Teen Fertility and Siblings' Outcomes

Evidence of Family Spillovers Using Matched Samples

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ABSTRACT

U.S. teen birth rates remain high relative to other industrialized countries. Despite extensive literature on teen mothers and their children, almost no research examines the effects of teen fertility on the rest of the mother's family. I address this gap, finding that teen birth negatively affects mothers' younger siblings. Using several matched control methods, I find that sisters of new teenage mothers experience a 3.8 percentage point decrease in test scores, a 7.6 percentage point increase in grade repetition, and a 9.3 percentage point increase high school dropout, while brothers experience a 9.2 percentage point increase in juvenile justice system exposure.

I. Introduction

Economists have long studied how the family environment affects child outcomes (for example, Becker 2009). While there is a large literature on children having children, there is almost no evidence on the effect of teen childbearing on the

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[Submitted February 2018; accepted March 2019]; doi:10.3368/jhr.56.1.0218-9341R2
JEL Classification: I21

other children already in the household.[1] This paper has three primary contributions. First, I show teen childbearing has negative spillovers to younger siblings of the teen mother. Second, I demonstrate that families where a teen birth eventually occurs are on a downward trajectory in terms of test scores for several years before the birth. Finally, I demonstrate that sibling fixed-effects estimates understate the true effect of teen pregnancy on the mothers' own outcomes.

Siblings share neighborhood environments, similar genetics, and (limited) parental resources, and it seems probable that an unexpected change in one sibling could 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.[2] The birth of a child to a teen mother is one presumably large shock that directly affects one child and may have ripple effects in the family.

Understanding the full consequences of teen motherhood matters for policymakers in the United States, which has the highest birth rate among teenagers of any industrialized country (Kearney and Levine 2012). Adding a newborn to the home might have profound effects on the whole family, including increased family conflict and loss of sleep. The new grandparents often take on childcare responsibilities, which descriptive studies suggest can take away their time to work outside the home, increase their stress levels, and reduce their time available for their other children (Bailey, Haynes, and Letiecq 2013; Chase-Lansdale et al. 1999; East 1998). After the birth, new grandmothers monitor and communicate less with their nonparenting children, relative to the same families before a daughter gave birth (East 1999). Comparing families with a teen pregnancy to families without teen pregnancy, and controlling for prenatal-period characteristics, the appearance of the baby is associated with an increase in family stress, which in turn is associated with harsher parenting and more family conflict (East and Chien 2013).

To date there has been almost no research on the effects of teen motherhood on the outcomes of other children in the family. Some prior work along these lines pertains to

1. Prior research has reached conflicting conclusions about whether teen pregnancy causes poor outcomes for the mother or whether teen pregnancy is a symptom of prior trends. Teen parenthood is popularly understood to be a negative outcome for the mother, including reduced education attainment and worse long-term economic prospects (Kane et al. 2013; Miller 2009). However, many such studies do not account for negative selection into pregnancy and, among those who get pregnant, positive selection into abortion. Several studies instead use miscarriage as an instrumental variable to examine teen motherhood, generally finding null or small effects (Ashcraft, Fernández-Val, and Lang 2013; Hotz, McElroy, and Sanders 2005; Hotz, Mullin, and Sanders 1997). Work by Fletcher and Wolfe (2009) argue that miscarriage is correlated with community-level factors, and after accounting for this correlation, they find that teen pregnancy reduces high school graduation rates and annual income.

2. We do have some information on family spillover. Younger siblings with disabilities or health problems can negatively affect their older siblings' educational outcomes (Black et al. 2017; Breining et al. 2015; Breining 2014), while higher-achieving older siblings can positively affect their younger siblings (Joensen and Nielsen 2018; Nicoletti and Rabe 2019; Qureshi 2018). Additionally, a small component of the correlation in sibling substance abuse is caused by older siblings modeling behavior to younger siblings (Altonji, Cattan, and Ware 2017). Parents may also shift their intrahousehold allocation of resources following shocks to one child. Among twins in China, healthy children whose twin sibling has a health shock at age zero to three receive less in medical spending but more in educational spending, compared to the less healthy twin (Yi et al. 2015).

sibling fertility: the younger siblings of teen mothers are at increased risk to become teen parents themselves (Monstad, Propper, and Salvanes 2011).[3] The siblings of teen mothers may also reallocate their time away from activities that improve their own human capital development. New responsibilities could involve either childcare or home production tasks that had previously been completed by other family members. In a difference-in-differences strategy, the siblings of teen parents had larger increases in drug and alcohol use and in frequency of sex from before or just after birth (T1) to 1.5 years later (T2), relative to families without teen pregnancy (East and Jacobson 2001). The siblings of teen parents also spent an average of 10.3 hours per week caring for the sister's child and were more likely to be pregnant with their own child relative to the control siblings in T2 (East and Jacobson 2001).

The present paper, along with Heissel (2017), represents the first research, to my knowledge, that studies the causal effects of teen motherhood on their siblings' short- and medium-term human capital development. While Heissel (2017) examined high school academic outcomes, this work adds juvenile justice system exposure, college attendance, college completion, and a supplementary analysis of time use to study potential mechanisms. This study also adds family trajectories to the matching algorithm. The data come from detailed longitudinal student data from a large Florida county-level school district linked to postsecondary data from the National Student Clearinghouse, as well as a separate file from the American Time Use Study.

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.[4] Thus, teen mothers—and their siblings—may be on a downward trajectory well before the birth. Indeed, I demonstrate that teen 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. This fundamental uncertainty underlies the ongoing debate on the effects of teen pregnancy (see Diaz and Fiel 2016). Moreover, many identification strategies that work for studying teen fertility (for example, the Buckles and Hungerman [2018] study of condom distribution programs) cannot disentangle the spillover consequences of teen pregnancy, especially given that siblings tend to be relatively closely spaced. In this paper, I make use of longitudinal school district data, in which children are observed annually throughout their schooling years, to conduct

3. Kearney and Levine (2015) show that the MTV show *16 and Pregnant* led to a decrease in teen pregnancy. Other work finds that peer pregnancy increases own pregnancy, at least among schools without family-planning services (Fletcher and Yakusheva 2016), while a friend's pregnancy, as opposed to a miscarriage, decreases teen pregnancy (Kapinos and Yakusheva 2016). The variety of results indicates that teenagers are responding to a variety of signals.

4. For instance, in a sample collected as a control group for families of teen mothers, siblings without a pregnant older sister had more maternal monitoring (by the siblings' shared mother), more school involvement, fewer depressive symptoms, less defiant and "partying" behavior, higher parental marriage rates, and less family disruption in the prenatal period than siblings with a pregnant older sister (Chien and East 2012; East and Chien 2013). East and Jacobson (2001) also averaged outcomes over time and found that siblings of teen parents had lower school aspirations and more school problems than siblings of nonparents.

an event study analysis. I match children in families experiencing a teen birth 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. Throughout the paper, I refer to the teen siblings of teen mothers as "teen aunts/uncles."

In terms of test scores, teen aunts/uncles and their matched comparators are on similar downward trajectories for several years prior to the start of pregnancy. They continue on that trajectory in the year of pregnancy. Teen aunts/uncles then diverge post-birth, with a marked drop in test scores between 4.2 and 4.4 percentile points, relative to those on a similar trajectory before birth (from a base at the 44th percentile). Among teen aunts (that is, sisters of the teen mothers), high school dropout is 9.3 percentage points more likely relative to females who had been on a similar trajectory (from a base of 14.2 percent); the effect is null for teen uncles. Among teen uncles, the chance of encountering the juvenile justice system increases by 9.2 percentage points after the birth (from a base of 12.9 percent); the effect is a null 2.2 percentage points for aunts (from a base of 4.0 percent). In the longer term, the chance of attending college drops by 5.4 to 9.7 percentage points across all children (from a base of 56.6 percent), though the effects are not consistently statistically significant.

I use data from the American Time Use Survey to study whether time allocation may drive the results. Using a proxy for the teen aunts/uncles, I find that the teen aunts spend more time on childcare on all days and less time on homework on weekends, relative to the teen uncles and other teen girls. Teen aunts also spend less time with friends and parents on weekends. Substitution of time from homework to childcare and reduced parental supervision may drive the academic results for the teen aunts.

While not the primary contribution of this paper, I can apply the same analytical strategy to add to the literature on the effect of teen childbirth on mothers' own outcomes, though remaining selection issues are arguably more important in the own-outcomes case than they are for the siblings. 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 graduation relative to female students who had been on a similar trajectory before the birth. 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. This difference in timing provides evidence that it is not some common shock to the family that leads to a decrease in test scores: instead, teen aunts/uncles diverge only once the baby arrives in the home.

Some concerns about unobservable differences between the treated siblings and their matched controls, as well as underidentification of teen mothers, remain in this analysis. Despite this weakness, the present analysis offers the best evidence to date on how teen pregnancy affects the other teenagers in the family. The finding of sibling spillovers offers a warning to researchers using sibling fixed-effects models. I show that sibling fixed-effects models can mechanically understate the negative estimates for teen motherhood. Within-family comparisons are a common tool in econometrics, and researchers should be careful to consider how a policy or phenomenon might affect the whole family—for teen pregnancy and other topics—before using sibling fixed effects.

II. Data

Data come from an anonymous large Florida school district's administrative files for the 1989–1990 through 2004–2005 school years (henceforth, 1990–2005). Data are limited to one large county in Florida for students in families with at least two siblings. The years of birth for the students range from 1974 to 1993.

One challenge in the analysis is that I cannot perfectly identify all teen mothers. I use two methods. First, in 2005 the district identified the school ID of parents of current students if the parents also attended school in the district. The mother's school data are then connected to the child's date of birth, which is used to calculate the mother's age at birth (the "birthday method"). Mothers are identified with this first method even if they dropped out of the public school system if their children were enrolled in school in the county in 2005. This method does not identify most births before 1982 because most of the children would have graduated by 2005. This does not change the available analysis, as I do not have the necessary test scores for teen mothers and teen aunts/uncles from early years.

Second, until 2003 the district identified when students became mothers, as long as they remained enrolled in public school in the county (the "district method"). This second method misses any teen 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 Method 1.

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 teen-parented children.[5] To avoid spillover contamination, younger siblings that become teen mothers themselves in later years are categorized as younger siblings of teen mothers. It is the younger siblings of teen mothers who are my main groups of interest in this study.[6]

I underidentify teen 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. My identified sample may then be more advantaged than the full population (if it missed teen mothers who dropped out or if the child attending an alternative school district implies negative selection) or less advantaged (if the child attending an alternative school district implies

5. Year $t = 0$ was the first academic year that the baby appeared in the home, but a portion of that year also occurred before the birth, when the teen mother-to-be was still pregnant. Similarly, $t = -1$ could contain almost all of the pregnancy (if the birth occurred at the beginning of the year) or none of it (if the birth occurred at the end of the year and year $t = 0$ contains all of the pregnancy). This timing issue adds noise to the test score estimates. This is less of a concern in longer-term outcomes, where monthly timing is less important.

6. Because I do not precisely identify every sibling of teen mothers, some siblings of teen mothers may be misclassified as control students. See the Online Appendix for a full discussion of this issue, including an exercise that shifts the number of matches used in the analysis.

positive selection). Underidentification may also attenuate the results if some of those who are really treated are available for matching in the control group. I discuss various checks under cases of underidentification in the Results section.

There are several outcomes of interest. The most immediate outcome is the nationally norm-referenced individual-level scores on the annual California Test of Basic Skills and later the Stanford Achievement Test in math and reading. Tested grades differed by year and ranged from Grades 1 to 10.[7] The data were reported on a 1–100 scale, representing the student's rank in the national distribution of test scores in each subject. About 4.8 percent of student–test years were missing test data that ranked them on a national percentile scale, but did have data from the Florida Comprehensive Assessment Test (FCAT). For each grade and subject (math and reading), I regressed the national percentile rank on a cubic function of the FCAT for the cases in which I observed both tests. I used these estimates to impute the estimated national percentile rank for the years missing data. I use these imputed scores for the pre-test trajectories, but not as outcomes.[8] 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 similar results.

The analysis also examines longer-term outcomes, including whether the student repeated a grade in at least one of the years following the birth, whether the student dropped out of school after the birth, and whether the student first encountered the juvenile justice system after the birth.[9] Testing did not occur in every grade, so the number of observations is lower in the test score analysis than in these other high school outcomes. The National Student Clearinghouse provide college-going data for the subsample of students expected to be in college during 1997–2006 (about 60 percent of the data).

Relative to families without a teen mother, both teen mothers and their siblings were more likely to be eligible for free- and reduced-price lunch (FRL), identify as Black, have lower first-observed test scores, and attend schools with a higher proportion of these characteristics in their first observation in the data (see Online Appendix Table A1). These differences highlight the importance of finding a good control group in the analysis.

III. Analytic Method

Research on teen pregnancy often compares teen mothers to girls in families without a teen mother. Selection into teen pregnancy means that such research may not provide reliable results: students 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. Kearney and Levine (2012), for example, argue that teen childbearing is a consequence of low economic prospects. Such prospects are not stationary over years, and I demonstrate that families that eventually have a teen birth are on a downward trajectory prior to the birth. This trajectory matters in

7. Students took the tests in the spring of Grades 3–8 in all years, and testing also occurred in Grade 1 in 1990, Grade 2 in 1990–1992, and Grades 9–10 in 2000–2005.

8. Including these imputed years for the outcomes does not change the estimates.

9. Juvenile justice exposure equals one if the district data indicated the student was sent through the county juvenile justice system. The age of majority in Florida is 18.

teen pregnancy analysis because a simple difference-in-differences approach would overstate the effect of teen pregnancy on teen mothers and their siblings relative to families without teen births. Indeed, this intuition of a differential trajectory is the reasoning behind the instrumental variables and other identification strategies pursued in the teen mother literature (Ashcraft, Fernández-Val, and Lang 2013; Fletcher and Wolfe 2009; Geronimus and Korenman 1992; Hotz, McElroy, and Sanders 2005; Hotz, Mullin, and Sanders 1997). If research does not account for preexisting trends, it could falsely create the appearance of a causal effect of the teen birth.

The primary contribution of the present paper tests whether teen birth changes the trajectory of teen aunts/uncles. I create several matched control groups to estimate whether observably similar students diverge after the eventual teen mother gives birth. I focus on teen aunts/uncles younger than the teen mother, both because older siblings were less likely to have the necessary test score data in the years leading up to the birth, and because theoretically there is a stronger influence from older to younger siblings (Monstad, Propper, and Salvanes 2011).

I use several different logit models to predict the probability of being the sibling of a teenage mother based on observable characteristics. For each model, I remove variables to minimize the Akaike information criterion (AIC). Each teen aunt/uncle is matched to five control students on the basis of this probability. Each matched control receives a weight of 0.2 per match. The matching procedure is based on the treated observation in the year before the birth. Potential controls in the matching pool are included in the sample in each available year. I only allow a given control student to match to a given teen aunt/uncle at one age. This requirement prevents a control student very similar to a given teen aunt/uncle from being matched to that teen aunt/uncle in multiple years. The procedure uses replacement, so a control could have a higher weight if selected as a match for multiple teen aunts/uncles. A given control student could be matched to different teen aunt/uncles at the same ages or at different ages.[10]

The first logit model includes the sort of early-year variables that might be available in a data set that follows students over time, but has several-year gaps between waves. Specifically, the model predicts the probability that a student becomes a teen aunt/uncle using the student's age; family size (as measured by the number of siblings in the data); early test score data (as measured by the first individual test score observed in the data); school characteristics for percent Black, percent FRL, and test scores in the first school the student attended; and indicators for identification as female, FRL (at first observation), and Black.[11] The control group produced by this procedure is referred to as the early matched control, abbreviated as EARLY.

Because I have annual data, I can conduct the matching procedure in years that are closer to the appearance of the baby in the home, which may better reflect the student's current situation. I create another match (called JUST BEFORE) that uses individual

10. For example, say teen aunt/uncle X is very similar to control student A. Control student A would be in the sample at every available age (say, 12, 13, 14), while teen aunt/uncle X would be matched at the age they were in the year before the birth (let's say age 13). Teen aunt/uncle X could only be matched to control student A once, likely at age 13. The other four matches would be to control students B, C, D, and E. Teen aunt/uncle Y might also be very similar to control student A. I would select the age for control student A that best matches that the observed characteristics of teen aunt/uncle Y (which could be 13, 14, or 15).

11. The modal first-observed score is in Grade 3. School average first-observed test scores provide an estimate of the characteristics of the school a student first attends. Data on Hispanic students are not included as inclusion could reveal the anonymous county.

test scores from the year before the pregnancy ($t = -2$) instead of the individual score from the early years of data.[12] The other variables are the same as in the EARLY analysis.

A trajectory-based method adds multiyear trends to the algorithm. For the teen aunt/uncle analysis, a majority of teen aunts/uncles have pre-birth test scores in years $t = -4$, -3, -2, and -1 (see Online Appendix Figure A1). However, the pregnancy itself in year $t = -1$ may affect outcomes, so I limit the trajectory matching to years $t = -4$, -3, and -2. Students must have at least two of the three years of prior data.[13] This trajectory method is called the "individual trajectory match" (abbreviated as IND). The other control variables are the same as in the previous methods.

A second trajectory method adds family trajectories, calculated as the mean scores for the families (not including the individual) in each of years $t = -4$ to -2 before birth. This second trajectory method is called the "individual and family trajectory match" (abbreviated as IND+FAM). In addition to requiring the students to have at least two of the three years of prior data, students must also have at least one sibling with test score data in the trajectory analysis. For this reason, the number of observations is somewhat smaller in the family trajectory analysis.

A final condition requires that the matched control be from the same microneighborhood as the teen aunts/uncles at the first observation in the data. Microneighborhoods are small areas similar to block groups and identified by the county. They contain an average of 103 students per neighborhood per year. This trajectory method is abbreviated as IND+FAM+NBHD.

Online Appendix Table A3 displays the results of the logit models predicting the probability of being a teen aunt/uncle under these various methods.[14] Table 1 displays descriptive statistics for the teen aunts/uncles (Column 1) and their matched control groups (Columns 2–6). Most characteristics of the matched control groups are similar to the teen aunts/uncles, but the prior test scores highlight the difference between the EARLY (Column 2) and trajectory methods (Columns 4–6). All methods produce control groups that are similar to the teen aunts/uncles at $t = -4$. At $t = -2$, however, the teen aunts/uncles' and trajectory controls' scores dropped relative to $t = -4$, while the EARLY matches' scores remained higher. The teen aunts/uncles scored around the 40th percentile at $t = -2$, while the EARLY matches scored 44.5 (p-value of difference between teen aunts/uncles and their EARLY matches = 0.054).

The final column requires the control students to have similar demographic and school characteristics as the teen aunts/siblings at the first time they are observed, to be on a similar trajectory in terms of individual and family test scores for several years, and to be from the same microneighborhood as the teen mothers at the first observation. The microneighborhood requirement may make unobservable local conditions

12. I do not use $t = -1$, because the pregnancy itself may have affected the teen aunts/uncles.

13. For years missing data, the FCAT approximation replaces missing data; otherwise I replace with the closest available prior year. Indicators for missing data are included in the logit model. EARLY and JUST BEFORE matches must have at least two of the prior three years of data to make the results comparable to the trajectory analysis. Online Appendix Figure A1 displays the percent of observations with test score data by year relative to birth for teen aunts/uncles and teen mothers. Similar analysis by alternative timeframes or more/fewer years of data required yield qualitatively similar results.

14. The trajectory models predict that teen aunts/uncles are more likely to be female, be aged 12–14, come from larger families, identify as FRL, identify as Black, and have first attended schools with more FRL students and higher first-observed test scores, holding other factors constant. No siblings are off-support of the control student propensity score distribution.

Table 1
Descriptive Statistics, Matched Controls for Teen Aunts/Uncles

	Teen Aunts/Uncles (1)	EARLY (2)	JUST BEFORE (3)	IND (4)	IND+FAM (5)	IND+FAM+NBHD (6)
Female (percent)	57.527	58.462	58.750	57.981	56.667	52.781
	(3.743)	(1.843)	(1.685)	(1.668)	(1.778)	(1.985)
p		0.847	0.786	0.941	0.835	0.262
Age at birth	13.849	13.929	13.858	13.877	13.837	13.497
	(0.090)	(0.041)	(0.040)	(0.040)	(0.041)	(0.051)
p		0.872	0.557	0.701	0.896	0.001
# of siblings in data	3.016	2.916	2.887	2.872	2.972	2.803
	(0.097)	(0.048)	(0.042)	(0.042)	(0.044)	(0.044)
p		0.763	0.543	0.451	0.677	0.044
FRL (percent)	80.108	80.673	80.577	80.673	80.215	77.535
	(3.212)	(1.395)	(1.308)	(1.314)	(1.373)	(1.631)
p		0.688	0.706	0.685	0.975	0.475
Black (percent)	55.914	55.865	57.500	57.019	56.452	54.417
	(4.238)	(1.959)	(1.719)	(1.699)	(1.874)	(2.137)
p		0.844	0.858	0.946	0.907	0.752
Test score at $t=-4$ (1–100)	45.927	47.395	44.038	45.491	47.207	46.864
	(1.915)	(0.975)	(0.859)	(0.910)	(0.914)	(1.039)
p		0.522	0.304	0.768	0.546	0.667

(*continued*)

Table 1 (*continued*)

	Teen Aunts/Uncles (1)	EARLY (2)	JUST BEFORE (3)	IND (4)	IND+FAM (5)	IND+FAM+NBHD (6)
Test score at $t = -2$ (1–100)	40.205	44.487	40.658	40.044	40.668	42.769
	(1.859)	(0.960)	(0.855)	(0.844)	(0.870)	(1.017)
p		0.054	0.983	0.770	0.821	0.226
School avg. FRL (percent)	47.348	48.021	47.902	48.216	46.845	47.376
	(1.359)	(0.564)	(0.575)	(0.533)	(0.640)	(0.720)
p		0.688	0.754	0.582	0.737	0.985
School avg. Black (percent)	18.893	19.251	19.041	18.953	19.254	18.986
	(0.639)	(0.266)	(0.264)	(0.274)	(0.330)	(0.317)
p		0.561	0.797	0.905	0.616	0.897
Mean school first-observed test (1–100)	55.188	54.831	54.904	54.937	55.118	55.304
	(0.676)	(0.283)	(0.282)	(0.263)	(0.314)	(0.349)
p		0.660	0.739	0.774	0.925	0.879
N	186	969	1,008	1,017	898	804

Notes: Standard errors in parentheses (clustered by family ID). Teen aunts/uncles 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. EARLY matches include matches from non-teen-childbearing families to teen aunts/uncles based on first-observed characteristics. JUST BEFORE matches replace first-observed test scores with scores from two years before birth. IND matches include matches from non-teen-childbearing families to teen aunts/uncles based on three-year test score trends and other observable characteristics. IND+FAM matches add three-year family average score trends. IND+FAM+NBHD matches add the requirement that matches be from the same neighborhood as the teen aunt/uncle at the first observation in the data. Includes p-value of t-test between matches and siblings. Comparison in Columns 5–6 against sibling samples in Column 1; comparison in Columns 2–4 against a slightly larger sample that does not have the family test score requirement. Test scores reported as nationally norm-referenced percentiles (1–100).

equal between the siblings and controls, which should reduce unobserved bias (Cook, Shadish, and Wong 2008). Indeed, Fletcher and Wolfe (2009) show that it is necessary to account for community factors in 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 may be difficult to perfectly match the students on the observable characteristics within the pool of potential controls from the same neighborhood. In the final column the IND+FAM+NBHD matched controls are younger at $t=0$ and from slightly smaller families, relative to the teen aunts/uncles. In the second-to-last column, the IND+FAM matched controls without the neighborhood requirement are similar to the teen aunts/uncles across all observable characteristics; however, the matches in Columns 2–5 may be different from the aunts/uncles on unobservable neighborhood characteristics. The main analysis presents results from multiple matched control groups to show how using different groups changes the estimates, though my preferred estimate is the IND+FAM model due to the consistent similarities across observables.

Using these matched control groups, the main analysis examines several outcomes of interest using ordinary least squares (OLS) regression, as follows:

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

where $TeenAuntUncle_i$ is an indicator variable equal to one if the student is the younger sibling of a teen mother, 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 main outcomes Y_i are observed once in the data (after the birth). The main outcomes examined are the test score in the year of the birth (at $t=0$), whether the student repeated a grade in at least one of the years following the birth, whether the student dropped out after the birth, whether the student encountered the juvenile justice system after the birth, college-going, and whether the student obtained any college certification/degree or a four-year degree.

An additional analysis includes multiple years of test score data, which allows the inclusion of age and individual fixed effects, as follows:

$$(2) \quad Y_{it} = \beta_0 + \beta_1 TeenAuntUncle_i \times PostBirth_t + \gamma_\mathrm{i} + \mu_t + \varepsilon_{it}$$

where $TeenAuntUncle_i \times PostBirth_t$ is equal to one for the teen aunts/uncles after the birth, γ_i is an individual fixed effect, and μ_t is an age fixed effect. The data includes $t=-5$ to $t=+2$. The year before birth ($t=-1$) is excluded because pregnancy itself may be a treatment. The age fixed effects capture year-over-year patterns in the population as a whole. Thus, β_1 provides an estimate of whether the average scores of teen aunts/uncles diverge from what would be expected from a similar control student of the same age, after accounting for individual fixed effects.

IV. Results

The analysis begins by examining whether the paths of the teen aunts/uncles diverge from their matched comparators after the birth. Figure 1 displays the test score patterns for teen aunts/uncles and various potential matches: all controls (that is, no matching), early test scores, recent test scores, and the three trajectory matches. Most

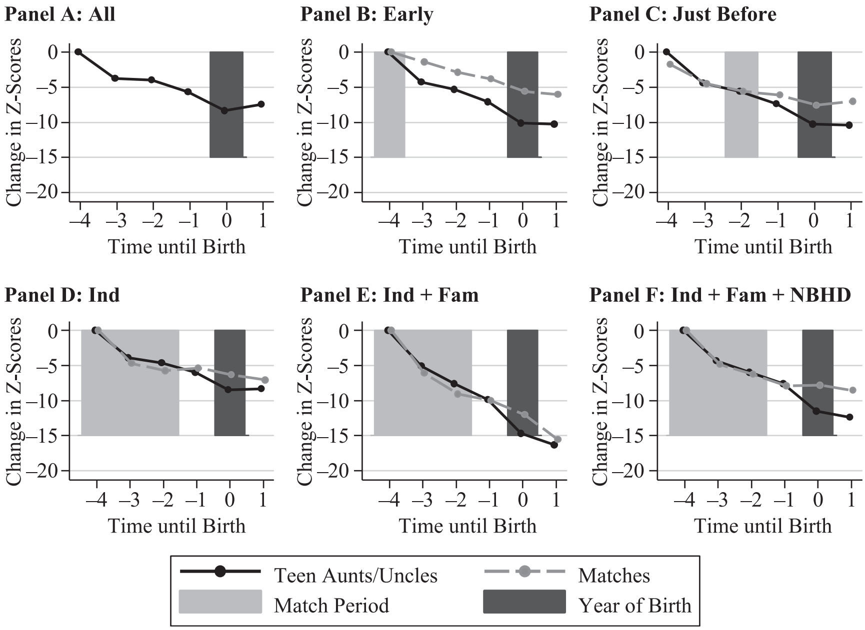

Figure 1

National Percentile Pre- and Post-Trends, by Group for Siblings

Notes: Teen aunts/uncles include all younger siblings from families where an older sister gave birth at age 15–17. EARLY matches include matches from non-teen-childbearing families to teen aunts/uncles based on first-observed characteristics. JUST BEFORE matches replace first-observed test scores with scores from two years before birth. IND matches include matches from non-teen-childbearing families to teen aunts/uncles based on individual three-year test score trends and other observable characteristics. IND+FAM matches add three-year family average score trends. IND+FAM+NBHD matches add the requirement that matches be from the same neighborhood at first observation. Estimates based on a regression of mean test score on years relative to birth (or time relative to the match year for the JUST BEFORE matches) with person and age fixed effects within the noted population. The lighter shaded area marks the timing of the trajectory matching; the darker shading marks the year of birth.

matches are relative to year $t=-4$, while the just before prior match is relative to the siblings at $t=-2$, adjusted to set the teen aunts/uncles equal to zero at $t=-4$. Each line displays the coefficient of a regression of the standardized test score on years relative to birth ($t=-4$ through $t=+1$) within the noted combined treatment and control population, holding individual and age fixed effects constant. The light gray box marks the period of the match used in matching the test scores; the darker gray box highlights the year of birth. To limit compositional effects, I require students to have two of three test scores observed before the pregnancy, per the matching methodology, as well as at least one test score after the birth (in year $t=0$ or $t=1$).[15]

15. This slightly differs from subsequent test score analyses where students would need a score in $t=0$ if I was examining the $t=0$ timeframe.

The solid black (teen aunt/uncle) and gray (match) lines in the top three panels highlight the importance of using trajectory matches. The top left panel displays the estimated patterns when all potential controls are included; the controls contribute to the estimate of age fixed effects. There is no estimate for matches in this panel, as the controls have no time relative to (matched) birth. If there was no difference in performance between teen aunts/uncles and others of the same age, the coefficient for teen aunts/uncles would be zero in each year. Instead, the figure demonstrates decreasing performance over time for the teen aunts/uncles relative to students of the same age. In the second panel, the control group is limited to students who were similar on early test scores and other observable characteristics, and I estimate year coefficients relative to the year of the match for the controls. Here, a null effect would be represented by overlapping lines. After the birth at $t=0$, the estimated difference between teen aunts/uncles and the first-observed matches is quite large, but this divergence began well before the birth and was not entirely caused by the new baby in the home. Using the more recent match from two years before birth might also result in overestimating the negative effect, as the matched controls are on a slightly flatter trajectory than teen aunts/uncles.

The bottom three trajectory match panels summarize the primary analytic strategy. Across all three trajectory-based matches, the lines move together in the four years before birth. The lines diverge after the birth, with a larger drop for the teen aunts/uncles than shown by their matched controls.

The figure also displays how requiring the matched controls to be from the same neighborhood changes the estimates. This requirement limits the number of potential matches, which leads to matches that are less similar to the siblings on some observable characteristics. However, they may be more similar on unobservable characteristics. In the individual and family trajectory models, the post-pregnancy gap between the matched control and the teen aunts/uncles appears to be smaller in the bottom-middle figure (without the neighborhood requirement) than in the bottom-right figure (with the neighborhood requirement). Prior research has also found larger effects of teen childbearing once community-level factors were included (Fletcher and Wolfe 2009), though caution should be taken given that the IND+FAM+NBHD matches were also younger and from smaller families than the teen aunts/uncles.

More formally, Table 2 shows the estimated differences in outcomes between siblings and various matched control options. In most models, the focus is on a particular year for the test scores and whether an event ever occurs after the birth for the other outcomes. Overall, the results are biased towards finding larger effects in the models that do not account for trajectories, with smaller estimated coefficients for the trajectory-matched samples.

The results in the first row again highlight the importance of the trajectory-matching methods, demonstrating that the teen aunts/uncles diverged from their EARLY controls even before the birth. The first two columns are based on early characteristics, without (Column 1) and with (Column 2) matching, and here teen aunts/uncles score a statistically significant –3.7 to –4.6 percentile points lower than their controls in the year before birth.[16] Matching instead on the more proximate test score in $t=-2$, the

16. In Column 1 without matching, $t=0$ is set at the seventh observation in the data for the controls; the mean age is 13.6. This ensures that outcomes that can only happen once (for example, dropping out) are not measured multiple times in the control sample.

Table 2
Estimated Effects of Teen Birth on Various Outcomes for Teen Aunts/Uncles

	Control Group					
	All Younger Siblings (1)	EARLY (2)	JUST BEFORE (3)	IND (4)	IND+FAM (5)	IND+FAM+NBHD (6)
Test scores at $t = -1$	–3.707***	–4.570**	–1.517	–1.701	–1.345	–0.612
	(1.007)	(1.509)	(1.160)	(1.149)	(1.221)	(1.326)
N	37045	1009	1030	1028	947	866
Mean match outcome	59.539	46.282	43.196	42.558	43.023	43.135
Test scores at $t = 0$	–5.483***	–5.932***	–2.925*	–3.166**	–4.222***	–4.390**
	(1.065)	(1.536)	(1.236)	(1.175)	(1.218)	(1.342)
N	29,886	862	878	892	809	755
Mean match outcome	60.028	45.357	42.677	41.997	43.552	45.278
Test scores, with age and individual FE	–4.287***	–2.495*	–3.155**	–2.407*	–2.481*	–3.710***
	(0.915)	(1.048)	(1.040)	(1.069)	(1.077)	(1.101)
Observations	386,713	6,614	7,084	7,096	6,276	5,669
N	85,860	1,095	1,168	1,177	1,029	938
Mean match outcome	60.238	47.032	43.494	42.965	43.851	45.099
Repeats grade in $t = 0$ or later	–4.287***	3.517	1.171	2.441	5.629	2.482
	(0.915)	(3.556)	(3.511)	(3.478)	(3.684)	(3.803)
N	86,265	1,177	1,216	1,225	1,084	990
Mean match outcome	19.966	29.423	31.731	30.673	27.097	28.899

(*continued*)

Table 2 (*continued*)

	Control Group					
	All Younger Siblings (1)	EARLY (2)	JUST BEFORE (3)	IND (4)	IND+FAM (5)	IND+FAM+NBHD (6)
Drops out in $t=0$ or later	8.074***	9.794**	7.172*	7.682**	6.618*	5.350+
	(1.925)	(3.008)	(2.919)	(2.928)	(3.008)	(3.171)
N	48,793	1,177	1,216	12,25	1,084	990
Mean match outcome	9.839	13.462	15.385	15.096	15.484	13.959
Juvenile justice in $t=0$ or later	7.694***	5.036*	6.027*	6.402*	5.143*	5.986*
	(1.841)	(2.536)	(2.494)	(2.515)	(2.603)	(2.823)
N	48,793	1,177	1,216	1,225	1,084	990
Mean match outcome	3.086	8.077	7.308	7.212	7.849	6.870
Ever attends any college	−13.048***	−9.655*	−6.761	−4.267	−5.360	−9.679+
	(3.369)	(4.714)	(4.501)	(4.542)	(4.846)	(5.085)
N	36,612	815	840	826	740	664
Mean match outcome	66.130	59.651	55.660	51.939	56.621	59.906
Obtains any degree or certificate	−4.526+	−4.546	−3.511	−2.940	−5.536	−3.526
	(2.731)	(3.980)	(3.884)	(3.762)	(4.049)	(4.087)
N	36,612	815	840	826	740	664
Mean match outcome	38.368	25.871	23.854	22.853	26.180	25.314

(*continued*)

Table 2 (*continued*)

	Control Group					
	All Younger Siblings (1)	EARLY (2)	JUST BEFORE (3)	IND (4)	IND+FAM (5)	IND+FAM+NBHD (6)
Obtains a four-year degree	–5.334*	–5.192	–3.806	–3.961	–5.252	–3.283
	(2.111)	(3.276)	(3.153)	(3.086)	(3.328)	(3.463)
N	36,612	815	840	826	740	664
Mean match outcome	28.142	17.024	14.690	14.681	17.047	15.723
First-obs. controls	Y	Y	Y	Y	Y	Y
First-obs. test score	Y	Y	N	N	N	N
Test score at $t = -2$	N	N	Y	Y	Y	Y
Test scores $t = -4$ to -2	N	N	N	Y	Y	Y
Fam. Scores $t = -4$ to -2	N	N	N	N	Y	Y
Match from neighbors	N	N	N	N	N	Y

Notes: Robust standard errors clustered by family ID. Analyses include all controls from Table 1. Column 1 includes all children from non-teen-childbearing families as controls, with control students' $t = 0$ set at their seventh observation (mean age = 13.6). Column 2 includes matches to younger siblings from non-teen-childbearing families to the teen aunts/uncles based on the first-observed characteristics, including first-observed test scores. Column 3 replaces first-observed test scores with scores from two years before birth. Column 4 replaces one observed test score with three-year test score trends ($t = -2$, -3, and -4). Column 5 adds three-year family average test score trends. Column 6 adds the neighborhood requirement. Scores at $t = 0$, repeats grade, drops out, juvenile justice, and college-going outcomes include one weighted observation from the siblings and their controls. Column 1 test scores at $t = 0$ include multiple observations per individual control. Fixed-effects models include all available observations from $t = -5$ to $t = +2$ from the siblings and their noted controls, excluding $t = -1$. Test scores reported as nationally norm-referenced percentiles (1–100). The mean for the preferred IND+FAM control group scores at the percentile 43.6 in $t = 0$, with 27.1 percent repeating a grade in any year following the birth, 15.5 percent dropping out following the birth, 7.8 percent juvenile justice system exposure following the birth, 56.6 percent college attendance, 26.2 percent obtaining any degree or certificate, and 17.0 percent obtaining a four-year degree.

estimated effect is a null −1.5 percentile points. The trajectory-based matches are similarly null, ranging from −0.6 to −1.7 percentile points.

Once the baby arrives, test scores are estimated to be lower for the teen aunts/uncles in the year following the birth across all estimates. The estimates are −5.5 to −5.9 percentile points for the models using early characteristics. However, using trajectory-based matches, the estimated drop in scores is −3.2 to −4.4 percentile points. The coefficients in Columns 5 and 6 are about 28 percent smaller than the coefficient in Column 2. The estimated effect is relative to a mean percentile score of 43.6 for the IND+FAM control group. The fixed-effect method finds a similar average drop of −2.4 to −3.7 percentile points for all post-birth test scores in the trajectory models; this is interpreted as the average difference from the trajectory controls after the birth. The general picture that emerges in the test score analysis is that there is a negative test score effect when a sibling gives birth, but it is smaller than what would be estimated without accounting for preexisting trajectories.[17]

When using the trajectory matching, I find no effect for grade repetition, a 5.4–7.7 percentage point increase in high school dropout (with significance ranges from the 1 percent to 10 percent level), and a 5.1–6.4 percentage point increase in exposure to the juvenile justice system. Higher dropout among teen aunts/uncles could mean that the test score analysis is biased due to compositional differences in who had testing data available. However, I do not find evidence that high school dropout substantially changes the estimates for test scores in $t=0$, mainly because most dropouts happen in year $t=+1$ or later (that is, after I measure the test score outcome).[18] The increases in dropout and juvenile justice exposure are economically significant, producing a 43 percent increase over the control group mean of 15.5 percent for the analysis based on individual and family trajectories. Similarly, teen aunts/uncles have a 65 percent increase in juvenile justice exposure over the control group mean of 7.8 percent.

The estimated effects are generally negative but statistically insignificant for college-going once the analysis accounts for preexisting trajectories (see final three rows).

A. Patterns for Teen Aunts/Uncles by Sex, Race, and FRL Status

Different subgroups may have different effect sizes. Perhaps lower-income families have fewer financial supports available for childcare than higher-income families. Or, perhaps low-income and Black communities, which have a higher prevalence of multigenerational families, offer supports to handle teen childbearing (Burton 1999; Fuller-Thomson, Minkler, and Driver 1997). Teen aunts may be expected to help with children more than teen uncles (East 1998), and a reduction in monitoring by the new grandparents (East 1999) may affect the sexes differently. Table 3 explores patterns by sex,

17. Online Appendix Table A5 includes alternative outcomes. The results are similar in the OLS model whether converting the outcomes to z-scores, looking only at math or reading, and imputing outcome test scores using the FCAT. For the FE model, results are similar whether converting outcomes to z-scores or imputing using the FCAT. The coefficient is larger for math than reading. The results are also similar when requiring the potential controls to have an older sister rather than an older sibling, though the number of potential matches is also lower.

18. Online Appendix Figure A2 and Online Appendix Table A5 provide additional information on the timing of dropouts and a bounding exercise that assumes all missing tests scored the minimum observed score on the test.

Table 3
Estimated Effects of Teen Birth on Various Outcomes for Teen Aunts/Uncles by Subgroups

	Baseline (IND+FAM) (1)	Female (2)	Male (3)	Black (4)	Non-Black (5)	FRL (6)	Non-FRL (7)
Test scores at $t=0$	–4.222***	–4.345**	–4.001*	–4.948**	–2.366	–4.646***	–2.517
	(1.218)	(1.563)	(1.837)	(1.586)	(1.957)	(1.343)	(3.021)
N	809	459	350	454	355	641	168
Mean match outcome	43.552	45.191	41.439	34.632	55.737	39.911	57.682
Test scores, with age and individual FE	–2.481*	–3.838**	–0.533	–3.195*	–1.491	–2.162+	–3.319
	(1.077)	(1.378)	(1.818)	(1.441)	(1.589)	(1.183)	(2.531)
Observations	6,276	3,608	2,668	3,340	2,936	4,907	1,369
N	1,029	580	449	547	482	810	219
Mean match outcome	43.851	45.343	41.810	35.749	54.461	40.430	57.323
Repeats grade in $t=0$ or later	5.629	7.759+	3.073	3.355	10.537*	3.488	18.333*
	(3.684)	(4.551)	(5.950)	(5.089)	(5.114)	(4.085)	(7.761)
N	1,084	611	473	598	486	864	220
Mean match outcome	27.097	21.822	33.995	35.048	16.790	30.831	11.957
Drops out in $t=0$ or later	6.618*	9.331*	2.675	4.454	10.467*	7.627*	–0.609
	(3.008)	(4.239)	(4.348)	(3.821)	(4.622)	(3.458)	(4.519)
N	1,084	611	473	598	486	864	220
Mean match outcome	15.484	14.231	17.122	17.714	12.593	16.622	10.870

(*continued*)

Table 3 (*continued*)

	Baseline (IND+FAM) (1)	Female (2)	Male (3)	Black (4)	Non-Black (5)	FRL (6)	Non-FRL (7)
Juvenile justice in $t=0$ or later	5.143*	2.179	9.240+	4.443	4.925	4.386	6.834+
	(2.603)	(2.369)	(4.957)	(3.633)	(3.138)	(2.909)	(4.122)
N	1,084	611	473	598	486	864	220
Mean match outcome	7.849	3.985	12.903	11.048	3.704	9.115	2.717
Ever attends any college	–5.360	–5.182	–5.672	–8.082	–1.693	–3.416	–15.761+
	(4.846)	(6.236)	(6.970)	(6.904)	(6.517)	(5.610)	(8.830)
N	740	432	308	389	351	574	166
Mean match outcome	56.621	61.082	50.186	57.500	55.556	53.282	69.065
Obtains any degree or certificate	–5.536	–6.981	–3.671	–6.722	–3.352	–6.499	–4.158
	(4.049)	(5.740)	(5.264)	(5.179)	(6.308)	(4.223)	(9.679)
N	740	432	308	389	351	574	166
Mean match outcome	26.180	29.897	20.818	22.222	30.976	22.201	41.007
Obtains a four-year degree	–5.252	–7.866+	–2.477	–6.483+	–3.090	–7.025*	–0.134
	(3.328)	(4.546)	(4.533)	(3.393)	(5.694)	(3.011)	(9.803)
N	740	432	308	389	351	574	166
Mean match outcome	17.047	20.103	12.639	12.222	22.896	13.707	29.496

Notes: Robust standard errors clustered by family ID. Column 1 is the preferred IND+FAM trajectory estimate from the Table 2; later columns repeat the analysis by subgroups. Test scores reported as nationally norm-referenced percentiles (1–100).

race (Black versus non-Black), and FRL status. To be conservative, I use the IND+FAM trajectory as a baseline, as I know that the teen aunts/uncles and their matches are similar on all observable baseline characteristics. The estimates using the IND+FAM+NBHD trajectory are available in Online Appendix Table A6. The Table 2 estimate is included in Column 1 for reference.

Teen aunts consistently have larger academic effects than teen uncles, though Hausman tests of the difference between estimates find no statistically significant differences.[19] One exception to the broad pattern is in juvenile justice, where teen uncles are 9.2 percentage points more likely to enter the juvenile justice system after the birth, compared to boys on a similar trajectory pre-birth. The effect for teen aunts is a 2.2 percentage point increase (p-value of difference using the Hausman test $=0.198$). Note that these changes occur on different margins, as 12.9 percent of boys in the matched control group are ever exposed to the system, compared to 4.0 percent of girls.

The estimated effects on most of the high school outcomes are generally similar in the Black versus non-Black and FRL versus non-FRL comparisons, and no group consistently has larger effects across outcomes.

B. Patterns by Baseline Test Scores

I also examine results by baseline test score distribution. In Figure 2, the horizontal axes are the test score at the first observation in the data, and the vertical axes are various post-birth outcomes for teen aunts/uncles and their IND+FAM matched controls. The gap in the horizontal lines is the unadjusted mean difference between these groups. The gray vertical lines divide the baseline scores into tertiles. From this analysis, it appears that the difference in test scores at $t=0$ between the treated and control students are evenly spread across the baseline scores, while there is a difference in post-birth grade repetition only on the lower end of the distribution. Dropout is larger in the middle and top tertiles, while the juvenile justice exposure effect is largest at the top of the distribution. The largest differences in the college outcomes appear in the middle group.

These broad conclusions are generally supported by a more formal OLS estimate by tertile (see Online Appendix Table A7), though the differences across tertile are only close to statistically significant at traditional levels under the IND+FAM model for juvenile justice exposure (p-value of the Hausman test $=0.141$). Note that the margins are different across groups; for instance, few (36.0 percent) of the low-tertile and many (72.9 percent) of the high-tertile students attend college in the control group. Overall, I take this as evidence that a sample with more power might detect differences, but I cannot confidently say that effects are driven by one group or another.

C. Placebo and Robustness Checks

Table 4 contains several additional checks and estimates, including two placebo tests that verify that the matching process does not mechanically create the estimated

19. The larger effects for teen aunts relative to teen uncles are not driven by teen aunts who eventually become teen mothers. The exception is for grade repetition, which is higher among eventual teen mothers. I also find no difference in estimates by family composition in terms of numbers of brothers and sisters in the family.

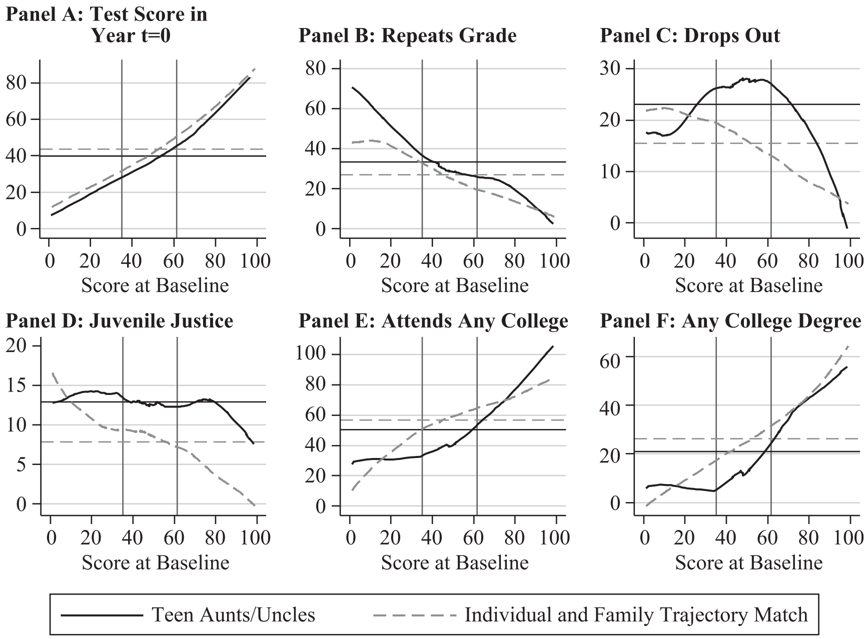

Figure 2

Distribution of Test Scores Following Birth for Teen Aunts/Uncles and Matched Controls

Notes: Displays a locally weighted regression of the outcome (y-axis, noted in the panel header) and first-observed test scores (x-axis) using Stata's *lowess* command. Teen aunts/uncles include all siblings from families where an older sister gave birth at age 15–17. Horizontal lines represent the unadjusted overall mean for the outcome for siblings (black solid line) and their matches (gray dashed line) based on IND+FAM matches. Vertical gray lines divide first-observed test scores into tertiles.

effects.[20] The first placebo test retroactively predicts differences in test scores, grade repetition, and juvenile justice exposure two years pre-birth (Column 1). The second placebo test randomly selects 200 students from nonchildbearing homes who were matched to teen aunts/uncles in the main analysis (Columns 2 and 3). I include these students and any nonoldest siblings from their families as placebo aunts/uncles. Each placebo aunt/uncle is matched to similar students from the remaining sample of younger siblings from non-teen-childbearing families. All placebo estimates are null at the 5 percent level of statistical significance. Grade repetition is statistically significant at the 10 percent level but in the opposite direction.

The final column of Table 4 examines aunts/uncles who are older than 18 when their sister gives birth. These aunts/uncles are older than their childbearing sisters. They may

20. Additionally, Online Appendix Figure A3 includes a check on how the estimates change as I match to more or fewer control students.

Table 4
Estimated Effects of Teen Birth on Various Outcomes for Alternative and Placebo Treatment Groups

	Placebo: Pre-Scores for Teen Aunts/Uncles ($t = -2$) (IND+FAM) (1)	Placebo: Nonchildbearing Families (IND+FAM) (2)	Placebo: Nonchildbearing Families (IND+FAM+NBHD) (3)	Alternative: Older Siblings as Treated (IND+FAM) (4)
Test scores	0.321	0.199	–0.614	
	(1.111)	(1.085)	(1.191)	
N	1,009	1,140	990	
Repeats grade	0.704	–5.216+	–5.119+	
	(0.954)	(2.917)	(3.044)	
N	1,078	1,696	1,392	
Drops out		–0.496	–0.130	
		(2.351)	(2.292)	
N		1696	1392	
Juvenile justice	–0.106	–1.470	–3.474+	
	(0.108)	(1.628)	(1.801)	
N	1,078	1,696	1,392	
Ever attends any college		1.857	0.980	–25.530+
		(3.839)	(4.121)	(14.174)
N		1,174	941	80
Obtains any degree or certificate		1.209	–0.674	17.431
		(3.261)	(3.446)	(10.750)
N		1,174	941	80
Obtains a four-year degree		–0.240	0.500	5.731
		(2.719)	(2.889)	(11.346)
N		1,174	941	80

Notes: Robust standard errors clustered by family ID. All analyses based on the trajectory matches with other controls and same neighborhood requirement. Column 1 conducts the analysis for the *older* siblings of teen mothers. These teen aunts/uncles are over 18 years old. I use an IND+FAM match because the IND+FAM+NBHD model has very low *N*. I use the six, seven, and eight years before the birth to estimate the trajectory, as these are the years with at least 50 percent of the test score data for the siblings who are 18 or older at the time of the birth. Outcomes are post-secondary, though it is possible that the siblings may have started or completed college by the time of birth. The outcome is Column 2 examines the outcomes for the IND+FAM control group two years before the birth. Columns 3 and 4 conduct the IND+FAM and IND+FAM+NBHD analysis using 200 randomly selected families without teen childbearing in their family who had been used in the IND+FAM analysis as controls. These former controls are treated as a placebo group and matched to a new set of matches, and the analysis displays the comparison between these placebos and their matched controls.

have moved out of their parents' house and may thus be less affected by the birth than siblings who remain in the home. Because these siblings are out of high school, the analysis must go farther back in time to find test scores for the trajectory matching. I use the years −7, −6, and −5 years relative to birth for the matching procedure. The number of observations here is low ($N = 80$) because it requires a long timeframe of observations. The outcomes are college-going/college completion, which are not ideal because these over-18-year-olds may have started college before the baby was born. However, it is the best available outcome for the older aunts/uncles. The results are noisy but, if anything, suggest that the birth may decrease college attendance by 25.5 percentage points (95 percent confidence interval: [–53.7 percentage points, 2.7 percentage points]).

D. Teen Time Use

I use data from the American Time Use Survey (Hofferth, Flood, and Sobek 2017) to examine how teens who live with a young child in their household differ in their time use, relative to those who do not. I restrict the analysis to 15-, 16-, and 17-year-olds to match the age range used in the main analysis. I create a proxy for teen aunts/uncles, based on whether a respondent lives with a child under five years old who is not their sibling or their own child, but is related to them in some other way. The control group includes families without such a relative. I exclude respondents who live with their own child. See the Online Appendix for more data details.

I examine how respondents spend their time and with whom they spend their time in a 24-hour period. I control for sex and other characteristics and estimate the additional time spent on various activities for teen aunts/uncles. I also test whether the gap between teen aunts and other females differs from the gap between teen uncles and other males. I group activities into categories: sleep, school, work, childcare as a primary activity, all childcare, time with friends, and time with family. The first four activities (sleep, school, work, and childcare) are mutually exclusive.

Table 5 displays the analysis. Panel A includes all data, while Panels B and C break the analysis into weekdays and weekends/holidays. Times spent on sleep, school, and work do not differ for the teen aunts/uncles in Panel A. Teen aunts spend 22 more minutes per day on childcare as a primary activity than other females, holding other observable factors constant. There is no difference between teen uncles and other males, but the estimated difference between teen aunts and teen uncles is not statistically significant (p-value = 0.249). Adding childcare as a secondary activity, teen aunts spend 1.9 more hours per day on childcare than other females. There is no difference between teen uncles and other males, and the estimated coefficients for teen aunts and teen uncles statistically differ (p-value = 0.001). Teen aunts also spend more than an hour less per day with friends than other females. There is no difference between teen uncles and other males, and estimated coefficients for teen aunts and teen uncles statistically differ (p-value = 0.087).

Splitting the analysis by weekdays and weekends/holidays, teen aunts spend more time than others on childcare during both. Teen aunts also spend 26 fewer minutes on schoolwork on weekends, relative to other females, and the estimated coefficients for teen aunts statistically differs from that for teen uncles (p-value = 0.051). The difference in teen aunt time with friends is driven by the weekends, perhaps because most of the weekday time with friends occurs during the school day. Teen aunts are also less likely to be with their parents on the weekends.

Table 5
Time Use for Teen Aunts/Uncles and Other Teenagers

	Sleep (Primary) (1)	School (Primary) (2)	Work (Primary) (3)	Childcare (Primary) (4)	Any Childcare (5)	With Friends (6)	With Parents (7)
Panel A: All Days							
Teen uncle	4.812	–2.664	2.885	7.019	–3.231	–2.099	21.606
	(20.904)	(20.004)	(16.351)	(7.076)	(14.486)	(30.464)	(26.865)
Teen aunt	7.400	–14.723	11.049	22.119*	111.989***	–70.723**	–31.444
	(21.835)	(19.940)	(15.398)	(11.024)	(30.928)	(26.194)	(21.306)
p(aunt = uncle)	0.932	0.668	0.715	0.249	0.001	0.087	0.121
Male mean	586.635	156.395	43.539	5.794	38.573	219.328	156.607
Female mean	581.855	168.880	44.051	10.471	55.714	215.013	180.022
Observations	7,440	7,440	7,440	7,440	7,440	7,440	7,440
Panel B: Weekdays							
Teen uncle	22.501	–27.047	14.259	2.175	–7.604	–32.508	–15.973
	(35.565)	(45.167)	(32.111)	(3.348)	(9.058)	(43.608)	(17.631)
Teen aunt	–15.085	–3.268	13.889	10.758	131.283**	–21.370	5.846
	(31.163)	(32.581)	(17.805)	(7.643)	(43.421)	(38.060)	(25.514)
p(aunt = uncle)	0.427	0.669	0.992	0.303	0.002	0.847	0.479
Male mean	534.053	300.695	40.392	4.632	30.575	219.358	104.021
Female mean	531.730	298.952	39.224	9.768	48.694	218.891	125.451
Observations	3,480	34,80	3,480	3,480	3,480	3,480	3,480

(*continued*)

Table 5 (*continued*)

	Sleep (Primary) (1)	School (Primary) (2)	Work (Primary) (3)	Childcare (Primary) (4)	Any Childcare (5)	With Friends (6)	With Parents (7)
Panel C: Weekends							
Teen uncle	−3.030	−1.193	−0.351	11.133	1.958	23.102	51.163
	(25.704)	(9.614)	(17.228)	(11.400)	(23.431)	(41.738)	(42.550)
Teen aunt	32.564	−25.852**	5.817	34.492	88.105*	−123.765***	−71.819*
	(29.390)	(8.314)	(25.636)	(21.539)	(42.622)	(29.212)	(33.548)
p(aunt = uncle)	0.361	0.051	0.841	0.339	0.076	0.004	0.023
Male mean	631.802	32.447	46.242	6.793	45.444	219.303	201.776
Female mean	627.025	51.668	48.401	11.105	62.041	211.519	229.198
Observations	3,960	3,960	3,960	3,960	3,960	3,960	3,960

Notes: Robust standard errors in parentheses. Outcomes are time use in minutes. Sleep, school, work, and childcare as primary activities are mutually exclusive. Any childcare time (Column 5) includes any time spent on childcare as a primary or secondary activity. Time with friends and parents are time spent on any activity where the respondent reported those individuals were present population includes 15-, 16-, and 17-year-old American Time Use Survey respondents who do not live with their own child (a proxy for nonparents). Displays the estimated difference in time use for the aunts/uncles of young children who live in the same home relative to other teens. Teen aunts/uncles identified as respondents with related, non-own, non-sibling child under the age of five in their household. Analysis controls for sex, own mother's education, race/ethnicity, metropolitan status, age fixed effects, day of the week, an indicator for holidays, and month fixed effects. Panels B and C split the analysis by week days and weekend/holidays, respectively.

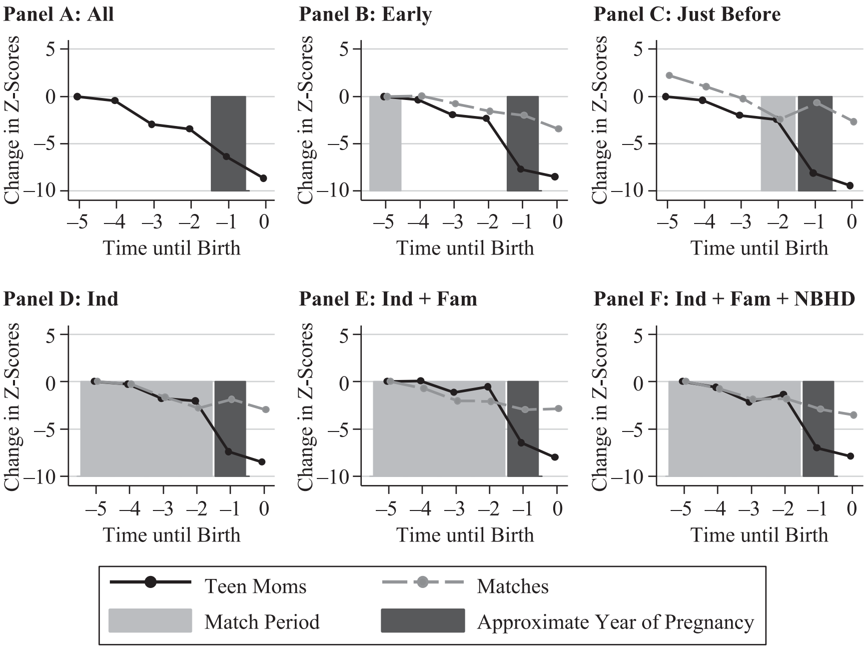

Figure 3

National Percentile Pre- and Post-Trends, by Group for Teen Mothers

Notes: Teen mothers include all females who gave birth at age 15–17. EARLY matches include matches from non-teen-childbearing families to teen mothers based on first-observed characteristics. JUST BEFORE matches replace first-observed test scores with scores from two years before birth. IND matches include matches from non-teen-childbearing families to teen mothers based on individual three-year test score trends and other observable characteristics. IND+FAM matches add three-year family average score trends. IND+FAM+NBHD matches add the requirement that matches be from the same neighborhood at first observation. Estimates based on a regression of mean test score on years relative to birth (or time relative to the match year for the JUST BEFORE matches) with person and age fixed effects within the noted population. The lighter shaded area marks the timing of the trajectory matching; the darker shading marks the year of birth.

E. Teen Mothers

A complementary analysis uses a strategy similar to the main teen aunt/uncle analysis to examine whether the birth also changes the trajectories of teen mothers. The full analysis is available in the Online Appendix. I show that teen mothers, like the teen aunts/uncles, are on a downward trajectory relative to other students of the same age. However, the downward pattern exhibited by teen mothers could change following the pregnancy or the birth of the child. Figure 3 displays the test score patterns for teen mothers and their matches. Each line displays the coefficients from regressions of national percentile rank on years relative to birth ($t=-5$ through $t=0$) within the noted combined treatment and control population, holding individual and age fixed effects constant. The control groups are created using a similar matching methodology as conducted for the teen aunt/uncle analysis. The light gray box marks the matching period, while the darker gray box indicates the approximate school year the pregnancy began. To be included in the figure, the students had to have the required test scores from before the pregnancy and a test score observed in $t = 0$.

While the black and gray lines (representing teen mothers and their matches, respectively) move together in the years used in the trajectory-based matching, there is a divergence in the year closest to the pregnancy ($t = -1$), with the teen mothers increasing their decline in test scores. The test scores remain low for teen mothers in $t = 0$, the year of the birth.

The teen mother estimates are interesting by themselves, but they also provide a useful check on the causality of the teen aunt/uncle analysis. Since the divergence from their respective matched comparators occurs at different times for the teen mothers and teen aunts/uncles, this provides evidence to rule out the possibility that a family-specific negative shock led to both the teen's pregnancy and the test score declines of her siblings.

V. Discussion

Children in families where teen motherhood occurred had downward trajectories in test scores that began well before the pregnancy. These patterns did not occur in nonchildbearing families, on average. However, not every family with a downward trajectory experienced teen motherhood. When the siblings of teen mothers were connected to students on a similar trajectory, there appeared to be negative spillovers of the birth to the siblings of teen mothers, with especially poor outcomes for the sisters of teen mothers.

The analysis can be interpreted as causal if accounting for 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 led to teens giving birth. For instance, perhaps parental job loss increased the chance that older females had a child, and parental job loss was also associated with poor outcomes for all siblings. Then, the underlying cause of the poor outcomes would be the job loss, not the teen birth itself.

Teen mothers began their drop in performance in year $t = -1$. If some external event led to both pregnancy and dropping scores in the family, then the drops in performance should have occurred in the same year for teen mothers and teen aunts/uncles. Instead, the teen aunts/uncles and their matched controls continued on parallel trends in the year of the pregnancy; the two diverged only after the birth in $t = 0$. This differential pattern decreases concerns about common shocks to the family and instead supports, at least for the teen aunts/uncles, that the baby's appearance in the home affected outcomes.

I do not take the teen mother analysis as causal. Because there is no differentiation in timing between pregnancy and the drop in scores, scores may drop due to pregnancy—or the pregnancy may be caused by some other unobserved shock. The estimates in the teen mother analysis are larger than in prior literature that used miscarriage as an instrument (for example, Fletcher and Wolfe 2009). To some extent, comparing completed pregnancies to miscarriages may understate the true effect of motherhood relative to no pregnancy, given that many teen mothers want to keep their babies and that miscarriage is associated with long-term psychological repercussions, such as elevated anxiety and depression (Edin and Kefalas 2011; Lok and Neugebauer 2007; Hunfeld, Wladimiroff, and Passchier 1997). Still, I mainly use the teen mother analysis to complement the timing analysis in the teen aunt/uncle analysis and to highlight potential problems with sibling fixed-effects models in the following section.

The effects are large for the younger siblings of teen mothers, especially for the teen aunts. The effect sizes for teen aunts/uncles found here are similar to or smaller than arguably causal estimates of teen birth for teen mothers. As a comparison, Fletcher and Wolfe (2009) found a decrease of 8.0–9.2 percentage points in high school diploma receipt for teen mothers, relative to teens who had a miscarriage.[21] Here, I found a 9.3 percentage point increase in dropout for teen aunts (over a baseline of 14.2 percentage points). The effect is a null 2.7 percentage points for teen uncles (over a baseline of 17.1 percentage points). I take the estimates as large, particularly for females, but not implausibly so, especially given that the burden of childcare is more likely to fall on the younger sisters than younger brothers of teen mothers.

A. Sibling Fixed-Effects Models

Prior work using sister fixed effects would understate the true effect for teen mothers if there are spillovers to the mothers' siblings (for example, Geronimus and Korenman 1992). To highlight this problem, I take the females from families with at least one sister who does and one sister who does not give birth as a teen. I can then run a sibling fixed-effects estimate, making the comparison between teen mothers and teen aunts in the same family (see Table 6). The sibling fixed-effects analysis is limited to outcomes where all family members have data to keep sample composition comparable; it excludes test score and college-going data due to a low number of families meeting these criteria.

The dropout estimate highlights the potential problem with sibling fixed-effects analysis with spillovers. Under my primary analytic strategy, I estimate that teen mothers have a 19.4 percentage point increase in dropout compared to females who had been on a similar trajectory. The within-family estimate instead compares the outcomes of the teen mothers to their sisters, finding a much smaller 9.1 percentage point increase in the likelihood of dropping out for teen mothers, relative to their non-childbearing sisters. However, my main analysis indicates that the sisters of teen mothers are also affected by the baby (with a 9.3 percentage point increase in dropout). The spillovers to the sisters works against the size of the coefficient for teen mothers in the sibling fixed-effect analysis, and the 9.1 percentage point sibling fixed-effects estimate is similar to the difference in point estimates between teen mothers and their sisters in the trajectory analysis (that is, 19.4 – 9.3 = 10.1 percentage points).

Grade repetition has a similar pattern. For juvenile justice exposure, the small increase in juvenile justice exposure for the younger sisters of teen mothers could contribute to the appearance of protective effects of teen motherhood under the sibling fixed-effects model. Sibling fixed-effects effects are generally interpreted as the effect of teen pregnancy, not

21. When limiting to teens who did not want to get pregnant (as proxied by being on birth control), the effect was 7.3–11.9 percentage points, but also not statistically significant in this subgroup.

Table 6
Comparison of IND+FAM Matching and Sibling FE Estimates

	Teen Moms, IND+FAM (1)	Teen Aunts, IND+FAM (2)	Sibling Comparison OLS (3)	Sibling Comparison FE (4)
Panel A: Repeats Grade in $t = 0$ or Later				
Teen mothers	13.464***		3.356	10.317
	(3.540)		(5.596)	(6.325)
Teen aunts		7.759+		
		(4.554)		
N	1,334	611	529	529
Groups				242
Sibling difference		5.705		10.317
Panel B: Drops out in $t = 0$ or Later				
Teen mothers	19.425***		23.277***	9.106
	(3.258)		(5.869)	(6.509)
Teen aunts		9.337*		
		(4.237)		
N	1,334	611	529	529
Groups				242
Sibling difference		10.088		9.106
Panel C: Juvenile Justice in $t = 0$ or Later				
Teen mothers	−0.431		−9.194**	−11.149**
	(0.908)		(2.971)	(3.777)
Teen aunts		2.146		
		(2.368)		
N	1,334	611	529	529
Groups				242
Sibling difference		−2.577		−11.149

Notes: Robust standard errors clustered by family ID. Column 1 is the preferred IND+FAM trajectory estimate for teen mothers from Table 5; Column 2 is the preferred IND+FAM trajectory estimate for teen aunts (the sisters of teen mothers) from Table 3. Column 3 runs an OLS estimate on the population of sets of sisters under age 18 where at least two sisters have the outcome data for a given row. Controls are the same as in the IND+FAM models. Column 4 uses the same population to conduct a sibling FE estimate with controls for age. The difference row displays the difference between the teen mother and teen aunt estimates (Column 1 minus Column 2) and the estimate from the sibling FE model (Column 4). Analysis does not examine test scores or college outcomes due to limited post-birth overlap in sibling pairs.

the additional effect of teen pregnancy beyond the spillover effects. In other words, sibling fixed-effects models may understate the true effect of a shock in cases of spillover.

B. Falling Birth Rates

Teen birth rates continue to fall in the United States (National Center for Health Statistics 2018). The teen birth rate in the anonymous county was around 25 per 1,000 women aged 15–17 in 2000; this was below the Florida average at the time (29.2 per 1,000 women aged 15–17) and the United States overall (26.9 per 1,000), but much higher than the U.S. average as of 2016 (8.8 per 1,000). The results here then represent the outcomes in a somewhat-advantaged county at a time of higher teen birth rates. Thus, the results are perhaps currently most applicable to places with higher birth rates (for example, Washington, DC, at 18.1 per 1,000, or New Mexico and Texas, at 15.1 per 1,000, as of 2016). The results also may be important if future programs and policies reverse progress made in reducing teen fertility rates; then, an accounting of the cost of such a reversal should include the spillover effects in the whole family.

Moreover, I found negative effects even for those in the bottom tertile of performance. Family spillover effects likely continue to occur in the present low-birth rate environment, even if it is the least-advantaged who have continued to have teen births. Examining sibling spillovers in the present context is a useful area for future study.

VI. Conclusion

The primary contribution of this analysis is to examine how teen fertility affects the younger siblings of teen mothers. Using several matched control methods, I show that teenage birth leads to a break in the trajectories of the younger siblings, particularly for younger sisters, who have a 3.8 percentage point decrease in test scores once the baby appears in the home. Relative to females who had been on a similar trajectory, these teen aunts have a 7.6 percentage point increase in grade repetition and a 9.3 percentage point increase in high school dropout following the arrival of the baby. Teen uncles have a 9.2 percentage point increase in juvenile justice system exposure.

Additionally, the patterns I document provide important warnings for researchers. The families of eventual teen mothers were on a downward trajectory well before birth, and I demonstrate that analyses that do not account for downward trajectories will overestimate the negative effects of teen pregnancy on the teen mothers and their siblings. Moreover, analyses using sibling fixed effects can understate the effects of teen pregnancy. Unexpected family spillovers are a cautionary note to researchers in topics beyond teen pregnancy. Given the popularity of sibling fixed effects in economics and other disciplines, researchers should carefully consider pathways by which the direct policy or change of interest may have spillovers on the family.

A supplementary analysis examines time use by using a proxy for sibling childbirth. Teen aunts spend more time on childcare and less on homework than either peer females or the teen uncles. These differences in time allocation provide suggestive evidence to explain the difference in academic outcomes for the teen aunts and uncles.

A limitation of this study is that I may underidentify teen aunts/uncles. This may lead to an underestimate of the effects if some unidentified teen aunts/uncles were in the control group. Future studies with less measurement error may have larger estimated effects. Additional research should also examine these patterns with data from an era of lower teen birth rates.

Overall, the findings indicate that teen motherhood had short- and long-term negative effects on the siblings of teen mothers. These findings highlight the importance of considering not just the teen mother and her child but potentially her whole family in any assessments of the costs of teen pregnancy. Future research should invest in examining how teen pregnancy and its interventions affect the whole family, including the siblings and parents of teen mothers. Current estimates may understate the true cost of teen pregnancy to families and society.

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