Essays on Policy Evaluation

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A dissertation

submitted in partial fulfillment of the

requirements for the degree of

Doctor of Philosophy

University of Washington

2017

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Abstract

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This dissertation evaluates the effects of tax policies on fertility and on women's return to work and effects of land policies on women's empowerment. In the first chapter, I study the impacts of the introduction of the child tax credit in 1998 and the expansions of the child tax credit and the child and dependent care credit (CDCC) in 2003. I use Current Population Survey data to evaluate the effects of tax policies on fertility. I find that the child-related tax credits have a positive and significant impact on the likelihood of having a child for married couples. The results suggest that providing monetary incentives is an effective way to encourage fertility.

In the second chapter, I examine the effect of tax policies on new mothers' return to work. As a part of Bush tax cuts, the Economic Growth and Tax Relief Reconciliation Act of 2001 increased the child tax credit and the CDCC and reduced the federal individual income tax rates.

With the limitation that the sample includes mothers of relatively young ages, the estimated results suggest that new mothers who graduated from high school may return to work sooner after the tax cuts. It provides some evidence that tax policies influence mothers' decision about the timing of return to work.

In the third chapter, which is a joint work with Karina Kloos, we assess the impacts of a homestead land titling program in West Bengal, India, that targets landless populations and promotes the inclusion of women's names on land titles. Using panel data collected in 2012 and 2015, we explore the effects of women's land titling on two measures of empowerment: perceptions of land tenure insecurity and land inheritance decision-making. We find consistent evidence that land titling strengthens women's perceived participation in the decision of land inheritance, but mixed evidence that the inclusion of women's names on land titles decreases their own perceived tenure insecurity. The findings suggest that formal land titling can strengthen the enforceability and durability of women's land rights. However, recognition and perceived legitimacy of those rights by family and community members especially may take more time to manifest and thereby contribute more fully to women's empowerment.

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ACKNOWLEDGEMENTS

I would like to express sincere gratitude to my adviser, Professor Elaina Rose, for her very helpful comments and guidance. I would also like to thank all of my other committee members, Professor Robert Plotnick, Professor Rachel Heath, and Professor Marieka Klawitter for their professional advice and comments. I also thank Professor Neil Bruce, Karina Kloos, Niketa Kulkarni, and the RME team in Landesa for their support and encouragement.

Chapter 1. Tax Incentives and Fertility

1.1 Introduction

Many OECD countries have experienced a decline in birth rates over the past few decades. In the United States, the birth rate has never been a major policy concern, but the birth rate dropped for six consecutive years since 2007 and hit a historical low in 2013. Can a government encourage fertility by providing monetary incentives? Many empirical researchers have tried to answer this question and uncover the relationship between the cost of children and fertility. This paper revisits this question and evaluates the effects of child-related tax credits on fertility.

The US fertility studies have concentrated on the effect of welfare programs on low-income or unwed mothers. The design of income-supporting programs may unintentionally encourage childbearing, which received a lot of attention during the 1990s (Schultz 1994; Lundberg and Plotnick 1995; Acs 1996; Robins and Fronstin 1996; Rosenzweig 1999; Hoffman and Foster 2000). Only a small branch of literature discusses tax policies and fertility, and the empirical results are inconclusive (Whittington et al. 1990; Whittington 1992; Baughman and Dickert-Conlin 2009). The introduction of the child tax credit and the expansion of the child and dependent care credit (CDCC) and the child tax credit provide exogenous variation in the cost of childbearing that may shed light on fertility responses to financial incentives.

While social welfare programs and the earned income tax credit (EITC) are targeted at low-income families, those families are unlikely to receive non-refundable tax credits since they have low or zero tax liability to claim the tax credits. The child tax was first implemented in 1998 with very limited refundibility. In 2003, the Bush tax cuts expanded both child tax credit and CDCC (the latter is non-refundable). I estimate the effects of the introduction of the child tax credit and

the Bush tax cuts on fertility of married couples using the difference-in-difference (DD) approach exploiting the fact that mid- to high-income families are more likely to be affected by changes in the child tax credit and CDCC. The results suggest that the probability of having a child increases by about 11% due to the 2003 tax cuts and about 12% due to the 1998 introduction of the child tax credit.

This study contributes to the literature in several ways. First, in the United States, only few fertility studies investigate the effect of tax policies. This study provides evidence that tax policies have an impact on marital fertility using data that are more recent. Second, studying two tax reforms makes it possible to disentangle the effects of different tax incentives. The 1998 tax reform introduced the child tax credit, while the 2003 tax cuts increased both the child tax credit and the CDCC. Third, I analyze how the effects of incentives vary by women's age and birth order. The findings not only give some hints of the effect of tax policies on total fertility but also provide some policy implications for designing an effective financial incentive to promote births.

In the following section, I summarize the previous literature on studies of welfare programs and tax policies. Section 3 illustrates the US tax credits that are related to children in detail and discusses how the tax policies influence fertility. In Section 4, I describe the data and empirical strategies adopted in the study. Sections 5 and 6 present the results of regression analysis of the 2003 tax cuts and the introduction of child tax credit, respectively. Section 7 explores the robustness of the empirical results. Section 8 concludes the findings.

1.2 Previous Research

The economic model of fertility was pioneered by Becker (1960); it suggests that policies that lower the price of a child can lead to an increase in the desired number of children. Variations in

transfer programs and taxes are two channels that are usually used to identify exogenous variations in the costs of childbearing. In the vast field of this literature, researchers investigate the impact of Aid to Families with Dependent Children (AFDC) on the fertility. Many of the studies rely on cross-state variation in the generosity of welfare benefit paid to families with children, yet findings are inconclusive and results are sensitive to methodology (Moffit 1998).

Schultz (1994) examines whether the number of children a woman has borne is related to the variation in welfare across states. Using the 1980 Census data, the OLS results indicate that for white women in the group aged 15–24, the coefficient for AFDC benefits is negative, whereas it is positive for black women aged 25–34. Lundberg and Plotnick (1995) find that state welfare (the AFDC cash benefit provided to a family with no other income plus the amount of food stamps it would receive) has significant effects on adolescent premarital childbearing decisions for white adolescents but no effect for black adolescents. While the eligibility of AFDC is contingent on the birth of the first child, welfare benefit typically increases with an additional child. Acs (1996) examines the relationship between welfare and births to women who already have a child. He finds that variations in welfare benefit levels and the incremental benefit have no statistically significant effects on the subsequent childbearing decisions of young mothers, whether they are welfare recipients or not. Robins and Fronstin (1996) find that the basic benefit level for a family of two (one adult and one child) and the incremental benefit for a second child have a positive effect on childbearing decisions of black and Hispanic women, but not of white women, and that the effects focus on high school dropouts. In contrast, Rosenzweig (1999) and Hoffman and Foster (2000) find that AFDC benefits have a statistically significant and quantitatively large positive effect on non-marital fertility in the early 20s.

In addition to welfare policies, empirical work has found that taxes also influence fertility. In time series studies, the tax exemption for dependents has been found to have a positive effect on fertility. Whittington et al. (1990) estimate an aggregate fertility equation using US data from 1913 to 1984, and the result shows that the personal exemption has a positive and significant effect on the national birthrate. Zhang et al. (1994) examine the relationship between fertility and tax policies, including the personal tax exemption for children, child tax credit, family allowance, and maternity leave benefit with Canadian data from 1921 to 1988. They find that the exemption, child tax credit, and family allowance have significantly positive effects on fertility. Crump, Goda, and Mumford (2010) revisit and reproduce the Whittington et al. (1990) paper exploiting increases in child tax subsidies in the 1990s and early 2000s, but they do not find enough evidence to support the model specification from the original paper.

A number of recent studies use individual-level data, and in general find a positive effect of tax benefits on fertility. Identifications in studies of tax policies rely on variation in generosity across states, across time, or both. Whittington (1992) uses PSID data to examine the relationship between the personal exemption and fertility of married couples from 1979 to 1983. The results support that the exemption has a significant positive effect on the likelihood of having a child during the period. Duchovny (2001) studies the impact of the changes in the EITC on fertility during the 1990s. She uses the CPS data and defines women who already have two children as a control group for women who have only one child. Employing a difference-in-difference approach, she finds strong evidence that married, white women and unmarried, nonwhite women increase their fertility following the expansion of EITC benefits. On the other hand, Baughman and Dickert-Conlin (2009) examine whether changes in EITC influence fertility in the United States. They use state-level data on birthrates between 1990 and 1999 and exploit

the large variation in the state EITC programs during this period to test whether the expansions in the credit influenced birthrate among targeted families. The results indicate that an increase in EITC produce only extremely small declines in higher-order fertility among white women.

The impact of fiscal inventive on fertility has been widely studied in other developed countries during the past ten years, and these studies mainly use a natural experiment approach in which the identification strategy is based on unanticipated changes to the generosity of tax benefits over time. Azmat and Gonzalez (2010) evaluate the effect of a 2003 reform to the Spanish income tax on fertility. The reform introduced a tax credit for working mothers with children under the age of three and expanded child deductions for all household with children. Theoretically, the effect of the reform is ambiguous, but the empirical result suggests that the reform increases fertility by almost 5%. Brewer et al. (2012) use the difference-in-difference method to estimate the effect of UK welfare reforms in 1999 (mainly the introduction of the Working Families' Tax Credit) on fertility, exploiting the fact that the reforms were targeted at low-income households. The empirical results provide evidence that the reforms have positive effects on births among coupled women, while no increase in births among single women is observed. Compared with these reduced-formed studies, Laroque and Salanie (2014) consider a structural model. Using individual data from the French Labor Force Survey, they estimate a discrete model of female participation and fertility, and their results indicate that the effect is stronger for the third birth; though the response is also positive for the first two births, it is smaller, especially for older women.

The US government has never adopted explicit pronatalist policies, while some countries provide specific tax benefits to encourage births. In the Canadian province of Quebec, the Allowance for Newborn Children (ANC) paid up to 8000 Canadian dollars to families having a

child during 1988–1997. Milligan (2005) exploits the introduction of the ANC in Quebec using a quasi-experiment, and the estimated results suggest that a 1000 Canadian dollar increase in the first-year benefit raises the probability of having a child by 16.9%. Cohen et al. (2013) use panel data from 1999 to 2005 in Israel to estimate the impact of changes in child subsidy for a marginal child on fertility. They find a positive and significant price effect in prominent magnitude on overall fertility and a small income effect on fertility.

In addition to the decision of whether to have another child, financial incentives may also distort the timing of birth. In the United States, the tax savings of having a child are realized only if the birth takes place before midnight January 1, which unintentionally creates an incentive to manipulate birth timing. Using the National Longitudinal Survey of Youth data, Dickert-Conlin and Chandra (2005) find some evidence that births are moved to the last week of December from the first week of January due to tax incentives. The regression analysis indicates that if the tax benefit increases by $500, the probability of having the child in the last week of December, rather than the first week of January, increases by 26.9 percent. In May 2004, the Australian government announced that it would provide a $3000 Maternity Payment for children born on or after July 1, 2004. The policy creates the potential for parents to behave strategically given the lag between the policy announcement and the introduction date. Gans and Leigh (2009) examine the introduction effect of "baby bonus" on birth timing, and they find that over 1000 births were "moved" to ensure that their parents were eligible for the bonus.

1.3 The US Tax System and Fertility

Unlike some Western European and Asian countries, fertility is not a major policy issue in the United States, and the government has never adopted policies to promote birth intentionally.

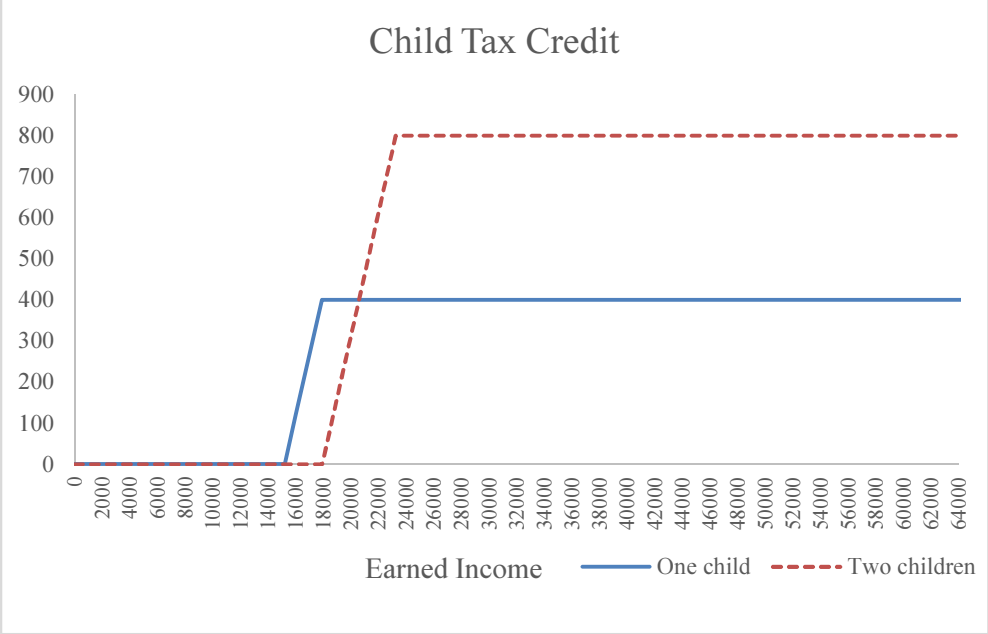
Welfare programs such as Temporary Assistance for Needy Families (TANF) and its precursor AFDC and tax policies like EITC, the child tax credit and the CDCC are not designed to increase fertility; however, these policies provide substantial monetary subsidies to families with children, and hence create a financial incentive to have a child. In this section, I briefly describe the US individual income tax system and its potential effects on fertility.

1.3.1 The Child Tax Credit

The child tax credit is a tax benefit offered for taxpayers raising dependent children under the age of 17. The child tax credit was enacted as part of the Taxpayer Relief Act of 1997. A $400 tax credit for each child under the age of 17 was introduced in 1998. The child tax credit is available to married couples who have a qualifying child within a family earning less than $130,000 per year filing a joint return, and the benefit starts to phase-out at $110,000. For married taxpayers who file separate returns, the phase-out begins at $55,000. For all other taxpayers, it starts at $75,000.[1] Figure 1.1 shows the value of child tax credit for married couples filing a joint tax return. Since the child tax credit is not refundable unless the taxpayers have three or more children in the tax year of 1998, families with two children need to have earned income more than $20,000 to receive the credit. Note that there is a range of income where the value of child tax credit for one child is greater than that for two children. As the number of children increases, the value of personal exemptions increases, leading to lower taxable income and smaller child tax credit.

[1] https://www.irs.gov/uac/ten-facts-about-the-child-tax-credit

Figure 1.1. 1998 Child Tax Credit



Note: Assume the taxpayer uses the standard deduction, no other deductions or exclusion, and the alternative minimum tax does not apply. Source: author's calculation using TAXSIM. (http://www.nber.org/taxsim/).

The Economic Growth and Tax Relief Reconciliation Act of 2001 (EGTRRA) made several changes to the child tax credit. EGTRRA set a continuous increase in the cap of the benefit from $600 beginning in 2001 to $1,000 in 2010, and the Jobs and Growth Tax Relief Reconciliation Act of 2003 (JGTRRA) sped up the increase and raised the credit to $1,000. Then the tax credit was extended several times, and it was made permanent in 2012. To sum up, the child tax credit was $400 for 1998, $500 for years 1999 and 2000, $600 for 2001 and 2002, and $1000 from 2003 to present.

The child tax credit is refundable to some extent, which is called the additional child tax credit. Before 2001, the refundability was limited: only families with three or more children were eligible for the additional child tax credit.[2] The 2001 EGTRRA extended the refundability to

[2] The child tax credit is refundable to the extent that the employee share of social security and Medicare taxes plus individual income taxes exceeds the EITC.

families with fewer than three children, but it required the total taxable earned income of married couples to exceed $10,000 in 2001, and the amount was inflation adjusted annually. From 2009, the amount was heavily reduced, to $3000. Recently, a taxpayer's total tax credit is refundable to the extent of the 15% of the taxpayer's earned income in excess of $3,000. With the movement from non-refundable to refundable, the child tax credit provides child tax benefits to families with a wide range of income, from $10,000 to 130,000. In general, to receive the full amount of the child tax credit, a taxpayer needs to have tax liability more than the value of the tax credit, and the filer's income must not be too high. Recently five states[3] provide state child tax credits, but I ignore the effect of state child tax credit in this study, focusing on the effect of the federal child tax credit on fertility.

Before the extension of the refundable portion of child tax credit (additional child tax credit), only people who earned more than moderate income were able to receive the benefit. The shares of returns and dollars of child tax credit are reported in Table A1.1. Almost no taxpayers who have income below $10,000 claim the credit, and the utilization focuses on people have more than $25,000 income. For the $10,000 to $25,000 category, the share of returns in 1998 is 24%, and the share of dollars is 15.4%, saying that people in this group may not be able to claim the full credit.

1.3.2 The Child and Dependent Care Credit (CDCC)

The CDCC is a tax credit for working families with children. The credit is available to taxpayers who have taxable earnings for expenses paid to a non-dependent individual older than 19 to care for a dependent child under age of 13 or a dependent of any age who is physically or mentally incapable of care of him or herself while the taxpayer works or looks for work. The provided

[3] New York, Oklahoma, Colorado, North Carolina, and California.

care may be inside or outside the household; however, the expense needs to be used to assure the well-being of the qualifying dependent. Eligible expenses must be reduced by pre-tax dependent care benefits provided by the employers if any. For married couples, it is required to file a joint return. Both the taxpayer and spouse must have earned income unless one spouse is a full-time student, and in general, an eligible expense must be lower than the secondary earner's income.[4]

The child and dependent care tax deduction was enacted as an itemized deduction in 1954, which was replaced with a CDCC in 1976, and then was revised in 1981 and 2001. The current revision of CDCC was implemented in 2003. As a part of the 2001 EGTRRA, the credit was expanded in two ways. First, it increased the maximum allowable expenses for child/dependent care from $2400 to $3,000 for one child and from $4800 to $6,000 for two or more children. Second, the credit rates were also expanded. The credit rate was raised from 30 percent to 35 percent for families earning adjusted gross income (AGI) $15,000 or less, and then the tax credit rate decreases one percent for every $2,000 increase in AGI until $43,000, where the credit rate reaches a minimum and constant rate of 20%. The maximum child care credits for each income category pre- and post-EGTRRA are reported in Panel A and Panel B of Table A1.2. For families with one child, the value of the tax credit increased by an amount between $120 and $402 depending on earned income.

At first glance, the child care tax provides the highest rate for low-income families, but most people who have low enough income to qualify for it do not have any tax liability. Table A1.3 shows the utilization of child care credit from 1998 to 2006. For all the years, approximately six million returns were filed claiming the child care credit, and the dollar value of the credit increased from about 2.6 to 3.6 billion dollars. Because the child care credit is not refundable,

[4] https://www.irs.gov/taxtopics/tc602.html

almost no taxpayers who earned less than 10,000 claim the credit, and the shares of returns and dollars are very concentrated (50–60 percent) on people in the high-income group (with more than $50,000 income). Some studies analyze the utilization of CDCC (Gentry and Hagy 1996; Eiler and Hrung 2003) and find that only very few low-income families take the credit. Though the credit rate increased in 2003, since it is not refundable, the low-income families may still not benefit from it.

1.3.3 The Potential Effects of Tax Benefits

Considering a simple economic model of fertility, price and income effects each have potential to affect the childbearing decision. Since the child tax credit and the CDCC are only available to families with children, the introduction or the increase of tax credits decreases the cost of raising children for eligible families and provides positive effects on fertility. The tax credits reduce taxes for eligible couples with children and increase net income, and hence create the income effect that increases demand for normal goods. Assuming children to be normal goods, higher net income as a result of the tax credits will encourage fertility. Taking both price and income effects into account, increasing child-related tax credits should have a positive effect on childbearing.

As discussed in the above, the introduction of child tax credit provides a positive effect on fertility for eligible families through price and income effects. Note that it does not require both parents to work to claim the child tax credit; the effect on labor supply decisions is only through income effect. The child tax credit is expected to have a negative effect on employment, and this could further encourage fertility. Besides, the child tax credit is available for every qualifying child under the age of 17; this creates a monetary incentive for having an additional child no

matter how many children the parents have. The child tax credit should undoubtedly have a positive impact on fertility for eligible families.

As a part of Bush tax cuts, the 2001 EGTRRA made significant changes to several areas of the US tax system. It reduced the individual income tax rates and decreased taxes by increasing the standard deduction; also, it increased the child tax credit and the CDCC. The child tax credit per child increased from $600 to $1000. The CDCC for one child expanded by an amount between $120 and $402 depending on income. Both tax credits were implemented in the tax year 2003. Figures 1.2 and 1.3 show the values of child tax credit (including refundable part) and the CDCC for married couples with two children in the tax years 2002 and 2003, respectively.

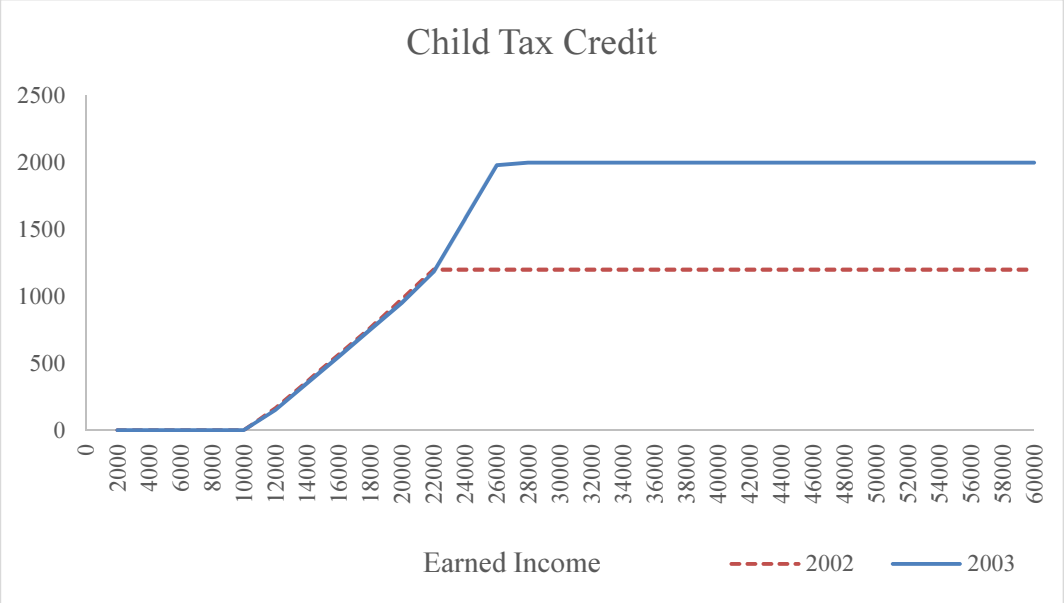
While the child tax credit provides an unambiguous incentive to have a child, the effect of the CDCC is less conclusive. The CDCC is conditional upon working and having a child under the age of 13. It creates the incentive for working women to have a child by lowering the cost of children. The purpose of the CDCC is to allow the taxpayers (mothers) to be gainfully employed; thus, an expansion in the CDCC provides an incentive for women with a small child to work or look for work. An increase in CDCC would result in a higher tendency to work, and that is expected to discourage births due to the interruption in career and potential wage losses, may indirectly dampen fertility. Even an increase in the CDCC decreases the child care cost and is expected to have a positive effect on fertility; it also affect mothers' labor force participation which is predicted to discourage fertility. The effect of the tax credit on fertility is hence ambiguous.

An increase in child tax benefits reduces the cost of raising children and encourage fertility, but are the tax credits large enough to influence the fertility decision? The U.S. Department of Agriculture (USDA) publishes the expenditures on children by families annually. The USDA

estimates that the average middle-income couple spent about $9500–10,500 (depending on the age of the child) to raise a child in 2003. The increase in tax credits counts 5% to 8% of the average expenditure. The expenditures on children depend on family income and parents' preference; the expansion of tax credits seem to be nontrivial, but whether this is sufficient to have an impact is an empirical question.

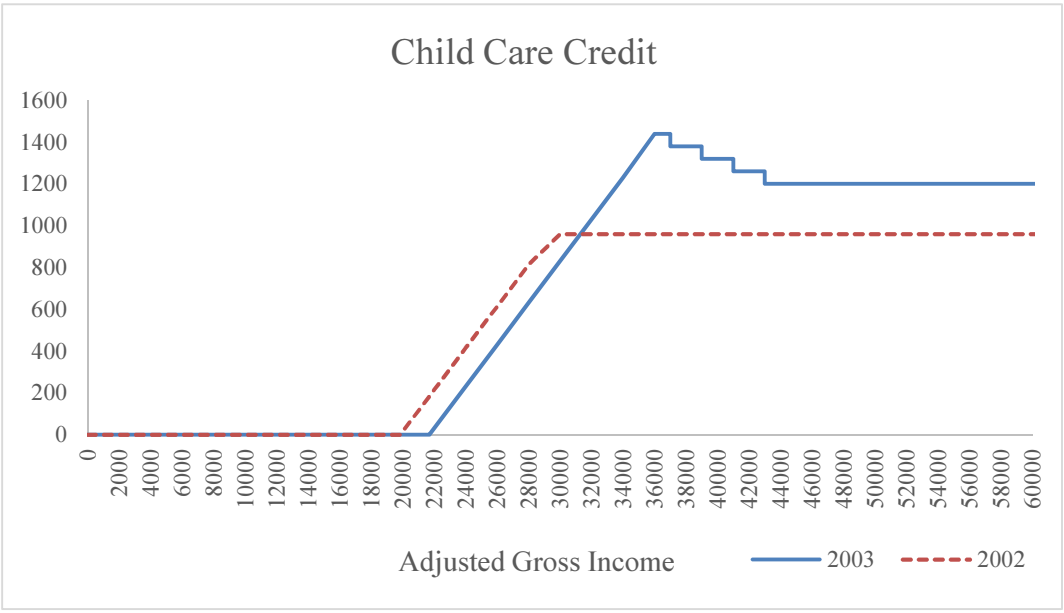
I examine the effect of the Bush tax cuts and the introduction of the child tax credit in 1998 on fertility. Because the introduction of the child tax credit provides an incentive of having a child directly and indirectly, we could expect the enactment of this new credit in 1998 to have a positive effect on fertility. The Bush tax cuts are also expected to influence fertility positively since both the child tax credit and the CDCC decrease the cost of children; nevertheless, the effect of the CDCC is ambiguous for mothers with small children, since it also influences the incentive to work.

Figure 1.2. Value of the Child Tax Credit – Married couple with two children before and after 2003



Note: Assume the taxpayer uses the standard deduction, no other deductions or exclusion, and the alternative minimum tax does not apply. Source: author's calculation using TAXSIM. (http://www.nber.org/taxsim/).

Figure 1.3. Value of the CDCC – Married couple with two children before and after 2003



Note: assume the taxpayer uses the standard deduction, no other deductions or exclusion, and that the alternative minimum tax does not apply. Assume the taxpayer uses the maximum expense and meets all requirements. Source: author's calculation using TAXSIM (http://www.nber.org/taxsim/).

1.4 Data and Methodology

1.4.1 Methodology

I exploit the introduction of the child tax credits and expansions to the child tax credit and the CDCC to estimate the effect of tax policies on fertility. Since not all the couples are eligible to claim the credits, and people who do not qualify for the tax credits should not be influenced by the expansion of the tax credits, I estimate the effect of the tax policies on fertility using the difference-in-difference method. This approach compares the outcome of an affected group to the outcome of a comparison group. To receive the full value of tax credits, families need to have more tax liability than the value of tax credits; thus, the low-income families are less likely to be affected by the introduction and expansions of the tax credits, as shown in Figures 1.1–1.3.

The amount of tax credit depends on family income; however, family income is likely to be correlated with the impact of the tax policies. It is likely to be endogenous that family income is affected by both birth and labor supply decisions, which are likely influenced by the tax reforms. In traditional labor literature, education is an important variable affecting employment and wages, and education is a time-invariant variable, at least in the short term. It should be plausible to assume that education is pre-determined. I instead use education as a proxy for income to define treatment and control groups, and define a treatment group of women who have high school diploma[5] and a control group of women who do not. A pitfall of using education is that it does not really capture eligibility of tax credits. Rather, women in the treatment groups are more

[5] I alternatively use college degree to define a treatment and a control group since the bachelor's degree is a watershed for wages. However, for example, in 2003, about 14% of married-couple families with total income less than $25,000 while about two thirds of women in the sample do not have a bachelor's degree. I argue that whether finished high school should be a better indicator of eligibility of the tax credits. Nevertheless, in general the effects of the 2003 tax cuts are found to be larger than those using high school graduates as a treatment group (as shown in Table 1.3). The estimated effects become insignificant for the introduction of child tax credit.

likely to be eligible to the tax credits and hence more likely to be affected by the changes on these tax credits, compared with women in the control group.

To be specific, I estimate the following equation:

$$Birth_{it} = \alpha + \lambda treat_{it} + \delta year_{it} + \gamma treat_{it} I(t \geq s) + X'_{it}\beta + \varepsilon_{it}, \quad (1.1)$$

where $Birth_{it}$ is equal to one if an individual *i* had a child during the 12 months preceding the survey and zero otherwise; $treat_{it}$ is equal to one if individual *i* is in the treatment group; $year_{it}$ is year dummies[6]; $I(t \geq s)$ is an indicator function equal to 1 if the period is after the policy change and zero otherwise; $\gamma treat_{it} I(t \geq s)$ is the variable of interest indicating being affected by the change of tax policies. X_{it} represents individual characteristics including wife's age, wife's age squared, dummy variables for race (non-Hispanic white), whether born in foreign countries, and metropolitan status, husband's age, husband's age squared, and husband's education level.

Two assumptions are needed for γ to be consistent. First, time effects must be common across the treated and comparison group in the absence of the tax reform. Second, the composition of the two groups must remain constant over the period. I include group-specific trends to test for common trends assumption in the robustness checks in Section 7.

Besley and Case (2000) indicate that it is important to investigate determinates of policies to avoid erroneous inference due to policy endogeneity. If some unobservable variables affect both the tax policies and fertility, then estimates of the effect of the policy will be problematic. In the United States, child tax benefits are in general intended to reduce the incidence of child poverty, encourage parents to seek employment, and ease the financial burden of raising children. To my knowledge, none of these are made to promote fertility. Further, the purpose of the 2003 tax cuts is mainly to stimulate economy, and the expansion of CDCC is intended to decrease child care

[6] I include year dummies instead of traditional "before/after" indicator to capture fluctuations of birth rates during periods of the study, as shown in Figure 1.4.

cost for low-income families; however, most low-income families are not able to benefit from the expansion since the CDCC is non-refundable.

1.4.2 Data and Descriptive Statistics

To test the hypothesis that the taxes influence fertility decisions, the empirical investigation is carried out using the Current Population Survey (CPS) March data. Since changes in the tax credits are more likely to affect planned births, I restrict the analysis to married women between ages 18 and 45 from 1994 to 2006. Each March CPS contains demographic data for individuals at the time of the survey, and data on employment and income for these individuals during the calendar year prior to the survey. The CPS is well suited to this study because CPS has a relatively large dataset and contains all the necessary information for this analysis, including age, education, race, marital status, state of residence, income, and the number and ages of children. I include data from year 1994 to 2001 to investigate the effect of the introduction of the child tax credit in 1998, and include data between 1999 and 2006 to examine the effect of the Bush tax cuts. Since the 1998 data includes children born in years 1997 and 1998, I omit 1998 data when analyzing the effect of the child tax credit; for the same reason, 2003 data are excluded from the 2003 Bush tax cuts analysis. The resulting sample sizes are 107,138 and 134,207, respectively.

Table 1.1. Descriptive Statistics

	High school dropout		Higher education	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Birth	0.091	0.287	0.082	0.274
Age	33.398	7.264	35.284	6.433
High school grad	0.000	0.000	0.679	0.467
College degree	0.000	0.000	0.321	0.467
Non-Hispanic white	0.342	0.474	0.768	0.422
Foreign born	0.533	0.499	0.138	0.345
Metropolitan status	0.781	0.414	0.762	0.426
Husband's characteristics:				
Age	36.914	8.736	37.968	7.732
College degree	0.025	0.155	0.328	0.469
No. of observations	22,417		176,293	

Note: The sample includes married women between 18 and 45 years of the age from the period of 1994 to 2006 except for1998 and 2003.

Table 1.1 reports the descriptive statistics of individual characteristics of both treatment and control groups. In the control group (high school dropouts), 91 out of 1000 women give birth during one year prior to the survey, while 82 out of 1000 women give birth in the preceding year in the treatment group (high school graduates). Women in the treatment group are slightly older, with an average age of 35, while the average age of women in the control group is about 33. In addition, 32% of higher educated women have college degree. For the treatment group, 77% are non-Hispanic white, and 14% were born in foreign countries; for the control group, these figures are 34% and 53%, respectively. Over three fourths of women live in the metropolitan area for both groups. The average age of the husbands is 38 for the treatment group, which is one year older than that for the control group. The proportion of husbands with a college degree is 33% for the treatment, but only 2.5% for the control group.

Figure 1.4 presents birth rates for both treatment and control groups over the period of 1990 to 2006, and the birth rates before/after the change of tax policies are summarized in Table 1.2.

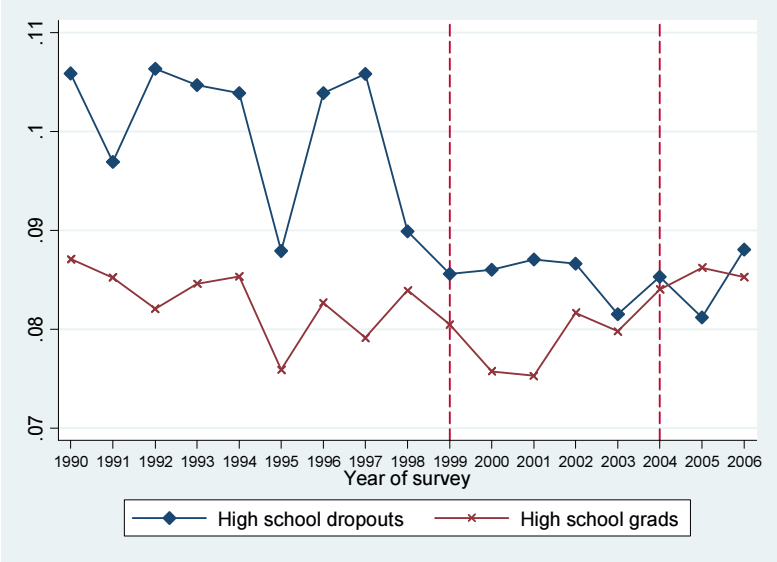
Compared with the control group, the birthrate of the treatment group was relatively stable. The birthrate of high school dropouts was more volatile during the 1990s; the volatility dropped largely in 1998, and then became steadier after 1999. The birthrate for the treatment group decreased by 4‰ after 1998, while the birthrate for the control group declined by 14‰. The difference of birthrates for the treatment and control groups becomes smaller over time. After the implement of the Bush tax cuts, the average birth rate of the treatment group increased by 6‰ while that of the control group decreased by 1‰.

Table 1.2. Average Birth Rates for Control and Treatment Groups

	High school dropouts		High school graduates	
	Before	After	Before	After
Child tax credit	0.100	0.086	0.081	0.077
Bush tax cuts	0.086	0.085	0.079	0.085

Before child tax credit: 1994–1997. After child tax credit: 1999–2001.
Before Bush tax cuts: 1999–2002. After Bush tax cuts: 2004–2006.

Figure 1.4. Birth rates by control and treatment groups



1.5 Bush Tax Cuts Results

1.5.1 Basic Results

Both child tax credit and the CDCC became more generous from the tax year 2003. I estimate the impact of the Bush tax cuts on fertility using higher-educated women as a treatment group and lower-educated women as a control group. As described in section 4.1, low-income families are less likely to benefit from the tax cuts. The next question is whether education splits can pick up the differential impact of the tax cuts. Using CPS data of 2003 (with 2002 income data), the median earned income of the treatment group is $62,000, while that of the control group is $27,000. These numbers show the extent to which education is reflected in the earned income of married couples.

Table 1.3 reports the effect of Bush tax cuts on fertility. All specifications have the same dependent variable – birth and include year dummies to control for time effects and a high school graduate dummy to control for the group effect. The interaction term of the group and the post-2003 dummies captures the difference in changes in birth probability for the treatment group after Bush tax cuts compared with the comparison group. I estimate Equation (1.1) using a linear probability model[7] with standard errors clustered on group-year to account for any dependence of the errors within group-year cells.

The first column of Table 1.3 presents the results of the regression including only year dummies, the group dummy, and the interaction term. Without demographic variables, the coefficient of the treatment effect measures the unconditional average difference in fertility of the treatment group and the comparison group post-2003 and pre-2003. The estimated treatment

[7] I also run probit regressions for all specifications, and results are in general consistent with those from a linear probability model. For ease of interpretation, especially for the interaction term, I report only coefficients from a linear probability model.

effect is 0.0077, and it is statistically significant. In Column (b), I include the mother's characteristics as controls, and expand to the father's characteristics in Column (c); the coefficients of interest are 0.0072 and 0.0068, respectively. After controlling for demographic variables, the results are still positive and statistically significant, indicating that the estimated effect in Specification (a) is not due to the difference in individual or family characteristics. Finally, state dummy variables and cohort fixed effects[8] are added to Column (d) to control for state-level socio-economic status and cohort preferences. The estimated treatment effect is 0.0087, and the average birth rate of the treatment group before 2003 is 0.079, implying an 11% increase in the probability of having another child in the treatment group after 2003.

The estimated coefficient on age is only significant in (b) and (c), and the estimated coefficients are negative, saying that the probability of another birth decreases with mother's age. When I control for state and cohort fixed effects, the negative effect becomes insignificant. Dummy variables for race and origin of mother are used to control for cultural elements. While the effect of the origin of mother is insignificant, women who are non-Hispanic white are more likely to have another child; however, the effect becomes smaller when more controls are included. The estimate of husband's age suggests that the probability of a newborn decreases with the husband's age. Husband's education level represents earning potential, and the positive coefficient of college dummy indicates a positive income effect on fertility.

[8] The data include seven cohorts: born before 1960, 1960–1964, 1965–1969, 1970–1974, 1975–1979, 1980–1984, and 1985–1988.

Table 1.3. 2003 Tax Reform Results

	(a)	(b)	(c)	(d)
Treatment effect	0.0077***	0.0072**	0.0068*	0.0087**
	(0.0022)	(0.0033)	(0.0034)	(0.0033)
High school grads	-0.0075***	0.0087***	-0.0009	-0.0028
	(0.0013)	(0.0026)	(0.0027)	(0.0028)
Age		-0.0153***	-0.0109***	-0.0071
		(0.0023)	(0.0025)	(0.0047)
Age squared		0.0001**	0.0000	-0.0000
		(0.0000)	(0.0000)	(0.0001)
Non-Hispanic white		0.0138***	0.0105***	0.0094**
		(0.0031)	(0.0029)	(0.0033)
Foreign-born		0.0038	0.0028	0.0014
		(0.0026)	(0.0026)	(0.0028)
Metropolitan status		0.0116***	0.0066**	0.0057*
		(0.0025)	(0.0026)	(0.0031)
Husband's age			-0.0075***	-0.0076***
			(0.0007)	(0.0007)
Husband's age squared			0.0001***	0.0001***
			(0.0000)	(0.0000)
Husband with college degree			0.0315***	0.0309***
			(0.0027)	(0.0026)
State & cohort fixed effects	No	No	No	Yes
N	134,207	134,207	134,207	134,207

Robust standard errors clustered on group-year are in parentheses. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

It is useful to compare the magnitude of the effect with previous studies to ensure the plausibility of this study. Brewer et al. (2012) examine the 1999 welfare reform in the UK (the mean benefit increases about £900 per year for the treatment group) and find a 15% increase in births among coupled women. Azmat and Gonzalez (2010) evaluate a 2003 reform in the Spanish income tax on fertility and find the reform increase the fertility by almost 5%. Milligan (2005) estimates the effect of the ANC in Quebec. He suggests that a C$1000 increase in benefit

received in the first year would increase the probability of having a child by 16.9%. It shows that the estimated effect of the Bush tax cuts is in alignment with earlier studies.

1.5.2 The Impact of Tax Credits on Different Birth Order

Since the value of tax credits depends on the number of children and income, individuals with different family structure may respond to financial incentive differently. Table 1.4 reports the effect of the Bush tax cuts on three subsamples comprising families with zero, one, and two or more older children. All the specifications include a full set of demographic controls, state dummy variables, and cohort fixed effects as Specification (d) in Table 1.3. For married couples without children before current birth, the probability of having the first child after tax cuts increases by about 4.3 percentage points. For married couples with one older child, the probability of having the second child rises by 2.2 percentage points. For higher parities, surprisingly, the estimated coefficient is statistically significantly negative. This indicates that compared with lower educated group, the probability of having the third or higher-order child decreases after the tax cuts.

There are some potential explanations for the negative impact on the higher-order births. To be eligible to claim the CDCC, the mother is required to work. An increase in CDCC would lead to a higher propensity to work, which is expected to discourage births due to the interruption in career and potential wage losses. Besides, for women with two children under the age of 13, and who use up the maximum eligible childcare expense for CDCC, the expansion of CDCC only creates an incentive to work though child tax credit still provides a financial incentive to have another child. The other possible explanation can be referred to Becker and Lewis (1973) quantity-quality trade-off model. For married couples with children, the expansion of tax credits

may also influence the quality of existing children. Parents may choose to substitute quality for quantity and spend more money on education.

Finally, I pool all the subsamples together, and interact the treatment effect with each of zero older children, one older child, and two or more older children. I also interact the treatment group dummy and the post-2003 dummy with each type of family structure. Under this specification, any group-specific effects and common trends in fertility for children of different birth orders are well controlled, but it allows for married couples with the different number of older children react differently to the expansion of tax credits. The results are reported in Column (d) of Table 1.4. The estimated effects are similar to those in subsample regressions, but now the negative effect on the third or higher parity become insignificant. Next, I test the homogeneity of the three main coefficients separately. I cannot reject the null hypothesis that the effect for zero older children is the same as the effect for one older child with a *p*-value of 0.4813, yet I reject the hypothesis that the effect for two or more children is the same as that for zero older children at 95% confidence level with a *p*-value of 0.0188. To conclude, the effect of Bush tax cuts on fertility is more prominent on women with less than two older children.

Table 1.4. 2003 Tax Reform Results (by number of older children)

	(a)	(b)	(c)	(d)
Treatment effect × No children	0.0430*			0.0382**
	(0.0173)			(0.0113)
Treatment effect × One older child		0.0219**		0.0257***
		(0.0075)		(0.0059)
Treatment effect × Two or more older children			-0.0059***	-0.0056
			(0.002)	(0.0052)
N	26,721	32,346	75,140	134,207

Robust standard errors clustered on group-year are in parentheses. All regressions include a group dummy and a full set of controls for wife's age, wife's age squared, race (non-Hispanic white), whether born in foreign countries, metropolitan status, husband's age, husband's age squared, husband's education level, year dummies, state dummies, and cohort fixed effects, as in Table 1.3, Specification (d). One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

1.5.3 Effect by Age

So far, I have studied the likelihood of births, but not examined the effect on the total fertility rate. Beside the number of children, the changes of tax policies also affect age of first birth and the spacing and timing of children. It is possible that more generous tax policies merely change timing of birth but have no effect on the number of children.

Table 1.5 breaks down the results by age. I divide women into five categories: younger than 25, 25–29, 30–34, 35–39, and older than 39, and the estimated effects are reported in Columns (a) to (e), respectively. The significant effects are only found for the two subsamples of women who are younger than 25 and of ages 25–29, suggesting that the effect is focused on younger women. This may be attributed to younger women having lower income, the tax credits accounting for a large share to their income. No effects (or even small negative effects) are observed for women older than 30; since older women may have more children, this may be related to the finding in the last paragraph – a small decrease in higher-order births. Given the estimated results, I cannot rule out that women in the sample solely change the timing of childbearing but not the number of children. However, it is far too early to make a conclusion. It is not possible to estimate the effect on total fertility until all women affected by the tax policies complete their periods of fertility.

Table 1.5. 2003 Tax Reform Results (by women's age)

	(a)	(b)	(c)	(d)	(e)
	< 25	25–29	30–34	35–39	> 39
Treatment effect	0.0421**	0.0303**	-0.0001	-0.0017	-0.0059*
	(0.0146)	(0.0112)	(0.0076)	(0.0049)	(0.0031)
N	8,815	19,688	28,837	33,611	43,256

Robust standard errors clustered on group-year are in parentheses. All regressions include a group dummy and a full set of controls for race (non-Hispanic white), whether born in foreign countries, metropolitan status, husband's age, husband's age squared, husband's education level, year dummies, and state dummies. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

1.5.4 DDD Results

When evaluating the effect of EGTRRA on fertility, it is not possible to isolate the effect of the expansion of the CDCC because it contains multiple changes in child-related tax credits. To distinguish the effect of the CDCC from other tax benefits, I consider the difference-in-difference-in-difference (DDD) approach. In 2003, twenty-seven states (including the District of Columbia) had the child care tax credit provisions, and most state[9] provisions are dependent on or tied to the federal credit. In other words, the state CDCC also expands after the Bush tax cuts. Compared with women living in the states that do not provide the CDCC, women living in the states with the state provisions receive more tax credits after the tax cuts. I exploit the variation of CDCC across states to examine the effect of the expansion of CDCC; the estimated effect is reported in Table 1.6. The coefficient of DDD treatment effect is 0.0196, showing that for higher educated women who live in the state with state CDCC provisions are more likely to have a child after the implementation of EGTRRA.

[9] Eighteen states provide a tax credit whose amount is a percentage of the federal credit; four states provide a tax deduction for expenses eligible for the federal credit; and three states provide a tax credit whose amount is a percentage of the expenses eligible for the federal credit. Among these, Maryland has both credit and deduction provisions. Table A1.4 shows state CDCC provisions.

Table 1.6. Results of DDD Specifications

	(a)
DDD treatment effect	0.0196**
	(0.0072)
N	134,207

Robust standard errors clustered on group-year are in parentheses. The regression includes the interaction of treatment dummy and state CDCC dummy, the interaction of state CDCC dummy and 2003 dummy, and a full set of controls for wife's age, wife's age squared, dummy variables for race (non-Hispanic white), whether born in foreign countries, and metropolitan status, husband's age, husband's age squared, husband's education level, year dummies, state dummies, and cohort fixed effects, as in Table 1.3, Specification (d). One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

1.6 Child Tax Credit Results

1.6.1 Basic Estimates

In this section, I measure the effect of the child tax credit on fertility of married women using the same approach as examining the effect of the Bush tax cuts in the last section. Since the refundability of the child tax credit was extended to families with less than three children in 2001, I use data from 1994 to 2001[10] to remove the effect of the additional child tax credit.

The main regression results of the introduction of the child tax credit in 1998 tax year are shown in Table 1.7. Specification (d) includes all demographic variables, state dummy variables, and cohort fixed effects[11]. The coefficient of the policy variable in specification (d) is 0.0095. Since the birth rate for the treatment group before 1998 is 0.081, it indicates an 11.7 % increase in the probability of having a birth in response to the introduction of child tax credit. Compared with the 2003 reform, the magnitude of estimated effect is slightly larger. The coefficients of demographic controls are generally consistent with findings for the Bush tax cuts, but there were some noticeable differences. The coefficient of age is about -0.011 to -0.017 in different specifications, indicating that the probability of another birth decreases with mother's age. The

[10] The data includes a small portion of women who gave birth in early 2001, but EGTRRA was signed in June 2001. Thus these mothers should not be affected by EGTRRA.

[11] Seven birth cohorts are included: born before 1955, 1955–1959, 1960–1964, 1965–1969, 1970–1974, 1975–1979, and 1980–1983.

effect of race becomes insignificant, while women born in foreign countries are more likely to have another child; however, the effect disappears when state dummies are included.

Note that the child tax credit is available to taxpayers who earn less than $130,000 per year as married couples filing a joint return, and the phase-out begins at $110,000. However, in the DD setting of this paper, it presumes that couples with income more than $130,000 belong to the treatment group. In the sample, about 4.3% of women in the treatment group had earned family income more than $130,000; this may negatively bias the treatment effect.

Table 1.7. The Child Tax Credit Results

	(a)	(b)	(c)	(d)
Treatment effect	0.0106***	0.0102***	0.0101***	0.0095***
	(0.0028)	(0.0030)	(0.0029)	(0.0031)
High school grads	-0.0192***	0.0019	-0.0059*	-0.0059*
	(0.0024)	(0.0032)	(0.0032)	(0.0032)
Age		-0.0168***	-0.0119***	-0.0167***
		(0.0029)	(0.0031)	(0.0031)
Age squared		0.0001**	0.0001	0.0001***
		(0.0000)	(0.0000)	(0.0000)
Non-Hispanic white		0.0034	0.0011	0.0012
		(0.0022)	(0.0020)	(0.0022)
Foreign-born		0.0102**	0.0095**	0.0057
		(0.0039)	(0.0040)	(0.0041)
Metropolitan status		0.0104***	0.0067***	0.0018
		(0.0019)	(0.0020)	(0.0024)
Husband's age			-0.0072***	-0.0072***
			(0.0010)	(0.0010)
Husband's age squared			0.0001***	0.0001***
			(0.0000)	(0.0000)
Husband with college degree			0.0246***	0.0243***
			(0.0021)	(0.0021)
State & cohort fixed effects	No	No	No	Yes
N	107138	107138	107138	107138

Robust standard errors clustered on group-year are in parentheses. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

Although the child tax credit is the same regardless of the birth order, fertility may still respond disparately by the number of older children. Table 1.8 reports the regression results of three subsamples comprising families with zero, one, and two or more older children, respectively, in the first three columns. The effects are positive for all three subsamples, but a significant change could only be observed for women with two or more children. In the pooled specification, the estimated results are quite different; the largest effect is found for women with one child, and that is significant at the 5% level. A test of the homogeneity of these three coefficients cannot be rejected at the 95% confidence level, with a *p*-value of 0.6301.

In the pooled specification in Column (d), the estimated effect for women with no children is insignificant, as found in Column (a). Recall that the strongest effect of the 2003 reform is found in women with no previous children, though only significant at the 10% level in the pooled specification. This may be attributed to the value of tax credit not being large enough ($400 to $500) to stimulate first birth during the period of study. For childless women, the financial incentives of the child tax credit are on the margin in determining whether to have a child; thus, the fertility elasticity might be different with those with existing children. Furthermore, the introduction of child tax credit has a positive effect on the third or higher birth order at 10% level, but the effect of EGTRRA is negative for this subgroup. The value of the child tax credit is equal for each child, and it is not conditional on work; thus, the tax structure may explain the different results for women with two or more children in the two tax reforms.

Table 1.8. Effects of The Child Tax Credit (by number of older children)

	(a)	(b)	(c)	(d)
Treatment effect × No children	0.0165			0.0087
	(0.0101)			(0.0114)
Treatment effect × One older child		0.0079		0.0164**
		(0.0045)		(0.0067)
Treatment effect × Two or more older children			0.0115***	0.0097*
			(0.0034)	(0.0050)
N	25,238	25,386	56,514	107,138

Robust standard errors clustered on group-year are in parentheses. All regressions include a group dummy and a full set of controls for wife's age, wife's age squared, dummy variables for race (non-Hispanic white), whether born in foreign countries, and metropolitan status, husband's age, husband's age squared, husband's education level, year dummies, state dummies, and cohort fixed effects, as in Table 1.7, Specification (d). One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

Tables 1.5 and 1.9 report the results of regressions on five subgroups divided by age including younger than 25, 25–29, 30–34, 35–39, and older than 39 in Column (a) to Column (e), respectively. While the 2003 results find no effect on older women, I find a positive and significant effect of the child tax credit on 35- to 39-year-olds and 40- to 45-year-olds, although the magnitude is much smaller for women aged 40 to 45. Since a positive effect is found for women aged 35 to 45, combined with the positive effect on higher parity births (see Table 1.8), it is possible that the child tax credit not only influences the timing of birth but also raises the number of children.

Table 1.9. Effects of the Child Tax Credit (by women's age)

	(a)	(b)	(c)	(d)	(e)
	< 25	25–29	30–34	35–39	> 39
Treatment effect	0.0118	-0.0053	0.0043	0.0266***	0.0055**
	(0.0086)	(0.0093)	(0.0072)	(0.0063)	(0.0025)
N	8,305	17,299	24,115	26,531	30,888

Robust standard errors clustered on group-year are in parentheses. All regressions include a group dummy and a full set of controls for race (non-Hispanic white), whether born in foreign countries, metropolitan status, husband's age, husband's age squared, husband's education level, year dummies, and state dummies. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

1.6.2 Additional Child Tax Credit

The analysis so far has ignored the effect of the refundable part of the child tax credit. In this section, utilizing the expansion of refundability in 2001, I measure the effect of additional child tax credit on women with lower education level. I consider the natural experiment approach and restrict the sample to women without high school diplomas during 1994 to 2003. To evaluate the effect of the child tax credit, I include two dummy variables post-1998 and post-2001. Post-1998 equals 1 if the survey is conducted during 1998 to 2001. Post-2001 take value one if the survey year is after 2001. In order to control for tax policies and welfare that may also affect fertility, I include the total value of federal plus state EITC[12] for one child and TANF/AFDC for 3-person family. I also include state GDP growth, state unemployment rate, and state average hourly wage to control for labor market conditions that could change the opportunity cost of parents' time. All state-level variables are lagged two years.

Table 1.10 reports the results of interest by parity. Column (a) shows the results for the full sample, and Columns (b)–(d) present the results for women with zero children, one child, and two or more children, respectively. The strongest effect is found among women with one child, but the coefficients of both post-1998 and post-2001 are insignificant in all specifications. There are a few potential explanations for the results. First, though the refundability was extended in 2001, families earning less than $10,000 are still not qualified for the refund, and to receive full credit requires income above $16,000 for one child and $22,000 for two children. Families with very low income are still not able to receive the credit. Second, taxpayers may not notice the changes in refundability immediately, and it takes time to respond. Although the results are not presented here, for state-level variables, only the average hourly wage is significant at the 5%

[12] State-level welfare data could be found here http://www.ukcpr.org/data

level in the childless women specification and it is positive, indicating that higher state average hourly wage is associated with higher probability of birth.

Table 1.10. Regression Results of Additional Child Tax Credit

	(a) All	(b) No older children	(c) One child	(d) Two or more children
Post 1998	0.0029	0.0171	0.01856	-0.0115
	(0.0103)	(0.0302)	(0.0257)	(0.0116)
Post 2001	0.0071	0.0308	0.0508	-0.0229
	(0.0183)	(0.0548)	(0.0458)	(0.0204)
N	19,704	3,058	4,397	12,249

All specifications include a full set of demographic control variables including wife's age, wife's age squared, dummy variable for race, whether born in foreign countries, and metropolitan status, husband's age, husband's age squared, husband's education level, state-level control variables including total value of federal plus state EITC for one child, TANF/AFDC for a 3-person family, GDP growth, state unemployment rate, and state average hourly wage, year dummies, state dummies, time trend, and time trend square. Robust standard errors are in parentheses. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

1.7 Further Analysis

The regression results show that fertility responds to the changes in tax policies substantially. In this section, I test whether the common trends assumption holds, and then check whether clustering and serial correlation problems harm statistics inference.

1.7.1 Common Trends Assumption

In this section, I examine the effect of tax credits in different specifications. I consider the 2003 tax reforms as an extension of the 1998 introduction of the child tax credit, but allow the treatment effect to differ in these two periods (1999–2002 and 2004–2006). To be specific, I evaluate the following equation:

$$Birth_{it} = \alpha + \lambda treat_{it} + \delta post1998_{it} + \gamma_1 treat_{it} \times period1_{it} + \gamma_2 treat_{it} \times period2_{it} + X'_{it}\beta + \varepsilon_{it}, \quad (1.2)$$

where $post1998_{it}$ equals 1 if survey year is after 1998, $period1_{it}$ equals 1 if survey year is

during the 1999–2002, $period2_{it}$ equals 1 if survey year is after 2003, and other variables are as defined in Equation (1.1). I estimate Equation (1.2) using longer period of time (going back to 1990), and the estimated results are reported in Column (a) of Table 1.11. The coefficients of treatment effects are 0.0139 and 0.0192 for periods 1 and 2, respectively. The 2003 treatment coefficient shows the combined policy effects for 1998 child tax credit and 2003 Bush tax cuts.

The key assumption to identify the DD approach is that fertility trends would be the same for both treatment and control groups in the absence of treatment. The DD approach is not valid if two groups are subject to differential time trends. In Column (b), I add time trend and differential time trend of the two groups to the regressions to verify the common time trends assumption. The results show that neither trend nor differential trend is statistically significant. The coefficients of treatment effects are similar with Column (a), and the 2003 treatment effect is still significant at the 5% level, indicating that the effects of 1998 and 2003 tax reforms are remarkably convincing.

Table 1.11. Robustness Checks for Common Trends Assumption

	(a)	(b)	(c)
1998 treatment	0.0139***	0.0121*	0.0147**
	(0.0035)	(0.0067)	(0.0057)
2003 treatment	0.0192***	0.0176**	0.0195***
	(0.0029)	(0.0072)	(0.0059)
Trend		-0.0003	
		(0.0006)	
Differential trend		0.0002	
		(0.0006)	
Macro-controls	No	No	Yes
N	274,535	274,535	274,535

Robust standard errors clustered on group-year are in parentheses. All specifications include a group dummy and a full set of demographic control variables including wife's age, wife's age squared, dummy variables for race (non-Hispanic white), whether born in foreign countries, and metropolitan status, husband's age, husband's age squared, husband's education level, state dummies, and the cohort effects. All specifications include CPS data from 1990 to 2006 except 1998 and 2003. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

Since women in the control group are likely to be eligible for the welfare, it should be confirmed that the decrease on birth is not caused by changes in welfare policies. In mid-1997, AFDC was replaced by the more restrictive TANF program, though the impact on married couples should be limited.[13] It may nevertheless promote marriage, and the control group is enlarged by former welfare recipients (former single mothers). If this is the case, then it would violate the condition that there should not be other shocks that affect treatment and comparison groups differently. Previous studies do not find a concrete relationship between welfare and marital status. Bilter et al. (2004) investigate the impact of welfare reform in 1996 on marriage, and the results suggest that welfare reform has led to fewer new divorces and fewer new marriages. Hoyne (1997) does not find evidence that welfare raises the propensity to form female-headed households for either whites or blacks after controlling for individual effects. Winkler (1995) exploits the cross-state variation in the generosity of AFDC benefit and the provision of AFDC-UP before the Family Support Act (FSA) of 1988 and finds that a state's provision of a UP program does not encourage two-parent families. On the other hand, during the 1990s, the EITC expanded benefits for taxpayers with two or more children. EITC may encourage mothers to participate in the labor force and hence decrease fertility; at the same time, the EITC may affect fertility in the opposite direction by reducing the cost of raising children. Baughman and Dickert-Conlin (2007) find that expanding EITC during the 1990s only produced extremely small reductions in higher-order fertility among white women.

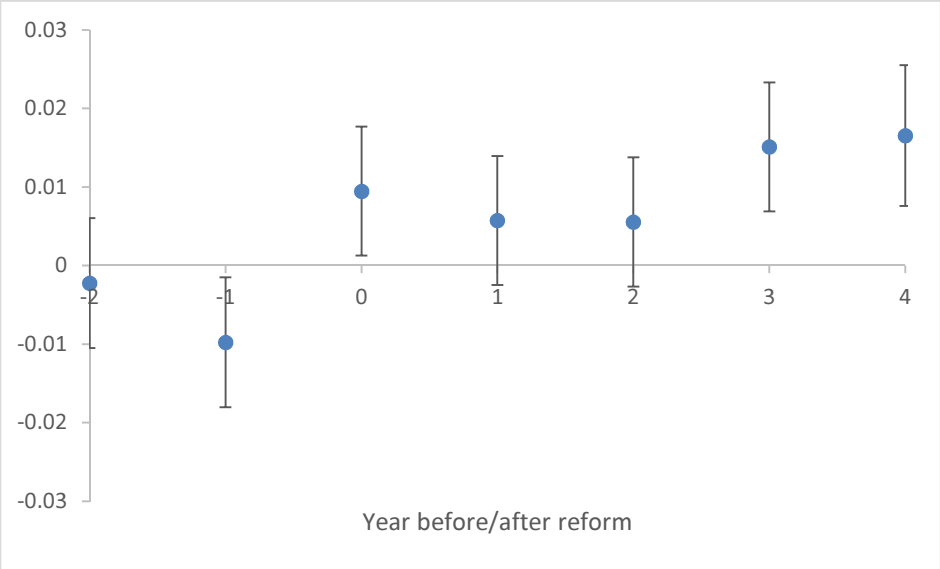
In Column (c), following the spirit of Card (1992), I include two years' lagged state-level macroeconomic and welfare controls that may affect fertility to explore the time effects and

[13] AFDC has more restrictive eligibility criteria for two-parent families. In addition to meeting other AFDC rules, a non-incapacitated two-parent family could only receive assistance if the primary wage earner had worked in at least six of the previous 13 calendar quarters (the "work history test"), the primary wage earner worked less than 100 hours per month (the "100-hour rule"), and at least 30 days had passed since loss of a job.

allow their effect to vary across two groups. These variables include the unemployment rate, GDP growth, maximum EITC for one child, and maximum AFDC/TANF for a 3-person family. I also allow the state effect to vary across the two groups. Controlling for welfare and state economic variables, the magnitudes of both coefficients increase, and are statistically significant at the 5% level.

Figure 1.5 provides an alternative check on DD specifications (Autor 2003). It plots the estimated effects and associated 95% confidence intervals from including interaction terms for the treatment group with year dummies for 1 and 2 years before 1998, 0–3 years after 1998, and year 4 and beyond. In this specification, I include a full set of demographic variables and year dummies for each year. The plot shows that the policy effects are all positive after 1998, that the effect decreases first then increases after 2001, and the largest effect is found after the implementation of EGTRRA, consistent with the results in Column (a) of Table 1.9.

Figure 1.5. Estimated impacts of the introduction/expansion of tax credits



1.7.2 Clustering and Serial Correlation

Moulton (1990) shows that in regressions with mixtures of individual and grouped data, the failure to account for common group errors can generate estimated standard errors that are biased downward dramatically. The difference-in-difference estimator is a special case of the Moulton problem. Serial correlation has long been ignored in the microeconometrics field, but the DD model also has a time dimension. Bertrand et al. (2004) show that in the typical DD model, the presence of clustering and serial correlation among residuals can have a major impact on statistical inference, over-rejecting the null hypothesis of no effect.

Bias from few clusters is a risk in both Moulton and serial correlation contexts because the inference is cluster-based in each case (Angrist and Pischke 2008). The best way to solve these problems is to increase the number of groups; however, it is not applicable in this analysis. In recent years, microeconometricians (Bertrand et al. 2004; Donald and Lang 2007; Cameron et al. 2008, 2013) have developed several methods to deal with the clustering and serial correlation problems in the DD model; nevertheless, a consensus has not yet been reached. In this section, I run several specifications proposed to correct clustering and serial correlation problems as a part of robustness checks. Results are summarized in Table 1.12.

First, I cluster one-level-up (Angrist and Pischke 2008) to estimate standard errors clustering by year. This should be able to correct clustering and serial correlation problems to a certain extent, though the number of clusters may be too small in this study. Then I make statistic inference based on *t*-distribution with (G-1) degrees of freedom and with (G-K) degrees of freedom (Donald and Lang 2007), where G indicates the number of group and K is the number of group-invariant repressor; in general, K equals 2. Inference based on (G-2) degree of freedom

leads the results to be more conservative, but *p*-values of both tax reforms are still smaller than 0.1.

Next, I consider the bootstrap-*t* procedure to improve statistic inference when bias presents in clustered standard errors. I implement pairs cluster and wild cluster bootstrap-*t* procedures. Cameron et al. (2008) show that these procedures perform well in presence with few clusters. The pairs cluster bootstrap procedure resamples the cluster with replacement from the original sample. However, this method is likely to be problematic for few clusters when there is a binary regressor that is invariant within a cluster. Some bootstrap resamples may have all clusters with the regressor taking only value 0 or 1. Since the key regressor is a binary variable, the *p*-value calculated by pairs cluster bootstrap-*t* needs to be interpreted with caution. The residual cluster bootstrap does not encounter this problem because it is not resampling the regressors. Wild bootstrap is a type of residual bootstrap; it resamples with replacement from the original residual vectors at cluster level to construct new values of the dependent variable, and for each replication the model is estimated with a value of the new dependent variable and cluster standard errors. Wild bootstrap relaxes some restrictions of residual bootstrap that assumes regression error vectors are *iid* and all clusters are the same size, and thus more flexible.

I conduct wild cluster bootstrap with Rademacher weights (+1 with probability 0.5 and -1 with probability 0.5) and impose the null hypothesis as suggested by Cameron et al. (2008). The *p*-values are reported in Table 1.12, and the results of the 2003 and 1998 reforms are 0.053 and 0.068, respectively. Although the *p*-value of pairs cluster bootstrap for the 2003 tax cuts makes the inference more conservative, the *p*-values calculated by different methods in general fall in the interval between 0 and 0.08, suggesting that the estimated effects in the analysis are reasonably credible.

Finally, I consider to use group average instead of microdata (Angrist and Pischke 2008), and ignore additional regressors, leading to an aggregated dataset with fourteen observations. This method works to correct the cluster problem but not serial correlation, and the standard errors are reliable even with few clusters. The DD estimates are reported in Table 1.12, and again ensure that the estimated effects in previous sections are fairly reliable.

Table 1.12. Robustness Checks for Clustering and Serial Correlation

	2003 tax cuts	1998 child tax credit
Linear probability model results	0.0087	0.0095
Group-year cluster	(0.0033)	(0.0031)
	[0.019]	[0.009]
Year cluster	(0.0039)	(0.0039)
Year cluster with t(G-1)	[0.068]	[0.042]
Year cluster with t(G-K)	[0.079]	[0.049]
Pairs cluster bootstrap-*t* (999 simulations)	[0.195]	[0.000]
Wild cluster bootstrap-*t* (999 simulations)	[0.053]	[0.068]
Aggregated data results (weighted by group size)	0.0075	0.0116
White-Robust standard error	(0.0027)	(0.0056)
	[0.019]	[0.071]

All specifications in the top panel include a group dummy and a full set of controls for wife's age, wife's age squared, race (non-Hispanic white), whether born in foreign countries, metropolitan status, husband's age, husband's age squared, husband's education level, year dummies, state dummies, and cohort fixed effects, as in Table 1.3, Specification (d). All standard errors are clustered by year unless specified. Standard errors are reported in parentheses. *p*-values are reported in square brackets.

1.8 Conclusion

The introduction and the expansions of child-related tax credits in the United States make it an excellent opportunity to study whether and how fertility responds to financial incentives. The primary contribution of this paper is to provide new evidence that tax credits have a positive and significant effect on the likelihood of having a child for married couples, consistent with Whittington (1992). The results suggest that the probability of having a child increases by 11% for the 2003 tax cuts and by about 12% for the 1998 introduction of the child tax credit.

Furthermore, I find some evidence of a heterogeneous response that the effects vary by birth order and women's age. The 1998 child tax credit has a positive and significant effect on women

with older children but no effects on childless women, while the most prominent effect of the 2003 tax cuts was found for childless women. Even though I study the likelihood of births, instead of the effect on the total fertility rate, the positive impacts on older women and higher birth-order after the introduction of the child tax credit in 1998 suggest that the child tax credit may influence the number of children and not merely the timing of birth. The results support that providing monetary incentives is an effective way to encourage fertility.

This paper aims to provide some evidence to policymakers who are concerned about birth rates; thus, the analysis includes only married couples. However, the marriage decision may also be influenced by the tax benefits. Since tax credits reduce the costs of childbearing, couples who want a post-marital birth may become more willing to get married. While marital status is endogenous, the estimated results may be biased. Although the primary interest of this study is planned childbirth, for those who are pregnant, the tax policies may affect abortion. When children become cheaper, women may be more inclined to continue the pregnancy even if they do not plan to have a child. Therefore, the tax benefits may reduce abortion and hence increase childbirth. Though understating these issues might weaken the credibility of the evidence provided in this study, I leave these concerns for the future study.

Appendix 1

Table A1.1. Shares of Child Tax Credit

	Year								
	1998	1999	2000	2001	2002	2003	2004	2005	2006
Panel A: Shares of Credit Returns by Income AGI range									
Below $10,000	0.1%	0.1%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
$10000 under $25,000	24.0%	22.8%	22.4%	20.0%	18.8%	16.9%	15.7%	14.8%	13.9%
$25000 under $50,000	36.8%	36.1%	35.9%	36.2%	36.6%	36.6%	35.0%	35.2%	34.5%
$50,000 and more	39.2%	41.0%	41.7%	43.8%	44.6%	46.4%	49.2%	50.0%	51.6%
Total	24,810,781	26,016,019	26,404,521	26,452,875	25,939,801	25,672,254	25,988,711	25,950,568	25,741,511
Panel B: Shares of Credit Dollars by Income									
Below $10,000	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
$10,000 under $25,000	15.4%	13.8%	13.6%	10.3%	8.6%	7.4%	5.3%	4.8%	4.8%
$25,000 under $50,000	39.4%	38.3%	37.7%	37.2%	36.4%	35.7%	30.4%	29.9%	28.9%
$50,000 and more	45.2%	47.9%	48.7%	52.6%	55.0%	56.9%	64.3%	65.2%	65.2%
Total	15,143,468	19,398,625	19,689,359	22,427,229	21,520,271	22,788,025	32,300,455	32,047,620	32,047,620

Source: IRS, Statistics of Income, Complete Year Data, Table 2. Money amounts are in thousands of dollars.

Table A1.2. The Maximum Child and Dependent Care Credit

Adjusted Gross Income (AGI)	Credit (%)	Maximum Credit ($)	
		One child	Two(+) Children
Panel A: Post 2003			
15,000	35	1050	2100
17,000	34	1020	2040
19,000	33	990	1980
21,000	32	960	1920
23,000	31	930	1860
25,000	30	900	1800
27,000	29	870	1740
29,000	28	840	1680
31,000	27	810	1620
33,000	26	780	1560
35,000	25	750	1500
37,000	24	720	1440
39,000	23	690	1380
41,000	22	660	1320
43,000	21	630	1260
43,000+	20	600	1200
Panel B: 1981 to 2002			
10,000	30	720	1440
12,000	29	696	1392
14,000	28	672	1344
16,000	27	648	1296
18,000	26	624	1248
20,000	25	600	1200
22,000	24	576	1152
24,000	23	552	1104
26,000	22	528	1056
28,000	21	504	1008
28,001+	20	480	960

Table A1.3. Shares of Child and Dependent Care Credit

	Year								
	1998	1999	2000	2001	2002	2003	2004	2005	2006
Panel A: Shares of Credit Returns by Income									
AGI range									
Below $10,000	0.1%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
$10000 under $25,000	17.2%	14.9%	14.1%	14.1%	14.7%	13.2%	12.5%	11.8%	11.1%
$25000 under $50,000	28.9%	27.4%	26.8%	26.4%	28.0%	26.3%	24.9%	26.2%	25.3%
$50,000 and more	48.2%	52.9%	54.5%	54.9%	54.1%	56.6%	59.7%	58.8%	61.0%
Total	6,128,155	6,182,193	6,368,101	6,184,507	6,185,855	6,313,297	6,316,649	6,500,596	6,466,792
Panel B: Shares of Credit Dollars by Income									
Below $10,000	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
$10,000 under $25,000	16.2%	13.4%	12.9%	12.7%	11.9%	10.6%	9.6%	9.2%	8.4%
$25,000 under $50,000	28.6%	27.3%	26.8%	26.3%	27.7%	30.0%	28.0%	29.3%	28.6%
$50,000 and more	50.4%	54.3%	56.1%	56.5%	56.9%	56.0%	59.2%	58.1%	60.2%
Total	2,660,573	2,675,147	2,793,860	2,721,061	2,706,539	3,206,890	3,337,984	3,462,104	3,486,637

Source: IRS, Statistics of Income, Complete Year Data, Table 2. Money amounts are in thousands of dollars.

Table A1.4. State Child and Dependent Credit in 2003

State	State CDCC	Tied to federal CDCC	State	State CDCC	Tied to federal CDCC
Alabama	No		Montana	Yes	No
Alaska	No		Nebraska	Yes	Yes
Arizona	No		Nevada	No	
Arkansas	Yes	Yes	New Hampshire	No	
California	Yes	Yes	New Jersey	No	
Colorado	Yes	Yes	New Mexico	Yes	No[14]
Connecticut	No		New York	Yes	Yes
Delaware	Yes	Yes	North Carolina	Yes	Yes
District of Columbia	Yes	Yes	North Dakota	No	
Florida	No		Ohio	Yes	Yes
Georgia	No		Oklahoma	Yes	Yes
Hawaii	Yes	No	Oregon	Yes	Yes
Idaho	Yes	Yes	Pennsylvania	No	
Illinois	No		Rhode Island	Yes	Yes
Indiana	No		South Carolina	Yes	Yes
Iowa	Yes	Yes	South Dakota	No	
Kansas	Yes	Yes	Tennessee	No	
Kentucky	Yes	Yes	Texas	No	
Louisiana	Yes	Yes	Utah	No	
Maine	Yes	Yes	Vermont	Yes	Yes
Maryland	Yes	Yes	Virginia	Yes	Yes
Massachusetts	Yes	Yes	Washington	No	
Michigan	No		West Virginia	No	
Minnesota	Yes	Yes	Wisconsin	No	
Mississippi	No		Wyoming	No	
Missouri	No				

[14] New Mexico provides a tax credit for a portion of childcare expenses, the amount of which is not determined by the federal credit but is affected by the amount of the federal credit claimed.

Chapter 2. Taxes and New Mothers' Return to Work after Childbirth

2.1 Introduction

Women face difficult decisions after childbirth, especially whether and when to resume work, as they traditionally serve as primary child care givers for their children. If a mother chooses to work, it incurs work-related child care expenses that reduce the net benefit of working and consequently discourage mothers to return to work. Public policies, including subsidies and tax credits, on child care that reduce the child care cost are expected to increase the probability of employment for mothers with young children.

High child care costs are considered as a barrier to employment for women with young children (Connelly 1992; Kimmel 1998; Baum 2002a; Lefebvre and Merrigan 2008). The U.S. Department of Agriculture (USDA) publishes the expenditures on children by families annually. The USDA estimates that the average middle-income couples spend about $12,680–13,900 (depending on the age of the child) to raise a child in 2015. From the same study, expenses on child care and education grew from 2%[15] of total child-related expenses in 1960 to 16% in 2015; much of this growth may be attributed to child care. The increased need for the child care is corresponding to the growth of the labor force participation of women since the 1960s.

The child and dependent care tax credit (CDCC) is a tax credit that helps working families pay expenses for the care of children, adult dependents, or an incapacitated spouse. It aims to enable parents to work or actively look for work. The child tax credit is a federal tax credit designed to help families offset the cost of raising children. Both taxes are designed to mitigate

[15] Note that child care and education expenses included families with and without the expense in 1960 while the expenses in 2015 are for families with the expense.

the financial burden of parents. The Economic Growth and Tax Relief Reconciliation Act of 2001 (EGTRRA) increased the per-child tax credit from $600 to $1000, and the amount eligible for credit spent on dependent child care increased from $2400 to $3000, thereby increasing maximum CDCCfrom $720 to $1050. The increase in tax credits counts 5% to 8% of the average annual expenditure on raising a child. In addition, EGTRRA generally reduced the rates of individual income tax, and hence increase net wage. Past studies (Leibowitz et al. 1992; Averett et al. 1997) show that the CDCC affects the labor supply decision of mothers with young children and that women who are eligible for child care tax are significantly more likely to return to work before three months after giving birth (Leibowitz et al. 1992).

Inspired by the first chapter, which finds that the CDCC may alter mothers' labor force participation and hence affect the decision to have an additional child, the goal of this study is to provide empirical evidence on women's labor force participation behavior following childbirth. In particular, I examine the effect on timing of return to work for new mothers using event-history analysis techniques. Illustrating maternity leave after childbirth is of interest because it affects their subsequent wages and work dynamics. Temporary labor market exits of mothers may lead to long-run deterioration of women's post-birth career (Lalive and Zweimüller 2009) and the large drop in wages (Harkness and Waldfogel 2003; Beblo et al. 2009; Ejrnæs and Kunze 2013). Lower child care prices may prevent mothers from being trapped at home and have persistent effects on women's employment and wages. In this paper, I test the hypothesis that mothers would return to work sooner after the tax cuts, and the results suggest that women who finished their high school education may return to work sooner after the tax cuts.

2.2 Earlier Studies

There is considerable literature analyzing women's return to work decisions after childbirth. Researchers find that public policies such as parental-leave, childcare subsidies, and in-work benefits are associated with women's post-birth employment patterns.

2.2.1 The Family and Medical Leave Act (FMLA)

The passage of the Family and Medical Leave Act (FMLA) in 1993 inspired many studies analyzing the effect of parental leave laws on labor supply decision after childbirth in the United States; however, the results are inconclusive. Hofferth and Curtin (2006) use PSID data from 1984 to 1997 to evaluate parental leave statutes and maternal employment after childbirth, and they find that women who had a child post-FMLA return to work sooner. Washbrook et al. (2011) estimate the effects of parental leave law using ECLS-B data, and they find that parental leave law has a negative effect on two months' work participation. Before the passage of FMLA, 12 states and the District of Columbia passed maternity leave legislation (MLL), which, like FMLA, allows mothers a period of leave from work after childbirth in the late 1980s and early 1990s. Using NLSY79 data for 1988–1994, Baum (2003) studies the impacts of MLL on mothers' leave-taking and return-to-work decisions. While no effect of MLL on leave taking is found, the results indicate that MLL significantly decreases the probability of returning to the same job in the second month and significantly increases the probability in the third month conditional on initially taking leave based on the dynamic multinomial model.

2.2.2 Child Care Costs

Child care costs and child care policies are also shown to affect the decision of labor supply for women with young children. Connelly (1992) evaluates the effects of child care costs on married

women's labor force participation, and the estimates of a structural model provide evidence that increased child care costs lower the probability of participation. Barrow (1999) uses NLSY79 data from 1979–1994 to analyze women's return to work decisions following first birth, and the results suggest that women facing lower child care costs are more likely to return to work within one year of their first child birth.

Child care costs are considered as a barrier to work, which may delay the timing of mothers' return to work after they give birth. The effect is expected to be larger for low-income mothers because child care costs account for a large portion of their income. Baum (2002a) investigates the effect of child care costs on the work decisions of low-income mothers with infants, and he finds that child care costs have a negative impact on the probability that mothers will return to work after childbirth, and that the negative effect is larger for low-income mothers than for non-low-income mothers. Washbrook et al. (2011) examine the effects of child care subsidies for low-income families on the timing of work participation after birth, and they find that the Child Care and Development Fund has positive effects for 2 months' and 9 months' work participation.

2.2.3 Tax Policies

There is some evidence that tax policies also affect the employment of mothers with young children. Leibowitz et al. (1992) analyze employment of new mothers and child care choice using NLSY79 data from 1979–1986, and they find that greater CDCC increase return to work within three months of giving birth, but that the effect on later labor supply is limited. Averett et al. (1997) explore the impact of the CDCC on the labor supply decisions (measured as annual hours of work) of married women with children under 6 years old using the 1986 wave of NLSY79 data. They explicitly incorporate the child care cost into a structural labor supply model. Their results suggest that government subsidies to child care significantly increase labor supply.

Brewer et al. (2006) also consider a structural model of labor supply. They investigate the impact of the introduction of the Working Families' Tax Credit (WFTC) in Great Britain, and they find that it increased labor supply of single mothers by around 5.1 percentage points and reduced labor supply of mothers in couples by 0.6 percentage points, compared with the program that preceded it. Joesch (1994) use PSID data during 1983–1986 to analyze the timing of women's paid work after childbirth, and the results show that the federal marginal income tax rate is positively associated with the hazard of starting paid work.

This paper aims to contribute to the empirical literature on the relationship of the return to work decision and tax policies by exploring the effects of the 2003 Bush tax cuts on new mothers' return to work decision after childbirth. To be specific, I test the hypothesis that mothers will begin to work more quickly after the tax cuts. The results suggest that women who finished their high school education may resume work sooner after the tax cuts, though the result seem to be more conservative if not controlling for pre-birth employment status.

2.3 Conceptual Framework

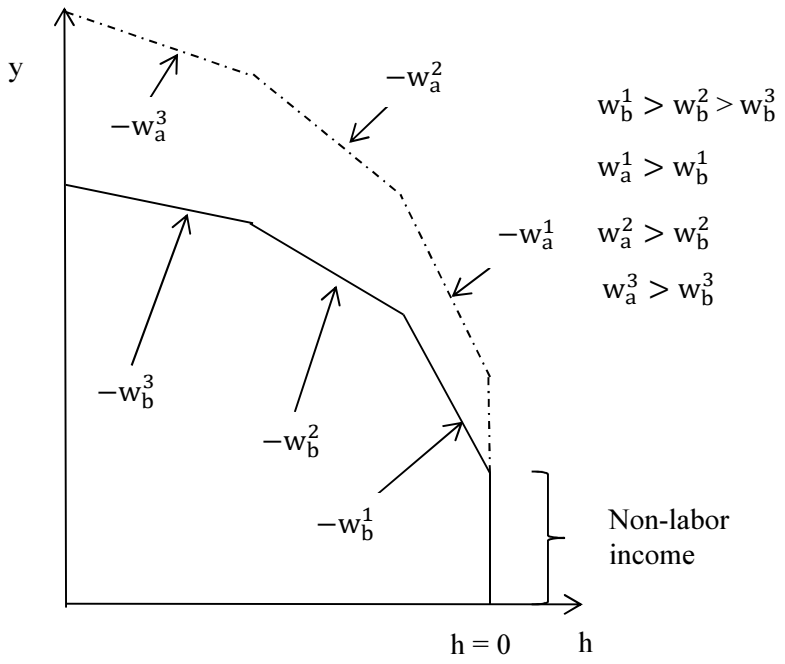
Whether to work after giving birth is a tough decision for mothers. Mothers' employment not only influences their labor market outcomes (Waldfogel 1997; Baum 2002b; Gangl and Ziefle 2009; Kahn et al. 2014; Pal and Waldfogel 2016) but also has indirect impacts on well-being of children (Waldfogel et al. 2002; Ruhm 2004; Gregg et al. 2005; Gupta and Simonsen 2010; Bernal and Keane 2011). After childbirth, the value of mothers' time at home is at its highest value, as infants require the most intensive care. The value of mothers' time depreciates as children grow since toddlers and preschool children need less-exhaustive care from their mothers. Mothers will reenter the labor market following childbirth when market productivity exceeds home productivity. The important factors affecting the timing of their return to work include the

wage rates they face in the labor market, child care costs, the husband's income and other unearned income, their home productivity, and other demographic characteristics. Consider the net wage as the market wage rate minus the child care costs, an increase in the wage rate or a decrease in the child care costs is expected to make a mother resume employment sooner.

In the next step, I take taxes into consideration. EGTRRA increased the CDCC, child tax credit, and the standard deduction and reduced individual tax rates. The CDCC that subsidizes the child care costs decreases the child care costs and increase the net wage. An increase in the net wage is expected to shorten mothers' time at home. Reduction in individual income tax rates increases the after-tax wage and the husband's income if married. A higher after-tax wage is expected to be associated with faster return to work. In addition, all of these tax benefits increase the family's disposable income; if not working is preferred to working, an increase in the family's disposable income is predicted to be associated with slower return to work. From the theoretical perspective, it is ambiguous whether the tax cuts are positively or negatively related to the timing of return to work.

Consider a progressive tax system, as shown in Figure 2.1, where h denotes hours worked, y represents income, and w_b and w_a indicate hourly net wages before and after the tax cuts, respectively. Assume there are three increasing marginal tax rates and hence three net wages rates (after-tax wage rate minus the child care cost per hour); the budget constraint before the tax cuts is drawn with solid lines. Since an individual faces lower individual tax rates, higher CDCC, and higher family disposable income after the tax cuts, the outer new budget constraint drawn with the dash lines expresses the higher non-labor income and the higher net wage rates.

Figure 2.1. Budget constraints before and after the tax cuts



In this paper, I evaluate whether the Bush tax cuts encourage mothers to work sooner after childbirth. Baum (2002a) suggests that child care costs have a larger negative effect for lower income mothers since child care cost counts for a larger portion of their income. Conditional on being eligible for the CDCC, the tax cuts may have larger influence on lower income mothers. However, the CDCC is non-refundable, meaning that only those who have tax liability can benefit from it. Women with higher education are expected to have higher market wages and are more likely to be eligible for the CDCC; thus I hypothesize that higher educated women will response more to the tax cuts. Note that the CDCC subsidizes paid care, so it alters the relative prices of paid and unpaid care. The expansion of the tax credit is expected to be more influential in return to work for mothers using formal child care; nevertheless, in this study, I leave this issue aside.

2.4 Data and Method

2.4.1 Data

To assess the effect of EGTRRA, I use the National Longitudinal Survey of Youth 97 (NLSY97). The NLSY97 follows the lives of a sample of American youth born between 1980 to 1984; 8,984 respondents were ages 12–17 when first interviewed in 1997, of who, 4,384 are female. I include individuals who gave their first birth during 2000 and 2006. To ensure a chance of employment, women who were under 18 years old at the time of giving birth are excluded. I further exclude women who gave birth to twins or triplets because the initial event is different from women with single births and there are not enough multiple births for evaluation. In the end, 1069 women are included in the study.

The NLSY97 data include work history. One can observe the employment gap – the interval between the exit and the reentry to the labor market – through it. I then extract the employment histories spanning the period of one month prior to birth and 24 months after childbirth of each mother. The employment status of each mother is tracked until she returned or began to work or giving birth to a second child. Because the period of observation ends at 24 months after birth, mothers not working in 24 months after birth are right censored. Table 2.1 shows the overall results of the dependent variable by employment status pre-birth and by education level. Of the 1069 mothers who had their first child during 2000 to 2006, 565 mothers were employed pre-birth, and 840 mothers had at least high school education. The majority of mothers began to work by 24 months. The medium return time is 3 months. As expected, mothers who were not employed before giving birth or had not finished their high school education returned to work later.

Table 2.1. Return to Work after Childbirth

	Number of Observations	Percentage of all births	Percentage who eventual work	Median return time
All Birth	1069	100%	87.8%	3 months
Employment Status				
Employed pre-birth	565	52.9%	96.5%	1 months
Not employed pre-birth	504	47.2%	78.8%	8 months
Education				
High school graduate	840	78.6%	90.5%	2 months
Had not finished high school	229	21.4%	78.2%	6 months

In this paper, I aim to measure the effect of EGTRRA, which was implemented in 2003. The only national dataset that tracks employment status before and after 2003 is NLSY97.[16] However, NLSY97 data collect information of youth born in 1980–1984, they are aged 18–26 during 2000–2006, and the sample over-represents women who gave birth of the first child at relatively young ages. Women who choose late motherhood may have higher education and higher income, are not included in this study. In addition, since the sample includes relatively young mothers, high percentages of the women are unwed mothers. The descriptive statistics are shown in Table 2.2.

[16] PSID (Panel Study of Income Dynamics) also track the employment status, however, interviews have been biennial since 1997.

Table 2.2. Descriptive Statistics

Variables	Mean	Standard deviation
High school graduates	0.7857	0.4105
Age	21.0795	2.0298
Employed pre-birth	0.5285	0.4994
Race		
Non-black/non-Hispanic (base)	0.4443	0.4971
Black	0.3188	0.4662
Hispanic	0.2368	0.4253
Married at the year of childbirth	0.2909	0.4544
Child male	0.5126	0.5001
Her mother with high school diploma	0.6969	0.4598

Note: Number of observations = 1069

2.4.2 Empirical Approach

Survival analysis is used to estimate the movement from not working (at home) to working. The survival function is the probability that duration, the time spent in a given state, equals or exceeds time *t.* Duration, which is denoted as *T*, is the time spent at home in this study. The survival function is defined as

$$S(t) = \Pr\ [T > t]\ = 1 - F(t), \tag{2.1}$$

where $F(t)$ is the cumulative distribution function of T. $F(t)$ indicates the probability that the duration is less than *t.* The density function of T is $f(t) = dF(t)/dt$.

Recent treatments of duration analysis tend to focus on the hazard function. The hazard function describes the instantaneous probability of leaving a state conditional upon survival to time *t.* The hazard function is defined as

$$\lambda(t) = \lim_{\Delta t \to 0} \frac{\Pr[t \le T \le t + \Delta t | T \ge t]}{\Delta t} = \frac{f(t)}{S(t)}. \tag{2.2}$$

It is the probability that the failure event (begin to work) occurs in a given interval, conditional upon the subject having survived (have not worked) to the beginning of the interval.

I use the Cox proportional hazards model to estimate the effect of EGTRRA on the hazard of starting to work – in other words, to estimate the probability of return or begin to work in month *t* after childbirth, given that the mother did not become employed prior to month *t*. The Cox proportional hazards model is commonly used in post-birth employment analysis (Hofferth and Curtin 2006; Berger and Waldfogel 2004; Joesch 1994) as it does not require specification of the function form of baseline hazard function. Specifically, I estimate the following equation

$$\lambda(t|\mathbf{x},\beta) = \lambda_0(t)\exp(\mathbf{x},\beta), \tag{2.3}$$

where $\lambda_0(t)$ is the baseline hazard function and is a function of *t* alone, and $\exp(\mathbf{x},\beta)$ is a function of **x** (the covariates) alone. Positive coefficient estimates for the explanatory variables indicate that higher levels of the variables increase the hazard of working; negative coefficient estimates should be interpreted in the opposite direction.

In this study, a birth of interest is the first birth of each woman who did so during 2000 to 2006. A woman is considered as starting to work if she is working for an employer or in active military service. The key independent variable is a dummy variable, Post-EGTRRA, which is equal to 1 if a child was born in 2003 or later, and equal to 0 otherwise. Since the CDCC requires mothers to work, its expansion is expected to have larger effect among women who with higher market productivity. To account for this, I include the interaction term of two dummy variables: Post-EGTRRA and whether a mother has high school diploma. In addition, I also include a dummy to describe the employment status a month before childbirth. It is easier for a mother to return to the same position if working during pregnancy; therefore, it reduces the cost of searching for a new job post-birth. Employment status during pregnancy may also be a signal of

a woman's preference for work. Given the above reasons, I hypothesize that women who were employed pre-birth are more likely to return to work sooner after childbirth. Other control variables include education, age, race, marital status, gender of the first child, mother's education level, and the unemployment rate and the annual GDP growth at the year of childbirth.

The advantage of the Cox proportional hazards model is that it makes no assumptions about the shape of the baseline hazard, which can thus be left un-estimated. However, there is a drawback to using Cox model: for any two individuals, the ratio of their hazards is the same over time (the proportional-hazards assumption). That is, comparing individual *j* to individual *m*, the model states that

$$\frac{\lambda\left(t\middle|\mathbf{x_j},\beta\right)}{\lambda(t|\mathbf{x_m},\beta)} = \frac{\exp(\mathbf{x_j},\beta)}{\exp(\mathbf{x_m},\beta)}, \tag{2.4}$$

which is constant assuming the covariates $\mathbf{x_j}$ and $\mathbf{x_m}$ do not change over time.

To test the proportional-hazards assumption, I use the method proposed by Grambsch and Therneau (1994). The test results show that some independent variables violate the assumption. In particular, employment status pre-birth was found to have different associations with the risk of work at different points of time. If not controlling for employment status before childbirth, then there is no evidence that the specification violates the proportion-hazards assumption. Following Joesch (1994), I interact employment status pre-birth with the logarithm of duration to account for the time-varying effect.

2.5 Results

2.5.1 Kaplan–Meier Estimates

The Kaplan–Meier estimator is a nonparametric estimate of the survival function, which is the probability of survival past time *t* – in other words, the probability of failing after time *t*. Figure

2.2 displays the Kaplan–Meier estimates of the survivor function for time not participating in the labor market for all new mothers. After the first month the survival probability is 0.63, suggesting that about 37 percent of the new mothers had terminated their initial state of not working in the labor market within a month after childbirth. Similarly, after 6 months and after 24 months of childbirth, the survival probabilities are 0.32 and 0.11, respectively. As expected, the survivor functions are quite different for new mothers who were employed pre-birth and who were not, as shown in Figure 2.3. The survival probability is 0.37 for women who were employed a month prior to the childbirth after the first month while the survival probability is 0.92 for women who were not employed pre-birth, suggesting that the majority of women who were employed pre-birth resume working in the first month after childbirth.

The primary interest of this study is to examine whether mothers return/begin to work sooner after tax cuts, and the survivor functions by the implementation of EGTRRA are presented in Figure 2.4. According to these estimates, mothers returned to work sooner after the implementation of tax cuts. Compared with other post-birth employment studies, the proportion of mothers who resume work in two years is slightly higher but still in a reasonable range (Hofferth and Curtin 2006; Joesch 1994). The explanation could be that this study only includes the first births. If mothers have two or more young children, they may be more reluctant to work due to high market child care costs. Besides, this study uses relatively young mothers, who have less work experience and hence have less income, and thus may not be able to afford maternity leave.

Figure 2.2. Kaplan–Meier survivor function of the proportion of women remaining home after birth, by month

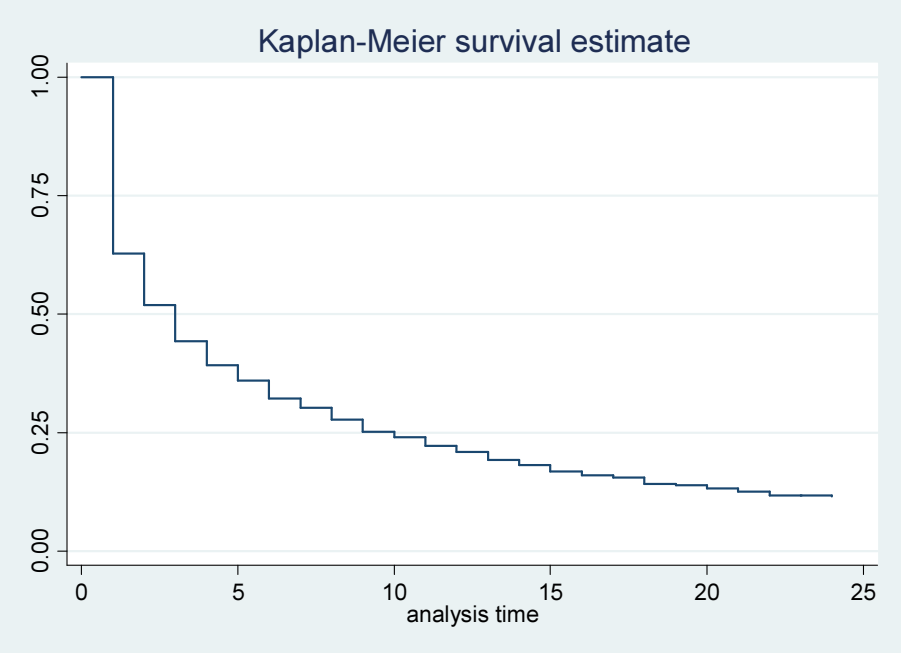


Figure 2.3. Kaplan–Meier survivor function of the proportion of women remaining home after birth, by month, and pre-birth employment status

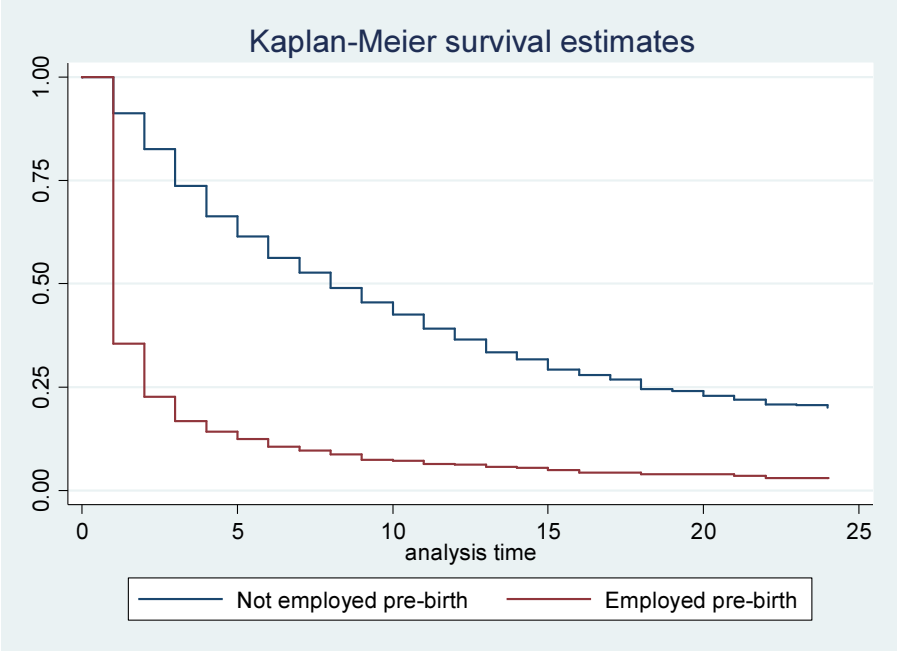
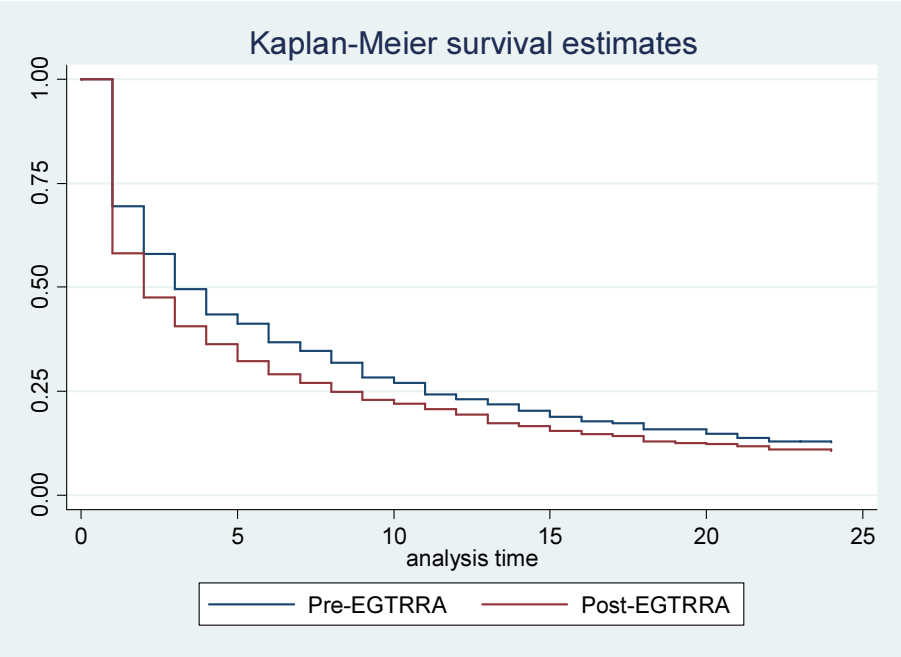


Figure 2.4. Kaplan–Meier survivor function of the proportion of women remaining home after birth, by month, and by before/post tax cuts



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2.5.2 Cox Proportional Hazards Model Estimates

The estimated results of the Cox proportional hazards model are reported in Table 2.3. In both Model (a) and Model (b), I include the interaction of the EGTRRA dummy and a dummy whether a mother has high school diploma to explain that mothers with higher education should be more responsive to the tax cuts. Model (b) controls for pre-birth employment status, and I find that the effect of being employed pre-birth has the largest impact, which is consistent with earlier studies (Berger and Waldfogel 2004; Joesch 1994).

The coefficient of the post-EGTRRA variable captures the effect of tax cuts on women who did not graduate from high school and the estimated coefficient for the interaction term of post-EGTRRA and the high school graduate dummy describes the effect on women who had finished high school education. From Model (a) and Model (b), the estimated coefficients show that

EGTRRA dummy are insignificant in both models while the coefficient of the interaction terms are positive and significant, indicating that EGTRRA may only affects mothers with high school diplomas. Among mothers with high school diplomas, the rate of employment is estimated to be about 38%[17] higher for women who gave birth after the tax cuts than for women who gave birth before the tax cuts, whether controlling for pre-birth employment status or not. These results are in line with the expectations that women with higher education assumed to have higher market productivity are more likely to be affected by the tax cuts. Age and education are both positively associated with faster return to work; however, when pre-birth employment status is controlled for, these effects become insignificant. Besides, I find that new mothers who were married when giving birth return to work later, facing about 0.85 to 0.9 of the hazard of mothers who were not married, with all else equal. The spouse may contribute income, and higher family income may make maternal leave more affordable.

[17] For a categorical 0/1 scalar variable, the impact of change from 0 to 1 is $\exp(\beta) - 1$, which measures impact relative to the baseline hazard.

Table 2.3. Results of Risk of Return to Work

	(a) Not controlling for employment status	(b) Controlling for employment status
Post-EGTRRA	-0.3254	-0.2770
	(0.2448)	(0.2084)
High school graduate	0.3204***	0.0261
	(0.1096)	(0.1017)
Post-EGTRRA* High school graduate	0.3085*	0.3220**
	(0.1865)	(0.1610)
Black	-0.0144	-0.0036
	(0.0769)	(0.0563)
Hispanic	-0.0957	-0.0472
	(0.0922)	(0.0689)
Age	0.0902***	0.0273
	(0.0237)	(0.0190)
Married at the year of childbirth	-0.1356*	-0.1014*
	(0.0803)	(0.0523)
Child male	-0.0840	-0.0792
	(0.0659)	(0.0483)
Her mother has high school diploma	0.0007	-0.0160
	(0.0791)	(0.0642)
Unemployment rate at the year of birth	-0.0750	-0.0248
	(0.0623)	(0.0482)
GDP annual growth at the year of birth	-0.0253	0.0022
	(0.0462)	(0.0373)
Time trend	0.0012	-0.0309
	(0.0424)	(0.0328)
Employed pre-birth		4.8026***
		(0.2351)
Employed pre-birth*log(duration)		-2.2767***
		(0.1139)
N	1069	1069

Coefficients and robust standard errors are reported. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

2.6 Conclusion

As a part of Bush tax cuts, the Economic Growth and Tax Relief Reconciliation Act of 2001 (implemented in 2003) increased the child tax credit and the CDCC and reduced the federal income tax rates. The tax cuts decreased the child care costs and raised the net wages and other family income. While the sign of the effect is theoretically indeterminate, I examine the effect of EGTRRA on women's decisions about when to return to work using data from National

Longitudinal Survey of Youth during 2000–2006. With the limitation that the sample includes mothers in relatively young ages, the estimated results suggest that new mothers who finished their high school education may return to work sooner after the tax cuts.

The results imply that the tax credits on child care costs may encourage mothers to be employed more quickly as its purpose is to enable parents to work or actively look for work, although it seems only affect women with high school education. Since women in the sample are not representative, the effects are expected to be stronger for women who have their first child late since they may be better educated and have higher income, and are thus more likely to receive the full amount of the credits.

Chapter 3. Land Titling and Women's Empowerment: Evidence from India

3.1 Introduction

Land is the most important asset for the majority of the rural poor in developing countries. The rural poor are often smallholder farmers where land is also the main source of livelihood. Moreover, land tenure security can provide key access to government and financial services. Women throughout the majority of the world, however, have long faced gender discrimination in land access and ownership. By one estimate, less than one quarter of agricultural land holdings in developing countries is operated by women.[18] In India, women only account for an estimated 12.8 percent of all land holders.[19] Despite legal frameworks that formally protect women's equal land rights in most countries, including India, the dominance of patriarchal customary traditions, where sons are favored over daughters for land inheritance; where a woman is not often perceived as a legitimate land user or owner; and where a woman's assertion of her land rights may cause tension or even escalate into conflict renders many women dependent on male household members for access to, ownership of, and control over land. Women who are in informal unions, widowed, divorced, or abandoned may be especially vulnerable to being dispossessed of their lands by family members or community members.

Economic theories have long predicted that access to assets, such as land, can provide financial security for women and improve their household bargaining power (Manser and Brown 1980; McElroy and Horney 1981; Lundberg and Pollak 1993). The improvement in bargaining power can reduce gender discrimination by giving women more control over decisions and re-

[18] http://www.fao.org/economic/es-policybriefs/multimedia0/female-land-ownership/en/
[19] 2010-2011 Agricultural Census data found here: http://www.fao.org/gender-landrights-database/data-map/statistics/en/

allocating resources towards women's preferences. Indeed, empirical studies have shown that an increase in women's asset ownership can be positively related to women's intra-household decision-making power (Allendorf 2007; Garikipati 2009; Swaminathan et al. 2012a; Wiig 2013; Ali et al. 2014; Mishra and Sam 2016). Furthermore, with increased intra-household decision-making power, women tend to make investments that benefit the whole family, contributing, for example, to children's nutrition and health (e.g. Quisumbing et al. 1995; Allendorf 2007; Menon et al. 2014). Conversely, studies have shown that gender inequality can hinder a household's agricultural efficiency (e.g. Urdy 1996).

In practice, the empowerment of women is widely regarded as an important goal of international development, including efforts to increase women's equal rights to economic assets. "Promote Gender Equality and Empower Women" was one of the eight Millennium Development Goals (MDGs) that were committed by world leaders in 2000. Given the persistence of gender inequality, empowering women and girls hence remains on the global agenda. The Sustainable Development Goals (SDGs) adopted in 2015 include a goal to "Achieve gender equality and empower all women and girls." One of the specific targets of this goal is to "undertake reforms to give women equal rights to economic resources, as well as access to ownership and control over land and other forms of property, financial services, inheritance and natural resources, in accordance with national laws."[20]

Joint titling programs and policies are being promoted to strengthen women's land rights in many countries. Land reforms, for example, the Land Tenure Regularization reform in Rwanda and the Special Land Titling and Cadaster Project in Peru, emphasize joint ownership of land for husbands and wives. Wiig (2013), assessing the impacts of joint titling in Peru, finds significant

[20] See https://sustainabledevelopment.un.org/sdg5, target 5.a; emphasis is the authors', not in the original.

positive effects on an aggregate empowerment index when women's names are included on land titles. Ali et al. (2014) suggest that in Rwanda the joint land titling program significantly improves gender equitable land inheritance.

In an effort to contribute to the body of empirical evidence, we use survey data collected in 2012 and 2015 from a land titling program in West Bengal, India to assess the impact of land titling on women's perceived tenure insecurity and land inheritance decisions. Exploiting the two-period panel data, we find that the inclusion of women's names on land titles is negatively associated with women's perceived tenure insecurity under certain conditions, and positively associated with women's participation in land inheritance decision-making. The paper implicitly supports continued efforts to empower women through land titling programs. However, we conclude that further efforts are necessary to fully secure women's land rights in order to bestow the economic and livelihood benefits that gender equality can have for women, their families, and their communities.

3.2 Background

3.2.1 West Bengal Land Titling Program

West Bengal is the fourth most populous state, of 29, in India, home to 7.5 percent of the country's population. It is also the second most densely populated state, at 1,028 persons per square kilometer. Moreover, while services account for two-thirds of the state economy, agriculture is still the leading occupation[21], placing further importance – and pressure – on land. Land scarcity and landlessness, the best predictor of poverty in India (World Bank, 1997), are

[21] https://www.ibef.org/

among the most prominent issues facing the state government and its constituents in West Bengal.

Land reform programs have a long history in West Bengal. Since independence, the government has adopted several tools including tenancy reform, ceiling laws, and land allocation and registration (Hanstad et al. 2008). Of note, in 1975 the state enacted the West Bengal Acquisition of Homestead Land for Agricultural Labourers, Artisans and Fisherman Act, which acquired and distributed land to rural households who were considered to be among the poorest.

With the persistence of landlessness and land inequality, however, more recent land ownership programs in West Bengal have been created to enable the most marginalized households to meet their basic needs by providing land and/or land title for homestead plots. For poor people in the rural areas, whose livelihoods largely depend on land, that land can provide their household with a site to build their home, space to grow sustenance food crops, and possibly construct an animal shelter (see, for example, Figure A3.1 in the Appendix). For these families, land can also be instrumental in securing government and financial services. In the long run, the programs are designed to promote increased investment in land, diversification of livelihood strategies, and economic resiliency where households are less vulnerable to economic and environmental disruptions.

Beginning in 2006, the Cultivation and Dwelling Plot Allocation (CDPA) Program was implemented by the Department of Land and Land Reform providing homestead land allocation and formal homestead titling. Under the CDPA program, the government purchased micro-plots of land (ranging from 0.04 to 0.16 acres) to be allocated to homesteadless families. In some areas, formal land titles were also distributed to those who occupied but did not have formal titles to their homestead plots on which they already reside. Following the 2011 elections and the

transition of political power to the All India Trinamool Congress (AITC) party, the program was slightly amended and renamed Nijo Griha, Nijo Bhumi (NGNB). Facing increasing land scarcity under the NGNB program, allocated plot sizes were generally reduced to a standard five decimals (0.05 acres). More recently, the government discontinued allocating new parcels of land to families though *regularization*, or formalization through land titling, is ongoing.

While many other states and countries have undertaken land reforms, the CDPA (and later NGNB) program is remarkable in its scope of distributing homestead micro-plots and titles. As of 2015, when the most recent wave of panel data was collected, the programs had collectively allocated new land and distributed land titles to approximately 62,000 households. In addition, more than 140,000 households received land titles for homestead plots that had not been formally registered.

Women's land titling

Although the West Bengal government had made tremendous efforts in redistributive land reforms, women's land rights were not explicitly addressed until the 1990s, and even then, not adequately so. In 1992, the government started to require government-allocated agricultural land to be granted either to a woman individually or jointly to husband and wife, but this policy did not extend to homestead land. Under both the CDPA and NGNB programs, land documents were intended to be issued in the woman's name for female headed households or jointly for married couples. The government worked with an international NGO, Landesa, to help identify female-headed households and prioritize their receipt of land and title. Government officials were also trained on the importance of women's land rights and formal documents were amended to include two spaces in the *pattas*, or formal land titles, to designate both the husband and wife as the land owners for married couples. Still, whether due to oversight, willful ignorance, or resistance, not all women were included. Of the households surveyed in 2015, only two-thirds of

land titles included women's names. In this paper, we exploit this quasi-experimental programmatic outcome to explore the differences in women's perceived land tenure insecurity and land inheritance decision-making for those households where women's names are and are not included in the land title.

3.2.2 Hypotheses

Agarwal (1997) argues that a rural person's bargaining strength within the family depends on private ownership and control over assets, as well as access to employment and other income generating activities; access to communal resources; access to traditional external social support systems; and access to support from the government or NGOs. Doss (2013) also suggests that assets, as well as education and income, are important aspects of women's bargaining power in developing countries. There is, however, mixed empirical evidence for these claims on the relationship between assets, namely land, and women's intra-household decision-making power.

Using Nepal Demographic and Health Survey data, Allendorf (2007) finds that women who own land are significantly more likely to have the final say on their own health care, large household purchases, household purchases for daily needs, and visits to family, friends and relatives. Another study from Nepal finds that land ownership has a positive and significant impact on women's decision-making power measured by household decisions of own healthcare, major household purchases, and visits to family or relatives (Mishra and Sam 2016). Assessing the impacts of joint titling in Peru, Wiig (2013) utilizes random variation in the introduction of the titling reform program as a natural experiment. He finds significant positive effects of joint titling on an aggregate empowerment index constructed from household decision-making variables. Ali et al. (2014) find that in Rwanda the joint land titling program is associated with more gender equitable land inheritance.

However, not all studies have reported consistently significant and positive findings. Swaminathan et al. evaluate the impact of house or land ownership on intra-household decision-making using data from the Karnataka (India) Household Asset Survey, 2010-2011.They find that property ownership improves women's decision-making power and mobility (Swaminathan et al. 2012a), but does not affect the egalitarianism of decision-making between couples (Swaminathan et al. 2012b). Doss et al. (2014) examine the relationship of women's individual and joint property ownership and the level of their household decision-making with data from four countries. While they find that women with individual land ownership have greater input into household decision-making than women with joint land ownership, both have more input than women who are not landowners in Mali, Malawi, and Tanzania. They do not, however, find such impacts in Orrisa, India. Behrman (2017) evaluates the hypothesis that women's land ownership is positively associated with women's decision-making power in multiple domains in the household using data from the 2010 Malawi Demographic Health Survey. She finds that women's sole ownership of land is positively associated with women's participation in both financial and reproductive decision-making. While women's joint land ownership is positively associated with their participation in household decision-making in the financial sphere, it is found to be negatively associated with participation in the reproductive health.

Building from these theoretical frameworks and previous studies, we aim to contribute to the empirical body of literature by exploring the relationship between women's land titling and women's empowerment in the context of West Bengal, India through the operationalization of two variables around women's strengthened bargaining power:

H1: Women with land titles will be less likely to feel tenure insecure

H2: Women with land titles will be more likely to feel that they have influence over

land inheritance decisions

Hypothesis 1 rests on the assumption that perceptions of tenure security are an important mediating factor in women's household bargaining. Due to the limitations of our data, however, we can only observe this first-order effect, leaving additional analyses for future study.

3.3 Data and Empirical Strategies

3.3.1 Data

In 2010–2011, international land rights NGO Landesa conducted an initial survey of 1,373 households across three districts of West Bengal: Japaiguri, Bankura, and Cooch Behar. All households had been previously identified by government officials as homesteadless and therefore potential program beneficiaries. The sampling for West Bengal is based on a rolling baseline as families become CDPA beneficiaries. When interviewed in 2010/11, 803 of those households were beneficiaries; around 64 percent of which included the woman's name. The remaining 570 had not yet received their land title through the CDPA/NGNB program.

Enumerators aimed to visit the same households[22] between October and November of 2012 for a second round of interviews. Different from the baseline survey, where "heads of household" were interviewed producing a predominantly male pool of survey respondents, adult women were intentionally interviewed, with a modified questionnaire. In this round, 779 households had received title, 75 percent of which included the women's name. Between June and August of 2015, a third round of interviews was conducted, again aiming to return to the same households[23] and interview adult women, which produced a sample of 787 titled

[22] Since some of households were not able to be reached, 338 households were replaced.
[23] Again, a replacement strategy was employed, contributing 435 new households.

beneficiaries and 592 untitled households. Women's names were included in 56 percent of the beneficiary households.

Due to the survey design, our research only exploits the latter two rounds of data collection when adult women were interviewed. From these two time periods, we were able to match 875 households, after accounting for replacement strategies employed in both the second and third rounds of the survey.[24] The potential reasons for attrition include migration, absence, and incapability of answering questions. Consider the attrition rate between two surveys is high, I conduct t-tests in demographic characteristics between households in the full sample of 2012 survey data and those who are able to be matched with the 2015 survey data. The comparison results are reported in Table A3.1. It indicates that the attrition rate is highest in Cooch Behar among three districts; this is consistent with Santos et al. (2013). They suggest that out-migration is common is in Cooch Behar. In addition, the average number of owned land of the full sample is slightly smaller than that of the matched sample. If migration is the main reason of attrition, the results imply that households with larger number of owned land may have stronger ties to land. While the selection into the matched sample could be endogenous, we find that the respondents' characteristics are similar across two samples.

Relevant to our study, the survey questionnaire included information on (1) demographics; (2) plot details including land access and ownership, types of land documents, and perceptions of land tenure security; and (3) household decision-making. The majority of decision-making questions included in the 2012 and 2015 surveys were in regards with agriculture. A minority of our sample uses its homestead land for any agricultural purposes. Land inheritance was the single variable available in both surveys that was relevant to our sample.

[24] We further exclude 65 households that do not report any land as their "homestead" land in either round.

The CDPA and NGNB programs include both allocation and regularization components, though more beneficiaries received new, or allocated, land under CDPA, and most NGNB beneficiaries received title for existing lands through the regularization scheme. The survey data does not allow us to differentiate the program through which a beneficiary received title, but we do explore potential differences between allocated beneficiaries and regularized beneficiaries, and find few significant differences.[25]

Under the land allocation program, beneficiaries received a new piece of land with the expectation that they would move to the new land, but moving is not ubiquitous. Local government officials can be slow to implement the program, preventing allocated beneficiaries from being able to move to the new land. Additionally, not all allocated beneficiaries choose to move to their new plot, due, for example, to lack of resources; lack of infrastructure; poor quality of land; or distance from existing community, as reported by survey respondents.

Given these nuances, for our primary analysis we define the treatment group as those households who are in possession of a titled plot that is currently used as a homestead, and the remaining as the control group. (As a robustness measure, we explore variations in how the treatment group is operationalized in section 4.2.) Since we are interested in the effects of women's titling, we further divide the treatment group into two subgroups by the inclusion or exclusion of women's names on the homestead land title. Despite the program requiring all titles to be issued in the woman's name only for female headed households, or jointly for married couples, from the 2015 survey data, approximately one-third of land titles issued do not include the woman's name. Our main independent variable is therefore a categorical variable equal to 1

[25] See Tables A3.2 and A3.3 in the Appendix. The only significant difference we detect is the effect on the inheritance decision of any land.

if only the man's name is included on the land title; 2 if the woman's name is included on the title; and 0 if the household does not possess a formal land title for the homestead plot.

The independent variable depends on two important factors that could arguably be endogenously driven: the selection of beneficiaries and the inclusion of a woman's name on the homestead title. The former can be attributed to the progress of the program, the characteristics of a household, and some unknown factors that affect the selection of beneficiaries. For the inclusion of women's names, even though personal and household preferences and social norms likely play a role, in general it appears that the local government officials decide whether to comply with the program guidelines. As self-reported by female beneficiary respondents in 2015, when asked who decided to have her name included, 85 percent credited the government official for driving the decision. While these observations do not rule out endogenous influences, we argue that the three groups are largely exogenously driven.

Table 3.1 summarizes demographic information from the 2015 survey organized by the status of no title; title in the man's name only; and title that includes the woman's name. All respondents are eligible for the program, so we would expect the demographic characteristics of respondents among these three groups to be similar. As Table 3.1 reports, the three groups are indeed fairly similar.

Table 3.1. Demographic Characteristics of Household and Adult Female Respondent

	No title	Man's name only	Woman's name included
Number of household plots owned[26]	1.30	1.16*	1.15*
Owns large animals[27]	0.18	0.21	0.15
Currently married	0.90	0.91	0.85
Resides with adult sons[28]	0.42	0.36	0.41
SHG member	0.18	0.16	0.20
District			
Bankura	0.36	0.33	0.29
Jalpaiguri	0.30	0.28	0.31
Cooch Behar	0.34	0.39	0.40
Number of observations	493	122	195

One asterisk indicates significance at the 5% level. The base category is households with no homestead land title.

3.3.2 Empirical Approach

As discussed in the previous section, the composition of treatment and controls groups is unlikely but still possible to be endogenously driven. Some unobserved characteristics of the respondent, household, geographic conditions, or the attitude of local government officials could affect the composition of the treatment and control groups and influence the outcome variables, biasing the estimated results.

To mitigate for endogenous factors, we use panel data with individual fixed effects to estimate the impact of the land titling programs. Explicitly, we estimate the following equation

$$`y_{it} = \alpha_i + \gamma_t + \beta_1 * land\ title_{it} + \beta_2 X_{it} + \epsilon_{it} \quad (3.1)$$

where y_{it} is the outcome for respondent i in period t; α_i captures individual-specific unobserved

[26] This variable reports the number of plots "owned" as reported by respondents. Ownership is defined as plots that are recognized by the community as belonging to the household, though they are not necessarily formally titled and legally recognized by the government.
[27] Large animals include buffalos, oxen, bullock, cows, horses, mules, and donkeys.
[28] Age of 15 or above

effects; γ_t is a time dummy variable, $land\ title_{it}$ is a categorical variable which equals zero if the household is a non-beneficiary, one if the household is benefited from the program but the woman's name is not included, and two if the respondent's name is included on the land title; X_{it} controls for demographic characteristics including respondents' marital status, whether the respondent resides with her sons, whether the respondent is a member of self-help group (SHG), and whether the household owns large animals; ϵ_{it} is the error term.

We analyze the equation using a linear probability model (LPM) with individual fixed effects. Although we can estimate a conditional logit model with individual fixed effects to eliminate unobserved heterogeneity, which would drop a large part of sample and need to plug into a value for the unobserved effects to compute partial effects. A linear probability model is attractive with regard to be able to handle unobserved heterogeneity if explanatory variables are binary (Wooldridge 2002). In our model, most of explanatory variables are discrete; hence fixed effects estimation of the LPM model should be able to provide reasonable estimates of average partial effects.

3.4 Results

3.4.1 Primary Results

We hypothesize that land titling decreases women's perceived land tenure insecurity and increases her decision-making with regards to land inheritance. We summarize the outcome variables in Table 3.2. All descriptive statistics are disaggregated by the status of homestead land title – no title (the base group), title under a man's name, and title under a woman's name only or joint with the husband. In the survey, we asked a series of questions about women's perceived tenure insecurity around their homestead land. Respondents were asked to consider the

likelihood of losing or having less access to their homestead land in five years. Respondents were also asked to consider the likelihood of losing access to their homestead land under several hypothetical situations including respondents having a disagreement with their households or becoming widowed. The reported frequencies across all three measures of tenure insecurity initially suggest that respondents are less tenure insecure if their households possess a legal title for their homestead land. Three years later, compared with the base group, the data suggest that this is only the case for the first measure of insecurity, that respondents are less likely to lose access to the homestead land in five years if their names are included on the land titles.

In addition to women's perceived tenure insecurity, we analyze women's perceived participation in land inheritance decisions. From the 2012 survey data, around 80 percent of female respondents whose names are included on the land title report that they are able to participate in the land inheritance decision, which is much higher than that of the other two groups. In 2015, while an overall high proportion of respondents self-report being able to participate in the decision of who will inherit land, there is a noticeable decrease in decision-making power for women whose households have become more secure, but they are excluded from the land title. In other words, formally securing men's assets while not simultaneously securing women's assets may have negative effects on women's overall security and bargaining power and further increase gender inequality.

Table 3.2. Descriptive Statistics of Outcome Variables

	2012			2015		
	No title	Man's name only	Woman's name included	No title	Man's name only	Woman's name included
Perception of tenure insecurity:						
Likely to lose or have less access to homestead land in 5 years	17%	3%*	0%*	20%	12%	5%*
Likely to lose access to homestead land if						
Disagreement with household member	30%	5%*	8%*	15%	11%	11%
Become widowed	28%	8%*	5%*	18%	19%	13%
Inheritance:						
Any land Inheritance	50%	54%	81%*	93%	82%*	96%
Homestead land inheritance	41%	54%*	81%*	89%	80%*	96%

One asterisk indicates significance at the 5% level. The reference group is households with no titles for homestead land.

To explore these initial observations further, we conduct a linear probability model with fixed effects to examine the effect of the land titling program. The estimated results of women's perceived tenure insecurity are presented in Table 3.3. In Column 1, we report the impact of the program on the expectation of land access in five years. The results show support for our first hypothesis that respondents are less likely to lose some or all access to the homestead land if their names are included on the land titles. The estimated effects on perceived tenure insecurity under the other two hypothetical situations -- if there is a disagreement with household members and if the husband dies -- are displayed in Column 2 and Column 3, respectively. We do not find significant effects across these two measures. However, the estimated effects suggest that respondents whose homestead land is formally titled under their husbands are less likely to feel

tenure insecure if they have disagreements with other family members. While not consistent with our expectations, one explanation may be that these results reflect women's perceptions of tenure security in relation to their in-laws.

We also find, as expected, that an increase in the number of plots that a household perceives as its own (regardless of formal title) is associated with the reduction of women's tenure insecurity across all three measures. We consider the number of plots of land as a proxy for the households' assets or wealth, hence our results suggest that an increase in the households' wealth provides women with greater tenure security.

We observe an interesting dynamic with regards to changes over time. The coefficients of the dummy variable for the year 2015 are significant across all three models, but the estimated effects move in opposite directions for the different measures of tenure insecurity. The positive coefficient for the expectation of losing access to the homestead land indicates that respondents, in general, increasingly expect to lose some or all access to their homestead land over time. This is likely a reflection of the increased population density and increased scarcity of land available in West Bengal. The negative coefficients in regards to household member dynamics or status suggest that in general women are increasingly feeling more tenure secure within their household regardless of individual or household titling status. This may be a reflection of the program, which has aimed to increase women's status and equality through land titling and, in some areas where government officials are proactive, awareness raising of women's land rights. It could also be a reflection of the broader environment in which women's empowerment and equality is espoused by some development and government institutions and officials.

Table 3.3. Titling Effects on Women's Perceived Tenure Insecurity

	(1) Access in 5 years	(2) Disagreement with household members	(3) Become widowed[29]
Titling			
Man's name only	-0.0775*	-0.1244**	-0.1070
	(0.0444)	(0.0555)	(0.0661)
Woman's name included	-0.1574***	-0.0409	-0.0562
	(0.0359)	(0.0424)	(0.0440)
Number of household plots	-0.0818***	-0.0613**	-0.0606**
	(0.0246)	(0.0266)	(0.0299)
Owns large animals	-0.0049	-0.0020	-0.0099
	(0.0360)	(0.0386)	(0.0425)
Married	0.0289	-0.0529	
	(0.0427)	(0.0666)	
Resides with adult sons	0.0135	-0.0282	-0.0115
	(0.0379)	(0.0422)	(0.0488)
SHG member	0.0109	-0.0408	0.0084
	(0.0328)	(0.0419)	(0.0430)
2015 dummy	0.0389**	-0.1034***	-0.0499**
	(0.0190)	(0.0213)	(0.0242)
Constant	0.2317***	0.4106***	0.3369***
	(0.0509)	(0.0704)	(0.0413)
Number of households	810	801	664

Robust standard errors are reported in parentheses. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

Table 3.4 reports findings for the second hypothesis regarding land inheritance decisions. The relationship between the inheritance decision for the titled land and the titling status is very straightforward. However, if it is the case that women have a say on inheritance decision for other land but not the homestead land, our results may overestimate the effect of land titling on women's decision-making. Thus we consider one more dependent variable – being involved in land inheritance decisions for at least one plot of land.

The results support our hypothesis that including women's names on land titles is positively associated with their decision-making power over land inheritance decisions, both for the

[29] We only include married women in this model.

homestead plot and any plots of land the household considers their own (though only significant at the 10% level). While the direct effect, decision-making over the titled homestead plot, is larger than the effect on any of unspecific plots of land, the findings are consistent with the seminal theoretical work and subsequent empirical findings that increased access to and control over assets will increase women's household decision-making, or bargaining power, more broadly (Lundberg and Pollak 1993; Allendorf 2007; Garikipati 2009; Swaminathan et al. 2012a; Wiig 2013; Ali et al. 2014; Mishra and Sam 2016). The results suggest that overall women are increasingly more likely to participate in the land inheritance decision, indicated by the positive and significant coefficients for the 2015 dummy variable.

While we do not find any significant effect of other explanatory variables in Column (1), there are some other factors may also have impacts on land inheritance decisions. Increased wealth, operationalized by the number of household plots, increases women's perceived decision-making power over household lands. Whether the respondent is a self-help group (SHG) member also has a positive effect. SHG members may have better connections to others in the same community, so they may be more aware of their rights. Another reason could be that SHG members may have better access to credit, and hence have more bargaining power in household decisions.

Table 3.4. Titling Effects on Women's Perceived Land Inheritance Decision-making

	(1) Homestead land	(2) Any land
Titling		
Man's name only	0.0922	-0.0536
	(0.0721)	(0.0624)
Woman's name included	0.2994***	0.0899*
	(0.0600)	(0.0460)
Number of household plots	0.0616	0.1230***
	(0.0498)	(0.0317)
Owns large animals	-0.0171	-0.0075
	(0.0629)	(0.0438)
Married	-0.0284	-0.0105
	(0.0803)	(0.0634)
Reside with adult sons	-0.0226	0.0139
	(0.0643)	(0.0530)
SHG member	0.0831	0.1110***
	(0.0573)	(0.0409)
2015 dummy	0.3502***	0.3156***
	(0.0320)	(0.0233)
Constant	0.3711***	0.4216***
	(0.1105)	(0.0763)
Number of households	465	674

Robust Standard errors are reported in parentheses. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

3.4.2 Alternative Analysis

Our estimation strategy may be biased under the following situations. First, we define treatment as those who have a formal land title, which does not take into consideration the effects of informal titles, such as the dolil, or deed which is commonly used in West Bengal. Under group B below, we relax these assumptions to include informal documents. Second, our assumptions in the primary analysis implicitly assume that respondents are not affected until they are in physical possession of the formal land title. Under groups C and D below, we consider that respondents may be cognitively affected in just learning about the program and anticipating their receipt of the patta through the government program. Since we do not know whether the anticipated

beneficiaries assume the title would include their name or not, we explore both possibilities. Third, our main explanatory variable is operationalized into three categories, distinguishing both titled and untitled households and titled and untitled female respondents. It is possible that we are underestimating gender effects, and so we reconfigure our analysis in group E below to be a binary measure around women's homestead land titling. Finally, in the primary analysis we consider land titling for the homestead plot only. Households which use the allocated land for other purposes or leave the land to be vacant are currently classified as part of the control group. For the model that includes decision of inheritance around "any plot," the estimated coefficient may therefore be conservatively low.

Table 3.5. Definitions of Comparison Groups

Group Summary	Base category	Man's name only	Woman's name only or joint title
A: Basic model	Households that do not have a formal land title (*patta* or *Khatiyan*) for the homestead land	Households have a formal title for the homestead land under the man's name	Households have a formal title for the homestead land, and the respondent's name is included on the title
B: Relax formal documents	Households that do not have a formal or informal (e.g. *dolil*) land title for the homestead land	Households have a formal or informal land title for the homestead land under the man's name	Households have a formal or informal land title for the homestead land, and the respondent's names is included on the title
C: Expect a formal document from government (assumes man's name only will be included)	Households that do not have a formal title for the homestead land nor do they expect to receive a formal title from the government	Households that have a formal for the homestead land under the man's name or households that expect to receive a formal title from the government	Households that have a formal title for the homestead land, and the respondent's names is included on the title
D: Expect a formal document from government (assumes woman's name will be included)	Households that do not have a formal title for the homestead land nor do they expect to receive a formal title from the government	Households that have a formal title for the homestead land under the man's name	Households that have a formal title for the homestead land, or households that expect to receive a formal title from the government and assume the respondent's name will be included
E: Binary comparison of women's homestead titling	Households where the respondent's name is not included on the land title regardless of possession of a formal title by the household		Households that have a formal title for the homestead land, and the respondent's name is included on the title
F: Any land title	Households that do not hold any land titles	Households hold land titles, but the respondent's name is not included on any of them	Households hold land titles, and the respondent's name is included on at least one formal land title

Using these various configurations, the estimated results of perceived tenure insecurity are shown in Table 3.6. The findings are generally consistent with the primary results reported in Column A, in that the inclusion of a woman's name reduces her perceived tenure insecurity around future access to land. Similar, too, are the findings that having a homestead title for the household decreases her insecurity if there is a disagreement with other household members, but has no effect by having her name, specifically, included. Departing from the previous results, we do observe one scenario that suggests having a household title under the husband's name will decrease her tenure insecurity if she were to become widowed.

Results for inheritance decisions are reported in Table 3.7. The findings are consistent with the primary analysis around the homestead land, with one exception. When we make the assumption that all households expecting a title from the government will include the woman's name, the findings do not hold. This is not surprising given that it is a generous assumption. The otherwise consistent results across the different operationalization of groups suggest that the relationship between women's homestead titling and homestead land inheritance decision-making is robust. Inheritance decisions for any plot of land are more nuanced. We find negative impacts of the man's name only on a land title if we move households which expect to receive a formal document to either man's name only group or woman's name only or joint title group. It, again, implies that formally securing men's assets without securing women's assets may have negative effects on the land inheritance decisions.

Table 3.6. Titling Effects on Women's Perceived Tenure Insecurity with Different Comparison Groups

	Comparison groups					
	A	B	C	D	E	F
Panel A: Access in 5 years						
Man's name only	-0.0775*	-0.0750**	-0.0332	-0.0731		-0.1171**
	(0.0444)	(0.0362)	(0.0339)	(0.0486)		(0.0486)
Woman's name included	-0.1574***	-0.1570***	-0.1524***	-0.0773***	-0.1417***	-0.2059***
	(0.0359)	(0.0344)	(0.0358)	(0.0293)	(0.0382)	(0.0452)
Panel B: Disagreement with household members						
Man's name only	-0.1244**	-0.1462***	-0.0758**	-0.1380**		-0.1060*
	(0.0555)	(0.0427)	(0.0374)	(0.0543)		(0.0548)
Woman's name included	-0.0409	-0.0401	-0.0403	-0.0474	-0.0160	-0.0201
	(0.0424)	(0.0416)	(0.0422)	(0.0325)	(0.0426)	(0.0507)
Panel C: Become widowed						
Man's name only	-0.1070	-0.1074**	-0.0184	-0.0973		-0.1001
	(0.0661)	(0.0520)	(0.0435)	(0.0601)		(0.0615)
Woman's name included	-0.0562	-0.0661	-0.0414	-0.0091	-0.0353	-0.0470
	(0.0440)	(0.0427)	(0.0449)	(0.0370)	(0.0503)	(0.0572)

Robust Standard errors are reported in parentheses. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

Table 3.7. Titling Effects on Women's Perceived Land Inheritance Decision-making with Different Comparison Groups

	Comparison groups					
	A	B	C	D	E	F
Panel A: Homestead land						
Man's name only	0.0922	0.0976	-0.0737	0.0036		0.1099
	(0.0721)	(0.0619)	(0.0550)	(0.0780)		(0.0746)
Woman's name included	0.2994***	0.2955***	0.2340***	0.0459	0.2701***	0.1831**
	(0.0600)	(0.0613)	(0.0632)	(0.0533)	(0.0570)	(0.0738)
Panel B: Any land						
Man's name only	-0.0536	-0.0607	-0.1650***	-0.1201*		0.1308**
	(0.0624)	(0.0482)	(0.0415)	(0.0620)		(0.0591)
Woman's name included	0.0899*	0.0835*	0.0593	-0.0621*	0.1071**	0.3132***
	(0.0460)	(0.0428)	(0.0435)	(0.0360)	(0.0433)	(0.0558)

Robust Standard errors are reported in parentheses. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

3.4.3 Women's Work Status

Access to employment or income generating activities is also seen as a factor to affect a woman's bargaining power within the family (Agarwal 1997; Anderson and Eswaran 2009; Doss 2013). Some researchers consider access to employment or ability to decide whether to be employed as an indication of women's empowerment (Kabeer 1995; Malhotra and Schuler 2005; Swaminathan et al. 2012b). From the survey data, we only observe whether a woman works or not. Those who do not work may have access to employment but choose not to work, and those who work may be forced to work because they need to contribute income to the family. It is therefore inaccurate to assume that working women are more empowered, but we are still interested in the employment status of women. Employed women do not necessarily have control over their income, but it can worsens a woman's position and diminish her contribution to the family if she loses access to employment.

To examine whether the land titling program affects women's employment status, we use whether a woman works for pay in the last three months as the dependent variable. The estimated coefficients are reported in Table 3.8. The results suggest that the land title status has an impact on respondents' employment status. We find a negative effect on women's employment status if only the man's name is included. It leads to a decrease in the likelihood of being employed by 14.65 percentage points while the inclusion of a woman's name on the title has no impact on employment status. One interpretation is that households in this group may be less deprived; therefore women in this category do not need work and contribute income to their families. Unfortunately, income data is not available in the 2015 survey. We try to control for wealth using the number of owned land and ownership of large-size animals as proxies, but we

did not find any significant effects of these variables. We cannot rule out the possibility that women in this group are less empowered and not able to choose to work.

Table 3.8. Titling Effects on Women's Employment

	Mean (standard deviation)	Program effect
Titling		
No titles (base category)	0.57	--
	(0.50)	--
Man's name only	0.43	-0.1465**
	(0.50)	(0.0623)
Woman's name included	0.54	-0.0753
	(0.50)	(0.0533)

Robust standard errors in parentheses. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

3.5 Discussion

One important goal of the land tilting program is to strengthen land tenure as well-defined land rights lower the risk of eviction or expropriation, increase incentives for land-related investment (Besley 1995). Consider land rights at the individual level, joint land titling usually serves as a tool to promote gender equality. However, the literature on whether formal titling improves or worsens women's land access is inclusive (Ali et al. 2014; Whitehead and Tsikata 2003). The inclusion of women's names may not be sufficient to ensure women's land rights. In addition to the land rights, women's land tenure security should also be taken into consideration. Although indicators of women's tenure security have not yet achieved a consensus, assessing the available measures, we aim to answer the question whether formal land tilting provides better conditions for women's empowerment.

The estimated results in Table 3.3 imply that ensuring the husband has title to the land is the important aspect of protecting the husband and wife from other people in the household. His land titling is more important than joint tilting or her titling under disagreements. On the other hand,

the survey data show that some respondents do not feel the need to include their names on land titles because the documents are under their husbands' names. The wife may believe that the husband's titling is sufficient for her and overlook the case of disagreements with her husband in which the husband has title cannot secure her land rights.

Economic theories suggest that ownership and control over assets increase a person's bargaining strength within the family. Joint titling programs that intend to ensure women's land rights should bring positive influences on their empowerment. While we find positive impacts of the land titling program on land inheritance decisions, the results about land tenure security are less conclusive. Suppose that in general women feel secure even without land titles, then we will find limited effects on women's tenure insecurity measures. Joint titling may enhance women's land inheritance decision through other channels rather than further securing women's land rights. For example, beneficiary women may have better literacy about their land rights and notice that it is legitimate to participate in land inheritance decisions, especially for the homestead land that their names are included on the titles.

Overall, our results suggest that land titling has a positive impact on women's tenure security in the perspectives of durability; nevertheless, their land rights may still be vulnerable when they experience the shocks or disagreements in their families. Also, given the short elapsed time since the introduction of the program, our study verifies the short-term impacts of the program while long-term effects could be either positive or negative. Empowering women has been proven to improve the wellbeing of the whole family, for future studies, we would be interested in exploring the effects on food security and the household vulnerability to provide more information to policy makers and advocates of land reforms.

3.6 Conclusion

Joint titling has been enthusiastically promoted as a gender equalizing policy, and is among one of the targets of the Sustainable Development Goals to undertake reforms to give women equal rights to economic assets including, specifically, land. In an effort to reduce poverty and inequality, the West Bengal government has conducted several land reforms. More recently, efforts have also been made to decrease gender inequality, specifically through women's land titling. The current land titling program requires that titles be issued with a woman's name solely or jointly with the husband if married. We examine the effects of the land titling program on women's tenure insecurity and the decision-making power in inheritance decisions. We find consistent evidence that land titling strengthens women's perceived participation in the decisions of land inheritance, but mixed evidence that the inclusion of women's names on land titles decreases perceptions of their own tenure insecurity.

Although women may perceive that it is sufficient to secure the household's land ownership, our results suggest that formally securing men's assets while not simultaneously securing women's assets may have negative effects on women's overall security and bargaining power and further increase gender inequality. Our results explain the rationale to promote joint titling when considering implementing formal titling.

Appendix 3

Figure A3.1. Sample homestead plot use

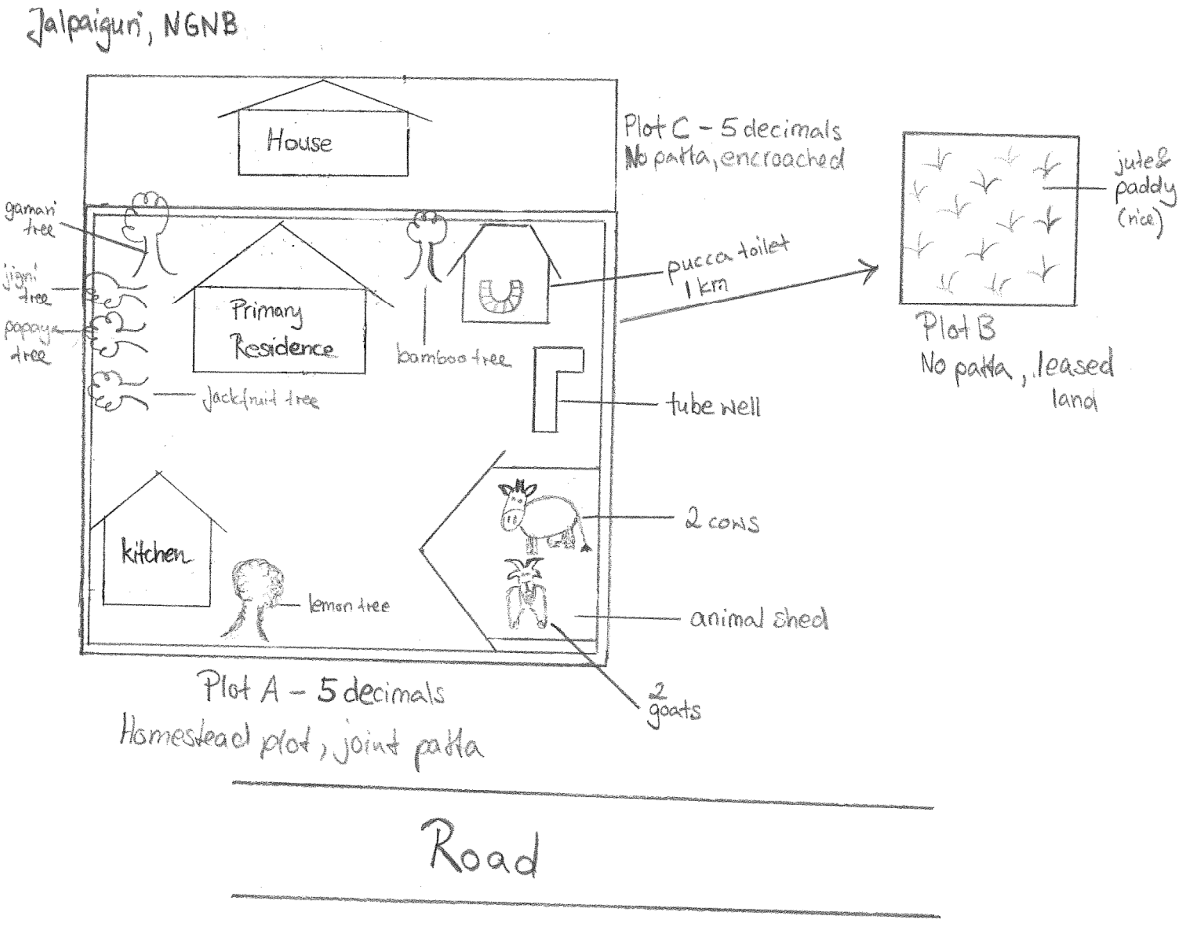

Table A3.1. T-tests for Differences in Means in 2012 Survey for Full Sample and Matched Sample

	Full sample mean	Matched sample mean	Significance
Number of household plots	1.15	1.20	***
Owns large animals	0.16	0.17	
Married	0.84	0.85	
Resides with adult sons	0.34	0.34	
SHG member	0.18	0.19	
District			
Bankura	0.32	0.36	**
Jalpaiguri	0.27	0.28	
Cooch Behar	0.41	0.36	**
Number of observations	1373	875	

One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

Table A3.2. Program Effects on Women's Perceived Tenure Insecurity

	(1) Access in 5 years	(2) Disagreement with household members	(3) Become widowed
Program			
Allocation	-0.1162**	-0.0489	-0.0405
	(0.0463)	(0.0517)	(0.0567)
Regularization	-0.1323***	-0.0140	0.0084
	(0.0454)	(0.0595)	(0.0729)
Number of HH plots	-0.0818***	-0.0585**	-0.0567*
	(0.0248)	(0.0268)	(0.0300)
Owns large animals	-0.0097	-0.0040	-0.0119
	(0.0360)	(0.0386)	(0.0424)
Married	0.0211	-0.0465	
	(0.0433)	(0.0669)	
Resides with adult sons	0.0125	-0.0221	-0.0044
	(0.0377)	(0.0424)	(0.0490)
SHG member	0.0099	-0.0415	0.0063
	(0.0331)	(0.0422)	(0.0432)
2015 dummy	0.0434**	-0.1117***	-0.0619**
	(0.0204)	(0.0227)	(0.0261)
Constant	0.2332***	0.3944***	0.3224***
	(0.0511)	(0.0706)	(0.0413)
Number of households	810	801	664

Robust standard errors are reported in parentheses. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

Table A3.3. Program Effects on Women's Perceived Land Inheritance Decision-making

	(1) Homestead land	(2) Any land
Titling		
Allocation	0.2307***	-0.0241
	(0.0697)	(0.0532)
Regularization	0.2470***	0.1619**
	(0.0783)	(0.0758)
Number of household plots	0.0568	0.1178***
	(0.0498)	(0.0316)
Owns large animals	-0.0124	-0.0026
	(0.0645)	(0.0439)
Married	-0.0225	0.0088
	(0.0831)	(0.0629)
Resides with adult sons	-0.0219	0.0171
	(0.0636)	(0.0529)
SHG member	0.0889	0.1052***
	(0.0574)	(0.0406)
2015 dummy	0.3302***	0.3125***
	(0.0345)	(0.0249)
Constant	0.3791***	0.4163***
	(0.1096)	(0.0751)
Number of households	465	674

Robust Standard errors are reported in parentheses. One asterisk indicates significance at the 10% level, two indicate the 5% level, and three indicate the 1% level.

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