The Long-Run Impacts of Special Education

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Abstract

Over 13 percent of US students participate in Special Education (SE) programs annually, at a cost of $40 billion. However, the effect of SE placements remains unclear. This paper uses administrative data from Texas to examine the long-run effect of reducing SE access. Our research design exploits variation in SE placement driven by a state policy that required school districts to reduce SE caseloads to 8.5 percent. We show that this policy led to sharp reductions in SE enrollment. These reductions in SE access generated significant reductions in educational attainment, suggesting that marginal participants experience long-run benefits from SE services.

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1 Introduction

Special Education (SE) program participation grew by over 40 percent between 1975 and 2018. Currently, over 13 percent of public school students participate in SE programs annually, at a cost of $40 billion (National Center for Education Statistics, 2015; Elder, Figlio, Imberman, & Persico, 2019). While the purpose of SE is to ameliorate the challenges students with disabilities may face throughout schooling and later in life, considerable uncertainty surrounds the effectiveness of SE spending. On the one hand, students are likely to benefit from the *individualized* educational support (such as one-on-one tutoring, a classroom aide, therapy, or standardized testing modifications) that SE offers. But for students with less severe conditions there are several reasons why SE participation could be harmful: being placed in segregated learning environments or held to relatively lower expectations regarding achievement may inhibit long-run success.

Despite significant increases in SE participation for students with less severe conditions, there is little consensus on how placement (or lack of) affects the long-run trajectories of marginal participants. The main difficulty in evaluating the effectiveness of SE programs is identifying a plausible counterfactual. Students are selected to participate in SE because teachers believe they are at risk of low achievement or poor behavioral outcomes. However, because SE inclusion criteria are neither straightforward nor standardized for students with less severe conditions, it is not possible to exploit discontinuities in SE diagnostic criteria to identify the causal impacts of SE participation.[1] Instead, exogenous changes in SE participation are required for causal identification. But because SE eligibility rules were determined federally in 1975 (with very minor changes since) it is difficult to identify variation in SE placement across locations or over time that is plausibly exogenous.

This paper provides evidence on the long-run effects of SE by exploiting a rare policy change that introduced exogenous variation in SE participation for marginal participants. In 2005, the Texas Department of Education implemented a district-level SE enrollment target of

[1] For the vast majority of SE students with learning or behavioral impairments, the most common symptoms are poor academic performance or classroom behaviors. Since many students exhibit these symptoms occasionally, there are inconsistencies in SE placement based on how teachers, parents, or diagnosticians perceive these symptoms.

8.5 percent. This policy led to an immediate drop in SE enrollment. Over the next ten years, statewide SE enrollment declined by 4.5 percentage points, from 13 percent to 8.5 percent. By 2018, roughly 225,000 fewer students were enrolled in SE programs annually across the state.[2] To our knowledge, this is the first major policy change that caused such a large and sudden change in SE participation for a large representative sample of students. Nearly 10 years after the policy was implemented, the federal government determined that the 8.5 district target violated federal disability law, which highlights why policies such as this one are so rare. We exploit this policy change using administrative data from Texas that follow the universe of public school students into adulthood, allowing us to provide the first long-run causal estimates of SE programs.

Our research design exploits the pre-policy variation in SE rates across districts, which led to significant differences in policy pressure to reduce SE enrollment. To identify the direct impacts of SE programs for students with disabilities, we focus on cohorts enrolled in SE before the policy's implementation and estimate the long-run effect of a reduction in SE access using two main identification strategies.[3] First, we use a difference-in-differences strategy that compares changes in SE removal, educational attainment, and labor market outcomes across cohorts with different amounts of expected school enrollment after 2005 in districts with lower versus higher pre-policy SE rates. This strategy estimates the average effect of reducing overall *access* to SE for students with disabilities. Second, we use exposure to the policy as an instrument for SE removal in an instrumental variables (IV) framework. This second strategy allows us to identify the long-run impacts of *SE removal* for students on the margin of SE placement decisions, precisely the group for whom the net benefits of SE are most unclear.

The validity of our research design rests on the assumption that districts facing more policy pressure had similar counterfactual trends as districts facing less pressure. Event study graphs

[2] This is computed by multiplying total enrollment in Texas public schools in 2018 (roughly 5 million) by the 4.5 percentage point reduction in total SE enrollment that occurred post-policy.

[3] This sample restriction ensures that all students in our sample will share the same underlying conditions. The policy pressure to reduce SE enrollment significantly changes the incentives to classify marginal students, which in turn changes the underlying conditions of SE students. As we will justify in Section 3.2, we focus on 5th grade SE cohorts. However, we demonstrate that our results are not sensitive to this grade cohort restriction.

demonstrate that the pre-policy district SE rate is uncorrelated with outcomes for cohorts who were too old to have been enrolled in school under the policy. Thus, it is unlikely our results are being driven by pre-trends. In addition, we show that changes in predicted outcomes and attrition are both uncorrelated with pre-policy rates of SE enrollment. This suggests that endogenous changes in district composition are not driving our results. For our IV approach to yield causal estimates of SE removal for marginal students, we must additionally assume that the policy only impacted students through changes in SE removal. We provide strong evidence in support of this exclusion restriction assumption. While the policy led to significant increases in the likelihood of SE removal, it did not generate other changes in instruction or resources for SE students.

Our results suggest that students who are denied access to SE services experience significant declines in educational attainment. Our difference-in-differences estimates imply that SE students enrolled in the average district experienced a 3.5 percentage points (or 12 percent) increase in the likelihood of SE removal, a 1.9 percentage points (or 2.6 percent) decrease in the likelihood of high school completion, and a 1.2 percentage points (or 3.7 percent) decrease in the likelihood of college enrollment after the policy's introduction.[4] For students on the margin of SE placement decisions, our IV estimates imply that SE removal decreases both high school completion and college enrollment by 52.2 and 37.8 percentage points, respectively. Lower-income and minority students experience larger increases in SE removal, and the negative impact of SE removal on educational attainment are concentrated among these students. We do not find that SE removal leads to significant declines in college degree attainment or earnings in the labor market, measured shortly after high school graduation (six years later). However, it is likely that the longer run college completion and employment effects may differ once these outcomes have time to fully realize.[5] The large reductions in high school completion and college enrollment that we document suggest that later life labor market outcomes are also likely to decline.

[4]These effect sizes are computed for SE students fully exposed to the policy after 5th grade and enrolled in the average school district that served 13 percent of their students in SE at baseline.

[5]Among SE students, the wage differential across college enrollment decisions emerges roughly six years after high school completion. This suggests that it may still be too early to measure these outcomes.

Why do marginal students (i.e. those with relatively mild conditions) experience such large reductions in educational attainment after SE removal? Part of the explanation is mechanical. SE students can satisfy modified high school graduation requirements, which may make educational attainment easier. For instance, students enrolled in SE may be able to graduate from high school without passing an exit exam, which is a typical high school graduation requirement. We find that students denied access to SE are significantly more likely to take the high school exit exam and significantly less likely to pass it. However, it is unlikely that changes in high school graduation requirements alone are driving our results. For instance, SE students are also likely to benefit from the additional resources and more focused attention they receive. We find that the long-run negative impacts of SE removal are concentrated in lower-resourced districts. This highlights the potential importance that additional SE resources provide, especially in districts with less available resources to prepare students with special needs for adulthood. Finally, it is important to highlight that we are inferring SE program effects based on SE removal. For students who are accustomed to receiving additional support in school, the negative impacts of SE removal could be particularly pronounced.[6]

Credibly estimating the long-run impacts of SE programs is difficult due to data limitations and the empirical challenges previously noted. The few studies that have examined SE access and placement have largely focused on short run outcomes and mostly find positive effects. Various identification strategies have been used in an attempt to account for the endogenous placement of students into SE. For example, using within student changes, Hanushek, Kain, and Rivkin (2002) find that SE participation improves math performance for students with mild learning and behavioral conditions. Using strategic placement in SE due to an accountability change that placed pressure on schools to improve overall student performance, Cohen (2007) finds that SE participation reduces absenteeism for marginal low-achieving students.[7] Only one paper (Prenovitz, 2017) finds that

[6]Long-run responses to never participating in SE may not mirror the impacts of SE removal. For instance, those never enrolled in SE do not incur any potential stigma associated with a disability label and do not become accustomed to additional supports during school. However, with our administrative data it is difficult to identify students on the margin of placement prior to a SE diagnosis.

[7]Cohen (2007) also finds suggestive evidence that SE placement reduces the probability of dropping out and improves GPA but these results are not significant at conventional levels.

SE participation harms student achievement. However, this difference from prior studies is likely driven by the context that she focuses on. Prenovitz (2017) infers SE program effects based on the introduction of No Child Left Behind (NCLB) that held schools accountable for SE subgroup performance. In this setting, schools faced incentives to assign SE to higher-achieving students and remove SE services for lower-achieving students, resulting in strategic SE placements for students most unlikely to benefit from SE. Moreover, these results offer little insight into the role of SE participation on adult outcomes. To date, the only evidence on the long-run impacts of SE has been descriptive and focused on small samples (Newman et al., 2011).

We contribute to the literature in several important ways. First, to our knowledge, we offer the first long-run causal impacts of SE participation for marginal students. Second, our focus on one of the largest and plausibly exogenous reductions in SE access allow us to isolate changes in SE access without having to make strong identification assumptions. Finally, using population data from Texas, a large and diverse state, we are able to estimate differential responses to SE access across many subgroups. We find that less advantaged students and those in lower-resourced or lower-performing districts are more negatively impacted by reduced SE access, suggesting that less access to SE programs may serve to expand pre-existing gaps in later life outcomes among these groups.

More broadly, our results speak to central questions of how to raise human capital for vulnerable student populations. First, we add to the literature that investigates the best way to allocate school resources. In particular, are targeted resources (such as those offered by SE) or broader improvements in school quality (that affect all students) more effective at improving long-run trajectories for at-risk groups? The closest related work by Setren (2019) finds that students with mild disabilities experience large achievement gains when they transition to Boston charter schools which reduce individualized instructional support (by removing students from SE), but offer higher quality overall instruction than Boston public schools. However, whether effective charter schools can be replicated is unclear. Our results suggest large returns to investing in specialized educational support when overall improvements in school quality are not possible. A rough comparison suggests that targeting additional educational resources to students with less severe disabilities offer returns that are significantly larger than reducing classroom sizes or increasing teacher salaries, but similar to early childhood programs such as Head Start or Perry-Preschool, which are commonly viewed as highly effective interventions (Levin, Belfield, Muennig, & Rouse, 2007).[8]

Second, we provide new evidence to the literature on the timing of human capital investments. While a large amount of evidence points to early childhood (i.e. before age 5) as the critical period to invest resources in vulnerable youth (Garces, Thomas, & Currie, 2002; Deming, 2009; Schweinhart et al., 2005), significantly less is known about the efficacy of interventions later during childhood. Our findings suggest that investing additional resources for vulnerable groups later during childhood can offer similar returns as early childhood investments do.

2 Background

2.1 Special Education Programs

The Individuals with Disabilities Education Act (IDEA) requires public schools to provide all students a "free and appropriate" education. Under IDEA, students with disabilities receive SE services to facilitate success in school and later in life. In Texas, as well as in other states, SE program eligibility depends on having a qualifying disability that adversely affects learning, as determined by teachers and specialists. The SE process begins when a parent, teacher, or school administrator requests that a student be evaluated for SE services. Once referred, a psychologist or special education teacher evaluates whether a student qualifies for SE services. SE students are re-evaluated once every three years (or sooner if a parent or teacher requests it). Typically students are first referred to SE during elementary school and continue to qualify for SE throughout their entire schooling. However, some students transition out of SE if a student no longer requires

[8]For this cost/benefit analysis, we use the social cost of a high school drop-out suggested by Levin et. al (2007). See Section 6 for more detail on the methodology used to compare the cost/benefit across programs.

additional educational support to be able to succeed in school.[9]

Participating students receive *individualized* services and accommodations aimed at ameliorating the challenges they are likely to face throughout schooling and later in life. Because of this individualization, what SE offers is wide-ranging. Students may receive instruction in general education classrooms accompanied by a classroom aid, in resource rooms for part of the school day, or in separate classrooms or schools entirely. Additionally, they may be eligible for extra time on standardized exams or take modified exams, which test content below grade level. Another important component of SE is the close tracking of goals in annual meetings with parents and teachers. Initially, yearly academic or behavioral goals are developed and tracked, and as students approach high school graduation the focus turns towards adulthood goals of either college enrollment or employment.[10]

As previously noted, SE participation has grown significantly since 1975 (from 8 to 13 percent). These increases in SE participation have been driven by large increases in learning disabilities, speech impairments, other health impairments (including ADHD), and emotional disturbance. Altogether, these conditions, hereafter referred to as "malleable disabilities", now represent over 90 percent of total SE enrollment in Texas. Unlike conditions that are physical or more cognitively severe, SE eligibility for these conditions often involve discretion on the part of diagnosticians, teachers, and parents. First, because the most common symptoms for these disabilities are poor academic performance and classroom behaviors, which many students exhibit occasionally, there are inconsistencies in SE referrals (Kauffman, Hallahan, & Pullen, 2017). Moreover, even after being referred, determining whether these conditions adversely affect learning without additional support (the main SE inclusion criteria) can be subjective, as can determining whether students should remain in SE over time (Association et al., 2013). This subjectivity

[9]For instance, in our sample over 70 percent of SE students who are diagnosed during elementary school (as of 5th grade) continue to participate in SE into high school.

[10]This preparation for adulthood is called transition planning. Students who aim to enroll in college typically receive guidance on which colleges they should apply to and which courses would best prepare them for college. Those focused on employment typically receive guidance on apprenticeships or other career/technical courses that may be beneficial once they enter the labor market. Specific examples of transition plans are included in Appendix Figures A.1 and A.2

underscores the empirical challenges involved in estimating the causal impact of SE participation.

2.2 Policy Background

In the 2004-05 academic year, Texas implemented the Performance Based Monitoring Analysis System (PBMAS) to monitor SE programs in public schools. Under the PBMAS, districts received annual reports, which included several indicators to monitor SE programs. Broadly, these indicators were aimed at limiting SE participation, improving SE students' academic and behavioral outcomes, and reducing the amount of services and accommodations being provided to SE students (i.e. reducing time spent in separate classrooms and modified test-taking). However, beyond introducing strong downward pressure on SE enrollment, this policy did not introduce significant policy pressure on districts to make other changes for SE students. At the time the policy was introduced, roughly 98 percent of districts met or nearly met policy thresholds related to behavioral and academic outcomes and 80 percent of districts met or nearly met policy thresholds related to the services and accommodations offered to SE students. In contrast, only 5 percent of districts met the thresholds related to SE enrollment.[11]

In this paper, we focus on the policy pressure to reduce SE enrollment due to the introduction of a district SE enrollment target of 8.5 percent. Under this policy, any district that served more than 8.5 percent of their students in SE faced state interventions ranging in severity based on a district's distance above the target.[12] Districts closer to the target were subject to developing monitoring improvement plans, while those further away were subject to third party on-site monitoring visits (Texas Education Agency, 2016).[13] The first PBMAS report was received by districts in December of the 2004-05 academic year, and was met by a sharp decline in SE enrollment.[14]

[11]Panel A of Appendix Table A.1 provides more detail on the fraction of districts that met, nearly met, did not meet, or were far out of compliance in each of SE monitoring area.

[12]Appendix Figure A.3 shows the rating that each district was assigned based on their SE rate.

[13]Despite minimal sanctions for districts closer to the target, districts responded strongly. Based on a series of interviews featured in a Houston Chronicle investigation of this policy school administrators report taking this target seriously. For instance, one special education director noted, "We live and die by compliance. You can ask any special ed director; they'll say the same thing: We do what the Texas Education Agency (TEA) tells us" (Rosenthal, 2016).

[14]Because the first PBMAS report was received in the middle of the 2004-05 school year (i.e. December 2004), in what follows, we consider the 2005-06 academic year as the first post-policy year. This was the first academic year where districts would have responded to the policy pressure to reduce SE enrollment.

Figure 1 demonstrates that while the fraction of students enrolled in SE programs was constant during the five years prior to the SE enrollment target (2000-2005), there was a sharp decline during the five years afterwards (2005-2010). The average district experienced a 4.5 percentage point drop with the largest reductions for districts furthest from the target.

In order to utilize the introduction of this policy to study the long-run effects of SE participation, it is important to establish that the introduction of the SE enrollment target was exogenous. Importantly, it appears to have been introduced in response to an unexpected state budget cut (Hill et al., 2004) rather than statewide trends in SE enrollment or expenditures. There is strong anecdotal evidence that it was unanticipated by districts (Rosenthal, 2018) and Figure 1 shows little indication of pre-trends in SE enrollment in the period leading up to the policy's introduction. In addition, it is important to establish that exploiting the cross-district variation in the pre-policy district SE rate will allow us to identify the effect of a reduction in SE access separate from other changes for SE students. Despite the SE enrollment target being introduced as part of a broader monitoring effort, it is unlikely districts would have made other changes for SE students beyond reducing their access to SE programs. As previously noted, the policy pressure to make any instructional changes beyond SE removal was minimal. Therefore, we assume that the introduction of PBMAS impacted students only through reducing their access to SE programs. Reassuringly, throughout the paper we demonstrate that the pressure to make other instructional changes for SE students are not driving our results, in support of this assumption.

3 Data and Summary Statistics

3.1 Data Sources

We leverage restricted-access administrative data from the Texas Schools Project (TSP). These data follow the universe of Texas public school students into adulthood, tracking key education and labor market outcomes. Specifically, we start with student-level records from the Texas Education Agency (TEA). These data contain records for all Texas public school students in grades K through 12, including yearly information on demographics, academic, and behavioral outcomes.[15] Importantly, these data include information on annual SE program participation, as well as disability type, the amount of time spent in resource rooms (i.e. receiving instruction in separate classrooms),[16] and whether students' took the unmodified version of standardized exams. Thus, we are able to carefully track changes in SE placement, as well as the types of accommodations being offered to students over time. We link these student-level school records from TEA to post-secondary enrollment data from the Texas Higher Education Coordinating Board (THECB), as well as to labor market earnings from the Texas Workforce Commission (TWC). The THECB data include enrollment and degree attainment information for all Texas universities and the TWC includes earnings records for all Texas employees subject to the state unemployment system.

These administrative data are advantageous both in terms of the number of long-term outcomes and the large sample size. One drawback of using administrative data from a single state is that we cannot track people who leave Texas. However, outmigration from Texas is quite low. Most people born in Texas remain in the state (Aisch, Gebeloff, & Quealy, 2014) and only 1.7 percent of Texas residents leave the state each year (White et al., 2016). In addition, we are able to link a subset of our sample to the National Student Clearinghouse (NCS) data in order to determine how often students attend college out of state. Only 1.7 percent of SE students enroll in college outside of Texas within two years of their high school graduation.[17]

3.2 Sample Construction

To identify the impact of SE on student outcomes, we focus on students enrolled in SE prior to the enactment of the target and infer program effects from policy-driven SE removals. In particular,

[15]Our data does not include performance on modified versions of standardized exams. Because the policy significantly reduced SE enrollment, the fraction of students observed in the achievement data will be increasing endogenously over time due to fewer students enrolled in SE students who have the option of taking the modified versions. This underscores why we do not focus on the impact of SE removal on achievement as a primary outcome in this paper.

[16]Specifically, we observe whether students spent all day in regular classrooms (or mainstreamed), less than 50 percent of the day in separate classrooms, or more than 50 percent of the day in separate classrooms.

[17]We demonstrate in Section 5.4 that our results are not sensitive to the inclusion of out of state college enrollment. When we focus the subset of students for whom we observe NSC data (i.e. 5th grade SE cohorts from 2001 through 2005), models that include out of state college enrollment provide nearly identical estimates to our main estimates which only include college enrollment within Texas.

we focus on students enrolled in SE programs as of 5th grade. We focus on 5th grade SE cohorts for several reasons. First, they capture a stable sample of SE students: as Appendix Figure A.4 makes clear, SE enrollment typically grows rapidly throughout elementary school and levels off by 5th grade (with very little new enrollment afterwards). Moreover, 5th grade cohorts have many remaining years in school making them more susceptible to the policy change than older cohorts would have been.[18]

Our main analysis sample consists of 5th grade SE cohorts enrolled between 1999-00 and 2004-05. The 2004-05 cohort was the last cohort diagnosed before the SE enrollment target was enforced in public schools.[19] Since the policy significantly changed the composition of students identified with disabilities, this cohort restriction is necessary in order to ensure that students in our sample have similar underlying conditions. Unless otherwise specified, we also restrict the earliest cohort to the 1999-00 cohort (rather than the 1995-96 cohort when our data begins). We make this additional restriction in order to avoid including cohorts affected by the introduction of school finance equalization in Texas that also affected SE classification incentives (Cullen, 2003). In particular, Cullen (2003) demonstrates that school finance equalization increased fiscal incentives to enroll marginal students in higher-wealth districts. By 1999-00, SE enrollment rates had leveled off.[20] Finally, we limit our sample to students in districts that served between 6.6 and 21.5 percent of their students in SE in 2004-05 to focus on districts with typical rates of SE.[21] The final sample consists of roughly 40,000 SE students from each cohort, for a total of 227,555 students.

To examine a particularly vulnerable subgroup, we use information about one's disability

[18] However, our results are not sensitive to this grade cohort restriction. In Appendix Table A.9 we demonstrate that the impact of SE removal for 4th and 6th grade cohorts provide similar estimates to 5th grade cohorts.

[19] Our data reports SE participation as of October. Thus the 2004-05 cohort was enrolled in SE as of October 2004 prior to when districts received the first PBMAS report in December 2004.

[20] While school finance equalization changed classification incentives, it led to relatively small changes in SE access. As such, in Appendix Table A.9 we demonstrate that our results are largely unchanged if we use the the extended number of cohorts (i.e. 1995-96 - 2004-05) or use cohorts in our main analysis sample (i.e. 1999-00 - 2004-05). Thus, it is sometimes helpful to extend the number of 5th grade cohorts back to 1995-96. For instance, in our event-study analysis extending the number of cohorts back to 1995-96 allows us to provide more visual evidence of pre-trends. If we use the 1995-96 as our oldest 5th grade cohort rather than the 1999-00 cohort, this will be clearly specified.

[21] This drops roughly 1% of the overall sample since district outliers with respect to SE rates are small. We demonstrate in Appendix Table A.9 that our results are nearly identical if these districts are included.

(measured as of 5th grade) to identify students whose diagnoses may have been easier to manipulate under the policy. We classify students as being more vulnerable to the policy pressure to reduce SE enrollment if they had a malleable disability (including learning disabilities, speech impairments, other health impairments (includes ADHD), or emotional disturbance) and if they received more than 50 percent of their instruction in general education classrooms at baseline.[22] In what follows, we refer to this subgroup as our "high-impact" sample consisting of 189,042 students.

3.3 Summary Statistics

Table 1 presents summary statistics for 5th grade cohorts enrolled between 1999-00 and 2004-05. Columns 1 vs. 2 compare students not enrolled in SE to those who are as of 5th grade. Students in SE are more likely to receive Free and Reduced-Price Lunch (FRL), are slightly more likely to be enrolled in the English Language Learner (ELL) program, have lower performance on standardized exams (conditional on taking the unmodified version of the tests), and have lower long-run outcomes (i.e. less likely to graduate, enroll in college, and have lower early labor market outcomes). These differences help to highlight the fact that raw comparisons between those who receive SE services and those who do not will be biased due to negative selection into SE programs.

Column 2 of Table 1 demonstrates that 91 percent of SE students diagnosed by 5th grade have malleable disabilities, the most common of which is a learning disability at 60 percent. The majority of SE students, 84 percent, receive over 50 percent of their instruction in regular classrooms and 30 percent take unmodified standardized exams. As previously noted, SE students may transition out of SE programs if SE services are no longer appropriate. Columns 3 vs. 4 of Table 1 compare SE students who continue in SE to those removed by 9th grade. 5th grade SE students who lose SE are less likely to receive FRL, less likely to participate in ELL, have higher achievement on standardized exams, and better long-run outcomes.[23] Nearly all students who lose SE have malleable disabilities (98 percent) and require fewer modifications to the regular

[22] The rationale for this restriction is that if students are receiving most of their instruction outside of general education classrooms then they are likely to have more severe conditions which may make it more difficult to justify SE removal.

[23] High school graduation is the one exception to this pattern which can likely be explained by accommodated graduation requirements available only to SE students.

curriculum; over 97 percent receive over 50 percent of their instruction in regular classrooms and 60 percent take unmodified standardized tests. These differences highlight the positive selection into SE removal; without exogenous changes in SE participation, comparisons between those who continue in SE vs. those who lose SE will be biased due to positive selection into SE removal.

4 Empirical Strategy

4.1 Difference-in-Differences Estimates of the Policy on Outcomes

We first estimate the causal impact of the policy pressure to reduce SE enrollment on student outcomes. The SE enrollment target was introduced in all districts at the same time, so it is not possible to use cross-district variation in implementation date. Instead, we use differences in treatment intensity, which varies across students in two ways. First, districts with higher pre-policy rates of SE enrollment faced stronger policy pressure to reduce SE enrollment. Thus, any effect of the policy should be increasing with a student's district's pre-policy SE enrollment rate.[24] Second, 5th grade cohorts were differentially treated under the policy based on the remaining number of years that they were expected to be enrolled in school after the policy's introduction in 2004-05.

We start by estimating an event-study specification that allows us to identify the impact of the policy for each 5th grade cohort separately as follows:

$$Y_{icd} = \gamma_0 + \sum_{j=c_{min}}^{2005} \gamma_j \mathbb{1}\{c = j\} \times \text{SERate}_d^{Pre} + \lambda_1 X_{icd} + \lambda_2 Z_{dc} + \gamma_d + \phi_c + \varepsilon_{icd} \quad (1)$$

where Y_{icd} is an indicator for SE removal or a long-run outcome for student i in 5th grade cohort c in district d. SERate_d^{Pre} is the fraction of SE students above the 8.5 percent target in a students' 5th grade district in the 2004-05 school year (the year before the policy was implemented)[25] and is

[24] While this district level treatment is continuous, it may be helpful to think about districts under more policy pressure as forming the "treated" group, whereas, those under less pressure form the "control" group.

[25] We assign policy exposure based on a student's 5th grade district (which was determined pre-policy). This ensures that our estimates will be free of bias from selection into districts under less policy pressure to reduce SE enrollment.

interacted with each 5th grade cohort indicator variables. We control for 5th grade district fixed effects γ_d and 5th grade cohort fixed effects ϕ_c. The vector X_{icd}, includes a dummy for gender, race, Free and Reduced-Price Lunch (FRL) status, English Language Learner (ELL) classification, gender-race interactions, primary disability, unmodified exam indicator, and level of classroom inclusion, all measured at baseline in 5th grade, in order to absorb differences by student demographics and disability type. Further, to control for changes in district-level demographics, Z_{dc}, includes the district percent of students by racial group, FRL, ELL, and gender for the full student population and for the SE student population all defined at baseline.[26]

The main variables of interest, γ_j, identify differences in outcomes across students in districts with higher versus lower pre-policy SE rates for each 5th grade cohort separately. We begin with this specification since it allows us to directly test our main identification assumption, that in the absence of the policy, districts with higher pre-policy SE rates would have exhibited similar trends to districts with lower pre-policy SE rates. If our results are not being driven by pre-trends, γ_j should be zero for 5th grade cohorts who were too old to be enrolled in school under the policy.

Next, we estimate the average effect of the policy using a difference-in-differences specification. We make two specification decisions that are motivated by our event-study results discussed in more detail in Section 5. First, cohorts with more years of policy exposure after 5th grade experienced the largest changes in SE removal. Thus, we choose a continuous measure of post-policy exposure that accounts for the number of years after 5th grade each cohort was expected to be enrolled in school after 2004-05. Second, we find that the policy led to significant declines in high school completion. Because this will change the composition of high school enrolled students, we decide to focus on policy exposure during a period right before high school drop-out decisions are made. In Texas, most students decide to drop-out of high school right after 9th grade (Texas Education Agency, 2018). Therefore, we define post-policy exposure between 5th grade and expected 9th grade.[27] Reassuringly, our results are not sensitive to small changes in the time period

[26]Controlling for average district characteristics allow us to account for overall changes in district demographics, while controlling for district averages using SE students accounts for compositional changes for students in our sample.

[27]This is defined as four years after 5th grade. If we measured policy exposure between 5th grade and observed 9th

used to measure policy exposure.[28] To illustrate the cross-cohort variation we utilize, Appendix Table A.2 shows policy exposure by each 5th grade cohort in our main analysis sample. Specifically, we estimate the following specification:

$$Y_{icd} = \delta_0 + \delta_1(\text{SERate}_d^{Pre} \times \text{FracExposed}_c) + \lambda_1 X_{icd} + \lambda_2 Z_{dc} + \gamma_d + \phi_c + \varepsilon_{icd} \quad (2)$$

where FracExposed_c is a continuous measure of policy exposure, defined as the share of time between 5th and expected 9th grade that each cohort was enrolled in school after 2004-05. All other variables are as previously defined. The main coefficient of interest, δ_1, represents the average impact of the policy pressure to reduce SE enrollment on student outcomes.

As previously noted, this approach relies on the assumption that districts under more policy pressure to reduce SE enrollment had similar counterfactual trends relative to districts facing less pressure. Our event-study plots discussed in more detail in Section 5 provide compelling evidence against the existence of pre-trends. Nevertheless, we perform three additional checks to rule out the possibility that our results are being driven by differential trends. First, we investigate whether the policy led to differential attrition. If more motivated parents (whose children would be expected to have better outcomes) in more treated districts decided to move their children out of Texas public schools after policy implementation, then it would be possible that are results are being driven by compositional changes, rather than the policy pressure to reduce SE enrollment. Columns 1 and 2 of Appendix Table A.3 provide estimates of δ_1 from Equation 2 where the outcome variable is an indicator for whether a student was enrolled in Texas public schools in expected 9th grade (conditional on being enrolled in 5th grade) or an indicator for whether a student's 9th grade district differed from their 5th grade district. These results provide strong evidence that our results are

grade we would mechanically assign more years of policy exposure to grade repeaters. By focusing instead on expected 9th grade we ensure that each student within a 5th grade cohort is assigned the same amount of policy exposure.

[28]When we focus on policy exposure between 5th grade and expected 8th grade or 5th grade and expected 10th grade we find similar results (Appendix Table A.9).

unlikely to be driven by differential attrition or district switching.[29]

Second, we ask whether *changes* in demographics were correlated with initial district SE rates. If there were demographic changes in more treated districts around the time that the policy was introduced, it is possible that these demographic changes (rather than the policy) could be driving our results.[30] To address this possibility, we test whether changes in predicted outcomes (based on student demographics)[31] and demographic characteristics were correlated with initial district SE rates. Columns 3, 4, and 5 of Appendix Table A.3 provide estimates of δ_1 from Equation 2 where the outcome variables are predicted outcomes for each of our main outcome variables (i.e. SE removal, high school completion and college enrollment). Columns 6, 7, and 8 of Appendix Table A.3 provide estimates of δ_1 from Equation 2 where the outcome variables are student demographic characteristics. If changes in district composition were uncorrelated with initial district SE rates, δ_1 should be zero. Across these outcomes only two are statistically significant for the full sample and one is for the high impact sample. For the full sample presented in Panel A, we find that predicted college enrollment and SE removal are positively correlated with policy exposure. For the high impact sample presented in Panel B, we find that predicted high school completion is positively correlated with policy exposure. None of these effects, however, are economically meaningful.[32] Moreover, the positive direction of these effects suggest, if anything, that students in more treated districts were becoming positively selected over time, which would underestimate the negative impact we find on long-run outcomes.

A final concern is that the estimated effect of the policy could be driven by differential trends in outcomes due to underlying population differences in more versus less treated districts.[33]

[29]While district switching does not pose a threat to our identification strategy since we assign treatment based on each student's 5th grade district, excessive district switching could attenuate our estimates of the policy on outcomes.

[30]For instance, changes in poverty or immigration could have differed by pre-policy SE rates.

[31]To obtain predicted outcomes, we regress our main outcomes (SE removal, high school completion, college enrollment) on all covariates in Equation 2, excluding treatment. Then, using the coefficients from this model, we predict SE removal, HS graduation, and college enrollment.

[32]For the full sample, these positive effects (significant at the 5 percent level) are small and correspond to a 1 percentage point (or roughly 3 percent) change for both outcomes. For the high impact sample, given that the coefficients in our main models are nearly ten times larger, we do not believe this positive relationship is of concern.

[33]It is important to highlight that while our identification strategy requires that district level policy exposure to be

Reassuringly, our results are robust to time trends interacted with district level characteristics measured in the 2004-05 school year. Appendix Table A.4 presents results where we include trends interacted with the baseline fraction of Hispanic students, fraction of FRL students, and total cohort size. In all cases, the estimates are robust to the inclusion of such trends, likely ruling out the possibility that differences in trends across student demographics are driving our results.

4.2 IV Estimates of SE Removal on Long-Run Outcomes

Next, we use changes in SE access as an instrument for changes in SE participation. Since our setting focuses on students already enrolled in SE programs, our first stage outcome is SE removal and our instrument is our measure of policy exposure (i.e. $\text{SERate}_d^{Pre} \times \text{FracExposed}_c$). With this approach, we identify the local average treatment effect (LATE) of SE removal on long-run outcomes for students on the margin of SE placement decisions, precisely the group for whom the net benefits of SE are most unclear.

This IV approach hinges on two assumptions. First, the policy must generate variation in SE removal. As we will demonstrate in Section 5.1, the policy significantly increased the likelihood of SE removal. Second, we must assume that the exclusion restriction holds. That is, policy exposure only impacted students through changes in SE removal. Thus, a potential concern is that the policy lead to changes that could affect student outcomes through other channels. For instance, if more treated districts made other instructional changes for SE students (i.e. changes in resources or the types of accommodations offered), then we would not be able to interpret our reduced form effect on student outcomes as the effect of SE removal alone.

To rule out other channels, we first consider whether more treated districts changed resources for SE students. Given these districts were reducing the number of students enrolled in SE, they may have shifted resources from SE programs to regular education, at the detriment of SE students' outcomes. Alternatively, if districts kept resources constant, students who continued to be

uncorrelated with *changes* in long-run outcomes, it does not require district level policy exposure to be uncorrelated with district characteristics. In fact we find that more treated districts were slightly less likely to be hispanic, slightly more likely to receive FRL, be located in rural areas, and have a smaller average cohort size.

enrolled in SE after the policy could have benefited from more resources per SE pupil. As shown in Table A.5, we find no significant impact of the enrollment target on district level SE or GE per-pupil spending or on student-teacher ratios during the five years after policy introduction, suggesting that changes in school-based resources for SE students are unlikely to be driving our results.

Next we examine whether students in more treated districts experienced other instructional changes. A potential concern is that the introduction of the PBMAS may have led to changes in the services and accommodations being offered to SE students. As previously noted in Section 2.2, due to the minimal policy pressure that the PBMAS placed on districts (except for the strong pressure to reduce SE access), we believe it is unlikely students would have experienced other changes in services or accommodations. Importantly, we can rule out this possibility empirically. Columns 2 through 4 of Appendix Table A.6 provide estimates of δ_1 from Equation 2 where the outcome variables are indicators for whether students spent minimal time in separate classrooms (i.e. resource rooms) or took the unmodified test (all measured during expected 9th grade).[34] Overall, we find little evidence that the policy introduced other changes in services or accommodations for SE students. We only find that the policy significantly increased the likelihood of taking the unmodified math exam. However, to interpret this positive finding, it is important to note that students no longer enrolled in SE will have to take unmodified exams, making it plausible that this effect is driven by SE removal as opposed to changes in how test-taking decisions for SE students are made. The magnitude of this positive coefficient is nearly identical to the magnitude of the coefficient on SE removal (both corresponding to a 4 percentage point increase), providing suggestive evidence in support of this conjecture.[35]

[34]These outcomes were chosen based on the specific indicators monitored under the PBMAS. The only indicators that we cannot directly test is whether districts were making efforts to improve the academic achievement of SE students. Since we only observe scores for the unmodified version of the exam, it is hard to address whether the academic performance of SE students was improving. However, as illustrated in Appendix Table A.1, 97% of all school districts were already meeting the academic standards outlined prior to policy implementation, suggesting very minimal policy pressure along this dimension. Furthermore, any pressure to improve academic outcomes would underestimate the negative effect of SE removal on long-run outcomes that we find.

[35]Furthermore, this increase in unmodified test taking would only introduce bias if the type of exams a SE student takes has a direct influence on long-run outcomes, which is a-priori unclear given the flexibility available to SE students regarding high school graduation requirements. For instance, even if SE students take the unmodified exams and fail them, high school graduation may still be deemed appropriate.

These findings provide strong evidence that beyond large increases in SE removal, students in districts under more policy pressure to reduce SE enrollment were unlikely to be impacted in other ways. Reassuringly, we demonstrate in Section 5.4 that our results are robust to dropping districts under pressure to reduce the amount of time spent in separate classrooms or taking modified exams. We also show that our results are robust to focusing on SE students who were already taking unmodified versions of standardized exams and spending less time in resource rooms (i.e. the level of additional services compliant under the PBMAS policy). The robustness of our results to these sample restrictions (which allow us to focus in on students likely to *only* be impacted by the policy pressure to reduce SE access), suggest that it is very unlikely our results are being driven by anything other than the pressure to reduce SE enrollment.[36]

5 Results

5.1 Difference-in Differences Results

SE Removal

We begin by establishing that the policy pressure to reduce SE enrollment increased the likelihood of SE removal. First, we examine the relationship between the 2004-05 district SE rate and the likelihood of SE removal for each 5th grade cohort separately with an event-study plot. While our main analysis sample includes SE students from 1999-00 through 2004-05 (as justified in Section 3.2), we extend the number of cohorts back to 1995-96 for this event-study analysis to provide additional visual evidence of pre-trends.[37] Figure 2 plots the full set of γ_j from Equation 1 where the outcome is an indicator for whether a student was removed from SE in the year they were expected to be in 9th grade. Cohorts expected to graduate high school before the policy or with late exposure (after expected 9th grade) did not experience increases in SE removal. This pattern provides strong

[36]Moreover, Panel B of Appendix Table A.1 demonstrates that compliance in other areas of SE monitoring was uncorrelated with the pressure to reduce SE enrollment. This suggests that even if SE monitoring did introduce changes in Texas, the pressure to make changes were not stronger for districts facing the most pressure to reduce SE enrollment.

[37]We also present event study results for our main analysis sample in Appendix Figure A.5. Event-study plots that include 5th grade cohorts between 1999-00 and 2004-05 demonstrate similar patterns to event-study plots that include an expanded number of 5th grade SE cohorts (i.e. between 1995-96 and 2004-05).

evidence that pre-trends in SE removal are unlikely to be driving our results. Cohorts exposed to the policy between 5th and 9th grade experienced significant increases in SE removal by expected 9th grade, with the largest increases for cohorts with more years of policy exposure before 9th grade.[38]

To quantify the magnitude of these effects, we turn to our difference-in-differences estimates for 5th grade SE cohorts between 1999-00 and 2004-05. Table 2 provides estimates of δ_1 from Equation 2 where the outcome variable is an indicator for whether a student lost SE services in the year that they were expected to be enrolled in 9th grade (or four years after 5th grade). We show results for the overall sample in Panel A and our high impact sample (those with mild malleable disabilities) in Panel B. Starting with a model that only includes 5th grade cohort indicators and district fixed effects, we successively add controls. For both samples, our estimated effects are largely stable to choice of specification, especially once we condition on individual disability type measured at baseline (i.e. as of 5th grade). In our fully specified model, the policy significantly increased the likelihood of SE removal in districts with higher pre-policy SE rates for both samples.

The results for the overall sample suggest that SE students at the average district (that was 4.5 percentage points above the SE enrollment target in 2004-05) who were fully exposed to the policy after 5th grade experienced a 3.5 percentage points (.00778*4.5) or 12 percent increase in the likelihood of SE removal. We observe larger effects for our high impact sample, implying that the policy had a larger impact on SE removal for students whose SE placement decisions may have been easier to manipulate. In the high impact sample, SE students at the average district who were fully exposed to the policy after 5th grade experienced a 4.2 percentage points (.00921*4.5) or 13 percent increase in the likelihood of SE removal. In addition, the policy had little impact on students whose SE removal would have been more difficult to justify with more severe or straightforward conditions. Appendix Table A.7 presents estimates for students with more severe malleable disabilities (who required separate instruction for more than 50 percent of the day) and those with non-malleable

[38] Appendix Figure A.6 shows an event study plot that looks at an indicator of ever losing SE as the outcome variable. This plot shows a very similar pattern to the one presented in Figure 2. 5th grade cohorts exposed in later grades (i.e. after 9th grade) are not more likely to lose SE despite being partially enrolled in school after the policy went into effect.

disabilities.[39] For both groups, the estimates are statistically indistinguishable from zero, implying that these students were unlikely to lose SE under the policy.

Educational Attainment

Next, we estimate whether less access to SE due to the policy impacted educational attainment decisions. Again, we start with event-study plots for an extended number of cohorts.[40] Figure 3 plots the full set of γ_j from Equation 1 where the outcome is an indicator for whether a student graduated from high school (Panel A) or enrolled in college within four years of their expected high school graduation (Panel B). Both plots demonstrate similar patterns. Cohorts expected to graduate high school before the policy was implemented or with late exposure did not experience significant declines in educational attainment. These patterns provide strong evidence that differential trends in educational attainment are unlikely to be driving our results. Moreover, the impacts of the policy are increasing across cohorts with the number of years that they were exposed to the policy after 5th grade and before 9th grade. These results demonstrate the relevance of our treatment margin, which defines treatment between 5th and expected 9th grade. Despite older cohorts being partially exposed to the policy later during high school, the effects on educational attainment are being driven by 5th grade cohorts who were exposed to the policy before the were expected to be in 9th grade.

To quantify the magnitude of these effects, we turn to our difference-in-differences estimates for students enrolled in 5th grade SE cohorts between 1999-00 and 2004-05. Table 3 provides estimates of δ_1 from Equation 2, where the outcomes is either an indicator for whether a student graduated from high school (Panels A and B) or whether a student enrolled in college within 4 years of their expected high school graduation (Panels C and D). We show the results separately for the full sample (Panels A and C) and the high impact sample (Panels B and D). Importantly, these estimates are very stable once individual disability type is controlled for, demonstrating that

[39]Non-malleable disabilities include autism, deafness, blindness, developmental delay, hearing impairments, intellectual disabilities, orthopedic impairments and traumatic brain injury.

[40]We also present event study results for our main analysis sample in Appendix Figure A.5. Event-study plots that include 5th grade cohorts between 1999-00 and 2004-05 demonstrate similar patterns to event-study plots that include an expanded number of 5th grade SE cohorts (i.e. between 1996-97 and 2004-05).

once we condition on a student's underlying condition, exposure to the SE enrollment target is independent of these outcomes. These results demonstrate that the policy significantly reduced the likelihood of high school completion and college enrollment for both samples.

The results for the overall sample suggest that at the average district (that was 4.5 percentage points above the SE enrollment target in 2004-05) full exposure to the policy after 5th grade decreased the likelihood of high school graduation by 1.9 percentage points (or 2.6 percent) and decreased the likelihood of college enrollment by 1.2 percentage points (or 3.7 percent). Moreover, the effects are stronger for students in our high impact sample who experienced a 2.2 percentage point (3.1 percent) decrease in the likelihood of high school graduation and a 1.7 percentage point (4.8 percent) decrease in the likelihood of college enrollment. The results for those with severe malleable disabilities and those with non-malleable disabilities are presented in Appendix Table A.7. These groups who were less likely to be impacted did not experience reductions in educational attainment due to the policy. Thus, the negative impacts on educational attainment outcomes are driven by the students who were likely to lose SE services. This is reassuring for our IV approach that assumes the reduced form effects are solely being driven by SE removal.

Early College Completion and Labor Market Outcomes

In this paper, we measure college completion and labor market earnings 6 years after expected high school graduation. This is the latest the youngest 5th grade cohort in our sample can be followed. Since 33 percent of SE students who attend college are still enrolled in college 6 years after high school, we acknowledge that the longer-term effects for these outcomes may differ once there is enough time post-policy for students to complete college.[41] Nonetheless, we present early college completion and labor market outcomes in Appendix Table A.8 for 5th grade SE cohorts between

[41] Appendix Figure A.7 plots the raw yearly earnings profiles associated with a SE student's college enrollment decision for the oldest cohort in our main analysis sample (i.e. those in SE in 5th grade in 1999-00), who can be followed up to 13 years after their expected high school graduation. While this plot does not account for selection into college enrollment, it does highlight the fact that the wage differential across the decision to enroll in college that arises roughly five years after high school graduation, and grows rapidly thereafter.

1999-00 and 2004-05, with the important caveat that these outcomes may be too early to measure. These estimates suggest a decline in earning a Bachelor's or Associate's degree, being employed, and earnings, all measured six years after each cohort's expected high school graduation. However, none are estimated precisely for either sample.

5.2 IV Estimates

Having demonstrated that the SE enrollment target significantly increased the likelihood of SE removal, we apply an IV approach to identify the causal impact of SE *removal* on long-run outcomes. The results of this IV analysis are presented in Table 4, where we present results for 5th grade SE cohorts between 1999-00 and 2004-05 in our high impact sample.[42] We start with OLS estimates of SE removal on educational outcomes in Column 3. Using OLS models, we find that SE removal is associated with small decreases in high school completion and small increases in college enrollment.[43] However, OLS estimates will be biased upwards since students who typically experience SE removal do so because they experience improvements in their learning or behavioral outcomes. Our IV estimates presented in Column 4 illustrate the extent to which OLS estimates of the impact of SE removal are biased upwards. Students in our high impact sample on the margin of SE placement were 52.2 percentage points less likely to graduate high school and 37.8 percentage point less likely to enroll in college, as a consequence of SE removal.[44] While these are large effects, given that SE removal is accompanied with a significant change in a student's instructional environment and high school graduation requirements (even for marginal students), we believe these estimates are of plausible magnitude. We consider the plausibility of these magnitudes in greater detail in Section 6.

[42] For reference, Columns 1 and 2 of Table 4 show the first stage effect (i.e. the impact of the policy on SE removal by 9th grade) and the reduced form effect (i.e. the impact of the policy on educational attainment outcomes), respectively.

[43] While we would expect to find that SE removal was associated with positive increases in high school completion, the negative correlation can likely be explained by differences in high school graduation requirements. Despite the fact that students removed from SE programs are positively selected, it is more difficult to graduate outside of SE programs which raises high school graduation standards.

[44] At the bottom of Table 4 we report the Kleibergen-Paap F-statistic to test whether our instrument is weak. The Kleibergen-Paap F-statistic of 17.02 is above critical values that test for weak instruments.

5.3 Heterogeneous Impacts

We next explore whether there are differential impacts of the policy by socio-economic background. Ideally, we would first determine how the underlying conditions of marginal students compare across subgroups. If the underlying conditions across subgroups were similar, we would be able to attribute differences in SE removal to differences in how different subgroups respond to SE access.[45] On the other hand, if the underlying conditions across subgroups differed, then differential responses to SE removal could be driven by the disability severity of marginal participants. Unfortunately, definitively establishing how the marginal SE student compares across student demographics is difficult with most available datasets (including our own). Recent evidence that has been able to account for a large number of student characteristics, namely health endowments or early achievement measures, points to minority students being less likely to be enrolled in SE (Elder et al., 2019) with fewer differences in SE access by family income (Hibel, Farkas, & Morgan, 2010).[46] Despite having access to fewer covariates than these recent studies, we arrive at a similar conclusion based on models that predict 5th grade SE receipt based on demographics and 3rd grade achievement.[47] Although our predictive models only offer suggestive evidence of how the underlying conditions compare across subgroups, we view it as likely that at baseline, minority students were likely to have more severe conditions than non-minority students (due to facing less SE access) but that there were fewer differences in disability severity across family income (since access to SE was more equal).[48]

[45]Even if marginal students across subgroups had similar underlying conditions, differential responses to SE removal could emerge if more advantaged youth attended higher-resourced schools or had parents that were better able to offset the negative consequences of SE removal by paying for services outside of school.

[46]Elder et al. (2019) link a rich set of health and economic endowments at birth to later SE participation. Hibel et al. (2010) utilize information on achievement prior to Kindergarten entry to predict SE participation.

[47]Specifically during the pre-policy period, we predict the likelihood of SE participation by 5th grade using 3rd grade characteristics. Before accounting for 3rd grade achievement, minority and FRL students are more likely to be enrolled in SE programs by 5th grade (Column 1, Appendix Table A.10). Once we condition on 3rd grade achievement, however, being FRL displays a relatively weak relationship with the likelihood of SE placement in 5th grade (i.e. only 0.5 percentage points less likely to be enrolled in SE), while being a minority student is a stronger predictor of not being enrolled in SE as of 5th grade (i.e. 4 percentage points less likely).

[48]In other words, the differences we document across race may partly reflect the fact that minority students were likely to have more severe conditions at baseline. However, the differences across family income are likely to reflect differences in how low-income students respond to less SE access.

Our estimates suggest that low-income and minority students are significantly more likely to lose SE as a consequence of the policy. Panel A of Table 5 demonstrates that the likelihood of losing SE is nearly twice as high for FRL students relative to non-FRL students (Columns 1 vs. 2).[49] On average, students eligible for FRL are 5 percentage points more likely to lose SE after the policy, while the estimates for non-FRL students are indistinguishable from zero. Moreover, this difference is statistically significant, with a p-value associated with the test of equality across coefficients of 0.02. Similarly, minority students are more likely to lose SE than white students. On average, minority students are 5 percentage points more likely to lose SE, while white students are 3 percentage points more likely to lose SE. This difference, however, is not statistically significant, with a p-value associated with the test of equality across coefficients of 0.25. These results are consistent with less advantaged parents being less able to challenge SE removal decisions being made by school personnel under pressure to reduce SE enrollment (Koseki, 2017).

We find that the reductions in educational attainment are driven by low-income and minority students. In Table 5 we show difference-in-differences and IV estimates for high school completion in Panel B and for college enrollment (within 4 years of expected high school graduation) in Panel C. IV estimates reveal that marginal FRL students are 49 percentage points less likely to graduate from high school and enroll in college if removed from SE. IV estimates reveal that marginal minority students are 57 percentage points less likely to graduate from high school and 68 percentage points less likely to enroll in college if removed from SE. In contrast, non-FRL and white students do not experience declines in educational attainment due to the policy. There is only one instance where we find an impact of the policy on longer-run outcomes for non-FRL students. Difference-in-differences estimates reveal that non˙FRL students are more likely to drop out of high school, however this could be driven by driven by higher income parents moving their children into private school or home schooling after 9th grade.

When interpreting these differences by race, it is important to highlight that districts were separately under pressure to limit SE enrollment for minority students if the rate of minority students

[49]Our sample includes 5th grade SE cohorts between 1999-00 and 2004-05 in our high impact sample.

in SE exceeded the rate of minority students in the district (referred to as "disproportionality") under the PBMAS. Districts facing both policy pressures would have more incentives to reduce SE enrollment among minority students, which could partly explain the larger impacts of SE removal among these groups.[50] Ballis and Heath (2019) show that limiting disproportionality has a separate effect on minority student outcomes compared to the effect of reducing overall access to SE programs. Interestingly, while reducing access to SE programs has a negative effect on later life outcomes, in Ballis and Heath (2019) we find that black students in districts with relatively higher rates of disproportionality experience small gains in long-run outcomes if removed from SE programs. We explore the mechanisms that drive these differences in Ballis and Heath (2019).

5.4 Robustness

We have thus far argued that the SE enrollment target was the only major policy change likely to impact SE students during this time, and have interpreted our results as the effect of losing access to SE services. We have already demonstrated that exposure to the SE enrollment target was uncorrelated with other changes in accommodations or services, likely ruling out the possibility that the other aspects of the PBMAS monitoring introduced other changes (beyond SE removal) for SE students. However, in this section we provide additional evidence to support this. First we re-estimate our results dropping districts under pressure to reduce the amount of time spent in separate classrooms or taking modified exams. Columns 2 and 3 of Appendix Table A.11 present these results which are nearly identical to our main estimates. This suggests that the small number of districts facing these additional pressures are not driving our results. Second, we rule out the possibility that districts facing additional pressures under the PBMAS were on differential trends by including trends interacted with the 2005 rating in each area of the PBMAS monitoring. Columns 4-6 of Appendix Table A.11 demonstrate that our results are robust to the inclusion of such trends.

As a final check, we re-estimate all of our results on a subset of students who were

[50] Importantly, controlling for the additional pressure to reduce disproportionality of minority groups leaves our overall and minority group estimates unchanged. We present results for all SE students in Column 8 of Appendix Table A.11, while results for minority students are available upon request.

receiving minimal accommodations at baseline (i.e. those taking unmodified exams and who spent minimal time in separate classrooms). Focusing on this sample ensures we are estimating the effect of the policy on students who would have been exclusively affected by the policy pressure to reduce SE enrollment (i.e. these were the students who were already receiving the level of services and accommodations that were deemed compliant under the PBMAS). Appendix Table A.12 presents these results. SE removal decreases the probability of graduating high school by 56.6 percentage points and enrolling in college by 31.8 percentage points. We no longer estimate statistically significant estimates for whether students enroll in college. However, this can likely be explained by the large reduction in sample size. The fact that the magnitudes for this subgroup who were already receiving minimal accommodations are similar to our main estimates presented in Table 4, suggests that other aspects of the policy are unlikely to be driving our results.

Next, we rule out the possibility that other educational policy changes are influencing our results. To our knowledge, the only other policy change around this time that could have influenced long-run trajectories was the introduction of No Child Left Behind (NCLB) in 2003. Since many features of NCLB mirrored those of the existing accountability system that had been in place in Texas since 1993, we do not expect that NCLB played a large role in Texas. Nonetheless, it did introduce one important change, namely that SE subgroups were held accountable as a separate group under accountability. Prenovitz (2017) demonstrates that in North Carolina NCLB's implementation led to incentives to alter the set of SE test-takers in order to improve the test performance of SE students. If low-performing SE students are losing SE in order to boost the SE subgroup's performance on standardized exams, we may be over-estimating the negative impact of SE removal for students on the margin. We present results that account for differences in pre-policy math test scores (measured in fourth grade) in Appendix Table A.13.[51] We find that the highest performing students were most likely to lose SE, ruling out this type of strategic placement.

As a final robustness check, we re-estimate all of our college enrollment estimates for

[51] We augment Equation 2 by including a term that interacts fourth grade standardized math test scores from fourth grade with treatment and 4th grade standardized test scores.

the subgroup of students for whom we have National Student Clearinghouse (NSC) data. These additional data allow us to address whether the lack of out of state college enrollment for our full sample is influencing our college enrollment estimates. For 5th grade cohorts from 2000-01 through 2004-05, we are able to follow out of state college enrollment up to two years after expected high school graduation. Panel A of Appendix Table A.14 presents results were we omit out of state college enrollment, and Panel B of Appendix Table A.14 includes out of state college enrollment within two years of expected high school graduation. We find minimal differences across panels. In our fully specified model in Column 5, we find nearly identical effects regardless of whether out of state enrollments are included.

6 Discussion and Interpretation

Our results suggest that removal from SE programs for marginal students significantly reduces educational attainment. This suggests that the potential costs of SE program participation (e.g. stigma effects or lowered expectations) are outweighed by the benefits. Ideally we would put our estimated effects in context by comparing them to other studies on SE effectiveness. Yet, as previously discussed, causal evidence on SE placement is sparse and primarily focuses on short-run outcomes. Thus, we can benchmark our results by comparing our estimates to the long-run impacts of other school-based programs that have reduced classroom size or changed resources. One caveat of this comparison, however, is that these other school-based programs affect all students, rather than only those with special needs. Other studies have found that reduced kindergarten classroom size increases college enrollment by 2.7 percent (Dynarski, Hyman, & Schanzenbach, 2013) and a 10 percent increase in school spending leads to 0.27 additional years of completed school (Jackson, Johnson, & Persico, 2015). We estimate that SE removal decreases college enrollment by 37 percentage points. While our effects are significantly larger, we are focused on a program that significantly alters a students' learning environment for a significantly longer time frame. Also, we are focused on students with disabilities who are a particularly vulnerable group.

Despite our focus on school-aged youth, it is also relevant to compare our estimates to the long-run impacts of early childhood programs. Similar to SE programs that target additional resources to students at risk of lower achievement, early education programs such as Head Start also target additional resources to vulnerable groups at younger ages. Head Start, an early childhood education program that provides additional services (i.e. educational, health, and nutrition related) to low-income children has been shown to have long-run positive impacts.[52] Garces et al. (2002) estimate that Head Start participation increases college enrollment by 9.2 percentage points. A rough back of the envelope calculation suggests returns to SE for marginal students that are nearly identical to the returns to early childhood programs such as Head Start.[53]

6.1 Mechanisms

Given the large estimated impact SE removal has on educational attainment decisions, we consider three potential mechanisms. As noted in Section 2.1, SE students may benefit from individualized instructional support (that could improve learning or behavioral outcomes), accommodations (that may make grade promotion or graduation requirements easier to meet), or transition planning services that hold students accountable for making progress towards adulthood goals. To the extent possible, we explore each of these potential mechanisms in turn.

First, we examine whether reductions in academic or behavioral outcomes are driving the long-run negative impacts of SE removal that we estimate. Unfortunately, due to the lack of available data on performance on modified exams, examining the effects of SE removal on student achievement is limited to the roughly 35 percent of SE students who take unmodified standardized exams. With this important caveat in mind, we estimate whether exposure to the SE enrollment target reduced affected students' engagement in school or standardized test performance. Four years

[52]Other preschool programs such as Abecedarian Project and Perry Preschool have also been shown to have long-run positive impacts. Campbell, Ramey, Pungello, Sparling, and Miller-Johnson (2002) estimate that the Abecedarian Project increased college enrollment by 22 percentage points, Schweinhart et al. (2005) estimate that Perry Preschool increased high school graduation rates by 50 percentage points for females with no effect on males.

[53]The average additional yearly cost to educate an SE student is $7,016.66 in Texas ($12,573.37 for SE students vs. $4,292.71 GE students). The estimated increase in high school graduation over the four years after 5th grade is 52 percentage points, yielding a per-graduate cost to educate an additional marginal SE student of $52,955.94 (=(100/53) *(7016.66*4)). Using the social cost of a high school drop-out of $256,000 estimated by Levin et. al (2007), suggests a benefit cost ratio of 4.8(=(256,000/52,955.94)). Based on a similar calculation, Deming (2009), who identifies the long-run impacts of Head Start participation, estimates a benefit cost ratio of 4(=256,000/65,116).

after 5th grade, we find little evidence that the enrollment target significantly impacted absences or grade repeating. We also find little evidence of an effect on standardized test performance on the high school exit exam (Panel A, Table 7). Overall, we only estimate one significant relationship at the 10 percent level, reading test scores increase by 0.04 of a standard deviation, on average. For the high impact sample, we do not estimate any relationship between policy exposure and standardized test performance, attendance, or grade repeating (Panel B, Table 7), suggesting that changes in behavior or achievement are unlikely to be driving our long-run findings.

Second, we explore whether the accommodations offered to SE students make high school graduation requirements easier to meet, and subsequent college enrollment more likely. Students enrolled in SE may be able to graduate without having to pass the high school exit exam, a necessary requirement for all other students. We find that the enrollment target significantly increased the likelihood that students took both the math and reading high school exit exam (Columns 5- 6, Table 7), and also increased the likelihood that students were unable to ever pass these exams (Columns 7-8, Table 7),[54] suggesting that SE loss would have made it harder to meet graduation requirements.

Finally, it is possible that SE participation raises long-run aspirations through transition planning. While we are unable to test this mechanism directly, we view it as plausible that holding students accountable for making progress towards adult goals could be a strong mechanism towards eventually meeting these goals.

6.2 Mitigating Factors

We next explore school-based factors that could plausibly mediate the impacts of SE removal. For instance, better resourced or higher-performing districts may have been able to mitigate the negative impact of SE removal on student outcomes. First, we explore differences by district wealth. Columns 2 and 3 of Table 7 show that while students in high wealth districts are more likely to experience SE removal (7 percentage points) relative to students in low wealth districts (4 percentage points), they

[54] In Columns 7-8 Table 7, our indicator equals 1 if the student took the high school exit exam and was not able to pass it on any attempt. While the indicator is equal to 0 if the student passed the high school exit exam on any attempt or never took the exam.

are less likely to experience negative long-run consequences associated with this SE removal.[55] Affected students attending low wealth districts are 2 percentage points less likely to graduate and enroll in college, while those attending wealthier districts there is no statistically significant impact on long-run outcomes. These differences suggest that either higher-wealth districts are able to help struggling students through better resources in general (e.g. more qualified teachers, better facilities), or are better able to target additional resources to struggling students.

Another way districts may have been able to accommodate students losing SE was through locally funded 504 plans (Samuels, 2018). 504 plans are an alternative way students with disabilities are provided accommodations in school.[56] In fact, after the enrollment target was implemented many districts expanded access to 504 plans in Texas, despite little change in 504 plan enrollment nationally. Appendix Figure A.8 demonstrates that while the fraction of students enrolled in SE rapidly declined after the enrollment target was introduced (2005-2010), there was a corresponding increase in the fraction of students with a 504 plan (2005-2010). Ideally, we would estimate whether the students who transitioned from SE programs to 504 plans were differentially impacted by SE loss. However, we do not have access to student level 504 plan data that would allow us to do this. Instead, we address the potential role that access to 504 plans had on longer-run outcomes by testing whether there were differential impacts across districts that experienced large growth in 504 plan enrollment, after the SE enrollment target was introduced. In Columns 4-5 of Table 7 we find that while all districts reduced access to SE, the negative impacts on long-run educational attainment were more negative for students in districts that had lower growth in 504 program enrollment.

Finally, we explore differences across different measures of average district performance to address whether being in a better performing district helps mitigate the negative long-run impact of SE removal. To explore this possibility, we compare across district level average test scores

[55] We classify high wealth districts as the top 12% of districts in terms of tax base wealth per-pupil during 2004-05. These are the districts that had to re-distribute their local tax revenues to poorer districts in 2004-05 as part of school finance equalization policy (Cullen, 2003).

[56] While students receiving 504 plans receive all of their instruction in general education classrooms, they receive additional accommodations intended to make the general education curriculum more accessible. Typical accommodations include preferential seating, extra time on tests, daily check-ins with teachers, verbal testing, or modified assignments (KidsHealth, 2016).

(Table 7, Columns 6-7) and also district value added (Table 7, Columns 8-9). Regardless of what measure of district performance we rely on, we find that while both high and low performing districts reduced SE enrollment, the negative impacts of SE removal on educational outcomes were concentrated among students in lower-performing districts.

7 Conclusion

In this paper, we present evidence on how access to SE programs affects long-run educational attainment. Specifically, we focus on how reduced students' access to SE programs during middle school and early high school ultimately affected their high school completion and post-secondary enrollment decisions. Our identification strategy is based on the implementation of an SE enrollment cap, which required school districts to have no more than 8.5 percent of their students enrolled in SE. This policy change lead to an immediate drop in SE enrollment, which varied across districts depending on their initial SE enrollment.

Our results strongly indicate that SE services prepare students with disabilities better for long-run success. We find that in the average school district (with initial SE enrollment of 13 percent), 5th grade SE cohorts experienced a 3.5 percentage point increase in the likelihood of losing SE four years after 5th grade, a 1.9 percentage point decrease in the likelihood of high school completion, and a 1.2 percentage point decrease in the likelihood of college enrollment. These outcomes are strong predictors of adult success. The magnitude of the estimates is larger among less-advantaged youth and among those attending school in districts with lower wealth and lower average achievement. Our results are robust to a number of specification checks, including student attrition from the sample and differences in trends across the types of districts that would have been closer to or further from compliance with the 8.5 percent threshold prior to implementation.

Having demonstrated that the imposition of the SE enrollment target impacted the likelihood of SE participation, we employ an IV approach that allows us to identify how *SE removal* impacts long-run educational outcomes. We use policy exposure as an instrument for SE removal and find that SE removal decreases the likelihood a student completes high school by 52.2 percentage points and decreases the likelihood of college enrollment by 37.8 percentage points. Again, we find that these results are driven by less-advantaged youth. Our results suggest there are large, meaningful, long-run returns to investing in SE services in the public K-12 school setting for students on the margin of placement, especially those from disadvantaged backgrounds.

While this paper shows robust evidence on the direct impact of SE placement on educational attainment decisions, the limited time after the policy does not yet allow us to fully follow students into the labor market. The large wage differential associated with one's decision to enroll in college suggests that reduced college enrollment is likely to have negative effects on later labor market outcomes, once these outcomes are able to fully realize. Understanding the longer-run labor market effects will be the focus of future research.

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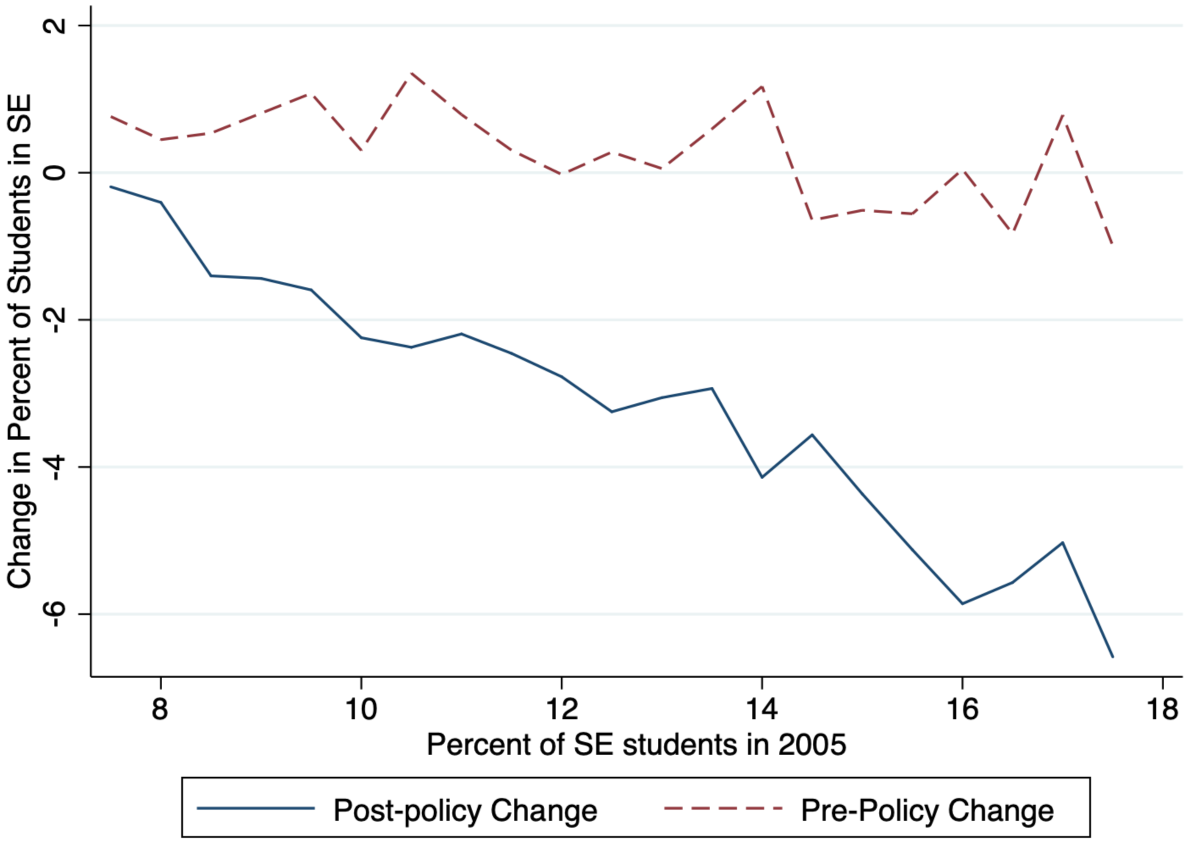
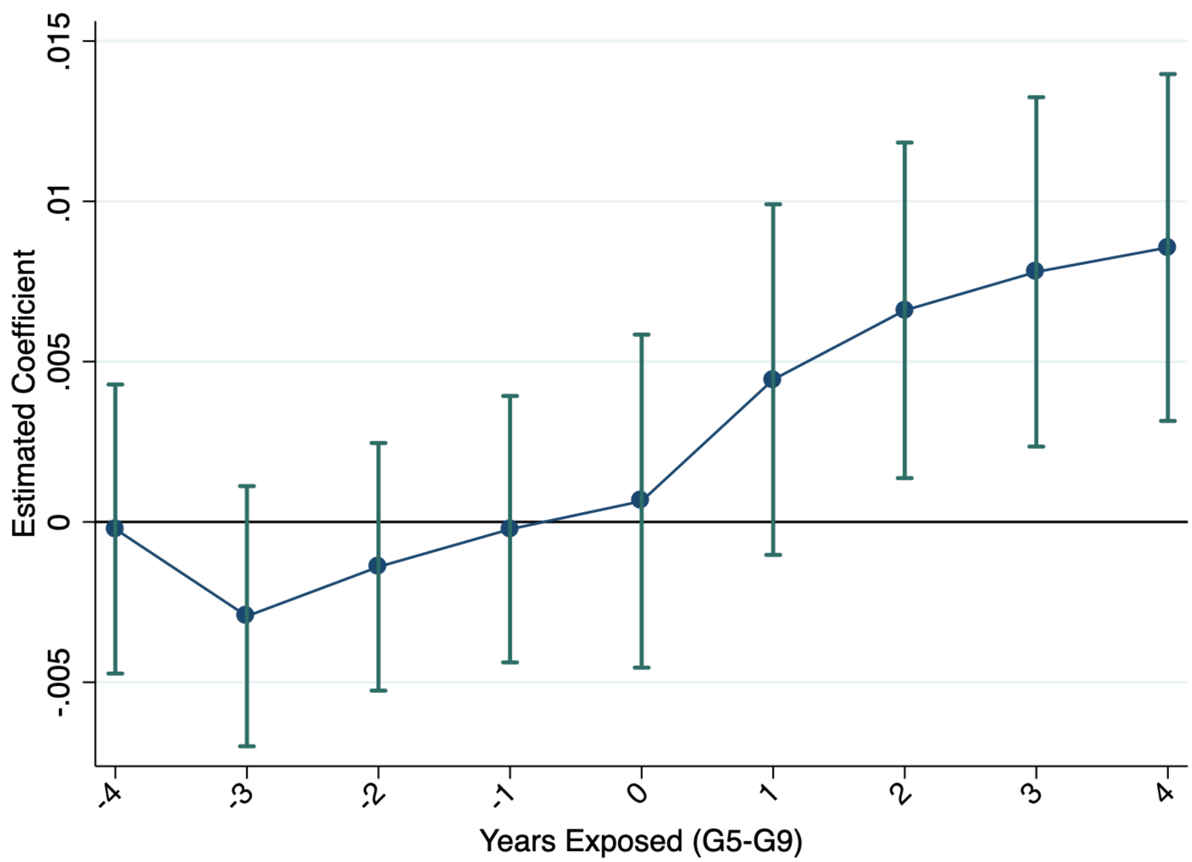
Figures/Tables

Figure 1: Change in District level SE enrollment during the pre-policy period (2000-2005) and the post-policy period (2005-2010)



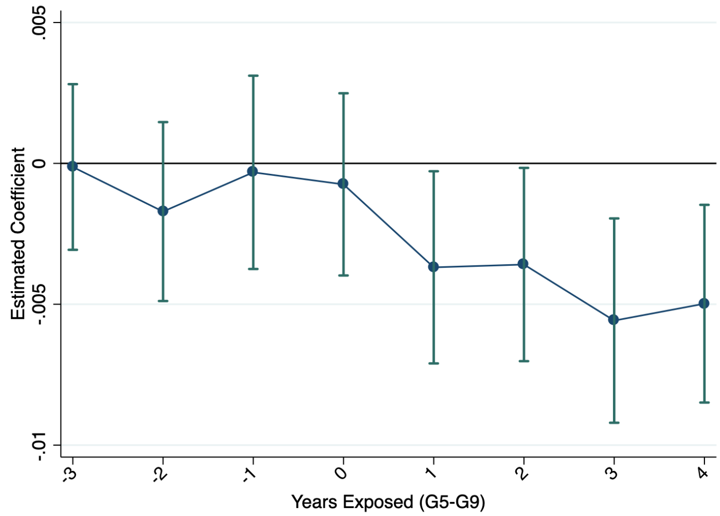
Note: This figure shows the change in SE enrollment between 2000-2005 and 2005-2010 by SE enrollment in 2004-05. The district level changes are weighted by 2004-05 district enrollment.

Figure 2: Event Study Estimates of the Impact of the Policy on SE Removal in Expected 9th Grade (High Impact Sample)

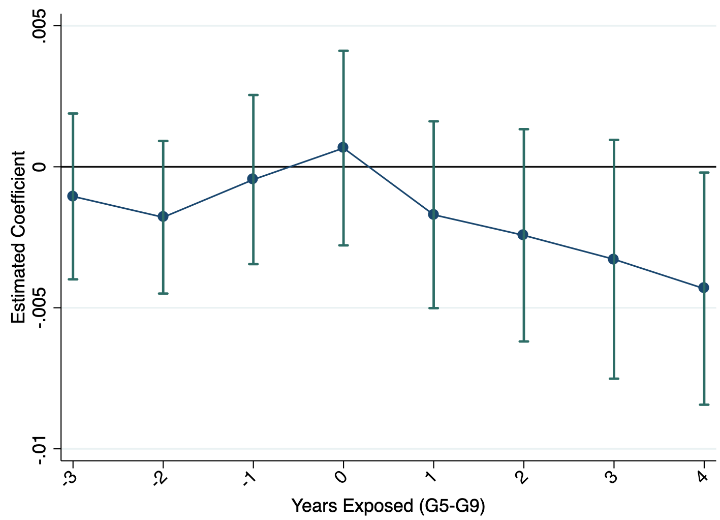


Note: This figure plots coefficients and 95% confidence intervals from an event-study regression that estimates interactions between 5th grade cohort dummies and the 2004-05 district SE rate. The outcome is an indicator for SE removal, measured in the year each student was expected to be enrolled in 9th grade (or 4 years after 5th grade). 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 SE between 1995-96 to 2004-05 in our high impact sample, including students with a malleable disability (including learning disabilities, speech impairments, other health impairments (includes ADHD), or emotional disturbance) who spent more than 50 percent of their instruction in general education classrooms at baseline (measured as of 5th grade). The 5th grade cohort from 1995-96 is omitted, so estimates are relative to that unexposed 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 SE sample and the full sample. Standard errors are clustered by district.

Figure 3: Event Study Estimates of the Impact of the Policy on Educational Attainment (High Impact Sample)



(a) High School Completion



(b) College Enrollment

Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimates interactions between 5th grade cohort dummies and the 2004-05 district SE rate. The dependent variable is shown in the sub-figure labels. College enrollment is measured within four years of each student's 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 SE between 1996-97 to 2004-05 who were in our high impact sample. The 5th grade cohort from 1996-97 is omitted, so estimates are relative to that unexposed cohort. See Figure 2 for more detail on the sample and the full set of controls. Standard errors are clustered by district.

Table 1: Summary Statistics - 5th Grade Cohorts Between 2000-2005

	GE	SE	SE Removal by G9 No	SE Removal by G9 Yes
	(1)	**(2)**	**(3)**	**(4)**
Hispanic	0.43	0.41	0.43	0.37
Black	0.14	0.19	0.20	0.16
White	0.40	0.39	0.36	0.45
FRL	0.53	0.64	0.69	0.54
ELL	0.12	0.16	0.18	0.11
Male	0.49	0.66	0.67	0.63
Std Math Score (G4)	0.07	-0.52	-0.87	-0.18
Std Reading Score (G4)	0.06	-0.53	-0.94	-0.20
Taking Reg Test Math (G4)	0.85	0.35	0.24	0.65
Taking Reg Test Reading (G4)	0.85	0.29	0.17	0.58
Long-Run Outcomes				
High School Completion	0.79	0.72	0.72	0.70
Attend College	0.56	0.33	0.27	0.48
College Completion	0.20	0.06	0.03	0.14
Employed	0.69	0.63	0.60	0.69
Annual Earnings ($)	14,073	10,324	9,399	12,766
Disability Type				
Learning Disability	-	0.60	0.65	0.48
Speech Impairment	-	0.14	0.04	0.39
Other Health Impairment	-	0.10	0.12	0.07
Emotional Disturbance	-	0.07	0.07	0.05
Intellectual Disability	-	0.05	0.06	0.00
Autism	-	0.02	0.02	0.00
Orthopedic Impairment	-	0.01	0.01	0.00
Auditory Impairment	-	0.01	0.01	0.00
Visual Impairment	-	0.00	0.01	0.00
Deafness and Blindness	-	0.00	0.00	0.00
Malleable	-	0.91	0.88	0.98
Less Malleable	-	0.09	0.12	0.02
Classroom Setting				
Mainstream	-	0.24	0.13	0.53
Separate Classroom ($\leq$50%)	-	0.64	0.71	0.45
Separate Classroom (>50%)	-	0.13	0.17	0.03
Total Students	1,448,003	227,555	165,043	62,512

Note: This table presents summary statistics for general education and SE students in 5th grade cohorts between 1999-00 to 2004-05. Among SE students, we also present summary statistics separately for those who continue or lose SE by their expected 9th grade (or 4 years after 5th grade). Malleable disabilities include learning disability, emotional disturbance, other health impairments, and speech impairments. Whether students attend college is measured within the four years of expected high school graduation. College Completion, Employment, and Earnings are measured six years after expected high school graduation. Earnings reported are not conditional on being employed. Those not employed are assigned 0 yearly earnings.

Table 2: The Impact of the Policy on SE Removal in Expected 9th Grade

	(1)	(2)	(3)	(4)	(5)
Panel A: Full Sample					
Treatment	0.0102***	0.00955***	0.00816***	0.00788***	0.00778***
	(0.00206)	(0.00210)	(0.00214)	(0.00188)	(0.00188)
	[0.048]	[0.043]	[0.037]	[0.035]	[0.035]
Mean (Y)	0.275	0.275	0.275	0.275	0.275
N	227,555	227,555	227,555	227,555	227,555
Panel B: High Impact Sample					
Treatment	0.0108***	0.0100***	0.00961***	0.00931***	0.00921***
	(0.00277)	(0.00279)	(0.00235)	(0.00213)	(0.00213)
	[0.049]	[0.045]	[0.043]	[0.042]	[0.042]
Mean (Y)	0.317	0.317	0.317	0.317	0.317
N	189,042	189,042	189,042	189,042	189,042
Controls					
Cohort FE	X	X	X	X	X
District FE	X	X	X	X	X
Individual Demo		X	X	X	X
Individual Disability			X	X	X
District-Cohort Demo				X	X
District Finance					X

Note: This table shows difference-in-differences estimates of the impact of the policy on SE removal. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 2. The outcome variable is an indicator for whether a student lost SE services the year they were expected to be enrolled in 9th grade (or four years after 5th grade). The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05. Panel A includes estimates for the full sample. Panel B includes estimates for our high impact sample that include students with a malleable disability (including learning disabilities, speech impairments, other health impairments (includes ADHD), or emotional disturbance) who received more than 50 percent of their instruction in general education classrooms at baseline (both measured as of 5th grade). Individual controls include 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, and baseline level of classroom inclusion (all measured in 5th grade). District demographic cohort controls include the percentage of students belonging to each racial group, receiving FRL, classified as ELL, and who are male for the SE sample and the full sample measured at baseline. District financial controls include tax base wealth per-pupil and the percent of tax base wealth that is residential. The effect for the fully exposed student at the average district are shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered by district. *p<0.10, ** p<0.05, *** p<0.01.

Table 3: The Impact of the Policy on Educational Attainment

	(1)	(2)	(3)	(4)	(5)
High School Completion					
Panel A: Full Sample					
Treatment	-0.00409***	-0.00455***	-0.00427***	-0.00428***	-0.00421***
	(0.00142)	(0.00141)	(0.00147)	(0.00141)	(0.00142)
	[-0.018]	[-0.020]	[-0.019]	[-0.019]	[-0.019]
Mean (Y)	0.718	0.718	0.718	0.718	0.718
N	227,555	227,555	227,555	227,555	227,555
Panel B: High Impact Sample					
Treatment	-0.00466***	-0.00515***	-0.00523***	-0.00510***	-0.00497***
	(0.00156)	(0.00156)	(0.00161)	(0.00156)	(0.00157)
	[-0.021]	[-0.023]	[-0.024]	[-0.023]	[-0.022]
Mean (Y)	0.710	0.710	0.710	0.710	0.710
N	189,042	189,042	189,042	189,042	189,042
College Enrollment					
Panel C: Full Sample					
Treatment	-0.00118	-0.00214*	-0.00223	-0.00266**	-0.00264**
	(0.00131)	(0.00129)	(0.00136)	(0.00134)	(0.00134)
	[-0.005]	[-0.010]	[-0.010]	[-0.012]	[-0.012]
Mean (Y)	0.327	0.327	0.327	0.327	0.327
N	227,555	227,555	227,555	227,555	227,555
Panel D: High Impact Sample					
Treatment	-0.00281*	-0.00377***	-0.00340**	-0.00376**	-0.00372**
	(0.00143)	(0.00140)	(0.00148)	(0.00149)	(0.00149)
	[-0.013]	[-0.017]	[-0.015]	[-0.017]	[-0.017]
Mean (Y)	0.354	0.354	0.354	0.354	0.354
N	189,042	189,042	189,042	189,042	189,042
Controls					
Cohort & District FE	X	X	X	X	X
Individual Demo		X	X	X	X
Individual Disability			X	X	X
District-Cohort Demo				X	X
District Finance					X

Note: This table shows difference-in-differences estimates of the impact of the policy on educational attainment decisions. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 2. The dependent variable is shown in bold. College enrollment is measured within four years of each student's expected high school graduation. Panels A and C include estimates for the full sample. Panels B and D include estimates for the high impact sample. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05. See Table 2 for more detail on the sample and the full set of controls. The effect for the fully exposed student at the average district are shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered by district. *p<0.10, ** p<0.05, *** p<0.01.

Table 4: OLS and IV Estimates of the Impact of SE Removal on Educational Attainment (High Impact Sample)

	FS (1)	RF (2)	OLS (3)	IV (4)
Dependent Variable:	*SE Removal - G9 (Expected)*	*High School Completion*		
Treatment	0.00921***	-0.00497***		
	(0.00157)	(0.00213)		
	[0.0414]	[-0.022]		
Mean (Y)	0.317	0.710		
SE Removal			-0.0800***	-0.522***
			(0.00213)	(0.184)
Dependent Variable:	*SE Removal - G9 (Expected)*	*College Enrollment*		
Treatment	0.00921***	-0.00372**		
	(0.00157)	(0.00149)		
	[0.0414]	[-0.017]		
Mean (Y)	0.317	0.354		
SE Removal			0.0717***	-0.378**
			(0.00361)	(0.187)
Kleibergen-Paap F-Statisitic	17.02			

Notes: This table reports difference-in-differences estimates of the impact of the policy on SE removal (by expected 9th grade) and educational attainment (Columns 1 -2). This table also reports OLS and IV estimates of SE removal on educational attainment outcomes (Columns 3-4). The dependent variable is shown in the panel headings. College enrollment is measured within four years of each student's expected high school graduation. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05 in our high impact sample (N=189,042). See Table 2 for more detail on the sample and for the full list of controls used. The effect for the fully exposed student at the average district are shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered by district. *p< 0.10, **p < 0.05, *** p< 0.01.

Table 5: Heterogeneity by Race and FRL Status (High Impact Sample)

	FRL	Non-FRL	Minority	White
	(1)	(2)	(3)	(4)
Panel A: SE Removal in G9 (Expected)				
Treatment	0.0114***	0.0053	0.0108***	0.0063**
	(0.0023)	(0.0030)	(0.0028)	(0.0024)
P-value		0.026		0.256
Mean (Y)	0.265	0.409	0.281	0.367
Panel B: High School Completion				
Difference-in-Differences Estimates				
Treatment	-0.0056**	-0.0050**	-0.0063**	-0.0030
	(0.0021)	(0.0019)	(0.0023)	(0.0018)
P-value		0.820		0.172
IV Estimates				
SE Removal	-0.49531**	-0.9568	- 0.57848*	-0.4083
	(0.1867)	(0.6065)	(0.2436)	(0.3179)
P-value		0.506		0.000
Mean (Y)	0.653	0.811	0.678	0.756
Panel C: College Enrollment				
Difference-in-Differences Estimates				
Treatment	-0.00566***	-0.0006	-0.00742***	0.0007
	(0.002)	(0.003)	(0.002)	(0.002)
P-value		0.060		0.000
IV Estimates				
SE Removal	-0.49879**	-0.1074	-0.68757*	0.1101
	(0.1777)	(0.5426)	(0.2809)	(0.3056)
P-value		0.000		0.000
Mean (Y)	0.268	0.506	0.313	0.410
N	120,565	68,429	112,462	73,959
KP F-statistic	23.7527	3.0476	14.8290	6.8305

Notes: Panel A reports difference-in-differences estimates of the impact of the policy on SE removal. Panel B reports difference-in-differences estimates of the impact of the policy on high school completion and IV estimates of SE removal on high school completion. Panel C reports difference-in-differences estimates of the impact of the policy on college enrollment and IV estimates of SE removal on college enrollment. College enrollment is measured within four years of each student's expected high school graduation. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05 in our high impact sample. See Table 2 for more detail on the sample and for the full list of controls used. The p-value row presents the p-value associated with the test of equality across the two coefficients (either Columns 1 vs. 2 or Columns 3 vs. 4). Standard errors in parentheses are clustered by district. *$p < 0.10$, **$p < 0.05$, *** $p < 0.01$.

Table 6: The Impact of the Policy on Intermediate Outcomes

	Share	Repeated	Std Score		Took HS Exit		Unable to Pass HS Exit	
	Absent	Grade 9	Math	Reading	Math	Reading	Math	Reading
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Full Sample								
Treatment	0.0001	0.0004	0.0014	0.0098*	0.0059***	0.0048**	0.0038***	0.0013**
	(0.0002)	(0.0005)	(0.0049)	(0.0052)	(0.0020)	(0.0020)	(0.0010)	(0.0006)
	[0.00]	[0.00]	[0.01]	[0.04]	[0.03]	[0.02]	[0.02]	[0.01]
Mean (Y)	0.069	0.026	-0.444	-0.545	0.314	0.324	0.100	0.044
N	227,555	227,555	99,029	102,346	227,555	227,555	227,555	227,555
Panel B: High Impact Sample								
Treatment	8.62e-05	-6.78e-05	-0.000929	0.00713	0.00628***	0.00525**	0.00454***	0.00164**
	(0.0003)	(0.0004)	(0.0048)	(0.0052)	(0.0022)	(0.0022)	(0.0012)	(0.0007)
	.	.	[-0.00]	[0.03]	[0.03]	[0.02]	[0.02]	[0.01]
Mean (Y)	0.069	0.020	-0.437	-0.530	0.355	0.366	0.111	0.048
N	189,042	189,042	92,753	95,430	189,042	189,042	189,042	189,042

Note: This table shows difference-in-differences estimates of the impact of the policy on intermediate outcomes. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 2. The dependent variable is shown in the column headings. The outcomes in columns 1 through 4 are measured in expected 9th grade. The outcomes in Columns 6 through 8 are are measured in expected 10th grade. Panel A estimates for the full sample. Panel B estimates from our high impact sample. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05. See Table 2 for more detail on the sample and the full set of controls. The effect for the fully exposed student at the average district are shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered by district. *$p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 7: Heterogeneity by School-Based Factors (High Impact Sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		District Wealth		504 Plan Growth		Average Test Scores		District Value Added	
	Baseline	High	Low	High	Low	High	Low	High	Low
Panel A: SE Removal in G9 (Expected)									
Treatment	0.00836***	0.0165**	0.00829***	0.0156***	0.00676***	0.00601**	0.0107***	0.0111***	0.00725**
	(0.00214)	(0.00632)	(0.00225)	(0.00460)	(0.00254)	(0.00264)	(0.00282)	(0.00310)	(0.00327)
	[0.04]	[0.07]	[0.04]	[0.07]	[0.03]	[0.03]	[0.05]	[0.05]	[0.03]
Mean (Y)	0.317	0.369	0.315	0.284	0.326	0.363	0.289	0.332	0.308
Panel B: High School Completion									
Treatment	-0.00509***	-0.00580	-0.00517***	-0.00147	-0.00569***	-0.00358	-0.00585***	-0.00366	-0.00591***
	(0.00157)	(0.00612)	(0.00163)	(0.00296)	(0.00183)	(0.00219)	(0.00205)	(0.00240)	(0.00215)
	[-0.02]	[-0.03]	[-0.02]	[-0.01]	[-0.03]	[-0.02]	[-0.03]	[-0.02]	[-0.03]
Mean (Y)	0.710	0.793	0.706	0.712	0.710	0.756	0.682	0.746	0.687
Panel C: College Enrollment									
Treatment	-0.00444***	-0.00407	-0.00459***	-0.00388	-0.00436**	-0.00201	-0.00629***	-0.00175	-0.00605***
	(0.00157)	(0.00366)	(0.00160)	(0.00290)	(0.00192)	(0.00291)	(0.00173)	(0.00200)	(0.00227)
	[-0.02]	[-0.02]	[-0.02]	[-0.02]	[-0.02]	[-0.01]	[-0.03]	[-0.01]	[-0.03]
Mean (Y)	0.354	0.452	0.349	0.330	0.360	0.419	0.314	0.390	0.331
N	189,042	9,810	179,232	37,561	151,481	72,317	116,725	73,598	115,444

Note: This table shows difference-in-differences estimates of the impact of the policy on SE removal and educational attainment decisions for different type of districts. High wealth districts had tax base wealth per-pupil greater than $323 (in thousands of dollars) and made up the top 12% of school districts in terms of tax-base wealth. We construct value-added as follows. We regress average standardized test scores on lagged test scores, indicators for a student's race, gender, SE status, Limited English Proficiency status, and Free and Reduced Price Lunch status. We split districts according to the median of each school-based measure, where those above the median are labelled "High" and those below are labelled "Low". Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 2. The dependent variable is shown in the panel headings. This table includes estimates from our high impact sample. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05. See Table 2 for more detail on the sample and the full set of controls. The effect for the fully exposed student at the average district are shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered by district. *p<0.10, ** p<0.05, *** p<0.01.

A Appendix

Figure A.1: Transition Plan College Bound Student – Page 1

Sample IEP Transition Plan for College-Bound Students

Name: Noah Lee Grade: 11

Projected Graduation Date: June 2018

Student's Strengths, Preferences and Interests

In transition planning, it's important to connect kids' strengths and interests to what they want to do after high school. IEP teams use interviews and career assessments to help gather this information.

Noah demonstrates he is a hardworking student. He reported in an interview that he enjoys spending time with family and going to the gym. Based on transition questionnaires, Noah wants to go to college and is interested in a career working with young kids. However, he isn't sure if he wants to be a classroom teacher.

His most recent evaluation and present level of performance in his current IEP indicate that Noah has a specific learning disability in reading comprehension. He also struggles with time management when completing schoolwork, but he is meeting most of his IEP goals. Noah has acknowledged that his difficulties with organization may be a barrier to his goals after high school.

Measurable Postsecondary Goals

Transition goals must target what kids will do after high school. It's helpful if the goals in your child's transition plan match up with IEP goals. Keep in mind that the goals may change over time as you and your child plan for the future.

Postsecondary Education / Vocational Training: Noah will attend a local four-year college after graduation. He will take courses leading to a major in early childhood education.

Jobs and Employment: The summer after graduation, Noah will work part-time at a local child-care center.

Independent Living (if needed): Noah already has these skills.

Figure A.2: Transition Plan College Bound Student – Page 2

Sample IEP Transition Plan for College-Bound Students

Supporting IEP Goals and Services

The IEP team can put in place IEP goals and transition services to support your child's transition goals. It's important that the plan list people and resources that can help. These include colleges, employment agencies and other transition specialists.

Supporting IEP Goal	Transition Activities / Services	Person / Agency Involved
By December 2017, Noah will fully complete two college applications with 100 percent accuracy.	Prepare a list of what Noah wants in a college. Research colleges and identify three he'd like to apply to that offer training and degree programs in early childhood education.	Noah, his parents, high school counselor
	Noah will obtain applications from each college and will plan a tour of at least one college of his choice.	Noah, his parents, college admissions offices
	Receive proofreading support to help check for errors in the applications.	Noah, transition specialist, local child-care centers
By May 2018, Noah will complete a college-skills course offered at a local college or nonprofit organization.	Look into possible programs that will help build organizational skills and prepare him for the demands of college. Apply to program.	Noah, his parents, school transition specialist (school staff member who helps students transition to life after high school)
By May 2018, Noah will identify three careers that involve working with young children.	Noah will find and interview three people in the community who work with young children.	Noah, transition specialist, local community members, possibly a private coach
By June 2018, Noah will apply to volunteer at a local child-care center.	Identify three local child-care centers. Ask about volunteering and complete an interview for each.	Noah, transition specialist, local child-care centers

Figure A.3: Performance Level Assignment for the SE Representation Rate Indicator

PERFORMANCE LEVEL ASSIGNMENT

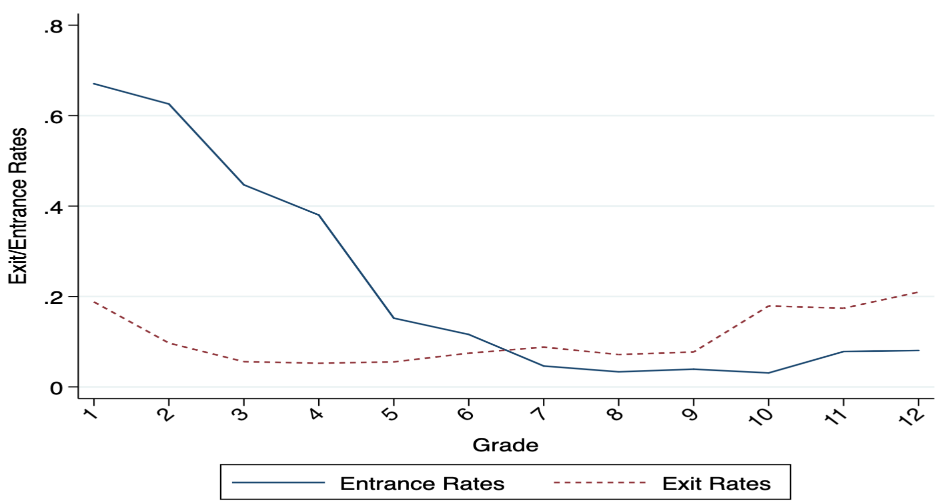
The district-level special education representation rate is compared to the PBMAS standards for the indicator, and performance levels are assigned as follows:

SPED #12: District Special Education Representation Rate				
Performance Level (PL) Assignments				
Performance Level = Not Assigned	**Performance Level = 0 (met standard) (Also includes 0RI)**	**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.

The PBMAS special analysis process is not applicable to this indicator. Performance levels are only assigned through standard analysis.

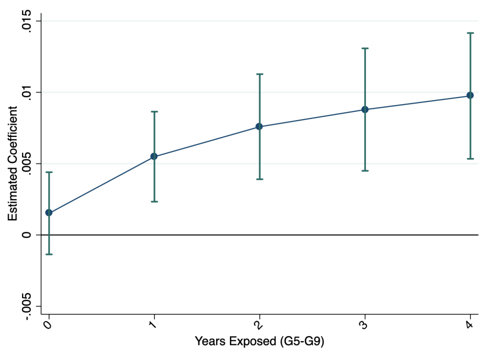
Note: State Interventions increased in severity with distance from 8.5, ranging from designing and implementing an improvement plan with school districts to state audits. Source: PBMAS 2004-05

Figure A.4: Entrance and Exit Rates for students with Malleable Disabilities

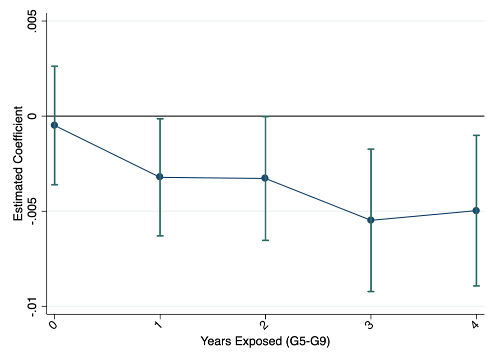


Note: The figure plots entrance rates (solid line) and exit rates (dashed line) in each grade for students with malleable disabilities. Entrance rates represent the number of new students enrolled in SE, divided by the total number of students enrolled in the current grade. Exit rates represent the number of students who lost SE, divided by the total number of students enrolled in SE during the grade prior. The sample includes cohorts who were completely unexposed to the policy.

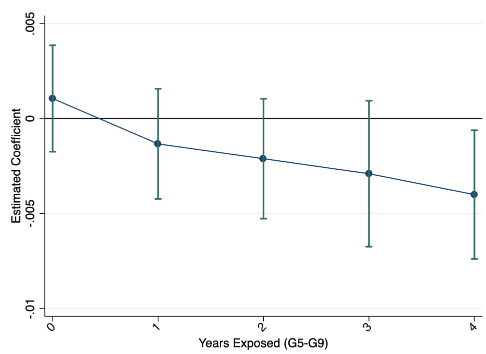
Figure A.5: Event Study Estimates of the Impact of the Policy on Educational Attainment (High Impact Sample)



(a) SE Removal in G9 (Expected)



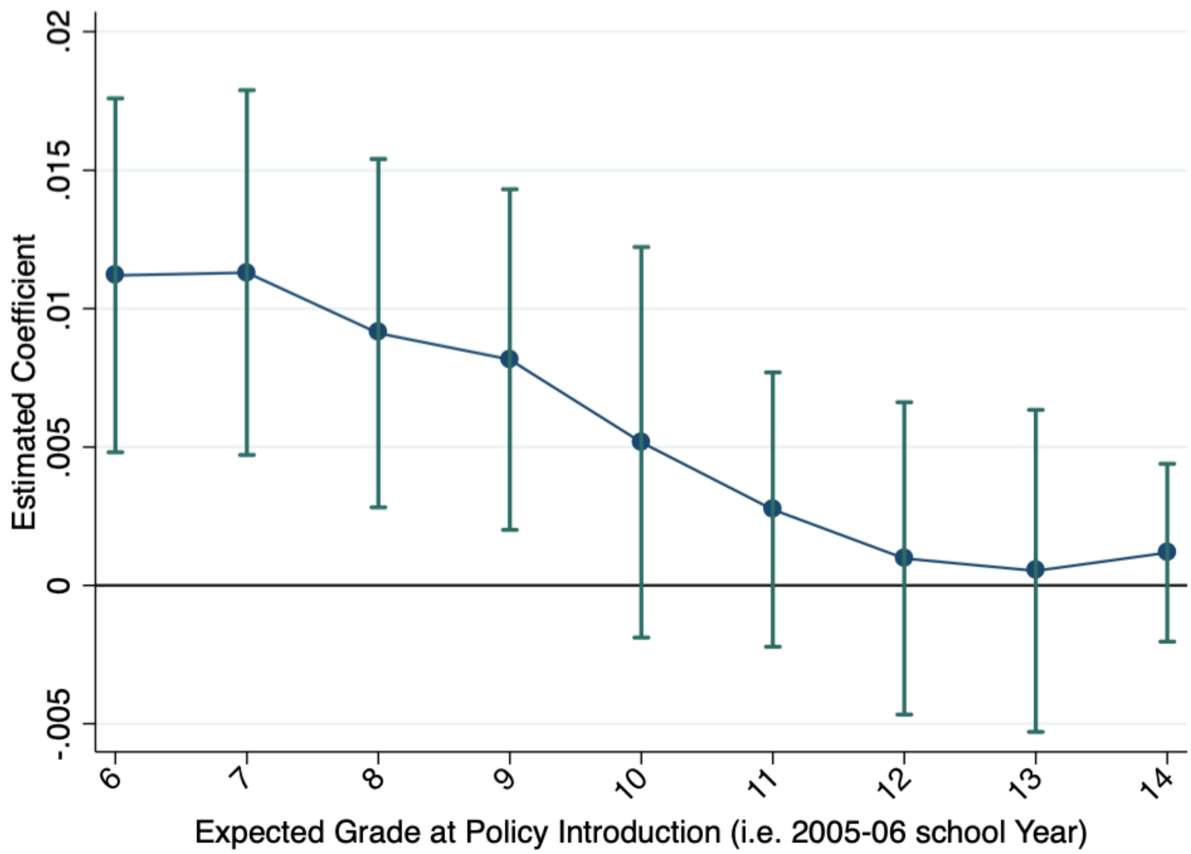
(b) High School Graduation



(c) College Enrolled

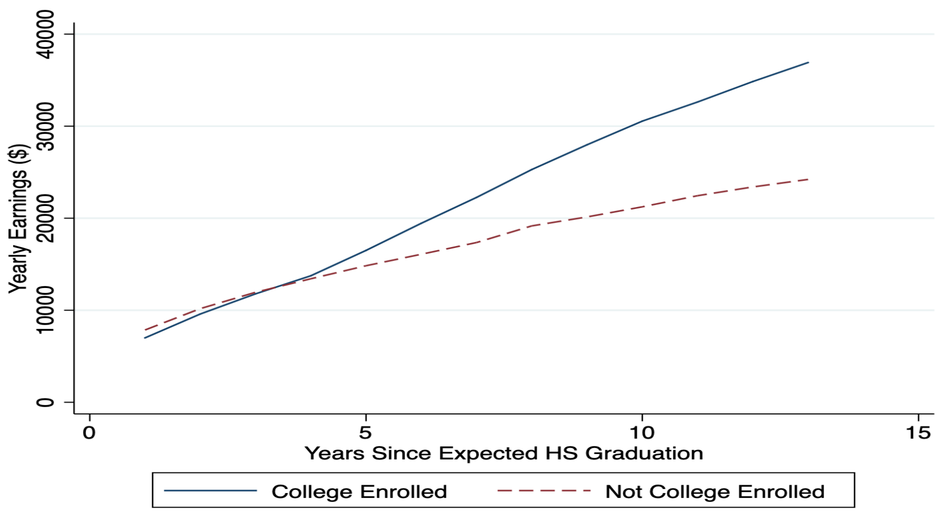
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimates interactions between 5th grade cohort dummies and the pre-policy district SE rate. The dependent variable is shown in the sub-figure labels. College enrollment is measured within four years of each student's 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 SE between 1999-00 to 2004-05 who were in our high impact sample. The 5th Grade cohort from 1999-00 is omitted, so estimates are relative to that cohort. See Figure 2 for more detail on the sample and the full set of controls. Standard errors are clustered by district.

Figure A.6: Event Study Estimates of the Impact of the Policy on SE Removal Any Time after 5th Grade (High Impact Sample)



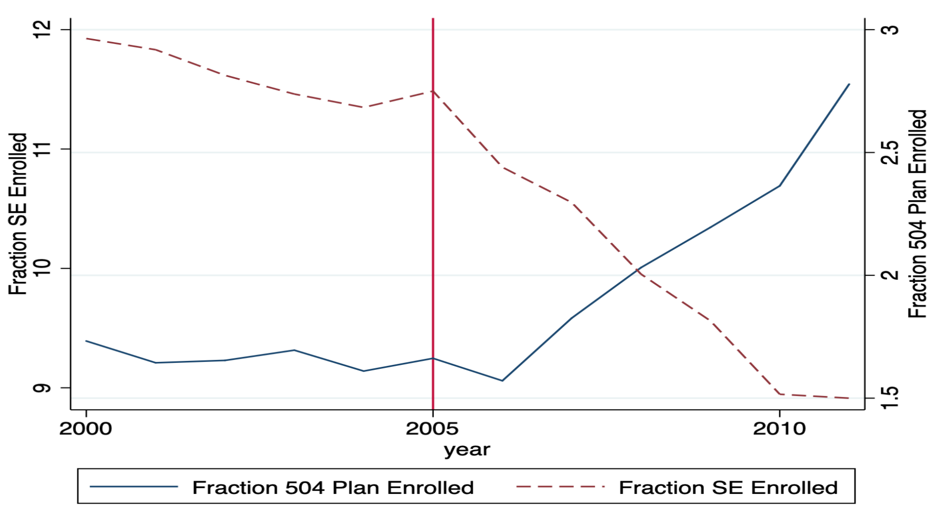
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimates interactions between 5th grade cohort dummies and the pre-policy district SE rate. The dependent variable is an indicator variable for whether a student ever lost SE services after 5th grade. Event time is defined as 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 SE between 1995-96 to 2004-05 who were in our high impact sample. The 5th Grade cohort from 1995-96 is omitted, so estimates are relative to that unexposedcohort. See Figure 2 for more detail on the sample and the full set of controls. Standard errors are clustered by district.

Figure A.7: Earnings Trajectories by College Enrollment Choice



Note: This figure plots the earnings trajectories 13 years after expected high school graduation based on an individuals' decision to enroll in college within four years of each student's expected high school graduation. The sample includes students who were in the 1999-00 5th grade SE cohort.

Figure A.8: State Level Participation in 504 Plans and SE Programs



Note: Data for this figure come from the IES. Averages represent district level population averages, that is, the number of students in special education (or 504 plans), divided by the total number of students in that district and year.

Table A.1: Policy Pressure due to PBMAS (2004-05 School Year)

Panel A: Fraction of Districts meeting standards in each PBMAS Monitoring Category							
						Reduce Services	
PBMAS	Reduce SE Enrollment			Improve Outcomes		Separate	Modified
Performance Level	Overall	Black	Hispanic	Behavioral	Academic	Instruction	Test-Taking
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
0 = Met	0.05	0.53	0.67	0.72	0.97	0.39	0.24
1 = Nearly Met	0.19	0.08	0.07	0.27	0.03	0.38	0.65
2 = Not Met	0.53	0.13	0.10	0.02	0.00	0.19	0.11
3 = Worst Rating	0.24	0.19	0.12	0.00	0.00	0.03	0.00

Panel B: Correlation b/w Pressure to Reduce Overall SE Enrollment and other PBMAS Pressures						
					Reduce Services	
	Reduce SE Enrollment		Improve Outcomes		Separate	Modified
	Black	Hispanic	Behavioral	Academic	Instruction	Test-Taking
	(2)	(3)	(4)	(5)	(6)	(7)
Correlation Coefficient	0.0122	0.0428	-0.0954*	0.0025	0.0604	0.0976*

Note: This table shows the policy pressure to make changes for SE due to the introduction of PBMAS Monitoring in 2005. Panel A shows the fraction of districts that met (0), nearly met standards (1), did not meet (2), or had the worst rating (3) under each area of SE monitoring during 2005. Panel B shows the raw correlation coefficient between the policy pressure to reduce overall SE enrollment (based on the distance above the 8.5 SE enrollment target get in 2005) and the average rating in each of the other PBMAS monitoring areas (measured in 2005). *$p< 0.10$, **$p < 0.05$, *** $p< 0.01$

Table A.2: Cross-Cohort Variation in Policy Exposure (5th Grade SE Cohorts)

Grade 5 Cohort	Policy Exposure by Year-Grade				Policy Exposure Before Expected
	6	7	8	9	9th Grade (FracExposed$_c$)
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 to 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. FracExposed$_c$ in Equation 2).

Table A.3: The Impact of the Policy on Attrition, Predicted Long-Run Outcomes, and Exogenous Student Characteristics

			Predicted					
	Enrollment (By G9)	District Switch (By G9)	SE Removal (By G9)	High School Completion	College Enrollment	Hispanic	White	FRL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Full Sample								
Treatment	-0.0003	0.0002	0.0030**	3.00e-05	0.0012**	-0.0016	0.0020	-0.0015
	(0.0007)	(0.0010)	(0.0012)	(0.0003)	(0.0005)	(0.0011)	(0.0012)	(0.0014)
	[-.001]	[.001]	[0.01]	[0.00]	[0.01]	[-0.01]	[0.01]	[-0.01]
Mean	0.902	0.235	0.275	0.718	0.332	0.412	0.386	0.644
N	252,315	227,555	227,555	227,555	227,555	227,555	227,555	227,555
Panel B: High Impact Sample								
Treatment	-0.0007	0.0006	0.0024	0.0004*	0.0009	0.0004	-0.0004	0.0004
	(0.0008)	(0.0011)	(0.0015)	(0.0002)	(0.0006)	(0.0003)	(0.0003)	(0.0003)
	[-.003]	[.003]	[0.01]	[0.00]	[0.00]	[0.00]	[-0.00]	[0.00]
Mean	0.905	0.234	0.317	0.710	0.359	0.421	0.391	0.638
N	208,944	189,042	189,042	189,042	189,042	189,042	189,042	189,042
Controls								
FE's	X	X	X	X	X	X	X	X
Cohort Demo	X	X	X	X	X	X	X	X
Addt'l Controls	X	X						

Note: This table shows difference-in-differences estimates of the impact of the policy on attrition (by expected 9th grade), predicted outcomes, and student demographic characteristics. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 2. The dependent variable is shown in the column headings. To obtain predicted values we generate fitted values from a regression of outcomes on the full set of controls (excluding treatment). Panel A include estimates for the full sample and Panel B include estimates from our high impact sample. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05. See Table 2 for more detail on the sample and the full set of controls. The effect for the fully exposed student at the average district are shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered by district. *p<0.10, ** p<0.05, *** p<0.01.

Table A.4: The Effect of the Enrollment Target on SE Placement and Education Attainment –Accounting for Differences in District Level Demographics (High Impact Sample)

	(1)	(2)	(3)	(4)
Panel A: Likelihood of Losing SE, 4 Years after G5				
Treatment	0.00836***	0.00775***	0.00799***	0.00818***
	(0.00214)	(0.00239)	(0.00266)	(0.00214)
	[0.038]	[0.035]	[0.036]	[0.037]
Mean (Y)	0.317	0.317	. 0.317	0.317
Panel B: Likelihood of Graduating from High School				
Treatment	-0.00509***	-0.00373**	-0.00442***	-0.00465***
	(0.00157)	(0.00155)	(0.00170)	(0.00145)
	[-0.023]	[-0.017]	[-0.020]	[-0.021]
Mean (Y)	0.710	0.710	0.710	0.710
Panel C: Likelihood of Enrolling in College				
Treatment	-0.00444***	-0.00401**	-0.00455**	-0.00412***
	(0.00157)	(0.00167)	(0.00190)	(0.00147)
	[-0.020]	[-0.018]	[-0.020]	[-0.019]
Mean (Y)	0.354	0.354	0.354	0.354
Controls				
Full Set	X	X	X	X
$f(t)\times$ Fraction Hispanic		X		
$f(t)\times$ Cohort Size			X	
$f(t)\times$ Fraction FRL				X

Note: This table shows difference-in-differences estimates of the impact of the policy on the likelihood of SE removal and educational attainment decisions. Within each panel, Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 2. The dependent variable is shown in the panel headings. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05 in our high impact sample. See Table 2 for more detail on the sample and the full set of controls. We also include linear time trends that vary by the fraction of the district that was Hispanic, and FRL in 2004-05, as well as a linear trend for the total cohort size in 2004-05. The effect for the fully exposed student at the average district are shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered by district. *p<0.10, ** p<0.05, *** p<0.01.

Table A.5: Impact of the Policy on District Resources: 2000-2010

District Level Outcome	N	Mean	Estimated Effect
Panel A: Denominator = All students in District			
Total Spending Per pupil	1,077	7,779	-20.60
			(50.30)
GE Spending Per Pupil	1,077	4,650	-16.67
			(27.57)
Instructional Spending Per Pupil	1,077	5,990	-23.72
			(38.96)
Administrative Spending Per Pupil	1,077	1,539	3.514
			(9.591)
Health Spending Per Pupil	1,070	371.0	0.105
			(3.491)
Student/Teacher (All)	1,198	14.73	-0.0353
			(0.0328)
Panel B: Denominator =SE Enrolled Students			
SE Spending Per SE-Pupil	1,076	9,371	66.98
			(65.50)
Student/Teacher (SE Only)	1,104	11.62	-0.262
			(0.265)

Note: This table shows difference-in-differences estimates of the impact of the policy on district level spending and resources. The dependent variable is shown in the first column. Panel A uses all students in a district to compute each measure. Panel B includes only students enrolled in SE programs to compute the measure. Each column reports estimates from district level regressions that regress district level exposure (i.e. ($\text{SERate}_d^{PRE} \times \text{FracExposed}_t$), where FracExposed_t is a continuous measure of policy exposure) on each of the dependent variables. Controls include year indicators, district fixed effects, and district by year demographic controls. Demographic district level controls include the percentage of students belonging to each racial group, receiving FRL, classified as FRL and who are male, as well as the total number of students enrolled in the district. Data from the years 1999-00 - 2009-10 are used in these regressions. The sample includes districts that served between 6.6 and 21.5 percent of their students in SE in 2005.

Table A.6: The Impact of Policy on Types of Services/Accommodations

	SE Removal	Regular Classroom ($\geq$ 79 % day)	Unmodified Exam Math	Unmodified Exam Reading	Ever Disciplined
	(1)	(2)	(3)	(4)	(5)
Panel A: Full Sample					
Treatment	0.00688***	0.00455	0.00685**	0.00484	-0.00175
	(0.00189)	(0.00316)	(0.00313)	(0.00323)	(0.00154)
	[0.03]	[0.02]	[0.03]	[0.02]	[-0.01]
Mean (Y)	0.725	0.670	0.435	0.450	0.405
N	227,555	227,555	227,555	227,555	227,555
Panel B: High Impact Sample					
Treatment	0.00836***	0.00502	0.00778**	0.00557	-0.00175
	(0.00214)	(0.00311)	(0.00342)	(0.00354)	(0.00158)
	[0.04]	[0.02]	[0.04]	[0.03]	[-0.01]
Mean (Y)	0.317	0.747	0.491	0.505	0.419
N	189,042	189,042	189,042	189,042	189,042
Controls					
Full Set	X	X	X	X	X

Note: This table shows difference-in-differences estimates of the impact of the policy on intermediate outcomes. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 2. The dependent variable is shown in the column headings. We identify a student as ever being disciplined in that year if the student had an in school or out of school suspension, expulsions, or other disciplinary action. All dependent variables are measured in the year each student was expected to be in 9th grade. Panel A estimates for the full sample. Panel B estimates from our high impact sample. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05. See Table 2 for more detail on the sample and the full set of controls. The effect for the fully exposed student at the average district are shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered by district. *p<0.10, ** p<0.05, *** p<0.01.

Table A.7: The Effect of the SE Enrollment Target on SE Removal and Educational Attainment (Low Impact Samples)

	SE Removal		High School Completion		College Enrollment	
Panel A: Severe Malleable Disabilities						
Treatment	0.00145	0.00127	-0.00397	-0.00732*	-0.000293	-0.000766
	(0.00398)	(0.00320)	(0.00455)	(0.00423)	(0.00405)	(0.00380)
	[0.007]	[0.006]	[-0.018]	[-0.033]	[-0.001]	[-0.003]
Mean (Y)	0.085	0.085	0.653	0.653	0.180	0.180
N	17,280	17,280	17,280	17,280	17,280	17,280
Panel B: Non-Malleable Disabilities						
Treatment	0.000718	-0.000364	0.00325	0.000720	0.00155	-0.00182
	(0.00259)	(0.00240)	(0.00263)	(0.00282)	(0.00402)	(0.00333)
	[0.003]	[-0.002]	[0.015]	[0.003]	[0.007]	[-0.008]
Mean (Y)	0.049	0.049	0.840	0.840	0.207	0.207
N	21,233	21,233	21,233	21,233	21,233	21,233
Controls						
Year FE	X	X	X	X	X	X
District FE	X	X	X	X	X	X
Additional Controls		X		X		X

Note: This table shows difference-in-differences estimates of the impact of the policy on SE removal and educational attainment. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 2. The dependent variable is shown in the column headings. We identify a student as ever being disciplined in that year if the student had an in school or out of school suspension, expulsions, or other disciplinary action. All dependent variables are measured in the year each student was expected to be in 9th grade. Panel A includes students who were receiving SE services in 5th grade who had malleable disabilities and spent the majority of the school day receiving instruction in separate classrooms. Panel B only includes students who were receiving SE services in 5th grade who had non-malleable disabilities. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05. See Table 2 for more detail on the sample and the full set of controls. The effect for the fully exposed student at the average district are shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered by district. *p<0.10, ** p<0.05, *** p<0.01.

Table A.8: The Effect of the SE Enrollment Target on College Completion and Labor Market Outcomes – 6 Years after Expected High School Graduation

	College Completion		Positive Earnings		Earnings	
Panel A: Full Sample						
Treatment	0.000683	1.22e-05	-0.000939	-0.00170	-25.89	-20.75
	(0.000543)	(0.000537)	(0.00185)	(0.00139)	(60.46)	(49.21)
	[0.003]	[0.000]	[-0.004]	[-0.008]	[-116.505]	[-93.375]
Mean(Y)	0.0627	0.0627	0.629	0.629	11335	11335
N	227,555	227,555	227,555	227,555	227,555	227,555
Panel B: High Impact Sample						
Treatment	7.29e-05	-0.000503	-0.000953	-0.000904	-52.66	-28.39
	(0.000651)	(0.000629)	(0.00156)	(0.00147)	(61.76)	(58.16)
	[0.000]	[-0.002]	[-0.004]	[-0.004]	[-236.970]	[-127.755]
Mean (Y)	0.0689	0.0689	0.674	0.674	12525	12525
N	189,042	189,042	189,042	189,042	189,042	189,042
Controls						
Year FE	X	X	X	X	X	X
District FE	X	X	X	X	X	X
Additional Controls		X		X		X

Note: This table shows difference-in-differences estimates of the impact of the policy on college completion (i.e. earning a BA or a MA), being employed, and yearly earnings. These outcomes are all measured in the 6th year after a student's expected high school graduation. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 2. The dependent variable is shown in the column headings. Panel A estimates for the full sample. Panel B estimates from our high impact sample. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05. See Table 2 for more detail on the sample and the full set of controls. The effect for the fully exposed student at the average district are shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered by district. *p<0.10, ** p<0.05, *** p<0.01.

Table A.9: Sensitivity of Results to Sample Restrictions and Treatment Definition (High Impact Sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Main Analysis	No Drops	More Cohorts	Cohort G4	Cohort G6	Policy Exposure 5th -8th Grade	Policy Exposure 5th -10th Grade
Panel A: Likelihood of Losing SE, 4 Years after G5							
Treatment	0.00836***	0.00835***	0.00954***	0.00856***	0.00801***	0.00730***	0.0102***
	(0.00214)	(0.00206)	(0.00196)	(0.00253)	(0.00190)	(0.00198)	(0.00248)
	[0.04]	[0.04]	[0.04]	[0.04]	[0.04]	[0.03]	[0.05]
Mean (Y)	0.683	0.683	0.686	0.613	0.758 0.730	0.649	
Panel B: Likelihood of Graduating from High School							
Treatment	-0.00509***	-0.00488***	-0.00587***	-0.00259*	-0.00526***	-0.00381***	-0.00511***
	(0.00157)	(0.00147)	(0.00151)	(0.00138)	(0.00135)	(0.00133)	(0.00158)
	[-0.02]	[-0.02]	[-0.03]	[-0.01]	[-0.02]	[-0.02]	[-0.02]
Mean (Y)	0.710	0.710	0.698	0.739	0.690	0.694	0.738
Panel C: Likelihood of Enrolling in College							
Treatment	-0.00444***	-0.00416***	-0.00427**	-0.00505***	-0.00288*	-0.00333**	-0.00462***
	(0.00157)	(0.00155)	(0.00188)	(0.00159)	(0.00156)	(0.00134)	(0.00161)
	[-0.02]	[-0.02]	[-0.02]	[-0.02]	[-0.01]	[-0.02]	[-0.02]
Mean (Y)	0.354	0.354	0.336	0.376	0.330	0.348	0.366
N	189,042	190,973	310,663	178,374	187,548	194,972	184,214

Note: This table shows difference-in-differences estimates of the impact of the policy on the likelihood of SE removal and educational attainment decisions. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 2. The dependent variable is shown in the panel headings. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05 in our high impact sample. See Table 2 for more detail on the sample and the full set of controls. This table tests the sensitivity of our main results to different choices. Column 1 is our baseline estimates, Column 2 does not drop the set of district outliers based on SE enrollment in 2004-05, Column 3 includes cohorts from 1996-07 through 2004-05, Columns 4 and 5 changes the share of time exposed after 5th grade that we use to define treatments, and Columns 6 and 7 focus on different grade cohorts where treatment is measured between the respective grade and expected 9th grade for each cohort. The effect for the fully exposed student at the average district are shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered by district. *$p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.10: Likelihood of Being Enrolled in SE in Fifth Grade

	(1)	(2)
FRL	0.0187***	-0.00513***
	(0.00119)	(0.000919)
Immigrant	-0.0301***	-0.0124***
	(0.00633)	(0.00384)
Male	0.0370***	0.0369***
	(0.000907)	(0.000930)
ELL	0.0306***	-0.0332***
	(0.00620)	(0.00345)
Native Amercian	0.00100	-0.00774
	(0.00532)	(0.00530)
Asian	-0.0491***	-0.0473***
	(0.00266)	(0.00269)
Black	0.00651***	-0.0395***
	(0.00226)	(0.00228)
Hispanic	-0.0218***	-0.0421***
	(0.00168)	(0.00156)
Std Red (G3)		-0.0551***
		(0.00191)
Std Math (G3)		-0.0279***
		(0.00121)
Constant	-0.902***	-0.420***
	(0.0276)	(0.0185)
N	1,345,875	1,345,875
R-squared	0.038	0.106
Mean	0.0674	0.0674

Note: This table contains results from linear prediction models that predict SE participation in 5th grade based on baseline demographics and achievement measured in 3rd grade. Standard errors in parentheses are clustered at the district level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.11: The Impact of the Policy on SE Placement and Educational Attainment –Accounting for Differences in Performance in SE Monitoring (High Impact Sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Likelihood of Losing SE (by Expected 9th grade)								
Treatment	0.00836***	0.00705**	0.00643**	0.00776***	0.00794***	0.00900***	0.00844***	0.00908***
	(0.00214)	(0.00304)	(0.00284)	(0.00256)	(0.00227)	(0.00222)	(0.00231)	(0.00220)
	[0.038]	[0.032]	[0.029]	[0.035]	[0.036]	[0.041]	[0.040]	[0.040]
Mean (Y)	0.683	0.326	0.338	0.683	0.683	0.683	0.683	0.683
Panel B: Likelihood of Graduating from High School								
Treatment	-0.00509***	-0.00499**	-0.00419**	-0.00528***	-0.00497***	-0.00545***	-0.00511***	-0.00538***
	(0.00157)	(0.00211)	(0.00182)	(0.00186)	(0.00160)	(0.00164)	(0.00161)	(0.00163)
	[-0.022]	[-.022]	[-.019]	[-0.024]	[-0.022]	[-0.025]	[-0.023]	[-0.024]
Mean (Y)	0.710	0.726	0.720	0.710	0.710	0.710	0.710	0.710
Panel C: Likelihood of Enrolling in College								
Treatment	-0.00444***	-0.00531**	-0.00623***	-0.00622***	-0.00398**	-0.00415**	-0.00435**	-0.00394**
	(0.00157)	(0.00242)	(0.00215)	(0.00196)	(0.00172)	(0.00167)	(0.00170)	(0.00168)
	[-0.020]	[-.024]	[-.028]	[-0.027]	[-0.018]	[-0.019]	[-0.020]	[-0.020]
	0.354	0.371	0.372	0.354	0.354	0.354	0.354	0.354
N	189,042	115,242	144,040	189,042	189,042	189,042	189,042	189,042
District Sample								
All	X			X	X	X	X	X
Reduce Separate Inst		Low						
Modified Test-Taking			Low					
Controls								
Full Set	X	X	X	X	X	X	X	X
$f(t)\times$ Modified Test-Taking			X					
$f(t)\times$ Separate Instruction					X			
$f(t)\times$ Black Overrepresentation						X		
$f(t)\times$ Hispanic Overrepresentation							X	
Black Overrepresentation $\times$ FracExposed$_c$								X
Hispanic Overrepresentation $\times$ FracExposed$_c$								X

Note: This table shows difference-in-differences estimates of the impact of the policy on the likelihood of SE removal and educational attainment decisions. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 2. The dependent variable is shown in the panel headings. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05 in our high impact sample. See Table 2 for more detail on the sample and the full set of controls. The effect for the fully exposed student at the average district are shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered by district. *p<0.10, ** p<0.05, *** p<0.01.

Table A.12: OLS and IV Estimates of the Impact of the Enrollment Target on Educational Attainment (Regular Math Test-Takers)

Y	Mean SE	Mean Y	OLS	FS	RF	IV
Graduated HS	0.516	0.749	0.05347***	0.01199***	-0.00758***	0.6323**
			(0.00542)	(0.00325)	(0.00196)	(0.208)
College Enrolled		0.477	-0.06852***		-0.00479*	0.39962
			(0.00480)		(0.00236)	(0.238)
Kleibergen-Paap F-Statisitic				13.78		

Notes: This table reports difference-in-differences estimates of the impact of the policy on SE removal (by expected 9th grade) and educational attainment (Columns 1 -2). This table also reports OLS and IV estimates of SE removal on educational attainment outcomes (Columns 3-4). The dependent variable is shown in the row labels. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05 in our high impact sample who were taking the regular math test at baseline (i.e. 4th grade) (N=76,237). See Table 2 for more detail on the sample and for the full list of controls used. Standard errors in parentheses are clustered by district. *p< 0.10, **p < 0.05, *** p< 0.01.

Table A.13: Triple-Difference Estimates for SE Placement and Educational Attainment, Regular Test-Takers in 4th Grade only

	SE Removal	HS Grad	College Enrolled
Treatment	0.0143***	-0.00736***	-0.00476*
	(0.00320)	(0.00212)	(0.00261)
Treatment × Std Test Score (G4)	0.00410***	-0.000440	0.00148
	(0.00103)	(0.000816)	(0.00101)
Mean	0.484	0.749	0.479
N	65,937	65,937	65,937

Notes: This table contains results obtained from a triple difference model where we augment Equation 2 by including a term that interacts 4th grade standardized math test scores from fourth grade with treatment and lagged 4th grade standardized math test scores from fourth grade. See Table 2 for the full list of controls and information about each of these outcome variables. The sample for these regressions includes students who were taking the unmodified math exam during 4th grade in SE cohorts between 2000 and 2005. Standard errors in parentheses are clustered at the district level. *p< 0.10, **p < 0.05, *** p< 0.01.

Table A.14: The Effect of the SE Enrollment Target on College Enrollment - NSC Cohorts (High Impact Sample)

	(1)	(2)	(3)	(4)	(5)
Panel A: College Enrollment, 2 Years after HS Graduation					
Treatment	-0.00338**	-0.00410***	-0.00507***	-0.00544***	-0.00545***
	(0.00155)	(0.00152)	(0.00157)	(0.00162)	(0.00162)
	[-0.02]	[-0.02]	[-0.02]	[-0.02]	[-0.02]
Mean(Y)	0.303	0.303	0.303	0.303	0.303
Panel B: College Enrollment - NSC, 2 Years after HS Graduation					
Treatment	-0.00252	-0.00331**	-0.00436***	-0.00454***	-0.00455***
	(0.00156)	(0.00153)	(0.00156)	(0.00160)	(0.00160)
	[-0.01]	[-0.01]	[-0.02]	[-0.02]	[-0.02]
Mean(Y)	0.322	0.322	0.322	0.322	0.322
N	156,717	156,717	156,717	156,717	156,717
Controls					
Year FE	X	X	X	X	X
District FE	X	X	X	X	X
Individual Demo		X	X	X	X
Individual Disability			X	X	X
District-Cohort Demo				X	X
District Finance					X

Note: This table shows difference-in-differences estimates of the impact of the policy on educational attainment decisions. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 2. The dependent variable is shown in panel headings. Panel A includes college in-state college enrollment. Panel B includes in-state and out of state college-enrollment using NSC data. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05 in our high impact sample. See Table 2 for more detail on the sample and the full set of controls. The effect for the fully exposed student at the average district are shown in brackets, and is defined as the coefficient multiplied by 4.5. Standard errors in parentheses are clustered by district. *p<0.10, ** p<0.05, *** p<0.01.