Immigration Enforcement and Student Outcomes

by

Laura Bellows

Public Policy
Duke University

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Anna Gassman-Pines, Supervisor

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Elizabeth Ananat

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Angel Harris

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Sarah Komisarow

Dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Public Policy Studies in the Graduate School of Duke University

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ABSTRACT

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Abstract

During the past 20 years, immigration enforcement increased dramatically in the U.S. interior. There is a growing recognition that immigration enforcement in the U.S. interior has spillover effects onto U.S. citizens, particularly the family of unauthorized immigrants. U.S. citizen children in mixed status families are particularly likely to be affected. Over 5 million children are estimated to have at least one unauthorized parent, and 80 percent of these children are U.S. citizens. These chapters contribute to a full accounting of the costs of immigration enforcement by investigating its impacts on educational outcomes, which have long-term ramifications for the United States.

I focus on the effects of partnerships between Immigration and Customs Enforcement (ICE) and local law enforcement. Although raids by ICE agents, whether at worksites or in the community, are particularly salient, the majority of arrests by ICE result not from direct arrests by ICE agents but from transfers to ICE from federal, state, or local custody. In my first chapter, I use the staggered rollout of Secure Communities, a biometric sharing program activated in every U.S. county between 2008 and 2013. I examine this program's effects on county-level academic achievement and school enrollment. In my second and third chapters, I examine the impacts of another type of partnership between ICE and local law enforcement, 287(g) programs, on achievement, attendance, out-of-school suspensions, and school mobility within North Carolina. In North Carolina, nine counties were approved to establish 287(g) programs, and another fifteen applied but were not approved to participate. I use a triple difference strategy in which I compare educational outcomes for different groups of students in these two sets of counties before and after activation of 287(g) programs.Together, these studies provide evidence on how partnerships between local law enforcement and ICE affect educational outcomes for students, as well as which students are likely to experience impacts.

I find that the activation of 287(g) programs decreases school engagement by decreasing attendance. This effect is concentrated at the top of the distribution, increasing chronic absenteeism (missing 15 or more days per year), and is driven by high school students. In contrast, I find more mixed results for the effects of both types of partnerships on math and English Language Arts (ELA) achievement in grades 3-8. Although I observe a small decline in ELA achievement for Hispanic students following the activation of Secure Communities, this decline may result from other factors correlated with activation. I observe no effect of 287(g) programs on achievement.

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Acknowledgements

I am very grateful to my advisor Anna Gassman-Pines for her support during the past five years. Thank you for treating me like your junior colleague from the beginning and providing me with the opportunity to own our work together, both in research and in teaching. Thank you for aggressively encouraging me through self-doubt. Thank you for standing up for me, even when it was not in your best interest. I will miss joking with you about gaydar and Kate McKinnon and everything else.

I want to thank the rest of my dissertation committee, Elizabeth Ananat, Seth Sanders, Sarah Komisarow, and Angel Harris, for their ongoing guidance. Thank you to Elizabeth Ananat for believing in me and my work, especially when I lacked confidence. Seth Sanders has the distinction of being both incredibly thoughtful about research and one of the kindest people I have ever met. Thank you also to Penny Sanders for her endless patience with me during this final year, as well as to Lisa Kukla, without whom I would have been lost long ago.

I also want to thank mentors and colleagues from outside of Duke. Thank you to Ashlyn Nelson and Cynthia Osborne for your advice and friendship. I've learned more about methods working with Paul von Hippel than I have from any statistics course; thank you for your support and faith in me. I am grateful for many of the colleagues I've met at other institutions but want to especially thank Stephanie Potochnick: I cold-emailed her as a third year graduate student about data on 287(g) program applications, and she not only sent me the data but also initiated me into a group of migration scholars. Her generosity inspires me to be a better, kinder academic.

I have been lucky to receive funding from both internal and external sources. Internally, I acknowledge the James B. Duke Fellowship, the Education and Human Development Scholars program through the Duke University Social Science Research

Institute, and the Katherine Goodman Stern Fellowship. Externally, my work has been supported by the Horowitz Foundation for Social Policy and the Russell Sage Foundation's Educational Opportunity Monitoring Project (award #83-17-01).

Thank you to my parents, Donald and Virginia Bellows, for your constant encouragement. And thank you to Tiffany Norman, the love of my life. This Ph.D. has been a journey for both of us. Thank you for shoring me up when I am down and never wavering in your support of my dreams. I am a better researcher because you push me to ask more relevant questions (one day, I will conclusively prove that children need food to eat).

I want to end this by thanking the people who helped me get through day-to-day, my friends in Durham. Outside of Sanford, Akshay Bareja, my concert and gym buddy, and Mai-Anh Vu, the best roommate ever, made me a more sane, tolerable human. Lucy Sorensen, Yulya Truskinovsky, Danielle Vance-McMullen, Puneet Chehal, Ying Shi, and Zoelene Hill, thank you for your guidance and advice. Thank you for practice interviewing me, reading job documents, and subsidizing my hotel rooms at conferences. Josh Rivenbark, Anna Birkenbach, and Faraz Usmani, thank you for letting off steam with me. Marwa AlFakhri, thank you for never getting embarrassed when I cry over breakfast at Elmo's. Kelly Hunter and Sarah Nolan, thank you for your support and friendship. Mercy DeMenno, I am always uncertain what I did to earn your good opinion but forever grateful to have it. Becca Lehrman, you are my ride-or-die friend. Ade Olayinka, you are one of the only people I can imagine spending two weeks bothering strangers with and not hating afterwards. Thank you for reminding me that everyone deserves grace. To everyone I've named and to many I have not, your friendship was the highlight of this process.

Introduction

Under the current presidential administration, the debate over immigration enforcement has grown in prominence, with some calling for increased removals of unauthorized immigrants from the United States and others calling for an end to the bureaucracy of immigration enforcement (#AbolishICE). However, the apparatus allowing for increased removals from the U.S. interior was established during the previous two administrations, which greatly expanded arrests in and removals from the U.S. interior using partnerships between Immigration and Customs Enforcement (ICE) and local law enforcement (Capps et al., 2018).

Immigration enforcement in the U.S. interior is likely to have spillover effects on U.S. citizens because many unauthorized immigrants are in households with U.S. citizens, referred to as "mixed-status families" (Vargas, 2015). A large number of U.S. citizens with unauthorized family members are children: of the 5.1 million children estimated to have at least one unauthorized parent in 2010, 80 percent are estimated to be U.S. citizens (Passel and Taylor, 2010). A growing body of literature is beginning to unpack the spillover effects of immigration enforcement. Researchers are also beginning to distinguish between different types of enforcement, with the insight that the effects of ICE raids, which occur semi-randomly, may be different than the effects of ongoing programs of surveillance, such as 287(g). I add to this literature by examining the effects of two different partnerships between ICE and local law enforcement, 287(g) programs and Secure Communities, on educational outcomes. I focus on children's educational outcomes because they are not only a measure of current well-being but are also linked to long-term outcomes, such as college attendance and earnings.

Understanding the effects of immigration enforcement is challenging for several reasons. First, the population most likely affected, unauthorized immigrants and their family members, are hard to identify in most data sources. Second, arrests and removals do not occur randomly: individuals are more likely to experience arrest and removal if they have previous contact with the criminal justice system, for example. Third, local law enforcement vary in their levels of cooperation with ICE, and local policies may be correlated with other county-level characteristics likely to affect the outcomes of unauthorized immigrants and their families. I add to this growing body of literature by using multiple methodological approaches and several sources of data to identify the effects of immigration enforcement on educational outcomes.

In the first chapter, to examine the impact of immigration enforcement on county-level outcomes, I use the staggered rollout of Secure Communities, a biometric sharing program activated in every U.S. county between 2008 and 2013. I match information on Secure Communities with newly available measures of average county achievement from the Stanford Education Data Archives (SEDA). I additionally use data on fingerprint matches and removals obtained from ICE. Although results suggest that Secure Communities reduced achievement in English Language Arts (ELA) for Hispanic students, as well as achievement in ELA and math for non-Hispanic black students, I present other results indicating that timing of Secure Communities activation was correlated with other county characteristics, including local law enforcement's previous application to partner with ICE. These counties may have applied to partner with ICE because of increasing levels of anti-immigrant animus, which might also affect test scores.

In the second and third chapters, I eliminate this source of bias by focusing only on counties that applied to establish 287(g) programs, in which local law enforcement agents are authorized by ICE to act as immigration agents. I compare educational outcomes for 24 counties in North Carolina: nine counties were approved to establish

287(g) programs, whereas another fifteen applied but were not approved to participate. I use a triple difference strategy in which I compare outcomes for different groups of students in these two sets of counties before and after activation of 287(g) programs. In the second chapter, I find that the activation of 287(g) programs increased absences for Hispanic students, as compared with white students, by over half a day per year. This increase appears to be driven by an increase in chronic absenteeism (missing 15 or more days per year) and also appears to be concentrated among high school students. I find no effects on mobility or out-of-school suspensions. In the third chapter, I find that the activation of 287(g) programs did not affect math or reading test scores for students in grades 3-8.

Together, these results suggest that partnerships between local law enforcement and ICE have negative effects on some student outcomes, particularly attendance. Attendance is a key measure of school engagement, and chronic absenteeism is associated with poor long-term educational outcomes, such as increases in dropout rates. I do not detect consistent effects on student achievement. It may be that these partnerships do not affect student achievement, or it may be that these partnerships have stronger effects on high school students, for whom I do not examine achievement outcomes.

Chapter 1

The Impact of Secure Communities on Student Achievement and Mobility

1.1 Introduction

Between 2007 and 2013, immigration enforcement increased dramatically in the U.S. interior (Figure 1.1). From 2003 to 2006, an average of 9000 individuals were removed from the U.S. interior each month. Between 2007 and 2013, that average nearly doubled: almost 17,000 individuals were removed from the U.S. interior each month. This increase was accomplished primarily through partnerships between local law enforcement and Immigrations and Custom Enforcement (ICE). Between 2003 through 2006, ICE issued fewer than 1000 detainers or immigration holds of individuals in law enforcement custody per month. Between 2007 and 2013, ICE issued an average of 19,000 detainers per month (Figure 1.2). Between FY 2008 and 2011, transfers from local and state law enforcement custody accounted for 85 percent of ICE arrests in the U.S. interior (Capps et al., 2018).

One partnership between local law enforcement and ICE was the Secure Communities program, "the largest expansion of local involvement in immigration enforcement in the nation's history" (Cox and Miles, 2013, 93). Despite Secure Communities' stated purpose to reduce crime by removing criminal aliens, two previous evaluations found no effects of Secure Communities on crime rates in activated jurisdictions (Miles and Cox, 2014; Treyger et al., 2014).[1] However, the rollout of Secure Commu-

[1] I occasionally use the terms "alien" or "criminal alien" because those are the official terms used

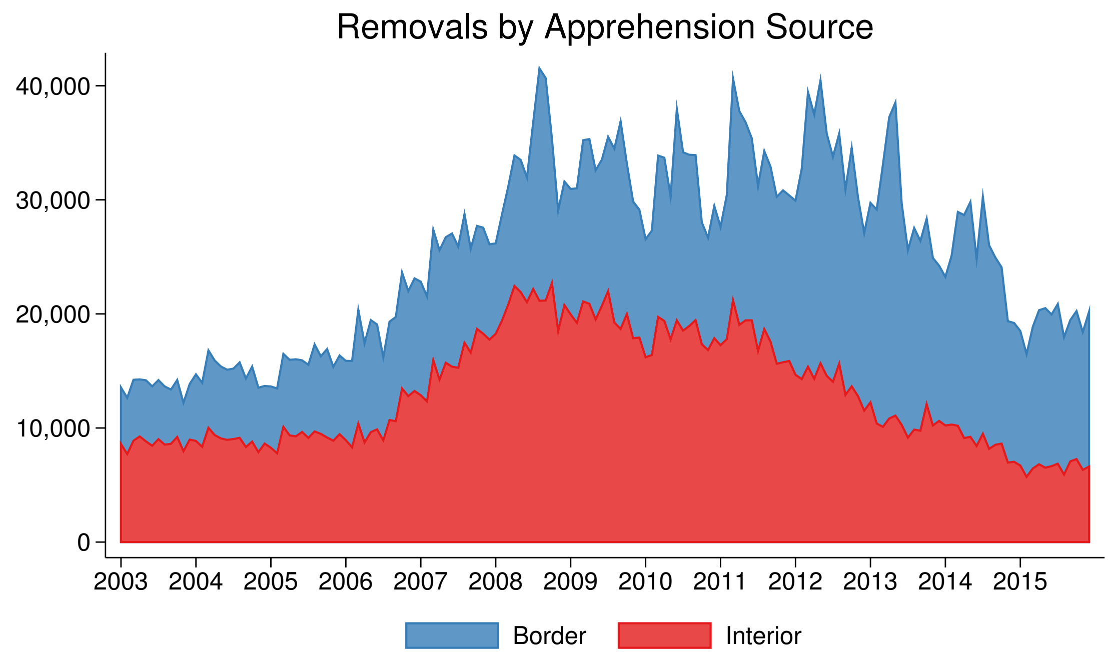

Figure 1.1: Pattern of Removals
Source: Transitional Records Access Clearinghouse (TRAC), Syracuse University

nities did impact children, increasing parent-child separations among deportees from Guatemala, Honduras, and El Salvador (Amuedo-Dorantes et al., 2015). Approximately 37 percent of individuals arrested via Secure Communities report having U.S. citizen children (Kohli et al., 2011). It is likely, however, that the enactment of Secure Communities affected the well-being of children who did not experience parent-child separations. Residing in a community with rising levels of detentions and removals increases stress and fear for both unauthorized parents and their children. These rising levels of stress and fear are likely to impact other child outcomes, including children's performance in school.

in government documents. Legally, alien refers to the broader class of foreign nationals who reside in the United States, including nonimmigrants who have been granted temporary status. However, alien is often used pejoratively, and I prefer to describe foreign nationals residing in the U.S. interior as immigrants, in recognition that individuals have likely made a long-term commitment to living in the U.S. I therefore use alien only when referring to official data or other U.S. government statements.

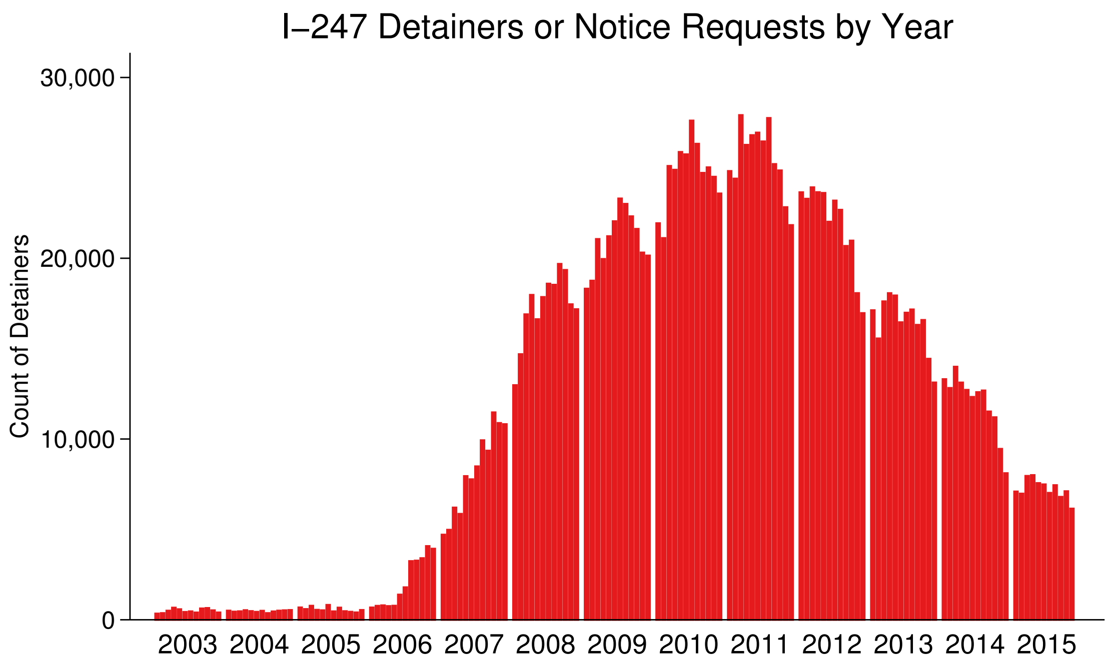

Figure 1.2: Pattern of Detainers Issued
Source: Transitional Records Access Clearinghouse (TRAC), Syracuse University

Stress and fear associated with immigration enforcement are likely greatest for the 5.1 million U.S.-resident children who are estimated to have at least one unauthorized immigrant parent (Passel and Taylor, 2010). Beyond children of unauthorized immigrants, the children of authorized immigrants may also feel stress and fear if these policies increase hostility towards immigrants. A broader population of children may be affected if they are exposed to immigration enforcement. Although the extent of children's exposure to immigration enforcement is unknown, nearly 40 percent of respondents in a recent survey of Latino adults reported knowing someone who had been detained or removed (Vargas et al., 2018). Hispanic children are the largest subgroup likely affected. About one quarter of Hispanic children are estimated to have an unauthorized parent (Clarke and Guzman, 2016), and Hispanic children with foreign-born parents account for 53 percent of the 17.5 million Hispanic children in the U.S. (Murphey et al., 2014).

This paper is the first to examine the impact of immigration enforcement on student achievement using administrative test score data from all U.S. counties. I use the staggered rollout of Secure Communities to examine the effects of immigration enforcement policy on county-level average Hispanic achievement during the 2008-2009 through 2012-2013 school years.[2] I find that Secure Communities decreased the average achievement of Hispanic students in English Language Arts (ELA), although not in math. I also examine how increases in removals affect student achievement and find that, as removals increased in a county, the average achievement of Hispanic students declined in ELA and math.

However, I also note several results that call into question the endogeneity of the rollout. First, I find that Secure Communities decreased the average achievement of non-Hispanic black students. These results are surprisingly robust and larger than would be anticipated if they were spillover effects alone. Second, I find that Secure Communities decreased the enrollment of black students. Third, I find that the activation of Secure Communities was correlated with county characteristics other than those previously known. I conclude that the timing of rollout is likely correlated with other unobserved county characteristics trending during this period.

1.2 Theoretical Framework

On average, Hispanic students enter school over half a standard deviation below white students in reading and approximately 70 to 80 percent of a standard deviation below white students in math (Fryer and Levitt, 2006; Stiefel et al., 2007; Reardon and Galindo, 2009; Reardon and Ho, 2015). As students progress through school, His-

[2] I use Hispanic rather than Latino/a/x throughout because students are classified as Hispanic or non-Hispanic in my source data.

panic students improve in performance relative to white students (Stiefel et al., 2007; Clotfelter et al., 2009; Reardon and Galindo, 2009; Reardon and Ho, 2015). However, most of that progress appears to be concentrated in early grades and may be related to improvements in English language skills (Reardon and Galindo, 2009); on average, disparities in achievement between Hispanic and white students are about 50 to 60 percent of a standard deviation by fifth grade (Clotfelter et al., 2009; Reardon and Galindo, 2009) and about 40 percent of a standard deviation by eighth grade (Clotfelter et al., 2009). As students typically gain between 1.2 and 1.5 standard deviations in math and reading from fourth to eighth grade and 0.6 and 0.7 standard deviations from eighth to twelfth grade, these gaps represents multiple years of learning (Reardon, 2011).

Immigration enforcement policies may decrease achievement for Hispanic students through several mechanisms. Most prominently, immigration enforcement policies likely affect the academic performance of children of immigrants by increasing child and parent fear and stress. Both children experiencing a parental detention or removal as well as children not experiencing a parental detention or removal but with an unauthorized parent exhibit higher levels of child distress and anxiety (Allen et al., 2015; Zayas et al., 2015). Unauthorized parents describe constant worry over detection by immigration officials (Menjivar and Abrego, 2012; Nguyen and Gill, 2015), worry which is likely translated to children. Additionally, children of authorized immigrants may experience an increase in stress and anxiety. First, some children of authorized immigrant parents may be confused over their parents' immigration status (Dreby, 2012). Second, authorized immigrants are subject to removal in certain circumstances. Thus, it is not surprising that Secure Communities specifically increased mental health distress among Hispanic immigrants living with non-citizen family members (Wang and Kaushal, 2018). Both child and parent stress are likely to negatively affect children's academic achievement.

Increases in immigration enforcement also could impact student achievement through losses of income and benefits. Families experiencing a detention or removal also typically lose family income (Capps et al., 2007; Dreby, 2012, 2015; Koball et al., 2015). This negative income shock spills over to create housing and childcare instability (Dreby, 2012, 2015; Rugh and Hall, 2016). However, families with unauthorized members not experiencing a detention or removal may also experience a decrease in resources if members reduce employment (Amuedo-Dorantes et al., 2018; East et al., 2018) or their interaction with social service agencies (Watson, 2014; Vargas, 2015; Vargas and Pirog, 2016; Potochnick et al., 2016; Alsan and Yang, 2018). Recent work finds that Secure Communities decreased Hispanic families' participation with the Supplemental Nutrition Assistance Program (SNAP) and the Affordable Care Act (ACA), as well as reduced employment for noncitizen men with lower levels of education (East et al., 2018). Decreases in resources affect children's educational achievement by reducing their family's ability to invest in children or further increasing family stress (Conger and Donnellan, 2007).

Additionally, newly enacted immigration enforcement policies may increase community stress, which could affect Hispanic and non-Hispanic students. An emerging body of research suggests that increases in community-level stress reduce test performance. Studies in Mexico, Brazil, New York City, Chicago, and Washington D.C. all suggest that exposure to community violence lowers student test scores (Sharkey, 2010; Michaelsen and Salardi, 2013; Monteiro and Rocha, 2013; Sharkey et al., 2014; Orraca Romano, 2015; Burdick-Will, 2018; Gershenson and Tekin, 2018). Increases in certain activities by local law enforcement, particularly "broken windows" style policing, also have negative effects on student achievement, although these effects have been previously found only for black boys (Legewie and Fagan, 2018). Since the main targets of Secure Communities were Hispanic immigrants, increases in racial profiling by local law enforcement may affect Hispanic as well as non-Hispanic black youth.

However, even in the face of falling achievement for individual children, immigration enforcement policies may increase measured average achievement by Hispanic students if newly implemented immigration enforcement policies lead to families with unauthorized members migrating or withdrawing children from school. Following increases in immigration enforcement, children of unauthorized immigrants are more likely to leave school (Amuedo-Dorantes and Lopez, 2015) and the activation of a different type of partnership between ICE and local law enforcement, 287(g) programs, decreased Hispanic enrollment in affected counties (Dee and Murphy, 2018). Considering that the children of unauthorized parents likely perform below other Hispanic children, in part because they belong to a more vulnerable, lower-income population, removing them from the school system may increase the average levels of performance for Hispanic students. However, this increase would be artificial because the most vulnerable Hispanic children are no longer being tested.

1.3 Background

Secure Communities required law enforcement agencies to automatically submit fingerprints of arrested individuals to the Department of Homeland Security's (DHS) Automated Biometric Identification System (IDENT). If a potential match was identified, additional data matching and prioritization occurred at the Law Enforcement Support Center (LESC), a centralized ICE location. If the match was determined to be a potentially removable alien, LESC notified an ICE field office within four hours and then could issue a detainer against the individual (Kohli et al., 2011; Rosenblum and Kandel, 2011). A detainer requests that local law enforcement hold the arrested individual for up to 48 hours for transfer into ICE custody. According to data from Syracuse's Transitional Records Access Clearinghouse (TRAC), Secure Communities was responsible for over 600,000 removals from the United States between 2009 and 2018.

Secure Communities was rolled out county-by-county across the U.S. between 2008 and 2013, as shown in Figure 1.3. Previous studies have shown the timing of rollout was related to the size of the Hispanic population, a county's distance from the Mexican border, and a county's previous partnerships between local law enforcement and ICE. However, Secure Communities was also implemented gradually because of resource constraints (Cox and Miles, 2013).

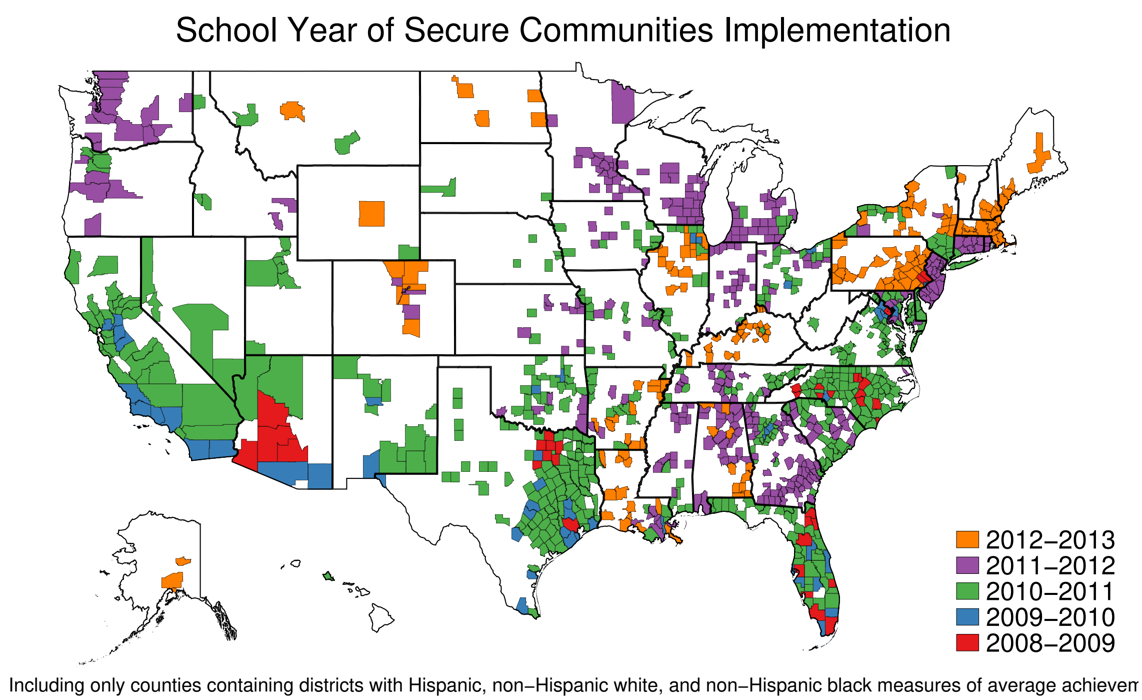

Figure 1.3: Staggered Implementation of Secure Communities
Source: Immigration and Customs Enforcement (2013, January 22). Activated jurisdictions. U.S. Department of Homeland Security.

Although Secure Communities was eventually activated in all U.S. counties, local law enforcement responded to the program in different ways. In early activating counties, ICE originally established memorandums of understanding with local law enforcement. Some states and counties asked to opt out of participation, which originally appeared to be an option. However, in January of 2012, an internal ICE memo was released that made explicit that Secure Communities was a mandatory program. By 2014, increasing criticism by immigration advocates resulted in the Obama administration halting Secure Communities in order to implement programs that better targeted serious criminal offenders (Capps et al., 2018).

During this period, Secure Communities was not the only partnership between ICE and local law enforcement. As I describe in greater detail in chapters 2 and 3, 287(g) programs predate Secure Communities and allow local law enforcement to act as immigration enforcement agents. Importantly, local law enforcement had to apply to participate in 287(g) programs. Because these programs are more resource-intensive for ICE than Secure Communities, they were implemented in a small set of jurisdictions (fewer than half of the local law enforcement agencies that ever applied to participate).

Several studies have used the rollout of Secure Communities to examine its effects on crime, public benefit receipt, and employment (Cox and Miles, 2013; Alsan and Yang, 2018; East et al., 2018). Although Secure Communities had no effect on crime (Cox and Miles, 2013), it decreased SNAP and ACA receipt for households with Hispanic heads (Alsan and Yang, 2018) and decreased employment for noncitizen men, particularly low-skilled noncitizen men (East et al., 2018). These findings match what theory would predict: under conditions of heightened immigration enforcement, families with unauthorized members are more fearful about interacting with the government, and individuals are more fearful about participating in the public sector. However, these papers also reach more unexpected conclusions. Not only does Secure Communities reduce employment for male noncitizens, it also reduces employment for citizen men (albeit by a lesser amount) (East et al., 2018). In at least one specification, Secure Communities appears to increase SNAP receipt for black households (Alsan and Yang, 2018).

In this paper, I use a similar strategy to examine the effects of this rollout on educational achievement, as well as enrollment. Both prior studies suggest short-term negative impacts of Secure Communities on families, in terms of reduced income and benefits, which may lead to longer-term negative impacts on educational attainment.

1.4 Data

I use newly available measures of average county achievement for Hispanic, white, and black students from the Stanford Education Data Archives (SEDA) (Reardon et al., 2017a). These data were constructed using the results of federally mandated grade 3-8 math and English Language Arts (ELA) tests in school years 2008-2009 through 2012-2013. Under No Child Left Behind (NCLB), all states are required to test grade 3-8 students annually in reading and math. However, as each state is allowed to designate its own test, results were not previously comparable across states. As described in Reardon et al. (2017b), SEDA has linked state achievement tests to states' National Assessment of Educational Progress (NAEP) results, which allows researchers to directly contrast student achievement in counties and districts across the United States for the first time.

Average achievement for student subgroups is measured for a particular grade, year, county, and subject if there are at least 20 students in that subgroup tested (in that grade, year, county, and subject). Additionally, SEDA does not include information on some grade, year, county, and subject observations if students took different tests within the state-subject-grade-year, if states had participation lower than 95 percent within a certain year, or if insufficient data were reported to ED*Facts* (Fahle et al., 2017). The first of these conditions results in a differing number of observations for ELA and math achievement: in California, Virginia, and Texas, students take end-of-course, rather than end-of-grade, assessments in 7th and 8th grade math. I exclude both subjects if the grade-year-county observation is missing one subject. Subgroups are mutually exclusive: students are classified as either Hispanic, white, or black, meaning that Hispanic and white or Hispanic and black students would be classified as Hispanic. SEDA provides several different versions of county averages; I use estimates of county averages standardized within subject and grade, measured in national student-level SD units. Additionally, SEDA also provides estimates of standard errors of average achievement measures, which I use to calculate precision weights. SEDA also makes available counts of students who took achievement tests by different subgroups.

One concern might be that first and second generation Hispanic students are less likely to take state tests and that state test results therefore do not capture the scores of students most likely to be affected by immigration enforcement policies. Indeed, NCLB exempts English Language Learner (ELL) students from testing in ELA during their first year in school; however, ELL students are required to test in math during their first year. After the first year, states are required to include ELL students in state tests, but ELL students are allowed to test in their own language. In 2012-2013, ten states allowed ELL students to test in a language other than English for accountability purposes, with nine of those states allowing Spanish-speaking ELL students to test in Spanish for math and five states allowing Spanish-speaking ELL students to test in Spanish for ELA (Boyle et al., 2015). In SEDA, all state assessments, including Spanish-language assessments, are included in calculations used to estimate county averages.

Estimating the effect of Secure Communities on student achievement requires an indicator for whether the program was activated prior to the beginning of the state's testing window for a particular year. Information on precise testing dates is unavailable in SEDA. Therefore, I collected state testing windows for the 2008-2009 through 2012-2013 school years using state department of education websites and through communication with state education administrators. State testing windows vary widely in length: although some states prescribe that all students test on a single day in a particular subject, other states allow school districts to schedule tests at any point over several months. The majority of testing windows begin in spring; however, a few states test in the fall on material that students covered in the previous academic year (Personal communication with education officials in Maine, Michigan, and Vermont). I combine information on state testing windows with publicly available information from ICE on the dates of Secure Communities activation to create my main variable of interest. I treat Secure Communities as active for that school year if Secure Communities was active prior to the beginning of the state's testing window for that particular school year.

The effect of Secure Communities may vary based on the operation of the program within a particular county. Through a Freedom of Information Act request to ICE, I obtained counts of submissions, matches, and removals associated with Secure Communities by county and month. Submissions refers to the number of fingerprint submissions to IDENT per month, indicating the number of individuals arrested per month in a particular county. Matches refers to the number of fingerprint submissions identified as potentially removable aliens per month in a particular county. Data on monthly removals by county are available from the 2008-2009 school year through the 2012-2013 school year, whereas data on monthly submissions and matches are available from the 2010-2011 through 2012-2013 school years.

SEDA only includes information beginning in 2008-2009, the same school year that the first counties were activated for Secure Communities. Therefore, in some supplementary analyses, I use as the outcome variable student enrollment counts from the National Center for Education Statistics' (NCES) Common Core of Data (CCD), which contains comprehensive information on enrollment and staffing within all K-12 public schools. I use CCD enrollment counts, disaggregated by race/ethnicity and aggregated to the county-level, from 2003-2004 through 2013-2014, which provide me with multiple years prior to the activation of Secure Communities.

1.5 Analytic Plan

To estimate the effects of increased immigration enforcement via Secure Communities on average achievement, I use weighted least squares (WLS) models with county, year, and grade fixed effects to account for any persistent differences between counties, nation-wide policy changes in particular years, and performance differences between grades. During this time period, several states instituted state-wide immigration policies, including requiring the use of E-Verify or passing state omnibus laws. These policies may be related both to other immigration enforcement policies and student achievement. I therefore also include state-by-year fixed effects to control for state-wide policy changes in a particular year.

I also control for several county-level time-varying characteristics to account for timing of activation being related to specific county characteristics. Prior work suggests that the timing of Secure Communities implementation was correlated with the total population, size of the Hispanic population, location near the border with Mexico, and presence of a 287(g) agreement (Cox and Miles, 2013). I therefore include time-varying controls for the size of the total and Hispanic populations as well as a control for an active 287(g) agreement. Because counties on the border with Mexico were early in the rollout, likely due to purposeful selection on the part of ICE, I exclude all counties located on the border with Mexico. My regression model is summarized below:

$$\text{Avg}_{ijt} = \alpha + \beta_1 \text{SC}_{jt} + \beta_2 287(\text{g})_{jt} + \beta_3 \text{Num}_{ijt} + \beta_4 \text{Tot}_{ijt} + \phi_j + \gamma_i + \eta_t + \sigma_t + \epsilon \tag{1.1}$$

where Avg is the average achievement of Hispanic students in grade i in county j in year t; SC is an indicator for the activation of Secure Communities prior to the beginning of the testing window in that county in year t; 287(g) is an indicator for the activation of a 287(g) program prior to the beginning of the testing window in that county in year t; Num is the number of tested Hispanic students in a particular grade i, county j, and year t observation; Tot is the total number of tested students in a particular grade i, county j, and year t observation; ϕ is a county fixed effect; γ is a grade fixed effect; η is a year fixed effect; and σ is a state-by-year fixed effect. I cluster standard errors at the county level. I weight by the precision of the estimated county averages, which is the inverse of the standard error of average achievement squared. Mostly, this weights up larger counties; I show results using alternative weighting schemes, as well. I run separate models for average achievement in ELA and math.

I estimate the same models with different dependent variables, substituting the average achievement of non-Hispanic white students and the average achievement of non-Hispanic black students in ELA and math for the average achievement of Hispanic students. In all models, I include only counties that have measures of average achievement for Hispanic students, non-Hispanic black students, and non-

Hispanic white students in that grade, year, and subject.

I also examine the relationship between removals per school year and student achievement. Models are similar to my main models, except that the main predictor variable of interest is removals that school year prior to the beginning of the testing window. I scale removals by the size of the foreign-born Hispanic population in the county using five-year estimates from 2005-2009 from the American Community Survey. I again cluster standard errors at the county level and weight by the precision of the estimated county average.

Because I only have information on average achievement at the county-level, any effects may result from shifts in student enrollment as well as effects on testing students. I therefore examine the effect of Secure Communities on the number of Hispanic, non-Hispanic black, and non-Hispanic white students, using the number of tested students in each subgroup per grade from the SEDA data. I also estimate these models using enrollment counts in grades 3-8 from the CCD, except that I treat Secure Communities as activated during that school year if Secure Communities was activated prior to October 20th (when CCD enrollment counts are required to be reported). Models are similar to those examining achievement, except that I do not control for enrollment variables. I again cluster standard errors at the county level.

1.6 Results

1.6.1 Descriptive Statistics

Table 3.7.1 presents descriptive information on academic test-taking for the subset of counties used in the main analysis. Average ELA and math achievement for all students, as measured in standard deviation units, is only slightly above 0 at 0.04. Average ELA achievement for Hispanic students is about a third of a standard deviation below average ELA achievement for all students, and average math achievement for Hispanic students is about a quarter of a standard deviation below average math achievement for all students. Average ELA achievement for non-Hispanic black students is 41 percent of a standard deviation lower than average ELA achievement for all students, and average math achievement for non-Hispanic black students is 46 percent of a standard deviation below average math achievement for all students. In contrast, average ELA and math achievement for non-Hispanic white students is about a quarter of a standard deviation above average ELA and math achievement for all students.

Table 1.1: Descriptives of Counties

	ELA			Math		
	Mean	Std. Dev.	Range	Mean	Std. Dev.	Range
Average Achievement						
All	0.0433	0.2381	-1.2437-0.8941	0.0390	0.2628	-1.3403-1.4042
Hispanic	-0.3112	0.2280	-1.5150-0.9996	-0.2505	0.2318	-1.4923-1.4618
White	0.2745	0.2081	-1.0185-1.6460	0.2565	0.2428	-1.1301-1.6164
Black	-0.4131	0.2131	-2.0553-0.8578	-0.4615	0.2301	-1.8931-0.9701
Number of Students Testing						
All	3364	6538	95-121,907	3399	6549	95-122,066
Hispanic	770	3265	20-77,810	772	3271	20-77,932
White	1665	2041	21-23,733	1662	2040	21-23,728
Black	626	1530	20-22,636	625	1530	20-22,678
Counties		1010			1010	
Observations		23,521			23,521	

All test score calculations precision-weighted.

Figure 1.4 shows the number of removals resulting from Secure Communities for each county between October 2008 and September 2013. Although a few areas had high numbers of removals associated with the program, the majority of counties had fewer than 100 removals between 2008 and 2013. High levels of removals were concentrated in more populous areas; high levels of removals were also more common in southern and western states. The 49 counties with over 1000 removals during this time period were in California, Arizona, Texas, Florida, Georgia, Nevada, North Carolina, Utah, Virginia, Oklahoma, and Tennessee, with the majority in California and Texas.

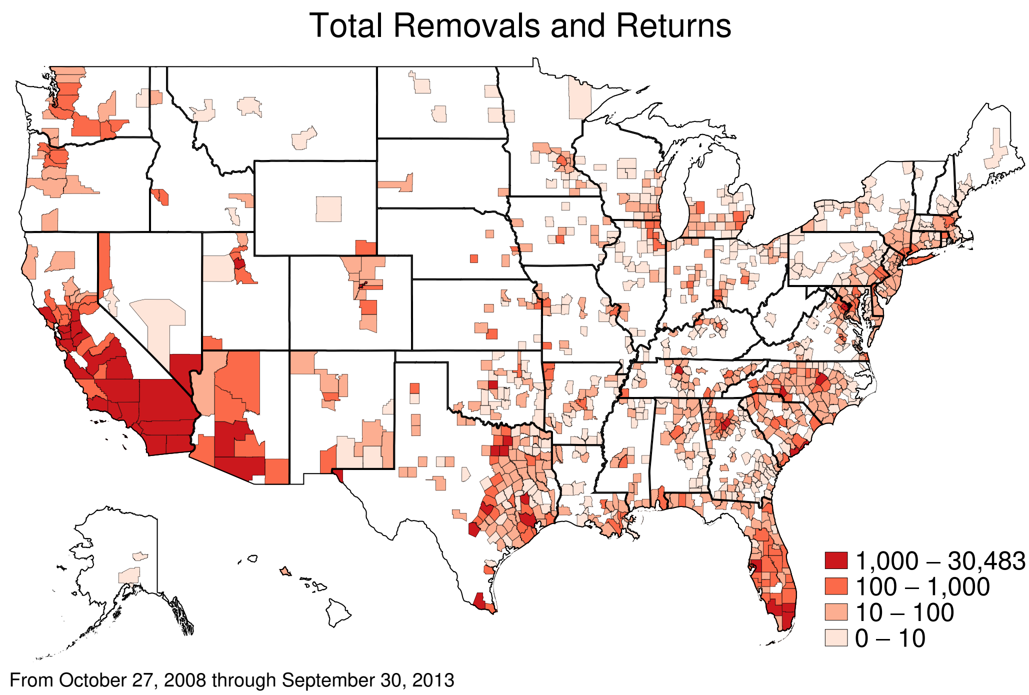

Figure 1.4: Removals Associated with Secure Communities
Source: Immigration and Customs Enforcement. Secure Communities: Monthly statistics through September 30, 2013. U.S. Department of Homeland Security.

1.6.2 Main Findings

As shown in Table 1.2, I find that the activation of Secure Communities reduced average achievement for Hispanic students in English Language Arts (ELA). I find no change to average achievement for Hispanic students in math. The activation of Secure Communities decreased academic achievement in ELA for a county's Hispanic students by approximately 0.88 percent of a standard deviation. This is a small effect: Hispanic students score over 30 percent of a standard deviation below the average for all students in ELA.

Table 1.2: Effect of Secure Communities on Average Achievement

Variables	(1) Hispanic ELA	(2) Hispanic Math	(3) White ELA	(4) White Math	(5) Black ELA	(6) Black Math
Secure Communities	-0.0088**	-0.0067	-0.0022	-0.0002	-0.0122***	-0.0082*
	(0.0043)	(0.0045)	(0.0023)	(0.0033)	(0.0039)	(0.0047)
Active 287(g) Agreement	0.0001	0.0120	0.0001	0.0133	-0.0009	-0.0026
	(0.0097)	(0.0133)	(0.0081)	(0.0090)	(0.0086)	(0.0111)
Observations	23,521	23,521	23,521	23,521	23,521	23,521
R-squared	0.8378	0.8100	0.9034	0.8851	0.8203	0.7955

Precision-weighted regressions control for grade, year, state-by-year, and county fixed effects
Robust standard errors, clustered at the county-level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.2 also presents results for non-Hispanic white and black students. The activation of Secure Communities also reduced non-Hispanic black students' average achievement in ELA by 1.2 percent of a standard deviation. Although results are only marginally significant, the activation of Secure Communities is also associated with a decline in math achievement for non-Hispanic black students. Secure Communities does not significantly impact the achievement of white students in either ELA or math.

1.6.3 Robustness Checks

Sampling Decisions

In main models, I restrict to counties that have average achievement measures for Hispanic, white, and black students, which excludes a large number of counties primarily because of the smaller number of counties with at least 20 black students testing in a grade-year observation. This limits the generalizability of results. In Table 1.3, I show results for a larger set of counties, which have average achievement measures for both Hispanic and non-Hispanic white students. I reach similar results, finding a decrease in ELA achievement for Hispanic students of about 0.80 percent of a standard deviation, with no effect of Secure Communities on the achievement of non-Hispanic white students. Similarly, in Table 1.4, I show results for all counties with measures of either math or ELA achievement. Here, results for Hispanic students are not as precisely measured, but I continue to find that activation of Secure Communities decreased ELA achievement by 0.70 percent of a standard deviation. I also continue to find similar results for black students, with Secure Communities reducing black students' ELA achievement by 1.1 percent of a standard deviation. Taken together, these results show that the set of findings in the main models are generalizable to the larger set of counties.

Table 1.3: Effect of Secure Communities on Average Achievement in Counties with Hispanic and White Test Scores

Variables	(1) Hispanic ELA	(2) Hispanic Math	(3) White ELA	(4) White Math
Secure Communities	-0.0080**	-0.0055	-0.0033	-0.0012
	(0.0038)	(0.0041)	(0.0022)	(0.0030)
Active 287(g) Agreement	-0.0017	0.0129	0.0009	0.0142*
	(0.0099)	(0.0130)	(0.0077)	(0.0082)
Observations	35,014	35,014	35,014	35,014
R-squared	0.8047	0.7697	0.8893	0.8714

Precision-weighted regressions control for grade, year, state-by-year, and county fixed effects
Robust standard errors, clustered at the county-level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.4: Effect of Secure Communities on Average Achievement in All Counties

Variables	(1) Hispanic ELA	(2) Hispanic Math	(3) White ELA	(4) White Math	(5) Black ELA	(6) Black Math
Secure Communities	-0.0070*	-0.0044	-0.0029	-0.0012	-0.0105***	-0.0088*
	(0.0039)	(0.0041)	(0.0022)	(0.0030)	(0.0040)	(0.0047)
Active 287(g) Agreement	-0.0021	0.0126	0.0009	0.0145*	-0.0006	-0.0024
	(0.0098)	(0.0129)	(0.0077)	(0.0082)	(0.0088)	(0.0111)
Observations	35,938	35,915	35,285	35,275	23,803	23,746
R-squared	0.8025	0.7662	0.8891	0.8709	0.8187	0.7936

Precision-weighted regressions control for grade, year, state-by-year, and county fixed effects
Robust standard errors, clustered at the county-level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Alternative Weighting Schemes

In main models, I use weighted least squares (WLS) to account for measurement error in the dependent variable. This produces more efficient parameter estimates than ordinary least squares (OLS) but risks producing misleading standard errors (Lewis and Linzer, 2005). Although this concern is reduced because I cluster standard errors at the county-level, I check for the robustness of estimates to alternative weighting schemes. The precision of average achievement measures is largely but not entirely determined by the number of Hispanic, white, or black students in a county-year-grade observation. I alternatively weight models by the percent of the total student population in that county out of the entire total student population for that year-grade observation (Table 1.5) and the percent of the Hispanic student population in that county out of the entire Hispanic student population for that year-grade observation (Table 1.6). Using both of these alternate weighting schemes, Secure Communities continues to decrease achievement for Hispanic in ELA and non-Hispanic black students in ELA and math but also now appears to have negative and large effects on the achievement of white students in ELA. I also show results without any weights (Table 1.7). I find no effects of Secure Communities on achievement in these models, suggesting that results in other models are being driven by counties with larger populations of Hispanic and non-Hispanic black students.

Table 1.5: Weighting by the Proportion of Students in the County Out of Total Students

Variables	(1) Hispanic ELA	(2) Hispanic Math	(3) White ELA	(4) White Math	(5) Black ELA	(6) Black Math
Secure Communities	-0.0088*	-0.0075	-0.0105***	-0.0079	-0.0153***	-0.0103*
	(0.0051)	(0.0048)	(0.0036)	(0.0057)	(0.0056)	(0.0055)
Active 287(g) Agreement	0.0116	0.0195	0.0107	0.0194*	0.0103	0.0143
	(0.0106)	(0.0130)	(0.0107)	(0.0106)	(0.0086)	(0.0105)
Observations	23,521	23,521	23,521	23,521	23,521	23,521
R-squared	0.8112	0.7944	0.9158	0.8965	0.7947	0.7888

Regressions control for grade, year, state-by-year, and county fixed effects
Robust standard errors, clustered at the county-level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.6: Weighting by the Proportion of Hispanic Students in the County Out of Total Hispanic Students

Variables	(1) Hispanic ELA	(2) Hispanic Math	(3) White ELA	(4) White Math	(5) Black ELA	(6) Black Math
Secure Communities	-0.0176***	-0.0169**	-0.0177***	-0.0169*	-0.0215**	-0.0203**
	(0.0062)	(0.0068)	(0.0053)	(0.0093)	(0.0101)	(0.0093)
Active 287(g) Agreement	0.0153	0.0346**	0.0265**	0.0359**	0.0137	0.0220**
	(0.0129)	(0.0165)	(0.0134)	(0.0149)	(0.0092)	(0.0100)
Observations	23,521	23,521	23,521	23,521	23,521	23,521
R-squared	0.8682	0.8576	0.9219	0.8986	0.7922	0.7907

Regressions control for grade, year, state-by-year, and county fixed effects
Robust standard errors, clustered at the county-level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.7: Effect of Secure Communities on Average Achievement Without Weights

Variables	(1) Hispanic ELA	(2) Hispanic Math	(3) White ELA	(4) White Math	(5) Black ELA	(6) Black Math
Secure Communities	0.0017	0.0020	-0.0029	-0.0024	-0.0027	0.0026
	(0.0051)	(0.0055)	(0.0028)	(0.0039)	(0.0044)	(0.0053)
Active 287(g) Agreement	0.0028	-0.0145	-0.0022	-0.0107	0.0056	-0.0047
	(0.0116)	(0.0161)	(0.0081)	(0.0146)	(0.0098)	(0.0134)
Observations	23,521	23,521	23,521	23,521	23,521	23,521
R-squared	0.6898	0.6551	0.8649	0.8326	0.6812	0.6542

Regressions control for grade, year, state-by-year, and county fixed effects
Robust standard errors, clustered at the county-level, in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Altering Time-Varying Controls

In main models, beyond fixed effects, I control for the time-varying size of the total and Hispanic populations in a particular grade, year, and county. Results are mostly robust to dropping these controls, controlling for the percent of students Hispanic and black rather than the total Hispanic student population, and using the natural log of the total and Hispanic populations as controls (not shown). Results are also robust to controlling for the unemployment rate during the past school year (not shown).

1.6.4 Potential Mechanisms

The activation of Secure Communities might affect average achievement by either affecting students' performance on tests or changing the composition of students within schools. To determine whether Secure Communities affects student composition, I substitute the log of the number of students who take the test as the outcome variable and estimate similar models. As shown in Table 1.8, the activation of Secure

Communities had no effect on the number of Hispanic students testing in either ELA or math. However, the activation of Secure Communities is negatively associated with the number of non-Hispanic black students testing in both ELA and math. I estimate the same models using CCD data, which allows me to include more years of data prior to Secure Communities activation. While not precisely measured, the estimated effect of Secure Communities on black enrollment is large and negative in model (5), which uses data from the same years available in SEDA (2008-2009 through 2012-2013). However, when I add more years of data (2003-2004 through 2013-2014), that effect disappears. Therefore, it seems unlikely that decreasing enrollment for black students is affecting black achievement in ELA or math.

If Secure Communities affected performance on exams, one mechanism through which it likely operated was by increasing stress in a community. I would expect stress to increase as removals increase within a community. Table 1.10 presents models using the rate of removals of the foreign-born Hispanic population as the key predictor of interest. Increases in removals within a county are associated with reduced average achievement in ELA for both Hispanic and non-Hispanic black students. A one percentage point increase in removals in a county decreased average Hispanic achievement in ELA and math by about two-thirds of a standard deviation. A one percentage point increase in removals also decreased average achievement in ELA for non-Hispanic black students by 0.47 percent of a standard deviation.

Table 1.8: Effect of Secure Communities on Number of Hispanic, Black, and White Students Using SEDA

Variables	(1) Hispanic ELA	(2) Hispanic Math	(3) White ELA	(4) White Math	(5) Black ELA	(6) Black Math
Secure Communities	0.0002	0.0014	-0.0019	-0.0014	-0.0104**	-0.0126***
	(0.0065)	(0.0064)	(0.0025)	(0.0024)	(0.0047)	(0.0047)
Active 287(g) Agreement	0.0210*	0.0191	0.0003	-0.0008	-0.0267*	-0.0243*
	(0.0120)	(0.0126)	(0.0121)	(0.0122)	(0.0137)	(0.0129)
Observations	23,521	23,521	23,521	23,521	23,521	23,521
R-squared	0.9922	0.9921	0.9967	0.9957	0.9932	0.9931

Regressions control for grade, year, state-by-year, and county fixed effects
Robust standard errors, clustered at the county-level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.9: Effect of Secure Communities on Number of Hispanic, Black, and White Students Using CCD

Variables	(1) Hispanic	(2) Black	(3) White	(4) Hispanic	(5) Black	(6) White
Secure Communities	0.0098	0.0001	0.0055	0.0013	-0.0224	-0.0017
	(0.0121)	(0.0155)	(0.0052)	(0.0104)	(0.0140)	(0.0035)
Active 287(g) Agreement	-0.0174	0.1012***	0.0192	0.0164	-0.0300	-0.0036
	(0.0187)	(0.0235)	(0.0134)	(0.0157)	(0.0199)	(0.0111)
Observations	25,344	25,344	25,344	11,520	11,520	11,520
R-squared	0.9866	0.9868	0.9955	0.9918	0.9914	0.9974
Years of Data	2003-2004 to 2013-2014			2008-2009 to 2012-2013		

Regressions control for year, state-by-year, and county fixed effects
Robust standard errors, clustered at the county-level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.10: Effect of Removals on Average Achievement

VARIABLES	(1) Hispanic ELA	(2) Hispanic Math	(3) White ELA	(4) White Math	(5) Black ELA	(6) Black Math
Percent of Removals	-0.0066***	-0.0061**	-0.0019	-0.0024	-0.0047**	-0.0028
	(0.0024)	(0.0030)	(0.0028)	(0.0034)	(0.0022)	(0.0031)
Observations	23,521	23,521	23,521	23,521	23,521	23,521
R-squared	0.8378	0.8100	0.9034	0.8851	0.8202	0.7954

Precision-weighted regressions control for grade, year, state-by-year, and county fixed effects
Robust standard errors, clustered at the county-level, in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Higher numbers of removals could indicate that law enforcement was cooperating with ICE by honoring detainers issued. Although I do not observe how many detainers were honored per county, I do observe both fingerprint match and removal counts, which allows me to construct the rate of removals per fingerprint match. Counties that have higher rates of removals per fingerprint match likely have higher cooperation rates with ICE (Pedroza, 2018b). Removals do not occur immediately when an individual is identified; I therefore use cumulative measures (from 2008 through 2013) of both removals and fingerprint matches and calculate the rate of removals per fingerprint match through 2013. This cumulative approach means that I can no longer control for county and year fixed effects; however, I continue to use grade fixed effects and instead control for 2009 test scores to account for prior achievement in that county. Although evidence is only suggestive, Table 1.11 shows that counties with higher rates of removals per fingerprint match over the course of Secure Communities experienced larger declines in ELA test scores by 2012-2013. If a county were to move from 0 percent of matches removed to 100 percent of matches removed, test scores for Hispanic students in ELA are predicted to decline by 13 percent of a standard deviation.

Table 1.11: Association Between Local Cooperation with ICE and Test Scores

	(1) Hispanic ELA	(2) Hispanic Math	(3) White ELA	(4) White Math	(5) Black ELA	(6) Black Math
% Matches Removed	-0.1279***	0.0985	-0.0307	-0.0094	-0.0326	0.0634
	(0.0467)	(0.0616)	(0.0330)	(0.0448)	(0.0655)	(0.0681)
Obs.	3,690	3,690	3,690	3,690	3,690	3,690
R^2	0.6437	0.5739	0.7629	0.7237	0.6088	0.5448

Precision-weighted regressions control for grade fixed effects and 2009 test scores
Robust standard errors, clustered at the county-level, in parentheses
*** p<0.01, ** p<0.05, * p<0.10

1.6.5 Threats to Validity

Check for Prior Trends

It is possible that other changes over time in counties implementing Secure Communities affected students' test scores, unrelated to the rollout of the program. I check for this possibility by running a specification in which I include two leading indicators of Secure Communities. Significant estimates from these regressions would suggest that any effects I previously attributed to the activation of Secure Communities may have been instead the result of differing pre-trends between activating and non-activating counties. As shown in Table 1.12, I have reduced power, and neither leading nor lagging indicators of Secure Communities reach statistical significance. However, coefficients on leading indicators, although not significant at conventional levels, are negative and large in models for black students. This suggests prior negative trends for black students that could bias estimates of effects of Secure Communities' activation. In particular, it appears that black students' scores were already declining during this time period, prior to the initiation of Secure Communities.

Table 1.12: Check for Prior Trends

VARIABLES	(1) Hispanic ELA	(2) Hispanic Math	(3) White ELA	(4) White Math	(5) Black ELA	(6) Black Math
2 Years Prior SC	0.0023	0.0061	-0.0036	-0.0074	-0.0004	0.0017
	(0.0114)	(0.0105)	(0.0047)	(0.0068)	(0.0099)	(0.0113)
1 Year Prior SC	0.0012	0.0019	-0.0083	-0.0082	-0.0049	0.0001
	(0.0140)	(0.0137)	(0.0067)	(0.0095)	(0.0136)	(0.0154)
SC Activated	-0.0074	-0.0041	-0.0112	-0.0097	-0.0172	-0.0078
	(0.0151)	(0.0160)	(0.0076)	(0.0113)	(0.0142)	(0.0172)
Active 287(g) Agreement	0.0003	0.0126	0.0007	0.0132	-0.0004	-0.0024
	(0.0098)	(0.0134)	(0.0081)	(0.0090)	(0.0085)	(0.0111)
Observations	23,521	23,521	23,521	23,521	23,521	23,521
R-squared	0.8378	0.8100	0.9035	0.8851	0.8203	0.7955

Precision-weighted regressions control for grade, year, state-by-year, and county fixed effects
Robust standard errors, clustered at the county-level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Endogeneity of Rollout

As previously stated, prior work finds that the timing of Secure Communities' activation was related to several county-level characteristics, including location along the border with Mexico, activation of a 287(g) program, share of the population that is Hispanic, and overall size of the population (Cox and Miles, 2013). In predicting the rollout of Secure Communities, Cox and Miles (2013) also include controls for location on the Gulf of Mexico, fraction of the population non-citizen, violent crime rate, property crime rate, income per capita, fraction in poverty, fraction of vote for Republican candidate in 2004, and count of local anti-immigrant legislation.

Table 1.13: Predicting Activation of Secure Communities

	(1)	(2)	(3)
Border County	3.6590***	3.7798***	3.7931***
	(0.8955)	(0.9258)	(0.9368)
Gulf County	1.1201	1.0971	1.1025
	(0.1966)	(0.1934)	(0.1952)
Percent Hispanic (2000)	2.2930**	2.3184***	2.4439***
	(0.7459)	(0.7532)	(0.8049)
Percent Noncitizen (2000)	0.7987	0.7256	0.7282
	(0.7422)	(0.6764)	(0.7839)
Percent Black (2000)	0.5570**	0.5838**	0.6244*
	(0.1341)	(0.1407)	(0.1533)
Logged Violent Crime Rate (2007)	1.0271	1.0274	1.0259
	(0.0338)	(0.0338)	(0.0338)
Logged Property Crime Rate (2007)	1.0071	1.0076	1.0039
	(0.0341)	(0.0342)	(0.0340)
Logged Population (2000)	1.2183***	1.2065***	1.1958***
	(0.0265)	(0.0265)	(0.0268)
Logged Per Capita Income (2001)	0.9762	0.9789	0.9812
	(0.0212)	(0.0213)	(0.0215)
Percent in Poverty (2000)	0.2245***	0.2398***	0.1812***
	(0.1174)	(0.1252)	(0.0982)
Percent Voting for Republican in 2004	0.6667*	0.6532*	0.6784*
	(0.1497)	(0.1466)	(0.1560)
287(g) Approved	3.0268***	3.1958***	3.1883***
	(0.4653)	(0.4964)	(0.4957)
287(g) Denied		1.4389**	1.4531***
		(0.2033)	(0.2064)
287(g) Pending		11.0146**	10.6757**
		(11.1188)	(10.7860)
287(g) Withdrew		1.2993	1.3047
		(0.2535)	(0.2546)
Change in Fraction Hispanic, 2000-2009			0.7971
			(0.9974)
Change in Fraction Black, 2000-2009			5.8647
			(8.9011)
Change in Fraction in Poverty, 2000-2009			4.7748*
			(4.2578)
Observations	3,142	3,142	3,142
State Fixed Effect	Yes	Yes	Yes

The table reports hazard ratios, with standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

I am unable to reproduce Cox and Miles (2013) exactly for several reasons. First, I do not have access to counts of local anti-immigrant legislation. Second, the *USA Counties* file Cox and Miles (2013) used is no longer available online; instead, I use data from USA Counties: 2011. Unfortunately, USA Counties: 2011 does not contain data for Puerto Rico or other territories, which I believe to be included in Cox and Miles (2013). Third, the coding of several variables is unclear. However, with these limitations, I estimate similar models predicting Secure Communities activation.[3]

As shown in Table 1.13, I also find that the timing of Secure Communities is correlated with location along the border with Mexico, percent of the population identified as Hispanic, total population, and active 287(g) agreements. However, the percent of the population identified as black and the percent of votes for a Republican candidate in 2004 are also correlated with later Secure Communities activation. Finally, I detect a relationship between percent of the population in poverty and later Secure Communities activation. This suggests that there may be other key county differences correlated with timing of activation that could also affect other county trends during this period.

I examine not only the relationship between timing of activation and prior 287(g) agreement but also the relationship between timing of activation and application for a 287(g) agreement. In models (2) and (3), I control not only for approved 287(g) agreements but also add indicators for counties that applied but were denied approval for a 287(g) program, applied but later withdrew the application for a 287(g) program, and had a pending application for a 287(g) program (with as of yet unresolved application status) (Pedroza, 2018a). I identify counties using only applications prior to the beginning of the Secure Communities rollout. I find that counties that applied

[3]Results using demographic information from other years or sources reach similar results and are available upon request.

for but were denied participation in 287(g) programs or had applications pending were likely to have Secure Communities activated earlier. This result suggests that there may be other factors known to ICE about county preferences for immigration enforcement that predict activation of Secure Communities. An early rollout of Secure Communities might reflect a strong desire by county officials to cooperate with ICE, and any effect of Secure Communities on student achievement may be driven by a county's interest in cooperating with ICE.

Another threat to validity would be if the rollout of Secure Communities was correlated with other county trends unrelated to the program. Cox and Miles (2013) also find no relationship between the change in the share of the population identified as Hispanic and timing of Secure Communities activation. In model (3), I reach similar results but find that areas that increased in the share of the population in poverty between 2000 and 2009 activated Secure Communities earlier. This suggests that counties hardest hit by the Great Recession may have also experienced earlier activation of Secure Communities. However, as previously stated, results in my main models are not altered by controlling for the county unemployment rate.

Varying Treatment of 287(g) Programs

In contrast to Secure Communities, to participate in 287(g) programs, local law enforcement had to apply and be approved by ICE. ICE then provided training, and subsequently local law enforcement were empowered to act as immigration agents. Although I control for 287(g) program activation in main models, this likely does not sufficiently account for endogeneity in the rollout of Secure Communities related to 287(g) programs because I also find that any county applying for a 287(g) program was likely to activate Secure Communities earlier. As described in further detail in chapters 2 and 3, counties may apply for 287(g) programs because of trends that are related to immigration, such as increasing levels of anti-immigrant animus or increasing crime levels in immigrant communities. These trends are likely to also affect student achievement. To examine the possibility that changing attitudes or actions towards immigrants are driving the results, I alternatively exclude all counties that were ever approved for a 287(g) agreement (Table 1.14) or that ever applied for a 287(g) agreement (Table 1.15). Effects of Secure Communities on achievement no longer reach conventional levels of statistical significance.

Table 1.14: Effect of Secure Communities on Average Achievement Excluding All Counties Approved for 287(g) Programs

Variables	(1) Hispanic ELA	(2) Hispanic Math	(3) White ELA	(4) White Math	(5) Black ELA	(6) Black Math
Secure Communities	-0.0070	0.0004	0.0009	0.0030	-0.0071*	0.0004
	(0.0043)	(0.0048)	(0.0024)	(0.0035)	(0.0042)	(0.0051)
Observations	22,066	22,066	22,066	22,066	22,066	22,066
R-squared	0.8305	0.8012	0.8990	0.8805	0.8081	0.7807

Precision-weighted regressions control for grade, year, state-by-year, and county fixed effects
Robust standard errors, clustered at the county-level, in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.15: Effect of Secure Communities on Average Achievement Excluding All Counties that Ever Applied for 287(g) Programs

Variables	(1) Hispanic ELA	(2) Hispanic Math	(3) White ELA	(4) White Math	(5) Black ELA	(6) Black Math
Secure Communities	-0.0068	-0.0021	-0.0012	0.0029	-0.0061	-0.0002
	(0.0043)	(0.0055)	(0.0026)	(0.0041)	(0.0044)	(0.0056)
Observations	19,512	19,512	19,512	19,512	19,512	19,512
R-squared	0.8319	0.8022	0.8934	0.8720	0.8041	0.7764

Precision-weighted regressions control for grade, year, state-by-year, and county fixed effects
Robust standard errors, clustered at the county-level, in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

1.7 Discussion

In prior work, immigration enforcement in the form of worksite raids negatively affects children's performance in schools (Capps et al., 2007; Zuniga, 2018); parental unauthorized status and experiences with immigration enforcement have also been associated with parental reports of lower academic achievement (Brabeck and Xu, 2010; Brabeck et al., 2015). This study builds on these prior findings by being the first to examine the effects of the Secure Communities program, a nationwide immigration enforcement policy, on student achievement. I find that immigration enforcement decreases average Hispanic achievement in ELA, as well as average non-Hispanic black achievement. These decreases are small, at around one percent of a standard deviation.

These findings build on prior work in multiple ways. First, this paper is the first to use administrative test score data for all counties across the United States to examine the effects of immigration enforcement on student achievement. Second, I use the rollout of Secure Communities and control for consistent characteristics of counties that might be correlated with lower student achievement. Studies that merely compare the educational achievement of children exposed to immigrant enforcement with the educational achievement of children not exposed may conflate the effects of immigration enforcement with the effects of other forms of disadvantage. Students with unauthorized parents are more vulnerable in multiple ways and may perform below students with authorized immigrant or U.S.-born parents because of these other sources of disadvantage. Similarly, removals are not a random process: for example, individuals are more likely to be removed if they have contact with the criminal justice system, which might also separately affect student achievement. Therefore, isolating the effects of immigration enforcement policies requires a strategy that controls for pre-existing differences.

It is unclear that Secure Communities would have strong effects within counties in which law enforcement were reluctant collaborators with ICE. I find some evidence for an interaction effect between Secure Communities' activation and cooperation by local law enforcement: first, increases in removals in a county are associated with drops in student achievement in ELA for Hispanic and non-Hispanic black students. The size of the decrease is rather large: moving from 0 percent to 10 percent of the likely affected population (foreign-born Hispanic individuals) would result in drops in achievement of around 6-7 percent of a standard deviation. Second, counties with higher rates of removals per fingerprint match experience larger declines in ELA test scores for Hispanic students. Third, results are not robust to excluding counties that applied to participate in 287(g) programs, suggesting that results are being driven by counties that were interested in collaborating with ICE.

Some of my results suggest that the rollout of Secure Communities was not exogenous, even when accounting for known factors that predict early activation. Although previous work found that an active 287(g) agreement was associated with earlier activation, I find that even applying for a 287(g) program is associated with earlier activation. ICE may have other, unobserved information it leveraged when determining which counties to activate. These preferences may relate to other unobserved county trends that are also affecting achievement during this time period, such as increases in local anti-immigrant animus.

Further evidence of other characteristics trending with Secure Communities are effects on black students. I consistently find that Secure Communities reduced both achievement and enrollment for black students; this decrease is as large as the decrease for Hispanic students. Although some black students also have immigrant family members, this population is not large and unlikely to be driving these results.

I also find that achievement for black students is trending downward in counties prior to Secure Communities activation, suggesting that the timing of activation is correlated with some other county characteristics affecting achievement for black students. I find that Secure Communities was activated earlier in counties increasing in poverty between 2000 and 2009, suggesting that Secure Communities may have been activated first in counties hit harder by the Great Recession. Similarly, in at least one specification, Alsan and Yang (2018) find an association between Secure Communities' activation and increased food stamp usage for black households, as compared with white households. Using more years of prior data could help account for differential trends and possibly provide more reliable estimates of Secure Communities' effects. When I use multiple prior years of enrollment data, I no longer detect this downward trend in black enrollment with the activation of Secure Communities. It is not possible to examine more years of achievement because the data from SEDA do not start until 2008-2009, the year when the rollout of Secure Communities began.

The growing body of literature on effects of Secure Communities contains both intuitive and counterintuitive findings. Two prior papers use the rollout of Secure Communities to estimate effects on safety net participation and labor force participation (Alsan and Yang, 2018; East et al., 2018). These papers reach expected results: Secure Communities reduced participation in SNAP and in the ACA by Hispanic households, and Secure Communities reduced employment by low-skilled, noncitizen men. However, these papers also contain more difficult to explain findings: Secure Communities decreased employment by more skilled, citizen men, and Secure Communities, in one specification, increased SNAP usage by black households. I similarly reach both expected and unexpected results: Secure Communities reduces achievement for Hispanic, but also black, students. The frequency of unexpected findings further suggests that the rollout was correlated with other local trends.

1.8 Conclusion

The Obama administration halted Secure Communities in favor of the Priority Enforcement Program, partially in response to criticism that Secure Communities did not achieve its stated purpose of targeting serious criminal offenders. However, the current administration has revived Secure Communities, as well as proposed redefining criminal alien to include a broader population of immigrants (Capps et al., 2018). Most recently, federal officials have been examining the citizenship of some naturalized citizens for potential fraud, leaving naturalized citizens also vulnerable to removal. In this climate, understanding the multiple impacts of intensified immigration enforcement is important.

Understanding the impacts of immigration enforcement is particularly challenging for several reasons. First, the population most likely affected is hard-to-identify, meaning that effects must be detected using a larger population (for whom effects are more likely to be diffuse). Second, immigration enforcement policies are not randomly assigned to areas, and more intense immigration enforcement is likely correlated with other local characteristics. In this context, the rollout of a national program like Secure Communities, the activation of which was determined by national rather than local priorities, seems plausibly exogenous. However, I present evidence that characteristics of counties other than those previously known were also used by ICE to determine timing of activation and that timing of activation was possibly correlated with counties hit hardest by the Great Recession.

I find some evidence that Secure Communities reduced achievement in ELA for Hispanic students and in ELA and math for black students. Effects depend of the level of cooperation between local law enforcement and ICE, as counties experiencing higher levels of removals have larger decreases in achievement. However, potential endogeneity with the the rollout, as well as my lack of results on achievement in chapter 3, suggest that main results may reflect county characteristics associated with the timing of rollout rather than effects of Secure Communities.

Chapter 2

The Impact of 287(g) Programs on Student Absences in North Carolina

2.1 Introduction

During the past ten years, partnerships between local law enforcement and Immigration and Customs Enforcement (ICE) greatly expanded the reach of immigration enforcement into the U.S. interior. The best known of these partnerships between ICE and local law enforcement, 287(g) programs, give authority to local law enforcement to act as immigration enforcement agents. When asked about their experiences with 287(g) programs, immigrants express high awareness of these programs, describing increases in contact with local law enforcement following program activation (Nguyen and Gill, 2015). In response, unauthorized immigrants, and the Hispanic community more generally, reduce their contact with public institutions to avoid exposing themselves, their friends, or their family members to detection by immigration enforcement. This reduction in contact with public institutions in the wake of immigration enforcement has been conceptualized as a form of administrative burden on families with unauthorized immigrant members, in that these families may be prevented or deterred from accessing services to which they are entitled because of restrictions or fear stemming from immigration enforcement (Heinrich, 2018). Although prior work finds that immigration enforcement consistently decreases affected families' engagement with social services and other public institutions (Hagan et al., 2010; Watson, 2014; Vargas, 2015; Vargas and Pirog, 2016; Alsan and Yang, 2018), less work has examined the effects of immigration enforcement on youth engagement with public institutions.

Most youth have substantial contact with public institutions via public schools; one measure of their engagement with schools is through attendance rates. Beyond its usefulness as a measure of short-term engagement, school attendance also affects a variety of other student outcomes. Each day of increased absence per year is associated with lower achievement (Gershenson et al., 2017, 2019). Chronic absenteeism, defined by the U.S. Department of Education's Office of Civil Rights as missing fifteen or more days per school year, is negatively associated with longer-term student outcomes. Students who are chronically absent for multiple years in early elementary school are less likely to read on grade level by third grade (Ehrlich et al., 2018). Middle school and high school students with high levels of absenteeism have greatly increased risk of dropping out of high school (Balfanz et al., 2007; Schoeneberger, 2012).

This paper adds to the extant literature by examining the effect of immigration enforcement, via the activation of 287(g) programs in North Carolina, on student attendance. In North Carolina, nine counties established 287(g) programs in different years, whereas another 15 counties applied for 287(g) programs but were denied. To isolate the causal effect of 287(g) programs on student attendance, I use a triple difference strategy in which I compare attendance for different groups of students in these two sets of counties before and after activation of 287(g) programs. I find that immigration enforcement increases absences and absence rates for Hispanic students, with suggestive evidence that this effect is driven by high school students and male students. Additionally, I find that immigration enforcement increases the likelihood that students will be chronically absent, a likely sign of school disengagement.

2.2 Background on 287(g) Programs

287(g) programs refer to Section 287(g) of the Immigration and Nationality Act (INA) and were first authorized as part of the 1996 Illegal Immigration Reform and Immigrant Responsibility Act, although the first 287(g) agreement was not implemented until 2002 (Rosenblum and Kandel, 2011). In 287(g) programs, ICE enters into agreements allowing state and local law enforcement to act as immigration enforcement agents. Under these arrangements, ICE provides training and other capacities to state and local law enforcement agents. In return, state and local law enforcement agents are able to question individuals about their immigration status and to issue detainers, or holds of up to 48 hours to transfer individuals into ICE custody. These programs follow two different models: the task-force model and the jail model, as well as a hybrid model that combines the two. In the task-force model, individuals are detained at the point of arrest. In the jail model, individuals are detained after being booked into a jail (Rosenblum and Kandel, 2011). However, particularly in early years of 287(g) programs, there may have been little practical distinction between models: police reports in North Carolina counties, for example, suggest that law enforcement in some counties with jail models questioned individuals about immigration status prior to arrest (Nguyen and Gill, 2010, 2015).

The express purpose of partnerships with local law enforcement agencies is to target criminal aliens, or noncitizens who have committed crimes (Rosenblum and Kandel, 2011). However, critics have noted that many of the individuals who are identified as a result of 287(g) programs are either noncriminals or have only low-level offenses, such as traffic violations. Therefore, it is unclear whether these programs are targeting criminal aliens or unauthorized immigrants, regardless of criminal status.[1]

[1] Unauthorized presence in the United States, absent other factors, is a civil, not criminal, offense.

As a "new destination" for Hispanic immigrants, North Carolina has been at the forefront of immigration enforcement (Nguyen and Gill, 2010). In 2006, Mecklenburg County became one the first U.S. counties to establish a 287(g) agreement. Mecklenburg County was also the first county to implement a "universal" model, in which local law enforcement did not specifically target serious criminal offenders but identified as many unauthorized immigrants as possible (Capps et al., 2011). As shown in Table 2.2, between 2006 and 2009, eight more local law enforcement agencies in North Carolina established 287(g) agreements. Another fifteen counties attempted to establish agreements during this time period but were rejected by ICE (Capps et al., 2011; Potochnick et al., 2016; Rugh and Hall, 2016).

Table 2.1: 287(g) Programs in North Carolina Requested, by Application Status

Law Enforcement Agency	Date Requested	Date Signed	Application Status
Alamance County Sheriff's Office	8/21/2006	1/10/2007	Approved
Alexander County Sheriff's Office	2/9/2007		Denied
Buncombe County Sheriff's Office	8/23/2007		Denied
Bunswick County Sheriff's Office	6/4/2007		Denied
Cabarrus County Sheriff's Office	11/8/2006	8/2/2007	Approved
Carteret County Sheriff's Office	12/3/2007		Denied
Catawba County Sheriff's Office	10/16/2006		Denied
Columbus County Sheriff's Office	6/22/2007		Denied
Cumberland County Sheriff's Office	5/16/2007	6/25/2008	Approved
Duplin County Sheriff's Office	2/7/2007		Denied
Durham Police Department	1/25/2007	2/1/2008	Approved
Gaston County Sheriff's Office	2/3/2006	02/22/2007	Approved
Guilford County Sheriff's Office	3/21/2007	10/15/2009	Approved
Henderson County Sheriff's Office	4/9/2007	06/25/2008	Approved
Iredell County Sheriff's Office	2/23/2007		Denied
Lee County Sheriff's Office	3/15/2007		Denied
Lincoln Sheriff's Office	6/28/2007		Denied
Mecklenburg County Sheriff's Office	11/3/2005	02/27/2006	Approved
New Hanover County (Wilmington Police Department)[2]	4/5/2007		Denied
Randolph County Sheriff's Office	5/10/2007		Withdrew
Stokes County Sheriff's Office	5/16/2007		Denied
Surry County Sheriff's Office	5/1/ 2007		Denied
Union County Sheriff's Office	4/17/2007		Denied
Wake County Sheriff's Office	11/28/2007	06/25/2008	Approved
Yadkin County Sheriff's Office	5/10/2007		Denied

As shown in Table 2.2, the majority of 287(g) programs in North Carolina are "universal" models.[3] In all but Guilford County, fewer than 50 percent of individuals were detained for serious criminal activities (Level 1 or Level 2).[4][5] Indeed, in Cabarrus, Gaston, Henderson, and Mecklenburg, the majority of immigrants processed in Fiscal Year (FY) 2010 were detained for minor traffic violations.

Table 2.2: 287(g) Programs in North Carolina by Immigrants Detained in FY 2010 (10/2009 - 8/2010)

County	Model	Level 1 or Level 2	Traffic	Total Detainers
Alamance	Jail	35.0%	45.1%	237
Cabarrus	Jail	32.0%	60.2%	181
Durham	Task Force	20.5%	0.0%	44
Gaston	Jail	29.4%	61.7%	282
Guilford	Task Force	50.0%	37.5%	8
Henderson	Jail	27.6%	53.7%	203
Mecklenburg	Jail	38.8%	52.4%	1723
Wake	Jail	37.4%	43.3%	1437

In December of 2012, ICE scaled back on 287(g) programs by not renewing any agreements for task force programs.[6] In 2013, ICE created a new MOA, and jail programs continued to operate.

[3]The Cumberland County Sheriff's Office also had a 287(g) program between May 2007 and October 2009, but the MOA was terminated prior to 2010.

[4]Percent of individuals with Level 3 or "Other" charges not shown.

[5]According to 2009 memorandums of understanding, Level 1 offenders are defined as "aliens who have been convicted of or arrested for major drug offenses and/or violent offenses such as murder, manslaughter, rape, robbery, and kidnapping;" Level 2 offenders are defined as "aliens who have been convicted of or arrested for minor drug offenses, and/or mainly property offenses such as burglary, larceny, fraud, and money laundering;" and Level 3 offenders are defined as "aliens who have been convicted of or arrested for other offenses" (Rosenblum and Kandel, 2011).

[6]FY 2012: ICE announces year-end removal numbers, highlights focus on key priorities and issues new national detainer guidance to further focus resources (2012, December 20). Retrieved from `https://www.ice.gov/news/releases/fy-2012-ice-announces-year-end-removal-numbers-highlights-focus-key-priorities-and`.

2.3 Theoretical Framework

Increases in immigration enforcement may affect student attendance through several mechanisms. Although the largest driver of absences is illness, other causes of absences include chronic health conditions, family responsibilities (such as caring for younger siblings or working), transportation issues, housing instability, exclusionary discipline, and bullying, as well as students not wanting to attend school (Balfanz and Byrnes, 2012). Immigration enforcement may increase student absence rates by leading families to disengage with public institutions, increasing parents' reluctance to venture into public spaces, increasing the likelihood of students experiencing poor physical or mental health, and/or making it more likely that students experience exclusionary discipline. However, immigration enforcement may decrease absence rates if immigration enforcement leads students to view schools as safe spaces and/or increases the likelihood that families comply with attendance policies in order to avoid drawing attention to themselves. Although some of these mechanisms would affect all children, others are likely to be particularly salient for children of certain ages or genders.

Under conditions of heightened immigration enforcement, families are less likely to engage with public institutions. Immigration enforcement produces a chilling effect on public benefit receipt, leading families to be less likely to apply or recertify for public benefits to which they are entitled (Watson, 2014; Vargas, 2015; Vargas and Pirog, 2016; Alsan and Yang, 2018). Because unauthorized immigrants are not themselves eligible to receive public benefits, these decreases in benefit receipt are decreases in benefit receipt for their children, who are U.S. citizens. Immigration enforcement also reduces engagement with public safety units, via reductions in crime reporting, and use of other public services, such as libraries and parks (Hagan et al., 2010; Nguyen and Gill, 2015). Immigration enforcement's effects on public benefit receipt, crime reporting, and use of public services all appear to be driven by fear around possible detection by immigration authorities (Nguyen and Gill, 2015; Alsan and Yang, 2018). Schools represent another government institution that could pose a danger to families with unauthorized members: families may fear school authorities' potential cooperation with ICE.[7] Under conditions of increased immigration enforcement, families may be less likely to bring children to schools because of fear of detection by immigration authorities. Similarly, older children, who have greater control over their own school attendance, may be more likely to miss school because of fear of detection by immigration authorities. Older children are also more likely to themselves be immigrants and may fear their own detection.

Immigration enforcement generally increases unauthorized immigrants' reluctance to be in public spaces, including places of employment, in which there is a chance of detection by immigration authorities (Stuesse and Coleman, 2014; Nguyen and Gill, 2015). This may have secondary effects on children's attendance. First, the majority of offenses for which unauthorized immigrants were arrested under partnerships between local law enforcement and ICE were traffic offenses. Unauthorized parents sometimes do not drive in the same car to reduce risk that, in cases of a traffic stop and arrest, their children lose both parents (Nguyen and Gill, 2015). In many states, unauthorized immigrants are not able to apply for driver's licenses. In some areas, local law enforcement set up checkpoints near neighborhoods with high concentrations of Hispanic residents and conducted frequent traffic stops. Unauthorized immigrants could be arrested for driving without a license and questioned about their immigration status. If parents are more afraid to drive children to school, children may end

[7]Several school districts have released statements emphasizing that school personnel would not report students to ICE for this reason; however, recent state law in Tennessee requires school resource officers to cooperate with ICE.

up missing school; this might just be a small increase in marginal absences if, for example, children typically take the bus but are late and miss the bus a few days a year. Second, employment by lower-educated noncitizen men decreases in the wake of increases in immigration enforcement (East et al., 2018). The authors suggest that this effect is via a chilling effect on employment, based in increases in fears over potential detection by immigration enforcement. Older students, particularly male students, may therefore leave school in order to supplement their families' incomes.

Immigration enforcement may increase student absences through negative effects on mental or physical health. In small clinical studies, children with detained or removed parents exhibit higher levels of psychological distress than children with unauthorized parents who have not been detained or removed (Allen et al., 2015; Zayas et al., 2015). Nationally, partnerships between local law enforcement and ICE also increase mental health distress for Latino immigrants (Wang and Kaushal, 2018). Immigration raids have adverse effects on birth outcomes (Novak et al., 2017), as well as adults' self-rated health (Lopez et al., 2017). To my knowledge, no studies have examined the effects of immigration enforcement on children's physical health outcomes, but children might experience worsened physical health through similar mechanisms. In particular, immigration enforcement may reduce access to health care services. Activation of 287(g) programs negatively affects receipt of prenatal care (Rhodes et al., 2015), and pediatricians in California report that patients have skipped appointments because of fears of potential removal.[8] Although American Medical Association (AMA) policy prohibits immigration officials from using medical records to identify individuals for removal, and ICE policy designates health care facilities as sensitive locations, this does not prevent cooperating local law enforce-

[8]Dembosky, April. "Doctors prepare for possible immigration enforcement visits at hospitals." KQED. Retrieved from `https://www.kqed.org/stateofhealth/362633/doctors-prepare-for-immigration-enforcement-visits-at-hospitals`.

ment from conducting arrests near health care facilities.[9] In North Carolina, sheriff's deputies have been reported waiting outside of migrant health clinics (Arriaga, 2017). Illness is the main reason students are absent from school, so even small declines in children's health are likely to increase absenteeism.

Immigration enforcement may also affect child health through reductions in health care coverage. Multiple studies have found that increases in immigration enforcement reduce families' likelihood of healthcare coverage via Medicaid and the Affordable Care Act (ACA) (Vargas, 2015; Alsan and Yang, 2018). Although children with breaks in health insurance coverage are equally likely to receive emergency care as children without breaks, children with breaks in health insurance coverage are less likely to receive preventive care and prescriptions (Olson et al., 2005). Breaks in health insurance coverage are particularly likely to create a detrimental effect for children with chronic conditions. Approximately 29 percent of school-age children currently have chronic conditions, with 8.8 percent suffering from asthma and 7.9 percent suffering from attention deficit disorder (ADD) or attention deficit hyperactivity disorder (ADHD), both of which require ongoing care and medication.[10] Parents report one challenge to asthma management being inconsistent Medicaid or Children's Health Insurance Program (CHIP) coverage (Laster et al., 2009). One cause of chronic absenteeism is chronic health issues, particularly asthma. If immigration enforcement increases the likelihood of breaks in insurance coverage, children with chronic health conditions will be less likely to receive care and more likely to miss school.

[9]ICE's sensitive locations policy states that "enforcement actions are not to occur at or be focused on sensitive locations such as schools, places of worship, unless; (1) exigent circumstances exist; (2) other law enforcement actions have led officers to a sensitive location; or (3) prior approval is obtained from a designated supervisory official.

[10]National Survey of Children's Health. NSCH 2011/12. Data query from the Child and Adolescent HealthMeasurement Initiative, Data Resource Center for Child and Adolescent Health website. Retrieved [08/07/2016]from www.childhealthdata.org.

After 287(g) program activation, students may be more likely to experience exclusionary discipline, resulting in increased absences. After a parent is detained or removed, children exhibit increases in both internalizing behaviors (such as depression and anxiety) and externalizing behaviors (such as aggression) (Allen et al., 2015). Externalizing behaviors are associated with office referrals, which may result in out-of-school suspensions (Mclntosh et al., 2009). Students who are not experiencing parental detention or removal may also experience behavioral changes. In Arizona, Hispanic male students who were more aware of state law SB1070, which required that local police check the immigration status of any suspected unauthorized immigrant, reported lower levels of classroom regulatory behaviors, such as following teacher directions and remaining quiet (Santos et al., 2018). In North Carolina, students are often suspended because of disruptive behavior or insubordination.

Immigration enforcement may also increase exclusionary discipline for Hispanic students via changes in teacher, rather than student, behaviors; this may particularly affect male youth. Partnerships between local law enforcement and ICE, sometimes referred as "crimmigation" policies, possibly increase the association of "Hispanic immigrant" (particularly male Hispanic immigrant) with criminal. If teachers increasingly associate being Hispanic with criminal behavior, they may be more likely to issue office referrals to Hispanic students for behaviors that previously would not result in office referrals. Absences due to out-of-school suspensions, whether those out-of-school suspensions result from changes to student or teacher behaviors (or both), are more likely to be concentrated amongst Hispanic male students. Hispanic female students did not appear to change behaviors based on knowledge of immigration enforcement policies (Santos et al., 2018), and, because Hispanic male immigrants have been the main targets of crimmigation policies, teachers are more likely to associate Hispanic male youth with criminal behavior than Hispanic female youth (Golash-Boza and Hondagneu-Sotelo, 2013).

However, because school attendance is mandatory, students may, conversely, increase attendance under conditions of heightened immigration enforcement. Under conditions of heightened immigration enforcement, even young children in mixed status families avoid breaking minor rules, such as not buckling seat belts, in order to keep their families from facing detection (Dreby, 2012). Although states vary in ages of required school attendance, in all states, compulsory school attendance laws require attendance from, at maximum, age eight to, at minimum, age sixteen.[11] Forty-three states, as well as the District of Columbia, have statewide truancy policies under which excessive unexcused absences result in sanctions for students and families, including fines, court referrals, and even jail time (Conry and Richards, 2018). In North Carolina, ten unexcused absences trigger potential investigations by social services. Excused absences require that parents contact schools, which they may be less willing to do under conditions of heightened immigration enforcement. Therefore, the course of action that brings the least amount of scrutiny and involves the least amount of parental contact with government authorities may be to send students to school, even when students would have otherwise missed school (in the case of illness or other family events).

Finally, students may increase school attendance following heightened immigration enforcement due to schools being perceived as stable environments. ICE designates schools as a sensitive location, meaning that immigration enforcement actions are only supposed to occur at schools under a restricted set of circumstances. Under unstable conditions, schools may serve as more stable environments (Chaudry et al., 2010). Additionally, since families are more likely to experience food insecurity (Po-

[11] National Center for Education Statistics. Table 5.1: Compulsory school attendance laws, minimum and maximum age limits for required free education, by state: 2017. Retrieved from `https://nces.ed.gov/programs/statereform/tab5_1.asp`.

tochnick et al., 2016) and discontinue use of SNAP (Alsan and Yang, 2018), children may be more motivated to attend school to take advantage of free and reduced breakfast and lunch programs.

2.3.1 Interactions with Student Age and Gender

Effects may differ based on student characteristics. Older students are possibly more likely to experience impacts, as they are likely to be better aware of their parents' status and the policy environment. As previously stated, older students may be more likely to miss school in order to help with family finances or to assist their parents with other tasks. Older students who are themselves unauthorized may also be more likely to be worried about their own susceptibility to arrest. If immigration enforcement increases exclusionary discipline, male students are particularly likely to be affected. Externalizing behaviors increased among male, but not among female, students in response to knowledge about harsher immigration enforcement laws (Santos et al., 2018). Similarly, as immigration enforcement primarily targets men, teacher bias is more likely to affect male students, particularly older male students (Golash-Boza and Hondagneu-Sotelo, 2013).

2.4 Prior Research

Prior research on the effects of immigration enforcement on student attendance is limited but reaches mixed conclusions. In one study, first and second generation Kindergarten through third grade immigrant students attend school at higher rates under higher levels of immigration enforcement (Sattin-Bajaj and Kirksey, 2019). However, this relationship is estimated using the number of ICE apprehensions at the nearest Enforcement and Removal Operations (ERO) office; there are only 24 ERO offices that cover relatively large geographic areas. It is unclear that apprehensions are evenly geographically distributed across areas covered and unlikely that apprehensions in Wisconsin should have large effects on children in Kentucky or Kansas, even though all three areas are covered by the Chicago ERO field office. Although authors use hierarchical linear models to account for part of the variation stemming from county and school characteristics, they are using cross-sectional data, and the positive relationship between apprehensions and attendance may reflect more about pre-existing county characteristics that have positive effects on attendance.

In contrast, immigration raids conducted by ICE agents, whether at worksites or in the community, have negative impacts on student attendance. Following worksite raids, children with an arrested parent miss school (Chaudry et al., 2010). Reports of immigration enforcement activities have large immediate effects on daily high school attendance (Kirksey et al., 2019). On average, students miss 2.05 periods of a school day following an immigration enforcement event and are 0.05 percentage points more likely to be chronically absent. Effects are strongest for students who were identified as English language learners, students receiving special education services, and students previously suspended from school.

Results described above may differ because of differences in immigration enforcement policies. ICE raids are a traumatic and salient form of immigration enforcement activity that may produce larger effects than other immigration enforcement policies. Worksite raids, in particular, appear to have large negative effects on student achievement, even when measured at the district level (Zuniga, 2018). In contrast, I find small or null effects of partnerships between local law enforcement and ICE, both 287(g) programs and Secure Communities, on student achievement in grades 3-8 (chapters 1 and 3). Understanding these different forms of impact is important because increases in immigration enforcement in the U.S. interior are primarily due to activity on the part of local law enforcement, not increases in ICE raids. The majority of arrests due to immigration enforcement result from transfers into ICE custody from federal, state, or local custody (TRAC Immigration, 2018). Because of the differing forms that these policies take, they may have differing effects on absences.

Little evidence is available isolating the effects of partnerships between ICE and local law enforcement on student attendance rates. However, prior studies suggest that these policies have negative effects on school engagement. In qualitative studies, fear of parental detention and removal under these policies reduces reported participation in extracurricular educational activities (Hagan et al., 2010; Koball et al., 2015). Using an index of policies, including 287(g) programs and Secure Communities, Amuedo-Dorantes and Lopez (2015) find that intensified immigration enforcement increases dropout rates and grade retention rates among the children of likely unauthorized immigrants. Comparing counties that activated 287(g) programs with counties that applied to participate but were not approved by ICE, Dee and Murphy (2018) find that Hispanic student enrollment declined in activating counties. The authors are unable to distinguish between increases in student mobility and increases in student dropout rates.

This project builds on previous work by examining the effects of immigration enforcement on student attendance. I build on previous work in several ways. First, I examine the effects of a specific type of partnership between ICE and local law enforcement, 287(g) programs. As previously reviewed, differing types of immigration enforcement are likely to have differing effects; the effects of ICE worksite raids are likely to be larger than the effects of partnerships between ICE and local law enforcement. However, understanding these effects is also important because the majority of ICE arrests and removals in the U.S. interior result not from direct ICE activity but from coordination with local law enforcement. Second, I use a quasi-experimental design to recover plausibly causal estimates. This design addresses endogeneity arising from local conditions that would motivate local officials to participate in partnerships with ICE. Third, I use administrative education data covering the entire student population of North Carolina over the course of nearly a decade. Unlike much work in this area, which uses data at a more aggregated level, I am able to track individual students from grade to grade, as well as identify their location prior to the activation of 287(g) programs. I ask how 287(g) programs affect absences for Hispanic students, as compared with white students, and disaggregate effects by grade level and gender. I additionally explore potential mechanisms, including exclusionary discipline and student mobility.

2.5 Data

Data for this paper come from two sources. First, from Immigration and Customs Enforcement (ICE), I use publicly available data on the dates of North Carolina 287(g) programs, as well as more detailed information on historical 287(g) agreements and applications made available to me by Juan Pedroza and Stephanie Potochnick (used in Potochnick et al. (2016); Rugh and Hall (2016); Dee and Murphy (2018)). Second, I match this information with individual-level student data on attendance for the 2003-2004 through 2012-2013 school years from the North Carolina Education Research Data Center (NCERDC), housed at Duke University. NCERDC maintains all of the administrative records on North Carolina public school students that are collected by the state Department of Public Instruction and makes them available to researchers.

2.5.1 Measures

I use information on days absent, as well as days in school membership, which is available for every student in grades 3-12 on a yearly (and later semester) basis from 2003-2004 forward. Although attendance is sometimes recorded for students in grades PK-2 in later years of data, this information is not required to be collected and is unavailable prior to the 2005-2006 school year. I therefore exclude students in pre-Kindergarten through second grade in all analyses. To generate absence rate, I divide students' days absent by days in membership for a given school year. I exclude students who have unrealistic values of days in membership or days absent (more days than in the year).

2.5.2 Defining Student Treatment Status

NCERDC also contains information about student demographics, which I use to define which students are most likely to be affected by immigration enforcement policies (e.g., the "treatment group"). In NCERDC, race and ethnicity are captured in most years by a single categorical variable, in which students are identified as American Indian, Asian-American, black, Hispanic, multiracial, or white (in some years, students are identified as Pacific Islander). In main analyses, I use Hispanic students as the treatment group, with white students as the control group, because immigration enforcement largely affects Hispanic immigrants (Rosenblum and Soto, 2015). I identify students' modal race and/or ethnicity, or the race and/or ethnicity category they are recorded as most frequently across years. Overall, about three percent of students are recorded in multiple race and/or ethnicity categories, with the majority who have more than one race and/or ethnicity recorded in two. This is most common for students who are recorded most frequently as multiracial: approximately

34 percent of students identified most frequently as multiracial have been identified in a different category during another year. However, it is also quite common for American Indian students, 22 percent of whom have been identified as a different race/ethnicity in another year. The three groups that I use in my analyses, Hispanic, white, and black students, have lower rates (all under 3%) of being identified in multiple race/ethnicity classifications.

In a supplementary analysis, I further restrict the likely treatment group to Hispanic students ever identified as Limited English Proficient, which I define using information on students' current Limited English Proficiency (LEP) status and year exiting LEP during years of data examined. There is some evidence that English proficiency may serve as a proxy for parental nativity. Among Mexican-origin kindergarteners, 95.4 percent of first generation and 74.8 percent of second generation students spoke only Spanish or predominantly Spanish at home, whereas 85.2 percent of third generation students spoke only English or predominantly English at home (Reardon and Galindo, 2009).

Families may migrate in response to 287(g) programs (Capps et al., 2011). Using NCERDC data, I am able to track these migrating families, as long as they remain in North Carolina.[12] Therefore, in these models I treat students' county as observed in the 2005-2006 school year as their "permanent county." In robustness checks, I investigate effects for non-migratory students as well as effects based on assigning students to the county observed in the 2004-2005 school year. In mapping students to counties, I use the substantial overlap between North Carolina district boundaries and county lines. I exclude students who were located at charter schools in 2005-

[12]According to the American Community Survey 5-year estimates for 2005-2009, approximately 2 percent of school-aged children living within North Carolina the previous year were living in another state during the following year. This does not include children who moved abroad in the following year.

2006 because I am not able to identify their counties. Between 2003-2004 and 2012-2013, approximately one percent of students in North Carolina (and half a percent of Hispanic students) were located in charter schools in any given year.

2.5.3 Control Variables

I control for certain student characteristics, particularly grade and gender. Gender is collected in every year; however, students may differ in terms of recorded gender across years. I observe that approximately 0.5 percent of students have more than one recorded gender across years. Although I again take students' most frequently recorded gender, these students may be transgender: indeed, recent estimates suggest that about 0.6 percent of the adult population identify as transgender. To my knowledge, the North Carolina Department of Public Instruction collects no information on students' sexual orientation or gender identity.

I also control for other county-level immigration enforcement policies. Between 2008 and 2011, all counties in North Carolina also were activated for Secure Communities, another type of partnership between ICE and local law enforcement. As previously reviewed, Secure Communities required law enforcement agencies to automatically submit fingerprints of arrested individuals to the Department of Homeland Security's (DHS) Automated Biometric Identification System (IDENT). If a match was determined to be a potentially removable alien, ICE might issue a detainer against that individual, or a request to local law enforcement to hold that individual for up to 48 hours for transfer into ICE custody (Kohli et al., 2011; Rosenblum and Kandel, 2011). I control for Secure Communities activation in a particular county school-year using publicly available data on Secure Communities' activation from ICE. In North Carolina, all 287(g) programs were active in approved counties prior to Secure

Communities' activation.

2.5.4 Sample

I restrict my sample to 516,065 unique students in grades 3-12, who are located in an approved or not approved county in the spring of the 2005-2006 school year and whose race/ethnicity are identified most commonly as Hispanic, white, or black. I only include student-year observations if the student has information on spring absences in that year; I therefore exclude student-year observations where the student is only observed in the fall. If students subsequently move (post 2005-2006) to another county in North Carolina, they remain in my sample. However, if students leave North Carolina, I am unable to observe them. The total number of observations used in these analyses is 3,064,752 student-years.

2.6 Analytic Plan

To estimate the effects of increased immigration enforcement via 287(g) programs on student attendance, I first compare counties with a 287(g) agreement and counties that applied but were rejected for a 287(g) agreement. Second, I compare counties pre- and post-activation of the 287(g) program. For counties that were denied participation, I define the post period as beginning one year post application because that roughly approximates implementation timing for counties that were approved for participation. This approach is similar to that used in several prior nationwide studies to estimate the effects of 287(g) programs on food insecurity, foreclosure rates, and school enrollment (Potochnick et al., 2016; Rugh and Hall, 2016; Dee and Murphy, 2018). Four out of nine 287(g) programs ended prior to the final year of my data, 2012-2013: the programs in Cumberland and Guilford discontinued after a single year, and the programs in Durham and Alamance ended in 2013. I continue to treat those counties as activated but show robustness checks in which I drop 2012-2013 entirely (effects remain similar).

In part because I have fewer counties than these previous nationwide studies and less variation in terms of timing of 287(g) program activation, I introduce a third difference comparing students who were most and least likely to be affected by immigration enforcement. In my main specifications, I use Hispanic students as a treatment group, and white students as a control group, for several reasons. First, as a new destination state, the majority of Hispanic students in North Carolina are the children of immigrants. Second, the majority of the unauthorized population is Hispanic, and Hispanic immigrants are particularly targeted for immigration enforcement. An estimated three quarters of unauthorized immigrants in the United States are from Mexico or Central America, and approximately 85 percent individuals removed from the U.S. interior are Hispanic immigrants (Rosenblum and Soto, 2015). Third, again because of its status as a new destination location, the larger Hispanic community in North Carolina may have more connections on average with unauthorized family members, friends, or neighbors than do U.S.-born Hispanic individuals in areas with historically established Hispanic populations, meaning they may be more likely to experience second-hand the effects of immigration enforcement through social ties (Nguyen and Gill, 2015). Fourth, from a practical perspective, race and ethnicity are collected consistently, while nativity is not. Less than one percent of students are missing data on race and ethnicity.

In an alternate specification, I compare Hispanic students ever identified as Limited English Proficient (LEP) to white students never identified as Limited English Proficient (LEP). This characteristic is more likely than race or ethnicity to be influenced by the activation of 287(g) programs: for example, parents may not want their children identified as Limited English Proficient (LEP) out of fear. LEP status also is not collected consistently across years and, when collected, is not collected in a consistent manner. I therefore suspect that, even though the majority of Hispanic students in my sample are identified as ever LEP, I am still underidentifying students as ever LEP.

This triple difference strategy helps to control for trends affecting the achievement of all students in approved counties as well as changes in education across counties for Hispanic students. The activation of 287(g) programs is an endogenous policy change: counties may have wanted to participate in 287(g) programs because of increases in violence in local immigrant communities or because of increases in anti-immigrant animus, both of which would negatively impact Hispanic students' attendance. Therefore, I compare only counties that applied to participate in 287(g) programs in an attempt to control for unobserved factors related to the local county's desire to participate in 287(g) programs. My main specification uses the following equation:

$$\begin{aligned} \mathrm{Y} = \alpha + \beta_1\mathrm{T} + \beta_2 287(\mathrm{g}) + \beta_3\mathrm{Post} + \beta_4\mathrm{T} \times 287(\mathrm{g}) + \beta_5\mathrm{T} \times \mathrm{Post} \\ + \beta_6 287(\mathrm{g}) \times \mathrm{Post} + \beta_7\mathrm{T} \times 287(\mathrm{g}) \times \mathrm{Post} + \beta_m\mathrm{Student} + \phi + \gamma + \eta + \epsilon \end{aligned} \tag{2.1}$$

Here, Y is the number of absences for an individual student in a particular school year or the percent of days absent out of a student's days in school membership during that school year. In all models, T is the treatment group of Hispanic students, with white students serving as the control group; 287(g) is an indicator variable that is 0 if a county applied but was not approved for a 287(g) program and 1 if a county applied and was approved for a 287(g) program; and Post is an indicator variable representing the years after 287(g) implementation (for counties that are rejected from 287(g) programs, Post represents a year or more following 287(g) application). I control for student gender to account for differences in attendance by gender. During this period, Secure Communities, another partnership between ICE and local law enforcement, was activated throughout North Carolina. Secure Communities (as described in chapter 1) may affect student attendance, so I use a time-varying control for its activation. In my preferred set of models, I also use permanent county fixed effects (ϕ); grade fixed effects (γ); and year fixed effects (η). These layers of fixed effects control for time-invariant characteristics of counties that affect attendance, persistent differences in attendance between grades, and any state-wide or national policy changes in a particular year. I use a wild cluster bootstrap (user-written `boottest` in Stata) to account for clustering at the county-level as well as small numbers of counties (Roodman et al., 2018).[13]

In robustness checks, I use modeling approaches other than ordinary least squares (OLS). When the dependent variable is the number of absences, I estimate Poisson as well as negative binomial regressions, as there is substantial overdispersion of the dependent variable. This type of overdispersion frequently results in Type 1 error when using Poisson models (Ryan et al., 2018). Because I reach similar results using all approaches, I use OLS in additional robustness and falsification checks for several reasons: results are easier to interpret; appropriate bootstrapping that accounts for clustered standard errors is more computationally intensive to implement following maximum likelihood (ML) estimation; and `boottest` does not produce confidence intervals following ML approaches.

[13]Merely bootstrapping standard errors results in standard errors that are much too small.

2.7 Results

2.7.1 Descriptive Statistics

As shown in Table 2.3, counties approved for 287(g) programs differ from counties not approved for 287(g) programs on several dimensions. First, counties approved for 287(g) programs have over twice as many students as counties not approved for 287(g) programs. Two of the most populous counties in North Carolina, Mecklenburg and Wake, activated 287(g) programs. As shown in Table 2.7.1, Mecklenburg, in which Charlotte is located, accounts for 16.44 percent of total students and higher shares of both Hispanic and black students. Wake, in which Raleigh is located, accounts for 17.72 percent of total students, with similar shares of white, Hispanic, and black students.

Second, although counties approved and not approved for 287(g) programs have similar percentages of Hispanic students (about 8 percent), a much larger share of students in counties not approved for 287(g) programs are white. Approximately three-quarters of students in counties not approved for 287(g) programs are white, as compared with half of students in counties approved for 287(g) programs. Correspondingly, in counties approved for 287(g) programs, approximately 35 percent of students are black, whereas only 15 percent of students are black in counties not approved. Very few students are identified as Asian-American, American Indian, or multiracial, although a higher share of Asian-American students reside in approved counties. In both sets of counties, the majority of white students are never identified as Limited English Proficient (99%), whereas the majority of Hispanic students are identified as LEP (75-78%).

Table 2.3: Demographic Information on Approved and Denied Counties

	Not Approved	Approved	Total
Hispanic	7.65%	8.29%	8.09%
White	73.04%	50.08%	54.7%
Black	14.71%	34.84%	0.9%
Asian-American*	1.82%	3.48%	2.97%
American Indian*	0.46%	0.49%	0.48%
Multiracial*	2.32%	2.82%	2.66%
Hispanic, Ever LEP	5.93%	6.23%	6.02%
White, Never LEP	72.44%	49.50%	56.59%
Female	49.45%	49.65%	49.59%
Grade 3	4.76%	4.94%	4.88%
Grade 4	6.28%	6.47%	6.41%
Grade 5	7.80%	8.00%	7.94%
Grade 6	9.40%	9.48%	9.45%
Grade 7	10.85%	10.95%	10.92%
Grade 8	12.16%	12.25%	12.22%
Grade 9	14.16%	14.56%	14.43%
Grade 10	13.83%	13.61%	13.68%
Grade 11	11.56%	10.96%	11.15%
Grade 12	9.20%	9.79%	8.91%
Total Student-Year Observations	948,124	2,116,628	3,064,752
Total Unique Students	156,329	359,736	516,065

*I exclude Asian-American, American Indian, and multiracial students in all analyses.

Table 2.4: Percent of Hispanic, White, and Black Students per County

	Hispanic	White	Black	Total
Alamance	5.01%	3.49%	2.65	3.29%
Alexander	0.50%	1.32%	0.15%	0.88%
Buncombe	2.76%	6.05%	1.42%	4.30%
Cabarrus	4.44%	4.82%	2.35%	3.98%
Carteret	0.28%	1.74%	0.30%	1.14%
Catawba	3.74%	4.51%	1.39%	3.65%
Columbus	0.56%	1.19%	1.85%	1.34%
Cumberland	5.29%	4.74%	12.01%	7.09%
Duplin	3.29%	0.95%	1.36%	1.21%
Durham	5.93%	2.05%	9.07%	4.49%
Gaston	2.61%	5.71%	2.96%	4.46%
Guilford	7.07%	7.79%	13.72%	9.81%
Henderson	2.19%	2.57%	0.25%	1.80%
Iredell	2.81%	4.86%	2.01%	3.73%
Lee	3.43%	1.15%	1.05%	1.29%
Lincoln	1.68%	2.57%	0.40%	1.78%
Mecklenburg	21.36%	11.51%	24.47%	16.44%
New Hanover	1.54%	3.96%	3.04%	3.43%
Stokes	0.26%	1.69%	0.19%	1.07%
Surry	2.28%	2.26%	0.24%	1.59%
Union	4.63%	5.96%	2.37%	4.60%
Wake	16.84%	17.80%	16.60%	17.72%
Yadkin	1.48%	1.32%	0.13%	0.93%
Total Student-Year Observations	247,663	1,750,106	875,681	3,064,752
Total Unique Students	42,079	292,486	149,668	516,065

Table 2.7.1 shows average days absent and absence rates for counties by 287(g) approval status. In denied counties, students are absent an average of 7.37 days per year, with an absence rate of 4.42 percent. In approved counties, students are absent slightly more: an average of 7.86 days per year, with an absence rate of 4.80 percent. Overall, over 90 percent of students have at least one absence during a school year, and nearly half of students are absent at least six days. A large number of students are absent ten or more (27%), fifteen or more (13%), or twenty or more days (7%).

Differences begin to emerge between counties by approval status in terms of high numbers of absences: counties approved for 287(g) programs have higher percentages of students who are absent for larger numbers of days. Once I control for student demographics (grade, gender, and race/ethnicity), these differences shrink but are still significant (not shown).

Table 2.5: Descriptive Statistics on Absences, by 287(g) Application Approval

	Not Approved	Approved	Total
Average Days Absent	7.37	7.86	7.71
Average Absence Rate	4.42%	4.80%	4.68%
No Absences	8.29%	8.74%	8.60%
Absent 1 or More Days	91.71%	91.26%	91.40%
Absent 2 or More Days	83.12%	82.20%	82.49%
Absent 3 or More Days	73.90%	72.88%	73.20%
Absent 6 or More Days	48.58%	48.51%	48.53%
Absent 10 or More Days	25.96%	27.18%	26.80%
Absent 15 or More Days	11.88%	13.64%	13.10%
Absent 20 or More Days	5.99%	7.55%	7.07%
Absent 50 or More Days	0.48%	0.87%	0.75%
Total Student-Year Observations	948,124	2,116,628	3,064,752
Total Unique Students	156,329	359,736	516,065

Figures 2.1-2.4 display the differences in absence counts and absence rates between different groups of students within counties approved for 287(g) programs and counties not approved for 287(g) programs. Prior to 287(g) program activation, differences between Hispanic and white students, as well as Hispanic, ever LEP and white, never LEP, follow similar patterns in counties activating and not activating 287(g) programs. In all counties, the difference in days absent appears to be increasing during this period, with white students being absent less frequently than Hispanic students. However, prior to activation of 287(g) programs (or one year post application), this increase was steeper in counties that were not approved.

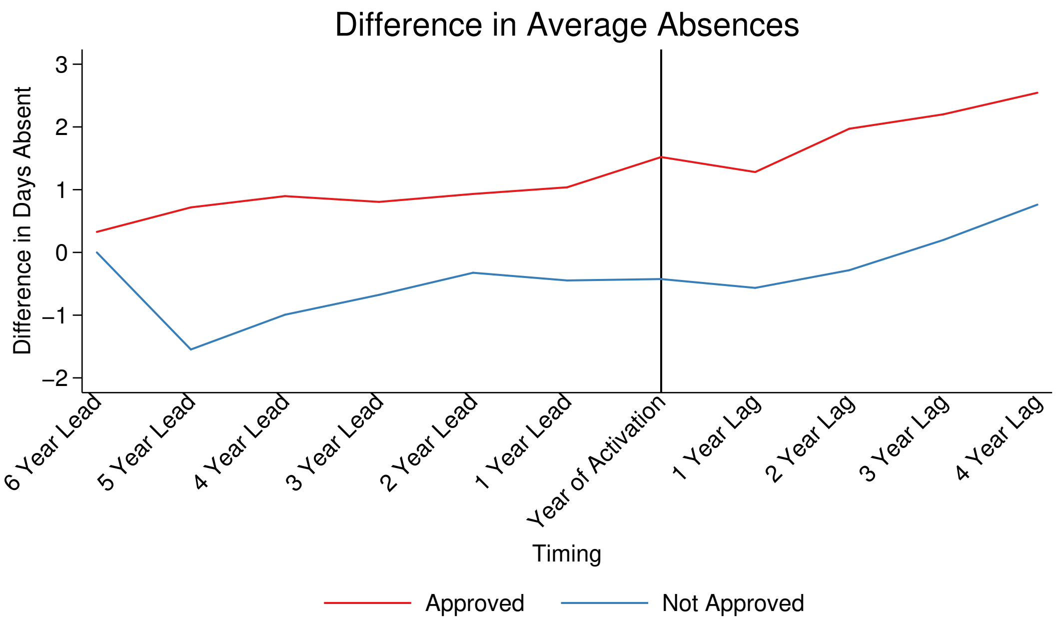

Figure 2.1: Difference in Average Days Absent Between Hispanic and White Students, Comparing Approved and Denied Counties

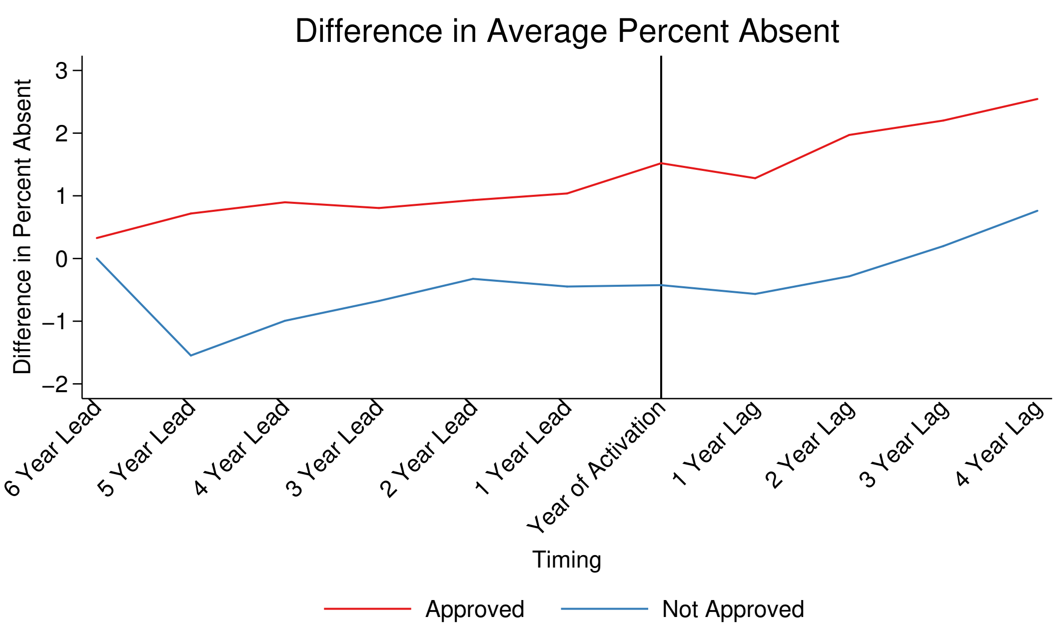

Figure 2.2: Difference in Absence Rate Between Hispanic and White Students, Comparing Approved and Denied Counties

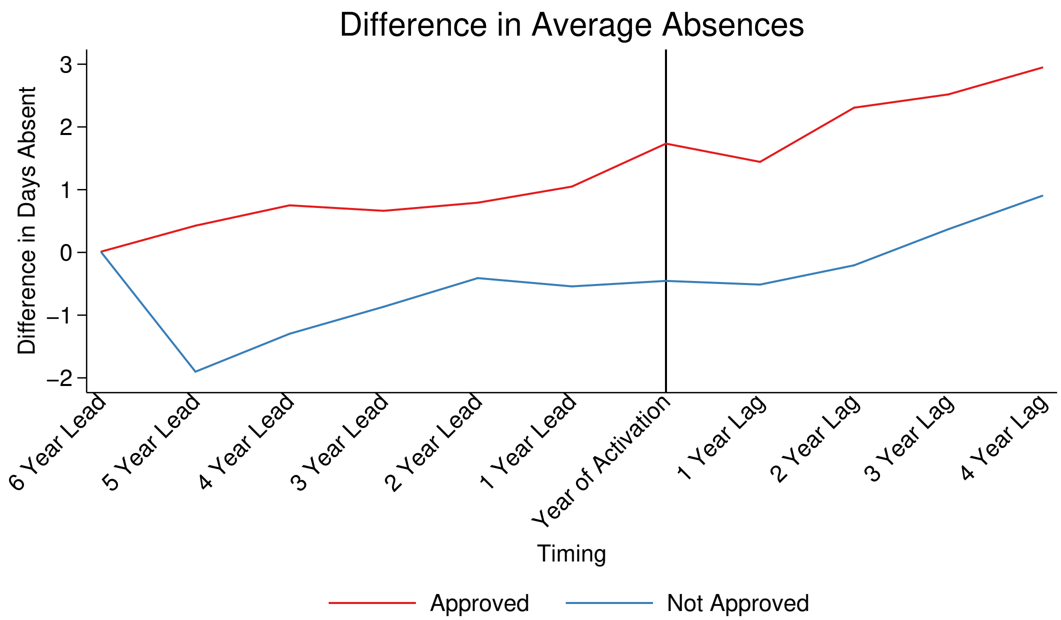

Figure 2.3: Difference in Average Days Absent Between Hispanic, Ever LEP Students and White, Never LEP Students

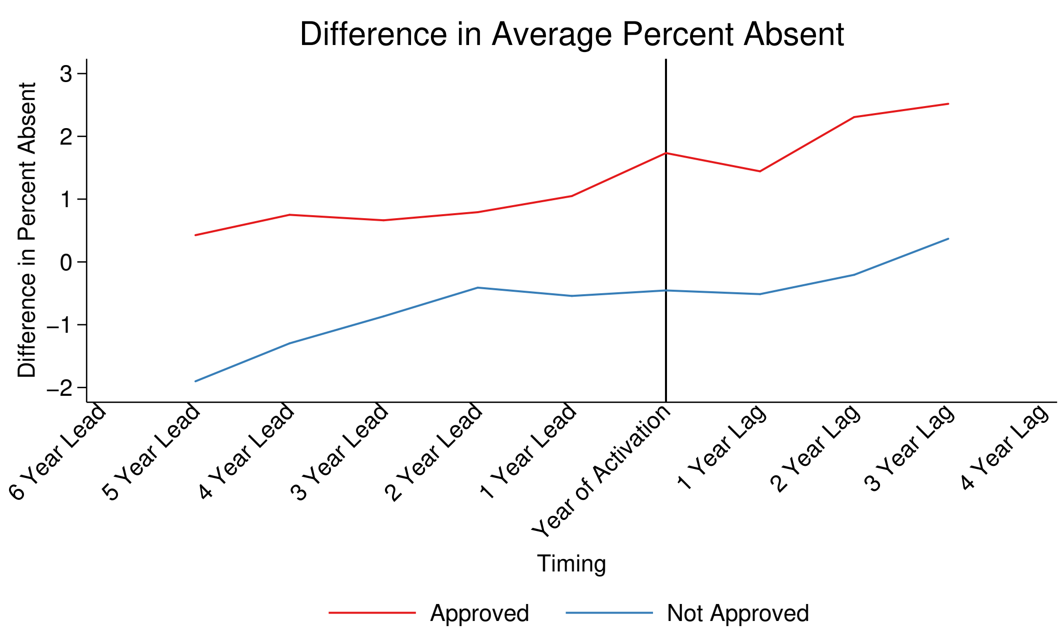

Figure 2.4: Difference in Absence Rate Between Hispanic, Ever LEP Students and White, Never LEP Students

After 287(g) program activation, trends for the differences in absences between Hispanic and white students appear to diverge slightly (Figures 2.1 and 2.2). This pattern is similar when examining the difference between Hispanic students ever identified as LEP and white students never identified as LEP (Figures 2.3 and 2.4).

2.7.2 Main Findings

As shown in Tables 2.6 and 2.7, I find that the activation of 287(g) programs increases the number of days absent for Hispanic students and increases the percent of days per semester Hispanic students are absent, as compared with white students, in approved counties compared to non-approved counties. For Hispanic students, activation of a 287(g) program increases the number of days absent by over half a day per year (80% of a day in my preferred specification) and increases the percent of days absent per semester by approximately 0.3-0.4 percentage points. As shown in Table 2.8, results are similar for days absent when I use Poisson or negative binomial models rather than OLS (although less precisely estimated).

Table 2.6: Effect of 287(g) Programs on Total Absences, Comparing Hispanic to White Students Using OLS Models

Variables	(1) Days	(2) Days	(3) Days
Hispanic (T)	-0.6351***	-0.4910*	-0.2747
	(0.0020)	(0.0501)	(0.2482)
	[-1.1910, -0.1468]	[-1.0743, 0.0016]	[-0.8166, 0.2137]
Approved (A)	-0.0282	0.0136	
	(0.9610)	(0.9840)	
	[-1.2073, 1.3812]	[-1.1634, 1.4807]	
Post (P)	0.3942***	0.1046	0.0957
	(0.0040)	(0.8519)	(0.6456)
	[0.1288, 0.8110]	[-0.9364, 1.5401]	[-0.3056, 0.6134]
T X A	1.5612**	1.5860**	1.3738**
	(0.0300)	(0.0260)	(0.0300)
	[0.1769, 2.7680]	[0.2176, 2.8235]	[0.1457, 2.4582]
T X P	0.6389**	0.5477**	0.5220**
	(0.0190)	(0.0340)	(0.0300)
	[0.2035, 1.0680]	[0.0496, 0.9815]	[0.0606, 0.9247]
A X P	0.1765	0.1961	-0.0446
	(0.5195)	(0.5355)	(0.8058)
	[-0.4130, 0.7454]	[-0.4495, 0.8340]	[-0.4122, 0.3763]
T X A X P	0.6434**	0.6301**	0.8020***
	(0.0190)	(0.0190)	(0.0010)
	[0.1209, 1.1780]	[0.1312, 1.1607]	[0.3116, 1.2870]
Observations	1,997,769	1,997,769	1,997,769
R-squared	0.0053	0.0134	0.0132
Grade and Gender	No	Yes	Yes
Secure Communities	No	Yes	Yes
Year FE	No	Yes	Yes
County FE	No	No	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 2.7: Effect of 287(g) Programs on Percent of Days in Membership Absent, Comparing Hispanic to White Students Using OLS Models

Variables	(1) Percent	(2) Percent	(3) Percent
Hispanic (T)	-0.0029*	-0.0022	-0.0009
	(0.0721)	(0.1491)	(0.5075)
	[-0.0061, 0.0002]	[-0.0056, 0.0009]	[-0.0043, 0.0021]
Approved (A)	0.0003	0.0005	
	(0.9339)	(0.9089)	
	[-0.0065, 0.0091]	[-0.0067, 0.0096]	
Post (P)	0.0016*	0.0005	0.0004
	(0.0651)	(0.8839)	(0.7247)
	[-0.0001, 0.0043]	[-0.0056, 0.0094]	[-0.0020, 0.0033]
T X A	0.0105**	0.0106**	0.0093**
	(0.0200)	(0.0110)	(0.0230)
	[0.0020, 0.0181]	[0.0022, 0.0178]	[0.0014, 0.0164]
T X P	0.0033**	0.0028*	0.0026*
	(0.0450)	(0.0631)	(0.0631)
	[0.0002, 0.0059]	[-0.0002, 0.0053]	[-0.0002, 0.0052]
A X P	0.0009	0.0011	-0.0004
	(0.5736)	(0.5626)	(0.7207)
	[-0.0024, 0.0040]	[-0.0028, 0.0049]	[-0.0025, 0.0019]
T X A X P	0.0033**	0.0032**	0.0043***
	(0.0190)	(0.0240)	(0.0030)
	[0.0005, 0.0060]	[0.0004, 0.0062]	[0.0016, 0.0070]
Observations	1,997,741	1,997,741	1,997,741
R-squared	0.0049	0.0111	0.0108
Grade and Gender	No	Yes	Yes
Secure Communities	No	Yes	Yes
Year FE	No	Yes	Yes
County FE	No	No	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 2.8: Effect of 287(g) Programs on Total Absences, Comparing Hispanic to White Students Using Poisson and Negative Binomial Models

Models	(1) Poisson	(2) Negative Binomial
Hispanic (T)	-0.0424	-0.0530*
	(0.2400)	(0.0800)
Post (P)	0.0137	0.0113
	(0.6100)	(0.5200)
T X A	0.1865**	0.1970***
	(0.0300)	(0.0000)
T X P	0.0760**	0.0830**
	(0.0200)	(0.0400)
A X P	-0.0092	-0.0063
	(0.6000)	(0.7900)
T X A X P	0.0580	0.0605*
	(0.1800)	(0.0400)
Observations	1,997,769	1,997,769
R-squared	0.0248	0.0057
Grade and Gender	Yes	Yes
Secure Communities	Yes	Yes
Year FE	Yes	Yes
County FE	Yes	Yes

P-values (in parentheses) obtained through score cluster bootstrap,
with clustering at the county-level (100 replications)
*** p<0.01, ** p<0.05, * p<0.10

When I compare Hispanic students ever identified as LEP with white students never identified as LEP, I see an increase in days absent of a similar magnitude, about 80 percent of a day per year (Table 2.9). Similarly, estimates for the effects of 287(g) programs on absence rate (about 0.4 percentage points) are comparable to the estimates comparing Hispanic and white students (Table 2.10).

Table 2.9: Effect of 287(g) Programs on Total Absences, Comparing Hispanic, Ever LEP Students to White, Never LEP Students

Variables	(1) Days	(2) Days	(3) Days
Hispanic, Ever LEP (T)	-0.8084***	-0.6200**	-0.4102
	(0.0020)	(0.0320)	(0.1281)
	[-1.4307, -0.2707]	[-1.2083, -0.0472]	[-1.0269, 0.1600]
Approved (A)	-0.0307	0.0111	
	(0.9770)	(0.9810)	
	[-1.2261, 1.4313]	[-1.2114, 1.3795]	
Post (P)	0.3907***	0.1058	0.1031
	(0.0070)	(0.8579)	(0.6126)
	[0.1197, 0.8328]	[-0.9524, 1.6533]	[-0.2936, 0.5835]
T X A	1.6442*	1.7118**	1.6516**
	(0.0591)	(0.0280)	(0.0130)
	[-0.0550, 3.2258]	[0.1437, 3.1646]	[0.4195, 2.9054]
T X P	0.9034***	0.7696**	0.7371**
	(0.0090)	(0.0160)	(0.0170)
	[0.3128, 1.4450]	[0.2093, 1.3285]	[0.1856, 1.2479]
A X P	0.1781	0.1976	-0.0321
	(0.5045)	(0.5616)	(0.8589)
	[-0.3993, 0.7106]	[-0.5405, 0.8500]	[-0.4205, 0.3735]
T X A X P	0.8283**	0.7658**	0.8505***
	(0.0150)	(0.0180)	(0.0040)
	[0.2131, 1.4463]	[0.1573, 1.3563]	[0.2599, 1.4069]
Observations	1,919,057	1,919,057	1,919,057
R-squared	0.0054	0.0132	0.0133
Grade and Gender	No	Yes	Yes
Secure Communities	No	Yes	Yes
Year FE	No	Yes	Yes
County FE	No	No	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 2.10: Effect of 287(g) Programs on Percent of Days in Membership Absent, Comparing Hispanic, Ever LEP Students to White, Never LEP Students

Variables	(1) Percent	(2) Percent	(3) Percent
Hispanic, Ever LEP (T)	-0.0040**	-0.0031*	-0.0019
	(0.0200)	(0.0721)	(0.2523)
	[-0.0075, -0.0005]	[-0.0066, 0.0004]	[-0.0059, 0.0014]
Approved (A)	0.0003	0.0005	
	(0.9239)	(0.8869)	
	[-0.0070, 0.0091]	[-0.0067, 0.0091]	
Post (P)	0.0015**	0.0005	0.0004
	(0.0470)	(0.8939)	(0.7407)
	[0.0000, 0.0040]	[-0.0059, 0.0100]	[-0.0020, 0.0034]
T X A	0.0114**	0.0117**	0.0113***
	(0.0240)	(0.0180)	(0.0060)
	[0.0014, 0.0206]	[0.0019, 0.0205]	[0.0037, 0.0188]
T X P	0.0050***	0.0043**	0.0041**
	(0.0090)	(0.0270)	(0.0270)
	[0.0015, 0.0082]	[0.0008, 0.0076]	[0.0009, 0.0072]
A X P	0.0009	0.0011	-0.0003
	(0.5526)	(0.5485)	(0.8198)
	[-0.0028, 0.0042]	[-0.0027, 0.0049]	[-0.0025, 0.0022]
T X A X P	0.0041**	0.0038**	0.0043**
	(0.0130)	(0.0140)	(0.0110)
	[0.0007, 0.0076]	[0.0005, 0.0072]	[0.0013, 0.0077]
Observations	1,919,030	1,919,030	1,919,030
R-squared	0.0049	0.0109	0.0109
Grade and Gender	No	Yes	Yes
Secure Communities	No	Yes	Yes
Year FE	No	Yes	Yes
County FE	No	No	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

2.7.3 Event History Analysis

My main models assume that the effect of 287(g) programs are consistent over the course of the program, which may not be true for several reasons. First, some programs terminated before the 2012-2013 school year: the programs in Cumberland and Guilford discontinued after a single year, and the programs in Durham and Alamance ended in 2013.[14] Second, individuals may adjust to the reality of a 287(g) program in their community. Conversely, the effect of a 287(g) program may become stronger over the course of the program, as individuals become more aware of immigration enforcement activities within their community. To assess whether 287(g) programs had differential effects based on timing of implementation, I employ an event study analysis in which I interact indicators for my treatment group and approved counties with a series of leading and lagged indicators, with the periods three years or more prior to activation serving as the base group.

As shown in Figures 2.5 and 2.6, I see no effect of 287(g) activation on a one or two year lead of program activation, giving me greater confidence that effects do not arise solely from prior trends in counties activating 287(g) programs. Although effects are not significant at conventional levels once I interact year of activation and year lags with program approval and treatment group, estimates of the effects of year of activation and lagging indicators appear fairly consistent over the course of the program.

[14]I show results in which I continue to count these programs as active in discontinued years. I do this to be consistent with how I count post-periods in counties that did not activate, but I show robustness checks in which I exclude 2013 (when Alamance and Durham had deactivated their 287(g) programs.

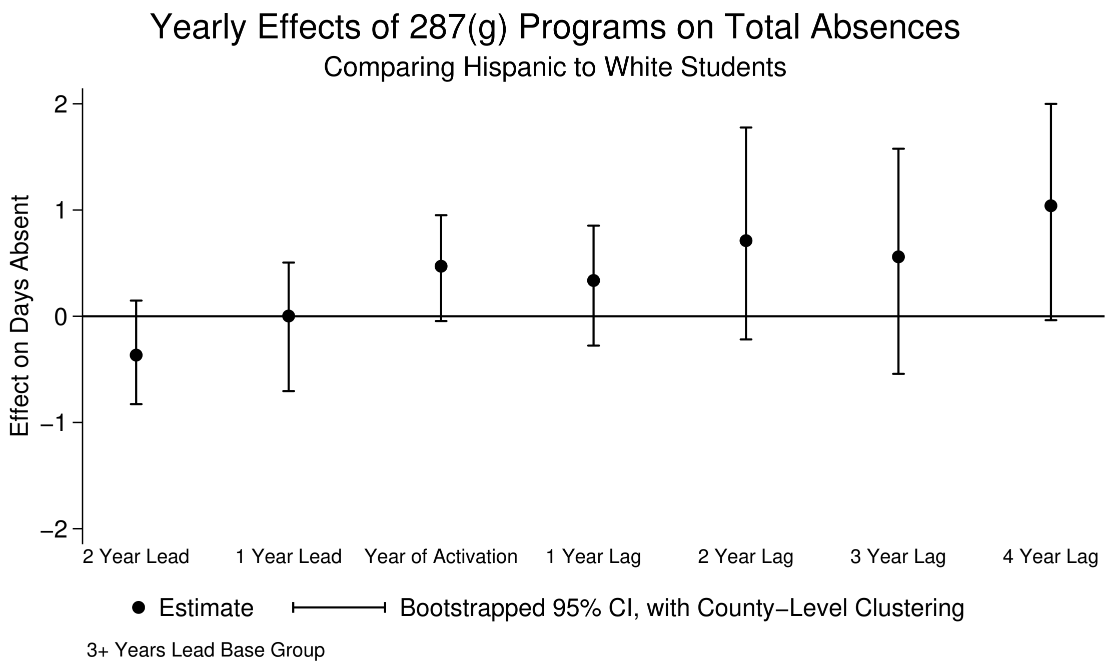

Figure 2.5: Event Study for Effects on Total Absences

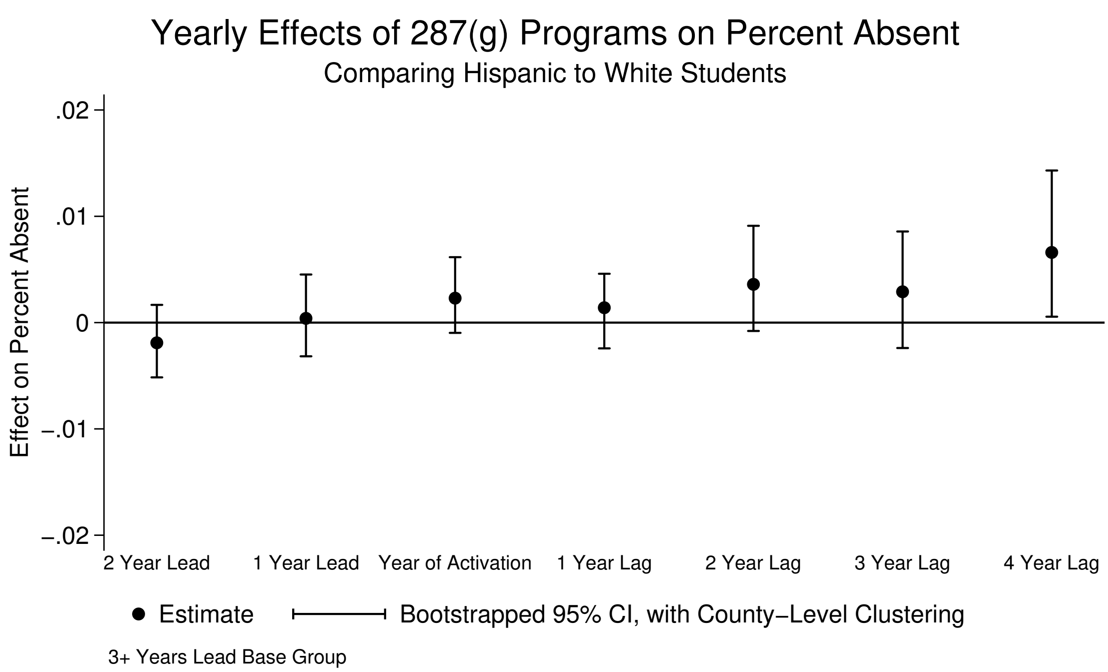

Figure 2.6: Event Study for Effects on Absence Rate

2.7.4 Robustness Checks

In Tables 2.11 and 2.12, I show results from different model specifications and data construction decisions. First, I modify models by including county time trends, to adjust for changing trends in absenteeism within counties: again, the activation of 287(g) programs appears to increase absences by about 65 percent of a day and increases the absence rate by about 0.3 percentage points. Second, I exclude 2013, in which Durham and Alamance had deactivated their 287(g) programs. The activation of 287(g) programs appears to increase absences by about 70 percent of a day and to increase the absence rate by 0.35 percentage points. Third, I include only students who do not move from their permanent county. The activation of a 287(g) program appears to have similarly large effects on days absent and the absence rate as in main models. Finally, in main models, I use students' county as identified in the 2005-2006 school year. Mecklenberg activated their 287(g) program in February of 2006, which is immediately before information on attendance for the year was collected but might have led to some student mobility. In an additional robustness check, I therefore use students' county as identified in the 2004-2005 school year. I find very similar results: the activation of 287(g) programs increases student absences by 90 percent of a school day and the absence rate by 0.4 percentage points.

Counties vary dramatically in population size, with Mecklenburg and Wake together accounting for approximately a third of all students. In order to determine whether one county is driving results, I iteratively drop counties (both approved and denied). As shown in Figures 2.7 and 2.8, results are largely robust to this test. However, when I drop Mecklenburg, which accounts for a large share of students and also was the first county to activate a 287(g) program, the effect of 287(g) program activation appears to be similar in size but no longer reaches conventional levels of statistical significance.

Table 2.11: Robustness Checks for Days Absent

Robustness Check	(1) With County Time Trends	(2) Excluding 2013	(3) Not Mobile	(4) 2005 County
Hispanic (T)	-0.2857	-0.2707	-0.2424	-0.1582
	(0.2282)	(0.2382)	(0.2943)	(0.5596)
	[-0.8324, 0.1714]	[-0.8546, 0.2007]	[-0.7986, 0.2186]	[-0.8295, 0.4246]
Post (P)	0.3143	0.1988	0.0795	0.1538
	(0.2893)	(0.2553)	(0.7377)	(0.5475)
	[-0.3162, 0.9597]	[-0.1672, 0.6445]	[-0.3262, 0.6452]	[-0.3252, 0.7700]
T X A	1.4383**	1.3821**	1.3781**	1.4729**
	(0.0220)	(0.0350)	(0.0320)	(0.0340)
	[0.2167, 2.5835]	[0.1159, 2.5196]	[0.1464, 2.4454]	[0.1192, 2.6626]
T X P	0.5488**	0.3633*	0.5252**	0.6738**
	(0.0210)	(0.0741)	(0.0300)	(0.0150)
	[0.1177, 0.9207]	[-0.0838, 0.7649]	[0.0821, 0.9578]	[0.1784, 1.1397]
A X P	-0.2605	-0.0396	-0.0319	-0.1191
	(0.6006)	(0.8168)	(0.8749)	(0.5696)
	[-1.3142, 0.6538]	[-0.4007, 0.3660]	[-0.4173, 0.4119]	[-0.5343, 0.3344]
T X A X P	0.6502***	0.7213***	0.8425***	0.9019***
	(0.0090)	(0.0000)	(0.0030)	(0.0080)
	[0.2126, 1.0736]	[0.3079, 1.1627]	[0.4100, 1.3156]	[0.3061, 1.4261]
Observations	1,997,769	1,920,342	1,939,223	1,824,657
R-squared	0.0142	0.0118	0.0135	0.0128
Grade and Gender	Yes	Yes	Yes	Yes
Secure Communities	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 2.12: Robustness Checks for Absence Rate

Robustness Check	(1) With County Time Trends	(2) Excluding 2013	(3) Not Mobile	(4) 2005 County
Hispanic (T)	-0.0009	-0.0009	-0.0007	-0.0003
	(0.4745)	(0.5095)	(0.6126)	(0.8589)
	[-0.0041, 0.0019]	[-0.0043, 0.0021]	[-0.0041, 0.0024]	[-0.0043, 0.0031]
Post (P)	0.0017	0.0010	0.0002	0.0007
	(0.3453)	(0.3063)	(0.8859)	(0.5796)
	[-0.0022, 0.0054]	[-0.0009, 0.0035]	[-0.0024, 0.0034]	[-0.0019, 0.0044]
T X A	0.0097**	0.0094**	0.0093**	0.0103**
	(0.0180)	(0.0230)	(0.0290)	(0.0130)
	[0.0018, 0.0161]	[0.0015, 0.0160]	[0.0011, 0.0158]	[0.0019, 0.0171]
T X P	0.0026*	0.0016	0.0026*	0.0036**
	(0.0541)	(0.1481)	(0.0521)	(0.0230)
	[-0.0001, 0.0048]	[-0.0008, 0.0039]	[-0.0001, 0.0052]	[0.0008, 0.0063]
A X P	-0.0016	-0.0003	-0.0003	-0.0009
	(0.5756)	(0.7868)	(0.7808)	(0.4805)
	[-0.0081, 0.0042]	[-0.0026, 0.0020]	[-0.0025, 0.0024]	[-0.0034, 0.0016]
T X A X P	0.0034***	0.0035***	0.0045***	0.0043***
	(0.0080)	(0.0050)	(0.0000)	(0.0080)
	[0.0011, 0.0060]	[0.0011, 0.0062]	[0.0019, 0.0074]	[0.0014, 0.0072]
Observations	1,997,741	1,920,314	1,939,198	1,824,617
R-squared	0.0116	0.0089	0.0109	0.0090
Grade and Gender	Yes	Yes	Yes	Yes
Secure Communities	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

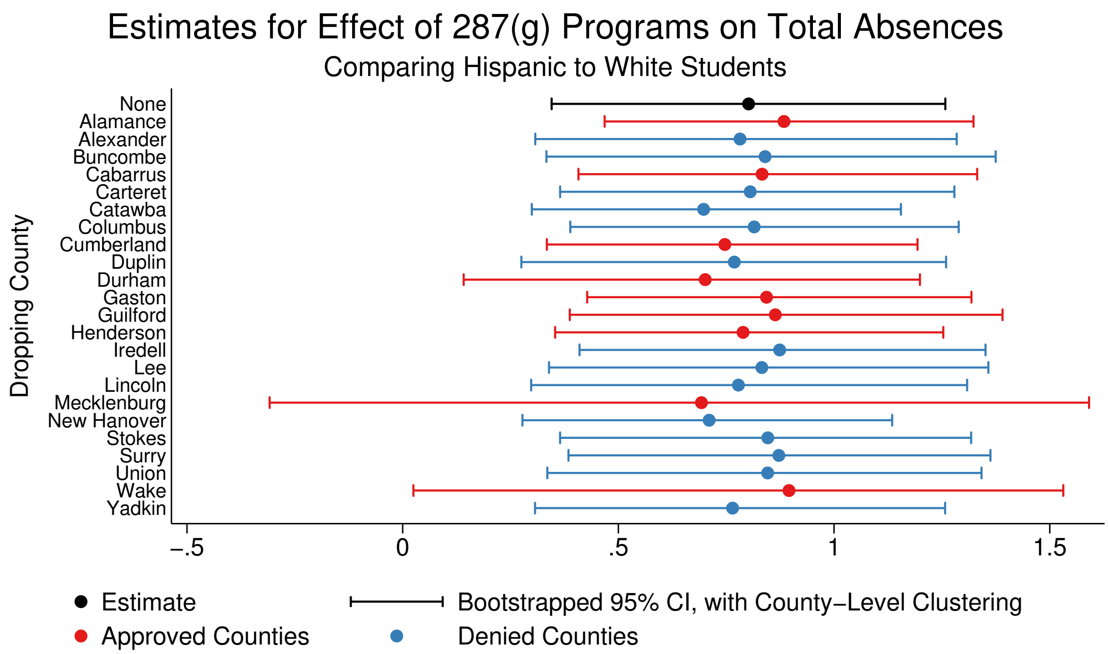

Figure 2.7: Robustness Check Dropping Counties Iteratively

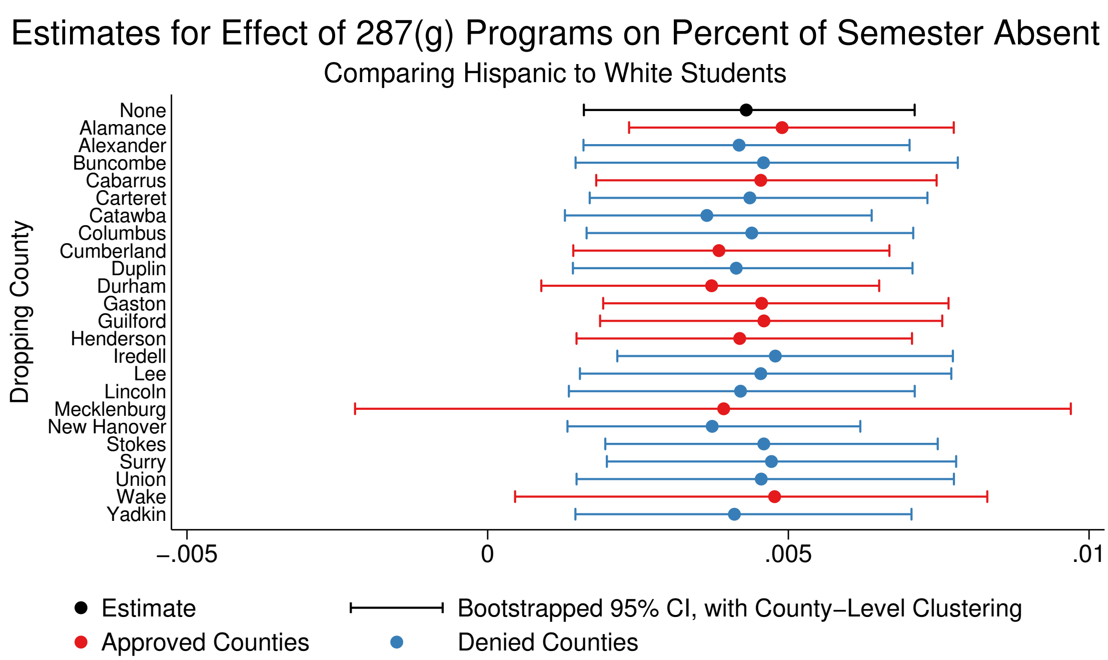

Figure 2.8: Robustness Check Dropping Counties Iteratively

I also estimate models substituting individual for county fixed effects. Here, estimates represent the change in yearly absences or the absence rate for any given student in North Carolina in response to activation of a 287(g) program. Although all students are included in analyses, effects are being estimated using students who can be observed before and after the activation of 287(g) programs. Fortunately, in part because of my restriction that students be assigned to their 2005-2006 county, the majority of students included in analyses can be observed in pre- and post-periods. There are approximately 21,617 Hispanic students in approved counties (80% of student-year observations) and 8,743 Hispanic students in not approved counties (83% of student-year observations) who can be observed in both pre- and post-periods. Similarly, 122,827 white students in approved counties (76% of student-year observations) and 79,391 white students in not approved counties (77% of student-year observations) can be observed in both pre- and post-periods. As shown in Tables 2.13, I again find effects of 287(g) program activation on the attendance of Hispanic students, as compared with white students. The effect for Hispanic students, an increase in absences of a day and a half per year, is larger than the estimate from my main specification.

Table 2.13: Effect of 287(g) Programs Using Individual Fixed Effects

Variables	(1) Days	(2) Percent
Post (P)	-0.4134	-0.0030
	(0.2242)	(0.1612)
	[-1.0052, 0.3134]	[-0.0066, 0.0015]
Approved (A) X P	0.1878	0.0013
	(0.7317)	(0.6987)
	[-0.8628, 1.2880]	[-0.0052, 0.0077]
Hispanic (H) X P	0.9194***	0.0051***
	(0.0090)	(0.0050)
	[0.5058, 1.2648]	[0.0026, 0.0072]
Black (B) X P	0.9418***	0.0057***
	(0.0010)	(0.0020)
	[0.5719, 1.3381]	[0.0035, 0.0088]
H X A X P	1.4821**	0.0082**
	(0.0100)	(0.0160)
	[0.3965, 2.4025]	[0.0017, 0.0140]
B X A X P	0.4509	0.0023
	(0.3974)	(0.5165)
	[-0.6071, 1.5036]	[-0.0040, 0.0088]
Observations	3,064,752	3,064,680
R-squared	0.0646	0.0504
Grade and Gender	Yes	Yes
Secure Communities	Yes	Yes
Year FE	Yes	Yes
Individual FE	Yes	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 2.14: Effect of 287(g) Programs on Absence Counts, Comparing Black to White Students

Variables	(1) Days	(2) Days	(3) Days
Black (T)	0.0038	0.0143	-0.1757
	(0.9940)	(0.9770)	(0.5656)
	[-0.6326, 0.7038]	[-0.6931, 0.7046]	[-1.0105, 0.3782]
Approved (A)	-0.0282	0.0161	
	(0.9610)	(0.9780)	
	[-1.2153, 1.3211]	[-1.1368, 1.3336]	
Post (P)	0.3942***	0.0100	0.0460
	(0.0040)	(0.9880)	(0.7918)
	[0.1222, 0.8416]	[-0.6868, 1.1461]	[-0.2766, 0.4425]
T X A	0.6394	0.6603	0.7284
	(0.3544)	(0.3564)	(0.2883)
	[-0.7278, 1.9330]	[-0.7628, 1.9593]	[-0.6723, 1.9951]
T X P	0.6914***	0.7018***	0.7021***
	(0.0000)	(0.0020)	(0.0020)
	[0.3639, 1.1150]	[0.3762, 1.1370]	[0.3237, 1.2980]
A X P	0.1765	0.1729	-0.0702
	(0.4875)	(0.6086)	(0.6837)
	[-0.3619, 0.7366]	[-0.4974, 0.8226]	[-0.3951, 0.2961]
T X A X P	-0.0808	-0.1149	0.0631
	(0.7257)	(0.6266)	(0.8058)
	[-0.5932, 0.3771]	[-0.5958, 0.3672]	[-0.5199, 0.5539]
Observations	2,625,787	2,625,787	2,625,787
R-squared	0.0042	0.0134	0.0127
Grade and Gender	No	Yes	Yes
Secure Communities	No	Yes	Yes
Year FE	No	Yes	Yes
County FE	No	No	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 2.15: Effect of 287(g) Programs on Percent of Days in Membership Absent, Comparing Black to White Students

Variables	(1) Percent	(2) Percent	(3) Percent
Black (T)	0.0010	0.0009	-0.0002
	(0.7738)	(0.8248)	(0.9419)
	[-0.0040, 0.0058]	[-0.0038, 0.0061]	[-0.0054, 0.0039]
Approved (A)	0.0003	0.0005	
	(0.9329)	(0.8909)	
	[-0.0070, 0.0097]	[-0.0067, 0.0091]	
Post (P)	0.0016*	-0.0001	-0.0000
	(0.0701)	(0.9690)	(0.9620)
	[-0.0001, 0.0044]	[-0.0045, 0.0061]	[-0.0022, 0.0026]
T X A	0.0049	0.0050	0.0053
	(0.2533)	(0.2643)	(0.2322)
	[-0.0042, 0.0134]	[-0.0044, 0.0136]	[-0.0040, 0.0139]
T X P	0.0040***	0.0041***	0.0041***
	(0.0050)	(0.0020)	(0.0030)
	[0.0017, 0.0071]	[0.0018, 0.0070]	[0.0014, 0.0078]
A X P	0.0009	0.0010	-0.0005
	(0.5766)	(0.5756)	(0.5906)
	[-0.0026, 0.0041]	[-0.0028, 0.0046]	[-0.0024, 0.0016]
T X A X P	-0.0011	-0.0014	-0.0003
	(0.4965)	(0.3954)	(0.8529)
	[-0.0044, 0.0021]	[-0.0047, 0.0018]	[-0.0043, 0.0033]
Observations	2,625,734	2,625,734	2,625,734
R-squared	0.0040	0.0106	0.0095
Grade and Gender	No	Yes	Yes
Secure Communities	No	Yes	Yes
Year FE	No	Yes	Yes
County FE	No	No	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

2.7.5 Falsification Check

As a falsification check, I estimate similar models in which I substitute black students for Hispanic students. Black students are less likely to be directly affected by 287(g) programs than Hispanic students, as few black students in North Carolina are immigrants or the children of immigrants. Additionally, although 287(g) programs may affect black students indirectly through increases in racial animus, all evidence in North Carolina suggests that both law enforcement and community members were aware that 287(g) programs were focused on the identification and removal of Hispanic immigrants. As expected, in Tables 2.14 and 2.15, I find that activation of 287(g) programs has no effect on black students in terms of number of days absent or absence rate. Indeed, in my preferred specification using controls as well as year and county fixed effects, the estimate of the effect of 287(g) program activation for black student attendance, relative to white students, is very close to zero.

2.7.6 Placebo Tests

I additionally run two separate placebo tests. In the first, I randomly assign Hispanic and white students from my main models to treatment or control condition and estimate my main specification, using control variables, year fixed effects, and county fixed effects, over one thousand simulations. In the second, I randomly assign students from my main models to approved or denied counties and again estimate my main specification over one thousand simulations. In results from these two placebo tests for both days absent and absence rate in Figures 2.9 through 2.12, I find that estimated coefficients from my main results (shown with red lines) are much larger than any estimates from placebo tests.

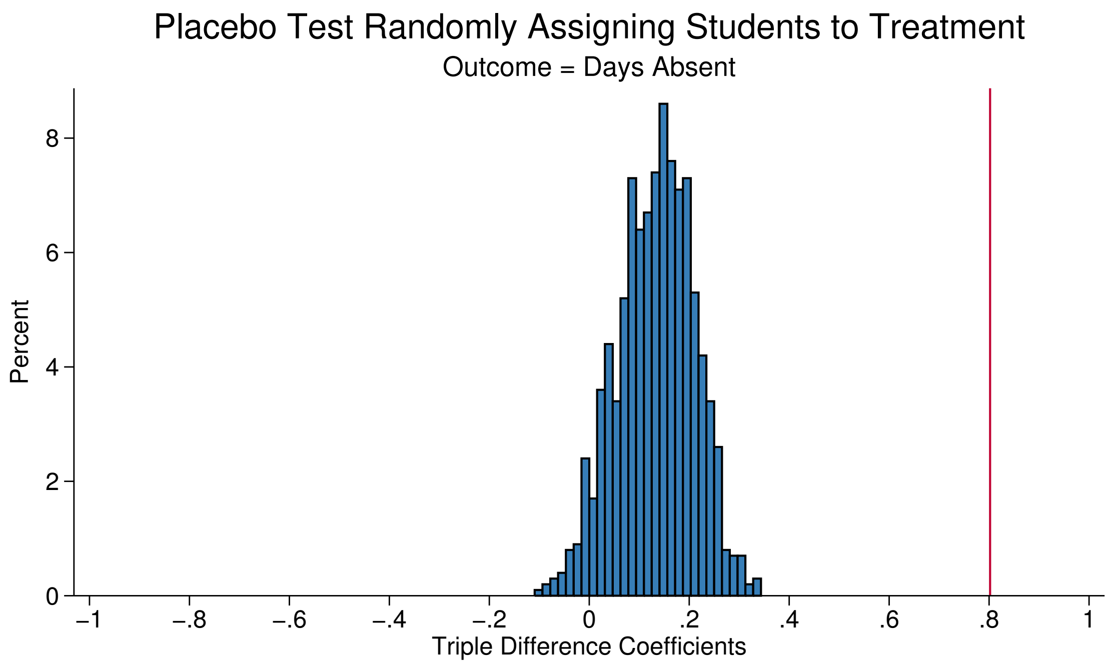

Figure 2.9: Placebo Test with Randomly Assigned Treatment (Absence Counts)

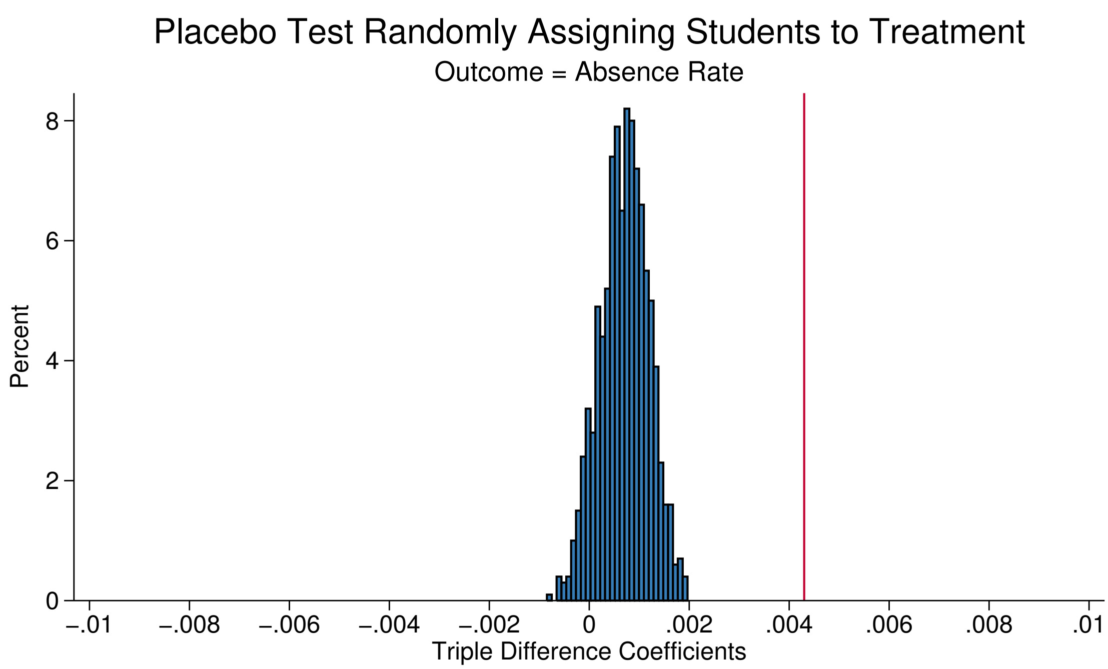

Figure 2.10: Placebo Test with Randomly Assigned Treatment (Absence Rate)

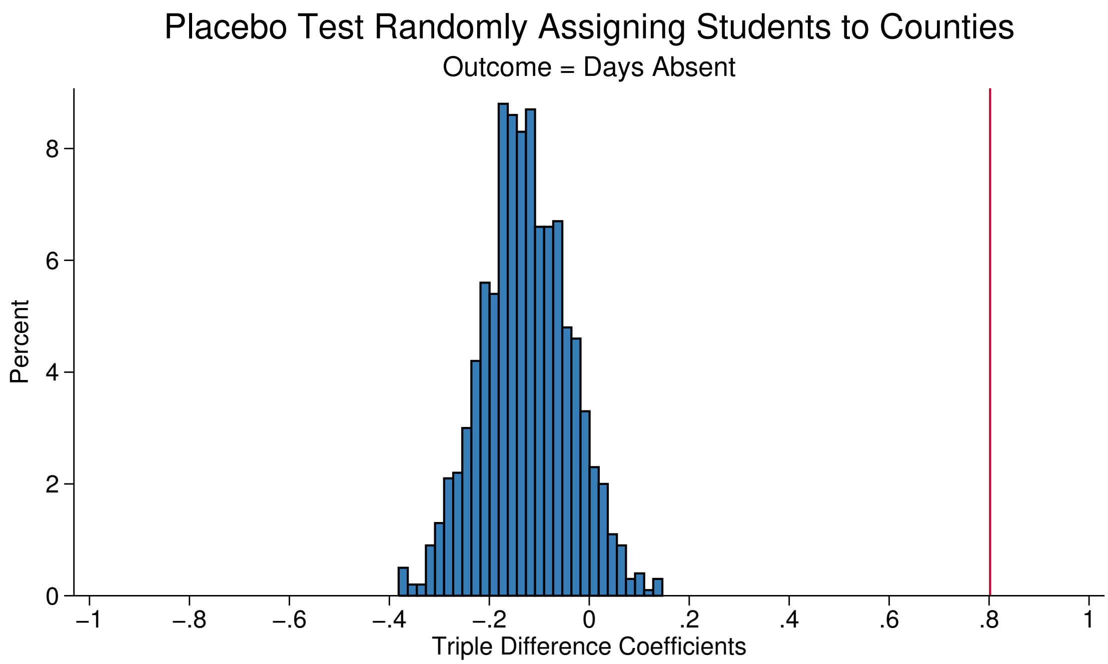

Figure 2.11: Placebo Test with Randomly Assigned Counties (Absence Counts)

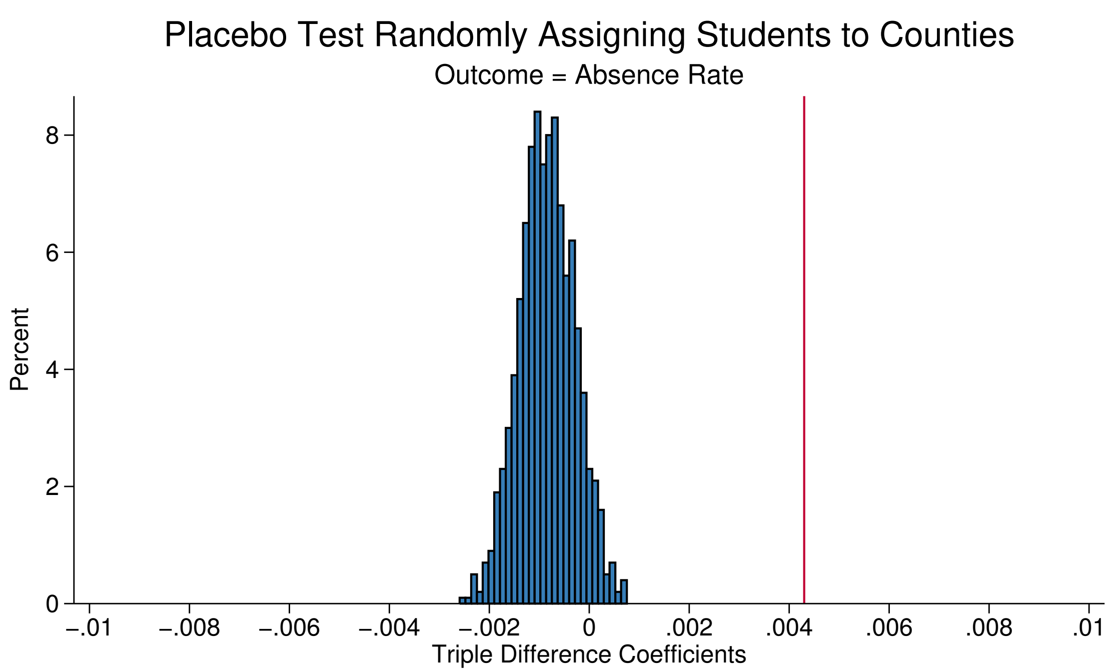

Figure 2.12: Placebo Test with Randomly Assigned Counties (Absence Rate)

2.7.7 Varying Effects Based on Number of Days Absent

Effects on number of days absent may result from increases in a few days absent or increases in many days absent. To identify where in the distribution of absences increases occur, I therefore estimate a series of linear probability models, using the same triple difference approach, in which the outcomes are indicators for number of absences: one or more absences, two or more absences, three or more absences, six or more absences, ten or more absences, fifteen or more absences, twenty or more absences, or fifty or more absences.

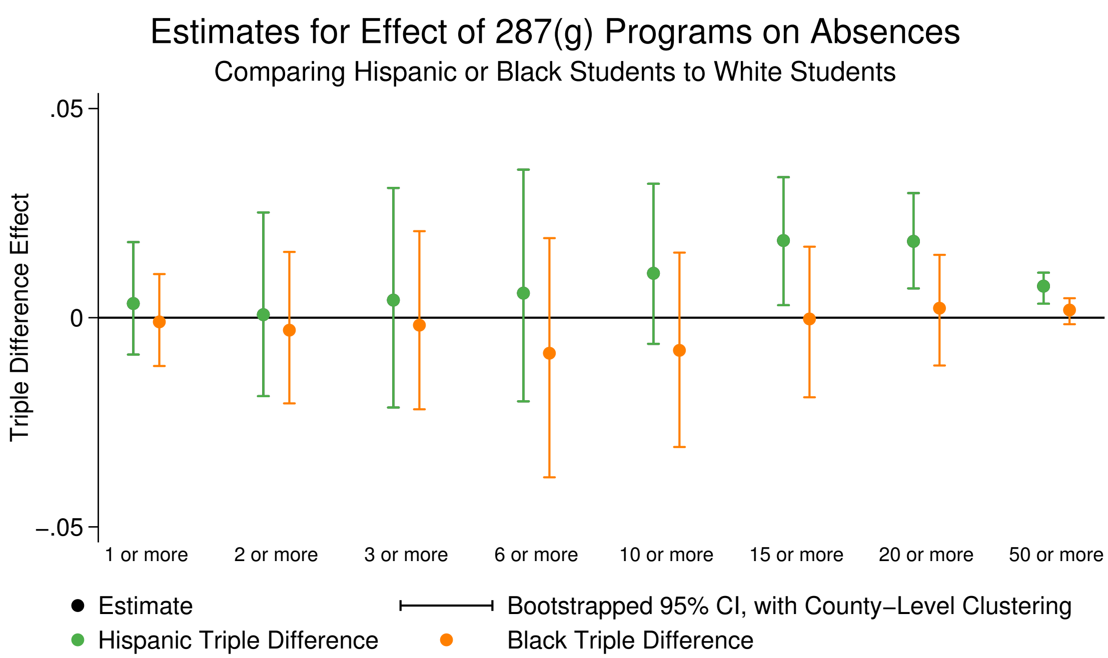

Figure 2.13: Effects of 287(g) Programs on a Series of Absence Indicators, Comparing Hispanic or Black Students to White Students

As shown in Figure 2.13, I find no results for increases in small numbers of absences. There are no effects of the activation of 287(g) programs on the likelihood that students will have up to ten absences. However, the activation of 287(g) programs appears to increase the likelihood that Hispanic students will have fifteen or more, twenty or more, or fifty or more absences, relative to white students. The activation of a 287(g) program appears to increase the probability that Hispanic students will be absent fifteen or more days by 1.8 percentage points, with a similarly sized increase in the probability that Hispanic students will be absent twenty or more days. As shown in Table 2.7.1, across approved and not approved counties, about 13 percent of students are absent fifteen or more days, and seven percent of students are absent 20 or more days. The activation of a 287(g) program also increases the probability Hispanic students will be absent fifty or more days by 0.75 percentage points; this represents a doubling in the likelihood of missing fifty or more days from the baseline rate of 0.75 percent of students overall who are absent 50 or more days.

2.7.8 Varying Effects Based on Grade Level

To investigate whether effects differ for students based on age, I split models by students in grades 3-5, students in grades 6-8, and students in grades 9-12. As shown in Tables 2.16 and 2.17, no estimates are statistically significant, but results are suggestive that effects of 287(g) programs on absences are concentrated among teens. Only in models restricted to students in grades 9-12 does the size of the estimated effect resemble the size of the estimated effect in my main models.

2.7.9 Varying Effects Based on Gender

To investigate whether effects differ for students based on gender, I split models by students identified as female or male. As shown in Tables 2.18 and 2.19, I find that effects twice as strong for male as for female students. For female students, the activation of 287(g) programs appears to increase absence counts and to marginally increase the absence rate. However, the effect is about half the size of the effect for male students: for male students, 287(g) program activation appears to dramatically increase absences, by over a day per year, and increase the total absence rate by about 0.6 percentage points.

Table 2.16: Effect of 287(g) Programs on Absence Counts, Comparing Hispanic to White Students, Split by Grade

	(1)	(2)	(3)
Grades	3-5	6-8	9-12
Hispanic (T)	-1.2338***	-0.6379*	1.0410**
	(0.0000)	(0.0821)	(0.0170)
	[-1.6311, -0.9503]	[-1.3903, 0.1508]	[0.2504, 1.8339]
Post (P)	0.1178	0.4411	0.6472*
	(0.3594)	(0.1141)	(0.0571)
	[-0.1103, 0.3977]	[-0.0980, 0.9773]	[-0.0170, 1.5468]
T X A	0.8642**	1.6692**	2.0210**
	(0.0140)	(0.0460)	(0.0350)
	[0.1979, 1.5057]	[0.0340, 3.0711]	[0.2308, 3.3513]
T X P	-0.3761	-0.3254	-0.0592
	(0.1021)	(0.3163)	(0.8839)
	[-0.7872, 0.0936]	[-1.0820, 0.2453]	[-0.9129, 0.6838]
A X P	-0.0469	0.0633	-0.4678
	(0.8238)	(0.8298)	(0.2382)
	[-0.4314, 0.4100]	[-0.6117, 0.6391]	[-1.4564, 0.3103]
T X A X P	0.3826	-0.0945	0.9208
	(0.5085)	(0.8058)	(0.1291)
	[-0.7656, 1.4878]	[-0.9115, 0.8814]	[-0.2710, 2.0927]
Observations	382,097	646,141	969,531
R-squared	0.0040	0.0070	0.0154
Grade and Gender	Yes	Yes	Yes
Secure Communities	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 2.17: Effect of 287(g) Programs on Absence Rate, Comparing Hispanic to White Students, Split by Grade

	(1)	(2)	(3)
Grades	3-5	6-8	9-12
Hispanic (T)	-0.0065***	-0.0027	0.0064**
	(0.0000)	(0.2262)	(0.0130)
	[-0.0091, -0.0045]	[-0.0073, 0.0020]	[0.0016, 0.0110]
Post (P)	0.0007	0.0024	0.0038**
	(0.2903)	(0.1672)	(0.0420)
	[-0.0005, 0.0021]	[-0.0008, 0.0060]	[0.0001, 0.0093]
T X A	0.0062***	0.0112**	0.0131**
	(0.0060)	(0.0250)	(0.0270)
	[0.0016, 0.0105]	[0.0012, 0.0205]	[0.0022, 0.0215]
T X P	-0.0029**	-0.0028	-0.0003
	(0.0460)	(0.1291)	(0.9069)
	[-0.0056, -0.0001]	[-0.0071, 0.0006]	[-0.0054, 0.0040]
A X P	-0.0006	0.0002	-0.0029
	(0.5896)	(0.9429)	(0.1742)
	[-0.0028, 0.0021]	[-0.0047, 0.0045]	[-0.0085, 0.0014]
T X A X P	0.0015	-0.0011	0.0050
	(0.6857)	(0.6707)	(0.1481)
	[-0.0051, 0.0077]	[-0.0062, 0.0050]	[-0.0028, 0.0117]
Observations	382,084	646,132	969,525
R-squared	0.0023	0.0064	0.0152
Grade and Gender	Yes	Yes	Yes
Secure Communities	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
County FE	Yes	Yes	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 2.18: Effect of 287(g) Programs on Absence Counts, Comparing Hispanic to White Students, Split by Gender

Gender	(1) Male	(2) Female
Hispanic (T)	-0.2017	-0.3536
	(0.3754)	(0.1632)
	[-0.7808, 0.2573]	[-0.9215, 0.2058]
Post (P)	0.0495	0.1440
	(0.8178)	(0.4705)
	[-0.3857, 0.6101]	[-0.2465, 0.6629]
T X A	1.3533**	1.3959**
	(0.0370)	(0.0420)
	[0.0921, 2.5135]	[0.0980, 2.5232]
T X P	0.3250	0.7270**
	(0.1552)	(0.0260)
	[-0.1753, 0.8455]	[0.1401, 1.1540]
A X P	-0.0331	-0.0562
	(0.8428)	(0.7608)
	[-0.3979, 0.4430]	[-0.4374, 0.3505]
T X A X P	1.0216***	0.5731***
	(0.0080)	(0.0090)
	[0.2629, 1.7049]	[0.1195, 1.1140]
Observations	1,018,118	979,651
R-squared	0.0129	0.0146
Grade	Yes	Yes
Secure Communities	Yes	Yes
Year FE	Yes	Yes
County FE	Yes	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 2.19: Effect of 287(g) Programs on Absence Rate, Comparing Hispanic to White Students, Split by Gender

	(1)	(2)
Gender	Male	Female
Hispanic (T)	-0.0005	-0.0014
	(0.6927)	(0.3423)
	[-0.0040, 0.0024]	[-0.0049, 0.0018]
Post (P)	0.0001	0.0007
	(0.9510)	(0.5576)
	[-0.0025, 0.0033]	[-0.0017, 0.0038]
T X A	0.0093**	0.0094**
	(0.0240)	(0.0220)
	[0.0015, 0.0162]	[0.0015, 0.0161]
T X P	0.0015	0.0038**
	(0.2793)	(0.0180)
	[-0.0016, 0.0050]	[0.0009, 0.0064]
A X P	-0.0003	-0.0004
	(0.7868)	(0.7057)
	[-0.0025, 0.0025]	[-0.0026, 0.0020]
T X A X P	0.0055***	0.0030*
	(0.0090)	(0.0571)
	[0.0016, 0.0091]	[-0.0001, 0.0065]
Observations	1,018,102	979,639
R-squared	0.0118	0.0107
Grade	Yes	Yes
Secure Communities	Yes	Yes
Year FE	Yes	Yes
County FE	Yes	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

2.7.10 Out-of-School Suspensions

Another mechanism through which immigration enforcement might increase absences is through increases in days out of school due to exclusionary discipline, particularly through out-of-school suspensions. For my years of interest, between 2003-2004 and 2012-2013, NCERDC also contains information about exclusionary discipline, including incident-level records of all out-of-school suspensions. For out-of-school suspensions, NCERDC also contains information on number of days of the suspension, or the number of days students were absent from school due to out-of-school suspension.

I conduct similar analyses at the county-level. For school years 2003-2004 through 2006-2007, the majority of records of disciplinary incidents are missing identifiers that would link students back to other student records within the same year, much less across years. I can identify individual students by district, school, race, gender, grade, and year within the disciplinary data files. Therefore, I count the number of students with at least one out-of-school suspension, disaggregated by these categories, and then calculate the overall suspension rate based on the number of students in the same categories in attendance files. I similarly sum up the days in out-of-school suspension for different groups and calculate the rate of days spent in out-of-school suspension using students' total days in membership as the denominator. Unfortunately, I am unable to identify students' county in 2005-2006, so these analyses do not account for student mobility. I use a similar triple difference approach to my main models.

$$\begin{aligned} \mathrm{Y} = \alpha + \beta_1\mathrm{T} + \beta_2 287(\mathrm{g}) + \beta_3\mathrm{Post} + \beta_4\mathrm{T} \times 287(\mathrm{g}) + \beta_5\mathrm{T} \times \mathrm{Post} \\ + \beta_6 287(\mathrm{g}) \times \mathrm{Post} + \beta_7\mathrm{T} \times 287(\mathrm{g}) \times \mathrm{Post} + \beta_m\mathrm{X} + \phi + \gamma + \eta + \epsilon \end{aligned} \tag{2.2}$$

Here, Y is the rate of students suspended or the rate of days spent in out-of-school suspension for students in a particular year, grade, county, gender, and race/ethnicity. In all models, T is the treatment group of Hispanic students, with white students serving as the base group; 287(g) is an indicator variable that is 0 if a county applied but was not approved for a 287(g) program and 1 if a county applied and was approved for a 287(g) program; and Post is an indicator variable representing the years after

287(g) implementation (for counties that are rejected from 287(g) programs, Post represents a year or more following 287(g) application). I control for student gender, as well as for whether the county also had Secure Communities activated during this period. I also use county fixed effects (ϕ) and year fixed effects (η). I again use a wild cluster bootstrap (user-written `boottest` in Stata) to account for clustering at the county-level as well as small numbers of counties (Roodman et al., 2018). To run models most similar to my main analyses focusing on student absences, I frequency-weight models by the number of students in a particular county, year, grade, gender, and race/ethnicity observation. However, results are similar when I do not weight models.

Table 2.20: Effect of 287(g) Programs on Rate of Students Experiencing Any Suspension, Comparing Hispanic to White Students

Students	(1) All	(2) Male	(3) Female	(4) Grades 3-5	(5) Grades 6-8	(6) Grades 9-12
Hispanic (T)	0.0217**	0.0272**	0.0154	0.0016	0.0474	0.0215
	(0.0310)	(0.0290)	(0.0420)	(0.8539)	(0.0110)	(0.0370)
Post (P)	-0.0047	-0.0073	-0.0016	-0.0029	0.0002	-0.0084
	(0.5676)	(0.4895)	(0.7197)	(0.3754)	(0.9800)	(0.4685)
T X A	0.0251	0.0345	0.0156	0.0100	0.0268	0.0391
	(0.2252)	(0.2062)	(0.2623)	(0.1782)	(0.4825)	(0.1381)
T X P	0.0012	0.0007	0.0019	0.0019	-0.0135	0.0115
	(0.7628)	(0.9079)	(0.5365)	(0.3554)	(0.2723)	(0.3323)
A X P	0.0012	0.0026	-0.0004	0.0022	0.0027	-0.0016
	(0.8148)	(0.7077)	(0.9049)	(0.4204)	(0.6987)	(0.8569)
T X A X P	0.0024	0.0038	0.0013	-0.0029	0.0110	-0.0000
	(0.8408)	(0.8438)	(0.8889)	(0.6016)	(0.7127)	(1.0000)
Observations	3,619,407	1,851,127	1,768,280	1,135,581	1,102,459	1,381,367
R-squared	0.7323	0.7222	0.6797	0.6612	0.7690	0.7384
Grade	Yes	Yes	Yes	Yes	Yes	Yes
Secure Communities	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes

P-values (in parentheses) obtained through
wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 2.21: Effect of 287(g) Programs on Rate of Days Out of School Due to Suspension, Comparing Hispanic to White Students

Students	(1) All	(2) Male	(3) Female	(4) Grades 3-5	(5) Grades 6-8	(6) Grades 9-12
Hispanic (T)	0.0016**	0.0026**	0.0005	0.0001	0.0036	0.0011
	(0.0170)	(0.0100)	(0.0921)	(0.7347)	(0.0010)	(0.1391)
Post (P)	0.0004	0.0006	0.0002	-0.0001	0.0009	0.0005
	(0.5485)	(0.4965)	(0.6206)	(0.4014)	(0.1201)	(0.6857)
T X A	0.0022	0.0032	0.0012	0.0001	0.0025	0.0049
	(0.3103)	(0.3594)	(0.1682)	(0.6066)	(0.5766)	(0.0681)
T X P	-0.0004	-0.0008	0.0000	-0.0001	-0.0019	0.0005
	(0.1692)	(0.1612)	(0.8418)	(0.7748)	(0.0981)	(0.3023)
A X P	-0.0008*	-0.0012**	-0.0004	0.0000	-0.0011	-0.0012
	(0.0711)	(0.0470)	(0.0661)	(0.7688)	(0.0611)	(0.0761)
T X A X P	-0.0006	-0.0007	-0.0003	0.0000	0.0001	-0.0024
	(0.7017)	(0.7187)	(0.5506)	(0.7988)	(0.9570)	(0.2723)
Observations	3,619,407	1,851,127	1,768,280	1,135,581	1,102,459	1,381,367
R-squared	0.5047	0.5348	0.4924	0.5105	0.5498	0.5066
Grade	Yes	Yes	Yes	Yes	Yes	Yes
Secure Communities	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes

P-values (in parentheses) obtained through
wild cluster bootstrap, with clustering at the county-level (999 replications)
*** $p<0.01$, ** $p<0.05$, * $p<0.10$

As shown in Tables 2.20 and 2.21, the activation of a 287(g) program has no effect on the number of Hispanic students suspended or the rate of days Hispanic students spent in out-of-school suspensions. Because results for absences appear to be driven by high school students and male students, I additionally split models by school level and gender. I see no separate effects on the number of Hispanic students suspended or the number of days Hispanic students spent in out-of-school suspensions by gender or school level. While it is possible that there exist effects of 287(g) programs on suspensions but that I do not have sufficient power to detect any, note that the main coefficients of interest in models estimating the effects of 287(g) programs on the number of days Hispanic students spent in out-of-school suspensions are mostly close to zero or negative. Therefore, it seems unlikely that increases in absences are primarily driven by increases in days spent in out-of-school suspensions.

2.7.11 Student Enrollment

Any effects from the activation of 287(g) programs may be underestimates of true effects if families move in response to the activation of 287(g) programs or students leave school. Using Common Core of Data (CCD) information on student enrollment from 1999-2000 through 2010-2011, Dee and Murphy (2018) find that the activation of 287(g) programs reduced the enrollment of Hispanic students in activated counties, compared with counties that applied but were not approved for 287(g) programs.

To investigate whether decreasing Hispanic enrollment could be affecting my results, I adopt this approach to investigate the effects of 287(g) program activation on the enrollment of Hispanic students within North Carolina. Although I do not have information on absences dating back to 1999-2000, I do have information on school enrollment from 2000-2001 through 2010-2011. I am therefore able to estimate their basic difference-in-difference specification (without additional time-varying control variables):

$$\mathrm{Y}_{ct} = \alpha_c + \gamma_t + \theta \mathrm{D}_{ct} + \epsilon_{ct} \tag{2.3}$$

Here, Y_{ct} is the natural log of Hispanic student enrollment in a particular county and year; α_c represents county fixed effects; γ_t represents year fixed effects; and D_{ct} is an indicator that is equal to one during a year in which a 287(g) program is active within the county. I cluster standard errors at the county-level. As shown in Table 2.22, I am unable to reproduce Dee and Murphy (2018)'s results. Whereas Dee and Murphy (2018) find that an active 287(g) agreement decreased Hispanic enrollment by about 8 percent, I find no effect on Hispanic enrollment; indeed, my result for

θ is positive. Although 287(g) programs may have decreased Hispanic enrollment in other parts of the United States, I am unable to detect this effect using North Carolina data.

Table 2.22: Effect of 287(g) Programs on Hispanic Student Enrollment

	(1)	(2)
VARIABLES	Log of Hispanic Enrollment	Log of Hispanic Enrollment
Active 287(g) MOA	-0.076**	0.005
	(0.035)	(0.042)
Obs.	1,862	253
R-squared	0.9932	0.9617
County FE	Yes	Yes
Year FE	Yes	Yes
Source	Dee and Murphy (2018)	North Carolina

Standard errors clustered at the county-level
*** p<0.01, ** p<0.05, * p<0.10

2.8 Discussion

In prior studies, immigration raids have been found to decrease student attendance (Chaudry et al., 2010; Kirksey et al., 2019); no prior research has examined the effects of partnerships between local law enforcement and ICE on school attendance rates. Work on achievement suggests that immigration raids appear to have stronger effects on schooling outcomes than partnerships between ICE and local law enforcement. However, prior studies suggest that partnerships between ICE and local law enforcement may decrease school engagement, via an increase in student dropout rates (Amuedo-Dorantes and Lopez, 2015; Dee and Murphy, 2018). My results also suggest the immigration enforcement decreases student engagement, via decreases in student attendance. Overall, I find that these partnerships increase absences for Hispanic students by about one-half to one day per year. This increase is primarily driven by an increase in chronic absenteeism, as defined by the U.S. Department of Education's Office of the Civil Rights as missing fifteen or more days of school. I find significant increases in the number of Hispanic students absent from school fifteen or more, twenty or more, and even fifty or more days.

Attendance is a marker of school engagement (Fredricks et al., 2004). My findings confirm a general pattern of disengagement with public institutions in the wake of immigration enforcement. Increases in absences appear to be driven by high school students, who have greater control over their own school attendance. Therefore, a decrease in attendance may represent decreased engagement with public institutions amongst youth. Although this increase in disengagement following the activation of partnerships between local law enforcement and ICE has been documented for adults, this is the first study to demonstrate the same pattern amongst youth.

Decreasing school attendance has ramifications for students and families. Absences decrease student achievement (Gottfried, 2011; Aucejo and Romano, 2014; Goodman, 2014; Gershenson et al., 2017), with larger effects stemming from unexcused absences (Gottfried, 2009; Gershenson et al., 2017). The effects of absences on student achievement may be particularly large for low-income and English Language Learner students (Gershenson et al., 2017). High rates of absenteeism have long been associated with increased risk of dropping out of high school (Balfanz et al., 2007; Schoeneberger, 2012). Here, increases in chronic absenteeism may be a precursor to students leaving school entirely. Increases in truancy rates render families more vulnerable to contact with immigration enforcement, through possible prosecutions for truancy.

Decreasing school attendance also has implications for schools. When students are absent, schools lose federal and state education funding based on daily attendance. Recently, schools' accountability for attendance has increased. Under the Every

Student Succeeds Act (ESSA), states are also required to report chronic absenteeism, or the rate of students absent fifteen or more days. Thirty-seven states and the District of Columbia have also incorporated chronic absenteeism into their additional “school quality or student success” (SQSS) accountability indicator required under the Every Student Succeeds Act (ESSA) of 2015, which revised the structure of No Child Left Behind (NCLB) . Districts and schools in these states are now held accountable for chronic absenteeism rates.[15] Although several promising practices suggest that in-school interventions can reduce chronic absenteeism rates (Cook et al., 2017), out-of-school policies may also be drivers of chronic absenteeism and can be addressed via policy.

The mechanism through which 287(g) programs affect attendance is unclear, although results are suggestive. I do not see increases in out-of-school suspensions. I do observe that increases appear to be concentrated amongst high school students, suggesting that worsened health is not the cause (unless health is more likely to worsen amongst older, rather than younger, students). I also see results on chronic absenteeism, which suggests that increases in episodic illness is not the cause (although the worsening of chronic health conditions could be). However, there are still multiple potential mechanisms. First, students may simply feel less motivated to attend school. Second, students may need to work more or assist their parents. Third, older students may be more likely to be at risk themselves for immigration enforcement, as opposed to younger students. In particular, high school aged male Hispanic students may be targeted by local law enforcement after 287(g) activation. Results appear to be unaffected by students moving or leaving school, as I am unable to reproduce Dee and Murphy (2018)'s result showing a decline in Hispanic enrollment within activated

[15]North Carolina happens to be one of the thirteen states not using chronic absenteeism in its “non-academic” accountability.

counties.

Because school attendance is mandatory, any effect I detect on student attendance is likely much larger for non-mandatory educational programming. Under conditions of increased immigration enforcement, students in families with unauthorized members are less likely to attend after-school programs or enroll in early childhood education programs. One limitation is that North Carolina does not consistently collect data on attendance for children in grades PK-2. Future work should examine effects of 287(g) programs on early childhood education and other educational programming outside of school.

2.9 Conclusion

During the second term of the Obama administration, immigration enforcement activity deescalated, as the administration put a stronger focus on identifying and removing serious criminal offenders. In contrast, the Trump administration has adopted a universal approach to immigration enforcement, with the goal to identify and remove as many unauthorized immigrants as possible, regardless of criminal status. From 2017 to 2018, the number of active 287(g) agreements grew from 30 to 76, the most in the history of the program (Capps et al., 2018). My results add to a growing body of literature suggesting that the likely ramifications of these policies will be decreased engagement on the part of unauthorized immigrants and their families with a variety of institutions. This disengagement has negative ramifications for immigrants themselves and their families but also for the broader community. In this specific case, school attendance is necessary for other positive schooling outcomes, and chronic absenteeism is linked particularly to increases in school dropout rates. Reducing overall levels of educational attainment will negatively impact the U.S. economy, as well as reduce the likelihood of political engagement.

In Plyler v. Doe, the Supreme Court overturned a Texas law withholding state funds for the education of unauthorized immigrant children and authorizing school districts to disenroll unauthorized immigrant children. In the opinion for the majority, Justice Brennan wrote, “It is difficult to understand precisely what the State hopes to achieve by promoting the creation and perpetuation of a subclass of illiterates within our boundaries, surely adding to the problems and costs of unemployment, welfare, and crime” (Plyler v Doe, 1982, p.230). Although immigration enforcement is a more subtle form of administrative burden than laws restricting access to education based on immigration status, it also has negative spillover effects on access to education for both unauthorized immigrant children and the larger population of children of unauthorized immigrants. When considering the full costs of immigration enforcement, policymakers should consider Justice Brennan’s implied question from nearly fifty years ago: are the costs associated with leading multiple generations of individuals to disengage from U.S. society really worth the benefits?

Chapter 3

The Impact of 287(g) Programs on Student Achievement in North Carolina

3.1 Introduction

During the second term of the George W. Bush administration and first term of the Barack Obama administration, immigration enforcement increased dramatically in the U.S. interior, driven by partnerships between local law enforcement and Immigration and Customs Enforcement (ICE). Although they are responsible for a low share of arrests in the U.S. interior, as shown in Table 3.1, the best known partnerships between ICE and local law enforcement are 287(g) programs. These programs only operated in a relatively small number of jurisdictions (72 in 2010) (Heinrich, 2018) but may have had strong impacts in the areas in which they were implemented. Prior quasi-experimental studies find that the activation of 287(g) programs increased food insecurity, increased foreclosure rates, and decreased school enrollment amongst Hispanic populations (Potochnick et al., 2016; Rugh and Hall, 2016; Dee and Murphy, 2018). Families are likely negatively affected by 287(g) programs because of rising levels of fear and stress for both unauthorized parents and their children. These rising levels of stress and fear are likely to impact child outcomes, including children's performance in school.

In this chapter, I continue work begun in chapter 2 to fill the gap in knowledge on how 287(g) programs affect child outcomes. Here, I examine the impact of increases in immigration enforcement, via the activation of 287(g) programs, on student achievement within one state, North Carolina. In North Carolina, nine counties established 287(g) programs in different years, whereas another 15 counties applied for but were denied participation. I use a triple difference strategy in which I compare academic achievement for different groups of students in these two sets of counties before and after activation of 287(g) programs. I find no effect of activation on the general group of Hispanic students, when compared with white students, and no effect of activation on a more specific group of Hispanic students ever identified as Limited English Proficient (LEP), when compared with white students never identified as LEP.

Table 3.1: Arrests by Interior Immigration Enforcement Programs Targeting Criminal Aliens

FY	National Fugitive Operations Program	Criminal Alien Program	287(g)	Secure Communities
2004	6,584	4,269	0	NA
2005	7,959	25,339	2	NA
2006	15,462	28,493	5,685	NA
2007	30,407	164,296	20,815	NA
2008	34,155	221,085	45,105	NA
2009	35,094	232,796	56,116	42,135
2010	35,774	219,477	46,467	111,093
2011	40,102	221,122	33,180	73,466
2012	37,371	200,253	31,478	98,117

3.2 Background

287(g) programs refer to Section 287(g) of the Immigration and Nationality Act (INA) and were first authorized as part of the 1996 Illegal Immigration Reform and Immigrant Responsibility Act, although the first 287(g) agreement was not implemented until 2002 (Rosenblum and Kandel, 2011). In 287(g) programs, ICE enters into agreements allowing state and local law enforcement to act as immigration enforcement agents. Under these arrangements, ICE provides training and other capacities to state and local law enforcement agents. In return, state and local law enforcement agents are able to issue detainers. These programs follow two different models: the task-force model and the jail model, as well as a hybrid model that combines the two. In the task-force model, individuals are detained at the point of arrest. In the jail model, individuals are detained after being booked into a jail (Rosenblum and Kandel, 2011). However, particularly in early years of 287(g) programs, there may have been little practical distinction between models: police reports in North Carolina counties suggest that law enforcement in some counties with jail models question individuals about immigration status prior to arrest (Nguyen and Gill, 2010, 2015).

The express purpose of partnerships with local law enforcement agencies is to target criminal aliens, or noncitizens who have committed crimes (Rosenblum and Kandel, 2011). However, critics have noted that many of the individuals who are identified as a result of 287(g) programs are either noncriminals or have only low-level offenses (such as traffic violations). Therefore, it is unclear whether these programs are targeting criminal aliens or unauthorized immigrants, regardless of criminal status.[1]

As a "new destination" for Hispanic immigrants, North Carolina has been at the forefront of immigration enforcement (Nguyen and Gill, 2010). In 2006, Mecklenburg County became one the first U.S. counties to establish a 287(g) agreement. Mecklenburg County was also the first county to implement a "universal" model, in which local law enforcement does not specifically target serious criminal offenders but identifies as many unauthorized immigrants as possible (Capps et al., 2011). As shown in Table 2.2, between 2006 and 2009, eight more local law enforcement agencies in North Carolina established 287(g) agreements. Another fifteen counties attempted to

[1]Unauthorized presence in the United States, absent other factors, is a civil, not criminal, offense.

establish agreements during this time period but were rejected by ICE (Capps et al., 2011; Potochnick et al., 2016; Rugh and Hall, 2016).

As shown in Table 2.2, the majority of 287(g) programs in North Carolina are "universal" models.[2] In all but Guilford County, fewer than 50 percent of individuals were detained for serious criminal activities (Level 1 or Level 2).[3][4] Indeed, in Cabarrus, Gaston, Henderson, and Mecklenburg, the majority of immigrants processed in Fiscal Year (FY) 2010 were detained for minor traffic violations.

In December of 2012, ICE scaled back on 287(g) programs by not renewing any agreements for task force programs.[5] In 2013, ICE created a new MOA, and jail programs continued to operate.

3.3 Theoretical Framework

Immigration enforcement policies may decrease achievement for Hispanic students through several mechanisms. In the most extreme circumstances, immigration enforcement leads to parental detention or removal, resulting in complete separation from a parent or both parents. Approximately 5.1 million children nationwide have at least one unauthorized parent and are risk for this outcome. Nationwide, about 1.5 percent of children in foster care are there due to parental detention or deportation;

[2]The Cumberland County Sheriff's Office also had a 287(g) program between May 2007 and October 2009, but the MOA was terminated prior to 2010.

[3]Percent of individuals with Level 3 or "Other" charges not shown.

[4]According to 2009 memorandums of understanding, Level 1 offenders are defined as "aliens who have been convicted of or arrested for major drug offenses and/or violent offenses such as murder, manslaughter, rape, robbery, and kidnapping;" Level 2 offenders are defined as "aliens who have been convicted of or arrested for minor drug offenses, and/or mainly property offenses such as burglary, larceny, fraud, and money laundering;" and Level 3 offenders are defined as "aliens who have been convicted of or arrested for other offenses" (Rosenblum and Kandel, 2011).

[5]https://www.ice.gov/news/releases/fy-2012-ice-announces-year-end-removal-numbers-highlights-focus-key-priorities-and

in counties that have implemented 287(g) programs, a larger fraction of children are in foster care for these reasons (Wessler, 2011). However, removal does not always result in foster care placement. Most children appear to remain with a second parent, typically the mother, but are separated from the detained or removed parent.

Research on the effects of parental detention or removal due to immigration enforcement on academic achievement does not exist, but research on the effects of parental absence for other reasons is instructive. Although parental absence due to migration for work-related reasons has mixed effects on educational outcomes (Kandel and Kao, 2001; Dreby, 2007; Lahaie et al., 2009; Giannelli and Mangiavacchi, 2010; Antman, 2012), military deployments, in which children may have greater concern about parental well-being, consistently reduce children's academic achievement (Lyle, 2006; Engel et al., 2010; Richardson et al., 2011). Parental detention or removal may most closely resemble parental incarceration because it is another case in which the parent has less choice over the absence. Although maternal incarceration is not consistently associated with reduced student performance (Cho, 2009, 2010), paternal incarceration has been consistently associated with negative school outcomes, including lowered school readiness, increased grade retention, and decreased educational attainment (Foster and Hagan, 2009; Haskins, 2014; Turney and Haskins, 2014). However, establishing causal effects of parental incarceration is challenging, and effects may result from other time-variant processes within families that both lead to parental incarceration and reduce test scores. Although parental detention or removal often resembles parental incarceration, detained or deported parents have less contact with their children than incarcerated parents (Dreby, 2012, 2015). This reduced contact may result in detention or removal having larger effects on child outcomes than parental incarceration.

Another mechanism that could link increases in immigration enforcement and children's academic achievement is elevated levels of child stress. Children experiencing a parental detention or removal, as well as the broader population of children with an unauthorized parent, exhibit high levels of distress and anxiety (Allen et al., 2015; Zayas et al., 2015). In the immediate aftermath of their parent being detained or removed, children experience shock and uncertainty about their future circumstances (Dreby, 2015). Longer term, children exhibit increases in both internalizing behaviors (such as depression and anxiety) and externalizing behaviors (such as aggression) (Capps et al., 2007; Allen et al., 2015). Children who have not experienced a parental detention or deportation also report fearing their parents' removal; even though these children have not yet been separated from their parents, their fear over their parents' possible removal is sometimes intensified by contact with other families that have experienced detentions or deportations (Dreby, 2012; Menjivar and Abrego, 2012). These elevated levels of stress are likely to decrease child achievement (Alva and de los Reyes, 1999).

Intensified immigration enforcement also increases fear and stress amongst unauthorized parents themselves, which has spillover effects for children. Unauthorized parents describe constant worry over detection by immigration officials and resulting family separation; fear of this prospect leads unauthorized parents to take extreme precautions, such as never riding together in a car, in order to ensure that their children will not lose both parents (Menjivar and Abrego, 2012; Nguyen and Gill, 2015). If a detention or removal occurs, two-parent households often become single-parent and female-headed (Chaudry et al., 2010; Dreby, 2012). Occasionally, detention or deportation can remove an undesirable or abusive partner (Dreby, 2012). However, in most cases, the remaining partner is left depressed and socially isolated, not only because of the loss of their partner but also because unauthorized friends and family fear close association (Koball et al., 2015; Dreby, 2015). In this respect, immigration enforcement again resembles incarceration, in which families experiencing an incarceration event often describe feeling shame and stigma. This family stress is likely to decrease child achievement.

Increases in immigration enforcement also could impact student achievement through losses of income and benefits. The activation of 287(g) programs specifically has been linked with increases in foreclosure rates and increases in food insecurity (Rugh and Hall, 2016; Potochnick et al., 2016), two measures of substantial economic hardship. Although the activation of 287(g) programs has not been directly linked with decreases in income or benefit receipt, increases in immigration enforcement generally raised the likelihood of household poverty for children living in households with likely unauthorized immigrants (Amuedo-Dorantes et al., 2018). Among a variety of immigration enforcement policies, police-based policies, such as 287(g) programs and Secure Communities, are mostly responsible for rising poverty rates. Decreases in resources affect children's educational achievement by reducing families' ability to invest in children or further increasing family stress (Conger and Donnellan, 2007).

Immigration enforcement might also affect student achievement through increases in absences, which have negative effects on student achievement (Gershenson et al., 2017). ICE activity has a demonstrated negative effect on student attendance: students are less likely to attend school in the immediate aftermath of workplace raids (Chaudry et al., 2010), and newspaper reports about ICE activity have large immediate effects on attendance (Kirksey et al., 2019). In chapter 2, I find that the activation of 287(g) programs in North Carolina increases days absent by 60-80 percent of a day per school year and that these increases were driven by increases in chronic absenteeism (fifteen or more days absent per year, as defined by the Office of the Civil Rights). However, my results appear to be largely driven by high school students, suggesting that increased absences (and possibly the processes underlying them, which may be concentrated among teens) are not relevant for grade 3-8 achievement.

3.4 Prior Research

Parental legal vulnerability due to unauthorized immigration status has been consistently associated with worse child outcomes on multiple dimensions (see Brabeck et al. (2014) for a review). In the short-term, having an unauthorized parent has been associated with parent reports of worsened child emotional well-being and school performance, as well as lower test performance (Brabeck and Xu, 2010; Brabeck et al., 2015). In the long-term, children of authorized immigrant parents from Mexico attain a year of education more than children of unauthorized immigrant parents from Mexico (Bean et al., 2011). However, children of unauthorized parents may be disadvantaged for multiple reasons beyond their exposure to immigration enforcement; indeed, their parents, as a result of immigration status, have poorer access to well-paid jobs and social services (Yoshikawa, 2011).

A growing body of evidence suggests that immigration enforcement negatively impacts student outcomes. In the wake of workplace raids, children with an arrested parent miss school, and many parents report declines in grades over the following six months (Chaudry et al., 2010). Using quasi-experimental methods to approach this question, Amuedo-Dorantes and Lopez (2015) find that increases in immigration enforcement raise the likelihood that students whose parents are likely unauthorized immigrants drop out of school. They find that effects are concentrated primarily amongst younger students, with the children of likely unauthorized immigrants aged six to thirteen more likely to repeat grades and drop out of school in the wake of immigration enforcement policies. The activation of 287(g) programs specifically decreased the school enrollment of Hispanic students (Dee and Murphy, 2018), although it is unclear whether this decrease is the result of migration or dropping out.

However, immigration enforcement policies may differ in effects based on strength or type of treatment. In chapter 1, I find that the activation of Secure Communities decreased county-level average achievement for Hispanic and non-Hispanic black students in English Language Arts (ELA), although the size of decrease is quite small. In contrast, worksite raids have large negative effects on achievement, even when achievement is measured at the school level (Zuniga, 2018). Worksite raids, a specific type of large-scale community trauma, likely produce larger effects than more diffuse forms of immigration enforcement, such as partnerships between local law enforcement and ICE. However, raids by ICE are responsible for a relatively low share of arrests in the U.S. interior. Between October of 2008 and December of 2013, approximately 60 percent of ICE arrests in the U.S. interior resulted from ICE assuming custody of an individual from a local jail or under a 287(g) program (TRAC Immigration, 2018). In contrast, during this same period, only 15 percent of arrests were made directly by ICE. Understanding the impacts of partnerships between ICE and local law enforcement, the mechanism through which ICE has greatly expanded its reach into the U.S. interior, is therefore important.

This project is the first to examine the effects of partnerships between ICE and local law enforcement on student achievement and builds on previous work in several ways. First, I use a quasi-experimental design to recover plausibly causal estimates. This design addresses endogeneity arising from local conditions that would motivate local officials to participate in partnerships with ICE. Second, I use administrative education data covering the entire student population of North Carolina over the course of nearly a decade. Unlike much work in this area, which uses data at a more aggregated level, I am able to track individual students from grade to grade, as well as identify their location prior to the activation of 287(g) programs.

3.5 Data

From Immigration and Customs Enforcement (ICE), I use publicly available data on the dates of North Carolina 287(g) programs, as well as more detailed information on historical 287(g) agreements and applications made available to me by Juan Pedroza and Stephanie Potochnick. I match this information with individual-level student data for 2003-2004 through 2012-2013 from the North Carolina Education Research Data Center (NCERDC), housed at Duke University. NCERDC maintains all of the administrative records on North Carolina public school students that are collected by the state Department of Public Instruction and makes them available to researchers.

3.5.1 Measures

I utilize end-of-grade (EOG) test scores in reading and math from the 2003-2004 through 2010-2011 academic years. All 3rd- through 8th-grade students in North Carolina are required to take EOG achievement tests in both reading and math. In October 2010, the State Board of Education stopped requiring schools' use of EOG scores in student promotion decisions in Grades 3, 5, and 8. However, EOG scores continued to be used to compute school growth and performance as required by North Carolina's ABCs Accountability Program and to determine adequate yearly progress (AYP). EOG tests in reading comprehension measure the ability to demonstrate understanding of a written passage and knowledge of vocabulary. EOG tests in math measure proficiency in five areas: numbers and operations; measurement; geometry; data analysis and probability; and algebra. EOG test score files include raw test scores, as well as students' race/ethnicity, sex, grade level, and school.

For my primary dependent variable of interest, I use any student with a valid first (regular administration) test score in either math or reading. I exclude retest scores, as well as students who had only retest scores, because students were scheduled to take retests nonrandomly (students who took retests were mostly students who scored at not proficient levels on the regular administration). I standardize test scores using the entire population of students within the same grade and subject. I include students who are repeating grades; if this biases my estimates, it likely biases estimates towards a null result (as students repeating grades are likely to score higher upon test repetition).

3.5.2 Defining Student Treatment Status

NCERDC also contains information about student demographics. In main analyses, I use Hispanic students as the treatment group, with white students as the control group, because immigration enforcement largely affects Hispanic immigrants (Rosenblum and Soto, 2015). I identify students' modal race and/or ethnicity as the race and/or ethnicity category they are recorded as most frequently across years. As previously reviewed, a high percentage of students most frequently recorded as multiracial or American Indian are recorded as a different race/ethnicity at least once. Fewer white, black, and Hispanic students are recorded in multiple race/ethnicity categories.

In a supplementary analysis, I further restrict the likely treated group to Hispanic students ever identified as Limited English Proficient, which I define using information on students' current Limited English Proficiency (LEP) status and year exiting LEP during years of data examined. There is some evidence that English proficiency may serve as a proxy for parental nativity. Among Mexican-origin kindergarteners, 95.4

percent of first generation and 74.8 percent of second generation students spoke only Spanish or predominately Spanish at home, whereas 85.2 percent of third generation students spoke only English or predominately English at home (Reardon and Galindo, 2009).

As shown in chapter 2, I am unable to replicate prior findings that families with unauthorized members migrated in response to 287(g) policies (Capps et al., 2011; Dee and Murphy, 2018). However, I continue to treat students' county as observed in the 2005-2006 school year as their "permanent county." In robustness checks, I investigate effects for non-migratory students as well as effects based on assigning students to the county observed in the 2004-2005 school year. In mapping students to counties, I use the substantial overlap between North Carolina district boundaries and county lines. I exclude students who were located at charter schools in 2005-2006.

3.5.3 Control Variables

I control for certain student characteristics, particularly grade and gender. Gender is collected in every year; however, students may differ in terms of recorded gender across years. I take students' most frequently recorded gender, which may mask transgender students. This is less of a concern in this analysis than my previous analysis on absences, as I only observe grade 3-8 students. However, some children identify as transgender in elementary or middle school.

I also control for other county-level immigration enforcement policies. Between 2008 and 2011, all counties in North Carolina also were activated for Secure Communities, another type of partnership between ICE and local law enforcement. As previously reviewed, Secure Communities required law enforcement agencies to automatically submit fingerprints of arrested individuals to the Department of Homeland

Security's (DHS) Automated Biometric Identification System (IDENT). If a match was determined to be a potentially removable alien, ICE might issue a detainer against that individual, or a request to local law enforcement to hold that individual for up to 48 hours for transfer into ICE custody (Kohli et al., 2011; Rosenblum and Kandel, 2011). I control for Secure Communities activation in a particular county school-year using publicly available data on Secure Communities' activation from ICE. In North Carolina, all 287(g) programs were active in approved counties prior to Secure Communities' activation.

3.5.4 Sample

I restrict my sample to 289,177 unique students in grades 3-8, with 1,292,720 student-year observations, who are located in an approved or not approved county in the spring of the 2005-2006 school year. Additionally, I include only students whose race/ethnicity are identified most commonly as Hispanic, white, or black. I only include student-year observations if the student tested in math or reading in that year. If students subsequently move (post 2005-2006) to another county in North Carolina, they remain in my sample. However, if students leave North Carolina, I am unable to observe them. Finally, I exclude observations for 2011-2012 and 2012-2013 because few students whose county I can observe in 2005-2006 are testing in those years; to be taking an EOG test in 2011-2012 or 2012-2013, a student who was in third grade in 2005-2006 would have to have repeated at least one grade. However, results are qualitatively similar when I include student-year observations from 2011-2012 and 2012-2013.

3.6 Analytic Plan

To estimate the effects of increased immigration enforcement via 287(g) programs on student achievement, I use a triple difference strategy. This is a more robust analysis than a difference-in-differences strategy because I am able to add counties not experiencing 287(g) programs (but that have applied for 287(g) programs) as well as a control group of non-Hispanic white students within treatment counties. This helps to control for time trends affecting the achievement of all students in individual counties as well as changes in education across counties for Hispanic students. Additionally, 287(g) program activation is not an exogenous policy change: counties may desire to participate in these programs because of increases in violence in local immigrant communities or because of increases in anti-immigrant animus, both of which plausibly affect the test scores of Hispanic students. By comparing only counties that applied for 287(g) programs, I remove the portion of policy change arising from the county-level factors that lead counties to be interested in partnering with ICE.

For the first difference, I compare Hispanic students to white students. For the second difference, I compare these students pre- and post- the activation of a 287(g) program. For the third difference, I compare between North Carolina counties with a 287(g) agreement and counties that applied but were rejected for a 287(g) agreement.

$$\begin{aligned}\text{Score} = \alpha + \beta_1\text{T} + \beta_2 287(\text{g}) + \beta_3\text{Post} + \beta_4\text{T} \times 287(\text{g}) + \beta_5\text{T} \times \text{Post} \\ +\beta_6 287(\text{g}) \times \text{Post} + \beta_7\text{T} \times 287(\text{g}) \times \text{Post} + \beta_m\text{Student} + \phi + \gamma + \eta + \epsilon\end{aligned} \tag{3.1}$$

Here, Score is a math or reading test score for an individual student in a particular year; T is the treatment group of Hispanic students, with white students serving as the base group; 287(g) is an indicator variable that is 0 if a county applied but was not approved for a 287(g) program and 1 if a county applied and was approved for a 287(g) program; and Post is an indicator variable representing the years after 287(g) implementation (for counties that are rejected from 287(g) programs, Post represents a year or more following 287(g) application). I control for student gender to account for differences in achievement by gender. During this period, Secure Communities, another partnership between ICE and local law enforcement, was activated throughout North Carolina. Secure Communities (as described in chapter 1) may affect student achievement, so I use a time-varying control for its activation. In my preferred set of models, I also use permanent county fixed effects (ϕ); grade fixed effects (γ); and year fixed effects (η) to control for time-invariant characteristics of counties that affect achievement, persistent differences in achievement between grades, and any state-wide or national policy changes in a particular year. I use a wild cluster bootstrap (user-written `boottest` in Stata) to account for clustering at the county-level as well as small numbers of counties (Roodman et al., 2018).

3.7 Results

3.7.1 Descriptive Statistics

As shown in Table 3.7.1, counties that were approved for 287(g) programs are quite different demographically from counties that were not approved. First, the total population in counties that were approved is twice as large as the total population in counties that were not approved for 287(g) programs. This is despite fewer counties being approved than denied for 287(g) programs. However, the counties that were approved - particularly Mecklenberg and Wake - are some of the most populous counties in North Carolina.

Table 3.2: Demographic Information on Approved and Denied Counties

	Not Approved	Approved	Total
Hispanic	8.35%	9.07%	8.85%
White	72.46%	49.31%	56.38%
Black	14.26%	34.39%	28.24%
Asian-American*	1.80%	3.55%	3.02%
American Indian*	0.45%	0.50%	0.49%
Multiracial*	2.68%	3.17%	3.02%
Female	49.71%	49.61%	49.64%
Grade 3	10.28%	10.46%	10.40%
Grade 4	13.51%	13.69%	13.64%
Grade 5	16.78%	16.88%	16.85%
Grade 6	20.14%	19.91%	19.98%
Grade 7	19.87%	19.78%	19.81%
Grade 8	19.41%	19.28%	19.32%
Total Student-Year Observations	401,615	891,105	1,292,720
Total Unique Students	88,703	200,474	289,177

*I exclude Asian-American, American Indian, and multiracial students in all analyses.

Second, counties that were not approved for 287(g) programs differ compositionally from counties that were approved. Over 70 percent of students in counties that were not approved for participation are white, whereas only about half of students in counties that were approved for participation are white. However, counties that were and were not approved for 287(g) programs have similar shares of Hispanic students.

Overall, math and reading achievement is higher in counties that were not approved for 287(g) programs, as shown in Figures 3.1 and 3.2. In denied counties, students score 16 percent of a standard deviation above the average student in North Carolina in math and 13 percent of a standard deviation above the average student in North Carolina in reading. In approved counties, students score 10 percent of a standard deviation above the average student in North Carolina in math and 8 percent of a standard deviation above the average student in North Carolina in reading.

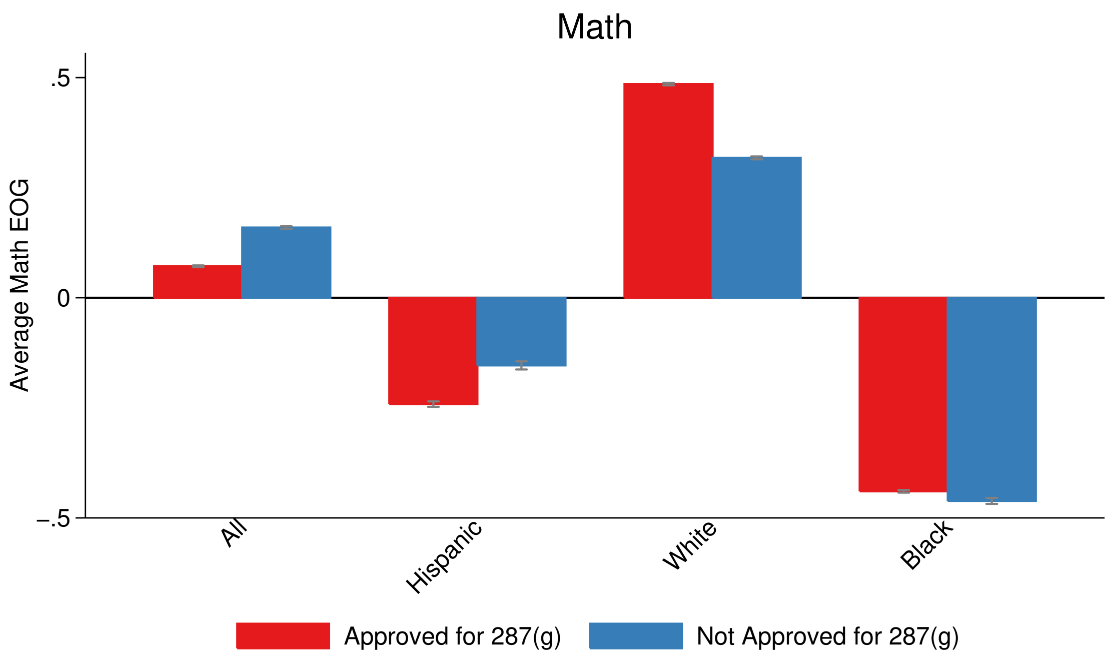

Figure 3.1: Average Math EOG Scores, Grades 3-8

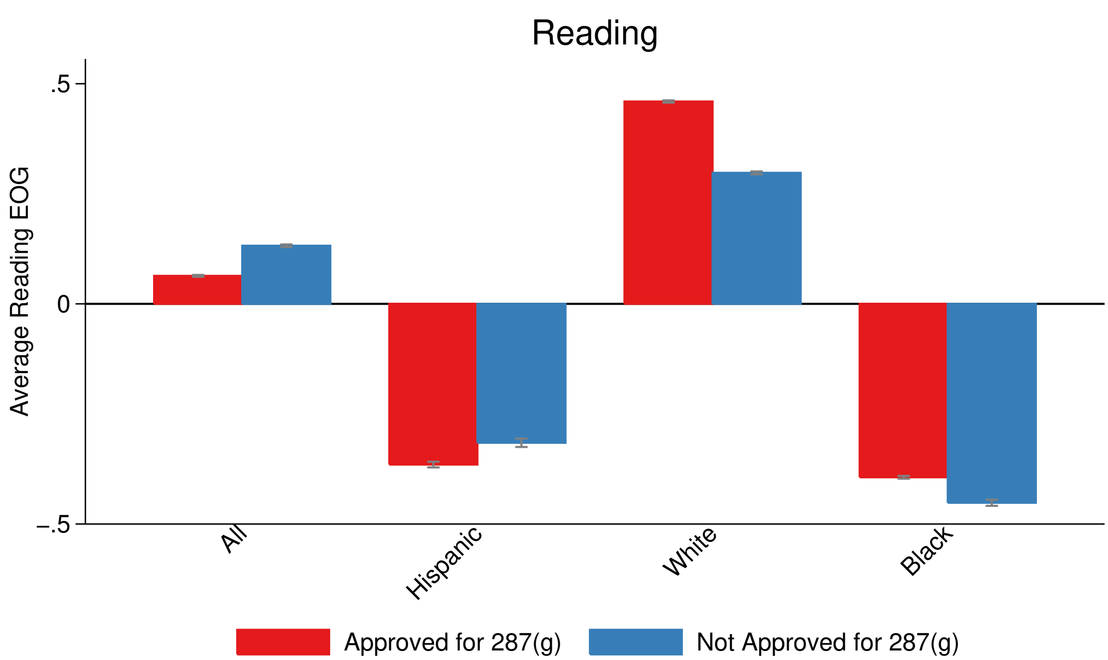

Figure 3.2: Average Reading EOG Scores, Grades 3-8

However, these average scores also reflect differences in demographic composition. When I disaggregate average scores by race/ethnicity, patterns in achievement are less clear. White students in counties approved for 287(g) programs score far above white students in counties not approved for 287(g) programs in both math and reading. Black students in counties approved for 287(g) programs also score slightly higher than black students in counties not approved for 287(g) programs in math and reading, although this difference is barely distinguishable for math. In contrast, Hispanic students in approved counties score below Hispanic students in counties not approved for 287(g) programs. Not approved counties appear to have higher overall achievement because there are higher shares of white students and lower shares of black students in these counties.

Figures 3.3-3.6 display the differences in average math and reading EOG scores between different groups of students within counties approved for 287(g) programs and counties not approved for 287(g) programs. Prior to 287(g) program activation, differences between Hispanic and white students, as well as the more specific groups of Hispanic, ever LEP and white, never LEP, follow similar patterns in counties activating and not activating 287(g) programs for math. However, differences in average reading EOG scores appear to be decreasing at a greater rate in counties not activating 287(g) programs. For both math and reading, differences between Hispanic and white achievement continue to appear parallel between approved and not approved counties post-activation of a 287(g) program, suggesting no effect of 287(g) programs on math or reading achievement.

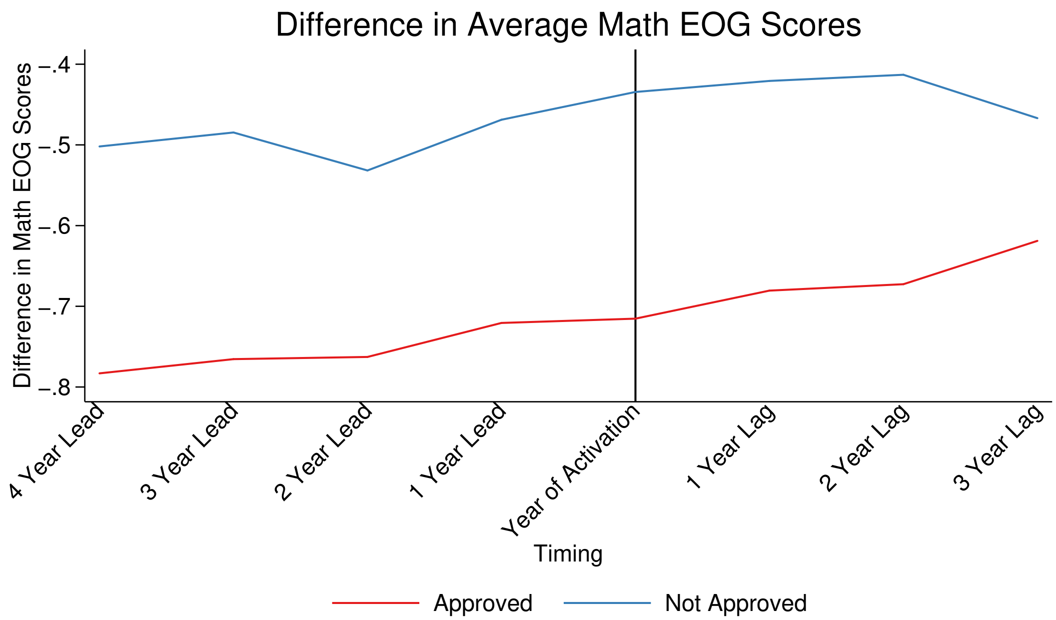

Figure 3.3: Difference in EOG Math Scores Between Hispanic and White Students, Comparing Approved and Denied Counties

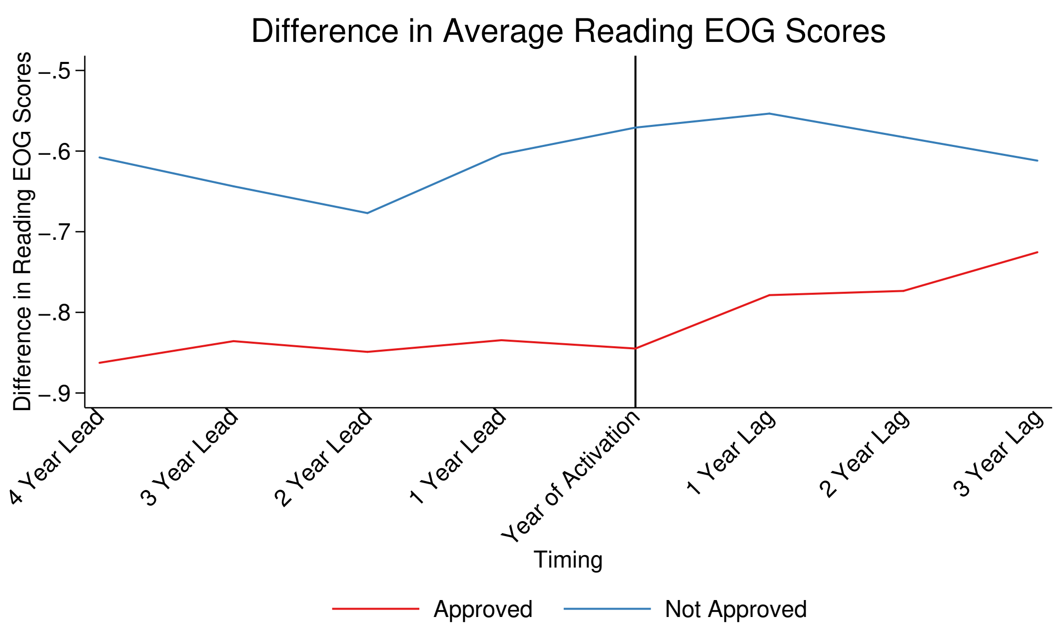

Figure 3.4: Difference in EOG Reading Scores Between Hispanic and White Students, Comparing Approved and Denied Counties

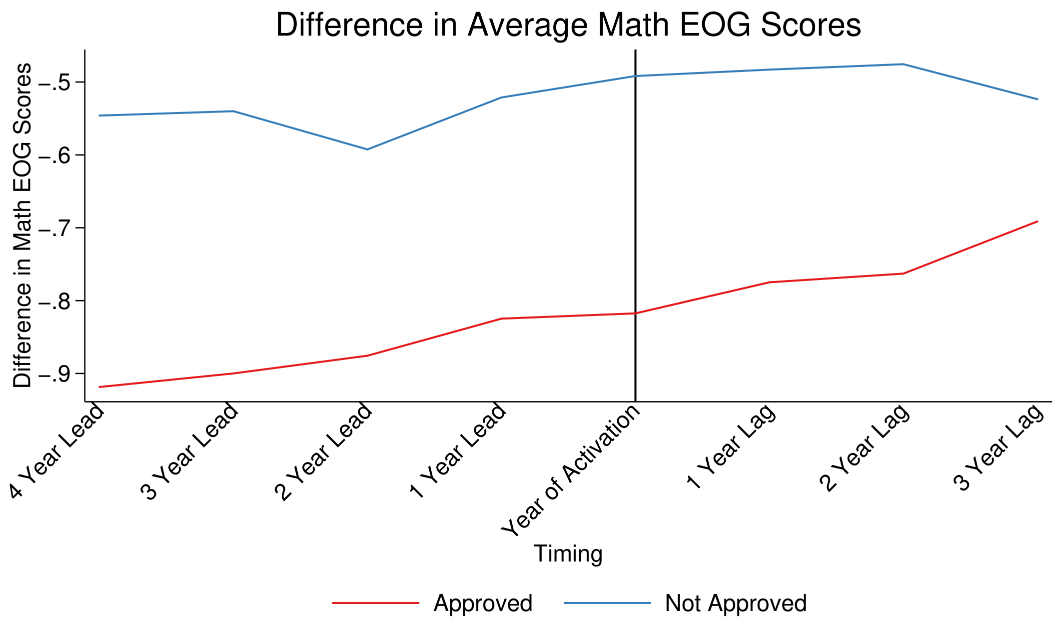

Figure 3.5: Difference in EOG Math Scores Between Hispanic, Ever LEP Students to White, Never LEP Students

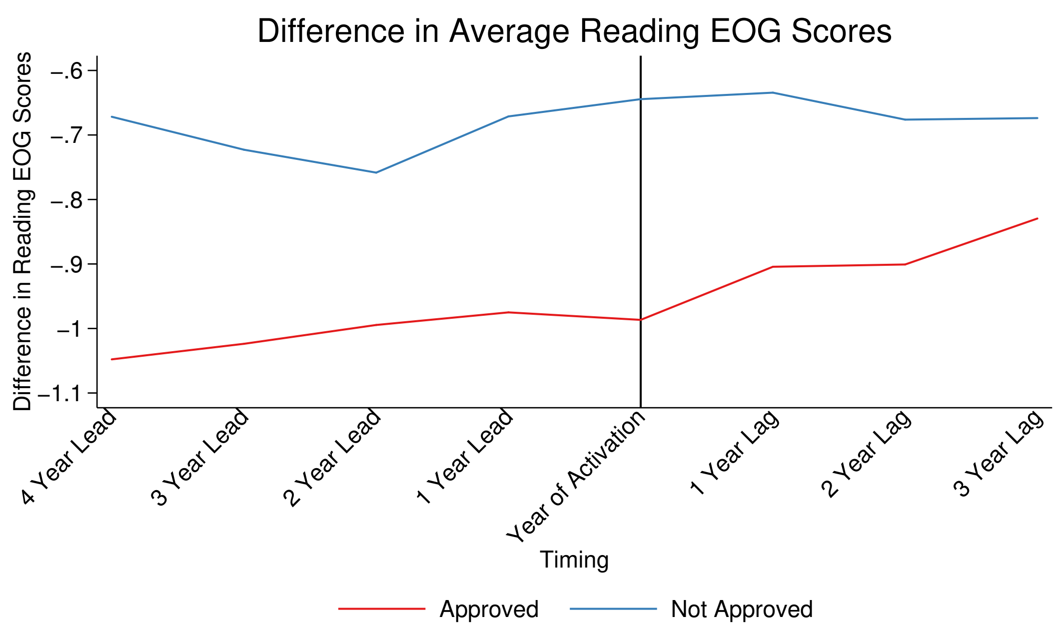

Figure 3.6: Difference in EOG Reading Scores Between Hispanic, Ever LEP Students and White, Never LEP Students

3.7.2 Main Findings

In my main results, I confirm these descriptive results suggesting no effect of 287(g) programs on student test scores. As shown in Tables 3.3 and 3.4, I find no effect of the activation of 287(g) programs in North Carolina for Hispanic students in either math or reading achievement. Similarly, when I further restrict my treatment group to Hispanic students ever identified as LEP and my control group to white students never identified as LEP, I find no effect of the activation of 287(g) programs for Hispanic students ever identified as LEP.

In chapter 1 of this dissertation, I find that the rollout of Secure Communities had effects on achievement for both Hispanic and black students. Therefore, I estimate similar models comparing black to white students and also find no effect of the activation of 287(g) programs in North Carolina on black students, in either math or reading achievement (Tables 3.7 and 3.8).

Table 3.3: Effect of 287(g) Programs on Math Achievement, Comparing Hispanic to White Students

Variables	(1) Math	(2) Math	(3) Math
Hispanic (T)	-0.4921***	-0.4908***	-0.4890***
	(0.0000)	(0.0000)	(0.0000)
	[-0.5736, -0.3846]	[-0.5785, -0.3836]	[-0.5660, -0.4008]
Approved (A)	0.1905	0.1912	
	(0.1471)	(0.1752)	
	[-0.0532, 0.4054]	[-0.0698, 0.4010]	
Post (P)	0.0420	0.0653	0.0206
	(0.1151)	(0.5766)	(0.3223)
	[-0.0127, 0.0852]	[-0.1492, 0.2309]	[-0.0279, 0.0670]
T X A	-0.2625*	-0.2624**	-0.2749**
	(0.0551)	(0.0360)	(0.0190)
	[-0.4896, 0.0076]	[-0.4891, -0.0122]	[-0.4569, -0.0391]
T X P	0.0638*	0.0598*	0.0688**
	(0.0621)	(0.0821)	(0.0400)
	[-0.0025, 0.1441]	[-0.0083, 0.1306]	[0.0028, 0.1457]
A X P	-0.0777	-0.0967	-0.0699*
	(0.5335)	(0.3924)	(0.0591)
	[-0.3101, 0.1808]	[-0.3030, 0.1220]	[-0.1359, 0.0028]
T X A X P	0.0091	0.0128	-0.0348
	(0.9249)	(0.8869)	(0.7047)
	[-0.1976, 0.2111]	[-0.1885, 0.2176]	[-0.2162, 0.1455]
Observations	901,185	901,185	901,185
R-squared	0.0586	0.0592	0.0598
Grade and Gender	No	Yes	Yes
Secure Communities	No	Yes	Yes
Year FE	No	Yes	Yes
County FE	No	No	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 3.4: Effect of 287(g) Programs on Reading Achievement, Comparing Hispanic to White Students

Variables	(1) Reading	(2) Reading	(3) Reading
Hispanic (T)	-0.6326***	-0.6306***	-0.6229***
	(0.0000)	(0.0000)	(0.0000)
	[-0.7283, -0.5285]	[-0.7219, -0.5274]	[-0.7115, -0.5370]
Approved (A)	0.1803*	0.1806	
	(0.0981)	(0.1111)	
	[-0.0317, 0.3796]	[-0.0480, 0.3726]	
Post (P)	0.0242	0.0048	-0.0162
	(0.2573)	(0.9449)	(0.3473)
	[-0.0187, 0.0679]	[-0.1581, 0.1358]	[-0.0498, 0.0213]
T X A	-0.2108*	-0.2128*	-0.2273*
	(0.0881)	(0.0881)	(0.0561)
	[-0.4217, 0.0289]	[-0.4112, 0.0300]	[-0.4126, 0.0071]
T X P	0.0621**	0.0563**	0.0609**
	(0.0250)	(0.0460)	(0.0130)
	[0.0118, 0.1138]	[0.0033, 0.1112]	[0.0183, 0.1097]
A X P	-0.0592	-0.0708	-0.0194
	(0.5345)	(0.4915)	(0.4785)
	[-0.2425, 0.1258]	[-0.2706, 0.1062]	[-0.0724, 0.0366]
T X A X P	-0.0112	-0.0036	-0.0401
	(0.8779)	(0.9570)	(0.6046)
	[-0.1774, 0.1477]	[-0.1812, 0.1552]	[-0.1913, 0.0982]
Observations	897,950	897,950	897,950
R-squared	0.0784	0.0839	0.0826
Grade and Gender	No	Yes	Yes
Secure Communities	No	Yes	Yes
Year FE	No	Yes	Yes
County FE	No	No	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 3.5: Effect of 287(g) Programs on Math Achievement, Comparing Hispanic, Ever LEP to White, Never LEP Students

Variables	(1) Math	(2) Math	(3) Math
Hispanic, Ever LEP (T)	-0.5459***	-0.5447***	-0.5369***
	(0.0020)	(0.0010)	(0.0000)
	[-0.6543, -0.4163]	[-0.6516, -0.4142]	[-0.6405, -0.4261]
Approved (A)	0.1950	0.1959	
	(0.1622)	(0.1421)	
	[-0.0658, 0.4138]	[-0.0576, 0.4120]	
Post (P)	0.0387	0.0690	0.0209
	(0.1542)	(0.5546)	(0.3403)
	[-0.0169, 0.0817]	[-0.1501, 0.2451]	[-0.0316, 0.0667]
T X A	-0.3291**	-0.3291**	-0.3564***
	(0.0250)	(0.0110)	(0.0010)
	[-0.5794, -0.0514]	[-0.5608, -0.0617]	[-0.5539, -0.1411]
T X P	0.0578	0.0528	0.0626*
	(0.1161)	(0.1421)	(0.0761)
	[-0.0137, 0.1389]	[-0.0248, 0.1320]	[-0.0102, 0.1381]
A X P	-0.0783	-0.0996	-0.0742**
	(0.5455)	(0.3954)	(0.0330)
	[-0.3162, 0.1639]	[-0.3245, 0.1137]	[-0.1434, -0.0078]
T X A X P	0.0430	0.0472	0.0053
	(0.6507)	(0.6486)	(0.9479)
	[-0.1572, 0.2625]	[-0.1589, 0.2748]	[-0.1673, 0.1877]
Observations	854,749	854,749	854,749
R-squared	0.0651	0.0659	0.0660
Grade and Gender	No	Yes	Yes
Secure Communities	No	Yes	Yes
Year FE	No	Yes	Yes
County FE	No	No	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 3.6: Effect of 287(g) Programs on Reading Achievement, Comparing Hispanic, Ever LEP to White, Never LEP Students

Variables	(1) Reading	(2) Reading	(3) Reading
Hispanic, Ever LEP (T)	-0.7069***	-0.7045***	-0.6932***
	(0.0000)	(0.0000)	(0.0000)
	[-0.8451, -0.5763]	[-0.8355, -0.5603]	[-0.8112, -0.5805]
Approved (A)	0.1817	0.1819	
	(0.1201)	(0.1221)	
	[-0.0383, 0.3671]	[-0.0432, 0.3690]	
Post (P)	0.0213	0.0085	-0.0169
	(0.3243)	(0.8959)	(0.3554)
	[-0.0238, 0.0650]	[-0.1596, 0.1452]	[-0.0495, 0.0202]
T X A	-0.3002**	-0.3024**	-0.3244***
	(0.0190)	(0.0150)	(0.0010)
	[-0.5252, -0.0442]	[-0.5156, -0.0554]	[-0.4994, -0.1216]
T X P	0.0575**	0.0504*	0.0545*
	(0.0470)	(0.0921)	(0.0581)
	[0.0007, 0.1198]	[-0.0091, 0.1137]	[-0.0016, 0.1074]
A X P	-0.0574	-0.0708	-0.0235
	(0.5766)	(0.5025)	(0.3754)
	[-0.2528, 0.1352]	[-0.2682, 0.1196]	[-0.0759, 0.0308]
T X A X P	0.0281	0.0375	0.0037
	(0.7187)	(0.6356)	(0.9550)
	[-0.1391, 0.1980]	[-0.1356, 0.2068]	[-0.1350, 0.1492]
Observations	851,653	851,653	851,653
R-squared	0.0904	0.0956	0.0939
Grade and Gender	No	Yes	Yes
Secure Communities	No	Yes	Yes
Year FE	No	Yes	Yes
County FE	No	No	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 3.7: Effect of 287(g) Programs on Math Achievement, Comparing Black to White Students

Variables	(1) Math	(2) Math	(3) Math
Black (T)	-0.7911***	-0.7921***	-0.7927***
	(0.0000)	(0.0000)	(0.0000)
	[-0.8475, -0.7143]	[-0.8490, -0.7127]	[-0.8738, -0.6802]
Approved (A)	0.1905	0.1908	
	(0.1542)	(0.1461)	
	[-0.0576, 0.4121]	[-0.0538, 0.3924]	
Post (P)	0.0420	0.0627	0.0158
	(0.1231)	(0.4635)	(0.5646)
	[-0.0150, 0.0851]	[-0.1079, 0.2081]	[-0.0465, 0.0705]
T X A	-0.1485	-0.1486	-0.1419
	(0.1702)	(0.1742)	(0.1652)
	[-0.3435, 0.0479]	[-0.3414, 0.0573]	[-0.3230, 0.0603]
T X P	0.0448**	0.0420**	0.0399***
	(0.0300)	(0.0310)	(0.0030)
	[0.0066, 0.0928]	[0.0035, 0.0903]	[0.0171, 0.0750]
A X P	-0.0777	-0.0897	-0.0549
	(0.5095)	(0.4555)	(0.2703)
	[-0.3258, 0.1520]	[-0.3132, 0.1445]	[-0.1600, 0.0436]
T X A X P	-0.0023	0.0022	-0.0494
	(0.9850)	(0.9880)	(0.5716)
	[-0.1963, 0.2164]	[-0.1981, 0.2085]	[-0.2087, 0.1159]
Observations	1,168,793	1,168,793	1,168,793
R-squared	0.1690	0.1693	0.1612
Grade and Gender	No	Yes	Yes
Secure Communities	No	Yes	Yes
Year FE	No	Yes	Yes
County FE	No	No	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 3.8: Effect of 287(g) Programs on Reading Achievement, Comparing Black to White Students

Variables	(1) Reading	(2) Reading	(3) Reading
Black (T)	-0.7480***	-0.7512***	-0.7577***
	(0.0000)	(0.0000)	(0.0000)
	[-0.8197, -0.6396]	[-0.8208, -0.6527]	[-0.8372, -0.6439]
Approved (A)	0.1803	0.1806*	
	(0.1101)	(0.0991)	
	[-0.0372, 0.3787]	[-0.0322, 0.3637]	
Post (P)	0.0242	0.0140	-0.0064
	(0.2713)	(0.8398)	(0.6687)
	[-0.0221, 0.0687]	[-0.1030, 0.1190]	[-0.0364, 0.0258]
T X A	-0.1092	-0.1086	-0.1039
	(0.2543)	(0.2492)	(0.2072)
	[-0.2922, 0.0752]	[-0.2852, 0.0811]	[-0.2717, 0.0707]
T X P	-0.0055	-0.0140	-0.0138
	(0.7397)	(0.5295)	(0.3173)
	[-0.0381, 0.0286]	[-0.0592, 0.0269]	[-0.0420, 0.0148]
A X P	-0.0592	-0.0705	-0.0144
	(0.5365)	(0.4795)	(0.7407)
	[-0.2408, 0.1296]	[-0.2722, 0.1191]	[-0.0961, 0.0690]
T X A X P	0.0171	0.0282	-0.0135
	(0.8398)	(0.7157)	(0.8218)
	[-0.1265, 0.1705]	[-0.1260, 0.1790]	[-0.1296, 0.0982]
Observations	1,166,671	1,166,671	1,166,671
R-squared	0.1509	0.1582	0.1514
Grade and Gender	No	Yes	Yes
Secure Communities	No	Yes	Yes
Year FE	No	Yes	Yes
County FE	No	No	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

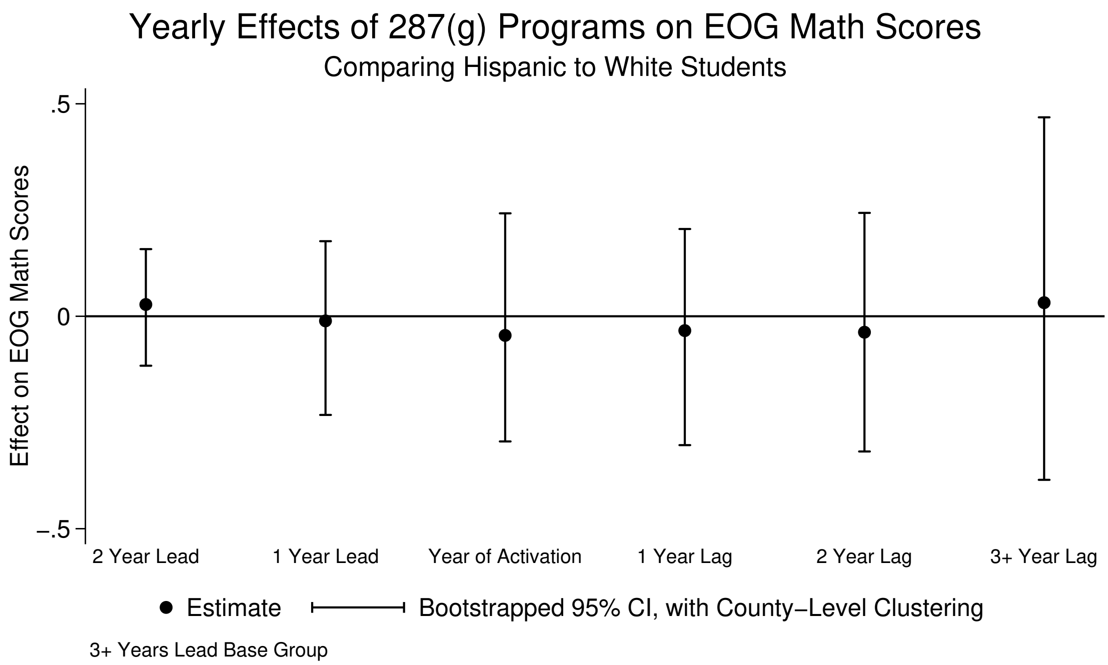

Figure 3.7: Event Study for Effects on EOG Math Scores

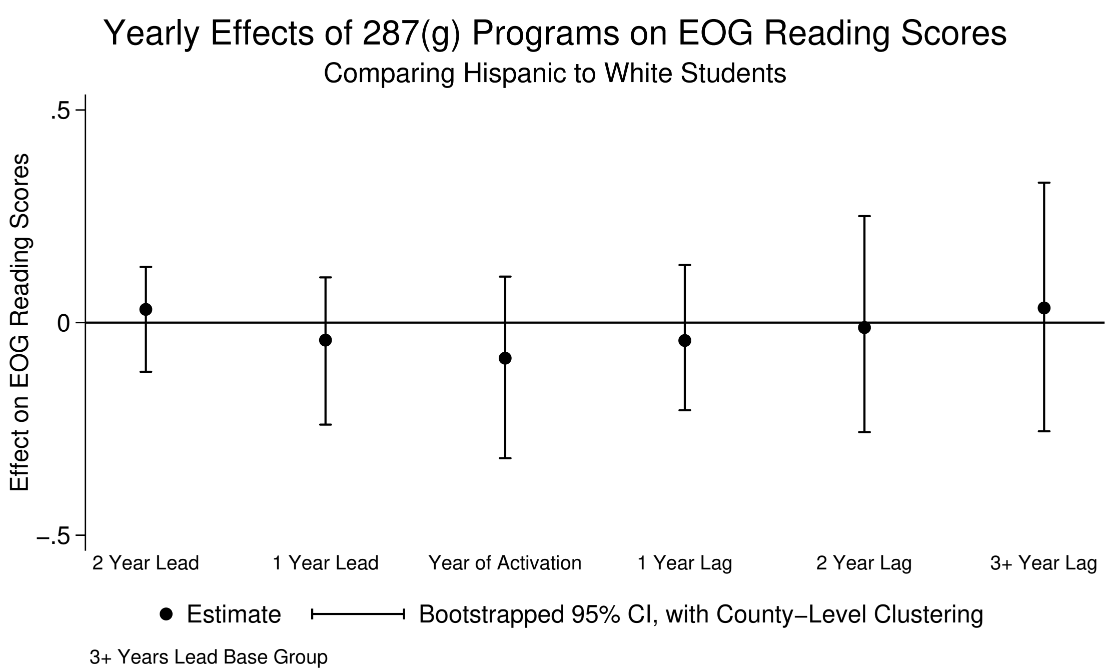

Figure 3.8: Event Study for Effects on EOG Reading Scores

3.7.3 Event History Analysis

I conduct an event history analysis in which I interact indicators for my treatment group and approved counties with a series of leading and lagged indicators, with the periods three years or more prior to activation serving as the base group. I do this both to verify my assumptions about parallel trends prior to activation of 287(g) programs and to determine whether effects of 287(g) programs differ over the course of the program. As shown in Figures 3.7 and 3.8, I see no effects of 287(g) activation on a one or two year lead of program activation. I continue to see no effects of 287(g) activation on math or reading test scores.

3.7.4 Robustness Checks

I conduct a series of analyses altering sample construction and modeling decisions. Results, shown in Tables 3.9 and 3.10, continue to show no effect of 287(g) program activation on math or reading EOG test scores for Hispanic students. First, I include county time trends. Second, I include students testing in 2011-2012 and 2012-2013. Third, I include only students whom I can observe making regular progress in grades 3-8 between 2003-2004 and 2010-2011: therefore, I exclude all students I observe repeating grades, skipping grades, or missing a state EOG in a particular year (or moving out-of-state). Finally, I use students' county as identified in the 2004-2005 school year.

Table 3.9: Robustness Checks for Effects of 287(g) Programs on EOG Math Scores

Check	(1) With County Time Trends	(2) Including 2012 and 2013	(3) Excluding Repeaters	(4) 2005 County
Hispanic (T)	-0.4908***	-0.4888***	-0.4302***	-0.4677***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
	[-0.5697, -0.3954]	[-0.5699, -0.3940]	[-0.4713, -0.3750]	[-0.5477, -0.3970]
Post (P)	0.0093	0.0197	0.0173	0.0256
	(0.5175)	(0.3634)	(0.4144)	(0.3363)
	[-0.0203, 0.0443]	[-0.0266, 0.0669]	[-0.0330, 0.0587]	[-0.0394, 0.0777]
T X A	-0.2721**	-0.2755**	-0.2595***	-0.3064*
	(0.0170)	(0.0210)	(0.0050)	(0.0531)
	[-0.4710, -0.0494]	[-0.4713, -0.0488]	[-0.3988, -0.0815]	[-0.4744, 0.0132]
T X P	0.0754**	0.0726**	0.0252	0.0636
	(0.0440)	(0.0310)	(0.5215)	(0.2833)
	[0.0072, 0.1488]	[0.0079, 0.1436]	[-0.0534, 0.1064]	[-0.0511, 0.2047]
A X P	-0.0493**	-0.0688*	-0.0631*	-0.0484
	(0.0420)	(0.0571)	(0.0611)	(0.2172)
	[-0.0996, -0.0018]	[-0.1403, 0.0034]	[-0.1259, 0.0029]	[-0.1362, 0.0323]
T X A X P	-0.0444	-0.0373	-0.0096	-0.0071
	(0.6416)	(0.6767)	(0.8989)	(0.9369)
	[-0.2359, 0.1372]	[-0.2132, 0.1374]	[-0.1425, 0.1290]	[-0.1921, 0.1945]
Observations	901,185	904,997	407,447	1,582,626
R-squared	0.0604	0.0606	0.0524	0.0403
Grade & Gender	Yes	Yes	Yes	Yes
SC	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 3.10: Robustness Checks for Effects of 287(g) Programs on EOG Reading Scores

Check	(1) With County Time Trends	(2) Including 2012 and 2013	(3) Excluding Repeaters	(4) 2005 County
Hispanic (T)	-0.6231***	-0.6240***	-0.5811***	-0.5947***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
	[-0.7108, -0.5327]	[-0.7124, -0.5378]	[-0.6418, -0.5149]	[-0.6741, -0.5247]
Post (P)	-0.0060	-0.0170	-0.0233	0.0022
	(0.6016)	(0.3664)	(0.1361)	(0.8669)
	[-0.0333, 0.0192]	[-0.0532, 0.0179]	[-0.0514, 0.0082]	[-0.0378, 0.0311]
T X A	-0.2259*	-0.2266*	-0.1910**	-0.2518*
	(0.0641)	(0.0531)	(0.0310)	(0.0811)
	[-0.4001, 0.0141]	[-0.4047, 0.0042]	[-0.3166, -0.0206]	[-0.4126, 0.0781]
T X P	0.0626**	0.0616***	0.0385	0.0579
	(0.0110)	(0.0090)	(0.1682)	(0.2673)
	[0.0203, 0.1092]	[0.0170, 0.1100]	[-0.0181, 0.0916]	[-0.0430, 0.1708]
A X P	-0.0326*	-0.0185	-0.0065	-0.0143
	(0.0631)	(0.5155)	(0.7858)	(0.5726)
	[-0.0641, 0.0023]	[-0.0728, 0.0453]	[-0.0587, 0.0500]	[-0.0650, 0.0373]
T X A X P	-0.0451	-0.0408	-0.0278	-0.0170
	(0.5445)	(0.5566)	(0.5786)	(0.8038)
	[-0.2007, 0.1022]	[-0.1926, 0.1041]	[-0.1325, 0.0782]	[-0.1775, 0.1416]
Observations	897,950	901,707	406,614	1,577,906
R-squared	0.0830	0.0834	0.0721	0.0595
Grade & Gender	Yes	Yes	Yes	Yes
SC	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** $p<0.01$, ** $p<0.05$, * $p<0.10$

3.7.5 Varying Effects Based on Grade Level

Although 287(g) programs do not have an overall effect on student test scores, these programs might affect achievement for subgroups of students. To investigate whether effects differ for students based on age, I split models by students in grades 3-5 and students in grades 6-8. As shown in Tables 3.11 and 3.12, no estimates are statistically significant, but results are suggestive. Coefficients for estimates of effects in elementary school are positive and close to zero, whereas coefficients for estimates of effects in middle school are negative and larger. This is similar to my finding that 287(g) programs had no effect on absences for either elementary or middle school students.

Table 3.11: Effect of 287(g) Programs on EOG Math Scores, Comparing Hispanic to White Students, Split by Grade

	(1)	(2)
Grades	3-5	6-8
Hispanic (T)	-0.4701***	-0.5127***
	(0.0000)	(0.0000)
	[-0.5177, -0.4096]	[-0.6454, -0.3794]
Post (P)	-0.0025	0.0051
	(0.9600)	(0.8308)
	[-0.0928, 0.0886]	[-0.0413, 0.0593]
T X A	-0.2744***	-0.2750*
	(0.0060)	(0.0641)
	[-0.4326, -0.0764]	[-0.5210, 0.0148]
T X P	0.0222	0.0978**
	(0.7187)	(0.0160)
	[-0.1424, 0.1860]	[0.0303, 0.1647]
A X P	-0.0019	-0.0724**
	(0.9800)	(0.0300)
	[-0.1580, 0.1365]	[-0.1348, -0.0074]
T X A X P	0.0118	-0.0467
	(0.9229)	(0.6436)
	[-0.2194, 0.2411]	[-0.2484, 0.1436]
Observations	368,656	532,529
R-squared	0.0612	0.0592
Grade and Gender	Yes	Yes
Secure Communities	Yes	Yes
Year FE	Yes	Yes
County FE	Yes	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 3.12: Effect of 287(g) Programs on EOG Reading Scores, Comparing Hispanic to White Students, Split by Grade

	(1)	(2)
Grades	3-5	6-8
Hispanic (T)	-0.6197***	-0.6235***
	(0.0000)	(0.0000)
	[-0.6808, -0.5459]	[-0.7496, -0.5118]
Post (P)	-0.0448	-0.0232
	(0.2392)	(0.5746)
	[-0.1408, 0.0275]	[-0.0799, 0.0287]
T X A	-0.2273**	-0.2303*
	(0.0220)	(0.0991)
	[-0.3763, -0.0413]	[-0.4697, 0.0438]
T X P	0.0064	0.0640**
	(0.8759)	(0.0220)
	[-0.1128, 0.1108]	[0.0121, 0.1142]
A X P	0.0518	-0.0307
	(0.4354)	(0.2853)
	[-0.0818, 0.1934]	[-0.0769, 0.0210]
T X A X P	0.0087	-0.0395
	(0.9369)	(0.5976)
	[-0.2009, 0.2132]	[-0.2113, 0.1115]
Observations	366,794	531,156
R-squared	0.0842	0.0815
Grade and Gender	Yes	Yes
Secure Communities	Yes	Yes
Year FE	Yes	Yes
County FE	Yes	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

3.7.6 Varying Effects Based on Gender

When I estimate the effects of 287(g) programs on student absences separately for male and female students, effects for male students are twice as large as effects for female students. I therefore split models investigating the effect of 287(g) programs on student achievement between students identified as either male or female. As shown in Tables 3.13 and 3.14, I continue to find no effect of 287(g) programs. Indeed, surprisingly, coefficient estimates are larger for female, rather than male, students, although no estimates are statistically significant.

Table 3.13: Effect of 287(g) Programs on Absence Counts, Comparing Hispanic to White Students, Split by Gender

	(1)	(2)
Gender	Male	Female
Hispanic (T)	-0.5071***	-0.4693***
	(0.0000)	(0.0000)
	[-0.5970, -0.3988]	[-0.5442, -0.3844]
Post (P)	0.0187	0.0227
	(0.4244)	(0.2893)
	[-0.0289, 0.0666]	[-0.0362, 0.0729]
T X A	-0.2773**	-0.2731***
	(0.0300)	(0.0090)
	[-0.4897, -0.0308]	[-0.4576, -0.0576]
T X P	0.0800**	0.0565*
	(0.0390)	(0.0951)
	[0.0068, 0.1592]	[-0.0093, 0.1397]
A X P	-0.0736**	-0.0656*
	(0.0410)	(0.0771)
	[-0.1418, -0.0047]	[-0.1366, 0.0088]
T X A X P	-0.0252	-0.0446
	(0.8078)	(0.5876)
	[-0.2217, 0.1667]	[-0.2098, 0.1216]
Observations	458,063	443,122
R-squared	0.0594	0.0607
Grade	Yes	Yes
Secure Communities	Yes	Yes
Year FE	Yes	Yes
County FE	Yes	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

Table 3.14: Effect of 287(g) Programs on Absence Rate, Comparing Hispanic to White Students, Split by Gender

	(1)	(2)
Gender	Male	Female
Hispanic (T)	-0.6282***	-0.6173***
	(0.0000)	(0.0000)
	[-0.7146, -0.5346]	[-0.7136, -0.5215]
Post (P)	-0.0141	-0.0186
	(0.4174)	(0.3493)
	[-0.0472, 0.0249]	[-0.0567, 0.0193]
T X A	-0.2316*	-0.2229**
	(0.0641)	(0.0420)
	[-0.4312, 0.0186]	[-0.3967, -0.0035]
T X P	0.0628**	0.0584**
	(0.0130)	(0.0330)
	[0.0171, 0.1117]	[0.0055, 0.1227]
A X P	-0.0322	-0.0060
	(0.2933)	(0.8288)
	[-0.0913, 0.0283]	[-0.0578, 0.0518]
T X A X P	-0.0179	-0.0620
	(0.8078)	(0.3714)
	[-0.1845, 0.1417]	[-0.1975, 0.0653]
Observations	455,800	442,150
R-squared	0.0750	0.0825
Grade	Yes	Yes
Secure Communities	Yes	Yes
Year FE	Yes	Yes
County FE	Yes	Yes

P-values (in parentheses) and confidence intervals (in brackets) obtained through wild cluster bootstrap, with clustering at the county-level (999 replications)
*** p<0.01, ** p<0.05, * p<0.10

3.7.7 Comparing Effects to County-Level Analysis

I am able to compare results from chapter 1 to results from this analysis. I limit years to 2008-2009 through 2012-2013 and do not account for mobility, meaning that

I use all students regardless of whether they can be observed prior to the activation of any 287(g) programs. I also include all public school students in North Carolina. I estimate the following equation:

$$\text{Score} = \alpha + \beta_1 287\text{(g)active} + \beta_3 \text{SC} + \beta_m \text{Student} + \phi + \gamma + \eta + \epsilon \qquad (3.2)$$

Here, Score is a math or reading test score for an individual Hispanic, black, or white student in a particular year; 287(g)active is an indicator variable for an active 287(g) program in that year; and SC is an indicator for an active Secure Communities program. I control for student gender, as well as county fixed effects (ϕ); grade fixed effects (γ); and year fixed effects (η). I cluster standard errors at the county-level.

As shown in Table 3.15, the activation of Secure Communities appears to reduce achievement for Hispanic students in math by about 3.6 percent of a standard deviation. However, this effect on math achievement for white students is similarly sized, at about 3.5 percent of a standard deviation. The effect on math achievement for black students is about 5 percent of a standard deviation.

Table 3.15: Comparing to Models in Chapter 1

	(1) Hispanic ELA	(2) Hispanic Math	(3) White ELA	(4) White Math	(5) Black ELA	(6) Black Math
287(g)	-0.0000	0.0006	-0.0085	-0.0080	-0.0141**	-0.0198**
	(0.0068)	(0.0094)	(0.0058)	(0.0076)	(0.0057)	(0.0076)
SC	-0.0220	-0.0366**	-0.0136**	-0.0347***	-0.0278**	-0.0501***
	(0.0191)	(0.0168)	(0.0059)	(0.0112)	(0.0115)	(0.0121)
Obs.	392,681	399,437	1,723,609	1,731,195	821,407	825,951
R^2	0.0149	0.0017	0.0974	0.1757	0.2185	0.0075

Regressions control for grade, year, and county fixed effects
Robust standard errors, clustered at the county-level, in parentheses
*** p<0.01, ** p<0.05, * p<0.10

Secure Communities also has negative effects for white and black students in ELA, reducing achievement by 1.4 percent of a standard deviation and 2.8 percent of a standard deviation, respectively. Although the decrease is not statistically significant for Hispanic students, it is about the same size: 2.2 percent of a standard deviation. Although an active 287(g) program has no effect on Hispanic students in math or ELA, it does reduce achievement for black students in both subjects.

3.8 Discussion

In qualitative work, increases in immigration enforcement appear to have strong effects on children's performance in schools (Capps et al., 2007); immigration raids in particular appear to decrease student achievement (Zuniga, 2018). My work is the first to examine the effects of partnerships between ICE and local law enforcement, specifically, on student achievement, and I reach more mixed results. In chapter 1, I find that the activation of Secure Communities decreased achievement in ELA. However, 287(g) programs, a more intense form of treatment than Secure Communities, did not affect student achievement for the broader group of Hispanic students or the most likely exposed group, Hispanic students ever identified as Limited English Proficient (LEP). This suggests that results for Secure Communities may reflect preexisting characteristics about the counties in which the program was first activated, rather than a true effect of the program.

These findings build on prior work in multiple ways. First, I use individual-level data on student achievement for the entire state of North Carolina, which allows me to follow individual students over time and control for time-invariant characteristics about counties. All prior work on immigration enforcement and achievement uses county-level data, which does not account for possible student mobility. Second, I use a quasi-experimental design that allows me to arrive at plausibly causal estimates. Since immigration enforcement is likely to be more intense in areas with higher pre-existing levels of anti-immigrant animus, associations between measures such as total removals and test scores are unlikely to reflect the effects of immigration enforcement programs alone.

The lack of effect on Hispanic students, as well as Hispanic students ever identified as LEP, is somewhat surprising. However, it matches with the results on absences presented in chapter 2 of this dissertation, in which it appears that effects of 287(g) programs on attendance are largely driven by high school students. However, in contrast with results for absences, I find no effect of 287(g) program activation on the math or reading achievement of male students. This again suggests that results for absences may be driven by male high school students. Next, I plan to investigate the effects of 287(g) programs on achievement for high school students, as well as dropout rates.

Hispanic immigrant communities are aware of 287(g) programs and experience increased stress; however, 287(g) programs do not affect student achievement in grades 3-8. In contrast, in chapter 2, I find that 287(g) programs increase absences and that the effect may be concentrated amongst high school students. Children in early and middle grades may be less aware of these programs than their parents or older siblings. 287(g) programs may not have effects on achievement because of their more isolated impacts. These programs likely do not have the immediate salience of a large immigration raid, particularly a raid in the same community in which many adults are arrested. Indeed, if a parent or family member is arrested and removed via a 287(g) program, families may avoid discussing this either out of fear of greater attention from immigration authorities or because of stigma associated with criminality.

3.9 Conclusion

Although removals were highest during the Obama administration, in recent years the administration had refocused priorities on detaining and removing serious criminal offenders. By January 2017, there were only 30 active 287(g) agreements. However, the program has expanded under the current administration: in February 2018, there were 76 active 287(g) agreements, the most in the history of the program (Capps et al., 2018). Understanding the impacts of these programs is therefore important, in the current context.

Conclusion

This dissertation investigates the effects of immigration enforcement on educational outcomes, using national data at the county-level and North Carolina data at the individual-level. Immigration raids have been associated with decreased attendance and achievement. However, these are, to my knowledge, the first papers to investigate the effects of partnerships between local law enforcement and ICE on student achievement, attendance, and out-of-school suspensions.

I reach mixed results. In chapter 2, I find that partnerships between local law enforcement and ICE decrease attendance, but in chapters 1 and 3 I do not detect consistent effects on math and English Language Arts (ELA) test scores in grades 3-8. This may either be because these partnerships do not impact test scores or because these partnerships have stronger effects on high school students, where effects on attendance are concentrated.

I find some suggestive evidence that cooperation on the part of local law enforcement matters for effects. In chapter 1, I find that areas with more intense enforcement, as measured by either change in removals from year to year or percent of identified individuals removed overall, have lower test scores. Without cooperation from local law enforcement, ICE has drastically reduced ability to arrest and remove individuals. Therefore, although immigration policy is thought of as being set at the national level, local policy is currently determining what immigration enforcement looks like across the U.S.

These studies also extend the literature on estimating the effects of immigration enforcement policies. I find that identification strategies relying only on policy activation (either of Secure Communities or 287(g) programs) may contain bias. Although Secure Communities appears to decrease ELA achievement for Hispanic students and ELA and math achievement for black students, these results are suspect because the timing of rollout appears to be correlated with other county characteristics and determined by factors known only to Immigration and Customs Enforcement (ICE).

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Biography

Laura Bellows matriculated at Duke in 2014 to begin a Ph.D. in Public Policy. While at Duke University, she was awarded the James B. Duke Fellowship (2014-2018), the Education and Human Development Scholars Award (2016), and the Katherine Goodman Stern Fellowship (2018-2019). She also was awarded grants from the Horowitz Foundation for Social Policy and the Russell Sage Foundation.

Laura has one article published in the *American Educational Research Journal* titled "Food Instability and Academic Achievement: A Quasi-Experiment Using SNAP Benefit Timing" (with Anna Gassman-Pines). She has three articles published on evaluating teacher preparation programs in *Economics of Education Review* and *Journal of Teacher Education* with Paul von Hippel, Cynthia Osborne, Jane Lincove, and Nicholas Mills.

Laura previously earned a B.A. in History and English from Rice University in 2006 and a Master of Public Affairs from Indiana University in 2011. She became interested in how out-of-school policies affect children's educational opportunities as a fourth grade teacher in the Houston Independent School District and a legislative aide for Texas State Representative Dora Olivo, who served on Texas's House Public Education Committee. She also spent three years as a research associate at the Child and Family Research Partnership at the LBJ School of Public Affairs. There, she evaluated programs for the Texas Education Agency (TEA), Health and Human Services Commission (HHSC), and Office of the Attorney General's Child Support Division.