IMPACT OF LEGAL AND PUBLIC POLICY CHANGES ON SOCIAL AND ECONOMIC BEHAVIOR

DISSertation

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Abstract

My dissertation consists of two chapters investigating impact of various legal and public policy changes on several important social and economic behaviors in the United States. The first chapter examines the long-term impact of legalized abortion on teenage out-of-wedlock childbearing, which has been in constant decline since the early 1990s in the U. S. My argument is that to the extent that it prevented unwanted births, legalized abortion could have reduced the likelihood of the teenage out-of-wedlock childbearing for the cohorts born after the legalization. This is analogous to the argument of Donahue and Levitt (2001) for crime but extends their analyses to a different context. I adopt a non-parametric approach that allows me to separately estimate the effects of the legal changes concerning abortion in the repeal states in 1970 and Roe v. Wade ruling in 1973 on Whites and African-Americans. My results show that legalized abortion can potentially account for a little less than one third of the decline in the teenage out-of-wedlock childbearing among 15-17 years olds for African-Americans and a little more than one third of this decline for Whites in the 1990s.

The second chapter explores two approaches to allow for the effect of eligibility to participate in Medicaid to differ across individuals. The first approach allows for interactions between eligibility and demographic variables in the linear probability model, which does not require distributional assumptions on the error terms. The second is based on a switching probit model and has the advantage of letting newly eligible
individuals to be different from those previously eligible in terms of both observed and unobserved characteristics. By exploiting exogenous policy changes in Medicaid eligibility in the late 1980s and early 1990s, we estimate the average Medicaid participation rate of many demographic groups. We also estimate how different demographic groups respond in terms of Medicaid participation to a hypothetical policy experiment of increasing income limits for Medicaid eligibility by 10 percent in 1995. Our results suggest that there are large variations among demographic groups in participation rates and in their reactions to a policy change.
To my parents Nurcan and Hatay Ozbeklik and my aunt Nezahat
Aktan
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CHAPTER 1

1. THE EFFECT OF ABORTION LEGALIZATION ON TEENAGE OUT-OF-WEDLOCK CHILDBEARING IN FUTURE COHORTS

1.1 Introduction

Total teenage childbearing has been in a long-term decline since the late 1950s when it hit its highest level of 96 births per 1000 women aged 15-19, except for a brief but steep increase from the mid-1980s until the early 1990s. The reasons for this decline are well documented; delayed marriage as well as reductions in the fertility of married teenagers being the most crucial explanations. On the other hand, births to unmarried teenagers show a different pattern. Between 1960 and 1994, the non-marital birth rate per 1000 women aged 15-19 almost tripled, before going down after 1994.

It is interesting to note that while there are many studies that attempt to explain the reasons for the increase in the number of single mothers in general, as well as those of single teen mothers, studies examining the decline after 1994 are non-existent. To our knowledge, only exception is a work by Levine (2001), though the outcome he studies is pregnancy risk of teenagers rather than their childbearing decisions. In that study, Levine

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1 We discuss trends in teenage childbearing in the United States at length in Appendix 1.
2 See Geronimus and Korenman (1992), Grogger and Bronars (1995, Hotz et al. (1997) to name a few studies for the negative consequences of giving birth in adolescence.
3 We became aware after the completion of the first draft that Donahue, Grogger and Levitt (2002), in a work in progress, look at the same question using a different identification strategy. We give a detailed review of a very preliminary version of their study below.
covers a wide range of issues regarding decisions of teenagers on sexual activity, birth control use and childbearing. After extensively reviewing the previous literature on these issues, he examines the impact of “costs” associated with becoming pregnant and childbearing on pregnancy risk of teenagers. These costs include labor market conditions, the AIDS incidence, the generosity of the welfare system, and abortion restrictions in a teen’s state of residence. His results suggest that more than a half of the decline in pregnancy risk among black, non-Hispanics that may have contributed the dramatic decline in teen fertility among this group cannot be explained by increases in these costs in the early to mid-1990s. Hence, we need to look at different avenues for a better and complete understanding of this crucial behavioral change.

This paper uses a non-parametric approach to investigate the legalization of abortion in the early 1970s in the United States as one of the possible explanations for this behavioral change. Abortion was illegal under any circumstance in the United States until the late 1960s. This started to change between 1967 and 1970 when Colorado, North Carolina, California, Florida Georgia, Maryland, Arkansas, Kansas, New Mexico, Oregon, Delaware and Virginia made abortion legal for a limited number of circumstances. These circumstances included: i) to prevent the death or serious impairment of the physical and mental health of the mother; ii) the fetus would be born with a serious physical or mental defect; or iii) if the woman was raped or was subject to incest. The next step towards abortion legalization was taken in 1970 by Alaska, Hawaii,

4 We are aware that there is an active debate in the United States regarding abortion. Our paper does not intend to take a position on this sensitive issue. Rather, it intends to extend the borders of the current empirical research on out-of-wedlock teenage childbearing by suggesting a different avenue for investigation. Therefore it is a positive, not a normative, study.

5 See Merz et al. (1995) for detailed history of state and federal laws concerning abortion in the 20th century, as well as before.
New York, and Washington when these states repealed their antiabortion laws, and by California when the California Supreme Court ruled that the state’s abortion ban was unconstitutional. Thus in 1970, legal abortion became widely available in these states. In the sections follow, we will call these five states as the repeal states and the rest of the states as the non-repeal states. The final step towards abortion legalization was taken in 1973 in the U. S. Supreme Court ruling in Roe vs. Wade, which made abortion legal in the rest of the United States. Even though we do not have credible data on number of illegal abortions performed in 1960s, after abortion became legal throughout the United States the total number of documented abortions rose sharply from 750,000 in 1973 to over 1.6 million, in 1980 (Donahue and Levitt, 2001). Moreover, after Roe v. Wade the financial and psychic cost of abortion decreased significantly making abortion much more widely accessible (Kaplan, 1988). Therefore, Roe v. Wade should have effects well beyond the replacement of illegal abortions with an equivalent number of legal abortions.

Legalized abortion could potentially account for part of the decline of teenage out-of-wedlock childbearing by changing the family environment in which teenagers were raised.6 Also, by increasing the socioeconomic status of the mothers it could lead to an average lower non-marital fertility of their children. One set of studies suggests that cohorts born after legalized abortion experienced a significant decline in a number of adverse outcomes such as living in a single parent family, living in poverty, receiving welfare and infant mortality (see Gruber et al., 1999). Furthermore, Angrist and Evans

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6 As one might expect, the number of abortions performed in the US increased significantly during the 1970s and early 1980s before reaching its peak in 1983. Even though the data on the number of illegal abortions before the legalization are not available, if legal abortions simply replaced illegal abortions, then one would not see the increase in the number of abortion performed during the 1970s and early 1980s (DL, 2001). See Appendix 3 for abortion rates per 1000 live births in the repeal and non-repeal states during the 1970s and the early 1980s.
(1999) find that African-American women who were exposed to abortion reforms experienced large reductions in teen fertility and teen out-of-wedlock fertility that appear to have led to increased schooling and employment rates. Another group of studies suggests that teens who grow up in a disadvantaged family are at greater risk of giving birth out-of-wedlock (e.g. Moore et al., 1995; McLanahan and Sandefur, 1995). This paper, along with Donahue, Grogger, and Levitt (2002) (hereafter DGL), fills the gap between these two separate literatures and gives a different perspective on the issue of out-of-wedlock teenage childbearing trends observed in the United States. Given this evidence, one might expect to see a relationship between the legalization of abortion in early the 1970’s and the childbearing behavior in their adolescence for cohorts affected by the legalized abortion. Obviously we do not claim that abortion legalization is alone responsible for these trends in the 1990s, but we believe that it has the potential to account for a significant component of the trend.

The link we will examine is in the tradition of Donohue and Levitt (2001). In their seminal paper, DL investigate how legalized abortion influenced the criminal behavior of cohorts affected by the changes in abortion laws in the early 1970s. They found that this change accounts for most of the reduction in crime in the 1990s by reducing the number of unwanted births through selection. Since crime is mostly associated with males, the impact of the legalized abortion on crime works through aborted male fetuses after legalization. Abortion legalization would also be expected to have an impact on the outcomes of females such as teenage out-of-wedlock childbearing, on which we focus in this paper. We find that for African-Americans, both the 1970 legalizations in the repeal

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7 See also Ananat et al. (2006) for the selection effect of legalized abortion on several young adult outcomes.
decision lead to long-term reduction in out-of-wedlock teenage childbearing. For Whites,
there is no evidence supporting a long-term effect of the 1970 legalizations, but the
cohorts born after Roe v. Wade in the non-repeal states (the rest of the United States
other than the repeal states) show a reduction in teenage out-of-wedlock childbirth. Our
findings are consistent with Levine et al. (1999), who find that the early legalization in
the repeal states had a much stronger effect on the immediate fertility of Non-Whites than
Whites. Our model also passes a test indicating that it is indeed valid to take the
legalization in the repeal states as exogenous. Finally, between 1994, the first year the
increasing trend was reversed for Whites, and 2001 the decline in the out-of-wedlock
birth rates among the 15-17 years olds was 24 percent for Whites. The same decline was
45 percent for African-Americans between 1991 and 2001. Our results show that
legalized abortion can potentially account for a little less than one third of this decline in
the teenage out-of-wedlock childbearing among 15-17 years olds for African-Americans
and a little more than one third of this decline for Whites in the 1990s. Also since the
fertility of African-Americans appears to be affected by legalized abortion earlier, our
results suggest a potential reason for why teen out-of-wedlock childbearing for African-
Americans started declining 3 years before than as it did for Whites.

The plan of the paper is as follows. In Section 2, we discuss further why there
might be a potential long-term relationship between legalized abortion in the 1970s and
the childbearing behavior of teens in the late 1980s and 1990s. We then review in Section
3 the literature on the potential reasons for giving birth out-of-wedlock in adolescence
and on the impact of legalized abortion. Section 4 discusses the data. We present the
econometric methodology used and our results in Section 5. After summarizing our findings and discussing their implications in Section 6, Section 7 concludes the paper.

1.2 A Potential Long-Term Effect of Legalized Abortion on Teenage Out-of-Wedlock Childbearing

Legalized abortion might have an impact on reducing per capita teenage out-of-wedlock childbearing through at least two channels. The first and more obvious channel is through facilitating the termination of unwanted pregnancies, which are more likely to be pregnancies of unmarried women. The second channel, upon which this paper focuses, is an indirect one and expected to show its impact in the longer run. Potentially, females born after abortion legalization may have a lower likelihood of experiencing out-of-wedlock childbearing in adolescence. The first reason for this is that women who are more likely to have abortions, i.e. teenagers, unmarried women and the economically disadvantaged, are those potentially at risk of giving birth to children who end up having children out-of-wedlock themselves (Moore et al., 1995; McLanahan and Sandefour, 1995). Gruber et. al. (1999) document that the early life circumstances of the children on the margin of abortion are difficult along many dimensions: infant mortality, growing-up in a single-parent family and experiencing poverty. Adolescent females raised in these family environments are more likely to give birth out-of-wedlock. Also, legalized abortion provides a woman the opportunity to delay childbearing if current condition are suboptimal and allow her to give birth in the environment which she thinks

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8 This first effect will drop out in the estimation strategies we will use below.
is better and more nurturing for the development of her child (DL, 2001). Finally, since the social and economic outcomes of black women were by pre-Roe reforms (Angrist and Evans, 1999), the increased socioeconomic status of the mothers may have lead to the average lower non-marital fertility of their children.

During the 1970s access of unmarried women to oral contraceptives also increased throughout the U.S. Therefore, to the extent that oral contraceptives prevented unwanted childbearing of unmarried women in the 1970s, one can argue that the effect we identify is due, at least in part, to oral contraceptives, rather than legalized abortion. While we believe that the increased availability of oral contraceptives in the 1970s potentially had a long-term impact on teenage out-of-wedlock childbearing similar to legalized abortion, we are not aware of a distinct change in the usage of pills around the time of legalizations in 1970 in the repeal states and in 1973 in the non-repeal states. Therefore, our results will not likely be affected from this confounding impact.

Before proceeding to our econometric analysis, we first present time series evidence on the teenage out-of-wedlock childbearing rates for the cohorts born between 1967 and 1982. Figures 1.1 and 1.2 respectively provide this for the total population and for African-Americans and Whites separately. The numbers in these figures can be interpreted as the cumulative number of births per 1000 women in the particular cohorts at the end of the teen years. For both the total population and African-Americans, teenage out-of-wedlock birth rates increase for the 1967-1973 birth cohorts (in the period 1982-1992). For the total population, this increase continues up until the 1976 birth cohort and then starts declining. The corresponding birth rates for African-Americans, however,

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9 Note that if the male fetuses that were aborted were more likely to father a child out-of-wedlock, the reduced out-of-wedlock births in future cohorts could also be due to selection on males.
show almost no change for the cohort born in 1974 and then start declining sharply for later cohorts. This time series evidence indicates that the trend for the non-marital birth rate of African-American teenagers experienced a break around the 1974 birth cohort, which is the first cohort affected by legalized abortion in the U.S among the non-repeal states.

1.3 Previous Literature on Teenage Fertility and the Impacts of Abortion

Legalization

1.3.1 Previous Literature on the Effects of Abortion Legalization

There have been two generations of studies looking at the potential effects of legalized abortion. The first generation studies explored the immediate impact of abortion legalization on different outcomes such as women’s fertility behavior (Baumann et al., 1977; Joyce and Mocan, 1990; Kramer, 1975; Levine et al., 1999 Quick, 1978; Sklar and Berkov, 1974; Tietze, 1973), schooling and labor market consequences (Angrist and Evans, 1999) and children’s living conditions (Gruber et al., 1999). Further, Levine et al. (1999) looked at the effect of abortion legalization on births in the 1970s in the United States by using the variation in the timing of legalization across states in the early 1970’s. They find that states legalizing abortion experienced a 5% decline in births relative to the rest of the country. The declines among teens, women over 35, and non-White women were even greater at 13%, 8% and 12% respectively. Angrist and Evans (1999) used the same variation to investigate the likely impact of legalization on
Figure 1.1: Cohort Out-of-Wedlock Birth Rates for Total Population between Ages 15-19, 1967-1982

Figure 1.2: Cohort Out-of-Wedlock Birth Rates for African-Americans and Whites between Ages 15-19, 1967-1982
education and labor market outcomes. Their results indicate that for White women, abortion reform did not appear to change schooling or labor market outcomes. On the other hand, African-American women who were exposed to abortion reforms experienced large reductions in teen fertility and teen out-of-wedlock fertility that appear to have led to increased schooling and employment rates. Finally, Gruber et al. (1999) examine the effect of the increased availability of abortion after abortion legalization on the average living conditions of children. They found that cohorts born after legalized abortion experienced a significant decline in a number of adverse outcomes such as living in a single parent family or in poverty, receiving welfare and dying as an infant.

The second generation of studies started with the seminal paper by DL (2001). In this controversial paper, DL argued that legalized abortion contributed significantly to the reduction in crime in the 1990’s. Since our paper is in the tradition of DL (2001), we will discuss their paper in more detail, starting with why one might expect to observe this relationship. In their paper, DL give two possible reasons. The first one is through reductions in cohort size resulting from abortion legalization. Smaller cohorts result in fewer young males in their primary crime years and thus less crime in the society. The more important reason is that children born after abortion legalization may have lower per capita crime rates since: (1) teenagers, unmarried women and economically disadvantaged women are those who are more likely to have abortion and also have the highest risk of giving birth to a child who could engage in criminal activity later in their lives; (2) abortion provides a woman the opportunity to delay childbearing if the current conditions in her life are not suitable for raising a child. Their results suggest that this

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10 Note that since women with higher education are less likely to give birth to a child who will have an out-of-wedlock pregnancy, this effect could also contribute to lower non-marital pregnancy in the future cohorts.
relationship between abortion legalization and crime exists because: (1) the five states that allowed abortion in 1970 experienced declines in crime earlier than the rest of the nation, which legalized abortion in 1973 with Supreme Court ruling on *Roe v. Wade*; (2) states with high abortion rates in the 1970s and 1980s experienced greater crime reductions in the 1990s; (3) in high abortion states, the number of arrests of “only” those born after abortion legalization fell relative to the low abortion states. This seminal article has initiated a renewed interest in the potential long-term impacts of legalized abortion.

There have been papers examining the impact of abortion legalization on crime and other socioeconomic outcomes for other countries (Pop-Eleches; 2003, Sen, 2002) as well as on substance use (Charles and Stephens, 2002). Also, another group of studies has examined the robustness of the DL results using different identification strategies and data different data (Joyce 2004a, 2004b; Foote and Goetz, 2005).11

In a recent working paper, Ananat et al (2006) examine the selection impact of legalized abortion on several young adult outcomes such as living in poverty, being a single parent, receiving welfare, dropping-out of high school, graduating from college and being employed. They use the 2000 decennial census of the United States to measure their outcomes of interest for individuals born in a given state and year. Their main identification strategy is to estimate some innovative instrumental variables regressions using several variables potentially measuring the cost of abortion interacted with time dummies in the 1970s as their instruments. Their results indicate that lower costs of abortion led to improved outcomes for the cohorts affected by the law change through

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11 In fact, these studies argue that DL’s results are sensitive to the identification strategy and the data used, so that their results are ambiguous at best. For DL’s reply, see DL (2004) and (2006).
increasing the likelihood of college graduation, the lower use of welfare, and decreasing the likelihood of being single parent.

Finally and most importantly, DGL (2002), in a work in progress, investigate the effect of abortion legalization during the 1970s on teenage childbearing in the cohorts affected by the legalization.\textsuperscript{12} They run a regression of the number of births on historical abortion rates in the period during which abortion was legalized controlling for state dummies and a number of control variables such as the current rate of abortions. At one point they use state dummies interacted with time dummies. They constrain the effect of abortions to be the same for Whites and African-Americans and for the 1970 legalization and \textit{Roe v. Wade}. They find strong statistically significant results regarding the impact of historical abortion rates on teenage childbearing in the long-run.\textsuperscript{13} Our work presented below differs from theirs in a number of ways. Most importantly, we estimate the effect of the legalizations on the birth rate directly. Also we allow these two legalizations to have different effects for African-Americans and Whites.\textsuperscript{14} This distinction provides a strong test of the hypotheses of DL and DGL in the following sense. Levine (1999) has shown that the effect of the early legalization was much stronger for Non-Whites than Whites. Thus if the effect on out-of-wedlock births is through abortion, rather than through some other contemporaneous factor, we should see the African-American out-of-wedlock birth rates falling in the cohorts that were exposed to early legalization in the repeal states but among Whites we should not see this until we reach the White cohorts

\textsuperscript{12} Most of our work was carried out before we were aware of this paper. The only draft of the paper we were able access is Donahue et al. (2002), which is a very preliminary version. Therefore, all our discussions of this paper are based on this preliminary version. We thank Professor Levitt for pointing out their work, and that of Ananat et al. (2006), to us.

\textsuperscript{13} Note that they also estimate their specifications for married teenagers.

\textsuperscript{14} We use the birth rate as our dependent variable in state s in year t, while they use the number of births as the dependent variable. Using the birth rate has the advantage that it will not be affected by cohort size.
who were affected by *Roe v. Wade*. We do know that the out-of-wedlock birth rates for African-American teenagers fell three years before whites. However, this is only suggestive since only a fraction of African-American cohorts who were living in the repeal states were affected by the 1970 legalizations. In other words, it is important that the selection effect is seen for African-Americans in the states that were affected by early legalization, not just for African-Americans as a whole. In fact, this is exactly what we do see in the data, providing strong support for the DL interpretation.

One advantage of our approach of looking at the effect of the law directly is that it is possible that DGL might obtain inconsistent estimates using the abortion rate for two reasons. First, it is widely thought that reported abortion rates underestimate true abortion rates, which suggests that their estimates of the effect of the historical abortion rate might potentially be upward biased.\(^\text{15}\) Secondly, abortion rates may be subject to classical measurement error. For example DL (2006) compares the abortion data collected by the Allan Guttmacher Institute (AGI) with the data collected by Center for Disease Control (CDC) after controlling for some observable factors and obtains a correlation of only .396, which indicates a large measurement error in the data. This classical measurement error will lead to a downward bias for the coefficient on historical abortions.\(^\text{16}\)

In other aspects the differences between our work and DGL represent the standard differences between parametric and nonparametric work. Their estimates are more efficient if the restrictions behind their linear model are correct, but are inconsistent if the restrictions do not hold. Clearly there are arguments for both approaches.

\(^{15}\) Note, however, that their estimate of the coefficient times the number of abortions is consistent.  
\(^{16}\) Of course, this suggests that one could use the CDC number as an instrument for the AGI number to eliminate this second source of inconsistency.
1.3.2 Previous Literature on Teenage Childbearing

There is a rich literature on the causes, as well as the consequences, of teenage childbearing. Since our paper is connected to the former, we will only review studies investigating the causes related to our argument. The first group of studies examine how individual factors, such as attitudes and expectations, risk taking behavior, school performance, and race influence teenage childbearing. These studies indicate that: (1) teens wanting a child or feeling ambivalent about having a child are more likely to experience adolescent childbearing (Zabin, 1994; Abrahamse et al., 1988; Hanson et al., 1989; Manlove, 1993); (2) educational expectations are negatively associated with the probability of adolescent childbearing (Moore et al., 1995b; Haggan and Wheaton, 1992; Sugland, 1992); (3) teens engaging in risky activities are more likely to give birth (Hanson et al., 1989; Serbin et al., 1991); (4) girls who fall behind in school and girls with a more negative attitude toward school are more likely to have a non-marital teen birth (Moore et al., 1995b; Zabin, 1994; Plotnick and Butler, 1991); and (5) even though African-Americans are more likely to have non-marital births in their teen years (Abrahamse et al., 1988; Moore et al., 1995b), race is not statistically significant after controlling for crucial family, neighborhood and policy variables (Haveman and Wolfe, 1994).

Another group of studies analyzes the relationship between family background and teenage childbearing. The findings of these studies single out several important family variables associated with teenage childbearing. The major conclusions of these studies can be summarized as follows: (1) women raised in single parent families or who

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17 See Moore et al. (1995a) for an excellent review of studies on teenage childbearing.
18 On the other hand, Plotnick and Butler (1991) could not find any association between expectations and teenage childbearing, or work attitudes and teenage childbearing.
experienced parental marital separation are more likely to have out-of-wedlock births as teenagers (Moore et al., 1995b; Wu and Martinson, 1993; McLanahan and Sandefur, 1994; Kahn and Anderson, 1992; Haveman and Wolfe, 1994; Wu, 1994); (2) daughters of teen mothers are more likely to be teen mothers themselves (Kahn and Anderson, 1992; Manlove, 1993, 1995; Horowitz et al., 1991); and (3) a higher maternal education is associated with a lower probability of teenage childbearing (Kahn and Anderson, 1992; Haveman et al., 1993).  

Last but not least, Levine (2001), in a study that is the most related to ours, examines the impact of “costs” associated with becoming pregnant and childbearing on pregnancy risk of teenagers using the multiple waves of the Youth Risk Behavior Survey (YRBS). These prices include labor market conditions, the AIDS incidence, the generosity of the welfare system, and abortion restrictions in a teen’s state of residence. His results suggest that more than a half of the decline in pregnancy risk among black, non-Hispanics that may have contributed the dramatic decline in teen fertility among this group cannot be explained by increases in these costs in the early to mid-1990s. For whites, the impact of these changes in the costs seems even smaller.

1.4 Data

Data used in this paper comes from several sources. Birth data are obtained from the Vital Statistics of the United States.  

There are also a large numbers of studies in both the economics and sociology literatures on the effects of public policy variables, especially welfare generosity, on teenage out-of-wedlock childbearing. These studies find that the effect of larger welfare benefits on teen fertility is quite modest at best, and the inconsistencies of the research findings on this issue weaken even this conclusion. See Moffitt (1998) for an excellent review of studies on the relationship between welfare policies and out-of-wedlock childbearing in general.

One limitation of the Vital Statistics data is that we observe the states where teens give birth but not the teen’s state of birth. Therefore we cannot account for selective migration occurring between the years when
teenage women as well as the total live births to all teenagers by state, race and age for the period between 1981 and 2001.\footnote{We follow the conventional definition and classify women aged 15-19 as teenagers.} Population data are provided by the National Cancer Institute (NCI). NCI has the county population estimates of the United States for the period covering 1969-2003. The estimates represent a modification by NCI of the annual time series of July 1 county population estimates by single year of age, sex and race that were originally produced by US Census Bureau.\footnote{For the full documentation of the modifications see http://seer.cancer.gov/popdata/} Since we need state level population data, the county level estimates are aggregated to the state level. We use state level population data to obtain a variant of a \textit{per capita} measure in order to isolate the long-term impact of abortion from the impact through \textit{a smaller cohort size} due to abortion. As we have already stated, our measure will be the birth rate per 1000 women in a given demographic group. Finally, abortion data is provided by Alan Guttmacher Institute.

In order to isolate the different trends associated with African-American and White teenage out-of-wedlock childbearing, we carry out analysis of the two races separately, in addition to performing our analysis for the total population. Since Idaho, Maine, Montana, New Hampshire, North Dakota, South Dakota, Vermont, and Wyoming have a very low number of African-American teenagers and of births to African-American teenagers as a result, we drop these states from our data while performing our analysis for African-Americans.\footnote{In order to check whether our results for Whites are different due to the inclusion of these states in the regressions for Whites, we run our regression specifications using the same states that we used in our specifications for African-Americans. Our results did not change.}
1.5 Econometric Methodology and Results

Our identification strategies exploit two important sets of legal changes regarding abortion laws that occurred in the early 1970s in the United States. The first set of legal changes took place in 1970 when Alaska, Hawaii, New York, and Washington repealed their antiabortion laws and the Supreme Court of California ruled in late 1969 that the state’s law banning abortion was unconstitutional (DL, 2001). Second, we will take advantage of the historical ruling of the United States Supreme Court in *Roe v. Wade* in 1973 that lifted the ban on abortion in the rest of the country. We are not, of course, the first to exploit these changes. Indeed, as noted above, there is a rich literature on the potential impacts of abortion legalization, and most of the recent papers in this literature used these changes in one way or another to obtain identification.

1.5.1 Abortion Legalization as a Natural Experiment: Threats to Identification

Finding a credible comparison group is very critical in a natural experiment exercise. In this section, we summarize what can go wrong in identification if we consider abortion legalization as a natural experiment and how we will address those issues.

Suppose for simplicity we are interested in the long term impact of the 1970 legalization in the repeal states. Let the birth equation be given by

\[
BR_{ijt} = \alpha_0 + \alpha_i \text{POST}70_{ij} + \gamma_i + \theta_j + \mu t + \delta_j t + \varepsilon_{ijt} .
\]  

(1)

---

24 See Meyer (1995) for excellent summary of potential treats to identification assumptions of natural experiments.
where \( BR_{ijt} \) is the birth rate of teenagers in state \( i \) (\( i \in \text{repeal} \)), age \( j \) and year \( t \);

\( POST70_{ijt} \) is a dummy that is equal to one if the birth rate in state \( i \), age \( j \) and year \( t \) belongs to teens who were born in the repeal states after abortion became legal; \( \gamma_i \) are state fixed-effects; \( \theta_j \) are age fixed-effects; \( \mu_t \) capture state-specific trends; and \( \delta_{jt} \) capture age-specific trends.

One possible approach is to choose \( i, j \) and \( t \) such that we compare teen non-marital birth rates of cohorts for the repeal states that were born before and after abortion became legal in the repeal states. This is the \textit{Before-After} approach and can be written in the following way

\[
\Delta BR_{ijt} = \alpha_i \Delta POST70_{ijt} + \mu_t + \delta_j + \Delta \varepsilon_{it}
\]

(2)

where \( i = 1,\ldots,5 \ (i \in \text{repeal}) \) and \( t = 1984,1986 \) and \( j=15 \). Thus those aged 15 in 1984 were not affected by legalized abortion in 1970 as they were born in 1969 while those aged 15 in 1986 were born in 1971 and affected by legalized abortion (see Figure 1.3). This procedure will allow for treating \( \gamma_i \) and \( \theta_j \) in (1) as fixed effects and \( \delta_j \) as constant such that \( \delta_j = \delta \) for all \( j \). However, it will not allow one to capture the state-specific time trend coefficient \( \mu_t \). In other words \( \mu_t \) acts like a fixed effect in first differences. Further, even if \( \mu_i = \mu \) for all \( i \) one would not be able to separately identify \( \alpha_i \) from \( \mu \). The latter problem can be addressed by, for example, using equation (2) and setting: 18
i = 1, ..., 5 (i ∈ repeal); i’ = 6, ..., 51 (i’ ∈ nonrepeal); t = 1984, 1986; and j = 15 as long as

\( \mu_i = \mu_i' = \mu \) for all i and i’ so that

\[
\Delta BR_{(r),jt} - \Delta BR_{(nr),jt} = \alpha_i \Delta POST70_{(r),jt} + (\Delta \bar{\epsilon}_{(r),jt} - \Delta \bar{\epsilon}_{(nr),jt}), \tag{3a}
\]

\[
\Delta BR_{(r),jt} = \frac{\sum_{i \text{ repeal}} \Delta BR_{ijt}}{N_{\text{repeal}}} \quad \text{and} \quad \Delta BR_{(nr),jt} = \frac{\sum_{i' \text{ nonrepeal}} \Delta BR_{ij't}}{N_{\text{nonrepeal}}}, \tag{3b}
\]

where \( N_{\text{repeal}} \) and \( N_{\text{nonrepeal}} \) are the numbers of the repeal and non-repeal states respectively. Notice that estimating above regression using the difference of first differences in non-marital teenage birth rates between the repeal and non-repeal states in non-marital teenage birth rates between the repeal and non-repeal states difference estimator (DD). Of course in practice it is inefficient to use only those aged 15 years old, and I will also use 16, 17, 18 and 19 year-olds for our DD estimation.

However, if \( \mu_i \neq \mu_i' \neq \mu \) for all i and i’, one has the problem so that equation (3) will be as follows\(^{25}\)

\[
\Delta BR_{(r),jt} - \Delta BR_{(nr),jt} = \alpha_i \Delta POST70_{ijt} + (\bar{\mu}_{(r)} - \bar{\mu}_{(nr)}) + (\Delta \bar{\epsilon}_{(r),jt} - \Delta \bar{\epsilon}_{(nr),jt}) \tag{4a}
\]

\[
\bar{\mu}_{(r)} = \frac{\sum \mu_i}{N_{\text{repeal}}} \quad \text{and} \quad \bar{\mu}_{(nr)} = \frac{\sum \mu_{i'}}{N_{\text{nonrepeal}}} \tag{4b}
\]

To address this problem, consider (4a) for another age group \( j' \) (\( j' \neq j \))

\(^{25}\) Note that \( \Delta POST70_{ij't} \) is zero since abortion was still illegal in the non-repeal states in 1971.
Figure 1.3: The Long-Term Impact of Legalized Abortion in 1970 in the Repeal States

- The 1969 cohort was born - not affected by legalized abortion in the repeal states in 1970.

- The 1971 cohort was born - affected by legalized abortion in the repeal states in 1970.

- The 1971 birth cohort is 15 year old. Their out-of-wedlock birth rate was affected by the 1970 legalizations in the repeal states since they were born after abortion became legal.

- The 1969 birth cohort is 15 year old. Their out-of-wedlock birth rate was not affected by the 1970 legalizations in the repeal states since they were born before abortion became legal.

- The 1986 - The 1971 birth cohort is 15 year old. Their out-of-wedlock birth rate was affected by the 1970 legalizations in the repeal states since they were born after abortion became legal.
\[ \Delta BR_{(r)jt} - \Delta BR_{(mr)jt} = \alpha_i \Delta POST 70_{(r)jt} + (\mu_{(r)} - \mu_{(mr)}) + (\Delta E_{(r)jt} - \Delta E_{(mr)jt}) \] (5)

For example we could look at the first differences of 15 and 17 year-olds between 1986-1984 (see Figure 1.4), and subtract (4a) from (5). Taking this extra difference will eliminate state-specific time trends in the data since eliminates \( \mu \). This will be the first estimator we will use and refer to it as the difference-

\[ (\Delta BR_{(r)jt} - \Delta BR_{(mr)jt}) - (\Delta BR_{(r)jt} - \Delta BR_{(mr)jt}) = \alpha_i \Delta POST 70_{(r)jt} + ((\Delta E_{(r)jt} - \Delta E_{(mr)jt}) -(\Delta E_{(r)jt} - \Delta E_{(mr)jt})) \] (6)

This is the Difference-in-Difference-in-Difference (DDD) estimator and in this case \( \alpha_i \) identifies the long-term impact of the 1970 legalizations in the repeal states allowing for state specific trends. \(^{26}\) In the sections follow, we will use the more conservative \( DD \) and \( DDD \) approaches and disregard the Before-After estimation.

### 1.5.2 Cross-State DD Estimation: A Comparison of Early Legalization States with the Rest of the United States

In this part of the paper, we first define the birth cohort years 1967-1970 as the pre-legalization years and 1971-1973 as the post-legalization years in the repeal states. Teenagers who were born in the pre-legalization years, whether they were born in the early legalization states or in the rest of the U. S., would not be affected by the changes in the abortion law that took place in 1970. On the other hand, for the cohorts born between

\(^{26}\) Notice that \( \alpha_i \Delta POST 70_{(r)jt} \) is zero since age group \( j' \) in the repeal states did not affected by the 1970 legalizations since they were born before 1970.
1971 and 1973 exposure to the legalized abortion will differ among teenagers; if they were born in the repeal states they would be affected, otherwise they would not. Therefore, our $DD$ will capture the change in birth rates of single teenagers between pre- and post-legalization years in the repeal states relative to the changes in the non-repeal states. If we want to put $DD$ in a regression framework, it can be written as follows$^{27}$

$$
\ln BR_{ijt} = \gamma_0 + \gamma_1 (REPEAL_i \times C7173_j) + a_j \cdot s_j + a_j \cdot y_t + u_{ijt},
$$

where $\ln BR_{ijt}$ is the logarithm of birth rates per thousand teen of age $j$, in state $i$ and year $t$; $REPEAL_i$ is the dummy variable equal to one if state $i$ is among the early legalizing states and $C7173_j$ is the 1971-1973 birth cohort dummy for year $t$ and age $j$. Further, we also control for age-state ($a_j \cdot s_j$) as well as age-year ($a_j \cdot y_t$) effects. In the above regression $\gamma_1$ is our parameter of interest, $DD$, and can be interpreted as the proportionate change in birth rates between the 1971-1973 and 1967-1970 birth cohorts in the repeal states relative to the rest of the United States. We expect this coefficient to be negative since our conjecture is that the teenagers who were born in the repeal states

$^{27}$ Notice that this regression is in levels rather than in differences. We also use more than one age group and include the 1972 and 1973 birth cohorts compare to a simple example of $DD$ we described in the previous section. Therefore it is more general compare to equation (3a) but $\gamma_1$ still identifies $DD$ estimator.

$^{28}$ For example, consider the observations for 1988. For this year, $C7173_{88}$ will be equal to 1 for 15, 16, and 17 years old women but zero for women aged 18 and 19. Note also that this is identical to $POST70_{ijt}$ dummy in the previous section.
The 1967 cohort was born - not affected by legalized abortion in the repeal states in 1970.

The 1969 cohort was born - not affected by legalized abortion in the repeal states in 1970.

The 1971 cohort was born - affected by legalized abortion in the repeal states in 1970.

1984 - The 1967 and 1969 birth cohorts are 17 and 15 years old respectively. Use the difference in their birth rates in the estimation.

1986 - The 1969 and 1971 birth cohorts are 17 and 15 years old respectively. Use the difference in their birth rates in the estimation.

Figure 1.4: The Long-Term Impact of the 1970 Legalizations in the Repeal States Using Within-State Comparison Group
between 1971 and 1973 are less likely to have a non-marital birth relative to the teenagers who were born in the non-repeal states during the same time period. Our assumption here is that there are no state-specific time trends so that the non-repeal states form a credible control group to examine the impact of legalized abortion for the repeal states. Table 1.1 presents DD estimation results. All regressions are carried out by weighted least squares, where the square root of the state teenage population for the corresponding race for a given year is the weight for a given observation. We estimate the impact separately for African Americans and Whites. We believe this provides a strong test of the idea that legalized abortion affected teenage out-of-wedlock childbearing. Levine et al. (1999) have shown that the early legalization in the repeal states had a stronger effect on the immediate fertility of Non-Whites than Whites in the early 1970s, so it should be the case that the effect of the early legalization was potentially stronger for African Americans than Whites.  

The first panel of the table presents the estimates for the total teenage population. Panels 2 and 3 show our estimates for the African-American and White teenage populations respectively. The first column of all the panels shows the results of our baseline specification. While for the total population and Whites, coefficients on $REPEAL_i \times C7173_{jt}$ are not significantly different from zero, the coefficient is much larger and statistically significant for African-Americans. Specifically, teenage out-of-wedlock childbearing declined about 10 percent for African-Americans in the repeal states relative to African-Americans in the non-repeal states for the 1971-1973 birth cohorts.

---

29 Here our assumption is the selection impact of legalized abortion for a birth aborted by African-Americans and Whites are the same so that lower fertility rates because of legalized abortion in the part of African-Americans in the 1970s should reflect a greater selection and a greater long-run impact.
In order to check the credibility of DD assumptions, we carry out several tests. First, if the teenagers in the non-repeal states are to be a credible comparison group, then trends in non-marital teenage childbearing for the repeal and non-repeal states must be comparable before the repeal states legalized abortion. To test this conjecture, we consider the following regression

\[
\ln BR_{ijt} = \gamma_0 + \gamma_1 (\text{REPEAL}_i \times C7173_{jt}) + \gamma_2 (\text{REPEAL}_i \times C70_{jt}) + a_j \cdot s_i + a_j \cdot y_i + u_{ijt} \quad (8)
\]

where \( C70_{jt} \) is the 1970 birth cohort dummy for year \( t \) and age \( j \), so that the interaction term is equal to one only for teens from repeal states who were born in 1970. The coefficient \( \gamma_2 \) captures the proportionate change in birth rates between the 1970 and 1967-1969 birth cohorts in the early legalizing states relative to the rest of the United States. If the trend in the out-of-wedlock teenage childbearing started diverging with the 1971 birth cohort, then this coefficient should be close to zero and not statistically significant. All other regression parameters are the same as in equation (7). This is the test that we refer to in the introduction.

Second, we expand our sample by adding data on childbearing behavior of teens born between 1974 and 1979, and add three extra interaction terms to our regression equation (8)

\[
\ln BR_{ijt} = \gamma_0 + \gamma_1 (\text{REPEAL}_i \times C7173_{jt}) + \gamma_2 (\text{REPEAL}_i \times C70_{jt}) \\
+ \gamma_3 (\text{REPEAL}_i \times C74_{jt}) + \gamma_4 (\text{REPEAL}_i \times C7576_{jt}) \\
+ \gamma_5 (\text{REPEAL}_i \times C7779_{jt}) + a_j \cdot s_i + a_j \cdot y_i + u_{ijt} \\ (9)
\]
where $C74_{jt}$, $C7576_{jt}$, and $C7779_{jt}$ are the birth cohort dummies for teens of age $j$ in year $t$ who were born in 1974, 1975-1976 and 1977-1979 respectively. Therefore, the coefficients $\gamma_3$, $\gamma_4$, and $\gamma_5$ capture how teenage childbearing in the non-repeal states affected by Roe v. Wade differs from that for the repeal states for the cohorts who were born in the years following Roe v. Wade. The coefficients $\gamma_3$, $\gamma_4$, and $\gamma_5$ should not be significantly different from zero if (1) legalized abortion was the only factor that accounted for the potentially diverging trends in the teenage childbearing for cohorts born in the 1971-1973 period between repeal and non-repeal states so that there was no state-specific time trends affecting birth rates; (2) both the 1970 legalizations in the repeal states and Roe v. Wade had the same impact on the out-of-wedlock childbearing of the birth cohorts affected by these law changes towards abortion. Thus, this is the test of the assumption underlying $DD$.

In the second column of Table 1 we present the results from these tests. The coefficients on $REPEAL_i \times C70_{jt}$ are not statistically different from zero for any of the groups. Thus our model passes a test which indicates that it is reasonable to treat the 1970 legalization in the repeal states as exogenous. The coefficients on $REPEAL_i \times C74_{jt}$, $REPEAL_i \times C7576_{jt}$, and $REPEAL_i \times C7779_{jt}$ are not statistically different from zero as the coefficient for the total population. For African-Americans, on the other hand, they are negative and as large as the coefficient on $REPEAL_i \times C7173_{jt}$. This may seem to be a surprising result since one might expect catching-up on the part of non-repeal states after Roe v. Wade legalized abortion in the rest of the country. But
Levine et al. (1999) show that while the birth rates of Whites living in the non-repeal and repeal states had converged after *Roe v. Wade*, for African-Americans this difference stayed about the same, with the rebound in births in the non-repeal states being small. For Whites who were born between 1970-1973, even though it seems that are no statistically significant differences in the change of teenage childbearing between repeal and non-repeal states, this difference becomes positive and statistically significant for cohorts born between 1974-1976. However, it becomes statistically insignificant and close to zero for the 1977-1979 birth cohorts.

We can summarize our findings in this section as follows. First, while the 1971-1973 African-American birth cohorts in the repeal states were 10 percent less likely to have non-marital childbearing in their teen years relative to their peers in the non-repeal states, we do not find any difference for Whites. Second, neither for African-Americans nor for Whites, the trends in the out-of-wedlock teenage childbearing differ between the repeal states and the non-repeal states for the birth cohorts that were not affected by the 1970 legalizations. Finally, the non-marital teenage childbearing of African-American cohorts born after *Roe v. Wade* did not converge between the repeal states and the non-repeal states for the either or both of the following reasons: (1) State-specific trends confounds with our *DD* estimates; (2) the 1970 legalizations in the repeal states and *Roe v. Wade* had different impacts on the out-of-wedlock childbearing of the birth cohorts affected by these law changes towards abortion.

To address these issues, we will first estimate the impact of the 1970 legalizations in the repeal states using a *DDD* strategy, which controls for possible state-specific trends. We will then estimate the impact of *Roe v. Wade* separately using a similar *DDD*
## Table 1.1: Cross-State DD Estimation Results for the Total, African-American and White Teenage Out-of-Wedlock Birth Rates

<table>
<thead>
<tr>
<th></th>
<th>TOTAL</th>
<th>AFRICAN-AMERICANS</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\text{REPEAL}<em>t \times C70</em>{jt}$</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>-0.0019</td>
<td>-0.0306</td>
</tr>
<tr>
<td></td>
<td>(.06129)</td>
<td>(.02371)</td>
</tr>
<tr>
<td>$\text{REPEAL}<em>t \times C7173</em>{jt}$</td>
<td>-0.0115</td>
<td>-0.0967</td>
</tr>
<tr>
<td></td>
<td>(.05246)</td>
<td>(.0160)***</td>
</tr>
<tr>
<td></td>
<td>-0.0125</td>
<td>-0.1090***</td>
</tr>
<tr>
<td></td>
<td>(.0269)</td>
<td>(.0220)***</td>
</tr>
<tr>
<td>$\text{REPEAL}<em>t \times C74</em>{jt}$</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>0.0461</td>
<td>-0.1049***</td>
</tr>
<tr>
<td></td>
<td>(.0421)</td>
<td>(.0204)***</td>
</tr>
<tr>
<td>$\text{REPEAL}<em>t \times C7576</em>{jt}$</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>0.0503</td>
<td>-0.1176</td>
</tr>
<tr>
<td></td>
<td>(.0392)</td>
<td>(.0218)***</td>
</tr>
<tr>
<td>$\text{REPEAL}<em>t \times C7779</em>{jt}$</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>-0.0034</td>
<td>-0.1338</td>
</tr>
<tr>
<td></td>
<td>(.0234)</td>
<td>(.0383)***</td>
</tr>
</tbody>
</table>
### Table 1.1: Cross-State DD Estimation Results for the Total, African-American and White Teenage Out-of-Wedlock Birth Rates (cont.)

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
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<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>$REPEAL_i \times C70_{jt}$</td>
<td>-</td>
<td>.0092 (.0083)</td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>$REPEAL_i \times C7173_{jt}$</td>
<td>.0286</td>
<td>.0310 (.0271)</td>
<td>.0286</td>
<td>.0310 (.0269)</td>
</tr>
<tr>
<td>$REPEAL_i \times C74_{jt}$</td>
<td>-</td>
<td>.1087 (.0445)**</td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>$REPEAL_i \times C7576_{jt}$</td>
<td>-</td>
<td>.0995 (.0425)**</td>
<td>-</td>
<td></td>
</tr>
<tr>
<td>$REPEAL_i \times C7779_{jt}$</td>
<td>-</td>
<td>-.0048 (.0308)</td>
<td>-</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses; *** significant at the 1% level. All regressions are clustered at the state level.

1.5.3 Difference-in-Difference-in-Difference (DDD) Estimation of the Impact of 1970 Legalizations

This strategy is also used by Joyce (2004) to investigate the impact of legalized abortion on the crime rate in the United States. The identifying assumption is that there is no distinct contemporaneous shock that affects the non-marital birth rates of teenagers affected by legalized abortion in the repeal states relative to those of teens who were not in the same state and year.

---

30 Our DDD specification is comparable to one of DGL’s specifications in which they use state*time interactions.
In Section 5.1 we explained how this estimator can be calculated for 15 year-olds who were born in 1971 and thus affected by the 1970 legalizations using 17 year olds as a comparison group. We can also compute the same DDD measure for the 16 and 17 years olds who were born in 1971 in the repeal states by using the 18 and 19 years olds as their respective comparison groups. Our final DDD estimate will be the average of these three measures. Note that using these particular cohorts and ages we observe the birth rates for 15 and 17 year-olds, 16 and 18 year-olds, and 17 and 19 year-olds in the same years so that both age groups exposed to same period effects (1984-1988).

This estimator could also be illustrated as follows:

\[
DDD = \frac{(\Delta BR_{(r),jt} - \Delta BR_{(ar),jt}) - ((\Delta BR_{(r),j't} - \Delta BR_{(ar),j't}))}{DD_{\text{affected}} - DD_{\text{unaffected}}}
\]  

(10)

\{j, j'; t\} = \{(15;17;1984),(16;18;1985),(15;17;1986),(17;19;1986),(16;18;1987),(17;19;1988)\}

are the complete account of years and ages we use in the estimation. Further, \(\Delta BR_{jt}\) and \(\Delta BR_{j't}\) are the first differences in the mean non-marital birth rates in the repeal states for age groups j and j’ as described in (3b). \(\Delta BR_{r,jt}\) and \(\Delta BR_{r,j't}\) are the same first differences in the non-repeal states for age groups j and j’.

The stacked version of this DDD estimation can be estimated in the logarithmic form as running the following regression:
\[
\ln BR_{ijt} = \delta_0 + \delta_1 \text{AGEAFFEC TED}_j + \delta_2 (\text{AGEAFFEC TED}_j \times \text{POST70}_t) \\
+ \delta_3 (\text{AGEAFFEC TED}_j \times \text{REPEAL}_i) + \delta_4 (\text{REPEAL}_i \times \text{POST70}_t) \\
+ \delta_5 (\text{AGEAFFEC TED}_j \times \text{REPEAL}_i \times \text{POST70}_t) + s_i + y_t + e_{ijt}.
\] (11)

where \( \text{AGEAFFEC TED}_j \) is a dummy variable taking value of one for age group affected by legalized abortion and zero otherwise; \( \text{REPEAL}_i \) is the dummy variable equal to one if state \( i \) is among the early legalizing states; and \( \text{POST70}_t \) is a dummy being one for the post legalization years. Further, \( s_i \) and \( y_t \) are state and year dummies respectively.\(^{31}\) In equation (10) the coefficient of the second order interaction (\( \delta_5 \)) captures the \( DDD \) estimate while other terms in (10) can be thought of independent variables controlling for state, year, and state-year effects.\(^{32}\)

The first panel of Table 1.2 presents the \( DDD \) results for the total population, African-Americans and Whites respectively. They show that while there is no statistically significant reduction in non-marital teenage childbearing for Whites there is a nearly 5 percent statistically significant decline for the African-American cohorts born after abortion became legal in the repeal states in 1970.\(^{33}\) While these estimates are consistent with our \( DD \) estimate for Whites in the previous section, the effect for African-Americans declines to about 5 percent from our previous estimate of 10 percent but is still statistically significant.

\(^{31}\) Notice that in (13) we do not include state-year dummies since the other variables included in the regression controls for state specific trends. See Appendix 2.

\(^{32}\) For those unfamiliar with \( DDD \), it may not be obvious that \( \delta_5 \) is the parameter of interest. This is shown in Appendix 2.

\(^{33}\) Clearly, the \( DDD \) estimate for the total population is a weighted average of \( DDDs \) for different racial groups. Since there appears to be a different impact for African-American and White treatment groups, we only will discuss our findings for these groups and ignore the estimates for the total population.
### Table 1.2: DDD Estimation Results for the Total, African-American, and White Teenage Out-of-Wedlock Birth Rates

<table>
<thead>
<tr>
<th></th>
<th>Total</th>
<th>African-American</th>
<th>White</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>1970 Legalization</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$AGE_{AFFECTED_j} \times REPEAL_t \times POST70_t$</td>
<td>-.0461</td>
<td>-.0531</td>
<td>-.0355</td>
</tr>
<tr>
<td>(t= 1984,…,1988)</td>
<td>(.0244)*</td>
<td>(.0149)***</td>
<td>(.0367)</td>
</tr>
<tr>
<td><strong>1973 Legalization (Roe v. Wade)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$AGE_{AFFECTED_j} \times NONREPEAL_t \times POST73_t$</td>
<td>-0993</td>
<td>-1337</td>
<td>-0905</td>
</tr>
<tr>
<td>(t= 1987,…,1991)</td>
<td>(.0100)***</td>
<td>(.0384)***</td>
<td>(.0168)***</td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses; * significant at the 10% level; *** significant at the 1% level. All regressions are clustered at the state level.

### 1.5.4 Difference-in-Difference-in-Difference (DDD) Estimation of the Impact of Roe v. Wade

In 1973 the U.S. Supreme Court ruling in *Roe v. Wade* legalized abortion in the rest of the U.S. One may think of this important decision as a kind of treatment reversal. Although women living in the repeal states had access to abortion after *Roe v. Wade* and women in these states had a head start in terms of abortion access, one would still expect to see an impact for the non-repeal states relative to the repeal states considering the extent of the change in non-repeal states. The figures that we presented before suggest that while for Whites there is indeed a convergence in non-marital childbearing rates between repeal and non-repeal states, this is not so for African-Americans. But these trends might conceal the real impact of *Roe v. Wade*, since there are potentially many
factors that might affect the childbearing decision of the teens, and thus it possible to confound the shock affecting all the teenagers in a particular state preventing teenage birth rates from converging to their pre-legalization trends. In order to isolate such a shock, we again consider the DDD estimation strategy to investigate the long term impact of Roe v. Wade. As before the DDD estimate will be the average of DDD measures of 15, 16 and 17 years olds and can be written as follows

$$DDD_{Roe} = \frac{\Delta BR(t,j,t) - \Delta BR(\text{nr},j,t)}{DD_{\text{affected}}} - \frac{((\Delta BR(t,j',t) - \Delta BR(\text{nr},j',t))}{DD_{\text{unaffected}}}$$

(12)

where \(t\) is year, and \(j\) and \(j'\) are age subscripts such that \((j,j';t) = \{(15;17;1987),(16;18;1988),(15;17;1989),(17;19;1989),(16;18;1990),(17;19;1991)\}\) are the complete age and years used in the estimation. The stacked version of this DDD estimation can be estimated in the logarithmic form as running the following regression

$$\ln BR_{jt} = \beta_0 + \beta_1 AGEAFFECTED_j + \beta_2 (AGEAFFECTED_j \times POST73_i) + \beta_3 (AGEAFFECTED_j \times NONREPEAL_i) + \beta_4 (NONREPEAL_i \times POST73_i) + \beta_5 (AGEAFFECTED_j \times NONREPEAL_i \times POST73_i) + s_i + y_t + \omega_{jt}$$

(13)

\(AGEAFFECTED_j\) is again a dummy variable taking a value of one for age group exposed to legalized abortion after Roe v. Wade and of zero otherwise; \(NONREPEAL_i\) is a dummy variable equal to one if state \(i\) legalized abortion after Roe v. Wade; \(POST73_i\) is a dummy being one for the post-Roe years; \(s_i\) and \(y_t\) are state and year dummies respectively.
In equation (13) the coefficient $\beta_3$ provides the $DDD$ estimate. Since abortion was legal in the repeal states before *Roe v. Wade* and the abortion rates in those states were still increasing during this time period, our coefficient potentially will be biased downward. In other words, *Roe v. Wade* can be thought of as a treatment that should have only affected the cohorts that were born in the non-repeal states after *Roe v. Wade*. However, in the repeal states the impact was the ongoing impact of the 1970 legalizations but not *Roe v. Wade* itself. Hence, our estimate is potentially a lower bound of the true effect of legalized abortion for teens born after *Roe v. Wade* in the non-repeal states.

The second panel of Table 2 presents $DDD$ estimates for the long-term impact of *Roe v. Wade* in the non-repeal states. As discussed before, abortion was legal in the repeal states before the *Roe v. Wade*, and the abortion rates in those states were still increasing during this time period. Hence, our estimates will be biased downward potentially. Surprisingly, our coefficients are large and statistically significant for both African-Americans and Whites. There are 13 percent and 9 percent declines respectively in the teenage out-of-wedlock birth rates for African-Americans and Whites affected by *Roe v. Wade*. Considering that these relatively large estimates are potentially lower bounds for the true effects of *Roe v. Wade*, one might conclude that it had a substantial impact on the teenage out-of-wedlock childbearing behavior of both African-Americans and Whites. $^{34}$

$^{34}$ To check the robustness of our results, we also used all teenage births in another specification and obtained very similar estimates.
1.6 Discussion

Our results in the previous section can be summarized as follows. First, abortion in 1970 had a significant impact on the childbearing behavior of African-American teens who were affected by the law change. However, the estimated impact declined by half when we control for state-specific time trend and we find no effect for Whites living in the repeal states. Second, *Roe v. Wade* affected the birth rates of both groups living in the non-repeal states but the effect was larger for African-Americans. Third, the impact of *Roe v. Wade* in the non-repeal states was larger than that of the earlier legalization in the repeal states for both races.\(^{35}\)

Our findings have several noteworthy implications. Between 1994, the first year the increasing trend was reversed for Whites, and 2001 the decline in the out-of-wedlock birth rates among the 15-17 years olds was 24 percent for Whites. The same decline was 45 percent for African-Americans between 1991 and 2001. Our the *DDD* estimates of the impact of *Roe v. Wade* that control for state-specific time trends show that legalized abortion can potentially account for a little less than one third of this decline in the teenage out-of-wedlock childbearing among 15-17 years olds for African-Americans and a little more than one third of this decline for Whites in the 1990s. Finally, since the fertility of African-Americans appears to be affected by legalized abortion earlier, our results suggest a potential reason for why teen out-of-wedlock childbearing for African-Americans started declining 3 years before than as it did for Whites.

\(^{35}\) One of the potential explanations for this finding is that since we do not account for the residents of the non-repeal states that commuted to the repeal states to obtain an abortion between 1970 and 1973, our estimates for the impact of the 1970 legalization might be downward biased. In order to deal with this problem, in another specification we dropped the non-repeal states, which are adjacent to the repeal states, from our sample. Our estimates using this smaller sample are not significantly different from the results presented in Table 2.
1.7 Conclusion

In this paper, we attempt to bridge the gap between two different literatures in order to give a different perspective on and a better understanding of the one of the important trend changes in the early 1990’s. Teenage out-of-wedlock childbearing has been in decline for more than a decade now, and studies - with the exception of Levin (2001) and DGL - (2002), considering why this behavioral change occurred are non-existent in the academic literature.\(^{36}\) First, our DD estimation provides a test of whether it is reasonable to treat the early legalization in the repeal states as exogenous, and we find that the model passes this test. Our findings show that for African-Americans both the 1970 legalization in repeal states and *Roe v. Wade* lead to long-term reduction in out-of-wedlock teenage childbearing. For Whites, there is no evidence supporting a long-term effect of the 1970 legalizations, but the cohorts born after *Roe v. Wade* in the non-repeal states show a reduction in teenage out-of-wedlock childbirth. Our results also offer a potential explanation for why teenage out-of-wedlock childbearing for African-Americans started declining three years before that of Whites. Our the DDD estimates of *Roe v. Wade* that control for state-specific time trends show that legalized abortion can potentially account for about a little less than one third of this decline in the teenage out-of-wedlock childbearing among 15-17 years olds for African-Americans and a little more than one third of this decline for Whites in the 1990s.

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\(^{36}\) Darroch and Singh (1999) document changes in teen sexual behavior, but do not try to explain why the behavior changed.
CHAPTER 2

2. ESTIMATING THE EFFECT OF POLICY CHANGES ON PARTICIPATION IN MEDICAID WHILE ALLOWING FOR HETEROGENEOUS TREATMENT EFFECTS BASED ON OBSERVABLES AND UNOBSERVABLES

2.1 Introduction

Participation in means-tested social programs such as Medicaid represents a significant research area in public economics, health economics and labor economics. An increasingly common way of modeling such participation is to estimate a linear probability model of participation where a dummy variable for eligibility for the program is an endogenous explanatory variable.\(^{37}\) An implication of this model is that the estimated effect of becoming eligible on the probability of participation is restricted to be constant across individuals. However, there are several reasons to believe that participation conditional on eligibility is not constant. If this is the case, using this approach can lead to misleading predictions of the effects of expanding eligibility if newly eligible individuals differ from the previously eligible in ways that affect take-up. In addition, this approach does not allow researchers to determine which groups have low participation.

rates of participation given eligibility, an important part of accurately targeting policies such as outreach programs.

In this paper we explore two approaches to allowing the effect of eligibility on program participation to differ across groups. The first incorporates interactions between eligibility and demographic variables in the now standard linear probability model approach. This method accounts for observable differences between the newly eligible and the previously eligible, is easy to use and interpret, and does not require a distributional assumption. The second method is based on a switching probit model. It allows newly eligible individuals to differ from those previously eligible in terms of unobserved as well as observed characteristics while addressing some technical problems that arise in the simultaneous equation linear probability model, albeit at the cost of making distributional assumptions. We show for both models how one can use the parameter estimates to calculate separate take-up rates across different demographic groups and the take-up rate among those made newly eligible by a policy change. While our emphasis is on Medicaid take-up, our approach is applicable more broadly. For example, it can be used to examine participation in the State Children’s Health Insurance Program (SCHIP), the crowding out of private insurance by public insurance, and take-up of other social programs such as Food Stamps and Section 8 housing.

The late 1980’s and early 1990’s were an era of significant changes in the legislative environment towards Medicaid. For the first time, eligibility for Medicaid among low-income children and pregnant women did not require that the individual also be eligible for Aid to Families with Dependent Children (AFDC). Family structure criteria for eligibility were loosened, and income limits for Medicaid eligibility were set
at levels far above AFDC levels. With these changes in eligibility criteria, about 30 percent of children aged 0 to 18 years had become eligible for public health insurance by 1996, and only about half of these children were from welfare-eligible families (Selden, Banthin, and Cohen 1998). These expansions of Medicaid eligibility for children outside the traditional welfare population raise the question of how newly eligible families respond to the policy change in terms of Medicaid participation, and how their coverage by private insurance is affected (crowding out).

In the last decade a number of studies have examined these issues. All of those studies used the form or timing of the expansions, and the fact that some groups were affected by the policy changes while others were not, to disentangle the effects of Medicaid expansions on the health insurance coverage of individuals. Although the identification strategies of the previous papers are similar in this very fundamental aspect, the methods used and the estimates obtained differ considerably. Some of the most important studies in this literature treat Medicaid eligibility as endogenous in an instrumental variable, linear probability model, where the instrument varies only due to the legislative amendments over the sample period. Although this method is simple and the coefficients are easy to interpret, it ignores the fact that different groups are likely to respond differently to a change in eligibility status. In particular, the reaction may differ across demographic groups, since over time the policy changes are making higher income families eligible.

Using data from the Survey of Income and Program Participation (SIPP) and exploiting the exogenous policy changes that took place in late 1980’s and early 1990’s, we estimate average take-up rates and the take-up response of different demographic
groups according to race, family structure, education and number of earners in the family using the two different methods described above. Our results suggest that groups differ substantially in their take-up rates under both the existing program and a counterfactual expansion of the program. While the fact that there are differences may not seem surprising, we note that we can quantify the differences in take-up across groups. The magnitude of such differences is not well known and has heretofore not been estimated in the literature. Further, both of our suggested approaches have the advantage that they can predict different take-up rates for different expansions, since different expansions will affect different parts of the population. The standard linear probability model approach is constrained to predict the same take-up rates for different expansions.

The plan of the paper is as follows. In the next section we review the Medicaid program and the previous studies that have used the two-equation linear probability model approach to estimate the take-up response to the Medicaid expansions. In Section 3, we outline this standard approach to public health insurance take-up and explain how one can extend it using: i) a linear probability model with interactions between eligibility and demographic variables and ii) a switching probit model. We then show how these extensions can be used to estimate average take-up rates for different groups. Next, we show how to use the parameter estimates to predict the response to any expansion in the coverage of the program. After discussing our data briefly in Section 4, we present our results in Section 5. Section 6 concludes the paper.

2.2 Medicaid Expansions and Previous Literature

Medicaid was first established as a public health insurance program for welfare recipients and low-income aged and disabled individuals. This focus largely remained
until the late 1980s, when expansions in eligibility first permitted, and then required, states to cover pregnant women and children with family incomes that made them ineligible for cash welfare. Following the federally-mandated eligibility expansions of 1989 and 1990, states were required to cover children age 6 or younger with family income up to 133 percent of the poverty line and children born after September 30, 1983 with family income up to 100 percent of the poverty line. States were also given the option to increase their eligibility thresholds up to 185 percent of the poverty line. As these eligibility limits were far more generous than the eligibility limits applying to AFDC, the link between Medicaid eligibility and AFDC eligibility greatly diminished for young, low-income children. By 1996, of the approximately 30 percent of children age 19 and younger who were eligible for Medicaid, only about half came from typically welfare-enrolled families (Selden, Banthin, and Cohen 1998). While families who enrolled in cash welfare programs were also automatically enrolled in Medicaid, newly eligible children were not. Consequently the establishment of new Medicaid eligibility raised an important policy question: to what extent did expanded eligibility lead to increased health insurance coverage for the targeted population of children?

There has been a substantial amount of research on this question, and a non-exhaustive list includes Currie and Gruber (1996a, 1996b), Cutler and Gruber (1996), Dubay and Kenney (1996), Thorpe and Florence (1998), Yazici and Kaestner (1999), Shore-Sheppard (2000), Blumberg, Dubay, and Norton (2000), Ham and Shore-Sheppard (2005), and Shore-Sheppard (2005)). Further, there is also research on the related question of how the further public health insurance expansions of the State Children’s Health Insurance Program (SCHIP) affected coverage (LoSasso and Buchmueller (2004),
Hudson, Selden, and Banthin (2005)). Since our aim in this paper is to suggest an econometrically preferable alternative to the approach used by a particular group of studies, rather than summarizing the literature, we focus on two of the studies that use this now standard approach.

An important study using this approach was the seminal paper of Cutler and Gruber (1996). Cutler and Gruber used the method outlined below in Section 3.1 and data on children from the March Current Population Survey (CPS) from 1988 to 1993 to estimate the effect of imputed Medicaid eligibility on insurance status, controlling for demographics and state and year effects. They used an instrumental variables approach since eligibility is likely to be endogenous. This potential endogeneity arises for several reasons. First, unobservable factors affecting eligibility are likely to be correlated with unobservable individual and family characteristics that determine take-up. Second, eligibility may proxy family income if income, which is also likely to be endogenous, is not included as an independent variable. Finally, parental wages, which in turn determine eligibility, are likely to be correlated with fringe benefits (including private health insurance) of the parents. These benefits are unobserved and must be treated as part of the error term, and will be correlated with eligibility, thus necessitating treating eligibility as endogenous.

As a solution to the problem of the endogeneity of the eligibility variable, Cutler and Gruber (1996) suggested an instrument, $\text{FRACELIG}$, that is the fraction of a random sample of 300 children of each age imputed to be eligible according to the rules in each state in each year. This instrument, which is essentially an index of the expansiveness of Medicaid eligibility for each age group in each state and year, is correlated with
individual eligibility for Medicaid but not otherwise correlated with the demand for insurance. They estimated that the average take-up rate for those affected by the expansions was 23.5 percent.

Ham and Shore-Sheppard (2005) used data from Survey of Income and Program Participation (SIPP) covering the period from October 1985 to August 1995 to replicate Cutler and Gruber’s analysis and found a smaller average take-up rate of 11.8 percent for those affected by the expansions. They attributed some of the differences in their results to different samples and recall periods in the data sets used. Ham and Shore-Sheppard slightly modified the Cutler-Gruber instrument $FRACELIG$, in their study by using all sample observations of children of a given age in a SIPP wave except for those from the state for which the instrument is being calculated. Since this instrument is created using a larger sample, it is theoretically superior to the version using a random sample; however in practice it made no difference to the results. We use the data and instrument of Ham and Shore-Sheppard in our empirical application of the approaches described in the next section.

2.3 Econometric Methodology

2.3.1 Standard Approach to Estimation

The standard evaluation of eligibility changes in the public health insurance literature involves estimating the following econometric model using a linear probability model. The index function for participation is given by

$38$ Of course, one has to assume that changes in a state’s Medicaid provisions are not correlated with changes in the state’s availability of private insurance (which are unobservable to the researcher).
\[ Part_i^* = X_i \beta + \gamma \text{elig}_i + u_i, \quad (1) \]

where \( X_i \) is a vector of demographic variables for child \( i \), \( \text{elig}_i \) is a dummy variable coded one if the child is eligible for a program and zero otherwise, and \( u_i \) is an error term. A child participates (\( part_i = 1 \)) if \( Part_i^* > 0 \). The index function for eligibility is given by

\[ \text{Elig}_i^* = Z_i \delta + e_i, \quad (2) \]

where \( Z_i = (X_i, z_i) \) and \( z_i \) is a variable that affects eligibility but not participation conditional on eligibility. An example of such a variable is the Cutler-Gruber instrument \( FRACELIG_i \) described above. For our purposes, the crucial issue is that in this model the change in the probability of participating from becoming eligible is a constant equal to \( \gamma \).

It is unlikely that this change in probability is actually constant for all individuals. For example, it is clear from the discussion in Cutler and Gruber (1996) that they have in mind a random coefficients model where the index function for participation is given by

\[
\begin{align*}
Part_i^* &= X_i \beta + \gamma \text{elig}_i + u_i \\
&= X_i \beta + \gamma \text{elig}_i + (\gamma_i - \gamma)\text{elig}_i + u_i \\
&= X_i \beta + \gamma \text{elig}_i + v_i. \quad (1')
\end{align*}
\]
As Cutler and Gruber note, the IV estimate of $\gamma$ will be the average of the $\gamma_i$ over the variation in their instrument $z_i$. While this average parameter provides a useful summary of a marginal policy change, it will be less useful for larger, nonmarginal, changes in eligibility since one would need an average of $\gamma_i$ over those affected by the change.

2.3.2 A Simple but Powerful Alternative: Linear Probability Model with Interactions (LPMI)

As an alternative, we suggest estimating the model

$$Part_i^* = X_i\beta + (X_i elig_i)\theta + u_i.$$  \hfill (3)

In (3) the natural instruments are $X_i$ and interactions between $z_i$ and $X_i$. We can use this model to calculate the average take-up rates (i.e. the probability of participation for $eligible_i = 1$ minus the probability of participation for $eligible_i = 0$) among different demographic groups by simply taking the mean of the following expression for eligible individuals in group $j$

$$\widehat{ATR}_{ij}^{lpmi} = \frac{\sum_{i \in j} X_i \hat{\theta}}{N_j},$$  \hfill (4)

where $N_j$ is the number of eligible individuals in group $j$ and $\hat{\theta}$ is the two stage least squares estimate of $\theta$. Since the measure in (4) is a linear sum of regression coefficients,
one can calculate the standard error of the measure in a straightforward manner using a standard statistical package, such as Stata.\textsuperscript{39} (Details on this calculation are given in the Appendix.)

In addition to calculating the average take-up response by different groups to the existing expansion, it is possible to estimate the take-up response to further policy expansions that make a new group of children eligible. First, one would determine which children become newly eligible under the expansion. Then a natural means of measuring the take-up rate among the newly eligible is

$$\hat{\tau}_{i_{\text{new}}} = \frac{\sum X_i \hat{\theta}}{N_{\text{new}}}, \quad (5)$$

where the sum is over the newly eligible children and $N_{\text{new}}$ is the number of newly eligible children. A standard error for the measure in (5) is obtained analogously to the standard error for the measure in (4). This measure takes into account the fact that the newly eligible have different demographic variables than the previously eligible children, but not the fact that they will have nonrandom values of $u_i$. We can also calculate this average take-up rate for subsets of the newly eligible, e.g. those with zero, one, or more than one earner per family.

Before concluding this section, we note four important problems with the standard approach (and our extension of it). First, it allows for a non-zero probability of participation even if the child is ineligible. Second, while it can account for the fact that

\textsuperscript{39} Our Stata programs to implement both approaches suggested in the paper will be available beginning February 1, 2007 at http://lanfiles.williams.edu/~lshore/ and http://www.rcf.usc.edu/~johnham/.
in the policy experiment the newly eligible will differ from the previously eligible in
terms of observables, it cannot address the fact that the newly eligible will also differ
from the previously eligible in terms of unobservables. Third, the estimated take-up rate
for an individual or group may be less than zero or greater than one, and we find that both
problems occur in our estimates below. Fourth, there is a technical problem with the
bivariate probit version of the standard model (1) and (2), and this also occurs in our
extended model based on (3). Assuming normality, the log likelihood for the model based
on (1) and (2) is

\[
L = \sum_{i=1}^{N} \log \Phi_{2}(q_{ii}(X_i \beta + \gamma \text{elig}_i), q_{2i}Z_i \delta, q_{ii}q_{2i}\tilde{\rho}),
\]

(6)

where \( \Phi_{2}(\bullet, \bullet, \tilde{\rho}) \) is the cumulative bivariate normal distribution function, \( q_{ii} = 2part_i - 1 \)
and \( q_{2i} = 2\text{elig}_i - 1 \). If there are no misclassification errors, i.e. there is no one classified
as ineligible who is seen to participate, this likelihood is maximized by letting the
intercept in the participation equation go to a large negative number approaching minus
infinity and letting the coefficient on eligibility go to minus this large negative number
plus a constant. In other words, the likelihood function for the simultaneous equation
probit model is unbounded if there is no misclassification, although one can identify all
the parameters besides the intercept. Since this issue would also occur in our extended
model (3), some researchers may find this to be a troubling aspect of the standard model.
Others may feel that since this last problem arises only in the probit version of the model,
and not in the linear probability model itself, it can be ignored.\footnote{However, note that linear probability estimates of (1) or (1)' can achieve a perfect fit (in the absence of classification error) for the non-eligible by setting $\beta = 0$.} However, even in that case the first three problems remain. We now turn to an approach that addresses all four of these problems, albeit at the cost of making a distributional assumption.

### 2.3.3 An Alternative Approach to Estimation: Switching Probit Model (SPM)

To avoid the econometric problems we use a switching model (Quandt 1958, 1960, 1972), which has been applied in the bivariate probit with selection case by van de Ven and van Praag (1981).\footnote{This model is also referred to as a bivariate probit model with sample selection in the econometrics literature.} The index function for eligibility is still (2), and $\text{elig}_i = 1$ if $\text{Elig}_i^* > 0$. However, we specify that the probability of participation given non-eligibility is zero. Further, for a randomly chosen individual, the index function for participation given eligibility is given by

\[ \text{Part}_i^* = X_i \mu + \varepsilon_i, \quad (7) \]

where $(\varepsilon_i, \varepsilon_i') \sim \text{iid} \mathcal{N}(0, \tilde{V})$ and $\tilde{V} = \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix}$.\footnote{Following the econometrics literature, it is natural to write the index function for take-up for a randomly chosen (in terms of $\varepsilon_i$) child. However, it is worth emphasizing that unless $\rho = 0$, those eligible will not be a randomly chosen subgroup of the population. Below we take this into account in calculating take-up rates.} The appropriate log likelihood is

\[
L = \sum_{\text{Elig}=1, \text{Part}=1} \log \Phi_2(X_i \mu, Z_i \delta, \rho) + \sum_{\text{Elig}=1, \text{Part}=0} \log \Phi_2(-X_i \mu, Z_i \delta, -\rho) \\
+ \sum_{\text{Elig}=0} \log \Phi_1(-Z_i \delta).
\]
As in the case of the linear probability model with interactions, we can look at the
predicted take-up rates among the eligible individuals in group j by calculating

\[
\frac{\hat{ATR}^j}{N_j} = \left[ \sum_{i=0}^{1-j} \Phi_i(-X_{ij} \hat{\mu}) \right] / N_j, 
\]  

(9)

where again \( N_j \) is the number of eligible individuals in group j and \( \hat{\mu} \) is the maximum
likelihood estimate of \( \mu \). The delta method can be used to calculate the standard errors
for these predicted take-up rates, again using a program like Stata. (Details about this
calculation are given in the Appendix.) To compute the participation responses of
different demographic groups, the process can simply be repeated for each group of
interest.

The above calculation does not take into account the fact that the eligible children
will not be a random sample in terms of the error \( \epsilon_i \) in the take-up index function (7). To
account for this, we use

\[
\frac{\hat{ATR}^j}{N_j} = \frac{1}{N_j} \sum_{i=0}^{1-j} \Pr(P_i^* \geq 0, E_i^* \geq 0) = \frac{1}{N_j} \sum_{i=0}^{1-j} \frac{\Pr(P_i^* \geq 0, E_i^* \geq 0)}{\Pr(E_i^* \geq 0)} = 
\]

\[
\frac{1}{N_j} \sum_{i=0}^{1-j} \frac{\Phi_2(X_{ij} \hat{\mu}, Z_i \hat{\delta}, \hat{\rho})}{\Phi_1(Z_i \hat{\delta})}, 
\]  

(10)

where \( \hat{\delta} \) and \( \hat{\rho} \) are the maximum likelihood estimators of \( \delta \) and \( \rho \) respectively.
To calculate the take-up rate among the newly eligible when we do not account for the fact that they will not be a random sample (in terms of $\varepsilon_i$) of the population, we use

$$\hat{T}_{\text{new}} = \frac{\sum_{i \in \text{new}} 1 - \Phi(X_i, \hat{\mu})}{N_{\text{new}}},$$

where again $N_{\text{new}}$ is the number of newly eligible individuals. However, one can improve the prediction of take-up among the newly eligible by also taking into account the fact that they will not be a random sample in terms of the unobservable $\varepsilon_i$. To do this, we calculate

$$\hat{T}_{\text{new}} = \frac{1}{N_{\text{new}}} \sum_{i \in \text{New}} \Pr(P_i^* \geq 0 \mid E_i^* \geq 0) = \frac{1}{N_{\text{new}}} \sum_{i \in \text{New}} \frac{\Pr(P_i^* \geq 0, E_i^* \geq 0)}{\Pr(E_i^* \geq 0)} = \frac{1}{N_{\text{new}}} \sum_{i \in \text{New}} \frac{\Phi(Z_i, \hat{\delta})}{\Phi(Z_i, \hat{\delta})}.\quad (12)$$

While the estimation of the parameters in (8), and the calculation of the policy effects and the respective standard errors in (9) through (12), may seem somewhat daunting, we emphasize that it can all be carried out in a program such as Stata, and contained in the Appendix.
2.4 Data

We use data from the Survey of Income and Program Participation (SIPP). SIPP is a nationally representative longitudinal household survey, which is specifically designed to collect detailed income and program participation information. It has a triannual feature such that the recall period between each interview is four months for every individual. For the time period covered in this paper (from October 1985 to August 1995), the length of the panels varies from 24 months for the 1988 panel to 40 months for the 1992 panel. Although the sample universe is the entire U.S., the Census Bureau does not separately identify state of residence for residents of nine low population states. Since state of residence information is critical for us to impute Medicaid eligibility, we drop all individuals whose state of residence information is not identified. We also restrict our sample to children living in original-sample households who are younger than 16 years old at the first time they are observed. Furthermore, we drop children who are observed only once, children who leave the sample and then return and children who move between states during the sample period for comparability with earlier studies. (In total, these observations constitute less than 8 percent of the sample.)

Although the four-month period increases the probability of accurate reporting, particularly relative to the fifteen-month recall period of the March Current Population Survey, the SIPP suffers from the problem of “seam bias.” Census Bureau researchers have shown that there are a disproportionate number of transitions between the last month of the wave and the first month of the next wave (see, e.g., Young 1989, Marquis

---

43 In total we used 7 panels from 1986 to 1993. The 1989 panel is not used because it was ended after only three waves. 
44 Bennefield (1996) finds that health insurance coverage in the early 1990s is measured more accurately in the SIPP than in the CPS, due in part to the shorter recall period.
and Moore 1990). Because of this seam bias problem, we estimate our models using only
the fourth month of the each wave, dropping the first three months. While this approach
has the disadvantage that information on the months other than the fourth month is lost,
the advantage is that the data in the fourth month of each wave may be the most likely to
be accurate since it is closest to the time of interview.

We impute eligibility in four steps. First, we construct the family unit relevant for
Medicaid program participation and determine family income. Second, we assign family-
specific poverty thresholds based on the size of the family and the year. Since Medicaid
eligibility results from AFDC eligibility, we then use information on the family income
and family structure, along with the AFDC parameters in effect in the state and year, to
impute eligibility for AFDC.\footnote{Families must pass two income tests to receive AFDC, the “gross test,” which requires that a family’s
gross income be less than 1.85 times the state’s need standard, and the “net test,” which requires that a
family’s income after disregards be less than the state’s payment standard. In determining AFDC
eligibility, families are permitted to disregard actual childcare expenses up to a maximum. Since we do not
know actual child care expenses, we assume that families deduct the full disregard for all children under
age 6, but nothing for older children. These assumptions were also made by Cutler and Gruber (1996).}
Finally, we assign Medicaid eligibility if any of the
following conditions hold: the child is in an AFDC-eligible family; the child is income
eligible for AFDC and either lives in a state with a “Ribicoff program” or lives in a state
with an AFDC-UP program and has an unemployed parent; or the child’s family income
as a percent of the relevant poverty line is below the Medicaid expansion income
eligibility cutoff in effect for that age child in his or her state of residence at that time.

In Table 2.1 we present the sample means for the variables used in our
regressions.\footnote{These sample means have not been weighted, so they should not be considered to be representative of the
nation.} Consistent with national trends, Medicaid coverage is higher in our sample
in later panels. Furthermore, starting with the 1990 panel, which covers the period 1990-
1992, there is a significant increase in our policy instrument $F R A C E L I G_i$ (discussed in
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Medicaid</td>
<td>0.1195</td>
<td>0.1158</td>
<td>0.1138</td>
<td>0.1619</td>
<td>0.1669</td>
<td>0.1810</td>
<td>0.2001</td>
</tr>
<tr>
<td>Imputed Eligibility</td>
<td>0.1871</td>
<td>0.1755</td>
<td>0.1792</td>
<td>0.2757</td>
<td>0.2953</td>
<td>0.3125</td>
<td>0.3341</td>
</tr>
<tr>
<td>White</td>
<td>0.8280</td>
<td>0.8301</td>
<td>0.8248</td>
<td>0.7817</td>
<td>0.8152</td>
<td>0.8047</td>
<td>0.8109</td>
</tr>
<tr>
<td>Male</td>
<td>0.5084</td>
<td>0.5154</td>
<td>0.5090</td>
<td>0.5120</td>
<td>0.5129</td>
<td>0.5201</td>
<td>0.5148</td>
</tr>
<tr>
<td>Two Parents</td>
<td>0.7585</td>
<td>0.7678</td>
<td>0.7677</td>
<td>0.7104</td>
<td>0.7439</td>
<td>0.7277</td>
<td>0.7335</td>
</tr>
<tr>
<td>Male Head Only</td>
<td>0.0220</td>
<td>0.0234</td>
<td>0.0186</td>
<td>0.0267</td>
<td>0.0289</td>
<td>0.0262</td>
<td>0.0212</td>
</tr>
<tr>
<td>No Earners</td>
<td>0.1402</td>
<td>0.1310</td>
<td>0.1235</td>
<td>0.1593</td>
<td>0.1539</td>
<td>0.1561</td>
<td>0.1619</td>
</tr>
<tr>
<td>One Earner</td>
<td>0.4109</td>
<td>0.4163</td>
<td>0.4188</td>
<td>0.4234</td>
<td>0.4204</td>
<td>0.4132</td>
<td>0.4065</td>
</tr>
<tr>
<td>Two Earners</td>
<td>0.3819</td>
<td>0.3901</td>
<td>0.3991</td>
<td>0.3677</td>
<td>0.3774</td>
<td>0.3816</td>
<td>0.3844</td>
</tr>
<tr>
<td>Age of Highest Earner</td>
<td>36.56</td>
<td>36.56</td>
<td>36.58</td>
<td>36.75</td>
<td>36.98</td>
<td>36.97</td>
<td>37.18</td>
</tr>
<tr>
<td>Education of Highest Earner</td>
<td>12.68</td>
<td>12.69</td>
<td>12.85</td>
<td>12.74</td>
<td>12.92</td>
<td>12.95</td>
<td>12.95</td>
</tr>
<tr>
<td>FRACELIG</td>
<td>0.1959</td>
<td>0.1861</td>
<td>0.1921</td>
<td>0.2823</td>
<td>0.3022</td>
<td>0.3208</td>
<td>0.3375</td>
</tr>
<tr>
<td>Years Covered</td>
<td>86-88</td>
<td>87-89</td>
<td>88-89</td>
<td>90-92</td>
<td>91-93</td>
<td>92-95</td>
<td>93-95</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>44016</td>
<td>45691</td>
<td>40895</td>
<td>99446</td>
<td>66991</td>
<td>108572</td>
<td>101967</td>
</tr>
</tbody>
</table>

Notes: Unweighted means from SIPP panels noted above. See text for description of sample construction.

**Table 2.1: Means of Variables Used in Estimation**
Section 2) capturing eligibility expansions at the federal level occurring during this time period. In addition to the variables in Table 2.1, we also use state, year and age dummies for each child in the $X_i$ vector in (3) and in our index functions (2) and (7) to control for the possible state-specific, age-specific and year-specific unobservables that could influence participating in Medicaid. Finally, since we use longitudinal data, we cluster the standard errors to account for dependency between person-specific observations.

2.5 Results

2.5.1 Parameter Estimates

Tables 2.2 and 2.3 show our parameter estimates for the linear probability model with interactions (LPMI) and the switching probit model (SPM). The two sets of estimates generally tell the same story. (The base case is a non-white female child in a female-headed family with more than two earners.) However, it is interesting to note that the effect of being white on take-up is not statistically significant in the LPMI while it is very significant and negative in the SPM, and that the normal statistics are substantially higher for the SPM.\[47\] In terms of the other coefficients, for both sets of estimates, the probability of take-up is significantly smaller than the base case when there is a male head, two parents present, the head is older and the head has more education, while it is significantly larger than the base case when there are fewer earners. The sex of the child does not affect the probability of take-up. In the LPMI the interaction terms for the demographics are jointly significant, and in the SPM the demographic variables in the participation or take-up equation (7) are jointly significant. It is worth stressing that even

\[47\] The higher normal statistics will reflect the normality assumption, and the fact that the LPMI will only use data on marginal individuals (in a local instrumental variables sense), while the SPM basically uses all of the data. Since the normality assumption cannot readily be justified on economic grounds, care must be exercised in considering these higher normal statistics as a plus for the SPM.
<table>
<thead>
<tr>
<th>Term</th>
<th>LPMI</th>
</tr>
</thead>
<tbody>
<tr>
<td>Eligibility</td>
<td>0.0662</td>
</tr>
<tr>
<td></td>
<td>(0.1086)</td>
</tr>
<tr>
<td>Eligibility*Size of Household</td>
<td>0.0337</td>
</tr>
<tr>
<td></td>
<td>(0.0056)**</td>
</tr>
<tr>
<td>Eligibility*White</td>
<td>0.0143</td>
</tr>
<tr>
<td></td>
<td>(0.0150)</td>
</tr>
<tr>
<td>Eligibility*Male</td>
<td>-0.0073</td>
</tr>
<tr>
<td></td>
<td>(0.0105)</td>
</tr>
<tr>
<td>Eligibility*Two parents</td>
<td>-0.1360</td>
</tr>
<tr>
<td></td>
<td>(0.0170)**</td>
</tr>
<tr>
<td>Eligibility*Male head only</td>
<td>-0.0962</td>
</tr>
<tr>
<td></td>
<td>(0.0278)**</td>
</tr>
<tr>
<td>Eligibility*No earners</td>
<td>0.3287</td>
</tr>
<tr>
<td></td>
<td>(0.0445)**</td>
</tr>
<tr>
<td>Eligibility*One earner</td>
<td>0.2497</td>
</tr>
<tr>
<td></td>
<td>(0.0347)**</td>
</tr>
<tr>
<td>Eligibility*Two earners</td>
<td>0.1325</td>
</tr>
<tr>
<td></td>
<td>(0.0331)**</td>
</tr>
<tr>
<td>Eligibility*Higher earner’s age</td>
<td>-0.0027</td>
</tr>
<tr>
<td></td>
<td>(0.0010)**</td>
</tr>
<tr>
<td>Eligibility*Higher earner’s education</td>
<td>-0.0184</td>
</tr>
<tr>
<td></td>
<td>(0.0025)**</td>
</tr>
</tbody>
</table>

Notes: All regressions include demographic main effects, year, age, state dummies, eligibility-year, eligibility-age and eligibility-state interactions. Standard errors have been corrected for repeated observations within individuals. *** denotes significantly different from zero at the 1% level of significance.

**Table 2.2: Estimates for Medicaid Participation from IV Linear Probability Model with Interactions**
<table>
<thead>
<tr>
<th></th>
<th>Participation (1)</th>
<th>Eligibility (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Size of Household</td>
<td>0.1508</td>
<td>0.2739</td>
</tr>
<tr>
<td></td>
<td>(0.0063)***</td>
<td>(0.0049)***</td>
</tr>
<tr>
<td>White</td>
<td>-0.3378</td>
<td>-0.2837</td>
</tr>
<tr>
<td></td>
<td>(0.0185)***</td>
<td>(0.0133)***</td>
</tr>
<tr>
<td>Male</td>
<td>0.0055</td>
<td>-0.0145</td>
</tr>
<tr>
<td></td>
<td>(0.0153)</td>
<td>(0.0104)</td>
</tr>
<tr>
<td>Two parents</td>
<td>-0.8578</td>
<td>-0.8488</td>
</tr>
<tr>
<td></td>
<td>(0.0212)***</td>
<td>(0.0156)***</td>
</tr>
<tr>
<td>Male head only</td>
<td>-0.7628</td>
<td>-0.2355</td>
</tr>
<tr>
<td></td>
<td>(0.0443)***</td>
<td>(0.0293)***</td>
</tr>
<tr>
<td>No earners</td>
<td>1.9092</td>
<td>2.4210</td>
</tr>
<tr>
<td></td>
<td>(0.0689)***</td>
<td>(0.0321)***</td>
</tr>
<tr>
<td>One earner</td>
<td>0.7761</td>
<td>0.9698</td>
</tr>
<tr>
<td></td>
<td>(0.0650)***</td>
<td>(0.0289)***</td>
</tr>
<tr>
<td>Two earners</td>
<td>0.4208</td>
<td>0.1173</td>
</tr>
<tr>
<td></td>
<td>(0.0639)***</td>
<td>(0.0285)***</td>
</tr>
<tr>
<td>Higher earner’s age</td>
<td>-0.0176</td>
<td>-0.0320</td>
</tr>
<tr>
<td></td>
<td>(0.0011)***</td>
<td>(0.0008)***</td>
</tr>
<tr>
<td>Higher earner’s education</td>
<td>-0.0739</td>
<td>-0.1474</td>
</tr>
<tr>
<td></td>
<td>(0.0033)***</td>
<td>(0.0020)***</td>
</tr>
<tr>
<td>FRACELIG</td>
<td>-</td>
<td>0.4661</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.0060)***</td>
</tr>
</tbody>
</table>

Notes: All regressions include year, age, and state dummies. Standard errors have been corrected for repeated observations within individuals.

*** denotes significantly different from zero at the 1% level of significance.

Table 2.3: Estimates for Medicaid Eligibility and Participation from Switching Probit Model
with very rich models in terms of having state, year and child age dummies interactions with eligibility in the LPMI (3), and having state, year and child age dummies in the take-up equation (7) for the SPM, the effects of demographics on take-up are generally well identified and precisely estimated. Given that all estimation and calculation of the standard errors are carried out in Stata, using these richer models gives important information about take-up without causing undue computational difficulties or a loss of statistical precision (see column (1) of Tables 2.4 and 2.5 below). As noted above, researchers will not be surprised by the sign of the coefficients for the LPMI model. However, our analysis provides a quantitative measure of the differences in take-up rates across groups, and this is not currently available in the literature.

Column 1 of Table 2.4 gives the predicted average take-up rates using the LPMI for different groups. For each group, the estimates give the take-up rate for eligible individuals within the category. For example, for each white child, we calculate the difference between the predicted probability when \( \text{Elig}_i = 1 \) and \( \text{Elig}_i = 0 \), and then take the average of the difference across white children.\(^{48}\) All of the individual demographic groups have take-up rates that are estimated precisely and follow the pattern in Table 2.2. However, note that the comparisons in Table 2.4 answer a slightly different question than the parameter estimates in Table 2.2. The estimates in Table 2.2 show the effect on take-up of having a certain characteristic holding other characteristics constant, while those in Table 2.4 show the average probability of take-up for individuals with a certain characteristic given the other characteristics of those children. The estimate for families with more than two earners

\(^{48}\) Note that the individual error term in the take-up equation differences out of this equation.
<table>
<thead>
<tr>
<th></th>
<th>Linear Probability with Interactions (1)</th>
<th>Switching Probit Random Error Draw (2)</th>
<th>Switching Probit Non-Random Error Draw (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>All Population</td>
<td>0.2022</td>
<td>0.4771</td>
<td>0.5108</td>
</tr>
<tr>
<td></td>
<td>(0.0183)</td>
<td>(0.0044)</td>
<td>(0.0029)</td>
</tr>
<tr>
<td>Race</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White</td>
<td>0.1864</td>
<td>0.4044</td>
<td>0.4409</td>
</tr>
<tr>
<td></td>
<td>(0.0164)</td>
<td>(0.0048)</td>
<td>(0.0033)</td>
</tr>
<tr>
<td>Non-White</td>
<td>0.2393</td>
<td>0.6474</td>
<td>0.6744</td>
</tr>
<tr>
<td></td>
<td>(0.0267)</td>
<td>(0.0049)</td>
<td>(0.0041)</td>
</tr>
<tr>
<td>Education</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>High School Drop-out</td>
<td>0.2768</td>
<td>0.6009</td>
<td>0.6232</td>
</tr>
<tr>
<td></td>
<td>(0.0242)</td>
<td>(0.0040)</td>
<td>(0.0036)</td>
</tr>
<tr>
<td>High School Graduate</td>
<td>0.1909</td>
<td>0.4444</td>
<td>0.4816</td>
</tr>
<tr>
<td></td>
<td>(0.0171)</td>
<td>(0.0050)</td>
<td>(0.0031)</td>
</tr>
<tr>
<td>Some College</td>
<td>0.1418</td>
<td>0.3993</td>
<td>0.4413</td>
</tr>
<tr>
<td></td>
<td>(0.0173)</td>
<td>(0.0056)</td>
<td>(0.0036)</td>
</tr>
<tr>
<td>Family Structure</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female Head</td>
<td>0.2815</td>
<td>0.6957</td>
<td>0.7201</td>
</tr>
<tr>
<td></td>
<td>(0.0272)</td>
<td>(0.0044)</td>
<td>(0.0035)</td>
</tr>
<tr>
<td>Male Head</td>
<td>0.1315</td>
<td>0.2873</td>
<td>0.3230</td>
</tr>
<tr>
<td></td>
<td>(0.0292)</td>
<td>(0.0125)</td>
<td>(0.0131)</td>
</tr>
<tr>
<td>Two parents</td>
<td>0.1254</td>
<td>0.2652</td>
<td>0.3082</td>
</tr>
<tr>
<td></td>
<td>(0.0136)</td>
<td>(0.0055)</td>
<td>(0.0035)</td>
</tr>
<tr>
<td>Number of Earners</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No earner</td>
<td>0.2886</td>
<td>0.7795</td>
<td>0.7928</td>
</tr>
<tr>
<td></td>
<td>(0.0347)</td>
<td>(0.0035)</td>
<td>(0.0032)</td>
</tr>
<tr>
<td>One earner</td>
<td>0.1714</td>
<td>0.2800</td>
<td>0.3271</td>
</tr>
<tr>
<td></td>
<td>(0.0108)</td>
<td>(0.0063)</td>
<td>(0.0040)</td>
</tr>
<tr>
<td>Two earners</td>
<td>0.0153</td>
<td>0.1277</td>
<td>0.1860</td>
</tr>
<tr>
<td></td>
<td>(0.0220)</td>
<td>(0.0068)</td>
<td>(0.0053)</td>
</tr>
<tr>
<td>More than two earners</td>
<td>-0.1569</td>
<td>0.0757</td>
<td>0.1120</td>
</tr>
<tr>
<td></td>
<td>(0.0354)</td>
<td>(0.0086)</td>
<td>(0.0110)</td>
</tr>
</tbody>
</table>

Note: Standard errors in parentheses. All take-up rates are significantly different from zero at the 1% level.

Table 2.4: Average Take-up Rates from Linear Probability with Interactions and Switching Probit Models, 1995 Data
demonstrates that the LPMI can run into the problem of a negative estimated probability if a group has a very low probability of take-up. In column 2 of Table 2.4 we show the estimates of the average take-up rate from the SPM for each demographic group treating each child as having a random draw from the error term distribution. Note that the predicted take-up rates follow the same rankings across groups as in the LPMI, but they are much higher than the LPMI. Although we do not know of a formal treatment of local instrumental variables for the LPMI, we conjecture that the LPMI will estimate take-up rates for marginal children for periods when there was variation in coverage across states and age groups conditional on the other regressors, while the SPM takes the average across children over the whole sample period. Since our sample contains data from years before Medicaid eligibility was expanded when take-up rates were very high, it is not surprising that the SPM predicts much higher take-up rates than LPMI. In this situation the LPMI predictions are likely to be more appropriate for predicting expansions in 1995. Column 3 shows the average take-up rates for the SPM when we take into account the fact that the eligible children will not represent a random draw of the population (because of the correlation between the error in the eligibility index function and the take-up index function). Not surprisingly the take-up rates in column 3 are larger than those in column 2, although the probabilities in column 3 are only larger by .04 to .06.

49 In other cases, some individuals have a predicted probability of participation greater than 1.
50 One means of adjusting the SPM to account for the change in the Medicaid eligibility regimes would be to use data only from the early 1990s and on when estimating the SPM.
<table>
<thead>
<tr>
<th></th>
<th>Linear Probability With Interactions (1)</th>
<th>Switching Probit Random Error Draw (2)</th>
<th>Switching Probit Non-Random Error Draw (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>All Newly Eligible Population (985 Observations)</td>
<td>0.0877 (0.0315)</td>
<td>0.2179 (0.0076)</td>
<td>0.2834 (0.0057)</td>
</tr>
<tr>
<td>Race</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White</td>
<td>0.0848 (0.0315)</td>
<td>0.1916 (0.0074)</td>
<td>0.2556 (0.0057)</td>
</tr>
<tr>
<td>Non-White</td>
<td>0.0995 (0.0347)</td>
<td>0.3238 (0.0096)</td>
<td>0.3951 (0.0077)</td>
</tr>
<tr>
<td>Education</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>High School Drop-out</td>
<td>0.1315 (0.0332)</td>
<td>0.2886 (0.0077)</td>
<td>0.3484 (0.0066)</td>
</tr>
<tr>
<td>High School Graduate</td>
<td>0.1049 (0.0314)</td>
<td>0.2144 (0.0081)</td>
<td>0.2798 (0.0060)</td>
</tr>
<tr>
<td>Some College</td>
<td>0.0810 (0.0321)</td>
<td>0.2171 (0.0084)</td>
<td>0.2867 (0.0063)</td>
</tr>
<tr>
<td>Family Structure</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female Head</td>
<td>0.1677 (0.0344)</td>
<td>0.3979 (0.0108)</td>
<td>0.4785 (0.0082)</td>
</tr>
<tr>
<td>Male Head</td>
<td>0.0540 (0.0429)</td>
<td>0.1691 (0.0123)</td>
<td>0.2305 (0.0137)</td>
</tr>
<tr>
<td>Two parents</td>
<td>0.0598 (0.0318)</td>
<td>0.1516 (0.0068)</td>
<td>0.2114 (0.0056)</td>
</tr>
<tr>
<td>Number of Earners</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No earner</td>
<td>0.1457 (0.0489)</td>
<td>0.6280 (0.0088)</td>
<td>0.6609 (0.0080)</td>
</tr>
<tr>
<td>One earner</td>
<td>0.1264 (0.0302)</td>
<td>0.2309 (0.0084)</td>
<td>0.2997 (0.0063)</td>
</tr>
<tr>
<td>Two earners</td>
<td>0.0146 (0.0374)</td>
<td>0.1133 (0.0075)</td>
<td>0.1813 (0.0067)</td>
</tr>
<tr>
<td>More than two earners</td>
<td>-0.1764 (0.0488)</td>
<td>0.0489 (0.0073)</td>
<td>0.0908 (0.0104)</td>
</tr>
</tbody>
</table>

Note: Standard errors in parentheses. All take-up rates are significantly different from zero at the 1% level.

Table 2.5: Average Take-up Rates for Newly Eligible Population after Raising the Income Limits by 10%, 1995 Data
Table 2.5 shows the predicted effect on participation in Medicaid using our parameter estimates of those made newly eligible by a policy experiment: increasing the 1995 income limits by 10% for data from 1995. Again we do this using the LPMI, the SPM where we do not take into account the fact that the children are not a random sample in terms of their error term in the take-up index function, and the SPM where we do take into account the distribution of the error term among the newly eligible. For each case the probability of take-up in Table 2.5 is lower than the respective estimate in Table 2.4, reflecting the fact that the newly eligible have demographic variables that make take-up less likely than those who were eligible under the actual 1995 rules. The comparison of Tables 2.4 and 2.5 shows one of the significant advantages of the two approaches we explore here over the standard approach, since using the standard approach all one can do is use the average take-up rate estimated for the whole sample to predict the effect of the expansion. The approaches we investigate here both adjust for the fact that different expansions will affect children from different families and thus have different take-up rates. Note again that the predicted take-up rates are much higher for each group using the SPM, and that the LPMI model again runs into trouble in terms of a negative predicted probability for families with more than two earners.

2.6 Conclusions

In this paper we suggest two approaches to estimating (i) the average take-up rate across demographic groups and (ii) the take-up response to an expansion of a social program attributable to a policy change in eligibility. Both of our approaches, the linear

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51 The standard approach produces a take-up rate of 0.12. In principle this is closest to our estimate of the overall effect of the expansion using the LPMI, and we see that they are quite close also in practice.
52 These are not completely independent, since the average take-up for the LPMI will be an average over probabilities of which some are estimated to be negative and some estimated to be greater than 1.
probability model with interactions (LPMI) and the switching probit model (SPM), allow demographics to play a significant role in take-up rates. The two approaches have some costs and benefits relative to each other. The linear probability model with interactions does not require a distributional assumption on the error terms, while the switching probit model does not suffer from criticisms directed at the linear probability model such as positive participation probabilities for ineligible individuals and the possibility that participation probabilities of individuals lie outside the unit interval. The SPM also allows us to take into account the fact that distribution of the error term among the newly eligible under expansions of these rules are different from those previously eligible. On the other hand, the LPMI exploits variation in the data from a time which is more comparable to the expansion we consider, and thus is likely to provide more appropriate predictions of take-up under the expansion. Both approaches are easily implemented with Stata, produce precisely estimated effects, and will automatically adjust for the fact that different expansions will affect different children.

Using data from Medicaid and exploiting the exogenous policy changes that took place in the late 1980’s and early 1990’s, we estimate average take-up rates and the take-up response of different demographic groups according to race, family structure, education and number of earners in the family. Our results indicate that groups differ substantially in their take-up rates under both existing rules and an expansion of eligibility, and more importantly, we are able to quantify these differences, thus providing policy makers with important information that they can use to target groups with low participation rates in outreach campaigns to encourage participation.
BIBLIOGRAPHY


APPENDICES
A.1 Trends in Teenage Childbearing

In this appendix we present the basic trends for teenage childbearing in the United States. In Figure A1, we present the trend for adolescent childbearing in general, without making any distinction between births to unmarried or married mothers. We then consider the trend for teenage out-of-wedlock childbearing in Figure A2. Furthermore, because the general trend for all teenagers hides important differences between Whites and African-Americans in levels as well as in trends, we offer the respective figures for these two groups in Figure A3 and A4.

The rate of teenage childbearing in the United States has declined precipitously since the late 1950’s, except for a brief, but steep, rise in the late 1980’s through 1994. The birth rate peaked in 1957 at a rate of 96 births per 1000 women aged 15-19, and then declined to its lowest level of 37 births in 2001 (Ventura et al., 2001). This general trend is a reflection of the birth rate for Whites, who comprise majority of the population, but the corresponding trend for African-Americans is similar, and in fact the gap between the birth rates for the two races shrank over this period. In 1960, the African-American

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53 Throughout this paper we will be using the birth rate per 1000 women in a specified group as our childbearing measure for teenagers. Consider, for example, African-American teenagers. The birth rate for this group will be calculated as the ratio of the total number of births to African-American women aged 15-19 to the total number of women in this age group for given year (and state when we need state specific information).
birth rate was 156 while it was 79 for Whites. By the year 2001, the birth rate declined to 72 for African-Americans and 41 for Whites.

While teenage childbearing has been in a long-term decline in the United States since the late 1950s, the story for the non-marital birthrate among teens is sharply different. In 1957 when total teenage child bearing was its highest all-time high level, the birth rate per 1000 unmarried women aged 15-19 was 16. Almost four decades later, the out-of-wedlock birth rate hit its maximum of 46 births in 1994. Between 1994 and 2001 it declined to 37 births. For African-Americans, a steep increase occurred in the out-of-wedlock birth rate from 1960 to 1991, followed by a period of constant decline. For Whites, there was a persistent increase (although to a lesser degree than African-Americans) from 1960 to 1994, and then a decline for the following decade (Figure A4). Even though the increase of unmarried teen mothers has drawn considerable attention in academic circles, the reasons behind this reversal in the teen out-of-wedlock birth rate in the early 1990s have not been investigated by many studies and are still ambiguous.

Darroch and Singh (1999) analyzed the reasons behind the recent decline in the U.S. teen pregnancy rate, using data from two comparable, large-scale government surveys, the 1988 and 1995 cycles of the National Surveys of Family Growth, as well as recent information on the rates of teenage pregnancy, births and abortions. Their analysis concluded that approximately one-quarter of the decline in teenage pregnancy in the United States between 1988 and 1995 was due to increased abstinence. (The proportion of all teenagers who had ever had sex decreased slightly, but non-significantly, during this period, from 53 percent to 51 percent) Approximately three-quarters of the decline
resulted from changes in the behavior of sexually experienced teens. (The pregnancy rate among this group had fallen percent, from 211 per 1,000 to 197.)

Darroch and Singh (1999) considered a number of behavioral changes that could explain why a smaller proportion of sexually experienced teenage women became pregnant in 1995 than in 1988, including the possibility that they were having less sex. However, they found that there was little change overall between the two years in how often sexually experienced teenagers had intercourse. Instead, the researchers found that overall contraceptive use increased—but only slightly, from 78 percent in 1988 to 80 percent in 1995. More importantly, teenagers in 1995 were choosing more effective methods (Boonstra, 2002).
Figure A.1: Total Birth Rate for Women Aged 15-19 in the U.S., 1960-2001

Figure A.2: Out-of-Wedlock Birth Rates for Women Aged 15-19 in the U.S., 1960-2001
Figure A.3: Birth Rates for African-American and White Women Aged 15-19 in the U.S., 1960-2001

Figure A.4: Out-of-Wedlock Birth Rates for African-American and White Women Aged 15-19 in the U.S., 1960-2001
Figure A.5: Abortion Rates per 1000 Live Births in the Repeal and Non-Repeal States, 1970-1985
A2. DDD Parameter in a Regression Framework

In this appendix we show that our coefficient of interest is indeed the corresponding DDD parameter. As we discussed in the methodology section, DDD parameter—suppose for simplicity we are only interested in the impact on 15 year-olds—can be written in the following way

\[
\delta_5 = \left( \Delta \bar{B}R_{(r),15,1986} - \Delta \bar{B}R_{(nr),15,1984} \right) - \left( \Delta \bar{B}R_{(r),15,1984} - \Delta \bar{B}R_{(nr),15,1986} \right) - \left( \Delta \bar{B}R_{(r),17,1986} - \Delta \bar{B}R_{(nr),17,1984} \right) - \left( \Delta \bar{B}R_{(r),17,1984} - \Delta \bar{B}R_{(nr),17,1986} \right)
\]

(A1)

where \( r \) and \( nr \) subscripts indicate the repeal and nonrepeal states respectively. Note that we use 17 year-olds as the unaffected comparison group. Also, we can write our regression without logarithmic transformation of the birth rates as

\[
\ln \left( B_{ijt} \right) = \delta_0 + \delta_1 \text{AGEAFFECTED}_j + \delta_2 (\text{AGEAFFECTED}_j \times \text{POST70}_i) + \delta_3 (\text{AGEAFFECTED}_j \times \text{REPEAL}_i \times \text{POST70}_i) + \delta_4 (\text{AGEAFFECTED}_j \times \text{REPEAL}_i \times \text{POST70}_i) + s_i + y_t + e_{ijt}.
\]

(A2)

Let us write each parameter in DDD expression in terms of the coefficients that capture particular birth rates

\[
\bar{B}R_{(r),15,1986} = \delta_0 + \delta_1 + \delta_2 + \delta_3 + \delta_4
\]

\[
\bar{B}R_{(r),15,1984} = \delta_0 + \delta_1 + \delta_3
\]

\[
\bar{B}R_{(r),17,1986} = \delta_0 + \delta_4
\]

\[
\bar{B}R_{(r),17,1984} = \delta_0
\]

(A3)

\[\text{Clearly, the coefficients will be different without the logarithmic transformation but we keep the same notation for the coefficients in this appendix for simplicity.}\]
\[ \overline{BR}_{(ar),15,1986} = \delta_0 + \delta_1 + \delta_2 \]

\[ \overline{BR}_{(ar),15,1984} = \delta_0 + \delta_1 \]

\[ \overline{BR}_{(ar),17,1986} = \delta_0 \]

\[ \overline{BR}_{(ar),17,1984} = \delta_0 \]

Now rewriting and rearranging the DDD equation given this information will yield

\[ DDD = ((\delta_0 + \delta_1 + \delta_2 + \delta_3 + \delta_4 + \delta_5 - \delta_0 + \delta_1 + \delta_2) - (\delta_0 + \delta_4 - \delta_0)) \]
\[ -((\delta_0 + \delta_1 + \delta_2 - \delta_0 - \delta_1) - (\delta_0 - \delta_0)) \]  
\[ = (\delta_0 + \delta_4 + \delta_5 - \delta_4) - \delta_2. \]

\[(A4)\]

\[ = \delta_5, \]

which is our coefficient of interest.
APPENDIX B

In this appendix, we describe how we calculate standard errors for each of our parameters of interest in the linear probability model with interactions (LPMI) and switching probit model (SPM) using the delta method.

B1. Calculating the Standard Errors for Linear Probability Model with Interactions (LPMI)

Calculating the variance of the average take-up rate in the LPMI is straightforward using the delta method since our parameter of interest is only a linear sum of regression coefficients multiplied by the corresponding explanatory variables

\[
\hat{A}\hat{TR}_{j}^{lpmi} = \sum_{i\in j} X_i \hat{\theta} / N_j. \tag{B1}
\]

So the variance of the average take-up rate among the eligible can be written as

\[
\text{var}(A\hat{TR}_{j}^{lpmi}) = \left( \frac{\partial A\hat{TR}_{j}^{lpmi}}{\partial \theta} \right)' \text{var}(\hat{\theta}) \left( \frac{\partial A\hat{TR}_{j}^{lpmi}}{\partial \theta} \right) \tag{B2}
\]

where

\[
\frac{\partial A\hat{TR}_{j}^{lpmi}}{\partial \theta} = \sum_{i\in j} X_i / N_j. \tag{B3}
\]
The variance of the take-up response among the newly eligible families after the policy expansion, \( \hat{\text{TR}}_{\text{pmi}}^{\text{new}} \), can be calculated in an identical fashion using the observable characteristics of the sample of the newly eligible families.

**B2. Calculating the Standard Errors for Switching Probit Model (SPM)**

**B2.1. Marginal Probability of Participation and Take-up Response without Accounting for Unobservables**

The marginal probability of participation, which is our average take-up rate as well as the take-up response for the newly eligible population in our policy experiment assuming that the error term in the index function for eligibility is random, is given by

\[
\Phi_{i}[X_{i}, \mu] = \int_{-\infty}^{X_{i}, \mu} \phi(\varepsilon_{i})d\varepsilon_{i}.
\]

(B4)

Our parameter of interest is

\[
\text{ATR}_{j}^{\text{spm}} = \left[ \sum_{i \in j} 1 - \Phi_{i}(-X_{i}, \mu) \right] / N_{j} = \left[ \sum_{i \in j} \Phi_{i}(X_{i}, \mu) \right] / N_{j}.
\]

(B5)

Then, the variance of \( \text{ATR}_{j}^{\text{spm}} \) is

\[
\text{var}(\text{ATR}_{j}^{\text{spm}}) = \left[ \frac{1}{N_{j} \sum_{i \in j} \frac{\partial \Phi_{i}[X_{i}, \mu]}{\partial \hat{\mu}}} \right] \text{var}(\hat{\mu}) \left[ \frac{1}{N_{j} \sum_{i \in j} \frac{\partial \Phi_{i}[X_{i}, \mu]}{\partial \hat{\mu}}} \right].
\]

(B6)
The derivative in (A6) can be written explicitly as

\[ \frac{1}{N_j} \sum_{i \in j} \frac{\partial \Phi[X_i \hat{\mu}]}{\partial \hat{\mu}} = \frac{1}{N_j} \sum_{i \in j} X_i \phi(X_i \hat{\mu}). \]  

(B7)

The variance of the take-up rate for the expansion is calculated in an analogous fashion.

**B2.2. Take-up Response Accounting for Unobservables**

Our parameter of interest is

\[ \widetilde{ATR}_j^{spm} = \frac{1}{N_j} \sum_{i \in j} \frac{\Phi_2(X_i \hat{\mu}, Z_i \hat{\delta}, \hat{\rho})}{\Phi_1(Z_i \hat{\delta})}, \]  

(B8)

where

\[ \Phi_2(X_i \hat{\mu}, Z_i \hat{\delta}, \hat{\rho}) = \int_{-\infty}^{x_i \hat{\mu}} \int_{-\infty}^{Z_i \hat{\delta}} \phi_1(e_i, \epsilon_i, \hat{\rho}) d\epsilon_i d\epsilon_i \quad \text{and} \]

\[ \Phi_1(Z_i \hat{\delta}) = \int_{-\infty}^{Z_i \hat{\delta}} \phi(e_i) de_i. \]  

(B9)

In A9, \( \phi_2(.,., \hat{\rho}) \) is the bivariate normal density, \( \phi(.) \) is the normal density, \( X_i \) is the vector of variables in the participation equation, \( Z_i \) is the vector of variables in the eligibility equation, and \( \hat{\mu} \) and \( \hat{\delta} \) are the vectors of coefficients in the participation and eligibility equations respectively.

The variance of \( \widetilde{ATR}_j^{spm} \) can be written as follows
\[
\text{var}(\hat{ATR}_j) = \begin{bmatrix}
\frac{\partial \hat{ATR}_j}{\partial \hat{\mu}} & \frac{\partial \hat{ATR}_j}{\partial \hat{\delta}} & \frac{\partial \hat{ATR}_j}{\partial \hat{\rho}} \\
\end{bmatrix}
\begin{bmatrix}
\text{var}(\hat{\mu}) & \text{cov}(\hat{\mu}, \hat{\delta}) & \text{cov}(\hat{\mu}, \hat{\rho}) \\
\text{cov}(\hat{\mu}, \hat{\delta}) & \text{var}(\hat{\delta}) & \text{cov}(\hat{\delta}, \hat{\rho}) \\
\text{cov}(\hat{\mu}, \hat{\rho}) & \text{cov}(\hat{\delta}, \hat{\rho}) & \text{var}(\hat{\rho}) \\
\end{bmatrix}
\begin{bmatrix}
\frac{\partial ATR_j^{\text{spm}}}{\partial \hat{\mu}} \\
\frac{\partial ATR_j^{\text{spm}}}{\partial \hat{\delta}} \\
\frac{\partial ATR_j^{\text{spm}}}{\partial \hat{\rho}} \\
\end{bmatrix}.
\]

(B10)

The derivatives can explicitly be written as

\[
\frac{\partial ATR_j^{\text{spm}}}{\partial \hat{\mu}} = \frac{1}{N_j} \sum_{i \in j} \frac{\partial \Phi_2(X, \hat{\mu}, Z, \hat{\delta}, \hat{\rho})}{\Phi_1(Z, \hat{\delta})},
\]

(B11)

\[
\frac{\partial ATR_j^{\text{spm}}}{\partial \hat{\delta}} = \frac{1}{N_j} \sum_{i \in j} \left( \frac{\partial \Phi_2(X, \hat{\mu}, Z, \hat{\delta}, \hat{\rho})}{\Phi_1(Z, \hat{\delta})} \right) \Phi_1(Z, \hat{\delta}) - \left( \frac{\partial \Phi_1(Z, \hat{\delta})}{\Phi_1(Z, \hat{\delta})} \right)^2 \Phi_1(Z, \hat{\delta}),
\]

(B12)

\[
\frac{\partial ATR_j^{\text{spm}}}{\partial \hat{\rho}} = \frac{1}{N_j} \sum_{i \in j} \frac{\partial \Phi_2(X, \hat{\mu}, Z, \hat{\delta}, \hat{\rho})}{\Phi_1(Z, \hat{\delta})},
\]

(B13)

where

\[
\frac{\partial \Phi_2(X, \hat{\mu}, Z, \hat{\delta}, \hat{\rho})}{\partial \hat{\mu}} = X, \phi(X, \hat{\mu}) \Phi[(Z, \hat{\delta} - \hat{\rho} X, \hat{\mu}) / \sqrt{1 - \hat{\rho}^2}],
\]

(B14)

\[
\frac{\partial \Phi_2(X, \hat{\mu}, Z, \hat{\delta}, \hat{\rho})}{\partial \hat{\delta}} = Z, \phi(Z, \hat{\delta}) \Phi[(X, \hat{\mu} - \hat{\rho} Z, \hat{\delta}) / \sqrt{1 - \hat{\rho}^2}],
\]

(B15)

\[
\frac{\partial \Phi_2(X, \hat{\mu}, Z, \hat{\delta}, \hat{\rho})}{\partial \hat{\rho}} = \phi_2(X, \hat{\mu}, Z, \hat{\delta}, \hat{\rho}) \quad \text{and}
\]

\[
\frac{\partial \Phi_1(Z, \hat{\delta})}{\partial \hat{\delta}} = Z, \phi(Z, \hat{\delta}).
\]

(B17)