THREE ESSAYS IN APPLIED MICROECONOMICS

AN ABSTRACT

SUBMITTED ON THE TENTH DAY OF DECEMBER

TO THE DEPARTMENT OF ECONOMICS

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS OF THE

SCHOOL OF LIBERAL ARTS

OF TULANE UNIVERSITY FOR THE

DEGREE

OF

DOCTOR OF PHILOSOPHY

BY

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Abstract

This dissertation contains three papers on applied microeconomics. The first chapter, *The Spillover Effects of Expanding Medicaid for Parents on Children's Preventive Health Services Use: Evidence from Louisiana,* estimates the effect of Medicaid expansion on well-child visits, dental exam visits and immunization visits for children aged 3 to 17. In this study, I use Louisiana Medicaid administrative data and difference-in-differences (DD) methodology. The results suggest that children whose parents gained coverage under the expansion are more likely to use preventive health services such as well-child visits, immunization visits and dental exam visits in the following year. I also find that these effects are larger for children under age 12 and for households with more children.

The second chapter, *The Impact of Parental Involvement in Abortion Laws on Women's Education, Future Income and Labor Force Participation,* focuses on four types of parental involvement laws in abortion and how these laws affect the women's long-term outcomes. I use a DD methodology to compare women who were exposed to the parental involvement laws with those women without exposing the laws between 15 and 17 years old. I find that parental involvement laws in abortion lowers the probability of completing high school and college, they also have the negative effects on labor outcomes and future income, with particularly profound effects for young black women.

The third chapter, *In Debt and Alone? How Student loans Shape Marriage and Childbearing in Young Adulthood* (co-authored with John H. Edwards), examine if student loan debt delays first marriage and first childbirth using data from the 1997 cohort of National Longitudinal Survey of Youth. we first use the hazard model to

evaluate whether young adults' education loan debt delays first marriage and having a first child. Considering the nonrandom selection of student loans, we also use the tuition of each state's flagship university and the distance to each state's flagship university for each respondent as the instrumental variables to evaluate the effect of student debt on the probability of first marriage and the probability of having a first child. We find that students who own student loan are less likely to have first marriage and have a first child. For the heterogeneous effects of the Hazard model on gender, the risk of transitioning to first marriage was lower for men with student loans than for women, while the risk of transitioning to first birth was lower for women with student loans than for men. We also find that the risk of transitioning to first marriage was lower for black and Hispanic than others.

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Chapter 1: The Spillover Effects of Expanding Medicaid for Parents on Children's Preventive Health Services Use: Evidence from Louisiana

Chapter 1: The Spillover Effects of Expanding Medicaid for Parents on Children's Preventive Health Services Use: Evidence from Louisiana

Cuicui Song†

Abstract: On July 1, 2016, Louisiana expanded Medicaid income eligibility limits for parents from 24% to 138% of the federal poverty level, resulting in substantial insurance coverage gains. Several studies have documented the effects of Medicaid enrollment on use of health care services by the direct beneficiaries, but far fewer have focused on spillovers to the children of those direct beneficiaries. I use a difference-in-differences (DD) regression analysis and Louisiana Medicaid administrative data to estimate the effect of Medicaid expansion on well-child visits, dental exam visits and immunization visits for children aged 3 to 17. My treatment group is comprised of children with Medicaid coverage whose parents became newly enrolled in Medicaid only after the expansion. The control group consists of children covered by Medicaid whose parents were consistently enrolled in Medicaid before and after the expansion. The results suggest that children whose parents gained coverage under the expansion are 2.20 percentage points (5.06%) more likely to have at least one well-child visit, 5.80 percentage points (8.41%) more likely to have an immunization visit and 0.30 percentage points (0.84%) more likely to have a dental exam in the following year. These effects are larger for children under age 12 and for households with more children.

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1 Introduction

Medicaid has traditionally provided health insurance coverage for low-income children, disabled people, and pregnant women, but many disadvantaged adults, particularly those with low incomes, remained uninsured. In response to the Affordable Care Act (ACA), many state governments expanded Medicaid health insurance coverage to some of these low-income adults starting in 2014. The Medicaid eligibility threshold for adults with children moved from a fraction of the federal poverty level (for example, Louisiana's eligibility threshold was 24% of the federal poverty level) to an upper income limit of 138% of the federal poverty level (FPL). Louisiana adopted this threshold on July 1, 2016. The expansion was intended to improve low-income adults' health outcomes by increasing their access to health care services and by providing them with financial protection from the high cost of illnesses. The expansion resulted in increased use of health care services by the children of parents who newly enrolled in Medicaid, and may have therefore resulted in improved children's health outcomes. These spillover effects could be categorized as positive externalities of the health insurance expansions.

Parental health insurance may affect children's health service use through various channels: Medicaid expansion can increase parents' access to healthcare and familiarity with the Medicaid system, making them more likely to take their children to see a doctor (Finkelstein et al., 2012; Deleire et al., 2013; Lipton & Decker, 2015), . Parents' exposure to health services would create familiarity with the Medicaid system (Gifford et al., 2005). When parents are enrolled in Medicaid, they will be more likely to know about the benefits of coverage and to understand that there is no copay for children's preventive service use. When parents engage with doctors about their own health, they enhance their knowledge about the importance of preventive health care. This may make them more likely to take their children to use preventive services like seeing a dentist or a pediatrician for regular check-ups. Medicaid expansion also improves low-income families' financial health by reducing medical bills (Finkelstein et al., 2012; Brevoort, 2017; Miller et al., 2019; Caswell and Waidmann, 2019). This income effect may allow parents to free up their working time or afford the cost of transportation for their children's health care use (Venkataramani et al., 2017). Also, the fixed costs of finding a doctor or of searching for information about the health care system can be used for both parent and child. Previous literature has found that participation in Medicaid increased with family size, providing supportive evidence that fixed costs play a role in the decision-making process (Currie, 2000). Lastly, the lower cost of seeing a doctor may lead parents to pay more attention to their own health. They may develop a habit of having annual check-ups or of using other preventive health services, and parents' habits may have positive externality effects on their children (Lipton, 2021).

Several studies have suggested that the Medicaid expansion has had positive effects on adults' Medicaid enrollment, access to health care, use of health care services and health outcomes (Finkelstein et al., 2012; Deleire et al., 2013; Lipton & Decker, 2015). Likewise, prior research has shown that the Medicaid expansion for adults has had positive spillover effects on children's coverage (Dubay and Kenney, 2003; Devoe et al., 2015; Hudson and Moriya, 2017). However, little is known about the spillover effects of the expansion of parental Medicaid coverage on children's health service use. Some related literature has suggested that insured children with uninsured parents have a lower probability of using health services than insured children with insured parents do (Davidoff et al., 2003; Gifford et al., 2005; Devoe et al., 2009), but these studies did not have rigorous identification strategies. Only two recent articles with rigorous identification strategies have examined the spillover effects of adult Medicaid coverage on low-income children's use of preventive services (Venkataramani et al., 2017) and dental health services (Lipton, 2021).

Venkataramani et al. (2017) used data from the 2001-2013 Medicaid Expenditure Panel Survey (MEPS) and instrument variable (IV) analysis to investigate the spillover effects of adult Medicaid expansions on children's use of preventive services. Lipton (2021) examined the effects of Medicaid adult dental coverage expansions on low-income children's receipt of dental health services using the 2000-2013 National Health Interview Survey (NHIS). Both studies used an eligibility simulation method that compared family-level income with state-specific Medicaid income eligibility thresholds to obtain the Medicaid enrollees' samples. This simulation method, however, did not identify parents who were actually enrolled in Medicaid. Thus, it might overestimate the treatment effect of parental Medicaid coverage status on children's health service use.

This paper complements and builds on existing literature by providing novel evidence of the causal link between parental Medicaid coverage and children's well-child visits[1], dental exam visits and immunization. It uses 2013-2018 administrative claims data for the Medicaid population in the state of Louisiana. The data provides information on demographic characteristics, Medicaid enrollment, and health service use for both adults and children. It allows me to identify parents who were actually enrolled in Medicaid. Another advantage of studying Louisiana's Medicaid expansion is that it resulted in one of the largest reductions in the uninsured rate among all expansion states, falling from 16.6 percent of the population in 2013 to below 8 percent in 2018 (United States Census Bureau, 2019). Lastly, the effects of Medicaid expansion on both parents' and children's health care utilization could provide evidence that is relevant to other poor states like Alabama and Mississippi, which have not yet expanded their Medicaid

[1] The well-child visit, also called a physical check-up, is a preventive measure for children's health. It is recommended that children receive multiple well-child visits before the age of 3, and once a year from ages 3 to 21. The schedule of well-child visits is listed in Table 1 of the Appendix.

coverage under the ACA.

I use a difference-in-differences approach to estimate the spillover effects of the expansion of adult Medicaid coverage on children's well-child visits, dental exam visits and immunization visit. The treatment group is comprised of children with Medicaid coverage whose parents became newly enrolled in Medicaid as a result of Medicaid expansion in Louisiana. The control group consists of children covered by Medicaid whose parents were continuously enrolled in Medicaid, both before and after (pre- and post-) expansion. Previous research showed that the impact of Medicaid coverage on children's preventive service use varied by race/ethnicity, family structure (single or not, family size) and age (Guendelman and Schwalbe, 1986; Flores et al., 1999; Gifford, 2005; Miller and Pylypchuk, 2014; Alexander et al., 2015). This paper also examines the heterogeneous effects by age, race/ethnicity and family structure.

The results suggest that parental Medicaid enrollment increased well-child visits by 2.20 percentage points (5.06%), dental exam visits by 0.30 percentage point (0.84%) and immunization visits by 5.8 percentage points (8.41). I also find that the treatment effects are concentrated among children under age 12 and households with 5 or more members. This study offers two primary policy implications: First, these spillover effects indicate that parental Medicaid enrollment is an important factor in improving children's receipt of the preventive healthcare services they need. It provides supporting evidence that states without Medicaid expansion need to focus on covering low-income families rather than just low-income children. Second, parents' use of preventive services has a great impact on children's use of preventive services. It might be necessary to implement some policies that encourage parents concerning their own health to reduce unmet preventive services among low-income children.

The structure of the paper is as follows: Section 2 discusses the background on

Medicaid expansion under the ACA and the related literature. Section 3 outlines the conceptual framework that justifies believing that enrolling parents in Medicaid might have effects on their children's preventive health care use. Section 4 describes the data sources and the sample selection used for this paper. Section 5 introduces the empirical strategy, and the results are discussed in section 6. Section 7 presents the results of the robustness check. Section 8 provides the cost-benefit analysis and my conclusions are presented in section 9.

2 Background and Literature Review

2.1 The Affordable Care Act Medicaid Expansion

The Patient Protection and Affordable Care Act — widely known as the Affordable Care Act (ACA) or Obamacare — was signed into law on March 23, 2010. The main purpose of the ACA is to provide affordable coverage options to uninsured people through Medicaid and the Health Insurance Marketplaces.

The ACA made a number of changes to Medicaid. The most widely discussed is the expansion of the Medicaid eligibility threshold to nonelderly adults. Figure A1 shows that Medicaid income eligibility levels for non-elderly adults vary across states. Most states with Medicaid expansion (32 states) took 138 percent federal poverty level (FPL) as the Medicaid eligibility threshold. To illustrate, in 2019, the threshold of 138 percent of FPL for families with 2 members was $23,791, for families with 3 members was $29,974, for those with 4 members was $36,156 and for families with 5 members was $42,338.

Following a 2012 Supreme Court ruling, states could freely decide whether to adopt the Medicaid expansion or not. As of December 2019, 34 states (including the District of

Columbia) had adopted the Medicaid expansion and 17 states had not adopted it. Most states' Medicaid expansion took effective in 2014 (see Table A1).

As of July 2018, Medicaid enrollment had increased by a total of 15.6 million among the 49[2] states. This represents a 27.5 percent increase over the baseline (July-September 2013). The growth of Medicaid enrollment in non-expansion states was lower than in Medicaid expansion states. Enrollment in the Medicaid expansion states increased by 13.6 million, or 35.9 percent, while in the non-expansion states, enrollment increased by 1.9 million, or 10.2 percent (Centers for Medicare and Medicaid Services-CMS, 2018). Recent growth in Medicaid enrollment has been driven primarily by nonelderly adults with low-income who were newly eligible for Medicaid. Although the children's eligibility threshold was not affected by Medicaid expansion, children's enrollment in Medicaid and CHIP has also increased in both expansion and non-expansion states.

2.2 The Affordable Care Act Medicaid Expansion in Louisiana

Medicaid expansion in the state of Louisiana took effect on July 1, 2016, making it the 31st state to adopt the program. Prior to Medicaid expansion, the Medicaid eligibility threshold for nonelderly adults in Louisiana (i.e., 24% of the FPL) had been unchanged since 2013 and childless adults were ineligible for coverage regardless of how low their incomes were. This left many low-income adults with no access to affordable health insurance until the Medicaid expansion program was adopted.

Residents who participated in SNAP (Supplemental Nutrition Assistance Program, often referred to as "food stamps") were expected to be auto-enrolled in Medicaid. After the implementation of the Medicaid expansion, there was a consistent increase in

[2] Connecticut and Maine were not included in the calculations depicting changes because these two states did not submit enrollment data for the period of July to September 2013.

Medicaid enrollment from 2016 to 2018 (see Figure 1). As of June 2018, more than 1.5 million people in Louisiana had health coverage through Medicaid, representing about 34.3 percent of Louisiana's total population in 2018. These newly enrolled in Medicaid individuals are now getting regular preventive care and have more access to health care services.

However, the purpose of this paper is not to study new coverage for adults, but rather the positive spillover effects of expanded adult Medicaid coverage on children's use of preventive health care services. Since the Medicaid eligibility criteria for pregnant women are different from those for other nonelderly adults (see Table 1), children whose mothers are pregnant were excluded.

2.3 Related Literature

This study builds upon four strands of literature. The first strand is related to studies about the direct effect that the Medicaid expansion under the ACA has had on health care coverage. Most studies demonstrate that the Medicaid expansion has had a positive impact on Medicaid enrollment or coverage gains, and that it has also reduced uninsured rates among the low-income population (McMorrrow et al., 2015; Vistnes and Cohen, 2016; Decker et al., 2017; Frean et al., 2017; Soni et al., 2018).

The second strand of literature is related to studies about the direct effects of Medicaid expansion under the Affordable Care Act on access to care, utilization, affordability and health outcomes. Most studies have shown that the Medicaid expansion has had a positive impact on access to health care, the utilization of services and the affordability of care (Choi, 2011; Finkelstein et al., 2012; Anderson et al., 2012; Lipton & Decker, 2015; Hayes et al., 2017; Long et al., 2017; Ghosh et al., 2017; Wherry, 2018). A few studies have found that there have been no significant effects in these areas (Wherry and Miller, 2016; Miller and Wherry, 2017). Several other studies have suggested that improved access to care and utilization has resulted in increased diagnoses of some chronic diseases, and has also led more adults to receive continued treatment for these diseases (Clemans-Cope et al., 2017; Soni et al., 2018; Ajkay et al., 2018; Loehrer et al., 2018).

Thirdly, this study relates to literature that has investigated the spillover effects of parents' insurance or Medicaid enrollment on children's Medicaid coverage. Most studies have found that parents' insurance or Medicaid coverage has had positive spillover effects on children's coverage (Dubay and Kenney, 2003; Sommers, 2006; Devoe et al., 2015; Hudson and Moriya, 2017). Hudson and Moriya (2017) showed a positive association between parents' eligibility for Medicaid and "welcome mat effects"[3] for their children under the ACA. Devoe et al. (2015) demonstrated a causal link between parents' access to Medicaid coverage and their children's coverage, using Oregon experimental trial data. Sommer (2006) suggested that the Medicaid expansion for parents improved the retention of their children enrolling in Medicaid. Dubay and Kenney (2003) found that expanding public health insurance coverage for parents has led to increases in Medicaid participation among children.

Finally, this study relates to literature that has investigated the spillover effects of parents with private insurance or Medicaid on their children's health care use. Davidoff et al. (2003) found that having an uninsured parent was associated with a lower likelihood of receiving a well-child visit. Gifford (2005) demonstrated that extending Medicaid to low-income parents would increase children's well-child visits. Devoe et al. (2009) showed that insured children with uninsured parents have a higher probability of not using necessary health services. However, these studies did not have rigorous identification strategies. Only two studies had rigorous causal research designs. They

[3] Welcome mat effects: Medicaid expansion increases health coverage among children already eligible for Medicaid when their parents become eligible as well.

presented spillover effects of adult Medicaid enrollment on children's use of preventive services. One study demonstrated that the Medicaid expansion on low-income adults increased the use of pediatric preventive care for their children (Venkataramani et al., 2017). Another study showed that parents with Medicaid dental coverage would increase their children's dental visits compared to those in states without adult dental coverage (Lipton, 2021). However, these two recent studies did not identify individuals who were actually enrolled in Medicaid. Both of them used an eligibility simulation method that compared the family-level income with state-specific Medicaid income eligibility thresholds to obtain their samples of Medicaid enrollees. They therefore could only estimate the local average treatment effect (LATE) of parental Medicaid coverage status on their children's healthcare service use. LATE is the treatment effect on the compliers and these compliers in the national samples may not have actually been enrolled in Medicaid, which would therefore have biased the estimates.

3 Conceptual Framework

A large number of children do not receive the preventive health care services they need, even though under Medicaid there is no copay or out-of-pocket costs for children's preventive health care services, such as well-child visits and dental exam visits. Some studies have shown that the low rate of using preventive services is due to children not having adequate access to health care providers who accept Medicaid (Aizer, 2007; Currie et al., 2008; Leininger and Levy, 2015). After the Medicaid expansion under the ACA, access to health care has improved significantly, but the rate of children's utilization of preventive services is still low. According to Medicaid and CHIP Program System reports for the year 2017, fifty-six percent of children under 21 had at least one well-child visit, and 48 percent of children ages 1 to 20 had at least one preventive dental service. One hypothesis of the unmet health care needs is that the parents' insurance status may affect children's preventive health services utilization. I test this hypothesis using Louisiana Medicaid administrative data. Specifically, I test whether children whose parents had Medicaid coverage would be more likely to use preventive services.

This hypothesis is plausible because children do not make their own decisions to seek health care. Parental attributes and behaviors may influence the health care decisions made for children. Several studies have demonstrated the positive relationship between parents' pattern of health care utilization and their children's use of health care services (Hanson, 1998; Freed et al., 1999; Goedken et al., 2014; Thakkar et al., 2019). Hanson (1998) concluded that both insured and uninsured children were more likely to have a physician visit if their parents have had a physician visit. Freed (1999) showed that children of parents who engaged in the health care system were more likely to have physician visits and complied with the immunization schedule than children of parents who did not use health services. Goedken (2014) found that children whose parents had physician visits were more likely to meet the recommended well-child visit than children whose parents had no visits. Thakkar (2019) showed that mothers' routine care use was found to promote their adolescent children's routine care use.

The literature cited above suggests a strong relationship between children's use of health services and their parents' utilization of health care services. Other parallel literature has shown that parents' own use of health services is highly dependent on their insurance status. Long (1994) found that uninsured adults used ambulatory health services only 60 percent as often as adults with insurance. In addition, several studies have shown that the Medicaid expansion has had a positive impact on access to care and the utilization of health services (Finkelstein et al., 2012; Lipton & Decker, 2015; Ghosh et al., 2017; Hayes et al., 2017; Wherry, 2018).

The studies cited above indicate that a change in parents' insurance status (including public and private insurance) could lead to a change in children's utilization of health care services. Relatively few studies have examined this effect. Davidoff (2003) and Devoe (2009) found that children with uninsured parents were associated with a lower likelihood of using necessary health services. Two additional studies have shown the positive spillover effects of parent's Medicaid status on children's use of preventive health care (Venkataramani et al., 2017; Lipton, 2021).

There are several potential mechanisms regarding why parental enrollment in Medicaid could have positive spillover effects on children's health care use. First, the Medicaid expansion increases parents' healthcare access and use of health service, and parents' experiences with using health service would add their familiarity to the Medicaid system (Gifford et al., 2005). For example, when parents are also enrolled in Medicaid, they will be more likely to know about the benefits of coverage, like there is no copay for children's preventive service use. Parents' engaging with doctors also enhance their knowledge about the importance of preventive health care. Thus, parents would be more likely to take their children to use preventive services like seeing a dentist or a pediatrician for regular check-ups and taking vaccines. Second, the Medicaid expansion improves low-income families' financial health by reducing medical bills (Finkelstein et al., 2012; Brevoort, 2017; Miller et al., 2019; Caswell and Waidmann, 2019). This income effect may allow parents to free up their working time or afford the cost of transportation for their children's health care use (Venkataramani et al., 2017). Third, fixed costs of finding a doctor or of searching for information about the health care system can be used for both parent and child. Previous literature found that participation in Medicaid increased with family size, providing supportive evidence that fixed costs play a role in a related decision-making process (Currie, 2000). Lastly, the lower cost of seeing a doctor may lead parents to pay more attention to their own health.

They may develop a habit of having annual check-ups or of using other preventive health services, and parents' habits may have positive externality effects on their children (Lipton, 2021).

4 Data

The main data source for this paper is the Louisiana Medicaid administrative database. It includes eligibility and claims information for everyone who is enrolled in Medicaid, with the sample covering July 2013 to December 2018 (rolling calendar year from July to next June since the Medicaid expansion in Louisiana took effect in July 2016, and year 2018 covers July 2018 to December 2018 because 2019 data is not currently available). My interest is to identify the effect of the Louisiana Medicaid expansion for newly enrolled parents on "always enrolled" children's use of well-child visits and dental visits. Eligibility data, such as eligibility ID for both parents and children and household ID, and the demographic characteristics such as gender, age, race, and household size were obtained from the eligibility database. Parents' education level, family gross income are obtained from Area Health Resources Files (AHRF). The main outcomes are the number of well-child visits (WCV), dental exam visits and immunization visits, which were taken from the claim databases. The Current Procedural Terminology (CPT) codes for well-child visits are 99381- 99385 and 99391-99395 (see Appendix Table A3). The CPT code for dental exam is D0191. The CPT codes for immunization visits are 90461, 90462, 90471-90474[4]. For this paper, I consider well-child visits for children ages 3 to 17 because the American Academy of Pediatrics' (AAP) schedule for well-child visits is once a year after 3 years of age, and many

[4] 90460 is used for the first immunization, 90461 is used for each additional immunization. Only use 90461 in conjunction with 90460. 90460-90461 are appropriate for immunization administration and counseling by physician or LIP (through 18 years of age). If immunization administration and counseling is provided by nurse- use codes 90471 – 90474.

children leave home for college after turning 18. In order to keep consistency analysis with well-child visits, I focus on dental exam visits and immunization visits for children ages 3 to 17 as well.

In this study, I exclude children whose mothers were pregnant at any point during the calendar year, because the Medicaid eligibility rules differ for pregnant women. In addition, parents are limited to non-disabled adults under 65 years of age since the Medicaid eligibility levels for disabled and aged people are different, as well. I also only include children who have been enrolled in Medicaid both prior to and after the Medicaid expansion period in order to rule out the effect of newly enrolled children on the outcomes. Because numerous studies have already shown that the Medicaid expansion has had a positive impact on Medicaid enrollment and the utilization of services (Decker et al., 2017; Soni et al., 2018; Wherry, 2018).

The outcome variables, the main independent variable and the control variables are shown in Table 2. Since Louisiana Medicaid Claim data are monthly data, I add up well-child visits, immunization and dental exam visits by year. I then define both outcomes as 1 if a child receives at least one well-child visit, immunization visit or dental visit each year, and 0 is assigned if a child does not have any visits in a year, the reason for this definition is that the American Academy of Pediatrics' (AAP) schedule of well-child visits is once a year after 3 years of age. In order to keep consistency analysis with well-child visits, I use the same definition for dental exam visits.

The pre-expansion periods are defined as from July 2013 to June 2016, and the post-expansion periods are from July 2017 to December 2018. I matched children with their parents using household ID numbers. The Medicaid-eligible children are divided into two groups based on the Medicaid enrollment of their parents: "Parent always enrolled Medicaid" and "Parent newly enrolled in Medicaid." The "Parent always enrolled in Medicaid" group contains children whose parents were enrolled in Medicaid for both the pre- and post- periods (the control group). Children whose parents were enrolled in Medicaid during the post-period are defined as "Parent newly enrolled in Medicaid" (the treatment group).

This study focuses on children ages 3 to 17. Table 3 presents summary statistics for all the demographic variables and the outcome variables for the main child samples. Among pre-period, the average well-child visits for treated group are less than that of control group, while the average well-child visits for treated group are larger than that of control group after 2016. The means of dental exam visits for treated group are little larger than that of control group before 2016, but the average dental exam visits for treated group are less than that of control group after 2016. The means of male, age, race and family size are all similar for both treated and control group. Most
children are either white or black, and Hispanic children only account for 0.2%. A possible reason
for this is that most parents of Hispanic children are not U.S. citizens and they are not eligible to apply for Medicaid in Louisiana.

5 Empirical Strategy

5.1 Difference-in-Differences Model

Using a difference-in-differences (DD) regression framework, I examine the spillover effects of the Medicaid expansion program for adults on their children's use of preventive health care services. This DD estimation compare children whose parents were newly enrolled in Medicaid (after the ACA's Medicaid expansion in Louisiana took effect in July 2016) with children whose parents were "always enrolled" in Medicaid over the same time period.

The regression model for the child analysis is:

$$Y_{it} = \beta_0 + \beta_1 ParMedicaid_i * Post_t + \beta_2 ParMedicaid_i + \beta_3 Post_t + \beta_4 family_{it} + \beta_5 child_{it} + \mu_t + \epsilon_{it} \quad (1)$$

In equation (1), Y_{it} are binary outcomes for child i at year t: A value of 0 is assigned if a child has no well-child visit, immunization visit or dental visit in a year, and 1 is assigned if a child receives at least one well-child visit, immunization visit or dental visit within a year; $ParMedicaid_i$ is an indicator variable, and is equal to 1 if parents enrolled in Medicaid after the ACA's Medicaid expansion in Louisiana and has a value of 0 otherwise; $Post_t$ equals 1 if it is after 2016; $Child_{it}$ is a vector of child characteristics including race, age and sex; $family_{it}$ is a vector of family characteristics including family gross income and household size; μ_t represents year fixed effects, ϵ_{it} is an error term.

The coefficient of interest is β_1, which captures the effect of the Medicaid expansion on the outcome variable Y_{it} for children ages 3 to 17. Since the outcomes are binary variables, I run both linear probability and logit models, which yielded similar results (logit results are presented in Appendix Table A4). Standard errors are clustered by Household ID to account for serial correlation in the policy variable.

5.2 Parallel Trends Assumption

A critical assumption in DID analysis is parallel trends: The treatment group and control group would have their outcomes move in parallel if the treatment group had not been treated. Figure 2 presents the trends of the three outcomes (well-child visits, dental visits and immunization visits) from 2013 to 2018. It shows the similar trends of both two outcomes for treated and control groups before the Medicaid expansion in 2016, and the share of well-child visits and dental visits are higher for children with parents enrolled in Medicaid than those with parents consistently enrolled in Medicaid.

5.3 Event Study Model

To display pre-trends among comparison groups and also to explore how the effect of policy evolves over time, I conduct event studies. The estimating equation for the event study takes the following form:

$$Y_{it} = \alpha_0 + \sum_{t\in\{2013,2014,2016,2017,2018\}} \alpha_t * Treated_i * I_t + \alpha_2 X_{it} + \mu_t + \varepsilon_{it} \qquad (2)$$

In equation (2), Y_{it} is the same as equation (1); $Treated_i$ is an indicator that identifies whether a child is included in the treated group; I_t represents an indicator variable for each year, indicating time relative to the implementation of the Medicaid expansion, and the last year prior to the Medicaid expansion implementation (year 2015) is omitted; X_{it} are control variables including family's and children's characteristics; μ_t represents year fixed effects.

6 Results

Results from Tables 4 to 9 are presented in two specifications: both model (1) and model (2) include year fixed effects. However, model (1) does not include family's and children's characteristics and model (2) does include these characteristics.

6.1 The effects of Medicaid expansion on well-child visits, immunization and dental visits for the whole sample

Table 4 provides the main results for the whole sample. Significant effects of the Medicaid expansion on children's well-child visits are observed for both models. Children with parents newly enrolled in Medicaid are associated with a 2.20 percentage point increase in the probability of having at least one well-child visit within a year. This increase represents a 5.06 percent increase relative to the mean of well-child visits. The DD estimated coefficients of the dental visits are also statistically significant for both models, but smaller than those of well-child visits. Children with parents newly enrolled in Medicaid have a 0.3 percentage point increase in the probability of having at least one dental exam visit within a year. This increase represents a 0.84 percent increase relative to the mean of dental exam visits. Children with parents newly enrolled in Medicaid are associated with a 5.80 percentage point increase in the probability of having at least one immunization visit within a year. This increase represents a 8.41 percent increase relative to the mean of immunization visits. The potential reason for these results is that people think medical health is more important than dental health.

To examine the evolution of post-policy effects, I conduct the event studies described in equation (2), and the results are presented in Figure 3. I take the year prior to treatment (2015) as the base year and set it at zero. Panel A shows that there is not a significant difference in the well-child visits between the treatment and control groups before the Medicaid expansion took effect in 2016. It also shows an increase in the well-child visits after the implementation of the Medicaid

expansion. Panel B and panel C show that there is not a significant difference in the dental and immunization visits before the Medicaid expansion as well. Therefore, the DD estimates in table 4 are valid.

6.2 The effects of the Medicaid expansion on well-child visits, immunization and dental visits for the subgroups

In this section, I estimate the effects of parental Medicaid enrollment on well-child visits, immunization and dental visits for different subgroups by age, the number of children and race.

Table 5 presents the difference-in-difference estimates of the effect of parental Medicaid enrollment on well-child visits, immunization visits and dental exam visits by age groups. I divide the samples into three groups: ages 3 to 6, ages 7 to 11 and ages 12 to 17. The reasons for this division are: Preschool is not mandatory for children ages 3 to

6; children must attend school from age 7 to 18, and also after age 12, children are in middle school, and they have more opportunity to get health knowledge in school or from other resources (Gifford, 2005).

Estimates of the effects of parental Medicaid enrollment on well-child visits are positive and statistically significant for children ages 3 to 6. The estimate magnitudes for this group are a little larger than those estimates for the whole sample. There is a 2.6 percentage point increase and 2.1 percentage point increase in the probability of having well-child visits for children ages 3 to 6 and 7 to 1 respectively. Children with parental newly enrolled in Medicaid have no significant effects on dental exam visits. There is a 5.9 percentage point increase in the probability of having immunization visits for children ages 3 to 6 whose parents were newly enrolled in Medicaid, and the magnitude for children ages 7 to 11 is 5.1 percentage point increase that is a little smaller than that for 3 to 6 years old group. The coefficients for children ages 12 to 17, however, are not significant for all of the three outcomes. There are two possible reasons for the different effects among subgroups: First, parents are less aware of adolescents' health care needs compared to those younger children. Second, parents' decision-making power and authority decrease with their children's age (Lipton, 2021).

Table 6 provides the estimates of the effect of parental Medicaid enrollment on well-child visits , immunization visits and dental visits by the number of children for a household. I divide the samples into three groups: family with one child, two children and three or more children. The effects of the Medicaid expansion on well-child visits are positive and statistically significant for all three groups. Specifically, an increase of 2.0 percentage points is observed in the probability of having well-child visits for children in families with one child and whose parents were newly enrolled in Medicaid. The effects are even greater for children living in households with two children: Children in this group with parents newly enrolled in Medicaid are associated with a 4.8

percentage point increase in well-child visits. The effects of the policy on dental exam visits for all three groups are statistically insignificant. Possible explanations for these results are that parents with more children have more experience and knowledge about taking care of children; In addition, mothers with more children are more likely to stay at home and therefore have more time to take children to see a doctor.

Table 7 shows the estimates of the impact of parental Medicaid enrollment on well-child visits, immunization visits and dental visits by race. Here I only present the estimates by white and black group since these two races account for 98 percent of the whole sample. The effects of the Medicaid expansion on well-child visits are positive and statistically significant for both groups. Specifically, an increase of 1.9 percentage points is observed in the probability of having well-child visits for white children and whose parents were newly enrolled in Medicaid. The effects are even greater for black children: Children in this group with parents newly enrolled in Medicaid are associated with a 2.1 percentage point increase in well-child visits. Estimates are larger for immunization visits. The effects of the policy on dental exam visits for both groups are statistically insignificant.

Event study estimates of the effects of Medicaid expansion on three outcomes by Age, the number of children and race are presented in figure 4 to figure 9. We can see that there is not a significant difference in the dental and immunization visits before the Medicaid expansion as well. Therefore, the DD estimates in table 4 to table 7 are valid.

7 Sensitivity Analysis

I use children whose parents never had Medicaid as a new treatment group to conduct a placebo test and presents the results in Table 8. There are no significant effects on children’s well-child visits, immunization visits and dental exam visits for this new treatment group. This indicates that the Medicaid expansion may only have positive spillover effects on utilizations of health care services for children whose parents newly enrolled in Medicaid.

To examine if the main results in Table 4 are caused by the Louisiana Medicaid expansion, which took effect in July 2016, I then use the same DD analysis but falsely set 2014 as the expansion initiation year. I also limit the sample from 2013 to 2015 to rule out the effects of Medicaid expansion. The results are presented in Table 9. As expected, the results show that all the coefficients are positive but statistically insignificant, which means there is no significant effect of the Medicaid expansion on children's well-child visits, immunization visits and dental visits when the post expansion periods are dropped.

8 Cost-Benefit Analysis

The above results show that there is positive association between Medicaid expansion and children's use of preventive service. However, whether the policy is an attractive option would depend on how the benefit of these additional visits compares to the cost of the policy. Previous studies showed that it is possible to obtain rough estimates of the cost of the additional dental visits (Buchmueller et al., 2015; Lipton, 2021). Therefore, I will use the similar method to estimate the cost of the additional dental visits and well-child visits in this paper. The incremental cost of a visit can be expressed as:

$$\frac{\Delta C}{\Delta V} = \frac{C_t \times V_t}{V_t - V_{t-1}} \qquad (3)$$

Where ΔC is the change in spending per child, ΔV represents the change in the number of visits, C_t is the average amount paid by Medicaid for each visit, V_t is the number of visits per enrollee after Medicaid expansion, and V_{t-1} is the number of visits per enrollee before Medicaid expansion.

Similarly, the incremental benefit (cost saving) of a visit can be expressed as:

$$\frac{\Delta B}{\Delta V} = \frac{B_t \times V_t}{V_t - V_{t-1}} \qquad (4)$$

Where ΔB is the change in cost-saving per child, ΔV represents the change in the number of visits, B_t is the average dollars saving for each visit, V_t is the number of visits per enrollee after Medicaid expansion, and V_{t-1} is the number of visits per enrollee before Medicaid expansion.

Firstly, I estimate the incremental cost and benefit of a well-child visit. I use $120 from Muhuri and Machlin (2017) as the average amount paid by Medicaid for each well-child visit. Since the number of well-child visits per enrollee is about 2.07 after Medicaid expansion and the number of well-child visits per enrollee before Medicaid expansion is about 1.19. Then the incremental cost of a well-child visit based on the above equation (3) is about $283.94 = ($120 * 2.07)/ (2.07 – 1.19). I estimate the Medicaid expansion is associated with a 2.2 percentage points increase in at least one well-child visit, so the total number of additional well-child visits equals to the number of children whose parents newly enrolled in Medicaid after Medicaid expansion times percentage change in well-child visit with Medicaid expansion, which is approximately 3911.20 = 177,782 * 0.022, then the total cost for the additional visits is $2,579,321.70 = 3911.20 * $283.94. Well-child visits could lower future health care costs by improving health and reducing later life hospital and emergency department use. Here, I use $1,172.36 from Wherry et al. (2015) to represent for the average cost saving of a well-child visit due to the reduction of emergency department use at age 25. Then the incremental benefit of a child dental visit based on the above equation (4) is about $2,757.72= ($1,172.36 * 2.07)/ (2.07 – 1.19). The total benefit for the additional dental visits is $10,785,991.29 = 3911.20 * $2,757.72. Thus, the net savings for the additional well-child visits are $8,206,669.59.

Then I estimate the incremental cost and benefit of a dental visit. I use $26.54 (2006$) from Buchmueller et al. (2015) to proxy for the average cost of a child dental visit (based on a weighted average index of procedures). Since the number of child dental visits per enrollee is about 0.49 after Medicaid expansion and the number of child dental visits per enrollee before Medicaid expansion is about 0.44. Then the incremental cost of a child dental visit based on the above equation is about $260.09= ($26.54 * 0.49)/ (0.49 – 0.44). This number is bigger than that of previous studies. Bachmueller et al. (2015) estimates a $218.84 incremental dental visit cost and Lipton (2021) estimates a $227.11 incremental dental visit cost in 2006 dollars. I estimate the Medicaid expansion is associated with a 0.3 percentage points increase in at least one dental exam visit, so the total number of additional dental visits equals to the number of children whose parents newly enrolled in Medicaid after Medicaid expansion times percentage change in dental visit with Medicaid expansion, which is approximately 533.35 = 177,782 * 0.003, then the total cost for the additional dental visits is $138,719.01 = 533.35 * $260.09. I use $31.03 from Lee et al. (2018) to represent for the average cost saving of a child dental visit. Then the incremental benefit of a child dental visit based on the above equation (4) is about $304.09= ($31.03 * 0.49)/ (0.49 – 0.44). The total benefit for the additional dental visits is $162,186.40 = 533.35 * $304.09. The net savings for the additional dental visits are $23,467.39.

At last, I estimate the incremental cost of routine childhood immunization[5]. I use $26 (2006$) from Zhou et al. (2014) to proxy for the average cost of one vaccination. Since the number of child immunization visit per enrollee is about 2.12 after Medicaid expansion and the number of child immunization visit per enrollee before Medicaid expansion is about 1.88. Then the incremental cost of a child immunization based on the

[5] Here, the routine childhood immunization includes DTaP, Hib, IPV, MMR, HepB, VAR, PCV7, HepA and Rota vaccines.

above equation is about $229.67= ($26 * 2.12)/ (2.12 – 1.88). I estimate the Medicaid expansion is associated with a 5.8 percentage points increase in at least one immunization visit, so the total number of additional dental visits equals to the number of children whose parents newly enrolled in Medicaid after Medicaid expansion times percentage change in dental visit with Medicaid expansion, which is approximately 10311.36 = 177,782 * 0.058, then the total cost for the additional dental visits is $2,368,175.68 = 10311.36 * $229.67. The average cost saving of one immunization is about $260.79 (Zhou et al., 2014). Then the incremental benefit of an immunization visit based on the above equation (4) is about $2403.61= ($260.79 * 2.12)/ (2.12 – 1.88). The total benefit for the additional dental visits is $24,784,534.41= 10311.36 * $2403.61. The net savings for the additional immunization visit are $22,416,358.73.

9 Conclusion and Discussion

Medicaid expansion in the state of Louisiana took effect on July 1, 2016. As a result of this expansion, the Medicaid eligibility threshold for nonelderly adults in Louisiana increased from 24 percent of the FPL to 138 percent of the FPL, dramatically increasing the health insurance coverage of low-income adults.

This study provides a new evidence of the spillover effects of Medicaid expansion on children's preventive well-child visits and dental visits using Louisiana Medicaid administration data. The advantage of this data is that it makes it possible to identify parents who actually enrolled in Medicaid after the expansion. In this paper, I examine the spillover impacts of the Medicaid expansion on children's well-child visits and dental exam visits using a difference-in-differences regression analysis framework, which compares children whose parents enrolled in Medicaid after the expansion with children whose parents always enrolled in Medicaid.

The results suggest that children with parents newly enrolled in Medicaid are associated with a 2.20 percentage point (5.06%) increase in their probability of having at least one well-child visit within a year, a 5.80 percentage point (8.41%) increase in their probability of having immunization visits and a 0.30 percentage (0.84%) increase in probability of having dental exam visits. Analysis for subgroups by age shows that the treatment effects are concentrated among children under age 12. The possible reasons for this result are that parents are more likely to take greater care of younger children, and parents' decision-making power and authority decrease as children grow older (Lipton, 2019). Analysis for subgroups by the number of children of a household shows that estimates for households with more children are bigger. This might be true because parents with more children have more experience and knowledge about taking care of children. In addition, mothers with more children are more likely to stay at home and may therefore have more time to take children to see a doctor.

The results of this study suggest that providing Medicaid coverage to parents has a positive impact on children's participation in recommended well-child visits, immunization visits and dental visits. These findings may have important policy implications. For example, some states do not provide Medicaid coverage for adults, which may affect access to preventive health services for both adults and their children. This study provides an example for states without Medicaid expansion to expand Medicaid coverage to include adults, especially for Louisiana's neighboring states like Alabama and Mississippi, both of the states have similar demographic characteristics to Louisiana based on the Census Bureau's American Community Survey.

This analysis still has several limitations. First, I do not take into account children's health status and parent's education level, which may also affect parents' decisions for taking them to see a doctor. Second, I only have information regarding parents who were covered by Medicaid, and do not consider whether parents without Medicaid have private insurance, which may also have impacts on children's use of health services.

This study only uses children's utilization of preventive services as outcomes, I will do further studies on children's other health outcomes such as asthma emergency department visits, fluoride varnish and development screening by 2 years of age. In addition, I would also examine whether mother's health behaviors have larger effects on children's health care use.

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TABLES AND FIGURES

Table 1. Louisiana Medicaid Income Eligibility as a Percent of FPL

Category	Before the Medicaid expansion (Jan 2013-Jan 2016)	After the Medicaid expansion (June 2016 -Jan 2019)
Adults under 65 with children	24%	138%
Children	212%	212%
Pregnant women	214%	214%

Source: Henry J Kaiser Family Foundation

Table 2. Definitions of Outcome Variables

Outcome variables	Definitions
Well-child visits	An annual check-up, which is a preventive measure for children's health
Dental exam visits	During a dental exam, the dentist or hygienist will clean the child's teeth and evaluate the child's risk of tooth decay.
Immunization visits	Getting scheduled vaccinations to prevent some contagious diseases.
Main independent variable	Parents newly enrolled in Medicaid after Medicaid expansion in July 2016.
Control variables	
Family characteristics	Family gross income, household size, number of children, single mother, parents' education level
Children's characteristics	Age, gender, race

Table 3. Summary Statistics: Louisiana Medicaid Administrative Data 2013-2018

	Pre-2016		Post-2016	
Variable	Treated	Control	Treated	Control
Male	0.497	0.497	0.499	0.497
Age	9.358	8.305	9.529	9.504
3-6 years	0.609	0.766	0.572	0.665
12-17 years	0.391	0.234	0.428	0.335
White	0.424	0.391	0.427	0.393
Black	0.553	0.595	0.550	0.592
Hispanic	0.002	0.001	0	0
Other races	0.023	0.014	0.023	0.014
Number of Children				
One	0.268	0.273	0.289	0.301
Two	0.475	0.450	0.482	0.473
Three or more	0.257	0.277	0.229	0.226
Well-child visit	0.435	0.440	0.468	0.458
Dental exam visit	0.359	0.351	0.487	0.496
Immunization visit	0.690	0.701	0.721	0.695
Observations	151,328	124,265	166,011	136,035

Notes: Presented here are the means of the summary statistics for children ages 3 to 17 in the Louisiana Medicaid administration data from 2013 to 2018. The race categories of the data include white, black, American Indian or Alaskan native, Asian, Hispanic, Native Hawaiian or other Pacific Island, Hispanic or Latino, and more than one race. For simplicity, this table provides some aggregated categories: "Hispanic or Latino" were combined with Hispanic; American Indian or Alaskan native, Asian, Hispanic, Native Hawaiian or other Pacific Island and more than one race were all combined as "other race". The main outcomes in this paper are well-child visits, immunization and dental exam visits and I defined them as a binary variable: 1 was assigned if a child received at least one well-child visit, immunization and dental exam visit each year, and 0 was assigned if a child had no visit in a year.

Table 4. Difference-in-differences estimates of the effect of parental Medicaid enrollment on well-child visits, dental and immunization visits among children ages 3 to 17

Variable	Well-child visits		Dental exam visits		Immunization	
	(1)	(2)	(1)	(2)	(1)	(2)
ParMedicaid*Post	0.021*** (0.003)	0.022*** (0.002)	0.004 (0.002)	0.003 (0.002)	0.056*** (0.004)	0.058*** (0.005)
Mean	0.435	0.435	0.359	0.359	0.690	0.690
Change %	4.83%	5.06%	1.11%	0.84%	8.11%	8.41%
Observations	577,639	577,639	577,639	577,639	577,639	577,639
Child's characteristics	No	Yes	No	Yes	No	Yes
Family's characteristics	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the difference-in-differences estimates for the whole sample based on Louisiana Medicaid administration data from 2013 to 2018. Child's characteristics include children's demographic characteristics such as sex, race and age. Family characteristics include characteristics such as household size and family gross income. The DD regressions compare well-child visits, immunization and dental visits among children whose parents were newly enrolled in Medicaid with children whose parents had always had Medicaid coverage, both before and after the Medicaid expansion in Louisiana. Model (1) and model (2) both include year and individual fixed effects, however, model (1) includes child's characteristics and family characteristics and model (2) does not include those controls. Robust standard errors are clustered at Household ID level. Standard errors are shown below the estimates in parentheses. Significant levels: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Data source: Louisiana Medicaid Administrative Data 2013-2018.

Table 5. Difference-in-differences estimates of the effect of parental Medicaid enrollment on well-child visits, dental and immunization visits by age group

Variable	Well-child visits		Dental exam visits		Immunization	
	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: 3- 6 years old						
ParMedicaid*Post	0.027***	0.026***	0.004	0.004	0.055***	0.059***
	(0.003)	(0.004)	(0.002)	(0.002)	(0.005)	(0.005)
Mean	0.426	0.426	0.348	0.348	0.756	0.756
Change %	6.34%	6.10%	1.72%	1.72%	7.28%	7.80%
Observations	159,770	159,770	159,770	159,770	159,770	159,770
Panel B: 7-11 years old						
ParMedicaid*Post	0.019***	0.021***	0.003	0.003	0.049***	0.050***
	(0.003)	(0.004)	(0.002)	(0.002)	(0.005)	(0.005)
Mean	0.432	0.432	0.356	0.356	0.702	0.702
Change %	4.40%	4.86%	0.84%	0.84%	6.98%	6.98%
Observations	232,990	232,990	232,990	232,990	232,990	232,990
Panel C: 12-17 years old						
ParMedicaid*Post	0.003	0.003	-0.005	-0.005	0.025	0.032
	(0.005)	(0.004)	(0.003)	(0.003)	(0.060)	(0.063)
Mean	0.390	0.390	0.383	0.383	0.698	0.698
Change %	0.77%	0.77%	1.31%	-1.31%	3.58%	4.58%
Observations	184,879	184,879	184,879	184,879	184,879	184,879
Child's characteristics	No	Yes	No	Yes	No	Yes
Family's characteristics	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the difference-in-differences estimates by age group of the parental Medicaid enrollment indicator and controls, based on Louisiana Medicaid administration data from 2013 to 2018. See notes to table 4 for details.

Table 6. Difference-in-differences estimates of the effect of parental Medicaid enrollment on well-child visits, dental and immunization visits by the number of children

Variable	Well-child visits		Dental exam visits		Immunization	
	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: One child						
ParMedicaid*Post	0.018***	0.020***	0.003	0.003	0.058***	0.059***
	(0.004)	(0.005)	(0.002)	(0.002)	(0.004)	(0.005)
Mean	0.432	0.432	0.398	0.398	0.689	0.689
Change %	4.17%	4.63%	0.75%	0.75%	8.42%	8.56%
Observations	154,807	154,807	154,807	154,807	154,807	154,807
Panel B: Two children						
ParMedicaid*Post	0.041***	0.048***	0.014	0.016	0.062***	0.065***
	(0.006)	(0.005)	(0.013)	(0.010)	(0.006)	(0.011)
Mean	0.436	0.436	0.371	0.371	0.730	0.730
Change %	9.40%	11.01%	3.77%	4.31%	8.49%	8.90%
Observations	272,646	272,646	272,646	272,646	272,646	272,646
Panel C: Three or more children						
ParMedicaid*Post	0.039***	0.042***	0.008	0.010	0.059***	0.061***
	(0.006)	(0.005)	(0.006)	(0.008)	(0.007)	(0.010)
Mean	0.453	0.453	0.373	0.373	0.723	0.723
Change %	8.61%	9.27%	2.14%	2.68%	8.16%	8.44%
Observations	150,186	150,186	150,186	150,186	150,186	150,186
Child's characteristics	No	Yes	No	Yes	No	Yes
Family's characteristics	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the difference-in-differences estimates by household size of the parental Medicaid enrollment indicator and controls, based on Louisiana Medicaid administration data from 2013 to 2018. See notes to table 4 for details.

Table 7. Difference-in-differences estimates of the effect of parental Medicaid enrollment on well-child visits, dental and immunization visits by race

Variable	Well-child visits		Dental exam visits		Immunization	
	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: White						
ParMedicaid*Post	0.015***	0.019***	0.003	0.003	0.032***	0.030***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.007)	(0.007)
Mean	0.401	0.401	0.380	0.380	0.687	0.687
Change %	3.74%	4.74%	0.79%	0.79%	4.66%	4.37%
Observations	211,058	211,058	211,058	211,058	211,058	211,058
Panel B: Black						
ParMedicaid*Post	0.019***	0.021***	0.004	0.004	0.080***	0.080***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.006)	(0.006)
Mean	0.390	0.390	0.365	0.365	0.715	0.715
Change %	4.87%	5.38%	2.00%	2.00%	11.19%	11.19%
Observations	294,068	294,068	294,068	294,068	294,068	294,068
Child's characteristics	No	Yes	No	Yes	No	Yes
Family's characteristics	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the difference-in-differences estimates by household size of the parental Medicaid enrollment indicator and controls, based on Louisiana Medicaid administration data from 2013 to 2018. See notes to table 4 for details.

Table 8. The effects of parental Medicaid enrollment on outcomes (children whose parents without Medicaid as the treatment group)

Variables	Well-child visit		Dental exam visit		Immunization visit	
	(1)	(2)	(1)	(2)	(1)	(2)
treated*post	-0.003	-0.004	-0.005	-0.004	-0.027	-0.030
	(0.002)	(0.003)	(0.003)	(0.005)	(0.021)	(0.023)
Mean	0.460	0.460	0.361	0.361	0.692	0.692
Observations	415,690	415,690	415,690	415,690	415,690	415,690
Child's characteristics	No	Yes	No	Yes	No	Yes
Family's characteristics	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

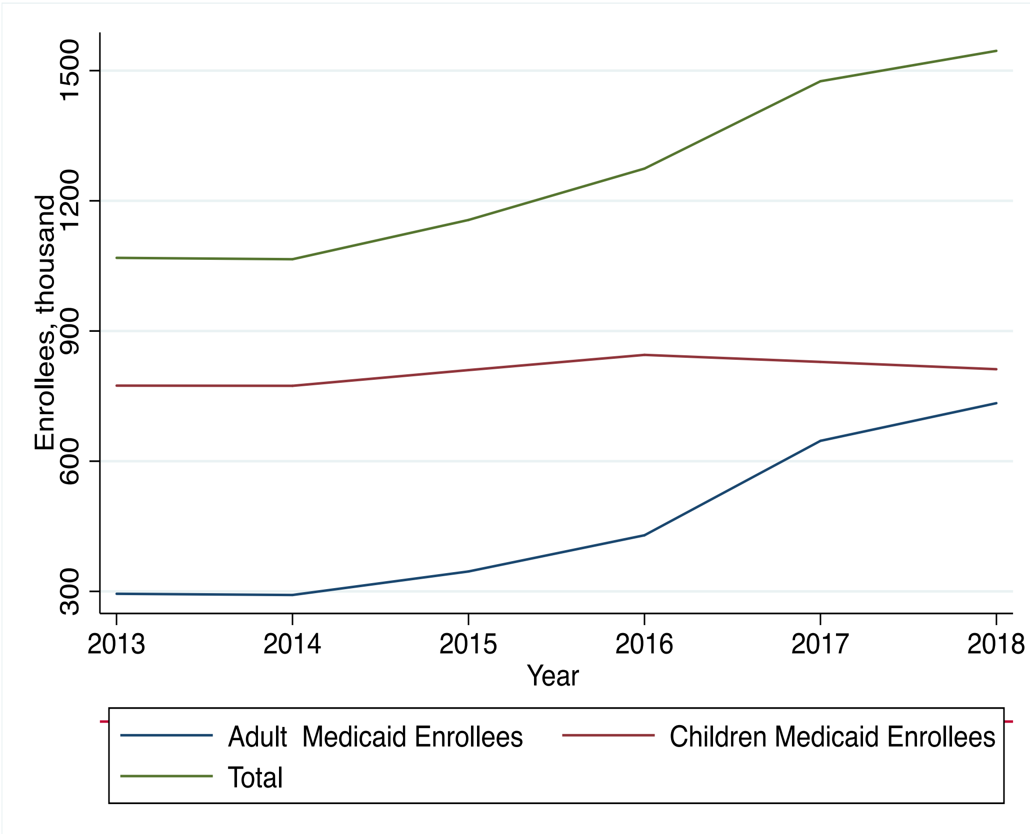
Notes: This table presents the difference-in-differences estimates of the parental never enrolled in Medicaid and controls, based on Louisiana Medicaid administration data from 2013 to 2018. See notes to table 4 for details.

Table 9. The effects of parental Medicaid enrollment on well-child visits, dental and immunization visits (without year 2016 to 2018)

Variables	Well-child visits		Dental visits		Immunization	
	(1)	(2)	(1)	(2)	(1)	(2)
ParMedicaid*post	0.001	0.001	0.005	0.005	0.016	0.018
	(0.004)	(0.004)	(0.004)	(0.003)	(1.010)	(0.014)
Mean	0.437	0.437	0.362	0.362	0.698	0.698
Observations	275,967	275,967	275,967	275,967	275,967	275,967
Child's characteristics	No	Yes	No	Yes	No	Yes
Family's characteristics	No	Yes	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the difference-in-differences estimates of the parental Medicaid enrollment indicator and controls, based on Louisiana Medicaid administration data from July 2013 to June 2016. See notes to table 4 for

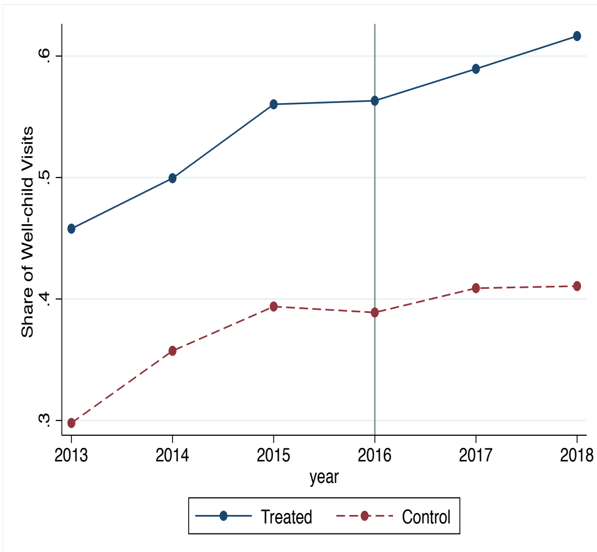
Figure 1. Louisiana Medicaid enrollees 2013-2018



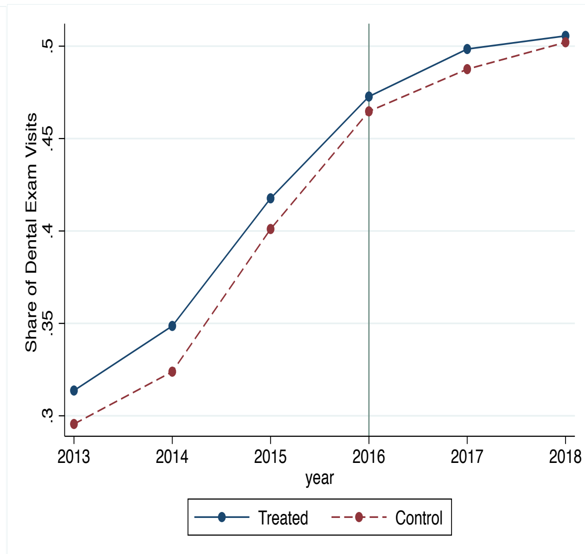
Data Source: Louisiana Medicaid Annual Report 2013-2018.

Figure 2. Trends in the share of well-child visits, dental exam visits and immunization Visits

A. Trends in the share of well-child visits



B. Trends in the share of dental exam visits

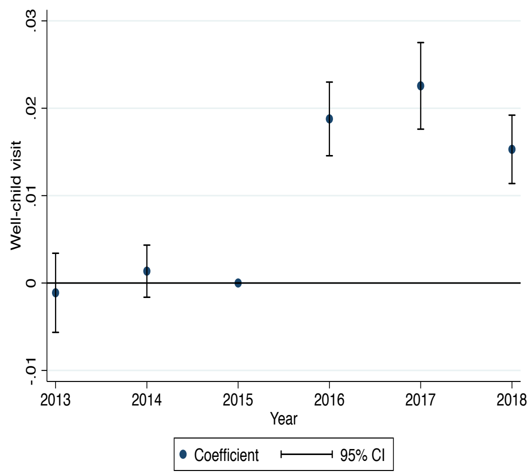


C. Trends in the share of immunization visits

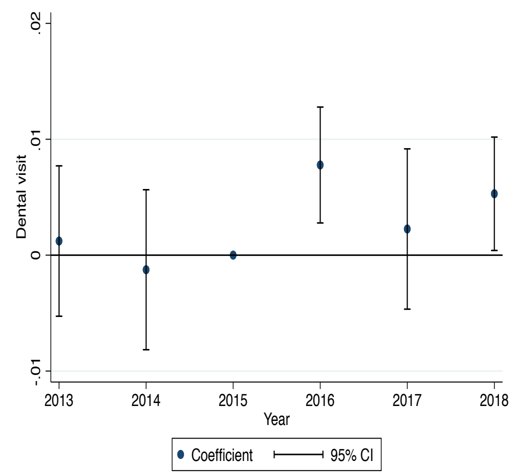
Notes: This figure plots the raw trends in the share of well-child visits, dental-exam visits and immunization visits for treated group and the comparison group. The vertical line at 2016 marks the year the Medicaid expansion policy is implemented.

Figure 3. Event study estimates of the effect of Medicaid expansion on three outcomes

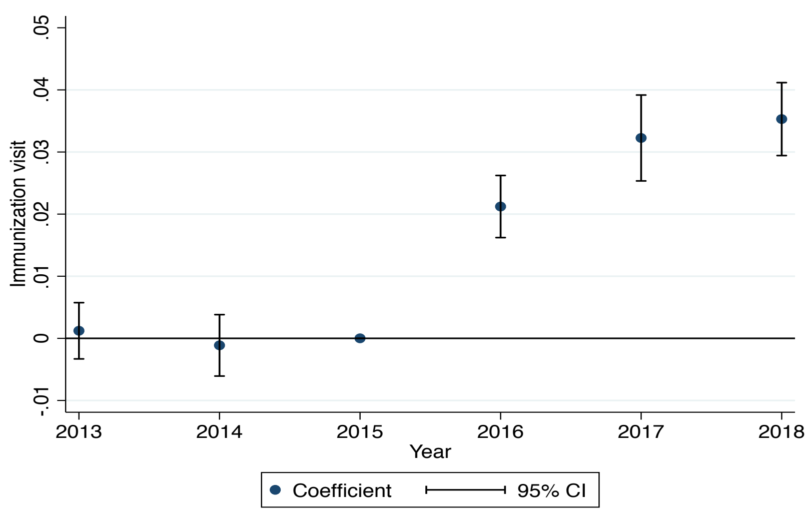
A. Estimated well-child visit effects by year



B. Estimated dental exam visit effects by year



C: Estimated Immunization visit effects by year



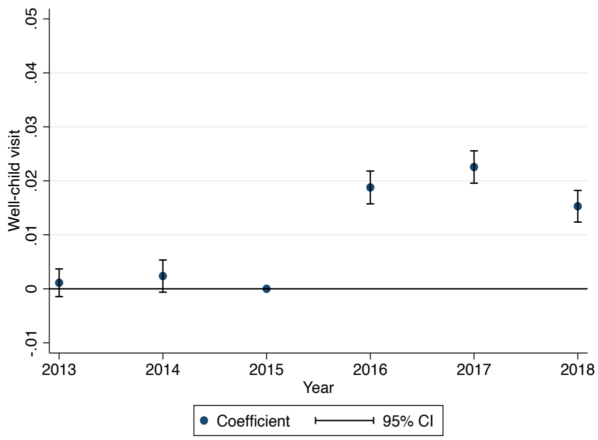
Notes: This figure displays coefficients and 95% confidence intervals of event study estimates for the whole sample. The event study is specified as follows:

$$Y_{it} = \alpha_0 + \sum_{t \in \{2013,2014,2016,2017,2018\}} \alpha_t * Treated_i * I_t + \alpha_2 X_{it} + \mu_t + \varepsilon_{it}$$

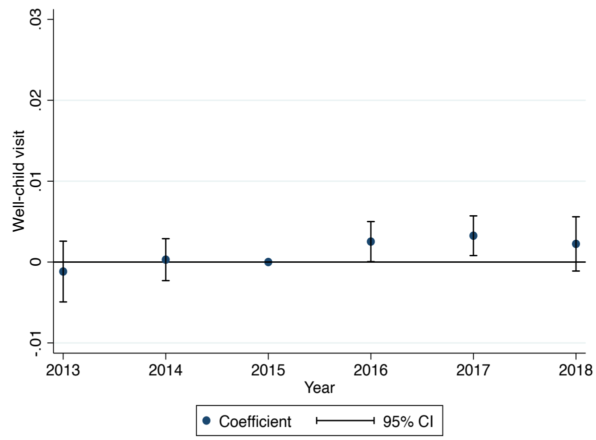
where Y_{it} are binary outcomes for child i at year t: A value of 0 is assigned if a child has no well-child visit, immunization visit or dental visit in a year, and 1 is assigned if a child receives at least one well-child visit, immunization visit or dental visit within a year; $Treated_i$ is an indicator that identifies whether a child is included in the treated group; I_t represents an indicator variable for each year, indicating time relative to the implementation of the Medicaid expansion, and the last year prior to the Medicaid expansion implementation (year 2015) is omitted; X_{it} are control variables including family's and children's characteristics; μ_t represents year fixed effects.

Figure 4. Event study estimates of the effect of Medicaid expansion on well-child visits by age

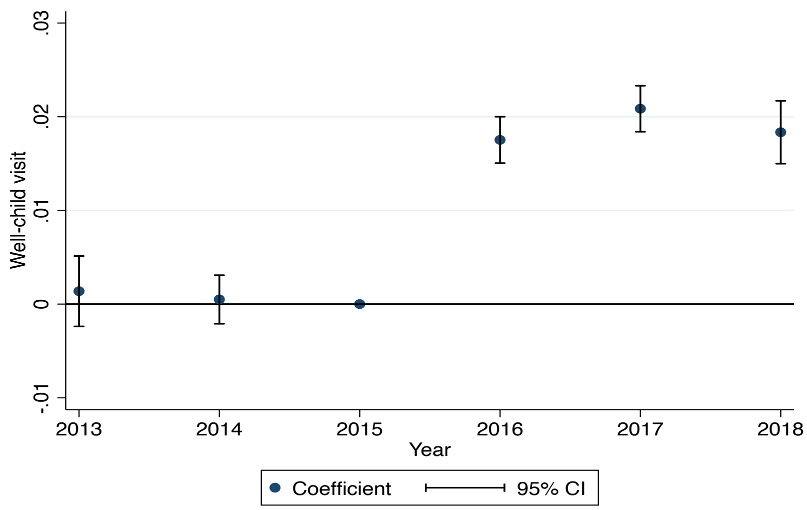
A. Age 3 to 6



B. Age 7 to 11



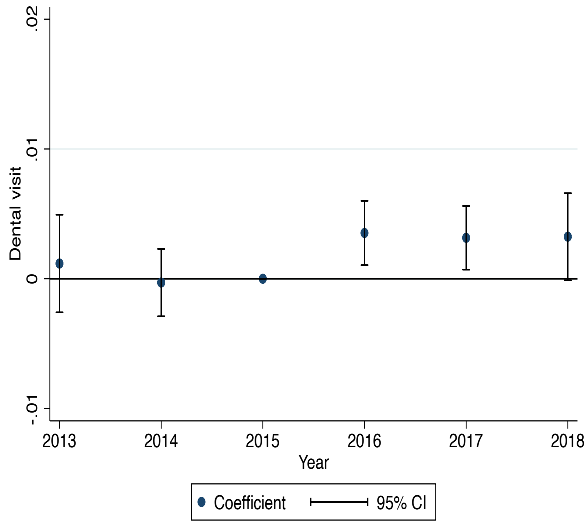
C. Age 12 to 17



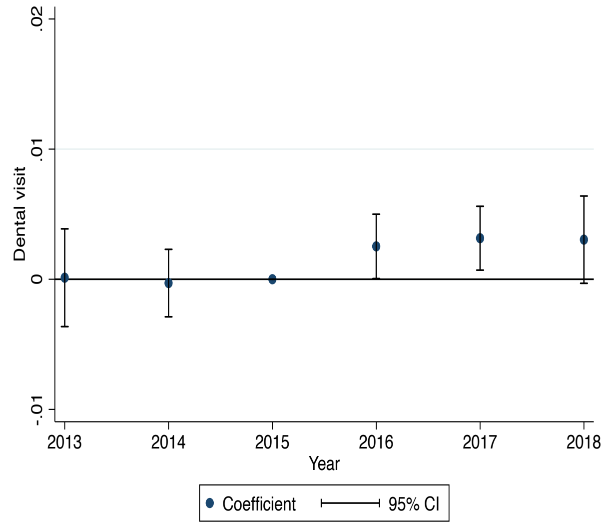
Notes: This figure displays coefficients and 95% confidence intervals of event study estimates on well-child visit by age. See notes to Figure 3 for details.

Figure 5. Event study estimates of the effect of Medicaid expansion on dental visits by age

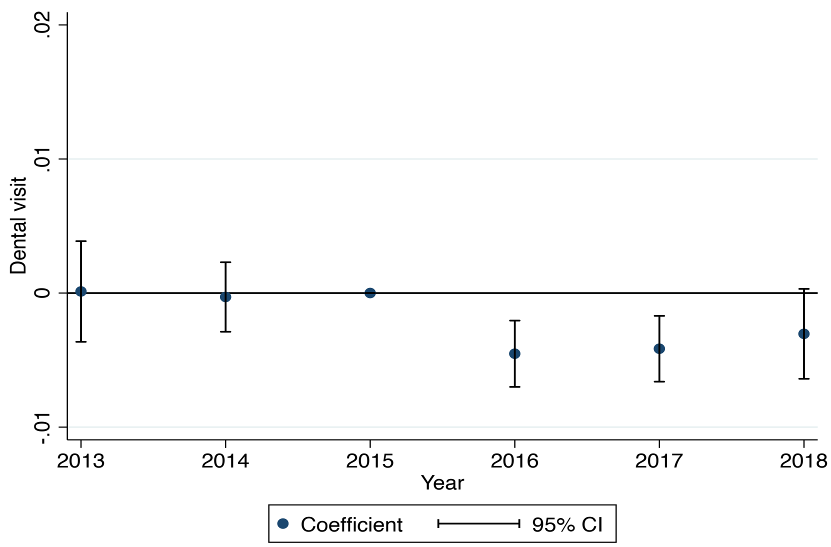
A. Age 3 to 6



B. Age 7 to 11



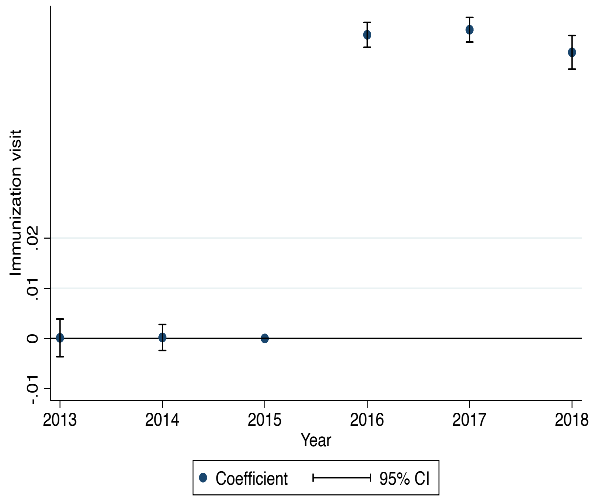
C. Age 12 to 17



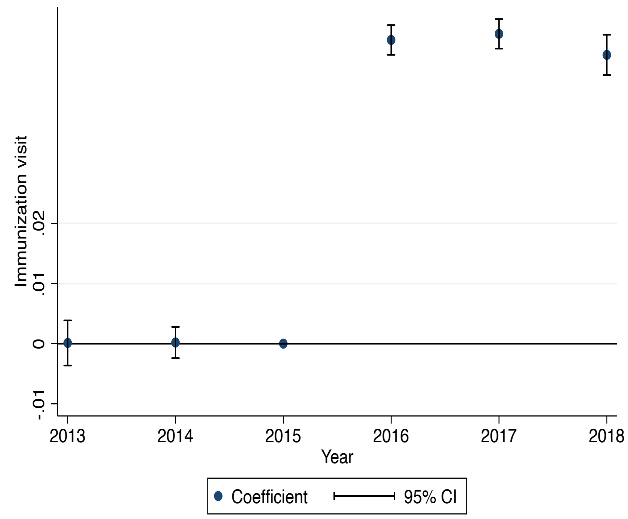
Notes: This figure displays coefficients and 95% confidence intervals of event study estimates on dental visit by age. See notes to Figure 3 for details.

Figure 6. Event study estimates of the effect of Medicaid expansion on immunization visits by age

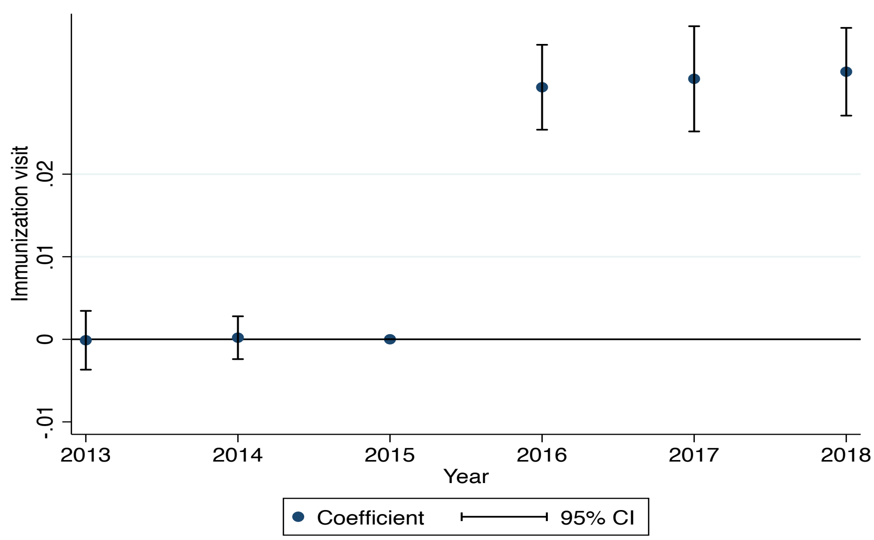
A. Age 3 to 6



B. Age 7 to 11

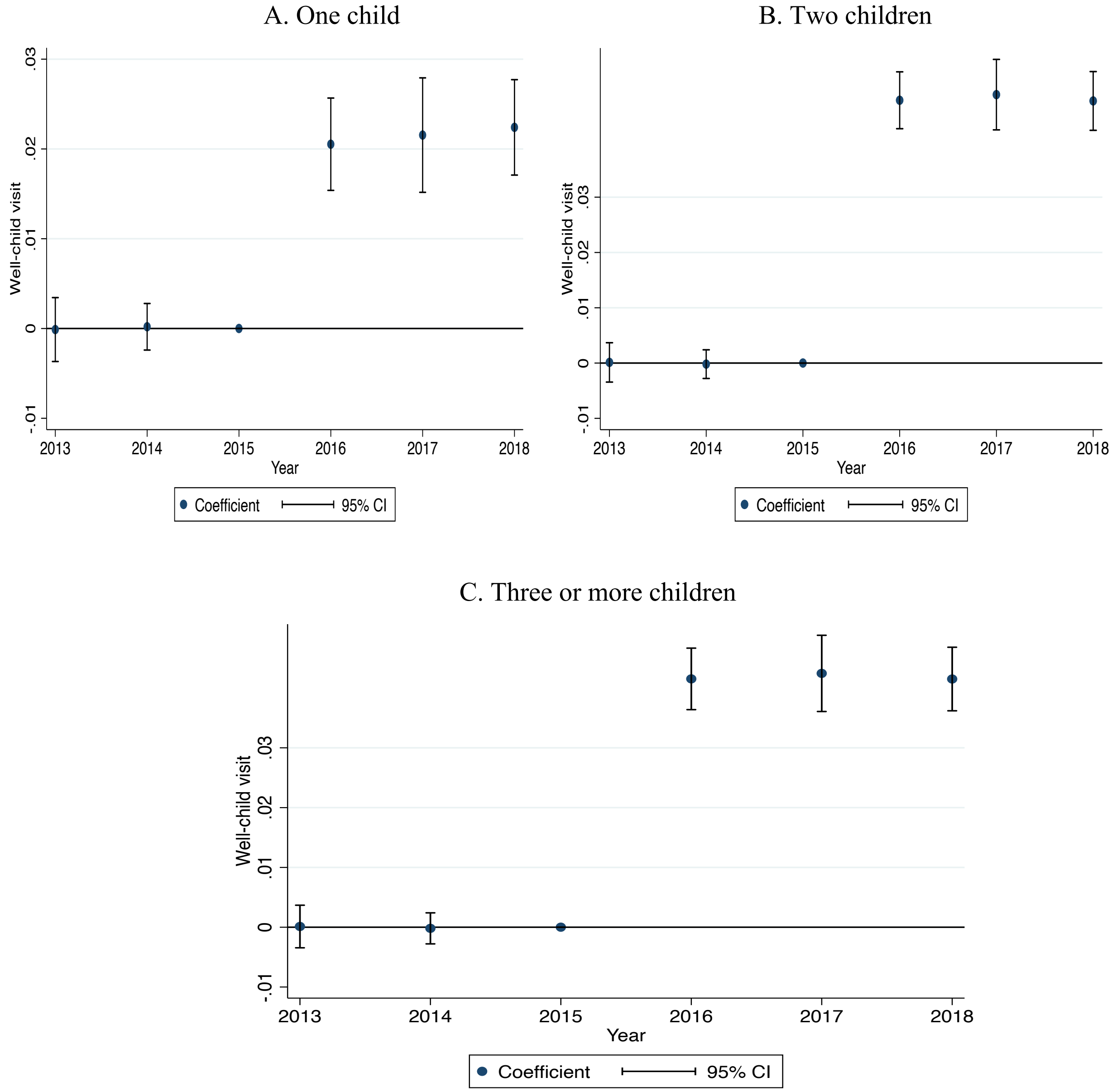


C. Age 12 to 17



Notes: This figure displays coefficients and 95% confidence intervals of event study estimates on immunization visit by age. See notes to Figure 3 for details.

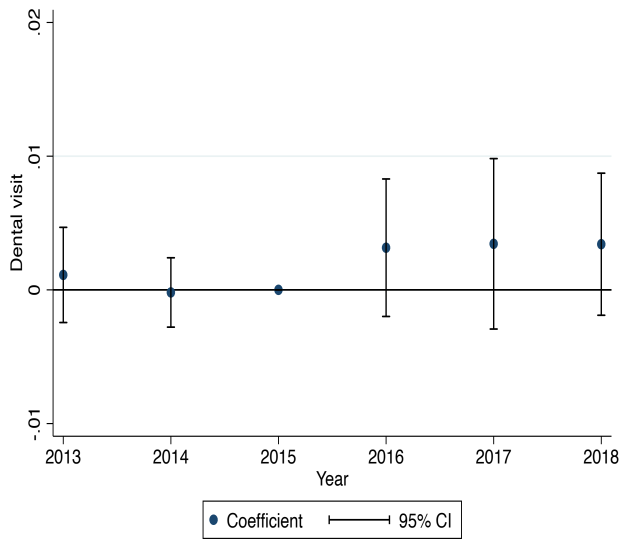
Figure 7. Event study estimates of the effect of Medicaid expansion on well-child visits by the number of children



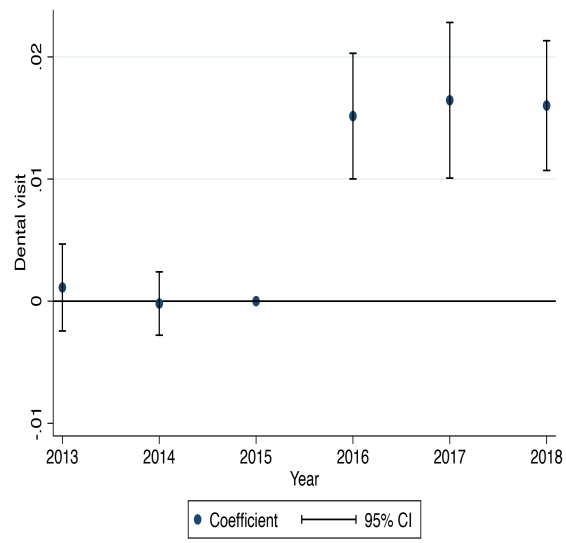
Notes: This figure displays coefficients and 95% confidence intervals of event study estimates on well-child visit by the number of children. See notes to Figure 3 for details.

Figure 8. Event study estimates of the effect of Medicaid expansion on dental visits by the number of children

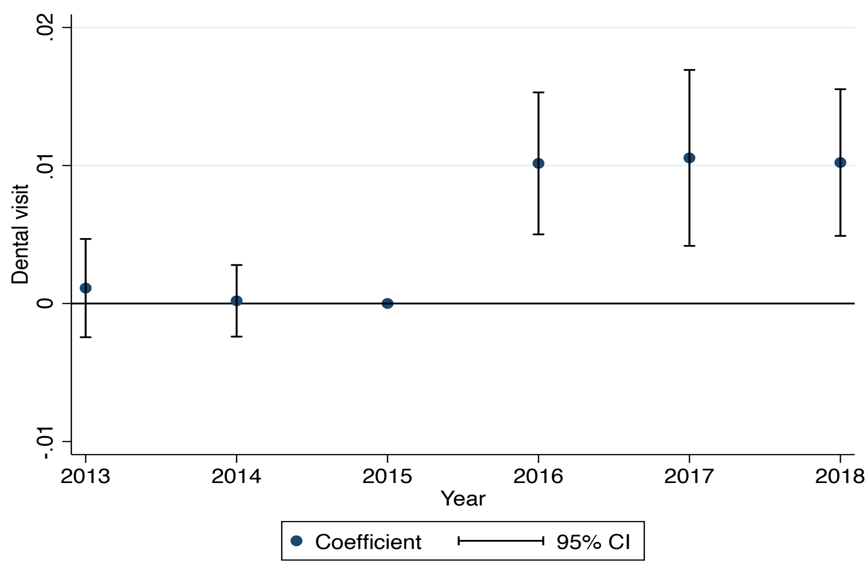
A. One child



B. Two children

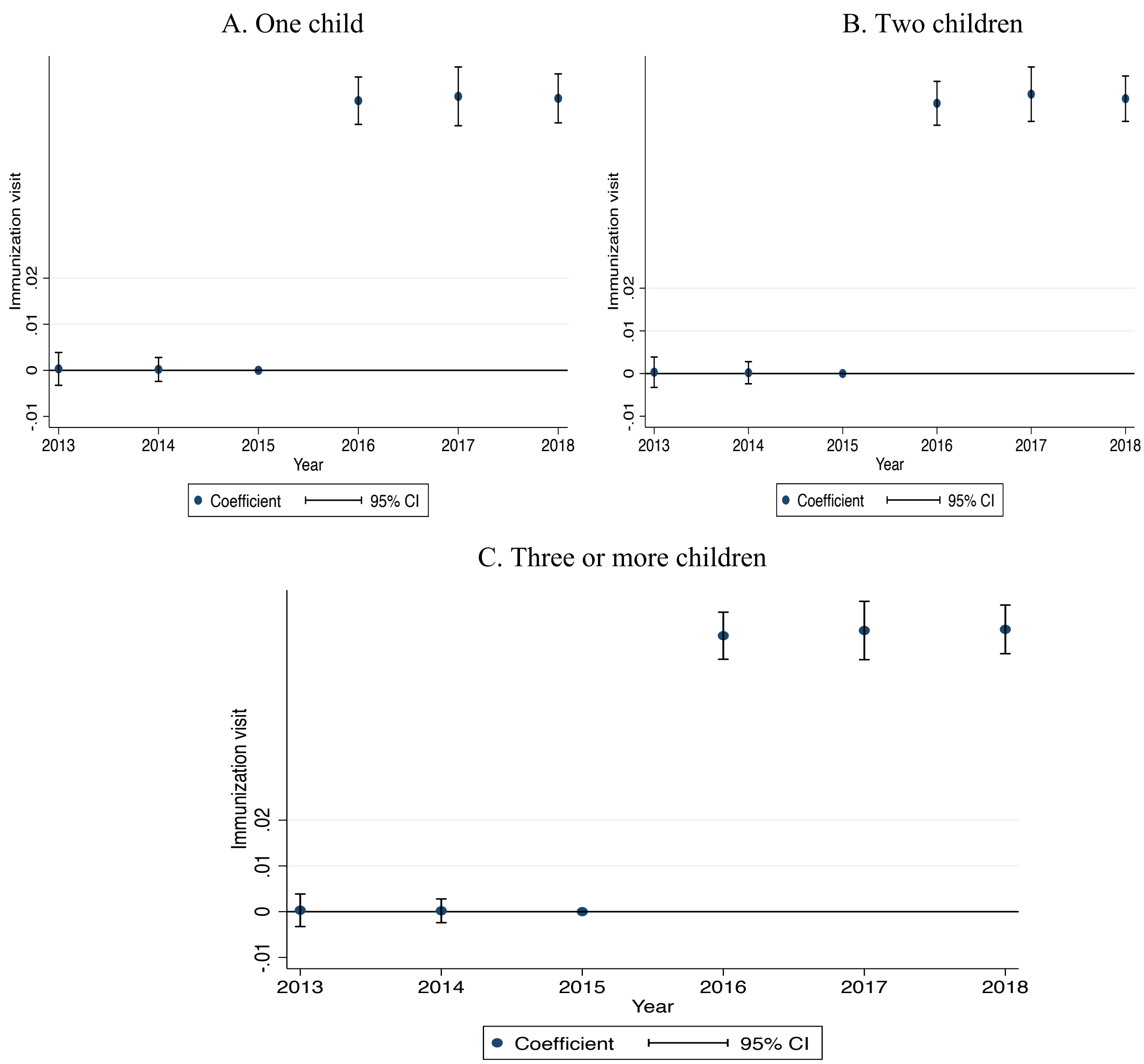


C. Three or more children



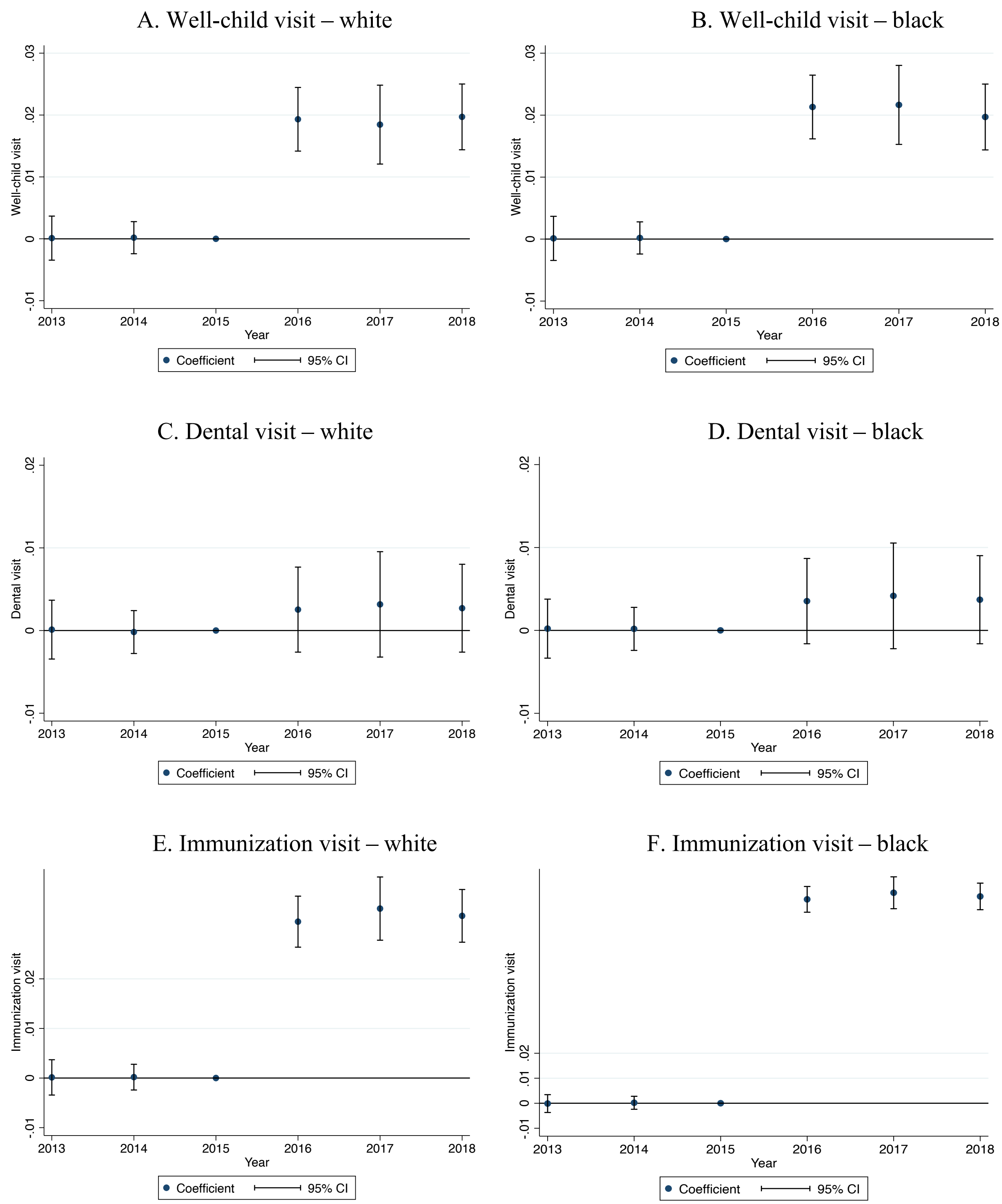
Notes: This figure displays coefficients and 95% confidence intervals of event study estimates on dental visit by the number of children. See notes to Figure 3 for details.

Figure 9. Event study estimates of the effect of Medicaid expansion on immunization visits by the number of children



Notes: This figure displays coefficients and 95% confidence intervals of event study estimates on immunization visit by the number of children. See notes to Figure 3 for details.

Figure 10. Event study estimates of the effect of Medicaid expansion on outcomes by race



Notes: This figure displays coefficients and 95% confidence intervals of event study estimates by race. See notes to Figure 3 for details.

Appendix: Additional Tables and Figures

Table A1. Status of states action on the Medicaid Expansion decision

States Adopted Medicaid Expansion (Year in Effect)	Arizona, Arkansas, California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Iowa, Maryland, Massachusetts, Kentucky, Michigan, Minnesota, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Dakota, Ohio, Oregon, Rhode Island, Vermont, Washington, West Virginia (2014) Alaska, Indiana, Pennsylvania (2015) Louisiana, Montana (2016) Maine, Virginia (2019)
States Not Adopted Medicaid Expansion	Alabama, Florida, Georgia, Idaho, Kansas, Mississippi, Missouri, Nebraska, North Carolina, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Wisconsin, Wyoming

Source: Henry J Kaiser Family Foundation

Table A2. Schedule of well-child visits/ dental exam visits by American Academy of Pediatrics (AAP)

Well-Child Visit	Dental Exam Visits
The first week visit (3 to 5 days old)	6 months – 1 year old: First dental exam
2 weeks	After 1 year old: twice a year
1 month old	
2 months old	
4 months old	
6 months old	
9 months old	
12 months old	
15 months old	
18 months old	
2 years old (24 months)	
2 ½ years old (30 months)	
once a year for ages 3 to 21	

Table A3. Age based CPT codes for well-child visits

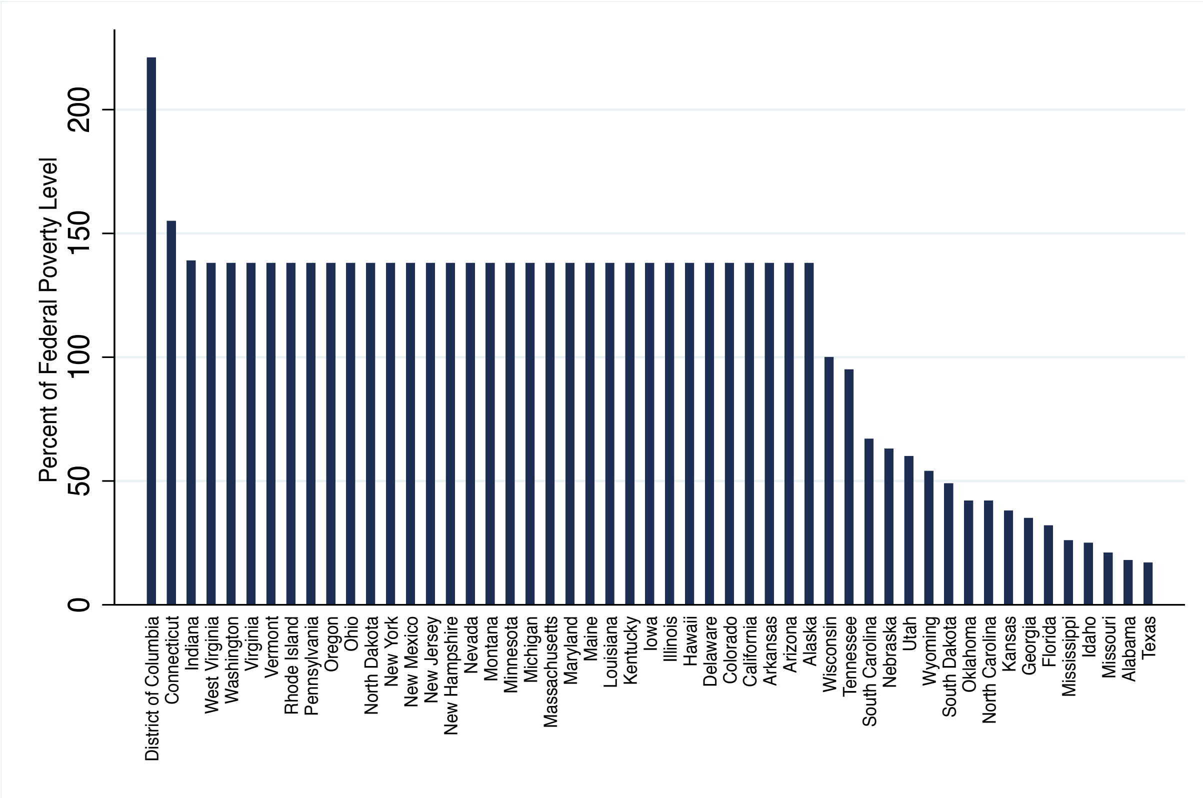
Patients' Age	CPT Code (New Patient/ Established Patient)
< 1 year	99381/99391
1 – 4 years	99382/99392
5 – 11 years	99383/99393
12 – 17 years	99384/99394
18 – 21 years	99385/99395

Table A4. Difference-in-differences estimates of the effect of parental Medicaid enrollment on well-child visits and dental exam visits among children ages 3 to 17 – logit regressions

Variables	Well-child visits		Dental exam visits	
	(1)	(2)	(1)	(2)
dy/dx	0.0190***	0.0209***	0.0010***	0.0038***
	(0.0027)	(0.0026)	(0.0006)	(0.0002)
Mean	0.435	0.435	0.359	0.359
Observations	577,639	577,639	577,639	577,639
Child's characteristics	No	Yes	No	Yes
Family's characteristics	No	Yes	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes

Notes: dy/dx refers to marginal average effect

Figure A1. Medicaid income eligibility limits for adults as a percent of January Federal Poverty Level, 2019



Data Source: Henry J Kaiser Family Foundation

Chapter 2: The Impact of Parental Involvement in Abortion Laws on Women's Education, Future Income and Labor Force Participation

Cuicui Song

Abstract: Most states require parental involvement in a minor's decision to obtain an abortion, that is, minors need to present evidence that one or both parents have been notified of or consented to the procedure before allowing an abortion to go forward. The years in which a parental involvement in abortion law is enforced vary by state. Several studies have examined the impact of parental involvement laws on abortion use and teen births, but the effect of this policy on long-term indicators such as educational attainment, employment status and future income is not well studied. Using American Community Survey (ACS) data and abortion data from Guttmacher Institute, this study investigates the impact of parental Involvement laws on young women's educational attainment, labor outcomes and future income in a difference-in-differences framework. The treatment group is comprised of women who were exposed to the policy between 15 and 17 while the control group consists of women who were not exposed to the policy. The results suggest that parental involvement laws in abortion lowers the probability of completing high school and college, they also have the negative effects on labor outcomes and future income, with particularly profound effects for young black women.

1 Introduction

Following the Supreme Court's Roe v. Wade ruling on the legalization of abortion in 1973, there was a notable rise in abortion rates among women aged 15 to 17. The rate increased from 17.1 in 1973 to 30.5 per 1,000 women in 1980. This rate remained steady at around 30.5 from 1981 to 1988. However, starting in the late 1980s, there was a substantial decline in these rates, dropping from 28.3 in 1989 to just 5.0 per 1,000 women in 2019. (Guttmacher Institute, 2020).

The decrease in minors' abortion rates may be attributed to the implementation of parental involvement laws, which require the notification or consent of one or both parents before a minor can obtain abortion services. In 1981, Louisiana, Massachusetts, and North Dakota were the first three states to enforce these laws. By 2021, 37 states had enacted similar legislation (see Appendix Table A2 and Figure 2).

Parental involvement laws can be barriers for minors using abortion service, potentially leading to a rise in the number of teenage mothers. Numerous Studies indicated that teenage mothers often face challenges in terms of education attainments and workforce engagement (Card and Wise, 1978; Mott and William, 1985; Angrist and Evans, 2000; Pop-Eleches, 2006; Bloom et al. 2009; Molland, 2016). Some studies examining the impacts of the nationwide legalization of abortion in 1973 on educational attainment and labor force participation found a negative effect, especially for black women (Angrist and Evans, 2000; Kalist, 2004; Ananat et al., 2009). Following the 1973 Roe v. Wade decision, several states implemented parental involvement laws, which have been documented to decrease abortion rates among minors. In addition to the impact of parental involvement laws on abortions, I hypothesize that the restrictions on abortion access could potentially affect women's economic outcomes, such as educational attainment, labor force participation and future income.

Parental involvement laws can impact a mother's educational attainment and employment status through three primary pathways. First, these laws can create obstacles for young people trying to access abortion services. Some young girls may feel scared to inform their parents when they become pregnant, and may delay seeking an abortion until it is too late, as many states prohibit abortions after 20 weeks of gestation (Appendix Table A2). Consequently, abortion rates may decrease, and teenage mothers may be forced to leave school due to inadequate childcare support or unaffordable daycare costs, preventing them from completing assignments and attending classes. Second, teenage mothers often face significant challenges, including social isolation, insufficient support from family, friends, and schools, which can also lead to them dropping out of school. In addition, Parental involvement laws can also have an influence on economic outcomes through an alternative mechanism involving expectations and aspirations. When a young woman anticipates having complete autonomy over her reproductive choices, including the decision of when and how many children to have, it profoundly shapes her outlook on the future. These expectations can drive her aspirations towards pursuing a professional career, thereby influencing her choices, efforts, and dedication towards education, training, participation in the workforce, and career advancement (Jones and Pineda-Torres, 2020).

In this study, I estimate the impacts of parental involvement laws across the thirty-seven states that have implemented them since 1981. Using the Public Use Microdata Sample (PUMS) data from American Community Survey (ACS) and a quasi-experimental approach, I am able to estimate the effects of exposure to parental involvement laws for women aged under 18[1]. I estimate the impacts on high school completion, college completion, labor force participation, employment status and future family income. By employing year and state fixed effects, I focus exclusively on the precise timing of parental involvement laws implementation to determine the causal effects on economic outcomes. The results suggest that parental involvement laws are associated with a reduction in education attainment, labor outcomes and family income, especially for young black women.

This work contributes to the literature in several ways. Several studies have investigated the impact of parental involvement laws on abortion use and teen birth rates (Haas-Wilson, 1996; Joyce and Kaestner, 1996; Kane and Staiger; Levine, 2003; Joyce, Kaestner and Colman, 2006; Joyce, Kaestner and Ward, 2019; Myers and Ladd, 2020). However, there is currently few literature that explores the relationship between parental involvement laws and long-term economic outcomes. Borelli (2011) is the only existing study on the impact of parental involvement laws on education attainment and labor outcomes. I expand this evidence on future family income and longer term effects. In addition, this is the first study to identify the effects of different types of parental involvement laws separately.

The remainder of the paper is organized as follows: In Section 2, I provide an overview of legal history of abortion and parental involvement laws in abortion. Section 3 presents a review of the relevant literature. Section 4 describes the data sources and sample selection criteria utilized in this study. Section 5 explains the empirical strategy. Section 6 presents and discusses the results. The conclusions and directions for future research are offered in section 7.

2 Background

2.1 Legal History of Abortion Laws in the US

In 1973, abortion became legalized throughout the country after the U.S. Supreme Court ruled that pregnant women had constitutional right to have an abortion without states' restrictions, which is known as Roe v. Wade. The court also held that a woman would not be forced to continue a pregnancy if her health or life were endangered. The right to obtain an abortion became legally protected to the same standard as freedom of speech or religion, and any law that pertained to abortion access would be subject to “strict scrutiny.” This ruling made the abortion service more accessible.

However, since the decision of Roe v. Wade, anti-abortion policymakers have passed a number of laws to restrict access to abortion. Some of these restrictions affect the availability of abortion services by focusing on the demand side – patients. This includes restrictions on Medicaid funding for abortion, mandatory provision of the information about the risks of abortion, and patients must wait a specific period between receipt of mandated information and an abortion. Parental Involvement for minors seeking an abortion is among these restrictive laws as well (Joyce, 2011; Guttmacher Institute, 2020b). The restrictions on the use of federal Medicaid funding for abortions, only allow coverage in the cases of rape, incest, or when the woman's life is at risk. In certain states, before undergoing an abortion, patients are mandated to participate in state-directed counseling. After receiving mandatory counseling, many states require patients to wait a certain period before the abortion can be carried out, leading patients to visit twice to the clinic. Some of these restrictions affect the abortion access by targeting the supply side – providers. This includes Targeted Regulation of Abortion Providers (TRAP) laws, gestational age limits and medication abortion restrictions. TRAP laws often mandate abortion clinics adhere to the same criteria as ambulatory surgical centers, which can lead to expensive adjustments that are sometimes not medically necessary for procedures conducted in these clinics. Regarding the gestational age limits, numerous states restrict to 20 weeks after fertilization or earlier, although there are exceptions considering the mother’s life or health. For the medication abortion restrictions, several states have attempted to limit medication abortions (using pills) by mandating that they be administered under the “FDA protocol” or by prohibiting the use of telehealth services for abortion.

In 1992, a significant shift occurred in the abortion regulation due to a crucial Supreme Court decision related to abortion access - Planned Parenthood v. Casey (referred to as 'Casey' hereafter). The Supreme Court, in a close 5-4 ruling, upheld the 1989 Pennsylvania Abortion Control Act. This Act comprised various provisions including state-mandated counseling, a compulsory 24-hour waiting period post-counseling before an abortion could be performed, mandatory parental consent for minors seeking abortions, stringent reporting requisites for abortion providers, and spousal notification if the woman requesting an abortion was married. Of all these requirements, only the spousal notification was ruled unconstitutional (Jones s et al., 2020; Mercier et al., 2018; Obos Abortion Contributors, 2014). Under Casey, the regulatory standards for legal abortion have changed a lot. The Casey decision marked a departure from the trimester framework established by Roe and permit states to introduce abortion regulations as long as they do not present an undue burden. The “undue burden” criteria introduced in Casey became a foundation for evaluating later abortion cases. It also recognized a state's concern for fetal life throughout pregnancy and allowed states more ways to regulate abortions. By reaffirming Roe's central holding, the Court confirmed the continued legal protection of abortion rights but acknowledged the possibility of more state restrictions. Overall, while Planned Parenthood v. Casey maintained the essential abortion rights to abortion recognized in Roe v. Wade, it also opened the door for states to implement abortion regulations, provided they do not place an "undue burden" on women seeking abortions.

Abortion laws are continually evolving in light of changes in the composition of the Supreme Court and ongoing state legislative efforts. The Guttmacher Institute reports that U.S. states have implemented 1,381 abortion restrictions since the Roe v. Wade decision (Guttmacher Institute, 2022). Furthermore, in 2022, the Supreme Court overturned both Roe v. Wade (1973) and Planned Parenthood v. Casey (1992), granting individual states the power to regulate areas of abortion not covered by federal regulations.

2.2 Legal History of Parental Involvement Laws in Abortion

Parental Involvement laws require that minors seeking abortion care must obtain the notification or consent of one or both parents. While these laws typically apply to those under the age of 18 in most states, in Delaware and Massachusetts, they apply to young women under the age of 16, and in South Carolina, they apply to those under 17. These laws can be categorized into two types: parental notification laws, which require medical providers to provide written notification to parents typically 24 to 48 hours prior to the abortion service, and parental consent laws, which require one or both parents to provide consent before a minor can access abortion services. In addition, most of these states provide a judicial bypass process for minors who are unable to notify their parents or obtain their consent. This process allows minors to obtain permission from a judge to have an abortion without parental involvement. It is worth noting that most states permit minors to access abortion in cases of medical emergencies or in situations of abuse, assault, incest, or neglect. Although a judicial bypass option is theoretically accessible in states that require parental involvement, several significant barriers can hinder young individuals from utilizing it. Many minors either aren't aware of this option or lack the knowledge to pursue it; they might not have the means to reach the relevant courts; or they might encounter uncooperative or prejudiced judges. For example, in 2013, the Nebraska Supreme Court denied a 16-year-old girl's request for an abortion, declaring she wasn't "mature" enough to make such a decision. This young woman had already faced the challenges of navigating the legal system, securing legal representation, and enduring delays as the courts deliberated – only to be told she had to continue with the pregnancy.

Parental involvement laws have been enacted in 37 states as of April 2021. Of these states, 18 require the consent of one parent (Alabama, Arizona, Arkansas, Idaho, Indiana, Kentucky, Louisiana, Massachusetts, Michigan, Missouri, Nebraska, North Carolina, Ohio, Pennsylvania, Rhode Island, South Carolina, Tennessee, Wisconsin), 3 (Kansas, Mississippi, North Dakota) require the consent of both parents, and 6 (Florida, Oklahoma, Texas, Utah, Virginia, Wyoming) require both parental notification and consent. In contrast, 9 states (Colorado, Delaware, Georgia, Illinois, Iowa, Maryland, New Hampshire, South Dakota, West Virginia) require only parental notification. Minnesota is the only state that requires both parents be notified (Guttmacher Institute, 2021).

Parental involvement laws vary across states, with each state imposing its own specific restrictions. According to the stringent criteria of the laws, this study categorizes the various of parental involvement abortion laws into four primary groups: any parental involvement laws; laws requiring parental notification only; laws requiring parental consent only and those requiring both parental notification and consent. States that mandate either parental notification or consent fall under the broader category of any parental involvement laws.

Since the early 1980s, there has been a growing trend among states to implement parental involvement laws. The enforcement dates of these laws in each state can be found in Table A1 (Myers, 2017). Figure 1 illustrates the number of states with parental involvement laws from 1981 to 2021. While only three states had implemented these regulations by 1981, the count increased to 13 by 1990, 25 by 2000, 29 by 2010, and reached 37 by 2021.

3 Related Literature

3.1 Impacts on Teenage Abortions or Birth

Various studies have investigated the effects of parental involvement laws on teenage abortions and/or births. However, the findings have been inconsistent. Most research suggested that parental involvement laws led to a decrease in the number of minors seeking abortions, while also resulting in an increase in teenage births. For instance, Haas-Wilson (1996) found that the implementation of parental involvement laws reduced the demand for abortions among minors by 13 to 25 percent. Another study by Joyce and Kaestner (1996) analyzed individual-level data from three southern states (South Carolina, Tennessee, and Virginia) to determine if these laws influenced young women's decisions to terminate their pregnancies. They concluded that only South Carolina's parental involvement laws had a negative effect on the likelihood of abortion for non-black minors who were 16 years old. Levine (2003) found that the introduction of parental involvement laws led to a decrease in minors seeking abortions. Joyce, Kaestner, and Colman (2006) studied the impact of a parental notification law enforced in Texas in 2000 on abortion and birth rates among 15 to 17-year-old teenagers. Their findings revealed a decrease in abortion rates and an increase in birth rates. Joyce, Kaestner, and Ward (2019) suggested that only parental involvement laws implemented before the mid-1990s were effective in reducing minors' abortions, and out-of-state travel did not significantly moderate the impact of such laws. However, some studies have shown no significant effect of parental involvement laws on abortion or birth rates. For example, Kane and Staiger (1996) found no significant impact of parental involvement laws on teen birth rates using county-level panel data. Levine (2003) also concluded that such laws had no clear effect on birth rates. Myers and Ladd (2020) investigated the distance minors would travel to obtain an abortion without parental notification and found that while parental involvement laws did not result in increasing teen births in the 1980s but increasing teen births in recent decades.

3.2 Nationwide Legalization of Abortion on Economic Outcomes

Several studies have explored how the legalization of abortion in the United States during the 1970s affected women's education and labor market outcomes. Angrist and Evans (2000) analyzed the impact of 1970 state abortion reforms on women's fertility, education, and employment. Their results showed that while the fertility changes had a positive effect on the education attainment and employment outcomes of black women, they did not have an impact on white women's schooling or labor market outcomes. Similarly, Kalist (2004) used a similar methodology to Angrist and Evans (2000) to investigate the impact of abortion on the likelihood of women working 40 or more weeks per year. He found that abortion legalization in the 1970s reduced fertility rates, which led to an increase in female labor force participation, particularly among black women. Ananat et al. (2009) compared cohorts that were exposed to national abortion legalization with those that were not. They found that the cohorts exposed to national abortion legalization had higher rates of college graduation and lower rates of poverty.

3.3 International Legalization of Abortion on Economic Outcomes

Some international studies have examined the impact of legal restrictions on female labor force participation. Pop-Eleches (2006) found that Romania's abortion ban increased birth rates, which led to improved educational and labor market outcomes for subsequent generations. Bloom et al. (2009) used abortion legalization as an instrument to estimate the impact of fertility on female labor force participation across 97 countries from 1960 to 2000. Their results suggested that each birth reduced a woman's labor supply by two years. Molland (2016) investigated the effects of teen access to abortion in Oslo on education attainment and labor market outcomes. He found that abortion access increased the likelihood of completing college and increased labor force participation at younger ages, between the 20s and 30s.

3.4 Differences by Race

Previous studies also show that the effects of abortion access can vary among different demographic groups. Numerous studies have found that impacts of legalizing abortion on birth rates are significantly larger for black women (or teens) compared to their white counterparts (Joyce and Mocan 1990; Gruber et al. 1999; Levine et al. 1999; Angrist and Evans 2000; Donohue et al. 2009; Ozbeklik 2014; Myers, 2017b). Myers and Ladd (2020) found that parental involvement laws led to a rise in birth rates among both white and black teenagers. A limited number of studies exploring the effects of abortion access on economic outcomes have also noted racial disparities. Angrist and Evans (2000) discovered that the influence of legalizing abortion on early childbirths leads to enhanced high school graduation rates and college admissions, particularly among black women. Both Angrist and Evans (2000) and Kalist (2004) found that the legalization of abortion affected employment rates of Black women, while the same was not true for White women. Borelli (2011) identified detrimental effects of parental involvement regulations on the educational achievements of black women, with no similar impact observed for white women. Jones and Pineda-Torres (2020) found women's exposure to a TRAP laws reduced women's educational attainment and future family income, and these impacts are robust only among black women.

Considering the evidence of the notable influence of abortion access on black women compared to other groups, it is plausible that parental involvement laws might similarly have different effects among black and white women groups. As a result, this study will examine the impacts of parental involvement laws by all women, black women and white women groups.

4 Data

The study utilized the 2015-2019 American Community Survey (ACS) Public Use Microdata Sample (PUMS) as its main data source. The PUMS contains detailed information on individual characteristics of each respondent in the ACS survey. The ACS PUMS is a weighted sample, I use population weights (pwgtp) to generate estimates and standard errors that represent the population (ACS PUMS handbook, 2019).

To identify individuals who were minors (aged 15-17) and exposed to parental involvement laws regarding abortion, I utilized year-of-birth information. The enforcement starting dates of these laws differ by state, as outlined in Appendix Table A1. For instance, Alabama has enforced these laws since 1987, so I used individuals born in 1969 or later as the treatment group. Delaware enforced these laws in 1995, so the treatment group comprised individuals born in 1977 or later. This study compares the cohorts who were exposed to the policy with those who were not within the same states to estimate the impact of parental involvement laws on education attainment and employment status. As a result, states where the policy was never enforced, such as California, Connecticut, D.C., Hawaii, Maine, Maryland, Montana, Nevada, New Jersey, New Mexico, New York, Oregon, Vermont, and Washington, were excluded from the analysis.

The primary outcomes of this study are educational attainment and employment status and real family income. Educational attainment is grouped into three categories: high school graduate, some college without a degree and bachelor's degree. For employment status, there are six categories in the raw data: Civilian employed, at work; Civilian employed, with a job but not at work; Unemployed; Armed forces, at work; Armed forces, with a job but not at work and not in labor force. I categorize this variable into two groups: in labor force and employed. Control variables at the individual level include race, age, sex and marital status. Race is recoded into three categories: White, Black, and Other. Marital status is categorized into five groups: currently married, widowed, divorced, separated, or never married. The study also includes state-level control variables, such as median household income, poverty rates, and unemployment rates. These data were obtained from the Bureau of Labor Statistics (BLS). Our analysis utilizes the varied implementation dates of parental involvement laws across states to show their effect on economic outcomes. However, there is possibility that some other policies that could also influence abortion rates or economic outcomes. To mitigate such confounding effects, I control for additional policies such as TRAP laws, state Medicaid funding for abortions, state requirement on insurance coverage for contraception, expanded Medicaid coverage for family planning (Guttmacher Institute, 2021).

Table 1 displays a summary of the statistics. The treatment group exhibits lower average rates of high school graduates, some college education, bachelor's degrees, and graduate degrees compared to the control group. For instance, 20.4% of individuals in the cohorts exposed to the policy have completed high school, while 26.4% of those not exposed to the policy possess a high school diploma. Furthermore, the treatment group has a higher mean for individuals who are not in labor force and those who are unemployed. Conversely, the employment rate is lower for the treatment group in comparison to the control group.

5 Empirical Methodology

In this study, I employ a difference-in-differences (DD) regression framework to assess the long-term effects of parental involvement laws in abortion on educational attainment, employment status and family income. The evaluation compares cohorts exposed to the policy with those not exposed to the parental involvement laws concerning abortion. I exploit the variation across states and over time in the enforcement of the parental involvement laws in abortion using the following equation:

$$Y_{ista} = \beta_0 + \beta_1 ParLaw_{ist} + \beta_2 X_{ist} + \theta_t + \gamma_s + \mu_a + \varepsilon_{ista}$$

Y_{ista} indicates the outcome for women *i* born in year *t* residing in state *s* who was age *a* at the time of interview. It includes outcomes such as high school completion, some college, bachelor's degree, labor force participation, employed and family income. $ParLaw_{ist}$ equals 1 if the person i from state s exposed to a parental involvement law between age 15 and 17. X_{ist} consists of demographic control variables such as age, race, sex, household income, marital status. It also includes state-level controls like median household income, poverty rates, unemployment rate and other related policies. θ_t represents year fixed effects, γ_s denotes state fixed effects, μ_a is age fixed effects. The standard errors are clustered at the state level to account for the correlation of errors within each state.

6 Results

The results from Table 2 to Table 8 are presented in two distinct specifications and organized similarly: In column (1), I incorporate control variables such as age, race, sex, household income, marital status. I also control for the state fixed effects, year fixed effects and age fixed effects. Column (2) additionally controls for other abortion policies, welfare and contraception policies. The table includes four panels, where each demonstrates the effects of a distinct category of Parental Involvement law: (A) any of the parental involvement laws, (B) parental notification only, (C) parental consent only, and (D) parental notification and parental consent.

6.1 The Effects of Parental Involvement Laws on women's Education Attainment

Table 2 presents the DD estimates of the impact of exposure to parental involvement laws on women's educational achievement for the full sample. Educational achievement is measured through high school completion, some college attendance, and bachelor's degree attainment. All models reveal significant effects of parental involvement laws on the likelihood of high school completion. The effects are similar across the different types of parental involvement laws. Exposure to these laws between the ages of 15 and 17 is linked to about a 0.93 percentage point decrease in the probability of completing high school. However, the impacts of parental involvement laws on the likelihood of attending some college differ for the other three types of the laws. The results in panel B indicate that the exposure to the parental notification only laws cause a 0.98 percentage decrease in the probability of attending college, while the impacts are bigger for the parental consent only law and both parental notification and consent, which corresponds to 1.05 and 1.06 percentage points. The DD estimates for obtaining a bachelor's degree are statistically significant in both models, with cohorts exposed to the parental involvement laws between 15 and 17 years old experiencing a 1.16 percentage points decrease in the probability of earning a bachelor's degree. Similarly, the effects of parental involvement laws on the likelihood of obtaining a bachelor’s degree differ for the other three types of the laws.

Tables 3 and 4 show the estimates of the impact of exposure to parental involvement laws on the educational achievement of white and black women, respectively. Exposure to any parental involvement laws between the ages of 15 and 17 is associated with a 0.92 percentage point decrease in the probability of completing high school for white women, while the estimates for black women are larger in magnitude (2.42 percentage points). A 1.06 percentage point decrease is observed in the likelihood of obtaining a bachelor's degree for white women exposed to the parental involvement laws. For black women exposed to these laws, there is a 1.73 percentage point decrease in the probability of obtaining a bachelor's degree. The impacts of exposed to the other three specific types of laws are also associated with a higher probability of completing the education attainment for black women.

6.2 The Effects of Parental Involvement Laws on Labor Market Outcomes

In this section, I estimate the impacts of parental involvement laws on labor market outcomes. Two outcomes are used in this part: the probability of being in the labor force and the probability of being currently employed.

Table 5 shows the results of the impacts of parental involvement laws on women's labor market outcomes. The DD estimates are statistically significant for both outcomes and both models. Cohorts who exposed to the parental involvement laws between 15 and 17 years old are associated with a 1.24 percentage points reduction in the probability of being in the labor force and 0.97 percentage points reduction in the probability of being employed.

Table 6 and Table 7 provides the DD estimates of the effects of parental involvement laws on women's labor market outcomes by subgroups: White women and black women. The estimates for white women are larger in magnitude compared with the estimates of the full sample, while the estimates for black women are larger in magnitude.

6.3 The Effects of Parental Involvement Laws on Income

Table 8 shows the impacts of exposure to parental involvement laws on the log of real family income (in 2015 USD). Panel A is shows that exposure to any parental involvement laws significantly decreases real family income by 0.92, 1.23 and 1.16 percent for all women, white women and black women, respectively. Regarding exposure to parental notification laws (panel B), it shows negative and significant effects on real family income. Exposure to parental consent laws (panel C) also causes a decrease in real family income by 0.9, 1.05 and 1.14 percent for different demographic groups. I do not identify significant effects of exposure to parental notification and consent laws on this outcome for all women, white women and black women groups. The results are consistent with theoretical model which suggest that women might experience lower earnings compared to those who are not exposed to parental involvement laws.

6.4 Mechanisms

According to above analysis, there is a significant effect of parental involvement laws on education attainment, labor participation and family income. The following part offers evidence that the effect of parental involvement laws on those economic outcomes is a result of decreasing abortion access and increasing early fertility.

The abortion data are obtained from Guttmacher Institute. It includes the information on the number of abortion occurrences and the number of births per 1000 women. Here, I use county level data. I will estimate the impact of parental involvement laws on abortion rates and birth rates through the following equation:

$$Y_{cst} = \theta_0 + \theta_1 ParLaw_{st} + \theta_2 X_{st} + \mu_t + \delta_c + \varepsilon_{cst}$$

where Y_{cst} represents the natural log of the number of abortions and the number of births per 1000 women for county c in state s and year t. $ParLaw_{st}$ indicates the parental involvement law was enacted in states and year t. X_{st} are controls for state-year level abortion policies. μ_t *and* δ_c are year fixed effects and county fixed effects.

In Table 9, the left panel shows the effects of parental involvement laws on aggregated abortion rates for women aged 15-24. There are no significant impacts on abortion rates among all women for every type of parental involvement laws. However, any parental involvement laws significantly reduce abortion rates by 1.4 percent for young black women. The right panel shows the impacts of parental involvement laws on birth rates. I notice that any parental involvement laws and parental notification only laws significantly increase birth rates by 1.5 and 1.3 percent among all young women. Among black women, I find there are robust impacts of every type of parental involvement laws on birth rates, increase birth rates by 1.6 to 2.9 percent.

The above results indicate that the main mechanism by which parental involvement laws affect educational, labor outcomes and future income of black women through increasing early childbirth.

7 Discussion and Conclusion

This paper studies the effects of parental involvement laws on women's education attainment, labor market outcomes and family income. The results suggest that the impact of the parental involvement laws varies by race. Consistent with previous research, young black women are the most affected. Specifically, women who exposed to any parental involvement laws between 15 and 17 years old are associated with 0.92 percent points decrease in probability of completing high school, 1.23 decrease in the probability of having some college, and 1.16 percentage points decrease in probability of obtaining a bachelor's degree. The estimates on educational attainment are larger in magnitude for black women, 2.42 percent points decrease in probability of completing high school, 2.74 decrease in the probability of having some college, and 1.73 percentage points decrease in probability of obtaining a bachelor's degree. Compared the four types of parental involvement laws, I notice more restrictive laws have larger impact on education outcomes.

The DD estimates of parental involvement laws on women's labor market outcomes show that the cohorts that exposed to the parental involvement laws will reduce both the probability of being in labor force and the probability of being employed. Among all young women, cohorts who exposed to any parental involvement laws between 15 and 17 years old are associated with a 1.24 percentage points reduction in the probability of being in the labor force and 0.97 percentage points reduction in the probability of being employed. Among black women, any parental involvement laws are associated with 2.32 and 2.71 in decreasing the probability of being in labor force and employed, respectively. Parental involvement laws lead to 0.92 percent decrease in future income for all young women, with a more pronounced decrease of 1.16 percent for young black women. According to the analysis of mechanism, parental involvement laws reduce abortion rates. Consequently, birth rates increase for women aged 15 to 24. The primary way these parental involvement laws affect economic outcomes is through increased early childbirth.

Future Work

First, use Census household data to examine the effects of parental involvement abortion laws on next generation's education and labor outcomes. Second, add sensitive analysis by changing the control group to those women who are 18-24 years old. Third, using sexual behavior data to test if the parental involvement laws affect young people's sexual behavior or contraceptive use to reduce the unwanted pregnancy. The limitation of ACS data is that the state of residence when they are minors cannot be identified, this paper just used the state of birth to proxy the state of minors' residence.

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Tables and Figures

Table 1. Summary Statistics

	Full Sample	Treatment Group	Control Group
Female	0.508	0.493	0.527
Age	38.729	22.031	59.298
White	0.758	0.721	0.804
Black	0.139	0.156	0.118
Other Race	0.103	0.123	0.078
Married	0.490	0.357	0.592
Widowed	0.060	0.004	0.104
Divorced	0.115	0.063	0.156
Separated	0.019	0.017	0.021
Never Married	0.315	0.559	0.127
High School Graduate or GED	0.901?	0.881	0.921
Some College	0.720	0.698	0.732
Bachelor’s Degree	0.407?	0.382	0.398
Graduate or Professional Degree	0.078	0.049	0.114
Family Income	47,358	45,236	48,965
Not in Labor Force	0.372	0.469	0.251
Employed	0.452	0.431	0.499
Unemployed	0.360	0.202	0.500
Observations	6,012	3,023	2989

Table 2. The effects of exposure to parental involvement laws on women's education attainment – all women

	High School Completion		Some college		Bachelor's degree	
	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: Any of the parental involvement laws						
$Parlaw_{ist}$	-0.0081***	-0.0092***	-0.0087*	-0.0123**	-0.0093**	-0.0116***
	(0.0025)	(0.0015)	(0.0055)	(0.0053)	(0.0049)	(0.0057)
Mean	0.881	0.881	0.698	0.698	0.382	0.382
Change %	-0.92	-1.04	-1.25	-1.76	-2.43	-3.04
Observations	6,812	6,812	6,812	6,812	6,812	6,812
Panel B: Parental notification only						
$Parlaw_{ist}$	-0.0082***	-0.0090***	-0.0093*	-0.0098*	-0.0121**	-0.0106**
	(0.0020)	(0.0021)	(0.0049)	(0.0057)	(0.0049)	(0.0050)
Mean	0.892	0.892	0.702	0.702	0.378	0.378
Change %	-0.92	-1.01	-1.32	-1.40	-3.20	-2.80
Observations	1,898	1,898	1,898	1,898	1,898	1,898
Panel C: Parental consent only						
$Parlaw_{ist}$	-0.0089***	-0.0093***	-0.0092*	-0.0105**	-0.0112*	-0.0114*
	(0.0010)	(0.0012)	(0.0059)	(0.0047)	(0.0058)	(0.0059)
Mean	0.836	0.836	0.679	0.679	0.356	0.356
Change %	-1.06	-1.11	-1.35	-1.55	-3.15	-3.20
Observations	3,358	3,358	3,358	3,358	3,358	3,358
Panel D: Parental notification and consent						
$Parlaw_{ist}$	-0.0088***	-0.0093***	-0.0103**	-0.0106**	-0.0099*	-0.0122*
	(0.0015)	(0.0013)	(0.0046)	(0.0047)	(0.0046)	(0.0051)
Mean	0.825	0.825	0.653	0.653	0.324	0.324
Change %	-1.07	-1.13	-1.58	-1.62	-3.06	-3.77
Observations	1,556	1,556	1,556	1,556	1,556	1,556
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Related policy control	No	Yes	No	Yes	No	Yes

Note: Dependent variable is an indicator variable of completing high school, attending college and obtaining bachelor's degree. Parental notification/consent means any parental involvement laws in abortion. Related policies include Targeted Regulation of Abortion Providers Law, exposure to state Medicaid funding for abortions, state exposure to insurance mandates to cover contraception and expanded Medicaid coverage for family planning. Standard errors are clustered at the state-of -birth level.

* 10% significance level, ** 5% significance level, *** 1% significance level.

Table 3. The effects of exposure to parental involvement laws on women's education attainment – white women

	High School Completion		Some college		Bachelor's degree	
	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: Any of the parental involvement laws						
$Parlaw_{ist}$	-0.0080**	-0.0092***	-0.0095*	-0.0112**	-0.0092**	-0.0106**
	(0.0037)	(0.0025)	(0.0055)	(0.0043)	(0.0042)	(0.0048)
Mean	0.883	0.883	0.712	0.712	0.402	0.402
Change %	-0.96	-1.04	-1.33	-1.57	-2.29	-2.64
Observations	5,177	5,177	5,177	5,177	5,177	5,177
Panel B: Parental notification only						
$Parlaw_{ist}$	-0.0081***	-0.0083***	-0.0094*	-0.0098*	-0.0119**	-0.0121**
	(0.0020)	(0.0021)	(0.0049)	(0.0056)	(0.0049)	(0.0050)
Mean	0.894	0.894	0.705	0.705	0.398	0.398
Change %	-0.91	-0.93	-1.33	-1.39	-2.99	-3.04
Observations	1,450	1,450	1,450	1,450	1,450	1,450
Panel C: Parental consent only						
$Parlaw_{ist}$	-0.0087***	-0.0094***	-0.0108*	-0.0112**	-0.0109*	-0.0113*
	(0.0010)	(0.0012)	(0.0049)	(0.0047)	(0.0057)	(0.0056)
Mean	0.837	0.837	0.685	0.685	0.368	0.368
Change %	-1.04	-1.12	-1.58	-1.64	-2.96	-3.07
Observations	2,537	2,537	2,537	2,537	2,537	2,537
Panel D: Parental notification and consent						
$Parlaw_{ist}$	-0.0090***	-0.0097***	-0.0105*	-0.0109***	-0.0104*	-0.0107**
	(0.0015)	(0.0013)	(0.0056)	(0.0047)	(0.0045)	(0.0048)
Mean	0.832	0.832	0.659	0.659	0.331	0.331
Change %	-1.08	-1.17	-1.59	-1.65	-3.14	-3.23
Observations	1,190	1,190	1,190	1,190	1,190	1,190
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Related policy	No	Yes	No	Yes	No	Yes

Note: Dependent variable is an indicator variable of completing high school, attending college and obtaining bachelor's degree. Parental notification/consent means any parental involvement laws in abortion. Related policies include Targeted Regulation of Abortion Providers Law, exposure to state Medicaid funding for abortions, state exposure to insurance mandates to cover contraception and expanded Medicaid coverage for family planning. Standard errors are clustered at the state-of -birth level.

* 10% significance level, ** 5% significance level, *** 1% significance level

Table 4. The effects of exposure to parental involvement laws on women's education attainment – black women

	High School Completion		Some college		Bachelor's degree	
	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: Any of the parental involvement laws						
$Parlaw_{ist}$	-0.0238**	-0.0242**	-0.0271**	-0.0274**	-0.0173***	-0.0176***
	(0.0092)	(0.0096)	(0.0123)	(0.0119)	(0.0064)	(0.0065)
Mean	0.881	0.881	0.698	0.698	0.387	0.387
Change %	-2.70	-2.75	-3.88	-3.93	-4.47	-4.55
Observations	1,635	1,635	1,635	1,635	1,635	1,635
Panel B: Parental notification only						
$Parlaw_{ist}$	-0.0248**	-0.0263**	-0.0180	0.0185	-0.0252***	-0.0253**
	(0.0097)	(0.0106)	(0.0129)	(0.0134)	(0.0089)	(0.0081)
Mean	0.892	0.892	0.702	0.702	0.398	0.398
Change %	-2.78	-2.95	-2.56	-2.64	-6.33	-6.36
Observations	458	458	458	458	458	458
Panel C: Parental consent only						
$Parlaw_{ist}$	-0.0247**	-0.0267**	0.0265**	0.0278*	-0.0189***	-0.0196***
	(0.0089)	(0.0107)	(0.0121)	(0.0145)	(0.0063)	(0.0065)
Mean	0.836	0.836	0.679	0.679	0.368	0.368
Change %	-2.95	-3.19	-3.90	-4.09	-5.14	-5.33
Observations	801	801	801	801	801	801
Panel D: Parental notification and consent						
$Parlaw_{ist}$	-0.0287***	-0.0286***	-0.0159	-0.0167	-0.0213**	-0.0221**
	(0.0097)	(0.0098)	(0.0121)	(0.0110)	(0.0081)	(0.0102)
Mean	0.825	0.825	0.653	0.653	0.331	0.331
Change %	-3.48	-3.47	-2.43	-2.56	-6.44	-6.68
Observations	376	376	376	376	376	376
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Related policy	No	Yes	No	Yes	No	Yes

Note: Dependent variable is an indicator variable of completing high school, attending college and obtaining bachelor's degree. Parental notification/consent means any parental involvement laws in abortion. Related policies include Targeted Regulation of Abortion Providers Law, mandatory delay laws, exposure to state Medicaid funding for abortions, state exposure to insurance mandates to cover contraception and expanded Medicaid coverage for family planning. Standard errors are clustered at the state-of -birth level.

* 10% significance level, ** 5% significance level, *** 1% significance level.

Table 5. The effects of parental involvement laws on women's labor market outcomes – all women

	Labor Force		Employed	
	(1)	(2)	(1)	(2)
Panel A: Any of the parental involvement laws				
$Parlaw_{ist}$	-0.0119*	-0.0124*	-0.0088*	-0.0097*
	(0.0058)	(0.0065)	(0.0055)	(0.0059)
Mean	0.831	0.831	0.701	0.701
Change %	-1.43	-1.49	-1.26	-1.38
Observations	6,812	6,812	6,812	6,812
Panel B: Parental notification only				
$Parlaw_{ist}$	-0.0090*	-0.0093*	-0.0092*	-0.0098*
	(0.0058)	(0.0060)	(0.0047)	(0.0056)
Mean	0.832	0.832	0.702	0.702
Change %	-1.08	-1.12	-1.31	-1.40
Observations	1,898	1,898	1,898	1,898
Panel C: Parental consent only				
$Parlaw_{ist}$	-0.0118*	-0.0120*	-0.0094*	-0.0107**
	(0.0058)	(0.0065)	(0.0061)	(0.0048)
Mean	0.836	0.836	0.689	0.689
Change %	-1.41	-1.44	-1.36	-1.55
Observations	3,358	3,358	3,358	3,358
Panel D: Parental notification and consent				
$Parlaw_{ist}$	-0.0119*	-0.0123*	-0.0105**	-0.0112**
	(0.0056)	(0.0067)	(0.0049)	(0.0045)
Mean	0.829	0.829	0.656	0.656
Change %	-1.44	-1.48	-1.60	-1.71
Observations	1,556	1,556	1,556	1,556
Year fixed effects	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes
Related policy controls	No	Yes	No	Yes

Note: Dependent variable is an indicator variable of labor force and employed. Parental notification/consent means any parental involvement laws in abortion. Related policies include Targeted Regulation of Abortion Providers Law, mandatory delay laws, exposure to state Medicaid funding for abortions, state exposure to insurance mandates to cover contraception and availability of emergency contraception over-the-counter. Standard errors are clustered at the state-of -birth level.

* 10% significance level, ** 5% significance level, *** 1% significance level.

Table 6. The effects of parental involvement laws on women's labor market outcomes – white women

	Labor Force		Employed	
	(1)	(2)	(1)	(2)
Panel A: Parental notification/consent				
$Parlaw_{ist}$	-0.0120*	-0.0126*	-0.0095*	-0.0112**
	(0.0059)	(0.0070)	(0.0055)	(0.0043)
Mean	0.852	0.852	0.732	0.732
Change %	-1.41	-1.48	-1.30	-1.53
Observations	5,177	5,177	5,177	5,177
Panel B: Parental notification only				
$Parlaw_{ist}$	-0.0093*	-0.0096*	-0.0094*	-0.0098*
	(0.0050)	(0.0057)	(0.0049)	(0.0056)
Mean	0.853	0.853	0.725	0.725
Change %	-1.09	-1.13	-1.30	-1.35
Observations	1,450	1,450	1,450	1,450
Panel C: Parental consent only				
$Parlaw_{ist}$	-0.0119*	-0.0120*	-0.0108*	-0.0112**
	(0.0060)	(0.0064)	(0.0049)	(0.0047)
Mean	0.847	0.847	0.724	0.724
Change %	-1.40	-1.42	-1.49	-1.55
Observations	2,537	2,537	2,537	2,537
Panel D: Parental notification and consent				
$Parlaw_{ist}$	-0.0120*	-0.0124*	-0.0105*	-0.0109***
	(0.0061)	(0.0068)	(0.0056)	(0.0047)
Mean	0.842	0.842	0.719	0.719
Change %	-1.43	-1.47	-1.46	-1.52
Observations	1,190	1,190	1,190	1,190
Year fixed effects	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes
Related policy controls	No	Yes	No	Yes

Note: Dependent variable is Dependent variable is an indicator variable of labor force and employed. Parental notification/consent means any parental involvement laws in abortion. Related policies include Targeted Regulation of Abortion Providers Law, mandatory delay laws, exposure to state Medicaid funding for abortions, state exposure to insurance mandates to cover contraception and expanded Medicaid coverage for family planning. Standard errors are clustered at the state-of-birth level.

* 10% significance level, ** 5% significance level, *** 1% significance level.

Table 7. The effects of parental involvement laws on women's labor market outcomes – black Women

	Labor Force		Employed	
	(1)	(2)	(1)	(2)
Panel A: Any of the parental involvement laws				
$Parlaw_{ist}$	-0.0238**	-0.0232**	-0.0271**	-0.0268**
	(0.0092)	(0.0096)	(0.0123)	(0.0119)
Mean	0.881	0.881	0.698	0.698
Change %	-2.70	-2.63	-3.88	-3.84
Observations	1,635	1,635	1,635	1,635
Panel B: Parental notification only				
$Parlaw_{ist}$	-0.0248**	-0.0263**	-0.0180	0.0185
	(0.0097)	(0.0106)	(0.0129)	(0.0134)
Mean	0.892	0.892	0.702	0.702
Change %	-2.78	-2.95	-2.56	-2.64
Observations	458	458	458	458
Panel C: Parental consent only				
$Parlaw_{ist}$	-0.0247**	-0.0267**	-0.0265**	-0.0278*
	(0.0089)	(0.0107)	(0.0121)	(0.0145)
Mean	0.836	0.836	0.679	0.679
Change %	-2.95	-3.19	-3.90	-4.09
Observations	801	801	801	801
Panel D: Parental notification and consent				
$Parlaw_{ist}$	-0.0287***	-0.0286***	-0.0159	-0.0167
	(0.0097)	(0.0098)	(0.0121)	(0.0110)
Mean	0.825	0.825	0.653	0.653
Change %	-3.48	-3.47	-2.43	-2.56
Observations	376	376	376	376
Year fixed effects	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes
Related policy controls	No	Yes	No	Yes

Note: Dependent variable is an indicator variable of labor force and employed. Parental notification/consent means any parental involvement laws in abortion. Related policies include Targeted Regulation of Abortion Providers Law, mandatory delay laws, exposure to state Medicaid funding for abortions, state exposure to insurance mandates to cover contraception and expanded Medicaid coverage for family planning. Standard errors are clustered at the state-of -birth level.

* 10% significance level, ** 5% significance level, *** 1% significance level.

Table 8. The effects of parental involvement laws on the log of real family income

	All women		White Women		Black Women	
	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: Parental notification/consent						
$Parlaw_{ist}$	-0.0081***	-0.0092***	-0.0087*	-0.0123**	-0.0093**	-0.0116**
	(0.0025)	(0.0015)	(0.0055)	(0.0053)	(0.0049)	(0.0077)
Observations	6,812	6,812	6,812	6,812	6,812	6,812
Panel B: Parental notification only						
$Parlaw_{ist}$	-0.0082***	-0.0090***	-0.0093*	-0.0098*	-0.0121**	-0.0122**
	(0.0020)	(0.0021)	(0.0049)	(0.0057)	(0.0049)	(0.0050)
Observations	1,898	1,898	1,898	1,898	1,898	1,898
Panel C: Parental consent only						
$Parlaw_{ist}$	-0.0089***	-0.0093***	-0.0092*	-0.0105**	-0.0112*	-0.0114*
	(0.0010)	(0.0012)	(0.0059)	(0.0047)	(0.0058)	(0.0059)
Observations	3,358	3,358	3,358	3,358	3,358	3,358
Panel D: Parental notification and consent						
$Parlaw_{ist}$	-0.0088	-0.0093	-0.0103	-0.0106	-0.0099	-0.0106
	(0.0075)	(0.0083)	(0.0096)	(0.0087)	(0.0086)	(0.0091)
Observations	1,556	1,556	1,556	1,556	1,556	1,556
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Related policy	No	Yes	No	Yes	No	Yes

Note: Dependent variable is proportion of women in cell with real family income. Parental notification/consent means any parental involvement laws in abortion. Related policies include Targeted Regulation of Abortion Providers Law, mandatory delay laws, exposure to state Medicaid funding for abortions, state exposure to insurance mandates to cover contraception and expanded Medicaid coverage for family planning. Standard errors are clustered at the state-of -birth level.

* 10% significance level, ** 5% significance level, *** 1% significance level.

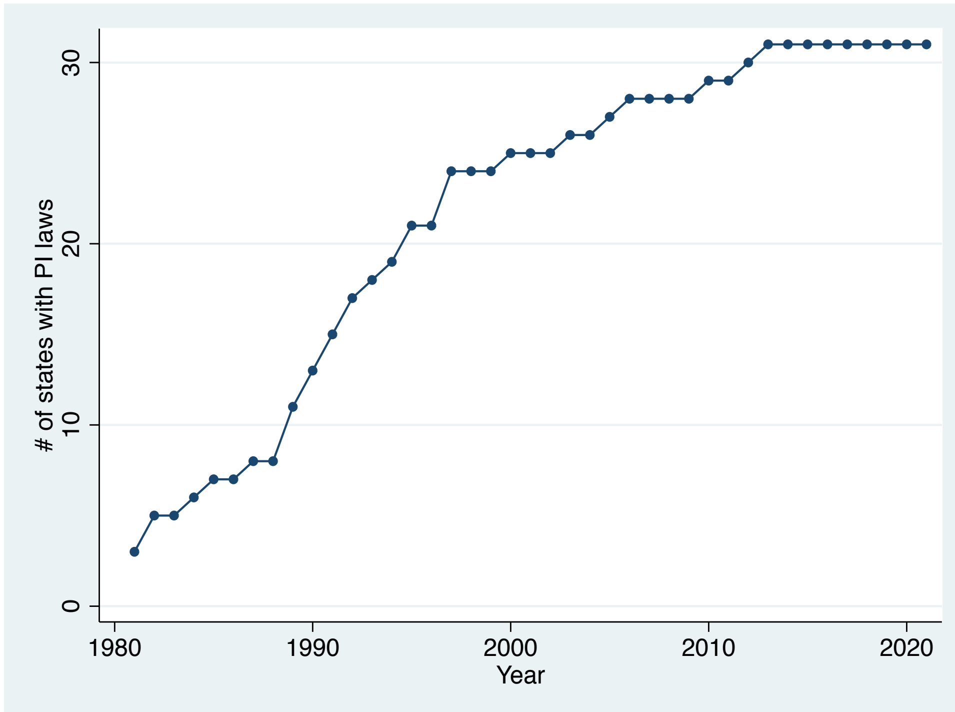
Table 9. The effects of parental involvement laws on abortion rates and birth rates

	Abortion rates		Birth rates	
	All women	Black women	All women	Black women
Any parental involvement laws	-0.0438 (0.0352)	-0.0142 (0.0126)	0.0150** (0.0053)	0.0224* (0.0109)
Parental notification only	-0.0249 (0.0197)	-0.0263 (0.0106)	0.0132** (0.0059)	0.0285* (0.0134)
Parental consent only	-0.0237 (0.0189)	-0.0267 (0.0168)	0.0263 (0.0219)	0.0278* (0.0145)
Parental notification and consent	-0.0507 (0.0397)	-0.0616 (0.0498)	0.0149 (0.0121)	0.0158** (0.0059)
Year fixed effects	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes
Related policy controls	Yes	Yes	Yes	Yes

Notes: The dependent variable is the log of the number of abortions and births for women aged 15-24. Related policies include Targeted Regulation of Abortion Providers Law, mandatory delay laws, exposure to state Medicaid funding for abortions, state exposure to insurance mandates to cover contraception and expanded Medicaid coverage for family planning. Standard errors are clustered at the state level.

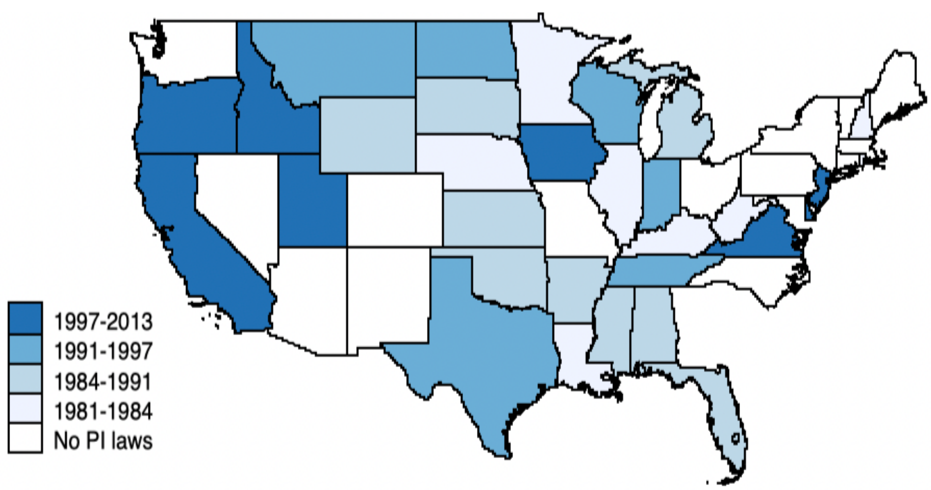
* 10% significance level, ** 5% significance level, *** 1% significance level.

Figure 1. The Number of states with parental involvement laws 1981 -2020



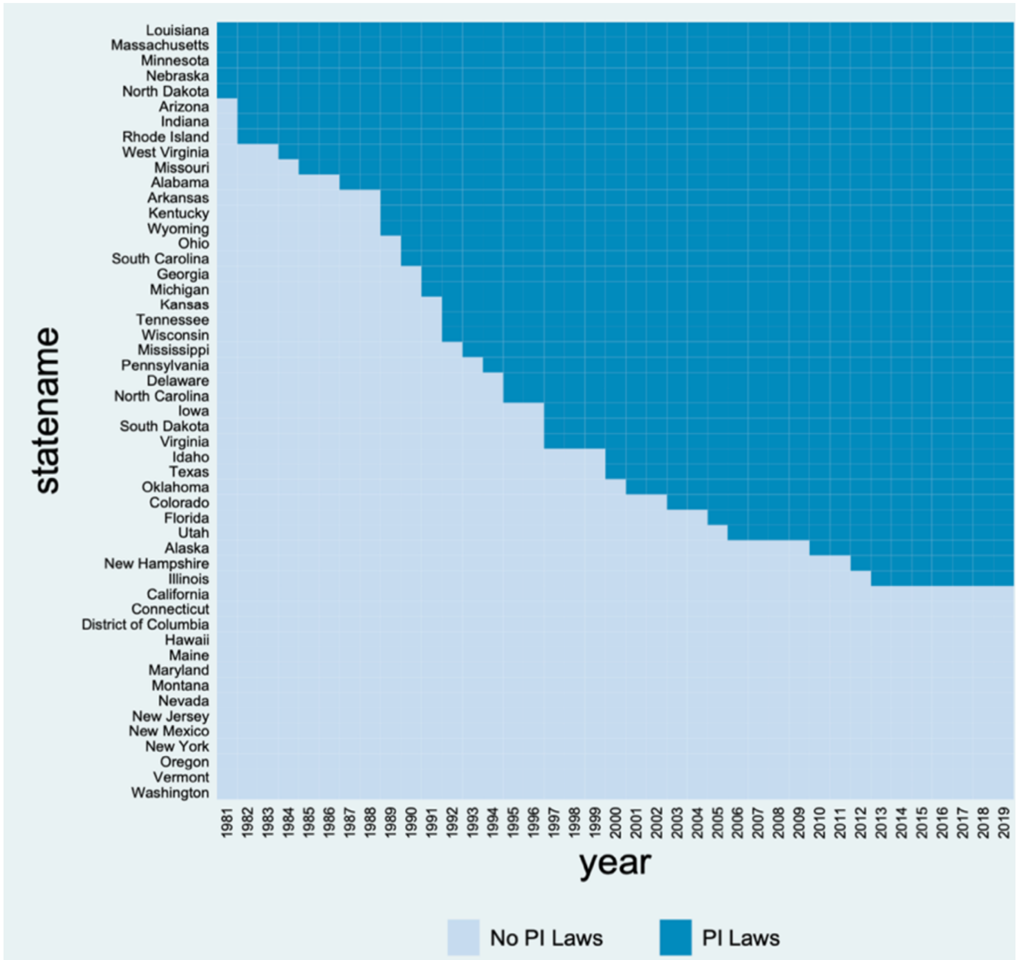
Note: This figure is based on the legal coding of parental involvement laws by Myers (2017b) and Guttmacher Institute (2020). Only include those states that consecutively enacted PI laws.

Figure 2. Enforcement of Parental Involvement Laws Map



Note: This figure is based on the legal coding of parental involvement laws by Myers (2017b) and Guttmacher Institute (2020).

Figure 3. States enacted parental involvements by year



Note: This figure is based on the legal coding of parental involvement laws by Myers (2017) and Guttmacher Institute (2020)

Appendix:

Table A1. Dates of enforcement of parental involvement laws, 1980-2020

State		Years	Treatment group
Alabama	AL	1987-present	1970 -1972 and after
Alaska	AK	2010-present	1993 and after
Arizona	AZ	1982-1987; 2003-present	1965-1972; 1986 and after
Arkansas	AR	1989-present	1972 and after
Colorado	CO	2003-present	1986 and after
Delaware	DE	1995-present	1978 and after
Florida	FL	2005-present	1988 and after
Georgia	GA	1991-present	1974 and after
Idaho	ID	2000-2004; 2007-present	1983-1990; 1990 and after
Illinois	IL	2013-present	1996 and after
Indiana	IN	1982-present	1965 and after
Iowa	IA	1997-present	1980 and after
Kansas	KS	1992-present	1975 and after
Kentucky	KY	1989-present	1972 and after
Louisiana	LA	1981-present	1964 and after
Massachusetts	MA	1981-present	1964 and after
Michigan	MI	1991-present	1974 and after
Minnesota	MN	1981-1986; 1990-present	1964-1972; 1973 and after
Mississippi	MS	1993 -present	1976 and after
Missouri	MO	1985-present	1968 and after
Nebraska	NE	1981-1983; 1991-present	1964-1969, 1974 and after
New Hampshire	NH	2012-present	1985 and after
North Carolina	NC	1995-present	1978 and after
North Dakota	ND	1981-present	1964 and after
Ohio	OH	1990-present	1973 and after
Oklahoma	OK	2001-2002; 2004-present	1984-1986; 1987 and after
Pennsylvania	PA	1994-present	1977 and after
Rhode Island	RI	1982-present	1965 and after
South Carolina	SC	1990-present	1973 and after
South Dakota	SD	1997-present	1980 and after
Tennessee	TN	1992-1996; 2000-present	1975-1982; 1983 and after
Texas	TX	2000-present	1983 and after
Utah	UT	2006-present	1989 and after
Virginia	VA	1997-present	1980 and after
West Virginia	WV	1984-present	1967 and after
Wisconsin	WI	1992-present	1975 and after
Wyoming	WY	1989-present	1972 and after

Data Source: Myers (2017b) and Guttmacher Institute (2020)

Notes: This table includes all states that have enacted parental involvement laws on abortion.

Table A2. States that have consecutively enacted parental involvement laws

State		Years	Treatment group
Alabama	AL	1987-present	1970 and after
Alaska	AK	2010-present	1993 and after
Arkansas	AR	1989-present	1972 and after
Colorado	CO	2003-present	1986 and after
Delaware	DE	1995-present	1978 and after
Florida	FL	2005-present	1988 and after
Georgia	GA	1991-present	1974 and after
Illinois	IL	2013-present	1996 and after
Indiana	IN	1982-present	1965 and after
Iowa	IA	1997-present	1980 and after
Kansas	KS	1992-present	1975 and after
Kentucky	KY	1989-present	1972 and after
Louisiana	LA	1981-present	1964 and after
Massachusetts	MA	1981-present	1964 and after
Michigan	MI	1991-present	1974 and after
Mississippi	MS	1993 -present	1976 and after
Missouri	MO	1985-present	1968 and after
New Hampshire	NH	2012-present	1985 and after
North Carolina	NC	1995-present	1978 and after
North Dakota	ND	1981-present	1964 and after
Ohio	OH	1990-present	1973 and after
Pennsylvania	PA	1994-present	1977 and after
Rhode Island	RI	1982-present	1965 and after
South Carolina	SC	1990-present	1973 and after
South Dakota	SD	1997-present	1980 and after
Texas	TX	2000-present	1983 and after
Utah	UT	2006-present	1989 and after
Virginia	VA	1997-present	1980 and after
West Virginia	WV	1984-present	1967 and after
Wisconsin	WI	1992-present	1975 and after
Wyoming	WY	1989-present	1972 and after

Data Source: Myers (2017b) and Guttmacher Institute (2020)
Note: This table only includes states that have consecutively enacted parental involvement laws on abortion.

Table A3. State abortion bans by gestational age

Gestational Age	State
20 weeks	MS
22 weeks	AL, AR, GA, IN, IA, KS, KY, LA, NE, ND, OH, OK, SC, SD, TX, WV, WI
24 weeks	FL, MA, NV, PA
Third trimester	VA
At viability (24-28 weeks)	AZ, CA, CT, DE, HI, ID, IL, ME, MD, MI, MN, MO, MT, NY, NC, RI, TN, UT, WA, WY

Data Source: Guttmacher Institute, 2020

Chapter 3: In Debt and Alone? How Student Loans Shape Marriage and Childbearing in Young Adulthood

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ABSTRACT: Student loan debt in the U.S. reached $1.5 trillion in the first quarter of 2019, which is an increase of 116% in 10 years. In 2018, the median age for a first marriage was 28 for women and 30 for men – roughly 8 years more than the median ages in the 1960s. Some mechanisms suggest that student loan debts may contribute to the delay of first marriage or first childbirth. Student loan debt can represent a certain buffer amount of wealth that people seek to accumulate before starting a family. Also, young adults may need to work more to make the loan payments and have less time for social and dating a potential spouse. Having student loan debt makes one person less competitive in the marriage market. This paper uses data from the 1997 cohort of National Longitudinal Survey of Youth to examine if student loan debt delays first marriage and first childbirth, and to evaluate the effect of student debt on the probability of first marriage and first childbirth. We first use the hazard model to evaluate whether young adults' education loan debt delays first marriage and having a first child. Considering the nonrandom selection of student loans, we also use the tuition of each state's flagship university and the distance to each state's flagship university for each respondent as the instrumental variables to evaluate the effect of student debt on the probability of first marriage and the probability of having a first child. The results of hazard model indicate that students who own student loan are 9.2% less likely to have first marriage and 9.1% less likely to have a first child, while the IV results suggest that students who own student loan debt are 19% less likely to get married and 8.4% less likely to have a child

1 Introduction

The amount of student loan debt more than doubled over the past ten years, from $700 billion to more than $1.5 trillion. Student loan debt is now the second-highest consumer debt category-behind only mortgage debt. During the same period, more young adults delayed their first marriage and first childbearing. The median age at first marriage for men increased from 27 in 2009 to 30 in 2019. It rose from 26 to 28 among women (U.S. Census Bureau). The average age of first-time mothers increased to 26.6 in 2016 (National Center for Health Statistics) compared to 24 in 1990. Student loan debt is an important factor to affect young adults' decisions about getting married or having a child. According to a survey conducted by American Student Assistance, 21% said that they postpone marriage as a result of their student loans, and 28% indicated that student debt has put off their decision to have a child. Delayed marriage and childbearing might have negative effects such as more single mothers, more high-risk pregnancies, and less labor force participation in the future. Therefore, understanding the relationship between student loan and family formation will provide valuable information for policymakers to make decisions on the inform of tuition and financial aid.

Previous literature examined the relationship between student loan debt and family formation, including marriage and childbearing. Most studies found that young people with student loan debt are more likely to delay their first marriage and first childbearing ((Marks, 2009; Dew and Price, 2011; Bozick and Estacion, 2014; Addo, 2014; Nau et al., 2015; Gicheva, 2016; Min and Taylor, 2018; Sieg and Wang, 2018; Addo et al., 2019; Velez et al., 2019). Only Velez et al. (2019) use enrollment- weighted average of in-state tuition of all public institutions as an instrumental variable to correct for the endogeneity prob- lem. However, they used the 2008/12 Baccalaureate and Beyond Longitudinal Study data and only focused on bachelor students who graduated in 2007-

2008. In this paper, we use data from the 1997 cohort National Longitudinal Survey of Youth (NLSY97), and our sample includes bachelor students, master students, and Ph.D. students from 1997 to 2015, which is a better source to examine the relationship between student loan debt and family formation.

There are three potential pathways that can explain student loan debt would increase young adults' age at first marriage and childbearing. First, there is a direct effect of student loans on marital and childbearing choices. There are fixed costs associated with marriage and childbirth, like wedding cost, the purchase of a home and household equipment, costs of childbirth and childcare, student loan debt would delay the young adults obtaining these necessary prerequisites, and thus delay their marriage and childbearing (Mira and Ahn, 2001). Second, student loan debt may indirectly affect young adults' behaviors and deci- sions on family formation through working longer hours. In order to make the required loan payments, the young adults need to work longer hours and have less time to spend on social activities that may lead to meeting a spouse (Gicheva, 2016). Finally, individuals with student loan debt are less competitive in the marriage market.

In this paper, we use data from NLSY97 to explore the relationship between student loan debt and family formation. Firstly, we evaluate the dynamic relationship between young adults' student loan debt and transitioning to their first marriage and first childbearing, using a discrete-time hazard model with discrete intervals measured in person-years, based on observable time-varying and invariant characteristics such as sex, race, cohabitation status, education attainment, and average income. Second, to correct for endogeneity problems arising from unobservable characteristics that related to graduate student loan debt, we use average region-year tuition as an instrumental variable to estimate the impact of student loan debt on the probability of first marriage and the probability of having a first child. We also examined whether the association between student loan debt and family formation differs by race and gender. The results of hazard model indicate that students who own student loan are 9.2% less likely to have the first marriage and 9.1% less likely to have a first child, while the IV results suggest that students who own student loan debt are 19% less likely to get married and 8.4% less likely to have a child.

This paper contributes to the literature in two ways. First, we estimate the effect using a nationally representative sample, which includes both undergraduate students and graduate students. Much of the previous research either focuses on undergraduate students or some specific majors, like MBA students and law school students. Second, we use the instrumental variables estimation strategy to address the endogeneity of student loan debt, which could minimize the influence of unobservable borrower characteristics.

The rest of this paper is organized as follows. Section 2 describes the trends in student loans, marriage, and childbearing. Section 3 reviews the related literature. Section 4 discusses the conceptual framework. Section 5 discusses the identification strategy. Section 6 describes the data source and presents summary statistics. Section 7 summarizes the empirical results. Section 8 provides conclusions.

2 Trends in Student Loan, Marriage, and Childbearing

2.1 Trends in Student Loan

Many students choose to borrow student loans paying the cost of college. There is a big increase in both the number of student borrowers and in the volume of total student loans since the early 2000s. There are 43 million borrowers who have student loans in 2020. As figure 1 shows, the total amount of student loan debt in the U.S. reached $1.69 trillion, an increase of 112 percent between 2006 and 2020.

In the United States, the student loan debt is currently the second-highest consumer debt category-behind only mortgage debt. The average amount of federal loans per full-time equivalent undergraduate student in 2017 dollars increased from $3,540 in the 1997- 98 academic year to $4,510 in 2017-18 academic year, while the corresponding change for master's students was from $10, 010 to $17,990 (College Board Research, 2019). The growth in student loan debt would potentially affect young adults' post-college decision making on marriage and having children.

2.2 Trends in Marriage and Childbearing

The share of married young adults decreased consistently from 1960 to 2016. Half of American young adults ages 18 and older were married in 2016, which is down 9 percent- age points since 1990 (Pew Research Center Analysis of Decennial Censuses). One factor that causes this change is that the young adults delay their marriage. Figure 2 presents the mean age at first marriage for both men and women in the United States, spanning from 1890 to 2020. As of 2020, the median age at which men first married had increased to 30 years, a rise from 26.1 years in 1990. Similarly, the median age at first marriage for women was 28.2 years in 2020, showing an increase from 23.9 years in 1990. Another explanation for the decline of the marriage rate is that more adults choose to live with a partner rather than getting married. The number of American adults in the cohabiting relationship rose from 14 million in 2007 to 20.1 million in 2021 (NCFMR analysis of Current Population Survey, 2021). About 30 percent of cohabiting adults indicated that they put off marriage as a result of their financial problems (Survey of U.S. Adults, 2020).

Since 2007, fertility rates in the United States declined steadily and hit a record low, approximately 60 births per 1,000 women in 2017 (National Vital Statistics Reports). One explanation for the decline in fertility rates is that women delayed their childbearing: The average age of women becoming mothers is 26.3 in 2014, compared with 23 in 1980 (Figure 2). The average age to have the first baby varies by marital status, education and where they live. For

example, the mean age of the first childbearing for married women is 28.8 years old, compared to 23.1 years for unmarried women. Women with a college degree have a first child at 30.3 years old on average, compared to 23.8 years for women without a college degree. The average age of first-time mothers is older in larger, more expansive cities.In addition, the average number of children per woman dropped from more than three in the 1970s to about two in 2016 (U.S. Census Bureau). According to a survey conducted by American Student Assistance, 28 percent said that the student loan debt had delayed their decisions to have a child.

3 Literature Review

There are a number of studies having examined the relationship between student loan debt and family formation. We will describe the existing research in two parts: the effects of student loan debt on decision making about marriage, and the effects of student loan debt on decision making about childbearing.

Several studies found that student loan debt was negatively associated with the age of the first marriage (Dew and Price, 2011; Bozick and Estacion, 2014; Addo, 2014; Gicheva, 2016; Sieg and Wang, 2018; Addo et al., 2019; Velez et al., 2019). Dew and Price (2011) found that visible financial marker such as the value of one's care positively predicted the likelihood of marriage using the hazard model and data of the National Survey of Family and Households (NSFH). Bozick and Estacion (2014) concluded that student loan debt delayed the first marriage for women but not men. Addo (2014) also found that women with education loan debt are more likely to delay marriage. However, men's probability of marriage is unaffected by educational debt. Gicheva (2016) showed that people entering an MBA program in their early to mid-twenties are less likely to marry over the next seven years if they accumulate student loans, and the relationship is much stronger for women than for men. According to the analysis of the impact of student loans on marriage choices of female lawyers. Sieg and Wang (2018) found that women with more student debt would postpone their marriage. Addo et al. (2019) analyzed the association between student loan debt and marital behavior for both the 1979 cohort and 1997 cohort and found that student loan debt delays the marriage only for the 1997 cohort. Velez et al. (2019) showed that four years after graduating, undergraduate debt decreased the likelihood of being married for both men and women. Only one study found no relationship between student loan debt and marriage (Zhang, 2013).

Previous studies suggested people with student loan debt are more likely to postpone childbearing (Marks, 2009; Nau et al., 2015; Min and Taylor, 2018; Sieg and Wang, 2018; Velez et al., 2019). Marks (2009) found that the magnitude of Australian higher education debt had a negative impact on the transition to parenthood. The study by Nau et al. (2015) indicated that this negative relationship was true for women only. Min and Taylor (2018) found there is racial and ethnic variation in the relationship between student loan debt and the transition to first birth, including the negative association for Hispanic women as well as for marital first birth among white women. Sieg and Wang (2019) concluded that females with more student loan debt are more likely to delay childbearing. Velez et al. (2019) found the negative effects of student loan debt on having a child is only significant for females.

Many of these existing studies used discrete-time hazard models, which can illustrate the relationship between student loan debt and the timing of marriage and childbearing but cannot solve the endogeneity problem. In this paper, we also use the instrumental variables estimation strategy to address the endogeneity of student loan debt, which could minimize the influence of borrowers' unobservable characteristics. In addition, our sample includes both undergraduate students and graduate students, while much of previous research either focuses on undergraduate students or some specific majors, like MBA students and law school students.

4 Conceptual Framework

According to the theory of marital timing (Oppenheimer, 1988), the employment situation is a very important factor in the marriage decision for young adults since employment could predict one's income or salary. Several empirical studies supported this argument (Oppen- heimer, 2003; Sassler and Goldscheider, 2004; Ahituv and Lerman, 2007). In addition to employment, other economic issues such as savings or visible assets like an expansive car could also affect young adults' decisions for marriage (Gibson-Davis et al., 2005; Smock et al. 2005; Dew and Price, 2011).

The above literature indicated that young adults thought they should have financial stability prior to form a family. Nowadays, more and more students use student loans to finance their post-secondary education; the student loan debt becomes a significant marker for financial wellbeing. As a result, student loan debt has an impact on young adults' decisions about transitioning to marriage or have children. Several studies examined the relationship between student loan debt and family formation, and most literature found that young people with student loan debt are more likely to delay their first marriage and first childbearing (Marks, 2009; Dew and Price, 2011; Bozick and Estacion, 2014; Addo, 2014; Nau et al., 2015; Gicheva, 2016; Min and Taylor, 2018; Sieg and Wang, 2018; Addo et al., 2019; Velez et al., 2019).

There are three potential mechanisms regarding why student loans would delay young adults' age of marriage and childbearing. First, there is a direct effect of student loans on marital and childbearing choices. Since there are fixed costs associated with marriage such as wedding cost, the purchase of a home and household equipment, student loan debt would delay the young adults obtaining these necessary prerequisites, and thus delay their marriage. The costs of childbirth and childcare have continued to rise, and student loan debt would prohibit young adults from saving for raising a child, which will delay their childbearing timing (Mira and Ahn, 2001). Second, student loan debt may indirectly affect young adults' behaviors and decisions on family formation through working longer hours. In order to make the required loan payments, the young adults need to work longer hours and have less time to spend on social activities that may lead to meeting a spouse. The opportunity cost of raising a child is higher for them, so they do not want to sacrifice working time for taking care of a child (Gicheva, 2016). Finally, individuals with student loan debt are less competitive in the marriage market. It takes a long time to pay off the student loans, which may delay purchasing a home or lower the standard of living, so some young people do not want to find a spouse with student loans.

5 Identification Strategy

First, we employ discrete-time hazard regression models to examine the relationship be- tween student loan debt and the probability of transitioning to first marriage or having a first child, based on observable time-varying and invariant characteristics such as sex, race, cohabitation status, education attainment, and average income.

Second, student loan debt relates to many characteristics, including observable and unobservable characteristics. We can control for differences in observable characteristics between students using some controls such as income, education attainment, gender, and race/ethnicity. The unobservable characteristics captured by the error term may be correlated to the student loan, which violates the exogeneity assumption and causes biased estimates.

To correct for this endogeneity problem, we employ a two-stage least squares (2SLS) instrumental variables estimation strategy to minimize the selection bias. We use the tuition of each state's flagship university and distance to each state's flagship university as an instrumental variables to estimate the impact of student loan debt on the probability of first marriage and the probability of having a first child. Specifically, in the first stage, we estimate the effect of average state tuition or distance to each state's flagship university (IV) on the value of student loan debt.

$$Studentloan_i = Averagetuition_i \alpha_1 + X_i \alpha_2 + \varepsilon_i$$

$$Studentloan_i = Distance_i \alpha_1 + X_i \alpha_2 + \varepsilon_i$$

$Studentloan_i$ is the average state-year tuition debts across the whole sample periods for each respondent, $Averagetuition_i$ is the average tuition in student i's state of permanent residence, $Distance_i$ is the distance to each state's flagship university, X_i are control variables including cohabitation status, gender, race, and income.

In the second stage, we estimate the impact of the predicted debt value for each student in the first stage on the outcomes.

$$Outcome_i = \widehat{Studentloan_i}\, \beta_1 + X_i \beta_2 + \mu_i$$

Outcomes include ever married and ever have a child, which are binary variables. $Studentloan_i$ is the predicted value of student loan debt from the first stage.

6 Data

This paper uses data from the National Longitudinal Survey of Youth 1997 (NLSY97), which is an annual study following a nationally representative sample of 12- to 17-year-olds living in the United States as of 1997. These surveys were administered annually from 1997 to 2011, and biannually thereafter. The NLSY97 includes extensive information about the youth educational debt, as well as their familial and relationship backgrounds, which is a perfect data source to explore the relationship between student loans and marriage and childbearing.

To evaluate the impact of student loans on the transition to first marriage and first birth, we limit our sample to those who are ever at risk of taking student loans, that is, those who at least have some college. We also drop those who have married or have a child before 18 years old since 18 is the average age entering into college. The final sample comprises 4,871 observations, and 2,669 of them ever had student loans.

Cohabitation and Marriage Young adults can transition from a single state into co- habitation or marriage. In each survey round, respondents are asked about their current marital status. A change of cohabitation status from not cohabitating to cohabiting indi- cates cohabitating in this period. Similarly, a change of marital status from not married to married suggests getting married in the current period.

Child Two questions related to the number of children were asked every survey year, one is the number of biological children born and residing in the household, and the other one is the number of biological children born but not residing in the household. We sum the two variables, and an increase in the total number of biological children suggests a new child born this year. The firs time of having a new child is the age of having the first child.

Education We first restrict the sample to the respondents with at least some college by keeping respondents' highest grade completed greater than 12 years. Then, we recode the education variable into four categories, some college but no degree, college degree, master's degree, and Ph.D. or professional degree (DDS, JD, MD).

Student loans In each survey round, for youth currently enrolled in any type of postsecondary or advanced degree program after high school are asked "Other than assistance you received from relatives and friends, how much did you borrow in government subsidized loans or other types of loans while you attended this school/institution?" and "How much is still owed on (this/these) loan(s)?" We sum the yearly responses to the second question for all schools and semesters as the total student loan amount of each year. A median value is assigned to youth who entered in a range (i.e., $0–$1000 was assigned a value of $500).

Income There are two variables related to income in each survey round. One is the total net income from wages and salary in the past year; another one is the total income from business or farm in the past year. We sum the two variables together and use the sum of them as a proxy of income of respondents.

Race The sample is categorized into three ethnocidal categories: black, Hispanic, non- black non-Hispanic (reference group).

Tuition We use tuition of each state's flagship university as a proxy of average tuition.

Distance to flagship university It is calculated from the following steps. First, I Collect the location data from the restricted NLSY97 geocode files. The location data include the information which state and county of each respondent. Second, I got the latitude and longitude of each county from the United States Counties Database and merge the counties coordinates data with location data using county-level FIPs codes. Third, merge the coordinates of each flagship university with location data using state FIPS. Lastly, calculate the distance from flagship university for each respondent.

Table 1 presents the summary statistics. There are1,608 observations in our sample that never have student loans across the whole sample period, and the rest 1,672 observations in the sample ever have student loans. There are 54% of them ever married in never have student loan group, while 61% of them ever married for ever have student loan group. There 62% of them ever have a child in never have student loan group, while 60% of them ever have a child for ever have student loan group. The mean age of first marriage in ever have student loans group (24.68) is younger than that of never have student loans group (26.60), while the mean age of having a first child in ever have student loan group (27.11) is older than that of never have student loans group (25.77).

7 Empirical Results

This section first shows the naive regressions of the OLS and the Probit model; then, we estimate a series of discrete-time hazard regression models predicting the odds of first marriage and having a first child as a function of time-varying student loan debt status. Also, we reestimate the results using the average tuition as the instrumental variable of average student loans to overcome the endogeneity problem.

Table 2 presents the naive estimates of the impacts of student loans on the first marriage and childbearing using the OLS model and the probit model. The first and the third columns present the OLS regressions using the age of first marriage and the age of first childbirth as the dependent variable. The second and the fourth columns present the probit regressions using ever married and ever have a child as the dependent variables. Control variables include sex, race, cohabitation status, and average income across the whole sample periods. As can be seen in column (1) and (3), ever having a student loan postpone about 1.1 years of first marriage and 1.68 years of first child respectively. Both the second and the fourth columns show that ever having a student loan will reduce the likelihood of marriage and having a child. Specifically, ever having a student loan will reduce 18% probability of getting married and reduce 21.7% probability of having a child.

Figure 3 presents the Kaplan-Meier estimates of getting first marriage. The figure is plotted after the split of having a student loan. The blue line represents the group of people before having student loan debt or never has a student loan, and the red line represents the group of people after having student loan debt. The horizontal axis is the analysis time in years, and the origin time is 18 years old. As figure 3 shows, for the same analysis time, having a student loan is more likely getting first marriage late. To further explore the accurate probability of getting first marriage for people who have student loan debt, we use Cox hazard regressions adjusting for their propensity of taking student loans.

Table 3 presents the hazard ratios of Cox proportional hazards regressions of owning student loans on the first marriage. The duration analysis is split by the age of student loans and the age of getting a college degree. Control variables include sex, race, cohabitation status, and average income across the whole sample periods. The hazard ratios are less than 1 for all the specifications, which means having a student loan will postpone the first marriage, and the hazard analyses are significant for full sample, female sample, male sample and Non-Black Non-Hispanic sample. Specifically, respondents with a student loan are 18.1% less likely to have the first marriage; Females and males are 16.3% and 19.5% less likely to have the first marriage if they have a student loan and this figure changes to 22.9 for Non-Black Non-Hispanics.

Figure 4 presents the Kaplan-Meier estimates of getting the first child. The figure is plotted after the split of having a student loan. The blue line represents the group of people before having student loan debt or never have a student loan, and the red line represents the group of people after having student loan debt. The horizontal axis is the analysis time in years, and the origin time is 18 years old. As figure 4 shows, for the same analysis time, having a student loan is more likely to postpone having the first child.

Table 4 presents the hazard ratio of Cox proportional hazards regressions of owning student loans on the first child. The duration analysis is split by the age of student loans and the age of getting a college degree. Control variables include sex, race, cohabitation status, and average income across the whole sample periods. The hazard ratios are less than 1 for all the specifications, which means having a student loan will postpone having the first child, and the hazard analysis is only significant for full sample, female sample and Non-Black Non-Hispanic sample. Specifically, having a student loan is 27.9% less likely to have a first child, while females are 38.9% less likely to have a first child if they have student loans.

Table 5 presents the estimates of the impacts of student loan debts on the probability of getting married. Row 1 shows the second stage results of using the tuition of each state's flagship university as the instrumental variable of average student loan. We can see an additional amount of student loan significantly reduces the likelihood of getting married for the specifications of full sample, male and Hispanic. Specifically, an additional one hundred dollars student loan across the whole sample reduces 1.3% probability of getting married. This effect is particularly larger for males than females. Row 2 uses the distance to each state's flagship university for each respondent as the instrumental variable of average student loan. An additional amount of student loan significantly reduces the likelihood of getting married for the specifications of full sample, male, black and Hispanic groups. Specifically, an additional one hundred dollars student loan across the whole sample reduces 1.5% probability of getting married. This effect is particularly larger for black and Hispanics than non-blacks/Hispanics. Row 3 displays the second stage estimates using both instruments. An additional amount of student loan significantly reduces the likelihood of getting married for the specifications of full sample, male, black and Hispanic groups. Specifically, an additional one hundred dollars student loan across the whole sample reduces 1.5% probability of getting married. The heterogeneous effects are significant for both Hispanics and non-blacks/Hispanics.

Table 6 presents the estimates of the impacts of student loan debts on the probability of having a child. Row 1 shows the second stage results of using the tuition of each state's flagship university as the instrumental variable of average student loan. We can see an additional amount of student loan significantly reduces the likelihood of having a child for the specifications of full sample, female and Black. Specifically, an additional one hundred dollars student loan across the whole sample reduces 1.5% probability of having a child. This effect is particularly larger for females than males. Row 2 uses the distance to each state's flagship university for each respondent as the instrumental variable of average student loan. It is not statistically significant for all of specifications. Row 3 displays the second stage estimates using both instruments. It is only statistically significant for black people. Specifically, an additional one hundred dollars student across the whole sample reduces 1.5% probability of having a child.

8 Conclusion

In this paper, we examine the causal relationship between student loan debt and the timing of first marriage and first birth. Our analysis seeks to better understand whether owing student loan debt delays the timing or influence the likelihood of first marriage and first birth. Overall, we find a pronounced effect of student loans on the timing of first marriage and first birth. Specifically, the results of hazard model indicate that students who own student loan debt are 9.2% less likely to have the first marriage and 9.1% less likely to have a first child, while the IV results suggest that students who own student loan debt are 19% less likely to get married and 8.4% less likely to have a child. For the heterogeneous effects of the Hazard model on gender, the risk of transitioning to first marriage was lower for men with student loans than for women, while the risk of transitioning to first birth was lower for women with student loans than for men. For the heterogeneous effects of Hazard model on race, the risk of transitioning to first marriage was lower for black and Hispanic than others, while the risk of transitioning to first birth does not show a similar pattern.

Even though we try our best to explore this study thoroughly, there are still some limitations. First, We are unable to fully eliminate the influence of parental assistance or outside guidance in selecting individuals into student debt, which may also influence their decisions about marriage and childbearing. Second, our study does not account for potential differences in college educational experiences, students from different majors may have divergent decisions upon marriage and childbearing. Third, we only consider the impacts of student loans on the first marriage and first child, which is only part of the story. Having student loans may also influence the stability of marriage and the number of births, which should be examined in future studies.

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Tables and Figures

Table 1. Summary Statistics

	Never have student loan		Ever have student loan	
	Mean	SD	Mean	SD
Male	0.53	0.50	0.46	0.50
Black	0.31	0.46	0.28	0.45
Hispanic	0.13	0.34	0.11	0.32
Non-Black Non-Hispanics	0.55	0.49	0.61	0.49
Ever cohabited	0.63	0.48	0.70	0.46
Ever married	0.54	0.50	0.61	0.49
Ever have a child	0.62	0.49	0.60	0.49
Age of first marriage	26.60	4.38	24.68	4.45
Age of cohabitation	24.26	3.58	24.57	3.53
Age of having the first child	25.77	4.29	27.11	4.19
Average student loan	0	0	5075.64	7681.80
Average tuition	4204.24	1237.85	4242.04	1254.18
Average income	60328.19	37121.86	65942.84	34507.74
Observations	1608		1672	

Notes: The table presents the summary statistics of variables for the sample that never have a student loan and the sample that ever have student loans.

Data source: National Longitudinal Surveys of Youth 1997 (NLSY97)

Table 2. OLS and Probit estimates of the impacts of student loans on the first marriage and childbearing

	Marriage		Childbearing	
	Age of first marriage (1)	Ever have Married (2)	Age of first child (3)	Ever have a child (4)
Ever have student loan	1.087*** (0.278)	-0.180* (0.102)	1.679*** (0.253)	-0.217*** (0.062)
Control Variables	Yes	Yes	Yes	Yes
Observations	1564	3280	1689	3280

Notes: The results presented here list the estimates of the impacts of student loans on the first marriage and childbearing. The first and the third columns present the OLS regressions using the age of first marriage and the age of first childbirth as the dependent variable. The second and the fourth columns present the probit regressions using ever married and ever have a child as the dependent variables. Control variables include sex, race, cohabitation status, and average income across whole sample periods. Standard errors are in parentheses. Significance levels: *$p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 3. Hazard model - estimates of the impacts of student loans on the first marriage

	Full Sample	Female	Male	Black	Hispanic	Non-Black/Hispanic
Post Student Loan	0.819*** (0.042)	0.837*** (0.055)	0.805*** (0.065)	0.884 (0.085)	0.863 (0.128)	0.771*** (0.051)
Observations	3280	1644	1622	965	685	1649

Notes: The results presented here list the hazard ratios of Cox proportional hazards regressions of owning student loans on the first marriage for the whole sample, female sample, male sample, black sample, Hispanic sample, and Non-black Non-Hispanic sample. The duration analysis is split by the age of student loans and the age of getting college degree. Control variables include sex, race, cohabitation status, and average income across whole sample periods. Standard errors in parentheses.

Significance levels: **p* < 0.10, ** *p* < 0.05, ****p* < 0.01.

Table 4. Hazard Model - estimates of the impacts of student loans on the first childbearing

	Full Sample	Female	Male	Black	Hispanic	Non-Black/Hispanic
Post Student Loan	0.721*** (0.064)	0.611*** (0.076)	0.870 (0.110)	0.798 (0.169)	0.746 (0.153)	0.683*** (0.076)
Observations	3280	1571	1709	611	631	2308

Notes: The results presented here list the hazard ratios of Cox proportional hazards regressions of owning student loans on the first childbearing for the whole sample, female sample, male sample, black sample, Hispanic sample, and Non-black Non-Hispanic sample. The duration analysis is split by the age of student loans and the age of getting college degree. Control variables include sex, race, cohabitation status, and average income across whole sample periods. Standard errors in parentheses.

Significance levels: **p* < 0.10, ** *p* < 0.05, ****p* < 0.01.

Table 5. Instrumental variable – estimates of the impacts of student loans on the probability of getting married

	Full Sample	Female	Male	Black	Hispanic	Non-Black/Hispanic
Student Loan - Tuition as IV	-0.00018*** (0.00004)	-0.00010 (0.00013)	-0.00013*** (0.00002)	0.00006 (0.00007)	-0.00016** (0.00008)	0.00014 (0.00001)
Student Loan - Distance as IV	-0.00015*** (0.00002)	0.00009 (0.00008)	-0.00013*** (0.00001)	-0.00020*** (0.00001)	-0.00021*** (0.00001)	0.00004 (0.0002)
Student Loan - Tuition and Distance as IVs	-0.00015*** (0.00002)	0.00004 (0.00008)	0.00004 (0.00008)	0.000059 (0.00007)	-0.00021*** (0.0005)	0.00014*** (0.076)
1st stage F-statistics excluded both IVs p-value: overidentification test	0.007	0.0002	0.414	0.000	0.035	0.000
Observations	3,280	1,571	1,709	611	631	2,308

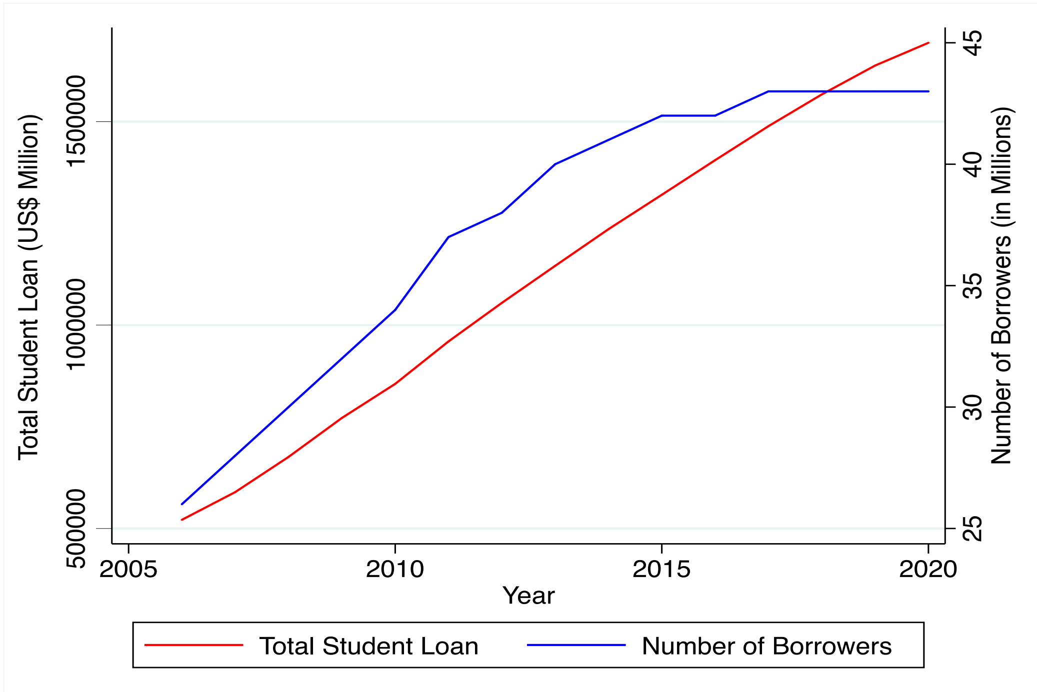
Notes: The results presented here list two-stage least square estimates of the impacts of owning student loan debts on the probability of getting married for the whole sample, female sample, male sample, black sample, Hispanic sample, and Non-black Non-Hispanic sample. Control variables include sex, race, cohabitation status, and average income across whole sample periods. Standard errors in parentheses. Significance levels: *$p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 6. Instrumental variable – estimates of the impacts of student loans on the probability of having a child

	Full Sample	Female	Male	Black	Hispanic	Non-Black/Hispanic
Student Loan - Tuition as IV	-0.00015*** (0.00003)	-0.00020*** (0.00013)	-0.00005 (0.00034)	-0.00013** (0.00004)	0.00005 (0.00017)	0.00010 (0.00011)
Student Loan - Distance as IV	-0.00008 (0.00010)	0.00009 (0.00008)	-0.00005 (0.00012)	-0.00004 (0.00047)	-0.00012 (0.00011)	0.00008 (0.00013)
Student Loan - Tuition and Distance as IVs	-0.00062 (0.00011)	-0.00006 (0.00009)	-0.00004 (0.00035)	-0.00015** (0.00008)	0.00010 (0.00013)	0.00012 (0.00020)
1st stage F-statistics excluded both IVs p-value: overidentification test	0.007	0.0002	0.414	0.000	0.035	0.000
Observations	3,280	1,571	1,709	611	631	2,308

Notes: The results presented here list two-stage least square estimates of the impacts of owning student loan debts on the probability of having a child for the whole sample, female sample, male sample, black sample, Hispanic sample, and Non-black Non-Hispanic sample. Control variables include sex, race, cohabitation status, and average income across whole sample periods. Standard errors in parentheses. Significance levels: *$p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

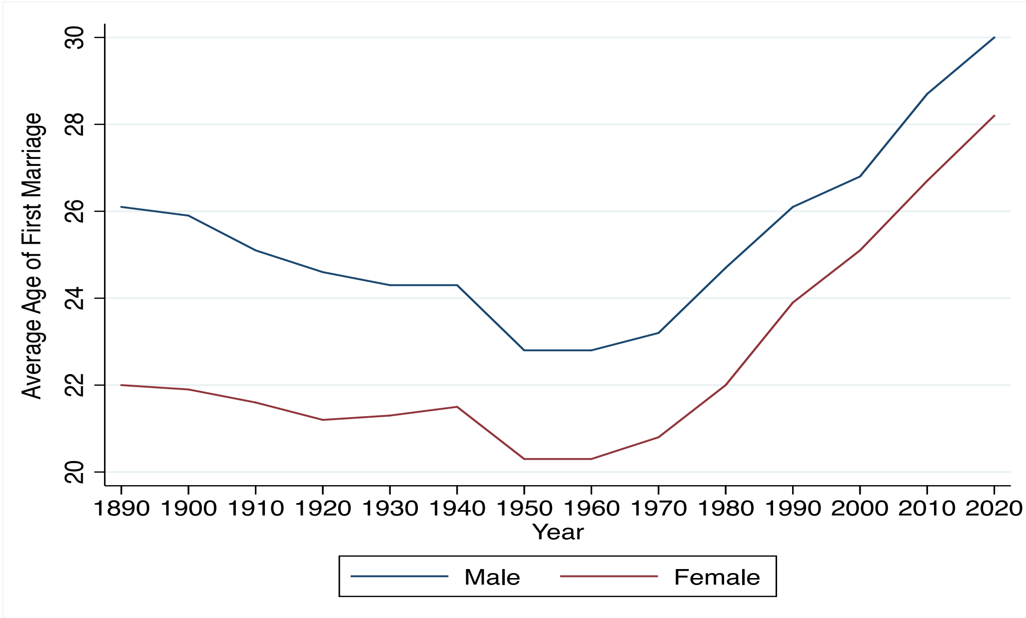
Figure 1 Total student loan debt trend

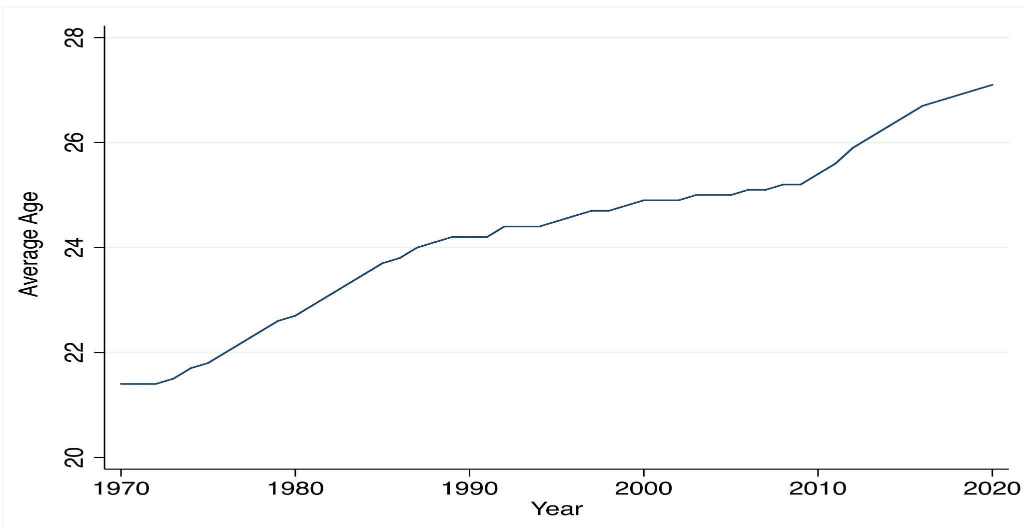


Notes: Total student loan debt is measured at the end of the fiscal quarter each year.

Data Source: Education Data Initiative.

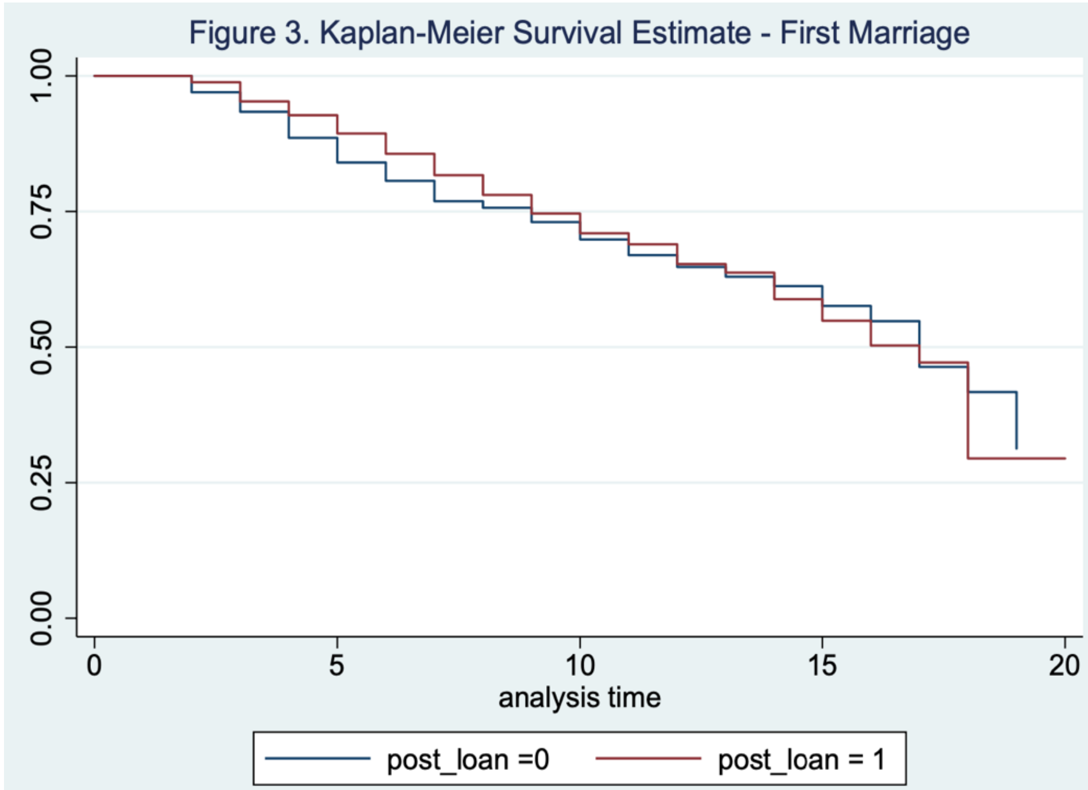
Figure 2 Mean age of first marriage and first-time mothers





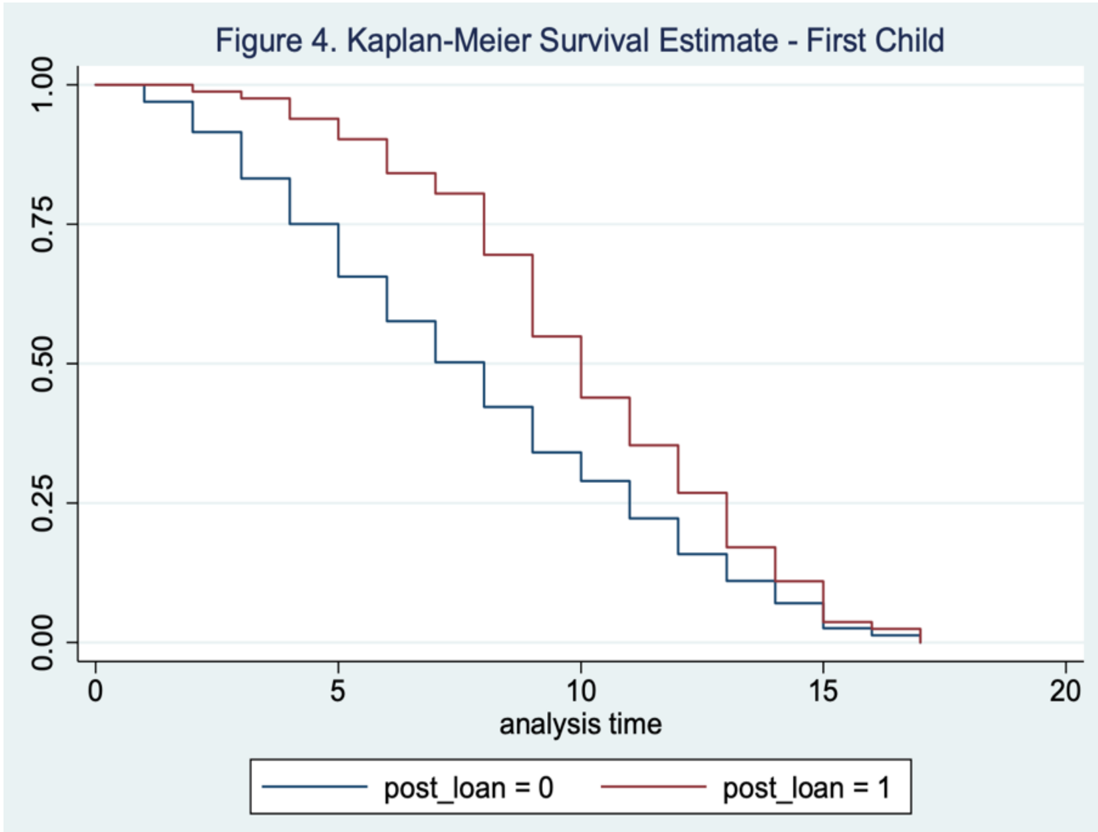
Data Source: U.S. Bureau of the Census and CDC/NCHS, National Vital Statistics System

Figure 3 Kaplan-Meier survival estimates – first marriage



Notes: This figure plots the Kaplan-Meier estimates of getting first child. The figure is plotted after the split of having a student loan. The blue line represents the group of people before having student loan debt or never has a student loan, and the red line represents the group of people after student loan debt.

Figure 4 Kaplan-Meier survival estimates – first child



Notes: This figure plots the Kaplan-Meier estimates of getting first child. The figure is plotted after the split of having a student loan. The blue line represents the group of people before having student loan debt or never has a student loan, and the red line represents the group of people after student loan debt.

Appendix Detail Regression Results of Instrumental Variable Models

Marriage

A1. Detailed regression results of Two-Stage least squares (2SLS) on marriage – full sample

First-stage regression

Source	SS	df	MS
Model	64505745.9	6	10750957.7
Residual	3.8170e+09	4,864	784749.588
Total	3.8815e+09	4,870	797028.284

Number of obs	=	4,871
F(6, 4864)	=	13.70
Prob > F	=	0.0000
R-squared	=	0.0166
Adj R-squared	=	0.0154
Root MSE	=	885.86

aveLoan	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
aveTuition	.0737093	.0129092	5.71	0.000	.0484014	.0990171
male	-119.5983	26.01273	-4.60	0.000	-170.595	-68.60159
black	108.0354	31.7747	3.40	0.001	45.74265	170.3282
hispanic	-31.71663	34.68074	-0.91	0.360	-99.70655	36.27328
cohabitation	60.98793	25.71804	2.37	0.018	10.56896	111.4069
aveIncome	.0027969	.0011798	2.37	0.018	.000484	.0051098
_cons	87.77818	73.14505	1.20	0.230	-55.61917	231.1755

Two-step probit with endogenous regressors

Number of obs	=	4,871
Wald chi2(6)	=	271.46
Prob > chi2	=	0.0000

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
aveLoan	-.0014058	.0003583	-3.92	0.000	-.002108	-.0007036
male	-.461469	.0681438	-6.77	0.000	-.5950283	-.3279097
black	-.3741302	.0730552	-5.12	0.000	-.5173157	-.2309448
hispanic	-.1897527	.0732865	-2.59	0.010	-.3333916	-.0461138
cohabitation	.163833	.0573697	2.86	0.004	.0513905	.2762755
aveIncome	.0000361	2.75e-06	13.12	0.000	.0000307	.0000415
_cons	.5826121	.178591	3.26	0.001	.2325802	.932644

Instrumented: aveLoan
Instruments: male black hispanic cohabitation aveIncome aveTuition

Wald test of exogeneity: chi2(1) = 30.32 Prob > chi2 = 0.0000

Marginal effects after ivprobit
y = Fitted values (predict)
= .14199334

variable	dy/dx	Std. Err.	z	P>|z|	[95%	C.I.]	X
aveLoan	-.0014058	.00036	-3.92	0.000	-.002108	-.000704	501.033
male*	-.461469	.06814	-6.77	0.000	-.595028	-.32791	.465408
black*	-.3741302	.07306	-5.12	0.000	-.517316	-.230945	.224595
hispanic*	-.1897527	.07329	-2.59	0.010	-.333392	-.046114	.180045
cohabi~n*	.163833	.05737	2.86	0.004	.05139	.276276	.552659
aveInc~e	.0000361	.00000	13.12	0.000	.000031	.000041	14031.3

(*) dy/dx is for discrete change of dummy variable from 0 to 1

A2. Detailed regression results of 2SLS on marriage – female sample

First-stage regression

Source	SS	df	MS			
				Number of obs	=	2,604
				F(5, 2598)	=	11.12
Model	41834736.1	5	8366947.22	Prob > F	=	0.0000
Residual	1.9540e+09	2,598	752131.286	R-squared	=	0.0210
				Adj R-squared	=	0.0191
Total	1.9959e+09	2,603	766758.285	Root MSE	=	867.26

aveLoan	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
aveTuition	.0767539	.0174501	4.40	0.000	.0425364	.1109715
black	201.8388	41.15957	4.90	0.000	121.13	282.5477
hispanic	2.904432	46.85253	0.06	0.951	-88.96764	94.7765
cohabitation	74.5153	34.67634	2.15	0.032	6.519243	142.5114
aveIncome	.0044881	.0018507	2.43	0.015	.0008592	.0081171
_cons	14.6799	96.54396	0.15	0.879	-174.631	203.9908

Two-step probit with endogenous regressors

Number of obs = 2,604
Wald chi2(5) = 104.63
Prob > chi2 = 0.0000

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
aveLoan	-.001813	.000536	-3.38	0.001	-.0028636	-.0007623
black	-.3926353	.1405497	-2.79	0.005	-.6681077	-.1171629
hispanic	-.159442	.1098928	-1.45	0.147	-.3748278	.0559439
cohabitation	.1931686	.0921345	2.10	0.036	.0125883	.3737488
aveIncome	.0000319	5.19e-06	6.14	0.000	.0000217	.000042
_cons	.8389228	.2346853	3.57	0.000	.378948	1.298898

Instrumented: aveLoan
Instruments: black hispanic cohabitation aveIncome aveTuition

Wald test of exogeneity: chi2(1) = 28.36 Prob > chi2 = 0.0000

Marginal effects after ivprobit
y = Fitted values (predict)
= .20618705

variable	dy/dx	Std. Err.	z	P>|z|	[95%	C.I.]	X
aveLoan	-.001813	.00054	-3.38	0.001	-.002864	-.000762	554.795
black*	-.3926353	.14055	-2.79	0.005	-.668108	-.117163	.246544
hispanic*	-.159442	.10989	-1.45	0.147	-.374828	.055944	.175499
cohabi~n*	.1931686	.09213	2.10	0.036	.012588	.373749	.583333
aveInc~e	.0000319	.00001	6.14	0.000	.000022	.000042	12092.3

(*) dy/dx is for discrete change of dummy variable from 0 to 1

A3. Detailed regression results of 2SLS on marriage – male sample

First-stage regression

Source	SS	df	MS			
Model	16443415.7	5	3288683.14	Number of obs	=	2,267
Residual	1.8530e+09	2,261	819566.861	F(5, 2261)	=	4.01
				Prob > F	=	0.0013
				R-squared	=	0.0088
				Adj R-squared	=	0.0066
Total	1.8695e+09	2,266	825015.043	Root MSE	=	905.3

aveLoan	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
aveTuition	.0673593	.0191962	3.51	0.000	.0297152	.1050034
black	-16.6482	49.72566	-0.33	0.738	-114.1609	80.86449
hispanic	-71.14506	51.48412	-1.38	0.167	-172.1061	29.816
cohabitation	52.40707	38.32239	1.37	0.172	-22.74366	127.5578
aveIncome	.0013729	.0015495	0.89	0.376	-.0016658	.0044115
_cons	60.53574	109.5725	0.55	0.581	-154.3374	275.4089

Two-step probit with endogenous regressors

Number of obs = 2,267
Wald chi2(5) = 213.14
Prob > chi2 = 0.0000

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
aveLoan	-.0008257	.0004822	-1.71	0.087	-.0017708	.0001195
black	-.2109658	.0846882	-2.49	0.013	-.3769516	-.04498
hispanic	-.1757341	.0988637	-1.78	0.075	-.3695033	.0180352
cohabitation	.1295406	.0696309	1.86	0.063	-.0069334	.2660146
aveIncome	.0000409	2.98e-06	13.74	0.000	.0000351	.0000468
_cons	-.2233595	.2122948	-1.05	0.293	-.6394497	.1927306

Instrumented: aveLoan
Instruments: black hispanic cohabitation aveIncome aveTuition

Wald test of exogeneity: chi2(1) = 4.40 Prob > chi2 = 0.0360

Marginal effects after ivprobit
y = Fitted values (predict)
= .07159749

variable	dy/dx	Std. Err.	z	P>|z|	[95%	C.I.]	X
aveLoan	-.0008257	.00048	-1.71	0.087	-.001771	.000119	439.278
black*	-.2109658	.08469	-2.49	0.013	-.376952	-.04498	.199382
hispanic*	-.1757341	.09886	-1.78	0.075	-.369503	.018035	.185267
cohabi~n*	.1295406	.06963	1.86	0.063	-.006933	.266015	.517424
aveInc~e	.0000409	.00000	13.74	0.000	.000035	.000047	16258.6

(*) dy/dx is for discrete change of dummy variable from 0 to 1

A4. Detailed regression results of 2SLS on marriage – black sample

First-stage regression

Source	SS	df	MS
Model	25113274.8	4	6278318.7
Residual	857434601	1,089	787359.597
Total	882547876	1,093	807454.599

Number of obs	=	1,094
F(4, 1089)	=	7.97
Prob > F	=	0.0000
R-squared	=	0.0285
Adj R-squared	=	0.0249
Root MSE	=	887.33

aveLoan	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
aveTuition	.0401913	.0296421	1.36	0.175	-.0179709	.0983534
male	-278.1308	54.82818	-5.07	0.000	-385.7116	-170.5499
cohabitation	23.0728	54.16022	0.43	0.670	-83.1974	129.343
aveIncome	.0075329	.002949	2.55	0.011	.0017465	.0133194
_cons	397.1598	160.0934	2.48	0.013	83.03333	711.2863

Two-step probit with endogenous regressors

Number of obs	=	1,094
Wald chi2(4)	=	11.84
Prob > chi2	=	0.0185

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
aveLoan	-.0040204	.0031668	-1.27	0.204	-.0102272	.0021864
male	-1.024097	.9105575	-1.12	0.261	-2.808756	.7605632
cohabitation	.1801722	.2413264	0.75	0.455	-.2928189	.6531633
aveIncome	.000069	.0000271	2.54	0.011	.0000158	.0001222
_cons	1.581116	1.917267	0.82	0.410	-2.176658	5.33889

Instrumented: aveLoan
Instruments: male cohabitation aveIncome aveTuition

Wald test of exogeneity: chi2(1) = 12.59 Prob > chi2 = 0.0004

Marginal effects after ivprobit
y = Fitted values (predict)
= -.29885966

variable	dy/dx	Std. Err.	z	P>|z|	[95%	C.I.]	X
aveLoan	-.0040204	.00317	-1.27	0.204	-.010227	.002186	586.198
male*	-1.024097	.91056	-1.12	0.261	-2.80876	.760563	.413163
cohabi~n*	.1801722	.24133	0.75	0.455	-.292819	.653163	.550274
aveInc~e	.000069	.00003	2.54	0.011	.000016	.000122	11609.2

(*) dy/dx is for discrete change of dummy variable from 0 to 1

A5. Detailed regression results of 2SLS on marriage – Hispanic sample

First-stage regression

Source	SS	df	MS
Model	19100762.2	4	4775190.55
Residual	540710960	872	620081.376
Total	559811722	876	639054.477

Number of obs	=	877
F(4, 872)	=	7.70
Prob > F	=	0.0000
R-squared	=	0.0341
Adj R-squared	=	0.0297
Root MSE	=	787.45

aveLoan	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
aveTuition	.1083723	.026608	4.07	0.000	.0561492	.1605955
male	-158.992	55.08039	-2.89	0.004	-267.0977	-50.88642
cohabitation	-95.47041	53.60929	-1.78	0.075	-200.6887	9.747902
aveIncome	.0067989	.002884	2.36	0.019	.0011385	.0124593
_cons	-65.28286	139.1157	-0.47	0.639	-338.3237	207.758

Two-step probit with endogenous regressors

Number of obs	=	877
Wald chi2(4)	=	51.21
Prob > chi2	=	0.0000

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
aveLoan	-.0007496	.0004435	-1.69	0.091	-.0016189	.0001197
male	-.5553662	.1252645	-4.43	0.000	-.80088	-.3098523
cohabitation	-.1320681	.1050499	-1.26	0.209	-.3379621	.073826
aveIncome	.0000402	6.35e-06	6.33	0.000	.0000277	.0000526
_cons	.259113	.225369	1.15	0.250	-.1826022	.7008282

Instrumented: aveLoan
Instruments: male cohabitation aveIncome aveTuition

Wald test of exogeneity: chi2(1) = 3.86 Prob > chi2 = 0.0496

Marginal effects after ivprobit
y = Fitted values (predict)
= .15356966

variable	dy/dx	Std. Err.	z	P>|z|	[95%	C.I.]	X
aveLoan	-.0007496	.00044	-1.69	0.091	-.001619	.00012	421.153
male*	-.5553662	.12526	-4.43	0.000	-.80088	-.309852	.478905
cohabi~n*	-.1320681	.10505	-1.26	0.209	-.337962	.073826	.511973
aveInc~e	.0000402	.00001	6.33	0.000	.000028	.000053	13537.2

(*) dy/dx is for discrete change of dummy variable from 0 to 1

A6. Detailed regression results of 2SLS on Marriage – Non-black Non-Hispanic

First-stage regression

Source	SS	df	MS
Model	30230704.9	4	7557676.23
Residual	2.3952e+09	2,895	827365.196
Total	2.4255e+09	2,899	836651.585

Number of obs	=	2,900
F(4, 2895)	=	9.13
Prob > F	=	0.0000
R-squared	=	0.0125
Adj R-squared	=	0.0111
Root MSE	=	909.6

aveLoan	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
aveTuition	.0719382	.0168004	4.28	0.000	.0389963	.10488
male	-46.44223	34.6558	-1.34	0.180	-114.3948	21.51031
cohabitation	129.5357	34.41514	3.76	0.000	62.05503	197.0163
aveIncome	.0004937	.0014444	0.34	0.733	-.0023386	.0033259
_cons	57.84203	93.69902	0.62	0.537	-125.8815	241.5655

Two-step probit with endogenous regressors

Number of obs	=	2,900
Wald chi2(4)	=	132.59
Prob > chi2	=	0.0000

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
aveLoan	-.0013085	.0004569	-2.86	0.004	-.002204	-.0004129
male	-.4561423	.0707736	-6.45	0.000	-.5948561	-.3174286
cohabitation	.2708541	.0912744	2.97	0.003	.0919595	.4497486
aveIncome	.0000311	3.02e-06	10.30	0.000	.0000252	.000037
_cons	.5447864	.2077798	2.62	0.009	.1375454	.9520273

Instrumented: aveLoan
Instruments: male cohabitation aveIncome aveTuition

Wald test of exogeneity: chi2(1) = 15.83 Prob > chi2 = 0.0001

Marginal effects after ivprobit
y = Fitted values (predict)
= .30329855

variable	dy/dx	Std. Err.	z	P>|z|	[95%	C.I.]	X
aveLoan	-.0013085	.00046	-2.86	0.004	-.002204	-.000413	493.061
male*	-.4561423	.07077	-6.45	0.000	-.594856	-.317429	.481034
cohabi~n*	.2708541	.09127	2.97	0.003	.09196	.449749	.565862
aveInc~e	.0000311	.00000	10.30	0.000	.000025	.000037	15094.5

(*) dy/dx is for discrete change of dummy variable from 0 to 1

Childbearing.

B1. Detailed regression results of 2SLS on childbearing – full sample

First-stage regression

Source	SS	df	MS
Model	64505745.9	6	10750957.7
Residual	3.8170e+09	4,864	784749.588
Total	3.8815e+09	4,870	797028.284

Number of obs	=	4,871
F(6, 4864)	=	13.70
Prob > F	=	0.0000
R-squared	=	0.0166
Adj R-squared	=	0.0154
Root MSE	=	885.86

aveLoan	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
aveTuition	.0737093	.0129092	5.71	0.000	.0484014	.0990171
male	-119.5983	26.01273	-4.60	0.000	-170.595	-68.60159
black	108.0354	31.7747	3.40	0.001	45.74265	170.3282
hispanic	-31.71663	34.68074	-0.91	0.360	-99.70655	36.27328
cohabitation	60.98793	25.71804	2.37	0.018	10.56896	111.4069
aveIncome	.0027969	.0011798	2.37	0.018	.000484	.0051098
_cons	87.77818	73.14505	1.20	0.230	-55.61917	231.1755

Two-step probit with endogenous regressors

Number of obs	=	4,871
Wald chi2(6)	=	278.41
Prob > chi2	=	0.0000

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
aveLoan	-.0006286	.0002742	-2.29	0.022	-.0011661	-.0000911
male	-.3992466	.0520446	-7.67	0.000	-.5012521	-.2972412
black	.4335779	.0565626	7.67	0.000	.3227172	.5444387
hispanic	.1177507	.0561789	2.10	0.036	.007642	.2278593
cohabitation	.4293992	.0438855	9.78	0.000	.3433851	.5154132
aveIncome	.0000186	2.04e-06	9.16	0.000	.0000147	.0000226
_cons	-.0084924	.136692	-0.06	0.950	-.2764039	.2594191

Instrumented: aveLoan
Instruments: male black hispanic cohabitation aveIncome aveTuition

Wald test of exogeneity: chi2(1) = 5.34 Prob > chi2 = 0.0209

Marginal effects after ivprobit
y = Fitted values (predict)
= .10822328

variable	dy/dx	Std. Err.	z	P>|z|	[95% C.I.]		X
aveLoan	-.0006286	.00027	-2.29	0.022	-.001166	-.000091	501.033
male*	-.3992466	.05204	-7.67	0.000	-.501252	-.297241	.465408
black*	.4335779	.05656	7.67	0.000	.322717	.544439	.224595
hispanic*	.1177507	.05618	2.10	0.036	.007642	.227859	.180045
cohabi~n*	.4293992	.04389	9.78	0.000	.343385	.515413	.552659
aveInc~e	.0000186	.00000	9.16	0.000	.000015	.000023	14031.3

(*) dy/dx is for discrete change of dummy variable from 0 to 1

B2. Detailed regression results of 2SLS on childbearing – female sample

First-stage regression

Source	SS	df	MS
Model	41834736.1	5	8366947.22
Residual	1.9540e+09	2,598	752131.286
Total	1.9959e+09	2,603	766758.285

Number of obs	=	2,604
F(5, 2598)	=	11.12
Prob > F	=	0.0000
R-squared	=	0.0210
Adj R-squared	=	0.0191
Root MSE	=	867.26

aveLoan	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
aveTuition	.0767539	.0174501	4.40	0.000	.0425364	.1109715
black	201.8388	41.15957	4.90	0.000	121.13	282.5477
hispanic	2.904432	46.85253	0.06	0.951	-88.96764	94.7765
cohabitation	74.5153	34.67634	2.15	0.032	6.519243	142.5114
aveIncome	.0044881	.0018507	2.43	0.015	.0008592	.0081171
_cons	14.6799	96.54396	0.15	0.879	-174.631	203.9908

Two-step probit with endogenous regressors

Number of obs	=	2,604
Wald chi2(5)	=	57.99
Prob > chi2	=	0.0000

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
aveLoan	-.0007419	.0003703	-2.00	0.045	-.0014677	-.000016
black	.4291931	.0982668	4.37	0.000	.2365936	.6217926
hispanic	.1004279	.0761947	1.32	0.187	-.048911	.2497669
cohabitation	.3888011	.0637287	6.10	0.000	.2638951	.5137071
aveIncome	9.67e-06	3.54e-06	2.73	0.006	2.73e-06	.0000166
_cons	.1879747	.1621278	1.16	0.246	-.12979	.5057393

Instrumented: aveLoan
Instruments: black hispanic cohabitation aveIncome aveTuition

Wald test of exogeneity: chi2(1) = 4.38 Prob > chi2 = 0.0364

Marginal effects after ivprobit
y = Fitted values (predict)
= .24355396

variable	dy/dx	Std. Err.	z	P>|z|	[95% C.I.	]	X
aveLoan	-.0007419	.00037	-2.00	0.045	-.001468	-.000016	554.795
black*	.4291931	.09827	4.37	0.000	.236594	.621793	.246544
hispanic*	.1004279	.07619	1.32	0.187	-.048911	.249767	.175499
cohabi~n*	.3888011	.06373	6.10	0.000	.263895	.513707	.583333
aveInc~e	9.67e-06	.00000	2.73	0.006	2.7e-06	.000017	12092.3

(*) dy/dx is for discrete change of dummy variable from 0 to 1

B3. Detailed regression results of 2SLS on childbearing – male sample

First-stage regression

Source	SS	df	MS
Model	16443415.7	5	3288683.14
Residual	1.8530e+09	2,261	819566.861
Total	1.8695e+09	2,266	825015.043

Number of obs	=	2,267
F(5, 2261)	=	4.01
Prob > F	=	0.0013
R-squared	=	0.0088
Adj R-squared	=	0.0066
Root MSE	=	905.3

aveLoan	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
aveTuition	.0673593	.0191962	3.51	0.000	.0297152	.1050034
black	-16.6482	49.72566	-0.33	0.738	-114.1609	80.86449
hispanic	-71.14506	51.48412	-1.38	0.167	-172.1061	29.816
cohabitation	52.40707	38.32239	1.37	0.172	-22.74366	127.5578
aveIncome	.0013729	.0015495	0.89	0.376	-.0016658	.0044115
_cons	60.53574	109.5725	0.55	0.581	-154.3374	275.4089

Two-step probit with endogenous regressors

Number of obs	=	2,267
Wald chi2(5)	=	209.80
Prob > chi2	=	0.0000

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
aveLoan	-.0003476	.0004186	-0.83	0.406	-.0011681	.0004729
black	.4796026	.0739883	6.48	0.000	.3345881	.624617
hispanic	.1671021	.0859693	1.94	0.052	-.0013947	.3355989
cohabitation	.480486	.0604741	7.95	0.000	.361959	.5990129
aveIncome	.0000249	2.41e-06	10.37	0.000	.0000202	.0000297
_cons	-.6797929	.1851037	-3.67	0.000	-1.04259	-.3169963

Instrumented: aveLoan
Instruments: black hispanic cohabitation aveIncome aveTuition

Wald test of exogeneity: chi2(1) = 0.59 Prob > chi2 = 0.4423

Marginal effects after ivprobit
y = Fitted values (predict)
= -.05173541

variable	dy/dx	Std. Err.	z	P>|z|	[95%	C.I.]	X
aveLoan	-.0003476	.00042	-0.83	0.406	-.001168	.000473	439.278
black*	.4796026	.07399	6.48	0.000	.334588	.624617	.199382
hispanic*	.1671021	.08597	1.94	0.052	-.001395	.335599	.185267
cohabi~n*	.480486	.06047	7.95	0.000	.361959	.599013	.517424
aveInc~e	.0000249	.00000	10.37	0.000	.00002	.00003	16258.6

(*) dy/dx is for discrete change of dummy variable from 0 to 1

B4. Detailed regression results of 2SLS on childbearing – black sample

First-stage regression

Source	SS	df	MS
Model	25113274.8	4	6278318.7
Residual	857434601	1,089	787359.597
Total	882547876	1,093	807454.599

Number of obs	=	1,094
F(4, 1089)	=	7.97
Prob > F	=	0.0000
R-squared	=	0.0285
Adj R-squared	=	0.0249
Root MSE	=	887.33

aveLoan	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
aveTuition	.0401913	.0296421	1.36	0.175	-.0179709	.0983534
male	-278.1308	54.82818	-5.07	0.000	-385.7116	-170.5499
cohabitation	23.0728	54.16022	0.43	0.670	-83.1974	129.343
aveIncome	.0075329	.002949	2.55	0.011	.0017465	.0133194
_cons	397.1598	160.0934	2.48	0.013	83.03333	711.2863

Two-step probit with endogenous regressors

Number of obs	=	1,094
Wald chi2(4)	=	82.12
Prob > chi2	=	0.0000

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
aveLoan	.0002117	.0011133	0.19	0.849	-.0019705	.0023938
male	-.1521079	.3193674	-0.48	0.634	-.7780565	.4738406
cohabitation	.6997358	.0846203	8.27	0.000	.5338831	.8655886
aveIncome	1.37e-06	9.50e-06	0.14	0.886	-.0000172	.00002
_cons	-.1094487	.6741536	-0.16	0.871	-1.430765	1.211868

Instrumented: aveLoan
Instruments: male cohabitation aveIncome aveTuition

Wald test of exogeneity: chi2(1) = 0.07 Prob > chi2 = 0.7969

Marginal effects after ivprobit
y = Fitted values (predict)
= .35267905

variable	dy/dx	Std. Err.	z	P>|z|	[95%	C.I.]	X
aveLoan	.0002117	.00111	0.19	0.849	-.00197	.002394	586.198
male*	-.1521079	.31937	-0.48	0.634	-.778056	.473841	.413163
cohabi~n*	.6997358	.08462	8.27	0.000	.533883	.865589	.550274
aveInc~e	1.37e-06	.00001	0.14	0.886	-.000017	.00002	11609.2

(*) dy/dx is for discrete change of dummy variable from 0 to 1

B5. Detailed regression results of 2SLS on childbearing – Hispanic sample

First-stage regression

Source	SS	df	MS
Model	19100762.2	4	4775190.55
Residual	540710960	872	620081.376
Total	559811722	876	639054.477

Number of obs	=	877
F(4, 872)	=	7.70
Prob > F	=	0.0000
R-squared	=	0.0341
Adj R-squared	=	0.0297
Root MSE	=	787.45

aveLoan	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
aveTuition	.1083723	.026608	4.07	0.000	.0561492	.1605955
male	-158.992	55.08039	-2.89	0.004	-267.0977	-50.88642
cohabitation	-95.47041	53.60929	-1.78	0.075	-200.6887	9.747902
aveIncome	.0067989	.002884	2.36	0.019	.0011385	.0124593
_cons	-65.28286	139.1157	-0.47	0.639	-338.3237	207.758

Two-step probit with endogenous regressors

Number of obs	=	877
Wald chi2(4)	=	65.30
Prob > chi2	=	0.0000

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
aveLoan	5.28e-06	.0004035	0.01	0.990	-.0007855	.0007961
male	-.3590351	.1125497	-3.19	0.001	-.5796285	-.1384418
cohabitation	.4331712	.0947646	4.57	0.000	.247436	.6189064
aveIncome	.0000274	5.69e-06	4.81	0.000	.0000162	.0000385
_cons	-.2948459	.2053565	-1.44	0.151	-.6973373	.1076455

Instrumented: aveLoan
Instruments: male cohabitation aveIncome aveTuition

Wald test of exogeneity: chi2(1) = 0.09 Prob > chi2 = 0.7689

Marginal effects after ivprobit
y = Fitted values (predict)
= .12760962

variable	dy/dx	Std. Err.	z	P>|z|	[95%	C.I.]	X
aveLoan	5.28e-06	.0004	0.01	0.990	-.000786	.000796	421.153
male*	-.3590351	.11255	-3.19	0.001	-.579629	-.138442	.478905
cohabi~n*	.4331712	.09476	4.57	0.000	.247436	.618906	.511973
aveInc~e	.0000274	.00001	4.81	0.000	.000016	.000039	13537.2

(*) dy/dx is for discrete change of dummy variable from 0 to 1

B6. Detailed regression results of 2SLS on childbearing – Non-black non-Hispanic sample

First-stage regression

Source	SS	df	MS			
				Number of obs	=	2,900
				F(4, 2895)	=	9.13
Model	30230704.9	4	7557676.23	Prob > F	=	0.0000
Residual	2.3952e+09	2,895	827365.196	R-squared	=	0.0125
				Adj R-squared	=	0.0111
Total	2.4255e+09	2,899	836651.585	Root MSE	=	909.6

aveLoan	Coef.	Std. Err.	t	P>|t|	[95% Conf.	Interval]
aveTuition	.0719382	.0168004	4.28	0.000	.0389963	.10488
male	-46.44223	34.6558	-1.34	0.180	-114.3948	21.51031
cohabitation	129.5357	34.41514	3.76	0.000	62.05503	197.0163
aveIncome	.0004937	.0014444	0.34	0.733	-.0023386	.0033259
_cons	57.84203	93.69902	0.62	0.537	-125.8815	241.5655

Two-step probit with endogenous regressors

Number of obs	=	2,900
Wald chi2(4)	=	110.87
Prob > chi2	=	0.0000

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
aveLoan	-.0010213	.0004056	-2.52	0.012	-.0018162	-.0002264
male	-.4212007	.0625806	-6.73	0.000	-.5438564	-.298545
cohabitation	.3996488	.0810277	4.93	0.000	.2408374	.5584602
aveIncome	.0000189	2.55e-06	7.41	0.000	.0000139	.0000239
_cons	.2087006	.1842229	1.13	0.257	-.1523695	.5697708

Instrumented: aveLoan
Instruments: male cohabitation aveIncome aveTuition

Wald test of exogeneity: chi2(1) = 9.44 Prob > chi2 = 0.0021

Marginal effects after ivprobit
y = Fitted values (predict)
= .01400159

variable	dy/dx	Std. Err.	z	P>|z|	[95%	C.I.]	X
aveLoan	-.0010213	.00041	-2.52	0.012	-.001816	-.000226	493.061
male*	-.4212007	.06258	-6.73	0.000	-.543856	-.298545	.481034
cohabi~n*	.3996488	.08103	4.93	0.000	.240837	.55846	.565862
aveInc~e	.0000189	.00000	7.41	0.000	.000014	.000024	15094.5

(*) dy/dx is for discrete change of dummy variable from 0 to 1

BIOGRAPHY

Cuicui Song is a Ph.D. candidate in the Department of Economics at Tulane University. She was raised in Xinxiang, China by her parents, Jianzhuang Song and Cunrong Wu. She has three sisters and one brother. Cuicui completed a B.A. in International Economics and Trade at Zhengzhou University, China and then obtained an M.A. in Economics from Henan Normal University and University of Nebraska-Lincoln. She currently lives with her husband ,Yanbin Lu and two sons , Adrian Lu and Nathan Lu in Kenner.