The Heterogeneous Effects of Parental Divorce on Children*

Briana Ballis
Andrew C. Johnston
Patrick Zimmer

September 28, 2026

Abstract

Does parental divorce harm all children, or only some? We link the universe of Texas birth records to marriage, divorce, school, and criminal records, following six million children through school, and compare the trajectories of children whose parents divorce early with those whose parents divorce later. Test scores, attendance, and behavior deteriorate starting at separation, reducing an index of predicted adult earnings by 1 percent. We then develop a method that recovers the variance of treatment effects from the excess dispersion in outcome changes among children of divorce, relative to children whose parents divorce later. The mean masks substantial heterogeneity: the standard deviation of effects is more than three times the mean, driven by behavioral rather than cognitive skills. Roughly a third or more of children experience no decline. This heterogeneity lies within families rather than between them: the effect on one child does not predict the effect on a sibling, and divorces of parents with criminal records are, on average, no less harmful. Observables explain little of the variance, though boys, lower-achieving students, and later-born children fare worse. It is therefore difficult to predict in advance which children will be harmed.

*Ballis, UC Merced: bballis@ucmerced.edu; Johnston, UT-Austin: andrew.johnston@austin.utexas.edu; Zimmer, UC Davis: ptzimmer@ucdavis.edu. We thank Raj Chetty, Martin Daly, Michael Dinerstein, Jamie Fogel, Andrew Garin, Laura Giuliano, Felipe Goncalves, Jon Gruber, Nathan Hendren, Dan Herbst, Edvin Hertegard, Mark Hoekstra, Caroline Hoxby, Melissa Kearney, Felix Koenig, Jessica Laird, Lars Lefgren, Alex Mas, Valerie Ramey, Heather Royer, Lucie Schmidt, and Till von Wachter. We are also grateful to numerous seminar and conference participants at UC Davis, Oregon State University, AEFP, SOLE, WEAI, the Economic Demography Workshop, and the All California Labor Confrence for helpful comments and conversations. This work has been supported by the Smith Richardson Foundation and UC Merced. Any opinions and conclusions expressed herein are those of the authors and do not necessarily reflect the views of the Texas Education Agency.

Each unhappy family is unhappy in its own way.

—Leo Tolstoy, *Anna Karenina*

1 Introduction

Nearly a third of American children experience their parents' divorce before adulthood (Kennedy and Bumpass, 2008; Johnston et al., 2025). A large body of research shows that these children fare worse on average than children whose parents stay married (Amato and Keith, 1991; Amato, 2001; McLanahan et al., 2013), and recent quasi-experimental work indicates that at least part of this gap is causal (Gruber, 2004; Frimmel et al., 2024; Holm et al., 2024; Johnston et al., 2025). But an average effect may conceal substantial heterogeneity. Divorce ends marriages of very different kinds—some violent, some merely unhappy—and there is little reason to expect it to affect every child in the same way. The question that matters for families, courts, and policymakers is not whether divorce harms children on average but how widely its effects vary, how many children are harmed, and whether one can tell in advance which. Existing studies cannot address these questions: they estimate mean effects, at most for a few subgroups, and the variance of treatment effects is not identified from means (Heckman et al., 1997; Bitler et al., 2006).

In this paper, we estimate both the average effect of parental divorce on children and how much that effect varies from child to child. We link the universe of Texas birth records for the 1976–1997 cohorts to marriage, divorce, school, and criminal records, and follow more than six million children from birth through school. We first estimate the average effect of divorce on test scores, attendance, discipline, program placement, and mental health, and on an index that combines these measures into each child's predicted adult earnings, comparing children whose parents divorce earlier with those whose parents divorce later. We then develop a method that recovers the variance of divorce effects from the dispersion of children's outcomes around separation. Combining the mean and the variance, we ask how many children are harmed, how many are not, and whether anything observable—family background, the child's characteristics, or a parent's history—predicts who is harmed by divorce and who is not.

Our empirical approach proceeds in two stages. First, we estimate the dynamic average effects of divorce by comparing children whose parents separate during grades 4–8 with children whose parents separate later. Because a child's household changes when the parents separate rather than when the decree is finalized, and we observe only the decree, we date each divorce 21 months before finalization, the average separation-to-divorce interval among Texas couples in the Panel Study of Income Dynamics.[1] Both the treatment and control group experience parental divorce, but at different points in childhood. Their outcomes nevertheless exhibit different trends before separation. We estimate the differential linear trend in the pre-separation period and measure subsequent departures from its projected path, which yields a conservative estimate.[2] Identification rests on the assumption that,

[1] The PSID Marriage History File reports the month of separation and of finalized divorce. Among 184 Texas marriages ending in divorce during 1993–2012, the mean interval is 21.7 months (median 10.5). Results are robust to intervals of 15, 18, and 24 months (Appendix Table A.1).

[2] Pre-separation trends may partly reflect anticipatory responses or family disruption already underway. To the extent that these changes are consequences of the divorce process, extrapolating the pre-separation trend incorporates some of those consequences into the counterfactual. Our estimates therefore capture departures from that trajectory and may understate the total adverse effect of family dissolution.

absent earlier separation, this differential trend would have continued over the observation window.

Second, we estimate how much divorce effects vary across children. The intuition is simple: if divorce affected every child in the same way, the outcomes of children of divorce would spread out no faster than those of comparable children whose parents have not yet separated. If instead the effects of divorce vary, children of divorce would fan out, exhibiting excess variation that begins around the time of divorce.[3] We measure this excess variation, combine it with the average effect, and translate the two, under a range of distributional assumptions, into the share of children who are not harmed by divorce and the share who are especially harmed, as measured by predicted earnings.

Two kinds of events might coincide with divorce. The first are shocks that may make divorce more likely and also affect children directly. For these to contaminate our estimates, such shocks would need to arise suddenly at the time of separation and be persistent, similar to the temporal structure of separation itself. Instead, the typical precursors of divorce are slow-moving and long-standing. In a survey of 1,000 divorced Americans, the most commonly reported causes included lack of family support (43 percent), incompatibility (31 percent), lack of intimacy (31 percent), and financial disagreement (24 percent) (Bieber and Ramirez, 2023). These typically build for years before a separation, and exposure to most of them falls rather than rises once the parents part.[4] Similarly, Johnston et al. (2025) use census data to measure potential shocks, finding no increase in job loss, psychiatric hospitalization, or incarceration in the years before divorce, and parental income is flat until the divorce itself. The second kind of coinciding event consists of consequences of divorce: lost economies of scale, residential moves, school changes, less parental time, the entry of stepparents, greater family complexity, and so forth. These are not confounders but mechanisms by which divorce affects children. Our estimand is the effect of family dissolution together with the changes it sets in motion, not the effect of the divorce decree holding living arrangements fixed.

Our event studies reveal that divorce generates modest negative effects on measured cognitive skills. Math and reading scores each fall by about 0.02σ within two years of divorce, with some evidence of recovery by the end of the window, though the later estimates are imprecise. Behavioral outcomes show larger relative changes: disciplinary infractions increase by 18 percent relative to the mean at peak, while absences rise by 8 percent. Educational classifications also shift, with gifted program designation falling by 6 percent, though we find no increase in classification for emotional disturbance. Classification changes may reflect both real changes in performance and reduced parental advocacy for educational placements.

These outcomes each capture a distinct margin of child development, but they raise a natural question: what do these disparate changes imply for children's long-run wellbeing? To answer this, we use administrative earnings records in Texas to combine our academic and behavioral measures into a single index of each child's predicted adult earnings, based on their contemporaneous outcomes, and trace how the index moves around divorce. We find

[3] Interpreting this excess variation as heterogeneity in effects requires that, without divorce, outcomes would have become more spread out at the same rate in both groups, which we check in the years before divorce, and that the effects of divorce be unrelated to how children's outcomes would otherwise have changed.

[4] Asked what signaled the end of the marriage, respondents pointed to lack of interest in one another (48 percent), poor conflict resolution (47 percent), and avoiding one another (45 percent).

that divorce reduces predicted earnings at age 27–29 by 1.0 percent, with no subsequent rebound. This is smaller than the effect of divorce on realized adult earnings in Johnston et al. (2025), who find that divorce at comparable ages reduces income at age 27 by roughly 6 percent. The index captures only the portion of the earnings effect that operates through skills measured in school through grade 8; changes in unmeasured characteristics, or effects on behavior that emerge after our window closes, contribute to realized earnings but not to the index.[5] Roughly three-quarters of this effect runs through non-cognitive skills, such as attendance and discipline, and about a third through cognitive skills, such as math and reading scores, with a small offsetting contribution from other measures.

We find that the effects of divorce vary substantially across children: the standard deviation of effects on predicted earnings is more than three times the mean effect. Because the mean and variance do not pin down the full distribution, we translate them into shares under a range of distributional assumptions. Under normality, roughly 38 percent of children experience a non-negative predicted effect and a similar share experience reductions more than twice the average. Allowing for skewness and heavy tails, the share not harmed ranges from about a third to well over half, and the estimated negative skewness pushes it upward. This dispersion arises largely from divorce's variable effects on non-cognitive skills, captured by school absences and disciplinary records. Effects on cognitive skills, as measured by math and reading scores, show no detectable heterogeneity: we cannot reject that divorce depresses measured achievement by a similar amount for all children.

The effect of divorce varies little with family background. The effect of divorce is similar across racial groups and between lower- and higher-income children. It varies more with characteristics of the child. The effect is larger for boys than for girls: by the end of our observation window, boys' predicted earnings fall nearly twice as much as girls'. It is also concentrated among younger siblings: higher-birth-order children experience declines more than three times as large as those of only children. Finally, the effect is larger for children who were not already high-achieving: those in the bottom lose roughly two and a half times as much predicted earnings as those in the top tercile, a gap driven primarily by larger declines in non-cognitive skills.

Much of the remaining variation in divorce's effects could reflect unobserved differences across families rather than across children within them. The consequences of divorce plausibly turn on two things a researcher cannot directly observe: the quality of the marriage being dissolved and the quality of the arrangement that replaces it. If such family-specific factors drive heterogeneity siblings should show correlated divorce effects.

We assess the importance of family factors by asking whether siblings who experience the divorce see similar changes in their outcomes. Because siblings share genes, a home, and a neighborhood, their outcomes are already correlated even before divorce. If divorce effects systematically differed across families, we would expect the trend of one sibling around divorce to predict the trend of his sibling. We find that it does not. Sibling trends are no more alike after as a result of divorce than are the children of parents who stay married. This suggests that systematic differences in the quality of marriages and the arrangements that replace them account for little of the

[5]This index need not capture the full effect of divorce on earnings. It may overstate the effect if the divorce-induced changes in these measures are less predictive of earnings than their average counterparts, or understate it if divorce affects unmeasured skills that also shape earnings.

variation in how children are affected.

As further test of a family component, we ask whether divorces likely to remove potentially disruptive parents are less harmful. We link children to their parents' criminal records and focus on families in which a parent had a documented history of violence or disorder before the divorce—cases in which separation might plausibly remove a child from a harmful environment. Children in these families are not less harmed by divorce on average compared to the divorce of parents with no such record.

Whatever determines whether a given child is harmed appears to be specific to that child rather than to the family — siblings living through the same divorce are affected differently — and largely unobservable in administrative data. The practical implication is that neither courts nor parents can reliably sort divorces into those that will hurt children and those that will not.

Lastly, we investigate the changes in children's circumstances that accompany dissolution. Within two years, eligibility for free or reduced-price lunch rises by about 16 percent of the comparison-group mean, and the probability of changing schools rises by a similar amount. After divorce, children are more likely to move to schools with lower estimated behavioral value add, but with similar levels of cognitive value add.

Our study contributes to a growing literature using administrative data to examine the effects of family structure on children. A central challenge in this literature is that families who divorce differ from those who do not, and recent work has attempted to address these challenges using quasi-experimental designs. One strand exploits variation in divorce laws. Early evidence from Gruber (2004) exploits state-level variation in divorce laws during the 1970s, finding that exposure to unilateral divorce laws reduced children's educational attainment and adult well-being, including higher suicide risk. Hertegård (2024) finds that a mandatory waiting period for divorce improves children's human capital formation on average. Together these studies suggest that easier access to divorce harms children while restrictions help them. They identify the effects of a policy choice, where we identify the effects of divorce itself. [6] A second strand estimates the consequences of divorce directly. Frimmel et al. (2024) instrument for divorce using workplace sex composition and find that induced divorces reduce children's later educational and labor market success. Their instrument identifies effects for couples whose divorce is induced by workplace sex composition, whereas we estimate effects for the broad population of children whose parents divorce.

Most similar to our work is that of Holm et al. (2024) who, like us, use a stacked difference-in-differences around divorce events to show negative effects on children's test scores, both immediate and medium-term in Denmark. Also similar to our work is Laird et al. (2023) who finds that younger divorces reduce school enrollment, completion, and exam scores using a sibling fixed-effect design. Finally, broadening the scope of outcomes, Johnston et al. (2025) examines event studies around divorce and finds they have a significant effect on child mortality and teenbirths, as well as college attendance and adult earnings in adulthood.

[6] Estimates based on divorce laws capture the effect of the policy on all exposed children, most of whose parents never divorce. Rescaling by the change in the divorce rate gives an effect per divorce for couples whose divorce status the law changed, but only if the law affects children solely through divorce. Stevenson and Wolfers (2006) show that this is unlikely, since unilateral divorce reduced domestic violence and female suicide, changing conditions within marriages that remained intact.

This paper makes three primary contributions to the literature on family structure and child development. First, we provide the first systematic analysis of heterogeneity in divorce effects. While previous studies have examined differences across subgroups (Gruber, 2004; Johnston et al., 2025), we quantify the overall variance in treatment effects. Second, we provide the most comprehensive evidence on immediate educational effects of divorce in the United States. Most existing evidence comes from Northern European countries with different institutional and cultural contexts. Third, we leverage exceptionally rich administrative data that follows children from birth through schooling, capturing not only academic achievement but also discipline, attendance, program placement, and mental health—providing an unusually holistic picture of how divorce shapes child development.

2 Background

2.1 The Divorce Process and Timeline

Divorce typically begins with one spouse filing a petition for marital dissolution, followed by temporary orders governing custody, support, and asset use during proceedings. Most cases involve negotiation or mediation, with only 5% going to trial (Maccoby and Mnookin, 1992). In our Texas data, the median divorce is finalized 11 months after filing, with 90% complete within two years.

From anthropological studies, children's awareness of the divorce often unfolds gradually. While legal filing marks the official start, many parents delay informing children until separation is certain, hoping to shield them from uncertainty (Hogan et al., 2002). Physical separation typically occurs around the time they file, making the dissolution visible to children even if not explicitly discussed. The period between filing and finalization—when custody arrangements remain unsettled and parental conflict may peak—can be particularly stressful for children (Kelly, 2012). This extended timeline means our "treatment" captures not a single shock but a prolonged and potentially heterogeneous period of family reorganization.

2.2 Divorce Law in Texas

Texas family law shapes how divorce affects children through custody, support, and property division rules. As a community property state, Texas divides marital assets equally, potentially reducing post-divorce conflict over property but not necessarily preserving resources for children since any division reduces economies of scale.

The Texas Family Code establishes a "standard possession order" that structures most custody arrangements: non-custodial parents (typically fathers) receive alternating weekends, one weeknight per week, and extended summer possession. Because this standardized schedule is the statutory default, most children experience a similar reduction to father involvement (Texas Family Code §153.258).[7] Joint managing conservatorship, Texas's version of joint legal custody, is presumed but grants primary residence to one parent, typically the mother.

[7] See (Texas Legislature, 2023).

Child support follows statutory guidelines based on the non-custodial parent's net resources: 20% for one child, 25% for two, up to 40% for five or more. However, enforcement remains incomplete, with only 60% of custodial parents receiving the full payment they are owed by statute (Grall, 2020). Though we cannot observe custody arrangements or child support payments in our data, heterogeneity in these could drive differences in the effects of divorce across families.

2.3 Conceptual Frameworks for Divorce Effects on Children

The literature identifies several mechanisms by which divorce may affect child development, each suggesting different patterns of heterogeneity.

Resource Dilution. Because divorce breaks the economies of scale in a home, it typically reduces the financial resources available to children. Economic resources initially fall by 50 percent and the average household has not recovered 10 years later (Johnston et al., 2025). In Texas, child support guidelines mandate 20% of net income for one child and 25% for two, but these transfers do not compensate for the roughly 50 percent decline children experience and also do not attempt to offset the economies of scale lost by maintaining two households. As stated above, in about half of cases even these modest child support requirements are not paid in full.

Time resources also diminish as one parent lives farther from the children and both parents increase their work hours Johnston et al. (2025). Non-custodial parents, typically fathers, reduce time investments dramatically—seeing children only during alternating weekends under standard possession orders (Seltzer, 1991). Even when physically present, divorced parents may be emotionally depleted by their own adjustment process, reducing the quality of parent-child interactions (Hetherington, 1999).

Social capital erodes as families lose connections to extended family networks, family friends, and community institutions. Children may change schools, lose peer networks, and experience reduced participation in extracurricular activities due to logistical and financial constraints (McLanahan et al., 2013). These cascading resource losses suggest divorce effects may be larger for children who previously enjoyed greater resources.

Family Stress Model. Beyond resource loss, psychological models posit that divorce generates acute and chronic stress that compromises parenting and child adjustment (Conger et al., 2010; Masarik and Conger, 2017). Under this framework, divorce operates not as a direct shock to children's resources, but rather through a cascade of stress-induced changes in the family system (Amato, 2010). The dissolution of marriage triggers multiple interconnected stressors—including ongoing legal and custody conflicts and significant emotional upheaval for parents—that strain family functioning along multiple dimensions.

According to the model, these stressors manifest most directly through changes in parenting quality. Parents in divorce often suffer increased psychological distress—including depression, anxiety, and anger—that diminishes their energy and capacity for effective parenting (McLoyd, 1990; Conger et al., 2010). This distress translates into concrete changes in parenting behavior: reduced warmth and responsiveness, less consistent monitoring, and permissive or more inconsistent discipline.

Simultaneously, inter-parental conflict often persists or intensifies around custody arrangements, financial support, and asset division, exposing children to ongoing arguments and forcing them to navigate the tensions between their parents. Children face their own stressors: divided loyalties, schedule disruptions, and uncertainty about the future. The cumulative burden of multiple transitions—residential moves, school changes, parental repartnering—can overwhelm children's coping capacity (Anderson, 2014).

The family stress model suggests possible heterogeneity in divorce effects based on the stresses imposed by divorce. It suggests, for instance, that couples able to dissolve their marriage cooperatively may have less negative divorce effects as they are able to maintain higher-quality parenting. Conversely, families with rancorous divorces may show larger negative effects as stressors compound.

Critically, the model also implies that some divorces may even improve child outcomes: when divorce ends a high-conflict or dysfunctional marriage, the reduction in chronic stress and improvement in household stability could outweigh the costs of family dissolution.

These mechanisms operate through resources that siblings share, and the resource account in particular predicts that more-advantaged children (who have more to lose) should be harmed most. We find neither pattern: siblings diverge no more than the children of continuously married parents, and the effect is no larger for higher-income children (see Section 5.7 for more detail). The heterogeneity we document instead varies with the individual child—sex, birth order, and prior achievement—rather than with the family's resources.

3 Data

Data for this project come from a novel restricted-access dataset that links multiple administrative data sources in Texas. We combine children's school records, birth records, and parental marital and criminal justice histories to follow over 6 million Texas children from birth through adulthood, observing both their developmental outcomes and evolving family structure. We describe each data source below, then detail our sample construction and present summary statistics.

3.1 Data Sources

Our analysis draws on linked administrative records covering the universe of births in Texas from 1976 to 1997, together with marriages, divorce, and criminal justice records spanning 1968 to 2017. We link these to educational records covering children's schooling from kindergarten through high school. By probabilistically linking these sources, we construct longitudinal histories for over 6 million children and their parents. This data library offers several key advantages: precise timing of family events, rich educational outcomes from kindergarten through high school, and the ability to track children before and after parental divorce. We describe each data source below and detail our linking procedure in Appendix B.

Birth Records: We begin with the Texas Birth Index (TBI), which records the universe of births in Texas

along with parents' full names (including maiden names for mothers) and birth years. Parent information is publicly available through the 1997 birth cohort, allowing us to observe children who experience divorce during their school years. These birth records serve as the basis for constructing family units: we use parental information to identify siblings and link children to their parents' subsequent marital and criminal justice records. We match records across datasets using a probabilistic matching algorithm based on names and birth years. While the algorithm allows progressively looser matches, for over 80% of the children in our sample, we can link parents through an exact agreement on names, which makes false positives unlikely (see Appendix B for validation details).[8]

Marital Histories: We construct detailed parental marital histories from the Texas Marriage and Divorce Indexes, which record all marriages and divorces occurring in Texas from 1968 to 2017. These records include the date of each marriage and the date each divorce was finalized, allowing us to identify precisely when children experience parental divorce. We merge these records to our parent-child linkages using full names, including mothers' maiden names.[9] Observing the exact timing of divorce is crucial for our event-study design, as it allows us to track children's trajectories before, during, and after family dissolution.

Schooling Records: Our primary outcome measures come from administrative records from the Texas Education Agency (TEA) spanning 1993 to present. We observe standardized test scores in math and reading for grades 3–8, which we standardize within grade-year to have mean zero and standard deviation one.[10] Beyond academic achievement, we track behavioral and developmental outcomes including school attendance, disciplinary infractions, special education placement, and gifted program participation from kindergarten through 12th grade. The data also contain markers of economic disadvantage—specifically eligibility for free and reduced-price lunch—allowing us to examine how divorce effects vary by socioeconomic status. These rich longitudinal records enable us to observe both immediate and medium-term effects of divorce on children's development.

Criminal Justice Records: While not used in our main analysis, these records enable a useful heterogeneity test. We observe parental interactions with the criminal justice system from the Texas Department of Public Safety. These data include conviction dates, sentence lengths, and offense types (e.g., property or violent crimes). These records allow us to test whether divorce effects vary with a parent's pre-divorce criminal record, as criminal behavior provides a plausible marker of marriages that may be less beneficial for child development. Specifically, we can identify households more likely characterized by violent or antisocial behavior and examine whether children from such families experience different—potentially even positive—effects from family dissolution.

[8] Several features of the data suggest these matches are of high quality. In the marriage records, 98.9% of couples have a unique name combination, so mismatches arising from shared names are unlikely to exceed roughly 1.1%. When linking parents to divorce records, we match on names and, where available, marriage date. Among couples married in Texas, marriage date combined with phonetically standardized surnames and the mother's and father's first names uniquely identifies 99.9%, leaving little room for false matches.

[9] We also utilize data from Docket Alarm, a repository for court filings and documents, to distinguish between divorce filing and finalization dates for robustness checks. These data also identify couples who initiated but did not finalize divorce proceedings, providing a useful comparison group.

[10] Beginning in 2013, test scores are available for grades 3–11.

3.2 Sample Construction and Treatment Definition

Our full dataset includes children born in Texas between 1976 and 1997 who appear in school records and whose parents we successfully identify in birth records. For our primary analysis, we focus on children born between 1985 and 1997—the cohorts for whom we observe complete test score histories from grades 3–8. We exclude children classified as Special Education or Limited English Proficient (29% of the sample) as their modified exams make scores difficult to compare over time.[11] This yields an analytical sample of approximately 4.3 million children, of whom 22% experience parental divorce during our observation window.

Most divorces occur before children enter school (Appendix Figure A.1). Among our sample, 71 percent of divorces occur before grade 4, 19 percent occur during grades 4-8 (our treatment window), and 9 percent occur after grade 8.

To ensure clean comparisons, we define the treatment group as children whose parents divorce between grades 4 and 8—the window over which we consistently observe outcomes—and the control group as children whose parents divorce in grades 9–12. This design compares families that all eventually divorce, exploiting the timing of divorce rather than selection into it. Throughout, we use expected rather than actual grade—grade progression assuming no retention—so that event time advances uniformly across children and our grade controls are not themselves affected by retention. To account for any test score advantages that retained students have from being older than their grade-level peers, we include grade-by-repetition fixed effects, described in detail in our empirical specification.

3.3 Summary Statistics

Table 1 documents our data linkage process and shows the extent of formal balance between the divorcing sample and comparison groups. Column 1 shows all Texas public school students, while Column 2 presents the 71 percent born in Texas for whom we can construct parent-child linkages through state birth records. The Texas-born sample closely resembles the full population, with the primary difference being lower limited English proficiency (LEP) rates—expected since non-Texas-born students disproportionately include recent immigrants.

We are able to match 50 percent of Texas-born students to marriage or divorce records. The unmatched 50 percent (Column 3) includes children whose parents never marry (roughly 20 percent based on vital statistics) and those whose parents married outside Texas. This unmatched group shows characteristics consistent with both unmarried parenthood and out-of-state origins: lower test scores, higher FRL eligibility, and larger shares of Black and LEP students.

Comparing always-married families (Column 4) to those that eventually divorce (Column 5) reveals the expected selection: children whose parents later divorce have somewhat lower baseline achievement (about 0.10σ) and higher economic disadvantage (8 percentage points in FRL eligibility) in 3rd grade. These gaps are small in absolute terms but not negligible—indeed, the difference in test scores is several times the size of the causal effects

[11] Including these students yields similar results (see Section 4.7).

we later estimate on those same tests, which is why we turn to a more tailored comparison.

Most importantly for our identification strategy, children whose parents divorce during grades 4–8 (our "treatment" group, Column 6) and grades 9–12 ("control," Column 7) are quite similar at baseline. The groups differ by 0.03–0.04σ in 3rd grade test scores and show nearly identical demographic composition and disadvantage rates. The similarity in baseline characteristics between treatment and control groups helps support our key identifying assumption of parallel pre-trends—that these observably similar children would have followed similar developmental trajectories absent the differential timing of their parents' divorces.

4 Average Effects of Divorce

4.1 Event-Study Difference-in-Differences

We estimate the dynamic effects of parental divorce on student outcomes using a stacked event-study design. The design compares within-student changes among children whose parents separate during grades 4–8 with contemporaneous changes among children whose parents separate later. The use of student fixed effects addresses time-invariant differences across children, while the later-treated comparison group provides a closely tailored comparison group to account for plausible counterfactual time trends. The design does not, however, eliminate the possibility that early- and later-separating families would have followed different outcome trajectories in the absence of early separation; we address this concern using the pre-trend adjustment described below.

We center event time on parental separation rather than the legal finalization of the divorce. Divorce finalization is an administrative endpoint that may occur long after spouses stop living together and children begin experiencing changes in parental contact, household resources, family conflict, and residential or school stability. Defining the event using finalization would therefore classify part of the family transition as occurring before treatment, potentially obscuring the timing of the effects of family dissolution.

Our Texas administrative records provide the quarter in which a divorce was finalized but do not identify the date of physical separation. We therefore impute separation using information from the Panel Study of Income Dynamics (PSID) Marriage History data, which report the month and year of both separation and divorce. The mean interval between separation and divorce finalization in those data is approximately 21 months.[12] Accordingly, we define the imputed separation date as seven quarters before the recorded quarter of divorce finalization.

We assign the imputed separation quarter to the corresponding school year and convert it to the child's expected grade, based on normal grade progression rather than realized progression. This avoids mechanically relating treatment timing to grade retention, which may itself be affected by divorce. Children whose imputed separation occurs in expected grades 4–8 form the treated cohorts, while children whose imputed separation

[12] We use the mean interval of 21.7 months from the uncensored Texas distribution reported in Appendix Table A.1, which is based on divorces finalized between 1993 and 2012 in the PSID Marriage History File. The Texas distribution closely resembles that of the rest of the United States: the median interval is 10.5 months in Texas and 12.0 months elsewhere, and roughly 60 percent of Texas divorces and 52 percent of other divorces are finalized within one year of separation. Capping intervals at 10 years lowers the non-Texas mean from 23.1 to 18.2 months but changes the Texas mean only slightly, from 21.7 to 20.4 months.

occurs in expected grades 9–12 serve as later-treated controls. Because separation is imputed rather than directly observed, we assess the sensitivity of our estimates to alternative separation-to-finalization intervals of 15, 18, and 24 months and find the results are largely robust (see Section 4.7).

To implement the stacked design, we construct a separate stack for each treated cohort $c \in \{4, \ldots, 8\}$, where c denotes the cohort's expected grade at imputed separation. Each stack contains the children whose own imputed separation occurs in grade c and the full set of later-treated children whose imputed separation occurs in grades 9–12. We assign the later-treated students grade c as a placebo event grade. Thus, each treated student appears only in the stack corresponding to their own imputed separation grade, whereas each eligible later-treated student appears in all five stacks.

For an observation in expected grade g and stack c, we define event time as

$$e_{igc} = g - c.$$

For treated students, c is their own imputed separation grade; for control students, it is the placebo grade assigned by the stack. Comparisons within a stack are therefore between treated and control students observed at the same event time relative to an imputed or placebo separation grade.

Our baseline specification allows treated and control students to have a differential linear pre-trend. Following Freyaldenhoven et al. (2021), we first estimate

$$\begin{aligned} Y_{igtc} = \rho \left[e_{igc} D_{ic} \mathbf{1}\{e_{igc} < 0\} \right] + \sum_{k=0}^{4} \pi_k \mathbf{1}\{e_{igc} = k\} D_{ic} \\ + \eta_{ic} + \lambda_{gc} + \tau_{tc} + u_{igtc}, \end{aligned} \tag{1}$$

where D_{ic} indicates that student i belongs to the treated cohort in stack c. The terms η_{ic}, λ_{gc}, and τ_{tc} denote student-by-stack, expected-grade-by-stack, and calendar-year-by-stack fixed effects, respectively. The coefficient ρ captures the pooled differential linear pre-trend between treated and control students. Including the post-event indicators in Equation (1) ensures that estimation of ρ is driven by pre-event differences.

We then remove the estimated differential trend from the treated students' outcomes:

$$\widetilde{Y}_{igtc} = Y_{igtc} - \widehat{\rho}\, e_{igc} D_{ic}.$$

Using this detrended outcome, we then estimate

$$\begin{aligned} \widetilde{Y}_{igtc} = \sum_{k=-4}^{-2} \gamma_k \mathbf{1}\{e_{igc} = k\} D_{ic} + \sum_{k=0}^{4} \alpha_k \mathbf{1}\{e_{igc} = k\} D_{ic} \\ + \eta_{ic} + \lambda_{gc} + \tau_{tc} + \varepsilon_{igtc}. \end{aligned} \tag{2}$$

We omit event time $e = -1$ as the reference period. The coefficients γ_k describe deviations from the extrapolated

differential pre-trend before imputed separation, while α_k capture corresponding differences during and after the imputed separation event. Standard errors are clustered at the student level, accounting for serial correlation within students and for the repeated appearance of control students across stacks.

The identifying assumption is that, absent separation during grades 4–8, the differential trend between early- and later-separating students would have continued linearly over the post-event period. The unadjusted estimates exhibit differential pre-trends for several outcomes, including absences, which motivates using the detrended specification as our baseline. These pre-event patterns may reflect differences in counterfactual trends, but they may also arise because the actual separation-to-finalization interval differs across families or because changes in conflict, household organization, and parental behavior begin before physical separation. Because family dissolution is a process whose consequences may begin before separation and continue afterward, we interpret event time as indexing this broader transition rather than an instantaneous response to the legal divorce decree.

In Section 4.7, we present estimates without detrending, allow the differential pre-trend to vary across treatment-cohort stacks, add controls for grade-repetition history, and vary the assumed separation-to-finalization interval.

4.2 The Average Effect of Divorce on Achievement

We begin by examining achievement among students whose imputed parental separation occurs in grades 4–8. Panel (a) of Figure 1 presents the event-study estimates for math scores, while Panel (b) presents the corresponding estimates for reading scores. The omitted period is the year immediately preceding the imputed separation, $e = -1$. Table 2 reports the event-study estimates by event year, showing the effect in each year following separation, while Table 3 pools the post-separation years into a single difference-in-differences coefficient.

In the baseline specification, the pre-event coefficients are small and statistically indistinguishable from zero in both subjects. Math scores remain approximately unchanged in the imputed separation year, with an estimated effect of -0.003 student standard deviations. They subsequently decline by 0.012 standard deviations at $e = 1$ and reach their largest decline of 0.022 standard deviations at $e = 2$, which generally corresponds to the school year in which the observed legal divorce is finalized.[13] The point estimates subsequently become less negative, declining in magnitude to 0.015 standard deviations at $e = 3$ and 0.004 at $e = 4$. The estimate at $e = 3$ remains statistically distinguishable from zero, whereas the estimate at $e = 4$ does not. This pattern suggests partial recovery by event-time 4, which is an increase from the trough ($p \approx 0.033$).

Reading scores respond somewhat earlier. The estimated effect is -0.008 student standard deviations in the imputed separation year, falls to -0.019 at $e = 1$, and reaches its largest magnitude of -0.021 at $e = 2$. Reading effects subsequently become less negative, with estimated effects of -0.010 standard deviations at $e = 3$ and -0.009 at $e = 4$. Although both estimates are statistically indistinguishable from zero, neither improvement relative to the maximum decline at $e = 2$ is itself statistically significant.

[13] Because we impute separation seven quarters before observed finalization and then convert both dates to school years, finalization falls at $e = 2$ for divorces finalized in the first, third, or fourth quarter and at $e = 1$ for divorces finalized in the second quarter.

When we pool the post-separation event years, the estimated average effect is a decline of 0.0097 student standard deviations in math (standard error 0.0033) and 0.0123 standard deviations in reading (standard error 0.0032). Because our baseline estimator removes an estimated differential linear pre-trend, the small adjusted lead coefficients should not be interpreted as an independent test of parallel trends. The causal interpretation instead relies on the identifying assumption described above: absent an earlier parental separation, the pre-event difference between the early- and later-treated groups would have continued along the estimated linear trajectory.

To provide a sense of magnitude, the maximum estimated math decline of approximately 0.022 student standard deviations is roughly one-half of the estimated effect of reducing class size by ten students in Cho et al. (2012). It is also approximately 14% of the effect of a one-standard-deviation increase in math teacher value-added for one school year estimated by Chetty et al. (2014).[14] These comparisons indicate that the peak achievement effects are meaningful but smaller than the effects of substantial changes in school inputs.

4.3 Effects on School Attendance, Discipline, and Grade Retention

We next examine effects on student engagement and behavior among students whose imputed parental separation occurs in grades 4–8. Figure 2 presents estimates for days absent, truancy (more than 10 days absent), the probability of disciplinary action against the student, and the number of days disciplined. We examine grade repetition separately below.

In the pre-trend-adjusted baseline specification, days absent increase modestly by 0.057 days in the imputed separation year and then rise more sharply, increasing by 0.344 days at $e = 1$ and reaching a maximum of 0.436 days at $e = 2$, which generally corresponds to the school year in which the divorce is finalized. The maximum effect represents approximately 8% of the comparison-group mean of 5.77 absent days per year. The estimates subsequently decline to 0.274 days at $e = 3$ and 0.178 at $e = 4$.

Truancy follows a similar pattern but does not increase significantly in the imputed separation year. The probability of missing more than 10 days rises by 1.44 percentage points at $e = 1$ and reaches a maximum increase of 1.77 percentage points at $e = 2$, approximately 11% of the comparison-group mean. The effect then declines to 0.98 percentage points at $e = 3$ and 0.37 percentage points at $e = 4$, with the latter estimate statistically indistinguishable from zero.

Disciplinary outcomes increase more gradually and remain elevated longer. The probability of receiving any disciplinary action rises by 1.04 percentage points at $e = 1$ and reaches its largest increase of 2.17 percentage points at $e = 3$, approximately 18% of the comparison-group mean. It remains elevated by 1.65 percentage points at $e = 4$. Days disciplined exhibit a similar pattern, increasing by 0.15 days at $e = 1$, 0.25 at $e = 2$, and 0.51 at $e = 3$. The estimate remains elevated at 0.44 days at $e = 4$. The maximum increase is approximately 56% of the comparison-group mean of 0.97 days disciplined per year.

[14] Cho et al. (2012) estimate that reducing class size by ten students increases test scores by approximately 0.04–0.05 student standard deviations. Chetty et al. (2014) estimate that a one-standard-deviation increase in math teacher value-added raises end-of-year math scores by approximately 0.163 student standard deviations.

In contrast, our estimates provide little evidence that parental separation increases grade repetition. Grade repetition occurs in approximately 0.85% of student-year observations. The event-study estimate rises by 0.13 percentage points at $e = 1$, but this estimate is only marginally statistically significant, and the subsequent estimates are small and statistically indistinguishable from zero. Consistent with this pattern, the specification pooling all post-separation years produces an estimated increase of only 0.06 percentage points (standard error 0.05 percentage points).

These patterns point to attendance and disciplinary behavior as important dimensions of school disruption around parental separation. Increased absences may reflect logistical instability as parents establish separate residences, changes in parental monitoring, illness, or children's responses to family stress. As we show below, however, we do not detect a statistically significant within-family correlation in divorce-related changes in absences or disciplined days among siblings. Although this result does not rule out common family-level mechanisms, it suggests that child-specific responses play an important role. The sustained increase in disciplinary outcomes may similarly reflect emotional distress or difficulty regulating behavior.

4.4 Divorce Effects on Educational Classifications

We next examine whether parental separation affects participation in special education (SpEd) and gifted programs. These classifications influence the educational services students receive, but they reflect both students' underlying needs and performance and decisions made by schools and families. Figure 3 presents the event-study estimates for each classification.

In the baseline specification, we find no evidence that parental separation changes overall SpEd participation. The adjusted pre-event coefficients are small, and the post-event estimates remain close to zero throughout the event window. At $e = 4$, for example, the estimated effect is -0.26 percentage points (standard error 0.22), which is statistically indistinguishable from zero. Estimates that separately examine entry into and removal from special education likewise reveal no statistically detectable changes (Appendix Table A.2). Still, the signs of the effects are worth noting. Increased removals are consistent with parents having less time to invest in the placement process, while increased additions may point to declines in academic or behavioral functioning.[15]

We also decompose SpEd participation by the student's recorded primary disability category in Appendix Table A.3. The post-separation point estimates for emotional-disturbance classification range from approximately 0.03 to 0.06 percentage points. The largest estimate, at $e = 3$, represents an increase of approximately 10% relative to the comparison-group mean. Although this magnitude is potentially meaningful, the estimates are imprecise and statistically indistinguishable from zero.

We similarly find no detectable increases in intellectual disability, learning disability, speech impairment, or the combined category of other severe disabilities. The only consistent change is a decline in classification for

[15]These estimates rule out immediate, statistically significant changes but not longer-run ones. Because SpEd identification is a lengthy process, a meaningful change in placement may take time to appear, so the absence of a detectable short-run effect does not preclude longer-run impacts.

"other" health impairment, which falls by 0.09 percentage points at $e = 1$, 0.13 at $e = 2$, 0.22 at $e = 3$, and 0.24 at $e = 4$. Because overall SpEd participation remains unchanged, this decline may reflect changes in how students are classified across disability categories rather than a change in the prevalence of underlying health conditions.

Gifted participation exhibits a different pattern. The adjusted pre-event coefficients are small, and participation remains approximately unchanged in the imputed separation year and at $e = 1$. It then declines by 0.51 percentage points at $e = 2$, which corresponds to the school year in which the legal divorce is finalized. The decline reaches 0.88 percentage points at $e = 3$ and remains similar, at 0.83 percentage points, at $e = 4$. These estimates are statistically significant from $e = 2$ through $e = 4$. The maximum decline is approximately 10% of the treated group's grade-3 gifted participation rate of 8.5%.

To clarify the source of this decline, we separately examine entry into gifted programs among students who were not classified as gifted in grade 3 and removal among students who were in Appendix Table A.2. The results point to changes along both margins. Gifted entry falls by 0.49 percentage points at $e = 3$ and 0.50 percentage points at $e = 4$. Both estimates are statistically significant at the 5% level and represent declines of approximately 8% and 9%, respectively, relative to the comparison-group mean of 5.9%.

Among students classified as gifted in grade 3, the probability of removal rises by 1.1 percentage points at $e = 1$, 3.1 percentage points at $e = 2$, 4.7 percentage points at $e = 3$, and 4.3 percentage points at $e = 4$. The estimate at $e = 1$ is statistically significant at the 5% level, while the estimates from $e = 2$ through $e = 4$ are significant at the 0.1% level. Relative to the comparison-group removal rate of 14.2%, these estimates represent increases of approximately 8%, 22%, 33%, and 30%, respectively. The decline in overall gifted participation therefore reflects both reduced entry and, particularly, increased removal from gifted programs.

These estimates do not identify why gifted participation changes. The pattern could reflect changes in academic performance, parental advocacy, student participation, or school classification decisions surrounding family dissolution. The results nevertheless indicate a reduction in gifted program participation in the years immediately following divorce, without a corresponding increase in special education.

4.5 A Predicted Adult-Earnings Index

The outcomes examined above capture different dimensions of children's educational trajectories and are measured in different units. Test scores are standardized, absences are measured in days, and discipline, grade retention, and educational classifications are measured as probabilities. Examining these outcomes separately is useful for understanding how family dissolution affects particular aspects of children's schooling, but it makes it difficult to summarize their combined importance.

We construct a predicted adult-earnings index that translates these educational outcomes into a common, economically interpretable metric. The index serves three purposes. First, it combines changes across achievement, attendance, discipline, grade progression, and educational classifications using weights determined by their empirical relationship with adult earnings rather than arbitrary equal weights. Second, it provides a summary

outcome with which to assess whether the overall effect of parental separation is robust across specifications, even when individual outcomes differ in their timing or precision. Third, it produces a relatively continuous measure that is well suited to our subsequent analysis of the variance of divorce effects. True continuity is not required for that analysis, but it provides substantially more information about changes in the outcome distribution than individual binary classifications whose variances are mechanically related to their means.

We construct the index using real annual earnings reported in the Texas Workforce Commission records. For students observed in the earnings records at ages 27, 28, and 29, we define

$$\overline{W}_i = \frac{W_{i,27} + W_{i,28} + W_{i,29}}{3}, \qquad L_i = \log(1 + \overline{W}_i),$$

where $W_{i,a}$ denotes student i's real annual earnings at age a. We do not assign zero earnings to individuals who are absent from the TWC records but simply take the mean of whatever wages are observable. The prediction target should therefore be interpreted as mean earnings conditional on appearing in the TWC earnings records.

For each grade $g \in \{3, \ldots, 8\}$, we estimate a separate prediction model using older cohorts without a linked parental divorce in grades 1–12 or the four years after predicted high school graduation:

$$L_i = \alpha_g + X_i'\gamma_g + \mu_{c(i),g} + \lambda_{b(i),g} + \sum_{k=3}^{g} Z_{ik}'\beta_{kg} + \varepsilon_{ig}. \tag{3}$$

Here, X_i contains sex and race indicators, $\mu_{c(i),g}$ denotes fixed effects for the student's grade-3 campus, and $\lambda_{b(i),g}$ denotes birth-cohort fixed effects. The vector Z_{ik} contains the educational outcomes observed in grade k: math and reading scores, days absent, the occurrence and duration of disciplinary incidents, grade retention, and participation in special education, LEP, and gifted programs.

Equation (3) is estimated separately for each terminal grade g. The grade-3 prediction therefore uses only grade-3 educational outcomes, the grade-4 prediction uses outcomes from grades 3 and 4, and so forth. Denote the resulting raw prediction by

$$\widetilde{P}_{ig} = \widehat{\alpha}_g + X_i'\widehat{\gamma}_g + \widehat{\mu}_{c(i),g} + \widehat{\lambda}_{b(i),g} + \sum_{k=3}^{g} Z_{ik}'\widehat{\beta}_{kg}.$$

We assess the predictive performance of the index and calibrate its scale using older students with a linked parental divorce and observed adult earnings. For each terminal grade, we estimate

$$L_i = a_g + \rho_g \widetilde{P}_{ig} + u_{ig},$$

where $\widetilde{P}_{ig}$ is the raw prediction obtained from Equation (3). For the model incorporating the full set of educational outcomes, the raw prediction explains 14.4% of the variation in realized adult log earnings. The estimated calibration coefficient is 0.89, indicating that differences in the raw prediction somewhat overstate the corresponding

differences in realized log earnings.

We therefore define the calibrated index as

$$P_{ig} = \widehat{\rho}_g \widetilde{P}_{ig}.$$

This rescaling ensures that a one-log-point difference in the calibrated index corresponds to a one-log-point difference in realized earnings in the calibration regression. We do not add the calibration intercept to the index because it cancels in the within-student changes and fixed-effects comparisons used in our analysis. Thus, the index is calibrated to be forecast-unbiased in changes, which is the relevant scale for our event-study and treatment-effect-variance analyses.

The index should be interpreted as a cumulative measure of the earnings-relevant information contained in a student's educational record rather than as a measure of the student's current educational condition. When a student advances from grade g to grade $g+1$, the new prediction incorporates the additional test scores, attendance, discipline, and classification records observed in grade $g+1$, while retaining the student's earlier outcomes. The prediction coefficients are re-estimated for each terminal grade rather than holding the earlier weights fixed. Consequently, a disruption in an earlier grade may continue to reduce the predicted-earnings index even if the student's current test scores or behavior subsequently recover.

We also construct domain-specific versions of the index. The cognitive index uses math and reading scores, while the behavioral index uses absences and disciplinary outcomes. The cognitive prediction explains 12.3% of the variation in realized adult log earnings and has a calibration coefficient of 0.96, indicating that it is already close to the scale of realized earnings. The behavioral prediction explains 10.6% of the variation in realized earnings and has a calibration coefficient of 1.12, indicating that differences in the raw behavioral prediction understate the corresponding differences in realized earnings. We multiply each raw component by its own calibration coefficient before using it as an outcome so that the index is calibrated to be forecast-unbiased in changes.[16]

Figure 4 presents the event-study estimates for the full predicted-earnings index. The adjusted pre-event coefficients are close to zero. Predicted log earnings decline by 0.0014 log points in the imputed separation year, 0.0057 at $e = 1$, 0.0096 at $e = 2$, 0.0101 at $e = 3$, and 0.0094 at $e = 4$. All five estimates are statistically significant. The estimates from $e = 2$ through $e = 4$ correspond to reductions in predicted adult earnings of approximately 1%.

These estimates describe the effect of parental separation on predicted, rather than realized, adult earnings. They may understate the long-run effect if parental separation affects earnings-relevant dimensions—such as mental health, family cohesion, or socioemotional skills—that are not captured in the educational records used to construct the index. Conversely, they may overstate the long-run effect if the divorce-induced changes in observed school outcomes are more transitory or less consequential for adult earnings than the cross-sectional relationships used to weight them, or if they are offset by improvements in unmeasured dimensions. The predicted-earnings

[16] We also construct an "other outcomes" prediction using grade retention and participation in special education, LEP, and gifted programs. This prediction explains 7.3% of the variation in realized adult log earnings and has a calibration coefficient of 1.08.

estimates should therefore be interpreted as an economically scaled summary of the observed educational changes, not as a direct estimate of the causal effect of parental separation on realized adult earnings.

The negative average effect on predicted earnings does not reveal whether the losses are broadly shared or concentrated among a subset of children. The same average could arise if nearly every child experiences a modest decline or if large losses for some children are offset by gains for others. We therefore estimate the variance of individual treatment effects and, under the distributional assumptions developed there, the share of children with nonnegative effects. We then apply the same analysis to the cognitive and behavioral indices to examine which dimensions of children's educational trajectories account for the heterogeneity in predicted-earnings effects.

4.6 Economic Disadvantage and School Mobility

We next examine changes in children's economic circumstances and school environments that accompany parental separation. Appendix Figure A.6 presents event-study estimates for free or reduced-price lunch (FRL) status, school moves, and several measures of school quality.

We first consider FRL status as a measure of household economic resources. The probability that a student is recorded as FRL eligible increases by 0.5 percentage points in the imputed separation year and by 2.2 percentage points at $e = 1$. The increase reaches 5.5 percentage points at $e = 2$ and remains between 5.1 and 5.6 percentage points through $e = 4$. Relative to the comparison-group mean of 32.2%, the estimates from $e = 2$ through $e = 4$ represent an increase of approximately 16–17%. This pattern is consistent with a decline in household economic resources following separation.

School mobility also increases following separation. The probability that a student attends a different campus than in the preceding grade rises by 1.0 percentage point in the imputed separation year, 1.4 percentage points at $e = 1$, and 3.1 percentage points at $e = 2$. The effect subsequently reaches approximately 5 percentage points at $e = 3$ and $e = 4$, or about 15% of the comparison-group mean of 34.4%. Most of these additional campus changes are not moves to a nearby school. The majority involve a change of district, and about half involve a change of county. This pattern indicates greater instability in children's school environments following separation, driven largely by residential moves rather than reassignment within a district, consistent with evidence that divorce increases residential mobility among families (Johnston et al., 2025).

The schools children move to are not worse on achievement, but they are worse on behavior. Event-study estimates are close to zero and statistically insignificant for both the average achievement level of the student's campus and a campus value-added measure based on achievement growth. By contrast, children shift toward campuses with higher value-added in disciplinary incidents and absences, meaning campuses where students accumulate more infractions and miss more school than their prior records would predict.[17] Thus, parental separation increases the frequency of school transitions and, while leaving the measured academic quality of children's

[17]The campus mean-achievement measure is estimated from campus fixed effects in a regression of combined math and reading scores, disciplinary incidents, or absences that also absorbs grade and year fixed effects. The value-added measure is based on campus fixed effects from a regression of current combined achievement on a fifth-order polynomial in lagged achievement. Both measures are constructed using a leave-out sample.

schools unchanged, moves some children into environments with more discipline and absence.

School moves may nevertheless matter for children's outcomes, both through the disruption of routines and relationships with teachers and peers and through the shift toward campuses with more discipline and absence.To gauge how much of the divorce effect runs through moves, we re-estimate the event study for predicted earnings adding controls for non-structural moves, that is, changes of campus that are not part of the normal progression from one school level to the next, such as the transition from elementary to middle school. We include these first as simple indicators for whether the child moved in each year and then interacted with grade to allow the consequences of a move to differ by age. Appendix Figure A.7 shows that the estimates barely change. Controlling for moves shrinks the post-separation decline in predicted earnings by roughly a tenth to a fifth, with the largest attenuation at the longest horizons, and the estimates with and without controls remain well within each other's confidence intervals. Non-structural school moves are thus a visible consequence of separation but account for only a modest share of its effect on children.

4.7 Robustness

The qualitative patterns are stable across many alternative control groups, sample definitions, linkage restrictions, and assumptions about treatment timing, although the degree of robustness differs across outcomes. Because we have many outcomes, we discuss the robustness of the predicted earnings outcome in detail here and present the corresponding sensitivity analyses for other outcomes including test scores, absences, and the likelihood of every being disciplined in Appendix Figures B.2–B.5..

For predicted wages, the estimates are very stable. Across the full set of specifications, the estimated effect at $e = 2$ ranges from approximately -0.008 to -0.013 log points, and every estimate is statistically significant. Among the remaining outcomes, we find that the decline in math achievement and the increases in absences and disciplinary incidents are the most stable findings. The magnitude of the reading effect is more sensitive to sample composition and trend adjustment.

Alternative Control Groups. Panel (a) of Appendix Figure B.4 compares the baseline control group, consisting of students whose parents separate in grades 9–12, with three alternatives. Because children in our comparison group could become treated as early as 9th grade, they may already be on a pre-trend. We therefore test whether pushing the comparison group's separations further out affects our estimates. Restricting the comparison group to separations in grades 10–12 or shifting it later to grades 11–14 produces estimates similar to the baseline. In every period, the estimated coefficients are nearly identical to the baseline under either alternative.

We also construct a one-to-one matched comparison group using students whose parents separate in grades 9–12. Matching is exact on sex, race, grade-3 free- or reduced-price-lunch status, and deciles of grade-3 math and reading achievement.[18] The matched estimates preserve the same dynamic pattern but are less precise because of the smaller sample. The point estimates are somewhat smaller in magnitude than the baseline, yet remain highly

[18] Within matching strata containing unequal numbers of early- and later-treated students, excess observations are randomly removed. Each retained early-treated student is then paired with one later-treated student, and the pair is assigned the early-treated student's separation grade.

statistically significant in every post-event period. These results indicate that our main findings are not specific to the later-treated comparison group. Because the gap between the matched and baseline estimates is not statistically significant, however, it does not by itself establish that the baseline overstates the effect.

Sample Restrictions. Panel (b) of Appendix Figure B.4 varies the baseline sample restrictions by separately including students who receive special education services, including LEP students, allowing students with incomplete test-score histories, excluding grade repeaters, and including all students observed in the TEA data throughout grades 3–8. The results are highly stable: the estimated effect at $e = 2$ ranges from approximately -0.0088 to -0.0097 log points across these samples.

Covariates, Treatment Timing, and Trend Adjustment. Panel (c) of Appendix B.4 varies the set of controls. Adding grade-by-demographic and grade-by-repetition-history fixed effects produces almost no change in the estimates. We also vary the assumed separation-to-finalization interval from the baseline of 21 months to 15, 18, or 24 months in panel (d). All post-divorce estimates remain negative under every timing assumption. Changing the assumed separation date primarily shifts the event-time location of the largest effects and the apparent timing of recovery. In particular, the later persistence of the absence effect is sensitive to this choice. Finally, the estimate is -0.0085 in panel (e) when we allow separate pre-trends by treatment-cohort stack and -0.0124 without detrending.

Record-Linkage Criteria. Because the educational and divorce records lack a common unique identifier, the analysis relies on probabilistic linkage (see Appendix B for more details). Panel (f) of Appendix Figure B.4 compares the baseline linkage against a stricter and a looser alternative. The baseline keeps matches that agree on names together with either the marriage date or birth years, and it treats names flexibly, allowing nickname corrections, phonetic matches, and names with or without a middle initial. The stricter alternative demands that every date align and that names match exactly, with no phonetic corrections. The looser alternative removes the date requirement altogether, matching on names alone. The negative effects persist across all three, and the similarity of the estimates suggests that our results are not driven by the choice of linkage rule. As we discuss in more detail in Appendix B, the combination of both spouses' names with the marriage or divorce dates is sufficiently unique that even our most aggressive matches rarely link couples who are not in fact the same.

Selective Attrition. Appendix Figure A.5 examines whether the availability of educational records changes around parental separation. Among students observed in the TEA data throughout grades 3–8, we find no statistically detectable change in whether math or reading scores are recorded in a given year. This means that, conditional on being enrolled, children whose parents divorce are no less likely to be present on the day of the exam. This does not rule out the possibility that the composition of enrolled students is changing.

To address this possibility, we separately examine whether students are absent from the Texas public-school records altogether. Relative to $e = -1$, the probability of a missing TEA record falls by 0.16 percentage points at $e = 0$, 0.40 percentage points at $e = 1$, and 0.35 percentage points at $e = 2$. It is statistically unchanged at $e = 3$ and rises by 1.09 percentage points at $e = 4$. The latter estimate represents an increase of approximately

16% relative to the comparison-group mean of 6.9%. Missing TEA records may reflect movement out of Texas, enrollment in private school, homeschooling, or other departures from the state public-school system, which cannot be distinguished in the administrative data. Importantly, the increase emerges after the largest achievement effects at $e = 1$ and $e = 2$. The late difference nevertheless warrants caution when interpreting estimates at $e = 4$.

To assess whether this differential attrition explains our results, we implement Lee (2009) bounds. Specifically, at each event time we trim the group with less attrition down to the other group's retention rate, dropping either the highest or the lowest values of the outcome. Appendix Table A.2 presents the results. Even under this conservative approach assuming maximal selection, negative divorce effects persist, confirming that attrition cannot explain our findings.[19]

5 Heterogeneity in Divorce Effects

Average effects, whether overall or by subgroup, can mask important heterogeneity in how children respond to divorce. While we find that divorce reduces student achievement on average, the effect likely varies.

We estimate the variance of the effect of parental separation across children using a difference-in-variances event-study design. The intuition is straightforward. If separation affected every child by the same amount, it would shift the mean of the outcome distribution but would not increase its residual variance. If its effects differ across children, outcomes among children exposed should become more dispersed relative to outcomes among the comparison group. We use this change in relative dispersion to recover the variance of individual treatment effects.

Our primary outcome is the predicted adult-earnings index constructed in Section 4.5. Its continuous and economically interpretable scale is particularly useful here because the method relies on changes in the first two moments of the outcome distribution, though we extend the method to the third and fourth moment in the Appendix. We also apply the method separately to the cognitive and behavioral components of the index to better understand the source of divorce effect heterogeneity.

This estimand differs from more commonly used measures of treatment-effect heterogeneity. Quantile treatment effects characterize how treatment changes different points of the marginal outcome distribution, but generally do not identify the distribution of individual treatment effects (Bitler et al., 2006). Methods that predict conditional average treatment effects identify heterogeneity associated with observed characteristics (Athey and Imbens, 2016; Wager and Athey, 2018). Our objective is instead to recover the total variance of individual effects, including heterogeneity that is not systematically related to observed characteristics. The method does not require rank invariance or assumptions about how children retain their positions in the outcome distribution. It does,

[19] Appendix Table A.3 presents the same Lee (2009) bounds for math and reading scores. When we assume attrition is driven by the lowest achievers, the coefficients are negative and statistically significant at every post-divorce event time. When we instead assume attrition is driven by the highest-achieving students, the coefficients remain negative but are smaller in magnitude and are statistically indistinguishable from zero at several event times, turning positive (though only marginally significant) at $e = 4$. Because this requires the most extreme selection assumption, we view the $e = 4$ bound as a boundary case rather than evidence that the true effect is zero, though it does mean a null effect cannot be fully excluded at that horizon.

however, require restrictions on the variance of untreated outcome changes and their covariance with individual treatment effects.

5.1 Methodology for Recovering Divorce Effect Heterogeneity

Changes in outcomes around the time of divorce are due to two primary sources of variation. First, there is random variation in outcomes (which we denote as the random variable R). This includes factors such as teacher assignments, changes in the classroom environment, and luck associated with test performance. Second, there may be variation in the causal effect of divorce across children (denoted by D). While divorce reduces outcomes on average, some divorces may be worse than others and some may actually improve outcomes—potentially when dissolving high-conflict or abusive marriages.

We begin by calculating the change in student outcomes around the time of divorce for each student i:

$$d_{i,s} = A_{i,s} - A_{i,pre} \tag{4}$$

where $d_{i,s}$ represents the change in predicted-log-earnings index from the before separation to s years after separation for student i, where the academic predicted adult log income is denoted by A.

For a child whose parents separate earlier, the change in predicted earnings can be written as

$$d_{i,s} = R_{i,s} + D_{i,s} \tag{5}$$

where R_{ig} denotes the change that would have occurred in the absence of parental separation and D_{ig} denotes the child's individual treatment effect. For a later-treated comparison child whose parents have not yet separated, we can recover a measure of the changes that occur in the absence of separation which we can use as a proxy for $R_{i,s}$ in the treatment group.

The component R includes all variation in educational trajectories that would have occurred without separation. It therefore includes ordinary changes in achievement, attendance, and behavior, as well as other shocks and measurement variation. By contrast, any change caused by parental separation—including changes operating through family income, parental time, residential mobility, or school responses—is part of D.

Using the additivity rule for variance, we can decompose the changes that take place around divorce as:

$$\text{Var}(d_{i,s}) = \text{Var}(R + D) = \text{Var}(R) + \text{Var}(D) + 2\text{Cov}(R, D) \tag{6}$$

The distinction between D and R is causal. The component D includes any change caused by divorce, including effects operating through household income, parenting, residential mobility, school changes, or a child's response to other circumstances. The component R captures the outcome changes that would have occurred in the absence of divorce, including ordinary educational fluctuations and shocks unrelated to family dissolution. Thus,

a move or teacher reassignment triggered by divorce is part of D, whereas the same event occurring independently of divorce is part of R.[20]

Our identification strategy builds on a simple intuition. If divorce affects all children in the same way, divorce should shifts the mean but not the variance in outcomes. Heterogeneous effects, by contrast, generate additional dispersion among children whose parents separate. We can measure the excess variance by comparing the variance of outcome changes among treated children to that of the control children. Under the two assumptions stated below, this excess variance identifies $\text{Var}(D)$.

Assumption 1 (Independence):

The outcome change that would have occurred in the absence of divorce is independent of the individual effect of divorce: $R \perp D$.

This assumption rules out a systematic relationship between the outcome changes children would have experienced absent divorce and the effects divorce has on them. It would be violated, for example, if children who are more adversely affected by divorce were also systematically more likely to experience different teacher or classroom shocks for reasons unrelated to the divorce. It does not rule out changes in educational circumstances caused by divorce, since those changes are themselves part of D.

Under Assumption 1, $\text{Cov}(R, D) = 0$, so the observed variance in outcome changes can be decomposed as:

$$\text{Var}(d_{i,s}) = \text{Var}(R + D) = \text{Var}(R) + \text{Var}(D). \tag{7}$$

Without independence, the decomposition would include a covariance term: $Var(R + D) = Var(R) + Var(D) + 2Cov(R, D)$. This assumption is not directly testable (we cannot observe counterfactual outcomes) but it is plausible if institutional factors generating random variation (classroom assignment, curriculum changes) operate independently of child-specific vulnerability to family disruption. Below, we present an indirect diagnostic based on whether divorce effects vary with pre-divorce outcome changes.

This gives us $\text{Var}(d_{i,s}) = \text{Var}(R) + \text{Var}(D)$, but we still cannot separately observe the two variance components. For divorcing families, we observe $d_{i,s} = R + D$—a mixture of random fluctuation and causal effect. To isolate $\text{Var}(D)$, we need an independent estimate of $\text{Var}(R)$. We obtain this from a sample of intact families who experience the same random shocks R but no divorce effect ($D = 0$). This strategy requires our second assumption.

Assumption 2 (Common Random Variation):

The variance of outcome fluctuations is the same for divorcing families and intact comparison families: that is, $\text{Var}(R_{divorce}) = \text{Var}(R_{intact})$ *for the chosen control set.*

[20] If divorce changes a child's susceptibility to an otherwise unrelated shock, the resulting change in the child's response is also part of D.

This assumption is analogous to the parallel trends assumption in standard difference-in-differences designs. While parallel trends requires that treated and control groups would have experienced the same change in mean outcomes absent treatment, our assumption requires they would have experienced the same variance in outcome changes absent treatment. Just as violations of parallel trends in means would bias estimates of average treatment effects, violations of common random variation would bias our estimates of treatment effect heterogeneity. Although the post-treatment counterfactual cannot be observed, Assumption 2 has testable implications in pre-treatment data.

We assess Assumption 2 by comparing the variance in outcome changes in the treatment and control groups before divorce. If the two groups have similar pre-divorce variance, control-group variance may provide a good proxy for the counterfactual variance of the treatment group. The estimated pre-divorce excess variance among treated children is small and statistically indistinguishable from zero, supporting the plausibility of common random variation.

$$r_{i,s} = A_{i,s} - A_{i,pre} \tag{8}$$

where s denotes event time relative to the imputed separation grade defining the stack. Under Assumption 2, $\text{Var}(r_{i,s})$ identifies the counterfactual variance $\text{Var}(R)$ for divorcing families. Combining Assumptions 1 and 2, our estimator for the variance of divorce effects is thus:

$$\widehat{\text{Var}(D_s)} = \text{Var}(d_{i,s}) - \text{Var}(r_{i,s}) \tag{9}$$

5.2 Implementation of the Variance Estimator

We implement the variance estimator using the predicted-log-earnings index. In practice, we measure each student's outcome change relative to grade 3:

$$\Delta A_{ig} = A_{ig} - A_{i3}. \tag{10}$$

We then organize these changes by event time relative to the imputed separation grade. Because grade 3 supplies the baseline and we require at least one subsequent pre-separation observation, the variance analysis includes children whose imputed parental separation occurs in grades 5–8. The grade-4 treatment cohort is therefore excluded. As in the average-effect analysis, children whose parents remain intact through grade 8 but divorce in grades 9–12 form the comparison group. The same comparison children appear in each treatment stack, with event time defined relative to the separation grade for that stack.

We estimate the variance of treatment effects in two stages. First, we estimate the stacked event-study average-effect specification using ΔA_{ig} as the outcome. This regression includes treatment-group-by-stack, grade-by-stack, and calendar-year-by-stack fixed effects and removes an estimated differential linear pre-trend between

the treatment and comparison groups. The event-time coefficients capture the average effect of divorce, while the residuals capture variation around the estimated mean outcomes. Because the outcome has already been differenced from grade 3, this stage does not include student fixed effects.

Second, we square the residuals from the first-stage regression and use the squares as the outcome variable in another stacked event study. The residuals measure how far each child's outcome lies from the estimated group mean. Because the residuals are centered at zero, their average squared value is the residual variance. The second-stage regression therefore tests whether outcome dispersion increases among treated children relative to comparison children after divorce.

The resulting specification is a difference-in-differences event study of residual variance. Under Assumptions 1 and 2, the coefficient at event time s provides an estimate of $\mathrm{Var}(D_s)$. We estimate this variance separately for each post-separation year, allowing treatment-effect heterogeneity to evolve over time.

This regression includes student-by-stack, grade-by-stack, and calendar-year-by-stack fixed effects and applies the same linear pre-trend adjustment we used in our average-effect analysis. The event-time coefficients measure the excess residual variance among treated children relative to comparison children. Under Assumptions 1 and 2, these coefficients identify the variance of individual divorce effects.

We omit student fixed effects when constructing the first-stage residuals because fixed effects estimated using both pre- and post-divorce observations would absorb part of any persistent child-specific divorce effect, mechanically attenuating the estimated treatment-effect variance.[21] Instead, we difference outcomes from their grade-3 values, removing permanent level differences using a pre-divorce baseline while preserving treatment-effect heterogeneity in the residuals.

The grade-3 baseline and the event-study reference period serve different purposes. Differencing from grade 3 removes persistent differences in outcome levels. The variance event study then uses $s = -1$ as its omitted period, so each coefficient measures the change in excess residual variance relative to the year before the imputed separation, net of the estimated pre-trend.

Our baseline estimator captures total treatment-effect heterogeneity, including heterogeneity associated with observable characteristics. We therefore do not remove group differences by sex, race and ethnicity, or free and reduced-price lunch status in the baseline specification. As a robustness check, we allow mean outcome changes to vary flexibly across these groups before constructing the squared residuals.

We assess the common-random-variation assumption by examining the pre-divorce coefficients from the squared-residual event study. Because the baseline estimates remove a differential linear pre-trend, we also report undetrended estimates as a more direct diagnostic of pre-existing differences in variance. As an indirect diagnostic of Assumption 1, we also compare divorce effects between children whose predicted-earnings index rose and fell between grades 3 and 4.

Finally, because comparison children appear in multiple treatment stacks, we cluster standard errors by

[21] Monte Carlo simulations confirm this pattern: differencing recovers the true treatment-effect variance, whereas the student-fixed-effects specification understates it.

student. This accounts for dependence across observations for the same child and across the stacks in which that child appears.

5.3 Variance in the Effects of Divorce

Figure 5 presents the event-time estimates of the average treatment effect and the variance of individual treatment effects on predicted log earnings. Panel (a) of Table 4 reports the corresponding coefficients. After removing the estimated differential linear pre-trend, the pre-event variance coefficients are small and statistically indistinguishable from zero. In the imputed separation year, the estimated variance of treatment effects is 0.00022 (standard error 0.00013), which is not statistically distinguishable from zero. It subsequently rises to 0.00079 at $s = 1$, 0.00126 at $s = 2$, and 0.00185 at $s = 3$. Each of these three post-separation estimates is statistically significant at the 1% level.

Because variances are measured in squared log points, their square roots provide a more intuitive measure of the magnitude of heterogeneity. The estimates imply that the standard deviation of individual effects on predicted log earnings increases from approximately 0.028 log points at $s = 1$ to 0.036 at $s = 2$ and 0.043 at $s = 3$. Thus, three years after the imputed separation, individual divorce effects have an estimated standard deviation of approximately 4.3% of predicted earnings. This dispersion is substantial relative to the average effect in the same sample, which is a decline of approximately 1.1%.

Figure 6 applies the same estimator separately to the cognitive and behavioral predicted-earnings indices. Both components decline on average following parental separation, although the decline in the behavioral index is larger. The variance results differ more sharply across the two domains. For the cognitive index, the estimated treatment-effect variance remains relatively small and is statistically indistinguishable from zero in every post-separation year. Even when pooling post-divorce time periods, we find no statistically detectable variance in the effects of divorce on the cognitive component (Appendix Table A.4).

In contrast, the variance of effects on the behavioral index increases substantially following separation. It rises to approximately 0.0007 at $s = 1$, 0.0010 at $s = 2$, and 0.0016 at $s = 3$, with each estimate statistically distinguishable from zero. These estimates correspond to standard deviations of approximately 0.027, 0.032, and 0.040 log points. The domain-specific estimates therefore indicate that the detectable heterogeneity in predicted-earnings effects is concentrated primarily in children's attendance and disciplinary outcomes rather than their test scores.[22]

The positive treatment-effect variance establishes that the negative average effect is not shared uniformly across children. The variance alone, however, does not determine how many children experience gains rather than losses. Answering that question requires combining the estimated mean and variance with an assumption about the shape of the treatment-effect distribution, to which we turn next.

[22] Because the cognitive and behavioral indices are separately calibrated to realized earnings, these estimates are not an exact additive decomposition of the variance of the full index. They instead show how much treatment-effect heterogeneity is detected when earnings are predicted from each domain separately.

5.4 What Fraction of Children Are Not Harmed by Divorce?

What fraction of children are not harmed by divorce, based on our predicted-earnings metric? We define a child as not harmed if the effect of divorce on the child's predicted-log-earnings index is non-negative, $D_{i,s} \geq 0$.[23]

The preceding analysis identifies the mean and variance of the treatment-effect distribution at each event time:

$$\mu_s = \mathrm{E}(D_{i,s}), \qquad \sigma^2_{D,s} = \mathrm{Var}(D_{i,s}). \tag{11}$$

Neither estimate requires treatment effects to be normally distributed. The mean identifies the average treatment effect, while the variance measures how widely individual effects differ around that average. These two moments alone, however, do not identify the fraction of children for whom $D_{i,s} \geq 0$. Distributions with the same mean and variance can place different shares of their mass above zero.

To calculate this fraction, we use normality as our baseline distributional assumption:

Assumption 3 (Normality):

At each event time s, individual divorce effects on the predicted-earnings index are normally distributed:

$$D_{i,s} \sim \mathcal{N}\left(\mu_s, \sigma^2_{D,s}\right). \tag{12}$$

We assess two implications of this assumption using higher moments of the treatment-effect distribution. A normal distribution has skewness equal to zero and kurtosis equal to three, or, equivalently, third and fourth cumulants equal to zero. We apply the same moment-recovery approach used to estimate treatment-effect variance to recover the third and fourth cumulants. This exercise extends the common-random-variation assumption to require that, absent divorce, the third and fourth cumulants of outcome changes would have evolved similarly in the treatment and comparison groups.

The estimated third- and fourth-cumulant effects are each statistically indistinguishable from zero.[24] We therefore find no detectable skewness or excess kurtosis attributable to divorce. These results are consistent with normality in the third and fourth moments, but they do not establish that treatment effects are normally distributed. Higher moments are estimated imprecisely, and nonnormal distributions can have both zero skewness and kurtosis equal to three.

The implied point estimate of treatment-effect skewness is -2.0, but it is estimated imprecisely, with a 95% confidence interval of $[-19.7, 8.6]$. We therefore do not interpret the point estimate as evidence that the treatment-effect distribution is necessarily left-skewed. The estimates are consistent with symmetry but also with substantial departures from it.

[23] This definition concerns only the predicted-earnings index. It does not imply that the child experiences no adverse effect on any individual educational outcome or that divorce does not reduce well-being along dimensions we do not observe.

[24] We test the third- and fourth-cumulant restrictions separately rather than conducting a joint test of normality.

Under Assumption 3, the fraction of children not harmed at event time s is

$$\Pr(D_{i,s} \geq 0) = 1 - \Phi\left(\frac{-\mu_s}{\sigma_{D,s}}\right) = \Phi\left(\frac{\mu_s}{\sigma_{D,s}}\right), \tag{13}$$

where $\Phi(\cdot)$ is the standard normal cumulative distribution function. At event times $s = 1$, $s = 2$, and $s = 3$, the estimates in Table 4 imply that approximately 41%, 38%, and 40% of children, respectively, experience a non-negative effect on predicted earnings. By symmetry of the normal distribution, the same shares experience predicted-earnings losses at least twice as large as the average loss. Thus, the negative average effect masks substantial dispersion in children's responses to divorce.

Because Equation (13) depends on the shape of the treatment-effect distribution, Table Table A.5 examines the sensitivity of the estimated share to alternative assumptions about skewness and kurtosis. Holding the estimated mean and variance at $s = 2$ fixed, we vary skewness across the columns and Pearson kurtosis across the rows. For each admissible combination, we select the continuous Johnson distribution that matches these four moments and calculate the share of its probability mass at or above zero. The displayed grid should therefore be interpreted as a sensitivity analysis over selected distributional shapes, not as a confidence region for the share of affected children whose predicted earnings do not decline from divorce.

Not every combination of skewness and kurtosis can characterize a valid distribution. Pearson's moment inequality requires $\kappa \geq 1 + \gamma^2$, where γ denotes skewness and κ denotes Pearson kurtosis. Equality is attained only by a distribution supported on two points, so we restrict the calculations to $\kappa > 1 + \gamma^2$, and we leave blank combinations that violate this restriction. Because the first four moments do not uniquely determine a CDF, selecting a moment-matched Johnson distribution constitutes an additional shape assumption.

At the skewness point estimate of -2, the implied share not harmed ranges from approximately 38% to 65% across the non-degenerate kurtosis values shown in the table. The estimates vary especially widely for distributions near the two-point boundary. Thus, the evidence that divorce effects vary across children does not depend on a distributional assumption, but the precise fraction of children experiencing a non-negative effect does.

5.5 Sensitivity to the Independence Assumption

Assumption 1 cannot be tested directly because R and D are not separately observed. We therefore examine how the estimated treatment-effect variance changes when the two components are correlated. Let $\rho = \mathrm{Corr}(R, D)$ and define the observed excess variance as

$$\Delta V_s = \mathrm{Var}(d_{i,s}) - \mathrm{Var}(r_{i,s}). \tag{14}$$

The treatment-effect standard deviation implied by a given value of ρ is

$$\sigma_{D,s}(\rho) = \sqrt{\Delta V_s + \rho^2 \sigma_{R,s}^2} - \rho \sigma_{R,s}. \tag{15}$$

Positive values of ρ attribute part of the observed excess variance to covariance between background outcome changes and divorce effects, reducing the implied treatment-effect variance. Negative values increase the implied variance. Appendix **??** reports estimates across a range of values for ρ.

We can also test the most plausible form of dependence directly. The concern is that children who would have declined anyway are the ones most affected by divorce, or conversely that divorce hits hardest those who would otherwise have improved. Although R is unobserved, part of it is predictable from a child's pre-separation trajectory. We therefore split children by whether their outcomes were trending upward or downward before separation and estimate the effect of divorce separately for each group. If D were correlated with R, average effects should differ across the two groups. Appendix Figure A.8 shows that they do not. The estimated effects are similar for children on positive and negative pre-separation trajectories, which is reassuring.

5.6 Heterogeneity Across Observable Characteristics

We next examine whether the average effect of parental separation varies with children's observable characteristics. We consider sex, race and ethnicity, free and reduced-price lunch (FRL) status, baseline achievement, and birth order. We use the predicted-log-earnings index as our primary summary outcome and its cognitive and behavioral components to examine the sources of any differences. Appendix Figures A.9–A.12 present the corresponding event studies.[25] Table 5 reports p-values that test, at each event year, whether the coefficients differ across groups.

Sex. Figure A.9 shows negative predicted-earnings effects for both boys and girls. The point estimates are consistently more negative for boys after separation, and the gap widens over time. At $e = 3$, predicted log earnings decline by 0.0119 log points for boys and 0.0084 for girls. At $e = 4$, the corresponding estimates are 0.0120 and 0.0069 log points. The confidence often intervals overlap, however, and only become statistically significantly different toward the end of the observation window.

The difference in predicted earnings is concentrated in the behavioral component. At $e = 3$, the behavioral prediction declines by 0.0102 log points for boys and 0.0064 for girls, whereas the cognitive declines are much more similar, at 0.0033 and 0.0028 log points. Math and reading effects also do not exhibit a consistent sex gradient. The results therefore suggest that boys may experience larger behavioral consequences of parental separation, consistent with evidence that boys are particularly sensitive to childhood disadvantage (Autor et al., 2019; Chetty et al., 2016).

Race and ethnicity. Figure A.10 provides little evidence of a systematic racial differences in the overall predicted-earnings effect, consistent with Johnston et al. (2025). At $e = 2$, predicted log earnings decline by 0.0108 log points among White children, 0.0086 among Black children, and 0.0098 among Hispanic children. The estimates remain similar through $e = 3$. The Black estimate becomes more negative at $e = 4$, but it is considerably less precise.

[25] The figures report estimates from separate regressions within each subgroup.

The underlying educational outcomes nevertheless differ across groups. The math decline is concentrated among White children: at $e = 2$, math scores fall by 0.035 standard deviations among White children, compared with point estimates of 0.014 among Black children and -0.007 among Hispanic children. In contrast, the behavioral predicted-earnings component declines by similar amounts across all three groups.

Economic disadvantage. Figure A.11 similarly shows little difference in the overall predicted-earnings effect by grade-3 FRL status. At $e = 2$, predicted log earnings decline by 0.0097 log points among children who were not eligible for FRL and by 0.0088 among eligible children. At $e = 3$, the corresponding estimates are 0.0101 and 0.0091.

Some individual outcomes exhibit a socioeconomic gradient even though the summary index does not. At $e = 2$, math scores fall by 0.030 standard deviations among non-FRL students but by only 0.005 among FRL students. Absences also increase more among non-FRL students at this event time. These differences, however, do not generate a corresponding significant difference in predicted earnings.

Baseline achievement. The clearest observable differences arise across terciles of grade-3 achievement. As shown in Figure A.12, the predicted-earnings decline is substantially smaller among students in the highest tercile, meaning high performers are less negatively affected. At $e = 2$, predicted log earnings decline by 0.0112 log points in the bottom tercile and 0.0124 in the middle tercile, compared with 0.0057 in the top tercile. At $e = 3$, the corresponding estimates are 0.0115, 0.0126, and 0.0064 log points.

These differences are driven primarily by behavioral outcomes. At $e = 3$, the behavioral predicted-earnings component declines by 0.0092 log points in the bottom tercile and 0.0113 in the middle tercile, compared with 0.0035 in the top tercile. The cognitive component does not display a similarly pronounced or monotonic gradient. These results therefore suggest that initially higher-achieving students respond to parental separation with smaller changes in attendance and disciplinary behavior, rather than with smaller declines in achievement.

Birth order. Children's responses also differ by birth order. Figure A.13 shows that by $e = 2$, the effects are similar for youngest and older siblings, at approximately 0.011 log points, compared with 0.0084 for only children, but their subsequent trajectories diverge. Among youngest siblings, the decline continues to grow, reaching 0.0141 log points at $e = 3$ and 0.0157 at $e = 4$. The estimates for older siblings become somewhat less negative, reaching 0.0111 and 0.0099, while those for only children decline in magnitude to 0.0069 and 0.0042.

This divergence is concentrated primarily in the behavioral component of predicted earnings. At $e = 3$, the behavioral prediction declines by 0.0111 log points among youngest siblings, 0.0083 among older siblings, and 0.0055 among only children. At $e = 4$, the corresponding declines are 0.0127, 0.0061, and 0.0045 log points. Attendance accounts for an important part of this pattern: annual absences among youngest siblings remain approximately 0.55 days higher at $e = 3$ and 0.57 days higher at $e = 4$, while the estimated increases among older siblings and only children become substantially smaller. Cognitive effects also remain more negative among youngest siblings, although these differences are smaller and less precisely estimated. The results therefore suggest that younger children respond to parental separation with more persistent changes in attendance and

behavior.

Overall, race and economic disadvantage explain relatively little of the variation in average divorce effects across children. The estimates suggest a larger behavioral response among boys and show clearer differences by baseline achievement and birth order. Students in the bottom and middle achievement terciles experience larger predicted-earnings declines than those in the top tercile, while the effects become particularly persistent among the youngest siblings. Even these observable differences account for only part of the heterogeneity documented above.

Figure A.14 shows this more directly. It plots the estimated effect for 36 cells defined by sex, race, baseline achievement tercile, and lunch status, with 95 percent confidence intervals and markers scaled by the number of treated observations. The cell estimates confirm the patterns documented above. Estimates are more negative for boys, differ little by race or lunch status, and are larger for children in the bottom and middle achievement terciles. The largest cells cluster more closely around the overall mean effect of $-.005$, and the estimates that stray farthest from the mean mostly belong to the smallest cells, whose confidence intervals generally include it. To quantify how much of the heterogeneity these characteristics capture, we compute the weighted variance of the cell estimates around their mean, which is the variance of effects that would exist if every child within a cell were affected identically. It equals 0.00003, compared with 0.00051 for the variance of individual effects from our baseline approach, a share of 5.1 percent.[26] This suggests that observable characteristics, account for only a small fraction of how differently children respond to divorce, and that the variance we document arises largely from differences we cannot see, either in families or in children themselves.

5.7 Are Divorce Effects Shared Within Families?

One potential source of treatment-effect heterogeneity is variation across families in how the developmental environment changes after separation. Some divorces may replace relatively stable marriages with less stable post-separation arrangements, while others may remove children from conflict, violence, or other harmful circumstances. If this family-level component accounts for much of the heterogeneity in divorce effects, siblings exposed to the same separation should respond similarly, and observable characteristics of the pre-divorce family environment should predict the effects of separation.

We conduct two tests of this hypothesis. First, we ask whether siblings' outcomes become more closely correlated around the time of their parents' separation. Siblings' educational outcomes are correlated even in the absence of divorce because they share genes, resources, neighborhoods, and other family circumstances. A simple post-separation sibling correlation would therefore not isolate a common divorce effect. Instead, we test whether the association between siblings' residual outcomes increases following separation among treated families relative to the corresponding association among comparison families.

[26] We compute the mean of the 36 cell estimates, weighting each cell by its share of treated observations, and then the weighted variance of the cell estimates around that mean. The share reported in the text divides this between-cell variance by the variance of individual effects from our baseline approach, estimated on the collapsed pre/post specification for the full sample.

To implement this test, we identify pairs of siblings, selecting the two closest in age when more than two children are observed. We begin with the residuals from our stacked event-study regressions, which remove average differences associated with event time, grade, calendar year, and treatment cohort. We then regress each child's residual outcome on the contemporaneous residual of the child's sibling and estimate whether this relationship becomes stronger around parental separation, consistent with a shared divorce effect.[27] The event-time coefficients measure the excess sibling correlation associated with parental separation.

Table A.6 presents the results. We find no statistically detectable increase in sibling correlations following separation. The post-separation coefficients are small and statistically indistinguishable from zero for predicted earnings and for the underlying measures of math and reading achievement, attendance, discipline, and special education. The conclusion is similar across specifications that allow the baseline sibling relationship to vary with grade and sibling age differences. Notably, we find no evidence that one child's divorce-related outcome deviation predicts a corresponding deviation in their sibling's outcome.

Our second test asks whether an observable characteristic of the family environment predicts the effect of separation. We focus on parental criminal history as a potential marker of a less stable or more conflictual environment before divorce. Prior research consistently documents an association between parental criminal-justice involvement and child maltreatment. A systematic review of ten studies finds that children whose parents had prior criminal-justice involvement were more likely to experience maltreatment (Austin, 2016). In linked administrative data from North Carolina, children whose parents had no criminal conviction were 66% less likely to be reported to Child Protective Services than children whose parents had a substance-related conviction (Gifford et al., 2016). Longitudinal evidence also shows that individuals with offending histories are more likely than non-offenders to perpetrate psychological and physical intimate-partner violence (Verbruggen et al., 2022).

Thus, parental criminal involvement provides one potential marker of a family environment in which remaining married may be less beneficial for children. If separation reduces a child's exposure to instability or harmful parental behavior, we would expect the effects of divorce to be less negative, or potentially positive, among children whose parent has a criminal record.

Figure A.15 compares children according to whether their father has a recorded criminal offense. Among children whose fathers have no recorded offense, predicted log earnings decline by 0.0092 log points at $e = 2$ and 0.0087 at $e = 4$. Among children whose fathers have an offense record, the corresponding declines are 0.0159 and 0.0204 log points. The estimates for the latter group are substantially less precise, and the differences between the two groups are not statistically conclusive. Nevertheless, the point estimates provide no indication that children experience smaller predicted-earnings losses when their father has a criminal record. The same pattern holds when we focus on fathers with a violent criminal history. If anything, they point in the opposite direction.[28]

Together, these tests provide little evidence that divorce-effect heterogeneity is primarily shared within fam-

[27] The specification allows the ordinary sibling association to vary with grade and the age difference between siblings.

[28] Estimates using maternal criminal records show that divorces with these women have smaller negative coefficients, but these are not statistically significant differences either.

ilies: we detect neither correlations between siblings response to divorce nor smaller losses among children with a criminally involved parent. Although neither test is dispositive, the results are more consistent with differences in how children respond to family dissolution than with an effect shared by siblings. This interpretation is also largely consistent with the subgroup patterns. The clearest differences in the estimated effects are by baseline achievement and sibling position, with suggestive differences by sex—all characteristics that can vary within families—whereas effects are broadly similar across race and ethnicity and economic disadvantage.

6 Conclusion

This paper studies how parental divorce affects children's educational trajectories and how widely those effects differ across children. We link statewide administrative records on births, marriages, divorces, schooling, and adult earnings in Texas. Our stacked event-study design compares within-student changes among children whose parents separate in grades 4–8 with contemporaneous changes among children whose parents separate later, in grades 9–12.

Parental separation produces declines across several dimensions of children's schooling. Math and reading scores fall modestly, by approximately 0.02 student standard deviations at their largest decline. Absences increase by approximately 0.4 days per year at their peak, or about 8% relative to the comparison-group mean, while the probability of receiving a disciplinary action rises by approximately 2 percentage points, or 18%. Gifted participation declines by nearly 1 percentage point, reflecting both reduced entry and increased removal from gifted programs. When we combine the earnings-relevant information in these outcomes into a common index, predicted adult earnings decline by approximately 1% within two years of separation and remain lower through the end of our observation window. The index summarizes the implications of the observed educational changes and is not a direct estimate of the effect of divorce on realized adult earnings or well-being more generally.

Average effects alone do not reveal whether these losses are broadly shared or concentrated among a subset of children. We therefore develop a method for estimating treatment-effect variance by comparing the dispersion of within-child outcome changes among treated and later-treated comparison children. Under assumptions that counterfactual outcome fluctuations are independent of individual divorce effects and have the same variance in the two groups, the excess post-separation variance among treated children identifies the variance of divorce effects. The method complements conventional subgroup analysis by recovering heterogeneity both across and within observable groups.

Applying this method to the predicted-earnings index reveals substantial treatment-effect heterogeneity. The estimated variance is positive and statistically significant from the first through the third post-separation years and grows over this period. Under a normal treatment-effect distribution, the estimates imply that approximately 38% of children experience a non-negative effect on predicted earnings. By symmetry, a similar share experiences a loss at least twice as large as the average loss. The estimated third and fourth cumulants are individually

statistically indistinguishable from zero, although these tests have limited power to detect modest departures from normality. When we allow moderate variation in skewness and kurtosis, the implied share not harmed remains between approximately 35% and 45%. Thus, the precise fraction depends on distributional assumptions, but the conclusion that divorce effects vary substantially across children does not.

We find substantial treatment-effect variance in the behavioral predicted-earnings index (from attendance and discipline records) but no statistically detectable variance in the cognitive index constructed from math and reading scores. The subgroup estimates provide additional evidence about observable differences in responses. Effects vary little with family background, such as race, ethnicity and economic disadvantage, but more with characteristics of the child, with the clearest differences appearing across baseline achievement and sibling position and more suggestive differences by sex.

Finally, we examine whether the remaining heterogeneity is primarily shared within families. We find neither an increase in sibling outcome correlations following separation nor systematically smaller losses when a parent has a criminal record, as might be expected if such divorces were more likely to remove children from harmful family environments. While neither test is dispositive, these results are more consistent with differences in how individual children respond to family dissolution than with an effect shared by siblings. This interpretation is also consistent with the subgroup patterns: the most pronounced differences occur across characteristics that can vary within families, whereas effects are broadly similar across characteristics commonly shared by siblings.

The central implication is that a negative average effect does not describe every child's experience of divorce. Some children experience little predicted loss or a gain, while others experience losses substantially larger than the mean, and the observable family characteristics available in administrative data do little to reveal in advance which outcome will occur. More generally, the variance estimator developed here provides a way to study this otherwise hidden dimension of causal effects in settings where researchers observe repeated outcomes and a credible comparison group but cannot observe individual treatment effects directly.

References

Abramitzky, Ran, Leah Boustan, Katherine Eriksson, James Feigenbaum, and Santiago Pérez, "Automated Linking of Historical Data," *Journal of Economic Literature*, 2021, *59* (3), 865–918.

Amato, Paul R., "Children of Divorce in the 1990s: An Update of the Amato and Keith (1991) Meta-Analysis," *Journal of Family Psychology*, 2001, *15* (3), 355–370.

Amato, Paul R, "Research on divorce: Continuing trends and new developments," *Journal of Marriage and Family*, 2010, *72* (3), 650–666.

Amato, Paul R. and Bruce Keith, "Parental Divorce and the Well-Being of Children: A Meta-Analysis," *Psychological Bulletin*, 1991, *110* (1), 26–46.

Anderson, Lydia R, "Multiple transitions: The impact of family instability on child well-being," *Child Development*, 2014, *85* (3), 1241–1257.

Athey, Susan and Guido Imbens, "Recursive partitioning for heterogeneous causal effects," *Proceedings of the National Academy of Sciences*, 2016, *113* (27), 7353–7360.

Austin, Anna, "Is Prior Parental Criminal Justice Involvement Associated with Child Maltreatment? A Systematic Review," *Children and Youth Services Review*, 2016, *68*, 146–153.

Autor, David, David Figlio, Krzysztof Karbownik, Jeffrey Roth, and Melanie Wasserman, "Family Disadvantage and the Gender Gap in Behavioral and Educational Outcomes," *American Economic Journal: Applied Economics*, July 2019, *11* (3), 338–81.

Bieber, Christy and Adam Ramirez, "Revealing Divorce Statistics In 2023," *Forbes Advisor*, Aug 2023. Updated: Aug 8, 2023, 10:47am; Editorial Note: We earn a commission from partner links on Forbes Advisor. Commissions do not affect our editors' opinions or evaluations.

Bitler, Marianne P, Jonah B Gelbach, and Hilary W Hoynes, "What mean impacts miss: Distributional effects of welfare reform experiments," *American Economic Review*, 2006, *96* (4), 988–1012.

Chetty, Raj, John N Friedman, and Jonah E Rockoff, "Measuring the impacts of teachers II: Teacher value-added and student outcomes in adulthood," *American economic review*, 2014, *104* (9), 2633–79.

_ , Nathaniel Hendren, Frina Lin, Jeremy Majerovitz, and Benjamin Scuderi, "Childhood Environment and Gender Gaps in Adulthood," *American Economic Review*, May 2016, *106* (5), 282–88.

Cho, Hyunkuk, Paul Glewwe, and Melissa Whitler, "Do reductions in class size raise students' test scores? Evidence from population variation in Minnesota's elementary schools," *Economics of Education Review*, 2012, *31* (3), 77–95.

Conger, Rand D, Katherine J Conger, and Monica J Martin, "Understanding the processes through which economic hardship influences families and children," *Handbook of families and poverty*, 2010, pp. 64–81.

Freyaldenhoven, Simon, Christian Hansen, Jorge Pérez Pérez, and Jesse M. Shapiro, "Visualization, Identification, and Estimation in the Linear Panel Event-Study Design," Working Paper 29170, National Bureau of Economic Research 2021.

Frimmel, Wolfgang, Martin Halla, and Rudolf Winter-Ebmer, "How does parental divorce affect children's long-term outcomes?," *Journal of Public Economics*, 2024, *239*, 105201.

Gifford, Elizabeth J., Lindsey M. Eldred, Frank A. Sloan, and Kelly E. Evans, "Parental Criminal Justice Involvement and Children's Involvement with Child Protective Services: Do Adult Drug Treatment Courts Prevent Child Maltreatment?," *Substance Use & Misuse*, 2016, *51* (2), 179–192.

Grall, Timothy, "Custodial mothers and fathers and their child support: 2017," Current Population Reports P60-269, U.S. Census Bureau 2020.

Gruber, Jonathan, "Is making divorce easier bad for children? The long-run implications of unilateral divorce," *Journal of Labor Economics*, 2004, *22* (4), 799–833.

Heckman, James J, Jeffrey Smith, and Nancy Clements, "Making the most out of programme evaluations and social experiments: Accounting for heterogeneity in programme impacts," *Review of Economic Studies*, 1997, *64* (4), 487–535.

Hertegård, Edvin, "Divorce law reform, family stability, and children's long-term outcomes," 2024. Working paper.

Hetherington, E Mavis, *Coping with divorce, single parenting, and remarriage: A risk and resiliency perspective*, Mahwah, NJ: Lawrence Erlbaum Associates, 1999.

Hogan, Diane, Ann Marie Halpenny, and Sheila Greene, *Children's Experiences of Parental Separation*, Dublin, Ireland: Children's Research Centre, Trinity College Dublin, 2002.

Holm, Mathilde Lund, Peter Fallesen, and Eskil Heinesen, "The Effects of Parental Union Dissolution on Children's Test Scores," 2024. Working paper, Department of Economics, University of Copenhagen; ROCKWOOL Foundation; Swedish Institute for Social Research, Stockholm University.

Johnston, Andrew C, Maggie R Jones, and Nolan G Pope, "Divorce, Family Arrangements, and Children's Adult Outcomes," Working Paper 33776, National Bureau of Economic Research May 2025. Forthcoming or in progress, as per publication date.

Kelly, Joan B, "Risk and protective factors associated with child and adolescent adjustment following separation and divorce: Social science applications," *Parenting plan evaluations: Applied research for the family court*, 2012, pp. 49–84.

Kennedy, Sheela and Larry Bumpass, "Cohabitation and Children's Living Arrangements: New Estimates from the United States," *Demographic Research*, 2008, *19* (47), 1663–1692.

Laird, Jessica, Nick Fabrin Nielsen, and Torben Heien Nielsen, "Differential Effects of the Timing of Divorce on Children's Outcomes: Evidence from Denmark," 2023. Working paper.

Lee, David S., "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects," *The Review of Economic Studies*, 07 2009, *76* (3), 1071–1102.

Maccoby, Eleanor E and Robert H Mnookin, *Dividing the child: Social and legal dilemmas of custody*, Cambridge, MA: Harvard University Press, 1992.

Masarik, April S and Rand D Conger, "Stress and child development: A review of the Family Stress Model," *Current Opinion in Psychology*, 2017, *13*, 85–90.

McLanahan, Sara, Laura Tach, and Daniel Schneider, "The Causal Effect of Father Absence," *Annual review of sociology*, 2013, *39*, 399–427.

McLoyd, Vonnie C, "The impact of economic hardship on black families and children: Psychological distress, parenting, and socioemotional development," *Child Development*, 1990, *61* (2), 311–346.

Seltzer, Judith A, "Relationships between fathers and children who live apart: The father's role after separation," *Journal of Marriage and Family*, 1991, *53* (1), 79–101.

Stevenson, Betsey and Justin Wolfers, "Bargaining in the shadow of the law: Divorce laws and family distress," *The Quarterly Journal of Economics*, 2006, *121* (1), 267–288.

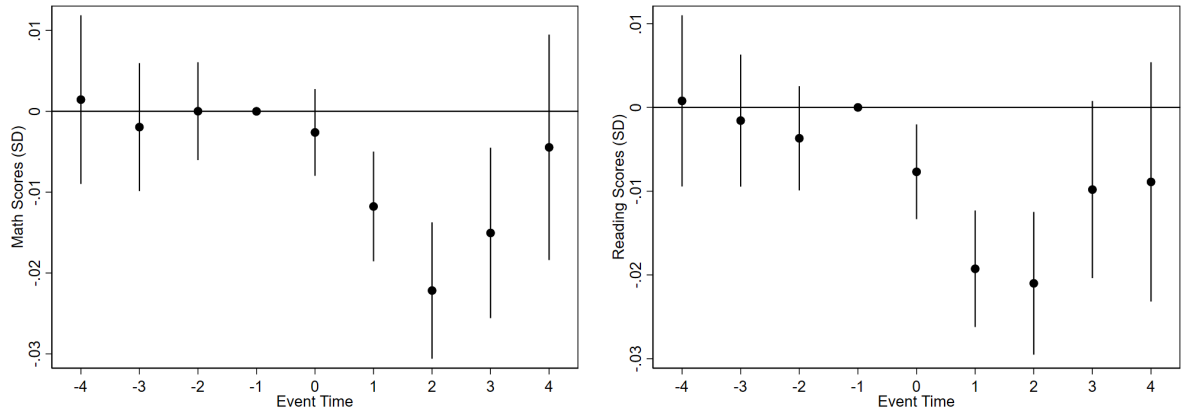
Texas Legislature, "Texas Family Code, Title 5: The Parent-Child Relationship," Texas Statutes 2023.

Verbruggen, Janna, Christopher D. Maxwell, and Amanda L. Robinson, "The Relationship Between the Development of General Offending and Intimate Partner Violence Perpetration in Young Adulthood," *Journal of Interpersonal Violence*, 2022, *37* (3–4), 1179–1205.

Wager, Stefan and Susan Athey, "Estimation and inference of heterogeneous treatment effects using random forests," *Journal of the American Statistical Association*, 2018, *113* (523), 1228–1242.

Tables and Figures

Figure 1: The Impact of Divorce on Achievement

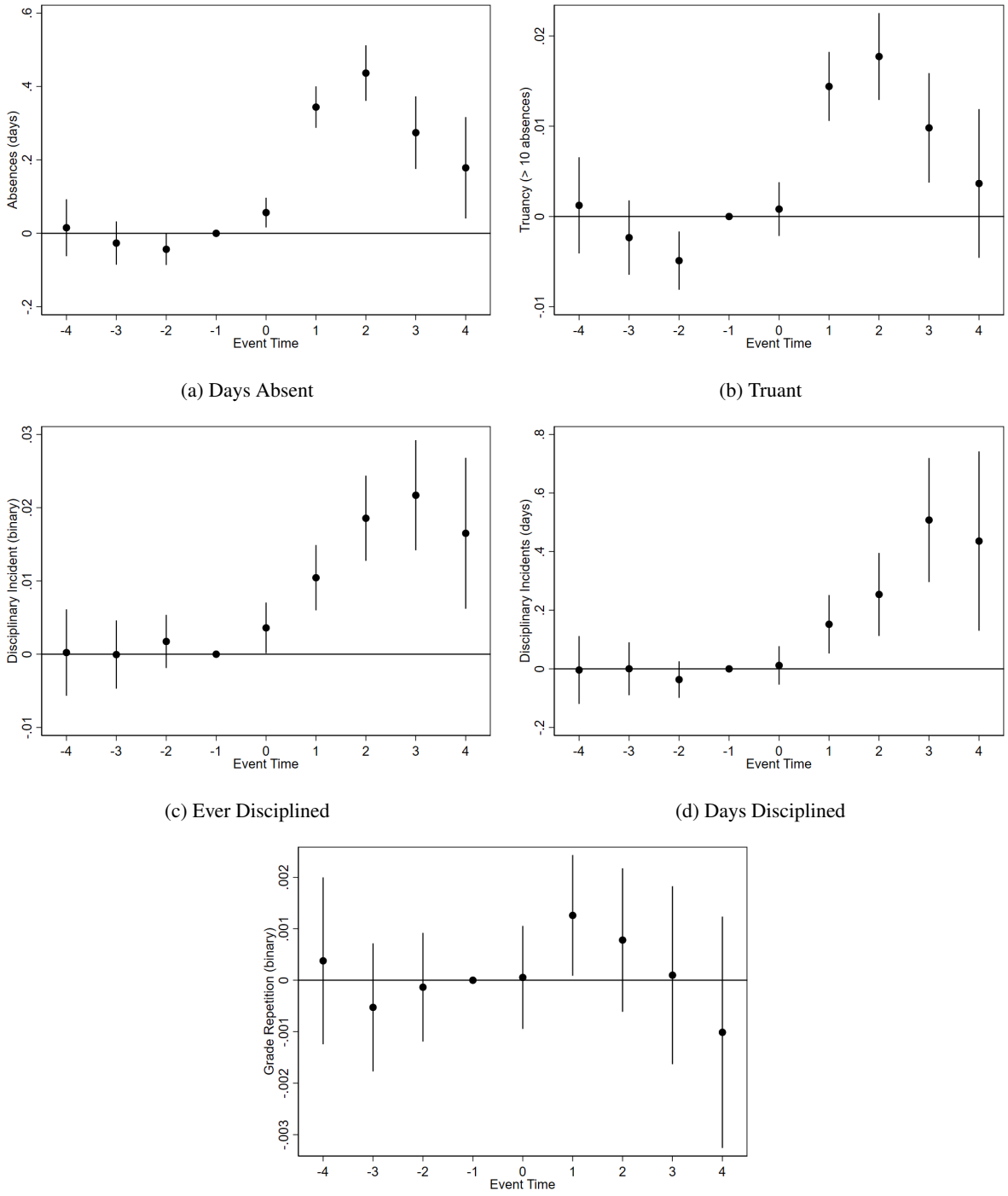


(a) Math

(b) Reading

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on student outcomes. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. We exclude children ever classified as special education or English language learners, and include only children observed in the outcome data in every grade from 3 through 8. Standard errors are clustered at the individual level.

Figure 2: The Impact of Divorce on Engagement and Behavioral Outcomes



(a) Days Absent

(b) Truant

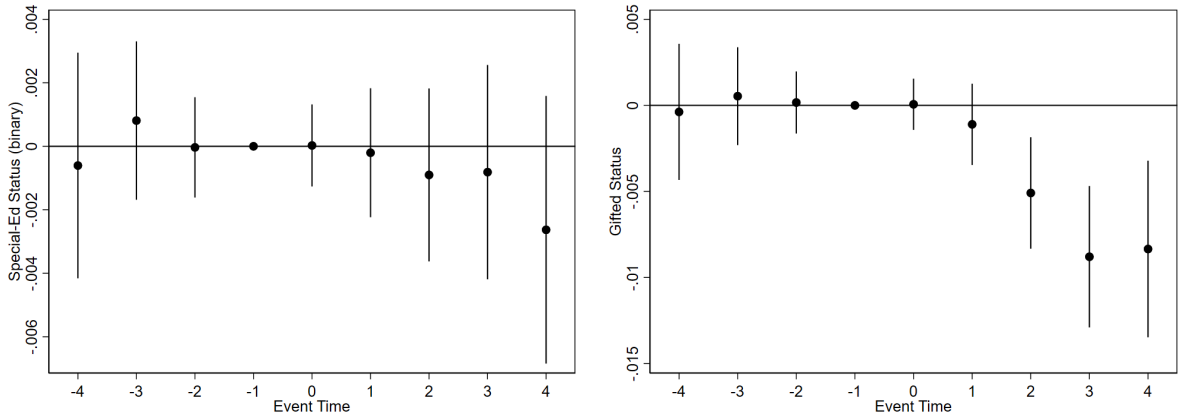
(c) Ever Disciplined

(d) Days Disciplined

(e) Repeated Grade

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on student outcomes. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. We exclude children ever classified as special education or English language learners, and include only children observed in the outcome data in every grade from 3 through 8. Standard errors are clustered at the individual level.

Figure 3: The Impact of Divorce on Educational Classifications

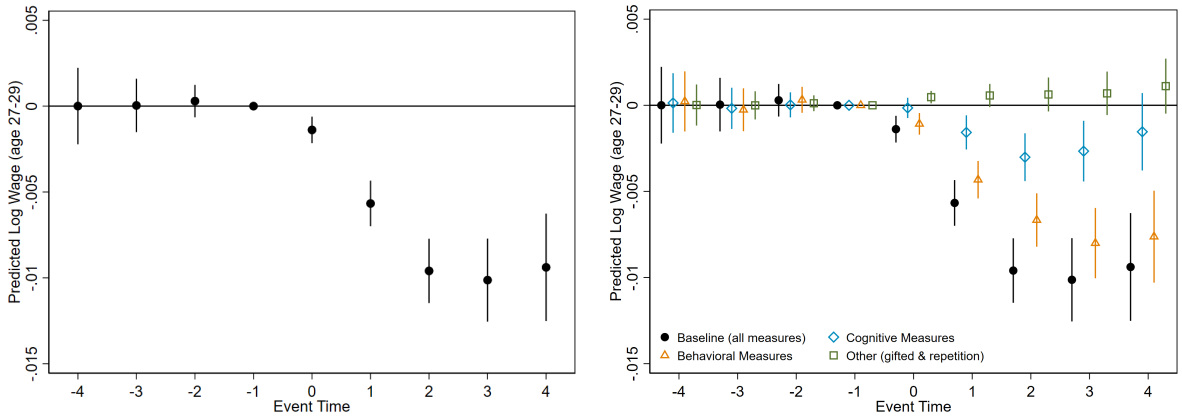


(a) Special Education Placement

(b) Gifted Placement

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on student outcomes. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. In Panel A, where the outcome is special education classification, we exclude children ever classified as English language learners. In Panel B, we exclude children ever classified as special education or English language learners. Both panels include only children observed in the outcome data in every grade from 3 through 8. Standard errors are clustered at the individual level.

Figure 4: The Impact of Divorce on Predicted Log Wages

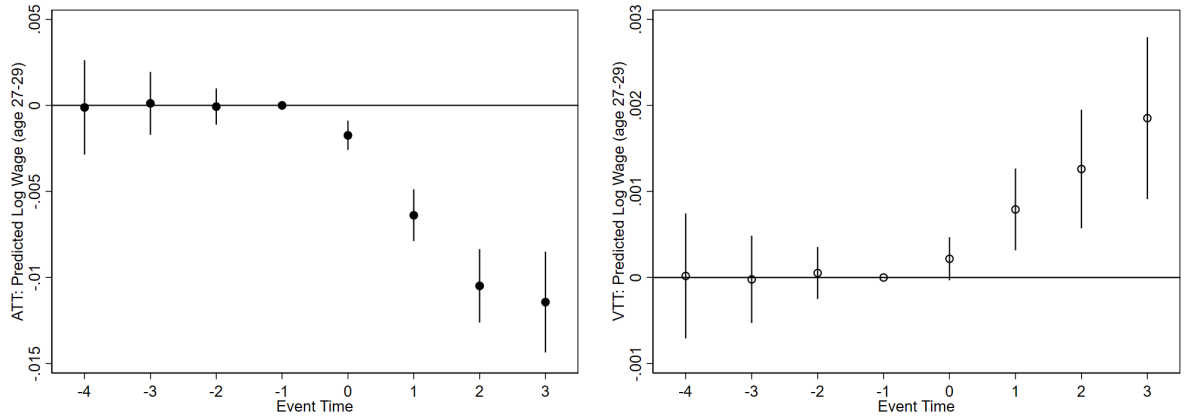


(a) Predicted Log Wages

(b) Predicted Log Wages by Component

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on predicted earnings. Each panel plots the estimated coefficients from Equation 2 on predicted earnings, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1990 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level. Predicted earnings in grade g are constructed from the history of short-run outcomes (reading and math scores, days absent, days disciplined, and indicators for ever disciplined, grade repetition, and gifted status) in grades 3 through grade g, fixed characteristics (race/ethnicity and sex), 3rd grade campus and year of birth fixed effects, which together are used to predict log average annual wages during ages 27-29. The prediction model is estimated on the sample of individuals whose parents did not divorce between 1st grade and four years after expected high school graduation, and predictions are applied out of sample to children of divorce. Panel A plots the estimates for the baseline predicted earnings measure. Panel B shows the same estimates (Black circles) alongside component-specific measures constructed from cognitive outcomes (reading and math scores) and non-cognitive outcomes (days absent, days disciplined and an indicator for ever disciplined). Each component is the portion of predicted earnings attributable to that group of outcomes, using coefficients from the single prediction model that includes all outcomes.

Figure 5: Heterogeneous Effects of Parental Divorce on Predicted Log Wages

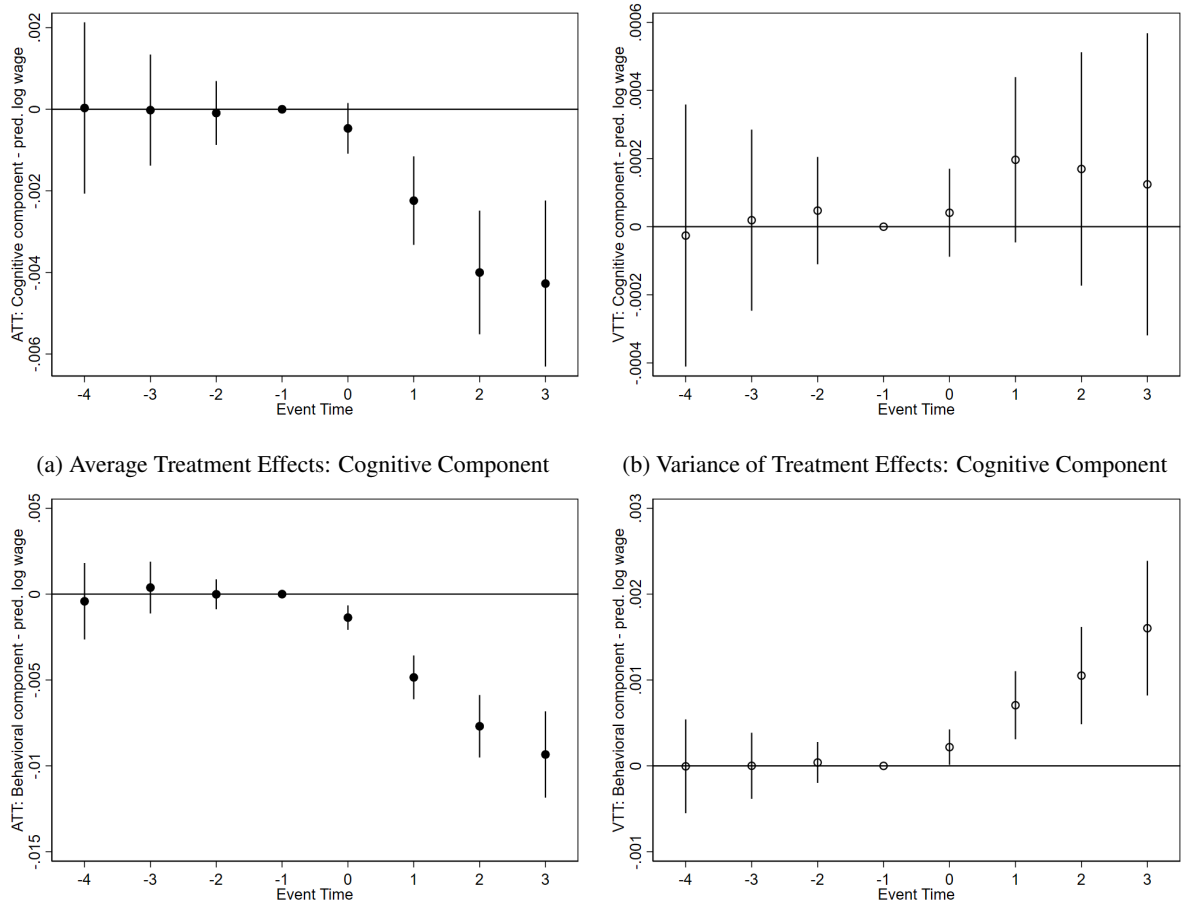


(a) Average Treatment Effects

(b) Variance of Treatment Effects

Note: These figures present coefficients and 95% confidence intervals of the average treatment effect, $E[D_s]$, and the variance of treatment effects, $\text{Var}(D_s)$, spanning four years before through three years after divorce, from a two-stage residual-variance event study (see Section 5.2 for more details). The specification includes grade fixed effects and year fixed effects. The sample includes children born in Texas between 1990 to 1997, whose parents divorced in expected grades 5-12. Divorces in grades 5-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level. See Figure 4 for more details on how predicted earnings are constructed.

Figure 6: Heterogeneous Effects of Parental Divorce on Predicted Earnings



(a) Average Treatment Effects: Cognitive Component

(b) Variance of Treatment Effects: Cognitive Component

(c) Average Treatment Effects: Behavioral Component

(d) Variance of Treatment Effects: Behavioral Component

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on predicted earnings, estimated separately for boys and girls. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level. See Table 4 for more details on how predicted earnings are constructed.

Table 1: Sample Characteristics by Family Structure

	All Students (1)	Texas Born (2)	No Marriage/ Divorce Record (3)	Always Married (4)	Ever Divorced (5)	Divorced Gr 4–8 (6)	Divorced Gr 9–12 (7)
Female	0.49	0.47	0.48	0.46	0.47	0.47	0.47
White	0.39	0.41	0.30	0.53	0.55	0.51	0.50
Hispanic	0.43	0.42	0.47	0.37	0.35	0.38	0.40
Black	0.14	0.14	0.20	0.08	0.09	0.09	0.08
Other Race/Eth.	0.03	0.02	0.03	0.02	0.01	0.02	0.02
3rd Grade:							
Age	8.15	8.14	8.16	8.12	8.13	8.12	8.10
LEP	0.20	0.17	0.21	0.12	0.11	0.13	0.15
Special-ed	0.11	0.12	0.12	0.11	0.12	0.11	0.11
Free Lunch	0.56	0.55	0.66	0.39	0.47	0.44	0.44
Gifted	0.08	0.08	0.07	0.11	0.08	0.09	0.09
Math Z	0.01	0.00	-0.11	0.17	0.07	0.09	0.12
Reading Z	0.01	0.00	-0.11	0.16	0.07	0.09	0.11
Combined Z	0.01	0.00	-0.12	0.17	0.07	0.09	0.12
Days Absent	5.27	5.37	5.61	4.87	5.41	5.32	5.14
Truant	0.14	0.14	0.16	0.11	0.14	0.14	0.13
Disciplined	0.06	0.06	0.07	0.04	0.05	0.04	0.04
Days Disc.	0.23	0.24	0.30	0.13	0.19	0.15	0.12
Repeated Gr.	0.02	0.02	0.03	0.01	0.02	0.02	0.02
Ever LEP (3-8)	0.21	0.17	0.22	0.12	0.11	0.14	0.16
Ever SpEd (3-8)	0.18	0.19	0.20	0.17	0.19	0.18	0.17
Ever Repeat (3-8)	0.08	0.08	0.10	0.05	0.07	0.07	0.07
N	3,578,410	2,541,118	1,276,784	641,054	559,543	104,843	46,666

Note: The sample is restricted to students born between 1985 and 1997 who are observed in TEA data in every grade from 3 through 8. Column 1 includes all such students; Column 2 restricts to those found in the Texas Birth Index, for whom parent–child linkages can be constructed. "No Marriage/Divorce Record" indicates either no matched marriage record on or before the year the child turns 23, or a marriage followed by a divorce before the birth year (and not in the ever-divorced group). "Always Married" indicates a marriage recorded on or before the birth year with no divorce recorded on or before the year the child turns 23. "Ever Divorced" indicates a divorce recorded between the birth year and the year the child turns 23. Columns 6 and 7 restrict the ever-divorced group to divorces occurring in expected grades 4–8 and 9–12, respectively. Marriage records are matched using mother's and father's names together, while divorce records are matched using marriage date, parents' age, father's full name, and mother's first name and middle initial. See Appendix B for more detail on the matching procedure.

Table 2: The Impact of Divorce on Children's Outcomes

	Math Scores (1)	Reading Scores (2)	Absences (3)	Ever Disciplined (4)	Days Disciplined (5)	Special Education (6)	Gifted (7)	Predicted Wages (8)
Event time:								
−4	0.0014	0.0008	0.0152	0.0002	−0.0040	−0.0006	−0.0004	0.0000
	(0.0053)	(0.0052)	(0.0396)	(0.0030)	(0.0591)	(0.0018)	(0.0020)	(0.0011)
−3	−0.0019	−0.0016	−0.0266	−0.0001	0.0002	0.0008	0.0005	0.0000
	(0.0040)	(0.0040)	(0.0301)	(0.0024)	(0.0461)	(0.0013)	(0.0014)	(0.0008)
−2	0.0000	−0.0037	−0.0434*	0.0017	−0.0367	−0.0000	0.0002	0.0003
	(0.0031)	(0.0032)	(0.0219)	(0.0019)	(0.0318)	(0.0008)	(0.0009)	(0.0005)
−1 (reference)	—	—	—	—	—	—	—	—
	—	—	—	—	—	—	—	—
0	−0.0026	−0.0077**	0.0565**	0.0036*	0.0116	0.0000	0.0001	−0.0014***
	(0.0027)	(0.0029)	(0.0207)	(0.0018)	(0.0335)	(0.0007)	(0.0008)	(0.0004)
1	−0.0118***	−0.0193***	0.3440***	0.0104***	0.1520**	−0.0002	−0.0011	−0.0057***
	(0.0035)	(0.0036)	(0.0288)	(0.0023)	(0.0509)	(0.0010)	(0.0012)	(0.0007)
2	−0.0222***	−0.0210***	0.4360***	0.0186***	0.2540***	−0.0009	−0.0051**	−0.0096***
	(0.0043)	(0.0043)	(0.0386)	(0.0030)	(0.0723)	(0.0014)	(0.0016)	(0.0010)
3	−0.0150**	−0.0098	0.2740***	0.0217***	0.5080***	−0.0008	−0.0088***	−0.0101***
	(0.0054)	(0.0054)	(0.0505)	(0.0038)	(0.1080)	(0.0017)	(0.0021)	(0.0012)
4	−0.0045	−0.0089	0.1780*	0.0165**	0.4360**	−0.0026	−0.0083**	−0.0094***
	(0.0071)	(0.0073)	(0.0705)	(0.0053)	(0.1560)	(0.0022)	(0.0026)	(0.0016)
Adj. R-Squared	0.675	0.621	0.523	0.303	0.190	0.750	0.761	0.941
Observations	1,253,838	1,276,649	1,377,085	869,405	869,405	1,666,836	1,377,790	788,596
Unique Students	96,701	98,454	106,678	67,754	67,754	129,731	106,679	61,351
Dep. Var. Mean	0.255	0.284	5.938	0.126	0.970	0.116	0.146	8.908
Treated Mean	0.218	0.256	6.310	0.137	1.114	0.122	0.139	8.894
Control Mean	0.272	0.296	5.769	0.121	0.903	0.114	0.150	8.914

Note: This table presents coefficients from stacked event-study regressions that estimate the dynamic effects of parental divorce on student outcomes. Each column reports the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997 (except for the predicted wage outcome which focuses on children born in Texas between 1990 and 1997), whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level.

Table 3: The Impact of Divorce on Children's Outcomes

	Math Scores (1)	Reading Scores (2)	Absences (3)	Ever Disciplined (4)	Days Disciplined (5)	Special Education (6)	Gifted (7)	Predicted Wages (8)
Post × Treated	-0.00970^{**}	-0.0123^{***}	0.246^{***}	0.00975^{***}	0.165^{**}	-0.000443	-0.00237	-0.00520^{***}
	(0.00331)	(0.00324)	(0.0290)	(0.00212)	(0.0537)	(0.00116)	(0.00137)	(0.000802)
Adj. R-Squared	0.675	0.621	0.523	0.303	0.190	0.750	0.761	0.941
Observations	1,253,838	1,276,649	1,377,085	869,405	869,405	1,666,836	1,377,790	788,596
Unique Students	96,701	98,454	106,678	67,754	67,754	129,731	106,679	61,351
Dep. Var. Mean	0.255	0.284	5.938	0.126	0.970	0.116	0.146	8.908
Treated Mean	0.218	0.256	6.310	0.137	1.114	0.122	0.139	8.894
Control Mean	0.272	0.296	5.769	0.121	0.903	0.114	0.150	8.914

Note: This table presents difference-in-differences estimates of the effect of parental divorce on student outcomes. Each column reports the coefficient on the interaction of an indicator for the post-divorce period with treatment status, estimated from a pre-post version of Equation 2 for a different outcome. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 and 1997 (except for the predicted wage outcome which focuses on children born in Texas between 1990 and 1997), whose parents divorced in expected grades 4–12. Divorces in grades 4–8 serve as treatments and those in grades 9–12 as controls. Standard errors, clustered at the individual level, are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Mean and Variance of Divorce Effects on Predicted Wages

	Years since divorce			
	0	1	2	3
Panel A: Recovered Moments				
ATE Estimate	-0.0017^{***}	-0.0064^{***}	-0.0105^{***}	-0.0114^{***}
	(0.0005)	(0.0009)	(0.0014)	(0.0020)
Variance of Treatment Effect Estimate	0.0002	0.0008^{***}	0.0013^{***}	0.0019^{***}
	(0.0001)	(0.0003)	(0.0004)	(0.0006)
N	537,179	537,179	537,179	537,179
Bootstrap Repetitions	400	400	400	400
Panel B: CDF Calculations Assuming Treatment Effect is Normally Distributed				
CDF @ 0	0.5470	0.5898	0.6161	0.6047
upper	—	0.6562	0.6861	0.6665
lower	0.5310	0.5696	0.5922	0.5823
% w/ opposite-signed Treatment Effect	0.4530	0.4102	0.3839	0.3953
Bootstrap CI Lower Bound	—	0.3438	0.3139	0.3335
Bootstrap CI Upper Bound	0.4690	0.4304	0.4078	0.4177

Note: Each column reports the recovered mean and variance of parental-divorce treatment effects on predicted log wages at a given number of years since divorce, from a variance-decomposition difference-in-differences estimator. Assuming treatment effects are normally distributed, CDF @ 0 is $\Phi(-\text{ATE}/\text{SD})$ with SD = $\sqrt{\text{Var}}$; the *upper* and *lower* rows re-evaluate this at the lower and upper confidence bounds of the variance, respectively. The share with an opposite-signed treatment effect is the mass of the distribution on the far side of zero from the ATE, with bootstrap bounds derived from the *upper*/*lower* rows. Standard errors (in parentheses) use 400 bootstrap repetitions; stars reflect a normal approximation (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). The omitted reference period is the year before divorce.

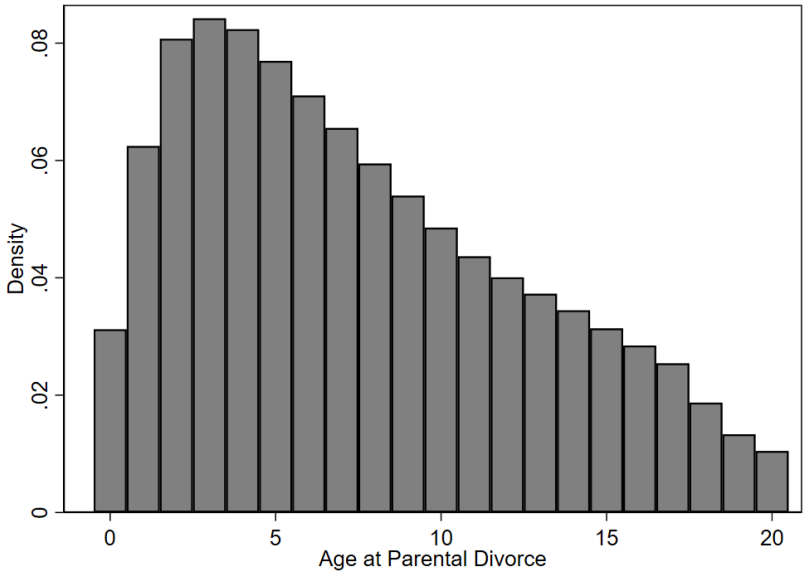
Table 5: Event-study estimates of effects on predicted log wages, by subgroup

	Years relative to event					
	$t = 0$	$t + 1$	$t + 2$	$t + 3$	$t + 4$	N
Panel A: Gender						
Boys	-0.002**	-0.006***	-0.011***	-0.012***	-0.012***	390,638
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	
Girls	-0.001*	-0.005***	-0.008***	-0.008***	-0.007**	397,958
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	
p-values, tests of equal coefficients:						
Boys = Girls	0.705	0.333	0.246	0.151	0.108	
Panel B: Free/reduced-price lunch						
Not FRL	-0.001*	-0.005***	-0.010***	-0.010***	-0.008***	516,134
	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)	
FRL	-0.002**	-0.006***	-0.009***	-0.009***	-0.011***	272,461
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	
p-values, tests of equal coefficients:						
Not FRL = FRL	0.170	0.687	0.664	0.705	0.446	
Panel C: Race/ethnicity						
White	-0.001**	-0.006***	-0.011***	-0.011***	-0.009***	441,606
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	
Black	-0.004**	-0.007**	-0.009**	-0.011**	-0.018***	76,507
	(0.001)	(0.002)	(0.003)	(0.004)	(0.005)	
Hispanic	-0.001	-0.006***	-0.010***	-0.011***	-0.009**	249,041
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	
p-values, tests of equal coefficients:						
White = Black	0.144	0.814	0.518	0.890	0.108	
White = Hispanic	0.964	0.795	0.644	0.916	0.862	
Black = Hispanic	0.179	0.710	0.743	0.949	0.169	
Panel D: Baseline achievement						
Tercile 1	-0.003**	-0.007***	-0.011***	-0.011***	-0.009**	183,907
	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	
Tercile 2	-0.002***	-0.008***	-0.012***	-0.013***	-0.012***	277,322
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	
Tercile 3	0.000	-0.003**	-0.006***	-0.006***	-0.006*	327,365
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	
p-values, tests of equal coefficients:						
Tercile 1 = Tercile 2	0.660	0.919	0.651	0.746	0.493	
Tercile 1 = Tercile 3	0.004	0.011	0.030	0.121	0.444	
Tercile 2 = Tercile 3	0.004	0.002	0.002	0.026	0.083	
Panel E: Sibling type						
Oldest sibling	-0.002**	-0.005***	-0.008***	-0.007**	-0.005	186,018
	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)	
Younger sibling	-0.002*	-0.007***	-0.012***	-0.015***	-0.017***	301,853
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	
Only child	-0.001	-0.005***	-0.008***	-0.007***	-0.004	288,805
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	
p-values, tests of equal coefficients:						
Oldest sibling = Younger sibling	0.487	0.363	0.114	0.008	0.003	
Oldest sibling = Only child	0.281	0.784	0.902	0.990	0.937	
Younger sibling = Only child	0.642	0.177	0.096	0.002	$<$0.001	

Notes: The dependent variable is predicted log wages. Each row reports post-event coefficients from a separate event-study regression estimated on the indicated subsample; event time -1 is the omitted period. Pre-event coefficients (event times -4 to -2) are small and statistically insignificant in every subsample. Standard errors in parentheses. The bottom rows of each panel report p-values from two-sided tests of equality of the event-time coefficient across the indicated pair of subgroups, treating the (disjoint) subgroup samples as independent. Pairwise p-values are not adjusted for multiple comparisons.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

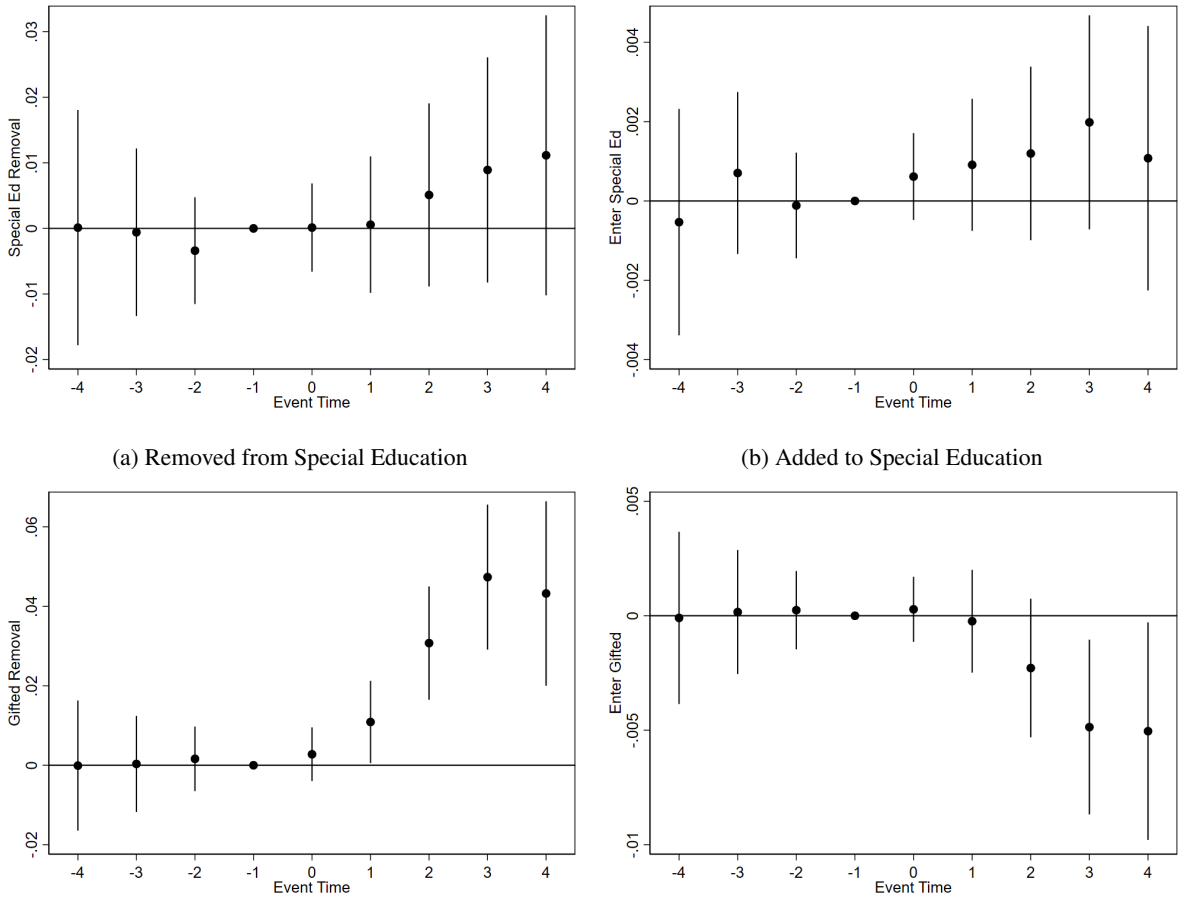
A Appendix Tables and Figures

Figure A.1: Age at Parental Divorce



Note: This figure presents the distribution of children's age at the time of parental divorce. Each bar represents the fraction of children whose parents divorced at a given age, measured in years. The sample includes children born in Texas between 1985 and 1997 whose parents divorced between ages 0 and 20.

Figure A.2: The Effect of Parental Divorce on Educational Classifications, Entries and Exits



(a) Removed from Special Education

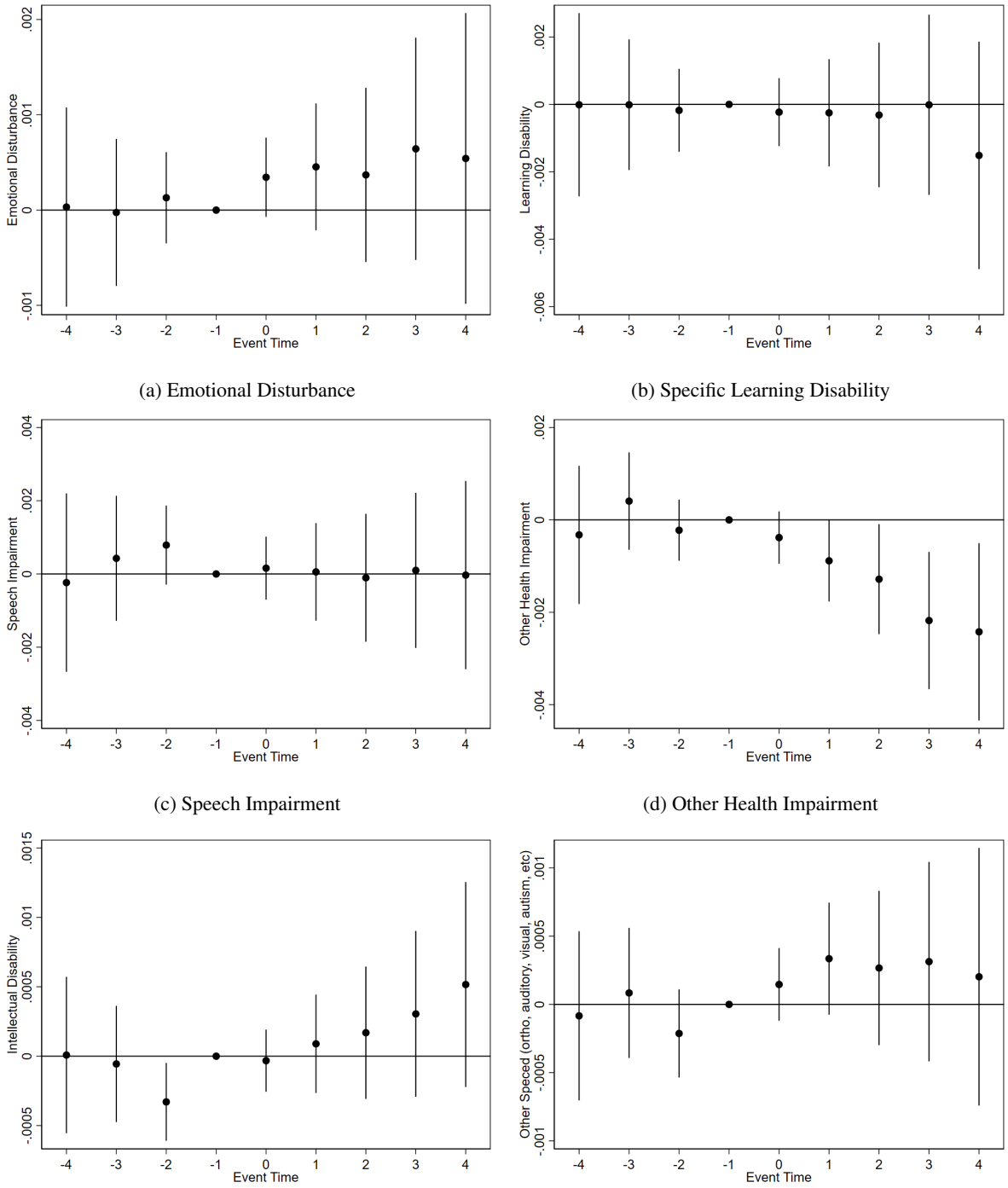
(b) Added to Special Education

(c) Removed from Gifted

(d) Added to Gifted

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on student outcomes. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level.

Figure A.3: The Effect of Parental Divorce on Special Education Placement, by Disability Type



(a) Emotional Disturbance

(b) Specific Learning Disability

(c) Speech Impairment

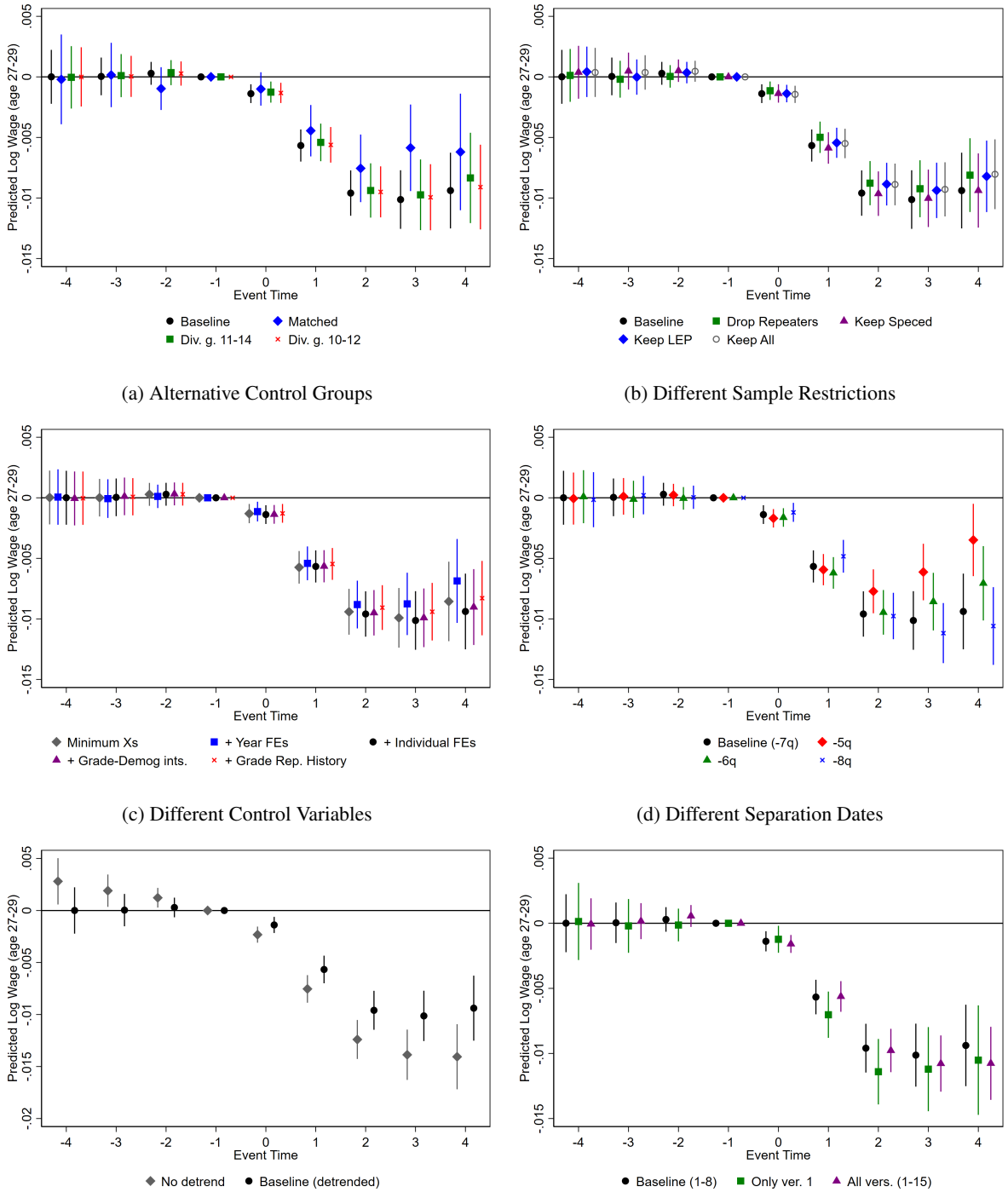
(d) Other Health Impairment

(e) Intellectual Disability

(f) Physical or Cognitively Severe

Note: These figures assess the robustness of the estimated effect of parental divorce on predicted log wages. Each panel presents coefficients and 95% confidence intervals from stacked event-study regressions based on Equation 2, spanning four years before through four years after divorce, where each series corresponds to an alternative specification choice: control group definition (Panel a), sample restrictions (Panel b), control variables (Panel c), the timing of parental separation (Panel d), detrending procedure (Panel e), and the version of the divorce match (Panel f). Unless otherwise noted, the baseline specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1990 and 1997 whose parents divorced in expected grades 4–12. Divorces in grades 4–8 serve as treatments and those in grades 9–12 as controls. Standard errors are clustered at the individual level. See Table 4 for details on the construction of predicted log wages.

Figure A.4: Robustness of the Effect of Parental Divorce on Predicted Log Wages



(a) Alternative Control Groups

(b) Different Sample Restrictions

(c) Different Control Variables

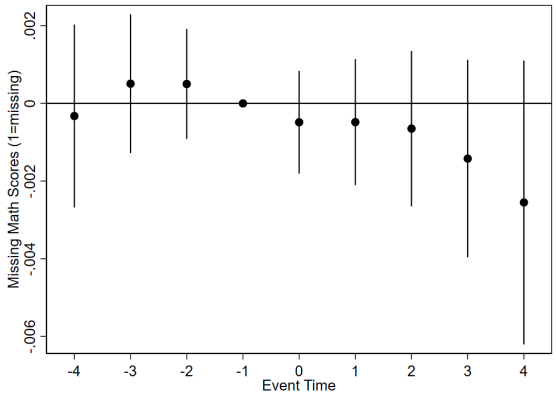
(d) Different Separation Dates

(e) Detrending Sensitivity

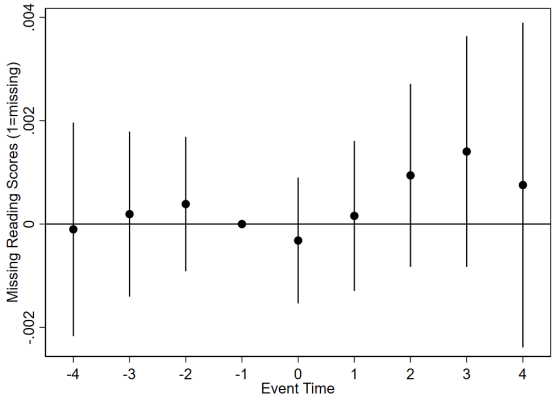
(f) Divorce Match Versions

Note: These figures assess the robustness of the estimated effect of parental divorce on predicted log wages. Each panel presents coefficients and 95% confidence intervals from stacked event-study regressions based on Equation 2, spanning four years before through four years after divorce, where each series corresponds to an alternative specification choice: control group definition (Panel a), sample restrictions (Panel b), control variables (Panel c), the timing of parental separation (Panel d), detrending procedure (Panel e), and the version of the divorce match (Panel f). Unless otherwise noted, the baseline specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1990 and 1997 whose parents divorced in expected grades 4–12. Divorces in grades 4–8 serve as treatments and those in grades 9–12 as controls. Standard errors are clustered at the individual level. See Table 4 for details on the construction of predicted log wages.

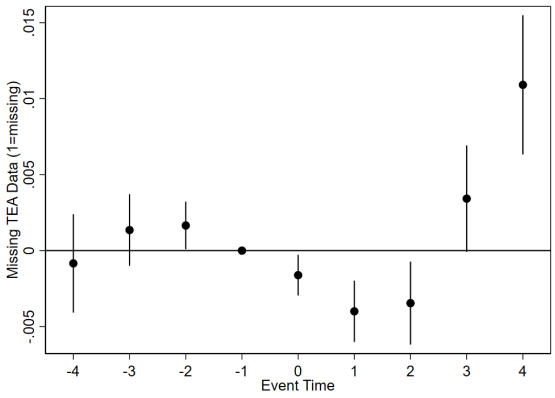
Figure A.5: Attrition



(a) Missing Math Score



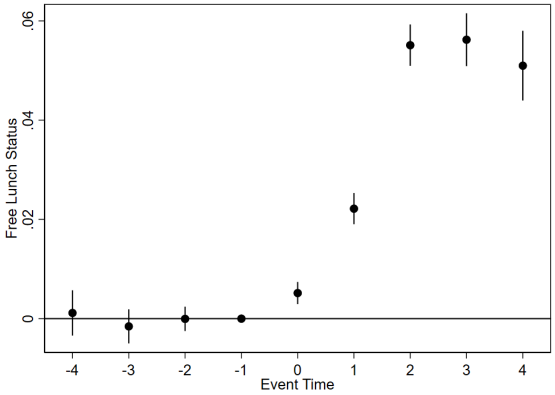
(b) Missing Reading Score



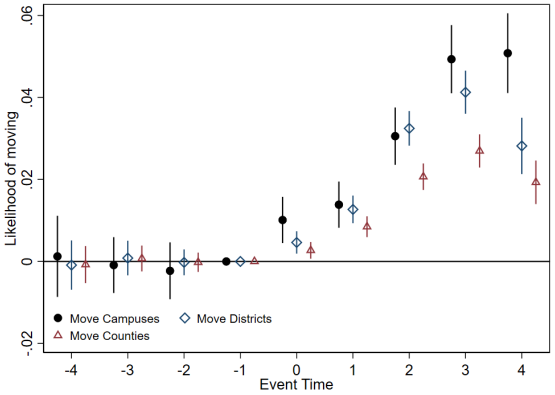
(c) Missing from TEA data

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on student outcomes. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level.

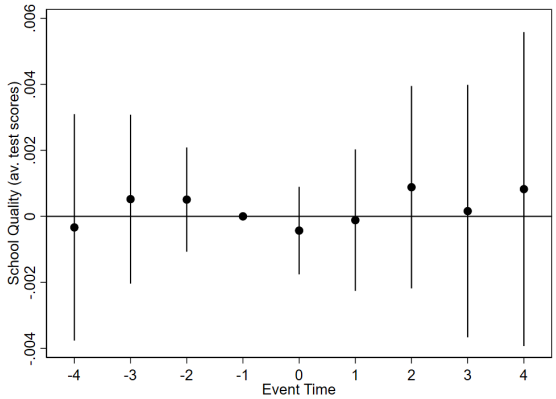
Figure A.6: Effects of Parental Divorce on Economic Disadvantage and School Mobility



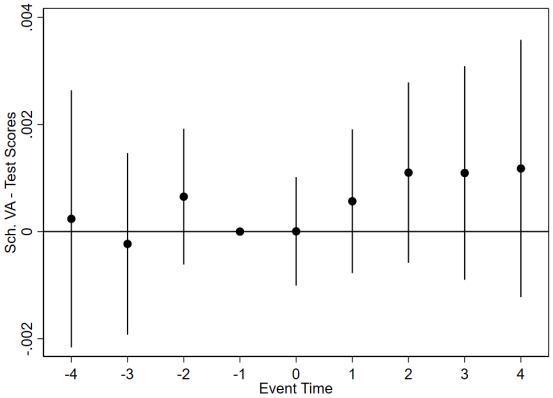
(a) Free Lunch Participation



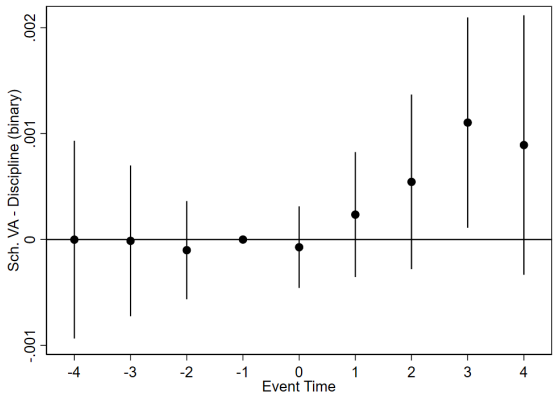
(b) Switched Campus, District, or County



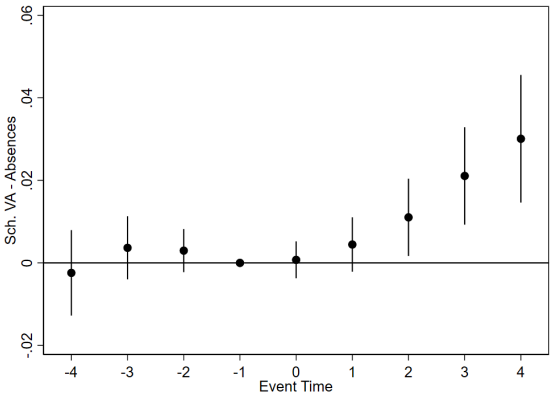
(c) Switched to Campus with Higher Scores



(d) Switched to Campus that Increases Test Scores



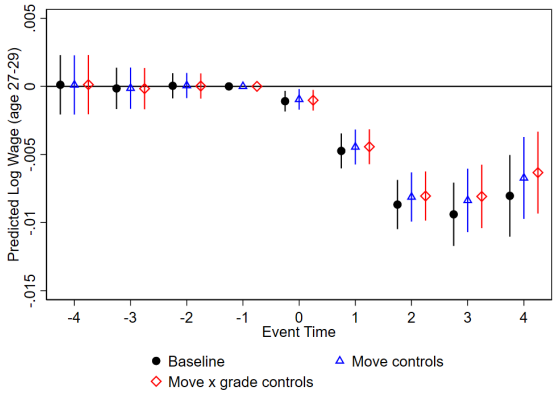
(e) Switched to Campus that Increases Discipline



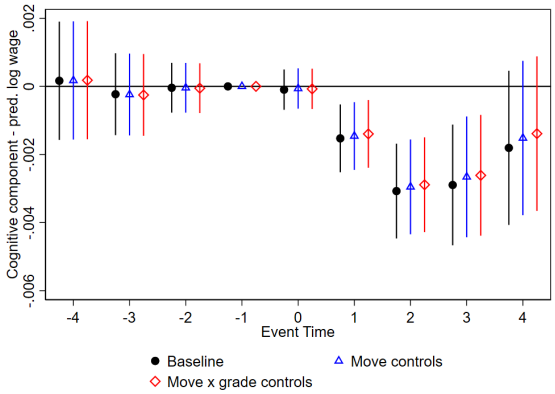
(f) Switched to Campus with Increases Absences

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on student outcomes. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level.

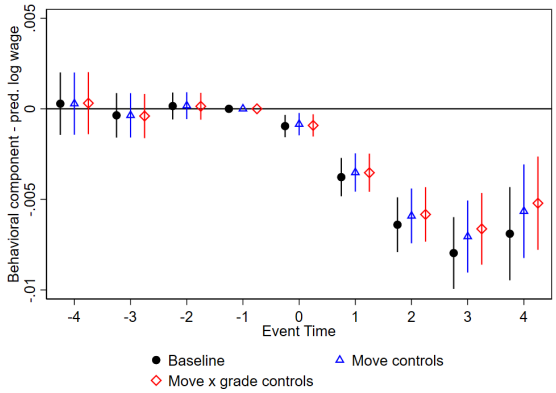
Figure A.7: Mediation Analysis: Controlling for Non-Structural School Moves



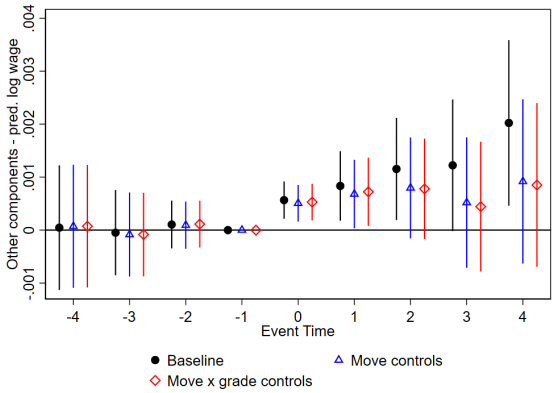
(a) Predicted Log Wage



(b) Predicted Log Wage: Cognitive Component



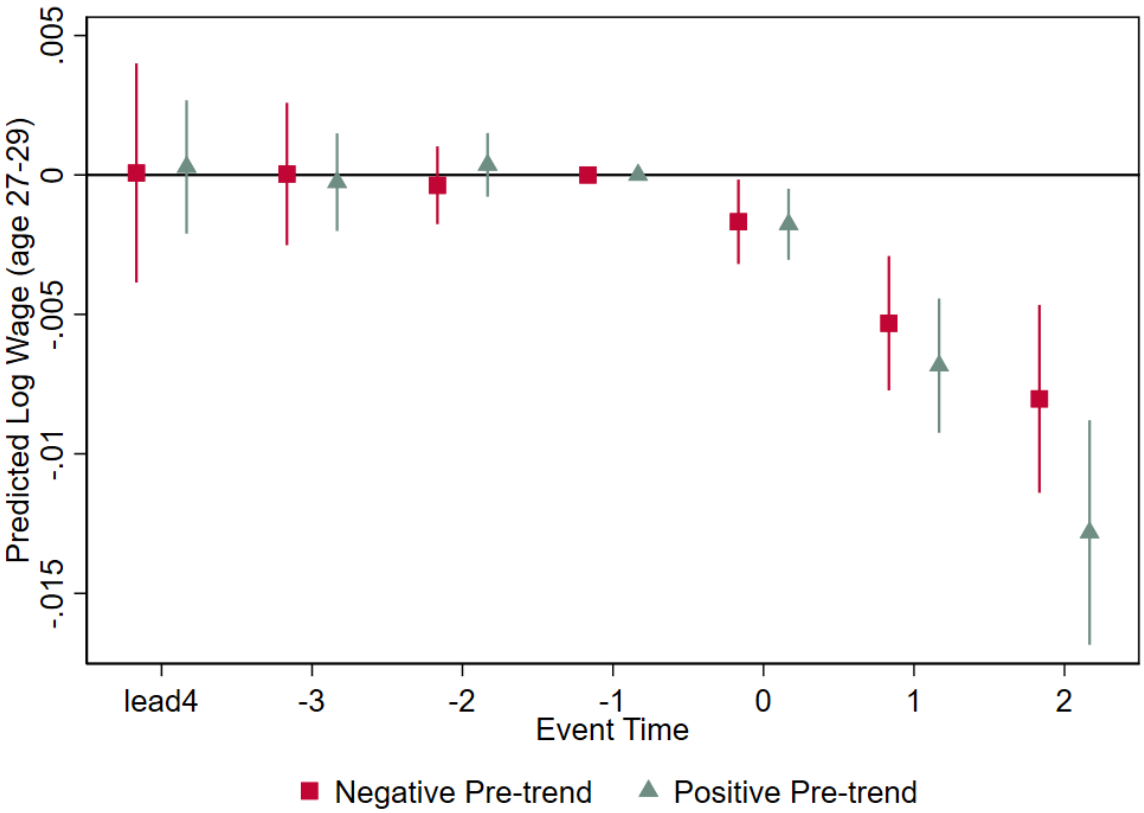
(c) Predicted Log Wage: Noncognitive Component



(d) Predicted Log Wage: Other Measures

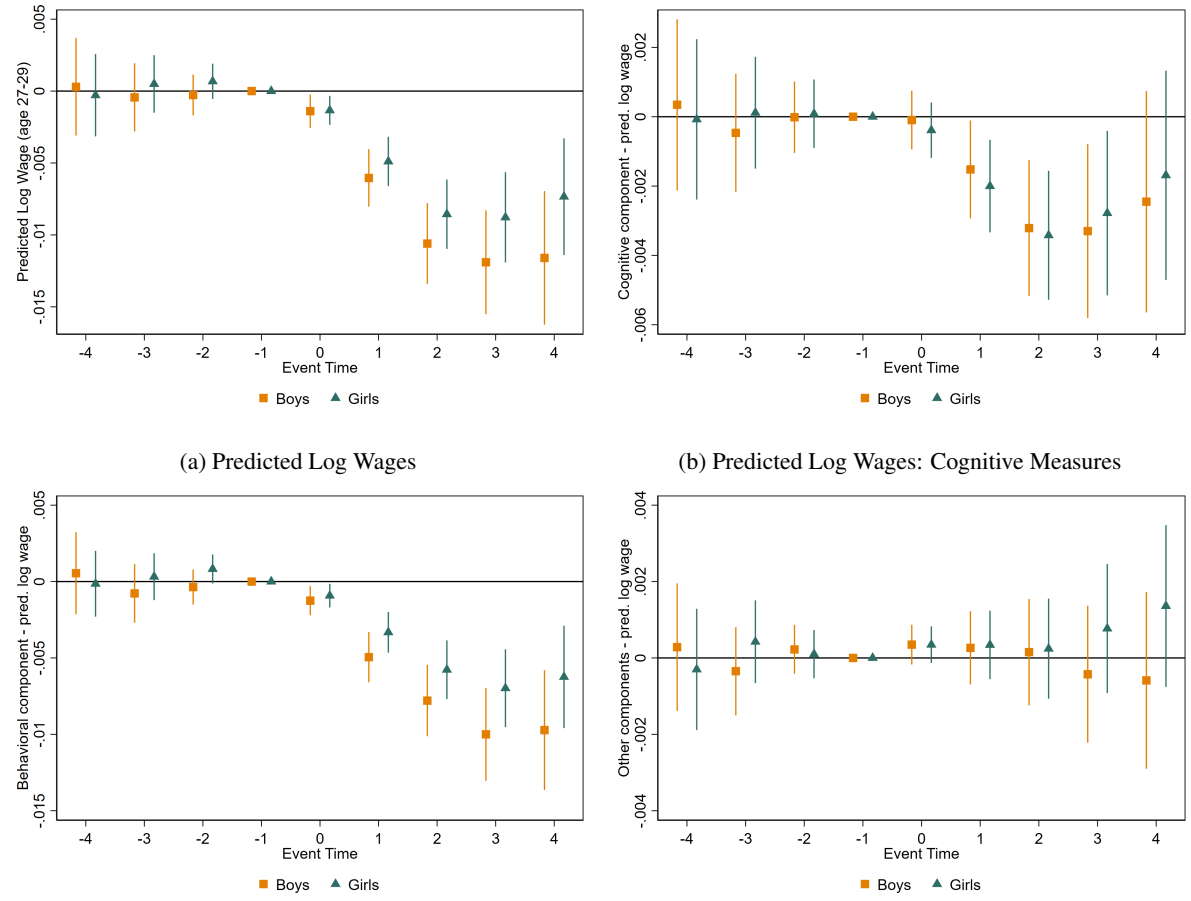
Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on student outcomes. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. Each panel compares baseline estimates to estimates that additionally control for non-structural school moves and for non-structural school moves interacted with grade. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level.

Figure A.8: Heterogeneous Effects of Parental Divorce on Predicted Earnings by Pre-Divorce Trajectories



Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on predicted earnings, estimated separately by pre-divorce trajectories. Children are classified by the direction of their trend in predicted earnings between event years -4 and -3: red squares denote children with a decreasing pre-divorce trend, and green triangles denote children with an increasing pre-divorce trend. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level. See Table 4 for more details on how predicted earnings are constructed.

Figure A.9: Heterogeneous Effects by Gender



(a) Predicted Log Wages

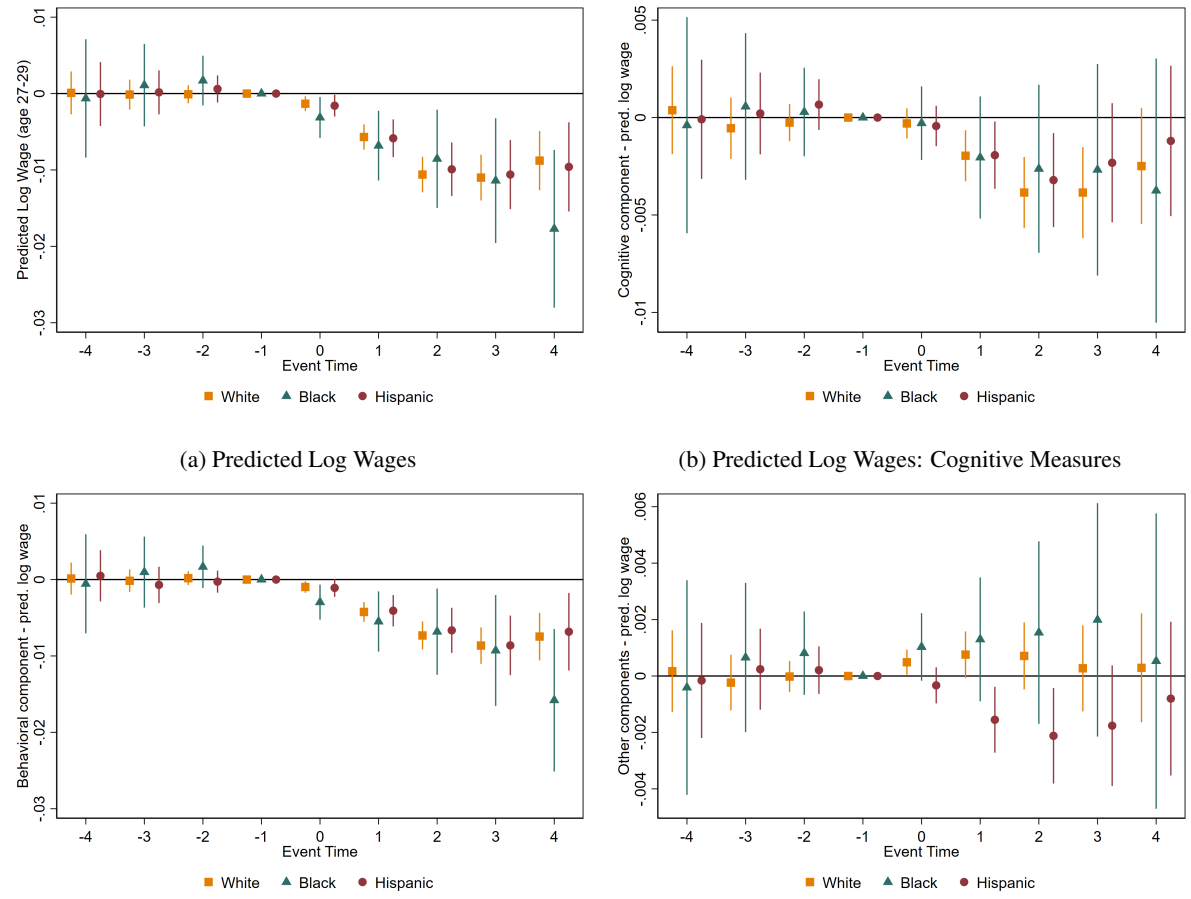
(b) Predicted Log Wages: Cognitive Measures

(c) Predicted Log Wages: Behavioral Measures

(d) Predicted Log Wages: Other Measures

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on predicted earnings, estimated separately for boys and girls. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level. See Table 4 for more details on how predicted earnings are constructed.

Figure A.10: Heterogeneous Effects by Race/Ethnicity



(a) Predicted Log Wages

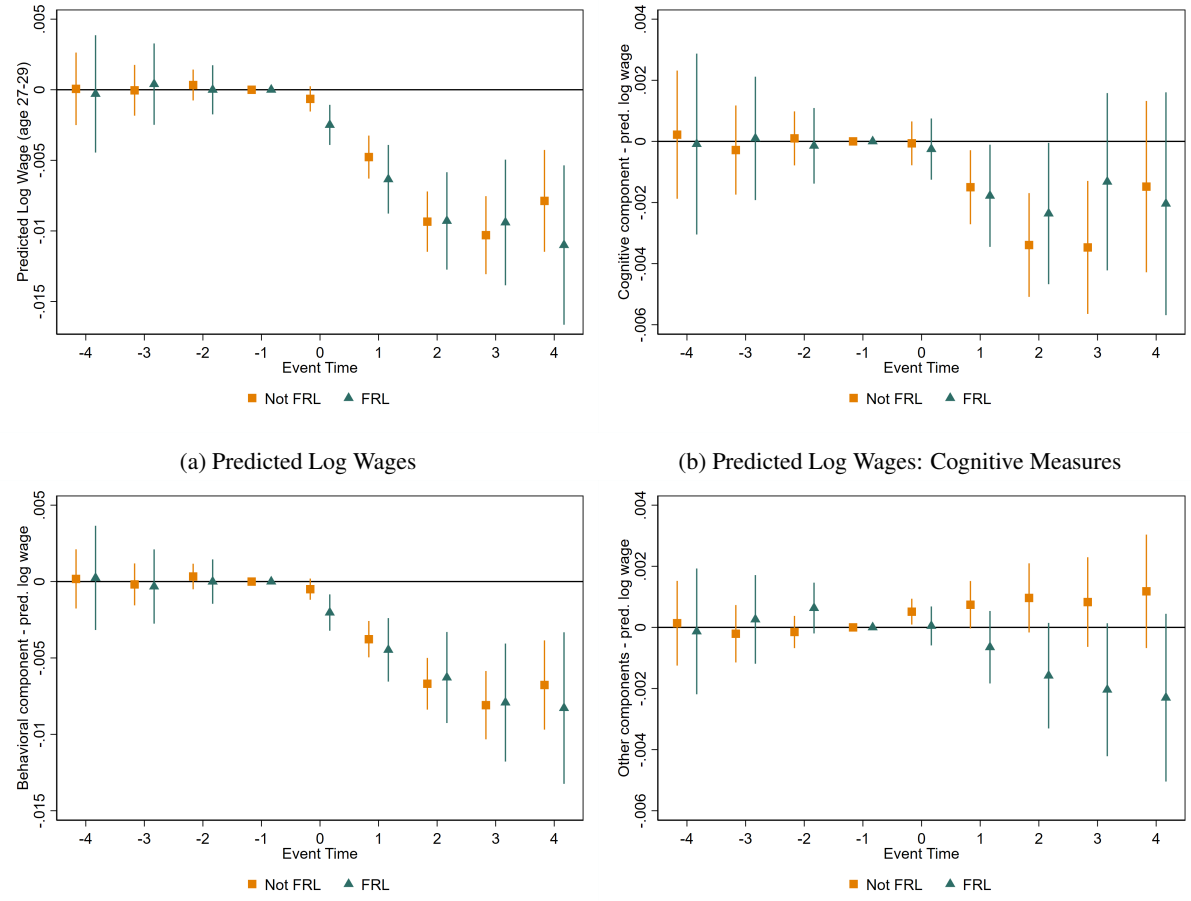
(b) Predicted Log Wages: Cognitive Measures

(c) Predicted Log Wages: Behavioral Measures

(d) Predicted Log Wages: Other Measures

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on predicted earnings, estimated separately for boys and girls. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level. See Table 4 for more details on how predicted earnings are constructed.

Figure A.11: Heterogeneous Effects by Free-Lunch Status



(a) Predicted Log Wages

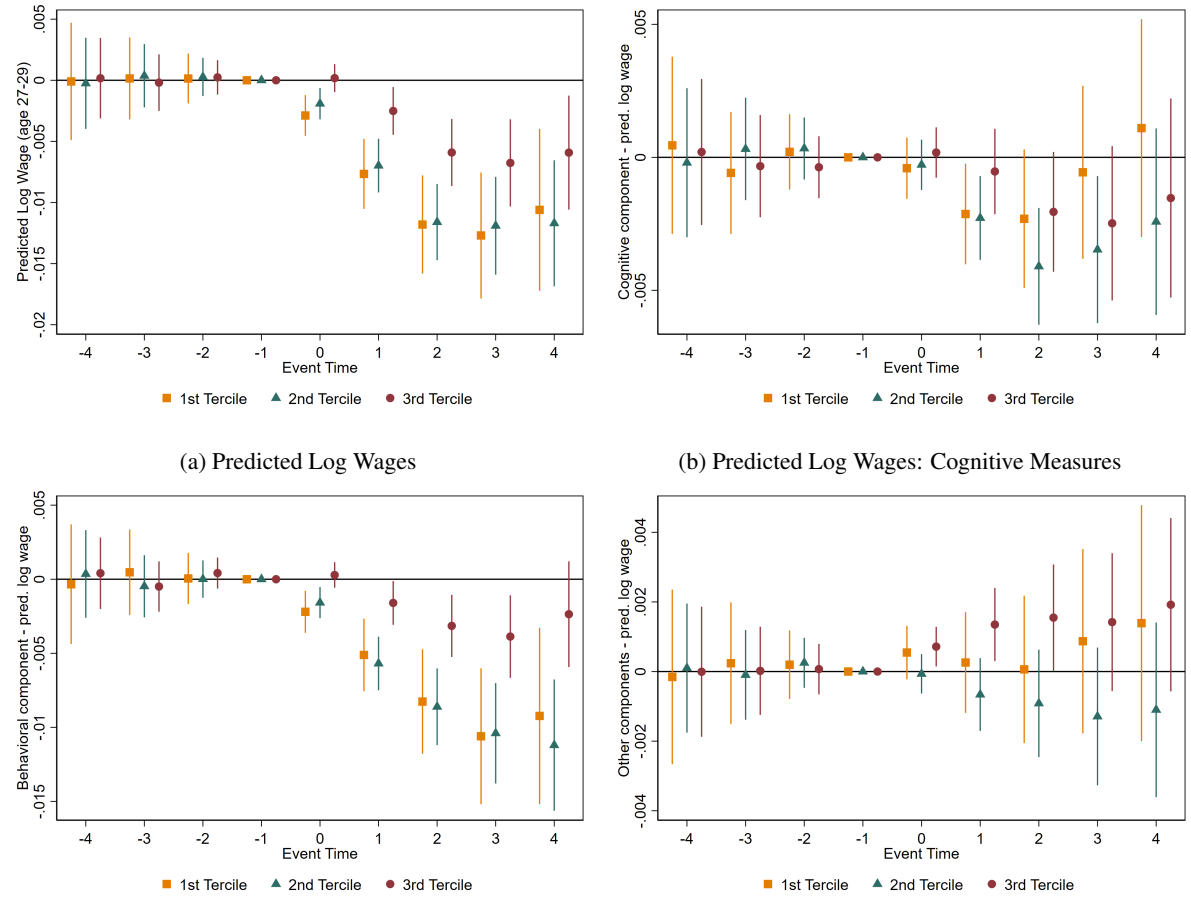
(b) Predicted Log Wages: Cognitive Measures

(c) Predicted Log Wages: Behavioral Measures

(d) Predicted Log Wages: Other Measures

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on predicted earnings, estimated separately for boys and girls. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level. See Table 4 for more details on how predicted earnings are constructed.

Figure A.12: Heterogeneous Effects by Baseline Achievement (Grade 3)



(a) Predicted Log Wages

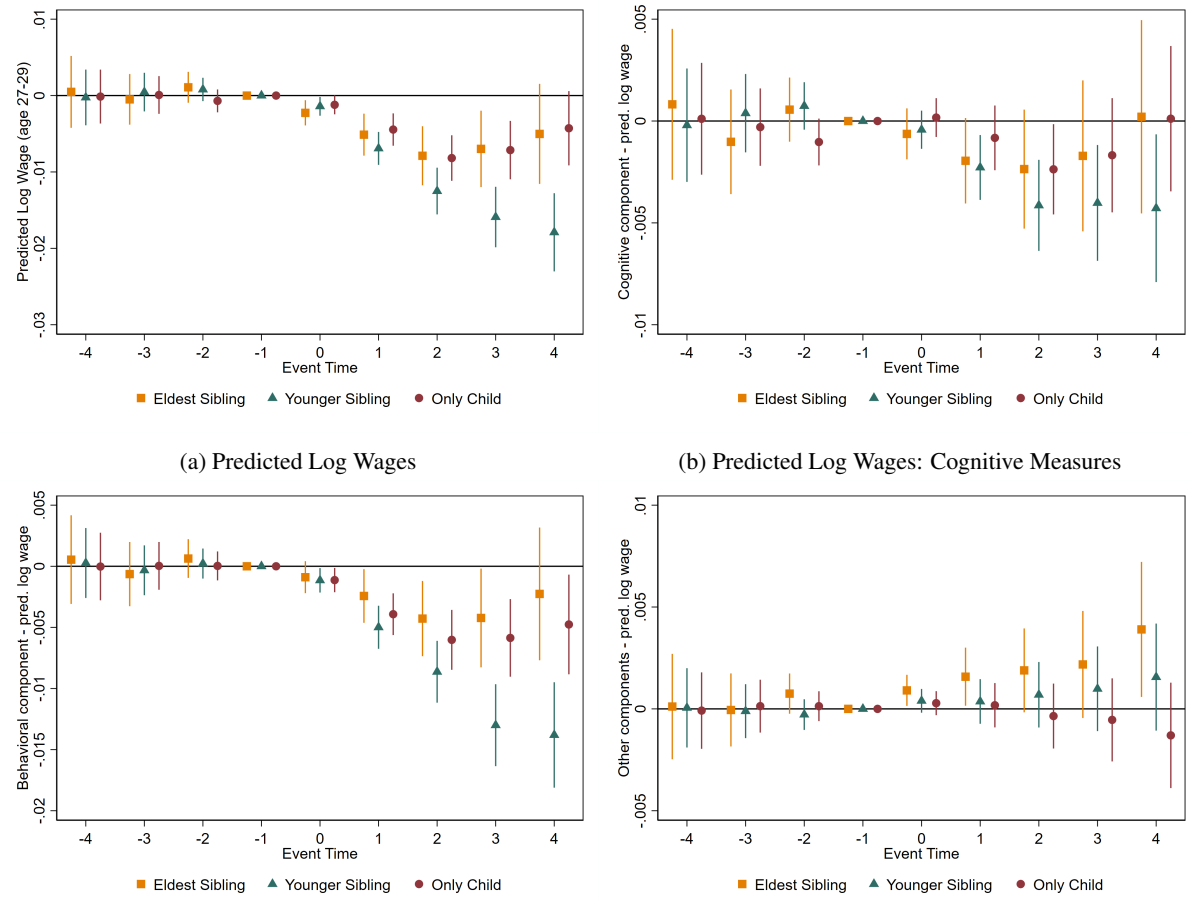
(b) Predicted Log Wages: Cognitive Measures

(c) Predicted Log Wages: Behavioral Measures

(d) Predicted Log Wages: Other Measures

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on predicted earnings, estimated separately for boys and girls. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level. See Table 4 for more details on how predicted earnings are constructed.

Figure A.13: Heterogeneous Effects by Birth Order



(a) Predicted Log Wages

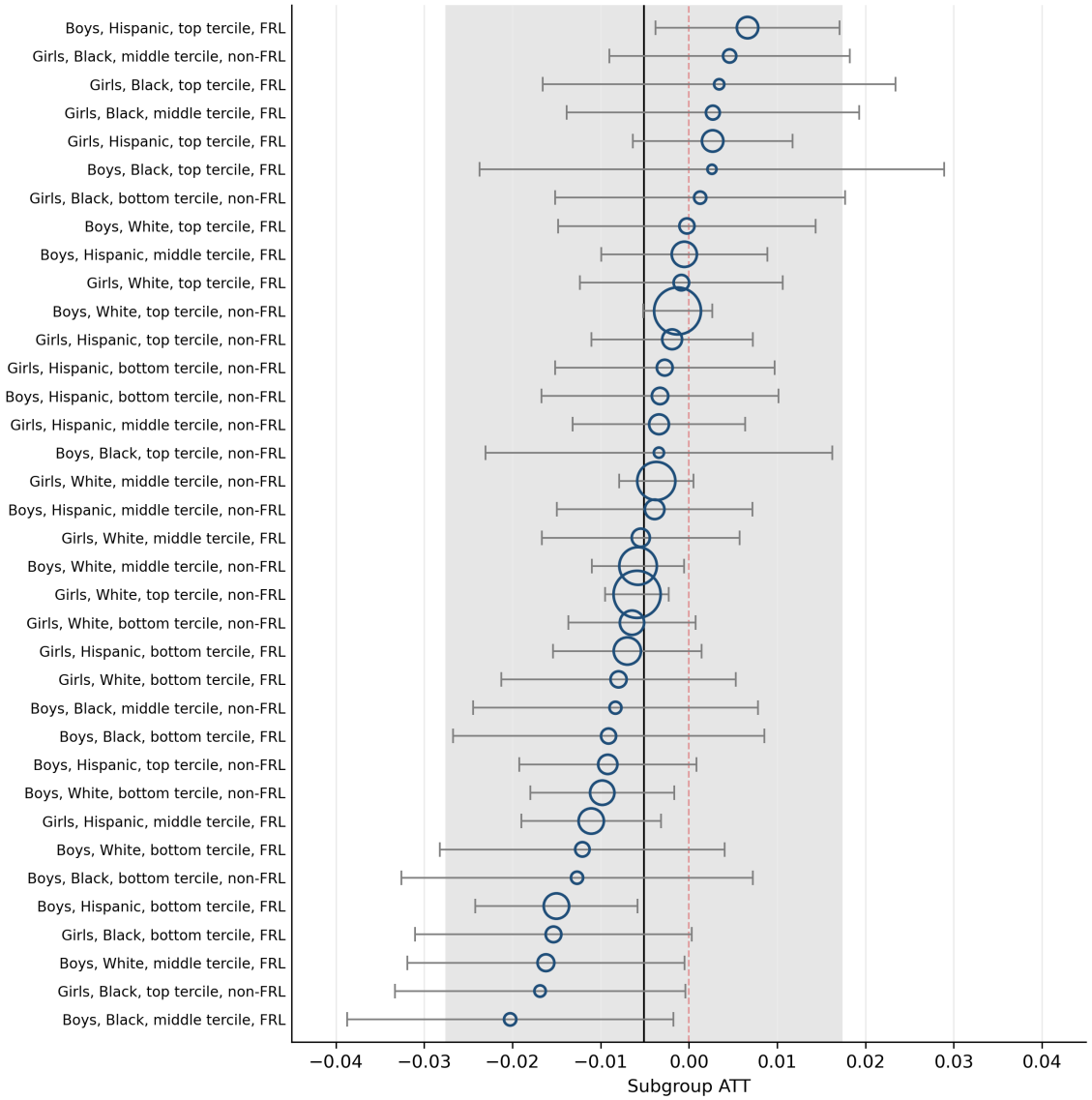
(b) Predicted Log Wages: Cognitive Measures

(c) Predicted Log Wages: Behavioral Measures

(d) Predicted Log Wages: Other Measures

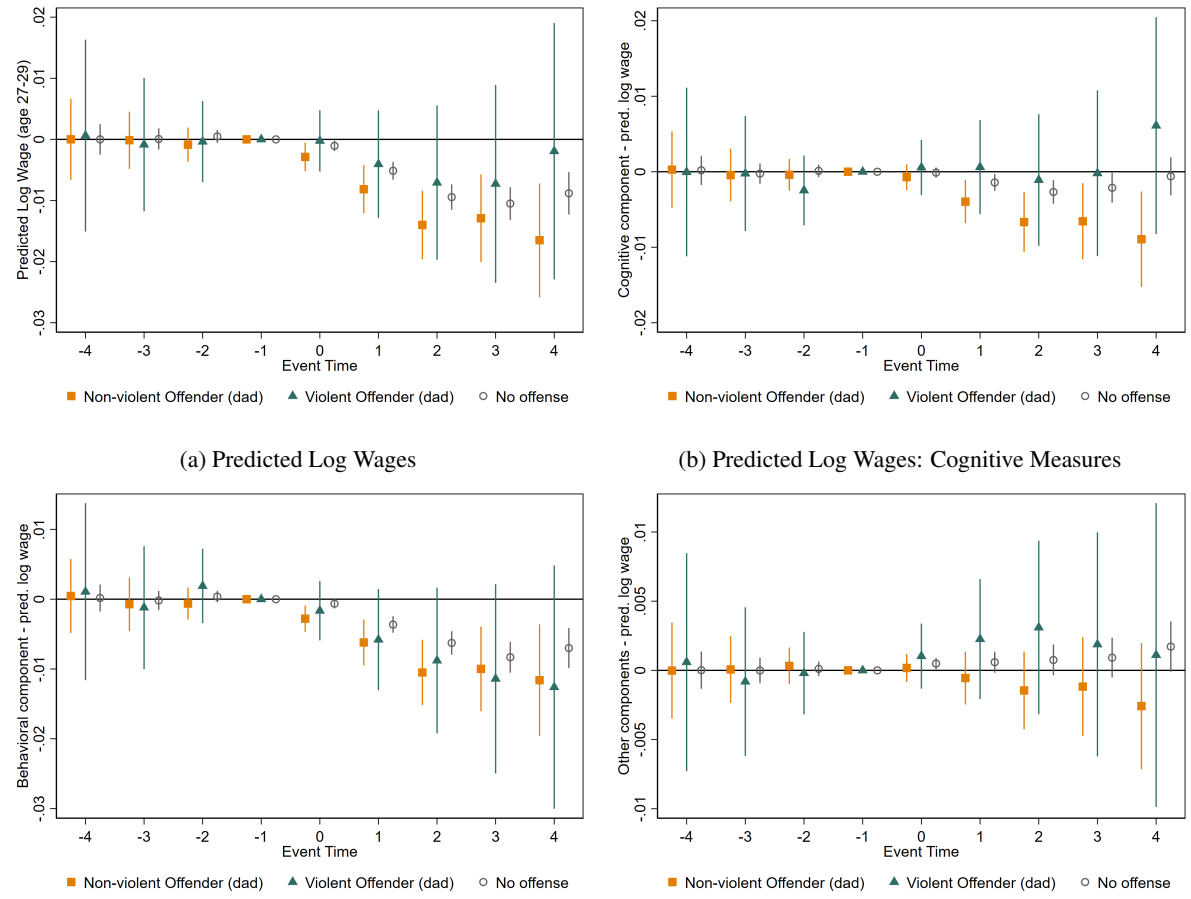
Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on predicted earnings, estimated separately for boys and girls. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level. See Table 4 for more details on how predicted earnings are constructed.

Figure A.14: Heterogeneity in divorce effects across observable subgroups



Notes: Each row reports the estimated effect of divorce on predicted earnings, pooling all post-separation years relative to pre-separation years, for one of 36 cells defined by sex, race, baseline achievement tercile, and free or reduced-price lunch (FRL) status, with 95 percent confidence intervals. Marker size is proportional to the number of treated observations. The dashed red line marks zero and the solid black line the overall mean effect (-0.005). The shaded band extends one standard deviation of individual treatment effects (0.023), estimated from the same pre/post specification, on either side of the mean.

Figure A.15: Heterogeneous Effects by Dad's Criminal Conviction status



(a) Predicted Log Wages

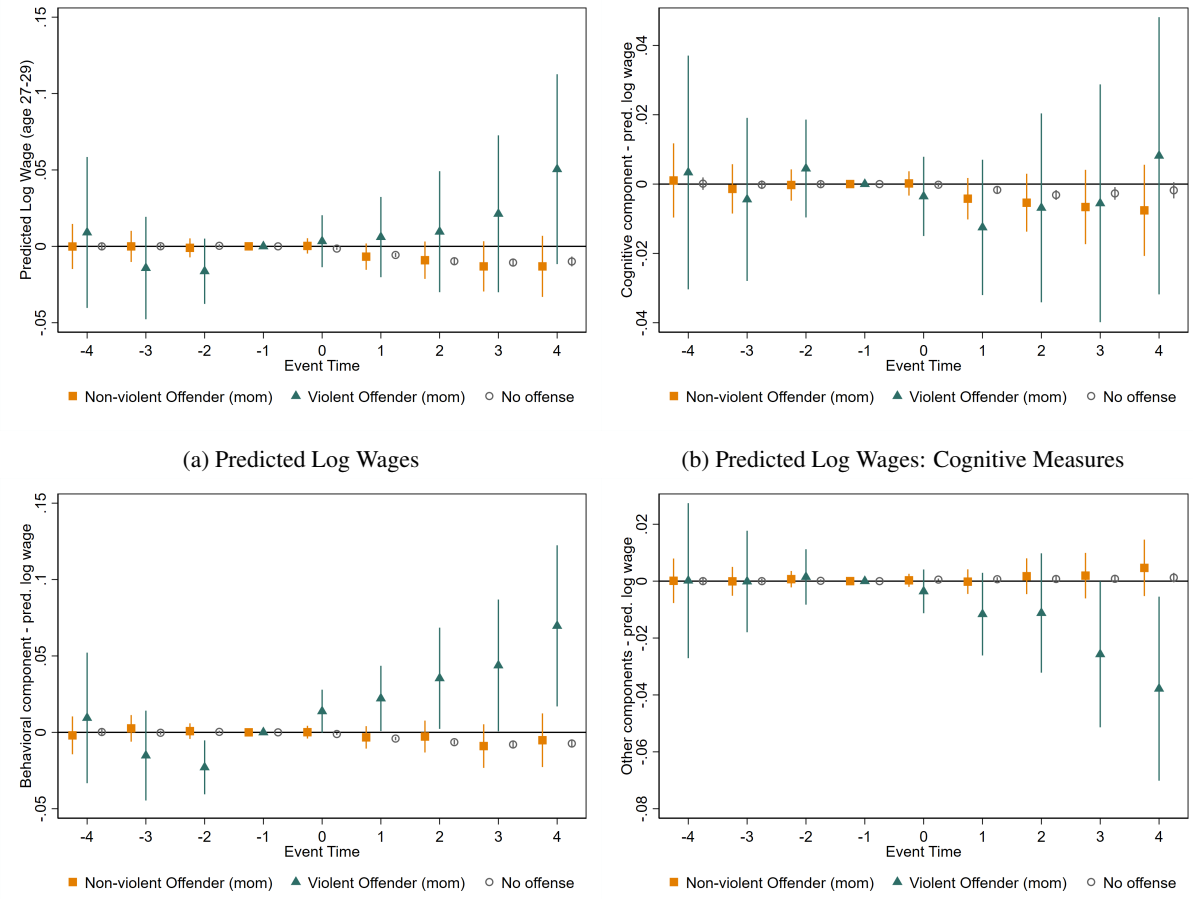
(b) Predicted Log Wages: Cognitive Measures

(c) Predicted Log Wages: Behavioral Measures

(d) Predicted Log Wages: Other Measures

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on predicted earnings, estimated separately for boys and girls. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level. See Table 4 for more details on how predicted earnings are constructed.

Figure A.16: Heterogeneous Effects by Mom's Criminal Conviction status



(a) Predicted Log Wages

(b) Predicted Log Wages: Cognitive Measures

(c) Predicted Log Wages: Behavioral Measures

(d) Predicted Log Wages: Other Measures

Note: These figures present coefficients and 95% confidence intervals from stacked event-study regressions that estimate the dynamic effects of parental divorce on predicted earnings, estimated separately for boys and girls. Each panel plots the estimated coefficients from Equation 2 for a different outcome, spanning four years before through four years after divorce. The specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1985 to 1997, whose parents divorced in expected grades 4-12. Divorces in grades 4-8 serve as treatments and those in grades 9-12 as controls. Standard errors are clustered at the individual level. See Table 4 for more details on how predicted earnings are constructed.

Table A.1: Separation-to-Divorce Interval: Texas versus the Rest of the United States

Divorces finalized 1993–2012

	Uncensored		$\leq$ 10 years	
	Non-Texas	Texas	Non-Texas	Texas
Sample size				
N (marriage records)	2610	184	2528	182
Distribution (months from separation)				
Mean	23.1	21.7	18.2	20.4
Std. deviation	35.8	28.6	20.9	26.2
10th percentile	1.0	2.0	1.0	2.0
25th percentile	5.0	5.0	4.0	5.0
Median	12.0	10.5	11.0	10.0
75th percentile	26.0	23.0	23.2	21.0
90th percentile	53.1	56.7	45.0	55.8
95th percentile	89.0	93.5	65.0	85.9
Cumulative fractions (%)				
$\leq$ 3 months	20.0	15.8	20.6	15.9
$\leq$ 6 months	32.8	34.8	33.9	35.2
$\leq$ 1 year	52.2	59.2	53.9	59.9
$\leq$ 2 years	73.9	76.1	76.3	76.9
$\leq$ 3 years	83.1	81.0	85.8	81.9
$\leq$ 5 years	91.1	90.8	94.1	91.8
$\leq$ mean	72.6	74.5	67.7	74.2

Notes: Data are from the PSID Marriage History File, 1985–2023 (Release 2, December 2025). Sample is restricted to marriages ending in divorce or annulment (MH12=4) for which both a separation date and a divorce finalization date are observed with month-level precision (seasons approximated as the midpoint month of the season), with divorce finalized between 1993 and 2012. Records with separation year before 1950 or divorce year after 2024 are excluded. When both spouses appear as PSID respondents, the marriage record is deduplicated by retaining one observation per unique couple and marriage year (526 duplicate records removed). Separations in 1993– 2012 that had not ended in divorce by the most recent interview in which the marriage was reported (the PSID waves run through 2023; 16% of separations in the window) are excluded by construction. The Marriage History File has no state variable. Each person is linked through the PSID Cross-year Individual File to the interview number of the household they lived in at each wave, and that household's state of residence (FIPS code) is taken from the PSID Family file for the wave nearest the year of separation (12 waves, 1994–2019; ties go to the earlier wave). Only waves in which the person was present in a responding family are used. % of records are matched to a wave within one year of the separation. Records for which no state can be assigned (, mostly persons who had left the panel by 1994) are excluded from both columns. Anchoring instead on the wave nearest the divorce year gives the same state for % of records and Texas divorces. A Kolmogorov–Smirnov test does not reject equality of the two uncensored distributions (D =, p =).

Table A.2: Bounding the effect of parental separation on predicted log wages under selective attrition

	Attrition	Baseline	Assume Attrition Driven by	
Event time	Missing from TEA	Predicted log Wage	Lowest Achievers	Highest Achievers
	(1)	(2)	(3)	(4)
0	-0.0017^{*}	-0.0016^{***}	-0.0016^{***}	-0.0013^{**}
	(0.0008)	(0.0004)	(0.0004)	(0.0004)
1	-0.0055^{***}	-0.0058^{***}	-0.0062^{***}	-0.0050^{***}
	(0.0013)	(0.0007)	(0.0007)	(0.0007)
2	-0.0048^{**}	-0.0099^{***}	-0.0103^{***}	-0.0090^{***}
	(0.0017)	(0.0010)	(0.0010)	(0.0009)
3	0.0025	-0.0104^{***}	-0.0110^{***}	-0.0101^{***}
	(0.0022)	(0.0012)	(0.0012)	(0.0012)
4	0.0109^{***}	-0.0095^{***}	-0.0112^{***}	-0.0084^{***}
	(0.0029)	(0.0016)	(0.0016)	(0.0016)
Observations	1,695,317	828,763	827,831	827,840
Distinct students	131,667	67,418	67,413	67,417

Notes: This table reports the impact of divorce on attrition in each post-divorce year alongside the baseline estimates for predicted log wages and estimates of the predicted log wages that attrition is driven by either the lowest or the highest achievers. Because the direction of differential attrition varies across event times, the group assumed to be missing is adjusted accordingly at each event time. See Table 2 for more details on the sample and specification. Standard errors, clustered at the individual level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Bounding the effect of parental separation on test scores under selective attrition

	Attrition	Baseline	Assume Attrition Driven by	
Event time	Missing from TEA	Standardized Score	Lowest Achievers	Highest Achievers
	(1)	(2)	(3)	(4)
Panel A: Math				
0	-0.0017^{*}	-0.0049	-0.0072^{**}	-0.0018
	(0.0008)	(0.0025)	(0.0025)	(0.0025)
1	-0.0055^{***}	-0.0123^{***}	-0.0207^{***}	-0.0051
	(0.0013)	(0.0032)	(0.0032)	(0.0032)
2	-0.0048^{**}	-0.0227^{***}	-0.0303^{***}	-0.0171^{***}
	(0.0017)	(0.0040)	(0.0039)	(0.0039)
3	0.0025	-0.0135^{**}	-0.0170^{***}	-0.0082
	(0.0022)	(0.0050)	(0.0049)	(0.0049)
4	0.0109^{***}	-0.0044	-0.0148^{*}	0.0143^{*}
	(0.0029)	(0.0066)	(0.0065)	(0.0064)
Observations	1,695,317	1,522,132	1,520,381	1,520,363
Distinct students	131,667	127,878	127,847	127,868
Panel B: Reading				
0	-0.0017^{*}	-0.0062^{*}	-0.0095^{***}	-0.0026
	(0.0008)	(0.0027)	(0.0027)	(0.0026)
1	-0.0055^{***}	-0.0172^{***}	-0.0272^{***}	-0.0081^{*}
	(0.0013)	(0.0033)	(0.0033)	(0.0032)
2	-0.0048^{**}	-0.0168^{***}	-0.0252^{***}	-0.0086^{*}
	(0.0017)	(0.0040)	(0.0040)	(0.0040)
3	0.0025	-0.0053	-0.0099^{*}	-0.0019
	(0.0022)	(0.0050)	(0.0050)	(0.0050)
4	0.0109^{***}	-0.0024	-0.0160^{*}	0.0104
	(0.0029)	(0.0068)	(0.0067)	(0.0067)
Observations	1,695,317	1,529,061	1,527,308	1,527,279
Distinct students	131,667	127,959	127,945	127,950

Notes: This table reports the impact of divorce on attrition in each post-divorce year alongside the baseline estimates for standardized math (Panel A) and reading (Panel B) scores, and estimates under the assumption that attrition is driven by either the lowest or the highest achievers. Because the direction of differential attrition varies across event times, the group assumed to be missing is adjusted accordingly at each event time. See Table 2 for more details on the sample and specification. Standard errors, clustered at the individual level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Mean and variance of divorce effects on predicted log wages: pooled estimates

	(1) Predicted log wages	(2) Cognitive component of predicted log wages	(3) Behavioral component of predicted log wages
Panel A: Average Treatment Effect on the Treated (ATT)			
$E[D_s]$	-0.00500^{***}	-0.00173^{**}	-0.00386^{***}
	(0.000781)	(0.000575)	(0.000659)
Observations	537,192	539,290	538,677
Panel B: Variance of Treatment Effects on the Treated (VTT)			
$\text{Var}(D_s)$	0.000625^{**}	0.0000966	0.000557^{**}
	(0.000234)	(0.000122)	(0.000192)
Observations	537,179	539,288	538,664

Notes: This table reports the average treatment effect, $E[D_s]$ (Panel A), and the variance of treatment effects, $\text{Var}(D_s)$ (Panel B), from a two-stage residual-variance specification (see Section 5.2 for more details), pooled over the post-divorce period. In each panel, the outcome is predicted log wages and its cognitive and behavioral components, respectively. The specification includes grade fixed effects and year fixed effects. The sample includes children born in Texas between 1990 to 1997, whose parents divorced in expected grades 5–12. Divorces in grades 5–8 serve as treatments and those in grades 9–12 as controls. Standard errors are clustered at the individual level. See Figure 4 for more details on how predicted earnings are constructed. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A.5: Share Not Harmed under Alternative Distributional Assumptions (Percent)

	Assumed skewness of treatment effects, γ					
Assumed Pearson kurtosis, κ	-4	-3	$-2^{\dagger}$	-1	0	1
3	—	—	—	51.8	38.4	31.2
6	—	—	65.1	41.7	36.0	32.5
11	—	75.6	46.8	38.1	34.1	31.5
18	0.0	51.1	40.9	35.7	32.7	30.5
25	56.2	46.5	38.3	34.3	31.7	29.8

Notes: Each body cell reports the percentage of children estimated to experience a non-negative effect of parental separation on predicted adult log earnings at event time $s = 2$. Columns vary assumed skewness and rows vary assumed Pearson kurtosis. We hold fixed the estimated mean treatment effect, $\widehat{\mu}_2 = -0.0104861$, and treatment-effect variance, $\widehat{\sigma}^2_{D,2} = 0.0012605$. For each pair (γ, κ), we select the continuous Johnson S_U (unbounded) or S_B (bounded) distribution that matches these four moments and report $100 \times \Pr(D_{i,2} \geq 0)$. † denotes the point estimate of treatment-effect skewness, -2.0. The $(\gamma, \kappa) = (0, 3)$ cell is the normal benchmark. The displayed grid is a sensitivity analysis over selected distributional shapes, not a confidence region. The kurtosis rows include the normal benchmark and values just above the minimum feasible kurtosis for skewness magnitudes one through four. We report no value when $\kappa \leq 1 + \gamma^2$: values below this boundary violate Pearson's moment inequality, while equality is attained only by a distribution supported on two points and is therefore outside the continuous Johnson system. The first four moments do not uniquely determine a CDF; selecting the moment-matched Johnson distribution is therefore an additional shape assumption. The value 0.0 in the $(-4, 18)$ cell reflects a bounded Johnson distribution that places no mass at or above zero, rather than a failed numerical fit.

Table A.6: Sibling correlations in outcome deviations before and after separation

Panel A: Predicted earnings and components

	Predicted log wage (1)	Cognitive component (2)	Behavioral component (3)	Other component (4)
Sibling residual	0.335***	0.250**	0.533***	0.901***
	(0.079)	(0.095)	(0.082)	(0.057)
Sibling residual × post	0.027	0.026	0.000	−0.065
	(0.030)	(0.031)	(0.035)	(0.049)
Observations	33,433	33,565	33,493	33,505
Sibling pairs	4,351	4,356	4,351	4,356

Panel B: Underlying outcomes

	Math score (1)	Reading score (2)	Combined score (3)	Discipline incidents (4)	Days assigned (5)	Days absent (6)	Special education (7)
Sibling residual	0.200**	0.204**	0.251**	0.283***	0.160	0.486***	0.349***
	(0.070)	(0.072)	(0.079)	(0.086)	(0.173)	(0.043)	(0.074)
Sibling residual × post	0.006	0.028	0.037	−0.040	−0.063	0.020	−0.013
	(0.021)	(0.020)	(0.022)	(0.030)	(0.084)	(0.028)	(0.023)
Observations	56,633	57,894	54,600	38,727	38,727	65,724	97,733
Sibling pairs	7,409	7,571	7,124	5,023	5,023	8,669	12,748

Notes: Each column reports an OLS regression of a child's outcome residual on the residual of their sibling, and its interaction with an indicator for post-separation years. Residuals are deviations from the counterfactual trajectory implied by our baseline event-study specification. The first row gives the sibling correlation in the pre-separation period; the second gives the change after separation. All specifications include controls allowing the baseline sibling relationship to vary with grade and sibling age difference. Standard errors, clustered by sibling pair, in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

B Data Matching Details

We construct our analysis sample through a sequence of linkages, each of which matches the parental names recorded on the TBI to names appearing in another administrative source. Every TBI record lists the full name of each parent, including the mother's maiden name. Because these datasets share no common identifier, this pair of names is what allows us to follow the same parents from one source to the next. We use these names first to determine whether a child's parents married, divorced, or were incarcerated, and then to identify siblings within the TBI itself. Before any matching, we clean each name — removing punctuation and extra spacing, standardizing capitalization, and converting accented characters to their plain equivalents — so that names differing only in how they were typed are treated as the same. We then begin with exact matches on the cleaned names and successively relax the criteria: we map common nicknames to their full given name,[1] group names that are spelled differently but sound alike,[2] and allow matches with and without middle initials (or middle name where available).

The linkages differ in the identifying variables each source provides, and Figure B.1 summarizes the sequence along with the fields used at each step. Marriage records report the first and last names of both spouses, including middle initials and the mother's maiden name. We require both parents to appear jointly on the same record, which makes spurious links unlikely.[3]

Divorce records report a narrower set of names. They report first names and middle initials for both parents but last names only for fathers, and they omit the mother's maiden name. To compensate for these missing names, we supplement the divorce match with a second identifier drawn from one of two sources. When a divorce record can be tied to a couple already located in the marriage files, we use the date of the marriage being dissolved, which the divorce record reports. When no such link exists, we match on birth years instead, recovering parental birth years by joining the TBI to the National Vital Statistics System (NVSS) natality data.[4] In our robustness checks, we also implement a version of the divorce match based on names alone. We view this as less reliable, since it matches on the father's full name but only the mother's first name, which risks false matches.

Incarceration records report full names and dates of birth. Because a single name carries considerably less identifying information than a pair of names, we require every incarceration match to agree on the parent's date of birth, either in full or on the birth year alone. The TBI does not record parents' dates of birth, so we recover this information in one of two ways. For parents born in Texas, we link them to their own TBI birth records, which yields a full date of birth. For the remaining parents, we recover an approximate birth year through the same

[1] Cleaning and nickname standardization are both implemented with `abeclean` (Abramitzky et al., 2021), which converts common nicknames to their full given names, replacing "Bill" and "Billy" with "William," for example. This step consults a fixed list of known name pairs rather than applying a rule to the string itself, so names absent from the list are unchanged.

[2] Specifically, we use Stata's `soundex` function, which converts a name into a short code based on how it is pronounced: it retains the first letter and assigns numeric codes to the consonants that follow, grouping letters that sound alike (such as *s* and *z*, or *c*, *k*, and *q*) and dropping vowels. Names sharing a code are treated as the same name, so "Briana" and "Breanna," or "Sanchez" and "Sanches," are matched. Because the code retains the first letter, Soundex cannot reconcile names whose initial letter is different.

[3] Using statewide Texas marriage records from 1966–2019, we find that 98.9% of spousal first–last name combinations are unique, implying that it is extremely unlikely for two unrelated couples to share the same full set of names, even when the individual names are common.

[4] The NVSS natality data is a deidentified file recording each birth's date, the child's sex, the county of birth, and the ages of both parents. While no individual is named, the joint detail across these fields is sufficient for a reliable probabilistic match to a child's birth record, from which we recover each parent's approximate age at the time of birth. Because the data report ages rather than birth years, we translate these into candidate birth years and allow a [-2, +2] window when matching to the divorce records.

NVSS procedure used in the divorce match, matching a parent's child to their birth record to obtain the parent's age at the time of that birth.[5] Finally, we identify siblings if they have the same mother, using maiden last name, and father's name as listed in the TBI. To further enhance accuracy, we use the TEA enrollment data and exclude sibling pairs who attend schools in non-adjacent counties.[6]

Appendix Table B.1 reports the share of records linked at each iteration of the algorithm, separately for each source. Because the iterations run sequentially, a record matched under an earlier rule is not eligible for a later one, so each cell reports the the incremental contribution of that rule. In general, most of the matches are made at the first iteration, and the strictest rules generally account for over 80% of the total. This matters for interpretation, since the rules that carry the greatest risk of false positives contribute a small minority of our links. The one exception to this pattern is that we also allow matches on birth years alone, which recovers additional divorces at later iterations. Our baseline estimates focus on divorces matched on both marriage date and birth years. As discussed in more detail in Section 4.7, Panel (f) of Appendix Figure B.4 confirms that these estimates are unchanged whether we tighten the criterion by requiring the marriage date and an exact name match, or loosen it by adding matches that require neither birth years nor marriage dates. This indicates that our results are not driven by false positives, even under the most permissive matching.

Because most links are made under the strictest rules, our principal concern is undermatching rather than false linkage. Three factors limit our ability to identify true matches. First, our administrative files cover Texas only, so parents who married, divorced, or were incarcerated in another state are classified as never having experienced these events. Second, our algorithm fails to reconcile some inconsistent spellings of parents' names across records, particularly where a name is transcribed in a way that neither the nickname crosswalk nor the phonetic code captures. Third, father's information is missing for approximately 15% of birth records during our study period, which removes the second name the marriage, divorce, and sibling linkages rely on. A missing father's name likely indicates that the parents were unmarried at the time of the birth, however, and couples who never married were not at risk of divorce to begin with. These records are therefore largely ones we would not use in any case, though we do lose the smaller number of parents who married after the birth and later divorced.

It is important to note that failing to identify some children with divorced parents does not threaten the internal validity of our estimates, but rather affects the composition of the population under study and external validity. Because we observe divorces only when they occur in Texas, our sample tilts toward families who remained in the state, and away from those who moved away before dissolving their marriage. Families who remain in Texas throughout the period differ from those who leave in ways that may be related to children's outcomes. Geographic stability is associated with stronger local support networks and with proximity to extended family, both of which may buffer children from the disruption of a parental divorce. If so, our estimates would

[5] Parents who were not born in Texas and whose child cannot be matched to an NVSS birth record have no recoverable birth year and are therefore not searched in the incarceration files. This restriction does not affect our analysis, however, since we use the incarceration linkage only for parents who appear in the divorce records, either to examine heterogeneity in the effects of divorce by whether a parent had a prior conviction or to identify convictions occurring after a divorce. Birth year is available for all such parents by construction.

[6] In this restriction, students are included if they attend schools that are in the same or adjacent counties at least 75% of the time.

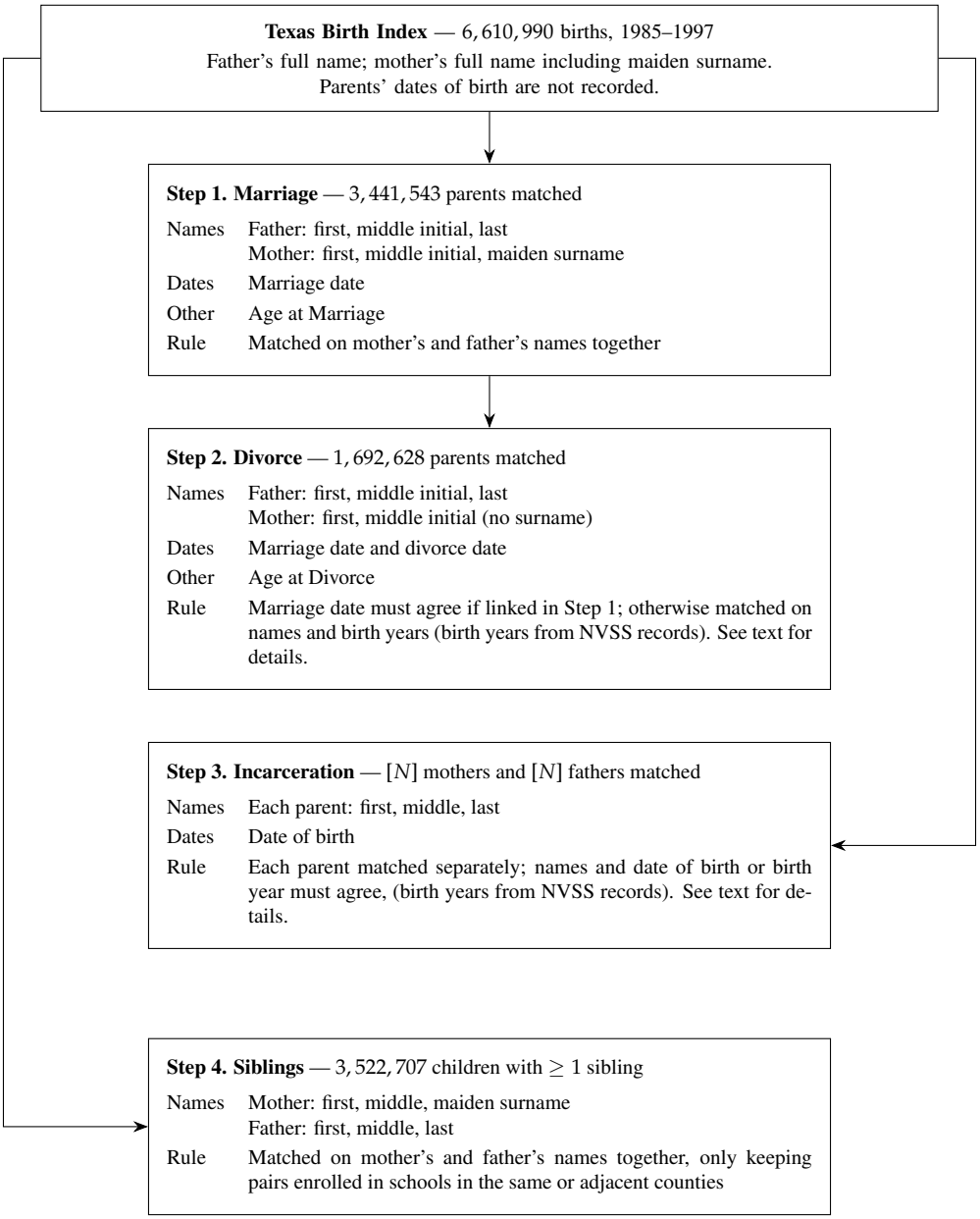
understate the effects of divorce for the more mobile families we do not observe.

Table B.1: Distribution of Matches Across Iterations, by Data Source

Iteration	Marriage	Divorce		Father Conviction		Siblings
		Marriage date	Birth year	Full birthdate	Birth year	
Panel A. Full middle names required						
(1) Exact (%)	—	—	—	51.5	29.3	86.4
(2) Nickname corrected (%)	—	—	—	20.8	11.4	1.3
(3) Phonetic correction (%)	—	—	—	6.4	8.3	4.2
Panel B. Middle initial required						
(4) Exact (%)	78.2	82.0	30.5	3.7	2.0	1.9
(5) Nickname corrected (%)	2.0	1.9	1.8	0.9	10.7	0.1
(6) Phonetic correction (%)	5.5	5.1	3.7	3.9	8.5	0.2
Panel C. No middle name						
(7) Exact (%)	11.4	9.4	25.0	9.1	13.1	4.6
(8) Nickname corrected (%)	0.4	0.3	6.6	1.8	3.4	0.2
(9) Phonetic correction (%)	2.3	1.3	32.4	2.0	13.3	1.9
Cumulative, Panels A–B (%)	85.8	89.0	36.0	87.2	70.2	94.0
Records matched (N)	3,441,543	753,622	939,006	752,355	645,687	3,522,707
Eligible records (N)	6,610,990	6,610,990	6,610,990	6,610,990	6,610,990	6,610,990
% of eligible records	52.1	11.4	14.2	11.4	9.8	53.3
% of matched marriages	—	21.9	27.3	—	—	—
All father convictions, % of eligible				21.1		
All divorces, % of eligible		25.4				
All divorces, % of marriages		48.9				

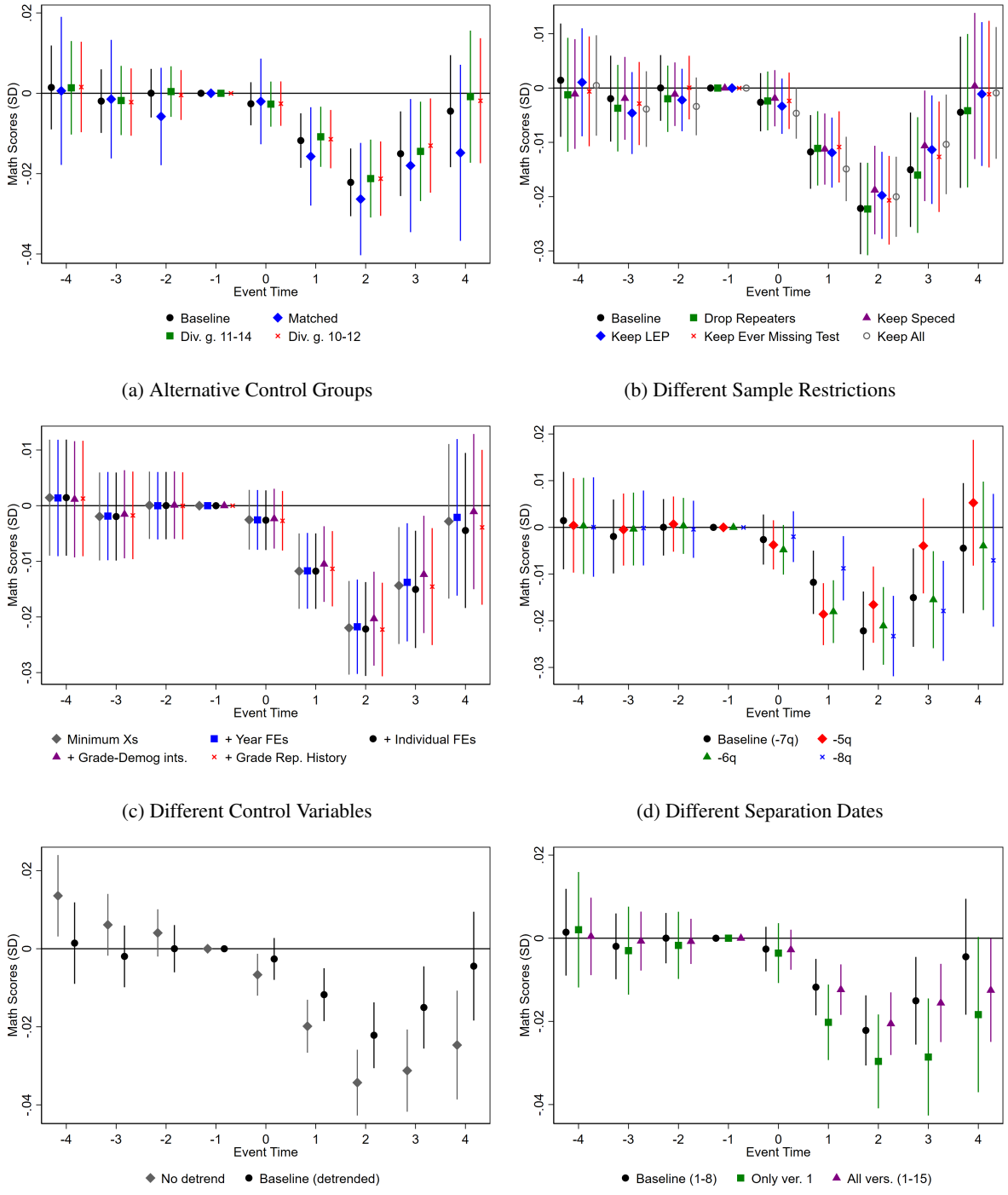
Notes: Each column reports the distribution of matched records across iterations, so entries sum to 100. Percentages are shares of all records matched for that linkage rather than of all records searched. Iterations are applied sequentially, so a record matched under an earlier rule is not eligible for a later one and each cell reports the incremental contribution of that rule. Panels differ in how much of the middle name must agree. Within each panel, iterations relax the standardization applied to first and last names, from exact agreement on cleaned names, to nickname correction, to phonetic (Soundex) correction. Panel A applies only to the incarceration and sibling linkages, whose source records report full middle names, since marriage and divorce records list at most a middle initial. Marriage records are matched on the father's and mother's names jointly. Divorce records are matched on father's full name and mother's first name and middle initial (if available). The two divorce columns apply the same rules twice, first restricting to couples linked to a marriage record and requiring agreement on the marriage date, and then searching all remaining couples on names alone. Because mother's lack maiden names without a marriage date we only match divorce records if middle initial is available. Incarceration is matched separately for each parent, since Texas Department of Public Safety records identify individuals rather than couples, and every incarceration match requires agreement on the parent's date of birth, either in full or on the birth year alone. Dashes indicate rules not applicable to a given source. The row that shows the percentage of divorces as a percentage of all marriages uses 3,441,543 as the denominator.

Figure B.1: Process for linking the Texas Birth Index to marriage, divorce, and incarceration records



Notes: Each box describes one linkage. The *Names* row lists the name fields used in the match, the *Dates* row lists the date fields available in the source file, the *Other* row lists additional fields recorded in that file that are used in a subsequent step, and the *Rule* row states the requirement a candidate pair must satisfy to be retained. Marriage, divorce, and sibling matches require both parents' names to agree on the same record. Incarceration records identify a single individual rather than a couple, so each parent is searched separately and agreement on date of birth is required. The Texas Birth Index does not record parents' dates of birth. For parents born in Texas we recover the full date by linking them to their own birth record; for parents linked in Steps 1 or 2 we infer birth year from the age reported at the time of the event. Parents in neither group are not searched. All matches proceed through the iterative name-standardization procedure described in Appendix Table B.1.

Figure B.2: Robustness of the Effect of Parental Divorce on Math Scores



(a) Alternative Control Groups

(b) Different Sample Restrictions

(c) Different Control Variables

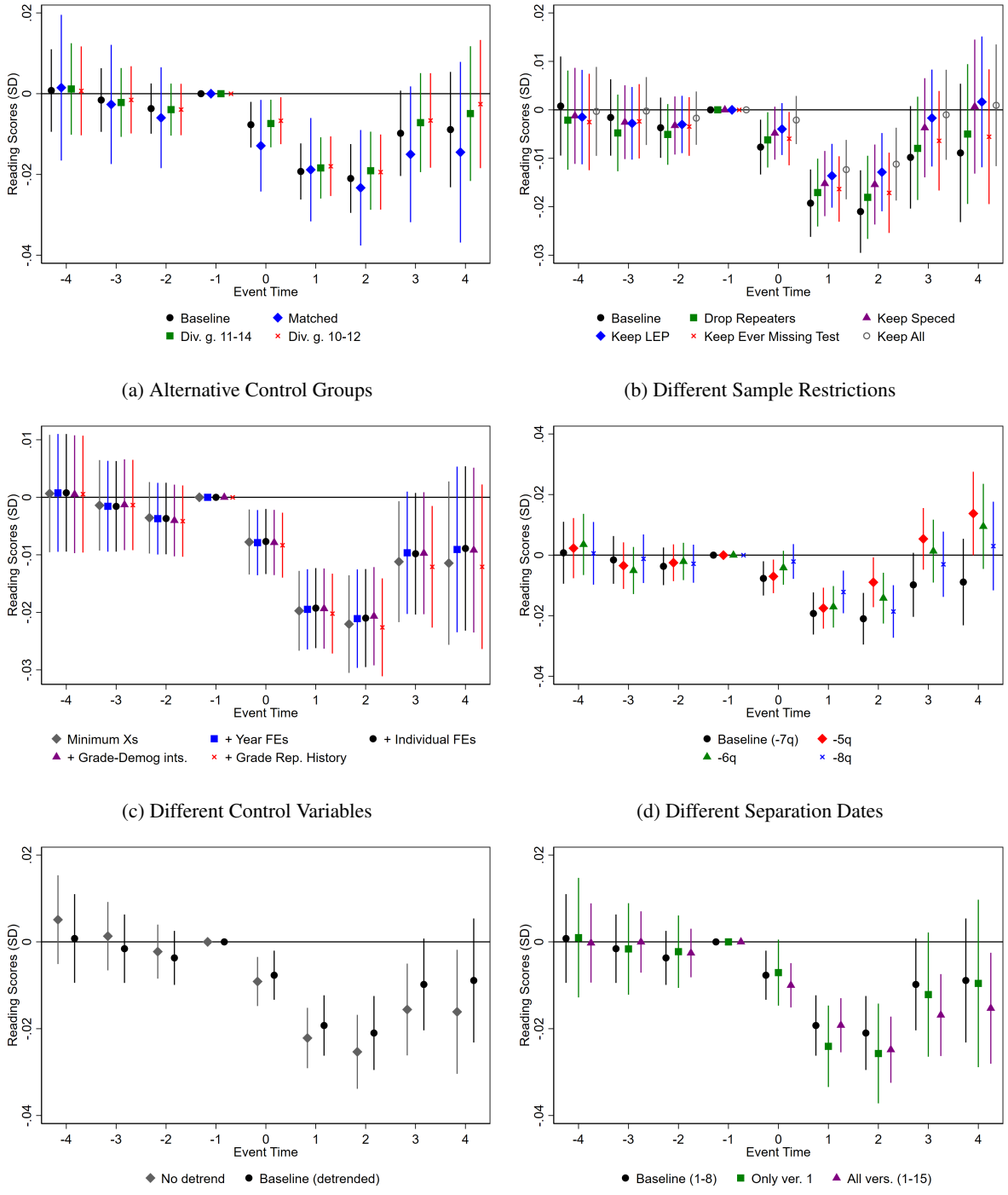
(d) Different Separation Dates

(e) Detrending Sensitivity

(f) Divorce Match Versions

Note: These figures assess the robustness of the estimated effect of parental divorce on predicted log wages. Each panel presents coefficients and 95% confidence intervals from stacked event-study regressions based on Equation 2, spanning four years before through four years after divorce, where each series corresponds to an alternative specification choice: control group definition (Panel a), sample restrictions (Panel b), control variables (Panel c), the timing of parental separation (Panel d), detrending procedure (Panel e), and the version of the divorce match (Panel f). Unless otherwise noted, the baseline specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1990 and 1997 whose parents divorced in expected grades 4–12. Divorces in grades 4–8 serve as treatments and those in grades 9–12 as controls. Standard errors are clustered at the individual level. See Table 4 for details on the construction of predicted log wages.

Figure B.3: Robustness of the Effect of Parental Divorce on Reading Scores



(a) Alternative Control Groups

(b) Different Sample Restrictions

(c) Different Control Variables

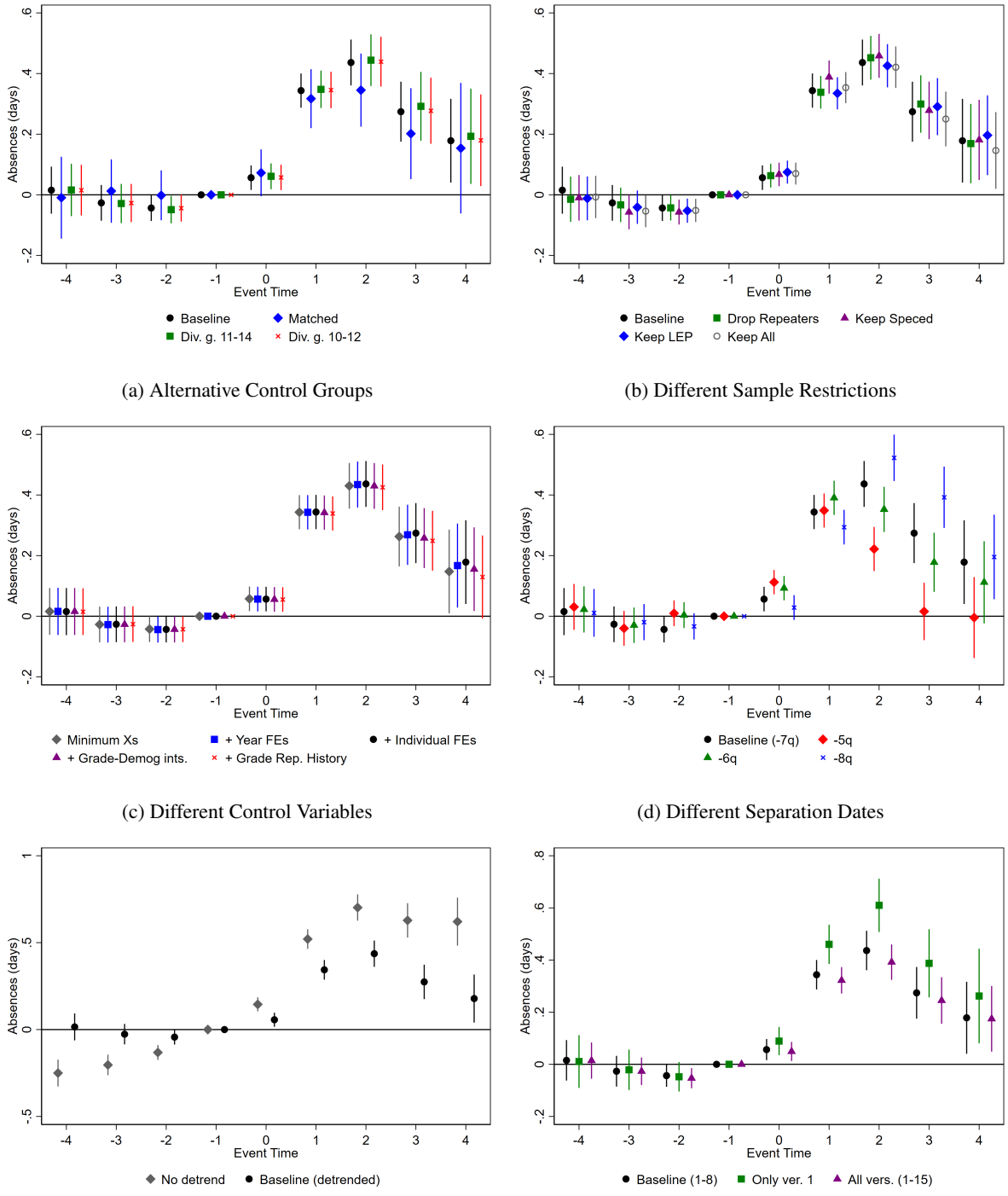
(d) Different Separation Dates

(e) Detrending Sensitivity

(f) Divorce Match Versions

Note: These figures assess the robustness of the estimated effect of parental divorce on predicted log wages. Each panel presents coefficients and 95% confidence intervals from stacked event-study regressions based on Equation 2, spanning four years before through four years after divorce, where each series corresponds to an alternative specification choice: control group definition (Panel a), sample restrictions (Panel b), control variables (Panel c), the timing of parental separation (Panel d), detrending procedure (Panel e), and the version of the divorce match (Panel f). Unless otherwise noted, the baseline specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1990 and 1997 whose parents divorced in expected grades 4–12. Divorces in grades 4–8 serve as treatments and those in grades 9–12 as controls. Standard errors are clustered at the individual level. See Table 4 for details on the construction of predicted log wages.

Figure B.4: Robustness of the Effect of Parental Divorce on Days Absent



(a) Alternative Control Groups

(b) Different Sample Restrictions

(c) Different Control Variables

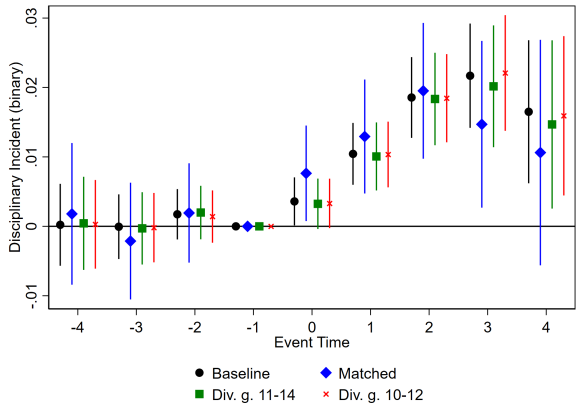
(d) Different Separation Dates

(e) Detrending Sensitivity

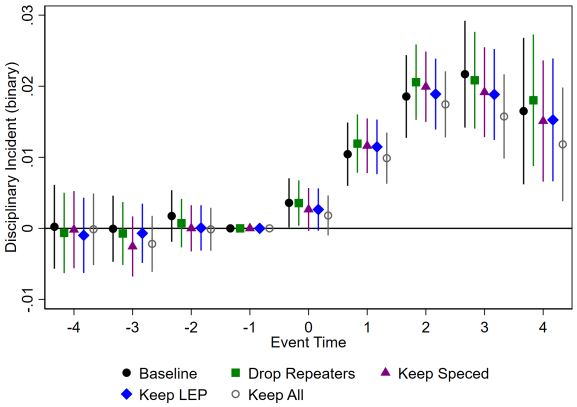
(f) Divorce Match Versions

Note: These figures assess the robustness of the estimated effect of parental divorce on predicted log wages. Each panel presents coefficients and 95% confidence intervals from stacked event-study regressions based on Equation 2, spanning four years before through four years after divorce, where each series corresponds to an alternative specification choice: control group definition (Panel a), sample restrictions (Panel b), control variables (Panel c), the timing of parental separation (Panel d), detrending procedure (Panel e), and the version of the divorce match (Panel f). Unless otherwise noted, the baseline specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1990 and 1997 whose parents divorced in expected grades 4–12. Divorces in grades 4–8 serve as treatments and those in grades 9–12 as controls. Standard errors are clustered at the individual level. See Table 4 for details on the construction of predicted log wages.

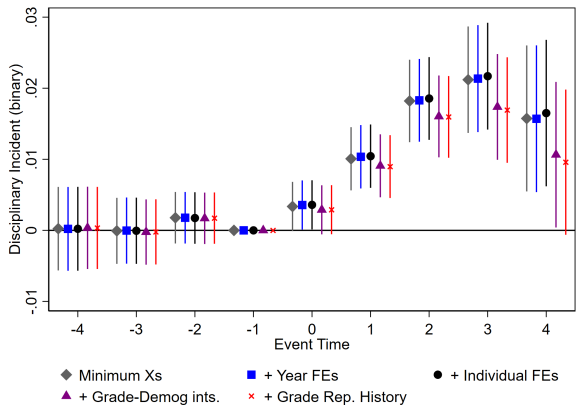
Figure B.5: Robustness of the Effect of Parental Divorce on Ever Being Disciplined



(a) Alternative Control Groups

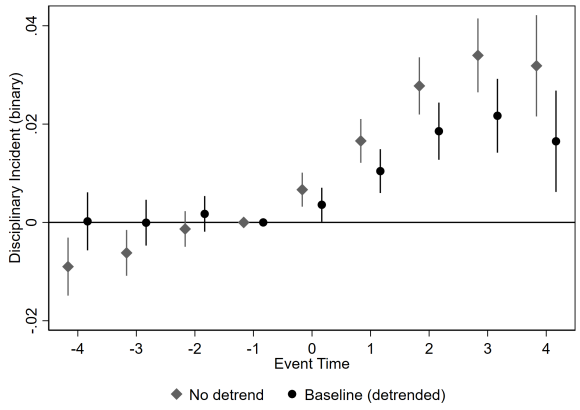


(b) Different Sample Restrictions

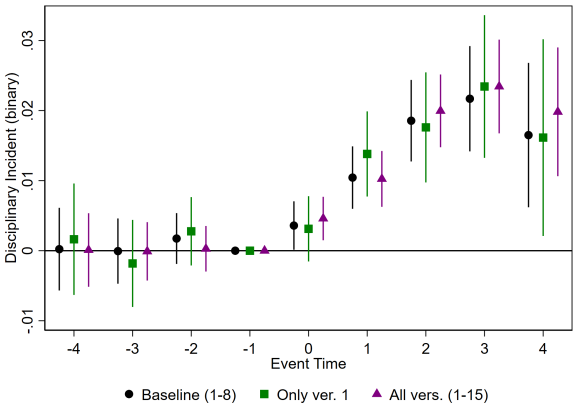


(c) Different Control Variables

(d) Different Separation Dates



(e) Detrending Sensitivity



(f) Divorce Match Versions

Note: These figures assess the robustness of the estimated effect of parental divorce on predicted log wages. Each panel presents coefficients and 95% confidence intervals from stacked event-study regressions based on Equation 2, spanning four years before through four years after divorce, where each series corresponds to an alternative specification choice: control group definition (Panel a), sample restrictions (Panel b), control variables (Panel c), the timing of parental separation (Panel d), detrending procedure (Panel e), and the version of the divorce match (Panel f). Unless otherwise noted, the baseline specification includes individual fixed effects, grade fixed effects, and year fixed effects. The sample includes children born in Texas between 1990 and 1997 whose parents divorced in expected grades 4–12. Divorces in grades 4–8 serve as treatments and those in grades 9–12 as controls. Standard errors are clustered at the individual level. See Table 4 for details on the construction of predicted log wages.