Essays on Labor Economics and Education

by

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Abstract

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Deep ethnic and socioeconomic gaps in the access and quality of education are pervasive in the United States. Many of these inequalities are at least partly determined by a historical legacy of exclusionary public institutions, the vestiges of which continues to be felt today. In particular, three key contemporaneous education policy issues – public school segregation on the basis of race, the emergence of a potentially predatory for-profit college sector, and unequal college access for minorities – are all directly connected to public institutions. In this thesis, I present empirical studies on the role and effect that institutions have in determining these gaps, with varying focus on mechanisms and causal effects across these different policy topics.

In Chapter 1, I study school attendance boundary policy, the most common student allocation mechanism in U.S. public schools, and its relationship to school racial segregation. I ask: given existing patterns of residential segregation, what do existing school attendance boundaries reveal about local government’s preferences over school integration? Using a novel database on the attendance boundary maps of hundreds of school districts, I define a desegregation policy index based on simple counterfactual attendance boundary maps. Exploiting this index, I find wide heterogeneity in the extent to which districts choose to desegregate their school systems by gerrymandering boundaries. I develop a theory of school attendance boundary choice, based on a trade-off between racial integration and aggregate daily commuting distance to school. I propose a methodology
to estimate the extent of this trade-off, using geographic census data on the spatial distribution of race. Estimating a model of desegregation policy level as a function of marginal commuting costs, I find evidence of district demand for racial integration. In addition, I find that court desegregation orders and greater levels of racial tolerance among local whites act as positive shifters of desegregation demand. These findings have far-reaching policy implications, the most important being that the tools developed here allow researchers to better monitor local governments' policies. I close this chapter with a case study evaluating of the stability of desegregation policy with respect to endogenous residential sorting, finding high residential compliance rates and little real estate valuation effects stemming from sudden changes in attendance boundary policy.

Chapter 2, joint work with Christopher Walters, studies how different structures in post-secondary education markets affect local student populations. For-profit college chains (FPCs) have rapidly expanded over the last two decades, opening almost 1,000 campuses across the U.S. First, we examine the determinants of FPC entry, finding that counties with worsening local unemployment and poverty rates are more likely to see the opening of an FPC campus. Then, exploiting variation in the timing of FPC entry, we estimate the impact of FPC entry on enrollment and degree completions. Using an event-study framework, our estimates show that FPC entry leads to increases in county-wide college enrollment and degree completions, with effects concentrated in short-term certificate programs. Additionally, we find little indication of negative enrollment effects at traditional public and non-profit private institutions, including community colleges. We interpret these findings as indication that for-profit chain colleges tend to enter markets facing excess demand for higher education, and that the extent to which they directly compete with traditional colleges is limited at best.

In Chapter 3, I zoom-in to a narrower topic, focusing on the issue of college access for undocumented high school students. Specifically, I estimate the impact of state level tuition equity reform on the educational outcomes of undocumented immigrant students in Texas. This type of reform, granting in-state tuition to qualifying undocumented students, can be interpreted as a partial relaxation of the institutional constraints associated with lack of legal immigration status. Exploiting administrative data from education agencies in Texas, I formulate a generalized differences-in-differences framework to produce within-school, across-cohort estimates of the impact of the 'Texas Dream Act' on a range of educational outcomes from college demand to college-bound investments during high school. Estimates show a significant closing of the college demand
gap between immigrant and control group high school graduates. However, estimates regarding college-bound investments contain mixed results. I attribute this to a complex policy environment in public high schools during the analysis period. The results suggest that affordable college access policies can have a significant impact on the attainment of the immigrant population at the college entrance stage, but that, given other policies in place, college tuition incentives down the educational ladder may not be sufficiently salient to generate spillover effects.
To my mother, Adela,

_Nunca hubiera llegado a estas alturas sin ti. Después de tanta batalla, ha llegado la hora de la cosecha._
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Chapter 1

Attendance Boundary Policy and the Segregation of Public Schools in the United States

1.1 Introduction

Over the second half of the twentieth century, the United States embarked on, then retreated from, a concerted effort to integrate schools. The landmark Brown decision in 1954 held that the then status quo system of "separate but equal" schools for black and white children was unconstitutional. Through the 1960s, 1970s, and early 1980s, many large districts implemented, under federal judicial oversight, ambitious programs designed to create "unitary" school systems. The numerous consequences of this historic national project have been the focus of a vast literature in the social sciences (Coleman et al. 1975; Welch and Light 1987; Clotfelter 2004; Reber 2005, 2010, 2011; Cascio et al. 2008, 2010; Guryan 2004; Ashenfelter et al. 2006; Hanushek et al. 2009; Johnson 2011; Deming 2011; Boustan 2012; Billings et al. 2014; Reardon and Owens 2014).

Since the late 1980s, however, federal enforcement of local school integration efforts has declined. Judicial oversight of desegregation plans has largely been withdrawn, and most school districts have been left to make decisions affecting school integration locally (Jackson 2009; Baum-Snow and Lutz 2011; Lutz 2011; Reardon et al. 2012). The most relevant decision local school governments regularly face in this regard is the setting of school zoning policies – rules linking student addresses with school attendance rights, often summarized as maps, and commonly referred to as school attendance boundaries. These geographic assignment rules are by far the most common way in which local
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governing districts allocate students across the nation. In addition, school zoning is known to be updated frequently due to local demographic change and the opening of new schools. Zoning decisions are typically made by school district staff and by locally elected school boards, often with substantial involvement from local parents. As I show below, these decisions have enormous implications for the racial integration of schools.

While the era of judicial remedies for school segregation has been studied extensively, there is relatively little known about the role played by school zoning choices (Saporito and Sohoni 2006; Richards 2014; Saporito and Van Riper 2016).1 This paper aims to remedy that, taking advantage of newly available database characterizing the school attendance boundaries (henceforth, SABs) of nearly every school district in the country for the school year 2013-14. The main question I seek to answer using this unique data is: given existing racial stratification patterns across neighborhoods, what do school zoning choices reveal about district leaderships’ preferences for racial integration within their school systems?

The analysis centers around a novel index of local desegregation policy measuring the extent to which school boundaries ameliorate the link between residential and school segregation. I use census block data to compute the racial composition of SABs. The index is based on a comparison between SABs observed in the data and a counterfactual set of SABs that implement neighborhood schools in the strict mathematical sense – hypothetical zones that minimize the distance between schools and student residences. Specifically, I measure district desegregation policy as the difference in racial integration (as defined by a commonly used index), between (1), actual boundaries drawn by policymakers, and (2), counterfactual boundaries based on the concept of neighborhood schools.2 I present alternative methods to measure the policy index, for which the same qualitative results hold.

The average district in my sample – consisting of the majority of large school districts in the country (as defined by those managing at least 5 elementary schools) – has SABs that approximately adhere to the counterfactual neighborhood schools plan, generating a school system about as racially segregated as the residences in its jurisdiction. However,

1To my knowledge, Saporito and Sohoni (2006) were the first to study the relationship between school segregation and attendance boundaries. See also Sohoni and Saporito (2009), Richards (2014), and Saporito and Van Riper (2016).

2I define integration in terms of a commonly used segregation index – the exposure gap index or variance ratio index. See Coleman et al. 1975 and Reardon and Owens 2014.
I also document ample policy heterogeneity. The distribution of SAB desegregation is centered around zero, but it has a thick right tail, meaning that sizable minority of school districts set SABs that alleviate racial imbalances.

To explain this policy variation, I develop a model of a school district’s SAB choice as a trade-off between competing interests. I posit that districts draw SABs to maximize a utility function that depends on the level of racial integration and the aggregate distance that students must travel daily to get to school, with relative weights that may vary across districts. These weights characterize district heterogeneity in preferences for racially integrated schools, in terms of their willingness to increase aggregate commuting costs. The districts’ budget constraints are also heterogeneous – in some places the travel cost of increasing integration may be high due to residential sorting patterns, while in other places, where the spatial configuration of race across the jurisdiction is different, SABs can be drawn to increase school integration at a relatively small aggregate travel cost. Realized desegregation policy choices are therefore generated by a district-specific mix of taste and cost shifters that could in principle be correlated.

I use the insights of the model to develop an empirical test of its validity. First, I estimate the rate of transformation between aggregate travel distance and racial integration. I do so by combining census block data on the spatial distribution of racial composition and data on school locations. The estimates are generated by a simple rezoning algorithm that gradually changes attendance boundaries – starting from minimum distance SABs, it iteratively reassigns small groups of residences between schools, with the aim of increasing district-wide racial integration. The result is a district-specific estimate of the per-unit travel distance cost of integration; an estimate that explicitly relies on holding racial sorting patterns generated by residential choice fixed. I document considerable variation in these estimates, which I interpret as the "price" of desegregation.

The price estimates allow me to test whether school districts’ zoning policy choices suggest the existence of demand for integrated schools. Specifically, I estimate a model of desegregation policy on prices, testing whether desegregation levels can be explained by marginal travel costs. In a discussion of the identification assumptions necessary to interpret this estimate as a demand elasticity, I claim that these estimates suffice to reject that the elasticity is zero.

My estimates suggest that the demand for desegregation policy slopes down with respect to marginal travel costs. Establishing that districts are more likely to integrate
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boundaries when it is cheaper to do so validates my stylized model, but it also has policy implications. For instance, it demonstrates that the average district values racial integration when setting zoning policy, and it implies that student transportation subsidies would induce some districts to enact zoning policy that is more integrative.

Nonetheless, after controlling for prices, I still observe considerable variation in desegregation policy across districts. I attribute this residual variance to heterogeneity in district preferences for school integration. In other words, residual variation provides a measure of districts’ integration "effort", allowing me to estimate which district observables act as desegregation demand shifters.

District effort toward racial integration, outside of the context of judicial oversight, has never previously been measured – most analyses focus on the realized level of integration, which reflects both residential patterns and district preferences and in practice is very strongly correlated with the former. My measurement contributions allow me to generate important new evidence about the distribution of districts’ effective preference for integration, as revealed by their school zoning choices. I focus on two important school district characteristics that appear to be important shifters of demand for school integration: active federal judicial oversight and local beliefs about the causes of racial inequality.

By examining the relationship between school zoning and judicial oversight, I shed light on an important policy lever used by local officials to abide by court desegregation orders. I estimate that the small number of districts that remain under court supervision have significantly higher demand for desegregation – they have stronger desegregation policy controlling for commuting costs. On the other hand, the considerably larger number of districts that were formerly under supervision but have since been released, show no statistically significant difference in integration effort compared to districts that never faced a court order. Taken together, these results suggest that SABs respond to government pressure to alleviate imbalance, but that zoning is sufficiently flexible so that districts tend to revert to neighborhood schools zoning plans when such pressure is taken away. These findings are in line with earlier work on the resegregation of schools that accompanied the end of the federal oversight era (Clotfelter, 2004; Reardon et al., 2010; Lutz, 2011).

I conclude the desegregation demand analysis with an investigation of the link between the racial tolerance of local white residents and school district integration effort.
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Following the earlier work of Cutler, Glaeser, and Vigdor (1999) and Card, Mas, and Rothstein (2008), I exploit census tract level data on racial attitudes from the General Social Survey to construct a district level index of racial intolerance on the part of local white residents. I find that racial intolerance from whites is highly predictive of lower demand for desegregation in school zoning choices. This result is robust to a range of specifications and, to my knowledge, I am the first to document such a link. This finding underscores the continuing role of white animus toward racial diversity as a barrier to school desegregation policy.

A central assumption in this analysis is that residential racial sorting patterns are fixed. This is reasonable, given the high stability of neighborhood composition over time (Card et al. 2008) and the relative frequency of SAB changes. Nevertheless, it is well documented that aggressive judicial desegregation efforts – and exposure to minorities in general – often led to "white flight" from urban school districts (Boustan 2010, Baum-Snow and Lutz 2011, Boustan and Margo 2013). It seems plausible that district-initiated integration efforts via school rezoning could produce a similar reaction. If so, this would serve as an additional constraint on the ability of a district’s leadership to achieve integration through SAB changes, and could invalidate some of the results.

To examine the robustness of my analysis to this caveat, I use drastic SAB changes in Charlotte Mecklenburg Schools in North Carolina as a case study. In 2002, in response to litigation challenging its desegregation plan and no longer under court supervision, CMS redrew its SAB map entirely. The previous map had used busing and highly non-contiguous attendance zones to achieve 30% more racial integration than would be supported by residential patterns alone, while the new SABs used a neighborhood schools plan, and reduced the travel distance per capita by 78%. This led to changes in school assignments for thousands of residences, scattered throughout the city. Some homes were reassigned from integrated schools to schools that were nearly 100% minority, while others were reassigned to nearly all white schools.

I contribute to the existing literature on "the end of busing" in Charlotte by estimating the causal effect of SAB racial composition on residential composition, over the succeeding ten years (Kane et al. 2005; Jackson 2009; Deming 2011; Billings et al. 2014; Tannenbaum 2015; Weinstein 2016). My estimates are based on comparisons in the 2010 fraction minority of residents within small neighborhoods that underwent school rezoning, but that were assigned to the same school prior to rezoning and that had similar
racial composition in 2000. The treatment is the 2010 fraction minority of the schools’ new attendance boundaries, measured using 2000 residential composition, thus isolating variation generated by the school rezoning reform. The identification assumption is that, controlling for the aforementioned factors, CMS officials did not select residences based on the dynamics of residential segregation when rezoning schools.

The findings indicate that the reassignment of neighborhoods to schools with higher minority composition led to limited residential white flight and had no effect on real estate values. I estimate that a reassignment leading to a 10 percentage point increase in school fraction minority reduces the residential fraction white of the neighborhood by 1.5 percentage points – suggesting an 85% white residential compliance rate with respect to integrative school rezoning. Moreover, across several specifications and robustness checks, I find no significant effects on real estate values, which is consistent with other studies concluding that the price mechanism is not the sole force driving residential segregation (Kruse 2005; Card et al. 2008). Insofar as the case study’s results generalize to other districts considering school rezoning, it implies that residential patterns will undo about 15% of the integrative effect of desegregation policy over a decade – an encouraging stylized fact for the main analysis, considering the relative frequency with which SABs are updated.

My paper is related to a number of literatures. One is the research on the effects of school desegregation policies which I cite above. This work nearly exclusively focuses on court-ordered policies in a much earlier era. There is much less work on districts’ unsupervised choice of SABs. The closest papers to mine are Richards (2014) and Saporito and Van Riper (2016), who measure district effort by the "bizarreness" of school attendance zone shapes (a measure drawn from the congressional gerrymandering literature. See Chen and Cottrell (2016) for an investigation similar in spirit to mine). While this captures something about district effort, it is often possible to achieve integration without adopting bizarre SABs. Moreover, bizarreness reflects both preferences and residential patterns – in a city with a 'checkerboard' residential layout, it is possible to achieve a high degree of integration or segregation with relatively little bizarreness.

My work is also related to the political economy literature on diversity and governance (Alesina et al., 2004). There is relatively little work in this literature on school boards, though this hyperlocal level is where much of the action occurs. One challenge is that it can be difficult to measure policy choices at this level. Macartney and Singleton (2017)
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estimate the effect of the partisan composition of school boards on segregation, finding that a higher Democratic share leads to lower segregation in the district. They provide suggestive evidence that the mechanism at play is SAB changes. My measurement of districts’ actual policies is complementary to theirs, and strengthens the point that school boundaries are levers that districts use to achieve this goal.

This paper also relates to the mechanism design literature on student assignment (see Pathak 2011 for a review), which focuses on parental choice assignment plans. The recent growth in this literature demonstrates that student assignment policy is an active area of both empirical and theoretical research. Insofar as residential zoning is a much more common assignment mechanism than parental choice across school districts, I consider my work to be as policy relevant as this literature.

My work makes several important contributions. First, I develop, implement, and validate a new index evaluating local school districts’ zoning choices. To my knowledge, my policy index is the first to quantitatively measure local governments’ choice over the degree of racial equity in their school systems. This is of course directly relevant to the segregation of schools, but is also one of the first measures of local governments’ policy stances, and will be useful in political economy studies of local government decision-making. Second, I characterize the cross-sectional distribution of desegregation and develop tools enabling the implementation of a demand analysis of school desegregation policy, producing several useful stylized facts.

Finally, I demonstrate that district policy choices have significant, yet modest, effects on neighborhood composition. This feedback effect is extremely important to designing integration policy in an era when the courts are mostly absent from the decision and therefore districts are constrained in the extent to which they can push back against residents’ preferences. It is also relevant in a large number of other settings where local governments are asked to enact policy priorities passed down from a higher level of government, as it demonstrates that so-called "Tiebout choice" is a real constraint, not just on tax and spending decisions but also on decisions explicitly focused on externalities for which the Tiebout mechanism is not likely to lead to efficient outcomes.

The rest of the paper proceeds as follows. Section 2 describes the historical background of school segregation and local governance, as well as institutional details of school attendance boundary policy. Section 3 develops minimum travel distance SAB counterfactuals and implements an index to describe the distribution of desegregation policy.
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Section 4 presents a model of school attendance boundary choice. Section 5 analyzes the travel cost of desegregation and develops an algorithm to estimate prices. Section 6 studies desegregation demand shifters, characterizing observable drivers of heterogeneity in policy. Section 7 presents the case study of abrupt boundary changes in Charlotte, estimating the causal effect of school boundary racial composition on neighborhood racial composition. Section 8 concludes.

1.2 Background

In this section I describe the institutional background of attendance boundary policy as it relates to school segregation. I provide a brief summary of the history of federal involvement in school desegregation and its relation to local school governance. Then, I discuss the institutional details of school zoning policy, focusing on two key facts: (i) SABs are the most common public school assignment mechanism across school districts; and (ii), SABs change with frequency due to neighborhood demographic change and new school construction.

School Segregation and Local Governance

In the mid twentieth century the United States placed a mandate on local education agencies to end the racial segregation of schools. The Supreme Court’s landmark Brown v. Board of Education decision in 1954, ruled that the ‘separate but equal’ school doctrine was unconstitutional. While the Brown decision marked the end of de jure segregation of schools, actual desegregation efforts did not begin until subsequent Supreme Court decisions forced school districts to act against de facto school segregation as well. It was during this era that many districts were placed under desegregation orders by lower federal courts and school segregation decreased dramatically across the nation.

However, more recent Supreme Court rulings have pushed in the opposite direction, severely altering the legal basis of court-ordered desegregation plans. For instance, Board of Education v. Dowell (1991) established that desegregation decrees were not permanent. The court ruled that school districts could be released from oversight by demonstrating they had complied in good faith and vestiges of past discrimination had been eliminated, regardless of contemporaneous school segregation levels. During this period many of
the school districts formerly under desegregation orders were declared "unitary" by the courts, being subsequently released from judicial oversight. While a minority of desegregation orders remain open today, the vast majority of districts that were once under federal oversight have been released from it. Today, the modal school district has freedom to choose whether or not to desegregate its schools. Whether they do so depends on the views of local school boards and communities, as well as the feasibility and transportation cost of desegregation, given existing residential segregation patterns.

Still, the end of the era of federal desegregation orders was not intended to signal the end of the mandate over local school desegregation. The most recent Supreme Court case on the topic made this point clear, even as it declared a desegregation plan unconstitutional. In PICS v. Seattle School Dist. No. 1, (2007), parents brought legal action challenging, via an equal protection argument, a student assignment plan that used individual racial classification to allocate slots in oversubscribed high schools. The court ruled in favor of the parents, declaring that the individualized use of race in student assignments was unconstitutional. While Supreme Court Justice Anthony Kennedy joined the majority opinion of the Court, he wrote a separate opinion to emphasize that the decision should not be understood as prohibiting local authorities from considering the racial makeup of schools. Kennedy recognized that public school districts have a compelling interest in both achieving diversity and avoiding racial isolation in schools. He went on to recommend policy alternatives to achieve school integration:

"School boards may pursue the goal of bringing together students of diverse backgrounds and races through other means, including strategic site selection of new schools; drawing attendance zones with general recognition of the demographics of neighborhoods; allocating resources for special programs; recruiting students and faculty in a targeted fashion; and tracking enrollments, performance, and other statistics by race." – Parents Involved in Cmty. Sch. v. Seattle Sch. Dist. No. 1, 551 U.S. 701, 720 (2007)

Notably, it was Justice Kennedy’s consideration that the manipulation of school attendance boundaries may be an effective way of achieving integration. These recommendations have since been published by the Department of Education as part of a document entitled "Guidance on the Voluntary Use of Race to Achieve Diversity and Avoid Racial Isolation in Elementary and Secondary Schools" (Department of Education, 2011).
Institutional Background of School Attendance Boundary Policy

School attendance boundaries (SABs) are zoning policies that link public school attendance rights with student residential addresses. Any assignment policy that links student addresses to schools can be summarized with a SAB map partitioning a district’s jurisdiction into zones corresponding to different schools. Importantly, it need not be the case that these zones be contiguous or compact. In fact, many districts draw discontiguous attendance boundaries in which spatially separate neighborhoods are given the same school assignment. Such assignments are typically associated with student busing schemes.\(^3\)

SAB policy is the most widely used student assignment mechanism in U.S. public school systems. According to the main database used in this study – the School Attendance Boundary Survey (SABS) – 95% of public schools in the U.S. operate residential zoning plans.\(^4\) Even in notable exceptions such as San Francisco USD and New York City Department of Education – both of which run full parental choice school assignment systems – the first layer of the assignment process is determined by student addresses. Consequently, by studying SAB policy, I am making statements that are relevant to most public school districts in the U.S.

Local school district officials draw school boundaries – effectively doling out rights to a key public good – with little federal or state government oversight. Anecdotally, the process involves the following steps: (i) a new SAB map is proposed by district staff; (ii) the school board makes public this proposed change, often times directly notifying residences subject to reassignment; (iii) Public meetings take place for individuals to voice their support or concern with the proposed changes; (iv) the board votes to approve the plan or to amend it. It is common for SAB changes to generate considerable community debate, with some members furiously opposing reassignments (Hannah-Jones, 2016). This is perhaps unsurprising, given the large empirical literature on the real estate capitalization of SABs (Black, 1999; Kane et al., 2003, 2005; Bayer et al., 2007).

\(^3\)In the typical school district, SABs can be thought of as a strict partition of the district’s jurisdiction into smaller polygons, each associated with a school. The number of polygons may be equal to the number of schools, or it may be greater, as is the case with student busing schemes involving discontiguous SABs.

\(^4\)Panel B in Table 1, provides the basis for this claim, showing that only about 5% of schools in the available nationwide data are ‘open enrollment’, in the sense that residences play no role in student assignment. Moving from column (1) to column (2) shows that more than half of open enrollment schools are located in small districts, typically rural, districts that administer a single school, know as ‘de facto’ school districts.
Additionally, SABs tend to be updated with relative frequency due to local demographic change and new school construction. While there is limited data available to assess the rate at which these changes occur directly, local media and school district websites provide extensive evidence for this claim. Local news media often covers stories informing parents about proposed changes to school boundaries (Hannah-Jones, 2016). Moreover, most school district websites feature a section explaining recent boundary changes, and the process undergone by officials to change them. Some commonly cited reasons for boundary changes include travel distance to school, neighborhood demographic change (e.g. school overcrowding), and new school construction. Thus, school districts seem to re-optimize boundary policy frequently, as it is a relatively cheap way of adjusting school populations in light of local demographic change.

1.3 School Attendance Boundary Desegregation

I now develop an empirical framework to measure and describe the extent to which existing SABs ameliorate or strengthen the link between residential and school segregation. I first describe the database on the universe of elementary SABs (for SY 2013-14) which I exploit for the rest of the study. Then, I propose a counterfactual SAB policy – mimicking a "neighborhood schools" plan – which provides a useful baseline to evaluate boundaries observed in the data. I conclude presenting a descriptive analysis of SAB desegregation policy defined relative to this counterfactual.

Data

The main data source enabling this study is the School Attendance Boundary Survey (SABS), administered by the National Center of Education Statistics (NCES) of the Department of Education, for the school year 2013-14. This survey is the Department’s first attempt to collect and harmonize the SAB maps of school districts across the U.S. I am chiefly interested in exploiting this data to measure the intended population and racial composition of schools, as generated by their corresponding SABs and the underlying residential layout. I do so by linking the SABS database to census block geography from the 2010 U.S. Census. Specifically, I link census blocks to schools by asking if the centroid of the census block located within a school’s 2013-14 SAB. I then compute total SAB population and the fraction of residents that are minorities – as defined by
black and hispanic residents. Then, aggregating these SAB level fractions into district level segregation indices allows me to measure the intended level of school district racial integration. See Appendix Figure 4 for an illustration of eight school district observations in the data.

To supplement the analysis, I bring in data from additional sources. First, I obtain data on school locations and other school level characteristics from the NCES Common Core of Data corresponding to SY 2013-14. Second, I make use of 2010 census block group data to measure median household income at the school district level. Third, I exploit the database on the status of school district desegregation orders made publicly available by Reardon et al. (2012), via Stanford’s CEPA. Finally, following the work of Card et al. (2008), I utilize census tract level data from the General Social Survey to compute an index of racial intolerance among white residents at the school district level.

I restrict attention to districts facing considerable choice when zoning schools by limiting the analysis sample to districts administering at least 5 elementary schools. The final analysis sample contains 1,519 school districts. In addition, I focus the analysis on elementary schools. A parallel analysis with middle and high schools is available upon request. Please see Appendix A for a more detailed description of the data build procedure and sample selection.

Table 1 Panel C summarizes the characteristics of school districts in the analysis sample. The average district in my sample administered approximately 15 elementary schools and has a total population of about one hundred thousand residents. On average, the district population in the sample is 27.4% minority. Median household income in the average district is approximately $56,000 (2010 USD). Furthermore, 99 school districts (7% of the sample) remain under active court supervision over school desegregation efforts, while 166 district (11% of the sample) were previously under judicial oversight but have since been released.

**SAB Desegregation – An Illustrative Example**

School districts face a fundamental trade-off when devising school zones for their jurisdiction. While minimizing the daily distance students must travel to school is a natural objective of SABs, such plans will replicate existing patterns of residential racial segregation. If the district is interested in alleviating school segregation, it must then partly
sacrifice the minimum distance objective. The following provides an intuitive example illustrating this point, as well as introducing the concept of minimum distance SAB counterfactuals.

How would the leadership of a school district go about drawing school attendance boundaries? As an example, Panel (1) of Figure 1 shows a map of Springfield School District No. 186 in Illinois. The map is composed of the census blocks that make up the district’s jurisdiction. The heat coloring denotes the fraction of residents in a given block that are minorities. Notably, Springfield is residentially segregated in a manner reminiscent of the archetypical American city. Near downtown, there is a pocket of residences that are almost exclusively inhabited by minorities. In contrast, residents in the outskirts of the city tend to be almost exclusively white. The red circles in this map denote the location of elementary schools that are administered by Springfield school district.

A natural objective of school boundaries is to minimize the distance that students must travel daily to school. Panel (2) of Figure 1 shows what school attendance boundaries would look like if student allocations focused on minimizing distance. The thick black lines in this map denote the hypothetical boundaries generated by such an assignment. Generically, minimum distance zoning will have as many attendance zones as there are schools, with schools located approximately in the center of each zone. This allocation is a "neighborhood schools" rule in the strict sense. Residential blocks are literally assigned to the nearest school, defined using the euclidean metric ("distance as the crow flies"). For all districts in the data, I assume that these minimum euclidean distance counterfactuals approximate the distribution of SAB racial composition in a minimum commuting cost student assignment policy. I refer readers interested in a discussion on the discrepancy between euclidean and true travel distance, as well as the role of school capacity to the Appendix, where I provide several robustness checks on this assumption.

Neighborhood schools assignment plans replicate residential racial segregation patterns in the school system. To see this, Panel (3) of Figure 1 summarizes the SAB racial compositions generated by the minimum distance assignment. The heat coloring denotes the SAB fraction minority of residences, which I compute by summing the total number of minorities residing within these hypothetical zones and dividing by the corresponding

---

5In mathematics, defining a set of points (school locations) and partitioning a space into minimum distance zones around them is known as the Voronoi map. To my knowledge, Richards (2014) was the first to use Voronoi maps as counterfactuals for SABs.
total population. Noticeably, under such an assignment there would be two schools near
the center of the jurisdiction with a more than 60% minority population assignment,
while schools closer to the edge of the district would be assigned a population that is less
than 20% minority.

Panel (3) of Figure 1 also reports a school racial integration index and the average
distance travelled per student under the hypothetical neighborhood schools zoning. I
define school racial integration as

\[ I = 1 - Seg \]  \hspace{1cm} (1.1)\]

where \( Seg \) is a district level segregation index – the exposure gap index – a function
SAB level racial compositions.\(^6\) Given this definition, \( I = 1 \) corresponds to perfect racial
integration, and \( I = 0 \) to perfect segregation.

Under minimum distance SABs, Springfield has a racial integration index of \( I^o = 0.821 \), implying that the average minority student would attend a school with 18 percent-
age points higher fraction minority than the school attended by the average white student
in the district. Moreover, each student would travel \( D^o = 1.12 \) kilometers on average
to get to school. Average distance to school is computed by summing the population-
weighted euclidean distance between each census block centroid and the location of its
assigned school and dividing by the total population in the district.

In order to achieve higher levels of school racial integration, the district must increase
daily school travel distance. Panel (4) of Figure 1 shows dark lines denoting the actual
school attendance boundaries enacted by this district for the 2013-14 school year. The
heat coloring denotes the racial composition of actual school assignments. The visually
apparent lack of variation in color demonstrates that the existing SABs have similar
racial compositions – between 20-40% minority each. Racial integration in this map is
\( I = 0.978 \), a level higher than the counterfactual plan by \( I - I^o = 0.157 \), almost 16

\(^6\)The "exposure gap" segregation index (also known as the "variance ratio" index) is defined as

\[ Seg = \frac{1}{N^m} \sum_{s \in S} n^m_s m_s - \frac{1}{N^w} \sum_{s \in S} n^w_s m_s \]

where \( N^m \) and \( N^w \) are the total number of minority and white students in the district; \( S \) is the set of
schools administered by the district; \( n^m_s \) and \( n^w_s \) are the number of minority and white students in school
\( s \); and \( m_s \) is the fraction of students in the school that are minority. Thus, segregation is defined as the
racial gap in average exposure to minorities.
percentage points. Relative to the neighborhood schools counterfactual, this district is considerably desegregated.

Springfield achieves school integration by drawing highly discontiguous SABs and accepting longer daily travel distances. Note in Figure 1 Panel (4) that the center of the city is fragmented into several small polygons – these are small neighborhoods which are assigned to schools outside of their immediate vicinity. This generates higher distance travelled per student – in actual SABs distance per student is $D = 1.899$ kilometers, meaning that students travel an excess distance of $D - D^o = 0.771$ kilometers relative to the minimum distance counterfactual.

The Distribution of SAB Desegregation

Generalizing the intuition of the above example, I compute counterfactual minimum distance SABs for all 1,519 districts in the sample, enabling the systematic evaluation of existing SABs across school districts. I propose two methods of measuring SAB desegregation relative to these counterfactuals. The first is a regression-based approach providing a useful summary of the link between residential and SAB segregation, as well as the mechanisms of desegregation plans. The second is a decomposition approach that breaks up the SAB racial integration index into a component due to residential segregation and a component due to SAB gerrymandering, the latter of which I term "desegregation policy". I employ both methodologies in a descriptive analysis of the distribution of SAB desegregation.

Figure 2 presents scatter plots summarizing the SAB choices of four school districts with a simple regression framework. In the horizontal axis I plot the racial composition of school "neighborhoods" – as defined by minimum distance SABs – ordered by their racial composition from lowest to highest fraction minority. The vertical axis measures the racial composition of the school’s actual assignments in SY 2013-14. This bivariate relationship is informative of district attempts to ameliorate the link between residential and school segregation. If the OLS fit between these two variables equals one, the district has chosen boundaries that are essentially replicating residential segregation patterns. I therefore show the 45 degree line for reference, as well as the OLS fit of this relationship. In addition, I report in the top left of the plots the OLS coefficients, as well as the integration index of assignments ($I^o$) and neighborhoods ($I^o$).
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The top left panel of Figure 2 corresponds to the working example, Springfield School District No. 186 in Illinois. As shown above, this district’s SABs are highly integrative, manifested in this figure by an OLS slope much lower than one. In addition, the plot elucidates the mechanism Springfield SD uses to achieve such an integrated system. Three schools with relatively high neighborhood minority composition are given assignments that are considerably lower than 40% minority. The pattern suggests that white students from the city outskirts are assigned schools in the city center, nearby high minority neighborhoods. In essence, white students are bused from the suburbs to schools downtown. In contrast, the district plotted on the top right of Figure 2 – corresponding to Midland Independent School District (ISD) in Texas – also desegregates SABs, but using a mixed approach. Midland reduces the link between residential and school segregation by drawing some SABs that bring minorities to schools located in white neighborhoods, and some that do the opposite.

The bottom panels of Figure 2 demonstrate that not all districts are willing to increase daily travel distances to achieve school integration. The bottom left shows a large urban school district – Philadelphia City School District in Pennsylvania – which replicates residential segregation in its 148 SABs almost exactly. In addition, some districts exacerbate neighborhood segregation in their school systems with the way they draw SABs. For instance, Dysart Unified School District in Arizona – the district summarized in the bottom right panel of Figure 2 – appears to do this by sending additional minority students to schools that are located in high minority neighborhoods, such that the neighborhood schools counterfactual is more racially integrated than the district’s existing SABs.

I summarize the distribution of SAB desegregation across school districts by computing the OLS gradients exemplified in Figure 2 for all districts \( j \) in the sample, and plot \( 1 - \beta_j \) in a district histogram presented in Figure 3. Values close to zero correspond to OLS coefficients close to one – districts that replicate residential segregation in their school systems. Positive values in this scale represent districts that ameliorate the link between residential and school segregation with their school zoning policy. Negative values refer to districts that draw SABs that worsen segregation in their schools, relative to the minimum distance counterfactual.

Figure 3 provides a telling picture of the empirical distribution of SAB desegregation. The mean district in the data enacted SABs to the right of zero in this scale,
suggestive of some integrative policy, but achieving very modest levels of desegregation. However, this masks considerable heterogeneity. The distribution has a thick right tail, implying that a minority of districts do a lot more to gerrymander SABs to achieve racial integration than the mean district. In contrast, a much smaller fraction of school districts appear to draw them to worsen SAB racial segregation.

The regression-based approach to measuring SAB desegregation is useful for descriptive analysis, but it does not bear a clear economic interpretation. To address this, I present an alternative measure of SAB desegregation policy based on a commonly used segregation index with direct economic interpretation (Card and Rothstein, 2007; Reardon and Owens, 2014), the "exposure gap" or variance-ratio index. Using the exposure gap, I compute the integration level of existing SABs and perform the following decomposition

\[ I_j = I^o_j + I_j - I^o_j \]

The first component, \( I^o_j \), is the level of integration achieved in the hypothetical minimum distance SABs. I term this component neighborhood integration, as minimum distance SABs replicate existing neighborhood segregation. The second component is the difference in the integration of existing SABs \( I_j \) and the integration of neighborhoods \( I^o_j \). I term this component "SAB desegregation policy". If this component is positive, the district has drawn SABs that generate a more racially integrated school system than a neighborhood schools plan would.

Table 1 Panel A summarizes the distribution of each component across districts in the sample. Column (1) shows averages for the full sample. The mean district has a total SAB integration level of 0.936, meaning that the average district had an exposure gap of about 6 points.\(^7\) The neighborhood integration component \( I^o \) – with a mean of 0.929 – drives the bulk of district variation in integration levels. However, the desegregation policy index \( I_j - I^o_j \) suggests that the average districts desegregates marginally, increasing integration relative to neighborhood schools by about 1 exposure point (the mean is .007). Moreover, the median district in the sample shows practically zero desegregation policy, again suggesting that the distribution of desegregation policy has a thick right tail –

\(^7\)As a point of reference, Charlotte-Mecklenburg Schools (NC) moved from integrated boundaries to a neighborhood schools scheme in 2002 – this is the subject of a case study in Section 6. The policy change led to a 20 point drop in the integration of CMS schools, from 0.801 to 0.610. Billings et al. (2014) document that considerable negative effects on student outcomes were caused by this change in school boundaries.
a minority of districts do a relatively good job at drawing integrative SABs. These patterns are consistent with the regression-based evidence in Figure 3. I summarize the neighborhood-assignment OLS gradient index in Panel A for comparison purposes.

Panel B in Table 1, summarizes the distribution of student distance to school in existing and counterfactual SABs, measured in kilometers. In the mean district, students reside 2.5 kilometers away from their assigned school. If all students were instead systematically assigned to the school that is closest to their home, students would live 2.06 km from their school. This implies that, relative to the minimum distance counterfactual, the average district has SABs that make students travel an excess distance of 0.44 kilometers.

Another interesting aspect of the distribution of the decomposition SAB integration is how it varies across space. Figure 4 displays two heat maps of mainland U.S. at the school district level. The small polygons correspond to the jurisdictions of the governing school districts in my sample. The heat coloring measures eight quantile categories in the cross-sectional distribution of each variable. The map in the top shows the spatial distribution of the neighborhood segregation component $I^o$ of SAB integration. One notable pattern is that the south’s school districts – typically defined along county lines for these states – have some of the most segregated neighborhoods. School districts in the urban centers of the west, specially in California, are also more residentially segregated than the median district in the sample.

The bottom map in Figure 4 corresponds to the spatial distribution of SAB desegregation policy component $I - I^o$. There is ample policy heterogeneity across space even within states. SAB desegregation is remarkably absent in residentially segregated California with the exception of a few districts. The majority of school districts that enact above median desegregation policy are located in the south. This makes sense given that the south was the focus of the desegregation movement of the 1960s and 70s, and it is also the region where the majority of districts that remain under judicial supervision are located (I formally test the link between court orders and policy in Section 6).

The evidence presented above provides partial validation of the proposed index of SAB desegregation – considering the historical legacy of desegregation efforts, the index is concentrated where one would expect to be. Another way to validate this index is by asking whether it is correlated with intermediate outcomes that one would naturally expect to be the affected by school desegregation. For instance, studies have documented
large consequential racial gaps in certain school attributes within segregated school systems, including teacher experience, student retention, and exposure to student tracking programs (Hanushek and Rivkin 2006; Jackson 2009; Card and Giuliano 2015, 2016). If my methodology captures SAB desegregation policy correctly, the index should be associated with smaller gaps in these intermediate school outcomes. I test for a link between desegregation policy and these intermediate outcomes by constructing district level racial gaps in these school characteristics using SY 2011-12 school level data from the Office of Civil Rights of the Department of Education. I define racial gaps as the difference in district level means between whites and minorities (blacks and hispanics). Teacher experience is measured as the fraction of school teachers in their first or second year in the profession; student retention rates by race are reported directly by the OCR; student tracking is measured as school level presence of the "gifted and talented" (GT) program.

Table 2 reports the results from a regression of racial gaps on school characteristics on the SAB desegregation policy index. For each outcome, I report results for a simple OLS regression as well as models controlling for state fixed effects, log population, population percent minority, log median household income, and the neighborhood integration level $I^o$. By controlling directly for neighborhood schools integration in the second specification, I allow SAB integration and neighborhood integration to have separate effects on these outcomes, and the coefficient on desegregation policy is to be interpreted as the effect SAB policy controlling for residential segregation patterns. Columns (1) and (2) present results for the gap in teacher experience. The mean of this variable is negative suggesting that minorities are more likely to be exposed to inexperienced teachers. Encouragingly, desegregation policy significantly closes this gap, a link that becomes much stronger when adding controls.

Columns (3) and (4) in Table 2 test the link between desegregation policy and the racial gaps in student grade retention rates. The mean of the dependent variable is also negative, meaning that on average minority students are more likely to repeat a grade than their white counterparts. The link between desegregation policy and retention gap is not significant in the univariate model in column (3), but it becomes significant and negative one I control for covariates. Finally, columns (5) and (6) estimate the relationship between SAB desegregation and the racial gap in exposure to the GT program. This gap has a positive mean implying that white students are more likely to be exposed to the GT program than minorities. SAB desegregation is significantly predictive of smaller district gaps in this outcome in both the univariate and controlled specifications – districts with
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more integrative SABs tend to see much smaller racial gap in student tracking.

Altogether, the results in Table 2 suggest that the SAB desegregation index constructed in this section does a good job of capturing district zoning policy that increases racial equity in the school system. Having established the validity of this policy measure, the rest of the analysis focuses on the the trade-offs school districts face when considering desegregation policy with the gerrymandering of SABs.

1.4 A Theory of School Attendance Boundary Choice

The evidence offered above documents the presence of wide heterogeneity in SAB desegregation policy across districts. To explain this variation, I posit a theory of SAB choice modeled as a trade-off between competing interests for the school district’s leadership – racial integration and aggregate daily student travel. Noting that student busing expenses have been historically cited as one of the most salient costs associated with school desegregation (Coleman et al. 1975; Welch and Light 1987; Clotfelter 2004), such a model can be naturally motivated given the legacy of the desegregation era. It is also possible to motivate this model is empirically.

Figure 5 shows a binned scatter plot summarizing the bivariate relationship between SAB desegregation and mean excess travel distance to school across districts. The figure details a significant positive correlation between SAB desegregation and excess student travel. The excess distance variable must be positive by construction, but the fact that districts that make each student travel an excess of 1 kilometer additional than necessary are precisely the ones that desegregate schools most suggests a trade-off – given pervasive residential segregation, higher levels of student distance are necessary to achieve greater SAB integration. I formalize the concept of a policymaker evaluating this trade-off when drawing SABs in a simple utility maximization model.

Consider a district policymaker \( j \) with the task setting school attendance boundaries. The policymaker chooses school boundaries to maximize a utility function \( u_j(I,D) \) defined over school integration \( (I) \) and distance travelled to school per student \( (D) \). Assume that \( \frac{\partial u_j}{\partial I} \geq 0 \) and \( \frac{\partial u_j}{\partial D} < 0 \) for all \( j \)’s.\(^8\) Each district has a benchmark set of boundaries

\(^8\)It is natural to assume that district utility is decreasing in distance travelled per student. Assuming that utility is increasing in integration, on the other hand, may be more controversial. I focus the theory on districts that seek to ameliorate segregation considering that, according to the descriptives, this fits
which minimize travel distance. I denote the distance-integration pair generated by this special set of SABs \((D_j^o, I_j^o)\). Relative to the minimum distance cost benchmark, a district’s leadership can obtain more racial integration by accepting longer travel distances. Denote the maximum increase in integration achievable for a given increase in travel distance per student by \(h_j(D - D_j^o)\). Function \(h_j(\cdot)\) has the following properties: (i) \(h_j(0) = 0\); (ii) \(\frac{\partial h_j}{\partial D} \geq 0\); and (iii) \(\frac{\partial^2 h_j}{\partial D^2} \leq 0\).

Given this framework, the district’s decision problem can be written as

\[
\max_D u_j(I, D) \quad \text{s.t.} \quad I \leq I_j^o + h_j(D - D_j^o).
\] (1.3)

The first order condition is

\[
\frac{\partial h_j}{\partial D} = -\frac{\partial u_j}{\partial D} \quad \frac{\partial u_j}{\partial I}.
\] (1.4)

The left hand side of the equation is the marginal rate of transformation between travel distance and school integration – the slope of the district’s budget constraint. The right hand side is the district’s marginal rate of substitution between distance and integration – the inverse willingness to pay for integration in units of travel distance.

Assume that the marginal rate of transformation and the marginal rate of substitution are in a family of functions each governed by a single parameter

\[
\frac{\partial h_j}{\partial D} = g_1(D; \psi_j) \leq 0
\] (1.5)

\[
-\frac{\partial u_j}{\partial D} = g_2(D; \theta_j) \geq 0.
\] (1.6)

Parameter \(\psi_j\) indexes districts’ technology. Higher values of \(\psi_j\) mean that the rate of transformation between travel distance and school integration is high, such that each unit of integration has a lower unit distance cost. Parameter \(\theta_j\) indexes district tastes for integration in units of distance. Higher values of \(\theta_j\) mean that the district is willing to have students travel farther to achieve a given level of integration. If the district follows the policy rule in equation (4), observed distance levels \(D_j\) can be written as an implicit function of the cost and taste parameters, \(D_j = f(\psi_j, \theta_j)\). Observed SAB integration \(I_j\) is given by \(I_j = I_j^o + h_j(f(\psi_j, \theta_j) - D_j^o)\). Therefore, heterogeneity in SAB integration is determined by the district level primitive parameters \((\theta_j, \psi_j, D_j^o, I_j^o)\). In other words, intended school integration is determined by existing residential levels of segregation, the typical district more than a model with segregation-loving districts.
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district willingness to pay, and integration technology. Figure 6 illustrates the logic of
the model using a familiar indifference curve plot in integration-distance space.

Building further on this model, consider a demand equation in which desegregation
policy is interpreted as quantity and the rate of transformation between integration and
distance is prices

\[ I_j - I_j^0 = \gamma_j + \beta h'_j(D_j - D_j^0) + \epsilon_j. \] (1.7)

Here, \( \beta \) is equivalent to the elasticity of demand for desegregation policy across school
districts; \( \gamma_j \) is a district-specific demand shifter that is independent of prices and a
function of the valuation parameter \( \theta_j \); and \( \epsilon_j \) is an idiosyncratic error component.

Provided this empirical framework, I am interested in two empirical tests. First,
I want to empirically test whether \( \beta \) in equation (7) is different from zero. Rejecting
the null that \( \beta = 0 \) implies that demand for desegregation slopes down with respect to
associated commuting costs. This would suggest that school districts behave rationally
in terms of the model when drawing school boundaries – they enjoy more integration
when it is cheaper to attain – which has policy implications that I detail in the next
section. Second, once I can control for price sensitivity, I am interested in the correlation
of demand shifters \( \gamma_j \) with observable characteristics of school districts.

One challenge in the estimation of (7) if that the unit price of desegregation, ob-
served at districts’ realized boundary choice, is correlated with both cost and preference
parameters. Estimation of \( \beta \) is not only complicated by the standard simultaneity bias,
however, it is also complicated by a measurement problem. What is the rate of transfor-
mation between aggregate travel and racial integration in any given district? I address
this measurement problem directly in the next section.

1.5 The Travel Distance Cost of SAB Desegregation

The demand estimation framework in equation (7) indicates the need of an estimate the
per unit price of racial integration, measured as marginal increases to aggregate travel
distance. It seems reasonable that this price would vary across school districts and that
it may partly explain observed variation in desegregation policy. Having a measure of
this prices would enable a test of this hypothesis. However, to my knowledge, such price
estimates do not exist. Taking residential racial sorting patterns as fixed, this section
proposes a novel methodology to estimate prices directly from GIS census block data.

To fix ideas, suppose that district integration technology is given by

\[ h_j(D - D_j^o) = \psi_j(D - D_j^o) + \kappa_1(D - D_j^o)^2. \]  

(1.8)

The first order term has a slope parameter \( \psi_j \) that is heterogenous, but higher order terms are governed by parameters that are homogeneous across districts.\(^9\) With this parametrization, there are two sources of district heterogeneity in the rate of transformation \( h_j'(D_j - D_j^o) \) – the per unit price of integration – observed at existing/chosen SABs. The first is variation in \( \psi_j \). Some districts are harder to integrate for reasons not determined by district preferences, such as the extent and particular configuration of residential segregation patterns. On the other hand, the higher order term in \( h_j'(D_j - D_j^o) \) also varies. This term is a function of integration-distance levels generated by chosen SABs, and hence it is partly determined by district preferences. I am interested in measuring the first component.

The parametrization in equation (8) implies that \( h_j'(0) = \psi_j \). In other words, a sufficient statistic for the plausibly exogenous portion of prices can be extracted by estimating the rate of transformation near the neighborhoods schools benchmark. The intuition is straightforward. Starting from the minimum travel cost set of boundaries, the rate at which travel distance can be transformed into integration is entirely governed by the spatial distribution of residential racial sorting patterns. However, as districts depart from the neighborhood schools benchmark to achieve greater racial equity, excess distance \( D_j - D_j^o \) grows and decreasing marginal returns kick in. Away from neighborhood schools, differences in \( h_j'(D - D_j^o) \) are governed by both \( \psi_j \) and district preferences.

I propose an algorithm exploiting census data to compute an estimate of \( h_j'(0) \), the price of integration evaluated at the neighborhoods schools benchmark. The basic idea is to gradually amend neighborhood schools SABs with the aim of improving school integration. "Amending boundaries" is defined as reassigning a block of residences from one school to another. The algorithm asks: which reassignment would produce the largest increase in school integration? Once a block is reassigned, this generates a marginally different set of boundaries that is slightly more integrated, but is also slightly more expensive – i.e. a new point \((D', I')\) with \( D' > D^o \) and \( I' > I^o \). The algorithm can then

\(^9\)The quadratic assumption is not crucial, I could instead have written a full Taylor expansion of \( h_j(D - D^o) \). As long as heterogeneity only enters the linear term, the argument holds.
be applied to the \((D', I')\) boundaries, reassigning a single block and producing a new point \((D'', I'')\) with \(D'' > D'\) and \(I'' > I'\), and so on. The gradient in integration-distance space generated by this routine is an approximation of \(\psi_j\).

Specifically, for each district in the sample I perform the following algorithm:

(i) Get census block geography of the jurisdiction and pixelate it into an arbitrary grid of evenly sized residential blocks.\(^\text{10}\)

(ii) Compute the minimum travel distance set of SABs, i.e. the "neighborhood schools" boundaries.

(iii) For each block along a boundary, compute how much integration would improve if the block were reassigned to the other school.

(iv) Reassign the block from (iii) that gives the highest improvement in integration per increase in distance per capita.

(v) Return to step (iii).

A stopping rule is triggered in this iterative loop once travel distance per student crosses a proportional threshold \(\alpha\) above \(D_j^0\). My preferred estimates use \(\alpha = 1.33\), but other stopping rules produce the same qualitative results.

Figure 7 plots the algorithm output for 700 districts in the sample. In panel (1) on the left, the vertical axis denotes school integration \(I\), while the horizontal axis denotes distance to school per student \(D\) in kilometers. Each locus of points represents a different district’s integration-distance budget. District budgets originate at the baseline neighborhood schools assignment \((D_j^0, I_j^0)\), as in the model. One striking feature highlighted by this plot is that school districts vary widely regarding budget origins. For example, Houston ISD in Texas has very low neighborhood integration compared to Tucson USD in Arizona. Even though Houston can obtain higher levels of integration for a much lower commuting cost than Tucson, it would take a large and costly departure from neighborhoods schools for Houston to achieve the level of integration that Tucson obtains cheaply.

\(^{10}\) I lay a square grid over census block geography and interpolate census block population into evenly sized "pixels". This greatly simplifies computations at the cost of losing some of the natural boundaries, see Chen (2013).
This highlights the need to control for neighborhood integration levels when interpreting existing SAB integration as a product of district policy.

Panel (2) of Figure 7 shows district budgets after differencing out \( (D^o,I^o) \). Plotting this data as desegregation policy against excess distance highlights variation in the rate of transformation across districts. For instance, both Houston ISD and Pittsburgh City Schools have a relatively high rate of transformation (i.e. a low integration price). They can increase SAB integration by more than 10 exposure points at the cost of increasing distance per student by less than half a kilometer. In contrast, San Diego USD and Tucson USD obtain a racial integration gain of less than 5 exposure points with a similar increase in distance. These districts have a low rate of transformation (i.e. a high price of integration). There are also plenty of districts in the middle of this spectrum. Charlotte-Mecklenburg Schools, East Baton Rouge Parish, and Springfield Public Schools are examples of districts facing an approximately median level of integration prices.

What drives district differences in the distance price of school desegregation? Figure 8 builds intuition by focusing on two districts in the sample. The top panel shows the census block geography of two school districts, Henrico County Public Schools, Virginia (left); and Little Rock School District, Arkansas (right). The bottom panel of the figure plots the approximate budget for each of these districts. The budgets originate at approximately the same point, meaning that minimum distance SABs produce similar levels of racial integration and travel distance for these districts. However, they differ considerably in prices. In order to achieve similar gains in racial integration, Henrico would need to increase travel distance by a lot more than Little Rock would.

The price discrepancy between Little Rock and Henrico is driven by differences in the spatial distribution of race. Although both have similar levels of neighborhood racial exposure, they differ considerably in the geographic proximity of racial enclaves. Little Rock has the familiar "suburban ring" segregation pattern, such that high fraction minority blocks are located in the center and directly surrounded by whiter neighborhoods. It is easy to see that students residing in the edge of minority side of the district could be transported to schools (denoted by red circles) on the edge of the white side with little excess travel. In contrast, the spatial distribution in Henrico is made up of a central minority enclave that is connected by a narrow path to two white enclaves in the edges of the jurisdiction. Desegregating schools located in the center of Henrico’s northern white enclave would entail transporting students from the minority enclave a
far distance, passing several other schools on the way and increasing excess distance by a large amount. Therefore, desegregation is relatively expensive in Henrico.

A potential objection to the algorithm generating these estimates is that it won’t perform well for higher values of \( D - D^o \). Starting from \( D^o \), this algorithm will find the value \( h_j(\varepsilon) \) for small \( \varepsilon > 0 \). But it isn’t guaranteed to be successful for larger values of \( D - D^o \), as for these one might want to consider alternatives that aren’t local perturbations (e.g. busing a group of students across town). Such alternatives involve discrete jumps in distance cost, which may be worthwhile if they achieve enough of an improvement in \( I \). For instance, consider a district with SABs that are three narrow strips – one white, one mixed, and one black. It might make sense to swap some students between the white and black strips, jumping over the mixed one in the middle. The "gradient descent" generated by this algorithm won’t find reassignments requiring such discrete jumps. Hence, we may expect that these approximations underestimate the true rate of transformation, specially far away from the neighborhood schools benchmark. This is acceptable given that I am considering perturbations local to \( h_j(0) \). Furthermore, this type of algorithm is becoming of wide use in the congressional gerrymandering literature in political science (Chen and Rodden 2013; Chen and Cottrell 2016; Chen 2017).

Having built intuition, I operationalize an estimate of the price of integration by computing the start-to-end-point slope of the algorithm output of each district

\[
\hat{\psi}_j = \frac{I_{\text{end}}^j - I^o_j}{D_{\text{end}}^j - D^o_j}
\]

(1.9)

where \((I_{\text{end}}^j, D_{\text{end}}^j)\) is the integration-distance pair achieved in the last iteration of the algorithm, and \((I^o_j, D^o_j)\) is the initial, neighborhood schools point.\(^{11}\)

Table 3 presents a descriptive analysis of variance in \( \hat{\psi}_j \). The average district has a rate of transformation of 0.112, meaning that a 1 kilometer increase in distance travelled per student can generate and increase of integration of 11.2 exposure points. Nonetheless, as is evident in Figure 8 Panel (2), the rate of transformation varies widely across districts, with a standard deviation of 12.5 exposure points. Column (1) shows that about half of this variation is driven by differences in the neighborhood schools bundle. It is difficult to desegregate districts that have a high level of neighborhood integration, hence the coefficient on this variable is negative and significant. It is also expensive, in terms of

\(^{11}\)By construction \( D_{\text{end}}^j / D^o_j \approx \alpha \).
travel distance, to desegregate jurisdictions that face higher travel costs regardless of policy, therefore the coefficient on neighborhood distance is also negative.

Column (2) of Table 3 includes additional characteristics driving desegregation prices. Higher population density – defined as the ratio of total district population to district acreage – predicts a higher rate of transformation, or lower desegregation prices. This is unsurprising as denser jurisdictions have less distance between residences and schools, making student transportation cheaper. Moving on, the number of schools administered is not strongly correlated with prices. On the other hand, overall district diversity is. I show this by fitting a quadratic specification of population fraction minority. The linear component is positive, while the quadratic component is large and negative. The concave shape implied by these estimates means that desegregation prices are higher in non-diverse districts, those at are almost entirely white or entirely minority. Prices are lower when the district is more diverse, when the fraction minority of residents is closer to 50%. Finally, Column (3) shows that these correlations hold when looking within states, demonstrating that they are not driven by regional differences.

While the estimate \( \hat{\psi}_j \) proposed here is certainly imperfect and inherently noisy, it creates an initial opportunity to test whether \( \beta = 0 \) in equation (7). In other words, it lets me test whether demand for school desegregation slopes down with respect to aggregate travel cost. However, doing so requires an additional identification assumption. Namely, I must assume that \( \hat{\psi}_j \) serves as a valid instrument for the true price of desegregation. In this regard, one benefit of my price approximations is that they are estimated near the neighborhood schools benchmark, and away from districts’ chosen SABs. The discussion of equation (8) provides some intuition of why this partially alleviates concerns for simultaneity bias. While there may be other threats to identification, I consider the current measure of prices acceptable for an initial test of the existence of demand, though perhaps not to claim a completely unbiased estimate of demand elasticity.\(^{12}\)

Table 4 presents estimates of equation (7), the demand for desegregation policy as a function of the travel distance price. The price measure is \( \hat{\psi}_j \) – the estimated rate of transformation between travel distance and integration – therefore, higher values of \( \hat{\psi}_j \) correspond to lower prices. The coefficient in column (1) is positive and highly significant.

\[^{12}\]For instance, it may be the case that \( \hat{\psi}_j \) is itself correlated with district preferences. It is plausible that jurisdictions that are easy to desegregate (have \( \hat{\psi}_j \) large and face low price) are governed by districts that have high willingness to pay for desegregation, which would bias estimates of \( \beta \) upward. I leave the discussion of an instrument for the current price measure to future work.
suggesting that the elasticity of demand is non-zero. Adding controls for neighborhood schools level of integration of distance – an important driver of prices – reduce the elasticity estimate. Nonetheless the qualitative result remains, as I can still firmly reject that the elasticity is zero. The same is true when adding additional covariates in column (3), as well as state fixed effects in column (4). Encouragingly, the coefficient stabilizes after controlling for population and fraction minority.

Altogether, the results in this section are compelling evidence of the existence of demand for school desegregation, one of the key results in this study. This has several implications. First, it validates the stylized model of SAB choice. The policy trade-off between aggregate travel distance and school racial integration is real. The data suggests that districts value desegregation, as they appear act rationally with respect to this trade-off. Second, the fact that demand for desegregation slopes down also implies that federal or state government interventions that reduce desegregation cost – perhaps as a student busing subsidy – would induce some districts to enact integrative school boundaries. Finally, now that I can directly control for the price sensitivity in SAB desegregation policy, I can switch focus and ask: which district characteristics shift demand for school desegregation?

1.6 Desegregation Demand Shifters

The framework developed in this paper tells us what school zoning choices reveal about a local government’s willingness to pay for a racially desegregated school system. Local school districts vary widely in the degree of racial equity they implicitly choose with their school boundaries, but also in the costs they would face trying to achieve it. Moreover, districts follow the law of demand when it comes to choosing to integrate – the cheaper it is to do, the more likely they are to do it. I now turn to a different question. Which school district characteristics shift the demand for desegregation outward? In other words, controlling for cost factors, which features of the district correlate positively with a preference for integrated schools?

An initial place to look is the shrinking number of school districts that remain under judicial desegregation orders. To this end, I have gathered data on judicial desegregation orders from the Office of Civil Rights and from Reardon et al. (2012). Importantly, this publicly available data also contains information about the current status of desegregation
orders, allowing me to separately identify the OLS coefficient on an indicator for being under federal oversight versus one for having been under oversight, but not any longer.\footnote{166 districts in my sample were once under judicial order but have since been released, while 99 districts in the data continue to be under judicial supervision.}

Table 5 presents the result. All specifications control for prices and for neighborhood schools levels of travel distance and integration. Column (1) shows that, controlling for prices, active judicial orders have a significant positive effect on SAB desegregation, on the order of half a standard deviation in the national distribution. This result is remarkable, providing further validation of the framework developed in this study. Previous work has shown that judicial orders work at forcing districts to enact efforts to integrate schools (Clotfelter, 2004; Johnson, 2010). The fact that my desegregation index picks this up implies that SAB gerrymandering is an actively used policy lever that can be manipulated by the legal system.

In contrast, the effect of released desegregation orders is insignificant. Districts that have been declared "unitary" by courts behave no differently than districts that have never faced a desegregation order. Most likely, these districts have re-optimized their SABs back to a distance minimizing neighborhood schools system (some such as Charlotte were forced to this by the courts), suggesting that these districts have re-segregated. This finding is in line with previous literature, which has reached similar findings by exploiting time variation in the date of release from judicial oversight (Lutz, 2011; Reardon et al., 2012).

Another potentially important shifter of desegregation policy is the level of racial animus in a district’s jurisdiction. A large chapter in the history of U.S. school desegregation is the furious and often violent resistance of whites. Since school zoning is typically approved by local school boards composed of elected officials, racially intolerant white parents may lobby to avoid SABs that integrate schools. On the other hand, it may be the case that district leaderships interpret local racial animus as a signal of the potential value of school integration.

To test these hypotheses, I gather census tract level data on racial intolerance from the General Social Survey (waves 1998-2016). I collect responses of seven survey questions regarding racial intolerance, restricting the sample to white respondents. To compute an index of white racial intolerance, I follow the procedure of Card, Rothstein, and Mas (2008). For each question, I compute an indicator for an intolerant response. I estimate
a linear probability model for each indicator, including school district fixed effects and controlling for gender, age, education, a socioeconomic status index, as well as survey year dummies. I extract the school district effects and standardize each set to have mean zero and standard deviation one. The GSS racial intolerance index is the simple average of these standardized school district effects.

Column (2) in Table 5 presents estimates of the effect of white racial intolerance with demand for SAB desegregation. The coefficient is negative and precisely estimated, indicating that greater levels of racial intolerance among whites are associated with weaker desegregation policy and closer adherence to neighborhood schools plans. It suggests that local resident’s negative views on racial diversity influence local officials’ policy decisions. Therefore, this remarkable result indicates that white racial intolerance continues to be a barrier to school desegregation efforts, even more than sixty years after the landmark Brown decision.

Next, Column (3) in Table 5 presents other district characteristics that correlate with effective preferences for desegregation. Relative intergenerational mobility – measured via county level estimates from Chetty et al. (2014) – draws a seemingly counterintuitive association with SAB desegregation. Districts with higher levels of intergenerational mobility desegregate less than others. This appears to be driven by regional differences, as the coefficient does not survive the addition of state fixed effects (see column (5)). The mobility measure is simply picking up the fact that the south is the region that has both the bulk of desegregation and also lowest mobility.

Column (3) also tests additional hypotheses of the drivers of desegregation efforts. If private or charter schools act as a potentially attractive outside option relative to public schools, districts may feel less empowered to enact controversial school assignments aimed at alleviating racial inequality. I test this by regressing policy on the number private or charter schools located in a district jurisdiction, measured relative to the number of public schools administered by the district. The coefficient on private schools is insignificant, but the coefficient on charters is significant and negative. This suggests that the presence of charter schools, but not privates, threatens districts’ perceived ability to desegregate schools, shifting demand down. One interpretation is that lowering local desegregation efforts is an unintended consequence of the celebrated charter school movement.

Column (4) tests whether additional demographics act as desegregation demand shifters. Controlling for prices, districts with higher population desegregate schools less.
A potential explanation for this is that giant urban districts such as LAUSD and Chicago SD tend to have a high rate of transformation – given how dense and diverse they tend to be – but would face enormous costs from a concerted desegregation plan. Therefore, population is estimated to act as a negative demand shifter. On the other hand, population fraction minority is an insignificant predictor of desegregation, as is log median household income. Diverse and rich districts behave no different than others when considering racial imbalance in school zoning.

Finally, Column (5) in Table 5 tests the robustness of these results in a pooled model of all the characteristics tested here, with the addition of state fixed effects. Encouragingly, the price measure $\hat{\psi}_j$ continues being predictive of desegregation policy. Similarly, the effect of judicial orders continues to be significant and is remarkably stable to the addition of covariates. The same goes for the racial intolerance effects, if anything this effect gets stronger when adding a battery of controls. In contrast, the measured effect of charter school presence is still present, but is less precisely estimated.

These results are the key findings of this study. To my knowledge, they constitute the first systematic empirical analysis of school zoning interpreted through the lens of racial desegregation. I have presented evidence consistent with a policy trade-off between racial imbalance and aggregate travel that is inherent in the setting of school zones. I also established that SAB desegregation is a policy lever that is responsive to external pressure, be it from higher levels of government or from local parents. Altogether, this study demonstrates that school attendance boundaries are endogenous to school district relative taste for educational equity.

1.7 Robustness Check – The Effect of SAB Racial Composition on Residential White Flight

One implicit assumption held constant throughout the analysis is that residential racial sorting patterns are fixed with respect to school boundary changes. This assumption allows me to hold 2010 census block geography constant when comparing racial compositions across different sets of SABs within a jurisdiction. Holding residential patterns constant is a strong assumption, however. If residential sorting responds drastically and immediately after changes in school boundaries take place, this would potentially invalidate the results of this study. I now turn to a sensitivity analysis regarding this
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assumption.

Previous work has established that increases exposure to minority populations systematically led to white flight in localities across the country, including school districts (Clotfelter 2004; Kruse 2005; Reber, 2005; Card, Rothstein and Mas, 2008; Boustan, 2010; Boustan and Margo, 2013). Many of these studies focus on historical events of national importance, such as the era of desegregation orders or the Great Migration. In order to check the robustness of my analysis to endogenous resorting effects, I need to estimate this white flight effect at a more granular spatial level, for the nuanced changes in the composition of schools generated by SAB changes. The 2002 abrupt end of a desegregation order in Charlotte-Mecklenburg Schools (CMS) in North Carolina presents an almost ideal scenario to estimate the effect of SAB changes on a district’s spatial distribution of race.

Historically, CMS administered one of the most influential school desegregation plans in the country. They were the plaintiff in the 1971 Supreme Court Case Swann v. Charlotte–Mecklenburg Board of Education – a pivotal decision which held that busing was an appropriate remedy for the problem of racial imbalance in schools given existing residential segregation. The decision had enormous implications, as it placed the burden on school districts to end de facto school segregation. In compliance, CMS enacted a heavily gerrymandered SAB map which involved busing students to distant schools. The plan then served as a model for other school districts that desegregated in similar fashion.

CMS’s influential desegregation plan remained in place for almost three decades until a series of lawsuits were brought to challenge it. In 1999, a lower federal court decision declared CMS as a "unitary" school system and ordered the district to cease using race as a factor in school assignments. CMS complied with this order and implemented a neighborhood schools plan in SY 2002–03. The effect of this dramatic policy shift has been studied extensively to estimate the effects of school segregation on student outcomes (Kane and Staiger, 2003; Billings et al., 2014; Tannenbaum, 2015; Weinstein, 2016). I intend to contribute to this literature by estimating the effect of this policy shock on the racial composition of residences. To this end, I have acquired school attendance boundary map data for CMS elementary schools for the school years 2000 through 2010.
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Descriptives

The end of desegregation busing in Charlotte brought about abrupt changes in school boundaries, generating exogenous changes in the racial composition of school assignments. Figure 9 illustrates these changes with two school boundary heat maps of CMS for the SY 2000-01 (left panel) and SY 2010-11 (right panel). The heat color corresponds to the racial composition of SABs using the 2000 census. Visually, it is clear that boundaries in 2000 were more racially integrated than in 2010. In 2000, boundaries were highly discontiguous and some of them stretched from the outskirts of the county (the suburbs) all the way the high-minority inner part of city. In contrast, 2010 boundaries look much more like the minimum-travel distance school boundaries first proposed in Figure 1 – they reflect a "neighborhood schools" assignment plan.

Figure 10 further illustrates the dynamics of SAB desegregation brought about by rezoning. The top panel shows a scatter plot similar to those in Figure 2. The horizontal axis measures the racial composition of neighborhoods – as defined by my counterfactual – and the vertical axis measures the composition of actual school assignments. The figure shows that 2000 boundaries attenuated neighborhood segregation considerably. However, by 2010 the boundaries almost perfectly replicate residential segregation in school assignments.

The bottom panel in Figure 10 shows the time trend of desegregation policy in CMS for each school year in the period 2000-2010. The end of busing in CMS brought about a dramatic drop in SAB desegregation. They went from an impressive 0.18 level of SAB desegregation in 2000 to almost an exact zero in 2010. The pattern also demonstrates that there were no changes in desegregation policy in the years between 2002 and 2010. In addition, the plot shows the trend in excess student distance to school. Encouragingly, the end of busing in Charlotte was also accompanied by a dramatic drop in transportation costs. During the busing days, distance travelled to school per student was 1.53 km. This number dropped to 0.34 km per student after the enactment of the 2002 neighborhood schools plan, reducing aggregate travel distance by 77.8%.

I want to estimate whether these policy changes led to a white flight effect. I exploit variation in SAB composition generated by this policy shock to estimate the causal effect it has on the composition of residences.\textsuperscript{14} Denote pre-policy period observations by $t = 0$

\textsuperscript{14}Weinstein (2016) explores a similar research question. He uses school district administrative data
(SY 2000-01), and post-period observations by $t = 1$ (SY 2010-11). I construct $M_{st}$, the fraction minority of residents in a given SAB $s$ in school year $t$, using 2000 census block geography. The bottom panel of Figure 9 presents a block level histogram of the school composition shock, $\Delta M_s$, generated by SAB reassignment. There is considerable support in both negative and positive values of the shock. This is the variation in treatment.

To measure the outcome of interest, I construct a novel longitudinal dataset of racial composition of census blocks for the 2000 and 2010 decennial census. Table 6 and Figure 11 summarize the resulting dataset. Figure 11 establishes the existence of strong mean reversion in the racial composition of residences. The top panel of Figure 11 shows a map of census block geography of CMS. The heat structure in this map corresponds to changes in the fraction minority of residents, with purple tones denoting increases (positive values), and orange tones denoting decreases (negative values). It is notable from this figure that the suburbs of Charlotte received large influxes of minorities during this period, while the center of the city saw decreases in the fraction of minority residences. These visual patterns suggest that Mecklenburg county underwent enormous demographic change between 2000 and 2010, with whites returning to the central area of the city, and minorities becoming more represented in its outskirts. One would not want to attribute all of these dynamics to school reassignments, motivating the use of lagged outcomes as controls in the regressions.

The bottom panel in Figure 11 shows a binned scatter plot of the decennial change in block racial composition against baseline racial composition in 2000. There is a clear pattern of mean reversion in the data: higher baseline levels predict increasingly negative changes. Notably, census blocks that were almost 100% white in 2000 gained 10 percentage points in fraction minority, while those that were almost 100% minority, gained a similar fraction of whites. I present a quadratic fit for this scatter, which does a good job at explaining the observed variation. Given these patterns, I use a quadratic control for baseline composition in the regressions.

Table 6 provides additional summary of the data, showing the average population of census blocks in 2000 and 2010. The average block in 2000 was 31% minority, and increased to 41% by 2010. In addition, I have constructed average block property prices to estimate the effect of SAB reassignments on residential composition. His analysis is enabled by the presence of student addresses in district records. My focus on data from residential census blocks, however, alleviates the attrition concerns that are present in his analysis. He reaches similar conclusions and the estimates are of similar magnitude to mine.
and appraisal values, using data from Mecklenburg County cadastre. The mean residential property price fell from $159,532 in 2000, to $138,690 in 2010. The bottom panel in Table 8 reports summary demographics at the SAB level. Importantly, SAB demographics are computed using 2000 census geography regardless of the school year. Thus, the variation in SAB demographics presented in Table 8 is driven solely by changes in SABs and not by residential changes. In 2000 CMS administered 67 schools, with an average 43% fraction minority. The number of schools went up to 91 in 2010, with the average composition of schools left unchanged.

Identification Strategy

The natural experiment that I am interested in studying can be illustrated with a simple example. Suppose that a given block of homes is reassigned from a school boundary that is 10% minority to one that is 40% minority. These homes face a change in SAB composition $\Delta M = 0.30$. I am interested in estimating whether the racial composition of this block of homes changes because of this reassignment. In other words, how many (if any) of the white inhabitants in this block would move away because they prefer schools that have a lower minority population? A first step toward assessing this residential resorting effect is looking at $\Delta y$ – the change in the block’s racial composition $y$. Nonetheless, we wouldn’t want to attribute all of the variation in $\Delta y$ to school reassignment, as it may be the case that the composition of these residences were changing for other causes that happen to be correlated with $\Delta M$ (e.g. neighborhood gentrification) – we would need to control for these confounders.

The patterns described above suggest that there are two obstacles to the identification of the causal effect of SAB composition. First, the fact that CMS had a desegregation plan in the pre period means that there is dependence between pre-period SAB composition and the baseline composition of residences. Indeed, CMS policymakers literally selected neighborhoods based on their composition when drawing pre-period SABs in order to desegregate schools. Second, the mean reversion pattern illustrated in Figure 11 are also correlated with the treatment. Figure 11 shows an influx of whites to the central part of the city, which used to be a high fraction minority area of the city in 2000. It was also from precisely this area of the city that a large fraction of minority students were bused out to faraway schools during the integration era. Thus, without making proper adjustments, the gentrification pattern in central Charlotte would lead to spurious correlation between
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the treatment and changes in the composition of residences.

Estimating the causal relationship between changes in residential composition and changes in SAB composition requires that we control flexibly for these confounding factors. I use the following regression specification:

\[ y_{ijs1} = \gamma_{j} + \beta M_{s1} + g(y_{ijs0}) + \epsilon_{ijs1} \]

(1.10)

where \( i \) denotes census blocks, \( j \) denotes neighborhoods (census tracts), and \( s \) indexes SAB; \( y_{ijs1} \) is a block’s racial composition in the post-period, the outcome of interest; \( \gamma_{j} \) are pre-period SAB by neighborhood fixed effects; \( g(y_{ijs0}) \) is a quadratic function of pre-period outcomes. I am interested in the effect on post-period SAB composition, \( M_{s1} \).\(^{15}\) OLS estimates of (11) capture the causal effect of school composition if the following identification assumption holds

\[ E[\epsilon_{ijs1} | M_{s1}, g(y_{ijs0})] = 0. \]

(1.11)

Once I control for fine-grained spatial fixed effects and a quadratic function of baseline block composition, unobserved determinants of post block composition are uncorrelated with post SAB composition. In other words, I assume that conditional on baseline racial composition, new SAB boundaries are as good as randomly drawn within small residential neighborhoods which used to have the same school assignment in the pre-period.

Notice that \( \beta \) can be interpreted as one minus the rate of compliance of white households with changes in SABs. Indeed, if we conceptualize SAB reassignment as the instrument in a two-equation system in which the endogenous variable is the composition of school enrollments, \( 1 - \beta \) is the coefficient in the first stage regression. The intuition is that \( \beta \) measures the fraction of whites that move residences in response to an increase in SAB minority composition. If whites sort away completely when faced with any increases in minority composition in assignment, we would expect \( \beta = 1 \). On the other hand, if whites do not respond at all to increases in the minority composition of their school assignments, we would get \( \beta = 0 \).

\(^{15}\)Billings, Deming, and Rockoff (2014), estimate similar models on student outcomes, finding large negative effects.
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Results

Panel A of Table 7 presents estimates of equation (11). The model in column (1) is a simple specification which controls for confounders using a linear term of pre-period SAB composition and a quadratic function of pre-period block composition (as motivated by Figure 9). The estimated effect in this model $\hat{\beta} = 0.153$ is highly statistically significant and it implies that whites have an 84.7% residential with compliance rate with increases in SAB composition. In other words, a 25 percentage point increase in the minority composition of a SAB leads to a $0.25 \times 0.153 \times 100 = 3.83$ percentage point increase in the fraction minority of residences, or a 11.6% increase relative to the average fraction minority of a Charlotte, NC census block. Columns (2) through (4) control more flexibly for the confounders, using within-neighborhood estimates of these effects. The strictest specification, in column (4), controls for old SAB by tract fixed effects (which are very fine-grained spatial controls) and gives an estimated effect of $\hat{\beta} = 0.143$, similar to the first specification.

In theory, SAB changes may change households willingness to pay for their residences. As resorting happens, demand for neighborhoods with high minority SABs may go down which would tend to push prices down. Hence, we expect the coefficient on $M_{s1}$ to be negative in these specifications. Table 7 Panel B tests for this effect using log property prices as the outcome. Since not all census blocks in Mecklenburg County had property sales, the number of observations in these regression is lower. None of these estimates are statistically significant – although, as we introduce additional controls the point estimates become increasingly negative. I interpret this as evidence that the end if integrated schools in CMS did not have a significant effect on local housing markets. This result is in line with the existing empirical literature on the effect of racial segregation on property prices, which has generated similar null results (Kruse 2005, Card et al. 2008, Tannennbaum 2015).

I close the analysis with a brief inspection of the demographic mechanisms generating my causal estimates. Two broad demographic patterns affected Charlotte between 2000–2010, immigration and gentrification. The combination of these generates an aggregate relationship between composition change and baseline composition akin to mean reversion, as can be seen in the bottom panel of Figure 9. The regression model in equation model (11) tells us that the treatment effect can be thought of as deviations from this mean reversion trend. Figure 10 provides a visual illustration of the dynamics at play.
Here, I plot the average change in block composition within deciles of baseline block composition for three different groups: (1) Blocks that had a change in SAB composition of less than -0.1, that is, blocks that got a whiter reassignment (negative treatment); (2) Blocks that saw SAB composition changes between -0.1 and 0.1 (a neutral ‘control’ treatment); and (3) blocks that got an increase in minority assignment of more than 10 percentage points, 0.1 (positive treatment).\textsuperscript{16} Neighborhood fixed effects have been partialled out from these variables – hence, they are to be interpreted as deviations from mean neighborhood compositions.

Figure 10 shows that households that resorted in light of boundary changes tended to looked different from the rest of their neighbors. The gray line – corresponding to the comparison group ($-0.1 < \Delta M < 0.1$) – trends down, and both the positive and negative treatment groups follow it in approximately parallel fashion, but with key deviations at the extremes. Second, the red line (increase in SAB composition, $\Delta M > 0.1$) is always above of the black line (decrease in SAB composition, $\Delta M < 0.1$). This means that, within all deciles of baseline composition, an increase in SAB composition leads to at least a weakly higher change in block composition. Hence, the pattern of these differences suggest that white flight is present across the board, although some of these differences are not statistically significant. Second, my white flight estimates are generated by two extremes of the distribution: the exit of whites from blocks that where whiter than the neighborhood average in the positive treatment group, and the arrival of whites to blocks that were less white than average in the negative treatment group. These patterns suggest that complier blocks in this natural experiment were those that looked different from the rest of their neighborhood. White households in minority neighborhoods exited once given a higher minority assignment, while minority household in white neighborhoods exited (at a higher rate). More detailed investigation into this mechanism would useful to policymakers, something that I leave for future research.

In summary: my estimates suggest that white households have an 85% residential compliance rate with SAB racial composition over a decade. In other words, one would predict to lose 3.8 percentage points of the fraction white in a neighborhood if it faced a 25 percentage point increase in the minority fraction of students assigned to a school. While this effect is significant, it is modest. It implies that the main analysis in this study

\textsuperscript{16}The threshold of 0.1 for the partition of the support of the treatment variable is arbitrary, it is motivated by the approximately multi-modal distribution of the treatment variable observed in the bottom panel of Figure 7.
is robust to endogenous residential sorting, especially considering the relative frequency of SAB changes. Moreover, I find that housing markets are not adversely affected by increases in school minority assignments, at least through the price mechanism. Taken together, I interpret these results as evidence that sorting equilibrium in the housing market is not particularly sensitive to SAB changes, at least in the short to medium run.

1.8 Conclusion

The racial integration of U.S. public schools has been a hotly debated policy topic for over half a century. Over the last three decades, the federal government has been in retreat from its efforts to force the hand of local jurisdictions to alleviate racial imbalances in public education. Today, decisions affecting school integration are largely left to these local policymakers. Understanding how these officials make choices that affect racial equity in their jurisdictions is thus of key importance in this debate. One key habitual policy decision they make is the setting of school attendance boundaries.

This paper developed a framework for interpreting local school zoning choices. I began by proposing a simple zoning counterfactual, enabling me to measure the extent to which chosen boundaries ameliorate existing residential segregation patterns. Relative to these counterfactuals, I found wide heterogeneity in district desegregation levels across the country, and underwent several empirical exercises establishing the validity of this index. To my knowledge, a desegregation policy index at this hyperlocal level is novel, my hope is that it will be used to monitor the policies of school districts and in future work on local governance.

I proposed a simple model in which school districts care about two features of SABs: the daily distance travelled by students to school, and the level of racial integration in the school system. The model allowed me to specify district demand for desegregation as a function of two key components: the price of desegregation in terms of extra travel distance and a district level demand shifter. To test the model, I constructed a measure of the price of desegregation using census block data and a simple rezoning algorithm. This approximated rate of transformation allows us to observe which jurisdictions are more difficult to desegregate than others, given residential patterns. In and of itself it also represents a measurement contribution which should facilitate further research.
CHAPTER 1. SCHOOL ATTENDANCE BOUNDARY POLICY

Having constructed both an index of the amount of SAB desegregation and also of its marginal cost, I estimated a cross-sectional model of district demand for desegregation. My findings suggest that districts follow the law demand when setting boundaries – the cheaper it is to achieve a racially balanced school system, the more likely they are to draw SABs that do so. The result validated my conceptual framework, by establishing empirical evidence supporting the policy trade-off at the center of my stylized model. Additionally, the finding indicates that federal or state policy subsidizing student transportation costs may lead districts to enact racially balanced SABs.

Next, I turned to an investigation of desegregation demand shifters. I found that active desegregation orders have a strong effect on SABs, shifting the demand for desegregation out. A complementary result was that districts no longer under judicial order show no such behavior. This evidence is in line with the empirical literature on the "Brown fades" phenomenon and school re-segregation. In addition, these findings demonstrate the school zoning policy is responsive to external pressure from higher levels of government.

Remarkably, I also found that racial intolerance among local whites – as measured by survey data – is significantly negatively correlated with the enactment of integrative SABs. This novel finding suggests that local residents’ negative views on racial diversity influence the policies enacted by local officials. It implies that the racial intolerance of certain white communities continues to be an effective barrier to school desegregation efforts, even more than sixty years after the Brown decision.

The study closes with an important robustness check – estimating the residential white flight effect spawning from SAB changes. I did this by exploiting policy variation in Charlotte school district’s SABs, after the abrupt end of a decades-old desegregation plan. I used census block data to estimate the residential compliance rate of whites with shifts in the racial composition of SABs. The findings suggest an 85% residential compliance rate, as well as little indication of effects on real estate values. They suggest that the endogeneity of residential sorting patterns is the result of long-run dynamics. Considering the relative frequency of updates to SAB policy, the results of this case study alleviate concerns of bias in the main analysis.

Altogether, this study establishes that school attendance boundary policy is not set in stone. Instead, it is endogenous to districts’ relative valuation of the multidimensional effects SAB policy has on the distribution of school characteristics. School boundaries
reveal information regarding these relative preferences. I have developed new tools for stakeholders to monitor these policy decisions at a local level. Future research on this topic should further our understanding on the impact that school zone desegregation has on racial achievement gaps, as well as of the local political processes that determine these policies.
Figure 1.1: School Segregation and School Attendance Boundaries (SABs) – Springfield School District No. 186, IL

Note: Panel (1) shows a heat map of the 2010 racial composition of residential census blocks in Springfield, IL. Light colors denote low fraction minority (black or hispanic) residential compositions. The red circles denote the location of elementary schools administered by this district. Panel (2) shows a minimum travel distance school SABs drawn over Springfield’s census blocks. The boundary lines are generated by assigning blocks to the nearest school. Panel (3) shows the distribution of school boundary racial composition generated by minimum travel distance SABs. I report $I^o$ – the level of school racial integration generated by the assignment (see Section 3 for the definition of this index) – and $D^o$ – travel distance to school per student. Panel (4) shows Springfield’s actual SY 2013-14 elementary school attendance boundaries, and the resulting distribution of school-level racial composition generated by them. These boundaries are highly discontiguous (due to a busing scheme) in such a way that results in higher integration $I$ and higher distance per student $D$. Springfield Public Schools District has had an active desegregation plan since 1976 (see Mcpherson V. School Dist. No. 186, Springfield Ill.)
Figure 1.2: SAB Desegregation Scatter Plot for Four School Districts

Note: Each plot is a weighted scatter of the racial composition of actual school boundaries (‘assignments’) against the composition of minimum distance school boundaries (‘neighborhoods’) for a single school district. Each bubble corresponds to a unique school administered by the district, weighted by population. I show the OLS fit of this relationship as well as the 45 degree line for comparison. In each plot I report the OLS coefficient and robust standard error, as well as the level of racial integration of minimum distance boundaries, $I^o$, and the actual actual boundaries, $I$. Integration is defined as one minus the exposure gap segregation index.
Figure 1.3: Cross-sectional Distribution of SAB Desegregation Policy

Note: Figure shows a histogram of \((1 - \hat{\beta}_j)\), where \(\hat{\beta}_j\) is the OLS coefficient of a within district regression of the racial composition (defined as the population fraction black or hispanic) of existing SABs on the racial composition of neighborhoods schools counterfactual SABs. See Section 3, for detailed description of definition of counterfactuals.
**Figure 1.4:** Spatial Distribution of Neighborhood Schools Integration and SAB Desegregation Policy, SY 2013-14 SABs

Note: The figure plots heat maps of mainland U.S. at the school district level. The red heat coloring in the top panel denotes eight quantiles neighborhood integration ($I^o$). $I^o$ is computed at the school district level as SAB integration that would be achieved under a minimum distance counterfactual SABs detailed in Section 3.1. Darker red tones denote higher levels of racial integration. The blue heat coloring in the bottom map corresponds to eight quantiles of SAB desegregation policy ($I - I^o$) – the difference in integration between SY 2013-14 SABs and neighborhood schools counterfactual SABs.
Figure 1.5: Cross-sectional Relationship of SAB Desegregation and Excess Travel Distance

Note: Figure plots the mean level of SAB desegregation across one hundred quantiles of excess distance per student. See Section 3 for the detailed description of construction of these variables. OLS fit for this relationship is shown, with the slope coefficient, standard error, and r-squared of this regression.
Figure 1.6: A School District’s Optimal Choice of School Boundaries in Integration-Distance Space

Note: Visual illustration of school attendance boundary (SAB) choice model developed in Section 4. The horizontal axis denotes aggregate distance travelled to school in the district, while the vertical axis measures the level of SAB racial integration. Districts have a heterogenous "origin" \((D^o, I^o)\), where \(D^o\) is lowest travel distance feasible given the geography of the jurisdiction and the location of schools. The district faces an integration-distance technology ("bufget") that is governed by \(I^o\) and \(h(D - D^o)\), where \(h(D - D^o)\) is the maximum increase in integration that can be achieved for an increase in distance \(D - D^o\). The district has preferences over \((D, I)\) bundles, represented by indifference curve \(u\). The district’s optimal SAB choice, \((D^*, I^*)\), satisfies the condition \(\frac{\partial h}{\partial D} = -\frac{\partial u / \partial D}{\partial u / \partial I}\).
Figure 1.7: Racial Integration - Travel Distance Budget for 700 School Districts

Note: This figure plots the output from the algorithm outlined in Section 5 for 700 school districts. The algorithm uses census block data to approximate integration-distance budget of a school district, providing an estimate of the rate of transformation between student travel and racial integration, see text. In Panel (1), the vertical axis measures school racial integration at the district level \( I \) and the horizontal axis measures distance travelled to school per student in the district \( D \). Each separate locus of points is a distinct district’s approximate integration-distance schedule, which originate at minimum cost bundle \( (D^0, I^0) \). Panel (2) shows a similar plot after differencing out minimum cost levels – plotting desegregation policy \( (I - I^0) \) against excess distance \( (D - D^0) \). It highlights differences in the marginal cost of desegregation. The heat coloring captures eight quantiles of baseline distance \( D^0_j \). I also highlight in boldface the budget of the following school districts: Houston ISD, TX; Pittsburgh City Schools, PA; Springfield Public Schools, IL; East Baton Rouge Parish, LA; Charlotte Mecklenburg Schools, NC; Wake County Schools, NC; San Diego USD, CA; and Tucson USD, AZ.
Figure 1.8: Racial Integration - Travel Distance budget, Henrico County Public Schools, VA and Little Rock School District, AR

Note: Top panel shows racial composition census block heat maps of the school district jurisdictions. Red circles denote elementary school locations. Bottom panel shows estimated budget for each. See Section 5 for a detailed procedure for approximating rate of transformation between aggregate travel distance and racial integration.
**Figure 1.9:** Distribution of SAB Racial Composition Changes. Charlotte, NC, 2000 and 2010.

Note: Top left panel shows a heat map of the composition of Charlotte’s school attendance boundaries (SABs) in SY 2000-01 under 2000 census block geography. Top right panel shows a similar heat map for SY 2010-11 SABs using 2000 census geography. Light colors denote low fraction minority school assignments, and vice versa. The bottom panel shows a histogram of the distribution of the change in the racial composition of SABs caused by these boundary changes. This is the distribution of treatment 'dosages' across the population in the district.
**Figure 1.10:** Time Trend of SAB Desegregation and Excess Travel Distance in Charlotte, NC.

Note: Top panel shows scatters of school assignment composition against neighborhood composition in CMS school district. Blue circles correspond to SY 2000-01 and black triangles for SY 2010-11. In 2001 CMS had a desegregation plan in place. In 2010 CMS implemented a neighborhood schools plan. I report \((1 - \hat{\beta})\), where \(\hat{\beta}\) is the OLS coefficient of these relationships. Racial composition is computed using 2000 census block data. The bottom panel reports SAB desegregation levels for each school year in 2000-2010. It also plots excess distance travelled to school per student in kilometers. See Section 3 for definitions.
Figure 1.11: Distribution of 2000-2010 Change in Racial Composition of Census Blocks, Charlotte, NC

Note: Top panel shows heat map of Charlotte census blocks. Darkening purple color denotes higher values of positive changes in racial composition, i.e. increases in minority population. Orange coloring denotes negative change, or increases white population. Bottom panel shows the bivariate relationship between changes and initial levels, i.e. $\Delta m_t$ against $m_{2000}$. It plots a quadratic OLS fit for this relationship. Coefficients reported in top right.
Figure 1.12: Mechanisms of Residential Resorting on the Basis of Race caused by School Composition Shocks

Note: Figure plots the mean 2000-2010 change in racial composition within ten quantiles of baseline (2000) racial composition for three groups in the sample. Both variables have been residualized with respect to baseline SAB-by-census tract fixed effects, so they are to be interpreted as deviations from the mean in the neighborhood, with zero representing no deviation from neighborhood mean. The three groups are defined by the magnitude of the change in school attendance boundary (SAB) composition generated by school reassignments, holding 2000 census geography constant. The three groups are: negative treatment ($\Delta M < -0.1$), neutral treatment ($-0.1 < \Delta M < 0.1$), and positive treatment ($\Delta M > 0.1$).
Table 1.1: School District Summary Statistics

<table>
<thead>
<tr>
<th></th>
<th>Mean (1)</th>
<th>SD (2)</th>
<th>p25 (3)</th>
<th>p50 (4)</th>
<th>p75 (5)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Racial Integration of SABs</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2013 school assignments ($I$)</td>
<td>0.936</td>
<td>0.078</td>
<td>0.913</td>
<td>0.966</td>
<td>0.988</td>
</tr>
<tr>
<td>Neighborhood schools counterfactual ($I^o$)</td>
<td>0.929</td>
<td>0.080</td>
<td>0.899</td>
<td>0.959</td>
<td>0.986</td>
</tr>
<tr>
<td>Desegregation policy ($I - I^o$)</td>
<td>0.007</td>
<td>0.026</td>
<td>-0.002</td>
<td>0.001</td>
<td>0.009</td>
</tr>
<tr>
<td>Neighborhood-assignment OLS ($1 - \hat{\beta}$)</td>
<td>0.127</td>
<td>0.180</td>
<td>0.016</td>
<td>0.092</td>
<td>0.204</td>
</tr>
<tr>
<td><strong>Panel B: Travel Distance to Schools (km per capita)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2013 school assignments ($D$)</td>
<td>2.500</td>
<td>1.564</td>
<td>1.356</td>
<td>2.069</td>
<td>3.166</td>
</tr>
<tr>
<td>Neighborhood schools counterfactual ($D^o$)</td>
<td>2.062</td>
<td>1.391</td>
<td>1.100</td>
<td>1.611</td>
<td>2.545</td>
</tr>
<tr>
<td>Excess travel distance ($D - D^o$)</td>
<td>0.437</td>
<td>0.357</td>
<td>0.196</td>
<td>0.335</td>
<td>0.557</td>
</tr>
<tr>
<td><strong>Panel C: District Characteristics</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of elementary schools administered</td>
<td>14.7</td>
<td>19.9</td>
<td>6</td>
<td>9</td>
<td>16</td>
</tr>
<tr>
<td>Total Population</td>
<td>103430</td>
<td>169584</td>
<td>36565</td>
<td>57680</td>
<td>105837</td>
</tr>
<tr>
<td>Fraction minority (black or hispanic)</td>
<td>0.27</td>
<td>0.22</td>
<td>0.10</td>
<td>0.20</td>
<td>0.39</td>
</tr>
<tr>
<td>ln(Median household income, 2014 USD)</td>
<td>10.94</td>
<td>0.35</td>
<td>10.69</td>
<td>10.90</td>
<td>11.18</td>
</tr>
<tr>
<td>Judicial desegregation order in effect</td>
<td>0.07</td>
<td>0.25</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Judicial desegregation order released</td>
<td>0.11</td>
<td>0.31</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>GSS white racial intolerance index</td>
<td>-0.050</td>
<td>0.505</td>
<td>-0.318</td>
<td>-0.057</td>
<td>0.265</td>
</tr>
</tbody>
</table>

Observations: 1519

Note: This table reports average characteristics of school districts in the sample. The district-level racial integration of schools index is defined as one minus the exposure gap index (a.k.a. variance ratio segregation index, see Reardon and Owens 2014). SAB integration is computed using SY 2013-14 boundaries obtained from NCES’s School Attendance Boundary Survey. Desegregation Policy is defined as the difference in integration between actual SABs ($I$) and integration in the neighborhood schools counterfactual SABs ($I^o$). Percentage desegregation policy is defined as $(1 - \hat{\beta})$, where $\hat{\beta}$ is the OLS coefficient of a regression of assignment composition on neighborhood composition, see Figure 2. I also report the district integration index for school enrollments, as measured in the NCES Common Core of data for SY 2013-14. Average district characteristics and funding statistics are derived form the Common Core of Data and from 2010 census block group geography, matched to districts using GIS software. District revenue per student is computed for SY 2013 and using the total count of the districts K-12 student body.
### Table 1.2: SAB Desegregation Policy and Racial Gaps in School Attributes

<table>
<thead>
<tr>
<th>SAB Desegregation Policy</th>
<th>Teacher Experience</th>
<th>Student Retention</th>
<th>GT Program</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td></td>
<td>0.0315* (0.0188)</td>
<td>0.0775*** (0.0195)</td>
<td>-0.0382 (0.0379)</td>
</tr>
<tr>
<td>Covariates</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>State Fixed Effects</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Dependent Var. Mean</td>
<td>-.005</td>
<td>-.005</td>
<td>-.009</td>
</tr>
<tr>
<td>Dependent Var. SD</td>
<td>.017</td>
<td>.017</td>
<td>.031</td>
</tr>
<tr>
<td>N</td>
<td>1519</td>
<td>1516</td>
<td>1519</td>
</tr>
<tr>
<td>R²</td>
<td>0.00231</td>
<td>0.122</td>
<td>0.00102</td>
</tr>
</tbody>
</table>

**Note:** Robust standard errors reported in parenthesis. The level of observation is a school district. In all specifications, the dependent variable is a district level racial gap, defined as $\bar{Y}_w^r - \bar{Y}_m^r$, where $\bar{Y}_w^r$ is the district average of the outcome for students in racial group $r$. SAB desegregation policy is defined as $I_j^o - I_j^s$, the difference in racial integration between a system with existing SABs and a system using minimum distance SAB counterfactuals. Columns (1) and (2) report estimates for racial gaps the fraction of school teachers in their first or second year in the profession. Column (3) and (4) show results for gaps in student grade retention rates. Columns (5) and (6) correspond to gaps in student exposure to the "gifted and talented" (GT) school program. Covariates include log population, log median household income, population fraction minority, and neighborhood schools integration level $I_j^o$ (as defined by minimum distance SAB counterfactuals).
Table 1.3: Correlates of the rate of transformation between travel distance and racial integration $\psi$

<table>
<thead>
<tr>
<th></th>
<th>Estimated rate of transformation ($\hat{\psi}$)</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Neighborhood integration ($I^o$)</td>
<td>-1.014***</td>
<td>-0.990***</td>
<td>-0.963***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0557)</td>
<td>(0.0778)</td>
<td>(0.0881)</td>
<td></td>
</tr>
<tr>
<td>Neighborhood distance per student ($D^o$)</td>
<td>-0.0275***</td>
<td>-0.0196***</td>
<td>-0.0179***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.00154)</td>
<td>(0.00270)</td>
<td>(0.00276)</td>
<td></td>
</tr>
<tr>
<td>Population density</td>
<td>0.114***</td>
<td>0.106***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0379)</td>
<td>(0.0387)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of schools administered</td>
<td>-0.000306</td>
<td>-0.000202</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.000192)</td>
<td>(0.000205)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Population fraction minority</td>
<td>0.189***</td>
<td>0.254***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0475)</td>
<td>(0.0574)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Population fraction minority, squared</td>
<td>-0.275***</td>
<td>-0.324***</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>(0.0486)</td>
<td>(0.0548)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Mean $\psi$ .112
SD $\psi$ .125
State Fixed Effects $\checkmark$
N 1519 1517 1514
R$^2$ 0.559 0.616 0.627

Note: Robust standard errors reported in parenthesis. Table shows OLS regressions of the estimated rate of transformation between distance and integration ($\hat{\psi}$) – as defined in Section 5 – on school district characteristics. Neighborhood integration and neighborhood distance per student correspond to minimum distance counterfactual SABs for each district. Integration is defined as $(1 - Seg)$ where $Seg$ is the exposure gap (variance ratio) segregation index, see Reardon and Owens (2014). Population fraction minority is the total minority (black or hispanic) population of the district divided by total population. Population density is defined as the ratio of total district population over the school district’s total acreage.
Table 1.4: The elasticity of demand for desegregation

<table>
<thead>
<tr>
<th></th>
<th>SAB Desegregation ($I - I^o$)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Rate of transformation ($\dot{\psi}$)</td>
<td>0.0471***</td>
</tr>
<tr>
<td></td>
<td>(0.00865)</td>
</tr>
<tr>
<td>Controls ($D^o, I^o$)</td>
<td>✓</td>
</tr>
<tr>
<td>Additional covariates</td>
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</tr>
<tr>
<td>State fixed effects</td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>1519</td>
</tr>
<tr>
<td>R²</td>
<td>0.0505</td>
</tr>
</tbody>
</table>

Note: Robust standard errors reported in parenthesis. Reports OLS estimates of SAB desegregation policy ($I - I^o$) regressed on the estimated rate of transformation between travel distance and racial integration ($\dot{\psi}$). Column (2) adds controls for neighborhood schools level of integration and travel distance. Column (3) adds covariates: log population, population fraction minority, and log median household income. Column (4) adds state fixed effects.
### Table 1.5: Desegregation demand shifters

<table>
<thead>
<tr>
<th></th>
<th>SAB Desegregation ((I - I^o))</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Rate of transformation ((\hat{\psi}))</td>
<td>0.0308***</td>
</tr>
<tr>
<td></td>
<td>(0.0117)</td>
</tr>
<tr>
<td>Judicial desegregation order in effect</td>
<td>0.0127***</td>
</tr>
<tr>
<td></td>
<td>(0.00432)</td>
</tr>
<tr>
<td>Judicial desegregation order released</td>
<td>-0.00353</td>
</tr>
<tr>
<td></td>
<td>(0.00322)</td>
</tr>
<tr>
<td>Racial intolerance of whites (GSS)</td>
<td>-0.00876***</td>
</tr>
<tr>
<td></td>
<td>(0.00303)</td>
</tr>
<tr>
<td>Relative mobility (Chetty et al. 2014)</td>
<td>-0.0404***</td>
</tr>
<tr>
<td></td>
<td>(0.0123)</td>
</tr>
<tr>
<td>Private to public school ratio</td>
<td>0.00118</td>
</tr>
<tr>
<td></td>
<td>(0.00196)</td>
</tr>
<tr>
<td>Charter to public school ratio</td>
<td>-0.00708***</td>
</tr>
<tr>
<td></td>
<td>(0.00267)</td>
</tr>
<tr>
<td>(\ln(\text{Total population}))</td>
<td>0.00523***</td>
</tr>
<tr>
<td></td>
<td>(0.00110)</td>
</tr>
<tr>
<td>Population fraction minority</td>
<td>0.000132</td>
</tr>
<tr>
<td></td>
<td>(0.00310)</td>
</tr>
<tr>
<td>(\ln(\text{Median household income}))</td>
<td>0.00363</td>
</tr>
<tr>
<td></td>
<td>(0.00229)</td>
</tr>
</tbody>
</table>

| Controls \((D^o, J^o)\) | ✓   | ✓   | ✓   | ✓   | ✓   |
| State fixed effects         | ✓   | ✓   | ✓   | ✓   | ✓   |
| \(N\)                      | 1519 | 1519 | 1517 | 1519 | 1514 |
| \(R^2\)                    | 0.106 | 0.0997 | 0.102 | 0.109 | 0.165 |

**Note:** Robust standard errors reported in parenthesis. Dependent variable in all models is district SAB desegregation as defined in Section 3. Indicators for desegregation orders and their status are obtained from the Department of Education Office of Civil Rights and from Reardon et al. (2012). White racial intolerance is measured using General Social Survey data, as in Card et al. (2008). Relative Intergenerational mobility data is from Chetty et al. (2014). Log median household income is computed using 2010 census block group geography. Number of privates schools is computed using the NCES 2013 Private School Survey. Number of charter schools is computed using the Common Core of Data for 2013.
Table 1.6: Summary Statistics - Mecklenburg County 2000 and 2010 Census Block Geography.

<table>
<thead>
<tr>
<th></th>
<th>2000</th>
<th>2010</th>
<th>Change</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>mean</td>
<td>sd</td>
<td>mean</td>
</tr>
<tr>
<td><strong>Census Block Demographics and Real Estate</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Block Population</td>
<td>141.10</td>
<td>220.95</td>
<td>175.65</td>
</tr>
<tr>
<td>Fraction Minority</td>
<td>0.33</td>
<td>0.35</td>
<td>0.41</td>
</tr>
<tr>
<td>ln(Mean Property Sales Price)</td>
<td>11.98</td>
<td>0.71</td>
<td>11.89</td>
</tr>
<tr>
<td>ln(Mean Property Appraisal Value)</td>
<td>11.92</td>
<td>0.70</td>
<td>11.84</td>
</tr>
<tr>
<td><strong>SAB Demographics (2000 Census Constant)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>SAB Population</td>
<td>803.52</td>
<td>254.08</td>
<td>621.20</td>
</tr>
<tr>
<td>Fraction Minority</td>
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<td>0.21</td>
<td>0.43</td>
</tr>
<tr>
<td>N</td>
<td>4393.00</td>
<td>4393.00</td>
<td>4393.00</td>
</tr>
<tr>
<td>N schools</td>
<td>67</td>
<td>91</td>
<td>91</td>
</tr>
</tbody>
</table>

*Note:* The top panel of this table reports the average and standard deviation of 2000 and 2010 census block population and racial composition for Mecklenburg County, North Carolina. I also report log of mean property sales prices and value appraisals from the Mecklenburg County cadastre and tax office, aggregated to census block geography. The bottom panel reports summary demographic characteristics of school assignments (SABs) to census blocks. Importantly, SAB demographics are 2000 census constant. That is, they are measured under 2000 census geography for both SY 2000 and SY 2010 SABs. This ensures that that variation in SAB composition used in the regressions is generated by SAB changes, and not changes in the underlying residential distribution of race. I restrict the sample to census blocks that had non-zero population in both 2000 and 2010. One measurement difficulty in this exercise is that, at the block level, census geography changes with every decennial census. In order to measure the change in block population, I develop an imputation method using GIS software to link 2000 and 2010 census block geography, see Appendix 3.
Table 1.7: The Effect of SAB Racial Composition on the Racial Composition of Residences.

<table>
<thead>
<tr>
<th>Panel A: Residential Block Composition</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
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<tr>
<td>School Boundary Composition</td>
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<td>0.193***</td>
<td>0.151***</td>
<td>0.143**</td>
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<tr>
<td></td>
<td>(0.0334)</td>
<td>(0.0283)</td>
<td>(0.0474)</td>
<td>(0.0601)</td>
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<td>Quadratic in Baseline Composition of Block</td>
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<td>✓</td>
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</tr>
<tr>
<td>Baseline Composition of SAB</td>
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<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Baseline SAB Fixed Effects</td>
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<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Census Tract Fixed Effects</td>
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<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
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<td>4393</td>
<td>4393</td>
<td>4393</td>
</tr>
<tr>
<td>N census tracts</td>
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<td></td>
<td></td>
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</tr>
<tr>
<td>N SABs (schools)</td>
<td>91</td>
<td>91</td>
<td>91</td>
<td></td>
</tr>
<tr>
<td>N tract-by-SABs</td>
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<td></td>
<td></td>
<td>309</td>
</tr>
<tr>
<td>R²</td>
<td>0.237</td>
<td>0.416</td>
<td>0.519</td>
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</table>

<table>
<thead>
<tr>
<th>Panel B: Mean Property Price</th>
<th>(1)</th>
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<th>(4)</th>
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</thead>
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<tr>
<td>School Boundary Composition</td>
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<td>-0.0174</td>
<td>-0.0666</td>
<td>-0.0282</td>
</tr>
<tr>
<td></td>
<td>(0.0812)</td>
<td>(0.0966)</td>
<td>(0.176)</td>
<td>(0.215)</td>
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<tr>
<td>Baseline Composition of SAB</td>
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<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Baseline SAB Fixed Effects</td>
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<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Census Tract Fixed Effects</td>
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<td>✓</td>
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</tr>
<tr>
<td>Baseline SAB-by-Census Tract Fixed Effects</td>
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</tr>
<tr>
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<tr>
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<td>79</td>
<td>79</td>
<td>79</td>
<td></td>
</tr>
<tr>
<td>N tract-by-SABs</td>
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<td></td>
<td></td>
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</tr>
<tr>
<td>R²</td>
<td>0.0407</td>
<td>0.0938</td>
<td>0.161</td>
<td>0.194</td>
</tr>
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</table>

Note: Standard errors (shown in parenthesis) are clustered at the census block group level in all models. The level of observation is a census block in Mecklenburg County, NC. In Panel A, the dependent variable is the 2000-2010 change in block racial composition (i.e. the change in the fraction of residents that are black or hispanic). In Panel B, the dependent variable is the logged mean property sales price at the census block level. Hedonics (including: number of bathrooms and bedrooms, square footage, presence of AC and heating, number of floors, etc.) were first partialled out from property prices in a first stage regression at the property (parcel) level, see Appendix. Estimated models are different versions of equation (12) in the text. Column (1) controls for a quadratic function for baseline (2000) block racial composition and the 2000 (baseline) composition of school attendance boundary (SAB) assignments. Column (2) controls more flexibly for baseline SAB effects by including fixed effects for the 2000 school attendance boundary (zone) of a given block. Column (3) adds census tract fixed effects. Column (4) adds baseline (2000) SAB - by - census tract fixed effects.
Chapter 2

The Impact of For-Profit College Chain Entry on Postsecondary Education Markets

2.1 Introduction

The rapid expansion of large for-profit private companies into postsecondary education markets in the U.S. is a controversial policy topic. Scholars have argued that such expansion can be attributed to slow growth in state funding for public institutions, such that for-profit chain colleges act to meet excess demand for higher education while simultaneously taking advantage of federal student financial aid (Deming, Goldin and Katz, 2013). However, there is also evidence that for-profit and public colleges compete directly for students (Cellini, 2009; Cellini, Dariola, and Turner, 2017) and that the labor market value of credentials obtained at for-profits is lower than that of public institutions (Deming, Yuchtam, Abulafi, Goldin and Katz, 2016; Cellini and Turner 2016).

Additionally, for-profit college chains predominantly enroll "non-traditional" students from disadvantaged backgrounds; they charge considerably higher tuition rates than their public counterparts; and they effectively generate the vast majority of their revenue from federal aid or student debt. Thus, it is perhaps of little surprise that several of these companies have faced considerable negative attention from both the media and the federal government, culminating in the recent closure of some big-name for-profit colleges and the enactment of stringent regulations over institution eligibility for federal aid (Smith, 2016; Department of Education, 2017).

Considering this backdrop, it is clear that the impact that for-profit colleges have
on localities will depend on the outside option faced by the average individual that enrolls with them. If these colleges simply expand into markets facing excess demand for postsecondary education, this means their potential students may not be able to acquire any higher education without them. On the other hand, if these institutions compete directly with non-profit colleges for enrollment, then the counterfactual scenario of a student at a for-profit may be attending a cheaper (and perhaps more effective) non-profit college.

This paper provides new evidence attempting to discern between these two scenarios. We estimate the impact of for-profit chain colleges on postsecondary education markets by exploiting variation in the timing of market entry generated by the steady expansion of for-profit college chains over the last two decades. Our main data source is the Integrated Postsecondary Education Data System (IPEDS), an annual survey of U.S. postsecondary institutions that all Title-IV eligible colleges are required to respond. We use the IPEDS for the years 1998-2013 to construct an annual panel of postsecondary markets measuring total enrollment and completions by college sector. We follow the existing literature on the for-profit sector and define postsecondary education markets at the county level (Cellini, 2009, 2010; Cellini, Dariola, and Turner, 2017).

Figure 1 presents trends in the annual share of national postsecondary enrollment going to for-profit college chains, as well as the total number of separate branches operated by for-profit chain companies. We define for-profit chains following Deming, Goldin and Katz (2012) – companies that operate five or more college branches or have branches across different states. Between 1998 and 2013, for-profit chain colleges saw massive growth in their share of national enrollment, which peaked in 2011 at 7.2% up from 2.6% in 1998. During this period for-profit chains opened almost 1,000 separate colleges, expanding away from large city centers and spreading throughout the U.S. mainland.

We exploit temporal variation in the roll out of these colleges across different markets to identify the effect of for-profit chain entry on total enrollment and completions, with an additional focus on treatment effect heterogeneity across different postsecondary sectors. Estimates of the impact of for-profit chain entry on total enrollment are OLS estimates of standard event study models with the number of for-profit chains in a given county-year as the treatment variable.

\footnote{We choose this sample period due to measurement issues in the IPEDS for the for-profit colleges.}
An advantage of this methodology is that it allows us to test for pre-trends in a county enrollment prior to market entry events. We show that, looking five years back before a for-profit college enters a market, there is little sign of pre-trends in the main outcomes. Interestingly though, we find evidence of pre-trends in county characteristics, including county unemployment rates, median household income, the poverty rate, and population aged 20 to 29 years old. The interpretation of these findings is that, as a firm entry model would predict, the entry of for-profit chains into higher education markets is not random. It is correlated with observable and unobservable features of a market which vary over time. Moreover, these correlations are in line with the notion that these companies actively seek out clients eligible for government financial aid, a potentially predatory practice. We control flexibly for these correlated covariates in our models estimating causal entry effects.

Further diagnostics of our empirical strategy reveal another potential identification challenge. Event study estimates with the number of non-chain for-profit colleges as the outcome – most of them being single-establishment companies – show that chain entry is positively correlated with entry of other smaller for-profits. This pattern in the data is persistent across multiple potential definitions of "for-profit chain" colleges, so we conclude that it is an inherent feature of this strategy. We thus caution that our estimates are better interpreted as the effect of entry of multiple for-profit colleges into a single market, being unable to accurately identify the effect of each additional for-profit campus opening.

We find that the entry of for-profit chains has a positive effect on total enrollment and completions in postsecondary education markets. Our estimates show that the entry of an additional for-profit chain college is associated with a 2.4% increase in total enrollment and a 3% in degree and award completions. These aggregate gains are largely driven by colleges classified as "less than 2-year" institutions which award certificates typically obtained within one-year, although we also detect enrollment increases at 2-year institutions and – to a lesser extent – 4-year colleges.

Moreover, we test for impacts of for-profit chain entry on enrollment at public and private non-profit colleges by sector. We find little evidence of negative effects on public institutions in the 2-year or 4-year sectors. However, there is some indication of temporary negative effects for private colleges, particularly those in the non-selective 4-year sector.
CHAPTER 2. FOR-PROFIT COLLEGE CHAIN ENTRY

Taken together, the results suggest that for-profit colleges tend to serve students that would otherwise not attend a college from a different sector. In other words, it indeed appears that for-profits address excess demand for higher education, and that competition between public and for-profit institutions is limited. Instead, other non-selective private colleges are more likely to suffer enrollment losses due to the entry of for-profit competitors. Hence, the evaluation of policies regulating the for-profit sector should consider a counterfactual in which students do not acquire higher education at all without the presence of for-profit colleges. This is consistent with the stylized fact that students at for-profit colleges tend to be older, more likely to work, and enroll in night time classes (Deming, Goldin, and Katz 2013). The fact that public and non-profit institutions are less likely to provide flexible options for this population generates business opportunities for for-profit college companies.

To our knowledge, we are the first to estimate the causal effect of the entry of for-profit chain colleges on postsecondary education markets. The most closely related paper is Cellini, Turner, and Dariola (2016), which estimates the impact of federal financial aid sanctions on postsecondary enrollment. They find that, between 1986 and 2005, the threat of federal sanctions on Title-IV-eligible 2-year or lower for-profit colleges, led to decreases in enrollment at for-profits which were counteracted by enrollment gains at public community colleges. At first glance, these results are inconsistent with our findings, but a brief analysis provides an explanation for this discrepancy. The elasticity we identify is inherently different – we estimate the effect of for-profit chain entry, while they estimate the effect of federal sanctions, which presumably prompt for-profit exit. Substitution from a public community college to an entering for-profit chain college is costlier than substituting away from a federally-sanctioned failing for-profit college along many dimensions. Such heterogeneity in substitution effects depending on entry or exit events leads us to call caution on generalizing the external validity of these studies to different circumstances.

The rest of the paper proceeds as follows. Section 2, describes the historical background of for-profit college chains and summarizes the analysis sample. Section 3 presents our research design and reports results from several diagnostic tests of its validity. Section 4 reports the main results of the paper as well as several robustness checks. Section 5 concludes.
2.2 Background and Data

The for-profit college sector has operated in the United States for over a century. Small proprietary schools specializing in technical, vocational, and applied subjects are common place in the majority of urban centers across the country (Deming, Goldin and Katz, 2012). However, attention has focused recently on the tremendous recent growth of for-profit college chain companies. Over the last two decades, these companies have opened almost 1,000 college branches and reached aggregate enrollment counts of over 1.5 million students by 2010 (without counting online enrollment), as shown in Figure 1. Enrollment in this sector peaked that year, and then initiated a relatively modest decline.

The entrance of large corporations (many of which are publicly traded) into the postsecondary education market has been controversial. A spawning literature across several disciplines demonstrates that these colleges frequently engage in "Wall Street" practices, setting them apart from the common notion of educational institutions as inherently invested in the success of their pupils. Aggressive cost minimization, frequently at the expense of educational resources and quality, as well as misleading marketing strategies that exaggerate the labor market gains of acquiring short-term certificates are common practice among for-profit college chains (Mcmillan Cottom, 2017; Deming, Goldin and Katz, 2012).

Importantly, the new generation of for-profit colleges offer associate, bachelor, and in some cases even graduate degree programs, potentially placing them in direct competition with public colleges. The price tag on these programs, however, is typically much higher than that of equivalent programs at public institutions. Moreover, the modal student at these colleges is from a low-income background, making the vast majority of for-profit college attendants eligible for federal financial aid. Consequently, the vast majority of the revenue these firms generate originates from federal tax dollars, with many of them reaching the federal cap of 90% of total revenues coming from financial aid. Critics thus have claimed that the principal business strategy of these colleges is to aggressively recruit disadvantaged students into overpriced programs effectively paid by the government – or via burdensome student debt – while providing subpar higher education and granting degrees that have little labor market value (Mcmillan Cottom, 2017).

In light of these circumstances, the Obama Department of Education developed "gainful employment" regulations for postsecondary institution eligibility for Title-IV funding
CHAPTER 2. FOR-PROFIT COLLEGE CHAIN ENTRY

in 2014 (Department of Education, 2017). The reforms placed strict limits on average debt-to-earnings ratios of college graduates X years after completion. In addition, in 2015 the Department of Education placed stricter financial scrutiny over some of the largest for-profit college companies, leading up to the high-profile closures of for-profit chains such as Corinthian Colleges and ITT Technical (Stratford, 2015; Smith, 2016).

Nonetheless, the advent of the Trump-Devos Department of Education has led to the freezing of gainful employment regulations, the removal of protections on student debt, as well as a recent rally in the stock price of publicly traded for-profit college companies (Brookings, 2017; Chicago Tribune 2017). Therefore, it may be safe to assume that for-profit college chains will continue to engage in similar practices and continue offering services to thousands of students coming years.

Data

Using the IPEDS for the survey years 1998 through 2013, we construct a longitudinal dataset of non-selective Title-IV-eligible postsecondary institutions.\(^2\) We focus on measuring 12-month total unduplicated enrollment and total degree and award completions.\(^3\) The resulting data is a highly unbalanced panel of colleges. Following the existing literature on for-profit postsecondary education. We define a postsecondary education market to be a U.S. county. Hence, an implicit assumption throughout our analysis is that institutions located within the same county could potentially compete with each other, but institutions located across county lines may not. Then, exploiting the unbalanced nature of the panel, we define entry into a postsecondary market as the first time a given institution reports having an operating branch in a given county.\(^4\)

We supplement the IPEDS with annual county data from the Bureau of Labor Statistics as well as the U.S. Census. We use these sources to measure county unemployment, the poverty rate, population among individuals aged 20 to 29 years, and median house-

\(^2\)Institution selectivity is defined using Barron’s definitions in 2009. We include in the sample colleges that were Title-IV eligible during at least one academic year between 1998-2013.

\(^3\)12-month unduplicated enrollment captures a wider net of students which don’t necessarily begin their studies at the beginning of a Fall or Spring semester as in regular public colleges. This is the correct way of measuring enrollment at for-profit colleges, as they frequently offer short programs that don’t follow the traditional academic calendar.

\(^4\)Some survey years in the IPEDS do not include county identifiers. When county identifiers are not present, I extrapolate the location of a college by looking at other survey years.
CHAPTER 2.  FOR-PROFIT COLLEGE CHAIN ENTRY

We define a For-Profit Chain (henceforth, FPC) postsecondary education company following Deming, Goldin, and Katz (2012) – an FPC is a for-profit institution that operates in more than one state or has more than five campus branches in operation. Moreover, institutions with the word "online" in their name or classified as "distance education" by IPEDS are removed from the sample. Using this definition, our sample contains 175 FPC companies operating 2,076 brick and mortar colleges across the U.S. mainland during 1998-2013.

Table 1 reports a list of the twenty largest FPC companies in our data. The ranking is defined using 2010 total enrollment – the year in which national enrollment in this sector peaked, as shown in Figure 1. The rate of growth of these companies is evidently striking. In 2010, ITT Technical was the largest FPC with 121 branches in operation and aggregate enrollment of 78,457 students – up 281% from their enrollment in 2000. Closely behind were Everest and the University of Phoenix, with total enrollment (branches) of 65,983 (78) and 64,381 (67) – up 256% and 432% from 2000 enrollment levels, respectively. The rate of growth of these companies over a ten year period is quite outstanding. By 2014, all three of these companies had come under heavy scrutiny from the Department of Education (Stratford, 2015; Smith, 2016).

Having classified FPC postsecondary institutions, we collapse the data to the county-year level measuring annual market enrollment and completions by institution sector. We restrict the sample to enforce a balanced panel of counties with non-zero college enrollment at any non-FPC colleges. After making these restrictions there are 1,316 counties in the sample. Out of these, 348 experience an FPC entry event during the sample period.

Table 2 presents summary statistics for the sample of counties used for our analysis for the years 2000, 2005, and 2010. We report mean county characteristics for the full sample, as well as for the sample for which we observe an FPC entry event. We show total postsecondary enrollment and institutions, as well the fraction of these going to the public, non-profit private, and for-profit sectors. Not surprisingly, FPC entry counties are typically larger than average both in terms of population and total enrollment. FPC entry counties also tend be more racially diverse; they have higher median household income; and they have larger concentrations of individuals ages 20 to 29.
Moreover, Table 2 provides suggestive evidence of enrollment substitution from the public and non-profit sectors to the for-profit sector. There is a positive trend in the share of enrollment going to for-profits, with a steeper growth rate in FPC entry counties. Concurrently, there is a downward trend in both public and non-profit enrollment and the rate of decline is stronger in FPC entry counties. While these patterns are suggestive evidence that for-profit chains cause students to switch out of public and non-profit colleges, there should be caution in interpreting them as such. For instance, it may be the case that for-profit chains enter markets in which public colleges don’t have enough seats to satisfy local demand due to declining state funding.

2.3 Research Design

We exploit within-county variation in the timing of FPC entry to estimate the causal impact of FPCs on postsecondary education markets. Therefore, the key identification assumption in our research design is that – when making strategic market entry choices – FPC companies do not select based on market-specific enrollment trends in the public, non-profit, or non-chain for-profit sectors. With this identification strategy in mind, we estimate impacts using the following class of regression models

\[
Y_{c,t} = \alpha_c + \gamma_t + \sum_{k=-K}^{K} \beta_k FPC_{c,t-k} + X'_{c,t} \Pi + \varepsilon_{c,t} \tag{2.1}
\]

where \( Y_{c,t} \) is the outcome of interest for county \( c \) in year \( y \), \( \alpha_c \) and \( \gamma_t \) are county and year fixed effects, \( FPC_{c,t-k} \) are leads and lags of the number of FPC branches present, and \( X'_{c,t} \) is a vector of time-varying county characteristics including: county unemployment rate, log population 20 to 29 years old, log median household income, and the poverty rate. We cluster standard errors at the county level across all regression models.

We are interested in the coefficients \( \beta_k \) for \( k = -K, \ldots, 0, \ldots, K \), which describe the relationship between number of FPCs in year \( t \) and outcomes \( k \) years earlier, controlling for permanent differences across counties, changes over time common to all counties, and time-varying covariates. For \( k > 0 \), the coefficient measures the effect of lags in the number of FPCs on current outcomes – testing whether the presence of FPCs in previous years is predictive of enrollment in the current year. For \( k < 0 \), \( \beta_k \) is the effect of leads of the number of FPCs on current outcomes – they test whether outcomes prior to treatment are correlated with the timing of FPC entry. The lead effects measure
pre-trends. Finally, the coefficient for \( k = 0 \) is the contemporary effect – the effect of the current number of FPCs on current enrollment and completions. It should be noted that, unlike standard event study models, effects estimated via equation (1) are interpreted additively. For instance, if we want to know the effect of FPC entry of enrollment two years after the entry event, we need to compute \( \beta_0 + \beta_1 + \beta_2 \).

In order to facilitate the interpretation of our results, we apply a transformation to the main outcomes of interest: county enrollment and completions. We are especially interested in these outcomes conditional on sector – e.g. enrollment at 2-year public colleges. Hence, there are many zeros in the data – leading to different sets of observations getting dropped in models across different outcomes – were we to use a simple log transformation. We consider such variation in the sample across regression models undesirable. Thus, in order to retain the interpretation of our findings in approximate percentage terms and fix the estimation sample, we opt for the inverse hyperbolic sine transformation. Results are robust to instead using the \( \ln(Y + 1) \) transformation.

**Pre-Trends in County Characteristics**

One of the advantages of our empirical strategy is that it allows us to correlate trends in the outcomes of interest with FPC entry prior to the entry event. In other words, it enables us to test for pre-trends. The existence of such pre-trends would lead one to cast serious doubt on the causal interpretation of our estimates. In section 4, we present evidence suggesting no pre-trends in the main outcomes of interest. We can also use this framework to test for pre-trends in county characteristics. Such an exercise informs us of which time-varying county characteristics are associated with FPCs strategic entry decisions.

Figure 2 presents graphs summarizing estimates of OLS models similar to equation (1), except that we exclude lags of FPCs present, focusing on the lead effects instead. Coefficients are arranged in event time so that negative indices correspond to lead values of number of FPCs present. The top left panel shows that within-county increases in the concentration of individuals aged 20 to 29 years is a marginally significant predictor of FPC entry, specially 4 to 5 years prior to the entry event. The top right panel shows that negative pre-trends in log median household income are stronger predictors of FPC entry – the entry of an FPC is associated with 0.2% lower than usual household income
two years prior.

Moreover, the bottom left panel of Figure 2 shows the existence positive pre-trends in county unemployment prior to FPC entry – in the year prior to the event FPC entry counties tend to have between 0.05% to 0.1% higher unemployment relative to other years. Finally, The bottom right panel presents compelling evidence that counties facing growth in the poverty rate of about 0.1% over the previous four years are significantly more likely to see the arrival of an FPC branch.

The consistency and statistical significance of these results paint a stark picture of the types of counties likely to be targeted by FPCs as potentially profitable postsecondary education markets. It is counties with lower than usual income as well as higher than usual unemployment and poverty that these companies are more likely to choose. This result is in line with the emerging consensus in the literature on the for-profit sector. FPC companies actively seek markets with growing concentrations of individuals from vulnerable backgrounds which are in turn more likely to be eligible for financial aid (Cellini 2010; Cellini and Goldin 2012; Deming, Goldin, and Katz, 2012).

In addition, the evidence presented in Figure 2 generates a concern for our identification strategy. Pre-trends in county characteristics leading to FPC entry imply the existence of time-varying systematic differences among treated and control counties. Moreover, there is evidence that variation in local unemployment rates is predictive of college enrollment (Deming and Walters, 2018). Given this reasoning, all specifications presented in our results control for leads and lags of the county characteristics available.

Diagnostics – Concurrent Entry of Other Institutions

One potential threat to our identification strategy is the presence of unobserved time-varying county features which are correlated with both FPC entry and our outcomes of interest. One way in which this could happen is if there are other colleges entering the market simultaneously to FPCs. It may be the case, for instance, that there is a county-year unobservable which makes the market particularly attractive for the for-profit market sector, leading to increased enrollment, out of which only a portion is driven by FPCs. Such a scenario would lead us to wrongly attribute some of the effect to FPCs which is in reality attributable to other institutions.

We test for this issue in Figure 3. We estimate models akin to equation (1), but using
the number of institutions across different sectors present in a given county-year as the outcome. The coefficients on leads and lags of FPC presence capture whether the timing of entry of FPCs correlates with the entry (or exit) of other institutions. The top left panel of Figure 3 reports results for the number of non-chain for-profit colleges present. The patterns suggest that there is concurrent entry of other for-profit colleges that do not fall into our FPC definition. A year prior to FPC entry (-1 in event time), treated counties are facing increases in the number of non-chain for-profit colleges. The pattern remains in both the entry year, as well as a year after.

The top right panel in Figure 3 tests for concurrent entry for non-profit private colleges. There is a statistically significant reduction in the number of private non-profit colleges, taking place on the year of FPC entry. This result is suggestive of a potential negative competitive effect on this sector, an issue we explore further in section 4. The bottom left and right panels test for concurrent entry of all public colleges as well as public colleges in the 2-year or lower sector (i.e community colleges), respectively. The results suggest that there is no concurrent entry or exit in the public sector associated with the timing of FPC entry.

The evidence reported in Figure 3 demonstrates that there is entry of non-chain for-profit colleges that is concurrent with FPC entry. Consequently, our estimates of the effect of each additional FPC branch on total enrollment in a local postsecondary market are likely to be biased upward, as we are actually measuring the effect of all for-profits that enter. We consider this to be acceptable, as the parameter being estimated is still policy relevant; we are capturing the effect of the emergence of a favorable conditions leading to multiple for-profit college entry on postsecondary markets.

2.4 Results

The top panel of Figure 4 estimates the effect of FPC entry on total county enrollment and completions for all postsecondary institutions. All regression estimates presented in this section control for county and year fixed effects as well as five leads and lags of county unemployment, log population 20-29 years old, poverty rate, and log median household income, as in equation (1). The top panel of Figure 4 reports effects on total enrollment. First off, there are no pre-trends in county college enrollment prior to FPC entry – as indicated by the negative event time coefficients oscillating about zero. Then, upon FPC
entry, total county enrollment increases by 2.3%, and a year after enrollment is up by an additional 1%. The enrollment effect then stabilizes, persisting over the following 3 years. The bottom panel of Figure 4 shows that total degree and award completions respond to FPC entry in a similar fashion. There is little sign of pre-trends and FPC entry leads to an immediate 3% increase in completions, with a marginally significant 0.6% additional increase a year after entry.

It is initially surprising to see degree completions react in the same year of FPC entry (event time 0). The common notion is that a college certificate takes more than a year to complete. In such a case, one would expect effects, if any, to become noticeable at a two or four year lag from the year of entry. Exploring this issue further, Figure 5 presents our estimates of the effect FPC entry on total enrollment and completions, by level of institution.

The top and bottom panels in the first column of Figure 5 show the effect of FPC entry on enrollment at 4-year institutions and bachelor’s degree completions. FPC entry causes an immediate 5.75% increase in enrollment at 4-year institutions. Although there is some evidence of pre-trends in this outcome, this result is in line with the stylized fact that FPC colleges have entered the 4-year market. Encouragingly, bachelor’s degree completions do not see a significant instantaneous effect. Instead, we see a marginally insignificant 4.1% increase in bachelor’s degree awards precisely 4 years after the entry of FPCs.

The panels in the second column of Figure 5 show entry effects on enrollment at 2-year institutions (top) and associate’s degree completions (bottom). We estimate a 10% county-wide positive effect on 2-year enrollment upon FPC entry. Puzzlingly, we observe a 3.33% instantaneous effect on associate degree completions. One explanation for this is that some of these completions are happening at smaller for-profits that lead the entry of FPC, as discussed in Section 3.2. The effect on associate’s completions grows considerably for a total effect of 8.66% over the next three years.

Lastly, the panels in the third column of Figure 5 show the effect of FPC entry on enrollment at 1-year institutions (top) and less than associate’s degree completions (bottom).\textsuperscript{5} It is these institutions which appear to drive the bulk of the aggregate effects we

\textsuperscript{5}We refer to "less-than-2-year" institutions in the IPEDs as "1-year" institutions. In reality these type of programs may take less than one year to complete as they include any program that award degrees and certificates lower than an associate’s degree.
CHAPTER 2. FOR-PROFIT COLLEGE CHAIN ENTRY

observe in Figure 4. Upon FPC entry 1-year enrollment rises by 22.5% and completions of less-than-associate’s degrees jump up by 6.77%. By the third year since entry these effects total 18.93% for enrollment and 8.74% for completions. Interestingly, our estimates for the 4th year suggest a significant fade-out of the effects for this sector which is not present for other sectors. An explanation for this is that FPC entry leads to crowd-in of 1-year institution enrollment into earlier years.

In summary, the evidence presented in Figure 5 suggests that FPCs increase enrollment across all levels of the postsecondary education market. Nonetheless, the bulk of enrollment gains are concentrated in 1-year programs and – to a lesser extent – 2-year programs. In terms of degree and award completions, the takeaway is similar – the entry of for-profit chains leads to quick gains in 1-year certificates and associate’s degrees. However, there is no significant effect on bachelor’s degree completions, despite enrollment gains in this sector.

We now shift our focus to the effect of FPC entry on substitution away from other institutions. Figure 6 reports the effect of FPC entry on enrollment at public and non-profit private colleges. The top panel shows a strikingly precise null result on enrollment at public institutions. There is no indication that FPC entry affects enrollment at public institutions at all. Moreover, we can firmly reject effects larger than 1.5% in absolute value. The bottom panel of Figure 6, corresponding to enrollment effects at non-profit privates, tells a different story. Our estimates suggest an instantaneous negative effect on private college enrollment, which is almost fully corrected by the first year after entry. The key takeaway from this evidence is that FPC entry is not strongly associated with enrollment declines at colleges in other sectors. At most, there is a brief competitive effect with non-profit private colleges.

We further investigate the potential presence of substitution effects generated by FPC entry in Table 3, which reports enrollment effects at public and non-profit institutions by institution level. We retain the order of the coefficients as in the event-study figures with negative event time corresponding to lead effects as in equation (1). The first noticeable pattern here is the lack of pre-trends across outcomes, with perhaps the exception of 2-year non-profit privates. The second apparent result from these estimates is the lack of statistically significant coefficients for contemporaneous, as well as lagged, FPC entry effects. Columns (1) through (3) show results for enrollment at public institutions, separate by 4-year, 2-year, and 1-years IPEDS categories. Effect estimates are statistically
in insignificant; the coefficients oscillate tightly around zero and we cannot reject that the total effect is zero across any of these specifications. Therefore, the evidence suggests that traditional public colleges do not face enrollment losses upon the entry of for-profit college chains.

Columns (2) through (6) in Table 3 test for enrollment effects at private institutions. FPC entry is associated with an immediate fall of about 4% enrollment at non-selective private 4-years – however, the effect fades out over the next two years. While the coefficients on the lagged variables are statistically insignificant, we cannot reject that the total effect (over the observed time period) is zero. A similar pattern emerges when examining impacts on 2-year and 1-year non-profit private institutions, as exemplified by columns (5) and (6). While statistically insignificant across the board, the estimates indicate that FPC entry leads to an initial and temporary decline in enrollment at non-profit private institutions. There are signs of pre-trends in these estimates, specially in private 2-year enrollment, complicating the interpretation of these results as causal. Altogether the results point to a modest temporary substitution effect away from private non-profits, but this result is weak relative to the aforementioned ones.

Robustness Checks

We now test the robustness of our main results to several potential threats to our empirical strategy. The focus of these robustness checks is on the choice of transformation of the outcome variables, the definition of treatment, and changes to the analysis sample.

Recall that we make use of the inverse hyperbolic sine transformation on the outcomes of interest: enrollment and completions, by sector. The motivation for this choice is the widespread existence of zeros in our outcome data (for instance, counties that do not have a non-profit private college will have zero enrollment in this sector). The first row in Table 6 reports our baseline results using this transform. Regression models in this table are akin to equation (1), with the exception that they exclude lags of $FPC_{c,t}$. Therefore, the coefficient on $FPC_{c,t}$ – reported in the table – is interpreted as the total pots-FPC-entry effect on county outcomes. Our baseline results are that county-wide enrollment and completions increase by 2.2% and 3.2%, respectively. Moreover, FPC entry has a statistically zero effect on enrollment at public and non-profit colleges. In the second row we use the $ln(Y + 1)$ transformation on the outcomes instead. The results
using this alternative transformation are essentially identical to our baseline.

One potential critique of our strategy is the choice of definition of a for-profit college chain company. We attempt to tie our hands regarding this by employing the definition put forth by Deming, Golding and Katz (2012). This definition results in 175 companies falling into the chain category in our data. Still, one could argue that some of the companies under this category are too small to generate salient effects. For this reason, we handpick a list of "Big Name" FPC companies and re-estimate our causal model with the presence of these companies as treatment.\footnote{"Big Name" for-profit chain companies are the following: ITT Technical, Everest University, University of Phoenix, Kaplan, Devry University, Strayer, Career Education Corporation, Brown Mackie, Heald, Sanford Brown, Virginia College, Fortis, Education Affiliates Inc., Lincoln Technical, and National American University.} In the third row of Table 6 we present the results. With this treatment definition, our causal estimates of FPC entry on county-wide enrollment and completions is a statistically significant 4.4\% and 4.8\%, respectively. Moreover, while point estimates for enrollment at public an non-profit 2-year institutions become negative, we cannot reject that they are zero. We conclude that big name for-profit chain colleges may lead to larger county-wide effects on enrollment and completions; but we find no indication that they generate significantly different effects on public and non-profit colleges relative to our baseline estimates.

Another potential issue with our baseline estimates is that our sample includes densely populated counties (e.g. Los Angeles and New York City), which in most cases already had at least one FPC branch present at the beginning of our sample in 1998. There may be two potential issues with keeping these counties in the sample. First, since we do not observe a strictly non-treated pre-period for these counties, they help identify the effect of an additional FPC college branch being opened. However, we are mainly interested in the effect of a salient FPC entry event in case in which no previous presence. Second, these counties are outliers with regard to total population, hence it is unlikely that FPC entry event would lead to substantial county-wide outcome changes. The row labeled "always-treated counties" in Table 6, shows that restricting the analysis sample to counties that had a non-treated pre-period see considerably larger (about 3 times larger than our baseline results) total enrollment and completion effects following FPC branch entry, likely due to the dropping of population outliers. Still, we find no indication of negative enrollment effects at public schools and noisy negative estimates in non-profit colleges.
Finally, we run a substantially more restrictive model that focuses exclusively on counties that go from having no FPC branches to having a single one. Such a model may be desirable as it emulates most closely a standard event study model – effects are estimated strictly off a dummy variable that measures the timing of treatment – making the interpretation of the estimates as straightforward as possible, at the cost of sample selection and loss of power. There is 90 counties in the data that see this type of entry event. The last row in Table 6 – labelled "only single-event counties" – reports our causal estimates for the restricted models. The effect of the entry of an FPC branch on county-wide enrollment is a statistically significant 11.7% for this sample, with a corresponding 14.1% increase in degree and certificate completions. These total enrollment effects are the largest we find, and yet there is still little indication of negative effects on enrollment at public or non-profit colleges – perhaps with the exception of non-profit 4-years, but the estimate is noisy.

2.5 Conclusion

Over the last two decades, corporations operating chain for-profit college brands have expanded aggressively into American postsecondary education markets. Theoretically, the effect that the entry of a for-profit college chain branch has on a locality’s college age population is ambiguous. There is a large amount of anecdotal evidence on predatory practices by these institutions, as well as evidence that degrees granted by these colleges are of low job market value. Nonetheless, policy implications differ on whether the counterfactual scenario facing the average for-profit college chain student is to attend a public (or non-profit private) college or to not attend college at all.

In this paper we contributed to the literature by estimating event study models of the effect of the entry of a for-profit college chain branch on postsecondary markets, using college survey data from the IPEDS. We showed that counties seeing the entry of these colleges are more likely to be facing economic downturns, as measured by a downward trend in median household income and upward trends in local unemployment and poverty rates. We also showed that the timing of entry of for-profit chains is correlated with the entry of smaller for-profit colleges, such that our estimates are best interpreted as the effect of a favorable environment for for-profit college entry.

Our main results are twofold. First, we showed that for-profit chain entry leads to
a significant increase in county postsecondary enrollment, as well as degree and certificate completions. Digging into this result further, we demonstrated that these effects are concentrated in short certificate programs (those that take less than 2 years to complete), although there is indication of relatively smaller gains in 2-year enrollment and associate’s degrees. Second, we showed that there is little to no indication of negative enrollment effects at public colleges, across all levels. We test this result against a battery of robustness checks, which do little to change the qualitative nature of our results. We do find a temporary negative enrollment effect on non-profit private colleges, but this result is sensitive to the particular specification.

Taken together we interpret these results as suggestive evidence that for-profit colleges serve markets facing excess demand for higher education. If, on the other hand, these colleges competed directly and cannibalized into the enrollment of traditional colleges, we would expect county-wide enrollment to remain constant and enrollment at public and non-profit private. Such a pattern is strongly rejected in the data. We therefore conclude that students attending for-profit college chains come from a different segment of the population than traditional college students, a theory that has been suggested repeatedly in the literature. We hope that further research will further investigate how students select into a for-profit college chain education, but given our findings we caution against hastily concluding that students directly substitute away from public colleges to attend for-profit institutions.

Still, even if the counterfactual for these students is the complete absence of postsecondary education, our findings should not be interpreted as placing for-profit college chain companies in a positive light. It is certainly the case that these colleges tend to engage un dubious education practices, to say the least. Our findings should instead inform policymakers of the right trade-off to have in mind when considering limiting the entry of for-profit college chain companies.
Figure 2.1: Growth in the For-Profit Chain Sector as a Share of National Postsecondary Enrollment, 1998-2013.

Note: National postsecondary enrollment is measured as 12-month unduplicated enrollment at non-selective Title-IV-eligible colleges in the U.S. mainland, reporting to the IPEDS for the years 1998-2013. An FPC college branch is defined as a postsecondary institution belonging to a for-profit college company with more than 5 branches or branches across state lines (see Deming, Golding and Katz, 2012).
**Figure 2.2:** Pre-Trends in County Characteristics Leading to the Entry of a For-Profit Chain (FPC) College Branch.

Note: Figures report OLS coefficient estimates of four regression models predicting the following county characteristics: log population 20 to 29 years old, log median household income (2014 USD), the county unemployment rate, and the county poverty rate (the latter two are measured in percentage points). The explanatory variables are $FPC_{c,t}$ – the number of FPC branches present in a county-year – as well as four leads of this variable. The figure also reports 95% confidence intervals. Standard errors are clustered at the county level in all regressions.
Figure 2.3: Association of For-Profit Chain (FPC) Entry with Entry/Exit of Colleges in other Sectors.

Note: Figures report OLS coefficient estimates of four regression models predicting the number of colleges present in a county-year, for the following sectors: non-chain ('other') for-profits, non-profit privates, public institutions, and public 2-years (community colleges). The explanatory variables are $FPC_{c,t}$ – the number of FPC branches present in a county-year – as well as four leads and lags of this variable, ordered in event time. The figure also reports 95% confidence intervals. Standard errors are clustered at the county level in all regressions.
Figure 2.4: The Effect of For-Profit Chain (FPC) Entry on County-wide Enrollment and Degree/Certificate Completions.

Note: Figures report OLS coefficient estimates of two regression models of county-wide postsecondary enrollment and degree/certificate completions. The explanatory variables are $FPC_{c,t}$ – the number of FPC branches present in a county-year – as well as four leads and lags of this variable, ordered in event time. The model also includes the following covariates: county unemployment and poverty rate, log median household income, and log population 20 to 29 years old; we also include four leads and lags of these covariates. The figure reports 95% confidence intervals. Standard errors are clustered at the county level in all regressions.
**Figure 2.5:** The Effect of For-Profit Chain (FPC) Entry on Total County Enrollment and Completions, by Institution Level.

Note: Figures report OLS coefficient estimates of six regression models of county-wide postsecondary enrollment and degree/certificate completions, by institution level as reported by the IPEDS. The explanatory variables are $FPC_{c,t}$ – the number of FPC branches present in a county-year – as well as four leads and lags of this variable, ordered in event time. The model also includes the following covariates: county unemployment and poverty rate, log median household income, and log population 20 to 29 years old; we also include four leads and lags of these covariates. The figure reports 95% confidence intervals. Standard errors are clustered at the county level in all regressions.
Figure 2.6: The Effect of For-Profit Chain (FPC) Entry on Total County Enrollment and Completions at Public and Non-profit Private Institutions

Note: Figures report OLS coefficient estimates of two regression models of county-wide postsecondary enrollment at public and non-profit private institutions. The explanatory variables are $FPC_{c,t}$ – the number of FPC branches present in a county-year – as well as four leads and lags of this variable, ordered in event time. The model also includes the following covariates: county unemployment and poverty rate, log median household income, and log population 20 to 29 years old; we also include four leads and lags of these covariates. The figure reports 95% confidence intervals. Standard errors are clustered at the county level in all regressions.
Table 2.1: List of Twenty Largest For-Profit College Chains by Total 2010 Enrollment

<table>
<thead>
<tr>
<th></th>
<th>2000</th>
<th></th>
<th>2005</th>
<th></th>
<th>2010</th>
<th></th>
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<tr>
<td></td>
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<td>Enrollment</td>
<td>Branches</td>
<td>Enrollment</td>
<td>Branches</td>
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<td>ITT Technical</td>
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<td>67</td>
<td>42450</td>
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<td>121</td>
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<td>Everest</td>
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<td>53059</td>
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<td>71904</td>
<td>59</td>
<td>64381</td>
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<td>Art Institute</td>
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<td>21</td>
<td>37962</td>
<td>26</td>
<td>56755</td>
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<tr>
<td>Kaplan</td>
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<td>32801</td>
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<td>Devry</td>
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<td>19963</td>
<td>19</td>
<td>38433</td>
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<td>Career Education Corp.</td>
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<td>27562</td>
<td>24</td>
<td>32301</td>
<td>29</td>
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<tr>
<td>Sanford Brown</td>
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<td>15</td>
<td>9215</td>
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<td>2904</td>
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</table>

Note: Source – IPEDS 1998-2013. Enrollment is the sum of 12-month unduplicated enrollment across all branches of a given for-profit college company.
Table 2.2: County Panel Summary Statistics

<table>
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<tr>
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<th>2000</th>
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<tr>
<td></td>
<td>All (1)</td>
<td>Entry (2)</td>
<td>All (3)</td>
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<tr>
<td>Postsecondary Enrollment</td>
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<td>Presence of LFP</td>
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<td>County Characteristics</td>
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<tr>
<td>Population 20 to 29 yrs</td>
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<td>Fraction Minority</td>
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<td>Median HH Income (2014 USD)</td>
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<td>Observations</td>
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<td>1316</td>
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Note: Sources – IPEDS, BLS, U.S. Census Bureau. Sample is all U.S. Counties with non-zero, non-LFP enrollment in all years 1998-2013. Columns (1), (3), and (5) report statistics for the sample of All counties with the presence of colleges reporting to IPEDS in the given year, as well as the sample for which for-profit college chain Entry is observed. Columns (2), (4), and (6) report statistics for the sample of counties that experience variation in the number of FPC branches present during the period 1998-2013, the treatment sample in our regression models.
Table 2.3: Effect of For-Profit Chain (FPC) Entry on Total Enrollment at Public and Non-Profit Private Colleges

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</thead>
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<td></td>
<td>(1)</td>
<td>(2)</td>
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<tr>
<td></td>
<td>4-year</td>
<td>2-year</td>
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<tr>
<td>– 4</td>
<td>-0.0278</td>
<td>-0.0452</td>
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<tr>
<td></td>
<td>(0.0226)</td>
<td>(0.0417)</td>
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<td>(0.0260)</td>
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<td>(0.0171)</td>
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<td></td>
<td>(0.00652)</td>
<td>(0.0218)</td>
</tr>
<tr>
<td>1</td>
<td>-0.00264</td>
<td>0.0114</td>
</tr>
<tr>
<td></td>
<td>(0.0221)</td>
<td>(0.0172)</td>
</tr>
<tr>
<td>2</td>
<td>0.00582</td>
<td>-0.0346</td>
</tr>
<tr>
<td></td>
<td>(0.0113)</td>
<td>(0.0331)</td>
</tr>
<tr>
<td>3</td>
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<td>-0.0165</td>
</tr>
<tr>
<td></td>
<td>(0.0140)</td>
<td>(0.0342)</td>
</tr>
<tr>
<td>4</td>
<td>0.0374</td>
<td>0.0183</td>
</tr>
<tr>
<td></td>
<td>(0.0490)</td>
<td>(0.0622)</td>
</tr>
</tbody>
</table>

|               | County FE              | ✓                         | ✓                         | ✓                         | ✓                         | ✓                         |
|               | Year FE                | ✓                         | ✓                         | ✓                         | ✓                         | ✓                         |
| N             | 21050                  | 21050                     | 21050                     | 21050                    | 21050                     | 21050                     |
| R²            | 0.962                  | 0.927                     | 0.782                     | 0.978                    | 0.824                     | 0.792                     |

Note: Standard errors are clustered at county level in all models. Table reports OLS coefficients of county-wide postsecondary enrollment at public and non-profit private institutions, by institution level. The explanatory variables are $FPC_{c, t}$ – the number of FPC branches present in a county-year – as well as four leads and lags of this variable, ordered in event time. The model also includes the following covariates: county unemployment and poverty rate, log median household income, and log population 20 to 29 years old; we also include four leads and lags of these covariates.
### Table 2.4: Robustness Checks on Main Results

<table>
<thead>
<tr>
<th></th>
<th>County-wide Effect</th>
<th>Public Enrollment</th>
<th>Non-Profit Enrollment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Enrollment Completions</td>
<td>2-year 4-year 2-year 4-year</td>
<td></td>
</tr>
<tr>
<td>Baseline Result</td>
<td>.022 (.005)</td>
<td>.014 (.032)</td>
<td>.019 (.019)</td>
</tr>
<tr>
<td>ln(Y+1) Transformation</td>
<td>.022 (.005)</td>
<td>.013 (.032)</td>
<td>.018 (.013)</td>
</tr>
<tr>
<td>Big Name FPCs Treatment</td>
<td>.044 (.013)</td>
<td>-.059 (.076)</td>
<td>.041 (.064)</td>
</tr>
<tr>
<td>No Always-Treated Counties</td>
<td>.077 (.022)</td>
<td>.111 (.097)</td>
<td>.03 (.055)</td>
</tr>
<tr>
<td>Only Single-Event Counties</td>
<td>.117 (.041)</td>
<td>.037 (.233)</td>
<td>.239 (.14)</td>
</tr>
</tbody>
</table>

*Note:* Standard errors are clustered at county level in all models. Each cell reports the OLS coefficient (and standard error in parenthesis) of $FPC_{c,t}$, which measures the number of For Profit Chains branch present in a county-year. All regressions include county and year fixed effects, leads and lags of the county unemployment, the poverty rate, median household income and population 20 to 29 years old, as well as leads of $FPC_{c,t}$. The number of observations in the first three rows is 21,050 (1,316 unique counties). In the fourth row the number of observations is 16,634 (1,040 unique counties). The last row uses 1,440 observations (90 unique counties).
Chapter 3

Evaluating the Texas Dream Act: The Effect of Tuition Equity Reform on the College Demand of Undocumented Students

3.1 Introduction

Immigration reform is one of the centerpieces of an increasingly polarized political debate in the United States. This debate has partly focused on the adjustment of status of unauthorized immigrants that have resided in the country beginning at a young age and for extended periods of time. Institutionally, this immigrant subgroup resides in a unique limbo: on the one hand they face many institutional constraints given their lack of status – for instance, being ineligible for employment and the social safety net. On the other hand, federal law mandates public schools to serve this subgroup, and several government entities (both at the state and federal level) have recently initiated efforts to alleviate institutional constraints for them. One of the key movements in this recent trend is the enactment of tuition equity reforms by state legislatures.

As of 2015, eighteen states in the nation had enacted tuition equity laws granting resident tuition rates to qualifying undocumented students.\(^1\) The state of Texas was the pioneer of this movement, approving House Bill 1403 in July of 2001. The initiative came to be known as the 'Texas Dream Act' (henceforth, TDA). It granted a large reduction in

\(^1\)These include: Texas, California, Minnesota, New Mexico, Illinois, New York, Michigan, Washington, Oregon, New Jersey, Maryland, Rhode Island, Connecticut, Kansas, Colorado, Utah, Nebraska, and Oklahoma.
the cost of college attendance for undocumented students, moving them from out-of-state to in-state status in terms of tuition and fees. The complex institutional environment faced by the affected population, however, hinders our ability to predict the effect of this reform on educational attainment on theoretical grounds or by extrapolating elasticities estimated over different populations.

Undocumented high schoolers are forbidden from participating in the formal labor market. While the net expected return to higher education for this group is difficult to assess, it is likely to be lower than that of natives, and it may not be higher than the value starting to earn at an earlier age – even after the price reduction generated by the reform. On the other hand, college education has consumption value in and of itself for undocumented immigrants, not just due to its intrinsic value but also because academic institutions have been identified as a partial safe haven from the duress of lack of immigration status once adulthood has been reached, including a reduced risk of deportation (Gonzales, 2010). Thus, the effect of tuition equity reforms on the post-secondary educational attainment of undocumented immigrant students is theoretically ambiguous, and better studied empirically.

This paper contributes to this endeavor by estimating the effect of the Texas Dream Act on the post-secondary educational attainment of undocumented public high school students. To do this, I exploit administrative data from Texas public education agencies – the Texas Education Agency (TEA) and the Texas Higher Education Coordination Board (THECB). I estimate the effect of the reform using a generalized differences-in-differences research design, in which non-immigrant hispanic high schoolers serve as a comparison group for the group affected by the reform. The identification assumption in my estimation is that, in the absence of the reform, non-immigrant and undocumented students attending the same high school would have followed similar trends in post-secondary attainment had the reform not taken place.

One of the main challenges in studying the outcomes of undocumented immigrants is measurement. Indeed, the very notion of undocumented status suggests that this population is not easy to identify in data sources, administrative or survey-based, as these individuals may not appear in formal records or may have incentives to deny information requests during surveys. In my case, this problem is manifested by the inability to

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2The political climate is much more relevant for the formation of expectations for this segment of the population, given that adjustment of immigration status and/or deportation has a direct effect on their ability to earn in the U.S. Hence, policy volatility has a large effect on expected labor market returns.
directly link Dreamer tuition flags in college records to administrative high school records. Therefore, I construct a proxy indicator of undocumented immigrant status based on high school data, a simple function of a student level "immigrant" flag in the data which is used purely for administrative purposes by Texas agencies.

Given these limitations, I develop an indirect empirical test to validate my approach. I note the fact that TDA beneficiaries were obliged to sign affidavits promising to attain legal immigrant status as soon as possible. Individuals who signed such an affidavit are coded in the administrative data under a specific tuition flag. The tuition flag for TDA beneficiaries is a clean indicator for undocumented status among college enrollees. By correlating the undocumented high schooler proxy indicator rates in high schools with affidavit rates at nearby community colleges, I am able to partially assess whether the proxy is capturing the correct population. I show that this correlation is strong and positive, indicating that the proxy does a fair job at capturing the treated population. While this test is by definition imperfect – the correlation confounds college enrollment rates, an endogenous outcome, with the accuracy of the proxy – it is a rare instance in which one can assess the extent of measurement error in an empirical study on undocumented immigrants.

After finding the treated, I estimate the effects of the reform using administrative data from Texas high schools in a generalized differences-in-differences framework. Specifically, I exploit within-school, between-cohort variation to draw comparisons between the educational outcomes of undocumented high school students across cohorts, using the outcomes of their native classmates as a fine-grained control. I present evidence that although control group students are substantially different to the treated in levels, their similarity in trends validates the research design. My preferred estimates of the impact of the reform on college demand are regression-adjusted differences in college planning rates between immigrant and non-immigrant Hispanic students attending the same high school, across the pre and post reform periods.

The analysis points to the following conclusions. First, undocumented students are a severely disadvantaged group. They rank lower than non-immigrant Hispanics (a group already considered disadvantaged in Texas) in many educational outcomes such as high school graduation rates, standardized exam scores, and college demand. Second,

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3My proxy function flags students as potentially treated if they were flagged as immigrants by the agency in 3 consecutive years. See data documentation for more detail on this administrative flag.
differences-in-differences estimates show that the reform led to a 5 to 6 percentage point increase in college demand among undocumented high school graduates – as measured by the share of students responding positively to a post-graduation survey regarding their college plans. The magnitude of these estimates imply a closing of the college demand gap between undocumented students and non-immigrants by 90%. I present suggestive evidence that these demand effects translated to an increase in college enrollment and completion for the treated. However, constraints in the administrative data limit my ability to estimate such enrollment effects directly. Moreover, my estimates show that, in the pre-reform period, the treated and comparison group followed statistically parallel trends in college demand. This is evidence supporting the key identification assumption of the research design.

Finally, I estimate similar models testing for downstream spillover effects of the reform. In other words, its effects on college-bound investments among younger high schoolers, prior to high school graduation. The results show that, while there were some relative improvements in standardized test scores among undocumented ninth graders starting high school after TDA enactment, fundamental outcomes such as graduation and dropout suffered during this time period. I attribute these mixed results to the complex changes in the institutional environment of Texas public schools that coincided with the advent of TDA – namely, the enactment of the No Child Left Behind doctrine, which took place in the same year as the passage of TDA.

This paper is related to an emergent literature on the effect of tuition equity policy on the outcomes of immigrant students (Kaushal, 2008; Flores, 2010; Chin et al., 2010; Conger et al., 2015). Several of these studies employ U.S. census data to draw between-state comparisons between immigrant and native college enrollment rates. While these identification strategies are appealing based on their representative nature, the mixed conclusions drawn from these studies and the coarse nature of the immigrant indicator found in census data, have been a shortcoming of this class of methods. To my knowledge, I am providing the first analysis of the tuition equity reform movement based on high-quality administrative panel data from a public education state agency. The ability to observe a range of demographic variables as well as longitudinal variation in immigrant indicators, allows me to construct a reliable proxy to study this elusive population. Furthermore, the richness of the data allows me to study effects on outcomes related to college-bound investments during high school, the first time this is done in the literature, to my knowledge.
Most relevant to this study, Conger and Turner (2015) exploit administrative data from the City University of New York to estimate the effects of a temporary increase in tuition for undocumented students. Their identification strategy is straightforward and quite credible as they can observe undocumented status directly from college records, thereby avoiding altogether the measurement issues that have plagued the literature. Their estimates show large effects of tuition shocks on the re-enrollment and degree attainment of undocumented college students. I see the results of my analysis as complimentary to theirs. Their estimates are valid for a special sample of undocumented students, namely those that are already attending college. My analysis can be interpreted as estimating the effect of this class of policies during other stages of educational development – perhaps the more policy relevant ones: high schoolers and high school graduates. I estimate effects at the key juncture of high school and college, and also during earlier high school years, the period in which key investments are made toward post-secondary education. From the perspective of policymakers, I estimate whether tuition-equity reform generates positive externalities (i.e. effects beyond the direct price effects of tuition changes on college enrollment) by generating improvements in outcomes among undocumented youth prior to high school graduation.

The rest of the paper is organized as follows. Section two describes the historical and legislative background to the advent of TDA, as well as theoretical considerations regarding its potential effects. Section three presents my empirical strategy, including data sources, measurement, and causal inference. Section four presents the causal estimates and interprets the results. Finally, section five concludes with policy implications and motivation for future research on this topic.

3.2 Background and Theory

In the groundbreaking Plyler v. Doe decision in 1982, the U.S. Supreme Court struck down Texas law withholding state funds for educating children who had not been legally admitted to the United States, and authorized local school districts to deny enrollment to such students. The decision effectively established that all school districts in the nation must provide public K-12 education to undocumented immigrants. In the case’s majority opinion, Justice Brennan observed that denying the children in question a proper

\footnote{457 U.S. 202}
education would likely contribute to "the creation and perpetuation of a subclass of illiterates within our boundaries, surely adding to the problems and costs of unemployment, welfare, and crime."

However, in 1996 the United States Congress enacted the *Illegal Immigration Reform and Immigrant Responsibility Act*, which allowed states to pass statutes denying undocumented students from in-state tuition, financial aid, or even bar their enrollment in public colleges and universities all together.\(^5\) Therefore, federal law guaranteed the right to a free K-12 public education for undocumented immigrants, but access to post-secondary education was up to state discretion.

Political rhetoric toward young undocumented immigrants began to change in 2001, with the introduction of the *Development, Relief, and Education for Alien Minors Act* (DREAM Act). It proposed conditional residency to undocumented immigrants that met the following conditions: had arrived to the U.S. before the age of 16; had spent at least 5 years in the country; had no known criminal record; and had graduated from a U.S. high school. While never enacted into law, the DREAM Act influenced the immigration debate within lower levels of government. The same year, Texas was the first state in the nation to pass legislation which permitted immigrant students to access in-state tuition and state-provided financial aid, with the approval of House Bill 1403 (it would soon be followed by California and others). The provision targets any student that is a non-U.S.-citizen, but it was expected to have its greatest impact on the undocumented immigrant population (Brennan, 2001). The law became known as the *Texas Dream Act* (henceforth, TDA), alluding to the similarity between this legislation and its federal counterpart: TDA beneficiaries must have clean criminal record, as well as graduated from a Texas public high school, and resided in the state for at least one year. Additionally, and of significant importance for this study, beneficiaries were expected to sign a notarized affidavit in which the individual made a legal promise to file an application for permanent residence "at the earliest opportunity that [the student is] eligible to do so" (HB 1403, Texas, 2001).

TDA greatly reduced the prospective cost of attendance for undocumented high school students. Depending on the type of college, the difference between non-resident and resident tuition can range between 50% and 75% in Texas.\(^6\) Before the reforms, this

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\(^6\)Author’s own calculations employing the Texas Higher Education Coordinating Board’s "Overview..."
price difference, coupled with the lack of access to government financial aid and credit constraints in the private sector, effectively raised prohibitive barriers to higher education for undocumented students, who are known to be relatively impoverished (Card and Raphael, 2013; Belanger, 2001; Passel and Cohn, 2009). Standard economic theory would suggest that such a reform – a subsidy to higher education coupled with relaxation of credit constraints – would naturally lead to a rise in college demand and enrollment. However, the educational choice environment for young undocumented immigrants is more complex than that of native-born students.

Undocumented students face a complicated cost-benefit analysis when deciding whether to attend college. An undocumented high school graduate not only needs to take into account the relative value of work experience versus human capital attainment, but also the heightened risk of deportation associated with undocumented work; the wage penalty associated with undocumented status; the limited access to high skill occupations; and the value of school as a safe haven from deportation authorities. Indeed, on purely theoretical grounds, it is uncertain whether the undocumented have a positive expected return to higher education, assuming that they stay in the U.S. and their status remains constant. Hence, the question of whether the price incentives generated by the TDA reform had a significant effect on undocumented graduates’ demand for college is an empirical one.

Another policy-relevant aspect of the effects of TDA is whether it had any "spillover effects" on the achievement of undocumented students prior to the decision to attend college. The definition of spillovers here refers to the policymaker’s perspective. The authors of the reform were mainly concerned with college access for undocumented immigrants, yet it is possible that this reform affected the decisions of undocumented students down the educational ladder, by potentially incentivizing students to improve performance during high school, given the prospect of affordable college access. Indeed, an influential literature in structural labor economics assumes that students engage in a dynamic optimization problem when making education choices during high school (Keane and Wolpin, 1997). Hence, an interesting question in this setting is whether the prospective price reduction generated by the reform incentivized students to engage to a greater deal

\footnote{of Tution and Fees", available at http://www.thecb.state.tx.us/reports.}

\footnote{For instance, the Family Educational Rights and Privacy Act (FERPA) forbids the use of unauthorized immigrant’s school records by immigration authorities as evidence for deportation proceedings. This regulation is specially relevant during post-secondary education, when immigrant students are receiving financial aid that can be used for subsistence.}
in 'college-bound' investments during high school. These investments can materialize in a number of ways, including endogenous changes in the following observable high school outcomes: graduation, drop out, enrollment in advanced placement (AP) courses, enrollment in dual credit courses (i.e. courses that count toward a college degree), course passing rate, attendance rate, standardized exam scores, and discipline events.

The ideal empirical environment to evaluate a reform such as TDA would hold constant any other government initiatives that intend to change the public education system in any form. This condition is generally difficult to achieve in practice, and this case is no exception. Most prominently, TDA’s enactment year partially coincided with the enactment of the federal No Child Left Behind Act (NCLB) and an introduction of a more rigorous (and notably more difficult) standardized exam in the state, the Texas Assessment of Knowledge and Skills (TAKS). The NCLB Act required all schools in the nation to administer standardized testing to their students; track the progress of the different vulnerable student subgroups within the schools (immigrants was one of these categories). While Texas already has standardized testing when NCLB passed, the introduction of the TAKS was meant to ‘raise the bar’ in the high school exit exam, which students had to pass in order to graduate.

Given this policy environment and the simultaneity of other reforms, causal estimates of the effect of TDA must be interpreted with caution. On the one hand, one would not expect changes in the standardized testing environment to affect the college going decisions of individuals that already have a high school diploma, precisely the population that the authors of TDA were targeting. As they already possess a high school diploma, college pricing is a more salient topic for them. On the other hand, students that are still in high school face a number of policy changes that could themselves affect the college-bound investment behavior. It may difficult to fully attribute changes in this population’s behavior to TDA, but the exercise is still informative.

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8The TAKS replaced the TAAS in 2003. According to the TEA, the TAKS was meant to be more comprehensive and rigorous than its predecessor. See: http://tea.texas.gov/student.assessment/taks/
3.3 Empirical Strategy

Measurement

The primary challenge to studying the socioeconomic outcomes of undocumented immigrants is measurement. This group is popularly referred to as 'living in the shadows' given that most of these individuals leave a minimal paper trail. A majority of studies regarding this population rely on U.S. census questions which ask how long an individual has resided in the country, and whether the individual is an American citizen (Kaushal, 2008; Flores, 2010; Chin et al., 2010). While this serves as a nationally representative benchmark, it is likely that census-based statistics on this population are flawed. Indeed, it is possible that the trust that undocumented immigrants give to federal surveys, and hence the reliability of their responses, fluctuates with the rhetoric of the national political debate on immigration reform.

My analysis attempts to ameliorate measurement concerns by exploiting TEA administrative school data. Arguably, these records provide an improvement on the accuracy of information about student’s immigration status for several reasons. First, personal information in educational records is protected by FERPA. This is made apparent to families when requests for information are made, which may reduce response anxiety. Second, enrollment in certain school programs is contingent on immigrant status. Hence, it may often be the case that families have an incentive to provide accurate information. Third, and most importantly for this analysis, school records allow one to observe the longitudinal dimension of a student’s immigrant indicator variable. I exploit this feature of the data, along with TEA guidelines for recording the immigrant indicator, and the fact that legal immigrant status is something that seldom changes from year to year.

The TEA records an immigrant flag for all K-12 students attending Texas public schools. According to data documentation, this flag is meant to capture all students that are between 3 to 21 years of age, were born outside the U.S., and have not attended a U.S. school for 3 full academic years. FERPA regulation forbids schools from requesting information on the legal immigration status of students. An important caveat of this flag for the purposes of this study is that immigrant flags are meant to be turned on for at most 3 years. If one were to naively use these raw indicators as a proxy for treatment status, one would only capture students that arrived to U.S. during their the last few
years since observation. This would miss any eligible students that have been in the US system for longer than a few years, precisely the reforms target population. I take this caveat into account when constructing the treatment proxy.

I define the treated as any hispanic student that was flagged as an immigrant for 3 consecutive years, at any point during their enrollment in the TEA system.\footnote{We choose to focus on Hispanic students since they are by far the largest immigrant group in Texas and because our control group is motivated similarities in educational attainment between hispanic natives and their immigrant counterparts, see section 3.2.} This definition implicitly assumes that a student that was an eligible immigrant during elementary school is still eligible through-out the rest of her K-12 education. It is difficult to assess the degree of misclassification generated by this assumption. On the one hand, ignoring the longitudinal aspect would lead us to throw out immigrants that have been in the country for extended periods of time and lack status, a population that has been the focus of the debate in recent times. On the other, this simple definition rules out the possibility that immigrant students can gain legal residence status during their K-12 education.

One unfortunate but unsurprising limitation of the data, is the inability to link the treated to college records. Cross-agency record linkage is made using social security numbers, which undocumented immigrants do not have. Still, as mentioned in Section 2, all TDA beneficiaries submit a notarized affidavit promising to attain legal residence status at the earliest opportunity. This information is available in the THECB administrative records and provides one the cleanest measures of undocumented status, akin to the one used by Konger and Turner (2015). Since this flag is only available in college records during the post-reform period, it cannot be directly exploited to estimate the impact of the reform on high school and earlier outcomes. Nonetheless, it opens an opportunity to indirectly validate the proxy treatment indicator.

To operationalize this test, I note the fact that 2-year colleges typically enroll local high school graduates. I compute sending rates from high school graduation cohorts to 2-year colleges, calling the college with the highest sending rate, the college that is "linked" to this high school. The idea behind the validation test is: if the proxy measure is good at capturing the treated, then high school cohorts with high proxy-immigrant shares should be linked to community colleges with high affidavit shares of incoming enrollment. In other words, when looking at a cross-section of high schools, the coefficient of an OLS regression of the affidavit-enrollment share for the linked 2-year
college on the proxy immigrant-cohort share of the high school, should be positive and statistically significant.

Figure 1 shows a binned scatter plot providing a visual representation of this test. The vertical axis shows the affidavit-enrollment share at linked community colleges, while the horizontal axis measures the immigrant-cohort share of the high school. The conditional expectation function of affidavit shares conditional on proxy shares is clearly positively sloped and concave. This function conflates the mean measurement error in our proxy with the mean college enrollment rate of undocumented students. Thus, it is not possible to directly assess the degree of measurement error in the proxy from this figure alone.

Nonetheless, Figure 1 elucidates two empirical patterns. First, a positive first derivative of this function suggests that our undocumented proxy indicator is not only capturing noise, it is predictive of undocumented status in a real sense. Second, a negative second derivative of this conditional expectation function suggests that schools that have high shares of immigrants have lower college enrollment rates than those with low immigrant shares. This pattern makes sense in a context of inequality in education between high poverty schools (which are the school that undocumented immigrants typically attend, see Table 1) and affluent ones. Inequality in educational resources in this respect has been documented widely in the economics of education finance literature (Cascio and Reber, 2013; Lafortune et al. 2016; Card and Krueger, 1992).

Data and Sample Selection

The analysis draws on data from the Texas Education Agency (TEA) via the Texas Education Research Center. These longitudinal student records consist of school identifiers, demographic characteristics, attendance, graduation, disciplinary action, dropout, and standardized test scores for every K-12 student attending public school in Texas. I analyze the effect of the reform by comparing the outcomes between cohorts of students. Data is available for the 1994 – 2014 school years, but I focus on 1998 – 2006 cohorts for the following reasons. First, the treatment definition relies on the longitudinal aspect of enrollment records. I need to be able to look back on a students’ enrollment at least three years to tag her as potentially eligible TDA beneficiary. Weighing the tradeoff between being able to correctly classify more students and having sufficient pre-periods to assess the validity of the identification assumptions, I choose the first cohort in the analysis to
be those that either graduated to entered high school in 1998.

Second, the main outcome of interest, whether a student is planning to attend college, is only recorded in high school graduation files up to 2006. It should be noted that my estimates of the impact of the reform are applicable only to the population of immigrant students conditional of having graduated high school. This is not an innocuous restriction given that this population has particularly low graduation rates, and should be kept in mind in assessing the external validity of these estimates.

Additionally, the Texas Dream Act applied to all eligible students attending a Texas high school after 2001. The law was passed during the summer of 2001 and came into effect immediately. Therefore, the first school cohort to graduate or enter high school with the reform in place was 2002. This logic defines the treatment year for our differences-in-differences analysis. Hence, by letting 2006 cohort to be the last cohort in the analysis, which also restricts our attention to a relatively tight time period around the reform, we end up with four cohort pre and post of the reform to drive our analysis.

In such a policy context the most natural counterfactual group for causal inference is students that are ineligible to benefit from the reform but that are otherwise identical to eligible students. In Texas, undocumented immigrant students are predominantly from hispanic descent (Passel and Cohn, 2009, 2016). For this reason, I restrict the analysis sample to hispanic students only. This decision is also done in light of the academic gaps that exist prominently in Texas’ public schools – hispanic and black students perform considerably worse across the board than their white and asian counterparts. These high-performing groups would not be a good counterfactual for the treated. Moreover, the comparability of the treated and control groups can be further improved by focusing on comparisons within schools. This feature of the analysis is important if one assumes that are temporally-fixed relevant characteristics of school quality that are unobservable in administrative records. I now present a formal econometric statement of the research design motivated by this intuition.

Identification Strategy

I model the statistical relationship between the the reform and the educational outcomes of high schoolers by adopting a generalized differences-in-differences (DD) econometric framework. This empirical strategy is based on the idea that students that attend the
same high school share similar unobserved characteristics, making their classmates a useful counterfactual for immigrant students, after accounting for fixed differences between schools, common time effects, and a number of observable of characteristics. The assumption required for identification is further relaxed by restricting the analysis sample to Hispanic students. I first estimate the following standard parametric DD model:

\[ Y_{ist} = \varphi_s + \alpha_t + \delta D_i + \beta D_i \times Post_t + X'_{ist} \Gamma + \varepsilon_{ist} \]  

(3.1)

where \( Y_{ist} \) is an educational outcome for student \( i \), attending school \( s \), in year \( t \); \( D_i \) is the proxy immigrant indicator; \( Post_t \) is an indicator for years after 2002, the post-reform period; \( X_{ist} \) is a vector of time-varying student characteristics\(^{10} \); \( \varphi_s \) and \( \alpha_t \) are school and year fixed effects; and \( \varepsilon_{ist} \) is an idiosyncratic error component. The effect of the reform is thus captured by the \( \beta \) coefficient in this specification.

I also fit flexible differences-in-differences specifications that allow for a statistical test of the common-trends identification assumption, as well as the temporal evolution of the effects of the reform:

\[ Y_{ist} = \varphi_s + \alpha_t + \sum_{t'=1998}^{2006} \beta_{t'} (D_i \times I(t = t')) + X'_{ist} \Gamma + \varepsilon_{ist} \]  

(3.2)

In this case, I am interested in the \( \beta_t \) coefficients. If the common trend assumption holds, coefficients that correspond to the pre-reform period should not be statistically different from zero. The coefficients corresponding to the post reform period estimate the immediate effect of the reform and its evolution over time. The parametric estimate, \( \beta \), from specification (1) can be computed as a weighted average of the post-reform \( \beta_{t'} \)'s in specification (2).\(^{11} \)

I test two hypothesis using this econometric framework. First, I estimate the direct impact of the reform on the college demand of undocumented high school graduates. Second, I test the spillover impact of this price-reduction on a number of high school outcomes related to college-bound investment behavior during high school. I test for these effects using a sample of high school entering cohorts.

\(^{10}\)They include the following: exit scores in math and reading, gender, age, English language learner status, free or reduced priced lunch status, TWC match status, Spanish-speaking household, gifted status, at risk of dropping out status, and special education.

\(^{11}\)Abstracting from the role of covariates and fixed effects in the mode, the weights here would simply correspond to the relative size of each cohort in the sample.
The main identification assumption can be summarized as follows: conditional on observable characteristics, TDA-eligible and control group students in the same high school follow similar trends in educational outcomes. As is standard in the literature, one can partially test this assumption by observing pre-trends prior to the advent of the reform on the outcomes of the treatment and control groups. I devote considerable attention to this test in Section 4.

Besides the common-trend assumption, identification of causal effects is also threatened by other scenarios. First, there could be time-varying selection of immigrants into high school graduating cohorts: If, for instance, better-prepared immigrant students are increasingly likely to appear in graduating cohorts across time. This would lead to omitted variable bias in the specification with respect to unobservable school-cohort effects, essentially biasing the first difference in the DD estimation, leading us to overestimate the impact of the reform.

Another potential threat is misclassification error in the treatment proxy. It could be the case that the undocumented immigrant proxy is not very good at capturing the eligible population. This concern is ameliorated to the extent possible in section 3.1. Finally, the co-enactment of other education policies could lead me to wrongly attribute the effect of other policies to TDA. As mentioned above this is not a concern for the graduating-cohort regressions, as they are no longer affected by K-12 education policy, but it is a concern in the entering-cohort regressions. I come back to this issue in the next section.

3.4 Results

Table 1 presents the mean characteristics of the graduating cohort analysis sample, separate by treatment status, for the period before and after the enactment of the reform. Column (1) reports the mean characteristics of the treated before the reform. On average, about 56% of proxy-eligible graduates report planning to attend college. Additionally, given that undocumented students are known to be economically disadvantaged and Spanish speakers, a large share of those tagged as eligible by our proxy have participated in the English Language Learner (ELL), Free or Reduced Lunch programs (FRL) and report that Spanish is the main language spoken at home.
Furthermore, Columns (1) and (2) show that the eligible group is significantly more disadvantaged than the control group, with lower test scores, higher risk of dropout, and higher poverty rates than the control group. They also attended larger high schools that have higher proportions of minority and economically disadvantaged students. Columns (3) and (6) show that although the treated and control groups differ in a number of characteristics, the patterns of observable inequality between these groups hold to roughly the same extent in the pre and post reform periods.\textsuperscript{12} Finally, taking the difference between columns (6) and (3) for the college plans row gives us an unadjusted differences-in-differences estimate of the effect of the reform, about a 6 percentage point increase in college plans for the treated.

Table 2 reports similar summary statistics for the entering cohort analysis sample. It shows mean end-of-high school outcomes for entering high school students. Outcomes are measured four years after entering high school for the first time. These statistics make it clear that both the treated and control groups are significantly disadvantaged, with high school graduation rates of 54\% and 60\%, respectively. Moreover, it is evident that this group is considerably in a worse position academically than the treated graduating cohort summarized in Table 1. This is evident for instance when looking at exit exam scores in column (1) of Table 2. Comparing these mean scores to column (1) in Table 1, it is easy to see that although TDA-eligible graduates are below median performers, they are positively selected from the pool of all eligible high schoolers. This is perhaps not surprising, but such selection should be kept in mind when interpreting the causal estimates in the next section. Moreover, Table 2 shows mixed changes in academic performance for eligible high schoolers after the introduction of TDA. While the test score gaps were reduced over the reform period, there were considerable losses for the treated in terms of high school graduation and drop out. I explore these puzzling patterns to a more detailed extent in the next section.

One of the main outcomes of interest in this analysis is whether a high school graduate is planning to attend college. This flag is recorded in the TEA’s high school graduation files. Given that it is not directly linked to actual college enrollment, a relevant question to ask is to what extent, if any, the college plans variable predicts actual enrollment. Unfortunately, due to data constraints, we cannot directly test the effect of the reform.

\textsuperscript{12}See Appendix Figure A3 for a visual evaluation of the similarity of trends in covariates between the control and treated group.
on college enrollment.\footnote{In Texas, there are no universal student ID numbers that are used both for K-12 records (TEA) and college records (Texas higher education coordinating board). The Texas ERC links data across these government agencies via scrambled social security numbers. However, since undocumented students don’t typically have SSN’s, this crosswalk does not function for this population and hence we cannot observe college enrollment for the TDA-eligible population.}

However, I can observe college enrollment for the control group. For this group I can ask: do college plans predict actual college enrollment? One concern with such exercise is that the control group has significantly different observable characteristics from the treated (in levels, but not trends). Hence, the correlation between college plans and enrollment may not be comparable between these two groups. With this issue in mind, I balance out the control group in terms of observables using a propensity score re-weighting exercise using the individual and school characteristics reported in Table 1. I then regress a college enrollment dummy on a college plans dummy for the propensity score re-weighted control group.

Table 3 presents the results. Columns (1) and (2) report the OLS coefficients when the outcome is enrollment in any higher education institution. Note that the OLS coefficient is positive and highly statistically significant whether we control for a variety of observable and unobservables or not. I obtain a similar result when focusing in on with 2-year colleges (columns (3) and (4)) and 4-year colleges (columns (5) and (6)). These results suggest that, after making the control group ‘look’ like the treated in terms of observables, planning to attend college is associated with a 16 to 22% increase in the probability of actually attending college.\footnote{Moreover, if TDA-eligible students have better unobservables than the control group (e.g. more intrinsic motivation), these figures should be interpreted as a lower bound of the association between college plans and college enrollment.} These figures are suggestive that college plans is a relevant variable for evaluating the effect of TDA. Therefore, I interpret college plans as a measure of college demand and use the terms interchangeably.

**College Demand among HS Grads**

I now present the main result of this study: causal estimates of the effect of TDA on graduating cohorts’ college demand. Table 4 shows estimates of the parametric DD model in equation (1). Column (1) of table (4) shows estimates for a basic DD model with no controls or fixed effects of any kind. I call these "raw" DD estimates. The first
thing to note is that there is a statistically significant pre-period gap in college plans between eligible graduates and control ones, of about 5.92%. Second, note that the raw DD estimate of the effect of the reform, a 5.88% increase in college plans for the treated, amounts to a complete closing of the gap.

Columns (1) through (4) gradually introduce a series control variables and fixed effects to this specification in order to assess the sensitivity of the estimates to omitted variable bias. Note that the coefficient of interest is quite stable across specifications, decreasing only slightly as I introduce controls. My preferred estimate of the effect of the reform is in column (4). When I control for school and cohort fixed effects, as well as a number of observables, I estimate that TDA caused a 5.27% increase in college demand. This estimate is statistically significant at the one percent level. Column (5) controls for treatment-group-specific time trends. The results are essentially unchanged, but given the statistical balance in trends (see Figure 2) we find this specification to be unnecessary for the validity of our causal estimates.

Figure 2 plots these results using OLS estimates of the non-parametric DD specification in equation (2). Each point on the graph corresponds to the coefficient on a treatment dummy with cohort year interactions, where the 2001 graduating cohort is the omitted category. The coefficients corresponding to cohorts from earlier than 2002 afford a statistical test for the parallel trend assumption central to the identification of these type of DD models. Note that the pre-treatment difference between the groups bounces around zero and that these coefficients are never statistically different from zero. I interpret this as the parallel trend assumption being satisfied.

Furthermore, the post-treatment coefficients in Figure 2 show an immediate effect of the reform which is stable across cohorts. The college demand gap between immigrants and natives seems to be closed immediately and permanently due to the reform. These results are reassuring that TDA had a significant effect on the human capital of the undocumented high school graduate population, providing the main result of the paper.

Another policy-relevant aspect of this reform is to identify precisely which type of eligible students became the beneficiaries of TDA. I approach this question in two ways. First, I perform the standard treatment effect heterogeneity analysis. Essentially, this method amounts to asking whether the average treatment effect of the reform was statistically different for different subgroups within the treated. Second, I employ a characteristics of the compliers analysis as developed in Card and Giuliano (2015). This method
estimates the mean characteristics of those that were induced by the reform to plan to attend college, by differencing out the characteristics of eligible students that would have positive college demand regardless of the introduction of TDA.

Table 5 presents estimates of treatment effect heterogeneity specifications for the parametric DD model (equation (1)). Each column corresponds to a different characteristic being tested for heterogeneity. Column (1) tests whether the effect was different across genders, which does not appear to be the case, given the coefficient on the interaction corresponds to less than a 1% difference in mean effects. The same pattern holds for most of the characteristics tested, with some notable exceptions. First, column (3), whether a student’s exit reading score is above the state-wide median. I find negative treatment effect heterogeneity for this group, that is, the law had a smaller effect for eligible individuals with high reading scores. This is most likely due to an ‘always-taker’ type of phenomenon; these students would have planned to attend college anyway.

Second, I find treatment effect heterogeneity depending on ELL program participation during high school, as shown in column (5) of Table 5. Students with this characteristic have a much higher average treatment effect. I interpret this as evidence that the reform mainly empowered fringe immigrant student groups that would have not, without the advent of TDA, planned to attend college. Finally, schools that have below median test scores and those that have above median immigrant shares saw larger increases in college demand due to introduction of TDA. This is consistent with the previous results, namely that the main beneficiaries of TDA were some of the most disadvantaged students in the state.

Next, I estimate the mean characteristics of the complier group. The motivation for this exercise is similar to that in Table 5, but it is of a different nature. The estimates in Table 5 answer the question: did different subgroups respond more or less strongly to the reform? The current exercise asks a fundamentally different question. It tells us what type of student on average is planning to attend college due entirely to the introduction of TDA. In essence, it characterizes the average demographics of the students that are driving the average treatment effect that we estimated in Table 4 and Figure 2.

The results of this exercise are shown in Table 6. Estimates suggest that the complier group is about 80% female, 89% economically disadvantaged, and 27% English learner. Furthermore, on average the compliers are not high performing in standardized exams, with only 53% and 26% performing above median in the math and reading exit exams,
respectively. Finally, the average complier student has 75% probability of graduating from a high school with an above state-wide median graduation rate. These estimates paint a telling picture: beneficiaries tend to be socioeconomically disadvantaged females attending relatively well performing high schools.

**College-Bound Investments**

The above section shows that TDA closed the gap in college demand between undocumented and native high school graduates. The strength of these effects motivates another policy-relevant question. Do the tuition reductions generated by TDA create dynamic incentives for students still attending high school? If this is the case, the benefits of tuition equity reform would be noticed at education levels prior to the college-entry, providing a stronger reasoning for policy makers across the country to adopt such a policy. I test this hypothesis by estimating DD models around the reform for ninth grade cohorts. The outcome variables in these regressions are high school outcomes related to college-bound investments, including: exit exam scores, high school graduation, dropout, course-passing rate, attendance rate, discipline event rate, number of credits attempted, advanced placement (AP) courses attempted, and dual credit courses attempted.\(^\text{15}\)

Table 7 and Figure 3 present estimates from the parametric and non-parametric DD models, respectively. Notable immediately is that the raw parametric model estimates are negative and statistically significant almost across the board of our college investment outcomes, with the exception of exit exam scores. Taking these results at face value would lead us to conclude that TDA reduced college-bound investments for undocumented high schoolers, a puzzling conclusion given the strong positive results in the previous section. However, taking a look at Figure 3, I find that the story is more complicated. Each of the graphs in Figure 3 corresponds to covariate-adjusted flexible DD estimates for a different outcome. Panels (1) and (2) report results for exit scores. We can see that there are some pre-trends for these outcomes, and that the positive results for test scores in Table 7 are driven by a weighted combination of negative and positive differences in the post-reform cohorts. In Table 8, I show that parametric DD estimates controlling for a range of covariates, time trends, and high school effects makes some of the negative effects

\(^{15}\)In the Texas education system, AP courses and dual credit courses can be redeemed as college credit toward a degree if certain conditions are met. Hence, students that enroll in these are typically in preparation for enrolling in college.
become insignificant, but many counterintuitive patterns remain in these estimates.

More concerning perhaps are the results in panels (3) and (4) of Figure 3. These show considerable losses in high school graduation and dropout for the treated. It is difficult to attribute these results to pre-trends in the data, as there is hardly any evidence of these for high school graduation, but perhaps so for drop out. A similar pattern holds when we look at panels (5) through (8). Finally, panels (9) and (10) show that dual-credit and AP course enrollment are too rare of an event for our models to be able to pick out any effects that are statistically significant.

I argue that the negative results observed in this section are due to simultaneous policy introductions that coincided with the enactment of TDA. First, the No Child Left Behind Act was enacted in early 2002. NCLB introduced a number of standard that schools needed to maintain in order to keep the autonomy in their leadership, which gave the NCLB provisions bite. A relevant part of the NCLB provisions was that performance standards need to be achieved within subgroups of students considered vulnerable. Immigrant students were one of these categories, prompting school staff to shift their policy for addressing these students. This change in school behavior toward the eligible group coincided with the enactment of TDA. NCLB effects on high school outcomes of the undocumented might have been positive or negative. This has been a point of contention for academics that have studied the effects of this reform (Deming et al., 2013; Ahn and Vigdor, 2014; Dee and Jacob 2009).

Second, Texas dramatically changed its standardized testing system in early 2003, when the TAKS replaced the TAAS. The new exam was meant to be considerably more difficult than its predecessor. In addition, unlike the TAAS, the TAKS could not be taken in Spanish. This fact could explain a differential effect of the introduction of this system on the TDA-eligible population, thus confounding our analysis.

The presence of these simultaneous policy introductions contaminate the treatment-post variable in the DD model, and compound the effect of all the reforms in my estimates. There is little to do to control for the effect of these policies. Due to this complication I conclude that this particular policy environment is too complex to elucidate whether tuition equity reform would have a trickle-down-type of effects to lower education levels, at least in a differences-in-differences framework. It should be noted that these econometric issues are not as much of a concern in the graduate cohort analysis in the previous section. Even though some of the later graduate cohorts were still in high school when
CHAPTER 3. EVALUATING THE TEXAS DREAM ACT

this package of policies was introduced, by conditioning on high school graduation (an important outcome), I eliminate most concerns of contamination.

3.5 Conclusion

A longstanding concern in the debate about undocumented immigration is the adjustment of status of minors who have attended U.S. public schools from a young age. Of special interest to education policymakers is whether the observed gaps in educational attainment between undocumented immigrant students and their native counterparts is due to differences in cognitive development or to the institutional constraints associated with lack of immigration status. The introduction of tuition equity reforms across different states in the nation provide a natural experiment to test these theories against each other. In light of this, an emerging literature has analyzed the effect of tuition equity reform on the educational outcomes of immigrant populations. These papers have generally employed survey-based data to estimate the impacts of the reform on college enrollment, with mixed results.

I contribute to this literature by providing the first estimates of the impact of tuition equity laws exploiting administrative data from K-12 public education agencies in Texas, which pioneered this legislation in 2001. These data allowed me to accurately zoom-in to the population that policymakers likely had in mind when drafting the reform: undocumented high school graduates. Moreover, motivated by the notion that college preparation starts well before high school graduation, I tested for effects on college-bound investments for high school entering cohorts. As opposed to previous work, I developed an empirical exercise providing evidence that the treated group in my study was indeed composed of undocumented immigrants, the group that benefitted from the reform. I then estimated generalized differences-in-differences models comparing the outcomes of eligible students to those of non-immigrant hispanics attending the same high school, a natural control group in this setting.

My preferred estimates show that the reform led to a 5 percentage point increase in college demand for eligible graduates. This increase represents a shrinking of the gap in college demand between natives and immigrants of about 90%, in essence closing the gap entirely. Further analysis shows that those induced by the reform were predominantly from academically disadvantaged backgrounds attending schools with high poverty rates.
On the other hand, estimates are mixed with respect to effects on college-bound investments. I attribute this to the complex policy environment that prevailed in K-12 schools during this time period, with the introduction of several initiatives targeting the performance of student subgroups, including immigrants. These concerns are not an issue for the first set of estimates, as they condition on high school graduation.

This analysis is subject to two important limitations, both related to the difficulty of studying undocumented immigration. First, I am unable to directly test the effect of the reform on college enrollment. Due to the manner in which identification systems work in education agencies it is impossible link the treated population to college enrollment records. Second, the treated will always be classified with measurement error. Even though my validation exercise shows that the treatment proxy is not off the mark, it still has measurement error, whose extent is difficult assess.

I conclude that although undocumented immigrant students are a severely disadvantaged group, policies aimed at relaxing the constraints associated with lack of immigration status can have a significant impact on their assimilation and attainment. These results are difficult to reconcile with a negative selection theory of immigration. Nonetheless, these findings apply only to those at the juncture of graduating high school and choosing whether to enter college, not an innocuous restriction in this setting. I hope further research will elucidate whether policies aimed at modifying undocumented immigration restrictions at other stages of the life cycle have an effect on relevant outcomes not just related to education, but also to income, remittances, and tax contributions.

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16Education agencies in Texas use anonymized (‘scrambled’) social security numbers to generate cross-agency student ID’s. The fact that unauthorized immigrants seldom have this document generates this problem.
Figure 3.1: Validation of Undocumented Immigrant Proxy

Note: Binned scatter plot with 30 quantiles. Sample is a cross section of all public high schools in Texas, linked to nearby 2-year colleges. The horizontal axis measures the share of a high school cohort that is tagged as an undocumented immigrant using our proxy, while the vertical axis measures the share of Texas Dream Act beneficiaries in the incoming freshman cohort at 2-year colleges located near the high school in question. The figure also shows a Lowess regression fit to the underlying micro data. It also reports the OLS coefficient and robust standard error of a simple univariate regression on these variables. The coefficient is positive and statistically significant.
Figure 3.2: Non-parametric DD estimates - Effect of TDA on Graduating Cohorts’ College Plans

Note: Figure presents non-parametric DD estimates computed via OLS as presented in equation (2). The blue line denotes to estimated coefficients, while the dotted gray line denotes the corresponding 95% confidence intervals of these estimates. The model controls for school fixed effects, cohort fixed effects, and the following observable characteristics: exit scores in math and reading, gender, age, ELL status, FRL status, TWC match status, Spanish-speaking household, gifted status, at risk of dropping out status, and special education. The estimation sample is the universe of hispanic high school graduates from Texas for the years 1998-2006. Standard errors are clustered at the high school level.
Figure 3.3: Non-parametric DD estimates - Effect of Reform on College-Bound Investments.

Note: Figure presents estimated OLS coefficients on treatment-cohort interactions as presented in equation (2), for a range of high school outcomes related to college-bound investments. The blue line denotes to estimated coefficients, while the dotted gray line denotes the corresponding 95% confidence intervals of these estimates. The model controls for school fixed effects, cohort fixed effects, and the following observable characteristics: exit scores in math and reading, gender, age, ELL status, FRL status, TWC match status, Spanish-speaking household, gifted status, at risk of dropping out status, and special education. The estimation sample is the universe of hispanic entering cohorts in Texas, for the years 1998-2006. Standard errors are clustered at the high school level.
Table 3.1: Summary Statistics - Graduating High School Cohorts

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Note: Mean characteristics of graduating cohort sample reported. Sample consists of hispanic students that have just graduated from a public high school in Texas. The immigrant group corresponds to those flagged by our proxy as eligible to be TDA beneficiaries. The control group corresponds to all hispanic students that are never flagged as an immigrant in the TEA records. The pre-reform period corresponds to the 1998-2001 graduating cohorts, and the post period to the 2002-2006 cohorts. The ELL and FRL indicators are measured longitudinally, that is they indicate whether the student has ever been in these programs.
Table 3.2: Summary Statistics - Entering High School Cohorts

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<td>-0.03</td>
<td>0.04</td>
<td>0.07</td>
<td>-0.03</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Special Education</td>
<td>0.04</td>
<td>0.13</td>
<td>-0.09</td>
<td>0.05</td>
<td>0.13</td>
<td>-0.07</td>
<td></td>
<td></td>
</tr>
<tr>
<td>At Risk</td>
<td>0.55</td>
<td>0.52</td>
<td>0.03</td>
<td>0.77</td>
<td>0.57</td>
<td>0.20</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Spanish Spoken at Home</td>
<td>0.89</td>
<td>0.38</td>
<td>0.52</td>
<td>0.90</td>
<td>0.44</td>
<td>0.46</td>
<td></td>
<td></td>
</tr>
<tr>
<td>ELL</td>
<td>0.96</td>
<td>0.41</td>
<td>0.55</td>
<td>0.99</td>
<td>0.52</td>
<td>0.46</td>
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<tr>
<td>FRL</td>
<td>0.96</td>
<td>0.86</td>
<td>0.10</td>
<td>0.97</td>
<td>0.90</td>
<td>0.08</td>
<td></td>
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</tr>
<tr>
<td>G8 Math Score</td>
<td>-0.49</td>
<td>-0.27</td>
<td>-0.22</td>
<td>-0.44</td>
<td>-0.25</td>
<td>-0.18</td>
<td></td>
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<tr>
<td>G8 Reading Score</td>
<td>-0.75</td>
<td>-0.31</td>
<td>-0.44</td>
<td>-0.69</td>
<td>-0.26</td>
<td>-0.43</td>
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<td></td>
</tr>
<tr>
<td><strong>School Characteristics</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cohort Size</td>
<td>585.84</td>
<td>514.49</td>
<td>71.36</td>
<td>584.23</td>
<td>525.39</td>
<td>58.84</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share FRL</td>
<td>0.64</td>
<td>0.55</td>
<td>0.08</td>
<td>0.67</td>
<td>0.60</td>
<td>0.07</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share Gifted</td>
<td>0.10</td>
<td>0.10</td>
<td>0.00</td>
<td>0.09</td>
<td>0.09</td>
<td>-0.00</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share Minority</td>
<td>0.83</td>
<td>0.72</td>
<td>0.11</td>
<td>0.82</td>
<td>0.74</td>
<td>0.09</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share Immigrant</td>
<td>0.09</td>
<td>0.03</td>
<td>0.05</td>
<td>0.11</td>
<td>0.05</td>
<td>0.05</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Observations: 20887 415996 436883 49509 613388 662897

Note: Mean characteristics of entering cohort sample reported. That is, those that were in 9th grade for the first time that year. Sample consists of hispanic students alone. The immigrant group corresponds to those flagged by our proxy as eligible to be TDA beneficiaries. The control group corresponds to all hispanic students that are never flagged as an immigrant in the TEA records. The pre-reform period corresponds to the 1998-2001 entering cohorts, and the post period to the 2002-2006 cohorts. The ELL and FRL indicators are measured longitudinally, that is they indicate whether the student has ever been in these programs.
Table 3.3: Correlation between College Plans and College Enrollment – P-score Re-weighted Control Group

<table>
<thead>
<tr>
<th></th>
<th>Any College</th>
<th>2-year College</th>
<th>4-yr College</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>College Plans</td>
<td>0.224***</td>
<td>0.165***</td>
<td>0.102***</td>
</tr>
<tr>
<td></td>
<td>(0.0123)</td>
<td>(0.00903)</td>
<td>(0.00738)</td>
</tr>
<tr>
<td>N</td>
<td>617427</td>
<td>617427</td>
<td>617427</td>
</tr>
<tr>
<td>adj. $R^2$</td>
<td>0.056</td>
<td>0.204</td>
<td>0.030</td>
</tr>
<tr>
<td>Covariates</td>
<td>✔</td>
<td>✔</td>
<td>✔</td>
</tr>
<tr>
<td>Year FE</td>
<td>✔</td>
<td>✔</td>
<td>✔</td>
</tr>
<tr>
<td>Campus FE</td>
<td>✔</td>
<td>✔</td>
<td>✔</td>
</tr>
</tbody>
</table>

Note: Standard errors are clustered at the high school level. The estimation sample is the control group, restricting to hispanic graduates in the 1998-2006 graduating cohorts. Observations are re-weighted to match the observable characteristics of undocumented immigrant graduates, using the standard propensity score re-weighting methodology. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. 


Table 3.4: Parametric DD Estimates – Effect of Reform on Graduating Cohorts’ College Plans

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post</td>
<td>0.0695***</td>
<td>0.0630***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.00911)</td>
<td>(0.00941)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Immigrant</td>
<td>-0.0592***</td>
<td>-0.0124</td>
<td>-0.0137</td>
<td>-0.0321**</td>
<td>-0.0290</td>
</tr>
<tr>
<td></td>
<td>(0.0192)</td>
<td>(0.0170)</td>
<td>(0.0171)</td>
<td>(0.0128)</td>
<td>(0.0188)</td>
</tr>
<tr>
<td>Immigrant × Post</td>
<td>0.0588***</td>
<td>0.0441**</td>
<td>0.0449**</td>
<td>0.0527***</td>
<td>0.0578***</td>
</tr>
<tr>
<td></td>
<td>(0.0194)</td>
<td>(0.0185)</td>
<td>(0.0185)</td>
<td>(0.0157)</td>
<td>(0.0177)</td>
</tr>
<tr>
<td>N</td>
<td>647091</td>
<td>647091</td>
<td>647091</td>
<td>647091</td>
<td>647091</td>
</tr>
<tr>
<td>adj. $R^2$</td>
<td>0.006</td>
<td>0.098</td>
<td>0.099</td>
<td>0.088</td>
<td>0.088</td>
</tr>
<tr>
<td>Covariates</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td></td>
</tr>
<tr>
<td>Year FE</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td></td>
</tr>
<tr>
<td>Campus FE</td>
<td>✓</td>
<td>✓</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Group Trends</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>✓</td>
</tr>
</tbody>
</table>

Note: Standard errors are clustered at the high school level. Table shows standard differences-in-differences estimates of the effect of the reform on the treated group’s plans to attend college. Covariates include: Exit scores in math and reading, gender, age, ELL status (ever and currently), FRL status (ever and currently), TWC match status, Spanish-speaking household, Gifted status, at risk status, and special education. *p < 0.10, **p < 0.05, ***p < 0.01.
Table 3.5: Parametric DD Estimates – Heterogeneity of Treatment Effect on College Plans.

<table>
<thead>
<tr>
<th>Individual Characteristics</th>
<th>School Characteristics</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) Female &gt;p50 Math &gt;p50 Reading FRL ELL At Risk Gifted</td>
<td>(8) &gt;p50 Grad Rate</td>
</tr>
<tr>
<td>X</td>
<td>0.0809***</td>
</tr>
<tr>
<td></td>
<td>(0.00433)</td>
</tr>
<tr>
<td>Immigrant</td>
<td>-0.0831***</td>
</tr>
<tr>
<td></td>
<td>(0.0212)</td>
</tr>
<tr>
<td>Immigrant × X</td>
<td>0.000535</td>
</tr>
<tr>
<td></td>
<td>(0.0120)</td>
</tr>
<tr>
<td>Immigrant × Post</td>
<td>0.0603***</td>
</tr>
<tr>
<td></td>
<td>(0.0209)</td>
</tr>
<tr>
<td>Imm. × Post × X</td>
<td>0.00498</td>
</tr>
<tr>
<td></td>
<td>(0.0130)</td>
</tr>
</tbody>
</table>

N | 647091 | 550897 | 535873 | 647091 | 647091 | 647091 | 647091 | 608440 | 647091 |
adj. R² | 0.016 | 0.031 | 0.031 | 0.011 | 0.016 | 0.030 | 0.019 | 0.009 | 0.009 |
Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
Campus FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
Group Trends | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

Note: Standard errors are clustered at the high school level. The estimation sample is the universe of Hispanic high school graduating cohorts 1998-2006. The title of each column denotes the trait which the specification is testing for treatment effect heterogeneity. The 'X's in the variable titles correspond to an indicator variable for said trait. *p < 0.10, **p < 0.05, ***p < 0.01.
Table 3.6: Characteristics of the Compliers - College Plans

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Individual Characteristics</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>0.78</td>
<td>(0.17)</td>
</tr>
<tr>
<td>&gt;p50 Exit Math Score</td>
<td>0.53</td>
<td>(0.20)</td>
</tr>
<tr>
<td>&gt;p50 Exit Reading Score</td>
<td>0.26</td>
<td>(0.19)</td>
</tr>
<tr>
<td>FRL</td>
<td>0.89</td>
<td>(0.18)</td>
</tr>
<tr>
<td>ELL</td>
<td>0.27</td>
<td>(0.19)</td>
</tr>
<tr>
<td>Gifted</td>
<td>-0.185</td>
<td>(0.13)</td>
</tr>
<tr>
<td><strong>School Characteristics</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>&gt;p50 Graduation Rate</td>
<td>0.74</td>
<td>(0.39)</td>
</tr>
</tbody>
</table>

Note: Standard errors are clustered at the high school level. The estimation sample is the universe of Hispanic high school graduating cohorts 1998-2006. Two-stage-least squares estimates of the characteristics of the complier group. Specifically, we estimate a 2SLS model in which the exogenous instrument in the post-treat indicator, and the outcome of interest is the interaction between treatment and the reported covariate. The endogenous variable is a treatment dummy. In addition, we control for group-specific trends in this specification. *p < 0.10, **p < 0.05, ***p < 0.01.
Table 3.7: Parametric DD Estimates – Effect of Reform on College-bound Investments – Raw Effects

<table>
<thead>
<tr>
<th></th>
<th>(1) Graduation</th>
<th>(2) Dropout</th>
<th>(3) Credits</th>
<th>(4) AP Courses</th>
<th>(5) Dual Credit</th>
<th>(6) Course-Passing Rate</th>
<th>(7) Exit Math Score</th>
<th>(8) Exit Reading Score</th>
<th>(9) Attendance Rate</th>
<th>(10) Discipline Event</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post</td>
<td>0.00289</td>
<td>0.0536**</td>
<td>1.539***</td>
<td>0.415**</td>
<td>0.0917***</td>
<td>0.0223***</td>
<td>-0.00220</td>
<td>0.0627***</td>
<td>0.00642***</td>
<td>0.148***</td>
</tr>
<tr>
<td></td>
<td>(0.00284)</td>
<td>(0.00203)</td>
<td>(0.149)</td>
<td>(0.0198)</td>
<td>(0.0316)</td>
<td>(0.00192)</td>
<td>(0.00896)</td>
<td>(0.00767)</td>
<td>(0.00678)</td>
<td>(0.00450)</td>
</tr>
<tr>
<td>Immigrant</td>
<td>-0.0551***</td>
<td>0.00645</td>
<td>2.164***</td>
<td>-0.149***</td>
<td>-0.0624***</td>
<td>0.0864***</td>
<td>-0.282***</td>
<td>-0.751***</td>
<td>0.0156***</td>
<td>-0.0653***</td>
</tr>
<tr>
<td></td>
<td>(0.00648)</td>
<td>(0.00403)</td>
<td>(0.294)</td>
<td>(0.0291)</td>
<td>(0.0155)</td>
<td>(0.00299)</td>
<td>(0.0208)</td>
<td>(0.0295)</td>
<td>(0.00128)</td>
<td>(0.00835)</td>
</tr>
<tr>
<td>Imm. × Post</td>
<td>-0.0269***</td>
<td>0.0210***</td>
<td>-1.419***</td>
<td>-0.0314</td>
<td>-0.0403***</td>
<td>-0.0130***</td>
<td>0.0882***</td>
<td>0.185***</td>
<td>-0.0048***</td>
<td>0.0325***</td>
</tr>
<tr>
<td></td>
<td>(0.00581)</td>
<td>(0.00414)</td>
<td>(0.250)</td>
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<td>(0.0188)</td>
<td>(0.00299)</td>
<td>(0.0184)</td>
<td>(0.0254)</td>
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<td>1099780</td>
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<td>1086130</td>
<td>1086130</td>
<td>786675</td>
<td>774269</td>
<td>1099089</td>
<td>1099780</td>
<td></td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.001</td>
<td>0.007</td>
<td>0.002</td>
<td>0.004</td>
<td>0.002</td>
<td>0.003</td>
<td>0.020</td>
<td>0.002</td>
<td>0.022</td>
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</tr>
</tbody>
</table>

Note: Standard errors are clustered at the high school level. The estimation sample is the universe of Hispanic 9th grade entering cohorts 1998-2006. The immigrant group corresponds to those flagged by our proxy as eligible to be TDA beneficiaries (see section 3.1). *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. 
Table 3.8: Parametric DD Estimates – Effect of Reform on College-bound Investments – Adjusted Effects

<table>
<thead>
<tr>
<th></th>
<th>(1) Graduation</th>
<th>(2) Dropout</th>
<th>(3) Credits</th>
<th>(4) AP Courses</th>
<th>(5) Dual Credit</th>
<th>(6) Course-Passing Rate</th>
<th>(7) Exit Math Score</th>
<th>(8) Exit Reading Score</th>
<th>(9) Attendance Rate</th>
<th>(10) Discipline Event</th>
</tr>
</thead>
<tbody>
<tr>
<td>Immigrant</td>
<td>0.123**</td>
<td>-0.00944**</td>
<td>8.419**</td>
<td>0.070**</td>
<td>0.0237</td>
<td>0.0963**</td>
<td>0.0910**</td>
<td>-0.0623**</td>
<td>0.0258**</td>
<td>-0.0439**</td>
</tr>
<tr>
<td></td>
<td>(0.00774)</td>
<td>(0.00417)</td>
<td>(0.364)</td>
<td>(0.0299)</td>
<td>(0.0189)</td>
<td>(0.00394)</td>
<td>(0.0153)</td>
<td>(0.0189)</td>
<td>(0.00136)</td>
<td>(0.00712)</td>
</tr>
<tr>
<td>Imm. × Post</td>
<td>0.0123</td>
<td>0.0373**</td>
<td>2.345***</td>
<td>0.0177</td>
<td>-0.00197</td>
<td>0.0202**</td>
<td>0.0742**</td>
<td>-0.00406</td>
<td>0.0101***</td>
<td>-0.0347***</td>
</tr>
<tr>
<td></td>
<td>(0.00923)</td>
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<td>(0.349)</td>
<td>(0.0422)</td>
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<td>(0.00385)</td>
<td>(0.0228)</td>
<td>(0.0253)</td>
<td>(0.00150)</td>
<td>(0.00759)</td>
</tr>
<tr>
<td>Observations</td>
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<td>10,973,800</td>
<td>10,863,390</td>
<td>10,863,390</td>
<td>10,863,390</td>
<td>7,860,735</td>
<td>77,426</td>
<td>10,999,890</td>
<td>10,997,800</td>
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</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.305</td>
<td>0.062</td>
<td>0.259</td>
<td>0.242</td>
<td>0.068</td>
<td>0.206</td>
<td>0.493</td>
<td>0.482</td>
<td>0.097</td>
<td>0.125</td>
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<td>✓</td>
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<td>✓</td>
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<td>✓</td>
</tr>
<tr>
<td>Year FE</td>
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<td>✓</td>
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<tr>
<td>Group Trends</td>
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</tr>
</tbody>
</table>

Note: Standard errors are clustered at the high school level. The estimation sample is the universe of Hispanic 9th grade entering cohorts 1998-2006. The immigrant group corresponds to those flagged by our proxy as eligible to be TDA beneficiaries (see section 3.1). *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. 
Bibliography


Appendix A

Chapter 1: Data Construction and Minimum Distance School Attendance Boundaries

Data Construction

First and foremost, I make use of the Student Attendance Boundary Survey (SABS) produced by the National Center Of Education Statistics (NCES) at the Depart of Education. This survey was the first attempt to collect data on school attendance boundaries of all districts in the U.S.\textsuperscript{1} The SABS data were collected over a web-based self-reporting system, through e-mail, and mailed paper maps. The creators then harmonized these different data types into GIS shape files, which greatly facilitates a systematic analysis. The universe of school districts is defined as those included in the Census Bureau’s SY 2013-14 School District Review Program and the Common Core of Data, specifically those that are denominated as a regular school district and has at least one school open during the school year. This frame resulted in more than 5,000 school districts eligible to participate in SABS. The survey collected data on attendance boundaries for all K-12 grade levels.

I measure the spatial distribution of racial composition in school district geography using census block-level race by age tabulations from the 2010 Census in combination with the 2010 census block GIS shape files produced by TIGER/Line. Total population, age, gender, and race are the only variables that the census makes available to the public at the census block level, but this suffices to measure segregation at a remarkably fine

\textsuperscript{1}This also means that there is limited panel data for SAZs with the exception of pilot and SABINS.
APPENDIX A. SCHOOL ATTENDANCE BOUNDARY POLICY

spatial level. In urban settings, census blocks’ area correspond to city blocks, while in rural areas they have larger size.

By laying the SABS geographic data over the census block mapping of a LEA jurisdiction, I can assign each block to a given school in the district, whose location is known from GPS coordinates present in the Common Core of Data. The resulting crosswalk between census blocks and schools is the policy which I study in this paper, a policy which the district chose deliberately. This crosswalk is the key object determining the mapping between many interesting economic and social outcomes. I study the relationship between neighborhood racial segregation and school segregation generated by this mapping, motivated by the literature on the effects of segregation and the historical relevance of race in U.S. socioeconomic patterns.

I choose to focus on elementary schools for a few reasons. First, elementary schools are smaller and hence generally more numerous within each district; this means that the district has more leeway in designing attendance zones for these schools than for higher grades. Second, a majority of districts operate a feeder system between schools, meaning that lower level schools feed into upper level schools systematically. Hence, middle school attendance boundaries can be roughly thought of as the union of a few elementary school zones. Finally, as elementary schools are the lowest rung of public education, they are the first point at which children engage in social interaction outside of the home, making them the first place in which school segregation takes place.

The main dataset used in this study is the School Attendance Boundary Survey (SABS) for SY 2013-2014, the Department of Education’s first attempt at characterizing the school attendance boundaries of every large school district in the country. Using GIS software, I link this spatial data to 2010 census block geography, for which I can observe population by race. This allows me to compute the racial composition of school assignments with great accuracy. Figure 1 depicts the main unit of analysis, a school district divided into elementary school attendance boundaries laid over the 2010 distribution of race.

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2Specifically, the assignment of blocks to SAZs is done by computing the block centroid and asking whether this point is inside the polygon defined by the SAZ. This may lead to errors in assignment. These are likely very limited, but also correlated with how close is to SAZ boundary, which should keep in mind when thinking of spatial RDs

3It should be noted that the term “elementary school” is used to refer to schools that serve different sets of grade levels, but generally up to fifth or sixth grade.
The SABS included a total of 33,638 attendance boundaries administered by 4,731 school districts. Given that I focus on the boundary-drawing problem and its relation to school segregation, I make a number of sample restrictions which discard cases in which this problem is degenerate. Table A.1 provides a gradual depiction of how sample restrictions change the distribution of SABs in the analysis sample. The first column in Table A.1 reports mean characteristics for all attendance boundaries in the sample. The average SAB has a total population of about 9,600 residents, 618 of which are aged 5 to 9 years, with an enrollment to population ratio (for grades K-4) of 84%, and an average student distance to school of about 2.75 kilometers. Column (2) shows how the sample changes when I drop de facto school districts (i.e. districts that serve a single school for each grade level) and those that exclusively use nonresidential assignments.

The majority of districts dropped here are rural, but this restriction also gets rid of the largest district in the country, New York City Schools. This is reflected in the mean SAB characteristics, with a decrease in both average population and distance to school. Further sample restrictions, such as discarding small districts that serve less than 2000 students or administer fewer than 5 schools (column (3) in Table 1), or dropping un-diverse districts with more than 97% or less than 3% minority (column (4) of Table 1), do not seem to significantly affect the mean characteristics of SABs or districts in the sample. The final analysis sample contains 23,823 attendance boundaries. The SABS data also reflects some of the complexities associated with student assignment rules. Some districts enact ‘choice zones’ which assign some residences to multiple schools which create overlapping SABs (I call this feature ‘multiple assignment’); 13% of SABs in the sample have this feature. Discontiguous SABs reflect student busing schemes and generate ‘satellite SABS’; 12% in the sample have this. Moreover, districts some times have some schools open to enrollment (no assigned residences); only about 2% of schools in the analysis sample.

In addition to data on SABs and the residential distribution of race, I use several other sources to measure attributes of school districts (and sometimes schools), including: desegregation court orders, median household incomes, education attainment of adults, school district finance, qualitative measures of school quality, racial gaps in stu-

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4Not all complexities are depicted the data, however. For instance, some districts operate magnet and other special schools which do not make use of a typical address-based attendance boundary system, these schools are not present in SABS. I discuss the implications of this restriction in the data appendix.

5I assign open enrollment schools the mean characteristics of the district. Please see Section 2.5 for a discussion of the implications of this imputations for the current analysis.
dent achievement, real estate values and characteristics, and others. The data sources are the U.S. Census, the National Center for Education Statistics, and the Office of Civil Rights of the Department of Education.

**Definition of Minimum Distance SABs**

SABs that minimize travel distance to school are a convenient counterfactual for existing zoning choices. Such a design is a "neighborhood schools" assignment scheme in the strict sense – the set of residences assigned to a school is precisely those that are not closer to any other school. Neighborhoods are thus simply defined areas of closest proximity to schools, creating a perfect partition of the jurisdiction into a number of polygons equal to the number of schools. In mathematics such partition of a space given a finite set of points (school locations) is called the Voronoi mapping. Drawing these zones requires a straightforward optimization procedure based on the distance matrix between census blocks and schools – each element of this matrix measuring the distance between a census block centroid and a school location.

In Figure 1, panels (2) and (4), the reader can see a comparison between Springfield’s actual school attendance boundaries and the minimum travel distance counterfactuals. By definition, the benchmark map has a lower mean distance to school per pupil than the actual map. I assume that a district’s minimum travel distance mapping with respect to it’s school locations is a feasible and reasonable counterfactual SAB assignment scheme.\(^6\)

Voronoi maps are theoretically well-defined no matter which distance metric is used. I choose euclidean distance both for ease of exposition and elegance. However, the empirical results presented in the rest of the paper do not rely heavily on this assumption. Providing evidence for this claim, I study an example in which Voronoi maps are constructed by combining real road networks and Dijkstra’s algorithm. I take census block geography of Fresno, CA. I obtain the "Roads" shapefile from the U.S. Census TIGER/Line, a map denoting all roads. Using this rich data, I construct the road network of the city, with road intersection representing nodes, and the roads themselves denoting network connections.

\(^6\)Richards (2014) was the first, to my knowledge, to introduce the use of minimum travel distance maps as counterfactual SAB schemes. The idea of using counterfactual maps to assess to the extent of boundary manipulation is also present in the political science literature on congressional gerrymandering, see (Chen 2013).
Using the road network, I construct the distance matrix between all census block centroids in Fresno and all of its elementary schools. The rows of this matrix correspond to unique city blocks, while each column corresponds to a unique school. Each element of this matrix is the minimum road network distance to each school, computed using Dijkstra’s algorithm, which finds the minimum distance path between any two points in a network. In my case, the two points is the closest road intersection to a census block and elementary schools and the network is the roads. Having constructed the distance matrix, I can quickly find the minimum road network distance zones, by assigning blocks to their closest school, in terms of road network distance.

Equipped with both minimum euclidean distance school assignments and minimum road network distance (henceforth, RND) assignments, I can empirically assess the degree of bias due to the euclidean distance assumption. Appendix Figure 1 plots a census block level histogram of the euclidean proximity ranking of schools, with school assignments corresponding to minimum road network distance. The vast majority of blocks are assigned to the same school regardless of which distance metric is used. About 500 blocks, or 13% of blocks in the city, obtain a different assignment with minimum RND criteria. The majority are linked to the second closest school in terms of euclidean distance, with very few exceptions.

Discrepancies between euclidean and RND Voronoi assignments may lead to systematic difference in the distribution of racial composition across SABs, potentially biasing my estimates of desegregation policy. To test this, appendix Figure 2 plots the racial composition of existing SABs against the composition of both the euclidean Voronoi and RND Voronoi zones. I plot the OLS fit for each relationship. The plot shows that, while there is indeed some variation in the racial composition of minimum distance zones across the euclidean and RND metrics, the resulting relationship with real 2013-14 SABs is almost exactly the same. Therefore, I would estimate almost exactly the same level of desegregation policy regardless of which counterfactual is being used. Considering the minimum RND zones are much more cumbersome to deal with (specially when dealing with hundreds of districts across the country), I opt for the euclidean metric instead for the main analysis.

A related but separate potential critique of the minimum distance SAB counterfactuals is that they are unrealistic, as they ignore concerns for school capacity. Indeed, by simply assigning residences to the school they are closest to, it may be the case the
resulting assigned population of each school may violate capacity constraints. While the spatial distribution of school locations is correlated with population density – schools are located closer to each other in densely populated districts, and Voronoi zones will therefore be smaller in denser districts by definition – this may be a potential source of bias. Still, in order for this to be the case a necessary condition is that SAB racial composition vary systematically with capacity constraint adjustments to minimum distance school assignments.

To partially alleviate this concern, Appendix Figure 3 plots a histogram of total population across all SABs in the data for existing 2013-14 zoning as well as for my euclidean minimum distance (Voronoi) counterfactuals. There is common support in both of these distributions, with very similar moments. If anything, it appears like Voronoi zones tend have lower population than actual SABs, as suggested by the relative excess mass in the left tail of the distribution. I interpret this as evidence that Voronoi zones do not systematically violate school capacity. I further assume that any capacity discrepancies could be adjusted without systematic variation in the distribution of school racial compositions.
Figure A.1: Euclidean Distance Ranking of Schools in Minimum Road Network Distance School Zones

Note: Figure presents census block level histogram of Fresno Unified School District with minimum road network distance (RND) school assignments. The horizontal axis measures the euclidean distance ranking of the school assigned to each block using minimum RND criteria. Lower ranking correspond to shorter distances. euclidean ranking equal to 1 means that closest school in terms of euclidean distance is the same as the closest school using RND.
Figure A.2: Bivariate relationship between racial composition of minimum euclidean distance zones against racial composition of minimum road network distance zones.

Note: Scatter plot summarizes two bivariate relationships of the racial composition of Fresno USD’s school attendance boundaries (SABs). In red circles, I plot the racial composition (fraction black or hispanic) of a school’s existing 2013-14 SAB against counterfactual racial composition based on minimum road network distance (RND) zoning. In black diamonds, I present a similar plot, with counterfactuals defined using the minimum euclidean distance instead.
Figure A.3: Total Population of SABs, SY 2013-14 and euclidean Voronoi Counterfactuals

Note: Figure plots histogram of total 2010 census population in actual existing 2013-14 school attendance boundaries (SABs) and in euclidean Voronoi counterfactual SABs.
**Figure A.4:** Eight School District Zoning Observations

Note: Figure plots the jurisdiction and 2013-14 school attendance boundaries (SABs) of eight school district observations in the analysis dataset (in order from top left across columns): City of Chicago SD 299, IL; Riverside USD, CA; Charlotte-Mecklenburg Schools, NC; Oakland USD, CA; Tucson USD, AZ; DC Public Schools, DC; Springfield SD, MA; East Baton Rouge Schools, LA. The analysis dataset contains approximately 1,500 of these maps. The heat coloring denotes the racial composition (fraction black or hispanic) of each SAB – lighter colors denoting low fraction minority.
### Table A.1: SABS Summary Statistics – Sample Restriction Cascade.

<table>
<thead>
<tr>
<th>Panel A: Demographics of SAB Assignments</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Total Population</strong></td>
<td>9637.15</td>
<td>8056.22</td>
<td>8332.30</td>
<td>8431.63</td>
</tr>
<tr>
<td><strong>Fraction Minority</strong></td>
<td>0.31</td>
<td>0.32</td>
<td>0.35</td>
<td>0.36</td>
</tr>
<tr>
<td><strong>Population 5 to 9 years old</strong></td>
<td>618.23</td>
<td>551.04</td>
<td>570.70</td>
<td>578.11</td>
</tr>
<tr>
<td><strong>Fraction Minority</strong></td>
<td>0.38</td>
<td>0.39</td>
<td>0.43</td>
<td>0.44</td>
</tr>
<tr>
<td><strong>Enrollment K4</strong></td>
<td>392.47</td>
<td>399.72</td>
<td>411.18</td>
<td>415.76</td>
</tr>
<tr>
<td><strong>Fraction Minority</strong></td>
<td>0.42</td>
<td>0.43</td>
<td>0.47</td>
<td>0.48</td>
</tr>
<tr>
<td><strong>Enrollment/Population Ratio</strong></td>
<td>0.84</td>
<td>0.87</td>
<td>0.87</td>
<td>0.87</td>
</tr>
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</table>

<table>
<thead>
<tr>
<th>Panel B: Characteristics of SABs</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Distance to School per Student (km)</strong></td>
<td>2.75</td>
<td>2.56</td>
<td>2.27</td>
<td>2.20</td>
</tr>
<tr>
<td><strong>Open Enrollment School</strong></td>
<td>0.05</td>
<td>0.02</td>
<td>0.02</td>
<td>0.02</td>
</tr>
<tr>
<td><strong>Multiple Assignment SAB</strong></td>
<td>0.14</td>
<td>0.14</td>
<td>0.13</td>
<td>0.13</td>
</tr>
<tr>
<td><strong>Satellite SAB</strong></td>
<td>0.12</td>
<td>0.12</td>
<td>0.12</td>
<td>0.12</td>
</tr>
<tr>
<td><strong>SAB Bizarreness</strong></td>
<td>0.21</td>
<td>0.21</td>
<td>0.22</td>
<td>0.22</td>
</tr>
<tr>
<td><strong>Title I Eligible School</strong></td>
<td>0.79</td>
<td>0.77</td>
<td>0.76</td>
<td>0.76</td>
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</table>

<table>
<thead>
<tr>
<th>Panel C: Characteristics of School Districts</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Total Population</strong></td>
<td>392874.21</td>
<td>343789.17</td>
<td>400068.53</td>
<td>411689.83</td>
</tr>
<tr>
<td><strong>Fraction Minority</strong></td>
<td>0.30</td>
<td>0.31</td>
<td>0.33</td>
<td>0.34</td>
</tr>
<tr>
<td><strong>Adult Fraction with College</strong></td>
<td>0.48</td>
<td>0.50</td>
<td>0.50</td>
<td>0.50</td>
</tr>
<tr>
<td><strong>Median Household Income</strong></td>
<td>56951.59</td>
<td>58252.85</td>
<td>58247.70</td>
<td>58541.62</td>
</tr>
<tr>
<td><strong>Median Property Value</strong></td>
<td>197244.27</td>
<td>201729.07</td>
<td>201474.55</td>
<td>203315.80</td>
</tr>
<tr>
<td><strong>Total Revenue per Pupil</strong></td>
<td>12358.65</td>
<td>12152.69</td>
<td>11943.70</td>
<td>11940.70</td>
</tr>
</tbody>
</table>


**Note:** This table reports mean characteristics of school attendance boundaries (SAB) in school districts included in the SY 2013-2014 School Attendance Boundary Survey (SABS), produced by NCES. Panel A summarizes the 2010 demographics of census blocks assigned to schools via attendance boundaries using GIS software. Fraction minority is the fraction of residents from the minority group (blacks and hispanics). Enrollment counts are from the Common Core of Data. Panel B reports geographical characteristics of SABs. Distance to school per student is measured as the population-weighted mean euclidean distance between census block centroids and school locations. Open enrollment schools do not use student residence as a factor in student assignments. Multiple Assignment SABs refers to instances in which boundaries overlap, such that residences can be assigned to more than one school. Satellite SABs refers to discontiguous attendance zones, such that separate distant neighborhoods can be assigned to the same school, generating SABS composed of several polygons. SAB bizarreness is computed as the ratio of SAB surface area to that of its convex hull (see Chambers and Miller 2010). Panel C reports characteristics of school districts from 2010 census block group geography, matched with GIS software. Columns show increasingly restrictive samples. Defacto school districts that administer only one school for each school grade, such that SABs are degenerate, taking on the entire school district boundary. Non-residential assignment districts are those that don’t use student residences in any school assignments. Small districts are defined as those with less than 5 schools or less than 2000 total population. Undiverse districts are those with less 3% fraction minority, and those with more than 97% fraction minority.
Table A.2: Interaction of SAB Racial Composition Shock with New School Construction

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Δ SAB Composition (2000 Census)</td>
<td>-0.0666</td>
<td>-0.0668</td>
<td>-0.0897</td>
<td>-0.0215</td>
</tr>
<tr>
<td></td>
<td>(0.176)</td>
<td>(0.175)</td>
<td>(0.168)</td>
<td>(0.206)</td>
</tr>
<tr>
<td>New School</td>
<td>0.00142</td>
<td>0.000682</td>
<td>-0.0563</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0516)</td>
<td>(0.0517)</td>
<td>(0.0566)</td>
<td></td>
</tr>
<tr>
<td>Δ SAB Comp. × New School</td>
<td></td>
<td></td>
<td>0.0510</td>
<td>-0.00787</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.177)</td>
<td>(0.216)</td>
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<th>(3)</th>
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</tr>
</thead>
<tbody>
<tr>
<td>Quadratic in Baseline Composition of Block</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Baseline Composition of SAB</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Baseline SAB Fixed Effects</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Census Tract Fixed Effects</td>
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<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Baseline SAB-by-Census Tract Fixed Effects</td>
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<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>N</td>
<td>3460</td>
<td>3460</td>
<td>3460</td>
<td>3451</td>
</tr>
<tr>
<td>N census tracts</td>
<td>97</td>
<td>97</td>
<td>97</td>
<td></td>
</tr>
<tr>
<td>N SABs (schools)</td>
<td>68</td>
<td>68</td>
<td>68</td>
<td></td>
</tr>
<tr>
<td>N tract-by-SABs</td>
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<td>242</td>
</tr>
<tr>
<td>R²</td>
<td>0.161</td>
<td>0.161</td>
<td>0.161</td>
<td>0.194</td>
</tr>
</tbody>
</table>

Note: Standard errors clustered at the census block group level in all models. The level of observation is a census block in Mecklenburg County, NC. In all models the dependent variable is the logged mean property sales price at the census block level. Hedonics (including: number of bathrooms and bedrooms, square footage, presence of AC and heating, number of floors, etc.) were first partialled out from property prices in a first stage regression at the property (parcel) level, see Appendix. Estimated models are different versions of equation (12) in the text, augmented for treatment effect heterogeneity with respect assignment to new schools. The indicator for New School is defined as school attendance boundaries (SABs) with identifiers that were not present in the 2000 CMS SAB map. Column (1) controls for a quadratic function for baseline (2000) block racial composition and the 2000 (baseline) composition of SAB assignments. Column (2) controls more flexibly for baseline SAB effects by including fixed effects for the 2000 school attendance boundary (zone) of a given block. Column (3) adds census tract fixed effects. Column (4) adds baseline (2000) SAB - by - census tract fixed effects.
Appendix B

Evaluating the Texas Dream Act: Additional Figures
Figure B.1: Graduating Cohort – Raw College Plans Trends

Note: Figure shows raw cohort shares for college demand, by treatment status. The estimation sample is the universe of Hispanic high school graduates. The vertical line denotes the year in which tuition equity reform was introduced.
Figure B.2: Entering Cohort College-Bound Investments – Raw Outcome Trends

Note: Figure shows raw outcome shares for a range of high school outcomes, by treatment status. The outcomes include: high school graduation, dropout, number of school credits attempted, number of Advanced Placement courses attempted, number of dual-credit courses, course-passing rate, standardized exam scores in mathematics and reading, attendance rate, and discipline event rates. The estimation sample is the universe of Hispanic high school entering cohorts. The vertical line denotes the year in which tuition equity reform was introduced.
Figure B.3: Graduating Cohort Covariate Trends

Note: Figure shows raw cohort shares for a range of covariates, by treatment status. The covariates include: free and reduced price lunch status (FRL), english language learner status (ELL), special education participant, exit standardized exam scores in mathematics and reading, gender, age at graduation, at risk of dropping out status, gifted and talented program participant, and indicator for whether spanish is the main language spoken at home, graduation rate at student’s high school, and TWC match status. The estimation sample is the universe of Hispanic high school graduates. The vertical line denotes the year in which tuition equity reform was introduced.
Figure B.4: Entering Cohort Covariate Trends

Note: Figure shows raw cohort shares for a range of covariates, by treatment status. The covariates include: free and reduced price lunch status (FRL), english language learner status (ELL), special education participant, 8th grade standardized exam scores in mathematics and reading, gender, age at graduation, at risk of dropping out status, gifted and talented program participant, and indicator for whether Spanish is the main language spoken at home, graduation rate at student’s high school, and an index combining all of these demographics to predict the probability of high school graduation (using a simple probit model and computing predicted values). The estimation sample is the universe of Hispanic entering cohorts. The vertical line denotes the year in which tuition equity reform was introduced.