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

One of the main contribution of the life-course framework to study crime has been to expand our understanding of criminal behavior over time, and to get a better notion of the complexities of the criminal careers and how some factors affect those trajectories. One of the theories highlight the role of life-course (transitions such marriage, employment, military service and parenthood) and how they affect criminal behavior trajectories and desistance (Sampson and Laub 1995; Laub and Sampson 2003).[1]

Previous research examining the consequences of life-course transitions or turning points have found that some life events such marriage, employment and parenthood correlate with reductions of criminal behavior. However, questions about the biases of previous estimates and the conditions under which these effects occur remain. However, because it is difficult, but impossible, to manipulate some of these events, it is not clear if life-course transitions are in fact causal. Those who have a higher tendency to get into a transition are also those with higher probability to desist from crime, or as Kazemian and Farrington (2010) put it: "Since turning points and life events are not randomly assigned among individuals, it is difficult to assess whether these events are *causes* or *correlates* of desistance" (p. 142). There are still good theoretical reasons to expect a causal effect of *parenthood* on crime, either because new parents become attached and invested in their children (informal social control), or because parental obligations reduces time spent in unstructured socializing (routine activities) or with deviant peers (social learning). More importantly, the relevance of estimating the consequences of parenthood 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).

Some scholars have pointed out that previous research on parenthood consequences in criminal behavior is mixed and contradictory (Siennick and Osgood 2008; Kreager et al.

[1]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).

2010; Giordano et al. 2011). However, little is said about what these mixed results mean. In addition to the ubiquitous selection processes that characterized social life and that make difficult the estimation of causal effects, we know that the consequences of life events are rarely deterministic. Social phenomena, given their nature, are contingent, and effects tend to differ by groups of people, context and time. The same idea with respect to criminal behavior has been described by Laub and Sampson (2003). They conceive life-course changes in crime as a result of the constant interaction between individuals, their environment, and random events. The result, *noisy and unpredictable developmental trajectories*. That is why it is not rare to find great heterogeneity in criminal offending over the life course that cannot be simply explained by individual differences. In this context, non-replicable and mixed results are not surprising at all.

Qualitative studies, which pay much more attention to details and heterogeneity, suggest that childbearing would reduce crime and drug use, but mainly in disadvantaged communities. Ethnographic research shows that among disadvantaged urban women the transition to parenthood is more a salient life-course turning point than the transition to marriage (Edin and Kefalas 2005; Giordano et al. 2011). These differences would explain the close link between motherhood and criminal desistance in poor communities. However, even within disadvantaged communities one might expect diverse effects. Persistent offenders, for instance, experience high levels of residential, marital, and job instability, in addition to failure in school, drug addictions, and long periods of incarceration. In that context of marginality and lack of structure, the effect of transition-to-adulthood events might be nil (Laub and Sampson 2003). Thus, it is possible that the inhibitory effects of parenthood on crime simply do not apply to individuals with high risk of institutionalization (Kreager et al. 2010).

The trade-off between dealing empirically with selection bias and heterogeneity is challenging (Xie et al. 2012; Diaz and Fiel 2016). We argue, however, that exploring heterogeneity without reasonably taking care of selection is misleading, not because we believe effects are

homogeneous, but because estimates of differences between groups may be just the result of selection bias and unobserved confounding (Breen et al. 2015). Given the nature of the events we are studying, a permanent issue in previous research has been the definition of an appropriate comparison group able to approximate the counterfactual condition (Kreager et al. 2010), and although most of the recent studies use strategies to overcome this limitation, identification problems still linger.

In this paper, we estimate the effect of live birth from first pregnancies on substance use and delinquency using a natural experiment: miscarriages. 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*. In order to assess some the arguments discussed in the literature, we also examine heterogeneity of the effects by time, gender, and social disadvantage. To our knowledge, no previous studies have explored the link between parenthood and delinquency among females and males using miscarriages as a natural experiment. We think that by applying this approach and improving internal validity, we extend the area of life-course research in a relatively understudied topic, crime and the transition to parenthood.

1.1 Why we should expect an effect of parenthood on crime?

Three main theoretical arguments suggest that parenthood should have an inhibitory effect on criminal behavior or a positive influence on desistance from crime. The first two explanations of the decline of criminal behavior during adulthood highlights the notion of *turning points*, that is, structural and situational changes in the life of offenders that in combination with individual actions and decisions activate a process of desistance. A third explanation emphasizes internal and subjective processes such as cognitive transformations and identity shifts.

First, it is argued that adult roles are simply incompatible with deviance and offending. Sampson and Laub (1995), for instance, indicate that desistance occurs through a gradual

development of *stakes in conformity* that increase the potential costs of criminal behavior. Under the idea that formal and informal social control can vary over the life course and that individuals adapt differently to *structural changes*, the consequences of changing roles will depend on the level of attachment to conventional society of a transition, or as Sampson and Laub (1995) put it, on the "quality, strength, and inter-dependence of social tie" (p. 21).

A second argument suggests that adulthood transitions restructure individuals' lives in ways that reduce opportunities to commit crime. Osgood et al. (1996), for example, explore changes in activity patterns (routine activities) 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 different interpretation to this process, suggesting that the reduction in crime will occur mostly among those who have deviant peers: transitions such as marriage or parenthood would reduce time spent with deviant friends, and consequently, should decrease criminal behavior.

While the first two approaches focus on social and external factors, a third explanation goes beyond events or transitions, proposing that changes in offending depend on *identity shifts*. According to Giordano et al. (2002), offenders experience a cognitive shift toward openness to change their behavior before desisting. Transitions into adult roles, thus, serve as *hooks of change* that give offenders opportunities and resources to desist from crime. The key of a successful desistance process, however, would not be the event itself, but the changes in identify (Giordano et al. 2002; Maruna 2001).

It is difficult to distinguish empirically between subjective (individual) and objective (social) changes as the cause of desistance. Identify shifts of offenders might increase their exposure to certain *events* (selection), but also transitions might modify self-perception and the signification of circumstances in life (re-signification of events always occurs *a posteriori*). Recent theoretical work, however, have tended to integrate both approaches. On the one hand, life events are thought to contribute to the desistance process, but their impact would

depend 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 (Siennick and Osgood 2008; Kazemian and Farrington 2010), parenthood has equal or more potential to be a life altering transition event involving changes in routine activities, identity, and life. Not only that, it also has the potential to affect positively children whose parents move away from crime. Parenthood, however, has received much less research attention than marriage as a plausible explanation for reductions in crime in young adulthood.

1.2 What do previous studies tell us on the relationship between crime and parenthood?

Empirical research on the effects of parenthood on criminal behavior has shown mixed and even contradictory results (Siennick and Osgood 2008; Kreager et al. 2010; Giordano et al. 2011). Some quantitative studies have found that the transition to parenthood is associated with reductions in crime (Monsbakken et al. 2013; Kreager et al. 2010; 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, although they increase afterward. This is one of the few studies that explores changes before and after the transition to parenthood using a within-individual model. even after controlling for invariant individual characteristics, the authors find that the decline of offending occurs well-ahead of the individual's first birth, suggesting selection bias. 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 and find a significant effect of childbear-

ing on subsequent reductions in self-reported offending and substance use. 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, these 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. Similarly, Giordano et al. (2002) report no additional effect of motherhood when controlling for marriage: neither having children nor the number of children were systematically related to desistance in a long-term follow-up of delinquent girls. Varriale (2008) also show that pregnancy was not related to girls' leaving gangs, and Thompson and Petrovic (2009) report no association between having children and desistance from illicit drug use. Finally, Blokland and Nieuwbeerta (2005), using a Dutch sample and retrospective self-report survey data, find no effect of becoming a parent on offending, regardless of marital status.

What do these results tell us about the phenomenon we are studying? Most studies test the statistical hypothesis that the effect of parenthood is zero, and either find significant negative effects of parenthood on criminal behavior (reduction of crime) or reject the null hypothesis. Only a couple of studies find statistically significant effects on the opposite direction of what theories suggest (i.e., increases of criminal behavior with parenthood) (add citations). These discrepancies across studies are usually explained by suggesting that effects are zero, they are very small to be detected, or they are heterogenous.

To conclude that an effect is zero because it is not possible to reject the null hypothesis is misleading. On the one hand, a statically *non-significant* result does not tell us that an effect is zero, but rather it has an equal probability to be either negative, positive or zero, conditional on the model specified. On the other, inherent variation in human actions and measurement plus good theoretical reasons and qualitative evidence of non-zero effects,

makes that hypothesis unappealing. More relevant and attractive would be to explore how large effects are and to what extent they can be generalize to subpopulations of interest.

The statistical power argument requires to have a good idea of the magnitude and variability of the true-effect of parenthood on criminal behavior. Due to differences on research design (i.e., measurement, sample, internal validity) comparison of effect sizes across studies is not straightforward, and can also be misleading. First, selection bias is a reasonable threat given the nature of adulthood transitions we are studying, even when using within-individual designs or matching procedures. Within-individual designs, for instance, assume that actors do not change except for the time-varying individual characteristics included in the model. Behaviors such as addiction and criminal behavior, however, are good examples of actions that shape future preferences and desires. People under the influence of drugs, for example, discount future more heavily, reducing the deterrence effect of the long-term consequences of addiction (Elster 2015). Within-individual studies, thus, are unlikely to control for all key time-varying individual beliefs, preferences, and behaviors, not ruling out the possibility that the post-childbirth changes in delinquency would have occurred even without a childbirth due to pre-childbirth changes, unless one has a very precise theory.[2] Studies using matching techniques, on the other hand, rest on the *strong ignorability* assumption, namely, selection into *treatment* would be completely a function of observed variables. This assumption is difficult to defend in desistance studies, as Laub and Sampson (2001) conclude: "Selection is (...) a threat to the interpretation of any desistance study" (p. 23). Second, when effects are small and noisy (due to measurement or heterogeneity) significant effects maybe in the wrong direction and greatly overestimated (Gelman and Carlin 2014). In the case of the relationship between transitions and criminal behavior, this would make sense provided that, as Laub and Sampson (2003) suggested, criminal trajectories over the life-course were characterized by an unpredictable and noisy development.

To what extend the evidence of the effect of parenthood may be weak because the effects

[2]This does not mean that estimation of causal effects using experiments or natural variation does not require also a good theory. See, for example, Sampson's 2010 discussion.

are likely to vary significantly across individuals and social context. Studying teh heterogeneity of the effects may be one way of addressing the disparity in results resported in the research literature, and the larger debate over incarcertion's positive and negative effects. Evidence for the heterogeneity of incarceration effects is abundant.

These examples do not suggest any systematic account of the heterogeneity of the effects of incarceration, but they do suggest that incarceration may vary greaterly in its effects. Currently, there is little understanding of whether this variation is systematic, perhaps unfolding in similar ways in different domains.

Incarceration may have different effect for different social units. The effects of incarceration on individuals, for example, may be quite different from the effects on families or neighborhoods. The idea that incarceration has aggregate-level effects, beyond the individuals incarcerated, turno on external effect

This discussion leads us to think on the conditions and circumstances under which a *prosocial* impact of parenthood might be observed. Some argue that the definition of the treatment (i.e., parenthood) is critical: it may be that active involvement of parents rather than simply having a child makes the difference hypothesized by theory (Siennick and Osgood 2008). This idea encourages us to focus not only on parenthood itself, but on the mechanisms that would trigger an effect of parenthood on crime. Although appealing from a theoretical point of view, estimation of mediation effects is difficult without a precise theory able to specify the causal links behind the data. Pearl (2014), for instance, proposes an approach to identify and estimate causal effects of mediators beyond the (rather stringent) sequential ignorability assumption.[3] A more flexible approach such as this, offers new opportunities to explore mechanisms of desistance and identify the critical assumptions under which identification is possible and how they vary across multiple scenarios. The goal of

[3]The sequential ignorability assumption would require, for instance, that *active involvement of parents* is uncorrelated with unobserved factors affecting both criminal behavior and parenting involvement, even if parenthood was assigned randomly. Consequently, including mediators or post-treatment variables would generally destroy the benefits of randomization. See Pearl (2014) for a discussion about less stringent conditions under which the identification of mediation causal effects would be possible.

this paper, however, is much more modest, but we think, still important: to extend previous research by increasing the internal validity of the estimates of the causal effect of parenthood on criminal behavior.[4]

Another argument to explain discrepancy across studies is that parenthood has different effects across groups or subpopulations. Kreager et al. (2010), for instance, based on qualitative evidence, argue that motherhood has a transforming effect only in disadvantaged neighborhoods, and a smaller or 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, educational or job success to define their adult identity (Edin and Kefalas 2005).[5] That would be 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 significant (and large) negative effects of motherhood on delinquency and substance use.

The disadvantage-heterogeneity argument, however, can also go the other way around. According to McMahon (1995), motherhood is seen as rewarding across different SES levels, with lower SES women being more likely to describe the transition in terms of new obligations and as part of a process of settling down.[6] 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 support from the family of origin and extended social networks) might provide the necessary means for suc-

[4]Other conditions or moderators discussed in the literature are *wantedness* and *relationship status* with the other parent. While *wantedness* would be an indicator of cognitive transformation or *readiness to change* so that having a baby that was wanted would have higher inhibitory effects on criminal behavior, most of the studies measure *wantedness* using retrospective questions (Giordano et al. 2011). These retrospective questions are problematic because answers may be affected by desirability bias and re-signification of past events. Another condition discussed in the literature is *relationship status*. Having a baby while being involved in a more serious relationship (e.g., married or cohabiting) may have a stabilizing effect and increase informal social control. *Relation status*, however, may just reflect selection mechanisms: having a child within marriage, cohabiting union, or not living with the other parent can be associated with different stages of the life-course and criminal involvement.

[5]Moloney et al. (2009) show similar evidence regarding men.

[6]This is also reported by Cobbina (2009) and Michalsen (2011, 2013).

cess in adapting to a new parent role. Giordano et al. (2011), for instance, show that young adults from highly disadvantaged families are less likely than their more advantaged peers to experience subsequent declines in criminal involvement after becoming parents. This might represent a long reach of socioeconomic status, not only as a direct influence of SES on crime but also by setting the conditions necessary to transform a major transition event into a process of desistance from crime. Kreager et al. (2010), in part, also follow a similar argument when explaining why their results differ from those of studies using highly disadvantaged samples (e.g., chronically unemployed, previously incarcerated people, and addicts). They speculate that the motherhood effect might follow an inverted U-shaped function across SES: "... absent among the affluent and severely impaired but present among most residents of disadvantaged neighborhood" (p. 249). To argue that the effect would be absent among the affluent and highly impaired might be too audacious, specially when compelling theoretical reasons suggest these effects would not be exactly zero in those groups. Structural factors likely play a critical role in shaping life chances and making role transitions, such as parenthood, triggers of a desistance process. That is why a binary conceptualization of the disadvantaged heterogeneity hypothesis (disadvantaged groups versus middle-class) sounds very simplistic.

Another important moderator of the parenthood-crime relationship discussed in the literature is gender. While most of studies focuses on mothers, there is emerging work that examines the influence of fatherhood in moving away from crime. Usually, the role of being a parent is experienced differently by men and women: daily tasks associated with childcare tend not to be divided equally, even regardless of couple's intentions (Monsbakken et al. 2013). The period of pregnancy, for example, might 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, the idea of substantially different childrearing roles for women and men, tend to overemphasize the centrality of the childbearing experience for females, and underemphasizes the role of the father (Casper and Bianchi

2009). Roles tend to be more dynamic and the meaning of parenthood is likely to vary across groups. Empirical evidence, again, 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 increase afterward (Monsbakken et al. 2013), or that parenthood reduced serious offending only for men (Zoutewelle-Terovan et al. 2014). This small literature also faces the same empirical limitations described above, leading to a lack of causal estimates.

Finally, much less attention has been paid to the duration of the effects. The expected effect's duration can be deduced from the mechanisms specified by theory. Turning points, for instance, are based on structural transitions that under the right conditions would trigger long-term changes in behavior, provided informal social control remains relatively stable over time. If a transition, in contrast, is mostly related to changes in routine activities and use of time and those activities have the potential to change substantially over time, we should expect parenthood effects are limited to the first years after childbirth.[7] Monsbakken et al. (2013), for instance, using a period from five years before to five years after parenthood, find no signs of long-term effect on the prevalence of offending among women (except to those not residing with other partner), and a gradual decline after parenthood for father not residing with other partner. Kreager et al. (2010), in contrast, find an amplification of motherhood effect across time: teenage motherhood effects tend to persist across time, and evidence of decay occur only several years after childbearing.

2 The current study

This study seeks to extend previous research on the relationship between parenthood, crime and substance use, by improving the internal validity of causal estimates and exploring

[7] Osgood and Lee (1993), for example, show evidence that very young children have the greatest impact on their parent's routines and schedules.

heterogeneity across domains such as time, gender, and social disadvantage. For this, we utilize 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) and has been used to measure the impact of teen motherhood on a host of outcomes, from educational attainment, earnings, and welfare dependence, to wellbeing. Our analyses, however, are not restricted to teen childbearing, and include pregnancies occurred during adolescence and young adulthood (13-31 years old). In addition, following Fletcher (2012), and given the unique characteristics of our data, we not only focus on motherhood but extend our analysis to young men whose partners experienced a miscarriage or gave birth. We also take advantage of the longitudinal nature of our data and explore temporal changes of the effects and assess directly the disadvantage heterogeneity hypothesis by examining differences in parenthood benefits across social disadvantage.

2.1 Data

We use data from the restricted version of the National Longitudinal Study of Adolescent to Adult 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 90 thousand students and an in-home questionnaire administered to a subsample of about 20 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 during Wave IV. About twelve thousand of the Vave 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 considered only respondents who reported a pregnancy in Wave III and

IV, and unified pregnancy records from both waves[8]. Following previous research (Ashcraft and Lang 2006; Fletcher and Wolfe 2012; Fletcher 2012), we limit our sample by focusing on *first pregnancies* that ended in either a live birth or a miscarriage (4786 females and 3389 males). We are estimating, thus, the effect of childbearing using a non-random group of young adults (those selected into pregnancy during the period of the study), and within that group those who had either a live birth or miscarriage (i.e., those who did not choose to have an abortion)[9]. Unlike research focusing only on teen pregnancies, our study includes a more diverse sample involving pregnancies that occurred both during adolescence and young adulthood: 52% of women interviewed in Wave III and IV (9,280) and 40% of men interviewed in Wave III and IV (8,557). Our analytical sample, however, is slightly older than the remaining Add Health sample, their delinquency and substance use are higher, and they show a lower socioeconomic status.[10]

A common matter of concern with respect to measuring miscarriage through self-reported questions is that some of the responses may in fact be misreported elective abortions (e.g., medical abortions). This could potentially bias our results if misreporting is related to characteristics that predict young adult delinquency and substance use. In the Add Health study, respondents used compute-assisted personal interview technology for sensitive questions. This should decrease misreporting of abortion (reduction of social desirability and stigma). Additionally, according to Fletcher and Wolfe (2012), self-report pregnancy outcomes in the Add Health data match more closely official Vital Statistics than other studies. There are still some differences with official reports what suggests some remaining bias may be present. There are also reasons to expect differences in pregnancy reports by gender.

[8]The procedure followed to combine pregnancy records is detailed in the *Data Appendix*.

[9]The selection of the analytic sample had also to meet the criteria detailed in the *Data Appendix*

[10]We show statistical comparisons in Table of the *Supplemental Material*. Although all of the comparisons are statistical significant at conventional levels, the distribution of the social disadvantage variables looks relatively similar (see Figure S1 of the *Supplemental Material*), except for education where our analytical sample has a much more lower proportion of parents with college education. Hence, we are using a sample that is hardly homogenous with respect to social disadvantage and pretty similar to the range of values observed in the Add Health, resulting appropriate to examine heterogeneity of the effect of parenthood on delinquency and substance use by social disadvantage.

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 that the fertility information reported by young men in the Add Health data, although no without measurement errors, seems to have no systematic problems.

As previous studies have acknowledged (Ashcraft and Lang 2006; Fletcher and Wolfe 2009), the abortion-miscarriage distinction may also not always be *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).[11] Most of this discussion has taken place when estimating the impact of teen motherhood on outcomes such as earnings, educational attainment and wellbeing. In our case, it is less clear how bias could behave, not only because of the nature of the dependent variables but also because our analysis is not restricted to teenagers. In order to simplify our analysis and have more flexibility when exploring heterogeneity we used the approach suggested by Ashcraft and Lang (2006).

2.2 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)

[11] A way to deal with this issue is 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. In the Add Health data, however, the duration of pregnancies is only available in Wave III and for women. Although, we conducted robustness checks using late miscarriages (not shown), due to the considerable reduction in our sample we were not able to obtain stable estimates and detect meaningful patterns.

using or threatening to use a weapon to get something from someone, (5) selling marijuana or other drugs, (6) stealing something worth less than $50, and (7) taking part in a fight where a group of your friend was against another group.[12] 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.[13] Because delinquency decreases with age, we standardized the delinquency scale by age at the time of the interview. This is an alternative way to control for age at interview and keep a standardized version of the delinquency scale. To complement our analyses we also used a binary version of the delinquency index - none (0) or any offenses (1). With respect to substance use, we considered 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).

Because we are interested in exploring the duration of the effect of parenthood on our outcome measures, we needed to know the age at which females and males' partners started their first pregnancy. Because we did not have the duration of gestation for all males' partners in Wave III and IV, and for females in Wave IV, we imputed the age at which their first pregnancy began by subtracting 20 weeks when the pregnancy resulted in a miscarriage, and 40 weeks when it resulted in a live birth.[14] The treatment variable indicates if the result of the first pregnancy was a live birth (1) or a miscarriage (0). It is important to note that, given the way we defined our treatment variable, we are estimating the effects of both pregnancy and childbearing. According to the literature, both effects are important with respect to delinquency and substance use, however, some have argued that collapsing pregnancy and parenthood into a single variable might result in an overestimation of the inhibitory effect of parenthood. Given our design, nevertheless, that risk is minimum. We always compare pregnant women and males with pregnant partners. At the same time, we are estimating the duration of the effects: after about a year differences between those who had a miscarriage and a live birth will not be likely attributable to pregnancy.

[12]We averaged items only when at least 50% of them were non-missing.

[13]The Cronbach's alphas were calculated using all the Add Health sample available per wave.

[14]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.

In order to assess the disadvantage heterogeneity hypothesis, we used three socioeconomic variables measured at Wave I: family income (reported by respondents' parents), highest educational level of parents (reported by respondents), and a disadvantage index based on census tract information. The disadvantage index corresponds to a factor score combining rates of unemployment, poverty, proportion of female-head households and people with less than high school at the census tract where the respondent lived during Wave I. The Cronbach's alpha of that score was 0.81. When examining heterogeneity, we dichotomized these disadvantage measures using the median: below the median (0), equal or higher than the median (1).

Tables 1 and 2 show descriptive statistics of the variables used in our models.[15]

2.3 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 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 so that it represents the time in years when Wave I - Wave IV outcomes took place relative to their first (partner's) pregnancy. Figure 1 represents a hypothetical example where a female respondent had her first pregnancy at age 20, Wave I took place when she was 15 years old, Wave II when she was 16, Wave III 22, and Wave IV 27 years old. As can be seen, *time* zero represents age 20, Wave I corresponds to $time = -5$, Wave II to $time = -4$, Wave III $time = 1$, and Wave IV to $time = 7$. Figure S2 shows interview's age box-plots by time. After time 0 (start of pregnancy), the interview's age distribution increases and shrinks.[16] Although this setup allows us to take

[15] We estimated the effect of miscarriages at Wave III on attrition at Wave IV. The average marginal estimate was very small 0.003 (SD = 0.014) and statistically insignificant.

[16] 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.

advantage of the longitudinal nature of the Add Health data, some outcome information at time periods is not covered by the four waves of the Add Health study. Thus, because we do not have annual panel data for the all periods before and after the first pregnancy, different sets of individuals are used to estimate the effect of parenthood on crime by time.[17]

The internal validity of our estimates is based on the assumption that miscarriages are 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, thus, 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 variation in the prevalence of miscarriages over time, which suggests miscarriages may be occurring quasi-randomly (i.e., instrument ignorability). There is still room for bias due to omitted risk behaviors correlated with pregnancy outcomes and delinquency-substance use after gestation, although as we show later that there are no clear signs of selection into pregnancy.

Violations to the exclusion restriction assumption are also possible. Having a miscarriage is often a traumatic event that could have direct effects on future delinquency and substance

[17] Although we could just compare delinquency and substance use between the live birth and miscarriage group without considering when pregnancies and measurement of our outcome variables took place, that would have precluded us from obtaining precise estimates of the effect of parenthood, that according to some, it would have a short duration (Theobald et al. 2015; Monsbakken et al. 2013; Siennick and Osgood 2008).

use, specially for women. However, after controlling for age of the first pregnancy and age at the interview, there is no evidence that the miscarriage group increases substance use or delinquent acts after the first pregnancy (not shown). In other words, our effects (difference in the delinquency and substance use levels between the live birth and miscarriage group) do not seems to be driven by a systematic effect of miscarriages on the outcome variables.

Under this setup we use generalized estimating equations (GEE) to estimate average differences in delinquency and substance use between the treatment and control group by time. We included *time* as set of dummy variables (with $time = 0$ as the reference category) and interact them with our treatment variable (live birth). We standardized the delinquency scale by interview's age, while for the binary outcomes, marijuana use and binge drinking, we control explicitly for age at the interview using a logistic regression model. The models presented below also control for the age at which the pregnancy begun.[18] When examining differences by social disadvantage, we included interactions between time, birth, and a binary version of the social disadvantage variables. We accounted for clustering associated with respondents' repeated observations and estimated robust standard errors using Huber-White sandwich estimators.[19]

When comparing live birth and miscarriage groups by time, the estimation depends on how long the miscarriage group takes to have their first live birth. Figure 3 shows how births are delayed after a miscarriage during the first pregnancy. As can be seen, a considerable percentage of women and men's partners with miscarriages ended up having a baby after their first pregnancy: 61% and 47% of women and men's partners, respectively. Thus, changes in the effect over time could be due to subsequent and multiple pregnancies and births, not only the duration of the effect of being a parent versus not being one. In order to obtain a cleaner estimation of the duration of parenthood effects, we also ran models selecting using

[18]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.

[19]We utilized an *exchangeable* or *compound symmetry* correlation structure. Although GEE estimates with different correlation structures may alter results, GEE is only slightly responsive to the choice of correlation structure (Liang et al. 1992).

as control group only those women who had a miscarriage group but did have a live birth by time x (although they might have been pregnant).[20]

3 Results

To summarize our models and facilitate interpretation, we simulate predicted values accounting for fundamental and estimation uncertainty.[21] Then, we compute expected values for the treatment condition (live birth) and control group (miscarriage), and calculate the difference between those expected values (i.e., first difference).[22] We report the mean, percentile 2.5th and 97.5th of 10,000 simulated expected differences.[23]

3.1 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 when 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). The largest effects during the first 3 years after pregnancy are about 0.25 standard deviations for delinquency, and 10 percentage points for marijuana and binge drinking.

Table 3 compares effects for different periods: (1) the effect one year before and one year

[20] These models, however, reduce our sample size precluding us from estimating effects for long periods after the first pregnancy without getting below our minimum sample size of 40 cases per group and time.

[21] We account for the residual (observation-level) variance and the uncertainty in the fixed coefficients. When predicting values, we use mean values for the age at first pregnancy: 20 for females and 22 for males.

[22] We used the procedure proposed by King et al. (2000).

[23] In the Supplemental Material, we show tables with the main models used to create our simulations.

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., on average, previous to parenthood the mean of delinquency and substance use is higher than afterward). However, the uncertainty bands are close to zero. The changes observed in delinquency seems to be slower than the changes in substance use.

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 6 shows differences in treatment effects for female respondents whose parents had *high school or less* and *some college or more* and at Wave I. The estimates are very uncertain and unstable. Due to sample size limitations, it is difficult to detect any difference or pattern. We obtain similar results with respect to income and the social disadvantage index (see Supplemental Material).

3.2 Men

Figure 5 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 uncertain. Delinquency and marijuana use show a drop after the first pregnancy begins, and no clear trends prior to gestation. The magnitude of the point estimates during the first 4 years after pregnancy are relatively similar to those observed for women.

Table 4 compares differences by periods. All marijuana comparisons are negative, although most of the uncertainty bands are close to zero. For delinquency, comparisons 1-3 and 1-4 are negative and statistically significant. Again, the drop in delinquency seems to be smoother than the one observed for marijuana.

Figure 7 shows different in treatment effects for male respondents whose parents had *high school or less* and *some college or more* at Wave I. Again, we cannot identify clear

differences and patterns by education. Similar results are observed for income and the social disadvantage index (see Supplemental Material).

4 Discussion

Under the assumption that miscarriages occurred *quasi-randomly*, we compare delinquency and substance use in females and males' partners who had either a live birth or miscarriage during their first pregnancy. Using this strategy, we show some evidence of the protective effects of pregnancy and childbearing on deviant behavior both for women and men. Our results, thus, are consistent with the few previous studies reporting positive effects in both genders.

Our findings also suggest short duration pregnancy/parenthood effects on delinquency and substance use. However, our inferences cannot be extended to all pregnancies and births. They are limited only to first pregnancy effects: our identification strategy relies on how females and males' partners catch-up their fertility behavior after a miscarriage in the first pregnancy.

We did not find evidence of heterogeneity by socioeconomic status. Rather than supporting the null hypothesis, the imprecision and instability of our estimates suggest we do not have enough power to detect meaningful differences.

There are a number of limitations that preclude our inferences. First, we rely on the assumption that miscarriages are a random shock that does not affect future levels of delinquency and substance use. Although previous research argues miscarriages might be random, there still room for remaining biases due to selection mechanisms (e.g., risk behaviors, environmental factors) and exclusion restriction violations. Second, even without any measurement error, the distinction abortion-miscarriages is not clean, precluding an appropriate definition of a counterfactual condition.

Third, we rely on self-reported measures of delinquency and substance use, and pregnancy

retrospective reports. Measurement error might be a serious issue. Without further research, it is difficult to assess their magnitude and consequences (e.g., biases).

Despite all these limitations, using recent empirical strategies to create better comparison groups contributes to this literature by supplementing previous study results on the consequences of childbearing on deviant behavior.

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5 Tables and Figures

Figure 1: Hypothetical Female Respondent
(FP = First Pregnancy)

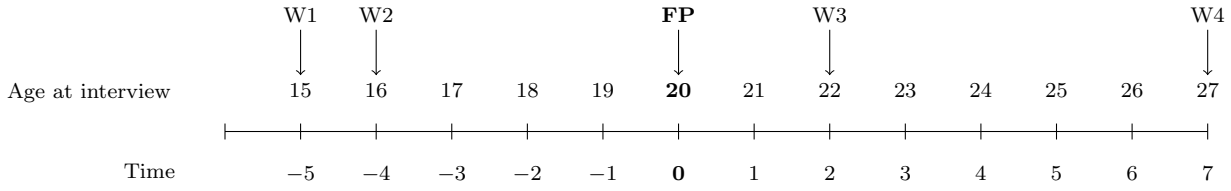


Table 1: Women Descriptive Statistics

Variable	Valid N	Mean	Sd	Min	Max
Delinquency last year W1*	4761	0.12	0.26	0.00	2.71
Delinquency last year W2	3508	0.10	0.24	0.00	3.00
Delinquency last year W3	4033	0.03	0.13	0.00	2.00
Delinquency last year W4	4552	0.02	0.10	0.00	2.71
Marijuana last month W1	4745	0.14	0.34	0.00	1.00
Marijuana last month W2	3451	0.15	0.36	0.00	1.00
Marijuana last month W3	3993	0.14	0.35	0.00	1.00
Marijuana last month W4	4536	0.10	0.30	0.00	1.00
Binge Driking last year W1	4738	0.26	0.44	0.00	1.00
Binge Driking last year W2	3482	0.26	0.44	0.00	1.00
Binge Driking last year W3	3987	0.33	0.47	0.00	1.00
Binge Driking last year W4	4530	0.31	0.46	0.00	1.00
Age Interview W1	4782	15.69	1.69	11.00	21.00
Age Interview W2	3515	16.36	1.61	12.00	21.00
Age Interview W3	4060	22.07	1.73	18.00	27.00
Age Interview W4	4557	28.61	1.74	24.00	34.00
Age first pregnancy	4788	20.45	3.71	13.00	33.00
Proportion Live Births	4788	0.81	0.39	0.00	1.00
Disadvantage score W1	4730	0.16	1.00	-1.28	7.03
Family income W1	3518	39.18	40.02	0.00	900.00
No high school	4447	0.17	0.38	0.00	1.00
High school	4447	0.43	0.49	0.00	1.00
Parents' education					
Some college	4447	0.15	0.36	0.00	1.00
College	4447	0.25	0.43	0.00	1.00

* Unstandardized score (average of 7 items with responses from 1 to 4).

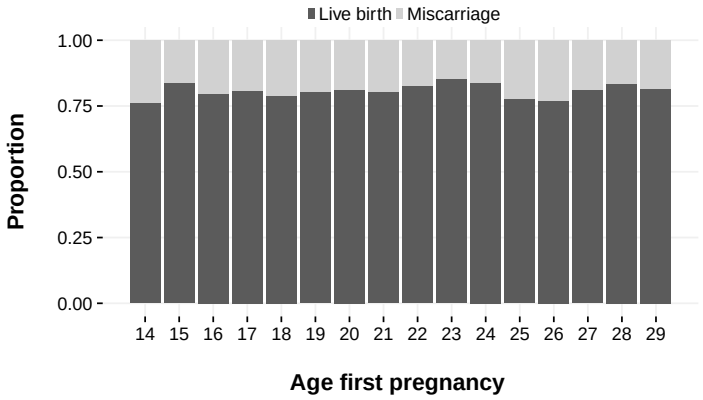
Table 2: Men Descriptive Statistics

Variable	Valid N	Mean	Sd	Min	Max
Delinquency last year W1*	3359	0.26	0.42	0.00	3.00
Delinquency last year W2	2446	0.20	0.35	0.00	3.00
Delinquency last year W3	2733	0.11	0.25	0.00	2.14
Delinquency last year W4	3162	0.05	0.16	0.00	1.86
Marijuana last month W1	3342	0.18	0.38	0.00	1.00
Marijuana last month W2	2362	0.19	0.40	0.00	1.00
Marijuana last month W3	2694	0.25	0.43	0.00	1.00
Marijuana last month W4	3139	0.19	0.40	0.00	1.00
Binge Driking last year W1	3337	0.34	0.47	0.00	1.00
Binge Driking last year W2	2414	0.35	0.48	0.00	1.00
Binge Driking last year W3	2701	0.55	0.50	0.00	1.00
Binge Driking last year W4	3142	0.50	0.50	0.00	1.00
Age Interview W1	3387	15.95	1.71	11.00	21.00
Age Interview W2	2460	16.61	1.66	13.00	22.00
Age Interview W3	2763	22.39	1.76	18.00	28.00
Age Interview W4	3172	28.94	1.76	24.00	34.00
Age first pregnancy	3389	21.96	3.64	13.00	31.00
Proportion Live Births	3389	0.81	0.39	0.00	1.00
Disadvantage score W1	3353	0.10	0.96	-1.24	4.07
Family income W1	2560	39.21	31.37	0.00	550.00
No high school	3155	0.15	0.35	0.00	1.00
High school	3155	0.42	0.49	0.00	1.00
Parents' education					
Some college	3155	0.14	0.35	0.00	1.00
College	3155	0.29	0.46	0.00	1.00

* Unstandardized score (average of 7 items with responses from 1 to 4).

Figure 2: Proportion Miscarriages by Gender and Time

(a) Women



(b) Men

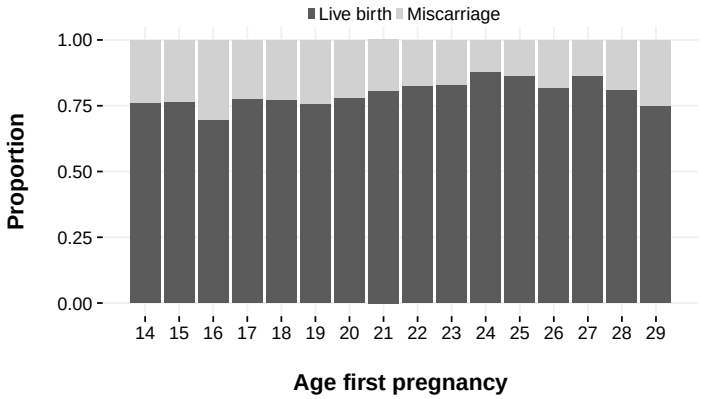
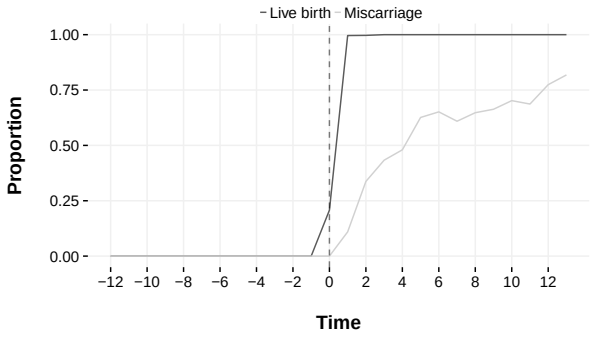


Figure 3: Proportion Any Birth by Pregnancy Outcome and Time

(a) Women



(b) Men

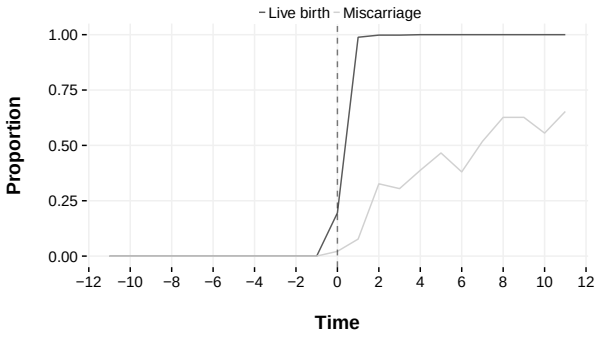
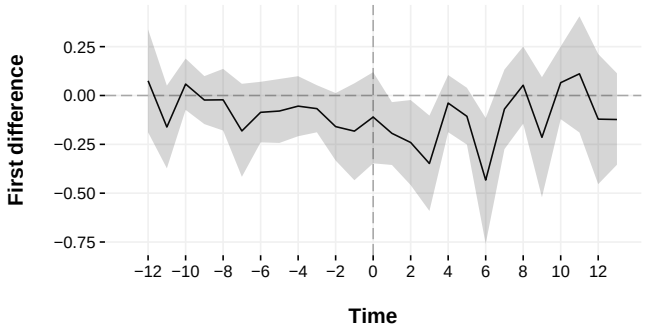


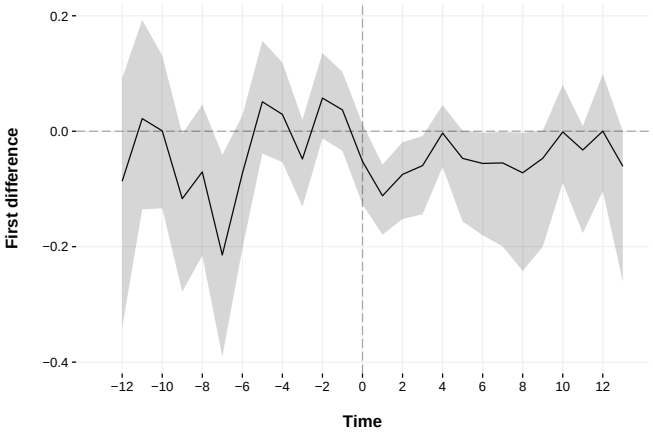
Figure 4: Treatment Effect for Women; GEE Models Control for Age-at-first Pregnancy
Mean + 95% Uncertainty

(a) Delinquency



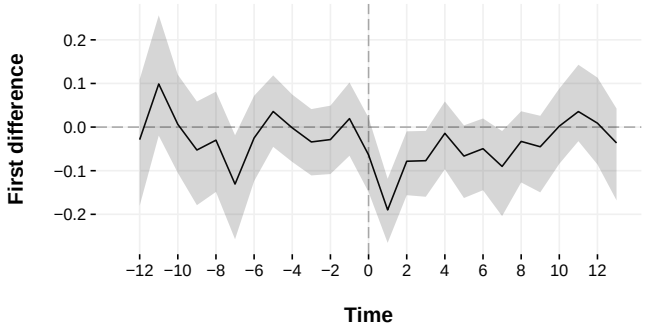
Note: Wald test for delinquency predicting miscarriages at time t < 0: chi−squared = 13.1 (df =

(b) Marijuana



Note: Wald test for marijuana predicting miscarriages at time t < 0: chi−squared = 15.9 (df = 12), p−value = 0.196

(c) Binge Driking



Note: Wald test for binge drinking predicting miscarriages at time t < 0: chi−squared = 12.3 (df =

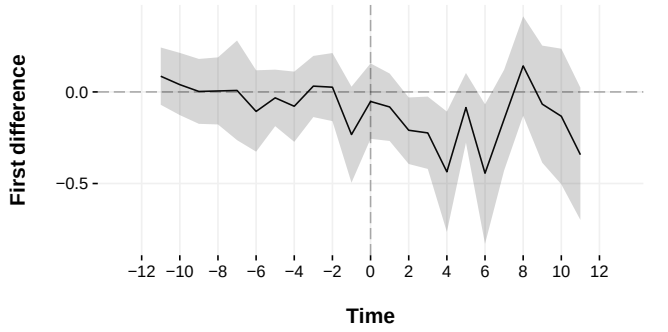
Table 3: Women Before-After Effect Comparisons

	1	1-2	1-3	1-4
Delinquency	-0.016 [-0.323, 0.28]	-0.049 [-0.257, 0.155]	-0.126 [-0.291, 0.039]	-0.09 [-0.225, 0.042]
Marijuana	-0.148 [-0.238, -0.06]	-0.14 [-0.207, -0.075]	-0.097 [-0.153, -0.043]	-0.081 [-0.131, -0.033]
Bing drinking	-0.21 [-0.321, -0.099]	-0.13 [-0.207, -0.05]	-0.101 [-0.163, -0.039]	-0.079 [-0.133, -0.026]

Mean + 95% uncertainty.

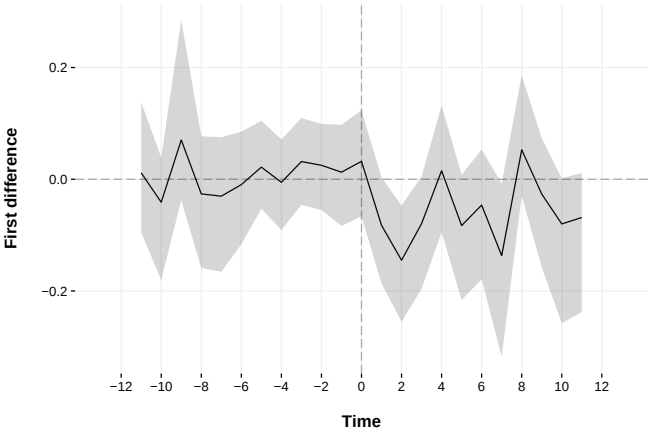
Figure 5: Treatment Effect for Men; GEE Models Control for Age-at-first Pregnancy
Mean + 95% Uncertainty

(a) Delinquency



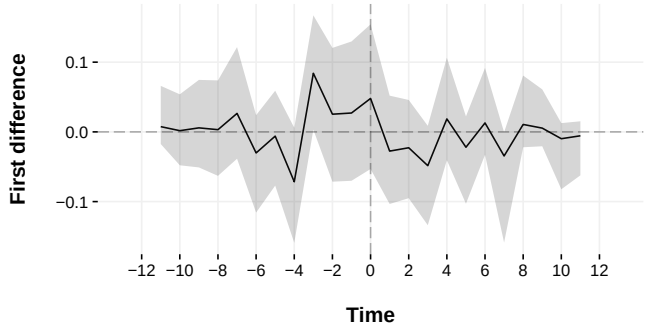
Note: Wald test for delinquency predicting miscarriages at time t < 0: chi−squared = 5.2 (df = 11

(b) Marijuana



Note: Wald test for marijuana predicting miscarriages at time t < 0: chi−squared = 13 (df = 11), p−value = 0.292

(c) Binge Driking



Note: Wald test for binge drinking predicting miscarriages at time t < 0: chi−squared = 8.6 (df =

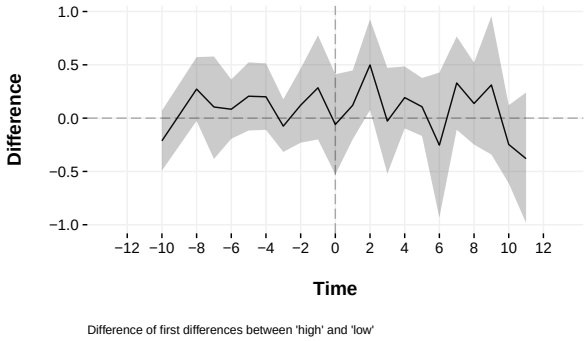
Table 4: Men Before-After Effect Comparisons

	1	1-2	1-3	1-4
Delinquency	0.151 [-0.164, 0.465]	-0.043 [-0.247, 0.163]	-0.114 [-0.278, 0.05]	-0.176 [-0.332, -0.019]
Marijuana	-0.096 [-0.23, 0.034]	-0.133 [-0.226, -0.041]	-0.125 [-0.203, -0.051]	-0.088 [-0.154, -0.023]
Bing drinking	-0.055 [-0.183, 0.074]	-0.052 [-0.138, 0.036]	-0.079 [-0.148, -0.011]	-0.036 [-0.094, 0.022]

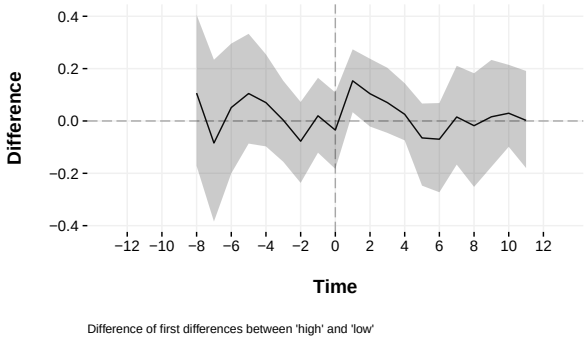
Mean + 95% uncertainty.

Figure 6: Delinquency Difference Treatment Effect for Women by Social Disadvantage Index Measured at Wave I, Models Controlling for Age-at-first Pregnancy

(a) Delinquency



(b) Marijuana



(c) Binge Drinking

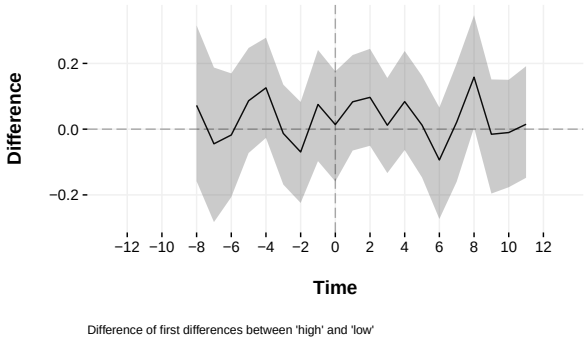
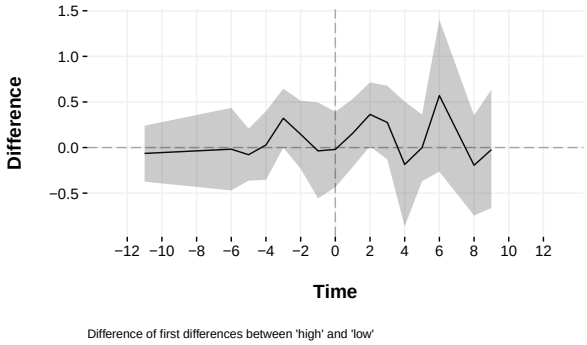


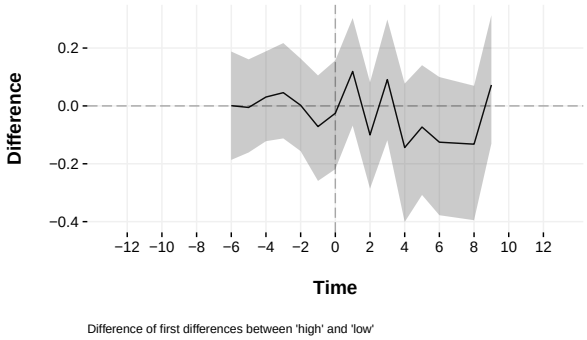
Figure 7: Delinquency Difference Treatment Effect for Men by Social Disadvantage Index Measured at Wave I, Models Controlling for Age-at-first Pregnancy

z

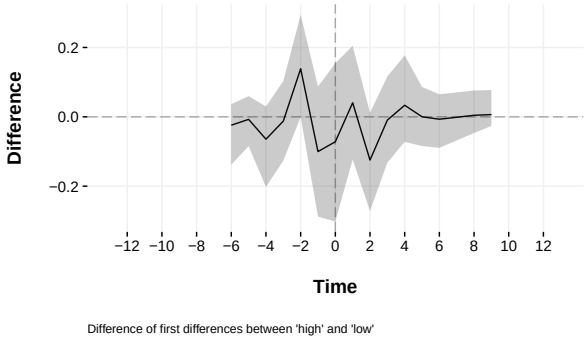
(a) Delinquency



(b) Marijuana



(c) Binge Drinking



Supplemental Material

Pregnancy data setup

We used the following steps 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 identification number (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 flag those cases. For men, select the record with outcome *live birth* and flag those cases. 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 $\geq$ 14, and with outcomes *live birth* or *miscarriage*. In total, 4788 cases for women, and 3390 for men.
- Because date of birth and date of end of pregnancy include only month and year, we set day as 15. We also set the day of the date of interview to 15 despite an exact date is available in the dataset.

- For women, when two pregnancies that ended up in a live birth and occurred within a period of less than 5 months, the earliest pregnancies is kept. Using this criteria, we removed 112 pregnancy records.
- 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.

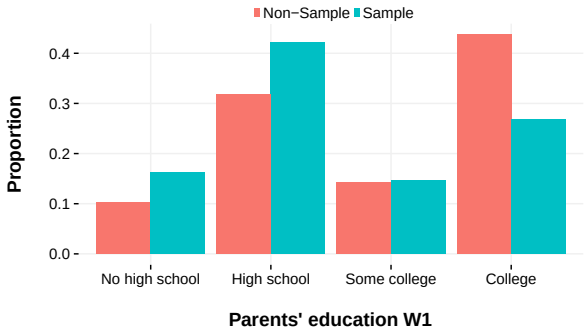
Tables and Figures

Table S1: Comparison between the Analytical Sample and Remaining Add Health Sample Means and p-values of difference (95% of confidence)

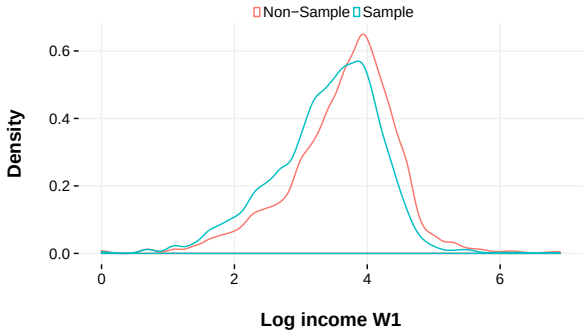
	Men			Women		
Variable	Selected	Excluded	t-test (p-value)	Selected	Excluded	t-test (p-value)
Age W1	15.95	15.49	0.000	15.69	15.32	0.000
Delinquency W1	1.77	1.39	0.000	0.87	0.77	0.008
Marijuana W1	0.18	0.14	0.000	0.14	0.11	0.001
Binge Drinking	0.34	0.27	0.000	0.26	0.21	0.000
Parents' education W1	2.58	2.89	0.000	2.48	2.94	0.000
Income W1	39.21	49.78	0.000	39.18	53.99	0.000
Disadvantage W1	0.10	-0.12	0.000	0.16	-0.14	0.000

Figure S1: Distribution of Disadvantage and Income by Sample

(a) Parents' Education



(b) Household Income



(c) Social Disadvantage

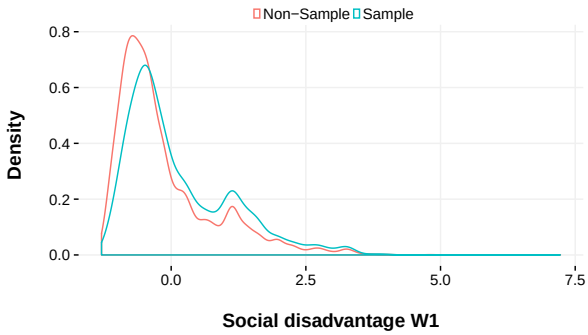
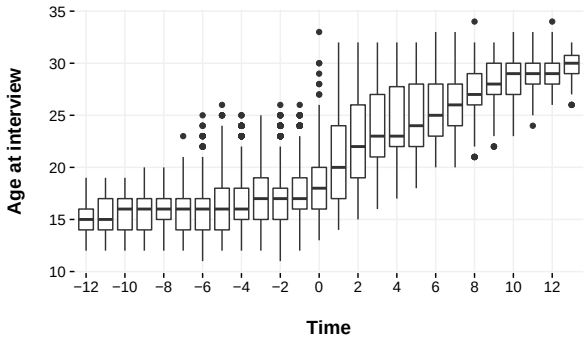
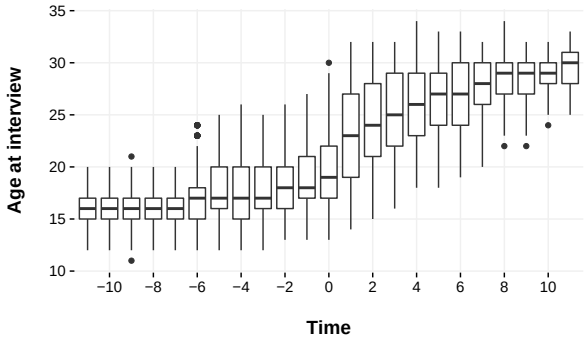


Figure S2: Distribution Age at Interview by Time First Pregnancy

(a) Women



(b) Men



0.1 Models

Table S2: Women Models

	Delinquency	Marijuana	Binge Drinking	Delinquency +	Marijuana +	Binge Drinking +
Intercept	1.22***	−2.29*	−2.89***	1.39***	−2.27*	−1.91*
	(0.30)	(1.05)	(0.77)	(0.35)	(1.15)	(0.84)
Age						
Age first pregnancy	−0.08**	−0.35	−0.29*	−0.10**	−0.37*	−0.55**
	(0.03)	(0.19)	(0.14)	(0.03)	(0.18)	(0.17)
Age first pregnancy^2	0.00**	0.00	0.01***	0.00**	0.01**	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Age at interview		0.50**	0.49***		0.52**	0.66***
		(0.18)	(0.13)		(0.17)	(0.15)
Age at interview^2		−0.01***	−0.01***		−0.01***	−0.01***
		(0.00)	(0.00)		(0.00)	(0.00)
Treatment						
Live birth	−0.11	−0.31	−0.26	−0.10	−0.26	−0.23
	(0.12)	(0.20)	(0.18)	(0.12)	(0.21)	(0.18)
Time						
Time -12	−0.33*	1.11	0.24	−0.31*	0.32	0.79
	(0.14)	(2.13)	(1.51)	(0.14)	(1.93)	(1.83)
Time -11	−0.16	0.61	−0.45	−0.13	−0.14	0.07
	(0.15)	(1.95)	(1.39)	(0.15)	(1.77)	(1.68)
Time -10	−0.31*	0.92	−0.02	−0.28*	0.33	0.48
	(0.12)	(1.77)	(1.26)	(0.13)	(1.60)	(1.52)
Time -9	−0.35**	1.19	0.16	−0.33*	0.65	0.65
	(0.13)	(1.59)	(1.15)	(0.13)	(1.44)	(1.38)
Time -8	−0.29*	1.13	0.11	−0.27*	0.70	0.58
	(0.13)	(1.43)	(1.02)	(0.14)	(1.29)	(1.23)
Time -7	−0.13	1.24	0.42	−0.11	0.91	0.85
	(0.15)	(1.26)	(0.91)	(0.16)	(1.14)	(1.09)
Time -6	−0.24	0.56	0.14	−0.23	0.27	0.51
	(0.14)	(1.08)	(0.78)	(0.14)	(0.98)	(0.93)
Time -5	−0.09	0.04	−0.16	−0.06	−0.17	0.13
	(0.14)	(0.91)	(0.65)	(0.14)	(0.83)	(0.78)
Time -4	−0.12	0.07	−0.01	−0.09	−0.10	0.29
	(0.13)	(0.74)	(0.53)	(0.14)	(0.68)	(0.63)
Time -3	−0.16	0.31	−0.01	−0.14	0.16	0.21
	(0.13)	(0.58)	(0.43)	(0.13)	(0.53)	(0.50)
Time -2	−0.01	−0.28	−0.03	0.01	−0.32	0.14
	(0.14)	(0.44)	(0.33)	(0.14)	(0.42)	(0.37)
Time -1	0.06	−0.30	−0.18	0.10	−0.25	−0.11
	(0.15)	(0.31)	(0.25)	(0.16)	(0.30)	(0.27)
Time 1	−0.06	−0.30	−0.33	−0.01	−0.23	−0.27
	(0.13)	(0.29)	(0.24)	(0.14)	(0.30)	(0.27)
Time 2	−0.13	−0.44	−0.68*	−0.05	−0.21	−0.37
	(0.16)	(0.43)	(0.33)	(0.19)	(0.44)	(0.39)
Time 3	0.05	−0.62	−0.78	0.15	−0.23	−0.43
	(0.17)	(0.58)	(0.43)	(0.22)	(0.56)	(0.53)
Time 4	−0.23	−1.39	−1.11*	−0.12	−0.69	−0.58
	(0.13)	(0.76)	(0.55)	(0.15)	(0.72)	(0.67)
Time 5	−0.21	−1.03	−0.93			
	(0.13)	(0.91)	(0.65)			
Time 6	0.15	−0.94	−1.04			
	(0.14)	(1.06)	(0.77)			
Time 7	−0.14	−1.12	−0.81			
	(0.15)	(1.24)	(0.89)			
Time 8	−0.19	−0.92	−1.13			
	(0.13)	(1.41)	(1.01)			
Time 9	−0.07	−1.41	−1.10			
	(0.19)	(1.59)	(1.13)			

	Delinquency	Marijuana	Binge Drinking	Delinquency +	Marijuana +	Binge Drinking +
Time 10	−0.30*	−1.73	−1.31			
	(0.14)	(1.76)	(1.25)			
Time 11	−0.23	−1.82	−1.75			
	(0.16)	(1.93)	(1.37)			
Time 12	−0.06	−2.16	−1.61			
	(0.17)	(2.11)	(1.50)			
Time 13	−0.17	−1.49	−1.55			
	(0.15)	(2.26)	(1.62)			
Interactions						
Time -12 x Live birth	0.19	−0.29	0.09	0.16	−0.32	0.06
	(0.18)	(0.64)	(0.41)	(0.18)	(0.65)	(0.41)
Time -11 x Live birth	−0.05	0.47	0.81*	−0.08	0.48	0.77*
	(0.16)	(0.49)	(0.38)	(0.16)	(0.51)	(0.38)
Time -10 x Live birth	0.17	0.31	0.29	0.15	0.23	0.23
	(0.14)	(0.40)	(0.33)	(0.14)	(0.41)	(0.34)
Time -9 x Live birth	0.09	−0.38	0.00	0.09	−0.45	−0.07
	(0.13)	(0.38)	(0.34)	(0.14)	(0.39)	(0.34)
Time -8 x Live birth	0.09	−0.08	0.12	0.09	−0.15	0.07
	(0.14)	(0.38)	(0.32)	(0.14)	(0.37)	(0.33)
Time -7 x Live birth	−0.07	−0.87*	−0.36	−0.06	−1.00**	−0.39
	(0.16)	(0.38)	(0.33)	(0.16)	(0.38)	(0.33)
Time -6 x Live birth	0.02	−0.08	0.15	0.02	−0.15	0.15
	(0.15)	(0.34)	(0.27)	(0.15)	(0.34)	(0.28)
Time -5 x Live birth	0.03	0.60	0.42	−0.00	0.50	0.42
	(0.15)	(0.32)	(0.25)	(0.15)	(0.32)	(0.26)
Time -4 x Live birth	0.06	0.47	0.25	0.04	0.41	0.19
	(0.14)	(0.30)	(0.24)	(0.15)	(0.30)	(0.24)
Time -3 x Live birth	0.04	0.06	0.12	0.05	0.04	0.09
	(0.13)	(0.28)	(0.24)	(0.14)	(0.28)	(0.24)
Time -2 x Live birth	−0.05	0.66*	0.14	−0.05	0.59*	0.12
	(0.15)	(0.30)	(0.24)	(0.15)	(0.30)	(0.24)
Time -1 x Live birth	−0.07	0.54	0.34	−0.10	0.43	0.32
	(0.16)	(0.28)	(0.24)	(0.17)	(0.29)	(0.24)
Time 1 x Live birth	−0.08	−0.73*	−0.68**	−0.13	−0.75*	−0.77**
	(0.14)	(0.29)	(0.24)	(0.15)	(0.31)	(0.25)
Time 2 x Live birth	−0.13	−0.35	−0.14	−0.21	−0.44	−0.48
	(0.16)	(0.30)	(0.25)	(0.20)	(0.34)	(0.27)
Time 3 x Live birth	−0.24	−0.26	−0.15	−0.33	−0.42	−0.56
	(0.17)	(0.31)	(0.26)	(0.22)	(0.35)	(0.30)
Time 4 x Live birth	0.07	0.29	0.18	−0.02	−0.11	−0.44
	(0.14)	(0.36)	(0.28)	(0.16)	(0.41)	(0.32)
Time 5 x Live birth	0.00	−0.25	−0.12			
	(0.14)	(0.35)	(0.26)			
Time 6 x Live birth	−0.32*	−0.30	−0.04			
	(0.15)	(0.32)	(0.27)			
Time 7 x Live birth	0.04	−0.33	−0.26			
	(0.16)	(0.32)	(0.28)			
Time 8 x Live birth	0.16	−0.45	0.06			
	(0.16)	(0.36)	(0.28)			
Time 9 x Live birth	−0.10	−0.29	−0.02			
	(0.20)	(0.39)	(0.29)			
Time 10 x Live birth	0.18	0.32	0.28			
	(0.15)	(0.40)	(0.30)			
Time 11 x Live birth	0.22	−0.15	0.54			
	(0.19)	(0.41)	(0.31)			
Time 12 x Live birth	−0.01	0.34	0.34			
	(0.19)	(0.46)	(0.33)			
Time 13 x Live birth	−0.01	−0.46	−0.02			
	(0.17)	(0.41)	(0.34)			
Num. obs.	16545	16420	16434	12016	11916	11931
Num. clust.	4788	4787	4787	4770	4768	4765

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. GEE models with an exchangeable correlation structure. + Control group consisting of those have not catched up.

Table S3: Men Models

	Delinquency	Marijuana	Binge Drinking	Delinquency +	Marijuana +	Binge Drinking +
Intercept	1.95***	−1.66	−5.91***	2.16***	−1.40	−5.75***
	(0.46)	(1.14)	(1.01)	(0.53)	(1.19)	(1.05)
Age						
Age first pregnancy	−0.15***	−0.36	0.13	−0.17***	−0.48	0.04
	(0.04)	(0.25)	(0.19)	(0.05)	(0.26)	(0.22)
Age first pregnancy^2	0.00**	0.01***	0.01**	0.00***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Age at interview		0.43	0.34		0.54*	0.43*
		(0.23)	(0.18)		(0.25)	(0.20)
Age at interview^2		−0.01***	−0.01***		−0.01***	−0.02***
		(0.00)	(0.00)		(0.00)	(0.00)
Treatment						
Live birth	−0.05	0.18	0.21	−0.04	0.18	0.18
	(0.10)	(0.26)	(0.23)	(0.10)	(0.27)	(0.24)
Time						
Time -11	−0.24*	−0.71	−4.60*	−0.21	−0.36	−4.34*
	(0.11)	(2.54)	(1.91)	(0.11)	(2.68)	(2.15)
Time -10	−0.24*	−0.19	−4.07*	−0.20	0.13	−3.86*
	(0.12)	(2.31)	(1.75)	(0.12)	(2.43)	(1.96)
Time -9	−0.20	−1.32	−3.69*	−0.17	−1.00	−3.50*
	(0.12)	(2.12)	(1.58)	(0.12)	(2.24)	(1.77)
Time -8	−0.17	−0.23	−3.30*	−0.13	0.13	−3.16*
	(0.12)	(1.86)	(1.41)	(0.12)	(1.96)	(1.58)
Time -7	−0.18	0.00	−2.89*	−0.13	0.27	−2.78*
	(0.16)	(1.63)	(1.24)	(0.15)	(1.72)	(1.39)
Time -6	−0.04	−0.18	−2.29*	−0.00	0.07	−2.14
	(0.13)	(1.40)	(1.06)	(0.13)	(1.47)	(1.19)
Time -5	−0.10	−0.20	−1.79*	−0.08	0.01	−1.69
	(0.11)	(1.17)	(0.89)	(0.11)	(1.23)	(1.00)
Time -4	−0.01	0.10	−1.09	0.03	0.20	−1.03
	(0.12)	(0.96)	(0.73)	(0.12)	(1.01)	(0.82)
Time -3	−0.11	−0.10	−1.38*	−0.09	0.00	−1.34*
	(0.11)	(0.74)	(0.57)	(0.11)	(0.78)	(0.64)
Time -2	−0.10	−0.31	−0.77	−0.06	−0.20	−0.75
	(0.12)	(0.55)	(0.44)	(0.12)	(0.57)	(0.48)
Time -1	0.18	0.05	−0.36	0.19	0.04	−0.39
	(0.14)	(0.37)	(0.31)	(0.14)	(0.38)	(0.33)
Time 1	0.02	0.53	0.51	0.10	0.63	0.51
	(0.11)	(0.38)	(0.31)	(0.11)	(0.40)	(0.33)
Time 2	−0.00	0.62	0.58	0.14	0.81	0.54
	(0.12)	(0.55)	(0.44)	(0.14)	(0.59)	(0.50)
Time 3	−0.02	0.34	1.30*	0.05	0.35	1.54*
	(0.13)	(0.77)	(0.60)	(0.14)	(0.82)	(0.68)
Time 4	0.24	−0.08	1.27	0.57*	0.27	1.36
	(0.19)	(0.99)	(0.77)	(0.27)	(1.05)	(0.88)
Time 5	−0.10	0.59	1.99*			
	(0.12)	(1.20)	(0.91)			
Time 6	0.22	0.57	2.07			
	(0.21)	(1.43)	(1.09)			
Time 7	−0.03	0.98	3.16*			
	(0.15)	(1.65)	(1.25)			
Time 8	−0.05	0.22	2.70			
	(0.14)	(1.88)	(1.42)			
Time 9	0.07	0.55	3.41*			
	(0.17)	(2.11)	(1.59)			
Time 10	0.02	0.66	3.95*			

	Delinquency	Marijuana	Binge Drinking	Delinquency +	Marijuana +	Binge Drinking +
	(0.19)	(2.34)	(1.76)			
Time 11	0.13	0.90	4.40*			
	(0.19)	(2.56)	(1.93)			
Time -11 x Live birth	0.14	−0.07	−0.03	0.13	−0.08	−0.03
	(0.13)	(0.44)	(0.34)	(0.13)	(0.45)	(0.34)
Interactions						
Time -10 x Live birth	0.09	−0.51	−0.16	0.08	−0.51	−0.13
	(0.14)	(0.41)	(0.37)	(0.13)	(0.41)	(0.37)
Time -9 x Live birth	0.05	0.50	−0.10	0.05	0.49	−0.07
	(0.14)	(0.57)	(0.39)	(0.14)	(0.57)	(0.39)
Time -8 x Live birth	0.06	−0.36	−0.16	0.05	−0.41	−0.08
	(0.14)	(0.43)	(0.38)	(0.14)	(0.42)	(0.38)
Time -7 x Live birth	0.06	−0.37	0.07	0.04	−0.36	0.16
	(0.17)	(0.40)	(0.36)	(0.17)	(0.40)	(0.37)
Time -6 x Live birth	−0.06	−0.23	−0.45	−0.08	−0.25	−0.43
	(0.15)	(0.37)	(0.31)	(0.15)	(0.37)	(0.31)
Time -5 x Live birth	0.02	−0.04	−0.24	0.02	−0.08	−0.22
	(0.13)	(0.34)	(0.30)	(0.13)	(0.34)	(0.30)
Time -4 x Live birth	−0.03	−0.20	−0.55	−0.04	−0.18	−0.49
	(0.14)	(0.33)	(0.30)	(0.14)	(0.34)	(0.30)
Time -3 x Live birth	0.08	0.01	0.18	0.09	0.02	0.23
	(0.13)	(0.34)	(0.30)	(0.13)	(0.34)	(0.31)
Time -2 x Live birth	0.08	−0.01	−0.10	0.07	−0.03	−0.09
	(0.14)	(0.35)	(0.30)	(0.14)	(0.35)	(0.31)
Time -1 x Live birth	−0.18	−0.10	−0.10	−0.18	−0.02	−0.05
	(0.16)	(0.34)	(0.29)	(0.15)	(0.34)	(0.30)
Time 1 x Live birth	−0.03	−0.59	−0.35	−0.10	−0.72*	−0.36
	(0.13)	(0.33)	(0.29)	(0.13)	(0.34)	(0.30)
Time 2 x Live birth	−0.16	−0.94**	−0.34	−0.29	−1.18***	−0.31
	(0.14)	(0.33)	(0.30)	(0.16)	(0.36)	(0.33)
Time 3 x Live birth	−0.17	−0.63	−0.57	−0.23	−0.72	−0.83*
	(0.15)	(0.36)	(0.32)	(0.16)	(0.39)	(0.36)
Time 4 x Live birth	−0.39	−0.08	−0.07	−0.71*	−0.50	−0.20
	(0.20)	(0.40)	(0.33)	(0.28)	(0.44)	(0.40)
Time 5 x Live birth	−0.03	−0.67	−0.46			
	(0.14)	(0.37)	(0.32)			
Time 6 x Live birth	−0.39	−0.47	−0.06			
	(0.22)	(0.36)	(0.32)			
Time 7 x Live birth	−0.10	−1.09**	−0.86*			
	(0.17)	(0.38)	(0.36)			
Time 8 x Live birth	0.19	0.20	−0.04			
	(0.17)	(0.40)	(0.34)			
Time 9 x Live birth	−0.01	−0.37	−0.09			
	(0.19)	(0.42)	(0.35)			
Time 10 x Live birth	−0.08	−0.83	−0.49			
	(0.21)	(0.44)	(0.37)			
Time 11 x Live birth	−0.29	−0.75	−0.41			
	(0.21)	(0.44)	(0.37)			
Num. obs.	11157	10997	11056	8810	8676	8730
Num. clust.	3387	3385	3387	3382	3378	3380

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. GEE models with an exchangeable correlation structure. + Control group consisting of those have not catched up.

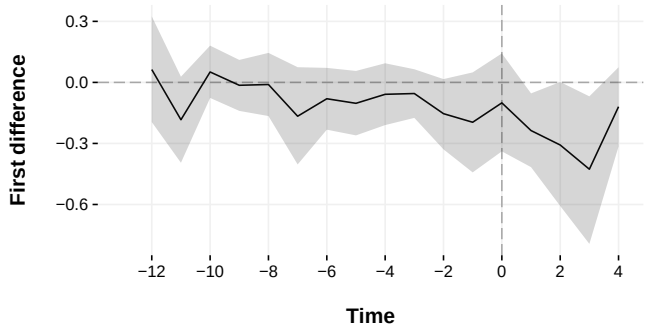
Table S4: Women Before-After Effect Comparisons with Non-Catchup Control Group

	1	1-2	1-3	1-4
Delinquency	-0.042 [-0.344, 0.265]	-0.098 [-0.337, 0.135]	-0.191 [-0.397, 0.01]	-0.159 [-0.324, 0.002]
Marijuana	-0.114 [-0.245, -0.021]	-0.116 [-0.205, -0.045]	-0.091 [-0.165, -0.03]	-0.082 [-0.147, -0.03]
Bing drinking	-0.163 [-0.296, -0.043]	-0.122 [-0.216, -0.037]	-0.108 [-0.188, -0.036]	-0.102 [-0.172, -0.037]

Mean + 95% uncertainty.

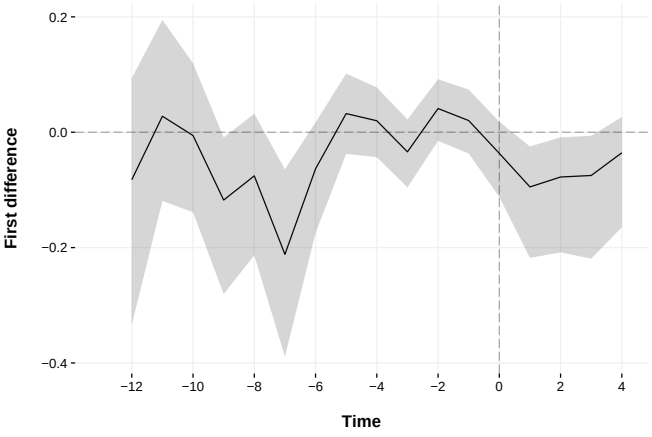
Figure S3: Treatment Effect for Women; GEE Models Control for Age-at-first Pregnancy
Mean + 95% Uncertainty; Control group does not catch-up at time x

(a) Delinquency



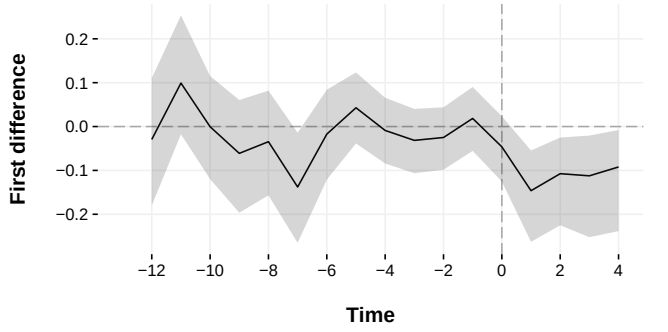
Note: Wald test for delinquency predicting miscarriages at time t < 0: chi−squared = 13.1 (df = 1

(b) Marijuana



Note: Wald test for marijuana predicting miscarriages at time t < 0: chi−squared = 15.9 (df = 12), p−value = 0.196

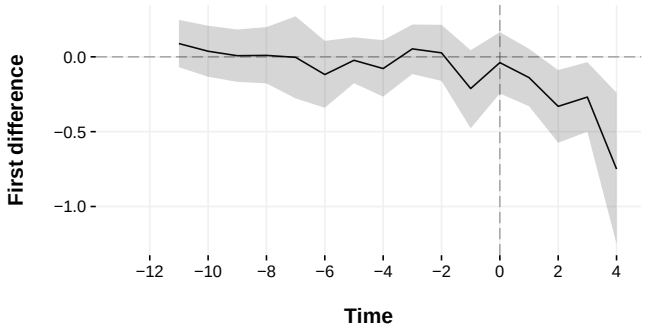
(c) Binge Driking



Note: Wald test for binge drinking predicting miscarriages at time t < 0: chi−squared = 12.3 (df =

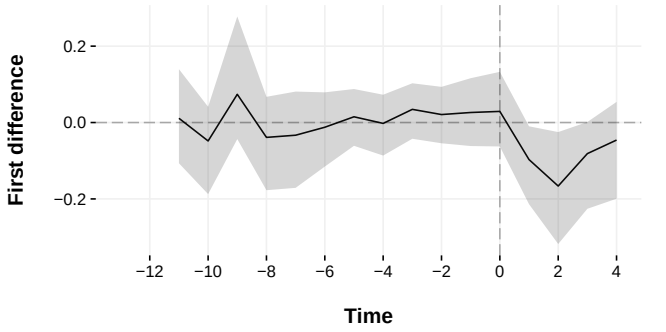
Figure S4: Treatment Effect for Men; GEE Models Control for Age-at-first Pregnancy
Mean + 95% Uncertainty; Control group does not catch-up at time x

(a) Delinquency



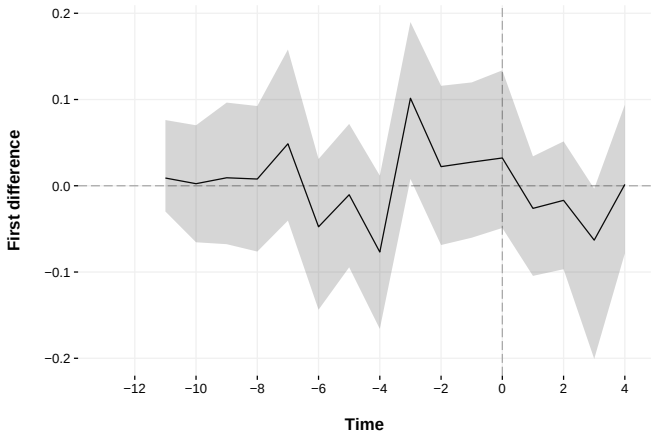
Note: Wald test for delinquency predicting miscarriages at time t < 0: chi−squared = 5.2 (df = 11

(b) Marijuana



Note: Wald test for marijuana predicting miscarriages at time t < 0: chi−squared = 13 (df = 11),

(c) Binge Driking



Note: Wald test for binge drinking predicting miscarriages at time t < 0: chi−squared = 8.6 (df = 11), p−value = 0.66

Table S5: Men Before-After Effect Comparisons with Non-Catchup Control Group

	1	1-2	1-3	1-4
Delinquency	0.075 [-0.25, 0.392]	-0.141 [-0.36, 0.078]	-0.202 [-0.382, -0.027]	-0.319 [-0.508, -0.133]
Marijuana	-0.122 [-0.268, 0.008]	-0.155 [-0.268, -0.044]	-0.141 [-0.232, -0.055]	-0.117 [-0.194, -0.042]
Bing drinking	-0.054 [-0.172, 0.055]	-0.046 [-0.13, 0.034]	-0.086 [-0.162, -0.017]	-0.045 [-0.107, 0.015]

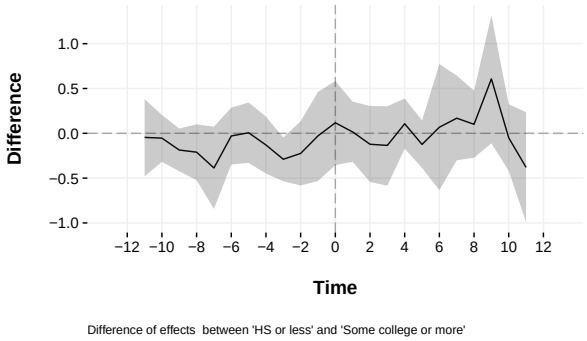
Mean + 95% uncertainty.

0.2 Results for Women by Education and Income

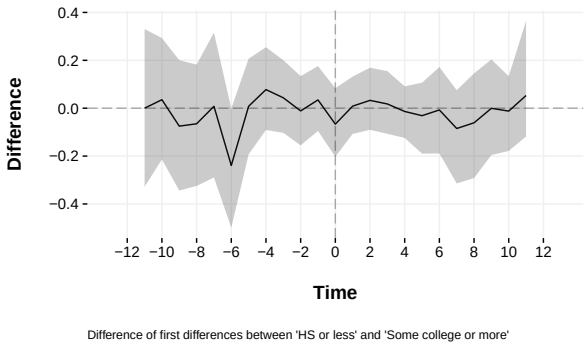
Figure S5: Delinquency Difference Treatment Effect for Women by Parents' Education Measured at Wave I, Models Controlling for Age-at-first Pregnancy

z

(a) Delinquency



(b) Marijuana



(c) Binge Drinking

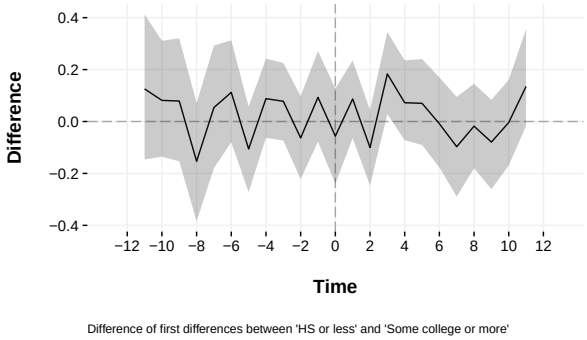
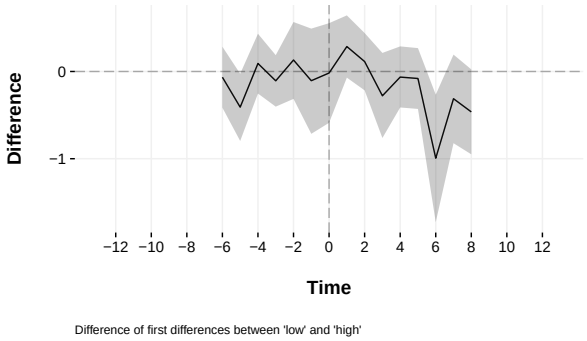


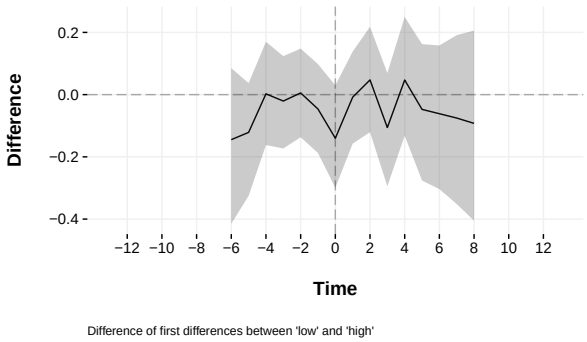
Figure S6: Delinquency Difference Treatment Effect for Women by Household Income Measured at Wave I, Models Controlling for Age-at-first Pregnancy

z

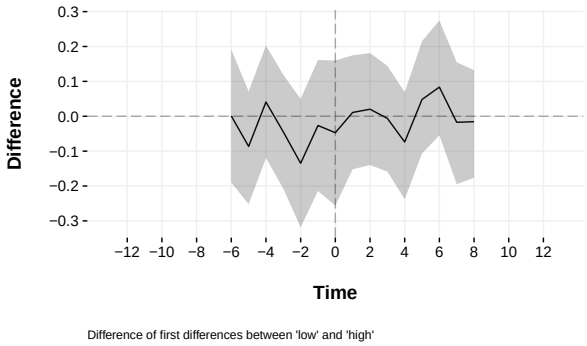
(a) Delinquency



(b) Marijuana



(c) Binge Drinking

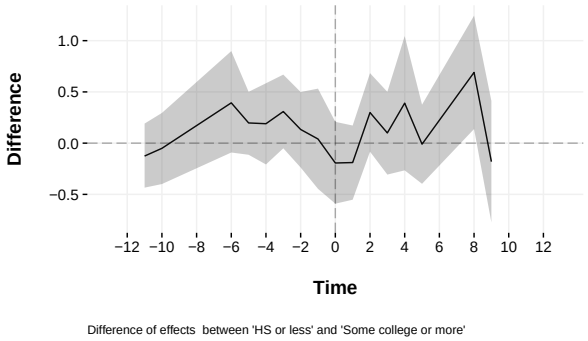


0.3 Results for Men by Education and Income

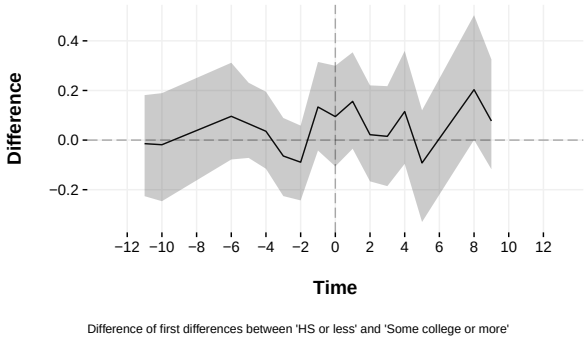
Figure S7: Delinquency Difference Treatment Effect for Men by Social Disadvantage Index Measured at Wave I, Models Controlling for Age-at-first Pregnancy

z

(a) Delinquency



(b) Marijuana



(c) Binge Drinking

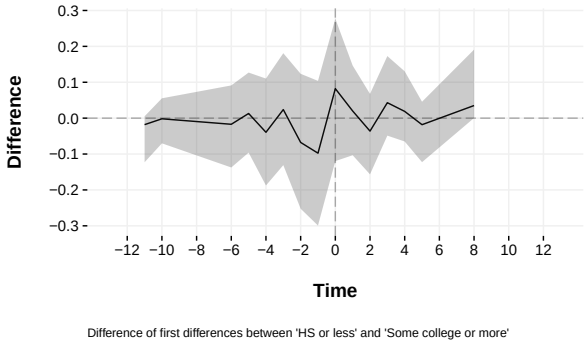
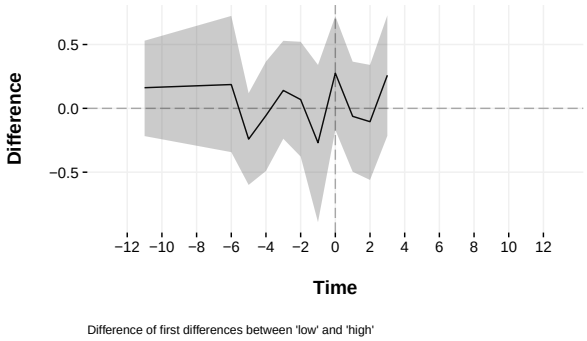


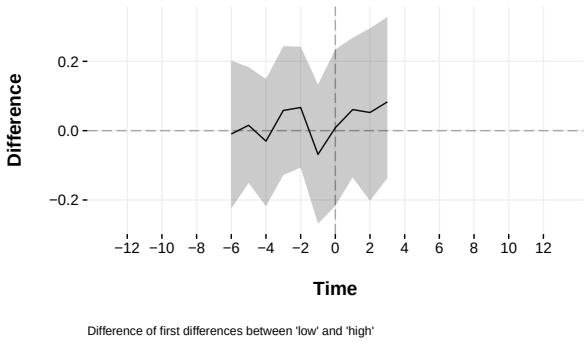
Figure S8: Delinquency Difference Treatment Effect for Men by Household Income Measured at Wave I, Models Controlling for Age-at-first Pregnancy

z

(a) Delinquency



(b) Marijuana



(c) Binge Drinking