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Out of the Shadows and into the Classroom: Immigrant Legalization, Hispanic Schooling and Hispanic Representation on School Boards

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Abstract

I exploit the 1986 Immigration Reform and Control Act (IRCA), which legalized millions of Hispanic migrants in the USA, to study the impact of immigrant legalization on schooling outcomes. Although undocumented migrants are entitled to public education, I find significant post-legalization increases in student enrollment and student-to-teacher ratios in public schools with greater exposure to IRCA migrants. This effect is driven by increased Hispanic enrollment, while whites sort out of public education and into private schooling. The IRCA differentially increases Hispanic school board members and school expenditure, highlighting legal status as a driver of Hispanic human capital accumulation and representation.

JEL-Codes: I210, J150, H520.

Keywords: schooling, human capital, minority representation, legal status.

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1. Introduction

Lack of legal status poses large social and economic costs for migrants and their families. These include limited labor market opportunities, increased stress and anxiety from “living in the shadows” and potential family separation as a result of deportation (Chaudry et al. 2010; Brabeck, Sibley, and Lykes 2016; Cortes 2013; Amuedo-Dorantes and Lopez 2015). Of particular concern are the effects of undocumented status on human capital accumulation. In the United States, for example, 41 percent of Hispanic adults over the age of 20 — of whom some 20 percent are estimated to be undocumented — lack a high school degree compared to 14 and 23 percent, respectively, of white and black adults (Liscow and Woolston 2018).¹ Hispanic participation in the administration of schooling is even more disparate. A 2018 survey by the National School Board Association found that just 3 percent of school board members across the country were Hispanic (compared to 78 percent white and 10 percent black), even though Hispanics represent 27 percent of the K – 12 student population.²

In this paper, I study the impact of changes in legal status on Hispanic schooling outcomes — enrollment and high school completion — and on Hispanic participation in the administration of schooling — specifically, Hispanic representation on school boards. I compare these outcomes across people, public schools and counties in the United States with differential exposure (at the county level) to immigrant legalization under the historic 1986 Immigration Reform and Control Act (IRCA) which legalized millions of Hispanic migrants. Crucially for identification, undocumented migrants are *already included* in census population estimates (Sabet and Yuchtman 2023). Legalization under the IRCA thus removed the costs of illegality and expanded Hispanic opportunity without affecting other demographic characteristics of the communities in which the migrants lived. As a first step in my analysis, therefore, I establish that the IRCA is not associated with differential trends in a wide range of county covariates, conditional on county and state-by-year fixed effects. This increases confidence that my regression framework is able to identify the effect of legal status on schooling outcomes independently of other demographic changes the IRCA may have stimulated.

1. Kuka, Shenhav, and Shih (2020) explain that undocumented youth account for 1.5 percent of all US minors and that 15 to 40 percent of these youth lack a high school degree.

2. The full survey results can be found here. Accessed 20 July 2023.

Ever since the Supreme Court's landmark ruling in *Plyer v. Doe* in 1982, four years prior to the IRCA, undocumented migrants have been legally entitled to public education. In theory, then, one might not expect a relationship between legal status and schooling outcomes because legalization under the IRCA did not affect school eligibility criteria (and, as I demonstrate later, the IRCA did not increase the size of the school age population). In practice, however, there are several reasons why legal status matters for educational access and attainment. These include limited labor market prospects which lower the returns to, and the incentive to invest in, education (Liscow and Woolston 2018; Amuedo-Dorantes and Lopez 2015). Relatedly, academic sorting mechanisms become pronounced in high school — including eligibility for scholarships and access to school resources like college counselors — which raise the cost of further investment in education (Castañeda 2019; Wong et al. 2016). Undocumented migrants may also hold misconceptions concerning the returns to education and experience differentially greater liquidity constraints compared to other migrant children (Kuka, Shenhav, and Shih 2020). Finally, important mental health factors, including stress and anxiety, stemming from constant effort to evade detection can increase the likelihood that undocumented children (or documented children from undocumented households) drop out or repeat grades (Amuedo-Dorantes and Lopez 2015). I therefore expect the expansion of Hispanic opportunity as a result of the removal of these (non-legal) barriers to education to improve schooling outcomes — enrollment and completion rates in particular — and I expect these improvements to be driven by Hispanics and at the level of high schools.

I empirically examine these claims first by comparing schooling outcomes — student enrollment, the size of the teaching staff and student-teacher ratios — in public schools located in counties with higher v. lower shares of IRCA applicants, before and after 1989 when IRCA applicants first gained legal status. I collect information on the universe of public schools in the United States from the Common Core of Data (CCD) Public Schools Universe in order to construct a panel of schools between 1982 and 2000. My baseline model includes a battery of county level covariates, measured in 1980 and interacted with a pre/post-1989 indicator. Because I leverage data from a panel of public schools, I am able to exploit within-school variation in my empirical model. This is particularly important when one considers that the

largest federal education program, Title I of the Elementary and Secondary Education Act, distributes financial aid to schools by way of formula that depends on local poverty rates among school-aged children (Gordon 2004). The inclusion of public school fixed effects thus enables me to separate the effects of legalization on schooling outcomes from the potentially confounding effects of Title I funding.

Using this regression framework, I find that public schools located in high-IRCA counties experience differential increases in student enrollment. The magnitude of the coefficient suggests that a 1 standard deviation increase in a county's share of IRCA applicants increases enrollment by around 5 students, which represents a 1 percent increase relative to the (average) pre-legalization size of the student body. I also find increases in the number of full-time equivalent of classroom teachers (i.e., teaching staff), though the increases in student enrollment appear larger than increases in staffing. This is reflected in the fact that classroom sizes, measured by student-to-teacher ratios, increase significantly in public schools located in high-IRCA counties. These effects are robust across a number of different empirical specifications and sample restrictions and they are not reflective of pre-trends: Prior to legalization, the differences in student enrollment and the size of the teaching staff in public schools located in high- v. low-IRCA counties are indistinguishable from zero (both individually and jointly), display a near zero pre-treatment slope, and increase sharply in 1990, immediately following legalization.

Because legal status becomes more relevant at later stages of public education, I test for heterogeneous effects of the IRCA in public high schools compared to public primary schools. I find that my effects are driven by increases in student enrollment in public high schools. In absolute terms, the magnitude of the coefficient triples compared to the baseline — a one standard deviation increase in a county's IRCA share increases enrollment by 15 students in high schools. Relative to the (average) pre-legalization size of the high school student body, however, the coefficient represents a two percent increase, an effect twice as large as the baseline. I also find increases in teaching staff though, as in the baseline, the overall classroom size also experiences differential increases in high schools.

The Public Schools Universe data include student enrollment by race as of 1987. This enables me to examine the extent to which student enrollment increases observed under the IRCA

are driven by Hispanic youth compared to youth of other racial backgrounds. Reassuringly, I find that the overall increases in student enrollment are driven entirely by Hispanics enrolling in public schools. The magnitude of the coefficient for Hispanic student enrollment is about twice as large as the baseline estimate and there are no effects for white and black students, confirming the view that the legalization of millions of mostly Hispanic migrants increased Hispanic student enrollment. Next, I study the IRCA's racial enrollment effects in public high schools compared to public primary schools. I find that the IRCA increases Hispanic student enrollment in both primary school and secondary school, though the effects for secondary school are about 50 to 60 percent larger than they are for primary schooling, consistent with the notion that legal status is most relevant for Hispanic students in high school. There are no differential patterns for black students in either high school or primary school. By contrast, I find large, negative effects for white student enrollment in public high schools, suggesting that the IRCA led to white dis-enrollment in public education.

To understand why this might be the case, I collect data from the 1980, 1990 and 2000 Decennial Censuses to examine whether the IRCA led to racial sorting into public and private education. Although a repeated cross-section, the patterns revealed in the data confirm those generated using the panel of public schools. I find that Hispanic students of high school age, compared to whites of the same age, are differentially more likely to enroll in public education in high-IRCA counties post-legalization. White students, however, are significantly more likely to enroll in private schooling, helping explain the negative enrollment patterns observed using aggregate data from public high schools. Using these same data, I find that the IRCA significantly increases the likelihood of Hispanics completing high school. There are no such effects for whites and blacks. Taken together, these results suggest that legal status has far-reaching educational consequences. Not only does it significantly increase Hispanic investments in education, it also has considerable spillover effects on the educational decisions of youth of other racial backgrounds, in particular whites who sort differentially into private schooling.

In the next part of my analysis, I study the impact of the IRCA on the administration of schooling. In particular, I study Hispanic representation on school boards (aggregated to the

county level) and on county level expenditure on education.

To examine Hispanic representation on school boards, I digitize a novel source of data with information on 15,000 Hispanic officials elected to public office between 1984 and 1994. I find that counties with higher share of IRCA applicants experience positive and significant increases in the number of Hispanics elected to school boards. What is more, I find that the IRCA does not lead to increases in the total number of local government units in a given county (i.e., the number of school districts, for example), nor does it increase the size of local government units (i.e., the number of officials elected to a school board), suggesting that Hispanic selection to school boards is not simply explained by increases in the number of school board positions. I also find significant reductions in the number of white and black individuals elected to public office as a result of the IRCA, suggesting that legalization increased the competitiveness of Hispanic candidates.³

Importantly, the increases in Hispanic representation on school boards in high-IRCA counties are not confounded by provisions of the 1965 Voting Rights Act (VRA) that also aimed at increasing minority representation. Specifically, Section 5 of the VRA required that jurisdictions covered by it obtain “pre-clearance” for any changes that affect voting. Section 2, meanwhile, prohibits racially discriminatory voting practices or procedures and in 1982, this provision was expanded to prohibit not just discriminatory *intent* but discriminatory *results*, a change which significantly increased minority representation in congress through the creation of minority-majority congressional districts (Washington 2012). To the extent that high-IRCA counties are also affected by these provisions, it is not unreasonable to think that the observed increases in Hispanic representation on school boards simply reflects conformity to VRA requirements. In my analyses (of both schooling outcomes and Hispanic representation on school boards), therefore, I include a specification that controls for the time-varying effect of whether a county is covered by Section 5 of the VRA. I also run my main specification on a sample that excludes counties located in Hispanic or Black majority districts so as to minimize

3. As discussed later in the paper, this increased competitiveness may be because fewer whites and blacks decide to *run* for office, thus increasing the number of available school board positions, or because it increases political mobilization and participation at the local level, enabling Hispanics to win a greater number of positions from candidates of other racial backgrounds, or both. Although I cannot test the extent to which the IRCA affected the number of school board candidates by race, I do present some suggestive evidence that the IRCA increased political participation among Hispanics.

the potentially confounding influence of Section 2 of the VRA. My findings are unaffected by these checks, suggesting that the IRCA's educational effects are not confounded by the VRA.

Finally, I examine how changes in the racial composition of school boards affects public education finance. In line with work by Fischer (2023), I find that the same counties that experience differential increases in Hispanic school board membership also experience positive, significant changes in education expenditure at the level of elementary and secondary schools.⁴ I find no increases in spending for post-secondary education in counties affected by the IRCA. To the extent that spending on education increases student performance (Jackson, Johnson, and Persico 2016), these findings might also explain why legal status matters so much for Hispanic school attendance and completion.

My findings contribute most directly to scholarship that examines the impact of legal status on educational attainment (Kuka, Shenhav, and Shih 2020; Liscow and Woolston 2018; Amuedo-Dorantes and Lopez 2015). Though these studies have done much to shed light on the relationship between legal status and educational outcomes of migrant youth, most of them rely on survey-based proxies to measure undocumented status from the Deferred Action for Childhood Arrivals (DACA) program which is a temporary and conditional amnesty (Kuka, Shenhav, and Shih 2020). My point of departure from this literature is twofold. First, I leverage administrative data on the universe of migrants who applied for, and were mostly granted, legal status under the IRCA, an unconditional, nearly universal, permanent amnesty. Most papers that study the impacts of the IRCA focus on labor market outcomes (Kossoudji and Cobb-Clark 2002; Amuedo-Dorantes and Bansak 2011; Pan 2012; Rivera-Batiz 1999), health outcomes (Baker 2010), crime (Baker 2015) or personal income tax participation (Cascio and Lewis 2019). Cortes (2013) does exploit the IRCA to study its educational responses but focuses her work on access to post-secondary education. To my knowledge, this study is the first to examine the effect of the IRCA on schooling outcomes at the primary and secondary levels.

Second, by examining the impact of the IRCA on Hispanic school board representation, I introduce a new dimension to the (educational) consequences of legal status. In so doing, the paper adds to the literature that examines the consequences of minority political representation.

4. These data do not enable me to distinguish spending at the elementary level compared to the secondary level.

This scholarship has examined Hispanic representation on school boards (though not as a response to immigrant legalization and only in California) (Fischer 2023), women as policy makers (Chattopadhyay and Duflo 2004) and Black office holding at the local level (Bernini, Facchini, and Testa 2023). My findings that the IRCA strengthens local representation of Hispanic migrants on school boards also complement Sabet and Yuchtman (2023) who find that the IRCA increased Hispanic descriptive and substantive representation at the Federal level through congressional redistricting.

In the remainder of the paper, I discuss the institutional details concerning the 1986 IRCA in Section 2 and in Section 3, I provide conceptual motivation as to why legalization under the IRCA should affect Hispanic schooling outcomes and Hispanic representation on school boards. In Section 4, I provide an overview of the data used in the study. In Section 5, I discuss and evaluate my identification strategy. I present my main results on Hispanic schooling outcomes in Section 6 and on Hispanic representation on school boards in Section 7. In Section 8, I present my spending results and I conclude the paper in Section 9.

2. Institutional Background: The 1986 Immigration Reform and Control Act (IRCA)

The Immigration Reform and Control Act (IRCA) of 1986 is, to date, the only piece of legislation passed by the United States government that provides permanent amnesty to undocumented migrants. The Act was signed into law by the Reagan Administration in November 1986 and consisted of three main parts: an employer sanctions provision that made it illegal for employers to knowingly hire unauthorized workers; increased funding for border security to discourage further illegal entry; and an amnesty program intended to legalize various unauthorized workers (Chishti and Kamasaki 2014).⁵ The aim of the Act was the expansion of opportunity. According to Reagan, the IRCA:

Will go far to improve the lives of a class of individuals who now must hide in the shadows, without access to many of the benefits of a free and open society.

5. Specifically, the Act provided two programs for two distinct groups of unauthorized migrants: those who resided in the country for an uninterrupted period from before 1 January 1982; and second a special program for seasonal agricultural workers, Special Agricultural Workers (SAWs), who could demonstrate that they carried out 90 days of work on select USDA defined seasonal crops in the year leading to 1 May 1986 (Rytina 2002).

Very soon many of these men and women will be able to step into the sunlight and, ultimately, if they choose, they may become Americans.⁶

At the time of the IRCA, there were some 3 million undocumented immigrants residing in the United States, corresponding to nearly 1 percent of the population (Rytina 2002). By the end of the application period in 1988, roughly 3 million people applied for temporary resident status, of whom some 2.8 million, or nearly 95 percent of all applicants, were granted permanent residence.⁷ On acceptance of their application, applicants were given temporary legal status under the title of Temporary Resident Aliens which could last for as long as 18 months. After this period, and upon successful completion of an English and civics test, applicants were given permanent resident status. Five years after permanent residency, those legalized by the IRCA were eligible to apply for naturalization (Cascio and Lewis 2019).

Table 1 shows the number of IRCA applicants who legalized (i.e., received permanent legal residency) and who naturalized (i.e., obtained citizenship and voting rights) under the IRCA over time, both in absolute terms and relative to the total number of legalized and naturalized immigrants in the country. As shown, the take up of legal status was immediate, beginning as early as 1989. By 1991, nearly 90 percent of the immigrants who were granted legal status under the IRCA — some 2.5 million immigrants — received it; the less than 200,000 remaining immigrants who received legal status under the IRCA did so, by and large, by 1994. Naturalization patterns, by contrast, look different. In total, just 33 percent of the IRCA migrants — or around 815,000 immigrants — went on to naturalize as US citizens by 2000 and the majority of these happened in the mid to late 1990s. The lag between legalization and naturalization rates under the IRCA reflects the institutional feature of the IRCA that mandated a five year window between legal status and application for citizenship.⁸

6. President Ronald Reagan's statement on signing the IRCA on 6 November 1986. Taken from the American Presidency Project on ebsite. Accessed 1 June 2023.

7. These figures are derived from the Legalization Summary Tapes of the Immigration Naturalization Service. Rytina (2002) suggests the figure stands at 2.68 million owing to duplicate records in the INS tapes. In either case, the take up of legal status under the IRCA was significant. Baker (2015) provides a number of reasons why the IRCA “represented a near-universal legalization of immigrants in the United States” including the fact that universal take up was a deliberate aim of policymakers at the time.

8. Online Appendix A provides details concerning the demographic characteristics of the IRCA applicants, including their geographic distribution. As shown, nearly 90 percent of the migrants legalized under the IRCA were of Mexican or Latin American origin. These migrants, moreover, were an economically active and self-reliant group, earning somewhere between the poverty threshold and median income.

As noted in Sabet and Yuchtman (2023), the IRCA provided legal status to immigrants who were already residing in the United States and who were already included in census population counts. The law thus shocked the legal status of a county's population without altering its underlying demographic or socioeconomic characteristics. What is more, as discussed in the next section, undocumented migrants were already legally entitled to public school access pre-IRCA. Accordingly, the IRCA did *not* affect population counts in the US, nor did it increase the size of the eligible school age population, factors which might have mechanically affected public school outcomes. The IRCA thus provides variation in legal status across US counties that is independent of other socio-demographic changes.

3. Conceptual Motivation

In lifting barriers of social exclusion, the IRCA led to the expansion of opportunity for millions of (mostly) Hispanic migrants. I study the impact of this expanded opportunity on two sets of schooling outcomes: Hispanic investments in schooling — measured by attendance and completion rates — and Hispanic participation in the administration of schooling — specifically, Hispanic representation on school boards. In this section, I provide conceptual motivation that links legal status to these two dimensions of schooling.

3.1 *The IRCA and Hispanic Investments in Schooling*

Access to K – 12 public education for undocumented children and youth prior to the IRCA was a contested issue. In 1975, for example, the state legislature of Texas amended its education laws so that the state could withhold “from local school districts state funds for the education of children who were not legally admitted into the United States” (Gallagher 1982). The 1975 amendment also provided authorization to districts to “deny enrollment in the public school” to undocumented children (Gallagher 1982). In responses to these changes, one school district in Texas — the Tyler Independent School District — adopted in 1977 a policy of charging tuition fees in the order of USD1,000 per undocumented student so as to compensate for lost state funding (Gallagher 1982). The issue found its way to the Supreme Court where, in 1982, it provided a landmark decision in *Plyer v. Doe* [457 US 202]. The Court ruled that the denial of

public education to students on account of legal status violates the Equal Protection Clause of the Fourteenth Amendment. The majority opinion explained that “[w]hatever his status under the immigration laws, an alien is a “person” in any ordinary sense of that term” and as such is entitled to protection from discrimination. The ruling in *Plyer v. Doe* thus provided legal access to public education to undocumented migrants four years prior to the IRCA, ruling out the possibility that immigrant legalization increased incentives to invest in education simply because it expanded the size of the eligible school age population. In theory, therefore, one might expect no relationship between legal status under the IRCA and public school outcomes because lack of legal status did not pose a barrier to public school access.

In practice, however, there are several reasons why undocumented status matters for educational access and attainment at the public school level, even if undocumented migrants were legally entitled to public education. These include limited labor market prospects which lower the returns to, and the incentive to invest in, education (Liscow and Woolston 2018). Relatedly, academic sorting mechanisms become pronounced in high school which differentially affect the incentives of undocumented migrants to invest in education. These include eligibility for scholarships and other forms of financial aid, access to school resources like college counselors and funds to pay for college preparation courses (Castañeda 2019; Wong et al. 2016). In this respect, other Supreme Court cases and federal legislation,⁹ have allowed some states to deny undocumented migrants access to things like in-state tuition or scholarships or even enrollment at public colleges and universities. Speaking to these issues, one undocumented migrant relates the following regarding the barriers of his (lack) of legal status to education:

[W]hen I realized I was undocumented around like 15, like, that’s when it really hit me. And I started doing a lot of research on it — on like what it means to be undocumented . . . I was very, like, afraid for my future because I realized, oh, wait, I can’t get a job. I’m not eligible for any financial aid. I wanted to have a career in civil service, and I realized I couldn’t do any of those things . . . And I was going like, what is the point of me even trying in school? What is the point of me doing anything if I’m not going to be able to have a career or be able to, I guess, be normal

9. In particular *Toll v. Moreno* (1979) as well as the 1996 Illegal Immigration Reform and Immigrant Responsibility Act.

(Liscow and Woolston 2018)?

Another reason why legal status might affect investment in education is because of misconceptions regarding the returns to education among undocumented households or because of the relatively high cost that public schooling presents to undocumented families (Kuka, Shenhav, and Shih 2020; Liscow and Woolston 2018). Finally, another barrier that undocumented children face differentially more than other children are mental health issues — depression, anxiety and stress from realizing the implications of their undocumented status and from trying to evade apprehension — that affect school performance and drop-out rates (Amuedo-Dorantes and Lopez 2015; Chaudry et al. 2010). Importantly, these issues may also affect children who themselves might be US citizens but who belong in mixed-legal status families. In these cases, scholarship has found that legal vulnerability of undocumented parents or guardians negatively affects the emotional well-being, academic performance, and overall family environment (including financial and non-financial aspects) of children who may nevertheless be citizens (Brabeck, Sibley, and Lykes 2016; Brabeck and Xu 2010; Connery 2018).

There is reason to believe, therefore, that the expansion of opportunity afforded by the IRCA will lead to large, positive effects on schooling investments of previously undocumented migrants and their families. I therefore expect student enrollment and high school completion rates to increase significantly post-legalization in public schools located in counties with higher shares of IRCA migrants. I also expect these increases to be most pronounced for Hispanic students and at the level of high schools when undocumented status becomes especially relevant for educational investment decisions.

3.2 The IRCA and Hispanic Participation in School Administration

I anticipate that the legalization of a large group of largely Hispanic undocumented migrants will differentially impact their participation in the administration of schooling. In particular, I examine Hispanic representation on school boards, arguing that the racial composition of such boards is important both for student achievement and targeted expenditure on education as demonstrated in Fischer (2023).

Although most of the migrants who received legal status under the IRCA could not vote

until the mid- to late-1990s, I posit that the IRCA nevertheless increases Hispanic political participation (and hence, representation) at the county level because the legalization of millions of Hispanic migrants lifted barriers of social exclusion not only for the migrants themselves but also for their families and social networks, members of which may already be US citizens.¹⁰ Lisa, a twenty-two year old US citizen part of a mixed-legal status family, explains the following:

Everybody is undocumented in my family, so that's all I really grew up knowing. Even though I am a U.S. citizen, I got used to those norms, so in a way it was like I was undocumented myself (Castañeda 2019).

The IRCA thus affected the social and political participation of not just the undocumented migrants but also of people like Lisa, of whom there were many. A survey of a sample of the migrants legalized by the IRCA, indicates that 42 percent of them had one or more family members, minors or adults, who were already citizens prior to the IRCA. Nearly 13 percent report having one or more relatives, aside from their spouse and children, who were citizens pre-IRCA.¹¹ Accordingly, I anticipate that counties with higher IRCA shares to experience differential changes in the racial composition of school boards because of the influence of legal status on the political participation of already legal citizens in communities and households of mixed legal status.

4. Data

I construct two main panel data sets for this study: (1) a panel of US counties from 1980 to 2000 and (2) a panel of US public schools from 1982 to 2000. In this section, I describe the main variables used in the analysis and their sources.

IRCA migrants: The primary regressor in this study is the total number of IRCA applicants per 1,000 county inhabitants in the United States measured in 1990. I obtain these data from the

10. Consistent with this view, McCann and Jones-Correa (2016) find, for example, that 80 percent of legal but non-citizen Hispanics participated in some form of local political initiatives, 20 percent had made contact with a government official about a particular concern and 10 percent indicated participation in formal political groups.

11. These figures are derived from data from the first wave of the Legalized Population Survey conducted in 1989 by the Immigration and Naturalization Service (INS).

Immigration and Naturalization Service (INS) Legalization Summary Public Use Tape data. As explained in Sabet and Yuchtman (2023), one limitation of these data is that there are no county identifiers for immigrants who applied from counties with populations less than 100,000 or with fewer than 25 applicants. I therefore impute the number of IRCA immigrants in counties with missing IRCA information in the same manner as Sabet and Yuchtman (2023) and I describe this imputation method in more detail in the Online Data Appendix.

I evaluate the quality of imputation in several ways. First, I conduct the main analyses on samples that exclude counties with missing IRCA information. Second, I use an alternative method of imputation where I predict the share of IRCA applicants in small counties whose IRCA share is not known using the estimated coefficients from a model that uses a rich set of county characteristics to predict the migrant shares for the large counties for which the IRCA share is known (additional details in the Online Data Appendix). Finally, I run additional specifications where I control for the time-varying effect of counties whose IRCA shares had to be imputed. Across these different checks, my results remain qualitatively and quantitatively similar, suggesting that my estimates based on imputed data are robust.

Public schools: I assemble a panel of some 208,000 public schools from 1982 to 2000 (except for 1985 for which data is not available) using information from the Public School Universe Data of the Common Core of Data (CCD) of the US Department of Education. The CCD, according to the Department of Education, is “a comprehensive, annual, national database of all public elementary and secondary schools and school districts.” This information includes the number of students enrolled and the number of full-time equivalent of classroom teachers (i.e., teaching staff) which I use to generate student-to-teacher ratios. As of 1987, two years prior to legalization, the CCD includes information on student enrollment by race which I leverage to test the effects of the IRCA on Hispanic students compared to students of other racial backgrounds. The Public School Universe Data also includes a unique school identifying code (NCES code) for each public school which enables me to exploit within-school variation in my analyses. The Public School Universe Data does not include the county FIPS code in which a school is located. Instead, it includes the unique school district code to which each school

belongs. For this reason, I link each public school from the Public School Universe Data to its school district from the CCD's Public Education Agency Universe data files. These latter data include both the unique school district code, which enables me to link public schools to school districts, and the unique county FIPS codes to which a school district belongs, which enables me to link county IRCA shares and other county covariates to these data.

Hispanic school board officials – the NALEO Roster: My measure of the number of Hispanics elected to school boards — and to public office more generally — comes from the historical archives of the National Association of Latino Elected and Appointed Officers (NALEO). NALEO is a non-profit, non-partisan organization which has gathered data on Hispanic persons elected to public office from the local to the federal level since 1984. Specifically, I digitize information collected by NALEO under the *National Roster of Hispanic Elected Officials*. My digitization work provides me with a data set of some 15,000 individual Hispanic officials between 1984 and 1994, five years before and after legalization. These data include the title of the role as well as the jurisdiction of the particular office (including the ZIP codes and county names) which I use in order to generate county-level aggregates of the total number of Hispanic public officials in a given county-year. I restrict my analysis to local level officials, which includes all Hispanic officials serving at the county level and lower.

Local government organization: I gather information on the structure and organization of local governments in the United States from the US Census Bureau's Historical Data Bases on Government Organization. These data include the number of county, municipal and township governments as well as the total number of school districts aggregated to the county level. I collect this information from 1977 to 1997 (available in 5 year intervals) to demonstrate that a county's IRCA share is unrelated to the size and structure of its local government. I then normalize the number of Hispanic officials at the county-year level by the number of 1987 local governments in order to generate a “per unit of government” measure of Hispanic elected officials at the county level.

County covariates: I assemble a range of county characteristics from 1980 to 2000 from the US Census Bureau. I collect these at the county-year level to test for differential changes in levels and trends of socioeconomic variables potentially associated with county IRCA shares before and after 1989 when applicants first started to receive legal status. I also control for the time-varying effect of these variables, measured in 1980, in my regression analyses.

County finances: I obtain measures of county expenditures (and revenues) from the US Census Bureau's internal data base on individual local government finances ("IndFin"). These data measure expenditure and revenues for local governments which includes counties, cities, municipalities and which are aggregated to the county level. The measures of county level expenditure on public education are disaggregated to the primary and secondary level on the one hand and higher education (post-secondary) on the other. This internal database is an archive of the data that were collected in the periodic (i.e., every 5 years) censuses of governments as well as annual surveys of government finances since 1970. I exploit the information that is available on an yearly basis as part of the annual survey of government finances.

Public/private enrollment and high school completion by race: I collect measures of public or private school enrollment at the individual level from the US Decennial Census. Although a repeated cross section, these data enable me to test whether the IRCA lead to differential sorting, by race, into public/private education. Additionally, I use this information to examine the extent to which a county's IRCA shares increased Hispanic high school completion rates.

American National Election Studies: I test whether the IRCA leads to increases in political participation at the individual level for people who live in high v. low IRCA counties. To conduct this exercise, I use restricted-use survey data from the American National Election Studies (ANES) with county identifiers which enable me to link IRCA information and other county covariates to this dataset.

5. Identification

I exploit a differences-in-differences regression framework in order to identify the effects of legal status under the IRCA on Hispanic schooling outcomes and on Hispanic representation on school boards. Given that undocumented migrants are not randomly distributed across counties of the United States, one might wonder to what extent counties with low IRCA shares are valid counterfactuals for those with higher IRCA shares. What makes identification credible is the fact that undocumented migrants are *already* included in census counts: the IRCA thus affected the legal composition of a county's *existing* population without triggering wider demographic change. In this section, I empirically validate this claim. To do so, I follow Sabet and Yuchtman (2023) by testing for differences in (a) trends and (b) levels in county covariates as a result of the IRCA.

5.1 IRCA and Differences in Trends

I test for differential trends in county characteristics associated with the IRCA by estimating the parameters of the following model:

$$y_{c,t} = \sum_{j=1980, j \neq 1989}^{2000} \beta_j (IRCA_{c,1990} \times D_j^t) + \delta_c + \zeta_{st} + \epsilon_{c,t} \quad (1)$$

Where y is a standardized measure of a socioeconomic covariate in county c in year t . These include county population, income, the size of the white and black population, population by age as well as measures of crime, financial institutions and housing values. A county's 1990 share of per 1,000 IRCA applicants (standardized) is denoted by $IRCA_{c,1990}$ and it is interacted with year dummies, D_j^t . The key parameter of interest is β_j which identifies the effect of a county's 1990 share of per 1,000 IRCA applicants in every time period between 1980 and 2000 compared to 1989, the first year IRCA migrants gained legal status. The model includes county fixed effects, δ_c , as well as state-by-year fixed effects, ζ_{st} , and standard errors are clustered at the county level, shown as $\epsilon_{c,t}$. I weight all regressions by the size of the 1980 county population.

I present the results in Figure 1. As shown, conditional on exploiting within county and state-year variation, the IRCA is not associated with differential changes across a wide

range of county covariates — including the size of the school age population — before and after legalization. This increases confidence that my empirical model is able to identify the effect of changes in legal status on schooling outcomes independently of changes to other socio-demographic characteristics in a county.

5.2 IRCA and Differences in Levels

I test for long-differences that the IRCA might have stimulated in the decade between 1980 and 1990 as well as in the two decades between 1980 and 2000. To this purpose, I estimate β and γ from the following two specifications:

$$\Delta y_{c,1990-1980} = \alpha_0 + \beta(IRCA_{c,1990} \times P_{89}) + \delta_c + \zeta_{st} + \epsilon_{c,t} \quad (2)$$

$$\Delta y_{c,2000-1980} = \alpha_0 + \gamma(IRCA_{c,1990} \times P_{89}) + \delta_c + \zeta_{st} + \epsilon_{c,t} \quad (3)$$

As in the previous specification, a county's per 1,000 IRCA share (standardized) is shown as $IRCA_{c,1990}$ and this measure is now interacted with an indicator variable, P_{89} , that is set to zero for 1980 and 1 for 1990 (specification 2) or 2000 (specification 3)¹² I cluster errors at the county level and present the results in panels (a) and (b) of Figure 2. As shown, there is little evidence that the IRCA prompted long term changes in county characteristics, measured either in the decade leading to 1990 or in the two decades leading to 2000.

This exercise is particularly important because three variables are unavailable on an annual basis prior to 1990. These include the size of the Hispanic population, the birth rate and the size of the population in poverty, leaving open the possibility that my results are driven by mechanical increases in either the Hispanic population or the birth rate or by reductions in poverty as a result of the IRCA. As shown in Figure 2, however, the IRCA does not lead to significant, long-run changes in these covariates, alleviating concerns that my results are confounded by them.

It is important to note what these result do and do not suggest. They do not suggest that IRCA migrants are randomly allocated across US counties; there are surely many county

12. The only exceptions, owing to data limitations, are for crime (for which post is zero for 1981) and population by age (for which post is set to 1 for the year 1999 in specification 3).

characteristics that determine the geographical selection of undocumented migrants — distance to the Mexican border and levels of pre-existing Hispanic immigration to name just two examples. Instead, the results suggest that, conditional on exploiting variation *within* counties and *within* a state-year, the share of IRCA migrants does not lead to differential changes in county characteristics in levels nor in trends.

6. The IRCA and Public Schooling: Enrollment, Teachers and Classroom Size

In this section, I examine the relationship between legalization under the IRCA and three schooling outcomes measured at the level of public schools: student enrollment, the number of teachers per school and student-to-teacher ratios. For each public school, ps , located in county c in year t I model these outcomes, y , as follows:

$$y_{ps,c,t} = \alpha_0 + \beta(IRCAC_{c,1990} \times P_{89}) + \delta_{ps} + \delta_c + \zeta_{st} + \theta(X_{c,1980} \times P_{89}) + \gamma(R_{c,1980} \times P_{89}) + \epsilon_{c,t} \quad (4)$$

As before, a county's per 1,000 IRCA share (standardized) is shown as $IRCAC_{c,1990}$ and it is interacted with an indicator variable, P_{89} , that is set to 1 for time periods on or after 1989, the first year as of which IRCA applicants acquired legal status, and zero otherwise. I exploit within-public school variation through the inclusion of public school fixed effects, denoted by δ_{ps} . This is especially important when one considers that the largest federal education program — Title I of the Elementary and Secondary Education Act — distributes aid to public schools and school districts by way of a formula that relies heavily on the local poverty rate among school-aged children (Gordon 2004). To the extent that IRCA migrants are located in schools eligible for such funding, one may be concerned that Title I funds may confound the results. The inclusion of public school fixed effects thus alleviates this concern. The model also includes county fixed effects, δ_c which capture fixed differences across county characteristics that may correlate both with legal status and school enrollment.¹³ I also include state-by-year

13. Public school and county fixed effects are not collinear in this model because public schools can, and do, change districts (and hence counties) from time to time owing to district consolidation or expansion. This is the case for 1,026 (or .5 percent) of the schools in the sample.

fixed effects, ζ_{st} , in the model to account for any state-specific, time-varying shocks, such as governor or party characteristics in a given state-year.

The model includes a range of socioeconomic characteristics, noted by $X_{c,1980}$, each measured in 1980 and interacted with the indicator P_{89} . These characteristics include: log of population, log of income, the size of the school age population (i.e., those between 5 and 19 years of age), and the size of the population below the poverty line. $R_{c,1980}$ is a vector of race-related controls. It includes the size of the population that is white, black and Hispanic. Because the IRCA was a landmark policy aimed at integrating undocumented migrants into the life of society, it is not unreasonable that it might have had an effect on communities of all racial backgrounds, regardless of the number of undocumented migrants in those communities. The inclusion of 1980 measures of different racial groups thus enables me to identify the effect of legalization under the IRCA on schooling outcomes separately from the differential, time-varying effect of a county's pre-existing racial composition. Standard errors are expressed as $\epsilon_{c,t}$ and are clustered at the county level. As before, all regressions are weighted by the size of the 1980 county population.

I present my results in Table 2. In Panel A, the outcome is the number of students enrolled at the public school level. In column 1, I present the results from my baseline estimating equation while in column 2, I present a parsimonious version of the model with no covariates included. In column 3, I include, in addition to the the baseline covariates, 1980 measures of the county birth rate, crime rate (1981 values), population under the age of 5, number of banking institutions as well as housing values, each interacted with P_{89} . In each of these three models, a one standard deviation increase in the share of IRCA applicants in a county increases enrollment by around four to five students, a one percent increase relative to the pre-legalization sample mean. In column 4, I control for the time-varying effect of whether a county is covered by Section 5 of the VRA and obtain results nearly identical to the baseline. In column 5, I omit counties with populations in the top 25 percentile to rule out the possibility that the effect is driven by very large, ethnically diverse counties that may provide a safe haven toward undocumented migrants (i.e., sanctuary cities). As shown, the effect persists even in a sample of small counties. In column 6, I omit counties that lay in Hispanic or Black majority districts prior

to the 1990 redistricting cycle so as to minimize the potentially confounding effects of Section 2 of the VRA. The magnitude of the coefficient decreases by some 40 percent but remains precise at conventional levels, suggesting that legal status drives student enrollment regardless of redistricting concerns. In column 7, I omit counties whose IRCA share had to be imputed owing to missing data in the INS Legalization Tapes. Although this considerably shrinks the size of my sample, the magnitude of the coefficient increases and remains quantitatively precise. In columns 8 and 9, I undertake two additional robustness exercises regarding the imputed nature of the IRCA data. In column 8, I control for the time-varying effect of counties whose IRCA shares were imputed and column 9, I employ an alternative imputation method. Across all these specifications and sample restrictions, the coefficient of interest remains stable and precisely estimated.

In Panels B and C, I run the same nine empirical specifications and sample restrictions described above using the number of full-time equivalent of classroom teachers (Panel B) and the student-to-teacher ratio (Panel C) as the outcomes. As shown, schools located in counties with larger shares of IRCA applicants experience significantly larger classroom sizes. Because the IRCA leads to both increases in student enrollment (Panel A) and teaching staff (Panel B), the larger classroom sizes appear to be the result of student enrollment outstripping teaching staff increases.

6.1 Pre-trends

To what extent are these results reflective of pre-trends? I address this question in Figure 3 where I plot regression coefficients when student enrollment and the number of teachers are regressed on the county's IRCA share interacted with year dummies. As shown, pre-legalization, the difference in both outcomes between public schools located in high- and low-IRCA counties is virtually zero (the six pre-treatment coefficients are both individually and jointly insignificant) and the trend in that difference is flat. Immediately following legalization, however, public schools located in counties with a high-IRCA share experience differential increases in both the number of students and teachers at the public school level, though the effects for student enrollment are slightly larger (both outcomes have now been standardized to ease comparison)

and are estimated with greater precision.

In Online Appendix B, I assess pre-trends more formally, following approaches introduced by Rambachan and Roth (2023) and Freyaldenhoven et al. (2021). Specifically, I estimate the line of best fit through the six pre-period coefficients shown in Figure 3 and linearly extrapolate this trend into the post-period. For student enrollment, the slope of the pre-period trend is virtually zero ($-.0000681$) with a p -value of 0.828. For the size of the teaching staff, the slope of the pre-period line of best fit is also extremely small ($-.0005071$) with a p -value of 0.22. For both outcomes, extrapolating the pre-period line of best fit into the post-period confirms the view that the positive, post-1989 effect of the IRCA on student enrollment or the size of the teaching staff is not merely an extension of pre-existing, linear trends in schools located in high-IRCA counties.

6.2 High School Heterogeneity

The realization of legal status, or the lack of it, becomes more relevant as undocumented migrants advance from primary to high school. Not only does the return to education decrease, academic sorting mechanisms, such as college or scholarship applications and access to school resources, become relevant. I therefore expect the IRCA to have a stronger effect on student enrollment in public high schools than in public primary schools. To this end, I estimate the parameters of a model that allows the effect of a county's IRCA share to vary pre/post legalization and between public high schools and public primary schools. I thus augment the baseline specification by adding to it a triple interaction, $IRCA_{c,1990} \times P_{89} \times \text{High School}_{ps}$ (lower order terms included), as follows:

$$y_{ps,c,t} = \alpha_0 + \beta(IRCA_{c,1990} \times P_{89}) + \gamma(IRCA_{c,1990} \times P_{89} \times \text{High School}_{ps}) + \delta_{ps} + \delta_c + \zeta_{st} + \theta(X_{c,1980} \times P_{89}) + \gamma(R_{c,1980} \times P_{89}) + \epsilon_{c,t} \quad (5)$$

The variable High School_{ps} is one if public school ps is a high school and zero if not. The parameter β in this model thus captures the effect of the IRCA on the three schooling outcomes for primary schools while the parameter γ captures the additional effect of the IRCA

on those same outcomes for high schools. I estimate the parameters of this model across the same nine empirical models as in my baseline analysis and plot the coefficients β and γ in Figure 4. As shown, the effects are driven entirely by greater student enrollment (and increases in teaching staff) in high schools; there are no effects in primary schools. The coefficient on student enrollment now suggests that a one standard deviation increase in a county's IRCA share is associated with a 15 student increase in enrollment in high schools, a magnitude which, in absolute terms, is thrice as large as the baseline estimate. However, relative to the pre-legalization size of the high school student body, 15 additional students represents a 2 percent increase in the student population, an effect twice as large (in relative terms) as the baseline. At the level of high schools, the increases in teaching staff in response to the IRCA are also larger and for this reason, the magnitude of the increase in classroom sizes in high schools is similar to that in the baseline model, though they are estimated with slightly less precision (most are significant at the 90 percent level).

6.3 *Hispanic Student Enrollment*

The Common Core of Data includes student enrollment by race as of 1987. This enables me to examine the extent to which student enrollment increases observed under the IRCA are driven by Hispanic youth. I begin by flexibly estimating the parameters of the model expressed in equation 1 using student enrollment by race as the outcome. I present the results in Figure 5a and the patterns are clear: the overall increases in student enrollment are driven entirely by Hispanics enrolling in public schools. By contrast, there are no effects for black or white students.

As shown, the pattern for Hispanic public school enrollment does display some pre-trends. While each of the pre-period coefficients is individually indistinguishable from zero, they are jointly significant with a p -value of 0.01. This is likely reflective of the fact that, upon acceptance of their application, IRCA migrants were first granted temporary legal status under the title of Temporary Resident Aliens before receiving permanent legal status in 1989. It is therefore surely the case that Hispanic student enrollment as a result of the IRCA began shortly after 1986 and not after 1989. Nonetheless, in Online Appendix B, I estimate a line of

best fit through the two pre-period coefficients and extrapolate the trend into the post-period. While the pre-treatment slope is indeed positive, it intersects the confidence intervals of just four of the eleven post-period coefficients, making it unlikely that the entire effect observed in Figure 5a is explained by linear pre-trends. Additionally, I conduct sensitivity analysis using relative magnitude restrictions introduced by Rambachan and Roth (2023). I present the results in Figure B.3 of the Online Appendix and find that the “breakdown” parameter lies at $\bar{M} = 0.5$, suggesting the casual effect of the IRCA on Hispanic student enrollment is valid so long as post-treatment violations of parallel trends are no larger than half the magnitude of the maximum pre-treatment violation.

In Figure 5b, I flexibly estimate the model presented in equation 5 which introduced the triple interaction $IRCA_{c,1990} \times P_{89} \times \text{High School}_{ps}$ into the analysis in order to allow a county’s IRCA share to have differential effects for high schools and primary schools (I now swap the indicator P_{89} for year dummies D_j^t). I now plot both β (effect for primary school enrollment, shown with blue squares) and γ (effect for high school enrollment, shown with green triangles) from that model for students of different racial backgrounds. Examining Hispanic enrollment, the patterns suggest that the IRCA does, in fact, increase Hispanic student enrollment in primary schools. This might reflect the reality that, for some families, the risk of even primary school enrollment pre-legalization might have been too high. As expected, however, the coefficient for Hispanic high school enrollment is significantly larger (around 50 to 60 percent larger) than the coefficient on primary enrollment. This is consistent with the idea that legal status is more relevant to human capital investment in the later stages of public education.

Figure 5b reveals further interesting patterns. While there are no differential effects of the IRCA for black enrollment in either primary or high schools, the IRCA leads to significantly less enrollment in high school for white students. One potential explanation might be that white students sort differentially into private schooling. This might be in response to either the larger class room sizes or to the changes in the racial composition of the student body, or both. Although I cannot identify the underlying reason for why whites differentially sort out of public education, I investigate whether racial sorting into public/private schooling is a plausible explanation for the negative patterns in white student enrollment in public high schools in the

following subsection.

6.4 *Private/Public Enrollment*

I test whether the IRCA leads whites and Hispanics to sort differentially into private and public schooling by collecting data from the 1980, 1990 and 2000 Decennial Census.¹⁴ These data include an indicator for whether a person is enrolled in private or public schooling. I run the same empirical specification as in my baseline analysis but now include several individual level controls (including age, biological sex, whether a person is foreign-born and whether the household is in poverty). I also include an interaction of the IRCA with a persons racial background (white or black), $IRCA_{c,1990} \times P_{89} \times Race$ (all lower order terms included), where Hispanics serve as the omitted category.

The results are presented in Table 3. In columns 1 and 2, the outcome is one if an individual is enrolled in public school and zero if they are enrolled in private school and I restrict the analysis to 13 to 19 year olds enrolled in schooling. In column 1, I find that Hispanic individuals residing in high IRCA counties are significantly more likely to enroll in public school. By contrast, the IRCA makes it increasingly likely that white students enroll in private schooling; the magnitude of the coefficient is about a third larger for whites than it is for Hispanics, suggesting that the IRCA leads to differential sorting into private schooling for whites compared to Hispanics and helping explain aggregate patterns in public school enrollment by race presented in Figure 5. In column 2, I repeat the analysis, this time comparing Hispanics and blacks and, as shown, the IRCA does not lead to private/public school sorting.

In columns 3 and 4, I examine high school completion. The outcome variable is now an indicator that is one if an individual has a high school degree and zero if not and I now restrict the sample to 18 and 19 year olds in a given county. In both columns, Hispanic youth residing in high IRCA counties are significantly more likely to complete high school post-legalization. There are no such patterns for whites or black youth; while their respective coefficients are precisely estimated, they are both smaller in terms of magnitude than the coefficient on Hispanic youth

14. The Common Core of Data also has a Private Schools Universe data set with enrollment information. However, I am unable to examine pre/post legalization differences using these data because (a) they begin only in 1989 and (b) do not include enrollment by race until 1993.

and they are of opposite sign.

Finally, in columns 5 and 6, I examine the possibility of geographical sorting among youth of high school age (i.e., 13 to 19). Although a county's white and black population displayed no differential trends in response to the IRCA (Figure 1), I nevertheless examine whether, in a repeated cross section, individuals of either race report moving counties in response to the IRCA. The outcome is 1 if an individual reported moving counties in the previous 5 years and zero if they remained in the same county. As shown, the coefficient for Hispanic and black youth are indistinguishable from zero while the coefficient for whites is negative, suggesting that the IRCA increases the likelihood that whites remain in the same county over time.

7. The IRCA and School Administration: Hispanic Representation on School Boards

In addition to influencing schooling outcomes, I argue that the legalization of some 3 million mostly Hispanic migrants has significant consequences for the administration of public schooling. I test this claim by studying the impact of the IRCA on two outcomes of interest: the number of Hispanics represented on school boards and the amount of public spending on education. In this section, I focus my analysis on the question of representation. To carry out this analysis, I digitize a novel source of data that includes information on some 15,000 unique Hispanics elected to public office from 1984 to 1994.

7.1 *Hispanic School Board Officials*

I begin by flexibly estimating the parameters of the following county-level econometric specification:

$$y_{c,t} = \sum_{j=1984, j \neq 1989}^{1994} \beta_j (IRCA_{c,1990} \times D_j^t) + \delta_c + \zeta_{st} + \epsilon_{c,t} \quad (6)$$

The outcome, y , in county c in year t is the number of Hispanics elected to school boards normalized by the 1987 number of school board districts in a given county. I plot the various β_j coefficients in Figure 6 and find that the IRCA has a strong, differential effect on the number

of Hispanics serving on school boards.¹⁵ What is more, the positive, post-legalization effect is not indicative of pre-trends: The five pre-period coefficients are individually and jointly insignificant and in Online Appendix B, I present results from more formal tests which suggest that the results are not reflective of potential violations of parallel trends.

I next test the strength of the relationship between the IRCA and Hispanic school board officials by estimating the parameters of my full estimation equation, reproduced in equation 7 but at the county-year level, again using the number of Hispanics elected to school boards normalized by the 1987 number of school board districts as the outcome. All terms are as previously defined and I weight the regressions by the 1980 size of the county population.

$$y_{c,t} = \alpha_0 + \beta(IRCA_{c,1990} \times P_{89}) + \delta_c + \zeta_{st} + \theta(X_{c,1980} \times P_{90}) + \gamma(R_{c,1980} \times P_{90}) + \epsilon_{c,t} \quad (7)$$

I estimate equation 7 for the same nine empirical specifications and sample restrictions as in my baseline analysis and report the results in Table 4. As shown, the IRCA leads to significant increases in the number of Hispanics serving on school boards. The magnitude of the coefficient is large: it suggests that a one standard deviation increase in the share of IRCA applicants leads to a 0.04 increase in the number of Hispanics per school board, a 55 percent increase relative to the pre-legalization sample mean of Hispanics on school boards. The magnitude of the coefficient is also stable: across all nine models, the coefficient is of similar size and distinguishable from zero at conventional levels (except for column 7 where the coefficient is qualitatively similar but quantitatively insignificant with a p -value of 0.28). Importantly, the coefficient remains almost unchanged, relative to the baseline, when controlling for the time-varying effect of Section 5 of the VRA or when excluding counties located in minority-majority districts pre-1990 redistricting. These specifications suggest that that the IRCA increased Hispanic representation on school boards independently of VRA provisions.

15. I present results for different categories of local offices, including mayors, county officials, city councilors, and local judicial officers in Online Appendix C. However, there are no positive effects for any other type of Hispanic elected officials.

7.2 *Size and Number of Local Government Units*

I next examine the relationship between the IRCA and the number and size of local government units (counties, municipalities, townships or school districts) in a county. I run the empirical model specified in equation 7 but use the total number of local government units in a county or the total number of publicly elected officials per government in a county-year as the outcome.¹⁶

I present the results in Table 5. In columns 1 and 2, the outcome is the total number of local government units (column 1) or school board districts (column 2). The results suggest that the IRCA has no impact on the number of local government units in a county. In columns 2 and 3, I examine the number of elected officials per government unit. In column 2, the outcome is the total number of locally elected officials normalized by the number of 1987 total local governments; in column 3, the outcome is the total number of school board members normalized by the 1987 number of school board districts in the county. Again, I find that the IRCA does not lead to an increase in the size of local government units. These results suggest that the post-IRCA increase in Hispanic representation on school boards is not explained simply by increases in the number of school boards or school board positions. In columns 5 and 6, I examine the effect of the IRCA on white and black elected officials. The outcome is the total number of white (column 5) or black (column 6) elected officials per 1987 number of governments in a county. As shown, the IRCA leads to significant decreases in the number of whites and blacks elected to public office. Together, these results suggest that legalization under the IRCA increases competitiveness of Hispanic candidates. This might be because fewer whites and blacks decide to run for office, leading to increases in the number of *available* school board positions, or because the IRCA increases political mobilization at the local level, enabling Hispanics to win a larger number of school board elections, or both. While I am unable to test whether the IRCA reduces the number of candidates (of any race) running for school board membership, I present some suggestive evidence, explained in the subsequent subsection, that the IRCA increased

16. These data are available every 5 years. Information on the number of government units is available from 1977 to 1997 in 5 year intervals; however the number of popularly elected officials is available only until 1992 and is missing data for 1982. For this reason, I restrict this analysis to only two time periods: 1987 and 1992, immediately before and after legalization. In Online Appendix D, I present event study estimates using the full span of data for the number of local government units and the patterns are very similar when using more time periods: the IRCA does not lead to any differential changes in the number of government units at the county level.

political mobilization at the individual level among Hispanics and non-Hispanics.

7.3 Political Participation

I present suggestive evidence in Online Appendix E that the IRCA led to increases in political mobilization at the local level. What motivates this analysis is the fact that the legalization of millions of Hispanic migrants lifts barriers of social exclusion not only for the migrants themselves but also for their families and social networks, members of which may already be US citizens. To this end, I utilize individual level survey data from the American National Election Studies (ANES) and find that individuals residing in high-IRCA counties participate significantly more in political campaign activities, engage more frequently in political discussions with friends and family and are more likely to donate to political campaigns. I also find that non-Hispanic people residing in IRCA counties report significantly higher levels of effort trying to *mobilize others* to vote in elections, in line with the view that the legalization of millions of Hispanic migrants politically mobilized communities and households of mixed legal status. I find no evidence that the IRCA increased anti-Hispanic sentiment, suggesting that the IRCA stimulated greater political participation without triggering nativist backlash.¹⁷

8. County Finances: Spending and Revenues

Finally, I study whether differential changes to the racial composition of school boards affects public spending on education. To this purpose, I collect data on county level education expenditures (and revenues) in order to estimate the following empirical model:

$$y_{c,t} = \alpha_0 + \beta(IRCA_{c,1990} \times P_{89}) + \delta_c + \zeta_{st} + \pi(\text{Title } I_{c,99} \times P_{89}) + \theta(X_{c,1980} \times P_{90}) + \gamma(R_{c,1980} \times P_{90}) + \epsilon_{c,t} \quad (8)$$

Where all terms are defined as before. Because I study public finances, I include in this model, denoted by $\text{Title } I_{c,99} \times P_{89}$, the time-varying effect of the 1999 share of public schools

17. The ANES explains that its data are nationally representative but not at smaller-geographical levels. The analyses using these data also include just 250 counties, a fraction of the total number in the country. For these reasons I take these results as informative but suggestive and present them in the Online Appendix.

in a given county that were recipients of Title I education funding. As of 1999, the Common Core of Data indicate whether a public school received Title I funding. I am therefore able to calculate the share of public schools in a given county eligible for such funding, thus controlling for the potentially confounding effect of federal education aid.¹⁸

Table 6 reports the results. In column 1, I find that a county's IRCA share positively and significantly predicts total per capita expenditure on education at the county level. Decomposing education expenditures, I see, in column 2, that almost the entire education effect is explained by greater spending on primary and secondary schools (the data group primary and secondary spending together so I cannot distinguish whether spending on one is greater than the other). The primary and secondary spending includes both direct spending (shown in column 3 and which includes expenditure on, among other items, teachers, principals and librarians) as well as capital outlays expenditure (shown in column 4 and which includes spending for construction of buildings, grounds, and other improvements, and purchase of equipment, land, and existing structures).¹⁹ By contrast, the IRCA is not predictive of expenditure on higher education (i.e., post-secondary) at the county level (column 5).

In columns 6 to 10, I examine per capita revenue generation at the county level. The IRCA is associated with large, significant increases in total revenues which are explained, partly, by increases in individual income tax (column 8) rather than total increases in tax revenue (column 7). Importantly, in column 9, I do not find increases in Federal intergovernmental revenue for education, suggesting that the increased expenditure on education observed in columns 1 to 4 are not reflective of greater financial support received by states from the federal government to respond to the increased demands of the IRCA. However, I do find increases in intergovernmental revenue from the state government to high IRCA counties (column 10), suggesting that discretionary transfers from the state government play a role in explaining IRCA related education expenditure.

18. Although 1999 is a decade post legalization, I assume a school's Title I eligibility to be correlated over time. The results are unchanged if I omit this control.

19. See this website for further details on the definition of what is included in each category of spending and revenue: <https://www.census.gov/glossary/?term=Capital+outlay>. Accessed 29 May 2023.

9. Conclusion

Economists have long argued that human capital is among the most important forms of capital that determine the success of both individuals and entire economies (Becker 1962, 2002; Barro 2001). In this paper, I examine the human capital response of millions of Hispanic migrants when one of the entrenched barriers to investments in public education — lack of legal status — is lifted in the context of the United States. Overall, I find that legalization significantly increases Hispanic investment in education: both Hispanic enrollment in public education and Hispanic high school completion increase in counties most affected by the IRCA. These results are accompanied by increases in Hispanics serving on school boards. My findings thus suggest that legalization has far-reaching consequences: it not only encourages greater Hispanic investments in schooling but strengthens Hispanic representation at the level of school administration.

Another noteworthy aspect of my results is the impact of legalization on the enrollment decisions of white youth. While the IRCA led to large, significant increases in public school enrollment among Hispanic youth, it led whites to sort differentially out of public education and into private schooling. Given that the IRCA did not trigger anti-Hispanic sentiment, the sorting of whites into private education as a result of the IRCA appears to be more because of the crowded classrooms the IRCA generated and not because of the changes in the racial composition of the student body.

Given that undocumented migration is a prominent issue in many counties today — including the US whose undocumented population has almost quadrupled since the 1980s — it is important to consider the generalizability of these results. On the one hand, given the strong labor market incentives that legal status provides, it is not unreasonable to think that immigrant legalization would encourage greater investment in public education in other settings. On the other, if measures are not taken to increase teaching staff to keep up with enrollment increases, the net result could be more crowded classrooms which might lead to sorting into private/public schooling among groups defined by such factors as race or socio-economic class.

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10. Figures

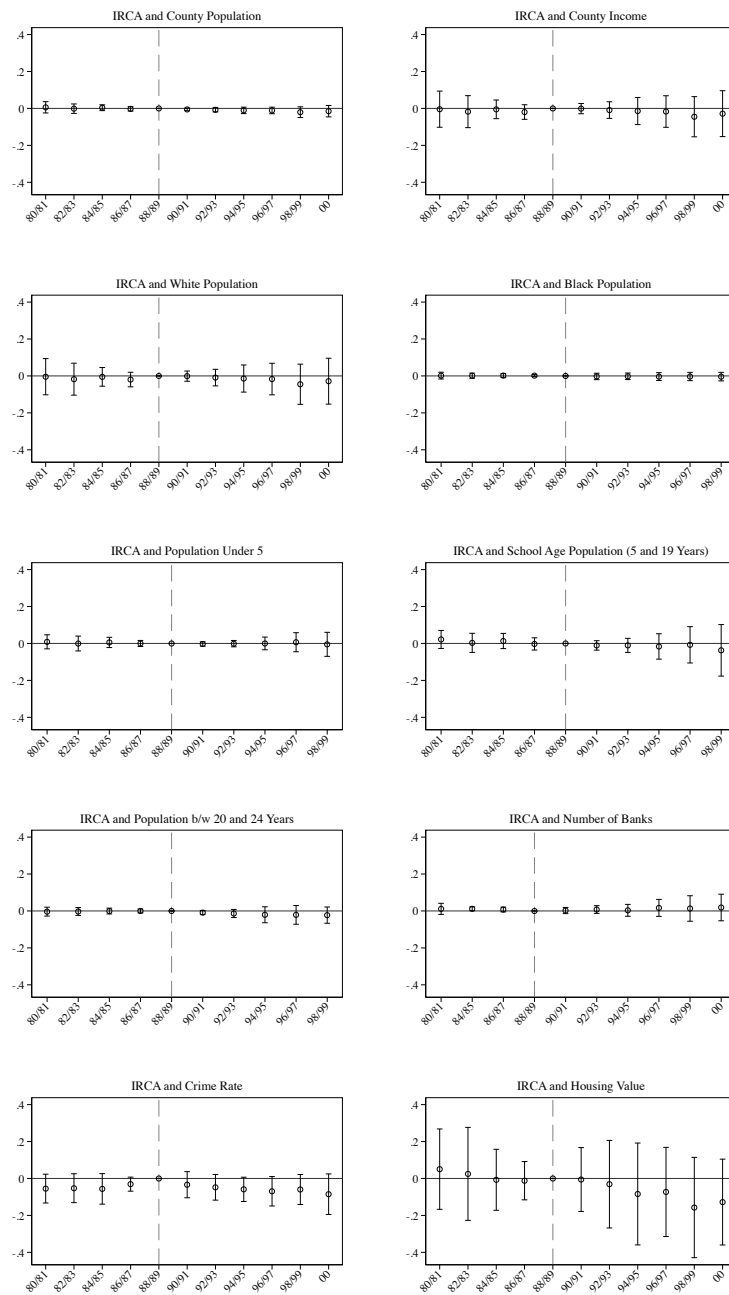


Figure 1
IRCA and Trends in County Characteristics

Notes: This figure plots the coefficient on IRCA₉₀, the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990, interacted with time dummies. The value of each outcome is averaged into two year bins and standardized. The regressions include county fixed effects and state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. All regressions are weighted by the size of the 1980 county population.

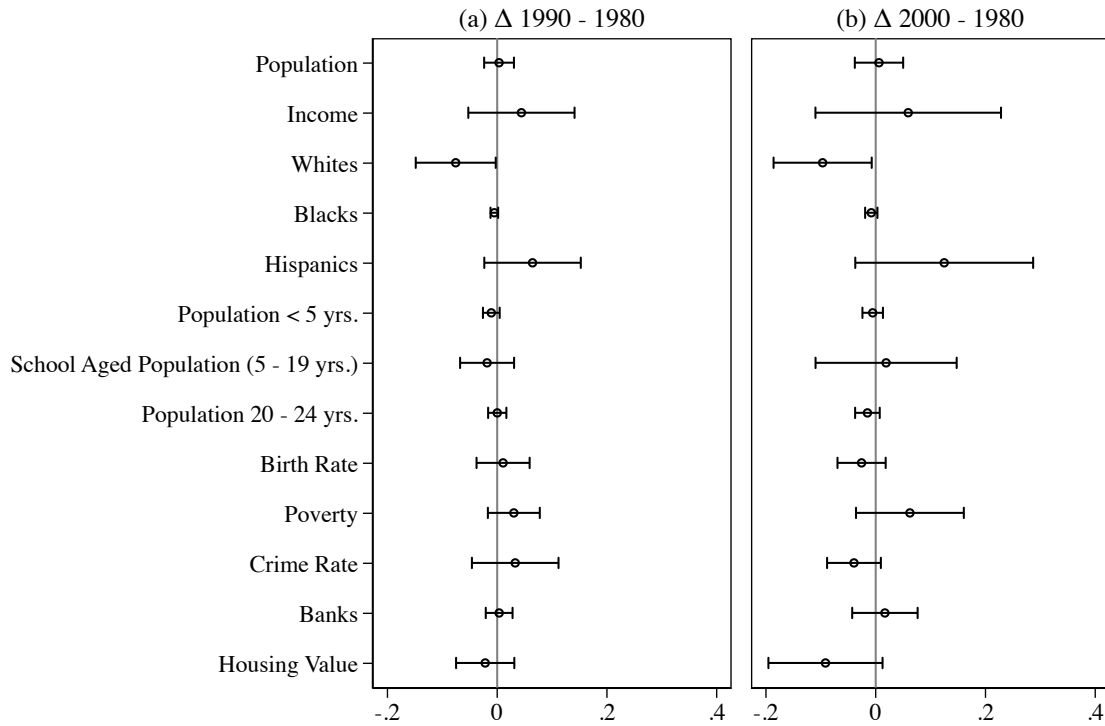


Figure 2
IRCA and Long Differences in County Characteristics

Note: This figure shows the coefficient β (panel (a)) from specification 2 and γ (panel (b)) from specification 3. Depending on the covariate, the regressions include either 3,102 clusters and 6,204 observations or 3,098 clusters with 6,196 observations. The regressions include county fixed effects and state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. Regressions are all weighted by the 1980 county population.

Standardized Values of Teachers and Students

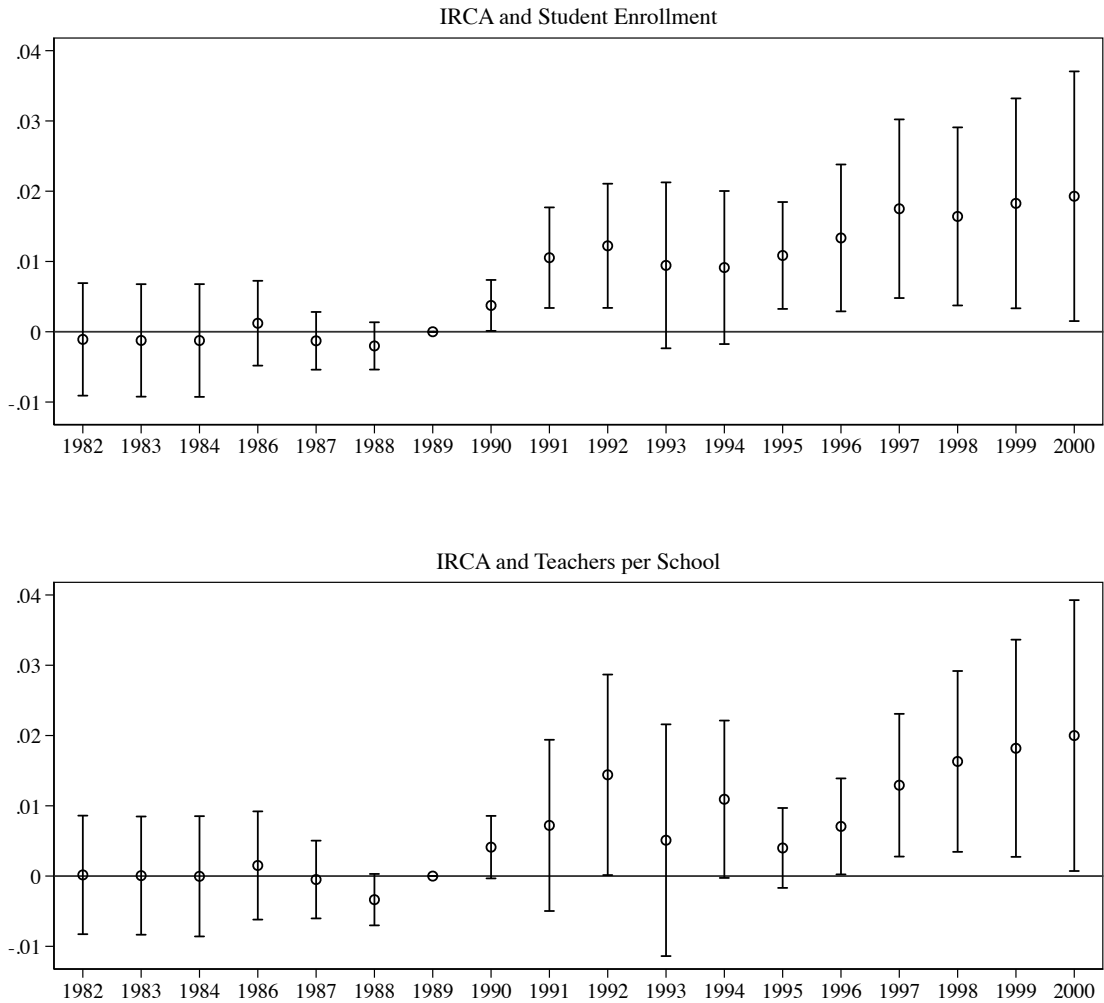


Figure 3
IRCA and Trends in Student Enrollment and Teaching Staff

Notes: This figure plots the coefficient on $IRCA_{90}$, the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990, interacted with year dummies. The outcome in the top panel is student enrollment at the public school level while the outcome in the bottom panel is the number of full-time equivalent of classroom teachers (i.e., teachers) also at the public school level. Both outcome variables have been standardized to facilitate interpretation. The regressions include county fixed effects and state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. Both regressions are weighted by the size of the 1980 county population. The p -values for a test of joint significance for the six pre-period coefficients are: 0.1787 for student enrollment and 0.1558 for teaching staff.

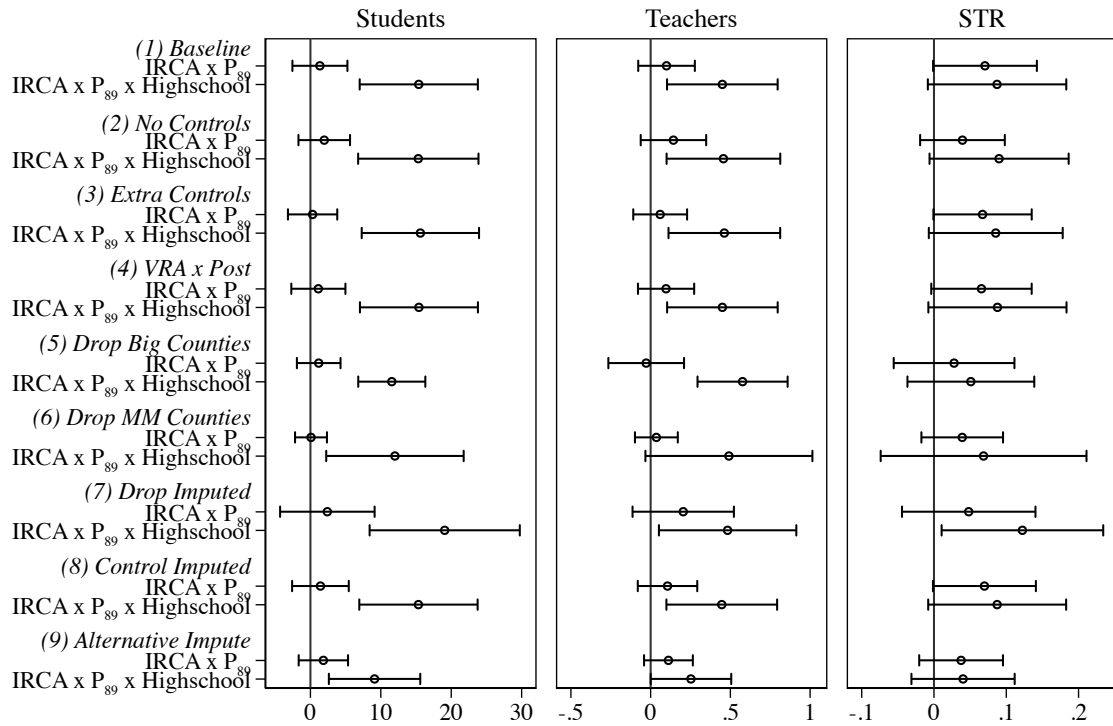
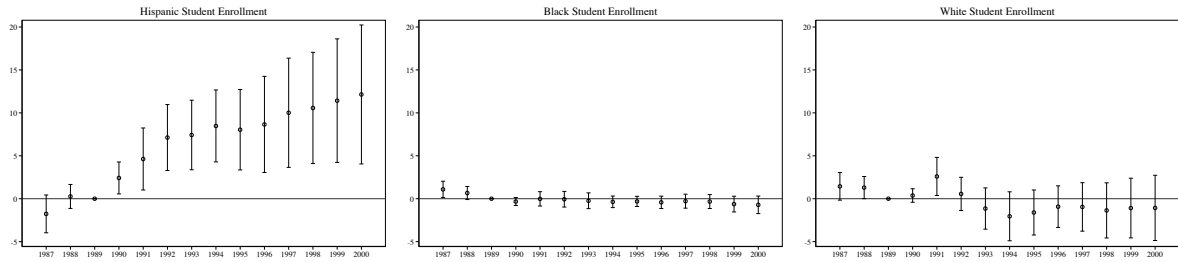
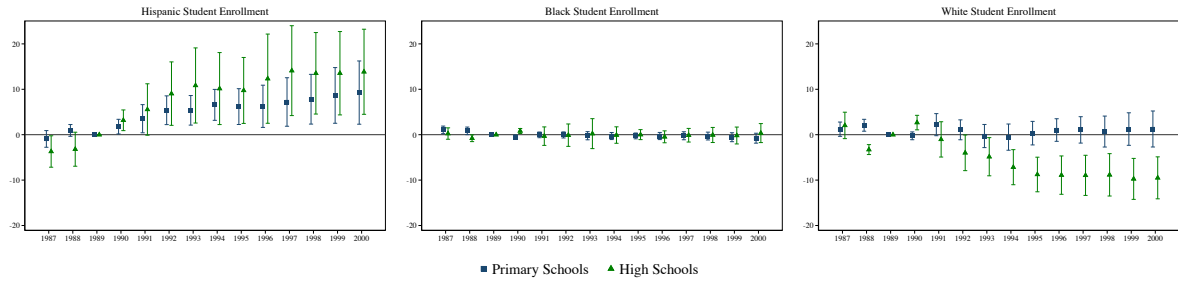


Figure 4
Heterogeneous Effects of the IRCA on Schooling Outcomes by Schooling Level

Notes: This figure plots the coefficient on $IRCA_{90} \times P_{89}$ as well as $IRCA_{90} \times P_{89} \times \text{High School}$ (all lower order terms included) for each of the nine specifications and sample restrictions shown in Table 2. Refer to the table notes of Table 2 for details concerning each specification. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. All regressions are weighted by the size of the 1980 county population. The mean [S.D] for each outcome for the baseline model for primary schools is as follows: Students: 425 [279]; Teachers: 22 [14]; STR: 18 [7.8]. For high schools: Students: 720 [656]; Teachers: 40 [32]; STR: 16 [7.6].



(a) The IRCA and Student Enrollment by Race



(b) The IRCA and Student Enrollment by Race and High school and Elementary School

Figure 5
The IRCA and Student Enrollment by Race and Level of Schooling

Notes: Panel (a) plots the coefficient on $IRCA_{90} \times D_j^t$ (where D_j^t denote year dummies) when student enrollment by race at the public school level is used as the outcome. Panel (b) plots the coefficient on $IRCA_{90} \times D_j^t$ (blue crosses) as well as $IRCA_{90} \times D_j^t \times \text{High School}$ (green circles) when they are both included in the same model (all lower order terms included), again using student enrollment by race as the outcome. The regressions all include county fixed effects as well as state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. All regressions are weighted by the size of the 1980 county population. The p -values for a test of joint significance for the two pre-period coefficients in panel (a) are: 0.01 for Hispanics; 0.025 for blacks; and 0.136 for whites.

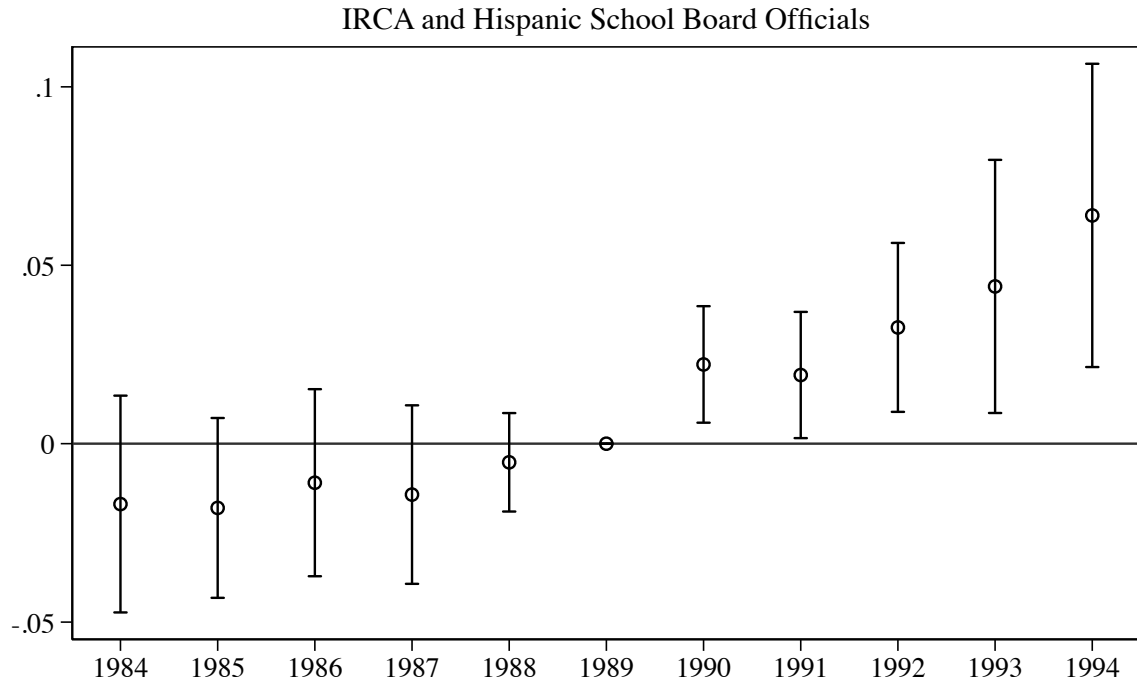


Figure 6
IRCA and Hispanic School Board Officials

Note: This figure plots the coefficient on $IRCA_{90}$, the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990, when it is interacted with year dummies. The outcome variable is the total number of Hispanic elected school board members divided by the 1987 number of school board districts, both aggregated to the county level. The regressions include county fixed effects and state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. All regressions are weighted by the size of the 1980 county population. The p -value for a test of joint significance for the five pre-period coefficients is 0.5638.

11. Tables

Table 1
Legalizations and Naturalizations in the United States, Total and under the IRCA

Year	Total Legal Residents		IRCA Legal Residents		Total Naturalizations		IRCA Naturalizations	
	(Number)	(Number)	(% of Total)	(Number)	(Number)	(% of Total)	(Number)	(% of Total)
1989	1,090,956	478,883	44	–	–	–	–	–
1990	1,543,812	880,940	57	270,101	133	0		
1991	1,837,207	1,134,509	62	308,058	115	0		
1992	972,557	165,089	17	240,252	218	0		
1993	905,111	16,702	2	314,681	881	0		
1994	791,218	4,083	1	434,107	11,048	3		
1995	723,641	2,898	0	488,088	65,490	13		
1996	916,521	3,037	0	1,044,689	227,905	22		
1997	781,892	1,300	0	598,225	136,084	23		
1998	653,207	820	0	463,060	85,647	18		
1999	644,788	6	0	839,944	151,829	18		
2000	841,001	271	0	888,788	135,385	15		
Total	11,701,911	2,688,538	23	5,889,990	814,735	14		

Notes: This table presents statistics on the number of legalized migrants in the entire US as well as those legalized under the IRCA. It also shows total naturalizations in the US and those as a result of the IRCA. The statistics are taken from Tables 2 and 3 from Rytina (2002).

Table 2
IRCA, Student Enrollment and Classroom Size

	Covariate Stability				Sample Stability			Imputation Stability	
	(1) Baseline Covariates	(2) No Covariates	(3) Additional Covariates	(4) VRA × Post	(5) Population < 75 th Pctl.	(6) Drop MM Counties	(7) Drop Imputed	(8) Control Imputed Counties	(9) Alternative Imputation
<i>Panel A. Students Enrolled</i>									
IRCA ₉₀ × P ₈₉	4.985** (2.049)	5.193** (2.037)	3.958** (1.815)	4.792** (2.013)	3.922*** (1.305)	2.990* (1.562)	6.759* (3.659)	5.055** (2.131)	3.913** (1.817)
\bar{Y}_{Pre} [S.D]	486 [407]	486 [407]	486 [407]	486 [407]	320 [258]	435 [348]	588 [454]	486 [407]	486 [407]
<i>Panel B. Teachers</i>									
IRCA ₉₀ × P ₈₉	0.202** (0.088)	0.235** (0.100)	0.164** (0.082)	0.200** (0.088)	0.108 (0.096)	0.154** (0.073)	0.311* (0.169)	0.207** (0.095)	0.166** (0.077)
\bar{Y}_{Pre} [S.D]	26 [21]	26 [21]	26 [21]	26 [21]	18 [14]	24 [19]	31 [24]	26 [21]	26 [21]
<i>Panel C. Student Teacher Ratio</i>									
IRCA ₉₀ × P ₈₉	0.091*** (0.033)	0.055** (0.027)	0.090*** (0.033)	0.086*** (0.033)	0.037 (0.043)	0.057** (0.027)	0.079* (0.044)	0.090*** (0.033)	0.047* (0.027)
\bar{Y}_{Pre} [S.D]	18 [7.8]	18 [7.8]	18 [7.8]	18 [7.8]	17 [7.6]	18 [7.5]	19 [8]	18 [7.8]	18 [7.8]
N	1,241,460	1,241,460	1,241,460	1,241,460	392,525	900,365	733,816	1,241,460	1,241,460
Clusters	3,088	3,088	3,088	3,088	2,308	2,847	521	3,088	3,088

Notes: The outcome variables in all three panels are measured at the public school level and are taken from the CCD Public Schools Universe. In Panel A the outcome is the number of students enrolled, in Panel B it is the number of teachers and in Panel C it is the student-to-teacher ratio. IRCA₉₀ is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990. P₈₉ is an indicator that is zero for time periods prior to 1989 and 1 for periods on or after 1989. All regressions include county fixed effects, public school fixed effects and state-by-year fixed effects. They also include, except for column 2, 1980 measures of county population, income, school-aged population (i.e., between 5 and 19), population that is poor as well as the white, black and Hispanic population size, each interacted with P₈₉. Additional covariates in column 3 include 1980 measures of county birth rate, housing values, number of banking institutions, population under the age of 5 and the crime rate (1981 values), each interacted with P₈₉. The specification in column 4 includes an interaction between P₈₉ and an indicator for whether a county is covered under the pre-clearance requirement of Section 5 of the VRA. In column 5, counties with populations in the top 25 percentile are dropped. The model in column 6 excludes counties located in Hispanic or Black majority districts pre-1990 redistricting. In column 7, counties whose IRCA shares had to be imputed are all dropped while the model in column 8 includes the time-varying effect of counties whose IRCA shares had to be imputed. The model in column 9 uses an alternative method to impute IRCA shares as explained in the text. Standard errors (in parentheses) are clustered at the county. All regressions are weighted by the size of the 1980 county population. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3
IRCA, Public and Private Enrollment

	Public or Private School		Complete High School		Moved Counties	
	(1) Hispanic v. White	(2) Hispanic v. Black	(3) Hispanic v. White	(4) Hispanic v. Black	(5) Hispanic v. White	(6) Hispanic v. Black
IRCA ₉₀ × P ₈₉	0.007* (0.004)	-0.000 (0.003)	0.016** (0.006)	0.037*** (0.010)	0.007 (0.005)	0.002 (0.007)
IRCA ₉₀ × P ₈₉ × White	-0.009*** (0.003)		-0.013*** (0.004)		-0.007** (0.004)	
IRCA ₉₀ × P ₈₉ × Black		-0.004 (0.003)		-0.021*** (0.004)		-0.005 (0.004)
<i>N</i>	1,383,183	400,430	437,917	131,450	884,905	238,993
Clusters	480	478	480	464	480	467
\bar{Y}_{Pre} [<i>S.D</i>]	.87 [.33]	.95 [.23]	.29 [.46]	.24 [.43]	.073 [.26]	.036 [.19]

Notes: The data come from the US Decennial census and include three time periods: 1980, 1990 and 2000. The outcome variable in columns 1 and 2 is an indicator if people between the ages of 13 and 19 are enrolled in public school (1) or private schooling (0). In columns 2 and 3, the outcome is an indicator if people between the ages of 18 and 19 have a high school degree (1) or not (0). In columns 5 and 6, the outcome is 1 if, in the last 5 years, a person (between the ages of 13 and 19) moved across counties within the state or 0 if they remained in the same county. IRCA₉₀ is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990. P₈₉ is an indicator that is zero for 1980 and 1 for 1990 and 2000. White is an indicator that is 1 if a person is white and 0 if they are Hispanic. Black in an indicator that is 1 if a person is black and 0 if they are Hispanic. All regressions include county fixed effects and state-by-year fixed effects as well as all lower order terms of the triple interactions. They also include 1980 measures of county population, income, school-aged population (i.e., between 5 and 19), population that is poor as well as the white, black and Hispanic population size, each interacted with P₈₉. They also include a persons age, sex, poverty status and an indicator for whether they are native or foreign born. Standard errors (in parentheses) are clustered at the county. All regressions are weighted by the size of the 1980 county population. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4
IRCA and Hispanic Representation on School Boards

	Covariate Stability				Sample Stability			Imputation Stability	
	(1) No Covariates	(2) Baseline Covariates	(3) Additional Covariates	(4) VRA × Post	(5) Population < 75 th Pctl.	(6) Drop MM Counties	(7) Drop Imputed	(8) Control Imputed Counties	(9) Alternative Imputation
IRCA ₉₀ × P ₈₉	0.0366*** (0.0130)	0.0417*** (0.0141)	0.0334** (0.0131)	0.0369*** (0.0131)	0.0453* (0.0237)	0.0294** (0.0132)	0.0129 (0.0119)	0.0368*** (0.0131)	0.0277* (0.0153)
<i>N</i>	33,352	33,363	33,352	33,352	24,838	30,756	5,610	33,352	33,352
Clusters	3,032	3,033	3,032	3,032	2,258	2,796	510	3,032	3,032
\bar{Y}_{Pre} [<i>S.D.</i>]	.077 [.48]	.077 [.48]	.077 [.48]	.077 [.48]	.073 [.49]	.08 [.5]	.12 [.55]	.077 [.48]	.077 [.48]

Notes: The outcome is the number of Hispanic school board officials per 1987 number of school boards in a given county. IRCA₉₀ is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990. P₈₉ is an indicator that is zero for time periods prior to 1989 and 1 for periods on or after 1989. All regressions include county fixed effects and state-by-year fixed effects. They also include, except for column 2, 1980 measures of county population, income, school-aged population (i.e., between 5 and 19), population that is poor as well as the white, black and Hispanic population size, each interacted with P₈₉. Additional covariates in column 3 include 1980 measures of county birth rate, housing values, number of banking institutions, population under the age of 5 and the crime rate (1981 values), each interacted with P₈₉. The specification in column 4 includes an interaction between P₈₉ and an indicator for whether a county is covered under the pre-clearance requirement of Section 5 of the VRA. In column 5, counties with populations in the top 25 percentile are dropped. The model in column 6 excludes counties located in Hispanic or Black majority districts pre-1990 redistricting. In column 7, counties whose IRCA shares had to be imputed are all dropped while the model in column 8 includes the time-varying effect of counties whose IRCA shares had to be imputed. The model in column 9 uses an alternative method to impute IRCA shares as explained in the text. The model in column 6 excludes counties located in Hispanic or Black majority districts while the model in column 9 includes the time-varying effect of counties whose IRCA shares had to be imputed. Standard errors (in parentheses) are clustered at the county. All regressions are weighted by the size of the 1980 county population. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5
IRCA and the Number and Size of Local Government Units

	Gov't Units		Elected Officials		Elected Officials by Race	
	(1) Total Local	(2) School Boards	(3) Total Local	(4) School Boards	(5) Total Whites	(6) Total Blacks
$IRCA_{90} \times P_{90}$	-0.0952 (0.0640)	-0.00503 (0.0152)	-0.0136 (0.0246)	-0.00784 (0.0107)	-0.0466*** (0.0155)	-0.00580** (0.00243)
N	6,064	6,064	6,064	6,064	6,062	6,062
Clusters	3,032	3,032	3,032	3,032	3,031	3,031
\bar{Y}_{Pre}	27	4.8	5.9	5.3	3.6	.13
[$S.D$]	[29]	[7]	[2.8]	[2.4]	[2]	[.33]

Notes: The outcome variable in columns 1 and 2 is the total number of local government units (columns 1) and school board districts (column 2) aggregated to the county level. In columns 3 and 4, the outcome is the total number of elected officials per 1987 number of total local government units (column 3) or per 1987 number of school board districts (column 4) at the county level. In columns 5 and 6, the outcome is the total number of elected officials (by race) per 1987 number of local government units at the county level. The regressions include two time periods: 1987 and 1992. $IRCA_{90}$ is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990. P_{89} is an indicator that is zero for time periods prior to 1989 and 1 for periods on or after 1989. All regressions include county fixed effects and state-by-year fixed effects. They also include 1980 measures of county population, income, school-aged population (i.e., between 5 and 19), population that is poor as well as the white, black and Hispanic population size, each interacted with P_{89} . Standard errors (in parentheses) are clustered at the county. All regressions are weighted by the size of the 1980 county population. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6
IRCA, Local Spending and Local Revenues

	Per Capita Expenditure					Per Capita Revenue				
	(1) Total Education	(2) Total Elementary	(3) Direct Elementary	(4) Capital Outlays Elementary	(5) Higher Education	(6) Total	(7) Total Tax	(8) Total Income Tax	(9) Federal IGR Education	(10) State IGR
$IRCA_{90} \times P_{89}$	3.627*** (1.133)	3.588*** (1.129)	3.631*** (1.155)	0.630*** (0.171)	0.0397 (0.0908)	15.42** (6.398)	3.484 (2.480)	0.344* (0.184)	0.0235 (0.0767)	6.197*** (2.129)
<i>N</i>	41,640	41,640	41,640	41,640	41,640	41,640	41,640	41,640	41,640	41,640
Clusters	3,022	3,022	3,022	3,022	3,022	3,022	3,022	3,022	3,022	3,022

Notes: The outcome in columns 1 to 5 is per capita expenditure at the county level on various educational categories as labelled. In columns 6 to 10, the outcome is per capita revenue at the county level from different sources as labelled. $IRCA_{90}$ is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990. P_{89} is an indicator that is zero for time periods prior to 1989 and 1 for periods on or after 1989. All regressions include county fixed effects and state-by-year fixed effects. They also include 1980 measures of county population, income, school-aged population (i.e., between 5 and 19), population that is poor as well as the white, black and Hispanic population size, each interacted with P_{89} . The regressions also include the time-varying effect of the 1999 share of public schools in a given county that were recipients of Title I education funding. Standard errors (in parentheses) are clustered at the county. All regressions are weighted by the size of the 1980 county population.

Online Appendix for Paper:
Out of the Shadows and into the Classroom:
Immigrant Legalization and Hispanic Schooling

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A. Demographic Characteristics of IRCA Migrants

In this appendix, I present data from the December 1991 report to Congress from the Department of Health and Human Services which captures some of the demographic characteristics of the IRCA applicants. As shown in Figure A.1 these data indicate that the newly legalized are predominantly of working age, healthy and with relatively few children. More than half and two-thirds, respectively, are single and male and the vast majority of applicants were engaged in full-time work. Fully 22 percent of all applicants reported a household income of over \$600 per week; well over the poverty line, which, in 1989 stood at \$6,311 for a single person (\$121 per week) and \$12,675 for a family of four (\$244 per week).²⁰ In fact, median take-home pay for IRCA applicants stood at \$400 per week. Median household income in the population in 1989 stood at \$23,745, or \$456 per week. The report also makes clear that no more than 5 percent of the migrants reported being unable to work in the prior month. Accordingly, IRCA applicants were, by and large, an economically active and self-reliant group earning somewhere between the poverty threshold and median income.

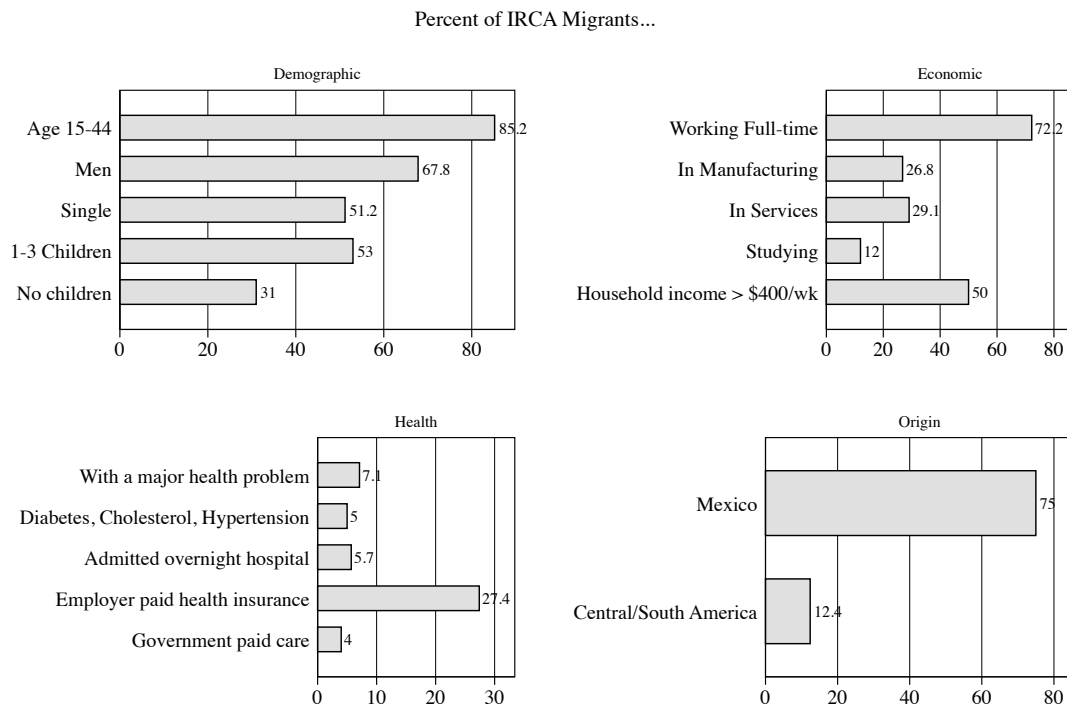


Figure A.1
Socio-economic characteristics of the IRCA applicants

Notes: These are the characteristics of the IRCA migrants as reported by Congress in 1991.
Source: DHHS (December 1991)

Figure A.2 reproduces the map presented in Sabet and Yuchtman (2023). It presents the geographic distribution of the IRCA applicants. Although many of those who applied for legal

20. These figures are taken from Robert A. Mosbacher and Barbara Everitt Bryant's 1991 report *Poverty in the United States: 1988 and 1989* for the US Census Bureau and can be accessed here: <https://www2.census.gov/library/publications/1991/demographics/p60-175.pdf>.

status under the IRCA did so from counties located in states near the border, the map also makes clear that IRCA applicants display variation both across the country and also within-states.

IRCA Migrants per 100,000 Population (by Decile)

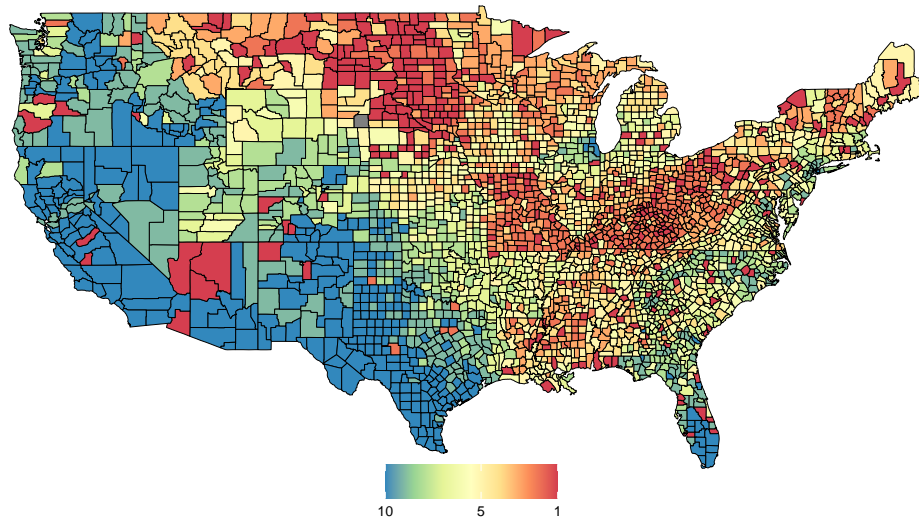


Figure A.2
Legalized migrants per 1,000 county inhabitants by decile

Source: Own data and reproduced from Sabet and Yuchtman (2023).

B. Pre-trends Testing and Sensitivity Analysis

In this Online Appendix, I assess pre-trends more formally, following approaches introduced by Rambachan and Roth (2023) and Freyaldenhoven et al. (2021), for the three main event studies presented in the paper: (1) student enrollment and the size of the teaching staff; (2) Hispanic student enrollment; and (3) Hispanic representation on school boards. For each event study, I estimate the slope of the line of best fit through each of the pre-period coefficients and then linearly extrapolate the trend into the post-period. For those figures where the linear pre-period slope is distinguishable from zero, I also report sensitivity analysis using relative magnitude restrictions.

B.1 Student Enrollment and Teaching Staff

I begin by reproducing the event study estimates for student enrollment and the size of the teaching staff (i.e., Figure 3 from the main draft). I now fit a linear regression through each of the six pre-period regression coefficients and extrapolate this line into the post period. The results are shown in Figure B.1, with the line of best fit shown in red.

For student enrollment, the slope of the pre-period trend is virtually zero ($-.0000681$) with a t -value of -0.23 (and p -value 0.828). Extrapolating this line into the post-period reveals a virtually flat trend, increasing confidence that the positive post-period effect is not merely reflective of a pre-existing linear trend.

I observe similar patterns for the size of the teaching staff. The slope of the pre-period line of best fit is $-.0005071$ with a t -value of -1.45 (p -value 0.220). Extrapolating this line into the post-period reveals a downward sloping trend that intersects the confidence intervals of very few of the post-period coefficients. This reinforces the view that the positive, post-1989 effect of the IRCA on the teaching staff is not merely an extension of pre-existing trends in schools located in high-IRCA counties.

Standardized Values of Teachers and Students with Linear Pre-Trend Extrapolation

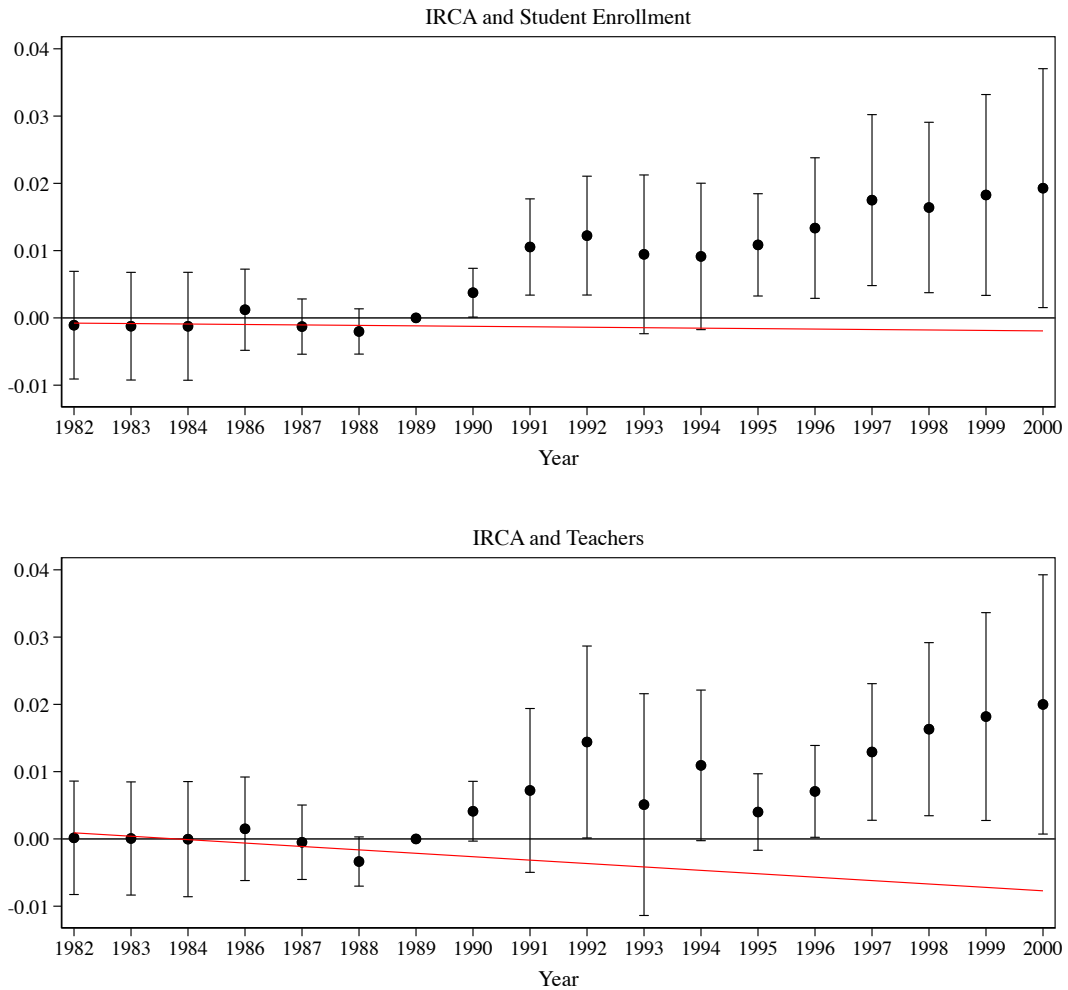


Figure B.1
IRCA and Student Enrollment and Teaching Staff with Pre-Trend Extrapolation

Note: This figure reproduces the two events studies shown in Figure 3 in the main draft but overlays, in red, the line of best fit estimated through the pre-period coefficients and extrapolated into the post period. The slope of this line is $-.0000681$ with a p -value of 0.828 for student enrollment and $-.0005071$ with a p -value of 0.220 for teaching staff.

B.2 Hispanic Student Enrollment

I carry out the same exercises for Hispanic student enrollment and present the results in Figure B.2. As shown, there is a positive pre-trend slope for Hispanic student enrollment.²¹ However, extrapolating this line into the post-period intersects the confidence intervals of just four of the eleven post-period coefficients. While there does appear to be a pre-trend in Hispanic student

21. The value of the pre-trend slope is 2.033. However, because there are only two pre-treatment coefficients, the estimated line of best fit has residual values of zero (and hence residual variance of zero) and for this reason does not return standard errors.

enrollment, therefore, it seems unlikely that the pre-trend explains the entire effect.

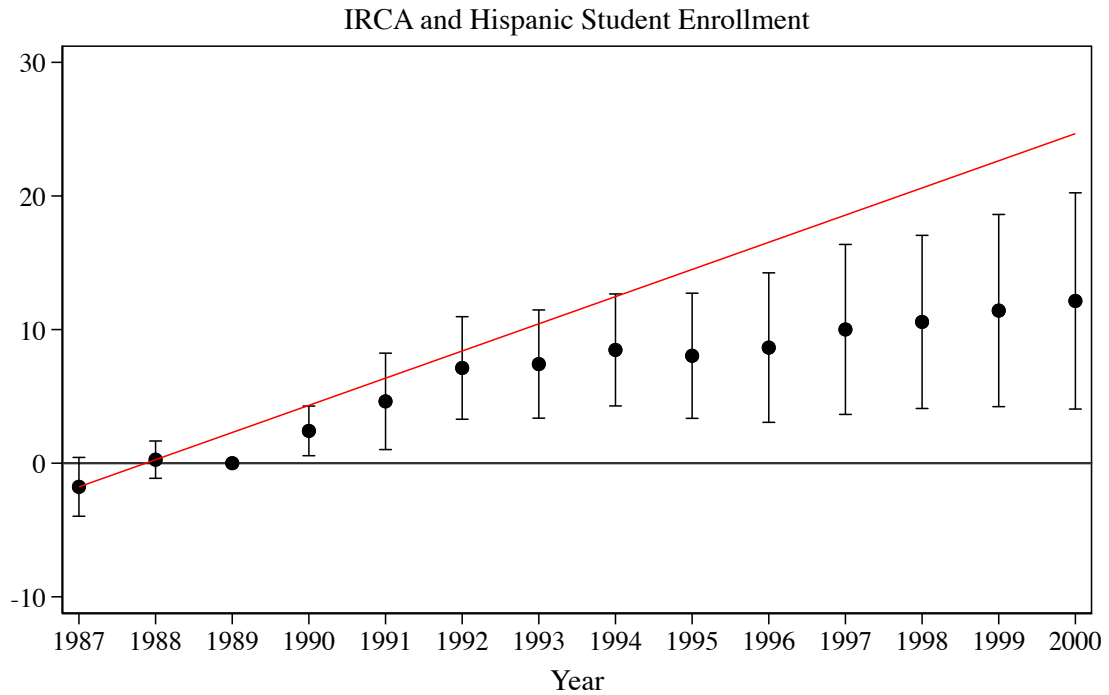


Figure B.2
IRCA and Hispanic Student Enrollment Pre-Trend Extrapolation

Note: This figure reproduces the the event study for Hispanic student enrollment shown in Figure 5a in the main draft but overlays, in red, the line of best fit estimated through the pre-period coefficients and extrapolated into the post period. The slope of this line is 2.033.

I carry out further analysis to determine how sensitive my conclusions are to possible violations of parallel trends using methods introduced by Rambachan and Roth (2023). I utilize their Stata package “honestdid” in order to place bounds on the relative magnitudes of violations of parallel trends in the post-period based on observed violations in the pre-period. I report the results in Figure B.3, which plots robust confidence intervals for the treatment effect of interest for $\Delta^{RM}(\bar{M})$ using different values of \bar{M} . The value of \bar{M} can be interpreted as how different violations of parallel trends can be in the post-period *relative* to those in the pre-period in order for inference to remain valid. As shown in the figure, the “breakdown” parameter for this effect lies at $\bar{M} = 0.5$, which suggests that the causal effect of the IRCA on Hispanic student enrollment is valid if we are willing to accept that post-treatment violations of parallel trends are no larger than half the magnitude of the pre-treatment violation of pre-trends.

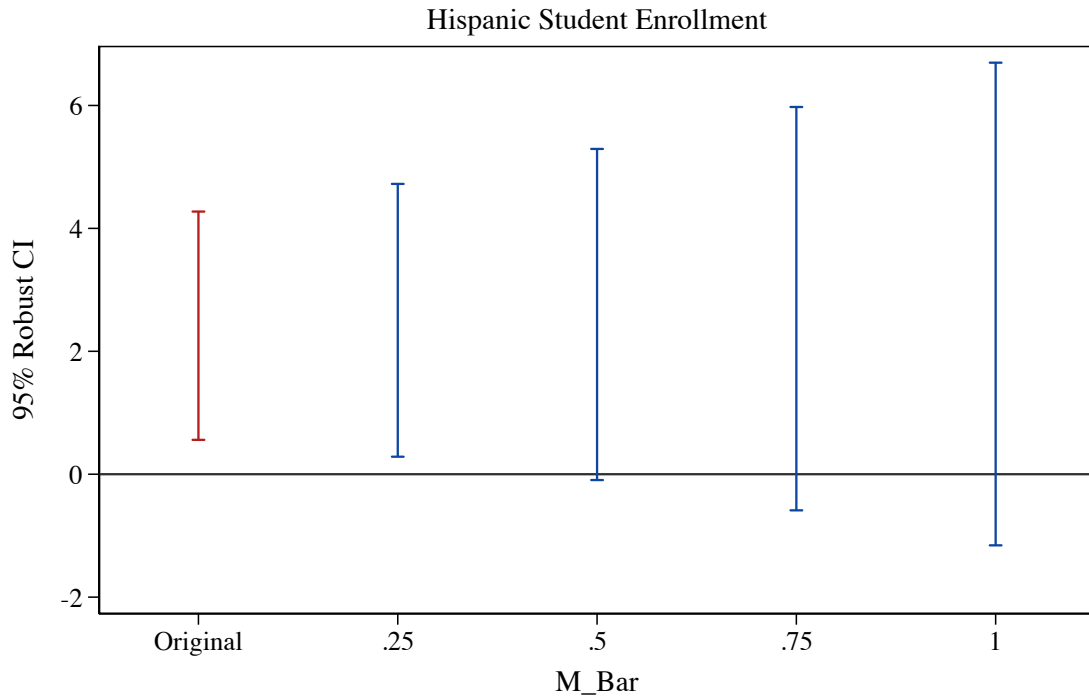


Figure B.3
 IRCA and Hispanic Student Enrollment with Relative Magnitude Restrictions

Note: This figure presents sensitivity analysis using relative magnitude restrictions $\Delta^{RM}(\bar{M})$ for the effect of the IRCA on Hispanic student enrollment.

B.3 Hispanic School Board Representation

In Figure B.4, I reproduce event study estimates of the effect of the IRCA on the number of Hispanics elected to school boards (Figure 6 of the main draft), again fitting a linear regression through the 5 pre-period coefficients and extrapolating this line into the post-period. The slope of this pre-trend line is .0024 with a p -value of 0.072, suggesting a positive pre-trend. As shown, however, when extrapolating this line into the post period, it does not intersect any of the confidence intervals of the post-treatment coefficients. This increases confidence that the positive effect of the IRCA on Hispanic school board representation is not merely reflective of pre-trends.

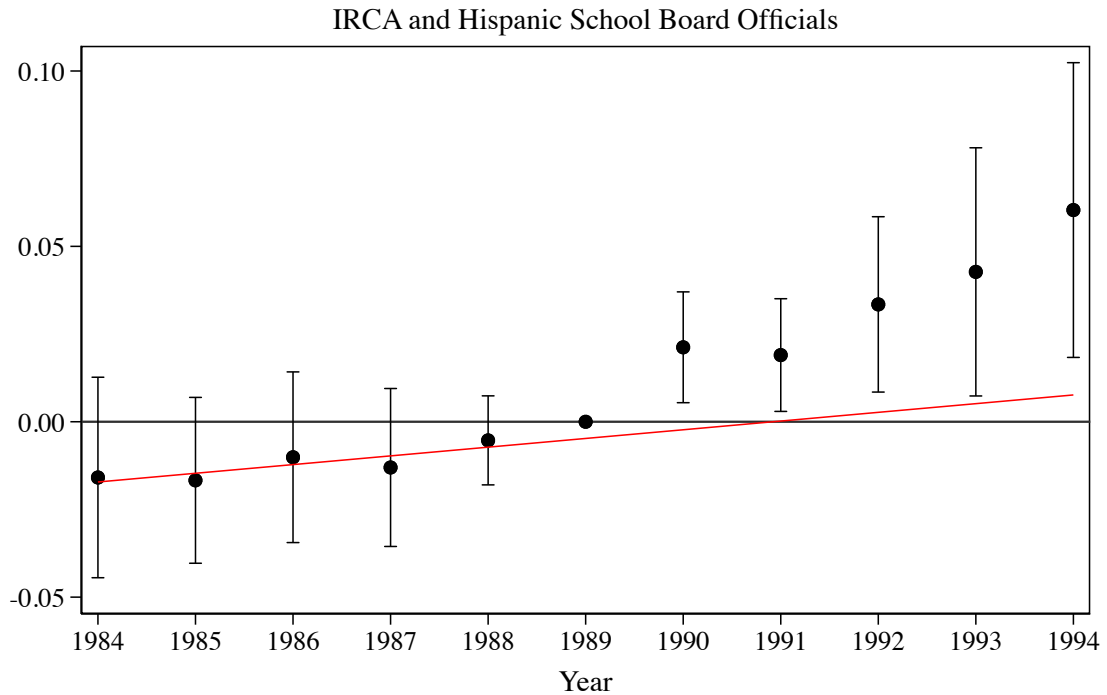


Figure B.4
IRCA and Hispanic Student Enrollment Pre-Trend Extrapolation

Note: This figure reproduces the the event study for Hispanic school board officials shown in Figure 6 in the main draft but overlays, in red, the line of best fit estimated through the pre-period coefficients and extrapolated into the post period. The slope of this line is .0024 with a p -value of 0.072.

In Figure B.5, I again present sensitivity analysis of my result using relative magnitude restrictions. The breakdown value for the parameter of interest now lies at $\bar{M} = 0.75$, which suggests that the positive and significant effect of the IRCA on Hispanic school board officials is valid to the extent that we are willing to impose that the post-treatment violations of parallel trends are no more than three quarters the size of the largest pre-treatment violation.

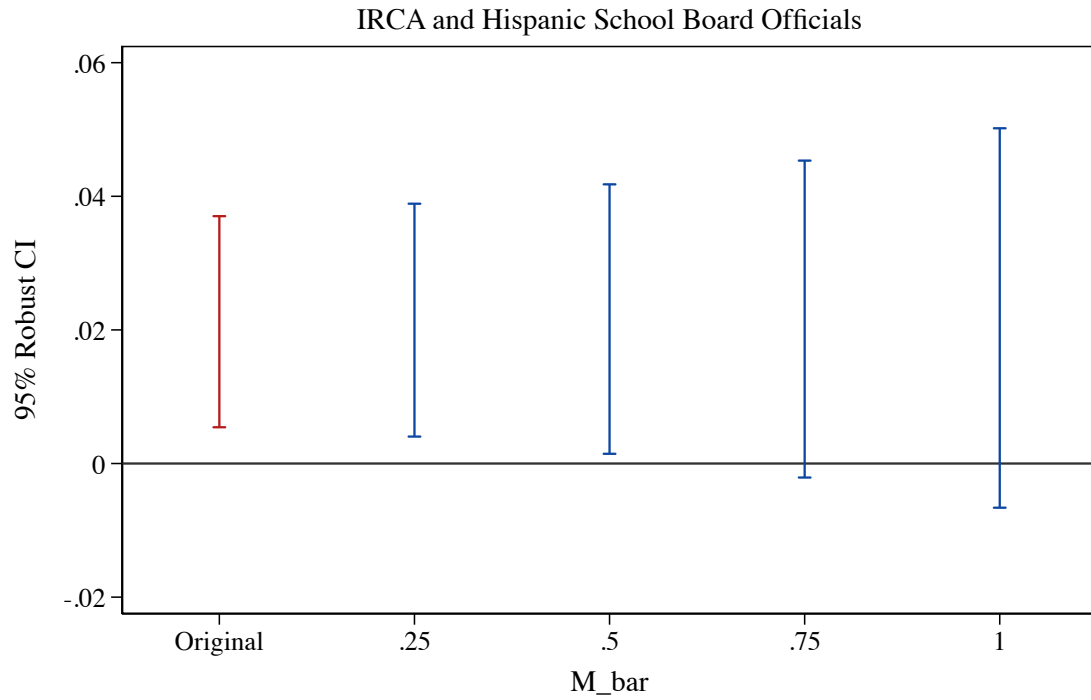


Figure B.5
IRCA and Hispanic School Board Officials with Relative Magnitude Restrictions

Note: This figure presents sensitivity analysis using relative magnitude restrictions $\Delta^{RM}(\bar{M})$ for the effect of the IRCA on Hispanic school board officials.

C. IRCA and Hispanic Elected Officials

In this Online Appendix, I present results of the IRCA for other types of Hispanic elected officials. I present the results in the form of an event study for Hispanic mayors, county officials, city Councillors and local judicial officers.²² As shown, the IRCA does not increase the number of Hispanic officials elected to any of these other types of local government units.

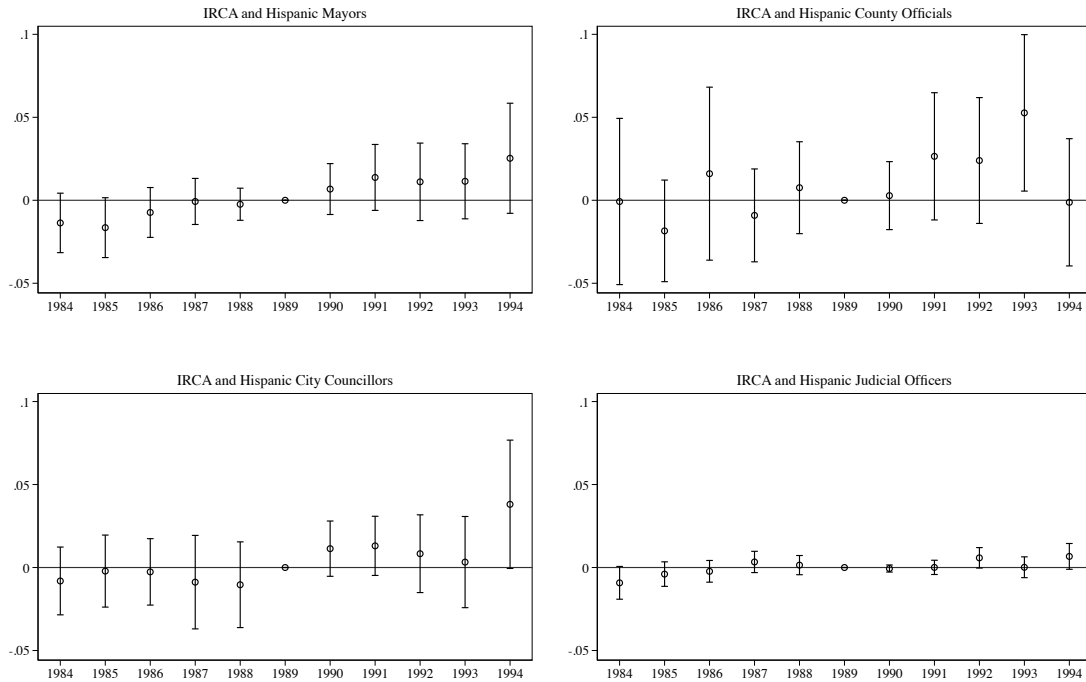


Figure C.1
IRCA and Hispanic Elected Officials

Note: This figure plots the coefficient on $IRCA_{90}$, the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990, when it is interacted with year dummies. The outcome variable is the total number of Hispanics elected to a respective public office in a county-year divided by the 1987 number of government units of that particular public office aggregated to the county level. For example: Hispanic Mayors and Hispanic City Councillors are each normalized by the 1987 number of municipal governments in a county, Hispanic County Officials are normalized by the 1987 number of county governments and Hispanic Judicial Officers are normalized by the total number of 1987 local government units (minus school districts) in the county. The regressions include county fixed effects and state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. All regressions are weighted by the size of the 1980 county population.

22. I normalize each type of public officer by the number of public offices of that type. For example, Hispanic Mayors and Hispanic City Councillors are each normalized by the 1987 number of municipal governments in a county, Hispanic County Officials are normalized by the 1987 number of county governments and Hispanic Judicial Officers are normalized by the total number of 1987 local government units (minus school districts) in the county.

D. IRCA and the Number of Government Units

In this Online Appendix, I present event study estimates using the full span of data for the number of local government units. I report the results in Figure D.1 and the patterns are clear. The IRCA does not lead to any differential changes in the number of government units at the county level. This result reinforces the view that the post-IRCA increase in Hispanic representation on school boards is not explained simply by increases in the number of school boards or school board positions.

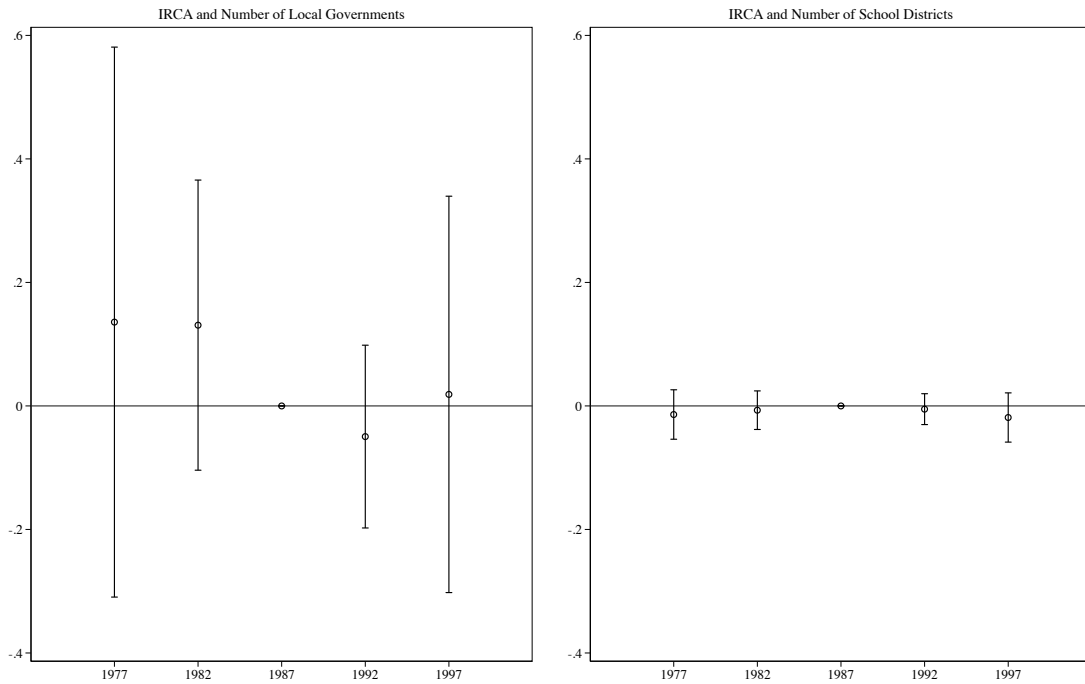


Figure D.1
Event Study Estimates of the IRCA and the Number of Local Government Units

Note: This figure shows the coefficient on $IRCA_{90}$, the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990, interacted with year dummies. The outcome variable is the total number of government units at the county level (in total and specifically school board districts). The regressions include county and state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. All regressions are weighted by the size of the 1980 county population.

E. IRCA and Political Participation

I present suggestive evidence in this Online Appendix that the IRCA led to increases in political mobilization at the local level. What motivates this analysis is the fact that the legalization of millions of Hispanic migrants lifts barriers of social exclusion not only for the migrants themselves but also for their families and social networks, members of which may already be US citizens. I therefore collect individual level survey data from the American National Election Studies (ANES) to test the extent to which the IRCA increased various measures of political participation.²³

For each person residing in a given county in the ANES, I link the 1990 number of per capita IRCA migrants in that county in order to test for two sets of outcomes: first, I examine whether the IRCA leads individuals to report differential levels of political participation and second, I test whether the IRCA leads to anti-Hispanic sentiment. For each outcome, I investigate the differential effect of the IRCA before and after legalization and for Hispanic and non-Hispanic persons. I present my results in Figure E.1, where I plot the coefficient on $IRCA_{c,1990} \times P_{89} \times Non - Hispanic$ and $IRCA_{c,1990} \times P_{89} \times Hispanic$ (all lower order terms included) when they are regressed against a range of individual survey responses.²⁴

I first test the effect of the IRCA on political participation. The outcome in model 1 is a count of the number of activities a person reported participating in during a political campaign and, as shown, there is evidence that the IRCA led to significant increase in participation for both Hispanics and non-Hispanics post-legalization. In model 2, the outcome is a count of the number of times a person reports discussing politics with friends and family. The coefficient for Hispanic persons, marginally significant with a p -value of .098, suggests that the IRCA stimulated greater political engagement within the social and familial networks of Hispanic persons. In model 3, I find that Hispanic persons are also significantly more likely to make donations to political campaigns (the outcome is one if a person donated money to a political campaign and zero if not). The outcome in model 4 is an indicator that is one if a person reported trying to mobilize others to vote during an election and zero if not. Interestingly, the coefficient is positive and significant for non-Hispanic persons and positive (but not significant) for Hispanic persons. This seems to suggest that the IRCA does, in fact, increase political mobilization in communities mixed legal status.

Of course, the positive and significant finding in model 4 could also reflect non-Hispanics mobilizing others to vote as a result of increased anti-Hispanic sentiment that the IRCA produced. In model 5, therefore, I leverage information on the Hispanic thermometer of the ANES to construct an indicator variable that is 1 if a person reports cold feelings towards Hispanics zero otherwise.²⁵ As shown, the IRCA is not associated with anti-Hispanic sentiment. Together, these results suggest that the IRCA increased political mobilization, among both Hispanics and non-Hispanics, without triggering anti-Hispanic backlash.

23. The ANES explains that its data are nationally representative but not at smaller-geographical levels. The analyses using these data also include just 250 counties, a fraction of the total number in the country. For these reasons I take these results as informative but suggestive.

24. The estimating model is the same as that presented in equation 7 but with two modifications: first, the outcome is measured at the level of people, living across US counties over time, and second, it includes a range of individual level controls including marital status, employment status, indicators if the person is black, white or Hispanic, age, income, education as well as an indicator if the person is native or foreign born.

25. The thermometer is on a scale from 0 (cold feelings) to 100 (warm feelings). The variable is 1 if a person reports a score between 0 and 40 and zero if they report feelings of 41 and higher. Changing this to a 50-50 split does not change the results.

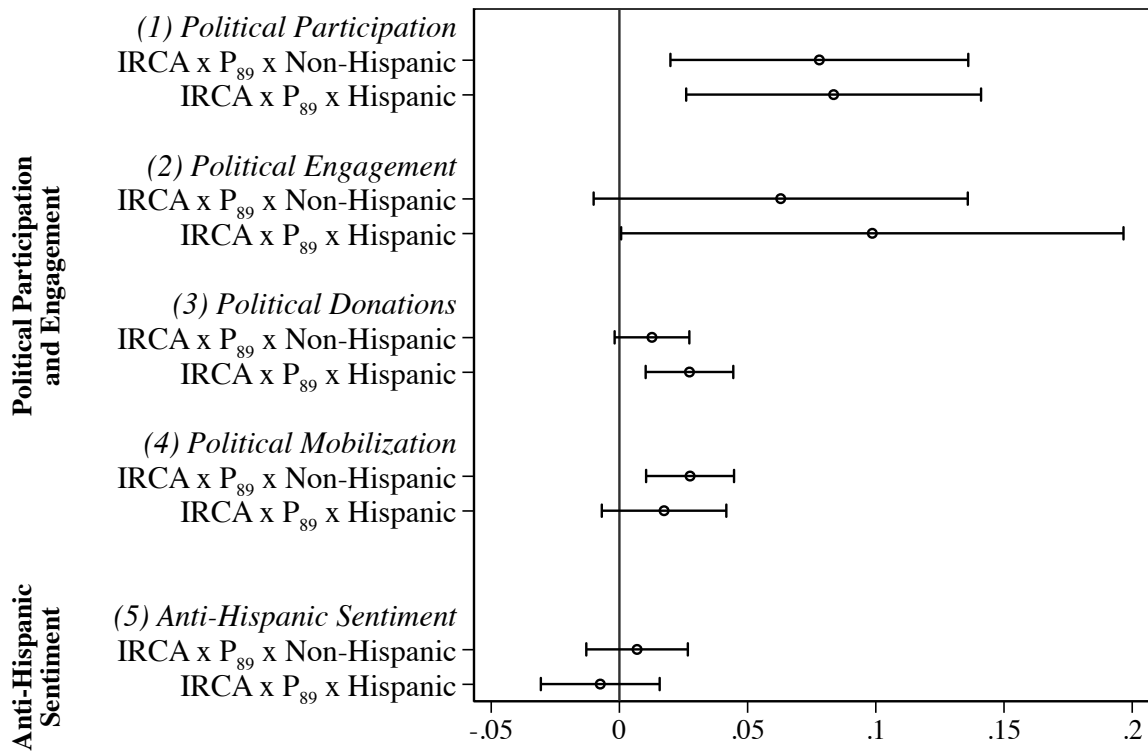


Figure E.1
Effects of the IRCA on Political Participation and Attitudes

Notes: This figure plots the coefficient on $IRCA_{90} \times P_{89} \times Non - Hispanic$ and on $IRCA_{90} \times P_{89} \times Hispanic$ (all lower order terms included) when regressed on individual survey outcomes from the ANES. Campaign Participation is a count of the number of campaign activities that a person reports participated while Political Engagement is a count of the number of times per week a person discusses politics with friends and family. All other outcomes are indicator variables that are 1 (and zero otherwise) if a person: donated to a political campaign; mobilized others to vote; or expresses cold feelings towards Hispanics. The regressions all include county fixed effects and state-by-year fixed effects as well as a range of county socio-economic and racial characteristics, measured in 1980 and interacted with P_{89} as described in table notes of Table 2. They also include individual level controls that include a persons: marital status, employment status, race (i.e., indicators if the person is black, white or Hispanic), age, income, education as well as an indicator if the person is native or foreign born. Standard errors are clustered at the county level and confidence intervals are drawn at 90 percent. For political participation outcomes, the regressions all include 254 clusters and N ranges between 12,599 and 12,935. For anti-migrant sentiment, the regression includes 219 clusters and 7,428 observations. All regressions are weighted by the size of the 1980 county population.