Determinants of Teenage Childbearing in the United States

by

Poh Lin Tan

Public Policy Studies
Duke University

Date:_______________________
Approved:

____
Philip J. Cook, Supervisor

____
M. Giovanna Merli

____
S. Philip Morgan

____
Seth G. Sanders

Dissertation submitted in partial fulfillment of
the requirements for the degree of
Doctor of Philosophy in Public Policy Studies
in the Graduate School
of Duke University

2015

ABSTRACT

Determinants of Teenage Childbearing in the United States

by

Poh Lin Tan

Public Policy Studies
Duke University

Date:_______________________
Approved:

Philip J. Cook, Supervisor

M. Giovanna Merli

S. Philip Morgan

Seth G. Sanders

An abstract of a dissertation submitted in partial
fulfillment of the requirements for the degree
of Doctor of Philosophy in Public Policy Studies
in the Graduate School of
Duke University

2015

Abstract

This dissertation consists of two original empirical studies on the determinants of teenage childbearing in the United States. The first study examines the impact of educational attainment on teenage childbearing, using school entry laws as an instrument for education and a highly detailed North Carolina administrative dataset that links birth certificate data to school administrative records. I show that being born after the school entry cutoff date affects educational success in offsetting ways, with a negative impact on years of education but positive impact on test scores. Using an IV regression strategy to distinguish the impacts of years of education and test scores, I show that both educational measures have negative impacts on teenage childbearing.

The second study examines potential causes of the decline in the U.S. teenage birth rate between 1991 and 2010. Using age-period-cohort models with Vital Statistics birth data and Census population counts, I show that the decline was driven by period changes in the early 1990s but by cohort changes between the mid-1990s and mid-2000s. I also use a difference-in-differences model to investigate the extent to which social policies in the 1970s-1980s can explain these cohort changes. The evidence suggests that while legalization of abortion for adult women and unilateral divorce laws had a significant impact on teenage birth rates in the 1990s-2000s, abortion legalization is unlikely to be a major explanation for the observed decline.

Dedication

This dissertation is dedicated in praise to my Creator for His faithfulness and loving-kindness, who freely gives every spiritual gift and daily renews His never-ending mercies. For You I do my best.

Contents

List of Tables

List of Figures

Acknowledgements

I would like to thank my committee for their excellent mentorship and guidance. I especially thank Philip J. Cook, whose patience, kindness, wisdom and unerring research instincts make for more than anyone can ask for in an advisor. I also thank M. Giovanna Merli, S. Philip Morgan and Seth G. Sanders, who continually inspire me with their ability to combine top scholarship with dedication to teaching and mentorship.

I am grateful to the Horowitz Foundation for Social Policy for funding this research. I would also like to thank the NC Department of Education and NC Department of Health and Human Services for permission to use their administrative data, and the NC Education Research Data Center, particularly Clara Muschkin and Kara Bonneau, for valuable help with accessing these data.

1. Demographic, Economic and Sociological Perspectives on Teenage Childbearing

This chapter provides an overview of recent trends in the U.S. teenage birth rate in the past two decades. It discusses the determinants of teenage childbearing from the perspectives of three disciplines: demography, which studies the biological and social proximate determinants of fertility; economics, which focuses on the costs and benefits of teenage childbearing within a rational decision making framework; and sociology, which pays particular attention to central key risk factors, including the intergenerational transmission of family formation. It explores two rationales for why the teenage birth rate should be further reduced. First, teenage childbearing is thought to have negative consequences for the mother, the child and the rest of society. While an influential body of literature has shown that the economic consequences for teenage mothers are likely to be small, there is some evidence that young motherhood may reduce women's marital prospects, increase children's exposure to neglect or abuse, and affect behavioral and socio-emotional outcomes in the next generation, particularly the fertility outcomes of teenage daughters. Second, most teenage mothers themselves state that they would have preferred a different life course at the time of pregnancy, but may be less able than older women to avoid childbearing due to lower cognitive, psychosocial and other resources. The chapter concludes with an overview of the impact of public policies on adolescent fertility behavior.

1.1 Recent Trends in Teenage Childbearing

Around 6.2 million or 10% of women aged 15-44 become pregnant every year in the United States. Of these pregnancies, around one million end in miscarriages or stillbirths; another 1.2 million end in abortion; and the remaining four million end in live births (Mosher 2010). (See Appendix A1 for a discussion of how these figures are estimated.) Pregnancy rates are highest among women in their 20s: around 16% of women aged 20-24 and 17% of women aged 25-29 were pregnant in 2008. Women aged 15-19 are at roughly half this risk of pregnancy, with only 7% pregnant in the same year. Moreover, only around 58% of teenage pregnancies end in a live birth, compared to around 65% among women aged 15-44. This difference is due to higher abortion rates among teenage pregnancies (25%, compared to 19% among women aged 15-44), whereas the proportion of pregnancies ending in involuntary fetal loss is around 17% in both age groups (Ventura, Curtin and Abma 2012).

The U.S. teenage birth rate is the highest in the developed world, around 2.5 times that of Canada, 4 times that of Germany or Norway, and almost 10 times that of Switzerland (Kearney and Levine 2012a). Within the U.S., there is substantial variation: the risk of teenage childbearing in Mississippi is 4 times that of New Hampshire (Kearney and Levine 2012a). This variation is partly due to demographic differences, since black and Hispanic teenagers are at much higher risk of childbearing (around 6% and 7% respectively in 2008, compared to 4% among all teenagers) (Ventura, Curtin and

Abma 2012), and partly due to other socioeconomic differences, including family background and access to educational opportunities. (See Appendix A1 for a discussion of how race and ethnicity are classified.) In a study on high school sophomores, Abrahamse, Morrison and Waite (1988) find that only 0.1% of white women from intact high-socioeconomic families give birth within two years, compared to 25% of black women from single-parent low-socioeconomic families. (Their study, which used data from the High School and Beyond Panel study, exclude women who dropped out of school prior to or during the study.)

Figure 1 shows that the teenage birth rate has fallen during the past two decades, from around 60 per 1,000 women in 1990 to around 50 in 2000 and 40 in 2005 (dash-and-dot line in red). The decline was most dramatic among non-Hispanic blacks (dotted line in purple): between 1993 and 2005, the teenage birth rate for this group fell by almost half, from 110 to 60 per 1,000 women. Since the Great Recession, the teenage birth rate has reached new historic lows at around 31.3 per 1,000 women in 2011 (Hamilton, Martin and Ventura 2012).

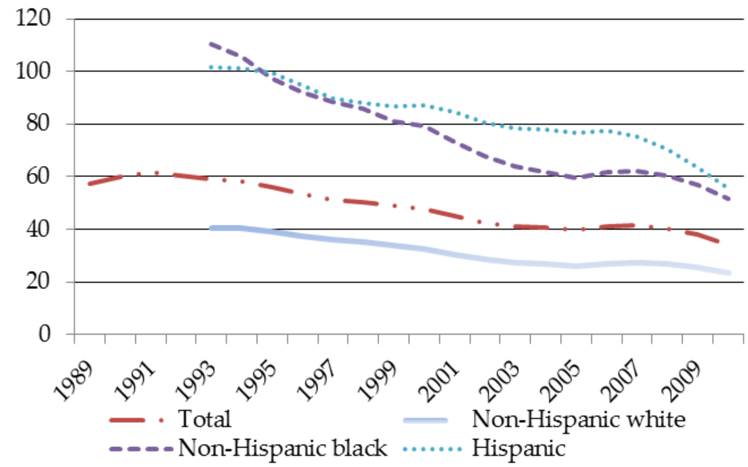


Figure 1: Number of births per 1,000 women aged 15-19, 1989-2010

Notes: The data are obtained from Vital Statistics. Race and ethnicity-specific data are available from 1993, the first year in which all 50 states provided data on ethnicity.

The decline in teenage birth rates can be found not only across all racial and ethnic groups, but also among both younger teenagers (ages 15 to 17) and older teenagers (ages 18 to 19), which fell from 77.1 to 39.5 and from 167.7 to 114.2 respectively (Ventura, Curtin and Abma 2012). Figure 2 shows that teenage birth rates are substantially higher among older teenagers (dotted line in green), especially among non-Hispanic blacks and Hispanics. Hence, one potential explanation for the decline in teenage birth rates is that the fraction of teenagers aged 15-17 may have increased during this period. Appendix A2 compares teenage birth rates, also shown in Figure 1, to teenage *fertility* rates, which assign equal weight to all age-specific birth rates and are hence unaffected by changes in age composition. The evidence suggests that age compositional changes explain very little of the observed trends, except for between 1991 and 1994 (Kearney and Levine 2012b).

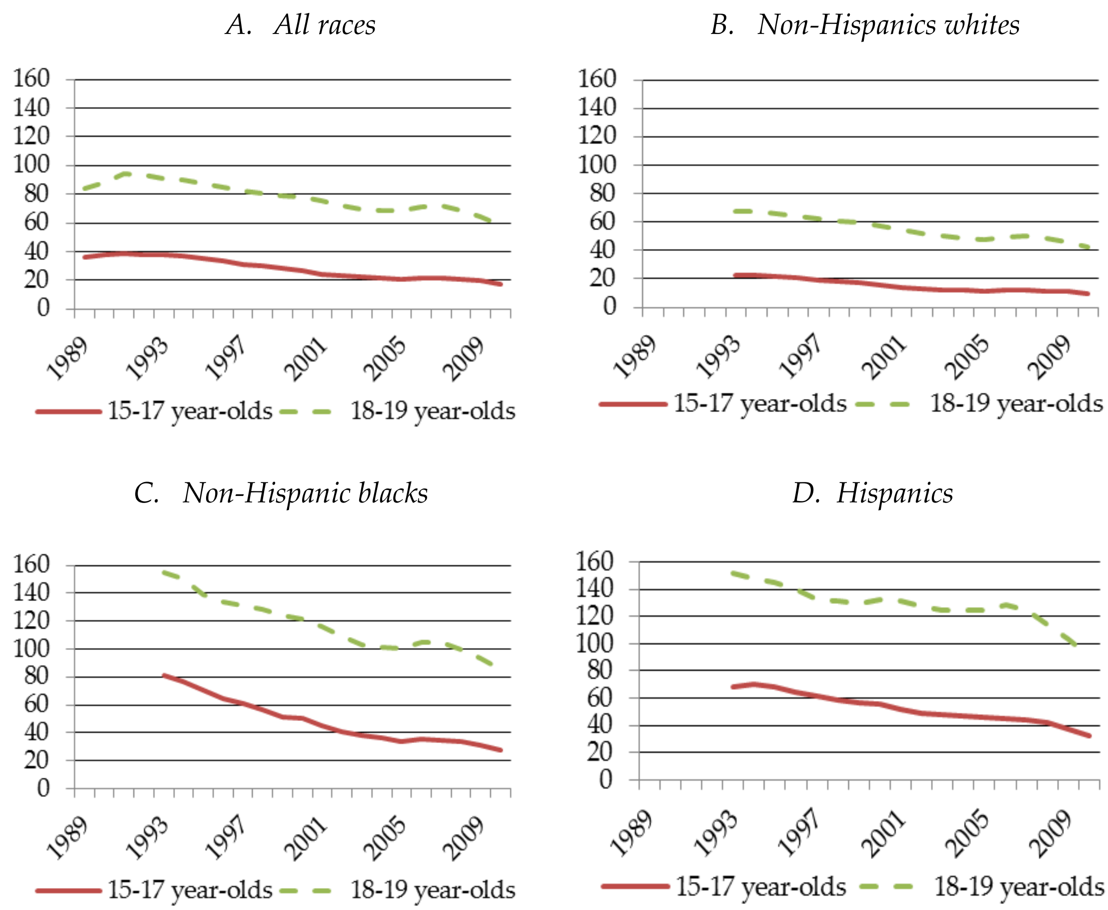


Figure 2: Number of births per 1,000 women aged 15-17 and 18-19, 1989-2010

Notes: The data are obtained from Vital Statistics. Race and ethnicity-specific data are available from 1993, the first year in which all 50 states provided data on ethnicity.

Finally, Figure 3 shows that there is a comparable decline in birth rates among women aged 20-24 (dashed line in red), whereas pregnancy and birth rates among older women have been rising rather than falling (Ventura, Curtin and Abma 2012). Among non-Hispanic whites, birth rates among 20-24 year-olds fell only in the 2000s; among Hispanics, birth rates for 20-24 year-olds fell only in the 1990s until the Great Recession in 2008; and among non-Hispanic blacks, birth rates for 20-24 year-olds *rose* in the late 1990s, possibly due to delayed childbearing among teenagers in the early 1990s.

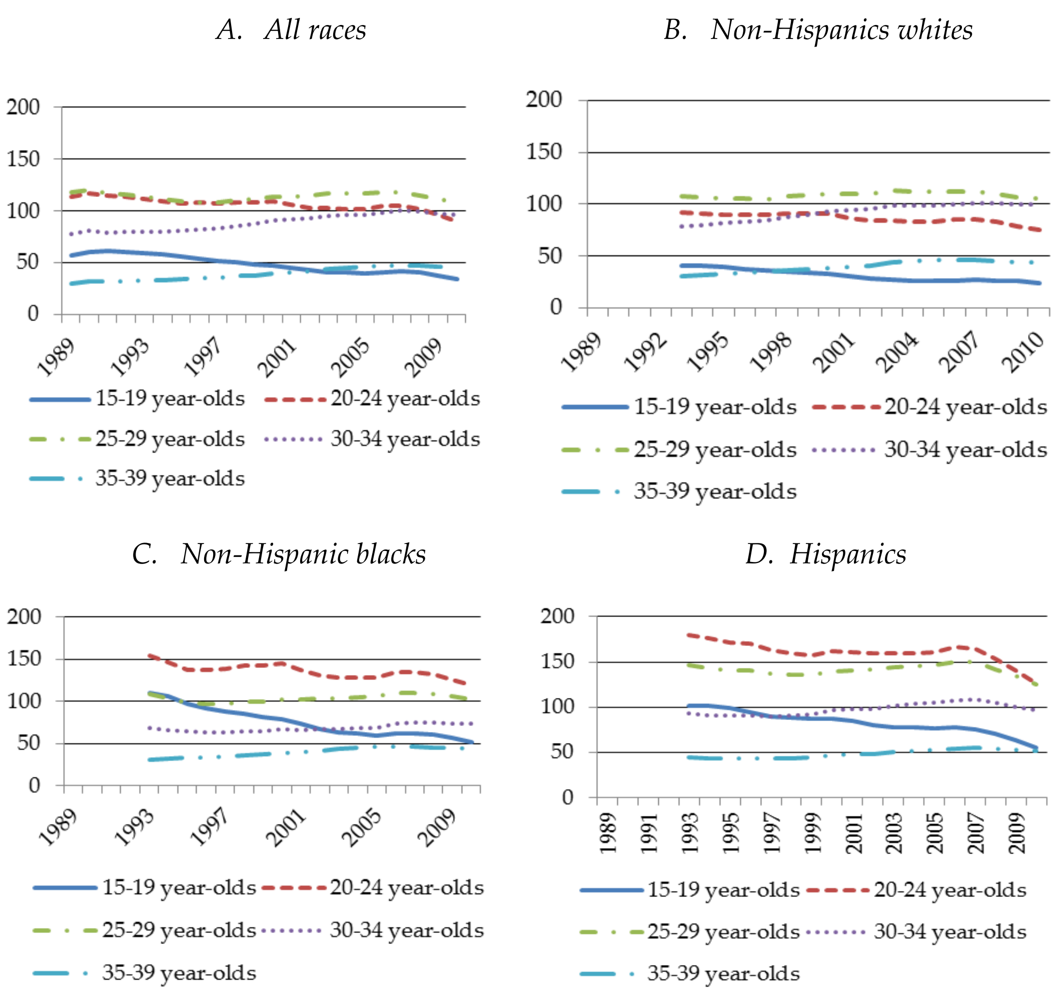


Figure 3: Number of births per 1,000 women, 1989-2010

Notes: The data are obtained from Vital Statistics. Race and ethnicity-specific data are available from 1993, the first year in which all 50 states provided data on ethnicity.

1.2 Determinants of Teenage Fertility

1.2.1 Bio-Demographic Perspectives: Proximate Determinants

Teenage pregnancy rates can be modeled as a function of fecundability and contraceptive use. Fecundability refers to the probability of conception within a month of unprotected intercourse among women who are not pregnant, sterile or amenorrheic, estimated to be typically around 0.2. This probability can be decomposed as the product of a) the probability that a menstrual cycle is ovulatory (around 0.95), b) the probability that coital activity occurs during the fertile period, a window of around two days around the time of ovulation, c) the probability that the ovum is successfully fertilized by a sperm (around 0.95), and d) the probability that the fertilized ovum is successfully implanted and survives past the first missed menses (around 0.5) (Bongaarts and Potter 1983). Hence, fecundability depends on both sexual activity and reproductive physiology,[1] and peaks in the early twenties, after which coital frequency decreases while the probability of fetal loss increases with age (Weinstein et al. 1990). Among teenagers, around 70% of teenagers have sex before age 20 (Guttmacher Institute 2013), and in 2006-2010, around half of these sexually experienced teenagers were sexually active, i.e. had intercourse in the last three months (Kearney and Levine 2012b). Finally,

---

[1] Both sexual activity and reproductive physiological factors may depend on age at menarche, a marker of female reproductive maturity which usually occurs between ages 12-14 in developed countries, which in turn depends on early health – for instance, it is positively correlated with birth weight but negatively correlated with weight gain up to the age of eight (Sloboda et al. 2007). However, there is mixed evidence as to whether age at menarche is associated with probability of teenage motherhood (Hardy et al. 1998; Meade, Kershaw and Ickovics 2008; Kahn and Anderson 1992).

contraceptive use refers to the use of any contraceptive method(s) and the effectiveness of the method(s). Over 85% of teenagers at risk of pregnancy use some form of contraception (Guttmacher Institute 2013; Mosher 2010); over 96% of sexually experienced teenagers have ever used condoms, and another 57% and 56% have also ever used withdrawal and the birth control pill (Guttmacher Institute 2013). Nevertheless, these rates are low compared to European countries, resulting in a relatively high teenage pregnancy rate despite similar levels of teenage sexual activity in both regions (Guttmacher Institute 2013).

Next, the probability that a teenage pregnancy ends in a live birth depends on the probabilities of involuntary fetal loss and induced abortion. Compared to pregnancies to women in their 20s, teenage pregnancies are more slightly likely to end in fetal loss (17%, compared to 14%) or in abortion (25%, compared to 24% and 17% for pregnancies to 20-24 and 25-29 year-olds respectively) (Ventura, Curtin and Abma 2012). Hence, the primary reason why teenage women have lower birth rates than women in their 20s is that they are only half as likely to become pregnant, due to lower physiological fecundability and potentially lower levels of sexual activity, and a secondary reason is that pregnant teenagers are more likely to miscarry or to seek abortions.

Which changes in proximate determinants account for the decline in teenage birth rates in the past two decades? Increased abortion rates is *not* part of the answer – in fact, the proportion of teenage pregnancies which end in abortion fell from 35% to 26% between 1990 and 2008, while the proportion which end in live births rose from 51% to 58%. (These figures suggest that the proportion of teenage pregnancies which end in involuntary fetal loss increased from 14% to 17%, since lower abortion rates increased the proportion of pregnancies at risk of miscarriage.) Instead, the decline in teenage birth rates was driven entirely by lower pregnancy rates, which fell from 12% to 7% (Ventura, Curtin and Abma 2012). Of the proximate determinants of teenage pregnancy, there is evidence that sexual activity declined, particularly among non-Hispanic blacks (Levine 2000), while contraceptive use increased (Guttmacher Institute 2013). Using data from the 1995 and 2002 waves of the National Survey of Family Growth, Santelli et al. (2007) construct a teenage pregnancy risk index based on the proportion of respondents who were sexually active, weighted by the failure rates of the contraceptive method(s) used, if any. They estimate that teenage pregnancy risks fell by 38% between 1995 and 2002, and that around 85% of the decline can be attributed to increased contraceptive use, including an increase in the probability of using at least one method (from 66% to 82%). (Their study finds larger declines in pregnancy risk for whites than for blacks or Hispanics, possibly due to small sample sizes for minority groups.) Kearney and Levine (2012b) take a different approach: using data from the National Survey of Family Growth and Youth Risk Behavior Surveillance System, they regress teenage birth rates on the proportions of teenagers who were sexually active and using contraceptives at the state level, and estimate that changes in sexual activity and contraceptive use account for around 27% and 52% of the decline in teenage birth rates respectively. This calculation leaves approximately 20% of the decline unexplained, which may point to data inaccuracies or to more effective use of current contraceptive methods (they find no evidence that the decline was due to a shift to more reliable forms of contraception). In summary, the decline in teenage birth rates was largely driven by increased contraceptive use among adolescents, replicating the European experience in the early 1980s (Santelli et al. 2007).

1.2.2 Economic Perspectives: Costs and Benefits

Economics studies seek to identify socioeconomic factors underlying the social proximate determinants of teenage childbearing. Teenagers are assumed to operate within a rational choice framework, where they choose to become sexually active, use contraceptives and have a teenage birth if the benefits exceed the costs (Levine 2000). Benefits and costs are modeled as functions of preferences for teenage sexual activity, contraceptive use and early childbearing, and the costs of these activities. These factors, in turn, depend on individual, family and community characteristics, including individual cognitive ability and health, parental income, age, education, parenting ability and family composition, and school quality, peer characteristics and community criminal activity levels (Levine 2000; Sanders 2011).[2] For example, individual cognitive ability and educational success increase the opportunity costs and reduce preferences for a teenage birth (McCrary and Royer 2011), and may increase ability to recognize the costs of unprotected sex in terms of pregnancy and sexually transmitted disease risks. Similarly, socioeconomic disadvantage increases preferences for a teenage birth by narrowing the set of known educational and economic opportunities and making early childbearing an appealing distraction from the continual pressures of economic deprivation (Kearney and Levine 2009), while reducing the affordability of contraceptives and abortion. Using data from the Panel Survey of Income Dynamics, Kearney and Levine (2009) find that teenage birth rates are more strongly predicted by community characteristics, particularly income inequality (Kearney and Levine 2012a), than by individual-level measures of socioeconomic disadvantage, which points to the importance of spillover peer effects such as economic expectations and cultural factors.

A central goal of the economic literature is to establish the causal influence of these individual, family and community factors, which can be difficult since individual cognitive ability and teenage childbearing outcomes, for instance, may be driven by common third factors such as poverty and peer characteristics, leading to omitted variable bias. Hence, economists have turned to experimental and quasi-experimental

---

[2] Sanders (2011) argues that a number of these antecedents, including maternal age and parental abuse, also predict juvenile criminal activity, and shows that teenage childbearing and juvenile crime follow similar trends in the United States between 1975 and 2001.

research designs to study the determinants of teenage childbearing. A number of papers exploit changes in compulsory schooling laws in Germany (Cygan-Rehm and Maeder 2013), Peru (Rivera 2013), the UK (Silles 2011), Norway (Black, Devereux and Salvanes 2008) and the United States (Black, Devereux and Salvanes 2008; Marcotte 2013) to study the impact of years of education on probability of teenage motherhood, and consistently find a significant impact.[3] McCrary and Royer (2011), on the other hand, use discontinuities in kindergarten entry age due to school entry policies in California and Texas and find no impact of years of education on adolescent fertility. Ludwig et al. (2013) present evidence from the Moving to Opportunity experiment suggesting that neighborhood environments have no significant impact on teenage childbearing, in contrast to Kearney and Levine (2009).

Other papers examine the impacts of plausibly exogenous changes in the external environment, such as policy reform. Levine (2000) finds that sexual activity and fertility outcomes among female teenagers are responsive to changes in labor market conditions and, to a more limited extent, AIDS incidence rates. On the other hand, examining the impact of policy changes on teenage birth rates between 1991 and 2008, Kearney and Levine (2012b) find that welfare generosity and access to family planning services through Medicaid explain only 12% of the observed decline during this period,

---

[3] A recent study by Stephens and Yang (2014), however, finds that the impacts of U.S. compulsory schooling law changes on economic outcomes, including wages and unemployment rates, disappear once state-specific trends are controlled for, suggesting that the findings for teenage fertility may also be less robust, at least in the case of the United States.

and find no evidence that abstinence or other sexual education programs, the Children's Health Insurance Program, the Minor Parent Provisions program, the Child Support Enforcement program or restricted access to abortion for minors had any impact (Kearney and Levine 2012b).

1.2.3 Sociological Perspectives: Central Risk Factors

Similar to demographers and economists, sociologists view teenage childbearing outcomes as a function of biology, health, and other individual, family and community characteristics. A sizable literature (some of which were published in demography or psychology journals) explores central risk factors using diverse datasets, including the High School and Beyond Panel study (Abrahamse, Morrison and Waite 1988), the Intergenerational Panel Study (Barber 2001), the Longitudinal Pathways to Adulthood Study (Hardy et al. 1998), the 1988 National Education Longitudinal Study (Manlove 1998; Moore et al. 1998), the 1997 National Longitudinal Survey of Youth (Meade, Kershaw and Ickovics 2008) and the 1988 National Survey of Family Growth (Kahn and Anderson 1992). These papers document the strong associations between teenage childbearing outcomes and individual academic success, family socioeconomic status and structure, maternal education and age at first birth, and parental communication and supervision. However, there is mixed evidence that peer characteristics predict teenage childbearing.

Among this myriad of risk factors, sociologists have paid particular attention to the role of maternal age and the intergenerational transmission of early childbearing. Studies consistently find that girls born to mothers who were teenagers at first birth are more likely to become teenage mothers themselves (Hardy et al. 1998; Kahn and Anderson 1992; Meade, Kershaw and Ickovics 2008). The association appears to be substantially more pronounced among whites, where daughters of teenage mothers are at more than twice the risk of a teenage birth, compared to only a one-third increase in risk among blacks (Kahn and Anderson 1992). Potential mechanisms for this association include the intergenerational transmission of socioeconomic disadvantage, limiting the opportunity costs and increasing the attractiveness of early childbearing (Barber 2001; Kahn and Anderson 1992); the transmission of more positive views of early or non-marital childbearing, in part due to higher maternal completed fertility, which increases ideal family size and lowers ideal age at first birth (Barber 2001; Kahn and Anderson 1992); the transmission of negative views or lack of knowledge regarding contraceptives (Barber 2001; Manlove 1997); and lower levels of parental involvement (Barber 2001; Kahn and Anderson 1992), potentially due to lower parenting ability and access to social support among younger mothers. (On the other hand, there is little evidence that the intergenerational transmission of teenage motherhood is due to the transmission of age at menarche (Kahn and Anderson 1992).)

Distinct but highly correlated with maternal age at first birth is maternal marital status, which may influence teenage childbearing outcomes through another set of mechanisms. Increased access to contraceptives, advancements in home technologies, falling gender wage gaps and liberalized divorce and other marriage laws have resulted in lower costs of delaying marriage as well as lower gains from household specialization, leading to lower marriage rates and higher out-of-wedlock fertility rates (Stevenson and Wolfers 2007). Among unwed mothers, the proportion of women who marry within three years of childbirth has fallen by 27% for whites and 60% for blacks between 1968 and 1989, reflecting a rising disassociation between marriage and childbearing, where childbirth is not seen as a sufficient reason for marriage and marriage is not seen as critical for child wellbeing (Gibson-Davis 2011; Gibson-Davis, Edin and McLanahan 2005). Absence of the biological father may lead to financial hardship, lower maternal mental health and lower levels of parental involvement (Barber 2001; Kahn and Anderson 1992; McLanahan 2009); having an unmarried mother may lower marriage expectations among daughters (McLanahan 2009), reducing the perceived costs of out-of-wedlock childbearing; unmarried mothers are much more likely to undergo partnership changes and have children with multiple partners, further reducing paternal investment (McLanahan 2009) and family resources per child; and daughters may face higher risk of conflict with stepfathers or other non-biological male adults, resulting in emotional distress (Barber 2001; Kahn and Anderson 1992; McLanahan 2009) and a desire to form their own separate family early in life.

Finally, the sociological literature suggests that race is strongly predictive of adolescent childbearing, even after accounting for individual academic performance and family socioeconomic status and structure (Abrahamse, Morrison and Waite 1988). There are a number of potential explanations for this finding. First, black women may be exposed to higher sexual activity among peers (Abrahamse, Morrison and Waite 1988). Second, black teenage mothers may receive more help with childrearing from the extended family (Meade, Kershaw and Ickovics 2008). Consistent with this hypothesis, there is some evidence that white teenage mothers are more likely to intend raising children with a partner, whereas black teenage mothers are more likely to intend to rely on support from the extended family (Smith Battle 2000), and indeed white teenage mothers are more likely to maintain frequent contact with the child's father (69%, compared to 42%) and less likely to live with their own mother (49%, compared to 79%) (Unger and Cooley 1992). Higher levels of family support may also help to explain why black teenage mothers are substantially less likely to drop out of school (around 50%, compared to 60% for whites and 70% for Hispanics) (Manlove 1998). Third, black women may be at higher risk of health complications associated with later childbearing (Meade, Kershaw and Ickovics 2008). Fourth, black teenage mothers may incur lower social stigma for having an out-of-wedlock birth (Abrahamse, Morrison and Waite 1988) as well as lower penalties in terms of marital prospects due to lower access to traditional routes of family formation. Black women are not only at much higher risk of non-marital childbearing (around 70% of black children were born out of wedlock in 2000, compared to 24% of whites and 42% of Hispanics), but also face higher rates of union dissolution (McLanahan 2009). This disparity in marital status is likely not due to lower value placed on marriage – indeed, black unmarried mothers have more positive views of marriage and are more likely to agree that "it is better for children if their parents are married" than their white counterparts – but rather due to the lower availability of potential marriage partners in terms of the ratio of employed men to women (McLanahan 2009) and the declining economic position of less educated men, resulting in increasing acceptability of childbearing outside marriage (Morgan and Rindfuss 1999).

1.3 Why Should Teenage Childbearing be a Matter of Public Concern?

Teenage birth rates have been of great interest to policymakers as an "urgent crisis" since the 1970s (Furstenberg 1991). There are several reasons why teenage childbearing has been allotted so much public attention. First, the proportion of births born to teenagers rose during the 1960s as the baby boom cohorts entered their teenage ages, producing the illusion that teenage childbearing was becoming a pandemic; moreover, an increasing proportion of teenage births were non-marital (although this is not a trend unique to teenage births) (Furstenberg 1991; Maynard 1997), rising from 40%

in 1976 to almost 70% in the 1990s (Kearney and Levine 2012a). Second, public observers noted rising expenditures on welfare programs, as well as a rising proportion of young never-married women among those on welfare (Maynard 1997). As a result, teenage childbearing as a policy issue has managed to appeal to both liberals interested in expanding social services and to conservatives interested in containing future spending on welfare (Furstenberg 1991). Underlying the public's concern and support for intervention is the perception that teenage motherhood is a costly interruption of women's educational progress, especially with rising returns to education, which leads to lower future labor productivity, increased dependence on social assistance and child poverty. Teenage motherhood is hence viewed as a failure of judgment and rational planning which places families and children at elevated risk of poverty.

1.3.1 Consequences of Teenage Childbearing for Mothers

A simple cross-sectional comparison suggests that teenage mothers do indeed have more adverse life outcomes than their non-childbearing peers. (In contrast, there is less evidence that teenage childbearing results in worse educational and economic outcomes for fathers (Maynard 1997).) Only half of teenage mothers complete high school by early adulthood, and many receive a GED instead of a high school diploma. Moreover, teenage mothers tend to space their births more closely and to end up with larger families (around 0.6 more children on average). As a result, around 40% live in poverty at age 27, with a higher proportion among black teenage mothers (two-thirds, compared to a quarter of whites and half of Hispanics). Around 84% of unmarried teenage mothers go on welfare, usually for at least five of the first ten years after their first birth. Less than half of unmarried teenage mothers get married during this period, and less than a third receive any financial support from non-resident fathers (Maynard 1997).

It is unclear, however, that the observed correlations between teenage motherhood and these adverse life outcomes are due to the fact that these women did not wait until their twenties to have their first child. As economists and sociologists point out, women who become teenage mothers tend to be disadvantaged and are disproportionately likely to be doing poorly in school, to come from single-parent and impoverished homes and neighborhoods, and to have experienced physical and sexual abuse (around 10% and 50% respectively among women who give birth before age 18) (Maynard 1997). Consistent with this argument, Upchurch and MacCarthy (1990) show that most early child-bearers drop out of high school *prior* to giving birth, and that once they drop out, the probability of eventually graduating is reduced to 30%, whether or not they go on to have an early birth.

Two seminal papers, Geronimus and Korenman (1992) and Hotz, McElroy and Sanders (2005) have tremendously influenced the current view of the consequences of teenage childbearing and inspired a substantial body of work. The two studies use very different approaches (but the same dataset, the National Longitudinal Survey of Youth) to address the issue of negative selection into teenage childbearing: Geronimus and Korenman compare the outcomes of sisters who give birth at different ages, removing family fixed effects from their estimates, while Hotz, McElroy and Sanders compare women who miscarry a teenage pregnancy with women who have a teenage live birth, where miscarriage is assumed to be a random event. Both papers find that the observed correlations between teenage motherhood and adverse life outcomes, including lower rates of high school completion and employment and lower family income, are largely due to negative selection – in fact, Hotz, McElroy and Sanders find that teenage childbearing *increases* number of hours worked and total earnings.[4]

Other researchers have questioned these findings. An important issue with Geronimus and Korenman's sister study is that even within families, women who have a teenage birth may be negatively selected compared to their sisters who avoid it – however, as the authors point out, this would mean that their estimates of the impacts of teenage childbearing are likely to be an upper bound. Investigating the issue of within-family heterogeneity by using household registry data on Swedish sisters, Holmund (2005) finds that teenage mothers do indeed tend to have slightly lower ninth grade test scores than their sisters, and that controlling for this difference reduces the estimated impact of teenage childbearing on years of education from around 0.9 to 0.6 years.

[4] The authors attribute their finding to the fact that teenage mothers are more likely to work in jobs which value job-specific skills more than educational attainment, so that completing childbearing early in life may be an advantageous option. They find that motherhood by age 18 increases family size and the probabilities of single motherhood and receiving welfare assistance, but only in the short run.

Beyond the issue of within-family heterogeneity, sister studies may be less reliable due to small sample sizes and post-birth transfer of resources between sisters, introducing bias to the estimates (Kane et al. 2013).

Similarly, Hotz, McElroy and Sanders's comparison of pregnant teenage girls who miscarried with those who carried their pregnancies to term have been subjected to various criticisms, including the fact that the study is limited to a disadvantaged sample (women who experience teenage pregnancy). Indeed, the authors themselves note that a substantial proportion of the women who miscarried in their sample still went on to give birth by their early 20s, so that the impacts of early motherhood may be greater than estimated due to imperfect "compliance" with the "treatment", i.e. avoiding teenage childbearing (Ashcraft, Fernandez-Val and Lang 2013). Moreover, using a similar statistical approach but with data on women who were teenagers in the late 1970s rather than in the early 1970s, Hoffman (2008) finds stronger evidence that teenage childbearing has negative economic consequences for women. Perhaps most importantly, others have suggested that miscarriage may be a non-random event. In particular, teenage women who choose not to get an abortion increase their exposure to the risk of having their pregnancies end in a miscarriage, and these women tend to be socioeconomically negatively selected (Ashcraft, Fernandez-Val and Lang 2013). Consistent with this argument, Fletcher and Wolfe (2009) find that miscarriages are correlated with community-level characteristics, such as measures of community disadvantage, average pregnancy outcomes of teenagers and state-level access to abortion. Accounting for selection into abortion and community-level fixed effects respectively, they find that the impacts of teenage motherhood are small but significant, on the order of five percentage points lower probability of high school graduation or obtaining a GED.[5]

Beyond these replication studies, researchers have used other econometric techniques, including propensity score matching (Kane et al. 2013; Levine and Painter 2003) and semiparametric maximum likelihood estimation using state minor abortion laws and other community characteristics as instruments (Kane et al. 2013). Kane et al. (2013) estimate that teenage motherhood reduces years of education by around 0.7 years, similar to Holmund (2005), and argue that the impact may decline with age as teenage mothers return to school after childbearing. Levine and Painter (2003), on the other hand, find comparatively large effects, including declines in the probabilities of receiving a high school degree by age 20 and attending college by 22 and 26 percentage points respectively.

The above review suggests that much of this body of literature focuses on the impacts of teenage childbearing on educational and labor market outcomes. As

---

[5] Ashcraft, Fernandez-Val and Lang (2013) find that teenage motherhood reduces the probability of obtaining a GED by five percentage points and years of education by 0.15 but find no impact on the probability of getting a high school degree, whereas Fletcher and Wolfe (2009) find that teenage mothers are 5-10 percentage points less likely to receive high school degrees. The former also calculate that a negative impact on working hours but no impact on income among working women, whereas Fletcher and Wolfe (2009) find that teenage mothers earn $1,000-$2,400 less as a young adult.

discussed in greater detail below in this chapter, teenage mothers are far more likely than slightly older counterparts to state that their births were unintended, and this may reflect costs that go beyond economic considerations, including a sense of loss from missing out on the "teen experience" (Herrman and Waterhouse 2011), as well as fatigue and mental stress associated with balancing schooling with new financial and other responsibilities. There is also evidence that teenage mothers may face lower marital prospects (Ashcraft, Fernandez-Val and Lang 2013; Geronimus and Korenman 1992) and experience a lower quality relationship with partners throughout their lives. Teenage mothers today are less likely than ever to be married at time of birth or to receive financial support from non-resident fathers (Furstenberg, Levine and Brooks-Gunn 1990). Qualitative studies suggest that while many teenage mothers expect some level of paternal involvement from their boyfriends, some view their own mothers as a more important source of support and assistance (Spear 2001). In some instances, the father is deliberately excluded from childrearing by the woman's family, especially in cases involving rape or incest (Furstenberg, Brooks-Gunn and Chase-Lansdale 1989). By age 40, unmarried mothers are not only less likely to have been married but also more likely to have been divorced if they did enter a union, which may be due in part to poorer match quality and conflict over resource use (Graefe and Lichter 2002). Importantly, the decline in marriage rates among teenage mothers may also mean that the economic consequences of teenage childbearing may be more serious than implied by the above literature review, since most of the studies use data based on older cohorts (Hoffman 1998); this point is consistent with Hoffman (2008) finding larger negative consequences of teenage childbearing among women who were teenagers in the late 1970s than among those who were teenagers in the early 1970s.

Finally, there is little evidence that teenage motherhood causes lower mental health. While depression rates are alarmingly high among teenage mothers (more than 25% among both whites and blacks, compared to only 13% among all women) (Kalil and Kunz 2002), psychological distress is likely to be a risk factor rather than a consequence of teenage motherhood (Mollborn and Morningstar 2009). Instead, maternal psychological wellbeing is much more strongly linked to marital status than to age at childbearing, and may ultimately be grounded in exposure to poverty and unmet emotional needs (Kalil and Kunz 2002).

In summary, there is strong evidence that much of the observed adverse educational and economic outcomes associated with teenage childbearing are attributable to negative selection, in contrast to the more severe views in the 1987 National Science Foundation report *Risking the Future*. However, young motherhood may nevertheless reduce women's marital prospects and exact other types of non-economic costs. It is also unclear how the impacts of teenage motherhood depend on socioeconomic status and pregnancy intentions. Two of the above studies suggest that the educational consequences are larger for women from socioeconomically advantaged backgrounds who were actively taking steps to avoid pregnancy (Fletcher and Wolfe 2009; Levine and Painter 2003). A qualitative study involving interviews with 16 non-Hispanic white and black teenage mothers from metropolitan West Coast programs by Smith Battle (1995), on the other hand, argues that compared to women from exceptionally economically and emotionally deprived backgrounds, women from more emotionally enriching or economically advantaged backgrounds may be in a better position to gain from positive post-birth psychological changes, such as willingness to give up partying, smoking and drinking, to change their peer groups and to return to school in order to secure a better future, and are likely to have more access to social support for pursuing these aspirations.

1.3.2 Consequences of Teenage Childbearing for Children

A closely related body of literature examines the consequences of teenage childbearing for the next generation. Children of teenage mothers tend to have poorer health than other children and are less likely to visit medical care facilities except for the emergency care room. They are also more likely to be victims of abuse and neglect and to spend time in foster care, especially among later born children. Potentially as a result, they have lower word recognition, reading comprehension and mathematics scores at age 14, higher probability of grade retention and less favorable ratings from teachers, and are less likely to graduate from high school. Finally, they are more likely to be incarcerated during adolescence, to become unmarried teenage mothers and to be absent from the labor force (Maynard 1997).

Similar to findings in the previous section, the evidence suggests that much of the observed adverse outcomes are attributable to the limited life options available to disadvantaged youth, rather than the fact that their mothers did not wait until their twenties to have their first child (Kearney and Levine 2012a). At the same time, young motherhood may present unique threats to child wellbeing. Children of teenage mothers are exposed to more fragile family structures and lower levels of paternal involvement (Furstenberg 1991), and are less likely to be intended (discussed in greater detail below), which may further reduce parental inputs and result in lower levels of schooling and wages and higher crime rates (Lin and Pantano 2011).

There is at best mixed evidence that teenage motherhood causes poorer child health. This is somewhat surprising, given that pregnant teenagers are less likely to receive prenatal care, more likely to smoke and drink, take less folic acid, gain less weight and are less likely to breastfeed their infants after birth (Botting, Rosato and Wood 1998; Meade, Kershaw and Ickovics 2008). These differences in maternal health behavior may be due to in part to higher probability of birth unintendedness, which is associated with greater risks of complications during pregnancy, birth and the postpartum periods at all maternal ages (Kissin et al. 2008; Mosher, Jones and Abma (2012). Consistent with these risk factors, births to teenage mothers are more likely to be preterm or have low birth weight, and are at greater risk of infant mortality or congenital abnormalities (Botting, Rosato and Wood 1998; Hofferth 1987; Meade, Kershaw and Ickovics 2008).[6] However, other studies suggest that children of younger mothers have similar health as their peers at ages three, except for a higher probability of injuries or burns (Rothenberg, Brickner and Varga 1981) and 14 (Shaw, Lawlor and Najman 2006). In addition, Brooks-Gunn and Furstenberg (1986) find that any differences in early health disappear once socioeconomic background is controlled for (except among children born to women under age 15), suggesting that most of the observed health disparities are due to socioeconomic inequality, lack of social support and lower access to quality health care rather than to maternal age (Botting, Rosato and Wood 1998; Hofferth 1987; Horon, Strobino and MacDonald 1983).

There is somewhat more convincing evidence that teenage motherhood causes elevated risks of child physical or sexual abuse and neglect. Around 51% of white teenage mothers and 21% of black teenage mothers do not live with their own mothers during the first two years after childbearing, especially among older teenage mothers, and only 15% and 20% respectively receive regular help from their mothers with childcare (Black and Nitz 1996; Unger and Cooley 1992). Using medical records for 400 inner-city children in New Haven, Stier et al. (1993) find that children of mothers aged

[6] Horon, Strobino and MacDonald (1983), however, find that babies born to mother below the age of 16 have shorter gestational ages but similar birth weight to those born to mothers aged 20-24 of the same race.

below 19 are at twice the risk of parental neglect than their peers, especially during the first two years of age. Similarly, following 45 teenage mothers in a low-socioeconomic urban setting, Flanagan et al. (1995) find that a third of the families experienced child maltreatment cases that were substantiated by a state child protection agency within two years. The higher risk of abuse also exposes children to higher probability of change in primary caretakers (Stier et al. 1993), which is likely to be a traumatic event in itself. Potential mechanisms for higher risk of abuse include maternal depression and stress, maternal immaturity, lack of social support and child unwantedness, which may result in poorer maternal decision-making, skills and attitudes (Brooks-Gunn and Furstenberg 1986; Elster, McAnarney and Lamb 1983; Flanagan et al. 1995; Mosher, Jones and Abma 2012). Parenting studies suggest that teenage mothers may indeed have poorer parenting skills: adolescent mothers tend to verbalize less and interact more physically with their children (Brooks-Gunn and Furstenberg 1986; Elster, McAnarney and Lamb 1983), provide less consistent but harsher discipline (Jaffee et al. 2001) and order their children to do things rather than encourage self-directed exploration (Elster, McAnarney and Lamb 1983). Younger mothers are also less knowledgeable about child developmental milestones and are less likely to bring their child to health clinics or dentists (Brooks-Gunn and Furstenberg 1986). However, they do not display significantly lower levels of maternal warmth (Brooks-Gunn and Furstenberg 1986). It is also worth noting that the abuser is typically not the mother, but rather the father, boyfriend or relative (Stier et al. 1993), and that a disproportionate number of children who experience abuse live in group homes or shelters, which may indicate that the mothers themselves were at high risk of abuse and neglect (Flanagan et al. 1995). Hence, the absence of social support may be a more important explanation for the elevated risk of child abuse than maternal immaturity or child unwantedness.

Another key outcome which has been studied extensively in the literature is the cognitive development and educational progress of children born to adolescent mothers. Children born to teenage mothers exhibit lower cognitive ability and school achievement (Card 1981; Francesconi 2008; Hofferth 1987; Jaffee et al. 2001; Moore and Snyder 1991; Shaw, Lawlor and Najman 2006), and the disparities tend to be larger for boys and to increase with age (Brooks-Gunn and Furstenberg 1986). Several studies argue that much of the observed differences are likely to be due to lower maternal cognitive ability and educational attainment, maternal depression, less enriching home environments as well as unstable family structures, leading to lower human capital accumulation and educational aspirations, rather than the causal impact of maternal age (Card 1981; Hofferth 1987; Moore and Snyder 1991; Shaw, Lawlor and Najman 2006). Non-traditional household structures, in particular, may be an important mechanism through increasing adolescent stress and family size (Card 1981), and are even more strongly associated with educational outcomes than household income among British children (Francesconi 2008).

There is more evidence that maternal age at first birth has some impact on behavioral outcomes. Hofferth and Reid (2002), using data from the National Longitudinal Survey of Youth and the Panel Study of Income Dynamics, find that once family characteristics are controlled for, maternal age is no longer associated with child cognitive test scores but continues to predict behavioral problems. Other studies have also found statistically significant correlations between maternal age and behavioral and socio-emotional outcomes, including psychological distress, self-esteem, self-control, ability to delay gratification, aggressiveness and speech problems (Brooks-Gunn and Furstenberg 1986; Hofferth 1987; Shaw, Lawlor and Najman 2006), especially among male children of black teenage mothers (Brooks-Gunn and Furstenberg 1986; Hofferth 1987). Disparities in these outcomes, which tend to emerge only after age seven, may help to account for the observed cognitive differences as well as higher juvenile criminal activity (Hofferth 1987), especially among boys. Among daughters, those born to teenage mothers are at especially high risk of adolescent sexual activity (Furstenberg, Levine and Brooks-Gunn 1990) and are substantially more likely to become teenage mothers themselves, even after controlling for background characteristics such as maternal education, marital status and parental involvement (Hardy et al. 1998; Kahn and Anderson 1992; Meade, Kershaw and Ickovics 2008). These links between maternal age and behavioral outcomes may reflect small but significant causal links between teenage childbearing and household poverty, unstable family structures, maternal psychological distress and insecure or insensitive parent-child attachment (Hofferth 1987; Shaw, Lawlor and Najman 2006), where maternal distress and lower quality parent-child attachment may result in poorer development of language skills and social adjustment, as well as increased vulnerability to stress and challenges (Elster, McAnarney and Lamb 1983; Mollborn and Morningstar 2009).

In summary, there is some weak evidence that teenage motherhood has consequences for child health or cognitive ability, and somewhat stronger evidence that it has an impact on child exposure to neglect or abuse as well as behavioral and socio-emotional outcomes, particularly the teenage childbearing behavior of daughters. These associations may not be surprising given the above review of consequences for women, and may in particular reflect the negative impacts of teenage motherhood on women's marital prospects, as well as the weak causal links between early childbearing and educational and economic success.

Finally, while much of the observed adverse outcomes among *individuals* born to teenage mothers are attributable to the limited life options available to disadvantaged youth, the consequences for the *next generation* in terms of population dynamics and composition may be more serious. Early childbearing is associated with closer spacing of births and higher completed fertility, although these associations have become weaker among more recent cohorts (Morgan and Rindfuss 1999), so that the teenage birth rate is directly proportionate to the fraction of births born to disadvantaged households, and potentially to the level of socioeconomic inequality, in future generations. Moreover, even small effects of teenage birth rates on future generational wellbeing may be compounded over time due to intergenerational transmission of adolescent childbearing, which is especially strong among whites.

1.3.3 Teenage Mothers' Intentions and Aspirations

A second reason why the public should care about teenage childbearing is that when asked, most teenage mothers indicate that they would have preferred a different life course at the time of pregnancy. Unsurprisingly, compared to their peers, teenagers who engage in unprotected sexual intercourse and become mothers tend to have more positive views of early childbearing (Abrahamse, Morrison and Waite 1988; Unger, Molina and Teran 2000). Nevertheless, around 80% of teenage births are unintended (21.8% and 56.5% are reported as unwanted and mistimed respectively), compared to around 45% of births to women in their early 20s (17.3% and 27.4% respectively) (Kissin et al. 2008).[7] (The proportions are even lower among women aged 25 to 44, at 11.7% and 12.1% respectively.) Similarly, the Guttmacher Institute (2013) reports that 82% of teenage pregnancies are unplanned, and that 87% of female teenagers and 80% of male teenagers state that they would be at least a little upset in the case of a pregnancy (58% and 48% state that they would be very upset). In an older study, Brooks-Gunn and

[7] Apart from teenagers, black, unmarried or less educated women are also more likely to state their births were unintended. The proportions of mistimed and unwanted among all women have been fairly stable across time (23% and 14% in 2006-2010, compared to 27% and 10% in 1982) (Mosher, Jones and Abma 2012).

Furstenberg (1986) find that 50% of pregnant adolescents could not bring themselves to tell their parents about their pregnancy for several months.

If most teenage mothers want to avoid early childbearing, why do they fail to take proper precautions or choose not to get an abortion? Using time diary and interview data, Bartz et al. (2007) find that adolescents who were trying to avoid pregnancy used contraceptives during only half of encounters involving coital intercourse; similarly, Coles, Makino and Stanwood (2011) find that half of adolescents who had an unintended birth report that they were not using contraception at the time of conception. This low rate of contraceptive use may be due to lack of access, negative views of contraceptives including fear of side effects such as weight gain, misinformation about pregnancy risk and lack of support from partners and peers (Bartz et al. 2007; Coles, Makino and Stanwood 2011).

Importantly, it has also been argued that a large proportion of teenagers with "unintended" births does not particularly wish to become pregnant but does not really mind either if it happens (Bartz et al. 2007; Kearney and Levine 2012a). This view has important implications for public intervention, since it suggests that teenage childbearing does not truly constitute an unwelcome disruption to teenagers' aspirations and life plans. To support their argument, Kearney and Levine (2012a) point out that when asked, 45% of 18-19 year-olds who do not use birth control do not provide any particular reason, which may reflect ambiguity towards the prospect of pregnancy. They further argue that this group of women may face pressure to state that their births were unintended in order to be socially correct in front of the interviewer. Indeed, there is some evidence that high school students can identify positive aspects of early parenthood, including gaining adult status, receiving financial and other assistance, strengthening relationships with boyfriends or family members, and having someone to love and cherish (Herrman and Waterhouse 2011; Unger, Molina and Teran 2000).[8] A qualitative study finds that early parenthood also allows disadvantaged youth to allay their fears of never becoming a parent or becoming "an 'old' mother", i.e. at age 25 or older, and that teenage years may be viewed as the best time for childbearing: "maybe when they get their careers jumping, being the best they can be, they ain't going to have time to have kids" (Schultz 2008).

Furstenberg (1991), on the other hand, argues that teenagers' failure to rigorously practice birth control does not necessarily imply ambiguous attitudes towards teenage motherhood. He points out that teenagers also inadequately protect themselves against sexually transmitted diseases, and that it is unlikely that they are ambiguous about becoming infected. Contrary to Kearney and Levine (2012a)'s above argument that the

[8] On a four-point scale, where one indicated total disagreement and four indicated total agreement, teenagers were most likely to agree that childbearing would not disrupt their educational progress (3.04), that they would receive help from their family (2.93) and that they would "feel like someone really needs me" (2.90); they were least likely to agree that they would be financially provided for (2.30), to "[f]or the first time…have someone who really loves me" (2.40) and to "feel more like an adult" (2.41) (Unger, Molina and Teran 2000). Overall, however, non-parenting teenagers tend to have negative views of early parenthood, including the loss of personal freedom and the "teen experience", disapproval by family members and conflict with educational and career goals (Herrman and Waterhouse 2011; Schultz 2008).

proportion of teenage pregnancies which are unintended may be overstated, Furstenberg, Brooks-Gunn and Chase-Lansdale (1989) contend that it may instead be understated due to selection for non-abortion and the tendency to view pregnancies more positively at later stages. Furstenberg (1991) further argues that instead of ambiguity, teenagers' low rate of contraceptive use may reflect low ability to assess the risks of sexual activity and potentially even limited control over their actions.

The hypothesis that adolescents may not be able to make informed decisions with regard to sexual activity, contraceptive use or childbearing has powerful implications from policy and ethical viewpoints. Specifically, it implies that society may have an obligation to help ensure that teenagers' outcomes are more closely aligned with their stated preferences. Teenagers have lower cognitive ability than adults, which may reduce their ability to predict the consequences of actions or resist sexual advances, and increase their need for peer acceptance (Abrahamse, Morrison and Waite 1988; Kearney and Levine 2009; Loewenstein and Furstenberg 1991; Shearer et al. 2002). Moreover, during puberty, the development of incentive processing outpaces the development of self-regulation brain systems, leading to heightened sensitivity to risk and sensation seeking and lower impulse control, especially during ages 16-17 (Steinberg 2013). Finally, teenagers in the United States receive "confused and confusing messages" (Furstenberg 1991) regarding the ethics of sexual activity which leads teenagers to have "difficulty accepting themselves as sexual beings who engage in sexual intercourse"

(Abrahamse, Morrison and Waite 1988) and encourages them to keep sex "an underground activity that resists and defies adult control" (Furstenberg 1991). Together, these factors place teenagers at uniquely high risk of poor decision making regarding reproductive behavior.

Observational studies suggest that teenagers take at least some costs and benefits into consideration – Levine (2000), for instance, finds that sexual activity among female teenagers is responsive to labor market conditions. However, teenagers exhibit high levels of time inconsistency, with a high level of attention given to immediate costs and benefits such as the discomforts of birth control, while assigning low weight to the effectiveness of birth control methods (Loewenstein and Furstenberg 1991), making the development of effective contraceptive habits and skills particularly difficult.[9] Beyond time inconsistency, teenage sexual activity tends to be characterized by impulsivity, with the vast majority of teenagers reporting that their first sexual experience was unplanned or unexpected (Loewenstein and Furstenberg 1991). This may be due to lower ability to plan ahead, but could also reflect fears that planned birth control would indicate sexual promiscuity (Loewenstein and Furstenberg 1991), which is especially morally reprehensible since teenagers are not supposed to be having sex. Condom use, in particular, not only runs up against teenagers' strong time preference due to its

[9] High rates of time preference are also observed among disadvantaged adult population – for instance, Cook et al. (1999) show that the decision to have an abortion among black and less educated women aged 18-29 is highly sensitive to out-of-pocket costs.)

inconvenience, disruptiveness to the romantic mood and diminishing of male pleasure, but also signals promiscuity and lack of trust in one's partner (Suvivuo, Tossavainer and Kontula 2009). Under these pressures and constraints, teenagers may find it easiest to adopt a passive attitude and gravitate towards default options, which are unplanned sex, inconsistent use of birth control and non-use of abortion (Furstenberg 1991), and may have low ability to resist unexpected pressure or immediate gratification (Suvivuo, Tossavainer and Kontula 2009). In a qualitative study involving interviews with eight pregnant teenagers, Spear (2004) finds that several of the women view past sexual encounters as pressured or detached and at times expressed regret; at the same time, they viewed their pregnancies as "meant to be" and decided against abortion (Spear 2001).

1.4 Teenage Childbearing and Public Policy

Previous estimates suggest that the annual public costs of motherhood under age 18 are on the order of $22 billion: $10 billion in increased expenditures and foregone tax revenues, including $1.5 billion for child foster care and $1.3 billion for adolescent incarceration, and another $12 billion in private costs to the involved parties, i.e. women and children (Maynard 1997). Even if these estimates are overstated by a factor of 10, teenage childbearing is still worth caring about from a fiscal perspective. Moreover, these figures do not take into consideration potential costs to the larger extended family in order to provide social support, particularly negative externalities to young cousins

(Furstenberg 1991), or the related costs such as treatment of sexually transmitted diseases, which, together with expenditures on 20-24 year-olds, cost the state $8 billion in 2004 (Brindis 2006).

To discuss how public policy can influence teenage birth rates, I follow a simple model of teenage childbearing outlined in Cook et al. (1999), where an individual's teenage childbearing outcome depends on her sexual activity, contraceptive use and abortion use. These factors, in turn, depend on the costs of these activities, including related costs such as the costs of travel, navigating legal barriers and perceived health risks. Interestingly, Cook et al. (1999) point out that the costs of abortion affect not only the probability of abortion given pregnancy, but also the probability of pregnancy itself, since lower costs of abortion reduce the costs of unwanted pregnancy. Hence, reducing the costs of abortion may, in theory, *increase* teenage birth rates.

The most prominent policy aimed at preventing or delaying teenage *sexual activity* is sex and family planning education programs, including media campaigns, abstinence programs in schools (which alone cost around $200 million in 2005) (Brindis 2006) and virginity pledges. The evidence from Table 1 below suggests that there is little evidence that these programs are effective. Other types of programs include youth development programs such as peer counseling and mentoring, job-readiness and life skills training and internship opportunities, which are aimed at increasing economic opportunities for youth and to reduce risk-taking behaviors.

On the other hand, programs which increase teenagers' access to *contraceptives* appear to have much more impact, estimated to prevent 385,000 unintended pregnancies and 183,000 abortions every year, resulting in net annual savings of over $900 million to the public, mostly due to lower Medicaid expenses (Brindis 2006). Similarly, Kearney and Levine (2012b) show that increased access to family planning services through Medicaid in the late 1990s and 2000s significantly reduced teenage birth rates. Conversely, as expected, policies which restrict access, such as parental consent requirements for younger adolescents, are likely to have the opposite impact. In the case of Texas, the implementation of new policies which require publicly funded programs to increase reporting of adolescent abuse and sexual activity is estimated to have led to 8,300 additional pregnancies and 1,650 additional abortions at the public cost of around $50 million (Brindis 2006). At the same time, programs which provide free condoms at school-based centers have relatively little impact on contraceptive use (Kirby and Coyle 1997), suggesting that other family planning services are influencing teenage fertility behavior.

Finally, somewhat surprisingly, there is little evidence that abortion restrictions for minors or increases in out-of-pocket costs of abortion affect adolescent abortion use.

Table 1: Impact of public policies on teenage sexual activity, contraceptive use and abortion use

Author(s) (year)	Type of program evaluated	Evaluation method	Findings
		Teenage sexual activity	
Bennett and Assefi (2005)	Abstinence-only programs	Literature review	No significant impact on age at first intercourse
Brindis (2006)	Media campaigns, school-based abstinence programs, virginity pledges	Literature review	No significant impact of abstinence programs on age at first intercourse; exposure to virginity pledges is associated with higher age at first intercourse but higher rates of non-coital intercourse and STDs
Kearney and Levine (2012b)	Mandatory sex education and abstinence-only programs	Difference-in-difference regression analysis	No significant impact on teenage birth rates
Kirby and Coyle (1997)	School-based abstinence education programs and sex education	Literature review	Most programs find no significant impact on age at first intercourse or frequency of intercourse
Trenholm et al. (2007)	School-based abstinence education programs	Experimental design for four programs	No significant impact on abstinence, unprotected sexual activity, age at first intercourse or number of partners
		Teenage contraceptive use	
Bennett and Assefi (2005)	Abstinence programs paired with information about contraceptives, community service activities or counseling services	Literature review	Significant increase in contraceptive knowledge as well as contraceptive use
Brindis (2006)	Family planning services, parental consent requirements, reporting requirements	Literature review	Significant impact on number of teenage pregnancies and abortions
Kearney and Levine (2012b)	Medicaid family planning expansions	Difference-in-difference regression analysis	Significant impact on teenage birth rates
Kirby and Coyle (1997)	School-based provision of condoms, health centers	Literature review	Most programs find no significant impact on contraceptive use

		Teenage abortion use	
Cook et al. (1999)	Increased out-of-pocket cost of abortion	Difference-in-difference regression analysis	No significant impact on abortion rates among women aged under 18
Kearney and Levine 2012b; Levine 2000	Abortion restrictions for minors	Difference-in-difference regression analysis	No significant impact on teenage birth rates

Other research has explored the impact of policies which may not be aimed explicitly at teenagers. For instance, there is some evidence that early childhood interventions such as the Abecedarian and Perry preschool programs led to lower probability of teenage childbearing later in life (Pungello, Campbell and Barnett 2006; Schweinhart et al. 1985). Similarly, welfare reform, which reduced benefits for low-income households with children and hence increased the costs of out-of-wedlock childbearing, resulted in lower teenage birth rates in the last two decades (Kearney and Levine 2012b). However, the impact appears to be small (Blank 2007), and Kearney and Levine (2012a) point out that European countries, which have more developed welfare state systems than the United States, also have lower levels of teenage childbearing. Indeed, living in an unequal community is the strongest predictor of teenage childbearing, explaining 10-50% of state variation in teenage birth rates (Kearney and Levine 2012a), potentially due to its negative impacts on educational and economic expectations among disadvantaged youth.

Beyond programs aimed at preventing teenage pregnancy and childbearing, public policy has also sought to ameliorate the consequences of teenage motherhood through programs such as school-based or community-based health and prenatal care services. Various studies have attributed improved mother and child outcomes to more supportive school policies regarding pregnant students, including higher probability of eventual graduation (Crean, Hightower and Allan 2001; Upchurch and MacCarthy 1990), more widely spaced births and lower completed fertility (Key et al. 2008; Morgan and Rindfuss 1999) and smaller impacts on educational outcomes in the next generation (Hofferth and Reid 2002). Other programs, such as the 1988 Family Support Act, the New Chance Demonstration and Ohio's Learning, Earning and Parenting, focus explicitly on transitioning teenage mothers from welfare to economic independence through restrictions coupled with services such as school and work requirements paired with parenting and job training workshops and child care assistance, with some evidence that the programs result in higher employment rates and earnings but little impact on fertility, parenting skills or child development (Aber, Brooks-Gunn and Maynard 2005; Granger and Cytron 1999). A third set of programs is more child-focused, including parenting education and home visits programs, child enrichment programs and child care. Examining the impacts of the Children's Health Insurance Program, the Minor Parent Provisions program and the Child Support Enforcement program in particular, Kearney and Levine (2012b) find no evidence that these policies affected teenage birth rates; on the other hand, there is some evidence that parent education and home visitation programs result in fewer days of hospitalization and higher rates of immunization among infants, fewer cases of child abuse or neglect as well as increased maternal educational attainment and spacing between births (Britner and Reppucci (1997; Koniak-Griffin et al. 2002).

The above brief review suggests that public policy can make a discernible difference to teenage childbearing outcomes. Indeed, the policy environment may be crucial during adolescence, which is a period of unusual developmental plasticity involving dramatic remodeling of brain systems, leading to strong sensitivity to environmental factors, including social information and stress. This plasticity may be evolutionarily advantageous since it allows adolescents to begin learning to operate independently. On the one hand, this means that adolescence is a stage of life where individuals are particularly vulnerable to addictions and psychiatric disorders (Steinberg 2013) as well as peer pressure (Loewenstein and Furstenberg 1991); on the other hand, development of self-regulation, which may be even more important to future success than intelligence, is more plastic during this period and may be taught or enhanced through interventions. This may be particularly important for lower socioeconomic groups, who may have lower resources or ability to promote self-regulatory behaviors (Steinberg 2013).

At the same time, there may be a need to ensure that efforts to help teenage women achieve their preferred educational and fertility goals should be framed in a manner sensitive to the socioeconomic constraints and pressures which push these women off their planned life courses in the first place. In particular, Geronimus (2003) points out that black teenage women may be at higher risk of childbearing due to lower educational and labor market opportunities, lower adult life expectancy, poorer maternal and infant outcomes at older maternal ages and more self-sufficient nuclear family structures. Hence, characterizing teenage childbearing as antisocial behavior, even if well-intentioned in the name of fighting poverty, inequality and promoting reproductive freedom, not only unfairly imposes the "cultural priorities" or the accepted norms and ideals of socioeconomically dominant groups on less advantaged groups, but may also lead to stigmatization, public exclusion and potentially a sense of "painful, poignant and potentially humiliating" personal failure (Geronimus 2003).

Finally, the fixation on childbearing during high school ages, rather than on childbearing up to the early 20s, may increasingly be less justified due to rising average educational attainment, rising returns to education and rising importance of female earnings to family income (Kane et al. 2013). Consistent with this argument, Hardy et al. (1998) find that while women who delayed their childbearing to ages 25 or above had significantly better educational, economic, physical and mental health outcomes, teenage mothers and women who gave birth ages 20-24 had more similar outcomes. While childbearing at these ages is less likely to be unintended, the potential consequences may nevertheless be considerable enough to also warrant public attention and intervention.

1.5 Contributions of This Dissertation

The rest of this dissertation consists of two original empirical studies which address two prominent puzzles in the above literature review on teenage childbearing in the United States. The first study addresses the contradictory findings from quasi-experimental studies as to whether years of education have a negative causal impact on teenage childbearing (see *1.2.2 Economic perspectives: costs and benefits*). This study seeks to reconcile these contradictory findings by re-examining the impacts of school entry laws using a highly detailed North Carolina administrative dataset which includes not only birth certificate data but also individually linked school administrative records.

The second study examines potential causes behind recent trends in the U.S. teenage birth rate (see *1.1 Recent Trends in Teenage Childbearing*). This study uses age-period-cohort analysis to investigate the extent to which the decline in teenage birth rates reflects changes in cohort behavior rather than simultaneous declines across multiple birth cohorts. I also use a difference-in-differences model to investigate the extent to which major social policies in the 1970s-1980s could have caused teenagers in the 1990s-2000s to behave differently from previous birth cohorts.

2. Re-examining the Impact of Education on Teenage Motherhood: Evidence from North Carolina

2.1 Introduction

The literature on teenage motherhood suggests that women with more years of education or better test scores are at lower risk of teenage motherhood. Abrahamse, Morrison and Waite (1988), Manlove (1997) and Meade, Kershaw and Ickovics (2008) show that teenage girls with higher test scores are at lower risk of childbearing, even after controlling for a number of observed family characteristics. There are a number of theoretical causal pathways through which educational success could lower risks of teenage childbearing. First, educational success may increase the opportunity costs of early childbearing in terms of higher foregone earnings and non-pecuniary benefits, including job prestige and satisfaction (Cygan-Rehm and Maeder 2013). Second, educational success may reduce tastes for teenage motherhood. Better educated girls can expect higher household incomes due to higher own earnings and higher spousal earnings, since they are more likely to be in stable marriages (Isen and Stevenson 2010) with better educated partners (Schwartz and Mare 2005). Economic models of fertility suggest that higher-income individuals choose to have fewer children and to invest more heavily in each child, so that educationally successful women may prefer smaller family sizes and avoid early childbearing (McCrary and Royer 2011). Girls with more years of education are also more exposed to positive influence from school-going peers (Cook and Kang 2013), which may lead to a more negative perception of teenage motherhood. Third, educational success may increase contraceptive knowledge (McCrary and Royer 2011) and the ability to accurately assess the risks of unprotected sexual activity.

Despite the plausible theoretical basis for a causal relationship between educational success and teenage motherhood, establishing this relationship empirically can be difficult for several reasons. First, poor educational outcomes and teenage childbearing may be driven by a common third factor such as poverty or negative peer influence, leading to omitted variable bias. Empirically, family and peer characteristics are difficult to fully control for – even within families, some girls may receive more resources than their sisters. Second, fertility intentions and outcomes may affect school performance and probability of dropping out, leading to simultaneous causality bias. To avoid these estimation issues, some researchers have turned to experimental and quasi-experimental approaches. Dobbie and Fryer (2013), Pungello, Campbell and Barnett (2006) and Schweinhart et al. (1985) show that children who were randomly selected to attend the intensive Abecedarian and Perry preschool programs or a high-performing charter school in Harlem had better future educational outcomes and were less likely to become teenage mothers. This evidence, while compelling, is based on small samples (n = 111, 123 and 407 respectively) of socioeconomically disadvantaged and predominately African American women. Another set of studies follows a larger-scale quasi-experimental approach by exploiting changes in compulsory schooling laws in Germany

(Cygan-Rehm and Maeder 2013), Peru (Rivera 2013), the UK (Silles 2011), Norway and the United States (Black, Devereux and Salvanes 2008). The studies consistently find that cohorts who were compelled to complete more years of schooling have lower rates of teenage childbearing. Marcotte (2013) uses both variation in compulsory schooling laws as well as in mandatory high school graduation exams and finds similar estimates of the impact of high school graduation on teenage childbearing using either instrument.

Another quasi-experimental approach makes use of the observation that individuals born in different times of the year are exposed to different conditions which could affect their educational outcomes. Angrist and Krueger (1991) note that individuals who are born earlier in the year reach the legal minimum dropout age after fewer years of education, and use quarter of birth as an instrument to estimate the impact of education on wages. Buckles and Hungerman (2013), however, show that season of birth is not randomly assigned, with a significantly higher proportion of winter babies and lower proportion of summer babies born to teenage, unmarried or less educated mothers, and that these differences in maternal attributes explain 25-50% of the associations between birth season and later life outcomes. More recent papers avoid this selection issue by comparing individuals born in a similar part of the year who are nevertheless exposed to different conditions. To estimate the impact of years of education on fertility among young women, McCrary and Royer (2011) compare individuals born up to 50 days before and after the kindergarten entry cutoff dates in

California and Texas, where those born after the cutoff date are compelled to enter kindergarten and each subsequent grade at an older age. The authors show that girls born after the cutoff date have significantly fewer years of education than their earlier-born counterparts but exhibit very similar fertility behavior, suggesting that years of education has no causal impact on early fertility. Their paper is the first to use school entry laws to examine the impact of education on fertility.[1] Hence, the evidence from quasi-experimental studies using compulsory schooling laws and school entry laws yield conflicting evidence as to whether years of education reduce teenage childbearing.

An important advantage of using school entry laws rather than compulsory schooling laws is that the "treatment" is targeted at individuals rather than at entire cohorts, so that there are no changes in labor market conditions which could account for the observed differences in outcomes (Black, Devereux and Salvanes 2011; Cook and Kang 2013). On the other hand, school entry laws may not provide a suitable instrument for education since school entry age affects educational success in multiple and offsetting ways: while it has a negative impact on years of education, it has a positive impact on test scores (Black, Devereux and Salvanes 2011; Cook and Kang 2013).

---

[1] Black, Devereux and Salvanes (2011) also examine the impact of school entry laws on teenage motherhood in Norway. However, since the authors find no impact of school entry laws on completed schooling at age 27 or older, their paper does not provide evidence for whether education has a causal impact on teenage childbearing. There are two possible explanations for why the authors, unlike McCrary and Royer (2011), find no impact on years of education: a) the effects of school entry age are weaker in Norway than in the United States, and b) while individuals born after the cutoff date are more likely to drop out of high school, they are also more likely to have more years of higher education (Bedard and Dhuey 2006), potentially due to better school performance.

McCrary and Royer argue that the positive impact on test scores diminishes with age and is unlikely to be large enough to fully offset the negative impact on years of education. The literature on school entry age and test scores, however, finds strong effects that persist at least up to the eighth grade (Bedard and Dhuey 2006; Cook and Kang 2013; Elder and Lubotsky 2008). Using two nationally representative surveys, the Early Childhood Longitudinal Study and the National Educational Longitudinal Study, Bedard and Dhuey show that individuals born after state school entry cutoff dates have math test scores that are higher by 0.36 of a standard deviation in fourth grade and 0.28 of a standard deviation in eighth grade; similarly, using North Carolina administrative public school records, Cook and Kang show that they have higher reading test scores in sixth, seventh and eighth grade. Another key piece of evidence which points to the ambiguous impact of school entry age on human capital is the finding that being born after the cutoff date in California and Texas not only has no impact on fertility, but also has no impact on a wide range of future economic outcomes, including employment rates, wages and home ownership (Dobkin and Ferreira 2010), which is unlikely to be the case if school entry age is a valid instrument for educational success.[2]

This chapter tests the hypothesis that both years of education and test scores have negative impacts on teenage motherhood, and that school entry age is not

---

[2] While McCrary and Royer (2011) and Dobkin and Ferreira (2010) both use data from California and Texas, the former's sample is on females who gave birth before age 25, whereas Dobkin and Ferreira's sample is on males and females who completed the 2000 Decennial Census Long Form in both states (around 15% of the population).

associated with any observed changes in fertility because the positive impact on fertility due to decreased years of education is offset to a large extent by the negative impact on fertility due to higher test scores. To perform this test, I use a detailed administrative dataset from North Carolina which contains information on not only years of education but also school test scores. Using this dataset, I confirm that school entry age affects educational success in offsetting ways, with a negative impact on years of education and a positive impact on test scores, and that school entry age is not associated with any differences in teenage fertility. Next, I use an instrumental variable regression model with multiple endogenous variables to distinguish the impacts of years of education and test scores on teenage motherhood, and show that both measures of educational success have significant negative impacts, consistent with the evidence from compulsory schooling laws.

2.2 Model

I model an individual's teenage motherhood outcome as a function of her educational success and other attributes, including her birth outcomes and family, school and school district environmental characteristics. Her educational success is, in turn, modelled as a function of a) her exact age in days Age_i, measured as the number of days she was born after July 2 (negative for those born on July 1 or earlier), b) a dichotomous indicator $Cutoff_i$ for whether she was born after October 17, the North Carolina cutoff date for entering kindergarten and c) her birth outcomes and family, school and school district environmental characteristics $\boldsymbol{X}_i$ and cohort-fixed effects θ_{ci}. If date of birth is randomly assigned, Age_i and $Cutoff_i$ are uncorrelated with $\boldsymbol{X}_i$ and may be used as exogenous instruments for educational success. I discuss the validity of this assumption below.

To confirm that school entry age affects educational success in offsetting ways, with a negative impact on years of education and a positive impact on test scores, I follow McCrary and Royer (2011) and Cook and Kang (2013) and use a local regression model which assigns more weight to data points closer to the cutoff date (and zero weight to distant data points). Algebraically, the method chooses parameter values which minimize the following:

$$\sum_i (E_i - \beta_1 Cutoff_i - \beta_2 Age_i - \beta_3 Cutoff_i{*}Age_i - \beta_4 X_i - \theta_c)^2 \mathrm{K}_h(Age_i), \tag{1}$$

where E_i is a measure of educational success (years of education or test scores) and $\mathrm{K}_h(Age_i)$ is a kernel function which assigns weights to individual observations. Cook and Kang (2013) note that the triangular kernel below has desirable statistical properties:

$$\mathrm{K}_h(Age_i) = (1 - \frac{|Age_i|}{h})1\{\frac{|Age_i|}{h} < 1\}. \tag{2}$$

where h is the bandwidth or smallest number of days an individual is born before or after the cutoff date if she is assigned zero weight in the local regression model.

To confirm that school entry age is not associated with any differences in teenage fertility, I repeat the above analysis with teenage motherhood B_i, a dichotomous variable for whether the individual gave birth prior to age 20, as the dependent variable. I focus on teenage motherhood (rather than on fertility up to age 23, as McCrary and Royer do) for two reasons. First, the dataset used in this study does not contain information on fertility outcomes after age 19 for most cohorts. Second, fertility outcomes are less reliably measured at older ages, since there is less evidence that the individuals have not moved out of state.

The above regression discontinuity approach estimates the combined impacts of fewer years of education and higher test scores on teenage motherhood. Hence, if school entry age has no impact on teenage motherhood, it could be that neither years of education nor test scores affects teenage fertility, or that both educational outcomes affect teenage fertility, but in offsetting ways. Similarly, if school entry age has a positive or negative impact on teenage motherhood, it could be that either only years of education or test scores affects teenage fertility, or that both outcomes affect teenage fertility, but with differential impacts.

To distinguish the impacts of years of education and test scores on teenage motherhood, I use a multiple endogenous-variable instrumental variable regression approach. The dependent variable is teenage motherhood B_i, which depend on two endogenous variables (years of education and test scores). I use six instruments which are correlated with the two endogenous variables, but are unlikely to be correlated with teenage motherhood outcomes except through the two endogenous variables. The six instruments are: Age_i, $Cutoff_i$, and four interaction terms between $Cutoff_i$ and individual-level, school-level and school district-level characteristics (see Appendix B10). Algebraically,

$$B_i = \beta_0 + \beta_1 YearsEduc_i + \beta_2 TestScore_i + \beta_3 X_i + \theta_c + \varepsilon_i, \tag{3}$$

$$YearsEduc_i = \alpha_0 + \alpha_1 Age_i + \alpha_2 Cutoff_i + \alpha_3 Cutoff_i{*}X_i + \alpha_4 X_i + \theta_c + \eta_i, \tag{4}$$

$$TestScore_i = \gamma_0 + \gamma_1 Age_i + \gamma_2 Cutoff_i + \gamma_3 Cutoff_i{*}X_i + \gamma_4 X_i + \theta_c + \upsilon_i, \tag{5}$$

where $YearsEduc_i$ and $TestScore_i$ are years of education and test scores respectively. The inclusion of X_i is not optional since their interaction terms with $Cutoff_i$ are used as instruments.

There are two important issues with the above multiple endogenous-variable IV regression model. One major concern is whether the model is able to identify the individual impacts of the two endogenous variables (years of education and test scores), especially since years of education may itself be a function of test scores. Below, I show using graphical evidence and regression discontinuity analysis that the instruments Age_i

and $Cutoff_i$ have opposite impacts on the two endogenous variables, and that the first-stage IV regression results consistently reflect these opposite impacts in the expected signs. In addition, much of the instrumental variable regression results are based on data on third grade test scores, which are less likely to affect years of completed schooling. Hence, while the results of multiple endogenous-variable IV regression models are generally more difficult to interpret compared to single endogenous-variable models, the first-stage results and dual nature of school entry laws, which affects educational success in two opposite and offsetting ways, suggest that the approach may be especially appropriate in this particular case.

Second, an important assumption in the model is that the instruments Age_i and $Cutoff_i$, which depend on exact date of birth, are uncorrelated with observed or unobserved characteristics. Following previous work (Black, Devereux and Salvanes 2011; Cook and Kang 2013; McCrary and Royer 2011), I restrict my sample to girls born around the cutoff date and compare the results from three different bandwidths: 45 days, 60 days and 75 days. These bandwidths are comparable to those used in previous papers (30 in Black, Devereux and Salvanes 2011; 60 in Cook and Kang 2013; and 50 and 70 in McCrary and Royer 2011).[3] Below, I provide evidence that restricting the sample to these bandwidths removes much of the season of birth effects documented by Buckles

[3] While Cook and Kang (2013) show using a very similar dataset that the optimal bandwidth for the regression discontinuity analysis is around 10 according to the "Rule of Thumb" method proposed by Fan and Gijbels (1996), they also show that the results are qualitatively robust to bandwidth choice.

and Hungerman (2013), and that this is especially true in the case of North Carolina, potentially due to its milder winters. I show using balancing tests that Age_i and $Cutoff_i$ have little correlation with observed characteristics once I restrict the sample to these bandwidths, and also show that the regression discontinuity estimates are similar whether individual covariates $\boldsymbol{X}_i$ are controlled for or not, which supports the assumption that girls born before and after the cutoff date are comparable on both observed and unobserved characteristics. Finally, I conduct tests for weak instruments using the Cragg-Donald statistic recommended by Stock and Yogo (2002) for multiple endogenous-variable IV regression models, and for endogenous instruments using the Anderson-Rubin and Basmann statistics for the limited information maximum likelihood (LIML) estimator.

2.3 Data

The dataset in this study follows six North Carolina birth cohorts and consists of three components. The first component is each individual's birth certificate, which provides information about her birth outcomes and family background in terms of mother's race, age, educational level and marital status at the time of birth. The second component is each individual's public school administrative records, which provide information about whether she was registered in the public school system and her age at registration for every grade between third and twelfth grade, as well as her end-of-grade test scores in the third and eighth grades.[4] This component also provides information about her school and school district environments in terms of student poverty rates and the proportion of students who passed end-of-grade tests.[5] The third component is any North Carolina birth certificate which lists an individual in the first and second components as the mother (in the rare cases where there are multiple birth certificates, only data from the first birth certificate are used), which provides information about her age and educational attainment at the time of birth. The dataset is de-identified and was obtained from the North Carolina Education Research Data Center, which performed all data linkages at the individual level, with permission from the NC Department of Health and Human Services.

The six birth cohorts are born between 1987 and 1992, where 1987 is the first year when linked birth certificates are available from the North Carolina Education Research Data Center. Since data on the second component are available up to and including 2012, there are complete public school educational records for all cohorts up to age 20. However, since data on the third component are available only up to and including

---

[4] Unfortunately, this dataset does not contain information on high school graduation or GED certification. Instead, grade attendance indicated by student registration is used as a proxy for educational attainment.
[5] While most of the individuals in this sample attended third to eighth grade between1996 and 2007, estimates of school poverty rates, proportions of school students who passed end-of-grade tests and the number of crimes per 100 school students are based on 2005-2010, 2001-2010 and 2004-2010 data respectively; estimates of school district poverty rates, proportions of school district students who passed end-of-grade tests and proportions of school district students in single parent homes are based on 2004, 2002-2010 and 2004 data respectively. Around 0.4% of observations have missing values for school or school district characteristics; for these observations, missing values are imputed using other school and school district characteristics.

2009, there are complete teenage childbearing histories only for the first three cohorts (1987-1989). Hence, while the analysis on the impacts of school entry age on educational success uses data from all six birth cohorts, the analysis on teenage fertility uses data from the first three cohorts only.

Between 1987 and 1992, 276,614 female singleton live births were born in North Carolina to mothers residing in North Carolina. This sample excludes out-of-state births to North Carolina residents (4.1% of all births), which are slightly more likely to be born to younger, less-educated and unmarried white mothers (see Appendix B1). Of this original sample, I drop 10,918 observations (3.6%) who are neither non-Hispanic white nor non-Hispanic black (data on Hispanic ethnicity were not available in 1987 and all white and black individuals were included), and another 739 observations (0.3%) who have missing data for any of the following: birth parity, birth weight, mother's age, mother's education or mother's marital status at the time of birth. Most significantly, I drop 100,720 observations (36.4%) who did not attend public school in third grade, eighth grade or at age 15, the final age before individuals are legally allowed to drop out of school in North Carolina. (A small number of observations who attended charter schools were also dropped due to lack of data on test scores and school/school district characteristics.) Hence, the final sample excludes individuals who did not survive to teenage years (including 2,419 observations with infant death certificates) as well as individuals who attended private schools or moved out of state during these ages. These individuals are excluded not just to obtain a dataset with more complete educational data, but also because the remaining individuals are less likely to have moved out of state, so that any teenage births they might have are more likely to be captured by the third component of the dataset. I find no evidence that school entry laws affect the probability of being included in the final sample (discussed in greater detail below). The sample over-represents girls from disadvantaged socioeconomic backgrounds, with a higher proportion born to black or unmarried mothers (34.1% and 30.8%, compared to 29.6% and 27.9% for the original sample), who are at higher risk of teenage motherhood. The final sample size based on the largest bandwidth (75 days) is 68,771 for the analysis on educational outcomes and 33,598 for the analysis on teenage fertility.

The dataset in this study has several important advantages for studying the impact of education on teenage motherhood. Most crucially, it contains information on the number of grades each individual attended by age 20 as well as her performance in end-of-grade tests in the third and eighth grades, which allows me to distinguish the impacts of school entry age on years of education and test scores. All North Carolina public school students in the same grade in the same year take the same end-of-grade test. In this study, each individual's test score is represented by her Z-score relative to the performance of all students (including male and other female students not included in the sample) who took the test.[6] For greater efficiency, each individual's test score in the third or eighth grade is measured using the average of her verbal and math Z-scores. For individuals who repeat an end-of-grade test, the results from the first test are used. Around 1.6% and 1.3% of third and eighth grade test scores are missing; for these observations, missing values are imputed using the individual's test score in the other grade, her age at the time of the end-of-grade tests, the number of grades she completes and her age at school exit, as well as her birth outcomes, her family, school and school district characteristics and cohort effects. In addition, this dataset has other important advantages (described in detail in Appendix B2), including higher quality educational data which are more representative of individuals' final educational attainment; individual-level rather than cohort-level data on teenage childbearing outcomes, allowing for more unbiased and precise estimation; and more detailed information about characteristics at the individual and school and school district levels, allowing a more effective search for heterogeneous treatment effects.

Table 2 provides some summary statistics for all individuals born in 1987-1989, and for individuals born up to 60 days before and after the cutoff date. Within this comparatively small window, season of birth effects are small (but statistically

---

[6] Since test scores are represented by their Z-scores relative to the scores of all students who took the test, the average test score should be zero if the sample is representative of North Carolina public school students. For this dataset, average scores are 0.07 and 0.00 for reading and math in the third grade, and 0.06 and 0.01 for reading and math in the eighth grade. Here, the reading scores may be slightly higher because only non-Hispanic white and black girls born in North Carolina, who are unlikely to be taking English as a second language, are included.

significant). Compared to individuals born shortly after the cutoff date, individuals born shortly before the cutoff date are 0.9% more likely to born to a white or married mother, and have mothers who have on average 0.05 more years of age and education. Below, I provide some evidence that season of birth effects are relatively weak for the bandwidths used in this study, and that the estimates are robust to the effects.

Table 2: Sample background characteristics

	All individuals born in 1987-1992	Individuals born up to 60 days before the cutoff date	Individuals born up to 60 days after the cutoff date
Individual-level characteristics			
First-born child to mother (%)	44.45	44.46	44.45
Birth weight (g)	3273	3265	3270
Mother is white (%)	65.94	65.68	64.79
Maternal age at time of birth	25.38	25.45	25.40
Maternal education at time of birth	12.41	12.42	12.37
Mother was married at time of birth (%)	69.17	68.83	67.95
School-level characteristics			
% of students who are poor	55.44	55.48	55.67
No. of crimes per 100 students	0.583	0.583	0.584
% of students passed their end-of-grade tests	77.26	77.23	77.15
School district-level characteristics			
% of population who are poor	16.65	16.65	16.69
% of students with one parent	22.74	22.72	22.78
% of students passed their end-of-grade tests	77.61	77.63	77.57
Number of observations	164,237	27,798	27,195

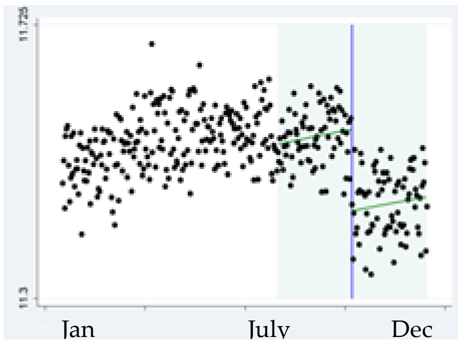
2.4 Results

2.4.1 Impacts of School Entry Age on Educational Attainment and Test Scores

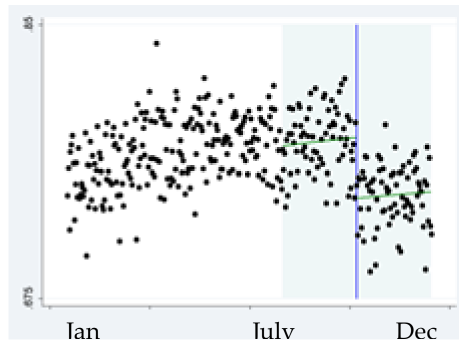
On average, girls in this sample attended 11.5 grades, with 76.4% attending 12th grade. Girls born up to 60 days before the cutoff date attended more grades on average (11.6, compared to 11.4) and were more likely to attend the 12th grade (77.7%, compared to 74.1%) than those born up to 60 days after the cutoff date. Figure 4 shows that the higher educational attainment among girls born before the cutoff date is entirely due to a sharp negative discontinuity in years of education at the cutoff date; apart from this break, the trend in educational attainment appears to be smooth and upwards sloping, so that girls born earlier in the year generally complete fewer rather than more years of schooling than their later born peers. (I discuss potential explanations for this upward trend later in this section.)

Table 3 provides estimates of the discontinuities at the cutoff date, where each figure is the estimate for the coefficient for being born after the cutoff date ($Cutoff_i$) in an individual regression. The results suggest that girls born immediately after the cutoff date attended 0.14 fewer grades, were 6% less likely to attend 12^{th} grade and had 0.19 fewer years of schooling than girls who were born a few days before them. Table 3 also shows that the estimates are fairly robust to bandwidth choice and are almost identical whether or not controls for individual, school and school district characteristics are included. Previous studies using discontinuities in school entry age also find that regression results are very similar whether or not controls are included (Black, Devereux and Salvanes 2011; Dobkin and Ferreira 2010; Elder and Lubotsky 2008).

A. No. of grades attended



B. Proportion attending 12th grade



C. No. of years of schooling (After third grade)

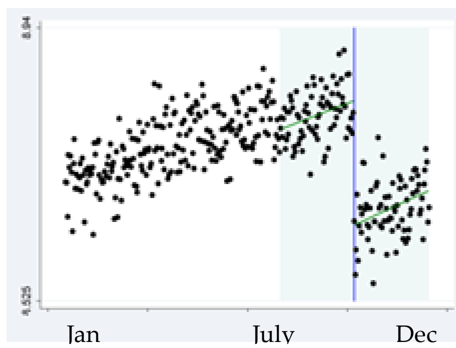


Figure 4: Years of education and highest grade attended by date of birth

Notes: The horizontal axis represents the date of birth, where zero refers to July 2. The dark blue line marks the cutoff date (October 16) and the light blue area shows the observations included in the 75-day window. The green lines on either side line of the cutoff date are separately computed using ordinary least squares for the 75-day bandwidth around the cutoff date.

Table 3: Impact of being born after the cutoff date on years of education and test scores (dependent variables in bold)

	45-day	60-day	75-day
Years of Education			
Grades attended			
No controls	-0.195	-0.132*	-0.130**
Controls included	-0.161	-0.131*	-0.137***
Attended 12th grade			
No controls	-0.079*	-0.059**	-0.052**
Controls included	-0.066	-0.059**	-0.055***
Years of schooling			
No controls	-0.261**	-0.191***	-0.188***
Controls included	-0.240**	-0.185***	-0.186***
Test scores			
Third grade			
No controls	0.279***	0.353***	0.368***
Controls included	0.310***	0.338***	0.345***
Eighth grade			
No controls	0.141	0.208***	0.224***
Controls included	0.173**	0.192***	0.200***
Number of observations	41,139	54,993	68,771

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.
Notes: Years of schooling refer to the number of years an individual was registered in the public pre-K12 system after third grade. Each value refers to the coefficient on being born after the school entry cutoff date from an individual regression, with the dependent variable given in bold. Coefficients are estimated using regression discontinuity models. Controls refer to individual, school and school district characteristics (see Table 2) and cohort fixed effects.

As expected, the estimates are similar to those in Cook and Kang (2013), who find a negative discontinuity of 4.45 percentage points in the probability of attending 12th grade in North Carolina. While the estimates are also similar to those in McCrary and Royer (2011), who find discontinuities in years of education on the order of -0.14 and -0.24 for California and Texas respectively, they are substantially larger than those in Dobkin and Ferreira (2010), who find only a discontinuity of 1 percentage point in the probability of completing 12th grade, also for Texas and California. Out of curiosity, I repeat the analysis using only birth certificate data, similar to McCrary and Royer. (I restrict my sample to girls who gave birth up to age 18 rather than age 23 for increased comparability, since the administrative educational data do not go beyond high school.) The discontinuities are substantially larger for this restricted sample (see Appendix B3), suggesting that the impact of school entry laws on educational attainment may be weaker in California and Texas than in North Carolina, possibly due to the higher minimum dropout age (18, compared to 16) or to other demographic and state-level differences.

In stark contrast to the negative impact of school entry age on education attainment, the impact on test scores is positive and large. Girls born up to 60 days before the cutoff date had lower average test scores not only in third grade (-0.09 of a standard deviation, compared to 0.15) but also in eighth grade (-0.04, compared to 0.09).[7] Figure 5 shows trends which are almost exactly opposite to those given in Figure 4: the trends in test scores are smooth and downwards sloping, so that girls born earlier in the year generally had higher scores than their later born peers, with sharp positive discontinuities at the cutoff date. Table 3 above provides estimates of these discontinuities, which are on the order of 0.35 and 0.2 of a standard deviation in third and eighth grade respectively, and are once again fairly robust to bandwidth choice and

[7] The discontinuities are comparable for reading and math (0.23 and 0.26 of a standard deviation in third grade, and 0.14 and 0.13 in eighth grade). The smaller discontinuities in eighth grade may be partly due to the fact that girls born before the cutoff date are more likely to be retained between ages 11 and 15 (Cook and Kang 2013), reducing the "intention-to-treat" effects.

the inclusion of controls. The results are consistent with Bedard and Dhuey (2006), who find discontinuities of 0.36 and 0.28 of a standard deviation in fourth grade and eighth grade math for the United States, as well as with Cook and Kang (2013) and Elder and Lubotsky (2008), who also find that the impact of school entry age persists at least up to the eighth grade.

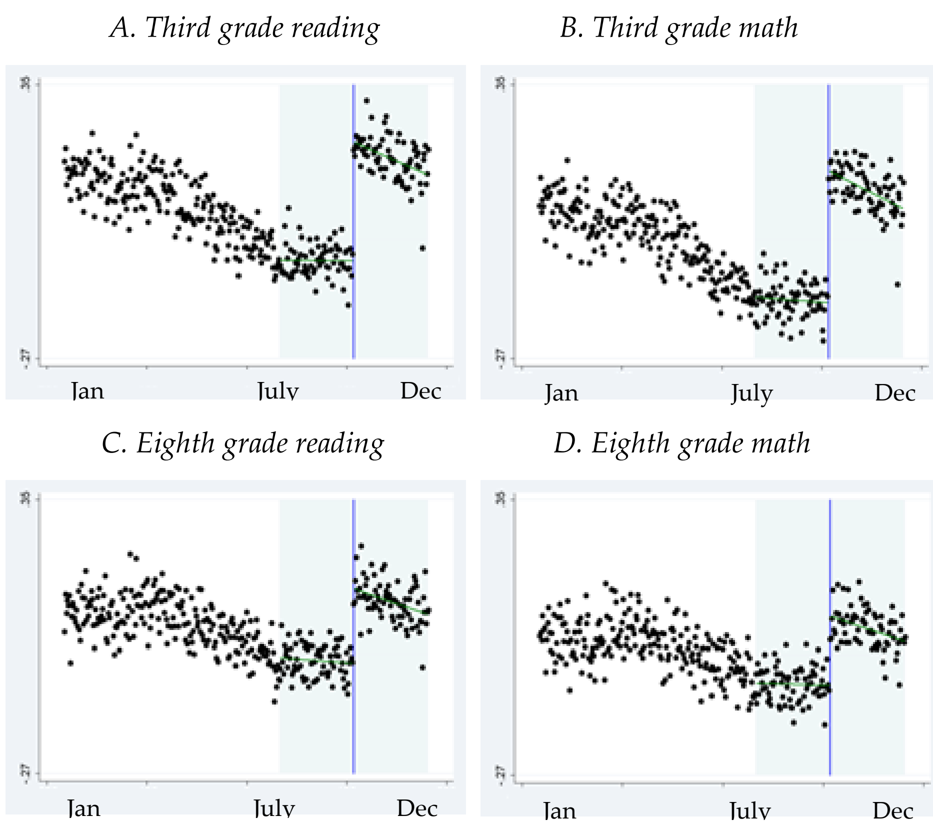


Figure 5: Third and eighth grade test scores by date of birth

Notes: The horizontal axis represents the date of birth, where zero refers to July 2. The dark blue line marks the cutoff date (October 16) and the light blue area shows the observations included in the 75-day window. The green lines on either side line of the cutoff date are separately computed using ordinary least squares for the 75-day bandwidth around the cutoff date.

Table 4 shows that the discontinuities in years of education and test scores are generally larger for socioeconomically advantaged groups, in large part due to higher rates of compliance with school entry laws (see Appendix B4). This is also consistent with McCrary and Royer (2011), who find larger effects for whites than for blacks, as well as with Elder and Lubotsky (2008), who find larger and more persistent effects on test scores among higher socioeconomic status groups. An exception to the above finding is that the impact on educational attainment is weaker for girls born to more educated mothers, who have very low dropout rates regardless of whether they were born before or after the cutoff date. The results also suggest that institutional factors matter: while test score discontinuities in third grade are similar in low-poverty and high-poverty school and school districts, they diminish less rapidly in low-poverty settings, possibly due to higher access to gifted programs and other forms of ability tracking.

Table 4: Heterogeneous impacts of being born after the cutoff date on years of education and test scores (dependent variables in bold)

	Mother is white	Mother is older	Mother is more educated	Mother is married	Low-poverty school	Low-poverty school district
Grades attended	-0.189***	-0.177***	-0.104	-0.144**	-0.113*	-0.136**
Attended 12th grade	-0.080***	-0.064**	-0.029	-0.056**	-0.042	-0.047
Years of schooling	-0.254***	-0.227***	-0.130*	-0.193***	-0.159***	-0.229***
Third grade test scores	0.383***	0.440***	0.428***	0.387***	0.344***	0.356***
Eighth grade test scores	0.241***	0.298***	0.278***	0.266***	0.247***	0.229***
Number of observations	44,778	36,510	22,356	46,963	33,973	36,899
	Mother is black	Mother is younger	Mother is less educated	Mother is unmarried	High-poverty school	High-poverty school district
Grades attended	-0.043	-0.098	-0.158**	-0.141	-0.160**	-0.133*
Attended 12th grade	-0.007	-0.047	-0.070**	-0.059	-0.066**	-0.060*
Years of schooling	-0.068	-0.144*	-0.220***	-0.186**	-0.214***	-0.130*
Third grade test scores	0.268***	0.235***	0.305***	0.257***	0.342***	0.333***
Eighth grade test scores	0.113**	0.086	0.163***	0.058	0.149**	0.165**
Number of observations	23,993	32,261	46,415	21,808	34,798	31,872

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.
Notes: Years of schooling refer to the number of years an individual was registered in the public pre-K12 system after third grade. Each value refers to the coefficient on late entry from an individual regression, with the dependent variable given in the first column. Coefficients are estimated using regression discontinuity models and the 75-day window, controlling for cohort fixed effects.

The evidence thus far strongly suggests that school entry age affects educational success in offsetting ways, with girls born shortly after the cutoff date having lower educational attainment but significantly better test scores in third and eighth grade. While it may be expected that girls born after the cutoff date would have higher test scores due to their age advantage or, alternatively, to increased human capital accumulation prior to entering kindergarten (Elder and Lubotsky 2008), it may seem surprising that their better academic performance does not go hand-in-hand with lower dropout rates. One important reason for their higher dropout rates is that girls who start school at an older age reach the minimum dropout age after completing fewer years of education (Angrist and Krueger 1991; McCrary and Royer 2011). This explanation does not provide a full account, since relatively few girls exited the public school system before ninth grade (see Appendix B5), even though the majority of girls born after the cutoff date would have been legally allowed to do so. Cook and Kang (2013) suggest that the higher dropout rates may instead be due to cumulative effect of increased exposure to the option of dropping out, while Dobkin and Ferreira (2010) theorize that they may be due to biological age effects. In either case, Appendix B5 reveals that girls born after the cutoff date had lower or equal dropout rates *at each age*, so that higher test scores are in fact associated with higher probabilities of grade progression.

2.4.2 Impacts of School Entry Age on Teenage Motherhood

Unlike the large impacts of being born after the cutoff date on educational outcomes, there is little evidence that being born after the cutoff date affects teenage fertility. For the first three birth cohorts, for whom there are complete teenage fertility histories, 18.7% had a teenage birth, with only 3.2% giving birth before age 17 (where the conception of most of these births would have occurred prior to the North Carolina minimum dropout age). The probabilities of giving birth before ages 20 and 17 are almost identical among girls born up to 60 days before and after the cutoff date (19.1%

and 3.3%, compared to 19.0% and 3.2%). Figure 6 shows that there are virtually no differences between the fertility schedules of the two groups, and Tables 5 and 6 show that the similarity in fertility outcomes extends to all windows and subgroups.[8] The evidence is consistent with Cook and Kang (2013) and McCrary and Royer (2011), although the discontinuity point estimates of around -2.5% are larger than the statistically significant estimates of -1.8% in Black, Devereux and Salvanes (2011).

A. Before age 20 *B. Before age 17*

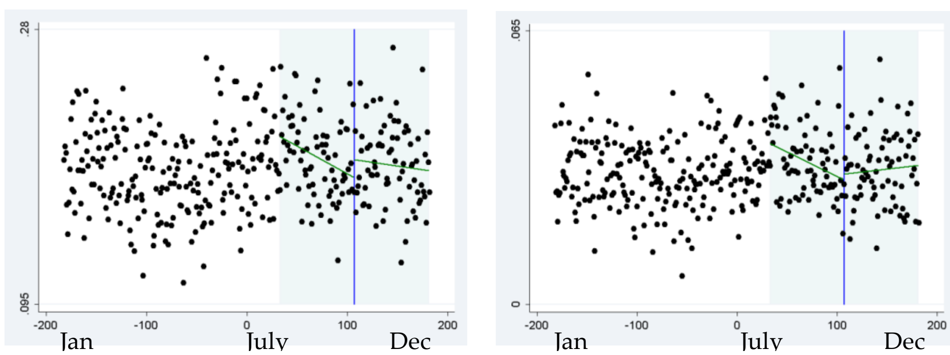


Figure 6: Proportions of teenage mothers by date of birth

Notes: The horizontal axis represents the date of birth, where zero refers to July 2. The dark blue line marks the cutoff date (October 16) and the light blue area shows the observations included in the 75-day window. The green lines on either side line of the cutoff date are separately computed using ordinary least squares for the 75-day bandwidth around the cutoff date.

---

[8] Cook and Kang (2013) find that school entry age reduces youth criminality among males at younger ages but increases it at older ages, and argue that the effect of higher test scores may dominate at younger ages while the effect of fewer years of education may dominate past the minimum dropout age. Unlike the authors, I find no differences between outcomes at younger and older ages, possibly due to the low incidence of childbearing at younger ages. The estimates are not significant for the 60-day and 75-day windows even when rectangular kernels are used, suggesting that the results are not due to insufficient power. A corollary of this result is that the discontinuity estimates in Table 2 reflect only the impacts of school entry age and are not due to any additional impacts caused due to changes in fertility timing (see, e.g., Kane et al. 2013).

Table 5: Impact of being born after the cutoff date on teenage childbearing (dependent variables in bold)

	45-day	60-day	75-day
Motherhood by age 20			
No controls	-0.024	-0.022	-0.021
Controls included	-0.049	-0.034	-0.025
Number of observations	20,022	26,844	33,598
Motherhood by age 17			
No controls	-0.017	-0.015	-0.013
Controls included	-0.018	-0.014	-0.011
Number of observations	41,139	54,993	68,771

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: Each value refers to the coefficient on late entry from an individual regression, with the dependent variable given in bold. Coefficients are estimated using regression discontinuity models. . Controls refer to individual, school and school district characteristics (see Table 2) and cohort fixed effects.

Table 6: Heterogeneous impacts of being born after the cutoff date on teenage childbearing

	Mother is white	Mother is older	Mother is more educated	Mother is married	Low-poverty school	Low-poverty school district
	-0.040	-0.026	-0.022	-0.020	-0.046	-0.034
Number of observations	21,969	17,754	10,532	23,803	16,306	17,885
	Mother is black	Mother is younger	Mother is less educated	Mother is unmarried	High-poverty school	High-poverty school district
	0.008	-0.021	-0.026	-0.033	-0.004	-0.014
Number of observations	11,629	15,844	23,066	9,795	17,292	15,713

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: Each value refers to the coefficient on late entry from an individual regression, with the dependent variable given in the first column. Coefficients are estimated using regression discontinuity models and the 75-day window, controlling for cohort fixed effects.

2.4.3 Impacts of Educational Attainment and Test Scores on Teenage Motherhood

I now turn to the results for the instrumental variable regression model, which distinguishes the simultaneous impacts of years of education and test scores on teenage motherhood. As discussed in the Model section, I use six instruments for the two endogenous variables (years of education and test scores): a) the individual's exact age in days Age_i, b) a dichotomous indicator for whether she was born after the kindergarten entry cutoff date $Cutoff_i$ and c) interaction terms between $Cutoff_i$ and four individual-level characteristics.[9] Table 7 shows that the first-stage IV regression results are consistent with the above regression discontinuity results, with a small positive coefficient for Age_i and a large negative coefficient for $Cutoff_i$ when regressed on years of education, but a small negative coefficient for Age_i and a large positive coefficient for $Cutoff_i$ when regressed on test scores. Hence, there is some evidence that the instruments identify the individual impacts of the two endogenous variables. Appendix B6 shows first-stage results when other measures of educational attainment and test scores are used (i.e. years of schooling and eighth grade test scores), as well as the results of diagnostic tests for weak and endogenous instruments, based on the Cragg-Donald statistic and the Anderson-Rubin and Basmann statistics respectively. The results of the tests are summarized in Table 8, which also presents the main results.

---

[9] For the IV regression analysis, student poverty rates at the school and school district levels are represented by binary rather than continuous variables, which take the value of one if the poverty rate is above the median level (around 55% and 16% at the school and school district levels respectively).

Table 7: IV regression first stage results

	45-day	60-day	75-day
	Endogenous variables: educational attainment only		
Grades attended			
No. of days born after July 2	0.000	0.000	0.001**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	-0.058	-0.080**	-0.112***
	(0.041)	(0.039)	(0.034)
Attended 12th grade			
No. of days born after July 2	0.000	0.000*	0.000**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	-0.038*	-0.043**	-0.051***
	(0.021)	(0.018)	(0.016)
	Endogenous variables: educational attainment and 3rd grade test scores		
Grades attended			
No. of days born after July 2	0.000	0.000	0.001**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	-0.058	-0.080**	-0.112***
	(0.041)	(0.039)	(0.034)
3rd grade test scores			
No. of days born after July 2	-0.000	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)
Born after cutoff date	0.287***	0.293***	0.265***
	(0.034)	(0.031)	(0.028)
Attended 12th grade			
No. of days born after July 2	0.000	0.000*	0.000**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	-0.038*	-0.043**	-0.051***
	(0.021)	(0.018)	(0.016)
3rd grade test scores			
No. of days born after July 2	-0.000	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)
Born after cutoff date	0.287***	0.293***	0.265***
	(0.034)	(0.031)	(0.028)

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: Each pair of values in the table refers to the coefficients on years of education and test scores from an individual regression, with the probability of motherhood by age 20 as the dependent variable and the endogenous variables given in italics. Coefficients are estimated using IV regression models, controlling for individual, school and school district characteristics (see Table 2) and cohort fixed effects.

Table 8: Impact of years of education and test scores on teenage childbearing

	45-day	60-day	75-day
Endogenous variables: educational attainment only			
Grades attended	-0.050	0.002	0.003
Evidence of weak instruments	No	No	No
Evidence of endogenous instruments	No	No	Yes
Attended 12th grade	-0.156	-0.058	-0.061
Evidence of weak instruments	No	No	No
Evidence of endogenous instruments	No	No	Yes
Years of schooling	-0.019	0.011	0.021
Evidence of weak instruments	No	No	No
Evidence of endogenous instruments	No	No	Yes
Endogenous variables: educational attainment and 3rd grade test scores			
Grades attended	-0.221	-0.158	-0.314**
Third grade test scores	-0.074	-0.070	-0.138**
Evidence of weak instruments	Yes	Yes	Yes
Evidence of endogenous instruments	No	No	No
Attended 12th grade	-0.228	-0.158	-0.228**
Third grade test scores	-0.018	-0.028	-0.050**
Evidence of weak instruments	No	No	No
Evidence of endogenous instruments	No	No	No
Years of schooling	0.409	0.007	-0.278
Third grade test scores	0.277	-0.002	-0.190
Evidence of weak instruments	Yes	Yes	Yes
Evidence of endogenous instruments	No	No	No
Endogenous variables: educational attainment and 8th grade test scores			
Grades attended	-0.461	-0.220	-0.324**
Eighth grade test scores	-0.309	-0.169	-0.251**
Evidence of weak instruments	Yes	Yes	Yes
Evidence of endogenous instruments	No	No	No
Attended 12th grade	-0.250	-0.153	-0.227**
Eighth grade test scores	-0.040	-0.049	-0.089**
Evidence of weak instruments	No	No	No
Evidence of endogenous instruments	No	No	No
Years of schooling	2.366	-0.048	-0.292
Eighth grade test scores	2.611	-0.067	-0.350
Evidence of weak instruments	Yes	Yes	Yes
Evidence of endogenous instruments	No	No	No
Number of observations	20,022	26,844	33,598

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: Years of schooling refer to the number of years an individual was registered in the public pre-K12 system after third grade. Each pair of values in the table refers to the coefficients on years of education and test scores from an individual regression, with the probability of motherhood by age 20 as the dependent variable and the endogenous variables given in italics. Coefficients are estimated using IV regression models, controlling for individual, school and school district characteristics (see Table 2) and cohort fixed effects.

Each pair of figures in Table 8 provides the estimated coefficients on years of education and test scores from an individual IV regression, where teenage motherhood is the dependent variable. When test scores are not included as an endogenous variable, the coefficients for years of education are small and statistically insignificant, with some evidence of instrument endogeneity. Once test scores is included as an endogenous variable to account for the offsetting impacts of time of birth on educational success, the coefficients for years of education and test scores become large and negative, and they are statistically significant for two of the three measures of educational attainment when the largest bandwidth is used. There is no evidence of instrument endogeneity after accounting for the impact on test scores, consistent with balancing test results showing that $Cutoff_i$ and Age_i have little correlation with individual, school and school district characteristics (see Appendix B7). Below, I present evidence and explanations for why seasonality effects are likely to be especially weak for this sample. However, only model specifications which use 12th grade attendance as the measure of years of education pass the test for weak instruments based on the Cragg-Donald statistic. Stock and Yogo (2002), who recommend this test, note that it is overly conservative (and hence less likely to reject the null hypothesis that the instruments are weak) when there are multiple endogenous variables. All IV regression results are produced using the LIML estimator, which is more robust to weak instruments (Staiger and Stock 1997; Stock and Yogo 2002) at the cost of larger standard errors.

The IV regression results suggest that both years of education and test scores have negative impacts on teenage motherhood. In particular, attending 12th grade leads to a reduction in the probability of teenage childbearing by around 23 percentage points. This estimate, while large, may be plausible since most school leavers get pregnant or give birth after, rather than prior to, dropping out of school (Mott and Marsiglio 1985; Upchurch and MacCarthy 1990).[10] Since around 36% of girls who did not attend 12th grade became teenage mothers, the results suggest that universal high school graduation would cut teenage childbearing rates to less than half their current levels.

Finally, using both the regression discontinuity and IV regression results, I decompose the overall impact of being born after the cutoff date on teenage motherhood into two components: a) the impact of being born after the cutoff date on the probability of attending 12th grade, multiplied by the impact of attending 12th grade on teenage motherhood, and b) the impact of being born after the cutoff date on third grade test scores, multiplied by the impact of third grade test scores on teenage motherhood. The

---

[10] Using data from the 1979 National Longitudinal Survey of Youth, Upchurch and MacCarthy (1990) estimate that only 5% of women aged 21-29 gave birth prior to dropping out or graduating from high school, and that among the 17.8% of women who dropped out of school before giving birth, 66.4% gave birth before graduating from high school (the vast majority of the sample eventually graduated from high school). Using the same dataset, Mott and Marsiglio (1985) find that among women aged 20-26, only 6% gave birth prior to dropping out or graduating from high school (another 3% were pregnant), and that among those who dropped out of school, 29% were pregnant or had given birth before dropping out, and 44% gave birth only after dropping out.

point estimates indicate that the two components are approximately 0.013 and -0.018 respectively, resulting in a small overall impact of -0.005 on the probability of teenage childbearing.

In summary, while the results support McCrary and Royer (2011) and Cook and Kang (2013) in finding that school entry laws are not associated with changes in teenage childbearing, they also suggest, consistent with the evidence from preschool interventions and compulsory schooling laws, that years of education and test scores have negative impacts on teenage motherhood. The overall impact of school entry laws is very small since they reduce the probability that girls born after the kindergarten entry cutoff date complete high school but increase the girls' test scores; however, other educational policies, such as extending the school day or school year, are likely to increase both years of education and test scores, and to reduce teenage childbearing.

2.5 Discussion

This chapter addresses the contradictory findings from quasi-experimental studies as to whether years of education have a negative causal impact on teenage childbearing. In particular, while studies using compulsory schooling laws show a significant impact, studies using school entry laws find no causal relationship. This study seeks to reconcile these contradictory findings by re-examining the impacts of school entry laws using a North Carolina administrative dataset which includes not only birth certificate data but also individually linked school administrative records.

Consistent with previous research (Cook and Kang 2013), the evidence suggests that being born after the school entry cutoff date affects educational success in offsetting ways, with a negative impact on years of education but positive impact on test scores. Using an IV regression strategy to distinguish the impacts of years of education and test scores, I show that both measures of educational success have negative causal impacts on teenage childbearing.

A key assumption in this study is that there are only minor season of birth effects within the restricted sample (girls born up to 75 days before and after the cutoff date), so that girls born before and after the cutoff date are otherwise comparable. The data suggest that this assumption is especially plausible in the case of North Carolina, where the season of birth effects between autumn and winter are relatively weak, although there continues to be a strong spike in the proportion of white and married mothers during summer (see Appendices B8 and B9). This is fortunate for the purposes of this study since the restricted sample is born between the end of summer and the beginning of winter (from early August to late December for the largest bandwidth). One likely explanation for the weaker seasonality effects in North Carolina is that season of birth effects are predominantly driven by expected temperatures at the time of birth, where socioeconomically advantaged women are more likely to give birth during warmer months and less likely to do so during the colder months (Buckles and Hungerman 2013), and North Carolina winters are substantially milder than in the most of the

United States. This explanation is consistent with Fan, Liu and Chen (2014), who find much weaker birth seasonality effects in Taiwan compared to the United States and attribute this to its warmer winters.

Even so, within the restricted sample, differences in maternal characteristics between girls born before and after the cutoff date (see Table 2) are statistically significant, although the differences are no larger for the 75-day bandwidth than for the two smaller bandwidths. There is some evidence of discontinuities in maternal race and marital status (individuals born after the cutoff date are 3.4% and 4.4% more likely to have a white and married mother respectively, significant at the 10% level for the 75-day window), but not for maternal age or education. However, the regression discontinuity estimates are robust to the inclusion of controls, similar to previous papers using discontinuities in school entry age (Black, Devereux and Salvanes 2011; Dobkin and Ferreira 2010; Elder and Lubotsky 2008), and there is no significant evidence of instrument endogeneity for any of the IV regression model specifications once test scores are accounted for, suggesting that the season of birth effects may be orthogonal to the impact of school entry age on educational and fertility outcomes.

I also test for selective sample attrition among girls born after the cutoff date, which may occur if, for example, being born after the cutoff date leads to higher test scores and a higher probability of transferring to private or out-of-state schools. Using local regression, I find no evidence of a discontinuity at the cutoff date in the probability of being included in the sample, similar to Cook and Kang (2013) who use a similar dataset.

Next, I conduct a number of robustness checks on the regression discontinuity and IV regression results. Appendix B10 shows that the regression discontinuity estimates are similar whether or not imputed values for observations with missing data on grades attended or test scores are used. To check whether the IV regression results are driven by any single instrument, I compare the results when each of the instruments is excluded. Appendix B11 shows that the estimates vary in statistical significance but are consistently large and negative. The point estimates vary significantly in magnitude when cohort interaction terms with $Cutoff_i$ are excluded, and the specification does not pass the test for weak instruments. Hence, the most rigorous and precise estimates are based on model specifications where all the instruments are included. Given this constraint on the covariates that can be excluded and the imprecision of the estimates when the sample is restricted to individual subgroups, a discussion of heterogeneous effects for this part of the analysis is not provided.

Finally, following previous related work (Cook and Kang 2013; McCrary and Royer 2011), this study assumes that school entry laws affect teenage motherhood primarily through their impacts on educational success, rather than through differences in relative peer characteristics or other age-related effects. In support of this assumption, Table 3 shows that the regression discontinuity estimates are robust to the inclusion of controls for peer characteristics at the school and school district levels. In addition, I repeat the analysis for childbearing at age 20 (conditional on not having a teenage birth), which are likely to be less influenced by school peer characteristics than at teenage ages. The results are similar to those for teenage childbearing, albeit statistically insignificant with the smaller sample sizes (see Appendix B12), suggesting that the bias from omitting differences in relative peer characteristics is likely to be small.[11] This is consistent with research showing that changes in relative peer characteristics due to school entry age have ambiguous impacts on educational and other outcomes, since having younger peers is associated with lower test scores but also with lower probability of diagnosed with a learning disability (Elder and Lubotsky 2008), lower exposure to risky peer behaviors and higher self-confidence (Black, Devereux and Salvanes 2011).

[11] These estimates are based only on data from the first two cohorts (born in 1987-1988), since data on fertility at age 20 are not available for the third cohort (born in 1989). The similarity of the results for teenage motherhood and childbearing at age 20 provides some inconclusive evidence for the argument that the negative impact of educational attainment on teenage motherhood is due to increased human capital accumulation rather than an "incarceration effect", consistent with evidence from compulsory schooling laws (Black, Devereux and Salvanes 2008).

3. Babies Having Fewer Babies: The Role of Social Policies and Cohort Effects in the 1990s-2000s

3.1 Introduction

The U.S. teenage birth rate, defined as the number of births for every 1,000 women aged 15 to 19, fell by more than a third (from 62 to less than 40) between 1991 and 2010. The decline was even more dramatic among black teenagers, with the birth rate falling by more than half (from 112 to 50) during this period. Figure 1 in the first chapter shows that the decline was preceded by a steep rise in the birth rate between 1988 and 1990, and that the birth rate stabilized briefly between 2005 and 2008 before plunging into an even more precipitous drop (Kearney and Levine 2014).[1] This chapter examines the causes of trends in the U.S. teenage birth rate up to 2010. In particular, it seeks to examine the extent to which the teenage baby bust is an unintended and unforeseen consequence of social policies in the 1960s-1980s, namely policies regulating access to the birth control pill and to abortion, as well as changes in divorce laws.

As noted in the first chapter, previous research strongly suggests that the teenage baby bust was caused by lower teenage pregnancy risks, rather than by increased use of abortion. Hogan, Sun and Cornwell (2000) find that the decline in teenage pregnancy risk due to increased contraceptive use is especially steep among whites and groups

---

[1] Kearney and Levine (2014) estimate that the teenage birth rate fell by 2.5% per year between 1991 and 2008, and by 7.5% per year between 2008 and 2012. They argue that causes of the decline are likely to differ for these two periods, with the Great Recession playing a large role for the latter period.

with higher levels of parental education, parental co-residence and church attendance. What remains unclear, however, is *why* teenagers are less likely to engage in sexual activity, and more likely to use contraceptives when they do.

Previous studies have focused on two fundamentally different sets of explanations for the teenage baby bust: "cohort" factors and "period" factors. Cohort factors cause individuals born in certain years (in this case, women born in the 1970s-1980s who became teenagers in the 1990s-2000s) to behave differently from previous birth cohorts. Donohue, Grogger and Levitt (2009) and Ozbeklik (2006) focus specifically on the impact of legalized abortion for adult women in the early 1970s, which reduced the proportion of teenagers in the 1990s who were unwanted at the time of birth. (Cohort factors, however, need not refer exclusively to events occurring around the time of birth, and include events which affect only a specific age group.) Hence, they hypothesize that the teenage baby bust occurred partly because the *cohorts born in the 1970s are different from previous cohorts* in terms of family background or wantedness at the time of birth, which may in turn lead to poorer child outcomes. (The impact of wantedness at time of birth on later adolescent outcomes, which may operate through unexpected stress on family resources or other mechanisms, is discussed in greater detail in the Model section below.)

Period factors, on the other hand, cause individuals who are at risk of teenage childbearing in certain years (in this case, women aged 15-19) to behave differently from individuals who were at risk in previous years. Period factors may be distinguished from cohort factors in that they simultaneously (and equally) affect the behavior of multiple birth cohorts during a single year. Kearney and Levine (2013) examine the impact of a range of policy changes in the 1990s-2000s, including welfare reform and increased access to family planning through Medicaid. Hence, they hypothesize that the teenage baby bust occurred partly because the *conditions for teenage childbearing are different in the 1990s and 2000s*.

Neither set of explanations has been able to put forth a convincing full account of the teenage baby bust. Donohue, Grogger and Levitt (2009) find that legalization of abortion for adult women in the 1970s accounts for only around 25% of the decline in the 1990s. More troublingly, while their story suggests that women born shortly after the policy reform should be less likely to become teenage mothers due to positive selection in terms of family background or wantedness at time of birth (see the discussion in the Model section below), the analysis in this chapter shows that the reverse is true, i.e. women born in the early 1970s are slightly *more* likely to become teenage mothers than earlier cohorts. Kearney and Levine (2013), on the other hand, find that policy changes in the 1990s and 2000s account for only 12% of the decline between 1991 and 2008 (the proportion is even smaller for black women), and conclude that "no policy or other environmental factor can be pinpointed as contributing substantially to the decline" (Kearney and Levine 2012). As discussed in the first chapter, their results suggest that only welfare reform and increased access to family planning through Medicaid are associated with declines in teenage fertility, with no evidence that sex education programs (including education on contraception), changes in minors' access to abortion, the Minor Parent Provisions program, the Child Support Enforcement program or the Children's Health Insurance Program had any impact. Their work is consistent with Kaestner, Korenman and O'Neill (2003), who also find that welfare reform reduced marriage and childbearing among older teenagers, and with Blank (2007), whose literature review suggests that the impact of welfare reform on teenage birth rates is likely to be small.

This chapter follows the first line of investigation: that the teenage baby bust occurred partly because the cohorts themselves are different from previous cohorts. I begin by using age-period-cohort analysis to investigate the extent to which the decline in teenage birth rates reflects changes in cohort behavior rather than simultaneous declines across multiple birth cohorts. I compare the results from three different approaches: a graphical method proposed by Cook and Laub (2002), as well as two commonly used models in age-period-cohort analysis, the equality-constraint estimator and the relatively new intrinsic estimator. I also test whether the results are robust to the inclusion of data on early adult childbearing (at ages 20 to 24). Next, I use a difference-in-differences model to investigate the extent to which social policies could have caused teenagers born in the 1970s-1980s to behave differently from previous birth cohorts. I compare the results from two specifications: the first controls only for state of residence, similar to previous work, and the second controls for both state of residence and state of birth. The social policies examined in this study are: access to the *birth control pill*; legalization of *abortion*; adoption of *unilateral divorce* laws; and adoption of *equitable division of property* divorce laws.

3.2 Cohort Factors: Social Policies in the 1960s-1980s

Table 9 below shows the timeline of these four sets of social policies. In the next section, I discuss how the policies may cause cohorts to be at lower risk of teenage childbearing than previous birth cohorts. For now, I review existing evidence on how these policies have affected cohort outcomes.

Table 9: Timeline of social policy changes

	1960s	1970s	1980s
Legalization of the birth control pill	• 49 states had legal access to the pill for women aged 21 or above by 1965. • 13 states had legal access to the pill for women aged 18-20 by 1965; this increased to 15 states by 1969.	• 35 states legalized the pill for women aged 18-20 between 1971 and 1974; another state legalized it in 1977. • 17 states legalized the pill for women aged 15-17 between 1971 and 1973; another 8 states legalized it between 1974 and 1977.	

Legalization of abortion	• 5 states legalized abortion for women aged 21 or above between 1970 and 1971; 44 states legalized it in 1973; and 1 state legalized it in 1974. • The timeline is similar for abortion for women aged 18-20. • 15 states legalized abortion for women aged 15-17 between 1970 and 1973; another 20 states legalized it between 1974 and 1979.	
Unilateral divorce laws	21 states adopted unilateral divorce laws between 1970 and 1973; another 6 states adopted them between 1974 and 1977.	
Equitable division of property laws	20 states adopted equitable division of property laws between 1970 and 1979.	8 states adopted equitable division of property laws between 1980 and 1988.

Notes: Data on the timing of legal access to the birth control pill and abortion are taken from Myers (2012), while data on timing of divorce laws are taken from Rasul (2003).

The studies most relevant to this study are Donohue, Grogger and Levitt (2009) and Ozbeklik (2006), who investigate the impact of legalized abortion for adult women in the early 1970s on teenage childbearing. The two papers make use of different instruments: Donohue, Grogger and Levitt use state abortion rates, while Ozbeklik uses the timing of state abortion legalization. Both papers find that states which had higher abortion rates or which legalized abortion earlier have lower teenage fertility in the next generation. Donohue, Grogger and Levitt estimate that increased access to abortion accounts for around 25% of the teenage baby bust in the 1990s, while Ozbeklik's estimates are somewhat larger, especially for black women.

There are several compelling elements in these studies. The authors find larger effects for black women, consistent with Figure 1. Moreover, if teenage childbearing and juvenile criminal activity share the same antecedents, namely childhood poverty, parental neglect and abuse, lower maternal age and larger family sizes, their hypothesis is also consistent with the contemporaneous fall in cohort juvenile crime rates among males in the 1990s (Sanders 2011). Indeed, abortion legalization did result in a lower proportion of children living in poverty or born to teenage mothers (Gruber et al. 1999; Levine et al. 1999).

However, the studies also leave several puzzles unsolved. First, the authors find that legalization of abortion for adult women accounts for only a fraction of the teenage baby bust. Second, while their story suggests that women born shortly after the policy reform should be less likely to become teenage mothers, the analysis in this study shows that the reverse is true, i.e. women born in the early 1970s are slightly *more* likely to become teenage mothers than earlier cohorts. Finally, the studies mostly consider only the short run impacts of abortion legalization through reducing the proportion of unwanted births. However, abortion legalization could also have resulted in cohort compositional changes in the long run. Ananat et al. (2009) show that pregnancy and abortion rates remained higher in early repeal states for a number of years even after *Roe v. Wade*, a 1973 Supreme Court decision which provided women with the right to terminate a pregnancy through induced abortion, and argue that the lower social costs of abortion in early repeal states resulted in more positive selection into childbearing in the long run, by allowing women to increase their number of pregnancies and to make use of increased information regarding the consequences of childbearing after pregnancy. They provide some evidence of these long run effects, including higher rates of college graduation and lower rates of welfare use in the next generation. Consistent with this, Ozbeklik (2006) finds that the impact of being born in an early repeal state (i.e., a state which decriminalized abortion before 1973) on teenage fertility continues to be significant for cohorts born after *Roe v.* Wade (in some cases, the long run effects were even larger than the short run effects).

Abortion legalization coincided with a number of other social policies in the 1970s, including changes in minors' access to the birth control pill and changes in divorce laws. While I am unaware of work investigating the impacts of these policy changes on teenage childbearing in the next generation, several papers provide important clues. Bailey (2013) finds that access to the birth control pill (and to federally funded local family planning programs) is associated with increased education, labor participation and wages in the next generation. On the other hand, Ananat and

Hungerman (2012) find that access to the birth control pill had a negative impact on cohort outcomes in the short run in terms of birth weight and parental poverty, as women who faced higher opportunity costs of early childbearing were more likely to make use of the pill, but a positive impact in the long run in terms of maternal education, as these women eventually gave birth under more favorable circumstances. Their work is consistent with other studies, which suggests that access to the pill increased participation in professional degree programs and marriage quality among female college graduates (Goldin and Katz 2002), as well as age at first birth and female labor participation among young women (Bailey 2006).

Finally, the literature suggests that unilateral divorce laws may also have affected cohort outcomes. (Much of the literature has focused on unilateral divorce laws; an exception is Gruber (2004), who also examines the impact of no-fault divorce laws and finds much smaller effects.) Children exposed to unilateral divorce laws before age 18 complete fewer years of schooling (especially for women) and are more likely to marry young, be institutionalized (for men) or unemployed and living in poverty (for women) (Caceres-Delpiano and Giolito 2008; Gruber 2004; Johnson and Mazingo 2000), with larger effects for children exposed before age 11 (Gruber 2004; Johnson and Mazingo 2000). More relevant for the purposes of this study, the studies also find that cohorts born shortly after the adoption of unilateral divorce laws are not only more likely to have children, but also tend to have more children (the opposite is true for cohorts who were older at the time of reform), potentially due to higher probability of having divorced parents as well as reduced parental investment in children (Johnson and Mazingo 2000) (see the discussion in the Model section below). Finally, Carceres-Delpiano and Giolito (2012) find that cohorts who were young or born shortly after reform have higher violent crime rates, but that there were no impacts on cohorts born more than five years after the reform. One explanation for their results is that while divorce rates spiked up shortly after the adoption of unilateral divorce laws, the trend was reversed within a decade, potentially due to positive selection into new marriages or early dissolution of bad marriages in the previous decade (Wolfers 2006).

3.3 Model

To understand how the above social policy changes could have led to a lower teenage birth rate in the next generation, I model an individual's teenage childbearing outcome as a function of a) her preferences for a teenage birth and b) the costs of sexual activity, contraceptive use and abortion.[2] Following this framework, there are at least two important mechanisms through which the social policies reduce influence teenage birth rates two decades later.

First, increased access to the birth control pill and abortion is associated with lower proportions of births which are unwanted or mistimed (Bailey (2013; Lin and

[2] Teenage childbearing outcomes also depend on the individual's fecundity and probability of miscarriage, which are in turn affected by health. Age at menarche, for instance, is positively correlated with birth weight but negatively correlated with childhood weight gain (Sloboda et al. 2007).

Pantano 2011).[3] Unwanted or mistimed births may place unexpected stress on family resources, leading to poorer child outcomes. In addition, women who do not successfully avoid early motherhood may transmit lower educational and marital expectations to their daughters (Manlove 1997), be less likely to emphasize traditional values, and be more tolerant of early sexual activity and non-marital childbearing (Abrahamse et al. 1988; Barber 2001; Kahn and Anderson 1992), increasing their daughters' preference for a teenage birth and lowering their costs of sexual activity. Consistent with these arguments, Lin and Pantano (2011) use abortion legalization and maternal religion as instruments for unwantedness and find that cohorts with higher proportions of unwanted or mistimed births complete less schooling, earn lower wages and commit more crimes.

Second, the social policies may lead to positive changes in family environments, especially in the long run. Ananat and Hungerman (2012) provides evidence that increased access to the birth control pill (and abortion, to a lesser extent) is associated with *negative* selection into childbearing in the short run, as women who faced higher opportunity costs of early childbearing were more likely to respond to the reform. In the long run, however, increased access to the pill and abortion may reduce teenage birth rates in the next generation by allowing women to improve their labor market outcomes

[3] Lin and Pantano (2011) estimate that legalization of abortion for adult women reduced the proportion of unwanted births by around 10% (27.5% among women whose religion is more permissive towards abortion and 1.4% among other women).

and give birth under more favorable circumstances (Ananat and Hungerman 2012; Bailey 2006), providing their children with more economic opportunities and lower incentives for teenage childbearing, as well as greater knowledge and access to contraceptives and abortion. The arguments are less clear in the case of unilateral (and equitable division of assets) divorce laws. While unilateral divorce laws led to lower levels of reported domestic abuse for both men and women, as well as lower rates of suicide and murder victimization for women (Stevenson and Wolfers 2006), they also lowered marital stability and increased the probability of children living with divorced mothers and in poverty (Caceres-Delpiano and Giolito 2008; Carceres-Delpiano and Giolito 2012). Beyond the splintering of family resources, unilateral divorce laws may also reduce parental incentives to invest in children for both parents, who may focus more on their own human capital in response to the increased probability of divorce (Reinhold, Kneip and Bauer 2013). The impacts on family environments are more likely to be positive in the long run as divorce rates fell after the initial spike, potentially due to positive selection into new marriages or early dissolution of bad marriages in the previous decade (Wolfers 2006).

3.3.1 Age-Period-Cohort Analysis

Before examining the impacts of the social policies on teenage birth rates in the next generation, I investigate the extent to which the decline in teenage birth rates can be attributed to changes in cohort behavior. In theory, it is possible to empirically distinguish the impacts of cohort and period changes: if there are only cohort changes, we would expect to see age-specific teenage birth rates falling with each successive cohort; but if there are only period changes, we would expect to see birth rates decline comparably at all teenage ages among multiple birth cohorts.

Age-period-cohort analysis is traditionally challenging due to the perfect multicollinearity problem (since an individual's age is uniquely identified by the year and her birth cohort). In this study, I consider three different approaches. I begin with a simple graphical method proposed by Cook and Laub (2002), which was originally used to distinguish between the contributions of cohort and period changes to youth violence trends. The method first "removes" age effects from age-specific teenage birth rates using the age structure in a given year as the standard, and then plots the standardized birth rates for each cohort (a curve containing five data points for 15 to 19 year-olds) against year. Cohort changes are represented by distances between cohort curves, while period changes are represented by simultaneous trends among multiple cohort curves. In a variant of this graphical method, I compare the birth rate trends for 15, 19 and 23 year-olds without controlling for age effects. An advantage of this approach is that it relies on very few assumptions; a disadvantage, however, is that it does not provide point estimates of cohort and period changes in teenage birth rates.

The other two approaches, which do provide point estimates of cohort and period changes, are both fixed-effects generalized linear regression models. The technical problem behind the perfect multicollinearity problem described above is that the design matrix **X**, which contains values of explanatory variables, is singular so that the cross-product matrix **X'X** is not invertible (Yang et al. 2008). One solution is to introduce an additional constraint by setting the coefficients for two ages, periods or cohorts to be equal, and this approach is known as the *equality-constraints estimator*. This approach, however, can lead to widely varying results depending on the choice of constraint. I present the results from both period and cohort constraints. The third approach is the *intrinsic estimator* proposed by Yang et al. (2008). Similar to the equality-constraints estimator, it introduces an additional constraint to the estimation procedure by assuming that the solution $\boldsymbol{\beta}$ is a function of the eigenvector to X'X (normalized to length one and multiplied by a scalar). Yang et al. (2008) argue that the constraint should not produce biased estimates since it is not a function of the dependent variable. While they find that their estimator has desirable statistical properties, including unbiasedness and consistency, Luo (2013) argues that the intrinsic estimator does not clearly perform better than the equality-constraints estimator, and has an additional disadvantage in that its assumption is more difficult to empirically verify. Compared to the Cook-Laub graphical method, the two estimators rely on more assumptions; in particular, the models assume that age effects are constant for all periods and cohorts, which may not be true if cohort factors such as legalization of abortion disproportionately reduce the risks of childbearing at earlier teenage ages.

In the next section, I present and compare the results from all three approaches.

3.3.2 Difference-in-Differences Model

To estimate the impacts of the social policies on teenage birth rates in the 1990s-2000s, I use a difference-in-differences model similar to the strategy used by Donohue, Grogger and Levitt (2009). The model assumes that teenage birth rates are a function of state-specific age, cohort and period effects. To estimate the impacts of the social policies on cohort effects, I control for age and period effects at the state level and cohort effects at the national level. Algebraically,

$$B_{ats} = \alpha_0 + \alpha_1 P21_{ats} + \alpha_2 P18_{ats} + \alpha_3 P15_{ats} + \alpha_4 A21_{ats} + \alpha_5 A18_{ats} + \alpha_6 A15_{ats} + \alpha_7 U_{ats} + \alpha_7 E_{ats} + \mu_s + \eta_{at} + \upsilon_{as} + \delta_{ts} + \varepsilon_{ats}, \tag{6}$$

where B_{ats} is the log of the age- and state-specific teenage birth rate in a year (where "a" stands for age, "t" stands for year and "s" stands for state). $P21_{ats}$, $P18_{ats}$ and $P15_{ats}$ are dichotomous indicators for whether there was legal access to the birth control pill in the state for women aged 21 or older, women aged 18 or older and women aged 15 or older respectively at the time of the individual's birth.[4] $A21_{ats}$, $A18_{ats}$ and $A15_{ats}$ are very similar except that they are indicators for whether there was legal access to abortion rather than

[4] Since the policy changes only affect segments of the total population at risk of childbearing ($P21_{ats}$, for example, affects only women aged 21 or above), the parameter of interest $\boldsymbol{\alpha}$ reflects the impact of social policy changes on entire cohorts' teenage childbearing outcomes, rather than the impact of being born to a mother who had access to the pill.

to the birth control pill. U_{ats} and E_{ats} are dichotomous indicators for whether unilateral divorce laws and equitable division of assets laws respectively have been adopted in the state at the time of the individual's birth. To explore the differential impacts of the policies in the short run and long run, I also run an alternate specification which replaces each of the dichotomous indicators with a vector of dichotomous indicators for birth during the following windows: 1-2 years *before* reform, and 0-1 years, 2-4 years, 5-7 years and 8 or more years after the law was passed. Finally, μ_s, η_{at}, υ_{as} and δ_{ts} are controls for state fixed effects, U.S. cohort fixed effects, state-specific age effects and state-specific period effects respectively. The controls for state-specific period effects take into account policy changes in the 1990s and 2000s, including welfare reform and increased access to family planning through Medicaid, as well as all other non-policy period changes. Data points are weighted by age-, year- and state-specific cohort sizes and all regression errors are clustered at the state level.

One issue with the above model is that it assumes that the state of residence, observed during teenage years, is also the state of birth. To allow for changes in state of residence since the time of birth, I use a difference-in-differences model which also controls for state of birth (in addition to state of residence) fixed effects. Algebraically,

$$B_{atsb} = \alpha_0 + \alpha_1 P21_{atb} + \alpha_2 P18_{atb} + \alpha_3 P15_{atb} + \alpha_4 A21_{atb} + \alpha_5 A18_{atb} + \alpha_6 A15_{atb} + \alpha_7 U_{atb} + \alpha_7 E_{atb} + \pi_b + \mu_s + \eta_{at} + \upsilon_{as} + \delta_{ts} + \varepsilon_{atsb}, \quad (7)$$

where B_{atsb} is the log of the age-specific teenage birth rate in a year for a given state of residence and state of birth (where "b" stands for state of birth), and π_b is a control for state of birth fixed effects. This model estimates the impacts of the social policies on teenage birth rates by comparing women who were born in different states but living in the same state during teenage ages. One important issue with this model, however, is that individuals who move out of state ("movers") may not be comparable to individuals who do not move out of state. Indeed, movers have substantially higher teenage birth rates prior to the 1990s (see Appendix C1). To address this selection issue, I control for state of origin and state of destination fixed effects, and compare the results from both models.

3.4 Data

I use data from multiple sources, all of which are publicly available for download online. To calculate teenage birth rates by single year of age and state of residence, I compute birth counts using de-identified birth certificate data from Vital Statistics and state-specific female population counts from the Census. To check that these age-specific birth rates are reliable, I compute national teenage birth rates and compare them to published rates. Appendix C2 shows that the computed birth rates are highly similar to those published by Vital Statistics.

To calculate birth rates by single year of age, state of residence and state of birth, I use data from the Integrated Public Use Microdata Series (IPUMS) housed at the

Minnesota Population Center to estimate the proportions of female population by state of residence and state of birth, since the Census does not produce population estimates by both state of residence and state of birth. These data are available only up to 1999, since newer population data are affected by group quarters accounting in the American Community Survey and are less comparable to Census estimates. I also use IPUMS data to compute female population counts by race, which are not available from the Census for some years. I restrict my final sample to observations where the female population count is 500 or above, and where the proportion of missing values for state of birth is less than 20% (89.2% of cases). Similarly, for analyses by race, I restrict the sample to observations where the total proportion of missing values for state of birth and race is less than 20% (89.1% of cases).

Data on the timing of legal access to the birth control pill and abortion are taken from Myers (2012), while data on timing of divorce laws are taken from Rasul (2003). In model specifications which do not control for state-specific period effects, I control for maximum welfare benefits for a family of three (as a proxy for welfare reform) and availability of Medicaid family planning expansions using data taken from the University of Kentucky Center for Poverty Research and Kearney and Levine (2012) respectively.

3.5 Results

3.5.1 Age-Period-Cohort Analysis

Figure 7 shows results using the graphical method proposed by Cook and Laub (2002), including the variant of this graphical method which plots the birth rate trends for 15, 19 and 23 year-olds without controlling for age effects. Figure 8 shows results using the intrinsic estimator based on a log-linear model, where the number of teenage births relative to the population at risk is modeled as a Poisson variate. I present results using the intrinsic estimator since the errors are the smallest, along with the equality-constraints model with cohort constraints (Appendix C3), and since the results from the equality-constraints estimator and the intrinsic estimator are qualitatively similar (see Figure 9; Appendix C4). The results are also robust to the inclusion of early adult ages (20 to 24), except that period effects in the late 2000s become weaker (see Appendix C5).

Figures 7 and 8 both suggest that the decline during the first half of the 1990s was characterized by period changes rather than cohort changes – in fact, the evidence suggests that women born in the early 1970s were *more* rather than *less* likely to have a teenage birth than previous birth cohorts. For Figure 7, this can be seen from the upward vertical shifts in the curves between cohorts born in 1970 and 1976 in the top panel, as well as the non-parallel shifts in the second graph on the bottom panel, suggesting that the decline in age-specific birth rates are largely due to period factors. For Figure 8, this can be seen from the rising cohort effects in the bottom graph, where each data point represents a multiplicative age, cohort or period effect, relative to the reference factor of 1. In a variant of this graphical method, I compare the birth rate trends for 15, 19 and 23 year-olds without controlling for age effects.[5] The rise in cohort fertility in the early 1970s (and later the decline in the 1980s) is even more pronounced for blacks than for whites (see Appendix C6). Hence, while cohort factors such as increased access to the birth control pill and abortion may reduce teenage childbearing in the next generation, they are unlikely to be major explanations for the decline in teenage birth rates in the past two decades. The results also help to explain the brief spike in teenage birth rates in the late 1980s, which coincided with a rise in period effects. Between the late 1990s and early 2000s, however, period effects had flattened out and were on the rise in the late 2000s,[6] so that the decline in teenage birth rates were characterized by declining cohort fertility among women born in the 1980s.

---

[5] For example, since the constant is 0.0371 and the data point corresponding to being 15 years old is 0.3259, the teenage birth rate for 15 year-olds is estimated to be 0.0371*0.3259 = 0.0121, or 121 births per 1,000 women aged 15 to 19, all period and cohort effects held constant. Most of the data points in Figure 3 are statistically different from one (the only exception is the period effect for 1987).

[6] This is somewhat surprising, since Kearney and Levine (2014) estimate that two period changes, the Great Recession and the show *16 and Pregnant*, account for 37% and 21% of the observed decline in teenage birth rates respectively between 2008 and 2012.

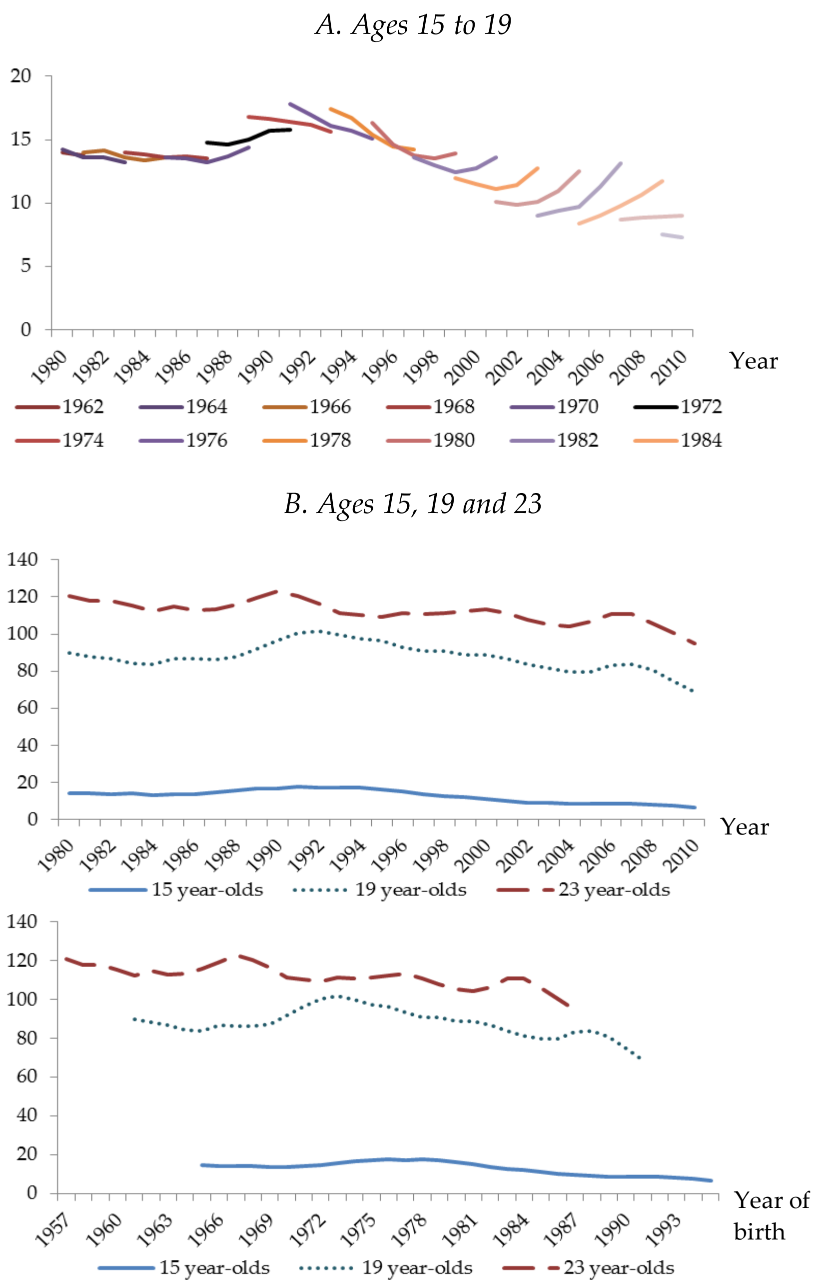


Figure 7: Cohort and period trends in teenage birth rates, 1980-2010

Notes: The y-axis represents the number of births per 1,000 women in a cohort at each age. For the top panel, birth rates are adjusted for age effects using the age structure in 1985 as the standard, and each birth cohort is represented by a curve with up to five data points. Cohort changes are represented by distances between cohort curves, while period changes are represented by simultaneous trends among multiple cohort curves. For the bottom panel, birth rates are unadjusted and are plotted against year and year of birth respectively.

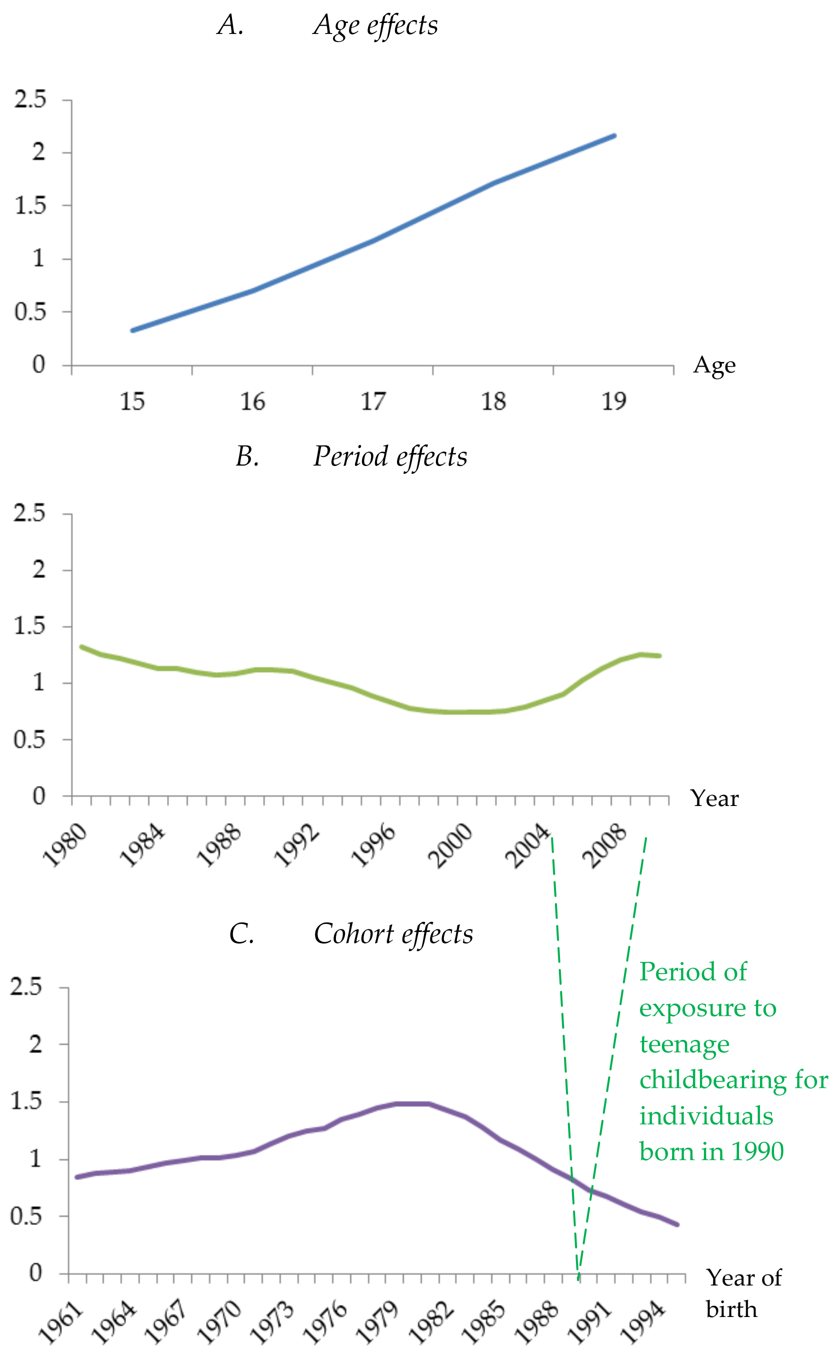


Figure 8: Age, period and cohort effects in teenage birth rates, 1980-2010

Notes: All estimates are produced using the intrinsic estimator and a log-linear model, where the number of teenage births relative to the population at risk is modeled as a Poisson variate. For cohort effects, the extreme data points are calculated using fewer observations.

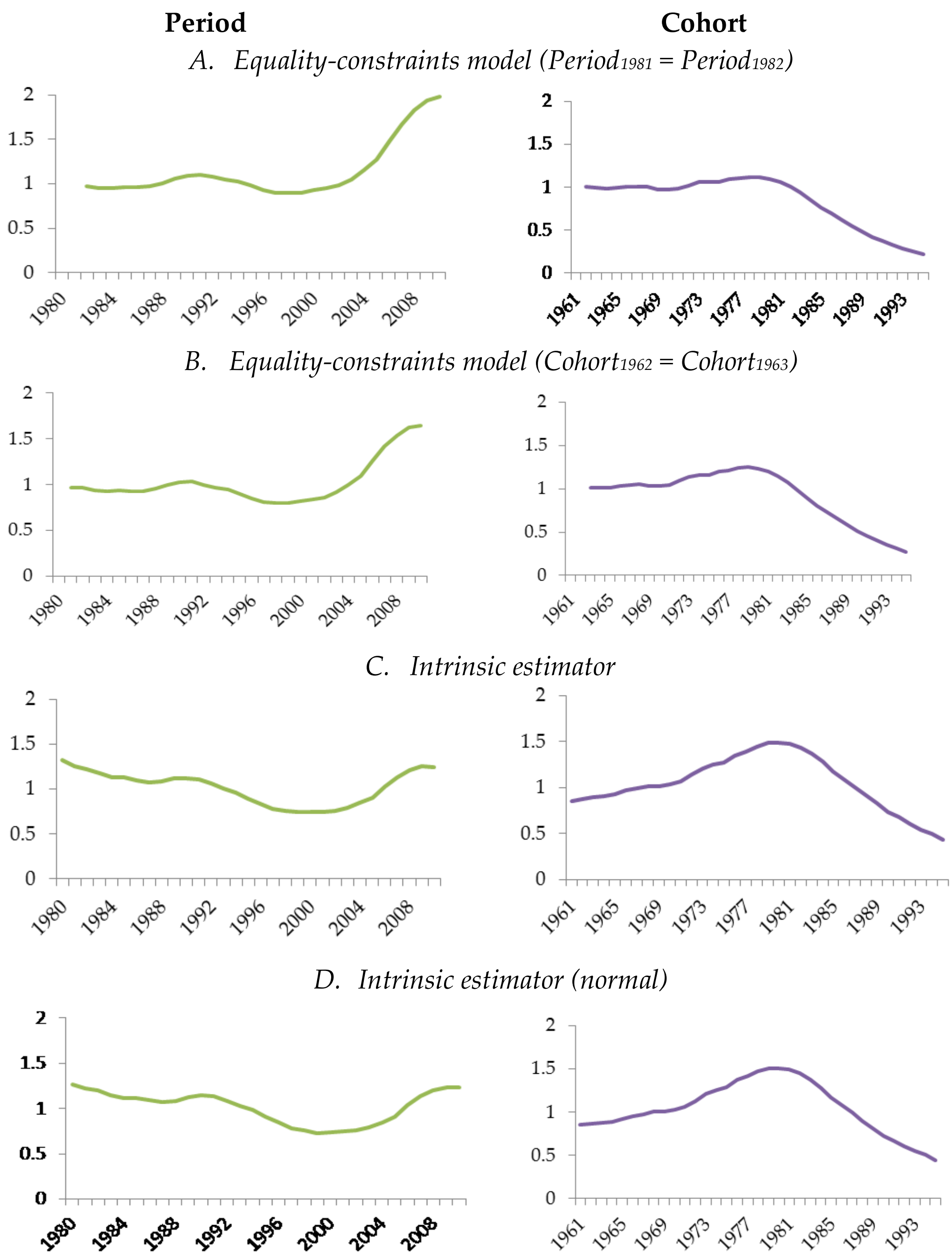


Figure 9: Period and cohort effects in teenage birth rates, 1980-2010

Notes: All estimates are produced using a log-linear model. The number of teenage births relative to the population at risk is modeled as a Poisson variate in the first three specifications, and as a normal variate in the fourth specification.

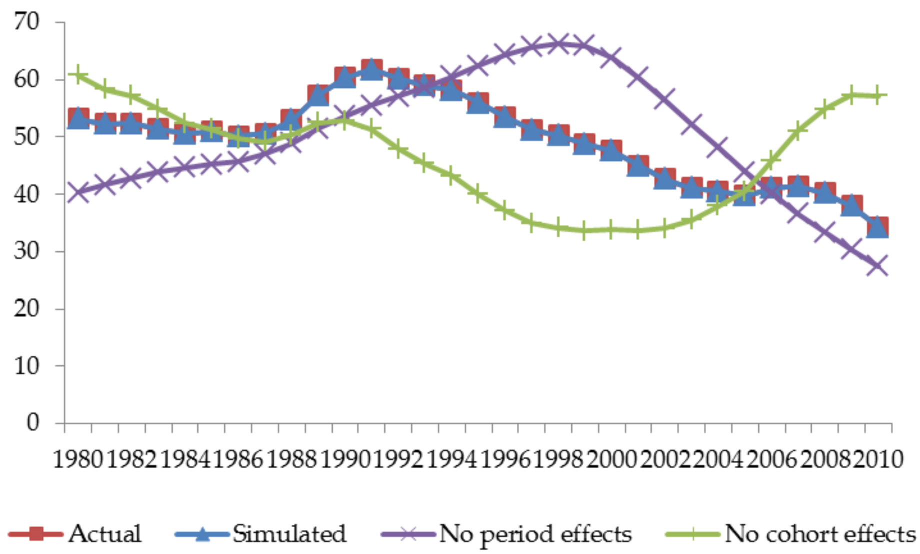


Figure 10: Simulated teenage birth rates, 1980-2010

Notes: All estimates are produced using the intrinsic estimator and a log-linear model, where the number of teenage births relative to the population at risk is modeled as a Poisson variate.

Figure 10 displays simulated teenage birth rates between 1980 and 2010 based on the estimates shown in Figure 8. In the absence of any period changes, the rise in teenage birth rates in the late 1980s would have continued through to the late 1990s. Conversely, in the absence of any cohort changes, teenage birth rates would have seen a smaller "hump" in the late 1980s, and have fallen even more quickly in the early 1990s before stabilizing in the mid-1990s.

The results from all three age-period-cohort approaches point to similar conclusions, and are consistent with some evidence from other studies. The decline in birth rates is similar for teenagers and for 20-24 year-olds between the mid-1980s and the mid-1990s (see Appendix C7), suggesting that the changes during these years are primarily driven by period factors. One potential explanation is changes in age composition: Kearney and Levine (2013) show that the proportion of teenagers who were 18-19 year-olds briefly rose from 40% to 44% in the late 1980s, which fell back to 40% by 1994. They estimate that this explanation accounts for one-third of the rise in teenage birth rates in the late 1980s and almost all of the decline between 1991 and 1994. (It is unclear, however, whether changes in age composition can also explain the decline in fertility among 20-24 year-olds.) Moreover, teenage girls born in the 1970s are substantially more likely to be sexually active (although they are also more likely to use contraceptives) than their counterparts born in the late 1960s, consistent with the rise in cohort fertility in the 1970s (Hogan, Sun and Cornwell 2000; Manlove et al. 2000; Manlove et al. 2009).

Finally, if teenage childbearing shares the same antecedents with juvenile crime and dropping out of high school, the results are also consistent with Cook and Laub (2002), who use their graphical method to show that black male youth homicide victimization rates (used as a proxy for youth crime rates) increased among cohorts born in the 1970s but fell among cohorts born in the early 1980s. They also find a decline in period effects in the early 1990s. Similarly, Marcotte (2013) finds that high school dropout rates began falling only in 1997, around the same time that cohort effects began to decline for teenage childbearing.

The above age-period-cohort analysis yields several insights. First, the decline in teenage birth rates in the 1990s and 2000s was due to both cohort and period factors. Second, the evidence suggests that cohorts born immediately after social policy changes in the early 1970s, including legal access to abortion for adult women, were no less likely to become teenage mothers than previous cohorts. Instead, it was only after almost a decade after these policy changes that cohorts (born in the early 1980s) started to have lower teenage fertility, potentially due to differential impacts of the policies in the short run and long run (Ananat and Hungerman 2012; Carceres-Delpiano and Giolito 2012; Gruber 2004; Johnson and Mazingo 2000). Third, the analysis suggests that policy changes in the late 1990s and 2000s, including welfare reform and Medicaid family planning expansions, are unlikely to explain much of the total decline in teenage birth rates given the flat/rising period effects, consistent with Kearney and Levine (2013).

3.5.2 Difference-in-Differences Model

I now turn to results from the difference-in-differences model, which investigates the extent to which social policies in the 1960s-1980s can account for the changes in cohort behavior observed above. Table 10 shows the results for two models: Model 1 compares women who live in different states during teenage years (assuming that the state of residence is also the state of birth), whereas Model 2 compares women who live in the same states during teenage years but were born in different states (assuming that state movers are comparable to state non-movers after controlling for fixed effects).

Since the observation unit for Model 1 is specific only at the state of residence level, whereas the observation unit for Model 2 is specific at both the state of residence and the state of birth levels, the number of observations for Model 2 is substantially larger (even though data are available only up to 1999). For each model, the first specification controls only for state, year, cohort and age fixed effects; the second also controls for state-specific year fixed effects and hence for the effects of policy changes in the 1990s and 2000s, including welfare reform and increased access to family planning through Medicaid; and the third controls for both state-specific year and age fixed effects, similar to Donohue, Grogger and Levitt (2009). Similar to the generalized linear models from the age-period-cohort analysis, the third specification assumes that state-specific age effects are constant for all periods and cohorts, which may not hold if some of the social policies, for example, disproportionately reduce the risks of childbearing at earlier teenage ages. The results in Table 11 are similar, except that the models allow for differential impacts of the policies over time.

Table 10: Impact of social policies on cohort teenage birth rates, 1980-2010 (dependent variable = log of births per 1,000 women)

	Model 1			Model 2		
	(1)	(2)	(3)	(4)	(5)	(6)
Born 1-2 years *before* pill was available to women aged 21+	0.030 (0.026)	-0.006 (0.048)	0.025 (0.025)	0.014 (0.039)	-0.061* (0.034)	-0.020 (0.029)
Born 0+ years after pill was available to women aged 21+	0.039** (0.016)	-0.039 (0.082)	0.028 (0.046)	0.023 (0.039)	-0.087* (0.049)	-0.022 (0.044)
Born 1-2 years *before* pill was available to women aged 18-20	0.022 (0.017)	0.016 (0.014)	0.004 (0.007)	0.008 (0.015)	0.010 (0.013)	-0.001 (0.011)
Born 0+ years after pill was available to women aged 18-20	0.041 (0.026)	0.034 (0.028)	0.011 (0.016)	0.003 (0.029)	0.011 (0.029)	-0.008 (0.024)
Born 1-2 years *before* pill was available to women aged 15-17	0.044** (0.020)	0.027 (0.017)	0.012 (0.008)	0.031* (0.016)	0.044*** (0.017)	0.033* (0.016)
Born 0+ years after pill was available to women aged 15-17	0.022 (0.017)	0.046 (0.032)	0.009 (0.015)	0.039* (0.022)	0.076** (0.031)	0.052* (0.030)
Born 1-2 years *before* abortion was available to women aged 21+	-0.049** (0.019)	-0.030*** (0.011)	-0.024*** (0.008)	-0.033* (0.019)	-0.033** (0.016)	-0.033** (0.014)
Born 0+ years after abortion was available to women aged 21+	-0.028 (0.038)	-0.041* (0.022)	-0.046** (0.018)	-0.059 (0.039)	-0.045 (0.032)	-0.055* (0.029)
Born 1-2 years *before* abortion was available to women aged 18-20	0.004 (0.027)	0.022 (0.016)	0.017 (0.011)	0.038 (0.024)	0.053*** (0.017)	0.056*** (0.016)
Born 0+ years after abortion was available to women aged 18-20	-0.014 (0.052)	0.005 (0.034)	0.004 (0.021)	0.047 (0.044)	0.048 (0.033)	0.059** (0.027)
Born 1-2 years *before* abortion was available to women aged 15-17	0.017 (0.012)	0.007 (0.018)	0.012 (0.011)	0.023** (0.010)	-0.010 (0.019)	-0.011 (0.015)
Born 0+ years after abortion was available to women aged 15-17	0.012 (0.024)	0.006 (0.035)	0.014 (0.020)	0.014 (0.019)	-0.029 (0.034)	-0.031 (0.025)
Born 0+ years after unilateral divorce laws passed	0.015 (0.017)	-0.052** (0.025)	-0.015 (0.011)	-0.002 (0.017)	-0.024 (0.028)	0.000 (0.018)
Born 0+ years after equitable division of property laws passed	-0.010 (0.015)	0.018 (0.018)	-0.017 (0.013)	0.005 (0.016)	0.035 (0.022)	0.010 (0.019)

State-specific year effects	No	Yes	Yes	No	Yes	Yes
State-specific age effects	No	No	Yes	No	No	Yes
Constant	2.717	3.000	3.012	3.200	3.101	3.159
Adjusted R^2	0.982	0.986	0.996	0.953	0.956	0.966
Root MSE	0.100	0.090	0.050	0.161	0.156	0.136
No. of observations	6,885	6,885	6,885	25,415	25,415	25,415

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: All specifications control for state of residence, year, cohort and age fixed effects. Model 2 specifications also control for state of birth as well as state of origin and state of destination (for individuals who moved between time of birth and during teenage ages) fixed effects. Model 2 excludes data for 2000-2010 since IPUMS female population counts are distorted by group quarters accounting in the American Community Survey. Despite this, the number of observations is larger for Model 2 since the observation unit for Model 2 is specific at both the state of residence and the state of birth levels, rather than only at the state of residence level. In specifications which do not control for state-specific year effects, I control for maximum welfare benefits for a family of three in a year and state (as a proxy for welfare reform) and a binary variable for whether the Medicaid family planning expansion was in force in a year and state.

Table 11: Impact of social policies on cohort teenage birth rates over time, 1980-2010 (dependent variable = log of births per 1,000 women)

	Model 1			Model 2		
	(1)	(2)	(3)	(4)	(5)	(6)
Born 1-2 years *before* pill was available to women aged 21+	0.034 (0.025)	-0.005 (0.062)	0.041 (0.031)	0.010 (0.035)	-0.068* (0.034)	-0.019 (0.028)
Born 0-1 years after	0.020 (0.024)	-0.031 (0.101)	0.063 (0.056)	0.010 (0.037)	-0.089* (0.049)	-0.016 (0.044)
Born 2-4 years after	0.040** (0.016)	-0.019 (0.133)	0.099 (0.075)	0.034 (0.036)	-0.078 (0.066)	0.000 (0.062)
Born 5-7 years after	0.066*** (0.017)	0.009 (0.151)	0.137 (0.088)	0.060 (0.037)	-0.055 (0.078)	0.021 (0.075)
Born 8+ years after	0.055 (0.034)	0.030 (0.161)	0.164* (0.096)	0.065* (0.037)	-0.045 (0.087)	0.033 (0.084)
Born 1-2 years *before* pill was available to women aged 18-20	0.025* (0.015)	0.023 (0.014)	0.004 (0.008)	0.010 (0.015)	0.021* (0.012)	0.001 (0.010)
Born 0-1 years after	0.046* (0.025)	0.056* (0.028)	0.016 (0.018)	0.010 (0.025)	0.034 (0.024)	-0.004 (0.019)
Born 2-4 years after	0.036 (0.032)	0.064 (0.042)	0.013 (0.026)	0.000 (0.003)	0.038 (0.036)	-0.012 (0.027)
Born 5-7 years after	0.022 (0.033)	0.055 (0.049)	0.005 (0.033)	-0.019 (0.035)	0.031 (0.041)	-0.022 (0.032)
Born 8+ years after	0.018 (0.030)	0.047 (0.058)	-0.007 (0.039)	-0.024 (0.034)	0.040 (0.044)	-0.016 (0.037)
Born 1-2 years *before* pill was available to women aged 15-17	0.022* (0.012)	0.036* (0.021)	0.017 (0.011)	0.023 (0.014)	0.040** (0.016)	0.026* (0.015)
Born 0-1 years after	0.014 (0.014)	0.051 (0.036)	0.014 (0.017)	0.022 (0.018)	0.056** (0.022)	0.034* (0.020)
Born 2-4 years after	0.024 (0.017)	0.093 (0.057)	0.033 (0.025)	0.034* (0.020)	0.082** (0.031)	0.051* (0.029)
Born 5-7 years after	0.032 (0.022)	0.137* (0.081)	0.054 (0.036)	0.033 (0.024)	0.095** (0.040)	0.056 (0.037)
Born 8+ years after	0.023 (0.023)	0.177* (0.0103)	0.074* (0.040)	0.034 (0.024)	0.092* (0.048)	0.026 (0.043)
Born 1-2 years *before* abortion was available to women aged 21+	-0.001 (0.018)	-0.019 (0.012)	-0.021* (0.011)	-0.023* (0.013)	-0.029** (0.013)	-0.036*** (0.011)
Born 0-1 years after	0.020 (0.037)	-0.017 (0.030)	-0.034 (0.028)	-0.046 (0.028)	-0.036 (0.029)	-0.058** (0.026)
Born 2-4 years after	0.109* (0.060)	-0.007 (0.041)	-0.057 (0.039)	-0.106** (0.053)	-0.081** (0.040)	-0.125*** (0.038)

Born 5-7 years after	0.245***	0.003	-0.052	-0.084	-0.061	-0.107**
	(0.072)	(0.048)	(0.046)	(0.061)	(0.046)	(0.045)
Born 8+ years after	0.351***	0.013	-0.049	-0.047	-0.028	-0.079
	(0.077)	(0.057)	(0.055)	(0.064)	(0.052)	(0.051)
Born 1-2 years *before* abortion was available to women aged 18-20	-0.001	0.003	0.010	0.028	0.033*	0.043**
	(0.020)	(0.021)	(0.015)	(0.018)	(0.019)	(0.016)
Born 0-1 years after	-0.037	-0.045	-0.007	0.058	0.030	0.060*
	(0.043)	(0.049)	(0.036)	(0.042)	(0.038)	(0.035)
Born 2-4 years after	-0.065	-0.079	-0.009	0.108	0.039	0.087*
	(0.067)	(0.069)	(0.046)	(0.064)	(0.052)	(0.047)
Born 5-7 years after	-0.083	-0.084	-0.010	0.141*	0.039	0.086*
	(0.083)	(0.086)	(0.054)	(0.073)	(0.060)	(0.051)
Born 8+ years after	-0.087	-0.080	-0.014	0.133*	0.009	0.059
	(0.090)	(0.103)	(0.061)	(0.077)	(0.066)	(0.056)
Born 1-2 years *before* abortion was available to women aged 15-17	0.017	0.001	0.010	0.026**	-0.005	-0.005
	(0.014)	(0.021)	(0.012)	(0.011)	(0.013)	(0.011)
Born 0-1 years after	0.013	-0.009	0.013	0.014	-0.025	-0.020
	(0.018)	(0.041)	(0.022)	(0.016)	(0.023)	(0.018)
Born 2-4 years after	0.009	-0.012	0.022	0.009	-0.029	-0.024
	(0.021)	(0.064)	(0.029)	(0.019)	(0.034)	(0.026)
Born 5-7 years after	-0.001	-0.024	0.026	0.006	-0.024	-0.016
	(0.024)	(0.088)	(0.038)	(0.024)	(0.052)	(0.039)
Born 8+ years after	-0.028	-0.044	0.020	0.010	0.000	0.001
	(0.029)	(0.110)	(0.042)	(0.041)	(0.007)	(0.058)
Born 6-14 years *before* unilateral divorce laws passed	-0.138***	-0.215***	-0.005	-0.090**	-0.284***	-0.041
	(0.044)	(0.032)	(0.109)	(0.038)	(0.070)	(0.136)
Born 3-5 years *before*	-0.137***	-0.250***	-0.019	-0.098**	-0.289***	-0.031
	(0.047)	(0.048)	(0.110)	(0.041)	(0.072)	(0.135)
Born 1-2 years *before*	-0.144***	-0.297***	-0.032	-0.103**	-0.300***	-0.025
	(0.050)	(0.069)	(0.115)	(0.042)	(0.072)	(0.134)
Born 0-1 years after	-0.149***	-0.339***	-0.043	-0.099**	-0.305***	-0.019
	(0.052)	(0.087)	(0.119)	(0.045)	(0.076)	(0.136)
Born 2-4 years after	-0.141**	-0.375***	-0.050	-0.103**	-0.311***	-0.021
	(0.053)	(0.106)	(0.123)	(0.046)	(0.082)	(0.137)
Born 5-7 years after	-0.134**	-0.403***	-0.053	-0.084*	-0.298***	-0.005
	(0.055)	(0.127)	(0.125)	(0.047)	(0.088)	(0.138)
Born 8+ years after	-0.124**	-0.428***	-0.054	-0.077	-0.296***	0.011
	(0.055)	(0.146)	(0.126)	(0.054)	(0.099)	(0.143)

Born 3-5 years *before* equitable division of property laws passed	-0.007 (0.012)	0.037** (0.019)	-0.003 (0.012)	0.025* (0.014)	0.038** (0.017)	0.017 (0.015)
Born 1-2 years *before*	-0.010 (0.016)	0.063* (0.036)	-0.015 (0.025)	0.036* (0.019)	0.069** (0.028)	0.036 (0.023)
Born 0-1 years after	-0.022 (0.019)	0.078 (0.049)	-0.034 (0.035)	0.027 (0.023)	0.085** (0.037)	0.042 (0.031)
Born 2-4 years after	-0.023 (0.021)	0.105 (0.064)	-0.050 (0.041)	0.027 (0.027)	0.101** (0.050)	0.037 (0.041)
Born 5-7 years after	-0.012 (0.023)	0.142* (0.078)	-0.056 (0.051)	0.034 (0.032)	0.118* (0.061)	0.048 (0.049)
Born 8+ years after	0.000 (0.028)	0.183** (0.090)	-0.058 (0.061)	0.058 (0.043)	0.147* (0.082)	0.074 (0.064)
State-specific year effects	No	Yes	Yes	No	Yes	Yes
State-specific age effects	No	No	Yes	No	No	Yes
Constant	2.987	3.195	3.042	3.180	3.358	3.172
Adjusted R^2	0.983	0.986	0.996	0.953	0.956	0.966
Root MSE	0.097	0.089	0.050	0.160	0.155	0.136
No. of observations	6,885	6,885	6,885	25,415	25,415	25,415

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: All specifications control for state of residence, year, cohort and age fixed effects. Model 2 specifications also control for state of birth as well as state of origin and state of destination (for individuals who moved between time of birth and during teenage ages) fixed effects. Model 2 excludes data for 2000-2010 since IPUMS female population counts are distorted by group quarters accounting in the American Community Survey. Despite this, the number of observations is larger for Model 2 since the observation unit for Model 2 is specific at both the state of residence and the state of birth levels, rather than only at the state of residence level. In specifications which do not control for state-specific year effects, I control for maximum welfare benefits for a family of three in a year and state (as a proxy for welfare reform) and a binary variable for whether the Medicaid family planning expansion was in force in a year and state.

There is generally little evidence that access to the birth control pill increases or reduces teenage birth rates in the next generation through positive selection in terms of family background or wantedness at the time of birth. This is somewhat surprising, given that the access to the pill increased participation in professional degree programs and marriage quality among unmarried college graduate women (Goldin and Katz 2002), increased the age at first birth and female labor participation among young women aged younger than 21 (Bailey 2006), and increased educational attainment, labor supply and wages (Bailey 2013) and reduced criminal activity (Pantano 2007) in the next generation.

I find more evidence that cohorts exposed to legalization of abortion for adult women at the time of birth had lower teenage birth rates. (However, access to the pill and abortion *for teenagers* increased teenage birth rates in the next generation, potentially due to positive selection into contraceptive and abortion use.) This is consistent with Myers (2012), who finds that abortion legalization had a much larger impact on young women's age at marriage and parenthood than access to the birth control pill. Cohorts born up to two years *prior* to reform also have lower teenage birth rates, possibly due to access to out-of-state abortions,[7] which in turn suggests that the estimated impacts of abortion legalization may be lower bounds. Similar to Ananat and Hungerman (2012)

---

[7] It is estimated that 42% of the legal abortions performed in New York in 1971 were on out-of-state women (Joyce, Tan and Zhang 2010 in Myers 2012).

and Ozbeklik (2006),[8] I find that the impact on cohort teenage birth rates appears to be larger for cohorts who were born two to seven years after reform than for cohorts born immediately after reform, consistent with evidence suggesting that there was no dramatic change in maternal age around the time of reform (Appendix C8).[9] One potential explanation is higher levels of positive selection in early repeal states, which continued to have higher pregnancy and abortion rates long after *Roe v. Wade* (Ananat et al. 2009). I also find larger short run impacts for black women, especially for cohorts born in the 1970s (Appendices C9 and C10), consistent with Donohue, Grogger and Levitt (2009) and Ozbeklik (2006); however, the long run impacts are larger and statistically significant only for white women. Again similar to Ananat and Hungerman, I find little evidence that the fall in teenage childbearing was replaced by higher fertility at ages 20-24 (Appendix C11). Together with the above age-period-cohort analysis, the evidence suggests that while legalization of abortion for adult women had a negative impact on teenage birth rates in the next generation, the impacts were offset by other cohort changes.

Finally, Table 12 shows that if the analysis were restricted to cohorts born in the 1970s or 1980s (rather than to all cohorts born between 1961 and 1995), the evidence suggests that exposure to unilateral divorce and equitable division of assets laws at the

---

[8] Ozbeklik (2006) finds that the effect of early access to adult abortion on out-of-wedlock teenage birth rates grew larger over the 1970s for blacks only; the reverse is true for whites.
[9] I use data on maternal age from birth certificates rather than data on maternal education or other characteristics, as this variable consistently has a low proportion of missing values for the years of interest.

time of birth or at young ages increased teenage birth rates among women born in the 1970s. The results are consistent with studies that find that the initial introduction of unilateral divorce laws had negative impacts on children's outcomes, especially children who were younger at the time of reform (Caceres-Delpiano and Giolito 2008; Gruber 2004; Johnson and Mazingo 2000). On the other hand, exposure to unilateral divorce laws *reduced* rather than increased teenage birth rates among women born in the 1980s, similar to Carceres-Delpiano and Giolito (2012), who find that only cohorts born before or fewer than five years after adoption of the laws had higher violent crime rates. One potential explanation for the differential impacts of unilateral divorce laws over time is that while divorce rates spiked up shortly after the adoption of unilateral divorce laws in the 1970s, the trend was reversed within a decade (Wolfers 2006), so that cohorts born in the 1980s are less rather than more likely to experience marital disruption. The estimates of the impacts of unilateral divorce laws in Table 12 are largely driven by results for white women (Appendix C10),[10] whereas Caceres-Delpiano and Giolito (2012) find similar results for white and black children.

[10] Exposure to unilateral divorce laws reduced rather than increased teenage birth rates among cohorts born in the 1970s for blacks, and the negative effects among cohorts born in the 1980s are significant only for whites.

Table 12: Impact of social policies on cohort teenage birth rates over time by decade of birth, 1980-2010 (dependent variable = log of births per 1,000 women)

	Model 1			Model 2		
	Cohorts born in the 1960s	Cohorts born in the 1970s	Cohorts born in the 1980s	Cohorts born in the 1960s	Cohorts born in the 1970s	Cohorts born in the 1980s
Born 1-2 years *before* pill was available to women aged 21+	0.007 (0.036)	0.025* (0.014)	-	-0.010 (0.023)	-0.029 (0.029)	-
Born 0-1 years after	0.004 (0.051)	-0.001 (0.029)	-	0.013 (0.036)	-0.027 (0.030)	-
Born 2-4 years after	0.004 (0.054)	0.015 (0.048)	-	0.049 (0.046)	-0.020 (0.057)	-
Born 5-7 years after	0.017 (0.055)	-0.005 (0.038)	0.039** (0.017)	0.064 (0.054)	-0.019 (0.066)	-
Born 8+ years after	0.007 (0.053)	-0.008 (0.040)	-	0.051 (0.060)	-0.043 (0.068)	-0.084** (0.037)
Born 1-2 years *before* pill was available to women aged 18-20	0.011 (0.019)	0.000 (0.011)	-	0.003 (0.022)	-0.009 (0.012)	-
Born 0-1 years after	0.025 (0.024)	-0.003 (0.014)	-	-0.023 (0.038)	-0.025 (0.017)	-
Born 2-4 years after	0.007 (0.031)	-0.010 (0.015)	-0.036* (0.020)	-0.055 (0.037)	-0.032 (0.022)	-
Born 5-7 years after	0.010 (0.034)	-0.008 (0.017)	-	-0.034 (0.045)	-0.029 (0.026)	-0.016 (0.035)
Born 8+ years after	-0.003 (0.046)	-0.007 (0.022)	-0.015 (0.015)	-0.063 (0.063)	-0.045 (0.035)	-0.001 (0.045)
Born 1-2 years *before* pill was available to women aged 15-17	-0.007 (0.019)	-0.008 (0.009)	-	0.015 (0.020)	0.043** (0.017)	-
Born 0-1 years after	-0.013 (0.028)	-0.013 (0.014)	-	0.047 (0.038)	0.059*** (0.021)	-
Born 2-4 years after	0.004 (0.037)	-0.006 (0.020)	1.289*** (0.035)	0.062 (0.047)	0.070*** (0.025)	1.349*** (0.133)
Born 5-7 years after	-	-0.008 (0.024)	1.308*** (0.032)	-	0.072** (0.028)	1.348*** (0.125)
Born 8+ years after	-	-0.008 (0.032)	1.324*** (0.027)	-	0.085** (0.037)	1.326*** (0.127)

Born 1-2 years *before* abortion was available to women aged 21+	0.004	-0.016	-	0.005	-0.002	-
	(0.009)	(0.016)		(0.030)	(0.021)	
Born 0-1 years after	0.010	-0.026	-	0.018	-0.031	-
	(0.025)	(0.026)		(0.073)	(0.043)	
Born 2-4 years after	-	-0.030	-	-	-0.079	-
		(0.033)			(0.058)	
Born 5-7 years after	-	-0.011	-0.037**	-	-0.061	-
		(0.039)	(0.016)		(0.062)	
Born 8+ years after	-	-0.025	-	-	-0.025	0.016
		(0.041)			(0.066)	(0.022)
Born 1-2 years *before* abortion was available to women aged 18-20	0.034	0.019	-	0.136***	0.012	-
	(0.026)	(0.014)		(0.041)	(0.015)	
Born 0-1 years after	-	0.010	-	-	0.037	-
		(0.026)			(0.035)	
Born 2-4 years after	-	0.007	-	-	0.043	-
		(0.033)			(0.045)	
Born 5-7 years after	-	0.003	0.034**	-	0.030	0.040**
		(0.042)	(0.016)		(0.054)	(0.020)
Born 8+ years after	-	0.088*	-	-	0.061	-
		(0.051)			(0.076)	
Born 1-2 years *before* abortion was available to women aged 15-17	-0.015	0.000	-	-0.094***	-0.002	-
	(0.023)	(0.010)		(0.022)	(0.007)	
Born 0-1 years after	-	-0.005	0.549***	-	-0.021	0.682***
		(0.015)	(0.045)		(0.013)	(0.101)
Born 2-4 years after	-	-0.010	0.543***	-	-0.024	0.694***
		(0.017)	(0.023)		(0.017)	(0.094)
Born 5-7 years after	-	-0.018	0.540***	-	-0.004	0.648***
		(0.021)	(0.018)		(0.024)	(0.098)
Born 8+ years after	-	-0.107***	0.542***	-	-0.051	0.636***
		(0.038)	(0.018)		(0.033)	(0.104)
Born 6-14 years *before* unilateral divorce laws passed	0.060	0.115***	-	-0.033	0.094**	-
	(0.128)	(0.009)		(0.087)	(0.044)	
Born 3-5 years *before*	0.052	0.111***	-0.389***	-0.034	0.151***	-
	(0.128)	(0.025)	(0.022)	(0.088)	(0.049)	
Born 1-2 years *before*	0.047	0.110***	-0.374***	-0.045	0.155***	0.071***
	(0.129)	(0.033)	(0.019)	(0.089)	(0.052)	(0.010)
Born 0-1 years after	0.069	0.106***	-0.387***	0.033	0.146**	-
	(0.131)	(0.037)	(0.017)	(0.091)	(0.055)	
Born 2-4 years after	-	0.107***	-0.268***	-	0.144**	-0.834***
		(0.038)	(0.017)		(0.057)	(0.115)

Born 5-7 years after	-	0.100** (0.04)	-0.282*** (0.012)	-	0.150** (0.061)	-0.817*** (0.090)
Born 8+ years after	-	0.096** (0.041)	-0.290*** (0.017)	-	0.162** (0.071)	-0.804*** (0.092)
Born 3-5 years *before* equitable division of property laws passed	0.010 (0.010)	-0.009 (0.007)	0.013 (0.014)	0.027 (0.018)	0.017* (0.009)	-0.037 (0.053)
Born 1-2 years *before*	0.017 (0.018)	-0.011 (0.011)	0.000 (0.016)	0.067** (0.029)	0.040** (0.017)	-0.030 (0.068)
Born 0-1 years after	0.013 (0.038)	-0.018 (0.017)	-0.017 (0.019)	0.039 (0.050)	0.044* (0.023)	-0.006 (0.078)
Born 2-4 years after	0.011 (0.040)	-0.032 (0.020)	-0.023 (0.021)	0.011 (0.054)	0.049* (0.028)	0.021 (0.080)
Born 5-7 years after	0.032 (0.044)	-0.037 (0.026)	-0.028 (0.027)	0.016 (0.067)	0.067* (0.038)	0.003 (0.090)
Born 8+ years after	0.020 (0.053)	-0.036 (0.033)	-0.035 (0.027)	0.011 (0.077)	0.077* (0.040)	0.011 (0.098)
Constant	3.412	2.917	1.515	3.234	2.825	1.969
Adjusted R^2	0.998	0.998	0.997	0.970	0.970	0.961
Root MSE	0.034	0.035	0.040	0.123	0.124	0.149
No. of observations	1,785	2,550	2,550	9,430	12,170	3,815

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.
Notes: All specifications control for state of residence, year, cohort, age and state-specific year and age fixed effects. Model 2 specifications also control for state of birth as well as state of origin and state of destination (for individuals who moved between time of birth and during teenage ages) fixed effects. Model 2 excludes data for 2000-2010 since IPUMS female population counts are distorted by group quarters accounting in the American Community Survey. Despite this, the number of observations is larger for Model 2 since the observation unit for Model 2 is specific at both the state of residence and the state of birth levels, rather than only at the state of residence level.

3.6 Discussion

The U.S. teenage birth rate fell by more than a third between 1991 and 2010. Previous studies have focused on two fundamentally different sets of explanations: "cohort" factors, which cause teenagers in the 1990-2000s to be different from previous cohorts, and "period" factors, which cause conditions for teenage childbearing to be different during the 1990-2000s. This chapter contributes to the literature in two ways.

First, it uses age-period-cohort analysis to show that the decline in the teenage birth rate was initially driven by period changes, but that the decline between the mid-1990s and mid-2000s was largely driven by cohort changes. Hence, policy changes in the early 1970s, such as increased access to the birth control pill for minors and abortion for adult women are unlikely to be major explanations for the decline in teenage birth rates in the past two decades. Second, it uses difference-in-differences models to examine the extent to which four social policy changes in the 1960s-1980s can account for these cohort changes. I find that cohorts who were born after abortion was legalized for adult women were less likely to become teenage mothers, especially among cohorts born 2-7 years after legalization, suggesting that the impacts were offset by other cohort changes. In addition, exposure to unilateral divorce laws are associated with higher teenage birth rates among cohorts born in the 1970s, but with lower birth rates among cohorts born in the 1980s. The evidence suggests that the teenage baby bust is partly an unintended and unforeseen consequence of social policies in the 1970s, particularly legalization of abortion for adult women and unilateral divorce laws.

Beyond the four policy changes examined in this study (access to the birth control pill; legalization of abortion; adoption of unilateral divorce laws; and adoption of equitable division of property divorce laws), other social policies may also help to explain trends in teenage birth rates in the last two decades. For example, the introduction of high school exit examinations in the late 1990s and 2000s resulted in lower high school graduation rates (Marcotte 2013), which may help to explain the rise in cohort effects among women born in the 1970s. Another potential explanation which has been explored in the literature is the rise in incarceration rates since the early 1980s, particularly among black men, which may have lowered the supply of male partners in the sexual relationship market and increased the risk of being infected by sexually transmitted diseases (Johnson and Raphael 2009), reducing the utility from sexual activity and childbearing for female teenagers.[11] Kamdar (2008) and Mechoulan (2011) find that male incarceration rates had a large negative impact on local teenage fertility – indeed, Mechoulan's estimates account for more than 100% of the observed decline in teenage birth rates in the 1990s. However, the studies leave several puzzles unanswered. First, they find that male incarceration rates are predictive of teenage fertility behavior in the 1990s, but not in the 1980s (Kamdar 2007). Second, Mechoulan (2011)'s estimates are largely driven by results for Texas, whereas the decline in teenage fertility is across all states (Ventura, Matthews and Hamilton 2002). An alternative hypothesis is that the rise in male incarceration rates may have reduced teenage birth rates not only through reducing the utility from sexual activity and childbearing for female teenagers, but also through improving maternal educational and labor outcomes and family environments for the next generation, consistent with the decline in cohort effects for women born in

[11] In theory, the fall in supply of male partners in the sexual relationship market may also lead to an increase in the average number of sexual partners per male, resulting in an overall increase in pregnancies and births. Kamdar (2008) and Mechoulan (2011), however, find that the correlation between male incarceration and fertility is overall negative, at least in the case of teenage women.

the 1980s and 1990s from the age-period-cohort analysis. The evidence suggests, for example, that higher male incarceration levels led to lower probability of marriage among women, but also lower probability of teenage childbearing as well as higher female educational attainment and labor supply (Charles and Luoh 2010; Mechoulan 2011).

While the evidence suggests that the teenage baby bust is partly the result of social policies between the 1970s and 2000s, it is likely that the period and cohort changes are largely driven by general social and health trends. As Kearney and Levine (2014) point out, the decline in U.S. teenage birth rates coincided with similar trends in other developed countries, and is therefore unlikely to be solely due to national policies – instead, rising female educational attainment, advancements in contraceptive technologies as well as falling serum lead levels in children since the early 1980s, which are associated with IQ and emotional development (Cook and Laub 2002), may be more important. Hogan, Sun and Cornwell (2000) find that the decline in teenage childbearing risks between 1985 and 1995 can be partly attributed to a 10% increase in the probability of having a parent with more than high school education, although the effects are offset by a 5% increase in the probability having cohabiting rather than married parents. Similarly, Manlove et al. (2000; 2009) find that compared to their counterparts in the 1980s, teenagers in the 1990s are more likely to engage in sexual activity, potentially due to higher risk of family disruption, but also more likely to use contraceptives, potentially due to increased maternal age and education as well as smaller family sizes.

The literature also suggests that falling unemployment rates in the 1990s may explain part of the decline in teenage birth rates. While Colen, Geronimus and Phipps (2006) provide support for this hypothesis, other studies suggest that local unemployment levels and job losses reduce rather than increase teenage fertility among black women, particularly among older and socioeconomically advantaged teenagers (Ananat, Gassman-Pines and Gibson-Davis 2013; Arkes and Klerman 2009). Kearney and Levine (2013) estimate that trends in unemployment rates explain only 5.5% of the decline in teenage birth rates between 1991 and 2008 (although the Great Recession may explain as much as 37% of the decline in teen birth rates between 2008 and 2012).

Finally, there is also mixed evidence for the role of HIV and AIDS prevalence rates, which have been rising since the 1980s, although the numbers of new AIDS cases and deaths have been declining since the mid-1990s (Centers for Disease Control and Prevention 2011). Ahituv, Hotz and Philipson (1996) show that local AIDS prevalence rates led to increased condom use, especially among blacks, and estimate that the AIDS epidemic accounts for half of the rise in condom use in the late 1980s. Their study is consistent with qualitative work by Katz et al. (1995), who report that the majority of public school students as well as incarcerated and emotionally disturbed adolescents stated that the AIDS epidemic affected their choice of sexual partners. There is also evidence that use of contraception, particularly condoms, rose between 1982 and 1995 but declined slightly by 2002 (Mosher and Jones 2010), which may reflect the decline in new AIDS cases and deaths. Several other studies, however, suggest that small fluctuations in local AIDS incidence rates have no impact on contraceptive use among teenagers, with mixed evidence on their impact on teenage sexual activity (Corman 2002; Levine 2001) – instead, teenage pregnancy rates appear to be responsive only to large changes in incidence rates (Altman-Palm and Tremblay 1998).

Conclusion

This dissertation consists of two original empirical studies which address two prominent puzzles in the above literature review on teenage childbearing in the United States. The first study addresses the contradictory findings from quasi-experimental studies as to whether years of education have a negative causal impact on teenage childbearing. In particular, while studies using compulsory schooling laws show a significant impact, studies using school entry laws find no causal relationship. This study seeks to reconcile these contradictory findings by re-examining the impacts of school entry laws using a highly detailed North Carolina administrative dataset which links birth certificate data to school administrative records. Consistent with previous research (Cook and Kang 2013), the evidence suggests that being born after the school entry cutoff date affects educational success in offsetting ways, with a negative impact on years of education but positive impact on test scores. Using an IV regression strategy to distinguish the impacts of years of education and test scores, I show that both measures of educational success have negative causal impacts on teenage childbearing, consistent with the evidence from changes in compulsory schooling laws.

The empirical results in this study are generally consistent with previous research using discontinuities in school entry age. The estimated discontinuities in years of education and fertility are similar to those in McCrary and Royer (2011), Cook and Kang (2013) and Dobkin and Ferreira (2010). The discontinuity results for test scores are also in line with the work by Bedard and Dhuey (2006), Cook and Kang (2013) and Elder and Lubotsky (2008), who similarly find that discontinuities in test scores persist up to the eighth grade and tend to be larger for girls from more advantaged family backgrounds. Finally, the IV regression results support the arguments of Dobkin and Ferreira (2010) and Cook and Kang (2013), who suggest that the mixed impact of school entry age on educational success accounts for its ambiguous impacts on multiple life outcomes, including employment rates, wages and youth criminality.

There are several potentially important limitations to this study. Although it provides some evidence that school entry laws affect teenage motherhood primarily through their impacts on educational success rather than through differences in relative peer characteristics, the dataset does not allow me to fully control for the latter. In addition, unlike in McCrary and Royer (2011), the dataset is too small to extend the analysis to infant health, since only a small fraction of observations gave birth as teenagers. Finally, the analysis in this chapter does not indicate whether the negative impacts of education on teenage childbearing are due to increased pregnancy prevention or higher abortion rates, which is likely to be important from a policy perspective.

The second study examines potential causes behind recent trends in the U.S. teenage birth rate. Previous studies have focused on two fundamentally different sets of explanations: "cohort" factors, which cause teenagers in the 1990-2000s to be different from previous cohorts, and "period" factors, which cause conditions for teenage childbearing to be different during the 1990-2000s. This study uses age-period-cohort analysis to investigate the extent to which the decline in teenage birth rates reflects changes in cohort behavior rather than simultaneous declines across multiple birth cohorts. I also use a difference-in-differences model to investigate the extent to which four social policies (access to the *birth control pill*; legalization of *abortion*; adoption of *unilateral divorce* laws; and adoption of *equitable division of property* divorce laws) could have caused teenagers born in the 1970s-1980s to behave differently from previous birth cohorts. I provide evidence suggesting that the decline in the teenage birth rate was initially driven by period changes, but that the decline between the mid-1990s and mid-2000s was largely driven by cohort changes. Hence, policy changes in the early 1970s, such as increased access to abortion for adult women, are unlikely to be major explanations for the decline in the teenage birth rate. Nevertheless, I find that the teenage baby bust is partly an unintended and unforeseen consequence of social policies in the 1970s, particularly legalization of abortion for adult women and unilateral divorce laws.

One important limitation of this chapter is the lack of discussion regarding Hispanic ethnicity, due in large part to the fact that birth certificate data on ethnicity were collected by all states only from 1993 onwards. Hence, the analysis does not take into account the increase in the proportion of teenagers of Hispanic ethnicity from 13% to 20% between 1994 and 2010 (Kearney and Levine 2013), which may explain some of the observed cohort or period changes. For example, it may help to explain the decline in teenage sexual activity, since Hispanic teenagers are on average older at first intercourse, although they are less likely to use contraceptives and overall at higher risk of teenage childbearing (Manlove et al. 2009). Further investigation may also indicate whether period and cohort factors and impacts of social policies on teenage fertility outcomes differ by ethnicity.

Both studies have implications for public policy. The first study suggests that the quasi-experimental literature points strongly and consistently to a negative causal relationship between educational attainment and teenage fertility. While the results support previous findings that school entry laws are not associated with changes in teenage fertility, they also suggest that other educational policies which are likely to increase both years of education and test scores, such as extending the school day or school year, would be likely to reduce teenage childbearing. The second study provides evidence on the potentially unforeseen consequences of two major U.S. social policies, increased access to abortion and liberalized divorce laws, on adolescent childbearing outcomes in the next generation.

Beyond the contributions of this dissertation, the literature review in the first chapter points to a number of other promising avenues for future research. While current research has produced compelling evidence that much of the observed adverse educational and economic outcomes associated with teenage childbearing are attributable to negative selection, much less is known as to how the impacts of teenage motherhood depend on socioeconomic status and pregnancy intentions. A stronger understanding of heterogeneous impacts is useful not only for identifying groups for which public intervention would be most and least justified, but may also allow for more effective targeting. There is also relatively little research exploring Furstenberg (1991)'s intriguing hypothesis that teenage women often fail to take proper precautions against pregnancy or choose not to get an abortion not because they are ambiguous about the prospect of becoming teenage mothers, but rather because they are overwhelmed by the "confused and confusing messages" regarding the ethics and permissibility of sexual activity. A better grasp of how different groups of teenagers navigate through ethically and socially contradictory environments may provide valuable insight into how public policy could design a more consistent, healthy and supportive society conducive for adolescents to pursue the aspirations and life plans they say they prefer.

Appendix A

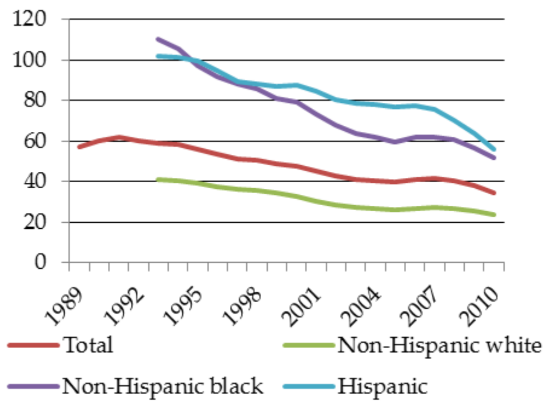
A1 Estimating Births, Pregnancies and Abortions, by Race and Ethnicity

Statistics on annual births, pregnancies and abortions published by the National Vital Statistics Reports are based on the following sources. The number of live births to teenage women in a given year is estimated using birth certificate data. The number of induced abortions to teenage women in a given year is estimated using CDC surveillance data collected from each state, which is adjusted by the Guttmacher Institute based on their survey of abortion providers. In 2008, data for five states (California, Florida, Maryland, New Hampshire and Wyoming) were not available and national estimates are based on interpolation between years when data are available. The proportion of teenage pregnancies ending in involuntary fetal losses is estimated using data from four waves of the National Survey of Family Growth: 1988, 1995, 2002 and 2006-2010. Given the small proportion of pregnancies ending in fetal loss, the average of proportions across all four waves is used. Finally, the number of pregnancies to teenage women in a given year is estimated by summing the numbers of live births, induced abortions and pregnancies ending in involuntary fetal losses (Ventura, Curtin and Abma 2012).

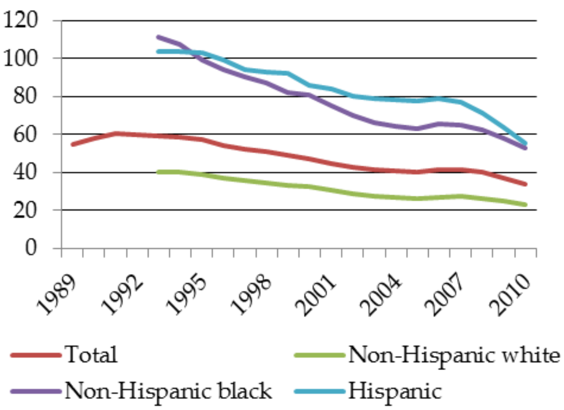
These statistics are available by race and ethnicity, based on self-reported data from birth certificates and the National Survey of Family Growth. (Data on ethnicity are collected through birth certificates from all 50 states beginning in 1993.) The three most commonly reported categories are non-Hispanic white, non-Hispanic black and Hispanic. The classification of race and ethnicity are subjective and depend in part on socioeconomic class, where individuals occupying higher social positions are more likely to both self-identify and be labelled by interviewers as white, while individuals occupying lower positions are more likely to self-identify and be labelled as black (Sapenstein and Penner 2012).

A2 Teenage Birth and Fertility Rates, 1989-2010

A. Teenage birth rate



B. Teenage fertility rate



Notes: Fertility rates are computed using birth data from Vital Statistics and Census population estimates.

Appendix B

B1 Maternal Characteristics in North Carolina, 1987-1992

	Total	% white	% below 20 years old	% with less than high school degree	% unmarried
	Vital Statistics				
1987	93,501	69.3	15.7	23.2	24.9
1988	97,579	67.6	16.0	23.2	26.3
1989	102,105	66.8	16.4	22.9	27.7
1990	104,525	66.5	16.2	22.7	29.4
1991	102,362	66.0	16.2	23.2	31.6
1992	103,967	65.9	15.4	22.1	31.3
	Birth certificates				
1987	89,704	69.0	15.2	22.7	24.1
1988	93,507	67.2	15.4	22.7	25.5
1989	97,996	66.5	15.9	22.3	27.0
1990	100,357	66.2	15.7	22.1	28.7
1991	98,123	65.7	15.7	22.6	30.8
1992	99,811	65.6	15.0	21.5	30.5

Notes: Vital Statistics data include out-of-state births to mothers residing in North Carolina, while birth certificate data include only in-state births to mothers residing in North Carolina. Vital Statistics data were obtained online from the Centers for Disease Control and Prevention at http://www.cdc.gov/nchs/data_access/vitalstats/VitalStats_Births.htm. Birth certificate data were obtained from the North Carolina Education Research Data Center.

B2 Advantages of the Dataset for the Study's Purposes

In addition to containing information on the number of grades each individual attended by age 20 as well as her performance in end-of-grade tests in the third and eighth grades, this dataset has a number of advantages for studying the impact of education on teenage motherhood.

First, this dataset uses individuals' administrative school records up to age 20 rather than self-reported age and education data from birth certificates, which are not only higher quality but are also representative of individuals' final educational attainment since the majority of girls who give birth at school ages eventually return and graduate from high school (Mott and Marsiglio 1985; Upchurch and MacCarthy 1990). Comparing data from administrative school records and data from birth certificates for mothers aged 18 or below in this dataset, I find that while actual age and self-reported age on birth certificates are almost always identical (99.2%), there is substantial disagreement between administrative records of grades attended and self-reported years of education. In particular, administrative records of number of grades attended exceed self-reported years of education for 40.3% of observations (and fall below self-reported years of education for another 19.7%, possibly due to private schooling or over-reporting.)

Second, this dataset excludes girls who did not attend in-state public school and are hence more likely to have moved out of state. Since births to these girls are more likely to be unobserved, excluding these observations from the sample reduces potential downward bias on birth probabilities. Moreover, data on teenage childbearing outcomes are available at the individual rather than the cohort level. Hence, this dataset allows for more unbiased and precise estimation of the impact of school entry age on early childbearing.

Third, this dataset includes all girls from six birth cohorts who attended public school in North Carolina between third grade and age 15, rather than a more restricted sample which only includes girls who have had live births. Hence, the analysis of the impact of school entry age on educational success in this study applies to a broader socioeconomic spectrum.

Fourth, this dataset contains more detailed information about characteristics at the individual and school and school district levels, allowing a more effective search for heterogeneous treatment effects.

B3 Impact of Being Born After the Cutoff Date on Educational Attainment

	Entire sample	Sample restricted to teenagers who gave birth before age 19
Grades attended	-0.130**	-0.361*
Attended 12th grade	-0.052**	-0.173**
Years of schooling	-0.186***	-0.351*
Self-reported years of education	-	-0.290
Number of observations	68,771	6,749

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: Years of schooling refer to the number of years an individual was registered in the public pre-K12 system after third grade. Each value refers to the coefficient on being born after the school entry cutoff date from an individual regression, with the dependent variable given in the first column. Coefficients are estimated using regression discontinuity models and the 75-day window, controlling for individual, school and school district characteristics (see Table 1) and cohort fixed effects.

B4 Heterogeneity in Compliance Rates and Impacts of School Entry Age

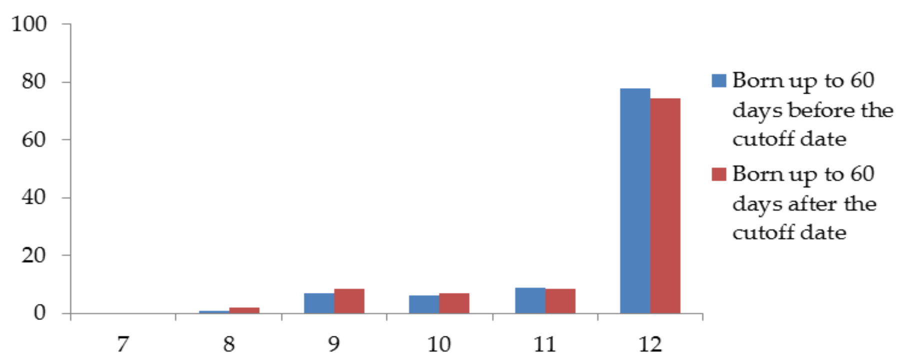
In practice, parents may choose not to comply with school entry laws by sending their children to private or out-of-state kindergartens with different entry cutoff dates, or by not sending their children to kindergarten at all (Cook and Kang 2013). Hence, the regression discontinuity results in this study reflect "intention-to-treat" effects which may be more strongly driven by some socioeconomic groups. Previous studies suggest that compliance with school entry laws are generally high (Elder and Lubotsky 2008), especially for girls (Cook and Kang 2013). In this dataset, almost all girls born after the cutoff date started third grade at an older age than the rest of their birth cohort peers, with 92.0% and 5.7% starting at ages 9 and 10, and only 2.3% starting at age 8. (I use age at third grade since data on age at kindergarten are not available.) Compliance rates were substantially lower among girls born before the cutoff date due to higher than expected ages at kindergarten entry and higher retention rates between kindergarten and third grade: only 77.5% started third grade at age 8, with 21.7% and 0.8% starting at ages 9 and 10. (Compliance rates are highest for the 75-day bandwidth and lowest for the 45-day bandwidth: for the former, 78.8% and 92.0% of girls born before and after the cutoff date start third grade at the expected ages, compared to75.9% and 91.9% for the 45-day bandwidth.) By eighth grade, compliance rates have fallen even further, with only 71.2% and 90.0% of girls born up to 60 days before and after the cutoff date starting eighth grade earlier than or at the expected ages.

There are two plausible reasons for why so many girls born just before the cutoff date start third grade at age 9 rather than at age 8. First, parents may feel that their children are still too young to start kindergarten. Second, girls born just before the cutoff date tend to have much weaker school performance and are hence more likely to be held back a year in kindergarten, first or second grade. Using nationally representative data, Bedard and Dhuey (2006) find that the first reason accounts for 57.4% of students born up to 30 days before state cutoff dates who enter third grade at ages 9 or older, while the second reason accounts for 42.6%. (In their sample, 41.4% of students born before the cutoff date enter third grade at higher-than-expected ages, which is substantially higher than in this study, possibly due to their narrower window and the inclusion of boys, who are more likely to be held back.) On the other hand, statistics on retention rates between kindergarten and third grade in North Carolina public schools suggest that the second reason is likely to be more important. According to the Kindergarten Readiness Issue Group (2003), the probability of being retained between kindergarten and third grade rose from around 9% in 1991-1992 to around 17% in 2001-2002, possibly reflecting the large increase in the proportion of students from Hispanic immigrant families during this period. Since only 11.6% of girls in this dataset start third grade at higher-than-expected ages, a majority of them are likely to have been retained at an early age.

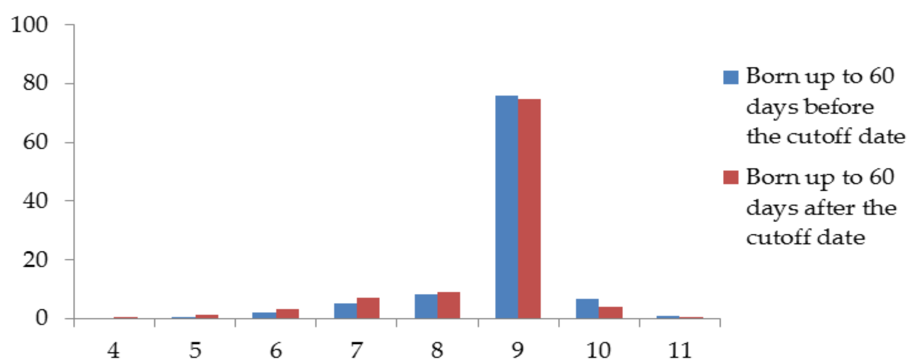
Comparing ages at third grade among various socioeconomic groups, the evidence suggests that girls born shortly before the cutoff date who started third grade at age 9 or older, rather than at age 8, were disproportionately likely to come from disadvantaged families. Compared to the girls who started third grade at age 8, they were 1.6% and 8.3% less likely to be born to white and married mothers, and their mothers have on average 0.7 and 0.6 fewer years of age and education. (The socioeconomic differences are larger in eighth grade, where girls who start eighth grade older than age 13 are 6.7% and 12.4% less likely to be born to white and married mothers, and their mothers have on average 1.10 and 0.85 fewer years of age and education.) These results for North Carolina differ from those in Dobkin and Ferreira (2010), who find lower compliance rates among individuals from white and more educated families, rather than from disadvantaged families, for California and Texas.

B5 Years of Education, Highest Grade Attended and Age at School Leaving

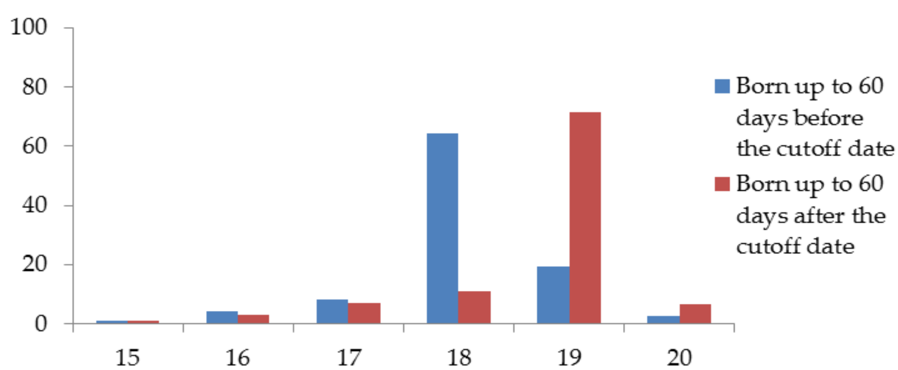
A. Number of grades attended



B. Number of years of schooling (After third grade)



C. Age at school leaving



Notes: An individual's age at school leaving is calculated using her age at her last end-of-grade exam in the public school system by age 20.

B6 IV Regression First Stage and Diagnostic Test Results

	45-day	60-day	75-day
	Endogenous variables: educational attainment only		
Grades attended			
No. of days born after July 2	0.000	0.000	0.001**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	-0.058	-0.080**	-0.112***
	(0.041)	(0.039)	(0.034)
Cragg-Donald statistic(1, 8)	9.707	10.804	12.029
Anderson-Rubin $\chi^2(7)$	11.188	6.335	16.060**
Basmann F-statistic(7, *N* - 22)	1.597	0.904	2.293**
Attended 12th grade			
No. of days born after July 2	0.000	0.000*	0.000**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	-0.038*	-0.043**	-0.051***
	(0.021)	(0.018)	(0.016)
Cragg-Donald statistic(1, 8)	10.152	12.227	15.377
Anderson-Rubin $\chi^2(7)$	10.082	5.956	15.565**
Basmann F-statistic(7, *N* - 22)	1.439	0.850	2.222**
Years of schooling			
No. of days born after July 2	0.001	0.001**	0.001**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	-0.167***	-0.179***	-0.199***
	(0.041)	(0.037)	(0.032)
Cragg-Donald statistic(1, 8)	25.142	29.659	33.371
Anderson-Rubin $\chi^2(7)$	11.882	6.202	15.469**
Basmann F-statistic(7, *N* - 22)	1.696	0.885	2.208**
	Endogenous variables: educational attainment and 3rd grade test scores		
Grades attended			
No. of days born after July 2	0.000	0.000	0.001**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	-0.058	-0.080**	-0.112***
	(0.041)	(0.039)	(0.034)
3rd grade test scores			
No. of days born after July 2	-0.000	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)
Born after cutoff date	0.287***	0.293***	0.265***
	(0.034)	(0.031)	(0.028)
Cragg-Donald statistic(2, 8)	1.700†	1.405†	2.230†
Anderson-Rubin $\chi^2(6)$	10.368	4.987	5.320
Basmann F-statistic(6, *N* - 22)	1.726	0.830	0.886

Attended 12th grade			
No. of days born after July 2	0.000	0.000*	0.000**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	-0.038*	-0.043**	-0.051***
	(0.021)	(0.018)	(0.016)
3rd grade test scores			
No. of days born after July 2	-0.000	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)
Born after cutoff date	0.287***	0.293***	0.265***
	(0.034)	(0.031)	(0.028)
Cragg-Donald statistic(2, 8)	4.291	5.922	8.358
Anderson-Rubin $\chi^2(6)$	9.767	4.674	10.618
Basmann F-statistic(6, *N* - 22)	1.626	0.778	1.769
Years of schooling			
No. of days born after July 2	0.001	0.001**	0.001**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	-0.167***	-0.179***	-0.199***
	(0.041)	(0.037)	(0.032)
3rd grade test scores			
No. of days born after July 2	-0.000	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)
Born after cutoff date	0.287***	0.293***	0.265***
	(0.034)	(0.031)	(0.028)
Cragg-Donald statistic(2, 8)	1.413†	1.174†	2.305†
Anderson-Rubin $\chi^2(6)$	9.552	6.202	10.639
Basmann F-statistic(6, *N* - 22)	1.590	1.033	1.772
Endogenous variables: educational attainment and 8th grade test scores			
Grades attended			
No. of days born after July 2	0.000	0.000	0.001**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	-0.058	-0.080**	-0.112***
	(0.041)	(0.039)	(0.034)
8th grade test scores			
No. of days born after July 2	0.000	-0.000**	-0.000**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	0.132***	0.158***	0.140***
	(0.033)	(0.031)	(0.028)
Cragg-Donald statistic(2, 8)	1.165†	1.057†	1.811†
Anderson-Rubin $\chi^2(6)$	7.099	4.054	4.033
Basmann F-statistic(6, *N* - 22)	1.182	0.675	0.672

Attended 12th grade			
No. of days born after July 2	0.000	0.000*	0.000**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	-0.038*	-0.043**	-0.051***
	(0.021)	(0.018)	(0.016)
8th grade test scores			
No. of days born after July 2	0.000	-0.000*	-0.000**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	0.132***	0.158***	0.140***
	(0.033)	(0.031)	(0.028)
Cragg-Donald statistic(2, 8)	3.817	5.324	7.221
Anderson-Rubin $\chi^2(6)$	9.513	4.637	10.409
Basmann F-statistic(6, N - 22)	1.584	0.772	1.734
Years of schooling			
No. of days born after July 2	0.001	0.001**	0.001**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	-0.167***	-0.179***	-0.199***
	(0.041)	(0.037)	(0.032)
8th grade test scores			
No. of days born after July 2	0.000	-0.000*	-0.000**
	(0.000)	(0.000)	(0.000)
Born after cutoff date	0.132***	0.158***	0.140***
	(0.033)	(0.031)	(0.028)
Cragg-Donald statistic(2, 8)	1.193	1.057†	1.901†
Anderson-Rubin $\chi^2(6)$	9.517	6.091	9.529
Basmann F-statistic(6, N - 22)	1.584	1.014	1.587
Number of observations (N)	20,022	26,844	33,598

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: Years of schooling refer to the number of years an individual was registered in the public pre-K12 system after third grade. Each pair of values in the table refers to the coefficients on years of education and test scores from an individual regression, with the probability of motherhood by age 20 as the dependent variable and the endogenous variables given in italics. Coefficients are estimated using IV regression models, controlling for individual, school and school district characteristics (see Table 2) and cohort fixed effects.

B7 IV Regression Balancing Test Results

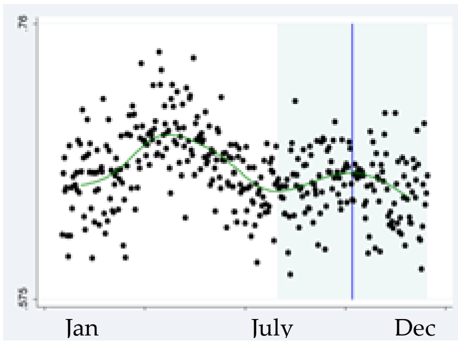
	Cutoff	Relative age in days	Cutoff* mother's race	Cutoff* mother's marital status	Cutoff* school poverty	Cutoff* school district poverty
Mother is white	-0.006	-0.347	0.491***	-0.003	-0.004	-0.001
	(0.005)	(0.434)	(0.041)	(0.004)	(0.003)	(0.001)
Mother's age	0.000	0.021	0.001*	0.001**	0.000	0.000
	(0.000)	(0.036)	(0.000)	(0.000)	(0.000)	(0.000)
Mother' education	-0.002**	-0.112	-0.001	-0.002**	-0.001**	-0.000**
	(0.001)	(0.095)	(0.001)	(0.001)	(0.001)	(0.003)
Mother is married	-0.003	-0.200	-0.003	0.491***	-0.002	-0.000
	(0.005)	(0.458)	(0.004)	(0.041)	(0.003)	(0.001)
Birth weight	0.000*	0.000	0.000	0.000	0.000**	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mother's first birth	0.001	-0.057	0.003	0.004	0.000	0.000
	(0.004)	(0.370)	(0.003)	(0.003)	(0.003)	(0.001)
School poverty rate	-0.005	-1.146	0.004	0.001	0.487***	-0.002
	(0.023)	(2.022)	(0.019)	(0.019)	(0.044)	(0.004)
Percent of school students passed their end-of-grade tests	-0.006 (0.058)	-3.674 (5.286)	0.029 (0.045)	0.020 (0.047)	-0.014 (0.038)	-0.004 (0.011)
No. of crimes per 100 students	0.001 (0.005)	-0.147 (0.400)	0.001 (0.003)	0.000 (0.003)	0.001 (0.003)	0.000 (0.001)
School district poverty rate	-0.025 (0.067)	-1.393 (6.095)	-0.034 (0.053)	-0.014 (0.055)	-0.000 (0.043)	0.492*** (0.043)
Percent of district students passed their end-of-grade tests	-0.045 (0.072)	-4.263 (6.845)	-0.043 (0.059)	-0.054 (0.061)	-0.024 (0.042)	-0.008 (0.014)
Percent of district students with one parent	-0.011 (0.064)	-4.172 (5.710)	-0.008 (0.053)	-0.027 (0.056)	-0.008 (0.040)	0.001 (0.013)
Constant	0.570	116.071	0.024	0.046	0.049	0.016
Number of observations			68,771			

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

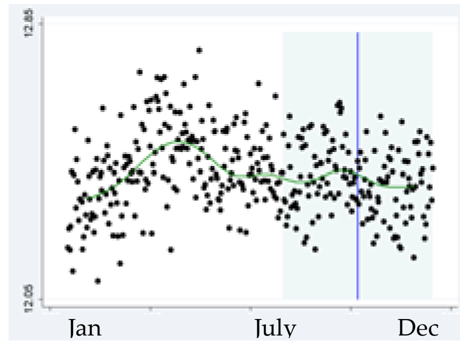
Notes: Each column in the table refers to the coefficients on individual, school and school district characteristics, with the dependent variable given in the top row. Coefficients are estimated using simple regression models and the 75-day window, controlling for cohort fixed effects.

B8 Background Characteristics by Date of Birth

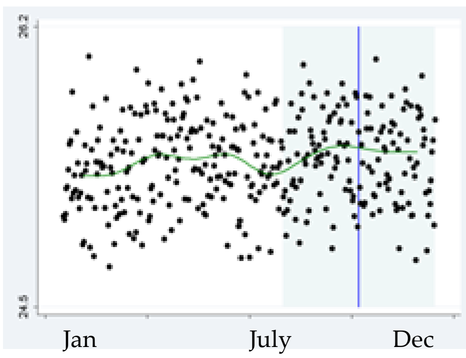
A. Proportion born to white mothers



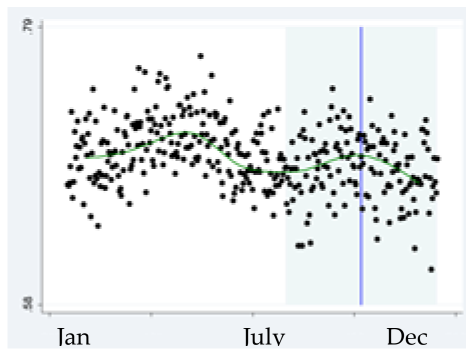
B. Average maternal education



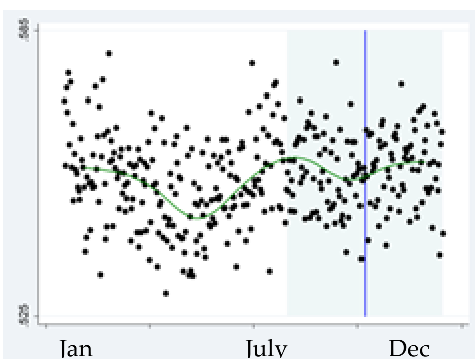
C. Average maternal age



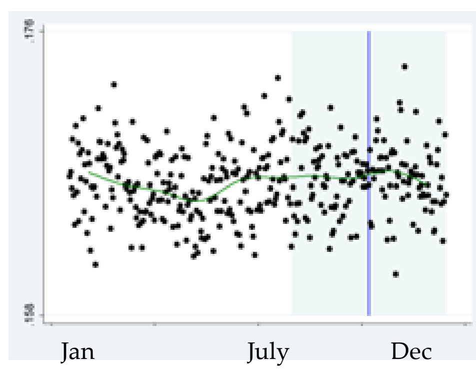
D. Proportion born to married mothers



E. Average school poverty levels



F. Average school district poverty levels



Notes: The horizontal axis represents the date of birth, where zero refers to July 2. The dark blue line marks the cutoff date (October 16) and the light blue area shows the observations included in the 75-day bandwidth. The green curve is a fitted spline line calculated using 10 cross-median knots.

B9 Maternal Characteristics at Time of Birth by Birth Month

	Mother is white	Mother is a high school graduate	Mother is a teenager	Mother is married
Month of birth				
January	-	-	-	-
February	0.002	-0.002	0.002	-0.000
March	0.022***	0.014***	-0.002	0.018***
April	0.038***	0.015***	-0.005	0.021***
May	0.029***	0.015***	-0.007	0.020***
June	0.010*	0.017***	-0.006	-0.002
July	-0.007	0.015***	0.000	-0.004
August	-0.002	0.008*	-0.003	-0.015**
September	0.005	0.019***	-0.004	0.003
October	0.003	0.013**	-0.006	-0.001
November	-0.003	0.004	0.002	0.010
December	-0.008	0.008*	0.004	-0.014**
Constant	0.652	0.752	0.168	0.691
Number of observations	164,237			

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

B10 Impact of Being Born After the Cutoff Date on Educational Attainment and Test Scores, With Imputed or Excluded Missing Data

	45-day	60-day	75-day
Grades attended			
Missing data imputed	-0.161	-0.131*	-0.137***
Missing data excluded	-0.163	-0.133*	-0.138***
Attended 12th grade			
Missing data imputed	-0.066	-0.059**	-0.055***
Missing data excluded	-0.069	-0.060**	-0.057***
Third grade test scores			
Missing data imputed	0.310***	0.338***	0.345***
Missing data excluded	0.310***	0.343***	0.349***
Eighth grade test scores			
Missing data imputed	0.173**	0.192***	0.200***
Missing data excluded	0.169**	0.186***	0.195***
Number of observations	41,139	54,993	68,771

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: Each value refers to the coefficient on being born after the school entry cutoff date from an individual regression, with the dependent variable given in bold. Coefficients are estimated using regression discontinuity models, controlling for individual, school and school district characteristics (see Table 2) and cohort fixed effects.

B11 Impact of Years of Education and Test Scores on Teenage Childbearing, with Excluded Instruments

	Excluded instruments						
	Cutoff	Relative age in days	Cutoff* cohort	Cutoff* mother's race	Cutoff* mother's race and cutoff* mother's marital status	Cutoff* mother's race and cutoff* school poverty	Cutoff* mother's race and cutoff* school district poverty
Attended 12th grade	-0.228**	-0.193*	-0.809**	-0.147	-0.143	-0.145	-0.145
3rd grade test scores	-0.050**	-0.049**	-0.124**	-0.039*	-0.039*	-0.040*	-0.039*
Evidence of weak instruments	No	No	Yes	No	No	No	No
Evidence of endogenous instruments	No	No	No	No	No	No	No
Number of observations	33,598						

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.
Notes: Each pair of values in the table refers to the coefficients on years of education and test scores from an individual regression, with the probability of motherhood by age 20 as the dependent variable. Coefficients are estimated using IV regression models and the 75-day window, controlling for individual, school and school district characteristics (see Table 2) and cohort fixed effects.

B12 Impact of Being Born after the Cutoff Date, Years of Education and Test Scores on Childbearing at Age 20

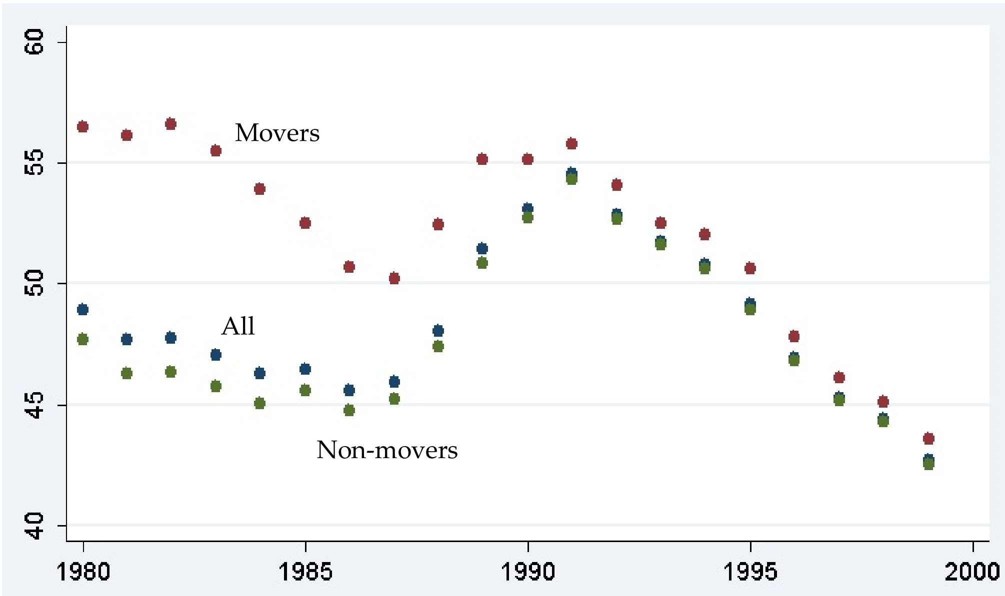
	45-day	60-day	75-day
Regression discontinuity			
No controls	-0.104*	-0.062	-0.027
Controls included	-0.112*	-0.068*	-0.030
IV regression			
Attended 12th grade	0.010	-0.202	-0.226
Third grade test scores	0.018	-0.022	-0.023
Evidence of weak instruments	Yes	Yes	Yes
Evidence of endogenous instruments	No	No	No
Number of observations	10,432	14,060	17,577

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: For regression discontinuity results, each value refers to the coefficient on being born after the cutoff date from an individual regression, with childbearing at age 20 (conditional on not having a teenage birth) as the dependent variable. Controls refer to individual, school and school district characteristics (see Table 2) and cohort fixed effects. For IV regression results, each pair of values in the table refers to the coefficients on years of education and test scores from an individual regression, with childbearing at age 20 (conditional on not having a teenage birth) as the dependent variable. All results are estimated controlling for individual, school and school district characteristics and cohort fixed effects.

Appendix C

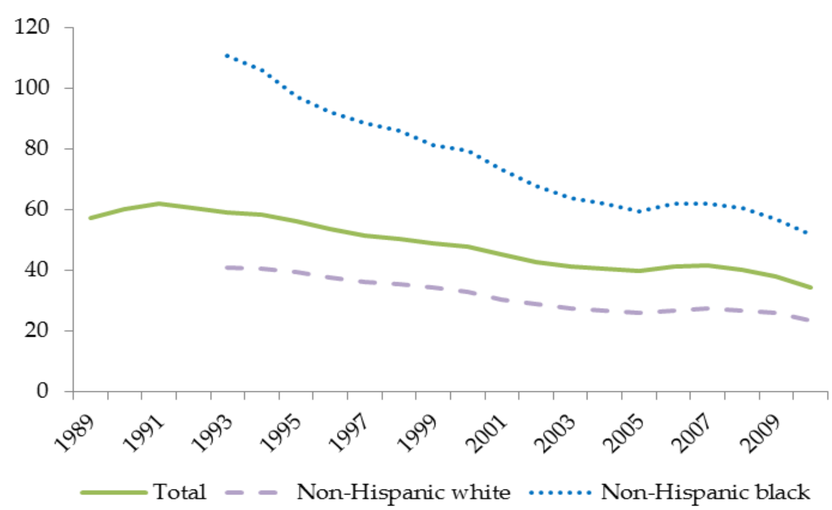
C1 Trends in Teenage Birth Rates by Change in State of Residence, 1980-2010



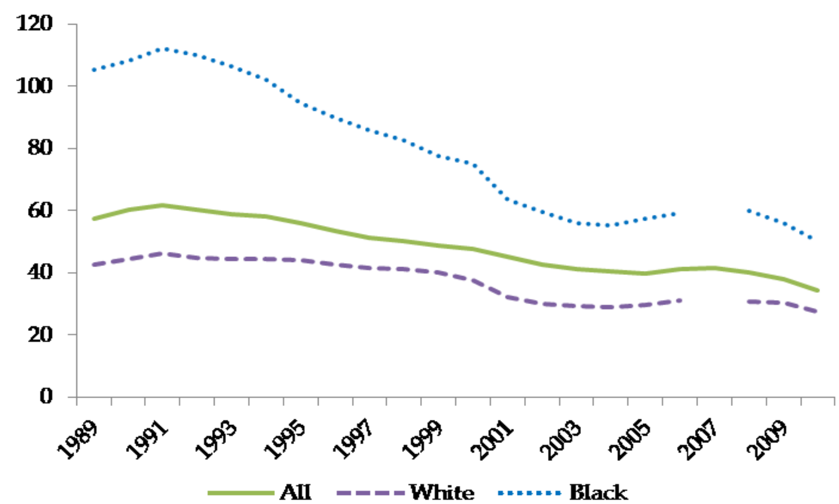
Notes: Movers refer to individuals whose state of residence is different from their mother's state of residence at the time of birth, while non-movers refer to individuals whose state of residence is the same as their mother's state of residence at the time of birth.

C2 Published and Calculated Teenage Birth Rates, 1989-2010

A. Published teenage birth rates



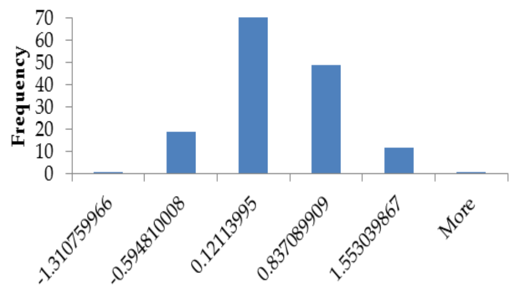
B. Calculated teenage birth rates



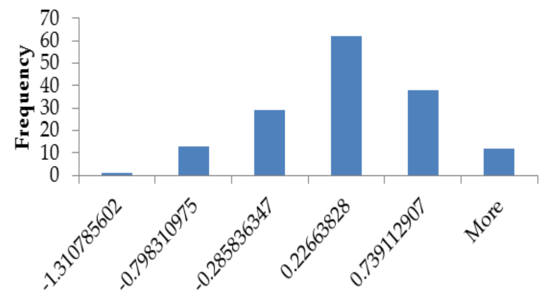
Notes: Teenage birth rates in Figure C2A are taken from Vital Statistics *Natality* Reports. Teenage birth rates in Figure C2B are calculated using de-identified birth certificate available online at http://www.cdc.gov/nchs/nvss/birth_methods.htm. Data by race are not available for 2007 for Figure C2B.

C3 Age-Period-Cohort Model Error Distributions

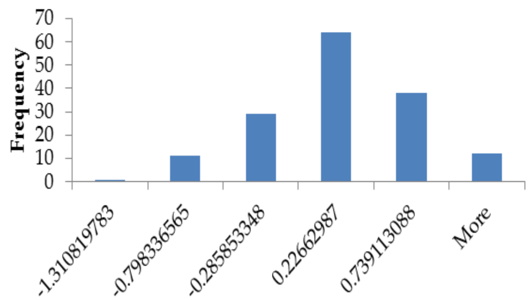
A. Equality-constraints model (Period$_{1981}$ = Period$_{1982}$)



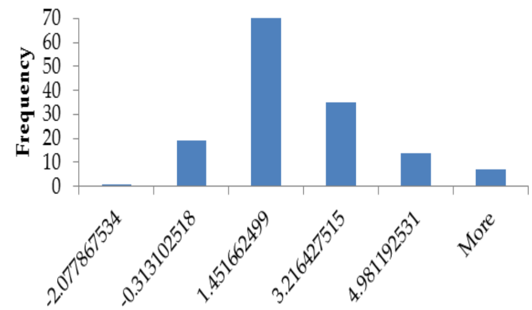
B. Equality-constraints model (Cohort$_{1962}$ = Cohort$_{1963}$)



C. Intrinsic estimator



D. Intrinsic estimator (normal)



Notes: All errors are based on log-linear models. The number of teenage births relative to the population at risk is modeled as a Poisson variate in the first three specifications, and as a normal variate in the fourth specification.

C4 Age-Period-Cohort Model Estimates for Teenage Birth Rates, 1980-2010

	Equality-constraints estimator		**Intrinsic estimator**	
	Period$_{1981}$ = Period$_{1982}$	Cohort$_{1962}$ = Cohort$_{1963}$	Poisson distribution	Normal distribution
Age effects				
15	-	-	0.326	0.329
16	2.106	2.119	0.704	0.709
17	3.435	3.477	1.176	1.173
18	4.885	4.976	1.714	1.704
19	6.015	6.164	2.163	2.146
Period effects				
1980	-	-	1.318	1.267
1981	-	0.970	1.255	1.227
1982	0.976	0.964	1.225	1.201
1983	0.957	0.940	1.172	1.144
1984	0.948	0.925	1.132	1.111
1985	0.967	0.938	1.127	1.113
1986	0.967	0.932	1.099	1.095
1987	0.971[a]	0.930	1.077	1.072
1988	1.000[c]	0.953[a]	1.083	1.086
1989	1.055	0.998[c]	1.114	1.125
1990	1.091	1.026[c]	1.124	1.151
1991	1.107	1.035[c]	1.113	1.140
1992	1.077	1.000[c]	1.056	1.083
1993	1.051[b]	1.971[c]	1.006	1.031[c]
1994	1.030[c]	0.945[a]	0.961	0.982[c]
1995	0.984[c]	0.898	0.897	0.916
1996	0.935[a]	0.847	0.831	0.843
1997	0.901	0.812	0.781	0.785
1998	0.898	0.804	0.760	0.756
1999	0.897	0.798	0.740	0.731
2000	0.927[a]	0.820	0.746	0.734
2001	0.949[c]	0.834	0.746	0.744
2002	0.985[c]	0.860	0.755	0.755
2003	1.053[c]	0.914[c]	0.787	0.793
2004	1.155	0.997[c]	0.843	0.849
2005	1.272	1.091[c]	0.905	0.914
2006	1.478	1.260	1.026	1.036[c]
2007	1.673	1.417	1.134	1.137
2008	1.826	1.538	1.207	1.202
2009	1.939	1.623	1.251	1.236
2010	1.981	1.648	1.247	1.231

Cohort effects				
1961	-	-	0.850	0.853
1962	1.004[c]	-	0.875	0.865
1963	0.998[c]	1.011[a]	0.892	0.875
1964	0.990[c]	1.008[c]	0.906	0.890
1965	0.992[c]	1.017[a]	0.931	0.918
1966	1.007[c]	1.038	0.968	0.954[a]
1967	1.004[c]	1.041	0.989	0.975[c]
1968	1.005[c]	1.049	1.015	1.001[c]
1969	0.979[c]	1.028[c]	1.013	1.007[c]
1970	0.974[c]	1.029[c]	1.033	1.024[c]
1971	0.983[c]	1.046[b]	1.070	1.058[a]
1972	1.020[c]	1.091	1.137	1.130
1973	1.057[a]	1.138	1.208	1.211
1974	1.065[a]	1.153	1.247	1.260
1975	1.060[a]	1.156	1.273	1.292
1976	1.093	1.198	1.345	1.371
1977	1.101	1.215	1.389	1.414
1978	1.119	1.242	1.447	1.469
1979	1.120	1.250	1.483	1.504
1980	1.094[a]	1.230	1.486	1.509
1981	1.063[c]	1.201	1.479	1.499
1982	1.004[c]	1.142	1.433	1.455
1983	0.936[c]	1.072[c]	1.369	1.378
1984	0.852	0.981[c]	1.277	1.275
1985	0.763	0.884[a]	1.172	1.165
1986	0.691	0.806	1.089	1.082
1987	0.621	0.728	1.001[c]	0.993[c]
1988	0.555	0.655	0.917	0.903
1989	0.490	0.581	0.830	0.816
1990	0.424	0.507	0.737	0.728
1991	0.381	0.458	0.679	0.673
1992	0.332	0.401	0.605	0.604
1993	0.290	0.353	0.543	0.550
1994	0.256	0.314	0.492	0.506
1995	0.220	0.271	0.433	0.443
Constant	0.015	0.015	0.037	0.038

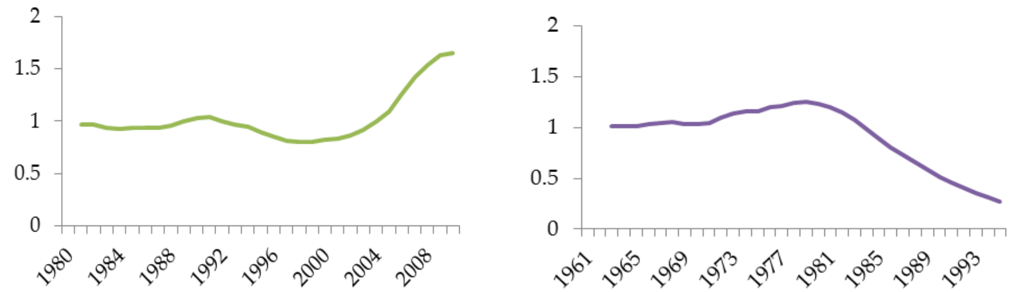
[a] $p > 0.01$, [b] $p > 0.05$, [c] $p > 0.1$

Notes: All results are produced using log-linear models, so that each estimate represents a multiplicative age, cohort or period effect, relative to a base factor of 1. The number of teenage births relative to the population at risk is modeled as a Poisson variate in the first three specifications, and as a normal variate in the fourth specification. All results are rounded to 3 decimal places.

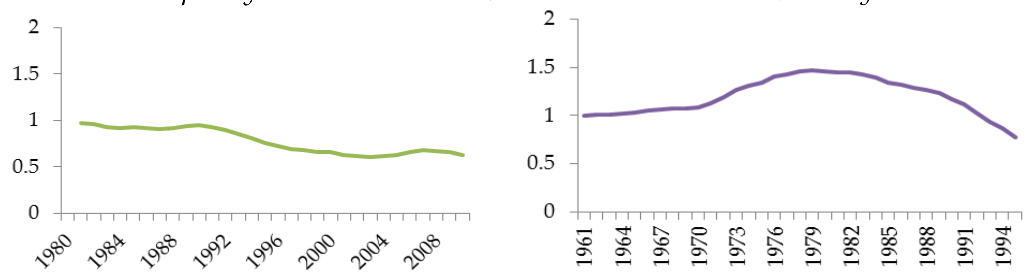
C5 Period and Cohort Effects in Birth Rates for 15-19 and 15-24 Year-Olds, 1980-2010

Period **Cohort**

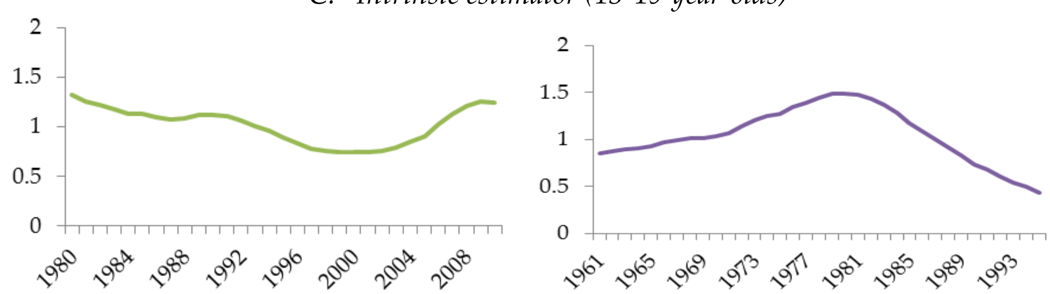
A. *Equality-constraints model (Cohort1962 = Cohort1963) (15-19 year-olds)*



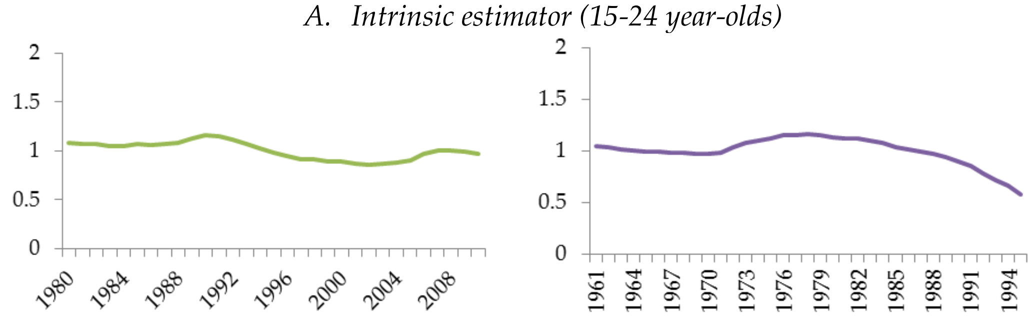
B. *Equality-constraints model (Cohort1957 = Cohort1958) (15-24 year-olds)*



C. *Intrinsic estimator (15-19 year-olds)*

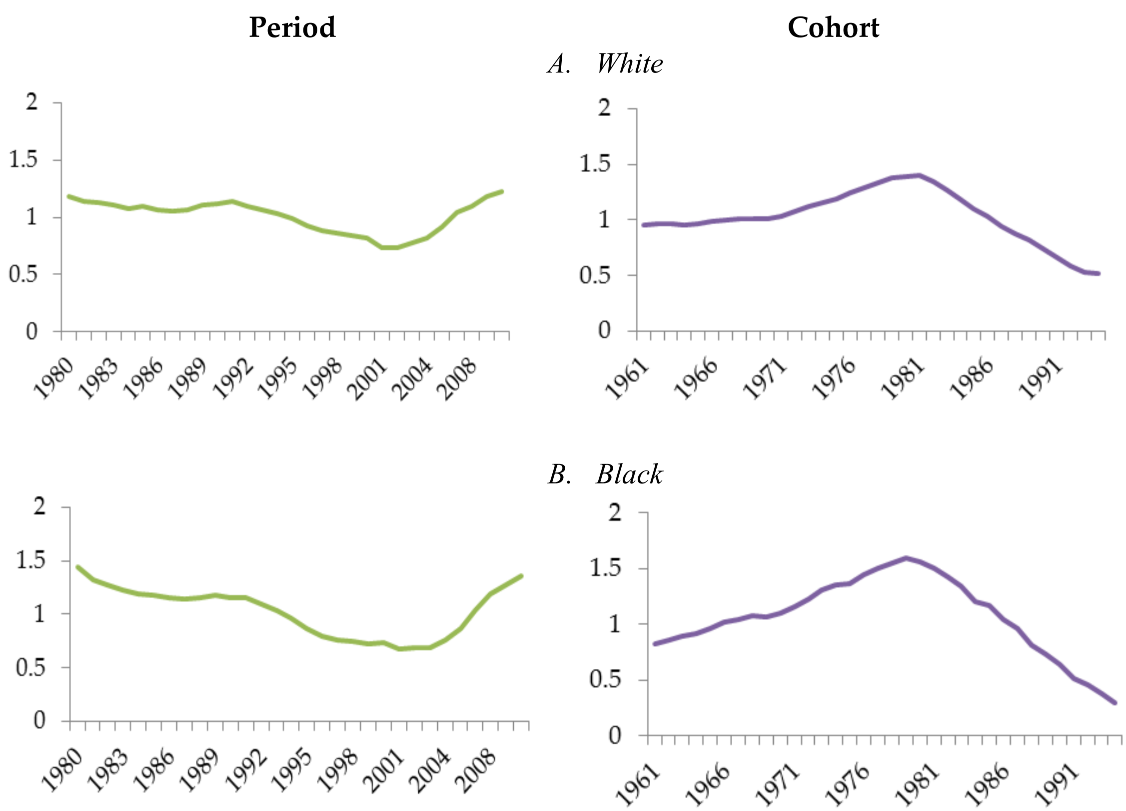


A. *Intrinsic estimator (15-24 year-olds)*



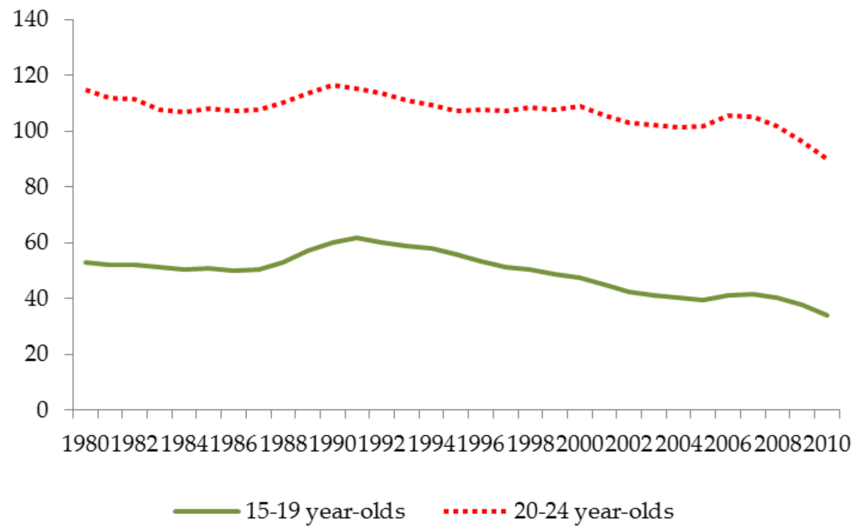
Notes: All estimates are produced using a log-linear model, where the number of births relative to the population at risk is modeled as a Poisson variate.

C6 Period and Cohort Effects in Teenage Birth Rates by Race, 1980-2010



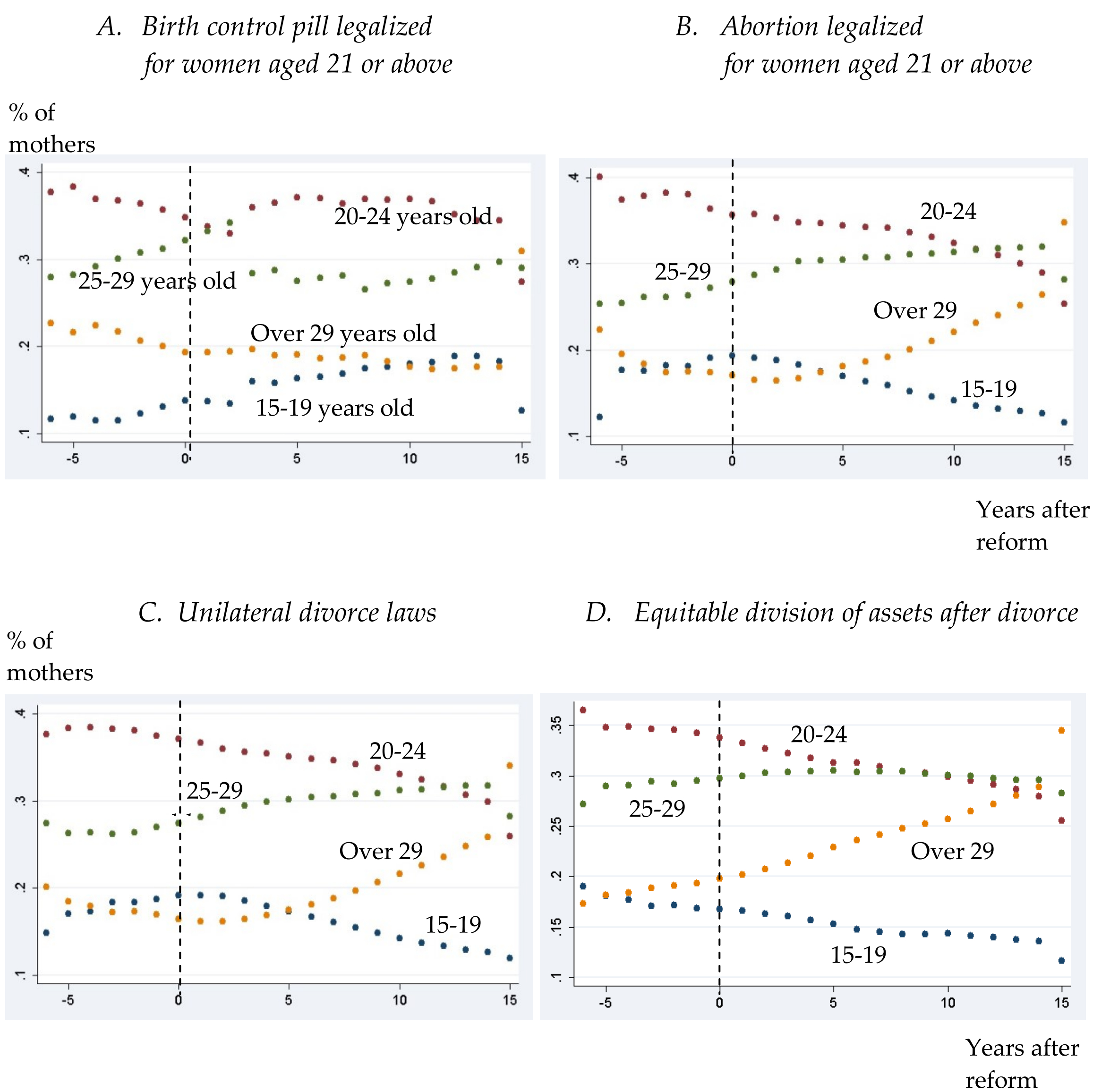
Notes: All estimates are produced using the intrinsic estimator and a log-linear model, where the number of teenage births relative to the population at risk is modeled as a Poisson variate.

C7 Birth Rates for 15-19 and 20-24 Year-Olds, 1980-2010



Notes: Birth rates are calculated using de-identified birth certificate available online at http://www.cdc.gov/nchs/nvss/birth_methods.htm.

C8 Trends in Maternal Age by Time of Social Policy Reform



Notes: Proportions of maternal age groups are calculated using de-identified birth certificate available online at http://www.cdc.gov/nchs/nvss/birth_methods.htm. States which did not pass unilateral divorce laws or equitable division of assets laws are excluded from calculations for their respective panels.

C9 Impact of Social Policies on Teenage Birth Rates by Race, 1980-1999 (Dependent Variable = Log of Births per 1,000 Women)

	White		Black	
	Model 1	Model 2	Model 1	Model 2
Born 1-2 years *before* pill was available to women aged 21+	0.054 (0.034)	-0.023 (0.033)	-0.046 (0.056)	-0.075 (0.056)
Born 0-1 years after	0.059 (0.068)	-0.029 (0.051)	-0.071 (0.080)	-0.097 (0.092)
Born 2-4 years after	0.085 (0.092)	-0.011 (0.070)	-0.101 (0.093)	-0.139 (0.121)
Born 5-7 years after	0.127 (0.106)	0.013 (0.086)	-0.106 (0.098)	-0.147 (0.138)
Born 8+ years after	0.159 (0.112)	0.028 (0.096)	-0.125 (0.106)	-0.177 (0.145)
Born 1-2 years *before* pill was available to women aged 18-20	0.010 (0.011)	0.012 (0.010)	-0.007 (0.015)	0.022 (0.022)
Born 0-1 years after	0.031 (0.022)	0.021 (0.019)	-0.021 (0.024)	0.018 (0.035)
Born 2-4 years after	0.045 (0.031)	0.020 (0.026)	-0.042 (0.035)	0.002 (0.050)
Born 5-7 years after	0.043 (0.039)	0.014 (0.032)	-0.047 (0.041)	-0.013 (0.056)
Born 8+ years after	0.045 (0.050)	0.021 (0.040)	-0.061 (0.046)	-0.013 (0.060)
Born 1-2 years *before* pill was available to women aged 15-17	0.011 (0.016)	0.027* (0.016)	0.016 (0.018)	-0.015 (0.033)
Born 0-1 years after	0.006 (0.029)	0.034 (0.020)	0.025 (0.028)	-0.033 (0.057)
Born 2-4 years after	0.008 (0.035)	0.054* (0.029)	0.040 (0.038)	-0.047 (0.090)
Born 5-7 years after	0.005 (0.047)	0.056 (0.038)	0.032 (0.050)	-0.059 (0.131)
Born 8+ years after	-0.012 (0.054)	0.029 (0.044)	0.040 (0.056)	-0.086 (0.175)
Born 1-2 years *before* abortion was available to women aged 21+	-0.050** (0.019)	-0.044*** (0.013)	-0.075*** (0.019)	-0.038* (0.020)
Born 0-1 years after	-0.076** (0.037)	-0.052* (0.028)	-0.124*** (0.037)	-0.099** (0.040)

Born 2-4 years after	-0.137**	-0.113***	-0.145***	-0.116*
	(0.054)	(0.040)	(0.045)	(0.059)
Born 5-7 years after	-0.153**	-0.093	-0.051	-0.014
	(0.061)	(0.057)	(0.049)	(0.081)
Born 8+ years after	-0.141**	-0.051	-0.017	0.055
	(0.065)	(0.074)	(0.074)	(0.101)
Born 1-2 years *before* abortion was available to women aged 18-20	0.022	0.039*	0.066***	0.028
	(0.022)	(0.022)	(0.019)	(0.025)
Born 0-1 years after	0.004	0.050	0.070*	-0.014
	(0.047)	(0.044)	(0.036)	(0.058)
Born 2-4 years after	0.001	0.077	0.076*	-0.050
	(0.069)	(0.059)	(0.040)	(0.083)
Born 5-7 years after	0.001	0.076	0.058	-0.084
	(0.078)	(0.066)	(0.056)	(0.104)
Born 8+ years after	-0.022	0.035	0.024	-0.137
	(0.084)	(0.068)	(0.073)	(0.138)
Born 1-2 years *before* abortion was available to women aged 15-17	-0.003	-0.020	0.001	0.048**
	(0.012)	(0.013)	(0.016)	(0.023)
Born 0-1 years after	-0.005	-0.038*	-0.006	0.084**
	(0.024)	(0.021)	(0.028)	(0.037)
Born 2-4 years after	-0.002	-0.041	0.001	0.138**
	(0.036)	(0.031)	(0.031)	(0.059)
Born 5-7 years after	0.005	-0.028	0.020	0.191**
	(0.043)	(0.045)	(0.053)	(0.087)
Born 8+ years after	0.015	0.006	0.069	0.266**
	(0.049)	(0.070)	(0.072)	(0.128)
Born 6-14 years *before* unilateral divorce laws passed	-0.056	-0.144	-0.089*	0.112***
	(0.098)	(0.115)	(0.051)	(0.025)
Born 3-5 years *before*	-0.071	-0.133	-0.063	0.184***
	(0.098)	(0.112)	(0.053)	(0.031)
Born 1-2 years *before*	-0.086	-0.122	-0.059	0.250***
	(0.102)	(0.111)	(0.062)	(0.052)
Born 0-1 years after	-0.108	-0.126	-0.037	0.336***
	(0.107)	(0.113)	(0.068)	(0.072)
Born 2-4 years after	-0.120	-0.122	-0.051	0.380***
	(0.112)	(0.116)	(0.075)	(0.087)
Born 5-7 years after	-0.103	-0.092	-0.059	0.418***
	(0.115)	(0.120)	(0.081)	(0.083)
Born 8+ years after	-0.093	-0.076	-0.075	0.470***
	(0.120)	(0.129)	(0.090)	(0.098)

Born 3-5 years *before* equitable division of property laws passed	0.001 (0.011)	0.001 (0.015)	-0.005 (0.017)	-0.003 (0.025)
Born 1-2 years *before*	0.004 (0.022)	0.020 (0.023)	0.010 (0.026)	0.013 (0.039)
Born 0-1 years after	-0.007 (0.030)	0.023 (0.033)	0.025 (0.033)	0.036 (0.047)
Born 2-4 years after	-0.022 (0.045)	0.012 (0.044)	0.036 (0.046)	0.052 (0.055)
Born 5-7 years after	-0.012 (0.058)	0.026 (0.054)	0.056 (0.052)	0.089 (0.053)
Born 8+ years after	0.009 (0.069)	0.058 (0.072)	0.085 (0.053)	0.182** (0.075)
Constant	2.484	2.592	3.883	3.670
Adjusted R^2	0.995	0.973	0.983	0.957
Root MSE	0.061	0.142	0.066	0.111
No. of observations	4,993	20,826	3,184	4,146

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: All specifications control for state of residence, year, cohort, age and state-specific year and age fixed effects. Model 2 specifications also control for state of birth as well as state of origin and state of destination (for individuals who moved between time of birth and during teenage ages) fixed effects. The number of observations is larger for Model 2 since the observation unit for Model 2 is specific at both the state of residence and the state of birth levels, rather than only at the state of residence level.

C10 Impact of Social Policies on Teenage Birth Rates by Race and Decade of Birth, 1980-1999 (Dependent Variable = Log of Births per 1,000 Women)

	White			Black		
	Model 1					
	Cohorts born in the 1960s	Cohorts born in the 1970s	Cohorts born in the 1980s	Cohorts born in the 1960s	Cohorts born in the 1970s	Cohorts born in the 1980s
Born 1-2 years *before* pill was available to women aged 21+	0.025 (0.032)	0.030* (0.018)	-	-0.073 (0.065)	0.104** (0.041)	-
Born 0-1 years after	0.018 (0.047)	0.024 (0.034)	-	-0.075 (0.095)	0.108** (0.052)	-
Born 2-4 years after	0.041 (0.052)	0.058 (0.065)	-	-0.093 (0.105)	0.063 (0.083)	-
Born 5-7 years after	0.045 (0.055)	0.069* (0.037)	-	-0.110 (0.115)	0.083 (0.137)	0.120*** (0.043)
Born 8+ years after	0.038 (0.057)	0.063 (0.041)	-0.118* (0.066)	-0.163 (0.132)	0.048 (0.140)	-
Born 1-2 years *before* pill was available to women aged 18-20	0.025 (0.017)	0.000 (0.013)	-	0.020 (0.038)	0.002 (0.022)	-
Born 0-1 years after	0.049** (0.024)	-0.004 (0.017)	-	-0.045 (0.055)	-0.016 (0.027)	-
Born 2-4 years after	0.013 (0.042)	0.003 (0.021)	-0.116** (0.057)	-0.062 (0.058)	-0.043 (0.031)	0.082 (0.074)
Born 5-7 years after	0.001 (0.052)	0.008 (0.024)	-	-0.018 (0.065)	-0.050 (0.032)	-
Born 8+ years after	-0.018 (0.076)	0.014 (0.027)	0.024 (0.055)	-0.023 (0.076)	-0.037 (0.041)	-0.007 (0.031)
Born 1-2 years *before* pill was available to women aged 15-17	-0.032** (0.016)	-0.001 (0.014)	-	0.015 (0.040)	0.060* (0.032)	-
Born 0-1 years after	-0.029 (0.028)	-0.006 (0.020)	-	0.062 (0.051)	0.071 (0.042)	-
Born 2-4 years after	-0.003 (0.049)	-0.013 (0.027)	-	0.093 (0.058)	0.070 (0.045)	-
Born 5-7 years after	-	-0.029 (0.035)	0.028 (0.032)	-	0.070 (0.049)	-0.037 (0.030)
Born 8+ years after	-1.042*** (0.104)	-0.032 (0.038)	0.024 (0.057)	-0.030 (0.079)	0.067 (0.068)	-0.071 (0.108)

Born 1-2 years *before* abortion was available to women aged 21+	0.004 (0.015)	-0.006 (0.017)	-	0.014 (0.023)	-0.028 (0.030)	-
Born 0-1 years after	0.018 (0.043)	0.000 (0.030)	-	0.052 (0.060)	-0.072** (0.029)	-
Born 2-4 years after	-	0.008 (0.039)	-	-	-0.091*** (0.031)	-
Born 5-7 years after	-	0.016 (0.045)	-	-	-0.063* (0.036)	-
Born 8+ years after	-	0.027 (0.045)	0.046 (0.041)	-	-0.106** (0.039)	-0.002 (0.035)
Born 1-2 years *before* abortion was available to women aged 18-20	0.011 (0.029)	0.016 (0.014)	-	-0.028 (0.037)	0.043 (0.028)	-
Born 0-1 years after	-	-0.019 (0.030)	-	-	0.059** (0.028)	-
Born 2-4 years after	-	-0.050 (0.039)	-	-	0.058** (0.026)	-
Born 5-7 years after	-	-0.038 (0.048)	-	-	0.050 (0.034)	-
Born 8+ years after	-	-0.017 (0.054)	-0.054 (0.039)	-	0.236*** (0.064)	-0.057 (0.043)
Born 1-2 years *before* abortion was available to women aged 15-17	0.017 (0.025)	-0.001 (0.011)	-	0.072* (0.038)	0.001 (0.018)	-
Born 0-1 years after	-	-0.011 (0.018)	0.251* (0.131)	-	-0.017 (0.027)	0.023 (0.091)
Born 2-4 years after	-	-0.016 (0.021)	0.329*** (0.122)	-	-0.025 (0.024)	0.002 (0.082)
Born 5-7 years after	-	-0.030 (0.025)	0.319** (0.133)	-	-0.029 (0.034)	-0.001 (0.085)
Born 8+ years after	-	-0.062** (0.030)	0.360*** (0.130)	-	-0.244*** (0.059)	0.005 (0.092)
Born 6-14 years *before* unilateral divorce laws passed	-0.044 (0.104)	0.021** (0.010)	-	-0.029 (0.083)	-	-
Born 3-5 years *before*	-0.049 (0.104)	0.084 (0.060)	-0.376*** (0.020)	-0.034 (0.083)	-0.563*** (0.068)	-
Born 1-2 years *before*	-0.047 (0.105)	0.078 (0.064)	-	-0.084 (0.084)	-0.554*** (0.061)	-
Born 0-1 years after	-0.022 (0.108)	0.058 (0.070)	-	-0.028 (0.107)	-0.531*** (0.064)	-
Born 2-4 years after	-	0.043 (0.072)	-0.440*** (0.136)	-	-0.542*** (0.069)	-

Born 5-7 years after	-	0.053 (0.074)	-0.416*** (0.124)	-	-0.559*** (0.065)	-0.077 (0.181)
Born 8+ years after	-	0.061 (0.075)	-0.400*** (0.124)	-	-0.603*** (0.059)	-0.086 (0.177)
Born 3-5 years *before* equitable division of property laws passed	0.025* (0.013)	-0.014 (0.012)	-0.032 (0.066)	0.012 (0.021)	-0.009 (0.010)	-0.061 (0.049)
Born 1-2 years *before*	0.060** (0.030)	-0.026 (0.022)	0.035 (0.062)	0.031 (0.028)	0.010 (0.020)	-0.014 (0.029)
Born 0-1 years after	0.005 (0.040)	-0.041 (0.030)	0.066 (0.088)	0.069 (0.073)	0.010 (0.028)	-0.010 (0.050)
Born 2-4 years after	-0.021 (0.046)	-0.046 (0.042)	0.081 (0.104)	0.062 (0.065)	-0.007 (0.034)	-0.001 (0.074)
Born 5-7 years after	0.011 (0.058)	-0.049 (0.053)	0.121 (0.115)	0.064 (0.069)	-0.002 (0.042)	-0.010 (0.091)
Born 8+ years after	0.010 (0.068)	-0.058 (0.058)	0.122 (0.120)	0.041 (0.087)	0.007 (0.046)	0.065 (0.100)
Constant	2.500	2.259	1.860	3.761	4.227	4.625
Adjusted R^2	0.997	0.996	0.995	0.987	0.987	0.911
Root MSE	0.046	0.050	0.064	0.051	0.054	0.061
No. of observations	1,750	2,497	746	1,092	1,591	501
			Model 2			
	Cohorts born in the 1960s	Cohorts born in the 1970s	Cohorts born in the 1980s	Cohorts born in the 1960s	Cohorts born in the 1970s	Cohorts born in the 1980s
Born 1-2 years *before* pill was available to women aged 21+	-0.003 (0.023)	-0.016 (0.038)	-	-0.076 (0.051)	0.101** (0.050)	-
Born 0-1 years after	0.009 (0.027)	-0.022 (0.040)	-	-0.055 (0.078)	0.122** (0.055)	-
Born 2-4 years after	0.048 (0.036)	-0.006 (0.064)	-	-0.059 (0.088)	0.109 (0.111)	-
Born 5-7 years after	0.065 (0.042)	0.006 (0.083)	-	-0.071 (0.095)	0.142 (0.187)	-
Born 8+ years after	0.073 (0.049)	-0.017 (0.080)	-0.078*** (0.029)	-0.127 (0.111)	0.116 (0.190)	-0.240*** (0.051)
Born 1-2 years *before* pill was available to women aged 18-20	0.013 (0.017)	-0.004 (0.011)	-	0.014 (0.035)	0.017 (0.023)	-
Born 0-1 years after	0.011 (0.029)	-0.009 (0.017)	-	-0.074 (0.052)	0.010 (0.033)	-
Born 2-4 years after	-0.043 (0.033)	-0.007 (0.025)	-	-0.121** (0.056)	-0.007 (0.040)	-0.065 (0.187)

Born 5-7 years after	-0.042 (0.036)	0.002 (0.029)	0.057 (0.060)	-0.087 (0.065)	-0.010 (0.044)	-
Born 8+ years after	-0.076 (0.053)	-0.002 (0.038)	0.102 (0.066)	-0.083 (0.084)	-0.015 (0.053)	0.052 (0.038)
Born 1-2 years *before* pill was available to women aged 15-17	-0.004 (0.018)	0.046** (0.021)	-	0.024 (0.037)	0.062 (0.039)	-
Born 0-1 years after	0.012 (0.027)	0.061** (0.027)	-	0.071 (0.051)	0.069 (0.051)	-
Born 2-4 years after	0.068* (0.039)	0.067** (0.032)	1.422*** (0.212)	0.095 (0.063)	0.065 (0.056)	2.252*** (0.240)
Born 5-7 years after	-	0.058* (0.034)	1.440*** (0.213)	-	0.068 (0.066)	2.271*** (0.245)
Born 8+ years after	-	0.064* (0.038)	1.440*** (0.211)	-	0.066 (0.086)	2.235*** (0.253)
Born 1-2 years *before* abortion was available to women aged 21+	0.008 (0.039)	0.002 (0.027)	-	0.042* (0.023)	-0.003 (0.043)	-
Born 0-1 years after	0.047 (0.096)	-0.025 (0.058)	-	0.091* (0.047)	-0.062 (0.061)	-
Born 2-4 years after	-	-0.066 (0.078)	-	-	-0.108 (0.074)	-
Born 5-7 years after	-	-0.060 (0.087)	-	-	-0.073 (0.079)	-0.093* (0.049)
Born 8+ years after	-	-0.011 (0.088)	0.014 (0.031)	-	-0.077 (0.078)	-
Born 1-2 years *before* abortion was available to women aged 18-20	0.102** (0.043)	-0.002 (0.017)	-	-0.057 (0.038)	0.035 (0.030)	-
Born 0-1 years after	-	0.017 (0.047)	-	-	0.058 (0.042)	-
Born 2-4 years after	-	0.016 (0.062)	-	-	0.062 (0.040)	-
Born 5-7 years after	-	0.011 (0.073)	-	-	0.048 (0.050)	0.091* (0.048)
Born 8+ years after	-	-0.057 (0.103)	-0.063** (0.025)	-	0.310*** (0.113)	-
Born 1-2 years *before* abortion was available to women aged 15-17	-0.081*** (0.027)	-0.006 (0.007)	-	0.103** (0.039)	0.014 (0.017)	-
Born 0-1 years after	-	-0.028** (0.012)	1.811*** (0.220)	-	0.009 (0.026)	1.680*** (0.244)
Born 2-4 years after	-	-0.034** (0.016)	1.885*** (0.214)	-	0.020 (0.030)	1.669*** (0.230)

Born 5-7 years after	-	-0.019 (0.024)	1.855*** (0.208)	-	0.037 (0.061)	1.634*** (0.219)
Born 8+ years after	-	0.039 (0.060)	1.870*** (0.208)	-	-0.240** (0.117)	1.651*** (0.222)
Born 6-14 years *before* unilateral divorce laws passed	-0.065 (0.086)	-0.025 (0.052)	-	-0.767*** (0.145)	-	-
Born 3-5 years *before*	-0.063 (0.085)	0.053 (0.064)	-0.369*** (0.014)	-0.758*** (0.138)	-2.559*** (0.098)	-
Born 1-2 years *before*	-0.048 (0.086)	0.058 (0.066)	-	-0.782*** (0.135)	-2.578*** (0.093)	-
Born 0-1 years after	0.032 (0.091)	0.029 (0.073)	-	-0.720*** (0.142)	-2.556*** (0.101)	-
Born 2-4 years after	-	0.024 (0.077)	-0.574** (0.231)	-	-2.558*** (0.108)	-
Born 5-7 years after	-	0.046 (0.080)	-0.500** (0.218)	-	-2.566*** (0.109)	1.930*** (0.426)
Born 8+ years after	-	0.056 (0.086)	-0.486** (0.222)	-	-2.573*** (0.113)	1.958*** (0.426)
Born 3-5 years *before* equitable division of property laws passed	0.029* (0.015)	0.007 (0.011)	-0.073** (0.030)	0.024 (0.019)	-0.007 (0.011)	-0.084 (0.070)
Born 1-2 years *before*	0.089*** (0.028)	0.025 (0.019)	-0.010 (0.042)	0.049 (0.042)	0.009 (0.017)	-0.067 (0.050)
Born 0-1 years after	0.034 (0.045)	0.020 (0.024)	0.029 (0.051)	0.113 (0.087)	0.009 (0.035)	-0.044 (0.076)
Born 2-4 years after	-0.005 (0.050)	0.029 (0.031)	0.028 (0.061)	0.125 (0.079)	0.009 (0.042)	-0.028 (0.088)
Born 5-7 years after	0.016 (0.054)	0.047 (0.037)	0.005 (0.084)	0.153* (0.087)	0.010 (0.050)	-0.099 (0.112)
Born 8+ years after	0.036 (0.060)	0.052 (0.041)	0.027 (0.098)	0.141 (0.104)	0.006 (0.064)	-0.044 (0.129)
Constant	3.375	1.766	4.194	4.752	7.059	6.694
Adjusted R^2	0.979	0.976	0.967	0.977	0.972	0.971
Root MSE	0.119	0.131	0.160	0.068	0.084	0.110
No. of observations	8,000	9,925	2,901	1,478	2,014	654

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: All specifications control for state of residence, year, cohort, age and state-specific year and age fixed effects. Model 2 specifications also control for state of birth as well as state of origin and state of destination (for individuals who moved between time of birth and during teenage ages) fixed effects. The number of observations is larger for Model 2 since the observation unit for Model 2 is specific at both the state of residence and the state of birth levels, rather than only at the state of residence level.

C11 Impact of Social Policies on Birth Rates at Ages 20-24, 1980-1999 (Dependent Variable = Log of Births per 1,000 Women)

	Model 1	Model 2
Born 1-2 years *before* pill was available to women aged 21+	-0.003	-0.014
	(0.008)	(0.011)
Born 0-1 years after	-0.006	-0.020
	(0.022)	(0.022)
Born 2-4 years after	0.009	-0.021
	(0.029)	(0.033)
Born 5-7 years after	0.023	-0.009
	(0.037)	(0.039)
Born 8+ years after	0.029	0.012
	(0.045)	(0.040)
Born 1-2 years *before* pill was available to women aged 18-20	0.002	0.001
	(0.006)	(0.007)
Born 0-1 years after	0.006	-0.001
	(0.012)	(0.011)
Born 2-4 years after	0.014	0.009
	(0.019)	(0.018)
Born 5-7 years after	0.017	0.016
	(0.024)	(0.023)
Born 8+ years after	0.016	0.012
	(0.026)	(0.028)
Born 1-2 years *before* pill was available to women aged 15-17	-0.003	0.008
	(0.007)	(0.010)
Born 0-1 years after	-0.008	0.014
	(0.011)	(0.015)
Born 2-4 years after	-0.013	0.024
	(0.016)	(0.022)
Born 5-7 years after	-0.010	0.030
	(0.020)	(0.027)
Born 8+ years after	-0.007	0.023
	(0.022)	(0.036)
Born 1-2 years *before* abortion was available to women aged 21+	-0.015	0.002
	(0.009)	(0.006)
Born 0-1 years after	-0.019	0.033**
	(0.015)	(0.015)
Born 2-4 years after	-0.023	0.031
	(0.014)	(0.032)
Born 5-7 years after	-0.017	0.100
	(0.020)	(0.062)
Born 8+ years after	-0.020	0.139*
	(0.024)	(0.083)

Born 1-2 years *before* abortion was available to women aged 18-20	0.014 (0.010)	0.019* (0.011)
Born 0-1 years after	0.001 (0.023)	0.005 (0.024)
Born 2-4 years after	-0.013 (0.028)	0.006 (0.037)
Born 5-7 years after	-0.026 (0.029)	0.001 (0.050)
Born 8+ years after	-0.051 (0.034)	0.065 (0.069)
Born 1-2 years *before* abortion was available to women aged 15-17	0.000 (0.007)	-0.009 (0.012)
Born 0-1 years after	0.000 (0.014)	-0.020 (0.018)
Born 2-4 years after	0.008 (0.018)	-0.014 (0.027)
Born 5-7 years after	0.016 (0.022)	0.023 (0.044)
Born 8+ years after	0.029 (0.023)	0.045 (0.056)
Born 6-14 years *before* unilateral divorce laws passed	-0.005 (0.017)	-0.063 (0.044)
Born 3-5 years *before*	0.001 (0.017)	-0.052 (0.045)
Born 1-2 years *before*	0.008 (0.018)	-0.054 (0.048)
Born 0-1 years after	0.008 (0.020)	-0.049 (0.049)
Born 2-4 years after	0.009 (0.022)	-0.045 (0.051)
Born 5-7 years after	0.007 (0.024)	-0.017 (0.059)
Born 8+ years after	-0.004 (0.025)	0.040 (0.071)
Born 3-5 years *before* equitable division of property laws passed	-0.003 (0.006)	0.022* (0.011)
Born 1-2 years *before*	-0.007 (0.009)	0.052** (0.022)
Born 0-1 years after	-0.026** (0.013)	0.059* (0.032)
Born 2-4 years after	-0.044** (0.019)	0.067* (0.036)
Born 5-7 years after	-0.054** (0.024)	0.098** (0.038)

Born 8+ years after	-0.054* (0.028)	0.122** (0.049)
Constant	4.788	4.850
Adjusted R^2	0.983	0.848
Root MSE	0.028	0.112
No. of observations	7,344	30,217

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Notes: All specifications control for state of residence, year, cohort, age and state-specific year and age fixed effects. Model 2 specifications also control for state of birth as well as state of origin and state of destination (for individuals who moved between time of birth and during teenage ages) fixed effects. Model 2 excludes data for 2000-2010 since IPUMS female population counts are distorted by group quarters accounting in the American Community Survey. Despite this, the number of observations is larger for Model 2 since the observation unit for Model 2 is specific at both the state of residence and the state of birth levels, rather than only at the state of residence level.

References

Aber, J. Lawrence, Jeanne Brooks-Gunn, and Rebecca A. Maynard. 2005. "Effects of Welfare Reform on Teenage Parents and Their Children." *Critical Issues For Children and Youths* 5(2): 53-71.

Abrahamse, Allan F., Peter A. Morrison, and Linda J. Waite 1988. "Beyond Stereotypes: Who Becomes a Single Teenage Mother?" Rand Report Series 3489.

Ahituv, Avner, V. Joseph Hotz, and Tomas Philipson. 1996. "The Responsiveness of the Demand for Condoms to the Local Prevalence of AIDS." *Journal of Human Resources* 31(4): 869-897.

Altman-Palm, Nancy, and Carol Horton Tremblay. 1998. "The Effects of Parental Involvement Laws and the AIDS Epidemic on the Pregnancy and Abortion Rates of Minors." *Social Science Quarterly* 79(4): 846–862.

Ananat, Elizabeth, Anna Gassman-Pines, and Christina M. Gibson-Davis. 2013. "Community-Wide Job Loss and Teenage Fertility: Evidence from North Carolina." *Demography* 50(6): 2151-2171.

Ananat, Elizabeth Oltmans, and Daniel M. Hungerman. 2012. "The Power of the Pill for the Next Generation: Oral Contraception's Effects on Fertility, Abortion, and Maternal and Child Characteristics." *Review of Economics and Statistics* 94(1): 37-51.

Ananat, Elizabeth Oltmans, et al. 2009. "Abortion and Selection." *Review of Economics and Statistics* 91(1): 124-136.

Angrist, Joseph D., and W. Evans. 1999. "Schooling and Labor Market Consequences of the 1970 State Abortion Reforms." *Research in Labor Economics* 18.

Angrist, Joshua D., and Alan B. Krueger. 1991. "Does Compulsory School Attendance Affect Schooling and Earnings?" *Quarterly Journal of Economics* 106(4): 979-1014.

Arkes, Jeremy, and Jacob Alex Klerman. 2009. "Understanding the Link between the Economy and Teenage Sexual Behavior and Fertility Outcomes." *Journal of Population Economics* 22(3): 517-536.

Ashcraft, Adam, Ivan Fernandez-Val, and Kevin Lang. 2013. "The Consequences of Teenage Childbearing: Consistent Estimates when Abortion Makes Miscarriage Non-Random." *Economic Journal* 123(571): 875-905.

Bailey, Martha J. 2013. "Fifty Years of Family Planning: New Evidence on the Long-Run Effects of Increasing Access to Contraception." NBER Working Paper 19493.

Bailey, Martha. 2006. "More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply." *Quarterly Journal of Economics* 121(1): 289-320.

Barber, Jennifer S. 2001. "The Intergenerational Transmission of Age at First Birth among Married and Unmarried Men and Women." *Social Science Research* 30: 219-247.

Bartz, Deborah, et al. 2007. "Pregnancy Intentions and Contraceptive Behaviors Among Adolescent Women: A Coital Event Level Analysis." *Journal of Adolescent Health* 41: 271-276.

Bedard, Kelly, and Elizabeth Dhuey. 2006. "The Persistence of Early Childhood Maturity: International Evidence of Long-Run Age Effects." *Quarterly Journal of Economics* 121(4): 1437-1472.

Bennett, Sylvana, and Nassim P. Assefi. 2005. "School-Based Teenage Pregnancy Prevention Programs: A Systematic Review of Randomized Controlled Trials." *Journal of Adolescent Health* 36: 72-81.

Black, Maureen M., and Katherine Nitz. 1996. "Grandmother Co-residence, Parenting, and Child Development Among Low Income, Urban Teen Mothers." *Journal of Adolescent Health* 18: 218-226.

Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes. 2011. "Too Young to Leave the Nest? The Effects of Schooling Starting Age." *Review of Economics and Statistics* 93(2): 455-467.

_______. 2008. "Staying in the Classroom and out of the Maternity Ward? The Effect of Compulsory Schooling Laws on Teenage Births." *Economic Journal* 118(530): 1025-1054.

Blank, Rebecca M. 2007. "What We Know, What We Don't Know, and What We Need to Know about Welfare Reform." National Poverty Center Working Paper Series No. 07-19.

Bongaarts, John, and Robert G. Potter. 1983. "Chapter 2: Natural Fertility and Its Proximate Determinants." In *Fertility, Biology and Behavior, 1st Edition*, 21-51. Academic Press.

Botting, Beverly, Michael Rosato, and Rebecca Wood. 1998. "Teenage Mothers and the Health of their Children." *Population Trends* 93(3): 19-28.

Brindis, Claire D. 2006. "A Public Health Success: Understanding Policy Changes Related to Teen Sexual Activity and Pregnancy." *Annual Review of Public Health* 27: 277-295.

Britner, Preston A., and N. Dickon Reppucci. 1997. "Prevention of Child Maltreatment: Evaluation of a Parent Education Program for Teen Mothers." *Journal of Child and Family Studies* 6(2): 165-175.

Brooks-Gunn, Jeanne, and Frank F. Furstenberg. 1986. "The Children of Adolescent Mothers: Physical, Academic, and Psychological Outcomes." *Developmental Review* 6: 224-251.

Buckles, Kasey S., and Daniel M. Hungerman. 2013. "Season of Birth and Later Outcomes: Old Questions, New Answers." *Review of Economics and Statistics* 95(3): 711-724.

Carceres-Delpiano, Julio, and Eugenio Giolito. 2012. "The Impact of Unilateral Divorce on Crime." *Journal of Labor Economics* 30(1): 215-248.

_______. 2008. "How Unilateral Divorce Affects Children." IZA Discussion Paper 3342.

Card, Josefina J. 1981. "Long-Term Consequences for Children of Teenage Parents." *Demography* 18(2): 137-156.

Centers for Disease Control and Prevention. 2011. "HIV Surveillance – United States, 1981-2008." Morbidity and Mortality Weekly Report 60(21): 689-693.

Charles, Kerwin Kofi, and Ming Ching Luoh. 2010. "Male Incarceration, the Marriage Market and Female Outcomes." *Review of Economics and Statistics* 92(3): 614-627.

Colen, Cynthia G., Arline T. Geronimus, and Maureen G. Phipps. 2006. "Getting a Piece of the Pie? The Economic Boom of the 1990s and Declining Teen Birth Rates in the United States." *Social Science and Medicine* 63: 1531-1545.

Coles, Mandy S., Kevin K. Makino, and Nancy L. Stanwood. 2011. "Contraceptive Experiences Among Adolescents Who Experience Unintended Birth." *Contraception* 84: 578-584.

Cook, Philip J., and Songman Kang. 2013. "Birthdays, Schooling and Crime: New Evidence on the Dropout-Crime Nexus." NBER Working Paper 18791.

Cook, Philip J. and John H. Laub. 2002. "After the Epidemic: Recent Trends in Youth Violence in the United States." *Crime and Justice* 29: 1-37.

Cook, Philip J., et al. 1999. "The Effects of Short-Term Variation in Abortion Funding on Pregnancy Outcomes." *Journal of Health Economics* 18(2): 241-257.

Corman, Amy Elizabeth. 2002. "How Teen Sexual Behavior Responds to Sexually Transmitted Disease Risk." Unpublished manuscript.

Crean, Hugh F., A. D. Hightower, and Marjorie J. Allan. 2001. "School-Based Child Care for Children of Teen Parents: Evaluation of an Urban Program Designed to Keep Young Mothers in School." *Evaluation and Program Planning* 24: 267-275.

Cygan-Rehm, Kamila, and Miriam Maeder. 2013. "The Effect of Education on Fertility: Evidence from a Compulsory School Reform." *Labour Economics* 25: 35-48.

Dobkin, Carlos, and Fernando Ferreira. 2010. "Do School Entry Laws Affect Educational Attainment and Labor Market Outcomes?" *Economics of Education Review* 29: 40-54.

Donohue, John J., Jeffrey Grogger, and Steven D. Levitt. 2009. "The Impact of Legalized Abortion on Teen Childbearing." *American Law and Economics Review* 11(1): 24-46.

Elder, Todd E., and Darren H. Lubotsky. 2008. "Kindergarten Entrance Age and Children's Achievement: Impacts of State Policies, Family Background and Peers." *Journal of Human Resources* 44(3): 641-683.

Elster, Arthur B., Elizabeth R. McAnarney, and Michael E. Lamb. 1983. "Parental Behavior of Adolescent Mothers." *Pediatrics* 71(4): 494-503.

Fan, Elliot, Jin-Tan Liu, and Yen-Chien Chen. 2014. "Is the 'Quarter of Birth' Endogenous? Evidence from One Million Siblings in Taiwan." NBER Working Paper 20444.

Flanagan, Patricia, et al. 1995. "Predicting Maltreatment of Children of Teenage Mothers." *Archives of Pediatrics and Adolescent Medicine* 149(4): 451-455.

Fletcher, Jason M., and Barbara L. Wolfe. 2009. "Education and Labor Market Consequences of Teenage Childbearing: Evidence Using the Timing of Pregnancy Outcomes and Community Fixed Effects." *Journal of Human Resources* 44(2): 303-325.

Francesconi, Marco. 2008. "Adult Outcomes for Children of Teenage Mothers." *Scandinavian Journal of Economics* 111(1): 93-117.

Furstenberg, Frank F. Jr. 1991. "As the Pendulum Swings: Teenage Childbearing and Social Concern." *Family Relations* 40(2): 127-138.

Furstenberg, Frank F. Jr., Judith A. Levine, and Jeanne Brooks-Gunn. 1990. "The Children of Teenage Mothers: Patterns of Early Childbearing in Two Generations." *Family Planning Perspectives* 22(2): 54-61.

Furstenberg, Frank F., Jeanne Brooks-Gunn, and Lindsay Chase-Lansdale. 1989. "Teenaged Pregnancy and Childbearing." *American Psychologist* 44(2): 313-320.

Geronimus, Arline T. 2003. "Damned if You Do: Culture, Identity, Privilege, and Teenage Childbearing in the United States." *Social Science and Medicine* 57: 881-893.

Geronimus, Arline T., and Sanders Korenman. 1992. "The Socioeconomic Consequences of Teen Childbearing Reconsidered." *Quarterly Journal of Economics* 107(4): 1187-1214.

Gibson-Davis, Christina. 2011. "Mothers But Not Wives: The Increasing Lag Between Nonmarital Births and Marriage." *Journal of Marriage and Family* 73: 264-278.

Gibson-Davis, Christina M, Kathryn Edin, and Sara McLanahan. 2005. "High Hopes But Even Higher Expectations: The Retreat From Marriage Among Low-Income Couples." *Journal of Marriage and Family* 67(5): 1301-1312.

Goldin, Claudia, and Lawrence F. Katz. 2002. "The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions." *Journal of Political Economy* 110(4): 730-770.

Graefe, Deborah Roempke, and Daniel T. Lichter. 2002. "Marriage Among Unwed Mothers: Whites, Blacks and Hispanics Compared." *Perspectives on Sexual and Reproductive Health* 34(6): 286-293.

Granger, Robert C., and Rachel Cytron. 1999. "Teenage Parent Programs: A Synthesis of the Long-Term Effects of the New Chance Demonstration, Ohio's Learning, Earning and Parenting, and the Teenage Parent Demonstration." *Evaluation Research* 23(2): 107-145.

Gruber, Jonathan. 2004. "Is Making Divorce Easier Bad for Children? The Long-Run Implications of Unilateral Divorce." *Journal of Labor Economics* 22(4): 799-833.

Gruber, Jonathan, Philip B. Levine, and Douglas Staiger. 1999. "Abortion Legalization and Child Living Circumstances: Who Is the 'Marginal Child'?" *Quarterly Journal of Economics* 114(1): 263–291.

Guttmacher Institute. 2013. "Facts on American Teens' Sexual and Reproductive Health." Accessed January 30, 2015. http://www.guttmacher.org/pubs/FB-ATSRH.pdf.

Hamilton, Brady E., Joyce A. Martin, and Stephanie J. Ventura. 2012. "Births: Preliminary Data for 2011." National Vital Statistics Reports 61(5): 1-19.

Hardy, Janet B., et al. 1998. "Like Mother, Like Child: Intergenerational Patterns of Age at First Birth and Associations with Childhood and Adolescent Characteristics and Adult Outcomes in the Second Generation." *Developmental Psychology* 34(6): 1220-1232.

Herrman, Judith W., and Julie K. Waterhouse. 2011. "What do Adolescents Think about Teen Parenting?" *Western Journal of Nursing Research* 33(4): 577-592.

Hofferth, Sandra L. 1987. "Chapter 8: The Children of Teen Childbearers." In *Risking the Future: Adolescent Sexuality, Pregnancy, and Childbearing, Volume II Working Papers*, edited by Cheryl D. Hayes, 174-206. Washington D.C.: National Academy Press.

Hofferth, Sandra L., and Lori Reid. 2002. "Early Childbearing and Children's Achievement and Behavior over Time." *Perspectives on Sexual and Reproductive Health* 34(1): 41-49.

Hoffman, Saul D. 2008. "Updated Estimates of the Consequences of Teen Childbearing for Mothers." In *Kids Having Kids: Economic Costs and Social Consequences of Teen Pregnancy (second edition)*, edited by Saul D. Hoffman, and Rebecca A. Maynard. Washington D.C.: Urban Institute Press.

_______. 1998. "Teenage Childbearing is Not So Bad After All…Or Is It? A Review of the New Literature." *Family Planning Perspectives* 30(5): 236-243.

Hogan, Dennis P., Rongjun Sun, and Gretchen T. Cornwell. 2000. "Sexual and Fertility Behaviors of American Females Aged 15-19 Years: 1985, 1990 and 1995." *American Journal of Public Health* 90(9): 1421-1425.

Holmund, Helena. 2005. "Estimating Long-Term Consequences of Teenage Childbearing: An Examination of the Siblings Approach." *Journal of Human Resources* 40(3): 716-743.

Horon, I.L.,D. M. Strobino, and H. M. MacDonald. 1983. "Birth Weights Among Infants Born to Adolescent and Young Adult Women." *American Journal of Obstetrics and Gynecology* 146(4): 444-449.

Hotz, V. Joseph, Susan Williams McElroy, and Seth G. Sanders. 2005. "Teenage Childbearing and Its Life Cycle Consequences: Exploiting a Natural Experiment." *Journal of Human Resources* 40(3): 683-715.

Isen, Adam, and Betsey Stevenson. 2010. "Women's Education and Family Behavior: Trends in Marriage, Divorce and Fertility." NBER Working Paper 15725.

Jaffee, Sara, et al. 2001 "Why are Children Born to Teen Mothers at Risk for Adverse Outcomes in Young Adulthood? Results from a 20-Year Longitudinal Study." *Development and Psychopathology* 13: 377-397.

Johnson, John H. IV and Christopher J. Mazingo. 2000. "The Economic Consequences of Unilateral Divorce for Children." University of Illinois CBA Office of Research Working Paper 112.

Johnson, Rucker C. and Steven Raphael. 2009. "The Effects of Male Incarceration Dynamics on Acquired Immune Deficiency Syndrome Infection Rates Among African American Women and Men." *Journal of Law and Economics* 52(2): 251-293.

Joyce, Theodore, Ruoding Tan, and Yuxiu Zhang. 2010. "Changes in Teen Fertility Following Access to the Pill and Abortion in the early 1970s." Unpublished manuscript.

Kaestner, Robert, Sanders Korenman, and June O'Neill. 2003. "Has Welfare Reform Changed Teenage Behaviors?" *Journal of Policy Analysis and Management* 22(2): 225-248.

Kahn, Joan R., and Kay E. Anderson. 1992. "Intergenerational Patterns of Teenage Fertility." *Demography* 29(1): 39-57.

Kalil, Ariel, and James Kunz. 2002. "Teenage Childbearing, Marital Status, and Depressive Symptoms in Later Life." *Child Development* 73(6): 1748-1760.

Kamdar, Amee. 2007. "Male Incarceration and Teen Fertility." Unpublished manuscript.

Kamdar, Amee. 2008. "Male Incarceration and Teen Fertility." PhD dissertation, University of Chicago. ProQuest Dissertation and Theses.

Kane, Jennifer B., et al. 2013. "The Educational Consequences of Teen Childbearing." *Demography* 50: 2129-2150.

Katz, Roger C., et al. 1995. "Knowledge and Attitudes about AIDS: A Comparison of Public High School Students, Incarcerated Delinquents, and Emotionally Disturbed Adolescents." *Journal of Youth and Adolescence* 24(1): 117-131.

Kearney, Melissa S. and Philip B. Levine. 2014. "Teen Births are Falling: What's Going On?" Brookings Economics Studies Policy Brief.

_______. 2013. "Investigating Recent Trends in the U.S. Teen Birth Rate." University of Maryland Department of Economic Research Paper.

_______. 2012a. "Why is the Teen Birth Rate in the United States so High and Why Does it Matter?" NBER Working Paper 17965.

_______. 2012b. "Explaining Recent Trends in the U.S. Teen Birth Rate." NBER Working Paper 17964.

_______. 2009. "Socioeconomic Disadvantage and Early Childbearing." In *The Problems of Disadvantaged Youth: An Economic Perspective*, edited by Jonathan Gruber. Chicago: University of Chicago Press.

Key, Janice D., et al. 2008. "Effectiveness of an Intensive, School-Based Intervention for Teen Mothers." *Journal of Adolescent Health* 42: 394-400.

Kindergarten Readiness Issue Group. 2003. "North Carolina Early Grade Retention in the Age of Accountability." Accessed August 30, 2014. http://prim.ncwiseowl.org/UserFiles/Servers/Server_4501234/File/retention_brief.pdf.

Kirby, Douglas, and Karin Coyle. 1997. "School-Based Programs to Reduce Sexual Risk-Taking Behavior." *Child and Youth Services Review* 19(5/6): 415-436.

Kissin, Dmitry M., et al. 2008. "Is There a Trend in Increased Unwanted Childbearing Among Young Women in the United States?" *Journal of Adolescent Health* 43: 364-371.

Koniak-Griffin, Deborah, et al. 2002. "Public Health Nursing Care for Adolescent Mothers: Impact on Infant Health and Selected Maternal Outcomes at 1 Year Postbirth." *Journal of Adolescent Health* 30: 44-54.

Levine, Philip B. 2000. "The Sexual Activity and Birth Control Use of American Teenagers." NBER Working Paper 7601.

Levine, Philip B., et al. 1999. "Roe v. Wade and American Fertility." *American Journal of Public Health* 90(2): 199–203.

Levine, David I., and Gary Painter. 2003. "The Schooling Costs of Out-of-Wedlock Childbearing: Analysis with a Within-School Propensity-Score-Matching Estimator." *Review of Economics and Statistics* 85(4): 884-900.

Lin, Wanchuan, and Juan Pantano. 2011. "The Unwanted: Negative Outcomes over the Life Cycle." Unpublished manuscript.

Loewenstein, George, and Frank Furstenberg. 1991. "Is Teenage Sexual Behavior Rational?" *Journal of Applied Social Psychology* 21(12): 957-986.

Ludwig, Jens, et al. 2013. "Long-Term Neighborhood Effects on Low-Income Families: Evidence from Moving to Opportunity." NBER Working Paper 18772.

Luo, Liying. 2013. "Assessing Validity and Application Scope of the Intrinsic Estimator Approach to the Age-Period-Cohort Problem." *Demography* 50: 1945-1967.

Manlove, Jennifer. 1998. "The Influence of High School Dropout and School Disengagement on the Risk of School-Age Pregnancy." *Journal of Research on Adolescence* 8(2): 187-220.

_______. 1997. "Early Motherhood in an Intergenerational Perspective: The Experiences of a British Cohort." *Journal of Family and Marriage* 59(2): 263-279.

Manlove, Jennifer, et al. 2009. " Trends in Sexual Experience, Contraceptive Use, and Teenage Childbearing: 1992-2002." *Journal of Adolescent Health* 44: 413-423.

Manlove, Jennifer, et al. 2000. "Explaining Demographic Trends in Teenage Fertility, 1980-1995." *Family Planning Perspectives* 32(4): 166-175.

Marcotte, Dave E. 2013. "High School Dropout and Teen Childbearing." *Economics of Education Review* 34: 258-268.

Mare, Robert D. 1991. "Five Decades of Educational Assortative Mating." *American Sociological Review* 56(1): 15-32.

Maynard, Rebecca A. 1997. "Chapter One: The Study, the Context and the Findings in Brief." In *Kids Having Kids: Economic Costs and Social Consequences of Teen Pregnancy*, edited by Saul D. Hoffman and Rebecca A. Maynard. Washington D.C.: Urban Institute Press.

McCrary, Justin, and Heather Royer. 2011. "The Effect of Female Education on Fertility and Infant Health: Evidence from School Entry Policies Using Exact Date of Birth." *American Economic Review* 101(1): 158-195.

McLanahan, Sarah. 2009. "Fragile Families and the Reproduction of Poverty." *Annuals of the American Academy of Political and Social Science* (261): 111-131.

Meade, Christina S., Trace S. Kershaw, and Jeanette S. Ickovics. 2008. "The Intergenerational Cycle of Teenage Motherhood: An Ecological Approach." *Health Psychology* 27(4): 419-429.

Mechoulan, Stephane. 2011. "The External Effects of Male Incarceration on Black Females." *Journal of Labor Economics* 29(1): 1-35.

Mollborn, Stefanie, and Elizabeth Morningstar. 2009. "Investigating the Relationship between Teenage Childbearing and Psychological Distress Using Longitudinal Evidence." *Journal of Health and Social Behavior* 50(3): 310-326.

Moore, Kristin A., et al. 1998. "Non-Marital School-Age Motherhood: Family, Individual, and School Characteristics." *Journal of Adolescent Research* 13: 433-456.

Moore, Kristin A., and Nancy O. Snyder. 1991. "Cognitive Attainment Among Firstborn Children of Adolescent Mothers." *American Sociological Review* 56(5): 612-624.

Morgan, S. Philip, and Ronald R. Rindfuss. 1999. "Reexamining the Link of Early Childbearing to Marriage and to Subsequent Fertility." *Demography* 36(1): 59-75.

Mosher, William D. 2010. "Use of Contraception in the United States: 1982-2010." Vital and Health Statistics 23(29): 1-44.

Mosher, William D., Jo Jones, and Joyce C. Abma. 2012. "Intended and Unintended Births in the United States." National Health Statistics Report 55: 1-27

Mosher, William D., and Jo Jones. 2010. "Use of Contraception in the United States: 1982-2008." National Center for Health Statistics *Vital and Health Statistics* 23(29): 1-44.

Mott, Frank L., and William Marsiglio. 1985. "Early Childbearing and Completion of High School" *Family Planning Perspectives* 17(5): 234-237.

Myers, Caitlin Knowles. 2012. "Power of the Pill or Power of Abortion? Re-examining the Effects of Young Women's Access to Reproductive Control." IZA Discussion Paper 6661.

North Carolina Education Research Data Center. ." Accessed March 23, 2013. http://www.childandfamilypolicy.duke.edu/project_detail.php?id=35.

Ozbeklik, Serkan. 2006. "The Effect of Abortion Legalization on Teenage Out-of-Wedlock Childbearing in Future Cohorts." University of Southern California Institute of Economic Policy Research Working Paper 06.50.

Pantano, Juan. 2007. "Unwanted Fertility, Contraceptive Technology and Crime: Exploiting a Natural Experiment in Access to the Pill." UCLA California Center for Population Research On-Line Working Paper Series.

Pungello, Elizabeth P., Frances A. Campbell, and W. Steven Barnett. 2006. "Poverty and Early Childhood Intervention." Center on Poverty, Work and Opportunity Policy Brief Series 1.

Reinhold, Steffen, Thorsten Kneip, and Gerrit Bauer. 2013. "The Long-Run Consequences of Unilateral Divorce Laws on Children – Evidence from SHARELIFE." *Journal of Population Economics* 26: 1035-1056.

Rivera, Ana Mylena Aguilar. 2013. "The Impact of Schooling on Teenage Fertility, Age at Marriage and Contraception Use: Evidence from Compulsory Education in Peru." Paper presented at the International Union for the Scientific Study of Population, Busan, South Korea, August 26-31.

Rothenberg, Pearila Brickner, and Phyllis E. Varga. 1981. "The Relationship between Age of Mother and Child Health and Development." *American Journal of Public Health* 71(8): 810-817.

Sanders, Seth G. 2011. "Crime and the Family: Lessons from Teenage Childbearing." In *Controlling Crime: Strategies and Tradeoffs*, edited by Cook, Philip J., Jens Ludwig, and Justin McCrary, 573-602. Chicago: University of Chicago Press.

Santelli, John S., et al. 2009. "Changing Behavioral Risk for Pregnancy Among High School Students in the United States, 1991-2007." *Journal of Adolescent Health* 45: 25-32.

Santelli, John S., et al. 2007. "Explaining Recent Declines in Adolescent Pregnancy in the United States: The Contribution of Abstinence and Improved Contraceptive Use." *American Journal of Public Health* 97(1): 150-156.

Saperstein, Aliya, and Andrew M. Penner. 2012. "Racial Fluidity and Inequality in the United States." *American Journal of Sociology* 118(3): 676-727.

Schultz, Katherine. 2008. "Constructing Failure, Narrating Success." In *Adolescent Identities, A Collection of Readings*, edited by Deborah L. Browing, 113-137. New York: Analytic Press.

Schweinhart, Lawrence J., et al. 1985. "Effects of the Perry Preschool Program on Youths Through Age 19: A Summary." *Topics in Early Childhood Special Education* 5: 26.

Shaw, Mary, Debbie A. Lawlor, and Jake M. Najman. 2006. "Teenage Children of Teenage Mothers: Psychological, Behavioural and Health Outcomes from an Australian Prospective Longitudinal Study." *Social Science and Medicine* 62: 2526-2539.

Shearer, Darlene L., et al. 2002. "Association of Early Childbearing and Low Cognitive Ability." *Perspectives on Sexual and Reproductive Health* 34(5): 236-243.

Silles, Mary A. 2011. "The Effect of Schooling on Teenage Childbearing: Evidence Using Changes in Compulsory Education Laws." *Journal of Population Economics* 24(2): 761-777.

Sloboda, Deborah M., et al. 2007. "Age at Menarche: Influences of Prenatal and Postnatal Growth." *Journal of Clinical Endocrinology & Metabolism* 92(1): 46-50.

Smith Battle, Lee. 2000. "The Vulnerabilities of Teenage Mothers: Challenging Prevailing Assumptions." *Advances in Nursing Science* 23(1): 29-40.

_______. 1995. "Teenage Mother's Narrative of Self: An Examination of Risking the Future." *Advances in Nursing Science* 17(4): 22-36.

Spear, Hila J. 2004. "Personal Narratives of Adolescent Mothers-to-Be: Contraception, Decision Making and Future Expectations." *Public Health Nursing* 21(4): 338-346.

_______. 2001. "Teenage Pregnancy: 'Having a Baby Won't Affect Me that Much.'" *Pediatric Nursing* 27(6): 574-580.

Staiger, Douglas, and James H. Stock. 1997. "Instrumental Variable Regression with Weak Instruments." *Econometrica* 65(3): 557-586.

Steinberg, Laurence. 2013. "How Adolescents Make Decisions." Presentation at Sulzberger Distinguished Lecture, Duke University, NC, September 25.

Stephens, Melvin Jr., and Dou-Yan Yang. 2014. "Compulsory Education and the Benefits of Schooling." *American Economic Review* 104(6): 1777-1792.

Stevenson, Betsey, and Justin Wolfers. 2007. "Marriage and Divorce: Changes and their Driving Forces." NBER Working Paper 12944.

Stier, Daniel M., et al. 1993. "Are Children Born to Teenage Mothers at Increased Risk of Maltreatment?" *Pediatrics* 91(3): 642-648.

Stock, James H., and Motohiro Yogo. 2002. "Testing for Weak Instruments in Linear IV Regressions." NBER Working Paper 284.

Suvivuo, Pia, Kerttu Tossavainer, and Osmo Kontula. 2009. "Contraceptive Use and Non-Use Among Teenage Girls in a Sexually Motivated Situation." *Sex Education* 9(4): 355-369.

Trenholm, Christopher, et al. 2007. "Impacts of Four Title V, Section 510 Abstinence Education Programs." Mathematica Policy Research Final Report 8549-110.

Unger, Donald G., and Marcia Cooley. 1992. "Partner and Grandmother Contact in Black and White Teen Parent Families." *Journal of Adolescent Health* 13: 546-552.

Unger, Jennifer B., Gregory B. Molina, and Lorena Teran. 2000. "Perceived Consequences of Teenage Childbearing Among Adolescent Girls in an Urban Sample." *Journal of Adolescent Health* 26: 205-212.

University of Kentucky Center for Poverty Research. "U.S. State Level Economic and Program Transfer Data." Accessed March 19, 2013. http://www.ukcpr.org/EconomicData/UKCPR_National_Data_Set_12_16_10_Public(1).xlsx.

Upchurch, Dawn M., and James MacCarthy. 1990. "The Timing of a First Birth and High School Completion." *American Sociological Review* 55(2): 224-234.

Ventura, Stephanie J., Sally C. Curtin, and Joyce C. Abma. 2012. "Estimated Pregnancy Rates and Rates of Pregnancy Outcomes for the United States." National Vital Statistics Reports 60(7): 1-21.

Ventura, Stephanie J., T. J. Matthews, and Brady E. Hamilton. 2002. “Teenage Births in the United States, 1990-2000: An Update.” National Vital Statistics Reports 50(9).

Weinstein, Maxine, et al. 1990. “Components of Age-Specific Fecundability.” *Population Studies* 44(3): 447-467.

Wolfers, Justin. 2006. “Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results.” *American Economic Review* 96(5): 1802-1820.

Yang, Yang, et al. 2008. “The Intrinsic Estimator: What It Is and How to Use It." *American Journal of Sociology* 113(6): 1697-1736.

Biography

Poh Lin Tan (born "Tan Poh Lin" or 陈宝铃, pronounced "Chen Baoling") was born in Singapore in 1985. She received a Bachelor of Arts in Economics from Princeton University in 2008. In 2010, she joined the doctoral program at the Sanford School of Public Policy, Duke University, and received an M.A. in Public Policy in 2013.

She received the Horowitz Foundation for Social Policy award in 2014 (16 out of over 360 applicants were selected), and was a Pre-doctoral Fellow at the Duke Population Research Institute between 2012 and 2014 (awarded through nomination). She is a two-time recipient of the Duke University Summer Research Fellowship and four-time recipient of the Sanford School of Public Policy Small Grants.

During her doctoral studies at Duke, Poh Lin Tan worked as a guest researcher at the Laboratory for Economic and Social Demography at the Max Planck Institute for Demographic Research, and joined the Department of Sociology at Oxford University as a visiting student in 2014-2015 under the supervision of Professor Francesco Billari.

She has presented her work at national and international conferences, including the Population Association of America (Tan et al. 2012; Tan and Morgan 2013; Tan and Cook 2013; Tan 2014; Tan and Cook forthcoming) and the International Union for the Scientific Study of Population meetings (Tan, Morgan and Zagheni 2013).

Her dissertation, titled "Determinants of Teenage Childbearing in the United States", was supervised by Professor Philip J. Cook.