Social Policy and Family Well-Being: Essays in Applied Microeconomics

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ABSTRACT

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In my dissertation, I study how individuals respond to changes in their options and constraints as a result of government policies and their local environments. I focus on issues in maternal and child well-being, as well as family structure and behavior, and draw implications for addressing the needs of disadvantaged populations in the United States. I use quasi-experimental empirical strategies with large and varied data sets to provide credible causal estimates. I believe that the results from my research can shed some light on the causes and consequences of disadvantage in the United States, contribute to cost-benefit analyses of some of the largest social welfare programs, and help inform decisions about public spending.

The focus on maternal and early childhood well-being is motivated by increasing support for the notion that fetal and infant health are predictive of individuals' later-life outcomes (Almond and Currie, 2011a,b). This evidence highlights the potential value in programs and policies aimed at pregnant women and new mothers. Indeed, successful programs that improve the welfare of disadvantaged women during pregnancy and post-partum may play an important role in ameliorating inequalities at birth, and thereby potentially mitigating the intergenerational transmission of low socio-economic status.

In the first essay, titled "The Effects of Maternity Leave on Children's Birth and Infant Health Outcomes in the United States" (published in the *Journal of Health Economics*, March 2011), I provide the first quasi-experimental analysis of the effects of the unpaid maternity leave provisions of the 1993 Family and Medical Leave Act (FMLA) on children's birth and infant health outcomes in the United States. My identification strategy uses

variation in pre-FMLA maternity leave policies across states and variation in which firms are covered by FMLA provisions. Using Vital Statistics data and difference-in-difference-in-difference methodology, I find that maternity leave led to small increases in birth weight, decreases in the likelihood of a premature birth, and substantial decreases in infant mortality for children of college-educated and married mothers. The fact that I only find positive impacts on the health of children of college-educated and married women, while children of less-advantaged women experience no health benefits, suggests that unpaid parental leave policies may exacerbate disparities in child health as they only benefit the parents who can afford to use them.

In the second essay, "Engaging Absent Fathers: Lessons from Paternity Establishment Programs," I examine behavior among parents who have children out-of-wedlock. Single-mother households are disproportionately disadvantaged, and children raised in two-parent households fare better along numerous measures of well-being. These facts motivate the implementation of policies that encourage father involvement among unmarried parents. I conduct the first comprehensive causal analysis of one of the largest U.S. policies that aims to engage unmarried fathers with their families, In-Hospital Voluntary Paternity Establishment (IHVPE), and place my findings in the context of a conceptual framework rooted in family economics theory (Edlund, 2011; Browning *et al.*, forthcoming). The program significantly reduces the costs of formal paternity estabishment, which is the *only* available legal contract that assigns partial parental rights and obligations to unmarried fathers.

Using data from a multitude of sources and variation in the timing of IHVPE initiation across states, I show that IHVPE achieves its stated goal of substantially increasing paternity establishment rates. However, I show that IHVPE also affects another margin of parental behavior. I find a *negative* effect on parental marriage — specifically, for each additional paternity established as a result of IHVPE, there are 0.13 fewer parental marriages occurring post-childbirth. Accounting for the decline in parental marriage, I find that the net effects on some measures of father involvement are negative, while overall child well-being is largely unaffected.

Why might paternity establishment serve as a substitute to marriage for some parents? To explain this finding, I offer a simple conceptual framework, in which parents trade-off their utility from access to children with their match quality. Paternity establishment offers an "intermediate" parental relationship option between the "extremes" of no formal relationship and marriage. When the cost of establishing paternity is lowered, parents who would have previously maintained no formal relationship *and* parents who would have previously been married are more likely to choose the intermediate contract. If fathers are more involved with their children when they have greater parental rights (Weiss and Willis, 1985; Edlund, 2011), then the net effect on father involvement is ambiguous, and can be negative if the increase in involvement among switchers out of no relationship is lower than the decrease in involvement among switchers out of marriage.

My results suggest that the trade-off between access to children and match quality is empirically relevant for parents who have children out-of-wedlock, and policies based on the notion that more father involvement is essential to child and family well-being must account for the parents' agency in choosing their partners. A paternity establishment program that intends to engage absent fathers and increase father involvement can actually have the opposite effects by discouraging some parents from marriage and reducing the support provided by otherwise married fathers.

Finally, in the third essay, titled "WIC in Your Neighborhood: New Evidence on the Impacts of Geographic Access to Clinics" (accepted at the *Journal of Public Economics*), I examine how geographic proximity to Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) clinics affects food benefit take-up, pregnancy behaviors, and birth outcomes. WIC is the major U.S. program with a goal of enhancing the health and nutrition of low-income pregnant women and children. Rigorous evaluation of the program is necessary both for policy-making purposes and for providing new estimates of the determinants of fetal and infant health. Although there are several studies that examine the relationship between WIC and birth outcomes (e.g. Bitler and Currie, 2005; Joyce *et al.*, 2005; Joyce *et al.*, 2008; Figlio *et al.*, 2009; Hoynes *et al.*, 2011), much less attention has been paid to the determinants of WIC benefit take-up. Moreover, consensus on the effectiveness of WIC has not been reached: the existing literature suffers from problems due to omitted variables bias, lack of data on important variables such as benefit take-up and breastfeeding, and other econometric and measurement issues. I employ a novel empirical approach on data from birth and administrative records over 2005-2009 that uses within-zip-code variation in WIC clinic presence together with maternal fixed effects, and accounts for the potential endogeneity of mobility, gestational-age bias, and measurement error in gestation. I find that access to WIC increases food benefit take-up, pregnancy weight gain, birth weight, and the probability of breastfeeding initiation at the time of hospital discharge. The estimated effects are strongest for mothers with a high school education or less, who are most likely eligible for WIC services.

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Dedication

For Andy.

Chapter 1

The Effects of Maternity Leave on Children's Birth and Infant Health Outcomes in the United States

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1.1 Introduction

Maternity leave policies are designed to address the challenges faced by working mothers and their newborn children. Before 1993, only thirteen U.S. states and Washington, D.C. had enacted maternity leave provisions, which enable women to take time off during pregnancy and the first months of their child's infancy while maintaining their health insurance and their right to resume work at the conclusion of the leave. The length of leave varied between six and sixteen weeks, and was unpaid except for some women in a few states, who received about half-pay with a benefit cap.[1] In 1993, President Bill Clinton signed into law the Family and Medical Leave Act (FMLA), which mandated a minimum of twelve weeks of unpaid maternity leave for the slightly more than half of working women who were eligible. Since 1993, only California (in 2004) and New Jersey (in 2008) have mandated paid maternity leave, so by law the vast majority of eligible working women are only entitled to unpaid maternity leave.[2] In this study, I measure how unpaid maternity leave affected children's outcomes at birth and infancy, and whether this policy has differential impacts on children from different socio-economic backgrounds. This is the first study to analyze the causal effects of the existing maternity leave provisions on children in the United States.

There are several mechanisms through which unpaid maternity leave may exert opposing impacts on child outcomes. The guarantee of maternity leave may reduce maternal stress during pregnancy. However, if a woman is forced to work more hours during pregnancy than she otherwise would have in order to qualify for the leave, then her stress level may be heightened. Given that stress during pregnancy adversely impacts birth outcomes (Copper

[1] After the 1978 Pregnancy Discrimination Act, five states (Rhode Island, California, New Jersey, New York, and Hawaii) allowed women to take up to 6 weeks of paid leave to recover from childbirth through the Temporary Disability Insurance system.

[2] Washington was planning to enact the Family Leave Insurance, which would provide 5 weeks of paid leave to new parents, in October 2009. However, due to state budget shortfalls, the program has been postponed to October 2012.

et al., 1996), the net effects of maternity leave on birth outcomes are theoretically ambiguous. After birth, maternity leave may affect the amount of time a child spends with his mother rather than in non-maternal care. Maternity leave will also affect the quality of time the child spends with the mother, depending on changes to her stress level and her satisfaction with the trajectory of her career. The quantity and quality of time a mother spends with her child in his first year of life matter for the child's well-being (Baum, 2003; Berger *et al.*, 2005). For example, a mother may have more time to take care of her ill child, to breastfeed, or to seek prompt medical care when she is able to take time off work. Further, the effect of maternal time depends on the quality of non-maternal care relative to the quality of maternal time. There exists substantial variation in the quality levels of non-parental care options available for children of working parents in the U.S., and the quality level plays an important role in child development (Lefebvre *et al.*, 2006; Loeb *et al.*, 2007; Gormley and Gayer, 2005). Finally, unpaid maternity leave may exert an effect on the mother's income and therefore the family's material resources available for child rearing. Hence, not all new mothers may be able to take advantage of unpaid leave, and it may have different implications for the welfare of children depending on whether they grow up in low-income and low-educated one-parent households or high-income and high-educated two-parent households, as these families likely face different constraints.

I examine the effects of unpaid maternity leave due to FMLA on birth outcomes and infant mortality rates using Vital Statistics natality and mortality data in difference-in-difference (DD) and difference-in-difference-in-difference (DDD) frameworks. My identification strategy relies on the facts that some states enacted maternity leave policies prior to FMLA, and that FMLA eligibility rules only apply to women who work in firms with 50 or more employees. Women employed by firms with fewer than 50 employees, and women living in states that had maternity leave prior to FMLA should not be affected by the policy, and can serve as a comparison group. Unfortunately, there does not exist a dataset that combines information on mother's firm size and children's outcomes during the relevant time period. To approximate maternity leave eligibility based on firm size, I use data from the County Business Patterns (CBP) for 1989-1997 to estimate the likelihood that a resident of a particular county is employed in a firm with 50 or more employees in each year. I link this information to the Vital Statistics data by county and year, and then split the sample into likely eligible and likely ineligible mothers. I compare the likely eligible and likely ineligible groups before and after FMLA and across states. I also conduct sub-sample analysis on children of college-educated and married mothers and children of less-educated and single mothers to test my theoretical predictions.

My results suggest that unpaid maternity leave due to FMLA led to small improvements in birth outcomes and substantial reductions in infant mortality rates for children of college-educated and married mothers, and had much smaller or non-existent effects on children of less-educated and single mothers. I also find effects on parity at birth that indicate that more previously childless women gave birth, while fewer women had later parity births. The effects on parity are especially present for the less-educated and single mothers, suggesting that these women were most constrained in their childbearing ability before a guarantee of maternity leave. However, given that these women are more likely to be low-income, the lack of effects on their children's health suggests that they could not afford to take the full twelve weeks of unpaid leave.[3] Further, the results on parity imply that any favorable effects of FMLA may be slightly understated, given that higher-parity children are likely to have better health at birth (Gluckman and Hanson, 2005). I find no consistent effects on risk factors or complications during pregnancy or at birth or on overall fertility. My findings of larger effects on infant mortality rates than on health at birth for children of college-educated and married mothers suggest that the effects of FMLA are concentrated on the care of children

[3]Han *et al.* (2009) find that college-educated and married women take more maternity leave than less-educated and single women in the U.S.

after birth.

My results are robust to a number of different specifications that especially test the validity of the eligibility approximation. Further, the robustness checks suggest that the results are not driven by differential selection into motherhood based on observable characteristics or by changes to county-level firm size distributions. Additionally, the magnitudes of the effects on parity are too small to produce substantial selection bias in the main results on infant mortality, suggesting that FMLA truly affected the likelihood of survival in infancy for children of mothers who were likely eligible for and able to take the full length of leave.

Recent studies on the effects of maternity leave policies in Canada (Baker *et al.*, 2008), Germany (Dustmann and Schönberg, forthcoming), and Sweden (Liu and Skans, 2010) find no effects on children's outcomes at various ages throughout their lives. However, the effect of U.S. maternity leave policies on children has not been previously examined, and my findings suggest that the institutions and available alternatives where these policies are enacted can determine the degree of their effectiveness. Additionally, a recent study on the long-term effects of a maternity leave expansion in Norway finds that children of mothers who were eligible for the leave (and thus likely affected by the expansions) are less likely to drop out of high school (Carneiro *et al.*, 2010). Thus, properly accounting for eligibility is important for identifying effects of maternity leave.

The paper proceeds as follows. Section 1.2 discusses FMLA. Section 1.3 reviews the relevant background literature. Section 1.4 describes the data. Section 1.5 discusses the empirical methods and presents summary statistics. Section 1.6 presents the results on the effects of unpaid maternity leave. Section 1.7 presents some robustness checks. Finally, section 1.8 concludes.

1.2 The Family and Medical Leave Act of 1993

FMLA affected the lives of women as mothers and workers by being the first federal law to grant unpaid maternity leave to women in every state in the U.S. Signed in January of 1993, the law actually went into effect on August 5, 1993. The federal government mandated that all eligible new mothers receive 12 weeks of unpaid leave with guaranteed health insurance coverage.[4] Eligibility requirements include having worked for at least 1,250 hours in the past 12 months for an employer with at least 50 employees, and approximately half of all private sector workers are estimated to be eligible for leave under FMLA (Han *et al.*, 2009; Ruhm, 1997).[5] As a result of FMLA, the share of full-time workers employed by firms with more than 100 employees that were covered by leave policies rose from 35% in 1988 to 86% in 1995, while the share of full-time workers in firms with less than 100 employees covered by leave policies rose from 19% in 1990 to 47% in 1995 (Waldfogel, 1999). The link between FMLA and child outcomes relies on the assumption that FMLA actually increased maternity leave-taking among women with infants. There is evidence that supports this assertion: in medium-sized firms, leave-taking increased by 23% due to FMLA for women with children under age 1 (Waldfogel, 1999). Other research shows that women took about 6 more weeks of unpaid leave due to FMLA (Ross, 1998). In more recent work, Han *et al.* (2009) find that US maternity leave policies are associated with a 13% increase in maternal leave-taking during the birth month, 16% increase during the month after birth, and a marginally significant 20% increase during the second month after birth. Further, they find that these policies only had an effect on leave-taking for college-educated and married mothers, and had no effect for less-educated and single mothers.

[4] Women are free to take the leave during their pregnancy and/or after childbirth.

[5] FMLA also provides unpaid leave for medical reasons and to take care of ill family members for male and female workers.

1.3 Background Literature

Much of the existing literature on effects of parental leave policies on childhood outcomes is somewhat limited in scope and suffers from endogeneity concerns arising in cross-sectional and cross-country regression methods. Several of such studies suggest a significant negative association between parental leave and post-neonatal mortality, as well as child mortality between ages one and five in European countries (Ruhm, 2000; Tanaka, 2005).

Three more recent studies use plausibly exogenous policy changes to identify the effects of maternity leave polices on children. Baker and Milligan (2010) find no statistically significant impacts of a recent expansion in paid maternity leave from six months to a year in Canada on early childhood development indicators for children up to 29 months old. Dustmann and Schönberg (forthcoming) find no statistically significant impacts of expansions in unpaid and paid maternity leave policies in Germany on any long-run child outcomes, including wages, employment, selective high school attendance, grade retention, and attendance.[6] Liu and Skans (2010) examine the effects of an expansion in paid leave from 12 to 15 months in Sweden, and find no impact on children's scholastic performance at age 16, or on intermediate outcomes such as mothers' subsequent earnings, measures of child health, parental fertility, divorce rates, and the mothers' mental health. They do find positive effects on age-16 educational achievement of children of well-educated mothers. Carneiro *et al.* (2010) analyze the long-term effects of a reform in Norway, which increased paid leave entitlements from 0 to 4 months, and unpaid entitlements from 3 to 12 months. Unlike in the other three studies, the authors are actually able to observe eligibility for maternity leave in administrative data. They find that children of mothers who were affected by the leave expansions are less likely

[6]In particular, the authors study three different expansions. The first policy reform increased paid maternity leave from 2 to 6 months in 1979. The second reform increased paid leave from 6 to 10 months in 1986. The third reform increased unpaid leave from 18 to 36 months in 1992.

to drop out of high school. However, to my knowledge, there has been no rigorous evaluation of the effects of maternity leave policies in the United States on children's outcomes at birth and in infancy.

While the existing evidence suggests that maternity leave policies in Canada, Germany, and Sweden may not have much impact on children's outcomes at various ages, the context where the policy is enacted matters. For example, a policy that expands paid leave from 12 to 15 months in a setting where the alternative is good-quality subsidized child care and universal health insurance (as is the case in Sweden) is quite different from one that provides maternity leave for the first time on a national level in a setting where neither child care nor health insurance is guaranteed (as is the case in the United States). So while the FMLA provides unpaid maternity leave that only lasts for 12 weeks, it may have impacts on children because of the lack of existing supports in the U.S. Further, effects may be heterogeneous across children from different backgrounds, as only mothers with sufficient income from their job and/or spouse may be able to afford to take unpaid leave. Among the several studies that examine the effects of maternity leave policies in other countries, only two have considered such heterogeneous effects (Liu and Skans, 2010; Carneiro *et al.*, 2010). I contribute to this literature by addressing these questions for mothers and children in the United States, where the institutions and unpaid leave policy may augment the importance of mother heterogeneity. Additionally, Carneiro *et al.* (2010) provide evidence that properly accounting for eligibility for maternity leave is important. In my analysis, I attempt to proxy for eligibility to the best of my ability given the data limitations.

Maternity leave policies can impact child outcomes through several mechanisms. First, a legal maternity leave policy that forbids employers to fire women due to pregnancy or childbirth potentially affects the stress levels of working women during pregnancy.[7] There

[7] To my knowledge, there are no existing studies that have analyzed the causal effect of FMLA policies on maternal employment and stress levels during pregnancy, perhaps due to data limitations. However, survey

is some medical evidence that maternal stress during pregnancy is associated with spontaneous pre-term birth (Copper *et al.*, 1996). Further, the fetal origins literature provides ample evidence for the importance of the prenatal environment on later child and adult outcomes. Aizer *et al.* (2009) find adverse effects of maternal cortisol levels during pregnancy on their children's educational attainment. Studies by Almond and Mazumder (2005), Almond (2006), and Almond *et al.* (2009) illustrate the harmful impacts of poor prenatal conditions on adult health, educational, and labor market outcomes. More relevant to my study on childhood outcomes, Kelly (2011) documents that in utero exposure of British children to the Asian flu in 1957 had detrimental effects on birth weight and cognitive outcomes at ages 7 and 11.

Maternity leave policies also affect child outcomes by increasing the amount of time a mother can spend with her child, especially in the first year of life. Several studies examine the relationship between maternal employment and child outcomes, but many suffer from omitted variables bias due to simple cross-sectional regression methodology. Maternal return to work within the first 12 weeks of her child's life is associated with reduced breastfeeding and immunizations and increased behavior problems in early childhood (Berger *et al.*, 2005). Although the negative effects of maternal work are partially offset by the positive effects of increases in income, conditioning on income, maternal work in the first year of life is associated with decreases in reading and math test scores at ages 3-11 (Baum, 2003). Han *et al.* (2001) find a negative association between maternal employment in the first year of life and white children's cognitive test scores at ages 3-8, but no effects on black children's

data suggests that the vast majority of women only take maternity leave after childbirth, and do not take leave during pregnancy in the U.S. (Declercq *et al.*, 2007). Thus, any effects of FMLA on birth outcomes likely operate through changes to maternal stress levels and overall conditions while she is employed during pregnancy. Numerous studies have found that stressful working conditions during pregnancy are adversely associated with birth outcomes (see Gabbe *et al.*, 1997 for a review). Thus, if there is a possibility that the enactment of FMLA changed working conditions for pregnant women, then one might expect there to be effects on birth outcomes.

scores.

A common side effect of early maternal employment is an increase in non-parental care for the child. Studies on the effects of non-parental childcare show mixed results for child outcomes. Full-day publicly provided childcare in Quebec has been shown to have substantial adverse effects on children's vocabulary scores at age 5, and motor and social development skills, emotional disorders, aggression, and overall health at ages 2-3 (Lefebvre *et al.*, 2006; Baker *et al.*, 2008).[8] For children below age 1 in the U.S., entry to non-parental care can have detrimental effects on cognitive and behavioral outcomes (Loeb *et al.*, 2007). Additionally, there is some evidence that the effects of non-parental care depend greatly on its quality, especially for the most disadvantaged children (Karoly *et al.*, 1998; Blau, 1999).[9]

My focus on early childhood outcomes is supported by a wealth of literature pointing to the importance of early childhood factors in predicting later-life educational, labor market, and health outcomes. Case and Paxson (2008) document a positive link between early childhood health (proxied by adult height) and cognitive test scores at ages 7, 10, and 11, which are in turn positively related to earnings and occupational status in the labor market in adulthood. Currie *et al.* (2010) find lasting impacts of birth weight and health at various ages throughout childhood on the likelihood of being on welfare, of being in grade 12 by age 17, on literacy, and on the likelihood of taking a college-preparatory math course in high school. Further, early childhood health accounts for 18% of the gap in cognitive ability

[8] In the United States, evaluations of universal public pre-kindergarten programs in Oklahoma and New Mexico find positive impacts on cognitive, motor skills, language and vocabulary scores at school entry (Gormley and Gayer, 2005; Gormley Jr, 2008; Hustedt *et al.*, 2008). However, increases in informal non-parental care due to changes in welfare laws in 1996 decreased test scores of children of single mothers aged 3-6 (Bernal and Keane, 2010). These findings suggest that the context where non-parental care arrangements are made and what the alternatives are matter.

[9] It is important to note that the effects of a maternity leave policy that substitutes maternal care for non-parental care depend on the quality of maternal care relative to the quality of non-parental care. Thus, although one might expect that mothers from high socioeconomic status backgrounds have access to better quality non-parental care options, it may still be the case that there are beneficial effects of maternal time at home for their children, especially in the first few months after childbirth.

and 65% of the gap in language ability between children of college-educated and children of less-educated parents in Germany (Salm and Schunk, 2008).

Given the importance of childhood factors for later life outcomes, and the different potential mechanisms through which maternity leave can affect childhood outcomes, a careful study of the impact of FMLA on children born to working mothers is warranted. To my knowledge, no other study has rigorously considered the causal impacts of maternity leave policies generated by FMLA on child outcomes. My research will reveal whether or not the unpaid maternity leave policies generated by FMLA were in fact sufficient for actually affecting child outcomes, and whether there were heterogeneous impacts across children of different types of mothers.

1.4 Data

To measure birth outcomes I use data from the National Center for Health Statistics Vital Statistics natality data. I collapse the universe of birth records in the United States for 1989-1997 into birth-year/birth-month/county/mother-education/mother-race/mother-age/mother-marital-status cells.[10] This results in 5,806,669 cells, with an average of 6.2 births per cell. Data on infant mortality comes from Vital Statistics mortality data for children under 1 year of age for 1989-1998.[11] . I collapse this data into county/year/birth-month cells using the exact age at the time of death to calculate the birth month, and merge it to the natality data (also collapsed into county/year/birth-month cells) by county, year, and birth month. On average, there are 137.7 births in each cell. I calculate the infant mortality

[10]I collapsed the data to make the sample size more manageable relative to the full data set of all individual birth records. Mother's education is in 4 categories (less than high school, high school, some college, college or more), mother's race is in four categories (white, black, Hispanic, or other), and mother's age is in 5 categories (less than 20 years, 20-24 years, 25-34 years, 35-44 years, and 45 or more years).

[11]The linked birth-infant death files are not available for 1991-1994, which are key years in my analysis.

rate by dividing the number of deaths for each birth month by the number of births in that birth month. The implicit assumption is that the out-of-county mobility between birth and the end of the first year of life is negligible.[12] Since this infant mortality rate approximation may be less valid for very small cells, I limit infant mortality analysis to cells with more than 25 observations to make sure that small cells do not drive my results.[13] I am left with 185,431 cells for infant mortality rate analysis.

I then merge the natality and infant mortality rate data with data from the CBP for 1989-1997 by county and year to construct a county-by-year measure of the likelihood of maternal employment in a firm with 50 or more employees (as only these firms are covered by the FMLA mandate). The CBP provides information on the total employment in each county and year, as well as the number of firms in different size categories. I estimate the conditional probability that a person in a given county and year is employed by a firm with at least 50 employees:

$$\text{Prob(Employed in firm with 50+ } | \text{ Employed)}$$
$$= \frac{\mu(50-99)\times n(50-99)+\mu(100-249)\times n(100-249)+\mu(250-499)\times n(250-499)+...}{\text{Total Employment}}$$

where $n(i-j)$ = number of firms with firm size between i and j, and $\mu(i-j)$ is the median number of employees in each firm-size category.

For lack of better information on average firm size in each firm-size category, I use the median of each firm-size category to approximate the number of workers. To check the validity of this method, I plot the total employment calculated this way versus actual employment in

[12]To check, I analyzed Current Population Survey (CPS) data over 1989-1997 on 28,815 households with a youngest child who is less than 1 year old. Fewer than 10% of these families reported moving out of county within the last year. Given that my results are concentrated on high-socioeconomic-status mothers who tend to have lower than average mobility (U.S. Census Bureau, 2000), it is unlikely that out-of-county mobility severely affects my conclusions.

[13]The omitted cells with 25 births or fewer are very similar in observable characteristics to the included cells. A table of summary statistics is available upon request.

Appendix Figure A.1. It seems like my method overestimates total employment somewhat, but slightly decreasing the number used to approximate average firm size in each firm-size category does not alter my results.

As a further check of the firm-size approximation, I use data from the Quarterly Workforce Indicators (QWI) database. This database is part of the U.S. Census Bureau's Labor Employment Dynamics database, which combines information from unemployment wage records and businesses on various labor force indicators, including average establishment size for every county and year. I do not use these data in my main analysis because only seven states (California, Colorado, Florida, Idaho, Oregon, Washington, and Wisconsin) have data before 1993. However, I can check whether my calculation of the conditional probability of working in a firm with 50 or more employees is consistent with QWI data for available counties and years. Reassuringly, across counties and years for which I calculate the conditional probability of employment in a firm with 50 or more employees to be greater than 0.5, the minimum average establishment size in the QWI is 60.

It is important to note that my approximation of eligibility based on firm size abstracts away from several issues. In particular, counties with lower total employment may by construction have higher conditional probabilities of employment in a firm with 50 or more employees. However, constructing unconditional probabilities of employment necessitates the use of yearly county-level population estimates which can be very unreliable, especially for small counties.[14] Further, female employment differs by industry, and industries differ by average firm size. Additionally, certain industries are more likely to have unionized occupations, which may have been more likely to offer maternity leave benefits prior to FMLA enactment.[15] Ideally, I would like to estimate the probability that a woman in a given county

[14]Nevertheless, I estimate the DDD model with unconditional probabilities as a robustness check in section 1.7 and all the results are quite similar to the main results.

[15]Concrete evidence on the provision of maternity leave benefits by industry or occupation is difficult to

would have been affected by the FMLA based on her employment status, occupation or industry, and firm size. However, given the lack of information on any of these variables in the natality and mortality data, and the lack of information on maternity leave provisions among different industries and occupations, I am forced to rely on crude approximations of eligibility using county-year level firm size information in the CBP. Nevertheless, my results are robust to a number of specification checks that test this eligibility approximation (presented in section 1.7), and my identification strategy is an improvement over existing analyses that attempt to estimate the causal effects of maternity leave provisions and maternal employment on child outcomes in the U.S.[16]

As a result, I obtain a range of conditional probabilities across the cells that vary on a county-year level. Because of the way they are calculated, these probabilities can potentially be greater than 1, but only 1,521 out of 5,806,669 cells (0.02%) have this problem in the natality data and 239 out of 185,431 cells (0.1%) have this problem in the infant mortality rate data. Omitting them from the analysis does not alter the main findings. I then compute the median probability. Cells that have a probability higher than the median belong to the likely eligible group and the rest to the likely ineligible group.[17] Section 1.7 presents several specification checks that test the validity of this eligibility approximation.

Since my eligibility approximation is made at the county level, I include a number of county-level controls in my estimation. In particular, I merge the natality and infant mortality data to county-level summary data from the 1990 Census by county of birth. For each county, I calculate the proportion of the population that is white, black, urban, living below

obtain. For instance, the CPS Survey of Employee Benefits Supplement of 1993 — the most comprehensive publicly available data source on employee benefits during that time — contains no questions about maternity leave benefits.

[16]The existing studies do not study FMLA specifically, and largely rely on multiple regression methods, which are subject to omitted variables bias concerns (for example: Berger *et al.*, 2005; Baum, 2003).

[17]In Section 1.7, I also present results that use the continuous measure of eligibility.

poverty, and female aged 18 to 44. I also calculate the proportion of the female population aged 15 and older that is employed and the proportion that is married, and the proportion of the female population aged 25 and older that has a college degree.

Finally, I merge the natality and infant mortality rate data with data on the monthly state-level unemployment rates from the Bureau of Labor Statistics by state, year, and month of birth to control for labor market conditions that may affect a mother's decision to work immediately after childbirth.

1.5 Estimation and Summary Statistics

In the DD specification, I use the fact that some states had maternity leave policies in place prior to FMLA and compare outcomes between children born in states that had maternity leave and states that did not, before and after FMLA. Hereafter, I will call the group of states that had some kind of maternity leave policies before FMLA as the control states, and the group with no maternity leave policies as the treatment states. I use information from Han *et al.* (2009) to determine which states to put into the treatment and control groups based on whether the states had maternity leave policies in 1989-1992. Table 1.1 presents information on the length of leave provided in the control states.[18]

[18]Since FMLA provided 12 weeks of unpaid maternity leave, some of the control states experienced an increase in the length of leave. Note that the states that originally guaranteed 16 weeks of leave (DC and Tennessee) continued to provide 16 weeks even after FMLA went into effect. In my main analysis, I am interested in the effect of maternity leave relative to no legal leave. Hence, I combine all the states that had any length of leave into the control group. In earlier versions of this paper, I analyzed the consequences of increasing leave from 6-8 weeks to 12 weeks, and my results suggest that there are no effects on infant health (results available upon request). Further, eliminating the states that had maternity leave that was less than twelve weeks prior to FMLA from the analysis does not alter the main results, suggesting that grouping all states with any leave into the control group is a sound strategy.

I first estimate the following equation:

$$Y_{iscmy} = \beta_0 + \beta_1 * POST_{my} + \beta_2 * TREATMENT_s + \beta_3 * POST_{my} * TREATMENT_s + \gamma' X_{iscmy} + \rho' C_c + \theta_m + \lambda_y + \delta_s + \delta_s * y + \psi * \eta_{smy} + \epsilon_{iscmy} \quad (1.1)$$

for each cell i in state s, county c, for birth month m, and birth year y. Y_{iscmy} is the outcome of interest, and $POST_{my}$ is an indicator equal to 1 if i's birth date is in or after August, 1993 and 0 otherwise. $TREATMENT_s$ is an indicator equal to 1 if the birth occurred in a state that had no maternity-leave policies prior to FMLA. X_{iscmy} is a vector of cell-specific control variables for maternal and child characteristics, C_c is a vector of county-level controls, θ_m is a month-of-birth fixed effect, λ_y is a year-of-birth fixed effect, δ_s is a state fixed effect, $\delta_s * y$ is a state-specific time trend, and η_{smy} is the unemployment rate in the state, year and month of birth. ϵ_{iscmy} is a cell-specific error term. The key coefficient of interest is β_3, which measures the DD estimate of the effect of FMLA on children born in treatment states.

Table 1.2 presents summary statistics for selected variables in the Vital Statistics data, for the whole sample and split according to the four groups in the DD specification. In the whole sample, average birth weight is about 3,300 grams, and about 7% of babies are born low birth weight (<2,500g). About 11% of all births are considered premature (born after less than 37 weeks of gestation). The total infant mortality rate over 1989-1997 is about 8 deaths per 1,000 births. Most mothers are between 25 and 34 years old and have a high school degree. About 40% of all mothers are unmarried at the time of giving birth. The differences between the four groups are quite negligible for most variables, although the large sample size will provide me with sufficient power to detect small effects on outcomes.[19] The control state mothers are more likely to be college-educated and either white or Hispanic,

[19]In section 1.7, I also present evidence that there are no selection effects of FMLA — the DDD coefficients for regressions that use maternal characteristics as dependent variables are mostly statistically insignificant.

while the treatment state mothers are on average less educated and more likely to be black.

A key assumption in DD analysis is that the underlying trends of the two groups being considered are similar. In particular, we must assume that in the absence of FMLA, the trends in birth outcomes and infant mortality rates between the treatment and control states would have been the same, and that no other factors that might affect these outcomes occurred at the same time as FMLA. If this assumption is violated, then the DD estimates will be biased. I include state-specific linear time trends in the DD specifications to partially address this issue. However, placebo checks for differential time trends presented in Section VII suggest that some of the DD results may still be slightly biased. Additionally, the DD specifications might produce inaccurate results because they do not take into account that many new mothers were not affected by the FMLA. In particular, the DD specification does not account for the fact that women working in firms with less than 50 employees are ineligible for maternity leave under FMLA. To address these issues, I employ a DDD technique by comparing children born to mothers who were likely eligible for FMLA benefits with children born to mothers who were likely ineligible, in the two groups of states, before and after FMLA went into effect. This specification also allows me to include a full set of state-year interactions to control for any other factors that are changing at the state-year level that may affect my outcomes of interest. The triple-difference estimation is my preferred specification.

More precisely, I estimate the following DDD model with state, year, and month of birth fixed effects, as well as state-year interactions:

$$\begin{aligned} Y_{iscmy} = {} & \beta_0 + \beta_1 * POST_{my} + \beta_2 * TREATMENT_s + \beta_3 * ELIG_{cy} + \beta_4 * POST_{my} * TREATMENT_s \\ & + \beta_5 * POST_{my} * ELIG_{cy} + \beta_6 * ELIG_{cy} * TREATMENT_s + \beta_7 * ELIG_{cy} * POST_{my} * TREATMENT_s \\ & + \gamma' X_{iscmy} + \rho' C_c + \theta_m + \lambda_y + \delta_s + \mu_{sy} + \psi * \eta_{smy} + \epsilon_{iscmy} \quad (1.2) \end{aligned}$$

where $ELIG_{cy}$ is an indicator equal to 1 if i's county and year of birth place i into the likely eligible group (estimated using CBP) and 0 otherwise. μ_{sy} is a state-year interaction effect. The rest of the variables and parameters are as defined before. The key coefficient of interest is β_7, which measures the estimate of the DDD effect of FMLA.

To get a better sense of the firm size variation used to identify the triple difference effects, I present some county-level summary statistics split by treatment and control states and by likely eligible and likely ineligible county-years in Appendix Table A.1. In both the treatment and control states, likely eligible county-years are more urban, have a lower proportion of whites and a higher proportion of blacks, and have a lower proportion of married females and a higher proportion of employed females. Interestingly, the likely eligible county-years also tend to have more college-educated females and fewer people living below poverty.

However, although there are clear differences between the likely eligible and likely ineligible counties, the validity of the DDD model relies on the assumption that in the absence of FMLA, the difference in outcomes between likely eligible and likely ineligible counties in treatment states after FMLA would have been similar to the difference in outcomes between these counties in control states and before FMLA went into effect. While I cannot rule out that there are other unobservable factors that have a divergent effect on this difference, a series of robustness checks suggests that this is unlikely. First, there are no changes to the difference in observable maternal characteristics between likely eligible and likely ineligible counties in treatment states after FMLA relative to the difference in control states and before FMLA went into effect. This suggests that my results are not driven by FMLA-induced selection into motherhood based on observable characteristics (for instance, the observed reduction in infant mortality is not a result of more educated women being more likely to have a child once FMLA went into effect). Second, the FMLA did not affect the county-year level firm size distribution. Thus, my results are not driven by endogenous selection of firms into firm-size categories (for example, this means that the observed reduction in infant mortality is not a result of lower-quality firms changing firm-size categories to be below the 50-employee cut-off and thus mothers with a higher probability of employment in a firm with 50 or more employees being employed in higher-quality firms). Section 1.7 discusses these issues in more detail.

1.6 Results on the Effects of FMLA

1.6.1 Effects on Birth Outcomes and Infant Mortality Rates

Figure 1.1 depicts a graphical representation of some of the key results from my analysis. In particular, for three outcomes (birth weight, an indicator for a premature birth, and the total infant mortality rate) I plot the yearly coefficients from the DDD model — each point represents the coefficient on the interaction between an indicator for a birth in a treatment state, an indicator for a birth in a likely-eligible county-year, and an indicator for the year on the x-axis. I include all the controls, fixed effects, and state-year interactions listed in equation (1.2) in the regressions used to estimate these coefficients. The graphs suggest that after 1993, there is a positive effect on birth weight and negative effects on the likelihood of a premature birth and on the infant mortality rate of being born in a treatment state and in a likely-eligible county-year. Thus, these graphs present suggestive evidence that FMLA may have improved infant health for children of mothers who were most likely affected by FMLA provisions.

Table 1.3 presents the regression results on the effects of FMLA on birth outcomes in the natality data. The first four columns list the coefficients from the DD specification based on treatment and control states, while the last three columns present the coefficients from the DDD specification. The regressions with controls include maternal and child cell-level characteristics (four dummies for mother's age category, three dummies for mother's education, three dummies for mother's race, a dummy for mother's marital status at the time of childbirth, and the proportion of male births), county-level characteristics (percent white population, percent black population, percent urban population, percent population below poverty, percent female aged 18-44 population, percent females employed, percent females married, and percent females aged 25+ with a college degree), and the unemployment rate in the state, year, and month of birth.[20] Robust standard errors are clustered on the state level, and all regressions are weighted by cell size. Notably, the coefficients of interest do not change significantly as fixed effects and control variables are added to the specifications, thus providing some validation of the identification strategy. While the DD results suggest that there are no effects on birth outcomes for the whole sample, the more reliable DDD results show small but statistically significant effects on birth weight and likelihood of a premature birth. The magnitudes of the coefficients imply that the FMLA is associated with a 0.2% increase in birth weight, a 0.04% increase in the gestation length, a 3% decrease in the likelihood of a low-birth-weight birth, and a 3% decrease in the likelihood of a premature birth.[21] There seem to be larger and more statistically significant effects on births by college-educated and married mothers, who were more likely to be eligible for FMLA and able to afford unpaid leave, than on births by less-educated and unmarried mothers. This is also consistent with evidence that U.S. maternity leave policies only affect the leave-taking of college-educated and married mothers (Han *et al.*, 2009).[22] I also conducted analysis on

[20]Given that Dehejia and Lleras-Muney (2004) find that business cycles at the time of conception affect selection into fertility, some might argue that it is also important to control for the unemployment rate at the time of conception. I estimated models that control for the state-level unemployment rate nine months before the month of birth, and the results are very similar to the ones presented here. These results are available upon request.

[21]I also estimated specifications the DD and DDD specifications for birth weight controlling for gestation length in weeks. The results are very similar to ones presented here and available upon request.

[22]Additionally, I conducted some analysis of CPS data over 1989-1997 to check whether employed college-educated and married mothers are more likely to take up leave benefits than less-educated and single mothers. I limited my analysis to 10,472 women with a youngest child under 1 year old who report being in the labor force. The CPS did not ask information about maternity leave until 1994, and thus Han *et al.* (2009) conducted their analysis on the effects of FMLA using a variable that indicates being absent from work "for

the five-minute Apgar score, risk factors, labor complications, and births with congenital anomalies, and found no statistically significant effects for the whole sample or for the sub-samples.

Table 1.4 presents the results on the effects of FMLA on infant mortality rates. Again, the first four columns of this table list the coefficients from the DD specification based on treatment and control states, while the last three columns present the coefficients from the DDD specification. The controls are the same as in Table 1.3, robust standard errors are clustered on the state level, and all regressions are weighted by cell size. There is a statistically significant and negative effect of FMLA on infant mortality for the college-educated and married sub-sample.[23] The results suggest that for this sub-sample, FMLA reduced the overall infant mortality rate by 6 deaths per 10,000 births (10% decline at the sub-sample mean), the post-neonatal mortality rate by 2 deaths per 10,000 births (10% decline at the sub-sample mean), and the neonatal mortality rate by 3 deaths per 10,000 births (7.5% decline at the sub-sample mean). Since there is no effect on the overall number of births,[24] these results are not mechanically driven by an increase in the denominator of the infant mortality rate. While these effects may seem large, it is important to note that they are only present for a specific sub-sample that was most likely to be eligible for FMLA and able to take unpaid leave, and in which the mean infant mortality rates are relatively low. There are no statistically significant effects on infant mortality in the less-educated

other reasons"; they argue that this is a good measure of maternity leave-taking for this group of women. Over 1989-1997, 22% of college-educated and married working women with an infant child report being absent from work "for other reasons", while only 4% of less-educated and single working mothers do so. Over 1994-1997, 11% of college-educated and married working mothers report taking maternity leave, while less than 2% of less-educated and single working mothers do.

[23]The college-educated and married sub-sample consists of county/birth-year/birth-month cells where the proportion of college-educated and married mothers is greater than the median in each state and year, while the less-educated and single sub-sample consists of cells where the proportion is less than the median in each state and year. The mortality records have no information on the mothers' characteristics, so I cannot get any more precise measure of education level or marital status.

[24]I discuss the effects on parity and fertility in the next subsection.

and unmarried sub-sample, suggesting that the children of these mothers were unaffected by FMLA's unpaid leave.

Appendix Table A.2 presents the DDD results on birth weight, premature births, and the total infant mortality rate for college-educated and married mothers in more detail across different specifications and with coefficients for the main effects and double interactions. As in the DD specifications, the DDD key coefficients of interest do not vary significantly as the various controls and fixed effects are added.

To further understand the mechanisms through which maternity leave might affect infant deaths, I estimate the triple-difference models for the number of deaths per 1,000 births separately by cause of death for the whole sample and the college-educated and married sub-sample. Table 1.5 presents the results from these regressions. For the whole sample, the decline in the infant mortality rate seems to be driven by deaths from ill-defined causes (which include deaths from the Sudden Infant Death Syndrome), while for the college-educated and married sub-sample it is also driven by deaths from congenital anomalies. In particular, at the sub-sample mean, the estimates suggest there is a 16% decline in deaths from congenital anomalies, and a 16% decline in deaths from ill-defined causes for the college-educated and married sub-sample.

To put this in context, we can consider that these estimates represent reduced-form or intent-to-treat (ITT) effects, given that I do not observe leave-taking. To calculate the treatment-on-the-treated (TOT) effects on women who actually take up maternity leave, consider that Han *et al.* (2009) estimate that about 36% of college-educated and married women take leave as a result of FMLA.[25] Using this as an estimate of the "first-stage" effect of FMLA on leave-taking among college-educated and married women, my results suggest

[25] Han *et al.* (2009) estimate coefficients of 0.13-0.17 for leave-taking during the birth month among college-educated and married women. They do not report sub-sample means, but at the whole-sample mean of 0.415, this corresponds to an effect size of 31-41%. I take the median of 36% in this illustration.

that FMLA led to a 47% reduction in the likelihood that a child born to one of these women dies from a congenital anomaly in infancy.[26] To calculate a confidence interval for this TOT effect size, I use the delta method to estimate a standard error of 0.15. With these estimates, I can conclude that the 95% confidence interval for the TOT effect size for infant mortality from congenital anomalies ranges from 18% to 76%. For overall infant mortality among children of college-educated and married mothers, the implied TOT effect size is 27% with a 95% confidence interval that ranges from 8% to 44%. Unfortunately, since, to my knowledge, this is the first paper to estimate the causal effect of U.S. maternity leave policies on infant mortality, there is no benchmark effect size for comparison. However, literature on the effects of air pollution on infant mortality in the U.S. reports relatively comparable effect sizes. In particular, Chay and Greenstone (2003) find that a 1% reduction in total suspended particulates (TSP) exposure leads to a 0.5% decrease in infant mortality. They write that TSP concentrations decreased from 95 μ/m^3 to 60 μ/m^3 over the 1970s and 1980s — a 37% decline. This means that the reductions in TSP exposure over this time period led to an 18% decline in total infant mortality. Currie and Neidell (2005) and Currie *et al.* (2009) estimate that a 1-unit increase in carbon monoxide exposure leads to a 2.5-5% increase in total infant mortality in California and New Jersey. Given they find that the effects of air pollution exposure on infant health are 2-6 times bigger for smokers and older mothers, their estimated effect sizes for these subgroups would overlap with the confidence intervals that I present. Thus, while seemingly large, the magnitudes that I estimate are not unreasonable especially at the lower end of the implied range of effect sizes and given the low incidence of infant deaths for the sample of interest.

The observed pattern of results suggests that FMLA allowed new mothers to take better care of their ill children by spending more time with them at home, by seeking prompt medical

[26]To calculate this magnitude: TOT = ITT/First Stage = 0.17/0.36 = 0.47.

care (with their guaranteed health insurance), and by breastfeeding them.[27] In particular, the American Academy of Pediatrics highlights the importance of prompt professional medical care for infants who have different kinds of congenital anomalies. For instance, parents of children with spina bifida, a congenital anomaly of the spine, should be very alert for signs of infection and ensure that their children receive immediate medical treatment (Harstad and Albers-Prock, 2011). It seems that mothers who are able to spend time at home with their newborns as a result of FMLA are less constrained in their ability to immediately respond to such signs, and thus potentially save their children's lives. While data limitations prevent me from analyzing these particular mechanisms empirically, the findings suggest that maternal time at home after birth may be a key factor for preventing infant deaths among ill newborns.

It is important to note that I find no statistically significant effects on risk factors during pregnancy, incidence of complications during labor, or on births with congenital anomalies. In fact, it appears that FMLA did not affect the number of births of ill children or children with birth defects, rather, mothers with more leave were better able to take care of their children and keep them alive. Finally, some may conclude that the fact that FMLA did not have an impact on deaths from external causes (such as homicides and traffic accidents) and causes originating in the perinatal period suggests that the policy had little net effect on mothers' stress levels or well-being during pregnancy and after childbirth (and this is also consistent with the very small effects on birth outcomes). However, evolving research on fetal origins suggests that *in-utero* conditions, including maternal stress during pregnancy, have lasting causal effects on adult health and well-being, and that these effects may not operate through impacts on birth weight and infant health (Gluckman and Hanson, 2005). Thus, while my results point to increased maternal time at home after childbirth following

[27]Experts on the Sudden Infant Death Syndrome (SIDS), which falls under the category of ill-defined causes, recommend breastfeeding and ensuring that a baby sleeps on his/her back to reduce the risk of SIDS (see https://health.google.com/health/ref/Sudden+infant+death+syndrome for more information.)

FMLA as being a crucial factor for improving infant health, I cannot rule out the possibility of effects on later-life health and other outcomes as a result of reductions in maternal stress during pregnancy due to FMLA enactment.

1.6.2 Effects on Parity of Birth

Given that maternity leave provisions may affect women's decisions to have children, it is important to determine whether the policy impacted selection into the birth sample. In particular, there is evidence that higher-parity births have healthier birth outcomes because of a better *in-utero* environment (Gluckman and Hanson, 2005).[28] So, if FMLA affects the ratio of first-parity births to later-parity births, one must account for the selection effects due to a change in the composition of the birth sample to isolate the true effect of FMLA on birth outcomes and infant mortality rates.

Table 1.6 presents the results on the effects of FMLA on parity. The DDD results suggest that overall there was an increase in first-parity births and a decrease in later parity births in the whole sample. Interestingly, this effect is rather persistent, and still present when one compares 1996-1997 outcomes to those pre-FMLA (results available upon request). However, there are no effects on overall fertility as measured by the number of births in each cell.[29] Further, unlike any of the other outcomes, the positive effects on first-parity births are driven by the less-than-college-educated and single mothers. One explanation for these findings may be that the guarantee of maternity leave (and the employers' inability to fire new mothers

[28]However, with regard to later-life outcomes, Black *et al.* (2005) argue that higher birth order is negatively related to children's educational attainment and earnings. This may be due to primogeniture effects on parental investment decisions.

[29]The coefficients in the DDD specifications estimating effects on fertility for the whole sample, the college-educated and married sub-sample, and the less-educated and single sub-sample are statistically insignificant. For the whole sample, the coefficient is 1.4930 with a standard error of 1.6122, for the college-educated and married sample, the coefficient is -0.1348 with a standard error of 1.4836, and for the less-educated and single sample, the coefficient is 2.2160 with a standard error of 1.5573.

for taking time off) lowered the costs of childbirth for some working mothers, and hence encouraged previously childless women to give birth.[30] Less-educated, single, and childless working women likely faced higher costs of childbirth prior to guaranteed maternity leave than college-educated and married women because they are less likely to have a safety net of savings, family support or a secondary income that can support them in case they lose their jobs. Hence, by eliminating the risk of unemployment due to childbirth, FMLA affected the decision to have a first child for this group of women. However, given that prior evidence suggests that these new mothers did not take much of the maternity leave (Han *et al.*, 2009), there are no noticeable effects of FMLA on their children's outcomes. In contrast, FMLA did not affect the decision to have a first child for college-educated and married women, but their children benefited from their mothers' ability to take advantage of the twelve weeks of unpaid leave.

The decline in later-parity births can be potentially explained by a change in the workplace culture for women after FMLA. Since a large fraction of women giving birth to higher-parity children gave birth to their firstborn children prior to FMLA, it is likely that these women face different costs of childbirth than the women giving birth for the first time after FMLA. Prior to guaranteed maternity leave, it may have been customary for these women to stop working for some time (or quit the labor force entirely) to care for their newborn child. With the advent of FMLA, the standard changed to just taking twelve weeks of leave, and this may be seen as inadequate by women who already have children, thus reducing the rate of later-parity births.[31]

[30]However, there is no effect on the timing of giving birth. Results in section 1.7 suggest that there is no effect of FMLA on maternal age at childbirth.

[31]There is some suggestive evidence on substantial heterogeneity in attitudes towards childbirth among working women in the United States and in the United Kingdom. Further, these studies point to workplace culture and leave policies playing a role in a woman's decision to have a child and to return to employment after childbirth (Declercq *et al.*, 2007; Dex *et al.*, 2001).

Regardless of the exact explanation for the parity effects, these results suggest that my findings of FMLA's favorable effects on birth outcomes and infant mortality rates may be slightly understated, given that the fraction of later-parity births declines. However, given that FMLA only increased the likelihood of a first-parity birth by 2.6%, decreased the likelihood of a second-parity and third-parity birth by 1.7%, and that the first-parity effects are not apparent in the college-educated and married sub-sample, the understatement of the true effects on infant mortality rates for this sub-sample is likely negligible.

1.7 Robustness Checks

My first specification check tests for differential time trends in the DD analysis. I include placebo interactions between indicators for years 1992 and 1991 and an indicator for treatment states into the DD model. If there are any differential time trends between treatment and control states, then we may see spurious effects in the years prior to FMLA enactment. Appendix Table A.3 presents the results of this specification check for college-educated and married mothers, as this is the sample for which I find the most results. The results for infant mortality rates suggest that there are mostly no spurious effects prior to FMLA (except for an increase in the treatment state post-neonatal mortality rate in 1992), thereby strengthening the validity of my findings for these outcomes. However, there seems to be a downward trend in birth outcomes for treatment states prior to FMLA. This suggests that the lack of DD effects for birth outcomes may be driven by a downward bias due to these differential trends. Hence, the DDD model is a necessary improvement upon the DD specification, as it does not rely on an assumption of similar time trends between treatment and control states. The remainder of this section tests the robustness of the DDD model.

First, since the split into likely eligible and likely ineligible groups across the median probability of employment in a firm with 50 or more employees is somewhat arbitrary, I also estimate a model using the continuous measure of the probability rather than a binary indicator. In particular, the effect of FMLA is measured by the coefficient on the interaction between the conditional probability that a mother is employed in a firm with 50 or more employees, an indicator for a birth occurring after August 1993, and an indicator for the state being in the treatment group. This coefficient represents the effect of the FMLA for a given conditional probability of being eligible. One can multiply this coefficient by the conditional probability to get the true treatment effect.

Table 1.7 presents the results on birth outcomes and infant mortality rates from estimating this model. In the natality data, the mean probability is 0.56, while the median is 0.58, the 25th percentile is 0.49, the 75th percentile is 0.65, and the 99th percentile is 0.80. The results are qualitatively consistent with those in Tables 1.3 and 1.4, suggesting small effects on birth weight and likelihood of a premature birth, and large negative effects on the infant mortality rate for the college-educated and married sub-sample only. The coefficients here can be interpreted as the effects for those mothers whose conditional probability of eligibility based on firm size equals 1, and so they are much larger than those in the triple-difference specifications. At the mean probability, the results suggest a 22g increase in birth weight, a 0.8 percentage point decrease in the likelihood of a premature birth, and a reduction in 1.6 deaths per 1,000 births for the college-educated and married sub-sample. However, these magnitudes should be interpreted with caution, as they are based only on calculations of conditional probability of employment in a firm with 50 or more employees using the year and county of birth, and cannot assess individual mothers' actual eligibility for FMLA.

As another robustness check, I conduct my analysis on "small-firm" counties where the likelihood of employment in a firm with 50 or more employees is less than 0.30, and "big-firm" counties where it is greater than 0.70.[32] If my analysis truly captures the effects of

[32]Ideally, I would like to consider counties where there are no firms with fewer than 50 employees and counties where there are no firms with 50 or more employees. However, there are no counties that fall in the

FMLA, then all the effects should be concentrated among the "big-firm" counties, and we should not see any effects for the "small-firm" counties, where mothers were likely ineligible for FMLA. More precisely, I estimate:

$$\begin{gathered}Y_{iscmy} = \beta_0 + \beta_1 * POST_{my} + \beta_2 * TREATMENT_s + \beta_3 * SMALL_{cy} + \beta_4 * BIG_{cy} \\ + \beta_5 * POST_{my} * TREATMENT_s + \beta_6 * POST_{my} * SMALL_{cy} + \beta_7 * POST_{my} * BIG_{cy} \\ + \beta_8 * SMALL_{cy} * TREATMENT_s + \beta_9 * BIG_{cy} * TREATMENT_s \\ + \beta_{10} * SMALL_{cy} * POST_{my} * TREATMENT_s + \beta_{11} * BIG_{cy} * POST_{my} * TREATMENT_s \\ + \gamma' X_{iscmy} + \rho' C_c + \theta_m + \lambda_y + \delta_s + \mu_{sy} + \psi * \eta_{smy} + \epsilon_{iscmy} \quad (1.3)\end{gathered}$$

where $SMALL_{cy}$ equals 1 if the birth occurred in a "small-firm" county, and 0 otherwise, and BIG_{cy} equals 1 if the birth occurred in a "big-firm" county, and 0 otherwise. If there are any effects of FMLA, then we should expect $\beta_1 0$ to be zero, and $\beta_1 1$ to be statistically different from zero.

Table 1.8 presents the results from estimating this specification for birth outcomes and infant mortality rates.[33] Notably, the estimate of the coefficient on the DDD effect for "small-firm" counties (β_{10} in equation (1.3)) is only statistically significant at the 5% level in one out of the 24 specifications. However, there are statistically significant effects for the "big-firm" counties (as measured by β_{11} in equation (1.3)) on the likelihood of a low-birth-weight birth, the likelihood of a premature birth, the overall infant mortality rate, and the post-neonatal mortality rate in the college-educated and married sample. These effects are consistent with

first category, as every county has at least one small firm. As a result, I chose cut-offs that would allow some variation across states and years. Slightly changing the cut-offs to different probabilities does not affect the results.

[33]As another robustness check, I limited my DDD analysis to counties that have a mean probability of employment in a firm with 50 or more employees in the bottom and top thirds of the distribution. The results from these regressions are consistent with those presented here and are available upon request.

those found in the main results, and suggest that FMLA had some impacts on the health of children of eligible women who could afford to take leave.

To further check the robustness of the eligibility approximation, I limit my analysis to counties that have zero firms with more than 50 employees and to counties that have fewer than 75 firms with less than 50 employees, but at least one firm with more than 50 employees.[34] Mothers in the former group of counties cannot be eligible for FMLA, while mothers in the latter group are much more likely to be eligible. Because there are only about 150 counties per year that fall into each group, I conduct the DDD analysis on individual-level births data (instead of cell data) in these two groups of counties. The results from this robustness check are generally consistent with my main results on birth outcomes despite substantial reductions in sample sizes that limit test power, and are available upon request.

An additional issue with the conditional probability measure is that it relies on total county-year employment. It is possible that differential changes in county-level employment that do not affect eligibility for FMLA are driving the results. To alleviate this concern, I use county-year population projections from the U.S. Census to estimate unconditional probabilities of employment in a firm with 50 or more employees. In particular, I use the county-year population estimates of individuals aged 15-64 instead of county-year total employment from the CBP as the denominator of the eligibility measure. To reduce measurement error, I drop counties that have ever had a year with a population of less than 1000 people over 1989-1997. Table 1.9 presents the results from these DDD models. Reassuringly, these results are quite similar to those in Tables 1.3 and 1.4.

Another concern is that the large effects on infant mortality are driven by selection into motherhood based on maternal characteristics. For instance, if more college-educated and

[34]For the second group, I attempted to only use counties that have fewer than 50 firms with less than 50 employees and at least one firm with more than 50 employees, but the resulting sample size is too small for any valid analysis.

married mothers choose to give birth as a result of FMLA, then the pool of healthier babies born to these mothers might increase, hence driving down the infant mortality rate. Similarly, selection based on other maternal characteristics may be driving the results. To check, I run regression (1.2) with all available maternal characteristics as dependent variables. Appendix Table A.4 presents the results of this exercise. None of the coefficients is statistically significant except for the one in the regression of an indicator for the mother having a high school degree. However, there are no effects on any of the other educational categories, or the age, race, or marital status categories. These results suggest that the effects of FMLA are not driven by selection into motherhood based on observable characteristics, thus providing more support for the validity of my identification strategy.

One more important concern is that FMLA led to endogenous selection of firms into firm-size categories. For instance, one might suppose that lower-quality firms or firms with less financial capital may choose to lower employment and move below the 50-employee firm-size cut-off to avoid bearing the costs of providing FMLA leave. As a result, after the FMLA, women in counties with a higher probability of employment in a firm with 50 or more employees would also be more likely employed in a better or wealthier firm. Thus, there could potentially be omitted variables that drive the results on birth outcomes and infant mortality for mothers in likely-eligible counties, in treatment states after FMLA.

While I cannot observe firm characteristics or firm behavior, I can test whether the FMLA induced changes to the county-year firm size distribution. I estimate equation (1.1) with my measure of the conditional probability of employment in a firm with 50 or more employees as the dependent variable. If women giving birth in treatment states after FMLA did not experience a different likelihood of employment in a firm with 50 or more employees, then there is evidence that FMLA did not lead to any changes in the firm size distribution during the time period of my analysis, thus making it unlikely that my results are driven by endogenous sorting of firms. Appendix Table A.5 presents the estimates of the key coefficient of interest for the whole sample, and for the two sub-samples that I analyze. Notably, this coefficient is not statistically significant at any conventional level in any of the samples. Given that my main results rely on this measure of conditional probability, it is reassuring that it is unlikely to suffer from endogenous selection issues.

1.8 Conclusion

High female labor force participation rates in the United States call attention to the importance of maternity leave policies. Unlike men, women who have children must take at least some time off from working during childbirth. Hence policies that ensure their job security during this time period are crucial for women's careers, health, and overall well-being. These benefits alone provide support for the enactment and continuation of maternity leave policies in this country.

The effects of maternity leave on children, however, are not well established. Existing studies in Canada, Germany, and Sweden do not find significant effects of maternity leave on either early childhood or later outcomes. Studies in the U.S. find some negative effects of maternal work during a child's first year of life, but do not evaluate current maternity leave policies. My study contributes to this literature as the first to analyze the causal effects of unpaid maternity leave due to FMLA on children's birth and infant outcomes. I use difference-in-difference and difference-in-difference-in-difference methodology and consider numerous outcomes. I also conduct sub-group analyses on children of college-educated and married women, as these are the women who are likely to be eligible under FMLA and able to take unpaid leave, and on children of less-educated and single mothers.

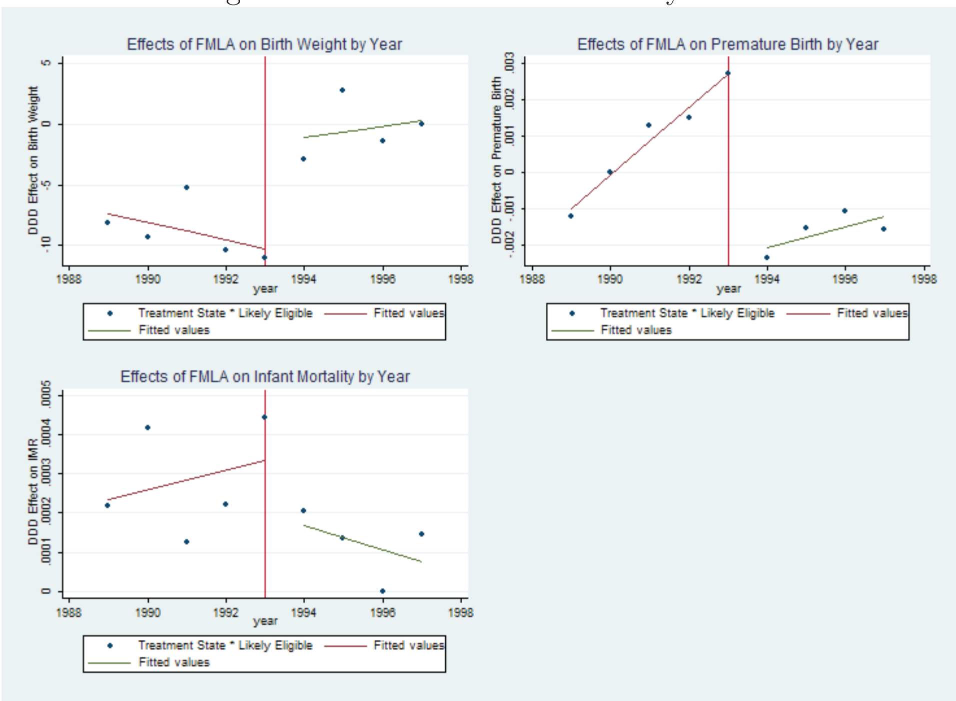
I find that for the college-educated and married sub-sample of mothers, FMLA led to small increases in birth weight, decreases in the likelihood of a premature birth, and considerable decreases in infant mortality rates that are driven by decreases in deaths from congenital anomalies and ill-defined causes. I also find the policy affected parity — there is an increase in first-parity births, which is offset by a decrease in later-parity births among all mothers. However, the effects on parity are too small to produce meaningful selection bias for the main results on infant health. Notably, there are no effects on birth outcomes or infant mortality rates for children of less-educated and single mothers.

These results suggest that for mothers who were able to take advantage of the full length of leave, the policy's effects on infant mortality are largely driven through increases in maternal time at home, and ability to provide and seek prompt care for an ill child during the first few months of life. Maternal stress and mental health during pregnancy and after childbirth are not affected, at least as measured by child outcomes that are likely impacted by maternal well-being.

The consistent finding of significant effects for the sub-sample of college-educated and married mothers and no effects for the sub-sample of less-educated and single mothers implies that FMLA may have increased disparities in early childhood health between children from different socio-economic backgrounds. Further, back-of-the-envelope cost-benefit calculations suggest that unpaid maternity leave may not be the most cost-effective way to help working mothers and their children. In particular, businesses and their employees bear the costs of FMLA's unpaid leave because of lost productivity, provision of health benefits while workers are on leave, foregone wages and benefits for employees, and administrative burdens. In 2004, FMLA cost approximately $21 billion (Neese *et al.*, 2009). However, the benefits of FMLA are only concentrated among mothers and children from high socio-economic backgrounds. In 2004, my results suggest that among the 1,011,125 births by college-educated and married mothers in the U.S., 607 of these babies were saved by FMLA. This implies that FMLA costs almost $35 million per life saved. Given that the incidence of poor birth outcomes and infant mortality is higher among women from lower socio-economic backgrounds, maternity leave policies that cover mothers and children from all backgrounds may result in much greater benefits that could outweigh the extra costs from covering more working women.

Children of poor, single and low-educated working mothers are a key vulnerable population that was not reached by the FMLA. However, these children and their families may benefit the most from policies that enable their mothers to take time off work during their early life without substantial losses in income. These mothers are often forced to work immediately after childbirth, and their newborn children are then placed in low-quality childcare. Their children already stand at a disadvantage for their later-life opportunities as they are born into low socio-economic status families, and lack of maternal time during their first few months of life may exacerbate this disadvantage. Thus, if policymakers are concerned with decreasing disparities in child health and well-being between children of different backgrounds, they need to consider the fact that an unpaid maternity leave policy may actually increase disparities because it only benefits those mothers who can afford to take it. On the other hand, paid maternity leave policies (such as those in California and New Jersey) may allow poor, single and working mothers to care for their newborn children at home, to seek prompt medical care when needed, and to develop a closer bond with them, thereby saving their lives and improving their life chances from the start.

Figure 1.1: The Effects of FMLA by Year



Notes: These figures plot the yearly DDD coefficients on the interaction between an indicator for a birth in a treatment state, an indicator for a birth in a likely-eligible county-year, and an indicator for the year on the x-axis. I include all the controls, fixed effects, and state-year interactions listed in equation (1.2) in the regressions used to estimate these coefficients.

Table 1.1: Maternity Leave Policies Prior to FMLA in Control States

State	**Number of Weeks of Maternity Leave Under State Laws in 1989-1992**
California	12
Connecticut	12
District of Columbia	16
Hawaii	6
Maine	10
Massachusetts	8
Minnesota	6
New Jersey	6
New York	6
Oregon	12
Rhode Island	6
Tennessee	16
Washington	12
Wisconsin	6

Source: Han, Ruhm, and Waldfogel (2009).

Table 1.2: Summary Statistics for Selected Variables in Vital Statistics Data

		WHOLE SAMPLE		CONTROL STATE AND PRE-FMLA		CONTROL STATE AND POST-FMLA		TREATMENT STATE AND PRE-FMLA		TREATMENT STATE AND POST-FMLA	
	N (whole sample)	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
OUTCOMES											
Birth weight in grams	5,806,085	**3324.915**	455.045	**3318.071**	456.333	**3298.097**	492.859	**3282.944**	495.475	**3297.621**	485.849
5-min Apgar Score	5,109,176	**8.970**	0.684	**8.949**	0.665	**8.958**	0.738	**8.940**	0.709	**8.951**	0.714
Gestation in weeks	5,787,698	**39.078**	2.059	**38.976**	2.013	**39.019**	2.309	**38.870**	2.253	**38.963**	2.229
Low birth weight (<2500g)	5,806,085	**0.074**	0.192	**0.075**	0.194	**0.079**	0.216	**0.083**	0.221	**0.079**	0.213
Total infant mortality rate	185,431	**0.008**	0.012	**0.007**	0.010	**0.006**	0.009	**0.009**	0.012	**0.008**	0.012
Low Apgar score (<8)	5,109,176	**0.035**	0.139	**0.033**	0.137	**0.036**	0.150	**0.034**	0.146	**0.035**	0.146
Born premature (<37 weeks of gestation)	5,787,698	**0.110**	0.230	**0.110**	0.232	**0.120**	0.261	**0.122**	0.263	**0.118**	0.255
Parity 1	5,787,041	**0.348**	0.393	**0.351**	0.373	**0.348**	0.376	**0.345**	0.397	**0.350**	0.399
Parity 2	5,787,041	**0.271**	0.341	**0.261**	0.312	**0.260**	0.315	**0.274**	0.348	**0.274**	0.350
Parity 3	5,787,041	**0.176**	0.295	**0.171**	0.269	**0.169**	0.271	**0.180**	0.302	**0.176**	0.301
CONTROLS											
Child is male	5,811,445	**0.512**	0.385	**0.513**	0.357	**0.512**	0.361	**0.512**	0.392	**0.511**	0.394
Mom <19 yrs old	5,811,445	**0.138**	0.345	**0.133**	0.340	**0.127**	0.333	**0.143**	0.350	**0.137**	0.344
Mom 19-24 yrs old	5,811,445	**0.340**	0.474	**0.318**	0.466	**0.312**	0.463	**0.349**	0.477	**0.346**	0.476
Mom 25-34 yrs old	5,811,445	**0.362**	0.481	**0.356**	0.479	**0.352**	0.478	**0.368**	0.482	**0.362**	0.481
Mom 35-44 yrs old	5,811,445	**0.157**	0.363	**0.187**	0.390	**0.200**	0.400	**0.138**	0.345	**0.153**	0.360
Mom 45+ yrs old	5,811,445	**0.003**	0.056	**0.005**	0.074	**0.008**	0.086	**0.002**	0.041	**0.002**	0.049
Mom < HS education	5,569,391	**0.281**	0.449	**0.278**	0.448	**0.263**	0.441	**0.294**	0.455	**0.273**	0.445
Mom has HS education	5,569,391	**0.341**	0.474	**0.324**	0.468	**0.320**	0.467	**0.350**	0.477	**0.342**	0.475
Mom has some college	5,569,391	**0.227**	0.419	**0.230**	0.421	**0.242**	0.428	**0.217**	0.412	**0.232**	0.422
Mom has college degree or more	5,569,391	**0.151**	0.358	**0.167**	0.373	**0.175**	0.380	**0.139**	0.346	**0.153**	0.360
Mom is non-Hispanic white	5,716,135	**0.623**	0.466	**0.583**	0.464	**0.564**	0.465	**0.642**	0.465	**0.633**	0.465
Mom is black	5,716,135	**0.212**	0.409	**0.174**	0.380	**0.170**	0.375	**0.231**	0.422	**0.217**	0.412
Mom is Hispanic	5,716,135	**0.080**	0.232	**0.103**	0.246	**0.122**	0.269	**0.062**	0.210	**0.077**	0.233
Mom is unmarried	5,811,445	**0.406**	0.491	**0.421**	0.494	**0.439**	0.496	**0.381**	0.486	**0.416**	0.493
Unemployment rate in state, year, and month of birth	5,811,445	**5.784**	1.420	**6.300**	1.647	**5.884**	1.420	**6.213**	1.409	**5.169**	1.093

Notes: The units of observation for the summary statistics presented here are county/year/birth month/mother age/mother race/mother education/mother marital status cells. Analysis is based on the universe of birth records in the United States for 1989-1997. Treatment state = birth occurred in a state that had no maternity leave laws prior to FMLA. Control state = birth occurred in a state that had some kind of maternity leave law prior to FMLA. Post-FMLA = birth occurred in or after August, 1993. Pre-FMLA = birth occurred before August, 1993.

Table 1.3: Effects of FMLA Maternity Leave on Birth Outcomes

OUTCOMES:	DD WHOLE SAMPLE				DDD WHOLE SAMPLE	DDD COLLEGE-ED AND MARRIED	DDD LESS THAN COLLEGE AND SINGLE
Birth weight in grams	-3.4727	-2.7452	1.0079	1.1151	6.5037***	9.1972**	7.1445+
	(2.0805)	(1.9993)	(1.8492)	(1.8974)	(1.7878)	(3.7886)	(3.5631)
N	5,806,085	5,806,085	5,806,085	5,488,302	5,483,611	671,425	2,053,113
Gestation in weeks	-0.0230+	-0.0211+	0.0012	0.0034	0.0173**	0.0246+	0.0065
	(0.0118)	(0.0115)	(0.0113)	(0.0113)	(0.0085)	(0.0143)	(0.0139)
N	5,787,698	5,787,698	5,787,698	5,478,550	5,473,862	670,866	2,048,140
Low birth weight	0.0010	0.0011	-0.0000	0.0001	-0.0020**	-0.0020+	-0.0023+
	(0.0007)	(0.0007)	(0.0005)	(0.0006)	(0.0006)	(0.0010)	(0.0012)
N	5,806,085	5,806,085	5,806,085	5,488,302	5,483,611	671,425	2,053,113
Premature	0.0011	0.0010	-0.0010	-0.0010	-0.0019**	-0.0029**	-0.0012
	(0.0012)	(0.0011)	(0.0006)	(0.0006)	(0.0009)	(0.0014)	(0.0015)
N	5,787,698	5,787,698	5,787,698	5,478,550	5,473,862	670,866	2,048,140
Controls	No	No	No	Yes	Yes	Yes	Yes
Year of birth fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Month of birth fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trends	No	No	Yes	Yes	No	No	No
State-year interactions	No	No	No	No	Yes	Yes	Yes

Notes: The results presented here list the DD and DDD effects on each of the outcomes listed in the first column. The units of analysis are county/year/birth month/mother's education/mother's race/mother's age/mother's marital status cells. The DD regressions compare births in states with and without maternity leave policies prior to FMLA, before and after August 1993. The DDD regressions compare likely eligibles with likely ineligibles (see text for description of how eligibility was calculated using County Business Patterns data), before and after August 1993, across states that had maternity leave policies prior to FMLA and states that did not. Controls include: 1) maternal and child cell-level characteristics — four dummies for mother's age, three dummies for mother's education, three dummies for mother's race, a dummy for mother's marital status at time of childbirth, the proportion of male births; 2) county-level characteristics — percent white population, percent black population, percent urban population, percent population below poverty, percent female aged 18-44 population, percent females employed, percent females married, percent females aged 25+ with a college degree; and 3) the unemployment rate in state, year, and month of birth. Robust standard errors are clustered on the state. All the regressions are weighted by the cell population.
Significance levels: + $p<0.10$, ** $p<0.05$, *** $p<0.001$

Table 1.4: Effects of FMLA Maternity Leave on Infant Mortality

OUTCOMES:	**DD WHOLE SAMPLE**				**DDD WHOLE SAMPLE**	**DDD COLLEGE-ED AND MARRIED**	**DDD LESS THAN COLLEGE AND SINGLE**
Total Infant Mortality	-0.0002	-0.0001	-0.0003**	-0.0003**	-0.0002+	-0.0006***	0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0003)
N	185,431	185,431	185,431	183,054	182,997	59,668	50,330
Mean of dep. var.	0.008	0.008	0.008	0.008	0.008	0.006	0.009
Infant Mortality: 28 days - 1 year	-0.0000	-0.0000	-0.0001**	-0.0001	-0.0001	-0.0002+	0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
N	185,431	185,431	185,431	183,054	182,997	59,668	50,330
Mean of dep. var.	0.003	0.003	0.003	0.003	0.003	0.002	0.003
Infant Mortality: <28 days	-0.0001	-0.0001	-0.0002**	-0.0002+	-0.0001	-0.0003**	0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0003)
N	185,431	185,431	185,431	183,054	182,997	59,668	50,330
Mean of dep. var.	0.005	0.005	0.005	0.005	0.005	0.004	0.005
Controls	No	No	No	Yes	Yes	Yes	Yes
Year of birth fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Month of birth fixed	No	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trends	No	No	Yes	Yes	No	No	No
State-year interactions	No	No	No	No	Yes	Yes	Yes

Notes: The results presented here list the DD and DDD effects on each of the outcomes listed in the first column. The units of analysis are county-year-birth month cells. Cells with fewer than 25 births are omitted from the analysis. Please refer to notes under Table 1.3 for details about controls and estimation methods. The college-educated and married sample includes cells where the proportion of college-educated and married mothers is greater than the median in that state and year, while the less than college and single sample considers cells with proportions below the median. Robust standard errors are clustered on the state. All the regressions are weighted by the cell population. Significance levels: + p<0.10, ** p<0.05, *** p<0.001

Table 1.5: Effects of FMLA Maternity Leave on Infant Mortality — By Cause

	Deaths per 1000 births from disease	Deaths per 1000 births from congenital anomalies	Deaths per 1000 births from causes originating in the perinatal period	Deaths per 1000 births from ill-defined causes	Deaths per 1000 births from accidents	Deaths per 1000 births from homicide	Deaths per 1000 births from other causes
Whole Sample DDD	-0.0316	-0.0244	-0.0120	-0.1587**	0.0202	0.0177	0.0335
	(0.0401)	(0.0568)	(0.0889)	(0.0545)	(0.0244)	(0.0158)	(0.0280)
N	182,997	182,997	182,997	182,997	182,997	182,997	182,997
Mean of Dep. Var	0.725	1.890	3.267	1.460	0.296	0.068	0.441
College-Ed and Married DDD	-0.0825	-0.2826**	-0.0177	-0.1962**	-0.0451	0.0328	0.0124
	(0.0559)	(0.0933)	(0.1201)	(0.0647)	(0.0364)	(0.0280)	(0.0459)
N	59,668	59,668	59,668	59,668	59,668	59,668	59,668
Mean of Dep. Var	0.572	1.640	2.841	1.057	0.205	0.063	0.352

Notes: The results presented here list the DDD effects on each of the outcomes listed in the top row. The units of analysis are county-year-birth month cells. Cells with fewer than 25 births are omitted from the analysis. Please refer to notes under Table 1.3 for details about controls and estimation methods. The college-educated and married sample includes cells where the proportion of college-educated and married mothers is greater than the median in that state and year, while the less than college and single sample considers cells with proportions below the median. Robust standard errors are clustered on the state. All the regressions are weighted by the cell population.
Significance levels: + $p<0.10$, ** $p<0.05$, *** $p<0.001$

Table 1.6: Effects of FMLA Maternity Leave on Parity

OUTCOMES:	DDD WHOLE SAMPLE	DDD COLLEGE-ED AND MARRIED	DDD LESS THAN COLLEGE AND SINGLE
Parity 1	0.0092**	-0.0006	0.0149**
	(0.0038)	(0.0022)	(0.0064)
N	5,476,003	670,659	2,050,009
Parity 2	-0.0047**	-0.0047**	-0.0062**
	(0.0017)	(0.0019)	(0.0028)
N	5,476,003	670,659	2,050,009
Parity 3	-0.0031**	0.0013	-0.0035
	(0.0011)	(0.0016)	(0.0030)
N	5,476,003	670,659	2,050,009

Notes: The results presented here list the DDD effects on each of the outcomes listed in the first column. Please refer to notes under Table 1.3 for details about the sample, controls, and estimation methods. Robust standard errors are clustered on the state. All the regressions are weighted by the cell population.
Significance levels: + p<0.10, ** p<0.05, *** p<0.001

Table 1.7: Effects of FMLA Using Continuous Probability Measure

	Birth Weight (g)	Gestation (weeks)	LBW	Premature	Total Infant Mortality	Infant Mortality: 28 days - 1 year	Infant Mortality: <28 days
WHOLE SAMPLE	27.8990	0.0284	-0.0068	-0.0068	-0.0007	0.0000	-0.0007
	(23.7618)	(0.0571)	(0.0062)	(0.0045)	(0.0007)	(0.0003)	(0.0006)
N	5,483,611	5,473,862	5,483,611	5,473,862	182,997	182,997	182,997
COLLEGE-ED AND							
MARRIED SAMPLE	38.4834**	0.1039	-0.0067	-0.0148**	-0.0029**	-0.0015**	-0.0014
	(14.1071)	(0.0813)	(0.0046)	(0.0069)	(0.0012)	(0.0006)	(0.0011)
N	671,425	670,866	671,425	670,866	59,668	59,668	59,668
LESS THAN COLLEGE AND							
SINGLE SAMPLE	29.1966	-0.0482	-0.0045	-0.0032	-0.0004	0.0002	-0.0006
	(32.1345)	(0.1000)	(0.0108)	(0.0100)	(0.0021)	(0.0010)	(0.0014)
N	2,053,113	2,048,140	2,053,113	2,048,140	50,330	50,330	50,330

Notes: The results presented here list the coefficients on the DD effects of the FMLA scaled by the conditional probability of employment in a firm with 50 or more employees (as a proxy for FMLA eligibility). In particular, the coefficients represent the effect of the FMLA for any given conditional probability. Please refer to notes under Tables 1.3 and 1.4 for details about the samples, controls, and estimation methods. Robust standard errors are clustered on the state. All the regressions are weighted by the cell population.
Significance levels: + $p<0.10$, ** $p<0.05$, *** $p<0.001$

Table 1.8: Comparing Effects of FMLA on Birth Outcomes and Infant Mortality in Big-Firm and Small-Firm Counties

	Birth Weight (g)	Gestation (weeks)	LBW	Premature	Total Infant Mortality	Infant Mortality: 28 days - 1 year	Infant Mortality: <28 days
WHOLE SAMPLE							
Small County * Post * Treatment State	0.7149	-0.0037	-0.0013	0.0009	0.0020+	0.0010	0.0009
	(4.3459)	(0.0264)	(0.0010)	(0.0025)	(0.0011)	(0.0008)	(0.0007)
Big County * Post * Treatment State	2.0436	0.0206	-0.0008	-0.0027	-0.0002	0.0001	-0.0002
	(6.2221)	(0.0337)	(0.0024)	(0.0029)	(0.0003)	(0.0001)	(0.0003)
N	5,806,400	5,788,023	5,806,400	5,788,023	185,431	185,431	185,431
COLLEGE-ED AND MARRIED SAMPLE							
Small County * Post * Treatment State	11.8667	0.0611	-0.0054	0.0015	0.0027	-0.0011	0.0038**
	(14.8911)	(0.0802)	(0.0039)	(0.0065)	(0.0022)	(0.0012)	(0.0018)
Big County * Post * Treatment State	14.9578+	0.0408	-0.0040**	-0.0043**	-0.0008**	-0.0004**	-0.0004
	(8.8581)	(0.0247)	(0.0019)	(0.0019)	(0.0004)	(0.0002)	(0.0003)
N	680,940	679,539	680,940	679,539	50,651	50,651	50,651
LESS THAN COLLEGE AND SINGLE SAMPLE							
Small County * Post * Treatment State	-6.5206	0.1024	-0.0034	0.0002	-0.0001	-0.0004	0.0004
	(17.1318)	(0.0611)	(0.0057)	(0.0044)	(0.0015)	(0.0009)	(0.0012)
Big County * Post * Treatment State	5.0248	0.0305	0.0010	-0.0014	0.0003	-0.0000	0.0003
	(7.4235)	(0.0347)	(0.0021)	(0.0028)	(0.0003)	(0.0003)	(0.0004)
N	2,085,200	2,079,522	2,085,200	2,079,522	60,026	60,026	60,026

Notes: The results presented here list the coefficients on the difference-in-difference-in-difference effects of the FMLA, separately for "big-firm" and "small-firm" counties. "Big-firm" counties are those that have a conditional probability of employment in a firm with 50+ employees greater than 0.70. "Small-firm" counties are those that have a conditional probability of employment in a firm with 50+ employees less than 0.30. Please refer to notes under Tables 1.3 abd 1.4 for details about the sample, controls, and estimation methods. Robust standard errors are clustered on the state. All the regressions are weighted by the cell population. Significance levels: + p<0.10, ** p<0.05, *** p<0.001

Table 1.9: Effects of FMLA Using Unconditional Probability of Eligibility

	Birth Weight (g)	Gestation (weeks)	LBW	Premature	Total Infant Mortality	Infant Mortality: 28 days - 1 year	Infant Mortality: <28 days
WHOLE SAMPLE	4.9278**	0.0252+	-0.0021***	-0.0028+	-0.0005**	-0.0001	-0.0005**
	(2.0877)	(0.0149)	(0.0005)	(0.0015)	(0.0002)	(0.0001)	(0.0002)
N	5,474,178	5,464,434	5,474,178	5,464,434	182,895	182,895	182,895
COLLEGE-ED AND MARRIED SAMPLE	6.0616**	0.0214+	-0.0026**	-0.0024**	-0.0006**	-0.0002	-0.0004+
	(2.6420)	(0.0107)	(0.0011)	(0.0010)	(0.0003)	(0.0002)	(0.0002)
N	669,830	669,270	669,830	669,270	59,877	59,877	59,877
LESS THAN COLLEGE AND SINGLE SAMPLE	5.6331+	0.0063	-0.0012	-0.0025+	-0.0003	-0.0001	-0.0001
	(3.2004)	(0.0119)	(0.0008)	(0.0014)	(0.0004)	(0.0002)	(0.0003)
N	2,050,765	2,045,790	2,050,765	2,045,790	50,458	50,458	50,458

Notes: The results presented here list the coefficients on the DDD effects of the FMLA on each of the outcomes listed in the top row. Likely eligibility is calculated using the *unconditional* probability of employment in a firm with 50 or more employees for a given county and year. This probability equals the ratio of approximate employment in all firms with 50 or more employees to the population aged 15-64 in each county and year. Counties that have ever had a year with population less than 1000 people over 1989-1997 are omitted. Please refer to notes under Tables 1.3 and 1.4 for details about the samples, controls, and estimation methods. Robust standard errors are clustered on the state. All the regressions are weighted by the cell population.
Significance levels: + $p<0.10$, ** $p<0.05$, *** $p<0.001$

Chapter 2

Engaging Absent Fathers: Lessons from Paternity Establishment Programs

2.1 Introduction

Some of the most disadvantaged families in the United States are headed by single mothers. In 2010, 43 percent of children in single-mother households lived below the poverty line (U.S. Census Bureau, 2010). As the rate of births by unmarried mothers has been rising over the last several decades — such that in 2009, 41 percent of all births were out-of-wedlock (National Center for Health Statistics, 2011) — policymakers have been increasingly concerned with alleviating the hardships faced by these families. Since unmarried mothers generally retain physical custody of their children and have full parental rights, one worry is that unmarried fathers are uninvolved with their families and do not provide financial or emotional support to their children and their children's mothers.[1] As a result, public policy has sought ways to encourage fathers to fulfill their parental responsibilities and stay involved with their families.

Some measures, such as the "Healthy Marriage Initiatives", explicitly promote parental marriage as the outcome that generates the highest level of father involvement.[2] Indeed, some advocates argue that parental marriage is "America's greatest weapon against child poverty" (Rector, 2010). Other policies are focused on a more immediate goal of increasing unmarried fathers' involvement with their families through formal paternity establishment and child support enforcement. If greater father involvement improves parental relationships and can eventually lead to marriage, then these policies can be viewed as "stepping-stones" to marriage. A common ingredient in all such policies is the underlying assumption that

[1]The dearth of financial support is documented in the March Current Population Survey: over 1989-2010, only 19% of never-married mothers report receiving any child support income.

[2]The Deficit Reduction Act of 2005 provided $150 million in funding every year for "healthy marriage promotion and father involvement". Most programs funded by these initiatives provide relationship education and counseling and conduct public advertising campaigns on "the value of healthy marriages". Many of these programs are explicitly targeted at unmarried pregnant women and expectant fathers (Administration for Children and Families, 1997).

father involvement is essential to child and family well-being.

In this paper, I provide the first comprehensive causal analysis of in-hospital voluntary paternity establishment (IHVPE), one of the largest U.S. policies that aims to engage unmarried fathers with their children and their children's mothers. I examine paternity establishment and marriage rates among parents, as well as a variety of outcomes related to father involvement and child well-being, and place my findings in the context of a conceptual framework rooted in family economics theory. My empirical analysis relies on several sources of data: 1) self-assembled data on the timing of IHVPE initiation across states from multiple sources, 2) 1992-2005 Office of Child Support Enforcement (OCSE) records, 3) 1994-2008 biannual March/April matched Current Population Survey Child Support Supplements (CPS-CSS),[3] 4) 1989-2010 March CPS annual demographic files, and 5) 1997-2010 annual Sample Child files of the National Health Interviews Survey (NHIS).

In the U.S., paternity establishment is the *only* available legal contract that can grant partial parental rights to unmarried fathers and obligate them to make child support payments.[4] Prior to the introduction of IHVPE, legal paternity establishment was a complicated and costly process that usually involved the court system and DNA testing, and of which parental awareness was low. Consequently, less than a third of all children born out-of-wedlock had paternity established (U.S. House of Representatives, Committee on Ways and Means, 2004). IHVPE programs aimed to increase paternity establishment rates by inform-

[3]Because of changes to the CPS-CSS in the early 1990s, data collected in or after 1994 are not compatible with those from earlier survey years (Freeman and Waldfogel, 2001).

[4]Child support payments can potentially constitute a substantial fraction of female-headed households' family incomes (Garfinkel *et al.*, 1998). However, as discussed in more detail in Section 2.6, this issue is more complicated for families in which the mother receives welfare benefits. In particular, states are free to decide how much formal child support income is ignored in the calculation of welfare benefits (Cancian *et al.*, 2007). Any child support paid by the father above the disregard amount is fully taxed away by the state. Thus, for welfare-receiving mothers, it is most reasonable to view paternity establishment as a mechanism for obligating unmarried fathers to make financial transfers to the family in states and years with higher disregard amounts, when formal child support payments actually increase incomes in single-mother households. I show that my results are consistent with this conjecture in Section 2.6.

ing *all* unmarried new parents about the rights and obligations related to legal paternity establishment and by allowing them to voluntarily establish paternity at the hospital at the time of their child's birth with a simple one-page form.[5] Policy advocates were very optimistic about the program, arguing that "the establishment of paternity in unmarried births is one way to address the deprivations associated with out-of-wedlock parentage," and citing multiple financial, emotional, medical, and social benefits (Pearson and Thoennes, 1995).

Using variation in the timing of program implementation across states together with paternity establishment data from OCSE annual reports, I find that IHVPE has been effective in achieving its stated goal of increasing paternity establishment rates. My results suggest that IHVPE raises paternity establishment rates by 20.2 percentage points — a substantial 40 percent effect at the pre-treatment mean.

However, I show that IHVPE also affects another margin of parental behavior. Specifically, I find that IHVPE has a *negative* effect on parental marriage: the likelihood that a mother of a child aged five years or less is married to the child's biological father is decreased by 2.7 percentage points. Relative to an approximate parental post-childbirth marriage rate of 13 percent, this is a 21 percent effect.[6] The magnitudes of my estimates imply that for every additional paternity established as a result of IHVPE, there are 0.13 fewer parental marriages occurring post-childbirth.

Why did paternity establishment serve as a substitute and not a "stepping-stone" to marriage for some parents? To explain this finding, I offer a simple conceptual framework, which also suggests that the net effects of IHVPE on father involvement and child well-being are potentially ambiguous. This framework is based on foundations developed in economic

[5]Both parents have to be present at the hospital and agree to establish paternity. There is no DNA testing involved.

[6]The best available data on *post-childbirth* marriage rates come from the Fragile Families and Child Well-Being Study. According to these data, 13 percent of parents who have children out-of-wedlock get married by the time their child is five years old.

theories on collective bargaining (Browning *et al.*, forthcoming) and the role of paternity rights in marriage (Edlund, 2011).

In my model, parents trade-off their utility from access to children with their match quality, and choose between three relationship options: marriage, paternity establishment, and no relationship. While mothers have equal parental rights in all three states, fathers only have parental rights in marriage and paternity establishment, and their rights in the former state are greater than in the latter. Additionally, parents are more sensitive to match quality in marriage than in paternity, and are unaffected by match quality in the case of no relationship. A decrease in the cost of paternity establishment induces more parents to choose this "intermediate" relationship option. The key insight is that the affected parents are comprised of *two* groups: those who would have previously maintained no relationship and those who would have previously been married. Further, if fathers are more involved with their children when they have greater parental rights (Weiss and Willis, 1985; Edlund, 2011), then the net effect on father involvement is ambiguous, and can be negative if the increase in involvement among switchers out of no relationship is lower than the decrease in involvement among switchers out of marriage.

My next set of empirical results shows that once paternity establishment becomes an easily available option, mothers can opt out of sharing a household with their children's fathers and potentially pick other, more preferrable partners. I find that as a consequence of the decline in parental marriage, mothers are more likely to remain never-married, be either cohabiting with or married to new partners who are *not* their children's fathers, and be married to "higher-quality" spouses who are slightly older and more likely to be employed.

I next turn to an analysis of the effects on father involvement, which is complicated by the IHVPE-driven decline in parental marriage that changes the composition of both married and unmarried parents. As a result, analyses of variables that are only available for unmarried parents, such as child support payments and non-pecuniary involvement measures in the Child Support Supplement (CSS) of the CPS, are likely biased due to selection into the unmarried sample of parents eligible to be asked CSS questions.[7] To account for selection into the CSS, I perform bounding exercises (Lee, 2009), which provide evidence of some positive effects on involvement among unmarried fathers: they are more likely to make all required child support payments, and to cover some food, childcare, and medical expenses for their children. These effects are likely driven by fathers who switch from maintaining no legal relationship with their families to establishing paternity.

However, the expected *net* effects on involvement are ambiguous due to the predicted decline in involvement among fathers who switch from marriage into paternity establishment. I employ several methods to study the net effects of IHVPE on father involvement. First, I consider private child health insurance provision, the only measure of parental involvement available for all children, regardless of whether their parents are married or not.[8] I find that IHVPE leads to a 4 percent decrease in children's private health insurance coverage provided by members of the household, which is not compensated by any increases in coverage provision by family members outside the household, implying a net decline in insurance provision, likely driven by previously married fathers. I also show that IHVPE leads to a 3 percent increase in maternal labor supply, suggesting that there is a net decrease in paternal monetary support, which mothers must compensate for by working. Finally, under the assumption that married resident fathers make greater financial and non-pecuniary investments in their children than unmarried non-resident fathers do, I provide some suggestive evidence of net negative effects on other measures of father involvement when accounting for selection out of marriage — fathers spend fewer days with their children, and are less likely to make any child support payments, have joint custody, and pay for childcare expenses.

[7] All household members aged 15 years or older who are a biological parent of a child in the household from an absent parent are asked CSS questions.

[8] In the CPS-CSS, all other questions regarding father involvement are only asked of CSS sample mothers.

Taken together, the results on father involvement present a collage of evidence that is consistent with the predictions of the model. While some measures of father involvement (i.e., child support payments and coverage of some food, childcare, and medical expenses) increase among fathers who switch from no relationship to paternity, the net effects on several available measures (i.e., health insurance and financial transfers) are negative, implying that the decline in these measures of involvement by switchers out of marriage outweighs any increases in involvement by switchers out of no relationship.

Although IHVPE has important effects on family structure and some measures of father involvement, the net impacts on child welfare are minimal. If children's well-being depends both on paternal investments and on the well-being of their mothers with whom they reside, then the effects of IHVPE on it are potentially ambiguous. While some children may suffer from a decrease in father involvement, they may also benefit from living with mothers who are happier as a result of decreased interaction with lower-than-desired quality partners. I find no effect of IHVPE on total family income, and no consistent effects on measures of children's physical or mental health. I do find a small negative effect on children's access to preventative care, which is perhaps driven by the decline in children's private health insurance coverage.

I test the credibility of my empirical results in a number of ways and show that they are: 1) robust across many different specifications and data sets, 2) *not* confounded by any spurious correlation between the timing of IHVPE implementation and other state time-varying characteristics or policies,[9] 3) *not* driven by any pre-existing trends in paternity

[9]The variables considered include: maternal and child characteristics, unemployment rates, poverty rates, state minimum wages, the percent of the population receiving welfare benefits, the welfare benefit for a 4-person family, the percent of the population on Medicaid, whether the governor is Democratic, the fraction of the state House that is Democratic, spending on the Children's Health Insurance Program (CHIP), adult male health insurance coverage, other child support enforcement laws (universal wage withholding, genetic testing for paternity, New Hires directory, and license revocation for non-payment), state Earned Income Tax Credit (EITC) expansions, and Aid for Families with Dependent Children (AFDC) waivers and Temporary Assistance to Needy Families (TANF) introduction.

establishment rates or marriage, and 4) operate on parental behavior *post-childbirth* — using data from the universe of U.S. birth records, I find no indication that IHVPE affects fertility or marriage *at the time of childbirth*, and these results are consistent with qualitative evidence that most unmarried parents are introduced to the program at the hospital at the time of giving birth (Martinez, 2011). These tests suggest that the identified relationships between IHVPE and the outcomes of interest are causal and not driven by other factors.

This paper contributes to a large literature on child support enforcement (e.g.: Garfinkel *et al.*, 1998; Freeman and Waldfogel, 2001; Aizer and McLanahan, 2006; Nepomnyaschy and Garfinkel, 2007, among others), which has not examined IHVPE specifically. The handful of papers on IHVPE have focused on a few states over short periods of time (Turner, 2001; Sorensen and Olivier, 2002; Mincy *et al.*, 2005a; Brown and Cook, 2008). Further, the existing literature has been limited in overcoming the challenges associated with isolating causal effects: IHVPE programs were implemented during a time of substantial growth in out-of-wedlock births, and observed changes in paternity establishment rates, family behaviors, and child well-being may be in part driven by compositional shifts in the distribution of births by unmarried mothers.

To my knowledge, my paper is the first to examine the impacts of IHVPE for a large number of states and years, and to use methods that can arguably identify true causal effects that are not confounded by other factors such as changes to the composition of out-of-wedlock births. By uncovering the causal effects of IHVPE, this paper sheds light on how policies that offer alternative contracts to marriage affect unmarried parents' decisions about their involvement with each other and their children. In particular, my results suggest that marriage is not the optimal choice for all parents: some parents prefer "intermediate" arrangements that outline partial parental rights and responsibilities for unmarried fathers. Therefore, policies based on the notion that more father involvement is essential to child and family well-being must account for the parents' agency in choosing their partners. More broadly, my findings highlight the empirical relevance of the trade-off between access to children and match quality — which has been long emphasized in family economics theory (e.g., Weiss and Willis, 1985; Edlund, 2011; Browning *et al.*, forthcoming) — in understanding the behavior of parents who have children out-of-wedlock.

The paper proceeds as follows. Section 2.2 presents the conceptual framework. Section 2.3 provides more information on IHVPE and reviews the relevant background literature, while Section 2.4 discusses the data sources and presents summary statistics. Section 2.5 discusses the empirical methods, and Section 2.6 presents the main results and robustness checks. Finally, Section 2.7 concludes.

2.2 Conceptual Framework

2.2.1 Overview

I provide a simple conceptual framework to guide interpretation of the possible impacts of a policy, such as IHVPE, which lowers the costs of entering a legal relationship contract that assigns partial parental rights and obligations to unmarried fathers.

Figure 2.1 depicts a decision tree outlining the set of relationship options that are potentially available to new parents. First, prior to childbirth (at node 1), individuals can choose to marry or not. At childbirth and in the subsequent short-term (at node 2), unmarried parents can either marry, establish paternity, or maintain no formal relationship. Each of the options at node 2 leads to further decisions that could occur by the time a child is five years old (the oldest age at which I observe children in my data): married parents can either stay married or get divorced; parents who establish paternity can get married or not; and parents who maintained no relationship at childbirth can still choose to either marry or establish paternity.

The boxes in Figure 2.1 show the predicted effects of lowering the costs of paternity establishment at childbirth through the implementation of IHVPE. While effects on marriage *before* childbirth are theoretically possible if parents anticipate IHVPE, my results suggest that this consideration is not empirically relevant — there is no effect of IHVPE on marriage before childbirth (see the discussion in Section 2.6 for more details). Consequently, I assume that decisions made prior to node 2 are unaffected by IHVPE. At node 2, as costs of paternity establishment are lowered, more parents are predicted to choose this option, leading to decreases in the proportions of parents who either marry post-childbirth or maintain no relationship.

The overall effect on parental marriage five years after childbirth is actually ambiguous — if paternity establishment is a "stepping-stone" to marriage (node 4), then an increase in marriage is possible once more parents establish paternity at birth. Further, if fewer parents marry in the short-term following childbirth and if this leads to an increase in average marriage quality, then divorce rates may fall, thereby increasing the proportion of parents who remain married five years after childbirth. Note that any *negative* effects on parental marriage are unambiguously driven by decisions made at node 2. As discussed in detail in Section 2.6, my empirical results point to a robust net negative effect on marriage among parents with children aged five years or less. Thus, to provide economic intuition for the findings in the data, my conceptual framework is focused on the decisions made at node 2.

More precisely, consider the three possible states at node 2: marriage (m), paternity (p), and no relationship (n). In each state $j \in \{m, p, n\}$, mothers (denoted by subscript x) obtain K_{xj} in utility from children, while fathers (denoted by subscript y) enjoy K_{yj} in utility from children. Since mothers have equal rights to their children regardless of the parental relationship, I assume that $K_{xm} = K_{xp} = K_{xn} = K_x$. In contrast, fathers have greater rights to their children in marriage than in paternity, and they have no rights in the state of no relationship: $K_{yp} = \alpha K_{ym}$ for some $0 < \alpha < 1$, and $K_{yn} = 0$.[10]

Couples are heterogeneous in match quality, θ, which is distributed according to a cumulative distribution function, $F(\theta)$, with support $(\underline{\theta}, \overline{\theta})$. I allow for θ to take on both positive and negative values; the negative values imply that some parents experience a *cost* from interacting with each other. The sensitivity of the parents' utilities to match quality depends on their level of interaction with each other: for states $j \in \{m, p, n\}$, each parent's marginal utility with respect to the θ is equal to δ_j, where $\delta_m = 1$, $\delta_p = \alpha$, and $\delta_n = 0$. In other words, the parents' sensitivity to match quality is directly proportional to the amount of parental rights that the father has (which determines their level of interaction).

Finally, I assume that there are fixed costs associated with both marriage and paternity establishment that are separate from match quality: c_m and c_p. Intuitively, one can think of the marriage cost, c_m, as incorporating the financial and non-pecuniary wedding-related costs, the legal and emotional costs of potential future divorce, as well as the option value costs associated with a limited ability to search for new partners. Further, individuals who experience greater negative social stigma associated with being an unmarried parent than others should face lower c_m, all else equal. The paternity cost, c_p, incorporates the informational, financial, time, administrative, and legal costs related to establishing paternity.

The parents' utility functions can be represented as follows:[11]

For the mother,

$$U_x = K_{xj} + \delta_j \theta - (1 - \gamma) c_j \qquad j \in \{m, p, n\}$$

[10]Note that the paternity state is similar to the "cohabitation" state described in Edlund (2011), although the framework presented here accommodates cases where a mother may not want to share a household with the father through cohabitation but can still assign some parental rights to him.

[11]I assume quasi-linear utility functions, which follows Edlund (2011) and Chiappori and Oreffice (2008), among others.

and for the father,

$$U_y = K_{yj} + \delta_j \theta - \gamma c_j \qquad j \in \{m, p, n\}$$

where $c_n = 0$ and $0 \leq \gamma \leq 1$. The parameter γ depicts how the parents share the costs of marriage and paternity establishment, and is exogenous to the model. We can represent the realizations of these utility functions in each of the possible states as follows:

	Marriage	**Paternity**	**No Relationship**
Mother	$K_x + \theta - (1-\gamma)c_m$	$K_x + \alpha\theta - (1-\gamma)c_p$	K_x
Father	$K_{ym} + \theta - \gamma c_m$	$\alpha K_{ym} + \alpha\theta - \gamma c_p$	0

2.2.2 Equilibrium Decisions About Parental Relationships

To solve for the equilibrium decisions in this model, I consider the joint surplus that the parents acquire in each of the three possible states:

$$\text{Marriage: } K_x + K_{ym} + 2\theta - c_m$$

$$\text{Paternity: } K_x + \alpha K_{ym} + 2\alpha\theta - c_p$$

$$\text{No Relationship: } K_x$$

Given their value of match quality, θ, parents will choose the state that maximizes their joint surplus. For each pair of states $\{j, k\} \in \{m, p, n\}$, it is possible to derive a value of θ at which parents are indifferent between the two options. Denote this threshold in θ as θ_j^k. It holds that:

$$\theta_p^n = \frac{c_p - \alpha K_{ym}}{2\alpha} \tag{2.1}$$

$$\theta_m^p = \frac{c_m - c_p}{2(1-\alpha)} - \frac{K_{ym}}{2} \tag{2.2}$$

$$\theta_m^n = \frac{c_m - K_{ym}}{2} \tag{2.3}$$

The relationship between θ and the optimal state for parents is as follows: no relationship if $\theta < \theta_p^n$ and $\theta < \theta_m^n$, paternity if $\theta < \theta_m^p$ and $\theta \geq \theta_p^n$, and marriage if $\theta \geq \theta_m^p$ and $\theta \geq \theta_m^n$.

Note that paternity will only be a feasible state if $\theta_m^p > \theta_p^n$, i.e., the match quality threshold between marriage and paternity is greater than the match quality threshold between paternity and no relationship. This condition will hold if $c_m > \frac{c_p}{\alpha}$, or the cost of marriage is sufficiently larger than the cost of paternity establishment. Further note that if $c_m > \frac{c_p}{\alpha}$, then it must be that $\theta_m^p > \theta_m^n > \theta_p^n$, which implies that conditions $\theta \geq \theta_m^n$ for marriage and $\theta < \theta_m^n$ for no relationship are non-binding. Then, the relationship between θ and the optimal parental relationship state can be simply represented as: no relationship if $\underline{\theta} < \theta < \theta_p^n$; paternity if $\theta_p^n \leq \theta < \theta_m^p$; marriage if $\theta_m^p \leq \theta < \overline{\theta}$. This is shown graphically in Figure 2.2.

The above condition implies that in the presence of heterogeneity in marriage costs in the population (for example, through heterogeneity in parental incomes, educational levels, cultural norms, etc.), paternity establishment is a relevant option for parents with relatively higher marriage costs. This claim is supported by empirical evidence from birth records data, which suggests that parents who have children out-of-wedlock (and for whom paternity establishment is therefore a relevant option) tend to be less educated and younger, and thus likely have fewer resources to cover the financial expenses of a wedding or a potential future divorce. Qualitative evidence on these parents suggests that many consider a wedding as an important prerequisite for marriage; they often aspire to save enough money to afford a "respectable" wedding someday (Edin and Kefalas, 2005). Further, many such parents disapprove of divorce, suggesting that they face higher stigma-related costs associated with being a divorced parent than with being a never-married parent. This evidence supports the claim that the parents in my study, for whom paternity establishment is a feasible option, likely face relatively high costs of marriage. Therefore, from here on, I assume that paternity establishment is a feasible option, i.e., $c_m > \frac{c_p}{\alpha}$.

2.2.3 Comparative Statics

IHVPE introduces an easily accessible and inexpensive way to establish paternity at childbirth along with widespread education of parents about fathers' rights and obligations. Consequently, I model the introduction of IHVPE as an exogenous decrease in c_p, the fixed cost of paternity establishment.

Consider what happens when c_p decreases. First, it holds that $\frac{\partial \theta_p^n}{\partial c_p} = \frac{1}{2\alpha} > 0$. Thus, if c_p falls, then θ_p^n will decrease as well. Hence, parents who were previously at the margin between no relationship and paternity will now choose paternity. In contrast, since $\frac{\partial \theta_m^p}{\partial c_p} = \frac{-1}{2(1-\alpha)} < 0$, as c_p falls, θ_m^p will rise. Consequently, parents who were previously at the margin between paternity and marriage will now also choose paternity.

These comparative statics imply that as the costs to establishing paternity are lowered, the range of match quality, θ, for which paternity establishment is the optimal choice widens, and more parents choose this option. The switchers into paternity include both parents who would have previously maintained no relationship and parents who would have previously married. Parents who are switching from no relationship to paternity are increasing their level of interaction, while parents who are switching from marriage to paternity are decreasing their level of interaction. This is shown graphically in Figure 2.3.

Note that the increase in θ_m^p also implies a change in the composition of both married and unmarried parents — the average match quality of unmarried parents ($\underline{\theta} < \theta \leq \theta_m^p$) and the average match quality of married parents ($\theta_m^p < \theta \leq \overline{\theta}$) will increase as θ_m^p rises.

2.2.4 Father Involvement

One can take two different approaches to model the impacts of IHVPE on father involvement with the family. In the collective bargaining framework, where marriage is modeled as a trade-off between economic gains and the non-pecuniary costs and benefits, children are commonly considered public goods (see Browning *et al.*, forthcoming, for a comprehensive review).[12] Such models imply that fathers' investments in children are higher in marriage than outside marriage: outside marriage, the non-custodial parent (the father) suffers a loss of control over the allocative decisions of the custodial parent (the mother), making it impossible for the parents to achieve a Pareto-optimal allocation of their joint resources, and resulting in under-provision of support from the father (Weiss and Willis, 1985).[13]

A different framework treats *rights* to children as private goods that generate transfers from fathers to mothers. This approach stems from the idea that marriage serves as a transfer of parental rights from mothers to fathers, which has long been emphasized in the anthropological and legal literature (Bohannan, 1949; Bohannan and Middleton, 1968; Grossbard, 1976; Posner, 1994; Edlund, 2006). In economics, this feature has been modeled explicitly by Edlund (1998, 2011), and has been used to explain: why prostitution is a well-paid profession (Edlund and Korn, 2002); why a political gender gap resulted from a decline in marriage and the subsequent decline in private transfers from men to women (Edlund and Pande, 2002); and the determinants of choice between co-parenting arrangements (Mincy *et al.*, 2005b).[14] The key insight from this framework is that fathers' transfers should be

[12] A related theoretical literature models marriage markets as matching equilibria (Becker, 1973, 1974, 1993; Mortensen, 1988; Roth and Sotomayor, 1992; Iyigun and Walsh, 2004; Choo and Siow, 2006; Chiappori *et al.*, 2006) or within search models (Burdett and Coles, 1997; Aiyagari *et al.*, 2000; Chiappori and Weiss, 2003, 2007).

[13] More recently, Chiappori and Weiss (2007) have shown that high expectations of remarriage can lead to an equilibrium in which divorced fathers commit to make more generous transfers as long as their ex-wives remain single. In related work, Aiyagari *et al.* (2000) constructed and simulated a model of the marriage market, where for certain parameters, an increase in mandated child support can raise overall welfare.

[14] There are several other papers that rely on this theoretical foundation to explain: the surplus of young women in urban areas resulting from the presence of high-wage men (Edlund, 2005); why women have higher status in individual-consent regimes where they are the recipients of the bride-price instead of their fathers (Edlund and Lagerlof, 2006); why an improvement in birth control technology increases the power of all women, including those who are not interested in the technology (Chiappori and Oreffice, 2008); why marriage affects returns to human capital differently for men and women (Saint-Paul, 2008); why the institution of marriage reduces cheating in society (Bethmann and Kvasnicka, 2011); and why humans predominantly live in families instead of in promiscuous arrangements (Francesconi *et al.*, 2010).

increasing with the amount of parental rights that they receive.

Although I do not explicitly model father involvement in my framework, it is straightforward to see that decreasing the cost of paternity establishment, c_p, should lead to an ambiguous effect on it. In particular, suppose that utility derived from children is a function of parental investment: for each parent $i \in \{x, y\}$ in each state $j \in \{m, p, n\}$, $K_{ij} = K_{ij}(I) = \beta_{ij} * K_i(I)$, i.e., for each parent, the benefit of investment varies depending on the state with some parameter β_{ij}, while $K_i(I)$ is constant across states. Assume that the benefit of investment, β_{ij}, is directly proportional to the degree of access to the child. Therefore, for mothers, $\beta_{xm} = \beta_{xp} = \beta_{xn}$, while for fathers, $\beta_{ym} > \beta_{yp} > \beta_{yn}$ (more precisely, $\beta_{yp} = \alpha\beta_{ym}$). In the simplest case of linear costs of investment (d), optimal investment will be determined by:

$$\beta_{ij} * K_i'(I^*) = d \qquad i \in \{x, y\}, j \in \{m, p, n\}$$

It follows that, consistent with both of the above theoretical approaches, fathers' investments should be higher in marriage than in paternity, and higher in paternity than in no relationship. Hence, following a decrease in c_p, families who switch from marriage to paternity should experience a decline in paternal investments, while families who switch from no relationship to paternity should experience an increase in paternal investments. Of course, this example does not rule out the possibility that with additional assumptions it may be possible to obtain clear-cut predictions about the net effect on father involvement. However, it shows that such assumptions are likely to be restrictive and that the net impact depends on the relative magnitudes of the two opposing forces: reduced involvement by switchers from marriage to paternity and increased involvement by switchers from no relationship to paternity.

2.2.5 Discussion

The conceptual framework is centered around a trade-off between match quality and access to children, which guides the decisions of parents regarding the type of relationship they want to maintain. Recent survey evidence suggests that this trade-off is especially relevant for the population of interest — parents who have children out-of-wedlock. Not all such parents want to get married, and mothers may actually be *less likely* to plan to marry than their children's fathers are. According to data from the Fragile Families and Child Well-Being Study, which follows a nationally-representative cohort of out-of-wedlock births in urban areas, only 74 percent of mothers predict that the chances of marrying their children's fathers are "50-50 or greater", in contrast to the 90 percent of interviewed fathers who predict a "50-50 or greater" chance of marriage. Similarly, while 77 percent of fathers "agree or strongly agree" with the statement that "marriage is better for kids", only 64 percent of mothers do (McLanahan *et al.*, 2003). This evidence suggests that unmarried mothers may be more sensitive to match quality (or, father quality) than unmarried fathers are. Empirically, I will focus on outcomes measuring father quality, as these are the best available proxies for match quality in my data.[15] Additionally, unmarried fathers' positive attitudes towards marriage suggest that they may in fact value having parental rights to their children.

Other sociological research provides further context. Recent work suggests that many poor women choose to have children outside marriage because of the high value they place on their roles as mothers against the backdrop of dire circumstances that present them with few other meaningful opportunities (Edin and Kefalas, 2005). These women do not reject marriage overall — on the contrary, marriage is a revered goal of lifetime commitment that should occur with the right person and at the right time. However, unmarried fathers, many

[15] The predictions of my framework would be the same if I modeled heterogeneity in father quality instead of match quality.

of whom relish the relationships with their children and view having parental rights as vital to their roles as men (Roy, 1999; Pate, 2002), are more favorably disposed to the idea of marriage and are more likely to raise the question of marriage than unmarried mothers are (Edin and Kefalas, 2005). Many women are cautious about marriage as they do not want to commit to something that could jeopardize their well-being and the well-being of their children. The men in their lives are often involved in criminal behavior, and exhibit patterns of "intimate violence, chronic infidelity, and an inability to leave drugs and alcohol alone" (Edin and Kefalas, 2005), and thus do not constitute ideal partners. For these women, the meaning of marriage has changed over the last few decades from being an institution primarily about childbearing and childrearing to being an elusive dream of personal fulfillment. It is plausible that the introduction of an intermediate relationship contract option that outlines fathers' partial rights and obligations outside marriage has contributed to this change: mothers no longer feel the need to rely on support from their children's fathers within marriage and can decline marriage offers in hopes of better future partners and life circumstances, while fathers can expect to receive some rights and responsibilities to their children without marriage.[16]

In summary, my framework yields four predictions: 1) IHVPE should increase paternity establishment rates by both decreasing the paternity threshold in match quality, θ_p^n, and raising the marriage threshold in match quality, θ_m^p; 2) IHVPE should reduce marriage rates through the increase in the marriage threshold in match quality, θ_m^p; 3) there should be a change in the composition of both the samples of married and unmarried parents: as θ_m^p rises, the average match quality of both married and unmarried parents should rise; and

[16]It is important to note that Edin and Kefalas (2005) conduct their study on 160 especially disadvantaged single mothers in inner-city Philadelphia. Consequently, these women's experiences are probably not representative of the experiences of average women, or even all unmarried mothers likely affected by IHVPE programs. However, given that 30 percent of single mother households live below the poverty line (U.S. Census Bureau, 2010), it is likely that a substantial fraction of the mothers in my data share similar experiences and attitudes with the poor women interviewed by Edin and Kefalas. Nevertheless, Edin and Kefalas (2005) provide, at least, anecdotal motivation for studying the effects of alternative parental contracts on subsequent marriage and family behavior.

4) there should be a decrease in paternal involvement among switchers from marriage into paternity and an increase in paternal involvement among switchers from no relationship to paternity, with an ambiguous net effect.[17]

2.3 Background on In-Hospital Voluntary Paternity Establishment and Other Related Literature

Without legal paternity establishment, unmarried fathers essentially have no rights or obligations with regard to their children. Paternity establishment grants fathers rights to request partial custody and visitation privileges from the court, to refuse requested adoptions, and to block foster care placements. Importantly, paternity establishment also allows mothers to seek a court order that obligates fathers to make child support payments.

In the 1970s and 1980s, paternity establishment was a relatively uncommon and costly process that occurred through the court system, and most paternities were only established several years after the child's birth, if ever (The Office of Child Support Enforcement, 1996). To address this issue, the Omnibus Budget Reconciliation Act (OMBRA) of 1993 required all states to implement IHVPE programs, and the 1996 Personal Responsibility and Work Opportunity Act (PRWORA) reinforced this requirement. Policymakers speculated that IHVPE programs would be effective as they attempt to reach families during the "happy hour" in the hospital following the birth of the child and encourage the father to stay involved in his family's life (U.S. Department of Health and Human Services, 1997a). As a result, all states have initiated an IHVPE program, in which all hospitals and birthing centers are required to provide adult unmarried new mothers and fathers with an opportunity to sign

[17]One can see that the net effect on child well-being is also ambiguous. On the one hand, children whose parents would have previously maintained no relationship will benefit from increased paternal investments. On the other hand, children whose parents would have been previously married may experience both welfare decreases due to lower paternal investments and welfare increases as a result of residing with mothers who are happier from less interaction with low-quality partners.

a voluntary paternity acknowledgement form. Both unmarried parents have to be present at the hospital to participate in IHVPE.[18] State child support agencies are required to make available materials for educating parents, and hospital staff must provide mothers and fathers with both written materials and oral explanations regarding the rights and responsibilities related to paternity establishment. Importantly, IHVPE involves no DNA testing for paternity — paternity is legally established after both parents sign the voluntary paternity acknowledgement form.

Despite the federal mandate, the administration of the in-hospital paternity acknowledgement process was mostly under state discretion. Variation in the timing of IHVPE implementation across states stems largely from the length of time that it took to forge relationships between state child support agencies, vital statistics registries, and hospitals (U.S. Department of Health and Human Services, 1997b). By 1997, 37 states reported full implementation of IHVPE, while the rest listed reasons such as "too early for the [office of child support] staff to have contacted every state birthing hospital" to explain the delays (U.S. Department of Health and Human Services, 1997b). The identification of the causal effects of IHVPE programs on paternity establishment and family behavior requires the assumption that the timing of implementation is uncorrelated with other time-varying determinants of these outcomes. This assumption is explored in detail in Section 2.6, with evidence suggesting that unobserved state time-varying omitted variables should not pose serious issues.[19]

While a unified source of information on the timing of IHVPE program implementation

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[18] According to data from the Fragile Families and Child Well-Being Study, over 1998-2000, 76 percent of unmarried mothers reported that the child's father came to the hospital at the time of the child's birth.

[19] Additionally, while it may be the case that early IHVPE implementers have more efficient administrative processes and more organized existing networks across state agencies, time invariant differences in these characteristics are absorbed by the inclusion of state fixed effects, while differences in linear trends should be accounted for by the inclusion of state-specific time trends.

across states does not exist, I obtained information on the year (and month if available) of program implementation from searches of state legal statutes on *LexisNexis Academic*, internet searches of state paternity programs, and direct conversations with officials at state child support agencies and IHVPE programs for most states. Additionally, as Nepomnyaschy and Garfinkel (2007) have collected this information for several states, I use their data as well. Figure 2.4 shows the variation in the timing of IHVPE program implementation across states, while Appendix Table B.1 presents more details for each of the 44 states in my data.[20] Births in these states account for about 96 percent of all births in the United States over the time period of analysis. Figure 2.5 plots the trend in the total number of paternities established in the United States over 1992-2007, and the substantial increase from about 600,000 to over 1.5 million in the late 1990s coincides with the time when most states implemented IHVPE programs.

The implementation of numerous child support enforcement measures (which include IHVPE programs, as well as automatic wage withholding, the new hires directory, and license revocation for non-payment among others) throughout the 1980s and 1990s across states created a "natural experiment" for researchers to study their overall effects. As a result, there is a wealth of literature that focuses on the effects of child support enforcement on numerous family and child outcomes (see Garfinkel *et al.*, 1998 for a review).[21] More recent research has considered the effects of child support enforcement on child support

[20]I do not have data for the following states: IA, MT, NH, NM, OK, WV, WY.

[21]The main conclusions that arise from studies of the 1980s and early 1990s are that 1) child support enforcement tends to increase father-child interactions and father influence in child support rearing, and 2) child support enforcement decreases the likelihood of remarriage and subsequent out-of-wedlock births for low-income non-resident fathers. It is important to note that studies in this review consider the effects of child support enforcement on remarriage of divorced non-resident fathers rather than effects on first-time marriage for never-married fathers. These studies generally focus on the 1980s and early 1990s — a time period prior to widespread paternity establishment for fathers who are unmarried at the time of childbirth. Hence, effects for never-married fathers are rarely considered as child support enforcement cannot affect them if they do not establish paternity.

payments (Freeman and Waldfogel, 2001), the characteristics of consequent out-of-wedlock births (Aizer and McLanahan, 2006), abortion (Crowley *et al.*, 2009), and domestic violence (Fertig *et al.*, 2007).

Most studies in this literature do not consider child support policies from the perspective of providing an "intermediate" option for parental interaction between the "extremes" of no relationship and marriage.[22] IHVPE is a particularly striking example of such an "intermediate" option, since paternity establishment delineates *both* rights and responsibilities for new unmarried fathers. This feature of IHVPE contrasts with other child support enforcement measures that usually just punish fathers for non-payment and do not explicitly address their parental rights. Yet rigorous research on the causal effects of IHVPE is quite sparse. The existing literature is limited to several reports on individual state programs (for example, Pearson and Thoennes, 1996; Ovwigho *et al.*, 2007; Brown and Cook, 2008; Wisconsin Bureau of Child Support, 2010) and analyses of a few states and over short periods of time (Turner, 2001; Sorensen and Olivier, 2002; Mincy *et al.*, 2005a). In a study most closely related to this one, Mincy *et al.* (2005a) find that establishing paternity in the hospital is associated with increased formal and informal child support payments and father-child visitation among children born out-of-wedlock using data from the Fragile Families and Child Well-Being Study. However, they do not exploit the state-year variation in program

[22]A related large strand of empirical literature studies how various welfare policies incentivize marriage and out-of-wedlock childbearing. Standard economic theories have clear predictions that greater financial benefits for single mothers should reduce marriage (Becker, 1993). Empirical studies of the effects of welfare generosity yield mixed results. Some studies of the effects of welfare reform on marriage find that the reduced generosity of the reform led to an increase in marriage (Schoeni and Blank, 2000; Bitler *et al.*, 2006), others find a negative effect on marriage (Rosenbaum, 2003; Bitler *et al.*, 2004; Fitzgerald and Ribar, 2005), while still others find insignificant effects (Ellwood, 2000; Kaestner and Kaushal, 2005). In a recent study using data from the Fragile Families and Child Well-Being Study, Knab *et al.* (2008) find that more generous welfare benefits are associated with a reduction in parental marriage post-childbirth. This finding is similar in spirit to my results, as an increase in welfare benefits leads to higher income for mothers outside marriage, which may allow them to increase the marriage threshold in match quality. However, a limitation of the Knab *et al.* (2008) study is that the authors are only able to rely on cross-city variation in welfare generosity, and thus their results cannot be readily interpreted as causal.

implementation and instead rely on cross-sectional and cross-city variation in in-hospital paternity establishment rates, which could be correlated with other factors that affect family well-being. Thus, despite controlling for a wide range of observable characteristics, their work is limited in its ability to establish a causal effect due to potential omitted variables bias.

2.4 Data and Summary Statistics

2.4.1 Paternity Establishment Data

Data on the number of paternities established over 1992-2005 in each state and year come from OCSE reports. Beginning in 1996, each report contains a table on the total number of paternities established in each state for five consecutive years (i.e., the 1996 report contains information for 1992-1996). Unfortunately, there is no concrete information on the number of paternities established in-hospital for all states.[23] However, since IHVPE programs can only affect paternity establishment rates at the hospital, one can interpret the changes in the total number of paternity establishments following IHVPE implementation as being driven by changes in in-hospital paternity establishments.[24] For the analysis, I use paternity data for the 43 states for which I have information on the year of IHVPE initiation and which initiated their programs in 1993 or later, which results in 601 state-year observations.[25]

[23]Some OCSE reports contain a table on in-hospital paternity establishments, but these data come from voluntary reports by states and the information is missing for many states and years.

[24]In fact, in the long run, we should expect paternity establishment rates outside the hospital to decrease as a result of IHVPE programs, as some families that would have established paternity later on instead establish it at the time of the child's birth.

[25]I exclude Washington, which initiated its IHVPE program in 1989. Additionally, Nevada is missing data on paternity establishments in 2000, so I exclude this state-year observation.

2.4.2 CPS-CSS Data

To analyze the effects of IHVPE programs on marriage behavior and some measures of father involvement, I use data from the biannual March/April matched CPS child support supplements (CSS) from 1994 to 2008. More details on the sample construction are presented in Appendix B.1. These data include households that were surveyed both in the March annual demographic file and in the monthly April CPS. In April, in addition to the standard CPS questions, all members of a household aged 15 and above who have a child in the household with a parent that lives outside the household are asked detailed questions regarding child support agreements, payments, and the involvement of the other parent.

My main analysis sample consists of all mothers with a youngest child aged five years or less in the household.[26] Since there is some variation in how minors are treated in IHVPE programs, I further limit the sample to mothers aged 18-45 at the time of childbirth, and drop all mothers who moved from outside the U.S. in the last year.[27] These restrictions leave me with $38,449$ mothers of youngest children aged five years or less in the CPS-CSS data, out of which $8,974$ are asked the CSS questions.

In this sample, a mother is categorized as married to the biological father if she is married and her child is coded as living with both parents in the household. A mother is categorized as married to someone other than the biological father if she is married, but the child is coded as living with only a mother in the household. Mothers who are married to the biological father are by construct ineligible to be asked CSS questions.

[26]I focus on the age of the *youngest* child because I want to examine the relationship between family outcomes observed at the time of the survey (such as marriage, father involvement, etc.) with IHVPE presence at the time of the *most recent* birth.

[27]In 2004, the following states had special provisions restricting participation for minors: CA, DE, IL, KS, KY, TN, TX, UT, VA, WI, and WY (Roberts, 2004).

2.4.3 March CPS Data

For some analyses, I take advantage of the larger sample sizes in the March CPS annual demographic supplement files (King *et al.*, 2010) relative to the CPS-CSS. I use March CPS data for 1989-2010, and follow the same method as in the CPS-CSS to link mothers to their youngest children and to calculate their children's birth years (see Appendix B.1). As before, I limit my sample to mothers aged 18-45 at the time of childbirth and drop all mothers who moved from outside the U.S. in the last year. The resulting sample size is $212,504$ women with youngest children aged five years or less in the household.[28]

2.4.4 NHIS Data

To examine the effects of IHVPE on child mental and physical health and access to care, I use the restricted version of the 1997-2010 Sample Child files of the NHIS with state identifiers.[29] These data contain detailed information on numerous parent-reported measures of health and access to care together with information on the state of residence and the year and month of birth for a randomly picked child within each NHIS sample household. I limit the analysis sample to mothers aged 18-45 at the time of childbirth, who reside with a sample child in the household aged 7 years or less.[30]

Additionally, these data contain more detailed information on household relationships

[28] Results using only 1994-2008 March CPS data are very similar to the ones presented in this paper.

[29] More information regarding access to restricted NHIS data is available here: http://www.cdc.gov/rdc/.

[30] The age cut-off is higher than the one used in the CPS analysis because the NHIS data span a later time period. Thus, to retain more children who were born pre-IHVPE initiation, I include slightly older children aged 6 and 7 years. Note that sample children are not necessarily the youngest children in the household. This complicates the analysis, as sample children born prior to IHVPE implementation may have younger siblings who were affected by IHVPE leading to spill-over effects on children that I consider untreated. To address this issue and for comparability with analysis using CPS-CSS and CPS data, I have estimated models limiting the sample to sample children who are the youngest in their households. The results from this analysis are very similar to the results from using all the sample children, although less precise due to sample size reductions.

than the CPS. Specifically, respondents are asked direct questions regarding cohabitation with unmarried partners. Consequently, I use these data to study the effects of IHVPE on the likelihood of parental cohabitation and the likelihood of the mother cohabitating with someone other than the child's father.

For confidentiality reasons, actual sample sizes from these data cannot be released. In the public-use version of the analysis sample that contains all states, the sample size is about $67,100$ mothers of sample children aged 7 years or less. This is a reasonable approximation of the size of the true analysis sample, which omits data from seven relatively small states.

2.4.5 Summary Statistics

Table 2.1 presents the sample means of key variables in each of the data sets used in my analyses.[31] The state-year means are weighted by state-year populations, while means of variables in the individual-level data sets are weighted using the provided sample weights.

The average ratio of paternities established to unmarried births is 0.89. Note that this average is an overestimate of the proportion of unmarried births with paternities established in-hospital, as my data are on paternities established for *all* children in each state and year (and not just newborns). Remarkably, there is a striking difference in this ratio pre- and post-IHVPE: the average among state-year cells without IHVPE is 0.49, while the average among state-year cells with IHVPE is 1.07.

The CPS-CSS data shows that about 78 percent of mothers with children aged five years or less are married — 77 percent are married to their children's fathers, while one percent are married to someone else. Sixty-three percent of mothers are non-Hispanic white, while

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[31] Washington was the first state to implement IHVPE in 1989 (see Appendix Table B.1). Since the state-year data on paternity establishment rates is available for 1992-2005, while the NHIS sample includes children born in 1989 or later, I exclude Washington from analyses with these data sets. In the CPS-CSS and the March CPS, I have observations on children born in 1988 or later, and therefore include mothers from Washington. Thus, the total number of states in the paternity establishment rate and NHIS analyses is 43, while the total number of states in the CPS-CSS and March CPS analyses is 44.

14 percent are black and 17 percent are Hispanic. Overall, about 88 percent of children have any health insurance coverage, with 67 percent having private coverage. Among mothers who are asked CSS questions, child support receipt rates are fairly low: in the year prior to the survey, only 36 percent received any child support payments, while only 22 percent received all of the child support that was due. However, about 70 percent of mothers state that the father has legal visitation rights, and non-resident fathers spend an average of 60 days out of the year with their children.

Splitting the sample by whether or not IHVPE exists in a given state and child's birth year demonstrates that there are some important differences. Most notably, children born in states and years with no IHVPE program tend to be older because more states implemented IHVPE programs as time went on, and so older children are more likely to have been born in a state and year without a program. I therefore include fixed effects for child's age (in years) in all specifications. In the crude comparison of state-year cells that do and do not have IHVPE programs, mothers in state-year cells with IHVPE programs are actually more likely to be married to their children's fathers. However, more rigorous regression analysis shows that as soon as state and year fixed effects are included, the effect is actually in the opposite direction. This discrepancy highlights that omitted variables bias likely poses problems in cross-sectional analyses of IHVPE programs.

2.5 Empirical Methods

In an ideal research setting, one would identify the causal effect of IHVPE by randomizing families into program treatment and control groups, and then comparing the outcomes of the two groups. However, absent such a design, I must rely on quasi-experimental variation in the timing of program initiation. This method depends on the assumption that the state-year variation in timing of IHVPE implementation is uncorrelated with other time-varying determinants of the outcomes of interest. While this assumption is not directly testable, in subsequent sections I present several indirect tests that suggest that this identification strategy is reasonably reliable for estimating causal effects of IHVPE.

Using state-year paternity establishment data from 43 states, I estimate a "first-stage" relationship between paternity establishment rates and IHVPE programs:

$$PropPat_{sy} = \beta_0 + \beta_1 * IHVPE_{sy} + \gamma' X_{sy} + \phi' C_{sy} + \mu_s + \alpha_y + \delta_s * y + \epsilon_{sy} \tag{2.4}$$

for each state s and year y. $PropPat_{sy}$ is the ratio of paternities established to the number of unmarried births in state s and year y,[32] $IHVPE_{sy}$ is an indicator for whether an IHVPE program is operating in state s in year y, X_{sy} is a vector of maternal and child characteristics, including the proportion births by white, black, and Hispanic mothers, the proportion male births, and the proportion births by mothers in different educational (less than high school, high school degree, some college, college or more) and age (less than 20, 20-24, 25-34, 35-44, 45+ years) groups. C_{sy} is a large vector of other state time-varying characteristics, including the state unemployment rate, the state minimum wage rate, the state poverty rate, the average AFDC/TANF benefit for a 4-person family, the proportion of the population receiving welfare benefits, the proportion of the population receiving Medicaid benefits, an indicator for whether the state's governor is Democratic, and the fraction of the state house that is Democratic in the year before, as well as indicators for whether different child support enforcement laws (automatic wage withholding, genetic testing for paternity establishment, a new hires directory, and license revocation for non-payment) are in effect, indicators for whether a state EITC has been enacted, and indicators for whether an AFDC waiver or the

[32] As discussed in Section 2.6, results using log number of paternities as the dependent variable and controlling for unmarried births are very similar.

TANF program have been implemented.[33] μ_s is a state fixed effect, α_y is a year fixed effect, $\delta_s * y$ is a state-specific time trend, and ϵ_{sy} is a state-year error term. Note that the inclusion of state and year fixed effects allows me to control for any time-invariant state-level variables and overall time trends that might affect paternity establishment rates. Further, the inclusion of state-specific time trends allows me to account for differential linear trends in paternity establishment rates across states over the time period of analysis.[34] The key coefficient of interest is β_1, which measures the effect of IHVPE on the paternity establishment rate.

The analyses of the CPS-CSS, March CPS, and NHIS data are on the individual level instead of the state-year level. Consequently, I estimate the following equation:

$$Y_{isty} = \beta_0 + \beta_1 * IHVPE_{sy} + \gamma' X_{isty} + \phi' C_{st} + f(t) + \mu_s + \alpha_y + \delta_s * y + \epsilon_{isty} \tag{2.5}$$

for each mother i, in state s, survey year t, with a youngest (or sample) child born in year y. Here, Y_{isty} is an outcome of interest, such as an indicator for whether the mother is married to the father of her child. In this specification, X_{isty} contains individual maternal and child characteristics, including indicators for maternal age group at childbirth, indicators for maternal education groups, indicators for maternal race, an indicator for child sex, and indicators for the child's single years of age. I include state and child birth year fixed effects, as well as state-specific time trends, as before. Additionally, since including indicators for

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[33]Maternal and child characteristics come from the National Center for Health Statistics (NCHS) Vital Statistics microdata from the universe of birth certificates in the United States over 1992-2005, collapsed into state-year cells. Data on various economic and program transfer variables come from a database maintained by the University of Kentucky Center for Poverty Research. Data on child support laws come from Nepomnyaschy and Garfinkel (2007) for states that established these policies prior to 1994, and from my own searches of state statutes for the other states using *LexisNexis Academic*. Data on state EITC (tax credits that supplement the federal EITC program) come from the Tax Credits for Working Families organization (see http://www.taxcreditsforworkingfamilies.org/ for more information). Information on AFDC waivers and TANF implementation comes from Table 1 in Bitler *et al.* (2006).

[34]In Section 2.6, I show that the results are not sensitive to the inclusion of the state time-varying controls and state-specific time trends.

the survey year induces multicollinearity with indicators for the child's age and birth year, I include a quadratic polynomial in the survey year instead. All the state time-varying controls are the same as in equation (2.4). Again, the key coefficient of interest is β_1, which measures the effect of the existence of an IHVPE program in the child's state and year of birth on the outcome of interest.[35]

2.6 Results

2.6.1 Effects of IHVPE on Paternity Establishment Rates

I first present some graphical evidence on the relationship between IHVPE program implementation and the paternity establishment rate. Figure 2.6 plots the average ratio of paternities established to unmarried births by the number of years from IHVPE program initiation. The figure assigns year 0 as the first year the program is in effect. I assume this year is the same as the year listed in Appendix Table B.1 if only the year is listed or if the month of initiation is June or earlier. If the month of initiation is known and it is July or later, then I assume the first year the program is in effect is the following year. The figure shows a substantial jump in the paternity establishment rate at year 0. Note that the above timing assignment allows for a possibility of a muted effect at year -1, which is also evident

[35]Note that since the CPS-CSS, March CPS, and NHIS data do not contain information on the child's state of birth, I assign the child's state of residence in the year before the survey as the child's state of birth (for non-movers, this variable is also the state of residence in the year of survey). This may be a problematic assumption if IHVPE program implementation is correlated with the likelihood of a mother moving out of her child's state of birth. For example, it may be the case that IHVPE programs have an effect on the likelihood of the mother moving in or out of the state where the father lives. However, I can test this directly in the CPS-CSS, and find no statistically significant effect of IHVPE on the likelihood of the father living in the same state as the mother and child at the time of the survey. (Specifically, the key coefficient from estimating equation (2.5) with an indicator for the father living in the same state as the child as the dependent variable is -0.0088 with a standard error of 0.0074.) Additionally, similar mobility assumptions are often used in the literature that studies the long-run effect of prenatal and early childhood interventions — in fact, an individual's county of residence during high school has been assumed to be his/her county of birth or of residence during early childhood (Ludwig and Miller, 2007; Sanders, Forthcoming). Such assumptions are likely subject to more measurement error than the assumption that I rely on in this paper.

in the graph.[36] Unfortunately, lack of earlier paternity data prevents me from creating a graph in which all sample states have pre- and post-treatment periods of the same length. As a result, in Figure 2.7, I limit my analysis to states that initiated IHVPE in 1996 or later, and include 4 years before and 7 years after program initiation for each state, effectively creating a symmetric graph in which equal numbers of states contribute data to each point. The pattern in Figure 2.7 is similar to the one in Figure 2.6 suggesting that the lack of a balanced panel in the full dataset should not pose serious problems.

Table 2.2 presents regression results on the effects of IHVPE programs on the paternity establishment rate, which support the graphical evidence. In this table, the units of observation are state-year cells, robust standard errors are clustered on the state level, and all regressions are weighted by state-year populations.[37] The results suggest that IHVPE implementation led to a 20.2 percentage point increase in the paternity establishment rate — a 40 percent increase at the pre-IHVPE mean.[38] Notably, once controls for maternal and child characteristics and state and year fixed effects are included (column 1), the inclusion of state time-varying characteristics, controls for child support laws, state EITC, and AFDC/TANF implementation, and state-specific linear time trends in columns 2-5 does not substantially alter the key coefficient of interest, providing some support for the validity of

[36] I plot paternities as a ratio relative to unmarried births instead of plotting the total number of paternities established because different numbers of states contribute to different points along the x-axis, and it is therefore important to control for the underlying population that could potentially be affected by IHVPE at each point. Although using the percentage of births by unmarried mothers as the denominator would be problematic if IHVPE programs had an effect on the likelihood of marriage *at the time of birth*, my results show that there is no statistically significant association between IHVPE and the proportion of unmarried births (see Table 2.4 and the discussion in Section 2.6.2). Indeed, the effects on marriage seem to be driven by parental behavior post-childbirth, as most parents only learn about the program at the hospital at the time of their child's birth.

[37] The unweighted regressions yield very similar results, which are available upon request.

[38] Results using log number of paternities established as the dependent variable yield similar magnitudes and are available upon request.

the identification strategy.[39]

My analysis relies on the assumption that the treatment and comparison states would have had similar trends in outcomes in the absence of IHVPE introduction. To assess the validity of this assumption, I present results from a regression where I include indicators for 3 years before IHVPE program initiation in Table 2.3. We should not expect to see significant differences in paternity rates in the years prior to IHVPE initiation. Reassuringly, in the fully-specified model, none of the coefficients on the indicators for years before IHVPE is statistically significant at the 5% level while the key coefficient of interest remains positive, large, and statistically significant. These results suggest that differential trends in paternity establishment rates prior to IHVPE should not pose serious concerns.

To further check the first stage effects on paternity establishment rates, I turn to a different source of data — micro data from the universe of U.S. birth certificates. An in-depth discussion of the issues in these data and the estimation methods that I use is available in Appendix B.2. I find that IHVPE leads to an increase in the likelihood that a father's information is listed on his child's birth certificate (a proxy for paternity establishment). This finding is reassuring because it implies that my results on paternity establishment rates are robust across different data sets and methods.

2.6.2 IHVPE Program Initiation and Other State Time-Varying Factors

The crux of my identification strategy relies on the assumption that the timing of IHVPE program implementation across states is uncorrelated with changes in other factors that are not captured by a linear time trend. While I cannot rule out the possibility that there are some unobservable variables that fail this assumption, the evidence suggests that this

[39]The sample size changes once controls for state time-varying characteristics are added as some of the variables are missing for certain state-year cells, and because I am missing data on the year of implementation for some child support laws for Kentucky and South Dakota.

is unlikely. In particular, Table 2.4 presents the β_1 coefficients from regressions that use various maternal, child, and state time-varying characteristics as dependent variables in the estimation of equation (2.4) with state and year fixed effects, and state-specific time trends, but without any other controls.[40] Out of 18 coefficients, none is statistically significant at the 5% level. The only marginally significant coefficient is a positive effect on the likelihood of the mother being black.[41] The timing of IHVPE program initiation is uncorrelated with numerous factors, including the log number of births, the educational and age distributions of mothers, state economic, political, and program transfer variables, the timing of AFDC/TANF legislation, the timing of state EITC programs, and the timing of other child support laws. Further, in additional results available upon request, I show that there is no effect of IHVPE initiation on pregnancy behaviors or birth outcomes.[42] Since IHVPE programs reach parents at the hospital immediately following childbirth, we should not expect to see any effects of IHVPE on these outcomes, and the empirical evidence reassuringly suggests that this is the case.

Perhaps most importantly, IHVPE program initiation is uncorrelated with the proportion of births by unmarried mothers as recorded in data on the universe of all U.S. births. This result is critical since I show below that IHVPE programs decrease the likelihood of marriage in the CPS-CSS and March CPS data. A potential concern with this finding is that it may be driven by selection — i.e., states that implemented IHVPE programs earlier also had

[40]Results from regressions without time trends are very similar.

[41]Minority mothers have higher rates of births out-of-wedlock - for example, in 2009, while overall, 41 percent of all births were by unmarried mothers, 71 percent of all births by black mothers were by unmarried mothers (National Center for Health Statistics, 2011). Thus, one concern might be that the negative effect on marriage in the CPS-CSS data is spuriously driven by the relative increase in black mothers. However, as shown below, the effect on marriage is robust to the exclusion of black mothers.

[42]There are no statistically significant effects on any of the following outcomes: 1st trimester prenatal care initiation, child sex, mother's weight gain during pregnancy, birth weight, indicator for low birth weight (<2500g), indicator for very low birth weight (<1500g), gestation length, any complications during labor or delivery, and any abnormal conditions of newborn.

higher growth rates in unmarried births. However, the fact that I find no effect of IHVPE programs on the proportion of births by unmarried mothers (if anything, the insignificant coefficient has the opposite sign of what would be consistent with selection), implies that the effects on marriage operate through behavior post-childbirth rather than selection.

One might also imagine that if knowledge about IHVPE spreads, individuals may change their marriage behavior prior to childbirth, in anticipation of IHVPE. This would imply that we should expect decreases in marriage rates *at childbirth* in years following IHVPE implementation. However, results from estimating a variant of equation (2.4) for the proportion of unmarried births as the dependent variable with a flexible specification that includes indicators for 5 years after IHVPE initiation suggest that this is not the case. There are no statistically significant coefficients on any of the indicators for years after IHVPE initiation (results available upon request). These results are consistent with anecdotal evidence from IHVPE program administrators that most people are unaware of the existence of IHVPE until the time of childbirth.[43]

Additionally, the fact that I find no correlation between IHVPE program initiation and the AFDC/TANF benefit in the previous year, the timing of AFDC/TANF legislation, or the timing of state EITC implementation suggests that the effects on marriage and labor supply are not driven by confounding impacts of welfare or tax credit generosity. Finally, the lack of correlation between IHVPE implementation and the percentage of the population on Medicaid in the previous year implies that the effects on child health insurance provision are not driven by changes in public health insurance generosity.

[43] I have also estimated regressions for maternal marital status at birth separately for first- and higher-parity births. I find no statistically significant effects for either group. While mothers giving birth to higher-parity children are perhaps more likely to know about IHVPE before the time of childbirth, the prediction for their marital status at birth is unclear. On the one hand, they may be less likely to be married if they anticipate the benefits of the "intermediate" IHVPE option and make their marriage decision prior to giving birth. On the other hand, consistent with results discussed below, women giving birth to higher-parity children may be more likely to be married to new partners who are not the fathers of their prior-born children.

Taken together, these findings suggest that the timing of IHVPE implementation is likely uncorrelated with other determinants of paternity establishment rates and family behavior, and hence can be used as a valid natural experiment for identification of causal effects.

2.6.3 Effects of IHVPE on Marriage

After confirming that IHVPE programs in fact lead to a substantial increase in paternity establishment rates, I turn to analysis of marriage behavior in the CPS-CSS data. Note that the assignment of treatment in the CPS-CSS and March CPS data sets is complicated by the fact that I do not observe children's years or months of birth. Therefore, as discussed in Appendix B.1, I assign the child's approximate birth year as *birth year* = *survey year* − *child age* − *1.* Since the surveys are conducted in March, this procedure assigns the correct birth years to children born in April-December, and incorrectly assigns the years immediately prior to their true birth years for children born in January-March. Consequently, some children who are affected by IHVPE may be erroneously assigned to the pre-treatment group. In addition, children born in months prior to the month of program implementation in any given year may be unaffected by IHVPE but erroneously assigned to the treatment group. As a result, these data allow for three possible treatment statuses: "pre-treatment" = *IHVPE first year* − *child's birth year* ≤ -2, "maybe-treatment" = *IHVPE first year* − *child's birth year* $\in \{-1, 0\}$, and "treatment" = *IHVPE first year* − *child's birth year* ≥ 1.

Figure 2.8 depicts these three possible treatment statuses and also presents some graphical evidence of the effects of IHVPE on parental marriage. I plot the coefficients from estimating an event-study version of equation (2.5) that includes indicators for 5 years before, the year of, and 5 years after IHVPE implementation relative to the child's approximate birth year, with the corresponding 95% confidence intervals. The omitted category is *IHVPE first year*

$-$ *child's birth year* $= -2$, the first year of sure pre-treatment.[44] The figure shows that in the certain pre-IHVPE period (year ≤ -2), there are no statistically significant trends in marriage rates. There is a slight decline in marriage in the "maybe-treatment" period (at year $= -1$ and year $= 0$), although it is not statistically distinguishable from zero at year $=$ -1, and barely statistically distinguishable from zero at year $= 0$. In contrast, in the sure post-IHVPE period (year $= 1$ and onwards), there is a clear drop in parental marriage, and the coefficients are consistently negative and statistically significant at the 5% level.

To check whether "maybe-treatment" is an important category, I have estimated regressions with separate indicators for "maybe-treatment" and "treatment" in supplementary analyses. The coefficients on "maybe-treatment" were consistently insignificant and smaller in magnitude than those on "treatment", while the coefficients on "treatment" remained very similar to those estimated with regressions where the "pre-treatment" and "maybe-treatment" groups are pooled into one. Since pooling the "maybe-treatment" group with the "pre-treatment" group creates more power in my analysis without substantially changing the coefficients of interest, I focus on results from these pooled regressions from here on.[45] If anything, the magnitudes of the results from these regressions may be slightly biased downwards as some potentially treated families are assigned to the pre-IHVPE period.

Table 2.5 presents results from estimating equation (2.5) on the analysis sample of mothers with a youngest child aged five years or less, which confirm the graphical evidence. All regressions are weighted by CPS person weights, and robust standard errors are clustered

[44]Specifically, I estimate: $Y_{isty} = \beta_0 + \sum_{k=-5}^{-3} \theta_k * IHVPE_{syk} + \sum_{k=-1}^{5} \theta_k * IHVPE_{syk} + \gamma' X_{isty} + f(t) + \mu_s + \alpha_y + \epsilon_{isty}$, where $IHVPE_{syk}$ is an indicator for k years from IHVPE implementation in state s and the child's birth year y. Figure 2.8 plots the θ_k coefficients and the corresponding 95% confidence intervals. The event-study regression is estimated without state-specific time trends, although a specification with the time trends yields very similar results.

[45]I have also estimated regressions omitting the "maybe-treatment" years. The results are very similar, although less precise for some outcomes perhaps due to lower power.

on the state level.[46] As in the regressions for paternity establishment rates, the key coefficient of interest does not vary significantly as state time-varying characteristics, controls for child support laws, state EITC, and AFDC/TANF implementation, and state-specific time trends are included, providing further support for a causal interpretation of my identification strategy.

The results suggest that IHVPE reduces the likelihood of marriage to the biological father by about 2.7 percentage points. To assess the magnitude of this effect relative to the average parental *post-childbirth* marriage rate, I turn to data from the Fragile Families and Child Well-Being Study, which suggests that about 13 percent of parents who were unmarried at childbirth will marry by the time their child turns five years old. With this estimate as a baseline, the approximate upper bound on the magnitude of the decrease in marriage post-childbirth is 21 percent (0.0270/0.13).[47] Taken together with the results on paternity establishment rates, these magnitudes imply that for every new paternity established as a result of IHVPE, there are 0.13 fewer parental marriages occurring post-childbirth.[48]

Next, I examine whether the decline in marriage is actually driven by the marginal parents who are expected to switch out of marriage as a result of IHVPE. My conceptual framework posits an increasing relationship between match quality and the level of parental interaction: "high-quality" couples marry, "medium-quality" couples establish paternity, while "low-quality" couples maintain no relationship. Consequently, parents on the margin of marriage should be located somewhere in the middle of the match quality dis-

[46] Unweighted regressions yield very similar results and are available upon request.

[47] The Fragile Families and Child Well-Being Study follows cohorts of births in 1998-2000. Most states had implemented IHVPE by this time. Consequently, it is likely that the baseline post-childbirth marriage rate prior to IHVPE was larger than 13%. This would imply that the true magnitude of the effect is somewhat lower than 21%. Unfortunately, CPS-CSS data do not have information on the percentage of parents who marry after childbirth.

[48] This elasticity is calculated by dividing the parental marriage coefficient (0.0270) by the paternity establishment rate coefficient (0.2019).

tribution. While I cannot test this conjecture directly in the data since I do not observe match quality, I construct a proxy for match quality by estimating a predicted probability of marriage based on mothers' observable characterstics using pre-IHVPE data.[49] I split the sample into deciles of the pre-IHVPE predicted marriage distribution, and estimate separate regressions of equation (2.5) with parental marriage as the outcome. Figure 2.9 presents the coefficients and confidence intervals from this exercise. The figure shows that the negative effect of IHVPE on parental marriage is concentrated around the 40th and 50th percentiles of the match quality proxy distribution. Consistent with the framework, this figure suggests that parents with very low and very high values for the match quality proxy do not change their marital behavior as a result of lower costs to establishing paternity; the marginal parents induced out of marriage by IHVPE indeed seem to be in the middle of the match quality distribution.

I conduct numerous robustness checks that test the validity of the result on marriage in the CPS-CSS data. I include indicators for pre-trends in marriage; I add quadratic state-specific time trends; I limit observations to mothers of children born within a four-year window around IHVPE implementation; I restrict the analysis to states for which I have the most accurate information on program implementation;[50] I omit black mothers (as results in Table 2.4 suggest a marginally significant positive correlation between IHVPE initiation

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[49]Specifically, I use a probit model to estimate a regression of the form: $Married_{isty} = \beta_0 + \gamma' X_{isty} + \phi' C_{st} + f(t) + \mu_s + \alpha_y + \delta_s * y + \epsilon_{sy}$, where $Married_{isty}$ is an indicator for the mother being married to the child's father, and the rest of the coefficients and variables are defined as before.

[50]Since I collected the data on the timing of IHVPE initiation from multiple sources, there are potential concerns about the accuracy of this information. The 27 "good info" states (AZ, CA, CO, CT, DE, DC, FL, ID, IL, IN, KY, LA, MD, MA, MI, MN, MS, MO, NJ, NY, OR, PA, RI, TN, TX, WA, WI) consist of four groups: 1) states for which I got data from Garfinkel & Nepomnyaschy, 2) states where I was able to speak directly to a representative from the child support office or from the IHVPE office, 3) states where the year and month of program implementation was listed on the program's website, and 4) states where the implementation year and month were clear from the statutes on *LexisNexis Academic*. For all other states not included in the "good info" subset, I assigned the year of implementation as the year of legislation or statute revision, which may not always be exact.

and births by black mothers); and I estimate regressions omitting one state at a time. I also estimate the effect on marriage using the larger March CPS sample. The results from these exercises are summarized in Table 2.6. Importantly, there are no statistically significant coefficients on marriage prior to IHVPE initiation (years -1 to -4) — in fact, the coefficients on indicators for years before IHVPE are all less than 0.015 and mostly positive, suggesting that the negative main effect is *not* driven by a downward trend in marriage. Further, the negative effect of IHVPE on parental marriage is robust to the addition of quadratic state-specific time trends, as well as to the exclusion of mothers of children born more than 4 years before or 4 years after IHVPE implementation, states with potentially less accurate information on the timing of IHVPE initiation, black mothers, and each state one at a time (despite reductions in sample size).[51] Finally, the coefficient estimated using March CPS data is similarly negative and statistically significant, although somewhat smaller in magnitude.

I next take advantage of the larger sample sizes in the March CPS files to investigate heterogeneous effects of IHVPE on marriage. The results in Table 2.7 suggest that the negative effects on marriage are concentrated among mothers who are less than 25 years old, who have a high school education or less, and who reported receiving any welfare income in the past year. Note that while it is possible that IHVPE impacts the likelihood of welfare receipt, I find no empirical evidence of such an effect.[52] Therefore, analyses that split the sample by welfare receipt should not be complicated by selection bias. Overall, the findings on heterogeneous impacts are consistent with the fact that IHVPE programs affect more disadvantaged parents on average, who are most likely to be unmarried at the time of childbirth.

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[51]Results for states other than CA, NY, and TX are available upon request.

[52]The key coefficient of interest from a regression for welfare receipt equals 0.005 with a standard error of 0.004.

The last two columns of Table 2.7 further split the welfare-receiving sample by the state-year child support disregard amount.[53] Disregard refers to the amount of child support income that is ignored in the calculation of welfare benefits (detailed discussion of disregard policies and other policies related to child support among welfare recipients is available in Cancian *et al.*, 2007). Essentially, for welfare recipients, any child support paid by the father above the disregard amount is fully taxed away by the state. In the extreme case of zero disregard, all formal child support paid by the father goes to the state, while mothers and children receive no transfers. In the conceptual framework, one can model this situation by lowering the benefit of investment in the paternity state (β_{yp}). Since $\beta_{yp} = \alpha\beta_{ym}$, this effectively means that the value of α is lowered. Recall that paternity is a feasible state if $c_m > \frac{c_p}{\alpha}$. For α small enough, paternity establishment becomes a strictly dominated state for all values of θ, and thus family behavior decisions should be unaffected by lowered paternity establishment costs. Empirically, this means that among mothers receiving welfare benefits, the effects of IHVPE should be concentrated in states and years with higher disregard amounts: in these states and years, α is higher as fathers' child support payments actually increase mothers' incomes and thus can benefit their children. The results in Table 2.7 suggest that this conjecture is true. The effect on parental marriage is negative and statistically significant among mothers on welfare with children born in states and years that had a minimum of \$50 per month in disregard. In contrast, the effect is insignificant (and the coefficient is positive) when the disregard is less than \$50 per month.[54]

Finally, in the first four rows of Table 2.8, I consider the effects of IHVPE on other

[53] I have also estimated the disregard regressions without conditioning on welfare receipt. The results are similar and available upon request.

[54] Most state-years have either \$0 or \$50 per month in disregard. Note that sample sizes in this analysis are reduced because child support disregard data is only available for years 1990-2003. Some states had fill-the-gap policies that increased the effective disregard amount differentially depending on family size (see Cancian *et al.*, 2007 for more details). For these states, I use the effective disregard amount for a 2-person household.

maternal relationship outcomes. I find that IHVPE increases the likelihood that a mother: 1) remains never married, 2) is married to someone other than the child's father, and 3) is cohabiting with someone other than the child's father. The next seven rows of Table 2.8 provide some suggestive evidence that IHVPE enables mothers to choose partners who likely have higher match quality. In these regressions, I estimate the effects of IHVPE on the characteristics of mothers' spouses in the March CPS data, regardless of whether or not they are the biological fathers.[55] The results suggest that following IHVPE, mothers tend to be married to men who are slightly more likely to be employed and older. These findings are consistent with the idea that IHVPE allows more mothers to choose to share a household with more preferrable new partners, who are not necessarily their children's fathers.

2.6.4 Changes in Match Quality Among Married Fathers

If IHVPE allows parents to raise the "marriage threshold" in match quality, then the average match quality among those who are married should increase. To test this hypothesis, I consider married fathers' demographic characteristics as proxies for match quality. Since survey and qualitative evidence suggests that mothers may be more sensitive to their partners' qualities than fathers are (McLanahan *et al.*, 2003; Edin and Kefalas, 2005), father quality may actually be the most relevant measure of match quality. With regard to the conceptual framework, this analysis then relies on the following assumption: given a mother's set of demographic characteristics, fathers with "higher-quality" demographic characteristics have greater match quality.

Table 2.9 presents the results from estimating equation (2.5) on a sample of mothers who are married to their children's fathers, using paternal characteristics as dependent variables. The results show that following IHVPE implementation, married fathers tend to be older

[55] I cannot study the characteristics of mothers' cohabiting partners as the CPS and NHIS do not provide a linkage across unmarried partners in the household.

and less likely to be receiving any public assistance. The coefficients for paternal education are not statistically significant but generally follow a similar pattern of an increase in average married father quality: post-IHVPE, married fathers are less likely to be high school dropouts and more likely to have a high school degree. While suggestive, this evidence is consistent with an increase in some observable proxies of match quality among married parents.

2.6.5 IHVPE and Father Involvement

Involvement Among Unmarried Fathers in the CSS

The CSS sample, which is limited to *unmarried* parents, contains many measures of father involvement. However, analysis of the effects of IHVPE on these outcomes is complicated by the negative effect on parental marriage, which induces selection into the CSS sample.

These complications are made clear by the predictions of my conceptual framework regarding the effects of IHVPE on unmarried parents. First, as the marriage threshold in match quality rises, the average match quality of *all* unmarried parents (i.e., those who maintain no relationship and those who establish paternity) should increase. Second, as some parents switch from no relationship to paternity establishment, average levels of father involvement should increase. Note that if match quality and father involvement are positively correlated, then the first effect should have a positive impact on unmarried father involvement as well.

Unfortunately, data constraints prevent me from clearly disentangling these two effects, as I do not observe the counterfactual parental experiences that would have occurred *in the absence of IHVPE*. Further, since the CSS only contains measures of involvement with children and has no information on fathers' demographic characteristics (such as age, race, education, income, etc.), I cannot study the effects of IHVPE on average match quality among unmarried parents with the same proxies as in the above analysis for married parents.

The first row in Table 2.10 documents selection into the CSS sample. Specifically, consistent with the negative effect on marriage, IHVPE leads to a 2 percentage point increase in the likelihood of being in the CSS sample. The other rows in the left-hand panel of Table 2.10 present the results from estimating regression (2.5) on the entire CSS sample. The results show positive and statistically significant effects on the likelihood of the father making all child support payments, providing food for the child, and covering the child's childcare and medical expenses. These results suggest that fathers in the CSS sample tend to be more involved following IHVPE, which is consistent with the predictions of the model.

To better understand the effects of IHVPE on father involvement among parents who switch from maintaining no relationship to establishing paternity, I perform a bounding exercise using the CSS data. The idea is to estimate the involvement regressions on a sample of unmarried parents who would have been in the CSS in the absence of IHVPE. Specifically, following methods described in Lee (2009), I trim the CSS sample by the number of "extra" mothers who are selected into the sample as a result of IHVPE.[56] The results from this bounding exercise are presented in the middle and right-hand panels of Table 2.10. Although not all coefficients are statistically significant, these results provide suggestive evidence that, consistent with the model, IHVPE increases some measures of father involvement among switchers from no relationship to paternity establishment.[57]

[56]I implement this method by estimating separate regressions of equation (2.5) with an indicator for being in the CSS sample as the outcome for 16 mutually exclusive groups of mothers defined by interactions between maternal education (less than high school, high school degree, some college, college or more) and race (non-Hispanic white, non-Hispanic black, Hispanic, and other) categories. For each group, g, I obtain the coefficient on the IHVPE indicator, β_g, and then trim the group by ($\beta_g * 100$) percent of the post-IHVPE sample if β_g is at least significant at the 10% level. To calculate the lower bound of the effect on each outcome, I drop post-IHVPE observations that are in the top ($\beta_g * 100$) percent of the post-IHVPE outcome distribution; for the upper bound, I drop post-IHVPE observations that are in the bottom ($\beta_g * 100$) percent of the post-IHVPE outcome distribution. Note that for binary outcomes, the lower bound trim drops ($\beta_g * 100$) percent of post-IHVPE observations that all have a value of "1", while the upper bound trim drops ($\beta_g * 100$) percent of the post-IHVPE observations that all have a value of "0".

[57]Results that simply trim the top and bottom 2% of each outcome distribution yield similar, but wider bounds. Lee (2009) shows that bounds calculated by conditioning on covariates (such as education and race above) are narrower than those calculated without controlling for any covariates.

Net Effects of IHVPE on Father Involvement

While the results discussed above suggest some positive effects on father involvement among unmarried parents who establish paternity instead of maintaining no relationship, the expected *net* effects on father involvement are ambiguous since the fathers who switch from marriage to establishing paternity are predicted to lower their involvement levels. However, the study of the net effects on paternal involvement requires having outcomes that are available for both married and unmarried parents. Since CSS involvement variables are only available for unmarried parents, I examine private health insurance provision, the *only* parental involvement variable available for individuals in both the CSS and non-CSS samples.

The results in Table 2.11 show that there is a negative effect on children's private health insurance coverage. I find that IHVPE leads to a 4 percent reduction in the likelihood that a child has private health insurance. As with the results on paternity establishment and marriage, the inclusion of state time-varying controls and state-specific time trends does not significantly alter the coefficients (see columns 1-5).

To better understand the effect on children's health insurance coverage, I distinguish between private coverage provided by individuals in and outside the household, and coverage through public health insurance programs such as Medicaid and CHIP.[58] In the last 5 columns of Table 2.11, I show that the negative effect on private health insurance coverage is driven entirely by a reduction in coverage provided by members of the household. There is no effect on health insurance provision by individuals outside the household. These findings imply that the decline in children's private health insurance coverage is driven by fathers' behavior: following IHVPE, fathers who are less likely to be married and in the same household are

[58]Information on CHIP coverage is only available in the CPS-CSS in 2002, 2004, 2006, and 2008.

also less likely to provide health insurance for their children.[59]

One concern is that the result on health insurance may be driven by a spurious correlation between IHVPE initiation and trends in access to health insurance in the U.S. To address this issue, I have estimated the relationship between IHVPE initiation and concurrent adult male health insurance coverage using data from the March CPS for 1989-2002.[60] While one might expect IHVPE to influence parental behavior and consequent child health insurance coverage, there should not be any effect on adult male health insurance coverage. However, if, for example, changes in employer-provided health insurance availability are correlated with IHVPE initiation, then the decrease in children's private health insurance coverage may be driven by a decrease in their fathers' access to health insurance, and would not be a marker of lower father involvement. The results from estimating equation (2.5) with an indicator for any health insurance coverage as the outcome for a sample of all males aged 18-64 in the analysis states are presented in Appendix Table B.2. The large sample sizes permit precise estimation of the coefficients of interest, which are reassuringly not statistically significant and very close to zero. These results provide further support for the finding on private child health insurance coverage and facilitate its interpretation as lowered father involvement.[61]

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[59]There is no change in mothers' health insurance coverage following IHVPE. For "mother has any health insurance" as an outcome, the key coefficient is 0.0016 with a standard error of 0.0042. For "mother has private health insurance" as an outcome, the key coefficient is -0.0091 with a standard error of 0.0093.

[60]In this analysis, variation in IHVPE implementation is at the state/survey-year level (rather than the state/child-birth-year level used in the rest of the regressions). Hence, I use data through 2002 only because all states in my sample initiated IHVPE by 1999.

[61]A related concern is the possibility of spurious correlation between IHVPE implementation and changes in public health insurance access. Throughout the 1990s and 2000s, many states changed their Medicaid eligibility thresholds, and all states implemented CHIP after October 1997. In Table 2.4, I have shown that IHVPE is uncorrelated with the proportion of the population receiving Medicaid, the major public health insurance program for low-income children in the 1990s. Since CHIP benefits became available after October 1997 and only eight states in my sample implemented IHVPE in 1998 or later, it is unlikely that IHVPE program implementation is correlated with CHIP availability at the time of childbirth. However, since the CPS-CSS data span 1994-2008, it is important to check the correlation between IHVPE implementation and CHIP benefits available at the time of observation in the data. I have estimated equation (2.5) with the log total and state spending in the year of and the year before observation as dependent variables using 1998-2008 data from the Henry J. Kaiser Family Foundation state health facts. I find no statistically significant

To assess the net effects of IHVPE on other measures of father involvement, I propose another solution to the problem of missing involvement data for married fathers. I assume that married fathers “make all child support payments”, “make child support payments on time most or all of the time”, have visitation rights and legal custody, spend the whole year with the child, provide food, clothes, and gifts for the child, and cover childcare and medical expenses. Clearly, these assumptions may not hold true for all married fathers. Yet since a large literature documents that married resident fathers have higher quality parenting skills and greater degree of involvement with their children than non-resident fathers (Cooksey and Craig, 1998; Kalmijn, 1999; Carlson *et al.*, 2008), these assumptions are not entirely unreasonable. Nevertheless, the findings from this analysis are merely suggestive and should be interpreted with caution.

The results, presented in Appendix Table B.3, show that when married fathers are included in the analysis, the effect on the likelihood of making any child support payments is negative and marginally significant, while the effect on the likelihood of making all required child support payments is negative and insignificant. Further, fathers now spend fewer days with their children, are less likely to have joint legal custody, and are less likely to cover childcare expenses. These findings provide suggestive evidence that net father involvement along measures other than private health insurance provision declines.[62]

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relationship between IHVPE implementation and CHIP generosity (results available upon request). These findings provide reassurance that the results on children’s private health insurance coverage are not driven by spurious correlations between public health insurance program changes and IHVPE.

[62]One potential worry with this analysis is that the negative effects on father involvement are mechanically driven by the decline in marriage — i.e., there are mechanically fewer fathers with a value of “1” for the indicator variables on father involvement due to the negative effect on marriage. To address this, I have estimated regressions treating the only variable available for both married and unmarried parents — children’s private health insurance coverage — in the same way by assigning a value of “1” for all married parents. Clearly, this is not an accurate assumption as only 78 percent of children in married households have private health insurance. However, as shown in Appendix Table B.4, analysis with this imputed health insurance variable yields results very similar to those from using the true child private health insurance coverage variable. The p-value on the F-test for equality of coefficients across the models is 0.8514, suggesting that the coefficients in the two models are not statistically different from each other. Thus, although not

Finally, I turn to an analysis of net paternal financial involvement, which is a variable that is unobserved for married parents. However, maternal labor supply — an outcome that is available for all mothers regardless of marital status — serves as arguably one of the best indirect proxies for paternal financial support, assuming that mothers are more likely to enter (leave) the labor force when financial support declines (increases).[63]

The results for maternal labor supply are presented in Table 2.12. IHVPE leads to a 3 percent increase in the likelihood that a mother reports working any hours in the last year in the March CPS, and the coefficient is consistent across specifications.[64] In Appendix Table B.5, I document that the labor supply effect is consistent across different definitions (mother is employed, mother is in the labor force, mother had any own wage income last year, and mother worked any hours last year). There is no effect on wages or hours worked on the intensive margin. This suggests that the effect of IHVPE operates on the extensive margin by inducing more mothers of young children to enter the workforce. Overall, the increase in maternal labor supply is consistent with the idea that fathers' net financial support has decreased as a result of IHVPE, as mothers are more likely to need to earn income.

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all married fathers provide complete involvement and support for their children, as long as married fathers are more likely than unmarried fathers to do so, the method of assigning values of "1" for measures of involvement for married fathers provides a reasonable upper bound for the magnitude of the IHVPE effect when accounting for selection out of marriage.

[63]Analysis of this outcome follows a vast literature (Blundell and MaCurdy, 1999 provide a survey). Most of the empirical literature on female labor supply has been concerned with estimating labor supply elasticities separately for married and unmarried women (for example, Blau and Kahn, 2007, and Bishop *et al.*, 2009, respectively), and studying the effects of various public programs, such as the EITC, welfare, and childcare subsidies, on single mothers' labor supply (Berger and Black, 1992; Eissa and Liebman, 1996; Keane and Moffitt, 1998; Meyer and Rosenbaum, 2001; Ellwood, 2000; Hotz *et al.*, 2002; Moffitt, 2002, among many others). Additionally, from a theoretical perspective, an intra-household bargaining framework has important predictions on the effects of changes in married women's outside options on their labor supply — if leisure is a normal good, then an increase in a married woman's relative bargaining power should lead to reduced labor supply (see Lundberg and Pollak, 1993, 1996, 2007; Gray, 1998; Chiappori *et al.*, 2002; Voena, 2011, among others). However, by considering married and unmarried women separately, these studies do not fully address the potential relationship between income shocks (or shocks to relative bargaining power), *transitions in and out of* marriage, and overall women's labor supply.

[64]Results using data from the CPS-CSS are qualitatively similar, although the coefficients are not statistically significant perhaps due to smaller sample sizes.

Taken together, the results provide several pieces of evidence on how IHVPE affects father involvement. Fathers who establish paternity instead of maintaining no relationship with their families are more involved along some dimensions (i.e., child support payments and coverage of some food, childcare, and medical expenses). However, the net effects on several available measures of involvement (i.e., health insurance and financial transfers) are negative, suggesting that fathers who establish paternity instead of marrying reduce their involvement levels, and that this decline is greater in magnitude than any increases in involvement by those who switch from maintaining no relationship.

2.6.6 Net Effects of IHVPE on Child Welfare

While the findings presented so far document that IHVPE influences family structure and parental behavior, they cannot speak to the net impacts on family well-being. However, at least from a policy perspective, one may particularly care about the overall effects of IHVPE on children's welfare. As previously discussed, the effects are theoretically ambiguous. Children whose parents would have maintained no legal relationship in the absence of IHVPE may benefit from increased paternal involvement. On the other hand, children whose parents would have been previously married may suffer from a decrease in paternal involvement, but may also benefit from living with mothers who experience higher utility from less interaction with lower-than-desired quality partners.

I first test whether IHVPE has affected total household income.[65] The first row in Table 2.13 presents results from estimating equation (2.5) with log total household income as the

[65] Total household income is defined as the sum of total personal pre-tax incomes earned over the previous year of all adult individuals residing in the household. These include incomes from wages, businesses, farms, welfare transfers, SSI, retirement, unemployment transfers, worker's compensation, veterans' transfers, disability, dividends, rent, educational assistance, child support, alimony, financial assistance from friends and family, and other sources.

dependent variable using March CPS data.[66] While the key coefficient is positive, it is quite small in magnitude and not statistically significant, suggesting that there is no net effect of IHVPE on total household income. This net zero effect may arise because any decline in paternal monetary support as a result of the decrease in marriage is compensated for by an increase in income due to higher maternal employment. Income from mothers' new partners may also offset any declines in paternal monetary support.

The other rows in Table 2.13 present results on outcomes measuring children's physical and mental health and access to care in the NHIS. The results suggest that there are no net effects of IHVPE on children's physical health. There is a marginally significant negative effect on the likelihood of a child having any learning disabilities, although other measures of mental health are unaffected.[67] However, there is also a net decline in children's access to care — the likelihoods that a child has any doctor visits and any well-child visits in the last 12 months both decrease by about 2 percent at the sample mean. This finding may be consistent with a decrease in children's private health insurance coverage.

On the whole, I find little evidence that IHVPE affected child welfare, at least as measured by household income and the health outcomes available in NHIS. This may be surprising as this policy was likely implemented in hopes of improving the well-being of children born to unmarried parents. However, although IHVPE achieved its first-order goal of raising paternity establishment rates, I have shown that it also had important effects on family structure and behavior. My results suggest that affected parents responded to IHVPE in a way that left the average welfare of their children unchanged.

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[66]Results using CPS-CSS data are qualitatively similar but less precise due to smaller sample sizes.

[67]I have also estimated regressions with the following physical and mental health outcomes: any skin allergies, any frequent diarrhea, any hearing problems, any vision problems, any mobility problems, any stuttering, and mental retardation, any developmental delay, and ever diagnosed with ADD. None of the results is statistically significant.

2.7 Conclusion

As more than one third of all children in the U.S. are borne by unmarried women every year, there is a clear need for policies that address the needs of these children and their families. The fact that children raised in two-parent households tend to fare better along numerous measures of well-being has prompted many policymakers to focus on ways to encourage absent fathers to become more engaged with their families. These sentiments underlie many child support enforcement efforts, programs like IHVPE, and marriage promotion campaigns.

An important feature of such policies, however, is that they effectively offer an "intermediate" option for a parental relationship that is situated between the "extremes" of maintaining no relationship and marriage. Indeed, this is a common feature of many policies that aim to improve the welfare of disadvantaged populations. For example, the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) provides free (basic) food and infant formula for low-income pregnant and post-partum women and children under age five.[68] While WIC may certainly improve the health outcomes of women and children who would have otherwise had even less access to these necessities, it may also have the unintended consequence of reducing the well-being of those who would have otherwise engaged in healthier behaviors such as breastfeeding or consuming more nutritious foods.[69] Clearly, while many disadvantaged women and children may still benefit from WIC, understanding the effects on both margins is crucial for overall welfare analysis.

In this paper, I provide a detailed analysis of the causal effects of IHVPE, an important "intermediate" policy in the realm of family economics, and interpret my findings in the

[68]WIC also provides health screenings, health education, and referrals to other social service agencies and public health clinics.

[69]WIC foods include iron-fortified infant formula and infant cereal, adult cereal, fruit and vegetable juices, milk, eggs, cheese, beans, and peanut butter. Note that there is some awareness of the possible unintended consequences of WIC on breastfeeding. As a result, many WIC programs provide breastfeeding education and offer free breast pumps to new mothers.

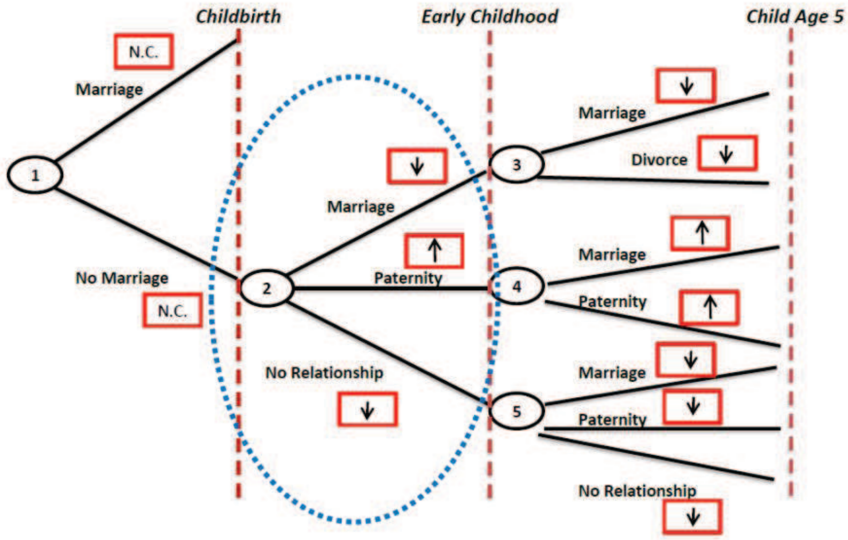
context of a conceptual framework. In the framework, parents trade-off their utility from access to children with their match quality. While mothers get equal rights to their children regardless of the parental relationship, fathers attain more parental rights in marriage than if they establish paternity, and have no parental rights if they maintain no relationship with their children's mothers. Additionally, marriage necessitates greater parental interaction than paternity, while no relationship involves no parental interaction. By substantially reducing the costs of the "intermediate" paternity establishment contract, IHVPE makes this option more attractive relative to the other options of no relationship and marriage. The decline in marriage occurs because at lower levels of match quality, some parents value an option that delineates partial parental rights and obligations to unmarried fathers over the greater commitment of marriage. This observation has important implications for more recent family policies that have promoted marriage through "Healthy Marriage Initiatives". If some parents opt out of marriage when offered an "intermediate" alternative arrangement for family involvement, then marriage may not be the optimal choice for all parents.

By impacting behavior on two opposing margins, IHVPE has a theoretically ambiguous net effect on father involvement. It depends on the relative magnitudes of the decline in involvement by fathers who would have otherwise been married and the increase in involvement by fathers who would have maintained no formal contact with their families in the absence of IHVPE. The effects on overall child welfare are similarly ambiguous: although some children may be hurt by the possible decline in paternal investments, they may also benefit from improvements to their mothers' well-being arising from reduced interaction with less-than-optimal partners.

Using variation in the timing of IHVPE implementation across states and years, I show that while IHVPE substantially increases paternity establishment rates, it also reduces parental marriage post-childbirth. In particular, mothers are less likely to be married to their children's biological fathers and more likely to remain never-married or be married to or cohabiting with new partners. Although there is some evidence of higher levels of involvement among unmarried fathers who establish paternity instead of maintaining no relationship with their families, I show that the *net* effects on several available measures of involvement are negative once I account for the selection out of marriage. Finally, I show that IHVPE has little net effect on overall child welfare, as measured by total household income and child physical and mental health.

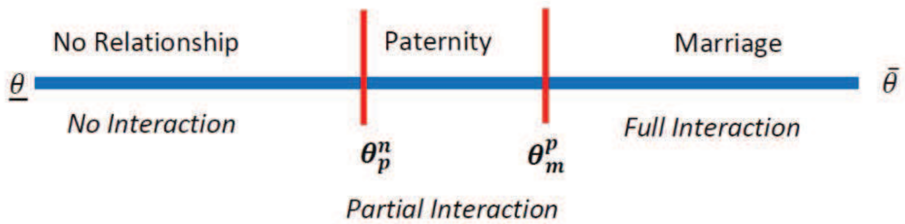
The results from my analysis suggest that unmarried parents face important trade-offs in their decisions regarding involvement with each other and their children. Women who bear children out-of-wedlock do not always view their children's fathers as ideal marriage partners, and are hesitant to commit to relationships that may impair their families' welfare (McLanahan *et al.*, 2003; Edin and Kefalas, 2005). As a result, a paternity establishment program that seeks to engage absent fathers and increase child support payments and father involvement can actually have the opposite effects by discouraging marriage and reducing the financial and non-pecuniary support levels among otherwise married fathers. Perhaps the hardships for mothers who bear children out-of-wedlock and their children can only be truly alleviated when economic opportunities for both poor men and women improve. Greater opportunities for women may lead them to delay childbearing until they are better able to provide for their children, while greater opportunities for men may increase the pool of marriageable partners who support and engage with their children and bring fulfillment to their wives.

Figure 2.1: Decision Tree of Parental Relationship Options



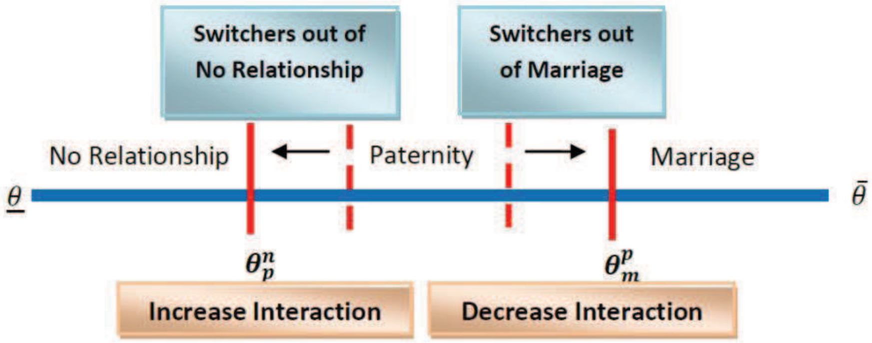
Notes: This figure shows a decision tree of parental relationship options before and after childbirth. Arrows in the boxes show predicted effects of IHVPE. N.C. = no change. My model focuses on the decisions made at node 2 (circled). Please see text in Section 2.2 for more details.

Figure 2.2: Relationship between Match Quality and the Optimal Parental Relationship



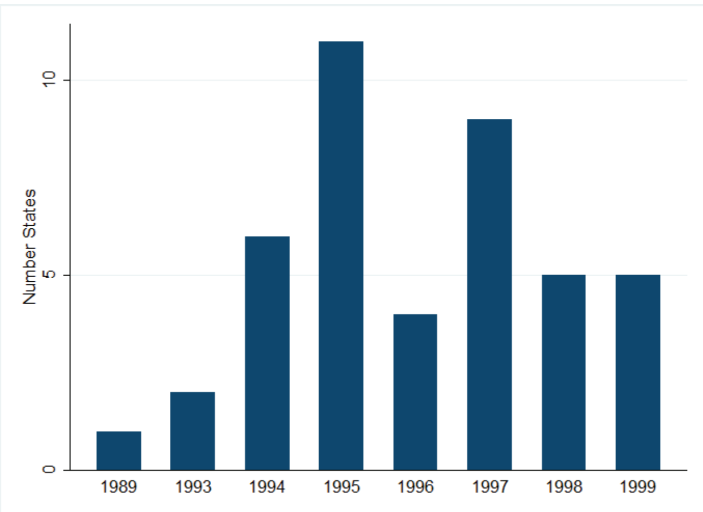
Notes: This figure shows the relationship between match quality, θ, and the optimal parental relationship choice.

Figure 2.3: The Effects of Lowering the Cost of Paternity on Optimal Parental Relationships



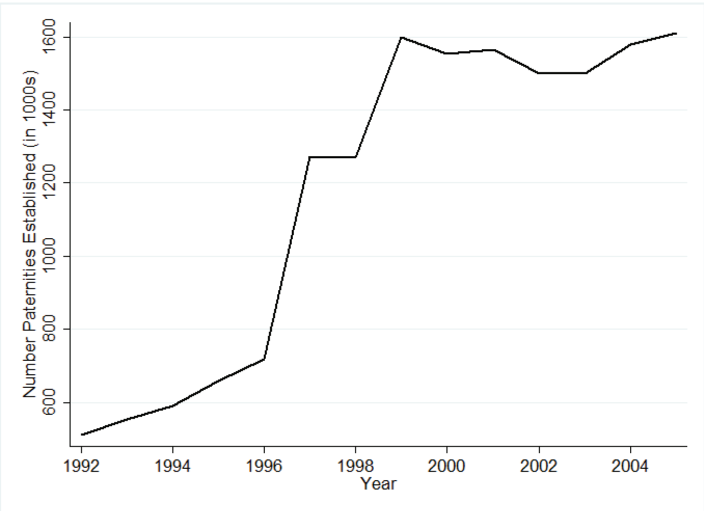
Notes: This figure shows the effects of lowering the cost of establishing paternity on parental decisions about their relationships: as θ_p^n falls, some parents switch from no relationship to paternity; as θ_m^p rises, some parents switch from marriage to paternity.

Figure 2.4: Variation in IHVPE Program Initiation Across States



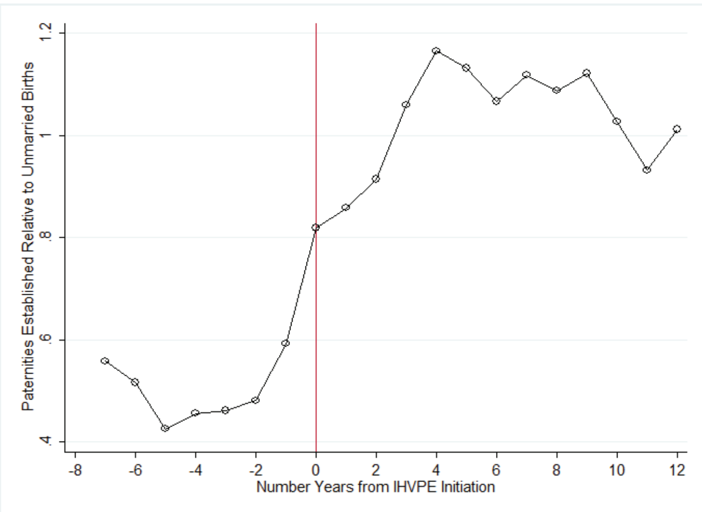
Notes: This figure plots the number of states that initiated IHVPE in each year. Forty-four states are included in the figure.

Figure 2.5: Number Paternities Established in the United States: 1992-2007



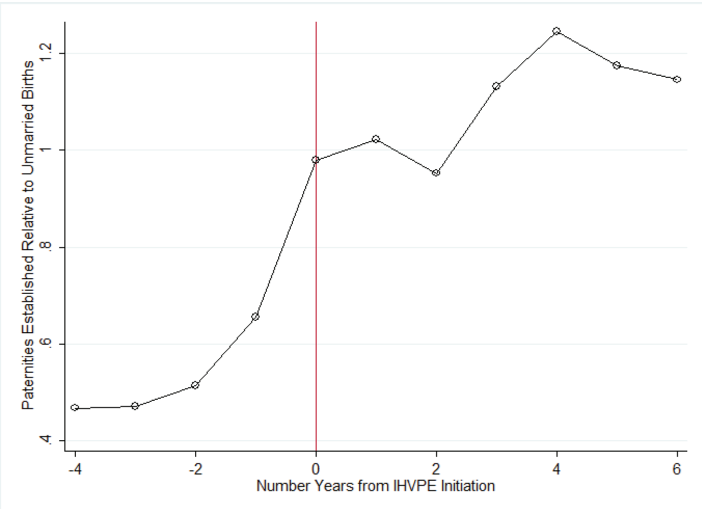
Notes: This figure plots the total number of paternities established in the U.S. in each year.

Figure 2.6: Number Paternities Established Relative to Number of Unmarried Births: 1992-2005



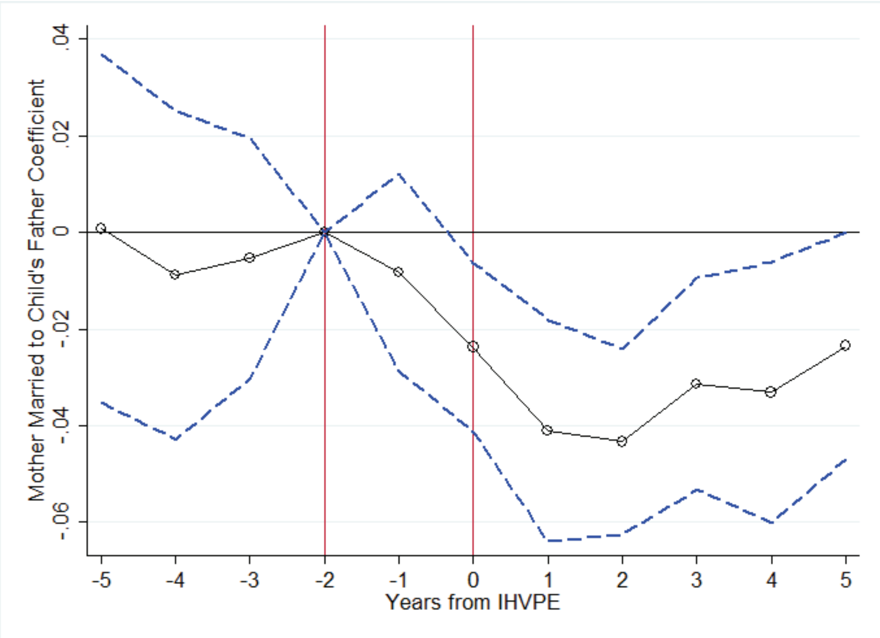
Notes: This figure plots the average of the ratio of the number of paternities established to the number of unmarried births in each year before and after IHVPE program implementation. Year 0 denotes the first year the program is in effect, which is the same as the year listed in Appendix Table B.1 if only the year is listed or if the month of initiation is June or earlier. If the month of initiation is known and it is July or later, then I assume the first year the program is in effect is the following year.

Figure 2.7: Number Paternities Established Relative to the Number of Unmarried Births: 1992-2005, States that Initiated IHVPE in 1996 or Later



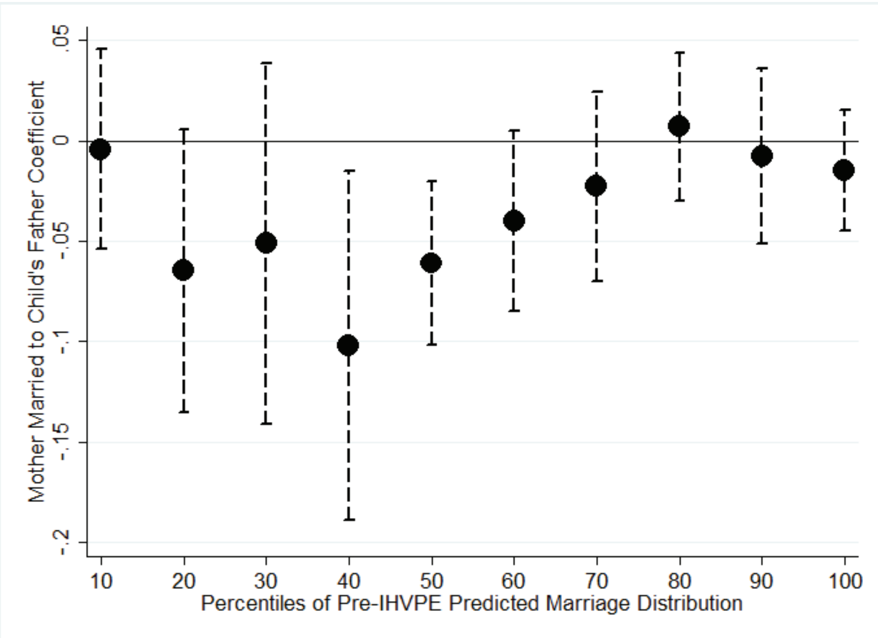
Notes: This figure plots the average of the ratio of the number of paternities established to the number of unmarried births in each year before and after IHVPE program implementation for states that initiated IHVPE in 1996 or later. See notes below Figure 2.6 on details about the timing of implementation.

Figure 2.8: Effects of IHVPE on Parental Marriage by Year



Notes: This figure plots θ_k coefficients (and 95% confidence intervals in dashed blue lines) from estimating the following equation: $Y_{isty} = \beta_0 + \sum_{k=-5}^{-3} \theta_k * IHVPE_{syk} + \sum_{k=-1}^{5} \theta_k * IHVPE_{syk} + \gamma' X_{isty} + f(t) + \mu_s + \alpha_y + \epsilon_{isty}$, where $IHVPE_{syk}$ is an indicator for k years from IHVPE implementation in state s and the child's approximate birth year. The omitted category is -2. I assign the child's approximate birth year as *birth year = survey year − child age − 1.* Since the surveys are conducted in March, this procedure assigns the correct birth years to children born in April-December, and incorrectly assigns the years immediately prior to their true birth years for children born in January-March. Consequently, some children who are affected by IHVPE may be erroneously assigned to the pre-treatment group. In addition, children born in months prior to the month of program implementation in any given year may be unaffected by IHVPE but erroneously assigned to the treatment group. As a result, these data allow for three possible treatment statuses: "pre-treatment" = *IHVPE first year − child's birth year* ≤ -2, "maybe-treatment" = *IHVPE first year − child's birth year* $\in \{-1, 0\}$, and "treatment" = *IHVPE first year − child's birth year* ≥ 1. These three groups are depicted using the vertical lines at -2 and 0 in the graph.

Figure 2.9: Effects of IHVPE on Parental Marriage by Deciles of the Pre-IHVPE Predicted Marriage Distribution



Notes: This figure plots the coefficients and 95% confidence intervals (shown as dashed bars) from estimating equation (2.5) for parental marriage as an outcome separately by deciles of the pre-IHVPE predicted marriage distribution. To obtain the pre-IHVPE predicted marriage distribution, I use a probit model to estimate a regression of the form: $Married_{isty} = \beta_0 + \gamma' X_{isty} + \phi' C_{st} + f(t) + \mu_s + \alpha_y + \delta_s * y + \epsilon_{sy}$, where $Married_{isty}$ is an indicator for the mother being married to the child's father, and the rest of the coefficients and variables are defined as in equation (2.5).

Table 2.1: Sample Means

	Whole Sample	IHVPE	No IHVPE
State-Year Data (N=601 State-Year Cells)			
Ratio of Paternities to Unmarried Births	0.891	1.067	0.489
CPS-CSS Data (N=38,449 Mothers with Youngest Children <6 years)			
Mother is Married	0.775	0.786	0.758
Mother is Never Married	0.137	0.138	0.135
Mother is Married to Child's Father	0.765	0.775	0.749
Mother is Married to Someone Else	0.010	0.011	0.009
Mother's Age at Birth: <20 years	0.045	0.043	0.047
Mother's Age at Birth: 35-45 years	0.193	0.208	0.168
Mother's Education: <HS	0.141	0.141	0.143
Mother's Education: HS	0.296	0.274	0.332
Mother's Education: Some College	0.292	0.287	0.300
Mother is Non-Hispanic White	0.632	0.616	0.657
Mother is Black	0.144	0.137	0.154
Mother is Hispanic	0.173	0.186	0.151
Child is Male	0.510	0.510	0.509
Child's Age	2.180	1.890	2.645
Child Has Any Health Insurance Coverage	0.883	0.893	0.866
Child Has Private Health Insurance Coverage	0.673	0.669	0.679
CPS-CSS Data (N=27,683 Mothers Married to Children's Fathers)			
Father's Age	34.686	34.676	34.704
Father Has Less than High School Education	0.131	0.130	0.134
Father Has High School Degree	0.284	0.271	0.306
Father Received Any Public Assistance	0.006	0.004	0.010

	Whole Sample	IHVPE	No IHVPE
CS Supplement Data (N=8,974 Eligible Mothers)			
Father Paid Any Child Support in the Past Year	0.358	0.359	0.356
Father Paid All Child Support in the Past Year	0.221	0.221	0.222
Father Has Legal Visitation Rights	0.702	0.701	0.703
Number Days Father Spent with Child in the Past Year	60.311	63.676	55.341
Father Provided Any Gifts, Clothes, Food, Childcare or Medical Help	0.558	0.566	0.546
March CPS Data (N=212,504 Mothers with Youngest Children <6 Years)			
Mother Worked Any Usual Hours in the Past Year	0.678	0.679	0.678
Real Household Income in the Past Year (2010 Dollars)	72,361.94	78,190.09	63,962.99
March CPS Data (N=162,691 Married Mothers)			
Mother's Spouse Had Any Own Income in Past Year	0.905	0.911	0.898
Mother's Spouse's Age	34.438	34.712	34.138
Mother's Spouse Has Less than High School Education	0.116	0.110	0.122
Mother's Spouse Has High School Degree	0.309	0.279	0.342
NHIS Data (Mothers of Sample Children <8 Years)			
Mother is Cohabiting with Child's Father	0.047	0.055	0.022
Mother is Cohabiting with Someone Else	0.019	0.018	0.022
Child Had Any Doctor Visits in Past 12 Months	0.852	0.858	0.833

Notes: The state-year data on paternity establishment rates is available for the following 43 states over 1992-2005: Alabama, Alaska, Arizona, Arkansas, California, Colorado, Connecticut, Delaware, DC, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Kansas, Kentucky, Louisiana, Maine, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Nebraska, Nevada, New Jersey, New York, North Carolina, North Dakota, Ohio, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, and Wisconsin. The CPS-CSS sample includes all women with a youngest child aged 5 years or less in the household who were between the ages of 18 and 45 at the time of childbirth in the above 43 states and Washington (44 sample states) in 1994, 1996, 1998, 2000, 2002, 2004, 2006 and 2008. The March CPS sample includes all women with a youngest child aged 5 years or less in the household who were between the ages of 18 and 45 at the time of childbirth in the 44 sample states over 1989-2010. The NHIS sample comes from the Sample Child Files over 1997-2010 on all women with a sample child aged 7 years old or less in the household who were between the ages of 18 and 45 at the time of childbirth in the 43 sample states. Sample sizes in the NHIS data cannot be released due to confidentiality concerns. Please see the text for more information about approximate sample sizes. All individual-level samples omit individuals who moved from abroad last year and assign the state of last year's residence as the state of child's birth. Mothers are coded as married to the biological father if they are married and their child is coded as living with both parents in the household. Mothers are coded as married to someone other than the biological father if they are married, but their child is coded as living with one parent in the household. The state-year means are weighted by state-year populations, while individual-level data means are weighted by the provided sample weights.

Table 2.2: Effects of IHVPE on Paternity Establishment Rates: 1992-2005

	Dependent Variable: Ratio of Paternities Established to Unmarried Births				
	(1)	(2)	(3)	(4)	(5)
IHVPE Program Exists in State and Year of Observation	0.2324***	0.1998**	0.2050**	0.2006***	0.2019***
	(0.0632)	(0.0603)	(0.0585)	(0.0537)	(0.0530)
Mother and Child Controls	√	√	√	√	√
Year FEs	√	√	√	√	√
State FEs	√	√	√	√	√
State Time-Varying Characteristics Controls		√	√	√	√
Child Support Laws Controls			√	√	√
State EITC Implementation				√	√
AFDC/TANF Implementation				√	√
State-Specific Time Trends					√
N	601	573	545	545	545
R-squared	0.7219	0.7623	0.7656	0.7794	0.8464

Notes: Each column is a separate regression. Units of observation are state-year cells consisting of 43 sample states (see notes under Table 2.1). I assume that the first year the program is in effect (year of initiation) is the same as the year listed in Appendix Table B.1 if only the year is listed or if the month of initiation is June or earlier. If the month of initiation is known and it is July or later, then I assume the first year the program is in effect is the following year.

The maternal and child controls include controls for the proportion of births with the following characteristics: mother's age (<20, 20-24, 25-34, 35-44, 45+), mother's education (less than HS, HS, some college, college+), mother's race (white, black, Hispanic), and child sex. The controls for state characteristics in the year before include the unemployment rate, the poverty rate, the state minimum wage, the percent of the population that receives AFDC/TANF benefits, the AFDC/TANF benefit for a 4-person family, the percent of the population on Medicaid, an indicator for a Democratic governor, and the fraction of the state House that is Democratic. The child support laws controls are indicators for whether the following child support enforcement laws are in place in the state and year of observation: universal wage withholding, genetic testing for paternity, new hires directory, and license revocation for non-payment. The state EITC implementation controls are indicators for whether a state EITC has been implemented in the state and year of observation. The AFDC/TANF implementation controls are indicators for whether an AFDC waiver or TANF has been implemented in the state and year of observation. All regressions are weighted by the state-year populations. Robust standard errors are clustered on the state level.

Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Table 2.3: Effects of IHVPE on Paternity Establishment Rates with Pre-Trends: 1992-2005

	Dependent Variable: Ratio of Paternities Established to Unmarried Births				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
3 Years Before IHVPE Program Initiation	-0.0608	-0.0585	-0.0604	-0.0729	-0.0354
	(0.0495)	(0.0518)	(0.0586)	(0.0510)	(0.0461)
2 Years Before IHVPE Program Initiation	-0.0477	-0.0077	-0.0156	-0.0397	-0.0390
	(0.0391)	(0.0437)	(0.0521)	(0.0466)	(0.0426)
1 Year Before IHVPE Program Initiation	-0.0878**	-0.0696+	-0.0756+	-0.0761+	-0.0750
	(0.0389)	(0.0372)	(0.0443)	(0.0435)	(0.0470)
IHVPE Program Exists in State and Year of Observation	0.1870**	0.1684**	0.1703**	0.1618**	0.1626**
	(0.0615)	(0.0631)	(0.0607)	(0.0557)	(0.0578)
Mother and Child Controls	√	√	√	√	√
Year FEs	√	√	√	√	√
State FEs	√	√	√	√	√
State Time-Varying Characteristics Controls		√	√	√	√
Child Support Laws Controls			√	√	√
State EITC Implementation				√	√
AFDC/TANF Implementation				√	√
State-Specific Time Trends					√
N	601	573	545	545	545
R-squared	0.7236	0.7635	0.7669	0.7808	0.8474

Notes: Units of observation are state-year cells. Please refer to Table 2.2 for details about sample restrictions and controls. All regressions are weighted by state-year populations. Robust standard errors are clustered on the state level.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Table 2.4: IHVPE Programs and Maternal and State Characteristics: 1992-2005

Dependent Variable	**Coefficient**	**SE**
Log Number Births	-0.0032	(0.0038)
Proportion Births by Unmarried Mothers	-0.0121	(0.0108)
Proportion Births with Mother's Age <20	-0.0003	(0.0006)
Proportion Births with Mother's Ed: <HS	0.0011	(0.0016)
Proportion Births with Mother's Ed: College+	-0.0025	(0.0024)
Proportion Births by Non-Hispanic White Mothers	-0.0067	(0.0044)
Proportion Births by Black Mothers	0.0022+	(0.0012)
Proportion Births by Hispanic Mothers	0.0025	(0.0024)
State Unemployment Rate in Previous Year	0.1109	(0.1519)
State Poverty Rate in Previous Year	0.4218	(0.3373)
State Minimum Wage in Previous Year	-0.0636	(0.0917)
Proportion of Population Receiving Welfare Benefits in Previous Year	0.0023	(0.0022)
Welfare Benefit for 4-Person Family in Previous Year	-10.4670	(7.0722)
Governor is Democratic in Previous Year	-0.0575	(0.0793)
Proportion of Population on Medicaid in Previous Year	-0.0038	(0.0040)
State EITC Implemented	0.0226	(0.0516)
AFDC Waiver Implemented	0.1337	(0.1279)
Universal Wage Withholding Implemented	0.0357	(0.0974)

Notes: N = 601 state-year cells. Each coefficient is from a separate regression. Please refer to Table 2.2 for details about sample restrictions. All regressions include state and year fixed effects, and state-specific time trends. Regressions with maternal characteristics as outcomes are weighted by the number of births in each state-year cell, while all other regressions are weighted by state-year populations. Robust standard errors are clustered on the state level.
Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.01$

Table 2.5: Effects of IHVPE on Parental Marriage: CPS-CSS 1994-2008

	Dependent Variable: Mother is Married to Child's Biological Father				
	(1)	(2)	(3)	(4)	(5)
IHVPE Program Exists in State and Year of Child's Birth	-0.0324***	-0.0258**	-0.0261**	-0.0268***	-0.0270**
	(0.0080)	(0.0073)	(0.0075)	(0.0074)	(0.0087)
Mother and Child Controls	√	√	√	√	√
Year FEs	√	√	√	√	√
State FEs	√	√	√	√	√
Quadratic Polynomial in Survey Year	√	√	√	√	√
State Time-Varying Characteristics Controls		√	√	√	√
Child Support Laws Controls			√	√	√
State EITC Implementation				√	√
AFDC/TANF Implementation				√	√
State-Specific Time Trends					√
N	38,449	37,457	36,243	36,243	36,243
R-squared	0.2232	0.2239	0.2236	0.2236	0.2244

Notes: Each column is a separate regression. The sample of analysis includes all women with a youngest child aged 5 years old or less in the household who were between the ages of 18 and 45 at the time of childbirth in the 44 sample states (see notes under Table 2.1) in 1994, 1996, 1998, 2000, 2002, 2004, 2006 and 2008. The sample omits all individuals who moved from abroad last year and assigns the state of last year's residence as the state of child's birth. Mothers are coded as married to the biological father if they are married and their child is coded as living with both parents in the household. The mother and child controls include controls for the woman's age at childbirth (<20, 20-24, 25-34, 35-44), woman's education (less than HS, HS, some college, college+), woman's race (white, black, Hispanic, other), child sex, and indicators for child's age in years. The time-varying state characteristics include the unemployment rate, the poverty rate, the state minimum wage, the percent of the population that receives AFDC/TANF benefits, the AFDC/TANF benefit for a 4-person family, the percent of the population on Medicaid, an indicator for a Democratic governor, and the fraction of the state House that is Democratic. The controls for child support laws are indicators for whether the following child support enforcement laws are in place in the state and year of observation: universal wage withholding, genetic testing for paternity, new hires directory, and license revocation for non-payment. The state EITC implementation controls are indicators for whether a state EITC has been implemented in the state and year of observation. The AFDC/TANF implemenation controls are indicators for whether the AFDC waiver or the TANF program is implemented by the state and year of observation. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Table 2.6: Effects of IHVPE on Parental Marriage: Robustness Checks

	Dependent Variable: Mother is Married to Child's Biological Father								
	CPS-CSS, with Pre Trends	**CPS-CSS, Quadratic State Trends**	**CPS-CSS, 4-Year Window**	**CPS-CSS, Good Info States**	**CPS-CSS, No Black Mothers**	**CPS-CSS, Omit CA**	**CPS-CSS, Omit NY**	**CPS-CSS, Omit TX**	**March CPS**
IHVPE Program Exists in State and Year of Child's Birth	-0.0209+	-0.0309**	-0.0231+	-0.0335**	-0.0243**	-0.0254**	-0.0279**	-0.0258**	-0.0139**
	(0.0123)	(0.0097)	(0.0130)	(0.0090)	(0.0092)	(0.0088)	(0.0089)	(0.0101)	(0.0051)
Child Born 1 Year Before Program Initiation	0.0062								
	(0.0124)								
Child Born 2 Years Before Program Initiation	0.0131								
	(0.0101)								
Child Born 3 Years Before Program Initiation	0.0069								
	(0.0103)								
Child Born 4 Years Before Program Initiation	-0.0048								
	(0.0138)								
N	36,243	36,243	17,263	26,148	31,907	32,553	33,910	33,831	184,562

Notes: Each column is a separate regression. Please refer to Table 2.5 for details about sample restrictions and controls. The "good info" states are AZ, CA, CO, CT, DE, DC, FL, ID, IL, IN, KY, LA, MD, MA, MI, MN, MS, MO, NJ, NY, OR, PA, RI, TN, TX, WA, WI; these are states for which I have the most accurate information on the timing of IHVPE implementation (please refer to the text for more details). All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, a quadratic polynomial in the survey year, and state-specific linear time trends. The regression in the second column also adds state-specific quadratic time trends. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Table 2.7: Heterogeneous Effects of IHVPE on Parental Marriage: March CPS 1989-2010

	Dependent Variable: Mother is Married to Child's Biological Father							
	Mothers Aged <25 Years	Mothers Aged 25-45 Years	Mothers with High School Degree or Less	Mothers with Some College or More	Mothers on Welfare	Mothers Not on Welfare	Mothers on Welfare with Disregard $50+	Mothers on Welfare with Disregard <$50
IHVPE Program Exists in State and Year of Child's Birth	-0.0265**	-0.0046	-0.0575***	0.0004	-0.0326+	-0.0102**	-0.0348+	0.0181
	(0.0090)	(0.0071)	(0.0154)	(0.0053)	(0.0169)	(0.0046)	(0.0200)	(0.0820)
N	43,483	140,673	81,242	103,320	13,080	171,482	8,110	1,137

Notes: Each coefficient is from a separate regression. The sample of analysis includes all women with a biological youngest child aged 5 years old or less in the household who were between the ages of 18 and 45 at the time of childbirth in the 44 sample states in the 1989-2010 Annual March CPS files. The sample omits all individuals who moved from abroad last year and assigns the state of last year's residence as the state of child's birth. All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, a quadratic polynomial in the survey year, and state-specific time trends. All regressions are weighted by the March CPS Supplement person weights. Robust standard errors are clustered on the state level.

Disregard refers to how much child support income is ignored in the calculation of benefits. A disregard of $50 means that a mother can keep up to $50 per month of the child support income without it affecting her welfare benefit receipt. Any child support income above the disregard amount is essentially taxed away by the state. Data on disregard amounts is available for 1990-2003 only. Please see the text for more details.

Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.01$

Table 2.8: Effects of IHVPE on Maternal Relationships and the Characteristics of Mothers' Spouses

Dependent Variable	N	Coefficient	SE
Mother is…			
Never Married (CPS-CSS)	36,243	0.0169**	(0.0061)
Married to Someone Other than Biological Father (CPS-CSS)	36,243	0.0060**	(0.0027)
Cohabiting with the Biological Father (NHIS)	--	-0.0037	(0.0033)
Cohabiting with Someone Other than Biological Father (NHIS)	--	0.0086**	(0.0024)
Mother's Spouse…			
Had Any Own Wage Income in the Past Year (March CPS)	140,487	0.0108**	(0.0044)
Worked Any Usual Hours in the Past Year (March CPS)	140,487	0.0042+	(0.0023)
Is Aged 20-24 Years (March CPS)	140,487	-0.0084**	(0.0031)
Is Aged 25-34 Years (March CPS)	140,487	0.0125+	(0.0072)
Is Aged 45+ Years (March CPS)	140,487	0.0073**	(0.0036)
Has Less than High School Education (March CPS)	140,487	-0.0011	(0.0043)
Has High School Degree (March CPS)	140,487	0.0072	(0.0073)

Notes: Each coefficient is from a separate regression. Please refer to Table 2.5 for details about the CPS-CSS data, and to Table 2.7 for details about the March CPS data. The March CPS sample is further limited to mothers who can be linked to their spouses in the household. The NHIS regressions use data from the Sample Child Files over 1997-2010 on all women with a sample child aged 7 years old or less in the household who were between the ages of 18 and 45 at the time of childbirth in the 43 sample states. Sample sizes in the NHIS data cannot be released due to confidentiality concerns. Please see the text for more information about approximate sample sizes. All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, a quadratic polynomial in the survey year, and state-specific time trends. The regressions using CPS-CSS and March CPS are weighted by the CPS person weights, while the regressions using NHIS are weighted by the sample child weights. Robust standard errors are clustered on the state level.
Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.01$

Table 2.9: Effects of IHVPE on Married Fathers' Characteristics: CPS-CSS 1994-2008

Dependent Variable	N	Coefficient	SE
Father's Age	27,683	0.1915	(0.1316)
Father's Age <20	27,683	-0.0001	(0.0011)
Father's Age 20-24	27,683	-0.0199**	(0.0063)
Father's Age 45+	27,683	0.0129+	(0.0075)
Father's Ed: <HS	27,683	-0.0030	(0.0083)
Father's Ed: HS Degree	27,683	0.0069	(0.0123)
Father's Ed: Some College	27,683	0.0034	(0.0139)
Father Receives Any Public Assistance	27,683	-0.0042**	(0.0015)

Notes: Each coefficient is from a separate regression. The sample of analysis includes all women married to the biological fathers of their children with a youngest child aged 5 years old or less in the household who were between the ages of 18 and 45 at the time of childbirth in the 44 sample states in 1994, 1996, 1998, 2000, 2002, 2004, 2006 and 2008. The sample omits all women who moved from abroad last year and assigns the state of last year's residence as the state of child's birth. All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, a quadratic polynomial in the survey year, and state-specific time trends. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.
Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.001$

Table 2.10: IHVPE and Formal and Informal Father Involvement: CPS-CSS 1994-2008

Dependent Variable	N	Coefficient	SE	N	Coefficient	SE	N	Coefficient	SE
Mother Eligible to be Asked CS Supplement Questions	36,243	0.0166**	(0.0082)						
	Whole CSS Sample			Trimmed CSS Sample - Lower Bound			Trimmed CSS Sample - Upper Bound		
Father Made Any CS Payments in Last Year	7,507	0.0135	(0.0204)	7,469	0.0125	(0.0206)	7,486	0.0146	(0.0205)
Father Made All CS Payments in Last Year	7,507	0.0472**	(0.0212)	7,470	0.0426**	(0.0207)	7,486	0.0473**	(0.0213)
Father Paid On Time All or Most of the Time in Last Year	6,141	0.0280	(0.0307)	6,108	0.0250	(0.0306)	6,121	0.0282	(0.0309)
Father Has Court-Ordered Visitation Rights	8,361	0.0063	(0.0307)	8,319	0.0045	(0.0307)	8,336	0.0074	(0.0308)
Number Days Father Spent with Child	7,733	7.4545	(5.0844)	7,696	6.0184	(4.7791)	7,713	7.5768	(5.0898)
Father Provided Gifts for Child	8,361	0.0501	(0.0353)	8,320	0.0469	(0.0351)	8,338	0.0511	(0.0353)
Father Provided Clothes for Child	8,361	0.0309	(0.0244)	8,319	0.0296	(0.0242)	8,338	0.0316	(0.0243)
Father Provided Food for Child	8,361	0.0441+	(0.0220)	8,318	0.0427+	(0.0220)	8,337	0.0441+	(0.0221)
Father Covered Childcare Expenses for Child	8,361	0.0322**	(0.0149)	8,322	0.0270+	(0.0147)	8,337	0.0326**	(0.0150)
Father Paid for Medical Expenses for Child	8,361	0.0551**	(0.0202)	8,322	0.0497**	(0.0202)	8,337	0.0553**	(0.0202)

Notes: Each coefficient is from a separate regression. The sample of analysis includes all women with a youngest child aged 5 years old or less in the household who were between the ages of 18 and 45 at the time of childbirth in the 44 sample states in the biannual Child Support Supplement (CSS) over 1994-2008. The sample omits all women who moved from abroad last year and assigns the state of last year's residence as the state of child's birth. Mothers are eligible to be asked CSS questions if they have a biological child in the household whose father lives outside the household.

The trimmed samples are constructed as follows. I estimate separate regressions of equation (2.5) with an indicator for being in the CSS sample as the outcome for 16 mutually exclusive groups of mothers defined by interactions between maternal education (less than high school, high school degree, some college, college or more) and race (non-Hispanic white, non-Hispanic black, Hispanic, and other) categories. For each group, g, I obtain the coefficient on the IHVPE indicator, β_g, and then trim the group by $(\beta_g * 100)$ percent of the post-IHVPE sample if β_g is at least significant at the 10% level. For each outcome, the lower-bound sample drops post-IHVPE observations that are in the top $(\beta_g * 100)$ percent of the post-IHVPE outcome distribution, while the upper-bound sample drops post-IHVPE observations that are in the bottom $(\beta_g * 100)$ percent of the post-IHVPE outcome distribution.

All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, a quadratic polynomial in the survey year, and state-specific time trends. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.

Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Table 2.11: Effects of IHVPE on Child Health Insurance Provision: CPS-CSS 1994-2008

	Any Private Health Insurance Coverage					Coverage by Member of Household	Coverage by Person Outside Household	Coverage by Medicaid	Coverage by CHIP	Any Health Insurance Coverage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IHVPE Program Exists in State and Year of Child's Birth	-0.0265**	-0.0222**	-0.0223**	-0.0216**	-0.0266**	-0.0283**	0.0017	0.0068	0.0184	-0.0070
	(0.0103)	(0.0101)	(0.0108)	(0.0107)	(0.0100)	(0.0111)	(0.0039)	(0.0105)	(0.0192)	(0.0107)
Mother and Child Controls	√	√	√	√	√	√	√	√	√	√
Year FEs	√	√	√	√	√	√	√	√	√	√
State FEs	√	√	√	√	√	√	√	√	√	√
Quadratic Polynomial in Survey Year	√	√	√	√	√	√	√	√	√	√
State Time-Varying Characteristics Controls		√	√	√	√	√	√	√	√	√
Child Support Laws Controls			√	√	√	√	√	√	√	√
State EITC Implementation				√	√	√	√	√	√	√
AFDC/TANF Implementation				√	√	√	√	√	√	√
State-Specific Time Trends					√	√	√	√	√	√
N	38,449	37,457	36,243	36,243	36,243	36,243	36,243	36,243	15,178	36,243
R-squared	0.2542	0.2557	0.2568	0.2569	0.2579	0.2474	0.0179	0.2056	0.0738	0.0493

Notes: Each column is a separate regression. Please refer to Table 2.5 for more details about sample restrictions and controls. All regressions are weighted by the CPS person weights. Information on CHIP coverage is only available in 2002, 2004, 2006, and 2008 in the CPS-CSS. Robust standard errors are clustered on the state level.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Table 2.12: Effects of IHVPE on Mothers' Labor Supply: March CPS 1989-2010

	Dependent Variable: Mother Worked Any Usual Hours Last Year				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
IHVPE Program Exists in State and Year of Child's Birth	0.0140**	0.0166**	0.0183**	0.0184**	0.0181**
	(0.0065)	(0.0074)	(0.0076)	(0.0075)	(0.0076)
Mother and Child Controls	√	√	√	√	√
Year FEs	√	√	√	√	√
State FEs	√	√	√	√	√
Quadratic Polynomial in Survey Year	√	√	√	√	√
State Time-Varying Characteristics Controls		√	√	√	√
Child Support Laws Controls			√	√	√
State EITC Implementation				√	√
AFDC/TANF Implementation				√	√
State-Specific Time Trends					√
N	212,504	190,249	184,562	184,562	184,562
R-squared	0.0640	0.0644	0.0640	0.0642	0.0652

Notes: Each column is a separate regression. Please refer to Table 2.7 for details about sample restrictions and controls. All regressions are weighted by the March CPS Supplement person weights. Robust standard errors are clustered on the state level.
Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.01$

Table 2.13: Effects of IHVPE on Measures of Child Well-Being

Dependent Variable	N	Coefficient	SE
Log Household Income (March CPS)	183,283	0.0074	(0.0099)
Any Asthma Episodes in Last 12 Months (NHIS)	--	0.0025	(0.0066)
Any Ear Infections in Last 12 Months (NHIS)	--	-0.0027	(0.0062)
Any Learning Disability (NHIS, ages 3+)	--	-0.0090+	(0.0052)
Child is Unhappy Sometimes or Often (NHIS, ages 2-3)	--	-0.0102	(0.0142)
Any Well-Child Visits in Last 12 Months (NHIS)	--	-0.0199**	(0.0094)
Any Doctor Visits in Last 12 Months (NHIS)	--	-0.0148**	(0.0069)

Notes: Each coefficient is from a separate regression. Please refer to Tables 2.7 and 2.8 for details about sample restrictions and controls in the March CPS and NHIS data. Sample sizes in the NHIS data cannot be released due to confidentiality concerns. Please see the text for more information about approximate sample sizes. The regressions using March CPS data are weighted by the March CPS Supplement person weights, while the regressions using NHIS are weighted by the sample child weights. Robust standard errors are clustered on the state level.
Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.01$

Chapter 3

WIC in Your Neighborhood: New Evidence on the Impacts of Geographic Access to Clinics

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3.1 Introduction

A growing body of evidence suggests that *in-utero* conditions and health at birth matter for individuals' later-life well-being (Almond and Currie, 2011a,b). The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) is the major program in the United States that aims to improve the health and nutritional well-being of low-income pregnant and postpartum women and children under age five, and thus has potential to improve the life chances of the children who benefit from it. Program participants receive free nutritional food packages, education about health, nutrition, and the benefits of breastfeeding, as well as referrals to other agencies such as public health clinics, Medicaid, the Supplemental Nutrition Assistance Program (formerly known as Food Stamps), housing services, and job banks, among others.

In 2011, Congress appropriated $6.7 billion to fund WIC, and the program serves approximately 2 million women and 7 million children per month.[1] Yet despite the continued growth of the program from its inception in 1974, the debate on the effectiveness of WIC has not been settled. This paper seeks to inform this debate in two ways. First, I analyze whether geographic access to WIC clinics affects WIC food benefit take-up, a question that has not been previously thoroughly addressed. Then, I use a novel identification strategy that relies on within-ZIP-code variation in WIC clinic openings and closings in Texas over 2005-2009 with maternal fixed effects, and accounts for the potential endogeneity of mobility, gestational-age bias, and measurement error in gestation, to provide plausibly causal estimates of the effects of access to WIC clinics on pregnancy behaviors, birth outcomes and breastfeeding.

The fact that not all eligible individuals take-up benefits has been documented for many

[1] Information about the WIC program is available at http://www.fns.usda.gov/wic/WIC-Fact-Sheet.pdf

public programs (see Currie, 2006 for a review), and for WIC in particular (Bitler *et al.*, 2003). One hypothesis is that geographic access to WIC clinics may affect WIC participation. However, no past studies have rigorously tested this hypothesis.[2] In fact, in a review of the literature on WIC, Ludwig and Miller (2005) write that "...more evidence on what drives WIC participation would be extremely valuable for both research and policy." Past research does find that distance to social service agencies that administer the childcare subsidy process affects the likelihood of childcare subsidy take-up (Herbst and Tekin, 2010).[3] Further, evidence from psychology and behavioral economics suggests that proximity to program offices may be particularly salient for take-up because it can lead to more awareness of program existence, more frequent reminders to sign up, and reduced "hassle" costs (Bertrand *et al.*, 2006). It is therefore conceivable that geographic access to WIC clinics determines pregnant women's likelihood of signing up for and receiving WIC benefits.

Additionally, whether WIC actually affects infant health and breastfeeding remains an open question. Many of the existing studies on WIC rely on comparisons between WIC participants and non-participants and may suffer from omitted variables bias due to non-random selection into WIC participation (e.g. Bitler and Currie, 2005; Joyce *et al.*, 2005; Joyce *et al.*, 2008; Figlio *et al.*, 2009).[4] Hoynes *et al.* (2011) present a notable improvement

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[2]It is important to note that Bitler *et al.* (2003) do include a measure of geographic access — the number of WIC agencies per capita in each state — in their study of the determinants of WIC benefit take-up. However, this variable may be correlated with other state time-varying characteristics that affect benefit take-up such as shocks to labor market conditions or demographic shifts that might affect the demand for WIC services. In my study, I exploit a much finer level of variation in geographic access at the ZIP code level, and include maternal fixed effects to limit the possibility of omitted variables bias.

[3]Past research has also considered distance to sites for health and educational services. For example, Kane and Rouse (1993) and Card (1993) use distance to the nearest college as an instrument for educational attainment, while Currie and Reagan (2003) estimate the impacts of distance to the nearest hospital on access to care.

[4]WIC participants may differ from non-participants along unobservable dimensions such as knowledge about the program, access to transportation, and the ability to navigate bureaucratic application processes. An additional issue is that many of these studies measure WIC participation with an indicator for receipt of food benefits. However, as discussed in more detail throughout this paper, WIC clinic access confers a

upon the existing literature by using county-year variation in the initial roll-out of the WIC program in the 1970s for identification. However, despite the important methodological contributions to the literature, their study is restricted by missing information on important variables such as WIC participation, food receipt, and breastfeeding in their older birth records data, and cannot speak to the impacts of WIC in current times.[5]

This paper uses restricted data from the universe of Texas birth records over 2005-2009 together with administrative data on the locations and dates of openings and closings of all WIC clinics that operated in Texas over this time period. The births data contain information on mothers' full maiden names, exact dates of birth, states or countries of birth, and ZIP codes of residence, which allows me to link siblings born to the same mother and determine whether mothers had an operating WIC clinic in their ZIP codes of residence during their pregnancies.[6] Additionally, unlike older birth records data, these data contain information on WIC food receipt during pregnancy, a wide range of pregnancy behaviors, as well as on breastfeeding at the time of hospital discharge.

My data allow me to compare births by mothers who did and did not have a WIC clinic in their ZIP code of residence during pregnancy, and to include maternal fixed effects to control for all time-invariant characteristics of mothers that may be correlated with residential location, WIC participation, and birth outcomes. However, there are several potential

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variety of other benefits to clients such as health check-ups, health education, and referrals to other services. Studies that focus on the effects of food benefit receipt cannot speak to the possible additional impacts of the other aspects of the WIC program.

[5]Since the authors do not observe WIC participation or food receipt in their data, their estimates represent intent-to-treat reduced-form effects of county-level access to WIC on birth weight, and cannot address a crucial question of the extent to which having a WIC clinic in one's county of residence affects WIC food benefit take-up. They do calculate "treatment-on-the-treated" effects by scaling their coefficients by estimates of WIC participation rates from other data sources. However, as discussed in more detail in Section 3.5, it is difficult to interpret these effects without a "first-stage" estimate of the effect of county-level WIC access on WIC participation.

[6]As discussed briefly in Section 3.5, I also use mothers' exact residence addresses in some alternative specifications that estimate the effect of distance to the nearest WIC clinic.

issues with this approach. First, a mother's residential location during pregnancy is possibly endogenous as her decision to move between pregnancies may be correlated with determinants of WIC clinic openings and closings (for example, unemployment shocks may lead to increased mobility following job loss and greater demand for WIC services). A second problem is that the use of maternal fixed effects may exacerbate biases due to measurement error. A third issue, which has been pointed out by other researchers (e.g., Joyce *et al.*, 2005; Ludwig and Miller, 2005; Joyce *et al.*, 2008), is that longer gestation periods are mechanically correlated with a higher likelihood of WIC use as women with longer pregnancies have more time to receive WIC services. Since gestation is correlated with other birth outcomes such as birth weight, the estimates of access to WIC on infant health will be biased upward as a result of this issue.[7]

To account for the potential endogeneity of maternal residence, measurement error, and the mechanical correlation between gestation and WIC, I implement a maternal fixed effects-instrumental variables strategy. My instrument is an indicator for whether the mother would have had an open WIC clinic during the current pregnancy *assuming she continued to live in the first ZIP code in which I observe her, and assuming her pregnancy lasted 39 weeks following her estimated week of conception.*[8] Since the first ZIP code is a fixed characteristic of the mother, the first ZIP code itself does not have an independent effect on WIC use or birth outcomes in models that include maternal fixed effects. This instrument is highly correlated with whether the mother had an open WIC clinic during the actual gestation length of her current pregnancy and in her actual ZIP code of residence, but should have no independent

[7]In the above design, since women with longer pregnancies are more likely to experience a clinic opening, there is a positive correlation between WIC clinic presence and gestation.

[8]The first ZIP code of observation is the mother's ZIP code of residence at the time of her first birth occurring between January 2005 and December 2009. The mother may have had children born prior to 2005, but those children are not included in the analysis. The week of conception is calculated by subtracting gestation length in weeks from the child's exact birth date.

effect on prenatal WIC food benefit receipt, other pregnancy behaviors, birth outcomes, or breastfeeding. I provide empirical evidence demonstrating that within-ZIP-code variation in WIC clinic presence is generally uncorrelated with changes in observable maternal characteristics, which reinforces the validity of my identification strategy. Importantly, unlike previous studies on WIC that use maternal fixed effects, my approach relies on variation in WIC access stemming only from WIC clinic openings and closings, rather than from other factors that may affect a woman's decision to participate in WIC during one pregnancy and not during another.

My results suggest that geographic access to WIC is fairly important. The presence of a WIC clinic in a mother's ZIP code of residence during pregnancy increases her likelihood of WIC food receipt by about 6 percent at the sample mean. The estimated coefficients are higher for mothers with a high school education or less, who are most likely to be eligible for WIC. Additionally, I find that the effect on WIC food benefit take-up is concentrated among mothers in urban areas and driven by access to WIC clinics located in non-health-department facilities such as community centers, schools, churches, and mobile vehicles. These clinics are more likely to have visible advertisements for WIC than those located inside county health departments or large hospitals.[9] These results suggest that proximity to WIC clinics affects take-up through dimensions other than travel cost savings — for example, women with WIC clinics in their (urban) ZIP codes may be more likely to physically see them on a regular basis and thus become aware of the program and be reminded to sign up.

I also find that WIC clinic access increases pregnancy weight gain and increases the likelihood that less-educated mothers are recorded as having diabetes or hypertension during pregnancy. The latter effect may be driven by higher diagnosis rates of such conditions at WIC clinics or through referrals from WIC, suggesting that aspects of the WIC program

[9] See, for example, the mobile WIC clinic in Figure 3.1.

other than the food benefits matter.[10]

Finally, I show that WIC clinic presence is associated with a 27 gram increase in average birth weight (a 0.8 percent increase at the sample mean). This increase is mostly concentrated in the middle of the birth weight distribution; while there is a marginally significant decline in low-birth-weight (less than 2,500 grams) births among low-educated mothers, there are no effects on the likelihood of high-birth-weight (above 4,500 grams) births. For mothers with a high school degree or less, I also document a positive effect on breastfeeding — the likelihood that a child is being breastfed at the time of discharge from the hospital increases by about 6 percent. This result is a novel estimate of the causal effect of access to WIC on breastfeeding, as most of the recent studies have not had this outcome available in their data (e.g. Hoynes *et al.*, 2011).

While I demonstrate that WIC clinic presence is a determinant of WIC food receipt, WIC clinics can affect birth outcomes and breastfeeding through other channels, such as the health check-ups, the provision of educational materials about maternal and child health, and referrals to other social services. My analysis is limited because I cannot estimate a first-stage effect that relates WIC clinic access to WIC participation — food benefit take-up is only a proxy for participation in the variety of services available through the WIC program. As a result, I cannot scale the reduced-form WIC clinic access coefficients to accurately measure the impacts of participation in WIC. However, my reduced-form estimates are nevertheless policy-relevant as they can help inform the debate on the costs and benefits of operating WIC clinics in the current policy context.

This paper proceeds as follows. I discuss the WIC program and the related literature in

[10]It is important to note that my results show that while WIC clinic access reduces the likelihood of gaining too little weight (defined as less than 16 lbs), it also increases the likelihood of gaining too much weight (defined as more than 40 lbs). These results point to a possible unintended consequence of WIC for maternal health and suggest that more emphasis on nutrition education may be needed in the program. The effects on diabetes and hypertension diagnoses could also be driven by the increase in the likelihood of gaining too much weight during pregnancy.

more detail in Section 3.2, and provide information on the data and sample in Section 3.3. Section 3.4 presents the empirical methods, while Section 3.5 discusses the results and some robustness checks. Section 3.6 concludes.

3.2 Background

The WIC program was first established as a pilot program in 1972, implemented in 1974, and then permanently expanded to most U.S. counties by the end of the 1970s (Hoynes *et al.*, 2011). The goal of the program is to improve the health and nutritional well-being of low-income pregnant and post-partum women, infants, and young children by providing them with nutritious food packages and health education. In Texas, as well as in other states, eligibility rules require participants to live in households with incomes below 185% of the poverty line and to be "at nutritional risk".[11] Participating pregnant and post-partum women, as well as parents and guardians of children under age five, receive monthly benefits from WIC that can be taken to grocery stores and used to buy nutritious foods. WIC foods include iron-fortified infant formula and infant cereal, iron-fortified adult cereal, vitamin C-rich fruit and vegetable juice, milk, eggs, cheese, beans, and peanut butter.

For pregnant and post-partum women, another important component of WIC is education about nutrition, health, and breastfeeding.[12] The specific emphasis on the importance

[11]In Texas, WIC clients receive an initial health and diet screening at a WIC clinic to determine nutritional risk. WIC uses two main categories of nutritional risk: (1) medically-based risks such as a history of poor pregnancy outcome, underweight status, or iron-deficiency anemia, and (2) diet-based risks such as poor eating habits that can lead to poor nutritional and health status. Clients are counseled at WIC about these risks and the outcomes influenced by nutrition education and nutritious foods provided by WIC. (Information from the Texas Department of State Health Services: http://www.dshs.state.tx.us/wichd/gi/eligible.shtm).

[12]According to the Texas Department of State Health Services website, "clients receive encouragement and instruction in breastfeeding. In many cases, breastfeeding women are provided breast pumps free of charge. WIC helps clients learn why breastfeeding is the best start for their baby, how to breastfeed while still working, Dad's role in supporting breastfeeding, tips for teens who breastfeed, how to pump and store breastmilk, and much more." (See http://www.dshs.state.tx.us/wichd/gi/eligible.shtm for more information.)

of breastfeeding provides motivation for rigorously evaluating the extent to which WIC affects breastfeeding rates of new mothers. This is particularly interesting given that WIC participants can also obtain free infant formula, so the effects of WIC on breastfeeding are *a priori* ambiguous. This paper attempts to address this question by using recent data with breastfeeding information together with an identification strategy that can arguably isolate the effects of access to WIC from other determinants of breastfeeding.[13]

Another important component of WIC in more recent years has been the coordination with other social services. The promotion of coordination efforts stems from the national level. For example, in 2000, the U.S. Department of Agriculture distributed a handbook to all WIC state and local agencies that outlines twelve "model coordination sites".[14] In Texas, the WIC Policy and Procedures Manual has a clause that WIC clinics must establish a "referral mechanism for the provision of health services to WIC participants in need of such services", and develop concrete strategies to identify "high-risk conditions requiring referral and the procedures for follow-up". WIC clinic staff are also instructed to provide referrals for clients to a number of other agencies including public health clinics, Medicaid, Food Stamps, Temporary Assistance for Needy Families (TANF), literacy services, job banks, housing services, child support enforcement, and drug and alcohol abuse programs, among others. The WIC Policy and Procedures Manual highlights that "posting information in a public area does not satisfy this requirement. Applicants must individually receive written information [about these services] at initial application."[15] Thus, WIC clinics can serve as

[13]The existing literature, which compares WIC participants to non-participants, finds mixed results on the association between WIC use and breastfeeding — Bitler and Currie (2005) find a positive relationship between WIC and breastfeeding, while Jacknowitz *et al.* (2007) and Chatterji *et al.* (2002) find a negative association. More recent studies that use more rigorous identification methods do not have data on breastfeeding, and thus cannot address this question (Hoynes *et al.*, 2011; Figlio *et al.*, 2009). An important distinction between my study and the others that consider breastfeeding is that I measure the effects of *access* to WIC rather than WIC participation.

[14]See http://www.fns.usda.gov/wic/resources/strategies.htm for more information.

[15]See the WIC Policies and Procedures Manual available at:

gateways for low-income women and children to receive other social services. As a result, access to a WIC clinic may have impacts on their health and well-being through other channels than just WIC food receipt. This issue has not been explicitly addressed in much of the previous literature.[16] This paper seeks to shed light on some mechanisms through which WIC may affect infant health by studying the effects of WIC clinic access on various pregnancy conditions and behaviors.

In Texas, geographic access to WIC clinics is likely important because clients must apply for WIC in person. Local WIC clinics are operated in a variety of facilities including county and city health departments, hospitals, schools, community centers, churches, and mobile vehicles, among others. Figure 3.1 shows an example of a mobile WIC clinic in McAllen, Texas, which is clearly labeled with an advertisement for offering WIC services. Prospective clients must schedule an appointment at a WIC clinic, and are required to bring documentation of their household income and Texas residence to the appointment.[17] During the appointment, applicants undergo a health screening, and receive education and counseling, as well as referrals to other agencies as applicable. At the end of the appointment, WIC eligibility is determined, and food benefits are provided to those who are eligible in three-month increments. WIC participants must go to a WIC clinic at least once every three months to receive their next set of food benefits, and can receive additional health and referral services at those times. Thus, it seems that, especially for low-income women who are likely to be time- and transportation-constrained, living in proximity of a WIC clinic may be particularly

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http://www.dshs.state.tx.us/wichd/policy/tocga.shtm for more information.

[16]An important exception is the study by Bitler and Currie (2005), which estimates the relationship between WIC use and prenatal care initiation. However, their analysis relies on comparing WIC participants to other mothers on Medicaid. WIC participants may be more knowledgeable about the program or have greater access to transportation than WIC-eligible non-participants who receive Medicaid. If these unobserved variables are correlated with prenatal care initiation, then their analysis may be confounded by omitted variables bias.

[17]WIC clients must be Texas residents. U.S. citizenship is *not* a requirement for WIC eligibility.

advantageous.

Further, a growing literature in behavioral economics can speak to the importance of contextual factors in people's decision-making processes (see Bertrand *et al.*, 2006 for an overview), and argues that small situational changes can have significant impacts on people's behaviors. With regards to welfare programs, Bertrand *et al.* (2006) highlight three factors that can serve as considerable barriers to take-up: lack of knowledge about the program, hassle costs (such as tedious and complicated application forms or long wait times at program offices), and procrastination. In the context of WIC, all of these factors may be affected by geographic access to clinics.[18]

My analysis uses variation in WIC clinic openings and closings to provide some of the first evidence on the impacts of geographic access to WIC clinics. While the empirical literature on WIC dates back several decades (see, for example, Devaney, 1992 and Ahluwalia *et al.*, 1998 for early evidence on the positive association between WIC food receipt and birth weight), many studies focus on participation in WIC rather than access, and face difficulties in overcoming the challenge of non-random selection into WIC participation (Besharov and Germanis, 2001). Further, most of the existing studies equate participation with food benefit receipt, which prevents them from measuring any additional impacts of the other WIC services.

Recent work has attempted to deal with the issue of selection into WIC participation by using more narrowly defined comparison groups (Bitler and Currie, 2005; Joyce *et al.*, 2005, 2008; Figlio *et al.*, 2009), employing propensity score matching methods (Gueorguieva *et al.*, 2009), including maternal fixed effects (Brien and Swann, 2001; Chatterji *et al.*, 2002; Kowaleski-Jones and Duncan, 2002), and using variation in state program parameters as

[18]Proximity to a WIC clinic likely increases the likelihood that a woman will see it on a regular basis, thus informing her of the existence of the program and serving as a reminder to sign up for services. Additionally, hassle costs may be reduced if women can more easily stop by either on the way to or from work, for example.

instruments (Chatterji *et al.*, 2002). Yet the findings from these studies are mixed, arguably in part because they are still plagued by identification issues.[19] Studies that rely on narrowly defined comparison groups and propensity score matching may still suffer from bias due to selection on unobservable variables (such as knowledge about the program, access to transportation, ability to navigate bureaucratic application processes, and changes in family circumstances that affect eligibility status), while studies that include maternal fixed effects may be confounded by other time-varying within-family changes between sibling births (such as parental marriage or divorce and changes in family wealth). Additionally, variation in WIC parameters across states is not large, and thus these variables create poor instruments for WIC participation (Bitler and Currie, 2005).

As mentioned above, Hoynes *et al.* (2011) rely on county-year variation in initial WIC program roll-out in the 1970s for identification and, to my knowledge, present the only existing evidence on the impacts of *geographic access to* WIC rather than WIC participation. They show that program implementation was uncorrelated with other determinants of birth outcomes, and find that county-level access to WIC services leads to modest improvements in birth weight. This paper builds on the work of Hoynes *et al.* (2011) by using finer variation in WIC clinic access within ZIP codes rather than counties, incorporating maternal fixed effects, and implementing an instrumental variables approach to address endogenous mobility and to account for the mechanical correlation between gestation length and WIC access. Further, this paper estimates the effects of WIC access on a wider range of outcomes including WIC food benefit receipt, pregnancy weight gain, pregnancy health conditions and behaviors, birth weight, and breastfeeding. Finally, estimates of access to WIC from a more current time period are arguably more valuable for policymaking purposes today.

[19] Estimates of the effect of WIC on the likelihood of low birth weight range from no effect for the whole sample (Joyce *et al.*, 2005) to a 30 percent reduction (Bitler and Currie, 2005) to an over 100 percent reduction (Figlio *et al.*, 2009).

3.3 Data and Sample

3.3.1 Data on WIC Clinics

My data on WIC clinic locations and opening and closing dates come from a public records request from the Texas Department of State Health Services. These data contain the names, addresses (including ZIP codes), and opening and closing dates for all WIC clinics in Texas that were either operating in 2010 or that were closed sometime over 1992-2010.[20] WIC clinic opening dates were not consistently reported in the 1990s, but have been much more reliably recorded over the last decade. For the purposes of my main analysis, I only use information on WIC clinic openings and closings over 2005-2009. I extend the time period to 2003-2010 for the placebo analysis, which relies on information on WIC clinics 6-9 months before conception or after childbirth, as discussed in Section 3.5.

Figure 3.2 plots the number of operating WIC clinics by month in Texas from January 2005 to December 2009, the time period of my analysis. The number of WIC clinics has decreased from 614 clinics in January 2005 to 564 clinics in December 2009. In Texas, local WIC agencies have control over opening, closing, moving, and consolidating WIC clinics in their jurisdictions. These decisions are made for a variety of reasons, such as for space constraints (since many WIC clinics are operated at churches, community centers, and schools) and for cost-efficiency when multiple WIC clinics are located in proximity to one another. Additionally, WIC clinics may be closed when there is no longer a WIC approved grocery store or

[20]Theoretically, the information on WIC clinic addresses should allow me to measure WIC clinic access using distances from mothers' homes rather than simply at the ZIP code level. However, street addresses are poorly recorded in the WIC clinic data. Geocoding these addresses introduced substantial measurement error, and hand checking a random sample of 50 WIC clinic addresses suggested that a large fraction of WIC clinic street addresses were incorrectly recorded. On the other hand, ZIP codes are generally reliably recorded and can be cleanly merged to the birth records data. Therefore, I rely on ZIP-code-level measures in my main analysis. I have also estimated some alternative specifications using continuous distance to the nearest WIC clinic as the measure of access to WIC. These results are qualitatively similar to the main results and are discussed in Section 3.5.

vendor operating in the area.[21] In recent work, Meckel (2012) shows that the introduction of the Electronic Benefit Transfer (EBT) system in 2004 has increased the surveillance of WIC vendors, and induced many vendors (who were likely previously engaged in fraudulent behavior) to exit the WIC program by either shutting down or no longer accepting WIC food benefits in Texas. Consequently, it may be the case that the decline in WIC clinics over 2005-2009 is at least in part driven by the decline in WIC grocery stores over the same time period.

In my data, 578 Texas ZIP codes have had at least one open WIC clinic sometime over 2005-2009. Only 65 ZIP codes have ever had more than one WIC clinic operating in any given month, so the relevant measure of access for most women is an indicator for having at least one open WIC clinic in their ZIP code of residence. Over 2005-2009, 114 Texas ZIP codes experienced either a WIC clinic opening or closing, providing substantial within-ZIP-code variation in WIC clinic access. Figure 3.3 provides some indication of this variation by showing a histogram of the distribution of the 87 ZIP codes that have had a non-zero change in the number of operating WIC clinics between January 2005 and December 2009. Note that this is an undercount of all ZIP codes that have had openings or closings since it just considers the difference in the number of WIC clinics between the first and last month in my data. Consistent with evidence on the decline in the number of WIC clinics from Figure 3.2, most ZIP codes with a change have had a one-clinic decrease over this time period.

3.3.2 Data on Births

I use restricted data from the universe of Texas birth records over 2005-2009, which are available through a special application process to the Texas Department of State Health Services. This data set contains 2,037,181 birth records. I limit the sample to singleton

[21] Information on the determinants of WIC clinic openings and closings comes from personal communication with Ellen Larkin, the WIC state program specialist at the Texas DSHS (Larkin, 2012).

births with mothers who are Texas residents, with non-missing information on the child's date of birth, mother's date of birth, mother's full maiden name, mother's birth state or country, and mother's ZIP code of residence (N=1,937,003). The 8,431 births with missing gestation or gestation less than 26 weeks are also dropped.[22] I match siblings to the same mother using information on her full maiden name, exact date of birth, and birth state or country. The resulting sibling sample consists of 612,694 births.

The births data are matched to WIC clinic data by the mother's ZIP code of residence. WIC clinic access during pregnancy is calculated by first estimating the conception date from information on the child's birth date and gestation length and then creating an indicator variable equal to 1 if at least one WIC clinic was operating at any point during the pregnancy in the mother's ZIP code of residence, and 0 otherwise.[23] The instrument is calculated similarly, except that the relevant ZIP code considered is the ZIP code of the mother's *first* pregnancy residence and gestation is assumed to be 39 weeks for all births.

Table 3.1 presents some summary statistics from the births data. Nearly 56 percent of all mothers report receiving WIC food benefits at some point during their pregnancies. Fifteen percent of mothers are aged less than 20 years at the time of childbirth, and 59 percent have a high school education or less. Fifty-nine percent of all mothers are married. Thirty-five percent of mothers are non-Hispanic white, 11 percent are black, while 51 percent are Hispanic. Average birth weight is around 3,300 grams, and 6 percent of births are low-birth-weight (<2,500g). Almost 75 percent of all mothers reported breastfeeding their infants at

[22]This results in less than 0.5 percent of the sample being dropped. Births with missing gestation are dropped because my analysis requires information on gestation to calculate the conception date. Births with gestation less than 26 weeks are dropped because they are extreme outliers in the data and have much worse outcomes than other births. For example, average birth weight among this group is 725.25 grams. Nevertheless, my results are not sensitive to this restriction.

[23]Results using an indicator equal to 1 if a WIC clinic was operating during the entire pregnancy (rather than at any point during pregnancy) are similar. Results using a continuous variable that measures the fraction of time during pregnancy that a WIC clinic was open are also similar. These results are discussed in Section 3.5.

the time of discharge from the hospital.

When I split the sample by whether or not the mother ever had a WIC clinic in her ZIP code of residence during any pregnancy, or by whether she had one during the current pregnancy, some differences emerge. WIC food benefit receipt is substantially higher among mothers living in the same ZIP codes as open WIC clinics. These mothers also tend to be less educated, are less likely to be married, and more likely to be Hispanic rather than non-Hispanic white or black. They also tend to have children with somewhat lower birth weights and have lower breastfeeding rates. These differences suggest that WIC clinics tend to locate in relatively less advantaged neighborhoods, where perhaps their services are most needed. As a result, simple comparisons between WIC participants and non-participants or comparisons of areas with and without WIC clinics will likely yield downward-biased results because of this negative selection. These differences point to the importance of finding methods that can overcome such selection issues to estimate the true causal effects of access to WIC on infant health and breastfeeding.

3.4 Empirical Methods

In an ideal research setting, one would conduct a randomized controlled trial to study the causal effects of WIC. One would randomly assign WIC access to women in a study population, and then compare the outcomes of the treatment and control groups.[24] However, absent such an experiment, researchers must develop identification strategies to overcome

[24]To my knowledge, only one study has conducted a randomized controlled trial to evaluate WIC. Metcoff *et al.* (1985) conducted a randomized study of WIC on 410 women in Oklahoma. Treatment women received WIC vouchers, while control women did not. They find that treatment group women had children with birth weights that were on average 91 grams higher than children of women in the control group. However, while these results are certainly supportive of a beneficial causal effect of WIC, external validity may be a problem due to the small, non-representative sample. Further, the study can only speak to the pure effects of WIC food receipt on birth weight in the 1980s, but cannot address the question of the effectiveness of other aspects of the WIC program, such as education and referrals, which are particularly prevalent today.

the issues resulting from non-random selection into WIC participation. In this study, I propose a novel identification strategy that relies on within-ZIP-code variation in WIC clinic openings and closings.

Without data on siblings, one could estimate the effects of access to WIC using the variation within ZIP codes over time. Specifically, one would use Ordinary Least Squares (OLS) to estimate an equation of the form:

$$Y_{iymzc} = \beta_0 + \beta_1 * WIC_{iymz} + \theta' X_{iymzc} + \alpha_z + \gamma_y + \delta_m + \rho_c * t + \epsilon_{iymzc} \tag{3.1}$$

for each child i born in year y, month m, with a mother residing in ZIP code z, and in county c. Y_{iymzc} is an outcome of interest such as an indicator for mother receiving WIC food benefits during pregnancy or birth weight. WIC_{iymz} is the key explanatory variable, which is equal to 1 if a WIC clinic was operating at any point during the time of the pregnancy in the mother's ZIP code of residence, and 0 otherwise. X_{iymzc} is a vector of maternal and child characteristics that includes indicators for mother's age (<20, 20-24, 25-34, 35-44, 45+), indicators for mother's race (non-Hispanic white, black, Hispanic, other), indicators for mother's education (less than high school, high school degree, some college, college or more), an indicator for the mother being married, and indicators for birth order. α_z are ZIP code fixed effects, γ_y are birth year fixed effects, δ_m are birth month fixed effects, while $\rho_c * t$ are county-specific linear time trends. ϵ_{iymzc} is a birth-specific error term. The key coefficient is β_1, which measures the effect of having an open WIC clinic in a mother's ZIP code of residence during her pregnancy on the outcome of interest.

Note that while ZIP codes with and without WIC clinics are likely different on a number of dimensions, time-invariant differences between them will be captured by ZIP code fixed effects. Additionally, county-specific linear time trends control for differences in linear trends in outcomes across counties. The identifying assumption for equation (3.1) is that the variation in WIC clinic openings and closings within ZIP codes is not correlated with other determinants of WIC participation or birth outcomes at the ZIP code level. This assumption may not be satisfied if common shocks lead to changes in the numbers of WIC clinics and also affect average ZIP-code-level birth outcomes. For example, if spells of foreclosures affect the characteristics of ZIP code populations, then they may change the demand for WIC services and also change average birth outcomes through selection effects and direct health effects.[25]

To address this issue, I take advantage of the data on sibling births, and estimate models that include maternal fixed effects. This is an improvement over a model with ZIP code fixed effects, since I can then control for all time-invariant observed and unobserved maternal characteristics by comparing children borne by the same mother. Specifically, I estimate:

$$Y_{ikymz} = \beta_0 + \beta_1 * WIC_{ikymz} + \theta' X_{ikym} + \alpha_k + \gamma_y + \delta_m + \epsilon_{ikymz} \tag{3.2}$$

for each child i, borne by mother k, in year y, month m, with the mother residing in ZIP code z during pregnancy. Now, α_k are mother fixed effects, and the vector X_{ikym} only includes time-varying maternal and child characteristics: indicators for mother's age, mother's education, mother's marital status, and birth order.[26] The rest of the coefficients and variables are the same as before. Note that several past studies have used mother fixed effects methods to estimate the effects of WIC (e.g., Brien and Swann, 2001; Chatterji *et al.*, 2002; Kowaleski-Jones and Duncan, 2002). However, the difference in the design presented here is that the within-mother variation in WIC access is coming only from WIC

[25]Currie and Tekin (2011) find that foreclosures have adverse effects on adult health. It is likely that pregnant women and infants would also experience such health effects.

[26]Note that for two-sibling families, a maternal fixed effects model is equivalent to a first-difference model, where maternal age is identified by the birth interval. I show below that in models with ZIP code fixed effects, WIC clinic access is uncorrelated with the number of births or with maternal age at childbirth. Consequently, it is reasonable to assume that maternal age is not endogenous, and can be included as a control.

clinic openings and closings, rather than from other (likely unobservable) factors that may influence whether a woman receives WIC services during one pregnancy and not during another.

In equation (3.2), the effect of WIC clinic access is identified using a sample of mothers who have at least one pregnancy in a ZIP code with an operating WIC clinic and at least one pregnancy in a ZIP code without a WIC clinic. These mothers are comprised of two groups: 1) mothers who always live in the same ZIP code but experience either a WIC clinic opening or closing between pregnancies, and 2) mothers who move ZIP codes between pregnancies and live in the same ZIP code as a WIC clinic during one pregnancy and not during another. However, the decision of whether to move or not may be correlated with other determinants of WIC clinic openings and closings, which could bias the estimates produced by equation (3.2). Additionally, fixed effects models may be biased towards zero in the presence of classical measurement error in the explanatory variable. The key explanatory variable in my analysis relies on information on gestational age to calculate exposure to a WIC clinic during the length of the pregnancy, and gestational age is likely to contain some measurement error.

A further issue with both equations (3.1) and (3.2) is gestational-age bias (Joyce *et al.*, 2005; Ludwig and Miller, 2005; Joyce *et al.*, 2008). In particular, women with longer gestation periods have more time to experience a WIC clinic opening or closing and to receive WIC services. Consequently, since women with longer pregnancies are more likely to experience a WIC clinic opening holding all else equal, we would expect to see a positive correlation between WIC clinic access and gestation, which in turn is correlated with better birth outcomes like higher birth weight. This would lead to an upward bias on the estimated effects of WIC access.

To address all of the above issues, I implement an instrumental variables - maternal fixed effects (IV-FE) approach. I consider the ZIP code in which I observe each mother during her *first* pregnancy. Then, for each subsequent pregnancy, I create a variable that is equal to 1 if a WIC clinic was operating at any point during the pregnancy in the mother's ZIP code *had she remained in her first ZIP code of residence and had her pregnancy lasted 39 weeks.*[27] In other words, this instrument measures the mother's hypothetical WIC clinic access if she never moved and if all of her pregnancies lasted the same length of time. This hypothetical variable is used to instrument the WIC_{ikymz} variable described above. Specifically, I estimate a second-stage equation of the form:

$$Y_{ikymz} = \beta_0 + \beta_1 * \widehat{WIC}_{ikymz} + \theta' X_{ikym} + \alpha_k + \gamma_y + \delta_m + \epsilon_{ikymz} \tag{3.3}$$

with the corresponding first-stage equation:

$$WIC_{ikymz} = \pi_0 + \pi_1 * FSTWIC_{iymk} + \phi' X_{ikym} + \psi_k + \sigma_y + \omega_m + u_{ikymz} \tag{3.4}$$

for each child i, borne by mother k, in year y, month m, with the mother residing in ZIP code z during pregnancy. Here, $FSTWIC_{iymk}$ is an indicator that is equal to 1 if a WIC clinic was operating at any point during the 39 weeks following conception in the mother's first-pregnancy ZIP code, and 0 otherwise. The other variables and coefficients are defined as before.

The idea behind this instrument is that although the mother's current pregnancy ZIP code is potentially endogenous, her first-pregnancy ZIP code of residence is controlled for by the inclusion of fixed effects. Consequently, identification comes only from variation in WIC clinic openings and closings in the mother's first-pregnancy ZIP code, which should be exogenous to any given mother. This instrument thus satisfies the conditions for being a valid instrument: it is highly predictive of WIC clinic presence in the mother's actual current

[27] The 39 weeks are counted forward from estimated conception week.

ZIP code of residence and during the actual gestation length of the current pregnancy (since many mothers do not move and have gestations close to 39 weeks), but it should have no effect on the outcomes of interest except through its effect on true WIC clinic access.[28]

3.5 Results

3.5.1 Relationship Between WIC Clinic Access and Maternal Characteristics

My identification strategy relies on within-ZIP-code variation in WIC clinic access over time. A crucial concern with this approach is that omitted variables are correlated with both WIC clinic access and pregnancy and birth outcomes. While I cannot directly test for all potential omitted variables, I can assess the degree to which the variation in WIC clinic access across space and time is correlated with maternal characteristics. Table 3.2 presents results from estimating a variant of equation (3.1) with various maternal characteristics as dependent variables, controlling for birth year and birth month fixed effects, and with standard errors clustered on the ZIP code level. I estimate these regressions both with and without ZIP code fixed effects.

The results without ZIP code fixed effects in Panel A point to substantial differences across areas that do and do not have WIC clinics. In particular, WIC clinics tend to locate in ZIP codes that have more disadvantaged mothers — mothers who are less than 20 years old, have a high school education or less, are unmarried and are Hispanic. This is perhaps not surprising as these mothers are also most likely to be eligible for WIC services. However, these differences also point to the fact that simply comparing outcomes in areas with and without WIC clinics will likely lead to downward biased estimates of the effects of WIC access on birth outcomes because of selection of WIC clinics into areas with potential recipients.

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[28]Other studies that use a very similar IV-FE design include Almond *et al.* (2011a) and Currie and Rossin-Slater (Forthcoming).

Panel B of Table 3.2 suggests that ZIP code fixed effects do a fairly good job of controlling for these differences. These regressions now test whether within-ZIP-code changes in WIC clinic access are correlated with changes in maternal characteristics. Most of the coefficients become much smaller and statistically insignificant, suggesting that trends in WIC clinic openings and closings are generally uncorrelated with trends in maternal demographics. However, there is still some evidence of selection — WIC clinics tend to operate in ZIP codes when they have fewer college-educated mothers and more black mothers. Note that this selection would likely lead to a downward bias on the results, as less-educated and minority mothers tend to have worse pregnancy and birth outcomes. Consequently, one can argue that my estimates of the impacts of WIC clinic access on these outcomes represent lower bounds for the true effects. These results also point to the potential benefits of including maternal fixed effects to compare children borne by the *same* mother, rather than simply using the within-ZIP-code variation in WIC clinic access with average ZIP-code-level outcomes.

In Appendix Table C.1, I examine the relationship between WIC clinic access and maternal mobility across ZIP codes. I estimate models of the form:

$$M_{ikymz} = \beta_0 + \beta_1 * WICfstpreg_{ikym} + \pi' WICfstpreg_{ikym} * X_{ikym} + \theta' X_{ikym}$$

$$+\alpha_z + \gamma_y + \delta_m + \epsilon_{ikymz} \tag{3.5}$$

for each child i, borne by mother k, in year y, month m, with the mother residing in ZIP code z during pregnancy. M_{ikymz} is an indicator that is equal to 1 if the mother moved ZIP codes between the current pregnancy and the first pregnancy, and 0 otherwise. $WICfstpreg_{ikym}$ is an indicator that is equal to 1 if a WIC clinic was operating in the mother's ZIP code of residence during her first pregnancy, and 0 otherwise. I estimate this equation with and without first ZIP code of residence fixed effects, α_z. The vector of coefficients on the interaction terms, π, allows me to assess whether moving likelihoods differ across maternal characteristics.

The results demonstrate that older, more educated, and married mothers with fewer children are more likely to move ZIP codes if there was a WIC clinic in their first ZIP code of residence. These findings suggest that women's decisions to move (or not) between pregnancies may be correlated with the determinants of WIC clinic openings and closings. In particular, less advantaged women tend to remain in the same ZIP codes if they had a WIC clinic during their first pregnancy, perhaps because common shocks lead both to increases in demand for WIC services and to decreases in mobility among these women. Consequently, implementing an IV-FE strategy to address endogenous mobility is essential for estimating the true causal effects of WIC clinic access on WIC food benefit receipt, pregnancy behaviors, birth outcomes, and breastfeeding.

3.5.2 WIC Clinic Access and Prenatal WIC Food Benefit Take-Up

Having provided some evidence for the validity of my empirical approach, I turn to the analysis of the effect of WIC clinic access on WIC food benefit take-up. Figure 3.4 provides some graphical representation of the relationship between WIC clinic access and the take-up of WIC food benefits during pregnancy among singleton births with mothers who reside in Texas.[29] I plot the average prenatal WIC food receipt by the number of months between conception and the time of at least one WIC clinic operating in the mother's ZIP code of residence. The sample is limited to conceptions in a 36-month period surrounding the time

[29] The sample is *not* limited to siblings in Figure 3.4.

that each ZIP code experienced a first WIC clinic opening or a last WIC clinic closing.[30] To combine conceptions in ZIP codes with first openings and last closings on the same graph, for conceptions in ZIP codes that experience a first WIC clinic opening, the x-axis variable is equal to "*year-month of the first WIC clinic opening* − *conception year-month*", while for conceptions in ZIP codes that experience a last WIC clinic closing, the x-axis variable is equal to "*conception year-month* − *year-month of last WIC clinic closing + 9*". Consequently, all x-axis values below 0 correspond to conceptions in ZIP codes with at least one WIC clinic operating during the entire pregnancy; x-axis values between 0 and 9 correspond to conceptions in ZIP codes where a WIC clinic opened or closed during pregnancy; and x-axis values above 9 correspond to conceptions in ZIP codes with no WIC clinics operating during pregnancy.[31]

The figure suggests that prenatal WIC food benefit receipt tends to be higher when at least one WIC clinic is present in the mother's ZIP code of residence.[32] The same pattern

[30]To understand the timing on the x-axis, consider for example all conceptions in January 2006. Assuming a 9 month gestation period, conceptions in ZIP codes with first openings before January 2006 will have WIC clinic exposure during the entire pregnancy; conceptions in ZIP codes with first openings between January 2006 and September 2006 will have partial WIC clinic exposure during pregnancy; while conceptions in ZIP codes with first openings after September 2006 will have no WIC clinic exposure during pregnancy. On the other hand, conceptions in ZIP codes with last closings before January 2006 will have no WIC clinic exposure during pregnancy; conceptions in ZIP codes with last closings between January 2006 and September 2006 will have partial WIC clinic exposure during pregnancy; and conceptions in ZIP codes with last closings after September 2006 will have WIC clinic exposure during the entire pregnancy. Mothers residing in ZIP codes that have experienced a first WIC clinic opening and a last WIC clinic closing within the same 36-month period are dropped (5 ZIP codes).

[31]Note that for first openings, if "*year-month of the first WIC clinic opening* − *conception year-month*" > 9, there is no WIC clinic exposure, if "*year-month of the first WIC clinic opening* − *conception year-month*" is between 0 and 9, there is partial WIC clinic exposure, and if "*year-month of the first WIC clinic opening* − *conception year-month*" < 0, there is WIC clinic exposure in the entire pregnancy. For last closings, if "*conception year-month* − *year-month of last WIC clinic closing*" > 0, there is no WIC clinic exposure, if "*conception year-month* − *year-month of last WIC clinic closing*" is between -9 and 0, there is partial WIC clinic exposure, and if "*conception year-month* − *year-month of last WIC clinic closing*" < -9, there is WIC clinic exposure in the entire pregnancy. To align the closing timing to the opening timing, I add 9 to the x-axis variable for closings.

[32]Additionally, Figure 3.4 may suggest an overall slight downward slope, although the estimates are relatively noisy. This slope could arise because knowledge about proximity to a WIC clinic takes time to spread

holds true in Figure 3.5, which limits the sample to sibling births, the main sample of my analysis. These figures suggest that there may be a relationship between geographical access to WIC and WIC food benefit take-up, which I explore more rigorously using regression methods next.

Table 3.3 presents the regression coefficients from estimating equations (3.1), (3.2), and (3.3) with an indicator for prenatal WIC food receipt as the outcome of interest. Appendix Table C.2 shows the first stage and reduced-form results corresponding to the IV-FE estimates. The first stage estimates are very strong (the partial F-statistics are equal to 1934.99, 2712.84, and 394.53 for the whole analysis sample, mothers with a high school degree or less at the time of the first birth, and mothers with a college degree or more at the time of the first birth, respectively), while the reduced-form estimates are appropriately scaled down from the IV-FE coefficients. The first stage results suggest that the presence of a WIC clinic in the mother's first ZIP code of residence during the 39 weeks post-conception is associated with a 76.2 percentage point increase in the likelihood of there being a WIC clinic in her current ZIP code of residence and during her actual pregnancy duration.

The first two columns of Table 3.3 use the universe of all singleton births in Texas, while the remaining columns use only the sibling sample. Further, the eighth and ninth columns split the sample by the mother's education level at the time of the first birth — high school degree or less versus college degree or more. Comparing results across these two education groups serves as a useful check of the identification strategy, as we expect access to WIC services to have a greater effect on the outcomes of low-educated mothers than the outcomes of high-educated mothers (as evidenced by the higher rate of WIC food benefit receipt among mothers with a high school degree or less relative to mothers with a college degree or more: 0.74 versus 0.07).[33]

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among eligible mothers.

[33]Earlier versions of this paper also presented results using a subsample of mothers whose first births were

The results suggest that having an operating WIC clinic in the mother's ZIP code of residence during any point of her pregnancy increases her likelihood of WIC food benefit receipt. The key coefficient of interest is positive and statistically significant across all specifications — those with ZIP code fixed effects, county-specific linear time trends, and mother fixed effects.[34] Importantly, the coefficient in the mother FE specification (column 5) is considerably smaller than the coefficients in the IV-FE specifications (column 6 and 7), suggesting that endogenous mobility and measurement error create substantial downward bias on the estimated effects. Additionally, the key coefficient of interest in the IV-FE model does not seem to be sensitive to the inclusion of controls for maternal and child demographics (column 6 versus column 7). According to the IV-FE estimate for the whole sibling sample, the magnitude of the effect is about 3 percentage points, corresponding to a 6 percent increase in WIC food benefit take-up at the sample mean of 0.56. As expected, the estimated coefficient is larger for mothers with a high school education or less. In contrast, the estimated coefficient for mothers with a college degree or more is insignificant and close

paid by Medicaid. Since the income threshold for Medicaid is the same as for WIC, one might suppose that Medicaid coverage is a good proxy for WIC eligibility. However, over the time period of the analysis, Texas experienced changes to the Medicaid system as many counties switched from fee-for-service to managed care plans. I find that mothers in counties with managed care are less likely to report being on Medicaid, perhaps because they now carry insurance cards from private HMOs. Consequently, I instead focus on results that split the sample by the mother's education level at the time of the first birth as education is a more consistent proxy for WIC eligiblity. Additionally, in supplementary analyses, I have estimated regressions for mothers residing in ZIP codes with average adjusted gross incomes for 2004 above and below the sample median of $35,891. The results split by average gross income in ZIP code of residence are similar to those split by mother's education.

[34] Except for column 6, all regressions include time-varying controls for mother's age, mother's education, mother's marital status, child birth order, as well as birth year and birth month fixed effects. The regressions in the first four columns additionally include controls for maternal race and ZIP code fixed effects. The regressions in columns 2 and 4 also include county-specific linear time trends. The regressions in columns 5-9 include mother fixed effects. The IV-FE specification in column 6 omits the time-varying mother and child demographic variables to test the sensitivity of the preferred model to the inclusion of control variables. To account for serial correlation at the level of variation in the key explanatory variable, in columns 1-5, standard errors are clustered on the ZIP code level, while in all of the IV-FE specifications (columns 6-9), standard errors are clustered on the mother's first ZIP code of residence. To create consistent sample sizes across specifications within the sibling sample, for each outcome, births by mothers who have at most one child with non-missing data for that outcome are omitted.

to zero. These results imply that geographic access to WIC clinics does matter, and seems to matter more for less advantaged women.

Table 3.4 presents additional results in which the sample is split by the type of WIC clinic location — urban versus rural ZIP codes, and "health-center" versus "non-health-center" facilities.[35] The results show that geographic access to WIC clinics is more salient for mothers in urban areas than in rural areas. Table 3.4 also suggests that the effects of access to WIC are driven by clinics located in "non-health-center" facilities such as community centers, schools, churches, and mobile vehicles. These clinics are more likely to have visible advertisements for WIC than those located inside county health departments or large hospitals. My results therefore suggest that despite the relatively small savings in travel times that arise from ZIP-code-level WIC clinic access (urban access represents only a 5 mile decrease in roundtrip travel distance on average, in contrast with the 14-mile average roundtrip distance decrease in rural ZIP codes), proximity to clinics is still important.[36] Having a WIC clinic in close proximity may reduce hassle costs and increase physical visibility, as women might become more aware of the program and able to stop on their way to or from work (Bertrand *et al.*, 2006).

[35]Data on urban and rural classification of ZIP codes comes from the WWAMI Rural Health Research Center at the University of Washington. The data contain Rural-Urban Commuting Area (RUCA) codes that classify ZIP codes into urban and rural areas. I follow their guidelines to classify ZIP codes with the following codes as urban: 1.0, 1.1, 2.0, 2.1, 3.0, 4.1, 5.1, 7.1, 8.1, 10.1. All other ZIP codes are classified as rural. More information is available here: http://depts.washington.edu/uwruca/ruca-approx.php. Health center WIC clinics are classified as those that contain one of the following strings in their name: "hospital", "medical", "health center", "health ctr", "health", "clinic".

[36]Roundtrip travel distance estimates are calculated by computing the average of the distances between mothers' residence homes and the locations of the nearest WIC clinics. For WIC clinics with missing or incorrectly recorded street addresses, I use the ZIP code centroid instead.

3.5.3 Effects on Pregnancy Behaviors

While receiving food benefits is an important aspect of the WIC program, access to a WIC clinic may affect mothers in several other ways. Women who come to a WIC clinic receive a health exam, which may increase the likelihood that they are diagnosed with various medical conditions such as hypertension or diabetes. They also receive information and education about nutrition and healthy behaviors. These services may encourage women to change their diet or exercise habits, or to stop smoking or drinking alcohol. Moreover, WIC clinics can serve as gateways to a range of other social services. For example, WIC staff can refer women to agencies that can help them enroll in other programs like Medicaid, Food Stamps, TANF, and housing assistance. They can also refer them to other services like counseling for substance abuse and job banks.

I test the extent to which some of these other mechanisms might matter in Table 3.5. This table shows the regression coefficients from the preferred IV-FE models, with various pregnancy behaviors and conditions as dependent variables. The controls and fixed effects are the same as described above, with standard errors clustered on the mother's first ZIP code of residence. These results suggest that maternal weight gain is affected by WIC clinic access. In particular, women are 2 percentage points (12 percent at the sample mean) less likely to gain too little weight during pregnancy (defined as less than 16 lbs), and 3 percentage points (13 percent at the sample mean) more likely to gain too much weight during pregnancy (defined as more than 40 lbs).[37] The estimated coefficients are larger in magnitude and more statistically significant for mothers with a high school education or less. Appendix Table C.3 suggests that the estimated increase in the likelihood of gaining too much weight is driven

[37]Information on pregnancy weight gain guidelines comes from the Mayo Clinic. All women with a BMI of less than 30 are recommended to gain at least 16 lbs. The maximum recommended weight gain even for underweight women (BMI less than 18.5) is 40 lbs. See http://www.mayoclinic.com/health/pregnancy-weight-gain/PR00111 for more details.

by mothers whose pre-pregnancy BMI for the first birth at which I observe them was 25 or greater (i.e., they would have been classified as overweight or obese).[38] These results point to the presence of opposing forces in the effect of access to WIC on pregnancy weight gain. On the one hand, the food benefits (and/or the nutrition education) may prevent some disadvantaged women from having an underweight pregnancy and putting themselves and their children at risk of various complications. On the other hand, the food benefits may lead to too much weight gain, especially among women who are overweight or obese. These findings suggest that more emphasis on nutrition education within the WIC program may be necessary.

I also find positive effects on low-educated mothers' likelihoods of having diabetes and gestational hypertension. These effects may be directly driven by the increase in excessive pregnancy weight gain. These could also be diagnosis effects, as women who show up at WIC clinics may be more likely to have these conditions be identified.[39] Notably, as with the results on food benefit take-up, there are no statistically significant effects of WIC clinic access on college-educated mothers' pregnancy behaviors, who are less likely to be impacted by proximity to WIC services.

Unfortunately, my results cannot conclusively speak to the degree to which WIC clinic access affects the take-up of other programs and services. Although the coefficients for the likelihoods of receiving prenatal care from a public clinic and of the birth being covered by Medicaid are positive and large relative to the sample means (especially for the low-educated sample), the standard errors are too large to draw conclusive inference from these results.

[38]Pre-pregnancy BMI is calculated from information in the births data on the mother's height and pre-pregnancy weight.

[39]Conditions such as hypertension and diabetes are generally reported at the hospital by either a doctor or a nurse. Consequently, some women may have these conditions recorded for their first births and not recorded for subsequent births, even if they did not actually "forget" that they had them. As a result, even women who had access to WIC during their first pregnancies and not during subsequent pregnancies may experience positive diagnosis effects.

My data do not have information on participation in other programs for which many WIC-eligible mothers are likely also eligible, such as Food Stamps, TANF, housing assistance, or child support enforcement.

Finally, there are some pregnancy behaviors and conditions which do not seem to be impacted by WIC. I have estimated regressions for the number of prenatal visits, prenatal care adequacy, and smoking during pregnancy, and found no statistically significant (or economically meaningful) results. Smoking is arguably expected to be most affected by the educational component of WIC, and my results suggest that this aspect of WIC may not have substantial influence on women's behavior during pregnancy.

3.5.4 Effects on Birth Outcomes and Breastfeeding

Having shown that WIC clinic access impacts pregnant women's food benefit take-up, weight gain, and diagnoses of some high-risk pregnancy conditions such as hypertension and diabetes, I next to turn to the analysis of the effects on birth outcomes and breastfeeding.

Table 3.6 presents results from the IV-FE specification for six different outcomes: birth weight in grams, an indicator for low birth weight (<2,500 grams), gestation in weeks, an indicator for a premature birth (<37 weeks gestation), an indicator for the child being breastfed at the time of discharge from the hospital, and an indicator for child death.[40] The results demonstrate that there is a positive effect of WIC clinic access on birth weight. According to the IV-FE estimate, birth weight is increased by about 27 grams, a 0.8 percent increase at the sample mean of 3,274 grams. The lack of statistically significant effects on

[40]In the Texas 2005-2009 birth certificate data, child death is recorded for children who could be matched to a death certificate by 2011. I have also estimated effects on child gender at birth to assess the relationship between WIC access and the likelihood of fetal death, since male fetuses are more susceptible to fetal death (Almond and Edlund, 2007). However, I find no statistically significant effects of WIC clinic access on the likelihood that a child is male. This may be due to the fact that the highest fetal death rates occur during the early part of the pregnancy, by which time many women may not have had time to visit a WIC clinic. Unfortunately, my data have no information on when the WIC benefits were received during pregnancy, so I cannot study this issue directly.

gestation and prematurity is consistent with studies that argue that any relationship between WIC and gestation is spurious because of a lack of medical evidence supporting a protective effect of WIC on prematurity (Joyce *et al.*, 2005, 2008).

Figure 3.6 investigates the effects of WIC clinic access throughout the birth weight distribution for the whole analysis sample, while Figure 3.7 limits the sample to mothers with a high school degree or less at the time of the first birth. The figures plot the coefficients and 95% confidence intervals from the IV-FE models with indicators for being above each specified number of grams (ranging from 1,500 grams to 4,500 grams in 500 gram increments) as outcomes. These results suggest that the effects on birth weight are concentrated in the middle of the birth weight distribution — WIC access leads to more children being born who weigh above 3,000 and 3,500 grams. For low-educated mothers, there is some evidence of movement across the low-birth-weight cutoff of 2,500 grams, although the effect is only marginally significant at the 10% level.[41] Importantly, despite the adverse effects on excessive maternal pregnancy weight gain, there is no effect on the likelihood of a child being born above 4,500 grams (the cutoff that classifies births as high-birth-weight). Additionally, consistent with the birth weight effects being concentrated in the middle of the distribution, I find no statistically significant effect on child mortality (see the last column of Table 3.6).[42] These findings imply that while WIC access improves average health at birth, children born at the very low end of the weight distribution, who are most likely to die within the first

[41]The marginal decrease in low-birth-weight births is also evident in Table 3.6. The magnitude of the decline in low-birth-weight further suggests that the average effect on birth weight is not perfectly uniform across the birth weight distribution. In particular, applying the 32.5-gram increase in average birth weight for low-educated mothers implies that children born between 2467.5 and 2500 grams would be moved above the low-birth-weight cutoff (0.006 of births among low-educated mothers). In my analysis, I find that 0.01 births are moved across the low-birth-weight cutoff. This finding suggests that the effect on birth weight may be larger around the low-birth-weight threshold. However, given that this estimate is only marginally significant at the 10% level, these findings should be interpreted with some caution.

[42]However, it is important to highlight that result on child mortality is not very precise due to the relatively large standard errors. Further research on the relationship between WIC clinic access and mortality is necessary to provide conclusive evidence.

year of life, are unaffected.

Table 3.6 also shows a positive and statistically significant increase in breastfeeding among mothers with a high school education or less — the likelihood of the infant being breastfed at the time of discharge is increased by about 6 percent at the sub-sample mean of 0.682. This effect implies that WIC emphasis on breastfeeding is relatively successful. However, an important limitation is that I cannot observe the duration of breastfeeding in my data. Therefore, while it may be the case that WIC encourages women to initiate breastfeeding, the provision of free formula may disincentivize breastfeeding in the long-run, as some past studies have shown (Jacknowitz *et al.*, 2007; Chatterji *et al.*, 2002).

As with the results on WIC food benefit take-up and pregnancy conditions, the estimated coefficients for birth weight and breastfeeding are larger in magnitude for mothers with a high school degree or less relative to mothers with a college degree or more. Specifically, for birth weight, the estimated effect for less-educated mothers is a statistically significant 1 percent increase at the subsample mean of 3,236 grams while the estimated effect for college-educated mothers is a statistically insignificant 0.4 percent increase at the subsample mean of 3,381 grams. For breastfeeding, in contrast to the above-mentioned estimated effect for less-educated mothers of 6 percent, the estimated effect for college-educated mothers is a statistically insignificant 2 percent decline at the subsample mean of 0.91. These findings confirm the conjecture that less-advantaged women are more likely to be affected by WIC clinic access.

3.5.5 Additional Results and Robustness

The key identification assumption in the above analysis is that WIC clinic openings and closings in the mother's first ZIP code of residence are uncorrelated with other time-varying variables that may affect WIC food receipt, pregnancy behaviors, birth outcomes, and breastfeeding. As an indirect test of this assumption, I check whether WIC clinic access either before the pregnancy or after childbirth is correlated with these outcomes. Since women have to be pregnant or post-partum to be eligible for WIC services, access to a WIC clinic before the start of the pregnancy should have no effect on the woman's pregnancy behaviors or her child's birth outcomes. Similarly, while women are eligible for WIC after giving birth, access to a WIC clinic after childbirth should have no effect on their behaviors during pregnancy or their children's outcomes at birth. However, if there is a correlation between WIC clinic openings and closings and maternal (unobservable) time-varying characteristics that affect these behaviors and outcomes, then we may detect some spurious placebo effects.

Table 3.7 presents the results from this placebo test. Here, the key explanatory variables are indicators for a WIC clinic operating in the mother's ZIP code of residence either 3-6 or 6-9 months before the start of the pregnancy or after childbirth, but no open WIC clinic during the actual pregnancy. Across all specifications, for all three main outcomes of interest (WIC food receipt, birth weight, and breastfeeding), and for both the whole sibling sample and the subsample of mothers with a high school education or less, none of the 48 coefficients on these placebo variables is statistically significant at the 5 percent level. The few marginally significant negative coefficients for birth weight and breastfeeding are opposite-signed than the coefficients on the main effects in Table 3.6.[43] These findings are reassuring as they imply that trends in WIC clinic access are likely uncorrelated with other unobservable maternal time-varying characteristics, providing further support for the validity of the identification strategy used in this paper.

I next test whether my results are sensitive to the definition of WIC clinic access. In the main analysis, the key explanatory variable of interest is an indicator for a WIC clinic operating in the mother's ZIP code of residence at any time during her pregnancy. In

[43]The general lack of statistical significance in this table seems to be driven by the fact that the standard errors associated with the placebo coefficients tend to be larger than those associated with the main effects discussed above.

Appendix Table C.4, I estimate regressions using two alternative definitions: an indicator for a WIC clinic operating in the mother's ZIP code of residence for the entire duration of the pregnancy, and a continuous variable that ranges from 0 to 1 and denotes the fraction of pregnancy duration days that at least one WIC clinic was operating in the mother's ZIP code of residence. The results for WIC food benefit receipt using these alternative definitions are very similar to the main results presented in Table 3.2.[44] This is likely due to the fact that not many women experience a last WIC clinic closing or a first WIC clinic opening at some point during their pregnancies (rather than before or after), so these variables have equal values for most observations in the sample. It is nevertheless encouraging that the effects are consistent across several definitions of WIC clinic access.

I also explore whether there are differences in effects of WIC clinic access between clinic openings and closings. In Appendix Table C.5, I show results for WIC food receipt separately for mothers who ever have any WIC clinic in their ZIP code of residence (60 percent of the analysis sample), mothers who experience an increase in the number of WIC clinics between their first and last birth in the data (12 percent of the analysis sample), and mothers who experience a decrease in the number of WIC clinics between their first and last birth in the data (15 percent of the analysis sample). Since the identification of the effect of WIC clinic access in the IV-FE model comes from mothers who experience changes in the number of WIC clinics in their first ZIP codes, it is reassuring that the key coefficients are larger in magnitude for these mothers (although they are less significant because of reduced sample sizes). However, there are no meaningful or statistically significant differences in the coefficients between mothers who experience more openings and mothers who experience more closings, suggesting that combining the two sources of variation is a reasonable strategy.

Appendix Table C.5 additionally addresses the concern that mothers with longer spacing

[44]Results for other outcomes are also similar.

between births are more likely to experience a change in WIC access. The last two columns show that the magnitude of the key coefficient is slightly larger and more significant for mothers with a space of two or more years between the first and last birth relative to mothers with shorter spacing. However, these coefficients are not statistically different from one another.

Another potential concern in my analysis relates to mis-specification of binary treatment. In particular, when a variable treatment is incorrectly parametrized as binary, the resulting estimate of the effect may be too large relative to the average per-unit effect along the length of the response function (Angrist and Imbens, 1995). In this context, this may be a problem because I determine access to WIC with an indicator for clinic presence in the mother's ZIP code, whereas the underlying treatment may be a continuous measure of distance to the nearest WIC clinic. However, as noted above, calculating distances is problematic because of substantial measurement error in recorded WIC clinic street addresses in my data. Nevertheless, I have conducted some supplementary analyses using a continuous distance measure.[45] I have estimated regression models as before, except with the key explanatory variable being the distance from the mother's home to the nearest WIC clinic in miles, and the instrument referring to the distance from the mother's first residence address (and assuming 39 weeks gestation). The results from this exercise are qualitatively consistent with the ZIP-code-level results, although they are not as precise. In particular, scaling the IV-FE coefficients on the effect of each additional mile to the nearest WIC clinic by the average roundtrip travel distance savings that ZIP-code-level access to WIC represents yields estimates that are similar to (although generally somewhat smaller than) those presented in

[45]To construct this measure, for each birth record with a non-missing mother's full residence address, I calculated the distance between the mother's home and the location of the nearest WIC clinic that is open during her pregnancy. For WIC clinics with missing or incorrectly recorded street addresses, I used the ZIP code centroid instead.

the main tables.[46] These results suggest that while the issue of mis-specification of binary treatment is a potentially relevant concern, it does not lead to substantial bias in my main estimates.

The presence of measurement error among WIC clinic street addresses may present concerns regarding the overall quality of the administrative data. Although numeric data fields for ZIP codes are likely less prone to be entered inaccurately than street address text fields, it may be the case that WIC clinics with wrong street addresses have other incorrectly recorded information as well (such as opening and closing dates). To address this concern, I have re-estimated the IV-FE models for the key outcomes (WIC food benefit receipt, birth weight, and breastfeeding) on a subset of the data that drops mothers residing in the 96 ZIP codes with at least one WIC clinic which could not be matched to longitude and latitude coordinates based on the street address provided in the raw data. The results from this exercise are presented in Appendix Table C.6. Reassuringly, these results are quite similar to the main results described above, suggesting that poor data quality is not a significant shortcoming.

I have also estimated heterogeneous effects of WIC clinic access by maternal race. In results not shown, I find that Hispanic mothers experience the largest increases in WIC food benefit take-up relative to non-Hispanic white and black mothers. However, Hispanics have the smallest estimated effects on birth weight out of the three groups. This discrepancy may exist because Hispanic mothers, who are less likely to be U.S. citizens relative to non-Hispanic white and black mothers, are less able to take advantage of referrals from WIC

[46]For example, I find that each additional mile to the nearest WIC clinic reduces WIC food benefit take-up by 0.2-0.3 percentage points, and reduces birth weight by 4-5 grams. To put these magnitudes in context of the ZIP-code-level analysis, I note that the average distance to the nearest WIC clinic for mothers with a WIC clinic in their ZIP code is 1.7 miles, whereas the average distance to the nearest WIC clinic for mothers without a WIC clinic in their ZIP code is 4.3 miles. Consequently, ZIP-code-level access to WIC represents savings of an average of 5.2 miles in roundtrip travel distance. Multiplying these coefficients by 5.2 yields estimates that are similar to those presented in the main tables.

to other agencies that have restrictions for non-citizens.[47] However, sample size limitations prevent me from having the power to detect statistically significant differences across races, so these results are merely suggestive.

Another important issue to address is whether WIC clinic access has an effect on the total number of births. In particular, if WIC has an effect on fetal deaths, then there could be a selection effect on birth outcomes as more "marginal" babies survive. Further, it is possible that WIC may incentivize women to become pregnant in order to receive the benefits. As a result, WIC access may affect the composition of births, which could bias the estimates on birth outcomes. I investigate this possibility in Table 3.8. I collapse the data into ZIP-code/birth-year/birth-month cells, and estimate regressions with the number of births and log number of births as dependent variables. I consider all singleton births, as well as all sibling births that are part of my main sample of analysis. All regressions include birth year, birth month, and ZIP code fixed effects, with standard errors clustered on the ZIP code level.

Across all specifications in Table 3.8, the results suggest that WIC clinic access is not correlated with the total number of births. This may be because the effect of WIC on fetal deaths is likely very small, since the highest fetal mortality rates occur in the early stages of the pregnancy, before many women have a chance to visit a WIC clinic. Further, these results suggest that WIC benefits do not have large incentive effects on conception. These findings are reassuring because they suggest that my main results are not driven by changes in the composition of births.

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[47]Following the enactment of the Personal Responsibility and Work Opportunity Act (PRWORA) in 1996, documented adult immigrants are subject to a five-year waiting period until they are eligible for Food Stamps, TANF, and Medicaid.

3.5.6 Discussion

My results suggest that ZIP-code-level access to WIC services increases food benefit take-up by 2 to 5 percentage points, raises birth weight by 22-32 grams, and increases the likelihood of breastfeeding at the time of hospital discharge by 4 percentage points for mothers with a high school degree or less. How meaningful are these effects? It is useful to put these magnitudes in the context of the existing literature.

The evidence on the relationship between WIC participation and birth outcomes provides a range of estimates. Earlier studies summarized in Currie (2003) find that participation in WIC is associated with a 10-43% reduction in the probability of low-birth-weight. More recently, Bitler and Currie (2005) have found that WIC participation is associated with a 64-78 gram increase in average birth weight and a 30 percent decrease in the likelihood of a low-birth-weight birth. Joyce *et al.* (2008) find smaller effects once they control for gestation length — 7-40 gram increases in average birth weight depending on the subsample considered, and a 0.7 percentage point (9 percent at the sample mean) reduction in low-birth-weight births. Figlio *et al.* (2009) find no statistically significant effects of WIC participation on average birth weight, but do find a substantial 13 percentage point decline in low-birth-weight births (which translates to a 160% reduction at the sample mean).

In comparing my results to the existing literature on WIC, it is important to again highlight that I measure *access to* WIC, rather than WIC participation. Consequently, my results are most comparable to Hoynes *et al.* (2011), the only other study on the relationship between geographic access to WIC and birth outcomes. Hoynes *et al.* (2011) find that county-level access to WIC leads to a 7 gram increase in birth weight among low-education mothers. While my estimates of 22-32 gram increases are higher, the 95% confidence intervals of the coefficients do overlap. The larger estimate in my study may also be explained by the fact that the WIC program operates on a much greater scale than it did at the time of its inception in the 1970s (the time period analyzed by Hoynes *et al.* (2011)), and offers a wider range of services. Additionally, expansions in Medicaid and Food Stamps may have made referrals to these other programs more relevant for a larger proportion of WIC-eligible women.

Similar to Hoynes *et al.* (2011), this paper measures the intent-to-treat (ITT) estimate of the effect of WIC access on pregnancy and birth outcomes. Yet we may also be interesed in the treatment-on-the-treated (TOT) effect on the population actually affected by WIC services as a result of geographic access. Hoynes *et al.* (2011) scale their ITT estimates by several different measures of WIC participation from other data sources over 1980-1988 to attain TOT effects on birth weight ranging from 18 to 37 grams. However, it is not clear how to interpret these calculations as they do not measure the "first-stage" impact of county-level access to WIC on WIC participation. In particular, these calculations assume that the 1980-1988 participation rates are equal to the percentage point increases in WIC participation resulting from county-level access in the year of pregnancy in the 1970s. If the increases in participation induced by county-level WIC clinic access in the 1970s are smaller than the participation rates measured in the 1980s, then the scaled TOT estimates should necessarily be larger.

In my data, I observe the effects of geographic access to WIC on food benefit take-up. Although one might argue that food benefit take-up is a reasonable proxy for participation, there is reason to believe that access to WIC affects several outcomes other than food benefit receipt. For example, in Texas, where women must physically go to a WIC clinic to determine their eligility for benefits and to receive them, ineligible women may still benefit from WIC clinic access through the initial health screenings, educational services, and the referrals to other programs for which they may be eligible. Unfortunately, the Texas WIC program does not keep track of the number of applicants who are ineligible, but anecdotal evidence from state program specialists at the Texas DSHS suggests that this is a non-negligible population

(Larkin, 2012).[48]

Even if WIC clinic access only affected women who were eligible, scaling the estimates of effects on birth outcomes by the effects on benefit take-up would provide TOT estimates of the effects of participation in WIC *and* participation in any other programs to which clients may have been referred to. Growing evidence suggests that many programs aimed at the low-income population, such as Medicaid, Food Stamps, and the Earned Income Tax Credit (EITC), have impacts on birth outcomes (see, for example, Currie and Gruber, 1996, Almond *et al.*, 2011b, and Hoynes *et al.*, 2012, respectively). Consequently, the impacts of WIC clinic access on birth outcomes are plausibly larger than the effects of food benefit receipt alone.

Indeed, scaling the estimated 95% confidence interval of the effect of WIC clinic access on birth weight for the low-educated sample by 0.04 (the coefficient for food benefit take-up) gives large estimates ranging from 337 to 1131 grams. However, it is difficult to interpret these estimates since I do not observe the take-up of most other programs or the proportion of people who are affected by WIC clinic access but do not receive food benefits.

3.6 Conclusion

Increasing support for the notion that fetal and infant health are predictive of individuals' later-life outcomes highlights the value of programs and policies aimed at pregnant women

[48]In Texas, participation in the WIC program is actually measured through the voucher and EBT data. As a result, only individuals who have used their food benefits at a WIC grocery store are recorded as participants. However, data from the United Way Food Support Connections program, which provides eligibility screening and enrollment support for Food Stamps at local offices throughout New York City, provides a ballpark estimate of the fraction of individuals who show up at a benefits office but are determined to be ineligible — in 2009, approximately 16 percent of individuals who were screened for eligibility by the program were determined to be ineligible (see http://unitedwaynyc.org/pages/food-card-access-project for more information about the program). Clearly, this estimate may not be directly applicable to Texas's WIC program. However, it at least provides some suggestive evidence that the number of women who could benefit from access to WIC clinics without actually receiving WIC food benefits is not trivial.

and new mothers. Indeed, successful programs that improve the welfare of disadvantaged women during pregnancy and post-partum may play an important role in ameliorating inequalities at birth, and thereby potentially mitigating the intergenerational transmission of low socio-economic status. WIC is the major program in the United States whose goal is to enhance the health and nutrition of low-income pregnant and post-partum women, infants, and children under age 5. Consequently, rigorous evaluation of the program is necessary both for policy-making purposes and for providing new estimates of the determinants of fetal and infant health.

Although there are many studies that examine the relationship between WIC and birth outcomes, much less attention has been paid to the determinants of WIC food benefit take-up. Moreover, consensus on the effectiveness of WIC has not been reached. Some of the existing literature on WIC may be affected by omitted variables bias due to non-random selection into WIC participation. Other studies suffer from lack of data on important variables such as WIC food benefit take-up and breastfeeding. Additionally, the mechanical correlation between gestation and WIC participation is not always carefully addressed. Finally, thorough evaluation of WIC in the current policy context, with the emphasis on coordination of services and during the time of the Great Recession, has not been done.

This paper uses restricted data on the universe of sibling births in Texas over 2005-2009 together with administrative data on all WIC clinic openings and closings during this time period to analyze the relationship between WIC clinic access, food benefit take-up, pregnancy behaviors, birth outcomes, and breastfeeding. My identification strategy relies on within-ZIP-code variation in WIC clinic openings and closings, together with mother fixed effects. Additionally, I use an instrumental variables technique to account for endogenous mobility between pregnancies, measurement error in gestation, and the mechanical correlation between gestation and WIC clinic access.

My results suggest that geographic access to WIC is a determinant of WIC food benefit take-up. Specifically, the presence of a WIC clinic in the mother's ZIP code of residence during pregnancy increases the likelihood that she receives food benefits by about 6 percent. The effects are driven by mothers in urban ZIP codes where travel distance reductions from ZIP-code-level access are relatively low, and by access to WIC clinics located in non-health-department facilities that are more likely to have visible adverstisements for services, implying that contextual factors of proximity to clinics may be influential. Further, WIC clinic access has two effects on pregnancy weight gain: although the likelihood of gaining too little weight is reduced, the likelihood of gaining too much weight is actually increased. These findings point to one potentially unintended consequence of WIC of an increase in the probability of excessive weight gain. The estimated effects on food benefit receipt and weight gain are larger in magnitude for mothers who have a high school education or less at the time of their first birth.

I also provide some suggestive evidence on the importance of other aspects of the WIC program such as health screenings, education, and referrals to other social services and programs. I show that access to a WIC clinic increases the likelihood that a mother with a high school education or less is recorded as having hypertension or diabetes, which may be either due to the effect on excessive weight gain or a result of an increase in the likelihood of diagnosis of such conditions at a WIC clinic and through a referrals.

Finally, I find that for mothers with a high school education or less, WIC clinic access increases average birth weight, and raises the likelihood of breastfeeding at the time of hospital discharge. The results on birth weight are concentrated in the middle of the distribution — there are no increases in high-birth-weight births despite the effects on excessive pregnancy weight gain. My results suggest that WIC is successful at improving health at birth for children of disadvantaged mothers, and that the effect may operate through multiple channels including food benefit take-up, health exams at clinics, and referrals to other agencies.

My findings can help inform the cost-benefit analyses of programs and interventions aimed at the low-income population in the U.S. Existing work provides some estimates of the benefits of other programs in terms of birth weight: Currie and Gruber (1996) find that a 30 percent increase in Medicaid eligibility leads to an 8 percent reduction in low-birth-weight; Almond *et al.* (2011b) show that access to Food Stamps leads to 15-20 gram increases in birth weight for whites and 8-12 gram increases in birth weight for blacks; Hoynes *et al.* (2012) find that the 1993 EITC expansion led to a 10 gram increase in average birth weight, and that a $1000 increase in EITC income leads to a 7-11 percent reduction in low-birth-weight. Although these estimates are not directly comparable with one another, this paper suggests that WIC clinic access leads to at least as much of an increase in birth weight as some of these other programs. Given that the costs of the WIC program are much lower than the costs of Medicaid, Food Stamps, and EITC, my results suggest that WIC, a program targeted at pregnant women and young children specifically, presents a relatively cost-effective way of improving child health at birth.[49] However, this comparison is of course very limited, as it does not factor in the potential adverse effects of excessive pregnancy weight gain for women or the benefits of breastfeeding, and does not account for the benefits of the other programs for any individuals except pregnant women and infants.

It is also possible to provide a sense of the value of the increase in birth weight in terms of other outcomes. For example, Almond *et al.* (2005) show that one standard deviation increase in birth weight leads to a 0.08 standard deviation reduction in hospital costs for delivery and initial care. Assuming that their findings are applicable to my sample, and assuming a linear relationship between birth weight and hospital costs, my estimate of a 0.05 standard deviation increase in birth weight would imply a 0.004 standard deviation reduction in costs.[50] Using an estimate of the standard deviation of approximately $39,000

[49]Ben-Shalom *et al.* (2011) show that monthly expenditures per recipient in 2007 dollars are $482 for Medicaid, $96 for Food Stamps, $165 for EITC, and $54 for WIC.

[50]I attain the 0.05 standard deviation magnitude from dividing the coefficient for birth weight on the main

in 2000 dollars from Almond *et al.* (2005), my findings suggest a $156 reduction in average hospital costs. In terms of the long-run value, Black *et al.* (2007) find that a 10 percent increase in birth weight is associated with a 1 percent increase in annual full-time earnings. Applying their findings for cohorts born in 1967-1981 in Norway to my setting of Texas sibling births in 2005-2009 maps to a 0.08 percent increase in annual full-time earnings facilitated by WIC clinic access. Note that these value calculations likely represent lower bounds since they rely on the ITT (and not TOT) estimates of the effect of access to WIC.

An important limitation of this paper is that the findings presented may only be relevant for sibling births in Texas, and may not be generalizable to the rest of the U.S. population. Data from the 2009 March Current Population Survey (CPS) suggests that the WIC receipt rate is higher among among mothers with youngest children less than one year old in Texas than in the rest of the country: the national rate is 0.27, while the Texas rate is 0.36.[51] However, infant health in Texas is comparable to the rest of the country: in 2009, the national rate of low-birth-weight was 8.2 percent, while the Texas rate of low-birth-weight was 8.5 percent. Clearly, further research on the effects of WIC access on the national level in recent years is necessary to assess the generalizability of my findings.

While this paper shows robust evidence on the effects of WIC clinic access on food benefit take-up, pregnancy behaviors, birth weight, and breastfeeding, my data do not allow me to follow children as they grow older. Understanding the long-run effects of WIC on children's outcomes should be the focus of future research.

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analysis sample (27 grams) by the standard deviation in the sample of 517 grams.

[51]Note that the Texas take-up rate in the CPS is much lower than the food benefit receipt rate recorded in my data from Texas birth certificates. The substantial underreporting of WIC receipt in the CPS has been documented by other researchers like Bitler *et al.* (2003).

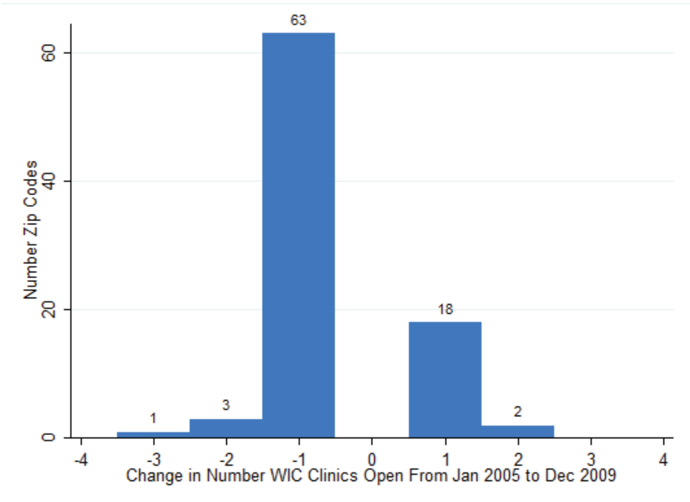
Figure 3.1: Mobile WIC Clinic in McAllen, Texas

Source: United States Department of Agriculture Blog. Accessed at:
http://blogs.usda.gov/2011/05/10/clinic-on-wheels-reaches-communities-once-isolated-from-wic-clinics/.

Figure 3.2: Number Operating WIC Clinics in Texas: 2005-2009

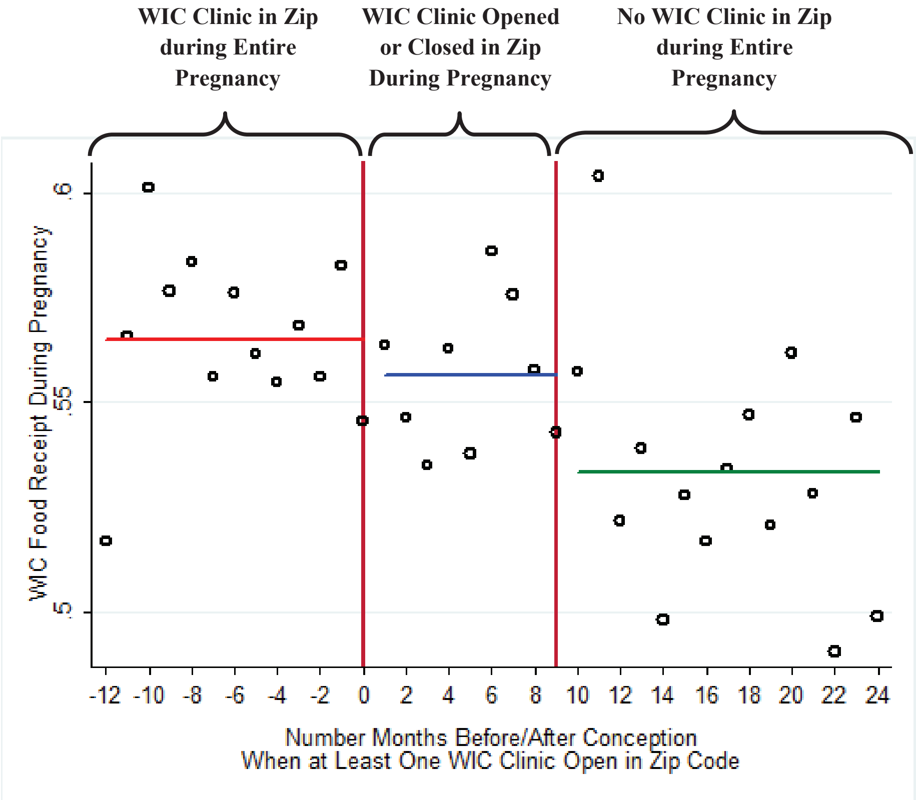
Notes: This figure plots the number of open WIC clinics in Texas by year-month from January 2005 to December 2009.

Figure 3.3: Variation in Within-ZIP Code Number of WIC Clinics Over 2005-2009



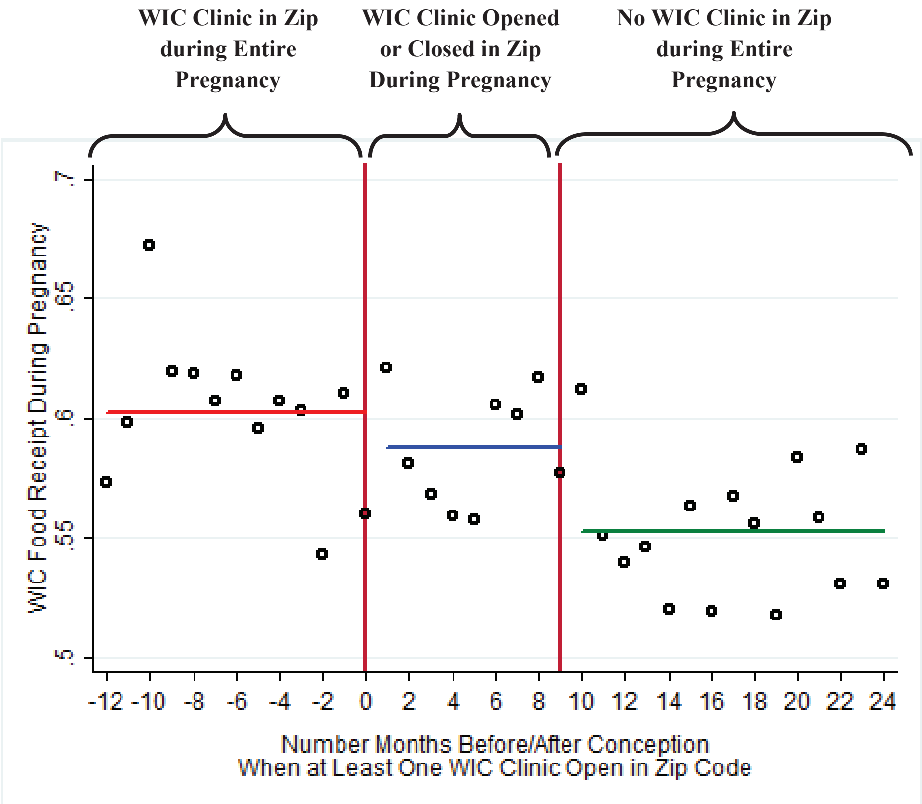
Notes: This figure is a histogram of ZIP codes that have had a non-zero change in the number of open WIC clinics between January 2005 and December 2009. There are 578 Texas ZIP codes that have ever had at least one WIC clinic operating between January 2005 and December 2009. Out of them, 114 ZIP codes had a non-zero change, while 464 ZIP codes experienced no change in the number of open WIC clinics over this time period. Note that 27 ZIP codes experienced a change in the number of open WIC clinics but had the same number of clinics in January 2005 and December 2009, and they are therefore excluded from the figure.

Figure 3.4: Prenatal WIC Food Receipt by Number Months Before/After Conception When At Least One WIC Clinic Was Operating in the Mother's ZIP Code of Residence: TX Births 2005-2009



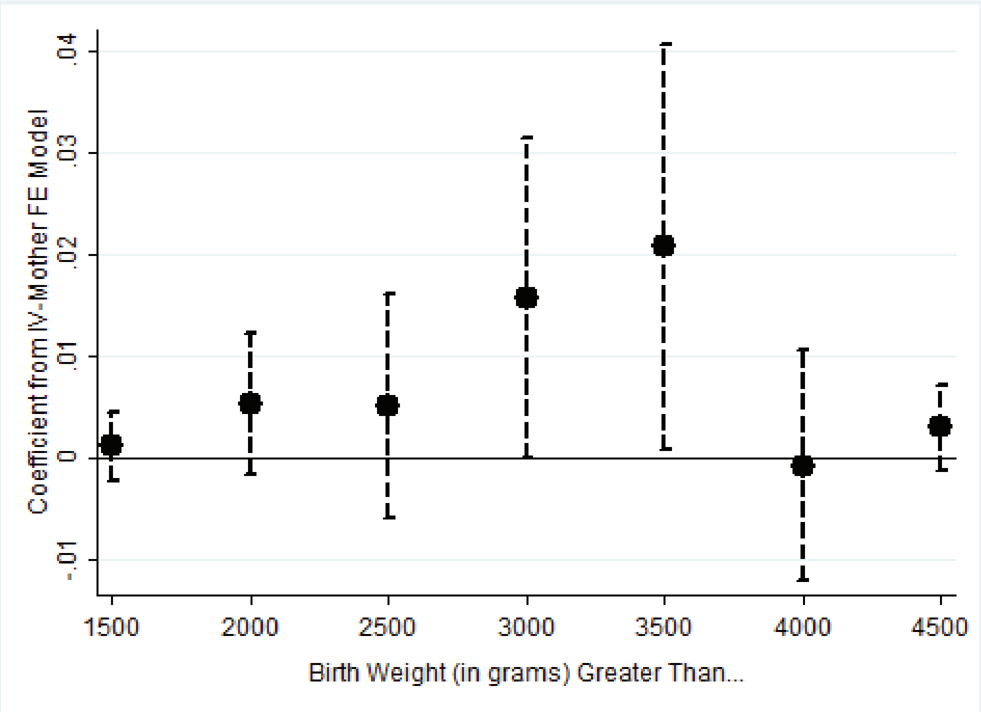
Notes: The sample of analysis consists of all singleton births in Texas over 2005-2009 with mothers who reside in Texas. The sample is limited to conceptions in a 36-month period surrounding the time that each ZIP code experienced a first WIC clinic opening or a last WIC clinic closing. Mothers residing in ZIP codes that have experienced a first WIC clinic opening and a last WIC clinic closing within the same 36-month period are dropped (5 ZIP codes). To combine these openings and closings on the same graph, for conceptions in ZIP codes that experience a first WIC clinic opening, the x-axis variable is equal to "year-month of the first WIC clinic opening − conception year-month", while for conceptions in ZIP codes that experience a last WIC clinic closing, the x-axis variable is equal to "conception year-month − year-month of last WIC clinic closing + 9". Consequently, all x-axis values below 0 correspond to conceptions in ZIP codes with at least one WIC clinic operating during the entire pregnancy; x-axis values between 0 and 9 correspond to conceptions in ZIP codes where a WIC clinic opened or closed during pregnancy; and x-axis values above 9 correspond to conceptions in ZIP codes with no WIC clinics operating during pregnancy. The horizontal lines depict the mean values of WIC food benefit receipt for each group of observations. Please refer to the text in Section 3.5.2 for more details.

Figure 3.5: Prenatal WIC Food Receipt by Number Months Before/After Conception When At Least One WIC Clinic Was Operating in the Mother's ZIP Code of Residence: TX Sibling Births 2005-2009



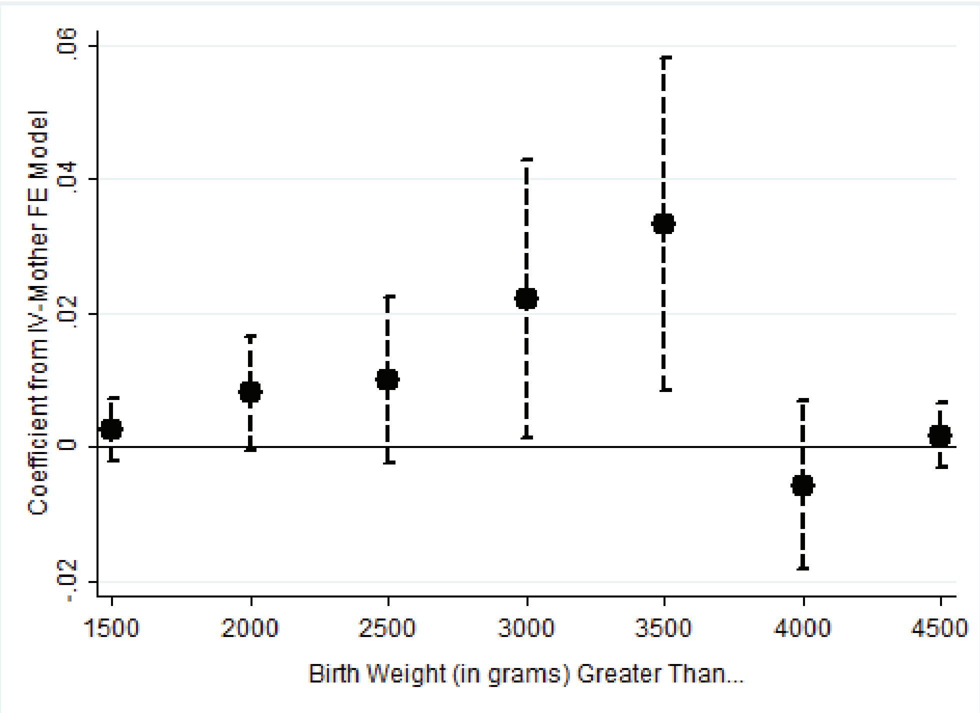
Notes: The sample of analysis consists of singleton sibling births in Texas over 2005-2009 with mothers who reside in Texas. See notes under Figure 3.4 for more information.

Figure 3.6: Effects of WIC Clinic Access in ZIP Code of Residence on Birth Weight Throughout the Distribution



Notes: This figure plots the key coefficients and 95% confidence intervals from estimating equation (3.3) for indicators for being above each specified number of grams as outcomes.

Figure 3.7: Effects of WIC Clinic Access in ZIP Code of Residence on Birth Weight Throughout the Distribution: Mothers with a High School Degree or Less at Time of First Birth



Notes: This figure plots the key coefficients and 95% confidence intervals from estimating equation (3.3) for indicators for being above each specified number of grams as outcomes. The sample is limited to births by mothers with a high school degree or less at the time of the first birth.

Table 3.1: Summary Statistics: Texas Sibling Births 2005-2009

	WHOLE SAMPLE (N=612,694)		BIRTHS BY MOTHERS WITH WIC CLINIC IN ZIP CODE DURING ANY PREGNANCY (N=360,799)		BIRTHS WITH WIC CLINIC IN ZIP CODE DURING PREGNANCY (N=297,552)	
	Mean	SD	Mean	SD	Mean	SD
Mother Received WIC Food During Pregnancy	**0.556**	0.497	**0.647**	0.478	**0.656**	0.475
Mother's Age <20	**0.150**	0.357	**0.178**	0.383	**0.179**	0.384
Mother's Age 20-24	**0.330**	0.470	**0.370**	0.483	**0.365**	0.481
Mother's Age 25-34	**0.447**	0.497	**0.399**	0.490	**0.401**	0.490
Mother's Age 35-44	**0.073**	0.260	**0.053**	0.224	**0.055**	0.227
Mother's Ed: <HS	**0.310**	0.462	**0.371**	0.483	**0.378**	0.485
Mother's Ed: HS degree	**0.281**	0.449	**0.305**	0.460	**0.303**	0.460
Mother's Ed: Some College	**0.220**	0.415	**0.207**	0.405	**0.203**	0.402
Mother's Ed: College+	**0.188**	0.391	**0.117**	0.321	**0.116**	0.320
Mother is Married	**0.587**	0.492	**0.525**	0.499	**0.529**	0.499
Mother is Non-Hispanic White	**0.353**	0.478	**0.276**	0.447	**0.263**	0.440
Mother is Black	**0.110**	0.312	**0.107**	0.310	**0.098**	0.297
Mother is Hispanic	**0.511**	0.500	**0.601**	0.490	**0.624**	0.484
Child is Male	**0.511**	0.500	**0.510**	0.500	**0.510**	0.500
Mother's Pre-Pregnancy BMI	**25.819**	6.079	**26.079**	6.202	**26.134**	6.215
Pregnancy Weight Gain <16 lbs	**0.146**	0.354	**0.159**	0.366	**0.161**	0.368
Pregnancy Weight Gain >40 Lbs	**0.191**	0.393	**0.186**	0.389	**0.183**	0.387
Number Prenatal Visits	**9.695**	4.039	**9.384**	4.148	**9.395**	4.152
Prenatal Care Received from Public Clinic	**0.092**	0.289	**0.106**	0.308	**0.109**	0.311
Diabetes	**0.035**	0.183	**0.034**	0.180	**0.034**	0.181
Gestational Hypertension	**0.043**	0.202	**0.041**	0.199	**0.041**	0.199
Eclampsia	**0.001**	0.031	**0.001**	0.032	**0.001**	0.032
Birth Paid by Medicaid	**0.480**	0.500	**0.554**	0.497	**0.553**	0.497
Birth Weight (g)	**3275.410**	517.173	**3254.084**	517.108	**3254.559**	516.061
Low Birth Weight (<2500g)	**0.060**	0.238	**0.064**	0.245	**0.063**	0.244
Very Low Birth Weight (<1500g)	**0.006**	0.080	**0.007**	0.082	**0.007**	0.081
High Birth Weight (>4500g)	**0.008**	0.089	**0.007**	0.086	**0.008**	0.087
Gestation (weeks)	**38.431**	1.748	**38.404**	1.789	**38.406**	1.786
Premature (<37 weeks)	**0.091**	0.288	**0.096**	0.294	**0.095**	0.294
Child is Breastfed at Time of Discharge	**0.745**	0.436	**0.710**	0.454	**0.709**	0.454
Child Matched to Death Record	**0.004**	0.066	**0.004**	0.066	**0.004**	0.067

Notes: The sample is limited to singleton sibling births with mothers that reside in Texas over 2005-2009. Births with missing gestation length or gestation less than 26 weeks are omitted. Exposure to a WIC clinic is calculated by considering length of pregnancy from the time of conception (estimated using the child's birth date and gestation length).

Table 3.2: Maternal Characteristics and WIC Clinic Locations in Texas

	Mother's Age <20	Mother's Age 35-44	Mother's Ed: <HS	Mother's Ed: HS degree	Mother's Ed: Some College	Mother's Ed: College+	Mother is Married	Mother is Non-Hispanic White	Mother is Black	Mother is Hispanic
A. No Zip Code Fixed Effects										
Any WIC Clinic in Zip Code of	0.0553***	-0.0345***	0.1310***	0.0436***	-0.0334***	-0.1413***	-0.1128***	-0.1763***	-0.0228**	0.2202***
Residence During Pregnancy	(0.0045)	(0.0032)	(0.0121)	(0.0073)	(0.0057)	(0.0129)	(0.0118)	(0.0192)	(0.0099)	(0.0220)
B. With Zip Code Fixed Effects										
Any WIC Clinic in Zip Code of	0.0038	0.0050	-0.0080	0.0155	0.0057	-0.0134**	-0.0106	-0.0073	0.0095**	-0.0020
Residence During Pregnancy	(0.0066)	(0.0048)	(0.0112)	(0.0100)	(0.0077)	(0.0057)	(0.0084)	(0.0068)	(0.0036)	(0.0072)
N	612,694	612,694	612,694	612,694	612,694	612,694	612,690	612,694	612,694	612,694

Notes: Each coefficient in each panel is from a separate regression. The sample is limited to singleton sibling births with mothers that reside in Texas over 2005-2009. Births with missing gestation length or gestation less than 26 weeks are omitted. All regressions include birth year and birth month fixed effects. The regressions in Panel B also include ZIP code fixed effects. Robust standard errors are clustered on the ZIP code level.
Significance levels: $+p<0.10$ $**p<0.05$ $***p<0.001$

Table 3.3: Effects of WIC Clinic Access in ZIP Code of Residence on WIC Food Receipt

	Dependent Variable: Mother Received WIC Food During Pregnancy								
	All Texas Singleton Births		Texas Sibling Singleton Births						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Zip FE	Zip FE and County Time Trends	Zip FE	Zip FE and County Time Trends	Mother FE	IV-Mother FE, No Controls	IV-Mother FE	IV-Mother FE; HS Education or Less at Time of First Birth	IV-Mother FE; College-Educated at Time of First Birth
Mean of Dependent Variable	0.537	0.537	0.556	0.556	0.556	0.556	0.556	0.740	0.072
Any WIC Clinic in Zip Code of Residence During Pregnancy	0.0147**	0.0148**	0.0221**	0.0220**	0.0075**	0.0317**	0.0308**	0.0443**	-0.0053
	(0.0075)	(0.0074)	(0.0084)	(0.0083)	(0.0023)	(0.0140)	(0.0138)	(0.0185)	(0.0137)
Mother's Age <20	0.1691***	0.1692***	0.1788***	0.1776***	0.0185		0.0181	-0.0569	
	(0.0100)	(0.0100)	(0.0287)	(0.0287)	(0.0468)		(0.0359)	(0.0924)	
Mother's Age 20-24	0.1499***	0.1499***	0.1631***	0.1619***	-0.0121		-0.0125	-0.0890	0.0337
	(0.0098)	(0.0098)	(0.0286)	(0.0286)	(0.0467)		(0.0357)	(0.0922)	(0.0254)
Mother's Age 25-34	0.0676***	0.0677***	0.0940***	0.0929**	-0.0130		-0.0133	-0.0793	0.0141
	(0.0096)	(0.0096)	(0.0284)	(0.0285)	(0.0466)		(0.0357)	(0.0922)	(0.0240)
Mother's Age 35-44	0.0285**	0.0286**	0.0672**	0.0661**	0.0125		0.0121	-0.0291	0.0165
	(0.0096)	(0.0096)	(0.0284)	(0.0284)	(0.0463)		(0.0354)	(0.0918)	(0.0237)
Mother's Ed: <HS	0.3475***	0.3469***	0.3784***	0.3776***	0.0678***		0.0678***	0.0561***	
	(0.0035)	(0.0035)	(0.0043)	(0.0043)	(0.0077)		(0.0063)	(0.0159)	
Mother's Ed: HS degree	0.3211***	0.3203***	0.3540***	0.3532***	0.0801***		0.0802***	0.0670***	
	(0.0043)	(0.0043)	(0.0049)	(0.0049)	(0.0073)		(0.0060)	(0.0161)	
Mother's Ed: Some College	0.1871***	0.1868***	0.2152***	0.2148***	0.0517***		0.0517***	0.0274+	
	(0.0045)	(0.0045)	(0.0054)	(0.0054)	(0.0068)		(0.0056)	(0.0164)	
Mother is Married	-0.1185***	-0.1183***	-0.1007***	-0.1006***	-0.0431***		-0.0430***	-0.0288***	-0.1154***
	(0.0030)	(0.0030)	(0.0032)	(0.0032)	(0.0034)		(0.0028)	(0.0031)	(0.0147)
Mother is Non-Hispanic White	-0.0499***	-0.0499***	-0.0484***	-0.0483***					
	(0.0027)	(0.0027)	(0.0038)	(0.0038)					
Mother is Black	0.0924***	0.0921***	0.0799***	0.0796***					
	(0.0034)	(0.0034)	(0.0045)	(0.0045)					
Mother is Hispanic	0.1339***	0.1333***	0.1290***	0.1285***					
	(0.0034)	(0.0034)	(0.0040)	(0.0040)					
N	1,918,123	1,918,123	607,002	607,002	607,002	607,002	607,002	366,865	107,175

Notes: Each column is a separate regression. In columns 3-9, for each outcome, births by mothers who have at most one child with non-missing data for that outcome are omitted. Unless otherwise noted, all regressions include fixed effects for birth order, birth year, and birth month. The first 4 columns include ZIP code fixed effects. Columns 2 and 4 add county-specific linear time trends. Columns 5-9 include mother fixed effects. In columns 6-9 the key explanatory variable is instrumented by an indicator for any WIC clinic in 39 weeks post-conception in the mother's first pregnancy ZIP code. Column 6 omits controls for maternal and child demographics and birth order, while column 7 includes all of the controls. In columns 1-5, standard errors are clustered on the mother's current ZIP code of residence, while in columns 6-9, they are clustered on the mother's first pregnancy ZIP code. Significance levels: +p<0.10 **p<0.05 ***p<0.001

Table 3.4: Geographic Access to WIC: By Type of Location and Clinic, IV-Mother FE Method

	Dependent Variable: Mother Received WIC Food During Pregnancy			
	Urban Zip Codes	**Rural Zip Codes**	**WIC Clinic in Health Center**	**WIC Clinic in Non-Health Center**
Any WIC Clinic in Zip Code of Residence During Pregnancy	0.0354**	0.0019		
	(0.0148)	(0.0257)		
Any Health Center WIC Clinic in Zip Code of Residence During Pregnancy			0.0047	
			(0.0150)	
Any Non-Health Center WIC Clinic in Zip Code of Residence During Pregnancy				0.0276**
				(0.0095)
N	507,851	76,449	607,002	607,002

Notes: Each coefficient is from a separate regression. See Table 3.3 for more information about the sample, controls, and the IV-Mother FE estimation method. Health center WIC clinics are those that contain one of the following strings in their name: “hospital”, “medical”, “health center”, “health ctr”, “health”, “clinic”. Significance levels: +p<0.10 **p<0.05 ***p<0.001

Table 3.5: Effects of WIC Clinic Access in ZIP Code of Residence on Pregnancy Behaviors and Conditions: IV-Mother FE Method

	Weight Gain <16 Lbs	Weight Gain >40 Lbs	Prenatal Care Received from a Public Clinic	Diabetes	Gestational Hypertension	Eclampsia	Birth Paid by Medicaid
A. All Mothers: IV-Mother FE							
Mean of Dependent Variable	0.146	0.191	0.092	0.030	0.036	0.001	0.480
Any WIC Clinic in Zip Code of Residence During Pregnancy	-0.0172+	0.0267+	0.0082	0.0070	0.0129**	0.0001	0.0499
	(0.0091)	(0.0137)	(0.0242)	(0.0049)	(0.0049)	(0.0008)	(0.0512)
N	589,574	589,574	612,686	612,686	612,686	612,686	604,661
B. Mothers with High School Degree or Less at Time of First Birth: IV-Mother FE							
Mean of Dependent Variable	0.167	0.182	0.132	0.028	0.034	0.001	0.625
Any WIC Clinic in Zip Code of Residence During Pregnancy	-0.0317**	0.0389**	0.0147	0.0127**	0.0130**	0.0005	0.0605
	(0.0127)	(0.0183)	(0.0352)	(0.0047)	(0.0056)	(0.0011)	(0.0627)
N	355,376	355,376	371,533	371,533	371,533	371,533	364,953
C. Mothers with College Degree at Time of First Birth: IV-Mother FE							
Mean of Dependent Variable	0.083	0.194	0.011	0.032	0.038	0.001	0.049
Any WIC Clinic in Zip Code of Residence During Pregnancy	-0.0063	-0.0052	0.0047	-0.0145	0.0033	0.0010	-0.0081
	(0.0111)	(0.0125)	(0.0076)	(0.0092)	(0.0092)	(0.0015)	(0.0092)
N	105,651	105,651	107,483	107,483	107,483	107,483	107,004

Notes: Each coefficient in each panel is from a separate regression. See Table 3.3 for more information about the sample, controls, and the IV-Mother FE estimation method.
Significance levels: +p<0.10 **p<0.05 ***p<0.001

Table 3.6: Effects of WIC Clinic Access in ZIP Code of Residence on Birth Outcomes: IV-Mother FE Method

	Birth Weight (g)	Low Birth Weight (<2500g)	Gestation (weeks)	Premature (<37 weeks)	Child Breastfed	Child Matched to Death Record
A. All Mothers: IV-Mother FE						
Mean of Dependent Variable	3274.410	0.060	38.431	0.091	0.745	0.004
Any WIC Clinic in Zip Code of	27.3023**	-0.0053	0.0582	-0.0037	0.0201	-0.0011
Residence During Pregnancy	(7.9839)	(0.0056)	(0.0417)	(0.0109)	(0.0168)	(0.0019)
N	612,640	612,640	612,686	612,686	608,982	612,551
B. Mothers with High School Degree or Less at Time of First Birth: IV-Mother FE						
Mean of Dependent Variable	3236.176	0.069	38.423	0.098	0.682	0.004
Any WIC Clinic in Zip Code of	32.5030***	-0.0100+	0.0711	-0.0054	0.0405**	0.0003
Residence During Pregnancy	(8.9690)	(0.0053)	(0.0569)	(0.0126)	(0.0185)	(0.0024)
N	371,504	371,504	371,533	371,533	369,000	371,424
C. Mothers with College Degree at Time of First Birth: IV-Mother FE						
Mean of Dependent Variable	3381.123	0.038	38.535	0.067	0.913	0.004
Any WIC Clinic in Zip Code of	16.3060	-0.0112	0.0929	0.0014	-0.0217	-0.0006
Residence During Pregnancy	(14.9501)	(0.0116)	(0.0753)	(0.0088)	(0.0168)	(0.0030)
N	107,477	107,477	107,483	107,483	107,076	107,477

Notes: Each coefficient in each panel is from a separate regression. See Table 3.3 for more information about the sample, controls, and the IV-Mother FE estimation method.
Significance levels: +p<0.10 **p<0.05 ***p<0.001

Table 3.7: Placebo Effects of WIC Clinics on Prenatal WIC Food Receipt, Birth Weight, and Breastfeeding

	Dependent Variable: Mother Received WIC Food During Pregnancy				Dependent Variable: Birth Weight (g)				Dependent Variable: Child is Breastfed at Time of Discharge			
	Zip FE	Zip FE & County Time Trends	Mother FE	IV-Mother FE	Zip FE	Zip FE & County Time Trends	Mother FE	IV-Mother FE	Zip FE	Zip FE & County Time Trends	Mother FE	IV-Mother FE
A. All Mothers												
At Least One WIC Clinic 3-6 Months Before or After Pregnancy & No WIC Clinics During Pregnancy	-0.0012	-0.0013	-0.0034	-0.0031	-8.8582	-8.8695	-19.9775	-35.4600	0.0047	0.0045	-0.0062	-0.0107
	(0.0154)	(0.0154)	(0.0176)	(0.0179)	(17.5737)	(17.5241)	(20.7472)	(23.5515)	(0.0108)	(0.0108)	(0.0147)	(0.0185)
At Least One WIC Clinic 6-9 Months Before or After Pregnancy & No WIC Clinics During Pregnancy	-0.0047	-0.0047	-0.0201	-0.0051	1.5355	0.7957	16.0030	29.3213	0.0041	0.0030	0.0122	0.0237
	(0.0118)	(0.0118)	(0.0161)	(0.0183)	(18.7600)	(18.7264)	(23.3029)	(21.9082)	(0.0142)	(0.0142)	(0.0150)	(0.0189)
N	607,002	607,002	607,002	607,002	612,640	612,640	612,640	612,640	608,982	608,982	608,982	608,982
B. Mothers with High School Degree or Less at Time of First Birth												
At Least One WIC Clinic 3-6 Months Before or After Pregnancy & No WIC Clinics During Pregnancy	0.0035	0.0036	-0.0051	-0.0058	-26.0558	-26.1545	-24.9839	-44.4283+	-0.0223	-0.0226	-0.0365+	-0.0517+
	(0.0229)	(0.0229)	(0.0246)	(0.0271)	(23.2901)	(23.3007)	(22.4614)	(23.6391)	(0.0144)	(0.0144)	(0.0189)	(0.0305)
At Least One WIC Clinic 6-9 Months Before or After Pregnancy & No WIC Clinics During Pregnancy	-0.0204	-0.0209	-0.0338	-0.0197	7.2430	6.0043	32.4890	43.7363	0.0126	0.0112	0.0269	0.0237
	(0.0191)	(0.0191)	(0.0244)	(0.0301)	(20.1771)	(20.1609)	(27.2863)	(28.0755)	(0.0201)	(0.0203)	(0.0192)	(0.0227)
N	366,865	366,865	366,865	366,865	371,504	371,504	371,504	371,504	369,000	369,000	369,000	369,000

Notes: Each column in each panel is from a separate regression. See Table 3.3 for more information about the sample, controls, and estimation methods. The key explanatory variables of interest are indicators that are equal to 1 if a first WIC clinic opens in the mother's ZIP code of residence 3-6 and 6-9 months after childbirth or if a last WIC clinic closes in the 3-6 and 6-9 months before conception, and zero otherwise. Significance levels: +p<0.10 **p<0.05 ***p<0.001

Table 3.8: Effects of WIC Clinic Access on Births in Texas

	Total Number Births	Log Total Births	Total Number Sibling Births	Log Sibling Births
Any WIC Clinic in Zip Code of Residence During Pregnancy	-0.5100	0.0076	-0.3808	0.0059
	(0.4162)	(0.0201)	(0.4843)	(0.0260)
N	94,796	94,796	76,945	76,945

Notes: Each coefficient is from a separate regression. Units of analysis are residence ZIP code - birth year - birth month cells. In the first two columns, the sample includes the universe of Texas singleton births over 2005-2009. The last two columns use the siblings sample. All regressions include birth year, birth month, and ZIP code fixed effects. Robust standard errors are clustered on the ZIP code level.
Significance levels: +p<0.10 **p<0.05 ***p<0.001

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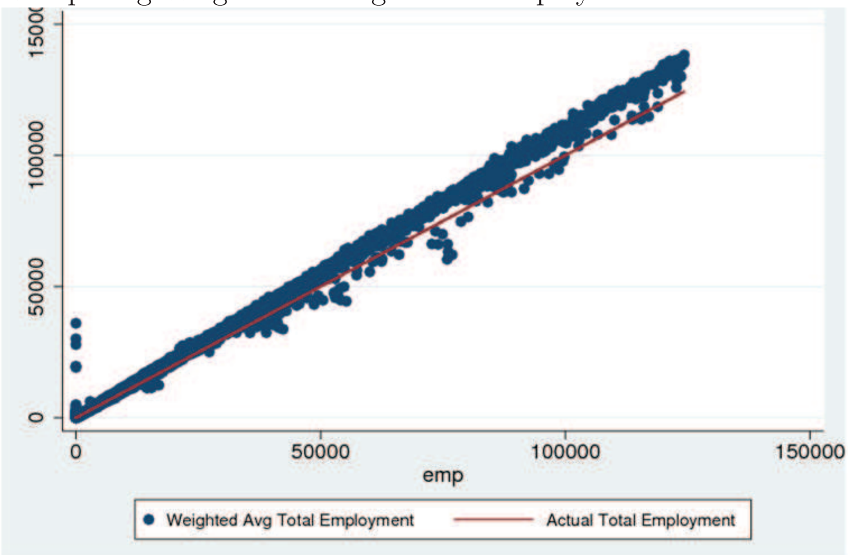
Appendix A

The Effects of Maternity Leave on Children's Birth and Infant Health Outcomes in the United States

Figure A.1: Comparing Weighted Average Total Employment with Total Employment



Notes: This figure plots the total employment in each county and year calculated using the formula in the text in Section 1.4 versus the actual total employment in the data.

Table A.1: Summary Statistics by County-Year Eligibility Status

County Characteristics:	CONTROL STATE AND LIKELY INELIGIBLE	CONTROL STATE AND LIKELY ELIGIBLE	TREATMENT STATE AND LIKELY INELIGIBLE	TREATMENT STATE AND LIKELY ELIGIBLE
Percent White	88.73%	82.85%	85.01%	80.72%
Percent Black	2.69%	9.18%	9.80%	15.87%
Percent Female Aged 18-44	20.02%	21.82%	19.63%	21.22%
Percent Females Aged 18-44 Employed	50.79%	54.96%	47.82%	52.29%
Percent Females Married	57.30%	51.68%	57.71%	53.65%
Percent Urban	30.59%	63.51%	16.35%	43.39%
Percent Females Aged 25+ College-Educated	15.26%	18.63%	12.69%	14.69%
Percent Below Poverty Line	12.75%	11.76%	17.96%	15.13%
Unemployment Rate	6.25%	5.95%	5.76%	5.63%

Notes: Data on county characteristics is from the 1990 US Census, while data on the unemployment rate is from the BLS. Treatment states that had no maternity leave laws prior to FMLA. Control states had some kind of maternity leave law prior to FMLA. In every year, likely ineligible counties have the likelihood of employment in a firm with 50 or more employees at or below the median, while likely eligible counties have the likelihood of employment in a firm with 50 or more employees above the median.

Table A.2: Effects of FMLA on Birth Outcomes and Infant Mortality Among College-Educated and Married Mothers: Different Specifications

	Birth Weight (g)					Premature Birth					Infant Mortality Rate				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Post	-12.3795+	-11.9781+	-13.4920**	2.0537	4.6993	0.0081***	0.0077***	0.0075***	0.0010	-0.0000	-0.0009***	-0.0006***	-0.0006***	-0.0003**	-0.0003**
	(6.4978)	(6.2551)	(5.9244)	(2.9542)	(3.0845)	(0.0013)	(0.0012)	(0.0013)	(0.0009)	(0.0009)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Treatment State	-26.1627+	-30.8211**	-33.1880**	-43.7308***	-15.5693	0.0073***	0.0074***	0.0043**	0.0056**	-0.0148***	0.0015***	0.0002	0.0004+	0.0005+	-0.0008**
	(13.0256)	(11.4788)	(11.5572)	(5.0283)	(23.8961)	(0.0015)	(0.0012)	(0.0015)	(0.0025)	(0.0037)	(0.0003)	(0.0002)	(0.0002)	(0.0003)	(0.0003)
Likely Eligible	-45.6306***	-26.4112***	-15.8827***	-8.9766**	-8.2797**	0.0030**	0.0008	0.0019+	0.0011**	0.0012**	-0.0008***	-0.0005**	-0.0002	-0.0003+	-0.0003**
	(4.9272)	(2.7280)	(3.6000)	(3.4590)	(3.7318)	(0.0010)	(0.0009)	(0.0011)	(0.0005)	(0.0006)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
Post*Likely Eligible	-6.6550	-4.7946	-3.8746	0.2427	-1.4702	0.0013	0.0010	0.0012	0.0002	0.0002	0.0000	0.0002	0.0001	0.0001	0.0001
	(4.5149)	(5.1525)	(4.8740)	(2.6041)	(3.4632)	(0.0012)	(0.0013)	(0.0014)	(0.0010)	(0.0011)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
Post*Treatment State	-12.9054+	-12.5691+	-11.6487+	-9.9532**	-14.3728***	0.0028+	0.0028**	0.0028+	0.0018+	0.0038**	0.0002	0.0002	0.0001	0.0002	0.0003
	(6.7856)	(6.6324)	(6.3241)	(3.4239)	(3.8499)	(0.0014)	(0.0013)	(0.0015)	(0.0010)	(0.0014)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
Post*Treatment State*Likely Eligible (DDD)	6.1276	6.2719	6.8207	6.1781+	9.1972**	-0.0029+	-0.0030+	-0.0032+	-0.0026**	-0.0029**	-0.0005**	-0.0006**	-0.0005**	-0.0004**	-0.0006***
	(5.2102)	(5.7143)	(5.4416)	(3.2086)	(3.7886)	(0.0015)	(0.0016)	(0.0017)	(0.0013)	(0.0014)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
N	679,557	671,556	671,425	671,425	671,425	678,902	670,997	670,866	670,866	670,866	60,011	59,668	59,668	59,668	59,668
R-squared	0.0072	0.1264	0.1297	0.1491	0.1500	0.0041	0.0265	0.0275	0.0306	0.0315	0.0312	0.0601	0.0642	0.0836	0.0895
Maternal and child controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
County controls	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Year of birth fixed effects	No	No	No	Yes	Yes	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Month of birth fixed effects	No	No	No	Yes	Yes	No	No	No	Yes	Yes	No	No	No	Yes	Yes
State fixed effects	No	No	No	Yes	Yes	No	No	No	Yes	Yes	No	No	No	Yes	Yes
State-year interactions	No	No	No	No	Yes	No	No	No	No	Yes	No	No	No	No	Yes

Notes: Each column is a separate regression. Please refer to Tables 1.3 and 1.4 for details about units of analysis, controls, and samples. Robust standard errors are clustered on the state. All regressions are weighted by the cell population.
Significance levels: + p<0.10 ** p<0.05 *** p<0.001

Table A.3: Placebo Effects in Difference-in-Difference Model for College-Educated and Married Mothers

	Birth Weight (g)	Gestation (weeks)	LBW	Premature	Total Infant Mortality	Infant Mortality: 28 days - 1 year	Infant Mortality: <28 days
Treatment State * 1991 (placebo 1)	-4.0925	-0.0300**	0.0014+	0.0021**	-0.0000	0.0000	-0.0001
	(2.7573)	(0.0089)	(0.0008)	(0.0007)	(0.0001)	(0.0001)	(0.0001)
Treatment State * 1992 (placebo 2)	-3.6875	-0.0195**	0.0009	0.0007	-0.0000	0.0002**	-0.0002
	(2.2018)	(0.0083)	(0.0006)	(0.0008)	(0.0002)	(0.0001)	(0.0002)
Treatment State * 1993+ (DD effect)	-5.3511+	-0.0222	0.0018**	0.0002	-0.0002**	-0.0000	-0.0001
	(2.8381)	(0.0163)	(0.0008)	(0.0011)	(0.0001)	(0.0001)	(0.0001)
N	672,850	672,290	672,850	672,290	59,683	59,683	59,683

Notes: The results presented here list the coefficients on the DD specifications which include placebo tests for treatment effects in the two years prior to FMLA. Please refer to notes under Tables 1.3 and 1.4 for details about controls and estimation methods. Robust standard errors are clustered on the state. All the regressions are weighted by the cell population.
Significance levels: + $p<0.10$, ** $p<0.05$, *** $p<0.001$

Table A.4: Selection into Motherhood

	Mom <19 yrs old	Mom 19-24 yrs old	Mom 25-34 yrs old	Mom 35-44 yrs old	Mom has < HS education	Mom has HS education	Mom has some college	Mom is non-Hispanic white	Mom is black	Mom is Hispanic	Mom is unmarried
DDD	0.0009	-0.0022	-0.0017	0.0031	0.0063	-0.0114**	-0.0008	0.0030	0.0032	-0.0029	0.0015
	(0.0020)	(0.0036)	(0.0036)	(0.0023)	(0.0116)	(0.0053)	(0.0050)	(0.0056)	(0.0052)	(0.0155)	(0.0067)
N	185,374	185,374	185,374	185,374	184,070	184,070	184,070	185,374	185,374	185,374	185,374
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-year interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The coefficients listed here are from DDD regressions that use the variables listed in the top row as dependent variables. The units of analysis are county-year-birth month cells. Cells with fewer than 25 births are omitted from the analysis. All regressions control for the unemployment rate in the state, year, and month of birth. Robust standard errors are clustered on the state. All the regressions are weighted by the cell population.
Significance levels: + $p<0.10$, ** $p<0.05$, *** $p<0.001$

Table A.5: Effects of FMLA on County-Level Firm Size Distribution

Outcome: Conditional Probability of Employment in Firm with 50+ Employees			
	WHOLE SAMPLE	**COLLEGE-ED AND MARRIED**	**LESS THAN COLLEGE AND SINGLE**
Treatment State * Post-FMLA	0.0008	0.0007	0.0004
	(0.0022)	(0.0017)	(0.0030)
N	5,487,100	671,783	2,054,466

Notes: The results presented here list the coefficients on the DD effects on the county-year level conditional probability of employment in a firm with 50 or more employees estimated using the CBP. Please refer to notes under Table 1.3 for details about the sample, controls, and estimation methods. Robust standard errors are clustered on the state. All the regressions are weighted by the cell population.
Significance levels: + $p<0.10$, ** $p<0.05$, *** $p<0.001$

Appendix B

Engaging Absent Fathers: Lessons from Paternity Establishment Programs

B.1 CPS-CSS Sample Construction

The CPS-CSS analysis sample is constructed as follows. I first create a "youngest child" data set by considering all individuals who are the youngest within their household and who are aged 5 years or less.[1] I drop all children who have been adopted, who have a parent that died, or who live with either no biological parent or only a father. All children who live with at least one parent have information on the line number of his/her parent in the household (which can be a mother or a father). Thus, I am able to merge children who list their mothers' line numbers directly to their mothers. I merge children who list their fathers' line numbers to their fathers and merge the fathers to their spouses in the household to obtain information on the mothers. I drop all father-child pairs in which the father cannot be merged to a spouse in the household.[2] This results in a data set of mother-child pairs,

[1] I randomly pick one child if there are multiple children that satisfy this condition (i.e., non-singleton children).

[2] I do this because I want to use the mother as the unit of observation and data limitations prevent me from observing information on the child's mother when the father is listed as the child's parent and the parents are not married. As a result, all mother-child pairs in which the unmarried parents are cohabiting and the child's parent is listed as the father are dropped. This results in only about 1% of the sample being dropped. This may still be problematic if there is an effect of IHVPE programs on the likelihood that unmarried parents cohabit. However, I can check this given that I do observe mother-child pairs in which the unmarried parents are cohabiting and the child's parent is listed as the mother. There is no statistically significant effect of

and I use the mother as the unit of observation in all analyses.

Next, using the child's age at the time of the survey, I calculate the child's approximate birth year: *birth year = survey year - child age - 1.*[3] Since there is some variation in how minors are treated in IHVPE programs, I limit my analysis to mothers aged 18-45 at the time of childbirth. Finally, I drop all mothers who moved from outside the U.S. in the last year.

B.2 Birth Records Data and Robustness Checks

Micro data from the universe of U.S. birth certificates may seem like a good source for examining the effects of IHVPE in detail. These data do not contain information on paternity establishment, but an indicator for whether the father's information appears on the birth certificate may seem like a natural proxy. Unfortunately, the law that states that a father's information cannot appear on a child's birth certificate unless paternity is established only went into effect following PRWORA (Mincy *et al.*, 2005a). Additionally, conversations with IHVPE program officials suggest that this law was not fully enforced until IHVPE programs were established. As a result, IHVPE programs had potentially offsetting effects on the likelihood of a father's information appearing on the birth certificate — on the one hand, there is a negative effect as it became more difficult to record a father's information on the certificate because paternity now needed to be established, while on the other hand, there is a positive effect as IHVPE programs encouraged paternity establishment and made it

IHVPE on cohabitation for these mothers - the coefficient of interest is -0.000082 with a standard error of 0.0005. Additionally, results from the NHIS data where respondents are explicitly asked about cohabitation suggest that there are no effects of IHVPE on parental cohabitation; instead, the likelihood that a mother cohabits with someone other than the father increases. Thus, I can conclude that this omission is likely negligible.

[3] I chose this specification because the data are collected in March; therefore, only individuals born in the first three months of the year will have had their birthday by the time of the survey.

significantly less costly.

However, research about the IHVPE program in Arizona suggests that there is potential for overcoming this issue in this state. In particular, prior to IHVPE implementation in late 1996 (the program was in effect in all Arizona hospitals by January 1997), unmarried fathers were required to fill out a "paternity presumption" form in order to be added to the child's birth certificate. Thus, unlike in many other states, mothers could not simply fill in the father's information on their own. "Paternity presumption" was not a legal form of paternity establishment, and there were no coordinated efforts by hospital staff to encourage fathers to sign the form and to consequently appear on their children's birth certificates. Once IHVPE programs were implemented, fathers had to sign a voluntary acknowledgement of paternity form (which legally established their paternity) in order to appear on the child's birth certificate. Additionally, as a result of these programs, all unmarried parents were approached and fathers were in effect encouraged to sign the form and appear on the child's birth certificate. Consequently, IHVPE programs did not substantially affect the difficulty of the father appearing on his child's birth certificate — he was required to sign a form both before and after the program. However, they did affect the likelihood of paternity establishment since all unmarried fathers were now presented with the option to sign the form.[4] All of these institutional factors allow me to estimate the effects of IHVPE program implementation in Arizona using a regression discontinuity (RD) framework.

Appendix Figure B.1 presents the graphical evidence of an RD in Arizona. Notably, there is about a 10 percentage point jump in the likelihood of the father's information appearing on the birth certificate among children of unmarried mothers at the time of IHVPE implementation.[5] Appendix Table B.6 presents the results from estimating different parametric RD

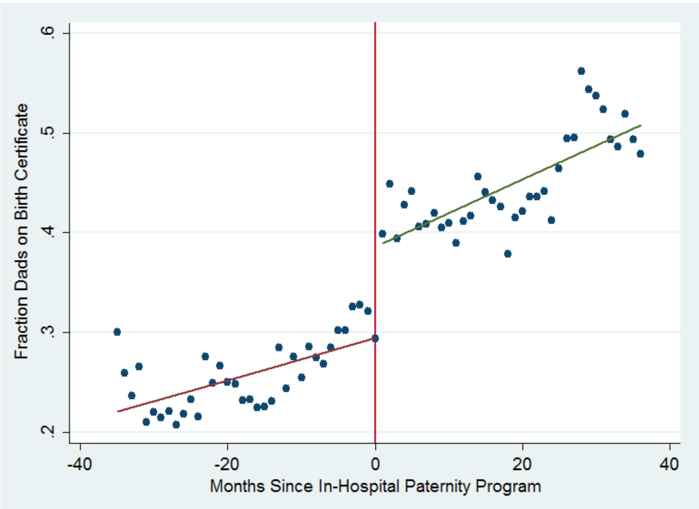
[4]Information about Arizona's IHVPE program comes from personal communications with Marjorie Cook (Cook, 2010) and Patricia Martinez (Martinez, 2011). I am grateful to both of them for sharing this information with me.

[5]The indicator for the father's information appearing on the birth certificate is equal to 0 if the father's

equations, in which the running variable is the child's year-month of birth and the dependent variable of interest is an indicator for whether the father's non-imputed education, race, or age is recorded on his child's birth certificate. Following standard RD methodology (Imbens and Lemieux, 2008), I include different order polynomials in the running variable, which are interacted with an indicator for being born in or after January 1997 (when all of Arizona's hospitals had an IHVPE program in place). The results are generally similar across the specifications and suggest that IHVPE led to about a 22 percent increase in the likelihood of the father being listed on his child's birth certificate in Arizona. I have also estimated non-parametric RD regressions (Lee and Lemieux, 2010) using different size bandwidths ranging from 3 to 25 months around the cutoff. The results from non-parametric RD specifications are similar and available upon request. Finally, standard checks for discontinuities in other variables suggest that there are no discontinuities in observable characteristics of unmarried mothers in Arizona at the time of IHVPE implementation (available upon request).

non-imputed age, race, and education are all missing, and 1 otherwise.

Figure B.1: Fraction Fathers with Information on Birth Certificates in Arizona: Unmarried Mothers, 1994-1999



Notes: This figure plots the proportion of births by unmarried mothers which have the father's information included on the birth certificate by month since IHVPE program initiation in Arizona in January 1997.

Table B.1: Timing of IHVPE Program Initiation

State	Year (and Month) of Initiation	Source	Minors Can Participate?
Alabama	1994	Alabama Code Section 26-17-22, part c)	
Alaska	1997	Alaska Statutes 18.50.165	
Arizona	July, 1996	Marjorie A. Cook, Arizona Department of Economic Security Division of Child Support Enforcement. Personal communication: 12/27/2010	
Arkansas	1994	Arkansas Code 9-10-120	
California	January, 1995	California Family Code 7571	Can rescind easily
Colorado	June, 1996	C.R.S. 25-2-112, Sec. 3.5	
Connecticut	July, 1994	Conn. Gen. Stat. Sec. 17b-27	Yes
Delaware	January, 1995	http://www.paternitynet.com/art04.html	Can rescind easily
District of Columbia	2/27/1998	D.C. Code Sec. 16-909.03	
Florida	August, 1997	Fla. Stat. Sec. 742.10	Yes
Georgia	1999	OCGA 19-7-27	
Hawaii	1999	HRS 584-3.5	
Idaho	May, 1998	http://www.healthandwelfare.idaho.gov/portals/_rainbow/manuals/cs/chapter_3/3.8_voluntary.htm	
Illinois	1997	Garfinkel & Nepomnyaschy (2007)	Can rescind easily
Indiana	1997	Angelica Carter, Attorney with the Indiana State Child Support Bureau. Personal communication: 4/13/2011	
Kansas	1997	KSA 38-1137	Can rescind easily
Kentucky	7/15/1996	KRS 406.025	No
Louisiana	July, 1998	La.R.S. 40:46.1	
Maine	1996	22 M.R.S. Sec. 2761-B	
Maryland	1997	Garfinkel & Nepomnyaschy (2007)	
Massachusetts	1994	Garfinkel & Nepomnyaschy (2007)	Yes
Michigan	1/21/1993	Public Health Code - Act 368 of 1978	
Minnesota	6/15/1995	Molly Mulcahy Crawford; Paternity Program Administrator, Minnesota Department of Human Services, Child Support Enforcement Division. Personal communication: 4/20/2011	Yes
Mississippi	1995	www.acf.hhs.gov/programs/cse/pubs/1998/best_practices/bppat98.htm	
Missouri	July, 1994	R.S. Mo 193-087	
Nebraska	1995	R.R.S. 43-1408.01	
Nevada	1995	Nev. Rev. Stat. Ann. 449.246	
New Jersey	July, 1996	Paternity Opportunity Program: http://pop.njchildsupport.org/	
New York	March, 1995	www.lawny.org/index.php/advocate-page-attorney-resources-119/38-public-advocate-information/171-paternity-for-advocates	
North Carolina	1997	GS 110-132	
North Dakota	1996	N.D. Cent. Code 14-19-06	Yes
Ohio	1999	ORC Ann. 3111.71	Yes
Oregon	November, 1995	Or. Admin. R. 333-011-0048	
Pennsylvania	January, 1998	23 PA Cons. Stat. Sec. 5103	
Rhode Island	January, 1995	R.I. Gen. Laws § 40-6-21.1	
South Carolina	1994	S.C. Code Ann. § 44-7-77	
South Dakota	1994	S.D. Codified Laws § 25-8-50	
Tennessee	1994	Garfinkel & Nepomnyaschy (2007)	Require parental consent
Texas	1999	Kevin O'Keefe, Texas Office of the Attorney General Child Support Division. Personal communication: 10/8/2010	Can rescind easily
Utah	1995	Utah Code Ann. 26-2-5	Require parental consent
Vermont	1997	Vermont Statutes Title 15, Ch. 5, § 307	
Virginia	1995	VA Code 63.2-1914	Can rescind easily
Washington	July, 1989	Paternity Affidavit Program: www.dshs.wa.gov/dcs/services/providers.asp	
Wisconsin	1999	Wisconsin Bureau of Child Support (2010), Department of Children and Families Report, "Voluntary Paternity Acknowledgement"	Require parental consent

Notes: Searches of state statutes were conducted using LexisNexis Academic.

Table B.2: Robustness - IHVPE and Health Insurance Coverage of Adult Males, CPS 1989-2002

	Dependent Variable: Respondent Has Health Insurance				
	(1)	(2)	(3)	(4)	(5)
IHVPE Program Exists in State of Residence and Year of Interview	-0.0034	-0.0013	-0.0012	0.0002	-0.0036
	(0.0031)	(0.0023)	(0.0026)	(0.0030)	(0.0029)
Demographic Controls	√	√	√	√	√
Year FEs	√	√	√	√	√
State FEs	√	√	√	√	√
State Time-Varying Characteristics Controls		√	√	√	√
Child Support Laws Controls			√	√	√
State EITC Implementation				√	√
AFDC/TANF Implementation				√	√
State-Specific Time Trends					√
N	567,318	474,176	460,473	460,473	460,473
R-squared	0.1363	0.1381	0.1386	0.1386	0.1390

Notes: Each column is a separate regression. The sample of analysis includes all men ages 18-64 in the 44 sample states over 1989-2002. The demographic controls include controls for age (<20, 20-24, 25-34, 35-44, 45-54, 55-64), education (less than HS, HS, some college, college+), race (white, black, Hispanic, other), an indicator for being married, and an indicator for being currently employed. The time-varying state characteristics include the unemployment rate, the poverty rate, the state minimum wage, the percent of the population that receives AFDC/TANF benefits, the AFDC/TANF benefit for a 4-person family, the percent of the population on Medicaid, an indicator for a Democratic governor, and the fraction of the state House that is Democratic. The controls for child support laws are indicators for whether the following child support enforcement laws are in place in the state and year of observation: universal wage withholding, genetic testing for paternity, new hires directory, and license revokation for non-payment. The state EITC implementation controls are indicators for whether a state EITC is implemented by the state and year of observation. The AFDC/TANF implementation controls are indicators for whether the AFDC waiver or the TANF program is implemented by the state and year of observation. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Table B.3: Net Effects of IHVPE on Formal and Informal Father Involvement: CPS-CSS 1994-2008 - Accounting for Selection Out of Marriage

Dependent Variable	**N**	**Coefficient**	**SE**
Father Made Any CS Payments in Last Year	34,510	-0.0145+	(0.0078)
Father Made All CS Payments in Last Year	34,510	-0.0101	(0.0083)
Father Paid On Time All or Most of the Time in Last Year	33,328	-0.0126	(0.0097)
Father Has Court-Ordered Visitation Rights	35,302	-0.0068	(0.0085)
Father Has Joint Legal Custody	35,302	-0.0221**	(0.0083)
Number Days Father Spent with Child	34,758	-5.8443**	(2.8764)
Father Provided Gifts for Child	35,302	-0.0030	(0.0091)
Father Provided Clothes for Child	35,302	-0.0087	(0.0074)
Father Provided Food for Child	35,302	-0.0078	(0.0088)
Father Covered Childcare Expenses for Child	35,302	-0.0151+	(0.0082)
Father Paid for Medical Expenses for Child	35,302	-0.0059	(0.0101)

Notes: Each coefficient is from a separate regression. Marriage is defined as a legal agreement between the mother and father. Married fathers are also assumed to have "visitation rights" and have "joint legal custody", and are assumed to have spent 365 days with the child in the past year. They are assumed to have provided gifts, food, clothes, childcare, and medical help for the child. Please refer to Table 2.5 for details about sample restrictions and controls. All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, a quadratic polynomial in the survey year, and state-specific time trends. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.
Significance levels: + $p<0.10$ ** $p<0.05$ *** $p<0.01$

Table B.4: Effects of IHVPE on Imputed Private Child Health Insurance Provision: CPS-CSS 1994-2008

	Dependent Variable: Child Has Private Health Insurance (=1 if married parents)				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
IHVPE Program Exists in State and Year of Child's Birth	-0.0278***	-0.0230***	-0.0226***	-0.0230***	-0.0235***
	(0.0059)	(0.0054)	(0.0055)	(0.0055)	(0.0061)
Mother and Child Controls	√	√	√	√	√
Year FEs	√	√	√	√	√
State FEs	√	√	√	√	√
Quadratic Polynomial in Survey Year	√	√	√	√	√
State Time-Varying Characteristics Controls		√	√	√	√
Child Support Laws Controls			√	√	√
State EITC Implementation				√	√
AFDC/TANF Implementation				√	√
State-Specific Time Trends					√
N	38,449	37,457	36,243	36,243	36,243
R-squared	0.1959	0.1970	0.1970	0.1970	0.1978

Notes: Each column is a separate regression. Children of married parents are coded as having private health insurance. Please refer to Table 2.5 for details about sample restrictions and controls. All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, a quadratic polynomial in the survey year, and state-specific time trends. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Table B.5: Effects of IHVPE on Mothers' Labor Supply — Different Definitions: March CPS 1989-2010

	Any Hours Worked	Mother is Employed	Mother is in Labor Force	Any Wage Income	Log Wage	Usual Hours Worked
Mean of Dependent Variable	0.678	0.582	0.628	0.643	9.427	23.600
IHVPE Program Exists in State and Year of Child's Birth	0.0181**	0.0144+	0.0117	0.0199**	0.0180	0.4751
	(0.0076)	(0.0081)	(0.0071)	(0.0077)	(0.0152)	(0.3090)
N	184,562	184,562	184,562	184,562	118,581	184,562

Notes: Each coefficient is from a separate regression. Please refer to Table 2.12 for details about sample restrictions and controls. All regressions include mother and child controls, controls for state time-varying characteristics, controls for child support laws, and controls for state EITC and AFDC/TANF implementation. All regressions include state and child birth year fixed effects, a quadratic polynomial in the survey year, and state-specific time trends. All regressions are weighted by the CPS person weights. Robust standard errors are clustered on the state level.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Table B.6: Parametric Regression Discontinuity Effects of IHVPE on Fathers on Birth Certificates in Arizona: Unmarried Mothers, 1994-1999

	Dependent Variable: Father's Info is on Birth Certificate				
	1st Order Polynomial	2nd Order Polynomial	3rd Order Polynomial	4th Order Polynomial	5th Order Polynomial
	Mean of Dependent Variable = 0.357				
Post-IHVPE Initiation	0.0747***	0.0755***	0.0953***	0.0321	0.0820**
	(0.0075)	(0.0111)	(0.0166)	(0.0203)	(0.0265)
Mother and Child Controls	Yes	Yes	Yes	Yes	Yes
Year and Month FEs	Yes	Yes	Yes	Yes	Yes
N	69,279	69,279	69,279	69,279	69,279
R-squared	0.0659	0.0668	0.0668	0.0672	0.0675

Notes: Each column is a separate regression. The running variable is the year-month of birth. The sample of analysis includes the universe of 1st parity births that occurred in hospitals by adult unmarried mothers in Arizona in 1994-1999. The mother and child controls are controls for the mother's age (<20, 20-24, 25-34, 35-44, 45+), mother's education (less than HS, HS, some college, college+), mother's race (white, black, Hispanic), and child sex. Standard errors are robust to heteroskedasticity.
Significance levels: + p<0.10 ** p<0.05 *** p<0.01

Appendix C

WIC in Your Neighborhood: New Evidence on the Impacts of Geographic Access to Clinics

Table C.1: Effect of WIC Clinic Presence During First Pregnancy on Probability of Moving ZIP Codes Before Next Pregnancy

	Outcome: Mother Moved Zip Codes Between Pregnancies			
	(1)	(2)	(3)	(4)
Any WIC Clinic in Zip Code of Residence	-0.0611***	0.0312+	-0.0122	0.0568**
During 1st Pregnancy	(0.0080)	(0.0179)	(0.0210)	(0.0196)
WIC Clinic * Mother is Non-Hispanic White			-0.0128	0.0102
			(0.0146)	(0.0148)
WIC Clinic * Mother is Black			-0.0163	-0.0228
			(0.0187)	(0.0178)
WIC Clinic * Mother is Hispanic			-0.0325+	-0.0256
			(0.0174)	(0.0182)
WIC Clinic * Mother's Age 20-24			0.0044	0.0038
			(0.0064)	(0.0062)
WIC Clinic * Mother's Age 25-34			0.0381***	0.0292**
			(0.0098)	(0.0094)
WIC Clinic * Mother's Age 35-44			0.0563***	0.0474***
			(0.0137)	(0.0135)
WIC Clinic * Mother's Age 45+			0.0431	0.0527
			(0.0801)	(0.0837)
WIC Clinic * Mother's Ed <HS			-0.0387**	-0.0422**
			(0.0140)	(0.0130)
WIC Clinic * Mother's Ed HS Degree			-0.0534***	-0.0475***
			(0.0132)	(0.0120)
WIC Clinic * Mother's Ed Some College			-0.0358**	-0.0330***
			(0.0112)	(0.0097)
WIC Clinic * Mother is Married			0.0260***	0.0212***
			(0.0060)	(0.0055)
WIC Clinic * Number Children			-0.0195**	-0.0146**
			(0.0060)	(0.0053)
Constant	0.1346***	0.0640***	0.1266***	0.0633***
	(0.0132)	(0.0136)	(0.0156)	(0.0135)
First Zip Code of Residence FE	No	Yes	No	Yes
N	612,690	612,690	612,690	612,690

Notes: Each column is a separate regression. The sample is limited to singleton sibling births with mothers that reside in Texas over 2005-2009. Births with missing gestation length or gestation less than 26 weeks are omitted. In addition to the listed covariates, all regressions include main effects for mother's race, age, education, marital status, and number of children as well as birth year and birth month fixed effects. The regressions in the 2nd and 4th columns also include fixed effects for the mother's first ZIP code of residence. All robust standard errors are clustered on the mother's first ZIP code of residence.
Omitted categories: mother's race - other; mother's age <20; mother's education college+; mother is unmarried.
Significance levels: +p<0.10 **p<0.05 ***p<0.001

Table C.2: First Stage/Reduced Form for IV-Mother FE

	First Stage: Any WIC Clinic in Zip Code of Residence During Pregnancy			Reduced Form: WIC Food Receipt During Pregnancy		
	All Mothers	Mothers with HS Education or Less at Time of First Birth	Mothers with College Degree or More at Time of First Birth	All Mothers	Mothers with HS Education or Less at Time of First Birth	Mothers with College Degree or More at Time of First Birth
Any WIC Clinic in Zip Code of Residence	0.7617***	0.7468***	0.8448***	0.0234+	0.0331+	-0.0044
at FIRST Birth During Current Pregnancy,	(0.0173)	(0.0143)	(0.0425)	(0.0123)	(0.0192)	(0.0163)
Assuming 39 Weeks Gestation						
Mother's Age <20	0.0186	-0.0212		0.0187	-0.0578	
	(0.0420)	(0.0719)		(0.0498)	(0.1275)	
Mother's Age 20-24	0.0219	-0.0165	0.0152	-0.0119	-0.0898	0.0336
	(0.0420)	(0.0721)	(0.0292)	(0.0495)	(0.1272)	(0.0359)
Mother's Age 25-34	0.0197	-0.0218	0.0031	-0.0127	-0.0803	0.0141
	(0.0423)	(0.0720)	(0.0264)	(0.0494)	(0.1273)	(0.0340)
Mother's Age 35-44	0.0194	-0.0269	0.0011	0.0127	-0.0303	0.0164
	(0.0415)	(0.0714)	(0.0252)	(0.0490)	(0.1267)	(0.0336)
Mother's Ed: <HS	0.0007	-0.0033		0.0679***	0.0559**	
	(0.0078)	(0.0175)		(0.0088)	(0.0218)	
Mother's Ed: HS degree	-0.0003	-0.0034		0.0801***	0.0669**	
	(0.0075)	(0.0174)		(0.0083)	(0.0221)	
Mother's Ed: Some College	-0.0037	-0.0035		0.0516***	0.0273	
	(0.0066)	(0.0177)		(0.0077)	(0.0225)	
Mother is Married	-0.0002	0.0006	-0.0024	-0.0430***	-0.0288***	-0.1154***
	(0.0037)	(0.0040)	(0.0202)	(0.0039)	(0.0042)	(0.0209)
Constant	0.1073**	0.1766**	0.0436	0.5317***	0.7452***	0.1842***
	(0.0467)	(0.0768)	(0.0428)	(0.0516)	(0.1319)	(0.0395)
R-squared	0.8087	0.7841	0.8628	0.7662	0.6504	0.7001
N	607,002	366,865	107,175	607,002	366,865	107,175

Notes: The partial F-statistics for the first stage are 1934.99, 2712.84, and 394.53, respectively. See notes under Table 3.3 for more information about the sample and estimation methods. All regressions also include mother fixed effects, and fixed effects for birth order, birth year, and birth month.

Significance levels: +p<0.10 **p<0.05 ***p<0.001

Table C.3: Effects of WIC Clinic Access on Pregnancy Weight Gain by Mother's Pre-First-Pregnancy BMI

	Weight Gain <16 Lbs	Weight Gain >40 Lbs
A. Mothers with Pre-First-Pregnancy BMI <25: IV-Mother FE		
Mean of Dependent Variable	0.147	0.191
Any WIC Clinic in Zip Code of Residence During Pregnancy	-0.0189**	0.0114
	(0.0090)	(0.0109)
N	353,798	353,798
B. Mothers with Pre-First-Pregnancy BMI >=25: IV-Mother FE		
Mean of Dependent Variable	0.209	0.186
Any WIC Clinic in Zip Code of Residence During Pregnancy	-0.0142	0.0477**
	(0.0158)	(0.0236)
N	234,846	234,846

Notes: Each coefficient is from a separate regression. See Table 3.3 for more information about the sample, controls, and the IV-Mother FE method.
Significance levels: +p<0.10 **p<0.05 ***p<0.001

Table C.4: Effects of WIC Clinic Access in ZIP Code of Residence on WIC Food Receipt: Alternative Definitions of WIC Clinic Access

	Dependent Variable: Mother Received WIC Food During Pregnancy					
	Zip FE	Mother FE	IV-Mother FE	Zip FE	Mother FE	IV-Mother FE
Any WIC Clinic in Zip Code of Residence Open During Entire Pregnancy	0.0209**	0.0072**	0.0234**			
	(0.0102)	(0.0022)	(0.0113)			
Fraction of Time During Pregnancy At Least One WIC Clinic Open in Zip Code of Residence				0.0237**	0.0073**	0.0316**
				(0.0109)	(0.0023)	(0.0138)
N	607,002	607,002	607,002	607,002	607,002	607,002

Notes: Each coefficient is from a separate regression. See notes under Table 3.3 for more information about the sample and estimation methods. In the first three columns, the key explanatory variable of interest is an indicator for any WIC clinic being open during the entire pregnancy in the mother's ZIP code of residence. In the last three columns, the key explanatory variable of interest is a continuous variable that is equal to the fraction of days during the pregnancy duration that at least one WIC clinic was operating in the mother's ZIP code of residence.
Significance levels: +p<0.10 **p<0.05 ***p<0.001

Table C.5: Effects of WIC Clinic Access in ZIP Code of Residence on WIC Food Receipt: Differences in Variation Due to Openings vs. Closings and Birth Spacing, IV-Mother FE Method

	Dependent Variable: Mother Received WIC Food During Pregnancy				
	Mothers with WIC Clinic In Zip Code of Residence During Any Pregnancy	**Mothers Who Experience an Increase in Number of WIC Clinics Between First and Last Birth**	**Mothers Who Experience a Decrease in Number of WIC Clinics Between First and Last Birth**	**Mothers with Less than 2 Years Between First and Last Birth**	**Mothers with 2+ Years Between First and Last Birth**
Any WIC Clinic in Zip Code of Residence During Pregnancy	0.0303**	0.0607	0.0577	0.0220	0.0339**
	(0.0138)	(0.0466)	(0.0451)	(0.0180)	(0.0165)
N	357,272	72,251	89,952	260,366	346,635

Notes: Each coefficient is from a separate regression. See Table 3.3 for more information about the sample, controls, and the IV-Mother FE method.
Significance levels: +p<0.10 **p<0.05 ***p<0.001

Table C.6: Robustness Check: Effects of WIC Clinic Access on Prenatal WIC Food Receipt, Birth Weight, and Breastfeeding; Dropping "Bad Data" ZIP Codes

	Mother Received WIC Food During Pregnancy	Birth Weight (g)	Child Breastfed
A. All Mothers: IV-Mother FE			
Mean of Dependent Variable	0.541	3279.029	0.748
Any WIC Clinic in Zip Code of Residence During Pregnancy	0.0346**	22.4266**	0.0213
	(0.0123)	(7.9759)	(0.0173)
N	554,498	559,904	556,437
B. Mothers with High School Degree or Less at Time of First Birth: IV-Mother FE			
Mean of Dependent Variable	0.731	3238.760	0.681
Any WIC Clinic in Zip Code of Residence During Pregnancy	0.0473**	25.1173**	0.0407**
	(0.0164)	(9.3908)	(0.0197)
N	328,231	332,702	330,334

Notes: Each coefficient is from a separate regression. See Table 3.3 for more information about the sample, controls, and the IV-Mother FE method. "Bad data" ZIP codes are ZIP codes that have at least one WIC clinic which could not be matched to a longitude and latitude coordinate using the address provided. There are 96 such ZIP codes in the data.
Significance levels: +p<0.10 **p<0.05 ***p<0.001