Every Year Counts: The Long-Run Consequences of Pregnancy Timing among Teenagers*

Jinyeong Son†

September, 2024

Abstract

While the teen pregnancy rate is high in the US relative to other countries, a notable change has occurred, with these pregnancies shifting toward later teenage years. However, little is known about the consequences of pregnancy timing among teenagers. This paper seeks to fill this gap in the literature by studying the effects of pregnancy timing among teenagers aged 15–18 on their short- and long-run educational and labor market outcomes. Specifically, I estimate the marginal impact of a one-year difference in pregnancy timing for each age interval—15–16, 16–17, and 17–18—leveraging linked administrative data from Texas. To identify the effect of pregnancy timing, this paper examines both within-individual changes in outcomes surrounding pregnancy and across-individual comparisons in outcomes after pregnancy, among matched individuals who are balanced on a wide range of characteristics but differ in the timing of pregnancy. The results indicate that experiencing pregnancy one year earlier increases absences and the likelihood of leaving school, particularly during the postpartum year. Further, the results indicate that becoming pregnant one year earlier has adverse long-term consequences: it reduces high school graduation by age 20, decreases college enrollment and completion in the early 20s, and leads to lower employment and earnings in the mid-20s, with these detrimental effects being most pronounced for the youngest group. Finally, I present suggestive evidence that providing parental support to teenage mothers during the postpartum year could mitigate the short-term disruptions they face, such as increased absences and higher dropout rates.

*This paper is a revised version of the first chapter of my Ph.D. dissertation at the University of Texas at Austin. I am sincerely grateful to my advisor, Marika Cabral, for her constant guidance and support throughout this project. I also greatly benefited from the insightful comments and suggestions of my dissertation committee members, Richard Murphy and Cody Tuttle. For providing helpful comments, I thank Manuela Angelucci, Joridan Barash, Magdalena Bennett, Scott Carrell, Gue Sung Choi, Eric Chyn, Prankur Gupta, Yumin Hong, Joonhwi Joo, Bokyung Kim, Brendan Kline, Gerald Oettinger, Francisco Pardo, Margarita Petrusevich, Mu Yang Shin, Stephen Trejo, and seminar participants at the University of Texas at Austin. The research presented here utilizes confidential data from the State of Texas. The views expressed are those of the authors and do not necessarily reflect the opinion or official position of the Texas Education Research Center, the Texas Education Agency, the Texas Higher Education Coordinating Board, the Texas Workforce Commission, or the State of Texas. All remaining errors are my own.

†Harvard University, Email: json@hks.harvard.edu.

1 Introduction

Over the past several decades, women have made dramatic progress in both educational attainment and the labor market (e.g., Blau and Kahn, 2000; Goldin, Katz and Kuziemko, 2006; Goldin, 2014). A large body of literature has documented that the ability to plan childbearing is one of the key drivers behind this achievement. For instance, prior work has shown that access to birth control can enable women to further invest in their education and careers (e.g., Goldin and Katz, 2002; Bailey, 2006; Myers, 2017). Recently, an emerging literature finds that pregnancy timing—the age at which women give birth—has important impacts on women's outcomes (e.g., Kleven, Landais and Søgaard, 2019; Eichmeyer and Kent, 2022; Massenkoff and Rose, 2022).[1] This literature has focused on pregnancy timing among *adults*, and little is known about its impacts among *teenagers*.

This is an important gap in the literature for at least two reasons. First, teen pregnancy rates are high in the US, and addressing concerns associated with teen pregnancy is of perpetual policy interest (Kearney and Levine, 2012).[2] While there is an extensive literature considering the impacts of teen childbearing, no prior research investigates the effects of pregnancy timing—age of mother at birth—on teenagers' short- and long-run educational and labor market outcomes. Understanding the consequences of pregnancy timing is also important for interpreting recent trends among teenage mothers—as there has been a notable shift toward pregnancies in later teenage years.[3] Moreover, evidence on the effects of pregnancy timing among teenage mothers can inform us about the consequences of policy efforts aimed at delaying or preventing teen pregnancy—such as regulations surrounding sex education (e.g., Carr and Packham (2017); Paton, Bullivant and Soto (2020)) and access to low-cost contraception (e.g., Packham (2017)).[4]

Second, the timing of pregnancy may have a greater impact on teenagers, as adolescence is a critical period for laying the foundations for adulthood. During this critical period, teenagers make key decisions that influence their educational attainment—which courses to take, how much

[1] Throughout the manuscript, particularly in the context of my empirical analysis, I use the terms *pregnancy timing* and *timing of pregnancy* to refer to the *age* at which an individual gives birth.

[2] For instance, in his 1995 State of the Union address, then-President Clinton pointed out teen pregnancy as "our most serious social problem," calling for a nationwide campaign against it.

[3] The share of teen births by those aged 18 and 19 has risen from 66.5% in 2000 to 76.1% in 2019 .

[4] For a comprehensive review on the impacts of various public policies on teen birth rates in the US, see Kearney and Levine (2015).

attention to dedicate to studies, whether to continue secondary education, and whether to attend college. Additionally, through social interactions, they develop non-cognitive skills such as conscientiousness, openness, and emotional control. Furthermore, they accumulate health capital as they undergo physical and brain development and build healthy behaviors and habits. It is well established that all these aspects of adolescence profoundly affect future well-being and life trajectories (e.g., Currie, 2009; Deming, 2022).

This paper begins to fill this gap in the literature by investigating the effects of pregnancy timing among teenage mothers aged 15–18 on their short- and long-run educational and labor market outcomes. Specifically, I *separately* estimate the marginal impact of a *one-year* difference in pregnancy timing for each age interval—15–16, 16–17, and 17–18.[5]

There are two key empirical challenges to studying this question: (i) typical data sources leveraged in prior research lack sufficient sample size and follow-up period, and (ii) the endogeneity of pregnancy timing. Regarding the first issue, individual-level *panel* data on a *large sample* of teenage mothers are difficult to obtain. In particular, data that closely track teen mothers both before and after their pregnancies, along with rich baseline characteristics, are rarely available to researchers. I address this challenge by utilizing high-frequency, individual-level administrative panel data from Texas, which contain extensive information on more than 250,000 pregnant teenagers (or students) who ever attended K–12 public schools in Texas. Another unique feature of the data is that for each pregnant student, I can observe their outcomes before, during, shortly after, and long after their pregnancies.

With respect to the second challenge, individuals who differ in pregnancy timing are likely to differ in unobservable ways, making it difficult to disentangle the effect of differential pregnancy timing from other confounding factors. I overcome this challenge by isolating the random component of pregnancy timing, which arises in my setting due to the extremely high incidence of *unplanned* teen pregnancies. Specifically, I match each teenager who gives birth to a teenager who gives birth a year later, where I require that this matched pair of teenagers shares a set of

[5] As detailed in Section 3, the ages 15 through 18 here refer to the ages at which individuals give birth, as opposed to when they become pregnant. Additionally, for these three comparison groups, I consistently define those who give birth one year earlier as the treated groups.

key characteristics that are known to correlate with adolescent childbearing (e.g., proxies for risky behavior). To validate this approach, I demonstrate that the matched pairs of individuals are observationally comparable across various baseline characteristics (not used in matching), implying that the sole difference—a one-year difference in the age at which they give birth—is largely driven *by chance*. In addition, by taking advantage of the high frequency of the data, I show that they display nearly identical levels and trends in absence rates—arguably the most important predictor for both academic and behavioral outcomes—throughout the period prior to pregnancy.

My short-run analysis compares within-individual changes in outcomes for matched mothers surrounding pregnancy using a difference-in-differences framework. Specifically, I analyze changes in outcomes within *treated* individuals—those who become a mother one year earlier (e.g., at age 15)—for two years before and after their pregnancies. These changes are then compared to those of matched *control* individuals—those who become a mother one year later (e.g., at age 16)—over the same age ranges for both groups (e.g., ages 12 through 15). The key identification assumption is that if the treated individuals had become pregnant one year later, their outcomes would have evolved similarly to those of their matched controls. I provide support for this assumption by illustrating that the two groups have very similar trends in outcomes throughout the entire pre-pregnancy period and begin to diverge when the treated individuals conceive.

I find that experiencing pregnancy one year earlier has a negative impact on school attendance, particularly during the postpartum year. Specifically, when comparing those who become mothers at ages 15 and 16, experiencing childbirth one year earlier leads to an increase of 2.1 and 5.7 percentage points in absence rates during the gestation and postpartum year, respectively. These changes correspond to 23.6% and 44.0% increases, relative to the control means during the same years. I find similar patterns in chronic absenteeism—having an absence rate equal to or greater than 10%. Furthermore, comparing mothers aged 16 to 17 and those aged 17 to 18 also yields similar estimates for absences and chronic absenteeism in both absolute and relative terms. I also find that individuals who give birth at age 15 are 1.9 percentage points (or 2.0% relative to the control mean) less likely to stay in school after giving birth, relative to those who give birth at age 16. In contrast to absence rates, the magnitude of these impacts grows for older treated individuals: a

3.5 percentage point (or 3.6%) reduction for mothers aged 16 and a 5.1 percentage point (or 5.3%) reduction for mothers aged 17. Finally, the patterns are consistent when investigating a composite measure of absences, which accounts for both absences and enrollment in schools.

My long-run analysis examines the impact of pregnancy timing on outcomes—such as high school graduation, college enrollment, and earnings—measured through age 25. Since long-run outcomes are only observed once for each individual after pregnancy, I cannot analyze changes in them within the same individuals. Instead, my long-run econometric model includes a full set of match group fixed effects to compare outcomes within matched pairs of treated and control individuals. This empirical strategy relies on a stronger identification assumption that pregnancy timing is as good as random within each matched group. I offer several pieces of evidence to support this assumption. For instance, I illustrate the stability of the estimates when including more or fewer controls for baseline observables. Additionally, when analyzing short-run outcomes within the long-run analysis framework, I obtain estimates that are nearly identical to the baseline estimates—those from the short-run model using the same outcomes.

I find that becoming a mother one year earlier has lasting implications for secondary educational attainment for teenage mothers. Specifically, my results indicate that a one-year earlier pregnancy universally leads to a decrease of 0.3 to 0.4 years of schooling for the treated mothers, compared to their counterpart matched controls. In addition, it also reduces the likelihood of high school completion by age 20 by 4.0 percentage points (or 10.3%), 9.8 percentage points (or 18.2%) and 11.2 percentage points (or 15.9%), for the treated individuals giving birth at ages 15, 16, and 17, respectively.

The results for college enrollment and completion suggest that reductions in secondary educational attainment lead to reductions in higher education achievement, particularly for the youngest treated individuals (i.e., those giving birth at age 15). Specifically, I find that getting pregnant one year earlier leads them to be 2.8 percentage points (or 9.2%) less likely to enroll in any college by age 23 and 1.3 percentage points (or 13.5%) less likely to obtain any college degree by age 25. Although smaller in magnitude, I continue to observe that conceiving one year earlier reduces any college enrollment by 2.2 percentage points (or 6.6%) and by 1.2 percentage points (or 3.2%) for

the treated individuals aged 16 and 17, respectively. However, I do not find significant impacts on college degree receipt for these older mothers.

Finally, consistent with the results for college education, the youngest treated individuals bear the largest cost of early childbearing in terms of forgone employment opportunity and earnings. Specifically, they experience a 2.7 percentage point (or 4.3%) reduction in the likelihood of employment and a $752.7 (or 9.0%) decrease in annual earnings, when averaging the estimates across ages 20 to 25. These labor market disadvantages are less pronounced, yet remain significant, for the individuals treated at age 16 and become statistically indistinguishable from zero for those treated at age 17.

A natural follow-up question to ask is whether the negative consequences of teen childbearing can be mitigated by providing support to teen mothers. To complement my primary analysis on pregnancy timing, I provide evidence on the role of parental support services in addressing short-term disruptions teen mothers experience during their postpartum period. To analyze this, I leverage a natural experiment in Texas, where funding for a teen parent support program in schools was zeroed out. Using a cohort-based difference-in-differences approach, I compare a recent cohort of teen mothers affected by this budget cut to an older cohort of teen mothers who were not. The results indicate that the funding withdrawal results in a 1.9 percentage point (or 14.1%) increase in absence rates and a 4.2 percentage point (or 5.2%) decrease in the likelihood of finishing the academic year, with both outcomes measured specifically in the postpartum year. These results highlight the importance of such support for teen mothers in overcoming immediate challenges after birth.

My paper contributes to three strands of literature. The first is a large literature on the socioeconomic consequences of teen childbearing. Given the prevalence of teen pregnancy in the US, numerous studies have attempted to estimate the causal effect of teen motherhood but have found mixed evidence on both the sign and magnitude of the impact (Geronimus and Korenman, 1992; Hotz, Mullin and Sanders, 1997; Levine and Painter, 2003; Holmlund, 2005; Fletcher and Wolfe, 2009; Ashcraft, Fernández-Val and Lang, 2013; Lang and Weinstein, 2015). This study adds to this literature in two ways. First, while these previous papers address the *extensive* margin ques-

tion—the impact of becoming a teenage mother or not—my paper explores the *intensive* margin question—the timing of becoming a mother while still a teenager.[6] Adding a new dimension to our understanding of the consequences of teen births is both timely and policy-relevant, given the recent trends in pregnancy timing among teenagers and the importance of targeting policy (e.g., sex education, access to birth control) toward groups that would be most likely to benefit. Moreover, my results are also informative about a natural counterfactual for many women. While the exact timing of pregnancy is partially due to random variation, many women at high risk of teenage pregnancy will give birth at some point during their teenage years. Second, unlike previous papers that primarily examine outcomes measured long after pregnancy, I use linked individual-level panel data that allow me to investigate how these individuals' outcomes evolve before, during, and after pregnancy. This enables me to credibly identify the short- and long-run impacts of pregnancy timing and to shed light on mechanisms behind the long-run effects.

Next, my work further contributes to a growing literature investigating the effects of motherhood, which relies on an event-study design to compare various outcomes among women who differ in the timing of their first birth. Existing work has focused on pregnancy timing among *adults*—showing the timing of pregnancy is closely associated with women's earnings trajectories and career progression (e.g., Kleven, Landais and Søgaard (2019); Angelov, Johansson and Lindahl (2016); Andresen and Nix (2022); Gallen et al. (2023)), homelessness and government program take-up (e.g., Eichmeyer and Kent (2022)), health behavior (e.g., Janssen and Parslow (2021)) and criminal behavior (e.g., Massenkoff and Rose (2022)). My paper extends this literature by analyzing the impact of pregnancy timing among *teenagers*. Adolescence is a critical period for the development of human capital, and pregnancy timing may be particularly consequential during these formative years. A remaining question in this literature is whether and to what extent pregnancy timing has lasting implications for teenagers who give birth during such a critical period. My findings complement prior work investigating pregnancy timing among adults by illustrat-

[6] My paper is most closely related to Schulkind and Sandler (2019), who compare those who become mothers right before and after high school graduation using the US decennial census and ACS data. They find that mothers who give birth just before high school graduation are 5.4 percentage points less likely to graduate from high school, are less likely to be married, and have more children compared to those who give birth just a few months later. While the key idea—comparing teenagers who give birth within a narrow time frame—is similar, distinctions emerge in terms of data, identification strategy, and outcomes.

ing the child penalty—the negative effects of having children earlier—extends beyond adults to teenagers. Moreover, the scope of the child penalty is broader among teenagers, and its impacts are intensified through the channel of reduced human capital accumulation. Finally, I demonstrate these adverse impacts are larger the younger the mother, highlighting the importance of considering pregnancy timing among those who have not yet reached adulthood.

Lastly, this paper contributes to a recent literature that studies the impact of family policies on women's economic outcomes, such as parental leave programs (e.g, Schönberg and Ludsteck, 2014; Dahl et al., 2016) and childcare provision (e.g., Havnes and Mogstad, 2011; Baker, Gruber and Milligan, 2008).[7] In contrast to prior research that focuses on programs aimed at adult women, the comprehensive parental support program I analyze is designed to support teenage mothers within a school setting. Teenage mothers may be more vulnerable and face greater challenges than women giving birth at older ages. The findings from this analysis highlight the potential importance of parental support for teenage mothers in alleviating short-term disruptions they face. In the same vein, my findings may inform policymakers considering improving access to parental support for teen parents, thereby addressing the high dropout rates associated with teen pregnancy and promoting their accumulation of human capital.

The remainder of this paper is organized as follows. Section 2 provides background information on teen pregnancy and describes the data. Section 3 outlines the research design and discusses the estimation strategy. Section 4 presents the main results. Section 5 offers the supplemental policy analysis. Section 6 concludes.

2 Background and Data

This paper leverages restricted-access administrative data that link individual-level records across several government agencies in the state of Texas.[8] In this section, I begin by providing background information on Pregnancy Related Services—the key information in the data that en-

[7] For a comprehensive review on the economic consequences of family policies across high-income countries, see Olivetti and Petrongolo (2017).

[8] I obtained access to the data through the Texas Education Research Center (ERC) housed at the University of Texas at Austin. For more details, see https://texaserc.utexas.edu (accessed August 2023).

ables me to identify pregnant students. I then describe the main data sources and provide details on the construction of the full sample.

2.1 Background on Pregnancy Related Services (PRS) in Texas

The Pregnancy Related Services (PRS) are support services that pregnant students in Texas may take up during the prenatal and postpartum periods. The PRS is designed to help pregnant students keep up with school, particularly by offering off-campus instruction when they are confined to their homes or hospital bedsides due to pregnancy-related medical reasons. In principle, each school district can choose whether to provide the PRS; however, as illustrated in panel (a) of Figure A1, it is widely available across school districts in Texas—with more than 90% of female students in public schools eligible for the program.

My primary data, detailed in the next subsection, include annual individual-level PRS take-up records but lack pregnancy outcomes (e.g., birth, miscarriage, abortion). Detailed anecdotal and quantitative evidence collectively suggests that pregnant students in my data (i.e., those who take up the PRS) are highly likely to give birth and become teen mothers, rather than choose abortion or experience a miscarriage. With this background in place, panel (b) in Figure A1 shows that teen births in the data are largely representative of those in Texas—covering roughly 80% of births among those who gave birth at ages 15 to 18 while attending secondary school.

2.2 Data

The primary data source is longitudinal, individual-level administrative data from the Texas Education Agency (TEA). These data cover the universe of students who attended public K–12 schools in Texas from the 1991–1992 to 2020–2021 academic years and provide comprehensive information on students' academic and non-academic records, including enrollment, attendance, graduation, standardized test scores, and disciplinary action histories.[9] The data also include details about student demographics, such as age, race/ethnicity, location of residence (based on

[9]The coverage of academic years varies across different data elements. For instance, student attendance records at the grading-period level are available from the 1992–1993 academic year, whereas disciplinary action records are available from the 1998–1999 academic year. For more details, see https://texaserc.utexas.edu/erc-data/data-inventory (accessed August 2023).

school district), and economically disadvantaged status. Most importantly, the Pregnancy Related Service (PRS) participation information in the data allows me to identify pregnant students and determine the academic year in which they concluded their pregnancies.

One of the key advantages of these data is the high frequency of attendance information, which is of great interest as the potential underlying channel for the adverse impact of teen pregnancy. Specifically, each student's attendance is recorded at the grading period level, which spans six calendar weeks, for each academic year. Note each academic year comprises six grading periods: the first three correspond to the fall semester, and the remaining three to the spring semester.

Using these records, I create four attendance-related outcomes at the grading-period level (or six-week level): (1) an absence rate, measured as the number of days absent divided by the number of days enrolled; (2) an indicator for whether an individual remains (or is enrolled) in the Texas public school system; (3) an imputed absence rate, which equals the absence rate for those enrolled but is assigned a value of 100% if the individual is not enrolled; and (4) an indicator for chronic absenteeism, defined as an absence rate of 10% or greater.[10] I further construct outcomes related to secondary-education attainment by combining enrollment, attendance, and graduation records. Specifically, I measure completion of grades 9 to 11, the highest grade achieved, and high school graduation status by the age of 20.

The post-secondary education data come from the Texas Higher Education Coordinating Board (THECB). The semester-level THECB data include individual-level enrollment and degree-attainment information for all public and most private higher-education institutions in Texas and are available from the 1990 fall semester.[11] In addition, I also use the National Student Clearinghouse (NSC) data, which contain similar information to the THECB data but cover out-of-state public and private colleges and universities from the 2008–2009 to 2016–2017 academic years.[12]

[10]The first and last outcomes can be measured only when a student is enrolled in Texas public schools, whereas the other two can be measured for all students. The third outcome can be considered a combination of the first two outcomes.

[11]Similar to the TEA data, the availability of years for THECB reports differs by both institute type and content. The data for public colleges and universities are mostly available from the early 1990s, whereas the data for private colleges and universities are available from the early 2000s. For more information, see https://reportcenter.highered.texas.gov/reports/data/glossary-of-data-terms and http://www.txhighereddata.org/ReportingManuals (accessed August 2023).

[12]This relatively limited coverage of academic years does not affect the internal validity of my research design, given the same birth-cohort restriction for both treated and control groups, as I describe in more detail in the next section.

I link both THECB and NCS data to the TEA records at the individual level and construct two categories of outcomes for academic achievement in tertiary education. Specifically, I create an indicator for having ever enrolled in higher education by the age of 23 and an indicator for having ever obtained a post-secondary degree by the age of 25. These outcomes are separately measured based on institution type: (1) any college/university,[13] (2) two-year public college, (3) four-year public university, (4) any private college/university, and (5) career/technology school.[14]

Finally, I utilize administrative earnings records from the Texas Workforce Commission (TWC) that include all Texas employees subject to coverage by the state Unemployment Insurance (UI) program.[15] Although the data are quarterly, I aggregate them by year using inflation-adjusted quarterly earnings (in the first quarter of 2020 dollars)[16] and then link them to the TEA data at the individual level. Using the TWC data, I create two labor market outcomes: (1) annual earnings and (2) an indicator for employment (measured by having positive earnings) for each age from 20 through 25. I treat individuals with missing earnings as those who are unemployed and therefore have zero earnings. Additionally, I lack information on employment and earnings outside of Texas.

2.3 Full-Sample Construction

The sample in this paper consists of students who completed their pregnancies during the academic years 1999–2000 through 2020–2021.[17] Among these students, I exclude those with repeated pregnancies and focus instead on those who completed their *first* and *only* pregnancy during the aforementioned academic years, following the literature on teen pregnancy (e.g., Ashcraft, Fernández-Val and Lang (2013) and Lang and Weinstein (2015)). After this restriction, my sample includes 238,446 students, and I refer to it as the *full sample* throughout the paper. In Sections 3 and

[13] This outcome can be interpreted as a measure that combines the remaining four outcomes.

[14] When constructing the five outcomes for college enrollment, I only consider records from semesters within academic years that follow the academic year in which an individual was last observed in the K-12 public school system. This refinement ensures the exclusion of THECB enrollment records generated from the dual credit system, where eligible high school students enroll in college courses and earn credit from both the college and their high school.

[15] According to Texas Workforce Commission (TWC) Rules 815.107 and 815.109, all employers in Texas, defined as those who employ one or more individuals for twenty weeks during a calendar year or pay wages of $1,500 or more in a calendar quarter, are required to report unemployment insurance (UI) wages and pay their quarterly UI taxes.

[16] The quarterly Consumer Price Indexes (CPIs) I used can be obtained at https://www.ssa.gov/oact/STATS/avgcpi.html (accessed August 2023).

[17] Recall that the academic year 1999–2000 is the earliest year for which the TEA data allow me to identify pregnant students through the Pregnancy-Related Service (PRS) information.

5, I provide further details on additional sample restrictions imposed to construct the *short-run*, *long-run*, and *policy-analysis* samples, all of which start from the full sample.

3 Research Design

In this section, I start by discussing how I isolate plausibly exogenous variation in differential pregnancy timing. I then describe estimating equations and identification assumptions for the short- and long-run analyses.

3.1 Identifying Plausibly Exogenous Variation in Pregnancy Timing

Fundamental Randomness in Pregnancy Timing There is substantial randomness in the timing and occurrence of pregnancy among teens. First, there is fundamental randomness in the occurrence and timing of pregnancy among individuals who engage in similar sexual activity and use similar contraceptive methods. Many factors must align for a woman to become pregnant and these factors are difficult to predict—given inherent uncertainty in the timing of ovulation and sperm availability, in the quality of egg and sperm, in random events that impact cell division and early embryonic development, and in factors that influence the uterine lining and the likelihood of implantation. Second, there is substantial additional randomness in the exact timing of pregnancy among teens because teen pregnancies are typically unplanned. More than 80% of teen pregnancies are unplanned, and in these cases, we would expect there to be substantial randomness in the exact timing of births.[18] Given all teens in my sample conceive while attending school and know they must attend school for a few more years to complete their secondary education, it is even less likely that they would plan their pregnancies and choose the precise timing.

[18]The nationwide survey conducted by the National Center for Health Statistics (NCHS) reveals approximately 80% of teen pregnancies are unplanned among teenagers ages 15–19 (Mosher, Jones and Abma, 2012). Two points should be considered when interpreting the survey results within my empirical setting. First, my analysis excludes pregnancies at age 19, becausestudents who become pregnant at that age are already likely to graduate from high school. Second, individuals who respond to the survey with "I do not know" are categorized as having planned pregnancies. Taking these two factors into account, the statistic might reasonably be interpreted as a conservative estimate for the proportion of unplanned teenage pregnancies in my context.

Two Complementary Strategies for Isolating Exogenous Variation While there is fundamental random variation in pregnancy timing conditional on sexual activity and contraceptive use, engagement in sexual activity and the use of contraceptive methods is not generally random and presents potential identification challenges. To confront these challenges, my research design aims to isolate the random component of pregnancy timing by identifying teenagers at similar risk of pregnancy—teenagers who all conceive while in school—but who differ in the exact timing of pregnancy. I employ two complementary strategies to ensure this analysis focuses on variation in pregnancy timing that plausibly exogenous. First, I specifically focus on comparing teenagers who differ in the age at which they give birth by one year (e.g., those giving birth at age 16 vs. 17), among whom the exact timing of pregnancy is more likely driven by chance relative to comparisons of teens who get pregnant at more dissimilar ages (e.g., age 15 vs. 18). Furthermore, I implement a matching procedure that combines exact and nearest-neighbor matching methods. Specifically, I match each teenager who gives birth to a teenager who gives birth a year later, requiring this matched pair of teenagers share a set of key characteristics that are known to predict adolescent childbearing (e.g., proxies for risky behavior). Below, I provide details on the matching procedure and demonstrate that the matched pairs are observationally nearly identical across a range of baseline characteristics—not only those used in the matching but also, more importantly, those not used in the matching. Moreover, I demonstrate that pre-pregnancy educational outcomes (such as absence) for these matched pairs track each other well in terms of levels and trends.

Matching Procedure and Balance Test Results I begin by considering all female students who completed their pregnancies between the ages of 15 and 18 from the academic years 2000–2001 to 2019–2020.[19] I then create three separate datasets, each of which includes all individuals with only a one-year age interval: ages 15 and 16 (referred to as the *Young* dataset), 16 and 17 (the *Middle* dataset), and 17 and 18 (the *Old* dataset). To be clear, the ages denoted here are those measured in the first postpartum academic year, as opposed to the year during pregnancy. And importantly, in each dataset, I define individuals who concluded their pregnancies a year earlier (i.e., the younger

[19]Note this is a subset of the full sample. In other words, the basic sample restrictions described in section 2.3 have already been applied.

group) as *treated* units and their older counterparts as *control* units.[20]

To be eligible for inclusion in a matching procedure implemented separately for each dataset, individuals should be observed in the TEA data for at least *one* grading period in each of the two academic years before the relevant treated individuals become pregnant. For instance, in the *Young* dataset, I require both treated and control students to be enrolled in any Texas public schools for at least one grading period each at ages 12 and 13. This restriction enables me to examine any systematic differences in baseline characteristics and compare the outcome paths between the two groups in pre-gestation periods.

I then match *each* treated individual to *all* control individuals on the basis of the following six characteristics: (1) year of birth, (2) race/ethnicity (categorized as Non-Hispanic White, Non-Hispanic Black, or Hispanic), (3) enrolled grade, (4) an indicator for economic disadvantage status (determined by eligibility for free or reduced lunch or other social assistance programs), (5) an indicator for annual disciplinary action records (measured as having any records), and (6) binary urban/rural classification for the county of residence based on the school district. I measure the last four time-varying matching variables in the academic year preceding gestation for treated units (e.g., at age 13 for the *Young* dataset).

The exact match above results in treated units having multiple matched controls. To select the controls that are the most comparable, I further use nearest-neighbor matching on *annual* absence rates in the year right before gestation for treated units (e.g., again at age 13 for the *Young* dataset). Specifically, for each treated individual, within the group of all control individuals matched on the six exact matching variables, I identify a single control individual with the level of annual absence rates closest to the treated unit.[21] Some cases could involve multiple nearest control units (also known as ties in the matching literature). In such instances, I include all of them and assign equally divided weights, following the recommendation of Abadie and Imbens (2006).[22]

[20] Note students who completed pregnancies at ages 16 and 17 are included in two separate datasets simultaneously but assume different roles depending on which dataset they are included in. For instance, the group of individuals denoted as age 16 serve as control units in the *Young* dataset, whereas they are treated units in the *Middle* dataset.

[21] I performed one-to-one nearest neighbor matching with replacement, but the results are similar to those from matching without replacement combined with a random order of matching.

[22] In almost all cases, the number of control individuals within the cells that share the same six exact matching characteristics is strictly greater than the number of treated individuals. However, in very rare instances, the opposite occurs. In such cases, for each control unit, I match them in reverse to a single treated individual with the most similar level of

Figure 1 provides an illustrative example of the matching procedure described above, including the timing of when the matching variables are measured. Importantly, the four time-varying exact-matching characteristics and one fuzzy-matching variable (i.e., annual absence rates) are all measured at the same age in the same academic year for both treated and control units (within match group), due to (time-invariant) matching on birth year. My final *short-run* samples include (1) 8,089 treated and 9,892 control individuals in the *Young* sample, (2) 24,547 treated and 26,995 control individuals in the *Middle* sample, and (3) 43,167 treated and 46,285 control individuals in the *Old* sample. When describing the results in Section 4, for brevity, I will refer to these sample names to clarify which treated individuals are relevant to the estimates being discussed.

Next, I perform separate balance tests to assess the exogeneity of the timing of pregnancy for each of the three datasets. Panel (a) of Figures 2 through 4 presents balance tests for the seven *matching* variables. Each row provides a separate test for the indicated pre-determined characteristic, and I plot the coefficients on the treatment group (or the mean differences between the treated and control groups). In the right side of each figure, I also report the estimates and p-values for these differences, along with the corresponding control group means. Not surprisingly, the two groups are identically balanced on the six exact-matching characteristics and nearly identically balanced on the fuzzy-matching characteristic across all three samples.

More importantly, if the remaining variation in pregnancy timing (after matching) is largely exogenous, we should not expect to see significant differences in other *non-matching* baseline characteristics. The results in panel (b) of Figures 2 through 4, which present a series of balance tests for *non-matching* pre-determined characteristics, generally support this idea. For instance, in Figure 2, panel (b), where I compare individuals who reach the end of pregnancies between ages 15 and 16, p-values exceed 0.10 for all characteristics except one (i.e., vocational education, p-value of 0.091). In the other two samples (reported in panel (b) of Figures 3 and 4), I observe a few pre-determined characteristics that are statistically different between the two groups; however, the magnitudes of these differences relative to the control means are minimal.

Finally, I note my short-run difference-in-differences model relies on the parallel-trends assumption, rather than an assumption requiring full balance between the treated and control in-

annual absence rates.

dividuals. Furthermore, it also includes individual fixed effects to account for any time-invariant differences between the two groups. If the unbalanced characteristics are stable over time, they would be largely captured by the individual fixed effects. Additionally, my long-run econometric model will directly control for all the unbalanced characteristics to mitigate any bias arising from them.

3.2 Short-Run Econometric Model

My short-run analysis focuses on absence-related outcomes measured in each six-week grading period over four academic years (or 24 grading periods). Specifically, I follow treated individuals and their matched controls at the *same ages* during the *same academic years*, spanning pre-gestation (12 grading periods), gestation (6 grading periods), and postpartum (6 grading periods) for the treated units (see Figure 1 for an illustrative example).

To present my short-run econometric model more concisely, let h denote the relative grading periods, where $h = 0$ represents the first grading period of the academic year during which treated individuals become pregnant. Note h is well defined for the matched counterparts because they are also tracked over the same 24 grading periods. For instance, in the *Young* dataset, $h = 0$ corresponds to the first grading period of the academic year at age 14 for both the treated and control groups, with the specific academic year varying by birth cohort for each matched group (e.g., the 2013–2014 academic year for the 1990 birth cohort).

In my short-run analysis, I employ a difference-in-differences model in which I compare changes in outcomes for treated students with those of matched control students before and after the pregnancy of the treated individuals. Specifically, I separately estimate the following equation, using the three *short-run* samples (i.e., the *Young*, *Middle*, and *Old* datasets):

$$Y_{igh} = \sum_{h=-12,\, h\neq -1}^{11} \left\{ \boldsymbol{\beta}_h \times Treat_i \times \mathbf{1}_h \right\} + \alpha_i + \mu_{gh} + \epsilon_{igh}, \tag{1}$$

where Y_{igh} is an outcome for individual i in match group g in relative grading period h (as defined above). $Treat_i$ is an indicator denoting treated individuals, that is, those who undergo pregnancies

one year earlier in each sample, and $\mathbf{1}_h$ is a set of relative-grading-period indicators. I include individual fixed effects, α_i, to account for any time-invariant differences between treated and matched control individuals. This specification also includes a full set of match-group x relative-grading-period fixed effects, μ_{gh}, to flexibly control for match-group-specific time trends in outcomes. Standard errors are clustered at the individual level, and all regressions are weighted using the weights derived from the matching procedure.

The key coefficients of interest are β_h, which estimate the differences in outcomes in relative grading period h between the treated and matched control units. I drop the last grading period of the pre-gestation academic years, so that each β_h can be measured relative to the difference that occurred in the omitted period (i.e., $h = -1$).

I also report the two distinct mean-effect estimates, γ and ρ, by estimating the following specification that groups the relative grading periods into either gestation or postpartum phases:

$$Y_{igh} = \gamma \times Treat_i \times \underbrace{\mathbf{1}\big(0 \leq h \leq 5\big)}_{\text{= Gestation Periods}} + \rho \times Treat_i \times \underbrace{\mathbf{1}\big(6 \leq h \leq 11\big)}_{\text{= Postpartum Periods}} + \lambda_i + \phi_{gh} + \varepsilon_{igh}, \qquad (2)$$

where $\mathbf{1}\big(0 \leq h \leq 5\big)$ and $\mathbf{1}\big(6 \leq h \leq 11\big)$ are indicators for the grading periods during and after pregnancy for treated individuals, respectively. As in equation (1), this estimation includes individual fixed effects (λ_i) and match group by relative-grading fixed effects (ϕ_{gh}). I note that in the specification above, the distinction within the post-pregnancy periods—dividing them into gestation and postpartum stages—helps us better understand the impacts on pregnant students during each phase.

The key identification assumption underlying my short-run econometric models is the parallel-trends assumption: had the treated individuals become pregnant one year later than they actually did, their outcomes would have evolved similarly to those of their matched controls. Although I cannot directly test this assumption, several pieces of evidence provide support for the assumption. First, as I show in the next section, the nearly identical raw *trends* and *levels* in outcomes throughout the entire pre-gestation periods corroborate the plausibility of the assumption (Kahn-Lang and Lang, 2020).

Second, contemporaneous shocks—which are a common concern within a standard difference-in-differences framework—are unlikely to bias my results. For such shocks to significantly confound the estimates, they would have to affect only the outcomes of the treated individuals, while not influencing the outcomes measured at the same ages during the same academic years for the matched control individuals who share very similar background characteristics. Furthermore, these shocks would need to coincide with the conception of the treated groups and occur repeatedly across different academic years for the majority of the 19 birth cohorts. Additionally, given the matching on grade, any confounding factors arising from grade progression would be equally present in both the treated and control units.

Finally, more broadly, matching on key observable characteristics along with demonstrated balance on a wide range of non-matching observables between the treated and matched control students alleviates concerns that the two groups are fundamentally different and thus might follow different paths in outcomes.

3.3 Long-Run Econometric Model

In my long-run analysis, I examine the impact of pregnancy timing among teenagers on secondary educational outcomes (e.g., high school graduation), post-secondary educational outcomes (e.g., college enrollment), and labor market outcomes (e.g., employment and earnings). For the long-run analysis, I focus on individuals who are at least 20 years old as of the 2020–2021 academic year—the last academic year in my dataset—when exploring outcomes associated with secondary education.[23] I further restrict my attention to those ages 25 or older when investigating college and labor market-related outcomes.[24]

In contrast to short-run outcomes that can be measured both before and after pregnancy, I

[23]This restriction leads me to focus on individuals born in 2001 or earlier. My final long-run analysis samples for secondary educational outcomes include (1) 7,623 treated individuals and 9,449 matched control individuals for the *Young* dataset, (2) 23,681 treated individuals and 26,117 matched control individuals for the *Middle* dataset, and (3) 42,249 treated individuals and 45,418 matched control individuals for the *Old* dataset.

[24]This restriction results in the exclusion of an additional five birth cohorts from 1997 to 2001, thereby focusing on individuals born in 1996 or earlier. My final long-run analysis samples for post-secondary educational and labor outcomes include (1) 5,921 treated individuals and 7,550 matched control individuals for the *Young* dataset, (2) 19,262 treated individuals and 21,554 matched control individuals for the *Middle* dataset, and (3) 34,928 treated individuals and 37,615 matched control individuals for the *Old* dataset.

can observe each long-run outcome only once after the postpartum period. Considering this constraint, which prevents me from including individual fixed effects, my long-run econometric model compares the average outcomes of treated individuals with those of matched control individuals within the matched group. The following equation outlines the regression form for this comparison:

$$Y_{ig} = \boldsymbol{\delta} \times Treat_i + \sigma_g + \boldsymbol{\Theta}' \boldsymbol{X}_i + v_{ig}, \tag{3}$$

where Y_{ig} represents an outcome for individual i in match group g, and $Treat_i$ is again an indicator for treated individuals within each dataset. I include a complete set of matched group fixed effects, σ_g, to address potential differences in outcomes across the matched groups. $\boldsymbol{X}_i$ is a vector of individual-level pre-determined control variables, including indicators for (1) participation in vocational and special education, (2) at-risk dropout students, (3) limited English proficiency (LEP), and (4) a binary measure of intensity for disruptive/risk behaviors based on the number of days assigned for disciplinary actions.[25] Additionally, $\boldsymbol{X}_i$ includes county fixed effects, using each individual's latest school district information from the TEA data. Importantly, I obtain almost identical results when excluding all of these control variables, $\boldsymbol{X}_i$.

In parallel with the short-run analysis, I estimate equation (3) separately for each comparison dataset, using the weights derived from the matching procedure and clustering standard errors at the individual level. The primary coefficient of interest is $\boldsymbol{\delta}$, which captures the mean difference in outcomes between the treated and matched control units across the matched groups.

The key identification assumption behind the specification above is that a one-year difference in the timing of pregnancy completion (or the assignment of $Treat_i$) is as good as random within each matched group. Although this assumption is fundamentally untestable, I provide several pieces of evidence that support the assumption. First, as discussed in section 3.1, treated and matched control individuals are balanced on a wide range of *non-matching* baseline characteristics. In addition, I demonstrate that the main results are robust to the exclusion of a set of controls ($\boldsymbol{X}_i$) (Altonji, Elder and Taber, 2005; Oster, 2019), and that the analysis of short-run outcomes within

[25] These control variables are measured in the same academic year in which I measure the time-varying matching variables (i.e., the academic year prior to gestation for the treated units in each matched group). Note these controls are the variables I found to be slightly unbalanced between treated and control individuals in the non-matching variable balance tests.

the long-run framework produces nearly identical results. These two additional analyses further strengthen the validity of the identifying assumption.

4 Results

4.1 Short-Run Effects

Raw Data Plot Figures 5 through 7 plot the raw means of my short-run outcomes for three samples: the *Young* sample (15 vs. 16), the *Middle* sample (16 vs. 17), and the *Old* sample (17 vs. 18). In each panel of the figures, the raw trends are presented separately for treated and matched control individuals over 24 grading periods—12 periods before, six periods during, and six periods after the pregnancy of the treated individuals.

In panel (a) of Figures 5 through 7, I observe nearly identical trends and levels in the absence rates throughout the pre-pregnancy periods between the treated and control groups. However, the two groups begin to diverge from the gestation year (shaded in light gray), with the treated groups showing substantially larger increases in the absence rates. This divergence becomes even more pronounced in the postpartum year (shaded in dark gray), further widening the gap between the two groups. The overall patterns are very similar across all three samples, including the magnitude of increased absence rates in the post-pregnancy periods.

Panel (b) in Figures 5 through 7 displays the raw trends in the proportion of students enrolled in the Texas public school system. Unlike the absence rates, which are only defined when students are in school, this outcome can be measured for all 24 periods. Similar to the absence rates, the treated and matched control individuals exhibit almost identical trends and levels during the pre-gestation periods. However, in contrast to the absence rates, the two groups only begin to diverge from the postpartum year, as opposed to the gestation year. When comparing panel (b) across the figures, I also see the fraction of treated students leaving public schools is highest in the *Old* sample, followed by the *Middle*, and then the *Young*.

Next, panel (c) of Figures 5 through 7 shows the raw trends in a composite measure of the two prior outcomes. This composite measure captures both margins—increased absence while in

school and being out of school (represented by an absence rate of 100%)—within a single metric. Once again, across all three samples, I observe that both the treatment and control groups have similar trends and levels in the two years prior to the treated individuals becoming pregnant; however, they begin to diverge thereafter. The divergence in the trends observed in panel (c) during the gestation year largely mirrors that of panel (a), indicating a higher increase in the absence rates among the enrolled treated individuals. The greater divergence during the postpartum year mostly reflects that in panel (b), demonstrating that many treated individuals do not enroll in school.

Finally, panel (d) of Figures 5 through 7 depicts the raw trends in the share of students who are chronically absent from school. These plots confirm that the treated and matched control units appear almost identical in terms of chronic absenteeism during the pre-conception periods. However, starting from the year when the treated individuals become pregnant, they experience a higher likelihood of chronic absenteeism compared to their matched controls, thereby diverging from them.

A couple of key takeaways emerge from the raw trends. First, for all outcomes, the treated individuals trend similarly to their matched controls throughout the pre-gestation phase, providing supporting evidence for the parallel trends assumption. Second, the absolute levels in these outcomes are also nearly identical during this phase, bolstering confidence in my research design. Third, the timing of the divergence in the raw trends coincides with the treated individuals' pregnancy or childbirth year, and the magnitude of the divergence increases over time.

Difference-in-Differences Results Figures 8 through 10 plot the difference-in-differences estimates from equation (1).[26] For all outcomes across the three samples, the plots show no systematic differences in trends during the pre-gestation periods, supporting the parallel-trends identification assumption. In addition, the event-study figures confirm that conclusions from the raw trends remain robust after regression adjustment.

Panels (a) and (d) of Figures 8 through 10 demonstrate that individuals who become pregnant

[26]It is worth noting that the results presented in each panel of Figures 8 through 10 correspond to the raw trends in the respective panels of Figures 5 through 7.

one year earlier experience a significant increase in the absence rates and chronic absenteeism following their pregnancies, relative to their matched controls. For these two outcomes, the estimates become larger during the pregnancy year and then peak in the middle of the subsequent year. Panel (b) of Figures 8 through 10 indicates no statistically significant changes in the likelihood of enrollment in public schools during the gestation year, yet shows significant declines in the postpartum year. Finally, in panel (c) of the figures, the treated students exhibit a larger increase in the composite measure of absence after the pregnancy than the matched controls, and the effects continue to strengthen over the entire post-pregnancy period.

To interpret the magnitude of these results, Table 1 displays estimates from equation (2). The first two columns of Table 1 report the mean effects of experiencing pregnancy one year earlier, separately for the gestation and postpartum year, using the *Young* sample. The results from the *Middle* and *Old* samples are presented in the next two columns and the last two columns, respectively, in a similar manner. All the coefficients reported in Table 1 are statistically significant and precisely estimated with a p-value of no more than 0.001. The discussion below focuses on the results from the *Young* sample, though I compare these results to those from the older samples throughout.

The first column in Table 1 show that experiencing pregnancy one year earlier leads to an increase of 2.1 and 5.7 percentage points in the absence rates during the gestation and postpartum year, respectively. Relative to the control means during the same years, these changes correspond to 23.6% and 44.0% increases. I obtain similar estimates for treated individuals in the older samples (columns 2 and 3).

The middle rows in column 4 in Table 1 indicate that the youngest treated individuals are 1.9 percentage points (or 2.0% relative to the control mean) less likely to stay in school after giving birth. The magnitude of these impacts is larger in the older samples. For instance, the middle rows in column 6 show that the oldest treated individuals are 5.1 percentage points (or 5.3%) less likely to be observed in the public school system in the postpartum year. The effects are relatively minimal during pregnancy for all samples, with a decrease of at most around 0.4 percentage points (or 0.4%).

Next, the top rows in columns 7 through 9 of Table 1 reveal that the composite measure of absence increases by 2.4 to 2.7 percentage points (or 22.1% to 26.2%) during the year of pregnancy. In the year of delivery, the measure sees a substantial increase across all samples, with a particularly pronounced rise when examining the older samples. For instance, the middle columns in column 9 of the table show that the measure goes up by 9.9 percentage points (or 71.9%) among those who become a mother at age 17, compared to those who do so a year later.

Lastly, the estimates in column 10 of Table 1 indicate that delaying childbirth from age 15 to 16 would reduce the rate of chronic absenteeism by 6.7 percentage points (or 19.6%) and 14.6 percentage points (or 32.1%) in the first two years following pregnancy. As with the absence rates presented in the first three columns, the estimates for chronic absenteeism are also comparable across the three samples (columns 10 through 12).

In summary, the results from this section demonstrate that experiencing pregnancy one year earlier has a negative impact on school attendance, particularly in the postpartum year. Importantly, the significant decreases in attendance and school enrollment could be key mechanisms through which earlier pregnancies cause adverse long-term consequences for teenagers. Specifically, reduced attendance—a critical input in the education production function—reduces years of completed secondary education and the likelihood of high school graduation. Moreover, reduced attendance may lead to declines in college attendance, college graduation, and later labor market earnings. In the next section, I investigate the effects on each of these long-run outcomes.

While the size of the short-run effects in the first two years appears broadly similar across the samples,[27] it is important to emphasize that women getting pregnant at younger ages have the potential to accumulate more absences, as they would need to stay in school longer to complete secondary education. Furthermore, even the same amount of short-run disruption might have a more profound impact on those affected at relatively younger ages.

[27] Appendix Figure A2 compares the key coefficients (γ and ρ) obtained from the estimation of equation (2) across the three samples for all outcomes.

4.2 Long-Run Effects

Figure 11 presents estimates of the effects of a one-year gap in pregnancy timing on secondary educational attainment by age 20. Specifically, the figure plots the coefficients and 95% confidence intervals from the estimation of equation (3), separately for each sample. From visually inspecting Figure 11, one can see significant adverse impacts on years of schooling and the likelihood of high school graduation across the board. The magnitude of the coefficients relative to the corresponding control means is broadly comparable across the samples, although the point estimates are larger in absolute value in older samples.

The corresponding regression results are reported in Table 2. The odd columns in Table 2 indicate a one-year earlier pregnancy universally leads to a decrease of 0.3 to 0.4 years of schooling. However, it is important to note that a similar decrease in years of schooling is likely to have different implications across the samples. Specifically, a decrease of 0.3 years in schooling for the youngest treated mothers means that their average years of schooling fall below ten years. However, the oldest treated mothers still have around 11 years of schooling even after experiencing a similar level of decrease.

Column 2 in Table 2 shows that becoming a mother one year earlier reduces the probability of high school completion by age 20 for the youngest treated individuals by 4.0 percentage points (or 10.3% relative to the control mean). Columns 4 and 6 in Table 2 report the analogous results for the *Middle* and *Old* samples, indicating a reduction of 9.8 percentage points (or 18.2%) and 11.2 percentage points (or 15.9%), respectively. All the results reported in Table 2 are precisely estimated, with a p-value of less than 0.001.

Figures 12 through 14 present the analogous results for college education. Specifically, the upper half of each figure displays the results for college enrollment by age 23, and the lower half displays the results for college graduation by age 25. For both sets of outcomes, the figures provide the estimates separately for each type of higher education institution. Comparing the estimates from the three figures, I see that the youngest treated women experience the largest decline in college enrollment and graduation rates, both in absolute and relative terms. The declines in college enrollment and graduation are smaller when examining older samples, resulting in the smallest

effects for the oldest sample.

Table 3 through 5 present the corresponding regression results. Column 1 of panel (a) in Table 3 indicates getting pregnant one year earlier leads teenagers to be 2.8 percentage points (or 9.2% with p-value < 0.001) less likely to enroll in any college by age 23. The next column confirms that this decrease is almost entirely driven by a reduction in two-year college enrollment. Moreover, column 1 in panel (b) of Table 3 shows that these individuals are 1.3 percentage points (or 13.5% with p-value $= 0.011$) less likely to obtain any college degree by age 25. In contrast to enrollment, this reduction is primarily due to a decrease in career/technical school.

Though smaller in magnitude, I continue to observe that conceiving one year earlier reduces any college enrollment by 2.2 percentage points (or 6.6% with p-value < 0.001) and by 1.2 percentage points (or 3.2% with p-value $= 0.001$) for the treated individuals aged 16 and 17, respectively. Both of these reductions are again largely attributed to declines in two-year college attendance. However, I do not find any significant impacts on receiving any college degree for both older samples, with p-values no smaller than 0.250. These results can be found in column 1 in panels (a) and (b) of Tables 4 and 5.

Figures 15 through 17 display the results from estimating equation (3) where the dependent variable is either annual employment status in panel (a) or annual earnings in panel (b), both of which are measured at ages from 20 through 25. Consistent with the results for college education, the youngest treated individuals bear the largest cost of early childbearing in terms of forgone employment opportunity and earnings. It is also noteworthy that the gaps in employment and earnings among those who differ in pregnancy timing by one year remain persistent and stable throughout the mid-20s for all samples.

The corresponding regression estimates are in Table 6 through 8. Specifically, Table 6 shows that the youngest treated individuals experience a 2.7 percentage point (or 4.3%) reduction in the likelihood of employment or a $\$752.7$ (or 9.0%) decrease in annual earnings, when averaging the coefficients across ages 20 to 25.[28] Next, Table 7 indicates that the analogous declines in the likeli-

[28] Referring to the results in Figure A3 and Table A1 in Online Appendix, which include only those with non-zero earnings, one can see that much of the reduction in annual earnings is driven by reductions in labor supply on the extensive margin.

hood of employment and annual earnings are 1.3 percentage points (or 2.1%) and $345.1 (or 3.8%), respectively, for the treated individuals who became mothers at age 16. Finally, Table 8 reveals that these labor market disadvantages are even smaller for the oldest treated individuals, with a 1.1 percentage point (or 1.7%) decrease in the probability of being employed and a $144.5 (or 1.6%) drop in annual earnings.

4.3 Robustness Analysis

Compatibility of Short- and Long-Run Models A potential concern with the main results is that the short- and long-run estimates are specific to their respective econometric models, as they are obtained from two distinct specifications. To address this concern, I re-examine my short-run outcomes within the long-run framework and assess the stability of the estimates between the two models. For this exercise, I first construct aggregated short-run outcomes by taking the simple average of their values separately for relative grading periods 0 to 5 (gestation period) and 6 to 11 (postpartum period). I then estimate my long-run model outlined in equation (3), using the aggregated short-run measures as the dependent variables. Figure A4 in Online Appendix displays the coefficients and 95% confidence intervals from these estimations and reproduces the baseline estimates for comparison. The estimates are nearly identical between the two models—across all four outcomes, three samples, and two post-pregnancy periods—indicating that my results are not sensitive to the choice of regression model. In addition, we would expect estimates from both within-individual and across-individual comparisons of outcomes to be closer when my research design effectively isolates random variation in pregnancy timing. Thus, these findings also bolster the credibility of the research design.

Exclusion of Covariates in Long-Run Model I also examined the sensitivity of my long-run regression coefficients to the exclusion of observable controls to assess whether my long-run results are driven by selection on unobservables (Altonji, Elder and Taber, 2005; Oster, 2019). If unobserved factors are indeed important sources of omitted variable bias and contaminate the results, excluding observable characteristics should have a substantial effect on the estimated coefficients. In Figures A5 through A11 in Online Appendix, I repeat the entire long-run analysis by

re-estimating equation (3) without any controls ($\boldsymbol{X}_i$). Specifically, in these figures, I overlay the estimates derived from this parsimonious specification—which controls only for match group fixed effects—with my baseline long-run estimates. I find that the exclusion of a set of covariates has a negligible effect on the estimated coefficients across all long-run outcomes and three samples. Thus, my key long-run findings may not be biased by unobservable differences.

5 Supplemental Policy Analysis

In the previous section, I demonstrated that the underlying sources of the adverse impact of early teen pregnancy—increased absence and dropout rates—are concentrated in the postpartum period rather than the pregnancy period. Motivated by this finding, this section investigates whether providing support during the postpartum year could mitigate these negative mechanisms for teenage mothers.

5.1 Background on Life Skills Program for Student Parents in Texas

The Life Skills Program for Student Parents (LSPSP) is designed to enhance attendance, increase high school completion, and improve parenting skills and job readiness for *expecting* female students and *parenting* students (both male and female).[29] It was initially introduced as a pilot program for targeted school districts during the 1989–1990 academic year and was formally established as an annually renewed grant program funded by the state government through Texas Legislature Senate Bill 1 in 1995 (Baenen, 1990).[30] Since its official inception, state lawmakers have approved the budget allocated to the LSPSP throughout the academic year 2010–2011. However, in 2011, the Texas Legislature passed House Bill 1, which removed the *entire* budget for the LSPSP as part of public education spending cuts. The funding has not been restored since then (or the 2011–2012 academic year).

Each school district can choose whether to seek (or apply for) funding to develop its own program for assisting teen parents. However, a school district that receives the grant shall include

[29] The LSPSP is formerly known as Pregnancy, Parenting, and Education (PEP).

[30] Here, the word "renew" has two implications. First, the state legislative reviews and approves whether to allocate the budget to the LSPSP each year. Second, each school district is required to apply for funding annually.

mandatory components in its program as stipulated by the law.[31] Among these required elements, many are associated with child care services. For instance, districts awarded LSPSP funds must provide child care, either through on-site facilities or by partnering with community child care centers. They should also arrange transportation for teen parents and their children to and from the campus and daycare facilities.

Note the funding withdrawal does not necessarily mean districts that lost the grant completely discontinued their services. These districts could offset the impact by utilizing other financial resources to maintain their support, either fully or at a reduced level.[32] Unfortunately, I cannot observe whether and to what extent each school district reallocated its budget in response to this grant elimination. Therefore, I focus on estimating the reduced-form effects of the LSPSP budget removal.

5.2 Data and Sample

The TEA data (detailed in Section 2) also contain information on the LSPSP at both the district and individual levels. Specifically, I can identify the LSPSP grant award status of each school district from the district-level data and observe the take-up status of each student from the individual-level data.[33]

Using the district-level information, I first limit my sample to districts that either *continuously* received the LSPSP grant (*treated* districts) or *never* received it (*control* districts) during the academic years from 2003–2004 to 2010–2011. This restriction ensures my sample consists of a clear treatment group and a clean control group, by excluding districts with an ambiguous treatment status, such as those that obtained the LSPSP fund only in specific academic years. Figure 18 dis-

[31] For additional information, refer to Texas Education Code Section 29.085, which is available at https://texas.public.law/statutes/tex._educ._code_section_29.085 (accessed August 2023).

[32] In Texas, both pregnant students and teen parents are classified as at-risk dropout students, and school districts need to combine local, state, and federal resources to deliver maximized services for at-risk students (TSAO, 2004). Given this context, each school district can redistribute their spending for at-risk students to make up for the insufficient budget for teen parents after the funding cut. Additionally, it is theoretically possible that districts without the LSPSP grant provide support to teen parents through other funding sources generally allocated to a broader population of at-risk students. For these reasons, I cannot present a single first-stage statistic accounting for these responses, due to data limitations.

[33] Enrolled parenting students are eligible to participate in the LSPSP program (for multiple academic years), as long as they are 21 years old or younger, and I can observe the take-up status for each student on an *annual* basis.

plays the treatment status (or identifying variation) for 310 school districts in my sample—169 treated districts and 141 control districts—representing approximately 77.1% of the female student population in Texas.[34]

Within these treated and control districts, I focus on pregnant students who entered the postpartum period at the age of 19 or younger and were in grades 9–12 during the academic years 2003–2004 to 2014–2015. I then further restrict attention to those who have stayed in the same school district for three years leading up to their postpartum year. This refinement also aims to drop individuals with the fuzzy treatment, due to moving across school districts. My final *policy-analysis* sample includes 73,778 students (64,069 from 169 treated districts and 9,709 from 141 control districts).

The two outcomes I consider are (1) *annual* absence rates measured during the postpartum academic year and (2) an indicator for attending *all* six grading periods in the postpartum academic year, both of which closely align with the motivation behind this policy analysis.

5.3 Econometric Model

To examine the effects of defunding the LSPSP grant, I use a difference-in-differences model, in which I compare changes in outcomes between older cohorts who had access to the LSPSP in treated districts and younger cohorts who did not in the same districts, relative to analogous changes in control districts. Specifically, I estimate the following equation:

$$Y_{idt} = \sum_{k=-8,\, k\neq -1}^{3} \left\{ \delta_k \times Treat_d \times \mathbf{1}\big(t - 2012 = k\big) \right\} + \theta_d + \pi_t + \boldsymbol{\Omega}' \boldsymbol{X}_i + \boldsymbol{z}_{dt} + \eta_{idt}, \tag{4}$$

where Y_{idt} is an outcome for student i in school district d in year t. $Treat_d$ and $\mathbf{1}\big(t - 2012 = k\big)$ are indicators for treated districts and relative years since the grant removal, respectively. District and year fixed effects are captured by θ_d and π_t. $\boldsymbol{X}_i$ represents individual-level characteristics, including enrolled grade, economically disadvantaged status, and an indicator for any disciplinary action records, all measured in the pre-gestation year. $\boldsymbol{X}_i$ also includes indicators for race/ethnicity

[34] The reported statistic is from the 2010–2011 academic year, that is, the year prior to the defunding of the grant.

and a full set of birth year x age (at the postpartum year) fixed effects.[35] $\boldsymbol{z}_{dt}$ denotes time-varying district-level controls—the number of female students in grades 7–12, the number of pregnant students counted by the PRS participation, and the ratio of the latter to the former, as a measure of congestion for pregnancy-related support. Standard errors are clustered at the district level. Note each individual is observed only once, and their outcome is measured at the postpartum year.

The relative-year specific coefficients $\boldsymbol{\delta_k}$ are the parameters of interest, measuring the mean differences in outcomes between treated and control districts, relative to those that occurred in the omitted year (i.e., the 2010–2011 academic year indexed by $k = -1$).

5.4 Results

I begin by presenting the fraction of pregnant students who participated in the LSPSP program. Figure 19 displays the take-up rates for the LSPSP for each academic year among those who completed their pregnancies in that year. As shown in Figure 19, the program served nearly all teenage mothers in treated districts, with rates reaching a mean of around 90% during the pre-budget-cut periods. However, these rates experienced a sharp decline to zero due to the complete removal of the budget starting from the 2011–2012 academic year.[36]

Figure 20 visually presents the key coefficients ($\boldsymbol{\delta_k}$) along with the associated 95% confidence intervals, with panel (a) displaying the results for the annual absence rates and panel (b) showing the results for an indicator for completing the academic year. For both outcomes, the plots show no evidence of systematic differences in trends prior to the discontinuation of the grant, providing support for the parallel-trends identification assumption.

Based on the aggregated post-period estimates (from the baseline specification),[37] which I report in the lower right corner of each figure (and in Table 9), the removal of the LSPSP budget

[35] Recall that the controls in $\boldsymbol{X}_i$ are similar to the variables I used for matching in the main analysis.

[36] As discussed in Section 5.1, whereas the mean take-up rate reported in Figure 19 is not suitable for scaling the reduced-form estimates, it is informative for understanding the estimates in the main analysis. As described in Section 2, one limitation of the PRS data is that I cannot *directly* observe pregnancy outcomes (e.g., birth, abortion, miscarriage, etc.) for pregnant students. However, given that the LSPSP is generally available for teen parents, the mean take-up rate *indirectly* informs us that at least approximately 90% of pregnant students in my sample indeed become mothers. Because one may choose either not to receive the LSPSP services or to participate in the LSPSP from the second postpartum year or later, the actual share of pregnant students who give birth would be larger than the reported mean.

[37] I obtained the mean post-period estimates by estimating equation (4) after replacing $\mathbf{1}\big(t - 2012 = k\big)$ with $\mathbf{1}\big(t - 2012 \geq 0\big)$.

resulted in an increase in annual absence rates in the postpartum year by 1.911 percentage points (or a 14.1% increase relative to the control mean). It also led to a 4.205 percentage point decrease (or a 5.2% decrease relative to the control mean) in the likelihood of finishing the postpartum academic year. Comparing across the specifications, Table 9 shows the estimates remain stable when I exclude controls, expand the sample (for the first outcome), and use an alternative measure (for the second outcome).

Given that the grant was zeroed out in the academic year 2011–2012, the time for other outcomes (e.g., college enrollment) to fully materialize has been insufficient, especially for those who recently became mothers (i.e., younger cohorts).[38] For this reason, I leave for future research the investigation of the impact of losing access to the LSPSP on these outcomes (i.e., post-secondary education or labor market).

6 Conclusion

This paper investigates the long-term consequences of differential pregnancy timing among teenagers who become mothers between the ages of 15 and 18. Specifically, I explore the impact of a one-year difference in age at birth on their educational and labor market outcomes, leveraging novel administrative data from Texas. To identify the effect of pregnancy timing, I examine both within-individual changes in outcomes surrounding pregnancy and across-individual comparisons in outcomes after pregnancy, among matched individuals balanced on various baseline characteristics but differing in the timing of giving birth by one year.

I find that teenagers who give birth one year earlier are more likely to be absent from or leave school in the first two years after pregnancy than comparable teenagers who give birth one year later. These immediate disruptions are universally experienced by teenage mothers, regardless of the age at which they give birth, but they are particularly pronounced during the year following childbirth. I further find that teenagers who become mothers one year earlier experience far-reaching negative impacts on their future outcomes. Specifically, these impacts include a reduction in completed secondary education by age 20, a decrease in college enrollment and graduation

[38] In addition, younger mothers in their early 20s are likely to be affected by COVID-19.

in the early 20s, and lower employment and earnings in the mid-20s. Notably, the younger the teenager is when giving birth, the greater the long-term adverse consequences they bear.

These findings have important policy implications. First, my results indicate that the benefits of delaying pregnancy vary by age of the mother—with the largest educational and labor market gains among the youngest mothers. These results can inform the targeting of policy aimed at preventing or delaying pregnancy among teenagers and policies aimed at mitigating the disruptive effects of childbirth among teenagers.

Second, when evaluating past policy initiatives, researchers and policymakers have focused solely on the extensive margin—the absolute decrease in the teen birth rate—perhaps due to the absence of causal evidence on the intensive margin—the change in age composition of teenage mothers (Kearney and Levine, 2015). However, my findings suggest that policies aimed at reducing teen pregnancy rates (through, for example, expanding birth control access or sex education programs) may result in improved outcomes for women not only through reductions in the teen birth rate but also through shifting the age at which teen mothers give birth toward later teenage years. Thus, my results suggest policymakers should broaden the scope of the impacts they consider when evaluating the benefits of such policies.

The estimated benefits of delaying pregnancy among teenagers may underestimate the total benefits because there are many unmeasured outcomes that this paper does not consider but could improve. At a minimum, for teenage mothers themselves, delaying childbirth could also lead to better health, reduced poverty, and decreased reliance on social programs. Additionally, given the documented persistent inter-generational effects of teen pregnancy (e.g., Aizer, Devereux and Salvanes (2020)), children of teen mothers might also benefit from the improved outcomes of their mothers. Furthermore, a recent study by Heissel (2021) suggests that these benefits might even extend to siblings and other family members of teen mothers. Exploring how pregnancy timing among teenage mothers impacts these understudied outcomes for both the mothers and those connected to them is an important area for future research.

References

Abadie, Alberto, and Guido W. Imbens. 2006. "Large Sample Properties of Matching Estimators for Average Treatment Effects." *Econometrica*, 74(1): 235–267.

Aizer, Anna, Paul Devereux, and Kjell Salvanes. 2020. "Grandparents, Moms, or Dads? Why children of teen mothers do worse in life." *Journal of Human Resources*.

Altonji, Joseph G., Todd E. Elder, and Christopher R. Taber. 2005. "Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools." *Journal of Political Economy*, 113(1): 151–184.

Andresen, Martin Eckhoff, and Emily Nix. 2022. "What Causes the Child Penalty? Evidence from Adopting and Same-Sex Couples." *Journal of Labor Economics*, 40(4): 971–1004.

Angelov, Nikolay, Per Johansson, and Erica Lindahl. 2016. "Parenthood and the Gender Gap in Pay." *Journal of Labor Economics*, 34(3): 545–579.

Ashcraft, Adam, Iván Fernández-Val, and Kevin Lang. 2013. "The Consequences of Teenage Childbearing: Consistent Estimates When Abortion Makes Miscarriage Non-random." *The Economic Journal*, 123(571): 875–905.

Baenen, Nancy R. 1990. "Pregnancy, Education, and Parenting Pilot (PEP): Evaluation 1989–90 (ED325519)." Austin Independent School District.

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

Baker, Michael, Jonathan Gruber, and Kevin Milligan. 2008. "Universal Child Care, Maternal Labor Supply, and Family Well-Being." *Journal of Political Economy*, 116(4): 709–745.

Blau, Francine D., and Lawrence M. Kahn. 2000. "Gender Differences in Pay." *Journal of Economic Perspectives*, 14(4): 75–99.

Carr, Jillian B., and Analisa Packham. 2017. "The Effects of State-Mandated Abstinence-Based Sex Education on Teen Health Outcomes." *Health Economics*, 26(4): 403–420.

Currie, Janet. 2009. "Healthy, Wealthy, and Wise: Socioeconomic Status, Poor Health in Childhood, and Human Capital Development." *Journal of Economic Literature*, 47(1): 87–122.

Dahl, Gordon B., Katrine V. Løken, Magne Mogstad, and Kari Vea Salvanes. 2016. "What Is the

Case for Paid Maternity Leave?" *The Review of Economics and Statistics*, 98(4): 655–670.

Deming, David J. 2022. "Four Facts about Human Capital." *Journal of Economic Perspectives*, 36(3): 75–102.

Eichmeyer, Sarah, and Christina Kent. 2022. "Parenthood in Poverty." CEPR Discussion Papers 17722.

Fletcher, Jason M., and Barbara L. Wolfe. 2009. "Education and Labor Market Consequences of Teenage Childbearing." *Journal of Human Resources*, 44(2): 303–325.

Gallen, Yana, Juanna Schrøter Joensen, Eva Rye Johansen, and Gregory F. Veramendi. 2023. "The Labor Market Returns to Delaying Pregnancy." *SSRN Electronic Journal*.

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

Goldin, Claudia. 2014. "A Grand Gender Convergence: Its Last Chapter." *American Economic Review*, 104(4): 1091–1119.

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

Goldin, Claudia, Lawrence F. Katz, and Ilyana Kuziemko. 2006. "The Homecoming of American College Women: The Reversal of the College Gender Gap." *Journal of Economic Perspectives*, 20(4): 133–156.

Havnes, Tarjei, and Magne Mogstad. 2011. "Money for nothing? Universal child care and maternal employment." *Journal of Public Economics*, 95(11): 1455–1465. Special Issue: International Seminar for Public Economics on Normative Tax Theory.

Heissel, Jennifer A. 2021. "Teen Fertility and Siblings' Outcomes." *Journal of Human Resources*, 56(1): 40–72.

Holmlund, Helena. 2005. "Estimating Long-Term Consequences of Teenage Childbearing." *Journal of Human Resources*, XL(3): 716–743.

Hotz, V. Joseph, Charles H. Mullin, and Seth G. Sanders. 1997. "Bounding Causal Effects Using Data from a Contaminated Natural Experiment: Analysing the Effects of Teenage Childbearing." *The Review of Economic Studies*, 64(4): 575–603.

Janssen, Aljoscha, and Elle Parslow. 2021. "Pregnancy persistently reduces alcohol purchases: Causal evidence from scanner data." *Health Economics*, 30(2): 231–247.

Kahn-Lang, Ariella, and Kevin Lang. 2020. "The Promise and Pitfalls of Differences-in-Differences: Reflections on 16 and Pregnant and Other Applications." *Journal of Business & Economic Statistics*, 38(3): 613–620.

Kearney, Melissa S., and Phillip B. Levine. 2012. "Why Is the Teen Birth Rate in the United States So High and Why Does It Matter?" *Journal of Economic Perspectives*, 26(2): 141–63.

Kearney, Melissa S., and Phillip B. Levine. 2015. "Investigating recent trends in the U.S. teen birth rate." *Journal of Health Economics*, 41: 15–29.

Kleven, Henrik, Camille Landais, and Jakob Egholt Søgaard. 2019. "Children and Gender Inequality: Evidence from Denmark." *American Economic Journal: Applied Economics*, 11(4): 181–209.

Lang, Kevin, and Russell Weinstein. 2015. "The Consequences of Teenage Childbearing before Roe v. Wade." *American Economic Journal: Applied Economics*, 7(4): 169–97.

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

Massenkoff, Maxim N, and Evan K Rose. 2022. "Family Formation and Crime." National Bureau of Economic Research Working Paper 30385.

Mosher, William D., Jo Jones, and Joyce C. Abma. 2012. "Intended and Unintended Births in the United States: 1982–2010." 55, Hyattsville, MD: National Center for Health Statistics.

Myers, Caitlin Knowles. 2017. "The Power of Abortion Policy: Reexamining the Effects of Young Women's Access to Reproductive Control." *Journal of Political Economy*, 125(6): 2178–2224.

Olivetti, Claudia, and Barbara Petrongolo. 2017. "The Economic Consequences of Family Policies: Lessons from a Century of Legislation in High-Income Countries." *Journal of Economic Perspectives*, 31(1): 205–30.

Oster, Emily. 2019. "Unobservable Selection and Coefficient Stability: Theory and Evidence." *Journal of Business & Economic Statistics*, 37(2): 187–204.

Packham, Analisa. 2017. "Family planning funding cuts and teen childbearing." *Journal of Health Economics*, 55: 168–185.

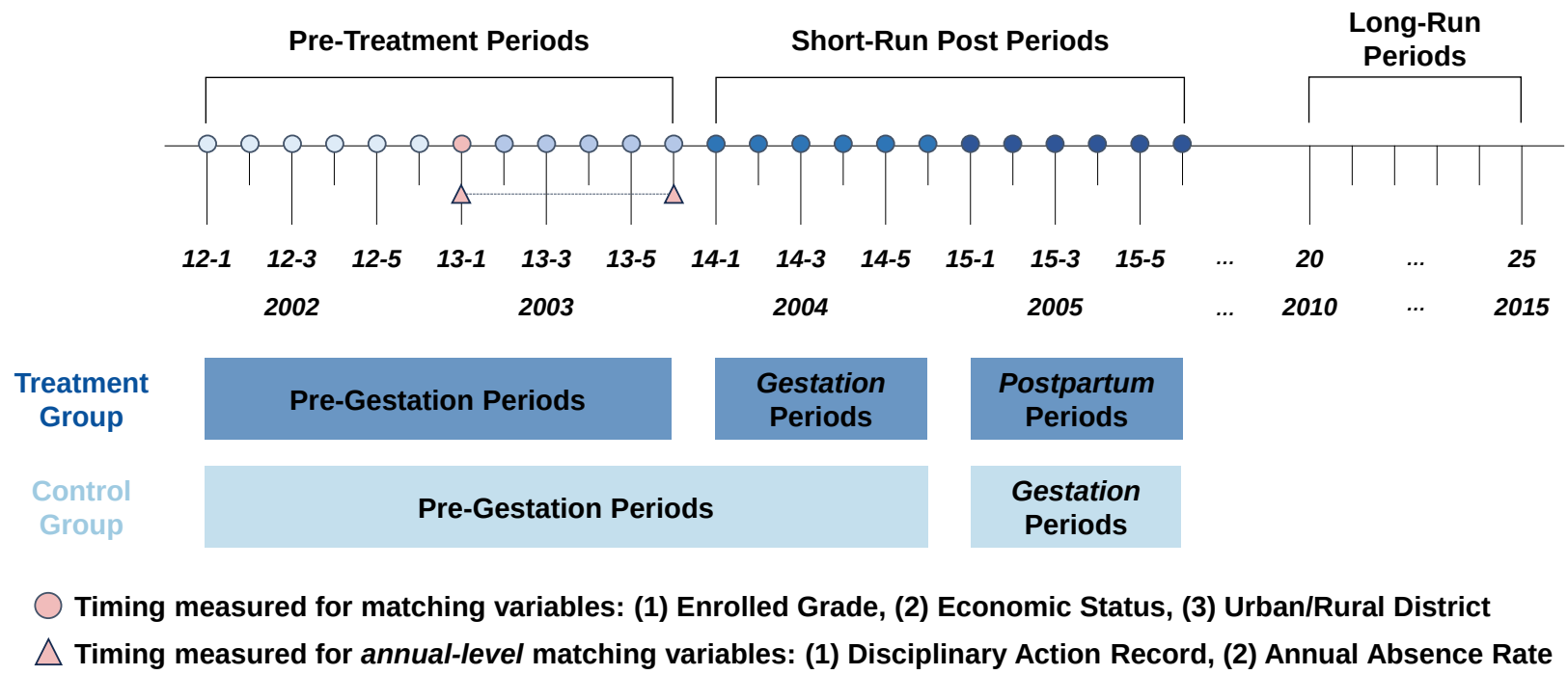
Paton, David, Stephen Bullivant, and Juan Soto. 2020. "The impact of sex education mandates on teenage pregnancy: International evidence." *Health Economics*, 29(7): 790–807.

Schönberg, Uta, and Johannes Ludsteck. 2014. "Expansions in Maternity Leave Coverage and Mothers' Labor Market Outcomes after Childbirth." *Journal of Labor Economics*, 32(3): 469–505.

Schulkind, Lisa, and Danielle H. Sandler. 2019. "The Timing of Teenage Births: Estimating the Effect on High School Graduation and Later-Life Outcomes." *Demography*, 56(1): 345–365.

Texas State Auditor's Office (TSAO). 2004. "An Audit Report on Measuring Effectiveness of State and Federal Funding for At-Risk Students: Report No. 05-009." Texas State Auditor's Office.

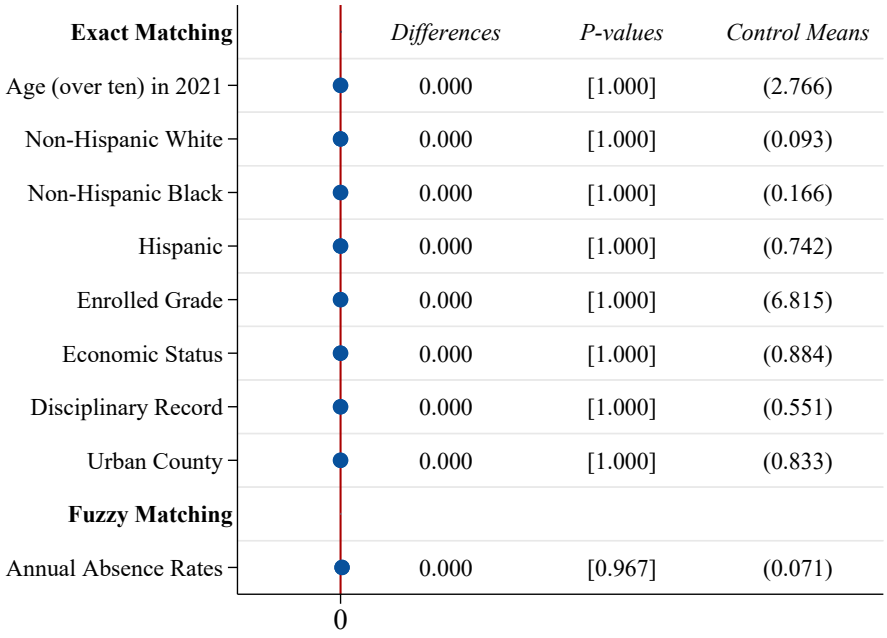
Figure 1: Follow-Up Timeline for Treated and Matched Control Individuals: Example of *Young* Sample (Age 15 vs. 16)



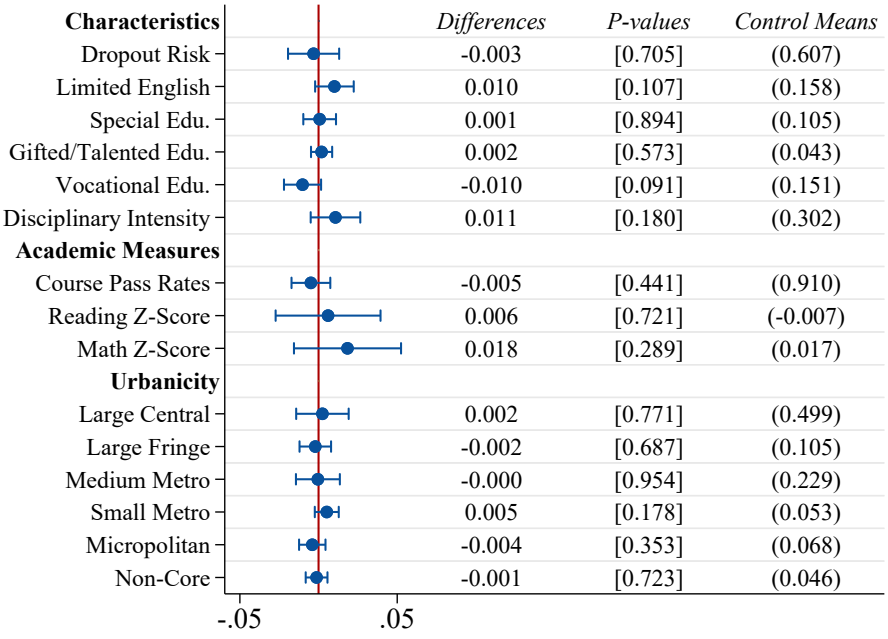
NOTES: The figure above illustrates the follow-up timeline and the timing of pre-determined characteristics measured for the *Young* sample. In the short-run analysis, I follow both treated and matched control individuals from ages 12 through 15, measuring their outcomes over these four years. In these four years, each year consists of six grading periods, totaling 24 periods. For treated individuals, the first 12 periods (i.e., at ages 12 and 13) represent the pre-pregnancy academic years, the next 6 periods (i.e., at age 14) represent the gestation year, and the final 6 periods (i.e., at age 15) represent the postpartum year. For the long-run analysis, I track both groups from ages 20 to 25, measuring their outcomes once at each age. Baseline characteristics used for matching are measured for both groups at age 13, as depicted in the figure. The follow-up timelines for the *Middle* and *Old* samples are analogous, but they start from ages 13 and 14, respectively, instead of age 12.

Figure 2: Balance Test for *Young* Sample—Become Mothers at Ages 15 vs. 16

(a) Matching Variables



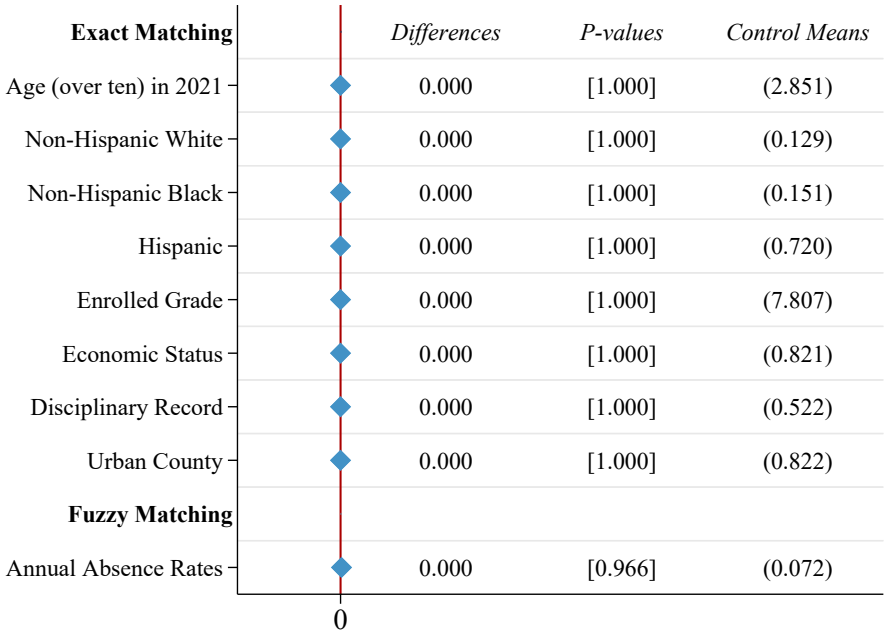
(b) Non-Matching Variables



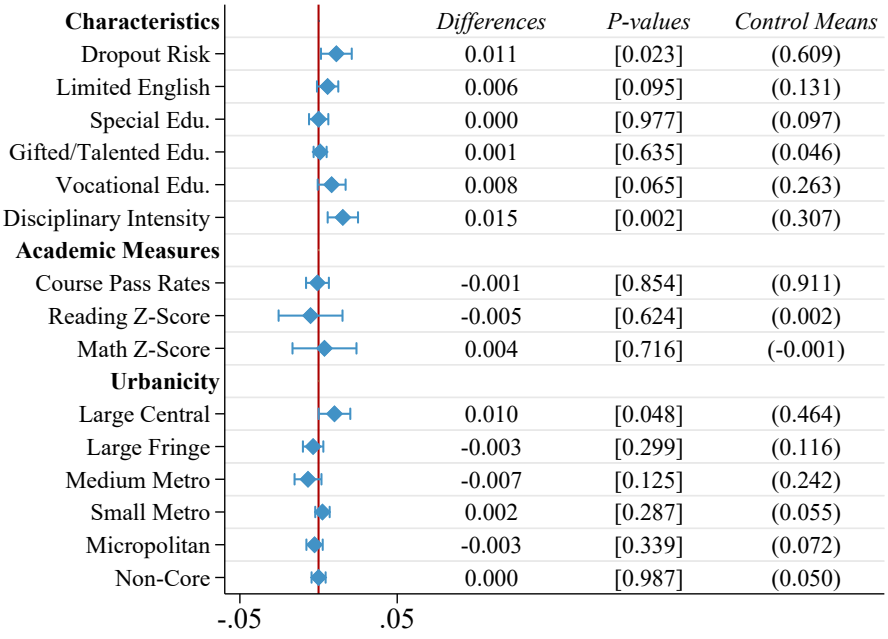
NOTES: The figure above displays a balance test for matching variables in panel (a) and for non-matching variables in panel (b), both for the *Young* sample. For each indicated characteristic, I plot the coefficients and 95% confidence intervals, with the mean difference being the treatment group minus the control group, for that characteristic. The estimates for differences, p-values [in brackets], and control group means (in parentheses) are presented on the right side of each panel.

Figure 3: Balance Test for *Middle* Sample—Become Mothers at Ages 16 vs. 17

(a) Matching Variables



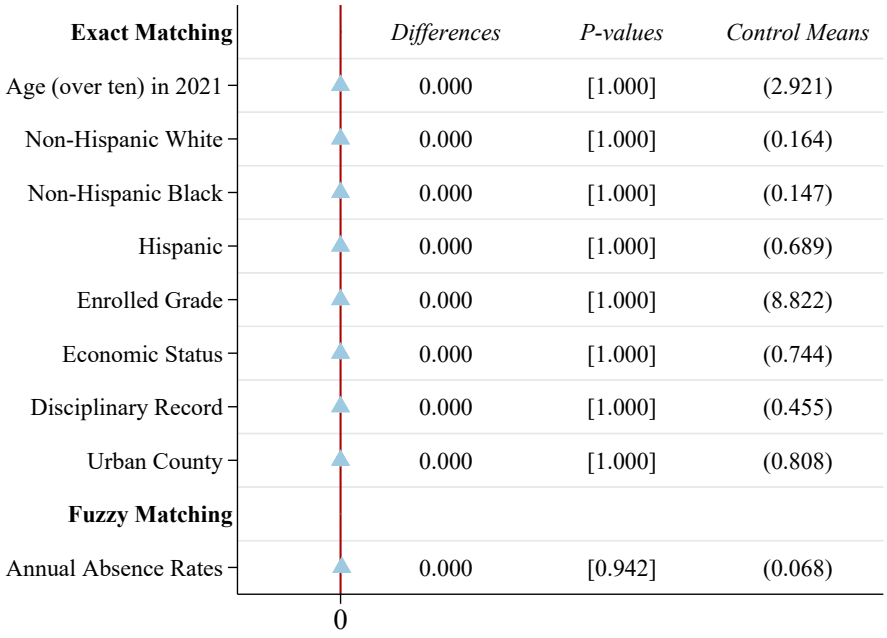
(b) Non-Matching Variables



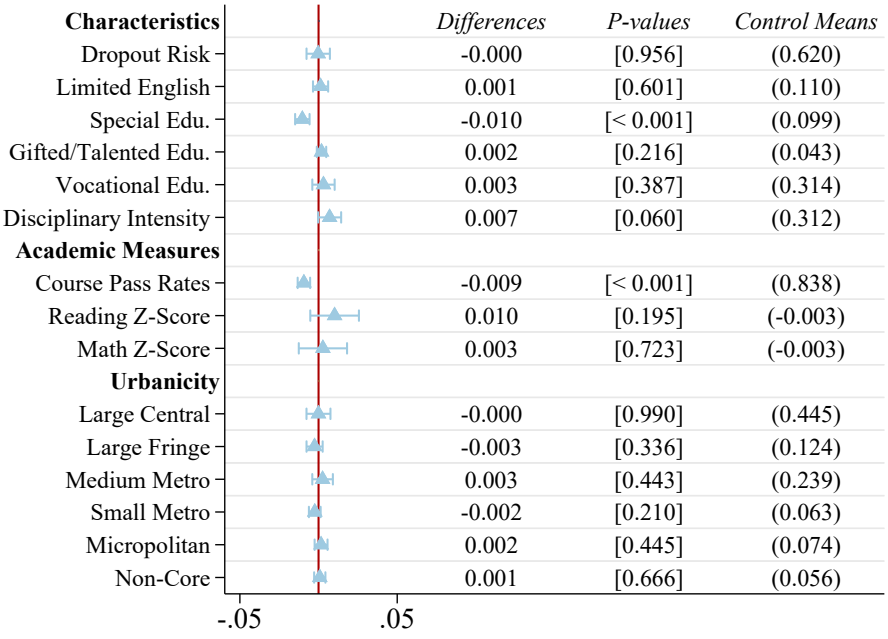
NOTES: The figure above displays a balance test for matching variables in panel (a) and for non-matching variables in panel (b), both for the *Middle* sample. For each indicated characteristic, I plot the coefficients and 95% confidence intervals, with the mean difference being the treatment group minus the control group, for that characteristic. The estimates for differences, p-values [in brackets], and control group means (in parentheses) are presented on the right side of each panel.

Figure 4: Balance Test the *Old* Sample—Become Mothers at Ages 17 vs. 18

(a) Matching Variables

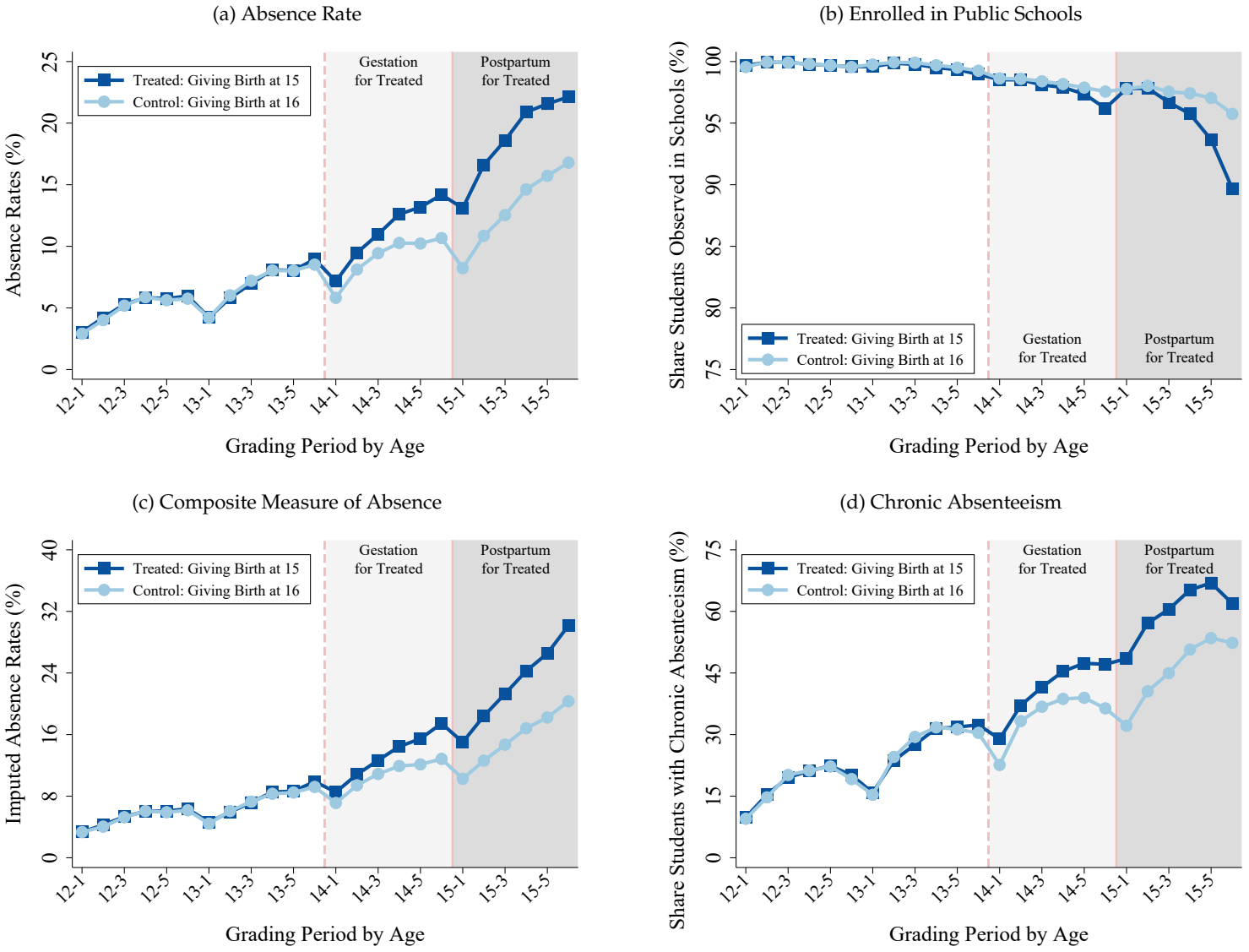


(b) Non-Matching Variables



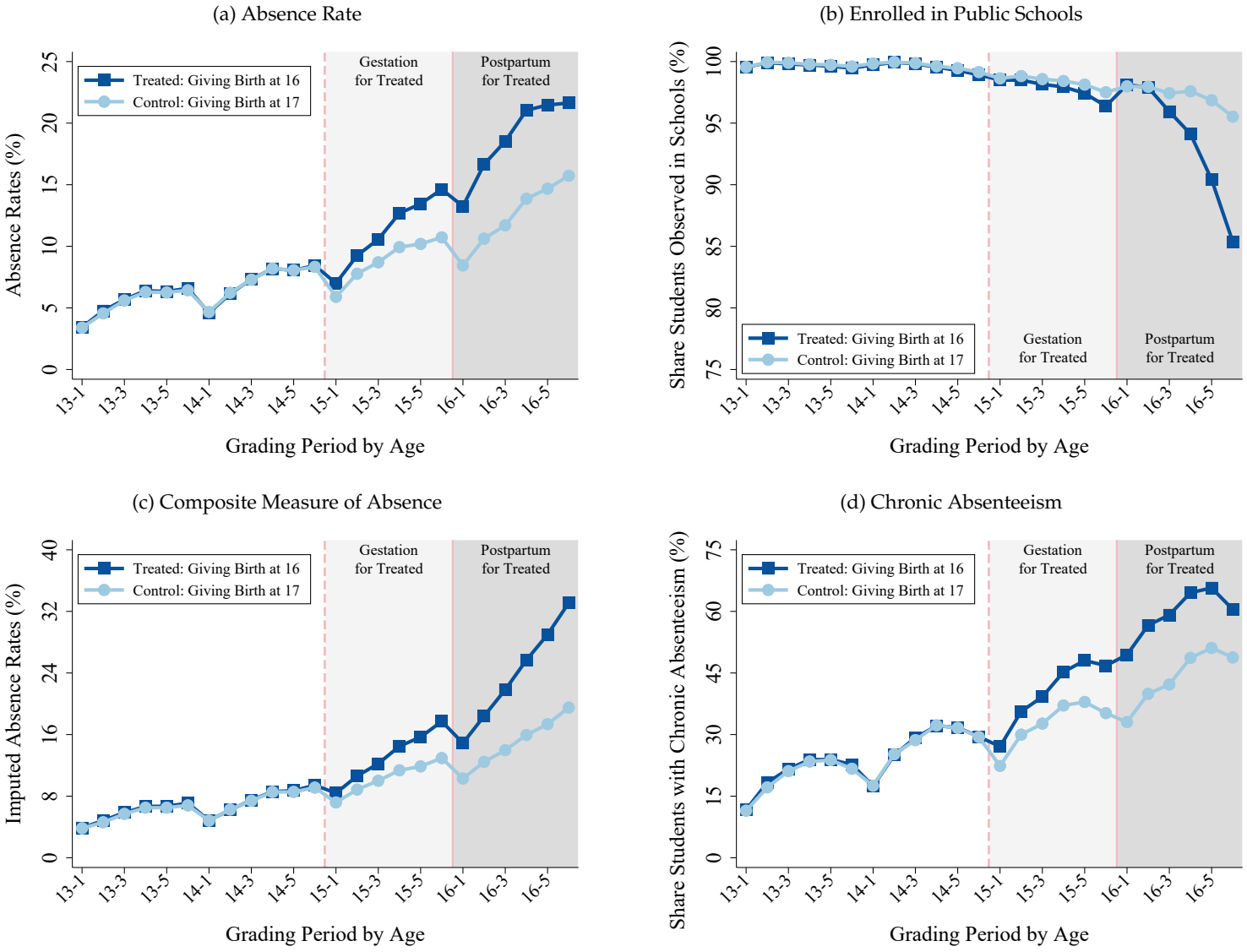
NOTES: The figure above displays a balance test for matching variables in panel (a) and for non-matching variables in panel (b), both for the *Old* sample. For each indicated characteristic, I plot the coefficients and 95% confidence intervals, with the mean difference being the treatment group minus the control group, for that characteristic. The estimates for differences, p-values [in brackets], and control group means (in parentheses) are presented on the right side of each panel.

Figure 5: Raw Trends: Short-Run Effects on Educational Outcome—Become Mothers at Ages 15 vs. 16



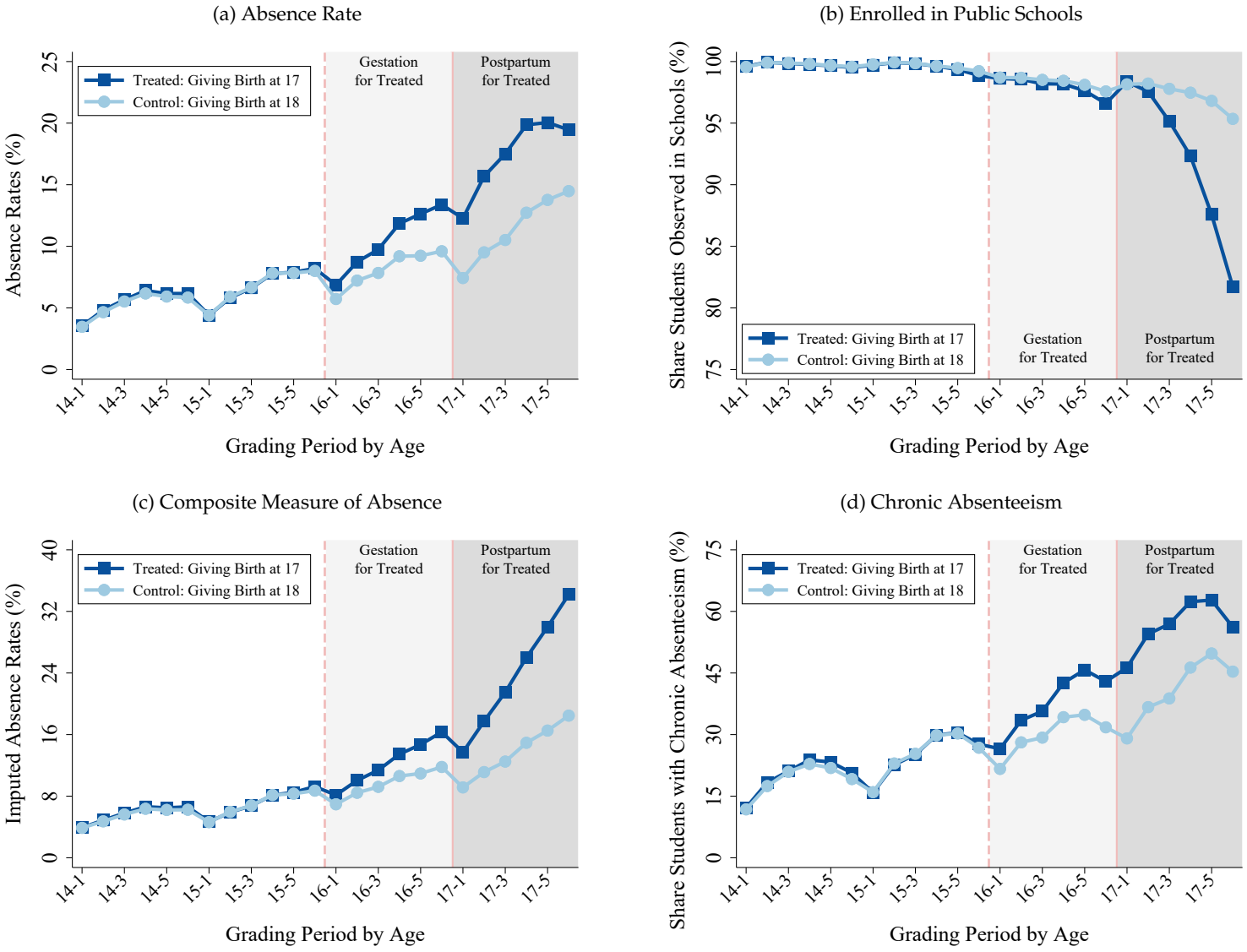
NOTES: The figure above displays raw trends in four short-run outcomes for the *Young* sample, separately for treated individuals (in darker blue) and matched control individuals (in lighter blue). Panel (a) plots the raw means of absence rates among those enrolled. Panel (b) plots the raw means of the share of students enrolled in public schools. Panel (c) plots the raw means of a composite measure of absences—the absence rate for enrolled individuals and 100% for those not enrolled. Panel (d) plots the raw means of the share of students with chronic absenteeism—defined as an absence rate of 10% or greater. For all panels, the area shaded in lighter gray represents the gestation year, and that in darker gray represents the postpartum year for treated individuals. See Figure 1 for more details on the timeline.

Figure 6: Raw Trends: Short-Run Effects on Educational Outcome—Become Mothers at Ages 16 vs. 17



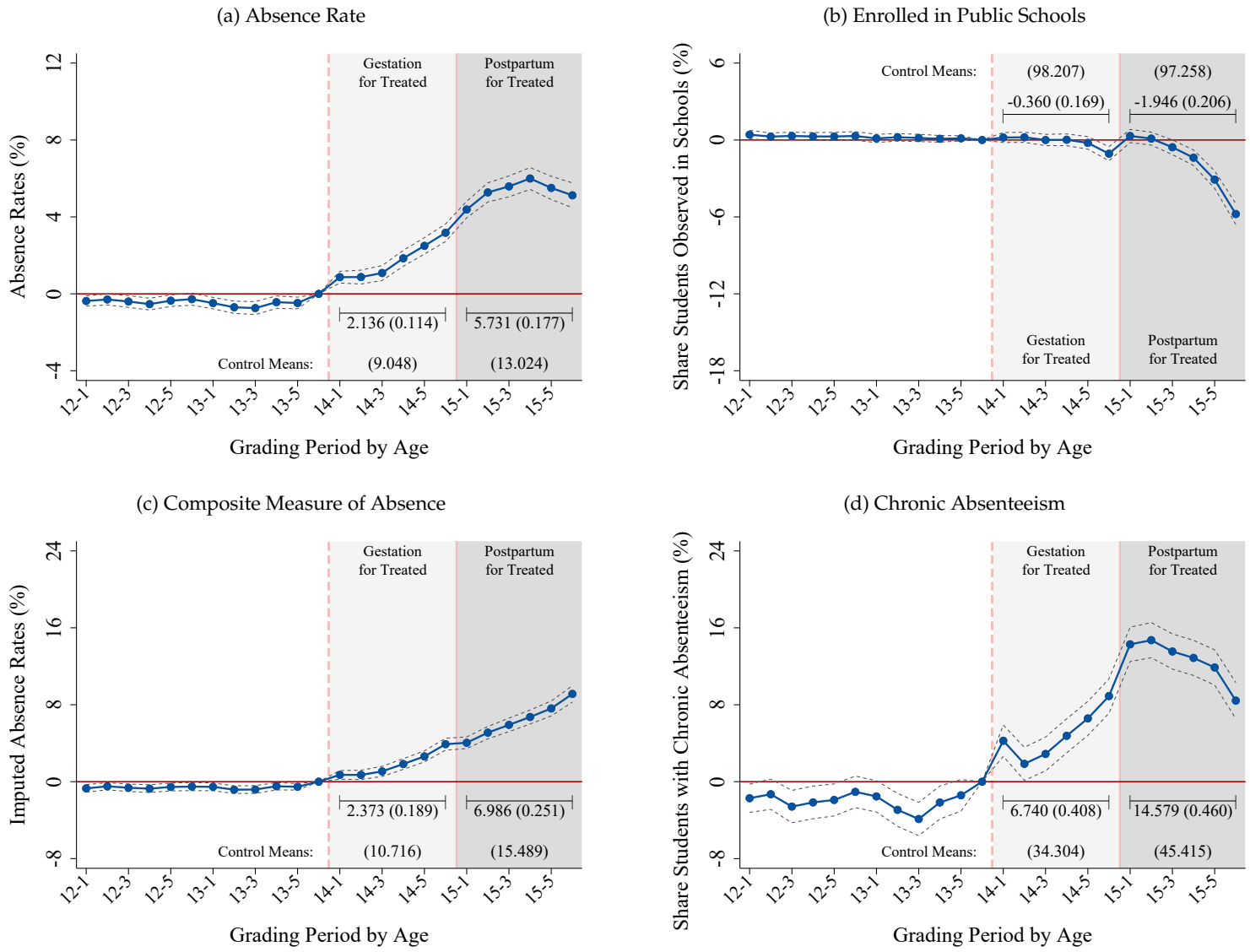
NOTES: The figure above displays raw trends in four short-run outcomes for the *Middle* sample, separately for treated individuals (in darker blue) and matched control individuals (in lighter blue). Panel (a) plots the raw means of absence rates among those enrolled. Panel (b) plots the raw means of the share of students enrolled in public schools. Panel (c) plots the raw means of a composite measure of absences—the absence rate for enrolled individuals and 100% for those not enrolled. Panel (d) plots the raw means of the share of students with chronic absenteeism—defined as an absence rate of 10% or greater. For all panels, the area shaded in lighter gray represents the gestation year, and that in darker gray represents the postpartum year for treated individuals. See Figure 1 for more details on the timeline.

Figure 7: Raw Trends: Short-Run Effects on Educational Outcome—Become Mothers at Ages 17 vs. 18



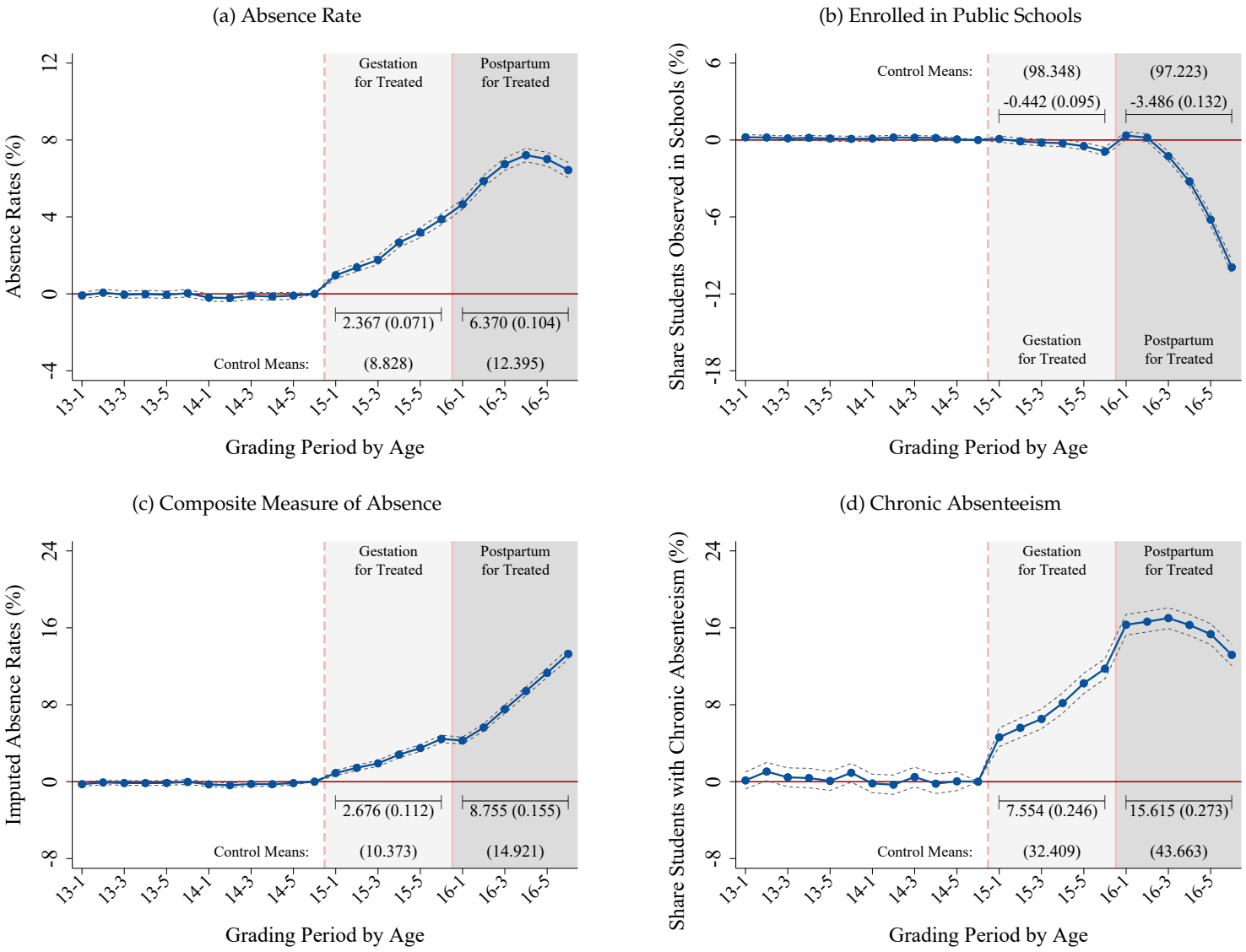
NOTES: The figure above displays raw trends in four short-run outcomes for the *Old* sample, separately for treated individuals (in darker blue) and matched control individuals (in lighter blue). Panel (a) plots the raw means of absence rates among those enrolled. Panel (b) plots the raw means of the share of students enrolled in public schools. Panel (c) plots the raw means of a composite measure of absences—the absence rate for enrolled individuals and 100% for those not enrolled. Panel (d) plots the raw means of the share of students with chronic absenteeism—defined as an absence rate of 10% or greater. For all panels, the area shaded in lighter gray represents the gestation year, and that in darker gray represents the postpartum year for treated individuals. See Figure 1 for more details on the timeline.

Figure 8: Main Results: Short-Run Effects on Educational Outcome—Become Mothers at Ages 15 vs. 16



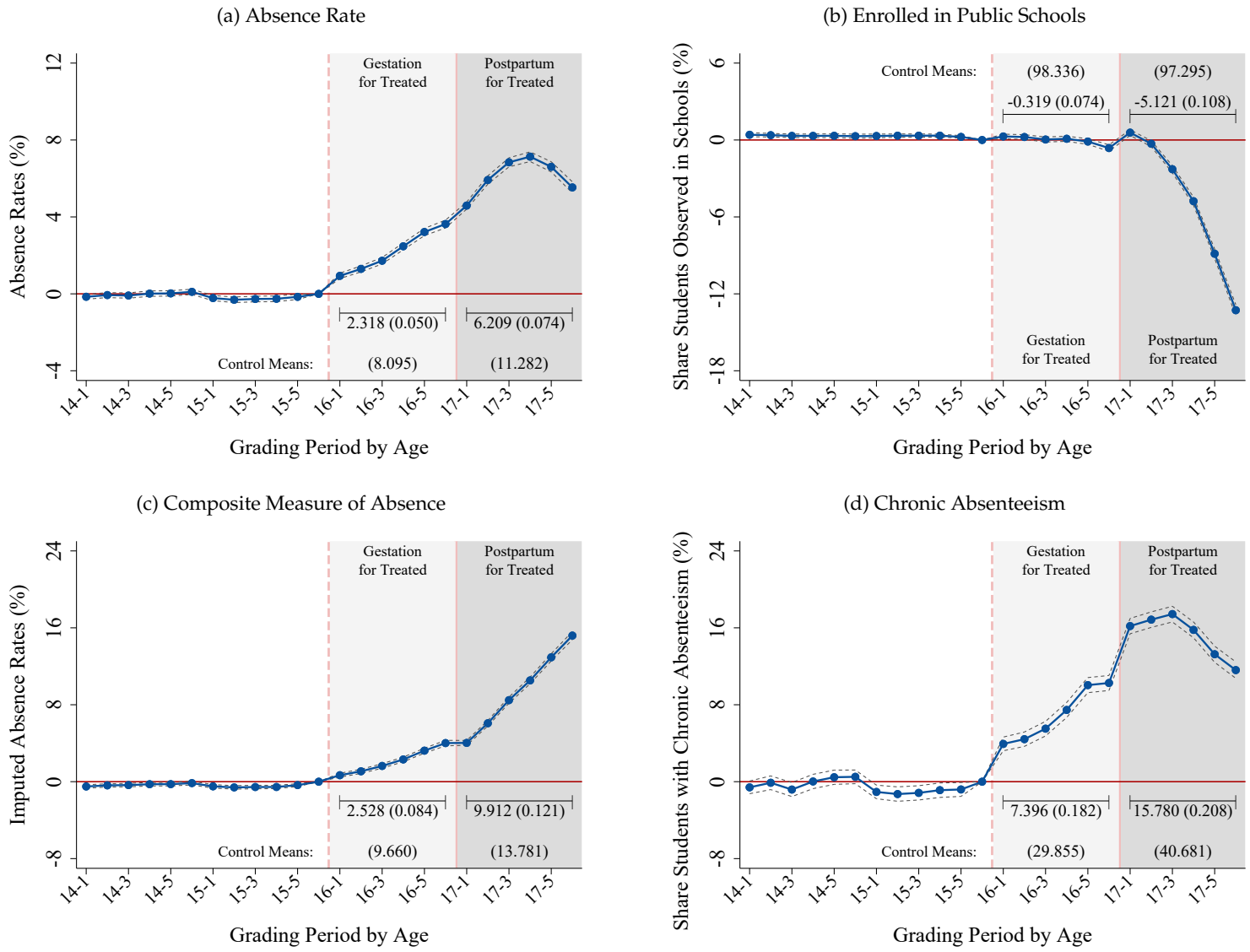
NOTES: The figure above plots coefficients on the interactions between the indicator for treated individuals and the indicators for each of the relative grading periods since pregnancy of the treated individuals from the estimation of equation (1). The dotted lines represent 95% confidence intervals calculated using standard errors clustered at the individual level. The last grading period in the year before the treated individuals become pregnant is the omitted category. The regressions use the *Young* sample and include individual and match group by relative grading period fixed effects. The mean effect estimates for gestation and postpartum years, obtained from the estimation of equation (2), are also displayed either at the bottom or top right of each panel. The corresponding raw trends can be found in Figure 5.

Figure 9: Main Results: Short-Run Effects on Educational Outcome—Become Mothers at Ages 16 vs. 17



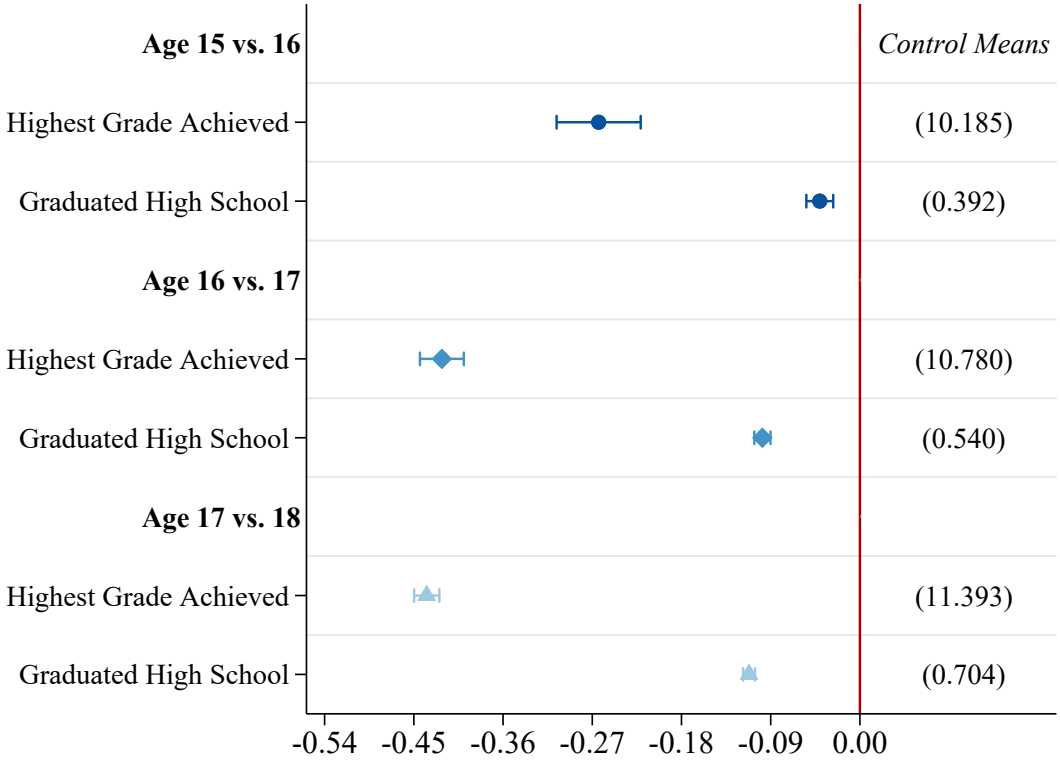
NOTES: The figure above plots coefficients on the interactions between the indicator for treated individuals and the indicators for each of the relative grading periods since pregnancy of the treated individuals from the estimation of equation (1). The dotted lines represent 95% confidence intervals calculated using standard errors clustered at the individual level. The last grading period in the year before the treated individuals become pregnant is the omitted category. The regressions use the *Middle* sample and include individual and match group by relative grading period fixed effects. The mean effect estimates for gestation and postpartum years, obtained from the estimation of equation (2), are also displayed either at the bottom or top right of each panel. The corresponding raw trends can be found in Figure 6.

Figure 10: Main Results: Short-Run Effects on Educational Outcome—Become Mothers at Ages 17 vs. 18



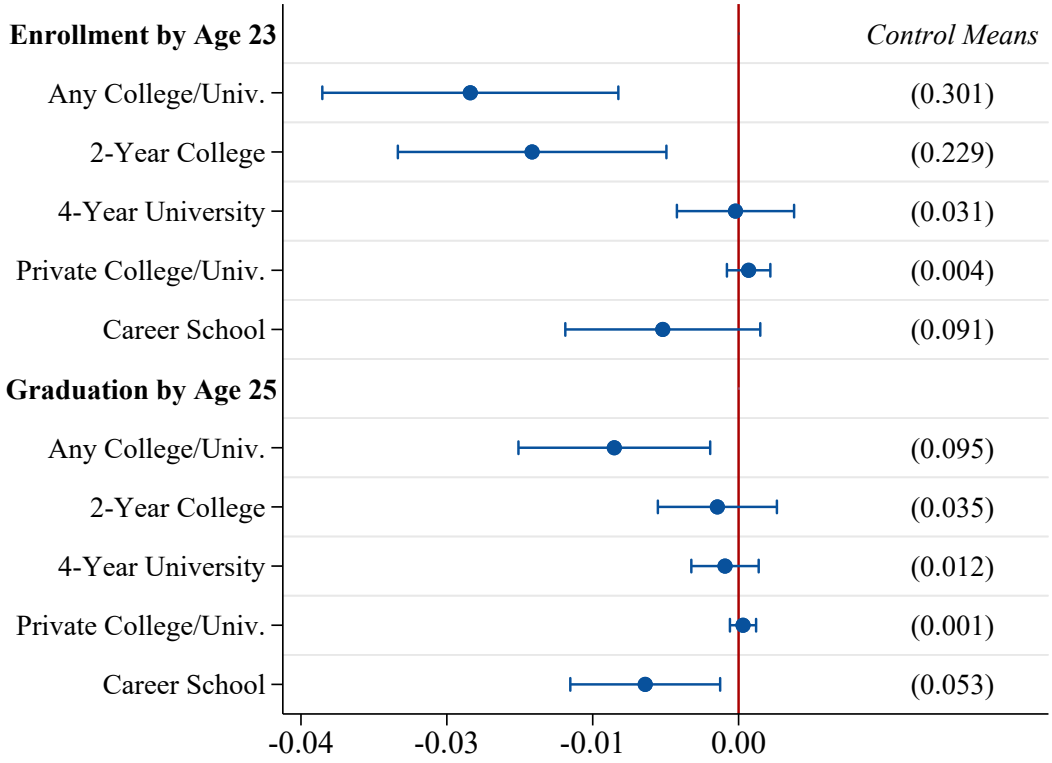
NOTES: The figure above plots coefficients on the interactions between the indicator for treated individuals and the indicators for each of the relative grading periods since pregnancy of the treated individuals from the estimation of equation (1). The dotted lines represent 95% confidence intervals calculated using standard errors clustered at the individual level. The last grading period in the year before the treated individuals become pregnant is the omitted category. The regressions use the *Old* sample and include individual and match group by relative grading period fixed effects. The mean effect estimates for gestation and postpartum years, obtained from the estimation of equation (2), are also displayed either at the bottom or top right of each panel. The corresponding raw trends can be found in Figure 7.

Figure 11: Main Results: Long-Run Effects on Completed Secondary Education by Age 20



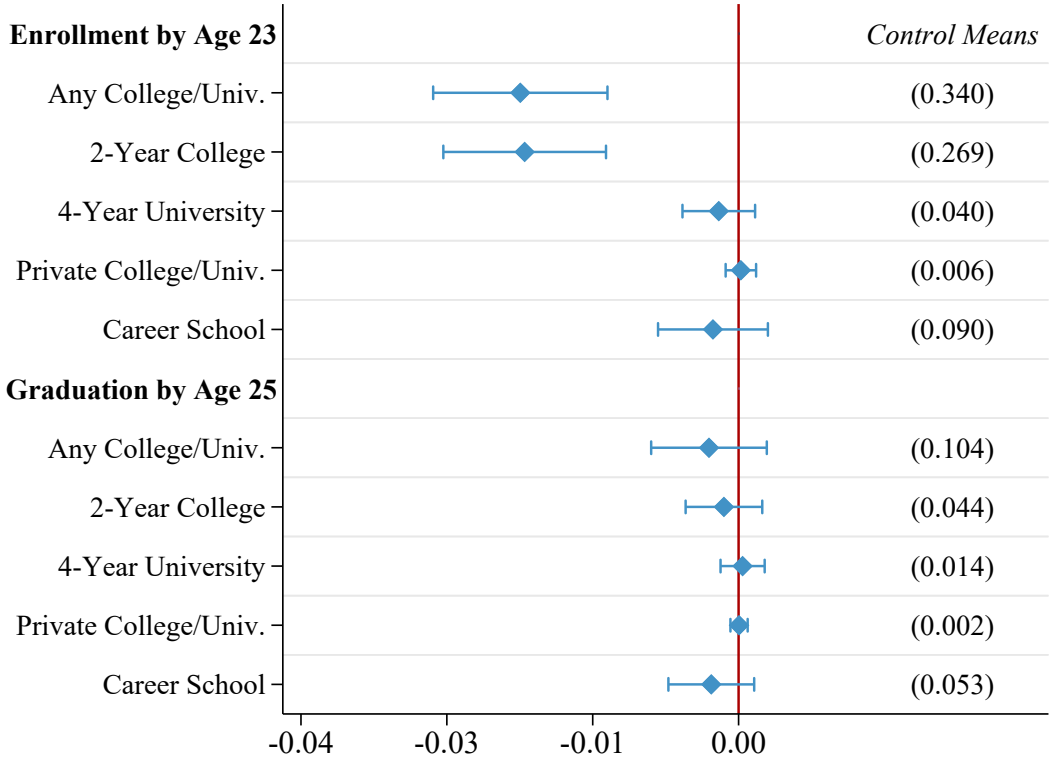
NOTES: The figure above plots the coefficients and 95% confidence intervals on the indicator denoting treated individuals from the estimation of equation (3) for all three samples. The first two rows present the results for the *Young* sample, the next two rows for the *Middle* sample, and the last two rows for the *Old* sample. The corresponding control group means for each outcome are reported on the right side of the figure. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. The estimated coefficients, standard errors, and p-values can be found in Table 2.

Figure 12: Main Results: Long-Run Effects on College Education—Become Mothers at Ages 15 vs. 16



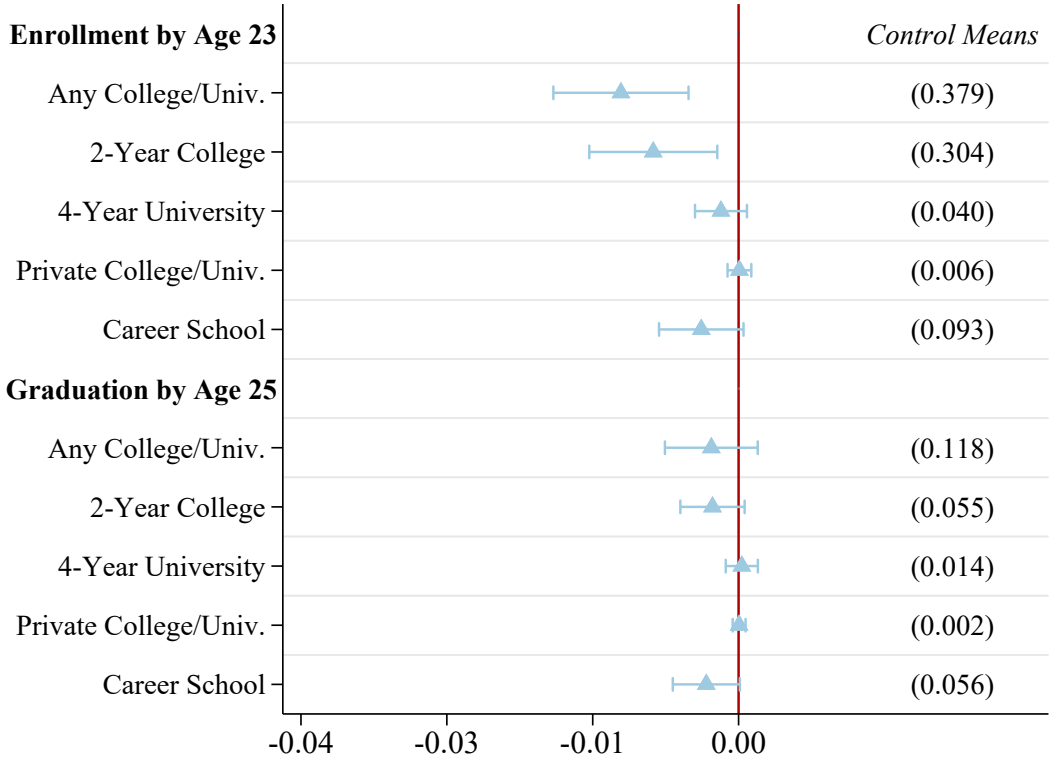
NOTES: The figure above plots the coefficients and 95% confidence intervals on the indicator denoting treated individuals from the estimation of equation (3) for the *Young* sample. In the figure, the upper half displays the results for college enrollment by age 23, and the lower half displays the results for college graduation by age 25. The corresponding control group means for each outcome are reported on the right side of the figure. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. The estimated coefficients, standard errors, and p-values can be found in Table 3.

Figure 13: Main Results: Long-Run Effects on College Education—Become Mothers at Ages 16 vs. 17



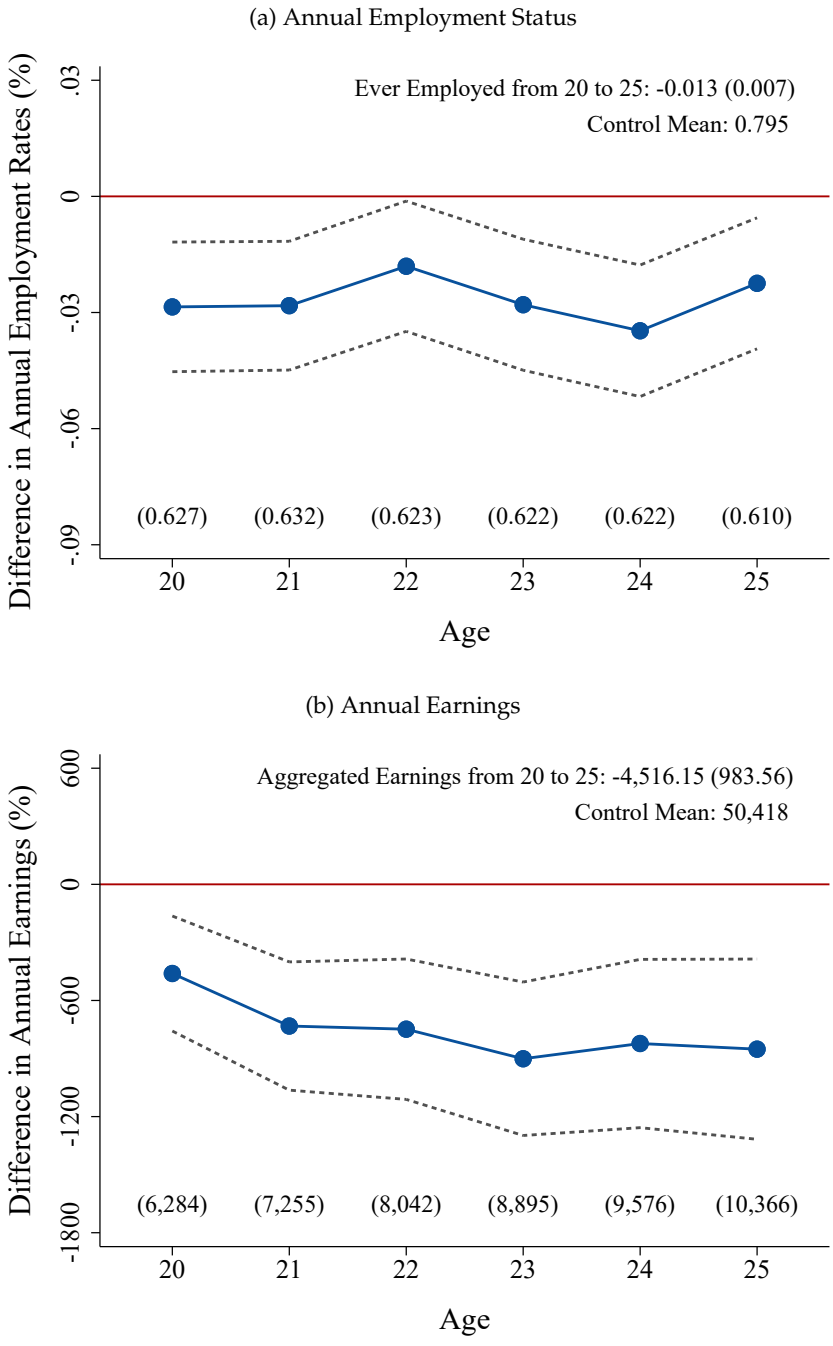
NOTES: The figure above plots the coefficients and 95% confidence intervals on the indicator denoting treated individuals from the estimation of equation (3) for the *Middle* sample. In the figure, the upper half displays the results for college enrollment by age 23, and the lower half displays the results for college graduation by age 25. The corresponding control group means for each outcome are reported on the right side of the figure. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. The estimated coefficients, standard errors, and p-values can be found in Table 4.

Figure 14: Main Results: Long-Run Effects on College Education—Become Mothers at Ages 17 vs. 18



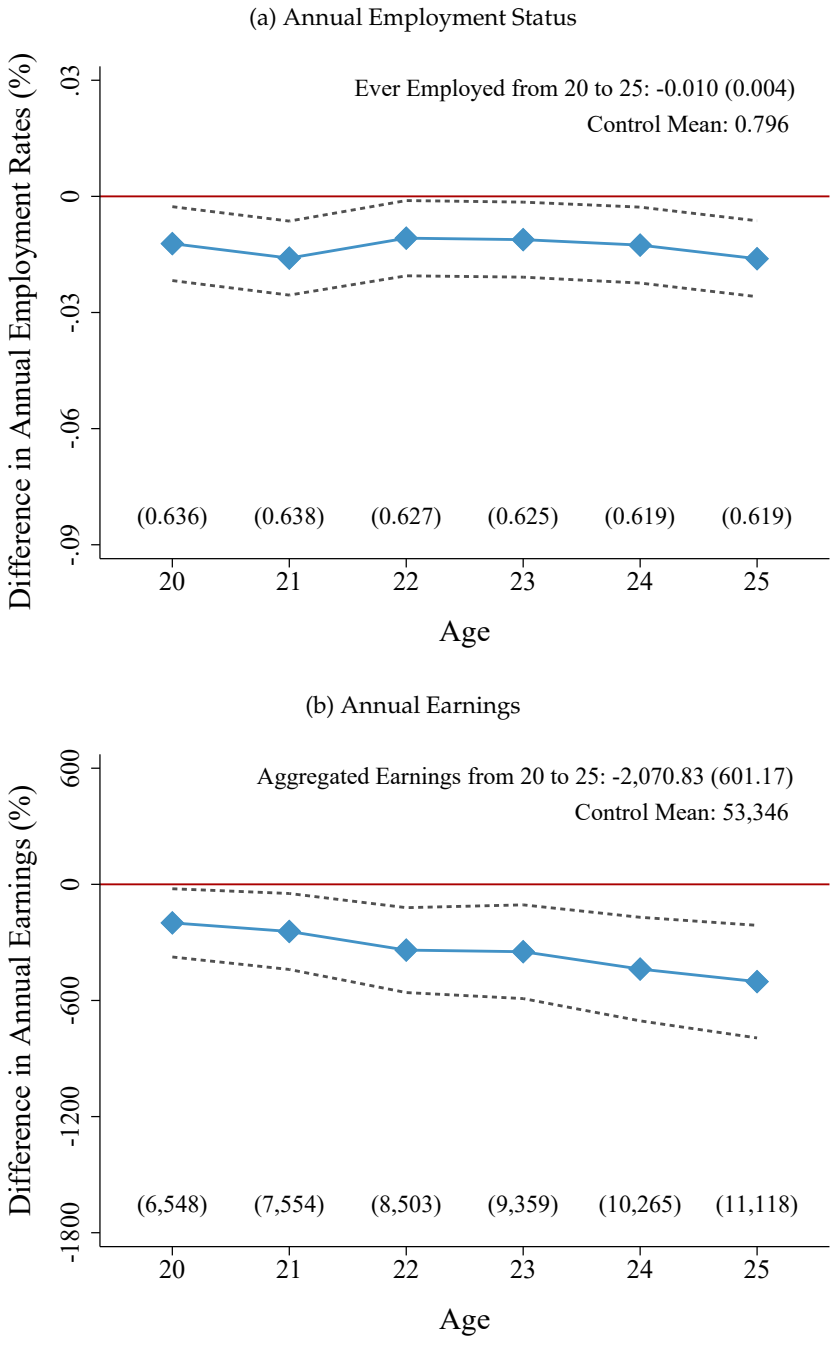
NOTES: The figure above plots the coefficients and 95% confidence intervals on the indicator denoting treated individuals from the estimation of equation (3) for the *Old* sample. In the figure, the upper half displays the results for college enrollment by age 23, and the lower half displays the results for college graduation by age 25. The corresponding control group means for each outcome are reported on the right side of the figure. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. The estimated coefficients, standard errors, and p-values can be found in Table 5.

Figure 15: Main Results: Long-Run Effects on Labor Market Outcomes—Become Mothers at Ages 15 vs. 16



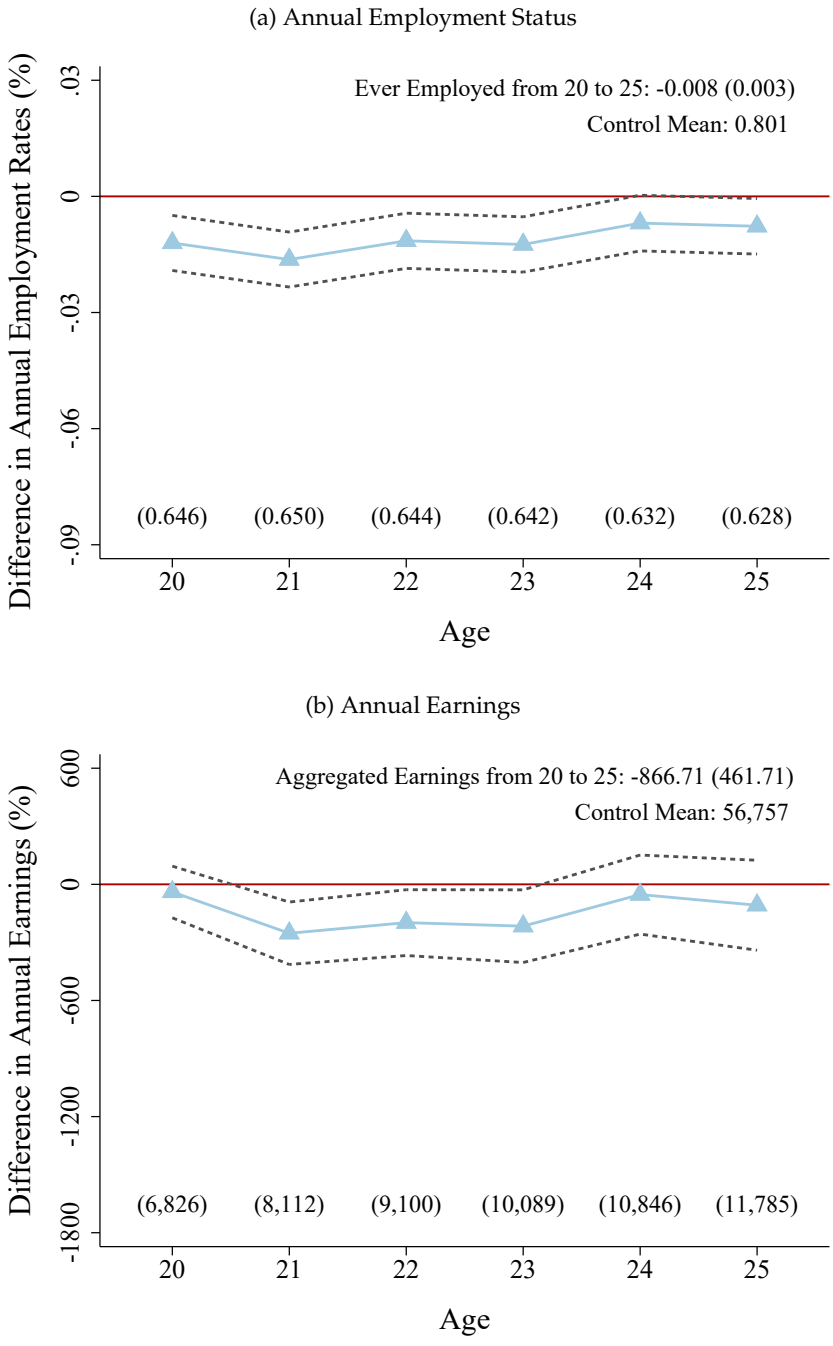
NOTES: The figure above plots the coefficients and 95% confidence intervals on the indicator denoting treated individuals from the estimation of equation (3) for the *Young* sample. Panel (a) displays the results for annual employment status, and panel (b) displays the results for annual earnings, both of which are separately measured at ages from 20 through 25. The corresponding control group means for each age are reported at the bottom of each panel. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. The estimated coefficients, standard errors, and p-values can be found in Table 6.

Figure 16: Main Results: Long-Run Effects on Labor Market Outcomes—Become Mothers at Ages 16 vs. 17



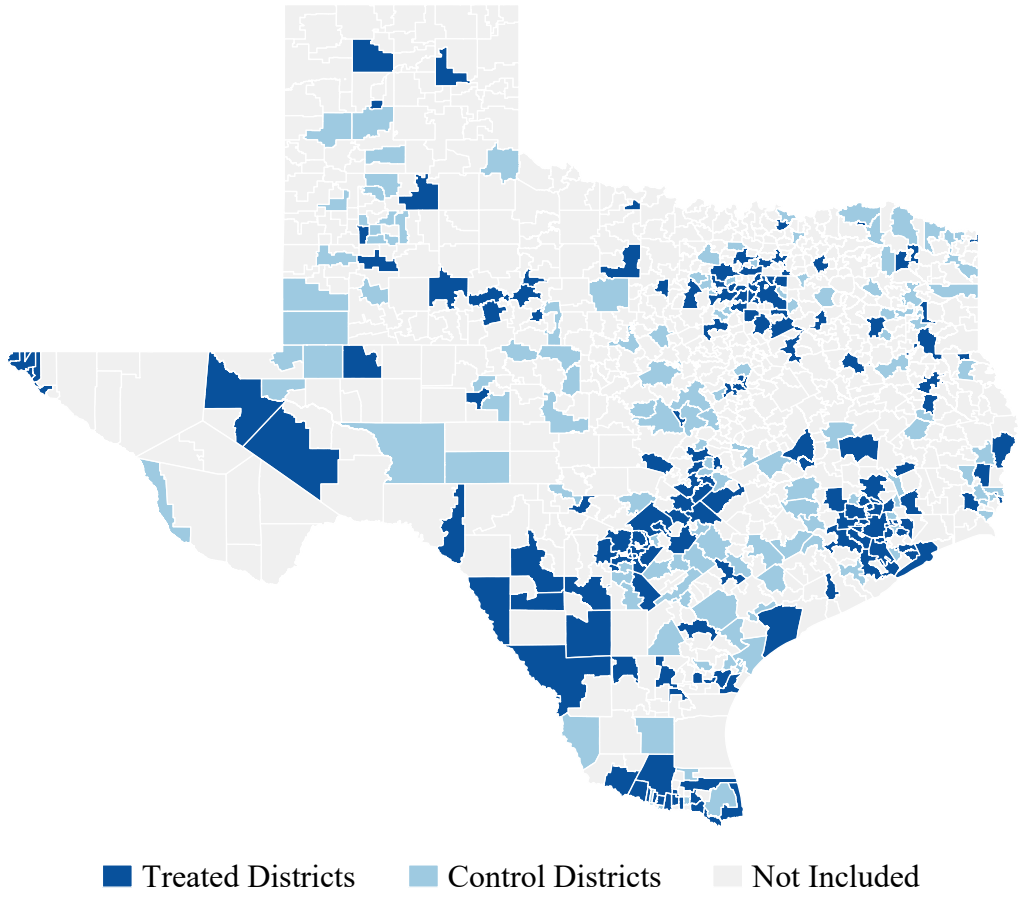
NOTES: The figure above plots the coefficients and 95% confidence intervals on the indicator denoting treated individuals from the estimation of equation (3) for the *Middle* sample. Panel (a) displays the results for annual employment status, and panel (b) displays the results for annual earnings, both of which are separately measured at ages from 20 through 25. The corresponding control group means for each age are reported at the bottom of each panel. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. The estimated coefficients, standard errors, and p-values can be found in Table 7.

Figure 17: Main Results: Long-Run Effects on Labor Market Outcomes—Become Mothers at Ages 17 vs. 18



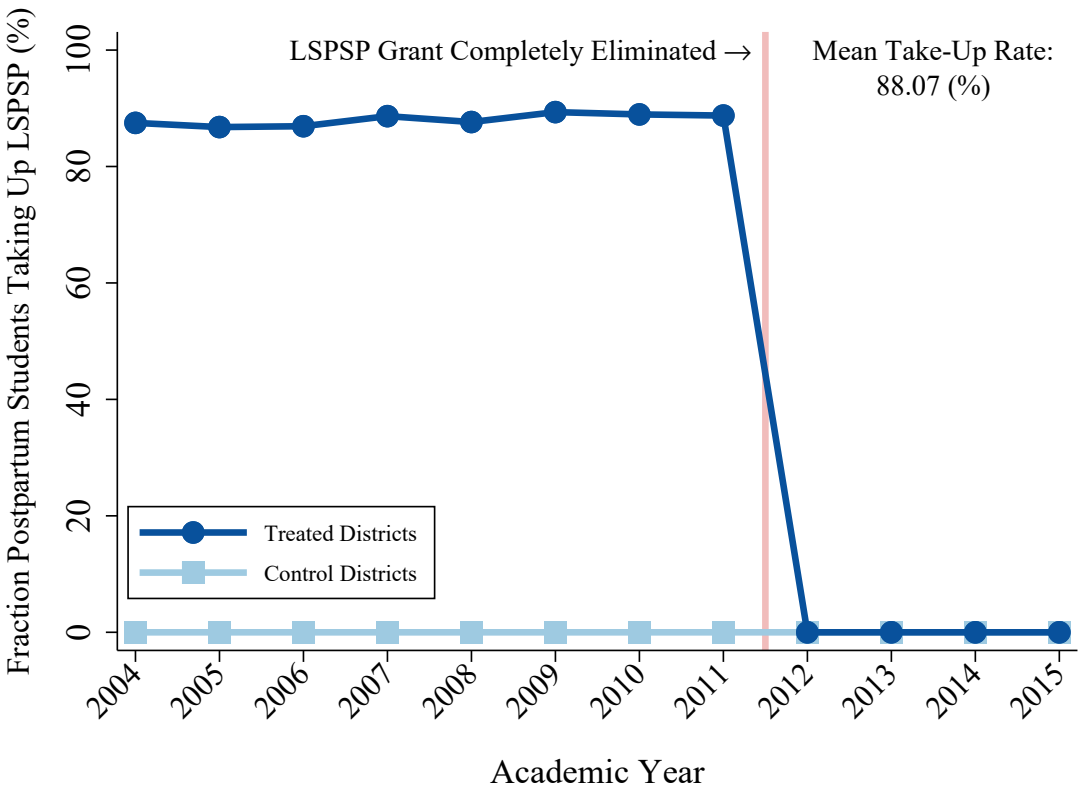
NOTES: The figure above plots the coefficients and 95% confidence intervals on the indicator denoting treated individuals from the estimation of equation (3) for the *Old* sample. Panel (a) displays the results for annual employment status, and panel (b) displays the results for annual earnings, both of which are separately measured at ages from 20 through 25. The corresponding control group means for each age are reported at the bottom of each panel. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. The estimated coefficients, standard errors, and p-values can be found in Table 8.

Figure 18: Identifying Variation: Districts with and without Budget Cuts for Teen Parent Support Program



NOTES: The figure above shows the identifying variation of the supplemental policy analysis in section 5. The darker blue districts represent the 169 treated districts that received the funding for the Life Skills Program for Student Parents (LSPSP) continuously until 2011 (i.e., the year before the budget cut), while the lighter blue districts represent the 141 control districts that have never received the funding for the LSPSP. The districts included in the analysis represent approximately 77.1% of the female student population in Texas. The districts in gray on the map are excluded from the analysis due to their ambiguous treatment status, as they received the funding only in some specific years.

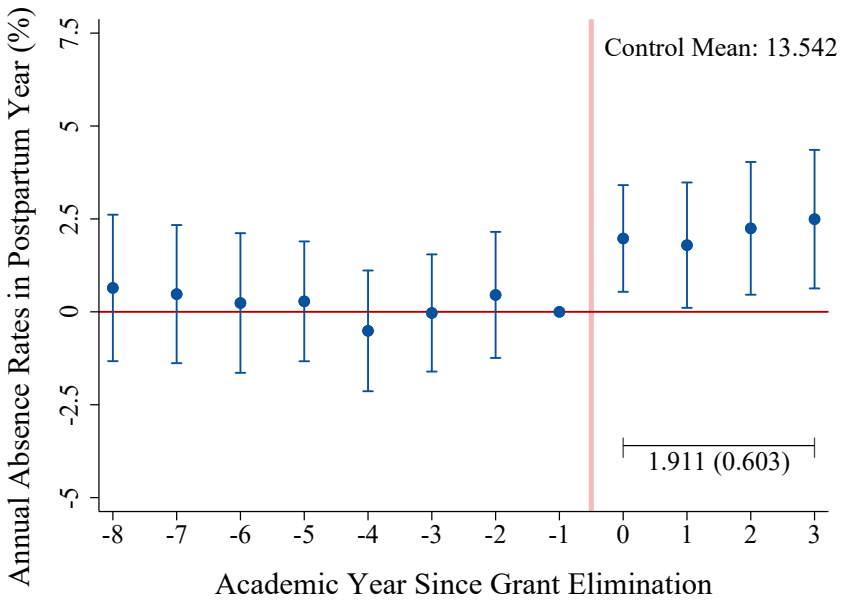
Figure 19: Take-Up Rate for the Life Skills Program for Student Parents (LSPSP)



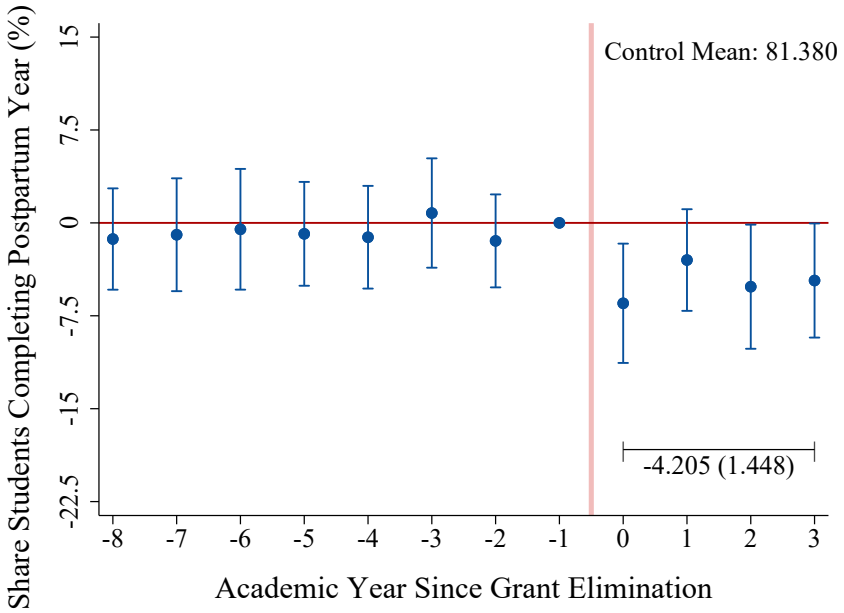
NOTES: The figure above displays the annual take-up rates for the Life Skills Program for Student Parents among female teen parents who are identified as those who give birth in each indicated academic year through Pregnancy Related Services (PRS), separately for treated and control districts. The vertical line in red is plotted to distinguish between years before and after the LSPSP budget elimination.

Figure 20: Effects of the LSPSP Budget Cut on School Attendance among Teenage Mothers

(a) Annual Absence Rates in Postpartum Academic Year



(b) Fraction of Pregnant Students Completing Postpartum Academic Year



NOTES: The figure above plots the coefficients and 95% confidence intervals on the interactions between the indicator for treated districts and the indicators for each of the relative academic years since the budget cut for the Life Skills Program for Student Parents from the estimation of equation (4). Panel (a) displays the results for annual absence rates measured in the postpartum academic year, and panel (b) displays the results for whether teen mothers complete the postpartum academic year (i.e., not dropping out). The year prior to the budget cut (i.e., the academic year of 2011) is the omitted category. The regressions include district fixed effects, year fixed effects, and individual- and district-level controls. The mean effect estimates for the post years are obtained by replacing the indicators for the relative academic years with the single post-period indicator and are displayed at the top of each panel (see Table 9). Standard errors are clustered at the district level.

Table 1: Main Results: Short-Run Effects on Educational Outcome

Dependent Variable:	Absence Rates			Enrolled in Public School			Combined Absence Measure			Chronic Absenteeism		
	Young (1)	*Middle* (2)	*Old* (3)	*Young* (4)	*Middle* (5)	*Old* (6)	*Young* (7)	*Middle* (8)	*Old* (9)	*Young* (10)	*Middle* (11)	*Old* (12)
$\gamma \times Treat_i \times \mathbf{1}(0 \leq h \leq 5)$	2.136	2.367	2.318	-0.360	-0.442	-0.319	2.373	2.676	2.528	6.740	7.554	7.396
	(0.114)	(0.071)	(0.050)	(0.169)	(0.095)	(0.074)	(0.189)	(0.112)	(0.084)	(0.408)	(0.246)	(0.182)
	[<0.001]	[<0.001]	[<0.001]	[0.033]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Control Mean During Gestation	9.048	8.828	8.095	98.207	98.348	98.336	10.716	10.373	9.660	34.304	32.409	29.855
Relative to Control Mean (%)	23.6%	26.8%	28.6%	-0.4%	-0.4%	-0.3%	22.1%	25.8%	26.2%	19.6%	23.3%	24.8%
$\rho \times Treat_i \times \mathbf{1}(6 \leq h \leq 11)$	5.731	6.370	6.209	-1.946	-3.486	-5.121	6.986	8.755	9.912	14.579	15.615	15.780
	(0.177)	(0.104)	(0.074)	(0.206)	(0.132)	(0.108)	(0.251)	(0.155)	(0.121)	(0.460)	(0.273)	(0.208)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Control Mean During Postpartum	13.024	12.395	11.282	97.258	97.223	97.295	15.489	14.921	13.781	45.415	43.663	40.681
Relative to Control Mean (%)	44.0%	51.4%	55.0%	-2.0%	-3.6%	-5.3%	45.1%	58.7%	71.9%	32.1%	35.8%	38.8%
Treated Individuals (unique)	8,089	24,547	43,167	8,089	24,547	43,167	8,089	24,547	43,167	8,089	24,547	43,167
Treated Individuals (weighted)	8,092	24,644	44,623	8,092	24,644	44,623	8,092	24,644	44,623	8,092	24,644	44,623
Control Individuals (unique)	9,892	26,995	46,285	9,892	26,995	46,285	9,892	26,995	46,285	9,892	26,995	46,285
Control Individuals (weighted)	8,092	24,644	44,623	8,092	24,644	44,623	8,092	24,644	44,623	8,092	24,644	44,623
Student-Grading Period Obs.	422,682	1,210,594	2,099,202	431,544	1,237,008	2,146,848	431,544	1,237,008	2,146,848	422,682	1,210,594	2,099,202
R-squared	0.639	0.599	0.576	0.506	0.468	0.462	0.625	0.595	0.579	0.588	0.549	0.521

NOTES: The table above reports estimates of the coefficients on the interactions between the indicator for treated individuals and the indicators for gestation and postpartum years for the treated individuals from the estimation of equation (2) for all three samples. For each short-run outcome, the first set of columns—(1), (4), (7), and (10)—provide the results for the *Young* sample, the next set of columns—(2), (5), (8), and (11)—for the *Middle* sample, and the last set of columns—(3), (6), (9), and (12)—for the *Old* sample. The regressions include individual and match group by relative grading period fixed effects. Robust standard errors clustered at the individual level are reported in parentheses, and p-values are reported in brackets.

Table 2: Main Results: Long-Run Effects on Completed Secondary Education by Age 20

Dependent Variable:	Maximum Grade Achieved			High School Completion		
	Young (1)	*Middle* (2)	*Old* (3)	*Young* (4)	*Middle* (5)	*Old* (6)
$\delta \times Treat_i$	-0.264	-0.422	-0.437	-0.040	-0.098	-0.112
	(0.022)	(0.011)	(0.006)	(0.007)	(0.004)	(0.003)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Control Mean	10.185	10.780	11.393	0.392	0.540	0.704
Relative to Control Mean (%)	-2.6%	-3.9%	-3.8%	-10.3%	-18.2%	-15.9%
Treated Individuals (unique)	7,606	23,675	42,245	7,606	23,675	42,245
Treated Individuals (weighted)	7,609	23,769	43,675	7,609	23,769	43,675
Control Individuals (unique)	9,434	26,112	45,413	9,434	26,112	45,413
Control Individuals (weighted)	7,611	23,770	43,674	7,611	23,770	43,674
Student-Grading Period Obs.	17,040	49,787	87,658	17,040	49,787	87,658
R-squared	0.546	0.513	0.506	0.497	0.462	0.434

NOTES: The table above reports estimates of the coefficients on the indicator denoting treated individuals from the estimation of equation (3) for all three samples. For each secondary educational outcome, the first set of columns—(1) and (4)—provide the results for the *Young* sample, the next set of columns—(2) and (5)—for the *Middle* sample, and the last set of columns—(3) and (6)—for the *Old* sample. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. Robust standard errors clustered at the individual level are reported in parentheses, and p-values are reported in brackets.

Table 3: Main Results: Long-Run Effects on College Education—Become Mothers at Ages 15 vs. 16

Panel (a): College/University Enrollment by Age 23

Dependent Variable:	Any College (1)	2-Year College (2)	4-Year Univ. (3)	Private Col/Univ. (4)	Career School (5)
$\delta \times Treat_i$	-0.028	-0.021	0.000	0.001	-0.008
	(0.008)	(0.007)	(0.003)	(0.001)	(0.005)
	[<0.001]	[0.003]	[0.917]	[0.365]	[0.128]
Control Mean	0.301	0.229	0.031	0.004	0.091
Relative to Control Mean (%)	-9.2%	-9.3%	-1.0%	26.1%	-8.6%
Treated Individuals (unique)	5,904	5,904	5,904	5,904	5,904
Treated Individuals (weighted)	5,907	5,907	5,907	5,907	5,907
Control Individuals (unique)	7,533	7,533	7,533	7,533	7,533
Control Individuals (weighted)	5,907	5,907	5,907	5,907	5,907
Student-Grading Period Obs.	13,437	13,437	13,437	13,437	13,437
R-squared	0.442	0.450	0.389	0.406	0.383

Panel (b): College/University Graduation by Age 25

Dependent Variable:	Any College (1)	2-Year College (2)	4-Year Univ. (3)	Private Col/Univ. (4)	Career School (5)
$\delta \times Treat_i$	-0.013	-0.002	-0.001	0.000	-0.010
	(0.005)	(0.003)	(0.002)	(0.001)	(0.004)
	[0.011]	[0.485]	[0.429]	[0.508]	[0.015]
Control Mean	0.095	0.035	0.012	0.001	0.053
Relative to Control Mean (%)	-13.5%	-6.3%	-12.1%	30.8%	-18.0%
Treated Individuals (unique)	5,904	5,904	5,904	5,904	5,904
Treated Individuals (weighted)	5,907	5,907	5,907	5,907	5,907
Control Individuals (unique)	7,533	7,533	7,533	7,533	7,533
Control Individuals (weighted)	5,907	5,907	5,907	5,907	5,907
Student-Grading Period Obs.	13,437	13,437	13,437	13,437	13,437
R-squared	0.388	0.397	0.401	0.407	0.362

NOTES: The table above reports estimates of the coefficients on the indicator denoting treated individuals from the estimation of equation (3) for the *Young* sample. Panel (a) displays the results for college enrollment by age 23, and panel (b) displays the results for college graduation by age 25. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. Robust standard errors clustered at the individual level are reported in parentheses, and p-values are reported in brackets.

Table 4: Main Results: Long-Run Effects on College Education—Become Mothers at Ages 16 vs. 17

Panel (a): College/University Enrollment by Age 23

Dependent Variable:	Any College (1)	2-Year College (2)	4-Year Univ. (3)	Private Col/Univ. (4)	Career School (5)
$\delta \times Treat_i$	-0.022	-0.022	-0.002	0.000	-0.003
	(0.005)	(0.004)	(0.002)	(0.001)	(0.003)
	[<0.001]	[<0.001]	[0.285]	[0.770]	[0.361]
Control Mean	0.340	0.269	0.040	0.006	0.090
Relative to Control Mean (%)	-6.6%	-8.2%	-5.1%	3.7%	-2.9%
Treated Individuals (unique)	19,255	19,255	19,255	19,255	19,255
Treated Individuals (weighted)	19,294	19,294	19,294	19,294	19,294
Control Individuals (unique)	21,548	21,548	21,548	21,548	21,548
Control Individuals (weighted)	19,296	19,296	19,296	19,296	19,296
Student-Grading Period Obs.	40,803	40,803	40,803	40,803	40,803
R-squared	0.390	0.385	0.336	0.329	0.324

Panel (b): College/University Graduation by Age 25

Dependent Variable:	Any College (1)	2-Year College (2)	4-Year Univ. (3)	Private Col/Univ. (4)	Career School (5)
$\delta \times Treat_i$	-0.003	-0.002	0.000	0.000	-0.003
	(0.003)	(0.002)	(0.001)	(0.000)	(0.002)
	[0.316]	[0.453]	[0.722]	[0.918]	[0.212]
Control Mean	0.104	0.044	0.014	0.002	0.053
Relative to Control Mean (%)	-2.9%	-3.4%	2.9%	2.2%	-5.3%
Treated Individuals (unique)	19,255	19,255	19,255	19,255	19,255
Treated Individuals (weighted)	19,294	19,294	19,294	19,294	19,294
Control Individuals (unique)	21,548	21,548	21,548	21,548	21,548
Control Individuals (weighted)	19,296	19,296	19,296	19,296	19,296
Student-Grading Period Obs.	40,803	40,803	40,803	40,803	40,803
R-squared	0.328	0.329	0.303	0.295	0.308

NOTES: The table above reports estimates of the coefficients on the indicator denoting treated individuals from the estimation of equation (3) for the *Middle* sample. Panel (a) displays the results for college enrollment by age 23, and panel (b) displays the results for college graduation by age 25. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. Robust standard errors clustered at the individual level are reported in parentheses, and p-values are reported in brackets.

Table 5: Main Results: Long-Run Effects on College Education—Become Mothers at Ages 17 vs. 18

Panel (a): College/University Enrollment by Age 23

Dependent Variable:	Any College (1)	2-Year College (2)	4-Year Univ. (3)	Private Col/Univ. (4)	Career School (5)
$\delta \times Treat_i$	-0.012	-0.009	-0.002	0.000	-0.004
	(0.004)	(0.003)	(0.001)	(0.001)	(0.002)
	[<0.001]	[0.009]	[0.184]	[0.768]	[0.083]
Control Mean	0.379	0.304	0.040	0.006	0.093
Relative to Control Mean (%)	-3.2%	-2.9%	-4.6%	1.3%	-4.1%
Treated Individuals (unique)	34,926	34,926	34,926	34,926	34,926
Treated Individuals (weighted)	36,194	36,194	36,194	36,194	36,194
Control Individuals (unique)	37,611	37,611	37,611	37,611	37,611
Control Individuals (weighted)	36,192	36,192	36,192	36,192	36,192
Student-Grading Period Obs.	72,537	72,537	72,537	72,537	72,537
R-squared	0.359	0.354	0.300	0.297	0.297

Panel (b): College/University Graduation by Age 25

Dependent Variable:	Any College (1)	2-Year College (2)	4-Year Univ. (3)	Private Col/Univ. (4)	Career School (5)
$\delta \times Treat_i$	-0.003	-0.003	0.000	0.000	-0.003
	(0.002)	(0.002)	(0.001)	(0.000)	(0.002)
	[0.250]	[0.112]	[0.695]	[0.703]	[0.059]
Control Mean	0.118	0.055	0.014	0.002	0.056
Relative to Control Mean (%)	-2.4%	-4.9%	2.4%	3.0%	-5.9%
Treated Individuals (unique)	34,926	34,926	34,926	34,926	34,926
Treated Individuals (weighted)	36,194	36,194	36,194	36,194	36,194
Control Individuals (unique)	37,611	37,611	37,611	37,611	37,611
Control Individuals (weighted)	36,192	36,192	36,192	36,192	36,192
Student-Grading Period Obs.	72,537	72,537	72,537	72,537	72,537
R-squared	0.292	0.297	0.258	0.249	0.270

NOTES: The table above reports estimates of the coefficients on the indicator denoting treated individuals from the estimation of equation (3) for the *Old* sample. Panel (a) displays the results for college enrollment by age 23, and panel (b) displays the results for college graduation by age 25. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. Robust standard errors clustered at the individual level are reported in parentheses, and p-values are reported in brackets.

Table 6: Main Results: Long-Run Effects on Labor Market Outcomes—Become Mothers at Ages 15 vs. 16

Panel (a): Annual Employment Status from Ages 20 to 25

Dependent Variable: *Being Employed at Given Age*	Age 20 (1)	Age 21 (2)	Age 22 (3)	Age 23 (4)	Age 24 (5)	Age 25 (6)	Ever 20–25 (7)
$\delta \times Treat_i$	-0.029	-0.028	-0.018	-0.028	-0.035	-0.022	-0.013
	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.007)
	[<0.001]	[<0.001]	[0.036]	[0.001]	[<0.001]	[0.009]	[0.073]
Control Mean	0.627	0.632	0.623	0.622	0.622	0.610	0.795
Relative to Control Mean (%)	-4.6%	-4.5%	-2.9%	-4.5%	-5.6%	-3.7%	-1.6%
Treated Individuals (unique)	5,904	5,904	5,904	5,904	5,904	5,904	5,904
Treated Individuals (weighted)	5,907	5,907	5,907	5,907	5,907	5,907	5,907
Control Individuals (unique)	7,533	7,533	7,533	7,533	7,533	7,533	7,533
Control Individuals (weighted)	5,907	5,907	5,907	5,907	5,907	5,907	5,907
Student-Grading Period Obs.	13,437	13,437	13,437	13,437	13,437	13,437	13,437
R-squared	0.421	0.422	0.418	0.412	0.414	0.421	0.425

Panel (b): Annual Earnings from Ages 20 to 25

Dependent Variable: *Annual Earnings at Given Age*	Age 20 (1)	Age 21 (2)	Age 22 (3)	Age 23 (4)	Age 24 (5)	Age 25 (6)	Aggregated 20–25 (7)
$\delta \times Treat_i$	-460.673	-732.020	-748.400	-901.147	-822.388	-851.518	-4,516.146
	(151.550)	(169.017)	(185.049)	(202.196)	(221.749)	(237.578)	(983.563)
	[0.002]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
Control Mean	6,284	7,255	8,042	8,895	9,576	10,366	50,418
Relative to Control Mean (%)	-7.3%	-10.1%	-9.3%	-10.1%	-8.6%	-8.2%	-9.0%
Treated Individuals (unique)	5,904	5,904	5,904	5,904	5,904	5,904	5,904
Treated Individuals (weighted)	5,907	5,907	5,907	5,907	5,907	5,907	5,907
Control Individuals (unique)	7,533	7,533	7,533	7,533	7,533	7,533	7,533
Control Individuals (weighted)	5,907	5,907	5,907	5,907	5,907	5,907	5,907
Student-Grading Period Obs.	13,437	13,437	13,437	13,437	13,437	13,437	13,437
R-squared	0.408	0.415	0.427	0.429	0.432	0.434	0.437

NOTES: The table above reports estimates of the coefficients on the indicator denoting treated individuals from the estimation of equation (3) for the *Young* sample. Panel (a) displays the results for annual employment status, and panel (b) displays the results for annual earnings, both of which are separately measured at ages from 20 through 25. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. Robust standard errors clustered at the individual level are reported in parentheses, and p-values are reported in brackets.

Table 7: Main Results: Long-Run Effects on Labor Market Outcomes—Become Mothers at Ages 16 vs. 17

Panel (a): Annual Employment Status from Ages 20 to 25

Dependent Variable: *Being Employed at Given Age*	Age 20 (1)	Age 21 (2)	Age 22 (3)	Age 23 (4)	Age 24 (5)	Age 25 (6)	Ever 20–25 (7)
$\delta \times Treat_i$	-0.012	-0.016	-0.011	-0.011	-0.013	-0.016	-0.010
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
	[0.012]	[0.001]	[0.029]	[0.024]	[0.012]	[0.001]	[0.017]
Control Mean	0.636	0.638	0.627	0.625	0.619	0.619	0.796
Relative to Control Mean (%)	-1.9%	-2.5%	-1.7%	-1.8%	-2.0%	-2.6%	-1.2%
Treated Individuals (unique)	19,255	19,255	19,255	19,255	19,255	19,255	19,255
Treated Individuals (weighted)	19,294	19,294	19,294	19,294	19,294	19,294	19,294
Control Individuals (unique)	21,548	21,548	21,548	21,548	21,548	21,548	21,548
Control Individuals (weighted)	19,296	19,296	19,296	19,296	19,296	19,296	19,296
Student-Grading Period Obs.	40,803	40,803	40,803	40,803	40,803	40,803	40,803
R-squared	0.353	0.354	0.346	0.349	0.340	0.340	0.354

Panel (b): Annual Earnings from Ages 20 to 25

Dependent Variable: *Annual Earnings at Given Age*	Age 20 (1)	Age 21 (2)	Age 22 (3)	Age 23 (4)	Age 24 (5)	Age 25 (6)	Aggregated 20–25 (7)
$\delta \times Treat_i$	-199.072	-243.471	-339.833	-348.095	-437.714	-502.641	-2,070.825
	(89.878)	(100.109)	(111.925)	(123.536)	(136.506)	(148.454)	(601.172)
	[0.027]	[0.015]	[0.002]	[0.005]	[0.001]	[<0.001]	[<0.001]
Control Mean	6,548	7,554	8,503	9,359	10,265	11,118	53,346
Relative to Control Mean (%)	-3.0%	-3.2%	-4.0%	-3.7%	-4.3%	-4.5%	-3.9%
Treated Individuals (unique)	19,255	19,255	19,255	19,255	19,255	19,255	19,255
Treated Individuals (weighted)	19,294	19,294	19,294	19,294	19,294	19,294	19,294
Control Individuals (unique)	21,548	21,548	21,548	21,548	21,548	21,548	21,548
Control Individuals (weighted)	19,296	19,296	19,296	19,296	19,296	19,296	19,296
Student-Grading Period Obs.	40,803	40,803	40,803	40,803	40,803	40,803	40,803
R-squared	0.336	0.344	0.341	0.345	0.345	0.339	0.352

NOTES: The table above reports estimates of the coefficients on the indicator denoting treated individuals from the estimation of equation (3) for the *Middle* sample. Panel (a) displays the results for annual employment status, and panel (b) displays the results for annual earnings, both of which are separately measured at ages from 20 through 25. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. Robust standard errors clustered at the individual level are reported in parentheses, and p-values are reported in brackets.

Table 8: Main Results: Long-Run Effects on Labor Market Outcomes—Become Mothers at Ages 17 vs. 18

Panel (a): Annual Employment Status from Ages 20 to 25

Dependent Variable: *Being Employed at Given Age*	Age 20 (1)	Age 21 (2)	Age 22 (3)	Age 23 (4)	Age 24 (5)	Age 25 (6)	Ever 20–25 (7)
$\delta \times Treat_i$	-0.012	-0.016	-0.011	-0.012	-0.007	-0.008	-0.008
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
	[<0.001]	[<0.001]	[0.002]	[<0.001]	[0.060]	[0.035]	[0.011]
Control Mean	0.646	0.650	0.644	0.642	0.632	0.628	0.801
Relative to Control Mean (%)	-1.9%	-2.5%	-1.8%	-1.9%	-1.1%	-1.2%	-0.9%
Treated Individuals (unique)	34,926	34,926	34,926	34,926	34,926	34,926	34,926
Treated Individuals (weighted)	36,194	36,194	36,194	36,194	36,194	36,194	36,194
Control Individuals (unique)	37,611	37,611	37,611	37,611	37,611	37,611	37,611
Control Individuals (weighted)	36,192	36,192	36,192	36,192	36,192	36,192	36,192
Student-Grading Period Obs.	72,537	72,537	72,537	72,537	72,537	72,537	72,537
R-squared	0.319	0.319	0.319	0.320	0.316	0.321	0.334

Panel (b): Annual Earnings from Ages 20 to 25

Dependent Variable: *Annual Earnings at Given Age*	Age 20 (1)	Age 21 (2)	Age 22 (3)	Age 23 (4)	Age 24 (5)	Age 25 (6)	Aggregated 20–25 (7)
$\delta \times Treat_i$	-39.402	-252.682	-197.796	-216.115	-52.801	-107.909	-866.705
	(68.134)	(82.225)	(86.723)	(95.755)	(104.143)	(118.600)	(461.710)
	[0.563]	[0.002]	[0.023]	[0.024]	[0.612]	[0.363]	[0.061]
Control Mean	6,826	8,112	9,100	10,089	10,846	11,785	56,757
Relative to Control Mean (%)	-0.6%	-3.1%	-2.2%	-2.1%	-0.5%	-0.9%	-1.5%
Treated Individuals (unique)	34,926	34,926	34,926	34,926	34,926	34,926	34,926
Treated Individuals (weighted)	36,194	36,194	36,194	36,194	36,194	36,194	36,194
Control Individuals (unique)	37,611	37,611	37,611	37,611	37,611	37,611	37,611
Control Individuals (weighted)	36,192	36,192	36,192	36,192	36,192	36,192	36,192
Student-Grading Period Obs.	72,537	72,537	72,537	72,537	72,537	72,537	72,537
R-squared	0.308	0.297	0.313	0.316	0.312	0.331	0.324

NOTES: TThe table above reports estimates of the coefficients on the indicator denoting treated individuals from the estimation of equation (3) for the *Old* sample. Panel (a) displays the results for annual employment status, and panel (b) displays the results for annual earnings, both of which are separately measured at ages from 20 through 25. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. Robust standard errors clustered at the individual level are reported in parentheses, and p-values are reported in brackets.

Table 9: Effects of the LSPSP Budget Cut on School Attendance among Teenage Mothers

Dependent Variable:	Annual Absence Rates			Complete Academic Year		
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_d \times \mathbf{1}(t - 2012 \geq 0)$	1.911	1.886	1.747	-4.205	-4.230	-4.020
	(0.603)	(0.705)	(0.615)	(1.448)	(1.501)	(1.430)
	[0.002]	[0.008]	[0.005]	[0.004]	[0.005]	[0.005]
Main Effects						
Year Fixed Effects	x	x	x	x	x	x
District Fixed Effects	x	x	x	x	x	x
Control Variables	x		x	x		x
Extended Sample			x			
Alternative Measure						x
Student Obs.	60,776	60,781	73,775	73,775	73,778	73,775
R-squared	0.161	0.068	0.177	0.092	0.032	0.089

NOTES: The table above reports estimates of the coefficients on the interactions between the indicator for treated districts and the indicator for the post-budget cut years from the estimation of a modified version of equation (4):

$$Y_{idt} = \delta \times Treat_d \times \mathbf{1}(t - 2012 \geq 0) + \theta_d + \pi_t + \mathbf{\Omega}' \boldsymbol{X}_i + \boldsymbol{z}_{dt} + \eta_{idt}.$$

The dependent variable for the first three columns is annual absence rates measured in the postpartum academic year, while the dependent variable for the last three columns is an indicator of whether teen mothers complete the postpartum academic year (i.e., not dropping out). Columns 1 and 4 replicate the baseline estimates presented in each panel of Figure 20. Columns 2 and 5 presents the estimates of the most parsimonious model controlling only for year and district fixed effects. Column 3 repeats the baseline specification but use an extended sample including all pregnant students who enrolled at least one grading period during the postpartum academic year. Column 6 repeats the baseline specification but uses an alternative measure of completing the postpartum academic year—whether being observed in the last grading period of that year—as the dependent variable. Robust standard errors clustered at the district level are reported in parentheses, and p-values are reported in brackets.

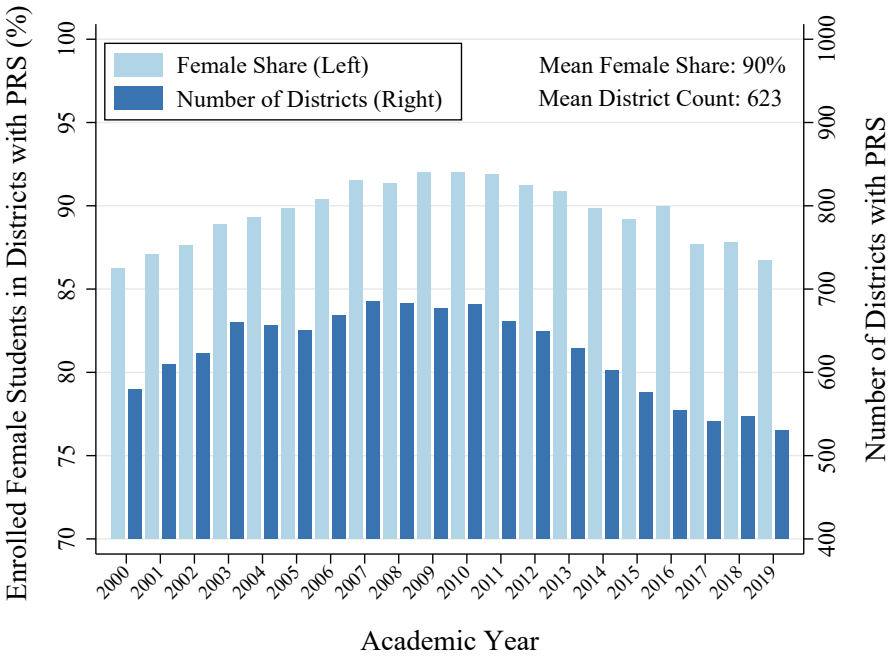
For Online Publication

Every Year Counts: The Long-Run Consequences of Pregnancy Timing among Teenagers

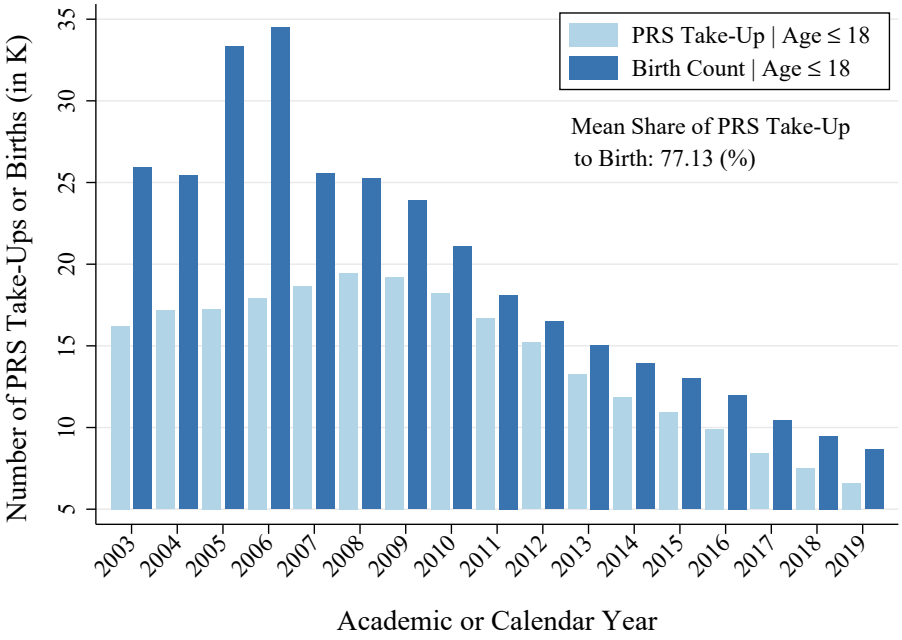
Jinyeong Son (2023)

Figure A1: Background on Pregnancy Related Services (PRS)

(a) Districts with Pregnancy Related Services (PRS)

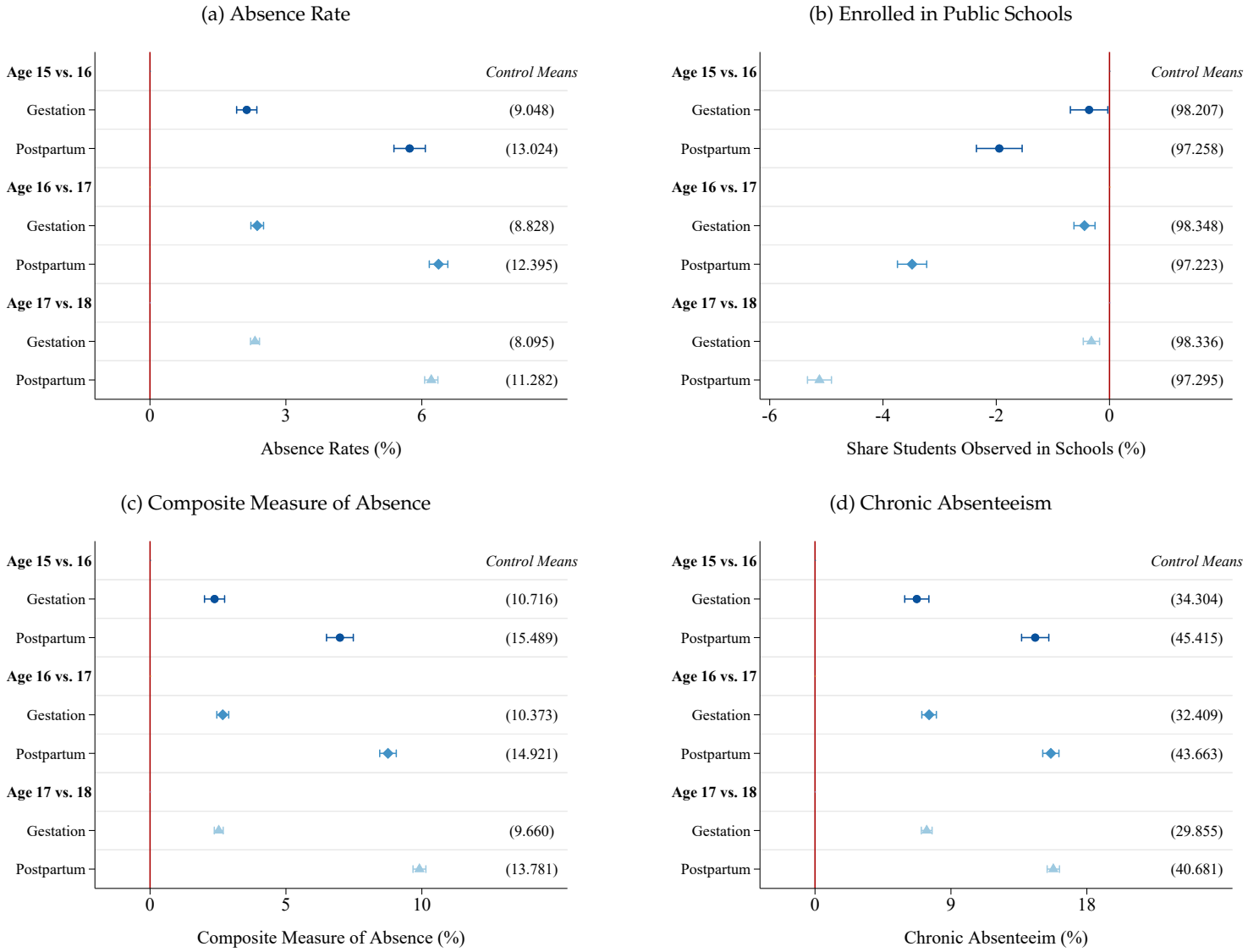


(b) Comparison Teen Births and PRS Take-Up Individuals in Texas



NOTES: The figure above provides background information on Pregnancy Related Services (PRS) in Texas. Panel (a) displays the number of school districts with PRS and the share of female students attending schools in those districts. Panel (b) compares the number of PRS take-up individuals to that of teen births in Texas, both conditional on ages 15 to 18 and attending K–12 schools.

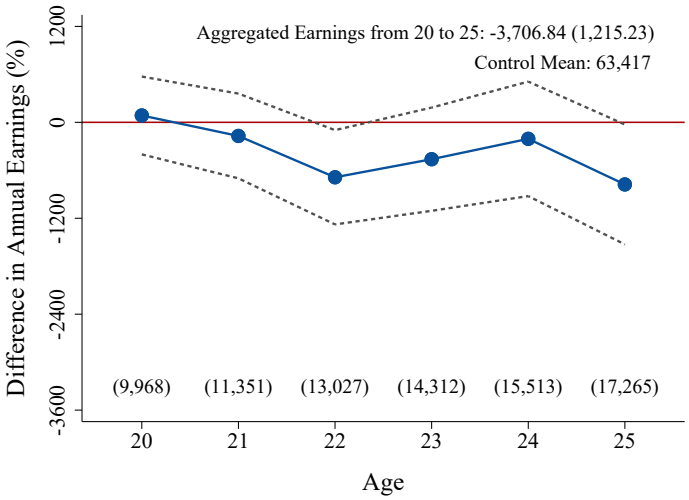
Figure A2: Comparison of Mean Effects for Short-Run Outcomes Across Three Samples



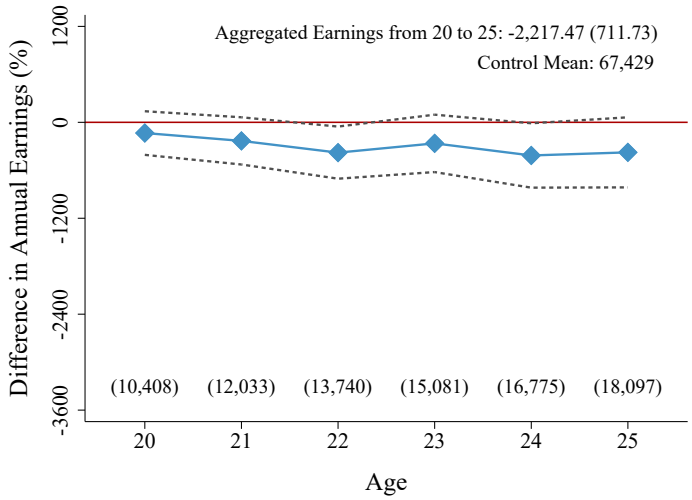
NOTES: The figure above reproduces the mean effect estimates obtained from the estimation of equation (2) for four short-run outcomes for easier comparison across three samples. For each panel, the first two rows present the estimates for the Young sample, the next two rows for the Middle sample, and the last two rows for the Old sample. The corresponding control group means for each outcome by sample are reported on the right side of the figure. The estimated coefficients, standard errors, and p-values can be found in Table 1.

Figure A3: Main Results: Long-Run Effects on Labor Market Outcomes—Intensive Margin

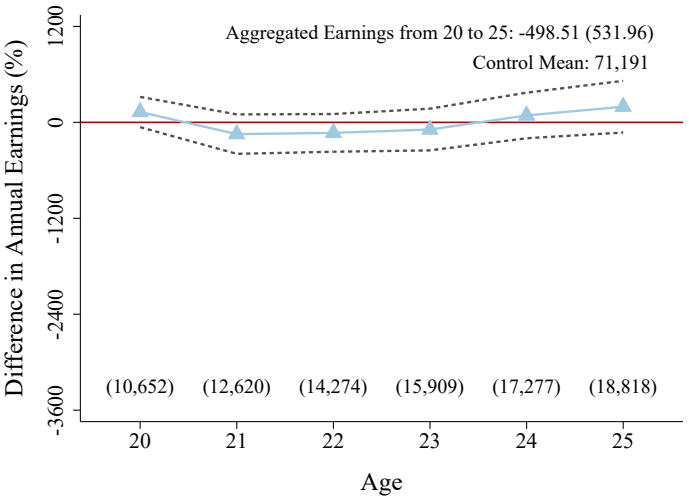
(a) *Young* Sample: Become Mothers at Ages 15 vs. 16



(b) *Middle* Sample: Become Mothers at Ages 16 vs. 17

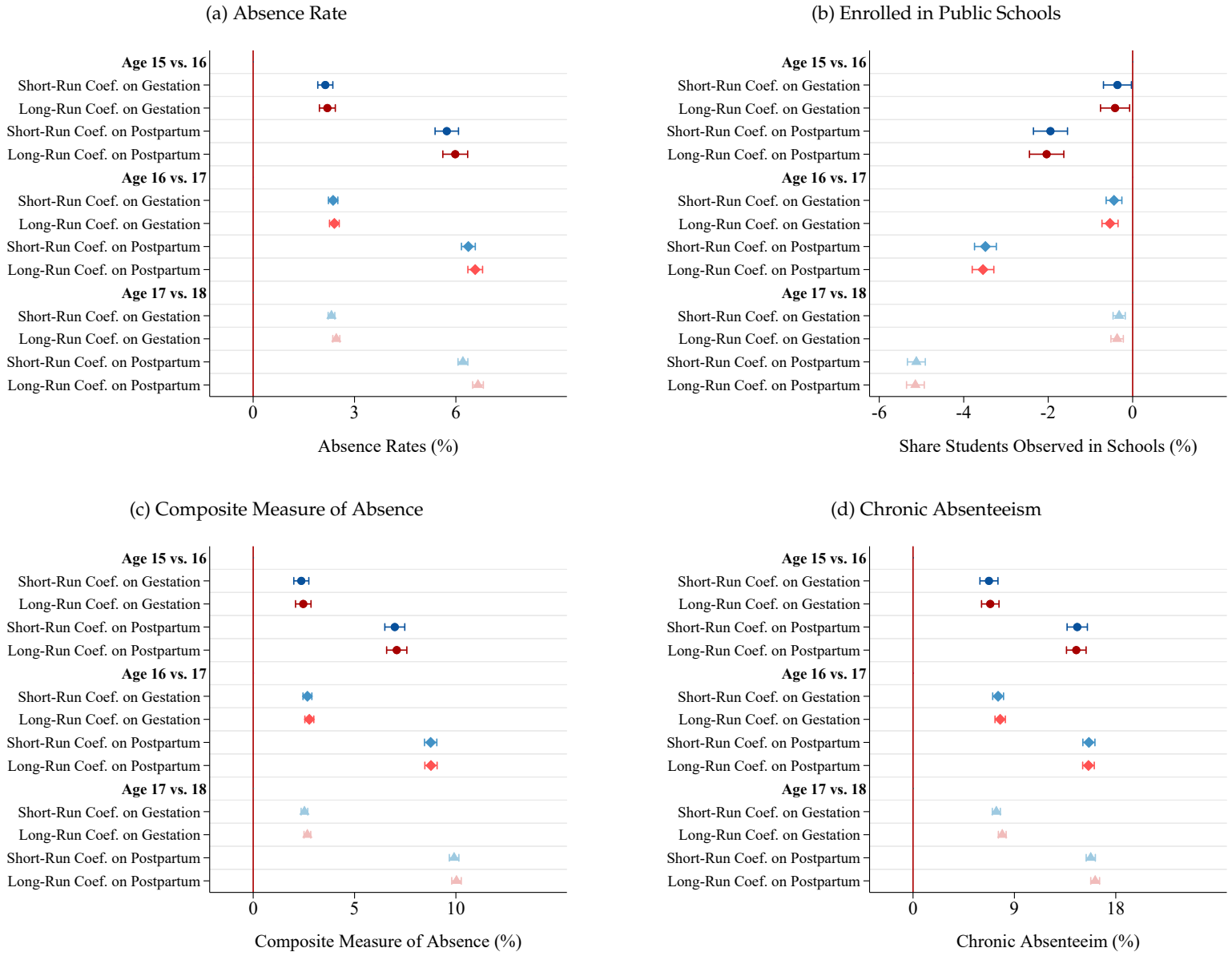


(c) *Old* Sample: Become Mothers at Ages 17 vs. 18



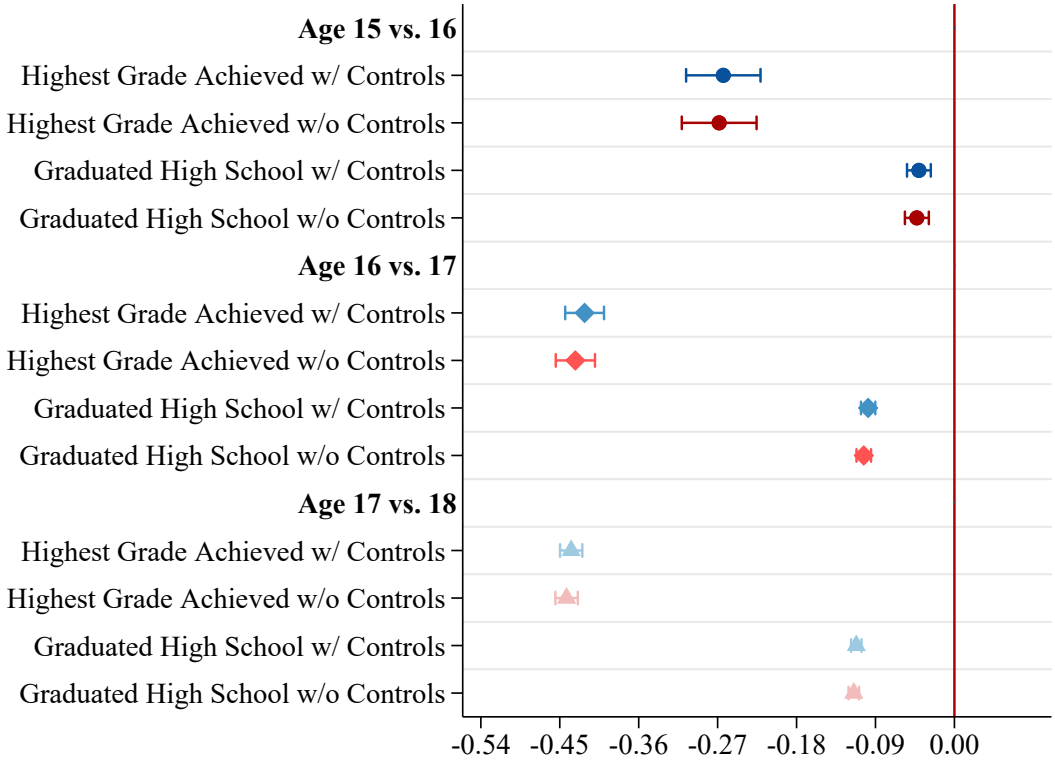
NOTES: The figure above plots the coefficients and 95% confidence intervals on the indicator denoting treated individuals from the estimation of equation (3) for all three samples. In contrast to panel (b) in Figures (6) through (8), the regressions in this analysis only includes those who have non-zero earnings at each age. Panel (a) displays the results for the *Young* sample, panel (b) for the *Middle* sample, and panel (c) for the *Old* sample. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. The estimated coefficients, standard errors, and p-values can be found in Table A1 in Online Appendix.

Figure A4: Robustness: Compatibility of Short-Run and Long-Run Econometric Models



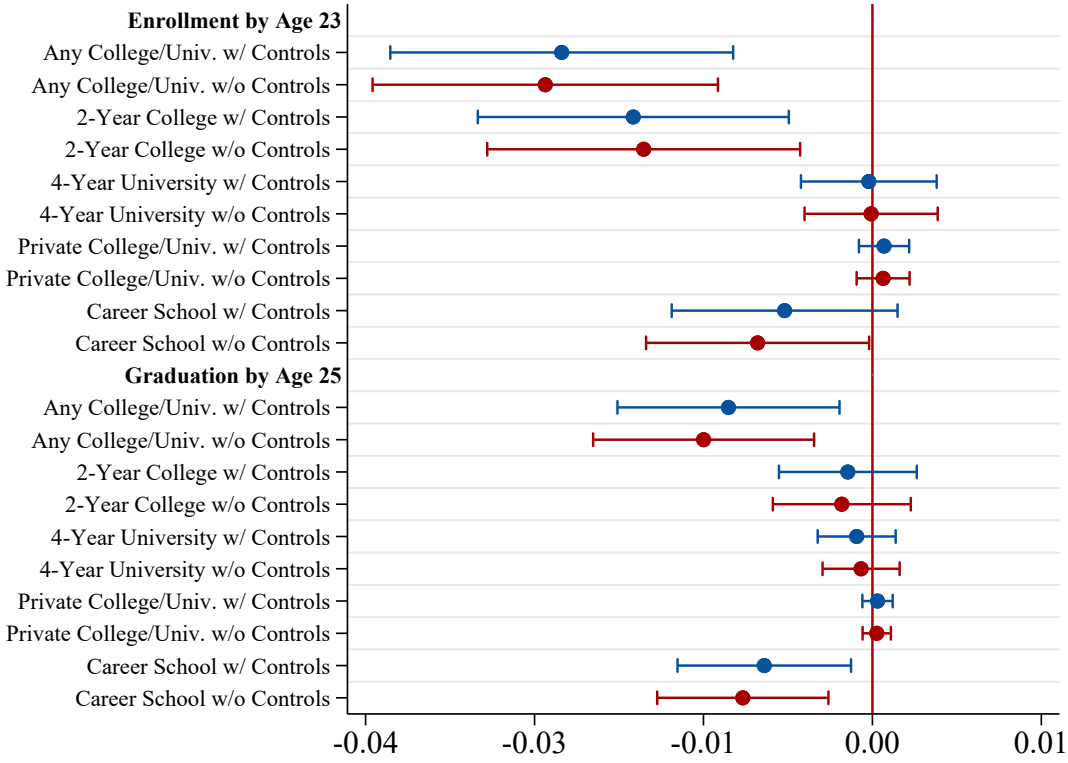
NOTES: The figure above plots the coefficients and 95% confidence (in reddish colors) from the estimation of my long-run econometric model (equation (3)), where the dependent variables are a version of short-run outcomes, aggregated separately for the gestation and postpartum periods. For comparison, my baselines estimates are reproduced in bluish colors. See section 4.3 for more details.

Figure A5: Robustness: Exclusion of Covariates When Estimating Completed Secondary Education by Age 20



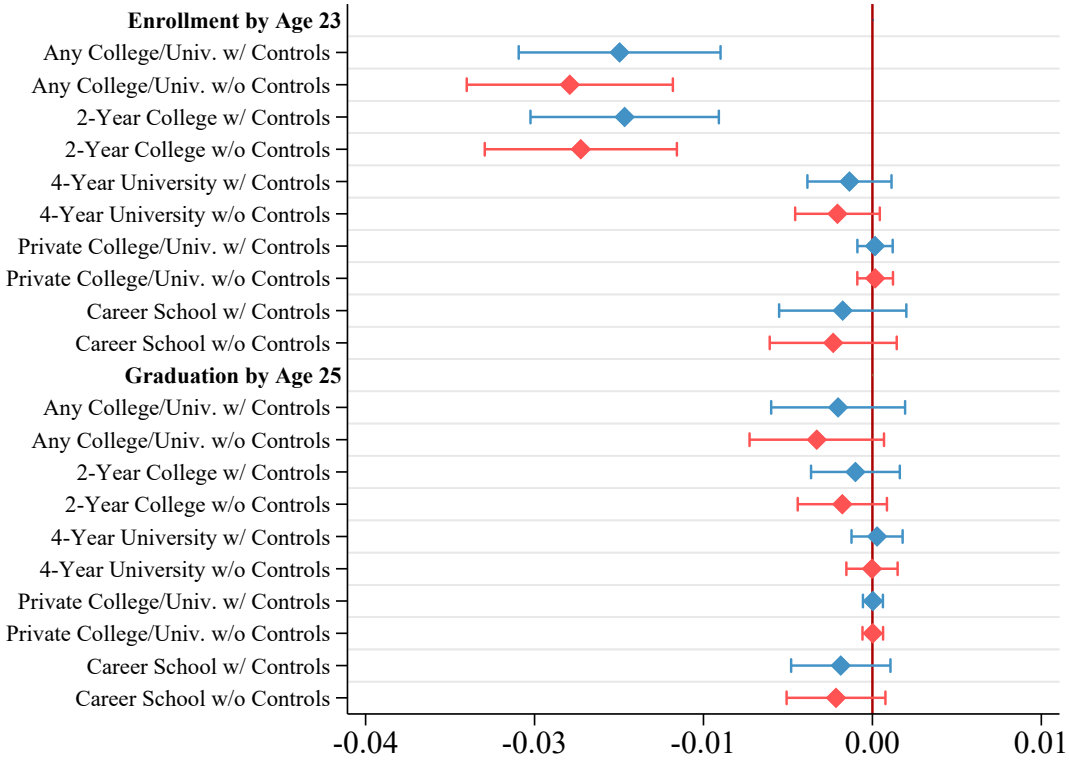
NOTES: The figure above plots the coefficients and 95% confidence intervals (in reddish colors) on the indicator denoting treated individuals from the estimation of equation (3) *without* any covariates ($\boldsymbol{X}_i$) for all three samples. $\boldsymbol{X}_i$ includes county fixed effects, and indicators for pre-determined characteristics—participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. All regressions include match group fixed effects. For comparison, my baselines estimates—*with* covariates ($\boldsymbol{X}_i$)—are reproduced in bluish colors.

Figure A6: Robustness: Exclusion of Covariates When Estimating College Education—Become Mothers at Ages 15 vs. 16



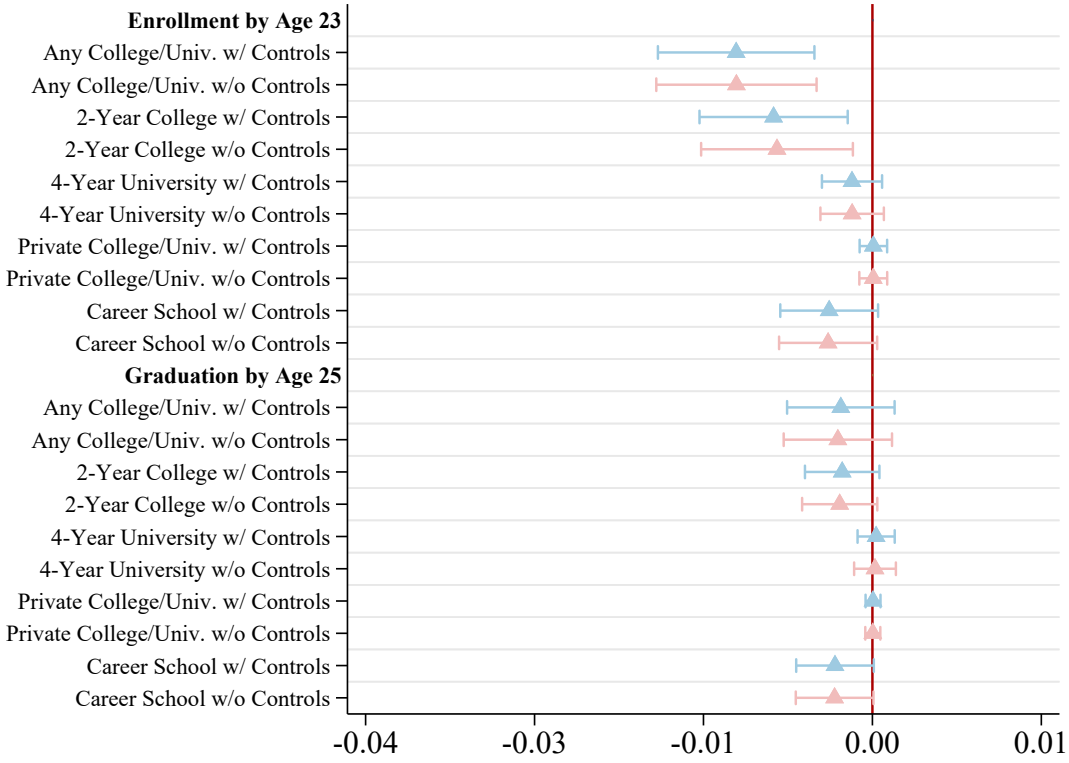
NOTES: The figure above plots the coefficients and 95% confidence intervals (in red) on the indicator denoting treated individuals from the estimation of equation (3) *without* any covariates ($\boldsymbol{X}_i$) for the *Young* sample. $\boldsymbol{X}_i$ includes county fixed effects, and indicators for pre-determined characteristics—participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. All regressions include match group fixed effects. For comparison, my baselines estimates—*with* covariates ($\boldsymbol{X}_i$)—are reproduced in blue.

Figure A7: Robustness: Exclusion of Covariates When Estimating College Education—Become Mothers at Ages 16 vs. 17



NOTES: The figure above plots the coefficients and 95% confidence intervals (in red) on the indicator denoting treated individuals from the estimation of equation (3) *without* any covariates ($\boldsymbol{X}_i$) for the *Middle* sample. $\boldsymbol{X}_i$ includes county fixed effects, and indicators for pre-determined characteristics—participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. All regressions include match group fixed effects. For comparison, my baselines estimates—*with* covariates ($\boldsymbol{X}_i$)—are reproduced in blue.

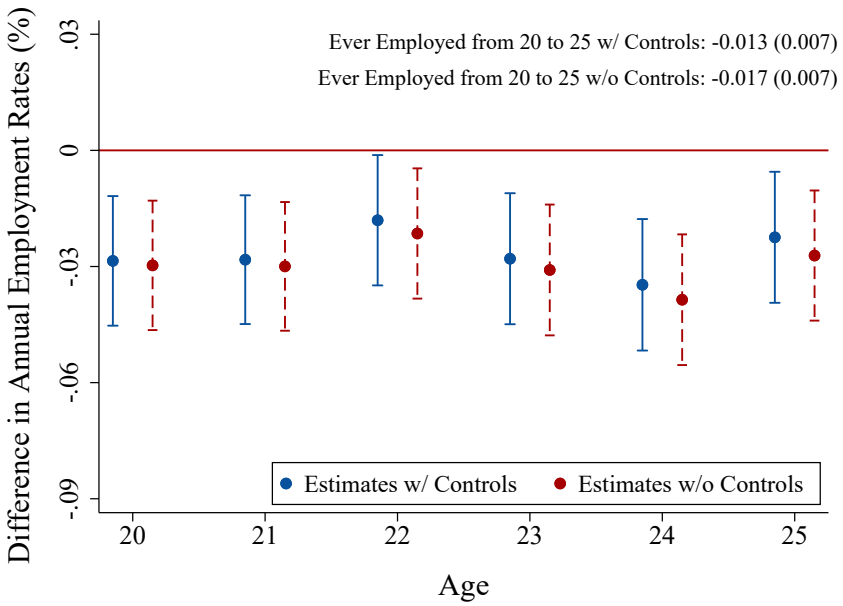
Figure A8: Robustness: Exclusion of Covariates When Estimating College Education—Become Mothers at Ages 17 vs. 18



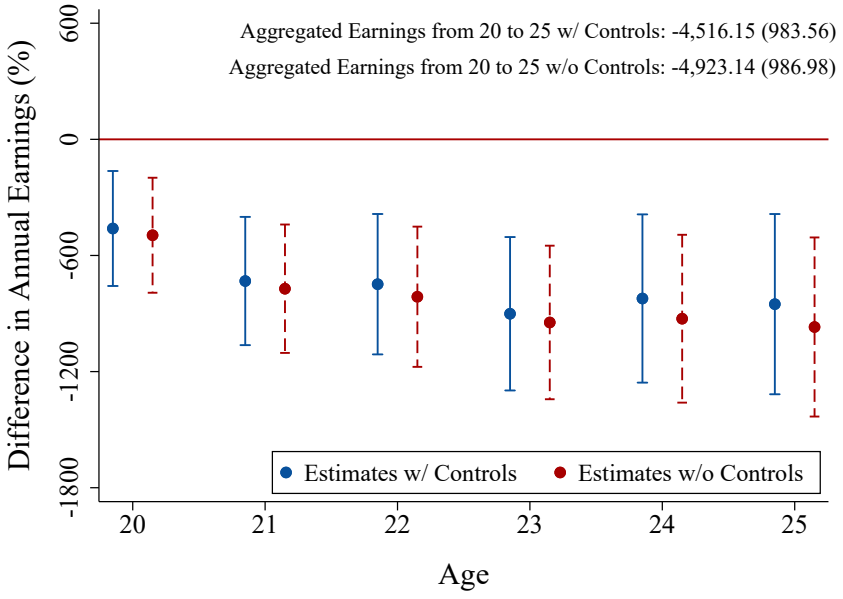
NOTES: The figure above plots the coefficients and 95% confidence intervals (in red) on the indicator denoting treated individuals from the estimation of equation (3) *without* any covariates ($\boldsymbol{X}_i$) for the *Old* sample. $\boldsymbol{X}_i$ includes county fixed effects, and indicators for pre-determined characteristics—participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. All regressions include match group fixed effects. For comparison, my baselines estimates—*with* covariates ($\boldsymbol{X}_i$)—are reproduced in blue.

Figure A9: Robustness: Exclusion of Covariates When Estimating Labor Market Outcomes—Become Mothers at Ages 15 vs. 16

(a) Annual Employment Status



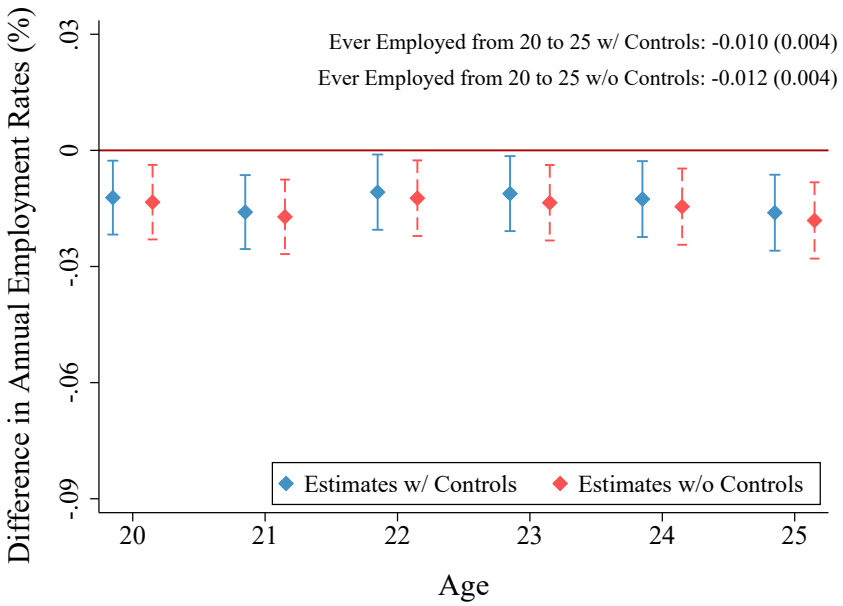
(b) Annual Earnings



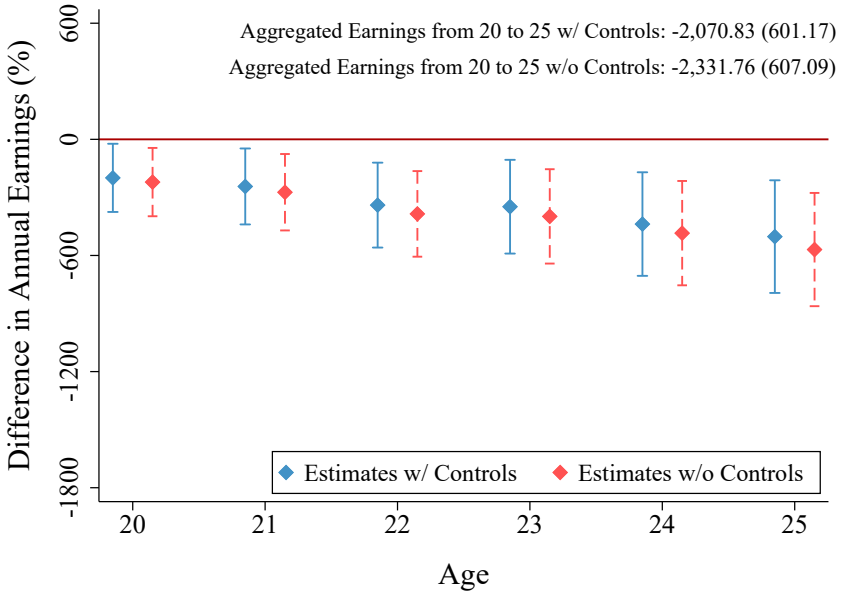
NOTES: The figure above plots the coefficients and 95% confidence intervals (in red) on the indicator denoting treated individuals from the estimation of equation (3) *without* any covariates ($\boldsymbol{X}_i$) for the *Young* sample. $\boldsymbol{X}_i$ includes county fixed effects, and indicators for pre-determined characteristics—participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. All regressions include match group fixed effects. For comparison, my baselines estimates—*with* covariates ($\boldsymbol{X}_i$)—are reproduced in blue.

Figure A10: Robustness: Exclusion of Covariates When Estimating Labor Market Outcomes—Become Mothers at Ages 16 vs. 17

(a) Annual Employment Status



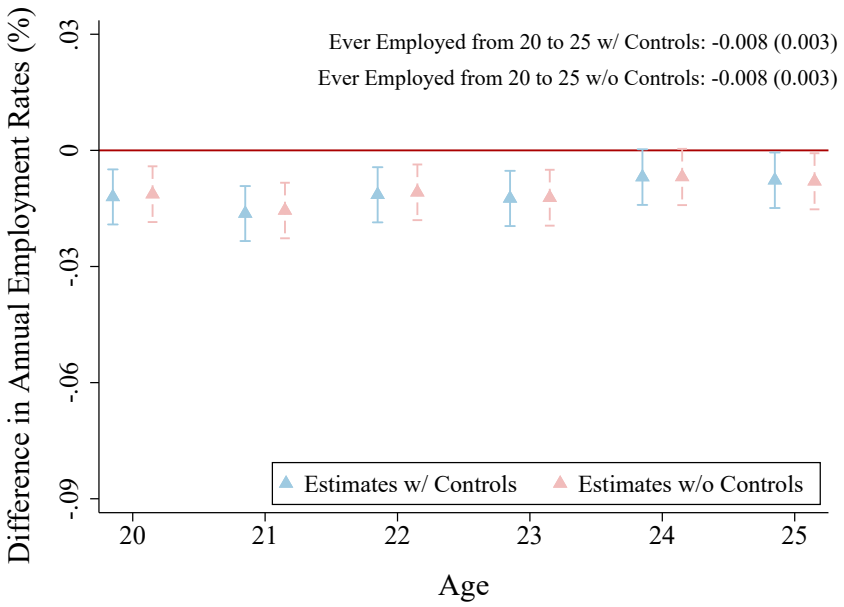
(b) Annual Earnings



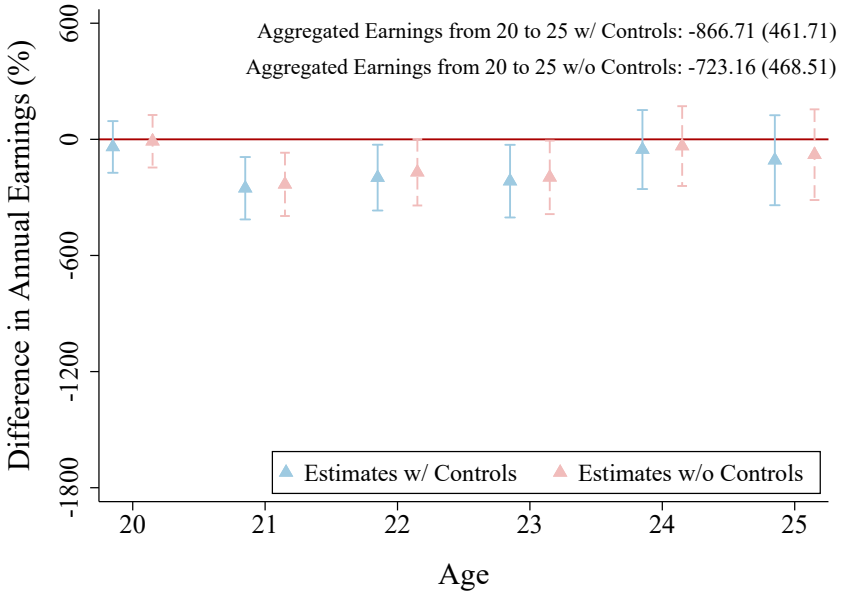
NOTES: The figure above plots the coefficients and 95% confidence intervals (in red) on the indicator denoting treated individuals from the estimation of equation (3) *without* any covariates ($\boldsymbol{X}_i$) for the *Middle* sample. $\boldsymbol{X}_i$ includes county fixed effects, and indicators for pre-determined characteristics—participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. All regressions include match group fixed effects. For comparison, my baselines estimates—*with* covariates ($\boldsymbol{X}_i$)—are reproduced in blue.

Figure A11: Robustness: Exclusion of Covariates When Estimating Labor Market Outcomes—Become Mothers at Ages 17 vs. 18

(a) Annual Employment Status



(b) Annual Earnings



NOTES: The figure above plots the coefficients and 95% confidence intervals (in red) on the indicator denoting treated individuals from the estimation of equation (3) *without* any covariates ($\boldsymbol{X}_i$) for the *Old* sample. $\boldsymbol{X}_i$ includes county fixed effects, and indicators for pre-determined characteristics—participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. All regressions include match group fixed effects. For comparison, my baselines estimates—*with* covariates ($\boldsymbol{X}_i$)—are reproduced in blue.

Table A1: Main Results: Long-Run Effects on Labor Market Outcomes—Intensive Margin

Panel (a): *Young* Sample							
Dependent Variable:	Age	Age	Age	Age	Age	Age	Aggregated
Non-Zero Earnings	20	21	22	23	24	25	20–25
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\delta \times Treat_i$	85.812	-170.443	-687.412	-461.252	-206.648	-777.117	-3,706.838
	(248.240)	(270.358)	(300.941)	(329.433)	(365.327)	(382.645)	(1215.232)
	[0.730]	[0.528]	[0.022]	[0.162]	[0.572]	[0.042]	[0.002]
Control Mean	9,968	11,351	13,027	14,312	15,513	17,265	63,417
Relative to Control Mean (%)	0.9%	-1.5%	-5.3%	-3.2%	-1.3%	-4.5%	-5.8%
Treated Individuals (unique)	2,853	2,883	2,860	2,796	2,757	2,759	4,203
Treated Individuals (weighted)	2,856	2,886	2,863	2,798	2,760	2,762	4,206
Control Individuals (unique)	3,960	3,994	3,922	3,887	3,879	3,815	5,471
Control Individuals (weighted)	2,990	3,046	2,967	2,919	2,936	2,893	4,295
Student-Grading Period Obs.	6,813	6,877	6,782	6,683	6,636	6,574	9,674
R-squared	0.479	0.491	0.513	0.511	0.507	0.528	0.489
Panel (b): *Middle* Sample							
Dependent Variable:	Age	Age	Age	Age	Age	Age	Aggregated
Non-Zero Earnings	20	21	22	23	24	25	20–25
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\delta \times Treat_i$	-133.971	-232.150	-379.116	-263.203	-414.392	-375.660	-2,217.465
	(139.171)	(150.699)	(166.320)	(183.185)	(205.475)	(223.677)	(711.732)
	[0.336]	[0.123]	[0.023]	[0.151]	[0.044]	[0.093]	[0.002]
Control Mean	10,408	12,033	13,740	15,081	16,775	18,097	67,429
Relative to Control Mean (%)	-1.3%	-1.9%	-2.8%	-1.7%	-2.5%	-2.1%	-3.3%
Treated Individuals (unique)	10,572	10,494	10,394	10,295	10,118	10,036	14,311
Treated Individuals (weighted)	10,593	10,514	10,414	10,318	10,141	10,055	14,336
Control Individuals (unique)	12,187	12,142	12,010	11,988	11,833	11,781	16,106
Control Individuals (weighted)	10,765	10,750	10,527	10,473	10,338	10,298	14,508
Student-Grading Period Obs.	22,759	22,636	22,404	22,283	21,951	21,817	30,417
R-squared	0.398	0.418	0.428	0.430	0.436	0.427	0.414
Panel (c): *Old* Sample							
Dependent Variable:	Age	Age	Age	Age	Age	Age	Aggregated
Non-Zero Earnings	20	21	22	23	24	25	20–25
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\delta \times Treat_i$	129.264	-147.208	-131.736	-89.868	86.312	195.791	-498.509
	(96.619)	(125.866)	(120.278)	(133.135)	(145.593)	(164.879)	(531.956)
	[0.181]	[0.242]	[0.273]	[0.500]	[0.553]	[0.235]	[0.349]
Control Mean	10,652	12,620	14,274	15,909	17,277	18,818	71,191
Relative to Control Mean (%)	1.2%	-1.2%	-0.9%	-0.6%	0.5%	1.0%	-0.7%
Treated Individuals (unique)	20,203	20,205	20,146	20,048	19,731	19,518	26,582
Treated Individuals (weighted)	20,929	20,979	20,906	20,808	20,500	20,270	27,590
Control Individuals (unique)	22,116	22,172	22,055	21,978	21,667	21,501	28,689
Control Individuals (weighted)	21,332	21,374	21,179	21,075	20,644	20,508	27,833
Student-Grading Period Obs.	42,319	42,377	42,201	42,026	41,398	41,019	55,271
R-squared	0.380	0.354	0.397	0.399	0.402	0.399	0.377

NOTES: The table above reports estimates of the coefficients on the indicator denoting treated individuals from the estimation of equation (3) for all three samples. In contrast to panel (b) in Tables (6) through (8), the regressions in this analysis only includes those who have non-zero earnings at each age. Panel (a) displays the results for the *Young* sample, panel (b) for the *Middle* sample, and panel (c) for the *Old* sample. The regressions include match group fixed effects, county fixed effects, and indicators for pre-determined characteristics, specifically participation in vocational and special education, at-risk dropout students, limited English proficiency, and above-median intensity for disruptive/risk behaviors. Robust standard errors clustered at the individual level are reported in parentheses, and p-values are reported in brackets.