Spillover effects within families: Evidence from teenage motherhood and sibling academic performance[*]

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Abstract

The United States has the highest teen birth rate of any industrialized country. The primary contribution of the present paper involves whether there are spillover effects of teen motherhood on the mothers' siblings. Using annual longitudinal data, I show that all children in families with teen childbearing are on a downward trajectory well before pregnancy. I use several synthetic control methods to show that methods that compare students based on first-observed characteristics, rather than trajectories, systematically overestimate the effects and spillover of teen pregnancy.

When compared to students on a similar trajectory in families without teenage childbearing, the siblings of teen mothers have worse test scores, higher high school dropout rates, lower college attendance, and lower college graduation. The change in test score outcomes only occurs after the baby is born, indicating that it is the appearance of the newborn that affects performance, rather than some unobserved occurrence that leads to both teen pregnancy and poor outcomes. Sisters of teen mothers have larger academic effects than brothers; brothers have larger juvenile justice effects. A similar analysis provides a maximum bound for the effects of teen pregnancy on mothers' outcomes, finding lower test scores, more grade repetition, higher high school dropout, less college-going, and less college graduation, relative to females on a similar downward trajectory from families without teenage childbearing. For mothers, the divergence in scores begins in the year of the pregnancy, not the year of birth.

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Siblings share (limited) parental resources, the same neighborhood environments, and similar genetics, and it seems probable that an unexpected change in one sibling would change the outcomes of children living under the same roof. However, given their shared context it is difficult to analyze the effect of one sibling on another, and little is known about how a negative shock to one sibling affects the rest of the family, particularly in older children (Black et al., 2016; Breining, 2014; Breining, Daysal, Simonsen, & Trandafir, 2015; Nicoletti & Rabe, 2014; Yi, Heckman, Zhang, & Conti, 2015). One presumably large shock that directly affects one child and may have ripple effects in the family is the birth of a child to a teenage mother. I use novel longitudinal data to study how teenage pregnancy changes siblings' outcomes following the birth, relative to students who did not have a teen birth in the family.

While there is an expansive literature on the effects of teenage motherhood on the mother's own outcomes, there is little research on what happens to the rest of the mother's family.[1] Understanding the full consequences of teenage motherhood matters for policymakers in the United States, which has the highest birth rate among teenagers of any industrialized country (Kearney & Levine, 2012). Adding a newborn to the home might have profound effects on the whole family, including family conflict, additional responsibilities, and loss of sleep (see, e.g.,

[1] Prior research has reached conflicting conclusions about whether teenage pregnancy causes poor outcomes for the teen mother herself or whether it is a symptom of prior trends. Teenage parenthood is popularly understood to be a negative outcome for the mother, including reduced education attainment and worse long-term economic prospects (Kane, Morgan, Harris, & Guilkey, 2013; A. R. Miller, 2009). However, many such studies do not account for the negative selection into pregnancy and, among those who get pregnant, the positive selection into abortion (Ashcraft, Fernández-Val, & Lang, 2013; Fletcher, 2011; Geronimus & Korenman, 1992; Hotz, McElroy, & Sanders, 2005; Hotz, Mullin, & Sanders, 1997). In other words, the type of women who become teenage mothers may have limited economic opportunities, and even if they had delayed pregnancy they likely would have had poor outcomes relative to other women. Instead, many of these studies use miscarriage as an instrumental variable to estimate the effects of teen motherhood, generally finding null or small effects (Ashcraft et al., 2013; Hotz et al., 2005, 1997). Recent work has argued that miscarriage is correlated with community-level factors, and after accounting for this correlation teenage pregnancy in the U.S. does indeed reduce high school graduation rates and annual income (Fletcher & Wolfe, 2009).

Meltzer & Montgomery-Downs, 2011). The grandparents of this child often take on child care responsibilities, which can take away their time to work outside the household, increase their stress levels, and potentially take away the time available for their other children (Bailey, Haynes, & Letiecq, 2013; Chase-Lansdale, Gordon, Coley, Wakschlag, & Brooks-Gunn, 1999; East, 1998). After the baby's birth, new grandmothers monitor and communicate less with their non-parenting children (East, 1999), perhaps allowing the siblings to make choices that harm their human capital development.

However, to date there has been almost no research on the effects of teen motherhood on the outcomes of other children in the family.[2] The only prior work along these lines pertains to sibling fertility: the siblings of teenage mothers are more likely to become teenage parents themselves, particularly when siblings are close in age and in low-income households (Anand & Kahn, 2013; Monstad, Propper, & Salvanes, 2011).[3] This paper represents the first research, to my knowledge, that studies the effects of teen motherhood on their siblings' human capital development. I make use of detailed longitudinal student data from a large anonymous Florida county-level school district linked to postsecondary outcome data from the National Student Clearinghouse to study the effects of teen motherhood on siblings' test scores, dropout likelihood, juvenile justice participation, college attendance, and college completion.

The primary causal identification problem is that teen pregnancy is generally not an exogenous event for the family, and the pregnancy itself may be a symptom of family conflict and disruption (Ellis et al., 2003; B. C. Miller, Benson, & Galbraith, 2001). Thus, teenage

[2] There is evidence that siblings can affect each other's educational outcomes in other contexts. Younger siblings with disabilities or health problems can negatively affect their older siblings' educational outcomes (Black et al., 2016; Breining, 2014; Breining, Daysal, Simonsen, & Trandafir, 2015), while higher-achieving older siblings can also positively affect their younger brothers and sisters (Joensen & Nielsen, 2015; Nicoletti & Rabe, 2014).
[3] While Kearney and Levine (2015) show that the MTV show *16 and Pregnant* led to a decrease in teen pregnancy, thanks in part to the demonstration effect of the ordeals of teen pregnancy and parenthood, it is clear from this other research that other teenagers in the household are responding to a variety of signals.

mothers may be on a downward trajectory well before the birth. Indeed, I demonstrate that both teenage mothers and their siblings have falling test scores for several years before the birth of the child. Unless researchers account for these underlying trends, the negative estimated consequences of a birth in the family may reflect these unobserved family factors rather than the spillover effects of teen motherhood per se. Many identification strategies that work for studying teen fertility (e.g., Buckles & Hungerman's (2016) study of the effects of condom distribution programs on teen fertility) cannot help to disentangle the spillover consequences of teen pregnancy, especially given that siblings tend to be relatively closely spaced. In this paper, I make use of the longitudinal nature of the school district data, in which children are observed annually throughout their schooling years, to conduct an event study analysis, and match children in families experiencing a teen pregnancy event to observationally equivalent children who were on the same trajectories, in terms of test scores, in the years leading up to their sisters' pregnancies.

I show that, in terms of test scores, siblings of teen mothers and their matched comparators are on a similar downward trajectory for several years prior to the commencement of their sisters' pregnancies. However, the siblings of teen mothers diverge after the birth. For the siblings of teen mothers, there is a marked relative decrease in test scores of about 2.0 to 3.3 percentile points following the birth, relative to those on a similar trajectory before birth. Among the sisters of teen mothers, dropout is 6.9 percentage points more likely; the effect is null for brothers. Among brothers, the chance of encountering the juvenile justice system increases by 7.1 percentage points after the birth. In the longer term, attending any college drops by 10.7 points, four-year college-going drops by 9.0 percentage points, and obtaining a degree drops by 5.8 percentage points, on average.

While not the primary contribution of this paper, I am also able to apply the same analytical strategy to add to the literature on the effect of teen pregnancy on mothers' own outcomes, though remaining selection issues are arguably more important in the own-outcomes case than they are in the sibling spillover case. Relative to female students who had been on a similar trajectory, following the birth the teen mothers display a marked decrease in test scores, an increase in grade repetition and high school dropout, and a decrease in college attendance and college graduation. Unlike their siblings, whose test scores begin to drop relative to matched comparators after the birth of the new child, teen mothers' relative test scores begin to drop in the year prior to the arrival of the baby.

The present analysis provides important evidence on the family-wide effects of teen pregnancy, as well as evidence of the important role that siblings can have on each other. Current estimates of the costs of teen childbearing that do not account for siblings may underestimate the true value of successful intervention programs, and the role of siblings also matters for researchers considering using sibling comparisons to study the effects of policies.

Data

Data come from an anonymous large Florida school district's administrative files for the 1989-1990 through 2004-2005 school years (henceforth, 1990 through 2005). Data are limited to one large county in Florida for students in families with at least two siblings, defined as co-resident children who share the same last name. The year of birth for the students range from 1974 to 1993.

I identify teen mothers in two ways. First, in the 2005 school year the county identified the school ID of parents if they also attended school in the district. The mother's school data is then connected to the child's date of birth, which is used to calculate the mother's age at birth.

Mothers are identified with this first method even if they dropped out of the public school system, but if their children enrolled in school in 2005. This method does not identify most births before 1989, because most would have graduated by 2005. The youngest children identified by this method were born in 2003, as they had entered a public pre-K program by 2005. Second, until 2003 the county identified when students become mothers, as long as they remained enrolled in public school in the county. This second method misses any teenage mothers who dropped out of school. Data were not reported for 2002, but limiting the analysis to mothers identified in 2001 and prior does not change the results. Combined, these methods identify those who became teen mothers until 2003, though the 2004-2005 data are retained to examine outcomes after the birth. I can combine the data in multiple ways: method 1 only, method 2 only, privileging the information in method 1 over method 2 (as in the main analysis), or privileging the information in method 2 over 1. Choice of method does not substantively change the results.

Last names and shared address at first observation identify siblings. The year of entry into teen motherhood is also the year that teen mothers' siblings became the aunts/uncles to teenage-parented children.[4] It is both the teen mothers and the younger siblings of teenage mothers who comprise my main groups of interest in the present study. Throughout the paper, I refer to the children of teenage parents as children and the siblings of teenage mothers (who are the aunts and uncles of the children) as siblings. Grandparents are the parents of the teenage mother. Students include teenage mothers, their siblings, and their classmates; the present analysis does not focus on the academic outcomes of the children of teenage mothers.

[4] The test score data used in this analysis occurs annually, allowing a year-by-year comparison of outcomes as teenage mothers move through the pre-period, pregnancy, birth, and a post-period. Year t=0 is the first academic year that the baby appears in the home – but a portion of that year also occurs before the birth, when the teen mother-to-be is still pregnant. Similarly, t=-1 may contain almost all of the pregnancy (if the birth occurs at the beginning of the year) or none of it (if the birth occurs at the end of the year and year t=0 contains all of the pregnancy).

The identification of teenage mothers means that I under-identify teenage mothers (and their siblings) if a teen mother both dropped out of public school *and* her child did not attend the same school district – or if she both dropped out *and* her child attended the same school district but not in 2005. The population of interest may then be positively selected (if it misses teenage mothers who drop out or if the child attending an alternative school district implies negative selection) or negatively selected (if the child attending an alternative school district implies positive selection). Under-identification of the teen mothers could attenuate the results, leading to an under-estimate of the true effect size. Under-identification may also affect external validity. However, both methods of identifying mothers produce similar results, increasing confidence in the estimates.

There are several outcomes of interest. The first, most immediate outcome is the nationally norm-referenced individual-level scores on the annual California Test of Basic Skills (CTBS) and later the Stanford Achievement Test in math and reading. Tested grades differed by year and ranged from grades 1 through 10. Students took the tests in the spring of grades 3 through 8 in all years, and testing also occurred in grade 1 in 1990, grade 2 in 1990-1992, and grades 9-10 in 2000-2005. The data are reported on a 1-100 scale, representing the student's rank in the national distribution of test scores in each subject.[5] About 4.8% of student-test years are missing test data that ranks them on a national percentile scale, but do have data from the Florida Comprehensive Assessment Test (FCAT). For each grade, and subject (math and reading) I impute national percentile ranking in the cases in which these national rankings are missing by regressing, for the cases in which I observe both tests, the national percentile rank on a cubic

[5] There are also some students in Grade 11 in the data. Tests are given by student grade, not test grade, and some eleventh graders may have been required to retake the test. The analyses limits data to grades where policy indicated students should be tested, which in practice means dropping some scores in grades 1-2 during untested years. Figure 7 in the Appendix displays the distribution of ages of available test data of teenage mothers and their siblings, before and after birth.

function of the FCAT. I then use these estimates to predict the estimated national percentile rank for those years missing data. That said, excluding these imputed years does not change the magnitude of the estimates. For brevity, I combine the math and reading scores to estimate the average percentile rank for each student in each year. Analyzing the data separately generally produces smaller results in reading and larger estimates in math, with both estimates going in the same direction as in the combined estimates.

The analysis also examines longer-term outcomes, including whether the student repeats a grade in at least one of the years following the birth, whether the student drops out of school after the birth, and whether the student first encounters the juvenile justice system after the birth.[6] Testing did not occur in every grade, so the number of observations is lower in the test score analysis than in the longer-term outcomes, particularly among mothers who are often older than tenth grade when they give birth. The National Student Clearinghouse (NSC) provides college-going data for the subsample of students expected to be in college 1997-2006 (about 60% of the data). Of these, about 38% did not appear in the college-going data, 59% are in the NSC data, and 2% blocked detailed reporting. I create four indicator variables using this data: ever attended any college, ever attended a four-year college, obtained any degree or certificate, and obtained a four-year degree.

Table 1 presents descriptive statistics. Relative to families without a teenage mother, both teenage mothers and their siblings are more likely to be eligible for the free- and reduced-price lunch (FRL) program, identify as black, have lower first-observed test scores, and attend schools

[6] Grade repetition is equal to one if the prior year grade equals the current grade. Dropout is equal to one if the student does not appear in a grade when they would be expected to appear. Juvenile justice exposure equals one if the district data indicated the student was sent through the county juvenile justice system.

with a higher proportion of these characteristics in their first observation in the data.[7] The average teen mother gave birth to the child at age 16.8, and her younger siblings were on average 13.2 at the time. All mothers are female, of course, and only younger siblings are included in the sibling analysis.

Family Trajectories

Kearney and Levine (2012) argue that teenage childbearing is a consequence of low economic prospects. Such prospects are not stationary over years, and this section investigates whether families that eventually have a teenage birth are on a stable or a downward trajectory prior to the birth. This distinction matters in teenage pregnancy analysis: if the families are on a downward trajectory then a simple difference-and-difference approach would overstate the effect of teenage pregnancy on teenage mothers and their siblings.

Figure 1 displays the trajectories of teen mothers and their siblings with age and individual fixed effects. Students from non-childbearing families are included to estimate the age fixed effects, which should capture any county-wide patterns in test scores.[8] The figure shows that, after controlling for typical scores at a given age, there is a strong downward pattern in both teen mothers and their siblings. The scores begin dropping well before the year of the birth (t=0) or even pregnancy (around t=-1). Note that t=-5 should be taken with some caution for siblings, because many siblings were not old enough to take a test in that year (see Figure 2, which

[7] First observation is the first test score observation in the data, which may be different grades for different students. The modal grade is grade 3.

[8] The analysis estimates the following model for both mothers and their siblings:

$$Y_{it} = \beta_0 + \beta_{1,t} \sum_{-5}^{-1} Birth_{\mathrm{year}_t=\mathrm{t}} + \gamma_{\mathrm{i}} + \mu_t + \varepsilon_{it}$$

where Y_{it} is the test score for individual i in year t, $Birth_{year\ t}$ is an indicator variable for each year relative to the birth of the first child in the family (t=-5 to -1, with t= 0 being the year of birth), γ_i is an individual fixed effect that controls for anything that is constant *within that individual* over the period, and μ_t is an age fixed effect that accounts for any constant differences in outcomes by age in the county. All students without a teen birth in the family form the control group in this analysis. Because there are student fixed effects, the purpose of these control students is to estimate the age fixed effects, as $Birth_{year\ t}$ =0 for all control observations. Standard errors are clustered by family.

displays the percent of observations with test score data by year relative to birth). Conducting the analysis for years -4 to -1 does not change the overall results, and Table 7 in the Appendix shows that excluding age fixed effects or adding school fixed effects does not change this pattern. Overall, the figure provides support for the theory that families that will eventually experience a teen pregnancy differ from families without teen pregnancy, where scores tend to be relatively flat over time, on average.

Analytic Method

Research on teen pregnancy often compares teenage mothers to girls in families without a teenage mother. Selection into teen pregnancy means that such research may not provide reliable results: the mothers and siblings from the sorts of families likely to contain teen mothers are disadvantaged relative to students from the sorts of families unlikely to contain teen mothers, even without a baby in the home. If research does not account for pre-existing trends, it can falsely create the appearance of a causal effect of the birth. In other words, some other change may have led to both the teenage motherhood and lower outcomes in the mother and her siblings. For instance, consider a family without obvious problems and typical academic achievement in Year 1. Perhaps job loss caused intra-family conflict in Year 2, leading to a drop in academic performance. By Year 3 one of the sisters had also given birth as a teenage mother, as her siblings continued to struggle in school. Comparing the average outcomes in Year 1 and 2 (before the birth) to Year 3 (after the birth) would falsely attribute the drop in *average* scores to the birth, when really it was the job loss and family conflict that led to worsening performance before the birth occurred.

The primary contribution of this paper tests whether teen birth changes the trajectory of the siblings of teen mothers. I create several synthetic control groups to estimate whether observably similar students diverge after the eventual teen mother gives birth. The first set of control analyses use the student's age; family size (as measured by the number of siblings in the data); first-observed individual test score; first-observed school characteristics for percent black, percent FRL, and first test score; and first-observed indicators for identification as female, FRL, and black.[9] The siblings and their matched controls are not allowed to be the oldest siblings in the family, both because theoretically there is a stronger influence from older to younger siblings (Anand & Kahn, 2013; Monstad et al., 2011) and because practically older siblings are unlikely to have the necessary test score data in the years leading up to the birth. Each sibling of a teen mother is only included once in the creation of the synthetic control; age is entered as her age at t=-1 (one year before birth). Each sibling is matched to five control students, who each receive a weight of 0.2 per match. Potential synthetic controls are included in each available year. The process allows the same control students to be used more than once for different siblings, possibly at different ages, but each control can only be matched to a given sibling at one age.[10]

Some of these variables (e.g., whether a student identifies as black) are fairly stable over time, while some of them (e.g., test scores) vary. The control group produced by this procedure is referred to as the first-observed synthetic control. An additional condition in some analyses requires that the synthetic control be from the same micro-neighborhood as the mothers. Micro-

[9] Data on Hispanic students are not included as it could reveal the anonymous county. School average first-observed test scores provides an estimate of the mean first-observed test score in the first grade observed for the students (with a modal of grade 3) in the school, regardless of the level of the school. For instance, a higher mean first-observed test score in a middle school indicates that the students in the middle school had high scores in their first test in elementary school.

[10] Specifically, I use a logit model to predict the probability of being the sibling of a teenage mother based on these characteristics. The results of this analysis are available in Table 8 in the Appendix. I remove variables to minimize the Akaike information criterion (AIC); this is the model I use in subsequent analysis. Based on this probability, I select the five nearest synthetic control matches for each sibling of a teen mother, allowing the same synthetic controls to be used for different siblings at various ages, though each control can only be matched to a given sibling at one age. This requirement prevents a control very similar to a given sibling from being matched to the sibling at ages 13 and 14, for instance.

neighborhoods are small areas, similar to block groups, identified by the county in the data. These are small areas, with an average of 103 students per neighborhood per year.

In addition to the first-observed synthetic control, a trajectory synthetic control adds three-year trends to the algorithm. In both the teen mother and sibling analysis, the synthetic control analysis uses the prior years that have over half of the available data. For the siblings of teen mothers, a majority of siblings have pre-birth test scores in years *t*=-4, -3, -2, and -1 (see Figure 2).[11] However, in theory the pregnancy itself in year t=-1 may affect outcomes, so the trajectory matching is limited to years *t*=-4, -3, and -2. This analysis produces three trajectory synthetic control groups: scores only, scores plus observable controls, and scores plus controls plus a requirement that the synthetic control be from the same micro-neighborhood as the siblings. Table 9 in the Appendix displays the results of the logit models.[12]

Table 2 displays descriptive statistics for the siblings of teenage mothers and these synthetic control groups. The means for the siblings in Column 1 differ slightly from the previous table because this table adds the requirement that the siblings have at least two of the prior three test scores used in the synthetic matching procedures (the mean is 2.1 out of 3 observations; requiring all three prior observations increases the standard error but does not change the magnitude or direction of the results). Most characteristics of the five synthetic control types are similar to the siblings of teen mothers in Column 1, but the prior test scores

[11] Because births to teen mothers can only be identified until 2003, and because matching occurs up to year *t*=-2, matches must occur in 2001 or prior. Students must have at least two of the three years of prior data. For years missing data, the closest available prior year approximates the missing data. Missing data are replaced only in the creation of the synthetic controls. Indicators for missing data in each year are included in the logit model. First-observed synthetic controls must also have at least two of the prior three years of data to make the results comparable. Similar analysis by alternative timeframes or more or fewer years of data required yield qualitatively similar results. Figure 2 shows how many years are missing for each year relative to birth for teen mothers and their siblings.

[12] The preferred trajectory model predicts that siblings of teen mothers are more likely to be female, older, come from larger families, identify as FRL, identify as black, and attend schools with more FRL students, holding other factors constant. The siblings of teen mothers are also less likely to be missing prior test score data, though both the siblings and the potential controls are required to have at least two of the three prior scores.

highlight the difference between the first-observed (Columns 2-3) and trajectory methods (Columns 4-6). Examining the columns with scores and controls (Columns 2 and 5), both methods produce control groups that are similar to the siblings at t=-4. At t=-2, however, the siblings and trajectory controls have lower scores than at t=-4, while the first-observed matches remain higher.

The final column requires the trajectory controls to be from the same mirco-neighborhood as the teen mothers at first observation. Such a requirement may make unobservable local conditions equal between the siblings and controls, which should reduce unobserved bias (Cook, Shadish, & Wong, 2008). Indeed, Fletcher and Wolfe (2009), show that it is necessary to account for community factors in the research designs that rely on miscarriages to create variation in teen parenthood, implying that neighborhoods can affect pregnancy outcomes in ways not picked up by other control variables. However, these local neighborhoods are small, and it could in theory be difficult to perfectly match the students on the observable differences within the pool of potential controls from the same neighborhood. In this final column the synthetic controls are older at t=0, from slightly smaller families, and from schools with slightly fewer black students and higher first-observed test scores. In the second-to-last column, the synthetic controls without the neighborhood requirement are more likely to be older, female, and from schools with slightly fewer black students and higher first-observed test scores. The main analysis presents results from multiple synthetic control groups to show how different groups change the estimates.

Using these synthetic control groups, the main analysis examines several outcomes of interest. Most of the analysis uses ordinary least squares (OLS) regression, as follows:

(1) $$Y_i = \beta_0 + \beta_1 TeenMomSibling_i + X_{\mathrm{i}}\alpha + \varepsilon_i$$

where $TeenMomSibling_i$ is an indicator variable equal to one if the student is the sibling of a teen mom, X_i includes the characteristics used in the matching procedure from above, and ε_i is an error term with a mean of zero. The outcomes Y_i are observed once in the data (after the birth). For the siblings of teen mothers, the outcomes examined are the test score in the year of the birth (at t=0), tenth grade test score, whether the student repeats a grade in at least one of the years following the birth, whether the student drops out after the birth, whether the student encounters the juvenile justice system after the birth, college-going, and whether the student obtains a college degree. An additional analysis includes all years of test score data, which allows the inclusion of age and individual fixed effects, as follows:

(2) $$Y_{it} = \beta_0 + \beta_1 TeenMomSibling_i \mathrm{\ X\ } PostBirth_t + \gamma_{\mathrm{i}} + \mu_t + \varepsilon_{it}$$

where $TeenMomSibling_i \mathrm{X} PostBirth_t$ is equal to one after the birth for the siblings of teen mothers, γ_i is an individual fixed effect, and μ_t is an age fixed effect. Thus, β_1 provides an estimate of whether the siblings of teen mothers' average scores diverge from what would be expected from a similar control student of the same age, after accounting for individual fixed effects. The age fixed effects capture year-over-year patterns in the population as a whole, so it is not necessary to include $PostBirth_i$ on its own.

Results

The analysis begins by examining whether there is negative spillover from the teen birth to the siblings of the teen mother. That is, given that students are on a similar trajectory, do the paths of the siblings of teenage mothers diverge after the birth? Figure 3 displays the test score patterns for siblings, their first-observed controls, and their trajectory controls. The first panel does not require the synthetic controls to be from the same neighborhood at first observation; the second panel does. Each line displays the coefficient of a regression of national percentile rank on years relative to birth (*t*=-4 through *t*=+1) within the noted population, holding individual fixed effects constant. The vertical line marks the end of the years used in the trajectory matches. To be included in the graph, the students had to have test scores observed both before the pregnancy and after the birth. The solid black (sibling) and gray (trajectory match) lines summarize the analytic strategy. While the lines move together in the four years before birth, there is a divergence after the birth, with the siblings continuing their decline in test scores and the controls leveling out.

Figure 3 also highlights the importance of using trajectory matches. After the birth at *t*=0, estimated difference between siblings and the first-observed matches are larger than the gap between siblings and trajectory matches, but this divergence began well before the birth and cannot be caused by the new baby in the home. Overall, this figure provides evidence that there may be some negative spillover from teen motherhood, but it is less than what would be estimated from an analysis that did not account for prior trends.

The figure also displays how requiring the synthetic controls to be from the same neighborhood changes the estimates. This requirement limits the number of potential matches, which leads to matches that are slightly more erratic and less similar to the siblings on this observable characteristic. However, they may be more similar on unobservable characteristics, and the post-pregnancy gap between the synthetic control and the siblings appears to be smaller in the second figure, particularly for the first-observed match.

More formally, Table 3 shows the estimated differences in outcomes between siblings and various synthetic control options. Column 1 uses all children from non-childbearing families, while Column 2 uses the baseline match with neighborhood requirement. Column 3 is the trajectory match based only on the test scores, Column 4 adds additional observable controls, and the preferred estimate in Column 5 adds the neighborhood requirement. In the high school outcomes, the estimates in first two columns are generally larger than those in the final three columns that use the trajectory-based synthetic control groups. For the test score in the year immediately following birth, the first column indicates that siblings score 5.1 percentage points lower, relative to all students (in all years) holding first-observed test score, demographics, and school characteristics constant (*p*-value<0.001). However, this estimate is likely biased, as from the figures above it is clear that the families that eventually experience teen pregnancy are on a downward trajectory well before the birth. In the final two columns, the estimate is -3.0 percentile points for the trajectory plus controls analysis (*p*-value<0.05) and -3.3 percentile points in the trajectory plus neighborhood requirement analysis (*p*-value<0.01). The coefficient in Column 5 is 36% smaller than the coefficient in Column 1. Note that this drop off is relative to a mean score at the 44^{th} percentile for the preferred control group (see Table 11 in the Appendix for the outcome means for the treatment and various control groups).

The finding of lower test scores appears to be limited to the top of the test-score distribution. The first panel of Figure 4 displays the distribution of test scores following the birth for the siblings and their trajectory-based synthetic controls. The figure shows that the top of the distribution of test scores shifts to the left in the year of the birth, relative to students who were on a similar trajectory in the years leading up to the birth. The gap in the vertical lines is the unadjusted mean difference between these groups.

There are similar results when only examining tenth grade scores, which are statistically significant in the first two columns (with coefficients of -3.9 and -5.6 percentile points, respectively) but smaller and not consistently statistically significant in the final three columns (with coefficients ranging from –0.5 to -2.9 percentile points). The fixed effect estimate in Row 3 can be interpreted as the average decrease in scores post-birth within siblings relative to their pre-birth average scores, holding individual and age effects constant, with the various control groups contributing to the age coefficients. The estimated coefficients are again slightly larger for the models that do not account for trajectories, with an estimate of -2.7 percentile points (p-value<0.001) in the first column compared to -2.0 percentile points (p-value<0.05) in the final column.

Rows 4-6 examine other high school outcomes. The magnitude of the coefficient for grade repetition declines across the columns, with no statistical effect by the final column. There is an increase in the probability of dropout (5.0 percentage points, p-value<0.05) and entering the juvenile justice system (4.9 percentage points, p-value<0.05) in the final preferred column. For perspective, the control group has a mean dropout rate of 13.3% and a mean juvenile justice participation rate of 7.8% after the match.

Finally, rows 7-10 examine post-secondary outcomes. The magnitudes of these results only slightly change when moving across the columns, and indicate that that having a sister who gives birth decreases the probability of both going to college and obtaining a degree. The probability of ever attending any college decreases by 10.7 points (p-value<0.01), of ever attending a four-year college decreases by 9.0 percentage points (p-value<0.05), of obtaining any sort of degree or certification decreases by 5.8 percentage points (p-value<0.10), and obtaining a four-year degree decreases by 7.6 percentage points (p-value<0.01). For reference, in the control population 55% attend any college, 51% attend a four-year college, 26% obtain any degree or certificate, and 18% obtain a four-year degree.[13]

Patterns for Siblings by Sex, Race, and FRL Status

An additional concern with the results might be that they are driven by a particular subgroup; for instance, perhaps lower-income mothers have fewer supports. Alternatively, perhaps low-income and black communities, which have a higher prevalence of multi-generational families, offer supports to handle teen childbearing (Burton, 1999; Fuller-Thomson, Minkler, & Driver, 1997). Sisters may be expected to help with children more than brothers (East, 1998), and a reduction in monitoring by the new grandparents (East, 1999) may affect the sexes differently. This section explores patterns by sex, race (black versus non-black), and FRL status (see Table 4). To be conservative, these estimates include the neighborhood requirement, as this requirement may account for unobserved neighborhood differences. The overall estimate from the previous table is included in Column 1 for reference.

Sisters consistently have statistically significant test score effects, while the estimates for brothers are indistinguishable from zero. By tenth grade, the estimates effect is -3.6 for sisters and -1.2 for bothers (*p*-value of difference between estimates=0.031). For grade repetition, girls experience larger effects than boys (5.3 versus -4.0 percentage points, *p*-value=0.097), though the estimates themselves do not differ from zero. Conversely, boys whose sisters give birth are 7.1 percentage points more likely to enter the juvenile justice system after the birth, compared to boys on a similar trajectory pre-birth (*p*-value<0.05). There is no effect for girls, and the difference between the estimates is statistically significant (*p*-value of difference of estimates=0.042). Note that these changes occur on different margins, as 11.0% of boys in the synthetic control group are ever exposed to this system, compared to 6.1% of girls. In the longer

[13] Note that attending a four-year college does not necessarily mean that a student is pursuing a bachelor's degree.

term, girls experience larger effects on obtaining any degree or certificate than boys (-11.0 versus 0.3 percentage points, *p*-value of difference=0.075). The effects on other post-secondary outcomes are statistically about the same for the sisters and brothers of teen mothers, though if anything the effects are larger for girls.

The estimated effects on high school outcomes are similar in the black versus non-black comparison. For post-secondary outcomes, the college-going effects are larger for the black students, though the difference is only statistically significant for four-year college-going (-13.4 versus -1.3 percentage points, *p*-value of difference=0.034).

Finally, estimated effects on test scores are also indistinguishable in the FRL versus non-FRL comparison in the year immediately following the birth, grade repetition, dropout, and post-secondary outcomes. The non-FRL students have worse outcomes than the FRL students in tenth grade (-8.9 for non-FRL versus -0.01 percentage points for FRL students, *p*-value of difference=0.031).

Falsification Tests

The biggest concern in the present analysis is that some external event led to both the pregnancy and a downward drop in scores in scores for the whole family. While the pregnancy itself is likely to affect the teen mother, it will not necessarily affect the siblings. Thus, one way to test whether some external event might have led to the drop in scores involves testing whether test scores drop in t=-1 (in the time leading up to and during pregnancy) or only after the baby appears in t=0.

A formal test for this runs the fixed effects analysis from above by year relative to birth and interacts each year with an indicator for being a sibling (with age fixed effects to match the previous analysis). There is no statistical difference in the interaction between sibling and the years leading up to year *t*=-2. This confirms the efficacy of the match. In *t*=-1, which is not restricted to be the same between the siblings and their synthetic controls. The coefficient on the interaction term is small (-0.447 percentile points) and does not statistically differ from zero. This indicates that the divergence between siblings and their synthetic controls only occurs after the appearance of a child in year *t*=0.

Given that the divergence only occurs in *t*=0, it is also possible to estimate the matching years by different timeframes. Figure 5 displays alternative trajectory specifications. Adding a year (*t*=-4 to -1) or shifting the match by a year (*t*=-3 to -1) in the trajectory procedure does not change the overall pattern.

Teen Mother Analysis

A complementary analysis in this paper uses a similar strategy to examine whether the teen birth also changes the trajectories of mothers. The teen mother estimates are interesting by themselves, but they also provide a useful check on the causality of the sibling analysis. If it is family disruption caused by the appearance of a child that changes the trajectories of the siblings of teen mothers, we would expect that pregnancy itself might affect the mothers but not the siblings. Moreover, if a divergence occurs at different times, it is unlikely that some common external event led to pregnancy and drops in scores in the family overall.

The variables used in the teen mother analysis are mostly the same as in the sibling analysis, though all synthetic controls for the teen mother analysis are also required to be female. The trajectory patterns are based on test scores in years *t*=-5, -4, -3, and -2, as these occur before pregnancy and at least 50% of the teen mothers have this data in each year (see Figure 2). This differs slightly from the previous analysis because the siblings are younger than the teen mothers, and the latter thus have more pre-trend (but less post-birth) data. Table 9 in the Appendix displays logit models predicting the probability of becoming a teen mother.[14]

Table 5 displays descriptive statistics for the teen mothers and their synthetic control groups. Most of the five synthetic control types are quite similar to the teen mothers, though the teen mothers are somewhat more likely to be older and be the oldest sibling in the data. As in the sibling analysis, the prior test scores highlight the difference between the first-observed and trajectory methods. Both methods produces control groups that are similar to the teen mothers at t=-5. At t=-2, however, both the teen mothers and trajectory controls have lower scores than at t=-5, while the first-observed matches are higher. The preferred synthetic control is in the final column, which requires the controls to be from the neighborhood as the siblings. Again, this requirement may reduce unobserved bias (Cook et al., 2008), though it can also make matching on the smaller pool of comparison students more difficult, as evidenced by the slightly smaller number of synthetic controls in the final column, relative to the second-to-last column.

The families of teenage mothers are on a downward trajectory relative to average families (see Figure 1). However, the downward pattern exhibited by the teen mothers could change following the pregnancy or the birth of the child. Figure 6 displays the test score patterns for teen mothers, their first-observed matches, and their trajectory matches. The first panel does not require the synthetic controls to be from the same neighborhood at first observation; the second adds that requirement. Each line displays the coefficients from separate regressions of national percentile rank on years relative to birth (t=-5 through t=0) within the noted population, holding individual fixed effects constant. The vertical line marks the end of the years used in the trajectory matches. To be included in the graph, the students had to have test scores observed

[14] The preferred trajectory model predicts that teen mothers are more likely to be older, be the oldest sibling in the family, and identify as FRL and black. Teen mothers are also less likely to be missing prior test score data.

both before the pregnancy and after the birth. The solid black (teen mothers) and gray (trajectory controls) lines again summarize the analytic strategy. While the lines move together in the years used in the matching, there is a divergence during the year closest to the pregnancy (t=-1), with the teen mothers increasing their decline in test scores and the controls leveling out. The test scores remain low for teen mothers in t=0, the year of the birth.

As in the sibling analysis, requiring the synthetic controls to be from the same neighborhood results in fewer matches for the mothers. However, the students from the same neighborhood may be more similar on unobservable characteristics, and the figure with the neighborhood requirement has less divergence between the lines after the birth, though the lines are somewhat more erratic.

Table 6 examines the outcomes for the mothers. The test score estimates replicate what would be predicted from Figure 6. The naïve estimates in the first column indicate that teen mothers have tenth grade test scores 8.1 percentile points lower than all female students from non-childbearing families, after controlling for all observable characteristics from Table 1 (p-value<0.001). The control population is all female students from families without teen childbearing in this first column. The gap is somewhat smaller at 5.7 percentile points when the control population is limited to the synthetic control group based on first-observed characteristics (and still controlling for observable variables; p-value<0.01). However, these estimates are likely to be biased, as the analysis shows that the families that eventually have teen pregnancy are on a downward trajectory well before the birth. The estimate shrinks once the match is instead restricted to the trajectory-based synthetic control, which matches students who are on a similar downward trajectory. For the preferred estimate in Column 5, teen mothers achieve tenth grade scores 4.4 percentile points lower than students who do not give birth but who were previously on a similar trajectory (*p*-value<0.05).

Among teen mothers, the finding of lower test scores appears to be limited to the top of the test score distribution. The second panel of Figure 4 displays the distribution of test scores in the year following the birth, which indicates that the right tale of the test score distribution shifts to the left in the year of the birth, relative to students who were on a similar trajectory in the four years leading up to the pregnancy. The gap in the vertical lines is the unadjusted mean difference between these groups.

Rows 2 and 3 examine grade repetition and dropout. The estimates relative to the trajectory-based synthetic controls are about the same as the other estimates, but all methods indicate that teenage pregnancy is associated with large increases in the probability of repeating a grade and/or dropping out post-birth for teenage mothers. The probability of grade repetition is 17.2 percentage points higher relative to the preferred trajectory-based synthetic control group (*p*-value<0.001), while the probability of dropping out is 24.1 percentage points higher (*p*-value<0.001). There is a small decrease in the probability of exposure to the juvenile justice system in the naïve estimate, but this disappears in the synthetic control estimates.

Turning to the college-going data, mothers are much less likely to attend any college, relative to students on a similar trajectory without a birth in the family (-18.6 percentage points, *p*-value<0.001). Mothers are also less likely to attend a four-year college (-19.9 percentage points, *p*-value<0.001), obtain a degree (-12.5 percentage points, *p*-value<0.001), or obtain least a four-year degree (-11.7 percentage points, *p*-value<0.001).

Next, I test whether the divergence in scores between the teen mothers and their synthetic controls starts in tests taken before the birth in t=-1 or after the birth in t=0. As expected, the scores for the years used in the matching process are statistically indistinguishable. At t=-1, the siblings score 4.5 percentile points lower than the matched controls (p-value<0.01). In other words, mothers' scores drop in the year nearest to pregnancy, while the siblings' scores do not drop until the child appears in the home.

Overall, this analysis indicates that teen motherhood is associated with poor academic outcomes. For completeness, the appendix also conducts the analysis by subgroup for black, non-black, FRL, and non-FRL students. The effects are large and robust across subgroups in both the high school and college-going years.

Discussion

The analysis indicates that children in families where teen motherhood occurs have downward trajectories in test scores that began well before the pregnancy. These patterns do not occur in non-childbearing families, on average. However, not every family with a downward trajectory experiences teenage motherhood. When the siblings of teenage mothers are connected to students on a similar trajectory, there appears to be a negative spillover of the birth to the siblings of teenage mothers, with especially large decreases in test scores for sisters, increases in dropout for sisters, and exposure to the juvenile justice system for brothers. Similarly, the teen mothers are much more likely to repeat a grade and drop out after the birth, relative to those on a similar trajectory in families without teen births. This pattern occurs across various subgroups.

The analysis can be interpreted as causal if accounting for the several years of pre-trend data, plus other baseline characteristics, captures any differences between families with and without teen births. One concern could be that some unobserved event leads to teenagers giving birth. For instance, perhaps parental job loss increases the chance that older females have a child, and parental job loss is also associated with poor outcomes for all siblings. Then, the underlying cause of the poor outcomes would be the job loss, not the teen pregnancy itself. If the appearance of the child, and not general family trends, causes drops in performance, test scores should diverge from prior patterns in the year the child appears (and possibly continue into subsequent years), as seen in Figure 3.

The analysis provides support for interpreting the sibling analysis as causal. In the above example, the siblings and the synthetic controls continued on parallel paths in the year of the pregnancy; it is only after the birth that the paths diverge. If some external event led to both pregnancy and dropping scores in the family, then the drops should occur in the same year. Instead, the scores of teen mothers and their siblings change in different years, and the sibling scores do not change until the baby appears in the family.

Mothers, on the other hand, begin their drop on performance in year t=-1 (see Figure 6). Any unobserved differences between mothers and non-mothers are likely to be negatively associated with the outcomes of interest, and thus this analysis provides an upper bound on the negative effects of teen pregnancy on various outcomes. The estimates in the teen mother analysis are larger than prior literature that uses miscarriage as an instrument. To some extent, comparing completed pregnancies to miscarriages may understate the true effect of motherhood relative to no pregnancy, given that miscarriage is associated with long-term psychological repercussions such as elevated anxiety and depression (Hunfeld, Wladimiroff, & Passchier, 1997; Lok & Neugebauer, 2007; Ptettyman, Cordle, & Cook, 1993). My estimated effects for teen mothers are likely an upper bound on the true effect, but the 24.1 percentage point increase in dropout in my preferred estimate, for instance, is broadly consistent with the 8-9 percentage point decrease in diploma receipt found by Fletcher and Wolfe (2009) if we take that estimate as a minimum bound.

Conclusion

The present analysis uses novel data to study several important questions. First, I present new evidence regarding spillover effects that accounts for the fact that families of teen mothers were on a systematically different trajectory pre-birth. The mothers' sisters experience the largest academic effects in K-12, while the implications for brothers are larger in terms of juvenile justice exposure. Both have lower college-going. This provides important evidence that estimates of the cost of teen pregnancy that only focus on the teen mothers or their children will be understated. It also indicates that analyses using sibling fixed effects will understate the effects of teenage pregnancy.

Second, analysis that does not account for downward trajectories will overestimate the negative effects of teen pregnancy on the mothers. Still, after accounting for these patterns, this paper finds a decrease in academic performance and an increase in grade repetition and dropout for teen mothers.

Third, the analysis provides evidence that the families of eventual teen mothers are on a downward trajectory well before birth. If teen pregnancy is a symptom of family dynamics in struggling families, then public policy could focus its efforts on targeting and assisting students from these families. Programs already use individual warning signs for dropout prevention programs (e.g., Kennelly & Monrad, 2007), and teen pregnancy is associated with behavior also associated with dropout (e.g., poor academic performance, grade repetition). Perhaps programs could incorporate family-wide patterns to identify who to target when attempting to reduce pregnancy and dropout.

Not every pregnancy leads to childbirth. In future research, other types of data sources could shed light on whether the same patterns occur for pregnancies with and without associated births. The present analysis cannot identify the particular pathways through which siblings are affected by their sister's childbearing. However, given the difference in spillover effects by sex, it is possible that sisters are more likely to spend time on childcare for their new niece/nephew than are brothers. Previous research has indicated that teen pregnancy might be "contagious" among sisters (Anand & Kahn, 2013; Monstad et al., 2011). Diminishing academic performance caused by the sister's childbearing could also lead to increased chances of teen pregnancy.

Overall, the findings provide evidence that families that experience teen pregnancy are on a downward trajectory well before birth, but that teen pregnancy has short- and long-term negative effects on teen mothers and their siblings. Teen motherhood is more common among low-income and under-represented minority groups. Identifying family-wide early warning signs could lead to targeted public policy to improve outcomes for at-risk groups. The present research also indicates that current estimates understate of the true cost of teen pregnancy to families and society.

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Tables

Table 1: Descriptive Statistics

	(1) Families w/o teen birth, all	(2) Teen mothers	(3) Siblings of teen mothers
Female	48.475	100.000	50.312
	(49.977)	(0.000)	(50.051)
Age at birth	N/A	16.804	13.197
		(0.791)	(2.469)
# of siblings in data	2.385	2.581	3.029
	(0.701)	(0.879)	(1.056)
Oldest sibling	45.655	57.196	0.000
	(49.811)	(49.526)	(0.000)
FRL	35.827	74.393	78.586
	(47.949)	(43.687)	(41.065)
Black	12.162	57.757	56.757
	(32.684)	(49.441)	(49.593)
First-observed test score %	59.319	39.824	42.209
	(26.623)	(24.192)	(26.084)
School avg. FRL	38.050	47.438	47.463
	(17.650)	(15.522)	(15.253)
School avg. Black	15.306	18.942	21.090
	(10.448)	(8.503)	(12.234)
Mean school first-observed test %	58.605	54.303	53.535
	(9.185)	(7.996)	(8.879)
N	102700	535	481

Mean coefficients; SD in parentheses. Families without teen births include all children from non-teenage-childbearing families. Teen mothers include all teen mothers from families of two or more where the mother gives birth at age 15-17. Siblings include all younger siblings from families where an older sister gave birth at age 15-17.

Table 2: Descriptive statistics, synthetic controls for siblings

		First-observed match		Trajectory match		
	(1) Siblings of teen mothers	(2) Scores & controls	(3) Scores, controls & nbhd	(4) Score only	(5) Scores & controls	(6) Scores, controls & nbhd
Female	50.214	57.115	53.675	47.788	58.846	54.932
	(2.290)	(1.662)	(1.933)	(1.606)	(1.677)	(1.890)
p	.	0.015	0.248	0.386	0.002	0.112
Age at birth	13.150	14.432	14.016	15.012	14.455	14.172
	(0.123)	(0.063)	(0.065)	(0.062)	(0.058)	(0.066)
p	.	0.000	0.000	0.000	0.000	0.000
# of siblings in data	3.021	2.920	2.786	2.637	2.907	2.766
	(0.070)	(0.045)	(0.043)	(0.030)	(0.052)	(0.043)
p	.	0.223	0.004	0.000	0.187	0.002
Oldest sibling	0.000	0.000	0.000	0.000	0.000	0.000
	(.)	(.)	(.)	(.)	(.)	(.)
p	.	.	.	.	.	.
FRL	78.419	79.615	77.273	47.308	79.712	78.723
	(2.147)	(1.320)	(1.602)	(1.616)	(1.307)	(1.472)
p	.	0.635	0.669	0.000	0.607	0.907
Black	56.410	56.827	53.578	20.673	57.115	55.513
	(2.980)	(1.729)	(2.084)	(1.352)	(1.742)	(1.974)
p	.	0.904	0.436	0.000	0.838	0.802
Test score % (four years before match)	46.036	47.828	42.169	47.077	47.540	43.794
	(2.063)	(1.048)	(1.260)	(0.930)	(0.963)	(1.255)
p	.	0.438	0.110	0.645	0.508	0.353
Test score % (two years before match)	41.621	46.954	47.309	41.062	41.366	42.881
	(1.592)	(0.916)	(1.021)	(0.797)	(0.818)	(0.943)
p	.	0.004	0.003	0.754	0.887	0.495
School avg. FRL	47.516	47.457	47.106	43.510	47.821	47.257
	(0.869)	(0.562)	(0.694)	(0.559)	(0.542)	(0.640)
p	.	0.955	0.713	0.000	0.765	0.811
School avg. Black	21.036	18.722	19.161	16.953	19.102	19.153
	(0.702)	(0.276)	(0.323)	(0.289)	(0.275)	(0.305)
p	.	0.002	0.015	0.000	0.010	0.014
Mean school first-observed test %	53.518	55.149	55.265	56.569	54.957	55.195
	(0.492)	(0.286)	(0.334)	(0.266)	(0.268)	(0.309)
p	.	0.004	0.003	0.000	0.010	0.004
N	468	1014	926	1028	1022	939

Mean coefficients; SE in parentheses (clustered by family ID). Siblings include all younger siblings from families where a sister gave birth at age 15-17 who had at least two of three years of pre-data. First-observed matches include synthetic matches from non-teenage-childbearing families to siblings based on first-observed characteristics. Trajectory matches include synthetic matches from non-teenage-childbearing families to the siblings based on three-year test score trends and other observable characteristics. Includes *p*-value of *t*-test between matches and siblings.

Table 3: Estimated effects of teen birth on various outcomes for siblings

	(1) All controls	(2) First-observed scores, controls, & nbhd	(3) Trajectory scores only	(4) Trajectory scores & controls	(5) Trajectory scores, controls, & nbhd
Test scores at *t*=0	-5.129***	-5.828***	-2.528+	-3.028*	-3.264**
	(0.978)	(1.259)	(1.444)	(1.234)	(1.245)
N	356055	977	958	1038	968
10th grade test scores	-3.946*	-5.579**	-0.491	-1.159	-2.914+
	(1.546)	(1.722)	(1.901)	(1.666)	(1.671)
N	14093	501	547	575	500
Test scores, with age and individual FE	-2.746***	-2.423**	-1.762+	-1.878*	-1.988*
	(0.798)	(0.935)	(0.929)	(0.920)	(0.935)
Observations	406441	6765	7961	7774	6941
N	87012	1059	1212	1156	1074
Repeats grade in *t*=0 or later	5.107*	3.956	3.596	3.403	1.644
	(2.313)	(2.895)	(3.319)	(2.860)	(2.884)
N	86265	1368	1470	1464	1381
Drops out in *t*=0 or later	0.698	4.910*	7.482**	7.570**	5.019*
	(1.660)	(2.265)	(2.618)	(2.310)	(2.300)
N	85402	1368	1470	1464	1381
Juvenile justice in *t*=0 or later	6.141***	6.860***	8.167***	5.716**	4.868*
	(1.716)	(1.976)	(2.323)	(1.927)	(2.076)
N	85402	1368	1470	1464	1381
Ever attends any college	-13.640***	-15.674***	-13.163***	-10.366**	-10.681**
	(3.041)	(3.804)	(3.983)	(3.670)	(3.799)
N	44974	862	977	970	889
Ever attends a 4-year college	-11.790***	-14.747***	-13.342***	-9.610**	-9.013*
	(2.909)	(3.718)	(3.845)	(3.560)	(3.704)
N	44974	862	977	970	889
Obtains any degree or certificate	-5.895*	-4.764	-4.553	-5.803+	-5.819+
	(2.518)	(3.291)	(3.483)	(3.227)	(3.291)
N	44974	862	977	970	889
Obtains a 4-year degree	-6.114**	-7.079**	-4.711	-7.141**	-7.556**
	(1.945)	(2.661)	(2.926)	(2.559)	(2.665)
N	44974	862	977	970	889

Note: Standard errors clustered by family ID. Analysis includes all controls from Table 1. "All" analysis includes all children from non-teenage-childbearing families. First-observed matches includes synthetic matches to younger siblings from non-teenage-childbearing families to the siblings based on the first-observed characteristics; matches must be from the same neighborhood. Trajectory matches (scores only) includes synthetic matches to younger siblings from non-teenage-childbearing families to the siblings based on three -ear score trends. Trajectory matches (scores and controls) adds other observable control variables from Table 1. Trajectory matches (scores, controls, and neighborhood) add the requirement that the match must be from the same neighborhood at first observation. Scores at *t*=0, 10th grade test scores, repeats grade, drops out, and juvenile justice include one weighted observation from the siblings and their controls. "All" test scores at *t*=0 includes multiple observations per individual. Fixed effects models include all observations from the siblings and their matched controls.

Table 4: Estimated effects of teen birth on various outcomes for siblings by subgroups

	(1) Baseline	(2) Female	(3) Male	(4) Black	(5) Non-black	(6) FRL	(7) Non-FRL
Test scores at *t*=0	-3.264**	-4.229**	-1.622	-1.864	-4.216*	-2.813*	-4.506^{+}
	(1.245)	(1.610)	(1.937)	(1.597)	(1.913)	(1.373)	(2.679)
N	968	532	436	521	447	751	217
10^{th} grade test scores	-2.914^{+}	-3.607^{+}	-1.206	-3.035	-2.029	-0.012	-8.940*
	(1.671)	(2.120)	(2.432)	(2.076)	(2.465)	(1.812)	(3.534)
N	500	289	211	280	220	373	127
Test scores, with age and individual FE	-1.988*	-2.945*	-0.676	-2.264*	-1.421	-1.389	-4.553*
	(0.935)	(1.142)	(1.549)	(1.150)	(1.538)	(1.048)	(2.041)
Observations	6941	3821	3120	3816	3125	5350	1591
N	1074	584	490	581	493	826	248
Repeats grade in *t*=0 or later	1.644	5.336	-4.048	5.559	-3.632	0.901	2.353
	(2.884)	(3.827)	(4.359)	(4.129)	(4.111)	(3.382)	(5.399)
N	1381	698	613	742	569	1022	289
Drops out in *t*=0 or later	5.019*	6.929*	-1.265	4.617	1.424	3.388	5.392
	(2.300)	(3.231)	(3.271)	(3.046)	(3.357)	(2.690)	(4.528)
N	1381	698	613	742	569	1022	289
Juvenile justice in *t*=0 or later	4.868*	-0.165	7.051*	3.940	1.121	1.791	9.089*
	(2.076)	(2.208)	(3.240)	(2.610)	(2.718)	(2.312)	(3.831)
N	1381	698	613	742	569	1022	289
Ever attends any college	-10.681**	-12.627*	-7.648	-15.652**	-5.054	-9.129*	-13.381*
	(3.799)	(5.380)	(5.231)	(5.577)	(5.315)	(4.515)	(6.751)
N	889	474	415	480	409	674	215
Ever attends a 4-year college	-9.013*	-11.249*	-6.204	-16.390**	-1.272	-8.536^{+}	-8.124
	(3.704)	(5.398)	(4.913)	(5.318)	(5.265)	(4.384)	(6.592)
N	889	474	415	480	409	674	215
Obtains any degree or certificate	-5.819^{+}	-10.951*	0.270	-5.516	-5.623	-5.150	-6.187
	(3.291)	(4.590)	(4.437)	(4.349)	(4.886)	(3.533)	(7.100)
N	889	474	415	480	409	674	215
Obtains a 4-year degree	-7.556**	-10.265**	-3.991	-8.936**	-6.817^{+}	-6.234*	-10.334
	(2.665)	(3.635)	(3.649)	(3.049)	(4.086)	(2.698)	(6.662)
N	889	474	415	480	409	674	215

Note: Standard errors clustered by family ID. All analysis based on the trajectory matches based on prior test scores and other controls, and require that the matched control be from the same neighborhood. Baseline result is from the previous table; later columns conduct the analysis by subgroups.

Table 5: Descriptive statistics, synthetic controls for teen mothers

		First-observed match		Trajectory match		
	(1) Teen mothers	(2) Scores & controls	(3) Scores, controls & nbhd	(4) Score only	(5) Scores & controls	(6) Scores, controls & nbhd
Female	100.000	100.000	100.000	100.000	100.000	100.000
	(.)	(.)	(.)	(.)	(.)	(.)
p	.	.	.	.	.	.
Age at birth	16.803	16.711	16.078	15.994	16.701	16.129
	(0.035)	(0.050)	(0.062)	(0.049)	(0.047)	(0.057)
p	.	0.132	0.000	0.000	0.083	0.000
# of siblings in data	2.589	2.572	2.547	2.499	2.538	2.537
	(0.039)	(0.027)	(0.030)	(0.021)	(0.026)	(0.030)
p	.	0.724	0.402	0.046	0.283	0.297
Oldest sibling	57.744	51.667	50.027	44.792	50.833	50.294
	(2.162)	(1.351)	(1.380)	(1.236)	(1.314)	(1.383)
p	.	0.017	0.003	0.000	0.006	0.004
FRL	74.570	76.615	76.644	50.781	74.687	76.750
	(1.906)	(1.073)	(1.212)	(1.289)	(1.112)	(1.205)
p	.	0.353	0.362	0.000	0.958	0.337
Black	57.361	60.104	57.082	24.740	61.146	57.616
	(2.165)	(1.322)	(1.534)	(1.144)	(1.317)	(1.497)
p	.	0.286	0.917	0.000	0.141	0.924
Test score % (five years before match)	40.501	43.258	41.871	38.709	39.850	39.999
	(1.386)	(0.725)	(0.869)	(0.687)	(0.687)	(0.790)
p	.	0.082	0.407	0.252	0.677	0.756
Test score % (two years before match)	37.651	40.009	41.535	37.601	36.936	39.665
	(1.354)	(0.731)	(0.816)	(0.679)	(0.693)	(0.762)
p	.	0.127	0.014	0.973	0.639	0.197
School avg. FRL	47.553	48.031	47.675	44.190	48.184	47.822
	(0.676)	(0.408)	(0.491)	(0.433)	(0.441)	(0.480)
p	.	0.550	0.885	0.000	0.440	0.748
School avg. Black	19.030	19.431	19.381	17.268	19.112	19.369
	(0.381)	(0.215)	(0.259)	(0.224)	(0.226)	(0.254)
p	.	0.365	0.451	0.000	0.855	0.465
Mean school first-observed test %	54.205	54.372	54.664	56.227	54.240	54.596
	(0.349)	(0.201)	(0.249)	(0.217)	(0.220)	(0.246)
p	.	0.682	0.290	0.000	0.934	0.364
N	523	1824	1705	1877	1836	1709

Mean coefficients; SE in parentheses (clustered by family ID). Teen mothers include all females who gave birth at age 15-17 who had at least two of four years of pre-data. First-observed matches include synthetic matches from non-teenage-childbearing families to teen mothers based on first-observed characteristics. Trajectory matches include synthetic matches from non-teenage-childbearing families to the teen mothers based on four-year test score trends and other observable characteristics. Includes *p*-value of *t*-test between matches and teen mothers.

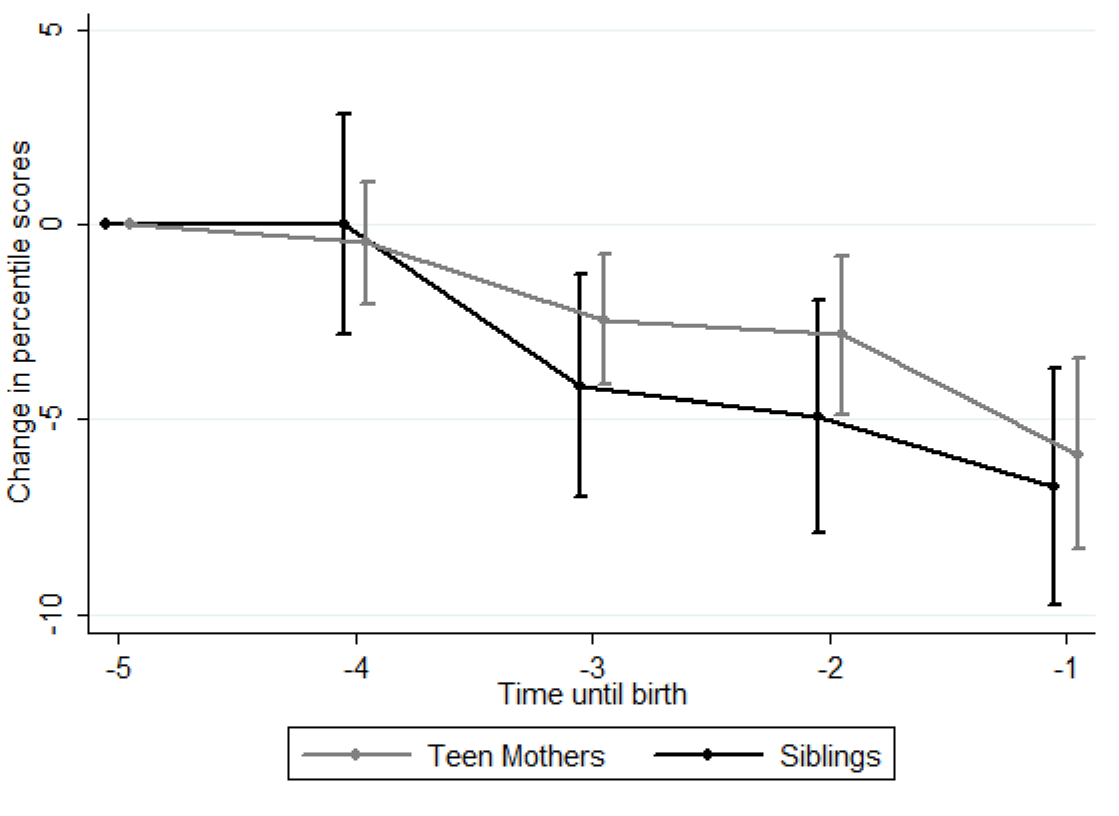
Table 6: Estimated effects of teen birth on various outcomes for teen mothers

	(1) All controls	(2) First-observed scores, controls, & nbhd	(3) Trajectory scores only	(4) Trajectory scores & controls	(5) Trajectory scores, controls, & nbhd
10th grade test scores	-8.129***	-5.667**	-1.463	-4.452*	-4.418*
	(1.795)	(1.866)	(1.864)	(1.736)	(1.822)
N	7200	612	769	681	633
Repeats grade in *t*=0 or later	25.426***	18.858***	17.557***	16.660***	17.244***
	(2.294)	(2.540)	(2.629)	(2.493)	(2.528)
N	42255	2189	2361	2320	2193
Drops out in *t*=0 or later	18.587***	26.414***	21.887***	22.692***	24.051***
	(2.186)	(2.253)	(2.417)	(2.230)	(2.270)
N	41832	2189	2361	2320	2193
Juvenile justice in *t*=0 or later	-1.675**	-1.112	-1.487*	-0.892	-0.903
	(0.578)	(0.702)	(0.683)	(0.697)	(0.688)
N	41832	2189	2361	2320	2193
Ever attends any college	-20.765***	-20.879***	-18.267***	-20.923***	-18.577***
	(3.314)	(3.705)	(3.681)	(3.562)	(3.674)
N	22169	1291	1541	1451	1324
Ever attends a 4-year college	-18.968***	-22.606***	-19.799***	-21.319***	-19.943***
	(3.248)	(3.648)	(3.589)	(3.507)	(3.612)
N	22169	1291	1541	1451	1324
Obtains any degree or certificate	-13.117***	-15.279***	-12.638***	-12.767***	-12.540***
	(2.375)	(2.807)	(2.619)	(2.457)	(2.656)
N	22169	1291	1541	1451	1324
Obtains at least a 4-year degree	-11.617***	-13.720***	-11.641***	-11.953***	-11.693***
	(1.811)	(2.012)	(1.748)	(1.739)	(1.888)
N	22169	1291	1541	1451	1324

Note: Standard errors clustered by family ID. Analysis includes all controls from Table 1. "All" analysis includes all children from non-teenage-childbearing families. First-observed matches includes synthetic matches to females from non-teenage-childbearing families to the teen mothers based on the first-observed characteristics; matches must be from the same neighborhood. Trajectory matches (scores only) includes synthetic matches to females from non-teenage-childbearing families to the teen mothers based on four year score trends. Trajectory matches (scores and controls) adds other observable control variables from Table 1. Trajectory matches (scores, controls, and neighborhood) add the requirement that the match must be from the same neighborhood at first observation. 10th grade test scores, repeats grade, drops out, and juvenile justice include one weighed observation from the siblings and their matched controls.

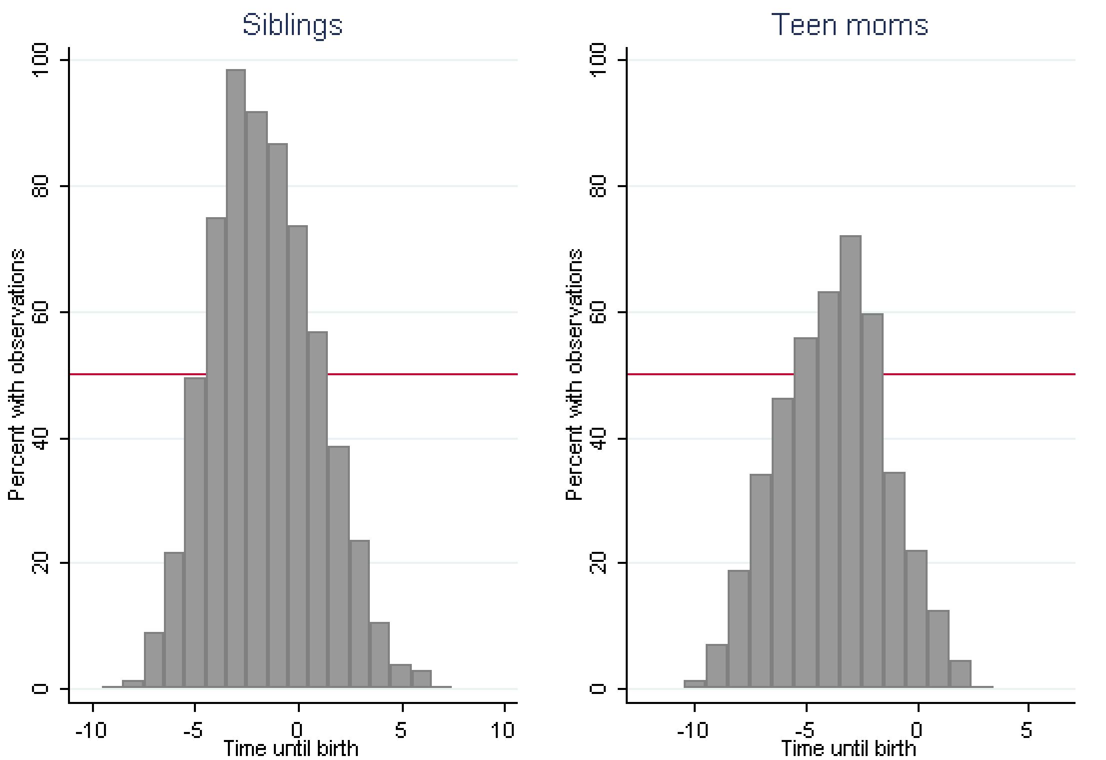
Figures

Figure 1: Trajectory pre-trends for teen mothers and siblings



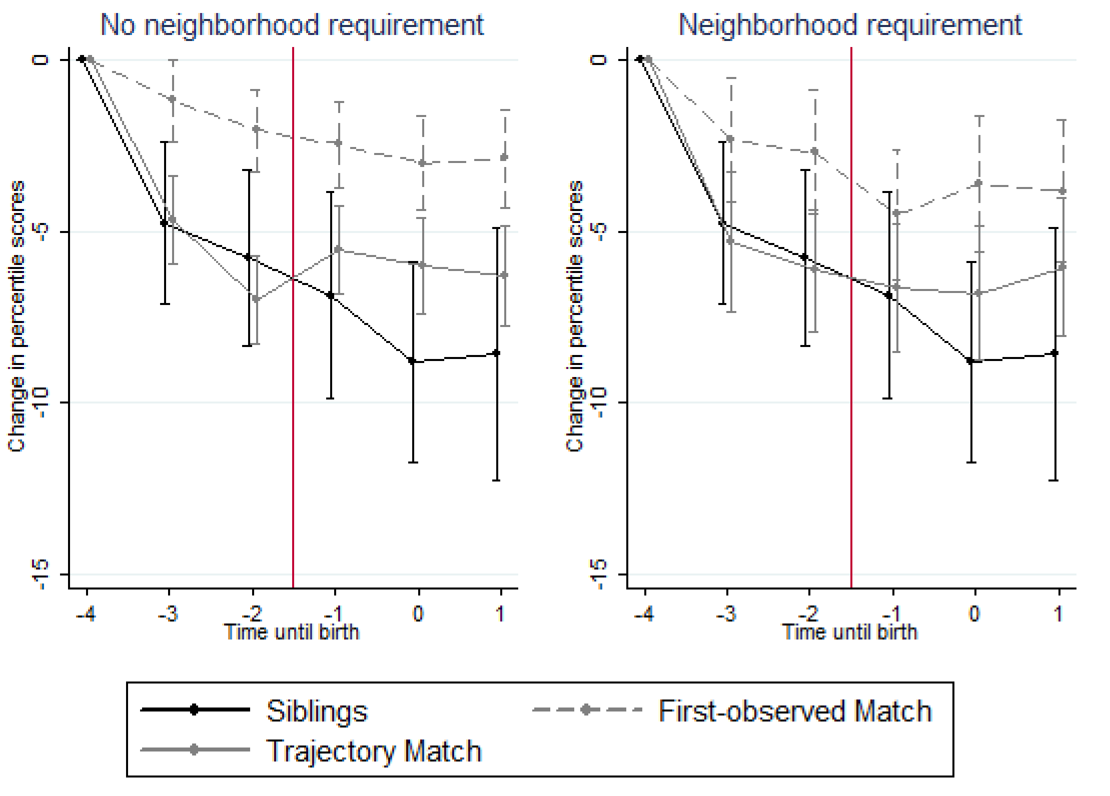
Note: Teen mothers include all teen mothers from families of two or more where the mother gave birth at age 15-17. Siblings include all younger siblings from families where an older sister gave birth at age 15-17. Estimates based on a regression of mean percentile rank (of math and reading) on age with person fixed effects within the noted population, with standard errors clustered by family ID.

Figure 2: Available pre- and post-birth grades for teen mothers and siblings



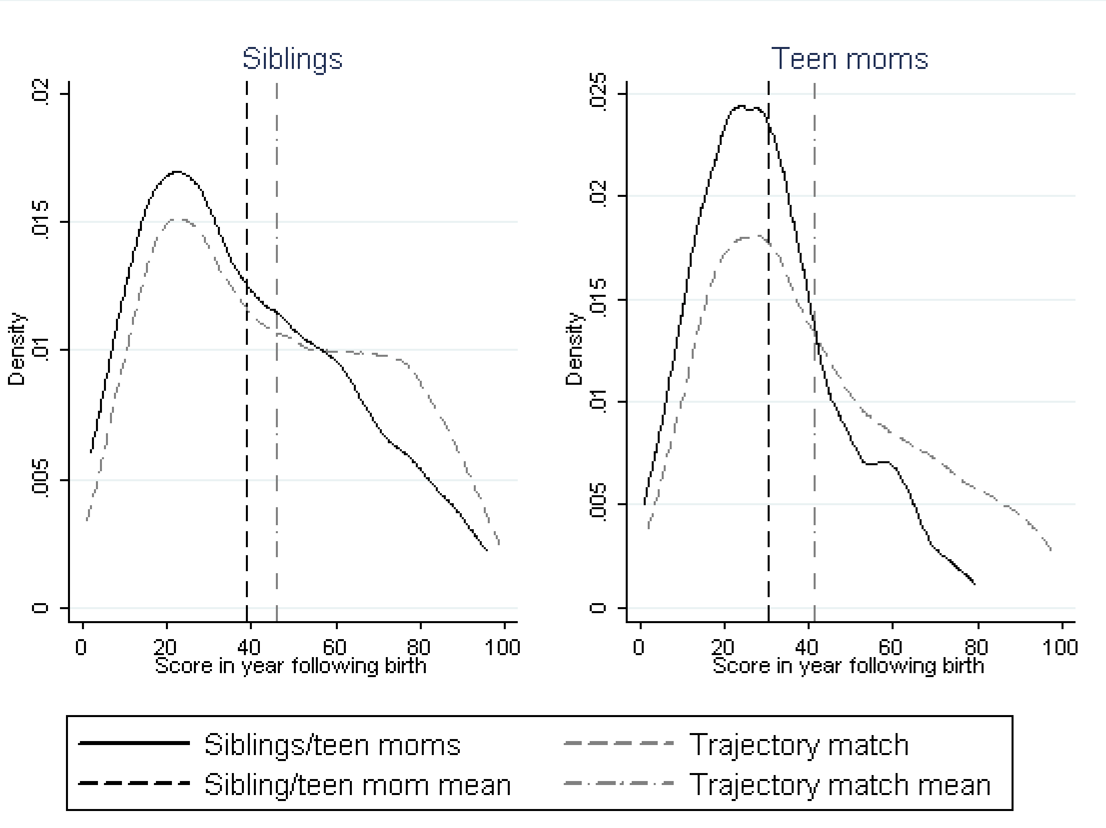
Note: Proportion of siblings and teen mothers with test score data available for the trajectory matching population.

Figure 3: National percentile pre- and post-trends, by group for siblings



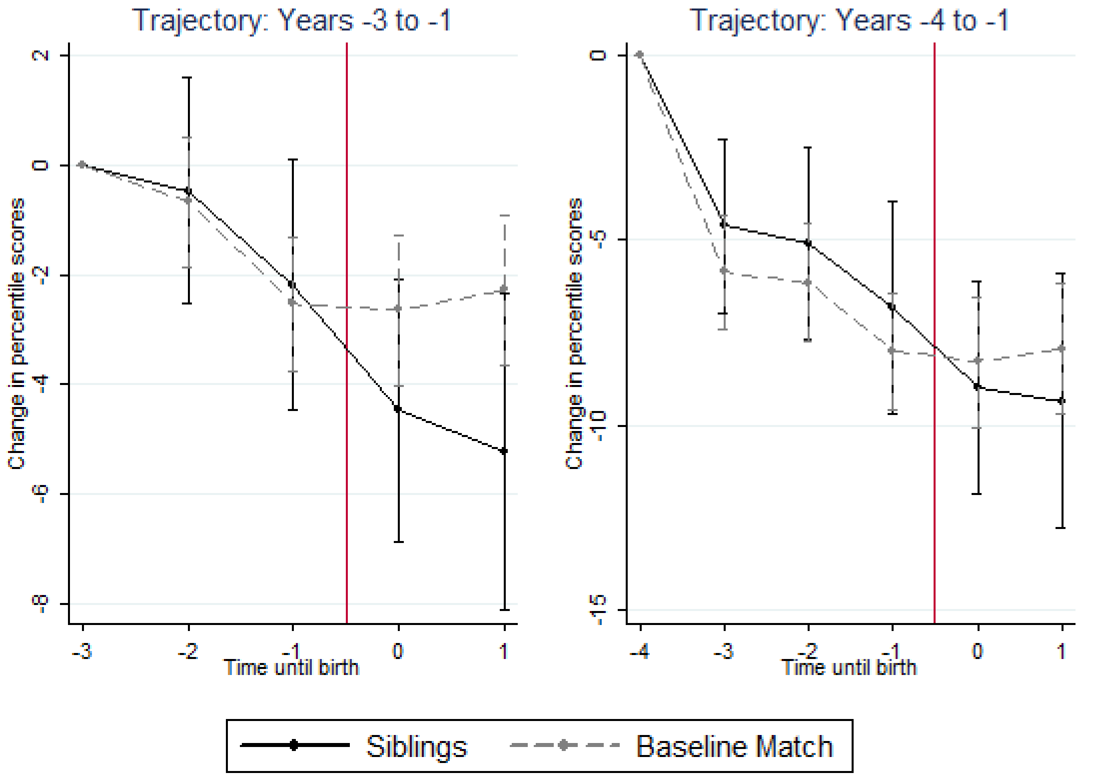
Note: Siblings include all siblings from families where an older sister gave birth at age 15-17. First-observed matches include synthetic matches from non-teenage-childbearing families to the siblings based on first-observed characteristics. Trajectory matches include synthetic matches from non-teenage-childbearing families to the siblings based on three-year test score trends characteristics and first-observed characteristics. Estimates based on a regression of mean percentile rank on years relative to birth (or time relative to the match year for the matches) with person fixed effects within the noted population. The second panel requires the matches to be from the same neighborhood at first observation as the siblings; the first panel does not.

Figure 4: Distribution of test scores following birth for teen mothers, siblings, and synthetic controls



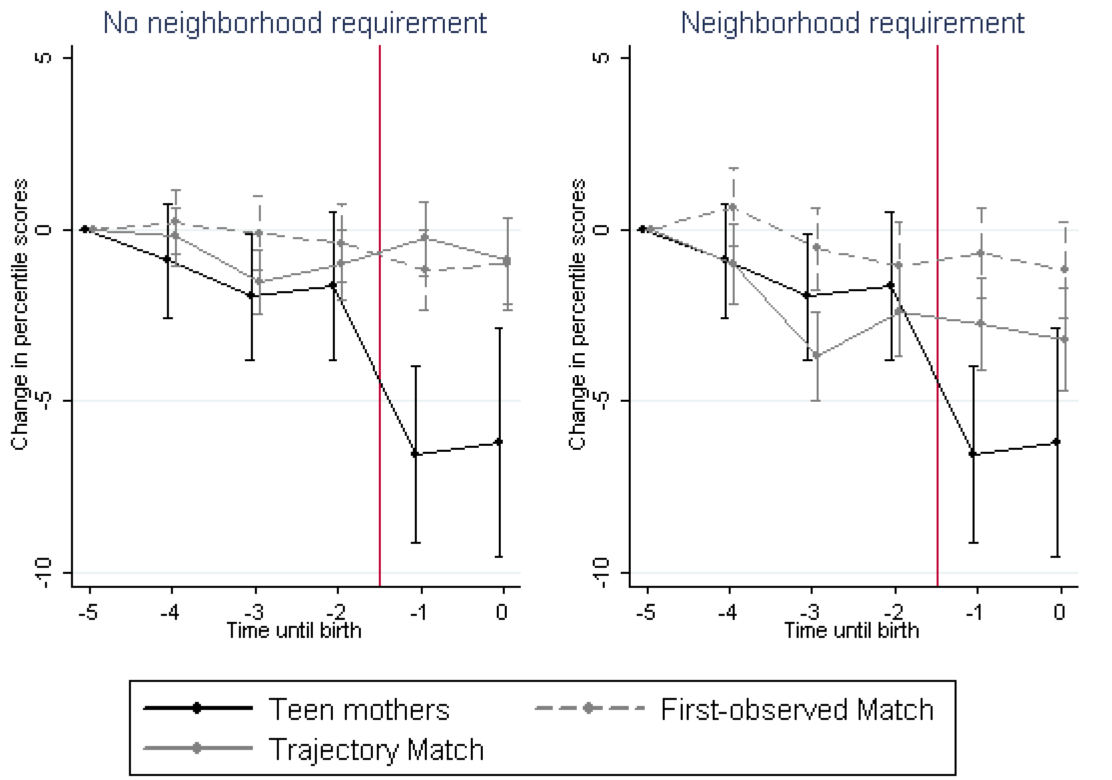
Note: Teen mothers include all females who gave birth at age 15-17. Siblings include all siblings from families where an older sister gave birth at age 15-17. Trajectory matches include synthetic matches from non-teenage-childbearing families to the teen mothers/siblings based on prior-year test score trends characteristics, other observable characteristics, and same-neighborhood requirements.

Figure 5: Alternative trajectory lengths for national percentile trends, by group for siblings



Note: Siblings include all siblings from families where an older sister had a baby at age 15-17. Trajectory matches include synthetic matches from non-teenage-childbearing families to the siblings based years of test score trends and other individual and school characteristics. Estimates based on a regression of mean percentile rank on time until birth (or time relative to the match year for the matches) with person fixed effects.

Figure 6: National percentile pre- and post-trends, by group for teen mothers



Note: Teen mothers include all females who gave birth at age 15-17. First-observed matches include synthetic matches from non-teenage-childbearing families to the teen mothers based on first-observed characteristics. Trajectory matches include synthetic matches from non-teenage-childbearing families to the teen mothers based on four-year test score trends characteristics and first-observed characteristics. Estimates based on a regression of mean percentile rank on year relative to birth (or time relative to the match year for the matches) with person fixed effects within the noted population. The second panel requires the matches to be from the same neighborhood at first observation as the siblings; the first panel does not.

Appendix

Patterns for Teen Mothers by Race and FRL Status

This appendix explores the subgroup analysis for the own effects on teen mothers. One concern with the results might be that they are driven by a particular subgroup; for instance, perhaps lower-income mothers have fewer supports.

Table 10: Estimated effects of teen birth on various outcomes for teen mothers by subgroups

	(1) Baseline	(2) Black	(3) Non-black	(4) FRL	(5) Non-FRL
10th grade test scores	-4.418*	-4.542*	-4.994	-4.096*	-5.842
	(1.822)	(2.200)	(3.087)	(2.025)	(4.091)
N	633	341	292	474	159
Repeats grade in *t*=-1 or later	17.244***	14.070***	21.838***	16.626***	19.415***
	(2.528)	(3.480)	(3.621)	(3.007)	(4.502)
N	2193	1253	940	1665	528
Drops out in *t*=-1 or later	24.051***	24.505***	22.926***	24.279***	23.475***
	(2.270)	(3.037)	(3.429)	(2.642)	(4.542)
N	2193	1253	940	1665	528
Juvenile justice in *t*=-1 or later	-0.903	-0.988	-0.830	-0.929	-0.866*
	(0.688)	(1.027)	(0.884)	(0.896)	(0.415)
N	2193	1253	940	1665	528
Ever attends any college	-18.577***	-17.168**	-19.920***	-18.290***	-20.356**
	(3.674)	(5.355)	(4.910)	(4.252)	(7.628)
N	1324	709	615	991	333
Ever attends at least at least a 4-year college	-19.943***	-17.147**	-23.182***	-20.244***	-19.004*
	(3.612)	(5.263)	(4.801)	(4.154)	(7.636)
N	1324	709	615	991	333
Obtains any degree or certificate	-12.540***	-13.687***	-11.159**	-10.092***	-19.798***
	(2.656)	(3.597)	(3.955)	(3.010)	(5.780)
N	1324	709	615	991	333
Obtains at least a 4-year degree	-11.653***	-8.459**	-14.969***	-9.260***	-17.230***
	(1.905)	(2.795)	(2.530)	(2.006)	(4.635)
N	1324	709	615	991	333

Note: Standard errors clustered by family ID. All analysis based on the trajectory matches based on prior test scores and other controls, and require that the matched control be from the same neighborhood. Baseline result is from the main table on teen motherhood; later columns conduct the analysis by subgroups.

Table 11: Descriptive statistics for outcome variables for the synthetic controls for siblings

	First-observed match			Trajectory match		
	(1) Siblings of teen mothers	(2) Scores & controls	(3) Scores, controls & nbhd	(4) Score only	(5) Scores & controls	(6) Scores, controls & nbhd
Test scores at t=0	38.985	47.130	47.930	44.874	43.658	44.248
	(26.55)	(48.68)	(48.22)	(46.98)	(47.60)	(45.70)
10th grade test scores	40.603	46.958	46.969	44.614	43.250	43.436
	(20.16)	(44.16)	(40.46)	(45.71)	(42.29)	(39.58)
Repeats grade in t=0 or later	34.402	28.269	27.079	25.385	28.269	30.174
	(15.65)	(19.98)	(18.53)	(18.69)	(20.06)	(20.13)
Drops out in t=0 or later	17.521	13.846	12.959	11.058	13.077	13.250
	(9.96)	(12.76)	(11.74)	(11.30)	(12.39)	(11.97)
Juvenile justice in t=0 or later	14.316	6.058	5.706	4.712	6.635	7.834
	(8.83)	(8.08)	(7.48)	(7.13)	(8.52)	(8.93)
Ever attends any college	37.553	62.210	61.010	56.785	55.511	54.980
	(11.91)	(34.36)	(31.52)	(31.39)	(30.45)	(28.43)
Ever attends a 4-year college	35.443	59.345	58.065	54.677	52.590	51.296
	(11.38)	(32.35)	(29.65)	(30.08)	(28.71)	(26.41)
Obtains any degree or certificate	16.034	27.831	26.087	26.877	26.428	26.330
	(6.71)	(16.63)	(14.97)	(16.60)	(16.34)	(15.38)
Obtains a 4-year degree	7.595	18.145	17.672	16.864	17.264	17.599
	(4.40)	(12.61)	(11.67)	(12.33)	(12.45)	(11.89)

Mean coefficients; SE in parentheses (clustered by family ID). Siblings include all younger siblings from families where a sister gave birth at age 15-17 who had at least two of three years of pre-data. First-observed matches include synthetic matches from non-teenage-childbearing families to siblings based on first-observed characteristics. Trajectory matches include synthetic matches from non-teenage-childbearing families to the siblings based on four-year test score trends and other observable characteristics, as noted.

repeats the previous analysis by subgroup. To be conservative, these estimates include the neighborhood requirement, as they account for unobserved neighborhood differences. Column 1 contains the preferred overall estimates for reference. There are few statistical differences in outcomes by subgroup, though if anything non-black and non-FRL students have worse outcomes following a birth. The only effect size difference close to statistical significance is obtaining four years college degrees (-8.5 percentage points for black mothers versus -15.0 percentage points for non-black mothers, *p*-value of difference=0.082).

Appendix Tables

Table 7: National percentile pre-trends, by group

	Teen mothers			Siblings		
	Individual FE	Individual & Age FE	Individual, Age & School FE	Individual FE	Individual & Age FE	Individual, Age & School FE
4 years before birth	-0.689	-0.668	-0.476	-0.714	0.063	0.015
	(0.804)	(0.803)	(0.807)	(1.447)	(1.435)	(1.434)
3 years before birth	-2.015*	-2.762**	-2.432**	-5.709***	-4.282**	-4.134**
	(0.863)	(0.866)	(0.864)	(1.477)	(1.444)	(1.447)
2 years before birth	-2.066*	-2.788**	-2.835**	-6.807***	-5.161***	-4.923**
	(1.048)	(1.047)	(1.043)	(1.538)	(1.502)	(1.511)
1 years before birth	-5.898***	-5.489***	-5.879***	-8.447***	-6.778***	-6.704***
	(1.233)	(1.249)	(1.242)	(1.574)	(1.548)	(1.545)
Observations	405606	405606	405586	405155	405155	405135
N	87030	87030	87026	86918	86918	86914

Robust standard errors in parentheses. Standard errors clustered by family ID.
Models include mothers and their siblings 1 to 5 years before birth. All children from non-childbearing families are included to estimate age and school fixed effects (included as noted in headings). Year *t*=-5 is the excluded category.

Table 8: Logit models predicting becoming a sibling of a teen mother

	First-observed controls		Trajectory controls		
	All	AIC-restricted	Scores only	Scores and controls	AIC-restricted
Female	0.272^{+}	0.266^{+}		0.293*	0.296*
	(0.141)	(0.141)		(0.142)	(0.142)
Age	-0.170***	-0.169***		-0.205***	-0.211***
	(0.035)	(0.035)		(0.042)	(0.041)
# of siblings in data	0.228***	0.228***		0.226***	0.225***
	(0.062)	(0.062)		(0.062)	(0.062)
FRL	1.239***	1.268***		1.163***	1.163***
	(0.198)	(0.195)		(0.199)	(0.199)
Black	1.604***	1.621***		1.495***	1.481***
	(0.160)	(0.152)		(0.162)	(0.158)
First-observed test score	-0.002				
	(0.003)				
First-observed school avg. FRL	2.410*	2.468*		2.241^{+}	2.329*
	(1.150)	(1.111)		(1.145)	(1.109)
First-observed school avg. Black	-0.556			-0.500	
	(1.175)			(1.175)	
First-observed school avg. first-observed test score	0.034	0.038^{+}		0.035	0.039^{+}
	(0.026)	(0.022)		(0.025)	(0.023)
Scores, year t=-2			-0.021***	-0.011*	-0.011*
			(0.005)	(0.005)	(0.005)
Scores, year t=-3			-0.017**	-0.013*	-0.013*
			(0.006)	(0.006)	(0.006)
Scores, year t=-4			0.009^{+}	0.014*	0.014*
			(0.005)	(0.005)	(0.005)
Missing score data, year t=-2			-0.530*	-0.151	
			(0.259)	(0.267)	
Missing score data, year t=-3			-1.570**	-1.160*	-1.132^{+}
			(0.585)	(0.589)	(0.587)
Missing score data, year t=-4			-0.170	-0.474**	-0.467**
			(0.165)	(0.180)	(0.180)
Constant	-8.834***	-9.327***	-4.855***	-7.748***	-8.086***
	(2.045)	(1.805)	(0.161)	(2.079)	(1.843)
Observations	128576	128576	128576	128576	128576

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Model predicts the probability of becoming a sibling of a teenage mother, among the younger siblings of teenage mothers and younger siblings from non-teenage-childbearing families. Requires at least 2 od 3 prior observations.

Table 9: Logit models predicting teen motherhood

	First-observed controls		Trajectory controls		
	All	AIC-restricted	Scores only	Scores and controls	AIC-restricted
Age	0.271***	0.271***		0.323***	0.326***
	(0.025)	(0.025)		(0.033)	(0.033)
# of siblings in data	0.070			0.073	
	(0.058)			(0.059)	
Oldest	0.295**	0.269**		0.319**	0.288**
	(0.106)	(0.103)		(0.107)	(0.104)
FRL	0.831***	0.842***		0.724***	0.742***
	(0.140)	(0.138)		(0.142)	(0.139)
Black	1.577***	1.580***		1.435***	1.455***
	(0.122)	(0.122)		(0.124)	(0.124)
First-observed test score	-0.017***	-0.017***			
	(0.002)	(0.002)			
First-observed school avg. FRL	-0.185			-0.393	
	(0.719)			(0.717)	
First-observed school avg. Black	-1.510^{+}	-1.485^{+}		-1.446^{+}	-1.393
	(0.851)	(0.840)		(0.861)	(0.856)
First-observed school avg. first-observed test score	-0.035*	-0.032***		-0.035*	-0.028**
	(0.016)	(0.009)		(0.016)	(0.009)
Scores, year t=-2			-0.012*	-0.005	
			(0.005)	(0.005)	
Scores, year t=-3			-0.011*	-0.006	-0.011*
			(0.005)	(0.005)	(0.004)
Scores, year t=-4			-0.009^{+}	-0.007	-0.013**
			(0.005)	(0.005)	(0.004)
Scores, year t=-5			-0.013**	-0.008	
			(0.005)	(0.005)	
Missing score data, year t=-2			0.360**	-0.144	-0.136
			(0.120)	(0.130)	(0.130)
Missing score data, year t=-3			-0.910***	-1.241***	-1.223***
			(0.199)	(0.203)	(0.202)
Missing score data, year t=-4			-0.415**	-0.486**	-0.482**
			(0.152)	(0.149)	(0.149)
Missing score data, year t=-5			-0.700***	-0.149	-0.125
			(0.132)	(0.136)	(0.135)
Constant	-7.861***	-7.954***	-3.296***	-7.822***	-8.310***

	(1.330)	(0.733)	(0.120)	(1.380)	(0.810)
Observations	130811	130811	130811	130811	130811

Standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Model predicts the probability of becoming a teenage mother, among the eventual teenage mothers and females from non-teenage-childbearing families. Requires at least 2 od 3 prior observations.

Table 10: Estimated effects of teen birth on various outcomes for teen mothers by subgroups

	(1) Baseline	(2) Black	(3) Non-black	(4) FRL	(5) Non-FRL
10th grade test scores	-4.418*	-4.542*	-4.994	-4.096*	-5.842
	(1.822)	(2.200)	(3.087)	(2.025)	(4.091)
N	633	341	292	474	159
Repeats grade in *t*=-1 or later	17.244***	14.070***	21.838***	16.626***	19.415***
	(2.528)	(3.480)	(3.621)	(3.007)	(4.502)
N	2193	1253	940	1665	528
Drops out in *t*=-1 or later	24.051***	24.505***	22.926***	24.279***	23.475***
	(2.270)	(3.037)	(3.429)	(2.642)	(4.542)
N	2193	1253	940	1665	528
Juvenile justice in *t*=-1 or later	-0.903	-0.988	-0.830	-0.929	-0.866*
	(0.688)	(1.027)	(0.884)	(0.896)	(0.415)
N	2193	1253	940	1665	528
Ever attends any college	-18.577***	-17.168**	-19.920***	-18.290***	-20.356**
	(3.674)	(5.355)	(4.910)	(4.252)	(7.628)
N	1324	709	615	991	333
Ever attends at least at least a 4-year college	-19.943***	-17.147**	-23.182***	-20.244***	-19.004*
	(3.612)	(5.263)	(4.801)	(4.154)	(7.636)
N	1324	709	615	991	333
Obtains any degree or certificate	-12.540***	-13.687***	-11.159**	-10.092***	-19.798***
	(2.656)	(3.597)	(3.955)	(3.010)	(5.780)
N	1324	709	615	991	333
Obtains at least a 4-year degree	-11.653***	-8.459**	-14.969***	-9.260***	-17.230***
	(1.905)	(2.795)	(2.530)	(2.006)	(4.635)
N	1324	709	615	991	333

Note: Standard errors clustered by family ID. All analysis based on the trajectory matches based on prior test scores and other controls, and require that the matched control be from the same neighborhood. Baseline result is from the main table on teen motherhood; later columns conduct the analysis by subgroups.

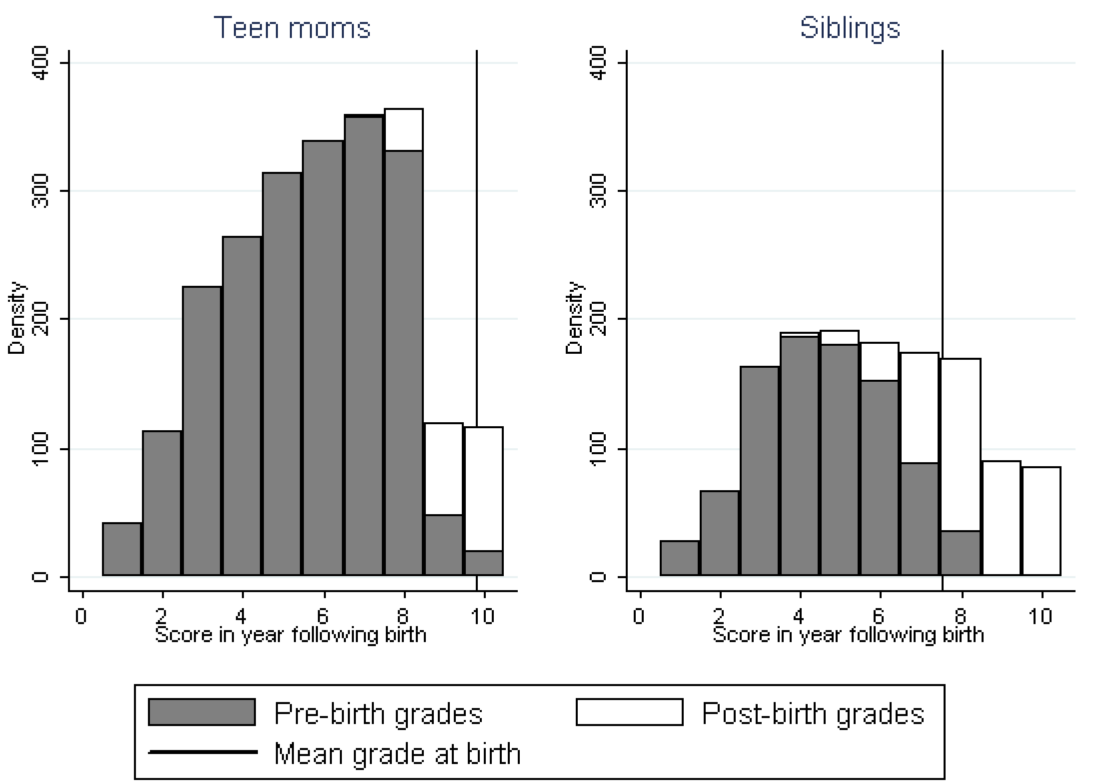
Table 11: Descriptive statistics for outcome variables for the synthetic controls for siblings

	First-observed match			Trajectory match		
	(1) Siblings of teen mothers	(2) Scores & controls	(3) Scores, controls & nbhd	(4) Score only	(5) Scores & controls	(6) Scores, controls & nbhd
Test scores at *t*=0	38.985 (26.55)	47.130 (48.68)	47.930 (48.22)	44.874 (46.98)	43.658 (47.60)	44.248 (45.70)
10th grade test scores	40.603 (20.16)	46.958 (44.16)	46.969 (40.46)	44.614 (45.71)	43.250 (42.29)	43.436 (39.58)
Repeats grade in *t*=0 or later	34.402 (15.65)	28.269 (19.98)	27.079 (18.53)	25.385 (18.69)	28.269 (20.06)	30.174 (20.13)
Drops out in *t*=0 or later	17.521 (9.96)	13.846 (12.76)	12.959 (11.74)	11.058 (11.30)	13.077 (12.39)	13.250 (11.97)
Juvenile justice in *t*=0 or later	14.316 (8.83)	6.058 (8.08)	5.706 (7.48)	4.712 (7.13)	6.635 (8.52)	7.834 (8.93)
Ever attends any college	37.553 (11.91)	62.210 (34.36)	61.010 (31.52)	56.785 (31.39)	55.511 (30.45)	54.980 (28.43)
Ever attends a 4-year college	35.443 (11.38)	59.345 (32.35)	58.065 (29.65)	54.677 (30.08)	52.590 (28.71)	51.296 (26.41)
Obtains any degree or certificate	16.034 (6.71)	27.831 (16.63)	26.087 (14.97)	26.877 (16.60)	26.428 (16.34)	26.330 (15.38)
Obtains a 4-year degree	7.595 (4.40)	18.145 (12.61)	17.672 (11.67)	16.864 (12.33)	17.264 (12.45)	17.599 (11.89)

Mean coefficients; SE in parentheses (clustered by family ID). Siblings include all younger siblings from families where a sister gave birth at age 15-17 who had at least two of three years of pre-data. First-observed matches include synthetic matches from non-teenage-childbearing families to siblings based on first-observed characteristics. Trajectory matches include synthetic matches from non-teenage-childbearing families to the siblings based on four-year test score trends and other observable characteristics, as noted.

Appendix Figures

Figure 7: Stacked distribution of grades available for teen mothers and siblings, by pre-versus post-birth



Notes: Distribution of test score data available by grade, before and after birth, for teen mothers and their siblings used in the preferred synthetic control analysis.