements and exemptions; I used her coding primarily as a check of my own. Dr. Katherine

Magnuson at the University of Wisconsin shared her data on state spending on child care subsidies from 1990-2000.

In general, I used multiple sources to "triangulate" the accuracy of data on state and county policies over time. When sources provided conflicting information, I favored whichever answer was supported by the most documents. I also favored evidence from studies that involved site visits or surveys, and from studies that focused specifically on individual states. I coded for the policy I knew to be in place in June of each year.[25]

Additional details about specific variables are provided below.

*Age-of-Youngest-Child Exemptions*

Exemption policies were coded in months, ranging from 0-48; if the exemption policy was in weeks, I converted the measure to months (e.g. 13 weeks = 3.25 months). If exemption rules differed for one- and two-parents families, I used the rules for one-parent families. I dated policies according to when they were first applied to any residents in the states, even if they were initially tested in select pilot counties before being implemented statewide.[26]

In three states—California, Colorado, and New York—age-of-youngest-child exemption lengths were set at the county-level after TANF implementation. Each of these states set a standard and then allowed states to decrease or increase the length within limits. I am still in the process of coding these county-level exemption policies for the years following TANF

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[25] The exception is that data taken from the Urban Institute's Welfare Reform Databooks (Rowe 2000; Rowe, McManus, and Roberts 2004; Rowe and Roberts 2004; Rowe and Russell 2004) were coded for July of each year. I used this data with the assumption that the same policies were in place the month before.

[26] This decision rule affected the policy coding for only one state, Utah, which eliminated the exemption in seven counties starting in 1993 and expanded this policy to the full state in 1996. I coded Utah's exemption policy as zero in 1994 & 1995.

implementation.  For the purposes of the preliminary analyses presented in this paper, I have

taken steps to complete missing data with reasonable assumptions.  In California, I have reliable

data from the All-County Implementation Survey—a survey of county TANF administrators—

conducted by RAND in 1998 (data available on-line at:

http://www.rand.org/labor/CalWORKs/datasets.html.) I coded policies in California in 2000 and

2002 with their 1998 values, making the assumption that these policies did not change over time.

Policies in Colorado and New York are coded as the state standard—zero in Colorado and 12

months in New York—unless I have information that a specific county instituted a different

policy. [0]

<p style="text-align:center;">*AFDC/TANF and Food Stamp Benefits*</p>

AFDC/TANF and Food Stamp benefit levels from 1990-1998 were taken from a

publicly-available dataset compiled by Dr. Robert Moffitt at Johns Hopkins University (Moffitt,

2002).  Using his variable definition (maximum combined AFDC/TANF and Food Stamp benefit

for a family of four), I completed this information for 2000 and 2002 using the U.S. House of

Representatives Ways and Means Committee's Green Book (2004), which provides program

descriptions and historical data on social and economic topics under the purview of the

committee.  I used January 2003 values for June 2002 because I was unable to find a source that

reported the June 2002 benefit levels.

<p style="text-align:center;">*Earnings disregards*</p>

Using the Welfare Databooks (Rowe, 2000; Rowe, McManus, & Roberts, 2004; Rowe &

Roberts, 2004; Rowe & Russell, 2004) and the SPDP, I calculated the dollar amount of a welfare

recipients first $600 in earnings that would be excluded from benefit calculations in the first and thirteenth month of welfare receipt.

*TANF policies*

I coded for the number of months before requirements begin, the hours required of parents of children under six years of age, and the state's work participation rate required by Federal government.  I compiled this information using the Welfare Databooks (Rowe, 2000; Rowe, McManus, & Roberts, 2004; Rowe & Roberts, 2004; Rowe & Russell, 2004) and the State Policy Documentation Project (SPDP) http://www.spdp.org/.  I checked my coding of these policies against similar variables created by Dr. Raquel Bernal at Northwestern University.

Sanction policies were divided into four categories based on the severity of the first and ultimate sanction: none; partial/partial; partial/full; full/full.  The sanction variable used in the model was a dichotomous variable equal to one if the sanction policy was partial/full or full/full, in other words it indicated whether the state had the option of a full family sanction.

I also coded for the length of the firm lifetime limit on TANF receipt.  Some state have no firm lifetime limit because they use state funds to continue TANF receipt past the federal lifetime limit.

*Child care spending*

Child-care spending data were compiled by Dr. Katherine Magnuson at the University of Wisconsin, Madison.

*Refundable EITC*

I determined whether a state had a refundable EITC based on information provided on the web-site, www.stateeitc.com, and several reports from the Center on Budget and Policy Priorities

(N. Johnson, 2001; N. Johnson & Lazere, 1998; N. Johnson, Llobrera, & Zahradnik, 2003; Lazere, 1998). States were coded as having a refundable EITC in effect *the year after* the legislation passed.

*Unemployment rates*

Seasonally-adjusted state unemployment rates were extracted from the Bureau of Labor Statistics, Local Areas Unemployment Statistics on June 15, 2006 (http://www.bls.gov/lau/). The rates reflect a new modeling approach and re-estimation as of March 2005. County-level unemployment rates were used in states with county-determined exemption lengths.

Appendix Table B.1  Logistic Regression Coefficients from State Fixed-Effects Models Estimating the Employment Effects of Age-of-Youngest-Child Exemptions

| Independent variables of interest | | Child age (in months) | | continued | |
|---|---|---|---|---|---|
| Eligible for exemption | -0.017 | 0 | – | 29 | 1.728** |
| | (0.039) | | – | | (0.138) |
| Eligible for welfare | -0.290** | 1 | -0.151 | 30 | 1.830** |
| | (0.065) | | (0.156) | | (0.134) |
| Eligible for welfare X Eligible for exe | -0.155** | 2 | 0.797** | 31 | 1.861** |
| | (0.057) | | (0.152) | | (0.303) |
| Demographic controls | | 3 | 1.320** | 32 | 1.727** |
| Age of mother | 0.030** | | (0.131) | | (0.132) |
| | (0.002) | 4 | 1.486** | 33 | 2.233** |
| Parity (mother's number of births) | -0.263** | | (0.145) | | (0.325) |
| | (0.011) | 5 | 1.598** | 34 | 1.710** |
| Race is white | – | | (0.125) | | (0.173) |
| | – | 6 | 1.565** | 35 | 1.782** |
| Race is black | 0.140** | | (0.129) | | (0.133) |
| | (0.043) | 7 | 1.665** | 36 | 1.607** |
| Race is other | -0.102+ | | (0.141) | | (0.145) |
| | (0.055) | 8 | 1.484** | 37 | 1.741** |
| | | | (0.133) | | (0.153) |
| Year dummies | | 9 | 1.631** | 38 | 1.814** |
| 1990 | – | | (0.122) | | (0.134) |
| | – | 10 | 1.543** | 39 | 1.801** |
| 1992 | 0.096 | | (0.157) | | (0.124) |
| | (0.060) | 11 | 1.557** | 40 | 1.830** |
| 1994 | 0.087+ | | (0.120) | | (0.114) |
| | (0.051) | 12 | 1.536** | 41 | 1.802** |
| 1995 | 0.113+ | | (0.161) | | (0.130) |
| | (0.061) | 13 | 1.635** | 42 | 1.768** |
| 1998 | 0.142 | | (0.117) | | (0.159) |
| | (0.092) | 14 | 1.686** | 43 | 1.780** |
| 2000 | 0.125 | | (0.161) | | (0.136) |
| | (0.129) | 15 | 1.633** | 44 | 1.714** |
| 1992 X Eligible for welfare | -0.128 | | (0.131) | | (0.154) |
| | (0.080) | 16 | 1.468** | 45 | 1.733** |
| 1994 X Eligible for welfare | -0.066 | | (0.143) | | (0.165) |
| | (0.092) | 17 | 1.648** | 47[a] | 1.845** |
| 1995 X Eligible for welfare | 0.038 | | (0.141) | | (0.163) |
| | (0.098) | 18 | 1.827** | 48 | 1.888** |
| 1998 X Eligible for welfare | 0.223* | | (0.134) | | (0.147) |
| | (0.100) | 19 | 1.761** | 49 | 1.639** |
| 2000 X Eligible for welfare | 0.549** | | (0.158) | | (0.137) |
| | (0.096) | 20 | 1.660** | 50 | 1.767** |
| | | | (0.144) | | (0.169) |
| State policies and unemployment rates | | 21 | 1.490** | 51 | 1.827** |
| Unemployment rate | -0.047** | | (0.131) | | (0.132) |
| | (0.018) | 22 | 1.701** | 52 | 1.968** |
| Full family sanction (0/1) | 0.109* | | (0.154) | | (0.139) |
| | (0.054) | 23 | 1.720** | 53 | 1.883** |
| Required TANF work rate | 1.027** | | (0.139) | | (0.171) |
| | (0.392) | 24 | 1.817** | 54 | 1.806** |
| $ disregarded of first $600 earned in | | | (0.162) | | (0.148) |
| 1st month of welfare receipt | -0.167 | 25 | 1.655** | 55 | 1.795** |
| | (0.134) | | (0.142) | | (0.163) |
| Maximum TANF/FS benefit for famil | 0.289 | 26 | 1.737** | 56 | 1.930** |
| | (0.267) | | (0.181) | | (0.124) |
| Child care spending per poor child un | 0.018 | 27 | 1.794** | 57 | 1.820** |
| | (0.066) | | (0.112) | | (0.158) |
| State EITC (0/1) | 0.006 | 28 | 1.802** | 58 | 2.130** |
| | (0.039) | | (0.145) | | (0.201) |
| | | | | 59 | 1.964** |
| | | | | | (0.138) |

Notes.
Sample includes all women 18-44 years old with a youngest child under 60 months.
*p<.10; **p<.05; ***p<.01
– identifies the omitted group

[a]There were no observations in which the youngest child age was 46 months in the data.

Appendix Table C.1 Data Sources and Variables Used in Chapter Three Analyses

| Source | Variables | Dates of fielding | Age of focal child at interview (in months) | |
|---|---|---|---|---|
| | | | Range | Average |
| **Fragile Families Survey** | | | | |
| Baseline mother | Race/ethnicity<br>Age<br>Months since last worked 2 consecutive weeks<br>In past 12 months, received TANF or other governmental assistance<br>Highest grade achieved in school<br>Number of children in household | May-September 2000 | 0 | 0 |
| Baseline father | Race/ethnicity<br>Age<br>Working in week prior to interview<br>Received gov't assistance in past 12 months<br>Highest grade achieved in school | May-September 2000 | 0-1 | 0 |
| One-year mother | Age of child when mother returned to work<br>Working in week prior to interview<br>Currently receiving TANF, Food Stamps, Unemployment Insurance, or other governmental assistance<br>Ever received TANF<br>Last month/year received TANF | April-November 2001 | 9-18 | 12 |
| One-year father | Working in week prior to interview<br>Receiving governmental assistance | March-December 2001 | 10-21 | 14 |
| **TLC3 Interviews** | | | | |
| Baseline couple | Use of government programs<br>Knowledge of welfare rules<br>Attitudes about welfare<br>Gender role beliefs<br>Employment status (mother and father)<br>School status (mother and father)<br>City of residence | May 2000-January 2001 | 1-8 | 3 |
| Baseline individual | Employment status (mother and father) | August 2000-March 2001 | 3-11 | 6 |
| One-year couple | Employment status (mother and father) | June 2001-January 2002 | 12-22 | 15 |

Notes. Dates and ages presented here are specific to the 30 Fragile Families/TLC3 cases involved in this analysis.

CURRICULUM VITAE

HEATHER D. HILL

*Education*

| | |
|---|---|
| December 2007 | Ph.D. in Human Development and Social Policy, School of Education and Social Policy, Northwestern University, Evanston, IL |
| May 1999 | Master in Public Policy, Gerald R. Ford School of Public Policy, University of Michigan, Ann Arbor, MI |
| June 1994 | Bachelor of Arts, University of Washington, Seattle, WA |

*Fellowships*

| | |
|---|---|
| 2006-2007 | Dissertation Year Fellowship<br>The Graduate School, Northwestern University |
| 2003-2004 | Sybil Heide Fellowship<br>School of Education and Social Policy, Northwestern University |

*Relevant Employment*

| | |
|---|---|
| 2002-2006 | Research Assistant, Northwestern University, Professor Greg J. Duncan |
| 2003-2004 | Research Assistant, Northwestern University, Professor Kathryn Edin |
| 1999-2002 | Research Analyst, Mathematica Policy Research, Inc., Washington, DC |
| Summer 1998 | Intern, MDRC, New York, NY |