

# Sibling Spillovers and Special Education Access

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## Abstract

Over 13% of U.S. children receive special education (SpEd) services, yet little is known about their impact on siblings. We study sibling spillovers using a 2005 Texas policy that capped district-level SpEd enrollment at 8.5%, leading to sharp declines in access. Linking birth and education records, we find that when one child is more likely to lose SpEd services, their general education sibling experiences improved long-run outcomes. Although these effects likely reflect a combination of changes in parental investment and direct sibling interactions, the evidence more strongly supports parental resource reallocation than direct sibling spillovers. Our results imply that evaluations of targeted education programs like SpEd should account for family spillovers and not just the treated child outcomes.

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# 1 Introduction

Special Education (SpEd) is a large and growing component of public education. Over 13% of U.S. children receive SpEd in each year, at an annual cost of \$50 billion ([Chambers, Parrish, & Harr, 2004](#)). Schools spend nearly twice as much to educate SpEd students compared to their general education (GE) peers, yet the returns on these investments remain a topic of debate. While SpEd provides accommodations and services intended to support students with disabilities, the net benefits for students on the margin of placement decisions are unclear. Additional accommodations likely improve outcomes, but the stigma from disability labels, lower academic expectations, and reduced access to GE classrooms may offset these gains for those with less severe conditions. Until recently, rigorous evidence on the direct effects of SpEd has been limited. Even less is known about its broader impacts within the household, particularly on the outcomes of siblings.

A change in one child's SpEd status is likely to have meaningful consequences for their siblings for at least two reasons. First, because sibling relationships are among the most enduring and influential in a child's life, shifts in one child's educational path can strongly affect the other. For instance if a SpEd status change aligns with a child's educational needs, they may become more engaged and cooperative at home, in ways that improve sibling relationships. If instead, a status change leaves needs unmet, the affected child may become more emotionally withdrawn or exhibit behavioral problems that strain sibling relationships. Second, SpEd placement can reshape how parents allocate their time and resources across children. If formal services ease care burdens, parents can reallocate resources to other children. However, formal SpEd placement could also draw additional attention and resources to the identified child (see, e.g., [Black et al. \(2021\)](#)).

Studying sibling spillovers in SpEd has proven difficult for two key reasons. First, placement into or out of SpEd is not random. For many disability types, eligibility is based on subjective assessments rather than clear thresholds, and decisions can vary substantially across evaluators, schools, and districts.<sup>1</sup> As a result, siblings of children who lose SpEd services may

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<sup>1</sup>As discussed in more detail in Section 2.1, eligibility is relatively straightforward for children with severe physical

differ systematically from siblings of children who remain in SpEd, making simple comparisons non-causal. Additionally, with federal SpEd rules mostly unchanged since the mid-1970s, opportunities for policy-driven identification are limited. Although the recent literature has made progress in credibly estimating SpEd’s direct effect using policy variation or individual fixed effects designs (e.g., [Ballis & Heath, 2021](#); [Hurwitz, Perry, Cohen, & Skiba, 2020](#); [Schwartz, Hopkins, & Stiefel, 2021](#)), spillovers on GE siblings have yet to be examined. This is because secondly, most administrative education datasets do not link siblings’ school records, and where linkages exist (e.g., Florida, Rhode Island), they either lack outcomes extending into adulthood or rely on small samples (e.g., [Chyn, Gold, & Hastings, 2021](#); [D. Figlio, Guryan, Karbownik, & Roth, 2014](#)). Moreover, these states have not experienced exogenous shifts in SpEd enrollment that enable causal identification. As a result, little is known about the broader impacts of SpEd participation on families.

We address these challenges by constructing a novel dataset and leveraging a rare policy change that led to one of the largest, plausibly exogenous shifts in SpEd participation to date. The dataset links the universe of Texas birth records to administrative schooling data, enabling us to identify siblings and track outcomes from childhood into adulthood. Our natural experiment is the Texas Education Agency’s 2005 cap setting district-level SpEd enrollment at 8.5%. As shown in [Figure 1](#), this policy led to a sharp decline in SpEd enrollment from 12 to 8% between 2005 and 2015, a stark contrast to national trends which remained flat over the same period. Importantly, the policy was not publicly disclosed and remained in place until 2016, when it was uncovered by an investigative journalist and later rescinded after a federal investigation found it violated disability law ([Rosenthal, 2016](#); [U.S. Department of Education, 2018](#)). Its covert and unlawful nature strengthens the case that the policy generated plausibly exogenous variation in SpEd access.

To identify sibling spillovers from SpEd placement, we focus on students who were *not* in SpEd themselves prior to fifth grade but had a sibling who was. We refer to the SpEd sibling as the *focal sibling*, and leverage two sources of variation in that sibling’s exposure to the SpEd enrollment

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or cognitive conditions. However, most children in SpEd have conditions, such as learning disabilities or ADHD, for which inclusion criteria are less clearly defined.

cap.<sup>2</sup> First, districts with higher pre-policy SpEd rates faced greater pressure to reduce enrollment, making SpEd removals for *focal siblings* more likely in these districts. Second, exposure varied across the *focal sibling's* 5th grade cohort: cohorts who were in earlier grades in 2005 had more years of exposure to the policy and thus higher rates of removal from SpEd. We combine these two sources of variation in a difference-in-differences design that compares *GE sibling's* outcomes within the same district and 5th grade cohort whose *focal sibling* had different policy exposure due to their grade at the time of introduction, and then tests whether the differences are larger in districts where the cap was more binding (i.e. those further from the cap at baseline). This strategy identifies how having a sibling who is more likely to lose SpEd affects the *GE sibling's* educational outcomes.

We begin by documenting that the policy led to significant decreases in SpEd placement and educational attainment for *focal siblings*. For the *focal* student with full policy exposure after 5th grade in the average district (4.5 points above the cap in 2004-05), SpEd removal by 9th grade increases by 5.3 p.p. (16 percent) and high-school completion falls by 3.1 p.p. (3.9 percent). These estimates closely mirror those in [Ballis and Heath \(2021\)](#), which studies the full SpEd sample.<sup>3</sup> These effects are concentrated among students in a “high impact” sample, where classification relies on more subjective judgment.<sup>4</sup> This group drives the reductions in SpEd enrollment and educational attainment. This supports the interpretation that SpEd removal was the primary channel through which the policy affected *focal* children's educational outcomes.

On the other hand, we find that having a sibling who was more likely to be removed from SpEd improved their *GE sibling's* educational outcomes. Our estimates suggest that, in the

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<sup>2</sup>As discussed in Section 3.2, we identify SpEd siblings as students placed in SpEd by 5th grade between 2000 and 2005, prior to the enrollment cap. This ensures all were diagnosed under a consistent policy regime and captures a relatively stable SpEd population, as identification rates grow substantially before 5th grade but level off afterward. However, we demonstrate that our results are not sensitive to this grade cohort restriction.

<sup>3</sup>Relative to [Ballis and Heath \(2021\)](#), we find similar magnitudes for the effects on college enrollment, though the coefficient is not statistically significant in our sample of children with siblings. This may reflect the smaller sample size or the slightly more positive selection in our sample, which includes only those with two parents recorded on the birth certificate (see [Appendix C](#) for details).

<sup>4</sup>As will be described in more detail in Section 2.1 we define our “high-impact” sample as those with learning disabilities, speech impairments, other health impairments (includes ADHD), and emotional disturbance who receive more than 50 percent of their instruction in General Education (GE) classrooms at baseline. These conditions have more discretion in the identification and evaluation and are groups of students that would be more susceptible to policy driven SpEd removals.

average district having a fully exposed *focal sibling* raises high school completion by 1.9 p.p. (2.5 percent) and college enrollment by 1.7 p.p. (3.4 percent).<sup>5</sup> These spillover effects are concentrated among students with *focal siblings* in the high impact sample who were more likely to lose SpEd, with no effects for those whose siblings were unlikely to lose SpEd. This pattern is consistent with a causal mechanism operating through the SpEd removal of their sibling, rather than broader cohort or district trends. We do not find a direct effect on subsequent SpEd placement and only find very modest evidence of classroom-level spillovers on college enrollment. Overall, the evidence indicates that the *focal sibling*'s SpEd removal is the main driver of these sibling gains.

What explains the positive spillovers from having a focal sibling who is more likely to lose SpEd services? The two primary channels we consider are direct sibling-to-sibling spillovers and indirect spillovers through a change in parental investments. While both mechanisms likely contribute to our findings, the evidence we find more strongly favors the latter. First, impacts are larger in higher-income families with greater resources to reallocate. Second, GE siblings are more likely to attend higher-quality schools following their focal sibling's SpEd removal, suggesting deliberate parental investment responses. Third, these spillovers persist even for sibling pairs with large age gaps, a setting where direct interactions should be weakest but where household resource allocation shifts can still operate. Similarly, these effects are larger when the focal sibling is older and in two-child families. When focal siblings are older, potential resource reallocation begins earlier in the GE sibling's educational trajectory during more formative years, while in two-child families resource trade-offs are likely more salient.<sup>6</sup> Finally, if peer effects were the primary mechanism, we would expect the strongest impacts to appear for same gender sibling pairs. Instead, we find that opposite gender pairs where the focal child is female show effects that are very similar to same gender pairs, which makes a pure peer effects explanation less likely.

Our paper makes several contributions to the literature. First, we provide the first causal

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<sup>5</sup>Throughout the paper, effect sizes for sibling spillovers refer to having a focal sibling who was fully exposed to the policy between 5th and 9th grade in the average district that was 4.5 percentage points above the 8.5 cap in 2005.

<sup>6</sup>The larger effects for older focal siblings could also reflect role-modeling, but combined with the other evidence, parental resource reallocation appears more likely. In families with three or more children, resources are divided among multiple siblings, potentially diluting the benefits to any single GE child.

estimates of sibling spillovers from SpEd placement, extending prior work that has focused on the direct impacts of SpEd on students’ short-run and long-run outcomes (e.g. [Ballis & Heath, 2019, 2021](#); [Cohen, 2007](#); [Hanushek, Kain, & Rivkin, 2002](#); [Schwartz et al., 2021](#)). We show that changes in SpEd access for one child meaningfully affect their sibling’s educational attainment, suggesting that the returns to SpEd are broader than typically measured. We also find novel evidence that parents may reallocate time, attention, and resources away from formally SpEd children to their other children following SpEd removal. This mechanism helps explain prior evidence of negative impacts on SpEd removal after the enrollment cap ([Ballis & Heath, 2021](#)), suggesting that some of the observed declines may be driven by parental reallocation.

Second, we contribute to the growing literature on sibling spillovers in education in two important ways. First, we provide new evidence on the educational spillovers stemming from a sibling’s disability. To our knowledge, [Black et al. \(2021\)](#) is the only prior study examining such spillovers.<sup>7</sup> We extend this literature by being the first to leverage policy-driven changes in SpEd placement to identify the causal spillover effects of targeted educational investments. Unlike [Black et al. \(2021\)](#), who focus on families with a severely disabled child, our design examines changes in placement for less severely disabled children, for whom the benefits of services are more uncertain.<sup>8</sup> Second, we extend the literature by examining longer-run outcomes such as high school completion and college enrollment. Much of the U.S. evidence on sibling spillovers relies on Florida administrative data, which capture short-run academic performance but lack longer-run measures (e.g., [D. N. Figlio, Karbownik, & Özek, 2023](#); [Karbownik & Özek, 2023](#)). A notable exception is [Altmejd et al. \(2021\)](#), who study spillovers from older siblings’ college choices. We contribute to this long-run evidence by examining spillovers from childhood interventions traced over a longer time horizon.

Third, our results underscore the importance of moving beyond the individual student

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<sup>7</sup>Related work in health economics documents sibling spillovers in disability diagnoses rather than educational outcomes, see, for example, ([Breining, 2014](#); [Persson, Qiu, & Rossin-Slater, 2025](#))

<sup>8</sup>[Black et al. \(2021\)](#) use a family fixed-effects design comparing the differential impact of a disabled third child on first- and second-born siblings. As they note, their estimates likely represent a lower bound because the comparison assumes second-born siblings are more affected by a disabled third sibling due to closer age proximity.

when analyzing the impacts of targeted educational investments. Targeted interventions are an increasingly popular way of allocating school resources (e.g. [Setren, 2021](#)). While SpEd is designed to support the identified student, we show that it can have significant ripple effects within the family. Our results reveal the potential for unintended consequences when policies target individual students without accounting for broader family dynamics. This has direct implications for how districts evaluate SpEd eligibility and service provision, and raises questions about other targeted interventions, such as gifted and talented programs ([Card & Giuliano, 2016](#)), disciplinary placements ([Bacher-Hicks, Billings, & Deming, 2024](#)), early intervention programs ([Bailey, Sun, & Timpe, 2021](#)), and advanced coursework ([Conger, Kennedy, Long, & McGhee, 2021](#); [Jackson, 2010](#)), where decisions made for one child may affect others in the household.

## 2 Background

### 2.1 Special Education Programs

The Education for All Handicapped Children Act (EAHCA) of 1975, now known as the Individuals with Disabilities Education Act (IDEA), established the framework for SpEd services in the US. Students with disabilities are guaranteed a free and appropriate education under current federal law. Since 1975, SpEd participation has grown significantly, from 8 to 13 percent. Today, these programs serve approximately 6.4 million public school students nationwide ([Bohrnstedt, Garet, Holtzman, Ogut, & Smith, 2015](#)).

Students enter SpEd through a formal referral and evaluation process. Teachers or parents can initiate referrals when students exhibit persistent academic struggles, behavioral challenges, or developmental delays that interfere with learning. Following referral and parental consent, a multidisciplinary team, including psychologists, SpEd specialists, and other professionals, conducts a comprehensive evaluation. Parents participate throughout this process, providing input and ultimately approving or rejecting the eligibility determination. Once enrolled, students undergo mandatory reevaluations every three years to determine whether services should continue.<sup>9</sup> While

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<sup>9</sup>Annual evaluations focus on adjusting services, and three-year evaluations assess whether continued eligibility for

some students do exit SpEd, the majority remain enrolled throughout their schooling.

Once a student qualifies for services, the school develops an Individualized Education Plan (IEP). These plans vary widely because they address each student's unique needs and disability. An IEP serves two main functions: first, it specifies the types of support and services the student will receive (such as speech therapy or classroom accommodations), and second, it establishes measurable annual goals and specifies how these goals will be tracked. For older students, IEPs also include transition planning to prepare for college enrollment or employment.

The substantial growth in SpEd enrollment since 1975 stems primarily from increased identification of learning disabilities, speech impairments, other health impairments (including ADHD), and emotional disturbance. We focus on students with these conditions receiving more than 50 percent of their instruction in GE classrooms at baseline, which we refer to as our “high impact” sample.<sup>10</sup> Unlike physical disabilities or severe cognitive impairments, these conditions often involve substantial discretion in the identification and evaluation processes. For example, distinguishing between a student who needs SpEd for a learning disability versus one who simply requires additional reading support can vary significantly across evaluators, schools, and districts.

Due to the subjective nature of these conditions, prior literature has found that school finance and accountability policies have influenced SpEd participation (Cohen, 2007; Cullen, 2003). This has led to debate about whether marginal students are helped or harmed by SpEd placement. On the one hand, students are likely to benefit from the individualized instruction, goal monitoring, and additional resources. However, these advantages may be offset by the stigma of a disability label and reduced time in GE classrooms. The effects of SpEd services on students are important and have been the focus of a growing, but still relatively limited, body of research. Yet, how changes in SpEd access shift family dynamics and resources remains unstudied, despite being crucial for contextualizing the direct effects of SpEd and understanding the full implications of SpEd policy.

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SpEd is appropriate (Texas Education Agency, 2024).

<sup>10</sup>The rationale for classroom inclusion restriction is that if students are receiving most of their instruction outside of GE classrooms then they are likely to have more severe conditions which may make it more difficult to justify SE removal.



## 2.2 Policy Background

In the 2004-05 school year, the Texas Education Agency (TEA) introduced a district-level SpEd enrollment target of 8.5%. At the time, the average district served 13% of its students in SpEd and about 90% of districts exceeded the threshold. Districts were required to demonstrate annual progress in reducing SpEd enrollment and faced state interventions if they failed to do so. The severity of these interventions varied with a district's distance above the target.<sup>11</sup> Districts near the 8.5% cap faced relatively mild sanctions, such as developing an improvement plan. In contrast, districts substantially above the cap were subject to stricter sanctions, such as on-site monitoring by third-party evaluators.

Districts complied with the policy. In December 2004, districts received a report identifying their SpEd enrollment rates, flagging those over the cap, and outlining sanctions for failing to reduce them. As shown in [Figure 1](#), SpEd enrollment in Texas had been stable at roughly 12% in the years prior. After the first report was sent SpEd enrollment fell sharply in Texas. This is a notable contrast to national trends, which remained relatively unchanged during the same period.

The timing and pattern of the decline suggest that the policy came as a surprise to districts. [Figure 2](#) shows little evidence of change in SpEd enrollment prior to the cap's implementation, indicating that it was both abrupt and unanticipated. Rather than being introduced through public discussion or consultation, the cap appears to have been introduced quickly and in response to a sudden state budget shortfall affecting the 2004–05 fiscal year ([Blanchard, 2016](#)). At the time of its introduction, teachers and school administrators believed it was based on research and best practices,<sup>12</sup> and the cap did not face widespread public scrutiny until 2016 when an investigative report by the Houston Chronicle exposed its existence ([Rosenthal, 2016](#)). In response to growing criticism and a subsequent federal investigation that concluded the policy violated federal law, the TEA formally rescinded the enrollment target in 2017 and it was officially repealed in 2018.

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<sup>11</sup> See Appendix [Figure A.1](#) for an illustration of performance level ratings.

<sup>12</sup> As a Houston Chronicle journalist reported, “[t]he few people in special ed departments who knew about it either assumed it was a federal mandate or thought it was backed by research . . . Neither of those things was true. In reality, it was completely arbitrary” ([McCartney, 2017](#)).

It is important to note that the SpEd enrollment cap was introduced as part of a broader monitoring initiative known as the Performance Based Monitoring Analysis System (PBMAS), which aimed to improve academic and behavioral outcomes for SpEd students and address the overrepresentation of minority groups in SpEd. However, as shown in [Ballis and Heath \(2021\)](#), while most districts were already meeting the other PBMAS benchmarks, the vast majority exceeded the SpEd enrollment cap, often by a substantial margin.<sup>13</sup>

## 2.3 Possible Impacts on Siblings

To motivate our empirical analysis, we draw on a simple human capital framework to illustrate how the removal of SpEd services for the *focal sibling* might affect the outcomes of *GE* siblings. We summarize the core intuition of this model here and provide additional details in [Appendix B](#).

In our framework, SpEd removal can affect a sibling's outcomes through two main channels: (1) direct sibling spillovers and (2) shifts in parental investment. We assume families interpreted SpEd removal as a *Positive Signal*, viewing it as evidence of genuine improvement in the *focal sibling's* condition and a reduced need for additional support. This interpretation is plausible given that the cap was implemented without the knowledge of parents or teachers, making removals appear educationally appropriate. Moreover, because parents must approve changes in SpEd placement, their consent indicates at least initial agreement with the decision.<sup>14</sup>

Direct spillover effects may be positive if the *focal sibling* appears to have overcome learning challenges and becomes a stronger role model. This effect may be particularly pronounced when the focal sibling is older. Conversely, spillovers effects may be negative if sibling rivalry

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<sup>13</sup>The only other PBMAS indicator affecting districts meaningfully was disproportionality monitoring in SpEd as shown in [Ballis and Heath \(2019\)](#). While this could create additional pressure to reduce enrollment for specific subgroups, our results are robust to controlling for the Black and Hispanic disproportionality measures.

<sup>14</sup>An alternative possibility is that some families viewed SpEd removal as a loss of needed services rather than a sign of progress. In this case, parents might have increased investments in the affected child, potentially at the expense of their GE sibling. However, we view this as unlikely given that families were unaware the removals were policy driven rather than educationally motivated. This explanation is also inconsistent with the positive spillover effects we find overall, though it is consistent with the less positive sibling spillovers observed among subgroups who were more harmed by declassification. If this effect is present, it is dominated by other channels for most families in our sample.

intensifies, manifesting as reduced self-esteem or increased pressure on the *GE* sibling following a perceived boost in their *focal* sibling's ability.<sup>15</sup>

In terms of parental investments, parents may follow either a compensatory or reinforcing investment strategy. Under a compensatory approach, parents allocate more time, attention, and resources to children with special needs relative to their siblings. Under a reinforcing approach, parents instead direct resources toward *GE* children if they are perceived as being higher-achieving. However, in the context of childhood disability, evidence suggests parents predominately follow a compensatory investment strategy (Black et al., 2021). When one child is classified with a disability, *GE* siblings may experience worse outcomes as parents shift focus to their children with greater needs. Assuming this pattern holds in our setting, SpEd removal could lead parents to reallocate resources back to the *GE* sibling, perceiving less need for additional support for the child formerly identified for SpEd.

A related channel operates through parental labor supply. Previous work has shown that mothers of children with disabilities or higher health needs tend to lower their labor supply (Corman, Reichman, & Noonan, 2005; Gunnsteinsson & Steingrimsdottir, 2019). If parents perceive the SpEd removal as an improvement, they may be more likely to return to work, potentially increasing household earnings and indirectly benefiting the *GE* sibling.

Section 6 investigates these predictions empirically. While both channels likely play a role, our evidence suggests that shifts in parental investment may be a more important driver than direct spillovers. This suggest that parents reallocate resources toward the non-disabled sibling after the focal child loses the disability label, consistent with findings in Black et al. (2021).

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<sup>15</sup>Another potential, but less likely channel is sibling caregiving. However, the policy primarily affected less severely disabled children who likely require minimal caregiving support. Additionally, we find larger effects for younger spillover siblings of older focal siblings, who would be capable of providing caregiving than older siblings.

## 3 Data and Summary Statistics

### 3.1 Data Sources

Our core dataset comes from the Texas Schools Project (TSP), a restricted-access administrative database tracking all public school students in Texas from 1994 to the present. While comprehensive, this dataset lacks family identifiers to link siblings. To address this limitation, we used the Texas Birth Index (TBI) from 1976 to 1997, which records all Texas births along with parental information. Using the TBI, we matched siblings who shared parents with the same names, incorporating adjustments for nicknames and employing phonetic matching techniques to account for misspelled names.<sup>16</sup> We then had the TEA link these identified siblings with the TSP records. The TEA’s merge, based on children’s full names and birthdates, successfully matched 70% of public school children to the TBI.<sup>17</sup> Among those who matched, around 54% had at least one sibling.

Notably, we identify less siblings than would be expected based on the ACS, as discussed in more detail in Appendix C. This is largely due to the fact that our sibling matching techniques may misclassify some true siblings as only children. While we implement name adjustments to improve match rates, our algorithm cannot fully account for spelling inconsistencies or naming variations. We also exclude children from single-mother households, since our match requires both parents’ names. These limitations do not compromise internal validity, but they may affect external validity. Specifically, the matched sibling sample is only slightly more positively selected, as shown by comparing characteristics of the full sample and the matched sibling sample in Section 3.3. We discuss the implications of this modest selection for generalizability in Appendix C. As discussed in more detail there, although using both parents’ names yields highly unique identifiers and makes false sibling matches unlikely, we further reduce potential mismatches by excluding sibling pairs

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<sup>16</sup>Appendix C describes our matching algorithm in more detail and shows that our results are very robust to variations in name matching, including identical name matches and incremental use of nicknames and phonetic adjustments. Because using both parents’ names yields highly unique identifiers, false sibling matches are unlikely. Nonetheless, we exclude sibling pairs who do not attend schools in the same district or geographic area, measured by contiguous counties, in at least 75% of the years in which both students are enrolled, which removes 5.3% of matched pairs.

<sup>17</sup>This match rate is consistent with findings from Florida, where birth records were matched to public school records (D. Figlio et al., 2014).

who do not attend schools in the same district or geographic area, measured by contiguous counties, in at least 75 percent of the years in which both students are enrolled.

The TSP data track a comprehensive set of education and labor market outcomes. Specifically, we start with student-level records from the Texas Education Agency (TEA) which cover the universe of Texas public school students from Kindergarten through 12th grade, providing yearly information on demographics, academic performance, and behavioral outcomes. To determine SpEd status, we utilize detailed SpEd data from the TEA, including annual program participation, disability type, standardized exam participation, and time spent in resource rooms. We then link these TEA school records to post-secondary enrollment data from the Texas Higher Education Coordinating Board (THECB), which includes enrollment and degree attainment information for all Texas universities. While these administrative data offer advantages in terms of sample size and comprehensiveness, one limitation is our inability to track individuals who leave Texas. Fortunately, outmigration from Texas is relatively low, with most Texas-born individuals remaining in the state [Aisch and Gebeloff \(2014\)](#) and only 1.7 percent of Texas residents leaving annually [White \(2016\)](#).

### 3.2 Sample Construction

We define *focal students* as those enrolled in SpEd as of 5th grade between 1999-00 and 2004-05, prior to the implementation of the 2005 SpEd enrollment cap. Since the policy significantly changed the composition of students identified for SpEd, this restriction ensures that students in our sample were diagnosed under a similar policy environment. As justified in [Ballis and Heath \(2021\)](#), we focus on fifth-grade SpEd cohorts because placement peaks in fifth grade (with few new entries thereafter) yielding a stable sample of SpEd students, and because these students still have many years of schooling ahead, making them more exposed to the 2005 cap and subsequent removals than older cohorts. Following [Ballis and Heath \(2021\)](#), we focus on districts serving between 6.6% and 21.5% of their students in SpEd during 2004-05, as this focuses on districts with typical SpEd rates.<sup>18</sup> In this paper, we are focused on siblings spillovers, so we further restrict our focal students

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<sup>18</sup>This restriction excludes roughly 1% of the sample, as district outliers tend to be small.

to those with siblings. Our final sample comprises of a total of 83,861 focal students, of which 71,598 are in the high impact sample.

Our primary focus in this paper is examining how the policy change affected the siblings of *focal* students. To construct our sibling sample, we start with the final sample of focal children defined above and identify their siblings using the TBI sibling linkages previously described. To ensure that focal children in each 5th grade cohort have both older and younger siblings, we extend the 5th grade cohorts from 1994 to 2007.<sup>19</sup> Rather than focusing on all of the siblings of focal children, we restrict our sample to GE siblings who were unlikely to be directly impacted by the policy. This allows us to isolate spillover effects from direct policy effects. Specifically, we exclude siblings who ever received SpEd services prior to 5th grade, dropping 49,321 students, or 39% of siblings.<sup>20</sup> This restricted sample, which we refer to as our “sibling spillover” sample, contains 75,625 siblings, of whom 36% have a younger SpEd sibling and 64% have an older one.

### 3.3 Summary Statistics

Table 1 reports summary statistics for 5th grade cohorts enrolled between the 1993-94 and 2006-07 school years. Columns 1-4 focus on SpEd students, while Columns 5-8 focus on GE students. In the full sample of public school students (Columns 1 and 5), SpEd students are more likely to be male, qualify for free or reduced-price lunch (FRL), and be Black. They are also slightly more likely to be classified as English Language Learners (ELL), perform worse on standardized exams (conditional on taking the exam), and experience lower educational attainment. These patterns illustrate the negative selection into SpEd and underscore the difficulty in identifying the causal impacts of SpEd programs. The most common disability category is a learning disability (over 60%), and approximately 85% of SpEd students spend all or most of the day in GE classrooms.

Next, within both SpEd and GE student groups, we compare those who matched to the

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<sup>19</sup>A downside of extending the cohorts is that diagnosis by 5th grade is no longer made under the same policy environment. Reassuringly, as discussed in more detail in Section 5.4, we find very similar results when we instead focus on GE siblings who are in 5th grade cohorts between 1999-00 and 2004-05.

<sup>20</sup>As shown in Appendix Table A.6, these restrictions have little effect on our estimates.

TBI (and were born in Texas) to those who did not. Overall, the matched sample is very similar to the overall population (Columns 1 vs. 2 and Columns 5 vs. 6). The only notable difference is in the proportion of students classified as ELL, which is expected given that the TBI sample is limited to those born in Texas and therefore excludes those who are foreign born.

Turning to the subset of students with siblings, we find that they are slightly more advantaged. Partly, this is explained by how we identify siblings, which requires siblings come from families where both the mother and father are listed on the birth record. Both GE and SpEd students with siblings tend to have higher test scores, better long-run outcomes, and are less likely to qualify for FRL. They are also less likely to be Black, which is consistent with the fact that Black mothers are more likely to be single parents than mothers of other races (McLanahan & Percheski, 2008). It is important to note that while this does not affect the internal validity of our estimates, in terms of external validity our sample is slightly more advantaged than the overall student population in Texas. We discuss the implications of this in more detail in Appendix C.

Finally, Column 8 presents our main analysis sample: GE students with siblings in SpEd. Compared to the full GE sibling sample in Column 7, these students are more likely to be eligible for FRL, and they tend to have lower test scores and educational attainment. This pattern aligns with existing research showing that students in SpEd are disproportionately from lower socioeconomic backgrounds (Schifter, Grindal, Hehir, & Schwartz, 2019). On average, students in this sample have one sibling in SpEd and come from families with approximately three children.

## 4 Empirical Strategy

### 4.1 DiD Estimates: Direct Impact of the SpEd Enrollment Target

We begin by estimating the causal impact of policy pressure to reduce SpEd enrollment on students receiving SpEd services, following the approach in Ballis and Heath (2021) but restricting the sample to those with siblings. To capture variation in policy pressure, we leverage two key sources. First, districts with higher pre-policy SpEd enrollment rates faced stronger incentives to reduce those

rates, so policy effects should increase with a district’s pre-policy SpEd rate.<sup>21</sup> Second, students in differing grades at the time of implementation experienced varying levels of exposure depending on how many years they remained in school after 2004–05. Students who were closer to graduation were less exposed, while those early in their schooling experienced more years under the policy.

On the sample of SpEd students with siblings our DiD estimating equation thus takes the form:

$$Y_{ikd} = \beta_0 + \beta_1(\text{SERate}_d^{\text{Pre}} \times \text{FracExposed}_k) + \lambda_1 X_i + \lambda_2 Z_{ikd} + \gamma_d + \phi_k + \varepsilon_{ikd} \quad (2)$$

where  $Y_{ikd}$  is either an indicator for SpEd removal or a long-run outcome for student  $i$  in 5th grade cohort  $k$  in district  $d$ . The model includes fixed effects for 5th grade district ( $\gamma_d$ ) and cohort ( $\phi_k$ ), as well as baseline controls for student and district characteristics. Specifically the vector  $X_i$  includes an indicator for gender, race, FRL status, ELL classification, gender-race interactions, primary disability type, unmodified exam indicator, and the level of classroom inclusion, measured at baseline.  $Z_{ikd}$  includes the baseline shares of students by race, FRL status, ELL classification, and gender, measured separately for their SpEd cohort. Standard errors are clustered by the student’s 5th grade school district.

The interaction term  $\text{SERate}_d^{\text{Pre}} \times \text{FracExposed}_k$  captures policy exposure.  $\text{SERate}_d^{\text{Pre}}$  measures treatment intensity at the district level and is defined as the percentage points above the 8.5 percent target in the student’s 5th grade district in 2004–05, and is set to zero for districts already below the threshold. This is interacted with  $\text{FracExposed}_k$ , which measures the fraction of years a student was enrolled under the policy between 5th and expected 9th grade.<sup>22</sup> We end exposure in 9th grade, as this precedes typical high school dropout decisions ([Texas Education Agency, 2017](#)).

$\beta_1$  captures the average effect of policy pressure to reduce SpEd enrollment on student outcomes. The identifying assumption is that, in the absence of the policy, districts facing greater versus lesser pressure would have followed similar trends in outcomes. [Ballis and Heath \(2021\)](#)

<sup>21</sup>While the district-level treatment is continuous, it can be conceptualized as districts under greater policy pressure forming the “treated” group and those under less pressure forming the “control” group.

<sup>22</sup>We use expected 9th grade (four years after 5th grade) to assign consistent exposure within cohorts and avoid overstating treatment for students who repeated a grade. Appendix [Table A.1](#) illustrates exposure by 5th grade cohort.



provide several pieces of evidence supporting this assumption for SpEd students. We further assess its validity for the sample of SpEd students *with siblings* using an event-study specification that replaces the continuous exposure measure with 5th grade cohort indicators. These estimates show parallel pre-trends across districts with high and low pre-policy SpEd rates. Additionally, Panel A of Appendix [Table A.2](#) shows that baseline demographics are uncorrelated with policy exposure, providing further evidence that districts with different pre-policy rates had similar student composition and would likely have continued on parallel trends absent the policy.

## 4.2 DiD Estimates: Impact of the SpEd Enrollment Target on Siblings

To identify sibling spillovers, we draw on the same two sources of variation used to estimate the direct effects, except instead of using a student’s own policy exposure, we use their focal sibling’s exposure. First, we compare students from the same districts and 5th grade cohorts whose focal SpEd siblings had varying degrees of exposure to the policy, based on the number of years they were exposed after 5th grade. Focal siblings exposed earlier in their schooling were more likely to be directly affected by the policy and are therefore expected to generate stronger spillover effects. Second, we exploit cross-district variation in baseline SpEd rates: students with focal siblings in districts with higher SpEd rates were significantly more likely to experience reductions in services than those in lower-rate districts, making their families more exposed to the policy change.

For those with SpEd siblings, our DiD estimating equation takes the following form:

$$Y_{jickd} = \delta_0 + \delta_1(\text{SERate}_d^{\text{Pre}} \times \text{FracExposed}_k) + \tau_1 X_i + \tau_2 Z_{idc} + \tau_3 X_j + \tau_4 Z_{jdc} + \gamma_d + \phi_c + \varepsilon_{jickd} \quad (4)$$

where  $Y_{jickd}$  represents a long-run outcome (like high school completion or college enrollment) for sibling  $j$  of focal student  $i$ . The specification includes fixed effects for student  $j$ ’s own 5th grade cohort ( $\phi_c$ ) and their focal sibling  $i$ ’s 5th grade district ( $\gamma_d$ ).<sup>23</sup>  $\text{SERate}_d^{\text{Pre}} \times \text{FracExposed}_k$  captures the focal siblings policy exposure as defined in Section 4.1. The model includes individual and district-level controls measured at baseline (i.e. 5th grade). For focal students, individual

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<sup>23</sup>Many siblings attend the same district, and results are insensitive to instead using student  $j$ ’s own 5th grade district.

level control ( $X_i$ ) and district-level controls ( $Z_{idc}$ ) are as previously defined. We include analogous individual ( $X_j$ ) and district-level ( $Z_{jdc}$ ) controls for siblings.<sup>24</sup> Lastly, all standard errors are clustered by the focal student's 5th grade district.

$\delta_1$  captures the average effect of having a sibling who is more likely to lose SpEd. We interpret this as a sibling spillover effect rather than a direct policy effect, as our sample is restricted to GE students unlikely to be directly affected by SpEd placement changes.<sup>25</sup> The identification assumption is that, absent their focal sibling's policy exposure, GE siblings in districts facing stronger versus weaker policy pressure to reduce SpEd would have followed similar outcome trends.

To assess the plausibility of this assumption, we perform similar checks to those described in Section 4.1. First, we estimate an event-study model that replaces  $\text{FracExposed}_k$  with indicators for the focal sibling's 5th grade cohort. This allows us to visualize outcome differences between those with SpEd siblings in high versus low baseline SpEd enrollment districts before and after policy implementation. To check whether parallel trends were likely to continue in the absence of the policy, we check whether focal siblings policy exposure is correlated with baseline sibling demographics (measured in 5th grade). Specifically, we replace each of our covariates as outcomes, as well as predicted high school completion and college enrollment.

Panel B of Appendix Table A.2 presents these results. After accounting for the fixed effects in our model, there is minimal association between the focal students' treatment and their siblings' characteristics or predicted outcomes. We do observe modest but statistically significant increases in the likelihood of being Hispanic and corresponding declines in the likelihood of being White. As we examine an extended cohort window during a period of significant demographic change in Texas driven by immigration, it is not surprising that some sibling characteristics would be correlated with treatment intensity, even if only coincidentally. Importantly, these demographic

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<sup>24</sup>Because our main siblings sample does not include children in SpEd, we omit the following individual controls: primary disability type, unmodified exam indicator, and the level of classroom inclusion.

<sup>25</sup>Section 6.1 shows that direct policy impacts operating through classroom spillovers are mostly null and, for college enrollment, modestly negative. Any such effects would therefore attenuate rather than explain the positive spillover effects we document.

shifts predict *lower* educational attainment, so any compositional bias would attenuate rather than explain our positive effects.<sup>26</sup>

To estimate the policy change’s impacts on siblings’ yearly achievement and behavioral outcomes, we use a slightly modified version of Equation 4. In this specification we interact the focal siblings policy exposure ( $SERate_d^{Pre} \times \text{FracExposed}_k$ ) with a post-policy indicator, equal to 1 for years after 2005. This allows us to identify whether the relationship between treatment intensity and outcomes shifts following the policy’s introduction. Instead of using 5th grade cohort indicators ( $\phi_c$ ), we include year-by-grade ( $\phi_{gt}$ ) and grade-by-school ( $\phi_{gs}$ ) fixed effects. We measure short-run outcomes between 3rd and 8th grade.<sup>27</sup> Students appear multiple times in the regression, with each observation weighted by the inverse of the number of times they appear in the sample.

## 5 Results

### 5.1 Direct Impacts

#### *SpEd Removal*

We begin by establishing that the policy pressure to reduce SpEd enrollment increased the likelihood of SpEd removal for SpEd children with siblings. To do so, we examine the relationship between each child’s 5th grade cohort and their district’s distance above the 8.5% cap. Panel (a) of Figure 3 presents these event study estimates, where the outcome is an indicator for SpEd removal. The figure shows that before the policy, students in districts with high and low baseline SpEd rates lost SpEd services at similar rates, providing compelling evidence for the parallel trends assumption. In contrast, for students exposed to the policy after 5th grade, being in a district with higher pre-policy SpEd enrollment is associated with a higher likelihood of SpEd removal. This effect grows with additional years of exposure, consistent with two mechanisms: (1) districts with higher enrollment

<sup>26</sup>Importantly, restricting the sample to siblings in 5th grade cohorts between 1999-00 and 2004-05 eliminates the impacts on Hispanic ethnicity. Our main results remain very similar in this restricted sample, indicating that compositional changes are unlikely to drive our findings.

<sup>27</sup>During this period, several policy changes affected testing exemptions for ELL and SpEd students. To avoid bias from selection into test-taking, we restrict our test score analysis to children who were never classified as ELL or SpEd.

face stronger incentives to remove students, especially when more years remain to contribute to the district's SpEd enrollment rate, and (2) removals often take time, making effects stronger for students with more years of policy exposure.

The difference-in-difference estimates are presented in [Table 2](#). Panel A replicates the results of [Ballis and Heath \(2021\)](#) for *all* SpEd students and finds that the policy led to significant increases in SpEd removal. Results remain similar when limiting the sample to those with siblings in Panel B, indicating that the policy's impact on SpEd removal was similar regardless of whether they had siblings or not. The estimates remain stable whether we include only 5th grade district and cohort fixed effects in odd-numbered columns or add the full set of individual and cohort-level controls in even-numbered columns. Among SpEd students with siblings in the average district (4.5 percentage points above the SE enrollment cap in 2005), we find that full exposure to the policy between 5th and 9th grade led to a 5 percentage point (16%) increase in SpEd removal, a magnitude very similar to the effect observed for the overall SpEd population. This large policy-driven change in SpEd removal provides an ideal setting to study spillover effects on siblings.

Moreover, as shown in Column 1 of Appendix [Table A.3](#), SpEd removals are concentrated among students in the high-impact sample, who experienced a 5.5 percentage point (or 15.4%) increase in the likelihood of SpEd removal.<sup>28</sup> In contrast, students with physical or more severe cognitive disabilities in the low-impact sample shown in Column 2 saw little change in SpEd removal. This is consistent with these conditions having clearer diagnostic criteria and less discretionary eligibility determinations, which are harder to manipulate.

### *Educational Attainment*

Having established that the enrollment cap led to more students being removed from SpEd, we next examine how these removals affected students' long-term educational attainment. Event study

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<sup>28</sup>The difference-in-differences estimates align with the event-study patterns in Panels (a) and (b) of Appendix [Figure A.2](#). For the high impact sample there are no pre-policy differences in SpEd removal, but a sharp increase in SpEd removal for those in districts with higher baseline SpEd enrollment that increases with the years of policy exposure. In the low-impact sample, the event study coefficients hover around zero both before and after the policy.

estimates are shown in panels (b) and (c) of [Figure 3](#). Like the SpEd outcome these plots demonstrate evidence of parallel trends and show that the negative impacts of the policy on educational attainment are increasing with the number of years of policy exposure.

Columns 3-6 of [Table 2](#) presents the difference-in-difference estimates of the impact of the policy on educational outcomes. For the full sample, being fully exposed between 5th and expected 9th grade decreased high school completion by 2 p.p. (2.7 percent) and college enrollment by 1 p.p. (3.3 percent) at the average district.

When we restrict the analysis to students with siblings, the high school completion effects remain similar in magnitude. However, while the college enrollment coefficients remain negative, they lose statistical significance. This attenuation likely reflects the positive selection of SpEd students with siblings who come from slightly more advantaged backgrounds than SpEd students without siblings, or the slightly smaller sample which could make it more difficult to detect effects.

Consistent with the policy’s impact on SpEd removal, these negative educational impacts are concentrated among children in the high-impact sample (see Appendix [Table A.3](#) for difference-in-difference estimates and Appendix [Figure A.2](#) for event-study estimates). Students with physical or severe cognitive disabilities show no declines in SpEd enrollment or their educational attainment, providing compelling evidence that the policy’s impact operated through SpEd removal.

## 5.2 Sibling Spillovers

### *Educational Attainment*

We next turn to estimating the impact of having a sibling who was more likely to lose SpEd because of the policy. Because our sibling sample is restricted to children who were never identified for SpEd prior to 5th grade, any effects are likely to operate through spillovers rather than direct policy exposure. We therefore focus on educational attainment outcomes only.<sup>29</sup> We first estimate an

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<sup>29</sup>Because most initial diagnoses occur before 5th grade, students in our sample are unlikely to be newly identified for SpEd afterward. Consistent with this, we find no evidence of sibling spillovers on SpEd identification in 9th grade. These results are available upon request.

event-study specification in [Figure 4](#) examining how the relationship between GE sibling outcomes and their district’s policy pressure to reduce SpEd enrollment varies across focal siblings’ 5th grade cohorts. The figure shows that GE sibling outcomes were similar in districts with varying policy exposure when the focal sibling had no policy exposure, providing compelling evidence for parallel trends. However, we find that having a focal sibling with more years of policy exposure improves educational attainment among GE students in districts with high baseline SpEd rates.

Comparing sibling spillovers between families with focal siblings in the high versus low impact samples provides a useful test of our identification strategy. Since only high impact focal students lose SpEd services, spillovers should be concentrated among their siblings. Reassuringly, as Columns 3 and 4 of Appendix [Table A.3](#) demonstrate, the impacts are indeed concentrated among siblings of focal students in the high impact sample (see also Appendix [Figure A.3](#) for event-study estimates). This concentration of positive spillovers among high impact families suggests the effects operate through the focal sibling’s SpEd removal rather than other channels.

The difference-in-difference estimates are presented in [Table 3](#). Consistent with the event-studies, we find significant positive spillovers from having a focal sibling who is more exposed to the policy (and more likely to lose SpEd). Specifically, GE students whose focal SpEd sibling was fully exposed to the policy from 5th through expected 9th grade experienced an increase of 1.9 p.p. (or 2.6 percent) in high school completion and 1.7 p.p. (or 3.4 percent) in college enrollment at the average district. We come to similar conclusions using a summary index which averages the standardized outcomes to provide a single measure. Columns 3-4 of Appendix [Table A.3](#) demonstrates that these sibling spillovers are entirely driven by families with high impact siblings.

#### *Academic Achievement and Other Short-Run Outcomes*

To better understand what led to the longer-run gains in educational attainment, we next examine short-run spillover effects on academic achievement, attendance, grade repetition and discipline. Difference-in-differences estimates from a slightly modified version of [Equation 4](#) are reported in Appendix [Table A.5](#). Academic achievement increases for both the overall and high impact family

subsample, consistent with the longer-run attainment gains. These coefficients reach conventional significance only for families in the high impact sample. For that subgroup, reading scores increase by approximately 0.02 SD for students with fully exposed focal sibling in the average district and the combined index shows similarly sized, but less precise, improvements. The magnitude of this effect compares very closely to [Carrell and Hoekstra \(2010\)](#) who study the classroom spillovers of disruptive peers. We find reductions in absenteeism (driven by high impact families) alongside a modest increase in discipline (1 p.p., or 6 percent) for the full sample.<sup>30</sup>

One caveat of these findings is that we observe outcomes for only a limited post-policy period. Most focal students likely did not lose SpEd access until two or three years after the 2005 policy. Additionally, if we assume it takes at least one year for sibling spillovers or parental resource reallocation to materialize, then the earliest measurable effects on siblings' test scores would occur at a minimum 3 years after the implementation of the policy (or 2008). Since we only measure test scores in grades three through eight and the youngest cohort was in fifth grade in 2007, we can observe post-policy outcomes for just three of our fourteen cohorts, and only in their later grades.<sup>31</sup> With this caveat on mind, we find these results indicative of improvements in achievement, but only suggestive given the limited post-policy period we have to measure these outcomes.

### 5.3 Heterogeneous Impacts

We examine heterogeneity in effects by income and race. Results are shown in [Table 4](#), where Panels A and B report estimates for focal children, and Panel C for their siblings. Because the marginal SpEd student may differ across race and income, heterogeneous effects could either reflect differences in focal children's initial conditions or in how families respond to SpEd removal.

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<sup>30</sup>The increase in discipline appears counterintuitive given the long-run gains. We view it as plausible that the transition period of having a sibling lose services could create household tension, and siblings may act out in response. However, the modest magnitude suggests this does not offset the positive longer-run effects.

<sup>31</sup>Our sibling sample spans fourteen fifth-grade cohorts (1994-2007). Assuming 2007 is the first year in which SpEd students were removed, a cohort must still be in grades 3-8 after 2007 to have post-policy test scores. The 2007 fifth-grade cohort can be observed post-policy in grades 6-8 (years 2008-2010); the 2006 cohort in grades 7-8 (2008-2009); and the 2005 cohort in grade 8 only (2008). All earlier cohorts (1994-2004) had already completed eighth grade by 2007.

Nonetheless, these analyses provide suggestive evidence on the potential role of parental investments, as outlined in Section 2.3 and tested more directly in Section 6.

By income, all focal children experienced SpEd removal, but declines were largest for lower-income children. This may reflect the fact that lower-income children are more likely to be near the eligibility margin with less severe conditions, or that higher-income parents were better able to challenge school recommendations of SpEd removal (Horvat, Weininger, & Lareau, 2003; Koseki, 2017). By race, SpEd removal was largest for Blacks, followed by Hispanics, with the smallest impacts for White children. The larger declines among Black students may reflect their over-identification for SpEd (e.g., National Center for Learning Disabilities (2020)), making them more likely to have less severe disabilities and therefore easier to remove.

Long-term impacts also varied by income and race. Consistent with Ballis and Heath (2021), low-income and Hispanic students experienced the largest declines in educational attainment. Hispanics tend to be underrepresented in SpEd, suggesting those who are identified have genuine need for services and are most harmed by removal. Although Black children had the highest rates of removal, they showed no negative long-run effects, supporting the hypothesis that some may be inappropriately placed in SpEd and could benefit from removal (Ballis & Heath, 2019).

Turning to sibling spillovers, we find positive effects across nearly all subgroups, consistent with parents reallocating attention and resources to the GE sibling when they perceive the SpEd child as doing better. Most impacts are statistically significant, except for those with focal siblings who are sometimes FRL and Hispanic. By race, the largest gains were for Black siblings, a pattern that is consistent with the absence of long-run declines for their focal siblings. In contrast, Hispanic siblings show no statistically significant effects despite the relatively large sample size. This null result is consistent with Hispanic children facing higher rates of under-identification for SpEd and experiencing the most negative long-term impact of SpEd removal in our study. Consequently, parents of Hispanic SpEd children may have been the least likely to interpret their child's SpEd removal as a positive signal, reducing the likelihood of resource reallocation to siblings.



By income, higher-income focal children experienced the smallest declines in SpEd enrollment, yet their siblings saw somewhat larger spillovers. As with Black families, the lack of a statistically significant negative long-run effects for higher-income SpEd children makes it plausible that parents viewed removal as a positive sign. In addition, higher-income families tend to invest more time and resources in their children (Guryan, Hurst, & Kearney, 2008; Kalil, Ryan, & Corey, 2012), which suggests that the benefits for GE siblings after parental reallocation of time and resources may be amplified in these higher income families.

## 5.4 Robustness

Our key identification assumption is that, in the absence of the policy, outcomes for those with SpEd siblings would have followed similar trends across districts with varying levels of SpEd enrollment. The event study estimates support this assumption, showing no evidence of differential pre-trends. It is also reassuring that policy exposure did not predict changes in baseline characteristics that would have predicted an increase in outcomes.<sup>32</sup> Moreover, results for our placebo group (i.e. siblings of children in the low impact sample) suggest that they were not affected by the policy, providing additional evidence that the observed sibling spillovers are driven by the policy itself rather than by underlying differences in trends across districts.

We have interpreted the impacts on GE siblings as driven by sibling spillovers within the household rather than changes occurring in schools. Several pieces of evidence support this interpretation. First, as just noted, the sibling spillovers are concentrated among families with a focal child in the high-impact sample, the only subgroup that experienced policy-driven reductions in SpEd enrollment. Second, as will be discussed in more detail in Section 6.1, a child's own exposure to the policy has minimal or negative impacts on their outcomes, indicating that direct exposure cannot explain the positive effects we document among siblings.

To rule out alternative school-based channels, we consider whether school resources

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<sup>32</sup>As previously discussed, our extended sibling sample experienced modest demographic shifts during this period, with declines in the share of white students and increases in Hispanic students. These changes, if anything, would predict achievement declines rather than improvements.

changed in response to the 8.5% cap. One possibility is that districts facing pressure to reduce SpEd enrollment shifted resources from SpEd programs toward GE students, benefiting GE students schoolwide. However, [Ballis and Heath \(2021\)](#) show that the enrollment target had no significant impact on district-level SpEd or GE per-pupil spending, nor on student-teacher ratios, during the five years following policy introduction (see their Appendix Table A.8). This suggests that changes in school-based resources are unlikely to be driving our results.

An additional concern is that differential attrition could explain our results. In particular, parents may have responded to reduced access to SpEd by moving their children to private schools or out of state. If such attrition were to occur, it would likely bias our estimates against finding positive sibling spillovers, as families with the financial resources to leave the public school system are likely more advantaged. Nonetheless, we formally examine the possibility of attrition in Appendix Table [A.4](#). We also formally test for district switching. Although switching does not threaten identification given that treatment is assigned based on the focal siblings baseline district, extensive switching could attenuate estimated effects. The estimates for focal children are shown in Columns 1-2 and the spillover effects on their GE siblings in Columns 3-4.

Focal children show no differential attrition or district switching, suggesting out migration is unlikely to explain their outcomes. In contrast, siblings of exposed focal students are less likely to be enrolled and in their same district 4 years after their *focal sibling's* 5th grade. If the reason that siblings are leaving the public school system is to enroll in private schools (perhaps due to an increase in investments of their parents) this would likely attenuate rather than inflate our estimated siblings spillovers since this behavior is more common among wealthy families. These patterns provide evidence that our positive findings are unlikely to be driven by compositional changes and, if anything, may understate the true effects. As discussed in more detail in Section [6](#), siblings do switch to better districts within two years of their focal sibling's exposure which is consistent with the increased mobility we document here.

Our estimates are also robust to alternative sample definitions. As previously noted, our

main estimates rely on siblings of focal children who were never in SpEd prior to 5th grade and who belong to 5th grade cohorts from 1994 to 2007. As discussed in Section 3.2, this extended cohort window ensures that focal children from all treatment cohorts have both older and younger siblings represented in the sample. A limitation, however, is that those who enter 5th grade after 2005 may have been diagnosed under a different policy environment. To address this, we restrict the siblings to 5th grade GE cohorts from 2000 through 2005 to ensure that siblings were not directly affected, and may have been identified in the absence of the policy, or affected by an earlier policy related to SpEd testing exemptions. Column 2 of Appendix Table A.6 shows that results from this restricted sibling sample. Column 3 relaxes the restriction that the sibling never be in SpEd prior to 5th grade, focusing on *any* siblings of focal students. While the estimates remain similar, they are slightly smaller, in Columns (1) and (3) likely due to the inclusion of students directly affected by the policy, whose outcomes tend to worsen after the policy's introduction.

We next consider whether our results are robust to controls for birth order and prior achievement. Our preferred specification does not include a birth order control because younger siblings are more likely to be in the cohorts more affected by the policy, making birth order correlated with exposure and potentially soaking up meaningful identifying variation. Column 4 shows, however, that controlling for sibling birth order has little impact on the high school completion results, but reduces the coefficient on college enrollment, leaving the overall pattern of estimates largely unchanged. Because sibling's own prior achievement (their average of math and reading measured in grade 3) is determined before the policy change, it is not related to exposure and does not threaten identification. However, it is useful to confirm that pre-existing performance differences are not driving the results. Column 5 shows that adding this control leaves the estimates largely unchanged.

Finally, as will be discussed in more detail in Appendix C, our results are robust to how we group siblings together. Our main measure focuses on children who have the same parents and who attend school in the same or contiguous counties. Our results are very robust to stricter or less

strict criteria for name matching and geographic proximity of siblings.

## 6 Channels

What explains the positive spillovers from having a sibling more likely to lose SpEd? As outlined in Section 2, they could arise from direct sibling influences or from changes in parental investment. Parental investments may shift through a reallocation of time and resources among children or through changes in overall household resources (such as increases in parental labor supply).

We examine whether spillover effects differ by family characteristics to help distinguish between direct sibling influences and parental reallocation. Table 5 presents results examining age proximity, birth order, and family size. If sibling interactions were the primary channel we would expect larger spillovers for siblings closer in age. Instead, Columns 1-2 show that positive spillovers persist even for larger age gaps, though effects are slightly larger for those closer together.

Additional patterns point more strongly toward parental reallocation. Columns 3-4 show stronger positive spillovers when the SpEd sibling is older, consistent with parents having more time to adjust investments before younger siblings reach critical educational transitions. While older siblings could serve as role models, we find no evidence that focal sibling outcomes improve after declassification, suggesting spillovers are driven by parental responses to the classification change itself. Columns 5-6 show larger effects in two-child families than in families with three or more children, consistent with parental attention being more diffused in larger families.

Table 6 provides further evidence against a pure peer spillover mechanism. If peer influences were the primary channel, we would expect the strongest impacts for same-gender sibling pairs who are more likely to share peer groups and activities. Columns 1-3 show that same-gender siblings experience positive spillovers regardless of whether both are male or female. Among opposite-gender siblings, however, spillovers depend on the focal child's gender (Columns 4-6). When the focal child is female, spillovers are positive and comparable in magnitude to same-gender pairs. When the focal child is male, spillovers are null for female GE siblings. This asymmetric

pattern, in which effects differ by focal child gender rather than sibling gender similarity, is difficult to reconcile with peer influences as the sole mechanism.<sup>33</sup>

Although we do not directly observe parental investments, we explore one behavioral response that may reflect increased family investment for the child not identified for SpEd: school choice decisions. Appendix [Table A.7](#) presents results for the question: Do families move their GE children to higher-performing districts after their SpEd child is more likely to lose services? Two years after the focal child's exposure, we find that their siblings are more likely to move to districts with higher average test scores and gain scores. These moves are concentrated among families in the high impact sample. We do not find similar improvements in district quality among families with a child with a low impact disability, consistent with the fact that the latter group was not directly affected by the policy. Improved school quality may reflect fewer location constraints after the focal child's SpEd removal, allowing families to prioritize the GE sibling's school.

While we lack data on parental labor supply, it is possible that SpEd removal could influence parental labor supply. Recent evidence shows that having a child with a *severe* disability has a negative impact on mother's labor market earnings ([Gunnsteinsson & Steingrimsdottir, 2019](#)). Given we are focused on the loss of SpEd for students who are on *the margin* of SpEd placement decisions with less severe conditions, it is unclear whether we would expect a strong labor market response from parents. However, we cannot definitively rule out that increased parental labor supply contributes to the improved sibling outcomes.

While direct sibling interactions likely play some role, the overall pattern of results suggests that parental investment reallocation likely plays the larger role in this setting. Our finding that parental investments are the most likely mechanism aligns with several empirical studies. For example, [Black et al. \(2021\)](#) show that siblings spillovers are largest in families where time or financial constraints are most likely to bind. Similarly, studies examining how a shock to one

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<sup>33</sup> Although we cannot isolate the precise mechanism, one possibility is that parental responses to SpEd removal differ by the focal child's gender. If the loss of SpEd services for male students induces greater compensatory adjustments such as increased supervision or structure, male GE siblings may benefit through shared routines, while female siblings experience the household disruption without comparable gains.

sibling’s health affects other siblings’ academic outcomes also point to parental responses as the main driver. [Daysal, Simonsen, Trandafir, and Breining \(2022\)](#), for instance, find that improved maternal mental health is likely the primary channel behind the observed spillovers in their setting.

## 6.1 Direct Impacts on GE Siblings

Thus far, we have shown that having a sibling who is more likely to lose SpEd has a positive effect and have attributed this to sibling spillovers. Existing evidence from [Ballis and Heath \(2021\)](#) suggests that the policy had negative impacts on SpEd students themselves and their classmates. As such, we have assumed that if students in our sibling sample were also exposed to the policy through their own cohort (either directly or through classroom peer effects) these negative impacts would likely attenuate our estimates toward zero.

To assess this possibility, we formally estimate the effect of the policy on our sibling sample using their own exposure to the policy, rather than their focal sibling’s exposure, as specified in Equation 2. [Table A.8](#) shows the direct impacts of the policy on these students. Overall, we do not find strong evidence of a direct effect on most outcomes. We do observe a modest decline in college enrollment, significant at the 10% level, consistent with the negative effects documented in [Ballis and Heath \(2021\)](#). Taken together, these findings suggest that our estimates of spillover benefits from focal sibling exposure may represent a lower bound of the true effects, as any negative direct or peer impacts from the siblings’ own cohort exposure would likely attenuate the observed gains from sibling spillovers.

## 7 Conclusion

In this paper, we examine the effects of changes in a sibling’s SpEd access on the medium-and long-run educational outcomes of their GE siblings. To do so, we constructed a novel linked administrative dataset that follows the universe of TX students from childhood into adulthood and identifies sibling relationships. We combine these data with a TX policy that capped district-level SpEd enrollment at 8.5 percent, and generated large, exogenous reductions in SpEd placement.

Our empirical strategy is a difference-in-differences design that leverages variation in policy exposure for the focal SpEd sibling. Specifically, focal students were more likely to be removed if they were in 5th grade cohorts exposed to the policy for longer and if they attended districts that were further above the 8.5 percent cap at baseline. To identify spillover effects on GE siblings, we exploit the fact that GE students in the same district and grade differ in how much policy exposure their SpEd sibling received based on the SpEd sibling's grade when the policy was implemented. By comparing spillover effects across high and low baseline enrollment districts, we can identify the impact of the policy on GE siblings' short and long-run educational outcomes.

We find that having a sibling who was more likely to lose SpEd generated positive spillovers on their GE siblings. Having a SpEd sibling fully exposed to the policy in the average district increases high school completion by 1.9 p.p. (or 2.5 percent) and college enrollment by 1.7 p.p. (or 3.4 percent). The results are driven by those with siblings in our high impact sample, who were more likely to lose SpEd as a result of the policy. While we find no overall changes in academic achievement, there are modest gains among students with high impact siblings.

We provide preliminary evidence suggesting that the sibling spillovers we estimate are driven, at least in part, by changes in parental investments. The effects are largest in higher-income families with more resources to reallocate and in two-child families where trade-offs are most salient. Positive effects persist even across large age gaps where sibling interactions are minimal and occur for opposite-gender pairs when the focal child is female, making a pure peer effects explanation less likely. We also find that parents are more likely to move their GE children to higher-quality schools after the SpEd sibling becomes more likely to lose services. However, these findings are only suggestive, and future work with richer data on parental decision-making could help confirm this mechanism.

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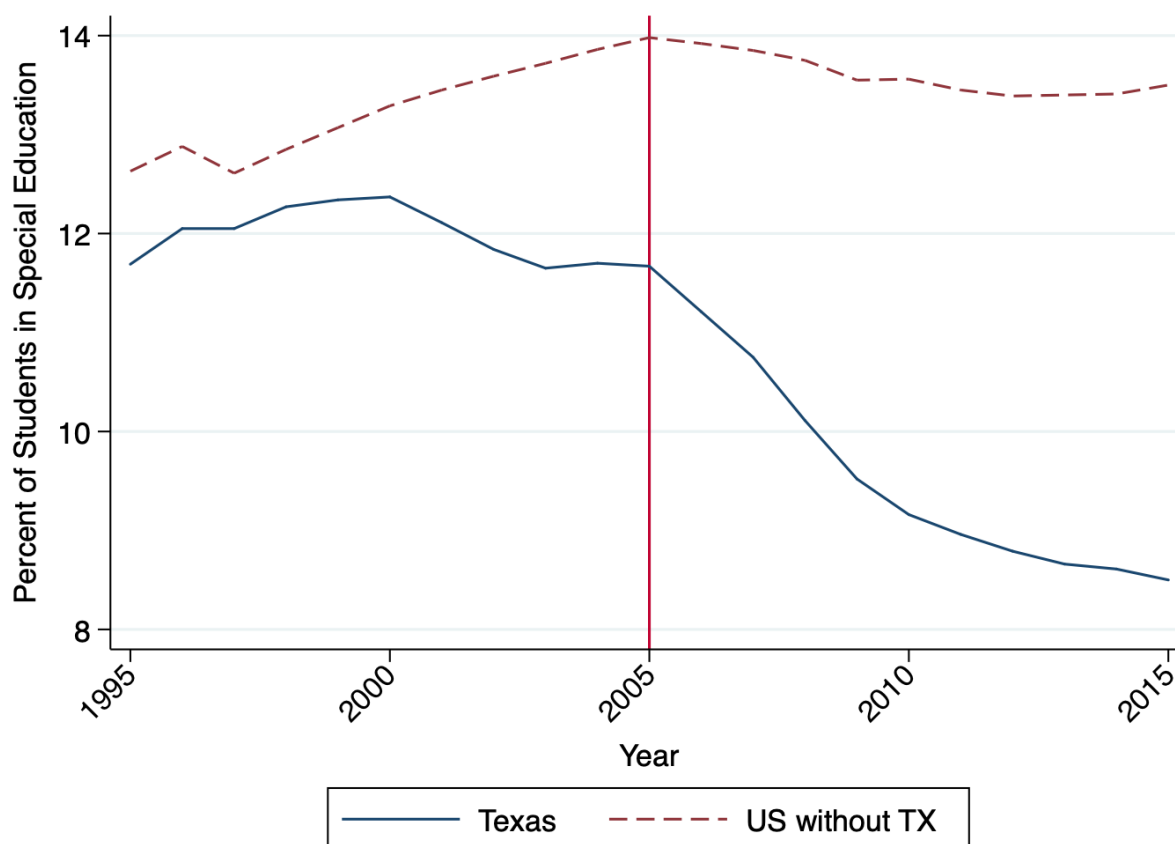


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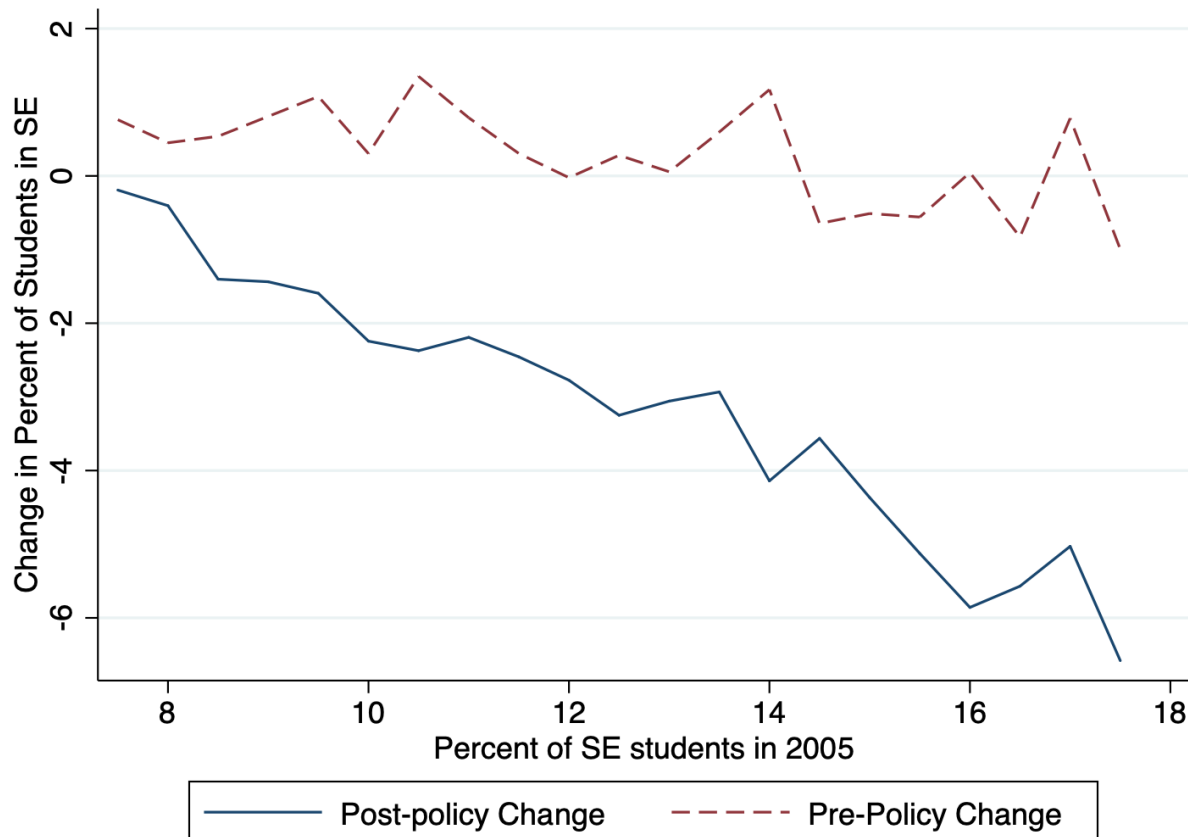
## Figures/Tables

**Figure 1:** District Level SpEd Enrollment in Texas vs the rest of the United States (1995-2015)



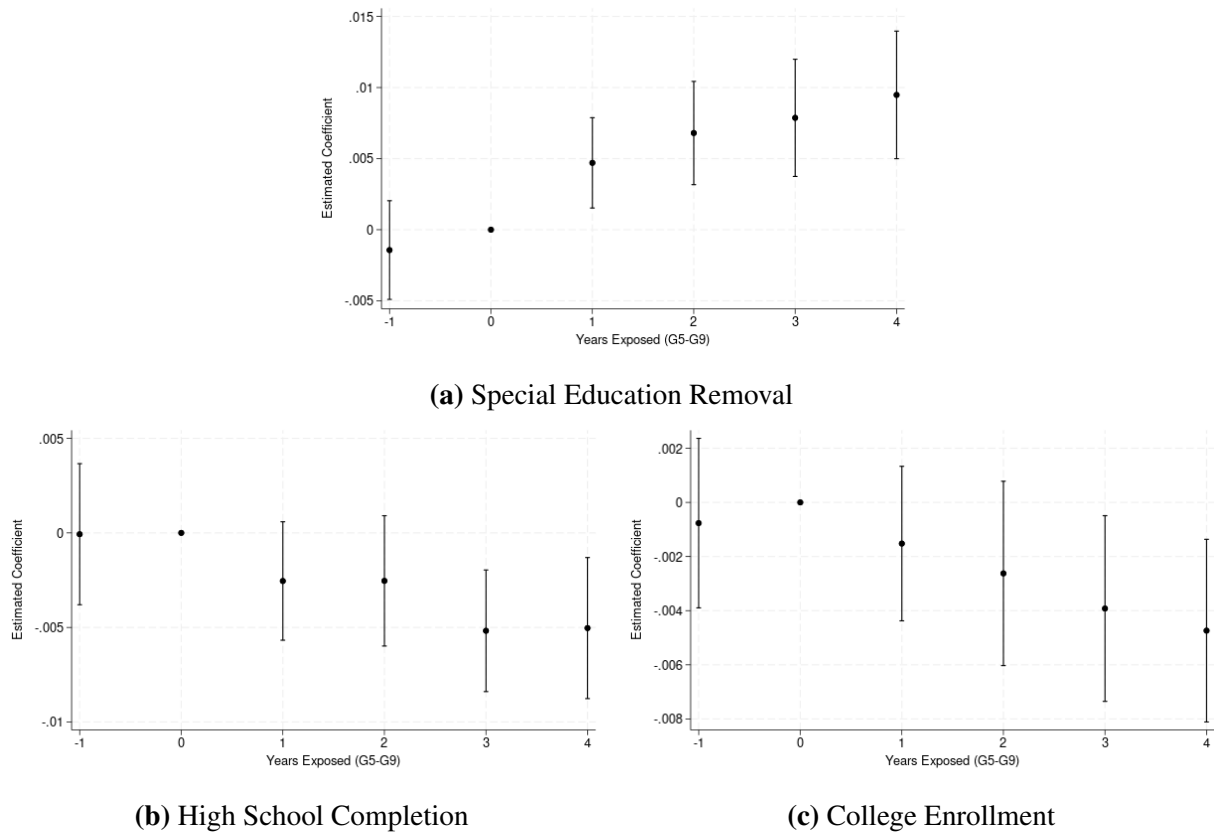
**Note:** This figure shows trends in special education (SpEd) enrollment as a percentage of total student enrollment from 1995 to 2015. The solid blue line represents Texas, while the dashed red line represents the United States excluding Texas. The vertical red line at 2005 marks the implementation of Texas's 8.5% cap policy on SpEd enrollment. While SpEd enrollment rates remained relatively stable or increased nationally, Texas experienced a sharp decline following the policy change.

**Figure 2:** Change in District Level SpEd Enrollment During the Pre-Policy Period (2000-2005) and the Post-Policy Period (2005-2010)



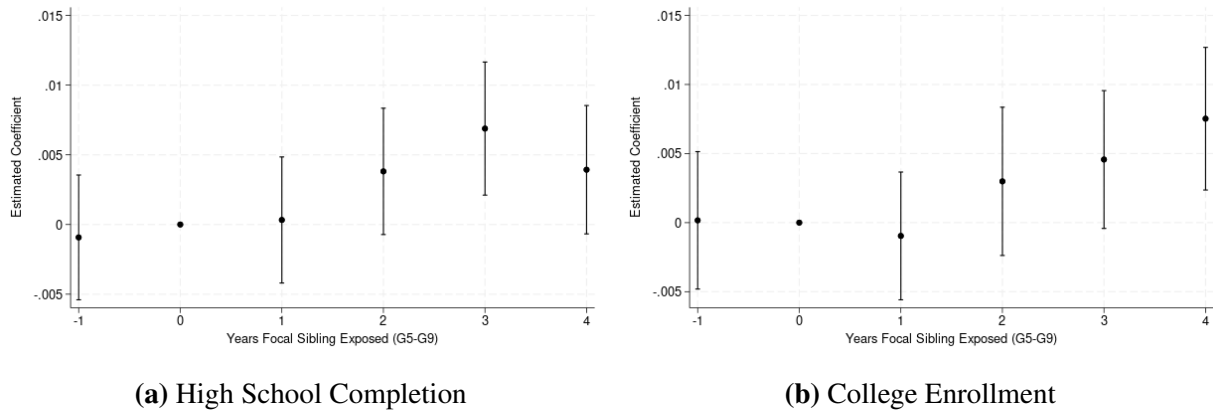
**Note:** The figure plots changes in district special education (SpEd) enrollment using the district’s baseline SpEd enrollment rate for the 2004–05 school year (when the policy was implemented). The dashed red series shows the pre-policy change in SpEd enrollment (2000–01 to 2004–05), while the solid blue series shows the post-policy change (2004–05 to 2009–10). While Pre-policy changes are small and display little systematic relationship with baseline SpEd enrollment, following the policy, districts with higher baseline SpEd enrollment exhibit much larger declines, consistent with the Texas 8.5% cap generating a sharp contraction in SpEd access.

**Figure 3:** Event Study Estimates of the Impact of the Policy on Special Education Removal and Educational Attainment for Special Education Students with General Education Siblings



**Note:** These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between 5th grade cohort dummies and the 2004-05 district SpEd rate. The outcome in figure (a) is Special Education (SpEd) Removal by expected 9th grade. Figure (b) shows high school completion, and Figure (c) shows college enrollment, measured within four years of expected high school graduation. Event time is computed by subtracting 9 from the grade each 5th grade cohort was expected to be enrolled in during the first year of the policy (or the 2005-06 school year). The sample includes 5th-grade cohorts enrolled in SpEd from 1999–00 to 2004–05 who have at least one general-education (GE) sibling with a 5th-grade cohort in the same window (GE status measured in 5th grade). The 1999–00 5th-grade cohort is omitted, so all estimates are relative to that cohort. This regression includes controls for 5th grade cohort indicators, district fixed effects, gender, race, FRL status, ELL classification, gender-race interactions, baseline primary disability, an indicator for whether a student took the unmodified version of the exam, level of classroom inclusion (all measured at baseline in 5th grade). This regression also includes controls for district controls that include tax base wealth per-pupil and the percent of tax base wealth that is residential, as well as the percentage of students in a district and cohort belonging to each racial group, receiving FRL, classified as ELL, and who are male for the SpEd sample and the full sample. Standard errors are clustered at the district level.

**Figure 4:** Event Study Estimates of Sibling Spillovers from the Policy on Educational Attainment



**Note:** These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between the focal child's 5th grade cohort dummies and their 2004-05 district SpEd rate. The outcome in figure (a) is high school completion, and Figure (b) shows college enrollment, measured within four years of expected high school graduation. Event time is computed by subtracting 9 from the grade each focal child's 5th grade cohort was expected to be enrolled in during the first year of the policy (or the 2005-06 school year). The sample consists of students from 5th grade cohorts 1999-00 to 2004-05 who have at least one sibling enrolled in special education. The sibling's special education status is determined as of their 5th grade year, and these siblings must also be from 5th grade cohorts within the 1999-00 to 2004-05 period. To focus on spillover effects, we restrict the analysis to students who had no prior special education enrollment before reaching 5th grade. The focal 5th grade cohort from 1995-96 is omitted, so estimates are relative to that cohort. This regression includes controls for 5th grade cohort indicators, district fixed effects, gender, race, FRL status, ELL classification, gender-race interactions, baseline primary disability, an indicator for whether a student took the unmodified version of the exam, level of classroom inclusion (all measured at baseline in 5th grade). This regression also includes controls for district controls that include tax base wealth per-pupil and the percent of tax base wealth that is residential, as well as the percentage of students in a district and cohort belonging to each racial group, receiving FRL, classified as ELL, and who are male for the SpEd sample and the full sample. Controls for the GE sibling and focal SpEd sibling are included. Standard errors are clustered at the district level.

**Table 1: Summary Statistics: Special Education vs General Education Students**

	Special Education Students				General Education Students			
	(1) Full	(2) Matched TBI	(3) Siblings	(4) SpEd Siblings	(5) Full	(6) Matched TBI	(7) Siblings	(8) SpEd Siblings
<b>Demographics</b>								
Hispanic	0.395	0.397	0.439	0.434	0.412	0.400	0.416	0.471
Black	0.186	0.190	0.0925	0.0928	0.136	0.142	0.0753	0.0853
White	0.407	0.404	0.459	0.464	0.422	0.436	0.488	0.433
Free lunch (5th grade)	0.638	0.641	0.601	0.599	0.518	0.506	0.465	0.609
ELL (5th grade)	0.150	0.132	0.149	0.146	0.121	0.0859	0.0882	0.110
Male	0.658	0.671	0.673	0.674	0.487	0.506	0.511	0.485
<b>Test Scores (4th grade)</b>								
Standardized math	-0.691	-0.716	-0.620	-0.643	0.0735	0.0537	0.141	-0.139
Standardized reading	-0.720	-0.750	-0.684	-0.707	0.0608	0.0394	0.104	-0.187
Regular test math	0.358	0.367	0.404	0.405	0.774	0.833	0.834	0.818
Regular test reading	0.301	0.307	0.337	0.339	0.775	0.833	0.834	0.818
<b>Educational Outcomes</b>								
Graduated HS	0.704	0.705	0.738	0.730	0.789	0.792	0.820	0.740
Attended college	0.244	0.246	0.278	0.308	0.546	0.570	0.601	0.489
Graduated college	0.044	0.043	0.056	0.062	0.189	0.189	0.214	0.140
<b>Disability Type (5th grade)</b>								
Learning disability	0.615	0.618	0.629	0.638	—	—	—	—
Speech impairment	0.131	0.129	0.149	0.148	—	—	—	—
Other health impairment	0.091	0.092	0.087	0.081	—	—	—	—
Emotional disturbance	0.070	0.070	0.054	0.054	—	—	—	—
Intellectual disability	0.050	0.049	0.040	0.040	—	—	—	—
Autism	0.015	0.015	0.015	0.012	—	—	—	—
Orthopedic impairment	0.011	0.011	0.010	0.010	—	—	—	—
Auditory impairment	0.011	0.010	0.010	0.010	—	—	—	—
Visual impairment	0.005	0.005	0.005	0.005	—	—	—	—
Deaf/blind	0.000	0.000	0.000	0.000	—	—	—	—
Malleable	0.907	0.909	0.919	0.922	—	—	—	—
Non-malleable	0.0927	0.0908	0.0811	0.0785	—	—	—	—
<b>Classroom Inclusion (5th grade)</b>								
Mainstream base	0.238	0.237	0.262	0.256	—	—	—	—
Resource room <50% base	0.620	0.622	0.619	0.623	—	—	—	—
Resource room 50%+ base	0.142	0.141	0.120	0.121	—	—	—	—
<b>Sibling Count</b>								
SpEd siblings	—	—	1.358	1.388	—	—	0.130	1.113
Any siblings	—	—	2.181	2.207	—	—	2.137	2.715
Observations	527654	416183	209244	188998	3307672	2430577	1326617	139132

**Note:** This table reports summary statistics on demographics, 4th-grade achievement, disability type, long-run educational outcomes, and family structure for 5th grade cohorts in Texas public schools from 1999–2000 to 2004–2005. Columns (1)–(4) show students in special education (SpEd) as of 5th grade; Columns (5)–(8) show general education (GE) students. Column (1) includes all SpEd students; Column (2) restricts to those matched to Texas birth records; Column (3) includes SpEd students with at least one identified sibling; Column (4) further restricts to those whose sibling appears in the analysis sample. Columns (5)–(8) follow the same structure for GE students, with Column (8) representing GE students who have a sibling in SpEd (our primary sibling spillover sample). Demographic characteristics (e.g., FRL, ELL, race/ethnicity, gender) are measured in 5th grade. Test scores are standardized based on 4th-grade performance. “Regular test” refers to the unmodified version of the standardized exam. College enrollment is defined as occurring within six years of expected high school graduation. Disability categories are shown only for SpEd students and reflect the primary 5th-grade diagnosis. Highly impacted disabilities include learning disabilities, emotional disturbance, speech impairments, and other health impairments (e.g., ADHD). Low-impacted conditions include autism, intellectual disabilities, and sensory impairments. Dashes (—) indicate measures not applicable to the given student population. The final panel summarizes observed family size based on sibling matches from birth records. “Any Siblings” counts all matched siblings; “Special Education Siblings” counts only those with SpEd placement. Because identification requires matched parental names and consistent spelling, these counts may understate true family size and reflect more advantaged, two-parent households.<sup>38</sup>

**Table 2:** The Direct Impact of the Policy on SE Students

	(1)	(2)	(3)	(4)	(5)	(6)
	SE Removal		HS Completion		College Enrolled	
<i>Panel A: Full Sample</i>						
Treatment	0.010*** (0.002) [0.046]	0.009*** (0.002) [0.039]	-0.004*** (0.001) [-0.019]	-0.004*** (0.002) [-0.020]	-0.001 (0.001) [-0.005]	-0.002* (0.001) [-0.011]
Mean (Y)	0.275	0.275	0.719	0.719	0.327	0.327
N	227,550	227,550	227,550	227,550	227,550	227,550
<i>Panel B: Sample with Siblings</i>						
Treatment	0.012*** (0.003) [0.056]	0.012*** (0.003) [0.053]	-0.007*** (0.002) [-0.031]	-0.007*** (0.002) [-0.030]	-0.002 (0.003) [-0.007]	-0.002 (0.003) [-0.007]
Mean (Y)	0.316	0.316	0.769	0.769	0.389	0.389
N	53,000	53,000	53,000	53,000	53,000	53,000
<i>Controls</i>						
Full Set		X		X		X

**Note:** This table presents difference-in-difference estimates of the impact of the policy on special education (SpEd) removal at expected 9th grade and educational attainment decisions. Within each panel, each column reports estimates of  $\delta_1$  from a separate regression of Equation 2, with the dependent variable shown in column headers. The sample includes 5th grade cohorts enrolled in SpEd between 1999-00 to 2004-05. Panel A reports results for the full sample and Panel B for students with a general-education (GE) sibling. Odd-numbered columns include only 5th-grade district and 5th-grade cohort fixed effects; even-numbered columns add the full set of controls. Individual baseline controls (measured in 5th grade) include sex, race, FRL, ELL, gender-race interactions, primary disability, an unmodified-exam indicator, and classroom inclusion level. District composition controls are cohort shares by race, FRL, ELL, and sex for both the SpEd and full populations. District finance controls include per-pupil tax-base wealth and the share of tax-base wealth that is residential. The effect for the fully exposed student at the average district is shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered at the district level. Standard errors are clustered at the district level. \*p<0.10, \*\*p<0.05, \*\*\*p<0.01.



**Table 3: The Sibling Spillovers of the Policy on Educational Outcomes**

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: High School Completion</i>					
Focal Treatment	0.005*** (0.001) [0.021]	0.006*** (0.001) [0.026]	0.006*** (0.001) [0.026]	0.006*** (0.001) [0.028]	0.004*** (0.001) [0.019]
Mean (Y)	0.743	0.743	0.743	0.743	0.743
<i>Panel B: College Enrollment</i>					
Focal Treatment	0.004*** (0.001) [0.016]	0.005*** (0.001) [0.022]	0.005*** (0.001) [0.023]	0.005*** (0.001) [0.024]	0.004*** (0.001) [0.017]
Mean (Y)	0.496	0.496	0.496	0.496	0.496
<i>Panel C: Summary Index</i>					
Focal Treatment	0.004*** (0.001) [0.019]	0.005*** (0.001) [0.024]	0.005*** (0.001) [0.024]	0.006*** (0.001) [0.026]	0.004*** (0.001) [0.018]
Mean (Y)	0.619	0.619	0.619	0.619	0.619
N	74,820	74,820	74,820	74,820	74,820
Distinct Students	69,789	69,789	69,789	69,789	69,789
<b>Controls:</b>					
Individual		X	X	X	X
District-Cohort			X	X	X
Focal Individual				X	X
Focal District-Cohort					X

**Note:** This table presents difference-in-differences estimates of sibling spillover effects from the policy on high school graduation, college enrollment, as well as a summary index based on the outcomes in Panels A-B. College enrollment is measured within four years of expected high school graduation. Each column reports estimates of  $\delta_1$  from a separate regression of Equation 4, with the dependent variable shown in panel headers. The sample consists of students from 5th grade cohorts 1993-94 to 2006-07 who have at least one sibling enrolled in special education. The sibling's special education status is determined as of their 5th grade year, and these siblings must also be from 5th grade cohorts within the 1999-00 to 2004-05 period. To focus on spillover effects, we restrict the analysis to students who had no prior special education enrollment before reaching 5th grade. Individual controls include gender, race, FRL, ELL, and gender-race interactions(all at 5th grade baseline). District demographic controls include cohort percentages by race, FRL status, ELL status, and gender for both special education and full samples. District financial controls include per-pupil tax base wealth and residential tax base percentage. For students with multiple focal siblings, we create one observation for each focal sibling-general education sibling pair. We weight observations by the inverse of the number of times each student appears in the regression. The effect for students with siblings fully exposed to the policy at the average district (coefficient  $\times$  4.5) is shown in brackets. Standard errors are clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table 4:** Heterogeneous Effects by Socioeconomic and Racial Group

	Free/Reduced-Price Lunch Status			Race/Ethnicity		
	Always	Sometimes	Never	White	Hispanic	Black
<i>Panel A: Focal SE Student — Lose Special Education</i>						
Treatment	0.015*** (0.004) [0.068]	0.009** (0.004) [0.039]	0.009* (0.005) [0.040]	0.009*** (0.004) [0.042]	0.014*** (0.004) [0.061]	0.019** (0.007) [0.086]
Mean (Y)	0.259	0.301	0.419	0.367	0.280	0.288
<i>Panel B: Focal SE Student — Summary Index of Long-Run Outcomes</i>						
Treatment	-0.010*** (0.003) [-0.045]	-0.0014 (0.004) [-0.007]	0.004 (0.003) [0.017]	0.002 (0.003) [0.010]	-0.009*** (0.003) [-0.039]	-0.005 (0.007) [-0.023]
Mean (Y)	0.490	0.542	0.735	0.635	0.525	0.592
N	23,643	11,052	16,772	22,473	24,107	4,277
<i>Panel C: Sibling of SE Student — Summary Index of Long-Run Outcomes</i>						
Focal Treatment	0.003* (0.002) [0.014]	0.001 (0.002) [0.002]	0.005*** (0.002) [0.023]	0.005*** (0.002) [0.021]	0.002 (0.002) [0.010]	0.008* (0.005) [0.036]
Mean (Y)	0.525	0.595	0.805	0.689	0.556	0.638
N	37,885	15,549	21,129	30,007	37,526	6,079

**Note:** Panel A presents difference-in-difference estimates of the impact of the policy on special education (SpEd) removal. Panel B and C report difference-in-difference estimates on a summary index of high school completion and college enrollment. College enrollment is measured within four years of expected high school graduation. Panels A and B focus on focal SpEd students, and within each panel, each column reports estimates of  $\delta_1$  from a separate regression of Equation 2. Panel C focuses on their siblings, and within each panel, each column reports estimates of  $\delta_1$  from a separate regression of Equation 4. See Table 2 for more detail on the sample and the full set of controls for the focal students shown in Panels A and B. See Table 3 for more detail on the sample and the full set of controls for their siblings shown in Panel C. The columns show different subgroups based on the characteristics of focal children. For free-lunch status (FRPL), we rely on pre-5th-grade records; “Always” means the child was classified as FRPL in every year prior to 5th grade, “Sometimes” means they were classified as FRPL in some years, and “Never” means they were not classified as FRPL in any year prior to 5th grade. Race/ethnicity is determined as of the focal child’s 5th grade. In Panels A and B the effect for the fully exposed student at the average district is shown in brackets, and is defined as the coefficient multiplied by 4.5. Similarly, Panel C shows the effect for the student with a sibling who was fully exposed to the policy, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered at the district level. Standard errors are clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table 5:** Treatment Effects on Educational Outcomes by Sibling Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Age		Focal Sibling		Family Size	
	Close	Far	Older	Younger	3+	2
<i>Panel A: High School Graduation</i>						
Focal Treatment	0.008*** (0.002) [0.034]	0.004** (0.002) [0.018]	0.007*** (0.001) [0.033]	0.000 (0.002) [0.002]	0.003 (0.002) [0.013]	0.005*** (0.002) [0.021]
Mean (Y)	0.724	0.756	0.724	0.773	0.733	0.760
<i>Panel B: College Attendance</i>						
Focal Treatment	0.005** (0.002) [0.024]	0.003* (0.002) [0.014]	0.006*** (0.002) [0.025]	-0.003 (0.003) [-0.014]	0.001 (0.002) [0.003]	0.006*** (0.002) [0.027]
Mean (Y)	0.489	0.500	0.487	0.507	0.476	0.525
<i>Panel C: Summary Index</i>						
Focal Treatment	0.007*** (0.002) [0.029]	0.004** (0.001) [0.016]	0.006*** (0.001) [0.029]	-0.001 (0.002) [-0.006]	0.002 (0.002) [0.008]	0.005*** (0.001) [0.024]
Mean (Y)	0.607	0.628	0.606	0.640	0.604	0.642
N	30,992	43,735	46,861	26,032	36,728	35,314
Distinct Students	29,785	41,269	43,813	24,636	34,957	33,271

**Note:** This table presents difference-in-difference estimates of sibling spillover effects from the policy on high school graduation, college enrollment, as well as a summary index based on outcomes in Panels A-B. College enrollment is measured within four years of expected high school graduation. Within each panel, each column reports estimates of  $\delta_1$  from a separate regression of Equation 4. See Table 3 for more detail on the sample and the full set of controls. Each column isolates a different dimension of sibling relationship—age proximity, birth order, or family size. Specifically, Columns (1)–(2) compare focal children with SE siblings who are close in age (within 2 years) versus further apart; Columns (3)–(4) distinguish whether the focal child is older or younger than their sibling; and Columns (5)–(6) compare families with 2 children versus 3 or more children. The effect for students with siblings fully exposed to the policy at the average district (coefficient  $\times$  4.5) is shown in brackets. Standard errors are clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table 6:** Treatment Effects on Educational Outcomes by Sibling Gender Composition

	(1) Same Gender	(2) Both Male	(3) Both Female	(4) Different Gender	(5) Male Focal	(6) Female Focal
<i>Panel A: High School Graduation</i>						
Focal Treatment	0.007*** (0.002) [0.033]	0.007*** (0.002) [0.031]	0.008** (0.003) [0.034]	0.001 (0.002) [0.004]	-0.001 (0.002) [-0.005]	0.005 (0.003) [0.025]
Mean (Y)	0.741	0.736	0.751	0.745	0.754	0.722
<i>Panel B: College Attendance</i>						
Focal Treatment	0.004** (0.002) [0.018]	0.004 (0.002) [0.017]	0.005 (0.004) [0.024]	0.004** (0.002) [0.016]	0.002 (0.002) [0.009]	0.008** (0.004) [0.035]
Mean (Y)	0.483	0.468	0.512	0.507	0.530	0.454
<i>Panel C: Summary Index</i>						
Focal Treatment	0.006*** (0.001) [0.025]	0.005*** (0.002) [0.024]	0.006** (0.003) [0.029]	0.002 (0.001) [0.010]	0.000 (0.002) [0.002]	0.007** (0.003) [0.030]
Mean (Y)	0.612	0.602	0.631	0.626	0.642	0.588
N	36,970	23,905	12,866	37,751	26,482	11,072
Distinct Students	35,210	22,704	12,308	36,040	25,234	10,611

**Note:** This table presents difference-in-difference estimates of sibling spillover effects from the policy on high school graduation, college enrollment, as well as a summary index based on outcomes in Panels A-B. College enrollment is measured within four years of expected high school graduation. Within each panel, each column reports estimates of  $\delta_1$  from a separate regression of Equation 4. See Table 3 for more detail on the sample and the full set of controls. Columns (1)–(6) examine heterogeneity by gender composition: Column (1) includes all same-gender sibling pairs; Columns (2)–(3) split same-gender pairs by whether both siblings are male or female; Column (4) includes all opposite-gender pairs; Columns (5)–(6) split opposite-gender pairs by whether the focal (SpEd) sibling is male or female. The effect for students with siblings fully exposed to the policy at the average district (coefficient  $\times 4.5$ ) is shown in brackets. Standard errors are clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## A Appendix Tables and Figures

**Figure A.1:** Performance Level Assignment for the Special Education Representation Rate

### PERFORMANCE LEVEL ASSIGNMENT

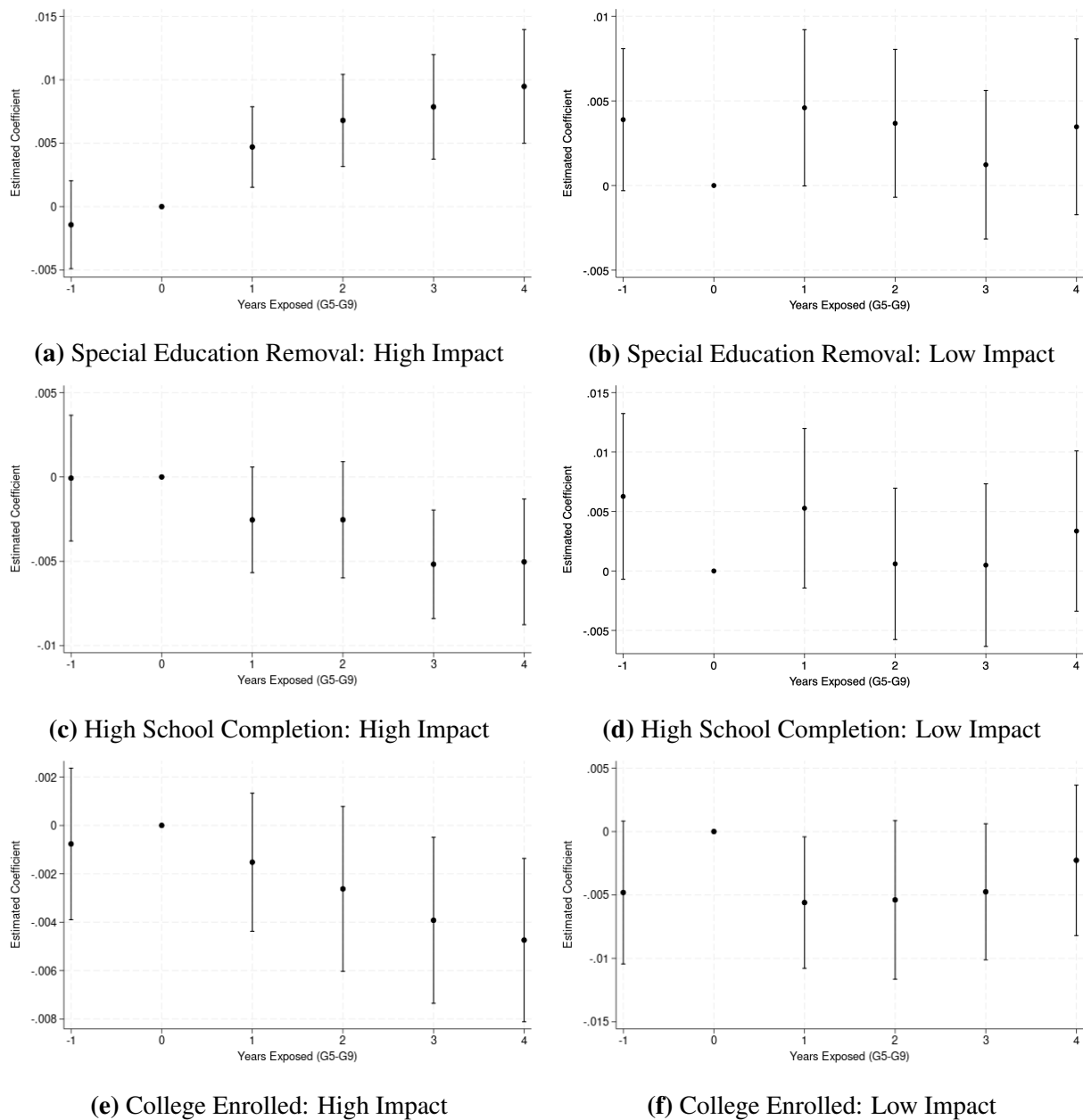
The district-level special education representation rate is compared to the PBMAS standards for the indicator, and performance levels are assigned as follows:

SPED #12: District Special Education Representation Rate				
Performance Level (PL) Assignments				
Performance Level = Not Assigned	Performance Level = 0 (met standard) (Also includes ORI)	Performance Level = 1	Performance Level = 2	Performance Level = 3
PL not equal to 0 and district does not meet minimum size requirements.	The district representation of students receiving special education services is 8.5% or lower. Minimum size requirements not applicable if PL = 0.	The district representation of students receiving special education services is between 8.6% and 12.0%.	The district representation of students receiving special education services is between 12.1% and 16.0%.	The district representation of students receiving special education services is 16.1% or higher.

**Note:** This figure shows the performance ratings districts received based on their special education rate. Performance level 0 is the “best” level, which indicates a district is in compliance with the target.

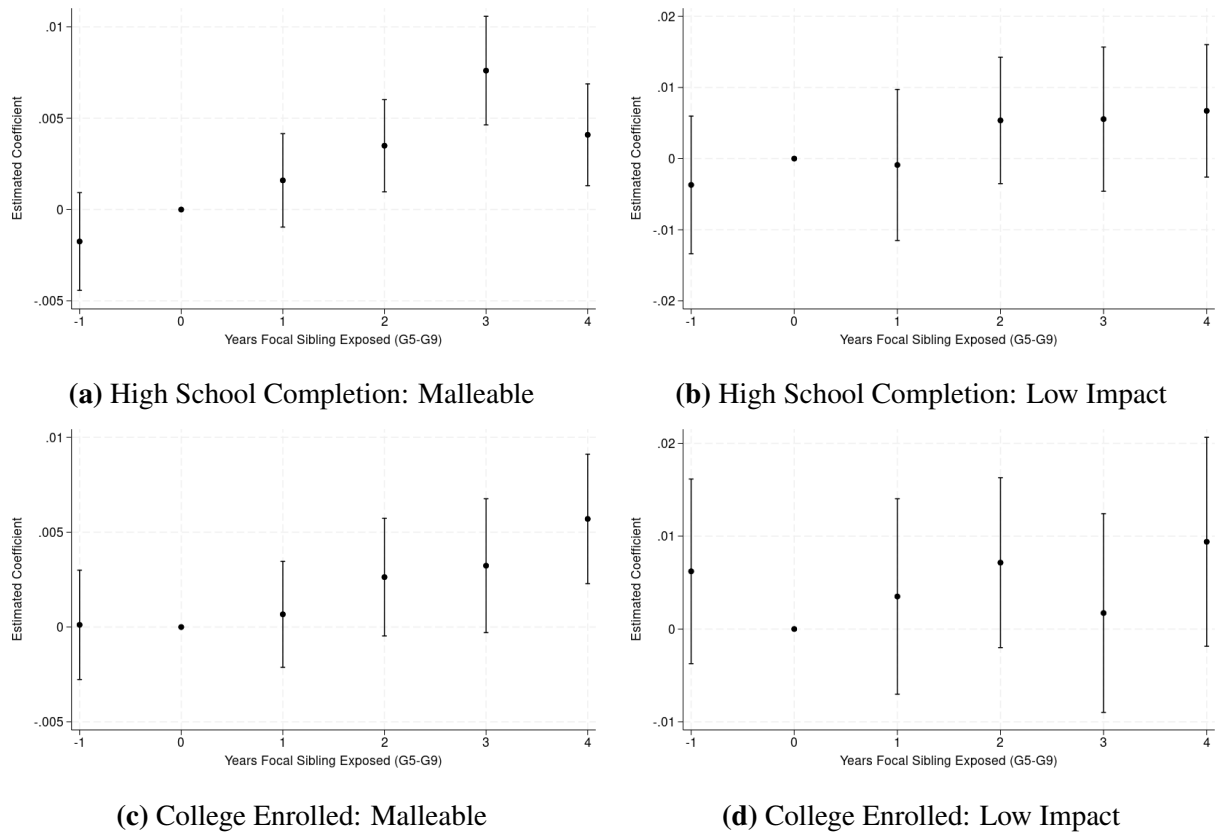
*Source:* Adapted and reprinted courtesy of the Texas Education Agency. ©Texas Education Agency, 2004-2021. All rights reserved. This figure is from the 2004 Performance Based Monitoring Analysis (PBMAS) Manual, available on the Texas Education Agency’s website at the following link: <https://tea.texas.gov/student-assessment/monitoring-and-interventions/rda/rda-and-pbmas-manuals>.

**Figure A.2:** Event Study Estimates of the Impact of the Policy on Special Education Removal and Educational Attainment for Special Education Students with General Education Siblings: By Disability Type



**Note:** Figures plot coefficients and 95% confidence intervals from event-study regressions of interactions between 5th-grade cohort indicators and the 2004–05 district SpEd rate. Figures (a)–(b) report Special Education (SpEd) removal by expected 9th grade; Figures (c)–(d) report high school completion; Figures (e)–(f) report college enrollment within four years of expected high school graduation. Event time is computed by subtracting 9 from the grade each 5th-grade cohort was expected to be in during the first policy year (2005–06). Left-hand panels restrict to “high impact” students, defined as those with learning disabilities, speech impairments, other health impairments, or emotional disturbance who received more than 50 percent of instruction in GE classrooms at baseline (5th grade). “Low impact” students are those with other disability types or less than 50 percent GE instruction at baseline. See [Figure 3](#) for details on the sample and controls. Standard errors are clustered at the district level.

**Figure A.3:** Event Study Estimates of Sibling Spillovers from the Policy on Educational Attainment: Heterogeneity by Disability Type of Focal Sibling



**Note:** These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between 5th grade cohort dummies of the focal sibling and the 2004-05 district SpEd rate. Figures (a)-(b) shows high school completion, and Figures (c)-(d) shows college enrollment, measured within four years of expected high school graduation. Event time is computed by subtracting 9 from the grade each 5th grade focal cohort was expected to be enrolled in during the first year of the policy (or the 2005-06 school year). The figures on the left hand side include families who had a focal sibling with a high impact conditions and figures on the right hand side include families who had a focal sibling with a low impact condition. See Appendix [Figure A.2](#) for more detail on the definition of high impact and low impact conditions. Families with children that have children with high impact and low impact conditions are dropped. See [Figure 4](#) for more detail on the sample and full set of controls. Standard errors are clustered at the district level.

**Table A.1:** Cross-Cohort Variation in Policy Exposure (5th Grade SE Cohorts)

Grade 5 Cohort	Policy Exposure by Year-Grade				Policy Exposure Before Expected 9th Grade ( $\text{FracExposed}_c$ )
	6	7	8	9	
1999 - 2000	2000-01	2001-02	2002-03	2003-04	0
2000 - 2001	2001-02	2002-03	2003-04	2004-05	0
2001 - 2002	2002-03	2003-04	2004-05	2005-06	1/4
2002 - 2003	2003-04	2004-05	2005-06	2006-07	1/2
2003 - 2004	2004-05	2005-06	2006-07	2007-08	3/4
2004 - 2005	2005-06	2006-07	2007-08	2008-09	1

**Note:** This table shows the cross-cohort variation in policy exposure by 5th grade cohort. The first year that districts faced pressure to reduce SE enrollment was during the 2005-06 school year, which we define as the first post-policy year. While all 5th grade SE cohorts were designated as SE before the policy was implemented, they differed in the amount of years that they were exposed to the policy after 5th grade. For each 5th grade cohort, this table highlights each year-grade of expected policy exposure and shows the share of time policy exposed between 5th grade and expected 9th (i.e.  $\text{FracExposed}_c$  in Equation 4).



**Table A.2:** The Impact of the Policy on Predicted Long-Run Outcomes and Exogenous Student Characteristics

	Hispanic	White	Black	Male	FRL	Predicted	
						High School Completion	College Enrollment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Focal Sample</i>							
Treatment	0.001 (0.002) [0.003]	0.000 (0.002) [0.000]	-0.001 (0.001) [-0.006]	0.003 (0.002) [0.013]	-0.000 (0.002) [-0.004]	-0.001 (0.000) [-0.002]	0.000 (0.001) [0.000]
Mean (Y)	0.469	0.433	0.086	0.680	0.579	0.769	0.389
N	53,000	53,000	53,000	53,000	53,000	53,000	53,000
<i>Panel B: Sibling Sample</i>							
Focal Treatment	0.006*** (0.001) [0.026]	-0.005*** (0.001) [-0.020]	-0.001 (0.001) [-0.005]	0.001 (0.001) [0.003]	0.002 (0.001) [0.007]	-0.001** (0.000) [-0.005]	-0.001** (0.000) [-0.007]
Mean (Y)	0.503	0.402	0.083	0.471	0.632	0.743	0.496
N	74,820	74,820	74,820	74,820	74,820	74,820	74,820
Distinct Students	69,789	69,789	69,789	69,789	69,789	69,789	69,789
<i>Controls</i>							
FE's	X	X	X	X	X	X	X
Cohort Demo	X	X	X	X	X	X	X
Add'l Controls						X	X

**Note:** This table shows difference-in-differences estimates of the direct impact and sibling spillovers of the policy on predicted outcomes and student demographics. In Panel A, each column reports estimates of  $\delta_1$  from a separate regression of Equation 4. The dependent variable is shown in the column headings. In Panel B, each column reports estimates of  $\delta_1$  from a separate regression of Equation 4. To obtain predicted values we generate fitted values from a regression of outcomes on the full set of controls (excluding treatment). Panel A includes estimates for the focal sample and Panel B includes estimates from the sibling sample. See Table 2 for more detail on the sample and the full set of controls for the focal students. See Table 3 for more detail on the sample and the full set of controls for their siblings. The effect for the fully exposed focal student at the average district is shown in brackets, and is defined as the coefficient multiplied by 4.5. For the siblings, the effect size for students with a fully exposed sibling at the average district (coefficient  $\times$  4.5) is shown in brackets. Standard errors in parentheses are clustered at the district level. \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.3:** Heterogeneous Effects by Disability Type of Focal Sibling

	Focal Sibling		GE Sibling	
	High Impact	Low Impact	High Impact	Low Impact
	(1)	(2)	(3)	(4)
<i>Panel A: Special Education Removal</i>				
Treatment	0.012*** (0.003) [0.055]	0.004 (0.004) [0.018]		
Mean (Y)	0.358	0.071		
<i>Panel B: High School Completion</i>				
Treatment	-0.007*** (0.002) [-0.033]	-0.002 (0.005) [-0.009]	0.003** (0.001) [0.015]	-0.003 (0.005) [-0.012]
Mean (Y)	0.761	0.829	0.745	0.752
<i>Panel C: College Enrollment</i>				
Treatment	-0.003 (0.003) [-0.013]	0.003 (0.006) [0.012]	0.004*** (0.002) [0.019]	-0.005 (0.006) [-0.022]
Mean (Y)	0.415	0.239	0.499	0.511
N	44,976	6,703	59,541	6569

**Note:** Panel A presents difference-in-difference estimates of the impact of the policy on special education (SpEd) removal. Panel B and C report difference-in-difference estimates on high school completion and college enrollment, respectively. College enrollment is measured within four years of expected high school graduation. Columns 1-2 focus on focal SpEd students, and within each panel, each column reports estimates of  $\delta_1$  from a separate regression of Equation 2. Columns 3 and 4 focus on their siblings, and within each panel, each column reports estimates of  $\delta_1$  from a separate regression of Equation 4. See Table 2 for more detail on the sample and the full set of controls for the focal students shown in Columns 1-2. See Table 3 for more detail on the sample and the full set of controls for their siblings shown in Columns 3-4. The table further drops families with both malleable and non-malleable focal children. Odd-numbered columns include the subset of SpEd children or families with a child with a high impact disability is defined as students with a malleable disability (i.e. learning disabilities, speech impairments, other health impairments, or emotional disturbance) who received more than 50 percent of their instruction in GE classrooms at baseline (i.e 5th grade). The low-impact group includes all other SpEd students. Even-numbered columns include the subset of SpEd children or families with a child with a non-malleable disability defined as .. The effect for the fully exposed student at the average district is shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered at the district level. Standard errors are clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.4:** The Impact of the Policy on Enrollment and District Switching

	Focal Sample		Sibling Sample	
	Enrollment (By G9)	District Switch	Enrollment (By G9)	District Switch
	(1)	(2)	(3)	(4)
Treatment	0.000 (0.001) [0.002]	0.000 (0.000) [0.000]	-0.010*** (0.001) [-0.044]	0.020*** (0.002) [0.0888]
Mean (Y)	0.942	0.185	0.915	0.165
N	56,294	53,034	52,960	49,266
Distinct Students	56,294	53,034	49,356	46,565
<i>Controls</i>				
FE's	X	X	X	X
Cohort Demo	X	X	X	X

**Note:** This table shows difference-in-differences estimates of the direct impact and sibling spillovers of the policy on enrollment by expected 9th grade and district switching between 5th and expected 9th grade. Columns (1)-(2) report estimates of  $\delta_1$  from separate regressions of Equation 4 for the focal sample. Columns (3)-(4) report estimates of  $\delta_1$  from separate regressions of Equation 4 for the sibling sample. The dependent variable is shown in the column headings. See Table 2 for more detail on the sample and the full set of controls for the focal students. See Table 3 for more detail on the sample and the full set of controls for their siblings. The effect for the fully exposed focal student at the average district is shown in brackets, and is defined as the coefficient multiplied by 4.5. For the siblings, the effect size for students with a fully exposed sibling at the average district (coefficient  $\times$  4.5) is shown in brackets. Standard errors in parentheses are clustered at the district level. \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table A.5:** Effects on Disciplinary and Academic Outcomes

	Reading Score	Math Score	Combined Score	Discipline Flag	Fraction Absent	Grade Repeat
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: All siblings</i>						
Focal Treatment	0.003 (0.003) [0.015]	0.000 (0.003) [0.002]	0.002 (0.003) [0.009]	0.002** (0.001) [0.009]	-0.001*** (0.000) [-0.002]	-0.000 (0.000) [-0.002]
Mean (Y)	-0.049	-0.065	-0.062	0.154	0.041	0.039
N	341,899	341,769	344,760	497,602	446,338	497,602
<i>Panel B: Siblings of those with Malleable Disabilities</i>						
Focal Treatment	0.005* (0.003) [0.022]	0.002 (0.003) [0.010]	0.004 (0.003) [0.016]	0.002 (0.001) [0.007]	-0.001*** (0.000) [-0.003]	-0.001 (0.000) [-0.002]
Mean (Y)	-0.044	-0.058	-0.056	0.152	0.041	0.039
N	269,238	269,135	271,438	394,818	353,704	394,818
<i>Panel C: Siblings of those with Non-Malleable Disabilities</i>						
Focal Treatment	0.002 (0.012) [0.007]	-0.005 (0.014) [-0.024]	-0.002 (0.012) [-0.007]	0.001 (0.003) [0.006]	-0.000 (0.001) [-0.001]	0.001 (0.001) [0.006]
Mean (Y)	0.014	-0.018	-0.006	0.147	0.041	0.040
N	25,698	25,719	25,944	41,034	36,319	41,034

**Note:** This table presents difference-in-difference estimates of the sibling spillover effects from the policy on impact on math and reading standardized test scores, disciplinary and academic outcomes. Panel A shows the full sample, Panel B shows the subset of families with a focal child who had a malleable disability, and Panel C shows the subset of families with a focal child who had a non-malleable disability. Panels B and C exclude families with both malleable and non-malleable focal children. Each column reports estimates from a slightly modified version of Equation 4, where we interact the focal siblings policy exposure ( $SERate_d^{Pre} \times \text{FracExposed}_k$ ) with a post-policy indicator, equal to 1 for years after 2005. Instead of using 5th grade cohort indicators ( $\phi_c$ ), we include year-by-grade ( $\phi_{gt}$ ). Outcomes are observed between 3rd and 8th grade. Students appear multiple times in the regression, with each observation weighted by the inverse of the number of times they appear in the sample. See Table 3 for more detail on the sample and the full set of controls. The effect size for students with a fully exposed sibling at the average district (coefficient  $\times 4.5$ ) is shown in brackets. Standard errors are clustered at the district level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A.6:** Robustness of Estimates Across Alternative Samples and Specifications

	Sibling Sample Restrictions			Specification Changes	
	General Education 1994-2007	General Education 2000 - 2005	All Siblings 1994 - 2007	Birth Cohort Control Included	Achievement Control Included
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: High School Graduation</i>					
Focal Treatment	0.004*** (0.001) [0.019]	0.006*** (0.002) [0.028]	0.003*** (0.001) [0.014]	0.003** (0.001) [0.014]	0.004*** (0.001) [0.019]
Mean (Y)	0.742	0.734	0.732	0.743	0.756
<i>Panel B: College Attendance</i>					
Focal Treatment	0.004*** (0.001) [0.017]	0.007*** (0.002) [0.033]	0.004*** (0.001) [0.019]	0.002 (0.001) [0.007]	0.003* (0.002) [0.013]
Mean (Y)	0.495	0.509	0.418	0.496	0.527
<i>Panel C: Summary Index</i>					
Focal Treatment	0.004*** (0.001) [0.018]	0.007*** (0.002) [0.030]	0.004*** (0.001) [0.016]	0.002** (0.001) [0.010]	0.004*** (0.001) [0.016]
Mean (Y)	0.619	0.621	0.575	0.619	0.642
N	75625	35955	124946	74820	59710

**Note:** This table presents difference-in-difference estimates of sibling spillover effects from the policy on high school graduation, college enrollment, as well as a summary index based on the outcomes in Panels A-B. College enrollment is measured within four years of expected high school graduation. Within each panel, each column reports estimates of  $\delta_1$  from a separate regression of Equation 4. See Table 3 for more detail on the sample and the full set of controls. Column (1) shows the baseline model, and the changes to the sample or specification are listed in column headers. The effect for students with siblings fully exposed to the policy at the average district is shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered at the district level. Standard errors are clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table A.7: Mechanisms: Parental School Choice**

	2 Years after Focal students' 5th grade Moved to a District with higher ..					
	Standardized Scores			Value-Added		
	Combined)	Math	Reading	Combined	Math	Reading
<b>Panel A: Full Sample</b>						
Focal Treatment	0.008*** (0.001) [0.034]	0.007*** (0.001) [0.031]	0.007*** (0.001) [0.032]	0.002*** (0.001) [0.011]	0.008*** (0.001) [0.037]	0.002*** (0.001) [0.010]
Mean (Y)	0.091	0.090	0.092	0.017	0.084	0.013
N	63,748	63,748	63,748	63,748	63,748	63,748
<b>Panel B: Malleable Siblings</b>						
Focal Treatment	0.007*** (0.001) [0.033]	0.007*** (0.001) [0.030]	0.007*** (0.001) [0.030]	0.003*** (0.001) [0.012]	0.008*** (0.001) [0.034]	0.002*** (0.001) [0.009]
Mean (Y)	0.090	0.089	0.091	0.017	0.083	0.013
N	50,707	50,707	50,707	50,707	50,707	50,707
<b>Panel C: Non-Malleable Siblings</b>						
Focal Treatment	0.001 (0.004) [0.005]	0.002 (0.004) [0.010]	0.001 (0.004) [0.006]	-0.001 (0.001) [-0.003]	0.005 (0.004) [0.024]	-0.001 (0.001) [-0.007]
Mean (Y)	0.082	0.081	0.081	0.015	0.076	0.014
N	5,533	5,533	5,533	5,533	5,533	5,533

**Note:** This table presents difference-in-differences estimates of sibling spillover effects from the policy on the likelihood of being enrolled in districts with higher average test scores or value added. [add a description of how average test scores and value added was generated. ]. The effect effect for students with siblings fully exposed to the policy at the average district (coefficient  $\times 4.5$ ) us shown in brackets. See Table 3 for more detail on the sample and the full set of controls. An additional sample restriction for this table is that they have to be enrolled 2 years after their focal sibling was in 5th grade. Standard errors are clustered at the district level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A.8: Direct Impact of the Policy on Siblings of SpEd students**

	Outcome Variables				
	Special Education	High School Graduation	College Attendance	Summary Index LR	Achievement Index
Treatment	-0.001 (0.001) [-0.005]	-0.002 (0.002) [-0.011]	-0.005* (0.003) [-0.025]	-0.004* (0.002) [-0.018]	0.004 (0.002) [0.018]
Mean (Y)	0.048	0.743	0.496	0.619	-0.184
N	74,672	74,672	74,672	74,672	453,771

**Note:** This table presents difference-in-difference estimates of the impact of the policy on special education (SpEd) removal, expected 9th grade, educational attainment decisions, and achievement. Within each panel, each column reports estimates of  $\delta_1$  from a separate regression of Equation 2, with the dependent variable shown in column headers. For the achievement outcomes, a slightly modified version of Equation 2 is run as outlined in the notes of Appendix Table A.5. See Table 3 for more detail on the sample and the full set of controls. The effect for the fully exposed student at the average district is shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered at the district level. \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## B Extended Human Capital Model

In this section, we formalize the human capital framework described briefly in Section 2.3. Specifically, we develop a model of sibling spillovers following the human capital accumulation framework of Becker (1981) & Yi, Heckman, and Zhang (2015). This model helps us understand how changes in SpEd enrollment affect not only the directly impacted students but also their siblings.

Consider a family with two children: child  $j$  who is enrolled in SpEd and child  $k$  who is not. The human capital production function for each child  $i \in j, k$  is:

$$\phi_i = \phi(I_i, \mu_i, \mu_{-i}) \quad (5)$$

where  $I_i$  denotes parental investments in child  $i$ ,  $\mu_i$  represents shocks directly affecting child  $i$ , and  $\mu_{-i}$  captures potential spillovers from shocks to the sibling.

Parents maximize utility from their own consumption and their children's human capital:

$$U = U(C, \phi_j, \phi_k) \quad (6)$$

Subject to the following budget constraint:

$$Y = C + I_j + I_k \quad (7)$$

where  $Y$  is family income,  $C$  is parental consumption and  $I$  is the investments allocated towards child  $j$  and  $k$  respectively.

To understand how a shock to the SpEd child affects their sibling, we examine the impact of the SpEd enrollment cap on the GE sibling's human capital. Taking the total derivative of sibling  $k$ 's human capital with respect to a shock  $\mu_j$  experienced by the SpEd sibling:

$$\frac{d\phi_k}{d\mu_j} = \frac{d\phi_k}{d\mu_j} + \frac{d\phi_k}{di} \frac{dI_k}{d\mu_j} \quad (8)$$

This decomposition reveals two distinct channels through which shocks to one sibling affect the other. The first term,  $\frac{d\phi_k}{d\mu_j}$ , captures the direct spillover effect-how the shock to sibling  $j$  directly influences sibling  $k$ 's human capital through mechanisms such as peer effects, role modeling, or psychological impacts. The second term,  $\frac{d\phi_k}{dI_k} \frac{dI_k}{d\mu_j}$ , represents the indirect investment effect, measuring how the shock affects sibling  $k$  through changes in parental investment allocation between children.

To understand the implications of this model, we need to consider how parents and GE siblings interpreted this shock. Families might have interpreted the reduction in SpEd enrollment in one of two ways. Under what we call the “Positive Signal” interpretation, families may view removal from SpEd as evidence that their child has made significant progress and no longer needs specialized services. This interpretation treats the removal as elimination of a stigmatizing label and recognition of improved academic ability. Crucially, this perception may persist even when students actually perform worse after losing services. Alternatively, under the “Service Loss” interpretation, families may recognize that their child’s underlying needs remain unchanged, but they have lost access to vital educational support due to administrative constraints. Under this view, the SpEd child faces greater academic challenges going forward.

We argue that the “Positive Signal” interpretation is more likely, primarily because the cap was implemented illegally and neither parents nor teachers knew about this arbitrary limit. When teachers recommended removing a student from SpEd services, families likely interpreted this as genuine progress in the student’s abilities rather than an administrative decision driven by enrollment caps.

## **B.1 Implications from changes in SpEd**

The direct sibling spillover could be either positive or negative. Positive spillovers would occur if the perceived ability of the SpEd sibling leads them to become better role models or mentors, which can improve the academic outcomes of the non-identified sibling. [Gottfried and McGene \(2013\)](#); [Nicoletti and Rabe \(2019\)](#). On the other hand, sibling spillovers may be negative if the improved perception of the SpEd siblings ability increases rivalry competition or stress [de Gendre \(2022\)](#). In



either case, these direct spillovers are likely to be strongest in scenarios in which sibling comparison is salient such as those who are close in age and of the same sex. These are sibling pairs who are more directly comparable in both academic and social domains as well as likely sharing similar peer environments.

On the other hand, indirect effects could operate through parental reallocation of time and resources. The literature identifies two possible parental responses: *reinforcing* investments (concentrating resources on higher-achieving children) and *compensatory* investments (directing resources toward needier children). If SpEd removal dramatically improves parents' perceptions of their child's ability—making them now view this child as higher-achieving than siblings—parents might pursue a *reinforcing* strategy. This would involve shifting additional resources toward the former SpEd child to capitalize on their perceived potential, as documented in studies of parental responses to health and cognitive differences [Datar, Kilburn, and Loughran \(2010\)](#); [Frijters, Johnston, Shah, and Shields \(2013\)](#). Alternatively, if SpEd removal simply equalizes how parents view their children's abilities, they might adopt a *compensatory* strategy. Having potentially under-invested in GE siblings while one child required special services, parents might now redirect resources toward these other children to promote equity across siblings, consistent with research on parental responses to health shocks [Fan and Porter \(2020\)](#); [Yi et al. \(2015\)](#). Finding positive sibling spillovers would suggest *compensatory* rather than *reinforcing* parental investments dominate.

The extent of parental reallocation likely depends on several factors, including parental income, education and cultural attitudes. While lower-income families face greater financial constraints, higher-income families invest significantly more time in their children [Guryan et al. \(2008\)](#); [Kalil et al. \(2012\)](#). Thus, it is a-priori unclear whether reallocation will be stronger for higher or lower-income families and in what direction. A growing literature begins to shed light on this question. For example, twin-based estimates for Chile find *no* systematic gap in behavior between low- and high-educated mothers [Abufhele, Behrman, and Bravo \(2017\)](#). However, stratified twin evidence for the United States and United Kingdom further shows that high-SES parents actively

reinforce early cognitive gaps, while disadvantaged parents largely remain indifferent [Grätz and Torche \(2016\)](#). Similarly, [Karbownik and Özek \(2023\)](#) find that by exploiting school entry cutoffs in Florida, sibling spillovers in educational achievement are positive among lower-income families but can be negative in more affluent households. These mixed results suggest that parental responses are likely to vary by context. In our setting, it is difficult to determine whether the heterogeneous effects by income reflect differences in the marginal SpEd students removal (for example, lower income children may have more severe conditions at baseline).

A final indirect channel operates through parental labor supply. Having children with severe disabilities could reduce mothers' labor force participation, as caring for these children requires substantial time investment. For instance, Norwegian register data show that severe child disability depresses maternal earnings and employment [Wondemu, Joranger, Åsmund Hermansen, and Brekke \(2022\)](#). However, the labor supply response in our context may be more muted since we focus on *marginal* SpEd students with less severe conditions. These milder disabilities may not constrain parental work to the same degree as severe disabilities. Without household income data, we cannot directly test this mechanism. Still, if SpEd removal reduces perceived caregiving demands and allows parents to increase their work hours, this could contribute to the positive sibling outcomes we observe.

## C Identifying Siblings

We identify siblings if they have the same mother (using maiden last name) and father's name as listed in the TBI. To account for potential misspellings, we incorporate nickname and phonetic standardizations and allow matches with or without middle names. Appendix [Table C.1](#) provides a breakdown of the matching iterations and the proportion of students matched at each stage. This approach should yield relatively few false positives, since two names together are fairly unique.<sup>34</sup>

To further enhance accuracy, we use the TEA enrollment data and exclude sibling pairs who attend

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<sup>34</sup>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.

schools in non-adjacent counties.<sup>35</sup> Nonetheless, our approach may misclassify some children with siblings as only children. These errors arise when our name matching algorithm fails to reconcile inconsistent spellings of parents across siblings birth records. This issue is further compounded by missing father's information, which is the case for approximately 18 % of birth records during our study period.<sup>36</sup> This helps to explain why we observe an overrepresentation of single-child families in our data compared to the American Community Survey (ACS) (see [Table C.2](#)).

It is important to note that failing to identify some students with siblings does not threaten the internal validity of our estimates, but rather affects the composition of the population under study and external validity. Because our sibling matches require both parents to be present in the birth data, our analysis is necessarily restricted to a somewhat more advantaged subset of families. Relative to single-parent households, two-parent households may have greater financial resources to buffer children from the adverse consequences of losing access to SpEd. However, as previously noted more advantaged families face greater time constraints. As such, it is unclear whether our estimates provide a lower or upper bound of the impacts. Nonetheless, given the close similarity between our sibling sample and the overall population as shown in [Table 1](#), we view any concerns about external validity as likely to be quite minimal.

To assess the robustness of our findings to alternative definitions of sibling groups, we re-estimate our baseline model under a sequence of increasingly restrictive specifications, summarized in [Table C.3](#). Column 0 represents our baseline estimates, whose sample restriction are the same as those in [Table 3](#). Our loosest definition (Column 1) defines siblings using TBI parental matches alone. Column 2 introduces a geographic restriction, keeping only siblings attending school in the same or contiguous counties. Column 3 further restricts the sample to sibling pairs linked using the strictest three versions of our name-matching algorithm, all of which require full middle names: (i) exact matches with no modifications, (ii) spelling and spacing-standardized matches,

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<sup>35</sup>In this restriction, students are included if they attend schools that are in the same or adjacent counties at least 75% of the time.

<sup>36</sup>[Table C.1](#) illustrates that the majority of siblings' matches rely on exact name matches, with a subset matched by nickname or phonetic correction.

and (iii) nickname-corrected matches (see Appendix [Table C.1](#)). Column 4 returns to the TBI definition but caps families at nine or fewer children to limit spurious matches in unusually large households. Column 5 combines the geographic restriction with the family-size cap. Finally, Column 6 implements the strictest definition, requiring TBI linkage, contiguous-county enrollment, capped family size, and matches restricted to the first three algorithmic iterations. Across all alternative definitions, the results remain robust: the estimated effects are consistently positive and statistically significant and the magnitudes of the effects are similar.

**Table C.1:** Distribution of Sibling Linkage Versions Using Name Matching Algorithms

Sibling Linkage Version (1-12)	Freq.	Percent	Cum.
Perfect name with full middle name	5,239,330	91.40	91.40
Cleaned name with full middle name	1,197	0.02	91.42
Nickname-corrected name with full middle name	43,759	0.76	92.18
Soundex name with full middle name	103,652	1.81	93.99
Perfect name with middle initial	114,760	2.00	95.99
Cleaned name with middle initial	45	0.00	95.99
Nickname-corrected name with middle initial	3,626	0.06	96.06
Soundex name with middle initial	13,240	0.23	96.29
Perfect name without middle name	163,967	2.86	99.15
Cleaned name without middle name	26	0.00	99.15
Nickname-corrected name without middle name	5,868	0.10	99.25
Soundex name without middle name	42,862	0.75	100.00
<b>Total</b>	<b>5,732,332</b>	<b>100.00</b>	

**Note:** This table summarizes the number and share of sibling linkages made across 12 iterative versions of name-matching algorithms. "Perfect" refers to exact string matches of full names. "Cleaned" versions apply case-insensitive and character-stripped normalization (e.g., removing punctuation or accents). "Nickname-corrected" uses mappings from ethnically appropriate nickname dictionaries that considers sex of individual (e.g., "Sam" → "Samuel" or "Sam" → "Samantha"), as in Abramitzky et al. (2020). "Soundex" applies a phonetic encoding algorithm designed to capture similarly pronounced names despite spelling differences. Each name version is attempted using full middle names, middle initials, and no middle names in descending order of matching precision.

**Table C.2:** Family Size comparing the ACS to TBI

<b>Siblings</b>	<b>ACS Percentage</b>	<b>TBI Percentage</b>
1	35%	62%
2	36%	27%
3	19%	8%
4	7%	2%
5+	2%	1%

**Notes:** This table compares the distribution of sibling group sizes from two sources: the American Community Survey (ACS) and the Texas Birth Index (TBI). Percentages represent the share of children living in families with the indicated number of siblings. The ACS percentages are calculated from children born in Texas between 1976 and 1997, while the TBI figures reflect counts of children born to the same mother and father in the birth records that matched to the TEA data between 1976 and 1997.

**Table C.3: Treatment Effects by Sibling Definitions**

	Baseline	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A: High School Graduation</b>							
Coefficient	0.004***	0.006***	0.006***	0.007***	0.006***	0.006***	0.007***
(SE)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Mean	0.743	0.734	0.734	0.742	0.734	0.734	0.742
PP Change	0.019	0.028	0.027	0.031	0.027	0.027	0.031
N	74,820	35,955	35,593	30,674	35,871	35,588	30,672
<b>Panel B: College Attendance</b>							
Coefficient	0.004***	0.007***	0.007***	0.007***	0.007***	0.007***	0.007***
(SE)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Mean	0.496	0.509	0.509	0.519	0.514	0.509	0.509
PP Change	0.016	0.033	0.031	0.031	0.033	0.032	0.031
N	74,820	35,955	35,593	30,674	35,871	35,588	30,672
<b>Panel C: Summary Index</b>							
Coefficient	0.004***	0.007***	0.006***	0.007***	0.007***	0.007***	0.007***
(SE)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Mean	0.619	0.621	0.621	0.630	0.621	0.621	0.630
PP Change	0.019	0.030	0.029	0.031	0.030	0.029	0.031
N	74,820	35,955	35,593	30,674	35,871	35,588	30,672
<b>Sibling Definition</b>							
Baseline (Main Results)	X						
TBI Families		X	X	X	X	X	X
Contiguous Counties			X	X		X	X
Capped Family Size					X	X	X
Sibling Match Versions (1–3)				X			X

**Note:** This table re-estimates the same model as specified in [Equation 4](#) under alternative sibling definitions. Column 0 is the baseline column that reproduces the estimates from [Table 3](#). Columns (1)–(6) re-estimate the same model under alternative sibling definitions designed to assess sensitivity to potential linkage error. Column (1) defines siblings using Texas Birth Index (TBI) parental-name matches only. Column (2) keeps pairs of siblings whose school counties are the same or adjacent in at least 75% of years both siblings are observed enrolled. Column (3) further restricts sibling linkages to the strictest name-matching iterations requiring full middle names (perfect match, cleaned/standardized match, or nickname-corrected match). See [Table C.1](#) for more information. Column (4) returns to the TBI-only definition but caps family size at  $\leq 9$  children to limit spurious matches in unusually large households. Column (5) combines the geographic screen with the family-size cap. Column (6) applies the strictest definition: TBI linkage + contiguous-county enrollment + family-size cap + strict name-match iterations (1–3).