Consequences of Childbearing in Delinquency and Substance Use

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Abstract

This paper uses the National Longitudinal Study of Adolescent to Adult Health (Add Health) and recent empirical strategies to examine the dynamic consequences of parenthood on delinquency and substance use. We take advantage of the miscarriage information available to form comparison groups. Our contribution is to extend the analysis of the effects of childbearing to deviant outcomes such as delinquency and substance use, and explore the differences between motherhood and fatherhood. Our preliminary results suggest reductions in risky behaviors from childbearing. We do not find evidence of heterogeneity across socioeconomic status.
1 Introduction

There is long-standing interest in understanding the consequences of *teenage childbearing* on a host of outcomes including, educational attainment, earnings, and health behaviors. Careful efforts to identify unbiased estimates in this literature show modest benefits of delaying childbirths, although there are also studies that report negative consequences of postponing parenthood (Hotz et al. 1995, 2005; Ashcraft and Lang 2006; Fletcher 2012; Fletcher and Wolfe 2012). Criminological studies, in contrast, have paid more attention to the role of adult transitions in the process of desistance from crime, in an attempt to explain age-linked changes in offending (Siennick and Os-good 2008). These studies, hence, look beyond *teenage childbearing*, and examines if parenthood at later ages (the 20s or 30s) can reduce delinquency. The relevance of estimating the consequences of role transitions on offending over the life-course not only relates to the individual benefits associated with parents moving away from crime, but also to the effects that offending may have on the next generation (Giordano 2010).

Different theoretical perspectives predict a *parenthood effect* on crime, either because new parents become attached and invested in their children (social control), or because parental obligations bring reductions in the time spent in unstructured socializing (routine activities) or with deviant peers (social learning). However, previous research has shown contradictory and mixed results. In part, these mixed findings likely stem from the challenge of creating a comparison group for those who have a child.

Another way to interpret differences across studies is effect heterogeneity. Based on qualitative research, it is argued that childbearing would reduce crime and drug use, but mainly in disadvantaged communities (Diaz and Fiel 2016; Edin and Kefalas 2005; Giordano et al. 2011; Kreager et al. 2010). As qualitative evidence shows, parenthood seems to be a *salient* life-course turning point for poor women. This hypothesis does not suggest that middle-class women would increase delinquency following childbirth, but that their effects would be lower or null. Heterogeneity in the opportunity costs of disadvantaged women, thus, would explain the close link between motherhood and criminal desistance reported by ethnographic research in poor communities (Edin and
To assume effect heterogeneity in social sciences is sensible. However, one of the key issues in previous studies is the definition of an appropriate comparison group able to approximate the counterfactual condition (Kreager et al. 2010). Although most of the recent studies use different strategies to overcome this limitation, identification problems still linger (e.g., time-variant unobserved confounders, selection bias). Unfortunately, to estimate effect heterogeneity when selection bias and unobserved confounding is a plausible threat may be misleading (Breen et al. 2015).

To overcome this issue, this article estimates the effect of live birth from first pregnancies on delinquency and substance use using the identification strategy proposed by Hotz et al. (1995, 2005), and extended by Ashcraft and Lang (2006).

We use a sample of pregnant women and males with pregnant partners to compare delinquency and substance use between those who either gave birth or miscarried, under the assumption that miscarriages happened quasi-randomly. We also examine the dynamic effects of having a child and by socioeconomic status in order to assess the heterogeneity hypothesis. To our knowledge, no previous studies have explored the link between parenthood and delinquency among females and males using miscarriages as a natural experiment. By applying advances found in the teenage motherhood literature, we extend the area of life-course research by focusing on the relatively understudied topic of parenthood and crime.

2 Background

The discussion about the consequences of parenthood on offending can be framed into the literature on desistance from crime, and how adulthood transitions (e.g., work, marriage, parenthood) affect deviant behavior. From this literature, we can identify three main arguments to explain how adult transitions decrease offending. The first one suggests that adult roles are simply incompatible with deviance and offending. Sampson and Wilson (1995), for instance, argue that desistance occurs

\footnote{Desistance can be defined as a social process by which criminal behaviors decline with time, or as a transition from a state of offending to non-offending (Mulvey et al. 2004).}
through a gradual development of *stakes in conformity* that increase the potential costs of criminal behavior. According to their age-graded theory of social control, the effect of changing roles will depend on the level of attachment to conventional society associated with a transition.

The second argument states that adulthood transitions restructure individuals’ lives in ways they reduce opportunities to commit crime. Osgood et al. (1996), for example, explore changes in activity patterns associated with role transitions and their consequences for offending. They argue that time spent in unstructured socializing is positively related to crime and deviance. Therefore, any role transition that reduces unstructured socializing should reduce offending. Warr (2002) provides a more precise interpretation of this process, suggesting the reduction in crime will occur in those with deviant peers. Thus, roles like marriage or parenthood will reduce time spent with deviant friends, decreasing deviant peer influence.

While the first two approaches focus on social and external factors, a third argument goes beyond role transitions and suggests that decreases in offending depend on *identity shifts*. According to Giordano et al. (2002), offenders, before desisting, experience a cognitive shift toward openness to change their behavior. Under this idea, transitions into adults roles serve as *hooks of change* that give offenders opportunities and resources to desist from crime. However, the consequences of these transitions would be contingent on changes in self-identity.

Without accounting for selection, it is difficult to distinguish empirically between subjective (individual) and objective (social) changes as the cause of desistance. Recent theoretical approaches, however, have tended to integrate both arguments. On the one hand, life events are thought to contribute to the desistance process, but the impact depends on the mindset of individuals. On the other hand, motivation would not be enough to move offenders away from crime, it also requires support from conventional social networks to maintain desistance efforts (Kazemian and Maruna 2009).

Although marriage and employment have been the cornerstones of explanations of the desistance process (Laub and Sampson 2003), becoming a parent may have higher potential to be a life altering transition event involving changes in routine activities, identity, and life. Parenthood,
however, has received much less research attention than has marriage as a potential explanation for reductions in crime in young adulthood, and despite compelling theoretical reasons to expect significant effects of parenthood on offending, empirical research has been somewhat equivocal and even contradictory (Siennick and Osgood 2008).

Some quantitative studies have found that the transition to parenthood is associated with reductions in crime (Barry 2010; Benda 2005; Cobbina 2009; Michalsen 2011, 2013; Monsbakken et al. 2013; Rodermond et al. 2015; Zoutewelle-Terovan et al. 2014; Theobald et al. 2015; Uggen and Kruttschnitt 1998; VanEseltine 2012; Monsbakken et al. 2013), for instance, use Norwegian register data on men and women who became parents and a within-individual design to show that parenthood reduces (registered) crimes preceding the first childbirth, but observe increases afterward. Kreager et al. (2010), using data from the Denver Youth Survey, examine within-individual changes in young adult women’s criminal behavior in a sample of disadvantaged respondents. The authors find a significant effect of childbearing on subsequent reductions in self-reported offending, and some evidence of decay in the teenage motherhood benefits. The decrease of the effect does not occur until about three years after childbirth, and longtime mothers continue to show lower offending than women without children. Theobald et al. (2015), using data from the Cambridge Study in Delinquent Development (411 males followed since childhood) and propensity score matching, report that while there are reductions in offending (number of convictions) comparing 5-10 years before childbearing to 5-10 years after the child’s birth, the effects are not large. Finally, using a sample of 540 high-risk men and women in the Netherlands, Zoutewelle-Terovan et al. (2014) find that having a first child reduces male offending more strongly than marriage, but female offending is not significantly influenced by marital status or motherhood. These studies that use within-individual changes have the disadvantage of being unable to control for time-varying individual characteristics, beliefs, and behaviors. Thus, the researchers are unable to rule out the possibility that the post-childbirth changes in delinquency would have occurred even without a childbirth due to pre-childbirth changes in behaviors.²

²A number of studies, on the other hand, show no evidence for the association between childbirth and crime. Giodano et al. (2002), for instance, find no additional effect of motherhood when controlling for marriage: neither having
Theoretical explanations of mixed results of previous research, although not in contradiction with the methodological factors discussed above, highlight effect heterogeneity or, more specifically, the conditions under which a *prosocial* impact of parenthood should be observed. Kreager et al. (2010), for instance, based on qualitative evidence, argue that motherhood has a transforming effect only in disadvantaged neighborhoods, and no effect in affluent neighborhoods. Qualitative research has found that motherhood is one of the most relevant available social roles for poor women, who often cannot count on intimate relationships or success in the realms of education and the labor force as positive sources of adult identity (Edin and Kefalas 2005). That is why we observe an illusive effect in broad populations that average across all neighborhoods. Although Kreager et al. (2010) do not test this hypothesis directly, they find a significant negative effect of motherhood on delinquency and substance use.

The disadvantage-heterogeneity argument, however, can go also the other way around. According to McMahon (1995), female respondents across the range of SES levels see motherhood as generally rewarding, but lower SES women are more likely to describe the transition in terms of new obligations and as part of the process of settling down. The idea that motherhood may represent a more distinct all-encompassing role for middle-class women has also been reported by research on parenting practices (Lareau 2002). Thus, greater access to resources of those with advantaged backgrounds (from better housing and income to social and tangible support from families of origin) might provide the necessary means for comfort and success in adapting to a new parent role. Giordano et al. (2011), for instance, show that young adults from highly disadvantaged families were less likely than their more advantaged peers to experience subsequent declines in criminal involvement after becoming parents. This might represent a long reach of socioeco-

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3 Moloney et al. (2009) find the same evidence for males. Some researchers have questioned the veracity of retrospective accounts of behavioral motivations because they can be only posthoc embellishments that justify decision to give a birth and keep the child. In other words, those reports may be affected by the treatment.

4 This is also reported by Cobbina (2009) and Michalsen (2011, 2013).
nomic status, not only as a direct influence on crime patterns, but also compromising the ability of major transition events to function smoothly as turning points in the desistance process.

While most of the literature on the impacts of parenthood focuses on mothers, there is emerging work that examines the impacts of fatherhood. Thus, gender is another potentially important moderator of the parenthood-crime relationship discussed in the literature. Usually, the role of being a parent is experienced differently by men and women, and the day to day tasks associated with childcare tend to be divided unequally, even when it is not the couple’s intention (Monsbakken et al. 2013). The period of pregnancy, for instance, seems to be experienced differently by mothers and parents. An obvious reason is the physical experience of gestation and breastfeeding. However, as family and gender scholars have noted, over the long run, the idea of distinct identify portfolios for adult women and men overemphasizes the centrality of the childbearing experience for females, and underemphasizes the fatherhood role (Casper and Bianchi 2009). Empirical evidence shows mixed results. While several studies find that the effect of having children is larger for women than for men (Benda 2005; Giordano et al. 2011; Uggen and Kruttschnitt 1998), others indicate that only males experience stabilizing crime levels after childbirth and females crimes levels increases afterward (Monsbakken et al. 2013), or that parenthood reduced serious offending only for men (Zoutewelle-Terovan et al. 2014). This smaller literature also faces the same empirical limitations described above, leading to a lack of causal estimates.

3 The current study

This study extends previous research on the relationship between parenthood, crime and substance use, by utilizing miscarriage as a strategy to partition random variation in childbirth from systematic variation due to unobservable factors. This strategy was pioneered by Hotz et al. (1995, 2005).

5 Other conditions of the parenthood-crime relationship discussed in the literature are wantedness and relationship status with the other parent. Wantedness would be an indicator of cognitive transformation or readiness to change (Giordano et al. 2002). Pregnancy intentions usually are assessed using retrospective questions about mothers’ feelings at the time they became pregnant and some researchers have questioned their veracity. Concerning the relationship status, it may reflect selection mechanisms. Having a child within marriage, cohabiting union, or not living with the other parent are to some extent associated with different stages of the life-course.
and has been used to measure the impact of teen parenthood on a host of outcomes, from educational attainment, earnings, and welfare dependence, to wellbeing. Following [Fletcher and Wolfe (2012)], we not only focus on motherhood, but we extend our analysis to young men whose partners experienced a miscarriage or gave birth. Taking advantage of the longitudinal nature of our data, we explore when any changes in offending are initiated and how long they last. Few studies have examined the duration of parenthood effects. All of them use a within-individual change research design ([Kreager et al. 2010; Monsbakken et al. 2013]). Finally, we assess directly the heterogeneity hypothesis examining differences in parenthood benefits across social disadvantage.

3.1 Data

We use data from the restricted version of the the National Longitudinal Study of Adolescent Health (Add Health). Add Health is a school-based longitudinal study of health-related behaviors of adolescents and their outcomes in young adulthood. The sample of schools was stratified by region, urbanicity, school type, size and ethnic mix. The survey was conducted in multiple waves. Wave I consists of an in-school questionnaire administered to over ninety thousand students and an in-home questionnaire administered to a subsample of about twenty thousand students and their parents. The in-home cohort was followed up approximately on year (Wave II), six years (Wave III), and thirteen year later (Wave IV). The respondents were 24-32 years old in the latest data available. About twelve thousand of the wave I in-home students comprise the main sample that represents a nationally representative sample of adolescents in grades 7 through 12 in the United States in the 1994-1995 school year.

In our analysis, we use data only from those respondents who reported a pregnancy by Wave III and IV. We unified pregnancy records from both waves using the procedures described in the Data Appendix. Following previous research ([Ashcraft and Lang 2006; Fletcher 2012; Fletcher and Wolfe 2012]), we limit our sample by focusing on first pregnancies that ended in either a live birth or a miscarriage, resulting in 4788 females and 3390 males. Thus, we are estimating the effect of childbearing on a select group, not a random young adult, and within this select group on
those who would not choose to have an abortion.

As previous studies have acknowledged (Ashcraft and Lang 2006; Fletcher and Wolfe 2009), the abortion-miscarriage distinction is not always clean, even with perfect data: some of those who abort would have had a miscarriage had they not aborted, and some of those who miscarry would have had an abortion had they not had a miscarriage. Those who miscarry, thus, may be an imperfect counterfactual. While the instrumental variable solution proposed by Hotz et al. (2005) would be biased in the direction of beneficial effects (when abortion is an option, teenagers who miscarry are less likely to be girls who would otherwise abort), the OLS solution proposed by Ashcraft and Lang (2006) would be biased towards finding adverse effects of childbearing (women who would otherwise have had an abortion, miscarry before the abortion can take place).

Finally, there are reasons to expect differences in pregnancy reports by gender. Males may not be aware of all pregnancies of their partner, and tend to be less accurate in reporting birth histories (Nock 1998; Lerman 2009). Fletcher and Wolfe (2009), however, show evidence suggesting that the fertility information reported by young men in the Add Health data, although no without measurement errors, seems to have no systematic problems.

3.2 Measures

Our outcome variables are self-reported delinquency and substance use. We averaged for each wave seven comparable items of delinquency: (1) deliberately damaging property, (2) stealing something worth more than $50, (3) going into a house or building to steal something, (4) using or threatening to use a weapon to get something from someone, (5) selling marijuana or other drugs,

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6 A matter of concern discussed in the literature is some of the miscarriages may in fact be misreported elective abortions (e.g., medical abortions). This could potentially bias results if misreporting is related to characteristics that might predict young adult delinquency and substance use. In the Add Health study, respondents used computer-assisted personal interview technology for sensitive questions. This should decrease misreporting of abortion because of the reduced effect of stigma. Additionally, according to Fletcher (2012), the self-report pregnancy outcomes in the Add Health data match more closely official Vital Statistics. There are still some differences with official reports, what suggests some remaining bias may be present.

7 It is possible to exploit the timing of miscarriages by focusing on late miscarriages (after eight, ten, or fourteen weeks of pregnancy), under the idea that most elective abortion procedures are usually conducted before the tenth week of gestation. The duration of pregnancy, however, is only available at Wave III and for women in the Add Health data. We are currently conducting robustness checks using late miscarriages (not shown).
(6) stealing something worth less than $50, and (7) taking part in a fight where a group of your friend was against another group. The Cronbach’s alpha of the delinquency scale was 0.74 at Wave I, 0.73 at Wave II, 0.66 at Wave III, and 0.58 at Wave IV. Because delinquency tends to decrease with age, we standardized the delinquency scale by age at the time of the interview (more details about this in the next section). Regarding substance use, we included two binary variables: marijuana use during the last month, and binge drinking during the last year (if a respondent drank 5 or more drinks in a row).

A key focus in exploring the timing of the effect of parenthood on our outcome measures is the age at which females and males’ partners started their first pregnancy. Because we do not have the duration of gestation for all males’ partners and females at Wave IV, we imputed the age when their first pregnancy began by subtracting 20 weeks if the result was a miscarriage, and 40 weeks if it was a live birth. The treatment variable indicates if the results of pregnancy was a live birth (1) or a miscarriage (0).

In order to assess the heterogeneity hypothesis, we use three socioeconomic variables measures at Wave I: family income (reported by the parents), highest educational level of parents (reported by young respondents), and a disadvantage index based on census tract information. The disadvantage index corresponds to a factor score combining rates of unemployment, poverty, female-head households, and people with less than high school. The Cronbach’s alpha of that score is 0.81. Tables 1 and 2 show descriptive statistics of the variable used in the models by gender.

### 3.3 Analytic strategy

Under the assumption that miscarriages are quasi-random, we examine the trajectories of delinquency-substance use for the years following the first pregnancy, and compare the miscarriage group to the

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8 We averaged the items only when at least 50% of them were non-missing.
9 The Cronbach’s alphas were calculated using all the Add Health sample available by wave.
10 We subtracted 40-20 weeks to the date of the end of the first pregnancy. Then, we computed the age at which the first pregnancy ended and truncated that value. Thus, we are estimating the effects of both pregnancy and childbearing.
11 This version of the paper does not include models with additional controls under the assumption that miscarriages occur quasi-randomly. Future versions will include robustness check with more controls.
live-birth group. For this, we set our analytic data following a long-person format so that each
data-row contains one record per outcome and wave for each person (everyone has a maximum of
four observations). We define a variable time, that is centered at the age of first pregnancy for all
individuals. Time represents the relative age (vs. time zero) that the Wave I - Wave IV outcomes
take place for each person (i.e., interview’s date). For example, a person who’s first pregnancy was
at age 20: time zero represents age 20; Wave I might reflect time = -5 (if she was 15 years old at
Wave I); Wave II would be time = -4; Wave III might be time = 1 ; and Wave IV might be time = 7.
Using this setup we estimate:

\[ Y_{ij} = \gamma_0 + \gamma_{10} \text{birth}_{ij} + \gamma_{20} \text{time}_{ij} + \gamma_{30} \text{birth}_{ij} \times \text{time}_{ij} + \gamma_{c0} \text{controls}_{ij} + U_{0j} + R_{ij} \]

Where \( R_{ij} \) is a residual term, and \( U_{0j} \) is a random-intercept term. Thus, we account for the
clustering associated with respondents’ repeated observations. In our final specification time is
included as a set of dummy variables with time = 0 as the reference category.

In the models presented below, we only controlled by the age at which the pregnancy started
and its squared term.\(^{12}\) Although this model allows us to take advantage of the longitudinal nature
of the Add Health data, there is missing outcome data at time periods that are not covered by Wave
I to Wave IV. In other words, because we do not have annual panel data for the periods before
and after the first pregnancy, we are using different set of individuals to estimate the effect of
parenthood on delinquency by time.

Figure 1 shows the interview’s age distribution by time. After time 0 (start of pregnancy), the
interview’s age distribution increases and shrinks. Because we are using outcomes measured at
different ages and delinquency tends to decline with age, we standardized the delinquency scale by
interview’s age using our analytic sample.\(^{13}\)

Finally, the internal validity of our estimates is based on the assumption that miscarriages is

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\(^{12}\) We decided to use a quadratic specification to control for the age at which the first pregnancy started instead of a
set of dummies, because the AIC and BIC indexes were considerably lower with the quadratic specification. The models
examining differences by social disadvantage include an interaction between time, birth, and social disadvantage.

\(^{13}\) In order to avoid the estimation of effects using cells with small sample size, we also excluded from the analysis
the cells by birth status and time that were smaller than 40.
an exogenous fertility shock, uncorrelated with unobserved characteristics of females and males’ partners. Although the most common cause of miscarriages is chromosomal abnormality (between 50% and 80% of all losses during a first semester, [American College of Obstetricians et al. 2013]), other frequent risk factors include anatomical and immunological abnormalities, infections, environmental factors (e.g., pollution) and risk behavior such as substance use (alcohol, cigarettes, and drugs) (Green et al. 2009; Lerner 2003; Matovina et al. 2004). The 2004 Surgeon General report, however, concludes there is no sufficient evidence to infer a causal relationship between smoking and miscarriages (US Department of Health et al. 2004). Previous research has argued that utilizing miscarriages to create comparison groups eliminates a substantial part, but not all, of the unobserved differences between treatment and control groups (Fletcher and Wolfe 2009; Ashcraft and Lang 2006; Hotz et al. 2005).

Figure 2 shows the proportion of first pregnancies that ended up in a live birth or miscarriages in our analytic sample. As can be seen, there is no signs of systematic patterns by time, which suggests miscarriages may be viewed as quasi-random events. However, there is still room for bias due to omitted risk behaviors correlated with pregnancy outcomes, and delinquency-substance use after gestation. In addition, potential violations of the exclusion restriction assumption are also plausible: having a miscarriage is often a traumatic event that could have direct effects on future delinquency and substance use. Thus, the results shown below should be interpreted with caution and in the light of these limitations.

4 Preliminary Results

First, given the setup of the data, our treatment consists of both pregnancy and childbearing. According to the literature, both effects are important with respect to delinquency and substance use. Second, our treatment and control condition correspond to first pregnancy’s outcomes. It does not consider subsequent pregnancies and births. Because the relationship between parenthood and delinquency is dynamic, our estimation is based on how a first pregnancy miscarriage extends the
time until first live birth occurs or the timing of the *catch up* of fertility behaviors. Figure 3 and 6 show how births are delayed after a miscarriage during the first pregnancy.

To summarize our models, we simulate predicted values accounting for fundamental and estimation uncertainty. Then, we compute expected values for the treatment condition (live birth) and control group (miscarriage), and calculate the difference between those two expected values (i.e., a first difference). We simulate 5000 data-sets and report the median, percentile 2.5th and 97.5th of first differences.

### 4.1 Women

Figure 4 shows the treatment effect for delinquency, marijuana use, and binge drinking by time, where *time 0* represents the start of the first pregnancy. Delinquency and substance use are lower in the live birth group (i.e., negative difference), although the uncertainty bands tend to cover the value 0. All the outcomes experience a drop after the first pregnancy begins, and no clear trends are observed previous to the conception. This is different from Monsbakken et al. (2013), who using a within-individual design, observe a decline of offending well-ahead of the individual’s first birth. Our results provide some evidence of the *quasi-random* nature of miscarriages (i.e., there is no apparent selection process prior to the first pregnancy).

Table 3 compares differences for different periods: (1) the effect one year before and one year after the first pregnancy (e.g., time -1 and 1); (1-2) the effect of the first two years before and after pregnancy (e.g., time -1, -2, and 1, 2); (1-3) the first three years, and (1-4) the first 4 years before and after gestation. As can be seen, all the estimates are negative (i.e., parenthood decreases delinquency and substance use), and all of them are statistically significant at conventional levels, except for delinquency during the first year before and after pregnancy. The changes observed in

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14 We account for the residual (observation-level) variance, the uncertainty in the fixed coefficients, and the uncertainty in the variance parameters for the grouping factors (respondents). When predicting values, we use 20 as age of first pregnancy for females, and 22 for males.

15 A predict value contains both fundamental (e.g., randomness in the world) and estimation uncertainty (e.g., finite number of observations), whereas an expected value averages over the fundamental variability and leaves only the estimation uncertainty (King et al. 2000).
delinquency seems to be slower than the changes in substance use. Since most of the women with miscarriages "catch-up" and have a baby after their first pregnancy, our estimates reveal primarily short-term effects.

We explore the heterogeneity hypothesis according to which there should be differences in the effects of parenthood on delinquency-substance use by socioeconomic status. Figure 5 shows treatment effects for female respondents whose parents had some college or more and high school or less at Wave I. Most of the effects overlap (darker area), and there is not a clear pattern by education. We obtain similar results with respect to income and the social disadvantage index (not shown).

4.2 Men

Figure 7 shows the dynamic treatment effects for delinquency, marijuana use, and binge drinking for men. They are similar to those observed for women, except binge drinking where differences between live births and miscarriages are small. Delinquency and marijuana use show a drop after the first pregnancy begins, and no clear trends prior to gestation.

Table 4 compares differences by periods. All marijuana comparisons are negative and significant, while only the comparisons 1-3 and 1-4 are negative and significant. Again, the drop in delinquency seems to be smoother than the one observed for marijuana.

Figure 8 shows treatment effects for male respondents whose parents had some college or more and high school or less at Wave I. Again, most of the effects overlap, and we cannot identify clear differences and patterns by education.

5 Conclusion (to be completed)
References


### 6 Tables and Figures

#### Table 1: Women Descriptive Statistics

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<th>Sd</th>
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<th>Max</th>
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* Unstandardized delinquency score.
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Parent’s education

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* Unstandardized delinquency score.
Figure 1: Distribution of Interview Age by Gender and Time

(a) Women

(b) Men
Figure 2: Proportion Miscarriages by Gender and Time

(a) Women

(b) Men
Figure 3: Number of Births and Proportion Any Birth for Women, by Pregnancy Outcome and Time

(a) Number of Births

(b) Any Birth
Figure 4: Treatment Effect for Women: Models Control for Age-at-first Pregnancy Median, Percentile 2.5th and 97.5th

(a) Delinquency

(b) Marijuana

(c) Binge Drinking
<table>
<thead>
<tr>
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<th>1-4</th>
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Median, 2.5th and 97.5th percentile. Results from comparing expected values of 5000 simulated predicted outcomes. All the models control for age of the first pregnancy.
Figure 5: Treatment Effect for Women by Parents’ Education Wave I, Models controlling for age-at-first pregnancy

(a) Delinquency

(b) Marijuana

(c) Binge Drinking
Figure 6: Number of Births and Proportion Any Birth for Men, by Pregnancy Outcome and Time

(a) Number of Births

(b) Any Birth
Figure 7: Treatment Effect for Men; Models Control for Age-at-first Pregnancy
Median, Percentile 2.5th and 97.5th

(a) Delinquency

(b) Marijuana

(c) Binge Drinking
Table 4: Men Effect Comparisons

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Median, 2.5th and 97.5th percentile. Results from comparing expected values of 5000 simulated predicted outcomes. All the models control for age of the first pregnancy.
Figure 8: Treatment Effect for Men by Parents’ Education Wave I, Models control for age-at-first pregnancy

(a) Delinquency

(b) Marijuana

(c) Binge Drinking
Script of the steps followed to create pregnancy records for females and males:

- Obtain all the pregnancies records available in Wave III and IV.
- Recode pregnancy outcomes: 1 live birth, 2 miscarriage, 3 abortion, 4 still pregnant.
- When a baby died at the hospital we set the outcome to miscarriage.
- For the still pregnant records, to set the date end of pregnancy to the corresponding interview date.
- Combine wave 3 and wave 4 records.
- Remove records (not cases) with missing pregnancy outcome and age of end of pregnancy. Flag those cases.
- Remove duplicates with same ID, end of pregnancy date, and pregnancy outcome.
- Remove the still pregnant records, and flag those cases.
- For the rest of duplicates (i.e., same ID and end of pregnancy date, but different pregnancy outcome), use Wave III information if a pregnancy finishes before Wave III interview date, and Wave IV information if pregnancy finishes after Wave III interview date. This is done at the pregnancy record level. Flag those cases.
- For the rest of duplicates and women, select one record randomly and I flag those cases. For men, select the record with outcome live birth. We are assuming that men can have multiple pregnant partners. For the rest of duplicates (miscarriage or abortion), select one randomly and flag records.
- For women, 5803 first pregnancy records were obtained, 329 of them were flagged using the procedures described above. For men, 4033 first pregnancy records were observed, 256 of them were flagged.
- Select only first pregnancies ending at ages ≥ 14, and with outcomes live birth or miscarriage. In total, 4788 cases for women, and 3390 for men.
- Impute the age at which individuals started their first pregnancy: 20 weeks for miscarriages and 40 weeks for live births. Use dates and then truncate ages.