Legal Status, Local Spending and Political Empowerment: The Distributional Consequences of the 1986 IRCA*

Navid Sabet† Christoph Winter

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Abstract

We study the impact of immigrant legalization on the distribution of public resources, exploiting variation in legal status from the 1986 Immigration Reform and Control Act (IRCA). Governors allocate more resources to IRCA counties, an allocation that responds to their electoral incentives and that ultimately increases Hispanic educational attainment. Importantly, the effect emerges prior to the enfranchisement of the IRCA migrants and we argue it is driven by the IRCA's capacity to politically empower already legal Hispanic migrants in mixed legal status communities. The IRCA increases voter turnout in large, Hispanic communities and the number of Hispanics winning public office. (*JEL*: J15, H72, P16)

Keywords: immigrant legalization, distributive politics, state and local government

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†Corresponding author at: Faculty of Economics and Business, Goethe University Frankfurt, Theodor-W.-Adorno-Platz 4, 60629 Frankfurt, Germany. Email: sabet@econ.uni-frankfurt.de. Telephone: +49 (69) 798-34803. The author thanks the LSE, University of Toronto and the University of Guelph for their hospitality.

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1. Introduction

Legal status has first order economic consequences on the lives of individual migrants and on the communities in which they reside. Legalized immigrants have, for example, been shown to enjoy better labor market outcomes (Kossoudji and Cobb-Clark 2002; Pan 2012; Rivera-Batiz 1999), educational outcomes (Cortes 2013) and health (Baker 2010) while at the same time lowering crime (Baker 2015; Pinotti 2017). Surprisingly, less attention has been paid to understanding the reasons *why* legal status affects outcomes in the ways that it does, especially when considering the pervasive social and economic effects that (il)legality has on communities of mixed legal status.

In this paper, we shed light on these issues by exploiting variation in legal status arising out of the one, and to date only, amnesty program in the history of the United States, the 1986 Immigration Reform and Control Act (IRCA) which legalized 2.8 million mostly Hispanic migrants. Our primary objective is to understand the impact of immigrant legalization on the distribution of public resources——specifically, per capita intergovernmental revenue from state to local governments. We digitize a new and novel source of micro-data—on the universe of Hispanic officials elected to public office—in order to understand the precise mechanisms that drive the result. The picture that emerges is one of democratic responsiveness: governors, regardless of party, allocate more per capita resources to IRCA-affected counties after 1986, an allocation that responds to their electoral incentives, suggesting that it is politically motivated. Moreover, the IRCA leads to significant increases in local educational expenditure and in the high school completion rates of Hispanic youth, but not of Caucasian, African-American or Asian youth, supporting the view that governors targeted resources to Hispanic communities affected by the law.

Importantly, the IRCA provided legal status—that is, lawful permanent residency but without the full rights of citizenship—to nearly all those who applied for it. Five years after permanent residency, those legalized by the IRCA could acquire voting rights through naturalization.[1] This unique feature of the IRCA allows us to decouple the effect of legal status from enfranchisement in the interpretation of our results and we find that legalization positively predicts transfers *prior to enfranchisement*.[2] We argue that legal status attracts differentially more resources from the state even in the absence of enfranchisement because of its unique capacity to politically empower already legal Hispanic citizens in communities of mixed legal status. We support this claim with a number of pieces of evidence.

1. Legal status was granted as early as 1987, making 1992 the earliest year in which IRCA migrants could naturalize. In some cases, this gap was three years if the legalized met certain requirements (i.e. if applicants had spouses that were US citizens which, according to the Legalized Population Survey, was the case for around 8 percent of the IRCA migrants.). In reality, however, less than 250 IRCA migrants had naturalized prior to 1992 (Rytina 2002). See Online Appendix A for exact figures on naturalization rates among IRCA migrants.

2. One consideration is whether elected politicians *anticipated* the eventual enfranchisement of the newly legalized in their allocation decisions. By 2000, however, just one third of the legalized had naturalized (Rytina 2002), making actual or potential enfranchisement an unlikely explanation for our results. As Rytina (2002) notes, "the impact of IRCA was much more concentrated with respect to legal immigration than naturalization".

First, we find that the baseline effect of the IRCA on state transfers displays significant heterogeneity according to the size of the pre-existing, legal Hispanic population: that is, IRCA migrants attract more resources from the state in counties with larger shares of already legal Hispanic migrants. Second, we find that the IRCA leads to significantly greater levels of Hispanic political representation. Counties affected by the IRCA experience differential increases in the number of Hispanics elected to public office, a result that holds for officials at all levels, but that is strongest for Hispanic officials at the local level, in particular for school board officials and mayors. Third, we demonstrate that the IRCA leads to significantly higher levels of political participation in Hispanic communities of mixed legal status. Specifically, we find that the IRCA differentially increases voter turnout precisely in those communities with larger shares of already legal migrants in them. Together, our results shed new light on the ways in which immigrant legalization leads to the political mobilization and empowerment of local, Hispanic communities and on the economic consequences that follow.

Using a differences-in-differences regression framework, we compare the distribution of intergovernmental revenue[3] in counties affected by the IRCA with those unaffected by it, before and after 1986. Our baseline estimate suggests that treated counties receive 8 percent more per capita transfers than untreated counties, an effect that is amplified in those counties with larger shares of already legal Hispanic migrants in the population. The transfers are funded as a result of increased revenue at the state level generated from property tax and are not reflective of differentially more financial aid received by states from the federal government. Moreover, the result is not driven by differences in county economic or demographic characteristics, is robust to a wide range of alternative specifications and samples and is not reflective of differential pre-trends between treated and untreated counties, strengthening confidence in our identifying assumption. Our model also includes the 1980 Hispanic share of a county's population interacted with time dummies, enabling us to identify the effect of immigrant legalization independently from the differential, time-varying effect of the pre-existing size of the Hispanic community.

Of course, it is possible that the distributional effect uncovered reflects purely mechanical or bureaucratic forces outside the control of the governor. It may also reflect his or her social welfare concerns to better service the areas where the documented migrants reside. To rule out these competing explanations—and to demonstrate instead that the distributional effect uncovered is politically motivated—we analyze the responsiveness of our results to the political incentives and constraints facing state governors. We find that IRCA counties receive more resources from the state when their governor is eligible for re-election, faces an upcoming election, or is politically aligned with the state legislature. We also uncover heterogeneity along party lines: Democratic governors give substantially more than their Republican counterparts. We find no such relationship when examining the partisan composition of state legislatures, suggesting that the state executive responds more to the political incentives created by amnesty.

3. We refer to intergovernmental revenue, state aid and state transfers interchangeably. This variable and its definition are explained in more depth in Section 4.

What is more, we find no evidence that the IRCA increased welfare spending at the state or local levels, reinforcing the view that our results are driven more by discretionary political forces than by mechanical welfare increases.

As a next step, we focus the analysis on local expenditure to understand in which areas and on which constituents county revenue is spent. In this respect, we find that the only category of spending that experiences a significant increase as a result of the IRCA is education and that Hispanic youth—as opposed to Caucasian, African American or Asian youth—residing in IRCA counties experience significant improvements in the likelihood of completing high school. Interestingly, these results are only discernible as of 1994, long after legal status was granted but soon after these counties experienced increases in public investment in education, supporting the view that the transfers were targeted to Hispanic communities. This finding is consistent with the fact that schooling is first order concern for children of undocumented migrants (Amuedo-Dorantes and Lopez 2015; Brabeck and Xu 2010). It is also in line with previous research that documents the positive impact of educational spending on the educational attainment of students (Jackson, Johnson, and Persico 2016).

In bringing significant numbers of migrants "out of the shadows", we argue that legalization, as distinct from enfranchisement, lifts entrenched barriers of social exclusion not only for the undocumented migrants but also for their families and co-ethnics, one expression of which is greater political mobilization and participation. We posit that governors differentially allocate resources as a response to this newfound political activity. We support this claim with two pieces of evidence.

First, using new and novel microdata, we document the effect of the IRCA on Hispanic political representation. To this end, we digitize data that contains information on more than 60,000 Hispanic officials elected to public office between 1984 and 2000. Using the same two sources of variation—between treated and non-treated counties before and after 1986—we identify a clear pattern: counties with larger shares of documented migrants experience positive and significant increases in the number of Hispanic migrants entering public office. The trends in the number of Hispanics elected to public office exhibit no distinguishable difference in counties affected by the IRCA compared to those unaffected by it in the two periods prior to the passages of the IRCA in 1986, suggesting that omitted factors with respect to where undocumented migrants reside play little to no role in Hispanic selection to public office.

Decomposing the data further, we find that, while there are increases in representation at the state and federal level, the effect is driven almost entirely by stronger representation at the local level, and in particular among Hispanic school board officials and mayors. What is more, counties affected by the IRCA not only elect more Hispanics to office but tend to select more politically mobile Hispanics to office, in the sense that the number of Hispanics who climb the political ladder and assume positions of greater influence increases as a function of the IRCA.

Second, we draw on county and individual level voting data in order to demonstrate the impact of the IRCA on political participation. The IRCA, we find, leads to significantly greater

levels of voter turnout precisely in those communities with larger shares of already legal migrants in them, pointing to the capacity of the IRCA to mobilize the existing Hispanic community. Individuals, by contrast, characterized by anti-immigrant sentiment residing in these same counties are no more likely than others to vote in either Presidential of Gubernatorial elections, suggesting that the IRCA led to meaningful Hispanic political participation without triggering nativist backlash.

Our work offers three main contributions. First, we add to the literature on the economics of legal status. In this respect, the IRCA has been used as a credible policy shock to identify the impact of legal status on various social and economic outcomes, primarily at the level of the individual migrant. These include the effect of legalization on educational outcomes (Cortes 2013) as well as on earnings, employment prospects and ability to speak English (Cascio and Lewis 2019; Kossoudji and Cobb-Clark 2002; Rivera-Batiz 1999; Pan 2012). Other studies have shown the positive effect of legal status on health outcomes (Baker 2010) and on lowering crime, both in the United States (Baker 2015; Freedman, Owens, and Bohn 2018) and in Italy (Pinotti 2017). While these studies have answered many questions regarding the social and economic effects of legal status, its distributional effect on public resources at the state and local level remains an open question. Moreover, by examining the impact of legal status on political representation at the local level, the paper advances the literature by introducing an entirely new dimension to the economics of legal status. To the extent that the other social and economic improvements of legal status are actually consequences of increased public investment and political representation that arise out of that status, the paper enhances our understanding of two key missing mechanisms in the literature.

Second, our paper contributes to the literature that examines the political and economic consequences of the expansion of voter franchise. This scholarship has examined the extension, or the de-facto extension, of voting rights to such groups as women (Miller 2008), African Americans (Cascio and Washington 2014), young people (Bertocchi et al. 2020) and lesser educated citizens (Fujiwara 2015). We advance this literature in a number of ways. First, by exploiting the unique institutional features of the IRCA, we are able to decouple immigrant legalization from immigrant enfranchisement in the explanation of our results. We argue that the effects of the former are more far-reaching than the latter because legal status lifts barriers of social exclusion not just for the undocumented but for their communities and family networks. Second, we uncover a new and novel political economy mechanism that links legal status to improvements in the socio-economic outcomes of individual migrants. Our data on Hispanic public officials, for example, enables us to show that legal status leads to a significant flourishing of political activity at the local level. Moreover, we are able to shed light on the precise political mechanisms that motivate a governor's distributional decisions, showing, for example, that the distributional response is borne of discretionary political choice and targeted to meet the educational needs of Hispanic youth. We are also able to distinguish the relative role that the state executive plays, as compared with the state legislature, in responding to the political

incentives created by amnesty.

Finally, the paper adds to our understanding of the process of political selection. This literature includes theoretical and empirical work that has helped us to better understand, among other things, the quality of public officials (Caselli and Morelli 2004), what attracts high-quality types to public office (Ferraz and Finan 2009), the effects of high-quality leaders on economic growth (Besley, Montalvo, and Reynal-Querol 2011), and to what extent it is possible to select leaders that are both of high quality and representative of a wide cross-section of society (Dal Bó et al. 2017) or of distinct groups (Besley et al. 2017). The question of politician identity has also received attention within the citizen-candidate framework put forward by Osborne and Slivinski (1996) and Besley and Coate (1997). Pande (2003) and Chattopadhyay and Duflo (2004), for example, both consider the impact of political reservation for minority groups—scheduled castes and women, respectively—on policy outcomes in India. This paper contributes a new, complementary dimension to this scholarship: it considers, in the absence of mandated political reservation, what factors enable underrepresented groups to gain stronger political representation in the first place. This question is of particular importance when considering the current political and economic climate of the United States. Hispanics are the largest ethnic minority in the country. Notwithstanding, Hispanic representation in politics is not reflective of their share in the population.[4] Accordingly, this paper contributes to enhancing our understanding of the institutional features—immigration policy in particular—that determine who gets elected to office.

The rest of this paper proceeds as follows: Section 2 discusses the historical background of the IRCA. In Section 3 we provide anecdotal evidence on the transformative influence of legal status on local communities and articulate theoretical motivation that guides our empirical analysis. Section 4 describes our data while Section 5 outlines our econometric methodology and, along with sections 6, 8 and 7, presents our results. Section 9 concludes.

2. The Immigration Reform and Control Act

The Immigration Reform and Control Act (IRCA) of 1986 was, to date, the most extensive piece of legislation put forward by the United States government to address the question of undocumented immigration. The passage of the IRCA was by no means straightforward. It began in the 1970s when the legislative and executive branches of government considered various elements of comprehensive immigration reform. These efforts gained momentum when, in 1977, Congress appointed the Select Commission on Immigration and Refugee Policy which presented, in 1981, a proposal for immigration reform which was ultimately rejected. In the years that followed, several other proposals were put forward and variants of the IRCA were

4. By way of example, consider the results of the 2020 congressional elections: 10.5 percent of that body (46 members) comprises representatives of Hispanic origin even though Hispanics make up around 18 percent of the United States populatio. See PEW research for figures on Hispanics in Congress (https://pewrsr.ch/3HStTww) and the US Census Bureau "Quick Facts" website for the share of the population of Hispanic origin (https://bit.ly/3r9CdSo).

passed through either the Senate or the House but none was able to win complete approval until the 99^{th} Congress passed the IRCA on 17 October 1986 and which was signed into law on 6 November 1986.[5]

The purpose of the IRCA was to restrict the flow of undocumented migrants into the United States. It rested on three main pillars: 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).

The legalization program is generally regarded as the law's most successful provision. It provided legal status to virtually all three million undocumented migrants in the country at this time (Baker 2015).[6] The Act provided two programs for two distinct groups of unauthorized workers. First, the "pre-1982s" under section 245A of the law enabled undocumented immigrants who resided in the country for an uninterrupted period from before 1 January 1982 to legalize (DHHS (December 1991), Cascio and Lewis (2019)). Second, the Special Agricultural Workers (SAW) under Section 210 of the law allowed applications from unauthorized migrants who could show that they carried out 90 days of work on select USDA defined seasonal crops in the year leading to 1 May 1986 (DHHS (December 1991); Cascio and Lewis (2019)). Pre-1982 applicants were eligible to apply within a 12-month time frame extending from May 1987 to May 1988 whereas SAW applicants had an 18-month application period from 1 June 1987 to 30 November 1988 (DHHS, December 1991). 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 for naturalization.

At the time of the Act, there were some 3 million undocumented immigrants residing in the United States, corresponding to nearly 1 percent of the population (Wasem 2012; Baker 2015). Both application periods—the 12 months for the pre-1982 program and 18 months for the SAW program—were strictly enforced and, by the end of the application period, roughly 3 million people applied for temporary resident status, of whom some 2.8 million (i.e. over 90 percent) were granted permanent residence.[7] Online Appendix A provides details concerning the demographic characteristics and geographic distribution of the IRCA applicants. As shown, the IRCA migrants, 87.4 percent of whom were of Mexican or Latin American origin, are

5. The timing of the IRCA's passage in 1986 was sudden and unexpected. Just days before its passage in Congress, "congressional leaders pronounced it dead, this time after more than fifteen months of hearings, legislative negotiations and debate" (Fuchs 1990). Speaking to this idea, Representative Daniel E. Lungren (R-California) remarked on the day of the bill's passage that the IRCA was "a corpse going to the morgue, and on the way to the morgue a toe began to twitch and we started CPR again" (Fuchs 1990).

6. Baker (2014) and Baker (2015) cite a number of reasons why the IRCA "represented a near-universal legalization of immigrants in the United States." Indeed, the original INS tape data confirm that, by 1990, less than 5 percent of the undocumented had their applications rejected.

7. These figures are derived from the Legalization Summary Tapes of the Immigration Naturalization Service and are confirmed by (Rytina 2002).

concentrated in around 330 counties across the country. They were, moreover, an economically active and self-reliant group, earning somewhere between the poverty threshold and median income.

3. Theoretical Motivation: The Transformative Influence of Legal Status

In our setting, an incumbent governor controls the distribution of transfers flowing from the state budget to the various counties in the state. The politician, we argue, is concerned both with the welfare of the population and his or her own re-election. A sudden change of legal status in a large and homogeneous group of residents in a county will thus change the politician's decision on how to distribute state resources so as to optimize his or her re-election chances and the welfare of the population. Theoretically, legal status affects this decision in four ways.

First, the most obvious channel linking legal status to targeted public expenditure is enfranchisement via naturalization. Indeed, legalization under the IRCA provided a path to naturalization for all those who were granted legal status under its provisions. In theory, therefore, we expect elected state politicians to target resources to counties affected by the IRCA because the law generated a sizable bloc of new, Hispanic voters. In practice, however, just 30 percent, or nearly 815,000, of the migrants legalized by the IRCA became naturalized citizens by 2000 and the overwhelming majority of these did not naturalize until the second half of the 1990s, long after legal status was granted (Rytina 2002).[8] Thus, while we cannot conclusively rule out immigrant enfranchisement as a potential channel, the relatively few number of migrants that actually naturalized as a result of the IRCA and the relatively long gap between legalization and naturalization makes enfranchisement an unlikely candidate to explain our results.

In this connection, we argue, second, that legal status does more to influence the social, economic and political participation of individual migrants than enfranchisement because the change from illegality to legality lifts barriers of social exclusion in ways that the change from disenfranchised to enfranchised does not. For example, legal status makes it possible for migrants to obtain authorization to work, loosens restrictions on social and physical mobility and removes the anxiety and stress associated with constant fear of deportation (Yoshikawa, Godfrey, and Rivera 2008; Sabo and Lee 2015; Brabeck, Sibley, and Lykes 2016). It also increases political participation, even though non-naturalized migrants who enjoy legal status are ineligible to vote in many elections. Using data from the Latino National Survey (LNS), McCann and Jones-Correa (2016) find, for example, that 80 percent of legal yet 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. Accordingly, we expect legalization, independent of naturalization and enfranchisement, to increase democratic responsiveness because of its ability to politically mobilize legalized migrants.

8. Online Appendix A provides the exact figures on persons naturalized—in total and from among the IRCA migrants—between 1990 and 2000.

Third, illegality among Hispanic migrants in the United States has been shown to cast a large shadow of social exclusion on households of mixed legal status. In such cases, families with even a single undocumented migrant report lower levels of access to basic institutional resources such as bank accounts, drivers' licenses, health care and funding for child care, all for fear of deportation and detention (Yoshikawa, Godfrey, and Rivera 2008; Brabeck, Sibley, and Lykes 2016). Children from such households report greater levels of anxiety and do less well in school as compared to children from households where both parents have legal status (Brabeck and Xu 2010; Brabeck and Sibley 2016). Lisa, a twenty-two year old college student in the Lower Rio Grand Valley region of Texas explains this influence in the following terms:

> 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)

Granting legal status, we argue, lifts barriers of social exclusion for people like Lisa, of which there were many. A survey of a sample of the migrants legalized by the IRCA, for example, indicates that 42 percent of them had one or more family members, minors or adults, who were already citizens prior to the IRCA. Eight percent report being married to a US citizen while nearly 13 percent report having one or more relatives, aside from their spouse and children, who were citizens pre-IRCA.[9] We therefore expect legalization to influence the allocation of state resources because of its influence on the political mobilization of the family networks of those legalized by the IRCA.

Finally, we expect legalization to influence the governor's decision to allocate resources because of the effect (il)legality has on Hispanic communities of mixed legal status. In this respect, a number of studies have documented the negative spillover effects of tough immigration policy on citizens and permanent residents of Mexican descent, especially in areas with large concentrations of undocumented migrants (Sabo and Lee 2015; Aranda, Menjivar, and Donato 2014). These include greater likelihood of being pulled over, detained and arrested for driving without a license (Donato and Rodriguez 2014), marginalization owing to changing perceptions among whites that all Hispanics are illegal and hence criminal (Flores 2014) and reluctance to use public services for fear of drawing attention and having their own legal status revoked (Sabo and Lee 2015). In this connection, Sabo and Lee (2015) find that permanent residents and citizens of Mexican descent in areas with large concentrations of undocumented migrants "internalize their subordinated racialized status" and fear that "their legal status can be easily revoked if they file complaints" with local officials, in particular the police and immigration officers. They go on to explain the effect of illegality on already legal residents and citizens in the following terms:

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

> . . . [I]n the border region, immigrants and migrants of Mexican descent with US permanent residence and citizenship feel vulnerable to being identified as "out of place"and, subsequently, the target of immigration enforcement. Immigration officials' presence was pervasive and not confined to the US port of entry but was experienced by participants in public spaces, including neighborhoods, work sites, and local markets.

The outcome of the IRCA was thus of major importance not just for the migrants themselves but also for their families and communities. We argue that granting legal status lifts barriers of social exclusion, enabling millions of migrants and millions more in their family and social circles to participate more freely in the affairs of society, one expression of which is greater political participation. Accordingly, we expect legalization to lead to significantly higher levels of political participation and engagement, especially in those counties with higher shares of already legal Hispanic residents in the population. And we posit that the incentive for politicians to distribute resources to counties affected by the IRCA was essentially a political one, intended to win not so much the political support of the 1 million new voters the law generated but primarily to capture the significantly larger spillover effects that legalization had on the political participation and engagement of already legal citizens in communities of mixed legal status. In the following sections, we empirically verify the validity this claim.

4. Data

In this section, we provide an overview the main variables used in the study. Further details on the variables and their sources are provided in the Supplemental Data Appendix.

IRCA Migrants: The key explanatory variable in our study is the cumulative number of IRCA applicants per 1,000 county inhabitants in the United States fixed to its 1992 level.[10] This value ranges from 0 to as many as 50 applications per 1,000 inhabitants. We obtain this information from Baker (2015) who, in turn, takes it from the Immigration and Naturalization Service (INS). An important limitation of this data is that there are no county identifiers for migrants who applied from counties with populations less than 100,000 or with fewer than 25 applicants. Fortunately, this issue (affecting less than 10 percent of the migrants in our data) does not impact our findings. In our baseline analysis, we 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. In Online Appendix C, we describe our baseline approach

10. We use applications per capita as opposed to legalized per capita so as to rule out any potential selection issues that might arise for those whose applications were actually accepted. Empirically, however, very few had their applications rejected (as explained in Section 2) and, in our analysis, it makes no difference whether we use legalization or application information, as shown in Column 5 of Table C.5 in the Online Appendix. As such, we use the term "applicants" and "legalized" interchangeably.

in more detail and explore three alternative approaches to address this issue—an alternative method for imputing the share of IRCA applicants in small counties, simply treating these counties as control counties (and hence assigning them a migrant share of zero) and controlling for the time-varying effect of small counties with unassigned migrant shares—and we find that our results are unaffected.[11]

County covariates: We also take from Baker (2015) measures of county poverty, population, unemployment and income, all of which are used as control variables in our analysis. Additional county covariates, including information on the Hispanic, White and African-American share of the population, the share of people over 18, the share of households with children, births per capita and educational attainment, come from the USA Counties Database.

County finances: We use per capita intergovernmental revenues (IGR) from state governments to local governments (counties, cities, municipalities aggregated to the county) as our primary dependent variable.[12] The Census Government Finance and Employment Classification Manual defines this variable as "[a]mounts received directly from the state government, including federal aid passed through the state government and state aid channeled through intermediate local government (e.g counties) which have no discretion as to its distribution. [It] includes state grants-in-aid, regardless of basis of distribution." Correspondence with staff at the Census Bureau confirms that "each state determines what specific funding sources (if any) are used for grants to local governments" and that "each state determines the nature, amount and distribution of state grants internally."[13] Online Appendix B details the budget making process in US states and brings to light the unique powers that governors have in formulating and allocating it. Table B.1 in that same Appendix details what is and what is not included in the intergovernmental revenue and gives an indication as to what types of local activity these revenues support.

Our data on county revenues and expenditures are all taken from the U.S. Census Bureau, Annual Survey of Local Government Finances and Census of Governments yearly series. This database spans fiscal years 1957, 1962, 1967, and 1970 to 2006, though the annual series begins as of 1973. Although an annual survey, not every variable, including intergovernmental revenue, is surveyed in every county in every year. This is the case for around 35 percent of the counties in our data. For this reason, we linearly interpolate the revenue data. However, as shown in Online Appendix C.3, our baseline estimate is not dependent on this linear interpolation nor is it affected by the fact that IGR is not surveyed in every county in every year.

11. It should also be noted that this issue is not relevant when we aggregate IRCA migrants to the state level (as we do in Table 2, for example.) This is because every migrant has a state identifier.

12. On average, counties in the sample receive USD 16 million in intergovernmental revenue per year, an amount which comprises approximately 30 percent of all local government revenue (shown in Figure B.1 in the Online Appendix).

13. Personal correspondence with Michael Fredericks of the Local Government Finance Statistics Branch of the Census Bureau on 26 November 2018.

Governors data: We utilize a host of governor related data including party affiliation, his or her name, and indicators for whether (s)he is a lame duck or in an election year in order to better understand the responsiveness of the governor to the IRCA. These data are obtained from Klarner (2013). We add to these data information on the partisan make up of state legislatures which we digitized, respectively, from the National Conference of State Legislatures.

Election data: We obtain Presidential election results from the USA Counties Database. In addition, we purchased county level election outcomes in Gubernatorial elections from Dave Leip's Atlas of U.S. Presidential Elections (http://uselectionatlas.org). However, these data are only available as of 1990 and we therefore use Presidential election results as a proxy where necessary. As shown in Figure G.1 in the Online Appendix, the two measures are highly correlated. For individual voting data, we rely on survey data from the American National Election Studies (ANES).

Hispanic Public Officials – the NALEO Roster: We argue that the political incentive to allocate resources to communities affected by the IRCA arose out of a broader political mobilization in these same communities. We assess this claim by testing whether legalizing undocumented Hispanic migrants bears any influence on their representation in politics, as measured by the number of Hispanics elected to public office. To measure the impact of legal status on this outcome, we digitize a novel source of data taken from the historical archives of the National Association of Latino Elected and Appointed Officers (NALEO), a non-profit, non-partisan organization which has, among other things, gathered data on Hispanic persons elected to public office from the local to the federal level since 1984.

Our digitization work provides us with a dataset of 60,096 individual officials with information on the level of office served, the title of the role, the political affiliation of the official, gender and, perhaps most importantly, their address (including ZIP code) which we use in order to generate county-level aggregates of the total number of Hispanic public officials in a given county in a given year. Officers at the county level and lower are aggregated to the county in which they serve whereas federal and state officers are aggregated to the county to which their constituency ZIP code corresponds. The more than 500 counties across which the Hispanic officials are distributed contain, on average, 9 officials, some of which contain just one officer and some of which contain up to 100. Further details concerning these data can be found in the Supplemental Data Appendix.

5. Immigrant Legalization and Intergovernmental Revenue

5.1 The Evolution of IGR: Raw Data

Our aim is to understand the impact of documenting undocumented migrants on the distribution of intergovernmental revenue from state to local governments. The primary identifying assumption of our econometric model is that intergovernmental revenue in treated and control counties evolved along similar paths prior to treatment and would have continued along similar paths in the absence of the IRCA. Prior to estimating the parameters of the model, therefore, it is informative to understand the evolution of IGR over time so as to lend credence to our identifying assumption. The top panel in Figure 1 shows the trends in IGR for the period between 1980 to 2000 in those counties that received applications for legal status with those that did not. As shown, the two county types developed along similar paths prior to the passage of the IRCA in 1986. It is only after 1986 that appreciable differences appear between the two county types.

As a more rigorous test for pre-treatment differences, we plot the coefficients of an event study as specified in equation 1:

$$y_{c,t} = \delta_c + \alpha_t + \sum_{j=1980}^{2000} \beta_j \left[IRCA_{c,1992} \times D_t^j \right] + \epsilon_{c,t} \tag{1}$$

Where $y_{c,t}$ is per capita intergovernmental revenue from state to local governments (in 1999 USD) in county c in year t; $IRCA_{c,1992}$ is the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level; and D_t^j is a dummy set to one when $t = j$ ($\forall\ j \neq 1986$). We capture county fixed effects by δ_c and year specific heterogeneities by α_t while $\epsilon_{c,t}$ is an idiosyncratic disturbance term clustered at the county level. The results are shown in the bottom panel of Figure 1 and indicate that the difference in transfers received between treated and non-treated counties only becomes positive and significantly different to zero after 1986 and not before, increasing confidence in our identifying assumption. Moreover, the figure reveals that the effect arises prior to 1992, the first year when IRCA migrants gained eligibility to vote, and remains stable thereafter. This highlights the important role of immigrant legalization, as opposed to immigrant enfranchisement, in driving transfers.

5.2 Baseline Estimates

We impose more structure on model 1 in order to estimate the parameters of a generalized differences-in-differences regression specified in equation 2.

$$ln(y)_{c,t} = \beta_0 + \delta_c + \zeta_{st} + \sum_{j=1980}^{2000} \gamma_j [LHISP_{1980} \times D_j^t] + \beta [IRCA_{c,1992} \times P_{86}] + \Theta \cdot \mathbf{X_{c,t}} + \epsilon_{c,t} \tag{2}$$

Where $ln(y)_{c,t}$ is the natural log of per capita intergovernmental revenue from state to local governments (in 1999 USD) in county c in year t and δ_c and $\epsilon_{c,t}$ are defined as before. Our measure of treatment intensity, $IRCA_{c,1992}$, is now interacted with a binary variable P_{86}, that is one if $t > 1986$ and zero otherwise and β is the coefficient of interest. In this model, we replace year fixed effects, α_t, with state-by-year fixed effects, ζ_{st}, to account for state-specific, time-varying shocks that might affect both the number of IRCA applicants and the amount of transfers, including governor specific characteristics or other state-year political or economic shocks.[14] The model also includes the interaction between the log of the 1980 Hispanic share of the population, denoted $LHISP_{1980}$, interacted with year dummies, indicated by D_j^t. This is especially important when considering that the Hispanic share of the population is not balanced between treated and control counties.[15] If, for example, the IRCA signaled to all Hispanic migrants that socio-political integration was one step closer, Hispanic communities might have reacted differentially to the IRCA regardless if they had undocumented migrants among them. The inclusion of this interaction thus allows us to identify the effect of immigrant legalization on transfers once the differential, time-varying effect of the pre-existing size of the Hispanic community has been accounted for. We include a vector of county-level covariates, $\mathbf{X_{c,t}}$, that includes poverty and unemployment rates, income, population as well as the share of the population that is white and African-American.

The trends shown in the raw data are borne out in the regressions. To ease interpretation, we estimate, in Panel A of Table 1, a simplified version of model specified in equation 2 where we replace our measure of treatment intensity with an indicator that is one if a county received one or more applications for legal status and zero otherwise. The table shows our results and we see precisely estimated coefficients of similar magnitude across a number of specifications. Column 1 is our baseline estimate and suggests that IRCA counties received 8.12 percent more per capita transfers than other counties. IRCA counties have, on average, 50,605 Hispanics; an 8.12 percent increase in intergovernmental revenue as a result of the IRCA thus implies an increase in state aid by around USD 1.3 million, or USD 25 per Hispanic per year. If we assume the gains are targeted to the IRCA applicants alone, the coefficient implies an increase of USD 182 per IRCA applicant per year. According to survey data on the IRCA migrants taken from the Legalized Population Survey (LPS), IRCA migrants came from households with an average size of 3.8 people; assuming, therefore, that the gains were targeted to IRCA households, USD 182 per household implies a gain of USD 48 per family member.

In Panel B of Table 1, we estimate the same parameters but using a measure of treatment intensity as specified in equation 2. As shown in Column 1, we obtain a positive and precisely

14. One example is the Reagan administration's 1986 tax reform which, among other things, lowered federal income taxes. State-year fixed effects will capture any changes in state tax revenue as a result of this reform which might potentially confound a governor's decision to allocate revenue to IRCA-affected counties. Another example is SLIAG funding: federal funds to help states cover the costs associated with the IRCA. Appendix D provides additional details as to why these reforms and programs do not confound our results.

15. The issue of balance is discussed briefly in this subsection and in more detail in Online Appendix C.2.

estimated coefficient on the cumulative number of IRCA migrants per capita. To ease interpretation, we run, in Column 1 of Table C.1 in the Online Appendix, the baseline after standardizing the variables used in the regression. Doing so suggests that a one standard deviation in IRCA intensity leads to a 0.012 standard deviation increase in per capita revenues. Because treatment intensity is a more precise measure of treatment, we will, henceforth, use the cumulative number of per 1,000 capita IRCA applicants fixed to its 1992 level as our main explanatory variable.

Sensitivity Analysis: To ensure that our results are not driven by confounding factors, we undertake a number of sensitivity checks in Columns 2 to 5 of Table 1. We begin by re-estimating the parameters of the model in a sample that omits the four most treated states which, in per capita terms, correspond to California, Texas, Illinois and New York. As shown in Column 2, the results hold. To alleviate concerns that the results are driven by very populous cities or counties—some of which may serve as sanctuary cities—we rerun the regression, in Column 3, on a sample that omits counties in the top 10 percentiles of the population distribution and obtain a precise coefficient, suggesting that the effect is not driven by very populated counties.

In Column 4, we rerun the baseline specification, adding to it county specific linear time trends. The idea is to capture any pre-existing, differential trends with respect to the outcome variable, trends which might render our identifying assumption implausible. This is a demanding specification and the fact that the model returns a coefficient comparable to that of the baseline further enhances the credibility of our results.

Finally, in Column 5, we test the main supposition of the paper: that legal status, as distinct from enfranchisement, drives the distributional response of state governors. To this end, we include in the baseline model an interaction of treatment with a binary variable that indicates whether $t > 1992$, the first year when IRCA migrants gained eligibility for naturalization, and hence, the right to vote.[16] As shown, there is no significant increase to the baseline post-1992, consistent with the patterns shown in the raw data in Figure 1. Of course, this does not rule out enfranchisement as a channel. Governors, and their parties, for example, could have anticipated the eventual enfranchisement of the legalized and begun to allocate resources accordingly. However, as mentioned in Section 3, very few of the IRCA migrants were enfranchised by 2000. Moreover, the magnitude and precision of the coefficients in Column 5 strongly suggest that legal status, as distinct from naturalization and enfranchisement, is a driving force in the distribution of state transfers.

Further Robustness: In Online Appendix C, we discuss four sets of robustness checks to further increase confidence in the reliability of our results. First, in Appendix C.1 we probe the strength, nature and timing of the baseline effect by including a long-difference estimation and using a county's 1980 population to carry out the per capita calculation of transfers and

16. In fact, the overwhelming majority of those who did naturalize did so after 1994. However, repeating this exercise and testing for a differential post-1994 produces the same patterns.

IRCA applicants so as to further ensure population changes are not driving our results. We also investigate whether the relationship between legalization and transfers is linear or quadratic and find evidence for a small concave relationship between the two. Finally, we estimate the parameters of our baseline model on a sample that excludes the four border states to ensure that our results are not confounded by the IRCA's border security funding provision and, as shown, no such confound exists.

In Appendix C.2 we address the issue of balance between treated and control counties. IRCA migrants are not randomly distributed in the US and there are level differences in demographic and economic characteristics between treated and control counties. However, in Appendix C.2 we show that there is little evidence of differential trends in socio-economic characteristics between treated and control counties.[17] Moreover, we show that our baseline is robust to estimation via propensity score matching where we identify a more comparable control group and to the inclusion of a range of additional controls. Finally, we run the baseline omitting all controls and generate results similar to the baseline. That our model returns similar results when including baseline controls, additional demographic controls and no controls, as well as when using a more tightly identified control group, strongly suggests that covariate (im)balance is not a driving force in the distribution of intergovernmental revenue.

In Appendix C.3, we estimate the baseline without linearly interpolating the revenue data so as to demonstrate that our results are not affected by the linear interpolation. We also rerun the analysis restricting the sample to only those counties whose revenue information is surveyed in every year and find that our results are unaffected.

Finally, in Appendix C.4, we carry out a number of checks concerning the IRCA data. These include discussing our baseline approach to imputing the unassigned migrant shares in small counties, replicating our baseline using two different imputation methods and controlling for the time-varying effect of small counties with unassigned migrant shares. Moreover, in this Online Appendix we corroborate our baseline findings using an alternative measure for the legalization data. Specifically, we use legalized migrants per capita as opposed to applicants per capita and find virutally identical results.

That the coefficient on legalization remains positive, precise and stable across all specifications and sub-samples explored in Appendix C indicates that the relationship between immigrant legalization and the distribution of state aid is in fact a robust one.

5.3 Population Considerations

One may wonder whether our results are simply explained by a mechanical effect of having more people in the population eligible for social programs. We rule out this possibility for three reasons.

17. This is because undocumented migrants were (and still are) included in population estimates, are eligible (especially their children) for basic public services such as health and education and, to some extent, pay tax as some undocumented migrants obtain illegal social security documents.

First, while the IRCA legalized approximately 3 million people, it did not lead to a corresponding increase in the population. This is because estimates of the undocumented population are obtained from a residual of two other population measures: (1) the total foreign-born population (obtained through the Census) and (2) the legally resident population (known by the INS). The undocumented population estimate is the residual when (2) is subtracted from (1); hence population estimates undertaken by the Census Bureau are inclusive of undocumented migrants (Baker and Rytina 2013). This fact is made evident in Figure C.2 which shows population growth in treated and untreated counties. As illustrated, neither type of county experienced appreciable changes in population in the years before or after the passage of the IRCA. Accordingly, even if funds were transferred by formula on the basis of a county's population, the fact that there is no population growth associated with the IRCA alleviates our concern that mechanical population forces drive our results.

Second, a feature of the IRCA was that it "barred" the newly legalized "from participation in programs of financial assistance furnished under federal law on the basis of financial need for a period of five years from the effective date of each alien's lawful temporary resident status" (DHHS, December 1991). Moreover, given the demographic characteristics of the newly legalized discussed previously and that the children of undocumented migrants were already eligible for public services such as schooling pre-IRCA, we find it unlikely that our results are explained by mechanical increases as a result of social assistance eligibility criteria being satisfied at the state level.[18] This is also in line with studies that find undocumented migrants to be net economic contributors to the American economy (Borjas 2017; Gee, Gardener, and Wiehe 2017).

Third, the dependent variable used throughout our study is a measure of *per capita* transfers from state to local governments. If the policy was simply associated with a mechanical, formula based increase in transfers, we might expect the overall *level* of transfers to increase as a function of IRCA migrants in a given county but there would be no reason, ex-ante, to expect that the amount of *per capita* transfers would be affected. That per capita transfers respond to the number of IRCA applicants in a county suggests that our results reflect more than a mechanical increase that might arise out of a transfer formula based on population considerations.

5.4 Legalized and Already Legal Hispanics

In Table 2, we investigate the relationship between the IRCA migrants and the already legal Hispanic population in driving targeted resource allocation. If, as we posit, IRCA applicants attract resources on account of their ability to mobilize already legal Hispanic migrants, then we would expect the IRCA to have heterogeneous effects according to the size of the already legal Hispanic community. We thus calculate the share of the Hispanic community that is already legal and construct an indicator that is one if a county's share of already legal Hispanic migrants

18. Moreover, in Table 2 and Figure 3, we demonstrate, respectively, that the IRCA has no predictive power on state or local welfare expenditure.

in 1980 is greater or less than the median.[19] In Column 1 of Table 2, we interact this indicator with our measure of IRCA applications and, in line with our thinking, we find that the effect of the IRCA on transfers is driven by those counties with larger shares of already legal Hispanic migrants. The coefficient on the interaction, whilst estimated with somewhat less precision, more than doubles when compared to the baseline, underscoring the capacity of IRCA migrants to work through existing Hispanic networks to attract resources.

5.5 Funding of Transfers

The stable unit treatment value assumption (SUTVA) maintains that the potential outcome of a unit of observation is unaffected by the treatment status of other units. In this particular context, therefore, a question arises as to whether counties affected by the IRCA receive their transfers at the expense of those counties not affected by the law or whether these funds come from other sources. To better understand the nature of the treatment effect, and to understand whether SUTVA holds in this particular setting, we undertake two exercises in Table 2.

First, we run the baseline specification using variation in the number of IRCA applicants only in those counties that received applications for legal status (i.e. only the treated counties). The intuition is that if the result is reflective of a distributive politics channel where the governor takes from control counties in order to give to treated counties, we should see little to no effect when we remove control counties from the analysis. In line with this thinking, the coefficient in Column 2 is of similar magnitude to the baseline but is imprecisely estimated, suggesting that control counties do play a role in funding the targeted resource allocation observed in the data.

To further investigate where the additional resources come from, we obtain state revenue data from the U.S. Census Bureau, Annual Survey of State Government Finances and Census of Governments yearly series and regress various measures of state revenue on the number of per 1,000 capita IRCA migrants at the state level. The results are presented in Columns 3 to 5 of Table 2. The result in Column 3 suggests that the IRCA did not lead to an overall increase in government revenue at the state level, consistent with the view that the targeted transfers to IRCA counties come from shuffling intergovernmental revenue from one set of counties to another.[20] However, as shown in Column 4, the transfers appear to be offset by significant increases in property tax revenue at the state level generated as a result of the IRCA. Importantly, just as states use intergovernmental revenue to provide budgetary support to counties, states also receive general revenue support from the federal government in the form of federal intergovernmental revenue. This support includes, among other things, Federal grants and aid and reimbursements

19. Specifically, we calculate the share of already legal Hispanics in a county by deducting the total number of IRCA applicants in that county from its 1980 Hispanic population and divide this difference by the total Hispanic population in 1980. We then create an indicator that is 1 or zero, depending on whether a county's 1980 share of already legal Hispanics is greater or less than the median.

20. This finding is also consistent with Cascio and Lewis (2019) who find that legalization through the IRCA provided little additional state personal income tax revenue in California. We also find no effects of the IRCA on tax revenue at the state level (results not reported).

for state activities. In Column 5 of Table 2, we see that the IRCA does not lead to more federal intergovernmental revenue in the state budget, ruling out the possibility that the transfers observed in the data are merely reflective of greater financial support received by states from the federal government. Finally, in Column 6 we regress per capita welfare expenditure at the state level on the number of IRCA migrants per state capita. As shown, there is no welfare effect associated with the IRCA, strongly suggesting (along with Figure 3 discussed in Section 7) that our results are not reflective of mechanical increases in welfare spending.[21]

6. Discretionary Political Transfers

In this section, we investigate to what extent the relationship between immigrant legalization and the distribution of state finances is reflective of discretionary, political choices made by state governors as opposed to mechanical or bureaucratic forces outside their control or social welfare considerations that oblige them to better serve the areas where the documented migrants reside. To distinguish between these competing explanations, we turn our attention to the political constraints and incentives of the state governor. The contention is simple. If the increases in per capita transfers associated with the IRCA are the result of mechanical or social welfare forces, the results ought to be entirely insensitive to political constraints and incentives of the state governor. If, however, the transfers are the result of discretionary choices made by governors in an effort to bolster political support, then it is not unreasonable to expect state aid to display heterogeneous effects according to political context. Table 3 presents the results when we test for heterogeneity across a range of political contexts which we describe in turn.

Political Party Heterogeneity: We begin by investigating heterogeneity to the party affiliation of the governor. Column 1 of Table 3 indicates that the per capita transfers a county receives in response to the IRCA are positive and significant when the governor is a Democrat and that this amount decreases almost entirely when the governor is a Republican, confirming that distributional impact of the IRCA is responsive to political factors at the state level.

Term Limits and Election Cycles: Next, because our data includes the names of state governors, we are able to compare state-to-county transfers under a single governor over time as he or she faces different political constraints and election cycles. By way of example, we consider the transfers in just one state, Georgia, over the political career of one of its governors,

21. This result also provides some evidence that the employer sanctions provision of the IRCA does not adversely affect our results. In general, it seems unlikely that the employer sanctions provision would have any effect on transfers because (a) there were significant issues related to enforcement and compliance (Vernez (1989), for example, explains that, at least initially, just "one out of two employers were fully or partially complying" to this provision) and (b) the number of investigations and warnings, and fines resulting from those investigations and warnings actually *declined* after the IRCA. This is because of lack of funding to properly enforce this provision (see https://www.migrationpolicy.org/print/4668 for full details). In any case, if anything, we would expect this provision to lead to firms to hire less undocumented migrants, thereby increasing their demands for welfare. That welfare expenses do not increase at the state level, suggests that this was not, in fact, an overwhelming concern.

Zell Miller (D), who served two terms in office: from 1990 to 1994 and from 1994 to 1998. Georgia is one of the more than 30 states that has a two-term limit constraint on its executive. Therefore Zell Miller was eligible for re-election in his first term but he was a lame duck in his second. Georgia comprises 159 counties of which we have data for 137: eight of these had IRCA applicants whilst 129 did not. Figure 2 shows the trends in transfers during Zell Miller's tenure as Governor. As shown, the counties unaffected by the IRCA experienced a steady decline in the amount of per capita transfers received. The eight counties that had IRCA applicants, by contrast, exhibit a great deal of variation. In Governor Miller's first term, transfers to these counties increased only to drop off drastically in his second term—and in particular in the final two years of his final term—when he is no longer eligible for re-election. The question that arises, therefore, is to what extent intergovernmental revenue differs as governors face term limits and how much of this difference is driven by the IRCA. Similarly, one wonders to what degree state aid fluctuates in the face of gubernatorial election cycles.

Columns 2 and 3 in Table 3 investigate these questions. In Column 2, we interact the number of per capita IRCA applications with a binary variable that is one when the governor is a lame-duck in the final two years of his or her term and the result indicates that governors with no electoral incentive allocate significantly less to IRCA-affected counties. In Column 3, we analyze sensitivity to the gubernatorial election cycle. Here, the IRCA variable is interacted with an indicator that is 1 if a governor is in an election year and zero otherwise and an indicator for whether or not a governor is eligible for reelection. We lag the outcome variable by one year to better understand the dynamics of intergovernmental revenue in the year prior to an election. The result suggests something of a political budget cycle: counties affected by the IRCA receive significantly more IGR when their governor, who is eligible for reelection, faces an election.

State Legislatures: Although governors do enjoy increasing power over the budget, state legislatures naturally play an important role in a state's financial affairs. Accordingly, we test, in Columns 4 and 5 of Table 3, the responsiveness of transfers to (a) the partisan composition of state legislatures and (b) the partisan relationship between the state governor and the state legislature. In Column 4, we interact the IRCA variable with a dummy that indicates whether both Houses of the state legislature enjoy a Republican majority (0) or a Democratic majority (1) and, as shown, the partisan composition of the state legislature plays no significant role in the distribution of state finances to counties affected by the IRCA. This is in contrast to the result in Column 1, which demonstrates a clear partisan pattern at the gubernatorial level. Finally, in Column 5, we generate an indicator that is one when the party of the governor is aligned with the partisan majority of the state legislature and zero otherwise and we interact this variable with our measure of IRCA applicants in a given county. The result indicates that, compared to the baseline, the effect of the IRCA on transfers increases by around 55 percent when there is partisan alignment between the executive and legislative branches of state government, further underscoring the politically discretionary nature of these transfers.

Together, the results in this section point to two noteworthy conclusions: First, the heterogeneities uncovered make clear that the incentives that prompt the differential allocation of resources as a result of immigrant legalization are primarily political in nature. Second, we have shown that a state's executive branch is more responsive to the incentives created by amnesty than its legislative branch.

7. Local Educational Expenditure and Hispanic Educational Outcomes

Next, we turn our attention from county revenue to county expenditure in an effort to understand in which areas and on which constituents county revenue is spent. To do so, we plot, in Figure 3, regression coefficients on the interaction between IRCA application intensity in 1992 and year dummies when various categories of per capita local expenditure are used as the outcome. As expected, the IRCA led to significant increases in local education expenditure from the early 1990s and beyond. By contrast, there is little to no increase in local welfare expenditure or on other major categories of local expenditure including health, highways and roads.

To understand whether these educational expenditures led to differential outcomes, we calculate race-specific high school completion rates and test whether the counties that were affected by the IRCA also experienced improvements in Hispanic high school completion. To carry out this exercise, we obtain data from the 2010 Decennial Census in order to estimate the impact of the IRCA on an individual's educational outcomes. Rather than compare individuals in treated and non-treated counties before and after the passage of the IRCA, we now compare individuals in treated and non-treated counties in cohorts that entered middle school before the passage of the IRCA (and hence were less likely to benefit from additional educational expenditure) with those in cohorts that entered middle school after the IRCA passed (and hence were more likely to benefit from additional funds). Accordingly, we construct 20 middle school entry cohorts from 1980 to 1999. An individual in the 2010 census is placed in a middle school entry cohort depending on which year he or she was 12 years of age. The specification is detailed in equation 3, where $H_{i,c,mse}$ is an indicator if individual i in county c and in middle school entry cohort mse has 12 years of education or more. County and middle school entry cohort fixed effects are captured by δ_c and ψ_{mse}, respectively and D^j_{mse} is an indicator that is one when $j = mse$ and zero otherwise $\forall \;\; j \neq 7$. All other terms are defined as before.

$$H_{i,c,mse} = \delta_c + \psi_{mse} + \sum_{j=1}^{20} \beta_j \left[IRCA_{c,1992} \times D^j_{mse} \right] + \epsilon_{c,mse} \tag{3}$$

We run the specification on different samples according to a person's race and plot the corresponding coefficients, β_j, as shown in Figure 4. This coefficient estimates the change in the slope of high school completion between individuals in high and low treatment intensity counties across various middle school entry cohorts. The estimates obtained using the Hispanic

sample clearly indicate that for Hispanic persons, residing in a county affected by the IRCA led to a positive and significant impact on that person's likelihood of completing high school, provided they entered middle school in 1994 or later. Indeed, there is no distinguishable difference in the likelihood of completing high school between individuals in high-treated and low-treated counties if they began middle school prior to this time. This fact suggests that the increased high school completion rates among Hispanics arises not just from legal status but from the additional resources for education that these counties receive on account of that status.[22] For Caucasian and Asian youth, by contrast, residing in an IRCA-affected county has no distinguishable impact on high school completion probability, regardless of when they entered middle school. There are some effects for African American youth but the magnitudes are much smaller and the patterns are not nearly as clear as they are for Hispanic youth. These results suggest that our results are not just politically motivated but targeted to Hispanics in communities affected by the IRCA. They also underscore the role of local public spending in linking legal status to improvements in various socio-economic outcomes, in this particular case education.

8. Political Economy Mechanisms: Hispanic Political Empowerment

We argue that the differential allocation of transfers to counties affected by the IRCA was motivated not so much to win the political support of the newly legalized migrants—many of whom earned the right to vote in the mid to late 1990s—but rather that of entire Hispanic communities that were politically mobilized as a result of the IRCA. In this section, we present two pieces of evidence to support our claim: the effect of legal status on Hispanic political representation and its effect on political participation in communities of mixed legal status.

8.1 Hispanic Political Representation

The claim being advanced is that the legalization of a group of some 3 million Hispanic migrants has far reaching social and political consequences, not just for the migrants themselves but for the on entire communities in which they reside. Legalization brings migrants "out of the shadows", as it were, and enables migrants and their networks of families and friends to participate more fully in the affairs of society, one expression of which is greater political participation. State governments, for their part, are sensitive to these changes and adjust their budgetary allocations in response. To test this assertion, we digitize a novel source of data that contains information on more than 60,000 Hispanics elected to public office from 1984 to 2000 in order to examine whether the IRCA led to a stronger representation of Hispanic interests in public office.

We begin by flexibly estimating the effect of the IRCA on the number of Hispanics elected to public office and plot the coefficient of interest, for various levels of office, in Figure 5. The

22. This becomes all the more plausible when one considers that IGR earmarked for education includes things like vocational training, school transportation and school health services.

figure reveals a number of noteworthy results. First, the trends in the number of Hispanics entering public office display no distinguishable difference in the counties affected by the IRCA as compared to those unaffected in the two periods prior to the passage of the law, alleviating concerns of pre-trends. Second, the number of Hispanics entering office increases at all levels, but the magnitudes are largest for those officials elected to local branches of government. Interestingly, the effects on state and federal officials becomes larger later in time, suggesting that local offices represent the first rung on the political ladder for many aspirants of higher political office. Third, the timing of the effect sheds light on the main mechanism that drives the effect. In this respect, counties affected by the IRCA do experience significant increases in the number of Hispanics entering public office—especially in higher offices—in the late 1990s, suggesting that enfranchisement via naturalization may have played a role in Hispanic selection to office. However, the effects—especially for local offices—are discernible in time periods (a) prior to enfranchisement or (b) when very few of the legalized had actually gained voting rights. This underscores the capacity of legal status to politically mobilize others in their communities. Finally, in the two bottom right panels of the figure, we decompose the results further by examining the effect of legalization on locally elected officials, and in particular for mayors and school board officials, two of the most common positions at the local level. The results indicate that legalization has a strong, positive effect on the number of Hispanics school boards officials and mayors entering office. Together, the results point to the role that immigrant legalization plays in stimulating meaningful political activity at the local level.

Sensitivity Analysis: In Table 4, we present our results when we regress the total number of Hispanics elected to public office in a given county-year on per capita IRCA applicants. Column 1 is the baseline. It suggests that counties affected by the IRCA experience an increase of 0.53 Hispanics entering public office after 1986, a 45 percent increase on the sample mean. In Column 2, we omit the four states with the highest per capita IRCA migrants, which, as explained earlier, correspond to California, Texas, Illinois and New York while in Column 3, we restrict the sample to counties with populations less than those in the top 10 percentiles of population distribution to again ensure that the results are not driven by a handful of large, migrant-friendly localities. The magnitudes of these two coefficients does decrease by some 50 percent, suggesting that the IRCA's impact on Hispanic political selection is strongest in states with many IRCA migrants and in large counties. However, the coefficients remain positive and significant at conventional levels, implying that effect of the IRCA on Hispanic political representation is still present even in small counties and in states with fewer IRCA applicants. In Column 4, we add county-specific linear time trends so as to identify the effect of the IRCA on the number of Hispanics elected to public office independently from potentially differential pre-trends. That the coefficient on per capita IRCA migrants remains stable and precisely estimated across these specifications increases confidence in the strength of the relationship between immigrant legalization and Hispanic political selection.

As a final exercise, in Column 5 we investigate to what extent legal status, as distinct from enfranchisement, drives Hispanic selection to public office. To this end, we include in the baseline model an interaction of treatment with P_{92}, a binary variable that indicates time periods before and after 1992, the first year IRCA migrants could, theoretically, gain the right to vote. As shown, the baseline effect is estimated with precision but there is no differential post-1992 effect, pointing to the distinctive role that legal status plays, as opposed to enfranchisement, in mobilizing Hispanic political interests.

Political Mobility: In Online Appendix E, we provide an additional piece of evidence linking immigrant legalization to Hispanic political representation. Specifically, we test for a *mobility* effect. That is, we test whether counties with many IRCA migrants not only elect more Hispanics to office but also elect candidates who tend to move higher up on the political ladder. Our results suggest that the IRCA does, in fact, lead to a significantly higher number of politically mobile officers being elected to office, reinforcing the idea that legalization was significant to Hispanic political representation and mobilization.

8.2 Political Participation

Electing Hispanics to office and motivating state officials to allocate resources as a result requires participation. In this subsection, we demonstrate that the IRCA led to significantly higher political participation without stoking strong, anti-migrant sentiment. We first study county level voter turnout and then turn to individual survey data.

County Level Turnout: To test the electoral relevance of the IRCA, we regress county level voter turnout in Presidential elections on the cumulative number of per 1,000 IRCA applicants. The results are shown in Columns 1 and 2 of Table 5 and indicate that, on its own, the IRCA in fact leads to a decrease in political participation. Importantly, however, this negative effect is wiped away and made positive in those counties with larger shares of already legal Hispanic migrants in them. Specifically, we interact the IRCA variable with an indicator that is one is the share of already legal Hispanic migrants in a given county in 1980 is above median and zero if not.[23] In Column 2, to help ease interpretation, we standardize the variables and we see that, in counties with large shares of already legal Hispanic migrants, a one standard deviation increase in the number of IRCA applicants increases turnout by around .018 standard deviations in those communities with large shares of already legal Hispanic migrants in them. That the interaction term in these regressions is positive and precise supports the idea that the IRCA was indeed a significant factor in the political empowerment of Hispanic communities of mixed legal status.

Anti-Migrant Voting: We argue that the IRCA led to significantly higher rates of political

23. See footnote 19 for details on how this indicator is constructed.

participation without stoking ant-migrant sentiment for two reasons. First, the IRCA did not lead to an influx of new migrants, as explained in subsection 5.3. Second, as shown in subsection 5.5, the transfers made to IRCA counties are offset by significant increases in state revenues generated as a result of property taxes. As a result, the IRCA does not appear to have generated serious economic "losers". Nevertheless, to test this proposition more systematically, we exploit individual survey data from the American National Election Studies (ANES) to examine whether individuals with anti-migrant sentiments residing in IRCA-affected counties exhibit differential voting behavior to those without such sentiments.[24] As shown in Columns 3 and 4 of Table 5, the IRCA prompted no differential voting behavior among individuals with negative feelings towards migrants, confirming our supposition that the IRCA led to increased political participation without triggering anti-immigrant voices. In Online Appendix F, we provide additional evidence, exploiting variation in vote shares from Proposition 187, an anti-migrant ballot initiative in California, that governors' allocation decisions were not intended to cater to anti-migrant sentiment.

9. Conclusion

Undocumented migration is a hotly contested issue in the United States where the number of such migrants has nearly quadrupled in the past thirty years. Although several studies have found a positive effect of legal status on an individual migrant's social and economic outcomes, the distributional impact on public resources of legalizing such a large number of undocumented migrants remains an open and highly salient question. This study answers this question by exploiting variation in legal status generated from the one, and to date only, amnesty experience in the history of the United States. We found that state governments allocate more resources per capita to counties affected by amnesty than counties unaffected by it. This central finding points to five main lessons which can inform current debates on immigration reform.

First, the transfers afforded to IRCA-affected counties come as a result of inter-county shuffling of resources and are offset by significant increases in state revenue generated from new, legalized migrants participating in the economy, and in particular in real estate markets. We found no evidence to suggest that the transfers are reflective of increased financial aid received by states from the federal government on account of the IRCA, nor did we find evidence of increased welfare expenditure as a result of the IRCA. This is in line with other research that highlights the role of migrants, documented or not, as net contributors to the economy.

24. To identify individuals with negative attitudes towards undocumented migrants, we exploit the "illegal alien" thermometer of the ANES. This question asks people to gauge their feelings towards undocumented migrants on a scale between 0 (very cold) and 100 (very warm). However, this measure is only available as of 1988, two years after the IRCA was implemented. We therefore use the African-American thermometer, scores for which are available since 1980, as a proxy for the illegal alien thermometer. Figure F.1 plots the residual values of these two thermometer scores once county and year fixed effects and a range of individual and county characteristics have been accounted for. As shown, the two measures follow each other rather closely, indicating that the choice of proxy is valid. We code individuals with thermometer scores less than 50 as "anti-migrant" and those with scores greater than 50 as "pro-migrant".

Second, we demonstrated that the distributional effect uncovered is politically motivated. That is, governors transfer more resources to IRCA-affected counties when they are eligible for re-election, when they are politically aligned with the state legislature and when they are Democrats. These results are especially noteworthy because they demonstrate that the relationship between legal status and the distribution of public resources is not borne of welfare considerations or mechanical forces outside the control of the state governor.

Third, we found that county expenditure in education increases significantly in IRCA-affected counties, leading to improvements in high school completion rates for Hispanic youth, but not for youth of other ethnicities, highlighting the targeted nature of our results. Importantly, these improvements were only discernible as of 1994, long after legal status was granted but soon after these counties experienced increases in the amount of public investment in education and in the number of Hispanic individuals entering public office. This finding points to the important role that political representation and local public expenditure play in linking legal status to improvements in socio-economic outcomes of individual migrants.

Fourth, we tried to understand *why* legal status affects the distribution of public finances in the way that it does. In doing so, we uncovered novel political economy forces at work. Specifically, we found evidence to suggest that governors targeted resources to IRCA affected counties not so much to win the votes of the newly legalized, relatively few of whom would go on to naturalize, but rather to capture political gains arising from entire communities that were politically transformed as a result of a landmark legalization policy. In this respect, we found, that the capacity of legal status to attract transfers from the state occurs prior to 1992, the first year where IRCA migrants gained eligibility to vote, indicating that the legalization of immigrants, as distinct from their enfranchisement, is a driving force in the distribution public resources. We also found that the baseline effect of the IRCA on state transfers is driven primarily by counties with large, pre-existing shares of already legal Hispanic migrants, pointing to the role of the IRCA in mobilizing the existing Hispanic community to attract resources. Moreover, we digitize a novel source of data that contains information on the universe of more than 60,000 Hispanic individuals elected to public office in order examine the extent to which legalization leads to a stronger representation of Hispanics interests in politics. Our findings are clear: counties affected by the IRCA experience significant increases in the number of Hispanics occupying public office at all levels, an effect which is most strongly felt at the local level, and in particular for mayors and school board officials. What is more, counties affected by the IRCA not only elect more Hispanics to office but tend to elect Hispanic officials who, over time, climb up the political ladder and assume positions of greater responsibility and influence. These findings are of particular importance when one considers inequality in political representation among ethnic minorities that characterizes much of US politics. They also shed new light on the ways in which immigrant legalization significantly influences the political participation and representation of entire Hispanic communities.

Finally, our analysis indicates that the IRCA led to significant increases in political

participation within Hispanic communities without triggering anti-immigrant sentiment. The IRCA, we found, leads to significantly greater voter turnout in Hispanic communities of mixed legal status, again pointing to the capacity of newly legalized migrants to contribute to the political mobilization and empowerment of existing Hispanic networks. By contrast, we found no increases in turnout among individuals with negative views towards undocumented migrants and little evidence to suggest that governors targeted resources towards them.

On the whole, our work points to a significant political economy dimension to immigrant legalization. To the extent that markets are social constructs, this paper has demonstrated that *who* participates in them matters. Offering legal status not only leads to social and economic improvements at the local level but also, by changing the very makeup of politics, provides politicians with strong electoral incentives to see that it does so.

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10. Figures

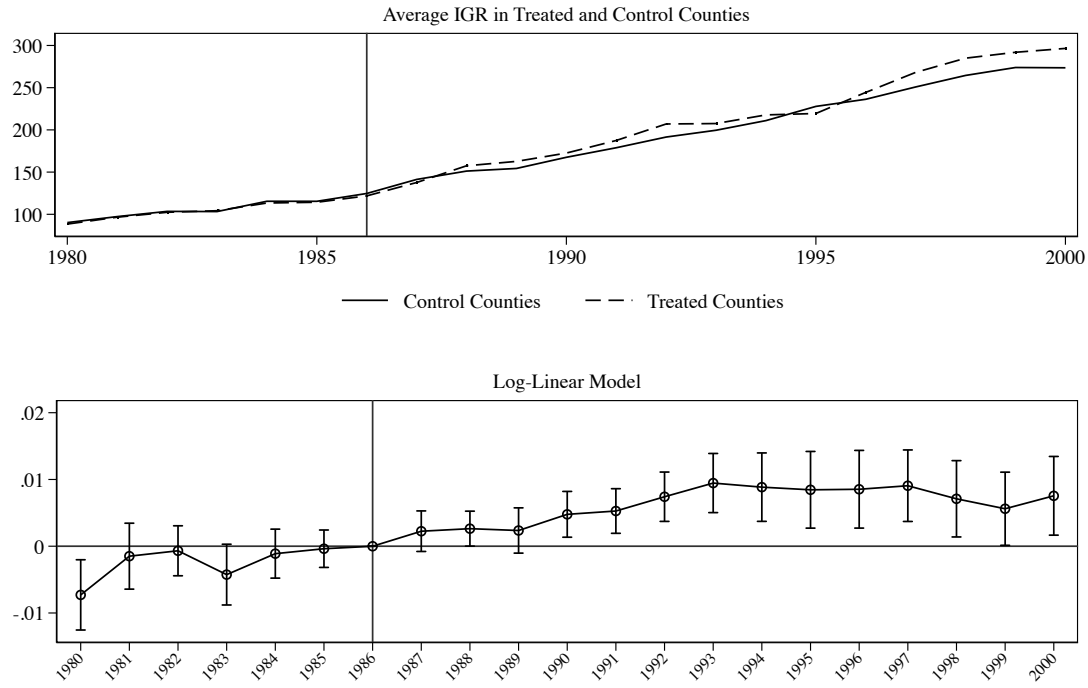


Figure 1
Parallel trends

Note: The top figure plots the mean value of per capita intergovernmental revenue (in USD1999) in treated and control counties. The bottom figure plots the regression coefficient on treatment intensity fixed to its 1992 level when it is interacted with year dummies as specified in equation 1. Standard errors are clustered at the county level and confidence intervals are drawn at 99 percent.

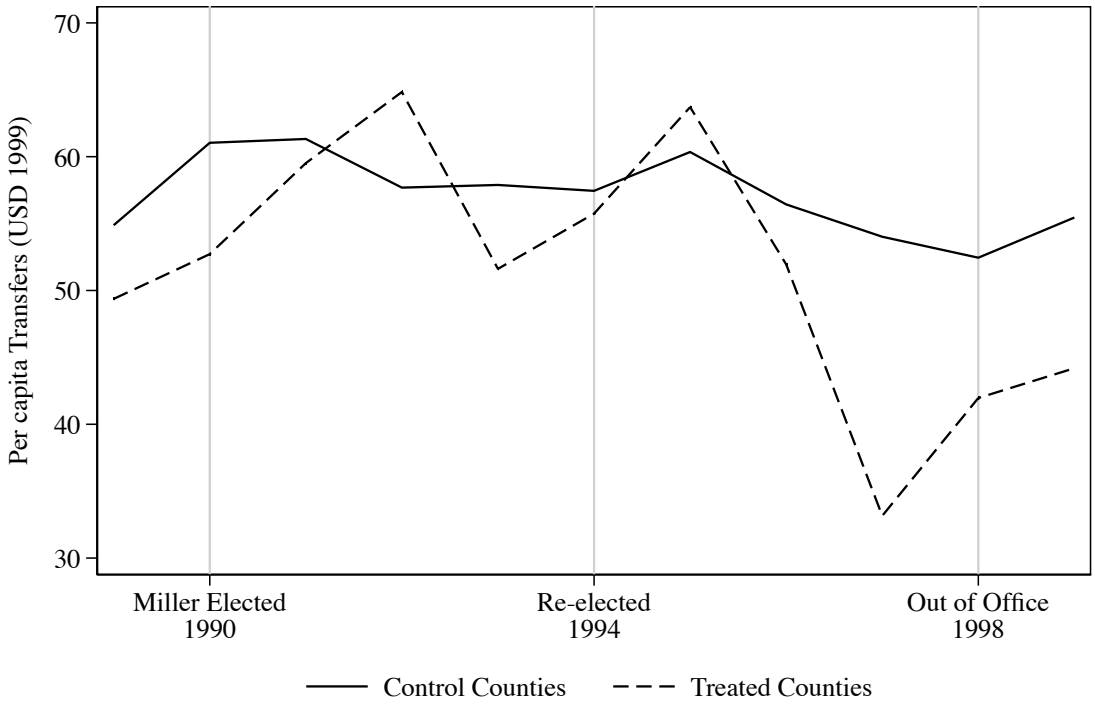


Figure 2
IGR in Georgia under Governor Zell Miller

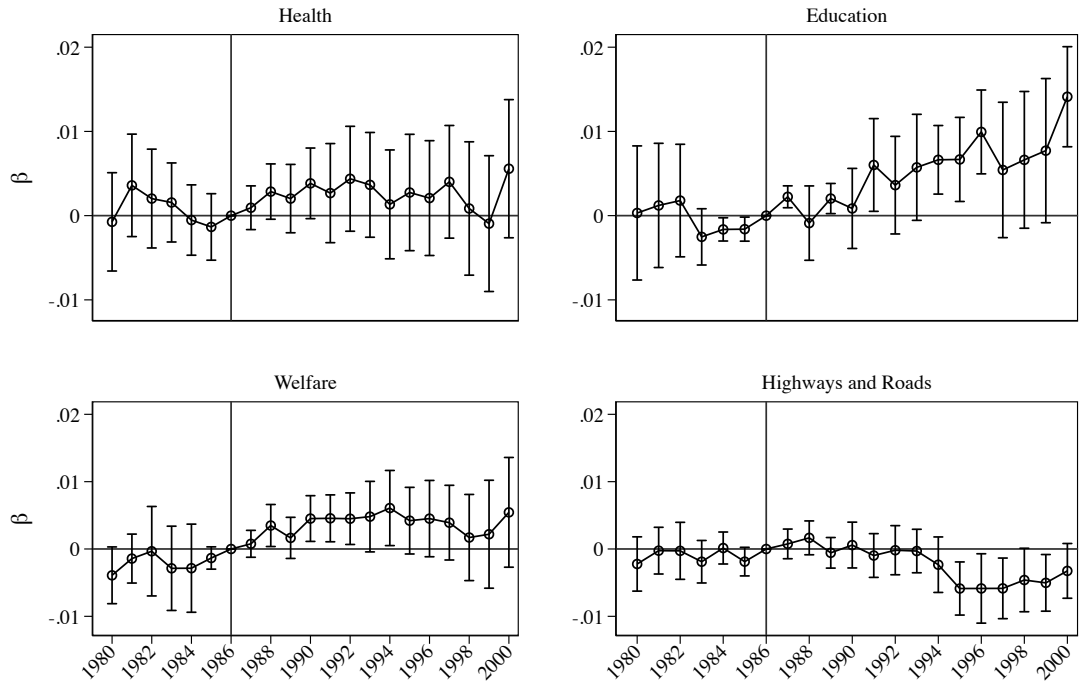


Figure 3
Event study estimates of local expenditure on legalization

Note: This graph plots the regression coefficient on the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level interacted with year dummies. The outcome variables are the log of per capita county expenditure in health, education, welfare and highways and roads. The regressions include county and year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 99 percent.

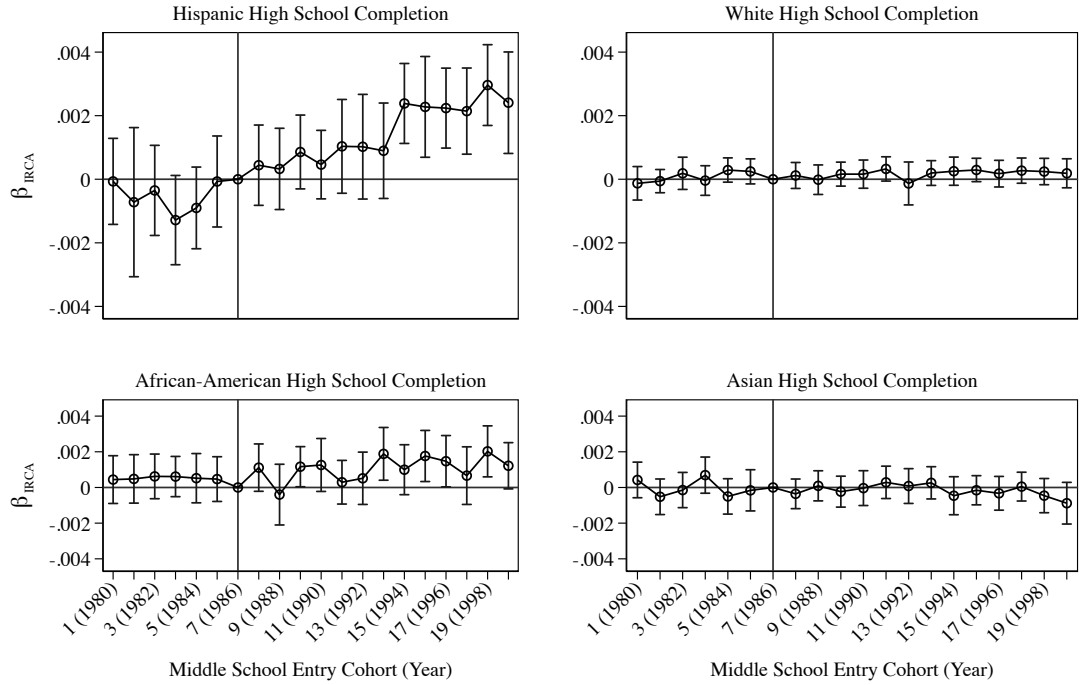


Figure 4
Event study estimates of high school completion on legalization

Note: This graph plots the regression coefficient on the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level interacted with middle school entry cohort dummies as shown in equation 3. The outcome variable, taken from the 2010 Decennial Census, is an indicator that is one if an individual in a given county and middle school entry cohort completed high school or more and zero otherwise. A person from the 2010 Census is placed in a middle school entry cohort depending on the year in which they were 12 years of age. The regressions include county and cohort fixed effects. The different panels plot, respectively, the coefficients when the sample is restricted to Hispanic, Caucasian, African American and Asian individuals. Standard errors are clustered at the county level and confidence intervals are drawn at 99 percent.

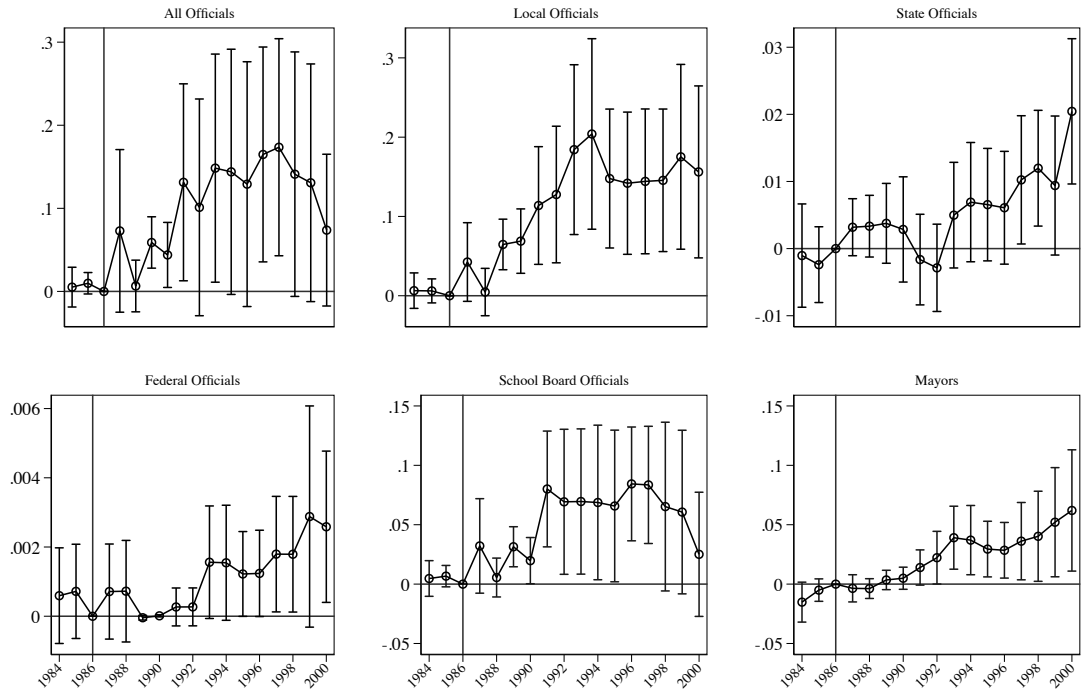


Figure 5
Legalization and Hispanic Public Officials

Note: This graph plots the regression coefficient on the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level interacted with year dummies. The outcome is the number of Hispanics in elected public office at a given level of office for a given county-year. The regressions include county and year fixed effects. Standard errors are clustered at the county level. Confidence intervals are drawn at 95 percent.

11. Tables

Table 1
Inter-Governmental Revenue on IRCA Legalizations

	Log of Inter-governmental Revenue (per capita)				
	(1) Baseline Effect	(2) Drop Top 4 IRCA States	(3) Pop ≤ 90^{th} Pctl.	(4) Linear Trends	(5) Differential P_{92} Effect
Panel A. Treatment Indicator					
Treated × P_{86}	0.0812***	0.109***	0.0568*	0.0535*	0.0653***
	(0.0240)	(0.0210)	(0.0334)	(0.0274)	(0.0220)
Treated × P_{92}					0.0312
					(0.0279)
Panel B. Treatment Intensity					
$IRCA_{92}$ × P_{86}	0.00319**	0.00551***	0.00402*	0.00275*	0.00260**
	(0.00145)	(0.00208)	(0.00234)	(0.00165)	(0.00126)
$IRCA_{92}$ × P_{92}					0.00109
					(0.00154)
Control Variables	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
$Hisp_{1980}$ × Year Dummies	Yes	Yes	Yes	Yes	Yes
County-Year Linear Trends	No	No	No	Yes	No
Observations	41,036	34,042	36,733	41,036	41,036
Number of Counties	2,204	1,834	2,014	2,204	2,204

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Panel A shows results when using a treatment indicator and Panel B shows results when using a measure of treatment intensity which is the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level. P_{86} and P_{92} are binary variables that indicate, respectively, time periods before and after 1986 and 1992. Control variables include county level poverty and unemployment rates, log of population, log of income and the share of the population that is white and Black. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2
The IRCA and the Funding of Transfers

	Log IGR		Log State Revenue			Log State Expenditure
	(1) Already Legal	(2) Treated Only	(3) Total	(4) Property Tax	(5) Federal IGR	(6) Welfare
$IRCA_{92} \times P_{86}$	0.000431	0.00324				
	(0.00177)	(0.00244)				
$IRCA_{92} \times P_{86} \times$ High Legal	0.00710*					
	(0.00409)					
State $IRCA_{92} \times P_{86}$			0.000646	0.0294**	0.00161	-0.00183
			(0.00108)	(0.0121)	(0.00189)	(0.00424)
County Controls	Yes	Yes	No	No	No	No
County Fixed Effects	Yes	Yes	No	No	No	No
State-Year Fixed Effects	Yes	Yes	No	No	No	No
$Hisp_{1980} \times$ Year Dummies	Yes	Yes	No	No	No	No
State Controls	No	No	Yes	Yes	Yes	Yes
State Fixed Effects	No	No	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	Yes	Yes	Yes	Yes
State $Hisp_{1980} \times$ Year Dummies	No	No	Yes	Yes	Yes	Yes
Observations	41,036	5,968	984	821	984	918
Number of Clusters	2,204	305	48	42	48	47

Notes: The dependent variable, in Columns 1 and 2, is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. In Columns 3 to 5, the dependent variable is log of per capita revenue received from various sources at the state level while the dependent variable in Column 6 is log of per capita welfare expenditure at the state level. $IRCA_{92}$ is the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level and State $IRCA_{92}$ is the same variable aggregated to the state level. P_{86} is a binary variable that indicates time periods before and after 1986. High Legal is an indicator that is 1 if the 1980 share of already legal Hispanic migrants in a county is greater than the median and zero otherwise. The regression in Column 1 includes all lower order interactions. In Column 2 we use only counties affected by the IRCA in the estimation. Control variables include poverty and unemployment rates, log of population, log of income and the share of the population that is white and Black, aggregated to the county level in Columns 1 and 2 and to the state level in Columns 3 to 6. Standard errors (shown in parentheses) are clustered at the county level in Columns 1 and 2 and at the state level in Columns 3 to 6. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3
Legalization and Political Heterogeneity

	Log of Inter-governmental Revenue (per capita)				
	(1) Gov. Rep.	(2) Lame Duck	(3) Election Cycle	(4) Party of Legislature	(5) Gov-Leg Alignment
$IRCA_{92} \times P_{86}$	0.00703***	0.00331**	0.00673**	0.00300**	0.000647
	(0.00196)	(0.00147)	(0.00269)	(0.00145)	(0.00130)
$IRCA_{92} \times P_{86} \times ...$	-0.00680***	-0.00523**	0.00519*	-0.00236	0.00673***
	(0.00178)	(0.00257)	(0.00300)	(0.00257)	(0.00195)
County Controls	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
$Hisp_{1980} \times$ Year Dummies	Yes	Yes	Yes	Yes	Yes
Observations	40,653	41,036	38,164	31,709	41,036
Number of Counties	2,204	2,204	2,200	2,096	2,204

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. $IRCA_{92}$ is the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level. P_{86} is a binary variable that indicates time periods before and after 1986. This baseline interaction is interacted with different indicator variables as labeled across Columns 1 to 5 and the coefficient of this triple interaction is reported in the second row. Governor Republican, for example, is an indicator that is 1 if the governor is a Republican and 0 if Democrat. Lame Duck is 1 if a governor is in his or her final two years of a lame duck term and 0 if not. Election Year is an indicator according to whether a governor is in an election year or not and the outcome variable is lagged by one year. The triple interaction in Column 4 is interacted with an indicator that is 1 when the governor is eligible for re-election and 0 if (s)he is a lame duck and the coefficient of this quadruple interaction is reported in the second row. Party of Legislature is a dummy when both houses of the state legislature have a Republican majority (0) or Democratic majority (1). Gov-Leg Alignment is 1 when the party of the governor is aligned with the partisan majority of the state legislature (regardless of party) and 0 when there is no such alignment. All regressions include all lower order interactions. Control variables include county level poverty and unemployment rates, log of population, log of income and the share of the population that is white and Black. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4
Hispanic Officials and the IRCA

	Outcome: Number of Hispanic Elected Officials				
	(1) Baseline Effect	(2) Drop Top 4 IRCA States	(3) Pop ≤ 90^{th} Pctl.	(4) Linear Trends	(5) Differential P_{92} Effect
Panel A. Treatment Indicator					
Treated × P_{86}	0.533**	0.212**	0.222	0.533***	0.421**
	(0.267)	(0.0860)	(0.138)	(0.186)	(0.179)
Treated × P_{92}					0.227
					(0.250)
Panel B. Treatment Intensity					
$IRCA_{92}$ × P_{86}	0.104*	0.0531**	0.0301*	0.0768*	0.0766**
	(0.0559)	(0.0261)	(0.0176)	(0.0392)	(0.0360)
$IRCA_{92}$ × P_{92}					0.0518
					(0.0487)
Control Variables	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
$Hisp_{1980}$ × Year Dummies	Yes	Yes	Yes	Yes	Yes
Observations	33,276	27,569	29,465	33,276	33,276
Number of Counties	2,222	1,847	2,005	2,222	2,222
Mean of Dependent Variable	1.19	.627	.799	1.19	1.19

Notes: The dependent variable is the number of Hispanic individuals in elected public office in a given county in a given year. Panel A shows results when using a treatment indicator and Panel B shows results when using a measure of treatment intensity which is the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level. P_{86} and P_{92} are binary variables that indicate, respectively, time periods before and after 1986 and 1992. Control variables include county level poverty and unemployment rates, log of population, log of income and the share of the population that is white and Black. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5
Legalization, Turnout and Anti-Migrant Voting

	County Data		Individual ANES Data	
	(1) Turnout	(2) Standardized Turnout	(3) Voted Pres.	(4) Voted Gov.
$IRCA_{92} \times P_{86}$	-0.000489***	-0.02773***	-0.00126	0.000349
	(0.000166)	(0.00941)	(0.00198)	(0.00157)
$IRCA_{92} \times P_{86} \times$ High Legal	0.000799**	0.04533**		
	(0.000386)	(0.0219)		
$IRCA_{92} \times P_{86} \times$ Anti-Black			0.00249	0.00106
			(0.00455)	(0.00244)
County controls	Yes	Yes	No	No
Individual Controls	No	No	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes
$Hisp_{1980} \times$ Year Dummies	Yes	Yes	Yes	Yes
Observations	12,027	12,027	4,105	4,484
Number of Counties	2,223	2,223	174	163

Notes: The dependent variable in Columns 1 and 2 is turnout in a given county in Presidential elections from 1980 to 2000. This data is obtained from the USA Counties Database. In Column 2, the outcome, the measure of treatment and the controls are all standardized. The dependent variables in Columns 3 and 4 are indicators for whether or not an individual voted in presidential or gubernatorial elections, respectively. These data are obtained from the ANES. $IRCA_{92}$ is the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level. P_{86} is a binary variable that indicates time periods before and after 1986. High Legal is an indicator that is 1 if the share of already legal Hispanic migrants in a county in 1980 is greater than the median and zero otherwise. Anti-Black is an indicator that is 1 if a person has negative feelings towards African-Americans which is used as a proxy for feelings towards undocumented migrants, as explained in the text. The regressions include all lower order interactions. County controls include poverty and unemployment rates, log of population, log of income and the share of the population that is white and Black. Individual controls include marital status, age, age^2, income, education and indicators for being white, Black or Hispanic as well as for being male or not. Included in the individual controls is the log of county population. Standard errors (in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Online Appendix for Paper:
Legal Status, Local Spending and Political Empowerment: The Distributional Consequences of the 1986 IRCA

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A. Background, Distribution and Naturalization of IRCA Migrants

In this appendix, we first provide details concerning the demographic background and geographic distribution of the IRCA migrants. We then present exact figures on naturalization rates among IRCA migrants.

A.1 Demographic background and geographic distribution of IRCA migrants

First, in Figure A.1, we 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. 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) (Mosbacher and Bryant 1991).[25] 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.

25. The National Longitudinal Survey of the U.S. Bureau of Labor Statistics suggests the poverty thresholds in 1989 were even lower: $5,980 for a single person and $12,100 for a family of four. Taken from http://bit.ly/2tGnz8V, accessed in August 2017.

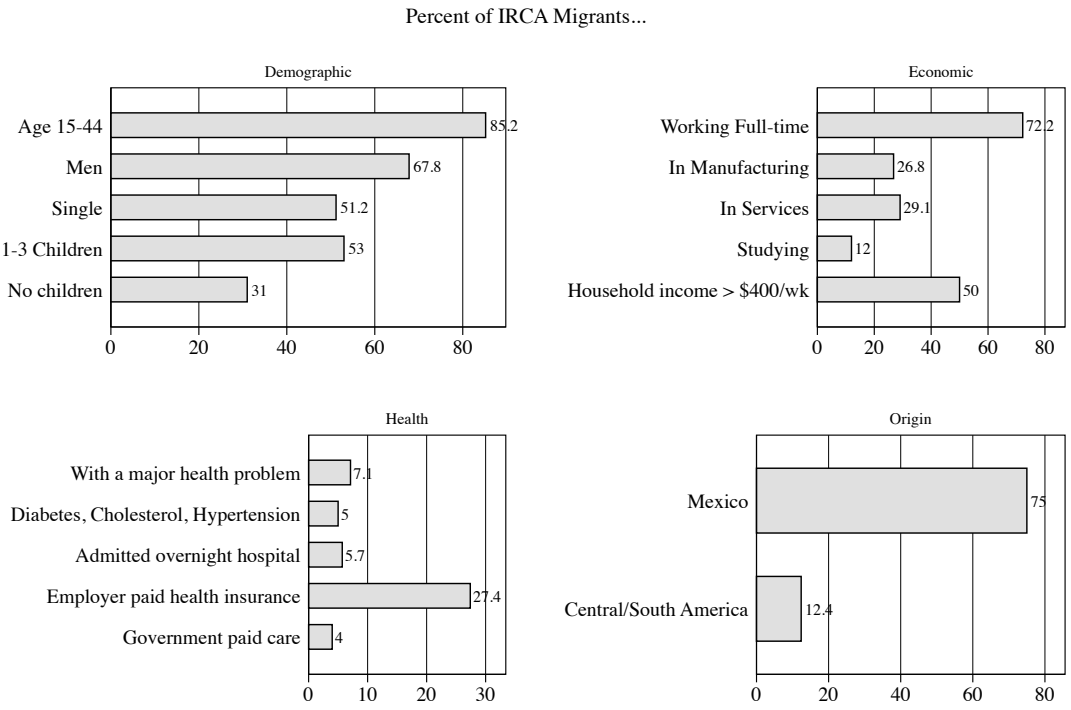


Figure A.1
Socio-economic characteristics of the IRCA applicants

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

Figures A.2 and A.3 provide further details on the distribution of the IRCA applicants. As shown, undocumented migrants applied for legal status from some 330 counties across the United States.[26]

26. We do not observe every county in every year because the intergovernmental revenue data, taken from the Annual Survey of Local Government Finances, is not surveyed in every county in every year. This issue is discussed in more detail in Section 4 and in Online Appendix C.3.

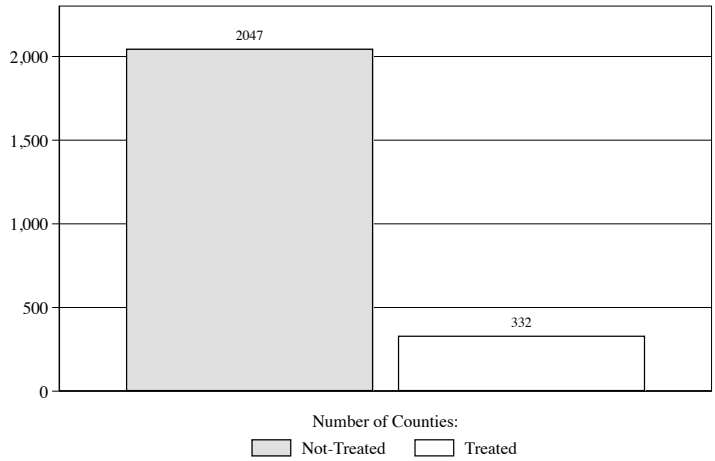


Figure A.2
Distribution of IRCA applicants as of 1992

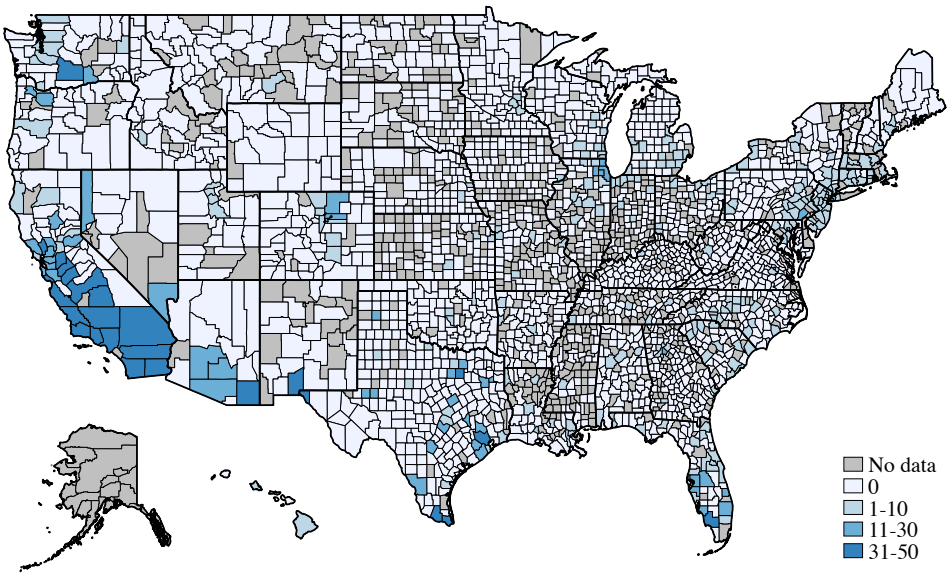


Figure A.3
Number of legalized migrants per 1,000 county inhabitants in 1992

A.2 Naturalization of IRCA migrants

In Table A.1 we present figures from the United States Immigration and Naturalization Service (INS) concerning total naturalization in the United States from 1990 to 2001 and the number and percentage of those naturalizations that came from IRCA migrants.[27] As shown, less than 815,000 of the migrants had been granted naturalization by 2000, constituting around one third of the 2.8 million that applied for, and were granted, legal status through the IRCA. This supports the conclusion that "the impact of IRCA was much more concentrated with respect to legal immigration than naturalization" (Rytina 2002). It also supports our argument that immigrant legalization and the political empowerment it generates is more a driving force in the explanation of our results than immigrant enfranchisement.

Table A.1
Persons Naturalized by Fiscal Year, Total and IRCA Immigrants

Fiscal Year	All Naturalizations (Number)	IRCA Naturalizations Number	(% of Total)
1990	270,101	133	0
1991	308,058	115	0
1992	240,252	218	0
1993	314,681	881	0
1994	434,107	11,048	3
1995	488,088	65,490	13
1996	1,044,689	227,905	22
1997	598,225	136,084	23
1998	463,060	85,647	18
1999	839,944	151,829	18
2000	888,788	135,385	15
Total	5,889,990	814,735	14

27. This table is taken from Exhibit 3 in Rytina (2002).

B. Intergovernmental Revenue and the Budget-Making Process

As mentioned in our Supplemental Data Appendix, all our data on local government revenues and expenditures are taken from the U.S. Census Bureau, Annual Survey of Local Government Finances and Census of Governments yearly series. This information comes from the U.S. Census Bureau's internal data base providing historical statistics on the finances of county governments and covers fiscal years 1957, 1962, 1967, and, on a yearly basis, from 1970 to 2006. The primary dependent variable we take from this data is per capita intergovernmental revenue (IGR) received by local governments (counties, cities, municipalities, aggregated to the county) from state governments. This is a discretionary fund that states use to provide budget support to local governments and each state has discretion to determine the nature, amount and distribution of these state grants to local governments.

In this appendix, we provide details on how important IGR is to local governments, what sorts of things state governments intend to support with it and how the overall budget process works within states and what role governors play in shaping it.

B.1 Intergovernmental revenue

First, IGR constitutes an an important component of general revenue at the local level. As shown in Figure B.1, just over 30 percent of all local revenue comes from intergovernmental revenue from state governments. Table B.1 provides examples, taken from the Census Government Finance and Employment Classification Manual, of what sort of things IGR from state governments to local governments are intended to support in the areas of education, health, highways and roads, and public welfare. In terms of education, IGR is intended to provide things like "equalization aid", "student transportation" and "school health", things which arguably are of more benefit to children of migrants than those of native white citizens. Finally, in Figure B.2 we show what share of local spending is dedicated to these areas. As shown, highways and roads, health and education consume 10 percent of more of local budgets and constitute the largest categories of local spending.

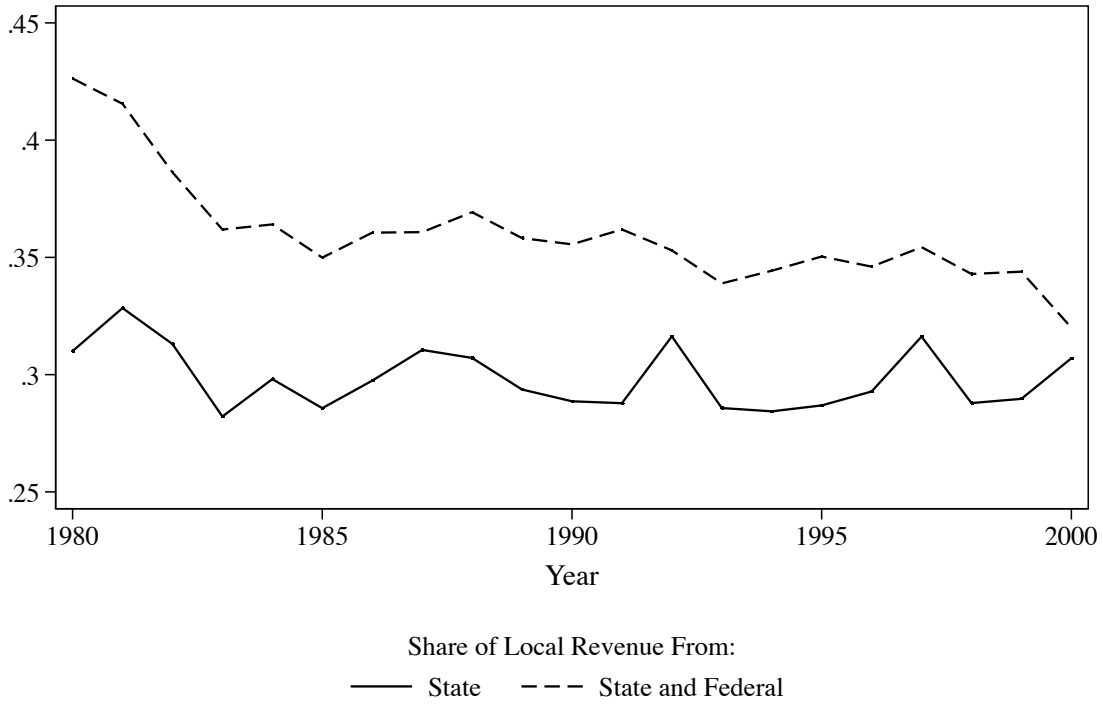


Figure B.1
Sources of local government revenue

Note: This graph plots the share of local government revenue (cities, municipalities and counties aggregated to the county) coming from state transfers and state and federal transfers.

Table B.1
Intergovernmental Revenue from State to Local Governments: Categories of Revenue

	Education	Health and Hospitals	Highways	Public Welfare
Includes	State aid for support of local schools; redistribution of federal aid for education; handicapped, special, and vocational education and rehabilitation; student transportation; equalization aid; school health; local community colleges; adult education; school buildings; and property tax relief related strictly to school funding.	State aid for local health programs; maternal and child health; alcohol, drug abuse, and mental health; environmental health; nursing aid; hospital financing (including construction); and hospitalization of patients in local government hospitals.	State aid for construction, improvement, or maintenance of streets, highways, bridges, tunnels, etc.; distribution of state fuel taxes; and aid for debt service on local highway debt.	State aid for public welfare purposes; medical care and related administration under public assistance programs (including Medicaid) even if received by a public hospital; care in nursing homes not associated with hospitals; federal categorical assistance (e.g., pass through of Aid to Families with Dependent Children, or AFDC); and administration of local welfare programs.
Excludes	State grants for libraries; state expenditures on behalf of local schools for textbooks, buses, school buildings, etc.; and value of donated food commodities (non-revenue).	State aid for medical care under public assistance programs such as Medicaid.	State grants for urban mass transit	

Notes: This table explains for what purposes intergovernmental revenue from state to local governments (counties, cities, municipalities aggregated to the county) is used for. We only observe these revenues in aggregate at the county level and do not observe the categories. This information is simply informative to give the reader an idea of the sorts of things a state governor can and cannot support with state-to-county transfers.

Source: Information taken from The Census Government Finance and Employment Classification Manual which can be accessed at: `https://www.census.gov/govs/www/classrevdef.html`

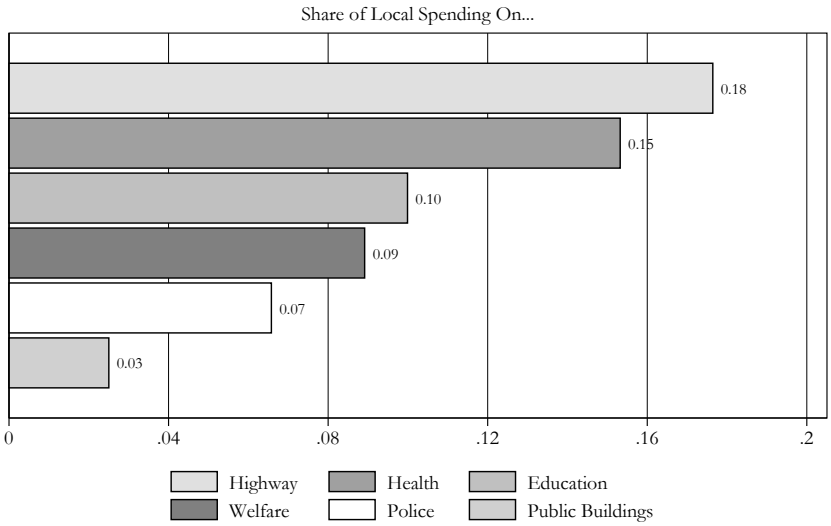


Figure B.2
Share of local expenditure on...

Note: This graph plots various categories of local government expenditure as a share of total local expenditure.

B.2 The budget-making process

Our main contention is that state governors use their budgetary powers to target resources to Hispanic communities that are politically empowered by the IRCA. A crucial question is thus how much power governors actually exert over the budget-making process. We take up this question in this section and demonstrate that, in fact, governors have substantial influence in the formulation and implementation of the states fiscal priorities.[28]

For the vast majority of states, the budget-making process takes an entire year: it begins sometime in July or August and for all but four states, the fiscal year begins on 1 July. The state budget office is responsible for the analysis and preparation of the budget on behalf of the governor. The budget-making process begins when the state budget office requests proposals from, and provides guidance to, various state-level agencies. This guidance typically includes state spending targets, assumptions for inflation and priorities of the governor. In the fall, the various agencies submit their budget proposals to the governor who reviews them and provides additional direction. Once the governor's recommendations are incorporated, he or she presents the proposed budget to the state legislature in the winter season. After the legislature passes the budget, it requires the governors signature to become law.

28. The information in this section draws from the National Association of State Budget Officers report on the budget-making process NASBO (2015).

C. Additional Robustness

In this appendix, we take up four sets of robustness checks. In section C.1 we undertake a number of additional checks on the strength, nature and timing of the baseline relationship between immigrant legalization and the distribution of intergovernmental revenue. In section C.2 we address the issue of balance between treated and control counties and in sections C.3 and C.4 we discuss a number of caveats concerning the main dependent and independent variables and demonstrate that our results are robust to these caveats.

C.1 Additional Robustness

We begin by further investigating the strength and nature of the relationship between immigrant legalization and the distribution of state aid. To start, in Column 1 of Table C.1 we rerun the baseline specification after standardizing the variables in the analysis in order to ease interpretation. As shown, a one standard deviation in the number of IRCA applicants per 1,000 capita leads to a .012 standard deviation increase in the amount of per capita revenues received by that county. In Column 2, we carry out a long difference estimation, considering only 1982 and 1992. The idea here is to skip intervening years to overcome issues with respect to timing of various sorts: different electoral cycles in different states, different budget response times and different IRCA application processing times. As shown, the IRCA variable maintains its predictive power over per capita intergovernmental revenue. Figure C.1 plots the coefficients from a number of such regressions, each using a different time period for the difference estimation. As shown, both the magnitude of the coefficient as well as its timing confirm the overall hypothesis put forward in this paper: the coefficient on legalization appears positive and significant in the early 1990s, prior to immigrant enfranchisement and remains more or less stable thereafter until the late 1990s, again underscoring the distinct role of immigrant legalization as opposed to enfranchisement in driving our results.

In Column 3 of Table C.1, we use a county's 1980 population as the denominator to calculate per capita transfers and IRCA applicants as yet another way of ensuring that population changes are not driving the results. As shown, the results are similar to the baseline. In Column 4, we include a quadratic term of the key explanatory variable and find evidence of a small but significant concave relationship between the IRCA and the distribution of state aid. Finally, in Column 5 we rule out the possibility that our results are driven by the provision of the IRCA that called for additional funding for border security. Specifically, we run our baseline model on a sample that omits the four border states and find that the coefficient of interest remains positive and precisely estimated.

Table C.1
Robustness Checks I

	Log of Inter-governmental Revenue (per capita)				
	(1) Std. Values	(2) Long Diff. $\Delta y_{1992\text{-}1982}$	(3) 1980 per capita	(4) IRCA^2	(5) Drop Border States
$\text{IRCA}_{92} \times P_{86}$	0.0118**	0.00526**		0.0121**	0.00562**
	(0.00583)	(0.00249)		(0.00494)	(0.00219)
$\text{IRCA}_{80} \times P_{86}$			0.00270**		
			(0.00110)		
$\text{IRCA}^2_{92} \times P_{86}$				-0.000222**	
				(0.000101)	
Control Variables	No	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
$\text{Hisp}_{1980} \times$ Year Dummies	Yes	Yes	Yes	Yes	Yes
Observations	41,036	3,908	39,337	41,036	35,509
Number of Counties	2,204	1,954	2,053	2,204	1,932

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. IRCA_{92} is the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level. IRCA_{80} is similarly defined but uses a county's 1980 population as the denominator for the per 1,000 capita calculation. The per capita transfers in Column 3 are also calculated calculated with 1980 county population in the denominator. P_{86} is a binary variable that indicates time periods before and after 1986. In Column 1, the outcome, the measure of treatment and the controls are all standardized. Control variables include county level poverty and unemployment rates, log of population, log of income and the share of the population that is white and Black. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

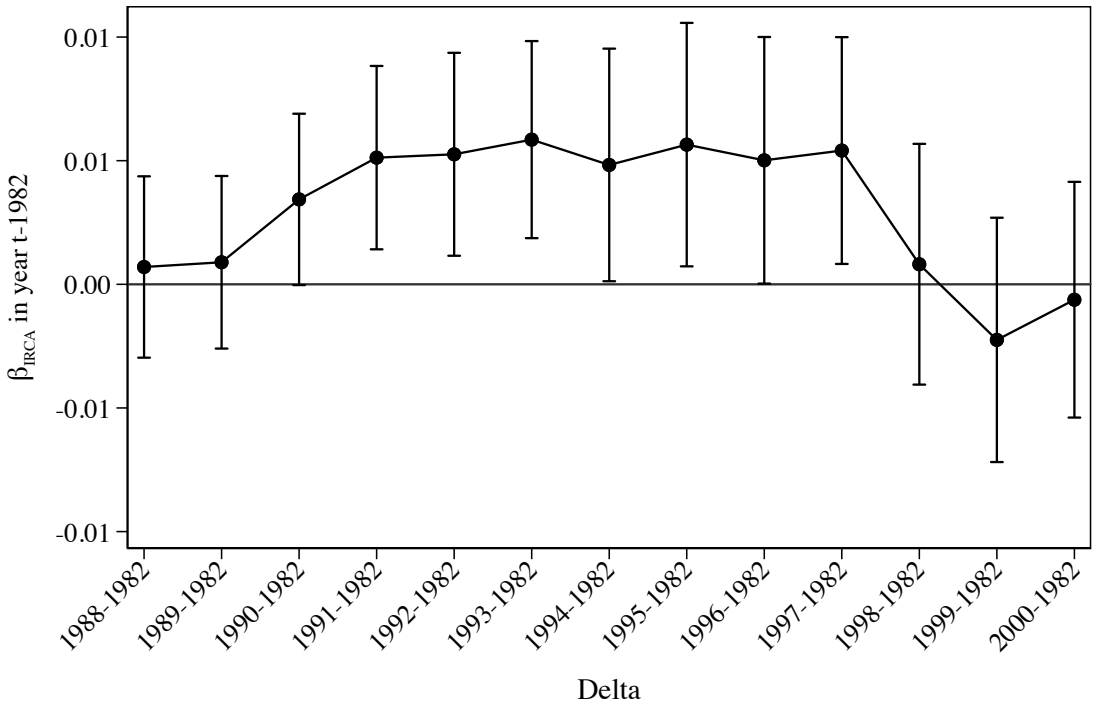


Figure C.1
First-difference coefficient estimates

Note: This graph plots the coefficients from various first-difference regressions from 1988 to 2000 using 1982 as the base year. The dependent variable is the log of per capita transfers from state to local governments (in 1999 USD) and β is the coefficient on the cumulative number of IRCA applicants per 1,000 county inhabitants fixed to its 1992 level interacted with an indicator for time periods before and after 1986. Control variables include county level poverty and unemployment rates, log of population, log of income and the share of the population that is white and Black. County fixed effects and state-year fixed effects are included in the estimations as are year dummies interacted with the log of the 1980 Hispanic share of the county population. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.

C.2 Balance

In this Appendix, we address the issue of balance. As shown in Table C.2, treated and control counties are not the same. There are, however, a number of reasons why we believe this not to be a concern.

First, our identifying assumption is that, absent the IRCA, intergovernmental revenue would have evolved the same in treated and control counties. In this connection, Figure 1 in the main paper demonstrated parallel trends with respect to the outcome variable of interest.

Second, in Figure C.2, we demonstrate that, whilst treated and control counties differ in terms of levels with respect to various covariates, trends in the raw data in those covariates did not experience a sharp or sudden changes after 1986 when the IRCA passed. This is mostly because undocumented migrants were (and still are) included in population estimates, are eligible (especially their children) for basic public services such as health and education and, to some extent, pay tax as some undocumented migrants obtain illegal social security documents.

Table C.2
Covariate Balance and the IRCA

	$\hat{\beta}$	p-value H_0 : $\beta = 0$
Unemployment Rate	.00346	0.225
Poverty Rate	-.04051	0.217
Log of Population	.08025	0.000
Log County Income	.00818	0.000
Tax Revenue (pc)	.007764	0.964
Share Hispanic	.00522	0.000
Share over 18	.00013	0.446
Share Households w Children	-.00064	0.005
Share Black	-.00061	0.008
Share White	-.00265	0.000
Births per 1000	.04921	0.000
Bachelor Degree Attainment	.20196	0.000
Votes, D-President (%)	-.17299	0.000
Republican Governor	-.00586	0.001

Notes: Each row reports β from the following regression: $X_{c,1980} = \alpha_0 + \beta(IRCA_{c,92}) + \epsilon_c$ where X_c is a covariate in county c measured in 1980 and $IRCA_{c,92}$ is the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level.

Nonetheless, in Table C.3 we run a number of additional checks against the potential concern of covariate imbalance. We begin by including additional demographic controls in Column 2. These include county level per capita tax revenue, the share of county population that is over 18, the share of county households with children, births per 1,000 county inhabitants and the percent of the county population with a bachelor degree. As shown, the coefficient on the IRCA variable becomes slightly larger and more precisely estimated with the inclusion of these controls. Next, in Column 3, we identify a more comparable control group on the basis of

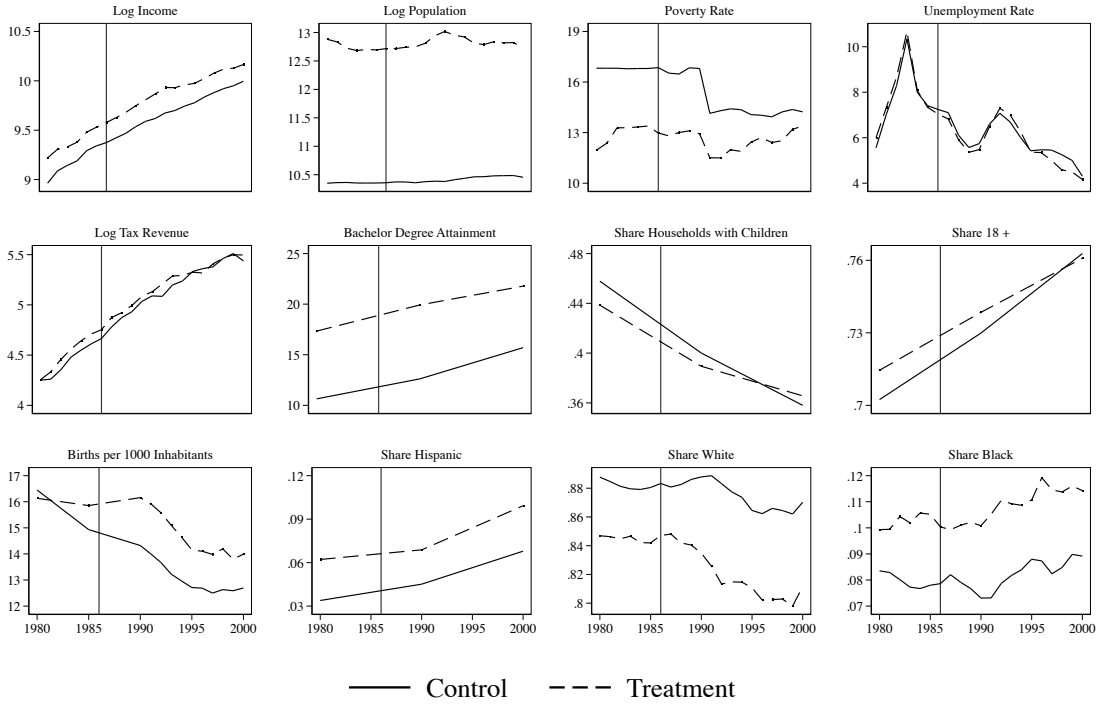


Figure C.2
Trends in county socio-economic characteristics

Note: This graph the evolution of various characteristics in treated and control counties. The data for the 4 figures in the top row are taken from Baker (2015) while the data for all remaining figures are taken from the US Census Bureau USA Counties Database.

propensity score matching. That is, for each treated county we identify its nearest neighbor from the control group on the basis of propensity scores derived from various covariates measured in 1980.[29] We rerun the baseline on this matched sample, and, as shown, the model returns a coefficient nearly identical to the baseline. Finally, in Column 4 we omit all controls. The idea here is that, if imbalanced covariates are driving the result, excluding all our controls should then produce a very different estimate. In contrast to this, we see that the model with no controls produces a very similar result to the baseline, strongly suggesting that covariate (im)balance is not a driving force in the distribution of intergovernmental revenue. Moreover, it is important to note that all our regressions include the 1980 Hispanic share of a county's population interacted with time dummies, enabling us to account for perhaps the most important imbalance between treated and control counties: that of the size of the Hispanic population.

29. Specifically, the covariates used in the matching process include county level poverty and unemployment rates, log of population, log of income and the share of the population that is white and Black, all measured in 1980.

Table C.3
Robustness Checks II

	Log of Inter-governmental Revenue (per capita)			
	(1) Baseline Effect	(2) Demographic Controls	(3) Propensity Matching	(4) No Controls
$\text{IRCA}_{92} \times P_{86}$	0.00319**	0.00493***	0.00315*	0.00300**
	(0.00145)	(0.00174)	(0.00187)	(0.00148)
Control Variables	Yes	Yes	Yes	No
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Demographic Controls	No	Yes	No	No
$\text{Hisp}_{1980} \times$ Year Dummies	Yes	Yes	Yes	Yes
Observations	41,036	16,832	12,321	41,036
Number of Counties	2,204	2,084	626	2,204

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. IRCA_{92} is the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level. P_{86} is a binary variable that indicates time periods before and after 1986. Control variables include county level poverty and unemployment rates, log of population, log of income and the share of the population that is white and Black. The additional county-level demographic controls in Column 2 are: per capita tax revenue, the share of county population that is over 18, the share of county households with children, births per 1,000 county inhabitants and the percent of the county population with a bachelor degree. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C.3 Measuring intergovernmental revenue

Our data on intergovernmental revenue is taken from the U.S. Census Bureau, Annual Survey of Local Government Finances and Census of Governments yearly series. Although this is a comprehensive survey, not every variable is surveyed in every county in every year. In terms of intergovernmental revenue, this is the case for around 35 percent of the counties in our sample. For this reason, we linearly interpolate the revenue data. In this appendix, we demonstrate that the linear interpolation of the revenue data is not adversely affecting our results. Specifically, in Column 2 of Table C.4, we rerun the baseline using a measure of intergovernmental revenue that is not linearly interpolated. As shown, we obtain a coefficient that, in terms of precision and magnitude, is similar to the baseline. In Column 3 we restrict the analysis to only those counties whose intergovernmental revenue is surveyed in each year throughout the sample. As shown, the coefficient on the IRCA measure, though somewhat less precisely estimated, is similar to the baseline, alleviating concerns that counties dropping in and out of the Annual Survey are adversely affecting our results.

Table C.4
Robustness Checks III

	Log of Inter-governmental Revenue (per capita)		
	(1) Baseline Effect	(2) No Linear Interpolation	(3) IGR Surveyed Every Year
$\text{IRCA}_{92} \times P_{86}$	0.00319**	0.00261**	0.00270*
	(0.00145)	(0.00125)	(0.00160)
Control Variables	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
$\text{Hisp}_{1980} \times$ Year Dummies	Yes	Yes	Yes
Observations	41,036	33,146	30,391
Number of Counties	2,204	2,201	1,463

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. IRCA_{92} is the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992. P_{86} is a binary variable that indicates time periods before and after 1986. Column 2 uses intergovernmental revenue without a linear interpolation while Column 3 restricts the analysis to only those counties whose intergovernmental revenue is surveyed in every year of the sample. Control variables include county level poverty and unemployment rates, log of population, log of income and the share of the population that is white and Black. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C.4 Measuring IRCA applicants

An important limitation of the IRCA data is that there are no county identifiers for migrants who applied from counties with populations less than 100,000 or with fewer than 25 applicants. Of the 3,016,055 applicants in the data, this is an issue that concerns 277,932 (9.2%) of them. In this Online Appendix, we describe our baseline approach to dealing with this issue. In addition, we explore three alternative approaches to address it—an alternative method for imputing the share of IRCA applicants in small counties, simply treating these counties as control counties (and hence assigning them a migrant share of zero) and controlling for the differential effect of counties with unassigned migrant shares—and show that our results are very similar across all approaches. Finally, we estimate our baseline specification using an alternative source and measure for IRCA migrants.

Baseline approach: In our our baseline specification, we address the unassigned migrants by first generating a linear prediction from a fitted model of IRCA migrant shares for those counties with populations greater than 100,000 and whose IRCA migrant shares are known. We then use the parameters of this model to generate an out-of-sample prediction for the IRCA migrant shares in those counties with populations less than 100,000. We fit the model using a rich set of county covariates, all measured in 1980. The county covariates include: poverty and unemployment rates, log of income, share of the population that is white, Black and Hispanic,

share of the population with a bachelor degree, share of households in a county with children under the age of 18, share of the population that is aged 18 and over and births per 1,000 county inhabitants. In addition, the model includes the interaction of each covariate with county population size in 1980 so as to rule out, or take into account, any heterogeneous effects these covariates may have along population size in predicting the unassigned migrant shares. Finally, the model includes state dummies. We plot the residuals of this model against log of county population in Figure C.3 and, as shown, the model does not display any systematic bias along population, alleviating concerns that the model over-predicts for large counties and under-predicts for small ones.

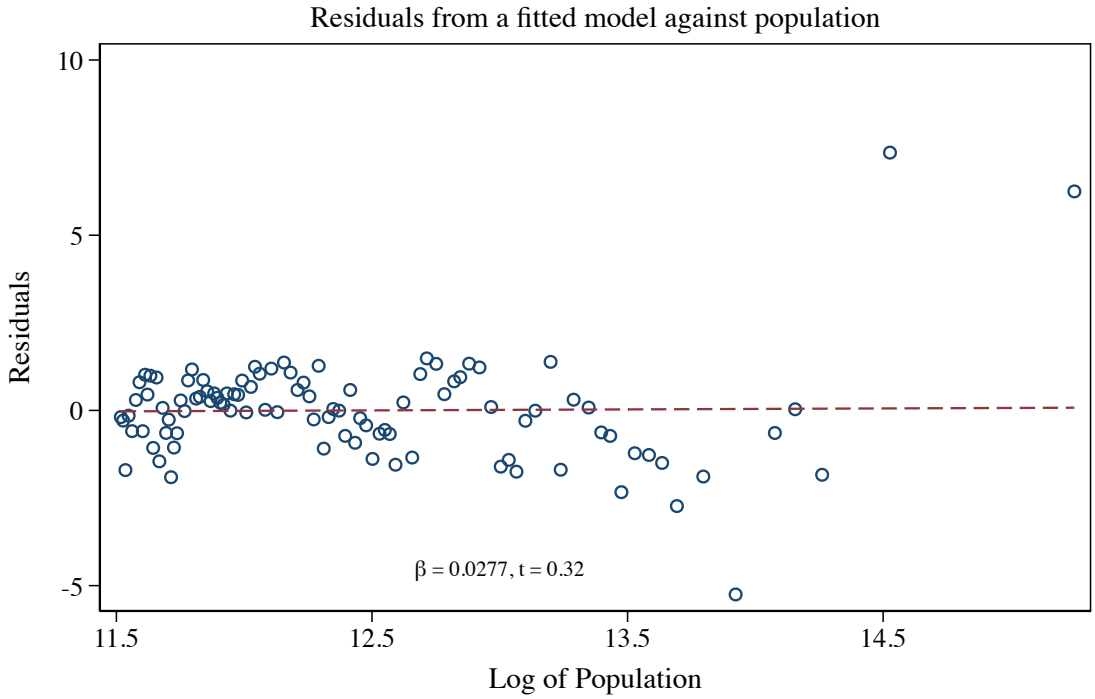


Figure C.3
Residuals of a fitted model against log of population

Note: This is a binscatter of residuals from a fitted model plotted against log of county population. The fitted model predicts IRCA migrant shares on the basis of 11 county covariates, all measured in 1980 and all interacted with 1980 county population.

There are a number of counties for which the model generates a negative predicted migrant share. To get around this issue, we run the same model as described above but using a logistic regression in order to predict the treatment status of small counties with unassigned migrants based on the treatment status of counties whose treatment status we observe in the data. We then set the predicted migrant shares to zero or non-zero based on predicted treatment status.

Alternative imputations: In Columns 2 and 3 of Table C.5, we present two alternative ways of dealing with this issue. First, in Column 2, we use the 1980 Hispanic share of the population as a proxy for the share of IRCA applicants in small counties. The share is calculated not as the number of Hispanics in 1980 divided by the overall county population in 1980 but

rather as the number of Hispanics in 1980 (in small counties) divided by the total Hispanic population, in 1980, of all small counties in a given state.[30] As shown in Column 2 of Table C.5, this imputation method returns a result very similar to our baseline.

In Column 3, we present results when ignoring the unassigned migrants altogether. As mentioned, this is an issue that concerns less than 10 percent of the 3 million migrants in the data, indicating that IRCA migrants tend to cluster in larger urban centers. Accordingly, we treat small counties with unassigned migrants as control counties (i.e. we assign them an IRCA migrant share of zero) and, as shown, the result is very similar to both the baseline and to the alternative imputation method using the Hispanic share as the proxy.

Controlling for unassigned counties: In Column 4 of Table C.5, we undertake a final check to ensure that the unassigned migrant shares are not adversely affecting our results. We rerun the baseline and include an indicator for a small county interacted with unrestricted year dummies. The idea here is to capture any time-varying differential effects that these counties might exert over the distribution of intergovernmental revenue. As shown, the coefficient on the IRCA variable is unaffected. That the coefficient remains stable and precisely estimated across all four approaches to this issue indicates that the unassigned migrants are not an overwhelming concern when it comes to the distribution of state finances.

Alternative measure and source of the IRCA data: Finally, in Column 5 of Table C.5 we reproduce the results using an alternative measure and source for the IRCA data. We draw on the INS Legalization Summary tape data and obtain the number of *legalized* migrants as a result of the IRCA. This corroborates our main findings and demonstrates that, in practice, the difference between those who applied for legal status and those who actually obtained it as a result of the IRCA is negligible when it comes to the distribution of public resources. When using the INS data, we apply the same imputation method as in our baseline to address the issue of migrants from small counties with no county identifiers.

30. Using the 1980 Hispanic share of the county population also produces a similar result.

Table C.5
Robustness Checks IV

	Log of Inter-governmental Revenue (per capita)				
	(1) Baseline Effect	(2) Imputation: Hispanic Proxy	(3) No Imputation	(4) Control Unassigned Counties	(5) INS Data: Legalized
$\text{IRCA}_{92} \times P_{86}$	0.00319**	0.00340**	0.00349**	0.00324**	0.00300**
	(0.00145)	(0.00156)	(0.00143)	(0.00145)	(0.00143)
Control Variables	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Small County × Year Dummies	No	No	No	Yes	No
$\text{Hisp}_{1980} \times$ Year Dummies	Yes	Yes	Yes	Yes	Yes
Observations	41,036	40,043	41,036	41,036	41,036
Number of Counties	2,204	2,200	2,204	2,204	2,204

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. IRCA_{92} is the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992. P_{86} is a binary variable that indicates time periods before and after 1986. Column 2 uses a different method to impute the unassigned migrant shares for counties with populations less than 100,000 as explained in the text. Column 5 repeats the baseline using the cumulative number of IRCA migrants who were actually legalized. This measure comes from the INS Legalization Summary tape data. Control variables include county level poverty and unemployment rates, log of population, log of income and the share of the population that is white and Black. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

D. Concurrent Reforms and Programs

In this appendix, we discuss two concurrent programs and reforms to the IRCA and rule out the possibility that they had a confounding influence on our results: the 1986 Reagan Tax Reform and the SLIAG program associated with the IRCA.

D.1 1986 Tax Reform

The Reagan administration passed a tax reform in 1986 which, among other things, lowered federal income taxes and expanded the earned income tax credit (EITC) which provides tax credits to low- to moderate-income working individuals and families. There are a number of reasons why this reform will not confound the distribution of intergovernmental revenue flowing from state to county governments as a result of the IRCA.

First, all our specifications include state-year fixed effects which eliminate state-specific time varying shocks, such as drastic changes to state tax revenue which may have come as a result of the tax reform and which may have given the governor additional funds to allocate.

Second, in Table C.3, as an additional check, we control for total tax revenue at the county level in case the tax reform has any differential effects on counties affected by the IRCA. As shown, the inclusion of this control does nothing to diminish the relationship between the IRCA and IGR.

Third, the EITC is an anti-poverty program and as mentioned in the main text, a feature of the IRCA was that it "barred" IRCA migrants "from participation in programs of financial assistance furnished under federal law on the basis of financial need for a period of five years from the effective date of each alien's lawful temporary resident status". What is more, the IRCA led to no significant welfare increases (bottom left panel of Figure 3 in the main text) further ruling out confounds due to anti-poverty programs like the EITC.

D.2 State Legalization Impact Assistance Grants (SLIAG)

Section 204 of the IRCA outlines the details associated with the State Legalization Impact Assistance Grants (SLIAG)—a $1B per year federal funding program for four years which could be spent over seven years until 1994. SLIAG was designed to compensate states for the extra costs they would incur as a result of the legalization program of the IRCA. Specifically, SLIAG funds were intended to assist states to defray expenses in the areas of public health, public assistance and education (Liu (1991); DHHS (December 1991)). SLIAG funds are not confounding our results for the simple reason that SLIAG was administered through an institutional arrangement entirely separate from that of regular state budgets and is not, therefore, part of the intergovernmental revenue variable that we exploit. More specifically, as part of the IRCA, The federal government instituted the Single Point of Contact (SPOC) system, whereby every state designated its own SPOC so as to create "state-level lead implementation agencies to manage the [SLIAG] program according to the unique needs and arrangements of the individuals states" Liu (1991). At the federal level, it was the Department of Health and Human Services that received applications for and disbursed the SLIAG funds but SLIAG required that SPOCs coordinate directly with state and local public health, public assistance and education organizations to receive the funds Liu (1991). Moreover, all our specifications include state-year fixed effects which capture any state-specific time varying shocks to revenue arising out of the IRCA, an example of which is SLIAG.

E. The IRCA and Hispanic Political Mobility

In this appendix, we provide an additional piece of evidence that might link the IRCA to Hispanic political selection: the effect of the IRCA on Hispanic political mobility. Specifically, we test whether counties with many legalized migrants not only elect more Hispanics to office but also elect more candidates who move higher up on the political ladder.

To carry out this exercise, we classify each NALEO official in each year into one of five political levels, ranging from school board officials to federally elected members of congress. For each officer, we measure, over the course of the sample, his or her highest and lowest political rank in order to identify those who moved up the political ladder and those who did not. We then aggregate, for each county, the total number of mobile officers regress this outcome on the number of per 1,000 capita IRCA applicants measured in 1992. The results are presented in Table E.1. As shown, the IRCA leads to significantly higher number of politically mobile officers at the county level, reinforcing the idea that legalization was significant to Hispanic political mobilization.[31]

Table E.1
IRCA and Hispanic Political Mobility

	Politically Mobile Hispanic Officers
	(1)
$IRCA_{92}$	0.0646***
	(0.0241)
Control Variables	Yes
State Fixed Effects	Yes
Observations	2,226
Number of Counties	2,226

Notes: The dependent variable is the total number of Hispanic officials who move up the political ladder in a given county over the course of the entire sample. $IRCA_{92}$ is the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level. Control variables include county level poverty and unemployment rates, log of population, log of income and the share of the population that is white, Black and Hispanic all measured in 1980. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

31. This regression is a cross section. This is because the outcome variable is time-invariant: we simply calculate, for each county, the total number of officers who moved up the political ladder over the course of the entire sample. It is for this reason that this model does not include county fixed effects, state-by-year fixed effects or the 1980 Hispanic share of a county interacted with year dummies. Instead, as shown, we include state fixed effects and control for the 1980 share of the population that is Hispanic.

F. Attitudes Towards Migrants

In this appendix, we provide further details on the construction of our proxy for anti-migrant voters in the ANES data and additional evidence to rule out the possibility that anti-migrant sentiment is driving our results.

F.1 Ruling out anti-migrant sentiment: Evidence from California

Like today, undocumented migration was a politically charged issue at the time of the IRCA. A notable opponent of the IRCA, and of undocumented migration more generally, was Governor Pete Wilson, Republican governor of California, who ran a campaign of fear and anti-migrant propaganda.[32] In his 1994 re-election campaign, Governor Wilson pinned his hopes onto Proposition 187, the "Save Our State" ballot initiative, and the Republican Party offered ideological and financial backing to see the proposition go through. Proposition 187 prohibited undocumented migrants from using non-emergency public services and required the providers of such services to immediately report undocumented migrants for deportation. It was passed by California's voters only to be struck down by a federal court. The proposition, and Wilson's campaign to support it, was highly controversial and left somewhat of an enduring legacy. Bowler, Nicholson, and Segura (2006), for example, find that racially charged ballot initiatives in California—and specifically Proposition 187—are significantly associated with a shift in political support away from the Republican party and towards the Democratic party on behalf of non-Hispanic white voters as well as Latino voters.

In light of this political context, it seems reasonable to ask to what degree our results are actually driven by governors catering to anti-migrant sentiment arising out of the IRCA rather than to the needs of the documented migrants themselves. We examine this question in Table F.1 by quantifying the impact of Governor Wilson's term in office and of Proposition 187 on state aid. In Column 1 we begin by estimating the parameters of the baseline specification excluding California, the state with the strongest expression of anti-migrant sentiment at the time and, as shown, the results not only hold but become larger and more precise. In Column 2, we restrict the sample to consider only California and exploit variation in county level vote shares for Proposition 187, which varied from as little as 29 percent to as much as 77 percent. Interestingly, neither the baseline effect nor the interaction are positive, suggesting that Governor Pete Wilson (who was in office from 1991 to 1999) was unresponsive both to the IRCA and to those voters who were most strongly against it. We take this as additional evidence that anti-migrant sentiment did not provide strong incentives for state governors in the face of the IRCA.

32. In a dramatic re-election advertisement, Governor Wilson states "I'm suing to force the Federal Government to control the border and I'm working to deny state services to illegal immigrants. Enough is enough." (Transcribed from the Television Ad which can be found at: `https://www.youtube.com/watch?v=lLIzzs2HHgY`. Accessed 8 March 2018.

Table F.1
IRCA and Anti-Migrant Sentiment

	Log of Inter-governmental Revenue (per capita)	
	(1) Exclude Cali	(2) Only Cali
$IRCA_{92} \times P_{86}$	0.00766***	0.00157
	(0.00255)	(0.00435)
$IRCA_{92} \times P_{86} \times$ Prop 187		-0.0000483
		(0.0000686)
Control Variables	Yes	Yes
County Fixed Effects	Yes	Yes
State-Year Fixed Effects	Yes	Yes
$Hisp_{1980} \times$ Year Dummies	Yes	Yes
Observations	39,906	1,130
Number of Counties	2,150	54

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. $IRCA_{92}$ is the cumulative number of IRCA applications per 1,000 county inhabitants fixed to its 1992 level. P_{86} is a binary variable that indicates time periods before and after 1986. Prop 187 is the county vote share for Proposition 187 held in California. This regression includes all baseline effects and lower order interactions. Control variables include county level poverty and unemployment rates, log of population, log of income and the share of the population that is white and Black. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

F.2 A proxy for anti-migrant voters in the ANES data

In section 8 of our paper, we investigate the impact of the IRCA on political participation. We draw on individual ANES data in order to identify people with negative attitudes towards undocumented migrants and then examine whether these people exhibit differential voting behavior if they live in counties affected by the IRCA.

In order to identify individuals with negative attitudes towards undocumented migrants, we exploit the "illegal alien" thermometer of the ANES. This question asks people to gauge their feelings towards undocumented migrants on a scale between 0 (very cold) and 100 (very warm). However, this measure is only available as of 1988, two years after the IRCA was implemented. We therefore use the African-American thermometer, scores for which are available since 1980, as a proxy for the illegal alien thermometer. Figure F.1 plots the residual values of these two thermometer scores once county and year fixed effects and a range of individual and county characteristics have been accounted for. As shown, the two measures follow each other rather closely, indicating that the choice of proxy is valid. We code individuals with thermometer scores less than 50 as "anti-migrant" and those with scores greater than 50 as "pro-migrant".

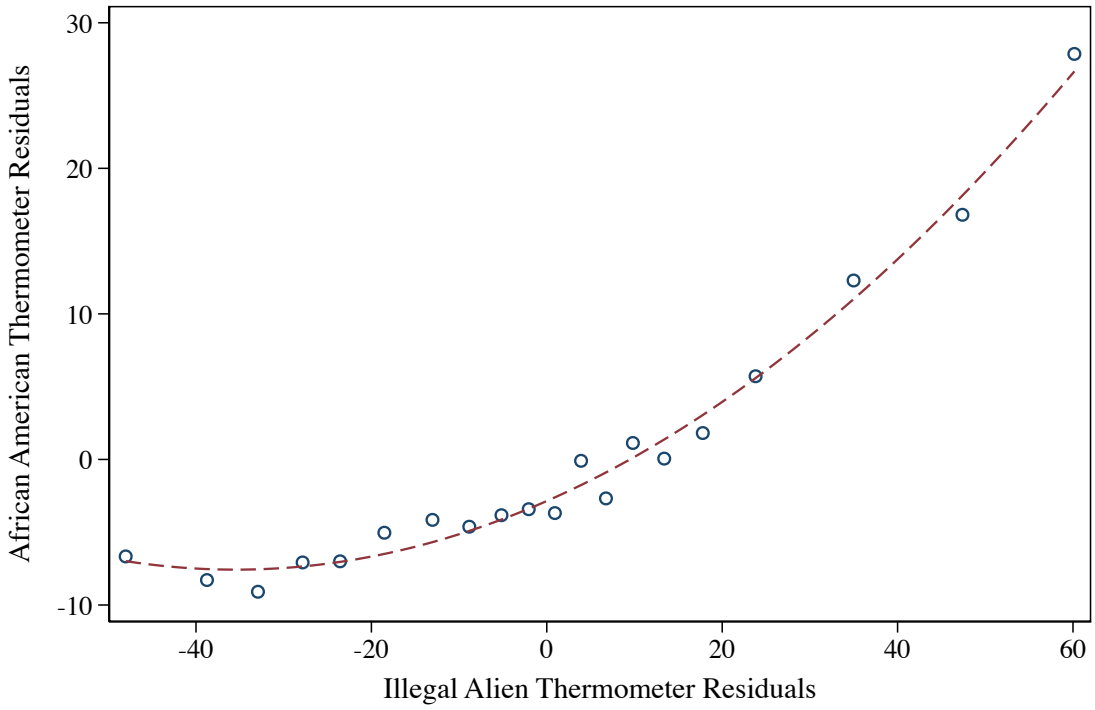


Figure F.1
Attitudes towards African Americans and Undocumented Migrants

Note: This graph uses ANES data and plots the residuals of the African-American and Illegal Alien thermometers once year and county fixed effects as well as individual race, income, education, marital status and county population have been accounted for. The 4,722 data points in this graph are placed in 20 bins.

G. Presidential and Gubernatorial Election Results

Throughout the paper, we use presidential election results at the county level as a proxy for governor election results. This is because the gubernatorial election results we have are available only as of 1990, after the IRCA was passed. In this section, we demonstrate that the choice of proxy is valid by presenting a partial correlation plot of Presidential and Governor election results at the county level. These are shown in Figure G.1.

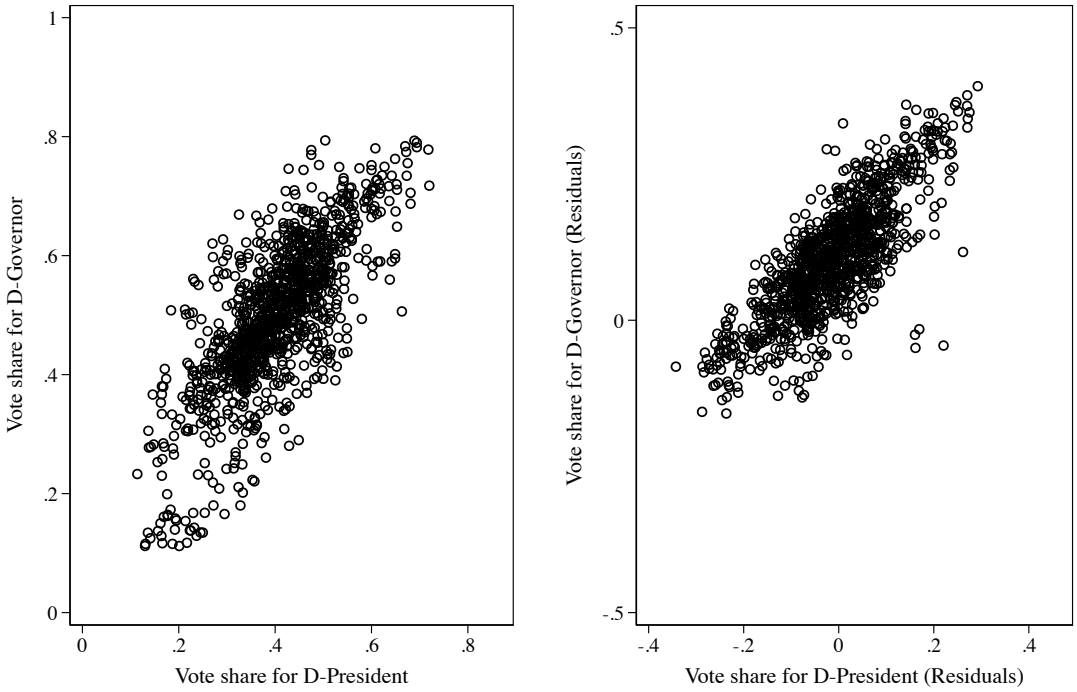


Figure G.1
Presidential and Gubernatorial election results

Note: These Figures plot the Democratic vote share at the county level in Presidential and Gubernatorial elections beginning in 1992. The scatter on the left plots the raw data while the scatter on the right plots the residuals once state-year fixed effects and county fixed effects have been accounted for. Presidential election results come from the USA Counties Database while Gubernatorial election results come from David Leip's Atlas of US Presidential Elections.