THREE ESSAYS ON THE ECONOMICS OF SPECIAL EDUCATION

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Over 13 percent of U.S. public school students participate in special education programs annually, at a cost of roughly $40 billion. However, the causal impacts of special education services remains unclear. In Chapter 1, which is coauthored with Briana Ballis, we use administrative data from Texas to produce the first causal estimates of the long-run effects of losing access to special education. Our research design exploits variation in special education placement driven by a policy change in Texas that required school districts to reduce special education enrollment to 8.5 percent. We show that this policy led to sharp reductions in special education participation. Our difference-in-differences estimates imply that special education students enrolled in the average district experienced a 12 percent increase in the likelihood of special education removal, a 2.6 percent decrease in the likelihood of high school completion, and a 3.7 percent decrease in the likelihood of college enrollment. For students on the margin of special education placement decisions, our instrumental variables estimates imply that special education removal decreases high school completion by 52.2 percentage points and college enrollment by 37.8 percentage points. Lower-income and minority students experience larger increases in special education removal, and the negative impacts of special education removal on educational attainment are concentrated among these students. These results suggest that marginal participants experience long-run benefits from special education services.

In chapter 2, also co-authored with Briana Ballis, we provide the first causal

estimates of the long-run impacts of limiting minority student access to special education, as well as the first causal spillover effects of limiting access to special education on general education students. Under the same policy change that was utilized in Chapter 1, Texas capped district-level black and Hispanic disproportionality, defined as the percent of black or Hispanic students in special education relative to the percent in a district overall. We employ a dose-response difference-in-differences estimation strategy with administrative data from Texas. We find that the black disproportionality cap led to small gains in high school completion and college attainment for black students in special and general education. In contrast, the cap on special education enrollment led to reductions in high school and college completion for black and Hispanic students in special and general education. We provide suggestive evidence that these heterogeneous treatment effects could be driven by unobserved differences in special education misclassification.

In Chapter 3, I explore the impacts of state special education funding formulas on special education funding, enrollment, and overall student performance. In 2008, New Jersey overhauled its state finance system for funding public school districts. This included changing the way special education funds are distributed across districts, from a census to a block grant system. Prior to 2008, categorical aid was given to local school districts for each additional student classified as special education. After the policy change, New Jersey implemented a block grant formula that gives each district an amount of money based on the *total* district enrollment and the statewide average special education classification rate. I implement a difference-in-differences specification that exploits variation in treatment intensity across districts based on their pre-policy special education rates to estimate the impacts of this funding change on special education funding per pupil, special education enrollment, and overall student achievement. I find that the policy reduced district-level special education funding per pupil (both special and general education pupils) by about 4%, reduced total special education funding by about 7%, and reduced special education enrollment by about 1.2%. However, I do not find economically meaningful impacts of the funding change on special and general education performance on math or reading exams, or on the proportion of students who drop out of high school.

BIOGRAPHICAL SKETCH

Katelyn Heath joined the graduate program in the Department of Economics at Cornell University in 2014. Her research interests include labor economics and the economics of education, with a particular emphasis on students with disabilities. She holds a Masters of Art in Economics from Cornell University. She also holds a Bachelor of Science in Economics and Mathematics from Saint Michael's University in Vermont, where she graduated summa cum laude. While at Cornell, Katelyn had the opportunity to work at the University of Texas at Dallas' Education Research Center, which houses the Texas Schools Project data that she utilizes in her dissertation. She was also a 2018 NAEd/Spender Dissertation Fellow.

This dissertation is dedicated to my parents, Hobart and Donna Heath.

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Introduction to the Dissertation

The fraction of students with disabilities, who receive Special Education (SpEd) services in public schools is on the rise, increasing nationwide from 8.3 percent in 1977 to 14 percent in 2018 (National Center for Education Statistics, 2017). Although SpEd students make up 14% of the public school population, they account for about 34% of federal education spending (U.S. Department of Education, 2018a). SpEd makes up the third largest category of federal spending at about $13 billion, behind the Title I and Pell Grant programs. Despite growing costs and program participation, little is known about the causal impacts of SpEd services on short- and long-run student outcomes.

The effects of special education could be positive or negative among marginal students (i.e. those not so severely disabled that they will always be placed in special education). On the one hand, special education could inhibit the growth and self-perceptions of students with marginal disabilities via the stigma of a disability label and holding students back with instruction that interferes with time in the regular classroom (Shifrer, 2013; Lackaye & Margalit, 2006; Bear, Clever, & Proctor, 1991). On the other hand, special education could provide individualized skills and instructional services that are necessary for student growth in the regular classroom and later in life (Hanushek, Kain, & Rivkin, 2002). There is likely a continuum of need for special education services, where the effect of special education is heterogeneous depending on the severity and type of disability and by the vulnerability of the population. Understanding the effects of special education classification is crucial given the rising participation rates and costs of special education in recent years.

Prior causal literature on the direct and indirect effects of SpEd services for students with disabilities is sparse. We cannot simply compare students in SpEd to students in general education, as students in SpEd select into the program based on a wide array of characteristics, namely the fact that they have a demonstrated disability that adversely affects their learning. In addition, exogenous variation in SpEd enrollment is extremely difficult to come by. The ideal randomized control trial in this setting, randomly placing students in either SpEd or general education, is both infeasible and unethical. Additionally, federal legislation, implemented in 1975, requires that all public schools provide students with disabilities a "free and appropriate" public education, making policy variation in access to special education particularly difficult to come by.

In two of the three chapters of this dissertation I, along with co-author Briana Ballis, utilize a unique source of exogenous variation in special education enrollment arising from a Texas policy change. We produce, to our knowledge, the first causal estimates of the long-run impacts of losing access to special education. In 2004, Texas implemented the Performance Based Monitoring Analysis System (PBMAS). This policy capped the district-level percent of students in SpEd at 8.5%, as well as black and Hispanic student disproportionality at 1%, where disproportionality is defined as the percent of black or Hispanic students in SpEd minus the percent of black or Hispanic students in a district overall. The 8.5% SpEd enrollment cap is rare, as evidenced by the fact that it was deemed illegal by the federal government in 2018. Despite the cap's legality, it was in place for more than 10 years before being removed from practice.

In the first chapter of this dissertation, we use administrative data from Texas to produce the first causal estimates of the long-run effects of losing access to special education. Our research design exploits variation in SpEd placement driven by the 8.5% cap on district-level SpEd enrollment. We implement a dose-response difference-in-differences estimation strategy that exploits cross-district and cross-cohort variation in treatment intensity. Our results imply that special education students enrolled in the average district experienced a 12% increase in the likelihood of SpEd removal, a 2.6% decrease in the likelihood of high school completion, and a 3.7% decrease in the likelihood of college enrollment. For students on the margin of SpEd placement decisions, we use an instrumental variables strategy to estimate the direct impacts of losing access to SpEd services. We find that SpEd removal decreases high school completion by 52.2 percentage points and college enrollment by 37.8 percentage points. Lower-income and minority students experience larger increases in SpEd removal, and the negative impacts of SpEd removal on educational attainment are concentrated among these students. These results suggest that marginal participants experience long-run benefits from SpEd services.

In the second chapter of this dissertation, we provide the first causal estimates of the long-run impacts of limiting minority student access to SpEd, as well as the first causal spillover effects of limiting access to SpEd on general education students. We utilize the caps that Texas imposed under the PBMAS on district-level black and Hispanic disproportionality in the context of a dose-response difference-in-differences estimation strategy. We find that the black disproportionality cap led to small gains in high school completion and college attainment for black students in SpEd and general education. In contrast, the cap on SpEd enrollment led to reductions in high school and college completion for black and Hispanic students in SpEd and general education. We provide suggestive evidence that these heterogeneous treatment effects may be driven by unobserved differences in SpEd misclassification. We do not find that this policy had economically meaningful or statistically significant impacts on Hispanic students.

Finally, in the third chapter of this dissertation, I explore the impacts of state special education funding formulas on SpEd funding, enrollment, and academic performance. There are two main types of formulas that states use to distribute funding to SpEd programs. The first, census formulas, provide additional funds for each additional student in SpEd. The second, block grant formulas, provide a lump-sum to districts. It has been documented in previous literature that census funding systems lead to an incentive to increase SpEd enrollment, due to the additional money each SpEd student draws into the school (Forster & Greene, 2002; Cullen, 2003; Dhuey & Lipscomb, 2011; Morrill, 2018). It has yet to be determined how these incentives affect the outcomes of students, however.

I utilize a funding change in New Jersey, which changed the way SpEd funds are distributed across districts from a census to a block grant system, to estimate the impact of these formulas on student achievement. I implement a dose-response difference-in-differences specification that exploits variation in treatment intensity across districts based on their pre-policy SpEd rates. I find that the policy reduced district-level SpEd funding per pupil (both special and general education pupils) by about 4%, reduced total SpEd funding by about 7%, and reduced SpEd enrollment by about 1.2%. However, I do not find economically meaningful impacts of the funding change on SpEd and general education performance on math or reading exams, or on the proportion of students who drop out of high school.

CHAPTER 1

THE LONG-RUN IMPACTS OF SPECIAL EDUCATION

Briana Ballis and Katelyn Heath

1.1 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, forthcoming). 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. On the other hand, 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 causal evidence on how SE placement (or lack thereof) affects long-run trajectories of marginal participants. The main difficulty in evaluating the effectiveness of SE programs is identifying a plausible counterfactual. Students are selected into SE because they are at risk of low achievement or poor behavioral outcomes. In addition, SE inclusion criteria are neither straightforward nor standardized for students with less severe conditions. Thus, 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. However, SE eligibility rules were determined federally in 1975 (with minor changes since), making it difficult to identify variation in SE placement across locations or over time that is plausibly exogenous.

In this paper, we provide, to our knowledge, the first casual estimates of the long-run impacts of losing SE by exploiting a rare policy change that introduced exogenous variation in SE participation. In 2005, the Texas Department of Education implemented a district-level SE enrollment target of 8.5 percent. 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. In 2016, more than 10 years after the policy was implemented, an investigative Houston Chronicle article was published that alerted the greater public, for the first time, of the existence of this policy, sparking much public outcry and debate. Subsequently, the federal government conducted their own investigation and in 2018 determined that the 8.5 percent district target was illegal and in violation of federal disability law. The fact that this policy was implemented and in place for so long illegally highlights why policies such as this one are so rare. We use this policy change in combination with

[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.

[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.

administrative data from Texas that follows the universe of public school students into adulthood, to provide the first long-run causal estimates of SE participation.

Our research design exploits 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 students who were already enrolled in SE prior to the policy change and estimate the long-run effect of a reduction in SE access using two different identification strategies.[3] First, we use a difference-in-differences strategy that compares changes in SE removal and educational attainment in districts with lower versus higher pre-policy SE rates across cohorts, who had varying levels of time exposed to the policy. This strategy produces estimates of 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. We additionally estimate effects of the policy on general education (GE) students, utilizing a similar difference-in-differences model. The results of this approach will provide spillover effects of the policy on the GE students in the classroom.

Our results suggest that SE students who lose access to SE services experi-

[3] The policy pressure to reduce SE enrollment significantly changed the incentives to classify marginal students, which in turn changed the underlying conditions of SE students in the post-policy period. This sample restriction ensures that our results will not be negatively biased as a result of the underlying changes in the ability of students in SE after the policy change. As we will justify in Section 1.3.2, we focus on 5th grade SE cohorts. We also demonstrate in that section that our results are not sensitive to this grade cohort restriction.

ence 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 high school completion by 52.2 percentage points and college enrollment by 37.8 percentage points. These large reductions in high school completion and college enrollment are suggestive that later life labor market outcomes may also be likely to decline. However, we leave these results for future work, given that the limited number of post-policy years has not yet allowed these outcomes to fully realize. Lower-income and minority students experience larger increases in SE removal, and the negative impact of SE removal on educational attainment is concentrated among these students.

Why do marginal SE students (i.e. those with relatively mild conditions) experience such large reductions in educational attainment after SE removal? There are at least two possible explanations. First, 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. Therefore, students may mechanically be less likely to graduate from high school if they no longer have the option of being exempt from the high school exit exam. Second, SE students likely benefit from the additional resources and more focused attention they receive. Thus, when SE services

[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.

are removed, students may experience a reduction in learning, which makes it more difficult for them to complete high school and attain post-secondary schooling.

We find that students who lose access to SE are significantly more likely to take the high school exit exam, suggesting that this mechanical effect could have played a role. However, it is unlikely that changes in high school graduation requirements alone are driving our results. The largest declines in high school completion are driven by students who would have taken the exit exam regardless of the policy, likely ruling out a purely mechanical effect. Moreover, the long-run negative impacts of SE removal are larger for students in lower-resource districts. This highlights the potential importance of additional SE resources, especially in districts with fewer available resources beyond SE services 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. Long-run responses for students who never participate 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, we are not able to identify students in the post-policy period who would have been in SE in the absence of the policy but are now not.

We also find declines in educational attainment among GE students due to the policy pressure to reduce SE enrollment. Our difference-in-difference estimates imply that GE students enrolled in the average district experienced a 1 percentage point (or 1 percent) decline in the likelihood of enrolling in college. We do not find that policy exposure led to significant declines in high school completion among GE students, although the point estimate is negative. We do not expect these effects on GE students are a result of increases in classroom size, since SE removal was driven by those who were already educated in GE classrooms for the majority of the day.[5] Instead, we interpret these results as suggestive evidence that the additional resources SE programs bring to GE classrooms, such as teacher's aides, may have positive spillover effects on GE students.

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 et al. (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 policy that placed pressure on schools to improve overall student performance, Cohen (2007) finds that SE participation reduces absenteeism for marginal low-achieving students.[6] Only one paper finds that SE participation harms student achievement (Prenovitz, 2017). However, this difference from prior studies is likely driven by the context the author 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 previous studies offer little insight into the role of SE participation on adult outcomes. To date, the only evidence

[5] In fact, the majority of SE students spend greater than 80% of their time in the GE classroom.

[6] 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.

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 using linked administrative data. Our focus on longer-run outcomes such as post-secondary attainment, rather than test-scores (which has been the primary focus of prior studies), is particularly advantageous given that SE students often received accommodated or modified versions of state standardized exams. Thus, estimating the effect of losing SE access on performance on exams could produce mechanically negative results, since students are often taking a more difficult version of the exam after losing their SE status. Second, our focus on the largest exogenous reduction in SE participation to-date allows 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 long-run differential responses to SE access across many subgroups. We find that less advantaged students and those in lower-resource or lower-performing districts are more negatively impacted, suggesting that reduced 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 (forthcoming) finds that students with mild disabilities experience large achievement gains when they transition to

Boston charter schools from traditional public schools. This reduces individualized instructional support (by removing students from SE), but offers 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).[7]

Second, we provide new evidence 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.

[7]For this cost/benefit analysis, we use the social cost of a high school drop-out suggested by Levin et al. (2007). See Section 1.6 for more detail on the methodology used to compare the cost/benefit across programs.

1.2 Background

1.2.1 Special Education Programs

The Individuals with Disabilities Education Act (IDEA) requires public schools to provide all students a "free and appropriate" public 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 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 additional educational support to be able to succeed in school.[8]

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 (potentially accompanied by a classroom aide), in resource rooms for part of the school day, or in separate classrooms or schools entirely. Additionally, they may be eligible to take standardized exams with a variety of accommodations (such as extra time)

[8]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.

or take modified exams, which test content below grade level. In terms of grade promotion and high school graduation, SE students could be held to different passing standards or be exempt from test-taking to meet these requirements. 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.[9]

As previously noted, SE participation has grown significantly nationwide 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", 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). Second, 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 (American Psychiatric Associ-

[9]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

ation, 2013). This subjectivity underscores the empirical challenges involved in estimating the causal impact of SE participation.

1.2.2 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 that 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 relating to behavioral and academic outcomes and 80 percent of districts met or nearly met policy thresholds relating to the services and accommodations offered to SE students. In contrast, only 5 percent of districts met the thresholds relating to SE enrollment.[10]

In this paper, we utilize the unique policy pressure to reduce SE enrollment to 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.[11] Districts closer to the target were subject to

[10]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 areas.

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

developing monitoring improvement plans, while those further away were subject to third party on-site monitoring visits (Texas Education Agency, 2016a). Despite minimal sanctions, districts responded strongly to the 8.5 percent target. 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 tells us" (Rosenthal, 2016). 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.[12] Figure 1.1 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 in districts furthest from the target.

There is compelling evidence to support that the introduction of the SE enrollment target was exogenous. It appears to have been introduced in response to an unexpected state budget cut (Hill, 2004) rather than statewide trends in SE enrollment or expenditures. There is strong anecdotal evidence that it was unanticipated by districts (Rosenthal, 2016) and Figure 1.1 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

rate.

[12]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.

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 that districts made other changes for SE students beyond reducing their access to SE programs. As previously noted, the policy pressure to make instructional changes beyond SE removal was minimal. Therefore, we assume that the introduction of the PBMAS impacted students only through reducing their access to SE programs. Throughout the paper we demonstrate that the pressure to make other instructional changes for SE students is not driving our results, in support of this assumption.

1.3 Data and Summary Statistics

1.3.1 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.[13]

[13]SE students may take modified versions of standardized exams if deemed necessary. However, 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, since fewer students are enrolled in SE. For this reason, we do not estimate the impact of SE removal on achievement as a primary outcome in this paper.

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),[14] and whether students took the unmodified version of standardized exams. We link these student-level school records from the TEA to post-secondary enrollment data from the Texas Higher Education Coordinating Board (THECB). The THECB data include enrollment and degree attainment information for all Texas universities.[15]

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.[16]

[14] Specifically, we observe whether students spent all day in regular classrooms (i.e. are mainstreamed), less than 50 percent of the day in separate classrooms, or greater than 50 percent of the day in separate classrooms.

[15] Although college completion and earnings are available in our data, we leave for future work the impacts of losing access to SE on completion and earnings. Given that the policy was implemented in 2005, not enough time has passed yet for these outcomes to have fully realized.

[16] We demonstrate in Section 1.5.5 that our results are not sensitive to the inclusion of out of state college enrollment. When we focus on the subset of students for whom we observe NSC data (5th grade SE cohorts from 2001 through 2005) models that include out of state college enrollment provide nearly identical estimates to our main results, which only include college enrollment within Texas.

1.3.2 Sample Construction

To identify the direct impact of SE programs on student outcomes, we focus on students enrolled in SE prior to the enactment of the target and infer SE participation effects from policy-driven SE removals. In particular, we focus on students enrolled in SE programs as of 5th grade prior to policy implementation. 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.[17]

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 placed in SE before the SE enrollment target was enforced.[18] Since the policy significantly changed the composition of students identified with disabilities, this restriction ensures that students in our sample were diagnosed under a similar policy environment. Unless otherwise specified, we restrict our earliest cohort to 1999-00 (rather than the 1994-95 cohort when our data begins). We make this restriction in order to avoid including cohorts affected by the introduction of school finance equalization in Texas, which impacted SE classification incentives (Cullen, 2003). In partic-

[17]However, our results are not sensitive to this grade cohort restriction. In Appendix Table A.2 we demonstrate that the impact of SE removal for 4th and 6th grade cohorts provide similar estimates to 5th grade cohorts.

[18]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.

ular, Cullen (2003) demonstrates that school finance equalization increased fiscal incentives to enroll marginal students in SE in higher-wealth districts. By 1999-00, SE enrollment rates had leveled off.[19] 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.[20] 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 (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 of reducing SE enrollment if they had a malleable disability type, which we define as 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.[21] In what follows, we refer to this subgroup as our "high-impact" sample consisting of 189,042 students.

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[19] While school finance equalization changed classification incentives, it led to relatively small changes in SE access. As such, in Appendix Table A.2 we demonstrate that our results are largely unchanged if we use the the extended number of cohorts (i.e. 1995-96 - 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.

[20] This drops roughly 1% of the overall sample since district outliers with respect to SE rates are small. We demonstrate in Appendix Table A.2 that our results are nearly identical if these districts are included.

[21] 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.

1.3.3 Summary Statistics

Table 1.1 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 (that is, 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 1.1 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 general 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 1.1 compare SE students who remain in SE to those removed from SE by 9th grade. 5th grade SE students who lose SE by 9th grade are less likely to receive FRL, less likely to participate in ELL, have higher achievement on standardized exams, and have better long-run outcomes.[22] Nearly all students who lose SE by 9th grade have malleable disabilities (98 percent) and require fewer modifications to the regular curriculum; over 97 percent receive over 50 percent of their instruction

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

in general 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.

Appendix Table A.3 presents summary statistics that compare districts that were more and less treated under the policy based on their initial SE enrollment rate. More treated districts have smaller proportions of Hispanic students, slightly more students receiving FRL, and fewer students participating in ELL. The average district size was also smaller for more treated districts and they were more likely to be located in rural areas. While our identification strategy does not require that the distance above the SE enrollment target be uncorrelated with district characteristics, it does require that the distance above the SE enrollment target is uncorrelated with changes in outcomes that occur for any reason other than the introduction of the SE enrollment target. Reassuringly, we demonstrate in Section 1.5.5 that our results are robust to the inclusion of time trends interacted with district demographics (measured at baseline in 2004-05). In addition, we account for differences in baseline characteristics in our empirical strategy by including controls for demographic variables at the individual and cohort-district level, described further in Section 3.4.

1.4 Empirical Strategy

1.4.1 DiD Estimates of the Impact of the SE Enrollment Target 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 district's pre-policy SE enrollment rate.[23] Second, 5th grade cohorts were differentially treated under the policy based on the number of years (after 5th grade) that they were expected to be enrolled in school after the policy's introduction in 2004-05.

Our difference-in-differences estimating equation thus takes the form:

$$Y_{icd} = \delta_0 + \delta_1(\text{SERate}_d^{Pre} \times \text{FracExposed}_c) + \lambda_1 X_{icd} + \lambda_2 Z_{dc} + \gamma_d + \phi_c + \epsilon_{icd} \quad (1.1)$$

where Y_{icd} is either an indicator for SE removal or a long-run outcome for student i in 5th grade cohort c in district d. We control for 5th grade district fixed effects γ_d and 5th grade cohort fixed effects ϕ_c. The term SERate_d^{Pre} is the percent of SE

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

students above the 8.5 percent target in a student's 5th grade district in the 2004-05 school year (the year prior to policy implementation), and is set to 0 if a district is already below the 8.5 percent.[24] This term is interacted with FracExposed_c, a continuous measure of policy exposure, defined as the fraction of years a student spent in school under the policy between 5th and expected 9th grade.[25] We choose the relevant period of policy exposure to end in 9th grade, right before high school drop-out decisions are typically made (Texas Education Agency, 2018b).[26] The vector X_{icd} includes a dummy for gender, race, FRL status, ELL classification, gender-race interactions, primary disability type, unmodified exam indicator, and level of classroom inclusion. These variables are all measured at baseline, 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.[27]

To identify the direct impacts of this policy change, we estimate the above

[24]We assign treatment intensity 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.

[25]We use *expected* 9th grade, i.e. 4 years after 5th grade, in order to ensure that students within a 5th grade cohort are assigned the same amount of policy exposure. This prevents more years of treatment being assigned to grade repeaters). To illustrate the cross-cohort variation we utilize, Appendix Table A.4 shows policy exposure by each 5th grade cohort in our main analysis sample.

[26]We also use event study specifications as a data-driven way to validate the relevance of this margin. Additionally, our results are not sensitive to accounting for policy exposure through high school (i.e. up to expected 12th grade).

[27]Controlling for average district characteristics allow us to account for overall changes in district demographics. Controlling for district averages using SE students only accounts for compositional changes for the students in our sample.

equation on 5th grade SE cohorts identified before the policy was introduced, as discussed in Section 1.3.2. The main variable of interest, δ_1, represents the average impact of the policy pressure to reduce SE enrollment on student outcomes. We also estimate the above equation on students in GE as of 5th grade prior to policy implementation to investigate spillover effects. The key identifying assumption for our model to produce causal estimates is that districts under more policy pressure to reduce SE enrollment had similar counterfactual trends relative to districts facing less pressure. To assess the plausibility of this assumption we present event-study estimates by replacing FracExposed_c in Equation 1.1 with 5th grade cohort indicators. This approach allows us to visualize any difference in outcomes in more and less treated districts before and after the policy went into effect.

To further assess the plausibility of this assumption we examine observed trends in SE removal, high school completion, and college enrollment across districts with high pre-policy SE rates relative to those with low pre-policy SE rates. In Figure A.5, we split districts into four categories based on their SE rate in 2005. The bottom series contains districts already in compliance with the enrollment target in 2005. The top three series split districts into terciles based on their 2005 SE rate, given that it is above 8.5%. Figure A.5 (a) illustrates that although the levels of SE were different across districts prior to policy implementation, the trend was fairly parallel over time. After 2005 this figure makes clear that districts with the highest SE rates in 2005 made the largest reductions to their SE rates post-policy in order to comply with the enrollment target, relative to districts with lower SE rates. This also provides additional intuition for using the distance to the enrollment target as the measure of treatment intensity in our empirical strategy. In Figures A.5 (b) and (c), we illustrate similar trends in the raw data for the long-run outcomes. Each of these figures demonstrate patterns that provide support for common trends in the pre-policy period.[28]

To additionally check whether parallel trends were likely to continue in the absence of the policy, we show that there were no trends in demographics by initial district SE rates in Columns 6, 7, and 8 of Appendix Table A.5. In addition, we use each of our covariates to generate predicted outcomes based on students' characteristics during the pre-policy period. Columns 3, 4, and 5 of Appendix Table A.5 show that conditional on 5th grade cohort and district fixed effects, there is generally little association between treatment and these predicted outcomes. In some instances the estimated effects are positive and significant, but they are not economically meaningful.[29] Moreover, the positive direction of these effects suggest, if anything, that students in more treated districts were becoming positively selected over time, which would lead us to underestimate the negative impact we find on long-run outcomes.

We also investigate whether the policy led to differential attrition, perhaps with students in high initial SE rate districts being more likely to drop out *before* expected 9th grade after the policy was implemented. As this type of attrition is more likely to occur for students with more family resources (who would be expected to have better long-run outcomes), this could have changed the underlying composition of students in a way such that parallel trends would not have

[28]The 5th grade cohort year 1998 corresponds to individuals who were in 12th grade in 2005, and thus completely unexposed to the policy.

[29]For the full sample presented in Panel A, there are positive effects (significant at the 5 percent level) on predicted college enrollment and SE removal, but both are small and correspond to a 1 percentage point (or roughly 3 percent) change for both outcomes. For the high-impact sample presented in Panel B, we find that predicted high school completion is positively correlated with policy exposure. However, given that the coefficients in our main models are nearly ten times larger, we do not believe this positive relationship is of concern.

been likely to continue. Reassuringly, we show that attrition from our sample is uncorrelated with initial SE rates in Column 1 of Appendix Table A.5.[30] We also investigate whether there was greater district switching in more treated districts, relative to less treated districts. While district switching would 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. We find no evidence of district switching, as shown in Column 2 of Appendix Table A.5.

Finally, our identification strategy requires that there were no contemporaneous shocks that differentially impacted districts by initial SE enrollment rates. We assess the plausibility of this assumption in Section 1.5.5, where we consider other education and economic policies during this period. Overall, we do not find evidence that there are any other contemporaneous policy shocks that could have significantly biased our results.

1.4.2 IV Estimates of SE Removal on Long-Run Outcomes

In order to estimate the direct effect of losing access to SE, we use changes in SE access as a result of policy pressure 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

[30]Specifically, to look at attrition our 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).

the net benefits of SE are most unclear.

This IV approach hinges on two identifying assumptions. First, the policy must generate variation in SE removal. As we will demonstrate in Section 1.5.1, 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 re-allocated district resources or made other instructional changes for SE students, then we would not be able to attribute the reduced form effect on SE student outcomes to SE removal alone. However, the exclusion restriction would not be violated if SE students were negatively impacted by the removal of specific services (e.g., one-on-one aide or the option of modified test-taking) in addition to potential spillovers that might occur when a large number of students within a district lose access to SE services at the same time. To the extent that losing SE when many other SE students within a district are also losing access is different than an individual student losing access within a district, it could limit the generalizability of these results.

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 general education, to the detriment of SE students' outcomes. Alternatively, if districts kept resources constant, students who continued to be enrolled in SE after the policy could have benefited from more resources per SE pupil. As shown in Table A.6, we find no significant impact of the enrollment target on district level SE or general education 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.[31]

Next we investigate whether SE students in more treated districts experienced other instructional changes. This is important to consider since at the time the SE enrollment target was introduced, other aspects of SE instruction also began to be monitored under the PBMAS.[32] Appendix Table A.7 provides estimates of δ_1 from Equation 1.1 for time spent in resource rooms and unmodifed test-taking as of expected 9th grade.[33] We find very little evidence that the policy pressure to reduce SE enrollment introduced other instructional changes in services or accommodations beyond SE removal. The only statistically significant change that we document is a small increase in the likelihood of taking the unmodified math exam. However, it is important to note that students no longer enrolled in SE will have to take unmodified exams, making it plausible that this positive effect

[31] We also note that we estimate IV effects on a high-impact sample of SE students who already spend the majority of their time in the general classroom. Thus, it is also unlikely that our results are biased by changes in the composition of students in the general classroom.

[32] As previously argued in Section 1.2.2, 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 instructional changes. Nonetheless, it is important to rule out this possibility empirically.

[33] These outcomes were chosen based on the specific indicators monitored under the PBMAS. The only indicator 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 standardized 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.

is driven by SE removal as opposed to changes in how test-taking decisions for SE students are made. The magnitude of this 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.[34] In Section 1.5.5 we provide further evidence that it is unlikely that the other aspects of the PBMAS are influencing our results. Overall, we find compelling evidence that beyond large increases in SE removal, it is unlikely that SE students in districts facing more pressure to reduce SE enrollment were impacted in other ways.

1.5 Results

1.5.1 Difference-in Differences Results

We begin by establishing that the policy pressure to reduce SE enrollment increased the likelihood of SE removal for our sample. 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 analysis. While our main sample includes SE students from 1999-00 through 2004-05 (as justified in Section 1.3.2), we extend the number of cohorts back to 1995-96 for the event-study analysis to provide additional visual evidence on pre-trends.[35] Figure 1.2 presents event-study

[34]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.

[35]We also present event study results for our main analysis sample in Appendix Figure A.6. Event-study plots that include 5th grade cohorts between 1999-00 and 2004-05 demonstrate

estimates where the outcome is an indicator for SE removal in the year a student was expected to be in 9th grade. We can see in the figure that cohorts expected to graduate high school before the policy and cohorts with late exposure (after expected 9th grade) did not experience increases in SE removal. This pattern provides strong 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 in cohorts with more years of policy exposure before 9th grade.[36]

The difference-in-differences estimates for 5th grade SE cohorts between 1999-00 and 2004-05 are presented in Table 1.2. We show results for the full 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. In line with our event-study results, we find that the policy significantly increased the likelihood of SE removal for students in districts with higher pre-policy SE rates. 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).

The results for the full 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 (0.00778*4.5) or 12 percent increase in the likelihood of SE removal. We

similar patterns to event-study plots that include an expanded number of 5th grade SE cohorts (i.e. between 1995-96 and 2004-05).

[36]Appendix Figure A.7 shows an event study that uses an indicator for ever losing SE as the outcome variable. This figure shows a very similar pattern to the one presented in Figure 1.2. Again, the 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.

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 (0.00921*4.5) or 13 percent increase in the likelihood of SE removal. In addition, the policy had no impact on students whose SE removal would have been more difficult to justify with physical or cognitively severe conditions. Appendix Table A.8 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 disabilities.[37] For both groups, the estimates are statistically indistinguishable from zero, implying that these students were not more likely to lose SE under the policy.[38]

Educational Attainment

Next, we estimate whether less access to SE due to the policy impacted educational attainment decisions. Again, we start with event-study figures for an extended number of cohorts.[39] Figure 1.3 presents the event-study results where the outcome is an indicator for whether a student graduated from high school (a) or enrolled in college within four years of their expected high school graduation (b). Both figures

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

[38]To understand which disabilities were most affected, we estimate the effects separately for each disability type. Appendix Table A.9 reveals that the effects are largely driven by students with learning disabilities (LD).

[39]We also present event study results for our main analysis sample in Appendix Figure A.6. Event-studies that include 5th grade cohorts between 1999-00 and 2004-05 demonstrate similar patterns to event-studies that include an expanded number of 5th grade SE cohorts (i.e. between 1995-96 and 2004-05).

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 not 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 in high school, the effects on educational attainment are driven by 5th grade cohorts who were exposed to the policy before they were expected to be in 9th grade.

Our difference-in-differences estimates for students enrolled in 5th grade SE cohorts between 1999-00 and 2004-05 are presented in Table 1.3. This table provides estimates of δ_1 from Equation 1.1, where the outcome 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 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 of SE students.

The results for the full 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.8. 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 are driven by the students who were most likely to lose SE services. This is reassuring for our IV approach, which assumes the reduced form effects are solely driven by SE removal.

1.5.2 IV Results

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 1.4, where we provide results for 5th grade SE cohorts between 1999-00 and 2004-05 in our high-impact sample.[40] We produce 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

[40]For reference, Columns 1 and 2 of Table 1.4 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.

increases in college enrollment.[41] 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.[42]

While these are large effects, given that SE removal is accompanied with a significant change in a student's instructional supports (e.g., teacher's aides, ability to work in smaller groups, additional time on tests or assignments, ability to type rather than hand-write) 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 1.6.

1.5.3 Heterogeneous Impacts

We next explore whether there are differential impacts of the policy across a variety of background characteristics. Ideally, we would first determine how the underly-

[41]While we might typically expect to find that exiting SE is associated with 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 leads to increased high school graduation requirements such as a high school exit exam.

[42]At the bottom of Table 1.4 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.

ing conditions of marginal students compare across subgroups. If the underlying conditions across subgroups were similar, we would be able to attribute differential responses to SE removal to differences in how subgroups respond to changes in SE access.[43] However, if the underlying conditions across subgroups differed, then differential responses to SE removal could be driven by the conditions of marginal participants. Unfortunately, definitively establishing how the marginal SE student compares across student demographics is difficult with most available datasets (including our own). Two recent papers that have been able to account for a large number of student characteristics, namely health endowments or early achievement measures, point to less SE access for minority students (Elder et al., forthcoming) with fewer differences in SE access by family income (Hibel, Farkas, & Morgan, 2010).[44] 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.[45] Although our predictive models only offer suggestive evidence of how the underlying conditions

[43]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.

[44]Elder et al. (forthcoming) 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.

[45]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, FRL status 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).

compare across subgroups, we find that at baseline minority students were likely to have more severe conditions than non-minority students, but there were fewer differences in disability severity across family income. Thus, 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 reduced access to SE.

We find that low-income and minority students are significantly more likely to lose SE as a consequence of the policy. Panel A of Table 1.5 demonstrates that the likelihood of losing SE is driven by FRL students (Columns 1 vs. 2).[46] 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 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. 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 1.5 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

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

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 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 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.[47] In a companion, paper Ballis and Heath (2020), we 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 (2020) 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 (2020).

[47] 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.

1.5.4 Impacts on General Education Students

We next examine the impacts of the policy on GE students, both to examine possible spillovers and as a falsification check. If we find impacts of the SE enrollment target on GE students this could be indicative of our results being driven by unobserved and confounding determinants of educational unique to high SE-rate districts at the time the SE target was introduced. However, it could also reflect spillovers effects, which are plausible for several reasons. First, GE students could have been affected by direct peer-to-peer influences. Policy induced SE removal could have led to more disruptive classroom behavior which could have negatively impacted GE peers. Second, any declines in learning or behavior among SE students may have changed the way teachers allocated resources within the classroom. For instance, if teachers tried to compensate for the loss in services among special needs students, this could have taken their attention from others in the classroom. Finally, GE students may have directly benefited from the additional resources SE students bring to general education classrooms, such as classroom aides or co-teachers. As these services were removed from GE classrooms due to policy-driven SE removal, GE students' outcomes may have also declined.[48]

Table 1.6 shows the impact of the policy for all students enrolled in 5th grade together, and then separately by their SE participation status as of 5th grade. These results demonstrate that the reductions in educational attainment documented in the combined sample of SE and GE students are largely driven by SE students, with smaller and less significant declines in educational attainment for

[48]Additionally, some general education students could have been on the margin of SE participation themselves. As previously noted, students with mild disabilities often transition in and out of SE programs, so it is possible that some general education students who would have been deemed eligible for services in later grades, are now not as a result of the policy.

GE students. GE students did not experience statistically significant declines in high school completion, although the point estimates are negative, suggesting a potential decline in high school completion. At the average district, GE students experienced a 1 percentage point (or 1 percent) decline in college enrollment, relative to a 2 percentage point (or 4 percent) decline among SE students in the high-impact sample. To better understand which GE students are driving the declines in college enrollment, we estimate effects by baseline achievement (measured in 4th grade) in Appendix Tables A.12. The negative impacts on college enrollment are largest for those who were initially in the middle of the ELA (Panel A) and math (Panel B) achievement distribution.

We view the declines in college enrollment among GE students as being driven by spillovers, rather than an internal-validity threat. If bias was driving our results, we would expect negative effects to occur for all GE students, regardless of their initial achievement level. The pattern of heterogeneity we document, with negative impacts being driven by middle-achievers, suggests that spillovers, and in particular a potential reallocation of teachers' attention, is a more likely explanation for our findings. For instance, if SE removal led to declines in learning and behavior, then teachers may have tried to compensate for the loss of services. Low-achieving GE students may not have been negatively impacted by the policy if this re-focusing of teachers was directed toward them. Higher-achieving GE students may not have been harmed if they were in separate classrooms (i.e., honors classes) or if they had the skills and ability to compensate for changes in teacher attention on their own. However, those in the middle of the achievement distribution may have been harmed as a result of teachers shifting their focus away from them, and towards the lower end of the achievement distribution to compensate for any loss in services among SE students.

1.5.5 Robustness

We have presented evidence in support of our parallel trends assumption in Section 1.4.1. However, an additional potential concern is that underlying population differences by initial SE rates could have led to a later divergence in trends. For instance, the estimated effect of the policy could be driven by differential trends in outcomes due to the underlying population differences between more and less treated districts as highlighted in Section 1.3.3. Reassuringly, Appendix Table A.13 demonstrates that our results are robust to the inclusion of time trends interacted with the baseline fraction of Hispanic students, fraction of FRL students, and total cohort size, all measured in the 2004-05 school year. This helps to rule out the possibility that differential trends driven by demographic differences are driving our results.

Next, we consider the other components of the PBMAS monitoring. While the SE enrollment target was the major component of the SE monitoring, as previously noted, there were other monitors put in place at the same time to reduce the amount of time spent in separate classrooms or taking modified exams.[49] In addition to evidence presented in Section 1.4.2 that the policy pressure to reduce SE enrollment did not led to changes in time spent in resource rooms or modified exam taking, we perform several additional checks to rule out the possibility that our estimates are confounded by other changes for SE students as a result of the PBMAS.

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

[49] Although there were also measures introduced to monitor performance, the standards set to monitor performance we very low and met by 97% of districts when they were introduced.

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. Finally, we re-estimate all of our results on a subset of students who were 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 (these were the students who were already receiving the level of services and accommodations that were deemed compliant under the PBMAS). Taken together, these four checks provide compelling evidence that other aspects of the PBMAS SE monitoring are unlikely to be driving our results.

Next, we consider other education policies affecting Texas public school students during this period. 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.14.[50] We find that the highest performing students were most likely to lose SE, ruling out this type of strategic placement.

We also consider the potential impact of the great recession, which officially occurred between December 2007 and June 2009. We believe it is unlikely that the great recession is influencing our results. The great recession was relatively mild in Texas. Between 2004 - 2006 and 2007 - 2009, the unemployment rate rose from 5.4 to 5.6 percent in Texas. In contrast, over the same two time periods, the unemployment rate rose from 5.1 to 6.6 percent nationwide (Andrews, Li, & Lovenheim, 2014). Moreover, we do not have any reason to believe that the great recession differently affected students in high vs. low treated districts. As previously presented in Appendix Table A.13, our results are robust to the fraction of a cohort that was FRL and the share of a district located in rural areas in 2004-05 interacted with time trends. This helps to rule out the possibility that educational attainment in districts that may have responded differently to the great recession, either due to their geography or underlying economic conditions, were on differential trends.

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

[50] We augment Equation 1.1 by including a term that interacts 4th grade standardized math test scores with treatment and 4th grade standardized test scores.

to follow out of state college enrollment up to two years after expected high school graduation. Panel A of Appendix Table A.15 presents results were we omit out of state college enrollment, and Panel B of Appendix Table A.15 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.

1.6 Discussion

We find 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, 2018). 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.[51] 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.[52]

[51]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.

[52]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).

1.6.1 The Role of the High School Exit Exam

Given the large estimated impact SE removal has on educational attainment, it is important to consider how much of this effect is driven by changes in graduation requirements (i.e., a "mechanical effect") versus changes in human capital accumulation driven by reduced services as a consequence of SE removal. As previously noted, if deemed appropriate, SE students can be exempt from the high school exit exam, which is a high school graduation requirement for general education students. Thus, SE removal is associated with an increase in graduation requirements for students who were previously exempt from the exit exam.[53] This could be an important factor in explaining the reductions in high school completion that we document.

To determine the extent to which this mechanical effect is driving the results, we begin by analyzing differential responses to the policy across SE students with different likelihoods of being impacted by any policy-driven changes in graduation requirements. Specifically, we use all of our covariates to predict the likelihood of being exempt from the exit exam based on SE students during the pre-policy period. Table 1.7 shows difference-in-differences estimates across this measure. From left to right, each column of the table spans SE students who were very likely to have been exempt from the exit exam in the absence of policy-driven removals to those who would have likely taken the exit exam regardless of the policy change. Panel A shows that we find significant increases in policy-driven SE removal for all students, regardless of their predicted likelihood of taking the exit exam, of roughly 4 percentage points. However, the increases in exit exam test-taking are *exclusively* driven by students who were least likely to take the exit exam pre-policy (Panel

[53] Most SE students are exempt from the exit exam, as only 22 percent take it.

B, Column 1), while the decreases in high school completion are *exclusively* driven by those who were most likely to take the exit exam pre-policy (Panel C, Column 4). Since the declines in high school completion are driven by students who were very unlikely to be affected by policy-driven increases in high school graduation requirements, this provides compelling evidence against a purely mechanical effect.

As a second test, we look at performance on the exit exam. Panel A of Table 1.8 provides difference-in-difference estimates, where the outcome is an indicator for whether students ever took the exit exam or passed the math or reading exit exam conditional on having taken the exit exam. We present results for the full sample, the high-impact sample, and for students who took regular standardized exams at baseline (i.e. in 4th grade), hereafter referred to as the regular exam sample. Since the full and high-impact sample include individuals induced into taking the exit exam due to the policy, we focus our attention on the regular exam sample who would have been most likely to have taken the exit exam regardless of the policy change. Among the regular exam takers, we find that the policy pressure to reduce SE enrollment did not impact the likelihood of ever passing either the math or reading exit exam,[54] but led to significant declines in high school completion (Table 1.8, Panel B, Column 4). We take this as suggestive evidence against a purely mechanical effect.

Finally, we look at drop-out before students would have taken the exit exam in 11th grade. Finding policy-driven declines in high school enrollment *before* the first attempt at the exit exam would provide suggestive evidence against a purely mechanical effect. Panel C of Table 1.8 presents difference-in-differences

[54]It is important to note that we also find that the policy did not impact the likelihood of passing either the reading or math portions of the exit exams for the overall and high-impact sample, despite endogenous changes in the sample.

estimates of the policy on enrollment in each high school grade separately. While we do not find that the policy impacted enrollment before the exit exam was first administered for the full and high-impact samples, we find significant declines in 11th grade enrollment for the regular exam sample, right before students would have first attempted the exit exam. These results provide evidence against a purely mechanical effect, but are somewhat less conclusive since we only find drop-out before the exit exam for this one subgroup. Taken together, we believe these analyses provide evidence that the effects we find are not solely driven by the mechanical effect of increases in graduation requirements. However, we cannot rule out this mechanism entirely, as we do not ultimately observe the reason why a student decided to drop out of high school.[55]

1.6.2 Other Mechanisms

We also explore other mechanisms to help shed light on whether the declines in educational attainment are driven by reductions in human capital accumulation are a result of losing access to SE services. We look at absences, grade repetition, and standardized test performance, again estimating effects separately for the full, high-impact, and regular exam takers. As previously argued, since the regular exam takers were likely to take unmodified exams regardless of the policy, focusing on this sample provides a unique opportunity to estimate effects for a group of students that was not endogenously changing over time due to the policy. Panel C of Table 1.8 presents difference-in-differences estimates of the impact of the policy

[55]It is also important to note that students have unlimited attempts at taking the high school exit exam. It is not uncommon for students who have been out of school to come back to take the exit exam to get a high school diploma.

on these intermediate outcomes, measured during expected 9th grade. Overall, we do not find any significant declines in absences, grade repetition, and standardized test performance for any of the three samples.

One interpretation of these results is that the policy did not have any ill effects on human capital accumulation. However, we caution against this interpretation, given the limited outcomes we analyze, and our inability to track changes in achievement for SE students taking modified versions of exams. Moreover, we cannot rule out the possibility that the policy did not affect the accumulation of non-cognitive skills such as motivation and interpersonal interactions, that could also be an important factor in driving the reductions in educational attainment that we document.

1.6.3 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 1.9 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 are less likely to experience negative long-run consequences associated with this SE removal.[56] Affected students attending low-wealth districts are 2 percentage points less likely to graduate and enroll in college, while

[56]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).

those in high-wealth districts do not experience statistically significant changes in long-run outcomes. These differences suggest that either high-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.[57] 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 1.9 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 plan enrollment.

[57]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).

Finally, we explore differences across various measures of average district performance to address whether being in a higher-performing district helps mitigate the negative long-run impact of SE removal. To explore this possibility, we compare across district-level average test scores (Table 1.9, Columns 6-7) and district value-added (Table 1.9, 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.

1.7 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 target, 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.

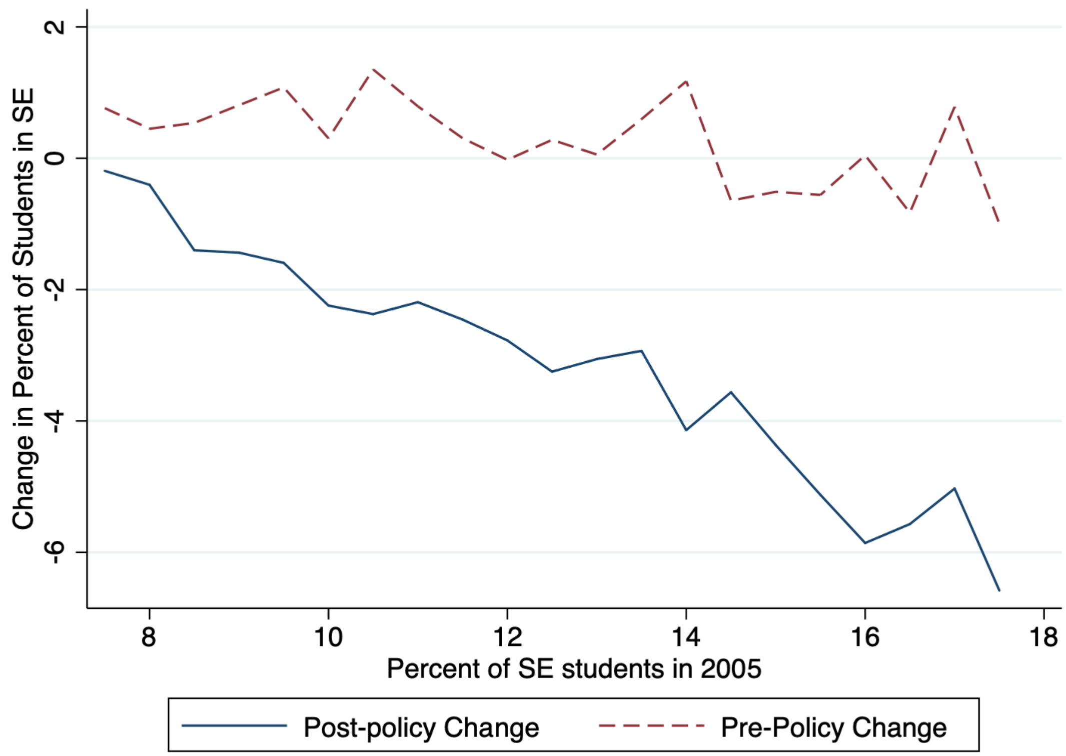
We find that SE services prepare students with disabilities for long-run success. We find that in the average school district (with initial SE enrollment of 13 percent), 5th grade SE cohorts experienced a 3.5 percentage point increase in the likelihood of losing SE four years after 5th grade, a 1.9 percentage 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.

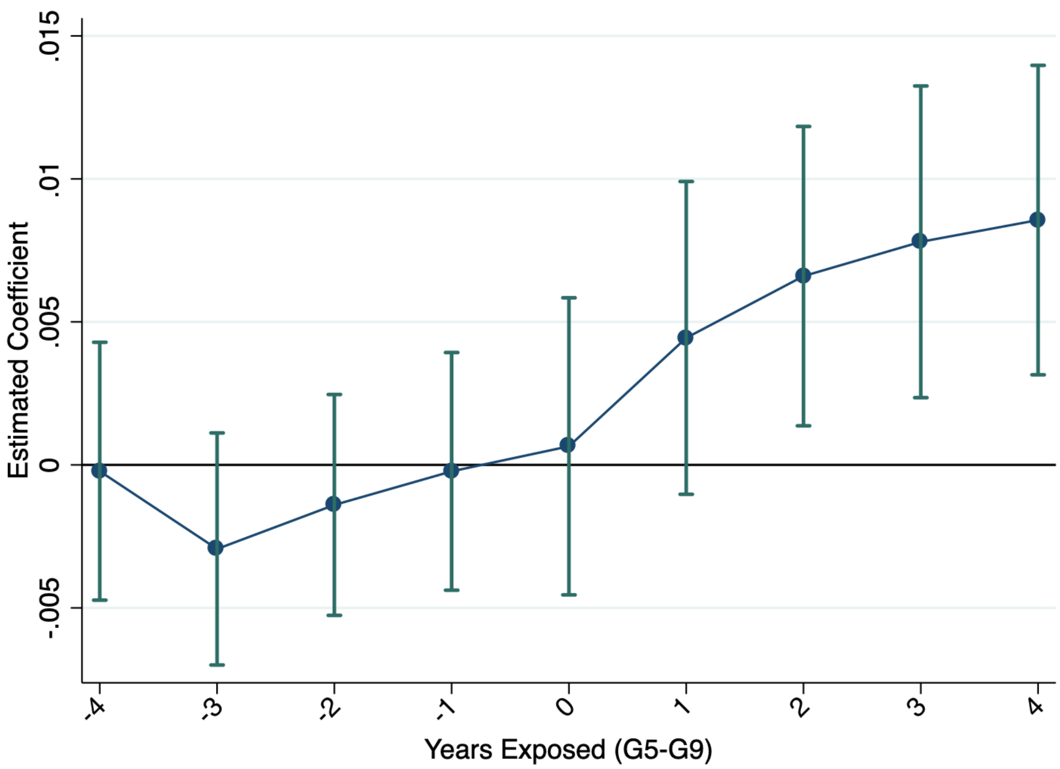
Figures and Tables

Figure 1.1: Change in District level SE enrollment during the pre-policy period (2000-2005) and the post-policy period (2005-2010)



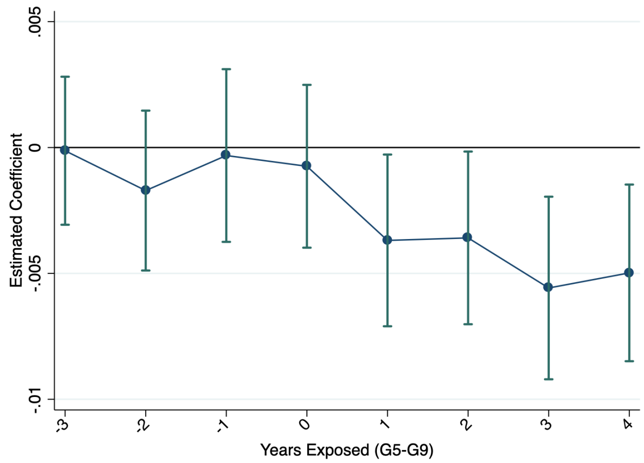
This figure shows the change in SE enrollment between 2000-2005 and 2005-2010 by SE enrollment in 2004-05. The district level changes are weighted by 2004-05 district enrollment.

Figure 1.2: Event Study Estimates of the Impact of the Policy on SE Removal in Expected 9th Grade (High Impact Sample)

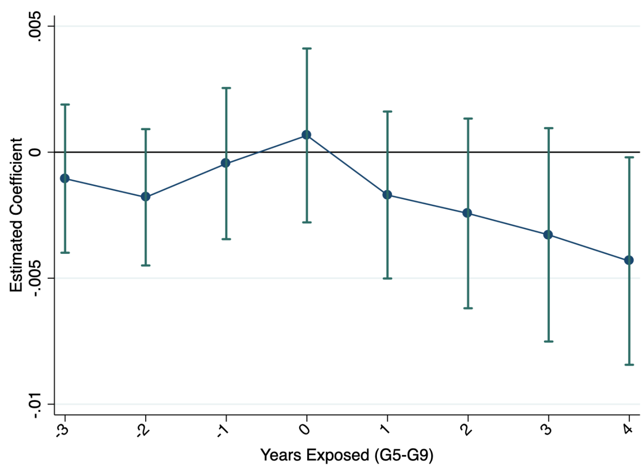


This figure plots coefficients and 95% confidence intervals from an event-study regression that estimates interactions between 5th grade cohort dummies and the 2004-05 district SE rate. The outcome is an indicator for SE removal, measured in the year each student was expected to be enrolled in 9th grade (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 1.3: Event Study Estimates of the Impact of the Policy on Educational Attainment (High Impact Sample)



(a) High School Completion



(b) College Enrollment

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 1.2 for more detail on the sample and the full set of controls. Standard errors are clustered by district.

Table 1.1 Summary Statistics - 5th Grade Cohorts Between 2000-2005

			SE Removal by G9	
	GE	**SE**	**No**	**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 (≤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

This table presents summary statistics for GE and SE students in 5th grade cohorts between 1999-00 to 2004-05. 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 are not conditional on being employed. Those not employed are assigned 0 yearly earnings.

Table 1.2 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

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 1.1. 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 1.3 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

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 1.1. 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 1.2 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 1.4 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			

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 1.2 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 1.5 Heterogeneity by Race and FRL Status (High Impact Sample)

	FRL (1)	Non-FRL (2)	Minority (3)	White (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

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 1.2 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 1.6 The Impact of the Policy on Outcomes by Participation in SE as of 5th Grade

	(1)	(2)	(3)	(4)
	GE+SE	GE	SE	SE - High Impact
Panel A: SE Participation in G9 (Expected)				
Treatment	-0.00294***	-0.00150***	-0.00820***	-0.00963***
	(0.000424)	(0.000284)	(0.00192)	(0.00215)
	[-0.0132]	[-0.00676]	[-0.0369]	[-0.0433]
Mean (Y)	0.122	0.0239	0.725	0.683
Panel B: High School Completion				
Treatment	-0.00274*	-0.000911	-0.00425***	-0.00481***
	(0.00150)	(0.000879)	(0.00148)	(0.00162)
	[-0.0123]	[-0.00410]	[-0.0191]	[-0.0216]
Mean (Y)	0.783	0.809	0.718	0.710
Panel C: College Enrollment				
Treatment	-0.00177	-0.00200**	-0.00262**	-0.00353**
	(0.00134)	(0.000967)	(0.00133)	(0.00148)
	[-0.00795]	[-0.00902]	[-0.0118]	[-0.0159]
Mean (Y)	0.530	0.620	0.327	0.354
N	1675558	1217396	227555	189042

This table shows difference-in-differences estimates of the impact of the policy on educational attainment decisions for students based on whether they were classified as special education as of 5th grade. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 1.1. The dependent variable is shown in the panel headings. The sample includes 5th grade cohorts enrolled in Texas public schools between 1999-00 to 2004-05. See Table 1.2 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 1.7 Heterogeneity by Predicted Likelihood of Taking the High School Exit Exam

Quartiles	≤25	25-50	50-75	≥75
	(1)	(2)	(3)	(4)
Panel A: SE Removal in G9 (Expected)				
Treatment	0.00677***	0.00935***	0.00857***	0.0108***
	(0.00151)	(0.00236)	(0.00327)	(0.00368)
	[0.0305]	[0.0421]	[0.0386]	[0.0488]
Mean (Y)	0.085	0.147	0.249	0.617
Panel B: Took High School Exit Exam				
Treatment	0.00802***	0.0103***	0.0107***	0.00460
	(0.00205)	(0.00275)	(0.00356)	(0.00330)
	[0.0361]	[0.0465]	[0.0483]	[0.0207]
Mean (Y)	0.124	0.232	0.381	0.693
Panel C: High School Completion				
Treatment	-0.00306	-0.00146	-0.00370*	-0.00786***
	(0.00230)	(0.00243)	(0.00204)	(0.00258)
	[-0.0138]	[-0.00657]	[-0.0166]	[-0.0354]
Mean (Y)	0.690	0.679	0.714	0.790
Panel D: College Enrollment				
Treatment	-0.00226	-0.00104	-0.00240	-0.00422
	(0.00167)	(0.00234)	(0.00230)	(0.00329)
	[-0.0102]	[-0.00468]	[-0.0108]	[-0.0190]
Mean (Y)	0.163	0.250	0.348	0.548
N	56,717	56,878	56,967	56,993

This table shows difference-in-differences estimates of the impact of the policy on SE removal and educational attainment decisions for students with different likelihoods of taking the high school exit exam. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 1.1. The dependent variable is shown in the panel headings. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05. See Table 1.2 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 1.8 Mechanisms

Panel A: High School Exit Exams	Took HS Exit (Math & Read.)	Ever Pass	
		Math	Reading
	(1)	(2)	(3)
Full Sample			
Treatment	0.00860***	-0.00373*	0.000605
	(0.00214)	(0.00225)	(0.00160)
Mean (Y)	0.432	0.525	0.831
High Impact Sample			
Treatment	0.00963***	-0.00282	0.000907
	(0.00236)	(0.00226)	(0.00160)
Mean (Y)	0.483	0.528	0.837
Reg Test-Taker Sample			
Treatment	0.00422	-0.00442	-0.000795
	(0.00263)	(0.00276)	(0.00143)
Mean (Y)	0.737	0.602	0.920

Panel B: Enrollment and Attainment	Enrolled G10	Enrolled G11	Enrolled G12	High School Completion	College Enrollment
	(1)	(2)	(3)	(4)	(5)
Full Sample					
Treatment	-0.000283	-0.00103	-0.00160	-0.00425***	-0.00262**
	(0.000490)	(0.000766)	(0.00112)	(0.00148)	(0.00133)
Mean (Y)	0.944	0.877	0.776	0.718	0.327
High Impact Sample					
Treatment	0.0000237	-0.000798	-0.00142	-0.00481***	-0.00353**
	(0.000545)	(0.000866)	(0.00126)	(0.00162)	(0.00148)
Mean (Y)	0.944	0.874	0.769	0.710	0.354
Reg Test-Taker Sample					
Treatment	-0.000785	-0.00385***	-0.00354**	-0.00831***	-0.00499*
	(0.000946)	(0.00136)	(0.00177)	(0.00218)	(0.00277)
Mean (Y)	0.957	0.907	0.817	0.762	0.518

Panel C: Intermediate Outcomes (During Expected 9th Grade)	Share Absent	Repeated Grade	Took Std Exam		Std Exam Score	
			Math	Reading	Math	Reading
	(1)	(2)	(3)	(4)	(5)	(6)
Full Sample						
Treatment	0.0001	0.0004	0.00685**	0.00484	0.0014	0.0098*
	(0.0002)	(0.0005)	(0.00313)	(0.00323)	(0.0049)	(0.0052)
Mean (Y)	0.069	0.026	0.435	0.450	-0.444	-0.545
High Impact Sample						
Treatment	8.62e-05	-6.78e-05	0.00778**	0.00557	-0.000929	0.00713
	(0.0003)	(0.0004)	(0.00342)	(0.00354)	(0.0048)	(0.0052)
Mean (Y)	0.069	0.020	0.491	0.505	-0.437	-0.530

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 1.1. 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 1.2 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 1.9 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

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 1.1. 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 1.2 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.

CHAPTER 2

DIRECT AND SPILLOVER EFFECTS OF LIMITING MINORITY STUDENT ACCESS TO SPECIAL EDUCATION

Briana Ballis and Katelyn Heath

2.1 Introduction

An important, yet understudied, aspect of racial disparities in education is the role of services for students with disabilities, known as special education (SpEd) in public schools. Although the returns to SpEd for minority students are unknown, policy that monitors minority student representation in SpEd has been in place for many years. In 1997, the federal government amended the Individuals with Disabilities Education Act (IDEA) to require that public schools monitor significant disproportionality of minority students in SpEd (Office of Special Education and Rehabilitative Services, 2009).[1] Texas, the focus of this paper, implemented a policy in 2004 that caps disproportionality at the district level. Texas requires that the percent of black or Hispanic students in SpEd be no greater than 1 percentage point higher than the percent of black or Hispanic students in the district overall.[2] These policies aimed at limiting disproportionality among minority students are based, at least in part, on the assumption that gaps between the classification

[1] The federal government defines disproportionality as the percent of a particular race in SpEd divided by the overall SpEd rate for all races.

[2] Although disproportionality is defined differently in Texas than by the federal government, an algebraic transformation can be performed to show that Texas is still technically monitoring disproportionality in accordance with the federal government's definition. However, they are doing so in a way that varies across districts based on the proportion of minority students and the proportion of SpEd students in a given district.

of minority and non-minority students in SpEd are detrimental to minority student outcomes. We are the first to test this assumption by causally estimating the impact of policy limiting disproportionality on minority students' outcomes in Texas.

In 2004, Texas implemented a policy that capped black and Hispanic student disproportionality at 1% as well as the district-level percent of students in SpEd at 8.5%. We employ a dose-response difference-in-differences estimation strategy to causally estimate the effects of these policies on minority students' long-run outcomes. This strategy exploits variation across districts in the SpEd enrollment rate as well as the black and Hispanic disproportionality rates, prior to policy implementation. We additionally leverage variation across cohorts in the amount of time students spent in school under the policy. Data come from the Texas Schools Project, a restricted-access administrative panel data set that allows researchers to link student-level records from the universe of Texas public K-12 students to public post-secondary school outcomes in Texas. Treatment effects are estimated separately for black and Hispanic students to determine the impact of limiting SpEd enrollment and black disproportionality on black students and the impact of limiting SpEd enrollment and Hispanic disproportionality on Hispanic students.[3]

Prior research has estimated that black students are about 1.4 times more likely to be receiving SpEd services relative to white students, while Hispanic students are about equally likely to be in SpEd relative to white students (Gordon, 2017; Hosp & Reschly, 2003; Donovan & Cross, 2002; Oswald et al., 1999). However,

[3]We incorporate additively the treatment variables for the SpEd enrollment cap and the disproportionality caps. We demonstrate in the empirical strategy section a lack of correlation between treatment variables, lending motivation to incorporating treatment additively and not including an interaction term.

after conditioning on important confounds such as prior academic achievement and socioeconomic status, minority students are less likely to be receiving SpEd services relative to their observationally-equivalent white peers (Elder et al., forthcoming; Morgan, Farkas, Hillemeier, & Maczuga, 2017; Morgan, Farkas, Cook, et al., 2017; Morgan et al., 2016; Shifrer et al., 2011; Hibel et al., 2010). At the heart of this literature is the concern that differences in SpEd classification between white and minority students imply that some minority students are inappropriately placed in SpEd. Blanchett (2006) writes that "the disproportionate referral and placement of African American students in special education has become a discursive tool for exercising white privilege and racism." While it is true that racism could play a role in the misclassification of minority students for SpEd, estimates of the representation of minority students in SpEd are not sufficient to guide policy. Prior research is not able to provide causal evidence on the impacts of policy limiting minority student representation in SpEd, and therefore cannot speak to whether minority students are appropriately served in SpEd. We extend the literature by providing the first causal estimates of the impacts of limiting minority student access to SpEd.

A priori, the net benefit of limiting access to SpEd services for minority students is unclear. On the one hand, SpEd could improve achievement if it provides individualized instructional services that are necessary for student growth in school and later in life (Hanushek et al., 2002; Ballis & Heath, 2019). On the other hand, SpEd could diminish achievement if it inhibits the growth and self-perceptions of students via the stigma of a disability label or by holding students back with instruction that interferes with time in the general classroom (Shifrer, 2013; Lackaye & Margalit, 2006; Bear et al., 1991). Prior literature estimating the impacts of SpEd placement on student outcomes is scarce and tends to focus on the short-run effects of test scores (Hanushek et al., 2002; Cohen, 2007; Prenovitz, 2017). For SpEd students, test scores are not a good proxy for long-run success since SpEd students may be exempt from or given modified version of standardized exams. In addition, these papers do not estimate effects separately by race. An exception in this literature is Ballis and Heath (2019). We find that losing SpEd services has a negative impact on high school and college enrollment for SpEd students. In that paper, as well as in the rest of the previous literature, treatment effects are not estimated for disproportionality polices. Disproportionality is a major concern for SpEd related policy, and we extend the literature here by providing the first causal estimates of the impacts of limiting disproportionality separately by race on long-run outcomes for students in both SpEd and general education.

The 2004 Texas policy change created incentives for districts to remove SpEd services from some students earlier than they would have otherwise and decrease the rate at which other students were newly identified for SpEd. However, we are not able to identify students who would have received SpEd services in the absence of the policy. Therefore, we estimate the effect of the policy on students in SpEd prior to policy implementation. Students designated as SpEd prior to policy implementation are defined as those receiving SpEd services as of 5th grade before the 2004-2005 school year. We choose 5th grade since this is the grade at which new entry into SpEd levels off, however results are robust to the choice of 4th or 6th grade instead.[4] In addition, we estimate spillover effects on students in general education as of 5th grade prior to policy implementation. If fewer students receive SpEd post-policy, there will be an increase in the number of unsupported and unaccommodated students in the general education classroom, who otherwise may have had teachers' aides or additional support services.[5] Thus, this policy could

[4]The pattern of SpEd entry across grades is illustrated in Figure B.7.

[5]We note that a removal of SpEd services does not necessarily lead to a physical shift of the

have meaningful impacts on both SpEd and general education students. Finally, we obtain effects on SpEd and general education students in the aggregate, which allows us to estimate effects for the full sample of students, in all grades, who enter school before and after the policy change.

We first estimate the effect of losing services for minority students already in SpEd prior to policy implementation. For black and Hispanic students receiving SpEd services as of 5th grade prior to the policy, the cap on SpEd enrollment led to a 1% decrease in the likelihood of receiving SpEd at 9th grade.[6] In the long-run, the SpEd enrollment cap led to a decrease in the likelihood of obtaining an associate's degree by 10% for black SpEd students; and a decrease in the likelihood of completing high school by 1.5%, enrolling in college by 2.8%, and earning an associate's degree by 7.1% for Hispanic students. These effects are precisely estimated and imply meaningful negative impacts on long-run outcomes for black and Hispanic students previously enrolled in SpEd, as a result of capping district-level SpEd enrollment at 8.5%. In contrast, the cap on black student disproportionality reduced the likelihood of receiving SpEd services, but slightly improved long-run outcomes. We estimate small increases in the likelihood of completing high school by 0.64% and enrolling in college by 2.1% for black SpEd students. The Hispanic disproportionality cap did not have statistically significant or economically meaningful impacts on Hispanic SpEd students.

Despite the fact that the SpEd enrollment and black disproportionality caps

number of students in the general education classroom, since 80% of SpEd students spend the majority of their time in the general education classroom in Texas.

[6] We estimate whether students remain in SpEd as of 9th grade, since this is prior to when most dropout decisions are made. In addition, we use *expected* 9th grade, defined as SpEd status 4 years after 5th grade to avoid endogenous changes in grade-repeating. This is detailed further in Section 3.4.

both had a similar negative impact on SpEd receipt, they had opposite effects on long-run outcomes for black students. This implies there are meaningful differences across students affected by each of the caps. Therefore, we seek to determine whether there are differences in the observable characteristics of black students more or less likely to be affected by one or the other caps. When the policy went into effect, districts removed greater proportions of students from SpEd between 5th and 9th grade. We estimate changes in the district-level composition of black students who lose SpEd services to investigate whether students were removed in a way that was correlated with observable characteristics. We do not find statistically significant differences in the district-level composition of observable characteristics of black students who were removed from SpEd over time as a result of either cap.

We conclude from this that there are likely to be differences in the reasons for initial classification of black students. At baseline, 5th grade black SpEd students in districts more affected by the SpEd enrollment cap are relatively higher performing on state standardized exams compared to those less affected by the SpEd enrollment cap. In contrast, black SpEd students who are more impacted by the black disproportionality cap are relatively lower performing, compared to those less affected by the black disproportionality cap. Intuitively, this is aligned with the notion that districts with higher baseline SpEd rates have a greater share of students higher up in the ability distribution enrolled in SpEd, with potentially more mild disabilities. We find that these relatively higher performers do in fact benefit from SpEd services. In comparison, districts with greater black disproportionality are more likely to place lower performing black students in SpEd relative to districts with lower black disproportionality rates. If it is the case that districts with high rates of black disproportionality misidentify low-performing black students as having disabilities at higher rates, this would lead to an improvement in long-run outcomes of black students removed from SpEd services, consistent with what we find. This would imply that these students would perhaps be better served by an alternative intervention.

The spillover effects on general education students are broadly consistent with the direct effects on SpEd students. As in Ballis and Heath (2019), we find negative long-run impacts on general education students of all races resulting from the SpEd enrollment cap. This is consistent with the intuition that general education students do worse in school when greater numbers of SpEd students (who may have already been in the general education classroom) are no longer supported by additional services, such as teacher's aides, in the general classroom. In terms of disproportionality, we find positive impacts on black general education students resulting from the black disproportionality cap. It is interesting that removing black students from SpEd as a result of capping disproportionality has positive impacts on both SpEd and general education students. Although we are not able to test directly the mechanisms behind this effect given data constraints, our results are consistent with a perceived reduction in racial bias in schools on the part of black general education students or an improvement in general education classroom instruction on the part of general education teachers.

Overall, we find effects that point to relatively higher performing minority students benefiting from SpEd services in districts with high rates of SpEd. These students' outcomes are harmed when SpEd enrollment is arbitrarily capped. In contrast, relatively lower-performing black students placed in SpEd in districts with high rates of disproportionality do not benefit from services. The cap on black disproportionality improved long-run outcomes for black SpEd students, and may be effective at reducing perceived racial bias in schools. This has meaningful spillover impacts on general education students in schools as well, whereby general education students' long-run outcomes are also affected by whether minority students are appropriately placed in SpEd. These findings point to the importance of carefully examining disability evaluation criteria to ensure that students of all races are appropriately evaluated and placed in SpEd.

2.2 Background

2.2.1 Special Education

In 1975, Congress enacted the Education for All Handicapped Children Act (later renamed the Individuals with Disabilities Education Act (IDEA)). This legislation introduced for the first time the requirement that schools provide a "free and appropriate" public education for all students regardless of physical or cognitive disability. Prior to this legislation, students with disabilities were often not served or not served appropriately in public school. In 1970, it is estimated that public schools educated only about 20% of children with disabilities (Office of Special Education and Rehabilitative Services, 2010). Services for students with disabilities provided under IDEA are now commonly referred to as Special Education services.

In order to qualify for SpEd under IDEA, students must fall within at least one of thirteen disability categories, which include autism, emotional disturbance, specific learning disability, other health impairment (which includes ADHD), and various physical disabilities (Reschly, 1996). To be evaluated for SpEd, a student is typically referred by a parent or general education classroom teacher. After the initial referral, the student is evaluated via a series of tests to determine what, if any, disability he has and whether his disability adversely affects his educational performance. These tests are typically administered by a special educator, a speech language pathologist, or a school psychologist. If a student is determined to be eligible, an Individualized Education Plan (IEP) is written for them by a team of professionals. This team involves both special educators and general education classroom teachers, in addition to the student's parent(s) or legal guardian(s). The IEP document states exactly what support and instructional services a student will receive over the course of the school year. This may include a teacher's aide in the classroom, direct instruction in small groups with a special educator or speech language pathologist, or direct services or consultation from other service providers such as occupational/physical therapists and social-emotional learning specialists. IEPs are reviewed at least once a year, and students are re-evaluated every three years to determine whether they still meet the eligibility requirements for SpEd (Office of Special Education and Rehabilitative Services, 2000). As per the IDEA, these services must be offered to all students who qualify at no cost to the students. IEPs are *individualized* and may vary widely so that each student receives a different set or combination of services depending on both the student's disability and the school they attend. This individualization makes it particularly challenging to determine the returns to SpEd. Students in SpEd have a wide range of disabilities and receive a wide range of services and supports. It is, therefore, difficult to ascertain whether differences in student performance across SpEd and general education students are the result of differences in the programs themselves or differences in the ability distribution of students in SpEd and general education.

2.2.2 Policy Environment in Texas

In the summer of 2004, the Texas Education Agency (TEA), introduced the Performance Based Monitoring Analysis System (PBMAS) (Texas Education Agency, 2016b). This system monitors three groups of students: Special Education, Bilingual/English as a Second Language, and Migrant students. For each group of students there is a set of outcomes that are monitored at the district level. Districts are assigned a *performance level* based on how they are performing relative to state standards for each of the monitored outcomes. If enough outcomes fall below a certain performance level, a district is *staged for intervention*, meaning they must develop a plan for improving their ability to meet adequate performance levels in subsequent school years (Texas Education Agency, 2016a). If districts are staged for intervention several years in a row and/or their performance levels are well below the compliance thresholds, the consequences can escalate from improvement plans, to on-site visits and third party consultations intended to provide feedback to districts on how to improve in the future.

One such monitored outcome for SpEd students under this policy was the requirement that districts have a disproportionality rate of 1 percent or less to be in compliance with state standards. The disproportionality rate is defined as the percent of black or Hispanic students in SpEd minus the overall district percent of black or Hispanic students. Appendix Figures B.1 and B.2 show tables from the original PBMAS 2004 Policy Manual illustrating the performance levels associated with varying levels of district disproportionality for black and Hispanic students. A second important outcome monitored under this policy was the requirement that districts have at most 8.5 percent of students in SpEd to be in compliance with state standards. Appendix Figure B.3 shows the table from the original PBMAS

2004 Policy Manual illustrating the performance levels associated with various rates of SpEd. We hereafter refer to this threshold as the SpEd enrollment cap.

The PBMAS, and in particular, the threshold for SpEd district enrollment was not widely publicized until Brian Rosenthal published an article exposing the policy in the Houston Chronicle in 2016 (Rosenthal, 2016). Much public debate ensued after the publication of this article, and it sparked an investigation by the Federal Department of Education that took place in February 2017 (Office of Special Education and Rehabilitative Services, 2017). In May 2017, the Texas Legislature passed a bill banning the use of targets in SpEd, and the SpEd cap has since been removed from practice. In January 2018, the Federal DOE released the findings of its investigation and concluded that the TEA had failed to comply with the federal law IDEA (U.S. Department of Education, 2018b). As a result, the TEA issued a corrective action plan in April 2018 to retroactively and proactively address its noncompliance with IDEA (Texas Education Agency, 2018a).

In Figure 2.1, we plot the percent of students in SpEd in Texas relative to the rest of the US. Prior to the policy's implementation, in the 2003-2004 school year, the statewide average percent of students in SpEd was about 14 percent. As of the 2016-2017 school year, the statewide district average had fallen to 9 percent. This is in contrast to the national average percent of students in SpEd, which remained approximately steady throughout the time period from 2004 to 2016. In Figure 2.2a, we show district level averages of the percent of students in SpEd in Texas overall and by race. Again, we see a sharp decrease in the rate of SpEd enrollment after 2004. Further, we show the district level rates of disproportionality among black and Hispanic students in Texas across our study period in Figure 2.2b. Of note is the fact that throughout, rates of disproportionality are much higher among black students compared to Hispanic students. By 2005, the statewide average Hispanic disproportionality rate was already below 0.

In addition to the outcomes described above, this policy monitors other outcomes related to performance of SpEd students. In Ballis and Heath (2019), we show that most districts were already meeting, or close to meeting, these other thresholds prior to policy implementation. We find no evidence that these indicators are biasing our results of the impacts of the SpEd cap on our sample of SpEd students. In 2005, 99% of districts were meeting or nearly meeting the threshold limiting disciplinary actions, 100% of districts were meeting or nearly meeting the academic performance thresholds, 77% were meeting or nearly meeting the inclusive setting threshold, and 89% were meeting or nearly meeting the unmodified test-taking threshold. Overall, districts were significantly less likely to respond to these thresholds, and policies which incentivized improved performance of SpEd students would bias us against finding negative impacts on SpEd students.[7]

2.3 Data

Data for this paper come from the Texas Schools Project (TSP), housed at the Education Research Center at the University of Texas at Dallas. This restricted-access administrative data set provides researchers the ability to link individual-level information from public school records from the Texas Education Agency to public post-secondary schooling from the Texas Higher Education Coordinating

[7]The monitored outcomes for Bilingual/English as a Second Language and Migrant students do not include any thresholds on the percent of students in these programs, rather they include passing rates on the standardized exams in math and reading and thresholds limiting high school dropout.

Board. We merge these data together to obtain a panel data set from 1994 to 2017, which contains a rich set of individual-level background characteristics including student performance on state standardized exams, high school graduation, and public post-secondary enrollment and attainment in Texas. The data we construct contain roughly 14.4 million unique individuals in public elementary and secondary school in Texas. Table 2.1 presents descriptive statistics for all students, black students, Hispanic students, and SpEd students in our sample. Notably, about 10.3% of students are in SpEd over the full time period of our data and 45% of students are Hispanic. Black students have a higher SpEd rate at 12.9% relative to Hispanic students at 9.4%.

Tables 2.2 and 2.3 illustrate raw differences in the characteristics of districts that are above and below the 8.5% threshold for SpEd enrollment, and above and below the 1% thresholds for the disproportionality caps among black and Hispanic students in 2004, one year prior to policy implementation. In Table 2.2, we see that districts already below the 8.5% SpEd threshold have fewer white students, more black and Hispanic students, and have slightly lower test scores on the math and reading exams. In Table 2.3, we see that districts already below the 1% threshold for black disproportionality had, on average, a greater proportion of Hispanic students and fewer black students. Most other characteristics do not vary significantly across districts above and below the black disproportionality threshold, and for those that do the differences are very small in magnitude. A similar pattern emerges comparing across districts above and below the Hispanic disproportionality cap. There are fewer Hispanic students and more black students in districts already in compliance with the Hispanic disproportionality cap. At baseline, districts are less likely to be meeting the black disproportionality cap (about 50%) compared to the number already meeting the Hispanic disproportionality cap (about 63%). We account for differences in baseline characteristics in our empirical strategy by including controls for each of these demographic variables at the individual, grade, and district level.

As we will describe in Section 2.4, we estimate the direct impacts of the policy on students in SpEd as of 5th grade prior to policy implementation. For these students, we estimate the likelihood of being in SpEd as of expected 9th grade. We use *expected* 9th grade to avoid endogenous changes in grade repeating resulting from the policy. We choose 9th grade in particular since this is a time before dropout decisions are made, although our results are robust if we instead use SpEd status by expected 8th grade. We do not estimate effects on math and reading exams for SpEd students. Students in SpEd are often exempt from the exams or take modified or accommodated versions of the exams.[8] Losing SpEd services is likely to reduce test scores mechanically as a result of no longer having access to modified or accommodated versions of the exam. In addition, modified and accommodated versions of the exams were not offered until 2001 and are not available in our data until 2008. Therefore, we do not expect the selected test scores of only those SpEd students who take unmodified versions of the exam to provide an accurate estimate of the effects of the policy on performance in school for SpEd students. For our models estimating the spillover effects of the policy on general education students, we provide estimates of changes in performance on the standardized exams in the Appendix. For general education students, we standardize raw test scores by grade, subject, and year such that each has mean 0 and standard deviation 1.[9] This allows us to compare performance on the exam

[8]Beginning in 2001, SpEd students are able to take accommodated and/or modified versions of the standardized exams. In certain cases, these exams cover lower than grade-content material, such that these exams are not equivalent to those offered to general education students.

[9]The standardized exam given to students changed twice during the study period (Texas

across years.

Our long-run outcomes include an indicator for whether an individual graduated from high school, attended a post-secondary institution in Texas, and obtained an associate's or bachelor's degree. We do not have reliable data on measures of dropout in Texas. Therefore, high school graduation is measured as an indicator for receiving a high school diploma within 2 years of expected graduation, for students observed in our data as of 9th grade. We choose 9th grade in particular to capture students before dropout decisions are made and to minimize counting other reasons for leaving the data in earlier grades as dropping out (such as moving out of state or to private school). Our results are robust to choosing 8th grade instead.

For post-secondary outcomes, we do not condition on high school graduation, and each outcome is censored such that individuals have 6 years after expected high school graduation to enroll in college and obtain an associate's or bachelor's degree. We highlight here that these data only capture college attendance in the state of Texas. However, outmigration from Texas is very low. As of 2012, Texas had the lowest outmigration of any state, with 82% of people born in Texas living in Texas (Aisch et al., 2014). College attendance out of state is also very low among students in Texas. In one estimate, for high school cohorts in 2008 and 2009, only 3.7% of students attended college out of state (Mountjoy, 2019)[10]. Finally,

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Education Agency, 2011). We use the math and reading test scores from the Texas Assessment of Academic Skills (TAAS) exam for students in grades 3 through 8, from 1994 to 2002. From 2003 to 2010, the state standardized exam was the Texas Assessment of Knowledge and Skills (TAKS), and from 2011 to 2017 the standardized exam was the State of Texas Assessment of Academic Readiness (STAAR).

[10]These estimates are limited to cohorts in the Texas Schools Project data that can be linked to the National Student Clearinghouse data, in order to estimate the fraction of Texas high

although earnings are available in the TSP data, the policy change occurs too close to the end of our earnings data to provide accurate estimates of changes in earnings. Ideally, we would like to estimate earnings 10 years after expected high school graduation in order to avoid changes in rates of college-going that could bias the results. Since the policy took place in 2004, this means that the most recent earnings estimate, measured in 2017, only covers up to high school cohort 2007. This cohort was only affected by the policy in 10th grade and later, which is particularly late given that we are considering changes in SpEd status which most often take place in grades K through 5. Thus, we leave for future work estimates of the impact of the policy on changes in earnings in the labor market.

2.4 Empirical Strategy

We estimate the causal impact of reducing minority students' access to SpEd services by exploiting cross-district and cross-cohort variation in exposure to the overall SpEd cap and the black or Hispanic disproportionality caps under the PBMAS policy. We employ a dose-response difference-in-differences estimation strategy to determine whether districts with higher rates of black or Hispanic disproportionality and overall SpEd enrollment at baseline experience larger gains or losses in outcomes. We estimate effects separately for black and Hispanic students. In each specification we include two treatment variables: (1) the 2004 overall SpEd rate interacted with the number of years in school under the policy; and (2) either the 2004 black or Hispanic disproportionality rate (depending on whether effects are being estimated for black or Hispanic students) interacted with the number of years an individual is in school under the policy. We choose 2004 since this is one

schoolers who attend college out of state.

year prior to policy implementation.[11]

Figures 2.3a and 2.3b illustrate the sources of treatment variation we use in each specification. We graph each district's SpEd rate in 2004 on the x-axis, and each district's black or Hispanic disproportionality rate on the y-axis. These figures illustrate the sources of variation coming from each of our treatment variables. There is little to no correlation between the treatment variables in each of the figures. The correlation coefficient between the two treatment variables in Figure 2.3a is 0.0022 and in Figure 2.3b is 0.0310. This motivates why we include these treatment terms additively into our model, without including an interaction term. We also note that the majority of districts were above the 8.5% SpEd threshold in 2004, with just 9% of districts already below 8.5%. By 2017, 46% of districts were meeting the 8.5% cutoff. In comparison, about 50% of districts were already below the threshold for black disproportionality and 60% of districts were meeting the threshold for Hispanic disproportionality. These ratios remain approximately the same for the fraction of districts meeting the black and Hispanic disproportionality thresholds in 2017.

Appendix Figures B.4, B.5, and B.6 further illustrate the intuition behind our treatment variables. In Figure B.4, we sort districts by their 2004 SpEd rate. The bottom series, denoted with circles, shows the average SpEd rate from 1994 to 2017 for districts already below the 8.5% threshold for SpEd enrollment in 2004. In the three top series, districts are split into terciles based on their 2004 SpEd rate, conditional on having a SpEd rate greater than 8.5%. Comparing the top most series, denoted with x's, to the bottom series illustrates that districts with the highest rates of SpEd made the largest reductions across the post-period in

[11] Our results are robust to an average of the pre-period years, and these estimates are available upon request.

their SpEd rates, indicating that they are more treated by the policy relative to those already meeting or nearly meeting the threshold. In Figures B.5 and B.6 we present analogous figures for the district-level black and Hispanic disproportionality rates, respectively. In these figures, we see a somewhat similar pattern in the topmost series, denoted with x's, illustrating that districts with the highest rates of disproportionality made the greatest reductions to their disproportionality rates in the post-period. However, we now find much less response in the first and second terciles above the 1% threshold.

We estimate effects for three groups of students: (1) the direct impacts on SpEd students, (2) the spillover impacts on general education students, and (3) the aggregate effect on all SpEd and general education students in school before and after the policy. Given the nature of the policy change, we are not able to causally estimate the effect of the policy by simply comparing SpEd student outcomes before and after policy implementation. When the policy is implemented, to reduce the SpEd rate districts must decide which students will be removed from SpEd and which students will not be placed in SpEd to begin with. These decisions will necessarily impact the underlying ability distribution of the students who remain in SpEd. Therefore, we cannot simply compare outcomes of SpEd students before and after the policy. Instead, we estimate the effect of limiting access to SpEd for students already identified for SpEd prior to policy implementation. To do so, we select students who were in SpEd as of 5th grade prior to policy implementation. 5th grade is a reasonable choice since most SpEd enrollment decisions take place prior to 5th grade.[12] However, when we use students in 4th or 6th grade prior

[12]New enrollment into SpEd levels off by 4th grade and begins to decline thereafter, as illustrated by Appendix Figure B.7. This figure plots the percent of all students entering SpEd by grade, and shows that the fraction of new entries levels off around 4th grade and drops each year after that.

to policy implementation instead, results remain qualitatively and quantitatively similar. To estimate spillover effects on general education students, we employ a similar strategy by estimating effects for students in general education as of 5th grade prior to policy implementation. These estimates are robust to choosing 4th or 6th grade cohorts as well.[13]

While the direct SpEd and spillover general education estimates provide interesting results for students in school prior to the policy, we cannot use this strategy for students who reach 5th grade after 2004. Therefore, we estimate aggregated effects on all SpEd and general education students in school before and after policy implementation in the aggregate. By aggregating SpEd and general education students we are able to avoid endogenous changes in ability across these two groups. Although these effects cannot distinguish between direct impacts on SpEd students and spillover effects on general education students, they are able to tell us whether these policies more broadly are welfare improving or welfare diminishing for students who enter school after policy implementation. These are students who are exposed to the policy in earlier grades, which has important implications for initial SpEd classification.

The outcomes we estimate differ slightly across models for the (1) direct effects on SpEd students, (2) spillover effects on general education students, and (3) aggregated effects on all students. For model (1), the direct effects on SpEd students as of 5th grade prior to the policy, we estimate the likelihood of losing SpEd services by expected 9th grade. In model (2), the spillover effects on general education students, we estimate changes in performance on the standardized exams. In model (3), the aggregated effects on all SpEd and general education students, our specification will be somewhat altered to estimate the likelihood of

[13] These estimates are presented in Appendix Tables B.2 and B.3.

being in SpEd. For this sample of students, we estimate the effect on SpEd status across all grades, rather than measuring SpEd status at a singular point in time. In terms of long-run outcomes, we estimate the impact of the policy on graduating high school, enrolling in college, and obtaining an associate's or bachelor's degree for students in all three models.

For models (1) and (2), as well as the long-run outcomes in model (3) we estimate:

$$Y_{idc} = \beta_0+\beta_1 SpEd_{2004,d}*Exposure_c+\beta_2 Disp_{2004,d}*Exposure_c+\beta_3 X_{idc}+\eta_d+\theta_c+\varepsilon_{idc} \tag{2.1}$$

where Y_{idc} is the outcome of interest for individual i, who attended school in district d, in cohort c. Students are assigned the district in which they are observed in kindergarten, and their cohort is the year they were in kindergarten. If students are observed in the data for the first time later than kindergarten, they are assigned the district in which they are first observed, and we use the year and grade of their first observation to compute the kindergarten cohort they would have been in. If a student repeats a grade, she remains assigned to her original cohort, to avoid endogenous changes in cohort year. Our first treatment measure is the interaction of $SpEd_{2004,d}$, the percent of students in SpEd in 2004 in each district and $Exposure_c$. For high school graduation and post-secondary outcomes, $Exposure_c$ is the number of years each cohort is expected to be in school between 5th and 12th grade under the policy. For expected 9th grade SpEd status, $Exposure_c$ is the number of years the cohort was exposed to the policy between 5th and 9th grade.[14] The second treatment measure is the interaction of $Disp_{2004,d}$, the dis-

[14]The number of years exposed varies across cohorts. Since cohort is defined net of endogenous changes in grade-repeating or dropping out, exposure is based on the expected number of years in school under the policy, rather than actual years to avoid endogenous changes in exposure.

trict black or Hispanic disproportionality rate and $Exposure_c$. Disproportionality is measured as the district SpEd black or Hispanic percentage minus district overall black or Hispanic percentage. Models are run separately for black and Hispanic students, such that the black disproportionality rate is included in models run on black students and the Hispanic disproportionality rate is included in models run on Hispanic students.

The term X_{idc} represents a vector of individual and district-cohort level controls. In model (1), when estimating the direct effects on SpEd students' likelihood of remaining in SpEd as of expected 9th grade, the individual level controls included in the model are disability type, an indicator for whether the student spent greater than 50% of the day in a general education classroom (as opposed to a resource room), gender, free and reduced-price lunch (FRL) status, English as a Second Language (ESL) status, gifted status, and Title I status measured as of 5th grade.[15] For long-run outcomes in all of our models the vector of controls are measured as the fraction of years in school an individual participated in the FRL, ELL, gifted, and Title I programs. In models (2) and (3) we do not use controls for 5th grade disability type and classroom setting. The district-cohort level controls are measured as the percent of the district that is male, black, Hispanic, FRL, ESL, gifted, and title I. We additionally include district fixed effects, η_d,[16] and cohort fixed effects, θ_c. Standard errors are clustered at the district level, since this is the level of treatment variation. The coefficients of interest in these regressions are β_1 and β_2. These are the difference-in-differences estimates of the effect of reducing SpEd enrollment and disproportionality, respectively, among black or

[15]English as a Second Language is the term previously given to the program now referred to as English Language Learner. We use the language that is in the Texas Schools Project.

[16]Our specifications are robust to including school fixed effects, rather than district fixed effects, and are available upon request.

Hispanic students.

For model (3), the aggregate effect of the policy on all SpEd and general education students in school before and after policy implementation, our estimation strategy is slightly altered to estimate the likelihood of being in SpEd as follows:

$$Y_{igdt} = \beta_0 + \beta_1 SpEd_{2004,d} * Post_t + \beta_2 Disp_{2004,d} * Post_t + \gamma X_{igdt} + \psi_g + \phi_d + \delta_t + \varepsilon_{igdt}, \tag{2.2}$$

where Y_{igdt} is an indicator of SpEd status for individual i, in grade g, in district d, in year t (for either black or Hispanic students). $SpEd_{2004,d}$ and $Disp_{2004,d}$ are defined as before. However, these terms are now interacted with $Post_t$, an indicator variable equal to 1 in post-period years (2005 to 2017). The term X_{igdt} is a vector of individual, grade, and district level background characteristics, including race, gender, FRL, ELL, gifted, and Title I controls as before. Finally, ψ_g, ϕ_d, and δ_t are grade, district, and year, fixed effects, respectively.[17] Standard errors for these regressions are again clustered at the district level.

The main identifying assumption for our models is: conditional on the fixed effects and observable characteristics, trends in long-run outcomes among districts with low SpEd rates provide an accurate counterfactual for trends among districts with high SpEd rates. Likewise, conditional on fixed effects and observable characteristics, trends in long-run outcomes among districts with a low rate of disproportionality (for either black or Hispanic students) provide and accurate counterfactual for trends among districts with a high disproportionality rate. We test this assumption directly by implementing an event study analysis. To do so, we parameterize $Exposure_c$, which corresponds directly to cohort, in models (1)

[17]Results are robust to the inclusion of grade-by-year fixed effects, as well as to using school fixed effects instead of district fixed effects.

and (2), such that $SpEd_{2004,d}$ and $Disp_{2004,d}$ are interacted with each cohort year. We then plot the coefficients of each of these interactions across time to present a visual of the trends in β_1 and β_2 over time. For predicted SpEd status in model (3), we parameterize $Post_t$, and interact each year before and after the policy with $SpEd_{2004,d}$ and $Disp_{2004,d}$ to provide a visual trend in β_1 and β_2 from equation (2) over time. The goal of our event study figures is to demonstrate whether trends in outcomes across districts with low versus high $SpEd_{2004,d}$ and $Disp_{2004,d}$ differ across pre-treatment years. The results of this analysis are presented in Section 5.

For our specifications to be identified it must also be the case that there are no contemporaneous shocks correlated with outcomes. The only policy, to our knowledge, implemented around the same time as the PBMAS, was the federal accountability system, No Child Left Behind (NCLB), implemented by President George W. Bush in 2003. Texas already had a statewide accountability system in place that had been implemented under President Bush when he was governor of Texas. The main difference between the Texas' state accountability system and NCLB is that NCLB monitored the performance of SpEd students as their own subgroup on the standardized exams.[18]. The vast majority of districts (97%) were already meeting the standardized performance ratings set by NCLB, which were identical to those under PBMAS (Ballis & Heath, 2019). In addition, Prenovitz (2017) finds that NCLB led to incentives to place relatively higher performing students into SpEd in order to boost the performance ratings of the SpEd subgroup, which is an incentive working in the opposite direction of the thresholds placed on SpEd enrollment in our setting.

[18]While NCLB did not monitor black SpEd students or Hispanic SpEd students separately, it likely still contributed to an incentive to improve the performance of black and Hispanic students in SpEd.

2.5 Results

2.5.1 Direct Effects on SpEd Students

In Figures 2.4a and 2.4b we present the results of our event study analysis on SpEd status as of expected 9th grade for black and Hispanic students, who were enrolled in SpEd as of 5th grade prior to the policy. On the x-axis we plot the 9th grade cohort year, since this is the year at which we estimate impacts on SpEd status. We plot on the y-axis the coefficients of indicator variables for each cohort year interacted with either the district 2004 SpEd rate or district 2004 disproportionality rate. The trend in the effect of the 2004 district SpEd rate is depicted in blue, and the trend in the effect of the 2004 disproportionality rate is depicted in orange. Our effect sizes are measured in percentage points, such that the scale on the y-axis for these graphs range from -1.5 percentage points to 1 percentage point. The shaded regions denote the boundaries of the 95% confidence intervals.

In Figure 2.4a, the effect of the SpEd enrollment cap on expected 9th grade SpEd status for black students appears to trend upward between 1999 and 2001, although this effect is not statistically significant at conventional levels. Additionally, a positive trend in the pre-period in the likelihood of being in SpEd at 9th grade would imply that more treated districts (with higher 2004 SpEd rates) were on a trend of increasing their SpEd rates prior to treatment, which is aligned with what we might expect. Additionally, this trend is in the opposite direction of the effects we estimate in the post-period. The trend in black disproportionality rates remains flat and statistically indistinguishable from 0 at conventional levels. After 2005 we see a distinct downward trend in the likelihood of being in SpEd.

This effect is negative for both caps, but stronger for the impact of the SpEd cap compared to the black disproportionality cap.

In Figure 2.4b we plot the results of the event study analysis for the impact of the SpEd cap and Hispanic disproportionality cap on the likelihood that Hispanic SpEd students were still in SpEd at expected 9th grade. We again see a somewhat positive trend in the effect of the 2004 district SpEd rate. This trend is, on the whole, not statistically distinguishable from 0 and is in the opposite direct of the effects we estimate in the post-period. The trend in the Hispanic disproportionality cap remains flat at 0 throughout the pre-period. In the post-period there is a statistically significant negative trend in the likelihood of SpEd at 9th grade for Hispanic students as a result of the cap on SpEd enrollment. However, there is no statistically significant deviation from 0 in the likelihood of SpEd at 9th grade for Hispanic SpEd students as a result of the Hispanic disproportionality cap.

Figure 2.5 presents the results of the event study analysis for the long-run outcomes for the 5th grade SpEd sample. These graphs are organized in the same way as before, however, we now plot on the x-axis the 12th grade cohort year. The effect sizes for the impact of the SpEd enrollment cap and the disproportionality caps in the years leading up to the policy are, for the most part, statistically indistinguishable from 0. The one exception is in Figure 2.5e, where we see a small positive effect in the likelihood of obtaining an associates degree as a result of the SpEd enrollment cap in 2002 for both black and Hispanic students. However, this effect size is very small and is in the opposite direction of the negative effects we estimate in the post-period. During the post-period we do not find statistically significant impacts of the policy in the first few cohorts after 2005 with the least amount of exposure. This aligns with the timing of Figures 2.4a and 2.4b. Since the long-run outcomes are estimated at 12th grade, students in the first 12th grade cohort after 2004 were only exposed to the policy for one year during their last year of high school. The first 12th grade cohort to have been exposed to the policy in 9th grade was the 2008 cohort. Indeed, in most of the event study figures we see the negative impacts of the policy beginning around 2008, particularly for high school completion and college enrollment.

In Table 2.4, we present estimates of the effect of the overall SpEd cap and the disproportionality caps on long-run outcomes for students who were in SpEd as of 5th grade prior to policy implementation. In columns (1) through (3) we present effects for black students. In column (1), we show estimates with no control variables, in column (2) we add individual level controls, and in column (3) we add both individual level and district-cohort level controls. We show each of these specifications in order to demonstrate the stability of our estimates as controls are added. Columns (4) through (6) present the same set of estimates for Hispanic students. Our preferred specification for black students is in column (3) and our preferred specification for Hispanic students is in column (6).

We first estimate the impact of the policy on SpEd status in expected 9th grade. For the cap on SpEd enrollment at 8.5%, we find for black SpEd students that a 1 percentage point increase in a district's 2004 SpEd rate is predicted to decrease the likelihood of being in SpEd in expected 9th grade by 0.25 percentage points for each additional year a student is in school under the policy. We scale our estimates to give an effect size for the average student by multiplying the coefficient by the average distance above the 8.5% cutoff in 2004, and by the average number of years a student was in school under the policy. The average district SpEd rate in 2004 was 11.7%, which is 3.2 percentage points above 8.5%. The average 5th grader was exposed to the policy between 5th and expected 9th grade for about 1 year. This implies, for the average black SpEd student, the likelihood of being in SpEd fell by 0.82 percentage points as a result of the cap on overall SpEd enrollment. This is a somewhat small effect size, representing a decrease of about 1%, given that 77.7% of black 5th grade SpEd students were enrolled in SpEd as of expected 9th grade. For the average Hispanic SpEd student, the likelihood of being in SpEd decreased by 0.75 percentage points, or about 1% as a result of the cap on SpEd enrollment. The cap on black disproportionality led to a 0.08 percentage point decline in the likelihood of being in SpEd for black SpEd students. The district average black disproportionality rate was 4.2% in 2004, which is 3.2 percentage points above the 1% threshold. This implies an effect size of 0.26 percentage points, or 0.34% for the average black SpEd student. For Hispanic SpEd students, we do not find statistically significant or economically meaningful impacts of the cap on Hispanic disproportionality on the likelihood of SpEd in expected 9th grade.

In terms of long-run outcomes, black students in SpEd as of 5th grade who were exposed to the policy and attended schools with high pre-period SpEd rates were less likely to obtain an associate's degree. To obtain the average effect of the SpEd cap in the context of long-run outcomes, we again multiply estimates by 3.2 percentage points for the average district's distance above the 8.5% threshold. However, we also multiply by 2.7, since the average student was in school under the policy between 5th and 12th grade for about 2.7 years. This implies that the cap on SpEd enrollment led to a 0.20 percentage point (or 10%) decline in the likelihood of obtaining an associate's degree for black SpEd students. This effect size is larger than our other estimates since the percent of black 5th grade SpEd students obtaining an associate's degree is quite low, at just 2.0%. We do not find statistically significant impacts of the SpEd cap on the remaining long-run outcomes for black SpEd students. For Hispanic SpEd students, the overall SpEd cap reduced the likelihood of completing high school by 0.85 percentage points (1.5%), reduced the likelihood of enrolling in college by 0.69 percentage points (2.8%), and reduced the likelihood of obtaining an associate's degree by 0.21 percentage points (7.1%). Overall, the SpEd cap worsened long-run outcomes for both black and Hispanic students in SpEd prior to policy implementation.

In contrast, the black disproportionality cap improved black SpEd student's long-run outcomes, although effect sizes are again quite small. For the average black SpEd student, the likelihood of completing high school increased by 0.37 percentage points (0.64%) and the likelihood of enrolling in college increase by 0.56 percentage points (2.1%), as a result of the cap on black disproportionality. For Hispanic students, the coefficients on the impact of the Hispanic disproportionality cap are very small and statistically insignificant at conventional levels. Therefore, we conclude that the Hispanic disproportionality indicator did not have an effect on Hispanic SpEd students' long-run outcomes. We investigate further in the Section 2.5.4 the mechanisms at play that lead to opposite long-run impacts of the SpEd cap and the black disproportionality cap on black SpEd students.

2.5.2 Spillover Effects on General Education Students

In Figure 2.6 we present our event study analysis for the effect of the policy on the general education sample, defined as those in general education as of 5th grade prior to policy implementation. For each of the outcomes we do not find evidence of differences in pre-treatment trends across districts more or less treated by the policy. The trends for the effects of the SpEd cap and the black and Hispanic disproportionality caps are statistically indistinguishable from 0 at conventional levels of significance.

In Table 2.5 we present estimates of the spillover effects of the policy on general education students' long-run outcomes.[19] We begin by estimating effects for black and Hispanic general education students separately. The estimates of the spillover effects of the policy by race provide estimates of the impact of the SpEd cap and black disproportionality cap on black general education students, and the impact of the SpEd cap and Hispanic disproportionality cap on Hispanic students. However, they do not provide estimates for students of other races in school. It is possible that an increase in the number of unsupported black and Hispanic students with disabilities in the general education classroom would have an impact on all students in the classroom, regardless of race. We therefore estimate the effects of all three caps on all general education students, by incorporating all three treatment variables additively into our model.[20] These effects are presented in Table 2.6.

Table 2.5 is organized in the same way as Table 2.4, with our preferred specification for black students presented in column (3), and our preferred specification for Hispanic students presented in column (6). The effects of the SpEd enrollment cap on black general education students are consistent with the direction of effects on black SpEd students. For the average black general education student, the SpEd enrollment cap reduced the likelihood of graduating high school by 0.76 percentage points (1.1%), reduced the likelihood of enrolling in college by 0.77 percentage

[19]Short-run estimates for the effect of the policy on test score outcomes can be found in Appendix Table B.15, and event study figures can be found in Appendix Figure B.9. We do not present these results as main outcomes, since we are not able to compare them to test score changes for SpEd students, and we view long-run outcomes as better indicators of later life success.

[20]Appendix Figure B.8 illustrates the correlation in the treatment variation between the 2004 district-level black and Hispanic disproportionality rates.

points (1.6%), reduced the likelihood of obtaining an associate's degree by 0.17 percentage points (3.5%), and reduced the likelihood of obtaining a bachelor's degree by 0.31 percentage points (2.8%). Similarly, for the average Hispanic general education student, the SpEd enrollment cap reduced the likelihood of completing high school by 0.43 percentage points (0.66%), reduced the likelihood of enrolling in college by 0.48 percentage points (1.1%), and reduced the likelihood of obtaining an associate's degree by 0.17 percentage points (2.2%).

Consistent with the effects we found for the impact of the black disproportionality cap on black SpEd students, we find improvements in long-run outcomes for black general education students resulting from the cap on black disproportionality. In particular, we find for the average black general education student that the black disproportionality cap increased the likelihood of completing high school by 0.42%, enrolling in college by 1.4%, and obtaining a bachelor's degree by 1.1%. Although the Hispanic disproportionality cap did not have an effect on Hispanic SpEd students, it had a negative impact on Hispanic general education students. This is likely a result of the impact the Hispanic disproportionality cap had on cohorts of students who entered before 5th grade after policy implementation, which we discuss for the aggregated sample of students next. The statewide district-level average Hispanic disproportionality rate was already below the 1% threshold in 2004. Thus, we interpret the impact of the Hispanic disproportionality cap simply as the effect of a 1 percentage point increase in the 2004 Hispanic disproportionality rate, keeping in mind that the impact for the average district would be much smaller. We still multiply coefficients by 2.7, since this is the average number of years an individual spent in school under the policy. For a 1 percentage point increase in the 2004 district Hispanic disproportionality rate, we estimate a reduction in the likelihood of completing high school by 0.11%, obtaining an associate's degree by 0.32%, and obtaining a bachelor's degree by 0.26%, for the average number of years a Hispanic student was exposed to the policy.

Table 2.6 presents estimates of the impact of all three caps on all general education students, regardless of race. This table is structured similarly to those before. Our preferred specification is column (3), which includes individual and district-cohort level controls. For the average general education student, we find that the cap on overall SpEd enrollment reduced the likelihood of completing high school by 0.55%, reduced the likelihood of enrolling in college by 0.48%, and reduced the likelihood of obtaining an associate's degree by 2.9%. Despite the fact that we find improvements in long-run outcomes for black general education students as a result of the black disproportionality cap, we do not find impacts of the black disproportionality cap on general education students of all races. The Hispanic disproportionality cap had negative impacts on long-run outcomes for general education students of all races. We again interpret effect sizes as the impact of a 1 percentage point change in the Hispanic disproportionality rate for the average number of years an individual spent in school under the policy. We find that a 1 percentage point increase in the Hispanic disproportionality rate led to a reduction in the likelihood of completing high school by 0.13%, enrolling in college by 0.21%, obtaining an associate's degree by 0.24%, and obtaining a bachelor's degree by 0.46% for general education students exposed to the policy for the average number of years. Overall these effect sizes are very small in magnitude, although precisely estimated.

2.5.3 Aggregated Effects

In Figures 2.7 and 2.8 we present our event study analysis for the effect of the policy on the aggregated sample of SpEd and general education students. In Figure 2.7 we estimate the impact of the SpEd and disproportionality caps on the likelihood of being in SpEd for all grades, from K to 12. For the SpEd enrollment cap for both black and Hispanic students we estimate a small upward trend in the likelihood of being in SpEd. This is again in line with the intuition that districts with higher rates of SpEd enrollment in 2004 were on an upward trend in the pre-period in terms of SpEd enrollment. However, this trend is in the opposite direction of the effects we estimate in the post-period. Throughout the post-period we estimate reductions in the likelihood of SpEd placement for both the SpEd and disproportionality caps for black and Hispanic students. In Figure 2.8 we estimate the effects of the caps on long-run outcomes. On the whole, we do not find evidence of trends that are statistically distinguishable from 0 at conventional levels.

Finally, we estimate the impacts of limiting SpEd enrollment and disproportionality on aggregated effects for all SpEd and general education students in school before and after policy implementation. These estimates are presented in Table 2.7, which is organized as before. Both the SpEd and disproportionality caps for black and Hispanic students had negative impacts on the likelihood of being in SpEd. To calculate an estimate of the effect on the average student we multiply estimates by 3.2 for the average distance in the 2004 district SpEd rate from 8.5%, and we multiply by 3 for the average number of years this sample of students spent in school under the policy. For the average black student, this represents a decline in the likelihood of SpEd by about 30%, and for the average Hispanic student, about 31%. For the black disproportionality cap, we find a 9.1% decline in the likelihood of SpEd placement for the average black student. For the Hispanic disproportionality cap, we estimate that a 1 percentage point increase in the 2004 district-level Hispanic disproportionality rate led to a 0.45% decline in the likelihood of SpEd placement for Hispanic students.

Overall, we find small negative impacts of the SpEd cap on long-run outcomes for black and Hispanic students, presented in Table 2.7. For the average black student, we find that the SpEd cap reduced the likelihood of high school graduation by 1.2%, enrolling in college by 1.5%, obtaining an associate's degree by 5.1%, and obtaining a bachelor's degree by 4.2%. For Hispanic students, a 1 percentage point increase in the 2004 district Hispanic disproportionality rate is predicted to decrease the likelihood of enrolling in college by 0.15%, and obtaining an associate's degree by 1% for the average number of years exposed. As before, our results are positive for the effects of the disproportionality cap on black students. We find small but statistically significant positive impacts on high school completion by 4.6%, college enrollment by 1.7%, and bachelor's degree attainment by 1.3% for the average black student. For Hispanic students, we find small but statistically significant negative impacts on high school completion and associate's degree attainment.

2.5.4 Mechanisms

To this point, we have found that the cap on overall SpEd enrollment had negative impacts on long-run outcomes, whereas the cap on black disproportionality had positive impacts on long-run outcomes for black students. This result is surprising and points to the fact that there are meaningful differences between marginal students impacted by the SpEd cap and marginal students impacted by the disproportionality cap. In this section we explore several potential explanations for these differences.

In prior literature, Elder et al. (forthcoming) link birth certificate records to public school data in Florida to develop a model to predict the likelihood of SpEd placement based on a rich set of individual characteristics. The authors find that minority students are more likely to be under-represented conditional on observables in heavily minority schools relative to white students, and are more likely to be conditionally over-represented in heavily white schools relative to white students. Building on this insight, we estimate the impact of the policy separately for districts we categorize as having an over- or under-representation of black or Hispanic students in SpEd. In theory, districts may be placing students in SpEd until the marginal costs of SpEd services exceed the marginal benefit. If it is the case that districts with an under-representation of minority students in SpEd have not yet reached the equilibrium level of SpEd placement, where the marginal benefit equals the marginal cost, this would be one reason why removal from SpEd is detrimental to long-run outcomes. Similarly, if it is the case that districts with an over-representation of minority students in SpEd have placed students in SpEd whose marginal cost exceeds the marginal benefit, this would imply that removal from SpEd would improve long-run outcomes. If this theory is true, and if districts with high SpEd rates are also districts with an under-representation of minority students, this would help explain why black students removed from SpEd in districts with high SpEd rates do worse in the long-run. Likewise, if districts with high black disproportionality rates are also districts with an over-representation of minority students, this would help explain why black students removed from SpEd in these districts do better in the long-run. We test this theory by estimating the effects of the policy separately for districts with an under-representation of black or Hispanic students in SpEd and districts with an over-representation of minority students in SpEd.

To sort districts into those with an over-representation of minority students in SpEd and those with an under-representation, we follow Elder et al. (forthcoming)'s approach to implement a Blinder-Oaxaca decomposition. First, we use a logit model to predict the likelihood of SpEd placement for white students, based on pre-treatment characteristics. Next, we apply the coefficients from this model to black and Hispanic students, to predict the likelihood of SpEd placement for minority students as if they were white. Then, we subtract the prediction from an indicator for whether a student is actually in SpEd. This gives us a measure of whether the student is over-represented or under-represented in SpEd relative to an observationally-equivalent white student. Finally, we aggregate these differences to the district-level, to obtain an estimate for whether each district has an over- or under-representation of black and Hispanic students in SpEd.

Our estimates for the impacts of the policy separately by districts over- and under-representing SpEd students are presented in Table B.4. Interestingly, the results do not vary by our prediction of whether districts are placing minority students into SpEd at rates which are higher or lower than white students. In columns (1) and (2) of Table B.4 we see that effect sizes are very similar for the impact of the SpEd cap and disproportionality cap on black SpEd students' outcomes across districts with an over- or under-representation of black students. In columns (3) and (4) results are very similar across districts for Hispanic students. While the significance of the effects is not always the same across districts over- and under-representing Hispanic students in SpEd, the magnitude and direction of effects are similar for both types of districts. Thus, we do not find results that point to major differences in the impacts of the policy based on whether districts are predicted to have higher or lower SpEd representation of minority students relative to white students.

The second approach we take to characterize the heterogenous treatment effects is to estimate differences in the types of black students more likely to be impacted by each of the caps. We estimate how the district-level composition of the attributes of those who lose SpEd changes over time, across districts more or less likely to be impacted by each of the caps. When the policy went into effect, districts exogenously increased the proportions of students removed from SpEd between 5th and 9th grade. We estimate changes in the district-level composition of students removed from SpEd to investigate whether students were removed based on observable characteristics. If it is the case that districts with high rates of SpEd enrollment removed certain types of black students from SpEd and districts with high rates of black disproportionality removed some other types of black students from SpEd, this would help explain why we find heterogeneous treatment effects of these two caps. These estimates are presented in Table 2.8. It turns out that we do not find that districts are removing students from SpEd between 5th and 9th grade in a way that is correlated with observable characteristics, as a result of either cap.

Based on the analyses we are able to perform using the observable characteristics of our data, we conclude that our results are likely driven by differences in unobservable characteristics that impact the ways in which students are first identified for SpEd across districts. We estimate differences in the pre-determined background characteristics of black students in SpEd at baseline, in 5th grade. In

Table 2.9, the largest differences we find for black students are in the performance on the standardized exams, measured prior to policy implementation in grade 4. We estimate that black students in districts more impacted by the SpEd cap are higher performing on the math and reading exams relative to students in districts less impacted by the SpEd cap. In contrast, black students in districts more impacted by the black disproportionality cap perform worse on the math and reading exams relative to students in districts less impacted by the black disproportionality cap. In addition, black students in SpEd at baseline in districts with high rates of black disproportionality are more likely to receive FRL.

Intuitively, this is aligned with the prior that districts with higher SpEd rates may be enrolling greater numbers of marginal SpEd students, who are relatively higher ability, compared to districts with low SpEd rates. The results of our analyses suggest that these students with relatively higher ability and potentially more mild disabilities benefit from SpEd placement. Districts with high rates of black disproportionality appear to be placing more lower-performing and lower-income black students in SpEd relative to districts with low rates of disproportionality. These students do not benefit from SpEd, and in fact do slightly better when removed from SpEd. This particular group of lower-performing black students may be misidentified for SpEd services, potentially as a result of racial biases originating in the referral or evaluation processes for SpEd classification. We conclude that these students may be better served by an alternative set of interventions and services aimed at boosting the academic achievement of relatively lower-performing black students.

For our spillover effects on general education students we also find opposite impacts of the SpEd enrollment cap and the black disproportionality cap on black general education student outcomes. In particular, we find that black general education students do worse as a result of the SpEd enrollment cap. This is aligned with the prior that general education students' long-run outcomes are negatively affected when there is an increase in the number of unsupported and unaccommodated students with disabilities in the general education classroom. It is perhaps somewhat surprising that black general education students are better off in the long-run as a result of the black disproportionality cap. Although we are not able to directly test the mechanisms behind this effect due to data constraints, we propose two potential mechanisms that are consistent with the results we find. First, it may be the case that all black students benefited from a perceived reduction in racial bias when the disproportionality policies went into place. If lower-performing black students were placed in SpEd for racially motivated reasons, all students may benefit from a reduction in racially biased policies. Second, if there is a reduction in the rate at which lower-performing students are receiving SpEd services, general education teachers may change their practices and techniques to compensate for the difficulties potentially associated with greater numbers of lower performers in the classroom. In this case, general education teachers may be improving instruction in a way that benefits both SpEd and general education students.

2.5.5 Heterogeneous Impacts

We next explore heterogeneous effects of the policy by disability type, income, and gender. Tables B.5 and B.6 present effects of the policy on SpEd students by disability type. Table B.5 column (1) replicates the effect of the policy on all black SpEd students from column (3) of Table 2.4 for comparison purposes. In columns (2) through (7) we estimate effects separately for a variety of disability types.

Columns (2) through (5) represent disabilities that we categorize as "malleable", which we determined to have a relatively greater amount of subjectivity in their evaluation criteria compared to more severe or physical disability types. These disabilities include specific learning disability (SLD), speech impairments, emotional disturbance (ED), and other health impairment (OHI) which is a category that includes ADHD. In column (6) we present results for intellectual disability (ID). This is a more severe cognitive disability, which students may be evaluated for using an IQ test. Finally, column (7) presents results for students with physical disabilities such as deaf or blindness, hearing impairments, visual impairments, or orthopedic impairments.

Overall, we find that our effects of the cap on SpEd enrollment are driven by black SpEd students with SLD and Speech impairments. In contrast, the positive long-run effects of the black disproportionality indicator appear to be driven mostly by students with SLD and OHI. We additionally find a small increase in the likelihood of obtaining a bachelor's degree as a result of the black disproportionality cap for black students with an ID. Importantly, we do not find significant impacts on the likelihood of SpEd placement as a result of the policy caps on students with physical impairments. As we would expect, it is much more difficult to deny SpEd services to students with relatively more objective physical disabilities. In Table B.6 for Hispanic students, we find that most of the impacts of the SpEd enrollment cap are driven by students with SLD, Speech Impairments, and OHI. Interestingly, we now find small reductions in the likelihood of SpEd placement for Hispanic students with ED and ID as a result of the Hispanic disproportionality cap. However, on the whole we still do not find significant changes in long-run outcomes as a result of the Hispanic disproportionality cap for Hispanic SpEd students.

Next, we estimate effects by gender and income for both SpEd and general education students. In Table B.7 we present these results for black and Hispanic SpEd students. Overall, we find larger effects of the SpEd enrollment cap on female black SpEd students relative to male black SpEd students. For female black SpEd students, we estimate that the impact of the SpEd enrollment cap on SpEd placement by 9th grade is roughly 1.5 times larger than the effect size we estimate for male black students. For the cap on black disproportionality, we estimate a decline in the likelihood of SpEd at 9th grade for female black student that is almost 3 times larger than the effect size for male black SpEd students. However, we find larger positive impacts of the disproportionality cap on male black SpEd students' likelihood of high school completion and college enrollment, relative to female black students. By income, overall, we find that black students receiving FRL are more negatively impacted by the SpEd enrollment cap compared to non-FRL students. Interestingly, we also find that FRL black students are more positively impacted by the cap on disproportionality. For Hispanic SpEd students, the impact of the SpEd cap is roughly the same for male and female students. However, we do find larger negative impacts of the SpEd cap on Hispanic SpEd students receiving FRL relative to non-FRL Hispanic students. On the whole, the Hispanic disproportionality cap did not have statistically significant impacts on Hispanic SpEd student outcomes by income at conventional levels of significance.

In Table B.9, we present estimates by gender and income for general education students. For the impact of the SpEd cap on general education black students, the magnitude of the effect is quite similar for male and female students for each of the outcomes. We find statistically significant negative impacts of the SpEd cap on high school completion, college enrollment, associate's degree attainment, and bachelor's degree attainment for both male and female black general education students. For the black disproportionality cap, we find stronger positive impacts on black male general education students in terms of the likelihood of SpEd placement and obtaining a bachelor's degree, whereas we do not find statistically significant impacts for female students for these outcomes. In terms of income, we again find larger negative effects of the SpEd cap on FRL black general education students' outcomes. However, for the impact of the black disproportionality cap we find positive spillover effects for non-FRL students' likelihood of completing high school and enrolling in college. Additionally, the effect sizes for FRL and non-FRL black students are very similar for the impact of enrolling in college.

For Hispanic general education students, the impacts of the SpEd enrollment cap are larger for female students than male students. We find that the negative impact on the likelihood of completing high school is roughly 1.5 times larger for female Hispanic general education students, relative to male Hispanic general education students. In addition, the effect sizes of the SpEd enrollment cap are about twice as large in magnitude for female students' likelihood of enrolling in college and obtaining an associate's degree relative to male students. In contrast, the disproportionality cap had roughly similar negative impacts on male and female students, particularly for the likelihood of obtaining an associate's degree and bachelor's degree. By income, effect sizes of the SpEd enrollment cap are similar in magnitude for FRL and non-FRL Hispanic students. Despite similarities in magnitude, the effects of the SpEd cap for non-FRL Hispanic general education students are not statistically significant for the likelihood of enrolling in college and obtaining an associate's degree, although these effects are significant for FRL students. The cap on Hispanic disproportionality has similar negative impacts, in terms of size and significance, on both FRL and non-FRL Hispanic general education students' likelihood of completing high school and obtaining an associate's degree.

Finally, we estimate effects on other individual-level outcomes including absences, suspensions, and expulsions in Table B.12, as well as district-level changes in spending in Table B.14. On the whole, we do not find statistically significant impacts of either the SpEd cap or disproportionality cap on the likelihood of being absent from school, with the exception of a small increase in the likelihood of being truant for 10 or more days as a result of the cap on overall SpEd enrollment on black students. In terms of the disciplinary outcomes, we find that the cap on black disproportionality increased black students' likelihood of being suspended, having multiple suspensions, or being expelled. The cap on Hispanic student disproportionality reduced the likelihood that Hispanic students were suspended or had multiple suspensions. It is perhaps somewhat surprising that we find increases in disciplinary outcomes for black students as a result of the disproportionality cap, given the positive long-run outcomes we find. In terms of spending, we find reductions in total district-level SpEd spending, but we do not find changes in the level of SpEd spending per SpEd students. We also do not find evidence of a change in general education spending per general education students.

2.5.6 Robustness

One potential concern for our results is the possibility that students move out of Texas public schools, into private schools, or to a different district to obtain SpEd services upon being denied in their current district. In Table 2.10, we directly test whether students are systematically moving out of public schools as a result of the policy. To do so, we estimate the effect of the policy on the probability of switching to a district already in compliance with the state standards, and by estimating the likelihood of switching districts or leaving the data between 5th and 9th grade for our sample of students in SpEd as of 5th grade. We note that our data can only follow students who remain in the public school system in Texas. Therefore, we are only able to observe exits from public school and cannot look at where students go after exiting. We do not find evidence of systematic changes in exiting or district switching as a result of the policy.

Next, we implement a series of modifications to our specification to test the sensitivity of our results. We perform these analyses for both our SpEd sample and general education sample, presented in Tables 2.11 and 2.12, respectively. We estimate effects for black SpEd students in columns (1) through (4) and Hispanic SpEd students in columns (5) through (8). Columns (1) and (5) replicate our main results from Tables 2.4 and 2.5, respectively, for comparison purposes. First, we estimate whether districts facing greater pressure under the policy were on differential trends in terms of background characteristics. We do so by including interaction terms of each of our district-cohort level control variables with indicator variables for each cohort year. These estimates are presented in columns (2) and (5) of Table 2.11. The controls include the district-cohort composition of male, black, Hispanic, other races, FRL, ESL, gifted, and Title I students. Overall, results with these flexible trends in demographic characteristics are qualitatively and quantitatively similar to our original estimates.

In columns (3) and (7) we include school fixed effects, rather than district fixed effects. In columns (4) and (8) we test the sensitivity of our continuous treatment measures by setting to 0 the SpEd and disproportionality rates in districts that are already below the thresholds in 2004. We also set the SpEd and disproportionality rates above the thresholds to the difference between their 2004 rate and the cutoff. Analogous estimates for the general education sample are provided in Table 2.12. Our estimates are robust to each of these changes in specification, giving results which are very similar in magnitude and statistical significance for both black and Hispanic students and SpEd and general education students. Finally, we test whether our results are sensitive to our decision to use the district SpEd rate in the single year, 2004, before policy implementation in our measure of treatment intensity. We estimate the results using an average of the SpEd and disproportionality rates in the pre-period from 1994 to 2004. These results are quantitatively and qualitatively similar to those using just the 2004 SpEd and disproportionality rates, and are available upon request.

2.6 Conclusion

We present estimates of the impact of limiting minority student access to SpEd on the likelihood of being in SpEd, high school completion, and post-secondary attainment for black and Hispanic students in SpEd and general education. Three policies in particular allow us to quantify causal estimates of the effect of reducing disproportionality and SpEd enrollment on long-run outcomes. Under the Performance Based Monitoring Analysis System (PBMAS) introduced in 2004, Texas capped the overall SpEd rate at 8.5% at the district level. Texas also capped the percent of black and Hispanic students in SpEd relative to the percent of black and Hispanic students in the district, known as disproportionality. We exploit cross-cohort and cross-district variation in how far districts were from meeting the cutoffs before PBMAS in a dose-response difference-in-differences estimation framework. When the policy went into effect in the 2005 school year, it impacted districts differentially based on their pre-treatment percent of students in SpEd and their pre-treatment disproportionality rates. We show that districts with the highest 2004 proportions of SpEd were impacted the most by the cap on district-level SpEd enrollment. These districts made the largest reductions in their SpEd rates to meet the standard set by the state. We show the analogous relationship for districts with the highest rates of black or Hispanic disproportionality.

We estimate effects separately for three groups of students: (1) students in SpEd as of 5th grade prior to policy implementation, (2) students in general education as of 5th grade prior to policy implementation, and (3) SpEd and general education students in school in all grades before and after policy implementation. Overall, we find that the SpEd and disproportionality caps led to meaningful reductions in the likelihood of receiving SpEd services among black and Hispanic students previously enrolled in SpEd. In addition, we consistently find negative impacts of the SpEd cap on black and Hispanic students' likelihood of completing high school, enrolling in college, and earning post-secondary degrees. In contrast, we find positive effects of the black disproportionality cap on long-run outcomes for black students in SpEd and general education. The Hispanic disproportionality cap did not have statistically significant or economically meaningful impacts on Hispanic SpEd students. Although we do find that the Hispanic disproportionality cap led to worse long-run outcomes for Hispanic SpEd and general education students in the aggregate.

The fact that the SpEd cap had negative effects on black students' long-run outcomes and the black disproportionality cap had a positive impact on black students' long-run outcomes implies that there are meaningful differences across the types of students affected by each of the caps. First, we explore the extent to which the differences in effects are driven by differences in whether black and

Hispanic students are conditionally over- or under-represented in SpEd across districts. However, we do not find meaningful differences across districts over or under-representing black students in SpEd. Second, we predict differences in pre-treatment characteristics of black students who lose SpEd to estimate differences in the types of students more or less likely to be impacted by each of the caps. We do not find differences in the observable characteristics of students who lose SpEd services over time between 5th and 9th grade. We conclude that our results point towards differences in unobserved characteristics leading to initial placement in SpEd.

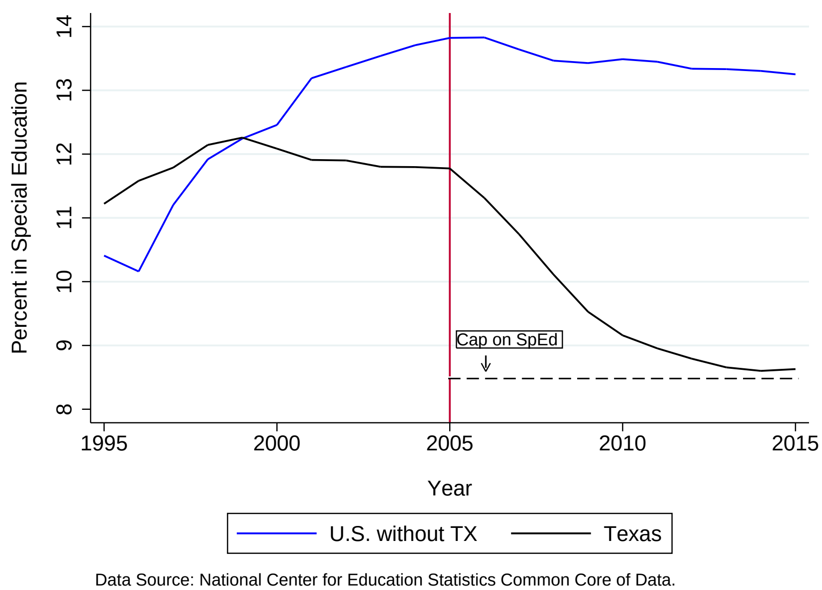
For our baseline sample of 5th grade SpEd students, we find that black SpEd students who are more likely to be impacted by the SpEd cap are predicted to be relatively higher performing on the 4th grade math and reading exams relative to black SpEd students less likely to be affected by the SpEd cap. In contrast, we find that black SpEd students who are more likely to be impacted by the black disproportionality cap have lower scores on the 4th grade math and reading exams relative to black SpEd students who are less likely to be impacted by the black disproportionality cap. This leads us to the conclusion that districts with high rates of SpEd are providing services to marginal students who are benefiting from this program. Districts with high rates of black disproportionality are placing a greater number of lower-performing black students in SpEd who do not benefit from the SpEd services they are receiving. These students may be misclassified for SpEd services and would potentially benefit more greatly in the long-run from a different set of interventions aimed at boosting academic achievement.

The impacts we find for general education students are consistent with the impacts we found for SpEd students. Students in general education as of 5th grade prior to policy implementation do worse in the long-run as a result of the cap on SpEd enrollment, regardless of race. This is likely a result of increases in the number of unsupported and unaccommodated students in the general education classroom. This could be driven by several potential mechanisms. Although SpEd students may have already been in the general education classroom for the majority of the day prior to SpEd removal, they may have had a teacher's aide or other accommodations that would have been aimed at boosting academic achievement. In the absence of these services, previously enrolled SpEd students may require additional attention from the teacher, which would leave less attention for the general education students in the classroom. Additionally, a teacher's aide in the classroom could have benefited both SpEd and general education students if they were able to answer questions and provide help to all students in the room. As a result of the cap on black disproportionality, we find improvements in long-run outcomes for black general education students. To the extent that the black disproportionality cap alleviates racial bias in schools, this could positively impact both SpEd and general education student outcomes. Finally, if limiting black disproportionality increases the number of unsupported low-performing black students in the general education classroom, this could lead to increases in teacher effort and effectiveness that would improve black general education outcomes.

Overall, the estimates we find have meaningful implications for SpEd policy in public schools. We find heterogeneous treatment effects for black SpEd students as a result of policies limiting SpEd enrollment. This points to the fact that students who require services greatly benefit from them in the long-run. In contrast, those who are misclassified for SpEd do worse in the long-run. SpEd is an intensive and costly intervention, and it is important both to schools and students themselves that individuals be appropriately placed in SpEd. It is therefore important to consider more broadly the eligibility criteria for SpEd services, particularly for low-performing black students. Finally, whether SpEd students are appropriately served also has important implications for all general education students in the classroom. This highlights the fact that both SpEd and general education students' outcomes should be considered when making SpEd policy-related decisions.

Figures and Tables

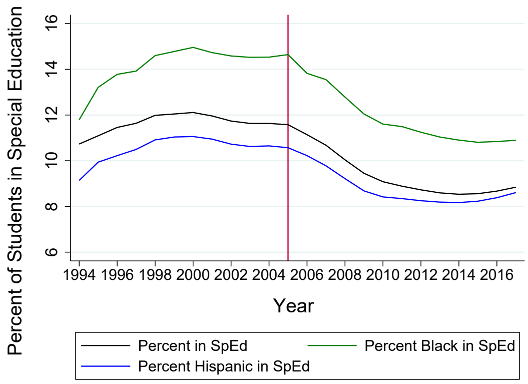
Figure 2.1: Percent of Students in Special Education



Data for this figure come from the Common Core of Data through the National Center for Education Statistics. Averages represent statewide population averages, that is, the number of students in a state in special education divided by the total number of students in that state.

Figure 2.2: Trends in Special Education Statewide

(a) Special Education Rate by Race



(b) Disproportionality Rate for black and Hispanic Students

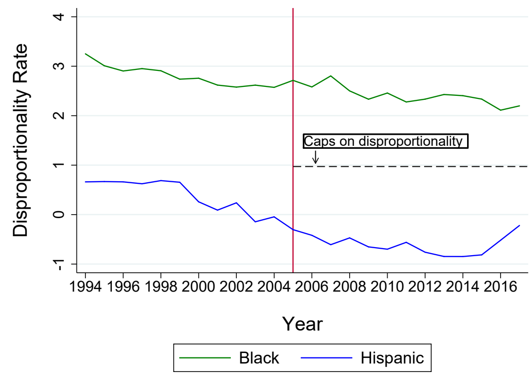
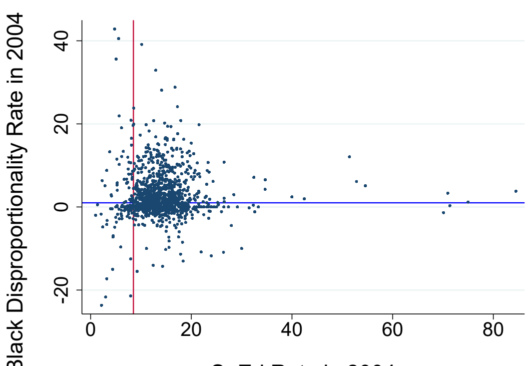


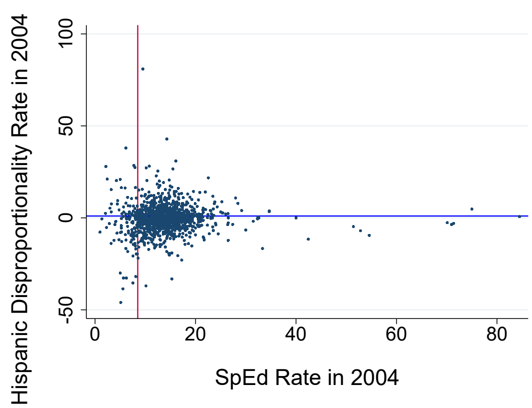
Figure (a) plots the percent of students in special education in Texas by race. Figure (b) plots the average disproportionality rate for black and Hispanic students. The disproportionality rate is measured as the percent of black or Hispanic students in special education minus the percent of black or Hispanic students in a given district.

Figure 2.3: Treatment Variation

(a) District Level Treatment Variation for black Students



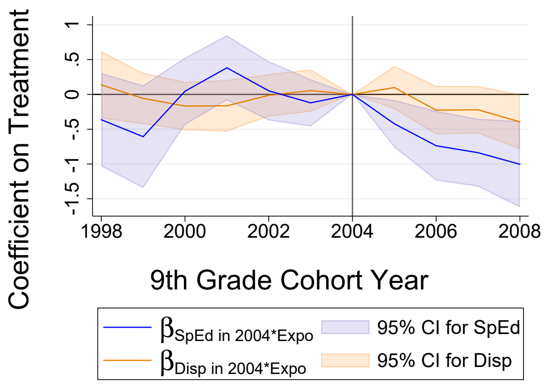
(b) District Level Treatment Variation for Hispanic Students



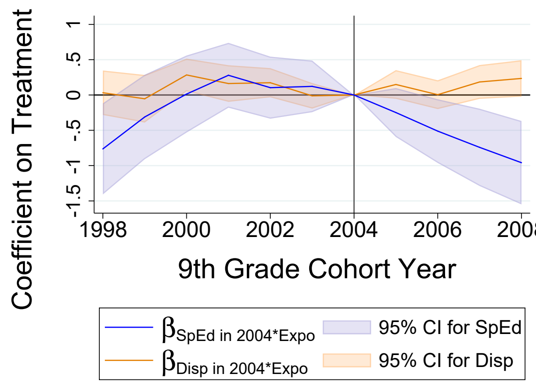
Each dot of the scatter plots represents a district. The x-axis is the 2004 district level SpEd rate, and the y-axis is the 2004 district level black or Hispanic disproportionality rate. The correlation coefficient in Figure 2.3a is 0.0022 and in Figure 2.3b is 0.0310***.

Figure 2.4: Event Study for Special Education Students

(a) G9 SpEd Status for black SpEd Students



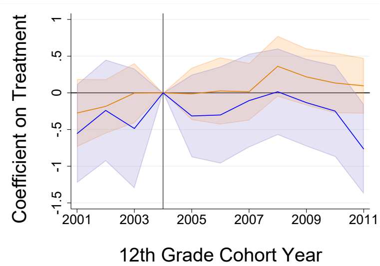
(b) G9 SpEd Status for Hispanic SpEd Students



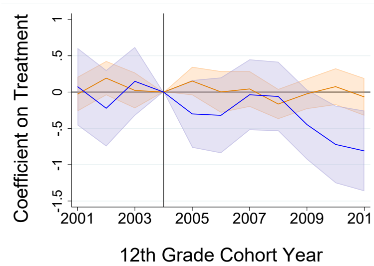
In each graph, the series in blue is the average black SpEd rate in each district in 2004 interacted with indicators for each 9th grade cohort year. The series in orange is the average black or Hispanic disproportionality rate in each district in 2004 interacted with indicators for each 9th grade cohort year. Regressions include controls for individual and district-cohort level race, ESL, FRL, and gifted status, along with district and cohort fixed effects.

Figure 2.5: Event Study for Special Education Students

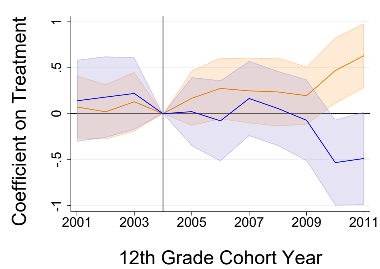
(a) HS Completion for black SpEd Students



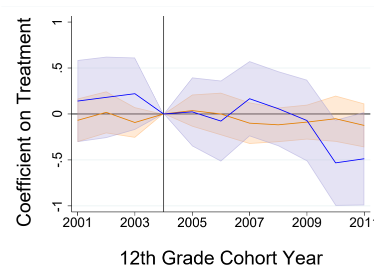
(b) HS Completion for Hispanic SpEd Students



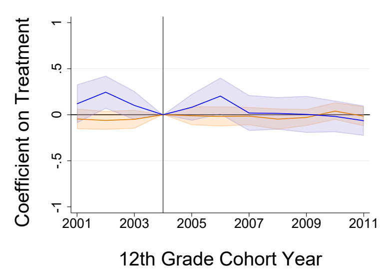
(c) Enroll College for black SpEd Students



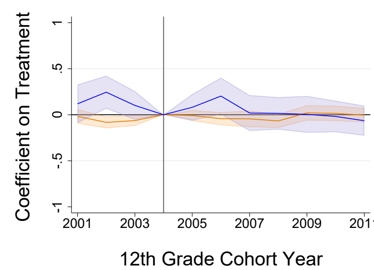
(d) Enroll College for Hispanic SpEd Students



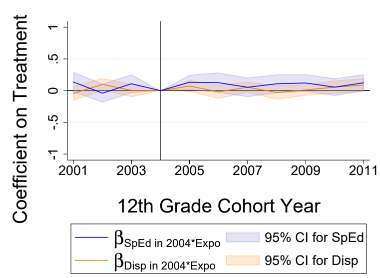
(e) Associate's Degree for black SpEd Students



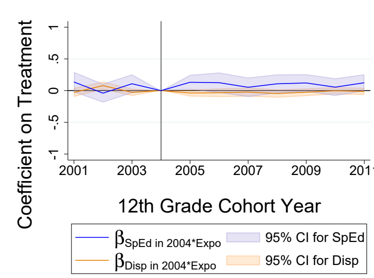
(f) Associate's Degree for Hispanic SpEd Students



(g) Bachelor's Degree for black SpEd Students



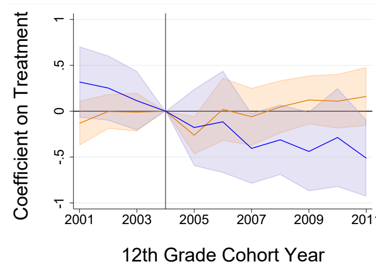
(h) Bachelor's Degree for Hispanic SpEd Students



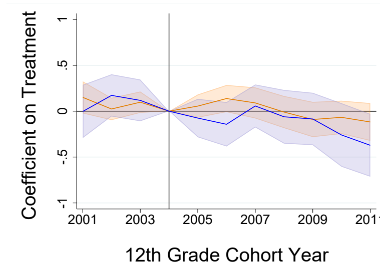
Regressions include controls for individual and district-cohort level race, ESL, FRL, and gifted status, along with district and cohort fixed effects.

Figure 2.6: Event Study for General Education Students

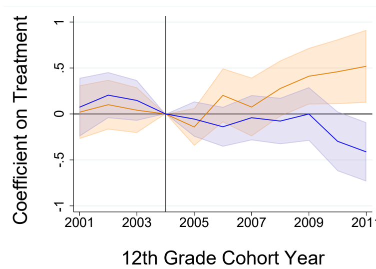
(a) HS Completion for black General Education Students



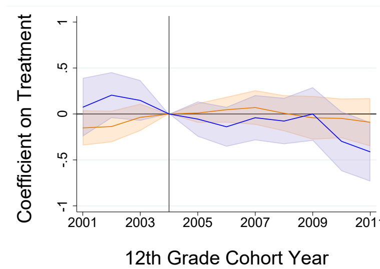
(b) HS Completion for Hispanic General Education Students



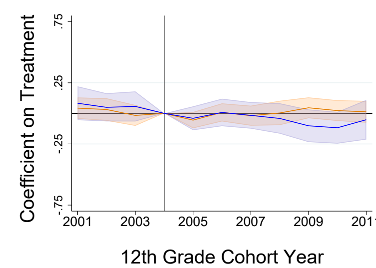
(c) Enroll College for black General Education Students



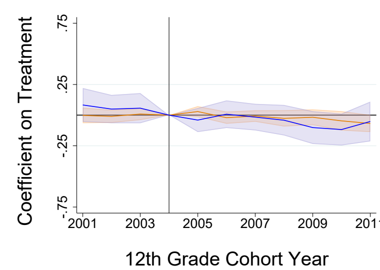
(d) Enroll College for Hispanic General Education Students



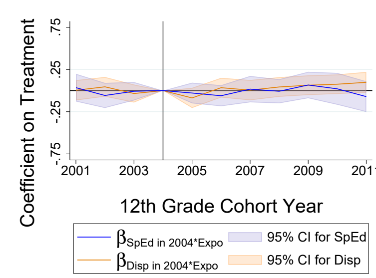
(e) Associate's Degree for black General Education Students



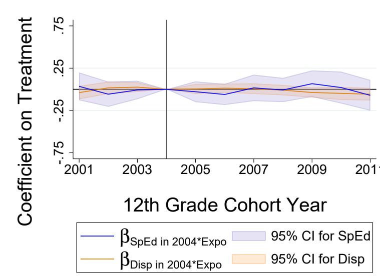
(f) Associate's Degree for Hispanic General Education Students



(g) Bachelor's Degree for black General Education Students



(h) Bachelor's Degree for Hispanic General Education Students

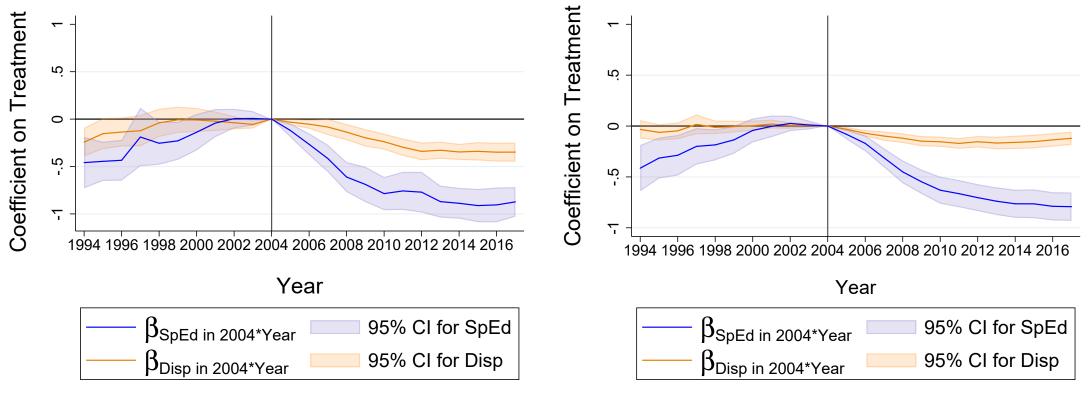


Regressions include controls for individual and district-cohort level race, ESL, FRL, and gifted status, along with district and cohort fixed effects.

Figure 2.7: Event Study for Aggregate SpEd and General Education Enrollment

(a) SpEd Status for SpEd and General Education Black Students

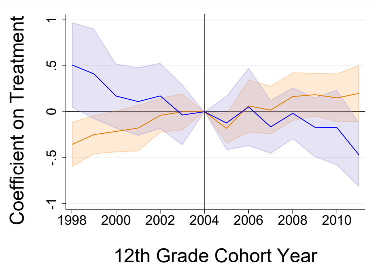
(b) SpEd Status for SpEd and General Education Hispanic Students



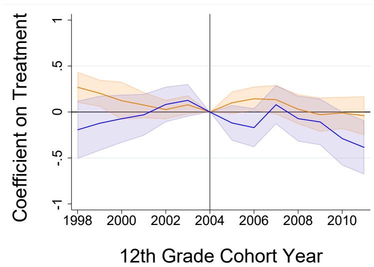
The series in blue is the average black SpEd rate in each district in 2004 interacted with indicators for each year. The series in orange is the average black disproportionality rate in each district in 2004 interacted with indicators for each year. Regressions include the SpEd cap and the disproportionality cap, along with controls for individual, district, and grade level race, ESL, FRL, Title I status, and gifted status, and district, year, and grade fixed effects.

Figure 2.8: Event Study for Aggregate SpEd and General Education Enrollment

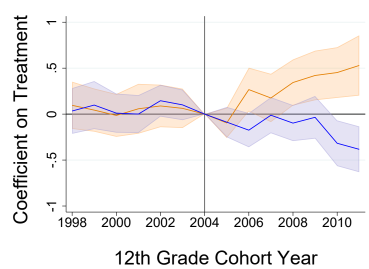
(a) HS Completion for black SpEd and General Educ.



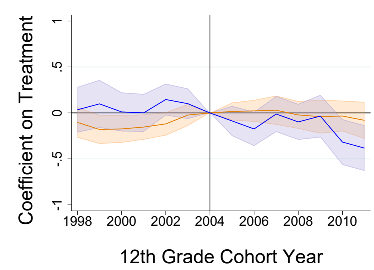
(b) HS Completion for Hispanic SpEd and General Educ.



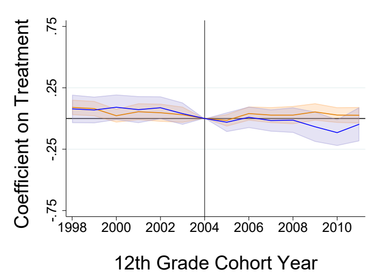
(c) College Enrollment for black SpEd and General Educ.



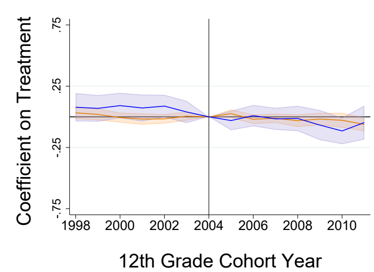
(d) College Enrollment for Hispanic SpEd and General Educ.



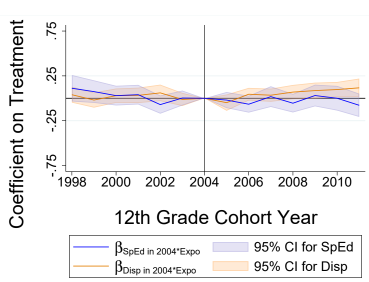
(e) Associate's Degree for black SpEd and General Educ.



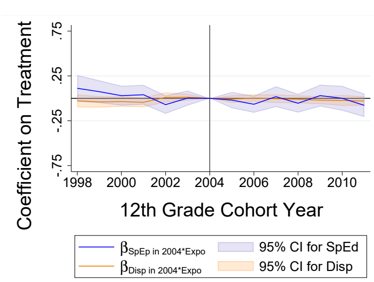
(f) Associate's Degree for Hispanic SpEd and General Educ.



(g) Bachelor's Degree for black SpEd and General Educ.



(h) Bachelor's Degree for Hispanic SpEd and General Educ.



Regressions include the SpEd cap and the disproportionality cap, along with controls for individual, district, and grade level race, ESL, FRL, Title I status, and gifted status, and district, year, and grade fixed effects.

Table 2.1 Descriptive Statistics for Grades K through 12

	All Students	Black Students	Hispanic Students	SpEd Students
	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Covariates				
Male	0.513	0.510	0.512	0.670
FRL	0.545	0.680	0.769	0.607
ESL	0.031	0.003	0.059	0.036
Title I	0.564	0.608	0.752	0.548
Gifted	0.078	0.047	0.057	0.010
White	0.374	·	·	0.397
Black	0.136	·	·	0.171
Hispanic	0.454	·	·	0.415
Other	0.036	·	·	0.018
SpEd Rate	0.103	0.129	0.094	
Standardized Math	0.030 (0.981)	-0.390 (1.033)	-0.142 (0.989)	-0.370 (1.097)
Standardized Reading	0.028 (0.983)	-0.277 (1.048)	-0.194 (1.021)	-0.374 (1.134)
Long-run Outcomes				
High School Diploma	0.652	0.596	0.590	0.591
Enroll College	0.446	0.390	0.359	0.302
Obtain Associate's	0.062	0.040	0.061	0.036
Obtain Bachelor's	0.146	0.089	0.078	0.054

Data come from the Texas Schools Project. Numbers represent the proportion of students in each demographic category, on a 0 to 1 scale. Standard deviations are omitted for averages of binary outcomes. There are about 14.4 million individuals from 1994 to 2018, 1,329 districts and 9,604 schools covering grades K to 12. FRL is an indicator for receiving free or reduced-price lunch. ESL stands for English as a Second Language program (now commonly referred to as English Language Learners). Gifted is a separately defined category from Special Education in Texas, and is a program for high achieving students. Math and reading test scores are standardized to mean 0, standard deviation 1 by test subject, type (accommodated or otherwise modified), grade, and year. High School diploma is an indicator for graduating from high school within 2 years of expected high school graduation, and conditional on a student observed in the data in grade 9. Indicators for enrolling in college and obtaining an Associate's or Bachelor's degree are censored to 6 years after expected high school graduation, but not conditional on high school diploma.

Table 2.2 Difference in Means Between Districts Above and Below 8.5% SpEd Cap in 2004

	Less than 8.5%	Greater than 8.5%	Difference
Male	0.501	0.516	-0.014***
White	0.256	0.581	-0.325***
Black	0.247	0.093	0.154***
Hispanic	0.465	0.314	0.151***
Other	0.032	0.012	-0.019***
Econ Disadvantage	0.559	0.542	0.018
ESL	0.019	0.022	-0.002
Title I	0.624	0.681	-0.058*
Gifted	0.056	0.079	-0.023***
Standardized Math	-0.313	-0.051	-0.261***
Standardized Reading	-0.200	-0.009	-0.191***
N	106	1,018	

*** p<0.01, ** p<0.05, * p<0.1 This table provides differences in characteristics across districts in 2004 for grades 3 through 8. We compare districts with greater than 8.5% of students in SpEd in 2004 to those with fewer than 8.5% of students in SpEd in 2004.

Table 2.3 Difference in Means Between Districts Above and Below 1pp Disp Cap in 2004

	Black Disp Rate			**Hispanic Disp Rate**		
	Less than 1pp	Greater than 1pp	Difference	Less than 1pp	Greater than 1pp	Difference
Male	0.512	0.514	-0.001	0.514	0.511	0.003
White	0.511	0.522	-0.011	0.506	0.536	-0.030
Black	0.074	0.170	-0.096***	0.152	0.072	0.080***
Hispanic	0.401	0.290	0.111***	0.328	0.371	-0.043***
Other	0.014	0.018	-0.004***	0.014	0.020	-0.007**
Econ Disadvantage	0.550	0.540	0.010	0.576	0.491	0.085***
ESL	0.023	0.019	0.004*	0.022	0.020	0.0002
Title I	0.677	0.664	0.013	0.685	0.644	0.041*
Gifted	0.092	0.100	-0.008***	0.071	0.079	-0.007**
Special Ed	0.120	0.125	-0.005	0.124	0.120	0.004
Standardized Math	-0.117	-0.089	-0.028	-0.155	-0.007	-0.148***
Standardized Reading	-0.057	0.035	-0.028	-0.097	0.046	-0.144***
N	557	567		703	421	

*** p<0.01, ** p<0.05, * p<0.1 This table provides descriptive statistics in 2004 for grades 3 to 8 in districts with less than a 1% disproportionality rate for black/Hispanic students and districts with greater than a 1% disproportionality rate for black/Hispanic students.

Table 2.4 Effect of Policy on students in SpEd as of 5th Grade Prior to Policy Implementation

	Black Students			Hispanic Students		
SpEd Status G9th	(1)	(2)	(3)	(4)	(5)	(6)
$SpEd_{d,2004} \times Post$	-0.2950***	-0.2985***	-0.2548***	-0.2938***	-0.2296***	-0.2349***
	(0.061)	(0.055)	(0.058)	(0.078)	(0.080)	(0.079)
$Disp_{d,2004} \times Post$	-0.0184	-0.0550	-0.0819**	0.0105	0.0189	0.0268
	(0.034)	(0.038)	(0.036)	(0.029)	(0.027)	(0.027)
Mean Dept Var	0.777	0.777	0.777	0.761	0.761	0.761
Observations	73,047	73,047	73,047	155,225	155,225	155,225
High School Diploma						
$SpEd_{d,2004} \times Exposure$	-0.0294	-0.0157	-0.0208	-0.0947***	-0.0873***	-0.0979***
	(0.031)	(0.042)	(0.038)	(0.034)	(0.032)	(0.032)
$Disp_{d,2004} \times Exposure$	0.0327	0.0677**	0.0428*	-0.0150	-0.0327**	-0.0161
	(0.024)	(0.028)	(0.024)	(0.013)	(0.013)	(0.013)
Mean Dept Var	0.581	0.581	0.581	0.575	0.575	0.575
Observation s	76,640	76,640	76,640	160,614	160,614	160,614
College Enrollment						
$SpEd_{d,2004} \times Exposure$	0.0422	0.0559	0.0345	-0.0591**	-0.0503*	-0.0803***
	(0.029)	(0.043)	(0.036)	(0.027)	(0.030)	(0.029)
$Disp_{d,2004} \times Exposure$	0.0484***	0.0842***	0.0645***	-0.0123	-0.0337**	-0.0141
	(0.017)	(0.022)	(0.018)	(0.011)	(0.014)	(0.014)
Mean Dept Var	0.267	0.267	0.267	0.247	0.247	0.247
Observations	76,640	76,640	76,640	160,614	160,614	160,614
Associate's Degree						
$SpEd_{d,2004} \times Exposure$	-0.0153	-0.0150	-0.0232**	-0.0207**	-0.0192**	-0.0246***
	(0.010)	(0.010)	(0.011)	(0.009)	(0.008)	(0.009)
$Disp_{d,2004} \times Exposure$	-0.0036	0.0014	0.0046	0.0039	0.0001	0.0056
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Mean Dept Var	0.020	0.020	0.020	0.030	0.030	0.030
Observations	76,640	76,640	76,640	160,614	160,614	160,614
Bachelor's Degree						
$SpEd_{d,2004} \times Exposure$	-0.0061	-0.0070	-0.0083	0.0095	0.0097	0.0060
	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)
$Disp_{d,2004} \times Exposure$	-0.0007	0.0080	0.0043	0.0009	-0.0061**	-0.0022
	(0.005)	(0.005)	(0.004)	(0.003)	(0.003)	(0.003)
Mean Dept Var	0.023	0.023	0.023	0.021	0.021	0.021
Observations	76,640	76,640	76,640	160,614	160,614	160,614
Associate's or Bachelor's						
$SpEd_{d,2004} \times Exposure$	-0.0214**	-0.0220*	-0.0315***	-0.0112	-0.0095	-0.0186*
	(0.011)	(0.011)	(0.012)	(0.011)	(0.010)	(0.010)
$Disp_{d,2004} \times Exposure$	-0.0043	0.0093	0.0089	0.0048	-0.0061	0.0034
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)
Mean Dept Var	0.043	0.043	0.043	0.051	0.051	0.051
Observations	76,640	76,640	76,640	160,614	160,614	160,614
Individual Controls		X	X		X	X
District-Cohort Controls			X			X

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. All specifications include cohort fixed effects and district fixed effects. Regressions are run on students in SpEd as of 5th grade prior to policy implementation. SpEd status is measured 4 years after 5th grade, to correspond to expected 9th grade. We control for disability type, SpEd setting, ESL, FRL, Title I, and gifted status as of 5th grade in our model predicting SpEd status. We control for fraction of time in ESL, FRL, Title I, and gifted for the long-run outcomes. High school diploma, college enrollment, and associate's and bachelor's degree attainment are conditional on being observed in Texas public schools as of 9th grade. Long-run outcomes are censored such that individuals have 2 years after expected high school completion to earn a high school diploma and 6 years after expected high school completion to enroll in college and obtain a degree. These regressions include kindergarten-cohorts from 1989 to 1999, to allow for cohorts old enough to be observed in the data in 5th grade when the data began, and young enough to be in school in 5th grade prior to policy implementation in the 2005 school year.

Table 2.5 Effect of Policy on Students in GE as of 5th Grade Prior to Policy Implementation

	Black Students			Hispanic Students		
High School Diploma	(1)	(2)	(3)	(4)	(5)	(6)
$SpEd_{d,2004} \times Exposure$	-0.0371	-0.0738**	-0.0875***	-0.0431*	-0.0003	-0.0501**
	(0.025)	(0.037)	(0.024)	(0.022)	(0.022)	(0.023)
$Disp_{d,2004} \times Exposure$	0.0255	0.0534**	0.0316*	-0.0455***	-0.0605***	-0.0260**
	(0.017)	(0.024)	(0.018)	(0.012)	(0.013)	(0.012)
Mean Dept Var	0.662	0.662	0.662	0.651	0.651	0.651
Observations	359,892	359,892	359,892	1,032,793	1,032,793	1,032,793
College Enrollment						
$SpEd_{d,2004} \times Exposure$	-0.0220	-0.0595	-0.0859***	-0.0537**	-0.0059	-0.0556**
	(0.027)	(0.039)	(0.030)	(0.024)	(0.027)	(0.025)
$Disp_{d,2004} \times Exposure$	0.0605***	0.0892***	0.0746***	-0.0133	-0.0309**	-0.0019
	(0.017)	(0.023)	(0.022)	(0.014)	(0.014)	(0.016)
Mean Dept Var	0.476	0.476	0.476	0.431	0.431	0.431
Observations	359,892	359,892	359,892	1,032,793	1,032,793	1,032,793
Associate's Degree						
$SpEd_{d,2004} \times Exposure$	-0.0156**	-0.0189**	-0.0196**	-0.0163*	-0.0072	-0.0191**
	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
$Disp_{d,2004} \times Exposure$	0.0022	0.0054	0.0029	-0.0130**	-0.0150***	-0.0088*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Mean Dept Var	0.048	0.048	0.048	0.074	0.074	0.074
Observations	359,892	359,892	359,892	1,032,793	1,032,793	1,032,793
Bachelor's Degree						
$SpEd_{d,2004} \times Exposure$	-0.0018	-0.0296**	-0.0360***	0.0010	0.0087	0.0013
	(0.009)	(0.012)	(0.011)	(0.011)	(0.012)	(0.011)
$Disp_{d,2004} \times Exposure$	0.0074	0.0230**	0.0140**	-0.0041	-0.0174***	-0.0090*
	(0.006)	(0.009)	(0.006)	(0.005)	(0.007)	(0.005)
Mean Dept Var	0.112	0.112	0.112	0.095	0.095	0.095
Observations	359,892	359,892	359,892	1,032,793	1,032,793	1,032,793
Associate's or Bachelor's						
$SpEd_{d,2004} \times Exposure$	-0.0173	-0.0485***	-0.0556***	-0.0153	0.0015	-0.0178
	(0.011)	(0.015)	(0.014)	(0.014)	(0.014)	(0.013)
$Disp_{d,2004} \times Exposure$	0.0096	0.0284**	0.0169**	-0.0172***	-0.0324***	-0.0178***
	(0.007)	(0.011)	(0.008)	(0.006)	(0.006)	(0.007)
Mean Dept Var	0.160	0.160	0.160	0.169	0.169	0.169
Observations	359,892	359,892	359,892	1,032,793	1,032,793	1,032,793
Individual Controls		X	X		X	X
District-Cohort Controls			X			X

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Robust standard errors are clustered at the district level. All specifications include cohort fixed effects and district fixed effects. Regressions are run on students in general education as of 5th grade prior to the policy (in kindergarten-cohorts 1989 to 1999). Outcome variables and controls are as defined in Table 2.4, with the exception of a control for disability type and setting, since general education students do not have a disability label.

Table 2.6 Effect of SpEd and Disproportionality Indicators for All Students in GE in 5th Grade Prior to Policy Implementation

	All Races		
High School Diploma	(1)	(2)	(3)
$SpEd_{d,2004} \times Exposure$	-0.0563***	-0.0203*	-0.0464***
	(0.012)	(0.012)	(0.011)
$Disp\,Black_{d,2004} \times Exposure$	-0.0100	0.0149	-0.0008
	(0.014)	(0.014)	(0.012)
$Disp\,Hispanic_{d,2004} \times Exposure$	-0.0262***	-0.0529***	-0.0354***
	(0.007)	(0.009)	(0.008)
Mean Dept Var	0.712	0.712	0.712
Observations	2,630,937	2,630,937	2,630,937
College Enrollment			
$SpEd_{d,2004} \times Exposure$	-0.0403**	0.0024	-0.0301*
	(0.018)	(0.020)	(0.017)
$Disp\,Black_{d,2004} \times Exposure$	-0.0112	0.0199	0.0014
	(0.018)	(0.016)	(0.015)
$Disp\,Hispanic_{d,2004} \times Exposure$	-0.0194*	-0.0529***	-0.0410***
	(0.011)	(0.012)	(0.013)
Mean Dept Var	0.523	0.523	0.523
Observations	2,630,937	2,630,937	2,630,937
Associate's Degree			
$SpEd_{d,2004} \times Exposure$	-0.0250***	-0.0215***	-0.0255***
	(0.005)	(0.005)	(0.005)
$Disp\,Black_{d,2004} \times Exposure$	-0.0023	0.0004	-0.0006
	(0.005)	(0.005)	(0.005)
$Disp\,Hispanic_{d,2004} \times Exposure$	-0.0067**	-0.0097***	-0.0066**
	(0.003)	(0.004)	(0.003)
Mean Dept Var	0.073	0.073	0.073
Observations	2,630,937	2,630,937	2,630,937
Bachelor's Degree			
$SpEd_{d,2004} \times Exposure$	-0.0072	0.0185	0.0053
	(0.011)	(0.012)	(0.009)
$Disp\,Black_{d,2004} \times Exposure$	-0.0182**	0.0020	-0.0089
	(0.009)	(0.007)	(0.006)
$Disp\,Hispanic_{d,2004} \times Exposure$	-0.0068	-0.0323***	-0.0290***
	(0.007)	(0.007)	(0.005)
Mean Dept Var	0.172	0.172	0.172
Observations	2,630,937	2,630,937	2,630,937
Associate's or Bachelor's			
$SpEd_{d,2004} \times Exposure$	-0.0321***	-0.0030	-0.0202**
	(0.011)	(0.012)	(0.010)
$Disp\,Black_{d,2004} \times Exposure$	-0.0205**	0.0024	-0.0095
	(0.010)	(0.009)	(0.007)
$Disp\,Hispanic_{d,2004} \times Exposure$	-0.0135**	-0.0420***	-0.0356***
	(0.006)	(0.006)	(0.006)
Mean Dept Var	0.246	0.246	0.246
Observations	2,630,937	2,630,937	2,630,937
Individual Demographic Controls		X	X
District-Cohort Level Controls			X

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. See Table 2.5 for details on outcome variables, controls, and cohort restrictions.

Table 2.7 Aggregate Effect of Policy on All SpEd and General Education Students

	Black Students			Hispanic Students		
SpEd Status	(1)	(2)	(3)	(4)	(5)	(6)
$SpEd_{d,2004} \times Post$	-0.4951***	-0.4038***	-0.4051***	-0.3324***	-0.3254***	-0.3020***
	(0.100)	(0.075)	(0.068)	(0.050)	(0.050)	(0.049)
$Disp_{d,2004} \times Post$	-0.0753*	-0.0661	-0.1219***	-0.1000***	-0.1291***	-0.1404***
	(0.043)	(0.043)	(0.045)	(0.022)	(0.029)	(0.024)
Mean Dept Var	0.129	0.129	0.129	0.094	0.094	0.094
Observations	13,982,403	13,982,403	13,982,403	46,738,978	46,738,978	46,738,978
Individual Controls		X	X		X	X
District and Grade Controls			X			X
High School Diploma	(1)	(2)	(3)	(4)	(5)	(6)
$SpEd_{d,2004} \times Exposure$	-0.0468***	-0.0862***	-0.0699***	-0.0175	0.0273*	-0.0164
	(0.013)	(0.016)	(0.015)	(0.014)	(0.015)	(0.014)
$Disp_{d,2004} \times Exposure$	0.0371***	0.0568***	0.0275***	-0.0611***	-0.0606***	-0.0283***
	(0.010)	(0.013)	(0.009)	(0.007)	(0.007)	(0.009)
Mean Dept Var	0.580	0.580	0.580	0.575	0.575	0.575
Observations	903,756	903,756	903,756	2,528,519	2,528,519	2,528,519
College Enrollment						
$SpEd_{d,2004} \times Exposure$	-0.0019	-0.0766**	-0.0620**	-0.0548***	0.0012	-0.0472**
	(0.022)	(0.031)	(0.026)	(0.020)	(0.023)	(0.020)
$Disp_{d,2004} \times Exposure$	0.0620***	0.1021***	0.0706***	-0.0089	-0.0270**	0.0030
	(0.012)	(0.017)	(0.015)	(0.010)	(0.011)	(0.012)
Mean Dept Var	0.390	0.390	0.390	0.359	0.359	0.359
Observations	692,711	692,711	692,711	1,841,412	1,841,412	1,841,412
Associate's Degree						
$SpEd_{d,2004} \times Exposure$	-0.0156***	-0.0231***	-0.0211***	-0.0181**	-0.0084	-0.0207**
	(0.005)	(0.006)	(0.006)	(0.009)	(0.009)	(0.009)
$Disp_{d,2004} \times Exposure$	-0.0031	0.0017	-0.0013	-0.0104***	-0.0119***	-0.0066*
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Mean Dept Var	0.040	0.040	0.040	0.061	0.061	0.061
Observations	692,711	692,711	692,711	1,841,412	1,841,412	1,841,412
Bachelor's Degree						
$SpEd_{d,2004} \times Exposure$	-0.0080	-0.0509***	-0.0393***	-0.0120	0.0027	-0.0077
	(0.009)	(0.014)	(0.009)	(0.008)	(0.010)	(0.008)
$Disp_{d,2004} \times Exposure$	0.0052	0.0287***	0.0125**	0.0012	-0.0163***	-0.0020
	(0.005)	(0.008)	(0.005)	(0.004)	(0.006)	(0.004)
Mean Dept Var	0.089	0.089	0.089	0.078	0.078	0.078
Observations	692,711	692,711	692,711	1,841,412	1,841,412	1,841,412
Associate's or Bachelor's						
$SpEd_{d,2004} \times Exposure$	-0.0236**	-0.0740***	-0.0605***	-0.0301***	-0.0057	-0.0284***
	(0.010)	(0.015)	(0.011)	(0.010)	(0.011)	(0.009)
$Disp_{d,2004} \times Exposure$	0.0020	0.0304***	0.0113*	-0.0092**	-0.0281***	-0.0087*
	(0.005)	(0.009)	(0.007)	(0.004)	(0.005)	(0.004)
Mean Dept Var	0.129	0.129	0.129	0.140	0.140	0.140
Observations	692,711	692,711	692,711	1,841,412	1,841,412	1,841,412
Individual Demographic Controls		X	X		X	X
District-Cohort Level Controls			X			X

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. SpEd status is measured as an indicator for being in SpEd between kindergarten and 12th grade. The model predicting SpEd status includes district, year, and grade fixed effects, as well as controls for gender, age as of sept 1, ESL, FRL, Title I, and gifted status at the individual, grade, and district levels. Long-run controls, outcome variables, and cohort restrictions are as defined in Table 2.5. Long-run specifications include cohort fixed effects and district fixed effects.

Table 2.8 District-Level Changes in the Composition of Students Who Lose SpEd at Expected 9th Grade, Given SpEd at 5th Grade

	Black Students				
	Male	ESL	FRL	Math	Reading
	(1)	(2)	(3)	(4)	(5)
$SpEd_{d,2004}$	0.1662	0.0073	-0.0992	-0.0861	0.0224
	(0.156)	(0.010)	(0.122)	(0.415)	(0.451)
$Disp_{d,2004}$	0.0169	0.0106	-0.0169	0.3385	0.1717
	(0.086)	(0.013)	(0.063)	(0.297)	(0.264)
Mean Dept Var	-0.047	0.001	-0.049	0.308	0.275
Observations	79,207	79,207	79,207	75,516	75,175
	SLD	Speech	ED	OHI	Autism
	(1)	(2)	(3)	(4)	(5)
$SpEd_{d,2004}$	-0.3504*	-0.2612	-0.0140	0.1182	0.0652**
	(0.212)	(0.314)	(0.147)	(0.085)	(0.030)
$Disp_{d,2004}$	-0.1275	0.1508	0.0482	0.1107***	0.0196
	(0.139)	(0.139)	(0.059)	(0.041)	(0.015)
Mean Dept Var	-0.083	0.223	-0.018	-0.027	-0.011
Observations	79,207	79,207	79,207	79,207	79,207

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. Regressions include cohort fixed effects. We regress the district-level difference between the percent of students with a particular attribute not in SpEd at grade 9, given SpEd at grade 5 and the percent of students with the attribute in SpEd at grade 5. Treatment variables are now replaced with the measure of the 2004 district SpEd or disproportionality rate (not interacted with exposure).

Table 2.9 Impact of Treatment Variables on Pre-Determined Characteristics of 5th Grade SpEd Students Prior to Policy Implementation

	Black Students						
	Male	ESL	FRL	Title I	Gifted	Math	Reading
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$SpEd_{d,2004} \times Post$	-0.0902***	-0.0057***	0.0241	0.2354***	-0.0145***	0.5067***	0.5917***
	(0.015)	(0.001)	(0.017)	(0.019)	(0.002)	(0.118)	(0.129)
$Disp_{d,2004} \times Post$	-0.0110	-0.0010	0.0639***	0.0613***	-0.0012	-0.2756***	-0.2599***
	(0.009)	(0.001)	(0.011)	(0.012)	(0.002)	(0.070)	(0.073)
Observations	86,644	86,644	86,644	86,644	86,644	21,207	20,139

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. Regressions include district and cohort fixed effects. We regress our treatment variables on pre-policy student characteristics, to predict which types of students are more or less likely to be affected by each of the treatment variables. Male, ESL, and FRL, Math, and Reading are measured as of 5th grade.

Table 2.10 District Switching or Leaving for SpEd Sample

	All Students	Black Students		Hispanic Students	
	Switch to Compliance Dist	Switch Districts	Enrolled in G9	Switch Districts	Enrolled in G9
	(1)	(2)	(3)	(4)	(5)
$SpEd_{d,2004} \times Expo$	0.2316	0.0712	-0.5041	-0.0171	0.1184
	(0.187)	(0.063)	(0.354)	(0.037)	(0.258)
$Disp\,Black_{d,2004} \times Expo$	-0.3213	0.0344	-0.1138		
	(0.201)	(0.040)	(0.181)		
$Disp\,Hispanic_{d,2004} \times Expo$	0.0675			-0.0326	-0.0645
	(0.182)			(0.022)	(0.137)
Mean Dept Var		0.367	0.748	0.292	0.797
Observations		86,643	8,409	179,451	15,493

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. Regressions include district and cohort fixed effects, along with individual-level and cohort-district controls. See Table 2.4 for full set of controls. In column (1) we estimate the likelihood that a student moves to a district that was already in compliance with state standards prior to policy implementation. Ins column (2) and (4) we estimate for our sample of black and Hispanic SpEd students, respectively, the likelihood of switching districts between 5th and 9th grade. In columns (3) and (5) we estimate the likelihood of being enrolled as of 9th for black and Hispanic SpEd students, respectively.

Table 2.11 Robustness Checks for Effect of Policy on SpEd students

	Black Students				Hispanic Students			
	Original	Time trends	Sch FE	Set Below to 0	Original	Time trends	Sch FE	Set Below to 0
SpEd Status G9	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$SpEd_{d,2004} \times Expo$	-0.2548***	-0.2656***	-0.1790***	-0.2445***	-0.2349***	-0.2363***	-0.1722*	-0.1991***
	(0.058)	(0.059)	(0.054)	(0.047)	(0.069)	(0.079)	(0.091)	(0.057)
$Disp_{d,2004} \times Expo$	-0.0819**	-0.0896**	-0.0701	-0.0921**	0.0268	0.0260	0.0133	-0.0016
	(0.036)	(0.035)	(0.044)	(0.037)	(0.027)	(0.026)	(0.031)	(0.048)
High School								
$SpEd_{d,2004} \times Expo$	-0.0208	-0.0236	-0.0838*	-0.0107	-0.0979***	-0.1048***	-0.1650***	-0.1008***
	(0.038)	(0.038)	(0.044)	(0.035)	(0.032)	(0.031)	(0.037)	(0.027)
$Disp_{d,2004} \times Expo$	0.0482**	0.0417*	0.0529*	0.0418*	-0.0161	-0.0175	-0.0114	-0.0534**
	(0.024)	(0.023)	(0.029)	(0.025)	(0.013)	(0.013)	(0.017)	(0.025)
College								
$SpEd_{d,2004} \times Expo$	0.0345	0.0384	-0.0041	0.0389	-0.0803***	-0.0830***	-0.1306***	-0.0830***
	(0.036)	(0.036)	(0.042)	(0.033)	(0.029)	(0.030)	(0.033)	(0.024)
$Disp_{d,2004} \times Expo$	0.0645***	0.0697***	0.0706***	0.0640***	-0.0141	-0.0154	0.0019	0.0023
	(0.018)	(0.019)	(0.022)	(0.018)	(0.014)	(0.014)	(0.017)	(0.030)
Associate's Degree								
$SpEd_{d,2004} \times Expo$	-0.0232**	-0.0199*	-0.0258*	-0.0195**	-0.0192**	-0.0224**	-0.0314***	-0.0197**
	(0.011)	(0.011)	(0.014)	(0.010)	(0.008)	(0.009)	(0.009)	(0.008)
$Disp_{d,2004} \times Expo$	0.0046	0.0074*	0.0049	0.0039	0.0001	0.0061	0.0098**	0.0173
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.011)
Bachelor's Degree								
$SpEd_{d,2004} \times Expo$	-0.0083	-0.0073	-0.0125	-0.0093	0.0060	0.0061	-0.0018	0.0024
	(0.008)	(0.008)	(0.009)	(0.007)	(0.007)	(0.007)	(0.009)	(0.006)
$Disp_{d,2004} \times Expo$	0.0043	0.0047	0.0035	0.0048	-0.0022	-0.0022	0.0013	0.0005
	(0.004)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)	(0.004)	(0.007)

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. See Table 2.4 for full list of controls. In columns (1) and (5) we have our original main results from columns (3) and (6) of Table 2.4 for black and Hispanic students, respectively. In columns (2) and (6), we include indicator variables for each cohort year interacted with baseline demographics, including gender, ESL, FRL, Title I, gifted, and race. In columns (3) and (7) we replace district-level fixed effects with school-level fixed effects. And in columns (4) and (8) we set district SpEd and disproportionality rates to 0 if they are already below the thresholds, and to the distance to the cutoff if they were above the cutoff (rather than using a continuous measure of treatment).

Table 2.12 Robustness Checks for Effect of Policy on General Education students

	Black Students				Hispanic Students			
	Original	Time trends	Sch FE	Set Below to 0	Original	Time trends	Sch FE	Set Below to 0
High School	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$SpEd_{d,2004} \times Exposure$	-0.0875***	-0.0931***	-0.0671**	-0.0656***	-0.0501**	-0.0688***	-0.0740***	-0.0559***
	(0.024)	(0.026)	(0.032)	(0.023)	(0.023)	(0.023)	(0.025)	(0.020)
$Disp_{d,2004} \times Exposure$	0.0316*	0.0260	0.0427*	0.0324*	-0.0260**	-0.0291***	-0.0281**	-0.0455**
	(0.018)	(0.017)	(0.026)	(0.020)	(0.012)	(0.011)	(0.012)	(0.021)
College Enrollment								
$SpEd_{d,2004} \times Exposure$	-0.0859***	-0.0819***	-0.0597**	-0.0715***	-0.0556**	-0.0604**	-0.1024***	-0.0410**
	(0.030)	(0.029)	(0.028)	(0.026)	(0.025)	(0.026)	(0.028)	(0.020)
$Disp_{d,2004} \times Exposure$	0.0746***	0.0669***	0.0763***	0.0792***	-0.0019	-0.0135	0.0167	-0.0039
	(0.022)	(0.021)	(0.023)	(0.024)	(0.016)	(0.016)	(0.016)	(0.023)
Associate's Degree								
$SpEd_{d,2004} \times Exposure$	-0.0196**	-0.0146	-0.0212**	-0.0153*	-0.0191**	-0.0199**	-0.0247**	-0.0207**
	(0.009)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.011)	(0.009)
$Disp_{d,2004} \times Exposure$	0.0029	0.0050	0.0043	0.0040	-0.0088*	-0.0102**	-0.0053	-0.0082
	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.004)	(0.009)
Bachelor's Degree								
$SpEd_{d,2004} \times Exposure$	-0.0360***	-0.0290***	-0.0293**	-0.0336***	0.0013	0.0038	-0.0145	-0.0053
	(0.011)	(0.010)	(0.012)	(0.009)	(0.011)	(0.012)	(0.012)	(0.011)
$Disp_{d,2004} \times Exposure$	0.0140**	0.0146**	0.0105	0.0134*	-0.0090*	-0.0090*	-0.0025	-0.0048
	(0.006)	(0.006)	(0.008)	(0.007)	(0.005)	(0.005)	(0.006)	(0.011)

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. See Table 2.5 for full list of controls and variable definitions. In columns (1) and (5) we have our original main results from columns (3) and (6) of Table 2.5 for black and Hispanic students, respectively. Remining columns are as defined in Table 2.11.

CHAPTER 3

HOW DO SPECIAL EDUCATION FUNDING INCENTIVES AFFECT STUDENT PERFORMANCE?

Katelyn Heath

3.1 Introduction

Special Education for individuals with disabilities is a large, increasingly expensive component of public education. Today, special education students make up about 14% of the public school population, yet, makes up the second largest category of federal elementary and secondary spending at $13 billion. At all levels of government, an estimated $40 billion is spent on special education programs (National Center for Education Statistics, 2015; Elder et al., forthcoming). Since the introduction of special education law in 1975, there has been much debate surrounding best practices for providing state-level funding to special education programs. Each state determines its own system for distributing funds to local school districts for special education services, and these systems can be broadly classified into two types: block grant or census. Under a block grant system, the amount of funds given from the state to each district is fixed regardless of the number of students in special education. Under a census system, state funding varies with the number of students receiving special education services in each district. In this paper, I investigate the effects of switching from a census to a block grant system on special education spending, program enrollment, and student performance.

More broadly, the question of whether money matters in education has been investigated time and again. More recently, researchers have taken advantage of the variation resulting from court-mandated school finance reforms to investigate this question. LaFortune, Rothstein, and Schanzenbach (2018) find that school finance reforms lead to increases in funding in low-income school districts and subsequently improved academic achievement of students in these districts. Jackson et al. (2018) use similar variation and find that increasing funding lead to an increase in the number of completed years of education, as well as higher wages in adulthood, again concentrated among low-income students. Focusing on special education programs in particular, the previous literature linking spending and student outcomes is much more sparse. A number of studies have documented associations between the type of state funding system and changes in special education enrollment. Forster and Greene (2002) and Dhuey and Lipscomb (2011) both document that census funding systems are associated with an increase in special education enrollment using an OLS model, likely as a result of the additional money each special education student draws into the school. Morrill (2018) finds that children who live in states with a census funding system are 15 percent more likely to report currently having ADHD and are about 22 percent more likely to be taking medication for ADHD, relative to states with a block grant system. Cullen (2003) uses variation over time in the amount of money districts receive under a census funding system in Texas. She finds that increasing fiscal incentives can explain almost 40 percent of the increase in student disability rates in Texas from 1991-1992 to 1996-1997.

I extend the literature by exploiting exogenous variation in the funding system in New Jersey to produce causal estimates of the impacts of switching from a census to a block grant funding system on special education spending, enrollment, and subsequent impacts on student performance in math and reading.[1] In 2008, New

[1]Note that given the nature of the policy change and the data, I will only be able to estimate the effect of the policy on general and special education performance on math and reading

Jersey reformed its state finance system for funding public school districts. This included changing the way special education funds are distributed across districts, from a census to block grant system. Prior to 2008, categorical aid was given to local school districts for each additional student classified as special education, over and above the general education aid. In 2008, New Jersey implemented a block grant formula, which gives each district an amount of money based on the *total* district enrollment and the statewide average special education rate. As illustrated in Figure 3.1, the average state special education revenue per special education pupil dropped from \$5,502 prior to the policy to \$4,883 in the post-policy period. The state average disability rate dropped from 17 percent to 15.92 percent, which can be seen in Figure 3.2. The fall in special education revenue per special education pupil and disability rate align with previous findings in the literature that block grant systems encourage identification of fewer students into special education.

Employing a dose-response difference-in-differences estimation strategy, I utilize cross-district and cross-cohort variation in treatment intensity to determine the impact of switching from a census to a block grant funding system on special education enrollment, funding, and overall changes in student achievement. The funding change impacted school districts across the state differentially, based on the pre-existing proportion of special education students in each district. The funding formula for the post-period uses the statewide average disability rate to determine the amount of funding a district will receive from the state for its special education students. This average was set at 16.57 percent at the time of the policy change. Thus, districts with larger than average proportions of students in special education pre-2008 should expect to face a decline in the per pupil special

standardized exams in the aggregate. This is explained in detail in Section 3.4.

education funding they receive from the state. Schools with lower than average special education rates pre-2008 are more likely to experience an increase in the per pupil funding from the state. Thus, the pre-policy special education rate provides a measure of treatment intensity across districts.

Utilizing district-level data from the New Jersey Department of Education, I find that the policy decreased district-level special education funding per pupil (both special and general education pupils), decreased total special education funding, and decreased special education enrollment.[2] In particular, I find that a one standard deviation increase in a district's 2007 special education rate is predicted to lead to about a 19% decline in special education funding per pupil, a 31% decline in total special education funding, and a 5% decline in special education enrollment. Given that the average district has about 2,290 students, this finding translates into roughly 20 fewer students in special education in each district.[3] Overall, these are sizeable reductions in special education funding and special education enrollment.

Finally, I estimate the impact of the funding change on academic achievement. An important caveat is that I am only able to estimate the impact of the policy on both special and general education students in the aggregate. Given that the policy change reduced special education enrollment, students in special education in the post-period of the policy are likely to be negatively selected in terms of

[2]As discussed in detail in Section 3.5, the results for the impact of the funding change on special education enrollment are robust to including controls for aggregate changes in total state revenue per pupil.

[3]A 5% decline in special education relative to the mean implies a 0.889 percentage point decline in special education, since the average special education rate pre-policy is 17%. Given that the average district size is 2,290 students, a 0.889 percentage point decline implies about 20 fewer students.

ability. Therefore, comparing special education student performance before and after the policy change would result in a potentially negative impact of the policy mechanically. Instead, I estimate the impact of the policy on the aggregated sample of students in both special and general education. The policy did not have a statistically significant impact on students' performance on the 4th grade math exam. Given that only district-level data is available, the small sample size leads to estimates that are very imprecisely estimated results. I can only rule effect sizes smaller than a 0.14 percentage point decline and results larger than a 0.23 percentage point increase in the percent of students scoring proficient or higher on the math exam. In terms of the reading exam, I find that the policy led to about a 3% increase in the proportion of students scoring proficient or higher on the exam, given a 1 standard deviation increase in a district's 2007 special education rate. This effect is very small in magnitude, but implies that students were not harmed by the reduction in special education funding, and in fact, some students may have benefited.

Finally, I do not find a statistically significant or economically meaningful impact of the policy on the likelihood that students dropped out of high school. Again, these results are imprecisely estimated, only ruling out effect sizes smaller than a 0.43 percentage point decrease or larger than a 1.12 percentage point increase in the dropout rate. While it is difficult to draw conclusively from these estimates given the imprecision, I take this as suggestive evidence that although this policy did have negative impacts on special education funding and enrollment, it did not have harmful impacts on overall academic achievement.

3.2 Background

3.2.1 Special Education

The Education of all Handicapped Children Act (EHA) introduced in 1975 was the first piece of legislation requiring schools to provide a "free and appropriate" education for all students regardless of physical or cognitive disability. EHA also began the use of Individualized Education Programs (IEP), a document which entitles students to special education and outlines the services he or she receives. In 1990, the law was re-authorized by congress, and is now known as the Individuals with Disabilities Education Act (IDEA)(U.S. Department of Education, 2007). Under IDEA, in order to qualify for special education services students must have a disability that falls within at least one of thirteen categories, and that disability must have an adverse affect on student learning in schools. The thirteen disability categories include autism, emotional disturbance, specific learning disability, various physical disabilities, or other health impairment (which includes ADHD) (Reschly, 1996).

To receive special education services, a student is first referred for testing, typically by a parent or general classroom teacher. After the initial referral, students are administered a series of tests to determine what, if any, disability a student has. These tests are usually administered by a special educator, a speech language pathologist, or a school psychologist. If a student is determined to meet the state disability criteria, an IEP is written for them by a team of professionals. This team typically involves both special educators and general classroom teachers, in addition to the student's parent(s) or legal guardians. The IEP documents exactly what support and instructional services a student will receive over the course of the school year. This may include a teacher's aid in the student's classroom, the ability to work in small groups with a teacher, or direct services provided outside of the general classroom. IEPs are *individualized* and may vary widely so that each student receives a different set or combination of services depending on both the student's disability and the school she attends.

3.2.2 New Jersey Funding System

New Jersey overhauled its system for distributing funding to districts with the passage of the School Funding Reform Act of 2008. Since 1998, New Jersey had a wealth-equalization system, whereby all districts received a base foundation aid intended to ensure that districts had enough recourses to provide students with a "thorough and efficient" education. The basic setup of the funding formula remained the same after 2008, with a similar wealth-equalization system (Verstegen, 2015). One of the main changes in the funding formula was switching special education funding from a census to a block grant finance system.

Prior to the funding change in 2008, categorical aid was given to a local school district for each additional student in special education. The amount of aid given was based on four tiers, depending on the severity of a student's disability. In Tier I, a qualifying student received \$305 from the state for each related service, up to four services (i.e. up to a total of \$1,220). This includes services such as counseling, occupational therapy, and physical therapy. In Tier II, the state gave a total of \$3,207 per qualifying student, which includes, but is not limited to, students who meet the disability criteria for specific learning disability, traumatic brain injury, and mildly cognitively impaired. Tier III gave \$4,276 and includes students who meet the criteria for emotional disturbance, multiply disabled, other health impairment, and moderately cognitively impaired. Finally, Tier IV gave \$12,827 and includes students who are severely cognitively impaired, autistic, or receiving intensive services such as individual instruction, a student to teacher-aid ration of 3:1 or less, or high level assisting technology. In the event that a local district had a student with particularly severe disabilities, the district could receive money from the state to pay for most of the cost above \$40,000 in the form of "extraordinary aid" (Parrish, Harr, Anthony, Merickel, & Esra, 2003).

After 2008, New Jersey implemented a formula that gives each district an amount of money based on the *total* district enrollment and the statewide average special education classification rate. The new state funding formula is defined as:

$$\text{SpEd Funding}_d = \text{Excess Cost} \times \text{State SpEd Rate} \times \text{Enrollment}_d, \tag{3.1}$$

where *Excess Cost* is the amount of money determined by the state to account for the additional cost of educating a student with disabilities. In 2008-2009, the excess cost was set to equal \$10,898 for all special education students except speech-only students, who receive \$1,082. The *State SpEd Rate* is the average special education rate in the state. For the 2008-2009 to 2010-2011 school years, the state average classification rate was determined by the state to be 16.86. Finally, $Enrollment_d$ is the number of general and special education students combined in a district. The excess cost and state average classification rates were established when the initial policy was implemented, and set to be updated by September 1st, 2010 and by September 1st every three years thereafter by recommendation made from the governor to the legislature via the Educational Adequacy Report (New Jersey State Legislature, 2008).

3.3 Data

Data come from the New Jersey Department of Education (NJDOE). The NJDOE contains district- and school-level information on the total number of students, the percent of students in special education, percent female, male, black, Hispanic, and white, and eligible for free or reduced-price lunch (FRL). At the district-level only, the NJDOE contain information on state funding, both total and for special education specifically. The majority of special education placements happen by the ages of 9 through 11, which is about 3rd through 5th grade. Figure 3.3 presents a graph of the Child Count Data reported to the U.S. Department of Education in the 2011-2012 school year for all public school students between the ages of 3 and 21. The child count reaches its maximum at ages 10 and 11, after which the number of students with disabilities begins to decline as students progress toward middle and high school.

The achievement measure used to examine student performance over the study period is the New Jersey Assessment of Skills and Knowledge grade 4 exam (ASK4). This exam was first introduced in the spring of the 2002-2003 school year in response to new testing requirements from the federal No Child Left Behind Act of 2001. The ASK4 exam is administered to 4th grade students throughout the study period from 2004 to 2014. Students can score partially proficient, proficient, or advanced proficient on the exam (New Jersey Department of Education, 2016b). Students who are enrolled in special education must also participate in state standardized exams, unless they receive an exemption. Students receive an exemption when their disability is so severe that they do not receive instruction in any of the knowledge and skill areas that are measured by the state assessment. If this is the case, students are given an alternate assessment to monitor their progress. In the

2014-2015 school year, 6.51 percent of special education students, or 1.07 percent of all students,[4] in fourth grade were given an alternate assessment (New Jersey Department of Education, 2015). Special education students who are able to take the state standardized exam may have access to a variety of accommodations to assist them. These accommodations include, but are not limited to, taking the exam in a separate classroom by themselves or in a small group, sitting near the proctor in the exam room, taking more breaks than scheduled, and allowing additional time as needed. While the complete list of accommodations is determined by the state, the specific set of accommodations a particular special education student receives is determined by their IEP, based on the individual student's needs and abilities (New Jersey Department of Education, 2016a).

Table 3.1 presents descriptive statistics for the final analysis dataset. The sample includes 6,416 districts and covers the years 2004 to 2014. The average special education rate in NJ across the period is 16.13%, roughly half the students are male, the majority are white at 66%, with 12.77% black and 14.18% Hispanic. The average district enrollment size is 2,290, with a total funding per pupil of \$4,603 and special education funding per special education pupil of \$5,277.

Figure 3.4 illustrates the trends in district-level spending between 2004 and 2014. The series in red denotes the total district funding per pupil, and the series in blue denotes special education funding per pupil. The total funding per pupil varies somewhat after 2007, and it is difficult to decern a clear pattern in changes in district funding as a result of the policy change in the raw data. Figure 3.5 illustrates the change in the special education rates across districts in the pre-period and the post-period. The average difference in the special education rate

[4]This number is based on the state average percent of special education students, which was 16.5 percent in 2014-2015 according to the NJDOE.

from 2004 to 2007 is plotted against the 2007 special education rate in each district, illustrated in red. The average difference in the special education rate from 2007 to 2014 for each district is depicted in black. This figure demonstrates that during the pre-policy period there was relatively little difference in trends across districts with higher versus lower special education rates in 2007. As expected, districts with the largest special education rates in 2007 had somewhat larger increases in special education from 2004 to 2007, as illustrated in the upward trend in the pre-period. In contrast, districts in the post-period with the largest special education rates in 2007 experienced the largest declines in their special education enrollment between 2007 and 2014. Given that districts only receive funding based on an expected average special education rate of 16.86%, this graph aligns with the intuition that the districts with the largest special education rates pre-policy made the largest reductions to their special education enrollment in the post-period as a result of this drop in funding.

3.4 Empirical Strategy

After the state funding system changed in 2008, districts in New Jersey no longer received additional funds for each student placed in special education. Instead, districts receive a fixed amount of money based on the assumption that districts should have about 16.89% of students in special education. During the our pre-policy study period, from 2004 to 2007, the average special education rate was 17%, and the average per pupil special education revenue was $694.89. When the funding system changed in 2008, districts with large proportions of special education students incurred a significant loss in per pupil special education funding. Special education funding was effectively capped by the state, independent of the special education rate in a particular district and varying only based on total district enrollment. These changes created downward pressure on the proportion of students enrolled in special education for those districts with larger than average special education rates pre-2008. In contrast, districts with smaller than average special education rates were more likely to face a potential increase in per pupil funding. Thus, I use the average percent of special education students in 2007 as a measure the intensity in the context of a dose-response difference-in-difference estimation strategy.

Formally, I estimate the following:

$$Y_{dt} = \beta_0 + \beta_1 \mathrm{SpEd}_d * \mathrm{Post}_t + \gamma_1 X_{dt} + \gamma_2 (\text{State Revenue per pupil})_{dt} + \phi_d + \delta_t + \varepsilon_{dt} \tag{3.2}$$

where Y_{dt} is the outcome of interest for district d, in year t. Outcome measures include district level total and special education funding, the percent of students in special education, and the percent of students scoring proficient or higher on the 4th grade math and reading standardized exams. The term, SpEd_d, is the percent of special education students in district d in 2007. $Post_t$ is an indicator variable equal to 1 in the post period, from 2008 onward, and equal to zero otherwise. The term X_{dt} is a set of district-level background characteristics as described in the data section. The total state revenue per pupil given to each district is also included in the regressions. The state funding system for the entire education system changed in 2008. This term is included to account for changes in total revenue from the state to a district as a result of the overall state funding change. Each regression includes district fixed effects and year fixed effects. Regressions are weighted by total district enrollment and standard errors are clustered at the district level.

The coefficient of interest in this model is β_1, which is the estimate of the effect of the funding policy change on special education funding, enrollment, and student performance. The model is identified by comparing changes in outcomes across districts with higher proportions of special education students to those with lower proportions, before and after the policy change. The identifying assumption is that districts with lower rates of special education do not trend differentially over time in terms of outcomes compared to districts with higher rates of special education. That is, districts with a low proportion of special education students provide an accurate counterfactual for the trend in outcomes among districts with high proportions of special education students in the absence of the policy change, all else equal. I provide evidence in support of this assumption using an event study analysis, presented in the next section.

In Table 3.2, I present a balance test of characteristics across districts with below-average and above-average special education rates in 2007. Districts with higher than average special education rates have slightly fewer white students, greater proportions of black students, greater proportions of students receiving FRL, and greater proportions of students dropping out. We account for differences in characteristics across districts by demonstrating the stability of the coefficients after including controls for each of these characteristics in the results. In addition, we show that our results are robust to including linear time trends interacted with the baseline percent of students who are black and receive FRL in Section 3.6.

3.5 Results

To test the identifying assumptions of the model, I begin by discussing the results of the event study analysis. The goal of this analysis is to present visual evidence of a lack of pre-treatment trends in the outcome variables as a function of the average proportion of special education students in 2007. The event study graphs, Figures 3.6 through 3.8, are created by replacing the $Post_t$ term in equation (1) with indicator variables for each year of the study period. The control variables included in the event study are those detailed in the data section, including district-level gender, race, and FRL. Results are weighted by district enrollment, with district fixed effects and year fixed effects. Standard errors are clustered at the district level. Year 2007 is set to zero so that all estimates are relative to the year before the policy change.

I first present estimates of the effect of the special education funding change on special education revenue. Figure 3.6 shows the event study for the effect of the funding change on the special education revenue per all pupils and per special education pupils. In the pre-period, funding per all pupils and funding per special education remains centered around zero. After the policy change in 2007 there is a sharp drop in special education funding per all pupils in Figure 3.6 (a). The trend in special education funding per special education pupils in Figure 3.6 (b) is less precise and centered around zero for the majority of the post-period years.

Table 3.3 shows the impact of the policy on special eduction funding per all pupils and special education funding per special education pupils. For each outcome, controls are added sequentially to demonstrate the stability of the estimates as they are added. Column (1) in Table 3.3 shows the impact of the policy on special education funding per pupil (i.e., those in both special and general education), controlling only for district and year fixed effects. Column (2) adds district-level background characteristics, and Column (3) adds a control for total district funding per pupil. The preferred specification is in Column (3). The coefficient on the interaction of the 2007 special education rate with an indicator for the post-period implies that a 1 standard deviation increase in a district's special education rate in 2007 is predicted to lead to a \$133 decline in special education funding per all pupils.[5] Given that the average special education funding per pupil is \$693, this implies a 19% decline in special education funding per pupil as a result of the policy change. Interestingly, the impact of the policy on special education funding per special education pupil is statistically indistinguishable from zero. This is potentially indicative of the fact that special education enrollment went down. Thus, although special education funding decreased, the proportion of students in special education decreased in order to offset this reduction in funding. Table 3.4 shows that the district-level total special education funding decreased by \$509,594 for a 1 standard deviation increase in a district's 2007 special education rate, or about a 31% decrease in total special education funding.

Figure 3.7 demonstrates the event study for the percent of students in special education. There does not appear to be any pre-treatment trend in the impact of the policy on the percent of special education students. Although the trend in the special education rate is not centered around zero in the pre-period, it is very small and trending upward, which is the opposite direction of the effect of the policy in the post-period. In Table 3.5, I present the results of the impact of the policy on special education enrollment. This table is organized as before, with controls added sequentially across columns. Again Column (3) is the preferred specification. I find that the funding policy change led to a 0.889 percentage point decline in special education enrollment, for a 1 standard deviation increase in a district's special education rate. This is about a 5.3% decline in the special education rate, relative

[5] A 1 standard deviation increase is about a 4.56 percentage point increase in the special education rate. Thus, to scale the estimates I multiply the coefficient of interest by 4.56.

to the mean. Overall, this is a small reduction in special education enrollment.

In Table 3.6, I present estimates of the effect of the policy on special education rates by disability type. In Column (1), the category malleable represents a collection of relatively more subjective or potentially mild disability types including individuals with learning disabilities, speech impairments, other health impairments (which includes ADHD), and emotional disturbance. The estimate in Column (1) implies that a 1 standard deviation increase in the 2007 district special education rate would lead to a 0.815 percentage point decline in special education enrollment, or about a 7.1% decline. Columns (2) and (5) demonstrate that the effect of the policy on malleable disability types appears to be driven by students with learning disabilities (SLD) and emotional disturbance (ED). The policy had a negative impact on the percent of students with SLD and ED by 12% and 18%, respectively, for a 1 standard deviation increase in a district's special education rate. Column (7) presents the effect of the policy on students with physical disability types, including deaf and blindness as well as orthopedic impairments. Reassuringly, the impact of the policy on physical impairments is very close to zero and economically and statistically insignificant. This is important, given that it is very difficult to manipulate special education enrollment for the most objective disability types. As expected, changes in funding do not impact students with physical impairments, to whom it is much more difficult to deny special education services.

I now turn to examining the effect of the policy on student performance. Figure 3.8 presents the results of the event study analysis for the impact of the policy on the percent of students scoring proficient or higher on the 4th grade math and reading standardized exams. In Figure 3.8 (a) there appears to be a slight downward trend in performance on the math exam, however, the effect of the policy on math performance in the post-period is very noisy and statistically indistinguishable from zero. The trend in the reading exam in Figure 3.8 (b) in the pre-period is statistically indistinguishable from zero, and in the post-period the trend is positive. Table 3.7 presents estimates for the effect of the policy on the math exam in columns (1) through (3) and the effect on the reading exam in columns (4) through (6). The columns are again organized before, with columns (3) and (6) containing the preferred specification. Column (3) illustrates that there is no economically or statistically significant impact of the policy on the performance of students (in special and general education) on the math exam. This estimate is imprecisely estimated, and we can only rule effect sizes smaller than a 0.14 percentage point decline and larger than a 0.23 percentage point increase, at a 95% confidence level. Column (6) indicates that a 1 standard deviation increase in a district's 2007 special education rate is predicted to increase the percent of students scoring proficient or higher on the 4th grade reading exam by 1.01 percentage points. This is a small effect, representing a 3% increase relative to the mean.

Finally, Figure 3.9 presents the event study results for high school dropout. In the pre- and post-period the estimates are large and statistically indistinguishable from zero. Table 3.8 presents the results of the effect of the policy on the percent of students dropping out of high school. The coefficients are statistically indistinguishable from zero and again imprecisely estimated. These results are only able to rule out effect sizes smaller than a 0.43 percentage point decrease or larger than a 1.12 percentage point increase in the dropout rate, at a 95% level of confidence. Overall, given the imprecise nature of these results, it is difficult to draw conclusions about the impact of the policy on student performance. Given the small positive impact on the reading exam, these estimates are perhaps suggestive that the policy at least did not harm students' academic achievement.

3.6 Robustness

I next present a variety of the robustness checks to provide evidence on the reliability and stability of the estimates. As noted in Section 3.2.2, the state funding system used to distribute money to districts changed for other types of funding as well, besides just special education funding. Each of the main results tables include controls for changes in total funding per pupil, and the estimated effects remain very stable with this addition. To further test whether other changes in the funding system were occurring with this policy change, I estimate changes in total funding as an outcome. Table 3.9 illustrates that there are no statistically significant changes in total funding in a way that correlates with pre-treatment special education enrollment, although this result is somewhat imprecisely estimated.

In addition to using the district special education rate in 2007 as a measure of treatment intensity, one may also consider including the average special education funding per special education pupil as a measure of treatment intensity. Prior to 2008, districts could conceivably choose to manipulate the amount of funding they received from the state by manipulating which tier a student falls under. That is, districts could potentially attempt to "bump up" a student into the next disability tier in order to obtain more money from the state. In practice, this is likely to be a difficult margin on which to manipulate special education enrollment. It is likely easier to manipulate the enrollment of students into special education on the extensive margin. Students who are on the margin of qualifying for special education have relatively more mild and subjective disabilities. However, to manipulate special education enrollment on the intensive margin, students must qualify for more severe and potentially more objective disabilities in order to be placed in a higher tier. In fact, students would need to qualify for a completely different disability in order to move into the next category up. Recall that Tier I only includes specific related services, such as counseling or physical therapy. To move to Tier II, a student would need to qualify as having a specific learning disability, a brain injury, or have a mild cognitive impairment. These disabilities are more difficult to qualify for and there is less room for manipulation of the system as the disabilities become more severe. Therefore, it may be less likely that districts would be able to falsely place students into a higher tier.

Regardless, I present in the appendix results which include the average proportion of special education students and the average special education funding per special education pupil in 2007 as treatment variables in tables C.1 through C.5. The results of the coefficients on the interaction of the special education rate in 2007 with an indicator for post-period years are similar in magnitude and significance to the main results. The coefficients on the interaction of special education funding per special education pupil in 2007 interacted with the post-period indicator are much smaller in magnitude for each of the outcomes. I conclude for these results that the special education funding measure is a less ideal measure for capturing the intensity of districts' responses to the funding policy change.

To further test the robustness of the specification, I run the analysis using the average special education rate, taken over the entire pre-period, as the measure to treatment intensity to avoid any potentially endogenous changes in special education enrollment leading up to the policy change. These results are presented in appendix tables C.6 through C.10. The results are very similar in terms of magnitude and significance. For the impact of the policy on special education funding per pupil, total special education funding, and special education enrollment the results are slightly larger in magnitude, but are otherwise similar in their implications.

Finally, Appendix Table C.11 presents results which include linear time trends in the baseline (i.e., 2007) percent of black students and percent of students receiving FRL for each district. Table 3.2 illustrates that the percent black and percent FRL are the two variables which have the largest discrepancies across districts above and below the average special education rate in 2007. Jaeger, Joyce, and Kaestner (2018) demonstrate the potential importance of including trends in baseline characteristics in cases where background characteristics across treated and control units trend differentially over time. Reassuringly, results which include trends in the percent black and percent FRL are qualitatively and quantitatively similar to the main results presented in Section 3.5.

3.7 Conclusion

In this paper, I estimate the impact of switching from a census to a block grant system for distributing funds from the state to local districts for special education programs. In New Jersey prior to 2008, districts received additional revenue from the state for each additional student enrolled in special education. After 2008 districts received special education funding based on the statewide average special education rate, and funding varied only by total district size. Using district-level data from the New Jersey Department of Education, I implement a dose-response difference-in-differences estimation strategy to determine the impact of this funding change on special education funding per pupil and special education enrollment.

Given that special education revenue in districts is limited by the statewide average special education rate after 2008, the percent of special education students in each district before the policy is used as a measure of treatment intensity in the estimation strategy. Districts with higher than average special education rates in the pre-period experience the effects of the policy change more intensely. It is the districts with the highest percentages of special education students who are affected the most by the policy change, since they experience the largest drop in per pupil special education funding. This in turn creates downward pressure on the percent of students that are subsequently enrolled in special education.

Overall, I find that for a 1 standard deviation increase in a district's special education rate, the policy lead to a 19% decline in special education funding per pupil, a 31% decline in total special education funding, and a 5% decline in special education enrollment, relative to the mean.[6] These are large changes in special education programs as a result of changing the system by which funds are distributed to districts.

This raises an important question. Do census funding systems lead to the unnecessary over-identification of students into special education, or do they allow districts to more freely identify students for special education who need it? To address this question, I estimate the impact of the New Jersey special education funding system change on overall student performance. I estimate the effects of the policy on the performance of special and general education students on the math and reading state standardized exams, as well as the percent of students who

[6]Special education funding per pupil is measured as the total special education aid a district receives from the state divided by total district enrollment. I do not find significant changes in special education funding per special education pupil, likely as a result of the simultaneous declines in special education enrollment.

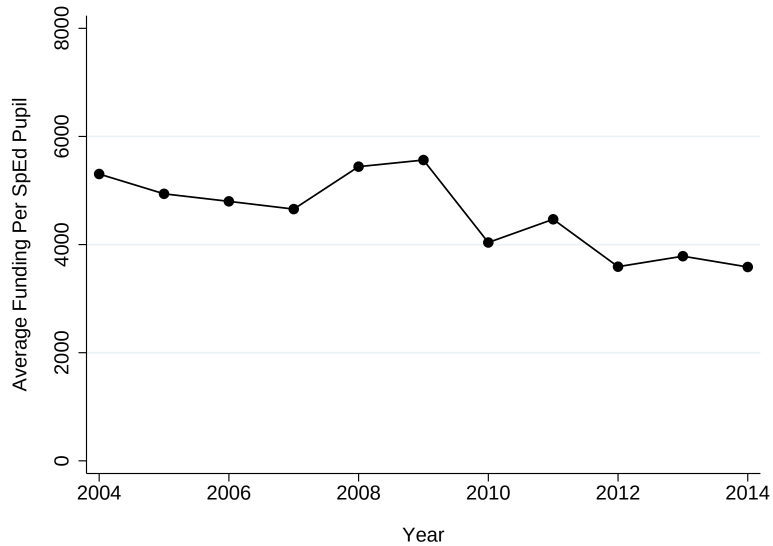
dropout of high school. Since the policy reduced special education enrollment, students in special education in the post-period are a negatively selected sample in terms of ability. Therefore, simply comparing the performance of special education students on the standardized exams before and after the policy change will produce a potentially negative effect of the funding change mechanically. Instead, I estimate the effect of the policy on an aggregated sample of students in both special and general education.

I find that the policy lead to a 3% increase the percent of students scoring proficient or higher on the reading standardized exam, for a 1 standard deviation increase in a district's special education rate. However, the estimates for the impact of the policy change on the math exam and dropout from high school are not statistically significant and are imprecisely estimated. It is difficult to determine conclusively from these estimates what the impact of the policy change was on student performance. However, these estimates provide suggestive evidence that the reduction in special education funding did not have negative impacts on overall student performance.

One important caveat is that this analysis is not able to estimate the impacts of the funding change on student performance separately for special and general education students. Given that only public district-level data are available in New Jersey before 2011, I am unable to separate out the impacts of the policy for students in special and general education. With individual-level data, I could identify students as being in either special or general education prior to the policy change. This would allow me to estimate the effect of the policy change separately for each group, and potentially reveal heterogenous impacts of the policy across special and general education students.

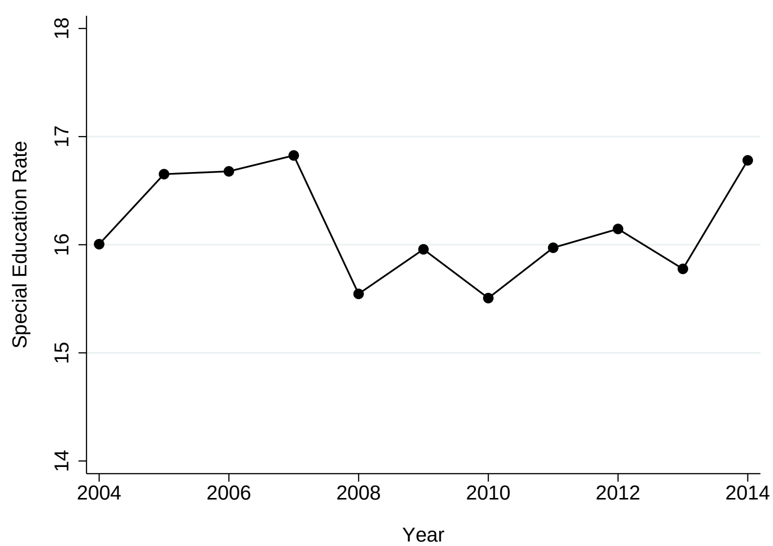
Tables and Figures

Figure 3.1: District-Level Special Education Funding Per SpEd Pupil



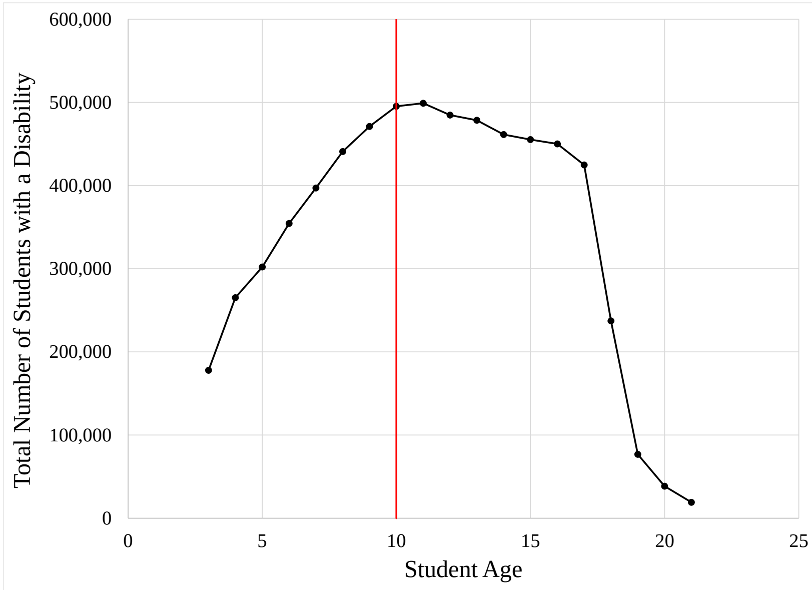
The series plots yearly averages of the special education aid given to each district per special education pupil.

Figure 3.2: Special Education Rate



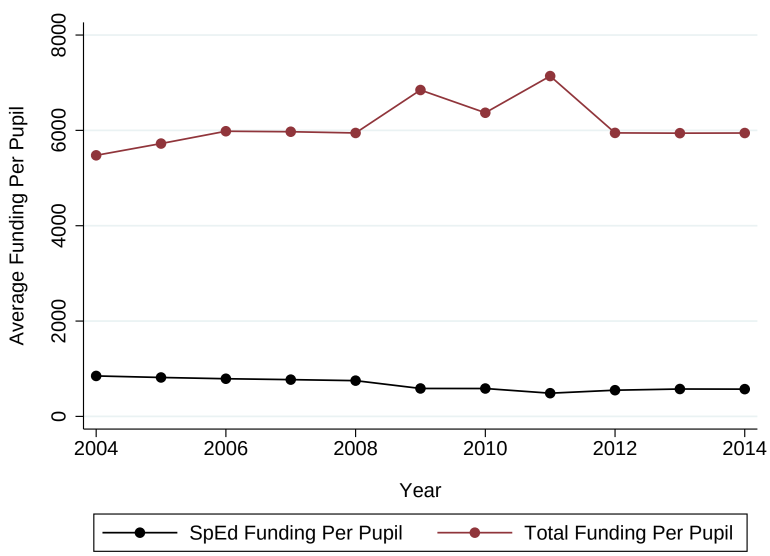
The series plots yearly average special education rate across each district.

Figure 3.3: Student Disability Count by Age from



Data represent national counts of students with a disability by age in 2011, and come from ideadata.org.

Figure 3.4: District-Level Funding Per Pupil



Each series plots yearly averages of the total aid and special education aid given to each district per all pupils (in both special and general education).

Figure 3.5: Change in Special Education Enrollment Between Pre-period and Post-period.

This figure plots, for each district, the difference between the average special education rate prior to 2008 and the average special education rate after 2007 against the special education rate in 2007.

Figure 3.6: Event Study for Special Education Funding

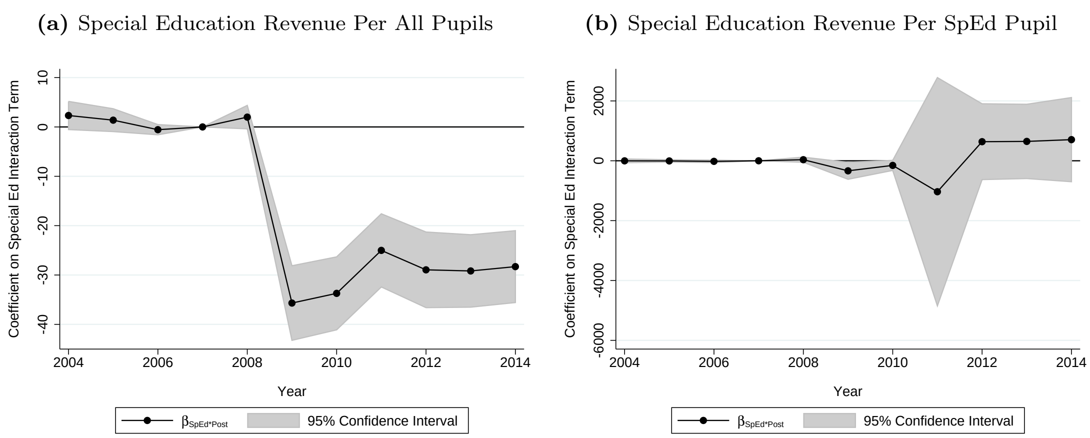
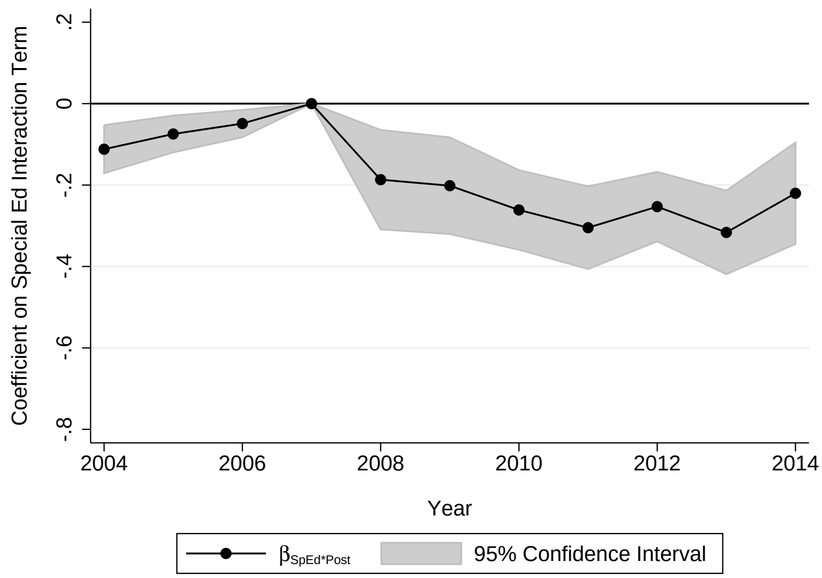


Figure (a) plots coefficients of the special education rate in each district in 2007 interacted with indicators for each year in a regression where the outcome is the average special education revenue per all (special and general education) students. In Figure (b) the outcome is the district-level special education funding per special education student. The shaded region in each figure denotes the band of the 95% confidence interval. Regressions include controls for district level gender, race, and FRL, as well as total funding per all pupils.

Figure 3.7: Event Study for Special Education Enrollment



In this figure the outcome is the district-level percent of students in special education. See Figure 3.6 for the full set of controls.

Figure 3.8: Event Study for Math and Reading Standardized Exams

(a) Math Exam

(b) Reading Exam

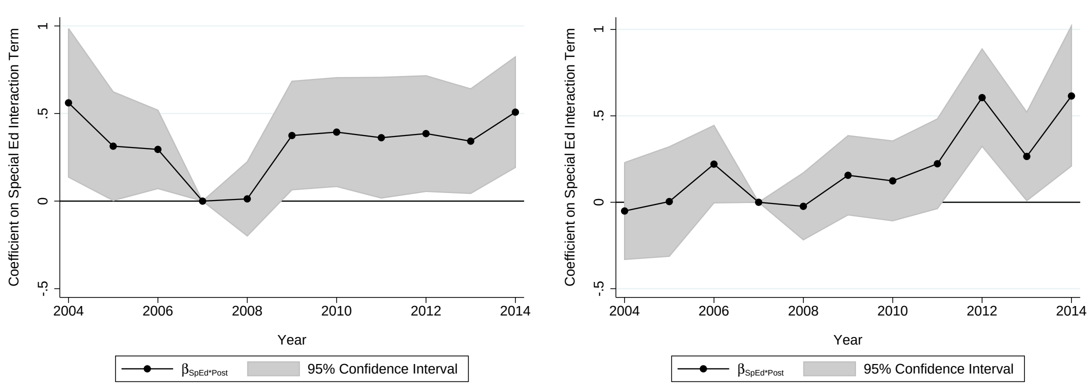
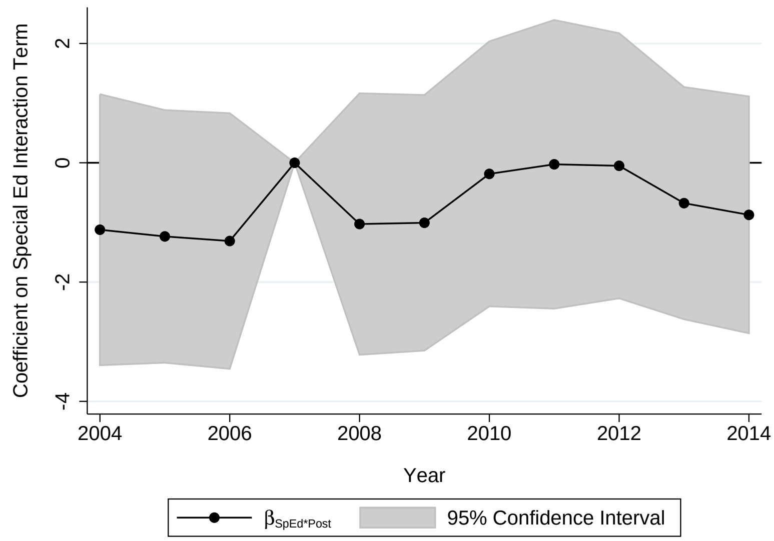


Figure (a) plots coefficients of the special education rate in each district in 2007 interacted with indicators for each year in a regression where the outcome is the percent of students scoring proficient or higher on the 4th grade math standardized exam. In Figure (b) the outcome is the percent of students scoring proficient or higher on the 4th grade reading exam. See Figure 3.6 for the full set of controls.

Figure 3.9: Event Study for High School Dropout



This figure plots the coefficients of the special education rate in each district in 2007 interacted with indicators for each year in a regression where the outcome is the percent of students who dropout of high school. See Figure 3.6 for the full set of controls.

Table 3.1 District-Level Descriptive Statistics

	Mean	Median	SD	Count
Pct SpEd	16.13	16.09	5.20	6416
Pct Malleable	11.11	11.34	4.36	6415
Pct SLD	5.63	5.43	3.10	6415
Pct Speech	3.08	2.97	2.17	6415
Pct OHI	1.97	1.86	1.44	6415
Pct ED	0.435	0.261	0.556	6415
Pct Autism	0.499	0.417	0.515	6415
Pct Physical	0.072	0.00	0.245	6415
Pct SpEd Pre-2008	17.00	16.60	4.54	6174
Pct SpEd Post-2007	15.92	16.03	4.06	6229
Pct SpEd 2007	17.29	16.82	4.58	6168
Pct Male	51.30	51.41	2.75	6416
Pct White	66.01	76.97	28.96	6416
Pct Black	12.77	4.12	20.12	6416
Pct Hispanic	14.18	7.11	17.13	6416
Pct Other Race	7.04	4.24	8.248	6416
Pct FRL	24.66	15.71	24.424	6306
Pct Dropout	7.85	3.63	14.94	2798
SpEd Aid per all	694.89	612.59	363.23	6017
SpEd Aid per SpEd	5,276.69	3,937.88	25,306.45	5992
Total Aid per all	4,602.94	3,428.44	4,367.07	6017
Dist Total Enrollment	2,289.71	1,144.00	3,392.55	6416

Data come from the New Jersey Department of Education. Numbers represent district-level averages. There are about 6,416 districts from 2004 to 2014. FRL is an indicator for receiving free or reduced-price lunch. LEP stands for Limited English Proficiency. Math and reading denote district-level percentages of students scoring proficient or higher on the 4th grade math and reading exams. High School dropout is the district-level percent of students dropping out of high school each year, calculated as the total number of students dropping out in a given district, divided by the total number of high school students in that district. The Malleable disability category includes individuals with specific learning disabilities (SLD), speech impairments, other health impairments (OHI), or emotional disturbance (ED). The physical disability category includes individuals with deaf or blindness, hearing or visual impairments, or orthopedic impairments.

Table 3.2 Difference in Means Between Districts Above and Below Average Set by State

	Below 16.86	Above 16.86	Difference
Pct SpEd	14.03	18.09	-4.06***
Pct Male	51.29	51.30	-0.01
Pct White	66.99	65.12	1.87***
Pct Black	9.55	15.77	-6.22***
Pct Hispanic	14.37	14.00	0.371
Pct Other Race	9.09	5.11	3.98***
Pct FRL	18.97	29.92	-10.95***
Pct Dropout	5.38	13.17	-5.04***
SpEd Aid per SpEd in 2007	4,644.29	4,568.95	75.34
Dist Total Enrollment	2,559	2,039	520***
N	3100	3315	

*** p<0.01, ** p<0.05, * p<0.1 See Table 3.1 for variable definitions. The funding formula change in 2008 is based on the assumption that the average SpEd rate is 16.59. Thus, districts are split into categories above and below a SpEd rate of 16.59 in 2007.

Table 3.3 Effect of Policy Change on Special Education Funding

	SpEd Funding Per All Pupils			SpEd Funding Per SpEd Pupils		
	(1)	(2)	(3)	(4)	(5)	(6)
$SpEd_{d,2007} \times$ Post	-28.897***	-28.127***	-29.069***	26.762	35.926	44.707
	(3.089)	(3.001)	(2.990)	(116.298)	(116.311)	(122.481)
Pct Male		4.558	4.126		-110.696	-106.671
		(4.262)	(4.114)		(300.892)	(298.566)
Pct White		2.905	1.330		-21.915	-7.231
		(2.739)	(2.511)		(37.145)	(41.748)
Pct Black		6.771*	7.296*		-9.726	-14.618
		(4.022)	(3.889)		(52.020)	(52.436)
Pct Hispanic		2.064	1.031		-53.735	-44.113
		(3.349)	(3.246)		(38.797)	(34.740)
Pct FRL		-1.618	-1.976		8.985	12.326
		(1.597)	(1.492)		(20.560)	(22.734)
Total Fund Per Pupil			0.0165***			-0.154
			(0.006)			(0.169)
Observations	5,842	5,842	5,842	5,842	5,842	5,842
R-squared	0.7302	0.7330	0.7369	0.117	0.117	0.117
Mean Dept Var	693.30	693.30	693.30	4,881.84	4,881.84	4,881.84

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. All specifications include year fixed effects and district fixed effects. Controls include district-level percent male, white, black, Hispanic, and percent of students receiving free and reduced price lunch (FRL). The outcome is district-level special education funding, either per all pupils (SpEd and general Ed) or per SpEd pupils.

Table 3.4 Effect of Policy Change on Special Education Funding

	Total Special Education Funding		
	(1)	(2)	(3)
$\text{SpEd}_{d,2007} \times \text{Post}$	-183,538**	-141,223**	-111,265**
	(87,4045)	(60,1855)	(44,742)
Pct Male		-88,878***	-75,146**
		(33,581)	(32,145)
Pct White		-83,436*	-33,344
		(49,696)	(36,866)
Pct Black		162,443**	145,753**
		(73,427)	(64,668)
Pct Hispanic		59,465	92,293*
		(50,181)	(53,173)
Pct FRL		-95,052***	-83,654***
		(30,864)	(27,422)
Total Fund Per Pupil			-524.519***
			(141.073)
Observations	5,842	5,842	5,842
R-squared	0.963	0.970	0.975
Mean Dept Var	1,645,332	1,645,332	1,645,332

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Robust standard errors are clustered at the district level. See Table 3.3 for description of controls.

Table 3.5 Effect of Policy Change on Special Education Enrollment

	Percent Special Education		
	(1)	(2)	(3)
$\text{SpEd}_{d,2007} \times \text{Post}$	-0.200***	-0.199***	-0.194***
	(0.045)	(0.046)	(0.045)
Pct Male		0.008	0.010
		(0.092)	(0.092)
Pct White		0.072**	0.080**
		(0.034)	(0.033)
Pct Black		0.188***	0.185***
		(0.050)	(0.050)
Pct Hispanic		0.105	0.111*
		(0.065)	(0.066)
Pct FRL		-0.030**	-0.028**
		(0.013)	(0.013)
Total Fund Per Pupil			-0.0001
			(0.0001)
Observations	5,842	5,842	5,842
R-squared	0.717	0.722	0.723
Mean Dept Var	16.62	16.62	16.62

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. See Table 3.3 for description of controls.

Table 3.6 Effect of Policy Change on Special Education Enrollment by Disability Type

	Malleable (1)	SLD (2)	Speech (3)	OHI (4)	ED (5)	Autism (6)	Physical (7)
$\text{SpEd}_{d,2007}$ × Post	-0.178***	-0.147***	-0.025	0.012	-0.018**	-0.005	0.008
	(0.043)	(0.026)	(0.015)	(0.013)	(0.008)	(0.005)	(0.012)
Pct Male	0.047	0.006	0.044**	-0.004	0.002	0.019***	0.001
	(0.040)	(0.030)	(0.017)	(0.015)	(0.006)	(0.006)	(0.003)
Pct White	0.052*	0.027	0.017	0.027**	-0.019***	-0.002	-0.001
	(0.030)	(0.020)	(0.011)	(0.011)	(0.005)	(0.004)	(0.003)
Pct Black	0.092**	0.047*	0.019	0.036**	-0.011	0.003	0.005
	(0.036)	(0.027)	(0.015)	(0.015)	(0.007)	(0.006)	(0.005)
Pct Hispanic	0.010	-0.001	0.036**	-0.013	-0.011*	-0.001	-0.000
	(0.035)	(0.027)	(0.014)	(0.013)	(0.006)	(0.005)	(0.004)
Pct FRL	-0.001	-0.007	0.011*	0.000	-0.005**	0.002	-0.004
	(0.013)	(0.009)	(0.006)	(0.005)	(0.002)	(0.001)	(0.003)
Total Fund Per Pupil	0.00001	0.00001	-0.00001	0.00001	-0.00001	0.00001	0.000001
	(0.00004)	(0.00004)	(0.00001)	(0.00002)	(0.00001)	(0.00001)	(0.00001)
Observations	5,842	5,842	5,842	5,842	5,842	5,842	5,842
R-squared	0.736	0.837	0.820	0.834	0.797	0.761	0.149
Mean Dept Var	11.43	5.72	3.20	2.06	0.456	0.534	0.075

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Robust standard errors are clustered at the district level. See Table 3.3 for description of controls. Each outcome is the district-level percent of students with each disability type. The Malleable disability category includes individuals with specific learning disabilities (SLD), speech impairments, other health impairments (OHI), or emotional disturbance (ED). The physical disability category includes individuals with deaf or blindness, hearing or visual impairments, or orthopedic impairments.

Table 3.7 Effect of Policy Change on Standardized Exams

	Math Exam			Reading Exam		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{SpEd}_{d,2007} \times \text{Post}$	-0.082	0.035	0.045	0.505***	0.251**	0.220**
	(0.102)	(0.098)	(0.095)	(0.112)	(0.101)	(0.095)
Pct Male		-0.015	-0.012		0.207	0.199
		(0.123)	(0.122)		(0.130)	(0.126)
Pct White		-0.434***	-0.418***		0.340***	0.292***
		(0.073)	(0.070)		(0.103)	(0.092)
Pct Black		-0.511***	-0.515***		-0.007	0.007
		(0.091)	(0.092)		(0.132)	(0.128)
Pct Hispanic		-0.567***	-0.557***		0.577***	0.547***
		(0.088)	(0.088)		(0.132)	(0.129)
Pct FRL		0.031	0.034		0.162***	0.152***
		(0.031)	(0.032)		(0.053)	(0.054)
Total Fund Per Pupil			-0.0002			0.0005**
			(0.0002)			(0.0002)
Observations	5,077	5,077	5,077	5,076	5,076	5,076
R-squared	0.608	0.618	0.618	0.925	0.933	0.933
Mean Dept Var	54.81	54.81	54.81	33.90	33.90	33.90

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. See Table 3.3 for description of controls. The outcome variables denote the percent of students scoring proficient or higher on the 4th grade math and reading standardized exams.

Table 3.8 Effect of Policy Change on High School Dropout

	Percent Dropout		
	(1)	(2)	(3)
$\text{SpEd}_{d,2007} \times \text{Post}$	0.322	0.405	0.345
	(0.486)	(0.430)	(0.396)
Pct Male		0.200	0.174
		(0.416)	(0.422)
Pct White		0.056	-0.024
		(0.248)	(0.261)
Pct Black		-0.435	-0.406
		(0.372)	(0.338)
Pct Hispanic		-0.221	-0.260
		(0.345)	(0.328)
Pct FRL		-0.124	-0.136
		(0.189)	(0.200)
Total Aid Per Pupil			0.001
			(0.001)
Observations	2,717	2,717	2,717
R-squared	0.599	0.602	0.603
Mean Dept Var	7.93	7.93	7.93

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Robust standard errors are clustered at the district level. See Table 3.3 for description of controls. The outcome variables denote the percent of students who dropout of high school. This is calculated by dividing the total number of students dropping out of high school by the total high school enrollment each year.

Table 3.9 Effect of Policy Change on Total District Funding

	Total Funding Per Pupil	Total Funding
	(1)	(2)
$SpEd_{d,2007} \times Post$	57.116	-41,419
	(36.125)	(720,839)
Pct Male	26.180	-128,809
	(28.870)	(291,582)
Pct White	95.501***	1,278,093***
	(28.096)	(488,303)
Pct Black	-31.821	89,753
	(34.060)	(351,981)
Pct Hispanic	62.586**	804,982**
	(27.743)	(348,050)
Pct FRL	21.730**	152,336
	(10.978)	(193,938)
Observations	5,842	5,842
R-squared	0.978	0.989
Mean Dept Var	4,625.18	15,079,638

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. See Table 3.3 for description of controls. The outcome variables denote the percent of students who dropout of high school. This is calculated by dividing the total number of students dropping out of high school by the total high school enrollment each year.

APPENDIX A

CHAPTER 1 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

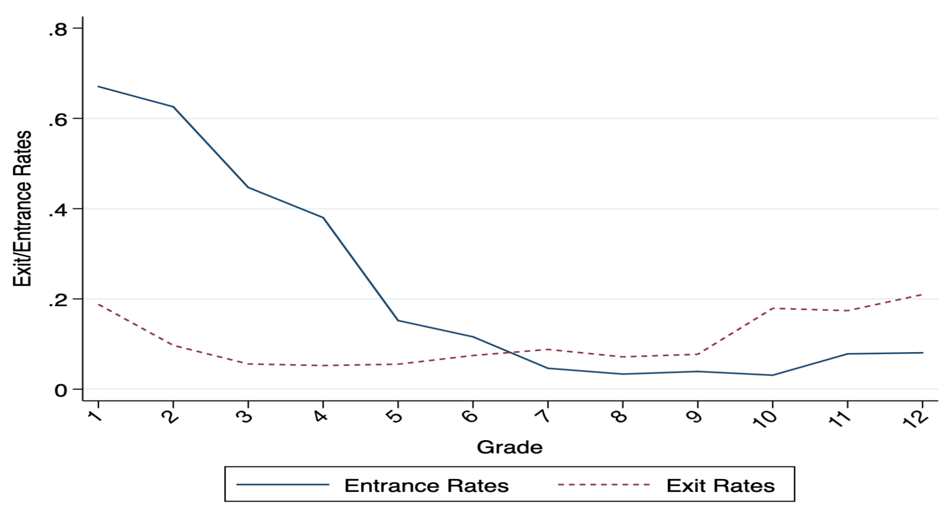
The district-level special education representation rate is compared to the PBMAS standards for the indicator, and performance levels are assigned as follows:

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.

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

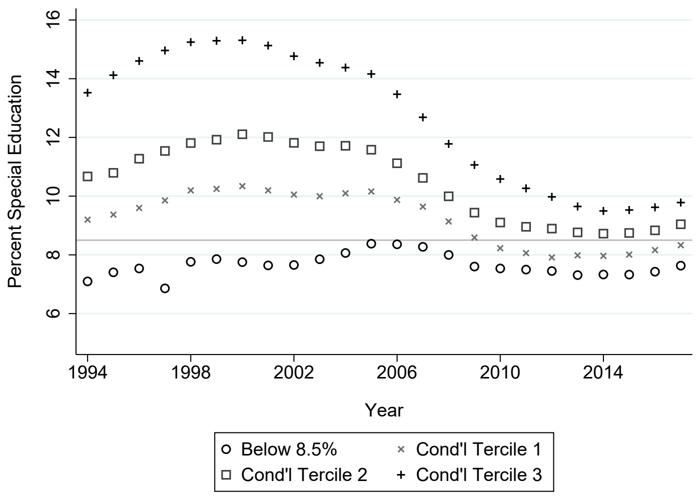
Figure A.4: Entrance and Exit Rates for students with Malleable Disabilities



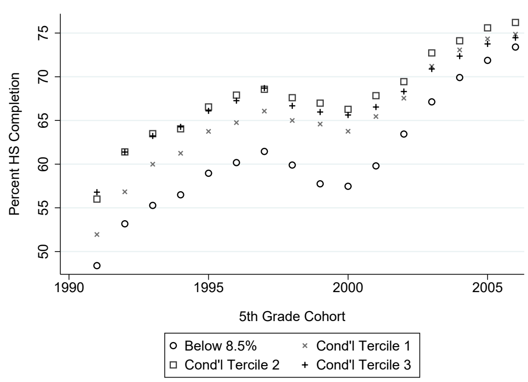
The figure plots entrance rates (solid line) and exit rates (dashed line) in each grade for students with malleable disabilities. Entrance rates represent the number of new students enrolled in SE, divided by the total number of students enrolled in the current grade. Exit rates represent the number 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: Average District SE Rates, High School Completion, and College Enrollment

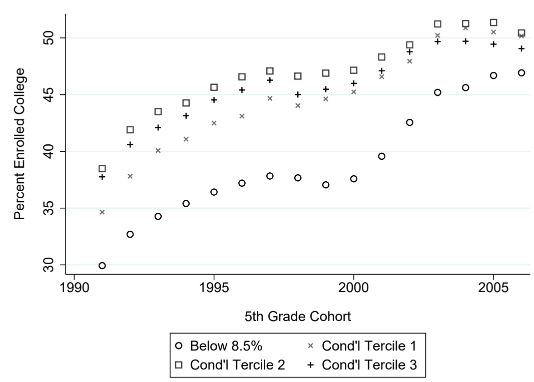
(a) SE Removal in G9 (Expected)



(b) High School Graduation



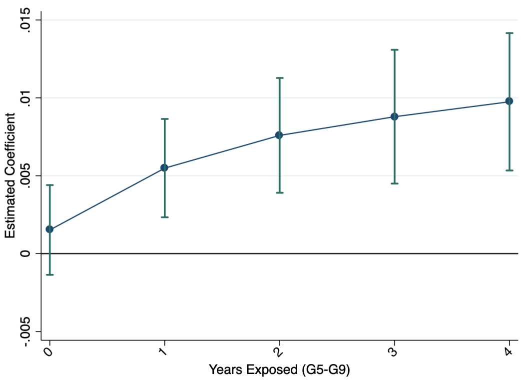
(c) College Enrolled



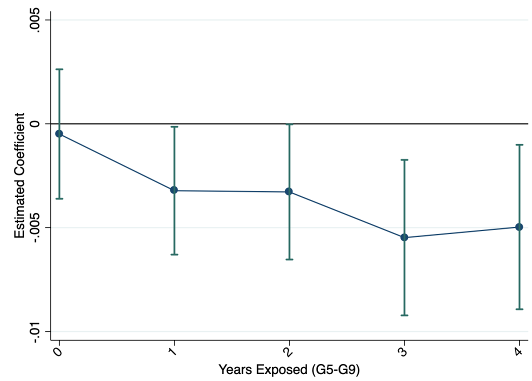
The bottom series denotes districts whose 2005 SE rate was below 8.5%. The top three series break districts into terciles based on their 2005 SE rate, conditional on being above 8.5%. Each series in Panel A plots the district average SE rate in each year, from 1994 to 2017. Each series in Panel B plots the percent of students who obtain a high school diploma, across 5th grade cohort years. Each series in Panel C plots the percent of students who enroll in post-secondary school, across 5th grade cohort years.

Figure A.6: Event Study Estimates of the Impact of the Policy on Educational Attainment (High Impact Sample)

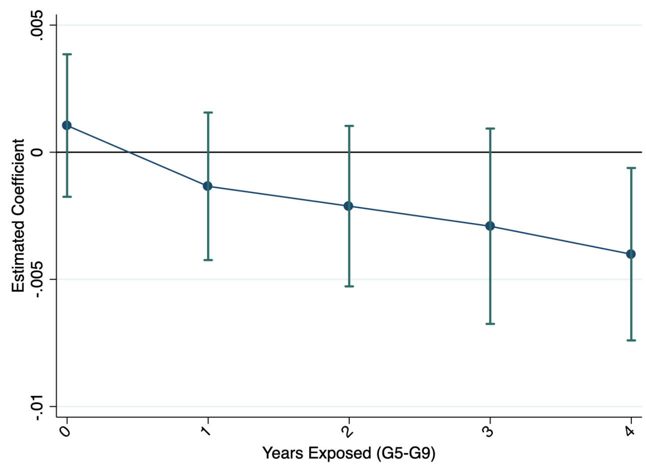
(a) SE Removal in G9 (Expected)



(b) High School Graduation

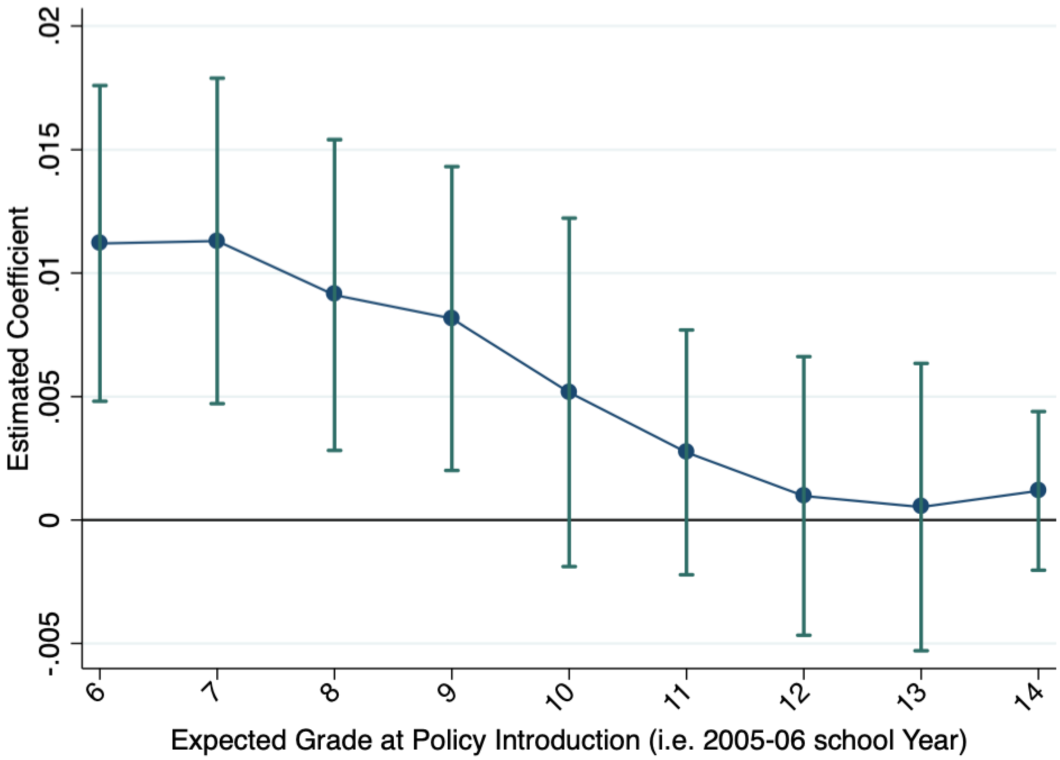


(c) College Enrolled



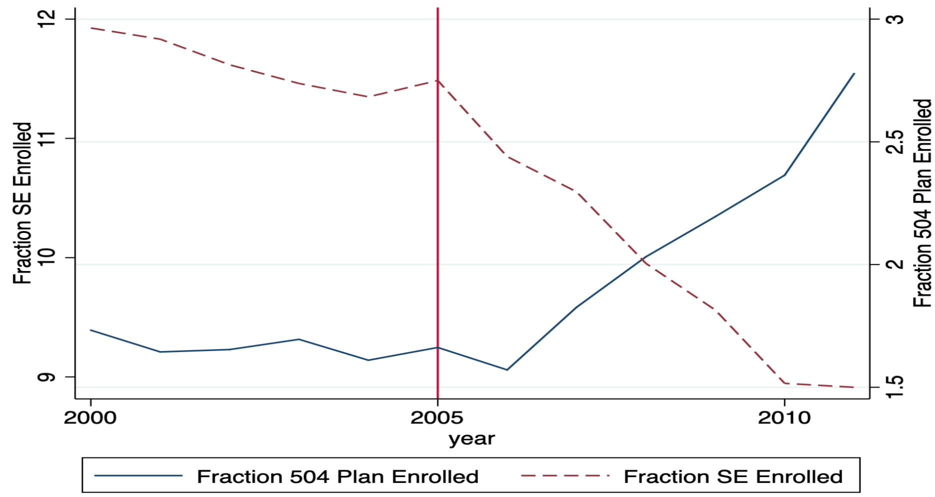
These figures plot coefficients and 95% confidence intervals from event-study regressions that estimates interactions between 5th grade cohort dummies and the pre-policy district SE rate. The dependent variable is shown in the sub-figure labels. College enrollment is measured within four years of each 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 1.2 for more detail on the sample and the full set of controls. Standard errors are clustered by district.

Figure A.7: Event Study Estimates of the Impact of the Policy on SE Removal Any Time after 5th Grade (High Impact Sample)



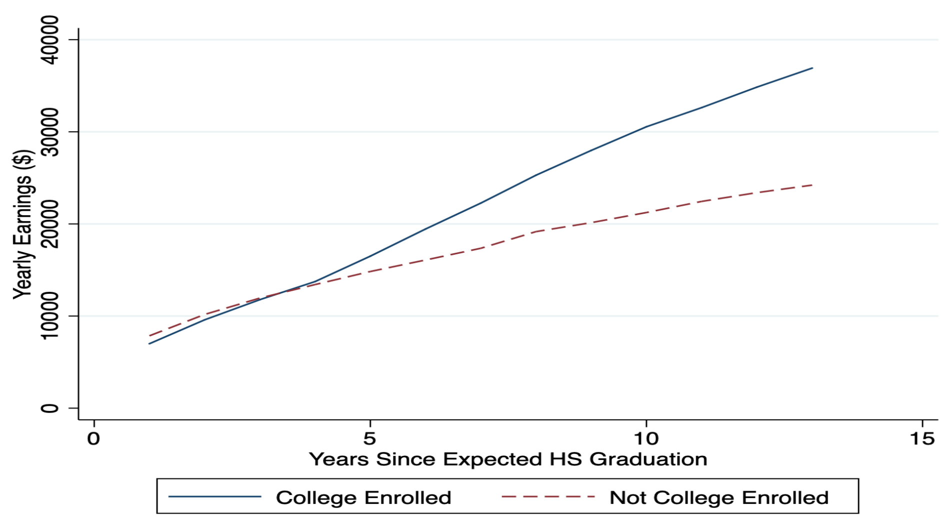
These figures plot coefficients and 95% confidence intervals from event-study regressions that estimates interactions between 5th grade cohort dummies and the pre-policy district SE rate. The dependent variable is an indicator variable for whether a student ever lost SE services after 5th grade. Event 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 unexposed cohort. See Figure 1.2 for more detail on the sample and the full set of controls. Standard errors are clustered by district.

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



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.

Figure A.9: Earnings Trajectories by College Enrollment Choice



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.

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*

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 Sensitivity of Results to Sample Restrictions and Treatment Definition (High Impact Sample)

	(1) Main Analysis	(2) No Drops	(3) More Cohorts	(4) Cohort G4	(5) Cohort G6	(6) Policy Exposure 5th -8th Grade	(7) 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

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 1.1. 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 1.2 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.3 Cross District Summary Statistics - Grade 5 SE Students

	District Fraction SE Enrollment 2004 -05			
	6.6-10	10.1 - 11.6	11.7-13.5	13.6-21.5
Panel A: Student Demographics				
Hispanic	0.41	0.40	0.33	0.25
Black	0.10	0.11	0.11	0.13
White	0.48	0.48	0.55	0.61
Free-Lunch	0.60	0.63	0.65	0.66
ELL	0.19	0.12	0.10	0.06
Male	0.66	0.66	0.65	0.65
Panel B: Baseline Disability Information				
Std Math (G4)	-0.56	-0.50	-0.55	-0.52
Std Reading (G4)	-0.59	-0.54	-0.60	-0.54
Reg Test Math (G4)	0.39	0.39	0.36	0.31
Reg Test Reading (G4)	0.33	0.31	0.29	0.25
Malleable	0.91	0.91	0.92	0.93
Other Impairment	0.11	0.10	0.09	0.09
Learning Disability	0.59	0.60	0.64	0.68
Emotional Disturbance	0.05	0.07	0.06	0.06
Speech Impairment	0.16	0.14	0.13	0.11
Less Malleable	0.09	0.09	0.08	0.07
Mainstream Only	0.28	0.28	0.25	0.24
Separate Classroom (<= 50%)	0.62	0.61	0.65	0.67
Separate Classroom (> 50%)	0.10	0.11	0.10	0.09
Total Students	57,350	56,543	57,474	56,188
C: District Level Information				
Rural	0.42	0.36	0.52	0.68
Average Cohort Size (SE)	74	63	44	21
Average Cohort Size (All)	754	541	327	136
Tax Base Wealth PP/1000	454.04	411.81	363.68	414.13
% Tax Base Wealth Residential	40.41	40.29	38.91	31.75
Total Districts	131	149	221	444

This table presents district level summary statistics where districts are grouped by 2004-05 district level SE enrollment. Low SE districts served between 6.6-10.4 percent, medium SE districts served between 10.5-12.9 percent, and high SE districts served 13-21.5 percent of their students in SE programs. The sample includes all students who were in 5th grade cohorts from 1999-00 - 2004-05 and the summary statistics are reported as of 5th grade or 4th grade for the test score outcomes.

Table A.4 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

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 1.1).

Table A.5 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						

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 1.1. 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 1.2 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.6 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)

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.7 The Impact of Policy on Types of Services/Accommodations

	SE Removal	Regular Classroom (≥ 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

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 1.1. 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 1.2 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 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

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 1.1. 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 1.2 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 The Effect of the SE Enrollment Target on SE Removal and Educational Attainment: Heterogeneity by Specific Disability Type

	(1)	(2)	(3)	(4)	(5)	(6)
	LD	Speech	Other	ED	Physical	Cog. Severe
Panel A: SE Removal in G9 (Expected)						
Treatment	0.00985***	0.00821**	-0.000932	0.00476	-0.00172	0.00114
	(0.00222)	(0.00382)	(0.00332)	(0.00421)	(0.00529)	(0.00171)
	[0.0443]	[0.0370]	[-0.0042]	[0.0214]	[-0.00772]	[0.00511]
Mean (Y)	0.219	0.792	0.177	0.200	0.111	0.021
Panel B: High School Completion						
Treatment	-0.00428**	-0.00648**	-0.000124	-0.00881*	-0.00350	0.00107
	(0.00173)	(0.00299)	(0.00380)	(0.00519)	(0.00602)	(0.00450)
	[-0.0192]	[-0.0292]	[-0.0006]	[-0.0396]	[-0.0158]	[0.00483]
Mean (Y)	0.695	0.789	0.745	0.573	0.824	0.823
Panel C: College Enrollment						
Treatment	-0.00269*	-0.00349	-0.00383	-0.0121***	0.00497	-0.00111
	(0.00141)	(0.00339)	(0.00341)	(0.00447)	(0.00736)	(0.00275)
	[-0.0121]	[-0.0157]	[-0.0172]	[-0.0545]	[0.0224]	[-0.00500]
Mean (Y)	0.304	0.535	0.350	0.249	0.408	0.0676
N	136,694	30,725	23,805	15,098	6,025	11,128

This table shows difference-in-differences estimates of the impact of the policy on SE removal and educational attainment decisions for students with different disabilities (as of 5th grade). Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 1.1. The dependent variable is shown in the panel headings and the disability is shown in the column titles. LD refers to students with learning disabilities, speech refers to students with speech impairments, other refers to other health impairments (which includes ADHD), and ED refers to emotional disturbance. Physical disabilities include orthopedic impairments, auditory impairments, visual impairments, and deafness/blindness. Cognitively severe disabilities include autism, intellectual disabilities, and brain injuries. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05. See Table 1.2 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.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

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

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 1.1. 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 1.2 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 General Education Students: Heterogeneity by Baseline Achievement (4th Grade)

Panel A: ELA Test Score Quintiles	≤20	20-40	40-60	60-80	≥80
	(1)	(2)	(3)	(4)	(5)
High School Completion					
Treatment	-0.00214	-0.00174	-0.000867	-0.000134	-0.000128
	(0.00151)	(0.00131)	(0.00108)	(0.000847)	(0.000792)
	[-0.010]	[-0.008]	[-0.004]	[-0.001]	[-0.001]
Mean (Y)	0.618	0.771	0.843	0.891	0.921
College Enrollment					
Treatment	-0.00123	-0.00489***	-0.00479***	-0.00188	-0.00018
	(0.00147)	(0.00154)	(0.00131)	(0.00117)	(0.00113)
	[-0.00552]	[-0.022]	[-0.0216]	[-0.00848]	[-0.000809]
Mean (Y)	0.39	0.557	0.659	0.736	0.787
N	230,078	262,820	238,007	252,496	203,904
Panel B: Math Test Score Quintiles	≤20	20-40	40-60	60-80	≥80
	(1)	(2)	(3)	(4)	(5)
High School Completion					
Treatment	-0.00286*	-0.00265**	-0.00121	0.000331	-0.000822
	(-0.00163)	(-0.00121)	(-0.00107)	(-0.000998)	(-0.000873)
	[-0.0129]	[-0.0119]	[-0.00544]	[0.00149]	[-0.0037]
Mean (Y)	0.605	0.769	0.844	0.889	0.927
College Enrollment					
Treatment	-0.00243	-0.00407***	-0.00218	-0.00223*	-0.00128
	(0.00151)	(0.0015)	(0.00134)	(0.0012)	(0.00118)
	[-0.0109]	[-0.0183]	[-0.00983]	[-0.01]	[-0.00574]
Mean (Y)	0.4	0.554	0.647	0.725	0.785
N	229,482	244,858	240,756	245,857	223,029

This table shows difference-in-differences estimates of the impact of the policy on SE removal and educational attainment decisions for students with different likelihoods of taking the high school exit exam. [discuss how the prediction was done] Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 1.1. The dependent variable is shown in the panel headings. The sample includes 5th grade cohorts enrolled in SE between 1999-00 to 2004-05. See Table 1.2 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.13 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

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 1.1. 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 1.2 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.14 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

This table contains results obtained from a triple difference model where we augment Equation 1.1 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 1.2 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.15 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

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 1.1. 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 1.2 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.16 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		

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 1.2 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.

APPENDIX B

CHAPTER 2 APPENDIX

Figure B.1: PBMAS Manual 2004 Criteria for District Level Black Disproportionality Rates

District Performance Level Criterion: District SPED African American Representation				
Performance Level (PL) Assignments				
Performance Level = Special Analysis	**Performance Level = 0 (met standard)**	**Performance Level = 1**	**Performance Level = 2**	**Performance Level = 3**
Fewer than 30 African American students or fewer than 30 students served in special education in the district in 2003-2004 and PL not equal to 0.	The district percent of special education students who are African American is no more than 1.0 percentage point higher than the percent of all district students who are African American. Minimum size requirements not applicable if PL = 0.	The district percent of special education students who are African American is between 1.1 and 2.0 percentage points higher than the percent of all district students who are African American.	The district percent of special education students who are African American is between 2.1 and 5.0 percentage points higher than the percent of all district students who are African American.	The district percent of special education students who are African American is at least 5.1 percentage points higher than the percent of all district students who are African American.

Source: Texas Performance Based Monitoring Analysis System Manual 2004.

Figure B.2: PBMAS Manual 2004 Criteria for District Level Hispanic Disproportionality Rates

District Performance Level Criterion: District SPED Hispanic Representation				
Performance Level (PL) Assignments				
Performance Level = Special Analysis	**Performance Level = 0 (met standard)**	**Performance Level = 1**	**Performance Level = 2**	**Performance Level = 3**
Fewer than 30 Hispanic students or fewer than 30 students served in special education in the district in 2003-2004 and PL not equal to 0.	The district percent of special education students who are Hispanic is no more than 1.0 percentage point higher than the percent of all district students who are Hispanic. Minimum size requirements not applicable if PL = 0.	The district percent of special education students who are Hispanic is between 1.1 and 2.0 percentage points higher than the percent of all district students who are Hispanic.	The district percent of special education students who are Hispanic is between 2.1 and 5.0 percentage points higher than the percent of all district students who are Hispanic.	The district percent of special education students who are Hispanic is at least 5.1 percentage points higher than the percent of all district students who are Hispanic.

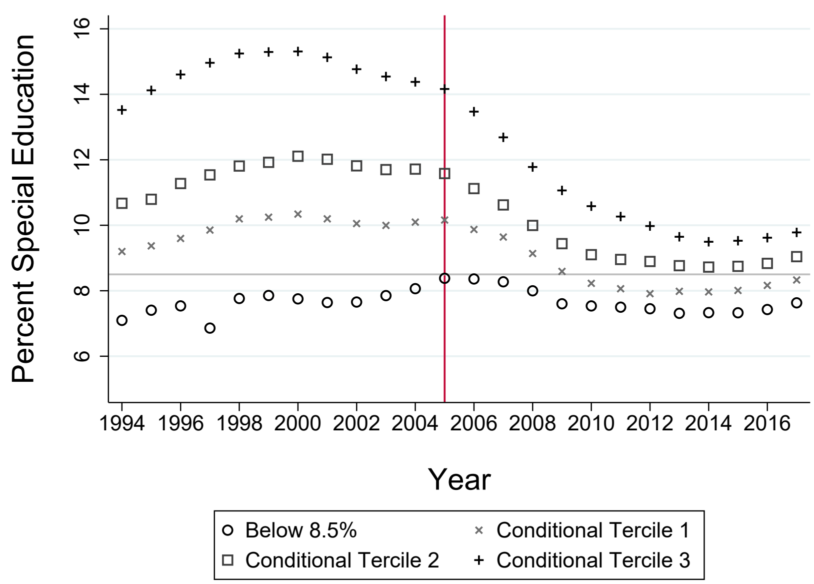
Source: Texas Performance Based Monitoring Analysis System Manual 2004.

Figure B.3: PBMAS Manual 2004 Criteria for District Level Special Education Rates

District Performance Level Criterion: District Percentage of Students Receiving SPED Services				
Performance Level (PL) Assignments				
Performance Level = Special Analysis	Performance Level = 0 (met standard)	Performance Level = 1	Performance Level = 2	Performance Level = 3
Fewer than 30 students in special education in the district in 2003-2004 and PL not equal to 0.	The district identification of students to receive special education services is 8.5% or lower. Minimum size requirements not applicable if PL = 0.	The district identification of students to receive special education services is between 8.6% and 11.0%.	The district identification of students to receive special education services is between 11.1% and 16.0%.	The district identification of students to receive special education services is 16.1% or higher.

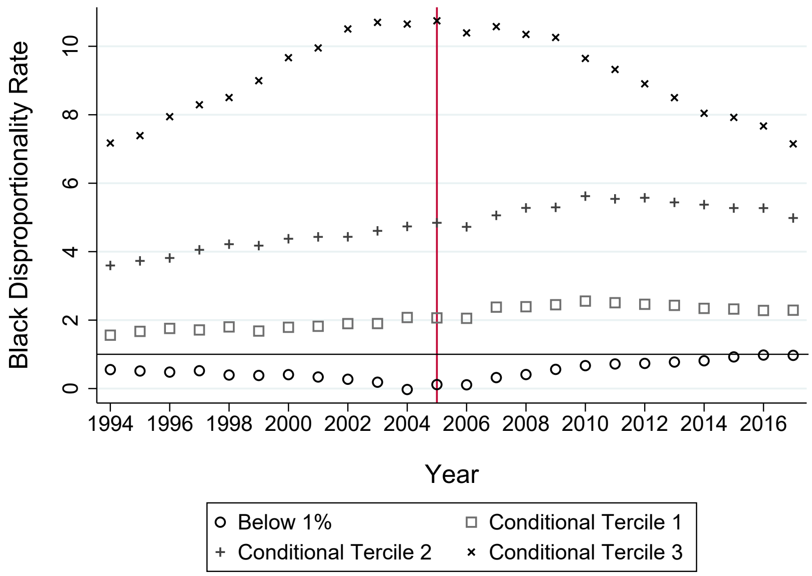
Source: Texas Performance Based Monitoring Analysis System Manual 2004.

Figure B.4: Percent of Students in SpEd by District SpEd Rate



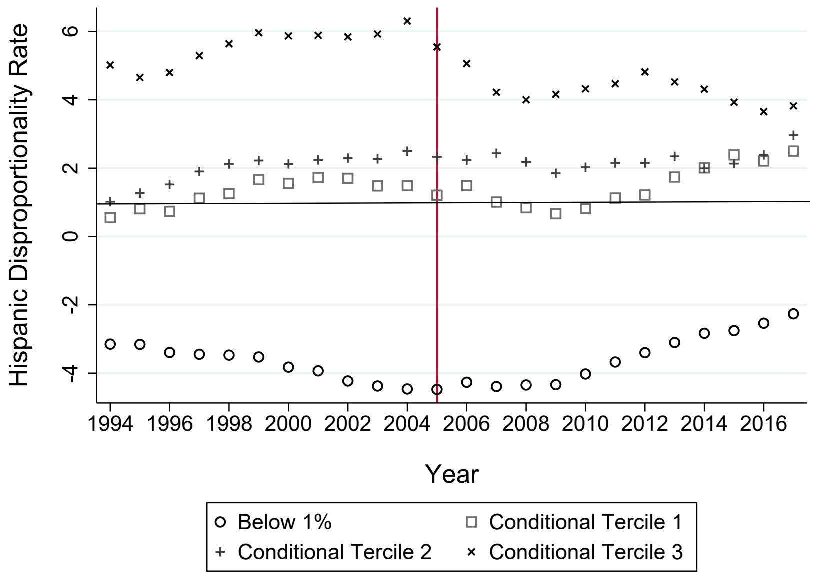
This figure plots the percent of students in SpEd from 1994 to 2017. Districts are split into four groups. The bottom series consists of districts with average SpEd rate already below 8.5% prior to 2004. The top three series split the remaining districts above 8.5% into terciles based on the pre-period percent of students in SpEd.

Figure B.5: Percent of Black Students in SpEd by District Black Disproportionality Rate



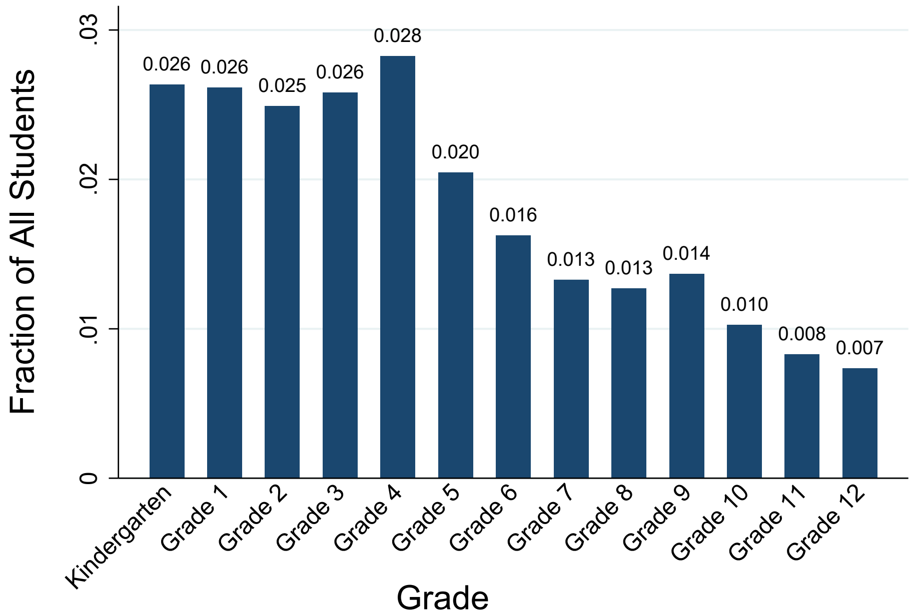
This figure plots the black disproportionality rate from 1994 to 2017. The bottom series consists of districts with black disproportionality rates less than 1% prior to 2004. The top three series split the remaining districts above with 1% black disproportionality rate into terciles based on the pre-period black disproportionality rate.

Figure B.6: Percent of Hispanic Students in SpEd by District Hispanic Disproportionality Rate



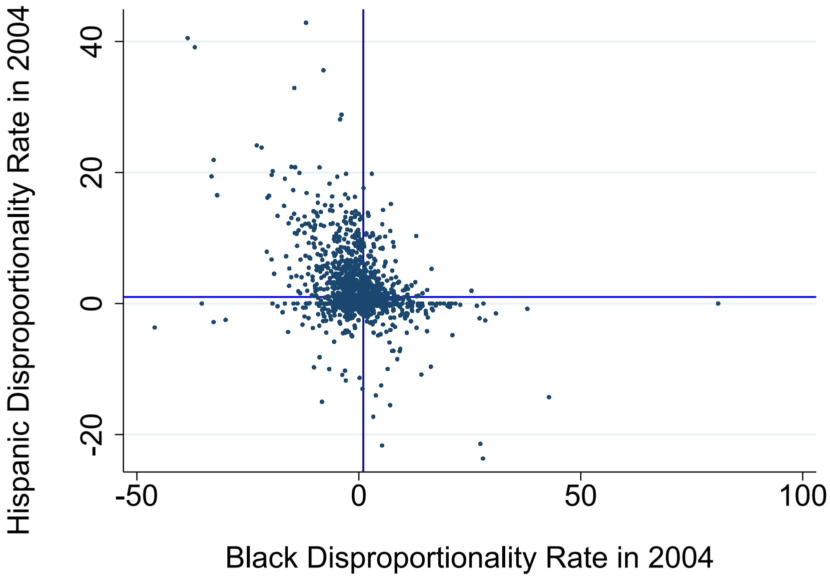
This figure plots the Hispanic disproportionality rate from 1994 to 2017. The bottom series consists of districts with Hispanic disproportionality rate less than 1% prior to 2004. The top three series split the remaining districts above with 1% Hispanic disproportionality rate into terciles based on the pre-period Hispanic disproportionality rate.

Figure B.7: Fraction of All Students Entering SpEd in Each Grade



Each bar represents the fraction of students entering SpEd in each grade, out of the total number of students in each grade.

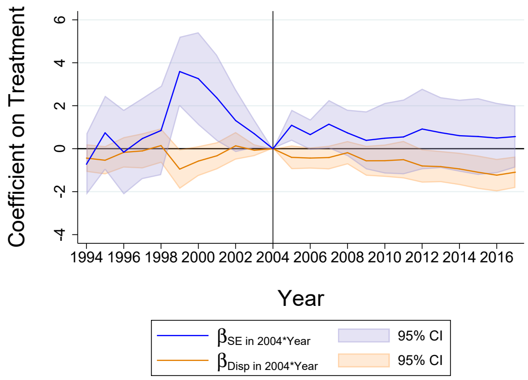
Figure B.8: District-Level Treatment Variation in Black and Hispanic Disproportionality



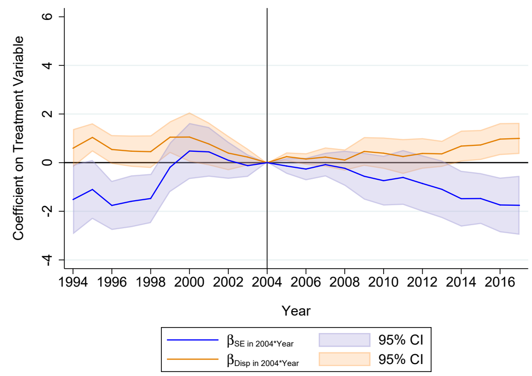
Each dot of the scatter plot represents a district. The x-axis is the 2004 district-level black disproportionality rate and the y-axis is the 2004 district-level Hispanic disproportionality rate. The correlation coefficient is -0.3506***.

Figure B.9: Event Study for Aggregate Sample of SpEd and General Education Students

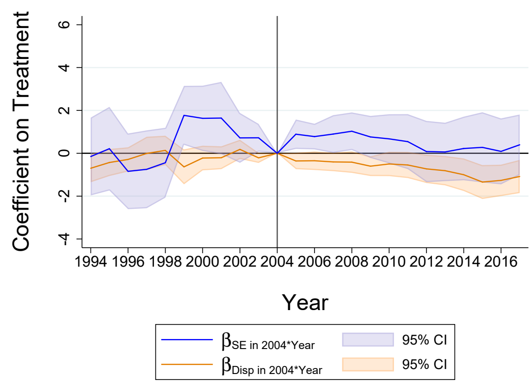
(a) Math Exam for Black SpEd and General Education Students



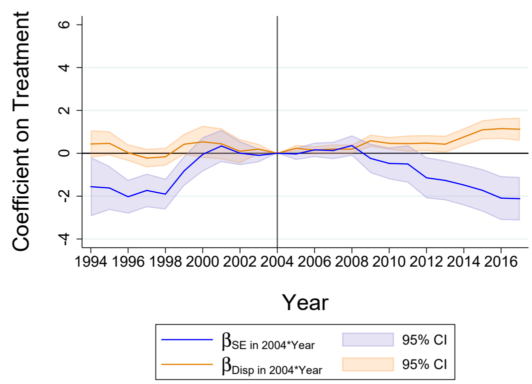
(b) Math Exam for Hispanic SpEd and General Education Students



(c) Reading Exam for Black SpEd and General Education Students



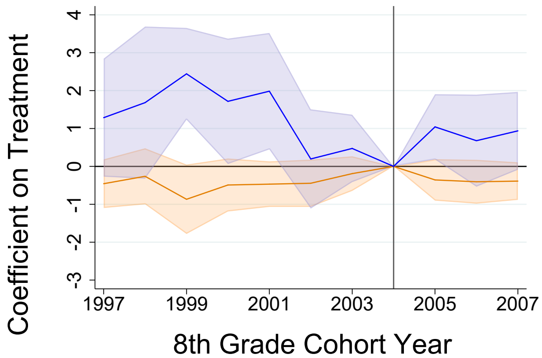
(d) Reading Exam for Hispanic SpEd and General Education Students



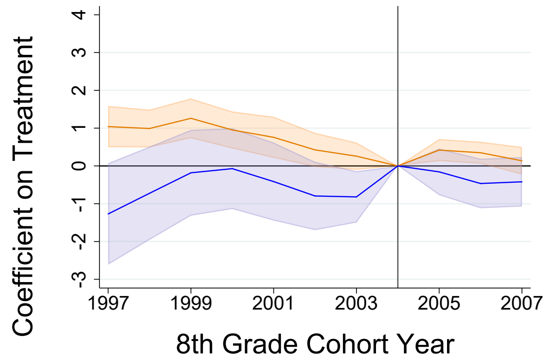
The series in blue is the average black SpEd rate in each district in 2004 interacted with indicators for each year. The series in orange is the average black disproportionality rate in each district in 2004 interacted with indicators for each year. Regressions include controls for individual, district, and grade level race, ESL, FRL, and gifted status, as well as district, year, and grade fixed effects.

Figure B.10: Event Study Estimates for General Education Students

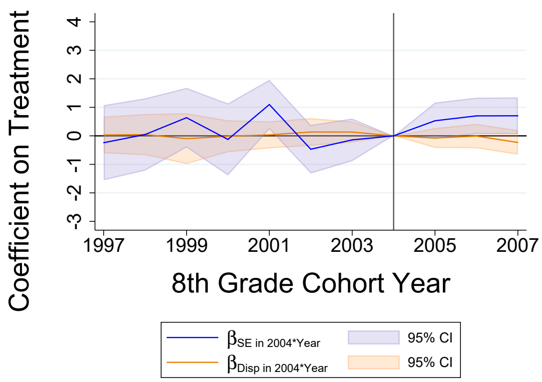
(a) G8 Math Exam for Black General Education Students



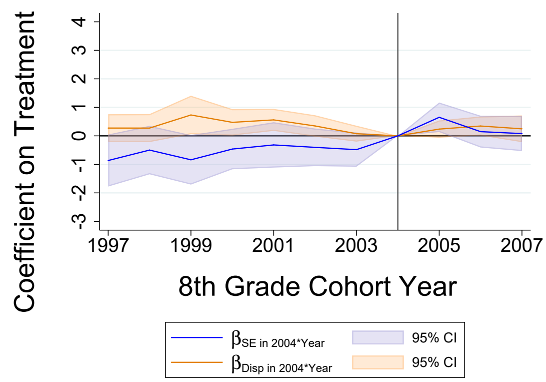
(b) G8 Math Exam for Black General Education Students



(c) G8 Reading Exam for Black General Education Students



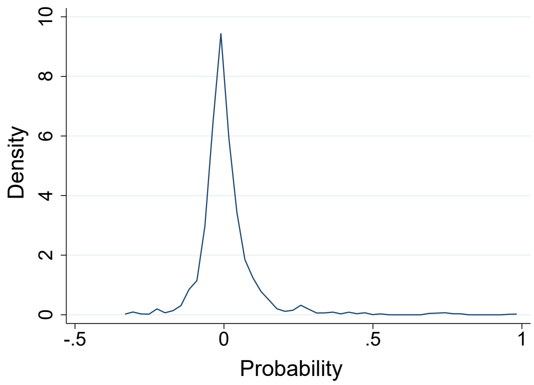
(d) G8 Reading Exam for Black General Education Students



In each graph, the series in blue is the average black SpEd rate in each district in 2004 interacted with indicators for each year. The series in orange is the average black difference score in each district in 2004 interacted with indicators for each year. Regressions include controls for individual, district, and grade level race, ESL, FRL, and gifted status, as well as district and cohort fixed effects.

Figure B.11: Probability of Special Education Model Estimates

(a) Prediction of SpEd Representation for Black Students



(b) Prediction of SpEd Representation for Hispanic Students

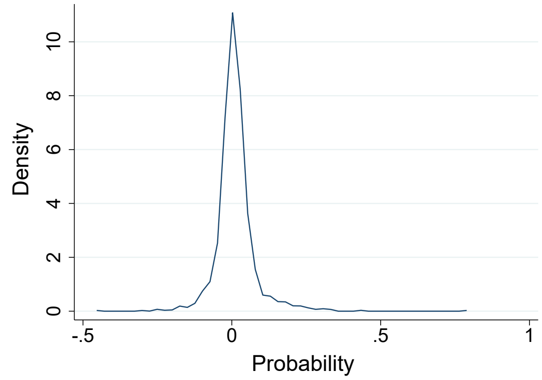


Figure (a) plots the kernel density of the predicted likelihood that black students are over- or underrepresented (corresponding to less than or greater than 0) in SpEd relative to their observationally equivalent white peers. This is based on a logit model of predicted SpEd placement as of 5th grade as a function of 3rd grade academic performance and background characteristics. This figure uses the epanechnikov kernel with a bandwidth of 0.0105. Figure (b) plots the analogous figure for Hispanic students. This figure uses the epanechnikov kernel with a bandwidth of 0.0078.

Table B.1 Disability Type by Race

Disability Type	Black	Hispanic	White
Orthopedic Impairment	0.145	0.158	0.157
Other Health Impairment	0.908	0.519	1.191
Auditory Impairment	0.149	0.155	0.141
Visual Impairment	0.082	0.065	0.086
Deaf/Blind	0.004	0.003	0.003
Learning Impairment	1.441	0.651	0.517
Emotional Disturbance	1.213	0.545	0.978
Learning Disability	7.829	6.352	6.046
Speech Impairment	2.181	2.010	2.858
Autism	0.207	0.086	0.195
Developmental Delay	0.005	0.004	0.012
Traumatic Brain Injury	0.022	0.015	0.024
Early Childhood Disability	0.068	0.047	0.045

We present the percent of all students with each disability type by race, for individuals in school in grades K to 12 prior to policy implementation.

Table B.2 Direct Effect on SpEd Students by Grade

	Black Students			Hispanic Students		
	3rd	4th	5th	3rd	4th	5th
SpEd Status	(1)	(2)	(3)	(4)	(5)	(6)
$SpEd_{d,2004} \times Expo$	-0.0860**	-0.1554***	-0.3767***	-0.1520***	-0.2133***	-0.3374***
	(0.044)	(0.059)	(0.100)	(0.055)	(0.073)	(0.126)
$Disp_{d,2004} \times Expo$	-0.0387	-0.1041***	-0.1303**	0.0150	0.0192	0.0148
	(0.026)	(0.032)	(0.064)	(0.023)	(0.027)	(0.051)
Mean Dept Var	0.705	0.743	0.823	0.652	0.713	0.818
Observations	42,784	58,721	57,993	95,644	128,555	121,882
High School						
$SpEd_{d,2004} \times Expo$	-0.0168	-0.0287	0.0225	-0.1019***	-0.0975***	-0.0815**
	(0.031)	(0.033)	(0.048)	(0.030)	(0.029)	(0.040)
$Disp_{d,2004} \times Expo$	-0.0288	0.0075	0.0585**	-0.0161	-0.0169	-0.0122
	(0.021)	(0.022)	(0.029)	(0.016)	(0.013)	(0.016)
Mean Dept Var	0.617	0.598	0.575	0.619	0.595	0.562
Observations	44,458	61,350	60,624	98,464	132,582	125,905
College Enrollment						
$SpEd_{d,2004} \times Expo$	-0.0259	0.0080	0.0633	-0.0775***	-0.0961***	-0.0565*
	(0.035)	(0.035)	(0.045)	(0.026)	(0.026)	(0.032)
$Disp_{d,2004} \times Expo$	0.0131	0.0431***	0.0447*	-0.0263**	-0.0171	-0.0126
	(0.018)	(0.017)	(0.023)	(0.012)	(0.012)	(0.016)
Mean Dept Var	0.291	0.279	0.257	0.290	0.266	0.228
Observations	44,458	61,350	60,624	98,464	132,582	125,905
Associate's Degree						
$SpEd_{d,2004} \times Expo$	-0.0129	-0.0153	-0.0256*	0.0060	-0.0065	-0.0179
	(0.012)	(0.011)	(0.014)	(0.008)	(0.007)	(0.012)
$Disp_{d,2004} \times Expo$	0.0007	0.0084*	0.0085	-0.0017	0.0045	0.0061
	(0.005)	(0.004)	(0.006)	(0.005)	(0.004)	(0.006)
Mean Dept Var	0.024	0.021	0.018	0.039	0.034	0.026
Observations	44,458	61,350	60,624	98,464	132,582	125,905
Bachelor's Degree						
$SpEd_{d,2004} \times Expo$	0.0045	-0.0032	-0.0072	0.0111	0.0085	0.0052
	(0.007)	(0.008)	(0.009)	(0.007)	(0.006)	(0.007)
$Disp_{d,2004} \times Expo$	0.0100**	0.0061	-0.0004	-0.0050	-0.0071**	-0.0045
	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.004)
Mean Dept Var	0.029	0.027	0.019	0.032	0.025	0.016
Observations	44,458	61,350	60,624	98,464	132,582	125,905

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. Regressions include district and cohort fixed effects. See Table 2.4 for full set of controls. Each sample contains estimates for students in SpEd as of 3rd, 4th, or 6th grade prior to policy implementation (rather than 5th grade).

Table B.3 Spillover Effect on General Education Students by Grade

	Black Students			Hispanic Students		
	3rd	4th	5th	3rd	4th	5th
High School	(1)	(2)	(3)	(4)	(5)	(6)
$SpEd_{d,2004} \times Expo$	-0.0645***	-0.0758***	-0.0967***	-0.0227	-0.0389*	-0.0397
	(0.021)	(0.023)	(0.029)	(0.021)	(0.022)	(0.025)
$Disp_{d,2004} \times Expo$	0.0271**	0.0330**	0.0354*	-0.0376***	-0.0304***	-0.0205
	(0.013)	(0.015)	(0.020)	(0.010)	(0.013)	(0.013)
Mean Dept Var	0.678	0.672	0.652	0.678	0.665	0.638
Observations	371,194	363,849	333,868	1,095,280	1,058,178	949,012
College Enrollment						
$SpEd_{d,2004} \times Expo$	-0.0733***	-0.0863***	-0.0955***	-0.0512***	-0.0598***	-0.0462*
	(0.023)	(0.027)	(0.036)	(0.018)	(0.021)	(0.028)
$Disp_{d,2004} \times Expo$	0.0686***	0.0730***	0.0783***	-0.0236*	-0.0104	0.0048
	(0.016)	(0.018)	(0.023)	(0.014)	(0.015)	(0.016)
Mean Dept Var	0.487	0.485	0.467	0.450	0.442	0.421
Observations	371,194	363,849	333,868	1,095,280	1,058,178	949,012
Associate's Degree						
$SpEd_{d,2004} \times Expo$	-0.0132**	-0.0106	-0.0248**	-0.0043	-0.0128	-0.0263***
	(0.006)	(0.008)	(0.011)	(0.009)	(0.009)	(0.009)
$Disp_{d,2004} \times Expo$	0.0071**	0.0069*	0.0013	-0.0109***	-0.0106**	-0.0076
	(0.004)	(0.004)	(0.006)	(0.004)	(0.004)	(0.005)
Mean Dept Var	0.046	0.047	0.048	0.074	0.075	0.074
Observations	371,194	363,849	333,868	1,095,280	1,058,178	949,012
Bachelor's Degree						
$SpEd_{d,2004} \times Expo$	-0.0021	-0.0215**	-0.0521***	0.0174	0.0068	0.0099
	(0.009)	(0.009)	(0.012)	(0.011)	(0.011)	(0.012)
$Disp_{d,2004} \times Expo$	0.0149***	0.0119**	0.0183**	-0.0158***	-0.0112**	-0.0078
	(0.005)	(0.005)	(0.007)	(0.005)	(0.005)	(0.005)
Mean Dept Var	0.103	0.109	0.113	0.089	0.093	0.095
Observations	371,194	363,849	333,868	1,095,280	1,058,178	949,012

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. Regressions include district and cohort fixed effects. See Table 2.4 for full set of controls. Each sample contains estimates for students in SpEd as of 3rd, 4th, or 6th grade prior to policy implementation (rather than 5th grade).

Table B.4 Effect of Policy by Prediction of District Over- or Under-representation relative to White Students

	Black Students		Hispanic Students	
	Over	Under	Over	Under
SpEd Status	(1)	(2)	(3)	(4)
$SpEd_{d,2004} \times Expo$	-0.2197**	-0.2460***	-0.1705***	-0.4567***
	(0.087)	(0.071)	(0.063)	(0.137)
$Disp_{d,2004} \times Expo$	-0.0461	-0.0610	0.0224	0.0891*
	(0.058)	(0.042)	(0.031)	(0.052)
Mean Dept Var	0.785	0.774	0.752	0.776
Observations	22,024	51,184	94,874	60,775
High School	(1)	(2)	(3)	(4)
$SpEd_{d,2004} \times Expo$	-0.0018	-0.0247	-0.1146***	-0.0487
	(0.055)	(0.054)	(0.039)	(0.054)
$Disp_{d,2004} \times Expo$	0.0328	0.0333	-0.0006	-0.0516***
	(0.033)	(0.032)	(0.019)	(0.019)
Mean Dept Var	0.632	0.559	0.598	0.539
Observations	23,109	53,717	98,095	63,013
College Enrollment				
$SpEd_{d,2004} \times Expo$	0.0255	0.0537	-0.1065***	0.0290
	(0.046)	(0.047)	(0.035)	(0.047)
$Disp_{d,2004} \times Expo$	0.0567**	0.0426*	-0.0043	0.0442**
	(0.029)	(0.028)	(0.017)	(0.021)
Mean Dept Var	0.296	0.254	0.277	0.200
Observations	23,109	53,717	98,095	63,013
Associate's Degree				
$SpEd_{d,2004} \times Expo$	-0.0105	-0.0296**	-0.0249**	-0.0135
	(0.015)	(0.014)	(0.010)	(0.016)
$Disp_{d,2004} \times Expo$	0.0005	0.0041	0.0041	0.0026
	(0.006)	(0.005)	(0.007)	(0.006)
Mean Dept Var	0.023	0.018	0.036	0.022
Observations	23,109	53,717	98,095	63,013
Bachelor's Degree				
$SpEd_{d,2004} \times Expo$	0.0020	-0.0130	0.0057	0.0148
	(0.015)	(0.009)	(0.005)	(0.011)
$Disp_{d,2004} \times Expo$	-0.0064	0.0055	-0.0030	-0.0009
	(0.006)	(0.006)	(0.005)	(0.004)
Mean Dept Var	0.026	0.022	0.025	0.014
Observations	23,109	53,717	98,095	63,013

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. Regressions include district and cohort fixed effects, along with individual and cohort-district level controls. See Table 2.4 for full set of controls. The category "Over" implies black or Hispanic students are over-represented in SpEd, that is, predicted to be more likely to be in SpEd relative to observationally-equivalent white peers. Likewise, "Under" implies under-representation in SpEd relative to white peers. Districts are split by whether black or Hispanic students were predicted to be over- or under-represented in SpEd prior to 2004.

Table B.5 Effect of the Policy on 5th Grade Black SpEd Students by Disability Type

	Black Students						
	All	SLD	Speech	ED	OHI	ID	Physical
SpEd Status	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$SpEd_{d,2004} \times Expo$	-0.2548***	-0.2844***	-0.5401**	-0.2588	0.0590	-0.0527	-0.0345
	(0.058)	(0.056)	(0.252)	(0.162)	(0.134)	(0.056)	(0.081)
$Disp_{d,2004} \times Expo$	-0.0819**	-0.0819*	-0.0301	0.0391	-0.1558*	-0.0872**	0.1500
	(0.036)	(0.044)	(0.115)	(0.096)	(0.080)	(0.035)	(0.154)
Mean Dept Var	0.777	0.811	0.326	0.799	0.855	0.957	0.919
Observations	73,047	44,724	8,097	5,997	5,268	6,493	1,489
High School							
$SpEd_{d,2004} \times Expo$	-0.0208	-0.0167	0.0314	0.0059	-0.0042	0.2592	-0.0624
	(0.038)	(0.050)	(0.109)	(0.103)	(0.132)	(0.163)	(0.104)
$Disp_{d,2004} \times Expo$	0.0428**	0.0388	0.0024	-0.0385	0.0524	0.0935	0.0524
	(0.024)	(0.027)	(0.062)	(0.059)	(0.067)	(0.089)	(0.214)
Mean Dept Var	0.581	0.587	0.636	0.426	0.613	0.586	0.667
Observations	76,640	46,827	8,360	6,590	5,536	6,807	1,520
College Enrollment							
$SpEd_{d,2004} \times Expo$	0.0345	0.0186	0.1138	0.1324	-0.1831	0.2833**	-0.0306
	(0.036)	(0.039)	(0.122)	(0.097)	(0.131)	(0.138)	(0.117)
$Disp_{d,2004} \times Expo$	0.0645***	0.0584***	0.0955	-0.0239	0.1181**	-0.0358	0.0072
	(0.018)	(0.021)	(0.072)	(0.052)	(0.059)	(0.074)	(0.209)
Mean Dept Var	0.267	0.268	0.405	0.199	0.319	0.110	0.345
Observations	76,640	46,827	8,360	6,590	5,536	6,807	1,520
Associate's Degree							
$SpEd_{d,2004} \times Expo$	-0.0232**	-0.0115	-0.1144***	-0.0108	-0.0116	-0.0300*	-0.0894*
	(0.011)	(0.010)	(0.038)	(0.026)	(0.042)	(0.017)	(0.053)
$Disp_{d,2004} \times Expo$	0.0046	0.0073	-0.0091	-0.0058	-0.0161	0.0019	-0.0047
	(0.004)	(0.005)	(0.025)	(0.015)	(0.022)	(0.014)	(0.109)
Mean Dept Var	0.020	0.018	0.041	0.014	0.025	0.003	0.044
Observations	76,640	46,827	8,360	6,590	5,536	6,807	1,520
Bachelor's Degree							
$SpEd_{d,2004} \times Expo$	-0.0083	-0.0097	-0.0581	0.0130	-0.0635	0.0168	-0.0090
	(0.008)	(0.008)	(0.067)	(0.029)	(0.046)	(0.010)	(0.049)
$Disp_{d,2004} \times Expo$	0.0043	-0.0000	0.0117	-0.0062	-0.0342	0.0073**	0.0522
	(0.012)	(0.005)	(0.032)	(0.012)	(0.027)	(0.004)	(0.108)
Mean Dept Var	0.023	0.018	0.077	0.012	0.021	0.002	
Observations	76,640	46,827	8,360	6,590	5,536	6,807	1,520

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. Regressions include district and cohort fixed effects. See Table 2.4 for full set of controls.

Table B.6 Effect of the Policy on 5th Grade Hispanic SpEd Students by Disability Type

	Hispanic Students						
	All	SLD	Speech	ED	OHI	ID	Physical
SpEd Status	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$SpEd_{d,2004} \times Expo$	-0.2349***	-0.2974***	-0.2558*	-0.2364	-0.2843*	-0.0044	0.1148
	(0.069)	(0.085)	(0.145)	(0.154)	(0.155)	(0.049)	(0.090)
$Disp_{d,2004} \times Expo$	0.0268	0.0613**	0.0694	-0.1833**	0.0576	-0.0461*	-0.0855
	(0.027)	(0.030)	(0.077)	(0.080)	(0.067)	(0.026)	(0.058)
Mean Dept Var	0.761	0.816	0.251	0.812	0.845	0.974	0.915
Observations	155,225	108,782	18,567	7,293	7,674	7,411	4,243
High School							
$SpEd_{d,2004} \times Expo$	-0.0955***	-0.0859**	-0.1906***	-0.2361**	-0.2000*	-0.2403	-0.1135
	(0.031)	(0.039)	(0.062)	(0.113)	(0.112)	(0.226)	(0.201)
$Disp_{d,2004} \times Expo$	-0.0102	-0.0142	-0.0805**	0.0842	0.0011	0.0309	-0.0405
	(0.013)	(0.015)	(0.039)	(0.058)	(0.053)	(0.103)	(0.124)
Mean Dept Var	0.554	0.573	0.645	0.442	0.604	0.466	0.699
Observations	167,864	112,580	18,981	7,877	7,923	7,583	4,382
College Enrollment							
$SpEd_{d,2004} \times Expo$	-0.0731***	-0.0711**	-0.2112***	-0.1923**	-0.1863	0.0435	0.3253
	(0.028)	(0.030)	(0.061)	(0.078)	(0.122)	(0.078)	(0.200)
$Disp_{d,2004} \times Expo$	-0.0108	-0.0151	-0.0527	-0.0383	-0.0558	-0.0125	-0.1768
	(0.013)	(0.014)	(0.035)	(0.048)	(0.056)	(0.046)	(0.115)
Mean Dept Var	0.217	0.233	0.411	0.179	0.271	0.037	0.340
Observations	179,451	112,580	18,981	7,877	7,923	7,583	4,382
Associate's Degree							
$SpEd_{d,2004} \times Expo$	-0.0239***	-0.0201*	-0.0742**	-0.0684**	-0.0255	-0.0042	0.1677
	(0.008)	(0.011)	(0.033)	(0.032)	(0.038)	(0.007)	(0.102)
$Disp_{d,2004} \times Expo$	0.0052	0.0044	0.0191	0.0022	0.0073	0.0042	0.0024
	(0.004)	(0.005)	(0.019)	(0.015)	(0.016)	(0.004)	(0.061)
Mean Dept Var	0.027	0.025	0.070	0.014	0.031	0.001	0.055
Observations	179,451	112,580	18,981	7,877	7,923	7,583	4,382
Bachelor's Degree							
$SpEd_{d,2004} \times Expo$	0.0044	0.0124**	-0.0604	0.0029	0.0116	0.0006	0.0747
	(0.006)	(0.006)	(0.047)	(0.023)	(0.037)	(0.002)	(0.076)
$Disp_{d,2004} \times Expo$	-0.0019	-0.0048	0.0150	-0.0045	0.0105	-0.0032	-0.0514
	(0.003)	(0.003)	(0.022)	(0.010)	(0.016)	(0.003)	(0.046)
Mean Dept Var	0.019	0.013	0.077	0.011	0.021	0.0003	0.036
Observations	179,451	112,580	18,981	7,877	7,923	7,583	4,382

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. Regressions include district and cohort fixed effects. See Table 2.4 for full set of controls.

Table B.7 Direct Effect on SpEd Students by Gender and Economic Disadvantage

	Black Students				Hispanic Students			
	Male	Female	FRL	Non-FRL	Male	Female	FRL	Non-FRL
SpEd Status	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$SpEd_{d,2004} \times Expo$	-0.2268***	-0.3111***	-0.2730***	-0.0513	-0.2193***	-0.2555***	-0.2469***	-0.0351
	(0.061)	(0.095)	(0.061)	(0.183)	(0.081)	(0.094)	(0.080)	(0.168)
$Disp_{d,2004} \times Expo$	-0.0575	-0.1431***	-0.0744**	-0.1026	0.0170	0.0431	0.0302	-0.0820
	(0.036)	(0.053)	(0.036)	(0.114)	(0.027)	(0.039)	(0.027)	(0.085)
Mean Dept Var	0.791	0.751	0.786	0.676	0.773	0.738	0.769	0.641
Observations	47,817	25,235	67,294	5,758	101,801	53,427	145,285	9,943
High School								
$SpEd_{d,2004} \times Expo$	0.0123	-0.0810*	-0.0123	-0.2174*	-0.0809**	-0.1164**	-0.1105***	0.0638
	(0.047)	(0.047)	(0.040)	(0.113)	(0.035)	(0.046)	(0.034)	(0.082)
$Disp_{d,2004} \times Expo$	0.0726***	-0.0271	0.0427*	-0.0776	-0.0351**	0.0120	-0.0098	-0.1030**
	(0.026)	(0.031)	(0.024)	(0.063)	(0.016)	(0.020)	(0.014)	(0.047)
Mean Dept Var	0.566	0.612	0.566	0.763	0.570	0.586	0.563	0.755
Observations	50,383	26,263	70,669	5,977	105,706	54,904	150,377	10,233
College Enrollment								
$SpEd_{d,2004} \times Expo$	0.0747*	-0.0474	0.0347	0.0107	-0.0831***	-0.0772*	-0.0841***	-0.0003
	(0.042)	(0.045)	(0.036)	(0.131)	(0.031)	(0.043)	(0.029)	(0.111)
$Disp_{d,2004} \times Expo$	0.0763***	0.0264	0.0588***	0.0694	-0.0229	0.0006	-0.0071	-0.1234**
	(0.021)	(0.028)	(0.018)	(0.068)	(0.016)	(0.019)	(0.014)	(0.054)
Mean Dept Var	0.251	0.296	0.247	0.506	0.239	0.261	0.229	0.504
Observations	50,383	26,263	70,669	5,977	105,706	54,904	150,377	10,233
Associate's Degree								
$SpEd_{d,2004} \times Expo$	-0.0024	-0.0693***	-0.0185*	-0.0754	-0.0278***	-0.0219	-0.0283***	0.0368
	(0.009)	(0.022)	(0.011)	(0.081)	(0.009)	(0.015)	(0.008)	(0.049)
$Disp_{d,2004} \times Expo$	0.0040	0.0033	0.0035	-0.0057	0.0066	0.0045	0.0063	0.0139
	(0.004)	(0.009)	(0.004)	(0.041)	(0.005)	(0.007)	(0.004)	(0.032)
Mean Dept Var	0.016	0.026	0.016	0.062	0.026	0.038	0.027	0.077
Observations	50,383	26,263	70,669	5,977	105,706	54,904	150,377	10,233
Bachelor's Degree								
$SpEd_{d,2004} \times Expo$	-0.0063	-0.0167	-0.0029	-0.1612***	0.0066	0.0067	0.0023	0.0217
	(0.009)	(0.013)	(0.007)	(0.061)	(0.008)	(0.011)	(0.005)	(0.060)
$Disp_{d,2004} \times Expo$	0.0087*	-0.0020	0.0012	0.0478	-0.0050	0.0009	0.0010	-0.0560
	(0.005)	(0.008)	(0.004)	(0.032)	(0.003)	(0.005)	(0.002)	(0.035)
Mean Dept Var	0.021	0.027	0.017	0.096	0.020	0.024	0.015	0.105
Observations	50,383	26,263	70,669	5,977	105,706	54,904	150,377	10,233

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. Regressions include district and cohort fixed effects. See Table 2.4 for full set of controls.

Table B.8 Direct Effect on SpEd Students by Rural or Urban Districts

	Black Students		Hispanic Students	
	Rural	Urban	Rural	Urban
SpEd Status	(1)	(2)	(3)	(4)
$SpEd_{d,2004} \times Expo$	-0.1974**	-0.2459***	-0.2061***	-0.0476**
	(0.082)	(0.092)	(0.064)	(0.020)
$Disp_{d,2004} \times Expo$	-0.0042	-0.1278***	0.0219	-0.2797**
	(0.055)	(0.048)	(0.030)	(0.130)
Mean Dept Var	0.840	0.756	0.795	0.747
Observations	16,383	53,961	39,783	109,968
High School	(1)	(2)	(3)	(4)
$SpEd_{d,2004} \times Expo$	0.0161	0.0122	-0.0082	-0.1562***
	(0.054)	(0.060)	(0.043)	(0.048)
$Disp_{d,2004} \times Expo$	0.0171	0.0529	-0.0258	0.0007
	(0.026)	(0.035)	(0.019)	(0.021)
Mean Dept Var	0.681	0.553	0.624	0.560
Observations	16,987	56,728	40,973	113,887
College Enrollment				
$SpEd_{d,2004} \times Expo$	0.0692	0.0102	0.0132	-0.1470***
	(0.051)	(0.054)	(0.035)	(0.043)
$Disp_{d,2004} \times Expo$	0.0288	0.0815***	-0.0171	-0.0202
	(0.029)	(0.024)	(0.017)	(0.022)
Mean Dept Var	0.276	0.266	0.223	0.258
Observations	16,987	56,728	40,973	113,887
Associate's Degree				
$SpEd_{d,2004} \times Expo$	-0.0163	-0.0381**	-0.0214*	-0.0213*
	(0.019)	(0.015)	(0.013)	(0.012)
$Disp_{d,2004} \times Expo$	-0.0073	0.0070	0.0142**	-0.0081
	(0.007)	(0.006)	(0.007)	(0.007)
Mean Dept Var	0.018	0.020	0.024	0.033
Observations	16,987	56,728	40,973	113,887
Bachelor's Degree				
$SpEd_{d,2004} \times Expo$	0.0021	-0.0105	0.0153	-0.0022
	(0.014)	(0.012)	(0.010)	(0.010)
$Disp_{d,2004} \times Expo$	-0.0047	0.0078	-0.0003	-0.0025
	(0.006)	(0.007)	(0.005)	(0.005)
Mean Dept Var	0.014	0.026	0.016	0.023
Observations	16,987	56,728	40,973	113,887

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors clustered at the district level. Regressions include district and cohort fixed effects. See Table 2.4 for full set of controls.

Table B.9 Spillover Effect on General Education Students by Gender and Economic Disadvantage

	Black Students				Hispanic Students			
	Male	Female	FRL	Non-FRL	Male	Female	FRL	Non-FRL
High School Completion								
$SpEd_{d,2004} \times Expo$	-0.1229***	-0.0879***	-0.1202***	-0.0815**	-0.0426*	-0.0726***	-0.0570**	-0.0531**
	(0.027)	(0.028)	(0.027)	(0.038)	(0.025)	(0.028)	(0.026)	(0.025)
$Disp_{d,2004} \times Expo$	0.0430**	0.0195	0.0243	0.0566***	-0.0470***	-0.0179	-0.0259*	-0.0301**
	(0.020)	(0.021)	(0.020)	(0.020)	(0.013)	(0.016)	(0.014)	(0.013)
Mean Dept Var	0.602	0.668	0.604	0.815	0.604	0.645	0.605	0.817
Observations	177,962	195,744	315,546	58,160	524,695	549,733	972,808	101,620
College Enrollment								
$SpEd_{d,2004} \times Expo$	-0.0922***	-0.1060***	-0.1240***	-0.0316	-0.0389*	-0.0663**	-0.0535**	-0.0452
	(0.027)	(0.029)	(0.028)	(0.059)	(0.023)	(0.026)	(0.023)	(0.044)
$Disp_{d,2004} \times Expo$	0.0766***	0.0711***	0.0616***	0.0661**	-0.0193	0.0049	0.0043	0.0123
	(0.017)	(0.019)	(0.016)	(0.030)	(0.014)	(0.015)	(0.013)	(0.021)
Mean Dept Var	0.315	0.401	0.337	0.464	0.314	0.372	0.324	0.513
Observations	227,349	243,171	389,398	81,122	629,826	652,036	1,148,298	133,564
Associate's Degree								
$SpEd_{d,2004} \times Expo$	-0.0116*	-0.0300***	-0.0163**	-0.0166	-0.0134*	-0.0211*	-0.0155**	-0.0143
	(0.007)	(0.010)	(0.007)	(0.021)	(0.007)	(0.010)	(0.008)	(0.019)
$Disp_{d,2004} \times Expo$	0.0052	-0.0011	0.0001	0.0113	-0.0072**	-0.0092*	-0.0067*	-0.0154*
	(0.004)	(0.005)	(0.003)	(0.011)	(0.004)	(0.005)	(0.004)	(0.009)
Mean Dept Var	0.036	0.036	0.036	0.036	0.059	0.059	0.059	0.059
Observations	227,349	243,171	389,398	81,122	629,826	652,036	1,148,298	133,564
Bachelor's Degree								
$SpEd_{d,2004} \times Expo$	-0.0263**	-0.0370***	-0.0383***	0.0010	0.0007	-0.0011	-0.0003	-0.0011
	(0.011)	(0.012)	(0.009)	(0.029)	(0.010)	(0.012)	(0.008)	(0.031)
$Disp_{d,2004} \times Expo$	0.0129**	0.0120	0.0079*	0.0122	-0.0078*	-0.0105*	-0.0031	-0.0011
	(0.006)	(0.007)	(0.004)	(0.014)	(0.004)	(0.006)	(0.004)	(0.014)
Mean Dept Var	0.062	0.108	0.067	0.176	0.062	0.090	0.061	0.204
Observations	227,349	243,171	389,398	81,122	629,826	652,036	1,148,298	133,564

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors clustered at the district level. Regressions include district and cohort fixed effects. See Table 2.5 for full set of controls.

Table B.10 Spillover Effect on General Education Students by Gender and Economic Disadvantage

	Male	Female	FRL	Non-FRL
High School Completion	(1)	(2)	(3)	(4)
$SpEd_{d,2004} \times Expo$	-0.0595***	-0.0776***	-0.0728***	-0.0476**
	(0.020)	(0.021)	(0.021)	(0.020)
$Disp\,Black_{d,2004} \times Expo$	0.0283	0.0184	0.0263	0.0154
	(0.019)	(0.020)	(0.019)	(0.016)
$Disp\,Hispanic_{d,2004} \times Expo$	-0.0092	0.0110	0.0078	0.0131
	(0.013)	(0.014)	(0.013)	(0.013)
Mean Dept Var	0.628	0.677	0.630	0.840
Observations	675,449	716,858	1,237,078	155,229
College Enrollment				
$SpEd_{d,2004} \times Expo$	-0.0543**	-0.0818***	-0.0711***	-0.0163
	(0.021)	(0.025)	(0.023)	(0.038)
$Disp\,Black_{d,2004} \times Expo$	0.0342*	0.0396*	0.0398**	0.0186
	(0.020)	(0.021)	(0.019)	(0.024)
$Disp\,Hispanic_{d,2004} \times Expo$	-0.0069	0.0192	0.0178	-0.0113
	(0.015)	(0.018)	(0.016)	(0.024)
Mean Dept Var	0.315	0.480	0.412	0.689
Observations	675,449	716,858	1,237,078	155,229
Associate's Degree				
$SpEd_{d,2004} \times Expo$	-0.0163**	-0.0279***	-0.0187***	-0.0151
	(0.007)	(0.010)	(0.007)	(0.020)
$Disp\,Black_{d,2004} \times Expo$	0.0036	-0.0013	0.0013	-0.0075
	(0.006)	(0.008)	(0.006)	(0.013)
$Disp\,Hispanic_{d,2004} \times Expo$	-0.0024	-0.0058	-0.0024	-0.0213*
	(0.005)	(0.006)	(0.004)	(0.012)
Mean Dept Var	0.049	0.085	0.065	0.092
Observations	675,449	716,858	1,237,078	155,229
Bachelor's Degree				
$SpEd_{d,2004} \times Expo$	-0.0070	-0.0096	-0.0073	0.0092
	(0.010)	(0.012)	(0.009)	(0.033)
$Disp\,Black_{d,2004} \times Expo$	0.0112**	0.0163**	0.0122**	0.0088
	(0.006)	(0.007)	(0.005)	(0.017)
$Disp\,Hispanic_{d,2004} \times Expo$	-0.0049	-0.0019	0.0012	0.0144
	(0.005)	(0.007)	(0.005)	(0.017)
Mean Dept Var	0.079	0.119	0.078	0.268
Observations	675,449	716,858	1,237,078	155,229

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors clustered at the district level. Regressions include district and cohort fixed effects. See Table 2.5 for full set of controls.

Table B.11 Spillover Effect on General Education Students by Rural or Urban Districts

	Black Students		Hispanic Students	
	Rural	Urban	Rural	Urban
High School Completion	(1)	(2)	(3)	(4)
$SpEd_{d,2004} \times Expo$	-0.0603	-0.0049	-0.0625**	-0.0337
	(0.045)	(0.033)	(0.031)	(0.032)
$Disp_{d,2004} \times Expo$	-0.0046	0.0491**	-0.0086	-0.0262
	(0.024)	(0.023)	(0.012)	(0.019)
Mean Dept Var	0.715	0.651	0.686	0.641
Observations	65,021	282,744	243,345	757,914
College Enrollment				
$SpEd_{d,2004} \times Expo$	-0.0079	-0.0600	-0.0280	-0.0764**
	(0.046)	(0.039)	(0.025)	(0.035)
$Disp_{d,2004} \times Expo$	0.0147	0.1081***	0.0006	-0.0133
	(0.024)	(0.029)	(0.011)	(0.029)
Mean Dept Var	0.503	0.471	0.440	0.430
Observations	65,021	282,744	243,345	757,914
Associate's Degree				
$SpEd_{d,2004} \times Expo$	-0.0028	-0.0265*	-0.0025	-0.0221
	(0.015)	(0.014)	(0.010)	(0.014)
$Disp_{d,2004} \times Expo$	-0.0010	0.0070	-0.0025	-0.0114
	(0.008)	(0.006)	(0.005)	(0.008)
Mean Dept Var	0.059	0.046	0.074	0.075
Observations	65,021	282,744	243,345	757,914
Bachelor's Degree				
$SpEd_{d,2004} \times Expo$	-0.0153	-0.0407**	0.0071	-0.0041
	(0.018)	(0.017)	(0.012)	(0.015)
$Disp_{d,2004} \times Expo$	0.0013	0.0262***	-0.0057	-0.0135*
	(0.008)	(0.010)	(0.005)	(0.007)
Mean Dept Var	0.097	0.117	0.093	0.096
Observations	65,021	282,744	243,345	757,914

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors clustered at the district level. Regressions include district and cohort fixed effects. See Table 2.5 for full set of controls.

Table B.12 Effect of Policy on 5th Grade SpEd Black Students

	Black Students					
	Absences			Disciplinary Actions		
	(1)	(2)	(3)	(4)	(5)	(6)
	% Days Absent	3+ Truant	10+ Truant	Suspended	Mult. Suspended	Expulsion
$SpEd_{d,2004} \times Post$	0.3177	-0.1417	-0.1313**	0.1025	0.1384	0.0485***
	(1.281)	(0.093)	(0.059)	(0.130)	(0.108)	(0.018)
$Disp_{d,2004} \times Post$	0.2140	0.0102	-0.0049	0.1941***	0.1689***	0.0185*
	(0.689)	(0.020)	(0.023)	(0.072)	(0.065)	(0.010)
Mean Dept Var	0.048	0.007	0.008	0.194	0.113	0.007
Observations	12,604,450	2,785,185	2,785,185	13,982,403	13,982,403	13,982,403
	Hispanic Students					
	Absences			**Disciplinary Actions**		
	(1)	(2)	(3)	(4)	(5)	(6)
	% Days Absent	3+ Truant	10+ Truant	Suspended	Mult. Suspended	Expulsion
$SpEd_{d,2004} \times Post$	0.5642	0.0063	-0.0839	0.0307	0.0065	0.0310
	(1.400)	(0.063)	(0.065)	(0.063)	(0.047)	(0.020)
$Disp_{d,2004} \times Post$	1.0456	-0.0499	-0.0286	-0.0806***	-0.0643****	0.0066
	(0.682)	(0.034)	(0.038)	(0.024)	(0.018)	(0.010)
Mean Dept Var	0.463	0.011	0.015	0.113	0.060	0.005
Observations	43,187,656	5,496,540	5,496,540	46,738,978	46,738,978	46,738,978

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. Regressions include district and cohort fixed effects. See Table 2.4 for full set of controls.

Table B.13 Effect of Policy on General and Special Education Spending

	SpEd Spending			
	(1)	(2)	(3)	(4)
	SpEd Spending Per All	SpEd Spending Per SpEd	Instr. SpEd Spending Per All	Instr. SpEd Spending Per SpEd
$SpEd_{d,2004} \times Post$	-1,341*	1,379	-1,067**	-1,167
	(752)	(4,132)	(490)	(3,009)
$Disp\,Black_{d,2004} \times Post$	-391	3,575	-294	2,635
	(331)	(3,283)	(249)	(2,728)
$Disp\,Hispanic_{d,2004} \times Post$	51	6,211**	92	4,835**
	(324)	(2,960)	(228)	(2,221)
Mean Dept Var	773	10,172	773	7,594
	General Education Spending			
	(1)	(2)	(3)	(4)
	GE Spending Per All	GE Spending Per GE	Instr. GE Spending Per All	Instr. GE Spending Per GE
$SpEd_{d,2004} \times Post$	1,112	-1,385	738	-827
	(1,019)	(1,166)	(703)	(794)
$Disp\,Black_{d,2004} \times Post$	783	785	42	-89
	(799)	(940)	(550)	(634)
$Disp\,Hispanic_{d,2004} \times Post$	-543	-717	-258	-504
	(724)	(827)	(500)	(552)
Mean Dept Var	4,272	4,781	3,480	3,892

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. Regressions include district and cohort fixed effects. See Table 2.7 for full set of controls.

Table B.14 Effect of Policy on Index Measure of Long-Run Outcomes

	SpEd Sample		**GE Sample**		**Aggregate Sample**	
	Black	Hispanic	Black	Hispanic	Black	Hispanic
	(1)	(2)	(3)	(4)	(5)	(6)
$SpEd_{d,2004} \times Post$	-0.0234	-0.1131***	-0.1352***	-0.0731**	-0.1163***	-0.0711***
	(0.040)	(0.035)	(0.035)	(0.031)	(0.031)	(0.025)
$Disp_{d,2004} \times Post$	0.0626***	-0.0115	0.0668***	-0.0302*	0.0693***	-0.0160
	(0.022)	(0.015)	(0.025)	(0.018)	(0.018)	(0.014)
Mean Dept Var	-0.276	-0.280	-0.036	-0.050	-0.120	-0.124
Observations	76,643	160,617	359,888	1,032,786	692,711	1,841,412

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. The outcome variable is a summary measure of all the long-run outcome variables. We standardize each outcome to have mean 0 and standard deviation 1, including indicators for high school graduation, college enrollment, associate's degree attainment, and bachelor's degree attainment. Then, we create one summary index of all the long-run outcomes by averaging across the standardized long-run outcomes for each individual. Specifications are run as before, with the summary index as the outcome variable. See Table 2.4 for full set of controls.

Table B.15 Effect of Policy on Students in GE as of 5th Grade Prior to Policy Implementation

	Black Students			Hispanic Students		
Math Exam in G8	(1)	(2)	(3)	(4)	(5)	(6)
$SpEd_{d,2004} \times Post$	-0.8433	-1.6107	-1.2532	-0.0650	-0.5120	-0.4822
	(1.025)	(1.171)	(1.075)	(0.822)	(0.861)	(0.835)
$Disp_{d,2004} \times Post$	0.4685	0.5404	0.5171	-1.1253**	-1.1937**	-0.9223*
	(0.539)	(0.590)	(0.553)	(0.476)	(0.465)	(0.476)
Mean Dept Var	-0.324	-0.324	-0.324	-0.165	-0.165	-0.165
Observations	288,021	288,021	288,021	876,316	876,316	876,316
Reading Exam in G8						
$SpEd_{d,2004} \times Post$	2.4213*	1.6562	1.6089	2.5012**	2.1601**	2.1472**
	(1.353)	(1.269)	(1.240)	(1.012)	(1.081)	(1.025)
$Disp_{d,2004} \times Post$	-0.3102	-0.2693	-0.1896	-0.6754*	-0.7476	-0.6473
	(0.668)	(0.690)	(0.659)	(0.378)	(0.489)	(0.444)
Mean Dept Var	-0.173	-0.173	-0.173	-0.184	-0.184	-0.184
Observations	288,511	288,511	288,511	876,416	876,416	876,416

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. All regressions include district and cohort fixed effects. See Table 2.5 for details on outcome variables, controls, and cohort restrictions.

Table B.16 Effect of SpEd and Disproportionality Indicators for All Students in GE in 5th Grade Prior to Policy Implementation

Math Exam in G8	(1)	(2)	(3)
$SpEd_{d,2004} \times Post$	-0.5828**	-0.6453**	-0.4044
	(0.260)	(0.269)	(0.247)
$Disp\,Black_{d,2004} \times Post$	0.3600	0.5546**	0.2356
	(0.239)	(0.248)	(0.240)
$Disp\,Hispanic_{d,2004} \times Post$	0.2328	-0.0286	-0.0520
	(0.194)	(0.209)	(0.185)
Mean Dept Var	0.094	0.094	0.094
Observations	2,320,827	2,320,827	2,320,827
Reading Exam in G8			
$SpEd_{d,2004} \times Post$	0.4410**	0.4890***	0.5174***
	(0.174)	(0.169)	(0.152)
$Disp\,Black_{d,2004} \times Post$	0.0924	0.2234	-0.0077
	(0.157)	(0.161)	(0.114)
$Disp\,Hispanic_{d,2004} \times Post$	0.1105	-0.1298	-0.1466
	(0.130)	(0.134)	(0.099)
Mean Dept Var	0.093	0.093	0.093
Observations	2,323,804	2,323,804	2,323,804
Individual Controls		X	X
District and Grade Controls			X

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. All estimates include district, year, and grade fixed effects. Regressions are run on students in GE as of 5th grade prior to the policy (up through K-cohort 1999). Test scores are measured in 8th grade, the last exam taken prior to the high school exit exam. Other outcome variables and controls are as defined in Tables 2.7.

APPENDIX C

CHAPTER 3 APPENDIX

Table C.1 Effect of Policy Change on Special Education Funding

	SpEd Funding Per All Pupils			SpEd Funding Per SpEd Pupils		
	(1)	(2)	(3)	(4)	(5)	(6)
$SpEd_{d,2007} \times Post$	-34.161***	-35.263***	-37.252***	3.082	3.046	10.940
	(2.455)	(2.151)	(1.813)	(126.228)	(128.796)	(137.994)
SpEd Fund Per SpEd $Pupil_{d,2007} \times Post$	-0.111***	-0.114***	-0.120***	-0.506**	-0.532**	-0.506**
	(0.007)	(0.007)	(0.007)	(0.216)	(0.214)	(0.237)
Pct Male		1.497	0.511		-184.557	-180.645
		(3.109)	(2.817)		(301.960)	(298.171)
Pct White		5.044**	2.442		-4.632	5.695
		(2.356)	(1.970)		(34.679)	(40.458)
Pct Black		2.098	2.733		-27.024	-29.544
		(2.813)	(2.416)		(57.518)	(57.515)
Pct Hispanic		2.646	0.906		-50.094	-43.192
		(2.594)	(2.243)		(39.236)	(34.072)
Pct FRL		0.893	0.419		19.732	21.614
		(0.964)	(0.752)		(16.165)	(18.334)
Total Fund Per Pupil			0.028***			-0.112
			(0.004)			(0.180)
Observations	5,690	5,690	5,690	5,690	5,690	5,690
R-squared	0.815	0.817	0.828	0.095	0.096	0.096
Mean Dept Var	693.30	693.30	693.30	4,881.84	4,881.84	4,881.84

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. All specifications include year fixed effects and district fixed effects. Controls include district-level percent male, white, black, Hispanic, and percent of students receiving free and reduced price lunch (FRL). The outcome is district-level special education funding, either per all pupils (SpEd and general Ed) or per SpEd pupils.

Table C.2 Effect of Policy Change on Special Education Funding

	Total Special Education Funding		
	(1)	(2)	(3)
$\text{SpEd}_{d,2007} \times \text{Post}$	-219,308***	-178,892***	-145,513***
	(83,180)	(56,881)	(42,246)
$\text{SpEd Fund Per SpEd Pupil}_{d,2007} \times \text{Post}$	-762.928***	-598.071***	-490.677***
	(160.007)	(139.682)	(147.746)
Pct Male		-105,948***	-89,406***
		(34,583)	(32,335)
Pct White		-73,390	-29,719
		(47,928)	(34,377)
Pct Black		137,428*	126,771**
		(71,960)	(64,423)
Pct Hispanic		63,460	92,648*
		(46,123)	(51,069)
Pct FRL		-81,903**	-73,945**
		(33,937)	(30,040)
Total Fund Per Pupil			-475.683***
			(158.660)
Observations	5,690	5,690	5,690
R-squared	0.968	0.973	0.976
Mean Dept Var	1,645,332	1,645,332	1,645,332

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. See Table 3.3 for description of controls.

Table C.3 Effect of Policy Change on Special Education Enrollment

	Percent Special Education		
	(1)	(2)	(3)
$SpEd_{d,2007} \times Post$	-0.207***	-0.206***	-0.201***
	(0.043)	(0.045)	(0.044)
SpEd Fund Per SpEd $Pupil_{d,2007} \times Post$	-0.0003**	-0.0002	-0.0002
	(0.0001)	(0.0001)	(0.0001)
Pct Male		-0.003	-0.001
		(0.094)	(0.093)
Pct White		0.075**	0.082**
		(0.033)	(0.032)
Pct Black		0.182***	0.180***
		(0.052)	(0.052)
Pct Hispanic		0.106*	0.111*
		(0.065)	(0.066)
Pct FRL		-0.026*	-0.024*
		(0.013)	(0.013)
Total Fund Per Pupil			-0.0001
			(0.0001)
Observations	5,690	5,690	5,690
R-squared	0.714	0.718	0.719
Mean Dept Var	16.62	16.62	16.62

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. See Table 3.3 for description of controls.

Table C.4 Effect of Policy Change on Standardized Exams

	Math Exam			Reading Exam		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{SpEd}_{d,2007} \times \text{Post}$	-0.072	0.054	0.069	0.552***	0.286***	0.253***
	(0.104)	(0.099)	(0.095)	(0.098)	(0.095)	(0.089)
$\text{SpEd Fund Per SpEd Pupil}_{d,2007} \times \text{Post}$	0.0002	0.0003	0.0003*	0.001***	0.001***	0.001**
	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0002)
Pct Male		-0.020	-0.018		0.186	0.183
		(0.124)	(0.124)		(0.130)	(0.127)
Pct White		-0.443***	-0.424***		0.327***	0.288***
		(0.072)	(0.071)		(0.098)	(0.090)
Pct Black		-0.504***	-0.508***		0.013	0.022
		(0.091)	(0.093)		(0.131)	(0.127)
Pct Hispanic		-0.572***	-0.560***		0.567***	0.542***
		(0.086)	(0.087)		(0.128)	(0.126)
Pct FRL		0.026	0.028		0.147***	0.141***
		(0.032)	(0.033)		(0.051)	(0.052)
Total Fund Per Pupil			-0.0002			0.0004**
			(0.0002)			(0.0002)
Observations	4,962	4,962	4,962	4,961	4,961	4,961
R-squared	0.610	0.620	0.621	0.927	0.934	0.934
Mean Dept Var	54.81	54.81	54.81	33.90	33.90	33.90

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Robust standard errors are clustered at the district level. See Table 3.3 for description of controls. The outcome variables denote the percent of students scoring proficient or higher on the 4th grade math and reading standardized exams.

Table C.5 Effect of Policy Change on High School Dropout

	Percent Dropout		
	(1)	(2)	(3)
$\text{SpEd}_{d,2007} \times \text{Post}$	0.483	0.608	0.563
	(0.506)	(0.463)	(0.424)
$\text{SpEd Fund Per SpEd Pupil}_{d,2007} \times \text{Post}$	0.003	0.003*	0.003*
	(0.002)	(0.002)	(0.002)
Pct Male		0.235	0.218
		(0.424)	(0.435)
Pct White		-0.007	-0.051
		(0.244)	(0.270)
Pct Black		-0.321	-0.310
		(0.313)	(0.299)
Pct Hispanic		-0.237	-0.259
		(0.355)	(0.341)
Pct FRL		-0.174	-0.179
		(0.211)	(0.217)
Total Aid Per Pupil			0.0004
			(0.001)
Observations	2,695	2,695	2,695
R-squared	0.606	0.608	0.609
Mean Dept Var	7.93	7.93	7.93

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. See Table 3.3 for description of controls. The outcome variables denote the percent of students who dropout of high school. This is calculated by dividing the total number of students dropping out of high school by the total high school enrollment each year.

Table C.6 Effect of Policy Change on Special Education Funding

	SpEd Funding Per All Pupils			SpEd Funding Per SpEd Pupils		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{SpEd}_{d,pre-2008} \times \text{Post}$	-32.428***	-31.595***	-32.609***	28.383	36.515	45.686
	(3.084)	(2.973)	(3.028)	(113.316)	(111.016)	(117.597)
Pct Male		3.637	3.165		-109.774	-105.505
		(4.127)	(3.967)		(298.730)	(296.333)
Pct White		2.549	0.904		-21.030	-6.150
		(2.608)	(2.378)		(39.674)	(44.758)
Pct Black		6.107	6.617*		-8.435	-13.042
		(3.770)	(3.646)		(55.282)	(55.427)
Pct Hispanic		2.000	0.915		-52.741	-42.928
		(3.123)	(3.005)		(35.247)	(31.544)
Pct FRL		-1.456	-1.823		8.911	12.227
		(1.541)	(1.433)		(20.621)	(22.731)
Total Fund Per Pupil			0.017***			-0.154
			(0.006)			(0.169)
Observations	5,842	5,842	5,842	5,842	5,842	5,842
R-squared	0.737	0.739	0.744	0.117	0.117	0.117
Mean Dept Var	693.30	693.30	693.30	4,881.84	4,881.84	4,881.84

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. All specifications include year fixed effects and district fixed effects. Controls include district-level percent male, white, black, Hispanic, and percent of students receiving free and reduced price lunch (FRL). The outcome is district-level special education funding, either per all pupils (SpEd and general Ed) or per SpEd pupils.

Table C.7 Effect of Policy Change on Special Education Funding

	Total Special Education Funding		
	(1)	(2)	(3)
$\text{SpEd}_{d,pre-2008} \times$ Post	-194,383**	-148,837***	-117,612***
	(77,132)	(53,164)	(40,502)
Pct Male		-92,854***	-78,321**
		(34,151)	(32,274)
Pct White		-86,321*	-35,660
		(51,252)	(37,100)
Pct Black		157,980**	142,294**
		(72,568)	(64,459)
Pct Hispanic		56,816	90,226*
		(49,686)	(52,373)
Pct FRL		-94,579***	-83,288***
		(31,476)	(27,877)
Total Aid Per Pupil			-523.728***
			(143.570)
Observations	5,842	5,842	5,842
R-squared	0.963	0.970	0.975
Mean Dept Var	1,645,332	1,645,332	1,645,332

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. See Table 3.3 for description of controls.

Table C.8 Effect of Policy Change on Special Education Enrollment

	Percent Special Education		
	(1)	(2)	(3)
$\text{SpEd}_{d,pre-2008} \times \text{Post}$	-0.292***	-0.293***	-0.289***
	(0.046)	(0.046)	(0.046)
Pct Male		-0.003	-0.001
		(0.090)	(0.090)
Pct White		0.077**	0.084**
		(0.035)	(0.034)
Pct Black		0.191***	0.189***
		(0.049)	(0.049)
Pct Hispanic		0.121*	0.126**
		(0.063)	(0.064)
Pct FRL		-0.026**	-0.024*
		(0.013)	(0.013)
Total Aid Per Pupil			-0.0001
			(0.0001)
Observations	5,842	5,842	5,842
R-squared	0.725	0.730	0.730
Mean Dept Var	16.62	16.62	16.62

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. See Table 3.3 for description of controls.

Table C.9 Effect of Policy Change on Standardized Exams

	Math Exam			Reading Exam		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{SpEd}_{d,pre-2008} \times \text{Post}$	-0.083	0.027	0.038	0.558***	0.293***	0.260***
	(0.102)	(0.096)	(0.093)	(0.111)	(0.100)	(0.094)
Pct Male		-0.015	-0.013		0.212	0.204
		(0.123)	(0.123)		(0.129)	(0.125)
Pct White		-0.432***	-0.416***		0.341***	0.293***
		(0.073)	(0.070)		(0.102)	(0.092)
Pct Black		-0.508***	-0.513***		-0.003	0.010
		(0.090)	(0.092)		(0.130)	(0.126)
Pct Hispanic		-0.564***	-0.554***		0.574***	0.544***
		(0.087)	(0.087)		(0.132)	(0.128)
Pct FRL		0.031	0.034		0.160***	0.151***
		(0.031)	(0.032)		(0.053)	(0.053)
Total Fund Per Pupil			-0.0002			0.0005**
			(0.0002)			(0.0002)
Observations	5,077	5,077	5,077	5,076	5,076	5,076
R-squared	0.608	0.618	0.618	0.925	0.933	0.933
Mean Dept Var	54.81	54.81	54.81	33.90	33.90	33.90

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Robust standard errors are clustered at the district level. See Table 3.3 for description of controls. The outcome variables denote the percent of students scoring proficient or higher on the 4th grade math and reading standardized exams.

Table C.10 Effect of Policy Change on High School Dropout

	Percent Dropout		
	(1)	(2)	(3)
$\text{SpEd}_{d,pre-2008} \times \text{Post}$	0.313	0.387	0.328
	(0.445)	(0.384)	(0.346)
Pct Male		0.215	0.186
		(0.415)	(0.421)
Pct White		0.066	-0.016
		(0.249)	(0.261)
Pct Black		-0.418	-0.391
		(0.360)	(0.328)
Pct Hispanic		-0.202	-0.244
		(0.363)	(0.346)
Pct FRL		-0.124	-0.136
		(0.191)	(0.201)
Total Aid Per Pupil			0.001
			(0.001)
Observations	2,717	2,717	2,717
R-squared	0.599	0.601	0.603
Mean Dept Var	7.93	7.93	7.93

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Robust standard errors are clustered at the district level. See Table 3.3 for description of controls. The outcome variables denote the percent of students who dropout of high school. This is calculated by dividing the total number of students dropping out of high school by the total high school enrollment each year.

Table C.11 Effect of Policy Change on Special Education Funding with Baseline Demographic Trends

	SpEd Funding Per All Pupils	SpEd Funding Per SpEd Pupils	Total SpEd Funding	Pct SpEd	Math	Reading	Dropout
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$SpEd_d \times Post$	-28.847***	53.805	-102,484**	-0.196***	0.035	0.242**	0.191
	(2.992)	(129.218)	(42,506)	(0.036)	(0.091)	(0.101)	(0.390)
Observations	5,690	5,690	5,690	5,690	4,962	4,961	2,695
R-squared	0.738	0.095	0.976	0.735	0.621	0.938	0.609
Mean Dept Var	693.30	4881.84	1,645,332	16.62	54.81	33.90	7.93

*** p<0.01, ** p<0.05, * p<0.1 Robust standard errors are clustered at the district level. All specifications include year fixed effects and district fixed effects. Controls include district-level percent male, white, black, Hispanic, and percent of students receiving FRL. Linear trends in the 2007 percent of black students and students receiving FRL are also controlled for.

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