The Heterogeneous Impacts of Divorce on Children's Human Capital Formation*

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Abstract

One in three children experience parental divorce in childhood, yet the effects of divorce remain opaque. We examine divorce effects on children's human capital formation using administrative data linking Texas children's school outcomes to divorce records. Our design compares children whose parents divorce to children whose parents will divorce in the future, using a staggered difference-in-differences framework to address selection. Achievement and attendance begin declining two years before divorce. After divorce, achievement falls by 0.02–0.03σ; math scores, which predict future earnings, remain depressed four years later. School absences and disciplinary actions rise significantly post-divorce and increasingly diverge over time. Using panel data, we develop a method to estimate the variance in divorce effects across children. We find significant heterogeneity in divorce effects on reading scores—with approximately one-third showing non-negative effects—and in absences, but cannot reject uniform negative effects for math achievement and disciplinary outcomes. Across demographic groups, boys, white children, and the non-poor experience more negative effects from divorce.

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"*Each unhappy family is unhappy in its own way.*"

—Leo Tolstoy, in *Anna Karenina*

1 Introduction

The decline of marriage—through lower marriage rates and historically high divorce rates—has coincided with declining economic mobility, raising questions about whether and how parental marriage affects children's outcomes (Chetty et al., 2014b, 2020; Putnam, 2016; Kearney, 2023; Hertegård, 2025). Though the influence of family structure is often opaque due to selection, divorce provides a useful setting to examine its effects, because through divorce we can observe outcomes for the same children in both married and divorced arrangements.

While a large literature documents that divorce disrupts children's development on average (Amato, 2001; Gruber, 2004; McLanahan et al., 2013; Johnston et al., 2025), these average effects likely mask substantial *heterogeneity* across families. The impact of divorce depends critically on two dimensions that researchers struggle to observe: the formative quality of the marriage being dissolved and the arrangements former spouses make after divorce. For children escaping high-conflict or abusive households, divorce may improve outcomes; for those from relatively safe marriages, the loss of parental time and resources, diminished family cooperation, and disrupted social networks may prove more harmful. Yet existing research focuses almost exclusively on average treatment effects, leaving open important questions about whether some children benefit while others are harmed by divorce.

To examine both average and heterogeneous divorce effects, we link the universe of Texas birth records (1976–1997) to detailed marriage, divorce, and school records. We follow more than 6 million children from birth through school, tracking their cognitive and behavioral development across multiple dimensions (including achievement, attendance, discipline, program placement, and mental health). These rich data allow us to observe how the same children's trajectories change around divorce events, identify which children are most affected by family dissolution, and estimate what fraction of divorces actually improve children's outcomes.

Our empirical approach proceeds in two stages. First, we use a stacked event-study difference-in-differences designs to estimate the dynamic average effects of divorce on children's academic and behavioral outcomes. Second, we develop a novel method to move beyond averages and recover the full distribution of divorce treatment effects. By comparing the variance in outcome changes between divorcing families and matched non-divorcing families, we can isolate how much the effects vary across children and calculate what share actually benefit from their parents' divorce.

Our event studies reveal that divorce generates modest negative effects on average. Math and reading scores fall by 0.02–0.03σ following divorce, with effects peaking two years after the divorce is finalized. While reading scores recover within four years, math scores—more predictive of future earnings—remain reduced throughout our observation window. Behavioral outcomes show even larger changes: disciplinary infractions increase by 34 percent relative to the mean by year four, while absences rise by 10 percent. Educational classifications also

shift substantially: special education placement rises steadily, reaching 1 percentage point (7 percent increase) higher post-divorce, while gifted program designation falls by 1.3 percentage points (9 percent decrease). These classification changes may reflect both real changes in performance and reduced parental advocacy for educational placements.

These average effects, however, may conceal variation across families. Using our variance decomposition method, we test whether divorce impacts all children equally or whether effects vary substantially across the distribution. Our results reveal a mixed picture: for reading scores, we find significant heterogeneity—even at the peak negative effect two years post-divorce, 34 percent of children experience non-negative effects while an equal share experience effects exceeding twice the mean. We also find significant heterogeneity in the effects of divorce on school absences. In contrast, for math scores and disciplinary outcomes, we cannot reject that divorce affects all children uniformly negatively. This pattern suggests that while some children may escape harm in specific domains, divorce appears broadly detrimental to mathematical achievement and behavioral development—outcomes that are particularly predictive of long-term success.

We validate our heterogeneity findings through two complementary tests. First, we examine whether siblings within the same divorcing family experience similar outcome changes around divorce. If family-specific factors drive heterogeneity—such as the quality of the dissolved marriage or post-divorce arrangements—siblings should show correlated effects. We find that siblings experience similar changes in reading scores following divorce, consistent with family-level variation in divorce effects. Second, we test whether pre-divorce parental characteristics predict the magnitude of divorce effects. Using criminal records to construct an index of parental antisocial behavior, we find that children whose parents have higher antisocial behavior show smaller negative (or even positive) effects from divorce, suggesting that dissolution of more dysfunctional marriages may be less harmful or even beneficial for children.

This heterogeneity follows systematic patterns revealing which children are most sensitive to parental divorce. Boys experience substantially larger negative effects than girls across multiple domains: at peak, boys' math scores decline by approximately twice as large as girls' (0.04σ versus 0.02σ), though reading scores show similar declines. The behavioral consequences are even more divergent—boys show significantly larger increases in disciplinary infractions following divorce, suggesting that family instability may be especially disruptive for boys' behavioral development and self-regulation.

Race is another dimension showing significant heterogeneity in divorce effects. White children experience the largest negative effects from divorce, with math scores declining by 0.05σ at peak, while the estimates for black and Hispanic children are near zero and statistically insignificant. White children are also significantly more likely to be placed in special education following divorce. However, the pattern reverses for behavioral outcomes: Black children show the largest increases in disciplinary problems following divorce, while Hispanic children show near-zero effects across all outcomes. This heterogeneity likely reflects differences in how divorce affects economic resources, parental availability, or other aspects of the home environment across racial groups.

Perhaps most surprisingly, we find that children from non-poor families suffer larger negative effects from divorce than poor children, both in test scores and disciplinary outcomes. Children not eligible for free or reduced-price lunch experience math score declines nearly twice as large as those eligible for subsidized lunch. This pattern—which appears across multiple outcomes—suggests that children in higher-income families may have more to lose from divorce, whether through greater disruption to resources, more dramatic lifestyle changes, or the loss of larger parental investments in education.

Our study contributes to a growing literature using administrative data to examine divorce effects on children. Recent work has attempted to address these challenges using quasi-experimental designs. 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. Subsequent studies have developed novel approaches using other sources of plausibly exogenous variation to study divorce. Hertegård (2024) finds that thresholds restricting access to divorce improves children's human capital formation on average. And 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.

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.

This paper makes four primary contributions to the literature on family structure and child development. First, we develop a novel econometric approach for identifying treatment effect variance that can be applied broadly to settings with panel data. Our method exploits differences in outcome variance between treated and control groups to recover the distribution of treatment effects—an insight that extends beyond divorce to any setting where researchers seek to understand treatment effect heterogeneity. Second, 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. Third, we provide the most comprehensive evidence to date on divorce in the United States. Most existing evidence comes from Northern European countries with different institutional contexts; U.S. setting shows similar effects to those reported for Denmark in Holm et al. (2024). Fourth, 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 complete picture of how divorce shapes child development.

Our findings have implications for both research and policy. While we document meaningful heterogeneity in some outcomes—particularly in reading scores—the more striking pattern is that divorce appears broadly

harmful across multiple dimensions of child development, with math scores and behavioral outcomes showing persistent negative effects for most children. The systematic differences we identify across gender, race, and income groups suggest that some children are particularly vulnerable to family dissolution: boys, white children, and those from higher-income families bear the largest costs. Perhaps most importantly, our novel method for estimating treatment effect variance provides researchers with a new tool for moving beyond average effects in any setting with panel data—revealing not just whether a treatment works on average, but how much its effects vary across the treated population.

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.

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. This standardized schedule—applied in approximately 80% of cases—means most children experience similar disruptions to father involvement (Texas Legislature, 2023). Joint managing conservatorship, Texas's version of joint legal custody, is presumed but grants primary residence to one parent, typically the mother.

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 imperfect, with only 60% of

custodial parents receiving full payment (Grall, 2020). Though we cannot observe custody arrangements or child support payments in our data, heterogeneity in these may drive differences in the effects of divorce across families.

2.3 Conceptual Frameworks for Divorce Effects on Children

Though not the subject of this paper, the literature identifies several mechanisms through 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 fully offset the economies of scale lost when maintaining two households and in about half of cases 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 should 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 profound 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, harsher or more inconsistent discipline, and diminished emotional attunement to children's needs.

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 ongoing 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 should 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.

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 1968 to 2017. By probabilistically linking birth records to marriage, divorce, education, and criminal justice records, we construct longitudinal histories for over 6 million children and their parents. This data infrastructure 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.

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, achieving a match rate of 78% with an estimated false positive rate below 2% (see Appendix X for validation details).

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 marriage and divorce dates, 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 where available.[1] 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.[2] 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 persistent effects of divorce on children's development.

Criminal Justice Records: While not used in our main analysis, these records enable crucial heterogeneity tests. 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 pre-existing family dysfunction, as criminal behavior provides a plausible marker of marriages that may be less beneficial for child development. Specifically, we can identify households likely characterized by violence or antisocial behavior and examine whether children from such families experience different—potentially even positive—effects from parental separation.

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 incomparable over time.[3] 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. Among our sample, X percent of divorces occur before grade 4, Y percent occur during grades 4-8 (our treatment window), and Z percent occur after grade 8.

To ensure clean identification, we define our treatment group as children whose parents divorce between grades 4–8 (when we consistently observe outcomes) and our control group as children whose parents divorce in grades 9–12. This design compares families that all eventually divorce, differing only in timing. Throughout, we use expected rather than actual grade—defined as grade progression assuming no retention—to avoid mechanical

[1] 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.

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

[3] Including these students yields similar results (see Section 4.5).

correlation between treatment assignment and grade repetition.[4] Table 1 presents summary statistics comparing our treatment and control groups across demographic characteristics and baseline outcomes.

3.3 Summary Statistics

Table 1 documents our data linkage process and demonstrates approximate balance between the treatment and control groups. Column 1 shows all Texas public school students, while Column 2 presents the 76 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.

Columns 4–7 stratify by family structure. Comparing always-married families (Column 4) to those that eventually divorce (Column 5) reveals expected selection patterns: children whose parents eventually divorce show modestly lower baseline achievement and higher rates of economic disadvantage in 3rd grade. However, these differences are relatively small—approximately 0.10σ in test scores and 8 percentage points in FRL eligibility.

Most importantly for our identification strategy, children whose parents divorce during grades 4–8 (our treatment group, Column 6) and grades 9–12 (control group, Column 7) are remarkably similar at baseline. The groups differ by only 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 supports 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 average dynamic effects of parental divorce on student outcomes using a stacked event-study design. This approach allows us to trace out how children's outcomes evolve before, during, and after their parents' divorce, while addressing potential selection bias by comparing early divorces to later ones.

Specifically, we estimate:

[4] To account for any test score advantages that retained students may have from being older than their grade-level peers, we include grade-by-repetition fixed effects, described in detail in our empirical specification.

$$Y_{ige} = \sum_{e=-4}^{-3} \gamma_e \cdot \mathbf{1}(e) \cdot D_i + \sum_{e=-1}^{4} \alpha_e \cdot \mathbf{1}(e) \cdot D_i + \eta_i + \lambda_t + \varepsilon_{ige} \tag{1}$$

where Y_{ige} denotes an outcome (e.g., test score) for student i measured in event time e. Event time is defined as $e = g - d$, the number of grades before or after divorce occurring in grade d. The indicator D_i equals 1 if the child is in the treated group (parents divorced in grades 4–8) and 0 if in the control group (parents divorced in grades 9–12). The coefficients γ_e capture pre-event dynamics up to four years before divorce, while α_e estimate the immediate and longer-run effects of divorce. We omit $e = -2$ as the reference category, since the year immediately preceding divorce may already be affected by separation, filing, and emerging conflict.[5]

All specifications include individual fixed effects η_i to account for time-invariant student characteristics, grade fixed effects λ_g and year fixed effects λ_t to absorb common shocks or yearly changes in the difficulty of the test. We also include grade-by-repetition-history fixed effects to account for any test score advantages retained students may have from being older than their grade peers.[6] Standard errors are clustered at the individual level to account for serial correlation.

To implement the stacked design, we form treatment cohorts by the grade in which divorce occurs (grades 4–8). For each treated cohort, we create a matched control group of students whose parents divorced in grades 9–12, assigning them the same placebo divorce grade to align outcomes in event time. This ensures all comparisons are made between treated and control students at the same event time relative to their (actual or placebo) divorce.

Our identifying assumption is that, absent divorce in grades 4–8, treated students would have followed similar trends to those whose parents divorced later. For outcomes like math and reading scores, we observe parallel pre-trends, supporting this assumption. For other outcomes like absences, we observe effects beginning before the legal divorce itself. We interpret these pre-divorce changes as part of the broader divorce process—capturing family disruption, conflict, and anticipatory effects that precede legal separation. Rather than violating our identification strategy, these patterns reveal that divorce's impact extends beyond the formal legal event to include the deteriorating family dynamics that precede it.[7]

4.2 The Average Effect of Divorce on Achievement

We begin by examining how divorce affects achievement in grades 4-8. Panel (a) of Figure 1 plots the estimated coefficients from Equation 1 on math test scores, and Panel (b) shows the same for reading scores. Full regression results appear in Table 2.

Math scores show statistically insignificant pre-trends in the three to four years before divorce. Scores begin

[5] Many parents delay informing children about divorce until separation is certain, hoping to shield them from uncertainty (Hogan et al., 2002). The average divorce is finalized in 11 months, with 90% finalized within 2 years of filing. Our results are robust to using filing rather than finalization dates.

[6] Specifically, for each student i in year t, we interact current grade g_{it} with their cumulative repetition history $\mathcal{H}_{it}$, ensuring comparisons are among students in the same grade with the same retention history.

[7] In Section 4.5, we also present estimates using the pre-trend–adjusted estimator of Freyaldenhoven et al. (2021), which isolates the effect of formal divorce from pre-existing trajectories.

declining two years before divorce, drop sharply during the year of divorce, and continue falling through two years after divorce, when the cumulative decline reaches 0.03 student standard deviations. Math scores recover slightly thereafter but remain negative and statistically significant up to four years after divorce. Reading scores show stable pre-trends, with scores declining sharply in the year of divorce and registering their largest decline of 0.023 standard deviations one year after divorce. Though the drop occurs more quickly than in math, it is slightly smaller in magnitude. Reading scores recover more quickly, becoming statistically indistinguishable from the control group three years after divorce. The insignificant pre-trends in both subjects suggest that these declines reflect the causal impact of divorce rather than pre-existing trends between divorcing and non-divorcing families.

The math effect of 0.03 student standard deviations is approximately 70 percent of the effect of reducing class size by 10 students Cho et al. (2012), or about 20% of the effect of increasing teacher value-added by one standard deviation for a year (Rockoff, 2004; Chetty et al., 2014a).[8] These comparisons suggest the effects are educationally meaningful, with potential long-term economic consequences. If we assume that divorce-induced achievement changes have similar long-run effects as teacher-induced changes, the math decline would translate to approximately a 0.26 percent reduction in earnings at age 28, though it's unclear how the effects from multiple years would aggregate.[9]

The differential recovery patterns may reflect differences in skill accumulation across subjects. For instance, if math learning is more cumulative and sequential (as suggested by Claessens and Engel, 2013; Träff et al., 2025), then disruptions to skill formation–even if only temporary–can create lasting gaps in foundational material that compound over time. Reading skills may depend less on strict sequential mastery, allowing for easier recovery once stability returns.

4.3 Effects on School Attendance, Discipline, and Grade Retention

We next examine whether divorce affects student behavior in grades 4-8, including school attendance, disciplinary incidents, and grade repetition. Figure 2 displays results for days absent, truancy (>10 days absent), disciplinary incidents, and grade repetition.

Absence-related outcomes start increasing significantly two years before the divorce is finalized, rising most sharply in the year of divorce itself. Two years after the divorce, rising absences stabilize at around 0.6 additional days absent per year (a 10 percent increase). Truancy follows a nearly identical pattern, with children becoming 3 percentage points (18 percent) more likely to miss more than 10 days by one year post-divorce, an effect that persists through four years after divorce.

Disciplinary outcomes also rise substantially after divorce, with small, usually insignificant pre-trend coefficients several years before divorce. Both the likelihood of being disciplined (extensive margin) and days disciplined (intensive margin) begin increasing substantially one year before divorce and continue rising steadily.

[8] They report that a 1σ increase in teacher value-added corresponds to a 0.16 student standard deviation increase in test scores.

[9] Chetty et al. (2014a) find that a 1 standard deviation increase in teacher value-added translates to a 1.3 percent increase in earnings at age 28.

Four years post-divorce, children are 3.73 percentage points (34 percent) more likely to receive any disciplinary action, and days disciplined increases by 0.75 days—an 82 percent increase relative to the mean.

Grade retention, though rare (affecting 0.75 percent of students each year), increases by approximately 0.5 percentage points (66 percent) four years after divorce. This represents a substantial effect given baseline rates and aligns with the test score declines documented earlier.

These patterns suggest multiple channels through which divorce may disrupt schooling. Increased absences and truancy may reflect logistical challenges in getting to school when parents establish separate residences, a reduced focus on academic engagement, an increase in child illness, reduced parental monitoring, or children's behavioral responses to family stress. Rising disciplinary incidents could indicate emotional distress or reduced behavioral regulation following family dissolution. The increase in grade retention is consistent with students falling sufficiently behind academically to trigger formal retention decisions.

4.4 Divorce Effects on Educational Classifications

We next examine how divorce affects educational classifications that determine resource allocation: special education (SpEd) and gifted program participation. Changes in these classifications may reflect both real shifts in student needs and strategic responses by schools and families to channel resources toward children experiencing family disruption. Figure 3 presents event study estimates for each classification.

Special education participation (Panel a) shows no pre-trend effects but increases steadily post-divorce, reaching 1 percentage point by year four—a 7 percent increase relative to the 11 percent baseline. This delayed response aligns with the lengthy SpEd determination process in Texas, which requires extended monitoring, evaluations, and committee meetings before classification changes. The increase may reflect both genuine effects—with divorce-related stress pushing borderline students over diagnostic thresholds—and institutional responses, as schools may strategically use SpEd classifications to provide additional support after family transitions.

Gifted program participation (Panel b) shows the sharpest response, declining by 1.3 percentage points (9 percent decrease) within four years of divorce. With flat pre-trends and declines beginning immediately post-divorce, this pattern may reflects multiple mechanisms: students falling below eligibility thresholds as academic performance declines and reduced parental advocacy for program placement. The asymmetry between increases in remedial services and decreases in enrichment programs is particularly striking.

4.5 Robustness

Our results prove robust to multiple specification choices, alternative control groups, sample restrictions, and matching criteria, strengthening confidence in our findings.

Alternative Control Groups: Panels (a) and (b) of Appendix Figure 1 compare our main specification to three alternatives. First, an extended control group including divorces up to two years post-graduation yields similar results but with slightly less parallel pre-trends, consistent with these divorces occurring further from our

treatment window. Second, a restricted control group limited to 11th-12th grade divorces—addressing concerns that 9th-10th grade divorces may show anticipatory effects—produces comparable estimates. Third, a matched control group constructed through exact matching on demographics and baseline achievement exhibits improved pre-trends and slightly larger negative effects.[10] The similarity across specifications, particularly with the matched sample showing larger effects, suggests our main estimates are conservative.

Sample Restrictions: Panels (a) and (b) of Appendix Figure 2 demonstrate robustness to sample composition. Our baseline excludes special education and LEP students while including grade repeaters. Results remain virtually unchanged when including SpEd/LEP students or excluding repeaters, confirming that our findings are not driven by sample selection decisions.

Matching Criteria: Since we lack unique identifiers linking children to divorces, we rely on probabilistic matching using names and birth years. Panels (a) and (b) of Appendix Figure 3 vary these criteria. A stricter match requiring marriage date alignment produces slightly better pre-trends and larger negative math effects, suggesting our baseline may be attenuated by measurement error from false matches.[11] Conversely, relaxing birth year requirements yields nearly identical estimates. This pattern indicates our main results represent conservative lower bounds.

Selective Attrition: Appendix Figure 4 examines whether divorce-induced attrition biases our estimates. Reassuringly, conditional on enrollment, divorce does not affect test score data presence (Panels A-B)—important given divorce's impact on SpEd/LEP classifications that sometimes exempt students from testing.

Panel C reveals an interesting pattern for school enrollment: divorce initially reduces exit rates, then increases them three to four years post-divorce by 1.3 percentage points (19 percent). The initial reduction likely reflects court-imposed mobility restrictions during proceedings, while the later increase may stem from economic pressures or post-custody relocations. To assess whether this differential attrition explains our results, we implement Lee (2009) bounds by trimming 1.3 percent of control observations with the most extreme test scores. Even under this conservative approach assuming maximal selection, negative divorce effects persist, confirming that attrition cannot explain our findings.

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 considerably across families. For some children, divorce may remove exposure to conflict or an abusive parent, potentially improving outcomes. For others, the disruption may be particularly severe, causing larger declines than the average suggests.

[10] The matched sample uses one-to-one exact matching on sex, race, third grade FRL status, and third grade math and reading score deciles. Within strata where treated and control counts differ, excess observations are randomly dropped.

[11] We don't use this stricter match as our baseline because it excludes divorces lacking matched marriage records.

We develop a novel method to estimate not just the mean but the full distribution of treatment effects, allowing us to quantify what fraction of children actually benefit from their parents' divorce.

Our method differs fundamentally from existing approaches to heterogeneous treatment effects. Quantile treatment effects (Bitler et al., 2006) reveal how treatment reshapes the outcome distribution, but not about the variance in individual treatment effects. Machine learning methods (Athey and Imbens, 2016; Wager and Athey, 2018) predict heterogeneity based on observable characteristics, yet much variation in divorce effects likely stems from unobservables—relationship quality, parental conflict style, or post-divorce cooperation. We seek to recover the total variance in treatment effects, capturing heterogeneity from both observable and unobservable sources.

Our approach is most similar to structural models that recover treatment effect distributions (Heckman et al., 1997), but with key advantages. While structural approaches require specifying and estimating a full behavioral model, we need only panel data and a comparison group. Recent work by (Ding and Li, 2019) uses randomization inference to bound treatment effect variance, but requires experimental variation we lack. Perhaps closest is (Frandsen and Lefgren, 2021), who estimate the treatment effect distribution in an experimental evaluation of KIPP charter schools.

A key advantage of our method is that it does not require rank stability or copula stability assumptions common in the heterogeneous treatment effects literature. These assumptions require that treatment does not change individuals' relative positions in the outcome distribution—a restriction unlikely to hold in settings with heterogeneity in treatment effects. If divorce effects are truly different, children experiencing large negative effects should fall in the achievement distribution while those experiencing smaller or positive effects may rise. Our variance-based approach allows for such reordering, recovering the total variance in treatment effects from both observable and unobservable sources.

By comparing outcome variance between treated and control units over time, we provide the first application of variance decomposition to estimate heterogeneous effects of family structure changes.

5.1 Methodology for Recovering Divorce Effect Heterogeneity

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

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

$$d_{i,s} = A_{i,s} - A_{i,-1} \tag{2}$$

Here, $d_{i,s}$ represents the change in achievement percentile from two years before divorce ($t = -2$) to s years after divorce for student i. We use $t = -2$ rather than $t = -1$ as our baseline because the typical divorce takes 11 months from filing to finalization, meaning year $t = -1$ often captures a period when divorce proceedings are already underway. Achievement measures are standardized to have a mean of 0 and a standard deviation of 1 within grade-year cells so yearly fluctuations in test difficulty do not affect our measures. Using the additivity rule for variance, we find:

$$\text{Var}(d_{i,s}) = \text{Var}(R + D) = \text{Var}(R) + \text{Var}(D) + 2\text{Cov}(R, D) \tag{3}$$

An important conceptual clarification concerns what constitutes the causal effect of divorce (D) versus random variation (R). Any changes caused by divorce—and the mechanisms through which they operate—are part of D, not R. Some of the heterogeneity in divorce effects operates through differences in how family life changes: differences in exposure to parents, changes in household income, or changes in the level of family cooperation. Other sources of heterogeneity may operate through changes in neighborhood, school enrollment, or responses by neighbors, schools, extended family, and the community. For instance, if divorce forces a family to move to a lower-quality school district, the resulting achievement decline is a mechanism through which divorce affects children, contributing to treatment effect heterogeneity. Similarly, if divorce triggers a school to reassign a struggling child to a different teacher, that reassignment and its effects are part of D. What R captures is variation genuinely independent of divorce: the routine teacher assignment process within a school, test-day performance fluctuations, or classroom disruptions unrelated to family circumstances.[12]

Our identification strategy relies on a simple insight: if divorce effects are heterogeneous, test score changes should exhibit greater variance among divorcing families than among intact families experiencing only random fluctuations. By measuring this excess variance—the difference between treatment and control group variances—we can isolate $\text{Var}(D)$. If divorce affected all children identically, both groups would show equal variance. This approach requires two identifying assumptions.

Assumption 1 (Independence):

The component of random test score fluctuations is independent of individual divorce effect heterogeneity: meaning, $R \perp D$.

This assumption rules out scenarios where children who would benefit most from divorce systematically experience different random shocks than those who would be harmed or unaffected. It would be violated if

[12] A related question is whether divorce might make children more susceptible to random shocks—for instance, if parental distress reduces a child's ability to cope with a difficult teacher assignment or disruptive classroom event. We view such changes in susceptibility as part of the causal effect of divorce. If divorce increases some children's vulnerability to environmental variation, the resulting dispersion in outcomes reflects heterogeneity in how divorce affects children. Our pre-period variance tests address whether such susceptibility differences exist *before* divorce occurs; the similarity of pre-treatment variances suggests they do not, meaning any post-divorce differences in variance reflect causal effects of divorce rather than pre-existing group differences.

unrelated factors coincided with divorce, such as if psychologically resilient children (who are less affected by divorce) happened to receive better teachers during their parents' divorce, or if children in high-conflict homes (who might benefit from divorce) happened to have worse test-day luck.[13] Under this assumption, the correlation between R and D is zero, allowing us to cleanly decompose the observed variance in test score changes:

$$\text{Var}(d_{i,s}) = \text{Var}(R + D) = \text{Var}(R) + \text{Var}(D) \tag{4}$$

Without independence, the decomposition would include a covariance term: $Var(R + D) = Var(R) + Var(D) + 2Cov(R, D)$. Since we never jointly observe both R and D for the same child, this covariance is unidentified. The independence assumption resolves this by setting it to zero. 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.

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 random achievement fluctuations is the same for divorcing and matched intact families: that is, $Var(R_{divorce}) = Var(R_{intact})$ *for the chosen control set.*

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. Importantly, like the standard parallel trends assumption, Assumption 2 is testable using pre-treatment data.

We test Assumption 2 by comparing pre-treatment variance in test score changes. If the treatment and control groups have similar variance in test score changes before treatment, it implies that in the absence of divorce, control variance is a good proxy for treatment variance. We compare the variance of score changes between $t = -3$ and $t = -2$ for divorcing families to that of controls. Figure XXX shows these pre-period distributions are remarkably similar, supporting the plausibility of common random variation.

[13] Importantly, this assumption is not violated if struggling children (who are more negatively affected by divorce) receive different teachers *because* the divorce triggers school responses (e.g., special education placement or assignments to teachers more effective at helping struggling students). These changes are part of the divorce effect itself and therefore contribute to D, not to the residual shock R.

This assumption allows us to infer $\text{Var}(R)$ from control families who later undergo divorce. We construct a sample of children whose parents remained married throughout the treatment period but divorce after our observation window ends, and we assign each intact family a pseudo-divorce date corresponding to their matched divorcing family. For each such child in the control group, we then calculate:

$$r_{i,s} = A_{i,s} - A_{i,-1} \tag{5}$$

where timing is relative to the pseudo-divorce date. Under Assumption 2, $\text{Var}(r_{i,s})$ provides an unbiased estimate of $\text{Var}(R)$. Our estimator for the variance of divorce effects is thus:

$$\widehat{\text{Var}(D_s)} = \text{Var}(d_{i,s}) - \text{Var}(r_{i,s}) \tag{6}$$

A complication arises when applying this estimator to pooled data containing multiple demographic groups. If different groups exhibit both different baseline volatility in test scores and different average divorce effects, the total variance of $d_{i,s}$ reflects not only individual-level heterogeneity within groups but also systematic differences in average effects across groups. For example, if boys experience both higher random variation in achievement and larger average divorce effects than girls, the pooled variance mixes these group-level differences with genuine individual-level heterogeneity. To isolate the individual-level treatment effect variance that is our focus, we residualize all achievement changes on observable characteristics before calculating variances. Specifically, we regress $d_{i,s}$ and $r_{i,s}$ on indicators for gender, race/ethnicity, and free/reduced-price lunch eligibility, then use the residuals in equation (5). This ensures our estimator captures within-group variance in divorce effects rather than between-group differences in average effects.

As a robustness check, we also estimate equation (5) without residualization and find very similar results. For instance, although boys exhibit both different average divorce effects and different baseline variability than girls, our estimated treatment effect variance is nearly identical whether we residualize on sex or not. This suggests that between-group differences in average effects are small relative to within-group heterogeneity in our setting, making our main estimates relatively insensitive to the residualization choice. While this does not guarantee that unobservable group differences would be similarly benign, it provides reassurance that our measure of individual-level heterogeneity is not primarily driven by compositional differences across observable demographic groups.

This difference-in-variance approach extends naturally to higher moments of the treatment effect distribution. In a companion paper, we develop analogous estimators for the skewness and kurtosis of D, allowing us to characterize the full shape of treatment effect heterogeneity (**?**). Here we focus on variance as our primary measure of heterogeneity.

We estimate $\widehat{\text{Var}(D_s)}$ separately for each post-divorce year s, allowing treatment effect heterogeneity to evolve over time. To assess whether our approach spuriously detects heterogeneity in the absence of treatment, we conduct a placebo test using the pre-divorce period. Specifically, we estimate "pseudo-heterogeneity" using

the change in test scores between periods $t = -3$ and $t = -2$, when no divorce has yet occurred. Under our identifying assumptions, this pre-period variance difference should be zero. We therefore normalize all estimates by subtracting this pre-period difference:

$$\widehat{\text{Var}(D_s)}^{\text{adj}} = \widehat{\text{Var}(D_s)} - \widehat{\text{Var}(D_{-2})} \tag{7}$$

where $\widehat{\text{Var}(D_{-2})}$ is the placebo estimate from the pre-period. Event study plots present these normalized estimates with period $t = -2$ set to zero by construction.

We construct confidence intervals using a bootstrap procedure. In each of 1,000 iterations, we (1) sample with replacement from divorced famlies, (2) sample the control counterparts, and then (3) calculate $\text{Var}(D_s)$ for each iteration. In figures and tables we use the 2.5th and 97.5th percentiles as confidence bounds. When testing for statistical significance in divorce effect heterogeneity, we use a one-sided test based on the bootstrap distribution.

5.2 How many children benefit from divorce?

Given estimates of the mean effect μ_s and variance $\text{Var}(D_s)$, we calculate the fraction of children benefiting from divorce. Under normality (Assumption 3), the fraction with positive effects is:

$$\text{Fraction positive} = 1 - \Phi\left(\frac{-\mu_s}{\sigma_{D,s}}\right) \tag{8}$$

where Φ is the standard normal CDF and $\sigma_{D,s} = \sqrt{\text{Var}(D_s)}$. We use the delta method to construct confidence intervals that account for uncertainty in both parameters.

5.3 Robustness to Assumptions

5.4 Heterogeneity Results

Applying this method reveals striking differences across outcomes. For reading scores, we find substantial heterogeneity: the standard deviation of divorce effects equals $XXX\sigma$, implying that 34% of children experience non-negative effects despite the negative average. In contrast, we cannot reject homogeneous effects for math achievement and disciplinary outcomes, suggesting divorce is uniformly harmful along these dimensions. These patterns indicate that while some children may escape harm in certain academic domains, divorce appears broadly detrimental for mathematical reasoning and behavior—outcomes particularly predictive of long-term success.

6 Patterns Across Subgroups

To better understand which children are most vulnerable to divorce, we examine heterogeneous treatment effects by sex, race, socioeconomic status, and baseline achievement. Most notably, we find that the effects of

divorce are larger among boys, whites, and the non-poor.

6.1 Sex Differences

Figure 4 reveals that boys bear substantially larger costs from parental divorce across all outcomes. In math, boys' scores decline nearly twice as much as girls' and remain depressed four years post-divorce, while girls show signs of recovery. Reading effects are more similar across sexes, though boys still experience steeper initial declines. Most strikingly, boys show dramatically larger increases in disciplinary infractions and are more likely to be placed in special education following divorce.

These patterns align with emerging evidence that boys are particularly vulnerable to family disadvantage (Autor et al., 2019; Chetty et al., 2016). The departure of fathers may disproportionately affect boys through the loss of same-sex role models. Additionally, boys tend to externalize stress through observable behavioral problems, while girls more often internalize distress as anxiety or depression—making boys' responses more visible in administrative records even if both sexes experience comparable psychological harm.

6.2 Race Differences

Figure 5 documents striking racial variation in divorce effects. White children drive most negative academic impacts, with sharp post-divorce declines in both math (0.05) and reading following parallel pre-trends. Hispanic children show near-zero effects across all outcomes. Black children display no significant academic effects but experience disciplinary increases comparable to white children—consistent with Chetty et al. (2016)'s finding that family instability affects Black boys primarily through behavioral and criminal justice pathways rather than academic achievement.

These patterns may reflect differential selection into divorce across racial groups. If minority families divorce only under severe relationship distress while white families divorce across a broader quality spectrum, the marginal divorce would be less harmful in minority communities. Additionally, if minority children already face greater instability through economic hardship or residential mobility, divorce may represent a smaller incremental shock.

6.3 Socioeconomic Differences

Figure 6 shows that non-poor children (those ineligible for free/reduced lunch) experience substantially larger divorce penalties than poor children. While both groups show qualitatively similar patterns, effects for non-FRL students are consistently larger and more precisely estimated across all outcomes. Math scores for non-FRL children decline by 0.04 versus insignificant effects for FRL children.

This gradient likely reflects both selection and diminishing marginal effects. Among lower-income families, divorces may disproportionately dissolve already-dysfunctional marriages, making the change less harmful.

Moreover, children already facing economic instability may experience smaller marginal disruption from divorce. If divorce operates partly through resource constraints, those already resource-constrained may have less to lose.

6.4 Baseline Achievement

Figure 7 reveals complex patterns by prior achievement. In math, low achievers decline first (one year pre-divorce), middle achievers during divorce, and high achievers post-divorce. Four years later, middle achievers have recovered completely while both tails show persistent effects. Reading shows different dynamics: all groups initially decline similarly, but lower achievers recover while high achievers remain depressed.

These patterns suggest different mechanisms across the achievement distribution. Low achievers appear most sensitive to pre-divorce conflict, consistent with already-fragile academic foundations. High achievers show delayed but persistent losses, potentially reflecting gradual erosion of parental investments that disproportionately benefit strong students. The lowest tercile drives special education increases—unsurprising given their proximity to classification thresholds—and shows the sharpest disciplinary increases four years post-divorce.

In summary, divorce effects vary systematically across student characteristics, with boys, white children, non-poor families, and achievement extremes bearing the largest costs. These patterns suggest both that vulnerability to family disruption depends on baseline circumstances and that divorce may operate through different channels for different groups. Understanding this heterogeneity is crucial for targeting support services and interpreting average treatment effects.

7 Conclusion

This paper examines how divorce impacts children's development using a novel linked administrative dataset that tracks complete marital and educational histories for children in Texas. Our data captures not only academic achievement but also discipline, attendance, gifted program participation, and mental health outcomes. We begin by estimating average divorce effects using stacked event-study designs. The results show that divorce leads to declines in academic achievement (0.02–0.03σ), increased disciplinary infractions by 34 percent, absences by 10 percent, special education placement by 7 percent, and lowered gifted program participation by 9 percent. These negative effects are concentrated among boys, white and higher-income children.

Moving beyond average effects (both overall and within subgroups) presents a fundamental empirical challenge: standard heterogeneity analyses can only partition effects across observable characteristics, not reveal the full distribution of treatment effects within groups. To address this, we develop a novel variance decomposition method that isolates treatment effect heterogeneity by comparing the variance of outcome changes between treated and a matched control group. Applied to our divorce setting, this method reveals not just whether divorce harms children on average, but how much these effects vary and what fraction of children actually benefit.

Our results indicate that average effects mask considerable variation in some domains but not others. For

reading achievement, 34 percent of children experience non-negative effects while an equal share suffers losses exceeding twice the mean—suggesting some children escape educational harm while others are particularly vulnerable. Similar heterogeneity emerges for attendance. Yet for math achievement and disciplinary outcomes, we find little variation—divorce appears uniformly detrimental, reducing mathematical skills and increasing behavioral problems across nearly all children. Since math achievement and behavioral regulation are strong predictors of adult earnings, this pattern implies that divorce carries economic consequences for most affected children, though the magnitude varies substantially across families.

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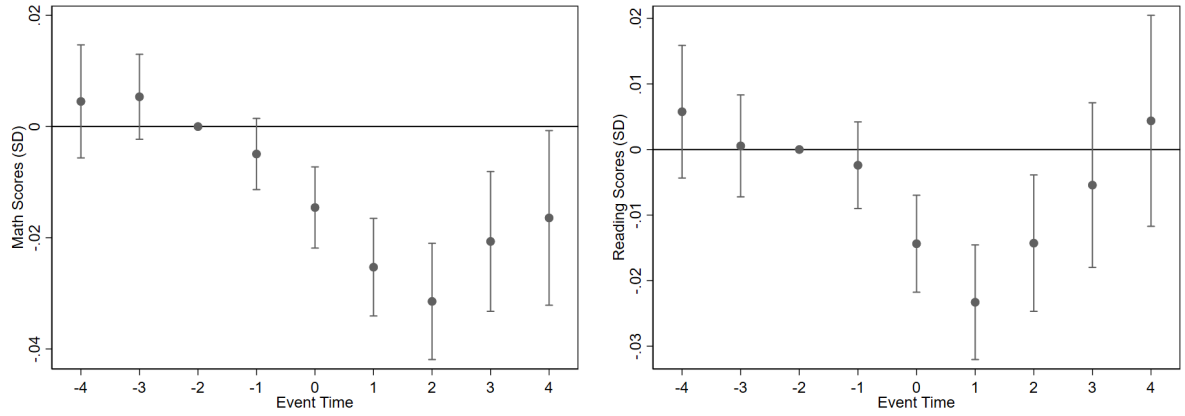
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Tables and Figures

Figure 1: The Impact of Divorce on Achievement

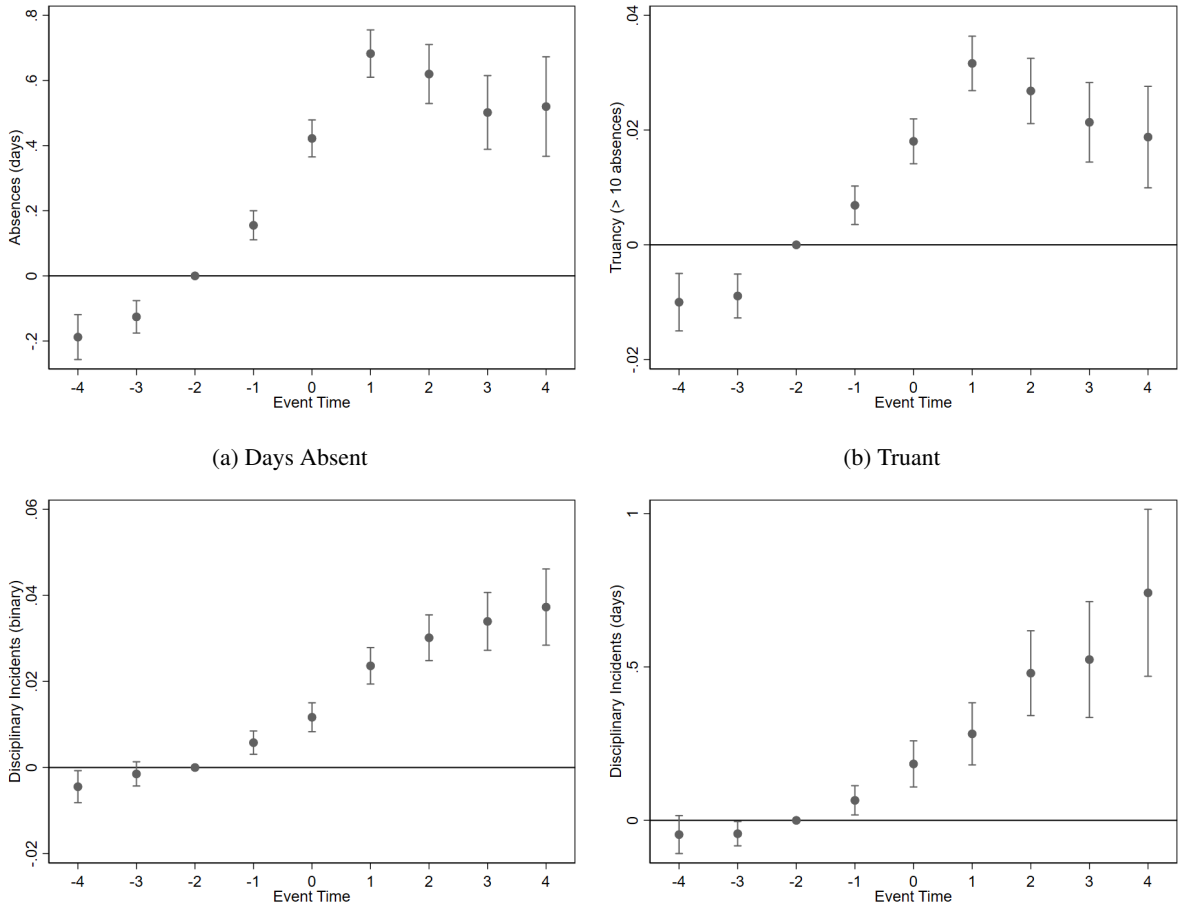


(a) Math

(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 1 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 2: The Impact of Divorce on Engagement and Behavioral Outcomes



(a) Days Absent

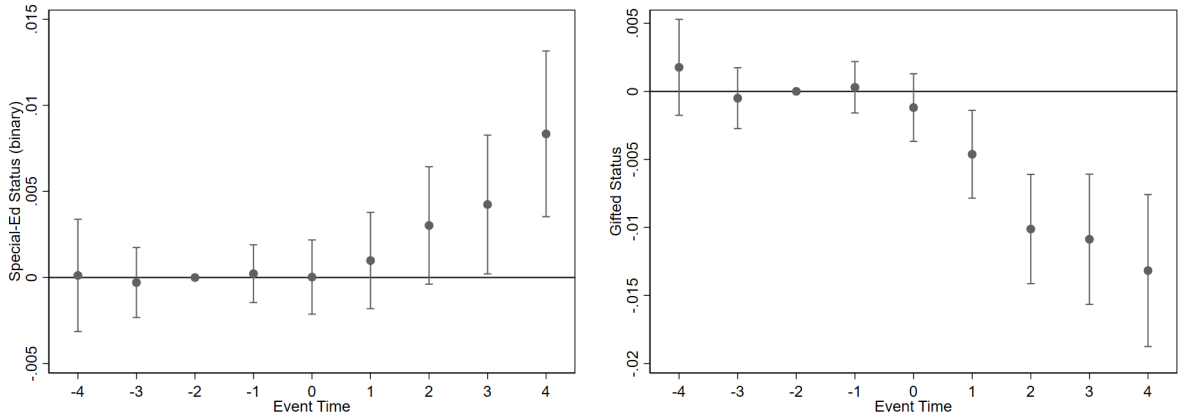
(b) Truant

(c) Ever Disciplined

(d) Days Disciplined

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 1 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 3: The Impact of Divorce on Educational Classifications

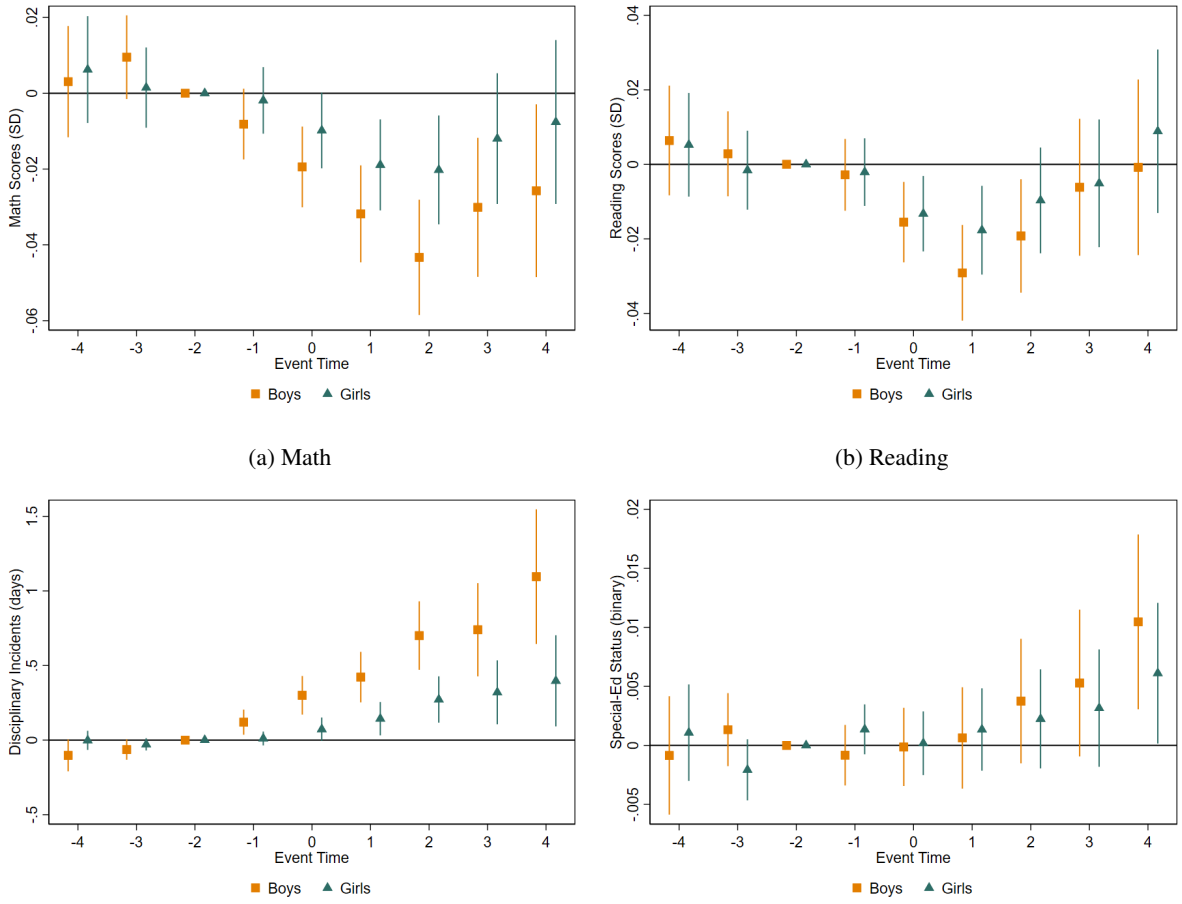


(a) Special Education Placement

(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 1 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 4: Heterogeneous Effects by Gender



(a) Math

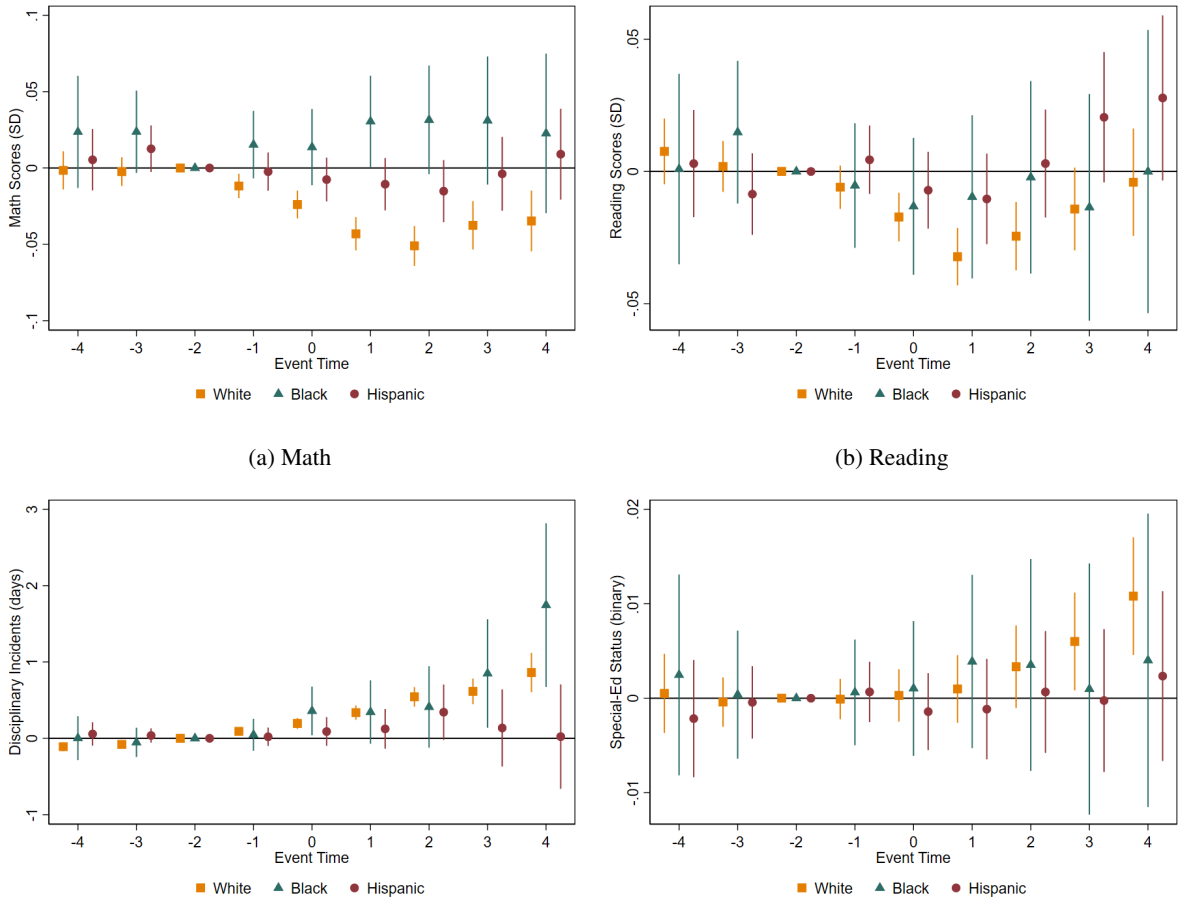
(b) Reading

(c) Days Disciplined

(d) Special Education

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 1 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 5: Heterogeneous Effects by Race



(a) Math

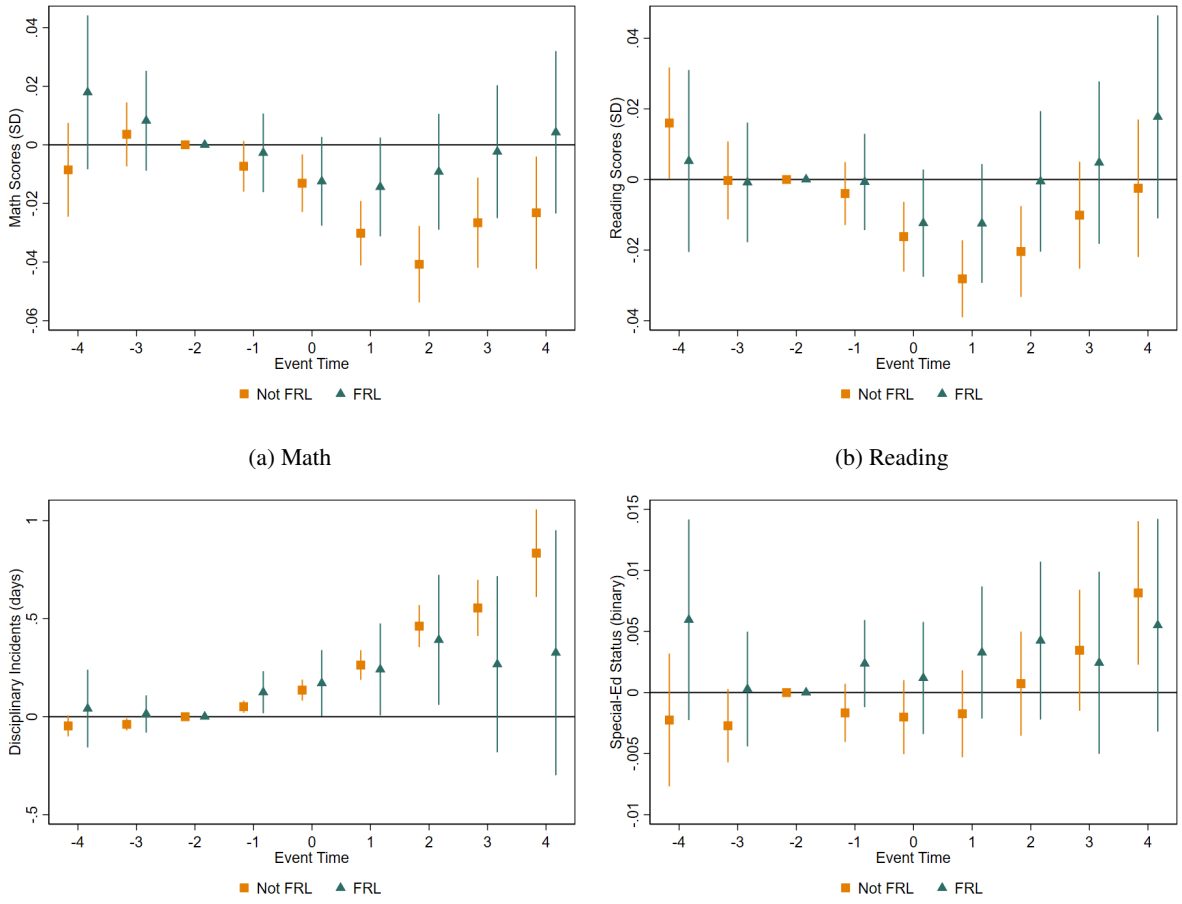
(b) Reading

(c) Days Disciplined

(d) Special Education

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 1 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 6: Heterogeneous Effects by Free and Reduced Price Lunch Status



(a) Math

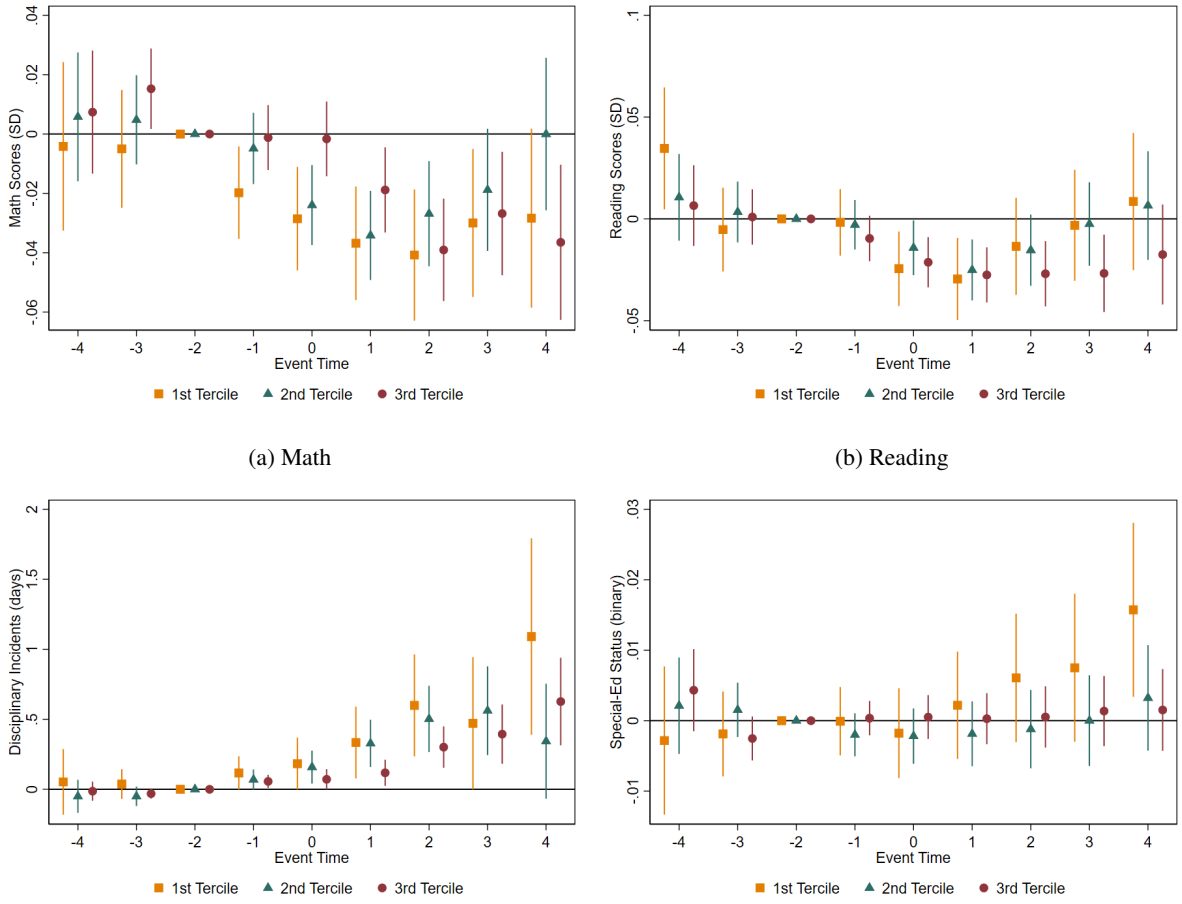
(b) Reading

(c) Days Disciplined

(d) Special Education

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 1 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 7: Heterogeneous Effects by Baseline Achievement



(a) Math

(b) Reading

(c) Days Disciplined

(d) Special Education

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 1 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.

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	0.48	0.48
White	0.41	0.42	0.32	0.54	0.56	0.53	0.52
Hispanic	0.42	0.41	0.46	0.37	0.34	0.36	0.38
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	0.01	0.02
3rd Grade:							
Age	8.13	8.13	8.14	8.10	8.12	8.11	8.09
LEP	0.20	0.16	0.20	0.11	0.11	0.13	0.14
Special-ed	0.11	0.12	0.12	0.11	0.12	0.11	0.10
Free Lunch	0.55	0.54	0.65	0.38	0.46	0.44	0.43
Gifted	0.08	0.08	0.07	0.11	0.08	0.09	0.09
Math Z	0.03	0.01	-0.10	0.16	0.07	0.09	0.13
Reading Z	0.02	0.01	-0.10	0.15	0.07	0.08	0.11
Combined Z	0.01	-0.01	-0.13	0.16	0.07	0.08	0.11
Days Absent	5.20	5.31	5.53	4.83	5.36	5.32	5.11
Truant	0.13	0.14	0.15	0.11	0.14	0.14	0.12
Disciplined	0.03	0.03	0.05	0.02	0.03	0.03	0.02
Days Disc.	0.13	0.14	0.19	0.07	0.11	0.09	0.07
Repeated Gr.	0.02	0.02	0.02	0.01	0.02	0.02	0.02
Ever LEP (3-8)	0.21	0.17	0.21	0.12	0.11	0.13	0.15
Ever SpEd (3-8)	0.19	0.19	0.21	0.17	0.19	0.19	0.17
Ever Repeat (3-8)	0.07	0.07	0.09	0.05	0.07	0.07	0.06
N	2,917,646	2,190,788	1,097,878	568,603	485,195	96,592	49,214

Note: The sample includes students born 1986-1997 who are observed in TEA in every grade from 3 through 8. "Ever divorced" indicates a divorce recorded between the birth year and the year the child turns 23. "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. "Never married" indicates either (i) no matched marriage record on or before the year the child turns 23 or (ii) a marriage followed by a divorce prior to the birth year.

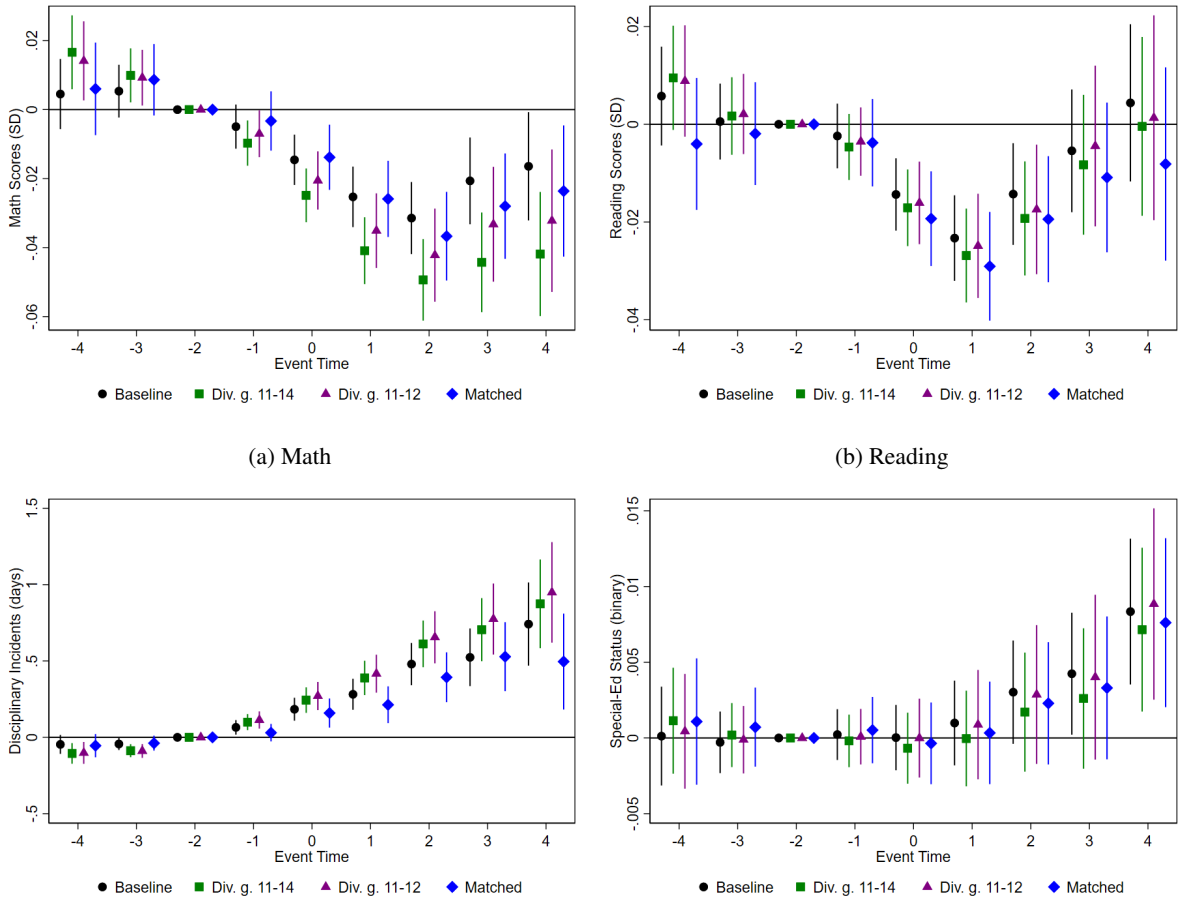
Table 2: The Impact of Divorce on Children's Outcomes

	Math Scores (1)	Reading Scores (2)	Absences (3)	Ever Disciplined (4)	Days Disciplined (5)	Special Education (6)	Gifted (7)
Event time:							
−4	0.00452	0.00576	−0.188***	−0.00446*	−0.0463	0.000122	0.00177
	(0.0052)	(0.0051)	(0.0351)	(0.0019)	(0.0315)	(0.0017)	(0.0018)
−3	0.00536	0.000551	−0.126***	−0.00149	−0.0436*	−0.000291	−0.000500
	(0.0039)	(0.0039)	(0.0254)	(0.0014)	(0.0202)	(0.0010)	(0.0011)
−2 (reference)	—	—	—	—	—	—	—
	—	—	—	—	—	—	—
−1	−0.00493	−0.00240	0.155***	0.00578***	0.0653**	0.000222	0.000301
	(0.0033)	(0.0034)	(0.0227)	(0.0014)	(0.0244)	(0.0009)	(0.0010)
0	−0.0146***	−0.0144***	0.422***	0.0117***	0.184***	0.0000252	−0.00119
	(0.0037)	(0.0038)	(0.0290)	(0.0017)	(0.0383)	(0.0013)	(0.0013)
1	−0.0253***	−0.0233***	0.682***	0.0236***	0.282***	0.000987	−0.00462**
	(0.0045)	(0.0045)	(0.0370)	(0.0022)	(0.0516)	(0.0014)	(0.0016)
2	−0.0314***	−0.0143**	0.620***	0.0302***	0.480***	0.00302	−0.0101***
	(0.0053)	(0.0053)	(0.0462)	(0.0027)	(0.0706)	(0.0017)	(0.0020)
3	−0.0207**	−0.00543	0.502***	0.0340***	0.524***	0.00424*	−0.0109***
	(0.0064)	(0.0064)	(0.0577)	(0.0034)	(0.0963)	(0.0021)	(0.0024)
4	−0.0164*	0.00438	0.520***	0.0373***	0.742***	0.00835***	−0.0132***
	(0.0080)	(0.0083)	(0.0780)	(0.0045)	(0.1390)	(0.0025)	(0.0029)
Adj. R-Squared	0.709	0.660	0.570	0.362	0.256	0.771	0.784
Observations	1,180,208	1,196,514	1,404,828	1,405,152	1,405,152	1,703,041	1,405,152
Unique Students	86,183	87,303	103,222	103,222	103,222	125,944	103,222
Dep. Var. Mean	0.261	0.285	5.920	0.109	0.895	0.116	0.148
Treated Mean	0.225	0.256	6.281	0.119	1.019	0.123	0.140
Control Mean	0.275	0.297	5.777	0.105	0.846	0.114	0.151

Note: This table present 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 1 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.

A Appendix Tables and Figures

Figure 1: Sensitivity to Control Group



(a) Math

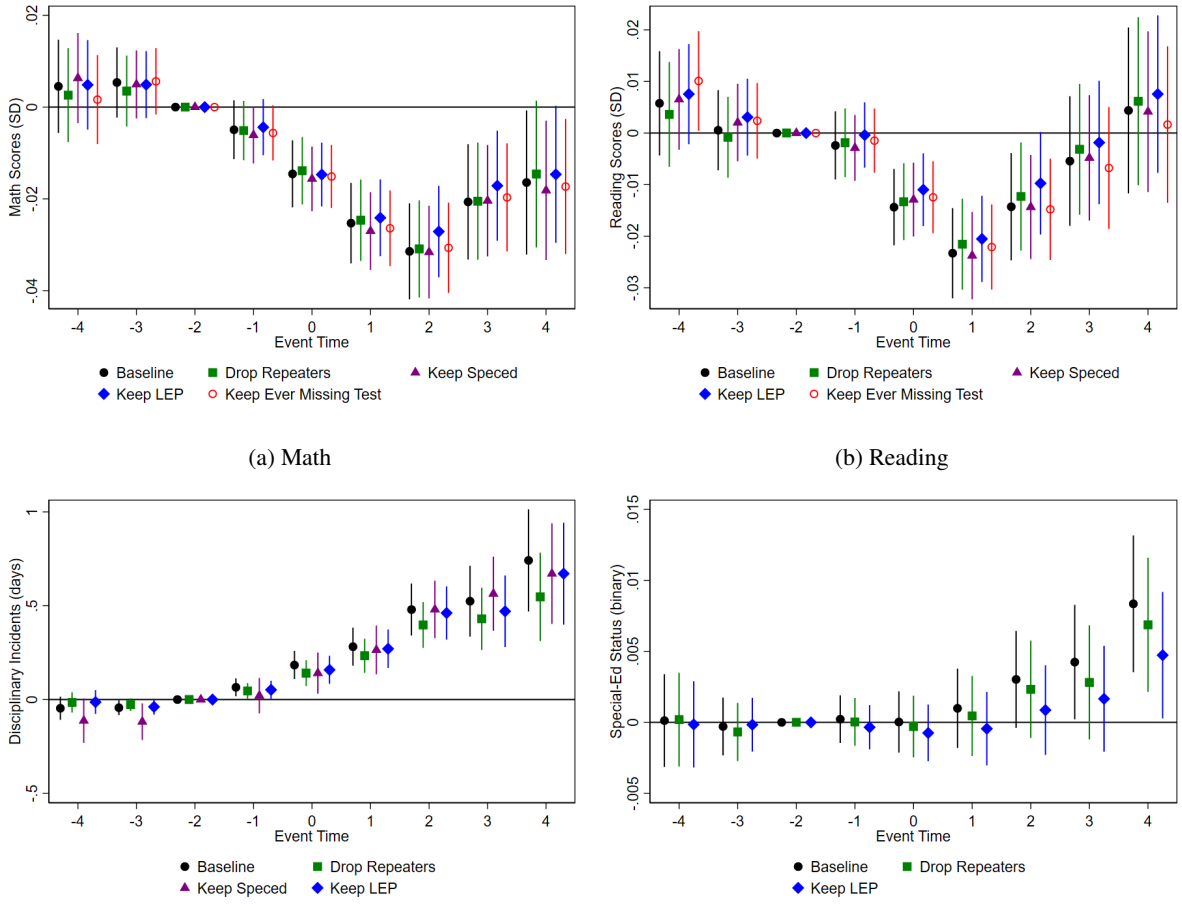
(b) Reading

(c) Days Disciplined

(d) Special Education

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 1 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 2: Sensitivity to Sample Restrictions



(a) Math

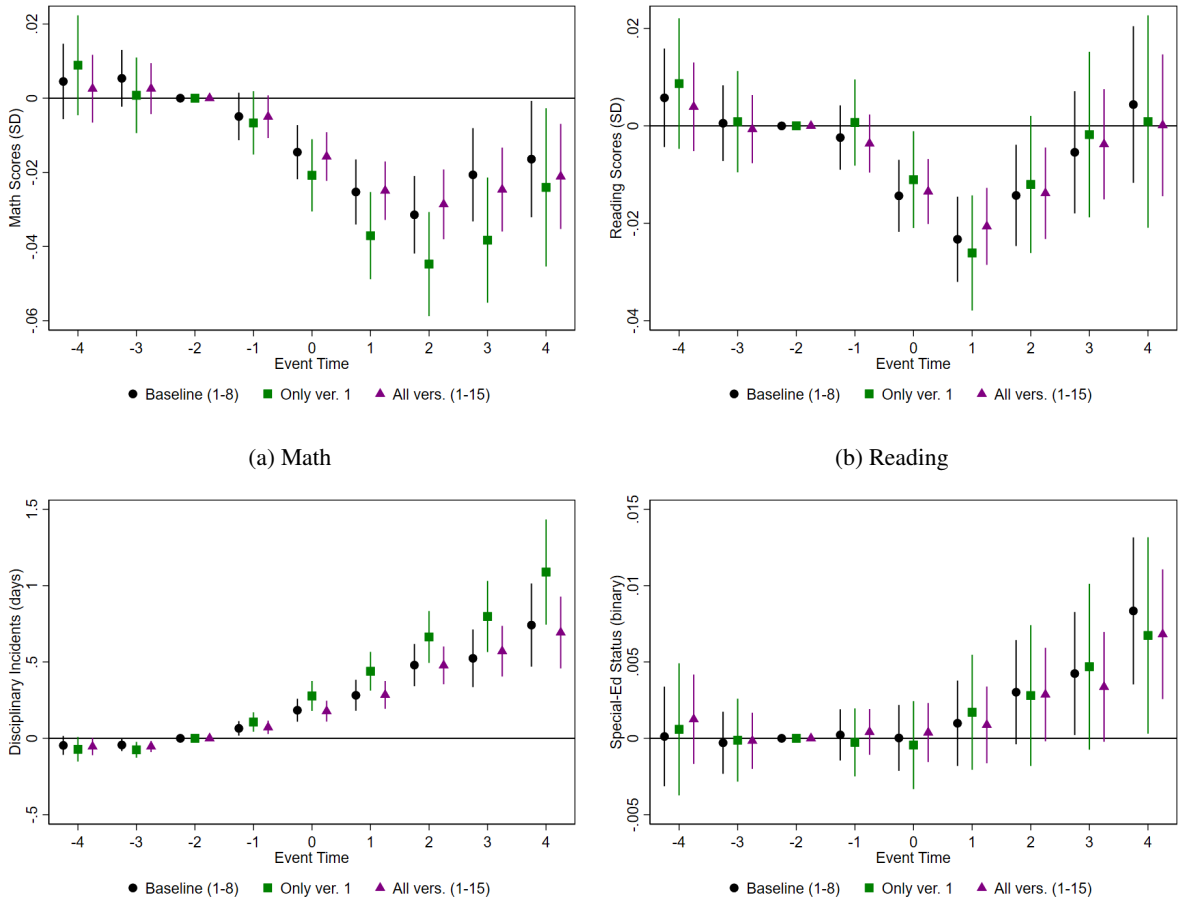
(b) Reading

(c) Days Disciplined

(d) Special Education

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 1 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 3: Sensitivity to Divorce Match



(a) Math

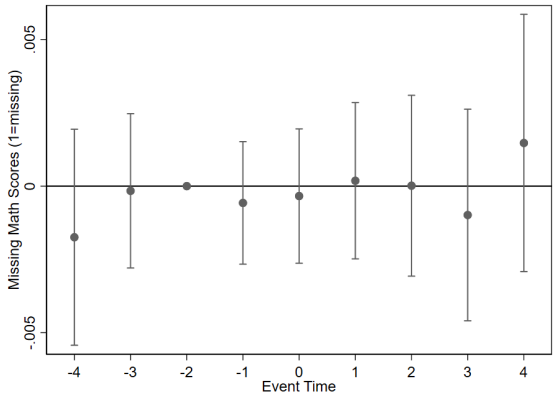
(b) Reading

(c) Days Disciplined

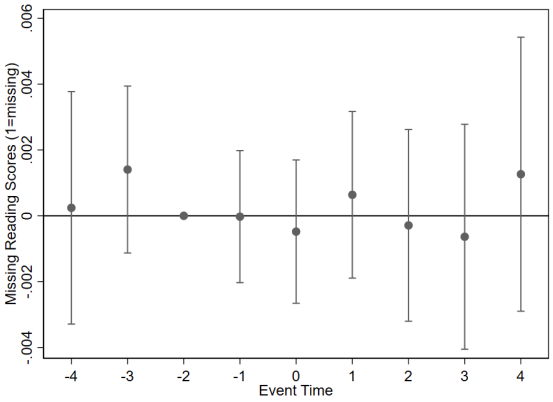
(d) Special Education

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 1 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.

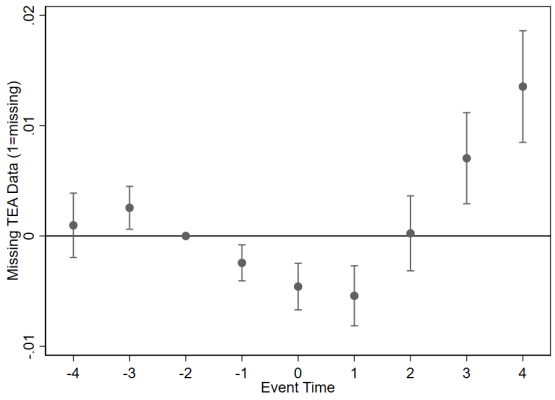
Figure 4: Attrition



(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 1 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.