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This thesis is dedicated to my mother Dulcina Cannonier.

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ABSTRACT

This dissertation consists of three essays analyzing the role of public policy in affecting such outcomes as fertility, educational attainment and women's preferences towards fertility and sexual activity.

First, I investigate how a government educational policy affected the fertility behavior of teenagers in the United States. Specifically, Title V State Abstinence Education (SAE) program appropriates funding to states for the purpose of educating minors on abstinence before marriage. Using state level data to analyze the impact of abstinence education on the birth rates for teens; I find that for an average state, increasing spending by $50,000 per year on state abstinence education can help avoid approximately four births to teenagers.

Next, I examine the fertility impact of the Family and Medical Leave Act (FMLA). The Family and Medical Leave Act (FMLA) which was implemented in August 1993, is a federal reform requiring firms of a certain size in all states to grant job-protected leave to any employee satisfying certain eligibility criteria. One of the provisions of the FMLA is to allow women to stay at home for a maximum period of 12 weeks to give care to the new born. Using data from the 1979 cohort of the National Longitudinal Survey of Youth (NLSY79) this study analyzes whether the FMLA has influenced birth outcomes in the U.S. over the period 1989 to 2006. I find that eligible women increase the probability of giving a first and second birth by about 6 and 4.2 percentage points, respectively. Compared to other women, eligible women are giving birth to the first child 11 months earlier and about 6 months earlier for the second child.

In the final part of this research, I estimate the impact of schooling on preferences among women. I first analyze the effect of a government policy that provides free primary education on completed schooling. From this investigation, I find that increased schooling changes a

woman's preference for the demand for children, delays the start of first sexual encounter and increases the use of modern contraceptives. Increased education has also empowered women.

CHAPTER 1. INTRODUCTION

This dissertation consists of three essays analyzing the role of public policy in affecting such outcomes as fertility, educational attainment and women's preferences towards fertility and sexual activity. It has its concentration in the fields of Health Economics, Economic Demography and Public Policy which also form the bulk of my research. The organization of the dissertation is as follows. The first part, comprising two-thirds of the essays, discusses my work on the role of policy in influencing fertility while the second part focuses on the impact of education on health-related outcomes and preferences. I also discuss my future research.

In the first chapter titled "State Abstinence Education and Teen Birth Rates in the U.S.", I investigate how a government educational policy affected the fertility behavior of teenagers in the United States. Specifically, Title V, section 510 of the Social Security Act, passed in 1996 and implemented in 1998, appropriates funding to states for the purpose of educating minors on the benefits of abstinence before marriage. Despite considerable research on the impact of abstinence education on teen fertility outcomes, high quality population-level studies on state abstinence education using panel data are absent. This chapter uses state-level data over the period 1991 to 2005 to analyze the impact of abstinence education on the birth rates for teens 15-17 years by evaluating the Title V, section 510 State Abstinence Education (SAE) program. I find that for an average state, increasing spending by $50,000 per year on SAE can help avoid approximately four births to teenagers, resulting in net savings of $15,652 to the public for each birth avoided.

The second essay "Does the Family and Medical Leave Act (FMLA) increase Fertility Behavior?" is similar in spirit to the previous chapter in that it examines the fertility impact of another universal policy undertaken in the United States around the same time period. The

Family and Medical Leave Act (FMLA) which was implemented in August 1993, is a federal reform requiring firms of a certain size in all states to grant job-protected leave to any employee satisfying the eligibility criteria. One of the provisions of the FMLA is to allow women to stay at home for a maximum period of 12 weeks to give care to the new born. While it remains an important policy question, the effect of this legislation on the fertility response of eligible women has received no attention by researchers. Using data from the 1979 cohort of the National Longitudinal Survey of Youth (NLSY79) this study analyzes whether the FMLA has influenced birth outcomes in the U.S. over the period 1989 to 2006. Specifically, I evaluate the effect of the FMLA by comparing the changes in the birth hazard profiles of women who became eligible for FMLA benefits such as maternity leave, to the changes in the control group who were not eligible for such leave. Using a discrete-time hazard model, results from the difference-in-differences estimation indicate that eligible women increase the probability of giving a first and second birth by about 6 and 4.2 percentage points, respectively. Compared to other women, eligible women are giving birth to the first child 11 months earlier and about 6 months earlier for the second child. I also find that if the criteria for eligibility were relaxed, the fertility impact of the policy could almost double, possibly through its effects on a broader spectrum of the labor force.

The study of the benefits of education has occupied a considerable part of the economics literature, but a large and disproportionate amount of interest has been dedicated to the private returns to education. Recently, economists have examined the role of education in influencing other outcomes such as health (Chou, Liu, Grossman and Joyce 2010; Currie and Moretti 2003; Grossman 1972a, 1972b; Lleras-Muney 2005; Kenkel 1991). In the third essay, the role of government is once again revisited, but this time within the context of how public policy influences preferences through education. In the chapter titled "Empowering Women: The Impact of Education on Preferences – Evidence from a Policy Experiment in Sierra Leone", I estimate the impact of schooling on preferences among women. I first analyze the effect of a government policy that provides free primary education on completed schooling. Variation in individuals' exposure to the policy (identified by year of birth) and the amount of education funding led to an increase of 0.85 years of completed schooling for the affected female cohort. By combining birth year and free primary education input to instrument for schooling, I find that schooling changes a woman's preference for the demand for children, delays the start of first sexual encounter and increases the use of modern contraceptives. Specifically, an additional year of schooling reduces desired fertility by 0.37 children while it increases the delay in sexual debut by 0.24 years. The results also indicate that increased education has empowered women by influencing their opinions regarding the rights of women to decide under what conditions they have sexual relations with their husbands.

CHAPTER 2. STATE ABSTINENCE EDUCATION PROGRAMS AND TEEN BIRTH RATES IN THE U.S.

2.1. Introduction

Although teen fertility rates are on the decline in the United States, the U.S. still has one of the highest rates of teen pregnancy, ranking first amongst the industrialized nations (Guttmacher, 2002; Sen, 2003; Barnett and Hurst, 2003). The cost of childbearing is estimated to have been approximately $9.1 billion in 2006 (Hoffman, 2006), affecting a number of institutions ranging from the health care system, child welfare, and public sector health care to the state and federal prisons systems. It is estimated that the average child born to a female under the age of 19 receives assistance from the public sector amounting to $1,430 every year.[1] The aim of this chapter is to use state-level panel data from the U.S. to examine the impact on teen births in relation to a federal government-sponsored State Abstinence Education (SAE) program that promotes the importance and consequences of abstaining from premarital sex.

Several studies have used state-level data to estimate pregnancy, births or abortions as a function of a set of determinants such as abstinence, abortion laws or some forms of sex education. For instance, the results of Levine (2003) provide strong support for an impact of parental-consent abortion laws on teen pregnancy rates, but no impact on births. Kearney and Levine (2009) find a decline in births when cheaper family planning services are made available to higher-income women. However, Paton (2002) finds no evidence that greater access to family planning services helps in reducing underage births or abortions.

[1] Monetary values are in 2004 dollars. For the period 1991-2004, it was estimated that taxpayers contributed $161 billion to support teen childbearing. Further, Hoffman (2006) states that the reduction in teen birth rates during the same period has resulted in an estimated savings of $6.8 billion in 2004. Private expenditures or savings are not included in these estimates.

Many previous studies investigating the effects of various public policy interventions have relied on randomized-control trials (RCTs).[2] Randomized experiments have a superior advantage in terms of their internal validity, which other methods such as difference-in-differences do not readily provide. If successful random assignment to control and treatment groups are undertaken, RCTs can provide accurate and useful information on observable differences that are attributable to intervention and not to confounding influences. However, controlled experiments lack external validity. These experiments tend to focus on a specific population with particular characteristics in a controlled environment and for this reason, the results cannot be readily generalized to other settings. Furthermore, a challenge for controlled designs is the potential for the control group, especially in the case of teens, to change its behavior in rather unexpected ways (Kirby et al., 1994) or for the treatment group to refuse program intervention. Although a large number of RCTs have been conducted to analyze the impact of abstinence education, population-level studies that use state-level panel data are absent. Despite the preponderance of individual-level studies, because of the shortcomings identified above, there is the possibility for the results of these studies to be overestimated and the external validity could be brought into question. This state-level analysis is designed to address the lack of external validity and to complement the work done in previous studies.

Using state-level panel data, I present evidence that SAE programs supported by the Federal Title V, Section 510 grants have led to a statistically significant decline in teen birth rates. I use difference-in-differences analysis where teens 15-17 years are the group targeted for

[2] An RCT is a trial involving two randomly selected groups: one is the experimental group, which receives the intervention that is being tested, and the other is the comparison or control group, which receives a different treatment. The different treatment may in fact be a placebo (fake or inertial treatment). Neither of the groups knows whether it is receiving the "real" treatment or the "control" one. For an example, see the meta-analysis by DiCenso et al. (2002).

the program, while 25-29 year-olds are used as the control group. The findings are robust to various model specifications, as well as to various control groups.

The remainder of the chapter is organized as follows: Section 2.2 presents background information on recent trends in teen birth rates in the U.S., institutional details on the Title V SAE program, analytical framework and a literature review of studies related to this research. In Section 2.3, I describe the data used in the analysis, while Section 2.4 describes the empirical strategy. The results of my findings are provided in Section 2.5, which is followed by sensitivity analyses in Section 2.6. Section 2.7 addresses some caveats to the research, as well as discusses some policy issues. Section 2.8 concludes.

2.2. Background

2.2.1. Teen Birth Rates in the United States

The U.S. has one of the highest teen birth rates in the developed world and there is reasonable consensus that the present rates are a source of concern from a social and economic standpoint. In 2005, there were approximately 415,000 teen births to women age 15-19, or roughly 41 out of every 1,000 females in this age range give birth. Teen birth rates in the U.S. continue to witness a decline, albeit at a slower pace, for more than a decade. To put into perspective, since their peak in 1991, teen birth rates have declined by 34 percent with the impact being largest amongst 15-17 year olds (Martin et. al., 2007). An increasing number of studies have concluded that abstinence and improved contraceptive practices are influential in causing much of the decline (Santelli et al., 2007).

There is substantial variation in teen birth rates across different strata: race, ethnicity and state of residence. In 2005, the birth rate among non-Hispanic Black teens was 60.9 per 1,000 which was more than twice the rate of 25.9 per 1,000 among non-Hispanic Whites. For

Hispanic teens, this rate was 81.7 per 1,000. Among states, birth rates to teens were lowest in New Hampshire with 17.9 births per 1,000 girls age 15-19 while the highest rate of 63.4 per 1,000 was recorded in the District of Columbia (Martin et al., 2007).

For more than a decade, federal and state policies on sex education have been skewed in favor of abstinence education which teaches the harmful physiological and psychological effects of premarital sex. While both forms of sex education – abstinence and contraception - are encouraged and allowed, none of the states require that contraception use be stressed. Sex education with a contraceptive focus is taught within the context of the riskiness in relying on failure-prone mechanisms. Currently, there is no federal government-funded program for comprehensive sex education while funding for abstinence-centered programs have been channeled through three main streams: the Adolescent Family Life Act (AFLA) in 1981, Section 510 of the Social Security Act (Title V) in 1996 and the Community-Based Abstinence Education (CBAE) in 2000. Greater emphasis has been placed on funding the Title V program.

There are several reasons why this has been the case. Total funding under the AFLA has been small compared to SAE funding which has tended to be at least four times as large. Further, less than half of the funding under the AFLA has been allocated for the prevention of teen pregnancy. The AFLA program is still ongoing. Under the CBAE arrangement, funding for abstinence education has been performed solely at the community level without any state approval. That is, grants awarded under the CBAE went directly to community-based organizations from the U.S. Department of Health and Human Services (HHS). In the case of the SAE program, all funding for abstinence education require state approval. Over time, both the AFLA and the CBAE have undergone legislative changes because of a perceived lack of focus or the restrictive nature of the programs.[3]

2.2.2. Title V State Abstinence Education (SAE) Program

Since 1998, in accordance with Section 510, Title V of the Social Security Act passed in 1996; the Federal government is mandated to make funds available to all states for the specific purpose of teaching and educating children to refrain from premarital sex through a State Abstinence Education (SAE) program.[4] The aim of the program is to educate and promote the idea that abstinence is the preferred way of preventing pre-marital teen pregnancy and births. The motivation for Title V arose out of the concern for the high rates of teen pregnancy prior to the 1990s and the substantial social costs to the public sector.

In 1998, the law required that the Federal government appropriates $250 million for states over five years ending in 2002. A criterion for receiving these funds includes a state contribution of three dollars for every four dollars of program-associated federal grants it receives. Thus, if states are to use all of the annual federal appropriation of $50 million in any one year, then the total grants available for the SAE program are estimated to be $87.5 million.[5] In 2002, the $50 million per year Federal Title V SAE program was extended for another five years until 2007. Since then, the U.S. Congress has made a request for further appropriations.

[3] See Advocates for Youth website the ACF website http://www.advocatesforyouth.org/index.php/publications/429.html?task=view (Accessed December 14, 2010).

[4] Since June 2004, the SAE program has been managed and administered by the Administration for Children and Families (ACF) as well as Family and Youth Services Bureau (FYSB). Previously, this was the responsibility of the Health Resources and Services Administration's (HRSA) Maternal and Child Health Bureau (MCHB). See the ACF website http://www.acf.hhs.gov/programs/fysb/content/abstinence/factsheet.htm (Accessed March 3, 2009).

[5] Monies are distributed between states according to the following process: Grants are awarded to states based on a statutory formula which is partly determined by the proportion of low-income children in a state to the total number of low-income children in all states according to the latest census data. The state is required to match 75 percent of Title V funds. See the ACF website http://www.acf.hhs.gov/programs/fysb/content/abstinence/factsheet.htm (Accessed March 3, 2009).

The target groups are children within the age range of 12-17 years. The Federal government provides funding in the form of grants to states; and there is "great diversity in how Section 510 abstinence funds are distributed" (Mathematica Policy Research, 2009). In any given year, the annual $50 million appropriation is divided into 80-90 awards, each with a value between $250,000 and $650,000. In the majority of the cases, states implement the SAE grants through local organizations, which may include churches and other non-profit organizations.[6] All users of the federal government funds must be guided by the "A-H definition".[7]

2.2.3. Analytical Framework and Related Literature

Education has long been considered an important factor in determining fertility and the behaviors that affect fertility outcomes. Three main channels have emphasized the causal effect of education. In the first, education increases an individual's permanent income through improved employment outcomes. As a result, the opportunity cost of raising children (in terms of potential foregone earnings) rises and so the optimal fertility choices result in fewer but higher

[6] Massachusetts is the only state that utilizes all of its funding in one block.

[7] This is not the first for abstinence-only education. Such programs have been ongoing for a few decades as far back as 1981 during the Ronald Regan administration (see Economist article "Just Say No" at www.economist.com, accessed February 12, 2009). However, the programs today are unique in their stated requirements and goals.
The "A-H" definitions require that each grant must:

A. Have as its exclusive purpose teaching the social, psychological, and health gains to be realized by abstaining from sexual activity,
B. Teach abstinence from sexual activity outside marriage as the expected standard for all school-age children,
C. Teach that abstinence from sexual activity is the only certain way to avoid out-of-wedlock pregnancy, sexually transmitted diseases, and other associated health problems,
D. Teach that a mutually faithful, monogamous relationship in the context of marriage is the expected standard of sexual activity,
E. Teach that sexual activity outside the context of marriage is likely to have harmful psychological and physical effects,
F. Teach that bearing children out-of-wedlock is likely to have harmful consequences for the child, the child's parents, and society,
G. Teach young people how to reject sexual advances and how alcohol and drug use increases vulnerability to sexual advances, and
H. Teach the importance of attaining self-sufficiency before engaging in sexual activity.

quality children (Becker, 1965; Becker et al., 1990). The second channel posits that higher educated females seek higher educated males (assortative behavior), causing permanent income to rise further with implications for fertility choices (Behrman and Rosenzweig, 2002). The third – allocative mechanism - suggests that education improves an individual's knowledge, thus increasing the ability to efficiently process this information in making optimal fertility choices (Grossman, 1972a).

The effect of education on fertility choices may be analyzed in the spirit of Grossman's (1972a, 2005) efficiency allocation framework, whereby higher educated individuals have access to more information to make the optimal fertility choices. Simultaneously, with this information, they also know the resultant harmful effects of choosing a sub-optimal path such as early childbearing. The problems of two-way causality and the possibility of third-variables influencing outcomes are also discussed in detail in Grossman (2005). These issues, as they pertain to this work, are addressed later in the chapter. These ideas can be summarized as

$$Y=f(C, X) \tag{2.1}$$

where Y measures some fertility outcome and C represents choices influencing fertility outcomes. For example, C may stand for whether a teenager decides to abstain or practice contraceptive use, both of which are choice variables affecting fertility. The vector X denotes all other factors that can affect the outcome.

As argued above, it is also true that schooling can influence the choices made as it relates to fertility. If schooling does not influence the exogenous factors in the vector X, choice outcomes may be analyzed as follows:

$$C=g(S, X) \tag{2.2}$$

where S is a schooling variable which also includes sex education. Substituting Equation (2.2) into Equation (2.1) yields

$$Y=h(S, X) \quad (2.3)$$

This is the reduced-form equation which is estimated in the chapter.

Following the general lack of consensus in explaining the unprecedented rise in pregnancies in the 1970s and 80s (Akerlof et al., 1996; Willis, 1999) and the subsequent decline in the 1990s, more attention has been paid to the impact of changes in teen fertility behavior as a result of increased sex education. Building on previous theories, economists such as Oettinger (1999) have incorporated rational choice models to explain how information affects individual behavior through a change in utility.

A number of policy approaches have been used to address the problems of early pregnancy and childbearing. There are three broad policy initiatives: those that increase access to contraceptives; those that alter the financial costs and incentives to childbearing; and those that highlight information through targeted interventions within schools and communities.[8] The latter consists of three categories: sex education with a focus on abstinence, sex education with a focus on contraceptive use, and a multi-component youth development program.[9] The investigation undertaken in this chapter addresses the last policy issue.

A number of studies, albeit with mixed results, have examined the relationship between abstinence education and pregnancy, and birth outcomes (Frost and Forrest, 1995; Doniger et al.,

[8] An example of altering the incentives to childbearing is the Family Cap welfare reform policy in the mid-1990s, which ended the practice of providing families on welfare with cash benefits upon the birth of a new child (Kearney, 2008).

[9] While this paper focuses on the first category, the youth development program – a relatively expensive initiative - incorporates a more holistic approach to addressing the issue of teen childbearing. See Kearney (2008) for a discussion of this issue.

2001; DiCenso et al., 2002). One major criticism of these studies is the over-representation of a particular group of participants. That is, the tendency has been to choose a disproportionate number of African-Americans and Hispanics, which represent the lower economic groups in society. Other wide-ranging studies that are void of this critique have also found mixed results in the outcomes of abstinence education on sexual behavior and fertility (Kirby et al., 1994; Cabezón et al., 2005; Borawski et al., 2005).

Closer to this chapter have been several studies analyzing the impact of Title V SAE programs; but none has been conducted using extensive state-level panel data analysis. Those studies that have utilized panel data have been limited to very short time-series components with the individual being the unit of analysis. The results have been mixed with some noting the ineffectiveness of the Title V SAE program (Barnett and Hurst, 2003; Devaney et al., 2008) while others (Denny et al., 1999; Carter-Jessop et al., 2000; Lieberman et al., 2000) have found that the programs have resulted in a decline in sexual activity. Card (1999, p. 279) has argued that the effects of SAE programs are still inconclusive, noting that abstinence education programs have been "a partial contributor to the country's focus on the problem of teen pregnancy, a focus that has led to the…decline in the teen birth rate."

Recently, one notable study examining the impact of Title V, Section 510 programs over a multi-year period is the April 2007 evaluation report by Mathematica Policy Research. The evaluation is a study of four abstinence programs.[10] The report has found that "none of the individual programs had statistically significant impacts on the rate of sexual abstinence…" (Mathematica Policy Research, 2007, p. 30). Despite carrying out a "gold standard" randomized trial design (Kearney, 2008), this study is not without concerns, some of which have been

[10]The programs are *My Choice, My Future* in Virginia; *ReCapturing the Vision* in Florida; *Families United to Prevent Teen Pregnancy* in Wisconsin; and *Teens in Control* in Mississippi.

highlighted in the Mathematica report. First, the results of the evaluation cannot be generalized for all teens, since the "findings provide no information on the effects the programs might have if they were implemented for high school youth..." (Mathematica Policy Research, 2007, p. 61).[11] A second concern relates to the possible lack of statistical power of the evaluation tests given the small sizes (including control group), which range from 447 to 714 individuals. This chapter uses a multi-year approach, but makes a different contribution relative to the Mathematica study, incorporating state-level panels in a difference-in-differences setting, controlling for observable and unobservable factors in further investigating the causal nature of the SAE program.

2.3. Data

The data consist of a panel of 49 U.S. states (including the District of Columbia) that receive funding for SAE as per Title V, section 510 of the Social Security Act.[12] I exclude the state of California because it has never applied for any funding during these years.[13] The observations are annual and span the period 1991-2005.[14]

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[11]Within the Mathematica report, the RCT programs being analyzed were aimed at children who attended elementary and middle schools.

[12] All states, with the exception of California, have received SAE funding from the Federal government since the implementation of the program in 1998. However, in 1997 a few states had been engaged in teaching abstinence using funding received from a Title XX grant under the Ronald Reagan Administration, also known as Adolescent Family Life Act (AFLA) enacted in 1981. These "modest" abstinence programs were supported by a Federal reform initiative called the Personal Responsibility and Work Opportunity Act of 1996 (Maureen Duran, "Re: Effects of Funding," email message to author, May 2008).

[13] Later, I include California in the analysis but the results remain largely unaffected.

[14] The period 1991-2005 is chosen since, in difference-in-differences estimation, we are examining the effect of the treatment, SAE (which began in 1998) by comparing the treatment group (birth rates for 15-17 year olds) after treatment, both to the treatment group before treatment and to a control group (birth rates for 25-29 year olds). Therefore it is necessary to have information before the treatment has started. The choice of period coverage is also based on data availability and also for the fact that teen birth rates began to fall in the early 1990s. As I show later, using different sample periods (sub-samples) still produces results, which are consistent with the main findings.

2.3.1. Birth Rates and State Abstinence Education (SAE) Funding

The birth rates for females 15-17 and 25-29 years are obtained from the Center for Disease Control (CDC), which publishes annual data on the number of births by state, age group and race. [15] Females who give birth during the ages 25-29 are considered to be the control group, while those giving birth during minor years act as the treatment group. Birth rates are expressed as births per 1,000 female age-specific population. I also analyze birth rates by race and ethnicity.

The data on Title V, section 510 SAE funding for each state and year come from the U.S. Department of Health and Human Services Administration for Children and Families (ACF). The Title V program has been in existence since 1998 and each subsequent year the Federal government has appropriated $50 million for the SAE program. The distribution of funds to each state is based on a formula that takes into account the proportion of low-income children in a state to the total number of low-income children in all states. Each state is then required to provide 75 percent of this amount in matching funds. The SAE data include the matching amount. Since the data are reported in current dollars, the dollar amounts are expressed in real terms using the consumer price index, which is obtained from the Federal Reserve Bank of Minneapolis.[16] To obtain the per capita SAE funding in real terms I then divide each state's SAE funds expressed in real terms by its population. In other specifications, I use other categories of population to deflate real SAE funds (example: population of 15-19 and 10-19 year-olds).

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[15] See website http://www.cdc.gov/nchs/vitalstats.htm (Accessed February 15, 2009).

[16] The base year for the consumer price index series from the Federal Reserve Bank of Minneapolis is 1982-1984. In this analysis, I use 2005 as the base year. See website http://www.minneapolisfed.org/ (Accessed February 23, 2009).

During the period under investigation, all states with the exception of California have requested Title V SAE funding.[17]

Table 2.1 displays summary statistics for the birth rates and SAE per capita funding variables that I use in this analysis. The definition of each variable is given in the first column. Columns 2-4 show the descriptive statistics (mean, standard deviation and number of observations) for all state and year combinations in the sample. The table also provides variable definitions and descriptive statistics for other controls used in the model.

2.3.2. Other Controls

In the regressions, I include social, economic, demographic as well as medical and health policy variables to control for other factors that affect teen birth rates. State and year fixed-effects are also included in all model specifications. All data sources are listed in Table A.1. Teen birth rates are likely to be affected by the rape rate. A number of studies, such as Donovan (1996), consider the effects of statutory rape on adolescent pregnancy, where some findings reveal at least 50 percent of children born to minors have an adult male as the father. The rape rate is calculated as the number of rapes per 100,000 of the population and is obtained from the Federal Bureau of Investigation (FBI) Uniform Crime Reports (UCRs). The robbery rate is also obtained from the FBI UCRs and it measures the number of robberies per 100,000 population. I control for another related variable, imprisonment rate, which measures the number of incarcerations per 100,000 population. These variables are proxies for difficult-to-observe state characteristics that may be correlated with teen births. The data source is the same as the previous two variables.

[17] This is important because this situation is like an exogenous event since all states accept SAE funding. If for some reason, some states decide not to take the SAE funding while others access it, then this creates an endogenous event.

Table 2.1: Summary Statistics: Difference-in-Differences Analysis for the period 1991-2005

Variables	Variable Definition	Mean	Std. Dev.	Obs.
	(1)	(2)	(3)	(4)
Birth rates (All)	Births per 1000 female population 15-17 and 25-29 years	71.59	45.608	1470
Birth rates (All Blacks)	Births per 1000 Black female population 15-17 and 25-29 years	77.59	33.05	1470
Birth rates (All Hispanics)	Births per 1000 Hispanic female population 15-17 and 25-29 years	83.00	43.62	1468
Birth rates (All Whites)	Births per 1000 White female population 15-17 and 25-29 years	68.92	48.90	1470
Birth rates (All)				
Age 15-17	Births per 1000 population of all females 15-17 years	28.293	11.736	735
Age 25-29	Births per 1000 population of all females 25-29 years	114.888	16.460	735
Birth rates (Black)				
Age 15-17	Births per 1000 population of Black females 15-17 years	53.002	24.013	735
Age 25-29	Births per 1000 population of Black females 25-219 years	102.179	19.968	735
Birth rates (Hispanic)				
Age 15-17	Births per 1000 population of Hispanic females 15-17 years	54.608	21.205	735
Age 25-29	Births per 1000 population of Hispanic females 25-219 years	111.469	41.721	733
Birth rates (White)				
Age 15-17	Births per 1000 population of White females 15-17 years	22.492	8.954	735
Age 25-29	Births per 1000 population of White females 25-29 years	115.351	19.192	735

Notes: Refer to the Appendix for a complete description of all variables and sources of the data.

(Table 2.1 continued)

Variables	Variable Definition	Mean	Std. Dev.	Obs.
	(1)	(2)	(3)	(4)
SAE Funding	State Abstinence education funding in real (2005) dollars per capita	0.169	0.184	1470
Rape	Rape rate: rapes per 100,000 of the population	36.982	13.255	1470
Robbery	Robbery rate: robberies per 100,000 of the population	143.429	144.269	1470
Poverty	The percentage of the population below the threshold poverty level	12.785	3.750	1470
High School grad. rate	Percentage of students graduating from high school (as a percentage of entering cohort)	70.485	9.067	1470
Unemployment	Unemployment rate: percent of persons unemployed	5.127	1.425	1470
Prison	Prison (incarceration) rate: the number of persons in prison per 100,000 of population	394.541	238.198	1470
Notification	Equals one if parental notification/informed consent laws exist, equals zero otherwise	0.627	0.483	1470
Abortion Medicaid	Equals one if Medicaid pays for therapeutic abortion, equals zero otherwise	0.279	0.448	1470
Hispanic	Proportion of the population consisting of Hispanics	0.066	0.080	1470
Black	Proportion of population consisting of Blacks	0.114	0.120	1470
Urbanization rate	Proportion of the population living in the urban area	0.717	0.151	1470

Notes: Refer to the Appendix for a complete description of all variables and sources of the data.

(Table 2.1 continued)

Variables	Variable Definition	Mean	Std. Dev.	Obs.
	(1)	(2)	(3)	(4)
	Changes in…			
ΔBirth rates (All)	Births per 1000 population of all females 15-17 and 25-29 years	-0.55	2.55	1372
ΔBirth rates (All Blacks)	Births per 1000 population of Black females 15-17 and 25-29 years	-1.75	14.17	1372
ΔBirth rates (All Hispanics)	Births per 1000 population of Hispanic females 15-17 and 25-29 years	3.13	14.40	1370
ΔBirth rates (All Whites)	Births per 1000 population of Whites females 15-17 and 25-29 years	-0.273	2.598	1372
ΔBirth rates (All)				
Age 15-17	Births per 1000 population of all females 15-17 years	-1.133	1.772	686
Age 25-29	Births per 1000 population of all females 25-29 years	0.033	3.031	686
ΔBirth rates (Black)				
Age 15-17	Births per 1000 population of Blacks females 15-17 years	3.105	9.749	686
Age 25-29	Births per 1000 population of Blacks females 25-219 years	-0.404	17.415	686
ΔBirth rates (Hispanic)				
Age 15-17	Births per 1000 population of Hispanic females 15-17 years	0.149	9.453	686
Age 25-29	Births per 1000 population of Hispanic females 25-219 years	6.127	17.558	684
ΔBirth rates (White)				
Age 15-17	Births per 1000 population of White females 15-17 years	-0.722	1.797	686
Age 25-29	Births per 1000 population of White females 25-29 years	0.176	3.142	686

Notes: Refer to the Appendix for a complete description of all variables and sources of the data.

(Table 2.1 continued)

Variables	Variable Definition	Mean	Std. Dev.	Obs.
	(1)	(2)	(3)	(4)
	Changes in…			
ΔSAE Funding	State Abstinence education funding in real (2005) dollars per capita	0.018	0.11	1372
ΔRape	Rape rate: rapes per 100,000 of the population	-0.509	4.272	1372
ΔRobbery	Robbery rate: robberies per 100,000 of the population	-5.265	22.647	1372
ΔPoverty	The percentage of the population below the threshold poverty level	-0.008	1.611	1372
ΔHigh School grad. rate	Percentage of students graduating from high school (as a percentage of entering cohort)	0.060	5.023	1372
ΔUnemployment	Unemployment rate: percent of persons unemployed	-0.141	0.644	1372
ΔPrison	Prison (incarceration) rate: the number of persons in prison per 100,000 of population	7.83	35.543	1372
ΔNotification	Equals one if parental notification/informed consent laws exist, equals zero otherwise	0.039	0.227	1372
ΔAbortion Medicaid	Equals one if Medicaid pays for therapeutic abortion, equals zero otherwise	0.016	0.188	1372
ΔHispanic	Proportion of the population consisting of Hispanics	0.002	0.0037	1372
ΔBlack	Proportion of population consisting of Blacks	0.0003	0.0023	1372
ΔUrbanization rate	Proportion of the population living in the urban area	0.717	0.151	1372

Notes: Refer to the Appendix for a complete description of all variables and sources of the data.

Economic conditions may affect adolescent birth rates. Therefore, I include the poverty rate, which is the percentage of the population below the poverty level. The data come from a variety of sources including the Census Bureau and the National Center for Education Statistics (NCES). I also control for the proportion of the state's population that is Black and the proportion of the state's population consisting of Hispanics. Both sets of variables come from the Population Division of the U.S. Census Bureau. High school graduation rate is also included, as some studies show a strong association between teen child bearing and high school completion (Hofferth et al., 2001). The variable high school graduation rate is constructed as the percentage of students graduating from high school.[18] I also control for the rate of urbanization of the state.[19]

Finally, I consider two health policy variables, which are indicator variables to represent whether or not a particular type of abortion restriction has been in place in a state in a given year. The first is Medicaid funding restriction, which prohibits the use of public resources to perform therapeutic abortions. This variable equals one for states where Medicaid pays for abortions and zero otherwise. The second restriction is a parental consent/notification or informed law, which requires a minor to notify, obtain consent from a parent or be given professional advice from medical personnel before proceeding with an abortion. The variable takes the value of one if such a state has a parental consent/notification or informed law in place in that year, otherwise the variable assumes a zero value. The information in constructing these variables comes from various issues of the Guttmacher Report on Public Policy, other reports from the Guttmacher

[18] Swanson (2004) provides an extensive account on how this variable is derived.

[19] I thank Naci Mocan for providing the data.

Institute and from various publication issues of the National Abortion and Reproductive Rights Action League (NARAL).[20]

2.4. Empirical Strategy

The empirical strategy is to estimate the impact of Title V SAE on the fertility rates of 12-17 year olds. The first procedure is to estimate a regression model in which the birth rate for teens 15-17 years old is explained by abstinence education through Title V SAE funding.[21] Equation (2.4) depicts this model:

$$\text{Birthrate}_{it} = \alpha_0 + \alpha_1 \text{SAE}_{it} + \alpha_2 \mathbf{X}_{it} + \lambda_i + \gamma_t + \eta_{it} + e_{it} \tag{2.4}$$

where i is an index for state and t indexes years. The dependent variable is the birth rate for 15-17 year-olds, SAE_{it} is the real per capita funding for SAE programs, which occur after the introduction of Title V. The vector $\boldsymbol{X}$ includes other variables (which I described earlier) that may be determinants of birth rates, such as demographic characteristics in the state, socio-economic conditions in the state, as well as medical and health policy variables of the state. I also control for state fixed-effects (λ_i), year fixed-effects (γ_t) and state-specific linear trends (η_{it}). The random error term is e_{it}.

In the specification above, a significant relationship between SAE funding and birth rates of 15-17 year-olds could be due to correlation between SAE funding and birth rates in general, leading to a spurious relationship. Therefore, I use the specification in Equation (2.4) to obtain

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[20] NARAL produces the publication "Who Decides? A State-by-State Report on the Status of Women's Reproductive Rights" that provides information on state laws and other legislative information that are related to reproductive rights.

[21] I use 15-17 year-olds as the treatment instead of 12-17 year-olds because data on the latter age group are not readily available over the length of the period under consideration. Further, the available births data for the excluded 12-14 year-olds account for a relatively small proportion of births for 12-17 year-olds.

counterfactual estimates of the impact of SAE funding on birth rates of older women, specifically 18-19 and 25-29 year-olds. The estimates should provide some measure of any benefits of the SAE program to older women. Since SAE is explicitly geared towards school-aged teens, then I do not anticipate any statistically significant results from this experiment.

As a second procedure, I employ a difference-in-differences method with the variables in first-differences exploiting the variation in the birth rates across states and time.[22] Difference-in-differences estimation allows the comparison of teen birth rates before and after the introduction of Title V SAE between 15-17 year-olds and 25-29 year-olds. Specifically, I estimate the effect of SAE funding on birth rates using first-differences depicted in Equation (2.5):

$$\Delta \text{Birthrate}_{ait} = \beta_0 + \beta_1 \Delta \text{SAE}_{it} + \beta_2 \text{Young}_{it} + \beta_3(\text{Young}_{it} * \Delta \text{SAE}_{it}) + \beta_4 \Delta \mathbf{X}_{it} + \lambda_i + \gamma_t + e_{it} \quad (2.5)$$

where the dependent variable is the change in birth rates from one period to the next for age group *a* where *a* stands for females 15-17 years old (the treatment group) or those in the control group who are older. On the right-hand side of the equation, ΔSAE_{it} is the change in real per capita funding for SAE programs, which occur after the introduction of Title V; $Young_{it}$ is a binary variable that equals one for birth rates for teens 15-17 years old and equals zero otherwise.[23] The coefficient on the interaction term, ($Young_{it} * \Delta SAE_{it}$) captures the association between differences in changes in birth rates specific to the treatment group (females 15-17 years old) relative to the control group in all states in the years after the SAE program was

[22] In addition, first-differencing eliminates different trends in age-specific birth rates that may exist.

[23] The SAE amount comprises the Title V, section 510 allocations from the federal government plus the 75 percent matching contribution of each state.

implemented (relative to the period prior to the Title V Section 510 law).[24] This is the difference-in-differences estimator of the effects of the change in SAE funding on changes in birth rates. The vector $\boldsymbol{X}$ is the same as the specification in Equation (2.4). Although state fixed-effects drop out by differencing, I include state dummies and keep the year dummies. The random error term is e_{it}. The identifying assumption is that any relative shift in the birth rate for teens 15-17 years is due to the implementation of the Title V SAE program.

In this analysis, a preferred control group might be females 25-29 years old. The reason for this selected group is non-trivial. The selection of this particular age group of females ensures there is no contamination of either group with the other during the period of SAE funding, which spans the period 1998-2005 in the sample. In this instance, females 25-29 years old in 2005 will have been in the 18-22 years range in 1998, which marks the start of the SAE program. At the same time, females 15-17 years and who have started the SAE program in 1998 will never become part of the control group even into the year 2005 where they will be 22-24 years old. Notwithstanding, consideration is given to the significant physiological differences between these two groups and so I use a younger control group of 18-19 year-olds in a restricted sample. I investigate this in the section on sensitivity analysis.

Figure 2.1 shows trends in birth rates for minors and older females 25-29 years for all states before and after the introduction of the SAE program. Birth rates for females 15-17 (25-29) years are declining (increasing) even before the Title V SAE program was implemented. This is important since time trends must be held constant to accurately estimate any effect of the program. These different age-specific trends in birth rates occur before the implementation of the Title V program and are not directly related to the SAE program. The presence of these

[24] The impact of SAE funding on birth rates of the older group (control) is β_1, the impact on the 15-17 year-olds is ($\beta_1+\beta_3$). Thus, the differential impact is represented by β_3, which is the coefficient of interest.

trends poses potential identification problems; and to eliminate the different trends that may exist, I perform the analysis in first-differences.

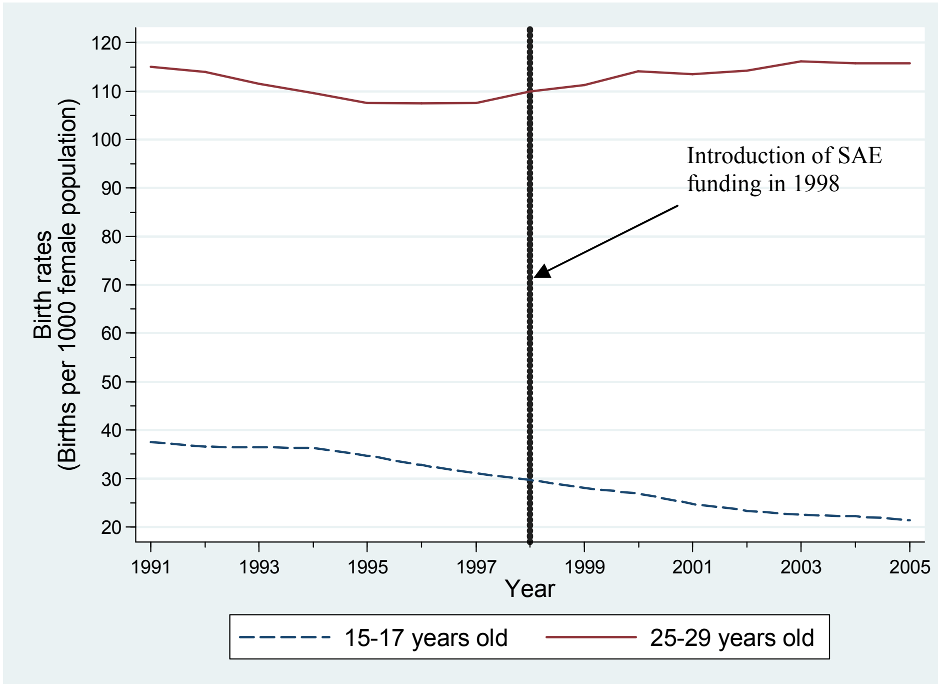

Figure 2.1: Trends in U.S. Birth Rates

2.5. Results

2.5.1. The Impact of State Abstinence Education on Births to Minors

Using the model in Equation (2.4), I estimate the relationship between Title V SAE funding and birth rates for females 15-17 years old. The results are reported in Table 2.2 where the main coefficient of interest is that related to the SAE funding variable. There is indication that SAE through Title V funding is associated with a decline in birth rates for females 15-17 years. Columns (1) and (2) of Table 2.2 present the results where the dependent variable is birth rates for 15-17 year-olds. Both specifications are similar except that column (2) includes a linear state-specific trend. In both specifications, the coefficient is negative and statistically significant.

These results may be due to correlation between funding and birth rates in general. If there is correlation between funding and birth rates for older women, then this can have implications in any effort to establish some possible causal relationship between abstinence education and birth rates for minors. I address this by performing a counterfactual analysis by running regressions similar to the specifications in the first two columns of Table 2.2, except that the dependent variable is birth rates for females 18-19 years (columns 3 and 4) and birth rates for females 25-29 years (columns 5 and 6). In all four specifications, abstinence education through Title V funding has no significant impact on birth rates for older women.

The above results provide a basis from which further analysis can be undertaken to investigate the impact of the program on teen birth rates using difference-in-differences analysis.[25]

2.5.2. Difference-in-Differences

Table 2.3, columns (1)-(2), shows the birth rates for females 15-17 years and 25-29 years, before and after the implementation of the SAE program in 1998. Prior to the introduction of the SAE program, the birth rate (births per 1,000 age-specific female population) for 15-17 year-olds was 33.6, compared to 113.3 for older females. After the introduction of the SAE program, the birth rates for 15-17 year-olds fell to 23.7, compared to an increase to 116.3 for 25-29 year-olds. Column (3) shows the difference in these two differences. The implied impact of Title V,

[25] Although not statistically significant, the coefficient estimate on the *SAE Funding* term for 25-29 year-olds appears quite large. This can influence the difference-in-differences estimation strategy when this older group of women is used as the control group. Upon closer examination, although the coefficient is large in absolute value, the change in birth rates compared to the baseline scenario is smaller than the changes which occur for 18-19 year-olds. For example, the coefficient of 2.122 (column 5, Table 2.2) implies an increase of less than a two percent change in birth rates for women 25-29 years. On the other hand, for the 18-19 year-olds with mean birth rate of about 28, the coefficient of 1.453 (column 2, Table 2.2) implies a more than 5 percent change. In addition, the results are statistically significant and of the expected sign for teenagers while for the older groups they are not significant.

section 510 SAE program on birth rates for younger females is a decline of 12.9 births per 1,000 age-specific females. In columns (4)-(6), I show the same results in first-differences terms, which suggest that the implied effect of changes in the SAE funding on changes in birth rates for 15-17 year-olds compared to older females is 2.1. With no control variables, these are the results from a difference-in-differences model in its simplest form. As explained earlier (see Figure 2.1), there were trending patterns in birth rates before the introduction of any Title V SAE program. This highlights the need to control for the differential trends across state and time in an econometric model.

Table 2.4 reports the difference-in-differences estimates from a regression model of the form of Equation (2.5), where the dependent variable is the change in birth rate. All variables are in first-differences. Taking first-differences allows for the elimination of trends. This is potentially important as the treatment and control group, with each containing births to women of different ages, may have differential trends over time.[26] In all specifications of the model, I cluster standard errors at the state level.[27] Although not reported, all results include the full set of controls, state fixed-effects and year fixed-effects. In column (1), which I consider as the base specification, the real SAE funding variable is deflated by the total population to obtain the per capita term. The Title V SAE program leads to a decrease in the birth rates for 15-17 year-olds; and this effect is statistically significant. Given the potential for birth rates to be influenced by

[26] There is also a statistical advantage to using the first-differenced model. If the errors in the first-differences model are serially uncorrelated, then it can be "shown that the first-difference estimator is most efficient…" (Wooldridge, 2002, p. 281). I test for serial correlation in the first-differences model by regressing the residual errors (obtained from a regression of Equation 5) on the lagged values. If there is a statistically significant coefficient on the lagged residual term this will indicate the presence of serial correlation. The coefficient of the lagged residual was -0.026 with a p-value of 0.337 and t-statistic of -0.96, suggesting there is no evidence of serial correlation.

[27] Angrist and Pischke (2008) also suggest that "reliable inference using a standard cluster adjustment" is achieved if the number of clusters is reasonably large (not less than 42). The authors further explain that "as far as serial correlation goes, most of the evidence suggests that when you are lucky enough to do research on US states…you are on reasonably safe ground..." (pp. 175-176). The idea is that if there are too few clusters, serial correlation will tend to be underestimated. This analysis contains 49 clusters.

non-linear effects, I expand the specification in column 1 to include a complete set of interactions: state*year, state*age and age*year effects. The results are shown in column 2. In this specification, the coefficient on the interaction term ΔSAE Funding*Young, increases in magnitude and remains negative and significant.

In columns 3 and 4 of Table 2.4, I extend the analysis of the impact of the SAE funding on birth rates by investigating different specifications of the real SAE funding variable deflated by different population subgroups. Whereas in columns 1 and 2 real SAE funding is deflated by total population to obtain real SAE funding per capita; in columns 3 and 4, I instead deflate the real SAE funding variable by the population of 15-19 year olds; while in columns 5 and 6, real SAE funding variable is deflated by the population of 10-19 year olds. Apart from using the various different population sub-groups as deflators, the column 1 and 2 are respectively identical to those in columns 3 and 4 and columns 5 and 6. Even with the change in specification of the real SAE funding variable, the results of the differential impact of SAE funding on birth rates for teens 15-17 years remain negative and statistically significant.

To put into context, consider the result in column 1 of Table 2.4 which is the preferred specification. If the change in real SAE funding goes up by one cent per capita, this will generate a reduction in the birth rate for 15-17 year-olds by 0.04 births per 1,000 age-specific population. In other words, a one cent per capita increase in SAE funding is equivalent to an injection of $50,000 for an average state per year. This increased spending could result in an average state avoiding approximately 4 teen births per year (see Appendix D for a comparison with another study on abstinence education).[28] The results are similar across the specifications.

[28] Increasing spending by one cent per capita means that for an average state with a population of five million, total spending will increase by $0.01 x 5,000,000 = $50,000. Given the average state population of 15-17 year-olds is 100,842; the decline in the number of births for an average state equals 4.03. That is, 100,842*(0.04/1000) = 4.03.

Table 2.2: Impact of State Abstinence Education Funding on Birth rate

Variable	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Birth rates for:					
	15-17 year-olds		18-19 year-olds		25-29 year-olds	
SAE Funding	-9.340***	-1.453*	0.200	0.200	2.122	-2.881
	(2.254)	(0.773)	(3.617)	(3.617)	(3.990)	(2.929)
Rape	0.062	0.020	0.103*	0.103*	-0.052	-0.096
	(0.042)	(0.025)	(0.053)	(0.053)	(0.057)	(0.081)
Robbery	0.031***	-0.000	0.027**	0.027**	0.026***	0.004
	(0.009)	(0.004)	(0.011)	(0.011)	(0.007)	(0.008)
Poverty	-0.042	0.028	0.109	0.109	-0.528***	-0.287***
	(0.105)	(0.071)	(0.172)	(0.172)	(0.146)	(0.104)
High school graduation	-0.039	0.005	0.033	0.033	0.062	0.021
	(0.024)	(0.023)	(0.065)	(0.065)	(0.052)	(0.047)
Unemployment rate	-0.311	-0.099	-0.446	-0.446	1.840***	0.649*
	(0.235)	(0.177)	(0.455)	(0.455)	(0.617)	(0.368)
Prison (incarceration)	0.004	0.002	0.015	0.015	0.011*	0.008
	(0.004)	(0.001)	(0.009)	(0.009)	(0.005)	(0.005)
Notification Law	0.769	0.828***	0.416	0.416	1.640*	-1.088
	(0.495)	(0.220)	(0.708)	(0.708)	(0.973)	(0.759)
Abortion Medicaid	0.506	-0.492	-0.144	-0.144	-2.417*	-0.049
	(0.933)	(0.510)	(0.723)	(0.723)	(1.382)	(1.099)
Hispanic	-8.091	-143.635***	162.806*	162.806*	63.006*	-132.392*
	(15.683)	(30.175)	(91.560)	(91.560)	(33.255)	(66.684)
Black	100.548*	-99.919	559.519***	559.519***	35.743	-6.252
	(56.331)	(63.445)	(116.282)	(116.282)	(53.642)	(121.661)
Urbanization	2.076	-47.410*	-72.488	-72.488	-57.201	-100.694**
	(25.032)	(28.054)	(51.008)	(51.008)	(51.848)	(38.822)
Constant	16.878	2815.129***	-9.164	-9.164	117.905***	-983.631***
	(11.083)	(107.280)	(43.652)	(43.652)	(23.158)	(318.943)
State fixed-effects?	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects?	Yes	Yes	Yes	Yes	Yes	Yes
Linear state trends?	No	Yes	No	Yes	No	Yes
Observations	735	735	441	441	735	735
R-square	0.97	0.99	0.98	0.99	0.94	0.97

Notes: Standard errors, reported in parentheses, are robust and adjusted for clustering at the state level. Statistical levels of significance are as follows: * means p<0.1, ** means p<0.05, *** means p<0.01.

Table 2.3: Differences in Mean (change in) Birth Rates between Age Groups

	Birth rates			Change in birth rates		
	Before 1998	1998 and after	**Difference**	Before 1998	1998 and after	**Difference**
	(1)	(2)	(3)	(4)	(5)	(6)
15-17 year-olds	33.571	23.675	**-9.896*** **	-1.140	-1.129	**0.011**
	(0.670)	(0.446)	**(0.788)**	(0.134)	(0.062)	**(0.137)**
25-29 year-olds	113.287	116.288	**3.001** **	-1.170	0.935	**2.105*** **
	(0.828)	(0.873)	**(1.213)**	(0.170)	(0.141)	**(0.220)**
Difference	**-79.716**	**-92.613**	**-12.897*** **	**0.030**	**-2.064**	**-2.094*** **
	(1.066)	**(0.980)**	**(0.053)**	**(0.217)**	**(0.155)**	**(0.010)**

Notes: Standard deviation in parentheses is reported below the mean. Statistical levels of significance are as follows:
* means p<0.1, ** means p<0.05, *** means p<0.01.

Table 2.4: Impact of State Abstinence Education (SAE) on Birth Rates: Models using various Population Groups to Deflate the SAE Funding (Control Group: Birth rates for females 25-29 years)

Variable	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: ΔBirth rates					
ΔSAE Funding *Young	-4.349***	-4.916***	-0.311***	-0.348***	-0.590***	-0.656***
	(1.238)	(1.348)	(0.107)	(0.120)	(0.207)	(0.233)
ΔRape	0.036	0.043	0.035	0.042	0.035	0.042
	(0.024)	(0.026)	(0.024)	(0.025)	(0.024)	(0.025)
ΔRobbery	-0.001	-0.002	-0.001	-0.002	-0.001	-0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
ΔPoverty	-0.009	0.008	-0.009	0.008	-0.009	0.008
	(0.048)	(0.057)	(0.048)	(0.056)	(0.048)	(0.056)
ΔHigh school graduation	-0.016	-0.023	-0.018	-0.024	-0.017	-0.024
	(0.012)	(0.014)	(0.013)	(0.016)	(0.013)	(0.015)
ΔUnemployment rate	-0.175	-0.240	-0.175	-0.240	-0.175	-0.240
	(0.188)	(0.188)	(0.186)	(0.187)	(0.186)	(0.187)
ΔPrison (incarceration)	0.001	0.001	0.001	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
ΔNotification Law	0.343	0.414	0.341	0.413	0.341	0.413
	(0.276)	(0.270)	(0.274)	(0.269)	(0.274)	(0.269)
ΔAbortion Medicaid	-0.524	-0.625*	-0.539	-0.636*	-0.538	-0.635*
	(0.377)	(0.342)	(0.386)	(0.347)	(0.385)	(0.347)
ΔHispanic	6.710	25.752	5.423	24.807	5.715	25.037
	(17.891)	(23.609)	(17.829)	(23.450)	(17.788)	(23.421)
ΔBlack	-55.976	-1.314	-56.066	-1.549	-56.116	-1.640
	(40.110)	(27.055)	(40.104)	(27.105)	(40.161)	(27.136)
ΔUrbanization	-36.304**	-21.659*	-37.126**	-22.286**	-37.228**	-22.356**
	(13.771)	(10.844)	(13.939)	(10.643)	(14.187)	(10.734)
State fixed-effects?	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects?	Yes	Yes	Yes	Yes	Yes	Yes
Additional Covariates?	Yes	Yes	Yes	Yes	Yes	Yes
State*year interactions?	No	Yes	No	Yes	No	Yes
State*age interactions?	No	Yes	No	Yes	No	Yes
Age * year interactions?	No	Yes	No	Yes	No	Yes
Denominator of ΔSAE per capita	Total population	Total population	Population of 15-19	Population of 15-19	Population of 10-19	Population of 10-19
Mean of ΔSAE	0.018	0.018	0.258	0.258	0.131	0.131
Observations	1372	1372	1372	1372	1372	1372
R-square	0.21	0.29	0.21	0.29	0.21	0.29

Notes: Standard errors, reported in parentheses, are robust and adjusted for clustering at the state level. Statistical levels of significance are as follows: * means p<0.1, ** means p<0.05, *** means p<0.01. The variable SAE funding is in per capita terms and this is obtained by the real SAE dollar amount by the following denominators: *Total population* (columns 1-2), *population age 15-19 years* (columns 3-4) and *population age 10-19 years* (columns 5-6).

2.5.3. Impact of the State Abstinence Education Program on Race-Specific Birth Rates

Given the results of Bronars et al. (1994) and Abrevaya (2001) that link race with fertility, it may be informative to investigate the impact of the program on the birth rates for Blacks, Whites as well as Hispanics. Table 2.5 shows the difference-in-differences analyses for Blacks (columns 1-3), Hispanics (columns 4-6) and Whites (columns 7-9). The results in columns 1-3 show no significant effects of SAE funding on birth rates for black teens 15-17 years, although the sign remains negative. In the case of Hispanics, the coefficient of interest is positive but is not significant in any of the specifications. In comparison, the results in columns 7-9 indicate that for Whites, there are significant effects of the Title V SAE program on birth rates for teens of the same age group.

There might be underlying reasons related to this differential impact such as income, education, family circumstances and other socio-economic phenomena. The fact that white births respond to the program as opposed to Black and Hispanic births, might also be reflective of the differential high school dropout rates between races. In 2005, the high school dropout rate for Blacks and Hispanics was 0.66 and 0.87, respectively, while for Whites it was 0.23.[29] Given that Whites remain in school for longer periods than Blacks or Hispanics, they are potentially exposed to treatment more fully. Also, those who attain low education levels tend to come from poorer families with low income; and this often results in lack of resources to be aware of the various opportunities at their disposal and to utilize them to their full advantage. Without this awareness, their ability to make the optimal decisions regarding pregnancy and childbearing may be compromised. Although this type of analysis is beyond the scope of the chapter, some studies find only a modest impact of "socioeconomic disadvantage" on early childbearing (Kearney,

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[29] Source: US Department of Education, National Center for Education Statistics *Dropout Rates in the United States: 2005. Compendium Report.* http://nces.ed.gov/pubs2007/2007059.pdf (Accessed December 19, 2010).

2009). Also, it could be the case that SAE funding is more effective in reducing births for a population, namely whites, that has less teenage births.

The above results provide support to the causal impact of the Title V SAE program. That is, for an average state, increasing spending by $50,000 per year on SAE can help avoid approximately four births to teenagers. This impact is concentrated among white teens.[30] The estimates are stable across different model specifications.

2.6. Sensitivity Analysis

2.6.1. Multi-way Clustering

In this first robustness check, I cluster the standard errors on two dimensions simultaneously (state and year) using multi-way clustering as proposed by Cameron, Gelbach and Miller (2006). Due to the panel structure of the data, it is important to adequately treat error terms. In the previous estimations, standard errors are clustered by state. This captures the correlation between observations for the same state in different years. One-dimensional clustering of this type assumes there is no correlation between observations on different states in the same year (i.e., there is no time effect). If this assumption is false, then in some situations, this can lead to under-estimated standard errors and consequently biased estimates. For example, country-wide shocks will induce correlations between states at a particular time and state-specific shocks will induce correlations across time. In addition, common shocks such as business cycles that can induce correlations between different states in different years.

Table A.2 displays estimates of Equation (2.5) after clustering standard errors on both state and year. The results in column 1 are comparable to those in column 1 of Table 2.4. By

[30]This is an important policy issue. While the program seems to have an effect on reducing teen childbearing, there is no evidence of a significant impact on Blacks (or Hispanics), who are more likely to be susceptible to early teen childbearing.

race and ethnicity, the results in columns 2, 3 and 4 can be respectively compared with those found in columns 1, 4 and 7 of Table 2.5. In all instances, the magnitude and signs of the key parameter estimates are unaffected to this form of clustering while for Blacks the estimated coefficient becomes statistically significant. The fact that the results remain robust to clustering on both state and time is not surprising. This is because when "there are only a few clusters in one dimension, clustering by the more frequent cluster yields results that are almost identical to clustering by both firm[state] and time" (Petersen, 2009). In this chapter, there are 49 states and 15 years of data.

2.6.2. Including Lags of the State Abstinence Education Funding Variable

In Panel 2 of Table A.2, I allow the SAE program to influence birth rates in a year after funding is received. This is done for a couple of reasons: First, SAE may have a lagged impact on birth rates; and second, in the event that a pregnancy occurs right after the SAE, an approximate 9-month gestation period is required before birth rates can be affected. In this specification, if there is a delayed effect on childbearing caused by SAE, then the coefficient on the lagged interaction term should be negative and statistically significant. The table shows results of three specifications using the same control group (females 25-29 years) as before: column (1) analyzes the full sample looking at the impact of the program on birth rates for all females regardless of race and ethnicity while columns 2 to 4 consider the effect on birth rates for Blacks, Hispanics and Whites, respectively. In all cases, the real SAE funding variable is deflated by total population to obtain the per capita term. In this particular specification, I add one lag plus a contemporaneous term. In large part, the results are consistent with those reported earlier. The impact is statistically significant for total births and those for Whites. Meanwhile, only the contemporaneous term is significant for Blacks. In the case of Hispanics, the program has been associated with a significant increase in birth rates. It must be noted however, that Hispanics include all persons of Hispanic origin of any race.

2.6.3. Lagged Dependent Variable

I also estimate models with the inclusion of a lagged dependent variable. Such a model is a robust representation of infinite (Koyck) distributed lagged models, which may more accurately capture the dynamic effect of the SAE program. Models with lagged dependent variables provide estimates that are efficient if the model with the lagged dependent variable does not suffer from serial correlation in the residuals. I find no evidence of serial correlation at the 10 percent level of significance. Panel 3 of Table A.2 presents the results of the model with the lagged dependent variable as a regressor.[31] As in previous robust tests, I only report the main coefficient of interest. Once again, the impact of the SAE program on birth rates for minors is remarkably stable and precisely estimated for both the sample of all births and for White and Hispanic births but not for Blacks. Using the results of the lagged dependent variable model specification in column (1), the long-run differential impact of changes in SAE on changes in birth rates is -4.3.[32,33]

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[31]I instrument for the lagged dependent variable using 2SLS with the following variables as external instruments: current, lagged and forward values of the birth rates in levels. The null hypothesis that the system is exactly identified is rejected (over-identification test). Also, the null hypothesis that there is no endogeneity cannot not be rejected at the conventional levels of significance (endogeneity test).

[32]This is obtained using the Koyck Transformation which considers an infinite distributed lag model of the form: $Y_t = \beta_0 + \beta_1 X_t + \lambda\beta_1 X_{t-1} + \lambda^2\beta_1 X_{t-2} + \ldots + \varepsilon_t$ where Y is some dependent variable, X is the independent variable of interest and the residuals decay at a geometric rate such that $\beta_k = \beta_1 \lambda^{k-1}$ for k=1,2,… and $0< \lambda<1$. Multiplying the distributed lagged model by λ, lagging one period and subtracting this from the original lagged distributed model gives $Y_t = \beta_0(1-\lambda) + \beta_1 X_t + \lambda Y_{t-1} + \nu_t$, where $\nu_t = \varepsilon_t - \lambda\varepsilon_{t-1}$. With a model in first-differences, this can be written as $\Delta Y_t = \beta_1 \Delta X_t + \lambda \Delta Y_{t-1} + u_t$, where $u_t = \Delta \nu_t$. In general, these models assume $\varepsilon_t \sim IID(0,\sigma^2)$ from $u_t = \rho u_{t-1} + e_t$ and that $|\lambda|$, $|\rho|<1$. While the Koyck transformation generally suffers from serial correlation, my model in first-differences does not encounter this problem.

2.6.4. Sample Size Modification

In this robustness checks performed in Table A.3, I repeat the analysis using the preferred specification as presented in column (1) of Table 2.5, but this time using different sample periods. Specifically, I exclude the first two years (1996 and 1997) immediately preceding 1998 when the SAE program funding began (Panel 1). I also exclude the year 2002, which seems to be an anomaly in birth rate data trends during the period 1991-2005.[34] In another sensitivity analysis, I included California in the sample (Panel 2) while in another I excluded those states with the highest and lowest per capita SAE funding (Panel 3). Finally, I narrow the sample size sufficiently small enough to exploit the impact of the program during those periods around which the most significant changes in SAE funding occurred (Panel 4). That is, the major jump in SAE funding took place in 1998 in all states. Therefore, this sensitivity analysis estimates the model using data around the narrow window from 1995 to 2000.

As can be seen in the results of Table A.3 (Panel 1), despite the sample period changes, the coefficient estimate is significant and the sign remains negative as before. This is true for all races/ethnicity combined (column 1), as well as for Whites separately (column 4). In Panel 2 of Table A.3, I include the state of California, which has never accepted Title V SAE funding. As the results show clearly, whether California is taken into account, the impact on birth rates remains largely unchanged. The results in Panel 3 exclude the states with the largest and

[33]Using the specification from Table A.2, Panel 3 (column1), the differential long-run impact (i.e. $\beta_1 \frac{1}{1-\lambda}$) of the change in the SAE funding on the change in birth rates of minors can be calculated using the following results: λ= -0.011 and β_1 = -4.315. Therefore, $\beta_1 \frac{1}{1-\lambda} = -4.315 * \frac{1}{1-(-0.011)} \approx -4.3$, where β_1, which is the same as β_3 in the model, is the coefficient on the interaction term ΔSAE*Young. Note, the long-run impact is the same as the short-run or instantaneous effect as shown in column 1 of Table 4.

[34]In 2002, birth rates show a remarkable dip in comparison to previous years; and this episode is more striking for some state-specific age groups relative to others. I conjecture that this phenomenon may be related to some major event occurring in a previous period (for example: the recession of 2001 and/or the impact of 9/11 terrorist activities).

smallest per capita SAE funding over the period of this study. These states are Mississippi and New Hampshire, respectively. The results are also robust to this specification. Finally, the last panel (Panel 4) of the table shows the point estimates for the reduced sample comprising the years 1995 to 2000. Although the estimates are smaller in magnitude than those reported in the previous sample modifications, in general the inferences are qualitatively similar.

2.6.5. Changes in the Control Group

I consider the possibility that different control groups may result in a different impact of the SAE program on birth rates for teens 15-17 years relative to an older group of females. Two other comparison groups are considered and the results are shown in Table A.4. In columns 1-4, the control group is females 18-19 years old over the restricted period 1991 to 1999. The restricted time-period is used because females who are 18-19 years in 2000 and beyond will have been subjected to SAE in 1998. Hence, they will not really be a control group after 1999. In columns 5-8, I use an older control group, namely birth rates for females 30-34 years over the period 1991-2005. While the SAE program should not affect the birth rates for this older control group, it is useful to check for any possible spurious impact.[35]

Beginning with the younger control group of 18-19 year-olds in columns 1-4, the SAE funding has resulted in a decline in birth rates for 15-17 year olds in both the full sample and the specification for white females only. As in most of the previous analysis, the impact of the program on the birth rates for Black or Hispanic females is not statistically significant. The results I obtain using this younger control group are consistent with the findings obtained earlier in the base analysis using the control group of 25-29 year-olds. One criterion that is used in

[35]I also consider females 20-24 years old as a control group. These results are not reported.

selecting a comparison group is for both treatment and control groups to be similar. Indeed, the control group of 18-19 year-olds is very similar to minors. In columns 5-8 where the control group is 30-34 year-olds, I obtain relatively weaker results of the impact of SAE funding on birth rates.

2.6.6. Multiple Control Groups

I re-estimate the baseline model shown in Equation (2.5), using multiple control groups instead of a single comparison group. In these analyses, the use of multiple comparison groups can help to strengthen the robustness of the previous results of the investigation.[36] In this robust check, I use two comparison groups together: females 18-19 and 25-29 years old. The treatment group remains those female minors 15-17 years old. To avoid the possibility that the treatment group of 15-17 year-olds can become part of the control group, the estimation period is 1991-1999. The results are shown in columns 1-4 of Table A.5. Indeed, the impact of the SAE funding on the birth rates for minors consistently produces a negative coefficient estimate, which is statistically significant for two of the three specifications. To utilize the full sample period, another set of multiple controls is required; and so I replace females 18-19 years with those 30-34 years to form another multiple control group of females 25-34 years old. Columns 5-8 of Table A.5 provide the results, which are very similar to those of columns 1-4. The sign of the coefficient is as expected, as is the significance of the estimate. In this case, the SAE program has a significant impact on the birth rates for minors across both races included in the specification.

[36]According to Meyer (1995, p. 157), "the more comparison groups the better."

2.6.7. Other Robustness Checks

I consider that each of the controls used in the model may have a different impact on birth rates for 15-17 year-olds and the older control group. For example, unemployment may reduce (or increase) births for the control group but may have a different effect on minors. To control for this, I include the interaction between each control variable and the age group (unemployment * young, robbery * young and so on). The results, which are largely unchanged with the additional covariates and different control groups, are shown in Table A.6.

I perform additional robustness checks to demonstrate the reliability of the results. First, I consider the possibility that certain states may be responsible for driving some of the observed impact. For instance, states with high pre-adoption birth rates may apply for more funding and so the aggregate state-level effects could be driven by a few states. In separate regressions, I address this possibility by a) dropping the top five states in terms of birth rates; b) removing the first five states with the lowest birth rates; and c) combining both (a) and (b).[37] Second, I also weighted the regressions by relevant populations such as total population, population of teenagers and so on. The results from these two approaches were comparable and qualitatively similar to the main findings. Using different comparison groups did not affect the results as before. These results are not shown, but are available upon request.

[37]The states with the highest teen (15-17 year olds) birth rates and rankings in 2005 were Texas (1), New Mexico (2), Mississippi (3), Arkansas (4) and Arizona (5). The five states with the lowest teen birth rates in 2005 were New Hampshire (50), Vermont (49), Massachusetts (48), Connecticut (47) and New Jersey (46).

Table 2.5: Impact of State Abstinence Education (SAE) on Race-Specific Birth Rates: Models using various Population Groups to Deflate the SAE Funding (Control Group: Birth rates for females 25-29 years)

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent Variable: ΔBirth rates for Blacks			Dependent Variable: ΔBirth rates for Hispanics			Dependent Variable: ΔBirth rates for Whites		
ΔSAE Funding *Young	-9.719	-0.618	-1.233	6.033	0.465	0.904	-4.351***	-0.317***	-0.592***
	(5.854)	(0.412)	(0.805)	(4.784)	(0.333)	(0.652)	(1.221)	(0.103)	(0.202)
Observations	1372	1372	1372	1370	1370	1370	1372	1372	1372
R-square	0.04	0.04	0.04	0.21	0.21	0.21	0.13	0.13	0.13
State fixed-effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Covariates?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Denominator of ΔSAE per capita	Total population	Population of 15-19	Population of 10-19	Total population	Population of 15-19	Population of 10-19	Total population	Population of 15-19	Population of 10-19
Mean of ΔSAE	0.018	0.258	0.131	0.018	0.258	0.131	0.018	0.258	0.131

Notes: Standard errors, reported in parentheses, are robust and adjusted for clustering at the state level. Statistical levels of significance are as follows: * means p<0.1, ** means p<0.05, *** means p<0.01. Data for Hispanics include all persons of Hispanic origin of any race.

2.7. Discussion

2.7.1. Caveats

The findings above suggest a causal effect of joint federal government and state-based abstinence-centered education on teen birth rates. That is, the SAE program appears to have resulted in some gains in reducing underage births to mothers.

However, a number of issues remain. The first relates to the choice of using births rather than pregnancies, especially since a preponderance of the research focuses on the latter. Although pregnancies are important to analyze, reliable annual state and age-specific measures are not readily available for an extended period of time. Furthermore, fertility is an important outcome to analyze because of its implications for long-term well-being of young mothers and their off springs.

A second criticism is that more-refined estimates can be obtained if the analysis is done at a less aggregated geographical level such as counties and cities. While age-specific birth rates can be disaggregated to lower levels of analysis, county or city-level SAE funding data are not readily available. Any attempt to estimate the effect of state-level SAE data on county-level fertility rates will result in one of the more serious forms of measurement error leading to biased coefficient estimates. [38]

Third, the model might be missing important variables such as measures of family planning (expenditure or clinics) and abortion notification laws. With respect to the latter, it can be expected or assumed that states that accept SAE grants are also more likely to introduce abortion notification legislation. Thus, the SAE variable might be picking up some of the

[38] In such a scenario, the measurement error will be on the right-hand side, specifically with the SAE funding variable. Greene (2003, section 5.61) emphasizes that such errors are cause for concern especially within a multiple regression framework. Measurement errors of this type not only bias the coefficient of interest, but also the other coefficients whose direction of bias is unknown.

notification effect. While this may be the case, initial evidence suggests that the "new parental consent or notification laws probably had at most a very small effect" (Henshaw, 1997, p. 121). I include a notification law dummy variable, as well as an indicator of whether or not states provide Medicaid assistance for abortion. The effects of the coefficients appear rather small. Also, it is not clear that acceptance of SAE grants is highly correlated with abortion notification since, as was mentioned earlier, all states excluding California have taken part in the Title V SAE funding program from its inception. With respect to not accounting for family planning expenditures, measures of family planning tend to be associated with funding for "comprehensive" sex education (CRE). Despite the absence of a proxy CRE variable, this effect is likely to be adequately controlled for by the presence of state fixed-effects and year fixed-effects.[39]

There might be other factors influencing the impact of the SAE program. For example, variation in SAE funding across states is due in part to child poverty. This is important because it raises the possibility that child poverty, which influences the level of SAE funding, may also be driving fertility patterns in these states and thus making SAE funding endogenous. However, in this study, cross-state variation is not a confounding factor since I am really comparing the effect of the SAE program within each state and not across states. Although child poverty is in some way associated with SAE funding it must be pointed out that SAE funding is not based solely on poverty within each state, but on the ratio of poor children in a state relative to the total

[39] The tendency is for every state to access similar and constant funding levels for CRE. Thus, there is likely to be less variation in CREs across states.

number of low-income children in all states. Based on this formula the ratio for each state is more likely to move very gradually overtime.[40,41]

2.7.2. Estimated Cost-Benefit Analysis

What is the potential impact of abstinence education spending on births avoided? One way to evaluate the effectiveness of the SAE program is to compare the spending costs for each avoided birth with the associated savings that will arise as result. According to Hoffman (2006), average public sector spending on a child born to a female 17 years and younger was $4,080 per annum (note this figure is higher than the annual cost of $1,430 for all teenagers mentioned earlier). Given that the average length of time in which a teenage mother receives public assistance is 6.9 years (Hoffman, 2006), this implies that the estimated public savings for each avoided birth is $4,080 x 6.9 = $28,152 (or $112,608 for every four births avoided).

I can then use the results provided in this chapter to provide a rough estimate of the annual cost associated with each birth avoided due to abstinence education. Recall, the analysis showed that on average a state can avoid four births by increasing abstinence education spending by $50,000. Comparing these two estimates, it implies a state that spends $50,000 on abstinence education will avoid four births and as a result saves approximately $112,608. This is a net

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[40] In fact, in the U.S. the poverty rate for children under the age of 18 had moved from 21.8 percent in 1991 to only 17.6 percent in 2005. This represents a decline in child poverty rates over the period. However, variations in the amount of SAE funding accessed by states were related to administrative changes in the operation of the SAE program and not due to any state demographics. See Children's Defense Fund website http://www.childrensdefense.org/child-research-data-publications/data/state-data-repository/census/census-2007-child-poverty-data.pdf (Accessed January 21, 2010).

[41] In spite of this, endogeneity of SAE funding cannot be completely discounted. To provide strong support to the identification strategy using difference-in-differences models, I undertake a number of robustness checks in the section on "Sensitivity Analysis". I also consider the use of instrumental variables to identify the effect of abstinence funding using the citizen and government ideology scores by Berry et al. (1998) as instruments. These variables are not expected to be correlated with birth outcomes but correlated with SAE funding. The results (not shown) of the instrumental variable strategy were not significant as the first stage results were rather weak.

savings of $112,608 - $50,000 = $62,808 for every four avoided births or $15,652 for each birth avoided.

2.8. Conclusion

In an era of heightened awareness of the high teen birth rates in the U.S., an intense debate has centered on the effectiveness of various policy approaches to reducing early childbearing. One view among researchers is that policies promoting abstinence and improved contraceptive practices can be influential in curtailing teen births.

I present evidence supporting the influence of a federal government-sponsored state abstinence education (SAE) program. Specifically, I estimate the impact of SAE funding from the federal government Title V, Section 510 appropriations on the birth rates for teens 15-17 years in the U.S. over the period 1991-2005. The results suggest that Title V SAE has led to a decline in birth rates for the targeted group of female minors. Using difference-in-differences methodology, I find for an average state, increasing spending by $50,000 per year on SAE can help avoid approximately four births to teenagers, resulting in net savings of $15,652 to the public for each birth avoided. Given that each teenage birth is associated with an annual social cost of $1,430, this particular education program seems to be cost-effective at the current margin. The overall findings appear to be robust to a wide range of specifications.

I further find that the SAE program has a consistently significant impact among Whites but not Blacks. This apparent heterogeneous influence of SAE on the birth outcomes of Blacks and Whites opens an avenue for future research. For example, it may be that Blacks (and Hispanics) - because of their lower socio-economic outcomes and other circumstances which influence years of schooling - have less exposure to this form of education than White teens. To investigate this hypothesis, one might have to rely on more disaggregated information at the individual level.[42]

[42] For the purposes of this study, the paper can benefit from two important extensions. First, is an analysis that includes data on abortions for a more informed discussion on the impact on pregnancies. A number of studies such as Levine (2000, 2003) have investigated this issue. Second, with the relevant data available, an investigation that looks at the specific counties and cities that have received appropriations of Title V SAE funding will provide greater insight and precision in determining the impact of the program.

CHAPTER 3. DOES THE FAMILY AND MEDICAL LEAVE ACT (FMLA) INCREASE FERTILITY BEHAVIOR?

3.1. Introduction

Since the 1990s, family-friendly policies such as maternity leave have increased throughout the United States. Universal maternity leave has become an important topic of discussion regarding its contribution to various aspects of work and family life, in particular, the effect on decisions related to childbearing.

For most countries, especially those in Europe such as Austria and Sweden, pronatalist policies - those designed to encourage fertility behavior - are quite common where parental leave is sufficiently generous allowing mothers to have two or possibly three separate gestations within the same leave period. In addition, these countries have provisions ensuring that employees are compensated while on leave. This is in sharp contrast to the United States (U.S.) where leave policies only became universal through the passage of the Family and Medical Leave Act (FMLA) in 1993.[1] Compared to other countries, the FMLA provides a shorter leave period amounting to 12 weeks unpaid leave. Most European and OECD countries offer an average of 36 weeks.[2] Further, new mothers in Europe get on average 14 to 16 weeks of **paid** maternity leave (Kamerman and Gatenio, 2002). Prior to the FMLA, only 12 states in the U.S. (plus the District of Columbia) possessed laws requiring job-protected maternity leave but with wide variation (from 4 to 18 weeks) in the length of time allowed.[3] An advantage of the FMLA over

[1] Although it is a federal policy, knowledge of the FMLA is still not as widespread since only 58.2 percent of employees at covered worksites have any knowledge of FMLA (Department of Labor, 2007).

[2] The United Nations watchdog on labor issues, the International Labor Organization (ILO) prescribes a minimum of 14 weeks maternity leave along with some cash benefit (International Labor Organization, 2000).

[3] The states are California, Connecticut, District of Columbia, Maine, Massachusetts, Minnesota, New Jersey, Oregon, Rhode Island, Tennessee, Washington, Wisconsin and Vermont. Information obtained from Waldfogel (1999).

the state-specific leave laws is that it is universal and so better able to influence childbearing preferences at the national level.

Outside of the U.S., incentive-driven policies have been shown to have an impact on fertility rates. Generous pronatalist policies such as the 1998 Quebec government "Baby Bonus" program in Canada have generated an increase in family size. In this program, mothers were rewarded as much as $500, $3,000 and $8,000 (Canadian dollars) for having a first, second and third birth, respectively (Milligan, 2002).[4] Other countries such as Austria, France, Germany, Italy, Sweden, Finland and the United Kingdom at various times have instituted welfare and social policies associated with cash incentives in an attempt to encourage fertility rates with some degree of success (Gauthier, 2007). In line with this research are those studies that examine the impact on family fertility of work-related policies such as maternity leave. Nearly all of the studies use micro-level data, producing mixed results: some showing that work-related incentives have a positive impact in raising fertility (Hyatt and Milne, 1991; Buttner and Lutz, 1990; Hoem, 1993; and Lalive and Zweimüller, 2009) while others have found no evidence of a policy impact (Gauthier and Hatzius, 1997 and Hoem *et al*., 2001).

At the other end of the spectrum are anti-natalist policies, those designed to restrict a certain type of fertility. These policies tend to be implemented in places where fertility rates are far above the replacement level and population growth is considered to be excessive. For instance, China's one-child family policy which began in the late 1970s is designed with the objective of curbing the rapid population growth as a means to aid development. Although both

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[4] During this period, the official exchange rate was US $1 to CAN $1.5. In terms of United States currency, these amounts approximated to US$333, US$2,000 and US$5,333, respectively. This is based on the exchange rate of US $1 to CAN $1.5 that existed at the time. See World Development Indicators at http://databank.worldbank.org/ddp/home.do?Step=12&id=4&CNO=2 (accessed October 9, 2010).

pronatalist and anti-natalist policies have an underlying economic objective, the latter has attracted numerous criticisms for being gender discriminatory among other things.

Despite the evidence of some positive impact of family leave polices on family formation, within the U.S., the fertility effect of maternity leave under the auspices of the FMLA has been ignored within the empirical literature.[5] This neglect is perplexing. A parallel debate regarding the merits of the FMLA is ongoing. For conservative opponents, the FMLA is merely a symbolic gesture catering to certain interest groups. They point to the lack of any firm evidence to suggest that such legislation can make a positive and significant impact on fertility rates. They also argue that women who otherwise (in the absence of the FMLA) will have quit their jobs to spend more time taking care of the newborn are now spending less time with their young infants because of this legislation. Proponents of the FMLA argue that this policy allows women to spend more time at home with the young without adversely affecting employment in general (Klerman and Leibowitz, 1998). Supporters of the FMLA further argue that not only is the FMLA a good thing but it must be improved since the U.S. lags behind most developed countries that have introduced and implemented maternity leave legislation on a more generous scale.[6]

In this chapter, I estimate the impact of the Family and Medical Leave Act (FMLA) on fertility outcomes. Specifically, I examine whether eligible women under the FMLA criteria are more likely to have a first and second birth compared to other women. I find evidence that implementation of the FMLA is associated with an increase in fertility outcomes among eligible

[5] Averett and Whittington (2001) investigate the impact of maternity leave on influencing births during the period 1985 to 1992, which was a period before the FMLA was introduced.

[6] The U.S. and Australia are perhaps the only two industrialized nations with unpaid maternity leave legislation, but Australia is set to implement its first compulsory paid parental leave scheme in January 2011 when eligible parents will be paid at minimum wage for 18 weeks while on leave (See the Australian Government Department of Families, Housing, Community Services and Indigenous Affairs website at http://www.fahcsia.gov.au/sa/families/progserv/paid_parental/Pages/default.aspx. Information accessed May 9, 2010)

women. Using panel data from the 1979 cohort of the National Longitudinal Survey of Youth (NLSY79) for the period 1989 to 2006, I find that the introduction of the FMLA has significantly increased the probability of eligible women having a first and second child. The results are robust to a variety of specifications. I also find that the impact of the legislation is evident amongst eligible whites and those with college-level experiences. These results are corroborated with the findings that eligible women are giving birth faster as a result of the policy.

I organize the rest of the chapter as follows: Section 3.2 provides background information on the FMLA. Section 3.3 provides a theoretical perspective to help motivate the study. Section 3.4 outlines the previous empirical literature. Section 3.5 presents the empirical strategy while Section 3.6 presents the data. The results are presented in Section 3.7. Section 3.8 discusses and concludes.

3.2. Background

3.2.1. The Family and Medical Leave Act (FMLA) of 1993

On February 5, 1993, President Bill Clinton signed his first piece of legislation by enacting the Family and Medical Leave Act (FMLA) of 1993, a federal law guaranteeing job-protected maternity leave along with other provisions. Prior to the FMLA, job-protected maternity leave – the right to take time off from work to give birth, care for the newborn and resume employment at the pre-birth job – was largely possible through the goodwill of employers and/or through state mandates.[7] In large part, employer and state policies towards

[7] Less than 25 percent of the contiguous states of the U.S. had any state legislation establishing job-protected maternity leave. There were 12 states plus the District of Columbia with similar maternity leave legislation prior to the enactment of the FMLA. These are: California, Connecticut, District of Columbia, Maine, Massachusetts, Minnesota, New Jersey, Oregon, Rhode Island, Tennessee, Washington, Wisconsin and Vermont (Waldfogel, 1999).

maternity leave were aimed at providing greater flexibility for women in the workplace and to strike some balance between work and parental duties.

Implemented on August 5, 1993, the FMLA provided benefits only to eligible employees. Under the FMLA, an eligible employee is one who:

(1) has been employed by a covered employer;[8]

(2) has worked for the employer for at least 12 months;

(3) has worked at least 1,250 hours over the previous 12 months; and

(4) is employed at a worksite in the United States or in any territory or possession of the United States where 50 or more employees are employed by the employer within 75 miles of that worksite.

Eligible employees are entitled to take up to 12 weeks of unpaid, job-protected leave each year for any of the following reasons: (a) to give birth and care for the newborn; (b) to adopt a child or to carry out foster care; and (c) to care for self, a spouse, a child or parent with a serious health condition.[9] The FMLA applies to all public agencies including those of the state, local and federal governments as well as schools. Employees in the private sector are also covered.

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[8] According to the U.S. Department of Labor, a covered employer is "any person engaged in commerce or in any industry or activity affecting commerce, who employs 50 or more employees for each working day during each of 20 or more calendar workweeks in the current or preceding year. Employers covered by FMLA also include any person acting, directly or indirectly, in the interest of a covered employer to any of the employees of the employer, any successor in interest of a covered employer, and any public agency. Public agencies are covered employers without regard to the number of employees employed. Public as well as private elementary and secondary schools are also covered employers without regard to the number of employees employed." Source: http://www.dol.gov/dol/allcfr/ESA/Title_29/Part_825/29CFR825.104.htm (Accessed August 11, 2010).

[9] Under the FMLA, if two spouses are employed by the same employer, then the combined leave allowed is 12 weeks.

3.2.2. Implications of the Family and Medical Leave Act (FMLA)

While the FMLA represents a marked improvement in legislative leave policies in the U.S., in many instances the resulting benefits are considered small in comparison to those of other industrialized nations. There are some notable differences in the FMLA and leave policies in other developed countries. The FMLA is broader in scope compared to its industrialized counterparts whose leave laws focus mainly on parental leave. Almost all industrialized countries provide some form of paid leave to employees while the U.S. does not. Also, eligibility for leave does not depend on company size in other industrialized countries. As a result, the FMLA only affects a relatively small proportion of companies.[10]

Although the FMLA has led to the standardization of maternity leave policies throughout the country, critics of the FMLA point to its symbolic nature since much of the present benefits were already instituted in workplaces in some states prior to 1993. Further, some argue that the FMLA is too restrictive since it is limited to companies with at least 50 employees and therefore a significant proportion of the labor force cannot benefit from this policy.

However, the FMLA as a policy may offer some insights into family-friendly policies in the U.S. There are a number of potential implications arising from such a policy. First, the FMLA can help to address the balance between women's work and family life. Second, it can help to address the length of delay in childbearing among career oriented women. Finally, there are also implications for the retention of women in the labor force given that educated women may opt out of paid employment after having children.

[10] Despite a small number of companies, the FMLA still manages to cover around half of the working population. The Department of Labor (2007) using data for 2005, reports that 76.1 million of the 141.7 million workers in the U.S. were eligible for FMLA benefits.

3.3. Theoretical Perspective

Two main approaches have been used to explain the relationship between economic factors and fertility: changes in relative income and the opportunity cost of time (Becker, 1960; Easterlin, 1975).

In Becker's (1960) model, children are treated as consumption goods, in that they provide utility to parents but they also represent a cost. An increase in income can have two effects: one that causes the consumption of the good to increase (income effect) and the other occurs where the relative increase in income causes the opportunity cost of consuming the good to increase thereby reducing consumption (price effect). The model appeals to a quantity-quality substitution process that determines the demand for children. For instance, rising costs of education will increase the fixed costs of raising children so that parents will find it relatively more costly to have an additional child (child quantity) than say to provide more schooling (child quality). In this case, an increase in income will cause a substitution away from quantity in favor of quality.

A negative relationship between income and fertility may occur if "time spent on consumption" is incorporated within the household production function. Not only is there a cost to obtaining the good (having a child), but there is also a cost – opportunity cost – in terms of parental time in consuming that good (for example, time away from work to raise kids). Because children are considered to be relatively more "time intensive" than other goods, the opportunity cost of raising children is proportionately higher than that of consuming other goods. Consequently, as incomes increase, the relative costs of raising children in terms of other goods increase, and so the demand for children (fertility) declines.

An alternative model of fertility subscribes to the notion that tastes or preference levels evolve over succeeding generations and they matter in determining fertility. Easterlin (1975) describes the concept of relative income as earnings relative to material aspirations. As an individual's standard of living rises, so does his/her material aspirations. In order to keep relative income constant, individuals will make adjustments to increase earnings such as working more. An increase in female labor force participation will likely lead to a postponement of child-bearing, while some women will also choose to have fewer children over their lifetime.

These income-fertility nexuses described in the theoretical literature can be observed in the data where women in higher income countries are showing a tendency to defer having children. This process of child postponement gets more acute because the more time women spend investing in human capital and integrating into labor market activities, their chances of conception progressively decreases due to physiological as well as other factors such as miscarriages which increase with age. The FMLA represents one policy option in addressing the falling or low fertility rates. By allowing women the right to return to a former job after maternity leave, the FMLA reduces some of the costs associated with children without necessarily affecting the labor market outcomes of women. Under the FMLA, women can avoid costs such as the following: costs related to a loss of employment or being forced to exit the labor force because of employment restrictions related to giving birth and returning to work, and costs of having to find a new job. By lowering opportunity costs of rearing children, the FMLA effectively increases the demand for children. Whether the FMLA increases the fertility behavior of women is an empirical question.

3.4. Previous Literature

3.4.1. Related Literature

Over the past decade, empirical findings have improved our knowledge about the effects of maternity leave as well as other family policies. I review the previous literature on family leave policies and legislation from several perspectives: (i) studies investigating the influence of parental leave on job continuity, labor supply and employment outcomes, (ii) studies that look at the impact of leave policies on leave-taking, (iii) research that examines the role of family leave legislation on child health outcomes and (iv) studies on the relationship between family leave initiatives and fertility outcomes. Greater attention is paid to the latter since this is of greater relevance to my analysis of the impact of a specific policy, the FMLA.

There is a growing literature providing evidence on the impact of maternity leave policies on the labor market outcomes of women, more so in the U.S. but also in other parts of the world such as Europe. An important aspect of maternity leave policies is the improvement in job continuity among new mothers post-birth. If maternity leave statutes improve job-continuity for new mothers (Baker and Milligan, 2008a), then on-the-job training for this group of women will increase and help to reduce some of the wage gaps that exist between mothers and non-mothers (Waldfogel, 1998; Klerman and Leibowitz, 1999). In the absence of maternity leave initiatives a woman, once she becomes pregnant and gives birth, may lack the incentive to immediately return to her old job or there is an increased likelihood that she will quit her current job to care for the newborn. With respect to the impact of the FMLA on job-continuity, Baum (2003) finds a relatively small effect of this federal policy, primarily because prior leave policies were already in place in some firms.

Some studies such as Baker and Milligan (2008b) have concurred that parental leave policies are associated with increases in female labor supply. On the other hand, the results of others have been less conclusive in the U.S. than in other countries with maternity leave policies offering more generous provisions. Waldfogel's (1999) is one of the few U.S. studies with definitive results finding that a higher proportion of women with very young children (less than one year) have been employed since the FMLA. Although maternity leave policies in other countries provide more generous benefits than in the U.S., only a few studies have done a direct examination of the labor supply effect of these policies (see Winegarden and Bracy, 1995; Ruhm, 1998; Baker and Milligan, 2008a). Studies such as Waldfogel (1999), investigating the impact on wages find that the impact of leave policies on wages is even less clear.

On the aspect of the role of parental leave on leave-taking, there has been limited research. The more recent research in this area (example: Waldfogel, 1999; Klerman and Leibowitz, 1998) finds an increase in the amount of leave taken as a result of these leave policies.

There is a strand of literature that assesses the role of parental leave policies on the health outcomes of new mothers and their children. Several cross-country analyses have found that maternity leave policies are associated with a significant improvement in child health outcomes (Winegarden and Bracy, 1995; Ruhm, 2000; Tanaka, 2005). Using the U.S. data, Berger *et al.* (2005) find that women taking less than the maximum allowable leave period under the FMLA have children with worse health outcomes, attributable in part to the lower levels of breastfeeding and immunizations.

There are only a few studies that investigate the consequences of parental leave laws on fertility outcomes. Hyatt and Milne (1991), using time-series Canadian data from 1948-1986, examine the potential impact of some government programs on fertility behavior. They find that under the Unemployment Insurance (UI) maternity benefits program, a one percent increase in the real value of this benefit was associated with an increase in fertility rate in the range 0.09 to 0.26 percent. Buttner and Lutz (1990) using data for Democratic Republic of Germany (East Germany) for the period 1964 through 1987, investigated the impact of a 1976 pronatalist social policy on age-specific fertility rates. The introduction of this policy resulted in substantial increases in parental leave from 18 weeks to 26 weeks (or 52 weeks in the case of mothers with at least two children). The authors find a strong, positive, causal effect on fertility rates up to five years after the policy. This impact appears to be greater for women with second and higher-order births.

Gauthier and Hatzius (1997) present data from 22 OECD countries over the period 1970 to 1990 showing that neither of the two maternity leave policy variables – duration of maternity leave nor maternity pay – has a statistically significant impact on fertility rates. To the authors' surprise, they find no positive effect of maternity pay on fertility rates. In a multivariate difference-in-differences analysis, they attribute this to the lack of variation in maternity payment over the study period. Hoem (1993) documents the role of a Swedish policy reform in influencing the speed of childbearing amongst women. Initially, the law required parents to retain their parental leave benefits up to two years following the birth of a child, but a policy change in 1986 led to a six-month extension of these benefits. With the possibility that women can have two or possibly three children consecutively before returning to work, the author finds that the incentives under this policy increased women's preferences for children relative to a career in the workplace.

To date and to my knowledge, only one published multivariate study has analyzed the impact of maternity leave on fertility outcomes in the U.S. None has examined the impact of the

FMLA. The study, which was done by Averett and Whittington (2001) examines whether employer-provided maternity leave affects fertility decisions of women. Specifically, they investigated the impact of maternity leave on the probability of observing a birth using a discrete-time hazard model. They find an increase in the likelihood of a woman giving birth as a result of maternity leave. This result is even stronger if the woman has had a child in the past.

There are other studies investigating the impact of government-mandated parental benefits on fertility using data from other countries. Hoem *et al*. (2001) find that, by extending the parental leave in 1990 in Austria, the change in policy slowed the declining rate of third births. More recently, Lalive and Zweimüller (2009) study the effects of changes in parental leave provisions on fertility as well as return-to-work behavior from a 1990 Austrian policy reform. The new policy allows women to obtain longer paid maternity leave from the child's first birthday to the second. Using treated and control groups in a regression discontinuity framework, the authors find that mothers are 15 percent more likely to get an additional child, an episode which coincides with more time off from work.

A related literature also examines the implications of financial incentives for fertility outcomes. Incentives in the form of subsidies from these pronatalist polices tend to have large positive impacts on fertility (Milligan, 2005). In general, the impacts of welfare reform programs aimed at either encouraging or prohibiting fertility behavior in the U.S. have been mixed. Some of the factors influencing the results relate to the sensitivity of the findings to model specification as well as difficulty in measuring the size of the impact which may be very small relative to the effects of other factors (Moffit, 1997; Blank, 2002).

3.4.2. Previous Research on the Family and Medical Leave Act (FMLA)

Previous empirical research provides evidence that the introduction of the FMLA has led to more coverage for eligible recipients who have increased the amount of leave taken (Ross, 1998; Waldfogel, 1999; Han and Waldfogel 2003; Han *et al.*, 2007). At the same time, it appears that the FMLA has not caused any significant changes in employment or wages. Berger and Waldfogel (2004) showed that FMLA-eligible women were more likely to access parental leave and then return to work after giving birth - a finding that may be attributable to the FMLA.

Numerous studies have explored the role of the FMLA on various outcomes of interests such as wages, employment and child health outcomes. To my knowledge, none of the studies have examined the fertility impact of the FMLA.[11] If the introduction of the FMLA has created an incentive for more women to access leave (including maternity leave) then the FMLA may have impacted fertility behavior.

This study expands on previous literature by investigating the causal effects of a universal policy (federal government legislation) on fertility behavior of women since the introduction of the FMLA. Using the 1979 cohort of the National Longitudinal Survey of Youth (NLSY79) data, I identify the fertility behavior of women who, according to the criteria as set out in the FMLA, are eligible for maternity leave. This research contributes to the existing literature in several distinct ways.

This is the first analysis to study the fertility effects of a federal government-mandated parental leave policy in the U.S. The study that comes closest is that of Averett and Whittington (2001) who investigate the fertility effects of employer-based maternity leave in the U.S., but this was prior to the implementation of the FMLA. One limitation of their study is that only a

[11] Averett and Whittington (2001) have looked at the impact of maternity leave on fertility, but their investigation was conducted prior to the introduction of the FMLA.

few states had laws providing job-protected maternity leave prior to the FMLA. Therefore, the present study makes it possible to undertake a more general investigation of the universal impact of a federal law. Consequently, the evidence that this chapter provides on the impact of maternity leave-related policies on fertility outcomes may have broader policy implications. Second, I am able to exploit the variation in exposure to FMLA benefits. There is also variation in covered employers within states because not all firms are covered under FMLA. Further, two individuals in the same covered firm may differ in eligibility status if one of them does not meet the criteria explained in Section 3.2. This allows for the use of a difference-in-differences (DID) strategy.

Third, I use a dataset which allows for the tracking of the behavior of a group of individuals over time and whose age group makes them most likely to be the first to be affected by this policy. Waldfogel (1999) recognizes the NLSY data as being a potentially good source of information in investigating the FMLA's impact on leave coverage.[12] The data sample, which spans the period 1989 to 2006, provides important information over a sufficiently long time frame allowing for an assessment of the FMLA. Ruhm (1997) expresses a similar sentiment by informing that over time, usage of the FMLA will likely increase.

3.5. Empirical Strategy

Since identification comes from an exogenous policy rule (eligibility criteria) based on length of current employment and company size, it is necessary to identify the policy impact by comparing the likelihood of a birth outcome between individuals eligible for FMLA benefits and

[12] At the time of her research, the span of the data was too short to carry out any meaningful investigation.

those who are ineligible.[13] To do this, I employ a difference-in-differences estimator exploiting the variation in exposure to FMLA benefits by identifying changes in fertility behavior between two groups. One group receives treatment and another group does not. The natural control group will comprise women who were not eligible for FMLA, while the treatment group comprises women who satisfied the criteria for FMLA benefits and so became eligible.

To evaluate the impact of the policy, I compare the changes in risk or probability of giving birth in the treatment and control group using the following discrete-time hazard model:

$$\text{logit}\,(h_{it}) = \alpha(t) + \beta_1 \text{FMLA}_t + \beta_2 \text{Eligible}_{it} + \beta_3\,(\text{FMLA}_t * \text{Eligible}_{it}) + \beta_4 \mathbf{X}_{it} \qquad (3.1)$$

where h_{it} is the hazard rate for individual i during interval t. The hazard rate is the probability that an event occurs at a certain time to a particular individual given that the individual is at risk to the event at that time. If the event is a first birth, then the hazard rate is the probability of a woman giving a first birth within the sample period among those women who are yet to have a first birth.

The right-hand side of the equation consists of an indicator variable, FMLA_t which equals one for the years 1993 to 2006 and zero otherwise, Eligible_{it} is a dichotomous variable which equals one for a woman that satisfies all the eligibility criteria for each year as defined in Section 3.2 and $\text{FMLA}_t * \text{Eligible}_{it}$ is the interaction term. The vector **X** contains a set of covariates which may be fixed or time-varying and controls for age, race and ethnicity, marital status, education, family income, whether the woman has an urban residence, job tenure and the number of children the woman has had previously. The βs represent the corresponding parameters to be estimated. Standard errors are clustered at the person level.

[13] Ineligible women are those who do not satisfy all the eligibility criteria and who have worked some positive number of hours in each of the years during the sample period 1989-2006. Therefore, women who never worked in any year during the sample period are excluded.

The term $\alpha(t)$ is a function of *t* and completes the baseline logit-hazard model as shown in Equation (3.1). In the case of discrete-time hazard models, the form of this function is usually determined by the researcher after inspection of data plots of the hazard function. A hazard plot with an approximately linear pattern might have a function of the form $\alpha(t)= \alpha_1+\alpha_2 t$ which comprises a constant and a time trend. This functional form may also include a quadratic term, t^2. In many cases, a plot of the hazard function may not be linear and so the function $\alpha(t)$ will have to take a more flexible form. The most flexible form of the function is when *t* is treated as a categorical variable for every time interval, except one (Allison, 1984). In this case, the function will take the form $\alpha(t) = \alpha_1 D_1 + \alpha_2 D_2 + \alpha_3 D_3 + \ldots + \alpha_k D_k$ or alternatively, this function can include an intercept term and *k-1* time dummy variables.

Interpretation of the coefficients is as follows: β_1 estimates the impact of the FMLA on the "risk" of giving a birth among ineligible women, β_2 estimates the impact on birth for women who would have been eligible before the FMLA and β_3 is the coefficient of the interaction term and represents the DID estimate. I am primarily interested in the coefficient β_3, which captures the policy impact. It measures the differences in the changes of the hazard estimates of the policy between the treatment and comparison groups. In other words, it shows whether FMLA-eligible women's response to a birth is different from that of ineligible women.

3.6. Data

3.6.1. National Longitudinal Survey of Youth 1979 Cohort (NLSY79)

The main dataset for the analysis of fertility outcomes is based on micro data from the 1989 to 2006 waves of the National Longitudinal Survey of Youth 1979 cohort (NLSY79). Since 1979, interviews have been conducted on 12,686 U.S.-born young men and women who were 14 to 22 years old in 1979 and the respondents have been followed and interviewed annually up until 1994 and biennially from 1996. The NLSY79 is a large nationally-representative survey designed to provide comprehensive information on labor market activities and demographic information. There is an oversampling of Blacks, Hispanics, and economically disadvantaged non-Black/non-Hispanic individuals in the NLSY79. However, the NLSY79 provides customized sampling weights to adjust for this oversampling.

This panel structure of the NLSY79 data offers several advantages over cross-sectional data. For example, the longitudinal nature of the data allows one to follow the same individuals. This is important for this chapter in analyzing how women changed fertility behavior over time.

The sample period is from 1989 to 2006. During this period, it is possible to observe how fertility behavior changed over the years in response to the FMLA policy. By the design of the sample, the youngest person is 24 years old in 1989, the beginning of the analysis period. This means that the youngest person in the sample was 28 years old when the FMLA was implemented. Although the sample may not be representative of fertility in the U.S. during the sample period, almost 65% of the women over the age of 25 had given birth to at least one child during the same time frame (U.S. Census Bureau, 2009, Table 89). Further, the provisions of the FMLA and its strict eligibility criteria for maternity leave (for example, being at work in the past 12 months with the same employee) are likely to impact a certain set of women, the greater proportion of whom are likely to be found in the sample.

3.6.2. Dependent Variables and Controls

The dependent variable is a binary indicator that equals one if the woman gave a birth during the sample period 1989 to 2006 and zero otherwise. Specifically, there are two outcome variables, one for whether the person has a first birth and the other is whether the individual has a second birth. For each outcome, I control for a number of characteristics that are likely to affect fertility behavior such as the age, race and ethnicity, education, marital status, cognitive ability in the form of the age-adjusted Armed Forces Qualification Test (AFQT), family income, the number of children a woman had in the past and tastes for children such as women's views on gender roles in the household. I also include work history information such as job tenure. Year dummies are also included to allow for differential characteristics over time. Table B.1 provides the definition of each of the variables used and the units of measurement.

I organized the data in a person-year format. The person-year format means that the same woman appears in the sample each year until she either gives birth in a particular year or if she never gives birth throughout the sample period. This data restructuring will typically result in the number of person-years being greater than the actual number of women in the sample. See Appendix E for a description of this process.

3.6.3. Measurement and Other Data Issues

There may be drawbacks in interpreting the relationship between the FMLA impact and fertility outcomes. Other influences might bias the results. For example, there is the potential for sorting to occur before the policy, whereby women who desire to have children will choose those jobs that provide maternity leave benefits. Specifically, women with strong fertility desires may be prone to take up jobs that offer maternity leave irrespective of the FMLA, while those women with no desire for children may choose firms not covered under FMLA. In this case, the impact of FMLA on fertility is not clearly identified because it is fertility desires that may be driving women to access leave, as opposed to the availability of leave under the FMLA that is influencing childbearing preferences. Although the study by Averett and Whittington (2001) did not investigate the impact of the FMLA, the authors found no evidence that women who prefer having children were self-selecting into employment providing maternity leave.[14] Selection of this type is unlikely because under the FMLA, there is an implicit waiting period since all FMLA-maternity leave beneficiaries ought to have worked at least one year with the same employer and must have accumulated at least 1,250 hours during the past year (see Section 3.2).

To test the above empirically, Table 3.1 provides results of some regressions where the dependent variable is a dummy variable that equals one if an individual is employed at a job that offers job-protected maternity leave and zero otherwise.[15] Each specification (columns 1 to 2) includes covariates controlling for various demographic and other characteristics of the person such as age, race and ethnicity and education. The bottom of the table lists all the controls used. I also include state and year fixed-effects.

In column (1), the variable of interest is the "desired number of children" which was asked of each respondent in the 1979 wave of the NLSY79. If women with strong fertility desires are sorting into jobs offering maternity leave then the coefficient on this variable should be positive and significant. It turns out that the coefficient is negative and not statistically significant; suggesting that desired fertility does not appear to have any impact on the probability

[14] The authors estimated the effect of the "desired number of children" on the probability of being in a maternity leave job. They found that the coefficient on "desired number of children" was negative and insignificant. A statistically insignificant result indicates little/no evidence of selection bias caused by women sorting into firms offering maternity leave because they want to have children.

[15] Each wave of the NLSY79 asks respondents whether they are employed at a job that offers job-protected maternity leave. Job-protected maternity leave guarantees an individual the right to return to work after being granted leave from the same employer conditional upon the employee spending no more than the maximum allowable leave. As mentioned earlier, varying degrees of job-protected maternity leave existed in states before the FMLA was implemented.

of a woman choosing such a job.[16] In another specification (column 2), the variable "desired number of children" is interacted with a dummy variable "Eligible" which equals to one for a woman who would have become eligible for the FMLA and zero otherwise. The coefficient on this interaction is positive but is not statistically significant. This is evidence against the possibility that women who might have been eligible for FMLA were already choosing maternity leave jobs in anticipation of the policy. In summary, these results offer evidence to dispel the notion that a widespread number of women with strong fertility desires were sorting into jobs offering maternity leave.[17]

Table 3.1: Impact of Fertility Desire on choosing a Maternity Leave Job: OLS Regressions

Variables	(1)	(2)
	Dependent variable: Employer provides job-protected maternity leave	
Desired number of children	-0.0025	-0.0039
	(0.0028)	(0.0033)
Eligible*Desired number of children		0.0053
		(0.0040)
Year dummies	Yes	Yes
State dummies	Yes	Yes
Controls	Yes	Yes
Woman-year observations	40,775	40,775
Log-likelihood	0.15	0.25

Notes: Robust standard errors clustered by individual are reported in parentheses. In addition, all regressions control for age, education, marital status, AFQT scores in percentile, race, ethnicity, union status, whether the individual resides in an urban area, family income, number of children ever head, tastes for children as measured by women and job tenure.

[16] As shown in column 1 of Table 3.1, the coefficient of -0.002 is almost identical to the result obtained by Averett and Whittington (2001).

[17] There is also a selection bias whereby women who desire children may choose to not work. In which case, the probability of giving a birth increases for these ineligible women. By including women who are not working, I am actually underestimating the impact of the policy. Also, women in larger firms with longer tenure and more hours may be more committed to career and may be less likely to have children or more likely to delay. This would bias against finding fertility effects of FMLA on eligible women.

There may also be causality issues where the take-up of maternity leave is itself endogenous. That is, the decision of working women to proceed on maternity leave may be due to child-friendly employment benefits unrelated to FMLA and offered in covered institutions. If eligible women decide to have children because of these benefits irrespective of the FMLA, then the impact of the FMLA on fertility outcomes of eligible women is likely to be biased. This potential problem is likely to be minimal because childcare in the U.S. is widely available and is not limited to FMLA-covered institutions.

Other issues surround the use of the NLSY79 data. Because the NLSY79 has only one cohort, one potential issue is that for the women in the sample, the FMLA occurs in the middle of their peak fertility years. For example, for the FMLA to have an effect on the timing of a woman's first birth, she must not have had a first birth before the age of 28. The characteristics of these women may result in selection bias on the fertility impact of the FMLA. This bias is likely to be downwards because for physiological reasons older, childless women tend to have a lower probability of having a first birth. It is also possible for an upward bias to occur to the extent that women who are more educated and/or career-oriented may further delay having a first birth, thus making the probability of a first birth more likely at a later age.

There is possible measurement error in deriving the "Eligible" variable. One of the criteria for eligibility for FMLA is that the individual must be employed in a covered institution (see Section 3.2). According to the definition, covered employers comprise all public institutions (including schools) irrespective of the number of employees, private institutions with at least 50 employees, and private elementary and secondary schools irrespective of the number of employees. Even though all public schools are covered employers, only some private schools are. Information on the type of private school employer (whether elementary or secondary) is not provided in the NLSY79 and so it is possible that some private school firms are incorrectly included in the covered category. This discrepancy is likely to be small since the total number of private schools constitutes less than one percent of all firms in the US.[18]

In some firms, women receive additional benefits in the form of compensation while on maternity leave. Therefore it may be possible to find two women working in separate firms both of whom are eligible for FMLA but one of them can access paid leave while the other does not. There is no information in the NLSY79 that allows the researcher to differentiate between these two women. If both women decide to have children and proceed on maternity leave, but one did so because of the compensation irrespective of the FMLA, then the impact of FMLA eligibility on the probability of giving birth will be biased. Since only a small fraction of workers enjoy paid family leave benefits this bias is likely to be small.[19]

3.7. Empirical Results

3.7.1. Descriptive Statistics

Table 3.2 shows the summary statistics of the NLSY79 sample of women used in the chapter. The first column shows the statistics for all women during the observation period 1989 to 2006. Columns 2 and 3 provide statistics for women who would have been eligible for FMLA benefits before and after the policy was introduced, respectively. Meanwhile, columns 4 and 5 give statistics for ineligible women. These ineligible women are those who have worked some

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[18] In 2007, there were 33,740 private schools in the US. See the U.S. Department of Education National Center for Education Statistics, Private School Universe Survey 2007-2008 available at http://nces.ed.gov/programs/digest/d09/tables/dt09_058.asp (accessed October 2, 2010). In 2002, the total number of firms as reported by the Census Bureau was 22,974,655. See the U.S. Census Bureau: State and Country Quick Facts available at http://quickfacts.census.gov/qfd/states/00000.html (accessed October 2, 2010).

[19] For example, in 2007, only 8% of private sector workers could access paid family leave benefits. See U.S. Department of Labor, Bureau of Labor Statistics, *National Compensation Survey: Employee Benefits in Private Industry in the United States* at http://www.bls.gov/ncs/ebs/sp/ebsm0006.pdf (accessed September 21, 2010)

positive number of hours in each of the years of the sample period. If sample selection exists, one way of detecting this is if the demographic characteristics of the two groups of women differ substantively. More than half of the women are married and this fraction is similar across all categories of women before and after the policy. Across two groups, the women are similar in age and on average they have acquired about 13 years of schooling. There is a higher proportion of Black eligible women compared to the proportion of Black ineligible women. The opposite is true for women of Hispanic origin.

3.7.2. Discrete-Time Hazard Estimates: Probability of Having a First Birth

In this section, I use a discrete-time hazard model to present formal estimates of Equation (3.1) to test whether the introduction of the FMLA resulted in a differential impact on the likelihood of eligible women giving birth relative to other women. Table 3 presents estimates of the impact on the probability of a first birth. Columns 1 and 2 present the marginal effects based on the logit model shown in Equation (3.1) with time dummies included in all specifications. The key variable of interest is the interaction term FMLA*Eligible which indicates whether the birth response among FMLA-eligible women differs to that of ineligible women. The variable Eligible equals one if the individual satisfies the main criteria to be eligible for FMLA benefits and equal zero otherwise (see Section 3.2 for the list of criteria), while FMLA equals one for all years (1993-2006) since the start of the policy and zero otherwise.

The specifications in columns 1 and 2 differ by only the additional controls included in the latter. In both specifications, the coefficient on the interaction term is positive and statistically significant at conventional levels. These results indicate that the introduction of the FMLA has led to a 3.7 percentage point and 6 percentage point increase in the probability that an eligible woman will give a first birth.

Table 3.2: Summary Statistics for Women in the NLSY79 sample: Means (standard deviations)

Variables	All women	Eligible women		Working Ineligible women	
	1989-2006	Before (1989-1992)	After (1993-2006)	Before (1989-1992)	After (1993-2006)
	(1)	(2)	(3)	(4)	(5)
Dummy Explanatory Variables					
Eligible	0.444	1.000	1.000	0.000	0.000
Married	0.554	0.507***	0.534	0.570	0.583
Black	0.287	0.317***	0.355	0.229	0.253
Hispanic	0.180	0.159***	0.180	0.177	0.193
Union	0.117	0.172***	0.211	0.045	0.057
Urban	0.766	0.817***	0.751	0.782	0.746
Gender role1	0.144	0.118***	0.121	0.156	0.169
Gender role2	0.842	0.862***	0.855	0.832	0.828
Gender role3	0.258	0.225***	0.238	0.266	0.286
Continuous explanatory variables					
Age	34.744	29.529***	37.953	29.419	37.824
	(5.834)	(2.495)	(4.918)	(2.507)	(4.905)
Education	13.338	13.598***	13.741	12.887	13.162
	(2.356)	(2.247)	(2.380)	(2.273)	(2.365)
Family Income	50.060	45.963**	54.771	42.782	52.498
	(72.589)	(87.66)	(49.627)	(93.071)	(65.860)
Parity	1.898	1.630***	1.802	1.962	2.060
	(1.330)	(1.229)	(1.266)	(1.347)	(1.390)
AFQT score	43.087	45.837***	43.627	42.430	41.766
	(27.920)	(27.822)	(27.881)	(27.890)	(27.918)
Job tenure	245.132	228.013***	374.417	119.572	222.336
	(258.188)	(182.891)	(305.738)	(144.003)	(251.470)
Other variables					
High school dropout	0.087	0.063***	0.053	0.133	0.099
High school graduate	0.415	0.390***	0.381	0.447	0.436
Some college	0.275	0.281***	0.297	0.250	0.270
College graduate	0.223	0.266***	0.270	0.171	0.195
Government sector	0.171	0.268***	0.311	0.064	0.076
Private sector	0.654	0.727***	0.554	0.760	0.640
MLS states	0.282	0.276***	0.263	0.298	0.289
Employees	2279.151	6056.807***	1984.748	2534.387	651.741
	(13,222.750)	(22004.320)	(10548.880)	(15129.530)	(6702.972)
Hours work	1799.651	2098.985***	2171.072	1414.506	1591.103
	(789.281)	(404.634)	(504.041)	(821.257)	(904.100)
Desired number of children	2.507	2.477**	2.458	2.525	2.550
	(1.494)	(1.432)	(1.411)	(1.528)	(1.565)
Maternity leave	0.641	0.854***	0.879	0.434	0.473
Covered	0.580	1.000	1.000	0.247	0.244
N	42,353	6,582	12,224	9,059	14,488

Notes: Eligible women are those who satisfy all the criteria to be eligible for FMLA benefits. See Section 3.2 for these eligibility criteria. Ineligible women are those who do not satisfy all the eligibility criteria and who have worked some positive number of hours in each of the years during the sample period 1989-2006. Therefore, women who never worked in any year during the sample period are excluded. Statistical levels of significance, based on the difference between means of eligible and ineligible women before FMLA, are as follows: ** indicates p<0.05, *** indicates p<0.01.

The coefficients on a number of independent variables in the model show the expected sign in Table 3.3 (column 2). The likelihood of a first birth increases with age. Although the education variable is not significant, the measure of cognitive ability (AFQT scores) is negative and significant. Being married increases the probability of giving a first birth while race, ethnicity, union status and place of residence do not have any significant impact.

As a whole, the objectives of the FMLA reflect a commitment towards improving employees' needs to balance work and family responsibilities, thus promoting family integrity without comprising productivity in the workplace. Prior to the FMLA, it is likely that individuals' desire and ability to access leave would have been influenced by the institutional policies of the organization to which they belong. Given the universal nature of the FMLA, it is particularly interesting to investigate what effect the FMLA and its eligibility criteria had on first birth among specific sub-groups of the sample. I explore this in Table 3.4, Panels 1 to 3, where the results are shown separately by institutional sector, race and ethnicity, and education level. The estimates are based on a model that includes control variables as well as year fixed-effects.

Panel 1 separates the results according to whether the individuals were employed in the government sector or other sectors. The coefficient on the interaction term is positive and statistically significant in other sectors (column 3). On the other hand, in the government sector the likelihood of eligible women giving a first birth is not statistically significantly different to other women. This result in part might be explained by the fact that all public institutions are *covered* employers regardless of the number of employees, therefore making it far easier for an otherwise ineligible woman to become eligible.[20]

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[20] See Section 3.2 for a definition of a covered employer and how this relates to a woman being eligible for FMLA benefits. I also present models where the sample is disaggregated according to firm size. For firms with less than 50 employees, the coefficient of interest was not significantly different from zero. This result is not surprising because

Panel 2 divides the sample by race and ethnicity: Blacks, Hispanics and Whites as shown in columns 2 to 4, respectively. From these grouping, the only significant result is obtained within the White category, suggesting that among whites, eligible women increase their probability of giving a first birth by about 5.8 percentage points which is similar to the result obtained in the full sample. In terms of education level (Panel 3), eligible women among those with at least some college experience increase the probability of giving a first birth by 5.2 percentage points. In the case of Panel 2, one plausible explanation for this outcome is that to the extent that whites are more likely to be married and have high income-earning husbands, it makes it more feasible for them to take unpaid leave to have children under the FMLA. Similarly, the results in Panel 3 might be associated with the fact that white eligible women who are utilizing the leave benefits under FMLA the most also happen to be the more educated.[21] This result is also consistent with the findings that college-educated females were more like to take unpaid maternity leave while the FMLA policy appeared to have no significant impact on women who had never gone to college (Han, Ruhm and Waldfogel, 2007).

3.7.3. Discrete-Time Hazard Estimates: Probability of Having a Second Birth

Table 3.5 presents estimates of the impact on the probability of a second birth. The results in this table come from the same model specifications as in Table 3.3. The coefficient on the interaction term is positive and significant across both specifications as shown in columns 1 and 2. Using the results of column 2, the value of the coefficient indicates that eligible women

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firms in this size category (with a few exceptions as noted in Section 2) by definition are not *covered* institutions, thus making the employees ineligible for FMLA benefits.

[21]Therefore, the more educated are better able to utilize the leave benefits of the FMLA because of better knowledge about policy. Recall, only 58.2 percent of employees at covered worksites had any knowledge of FMLA (Department of Labor, 2007).

increase their probability of giving a second birth by about 4.2 percentage points following the introduction of the FMLA.

Independent variables that measure age, marital status and cognitive ability all have significant effects on the probability of giving a second birth. Age and marital status are associated with increased probability while AFQT scores have a downward influence on the probability of a second birth.

Similar to the first birth outcomes, I run the models with the probability of a second birth as the dependent variable with the results shown separately by the type of sector, race and ethnicity, and education. The results are presented in Table 3.6 and the specifications are the same as those found in Table 3.4. Specifically, Panels 1 to 3 of Table 3.6 correspond to the respective Panels 1 to 3 of Table 3.4. In almost all of the cases, the results are qualitatively similar in terms of statistical significance whereby eligible women working in other sectors significantly increase the probability of giving a second birth (Panel 1, column 3). On the other hand, white eligible women (Panel 2, column 4) and eligible women with at least some exposure to college education (Panel 3, column 3) have increased the probability of giving a second birth by 4.4 and 2.9 percentage points, respectively.

3.7.4. Some Calculations

Overall, the above results indicate a positive effect of FMLA on eligible women resulting in the increased probability of giving a first and second birth. The magnitude of the effect is larger for a first birth. Using the results in column 2 of Tables 3.3 and 3.5, I calculate the predicted probabilities of giving a first and second birth for eligible women. These are shown in the top panel of Table B.2. The probability of an eligible woman giving a first birth before the

Table 3.3: Estimates from Logit Regression: Likelihood of a First Birth (Marginal Effects)

Variables	(1)	(2)
	Dependent variable: first birth	
Eligible	-0.0130***	-0.0101***
	(0.0042)	(0.0024)
FMLA	-0.1343***	-0.9938***
	(0.0294)	(0.0023)
FMLA*Eligible	0.0367**	0.0597**
	(0.0171)	(0.0285)
Age		0.0168***
		(0.0040)
Age squared		-0.0002***
		(0.0001)
Education		-0.0002
		(0.0004)
Married		0.0335***
		(0.0030)
AFQT percentile		-0.0001**
		(0.0000)
Black		0.0012
		(0.0029)
Hispanic		-0.0019
		(0.0024)
Union		0.0024
		(0.0031)
Urban		-0.0012
		(0.0022)
Family income		-0.0000
		(0.0000)
Parity		0.0191***
		(0.0017)
Gender role1		-0.0014
		(0.0028)
Gender role2		-0.0031
		(0.0027)
Gender role3		0.0024
		(0.0026)
Job tenure		0.0000
		(0.0000)
Job tenure squared		-0.0080
		(0.0171)
Year dummies	Yes	Yes
Woman-year observations	12,277	11,981
Log-likelihood	-2668.420	-2024.656

Notes: Robust standard errors clustered by individual are reported in parentheses. Statistical levels of significance are: * indicates p<0.1, ** indicates p<0.05, *** indicates p<0.01.

Table 3.4: Estimates from Logit Regression: Likelihood of a First Birth by Sector, Race and Ethnicity and Education Level (Marginal effects)

Variables	(1)	(2)	(3)	(4)
PANEL 1: Sector	Full sample	Government	Other sectors	
FMLA*Eligible	0.0597**	0.0084	0.0605*	
	(0.0285)	(0.0198)	(0.0305)	
Woman-year observations	11,981	2236	9745	
Log-likelihood	-2024.656	-353.711	-1663.319	
PANEL 2: Results by race and ethnicity				
	Full sample	Black	Hispanic	Others (Whites)
FMLA*Eligible	0.0597**	0.0152	0.0118	0.0582*
	(0.0285)	(0.0141)	(0.0168)	(0.0312)
Woman-year observations	11,981	2946	1732	7303
Log-likelihood	-2024.656	-368.175	-281.577	-1331.076
PANEL 3: Results by level of education				
	Full sample	High school or less	At least Some College	
FMLA*Eligible	0.0597**	0.0145	0.0520*	
	(0.0285)	(0.0125)	(0.0284)	
Woman-year observations	11,981	3960	8021	
Log-likelihood	-2024.656	-616.706	-1391.836	
Year dummies	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered by individual are reported in parentheses. Statistical levels of significance are: * indicates p<0.1, ** indicates p<0.05.

Table 3.5: Estimates from Logit Regression: Likelihood of a Second Birth (Marginal effects)

Variables	(1)	(2)
	Dependent variable: Second birth	
Eligible	-0.0128***	-0.0054***
	(0.0024)	(0.0011)
FMLA	-0.1390***	-0.9937***
	(0.0389)	(0.0033)
FMLA*Eligible	0.0590***	0.0418**
	(0.0162)	(0.0183)
Age		0.0080***
		(0.0018)
Age squared		-0.0001***
		(0.0000)
Education		-0.0003
		(0.0002)
Married		0.0106***
		(0.0013)
AFQT percentile		-0.0000*
		(0.0000)
Black		0.0019
		(0.0014)
Hispanic		0.0008
		(0.0012)
Union		0.0002
		(0.0014)
Urban		-0.0010
		(0.0010)
Family income		-0.0000
		(0.0000)
Parity		0.0104***
		(0.0009)
Gender role1		-0.0005
		(0.0015)
Gender role2		0.0002
		(0.0013)
Gender role3		0.0016
		(0.0013)
Job tenure		0.0000*
		(0.0000)
Job tenure squared		-0.0037
		(0.0068)
Year dummies	Yes	Yes
Woman-year observations	21671	21059
Log-likelihood	-3281.282	-2409.276

Notes: Robust standard errors clustered by individual are reported in parentheses. Statistical levels of significance are: * indicates p<0.1, ** indicates p<0.05, *** indicates p<0.01.

Table 3.6: Estimates from Logit Regression: Likelihood of a Second Birth by Sector, Race and Ethnicity and Education Level (Marginal Effects)

Variables	(1)	(2)	(3)	(4)
PANEL 1: Sector	Full sample	Government	Other Sectors	
FMLA*Eligible	0.0418**	0.0005	0.0487**	
	(0.0183)	(0.0133)	(0.0230)	
Woman-year observations	21059	3837	17222	
Log-likelihood	-2409.276	-392.058	-2087.414	
PANEL 2: Results by race and ethnicity				
	Full sample	Blacks	Hispanics	Others (Whites)
FMLA*Eligible	0.0418**	0.0324	0.0040	0.0444*
	(0.0183)	(0.0281)	(0.0117)	(0.0253)
N (in person-years)	21059	5645	3242	12172
Log-likelihood	-2409.276	-553.178	-465.181	-1431.350
PANEL 3: Results by level of education				
	Full sample	High school or less	At least Some College	
FMLA*Eligible	0.0418**	0.0445	0.0287*	
	(0.0183)	(0.0313)	(0.0177)	
Woman-year observations	21059	8410	12649	
Log-likelihood	-2409.276	-1067.191	-1391.289	
Year dummies	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered by individual are reported in parentheses. Statistical levels of significance are: * indicates p<0.1, ** indicates p<0.05.

FMLA was introduced is 25.96 percent. After the FMLA, the probability of giving a first birth rises by 5.97 percentage points to 31.87 percent. Similarly, for a second birth, the probability rises by 4.18 percentage points from 28 percent to 32.18 percent. By taking a simple average, the probability of giving a first or second birth rose by an average of 5.08 percentage points to 32.03 percent after the FMLA. This represents an increase of approximately 19 percent.

While there are currently no known studies examining the fertility impact of the FMLA on eligible women, the results can be compared with a U.S.-based study that investigates the impact of having a maternity leave job on fertility. In Panel B of Table B.2 I provide the implied probabilities using estimates from a study by Averett and Whittington (2001). As mentioned earlier, the authors in this study examined the impact of being employed in a job offering maternity leave on the probability of giving birth to a child. Their sample comprises all working women and covers the period 1985 to 1992. The baseline probability of having a child for working women is found by dividing the number of births over the sample period by the number of women in the sample. That is 1325/3590 equals 36.9 percent. The marginal effect of the impact of having a maternity leave job on the probability of having the first child is 3.7 percentage points while the marginal effect of having a higher order birth due to maternity leave is 2.8 percentage points. Therefore, the average marginal effect is 3.25 percentage points which is the average of 3.7 and 2.8. Using this information, the predicted probability of having any child after having a maternity leave job is approximately 40.15 percent (36.9+3.25=40.15).

How do the implied calculations from the Averett and Whittington study compare? On average, maternity leave increased the probability of working women having a child by 3.25 percentage points or about 8.8 percent. The calculations in the current study suggest that FMLA increased the probability of eligible women having a child by an average of 3.97 percentage points or by about 19 percent. The results of this chapter are closer to those of Lalive and Zweimüller (2005) who finds that women were 15 percent more likely to have an additional birth under a policy in Austria.

3.7.5. Sensitivity Analysis

Some states had maternity leave statutes (MLS) prior to the introduction of the FMLA. It is possible individuals residing in states that already had some form of MLS are unlikely to be affected as much by the FMLA, since in some cases, the benefits provided under the FMLA may have been less generous. Also, in 2002 California introduced the Paid Family Leave Bill that provides disability compensation for employees unable to perform work duties due to the birth of a child along with other provisions. In light of these circumstances, I estimate the main model shown in Equation (3.1) by excluding California. In a separate regression, I exclude all states that had an MLS (California is included as well) prior to the FMLA. The results are shown in Table 3.7 (columns 2 and 3, respectively). Panels 1 and 2 present the results when the dependent variable is the probability of a first birth and second birth, respectively. Even with these exclusions, there is still evidence of an increase in the probability of eligible women giving a first and second birth.

Instead of removing these states that had some form of maternity leave statutes, I kept them, coding such states with a dummy variable and interacting with eligibility status. This would give some variation in access to leave that did not turn on in 1993. The results as shown in column 4 (both panels), are robust to this specification. In column 5, I re-estimate the model with a placebo effect. Here, I assume that the policy started in some period other than the year it actually took place. In panel 1, the policy is assumed to have taken place during the period

Table 3.7: Estimates from Logit Regression: Sensitivity Analysis on the likelihood of a Birth (Marginal Effects)

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PANEL 1: Dependent variable - First birth	Full sample	Excluding California	Excluding MLS states	Including Eligible * MLS interaction term	Placebo Effect: FMLA lasted 1990-1992	Linear time trend interacted with eligibility (OLS)	Eligibility based on firm size	Eligibility based on hours worked	Eligibility based on first year eligibility status
FMLA*Eligible	0.0597**	0.0599**	0.0586*	0.0640**	-0.0004	0.0242*	0.0669**	0.1167**	0.05187**
	(0.0285)	(0.0300)	(0.0324)	(0.0317)	(0.0083)	(0.0133)	(0.0312)	(0.0510)	(0.0247)
Woman-year observations	11,981	10,594	8,167	11,981	11,981	11,981	11,981	11,981	11,981
Log-likelihood/R-square	-2024.656	-1786.173	-1407.624	-2023.696	-2030.963	0.13	-2021.654	-2000.465	-2026.407
PANEL 2: Dependent variable – Second birth	Full sample	Excluding California	Excluding MLS states	Including Eligible * MLS interaction term	Placebo Effect: FMLA began in year=1990	Linear time trend interacted with eligibility (OLS)	Eligibility based on firm size	Eligibility based on hours worked	Eligibility based on first year eligibility status
FMLA*Eligible	0.0418**	0.0464**	0.0438*	0.0346**	0.0584	0.0138*	0.0410**	0.0657**	0.0288**
	(0.0183)	(0.0232)	(0.0246)	(0.0171)	(0.0360)	(0.0084)	(0.0181)	(0.0262)	(0.0135)
Woman-year observations	21,059	18,752	14,903	21,059	21,059	21,059	21,059	21,059	21,059
Log-likelihood/R-square	-2409.276	-2237.696	-1741.871	-2492.559	-2497.656	0.09	-2494.485	-2477.452	-2502.871
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered by individual are reported in parentheses. Statistical levels of significance are: * indicates $p<0.1$, ** indicates $p<0.05$.

1990-1992 while in column 2, it is assumed that the FMLA started in 1990. The estimates of the placebo effects can be compared with the estimates of the policy effect in column 1. As one would expect, the placebo effects are not statistically significant, thus providing further support to the identification strategy. In column 6, I consider the possibility that there may be some pre-existing differential trends in fertility between eligible and non-eligible women. To control for this, I include a linear time trend and interacted it with the "Eligibility" dummy. Once again, the results are robust to this specification.[1]

In the next set of robustness checks, I assume that the criteria for eligibility is based either on firm size, with hours worked allowed to vary (column 7) or eligibility is based solely on hours worked with the size of the firm allowed to vary (column 8). Recall in Section 3.2, for the purposes of the FMLA, eligibility is based on both hours worked and size of firm in which the individual is employed. Compared with the base estimates in column 1, the results in columns 7 and 8 are larger in the majority of the cases. Even more interestingly are those estimates in column 8. The coefficient is almost twice as large as in the case of a first birth. This is interesting because the results can have important policy implications. One interpretation of this finding is as follows: if the eligibility criteria for receiving FMLA benefits were based only on the amount of hours an employee works (disregarding the size of the firm), then the fertility impact will be larger, almost doubling in the case of a first birth outcome. Given that almost 90 percent of the firms in the U.S. employ 20 or less individuals, it would appear that a change in policy that relaxes the eligibility criteria can have a profound fertility impact through its effect on a broader spectrum of the labor force.[2]

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[1]I also ran the same regression, this time including a quadratic trend and interacting with eligibility status. The results (not shown) are also robust to this specification.
[2]Data source: http://www.census.gov/epcd/www/smallbus.html (accessed Jan 25, 2011).

One final piece of robustness check is that I use eligibility status in the first year as opposed to the current year. That is, in the year an individual first becomes eligible for FMLA benefits, the assumption is that individual remains eligible through the remainder of the sample period. The results are shown in column 9, panels 1 and 2. Even with this static form of eligibility, the results remain significant and of the expected the sign.

While it is not possible to guarantee that the FMLA was the sole cause of the relative increase in fertility behavior among eligible women, there is no evidence that some other factor(s) with a similarly universal impact and unrelated to the FMLA could have affected eligible women to the same degree as the previous findings indicate.

3.7.6. Parametric Estimation Using Log-normal Regressions

The results obtained above have been generated from a set of semi-parametric models. These models are more appropriate in cases where the distribution of the outcome is not exactly known. However, there are some parametric (distribution-based) models prevalent in the social sciences and that are appropriate in this analysis. One commonly–used model is the log-normal model associated with skewed outcomes and is also used to measure the response times of an individual to an event.[3] The model measures the log of survival time. Specifically, the log-normal model assumes a log-normal distribution of the form:

$$\log(t_i) = \beta_1 FMLA_t + \beta_2 Eligible_{it} + \beta_3 (FMLA_t * Eligible_{it}) + \beta_4 \mathbf{X}_{it} + \boldsymbol{\varepsilon}_{it} \qquad (3.2)$$

[3] Inspection of the data reveals positive skewness in the outcomes used in this analysis.

where the dependent variable measures the log of survival time in years, t. The remainder of the model is the same as in Equation (3.1). The error term is assumed normal with zero mean and constant variance.[4]

The log-normal model is one of several duration/survival models known as accelerated failure time (AFT) models.[5] In these models the researcher is interested in determining how long a case will last before a "failure" (in this case, giving a birth) occurs. Related to this study, I am interested in determining the length of time a woman takes or "survives" before giving a birth. Although the log-normal model does not have a hazard-form interpretation, its AFT form of interpretation is related to hazard functions: in general an increase in the hazard of an event implies a decrease in the expected log survival time. Because of the form of the log-normal model, interpretation of the coefficients is straightforward: a unit increase in the covariate leads to a β increase/decrease in the logged survival time. Put another way: an increase in the covariate leads to a $100*\beta$ percent change in survival times. Based on the previous findings which show that eligible women have a higher "hazard" of giving birth since FMLA, this implies that eligible women ought to have shorter survival times before giving a birth. Therefore, the coefficient on the interaction term (FMLA*Eligible) in the log-normal regressions is expected to be negative.

Table 3.8 presents the results of the log-normal time-to-birth regressions for the two birth outcomes. The specifications in columns 1 and 2 are similar to the models shown in columns 2 of Tables 3.3 and 3.5, respectively. The sign of the coefficient on the FMLA*Eligible interaction term in columns 1 and 2 of Table 3.8 is opposite those of the discrete time hazard models in columns 2 of Tables 3.3 and 3.5, respectively. Since eligible women after FMLA have

[4] The dependent variable "t" is distributed log-normal.

[5] Other models of the AFT type include Exponential, Weibull and log-logistic.

a *greater* hazard of giving birth then it implies that these women will take *less* time on average to do so. In both outcomes, the coefficients are statistically significant. For an interpretation of the coefficient on the interaction term in Table 3.8: following the FMLA, eligible women are taking on average 37.8 percent less time before having a first birth (column 1) while taking 21.1 percent less time before having a second birth (column 2).

To put this into context, Table B.3 presents some calculations on the time taken for eligible women to give a first and second birth before and after the FMLA. Column 1 gives the baseline average waiting time in years. In the sample, eligible women took just over two years before giving a first or second birth. With the introduction of the FMLA, they have reduced this waiting time by 0.88 years or about 11 months for a first birth and by 0.5 years or six months for a second birth, resulting in a fall in the average waiting time before giving a birth of 0.69 years or approximately 8 months (column 2). The predicted new waiting times are approximately 1.4 years for giving a first birth and just under two years for a second birth (column 3). On average, the waiting time has been reduced to 1.65 years or approximately 20 months.

To illustrate how the probability of giving a birth changed over the years, Figures 3.1 and 3.2 plot the fitted log-normal survival functions between FMLA-eligible women and ineligible women for the first and second birth outcomes. The plotted predicted survival functions clearly indicate eligible women have lower probabilities of not having either a first or a second birth. The differences in the probabilities between the two groups of women get larger after about the fourth year which is when the FMLA began.

3.8. Discussion and Conclusion

This chapter examines the impact on fertility outcomes of a policy - the Family and Medical Leave Act (FMLA) - that allows women to proceed on job-protected maternity leave. Because the FMLA has clearly-defined eligibility criteria and empirical evidence suggests that it may have been associated with an increase in work leave taken, it is possible to identify the impact on fertility by comparing the outcome among eligible women (who are able to access FMLA leave) with the fertility outcomes of those who are not eligible for FMLA.

I find that the implementation of the FMLA has resulted in eligible women increasing their probabilities of giving birth to a first and second child. The magnitude of the effects appears larger for a first birth. Specifically, among eligible women, the FMLA increased the probability of giving birth by 6 percentage points for a first birth and 4.2 percentage points for a second birth. These changes are equivalent to respective increases in probabilities of 23.1 and 14.9 percent from the baseline. The results of the analysis also show that eligible women are giving birth to the first child 11 months earlier and about 6 months earlier for the second child.

I also consider the impact of the policy across sectors, race and ethnicity, and education level. These results indicate that the FMLA is more effective in non-government sectors. There is no difference in the responsiveness to birth among Blacks and Hispanics. Meanwhile, among Whites, eligible females have significantly higher probabilities of giving a first or second birth. Eligible women with at least some education experience at the college level are more likely to give a first or second birth after FMLA.

These results are consistent with increased leave being taken by eligible recipients (Ross, 1998; Waldfogel, 1999; Han and Waldfogel 2003; Han, Ruhm and Waldfogel 2007). The results are also consistent with the findings that college-educated females were more like to take unpaid maternity leave while the policy appeared to have no significant impact on women who had never gone to college (Han, Ruhm and Waldfogel 2007). In addition to the effectiveness of the policy in influencing child preferences, by allowing eligible women the right to return to their former jobs after giving birth, the FMLA has effectively improved the labor outcomes of new parents (Waldfogel, 1999). Beyond these, the findings raise the possibility that the policy may have improved other outcomes not yet explored. For instance, the long-term educational attainment and health outcomes of children born to FMLA-eligible women represent important areas for future research.[6]

This research offers some insights into family friendly policies in the U.S. There are a number of potential implications arising from such a policy. First, the FMLA can help to address the balance between women's work and family life. Second, it can help to address the length of delay in childbearing among career oriented women. Finally, there are also implications for the retention of women in the labor force given that educated women may opt out of paid employment after having children.

There is evidence that the FMLA is under-utilized. For instance, only 58.2 percent of employees at covered worksites had any knowledge of FMLA (Department of Labor, 2007). In addition, I find that if the criteria for eligibility were relaxed, the fertility impact of the policy could almost double, most notably by impacting a wider spectrum of the labor force. On these bases alone, the effect of the FMLA on fertility outcomes can increase overtime through greater public awareness and perhaps a revisiting of the policy.

[6] Using the NLSY, specifically the NLSY79 Children and Young Adults, it is possible to link these eligible women directly to their children. Since 1988, the NLSY has collected information on children born to the 1979 women cohort. These children were age 10 and older. There is a variety of outcomes of interests such as schooling, health and fertility. Berger *et al*. (2005) find that women taking less than the maximum allowable leave period under the FMLA have children with worse health outcomes, attributable in part to the lower levels of breastfeeding and immunizations. These are short-term outcomes and so the impact on longer-term outcomes of interest is worth investigating.

Table 3.8: Estimates from Log-normal Regression: Time-to-birth in years

Variables	(1)	(2)
	Dependent variable: log of Time to birth in years	
	First birth	Second birth
Eligible	0.267***	0.253***
	(0.062)	(0.058)
FMLA	0.901***	0.773***
	(0.087)	(0.073)
FMLA*Eligible	-0.378***	-0.211**
	(0.104)	(0.091)
Age	-0.588***	-0.555***
	(0.090)	(0.094)
Age squared	0.010***	0.010***
	(0.001)	(0.001)
Education	0.014	0.022**
	(0.012)	(0.011)
Married	-0.679***	-0.526***
	(0.058)	(0.052)
AFQT percentile	0.002*	0.003**
	(0.001)	(0.001)
Black	-0.038	-0.081
	(0.073)	(0.064)
Hispanic	0.056	0.017
	(0.076)	(0.065)
Union	-0.023	-0.003
	(0.077)	(0.074)
Urban	-0.011	0.003
	(0.063)	(0.054)
Family income	-0.000	0.000
	(0.000)	(0.000)
Parity	-0.563***	-0.640***
	(0.028)	(0.032)
Gender role1	0.072	0.019
	(0.080)	(0.074)
Gender role2	0.059	-0.072
	(0.074)	(0.066)
Gender role3	-0.029	-0.009
	(0.066)	(0.059)
Job tenure	-0.000	-0.001***
	(0.000)	(0.000)
Job tenure squared	0.045	0.475
	(0.481)	(0.395)
Woman-year observations	11981	21059
Log-likelihood	-1389.944	-1734.124

Notes: Robust standard errors clustered by individual are reported in parentheses. Statistical levels of significance are: * indicates p<0.1, ** indicates p<0.05, *** indicates p<0.01.

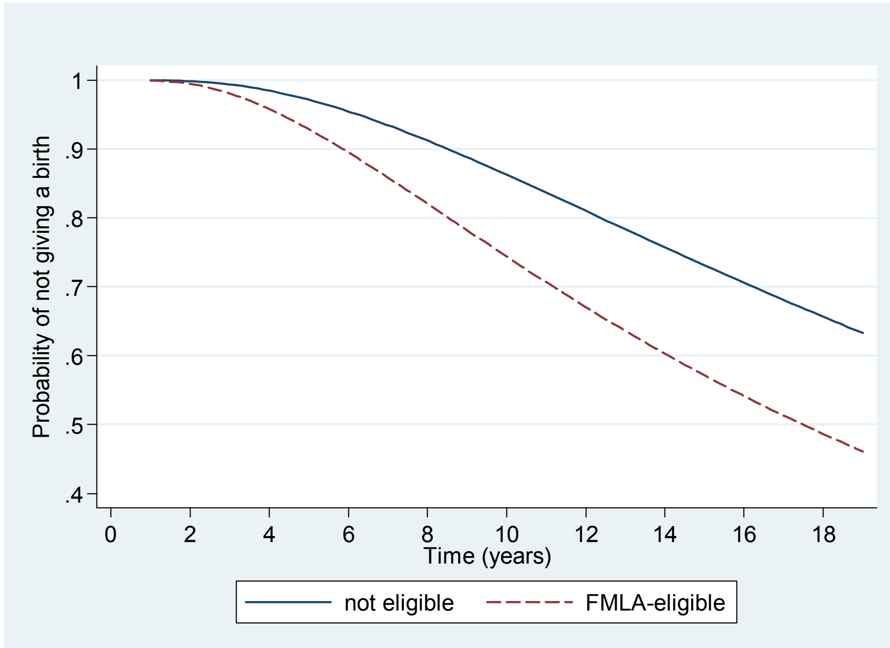

Figure 3.1: Log-normal Survival Plots of not giving a first birth

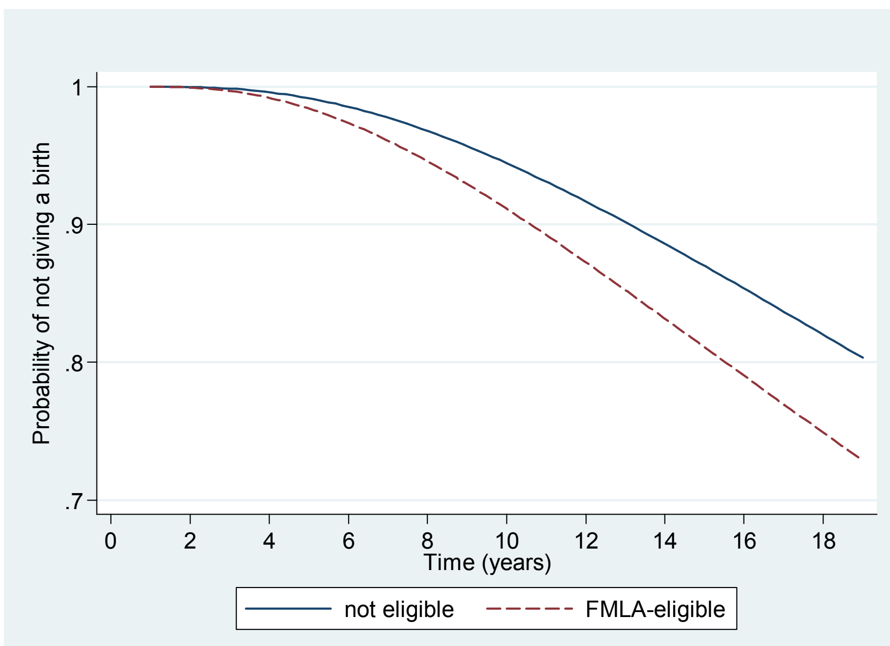

Figure 3.2: Log-normal Survival Plots of not giving a second birth

CHAPTER 4. EMPOWERING WOMEN: THE IMPACT OF EDUCATION ON PREFERENCES – EVIDENCE FROM A POLICY EXPERIMENT IN SIERRA LEONE

4.1. Introduction

The study of the benefits of education has occupied a considerable part of the economics literature, but a large and disproportionate amount of interest has been dedicated to the private returns to education. Recently, economists have examined the role of education in influencing other outcomes such as health (Chou, Liu, Grossman and Joyce 2010; Currie and Moretti 2003; Grossman 1972a, 1972b; Lleras-Muney 2005). Differential levels of schooling are linked with income inequality and lower levels of productivity which affect economic well-being and development (Chen, 2005; Glomm and Ravikumar, 1992). In the human capital literature, the benefits of education through wage increments raise income *per capita.* Human capital investment through higher education influences household decisions on the number of children to produce (Becker 1991). In developing countries, low levels of human capital investment combined with high fertility rates are associated with individuals possessing relatively fewer years of schooling.

Another important correlate of education is time preference. Education alters time preferences as people become more future-oriented and invest in human capital (Becker and Mulligan 1997). The education-preference link helps to alter family structure and fertility patterns. With more education and increased knowledge, women become more efficient in choosing the desired family size. Consequently, other preferences such as the type of contraceptive to use and decision regarding sexual initiation are altered. If education alters preferences, then schooling may impact both private returns and other outcomes. In this case, private returns will underestimate the value of education without accounting for social returns.

Despite the evidence on the association between education and other outcomes, a causal link between the two is not well established in the empirical literature. The key challenge in estimating the impact of education on preferences is that causality can run in the opposite direction. Individuals who are more future-oriented are likely to invest more in human capital by obtaining more education. In which case, education is no longer exogenous.

In this chapter, I address this endogeneity by using changes in an individual's year of birth and exposure to differential amounts of education input to instrument for schooling. These changes in year of birth and level of education investment were induced by a policy reform that provided free primary education to children in Sierra Leone. Individuals that were of the primary school age (6 to 11 years) when the policy began in 2001 were treated with free and compulsory primary education. In places where education is least accessible, public investments in education were higher. The policy had a significant effect on education. There is no evidence that changes in education reflected past trends. Further, increases in education investment under the policy raised education attainment among those who were of primary school age but had no effect on older individuals. I also examined whether increases in schooling altered preferences among women. They did.

Prior to the Free Primary Education (FPE) policy, payment of tuition fees, teaching and learning materials as well as text book purchases were the responsibility of parents. In addition, every primary school student was required to take the National Primary School Examination (NPSE) before enrolling in a secondary school. Students (or their parents rather) were also responsible for the NPSE fee. Under the FPE, the Government eliminated all such fees at the primary school level (UNESCO, 2003).

The FPE involved a large construction program rehabilitating damaged schools as well as building additional schools aimed at revitalizing the education system. Implementation of the policy enabled Sierra Leone to witness an unprecedented increase in the number of primary schools and access to education. With free and compulsory education, many disadvantaged groups such as females who had little or no opportunity for education in the past, were now in school. Following the introduction of the FPE policy, primary school enrollments doubled between the years 2001 and 2004 (Statistics Sierra Leon, 2008). During the same period, the number of recognized primary schools rose by more than 20 percent from 3,467 to 4,298 with about 30 percent of these newly constructed. In contrast, the total number of schools was just 2,240 in 1989 (World Bank, 2007).

Using cross-sectional data from the 2008 Sierra Leone Demographic and Health Survey (SLDHS), I link individual fertility and education data from the SLDHS with district-level data to reflect variation in the amount of investments in education since the introduction of the FPE policy. In determining an individual's exposure to the program, I consider the age of the individual when the policy began and the district of residence where higher educational inputs were devoted to those residing in areas with the least access to schooling.[1] Educational inputs are measured as the amount of funding allocated for primary school education per 100 teachers.[2] I restrict the sample to only women. Women who were of primary school age (6 to 11 years) were treated while older women were deemed to be in the control or untreated group. After

[1]There is the potential for biased estimates if mass migration occurred during the period of the policy. For instance, it may be the case that an individual's current place of residence may not be the same place where schooling was received. Ideally, I would like to know the respondent's childhood place of residence but this information is not available in the SLDHS. However, later in the paper, I address this potential problem by only considering those individuals who have never migrated or changed place of residence.

[2]Funding per teacher is an indicator of quality of education. More funding per teacher implies an increase in teacher resources available for education. Anecdotal evidence indicates that the greater share of education investment was committed to improving the infrastructure. This was in spite of the fact that there was a shortage of teachers.

controlling for year of birth and district fixed-effects, it is plausible to assume that the interaction of the treatment (those who were of primary school age during the FPE policy) and the level of educational input will be unrelated to preferences. I therefore use this interaction term to instrument for years of schooling.

Overall, the results suggest that the FPE policy was associated with increased schooling opportunities and the associated impact of education in altering preferences among women. The policy increased schooling by 0.85 years. More years of schooling altered the fertility desires of women by delaying the start of first sexual encounter and increasing the use of modern contraceptives. The results also indicate that education has increased empowerment among women by influencing their opinions regarding the rights of women to decide under what conditions they have sexual relations with their husbands.

I believe this analysis contributes to the literature in two distinct ways. First, whereas most studies typically focus on a single link involving education and a particular outcome, this chapter provides the causal links from policy to education to preferences. Only a handful of studies have investigated the impact of large education investment outlays on schooling, as well as the impact of education on fertility-related preferences. Osili and Long (2008) and Breierova and Duflo (2004) show how variation in the timing and specific intensity of large school construction programs are used to measure the direct impact of education on fertility in Nigeria and Indonesia, respectively. Their findings indicate that an additional year of schooling leads to a reduction in the average number of children born by as much as 5 percent among women under the age of 25. Second, I use a recently-compiled dataset for a country on which no known recent research work has been conducted in this area.

The remainder of the chapter is organized as follows. Section 4.2 provides some background information on the educational system in Sierra Leone and the FPE policy. Section 4.3 describes the dataset used in the analysis. Section 4.4 describes the empirical strategy. In Section 4.5, I discuss the results of the impact of the policy on schooling and the associated impact on preferences. Section 4.6 concludes.

4.2. Education in Sierra Leone and the Free Primary Education (FPE) Policy

4.2.1. Some Background on Education in Sierra Leone Prior to the FPE

Prior to and just after its independence in 1961, Sierra Leone had an education system that was comparable to many countries including those of the industrialized world, and was appropriately dubbed the "Athens of West Africa". Nineteenth and early twentieth century Sierra Leonean society may have been characterized as having an education culture as the population sought greater empowerment post-colonial rule. This educational thrust and the realization of the goals of education were partly influenced by the sustained economic growth occurring at the time. From the mid-1970s, Sierra Leone began to experience difficult economic conditions and increased political instability. Government's apathy towards education contributed to a decline in the educational standards. In the midst of the economic turmoil, parents found it increasingly difficult to keep their children in school, oftentimes choosing which of their children to invest in (National Research Council, 1993).[3] The period from 1970 to 1990 witnessed a decline in the growth of school enrollment rates, particularly in primary schools, and the quality of education had declined.

[3] At the time, Sierra Leone was already one of the poorest regions in Africa. The war and deteriorating economic conditions only served to exacerbate the situation.

In 1991, Sierra Leone was engaged in an eleven-year long civil war which ultimately led to the killing of at least 50,000 individuals (World Bank, 2007), the displacement of a couple million to neighboring countries while thousands were injured and/or maimed.[4] One of the contributing factors to this violence was a reluctance and failure of government to provide educational opportunities, thus providing the impetus for a rebellion of the disenfranchised youths against the state. To put this into context: by the end of the war in 2002, 1,270 primary schools had been destroyed (International Monetary Fund, 2001), representing over 70 percent of the 1,795 schools estimated to have been in existence before the conflict. As a result, more than half of the primary schools had disappeared and this led to almost 70 percent of school-age children being out of school (International Monetary Fund, 2001).

Although the war began with an invasion of rebels from the Eastern region of the country, the violence eventually spread to all four regions (see Figure 4.1). Consequently, there was substantial variation in regional damages to primary school infrastructure as shown in Figure 4.2. In the figure, a higher index corresponds to greater damage: a damage index of 1 indicates a classroom is in usable condition, index 2 indicates that a classroom is in need of minor repairs, index 3 is for a classroom in need of rehabilitation and index 4 means a classroom requires reconstruction. As illustrated, most of the damages occurred in the Eastern region where the war began, while the least damage appears to have been in the Western region where the capital is located and where the greatest resistance to the rebel forces occurred. The Northern region comprises most of the country's diamonds for which much of the war was being fought.

In summary, a country that once gained notoriety for having one of the better education system in the colonial and post independence era, had now become one of the lesser-known on the African continent by the late 1990s. A weakening education system brought on by

[4]The population in 1991 was 4.1 million. By the end of 2005, it had risen to 5.5 million.

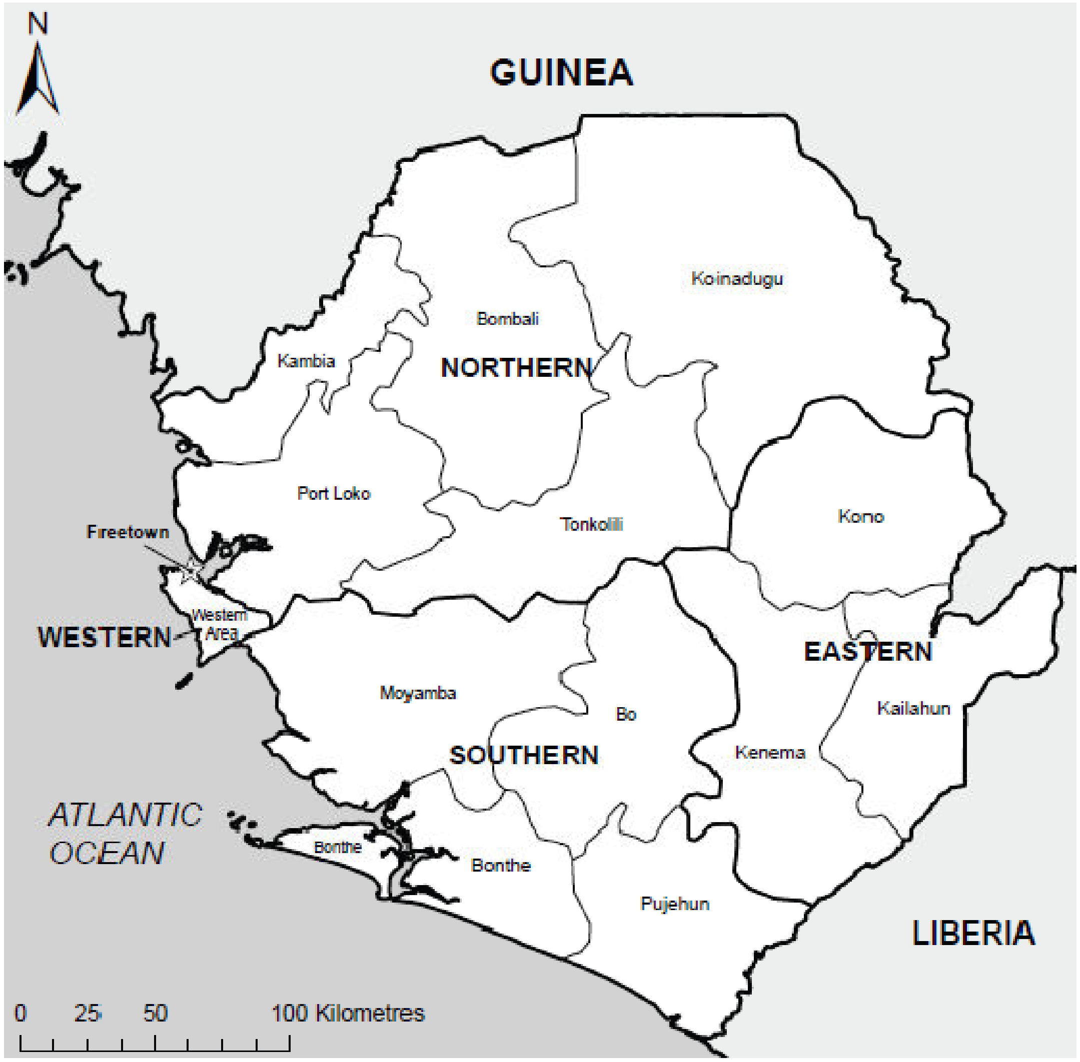

Figure 4.1: Sierra Leone divided by districts in 2004

Notes: The fourteen districts are as follows: Eastern region consists of Kailahun, Kenema and Kono; Western region consists of the Western Urban Area (comprising mainly the capital Free Town) and the Western Rural Area; Northern region consists of Bombali, Kambia, Koinadagu, Port Loko and Tonkolili and the Southern region comprises Bo, Bonthe, Moyamba and Pujehun.

Source: Sierra Leone Demographic and Health Survey 2008 Report.

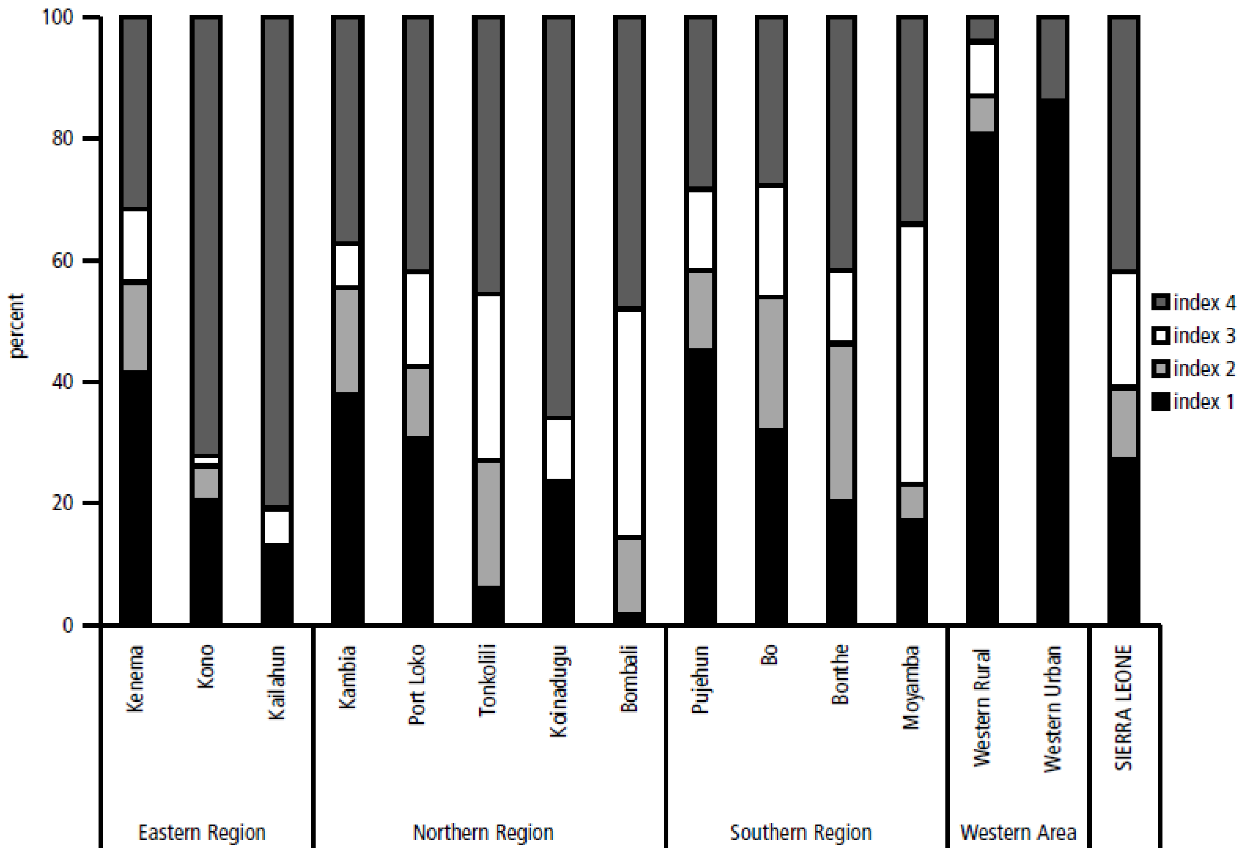

Figure 4.2: Distribution of Primary School Damage Index in 2004

Notes: In the figure, a higher index corresponds to greater damage: a damage index of 1 indicates a classroom is in usable condition, index 2 indicates that a classroom is in need of minor repairs, index 3 is for a classroom in need of rehabilitation and index 4 means a classroom requires reconstruction. As illustrated, most of the damages occurred in the Eastern region where the war began, while the least damage appears to have been in the Western region where the capital is located and where the greatest resistance to the rebel forces occurred. The Northern region comprises most of the country's diamonds for which much of the war was being fought. See Figure 1 showing a map of the country.

Source: World Bank (2007).

worsening economic conditions was further exacerbated by an eleven-year civil war and corruption. With a return to civilian rule, a new political regime, committed to nation- building and the adoption of appropriate education policies served as the prelude to the free primary education (FPE) policy being introduced.

4.2.2. An Overview of the Free Primary Education (FPE) Policy

In 2001, Sierra Leone implemented a policy of providing free primary education (FPE) for all pupils in government-owned and government-assisted schools.[5] Prior to the FPE policy, payment of tuition fees, teaching and learning materials as well as text book purchases were the responsibility of parents. In addition, every primary school student was required to take the National Primary School Examination (NPSE) before enrolling in a secondary school. Students (or their parents rather) were also responsible for the NPSE fee. Under the FPE, the Government eliminated all such fees at the primary school level (UNESCO, 2003).

Financed largely from domestic public funds, donor contributions from international financial institutions and foreign governments, the FPE was part of an overall universal primary education policy aimed at constructing and rehabilitating basic education facilities as well as providing greater access and increased educational opportunities to disadvantaged youths such as women. The goal was to improve the quality of basic education (comprising six years of primary education and three years of secondary education), making it free and compulsory. By far, this has been the largest education funding initiative to be undertaken since independence. The provision of free basic education, in particular primary education, became a policy priority for the government of Sierra Leone. During the period 2001 to 2005, the government committed

[5]At least 95 percent of all recognized primary and secondary schools are either government-owned or government-assisted (World Bank, 2007).

almost 20 percent its expenditures (equivalent to 4 percent of Gross Domestic Product) towards education. Almost half of all education expenditure was devoted to primary education (UNDP, 2007). At the same time, the number of schools grew rapidly and by the end of 2005 there were at least 4,300 primary schools of which approximately 30 percent were newly constructed since 2001. To put this in perspective, in 1985 Sierra Leone had a grand total of 2,240 schools!

The introduction of the FPE policy was associated with a pronounced change in primary school enrollments. Primary school gross enrollment rates almost doubled from 89 percent to 162 percent in 2004.[6] This helped to narrow the gender gap to the point where girls accounted for 45 percent of the enrollment. In 2004, Sierra Leone had the highest primary school enrollment rates amongst all Sub-Saharan countries (World Bank, 2007). The stock of students between 2001 and 2004 doubled from 0.6 million to 1.3 million students. Within this context, the FPE program has been influential in the rapid growth and progress in the education sector.

4.3. Data

4.3.1. Sierra Leone Demographic and Health Survey (SLDHS)

For this investigation, I mainly use data from the 2008 Sierra Leone Demographic and Health Survey (SLDHS). The SLDHS is part of a broader program of Demographic and Health Surveys conducted in other developing countries. Interviews for the SLDHS were conducted from April to June 2008. While funding for the survey came from a variety of international agencies, the collection, processing and dissemination of the data was undertaken by Statistics Sierra Leone in collaboration with the Ministry of Health and Sanitation, with technical support

[6]Primary school gross enrollment rate for any given year is the number of students enrolled in primary school expressed as a percentage of the population of primary-school-age children. In Sierra Leone, the official primary school age ranges from 6 to 11 years. It is possible for the primary school gross enrollment rate to be above 100 percent. This is because some of the enrolled may consist of older-aged children who have returned to school after the war, pre-primary school-aged kids attending primary school as well as repeaters.

provided by ORC Macro International. The data are considered to be very good quality and the survey methods used to obtain information are similar to those utilized in the 2004 Population and Housing Census. The SLDHS is a cross-sectional nationally representative household survey and is the first of this kind to be conducted in Sierra Leone.[7] It contains a wealth of information for over 10,000 individuals consisting of 7,374 women and 3,280 men who were aged 15-49 and 15-59, respectively in 2008. For this study, I limit the sample to females only.

I use a variety of dependent variables measuring preferences towards fertility, sexual behavior, contraceptive use as well as measures of women's empowerment. As a proxy for fertility preference, I use a variable measuring the number of children desired by each female. In general, a more appropriate variable will be the total number of children ever born. While this information is available, it does not fully reflect the fertility outcomes of the FPE cohort consisting of 15 to 18 year olds who have not completed their fertility cycle. As a measure of sexual behavior, I use a variable measuring "age at first sex" while contraceptive use is measured with a dichotomous variable equaling one if the individual uses a modern contraceptive method and zero otherwise. The following indicators of women's empowerment were also considered and used: whether a wife is justified in refusing to have sex when she is tired or not in the mood, whether the respondent has used any modern contraceptive, whether the respondent has ever been tested for the AIDS virus, whether a wife is justified in refusing to have sex with her husband if she knows he has a sexually transmitted disease (STD), whether a wife is justified in asking that they use a condom if she knows her husband has an STD and whether the respondent believes a husband is justified in hitting or beating his wife if she refuses to have sex with him. Appendix F provides the list of the questions as they were asked to the respondents.

[7]Demographic and Health Surveys (DHS) have been conducted for more than 75 developing countries since 1985. In most of these countries, there are several waves of the survey. The DHS is not a panel but rather consists of repeated cross-sectional data and so it is not possible to follow the same household over time.

To test for the effect of education on preferences, I include a number of covariates to control for potential confounding factors impacting both the outcome variable and education. These controls consist of measures relating to individual demographic, social and economic characteristics. Although its primary role is to collect information on fertility history and reproduction, the SLDHS contains valuable information allowing for an extensive set of covariates. In addition to education which is measured as the total years of completed schooling, I also include the following explanatory variables:

- Socio-demographic variables: age, marital status, ethnicity and religion
- Welfare variables: whether or not the person is employed and a wealth index
- Economic status: individuals that have durable consumer goods (television, radio and refrigerator) and urban residence

In general, the SLDHS has many advantages such as an extensive set of questions seeking information on women's preferences and opinions on women's issues and rights. However, there are some limitations to using these data. Longitudinal data are preferred and tend to be more reliable than one-shot retrospective surveys such as the SLDHS. Although there are measures of wealth, the SLDHS lacks information on income. The SLDHS data include no direct measure of region of birth, childhood place of residence or place of education. Either one or a combination of these provides information in determining an individual's level of exposure to the FPE program. For instance, if an outcome is observed for an individual in a district which is different to the district of education because of migration, then this will lead to a bias in the program effect. The extent of the bias will be influenced by the amount of migration and the type (rural-urban or within-district). In recent years and with the end of the civil war, rural-urban migration has been on the decline. About 50 percent of the individuals in the sample indicated ever having moved. However, among this group of "movers", there is evidence that most of this migration consisted of two forms: movement between cities in the same urban sprawl and movements from cities and towns to the countryside. Migration tends to be more common among the older groups compared to younger individuals who comprise the greater proportion of this investigation. Because the SLDHS does not provide information on where an individual's education began or ended, the extent or direction of the biasness of the effect of the program cannot be easily determined. Later, I address this issue by using a sample of individuals who have never moved from their current place of residence.

Table 4.1 (column 2) reports descriptive statistics for all women in the 2008 SLDHS. On average, women desire to have approximately five children over their lifetime, they tend to have their first sexual encounter at the age of 14 and less than one-quarter of the sample uses some form of modern contraception. Education levels are low in comparison to developed countries. The average years of completed schooling is less than three compared with an average of at least 12 in developed countries such as the United States. The women in the sample are less than 30 years of age and their religious affiliation is mainly Islam or Christianity. There are two main ethnic groups: the Temne and Mende.

4.3.2. Other Data

In addition to the SLDHS, I use other sources of data to carry out this investigation. In particular, district-level data were obtained and matched to the individual-level SLDHS data. The district-level information include the number of schools constructed, funds allocated to primary education and school enrollment data which are related to the FPE program. Descriptive statistics of the district-level data are displayed in Table 4.2.

4.4. Empirical Strategy

In order to investigate the impact of schooling on preferences, one must show that more schooling changes preferences. This is important because causality can occur in the opposite direction whereby individuals who are more future-oriented are more likely to increase their years of schooling. In order to address this issue, I need at least one exogenous variable which influences education and is unrelated to preferences at the time. In this chapter, two such factors are considered. The first is year of birth or age. Specifically, only individuals who were between 6 and 11 years (those who were born between 1990 and 1993) received free primary education. The second is the intensity of free primary education. More educational inputs are provided in districts where access to basic education is lower.

4.4.1. Treatment (FPE Cohort) Effect on Schooling

In Sierra Leone, primary school education on average lasts a total of six years, from age 6 to 11. At the start of the FPE in 2001, all women born during the period 1990 and 1993 were between the ages 8 and 11 years and so would have been exposed to the program in 2001. These people represent the treated group or the free primary education cohort (FPE cohort). Those who were 11 years or older were not treated.

For greater comparability between the two groups, I restrict the sample to women who were 8-21 years old when the FPE was introduced in 2001. The main sample therefore will consist of the following two groups: The first group consists of those individuals those who are 15 to 18 years old but were 8 to 11 years old when the FPE was introduced and received free primary education. This is the FPE cohort and they were born during the years 1990 to 1993. The second group comprises those who are 22 to 28 years but were 16 to 21 year old when the

FPE was implemented. This is the control group and these women were born during the period 1980 to 1985. This group should not have received free primary education. Summary statistics for these two groups of women are shown in columns 3 and 4 of Table 4.1. The differences in the means between the treated group and the control group are shown in column 5. In most of the cases, these differences are statistically significant at the 5 percent level. Of particular interest are the years of completed schooling between the treated group and the control group. The former group has achieved more than 2 years of schooling compared to the control group.

Note that the main sample excludes women aged 12 to 15 when the program was introduced. The reason for this is non-trivial. Persons in this group should have completed primary school and not have benefitted from the program. In reality, this may not have been the case. As mentioned earlier, the high primary school gross enrollment rates were partly due to a high proportion of late entrants to primary school as well as grade repetitions. That is, many children attend primary school at an age which is above the official age for that particular grade.[8] Therefore, it is possible that some children benefitted from the program when it fact they should not have. If this is the case, then the control group is not free of contamination. This gives rise to biased estimates.[9] To ensure that the chosen control group had minimal or no exposure to the program since its introduction, an appropriate set of individuals will be those who were 16 to 21 in 2001 (these were born 1980 to 1985).[10]

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[8]In 2004, more than half the children enrolled in the first grade of primary school (where the official age is six years) were above the age of six. This was true for the other grade levels of primary school (World Bank, 2007).

[9]In any event, including this group of women in the estimation will only underestimate the impact of the policy, if in fact there was widespread over-age enrollment at the primary school level.

[10]The reason for using this specific group of individuals as the control group is non-trivial. The majority of over-age children attending primary school tend to be no more than three to four years above the legal primary school-age range of 6-11. Therefore, someone who is in grade 6 of primary school and is over-aged can be expected to be at

The education of those who were treated (FPE cohort) and those who were not can be compared using an equation of the form:

$$S_{ijk} = \alpha_0 + \alpha_1 \text{FPE Cohort}_k + \alpha_2 \mathbf{X}_{ijk} + \boldsymbol{\varepsilon}_{ijk} \quad (4.1)$$

where S_{ijk} stands for years of schooling of individual i who resides in district j and belongs to cohort k, "FPE Cohort" equals one for an individual born during the period 1990 and 1993 and equals zero otherwise (this denotes those females age 8 to11 when the FPE program was introduced in 2001), $\mathbf{X}_{ijk}$ is a vector of measured covariates including demographic, socio-economic and cultural variables that are expected to influence education. The coefficient α_1 is the parameter of interest and it measures the treatment effect on schooling. One would expect this coefficient to be positive since the "treated" are provided with more opportunities for schooling because education is free and compulsory. The model also has a constant term, α_0 and a random error term $\boldsymbol{\varepsilon}_{ijk}$. Standard errors are robust and clustered at the district level and year of birth. The regression also controls for district fixed-effects.

4.4.2. Interactions between Treatment (FPE Cohort) Effect and Education Funding (FPE Inputs) on Schooling

The second aspect of the identification exploits the fact that the FPE policy provided differential amounts of free primary education inputs according to the education requirements in each district.[11] That is, more education inputs are provided in those areas where there is a greater

most 14 to 15 years old. However, the number of people 14 to 15 years and attending primary school is considered to be very small.

[11]Sierra Leone is divided into four main regions (East, West, North and South) comprising a total of 14 districts where one leader from each district has a seat in the country's parliament. The design of the SLDHS was done to take this district-level grouping into consideration. Figure 1 shows the division of these districts according to regions.

Table 4.1: Summary Statistics: 2008 SLDHS Sample (Individual Level Data)

Variables	Variable Definition	Full sample	Treated group	Control group	Difference (3)-(4)
		Mean (std. dev)	Mean (std. dev)	Mean (std. dev)	Mean (std. dev)
	(1)	(2)	(3)	(4)	(5)
Desired number of children	Number of children desired	5.059	4.116	4.832	-0.717***
		(2.309)	(0.065)	(0.048)	(0.081)
Age at first sex	Age at first sexual encounter	14.452	8.050	15.550	-7.500***
		(4.853)	(0.243)	(0.071)	(0.207)
Modern contraception	Equals one if uses modern contraceptive, zero otherwise	0.236	0.082	0.256	-0.174***
Tested for AIDS	Equals one if respondent has ever been tested for the AIDS virus, zero otherwise	0.193	0.059	0.181	-0.122***
Wife beating justified in refusing sex	Equals one if respondent thinks that wife beating is justified if she refuses to have sex with husband, zero otherwise	0.373	0.239	0.386	-0.146***
Wife justified in refusing sex when tired	Equals one if respondent thinks a wife is justified in refusing to have sex with her husband when she is tired or not in the mood, zero otherwise	0.602	0.514	0.621	-0.107***
Wife justified in refusing sex with STD husband	Equals one if respondent thinks a wife is justified in refusing to have sex with a husband having an STD, zero otherwise	0.536	0.456	0.554	-0.098***
Wife justified in asking to use a condom with STD husband	Equals one if respondent thinks a wife is justified in asking that a condom be used when having sex with a husband having an STD, zero otherwise	0.578	0.513	0.615	-0.102***
Schooling	Years of schooling completed	2.871	4.745	2.394	2.353***
		(4.224)	(0.114)	(0.098)	(0.158)
No education	Equals one if no education, zero otherwise	0.625	0.285	0.695	-0.410***
Some primary only	Equals one if some primary education, zero otherwise	0.098	0.212	0.090	0.122***
Primary	Equals one if full primary education, zero otherwise	0.039	0.076	0.021	0.056***
Some secondary	Equals one if some secondary education, zero otherwise	0.186	0.416	0.130	0.286***
Secondary	Equals one if full secondary education, zero otherwise	0.025	0.008	0.034	-0.026***
Higher	Equals one if higher than secondary education, zero otherwise	0.028	0.002	0.030	-0.028***
Year of birth					
Born 1958-1961	Equals one if born between 1958 and 1961, zero otherwise	0.031	0.000	0.000	
Born 1962-1967	Equals one if born between 1962 and 1967, zero otherwise	0.095	0.000	0.000	
Born 1968-1973	Equals one if born between 1968 and 1973, zero otherwise	0.188	0.000	0.000	

(Table 4.1 continued)

Variables	Variable Definition	Full sample	Treated group	Control group	Difference (3)-(4)
		Mean (std. dev)	Mean (std. dev)	Mean (std. dev)	Mean (std. dev)
	(1)	(2)	(3)	(4)	(5)
Born 1974-1979	Equals one if born between 1974 and 1979, zero otherwise	0.171	0.000	0.000	
Born 1980-1985	Equals one if born between 1980 and 1985, zero otherwise	0.241	0.000	1.000	
Born 1986-1989	Equals one if born between 1986 and 1989, zero otherwise	0.145	0.000	0.000	
Born 1990-1993 (FPE Cohort)	Equals one if born between 1990 and 1993, zero otherwise	0.129	1.000	0.000	
Age	Age in years	28.975	16.518	25.434	-8.916***
		(8.928)	(0.037)	(0.039)	(0.061)
Employed	Equals one if employed, zero otherwise	0.705	0.434	0.740	-0.306***
Wealth	Wealth index ranging from 1 to 5	3.216	3.504	3.192	0.312***
		(1.452)	(0.046)	(0.034)	(0.057)
Married	Equals one if married, zero otherwise	0.634	0.180	0.718	-0.538***
Radio	Equals one if has a radio, zero otherwise	0.623	0.656	0.628	0.028
Fridge	Equals one if has a refrigerator, zero otherwise	0.086	0.114	0.081	0.033***
TV	Equals one if has a television, zero otherwise	0.138	0.171	0.129	0.043***
Urban	Equals one if resides in an urban area, zero otherwise	0.429	0.534	0.411	0.122***
Religion					
Christian	Equals one if belong to Christian religion, zero otherwise	0.263	0.298	0.237	0.062***
Islam	Equals one if belong to Islam, zero otherwise	0.730	0.693	0.756	-0.062***
Other religion	Equals one if belong to another religion, zero otherwise	0.007	0.009	0.011	0.002
Ethnicity					
Temne	Equals one if belong to Temne ethnic group, zero otherwise	0.284	0.275	0.311	-0.036**
Mende	Equals one if belong to Mende ethnic group, zero otherwise	0.369	0.382	0.349	0.033*
Other ethnicity	Equals one if belong to another ethnic group, zero otherwise	0.347	0.343	0.340	0.003
Observations		7,374	950	1777	

Notes: Statistical levels of significance are: * indicates $p<0.1$, ** indicates $p<0.05$, *** indicates $p<0.01$. The "treated group" is also the FPE cohort consisting of those individuals born during the period 1990 to 1993. The control group consists of those individuals born during the period 1980 to 1985. Differences may not match exactly due to rounding.

Table 4.2: Summary Statistics: Variables Used in the Analysis (District-level Data)

Variables	Observations=14	
	Mean	Standard deviation
	(1)	(2)
Imputed Primary Education funds allotted to districts (Le million)	1.001	(0.533)
Share of local government development grants (FY 2006) (in percent)	7.15	(3.804)
Female-male primary school enrollment ratio	0.813	(0.103)
Number of primary schools per district in Fiscal year 2004	306.786	(132.089)
Number of school teachers by district in 2004	1,379.714	(784.518)
FPE input: Log of funding allocated for primary school education for every 100 teacher in 2004	1.294	(0.637)
Proportion of FPE cohort women in each district	0.121	

Sources: UNDP (2007), Statistics Sierra Leone (2008), Statistics Sierra Leone (SLL) at http://www.statistics.sl/ (accessed August 31, 2010) and Government of Sierra Leone Government Budget and Statement of Economic and Financial Policies for Financial Year 2003 address at http://www.statehouse-sl.org/gov-budget-2003.html (accessed August 31, 2010).

need for basic education. Therefore to estimate the full impact of the policy, the following equation applies:

$$S_{ijk}= \varphi_0 + \varphi_1\text{FPE Cohort}_k + \varphi_2\text{FPE Inputs}_j +\varphi_3(\text{FPE Cohort}_k *\text{FPE Inputs}_j) + \varphi_4\mathbf{X}_{ijk} +\boldsymbol{\varepsilon}_{ijk} \quad (4.2)$$

where the control variable "FPE Inputs" is the logarithm of funding allocated for primary school education for every 100 teacher in 2004. Data on the amount of funding allocated to primary school education was imputed using two sets of information: (1) aggregate primary education funding and (2) the share of local government development grants for each district. I imputed the primary education funding to each district using the formula: $E_j = E*s_j$, where E_j is the amount of primary education funds allocated to district j which is derived from the product of total allocation of funds for primary education (E) and the share of local government development grants for district j (s_j).[1]

[1] Information on the district-level share of local government development grants and matching grants was for fiscal year 2006 (see UNDP, 2007, p.79), while total funding allotment for primary education came from the Government of Sierra Leone Budget and Statement of Economic and Financial Policies for the financial year 2003 (accessed at http://www.statehouse-sl.org/gov-budget-2003.html on August 31, 2010).

The coefficient on the interaction term is the independent variable of interest. Those who are treated and resided in districts where the policy has the greatest impact should have higher levels of schooling. Therefore, one expects φ_3 to be positive.

4.4.3. Schooling on Preferences: Instrumental Variables (IV) Strategy

To estimate the effect of schooling on preferences, the following equation is specified:

$$Y_{ijk}= \beta_0 + \beta_1 S_{ijk} +\beta_2 \mathbf{X}_{ijk} +\boldsymbol{\varepsilon}_{ijk} \tag{4.3}$$

where Y_{ijk} comprises the following preference variables: the desired number of children, age at first sexual intercourse along with the following dummy variables: whether a wife is justified in refusing to have sex when she is tired or not in the mood, whether the respondent has used any modern contraceptive, whether the respondent has ever been tested for the AIDS virus, whether a wife is justified in refusing to have sex with her husband if she knows he has a sexually transmitted disease (STD), whether a wife is justified in asking that they use a condom if she knows her husband has an STD and whether the respondent thinks a husband is justified in hitting or beating his wife if she refuses to have sex with him. The variable S_{ijk} is the number of years of schooling and $\mathbf{X}_{ijk}$ is the vector of individual characteristics.

It is easy to run Equation (4.3) in a regression using Ordinary Least Squares (OLS) and obtain an estimate of β_1, the parameter of interest. However, β_1 can only be interpreted as having a causal effect if $E(\mathbf{X}_{ijk}, \boldsymbol{\varepsilon}_{ijk})=0$ and $E(S_{ijk}, \boldsymbol{\varepsilon}_{ijk})=0$. If schooling and the error term are correlated (i.e. $E(S_{ijk}, \boldsymbol{\varepsilon}_{ijk})\neq 0$), then β_1 cannot be interpreted as the causal effect of education on preferences. In this case, education is deemed to be endogenous. Endogeneity of education may arise from omitted variables that influence both schooling and preferences in a preference equation. It may also be the case that education is a proxy for unobservable personal traits such as ability and motivation and family characteristics which are part of the error term in a preference equation. In these situations, the OLS estimate is biased.

To address this endogeneity bias and establish a causal link between education and preferences, I use instrumental variable (IV) estimation. For a variable to serve as a valid instrument it must be correlated with the endogenous explanatory variable (schooling) and also be exogenous. The first feature implies that the instrument must directly influence the level of schooling while exogeneity suggests that the instrument's impact on preferences only occurs via the education channel. A key assumption of this chapter is that any non-stochastic changes in preferences and education among cohorts are due to variation in the impact of the FPE across districts. Put differently, in the absence of FPE, there would be no systematic variation in schooling and preferences from one district to another. Further, it is also assumed that FPE is not directly related to preferences and its effect on preferences is through the education variable. If these conditions are satisfied, then the FPE program can be used to construct instruments for schooling.

By invoking the assumption that exposure to FPE (based on program intensity within districts and year of birth) is correlated with schooling but have no direct effect on preferences, I can then use the interaction of year-of-birth and FPE inputs as an instrument for schooling.

4.5. Results

4.5.1. Impact of Treatment (FPE Cohort) on Schooling

Table 4.3 displays the estimation of Equation (4.1) which compares years of schooling for the treatment or FPE cohort (those born between 1990 and 1993 or who were 8 to 11 years in

2001) versus the untreated group of women who were born between 1980 and 1985 (ages 16 to 21 when the FPE policy started in 2001). The table shows two specifications of the regression: the specification in column 1 does not included district fixed-effects while column 2 does. District fixed-effects control for unobserved heterogeneity between districts that do not change over time. There may be substantial variation in district-level schooling attributable to unobserved district characteristics such as the quality of teaching, differences in economic development and other social factors. This is important because in many cases, districts that were impacted the most by the program also tended to be the lesser developed areas.

The regressions in both columns have controls for age, employment status, an index of wealth, marital status, religion and ethnicity binary variables as well as dummies for whether the household has a radio, refrigerator or TV. Also, to help isolate the impact of the program on education I control for the ratio of female-male primary school enrollment. The specifications also include an interaction of this ratio and the FPE cohort dummy. Regressions are also weighted by female population and number of teachers. Standard errors are clustered at the district and birth year level.

The first row of Table 4.3 displays the primary variable of interest. Recall that “FPE Cohort” is defined as those women who were 8 to 11 years when the program was implemented in 2001. The coefficient on FPE Cohort in this table is positive and statistically significant across both specifications. These results provide evidence that the impact of the treatment increased years of education.

Table 4.3: The Impact of Treatment on Schooling - OLS Regressions

Variables	(1)	(2)
	Dependent variable: Years of schooling	
FPE Cohort	3.927***	3.734***
	(1.254)	(1.197)
Age	0.962**	0.979***
	(0.380)	(0.350)
Age squared	-0.023***	-0.023***
	(0.008)	(0.007)
Employed	-0.936***	-0.931***
	(0.223)	(0.229)
Wealth	0.372***	0.365***
	(0.071)	(0.069)
Married	-2.484***	-2.556***
	(0.268)	(0.263)
Radio	0.225	0.211
	(0.239)	(0.241)
Fridge	1.128**	1.137**
	(0.507)	(0.511)
TV	1.485***	1.474***
	(0.268)	(0.273)
Christian	1.933***	2.002***
	(0.470)	(0.474)
Islam	0.549	0.655
	(0.468)	(0.441)
Temne	-0.178	-0.347
	(0.289)	(0.341)
Mende	0.219	0.344
	(0.222)	(0.236)
Urban	1.010***	1.174***
	(0.234)	(0.235)
District fixed-effects	No	Yes
Observations	2667	2667
R-square	0.43	0.43

Notes: Standard errors in parentheses are clustered at the year*district level. Statistical levels of significance are: * indicates p<0.1, ** indicates p<0.05, *** indicates p<0.01. The sample comprises the FPE cohort (those born during the period 1990 to 1993) and those females born during the period 1980 to 1985. The model also includes a variable for female-male primary school enrollment ratio in 2004 in the district. This variable is also interacted with "FPE Cohort" which equals one for individuals born 1990-1993 and zero otherwise. Regressions are weighted by female population*number of teachers. Constant term is not reported.

4.5.2. Impact of Treatment and Education Funding on Schooling

Table 4.4 reports the results of the model from Equation (4.2) which fully reflects the impact of the FPE program on schooling. The sample is the same as the model used in the previous sub-section. That is, it comprises those females born between 1990 and 1993 (or who were 8 to 11 years in 2001) and females born between 1980 and 1985 (who were 16 to 21 years in 2001). The former group is the treatment or FPE cohort while the latter group is the control or untreated group. The dependent variable is years of schooling. I include the same controls as in Table 2. The effect of treatment and level of education funding is measured by the coefficient on the interaction term FPE cohort and FPE Inputs. This effect is measured by the coefficient φ_3 and it represents the primary coefficient of interest.

Each column of the table represents a different specification of the regression. The first specification comprises only the covariates while the second specification includes both the covariates in the first specification plus district fixed-effects. The third and final specification includes year of birth or birth year fixed-effects to keep constant time-varying unobserved factors that might be correlated with the program. Such factors might include other government programs and policies around the same time as the FPE.

The results in columns 1 to 3 bear out the predictions of the model. The coefficients on the interaction terms are positive and statistically significant throughout all specifications. Using the preferred specification (column 3), the FPE policy resulted in an increase in schooling of about 0.85 years.[2] Given that the average years of schooling are quite low at 2.87, this represents

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[2] Using Equation 4.2 and the results from column 3 of Table 4.4, this amount is obtained as follows: The effect of treatment (T) on schooling (S) is given by $\frac{\partial S_i}{\partial T_i} = \widehat{\varphi_3} * \overline{\text{FPE input}}$, where $\widehat{\varphi_3}$ is the estimated coefficient on the interaction term in Equation 2 and $\overline{\text{FPE input}}$ is the mean log of primary school funding for every 100 teacher (see Table 4.2). By using the average log of primary school funding for every 100 teacher of 1.294, the effect of the policy is to increase average schooling of the treated group by $\widehat{\varphi_3} * \overline{\text{FPE input}}$ = 0.654*1.294=0.85 years.

Table 4.4: The Impact of Treatment and Exposure on Schooling - OLS Regressions

Variables	(1)	(2)	(3)
	Dependent variable: Years of schooling		
FPE Cohort	2.713**	2.607**	
	(1.317)	(1.278)	
FPE Input	-0.393**	0.004	-0.055
	(0.190)	(0.253)	(0.320)
FPE Cohort * FPE input	0.730***	0.689***	0.654***
	(0.265)	(0.258)	(0.234)
Age	0.959**	0.978***	1.092
	(0.367)	(0.336)	(0.753)
Age squared	-0.022***	-0.023***	-0.043**
	(0.008)	(0.007)	(0.019)
Employed	-0.943***	-0.939***	-0.966***
	(0.222)	(0.229)	(0.216)
Wealth	0.373***	0.370***	0.383***
	(0.070)	(0.069)	(0.069)
Married	-2.487***	-2.558***	-2.537***
	(0.267)	(0.263)	(0.255)
Radio	0.210	0.203	0.210
	(0.242)	(0.243)	(0.217)
Fridge	1.126**	1.141**	1.170**
	(0.507)	(0.511)	(0.465)
TV	1.483***	1.468***	1.343***
	(0.269)	(0.273)	(0.260)
Christian	1.910***	1.983***	1.934***
	(0.460)	(0.467)	(0.458)
Islam	0.533	0.644	0.577
	(0.457)	(0.432)	(0.434)
Temne	-0.191	-0.348	-0.318
	(0.290)	(0.341)	(0.339)
Mende	0.183	0.355	0.344
	(0.223)	(0.236)	(0.252)
Urban	0.973***	1.159***	1.093***
	(0.236)	(0.234)	(0.232)
District fixed-effects	No	Yes	Yes
Year of birth fixed-effects	No	No	Yes
Observations	2,667	2,667	2,667
R-square	0.43	0.43	0.44

Notes: Standard errors in parentheses are clustered at the year*district level. Statistical levels of significance are: * indicates $p<0.1$, ** indicates $p<0.05$, *** indicates $p<0.01$. The sample comprises the FPE cohort (those born during the period 1990 to 1993) and those females born during the period 1980 to 1985. The model also includes a variable for female-male primary school enrollment ratio in 2004 in the district. This variable is also interacted with "FPE Cohort" which equals one for individuals born 1990-1993 and zero otherwise. FPE input is calculated as the log of funding allocated for primary school education for every 100 teacher in 2004. The dummy variable "FPE Cohort" which equals one for individuals born 1990-1993 is not included in some specifications because the interpretation changes when birth-year fixed-effects are included in the regressions. Regressions are weighted by female population*number of teachers. Constant term is not reported.

a fairly large impact. This is approximately 30 percent increase in the mean years of schooling which is identical to the results in Osili and Long (2008, pg. 73) who investigated the impact of schooling using a similar experiment in the case of Nigeria. Duflo (2001) undertook a similar strategy for a more developed country, Indonesia, and found a much smaller impact on schooling. It represents about 36 percent of the difference in the mean years of schooling between the treated group and control group.

Other independent variables such as age, whether employed, wealth, marital status, household possessions, being a Christian and living in an urban area are significant determinants of schooling and carry the expected signs in most cases.

4.5.3. Further Support for the Identification Strategy

So far in the discussion, I have used as a control group those women who were born 1980 to 1985 (ages 16 to 21 when the FPE started in 2001). This was done to avoid the control group being "contaminated" with over-aged women who may have benefitted from the program when in fact they should not have. The group of over-aged women who may have benefitted from the FPE policy would have likely come from those in the age range 12 to 15 when the policy began. For greater comparability, this group would be ideal since the women are closer in ages to the treatment group. If overage enrollment was widespread among these women then using them as the control group should result in no significant difference in schooling between them and the treatment as a result of the program. I examine this issue in Table C.1. The specifications in columns (1) to (3) are identical to those in Table 4.4 and the estimates are obtained from the same regression model as shown in Equation (4.2). The results indicate that the coefficient on the interaction term is still positive and statistically significant throughout all specifications.

This is evidence to suggest that the FPE did not have a significant effect on the control group and so it does not appear that overage enrollment at the primary school level was widespread.

4.5.4. Some Robustness Checks

An important concern in this analysis relates to migration. As with most African countries, Sierra Leone has had a history of population mobility. Such migration may be external or internal (either within or across districts). The crux of this investigation and identification strategy is that an individual's exposure to the program is determined by district of education. If migration patterns are such that an individual whose current place of residence is different to the one where his/her education was received then this could lead to biased estimates due to measurement error. Almost half of the sample has lived at a previous residence. To address this measurement error, I exclude all women who have at one point lived in a different residence in the past. The sample consists of the treatment group (those born 1990 to 1993) and the control group of women born between 1980 and 1985. In another specification, I change the control group to those women much closer in age to the treatment. Specifically, the control group in this case is those born 1986 to 1989. The respective results are presented in columns 1 and 2 of Table C.2. For simplicity, I only show the results of the coefficient on the interaction term. Once again, the coefficient estimates on the interaction term carry the expected signs and are statistically significant for both specifications.

There are a couple of considerations arising from these results. First, although there appears to be substantial movement of individuals from their previous place of residence, it is not clear whether this migration was across districts or within the same district. If migration is more prevalent in the latter case, then measurement error is not too costly. Second, migration patterns in Sierra Leone have long tended to be more temporary in that many individuals often leave their home to undertake seasonal employment to other areas and then return to their region of birth. Recent studies have tried to emphasize the importance of temporary migration as an alternative explanation to the "rural-urban" form which pervades the literature (de Haan and Rogary, 2002). According to these studies, the extent of rural-urban migration and the high urban population growth that is presumed to occur may be exaggerated or not well understood.

The next robustness check is a falsification exercise. This is to test whether the results may be capturing the effect of some other reform or features of the data which might possibly lead to spurious results. For this, I assume that the reform occurred much earlier than it did. Specifically, I use an older group of individuals born between 1968 and 1973 (these were ages 28 to 33 in 2001 when FPE was introduced) as the treated group while maintaining the same control group of individuals born between 1980 and 1985 (ages 16 to 21 in 2001). If FPE exposure had a significant impact on this older cohort, then it raises questions as to whether the identification strategy is the appropriate mechanism that links the changes in these outcomes. The results of this exercise are shown in column 3 of Table C.2. The impact of treatment and exposure has no significant effect on schooling.

The results of the above analysis provide additional evidence supporting the identification which comes from variation in the treatment and exposure to influence years of schooling. This provides a basis for examining the impact of schooling on preferences.

4.5.5. Impact of Schooling on Preferences: OLS Estimates

I begin by analyzing the impact of education on preferences using Ordinary Least Squares (OLS) estimation. These estimates are obtained by running regressions from the model as shown in Equation (4.3). The primary measure S_{ijk} is *Schooling,* which stands for the number of years of completed schooling. Table 4.1 provides summary statistics of all the outcome variables used in this regression. As shown in column 2, women in the sample desire roughly five children on average, they tend to have their first sexual encounter at the age of 14, approximately 24 percent of the women have used modern contraceptives before, while 19 percent have been tested for the AIDS virus in the past. Additional outcome variables were used to reflect preferences. The summary statistics reveal that almost 40 percent of women think that a husband is justified in hitting or beating his wife if she refuses to have sex; 60 percent of the women believe that a wife is justified in refusing to have sex with her husband if she is feeling tired or not in the mood; over half of the sample think that a wife is justified in refusing to have sex with a husband who she knows has a sexually-transmitted disease (STD) while almost 60 percent thinks that a wife is justified in asking that a condom be used when having sex with a husband having an STD. Summary statistics for these outcome variables are shown for both the treated group (FPE cohort) and the control group in columns 3 and 4, respectively, while the difference in the means of these two groups is shown in column 5.

Table 4.5 reports the results of the specification that includes the covariates, district fixed-effects and birth year fixed-effects. Similarly, the sample comprises the same two groups of women used in the previous estimation with the exception that all women who have never had a previous sexual encounter are excluded from the sample.[3] The OLS results in Table 4.5 show that schooling has an impact on almost all of the outcomes used in the model. In column 1, an additional year of schooling reduces the number of desired children by 0.06. Taken at the mean desired number of children, this represents a decline of about one percent. Put another way, a

[3] This was done because some questions, such as those pertaining to contraceptive use, are relevant to women who have had sexual intercourse at least once. For consistency purposes, I maintain the same sample throughout. The results do not change in other regressions when women who have never had sex are included in the sample.

woman with eight additional years of schooling will on average desire 0.5 fewer children. These results are similar to the lower range of the estimates obtained by Ainsworth *et al.* (1998) who study the fertility impact of education in several sub-Saharan African countries. In column 2, an additional year of schooling is associated with a delay in first sexual encounter by about 0.1 years or approximately one month. Also, women are more likely to use modern contraceptives (column 3) and to have taken a test for the virus that causes AIDS (column 4). The likelihood of choosing modern contraception increases by 1.6 percentage points while the probability of taking a test for the AIDS virus increases by 1.7 percentage points.

Schooling also has an impact on women's empowerment as revealed in their preferences. These preferences are reflected in the answers to questions about women's opinion on issues pertaining to the conditions under which a woman should have sex with her husband and whether wife beating is justified if the woman chooses not to have sexual intercourse with the husband. These results are shown in columns 5 to 8. Women with an additional year of schooling are likely to denounce wife beating as justification for not having sex (column 5) and they are more likely to believe that a woman is justified in refusing to have sex with her husband if she is tired or not in the mood (column 6). Although women with additional years of schooling are likely to think that a woman is justified in refusing to have sex with a husband with an STD, this result is not significant (column 7). However, women with an additional year of schooling believe that a wife is justified in asking that a condom be worn when having sex with her husband if he has an STD (column 8).

4.5.6. Impact of Schooling on Preferences: Instrumental Variables (IV) Estimates

While the OLS estimates can provide some indication of the association between schooling and the outcomes under consideration, proper identification of the effect of education on these outcomes is often problematic due to the endogeneity of schooling. If schooling is endogenous then the estimates obtained will be biased and inconsistent. One solution to address this potential problem is to use an instrumental variables strategy.

This analysis focuses on the use of instrumental variables to identify the causal impact of schooling on preferences. The instrumental variable, in order to be effective, must be correlated with schooling and must affect the outcomes only through years of schooling. Under the assumption that the FPE program only affected the amount of schooling received by individuals of a certain age, the interactions of birth year and the FPE inputs may be used as instruments for education. Specifically, I use the interaction term FPE cohort*FPE input as an instrument for years of schooling. This is the same interaction term used in Equation (4.2). Summary statistics for the FPE input variable are shown in Table 4.2.

The order of the specifications used and the included control variables are the same as those used in the OLS estimation. Individuals born between 1980 and 1985 comprise the reference or control group while the treatment group consists of women who are born between 1990 and 1993. Standard errors are clustered at the district and birth year level.

Table 4.6 presents the IV results after estimating Equation (4.3) assuming that schooling is endogenous. Similar to the OLS results obtained in Table 4.5, the IV estimates show that schooling has a significant impact on almost all of the outcomes under consideration. The F-statistics from the first–stage regressions confirm that the instruments are not weak (instrument validity). An additional year of schooling leads to a reduction in the number of children desired by 0.37 (column 1). The IV results are very close to those of Lam and Dureyea (1999) and Osili and Long (2008) using data for Brazil and Nigeria, respectively. Compared to other studies such as León (2004), the findings in this chapter are almost identical. León (2004) finds that an additional year of schooling for U.S. women between the ages of 40 and 49 years reduced completed fertility by 0.3.

With one more year of schooling, women delay the start of sexual activity by about 0.2 years or approximately two months and they are 5 percentage points more likely to have taken a test for the AIDS virus in the past. All of the outcome variables in columns 5 to 8 are significant and of the same sign as the results obtained in the OLS regressions. That is, more educated women are less likely to think wife beating is justified when a wife refuses to have sex; additional years of schooling will lead women to think a wife is justified in refusing to have sex if she is tired or not in the mood. Similarly, with more education, women are more in favor of a wife refusing to have sex with a husband who she knows has an STD; and if she decides to have sex with her infected partner then she is justified in requesting that a condom be used during sexual intercourse.

As an additional robust check, I ran the same regression from Equation (4.3), this time using the excluded group of women born 1986 to 1989 (or 12 to 15 years old in 2001) as the control group. This group is closer in age to the treatment group who were born 1990 to 1993 (or where 8 to 11 years in 2001). They were excluded from the base sample because of possible contamination caused by over-age. That is, some of these women (although older than the statutory primary school age) may have been in primary school and directly benefitted from the policy. The IV results are shown in Table C.3. All specifications are the same as those in Table 4.6. The high first-stage F-statistics indicate the instruments remain strong. The coefficients on

Table 4.5: The Impact of Schooling on Preferences – OLS Regressions

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variables							
	Desired number of children	Age at first sexual inter-course	Use modern contracep-tive	Tested for AIDS virus	Wife beating justified in refusing sex	Wife justified in refusing sex when tired	Wife justified in refusing sex with STD husband	Wife justified in asking to use a condom with STD husband
Schooling	-0.064***	0.108***	0.016***	0.017***	-0.015***	0.011***	0.004	0.010***
	(0.013)	(0.019)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Age	0.698	1.193**	0.064	-0.202*	0.132	-0.199	0.052	-0.152
	(0.619)	(0.556)	(0.267)	(0.119)	(0.164)	(0.230)	(0.161)	(0.169)
Age squared	-0.012	-0.021*	-0.002	0.004	-0.003	0.003	-0.002	0.003
	(0.013)	(0.012)	(0.006)	(0.002)	(0.004)	(0.005)	(0.003)	(0.003)
Employed	0.242**	-0.540***	0.016	0.028	0.052**	-0.017	-0.028	-0.012
	(0.095)	(0.135)	(0.038)	(0.020)	(0.024)	(0.032)	(0.036)	(0.035)
Wealth	-0.133**	-0.032	0.022**	0.023*	-0.026*	0.008	0.005	0.022
	(0.056)	(0.060)	(0.011)	(0.012)	(0.014)	(0.015)	(0.013)	(0.013)
Married	0.315***	-0.394**	-0.120***	0.090**	0.043	-0.072**	-0.106***	-0.074**
	(0.078)	(0.168)	(0.032)	(0.039)	(0.034)	(0.036)	(0.038)	(0.036)
Radio	-0.029	0.154	-0.020	-0.003	0.028	0.042*	0.020	-0.016
	(0.129)	(0.168)	(0.020)	(0.027)	(0.027)	(0.024)	(0.024)	(0.031)
Fridge	-0.214	0.331	0.064	0.048	-0.009	0.023	0.070	0.110**
	(0.205)	(0.222)	(0.061)	(0.047)	(0.039)	(0.060)	(0.058)	(0.051)
TV	0.069	0.096	0.098	0.007	-0.069*	0.028	-0.033	-0.031
	(0.276)	(0.104)	(0.071)	(0.064)	(0.040)	(0.080)	(0.077)	(0.047)
Christian	-0.846**	-0.196	0.068	0.044	-0.195	-0.034	-0.036	-0.180
	(0.417)	(0.382)	(0.062)	(0.050)	(0.138)	(0.097)	(0.143)	(0.125)
Islam	-0.509	-0.239	0.026	-0.019	-0.157	-0.008	-0.045	-0.220*
	(0.417)	(0.363)	(0.056)	(0.045)	(0.137)	(0.092)	(0.139)	(0.123)
Temne	0.010	-0.401***	-0.016	-0.034	0.074**	-0.017	0.051	-0.025
	(0.097)	(0.134)	(0.050)	(0.037)	(0.032)	(0.030)	(0.038)	(0.031)
Mende	-0.101	0.021	-0.098***	0.077*	-0.028	-0.124**	-0.104	-0.022
	(0.146)	(0.191)	(0.027)	(0.046)	(0.036)	(0.050)	(0.065)	(0.042)
Urban	-0.176	-0.019	0.015	0.016	-0.001	-0.043	0.014	-0.020
	(0.116)	(0.165)	(0.036)	(0.023)	(0.038)	(0.033)	(0.032)	(0.039)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,251	2,058	2,251	2,205	2,246	2,216	2,232	2,239
R-square	0.243	0.238	0.248	0.222	0.137	0.096	0.072	0.106

Notes: Standard errors in parentheses are clustered at the year*district level. Statistical levels of significance are: * indicates $p<0.1$, ** indicates $p<0.05$, *** indicates $p<0.01$. The sample comprises the FPE cohort (those born during the period 1990 to 1993) and those females born during the period 1980 to 1985, but only those who have had sex in the past. The model also includes a variable for female-male primary school enrollment ratio in 2004 in the district. This variable is also interacted with "FPE Cohort" which equals one for individuals born 1990-1993 and zero otherwise. Regressions are weighted by female population*number of teachers. Constant term is not reported.

Table 4.6: The Impact of Schooling on Preferences – Instrumental Variables Regression

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variables							
	Desired number of children	Age at first sexual inter-course	Use modern contracep-tive	Tested for AIDS virus	Wife beating justified in refusing sex	Wife justified in refusing sex when tired	Wife justified in refusing sex with STD husband	Wife justified in asking to use a condom with STD husband
Schooling	-0.370***	0.203*	0.048	0.049**	-0.111**	0.090**	0.114**	0.081**
	(0.118)	(0.118)	(0.041)	(0.023)	(0.047)	(0.039)	(0.045)	(0.037)
Age	0.668	1.188**	0.067	-0.211*	0.128	-0.190	0.087	-0.130
	(0.646)	(0.578)	(0.239)	(0.126)	(0.177)	(0.251)	(0.187)	(0.163)
Age squared	-0.018	-0.019	-0.001	0.005	-0.005	0.004	-0.000	0.004
	(0.014)	(0.012)	(0.005)	(0.003)	(0.004)	(0.005)	(0.004)	(0.004)
Employed	-0.048	-0.457***	0.046	0.060**	-0.043	0.064	0.084	0.058
	(0.161)	(0.165)	(0.055)	(0.030)	(0.049)	(0.055)	(0.053)	(0.051)
Wealth	-0.018	-0.068	0.010	0.009	0.011	-0.022	-0.038*	-0.006
	(0.071)	(0.071)	(0.019)	(0.017)	(0.022)	(0.021)	(0.020)	(0.020)
Married	-0.423	-0.139	-0.042	0.164***	-0.188	0.124	0.166	0.102
	(0.308)	(0.338)	(0.103)	(0.061)	(0.117)	(0.101)	(0.121)	(0.103)
Radio	0.069	0.130	-0.031	-0.012	0.061	0.017	-0.017	-0.039
	(0.145)	(0.163)	(0.025)	(0.033)	(0.038)	(0.040)	(0.047)	(0.040)
Fridge	0.320	0.172	0.008	-0.008	0.165*	-0.106	-0.124	-0.018
	(0.318)	(0.334)	(0.090)	(0.060)	(0.099)	(0.089)	(0.115)	(0.095)
TV	0.559	-0.070	0.046	-0.048	0.083	-0.103	-0.201*	-0.143
	(0.342)	(0.222)	(0.106)	(0.080)	(0.098)	(0.115)	(0.118)	(0.090)
Christian	-0.101	-0.419	-0.010	-0.035	0.040	-0.224	-0.302	-0.353**
	(0.560)	(0.487)	(0.108)	(0.083)	(0.175)	(0.154)	(0.195)	(0.154)
Islam	-0.274	-0.313	0.001	-0.043	-0.083	-0.070	-0.131	-0.275**
	(0.455)	(0.376)	(0.054)	(0.051)	(0.143)	(0.104)	(0.153)	(0.123)
Temne	-0.115	-0.344**	-0.003	-0.021	0.034	0.016	0.097	0.007
	(0.155)	(0.157)	(0.053)	(0.034)	(0.053)	(0.050)	(0.060)	(0.039)
Mende	0.019	-0.009	-0.110***	0.064	0.012	-0.145***	-0.141**	-0.045
	(0.194)	(0.195)	(0.036)	(0.046)	(0.047)	(0.047)	(0.061)	(0.045)
Urban	0.107	-0.110	-0.015	-0.011	0.087	-0.117**	-0.085	-0.088*
	(0.194)	(0.192)	(0.045)	(0.028)	(0.063)	(0.054)	(0.069)	(0.053)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First stage (F-stat.)	15.31	18.92	15.31	14.33	15.22	16.25	15.46	16.16
Observations	2251	2058	2251	2205	2246	2216	2232	2239

Notes: Standard errors in parentheses are clustered at the year*district level. Statistical levels of significance are: * indicates p<0.1, ** indicates p<0.05, *** indicates p<0.01. The sample comprises the FPE cohort (those born during the period 1990 to 1993) and those females born during the period 1980 to 1985, but only those who have had sex in the past. The model also includes a variable for female-male primary school enrollment ratio in 2004 in the district. This variable is also interacted with "FPE Cohort" which equals one for individuals born 1990-1993 and zero otherwise. Instrument: interaction of FPE cohort and FPE input, where FPE input is calculated as the log of funding allocated for primary school education for every 100 teacher in 2004. Regressions are weighted by female population*number of teachers. Constant term is not reported.

the schooling variable for the most part are significant and all coefficients carry the same sign as those in Table 4.6. As expected, the magnitudes of the coefficients are lower when this group of women is used as the control group. This result may be evidence that some of these women may have directly benefitted from the FPE policy even though they were over-aged.

4.6. Conclusion

The benefit of education on private returns is well documented in the empirical literature with a range from 6 to 11 percent (Duflo, 2001). However, the impact of education on market outcomes explains only a fraction of the story. Education provides various social benefits. One of the ways it can do so is to alter preferences among individuals. By altering preferences, education may affect both private and social returns. If there are positive returns to education brought about by changing preferences, then private returns underestimate the value of education without accounting for social returns.

In order to show how schooling alters preferences, one must show that more schooling causes changes in preferences. This is because causality can run in the opposite direction. More future-oriented individuals (that is, those having increased preferences for future benefits) are more likely to increase their schooling. To address this, it requires exogenous variation in factors that are highly correlated with schooling. This study took advantage of the nature of the Free Primary Education (FPE) policy in Sierra Leone that led to a differential impact on education for children of primary school age due to year of birth and the level of education investments. I began by presenting evidence that this policy did increase the number of years of schooling of women receiving treatment by about 0.84 years. I then use the exogenous variation in both an individual's year of birth and the amount of education funding to identify the impact of schooling on preferences. This identification strategy works because both year of birth and education funding affect individual level of schooling. Both OLS and IV estimates produce similar conclusions about the impact of schooling on preferences.

I estimate that an additional year of schooling decreases a woman's desire for children. Using the IV results, eight additional years of schooling will lead to women demanding one fewer child. This preference for fewer children can be attributable to women maintaining their virginity for longer periods as well as an increase in the use of modern contraceptives. Through education, women have become empowered and this empowerment is revealed in their preferences. These preferences are reflected in women's beliefs that support the rights of women to decide under what conditions to have sexual relations with their husbands.

Given the consistency of the findings which compare favorably with other studies, I conclude that the estimated effects of education are robust to unobserved characteristics of the women in the sample or unobserved policies that may have been occurring at the time and that affect both preferences and schooling. Education alters preferences. To the extent that change in preferences may impact both private and social returns to schooling, I argue that private returns underestimate the value of education without accounting for the effect on other outcomes.

These findings are important for economic policy as it pertains to education and women's issues. The FPE policy achieved its objective of increasing the quantity of schooling. Policies of this type that set out to increase schooling may suffer unintended consequences such as a decline in education quality. This does not appear to be the case because by increasing education, women have become empowered as revealed in their preferences.

Beyond the effectiveness of this policy on these women, there is a possibility that the FPE also had large secondary effects on the Sierra Leone economy. If women are better able to control their fertility by say, increased modern contraceptive use and delaying the start of sexual encounters, then with increased education, women are also enhancing their stock of human capital. Consequently, one might expect labor market outcomes for women to improve. The children born to these women also stand to benefit. Parental education is considered a key factor in influencing the outcomes of children (Haveman and Wolfe, 1995). Therefore, the impact of education on the labor market experience of women and the impact of these women's education on their children's outcomes represent important areas of future research.

CHAPTER 5. SUMMARY AND CONCLUSIONS

This dissertation is a compendium of essays analyzing the role of public policy in affecting such outcomes as fertility, educational attainment and women's preferences towards fertility and sexual activity.

In the first essay, I present evidence supporting the influence of a federal government-sponsored state abstinence education (SAE) program. In an era of heightened awareness of the high teen birth rates in the U.S., an intense debate has centered on the effectiveness of various policy approaches to reducing early childbearing. One view among researchers is that policies promoting abstinence and improved contraceptive practices can be influential in curtailing teen births. Specifically, I estimate the impact of SAE funding from the federal government Title V, Section 510 appropriations on the birth rates for teens 15-17 years in the U.S. over the period 1991-2005. Using difference-in-differences methodology, the results suggest that Title V SAE has led to a decline in birth rates for the targeted group of female minors. I further find that the SAE program has a consistently significant impact among Whites but not Blacks.

This next essay examines the impact on fertility outcomes of a policy - the Family and Medical Leave Act (FMLA) - that allows women to proceed on job-protected maternity leave. Because the FMLA has clearly-defined eligibility criteria and empirical evidence suggests that it may have been associated with an increase in work leave taken, it is possible to identify the impact on fertility by comparing the outcome among eligible women with the fertility outcomes of those who are not eligible for FMLA. I find that the implementation of the FMLA has resulted in eligible women increasing their probabilities of giving birth to a first and second child. The magnitude of the effects appears larger for a first birth. The results of the analysis also show that eligible women are giving birth to children much earlier. I also consider the impact of the policy across sectors, race and ethnicity, and education level. These results indicate that the FMLA is more effective in non-government sectors. There is no difference in the responsiveness to birth among Blacks and Hispanics. Meanwhile, among Whites, eligible females have significantly higher probabilities of giving a first or second birth. Eligible women with at least some education experience at the college level are more likely to give a first or second birth since the FMLA.

This research offers some insights into family friendly policies in the U.S. There are a number of potential implications arising from such a policy. First, the FMLA can help to address the balance between women's work and family life. Second, it can help to address the length of delay in childbearing among career oriented women. Finally, there are also implications for the retention of women in the labor force given that educated women may opt out of paid employment after having children.

The final essay takes advantage of the nature of the Free Primary Education (FPE) policy in Sierra Leone that led to a differential impact on education for children of primary school age due to year of birth and the level of education investments. I began by presenting evidence that this policy did increase the number of years of schooling of women receiving treatment by about 0.84 years. I then use the exogenous variation in both an individual's year of birth and the amount of education funding to identify the impact of schooling on preferences. This identification strategy works because both year of birth and education funding affect individual level of schooling. Both OLS and IV estimates produce similar conclusions about the impact of schooling on preferences.

I estimate that an additional year of schooling decreases a woman's desire for children. Using the IV results, eight additional years of schooling will lead to women demanding one fewer child. This preference for fewer children can be attributable to women maintaining their virginity for longer periods as well as an increase in the use of modern contraceptives. Through education, women have become empowered and this empowerment is revealed in their preferences.

The above research has provided a platform for an interesting research agenda. Moving forward, I plan to extend my work in the following areas:

i) Using individual-level data over some sufficiently long time period, I want to analyze the impact of state-abstinence education programs on other outcomes such as abortions. Data at this disaggregated level will allow for a detailed analysis of the SAE programs on fertility and other related outcomes in places where these programs are specifically implemented. This may include data at the state, city or even school-level.

ii) In addition to the effectiveness of the FMLA policy in influencing child preferences, by allowing eligible women the right to return to their former jobs after giving birth, the FMLA has effectively improved the labor outcomes of new parents (Waldfogel, 1999). Beyond these, the findings raise the possibility that the policy may have improved other outcomes not yet explored. For instance, the long-term educational attainment and health outcomes of children born to FMLA-eligible women represent important areas for future research. Using the NLSY, specifically the NLSY79 Children and Young Adults, it is possible to link these eligible women directly to their children. Since 1988, the NLSY has collected information on children born to the 1979 women cohort. These children were age 10 and older. There is a variety of outcomes of interests such as information such as schooling, health and fertility. Berger *et al*. (2005) find that women taking less than the maximum allowable leave period under the FMLA have children with worse health outcomes, attributable in part to the lower levels of breastfeeding and immunizations which are short-term outcomes.

iii) Beyond the effectiveness of the free primary education (FPE) policy on these women, there is a possibility that the FPE policy also had large secondary effects on the Sierra Leone economy. If women are better able to control their fertility by say increased modern contraceptive use and delaying the start of sexual encounters, then with increased education, women are also enhancing their stock of human capital. Consequently, one might expect labor market outcomes for women to improve. The children born to these women also stand to benefit. Parental education is considered a key factor in influencing the outcomes of children (Haveman and Wolfe, 1995). Therefore, the impact of education on the labor market experience of women and the impact of these women's education on their children's outcomes represent important areas of future research.

As the availability of data increases, impact evaluation programs have become more prominent in research. I am interested in assessing the impact of various policy interventions on a variety of outcomes that seek to improve the lives of people, in particular those in poorer developing countries. I am planning to study the effects of a conditional cash transfer program in Jamaica. Specifically, I want to investigate whether the PATH program in Jamaica had an impact on the education of the program's recipients and if there was an impact did it ultimately affect criminal behavior among these vulnerable groups of the population.

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APPENDIX A: SUPPLEMENTARY TABLES FOR CHAPTER 2

Table A.1: Variable Definitions and Sources

Variable	Definition	Source(s)
BIRTH RATES	**Dependent variable.** The birth rates for females aged 15-17 and 25-29 years per 1,000 age-specific females in the population. Data were also obtained for other age categories and also by race and ethnicity.	Data on births come from the Centers for Disease Control and Prevention. National Center for Health Statistics. Vital Stats. http://www.cdc.gov/nchs/vitalstats.htm. [Accessed March 27, 2009]. Population data come from Population Division, U.S. Census Bureau.
SAE per capita	State Abstinence Education funding as per Title V, Section 510. It includes each state's contribution of three dollars for every four dollars in grants received. Amount is in real dollars per capita. That is, I deflate the nominal amount by an appropriate consumer price index.	The U.S. Department of Health and Human Services (HHS) Administration for Children and Families (ACF).
Black	The proportion of population of people who are black.	Population Division, U.S. Census Bureau.
Hispanic	The proportion of population who are Hispanic.	Population Division, U.S. Census Bureau.
Rape	Rape rate: rapes per 100,000 of population.	FBI, Uniform Crime Reports.
Robbery	Robbery rate: robberies per 100,000 of population.	FBI, Uniform Crime Reports.
Prison	Prison (incarceration) rate: the number of persons in prison per 100,000 of population.	Bureau of Justice Statistics Programs online: http://www.ojp.usdoj.gov/bjs/prisons.htm (Accessed March 24, 2008).
Unemployment rate	Unemployment rate: percent of persons unemployed. Figures are seasonally-adjusted. I use the rate for December of each year.	U.S. Department of Labor: http://www.dol.gov (Accessed March 24, 2008); Bureau of Labor Statistics: http://data.bls.gov/map/servlet/map.servlet.MapToolServlet?datatype=12_month_net&year=1990&period=M01&survey=la&map=state&seasonal=s (Accessed March 24, 2008)
Urbanization	Proportion of total population living in urban areas.	Data provided by Naci Mocan.

(Table A.1 continued)

Variable	Definition	Source(s)
Poverty	The percentage of the population below the poverty level. This is the poverty status of children under 18 years of age by state. Poverty status is based on the ratio of total income to threshold income for a family of a certain size and number of members. The figure comes from the variable POV46. I use 1990 data to represent 1991. Data for 1997 and 1998 are based on three-year moving averages, 1996-1998 and 1997-1999, respectively. Data for 2002-2005 shows the percent of the people below poverty level in the past 12 months (for whom poverty status is determined).	Data for 1991 come from the National Center for Education Statistics: http://nces.ed.gov/programs/digest/d96/d96t019.asp (Accessed March 24, 2008). Data for 1992-1994 comes from http://tiss.zdv.uni-tuebingen.de/webroot/sp/spsba01_W98_1/usa4.htm#table3 (Accessed March 24, 2008). Data for 2000-2005 comes from the Census Bureau: http://www.census.gov/acs/www/Products/Ranking/2000/R01T040.htm and http://pubdb3.census.gov/macro/032007/pov/toc.htm (Accessed March 24, 2008).
HIGH SCHOOL GRADUATION	Percentage of students graduating from high school. It is based on an elaborate formula called the cumulative promotion index developed by Swanson (2004).	Education Counts Database (http://www.edweek.org/) Data for 1993-2004 - http://www.edcounts.org/createtable/viewtable.php ; http://www.manhattan-institute.org/html/ewp_08.htm . All data accessed March 24, 2008.
NOTIFICATION LAW	Dummy variable equals one for states with an enforced abortion notification or parental consent for abortion law (includes informed consent laws), zero otherwise.	• Guttmacher Report on Public Policy (various issues); Sexual and Reproductive Health Issues in States (various issues) from Guttmacher Institute. www.guttmacher.org (Accessed March 7, 2009) • "Who Decides? A State-by-State Report on the Status of Women's Reproductive Rights" found on the NARAL Pro-Choice America's website: http://www.ProChoiceAmerica.org. (Accessed March 7, 2009).
ABORTION MEDICAID	Dummy variable equals one in states where Medicaid pays for therapeutic abortion, zero otherwise.	• Guttmacher Report on Public Policy (various issues); Sexual and Reproductive Health Issues in States (various issues) from Guttmacher Institute. www.guttmacher.org (Accessed March 7, 2009) • "Who Decides? A State-by-State Report on the Status of Women's Reproductive Rights" found on the NARAL Pro-Choice America's website: http://www.ProChoiceAmerica.org. (Accessed March 7, 2009).

Table A.2: Difference-in-Differences: Outcomes of Change in Birth Rates: Control Group: Birth rates for females 25-29 years

Variable	(1) All	(2) Blacks	(3) Hispanics	(4) Whites
	Dependent Variable: ΔBirth rates			
Panel 1: Multi-way clustering approach				
ΔSAE Funding *Young	-4.349***	-9.719***	6.033	-4.351**
	(1.491)	(3.459)	(6.295)	(1.830)
Observations	1372	1372	1370	1372
R-square	0.21	0.04	0.21	0.13
Panel 2: SAE Funding variable lagged once				
ΔSAE Funding *Young	-4.734***	-10.551*	8.601*	-4.770***
	(1.183)	(5.704)	(4.772)	(1.168)
ΔSAE Funding *Young (lagged once)	-3.914***	-7.095	15.570***	-3.936**
	(1.027)	(7.201)	(5.725)	(1.213)
Observations	1274	1274	1273	1274
R-square	0.22	0.04	0.23	0.14
Panel 3: Include once-lagged dependent variable				
ΔSAE Funding *Young	-4.315***	-6.189	-15.759***	-4.342***
	(1.208)	(4.575)	(5.286)	(1.214)
ΔBirth rates (lagged once)	-0.011	-0.519***	-0.254***	-0.016
	(0.068)	(0.030)	(0.053)	(0.048)
Observations	1274	1274	1272	1274
R-square	0.21	0.30	0.27	0.13
State fixed-effects?	Yes	Yes	Yes	Yes
Year fixed-effects?	Yes	Yes	Yes	Yes
Additional Covariates?	Yes	Yes	Yes	Yes
Mean of ΔSAE	0.018	0.018	0.018	0.018

Notes: Standard errors, reported in parentheses, are robust and adjusted for multi-way clustering at the year and state level (Panel 1) and adjusted for clustering at the state level (Panels 2 and 3). Statistical levels of significance are as follows: * means p<0.1, ** means p<0.05, *** means p<0.01. The real SAE funding variable is deflated by the total population to obtain per capita term. Data for Hispanics include all persons of Hispanic origin of any race.

Table A.3: Difference-in-Differences: Outcomes of Change in Birth Rates:
Control Group: Birth rates for females 25-29 years (Sample size modification)

Variable	(1) All	(2) Blacks	(3) Hispanics	(4) Whites
	Dependent Variable: ΔBirth rates			
Panel 1: Sample excludes years 1996, 1997 and 2002				
ΔSAE Funding *Young	-4.688***	-9.427	0.783	-4.617***
	(1.270)	(6.034)	(4.888)	(1.253)
Observations	1078	1078	1076	1078
R-square	0.21	0.05	0.11	0.13
Panel 2: California included in sample				
ΔSAE Funding *Young	-4.360***	-9.740	6.027	-4.348***
	(1.237)	(5.845)	(4.775)	(1.220)
Observations	1400	1400	1398	1400
R-square	0.21	0.04	0.21	0.13
Panel 3: Sample excludes the state with the highest and lowest real State Abstinence Education (SAE) funding per capita				
ΔSAE Funding *Young	-4.189**	-9.379	4.980	-4.465**
	(1.337)	(6.365)	(4.908)	(1.329)
Observations	1316	1316	1316	1316
R-square	0.21	0.04	0.22	0.12
Panel 4: Sample period from 1995 to 2000				
ΔSAE Funding *Young	-2.616*	-8.617	16.417***	-2.695**
	(1.314)	(6.998)	(5.533)	(1.213)
Observations	588	588	588	588
R-square	0.34	0.05	0.31	0.31
State fixed-effects?	Yes	Yes	Yes	Yes
Year fixed-effects?	Yes	Yes	Yes	Yes
Additional Covariates?	Yes	Yes	Yes	Yes

Notes: Standard errors, reported in parentheses, are robust and adjusted for clustering at the state level. Statistical levels of significance are as follows: * means p<0.1, ** means p<0.05, *** means p<0.01. The real SAE funding variable is deflated by the total population to obtain per capita term. Data for Hispanics include all persons of Hispanic origin of any race.

Table A.4: Difference-in-Differences: Outcomes of Change in Birth Rates (Changes in the Control Group)

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Blacks	Hispanics	Whites	All	Blacks	Hispanics	Whites
	Dependent Variable: ΔBirth rates (Control group: females 18-19 years)				Dependent Variable: ΔBirth rates (Control group: females 30-34 years)			
ΔSAE Funding *Young	-3.078**	-12.756	1.663	-2.652***	-1.440*	-8.181	5.354	-1.884**
	(1.270)	(10.394)	(7.764)	(0.949)	(0.853)	(7.497)	(4.836)	(0.898)
Observations	784	784	784	784	1372	1370	1370	1372
R-square	0.18	0.07	0.08	0.16	0.42	0.06	0.26	0.33
State fixed-effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Covariates?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of ΔSAE	0.044	0.044	0.044	0.044	0.018	0.018	0.018	0.018

Note: See notes to previous table

Table A.5: Difference-in-Differences: Outcomes of Change in Birth Rates (Multiple Control Groups)

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Blacks	Hispanics	Whites	All	Blacks	Hispanics	Whites
	Dependent Variable: ΔBirth rates (Multiple Control groups: females 18-19 and 25-29 years)				Dependent Variable: ΔBirth rates (Multiple Control groups: females 25-29 and 30-34 years)			
ΔSAE Funding *Young	-4.523***	-13.110*	5.563	-4.331***	-2.895***	-8.912**	5.687	-3.118***
	(1.203)	(7.312)	(4.608)	(0.825)	(0.921)	(4.252)	(4.036)	(0.930)
Observations	1176	1176	1174	1176	2058	2056	2054	2058
R-square	0.15	0.04	0.10	0.12	0.26	0.04	0.31	0.18
State fixed-effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Covariates?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of ΔSAE	0.018	0.018	0.018	0.018	0.018	0.018	0.018	0.018

Note: See notes to previous table

Table A.6: Difference-in-Differences: Outcomes of Change in Birth Rates (Allowing covariates to affect age groups differently)

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Blacks	Hispanics	Whites	All	Blacks	Hispanics	Whites
	Dependent Variable: ΔBirth rates (Control group: females 18-19 years)				Dependent Variable: ΔBirth rates (Control group: females 25-29 years)			
ΔSAE Funding *Young	-3.035**	-14.922	2.134	-2.251*	-5.017***	-11.045*	6.335	-4.766***
	(1.472)	(9.876)	(7.926)	(1.148)	(1.422)	(6.451)	(5.124)	(1.532)
Observations	784	784	784	784	1372	1372	1370	1372
R-square	0.22	0.08	0.09	0.18	0.25	0.05	0.22	0.16
State fixed-effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Covariates?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age*other covariates interactions?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of ΔSAE	0.044	0.044	0.044	0.044	0.018	0.018	0.018	0.018

Note: See notes to previous table

APPENDIX B: SUPPLEMENTARY TABLES FOR CHAPTER 3

Table B.1:Description of Variables

Variables	Variable Definition
Outcome variables	
First birth	Equals one if first child is born on or after 1989 and zero otherwise
Second birth	Equals one if second child is born on or after 1989 and zero otherwise
Dummy explanatory variables	
FMLA	Equals in a year the FMLA is in effect, zero otherwise
Eligible	Equals one if individual satisfies the eligibility criteria for obtaining benefits under the FMLA, zero otherwise
Married	Equals one if married, zero otherwise
Black	Equals one if Black, zero otherwise
Hispanic	Equals one if Hispanic, zero otherwise
Union	Equals one if part of a union in primary job, zero otherwise
Urban	Equals one if current residence is urban, zero otherwise
Gender role1	Equals one if respondent thinks a woman's place is in the home, zero otherwise
Gender role2	Equals one if respondent thinks men should share housework, zero otherwise
Gender role3	Equals one if respondent thinks women should perform traditional roles, zero otherwise
Continuous explanatory variables	
Age	Age in years
Education	Years of schooling (Highest grade completed)
Family income	Total net family income in thousands of dollars
Parity	Number of children ever born
AFQT score	Armed Forces Qualifying Test (AFQT) score in percentile
Job tenure	Total tenure (in weeks) with employer at primary job
Other variables	
High school dropout	Equals one if highest grade completed is less than 12, zero otherwise
High school graduate	Equals one if highest grade completed is 12, zero otherwise
Some college	Equals one if highest grade completed ranges from 13 and 15, zero otherwise
College graduate	Equals one if highest grade completed is at least 16, zero otherwise
Employed	Equals one if respondent is currently employed, zero otherwise
Government sector	Equals one if employed with the state or federal government, zero otherwise
Private sector	Equals one if employed with a private-owned organization, zero otherwise
MLS states	Equals one if state is had a job-protected Maternity Leave statute (MLS) prior to the FMLA, zero otherwise
Employees	Number of employees at respondent's current job location
Hours work	Number of hours worked in the past calendar year
Number of children desired	Number of children desired
Maternity leave	Equals one if employer offers job-protected maternity leave, zero otherwise
Covered	Equals one if individuals works in a covered institution, zero otherwise

Table B.2: Some Calculations

Panel A: Predicted Probabilities of a birth (Eligible Women)

	(1)	(2)	(3)=(1)+(2)	(4)=(2)/(1)*100
	Eligible Women			
	Before FMLA (Baseline)	Marginal effect	After FMLA	% increase
First birth	25.96	5.97	31.87	23.05
Second birth	28.00	4.18	32.18	14.93
Simple Average	**26.98**	**5.08**	**32.03**	**18.99**

Notes: Probabilities were calculated using the coefficients presented in column 2 of Table 3 (first birth) and column 2 of Table 5 (second birth). The sample period is from 1989 to 2006. Average age of the eligible women at the baseline probability is about 30 years. All evaluations are done at the sample means.

Panel B: Implied Probabilities of any birth for all working women using estimates from Averett and Whittington (2001)

	(1)	(2)	(3)=(1)+(2)	(4)=(2)/(1)*100
	Working women			
	No maternity leave (Baseline)	Marginal effect	Maternity Leave	% increase
A child	36.9	3.25	40.15	8.81

Notes: The study by Averett and Whittington examines the impact of maternity leave on the probability of having a child during the sample period 1985 to 1992 using the NLSY79. Average age of women at baseline is 27 years. The baseline probability is obtained by dividing the number of births over the sample period divided by the number of women in the sample. That is, 1325/3590=0.369 or 36.9 percent. The marginal effect is the average of the marginal effects in models 1 and 2 of Table 3.

Table B.3: Predicted response time (years) before getting a birth (Eligible Women)

	(1)	(2)	(3)=(1)+(2)
	Eligible Women		
	Before FMLA (Baseline)	Marginal effect Units (%)	After FMLA
First birth	2.32	-0.88 (-37.8%)	1.44
Second birth	2.37	-0.50 (-21.1%)	1.87
Simple Average	**2.35**	**-0.69**	**1.65**

Notes: Probabilities were calculated using the coefficients presented in columns 1 and 2 of Table 8 for a first birth and second birth, respectively. All evaluations are done at the sample means.

APPENDIX C: SUPPLEMENTARY TABLES FOR CHAPTER 4

Table C.1: The Impact of Treatment and Exposure on Schooling - OLS Regressions (Different Control Group: Individuals born 1986-1989 or 12-15 years in 2001)

Variables	(1)	(2)	(3)
	Dependent variable: Years of schooling		
FPE Cohort	1.520	1.480	
	(0.990)	(1.003)	
FPE Input	-0.704***	-0.352*	-0.429
	(0.185)	(0.195)	(0.273)
FPE Cohort * FPE input	0.792***	0.731***	0.734***
	(0.208)	(0.183)	(0.221)
Age	1.355	1.371	2.635
	(1.016)	(1.022)	(2.828)
Age squared	-0.032	-0.032	-0.089
	(0.030)	(0.030)	(0.083)
Employed	-1.435***	-1.419***	-1.418***
	(0.149)	(0.150)	(0.145)
Wealth	0.237**	0.235**	0.234**
	(0.113)	(0.119)	(0.116)
Married	-3.779***	-3.801***	-3.807***
	(0.533)	(0.535)	(0.545)
Radio	0.113	0.076	0.088
	(0.133)	(0.133)	(0.152)
Fridge	0.336	0.352	0.438
	(0.459)	(0.467)	(0.425)
TV	1.067**	1.074**	0.976**
	(0.459)	(0.461)	(0.413)
Christian	2.072*	1.989*	1.966*
	(1.098)	(1.083)	(1.076)
Islam	1.571	1.532	1.490
	(0.996)	(0.968)	(0.954)
Temne	0.044	-0.105	0.004
	(0.252)	(0.297)	(0.286)
Mende	-0.217	-0.209	-0.207
	(0.201)	(0.224)	(0.257)
Urban	1.128***	1.229***	1.132***
	(0.272)	(0.286)	(0.283)
District fixed-effects	No	Yes	Yes
Year of birth fixed-effects	No	No	Yes
Observations	1,974	1,974	1,974
R-square	0.46	0.46	0.47

Notes: Standard errors in parentheses are clustered at the year*district level. Statistical levels of significance are: * indicates p<0.1, ** indicates p<0.05, *** indicates p<0.01. The sample comprises the FPE cohort (those born during the period 1990 to 1993) and those females born during the period 1986 to 1989. The model also includes a variable for female-male primary school enrollment ratio in 2004 in the district. This variable is also interacted with "FPE Cohort" which equals one for individuals born 1990-1993 and zero otherwise. FPE input is calculated as the log of funding allocated for primary school education for every 100 teacher in 2004. The dummy variable "FPE Cohort" which equals one for individuals born 1990-1993 is not included in some specifications because the interpretation changes when birth-year fixed-effects are included in the regressions. Regressions are weighted by female population*number of teachers. Constant term is not reported.

Table C.2: The Impact of Treatment and Exposure on Schooling – OLS Regressions (Robustness Check)

Variables	(1)	(2)	(3)
	Dependent variable: Years of schooling		
	Sample of non-movers (Base sample)	Sample of non-movers (Alternate sample)	False treatment
FPE Cohort*FPE input	0.676**	1.179***	-0.177
	(0.318)	(0.267)	(0.159)
Control variables	Yes	Yes	Yes
District fixed-effects	Yes	Yes	Yes
Birth year fixed-effects	Yes	Yes	Yes
Observations	1,207	915	3,107
R-square	0.53	0.55	0.39

Notes: Standard errors in parentheses are clustered at the year*district level. Statistical levels of significance are: ** indicates p<0.05, *** indicates p<0.01. The sample in column (1) comprises the FPE cohort (those born during the period 1990 to 1993) and those females born during the period 1980 to 1985, but only those who have never moved residence at the time they were interviewed. The sample in column (2) comprises the FPE cohort (those born during the period 1990 to 1993) and those females born during the period 1986 to 1989, but only those who have never moved residence at the time they were interviewed. The sample in column (3) comprises the "False treatment" (those females born during the period 1968 to 1973) and those females born during the period 1980 to 1985, but only those who have never moved residence at the time they were interviewed. The model also includes a variable for female-male primary school enrollment ratio in 2004 in the district. This variable is also interacted with "FPE Cohort" which equals one for individuals born 1990-1993 and zero otherwise. FPE input is calculated as the log of funding allocated for primary school education for every 100 teacher in 2004. Column 1 and 2 regressions are weighted by female population*number of teachers. Constant term is not reported.

Table C.3: The Impact of Schooling on Preferences – Instrumental Variables Regression (Different Control Group: Individuals born 1986-1989 or 12-15 years in 2001)

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variables							
	Desired number of children	Age at first sexual inter-course	Use modern contracep-tive	Tested for AIDS virus	Wife beating justified in refusing sex	Wife justified in refusing sex when tired	Wife justified in refusing sex with STD husband	Wife justified in asking to use a condom with STD husband
Schooling	-0.295***	0.109	0.029	0.065***	-0.003	0.038	0.080**	0.108***
	(0.111)	(0.112)	(0.036)	(0.024)	(0.036)	(0.033)	(0.039)	(0.040)
Age	1.305	1.746	-0.080	-0.390	0.345	-0.473	-0.498	-0.629
	(1.813)	(1.252)	(0.434)	(0.397)	(0.363)	(0.431)	(0.526)	(0.412)
Age squared	-0.038	-0.039	0.001	0.011	-0.009	0.011	0.015	0.018
	(0.050)	(0.034)	(0.011)	(0.011)	(0.010)	(0.011)	(0.015)	(0.011)
Employed	-0.132	-0.287	0.083	0.147**	0.037	0.066	0.125*	0.160**
	(0.190)	(0.220)	(0.068)	(0.058)	(0.065)	(0.066)	(0.076)	(0.072)
Wealth	-0.095	-0.114	0.002	0.017	-0.005	0.020	0.023	0.004
	(0.073)	(0.091)	(0.014)	(0.016)	(0.018)	(0.018)	(0.024)	(0.021)
Married	-0.400	-0.154	-0.045	0.324***	0.139	0.061	0.228	0.258
	(0.487)	(0.476)	(0.135)	(0.112)	(0.138)	(0.122)	(0.155)	(0.162)
Radio	-0.102	0.146	0.036	-0.056**	-0.036	0.108***	0.020	0.002
	(0.174)	(0.157)	(0.029)	(0.028)	(0.038)	(0.038)	(0.038)	(0.038)
Fridge	0.174	0.448***	0.063*	0.068	0.002	-0.078*	0.118*	0.147*
	(0.195)	(0.141)	(0.035)	(0.049)	(0.046)	(0.046)	(0.066)	(0.080)
TV	0.042	-0.190	0.150**	-0.199***	-0.130**	0.090	-0.117	-0.157*
	(0.281)	(0.178)	(0.071)	(0.062)	(0.061)	(0.058)	(0.104)	(0.092)
Christian	-0.266	0.221	-0.101	-0.187	-0.045	0.227*	-0.012	-0.042
	(0.828)	(0.459)	(0.167)	(0.180)	(0.140)	(0.135)	(0.148)	(0.139)
Islam	-0.015	0.314	-0.160	-0.220	-0.052	0.197	0.051	0.000
	(0.775)	(0.426)	(0.160)	(0.181)	(0.136)	(0.122)	(0.134)	(0.139)
Temne	0.081	-0.354*	-0.062	0.050*	0.081**	0.041	0.058	0.082*
	(0.184)	(0.211)	(0.052)	(0.027)	(0.041)	(0.036)	(0.053)	(0.047)
Mende	-0.182	-0.029	-0.006	0.020	-0.025	0.040	-0.034	0.138**
	(0.150)	(0.258)	(0.069)	(0.065)	(0.044)	(0.089)	(0.105)	(0.065)
Urban	0.068	0.411*	0.012	-0.003	-0.132**	-0.123**	-0.122**	-0.144**
	(0.217)	(0.220)	(0.039)	(0.036)	(0.061)	(0.050)	(0.062)	(0.063)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First stage (F-stat.)	25.80	22.51	25.80	25.56	25.76	24.68	26.64	25.41
Observations	1489	1398	1489	1455	1488	1473	1477	1486

Notes: Standard errors in parentheses are clustered at the year*district level. Statistical levels of significance are: * indicates p<0.1, ** indicates p<0.05, *** indicates p<0.01. The sample comprises the FPE cohort (those born during the period 1990 to 1993) and those females born during the period 1986 to 1989, but only those who have had sex in the past. The model also includes a variable for female share of primary school enrollment in 2004 in the district. Instrument: interaction of FPE cohort and FPE input, where FPE input is calculated as the log of funding allocated for primary school education for every 100 teacher in 2004. Regressions are weighted by female population*number of teachers. Constant term is not reported.

APPENDIX D: SUPPLEMENTARY MATERIAL FOR CHAPTER 2

Comparison of results with other studies

Although I show the results to be quite robust, a potential source of contention relates to the magnitude of the estimated impact of the SAE program. Recall that the findings indicate that for an average state, an expenditure of $50,000 can help to avoid approximately four births to teenagers. To my knowledge, there are no other state-level based studies to which the results can be compared.

However, a comparison with the implied outcomes of a county-based abstinence study does shed some light on the credibility of the results. Specifically, Doniger et al. (2001) looked at the impact of an abstinence education campaign (*Not Me Not Now*) on several outcomes, including pregnancy rates amongst Monroe County (New York) females in the age range 15-17 years. Using a cross-sectional time series study on an average of 1,611 children from middle schools over the period 1993-1996 and after controlling for other pregnancy-causing factors, their results showed that the pregnancy rates declined by 13.9 per 1,000 among the 15-17 year old females who were treated by the campaign. During the same time period the largest decline in pregnancy rates in a comparable county was 6.3, while the decline was by 5.9 in the state of New York. Therefore, the implied net decline in pregnancy rates in Monroe County was 7.6 to 8 (i.e. 7.6=13.9-6.3 and 8=13.9-5.9) over a three year period, suggesting an annual decline of about 2.6 pregnancies per 1,000 15-17 year old females. With an average annual sample size of 1,611, this in turn implies that the program may have been associated with avoiding approximately four pregnancies per year (=2.6/1000*1,611). Funding for the *Not Me Not Now* program was $300,000 over the four years 1993-1996. This meant that on average, $75,000 was spent per annum on the campaign. Assuming a constant rate of return, if annual expenditure was $50,000, then approximately 2 pregnancies to 15-17 year-olds will be avoided.

APPENDIX E: SUPPLEMENTARY MATERIAL FOR CHAPTER 3

Discrete-Time Hazard Analysis and Data Construction Process

Hazard (Event History) Analysis

To investigate the impact of the FMLA policy on fertility behavior, I apply hazard or event history models. These models go by different names with the most commonly used in economics being discrete or continuous time hazard models. In these models, the objective is to model the hazard of an event that has happened to a sample of individuals and the possible causes of this event overtime. Some of these causes (explanatory variables) may either be constant or they may vary with time. Although similar, there is a major difference in panel data models and hazard models: panel models tend to model an outcome with different values over some time period while hazard models model the one-time occurrence of an event.

I define a hazard as the probability that an individual who is at risk of experiencing an event does experience the event at a specified time conditional on the individual "surviving" up to that time. In my framework, the hazard might be the probability that a woman eligible for maternity leave under the FMLA does experience a birth during some period. Compactly, this may be written as:

$$\mathbf{h(t) = \lim_{\Delta t \to 0} \frac{P(E(t, t + \Delta t)|S(t))}{\Delta t}}$$

Alternatively, this can be written as

$$\mathbf{h(t) = \lim_{\Delta t \to 0} \frac{P(t \le T < \ \mathit{t} + \Delta t | T \ge t)}{\Delta t}}$$

where h(t) is the hazard or hazard rate; $\mathbf{P(E(t, t + \Delta t)}$ is the probability that the event occurs during the time interval [t, t+Δt). The duration of the event is T ≥ t, S(t) suggests the individual survives to (past) time t; t is a particular value of T; and Δt is some small period of time.

Upon closer examination, the hazard rate, h(t) is calculated as follows:

$$\mathbf{h(t)} = \frac{\textbf{number of individuals who experience the event}}{\mathbf{t} * \textbf{number of individuals exposed in the interval}}$$

where the denominator is a measure of person-time-units of exposure. The idea is that a separate observation is recorded for each unit of time an individual is exposed (or at risk) to an event. For instance, a woman who experiences a first birth after say one year will have contributed to one person-year while someone who gives a first birth after five years will have contributed five person-years. In hazard models, the outcome is the hazard rate. The models may be estimated using continuous time or discrete time. Typically, if the time intervals are very small units such as days, minutes, seconds, etc, then continuous time is preferred to discrete time hazard models. Models in discrete time are better utilized where there are larger time intervals such as months and years. In some instances, the decision to use the continuous or the discrete-time method is not always clear. In any event, the discrete method converges to the continuous time or (proportional hazard) model as the interval gets progressively smaller.

Data Construction for Discrete Time Hazard Analysis

I use discrete-time hazard approach to model the effect of the FMLA on the outcome variables. There are several advantages to using the discrete-time approach relative to the continuous method. First, as I mentioned earlier, the sample time period is in years which may best be utilized with the discrete time method. Second, it is very practical to use the discrete-time approach in non-proportional hazards with time-varying covariates. Third, discrete-time hazard models work much in the same was as logit regression models which are very common in the social science literature. As far as disadvantages in using the discrete-time approach, these models require additional manipulation of the data and this tends to increase the size of the dataset. With most computer software being equipped to deal with this issue, the potential for long computational time is minimized. On the other hand, the additional manipulation required in discrete-time models turns out to be an advantage in that there is greater transparency in estimation relative to continuous-time models where most of the estimation procedures are already programmed within the software and concealed from the user.

Consequently, the first step in undertaking a discrete-time hazard analysis is to expand the dataset in the following manner. Let y_i represent the time the event (or the censoring) occurs for individual i, c_i is a censoring binary indicator and the response variable is Y_{it}. Therefore, we have:

$$\mathbf{Y_{it}} = \begin{cases} \mathbf{0}, & \mathbf{t < y_i} \\ \mathbf{0}, & \boldsymbol{t} = \mathbf{y_i}, \mathbf{c_i} = \mathbf{1} \\ \mathbf{1}, & \boldsymbol{t} = \mathbf{y_i}, \mathbf{c_i} = \mathbf{0} \end{cases}$$

As an example of how to interpret the above, if a person experiences an event during the second time interval then the discrete response will be $(Y_{i1,}, Y_{i2})$=(0,1), while a person who is censored in the third period will have the discrete response $(Y_{i1,}, Y_{i2}, Y_{i3})$=(0,0,0) , and so forth. The restructured dataset is called a person-period (person-year) file. If the “event” is a first birth, for each year that a woman does not experience an event conditional on being at risk (eligible for FMLA), a separate observation (person-year) is created. A woman gives a first birth is subsequently removed from the sample and adds no additional person-years to the sample. For instance, beginning 1989 (the first year of the sample), a woman who experiences a first birth in this year, contributes one person-year. If she does not give a first birth until say 1995, then she contributes seven person-years. Censored women – those who did not experience a birth during the sample period – will have contributed a total of 18 person-years. This data restructuring will typically result in the number of person-years being greater than the actual number of women in the sample.

APPENDIX F: SUPPLEMENTARY MATERIAL FOR CHAPTER 4

Preference questions as appearing in the SLDHS

1. If you could go back to the time you did not have children and could choose exactly the number of children to have in your whole life, how many would that be? Or for those with no living children, the following question was asked: If you could choose exactly the number of children to have in your whole life, how many would that be?
2. How old were you when you had sexual intercourse for the very first time?
3. Is a wife justified in refusing to have sex with her husband when she is tired or not in the mood?
4. Have you ever used any (any of the contraceptive methods)?
5. I don't want to know the results, but have you ever been tested to see if you have the AIDS virus?
6. Husband and wives do not always agree in everything. If a wife knows her husband has a disease that she can get during sexual intercourse, is she justified in refusing to have sex with him?
7. If a wife knows her husband has a disease that she can get during sexual intercourse, is she justified in asking that they use a condom when they have sex?
8. Sometimes a husband is annoyed or angered by the things that his wife does. In your opinion, is a husband justified in hitting or beating his wife in the following situations: if she refuses to have sex with him? (Other situations were given to the respondent as well)

VITA

Colin Cannonier was born in Basseterre, the capital of the twin-island federation of St. Christopher (St. Kitts) and Nevis. In 1999, he earned his Bachelor of Science in Business, Economic and Social Statistics from the University of the West Indies, Jamaica. Colin also holds a Master of Arts in Development Economics from Williams College in 2004 and a Master of Science in economics from Louisiana State University in 2008, both in the U.S. While at Louisiana State University, Colin received the Huel Perkins Diversity Fellowship for four years to pursue a doctorate under the supervision of Dr. Naci Mocan. Colin has a paper forthcoming in the journal *Economics of Education Review* co-authored with Dr. Mocan. He also has another paper that constitutes a chapter of the dissertation and is currently being revised for resubmission to the *Review of Economics of the Household.*

His previous work experience is that of a country economist and Balance of Payments statistician at a regional central bank in the Eastern Caribbean. His interests also extend to sports. He has represented his country and the sub-region with distinction as a player and captain in cricket.

The title of his dissertation reflects his primary fields of interest, specifically, looking at the role of public policy in affecting such outcomes as fertility, educational attainment and preferences towards fertility and sexuality. Colin will complete the degree of Doctor of Philosophy in August 2011.