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Essays in Health Economics

Essay I: Effects of Welfare Reform on Prenatal Care Utilization and Birth Outcomes
Essay II: Abortion Availability and Unintended Births

By

Won Chan Lee

A dissertation submitted to the Graduate Faculty in Economics in partial fulfillment of the requirements for the degree of Doctor of Philosophy, The City University of New York

2001

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Approval

This manuscript has been read and accepted for the Graduate Faculty in Economics in satisfaction of the dissertation requirements for the degree of Doctor of Philosophy.

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THE CITY UNIVERSITY OF NEW YORK

Abstract

Essays in Health Economics

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Adviser: Distinguished Professor Michael Grossman

This dissertation consists of two papers, not directly related yet both involved with infants' well being. The first paper investigates the effects of welfare reform, a social change that transformed the United States during the 1990's, on prenatal care utilization and birth outcomes. Natality files from 1991 to 1998 were the data set of this research. Employing a treatment group-focused multivariate estimation bolstered by a difference in difference econometrics method, the study found a negative association of welfare reform both with prenatal care utilization and birth outcomes. The magnitudes are quite small, yet discernable negative magnitudes are consistently present.

However, "low" intensity reform, which indicates the states that implemented only " time limit," without other welfare reform components, is associated with enhanced prenatal care utilization although a negative association obtains between this " low" intensity reform and birth outcomes. Among the components of welfare reform, sanction and family cap appear to play particularly negative roles in prenatal care utilization and birth outcomes, while income disregard policy dampens the negative effect.

The second paper bears on the extent to which the availability of abortion, measured by number of abortion providers and distance to nearest abortion provider, affects the probability of unintended birth. An association between abortion access measures and the probability of unintended/unwanted birth is, albeit not strong, positive, at the aggregate level. When education and Medicaid receipt are proxied for income and opportunity cost, these variables interact positively with abortion access measures, particularly least endogenous variables. These variables tend to increase by 0.8 to 6 percentage points the likelihood that a woman will deliver unwanted babies, relative to the mean of 11.7% from 1993 to 1997. The positive association implies that notwithstanding the presence of providers, impoverished women will either fail to abort or fail to use contraception, thereby increasing the probability of unintended/unwanted birth.

Acknowledgements

There is no such thing as a free lunch; there is no such human being as a completely self-made man. As with major undertaking, this dissertation could not have made the journey through unfocused ideas to bound pages without the enthusiasm, prodding, and encouragement of a wide array of people.

I owe an immeasurable debt to my dissertation supervisor, Michael Grossman. His insight and suggestions as an economist have never ceased to surprise me. As a human being, his evenhandedness and compassion are unsurpassed. I am equally indebted to Theodore Joyce and Robert Kaestner. Their suggestions and guidelines have been just as valuable, and I feel grateful for the concern that they showed during the difficult time that I encountered. They allowed me to use NBER office space, a window of opportunity for the contact and the exchange of ideas with many stellar people.

My life would have not known a turnaround without the encouragement and suggestions of Linda Edwards. I met her when I came in New York in 1997 as an exchange program student from Paris. While working as her research assistant and ever since, I have found her encouragement particularly kind and helpful. Much appreciation is due to Thomas Thurston for helping me focus my energies and broaden my horizons. Many thanks go to the Graduate Center of the City University of New York for the award that enabled me to pursue the completion of my dissertation without financial burden.

My parents have given me constant support, both emotional and financial, and to them I dedicate this dissertation. I thank them for their unconditional love, and I feel grateful to the many friends who have been supportive of me during many difficult moments.

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Essay I: Effects of Welfare Reform on Prenatal Care Utilization and Birth Outcomes

Essay II: Abortion Availability and Unintended Births

Introduction

Public policy toward the disadvantaged has shifted dramatically in recent years. The major change is that social programs are putting more emphasis on personal rather than social responsibility. Reflecting this social trend, welfare reform is intended to reduce families' dependence on government financial support by encouraging employment and changing behaviors related to marriage and fertility.[1] Judging by changes in caseloads nationally, welfare reform has been a success. Between January 1994 and December 1999, welfare caseloads fell 53 percent.

Though national legislation was passed in August 1996 (the Personal Responsibility and Work Opportunities Reconciliation Act—PRWORA), a number of states had already begun reforming welfare in the early nineties. Welfare caseloads started to decline soon thereafter. Although the pace was modest at first, welfare rolls fell faster as more states adopted reform. After federal legislation was passed, caseloads declined even more precipitously. Between January 1997 and December 1999, 1.8 million families left the welfare rolls, almost double the number that moved off welfare in the three years prior to that period. In 1994, 5.5 percent of the US population was dependent on welfare. Five years and a series of state and federal welfare reforms later, the proportion had shrunk to 2.3 percent (Graph 1).

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[1] "The Findings" section of The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA) concludes: " Therefore, in light of this demonstration of the crisis in our nation, it is the sense of the Congress that prevention of out-of-wedlock birth is a very important government interest and the Policy contained in part A of Title IV of the Social Security Act .... is intended to address the crisis. Out-of-wedlock births reduce a woman's ability to remain financially independent and to raise children in a healthy and secure environment." Indeed, under the "Illegitimacy Bonus" program, the Federal government awards $20 million each year to the 5 states that most reduce their out-of-wedlock birth rate.

The shrinking number of welfare recipients has raised concerns about the plight of poor single mothers and their dependent children, the main beneficiaries of welfare or what is now called Temporary Aid to Needy Families (TANF). Since the passage of state and federal welfare reform legislation, numerous researchers have studied the employment status and earnings of women who have left the AFDC/TANF rolls (Schoeni and Blank, 2000). Some have analyzed the reasons for the marked decline in caseloads since the mid 1990s (Grogger, 2000)). The effects of welfare reform, however, need not be restricted to declines in caseloads and changes in employment. There may be unintended consequences associated with the transition from welfare to work and the greater labor market activity it entails.

Welfare reform may lead to a loss of health insurance, greater work-related stress, changes in a woman's self-esteem, and changes in family income. All of these changes may affect the health and health-care utilization of poor women and infants, and some changes are expected to adversely affect these outcomes.

Unlike most developed countries, the United States has an unusually high level of infant mortality (7.2 per 1000 live births in 1997) coupled with a high rate of relatively low birth weight, factors which are considered an objective measure of birth outcomes. Moreover, variations in birth weight seem to coincide with socioeconomic factors—women with more resources and greater access to healthcare are consistently having healthier children (Morbidity and Mortality Weekly Report, 2000).[2] Not only was it clear

[2] MMWR (2000) reports that black infants are more than twice as likely to die as white infants; this ratio has increased in recent decades. The higher risk for infant mortality among blacks compared with whites is attributed to higher preterm rate and low birth weight incidence.

that not all women in the U.S. were receiving the same quantity or quality of healthcare as women in other developed countries; there were significant differences across classes in this country as well.

In the political arena, the debate over how to address this problem has been split into two camps. Some policy makers view low birth outcomes as a result of individual failure, pinning the problem on the personal health habits of pregnant women and out-of-wedlock mothers (Rosenzweig and Wolpin 1991, Evans 1999).[3] In the 1980s, a rise in cocaine use and the increasing rate of teenage births were also viewed as contributing to low birth outcomes. For some, the conclusion was plain—only individual efforts by pregnant women could have an impact on infant health.

Others argued that financial barriers prevented poor and near-poor women from receiving timely and high quality prenatal care. Whatever their individual health habits, these women experienced adverse birth outcomes due to inadequate or inaccessible health care. Following this line of thinking to its logical end, it seemed clear that the lack of insurance among the poor and near poor was the main cause of unfavorable birth outcomes. Despite vehement arguments on both sides, the problem was ultimately viewed as a social one, and policy makers adopted legislation governing expansion of Medicaid eligibility, opening up health care to a new segment of the underserved population.

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[3] It has been proved that smoking during pregnancy lowers birth weight. Rosenzweig and Wolpin (1991) employed the analysis of variances, and they concluded that eliminating smoking among all pregnant women would increase the average birth weight of first borns by 1.2 ounces. In Sexton and Hebel's clinical trial (1984), the treatment group was 92 grams higher in birth weight than the control. Employing cigarette taxes as an instrument of smoking variable in birth production function, Evans and Ringel (1999) obtained a smoking effect of the same magnitude.

Accordingly, the late 1980's and early 1990's were marked by a rapid expansion in Medicaid eligibility and enrollment for pregnant women, infants, and children. The series of legislative reforms on Medicaid eligibility was intended to serve as a vehicle to increase infant health. The percentage of births financed by Medicaid rose from 21% in 1988 to 48% in 1992, and it has remained relatively stable since 1992 (*Maternal Child Health Update*, 1988-1992). At the same time, the enrollment of children in Medicaid increased from 10.3 million to 17.1 million between 1989 and 1995, a rise of 15 percentage points (Newacheck et al., 1998). More recently, the State Children Health Insurance Program (SCHIP) was created to increase the enrollment of near-poor children in publicly funded health insurance.

Of widespread concern, however, has been that welfare reform, which culminated in the 1996 Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), may undo the gains associated with the Medicaid expansions. The significant declines in welfare caseloads may have resulted in a greater number of uninsured.

A Possible Decline in Medicaid Enrollment

Established in 1965, Medicaid is the largest single spending program in the United States assisting the poor and nearly poor. Eligibility to receive Medicaid is determined by the states, but the federal government requires that pregnant women and infants in families with income below 133 percent of the official poverty threshold must be mandatorily eligible. Medicaid is not automatic for families who receive TANF cash assistance as it was under the old AFDC program, although TANF recipients are eligible

in most states. Most Medicaid recipients must have income and assets below specified amounts. These amounts vary from state to state but are determined within federal guidelines.

According to Maternal and Child Health Update (1999), Medicaid paid for approximately 1 million births in 1997. The MCH Update indicates that the majority of states reported fewer Medicaid births in 1997 than in 1996. Medicaid births as a percentage of total births in each state ranged from a high of 51 percent in New Mexico to a low of 20 percent in New Hampshire. The median percentage of Medicaid births was 33 percent in 1997, compared with 38 percent in 1996, an indicative finding that pregnant women may have lost public insurance.

Channels of Welfare Reform Affecting Health Outcomes

Welfare reform may have triggered a decrease in the Medicaid enrollment of pregnant women and infants. Welfare reform may alter insurance status in two ways. First, increased labor force participation induced by welfare reform may make more women, infants and children ineligible for Medicaid. Second, the uncoupling of Medicaid and cash assistance may make Medicaid enrollment more difficult to obtain (Chavkin et al. 1999). On the other hand, greater labor market activity may increase the incidence of private insurance coverage among poor families.

Such a change in insurance status may alter health care utilization. Welfare reform may also directly affect health care utilization without a significant change in insurance status. If changes in health care utilization had an effect on infant health, then welfare reform could have a deleterious impact on infant health (Alexander and

Korenbrot, 1995). When insurance status is held constant, time constraints and logistical barriers will cause delays in prenatal care or a decrease in prenatal care visits. Welfare reform may also affect health outcomes through mechanisms other than its effects on insurance status and health care utilization. Examples include stress levels and time constraints as well as sleep, diet, and smoking patterns. Women induced to work by TANF are, like other low-wage workers, likely to perform physically demanding jobs. Shifts in the average physical and mental activity level of women of child-bearing age may elevate their stress level and thus cause a higher rate of smoking and an increase in other stress-reducing behaviors that are detrimental to health. Indeed, Rosenzweig and Schultz (1983) found that mothers appear to smoke less where female unemployment rates are high. Welfare reform might lower infant health if smoking and labor force participation were complements. Because women would also face more severe time constraints and logistical barriers, direct investment in health inputs such as proper diet and sleep may have been reduced. These various channels can be summarized in the following chart. For welfare reform to affect birth outcomes, it should be a causal link in this chain.

Various Channels Affecting Birth Outcomes

Welfare reform → Changes in Insurance Status → Changes in Prenatal Care → Infant Birth Outcomes

Welfare reform → Changes in Prenatal Care → Infant Birth Outcomes

Welfare reform → Infant Birth Outcomes

Purpose of the Study

This study investigates the effects of state and federal welfare reform on the health care utilization and health outcomes of pregnant women and infants. Several specific questions arise. First, has welfare reform increased the number of women who delay or reduce prenatal care visits, and if so, has it resulted in worse infant health outcomes? In other words, are birth outcomes and use of maternal prenatal care responsive to welfare reform? Second, if welfare reform may have adverse consequences for birth outcomes, my study attempts to identify how that causality operates. Third, I examine whether welfare reform has affected marriage rates among women who gave birth. While not a health outcome, non-marital fertility is a central concern of welfare reform and one that has received relatively little study. I include the study of this issue in my research because, as I describe below, it is a necessary step toward a better understanding of the central questions I have posed.

Major Components of Welfare Reform

In the decade preceding welfare reform in 1996, states had already begun to experiment with shifts in their state regulations governing welfare policy under federal authorization.[4] These granted modifications of state policy are commonly referred to as waivers. Between January 1993 and August 1996, the Department of Health and Human Services approved welfare waivers in 43 states. Some of these waivers supported modest

[4] The U.S. department of Health and Human Services maintains a web site (http://aspe.hhs.gov/hasp/waiver-policies99/policy_CEA.htm) providing information by state on the timing of major changes to welfare policies under both the AFDC program (1992-1996) and the TANF program (1996-1998).

demonstration projects limited to a few counties, but many states instituted statewide modifications of the AFDC program. These waivers bear on five general welfare policies: time limits, sanctions, income disregard, Family Cap, and work exemption.

Under AFDC requirements, recipients were, unless exempt, required to participate in the Job Opportunity and Basic Skills Training (JOBS) program that provided education, training, and work experience activities. However, the exemption categories were claimed to be overly broad. As a result, recipients tended to be, more often than not, exempt from the JOBS program. This lax requirement prompted some states to obtain autonomous control of the JOBS program, making it mandatory for a greater number of AFDC recipients. A particular concern for my study is that waivers reduced the exemptions for pregnant women to exclude all but those with medical reasons. Further, states were allowed to implement sanctions for non-compliance with JOBS. For example, 23 states received waivers that allowed them to impose full-family sanctions (i.e, termination of families' AFDC grant).

More dramatic change concerns the duration of benefits that welfare recipients may receive. Under AFDC rules, families were entitled to receive assistance for as long as they met the eligibility standards. However, through waivers, the Federal government began to allow states to set time limits on welfare entitlement. By and large, three types of time limits can be categorized under this waiver. These time limits include the "Termination Time Limit," the "Work Requirement Time Limit," and the "Reduction Time Limit." The "Termination Time Limit" denies further AFDC benefits once families have used up their allotted limit. The " Work Requirement Time Limit" imposes a mandatory work requirement but does not cut off the benefits. The "Reduction Time

Limit" reduces the benefits after families have been on welfare for a certain period of time.

Another feature of welfare reform is called the " Family Cap." The implementation of this waiver policy hinges on the argument that AFDC rules may create an incentive for welfare recipients to have more children in order to receive a larger grant amount (Jackson and Klerman, 1994; Matthews, Ridar and Wilhelm 1997). Jackson and Klerman (1994) found that AFDC benefits had large positive effects on the birthrate among white women but no association among blacks. A simple reason is that the total amount that a family may request is based on family size. This argument permitted some states to implement "Family Cap" waivers. These waivers eliminate or reduce the increase in benefits upon the birth of an additional child following the family's initial reception of AFDC.

Under AFDC rules, all recipients who worked were entitled to a $90 work expense disregard. In addition, for the first four months of AFDC receipt, the next $30 of earned income, plus one-third of the remainder, was disregarded in calculating eligibility and benefits. Many states came to the conclusion that the termination of the earned income disregard after a short period removed the economic incentive for AFDC recipients to work. Therefore, some states removed the time limit on the $30 and one-third disregard, or even disregarded all income up to the poverty line.

The effect of the " Family Cap" on infant birth outcomes is not easily predictable. Neither is the effect of the various time limits. If the "Family Cap" indeed influences fertility and thus reduces the number of children, it may have a positive impact on infant health since parents allocate both their time and income more intensively to the selected

deliveries. However, under the "Family Cap," withholding AFDC assistance following additional births would cause a negative impact on birth outcomes. The impact of the time limit policy is also ambiguous. As long as women have not reached the time limit, they still receive cash assistance; however, women approaching the time limit are required to find a job in the near future. Those who seek or obtain employment are confronted with more time constraints and logistical barriers regarding their prenatal care. Unless the income from the job surpasses the level of the previous cash assistance, the effect of time limits on infant birth outcomes is expected to be negative. Alternatively, women getting off welfare are likely to experience enhancement of self-esteem, which may induce early check-ups for their pregnancy and increase the quality of prenatal care visits.

Similar to time limits, more stringent JOBS requirements also increase the time constraints and logistical barriers for pregnant women seeking their prenatal care visits, and they appear to be negatively associated with both prenatal care visits and birth outcomes. JOBS would also elevate the opposite possibility, raising their income as well as their self-esteem. Then the increased income and enhanced self-esteem should trigger a positive consequence on prenatal care and birth outcomes.

Thus, each of the waivers, including the "Family Cap," time limits, and JOBS requirement, has no determined sign. However, a component of waivers, which is called "earned income disregard, " will be most likely to produce a positive impact on prenatal care and birth outcomes. Yet a sufficient increase in the income of Medicaid recipients may invalidate their enrollment in the program, leaving them uninsured. If this event is

propelled, albeit unlikely, by "earned income disregard," this welfare component may adversely affect prenatal care utilization and birth outcomes.

TANF follows the nature of most waivers. However, TANF is more stringent on the work and training requirement. It requires a 25 percent participation rate in 1997, which increases to 50 percent by 2002. The TANF law forbids the use of federal funds to provide assistance to a family that includes an adult who has received assistance for 60 months or less at state discretion. However, TANF does not require states to base levels of assistance on the number of people in the family. Therefore, each has the authority to establish its own "Family Cap" or none at all. Nor is the TANF state required to adopt any particular earned income disregards. TANF was implemented in 1996 in all states, and thus the impact of TANF has been expected to be greater than that of waivers.

My model uses the dates of implementation, not the dates of approval, in order to classify the waiver variables prior to the TANF implementation because some states, although they obtained an approval from the federal government, did not opt for the implementation of the waiver.

Analytical Framework

The theoretical framework that will serve for my empirical analysis is derived from the following four-equation econometric model. The first one is a structural equation for infant health production function.

(1) $BO = f(PC, X, WF)$

BO is a measure of birth outcomes, either a continuous birth weight variable or the dichotomous indicator variables of low-birth outcome below 2500 grams and of very-low birth outcome below 1500 grams. A vector of X encompasses the individual's demographic, socioeconomic and personal variables. WF, indicating welfare reform, may affect birth outcomes directly.

The second equation illustrates the demand for prenatal care visits.

(2) $PC = g(p(WF), Y)$

PC denotes prenatal care visits. PC measures not only frequency of prenatal care visits, but also early or late initiation of prenatal care. P is the "full price" of prenatal care. Y is a vector of determinants of prenatal care, which includes household income.

The standard model of parental investment in children's health capital explains that programs providing aid to mothers with dependent children have reduced the cost of prenatal care. The subsequent increase in prenatal care enhances birth outcomes in turn. Conversely, welfare reform may raise the cost of prenatal care. Consider that when deprived of cash assistance, women complying with TANF earn wages approximately equivalent to or lower than their prior government subsidies, when household income is held constant. The demand for prenatal care would decline if prenatal care is a time-intensive commodity because the substitution effect reinforces the income effect. Even if these women's earnings are somewhat higher than the amount of their previous cash assistance, it is likely that the substitution effect will dominate the income effect.

Meanwhile, clinical studies on the effects of prenatal care on birth weight have recorded small effects. Klerman et al. (2000) found that prenatal care of high quality, offered in a supportive environment emphasizing health promotion and education, did not reduce the rate of low birth weight within the group of African American women participating in a randomized trial. Goldenberg and Rouse (1998) documented that various interventions aimed at reducing premature births do not work.

Although augmenting quality or simply increasing the number of prenatal care visits may not enhance birth outcomes a great deal, it appears that preventive prenatal care reduces the likelihood that a woman will deliver low weight babies. The early initiation of prenatal care has been proven to be more important than the total number of prenatal care visits in an attempt to reduce the incidence of low weight births (Institute of Medicine, 1985). Women who receive delayed or no prenatal care are at risk of having undetected complications of pregnancy that can result in severe morbidity and mortality rates for their infants. It is thus crucial to explain both of the behavioral changes in prenatal care: how early pregnant women initiate their prenatal care and whether an increasing proportion of women receives no prenatal care at all.

The choices made by a pregnant woman when deciding between an extra unit of health care and some other expenditure or use of her time will probably depend upon how forward-looking the individual in question is. Those who base their decisions on immediate consequences potentially underestimate the importance of preventive care in promoting their longer-term utility. Pregnant women may underestimate the marginal utility of preventive medical care when they are less forward-looking. Welfare reform should have altered the "time preference" of pregnant women. They may have become

more or less forward-looking following a change in self-esteem, income, and employment. Because welfare reform determines the price of prenatal care visits and " time preference," welfare reform will influence both the number of prenatal care visits and the probability of early or late initiation of care.

When the welfare reform variable is used to proxy for prenatal care price and "shifts in time preference," we obtain the reduced form equation (3), and the substitution of equation (2) into (1) yields a reduced form equation (4). We estimate the two reduced form models by ordinary least squares. My reduced form models analyze the relationships that obtain between welfare reform and prenatal care and between welfare reform and infant birth outcomes.

(3) $PC = j(WF, Y)$

(4) $BO = h(WF, X)$

The partial derivative of the equation (1) yields $\partial BO/\partial WF=(\partial BO/\partial PC)*(\partial PC/\partial WF)$. Knowing $(\partial PC/\partial WF)$, we can derive $(\partial BO/\partial PC)$. One possible result is that in the first equation, the welfare reform coefficient may not be statistically significant. However, it may be significant in the second model. This outcome would imply that welfare reform directly affects infant birth weight through channels other than prenatal care visits. Income or stress level would be a direct factor in the demand for infant health.

Shifts in prenatal care and welfare policy will influence women's determination on both the number and quality of babies (Becker and Lewis, 1973). Welfare reform increase the cost of both the number and quality of children: both the quantity and quality

decline in response to welfare reform as parents increase consumption of other goods that are now cheaper relative to the "cost of babies." Another possibility is that demand for infant "quality,"- for instance infant health- decreases, thereby decreasing the price of having additional children. Parents may also respond to welfare reform by increasing investments in newborn health, thus raising quality per child relative to quantity. Thus, it is an empirical question as to whether the effect of welfare reform on infant health outcomes is positive or negative.

Previous Research

Currie and Cole (1994)'s study, while not directed related to welfare reform, bears on the effects of maternal participation in the AFDC program spanning the period prior to welfare reform on the well-being of children. An underlying assumption is that AFDC cash transfers, by increasing income, will increase birth weight and enable mothers to purchase "inputs" like prenatal care in infant health production. On the other hand, participation in AFDC during pregnancy is associated with behaviors that are known to decrease birth weight. Using NLSY (National Longitudinal Survey) from 1979 to 1988, their OLS results show that children whose mothers received AFDC during pregnancy are of lower birth weight than other children, after controlling for household income, smoking, drinking and use of prenatal care. It appears that the OLS coefficient of the AFDC participation can be biased downward by the omission of important variables and the endogeneity of such variables as smoking, drinking and prenatal care used in their regression. When those variables are instrumented, the sign of the coefficient becomes positive, suggesting that all of the observed negative correlation between AFDC

participation and birth weight is due to omitted variable bias. Thus they establish a non-causal link between AFDC participation and birth weight. Taken at face value, this study predicts that welfare reform may have no causal link to birth outcomes if welfare reform induces former AFDC recipients to opt out of the welfare rolls. However, welfare reform would not simply mean this group's non-participation in AFDC.

While researching the effects of welfare reform policies on reproductive and infant health, Wise *et al.* (1999) explored multiple study designs. Although this article does not conduct empirical research, it does provide some background on welfare reform and the significance of research of this kind.

To date, only one empirical study pertaining to this research field has been conducted. Currie and Grogger (2000) examined the effects of Medicaid income eligibility ceilings, administrative reform, and declines in welfare caseloads on prenatal care and birth outcomes during the period 1990-1996. They used early or late prenatal care visits as a measure of adequate prenatal care. Dichotomous indicators of low birth weight (less than 2500 grams) and very low birth weight (less than 1500 grams) were used as measures of infant health outcomes. The fundamental caveat is that the data used predate the replacement of AFDC with the Temporary Assistance for Needy Families (TANF) program in August, 1996. Thus, this study concentrates mainly on the period prior to the federal enactment, the years during which states were granted the authority to implement their own welfare reform programs. Changes in welfare policy prior to PRWORA may have been too modest to affect prenatal care and infant health outcomes. Currie and Gorgger's results show that the decrease in welfare caseloads reduced prenatal care utilization. The increase in the income eligibility cutoffs to above 133% of the

poverty level for the Medicaid program raised utilization. The administrative reforms had no effect on utilization. Among whites, increases in the use of prenatal care reduced the incidence of very low birth weight by a small but statistically significant amount. There was no effect on white low birth weight and no effect on either of the outcomes for blacks. However, a striking finding is that decreases in welfare caseloads were associated among blacks with reduced use of prenatal care and reduced incidence of low and very low birth weight. Presumably, this association is attributable to two factors. First, the quality of prenatal care cannot be readily incorporated into such a study. Second, mechanisms other than the links between welfare and the use of prenatal care may be at work.

Currie and Grogger's regression analysis includes all women, married or unmarried, educated or uneducated. This method masks the real impact of welfare reform on a target group. Since both married and childless women are ineligible for welfare, none of the welfare reform variables affect their prenatal care. In a regression of welfare reform on prenatal care use, eachl of the welfare reform coefficients should be close to zero for married women. Since women who are ineligible for welfare outnumber those who are eligible, analyzing the effect of welfare on all women could lead one to conclude that welfare reform had little or no effect on welfare use, even if its effect on eligible women was substantial.

In an effort to control for economic conditions, the state level unemployment rate is included in regression models. It appears that this study merged the state level unemployment rate and individual data based on year of birth. However, for a prenatal

care regression, it would have been appropriate to use the one year lagged unemployment rate.

The econometric analysis employed the number of women on welfare as an independent variable in the reduced form regression. But this variable is not necessarily exogenous because the number of women on welfare correlates with other state policies. Therefore, the choice of this variable will inevitably bias the results. In this respect, my model specification differs from this study. In contrast to Currie and Grogger, I implement a dichotomous indicator of welfare reform. This dummy variable allows me to reduce bias caused by endogeneity as well as to explore different aspects of welfare reform since I will be able to create separate dummies for different aspects of welfare reform.

Data

Data on the outcomes of interest are taken from Natality detail files. The data are taken directly from birth records and includes information regarding birth outcomes, demographic characteristics, and maternal smoking. Since the health care provider collects much of the information at the time of the birth, information on birth outcomes should be reported accurately.

The Natality data lack detailed information on household income and insurance status. They do, however, describe mother's education level, marital status, age, previous birth, race, and singleton birth status. Those variables can be used as a proxy for socioeconomic status.

The Natality files encompass virtually all the births that take place every year. For the ease of running the computer program, my study took a 20% random sample of

The Natality files encompass virtually all the births that take place every year. For the ease of running the computer program, my study took a 20% random sample of whites and all the black births from the data set spanning 1991 to 1998. State and federal laws governing the receipt of cash assistance changed between 1991 and 1998. The eventual sample is restricted to infants born to women below 12 years of education, the target group of welfare reform. Information on key elements of the welfare policies to which each respondent was exposed is merged with the basic Natality files.

Welfare reform may be correlated with some underlying state characteristics. A host of control variables influencing waivers includes the unemployment rate and the state Medicaid eligibility policy. Substantial changes have occurred over the past few years in terms of eligibility for Medicaid enrollment. In particular, an inclusion of Medicaid eligibility income ceilings allows us to test whether welfare reform behaves as a barrier to accessing the Medicaid Program. The multivariate analysis described below will include a set of such policy variables. Also, it is important for our analysis to include measures that depict the variation in policies across states at a moment in time, changes over time and state specific time trend. Thus, the model employs state fixed effect, time trend and state specific time trend.

Empirical Methods

To investigate the effect of welfare reform on prenatal care utilization and infant health outcomes, I use two types of analysis. The first is a pre-post comparison of those exposed to welfare reform and those unexposed during the years 1991-1998. Infants born to unmarried women with 12 or fewer years of education will approximate the

"treatment" group, the group most affected by welfare reform. Infants of married women with 12 or fewer years of schooling will constitute the "control" group, a group similar to the "treatment" group but unaffected by welfare reform.

The empirical strategy is relatively simple. My study uses changes in outcomes before and after welfare reform to measure its impact. The obvious problem is that changes in outcomes over the relevant period may reflect changes that would have occurred in the absence of welfare reform. Thus, the "first difference" may be a poor measure of the effect of welfare reform. To account for these unmeasured time effects, we will subtract the changes in each outcome for the control group from those corresponding changes for the treatment group. This "Difference in Difference" will eliminate temporal factors such as changes in the strength of the economy that may vary together with welfare reform and affect the outcomes of interest.

The underlying logic behind the DD (Difference in Difference) estimation is as follows:

The Incidence of Infant Health Before and After Welfare Reform

Period/Group	Before Welfare Reform	After Welfare Reform	Difference
Treatment Group	A	B	A-B
Control Group	C	D	C-D
Difference in Difference	(A-B)-(C-D)		

The difference A-B measures the change in infant health or prenatal care utilization among the treatment group—target individuals before and after welfare reform. By contrast, the difference C-D measures the changes in infant health and prenatal care

utilization among women who are unaffected by welfare reform. An important characteristic of the DD estimator is that it adjusts for time-varying state effects by exploiting a within-state control group. However, the choice of treatment and control are critically important. The simple DD analysis is easily transferred to a regression context.

The model can be, *mutatis mutandis*, represented algebraically as follows:

$$Y_{ijt} = \alpha_1 \text{TREAT}_i + \alpha_2 \text{REFORM}_{jt} + \alpha_3 (\text{TREAT}_i \text{ x } \text{REFORM}_{jt}) + \alpha_4 X_{ijt} + \alpha_5 t + \sum \gamma_j D_j + e_{ijt}. \quad (1)$$

The subscripts i, j and t denote individuals, states and time respectively. The dependent variables Y_{ijt} include prenatal care utilization and birth outcomes of pregnant women. I will proxy health care utilization by various measures: prenatal care initiation, number of prenatal care visits and proportion of no prenatal care. Finally, birth outcomes can be measured by a continuous variable of birth weight and two indicator variables for low birth weight (below 2500 grams)[5] and very low birth weight (below 1500 grams).

The variable TREAT is a dummy variable indicating whether this is a treatment or a control group. REFORM is a dummy variable indicating whether this is the period before or after welfare reform. The vector of variables, X, includes maternal characteristics such as race, age, and previous births. These variables control for compositional changes before and after welfare reform. State unemployment rates are

[5] Birth weight is known to be informative of future well being in the life cycle. Birth weight not only predicts subsequent child mortality and morbidity but also is a significant correlate of children's intellectual and physical development. Two other indicators, infant mortality rate and incidence of low birth weight, are also frequently cited in health economics literature. Improving only birth weight may have no benefit if mortality rate and average health condition in childhood do not significantly improve.

also included in an effort to determine whether the difference in changes in outcomes between these two groups during the period is due to economic buoyancy or to welfare reform.

The model specification further includes state dummies (D_j), a time trend (t) and a disturbance term (e_{ijt}) that is by assumption not correlated with included variables. The use of the state dummy variable controls for unobserved state heterogeneity. The inclusion of the state dummy has no effect on the estimated coefficient α_3 but reduces the standard error associated with the coefficient of welfare reform. A linear time trend rather than a quadratic time trend will be implemented; prenatal care visits and birth outcomes have manifested linear trend (Graphs 5-7). The effect of welfare reform is identified by within-state changes among the treatment group relative to the control group before and after the implementation of welfare reform, conditional on measured characteristics. It is given by the coefficient (α_3) of the interaction between the variables TREAT and REFORM.

This DD estimation procedure has strength in the sense that it controls for time variation in outcomes that are unrelated to welfare reform, such as a factor occurring due to macroeconomic changes. Because of this parsimonious control for time variation in outcomes, it is not as crucial in this DD analysis to control for macroeconomic factors as it is in other approaches. Nevertheless, state unemployment rate is included, simply to be cautious.

The second empirical approach focuses solely on the "treatment" group. The underlying assumption of the difference-in-difference approach is that unmeasured temporal factors affect the "treatment" and "control" groups equally. This may not be

valid. Our alternative procedure accounts for these unmeasured trends using multivariate regression analysis that includes controls for state-specific trends. Algebraically, this model is

$$Y_{ijt} = \alpha_1 REFORM_{jt} + \alpha_2 X_{ijt} + \alpha_3 t + \sum \gamma_j D_j + \sum \delta_{jt} t * D_j + e_{ijt}. \quad (2)$$

The important points to note about equation (2) are that the sample consists solely of members of the treatment group and that state-specific time trends given by the coefficients (δ_{jt}) of the state-time interactions ($t*D_j$) are included in the model.
A similar analysis can be done using the sample of married women, or what we have referred to as the control group. In this case, we expect the effect of welfare reform to be close to zero since few women in this group are affected by welfare reform. At the very least, we would expect any effect of welfare reform to be much smaller in this group than for unmarried women. This provides a sensitivity test that can be used to bolster the credibility of our findings related to unmarried women.

Some 30 states adopted reform measures prior to the passage of PRWORA under waivers granted by the Department of Health and Human services. For each state and for each year we will include dummy variables indicating whether a particular policy was in effect either under a waiver or under TANF. Among the policies that are candidates for inclusion in our analysis are: "Termination Time Limit"; " Reduction Time Limit";" Work Requirement Time Limit"; sanctions; work exemptions; "Family Cap"; and earnings disregards. I group the " Termination Time Limit" and " the " Reduction Time Limit" into a time limit 1 category, and reclassify the "Work Requirement Time Limit"

as time limit 2 in the multivariate method. I also include the state income ceilings governing Medicaid eligibility, and 100% of federal poverty threshold is normalized to 1 when used as an independent variable in the regression. Table 2 shows the 10 biggest states' income ceilings relative to the federal poverty thresholds. The State unemployment rate is also included in an effort to control for economic conditions.

My research limits the sample to women with 12 or fewer years of education since women with higher levels of education may not be affected by welfare reform. If I do not restrict the sample and instead estimate parameters using the pooled sample, a heterogeneity problem arises. That is, since women who are ineligible for welfare outnumber those who are eligible, analyzing the effect of welfare reform on all women could lead one to conclude that welfare reform had little or no effect on welfare use, even if its effect on eligible women was substantial.

Thus restricting the sample is desirable. However, this process may also engender a different type of bias because welfare reform generally alters marriage and childbearing incentives by changing the composition of the women representing the population of people who are likely to be affected by welfare reform. If the compositional change in terms of marital status would be virtually unchanged during this period, the bias would be considerably reduced. Thus I first investigate whether welfare reform significantly altered marital status.

Empirical results

1) Time Trend and Summary Statistics

Table 1 shows descriptive statistics of women with 12 and fewer years of educational attainment, the main target group of welfare reform. Welfare reform is intended to address the increasing incidence of out-of-wedlock births. The rate of overall out-of-wedlock births among this segment of the population is 43.17%. For blacks the figure is 78.77 %. The overall out-of-wedlock birth rate[6] for the entire population has never ceased to rise since 1980 (Graph 2 and Graph 3). A salient fact is that whites' out-of-wedlock birth rate has steadily risen, whereas the rate for blacks has gradually declined.

In 1998, according to the HRSA (Human Resources and Services Administration)'s recent report, 298,208 babies (7.6 percent of all live births) were of low birth weight, weighing less than 2,500 grams, or about 5.5 pounds, at birth. The overall rate of low birth weight (LBW) rose from 6.8 to 7.6 percent for 1985-1998. This figure has increased 9 percent for the 1990's. The HRSA reports that most of the current year's rise, and much of the rise since 1990, can be attributed to increases in the multiple-birth rate because multiple births are at much greater risk of low birth weight than singletons. Indeed, the incidence of low birth weight babies among singleton births declined slightly for 1997 and 1998 from 6.08 to 6.05 percent for the entire population.

Our study sample (Graph 8) shows a rise in the percentage of multiple births from 2.23% in 1995 to 2.48% in 1998, with a similar increase among blacks from 2.72% to 2.99% during the same period. The incidence of low birth weight rose steadily from

[6] Rates refer to the proportion of live births to unmarried women per 1,000 unmarried women.

7.8% to 8.18% (Graph 7), albeit accompanied by improved prenatal care. The proportion of women who receive no prenatal care at all decreased from 2.54 % to 1.67% (Graph 6). While 6.97 % of women were putting off their prenatal care in 1991, 6.48% did so in 1998 (Graph 5). In an effort to tease out how much the incidence of low birth weight and prenatal care are associated with welfare reform, my study resorts to a multivariate analysis controlling for other covariates.

2) Model Descriptions

All models include a host of individual characteristic variables including age, education, race, and an indicator of previous birth. They also include state unemployment level and Medicaid income thresholds, but Model 1 excludes welfare reform variables, a broad measure capturing both 1997 welfare reform and the previous waivers granted to some states. These are dichotomous variables indicating whether women conceived their babies prior to welfare reform or afterwards. Model 2 includes the welfare reform dummy variable. Model 3 divides welfare reform into two categories: TANF and waivers. The waiver dummy in Model 3 indicates whether a state implemented any of the following waiver components: time limit 1("Termination and Reduction Time Limit"), time limit 2 ("Work Requirement Time Limit"), sanctions, income disregard, Family Cap, and work exemption. Model 4 refines the different characteristics of waivers into separate dummies but excludes the TANF variable.

The income ceiling for Medicaid eligibility is included as a control variable. The Medicaid eligibility variable will also serve to assess whether welfare reform is negatively associated with Medicaid generosity; welfare reform may have reduced the

benefits of increased income ceilings governing Medicaid eligibility. Previous literature (Cole, 1995; Currie and Gruber, 1996; Long and Marquis, 1996; Piper, Mitchel and Ray, 1994) has documented some beneficial effects of increases in eligibility for Medicaid coverage governing income ceilings.

Table 2 shows Medicaid eligibility thresholds or income ceilings implemented by the 10 biggest states. These ceilings are determined once the official poverty threshold is obtained. The OPT (Official Poverty Threshold) is calculated by multiplying the cost of the economy food plan by three for a typical family of three. For instance, Florida has an income ceiling of 185% in 1999 for Medicaid eligibility. This means that families whose income is below 185% of the OPT are eligible for the public insurance program. For ease of interpretation, I normalized the income ceilings, and thus 100% of the threshold becomes 1.

In the birth outcomes regressions, the model includes a dummy variable indicating singleton births. Without taking into account a considerable increase in multiple births during the recent years, welfare reform may appear responsible for the increase in the incidence of low birth weight. All models employ a linear probability model but my study also reports the results of a logistic regression model for selective variables to demonstrate that marginal effects of the variables obtained from both methods are quite similar:

$$P = \frac{1}{1 + e^{x_i / \beta_i}}$$

where P as a dummy variable is defined as 1 or 0. The X_i's are covariates, and B_i's the estimated parameters. A positive value implies that the probability of welfare reform is positively associated with that variable. The easiest way to interpret these coefficients is to convert them into the partial effect of the exogenous variable on the probability $\partial p / \partial x$, equal to $B \cdot P \cdot (1 - P)$, usually expressed at the mean value of P.

One particular concern is that welfare reform dummy variables may be highly correlated, especially between TANF and the different components of waivers. For that particular reason, Model 4 excluded the TANF dummy variable that should be highly correlated with other waiver dummies. The different components of waivers were launched at different times, and so they should be less correlated. Indeed, the correlation coefficients among those dummy variables are very low, ranging from -0.21(between timelimit2 and sanction) to 0.42 (income disregard and sanction). Nevertheless, the large size (6,688,852 observations) of our data allows more accurate estimates than would a small one, since a large sample normally reduces somewhat the variance of estimated coefficients, diminishing the impact of mutilcollinearity.

In an attempt to avoid the possible muticollinearity problem and address the issue of the extent of reforms, I also created two dummy variables indicating "low" and " high" intensity reforms. One of the most profound changes in welfare policy was the imposition of time limits. Welfare recipients can receive benefits for no more than 60 months during their lifetime. I created a dummy variable, "time limit" incorporating either "timelimit1" or "timelimit2" for the states that implemented those policies without sanctions, work exemption or family cap. The second variable is "high" that indicates states

implementing both time limits and one or more of such variables as sanctions, work exemption and family cap. This exercise will lead to investigate whether " low " and " high" intensity reforms have differential impact on prenatal care utilization and birth outcomes.

First, I will present the results of the multivariate analysis of the equation (2). These regressions concentrate on the treatment group. As stated earlier, this method is complementary to the difference in difference method in that the outcomes of the equation (2) may bolster those of the difference in difference method. This will be the case, particularly when a misclassification of the treatment group and the control group is minimized.

3) Econometric Results: Much Ado About Very Little

1. Treatment Group Estimation

a) Effects of Welfare Reform on Prenatal Care Visits (Table 3 to Table 6)

It turns out that welfare reform is associated with both delays in prenatal care visits and absent prenatal care, although it appears to increase number of prenatal care visits by two fifths of a prenatal care visit, a negligibly positive increase. The magnitudes of these associations are, however, quite small. Relative to the mean, welfare reform (Model 2) increases by 0.4 percentage points the probability that women will initiate their prenatal care in the third trimester. This rise from 9.4% to 9.8% constitutes a 4 percent change. Welfare reform reduces the probability that women will begin their visits to doctors in the first trimester by 0.9 percentage points, namely 2% relative to the mean of 47 percent. Welfare reform also raises the likelihood that women will not make any

prenatal care visits at all. The magnitude of this effect is 0.5 percentage points relative to the mean of 3.9%, which corresponds to a 12.8 percent increase.

The consistent effects of welfare reform on prenatal care are potential evidence of causality linking welfare reform and prenatal care visits. This would imply that welfare reform triggered the behavioral shifts in prenatal care visits. Model 3 shows inconsistent signs of the waiver variable. The waiver variable tends to be positively associated with early prenatal care but increase the likelihood that women will receive no prenatal care at all. The signs of the various waiver components (Model 4) are for the most part consistent. Except for the number of prenatal care visits, both the "Family Cap" and the sanction variables are negatively associated with prenatal care visits and are statistically significant for the three regressions including Table 3, Table 4 and Table 6. However, work exemption appears to elicit early prenatal care visits and reduce the probability that women delay their prenatal care visits. The "income disregard" variable shows an expected positive effect on prenatal care except for the number of prenatal care visits.

These mixed signs of the variables may explain the inconsistency of the waiver variable in Model 2. The overall effect of the different components of waiver variables, when grouped together into the waiver variable in Model 3, may be ambiguous. The magnitudes of the waiver variables in Model 4 are all minute. Only the magnitude of the income regard variable in Table 3 and the sanction variable in Table 6 exceeds that of welfare reform in Model 2. In light of these findings, welfare reform has little impact on prenatal care visits.

b) Effects of Welfare Reform on Birth Outcomes (Table 7 to Table 9)

These reduced form birth outcome equations reflect the direct marginal products of welfare reform as well as the effects of welfare reform on birth outcomes through prenatal care inputs. The association between welfare reform and prenatal care visits is consistently negative, as is the association between welfare reform and birth outcomes. These findings suggest that a causal mechanism is at work. Welfare reform triggers behavioral changes in prenatal care and these changes may worsen birth outcomes. Again, the magnitudes are small and even negligible, and the correlation may be quite tenuous.

Welfare reform (Model 2) increases the incidence of low weight babies by 0.7 percentage points, a 6.4 percent increase relative to the mean of 12.48%. Also, welfare reform (Model 2) increases the incidence of very low weight babies by 0.4 percentage points. The magnitudes are quite small, and even the observed associations might result from a failure to control for omitted factors. Further, consider that $\partial BO/\partial WF=(\partial BO/\partial PC)*(\partial PC/\partial WF)$. Knowing $(\partial PC/\partial WF)$ and $(\partial BO/\partial WF)$, we can derive $(\partial BO/\partial PC)$. For the moment, assume, based on the results from Table 3 and 7, that welfare reform causes an increase in prenatal care delay by 0.4 percentage points (Table 3) and welfare reform also triggers a 0.7 percentage point increases in the incidence of low birth weight. Then $(\partial BO/\partial PC)$, namely, the increase in the incidence of low birth weight caused by delay in prenatal care is 1.7 percentage points. But this computation is based on the assumption that the 0.7 percentage points in the incidence of low birth weight is all due to the delay in prenatal care. Thus the 1.7 percentage point change is an upper bound estimation. If we assume that the changes in prenatal care visits

do not alter birth outcomes, the 0.7 percentage point change in the incidence of low birth outcomes should be entirely due to the direct impact of welfare reform on birth outcomes. As discussed earlier, the direct impact may work through factors including income changes, stress and diet etc, but it also appears to be too small to be significant.

The various waiver variables in model 4 show consistent and expected signs. The "income disregard" variable is positively associated with birth outcomes, whereas sanction and time limit 2 variables are negatively correlated with birth outcomes. Those three variables are statistically significant in both very low and low birth weight regressions. Just as much as in the prenatal care visit regressions, sanction and income disregard variables seem to come into play more significantly than the other variables. They have consistent signs, and their magnitudes are relatively larger than those of the others. In particular, the "income disregard" variable plays a pivotal role in minimizing the negative effects that may be caused by other welfare waiver variables; it appears to dampen the negative impacts of the other waiver variables.

C) Effects of Medicaid Income Ceilings on Prenatal Care Utilization and Birth Outcomes

The Medicaid generosity variable shows the expected opposite sign of welfare reform, and its effects on prenatal care visits are consistently positive. Its magnitudes are, however, bigger relative to those of the welfare reform variable. In particular, a 100 percent increase in Medicaid income eligibility raises the probability that women initiate their prenatal care in the first trimester by 1.5 percentage points. This increase may be rendered possible because more impoverished women, previously uninsured, become

publicly insured. This finding supports the results provided by previous researchers (Currie and Gruber, 1996).

The magnitude of the Medicaid generosity variable in Model 1 becomes bigger relative to Model 2 in all three prenatal care regressions, including Table 3, Table 4, and Table 6. This result confirms the negative association between welfare reform and Medicaid generosity, suggesting that welfare reform may have played a deterrent role to Medicaid eligibility expansions. If enrollment in Medicaid had declined in the wake of welfare reform, the benefits of Medicaid eligibility expansions would have diminished. The Medicaid generosity variable also has the expected sign on the birth outcome regressions. It is positively associated with measures of birth outcomes but its magnitude is negligible. However, the negative association between welfare reform and Medicaid generosity is also at work.

d) Effects of State Unemployment on Prenatal Care Utilization and Birth Outcomes

The positive association between the state unemployment rate and prenatal care visits suggests that an increase in time away from work is an important factor determining behavioral changes in prenatal care visits. The state unemployment rate variable is, as with prenatal care utilization, positively associated with birth outcomes, but with a negligible magnitude. Rhum (1998) shows that entitlements to parental leave are negatively correlated with postneonatal and child mortality rates. Since raising children is an extremely time-intensive activity, parental leave is likely to affect child health by making more time available to parents. Pregnant women's employment may increase their potential incomes but also their time constraints. If the negative impact of

time constraints on prenatal care visits is likely to outweigh the positive impact of income gains, then it seems logical to observe a positive association between the state unemployment rate and prenatal care utilization. If data on pregnant women's unemployment were readily available and used in the regression, the result might have shown a much stronger negative sign.

Ruhm (2000) also shows that total mortality and eight of ten sources of fatality are inversely related to state unemployment rates. When referring to the Grossman Model (1972), one may discover that Ruhm's findings are supportive of a consumption model rather than an investment model. However, much of the previous literature provided evidence of an investment model because an increase in wage was found to increase health outcomes (Wagstaff, 1986; Erbsland, Ried, and Ulrich, 1995).

e) Discussion of other individual characteristics:

Each model includes an indicator of singleton birth. A multiple birth tends to be at a 54% higher risk than its counterparts of being low birth weight. All the other variables showed the expected signs with reasonable magnitudes. Black women deliver low weight babies with a 6% higher probability than white women. High school graduates show a 2% lower risk of delivering low weight babies than do those with less than a high school education. Following the addition of a host of welfare reform variables, the coefficients are virtually unchanged from Model 1 to Model 4, indicating that such variables as age, education, race and previous births are independent of welfare reform.

f) Different Model Specifications (Table 10)

Table 10 shows the coefficient of the welfare reform variable obtained by different model specifications. Model 2 is the one used from Table 3 to Table 9. The reason why preference is given to this model over the other two is to control for state specific linear time trend. The inclusion of the state-specific time trend variable would have alleviated potential biases had welfare reform been correlated with state specific time trend. Since Model 2 and Model 5 are similar in the magnitudes of their coefficients, the association between welfare reform and state specific time trend is almost absent. The difference between Model 5 and Model 6 is to replace linear time trend with year fixed effect. The similar results of the two models suggest that year fixed effects are equivalent to controlling linear time trends. Indeed, linear time trends in all dependent variables are apparent as shown in Graphs 5 to 9.

After all, the coefficients of the welfare reform variable appear to be robust to different model specifications. Thus, it seems that all the other variables used in Tables 3 to 9 will have the same robustness. As suggested previously, the number of prenatal care visits variable behaves in a completely different manner; specifically it flips the sign depending on the model, and the variation of the magnitudes is also substantial.

g) Comparison between Linear Probability Model and Logit Model (Table 11)

Table 11 compares the coefficients obtained by a linear probability model and a logit model. We obtain the marginal effects explained earlier in the logit model. For instance, the marginal effect of early prenatal care visit induced by welfare reform is –0.0071. The number is obtained by multiplying the coefficient, -0.0394 by P(1-P) at the mean value of P, the probability of early prenatal care visit. The mean of P is 47%, and

the marginal effect becomes –0.0098, which is nearly identical to the figure of –0.0089 that was obtained by the linear probability model. For late prenatal care visit, low birth weight and very low birth weight regression, the marginal effects of the logit model provide 0.0044, 0.0079, and 0.0038 respectively.

h) "Low" and "High" Intensity Reforms (Table 12)

The first column in Table 21 shows the states that implemented a minor reform. "Low" intensity reform indicates states that only implemented time limits without any other reforms. Indeed, one of the most controversial welfare reform measures was the imposition of time limits on cash aid to welfare recipients. Time limits represent a grand sweeping shift in welfare policy. This low intensity reform is positively associated with prenatal care utilization. For instance, it increases the probability of early prenatal care and reduces the likelihood that women will put off prenatal care or receive no prenatal care at all. These outcomes are the opposite of what the "welfare reform" variable produced, but they are the approximate average of both the " time limit 1" variable and the "time limit 2" variable in Model 4. The magnitudes are relatively large.

The positive result on prenatal care utilization may stem from an alteration in the " time preference" of pregnant women; they may have become more forward-looking due to these policy changes. But it appears that these changes do not necessarily trigger better birth outcomes since birth outcomes are negatively correlated with "low" intensity reform. This result suggests that the negative association between the birth outcomes and "low" intensity reform may be due to factors other than prenatal care utilization.

"High" intensity reform indicates a dummy for the states that implemented not only time limits, but also one of such welfare components as family cap, work exemption, and sanction. "High" intensity reform seems to play a negative role in prenatal care utilization as opposed to "low" intensity reform. When more than one of the variables such as family cap, work exemption, and sanction are imposed, that would have dampened the positive effect of the " timelimit" variable. Particularly, in the view that family cap and sanction are income-related variables, the negative impact of "high" intensity reform suggests that the negative income effect adds to the negative substitution effect followed by the rising full price of prenatal care. If so, a logical conclusion should be that one may expect more negative impact of the high intensity reform variable on birth outcomes, a finding that will support a causal interpretation. Indeed, it turns out that the magnitudes of the birth outcome coefficients are larger that those in the "low" intensity regressions.

i) Separate Regressions for Blacks and Whites (Table 13 to 19)

I conduct separate analyses by race because of the evidence of systematic differentials in birth outcomes and prenatal care utilization. The separate regression results conform to what one might intuitively expect. Welfare reform coefficients from Model 2 to Model 3 display larger magnitudes for blacks than for whites. The sanction and income disregards variables show the same signs as ones obtained from the pooled regression. The sanction variable is negatively associated with prenatal care utilization and birth outcomes, and the income disregards variable appears to positively affect

prenatal care utilization and birth outcomes. The effects of these two variables are consistent along the different regressions (Table 13 to Table 19).

One striking feature is that the income disregards variable has an impact only on blacks, not on whites. Presumably, the models do not hold household income constant. An inclusion of the income variable should have mitigated the results shown for blacks of the income disregards variables. However, the income disregards variable appears to be as important as additional income. The same is true of the sanction variable. The magnitudes of the sanction variables are much greater for blacks than for whites. Coupled with the income disregards variable, the work exemption variable operates positively on prenatal care utilization and birth outcomes, but the favorable effects are only apparent among blacks.

2) Impact of Welfare Reform on Marital Status

Prior to my implementation of difference in difference multivariate method, I investigate the legitimacy of the use of marital status to select the sample and define treatment and control groups. This analysis serves to detect whether welfare reform may have affected the marriage decision. If so, there will be compositional change in the group of married or unmarried women before and after welfare reform. Such compositional change may confound estimates of the effect of welfare reform; our control group consistes of married women with fewer than 12 years of schooling, and the treatment group is unmarried women with fewer than 12 years of schooling. A study on the impact of welfare reform on marital status has its own value since welfare reform is intended to reduce the wedlock birth rates. For this analysis, I limited the sample to both

married and unmarried women with 12 or fewer years of education and estimated a model similar to equation (2) except I used marital status as the dependent variable.

Given a significant body of literature that AFDC benefits are not related to the probability of an out-of-wedlock birth (Moore and Caldwood, 1977; Duncan and Hoffman, 1990), one would expect that welfare reform, unless dramatic, would not alter marital decision considerably. A main finding is that welfare reform as a whole is not associated with the marital status of this target group. However, the TANF alone would have negatively affected marital status, whereas the waiver variable has the positive sign on the probability of being married, after controlling for other individual covariates. The positive effect of the " waiver" variable seems to be driven mainly by the " low" intensity reform variable. The latter variable tends to increase the likelihood that babies are born from married couples by 1.2 percentage points. This increase constitutes a 2 percent change. But " high" intensity reform has the opposite result, reducing the probability of births to married women. The magnitude is quite similar to that of "low intensity reform," albeit in the opposite direction. Thus, these contrary results determine that the constellation of all welfare components is not associated with a shift in marital status.

Thus, the constellation of all welfare components, which is the welfare reform variable, turns out to display weak explanatory power within this marital status regression; the magnitude is negligible and statistically insignificant. In light of the finding that there is strong evidence that welfare reform as a whole does not affect marriage, the use of marital status in dichotomizing the treatment group and the control group is warranted in the difference in difference estimation.

3) Difference in Difference Estimation

Table 21 shows coefficients of the treatment group and the welfare reform interaction term in the econometric equation (1), a difference in difference econometric estimation procedure described earlier. The signs of all the coefficients conform to the previous findings, although the magnitudes are smaller. Welfare reform is negatively associated with prenatal care utilization and birth outcomes. For instance, the treatment group, unmarried women with fewer than 12 years of schooling, is likely to reduce by 0.5 percentage points its first trimester prenatal care visits, and is 0.6 percentage points more likely to receive no prenatal care at all. A negative association is also found between welfare reform and the birth outcomes of the treatment group.

The magnitudes that the difference in difference estimation provides are somewhat smaller than those of the previous method using only the treatment group. A possible reason is that selection bias is at work where welfare reform induces women to get married. Then the coefficients may be downward biased. However, because Table 12 indicates that there is no major association between welfare reform and marital status, change in marital status may not be responsible for the putative bias. The discrepancy between the previous estimation and the difference in difference estimation should be caused mainly by inevitable misclassification of the treatment group and the control group

For instance, let us consider that most women in the target group, undereducated, and unmarried women in our classification, are affected by welfare reform. A significant number of women in the comparison group, educated and married, may be borderline

welfare recipients and thus affected by welfare reform. If this misclassification is relatively minor, DD estimates will be biased downward, but if it is extreme, DD estimates may even display the wrong sign.

Let us discuss more systematically this potential bias issue. Let the true estimate of the effect of welfare reform on prenatal care inputs and birth outcomes be DD=(A-B)-(C-D) as illustrated earlier. If we assume that there is misclassification only among the control group and that in fact 20 percent of the control group consists of members of the treatment group, then the estimate becomes DD'= .8DD =(A-B)-{(.8C+.2A)-(.8D+.2B)}=.8A-.8B-.8C+.8D=.8{(A-B)-(C-D)}. Then the difference in difference estimation will underestimate the true effect of welfare reform by 20 percent. It can be shown that our DD estimate will have the wrong sign if and only if the sum of the proportion of misclassified members of the treatment and control groups is greater than one, an extremely unlikely case.

This inevitable minor misclassification into treatment group and control group in a social experiment, unlike in a clinical trial, triggers some biased estimation but provides estimates bearing correct signs as long as misclassification is not substantial. This is exactly the case for our possible downward biased difference in difference estimation results with the sign conforming to the previous results. Thus, it turns out that the DD results bolster the previous regression results obtained using only the treatment group, and the causal mechanism is apparent; welfare reform negatively affects prenatal care utilization and birth outcome among the target group, mostly unmarried and less educated women.

Conclusion

Econometric estimations offer consistent evidence that welfare reform is negatively associated with prenatal care utilization and birth outcomes. Further, welfare reform appears to dampen the beneficial effects of Medicaid expansions on income ceilings. The negative effects of welfare reform on prenatal care utilization, birth outcomes, and Medicaid generosity are quite minute. However, from a policy perspective, it is important to acknowledge that welfare reform triggered the behavioral shifts, however small their magnitude, in prenatal care visits, probably through a decreased enrollment in Medicaid and thus birth outcomes. An effort should be made to ensure that the negative impact is not intensifying for the longer run. The mixed effects of various aspects of welfare reform on prenatal care visits suggest that the entire constellation of welfare reform (Model 2) rather than each of welfare reform components should be considered.

The "sanction" has the potential to increase the incidence of low weight births. However, "income disregard" may reduce the potential negative impact. Policy makers should keep investigating the time trend of both variables and its impact on prenatal care visits and births outcomes, and they should be able to use income disregard to cushion the negative impact of other components of welfare variables should such a circumstance arise. Income disregard and sanctions are both directly related to cash benefits. This view is particularly supported by the finding that "high" intensity reform incorporating either sanctions, family cap or work exemption, offsets the positive effect of "low" intensity reform on prenatal care utilization.

This fact implies that insufficient income would adversely affect prenatal care utilization and infant birth outcomes, thus making the continuous entitlement to Medicaid assistance more important than ever. Finally, it should be kept in mind that birth weight is only one measure of child health and that maternal compliance with welfare reform may have a negative impact on the health and development of older children.

Table 1

Descriptive Statistics (Up to 12 years of Educational Attainment by Race) from 1991 to 1998

	Overall	Blacks	Whites
Out-of wedlock births	43.17	78.77	36.04
Education			
High school drop out	27.77	28.30	32.06
High school diploma	72.23	71.70	67.94
Age			
Less than 20 Years	21.15	31.20	19.14
20 to 24 years	32.24	32.88	32.12
25 to 30 years	24.48	19.14	25.55
30 to 34 years	15.15	11.19	15.94
Over 35 years	6.98	5.59	7.26
Multiple Births	2.29	2.79	2.18
Parity			
First Child	39.27	37.03	39.72
Second Child	30.64	27.47	31.28
Third Child	17.12	17.58	17.02
Fourth Child ond over	12.97	17.92	11.98
Birth Outcomes			
Gestational Weeks	39.56	38.82	39.7
	(11.45)*	(6.51)	(6.61)
Preterm Delivery	11.82	18.84	10.42
Mean Birth Weight	3297.75	3087.5	3339.8
	(1102.37)	(694.26)	(615.73)
LBW***	7.99	13.38	6.82
VLBW****	1.43	2.96	1.12
Prenatal Care Utilization			
Late Prenatal Care	6.51	9.43	5.92
Early Prenatal Care	73.83	64.72	75.67
No Prenatal Care	1.96	4.03	1.54
Smoking	21.44	13.61	23.26
# of Observations	6688852	3443815	3445037

* Standard deviations are in parenthesis

** Excluding the gestational weeks and birth weight variables, all the other variables indicate percentages.

*** LBW indicates the percentage of low birth weight babies, weighing less than 2,500 grams

****VLBW indicates the percentage of very low birth weight babies, weighing less than 1,500 grams

Table 2
Medicaid Eligibility Thresholds

States/Year	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999
California	185*	185	185	185	185	185	200	200	200	200	300
Florida	150	150	150	150	185	185	185	185	185	185	185
Illinois	133	133	133	133	133	133	133	133	133	133	200
Massachusettes	185	185	185	185	185	185	185	185	185	185	200
Michigan	185	185	185	185	185	185	185	185	185	185	185
New Jersey	133	133	133	185	185	185	185	185	185	185	185
New York	100	185	185	185	185	185	185	185	185	185	185
Ohio	133	133	133	133	133	133	133	133	133	133	133
Pennsylvania	133	133	133	133	185	185	185	185	185	185	185
Texas	133	133	133	185	185	185	185	185	185	185	185

* All numbers indicate percentage of the OPT (Official Poverty Threshold)
**Sources: *MCH (Maternal and Child Health) Update,* National Governors Association from 1989 to 1999

Table 3
Effects of Welfare Reform on Prenatal Care Use (Third Trimester Care)
Unmarried and under 12 years of educational attainment

	Model 1	Model 2	Model 3	Model 4
Welfare Reform		0.0038***		
		(0.0004)		
TANF			0.0053***	
			(0.0004)	
Waiver			-0.0057***	
			(0.0006)	
Time limit 1				-0.0006
				(0.0014)
Time Limit 2				-0.0021*
				(0.0012)
Sanction				0.0029**
				(0.0013)
Income disregard				-0.0081***
				(0.0012)
Family Cap				0.0022**
				(0.0009)
Work exemption				-0.0058***
				(0.0013)
Medicaid generosity	-0.0069***	-0.0075***	-0.0067***	-0.0081***
	(0.0012)	(0.0012)	(0.0012)	(0.0011)
Black	0.0172***	0.0172***	0.0172***	0.0172***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Previous Birth	0.0370***	0.0370***	0.0370***	0.0370***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
High School	-0.0227***	-0.0227***	-0.0228***	-0.0227***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Age 20-24	-0.0140***	-0.0140***	-0.0139***	-0.0140***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Age 25-29	-0.0206***	-0.0206***	-0.0206***	-0.0206***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Age 30-34	-0.0120***	-0.0197***	-0.0197***	-0.0197***
	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Age >34	-0.0146***	-0.0145***	-0.0145***	-0.0145***
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Unemployment rate	-0.0018***	-0.0017***	-0.0017***	-0.0019***
	(0.0003)	(0.0003)	(0.0002)	(0.0003)
Mean of Dep. Var.	0.0948	0.0948	0.0948	0.0948
Number of Obs.	3954070	3954070	3954070	3954070

All models also include dummy variables indicating state of residence, linear time trend, and state-specific time trends. Standard errors are in parentheses.

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01

Table 4
Effects of Welfare Reform on Prenatal Care Use (First Trimester Care)
Unmarried and under 12 years of educational attainment

	Model 1	Model 2	Model 3	Model 4
Welfare Reform		-0.0089***		
		(0.0007)		
TANF			-0.0082***	
			(0.0007)	
Waiver			0.0030***	
			(0.0012)	
Time limit 1				-0.0012
				(0.0023)
Time Limit 2				0.0032
				(0.0022)
Sanction				-0.0007
				(0.0022)
Income disregard				0.0018
				(0.0019)
Family Cap				-0.0040**
				(0.0016)
Work exemption				0.0028
				(0.0022)
Medicaid generosity	0.0150***	0.0164***	0.0155***	0.0143***
	(0.0020)	(0.0020)	(0.0020)	(0.0020)
Black	-0.0379***	-0.0378***	-0.0378***	-0.0379***
	(0.0005)	(0.0006)	(0.0006)	(0.0006)
Previous Birth	-0.0851***	-0.0850***	-0.0851***	-0.0851***
	(0.0006)	(0.0006)	(0.0005)	(0.0006)
High School	0.0648***	0.0539***	0.0539***	0.0539***
	(0.0006)	(0.0005)	(0.0005)	(0.0005)
Age 20-24	0.0647***	0.0648***	0.0647***	0.0648***
	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Age 25-29	0.1018***	0.1018***	0.1018***	0.1018***
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Age 30-34	0.1051***	0.1051***	0.1051***	0.1051***
	(0.0010)	(0.0010)	(0.0010)	(0.0010)
Age >34	0.0900***	0.0900***	0.0900***	0.0900***
	(0.0013)	(0.0013)	(0.0013)	(0.0013)
Unemployment rate	0.0015***	0.0012***	0.0014***	0.0016***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Mean of Dep. Var.	0.4738	0.4738	0.4738	0.4738
Number of Obs.	3954070	3954070	3954070	3954070

All models also include dummy variables indicating state of residence and state-specific time trends. Standard errors are in parentheses.

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 *0.00< p-value < 0.01

Table 5
Effects of Welfare Reform on Prenatal Care Use (Number of Prenatal Care Visits)
Unmarried and under 12 years of educational attainment

	Model 1	Model 2	Model 3	Model 4
Welfare Reform		0.0679**		
		(0.0289)		
TANF			0.2459***	
			(0.0272)	
Waiver			-0.3900***	
			(0.0466)	
Time limit 1				0.1879***
				(0.0915)
Time Limit 2				-0.1193
				(0.0804)
Sanction				0.3061***
				(0.0824)
Income disregard				-0.8788***
				(0.0834)
Family Cap				-0.1314*
				(0.0686)
Work exemption				-0.9933***
				(0.0851)
Medicaid generosity	-0.5872***	-0.5981***	-0.5625***	-0.4157***
	(0.0726)	(0.0729)	(0.0730)	(0.0746)
Black	0.9406***	0.9403***	0.9402***	0.9407***
	(0.0217)	(0.0217)	(0.0217)	(0.0217)
Previous Birth	-0.6767***	-0.6770***	-0.6770***	-0.6775***
	(0.0222)	(0.0222)	(0.0222)	(0.0223)
High School	0.3704***	0.3705***	0.3705***	0.3703***
	(0.0204)	(0.0204)	(0.0204)	(0.0204)
Age 20-24	0.3128***	0.3127***	0.3122***	0.3124***
	(0.0250)	(0.0250)	(0.0250)	(0.0250)
Age 25-29	0.5768***	0.5768***	0.5766***	0.5770***
	(0.0328)	(0.0328)	(0.0328)	(0.0328)
Age 30-34	0.5937***	0.5938***	0.5933***	0.5939***
	(0.0418)	(0.0418)	(0.0418)	(0.0418)
Age >34	0.7761***	0.7762***	0.7764***	0.7768***
	(0.0571)	(0.0571)	(0.0571)	(0.0571)
Unemployment rate	0.0090	0.0112***	0.0097	0.0087
	(0.0180)	(0.0180)	(0.0180)	(0.0181)
Mean of Dep. Var.	13.8475	13.8475	13.8475	13.8475
Number of Obs.	3952931	3952931	3952931	3952931

All models also include dummy variables indicating state of residence and state-specific time trends. Standard errors are in parentheses.

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01

Table 6
Effects of Welfare Reform on Prenatal Care Use (No Prenatal Care)
Unmarried and under 12 years of educational attainment

	Model 1	Model 2	Model 3	Model 4
Welfare Reform		0.0051***		
		(0.0003)		
TANF			0.0021***	
			(0.0003)	
Waiver			0.0053***	
			(0.0004)	
Time limit 1				-0.0068***
				(0.0009)
Time Limit 2				0.0008**
				(0.0007)
Sanction				0.0096***
				(0.0008)
Income disregard				0.0008
				(0.0007)
Family Cap				0.0027***
				(0.0005)
Work exemption				-0.0024***
				(0.0009)
Medicaid generosity	-0.0028***	-0.0035***	-0.0037***	-0.0038***
	(0.0009)	(0.0009)	(0.0009)	(0.0009)
Black	0.0169***	0.0168***	0.0168***	0.0168***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Previous Birth	0.0260***	0.0260***	0.0260***	0.0260***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
High School	-0.0187***	-0.0187***	-0.0187***	-0.0187***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Age 20-24	0.0004***	0.0004	0.0004	0.0004
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Age 25-29	0.0100***	0.0097***	0.0097***	0.0097***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Age 30-34	0.0188***	0.0189***	0.0188***	0.0188***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Age >34	0.0233***	0.0233***	0.0233***	0.0233***
	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Unemployment rate	-0.0004**	-0.0002	-0.0003	-0.0004**
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Mean of Dep. Var.	0.0393	0.0393	0.0393	0.0393
Number of Obs.	3954069	3954069	3954069	3954069

All models also include dummy variables indicating state of residence and state-specific time trends. Standard errors are in parentheses.

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01

Table 7
Effects of Welfare Reform on Birth Outcome (Low Birth weight)
Unmarried and under 12 years of educational attainment

	Model 1	Model 2	Model 3	Model 4
Welfare Reform		0.0073***		
		(0.0005)		
TANF			0.0053***	
			(0.0005)	
Waiver			0.0042***	
			(0.0007)	
Time limit 1				0.0018
				(0.0015)
Time Limit 2				0.0047***
				(0.0014)
Sanction				0.0082***
				(0.0015)
Income disregard				-0.0042***
				(0.0013)
Family Cap				-0.0009
				(0.0011)
Work exemption				-0.0026*
				(0.0015)
Medicaid generosity	-0.0004	-0.0016	-0.0017	-0.0011
	(0.00143)	(0.0014)	(0.0014)	(0.0014)
Black	0.0558***	0.0558***	0.0557***	0.0557***
	(0.0004)	(0.0003)	(0.0004)	(0.0004)
Previous Birth	-0.0118***	-0.0118***	-0.0118***	-0.0118***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Male	-0.0203***	-0.0203***	-0.0203***	-0.0203***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
High School	-0.0177***	-0.0177***	-0.0177***	-0.0177***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Age 20-24	-0.0002	-0.0002	-0.0002	-0.0002
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Age 25-29	0.0205***	0.0205***	0.0205***	0.0205***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Age 30-34	0.0502***	0.0502***	0.0502***	0.0502***
	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Age >34	0.0735***	0.0735***	0.0735***	0.0735***
	(0.0010)	(0.0010)	(0.0010)	(0.0010)
Singleton Births	-0.5399***	-0.5399***	-0.5399***	-0.5399***
	(0.0015)	(0.0015)	(0.0015)	(0.0015)
Unemployment rate	-0.0004	-0.0001	-0.0002	-0.0002
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Mean of Dep. Var.	0.1248	0.1248	0.1248	0.1248
Number of Obs.	3954070	3954070	3954070	3954070

Table 8
Effects of Welfare Reform on Birth Outcome (Very Low Birth weight)
Unmarried and under 12 years of educational attainment

	Model 1	Model 2	Model 3	Model 4
Welfare Reform		0.0036***		
		(0.0002)		
TANF			0.0022***	
			(0.0002)	
Waiver			0.0021***	
			(0.0003)	
Time limit 1				0.0010
				(0.0007)
Time Limit 2				0.0025***
				(0.0006)
Sanction				0.0045***
				(0.0007)
Income disregard				-0.0012**
				(0.0006)
Family Cap				0.0010
				(0.0007)
Work exemption				-0.0002
				(0.0007)
Medicaid generosity	-0.0017**	-0.0023***	-0.0023***	-0.0020***
	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Black	0.0145***	0.0145***	0.0145***	0.0145***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Previous Birth	-0.0054***	-0.0055***	-0.0055***	-0.0054***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Male	-0.0002	-0.0002	-0.0002	-0.0002
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
High School	-0.0015***	-0.0015***	-0.0015***	-0.0015***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Age 20-24	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Age 25-29	0.0051***	0.0050***	0.0050***	0.0050***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Age 30-34	0.0114***	0.0114***	0.0114***	0.0114***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Age >34	0.0156***	0.0156***	0.0156***	0.0156***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Singleton Births	-0.1487***	-0.1487***	-0.1487***	-0.1487***
	(0.0012)	(0.0012)	(0.0012)	(0.0012)
Unemployment rate	-0.0000	0.00010	0.0000	0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Mean of Dep. Var.	0.0252	0.0252	0.0252	0.0252
Number of Obs.	3954070	3954070	3954070	3954070

Table 9
Effects of Welfare Reform on Birth Outcome (Log Birth weight)
Unmarried and under 12 years of educational attainment

	Model 1	Model 2	Model 3	Model 4
Welfare Reform		-0.0071***		
		(0.0004)		
TANF			-0.0045***	
			(0.0004)	
Waiver			-0.0049***	
			(0.0006)	
Time limit 1				-0.0029**
				(0.0013)
Time Limit 2				-0.0073***
				(0.0011)
Sanction				-0.0103***
				(0.0012)
Income disregard				0.0044***
				(0.0011)
Family Cap				-0.0014*
				(0.0009)
Work exemption				0.0013
				(0.0012)
Medicaid generosity	0.0017	0.0028**	0.0029**	0.0020*
	(0.0012)	(0.0012)	(0.0012)	(0.0012)
Black	-0.0709***	-0.0707***	-0.0708***	-0.0708***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Previous Birth	0.0181***	0.0181***	0.0181***	0.0181***
	(0.0003)	(0.0013)	(0.0003)	(0.0003)
Male	0.0334***	0.0334***	0.0334***	0.0334***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
High School	0.0174***	0.0174***	0.0174***	0.0174***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Age 20-24	0.0030***	0.0030***	0.0030***	0.0030***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Age 25-29	-0.0086***	-0.0086***	-0.0086***	-0.0086***
	(0.00050	(0.00050	(0.0005)	(0.0005)
Age 30-34	-0.0281***	-0.0281***	-0.0281***	-0.0281***
	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Age >34	-0.0421***	-0.0421***	-0.0421***	-0.0421***
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Singleton Birth	0.4220***	0.4220***	0.4220***	0.4220***
	(0.0014)	(0.0014)	(0.0015)	(0.0015)
Unemployment rate	0.0005	0.0005	0.0004	0.0005
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Mean of Dep. Var.	8.0181	8.0181	8.0181	8.0181
Number of Obs.	3954070	3954070	3954070	3954070

Table 10
Different Model Specifications for the " Welfare Reform" Variable

	Model 2*	Model 5**	Model 6***
Late Prenatal Care	0.0038***	0.0035***	0.0050***
	(0.0004)	(0.0004)	(0.0003)
Early Prenatal Care	-0.0089***	-0.0094***	-0.0107***
	(0.0007)	(0.0007)	(0.0008)
No Prenatal Care	0.0051***	0.0056***	0.0067***
	0.0003	(0.0003)	(0.0003)
# of Prenatal Care	0.0679**	-0.0566**	0.0258
	(0.0289)	(0.0281)	(0.0305)
Low Birth Weight	0.0073***	0.0076***	0.0096***
	(0.0005)	(0.0005)	(0.0006)
Very Low Birth Weight	0.0036***	0.0035***	0.0047***
	(0.0002)	(0.0002)	(0.0003)
Log Birth Weight	-0.0071***	-0.0070***	-0.0080***
	(0.0004)	(0.0004)	(0.0003)

Standard errors are in parentheses.
*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 *0.00< p-value < 0.01

Model 2 includes state fixed effect, linear time trend and state and time trend interaction.
Model 5 includes state fixed effect and linear time trend.
Model 6 includes state and year fixed effects.

Table 11
Model 2 Welfare Reform Variable

	Linear Probability Model Coefficients	Logistic Model Coefficients
Early Prenatal Care	-0.0089*** (0.0007)	-0.0394*** (0.0032)
Late Prenatal Care	0.0038*** (0.0004)	0.0516*** (0.0028)
Low Birth Weight	0.0073*** (0.0005)	0.0729*** (0.0049)
Very Low Birth Weight	0.0036*** (0.0002)	0.1542*** (0.0102)

All models also include dummy variables indicating state of residence, linear time trend, and state-specific time trends. Standard errors are in parentheses.

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01

Table 12
Low Intensity Reform* and High Intensity Reform**

	Low Intensity Reform	High Intensity Reform
Early Prenatal Care	0.0104***	-0.0057***
	(0.0021)	(0.0016)
Late Prenatal Care	-0.0086***	0.0020***
	(0.0011)	(0.0005)
Number of Prenatal Visits	-0.6462***	0.2445***
	(0.0734)	(0.0516)
No Prenatal Care	-0.0045***	0.0040***
	(0.0007)	(0.0009)
Low Birth Weight	0.0023***	0.0039***
	(0.0013)	(0.0011)
Very Low Birth Weight	0.0012***	0.0012***
	(0.0006)	(0.0005)
Log Birth Weight	-0.0031***	-0.0044***
	(0.0011)	(0.0008)

Standard errors are in parentheses.
*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01

- Low Intensity Reform includes only states that implemented time limits without other reforms.
- High Intensity Reform includes not only time limits and but also one of the following reforms: family cap, work exemption and sanction.

Table 13
Dependent variable: Third Trimester Care

	Variable	Blacks	Whites
Model 2	Welfare Reform	0.0039***	0.0037***
		(0.0005)	(0.0007)
Model 3	TANF	0.0056***	0.0045***
		(0.0005)	(0.0007)
	Waiver	-0.0067***	-0.0035***
		(0.0009)	(0.0011)
Model 4	Time Limit 1	0.0040**	-0.0074***
		(0.0018)	(0.0022)
	Time Limit 2	0.0029*	-0.0091***
		(0.0017)	(0.0017)
	Sanction	0.0060***	-0.0002
		(0.0017)	(0.0020)
	Income disregard	-0.0178***	0.0002
		(0.0015)	(0.0018)
	Family Cap	-0.0021	0.0075***
		(0.0013)	(0.0013)
	Work exemption	-0.0081***	-0.0016
		(0.0016)	(0.0021)

All models include education, previous births, Medicaid generosity, unemployment rate, state of residence, linear time trend, and state-specific time trend.
Standard errors are in parentheses.
*** 0 < p-value < 0.01 **0.01< p-value <0.05 * 0.00< p-value < 0.01

Table 14
Dependent variable: First Trimester Care

	Variable	Blacks	Whites
Model 2	Welfare Reform	-0.0086***	-0.0079***
		(0.0009)	(0.0010)
Model 3	TANF	-0.0082***	-0.0082***
		(0.0008)	(0.0011)
	Waiver	0.0037***	0.0010
		(0.0014)	(0.0019)
Model 4	Time Limit 1	-0.0065**	0.0049
		(0.0030)	(0.0038)
	Time Limit 2	-0.0053*	0.0152***
		(0.0030)	(0.0031)
	Sanction	-0.0036	0.0081**
		(0.0028)	(0.0036)
	Income disregard	0.0060**	-0.0051
		(0.0025)	(0.0032)
	Family Cap	0.0009	-0.0041
		(0.0022)	(0.0026)
	Work exemption	0.0057**	-0.0023
		(0.0027)	(0.0038)

All models include education, previous births, Medicaid generosity, unemployment rate, state of residence, linear time trend, and state-specific time trend.
Standard errors are in parentheses.
*** 0 < p-value < 0.01 **0.01< p-value <0.05 * 0.00< p-value < 0.01

Table 15
Dependent variable: No Prenatal Care

	Variable	Blacks	Whites
Model 2	Welfare Reform	0.0053***	0.0045***
		(0.0003)	(0.0004)
Model 3	TANF	0.0018***	0.0024***
		(0.0006)	(0.0004)
	Waiver	0.0062***	0.0036***
		(0.0006)	(0.0006)
Model 4	Time Limit 1	-0.0079***	-0.0038***
		(0.0012)	(0.0011)
	Time Limit 2	-0.0022*	-0.0015
		(0.0011)	(0.0009)
	Sanction	0.0120***	0.0036***
		(0.0012)	(0.0011)
	Income disregard	0.0004	0.0006
		(0.0010)	(0.0010)
	Family Cap	0.0024***	0.0010
		(0.0079)	(0.0008)
	Work exemption	-0.0039***	0.0010
		(0.0012)	(0.0012)

All models include education, previous births, Medicaid generosity, unemployment rate, state of residence, linear time trend, and state-specific time trend.
Standard errors are in parentheses.
*** 0 < p-value < 0.01 **0.01< p-value <0.05 * 0.00< p-value < 0.01

Table 16
Dependent variable: Number of Prenatal Care Visits

	Variable	Blacks	Whites
Model 2	Welfare Reform	0.0735***	0.0569
		(0.0362)	(0.0465)
Model 3	TANF	0.3008***	0.1317***
		(0.3048)	(0.0421)
	Waiver	-0.4530***	-0.2644***
		(0.0601)	(0.0716)
Model 4	Time Limit 1	0.4197***	-0.2099
		(0.0074)	(0.1508)
	Time Limit 2	-0.0046	-0.3304***
		(0.1153)	(0.1078)
	Sanction	0.5437***	0.0610
		(0.1137)	(0.1220)
	Income disregard	-1.3425***	-0.0587
		(0.1137)	(0.1115)
	Family Cap	-0.2822***	0.0606
		(0.0989)	(0.0933)
	Work exemption	-1.2191***	-0.6008***
		(0.1106)	(0.1294)

All models include education, previous births, Medicaid generosity, unemployment rate, state of residence, linear time trend, and state-specific time trend.
Standard errors are in parentheses.
*** 0 < p-value < 0.01 **0.01< p-value <0.05 * 0.00< p-value < 0.01

Table 17
Dependent variable: Low Birth Weight

	Variable	Blacks	Whites
Model 2	Welfare Reform	0.0090***	0.0037***
		(0.0060)	(0.0007)
Model 3	TANF	0.0066***	0.0025***
		(0.0006)	(0.0006)
	Waiver	0.0048***	0.0032***
		(0.0010)	(0.0011)
Model 4	Time Limit 1	-0.0006	0.0037*
		(0.0021)	(0.0021)
	Time Limit 2	0.0076***	0.0049***
		(0.0021)	(0.0016)
	Sanction	0.0141***	0.0061***
		(0.0020)	(0.0021)
	Income disregard	-0.0056***	-0.0008
		(0.0018)	(0.0018)
	Family Cap	0.0021	0.0002
		(0.0015)	(0.0013)
	Work exemption	-0.0024	-0.0027
		(0.0019)	(0.0022)

All models include education, previous births, Medicaid generosity, unemployment rate, state of residence, linear time trend, and state-specific time trend.
Standard errors are in parentheses.
*** 0 < p-value < 0.01 **0.01< p-value <0.05 * 0.00< p-value < 0.01

Table 18
Dependent variable: Very Low Birth Weight

	Variable	Blacks	Whites
Model 2	Welfare Reform	0.0044***	0.0019***
		(0.0003)	(0.0003)
Model 3	TANF	0.0028***	0.0010***
		(0.0003)	(0.0003)
	Waiver	0.0024***	0.0016***
		(0.0004)	(0.0005)
Model 4	Time Limit 1	0.0012	0.0006
		(0.0010)	(0.0009)
	Time Limit 2	0.0042***	0.0012*
		(0.0010)	(0.0007)
	Sanction	0.0055***	0.0028***
		(0.0010)	(0.0009)
	Income disregard	-0.0016*	-0.0008
		(0.0009)	(0.0008)
	Family Cap	0.0004	-0.0001
		(0.0008)	(0.0006)
	Work exemption	-0.0006	0.0003
		(0.0009)	(0.00090

All models include education, previous births, Medicaid generosity, unemployment rate, state of residence, linear time trend, and state-specific time trend.
Standard errors are in parentheses.
*** 0 < p-value < 0.01 **0.01< p-value <0.05 * 0.00< p-value < 0.01

Table 19
Dependent variable: Log Birth Weight

	Variable	Blacks	Whites
Model 2	Welfare Reform	-0.0085***	-0.0040***
		(0.0005)	(0.0006)
Model 3	TANF	-0.0054***	-0.0024***
		(0.0005)	(0.0001)
	Waiver	-0.0057***	-0.0032***
		(0.0008)	(0.0009)
Model 4	Time Limit 1	-0.0030*	-0.0025
		(0.0017)	(0.0018)
	Time Limit 2	-0.0105***	-0.0051***
		(0.0018)	(0.0014)
	Sanction	-0.0120***	-0.0074***
		(0.0016)	(0.0017)
	Income disregard	0.0057***	0.0025***
		(0.0015)	(0.0017)
	Family Cap	-0.0018	0.0001
		(0.0013)	(0.0012)
	Work exemption	0.0014	0.0018
		(0.0016)	(0.0018)

All models include education, previous births, Medicaid generosity, unemployment rate, state of residence, linear time trend, and state-specific time trend.
Standard errors are in parentheses.
*** 0 < p-value < 0.01 **0.01< p-value <0.05 * 0.00< p-value < 0.01

Table 20
Dependent variable: Marital Status (1=Married and 0= unmarried)

	Variable	Coefficients*
Model 2	Welfare Reform	-0.0002 (0.0005)
Model 3	TANF	-0.0047*** (0.0005)
	Waiver	0.0048*** (0.0008)
Model 4	Time Limit 1	0.0046*** (0.0015)
	Time Limit 2	-0.0026* (0.0015)
	Sanction	0.0006 (0.0014)
	Income disregard	-0.0016 (0.0013)
	Family Cap	-0.0105*** (0.0011)
	Work exemption	0.0058*** (0.0014)
Model 5	Low Intensity	0.0121*** (0.0015)
	High Intensity	-0.0147*** (0.0011)
Model 6	TANF	-0.0033*** (0.0004)
Mean	56.83	56.83
# of observations	6884220	6884220

All models include race, education, previous births, Medicaid generosity, unemployment rate, state of residence, linear time trend, and state-specific time trend.
Standard errors are in parentheses.
*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 *0.00< p-value < 0.01

Table 21
Difference in Difference Estimation for the "Treatment Group and Welfare Reform Interaction Variable"

	Welfare Reform*	"Low" Intensity Reform**	"High" Intensity Reform***
Early Prenatal Care	-0.0051***	0.0289***	-0.0174***
	(0.0009)	(0.0019)	(0.0011)
Late Prenatal Care	-0.0019***	-0.0065***	0.0101***
	(0.0005)	(0.0011)	(0.0005)
Number of Prenatal Visits	-0.4027***	0.3229***	-0.8351***
	(0.0368)	(0.0790)	(0.0311)
No Prenatal Care	0.0059***	-0.0093***	-0.0069***
	(0.0003)	(0.0005)	(0.0003)
Low Birth Weight	0.0047***	-0.0055***	-0.0024***
	(0.0006)	(0.0012)	(0.0006)
Very Low Birth Weight	0.0015***	-0.0010***	0.0005*
	(0.0003)	(0.0001)	(0.0003)
Log Birth Weight	-0.0047***	0.0056***	-0.0009*
	(0.0005)	(0.0010)	(0.0005)

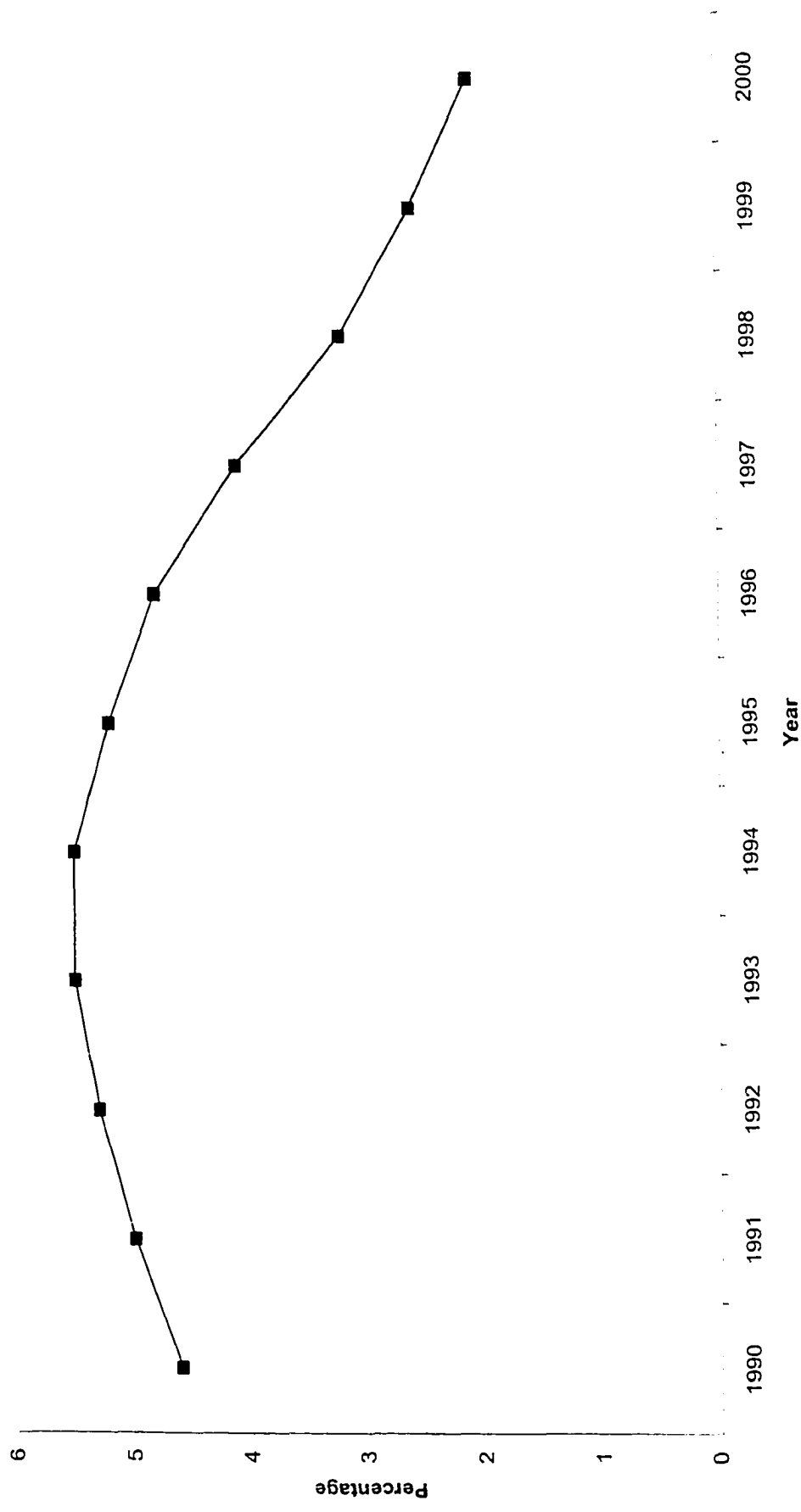
Standard errors are in parentheses.
*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01

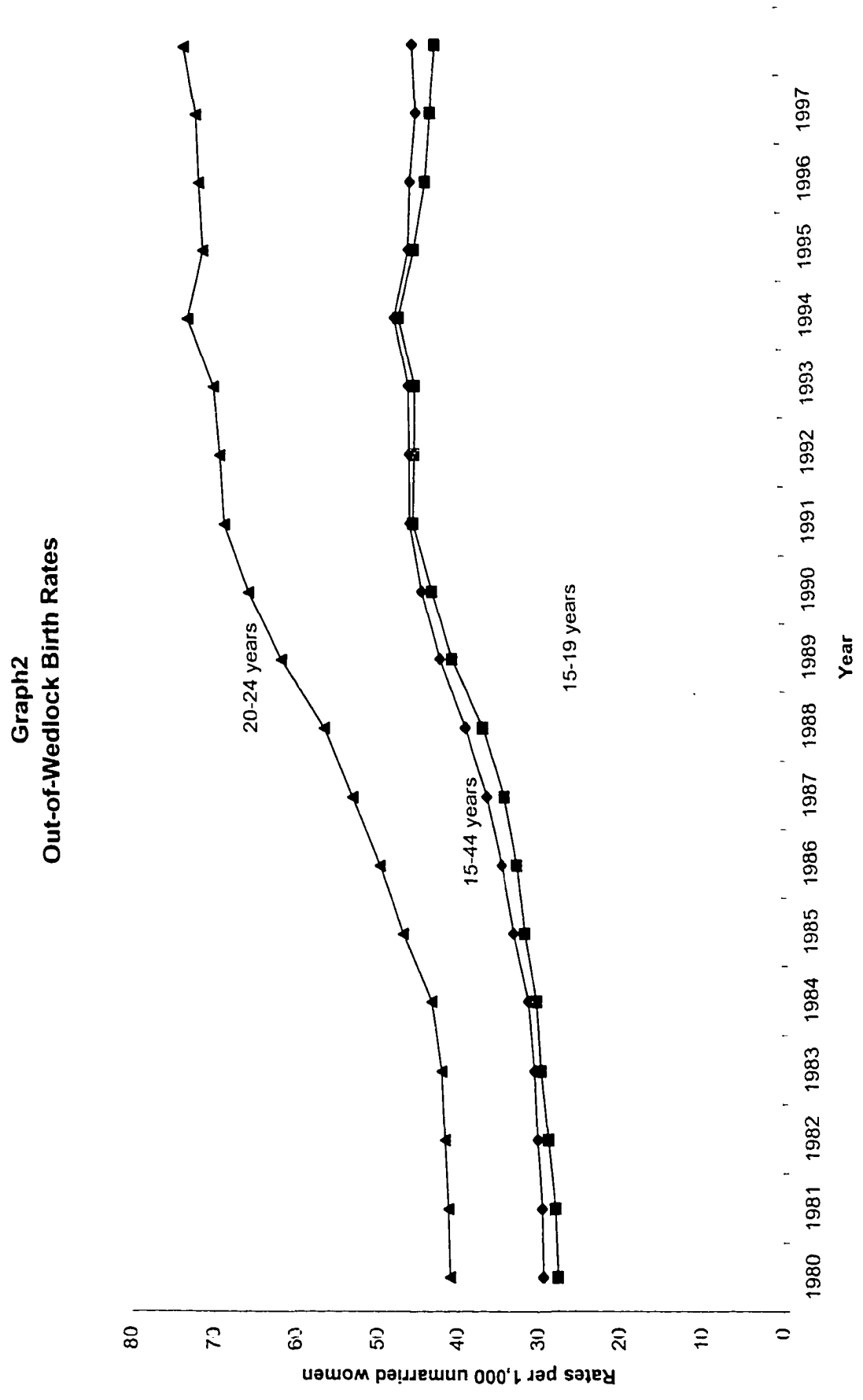
* The model includes race, education, previous births, Medicaid generosity, unemployment rate, treatment group, welfare reform, interaction of welfare reform with treatment group, state of residence, linear time trend, and state-specific time trend.

** The model includes race, education, previous births, Medicaid generosity, unemployment rate, treatment group, "low" intensity reform, interaction of "low" intensity reform with treatment group, state of residence, linear time trend, and state-specific time trend.

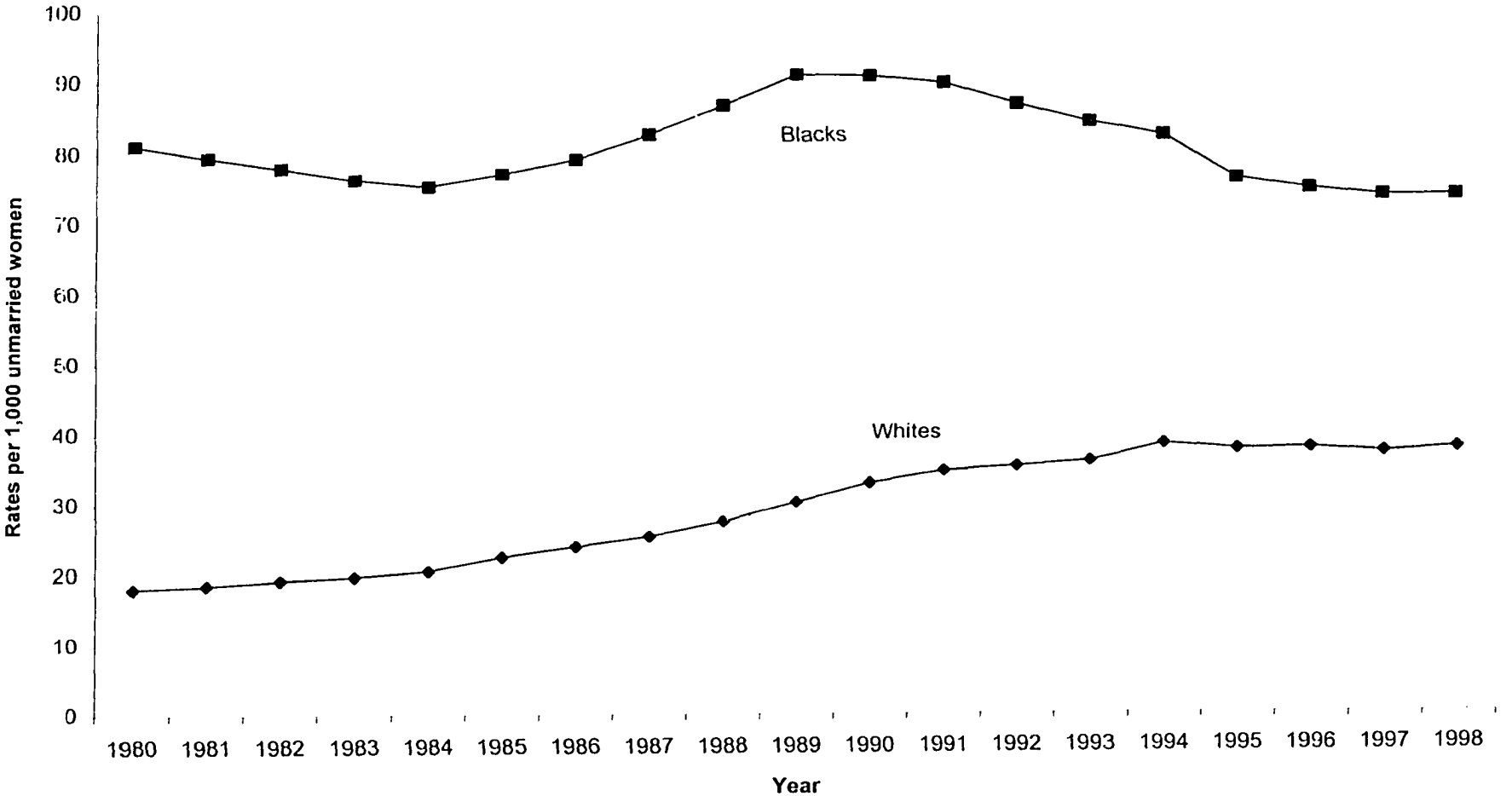
*** The model includes race, education, previous births, Medicaid generosity, unemployment rate, treatment group, "high" intensity reform, interaction of "high" intensity reform with treatment group, state of residence, linear time trend, and state-specific time trend.

Graph 1
AFDC/TANF Percentage of Total U.S Population (1991-2000)

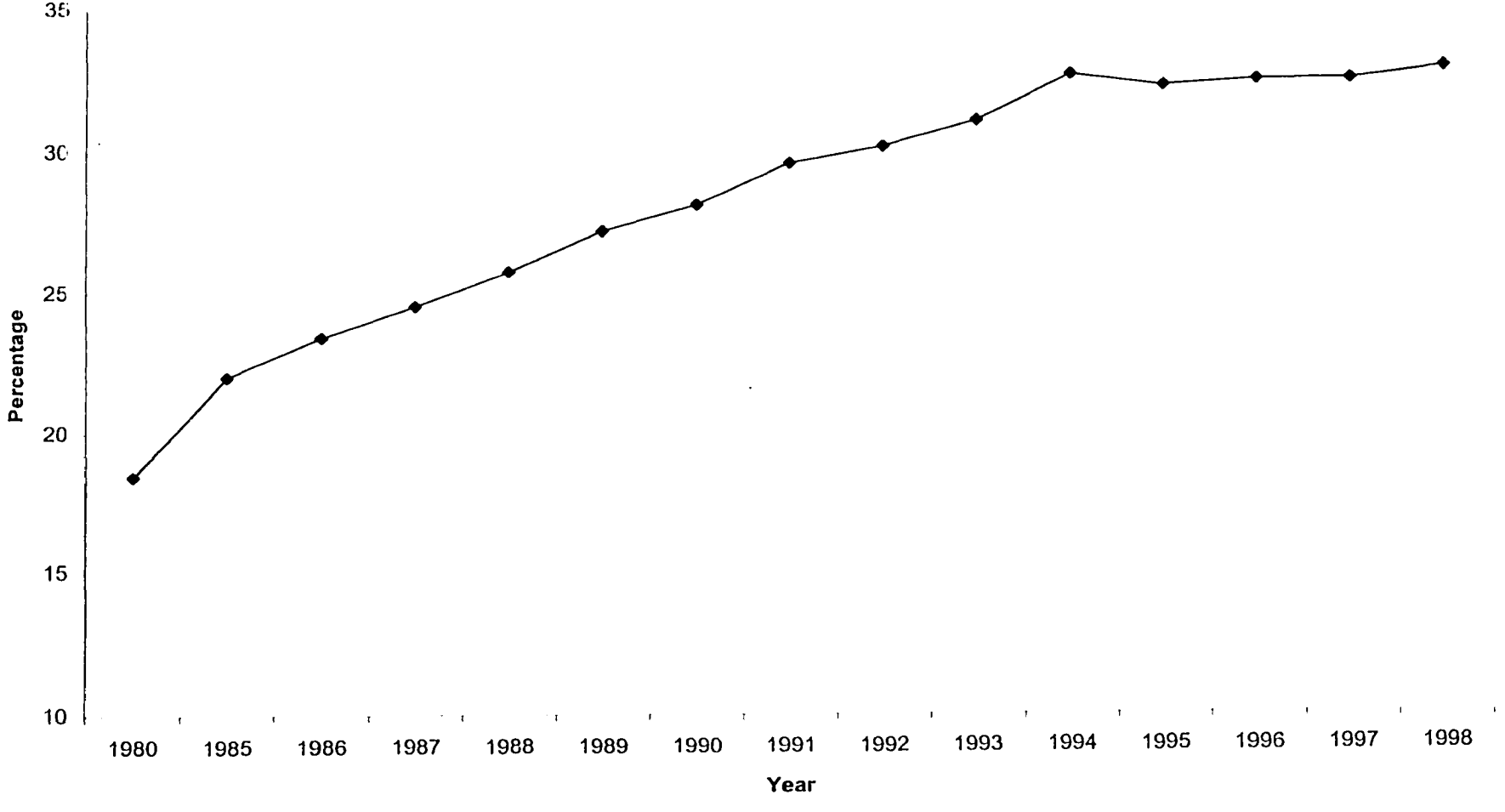




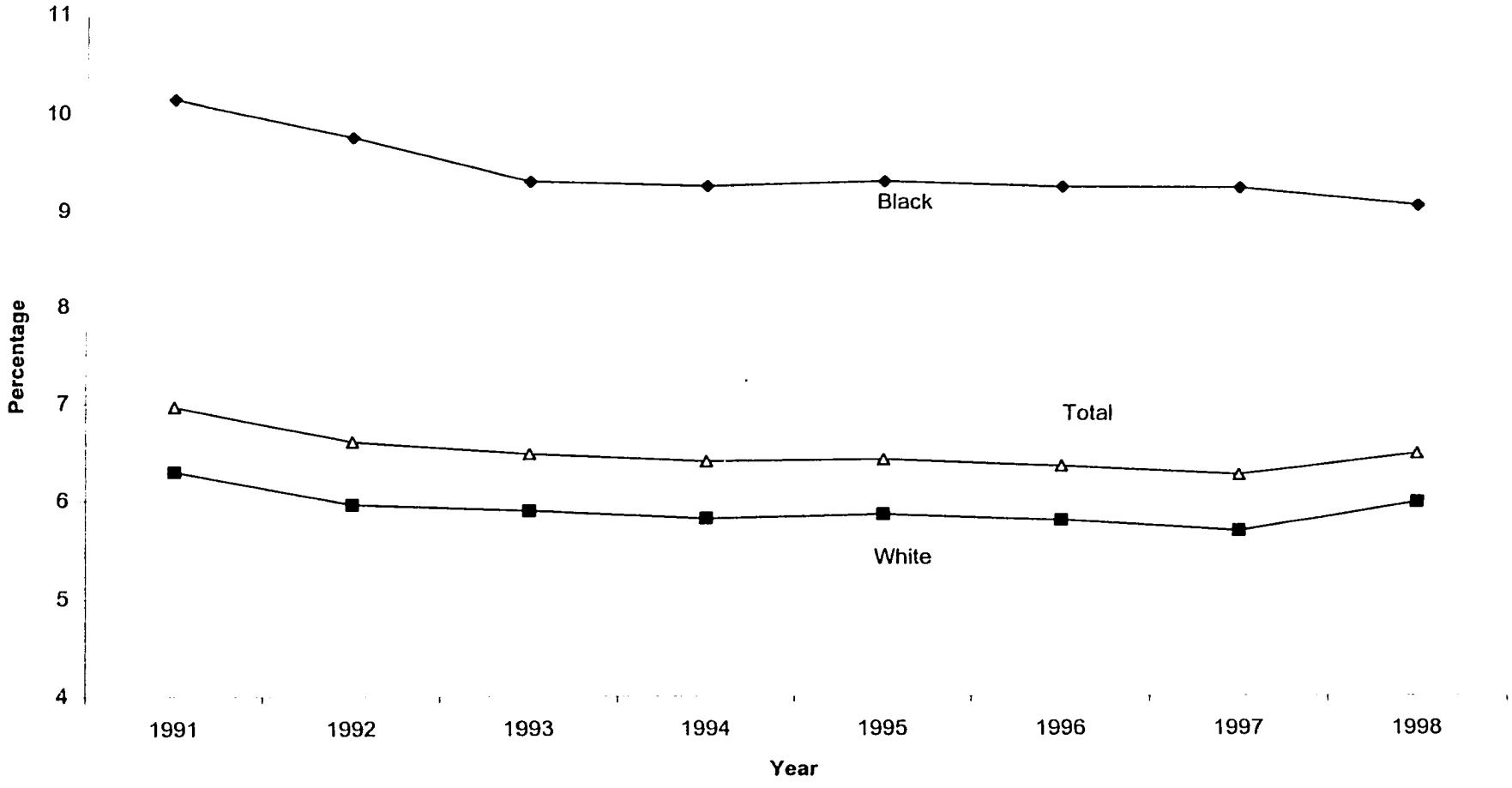
Graph 3
Out-of-Wedlock Birthrates by Race



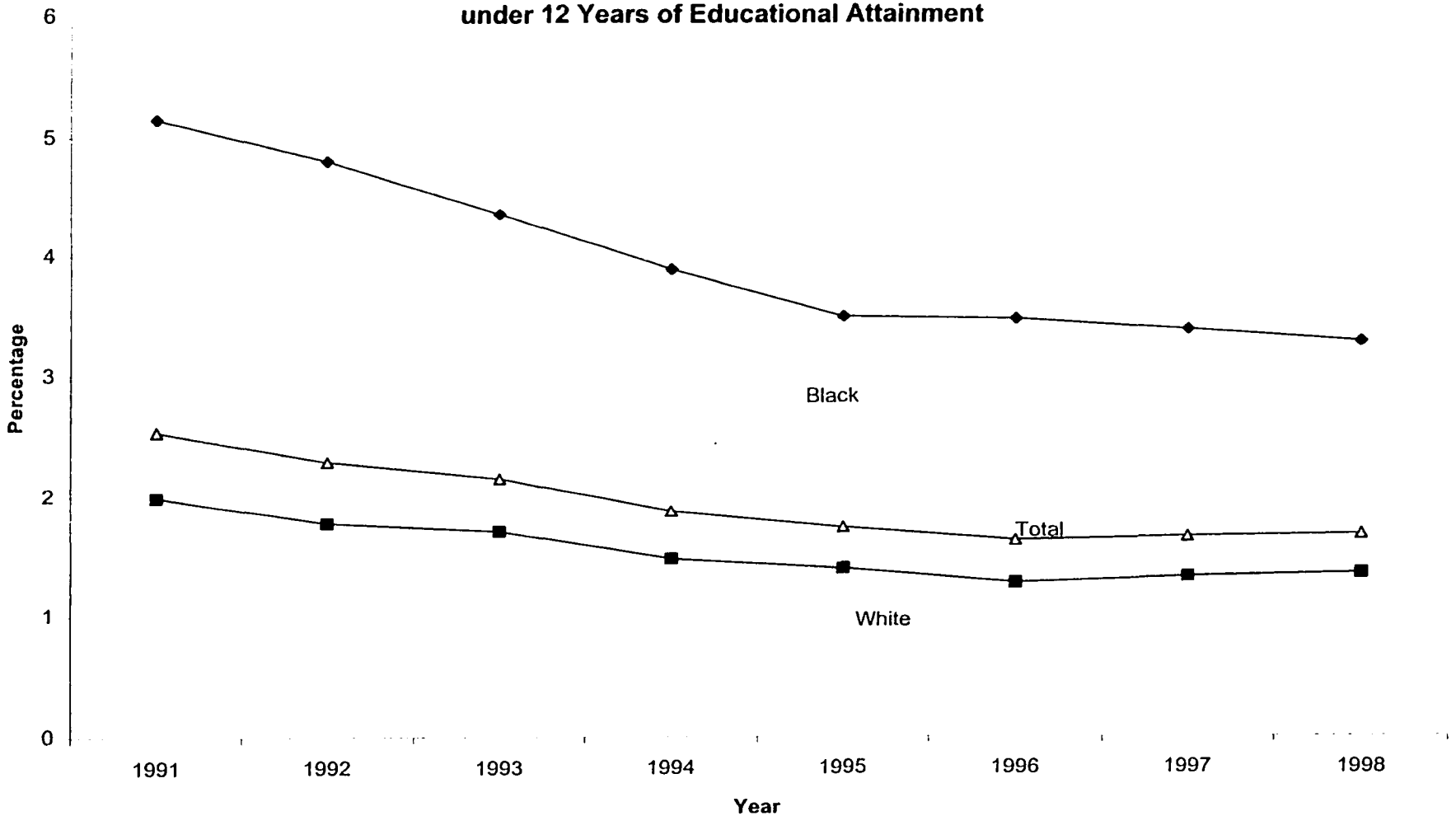
Graph 4
Proportion of Births to Unmarried Women



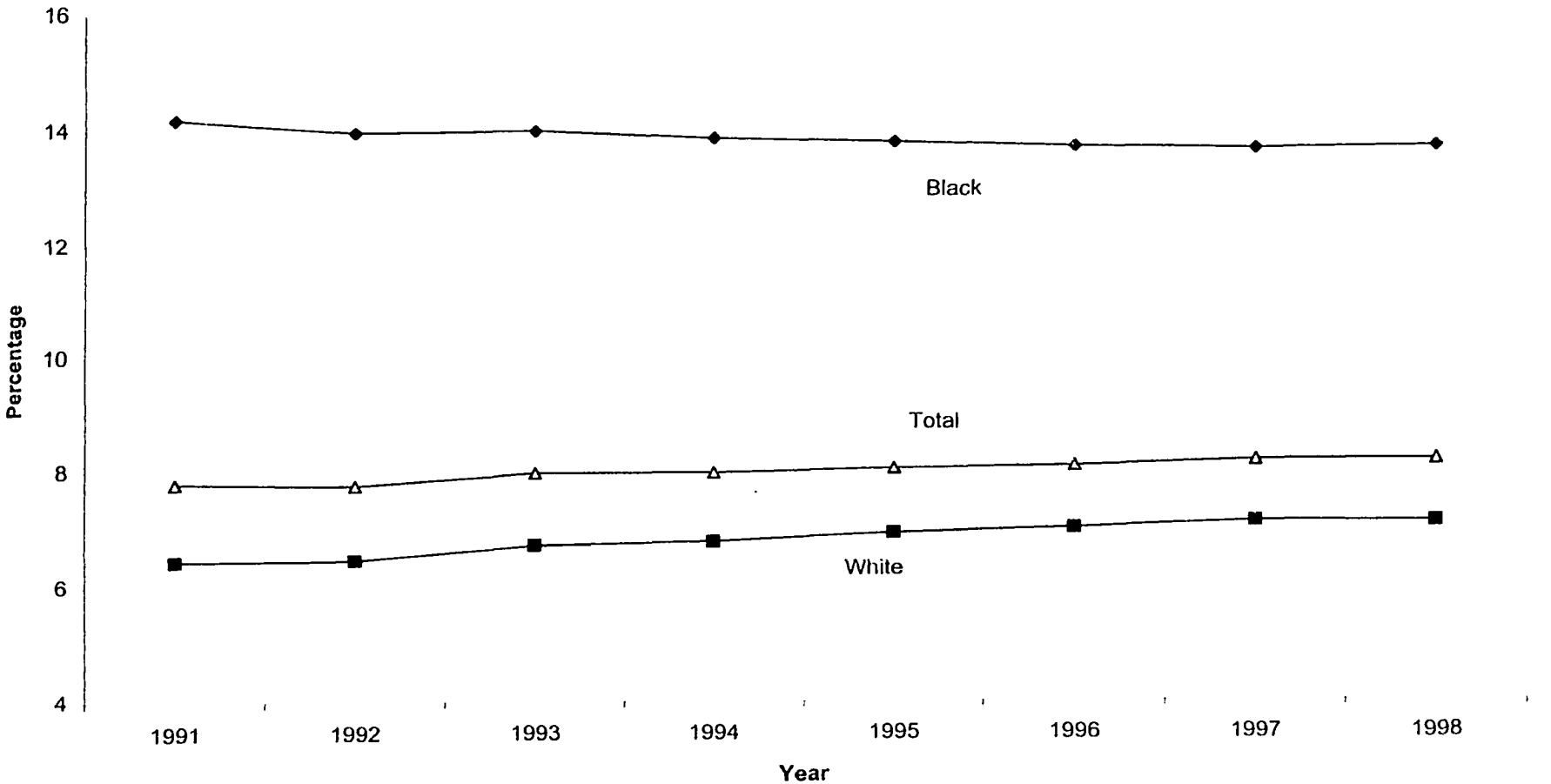
Graph 5
Late Prenatal Care Initiation
under 12 Years of Educational Attainment



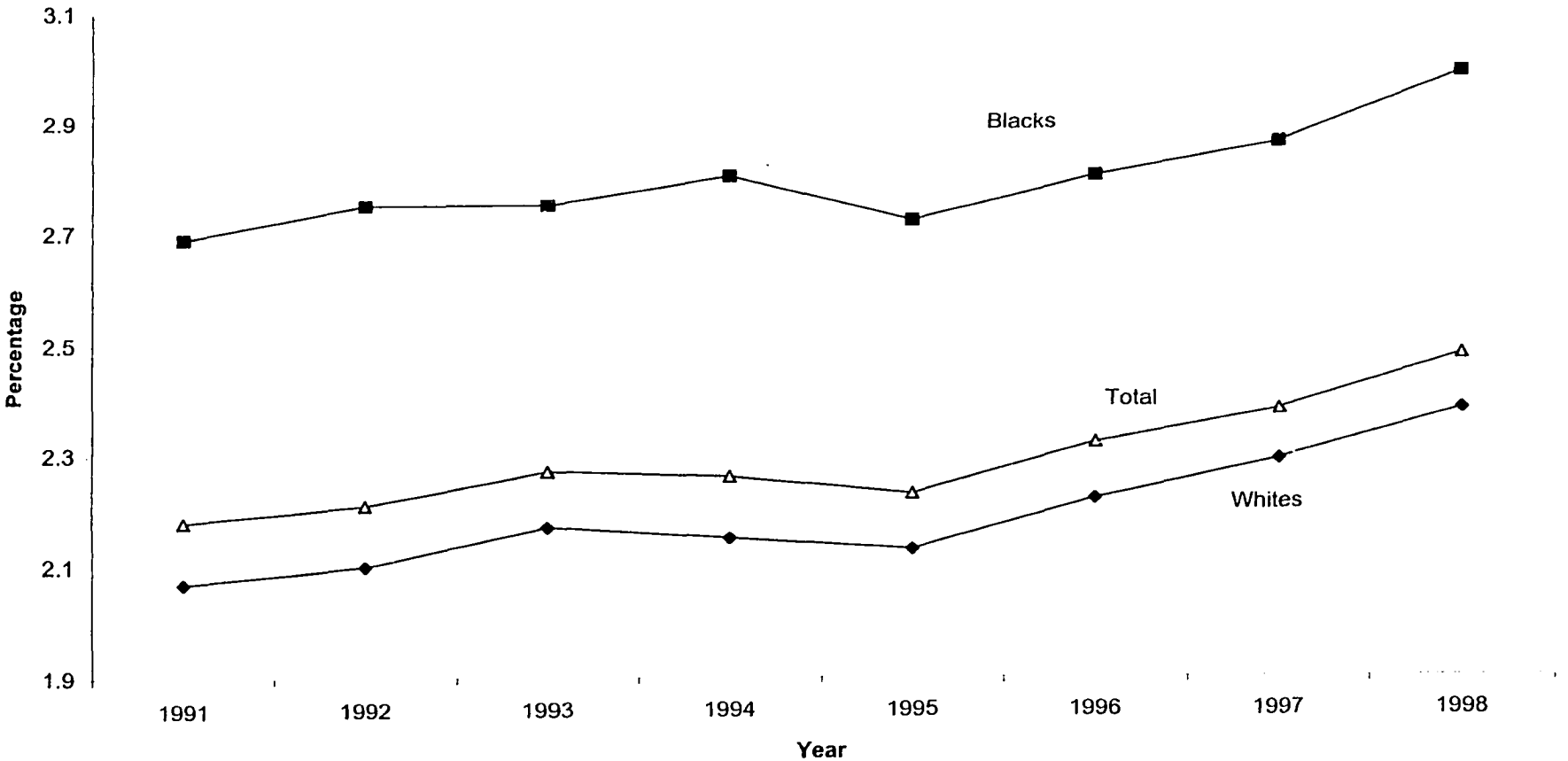
Graph 6
No Prenatal Care
under 12 Years of Educational Attainment



Graph 7
Low Birth Weight
under 12 Years of Educational Attainment



Graph 8
Multiple Births
under 12 Years of Educational Attainment



Introduction

The nation's high abortion rate, 23 per 1,000 women of reproductive age,[1] reflects the high level of unintended pregnancies, and a large proportion of births to term has been considered unintended. (Table 1).[2] From a policy perspective, it is essential to enable women to avoid unplanned pregnancies and unintended births and thus to help them avoid or escape poverty. While there has been a great deal of work on abortion rates and their determinants, little has been known about the extent to which the availability of abortion affects the probability of unintended births. My study attempts to identify the causal mechanisms linking unintended births, the abortion rate and the availability of abortion providers, factors that have received little attention.

Conventional wisdom holds that an increase in access to abortion providers should lead to a decline in births from unintended pregnancies. Easy access to abortion providers would lower the cost of fertility control, reducing the total number of births including births that are unintended. This scenario is most likely when pregnancy rates are unaffected by shifts in abortion access. The opposite relationship is possible if a decrease in the cost of abortion triggers behavioral changes in contraceptive effort and frequency of sexual intercourse, thereby altering pregnancy rates. Then easy access to abortion may increase both unintended pregnancies and births from unintended pregnancies unless a more than commensurate rise in the abortion rate offsets increased unintended pregnancies.

Similarly, restricting access to abortion would have the potential to increase or decrease unintended births. The restrictions imply higher abortion cost, which will

[1] Alan Guttmacher Institute (1997)

decrease indirect utility associated with pregnancy. An increase in abortion cost may encourage women to exercise more caution about pregnancy with wider contraceptive use, and this possibility may lead to a considerable decline in unintended pregnancies. In this case, the majority of babies conceived over time should be intended, and if so, the number of abortions should also decline. Then, restricting access to abortion would be positively associated with a decline in births from unintended pregnancies.

However, an alternative consequence is also plausible. The highly prohibitive cost of abortion does not alter pregnancy resolution, simply discouraging women from aborting unintended pregnancies. The abortion rate may decline, while unintended births increase. Consequently, the net effect of abortion availability on the probability of unintended births is ambiguous and subject to empirical study due to the complex relationship among pregnancies, abortions, and births.

Purpose of the Study

The reasons for this research are threefold. First, hardly any work has been conducted on the subject. At most two papers, neither directly related to unintended births, deal with abortion access and out-of-wedlock births (Kane and Staiger, 1996; Alkelof, Yellen and Kats, 1996). Kane and Staiger's study does not provide conclusive evidence linking unintended births and abortion access. They used out-of-wedlock as a crude proxy of unintended births.[3] Indeed, our summary statistics suggest that a significant proportion of in-wedlock births is unintended, and many out-of-wedlock

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[2] The PRAMS (Pregnancy Risk Assessment Monitoring System) that my study uses reports that on average 45 % of all births were unintended from 1993 to 1997.

[3] Kane and Staiger state, "the in-out of wedlock distinction is used as a crude proxy for the desirability of a birth," (473).

births are intended (Table 1). They used state-level aggregated data and thus could not control for individual characteristics, including marital status. However, their theory provides the insight that a shift in abortion cost may affect birth rates, and therefore presumably unintended births, of various segments of the population differently. In other words, the probability of unintended birth will be responsive to a change in abortion access to varying degrees depending on the socioeconomic characteristics of pregnant women.

Kane and Staiger discovered that restricting access to abortion is consistently associated with a small but significant decline in the teen birthrate, with most of the decline occurring among in-wedlock births and with out-of-wedlock births relatively unaffected. A 25 mile increase in distance to nearest provider is associated with a reduction in teen births of about 1 percent relative to the mean birthrate. An increase of 25 miles to nearest provider is associated with a 2 percent decline in in-wedlock births and a 1 percent increase in out-of-wedlock teen births. This result implies that the fraction of births that are out-of-wedlock increases with a restriction to distance. The distance effects seem too large in the sense that a 100 mile increase in distance to a nearest provider is likely to reduce the teen birth rate by 4%. Our empirical study will provide some ideas as to whether these results will be borne out.

Kane and Staiger explore a simple model of rational decision-making under uncertainty in which pregnancy is an endogenous decision. In their model, an increase in access to abortion services may or may not increase birth rates through a change in pregnancy rates. Kane and Staiger's model describes two types of women: type A exhibiting a high probability of getting married and type B with a low probability. For

instance, in response to increased abortion cost, type B women do not alter their pregnancy resolution, but they stop having abortions if the cost of aborting is exorbitant, thereby increasing the probability of birth. Type A women will stop getting pregnant, reducing the probability of both abortion and birth. Therefore, an increase in the cost of abortion unambiguously reduces pregnancy, abortion and women's utility, but it has ambiguous effects on births.

The model provides no linearity between birth rates and an increase in the cost of abortion. A small increase in abortion cost reduces pregnancy and birth rate for type A. A more dramatic increase in abortion cost may still decrease birth rate for type A but increase birth rate for type B.

The second paper (Akeloff, Yellen and Katz, 1996) explains why there might be a link between contraception, the legalization of abortion, and the declining rate of shotgun marriage. Abortion technology enhances the welfare of both the women adopting the technology and the men not subject to shotgun marriage. After the introduction of abortion, men do not have to provide a marriage promise on the occasion of premarital sex, nor do they end up marrying the women who opt out of abortion. The paper illustrates two types of women. One type adopts the abortion technology and participates in premarital sex regardless of the marriage promise of her partner. The other type of woman is put in competitive circumstances, engaging in premarital sexual activities for the fear of losing partners. The advent of abortion may result in an unwanted increase in sexual participation for those who reject the new technology. These women may have moral and religious beliefs that increase their reluctance to seek abortions.

Thus, the authors argue that the technological shock of abortion may have played a role in the rise of out-of-wedlock childbearing. Unlike the previous model, this model implies that an increase in access to abortion results in more unwanted, out-of-wedlock births and fewer in-wedlock births. The previous model would have predicted that an increase in abortion access decreases out-of-wedlock births. However, the increase in out-of wedlock births and the decrease in in-wedlock births may or may not lower the overall birth rate although the proportion of in-wedlock births is larger than that of out-of-wedlock births in the total births.[4]

The second reason for my research is that, while not directly related to my study, an increase in unwanted births has a potential impact on schooling (Angrist and Evans, 1999), cognitive aspects of child development, and child poverty (Gruber, Levine and Staiger, 1999). Unwanted infants are at greater risk of weighing less than 2,500 grams at birth and of dying in their first year of life. Their mothers are more likely to seek prenatal care after the first trimester or to obtain no care at all (Joyce, Kaestner and Koreman, 2000; Joyce and Grossman, 1990).

This point provides a rationale behind the finding that a decline in infant mortality is associated with an increase in abortion (Grossman and Jocobowitz, 1981). The argument is that births to term should be intended or more favorably selected infants with a rising abortion rate over time. The results suggest a positive selection among women

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[4] Let b_m be the birth rate of married women (births to married women divided by number of married women) and let b_u be the birth rate of unmarried women. Then the overall birth rate is

$$b = mb_m + (1 - m)b_u,$$

where m is the fraction of married women. Let x be any variable. Then

$$\partial b/\partial x = m(\partial b_m/\partial x - \partial b_u/\partial x) + (b_m - b_u)\partial m/\partial x.$$

carrying pregnancies to term in that women use abortion to avoid bearing children under adverse circumstances. Their opposite finding would have led one to think of a negative selection if disadvantaged women, who are constrained in their abortion access, deliver babies to term increasingly over time. A recent study (Gruber, Levine and Staiger, 1999) also confirms a positive selection. This research shows that abortion legalization appears to be associated with an improvement in average living circumstances and birth outcomes. Thus, children not born due to abortion availability would have grown up in adverse living circumstances.

Finally, the results of my study attempt to shed light on the impact of the increase in abortion on the decline in crime rate (Donahue and Levitt, 2000). Donahue and Levitt's argument is heavily based upon the hypothesis that abortion legalization *after Roe v. Wade* resulted in an increase in "intended" or "wanted" births. Unwanted children are more likely to grow up devoid of their parents' attention and are therefore likely to be delinquent. The reduction in unwanted births following the increase in abortion will lower criminal activity. Donahue and Levitt's finding is that, controlling for economic conditions, policy strategies, and the availability of guns and drugs, the abortion decline alone might account for a 50% drop in crime. However, the magnitude appears implausibly large partly because of the confounding factors of the crack epidemic and the spread of guns. After all, the causal mechanism linking the abortion rate to the crime rate appears to be tenuous.

Literature Review

A vast body of literature attempts to understand the determinants of abortion. Some studies focus on individual socioeconomic characteristics, whereas others examine public policies including the parental consent law and the restriction of Medicaid funding of abortion. There is much evidence that restricting abortion access reduces abortion rates (Trussell et al. 1980; Blank, George and London, 1996; Henshaw and Skatrud, 1997; Matthews, Ribar and Wilhelm, 1997). However, many studies provide disparate results on the relationship between abortion access and birth rates, thus making causal interpretation difficult (Matthews, Ribar and Wilhelm, 1997;Kane and Staiger, 1996). No prior study has been conducted using micro level data in an effort to investigate individual reproductive decision making in response to abortion access.

Joyce (1988), using the New York City Department of Health data source on abortion, found that women who were young and unmarried were more likely to abort. Those who had either previously had an abortion or experienced a greater number of pregnancies tended to have a greater number of abortions than those who had not. Whites and Medicaid recipients also showed a high abortion rate as opposed to their counterparts. Leibowitz, Eisen and Chow (1986) found evidence that "girls who reported better high school grades were more likely to choose abortion."

While many studies hold that more educated women are more likely to abort, it seems equally logical that the more highly educated a woman is, the more likely it is that her pregnancy was intended. Likewise, a body of literature (Michael, 1973; Michael and Willis, 1976; Rosenzweig and Seiver, 1982) shows that the likelihood of more educated

women having an unwanted pregnancy is much lower. A principal reason is that women whose time is valued more highly or who are more highly educated are more likely to use contraception and to do so more effectively. Accordingly, the relation between the woman's time value and her choice of whether to have an abortion appears complex. Apparently, there is a significant level of interaction among education, contraception, and abortion. Taking women's age into account further complicates the interaction.

The positive relation between education and abortion may prevail to a greater extent in women's earlier years. During this period, pregnancies are more likely to be inadvertent and due to their opportunity cost, abortion rises. But the positive association between education and abortion may disappear later in women's lives as they become more careful about contraception. The contraception effect may dominate abortion resolution in the older, yet better educated cohort.

There is a growing body of evidence of price sensitivity on the part of lower-income women. Trussell et al. (1980) documented that the ending of Medicaid funding for abortion in Ohio and Georgia was associated with a substantial decrease in the number of abortions performed in those states. Similarly, Blank, George and London (1996) put forward that 19-25% of publicly-funded abortions among low-income women cease to occur once funding is eliminated. Joyce, Henshaw, and Skatrud (1997) compared abortion behavior in Mississippi before and after the mandatory waiting period which took effect in 1992, controlling for the time trend and holding constant the pattern of behavior in two other states. They found a clear decline in the abortion rate during the year following the implementation of the new law. Joyce and Kaestner (1996) found that the extended medical coverage (Medicaid eligibility above 50 but below 185 percent of

their poverty threshold) was associated with a 2-5 percentage point reduction in abortion for whites but no corresponding reduction for blacks.

Blank et al. (1996) conclude that the primary effect of provider availability is to lead women to change the location of their abortions. Thus, as number of providers shrinks, more women go out of state for their abortions. This conclusion is based on the finding that there is no effect of number of abortion providers on abortion rate by state of residence but some effect on abortion rate by state of occurrence. The finding is that a 10% increase in the number of abortion providers at the mean leads to a 5% increase in the abortion rate by the state of occurrence. Acknowledging the possible endogeneity of the measures on abortion providers, they instrumented them with the total number of non Ob/Gyn physicians and the total number of hospitals in each state and year. The results of OLS and TSLS have similar magnitude. When Blank et al. (1996) refine abortion providers into hospital and non-hospital providers, only the number of non-hospital abortion providers remains statistically significant to explain abortion rate. These non-hospital providers are less susceptible to the climate change in abortion rate, and thus serve as a more reliable source in the regression facing the endogeneity problem. Similarly, Matthews, Ribar and Wilhelm (1997) demonstrate that the number of large abortion providers better reflects the effects of exogenous variation than small providers because the number of large ones is not sensitive to short-term variation of abortion rates, and that abortion rates are predominately driven by large providers.

The determinants of annual abortion rates and birthrates were the subject of their principal investigation. They used state level data from the years 1978 –1988 and found a significant and positive coefficient for abortion provider access on the abortion rate. In

the same vein, they also discovered significant negative coefficients for both Medicaid funding restriction and prenatal consent or notification laws on abortion rate. The coefficient of a variable representing the proportion of women living in counties with abortion providers turns out to be positive and significant for abortion rates but not for birth rates. Finally, they found that, as distance to abortion provider increases, the abortion rate declines significantly but the distance measure, either average miles to nearest in-state provider or to nearest out-of-state provider, had no effect on birth rates.

These findings support the view that abortion availability shifts the abortion rate but does not significantly affect the birth rate. The authors found that a change in contraceptive effort may be at work so that abortion access increases abortion rates but does not significantly lower birth rates.

Levin et al. (1996) found that abortion legalization appears to be correlated with roughly a six percent decline in relative birth rates. They also found that births to teens, women over 35, nonwhite women, and unmarried women fell the most in response to abortion legalization. This empirical finding supports Kane-Staiger's theory, but not that of Alkeloff, Yellen and Katz. Gruber, Levine and Staiger (1999) use this regression as a first-stage regression to instrument for birth rate from 1965 to 1979, and the decline in births serves as a regressor to predict a wide range of living circumstances of children not born due to abortion legalization. Those outcomes include the percentage living with a single parent in 1980, the percentage living in poverty in 1980, the percentage with welfare receipt in 1980, and the percentage with low birth weight. The reduced form regression and the structural regression both by OLS and TSLS show that the marginal

children who were not born due to abortion legalization would have lived in more disadvantaged circumstances than the average child in their cohort.

This result suggests that our empirical study would find a negative association between unwanted birth and abortion access among underprivileged groups. It appears, according to Gruber, Levin and Staiger, that positive selection prevailed after abortion legalization. However, it is noteworthy that the recent period marked by abortion restrictions, new legislation, and a decline in abortion providers may not guarantee an opposite outcome.

Many studies conducted to investigate the association between abortion access and birth rates in aggregated data offer little insight into the selection process. A simple reason is that the aggregated data do not readily sort out pregnant women into different socioeconomic categories. Gruber, Levine and Staiger (1999) and Kane and Staiger (1996) address the selection process issue both theoretically and empirically, but their results can serve only as aggregated approximations. Our research has two advantages over previous research in addressing the selection issue. First, our study does not deal with aggregate birth rates but the proportion of unintended births among total births, thus addressing directly the probability of unintended birth associated with abortion access. Second, our empirical analysis complements the previous research using micro level data, enabling us to control for individual characteristics and identify the segment of the population that may be affected by abortion access.

Data

In 1988, the Center for Disease Control and Prevention (CDC) began the Pregnancy Risk Assessment Monitoring System (PRAMS), an in-depth survey of behaviors, practices, and experiences during women's pre-pregnancy, prenatal, and postpartum periods. Four states (Maine, Michigan, Oklahoma, and West Virginia) participated in this program at the beginning, but as of 1997, data from 13 states are available. New mothers are selected monthly from birth certificates by stratified systematic sampling with a random start. Stratification variables, such as birth weight and race/ethnicity, vary among states. However, all the states oversample women at increased risk for adverse pregnancy outcomes. Thus, proper weighting methods should be implemented. The ten states and five years, 1993-1997 that contributed to my study are illustrated in the table (Table 2).

Of particular interest for our study is that the CDC collects data on unintended, unwanted and mistimed births. The depth and detail of the PRAMS data offer us a unique opportunity to explore the effect of the availability of abortion providers on unintended births and come up with an evaluation of its ability to reduce them.

Pregnancy intention is a dichotomous measure. The survey asks women who have recently given birth several questions. If a woman wanted to be *pregnant at that time or sooner in the period just before conception, not at the time of birth*, her pregnancy is defined "intended." If she wanted to become pregnant later, then the term "mistimed" is used. Finally, pregnancy intention is categorized " unwanted" if the women did not want

to be pregnant either then or in the future. Unintended pregnancies are thus either mistimed or unwanted.

Notice, however, that we have only a sample of women that carry their pregnancy to term. Indeed, these data deal with a number of unintended births that, while unintentionally conceived, could not be terminated. Put differently, the data allow us to account for the unintended births derived from unintended pregnancies. If one wanted to know the number of unintended pregnancies in a given year, one would have to factor in the number of unintended births and the number of abortions.

It is noteworthy that the level of "wantedness" can vary both before and during pregnancy. The evaluation of unwanted delivery can be influenced by both marital status and income change during pregnancy. Even though women may have unintentionally conceived babies, they would tend to claim otherwise if their situation altered at delivery. Conversely, while women may have had an intended conception, they may not admit this fact if their situation at delivery makes their babies unwanted. In either case, measurement errors inevitably take place.

The total number of births in PRAMS data is underreported compared with the CDC total number of births, but the time trends of both PRAMS and NCHS data sets follow the same pattern (Table 2 and Table 3). These similar trends indicate that the PRAMS data are a good national representation (Graph 2).

Abortion Rates, Abortion Access Measures and Unintended Births

My study uses abortion rates by state of occurrence as measured by the CDC. The data by state of residence during my study period are not available. The other data source of abortion rates available at the Alan Guttmacher Institute reports 10 to 15 % higher

figures than those of the CDC. The AGI is a private organization that directly contacts abortion providers to obtain information on the total number of abortions performed.

The Alan Guttmacher Institute reports that the number of abortions declined by 17.4 percent in just seven years, to a low of 1,328 million in 1997 from a peak of 1,608 million abortions in 1990. A CDC report also confirms the same trend. The abortion rate,[5] which was 18.7 percent in 1988, fell 15.84 % by 1997 (Table 4). Table 4 shows that the CDC abortion rates describe a trend similar to that depicted by the PRAMS abortion rate of the ten states. Thus, we do have data representing the national trend. Some may argue that econometric results, which we will discuss later, may be idiosyncratic to the ten states participating in the PRAMS data. However, the 10 states that our data provide are not such states as Utah where extreme antiabortion sentiment prevails; the ten states differ greatly over abortion pattern, abortion accessibility and attitude toward abortion. If anything, this study based on ten states merits discussion as an effort to infer a national implication from empirical results.

Three possible reasons for the declining abortion rate may be used as a first approximation prior to more rigorous empirical study. First, Medicaid funding restrictions for abortion and laws requiring informed consent and parental notification discouraged women from seeking abortions. Second, couples resorted to better and wider contraceptive methods, and thus unintended pregnancies have declined, Third, access to abortion services became more difficult, leading women to carry unplanned pregnancies to term.

[5] Number of abortions per 1,000 aged 15-44 years. Abortion ratio defined by number of abortions per 1,000 live births also declined.

Indeed, the number of abortion providers declined by 21%, from 2585 in 1988 to 2042 in 1997 (Table 5). The number of large abortion providers was 788 in 1988, but only 738 in 1997 (Table 5). The percentage of women living in a county with at least one provider has not changed. Since 1988, this figure has remained steady at 62%. This fact implies that 38 % of women cannot find an abortion provider, small or large, in their county. In 1988, 63 % of American counties did not have even one provider, and the percentage has risen to 66% in 1997 (Table 5).

A woman seeking an abortion with a large non-hospital provider had to travel more than 85 miles during the period from 1988 to 1996 (Table 6). A woman who wanted to abort at a nearest non-hospital provider traveled more than 83 miles during the same period. The distance was 75 miles in 1988, but it rose to 90 miles in 1996. Abortion services are mainly concentrated in a small number of large providers. The implication of the concentration is that many patients have to travel long distances. Several indicators of the extent to which women must travel for abortion services have been used in past studies (Kane and Staiger 1996, Matthew et al. 1997). All the abortion accessibility measures consistently show that abortion providers became less available over time.

Meanwhile, the PRAMS data show that the proportion of unintended births among total births has increased slightly over time (Graph 1),[6] an alarming fact that can be analyzed with respect to the declining abortion rate. Unintended births constituted 45.3% of total births in 1988, whereas in 1996 the percentage was 49.6%. Similarly, 10.55% were unwanted relative to 12% in 1996. Mistimed births were 30.2% in 1988, but they increased by 3 percentage points in 1996. This trend suggests two alternate

[6] The time trend is obtained by a regression analysis controlling for year and state due to the nature of the PRAMS data.

hypotheses. First, the recent decline in the proportion of unintended pregnancies that end in abortions rather than births suggests that abortion costs are too high and that the barriers to abortion providers may now be insurmountable to more women. In other words, the increasing barriers to abortion may reduce the abortion rate, thus affecting unintended births. Second, a decline in unintended pregnancies may have contributed to a decline in the abortion rate. As a result, the number of abortion providers has declined. Therefore, causality may be running in the opposite direction. In this case, the likelihood of delivering unintended births should be independent of the abortion rate and abortion access measures. Then some extraneous factors affecting unintended pregnancies, *not abortion access*, will account for the probability of unintended births.

Economic Model

Pregnancy is a function of contraceptive use and frequency of sexual intercourse. If abortion and contraception were perfect substitutes, an increase in abortion availability might simply raise the pregnancy rate with no impact on the birth rate. In this case, abortion acts like insurance, and the unintended birth rate might stay the same. However, such perfect substitution is implausible in that contraception and abortion can not be determined contemporaneously. Rather, the substitution between abortion and contraception is based on an intertemporal choice, and during the lapse of time between conception and abortion many factors come into play. For instance, From the moment of conception to the moment of deciding on abortion, the probability of women's marrying with their partners varies widely.

The following unpublished Grossman model sheds light on the complexity among pregnancy, abortion, and birth. It emphasizes that an increase in abortion availability raises the price of children but lowers the price of sex. The advantage of this model over the one used by Kane-Staiger or Akerloff-Yellen-Katz is that an increase in abortion availability can raise the number of births because two prices are changing at the same time. This result emerges without the need to tell stories about the strategic behavior discussed in Kane and Staiger model as well as Akerloff-Yelln-Katz.

(1) $U = U(S, N, X)$

Utility is a function of sex (S), Children (N) and other consumption (X). The price of children is p, the price of abortion is q, and the price of other consumption, X, is normalized to one, thus yielding a budget constraint:

(2) $I = X + p \cdot N + q \cdot A$

Note that abortion and sexual abstinence are only forms of birth control. In addition, the number of children (N) is a function of the number of times a woman has sexual intercourse (S) and the number of abortions (A). The number of pregnancies is αS, and let us assume for the time being that the number of unintended pregnancies is $\alpha S - N$. Here the assumption is that all unintended pregnancies are aborted because $\alpha S - N = A$ So we have two equations.

(3) $N = \alpha \cdot S - A \quad \alpha < 1.$

(4) $A = \alpha \cdot S - N$

The substitution of equation (3) into equation (1) and (2) gives rise to the Lagrangian equation.

(5) $L = U(S, \alpha \cdot S - A, X) + \lambda(I - X - p \cdot \alpha \cdot S - p \cdot A + q \cdot A)$

(6) $L_s = U_s + \alpha U_n - \lambda P\alpha + 0$

(7) $L_n = -U_n + \lambda(p - q) = 0$

(8) $L_x = U_x - \lambda = 0$

(9) $L_\lambda = I - X + p\alpha S - PA + qA = 0$

Where U_i denotes the marginal utility of sex (i=S), children (i=N), and other consumption (i=X). From equations (5) and (6), we derive the following equation:

(10) $\frac{U_s}{U_n} = \frac{\alpha q}{p - q}$

Equation (10) is the equilibrium marginal rate of substitution between sex (S) and children (N). Note that the price of abortion (q) is found in both the numerator and denominator. Thus, the price of abortion affects the price of sex and the price of children: an increase in the price of abortion increases the price of sex and lowers the price of children.

Now we need to examine what happens to the frequency of sexual intercourse (S) and births (N) when there is an increase in the price of abortion. Assuming constant utility, we derive the following equations. Note that E is a log operator.

$$EN = K_n \sigma_{nn} E(p-q) + K_s \sigma_{ns} Eq$$

K_n and K_s denote the share of children and the share of sex, respectively, in the total budget. Similarly, σ_{nn} and σ_{ss} are the own-partial elasticity of substitution of N and S, respectively.

$$\frac{EN}{Eq} = K_s \sigma_{ns} + K_n \sigma_{nn} \frac{E(p-q)}{Eq}$$

$$\frac{E(p-q)}{Eq} = \frac{\partial(p-q)}{\partial q} \cdot \frac{q}{p-q} = -\frac{q}{p-q}$$

$$\frac{EN}{Eq} = -\frac{q}{p-q} k_n \sigma_{nn} + K_s \sigma_{ns}$$

We know that $-k_\sigma \sigma_{nn} > 0$, but the sign of $K_s \sigma_{ns}$ is ambiguous. If sex and children are substitutes ($\sigma_{ns} > 0$), a rise in the cost of abortion will increase the number of children ($\frac{EN}{Eq} > 0$). If sex and children are complements ($\sigma_{ns} < 0$), an increase in the price of abortion may decrease the number of children ($\frac{EN}{Eq} < 0$).

Similarly, I can develop an equation for the change in sexual activities with respect to the price of abortion.

$$ES = Ks \sigma_{ss} Eq + K_n \sigma_{nn} E(p-q)$$

$$\frac{ES}{Eq} = K_s \sigma_{ss} - \frac{q}{p-q} K_n \sigma_{sn}$$

Suppose $\frac{EN}{Eq}=0$ so $K_s\sigma_{ns}=\frac{q}{p-q}K_n\sigma_{nn}$, $\sigma_{ns}=\frac{q}{p-q}\frac{K_n}{K_s}\sigma_{nn}$

Thus $\frac{ES}{Eq}=K_s\sigma_{ss}-\left(\frac{q}{p-q}\right)^2\frac{k_n^{\,2}}{k_s}\sigma_{nn}$

$\frac{ES}{Eq}<0$ if $K_s\sigma_{ss}<\left(\frac{q}{p-q}\right)^2\frac{k_n^{\,2}}{k_s}\sigma_{nn}$

This equation suggests that in theory it is possible for a reduction in the price of abortion to raise the frequency of sex and have no effect on the number of children. The following proof helps us explain the reason why the condition is very likely to hold. $\hat{\sigma}_{nn}=-\sigma_{nn}$, $\hat{\sigma}_{nn}$ being the absolute value of the own-partial elasticity of consumption of children. $\hat{\sigma}_{ss}=-\sigma_{ss}$, $\hat{\sigma}_{ss}$ being the absolute value of the own-partial elasticity of substitution in consumption of S.

$k_s\hat{\sigma}ss>\left(\frac{q}{p-q}\right)^2\frac{k_n^{\,2}}{k_s}\sigma_{nn}$

$\hat{\sigma}_{ss}>\left(\frac{q}{p-q}\right)^2\left(\frac{k_n}{k_s}\right)^2\hat{\sigma}_{nn}$

Because we know that $k_n=N(p-q)/I$ and $k_s=sq/I$, we can rearrange the terms,

$\hat{\sigma}_{ss}>\left(\frac{q}{p-q}\right)^2\frac{(p-q)^2N^2}{g^2S^2}\hat{\sigma}_{nn}$. Finally, we obtain this expression: $\hat{\sigma}_{ss}>\frac{N^2}{S^2}\hat{\sigma}_{nn}$

Since the square of the ratio of optimal number of children to optimal amount of sex is much smaller than 1, the condition is likely to hold. This result holds only if sex and number of children are complements because $K_s\sigma_{ns}=\frac{q}{p-q}K_n\sigma_{nn}$.

A similar exercise can be conducted assuming the constant marginal utility of income (λ). Taking the partial derivatives of equation (6) and (7) with respect to the price of abortion (q) yields the following expressions for the change in births (N_q).

$$N_q = -\lambda(U_{ss} + \alpha U_{sn}) / H$$

where $H > 0$ is the determinant of the Hessian matrix of second derivatives of the problem and $U_{ss} < 0$. If $U_{sn} < 0$, then an increase in the price of abortion would result in a rise in births and a fall in abortion. This result is what intuition would predict. If sex and children are complements ($U_{sn} > 0$), an increase in the price of abortion raises the price of having sex and lowers the price of having children. The lower price of children induces the woman to have more children, and the increase in the price of sex will tend to reduce the number of children. Thus the net effect of the increase in the price of abortion on births is ambiguous.

In this model, all unintended pregnancies are aborted. Then how can we account for births from unintended pregnancies? Let us introduce intended birth, N^i and unintended birth, N^u. Assume that $N = N^i + N^u$. The model stays the same except that N now has two components. We now investigate whether N^u / N is likely to rise or decline after a change in abortion cost, q.

$$A = \alpha S - N$$

$$A = \alpha S - (N^i + N^u)$$

Thus, $\frac{dN^u}{dq} = \alpha \frac{dS}{dq} - \frac{dA}{dq} - \frac{dN^i}{dq}$.

If $\frac{dN^i}{dq} = 0$ and N does not change, it is logical to observe $\frac{dN^u}{dq} > 0$ if $\alpha \frac{dS}{dq} > \frac{dA}{dq}$. We know that $EN / Eq = 0$ if and only if $[q/(p-q)]k\sigma_{nn} = ks\sigma_{ss}$. If N does not change, then N^u / N does not change as long as $\alpha \frac{dS}{dq} = \frac{dA}{dq}$ and $\frac{dN^i}{dq} = 0$. Further, we can conceptualize unintended birth in this model when S and N are complements. For example, an increase in S raises the marginal utility of N in demand functions that hold the marginal utility of income constant. Given that S and N are complements, a reduction in the cost of abortion can raise N because it lowers the cost of S. A decline in the cost of S raises in turn the optimal values of S, and a rise in the optimal S raises the optimal value of N. The additional number of births can be considered unwanted in the sense that they would not occur in a model in which people derived no utility from sex. The same would be true where S and N were substitutes.

Econometric Model Description

Pregnant women's decision to carry an unintended pregnancy to term depends on various observed and unobserved factors. The observed factors include women's household income, opportunity cost, direct abortion and contraception costs, and childbearing costs. Such available information as education, race, age and marital status will proxy the aforementioned factors. Attitudes toward and preferences for children,

religious values, and the stigma attached to abortion are, among others, some unobserved maternal characteristics that have the potential to determine the probability of delivering a baby given unintended pregnancy.

The variables of interest to us are two types of abortion access measures: the first is number of abortion providers variables, and the second is distance barrier variables. These variables include number of abortion providers, dummy indicating a county with an abortion provider, and the average distance to nearest abortion provider. All the detailed measures are described in Table 7. The model investigates the effect of abortion access on the dichotomous measure of unintended/unwanted birth, holding constant individual characteristics such as race, education, age, and marital status.

$$Y_{ijt} = \alpha_0 + \sum_{k=1}^{n} \alpha_k \cdot X_{ijt} + \beta_0 P_{jt} + \sum \gamma_j S_j + \sum \delta_t D_t + e_{ijt} \quad (1)$$

The subscripts i, j and t denote individuals, states and time respectively. The dichotomous dependant variables Y_{ijt} indicate whether a pregnant woman delivered a baby given an unintended pregnancy. P_{jt} denotes a vector of county-level abortion access measures. The vector of variables, X, includes maternal characteristics such as race, age, marital status, and educational attainment. The previous discussion on the determinants of abortion leads us to control for all these variables. The model specification further includes state dummies (S_j), a time trend (D_t), and a disturbance term (e_{ijt}) that is by assumption not correlated with the included variables.

$$Y_{ijt} = \alpha_0 + \sum_{k=1}^{n} \alpha_k \cdot X_{ijt} + \beta_0 P_{jt} + \beta_1 P_{jt} \cdot Z_{ijt} + \sum \gamma_j S_j + \sum \delta D_t + e_{ij} \qquad (2)$$

Since higher educational attainment reflects greater opportunity cost, educated women may opt for abortion regardless of or because of easy access to abortion providers. Abortion accessibility may facilitate abortion for this group of women. Less educated women may respond in a different manner to abortion access. Thus, I also investigate the extent to which an interaction term between education level and abortion access measures may account for an association between the two variables.

These individual observations can be augmented with some state level variables. Candidates for these variables include maximum monthly TANF benefits, monthly Medicaid benefits per TANF recipient, unemployment rate, and number of family planning clinics per 1,000 women. Because these variables are not subject to our immediate attention, and fixed effects regression can account for a considerable portion of the cross-state differences and time-varying factors, we simply include state dummy variables and year dummies. First, state dummy variables can remove the consequences of time-invariant differences among states on the dependent variables associated with the other independent variables. For instance, state dummies control for such unobserved heterogeneity as abortion attitude, religious belief, and attitude for childbearing across states without the need to measure them explicitly. Second, the inclusion of year dummies controls for the effects of time-series differences over time.

If there is an association between abortion access measures and probability of unintended birth, a mechanism may be operating through abortion rate, partly because abortion accessibility determines abortion rate. Therefore, a state level regression will be

conducted to investigate a possible negative association between abortion accessibility and abortion rate. Further, a regression similar to that depicted in equation (1) will be analyzed after replacing the abortion access measures with state abortion rate. This structural regression represents that probability of unintended/unwanted birth is a function of state level abortion rates. Intuition suggests that abortion rate and probability of unintended birth should be negatively correlated.

These regressions attempt to account for causal mechanisms linking unintended births, the abortion rate, and the availability of abortion providers. One fundamental problem is that the variables for abortion providers may be endogenous in equations (1) and (2). The supply of abortion providers is partly determined by the demand for abortion, and abortion rates are in turn determined by the probability of delivering unwanted births. In the absence of appropriate instrumental variables, a possible way to detect whether there is a substantial bias in the regression that I implement is to investigate the different coefficients and the signs of many different access measures. The variables, number of large abortion providers and their presence relative to small providers in a particular county may be rather exogenous because the large providers should be less affected in the short run by the demand for abortion. Therefore, the coefficients are unlikely to be contaminated relative to other measures. If the regression results register the opposing signs for those least likely endogenous variables and the rest of the variables, one would infer that an endogeneity problem is running rampant. The regressions dealing with abortion rate and abortion accessibility also face a similar endogeneity problem. We will also infer in this regression whether the abortion access measures are seriously contaminated, comparing the coefficients of the variables, number

of large abortion providers and other variables. All models are estimated by a linear probability model, which facilitates an interpretation of marginal effect of abortion access measures. Further, coefficients provide similar magnitudes of marginal effect compared to logit and probit model.

Empirical Study

1) Summary Statistics

Out of the total births, 45% are unintended, either unwanted or mistimed. This high percentage is driven up by the relative frequency of unintended births among less educated, unmarried and younger women. While women with education beyond college report only 5 percent unwanted births, the figure is 18 percent for women who have not received a high school diploma. The racial disparity is also alarming. 69 percent of black mothers deliver unintended babies, whereas white mothers report that 38% of births are from unintended pregnancies.

The different age categories do not follow a monotonic increase in unintended births; rather, they follow a U-shaped pattern. The youngest category, mothers younger than 20, shows the highest unintended percentage, namely, 74 percent, and the percentage decreases to 30.55 percent when the age category reaches 30 to 34. Women over 35 show a somewhat higher percentage of unintended births than the previous category. This high percentage is driven mainly by the high percentage of unwanted births (20 percent) rather than mistimed births (13 percent).

2) Econometric Results

a) The aggregated regression:

The variables of interest, both the number of abortion providers and the distance barriers, are described in Table 7. These variables are all county level. All models include a host of individual characteristic variables encompassing race, education level, age, and marital status. These abortion access variables are used as repressors in separate regressions. In addition to the individual characteristic variables, Model 1 further includes county and year fixed effect. Model 2 instead includes state and year fixed effect. State-specific linear time trend is included in Model 3 in addition to all the variables in Model 2. Both Model 2 and Model 3 employ the number of women aged 15-55 as a county level weight since our repressors are the county level variables except for the first three variables indicating the number of providers listed in Tables 10-15.

The descriptions of the probability of unintended births and the probability of unwanted births can be found, respectively, in Table 8 and Table 9 showing the results of linear probability model for the selected abortion access measures. All the individual variables show the expected sign and for the most part are statistically significant. From Table 8, the college educated and more than college educated are nine percentage points less likely to have unintended births relative to non high school diploma holders. Teen mothers display a 13.4 percentage point higher probability of unintended births than do women over 35. White mothers are 15 percentage points less prone to unintended births than black mothers. White mothers are 14.3 percentage points less likely than their black counterparts to deliver unintended babies. Clearly, marital status turns out to be a major component determining unintended births because the disparity between married women

and unmarried women in the probability of unintended birth is 25 percentage points, controlling for such factors as education, age, and race. These magnitudes are virtually unchanged regardless of the inclusion of any abortion access variables.

However, Table 9 exhibits somewhat different magnitudes. Further, the sign of the age variable becomes positive. The older a woman is, the more likely it is that her birth is unwanted. For instance, women over 35 compared deliver unwanted births 15.8 percentage points more frequently than teenage mothers. The probability of unwanted births among married women is 8.7 percentage points lower than among unmarried women. These results suggest that the 25 percentage point disparity between unmarried and married in the probability of unintended births is driven mainly by the probability of mistimed births. Marital status and age of pregnant women for obtaining the probability of unwanted babies seem not as important as for determining the probability of both mistimed births and unintended births.

In Tables 10-15, separate regressions are performed using different abortion access measures. When the dummy for a county with more than one non-hospital abortion provider is included in the regression, the dummy for a county with only one non hospital provider is controlled for. The same is true of the variable, the dummy for a county with more than one provider. Thus, the reported coefficients of the set of two variables are from the same regressions.

None of the coefficients in Model 1 is statistically significant, but only one variable in Model 2 and Model 3, the dummy for a county with at least an abortion provider, appears to be statistically significant at 10 percent p-value. A main reason may be that county fixed effects generate large standard errors, indicating that county

variation may not be sufficient to lead to statistical significance. I also estimated county and year fixed effects with county specific-linear time trends. The results are not reported because the outcomes are similar to Model 1. The signs of the variable for the number of abortion providers are, for the most part, positive, albeit statistically insignificant, indicating that the greater the number of abortion providers, the more likely it is that women will deliver unintended births. But the sign for the distance barrier variable is not consistent with what we found for the number of abortion provider variables. The positive sign means that the greater the distance separating a woman and the nearest abortion provider, the more likely it is that unintended babies are born. However, the coefficients are not significant and thus a causal interpretation is not warranted. In Model 3, I add a state and linear time interaction term, but the coefficients and the signs stay the same as in Model 2.

We need to exercise caution about interpretation in that the abortion access variables may be endogenous. However, the variables, both number of large providers per 100,000 women and dummy for a county with at least a large non-hospital provider, are likely to be exogenous, and the signs are positive. The positive sign of these variables does not conflict with the rest of the number of abortion provider variables. If there were a serious bias caused by endogeneity of the independent variables, they could have shown different signs, but they are for the most part positive.

In Table 11, Models 2 and 3 show that both the number of large non hospital providers per 100,000 women and a dummy for a county with at least one non-hospital abortion provider variable are statistically significant. Again, all coefficients in Table 10 are statistically insignificant, and most of them are positive as can be seen in Table 3. The

coefficient of the number of large non-hospital providers per 100,000 women suggests that an increase of one large non hospital provider per 100,000 women in a county may induce 0.4 percentage points *greater* probability of unintended births relative to the mean, 11.94 percent. Similarly, the coefficient of the dummy variable for a county with at least a large non-hospital abortion provider has the same positive sign and the magnitude is quite large. This result suggests that a county with at least a large non- hospital abortion provider exhibits 1.2 percentage points higher probability of unwanted births relative to its counterpart.

It is noteworthy that the sign may be what our economic theory suggests. All in all, there is no strong evidence of any association between various abortion access measures and the probability of unintended births, but there is some evidence that certain abortion access measures are positively correlated with probability of unwanted births. The weak association between unintended births and abortion access measures is apparent at the aggregated level, which may corroborate the previous findings (Matthews et al., 1997) that abortion access measures have no impact on birthrates. Reverting to our previous theoretical discussion, we discover that $EN / Eq = 0$ if and only if $[q/(p-q)]k\sigma_{nn} = ks\sigma_{ns}$. If N does not change, then it is that the probability of unintended births out of total births, N^u / N, does not change as long as $\alpha \frac{dS}{dq} = \frac{dA}{dq}$ and $\frac{dN^i}{dq} = 0$. However, it is important that some evidence linking the probability of unwanted births and some abortion access measures is found in the least endogenous variables. The latter variables are the number of large non-hospital providers per 100,000 women and the dummy for a county with at least a large non-hospital provider.

b) Interaction with Education Level

A major difference from the previous model is to incorporate an interaction term within our econometric equation in an effort to investigate whether women with low education respond in a different manner to the set of different abortion access measures. The reasons for this exercise are twofold. First, the previous model may have masked heterogeneous effects of abortion access on different segments of the population. Indeed, Cook, Parnell, Moore and Pagnini (1999) used stratified subgroups, young and old, black and white, married and unmarried in an effort to investigate an association between the availability of state abortion funding and abortion rate. Kane and Staiger (1996) discovered the differential impacts of abortion availability on out-of-wedlock birth rate and in-wedlock birth rate. Second, when referring to Table 1, 8 and 9, one discovers that there is a threshold separating women who have not achieved a high school diploma and women who have advanced beyond high school. In the summary statistics, the proportion of unintended births among more educated women is below the mean, 45 percent. The multivariate results in Tables 8 and 9 indicates that women with higher education are less prone to deliver unintended births and unwanted babies.

One explanation for the positive relationship shown in Table 8-11 between abortion access and unintended births is that relatively low-risk women opt out of pregnancy given that their pregnancies are unintended. Since unintended pregnancy is highly correlated with low socioeconomic status, it may be that high-risk women, given pregnancy, carry to term. Currie, Nixon and Cole (1996) found that AFQT was positively related to abortion given pregnancy. Many studies suggest that more educated women

choose abortion more readily than less educated women. Income also comes into play because it seems to be an important component in abortion decision-making. Income and opportunity cost are important determinant factors in seeking an abortion.

Thus, the education variable serves as a proxy for income and opportunity cost. Subsequently, the dummy variable indicating low education (with 12 or fewer years of schooling) interacts with a host of different abortion access measures. If the impact of abortion access measures on the probability of unintended births and unwanted births were not the same for educated women and less educated women, one would have a significant interaction effect between abortion access measures and the dummy for low education. In an effort to investigate this potential effect, separate regressions can also be conducted using a subgroup of less educated women, but preference is given to the previous method because the latter method would deprive us of a significant number of observations.

The three models ranging from 1 to 3 in Tables 12-15 have the same model specifications as the ones without the interaction terms shown in Table 8-11. None of the coefficients in Table 12 is statistically significant. However, the interaction terms are unanimously positive, and the magnitudes are larger than the abortion access measures alone. Table 13 indicates that most interaction terms as well as nearly all abortion access variables become statistically significant, even in Model 1. The county fixed effect in Table 13 does not generate standard errors as large as those shown in Table 12, thus causing the coefficients to be statistically significant for all three models.

The three interaction variables, including number of providers per 100,000 women, number of non-hospital providers per 100,000 women, and number of large non-

hospital providers per 100,000 women, become statistically significant in all three models and show the positive sign. This result implies that among women with low education, thus likely to be more impoverished and to have lower opportunity cost than their counterparts, the association between abortion access and the probability of unwanted births is positive. The last variable, number of large non-hospital providers per 100,000 women, displays larger magnitude than the other two. It shows that an increase in the number of large non hospital providers per 100,000 in a county tends to raise the probability of unwanted births by 0.8 percentage points among less educated women. This magnitude is larger than the one found in the model without interaction, 0.4 percentage points among all women (Table 11) that would have been driven mainly by the 0.8 percentage point figure among less educated women. This variable is considered less biased because it should be least endogenous. The second largest magnitude is manifest in the other variable least likely to be endogenous, the dummy for a county with at least a large non-hospital provider. When there is a large non-hospital provider in a county, women with low education tend to deliver unwanted babies 1.8 percentage points more often than when there is no such facility. Similarly, if a woman lives in a county with at least one abortion provider, this increases the likelihood of unwanted birth by 1.9 percentage points.

These findings are disparate, in opposition to the view that women seek an abortion more frequently with large non-hospital providers than other types of providers. The presence of a large non-hospital provider seems to facilitate women's achievement of their desired abortion. Regardless of an increase in abortion rate, abortion access

measures appear to raise the probability of unwanted births. Then, the causal mechanism should have operated through factors other than abortion rate.

The variable, the dummy for a county with at least a non-hospital provider displays a smaller magnitude relative to the variable, the dummy for a county with more than one non-hospital abortion provider. It suggests that an increase in the number of non-hospital abortion providers in addition to a hospital provider is associated with an increase in the probability of unintended birth among less educated women. However, that mechanism is not apparent between the coefficients of the two variables, the dummy for a county with at least an abortion provider and the dummy for a county with more than one abortion provider.

The distance barrier variables do not show consistent results. The coefficient of the variable, nearest non hospital provider, estimates that an decrease in 100 miles to nearest non-hospital provider and thus easier access to a non hospital provider raises the probability of unwanted births by 1.6 percentage points among women with low education. However, for the same women, an increase in 100 miles to a nearest provider is likely to raise the probability of unwanted births by 7 percentage points. This contrary result suggests that the variable, distance to a nearest abortion provider, is not a good access measure compared with distance to a nearest non-hospital provider, thus producing implausible results. A simple reason is that the distance to a nearest abortion provider variable includes hospital and non-hospital and that hospitals are more sensitive to the short-term fluctuation of demand for abortion. Then one would have statistical significance and the negative sign in the coefficient of the variable, distance to nearest

large non-hospital provider because the variation of the distance is not driven by short-term demand.

Nevertheless, these contrary results among distance barrier variables lead one to consider that without controlling for the number of abortion providers, the abortion distance barrier measures will not proxy abortion cost accurately. Thus, I include these distance variables, holding the number of abortion providers constant. For instance, I used two independent variables, number of providers per 100,000 women and distance to a nearest provider, in the same regression. Then, neither of these two variables becomes statistically significant. When the two variables, number of non-hospital providers per 100,000 women and distance to nearest non hospital provider, are used together in the regression, the distance variable remains statistically significant and displays the same negative sign with a 0.3 percentage points increase in the magnitude. But the number of non-hospital providers per 100,000 women turns out to be no longer significant. The same exercise is done with the two variables, number of large non-hospital provider and distance to nearest large non-hospital provider. Then the distance variable stays insignificant, and the number of large non-hospital abortion providers remains significant with a 0.8 percentage point increase in magnitude.

C) Interaction with Medicaid Recipients

Tables 14 and 15 employ a dichotomous variable indicating, as a proxy for opportunity cost and income measures, whether or not woman is a recipient of Medicaid rather than low and high education. Medicaid enrollees are predominantly impoverished single mothers. This variable may more accurately identify a disadvantaged group in

terms of income and employment; most Medicaid recipients are welfare recipients or often unemployed. Thus, this proxy variable may improve our measure of opportunity cost and income.

Five patterns emerge. First, all interaction terms are positive and statistically significant in each of our three models. Nearly all of the abortion provider measures flip the sign from positive to negative and from statistically insignificant to significant compared with the previous model with the education proxy. Second, the magnitudes are much larger than the ones in the previous model using education proxy. For instance, a county with at least a non-hospital provider raises the probability of unintended births by 10.8 percentage points among Medicaid recipients. Third, distance measures become positive and significant across the board, a finding that negates that suggested by the number of abortion provider variables. Fourth, unlike previous models, Model 2 and Model 3 show quite a substantial difference, indicating that state-specific linear time trends come into play, and that the Medicaid variable seems correlated with the state specific time trends. Fifth, the magnitudes in Table 14 are larger than those in Table 15.

Facing the positive signs of the distance barrier measures, one may argue that women may want to travel to have an abortion with a large out-of- state abortion provider despite the presence of some abortion providers in their county or states. Travel may not deter abortion as long as women find a reliable, large abortion provider. The finding of Blank et al. (1996) was that the primary effect of provider availability is to lead women to change the location of abortion, not the actual propensity to abort. It was documented (Abortion Fact Book, 1992) that there are several reasons why women may travel farther to reach a large clinic. First, large clinics are less expensive, on average. Second, they are

more likely to offer general anesthesia and second-trimester services. Third, they may advertise in telephone books and newspapers in distant areas and be better known by reputation, and women may feel that these services are more confidential than those offered by small, local providers. However, one may cast doubt on the possibility that the majority of women, even among Medicaid recipients, are willing to travel a long distance for an abortion.

In light of these findings, a causal interpretation becomes quite difficult for distance barriers. Nevertheless, the consistent results from Tables 8 to 15 among abortion provider variables, particularly the variables least likely endogenous, suggests that the association, if any, between the presence of abortion providers and the probability of unintended birth is positive. This positive association implies that notwithstanding the presence of abortion providers, it is likely that impoverished women will either fail to abort or fail to use contraception, thereby increasing the probability of unintended birth. Given the evidence shown in the previous literature, abortion rate is negatively associated with abortion access. Then, one may posit that although abortions have increased, there should have been an increase in unintended pregnancies, which may have contributed to the greater probability of unintended birth. A possible reason is that the presence of abortion provider may have altered contraceptive effort. Alternatively, there may be negative selection for abortion were educated women to resort to better contraception in an environment where access to abortion becomes increasingly difficult. To investigate whether this view can be validated, we now turn to the regression results of the effects of abortion rate on probability of unintended/unwanted birth.

d) The effects of Abortion Rates on the Probability of Unintended/Unwanted birth

It appears that the overall abortion rate and the probability of unwanted births are negatively associated (Table 22) among women with 12 or fewer years of schooling, although the negative association is not present in other regressions. Much precaution is needed in interpreting these results because changes in unintended pregnancies may affect both unintended births and abortion rates. For instance, an increase in unintended pregnancy would have raised abortion rates and thus decreased the probability of unwanted birth (Table 22). Inversely, an increase (a decrease) in unintended pregnancy would have raised (lowered) both abortion rate and the probability of unwanted birth, a finding in Tables 19-21. These regressions alone would not provide a complete mechanism linking abortion access measures, abortion rate, and probability of unintended birth. The following reduced form regression,[7] where exogenous abortion access measures are used, helps us to infer the mechanism abortion access measures and the probability of unintended/unwanted birth.

e) The Effects of Abortion Access on Abortion Rate

Tables 16-18 show an association between state-level abortion rates and state level abortion availability. All the county level data on abortion providers and abortion distance are aggregated at the state level. For instance, the variable, "percent of women living in a county with at least a provider," is obtained in the following way. First, multiply the dummy variable, 1 or 0, by women's population of reproductive age at the county level. Second, aggregate all the numbers in a state and divide those numbers by

the state's total population of reproductive age. In the regression, the dependent variables are natural logarithms of the state-level abortion rate, as are the independent variables indicating the number of abortion providers. The rest of the independent variables are level. All regressions are weighted by the number of women aged 15-44 in a state. When calculating mean distance to a nearest provider, I used county level population of women from 15 to 44 as a weight. One of the main purposes of this exercise is to demonstrate that the PRAMS 10 states not be an idiosyncratic sample out of all states. The PRAMS 10 states indeed reflect the trends and nature of the data incorporating all states.

From Tables 16 through 18, Model 1 includes state fixed effect and year fixed effects. Model 2 further includes state specific linear time trends. The regression results are not robust to the inclusion of state-specific linear time trends; there are substantial changes in the magnitudes of the coefficients. Table 16 shows, using all states, the association between abortion availability and abortion rate during the period of our study, 1993-1997. As expected, an independent variable, the number of large non-hospital providers, displays great explanatory power, coupled with statistical significance. It indicates that a 10 percent increase in the number of large non-hospital providers per 100,000 women gives rise to a 2 percent decline in abortion rate. As expected, the variable, the number of non-hospital providers, large or small, per 100,000 women, has smaller magnitude of the coefficient, positive yet statistically insignificant. The rest of the variable does not exhibit the expected sign. This result seems to be caused by the endogeneity of the variables, indicating that the coefficients are contaminated. The number of non-hospital providers per 100,000 women and the number of large non-

[7] The two reduced form regressions are "probability of unintended birth $=f$ (abortion access measures)" and "abortion rate $= g$ (abortion access measure)," the more structural form being " probability of unintended

hospital providers per 100,000 women are the ones that have remained relatively stabilized over time, at least for a short period time, in spite of changes in the abortion demand.

The Model 2 result in Table 18 for the PRAMS states shows a pattern similar to the one we obtained from the pooled sample based on all states. A unique variable, statistically significant, is the number of large non-hospital providers per 1000,000 women. Although the magnitude is somewhat smaller than that shown in Table 16, the coefficient has the expected positive sign. This result suggests that some other abortion availability measures would have had a positive effect on abortion rate had the endogeneity problem been resolved for the other variables.

One shortcoming of this state level regression is that one could not sort out women who choose to have an abortion due to abortion access. Previous literature documented that women with high income or high education will seek an abortion regardless of barriers to abortion providers. As the opportunity cost of the women's time rises, the cost of bearing and rearing children also increases. On the other hand, this group of people has the financial resources to obtain an abortion whether or not the procedure is not generally available. Clearly, the lack of financial means deters women from aborting. This point is supported by the findings that restricting Medicaid financing of abortion to low income women is negatively associated with abortion rate. Then, when income is held constant, abortion access would have had greater influence on the abortion decision of a group of women, younger, unmarried, and uneducated, compared with its counterpart. This group of women was proxied by the Medicaid variable in the regressions (Tables 14-15). The positive interaction of this proxy variable with abortion

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birth =*h* (abortion rate)."

access measures suggests that abortion access would have facilitated the contact of underprivileged women with abortion clinics and facilities, income held constant, but it would also have increased unintended pregnancies.

Clearly, abortion access, namely abortion cost, may directly affect abortion rate, but abortion access may alter unintended pregnancies, thereby affecting abortion rate.[8] All in all, it appears that abortion access may induce impoverished women to seek an abortion, but it should have increased unintended pregnancies, predominantly mistimed pregnancies partly by altering their contraceptive behavior. Our empirical findings suggest that easy access to abortion increase both unintended pregnancies and births given unintended pregnancies. These findings also imply that a rise in abortion rate among the underprivileged group of women may have not offset an increase in unintended pregancies.

Conclusion

It appears that there is no association between unintended births and abortion access measures at the aggregated level. However, there is some evidence that abortion access measures are positively associated with unintended births and unwanted births for uneducated women or Medicaid recipients. The previous consistent results corroborate

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[8] α denotes probability of a birth from unintended pregnancy, β is probability of unintended pregnancy, and γ is probability of abortion given unintended pregnancy. Assume that X is a vector of abortion access measures. Since $\alpha = \beta - (\beta \cdot \gamma)$, then we derive $\frac{\partial \alpha}{\partial X} = \frac{\partial \beta}{\partial X} - (\gamma \cdot \frac{\partial \beta}{\partial X} + \beta \cdot \frac{\partial \gamma}{\partial x})$. Thus, if $\frac{\partial \alpha}{\partial X} > 0$, then it should be that $(1-\gamma) \cdot \frac{\partial \beta}{\partial X} > \beta \cdot \frac{\partial \gamma}{\partial x}$. This result can be supported in the following proof. Let us denote N, the total number of births , N^u, births from unintended pregnancies, UP, unintended pregnancies, and A, abortion. Then N^u is clearly UP-A. The probability of a birth from unintended pregnancies is N^u/N. N^u/N =(UP/N) – {(UP/N)(A/UP)}, (UP/N) being probability of unintended pregnancies and (A/UP) being probability of abortion given unintended pregnancies.

the hypothesis that unintended pregnancies may have drastically increased for this segment of the population, partly due to a change in contraceptive effort.

From a policy perspective, three concerns surface from the findings of our research. First, our results do not support the findings of Donahue and Levitt; they showed that a decline in crime rate is more than 50 percent accounted for by an increase in abortion rate after abortion legalization. Despite a rise in abortion rate followed by enhanced abortion access, unintended births may have risen among underprivileged groups of families, while this segment of the population would have committed a significant proportion of crimes. Second, it seems important that easier access to abortion enables women to terminate unintended pregnancies; consequently, maintaining abortion providers and federal funding for abortion contributes to decreasing unintended births. If restricting state funding for abortion or lack of abortion providers induces fewer abortions and more births, and these marginal babies are unwanted, the social cost of rearing unwanted babies (Angrist and Evans, 1999, Gruber, Levine and Staiger, 1999) seems far greater than funding for the abortion. Third, without encouraging underprivileged groups of women to exercise wider contraceptive effort, providing abortion funding or abortion facilities alone may not reduce the nation's large proportion of unintended births.

Table 1
Summary Statistics from 1993 to 1997

	Unintended	Unwanted	Mistimed
Overall Mean	45.28	12.12	33.16
Race			
Black	68.88	23.79	45.09
White	38.42	8.81	29.62
Others	45.05	10.62	34.43
Education			
Less than High School	53.38	18.39	35.00
High School Dropout	64.60	18.69	45.91
High School	48.41	13.50	34.91
Some College	43.20	10.68	32.52
More than College	24.85	4.91	19.94
Marital Status			
Married	33.27	8.15	25.12
Unmarried	71.83	21.09	50.75
Age			
Fewer than 20 Years	74.00	13.96	60.04
20 to 24 Years	57.32	12.30	45.02
25 to 30 Years	38.92	9.82	29.10
30 to 34 Years	30.55	10.87	19.68
More than 35 Years	32.88	19.69	13.19
Parity			
Previous Births	44.35	6.39	29.48
First Child	46.05	16.57	37.95
Medicaid Recipients	63.43	17.71	45.72
Prenatal Care and Birth Outcomes			
Smoking*	53.36	17.09	36.29
No Prenatal Care	70.71	33.30	37.42
Early Prenatal care**	65.51	20.70	44.91
Late Prenatal Care***	69.39	29.21	40.18
Low Birth Weight***	51.30	15.88	35.43
Very Low Birth Weight*****	48.47	14.89	33.58
Total Observation	79566	79566	79566

Note: all numbers are in terms of percentage. Thus 68.88% at the intersection of the row, "black" and the column, "unintended" indicates that 68.88% out of all babies born to black mothers were reported unintended.

* proportion of women who smoked during pregnancy
** proportion of women who initiated their prenatal care in the first trimester
*** proportion of women who initiated their prenatal care in the third trimester
****proportion of new born infants whose weight is below 2,500 grams
*****proportion of new born infants whose weight is below 1,500 grams

Table 2: States and Years participating in PRAMS project

	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997
Alaska			•	•	•	•	•	•	•	•
Alabama					•	•	•	•	•	•
Florida						•	•	•	•	•
Georgia						•	•	•	•	•
Maine	•	•	•	•	•	•	•	•	•	•
Michigan						•	•	•	•	•
Oklahoma	•	•	•	•	•	•	•	•	•	•
S. Carolina					•	•	•	•	•	•
Washington					•	•	•	•	•	•
West Virginia	•	•	•	•	•	•	•	•	•	•

Table 3: NCHS Total Births

	89	90	91	92	93	94	95	96
Maine	17466	17357	16752	16057	15065	14441	13896	13774
West Virginia	22163	22585	22508	22170	21972	21375	21162	20750
Oklahoma	47385	47647	47795	47557	46243	45703	45672	46193
Alaska		11902	11686	11726	11073	10678	10244	10037
S. Carolina				56192	53835	52043	50926	51117
Alabama				62260	61706	60939	60329	60488.
Florida					192539	190654	188723	189392
Georgia					110622	111011	112282	114043
Indiana					83949	82595	82835	83513
Michigan					139855	138028	134642	133387
Washington					78645	77358	77228	77945
Total	87014	99491	98741	215962	731555	722230	715103	717126

Table 4: Prams Total Births

	89	90	91	92	93	94	95	96
Maine	16121	16119	16035	15309	14466	13837	13278	13216
West Virginia	19798	20296	22264	20031	19793	19320	18951	18612
Oklahoma	42192	43257	44956	43335	38547	42454	42711	43126
Alaska			11622	11590	10978	10296	9870	9892
S. Carolina				12483	51304	49005	47852	47961
Alabama				10030	59200	58431	57875	57954
Florida					159199	187329	185388	185992
Georgia					107114	107742	109193	110581
Michigan					136143	134277	130980	129704
Washington					63511	73803	73819	74495
Total	78111	79672	94877	112778	660255	696464	689917	691533

Table 5
Abortion Accessibility Measures from 1988 to 1997

	Mean	1988	1992	1993	1994	1995	1996	1997
Number of abortion providers	2405	2585	2380	2380	2087	2087	2042	2042
Number of large abortion providers	753	788	755	755	732	732	738	738
Percent of women living in a county with at least one provider	63.32	62.30	63.96	63.92	62.53	62.63	62.09	62.07
Percent of women living in a county with at least a large provider	54.32	55.20	54.45	54.32	53.92	53.71	53.86	53.79
Number of 1-24 hospitals*	425	464	439	439	407	407	361	361
Number of 25-394 hospitals	360	493	351	351	285	285	294	294
Number of 395+ hospitals	62	83	66	66	50	50	48	48
Number of 1-24 non hopitals	217	214	260	260	176	176	187	187
Number of 25-94 non hospitals	254	273	267	267	227	227	214	214
Number of 95-394 non hospitals	296	350	308	308	260	260	248	248
Number of 394-994 non hospitals	236	233	237	237	237	237	246	246
Number of 995+ non hospitals	454	472	452	452	445	445	444	444

Sources: Alan Guttmacher Institute

* the number indicates the number of abortions performed in a year

Table 6
Distance Barriers to Abortion Providers from 1988 to 1997*

	Mean	1988	1992	1994	1996
Nearest Hospital Provider	82.96	74.97	81.63	89.5	89.71
Nearest Non Hospital Provider	66.47	61.99	66.44	69.46	69.37
Nearest Provider	60.99	56.31	60.41	64.9	65.05
Nearest Provider (In state)	149.26	144.73	148.57	153.52	153.79
Nearest Provider (Out of State)	122.53	118.5	122.09	125.93	125.98
Nearest Large Hospital Provider**	244.68	209.17	218.34	306.37	307.31
Nearest Large Non Hospital Provider***	84.56	86.11	81.02	87.07	86.93
Nearest Large Non Hospital Privider(In state)	571.44	597.79	425.53	607.27	607.47
Nearest Large Non Hospital Privider(Out of State)	149.93	150.94	147.59	150.65	150.26

Source: Alan Guttmacher Institute
* All variables are measured by miles
** Large hospital provider indicates one that performs more than 395 abortions per year
*** large nonhospital provider indicates one that performs more than 995 abortions per year

Table 7
Abortion Access Measures Used in Regression Analysis

Variables	Means	
	For All States	For PRAMS States
Number of abortion providers		
Number of abortion provider*	3.51	2.7
Number of non hospital providers**	2.64	1.89
Number of large non hospital providers***	0.73	0.73
County with at least an abortion provider	67.05	58.29
County with more than one abortion providers	58.35	50.34
County with at least a non hospital providers	62.51	53.12
County with at least a large non hospital providers	0.53	0.45
County with more than one non hospital providers	52.6	42.73
Distance Barriers to Abortion Providers+		
Nearest provider, either hospital or non hospital	24.42	32.37
Nearest non hospital provider	27.71	35.88
Nearest large non hospital provider	36.06	44.3

Distance Barriers are measured by miles
* ** *** are numbers per 100,000 Women
Note: all means are weight by the nubmer of women 15-44 in a county.

Table 8
The Effect of Abortion Access on the Probability of Unintended Births, 1993 –1997

	Model 1	Model2	Model 3
Dummy for a county with at least one abortion provider	0.0011 (0.0331)	0.0138* (0.0077)	0.0142* (0.0077)
Race			
White	-0.1431*** (0.0080)	-0.1440*** (0.0079)	-0.1441*** (0.0079)
Other	-0.0906*** (0.0161)	-0.0942*** (0.0154)	-0.0939*** (0.0154)
Age			
20-24 years	-0.0923*** (0.0079)	-0.0939*** (0.0082)	-0.0936*** (0.0082)
25-30 years	-0.0071 (0.0082)	-0.0092 (0.0085)	-0.0093 (0.0085)
30-34 years	-0.1270*** 0.0094	-0.1322*** 0.0098	-0.1319*** 0.0098
Over 35 years	-0.1337*** (0.0124)	-0.1402*** (0.0129)	-0.1403*** (0.0129)
Educational Attainment			
High school dropout	0.0445*** (0.01520	0.0560*** (0.0158)	0.0562*** (0.0158)
High school diploma	0.0084*** (0.0141)	0.0191 (0.0147)	0.0189 (0.0147)
Some college	0.0130 (0.0148)	0.0216 (0.0153)	0.0213 (0.0153)
More than college	-0.0815*** (0.0151)	-0.0732*** (0.0157)	-0.0733*** (0.0157)
Married	-0.2481*** (0.0081)	-0.2494*** (0.0084)	-0.2492*** (0.0084)
Mean	45.01	45.01	45.01
# of Observation	76655	76655	76655

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

Model 1 includes county fixed effect and year fixed effects
Model 2 includes state fixed effect and year fixed effects
Model 3 includes state, year fixed effects and state and linear time trend interaction

Table 9
The Effect of Abortion Access on the Probability of Unwanted Births, 1993 –1997

	Model 1	Model2	Model 3
Number of large Non Hospital Providers per 100,000 Women	0.0014 (0.0104)	0.0040* (0.0022)	0.0041* (0.0022)
Race			
White	-0.1053*** (0.0066)	-0.1056*** (0.0052)	-0.1056*** (0.0062)
Other	-0.0978*** (0.0102)	-0.0973*** (0.0099)	-0.0971*** (0.0099)
Age			
20-24 years	0.0326*** (0.0052)	0.0326*** (0.0052)	0.0327*** (0.0052)
25-30 years	0.0124** (0.0055)	0.0115** (0.0055)	0.0117** (0.0055)
30-34 years	0.0678*** (0.0062)	0.0663*** (0.0062)	0.0664*** (0.0062)
Over 35 years	0.1586*** (0.0099)	0.1557*** (0.0100)	0.1558*** (0.0100)
Educational Attainment			
High school dropout	0.0111 (0.0114)	0.0134 (0.0113)	0.0133 (0.0101)
High school diploma	-0.0212** (0.0102)	-0.0213** (0.0101)	-0.0216** (0.0101)
Some college	-0.0405*** (0.0105)	-0.0460*** (0.0104)	-0.0953*** (0.0104)
More than college	-0.0933*** (0.0104)	-0.0950*** (0.0102)	-0.0953*** (0.0102)
Married	-0.0875*** (0.0062)	-0.0887*** (0.0062)	-0.0886*** (0.0062)
Mean	11.94	11.94	11.94
# of Observation	76655	76655	76655

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

Model 1 includes county fixed effect and year fixed effects
Model 2 includes state fixed effect and year fixed effects
Model 3 includes state, year fixed effects and state and linear time trend interaction

Table 10
The Effect of Abortion Access on the Probability of Unintended Births, 1993 -1997

	Model 1	Model 2	Model3
Number of Abortion Providers			
Number of providers per	0.0023	0.0007	0.0006
100,000 Women	(0.0027)	(0.0008)	(0.0008)
Number of non-hospital	0.0021	0.0005	0.0005
Providers per 100,000 Women	(0.0035)	(0.0013)	(0.0012)
Number of large non-hospital providers	0.0209	0.0027	0.0022
Per 100,000 Women	(0.0158)	(0.0033)	(0.0033)
Dummy for a county with at least	0.0011	0.0137*	0.0142*
an abortion provider	(0.0331)	(0.0077)	(0.0077)
Dummy for a county with more than	-0.0276	0.0122	0.0123
one abortion provider	(0.0575)	(0.0084)	(0.0084)
Dummy for a county with only one	0.0010	0.0180	0.0196*
abortion provider	(0.0331)	(0.0118)	(0.0019)
Dummy for a county with at least	-0.0247	0.0097	0.0100
a non-hospital provider	(0.0523)	(0.0078)	(0.0078)
Dummy for a county with at least	0.0106	0.0054	0.0052
a large non-hospital provider	(0.0623)	(0.0081)	(0.0081)
Dummy for a county with more than	-0.0246	0.0031	0.0031
one non-hospital abortion provider	(0.0522)	(0.0088)	(0.0088)
Dummy for a county with only one	0.0161	0.0214*	0.0224
Non-hospital abortion provider	(0.0081)	(0.0184)	(0.0109)
Distance Barriers to Abortion Providers			
Nearest Provider	0.0502	-0.0932	-0.1024
(Hospital or non-hospital)	(0.0455)	(0.0992)	(0.0993)
Nearest non-hospital provider	0.0361	0.0249	0.0113
	(0.0280)	(0.0266)	(0.0270)
Nearest large non-hospital provider	0.0259	0.0112	0.0036
	(0.0272)	(0.0250)	(0.0252)
Mean	45.01	45.01	45.01
# of Observations	76655	76655	76655

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

Model 1 includes county fixed effect and year fixed effects
Model 2 includes state fixed effect and year fixed effects
Model 3 includes state, year fixed effects and state and linear time trend interaction

Table 11
The Effect of Abortion Access on the Probability of Unwanted Births, 1993 -1997

	Model 1	Model 2	Model3
Number of Abortion Providers			
Number of providers per	0.0014	0.0002	0.0001
100,000 Women	(0.0015)	(0.0005)	(0.0005)
Number of non-hospital	0.0007	-0.0004	-0.0005
Providers per 100,000 Women	(0.0020)	(0.0008)	(0.0008)
Number of large non-hospital providers	0.0014	0.0040*	0.0041*
Per 100,000 Women	(0.0104)	(0.0022)	(0.0022)
Dummy for a county with at least	0.0043	0.0067	0.0066
an abortion provider	(0.0195)	(0.0051)	(0.0051)
Dummy for a county with more than	0.0276	0.0091	0.0089
one abortion provider	(0.0406)	(0.0057)	(0.0057)
Dummy for a county with only one	0.0043	-0.0000	-0.0001
abortion provider	(0.0194)	(0.0072)	(0.0073)
Dummy for a county with at least	-0.0067	0.0054	0.0053
a non-hospital provider	(0.0262)	(0.0052)	(0.0052)
Dummy for a county with at least	0.0176	0.0124**	0.0124**
a large non-hospital provider	(0.0242)	(0.0055)	(0.00550
Dummy for a county with more than	0.0023	0.0013	0.0009
one non-hospital abortion provider	(0.0394)	(0.0058)	(0.0058)
Dummy for a county with only one	-0.0066	0.0128*	0.0131*
Non-hospital abortion provider	(0.0262)	(0.0077)	(0.0077)
Distance Barriers to Abortion Providers			
Nearest Provider	0.0271	-0.0658	-0.0655
(Hospital or non-hospital)	(0.0232)	(0.0623)	(0.0622)
Nearest non-hospital provider	-0.0140	-0.0085	-0.0045
	(0.0188)	(0.0181)	(0.0186)
Nearest large non-hospital provider	-0.0081	-0.0086	-0.0065
	(0.0189)	(0.0175)	(0.0177)
Mean	11.94	11.94	11.94
# of Observations	76655	76655	76655

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

Model 1 includes county fixed effect and year fixed effects
Model 2 includes state fixed effect and year fixed effects
Model 3 includes state, year fixed effects and state and linear time trend interaction

Table 12
The Effect of Abortion Access on the Probability of Unintended Births, 1993 -1997

	Model 1	Model 2	Model3
Number of Abortion Providers			
Number of providers per 100,000 women	0.0014 (0.0028)	0.0000 (0.0012)	-0.0001 (0.0012)
Number of providers per 100,000 women * Low education	0.0008 (0.0016)	0.0011 (0.0016)	0.0011 (0.0016)
Number of non-hospital providers per 100,000 women	0.0017 (0.0038)	0.0003 (0.0017)	0.0003 (0.0017)
Number of non-hospital providers per 100,000 women *low education	0.0001 (0.0023)	0.0002 (0.0023)	0.0003 (0.0023)
Number of large non-hospital providers per 100,000 women	0.0152 (0.0162)	0.0015 (0.0047)	0.0012 (0.0047)
Above dummy * Low education	-0.0020 (0.0064)	0.0018 (0.0064)	0.0017 (0.0064)
Dummy for a county with at least an abortion provider	0.0010 (0.0338)	0.0046 (0.0103)	0.0051 (0.0103)
Above dummy * Low education	0.0055 (0.0125)	0.0147 (0.0126)	0.0144 (0.0126)
Dummy for a county with more than one abortion provider*	-0.0223 (0.0585)	0.0090 (0.0108)	0.0091 (0.0108)
Above dummy * Low education	-0.0016 (0.0123)	0.0051 (0.0124)	0.0049 (0.0124)
Dummy for a county with only one abortion provider	0.0040 (0.0334)	0.0168 (0.0120)	0.0182 (0.0121)
Dummy for a county with at least a non hospital provider	-0.0225 (0.0528)	0.0039 (0.0102)	0.0041 (0.0102)
Above dummy *Low education	0.0012 (0.0124)	0.0095 (0.0124)	0.0094 (0.0124)
Dummy for a county with at least a large non-hospital provider	0.0108 (0.0435)	0.0016 (0.0103)	0.0014 (0.0103)
Above dummy * Low education	-0.0029 (0.0637)	0.0074 (0.0124)	0.0074 (0.0124)
Dummy for a county with more than one non-hospital abortion provider	-0.0022 (0.0637)	0.0053 (0.0109)	0.0053 (0.0110)
Above dummy * Low education	-0.0094 (0.0124)	-0.0048 (0.0125)	-0.0049 (0.0125)
Dummy for a county with only one Non-hospital abortion provider	-0.0221 (0.0528)	0.0210 (0.0110)	0.0218** (0.0110)

Distance Barriers to Abortion Providers			
Nearest provider (Hospital or non-Hospital)	0.0455 (0.0545)	-0.1251 (0.1067)	-0.1330 (0.1069)
Nearest provider *Low education	-0.0214 (0.0475)	0.0237 (0.0984)	0.0215 (0.0990)
Nearest non-hospital provider	0.0396 (0.0280)	0.0270 (0.0266)	0.0142 (0.0271)
Nearest non-hospital provider * Low education	-0.0014 (0.0013)	-0.0011 (0.0013)	-0.0011 (0.0013)
Nearest large non-hospital provider	0.0303 0.0273	0.0142 0.0251	0.0070 0.0253
Nearest large non-hospital provider *Low education	-0.0012 (0.0013)	-0.0009 (0.0012)	-0.0010 (0.0012)
Mean	45.01	45.01	45.01
# of Observations	74544	74544	74544

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

Interaction terms are reported just below each abortion access measure.

Model 1 includes county fixed effect and year fixed effects
Model 2 includes state fixed effect and year fixed effects
Model 3 includes state, year fixed effects and state and linear time trend interaction

Table 13
The Effect of Abortion Access on the Probability of Unwanted Births, 1993 -1997

	Model 1*	Model 2	Model3
Number of Abortion Providers			
Number of providers per 100,000 women	-0.0006 (0.0016)	-0.0010 (0.0007)	-0.0010 (0.0007)
Number of providers per 100,000 women * Low education	0.0026** (0.0010)	0.0022** (0.0010)	0.0022** (0.0010)
Number of non-hospital providers per 100,000 women	-0.0007 (0.0021)	-0.0017* (0.0010)	-0.0018* (0.0010)
Number of non-hospital providers per 100,000 women *low education	0.0029** (0.0015)	0.0027* (0.0014)	0.0027* (0.0014)
Number of large non-hospital providers per 100,000 women	-0.0050 (0.0107)	-0.0003 (0.0028)	-0.0002 (0.0028)
Above dummy * Low education	0.0084** (0.0043)	0.0081* (0.0063)	0.0081* (0.0042)
Dummy for a county with at least an abortion provider	-0.0062 (0.0198)	-0.0036 (0.0063)	-000039 (0.0063)
Above dummy * Low education	0.0190** (0.0080)	0.0167** (0.0080)	0.0619** (0.0080)
Dummy for a county with more than one abortion provider*	0.0155 (0.0413)	-0.0008 (0.0067)	-0.0012 (0.0067)
Above dummy * Low education	0.0173** (0.0079)	0.0155** (0.0079)	0.0158** (0.0079)
Dummy for a county with only one abortion provider	0.0044 (0.0196)	0.0006 (0.0074)	0.0005 (0.0075)
Dummy for a county with at least a non hospital provider	-0.0119 (0.0267)	-0.0024 (0.0063)	-0.0027 (0.0063)
Above dummy *Low education	0.0147* (0.0080)	0.0126 (0.0078)	0.0129 (0.0079)
Dummy for a county with at least a large non-hospital provider	0.0080 (0.0246)	0.0033 (0.0065)	0.0030 (0.0065)
Above dummy * Low education	0.0181** (0.0080)	0.0162** (0.0080)	0.0165** (0.0080)
Dummy for a county with more than one non-hospital abortion provider	-0.0072 (0.0399)	-0.0079 (0.0069)	-0.0085 (0.0069)
Above dummy * Low education	0.0161** (0.0081)	0.0147* (0.0080)	0.0150* (0.0080)
Dummy for a county with only one Non-hospital abortion provider	-0.0043 (0.0081)	0.0127 (0.0078)	0.0129* (0.00780

Distance Barriers to Abortion Providers			
Nearest provider (Hospital or non-Hospital)	-0.0378 (0.0339)	-0.1000 (0.0667)	-0.0994 (0.0666)
Nearest provider *Low education	0.0772** (0.0326)	0.1242** (0.0623)	0.1260** (0.0622)
Nearest non-hospital provider	-0.0137 (0.0188)	-0.0085 (0.0181)	-0.0038 (0.0186)
Nearest non-hospital provider * Low education	-0.0016** (0.0008)	-0.0013* (0.0008)	-0.0013* (0.0008)
Nearest large non-hospital provider	-0.0073 (0.0180)	-0.0012 (0.0008)	-0.0055 (0.0178)
Nearest large non-hospital provider *Low education	-0.0013 (0.0008)	-0.0012 (0.0008)	-0.0012 (0.0008)
Mean	11.95	11.95	11.95
# of Observations	74554	74554	74554

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

Interaction terms are reported just below each abortion access measure.

Model 1 includes county fixed effect and year fixed effects
Model 2 includes state fixed effect and year fixed effects
Model 3 includes state, year fixed effects and state and linear time trend interaction

Table 14
The Effect of Abortion Access on the Probability of Unintended Births, 1993 -1997

	Model 1*	Model 2	Model3
Number of Abortion Providers			
Number of providers per 100,000 women	-0.0062** (0.0028)	-0.0026 (0.0016)	-0.0060*** (0.0011)
Number of providers per 100,000 women * Medicaid	0.0160*** (0.0015)	0.0077*** (0.0010)	0.0156*** (0.0014)
Number of non-hospital providers per 100,000 women	-0.0065* (0.0038)	-0.0035 (0.0021)	-0.0073*** (0.0015)
Number of non-hospital providers per 100,000 women *Medicaid	0.0211** (0.0021)	0.0105*** (0.0015)	0.0204*** (0.0021)
Number of large non-hospital providers per 100,000 women	0.0026 (0.0159)	-0.0092 (0.0105)	-0.0187*** (0.0041)
Above dummy * Medicaid	0.0500*** (0.0058)	0.0288*** (0.0043)	0.0505*** (0.0057)
Dummy for a county with at least an abortion provider	-0.0456 (0.0329)	-0.0215 (0.0191)	-0.0364*** (0.0087)
Above dummy * Medicaid	0.1146** (0.0094)	0.0567*** (0.0067)	0.1184*** (0.0092)
Dummy for a county with more than one abortion provider*	-0.0773 (0.0571)	0.0011 (0.0402)	-0.2297*** (0.0086)
Above dummy * Medicaid	0.1063** (0.0099)	0.0556*** (0.0071)	-0.0318*** (0.0093)
Dummy for a county with only one abortion provider	0.0012 (0.0334)	0.0018 (0.0190)	0.0163 (0.0119)
Dummy for a county with at least a non-hospital provider	-0.0608 (0.0515)	-0.0257 (0.0262)	-0.0353*** (0.0087)
Above dummy *Medicaid	0.1083*** (0.0096)	0.0572*** (0.0070)	0.1113*** (0.0099)
Dummy for a county with at least a large non-hospital provider	-0.0309 (0.0427)	-0.0379*** (0.0090)	-0.0382*** (0.0090)
Above dummy * Medicaid	0.1032*** (0.1012)	0.1081*** (0.0090)	0.1080*** (0.0105)
Dummy for a county with more than one non-hospital abortion provider*	-0.0556 (0.0630)	-0.0239 (0.0394)	-0.0358*** (0.0097)
Above dummy * Medicaid	0.1025*** (0.0104)	0.0563 (0.0054)	0.1032*** (0.0107)
Dummy for a county with only one non-hospital abortion provider	-0.0224 (0.0524)	-0.0054 (0.0263)	0.0211* (0.0109)

Distance Barriers to Abortion Providers			
Nearest provider (Hospital or non-hospital)	-0.0396 (0.0688)	-0.0356 (0.0370)	-0.2538** (0.1014)
Nearest provider *Medicaid	0.2769*** (0.0511)	0.2051*** (0.0397)	0.6985*** (0.0891)
Nearest non-hospital provider	0.0307 (0.0282)	-0.0119 (0.0188)	0.0214 (0.0268)
Nearest non-hospital provider *Medicaid	0.0169*** (0.0017)	0.0079*** (0.0012)	0.0154*** (0.0016)
Nearest large non-hospital provider	0.0192 (0.0274)	-0.0016 (0.0180)	0.0049 (0.0252)
Nearest large non-hospital provider *Medicaid	0.0171*** (0.0017)	0.0082*** (0.0012)	0.0155*** (0.0016)
Mean	44.70	44.70	44.70
# of Observations	75403	75403	75403

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

Interaction terms are reported just below each abortion access measure.

Model 1 includes county fixed effect and year fixed effects
Model 2 includes state fixed effect and year fixed effects
Model 3 includes state, year fixed effects and state and linear time trend interaction

Table 15
The Effect of Abortion Access on the Probability of Unwanted Births, 1993 -1997

	Model 1*	Model 2	Model3
Number of Abortion Providers			
Number of providers per 100,000 women	-0.0035 (0.0021)	-0.0060*** (0.0011)	-0.0031** (0.0006)
Number of providers per 100,000 women * Medicaid	0.0105*** (0.0015)	0.0156*** (0.0014)	0.0074*** (0.0010)
Number of non-hospital providers per 100,000 women	-0.0092 (0.0105)	-0.0073*** (0.0015)	-0.0046*** (0.0009)
Number of non-hospital providers per 100,000 women *Medicaid	0.0288*** (0.0043)	0.0203*** (0.0021)	0.0149*** (0.0014)
Number of large non-hospital providers per 100,000 women	-0.0125 (0.0191)	-0.0187*** (0.0041)	-0.0084*** (0.0025)
Above dummy * Medicaid	0.0567*** (0.0067)	0.0505*** (0.0057)	0.0292*** (0.0025)
Dummy for a county with at least an abortion provider	-0.0729 (0.0191)	-0.0364*** (0.0097)	-0.0163*** (0.0055)
Above dummy * Medicaid	0.0567*** (0.0067)	0.1184*** (0.0097)	0.0561*** (0.0069)
Dummy for a county with more than one abortion provider*	0.0012 (0.0402)	-0.0318*** (0.0093)	-0.0125*** (0.0062)
Above dummy * Medicaid	0.0556*** (0.0072)	0.1088*** (0.0102)	0.0549*** (0.0074)
Dummy for a county with only one abortion provider	0.0018 (0.0191)	0.0163 (0.0119)	-0.0003 (0.0073)
Dummy for a county with at least a non-hospital provider	-0.0257 (0.0262)	-0.0353*** (0.0087)	-0.0172*** (0.0073)
Above dummy *Medicaid	0.0572*** (0.0070)	0.1113*** (0.0087)	0.0564*** (0.0072)
Dummy for a county with at least a large non-hospital provider	-0.0091 (0.0246)	-0.0111* (0.0059)	-0.0114*** (0.0059)
Above dummy * Medicaid	0.0608*** (0.0076)	0.0607*** (0.0079)	0.0610*** (0.0078)
Dummy for a county with more than one non-hospital abortion provider*	-0.0239 (0.0394)	-0.0358*** (0.0097)	-0.0196*** (0.0063)
Above dummy * Medicaid	0.0563*** (0.0075)	0.1032*** (0.0107)	0.0549*** (0.0077)
Dummy for a county with only one non-hospital abortion provider	-0.0057 (0.0263)	0.0211* (0.0109)	0.0135* (0.0078)

Distance Barriers to Abortion Providers			
Nearest provider (Hospital or non-hospital)	-0.0357 (0.0370)	-0.2538** (0.1014)	-0.1620** (0.0634)
Nearest provider *Medicaid	0.3051*** (0.0397)	0.6986*** (0.0891)	0.4363*** (0.0674)
Nearest non-hospital provider	-0.0119 (0.0188)	0.0214 (0.0268)	-0.0099 (0.0183)
Nearest non-hospital provider *Medicaid	0.0079*** (0.0082)	0.0154** (0.0016)	0.0075** (0.0011)
Nearest large non-hospital provider	-0.0066 (0.0180)	0.0049 (0.0252)	-0.0095 (0.0069)
Nearest large non-hospital provider *Medicaid	0.0082*** (0.0012)	0.0155** (0.0016)	-0.0075*** (0.0011)
Mean	11.7	11.7	11.7
# of Observations	75403	75403	75403

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

Interaction terms are reported just below each abortion access measure.

Model 1 includes county fixed effect and year fixed effects
Model 2 includes state fixed effect and year fixed effects
Model 3 includes state, year fixed effects and state and linear time trend interaction

Table 16
The Effect of Abortion Access on Abortion Rate, 1988 –1997 for all states

	Model 1	Model2
Number of Abortion Providers		
Number of providers per 100,000 women	0.0525 (0.0959)	-0.0791 (0.1385)
Number of large providers per 100,000 women	-0.2370 (0.1717)	0.0046 (0.1584)
Number of non hospital providers per 100,000 women	-0.0353 (0.0627)	0.1106** (0.0491)
Number of large non-hospital providers per 100,000 women	0.0531 (0.0663)	0.1018** (0.0509)
Percent of women living in a county with at least a provider	-0.0063* (0.0032)	-0.0044 (0.0060)
Percent of women living in a county with at least a large provider	-0.0015 (0.0040)	-0.0033 (0.0034)
Percent of women living in a county with at least a non-hospital provider	-0.0043 (0.0032)	0.0037* (0.0027)
Percent of women living in a county with at least a large non-hospital provider	-0.0003 (0.0018)	-0.0015 (0.0014)
Distance Barriers to Abortion Providers		
Mean distance to a nearest provider	0.0026 (0.0023)	0.0008 (0.0016)
Mean distance to a nearest non-hospital Provider	0.0012 (0.0017)	-0.0001 (0.0012)
Mean distance to a dearest non-hospital large provider	-0.0009 (0.0009)	-0.0005 (0.0009)
Mean	2.96	2.96
# of Observations	501	500

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

* Model 1 includes state fixed effect and year fixed effects. The dependant variables are the natural logarithms of the state-level abortion rate. The independent variables indicating the number of abortion provider are also the natural logarithms. The rest of the variables are the level.
** Model 2 further includes state-specific linear time trends. The dependant variables are the natural logarithms of the state-level abortion rate. The independent variables indicating the number of abortion provider are also the natural logarithms. The rest of the variables are the level.

Table 17
The Effect of Abortion Access on Abortion Rate, 1993 –1997 for all states

	Model 1	Model2
Number of Abortion Providers		
Number of providers per 100,000 women	-0.0722 (0.0180)	-0.3555 (0.3994)
Number of large providers per 100,000 women	-0.2409 (0.3077)	-0.2953 (0.5506)
Number of non hospital providers per 100,000 women	0.0463 (0.0887)	0.1274 (0.1083)
Number of large non-hospital providers per 100,000 women	0.1071 (0.0745)	0.2038* (0.1170)
Percent of women living in a county with at least a provider	-0.0106 (0.0094)	-0.0202 (0.0241)
Percent of women living in a county with at least a large provider	-0.0018 (0.0065)	-0.0145 (0.0147)
Percent of women living in a county with at least a non-hospital provider	-0.0024 (0.0060)	0.0040 (0.0113)
Percent of women living in a county with at least a large non-hospital provider	-0.0012 (0.0019)	-0.0331 (0.0308)
Distance Barriers to Abortion Providers		
Mean distance to a nearest provider	0.0005 (0.0031)	0.0028 (0.0068)
Mean distance to a nearest non-hospital Provider	-0.0003 (0.0020)	0.0007 (0.0035)
Mean distance to a dearest non-hospital large provider	0.0011 (0.0012)	0.0015 (0.0020)
Mean	2.89	2.89
# of Observations	251	251

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

* Model 1 includes state fixed effect and year fixed effects. The dependant variables are the natural logarithms of the state-level abortion rate. The independent variables indicating the number of abortion provider are also the natural logarithms. The rest of the variables are the level.
** Model 2 further includes state specific linear time trends. The dependant variables are the natural logarithms of the state-level abortion rate. The independent variables indicating the number of abortion provider are also the natural logarithms. The rest of the variables are the level.

Table 18
The Effect of Abortion Access on Abortion Rate, 1993 -1997 for PRAMS 10 states

	Model 1	Model2
Number of Abortion Providers		
Number of providers per 100,000 women	0.2539** (0.0962)	-0.0168 (0.0332)
Number of large providers per 100,000 women	0.1940 (0.1681)	0.0015 (0.0834)
Number of non hospital providers per 100,000 women	0.1527 (0.1018)	0.0207 (0.0973)
Number of large non-hospital providers per 100,000 women	0.3510** (0.1714)	0.1299* (0.0739)
Percent of women living in a county with at least a provider	-0.0017 (0.0059)	0.0019 (0.0067)
Percent of women living in a county with at least a large provider	-0.0115 (0.0100)	-0.0048 (0.0031)
Percent of women living in a county with at least a non-hospital provider	-0.0135 (0.0099)	0.0035 (0.0101)
Percent of women living in a county with at least a large non-hospital provider	-0.0029 (0.0092)	-0.0025 (0.0030)
Distance Barriers to Abortion Providers		
Mean distance to a nearest provider	-0.0019 (0.0011)	-0.0013 (0.0024)
Mean distance to a nearest non-hospital Provider	-0.0017 (0.0011)	-0.0015 (0.0024)
Mean distance to a nearest non-hospital large provider	0.0008 (0.0024)	0.0006 (0.0018)
Mean	2.80	2.80
# of Observations	50	50

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

* Model 1 includes state fixed effect and year fixed effects. The dependant variables are the natural logarithms of the state-level abortion rate. The independent variables indicating the number of abortion provider are also the natural logarithms. The rest of the variables are the level.
** Model 2 further includes state specific linear time trends. The dependant variables are the natural logarithms of the state-level abortion rate. The independent variables indicating the number of abortion provider are also the natural logarithms. The rest of the variables are the level.

Table 19
The Effect of Abortion Rate on the Probability of Unintended Births, 1993 –1997

	Model 1	Model2
Abortion rate	0.0023	0.0002
	(0.0030)	(0.0005)
Race		
White	-0.1453***	-0.1470***
	(0.0075)	(0.0072)
Other	-0.0918***	-0.0924***
	(0.0153)	(0.0080)
Age		
20-24 years	-0.0088	-0.0087
	(0.0083)	(0.0083)
35-30 years	-0.1349***	-0.0936***
	(0.0126)	(0.0080)
30-34 years	-0.1291***	-0.1300***
	(0.0095)	(0.0095)
Over 35 years	-0.1349***	0.1360***
	(0.0154)	(0.0125)
Educational Attainment		
High school dropout	0.0488***	0.0518***
	(0.0154)	(0.0152)
High school diploma	0.0105	0.0125
	(0.0142)	(0.0140)
Some College	0.0159	0.0176
	(0.0148)	(0.0147)
College and more	-0.0805***	-0.0792***
	(0.0016)	(0.0140)
Married	-0.2473***	-0.2465***
	(0.0082)	(0.0082)
Mean	45.16	45.16
# of Observations	79513	79513

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

* Model 1 includes state fixed effect and year fixed effects, along with individual characteristic variables such as race, education, age, and marital status.
** Model 2 excludes state and year fixed effects.

Table 20
The Effect of Abortion Rate on the Probability of Unwanted Births, 1993 –1997

	Model 1	Model2
Abortion rate	-0.0020 0.0020	0.0001 0.0003
Race		
White	-0.1065*** (0.0060)	-0.1073*** (0.0058)
Other	-0.0972*** (0.0100)	-0.1000*** (0.0093)
Age		
20-24 years	0.0114** (0.0054)	0.0114** (0.0054)
25-30 years	0.0336*** (0.0051)	0.0336*** (0.0051)
30-34 years	0.0674*** (0.0061)	0.0673*** (0.0061)
Over 35 years	0.1597*** (0.0098)	0.1595*** (0.0098)
Educational Attainment		
High school dropout	0.0096 (0.0113)	0.0136 (0.0113)
High school diploma	-0.0293*** (0.0102)	-0.0258** (0.0101)
Some College	0.0520*** (0.0104)	-0.0488*** (0.0103)
College and more	-0.0103*** (0.0103)	-0.1002*** (0.0102)
Married	-0.0889*** (0.0061)	-0.0884*** (0.0060)
Mean	12.13	12.13
# of Observations	79213	79213

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

* Model 1 includes state fixed effect and year fixed effects, along with individual characteristic variables such as race, education, age, and marital status.
** Model 2 excludes state and year fixed effects

Table 21
The Effect of Abortion Rate on the Probability of Unintended Births, 1993 –1997
Up to 12 years Educational Attainment

	Model 1	Model2
Abortion rate	-0.0020	-0.0002
	(0.0041)	(0.0007)
Race		
White	-0.1431***	-0.1450***
	(0.0097)	(0.0091)
Other	-0.0877***	-0.0884***
	(0.0221)	(0.0199)
Age		
20-24 years	-0.0525***	-0.0525***
	(0.0100)	(0.0100)
35-30 years	-0.1086***	-0.1087***
	(0.0105)	(0.0105)
30-34 years	-0.1486***	-0.1485***
	(0.0143)	(0.0143)
Over 35 years	-0.1333***	-0.1325***
	(0.0192)	(0.0192)
Educational Attainment		
High school dropout	0.0539***	0.0547***
	(0.0185)	(0.0184)
High school diploma	0.0132	0.0130
	(0.0178)	(0.0178)
Married	-0.2178***	-0.2167***
	(0.0101)	(0.0101)
Mean	53.45	53.45
# of Observation	45818	45818

*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

* Model 1 includes state fixed effect and year fixed effects, along with individual characteristic variables such as race, education, age, and marital status.
** Model 2 excludes state and year fixed effects

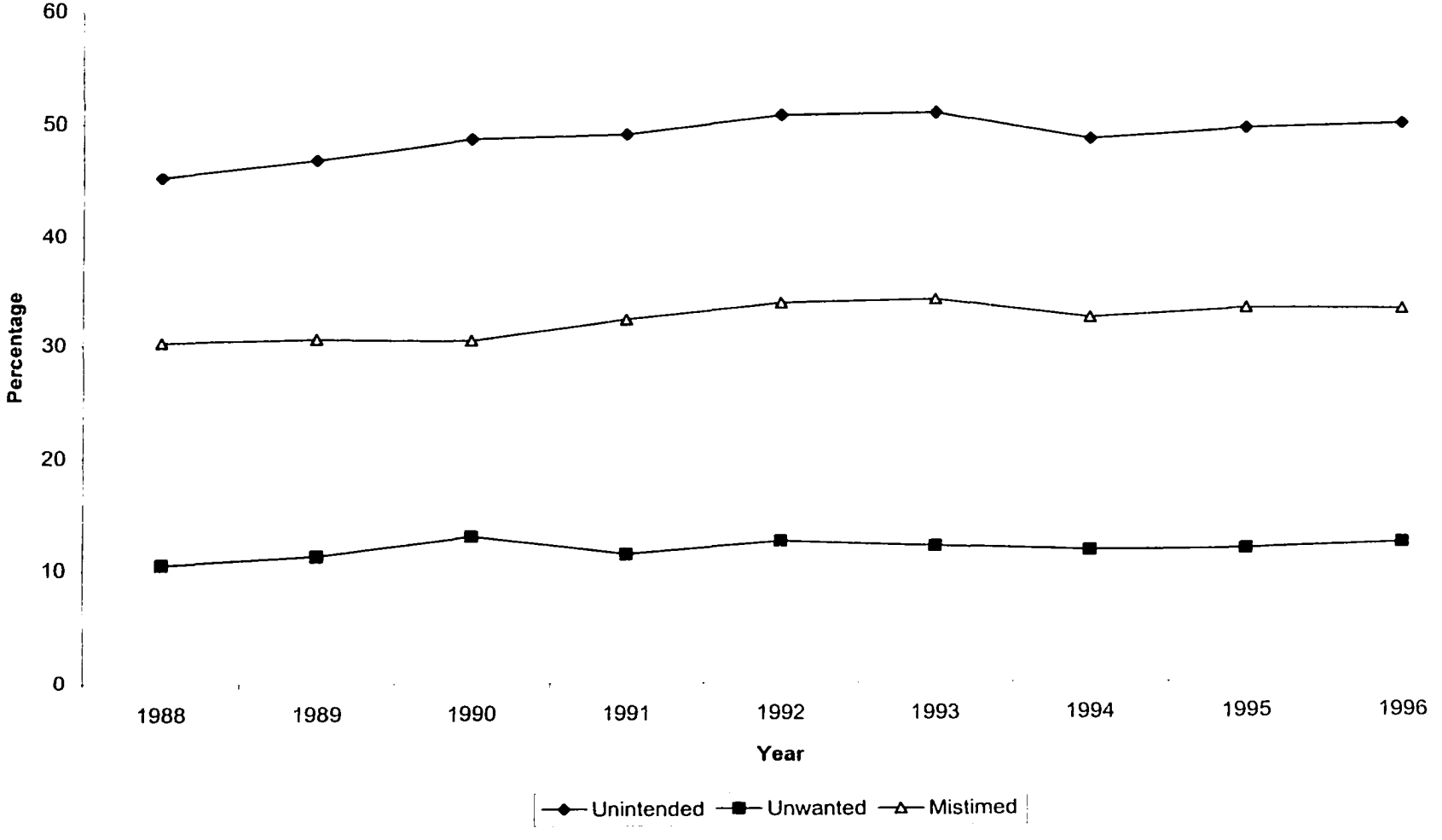
Table 22
The Effect of Abortion Rate on the Probability of Unwanted Births, 1993 –1997
Up to 12 years Educational Attainment

	Model 1	Model2
Abortion rate	-0.0061**	0.0001
	(0.0029)	(0.0005)
Race		
White	-0.1237***	-0.1244***
	(0.0081)	(0.0077)
Other	-0.1074***	-0.1093***
	(0.0165)	(0.0133)
Age		
20-24 years	0.0259***	0.0260***
	(0.0069)	(0.0069)
35-30 years	0.0481***	0.0486***
	(0.0072)	(0.0072)
30-34 years	0.0931***	0.0936***
	(0.0102)	(0.0103)
Over 35 years	0.2137***	0.2151***
	(0.0167)	(0.0167)
Educational Attainment		
High school dropout	-0.0088	-0.0083
	(0.0132)	(0.0139)
High school diploma	-0.0548***	-0.0550***
	(0.0132)	(0.0132)
Married	-0.0880***	-0.0879***
	(0.0076)	(0.0075)
Mean	15.38	15.38
# of Observation	45818	45818

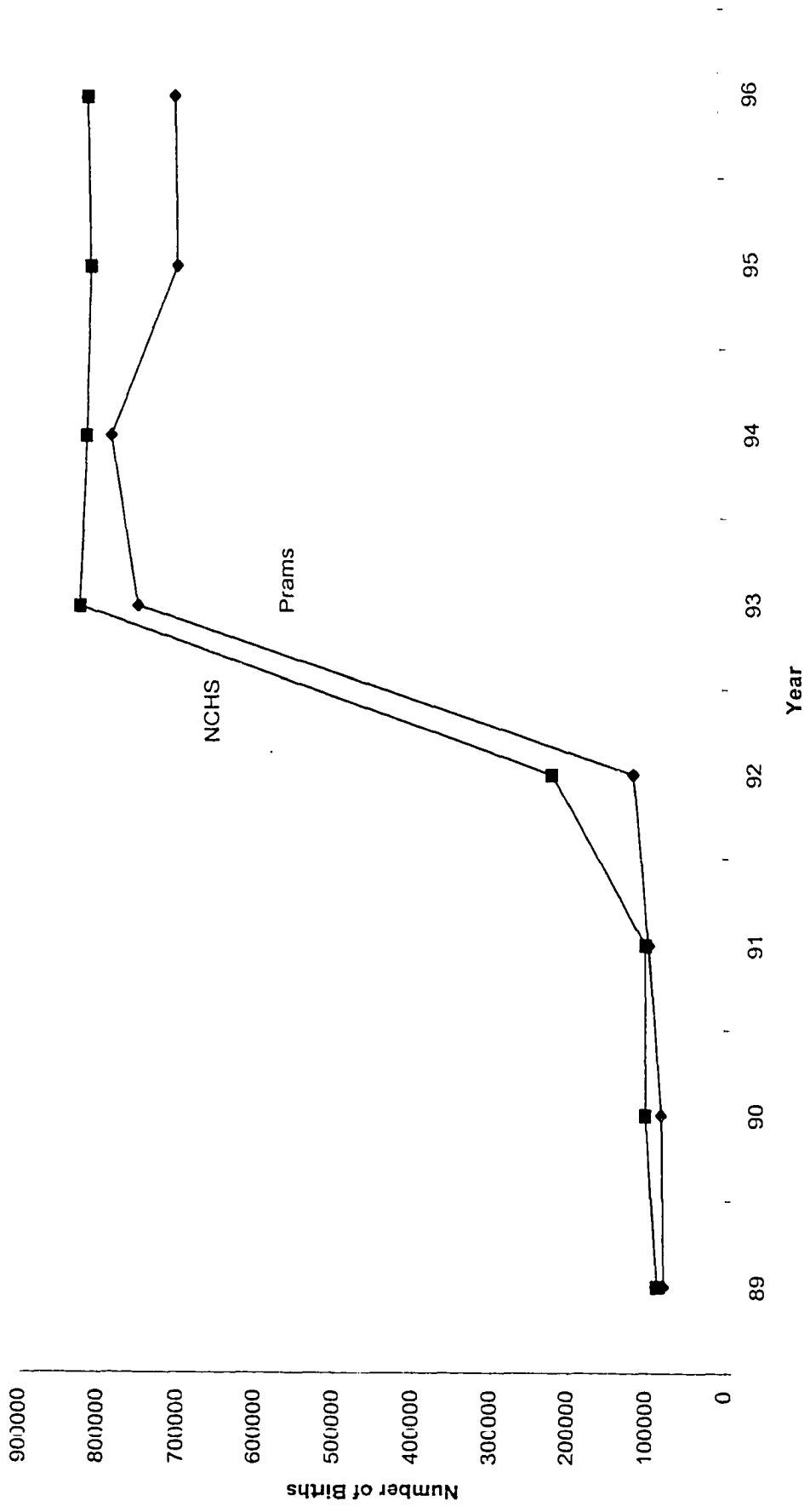
*** 0 < p-value < 0.01 ** 0.01< p-value <0.05 * 0.00< p-value < 0.01
Robust standard errors are reported in parentheses.

* Model 1 includes state fixed effect and year fixed effects, along with individual characteristic variables such as race, education, age, and marital status.
** Model 2 excludes state and year fixed effects

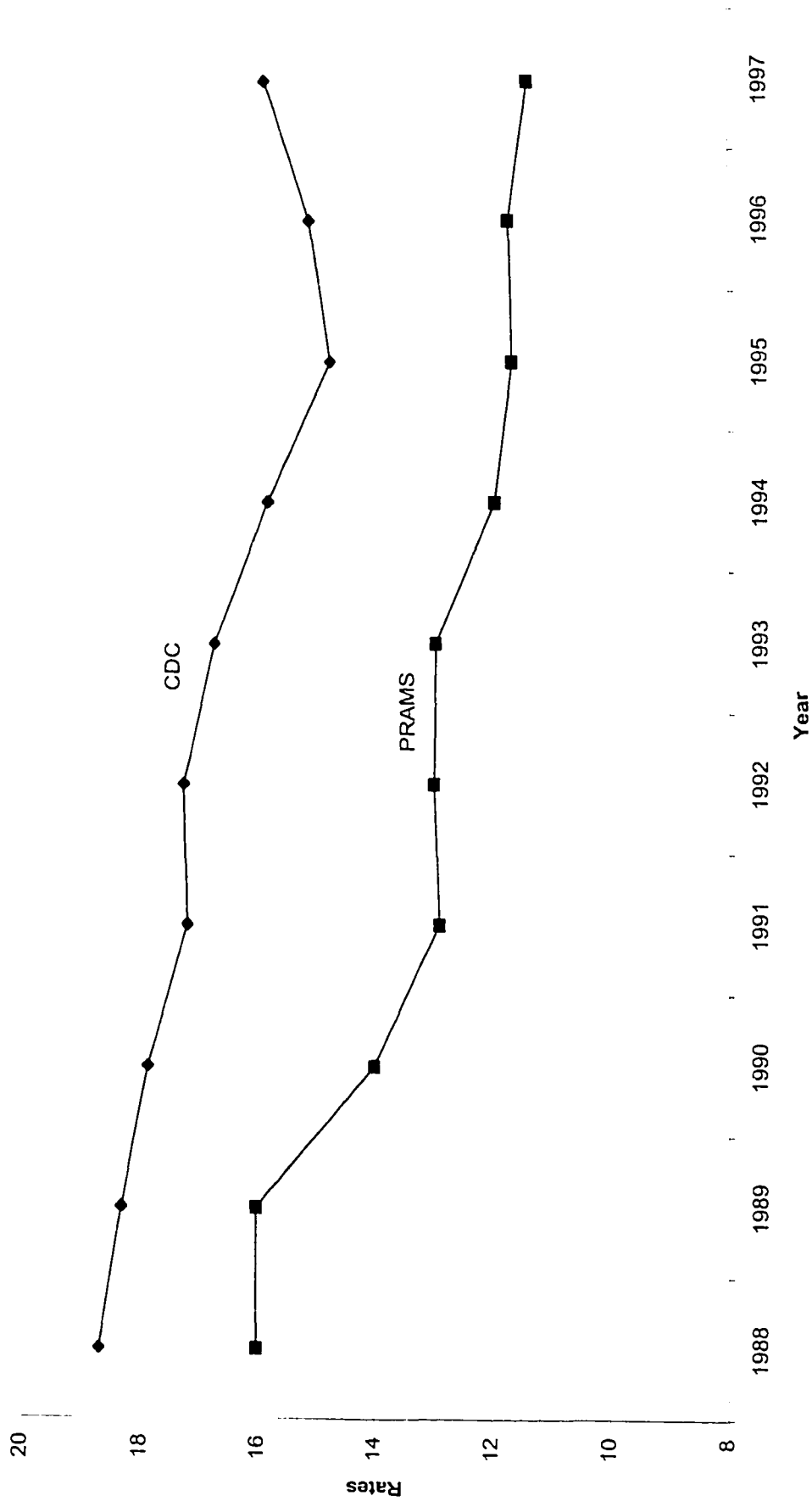
Graph 1
Time Trend of Proportion of Unintended, Unwanted and Mistimed Births out of Total Births

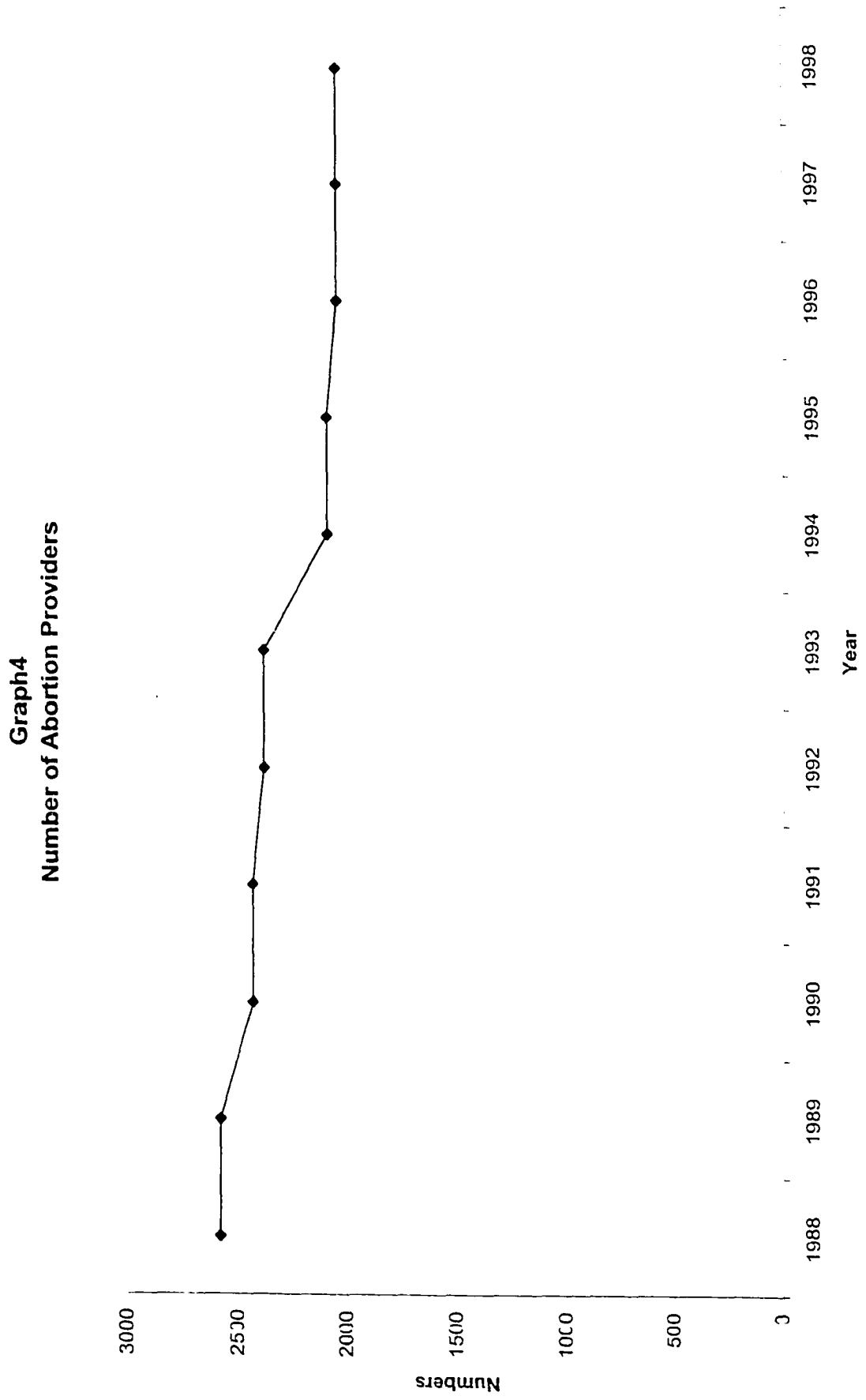


Graph 2
NCH and Prams total Births

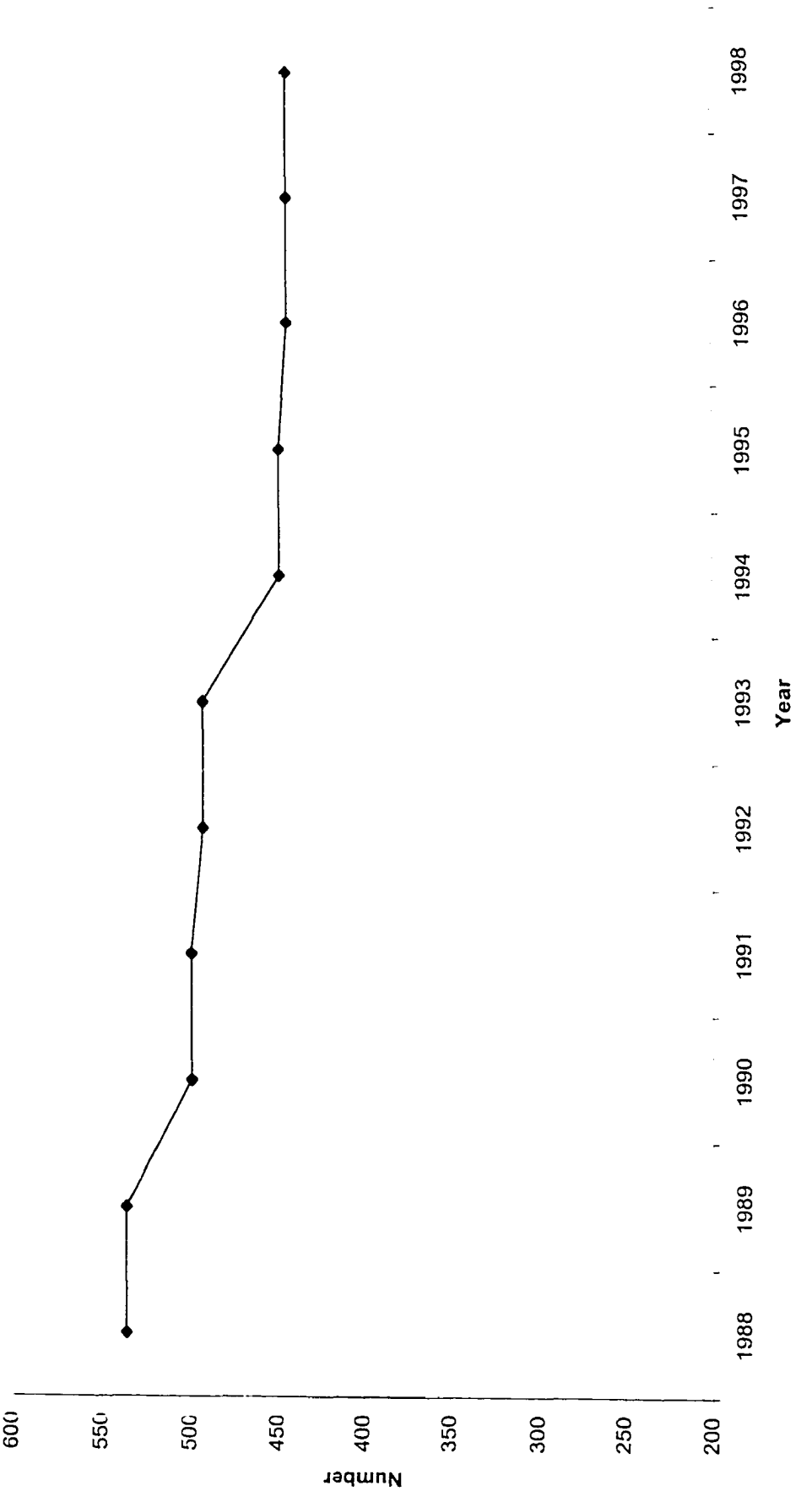


Graph 3
Abortion rates

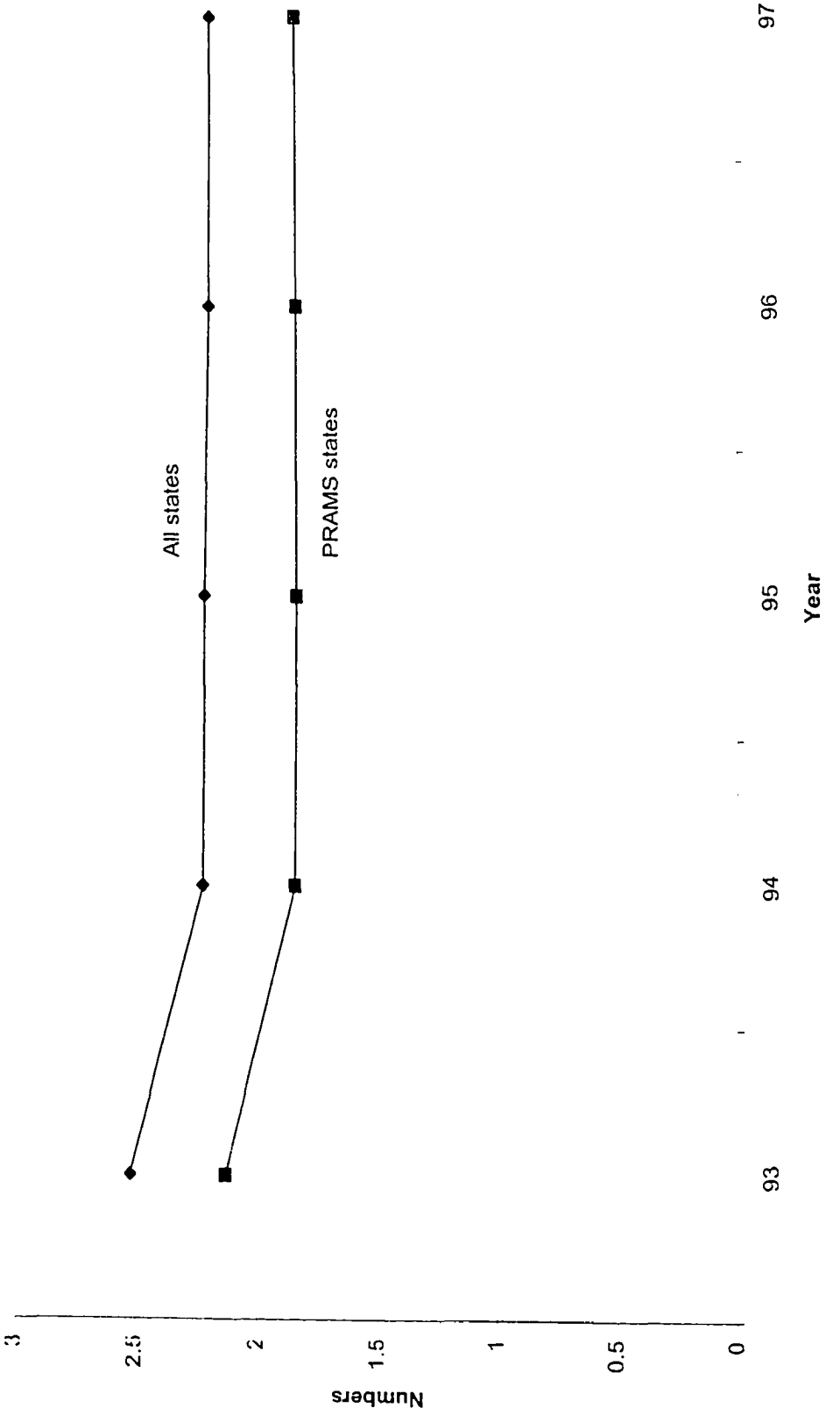




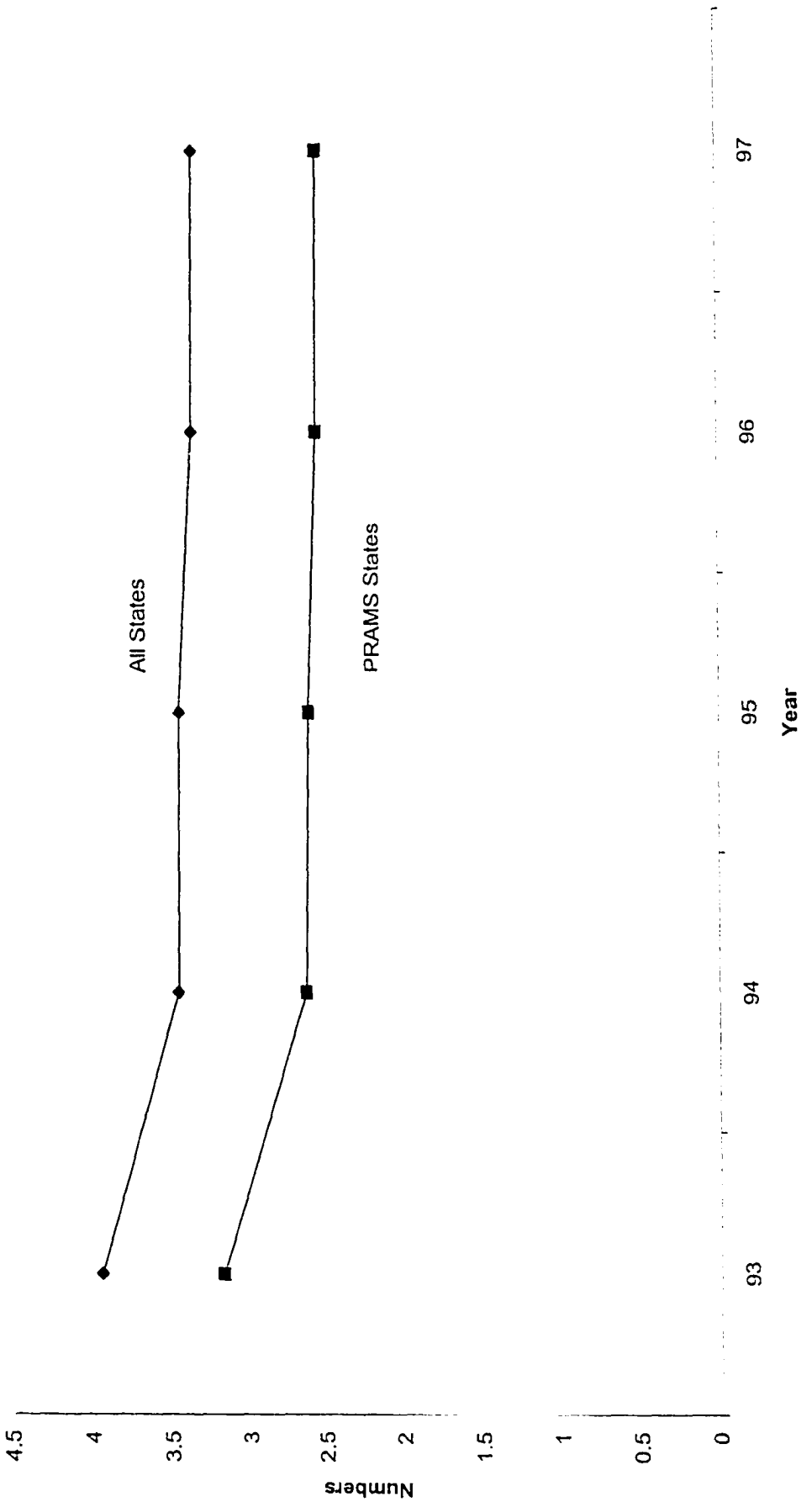
Graph 5
Proportion of Women Living in a County with at least one Provider



Graph 6
Number of Non Hospital Abortion Providers per 100,000 Women



Graph 7
Number of Abortion Providers per 100,000 Women



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