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

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) which legalized 2.8 million mostly Hispanic migrants. Governors, we find, allocate more per capita resources to IRCA-affected counties, an allocation that is responsive to their electoral incentives, targeted toward educational expenditure and that ultimately increases Hispanic high school completion rates. Importantly, our baseline effect arises prior to 1992, the first year IRCA migrants gained eligibility to vote. This allows us to decouple immigrant legalization from enfranchisement in the interpretation of our results. We argue that legal status attracts differentially more resources from the state owing to its capacity to politically empower already legal Hispanic citizens in communities of mixed legal status. Consistent with this mechanism, IRCA counties experience significant increases in voter turnout and in the number of Hispanics winning public office.

JEL-Codes: J150, H720, P160.

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

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This paper brings together and supersedes two previous working papers: *Legal Status and Political Representation: The 1986 IRCA and Hispanic Public Officials* (by Navid Sabet) and *The Political Economy of Immigrant Legalization: Evidence from the 1986 IRCA* (by Navid Sabet and Christoph Winter). Navid Sabet thanks the LSE, University of Toronto and the University of Guelph for their hospitality. ‡We owe a debt of gratitude to Davide Cantoni for his support and advice throughout the project. Noam Yuchtman provided incisive insight that sharpened our analysis in the later stages of the work. In addition, we thank Nava Ashraf, Tim Besley, Mathias Bühler, Marvin Deversi, Jonathan de Quidt, Jeremiah Dittmar, Florian Englmaier, Francesco Fasani, Greg Fischer, Joshua Gottlieb, Yosh Halberstam, Benjamin Häusinger, Mitch Hoffman, Ethan Ilzetzki, Michael Kosfeld, Gerard Padró i Miquel, Andreas Peichl, Torsten Persson, Andreas Steinmayr, Till Stowasser, Daniel Sturm, Munir Squires, Marco Tabellini, Fabian Waldinger, Ebonya Washington, Daniel Wissmann and Stephane Wolton as well as seminar participants at the University of Munich, SWEAT at the University of Toronto, the University of Guelph, Lund University, the University of Haifa, ZEW, Goethe University Frankfurt, the 2020 Congress of the EEA, the 2018 EBE Summer Meeting, the 2018 CEMIR Junior Economist Workshop on Migration Research at the ifo Institute, the 2018 Congress of the Verein für Socialpolitik, the 20th annual INFER conference, and the 2018 Econometric SocietyWinter Meeting for constructive comments and criticisms. Both authors gratefully acknowledge funding through the Elite Network of Bavaria.

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 inter-governmental transfers from state to local governments. We digitize two new and novel sources of micro-data—on the universe of Hispanic officials elected to public office and on the universe of state governors in office and their political incentives—in order to understand the precise mechanisms that drive the result. The picture that emerges is one of democratic responsiveness: governors, regardless of their party affiliation, allocate more per capita resources to IRCA-affected counties after legal status was granted, an allocation that is targeted toward local educational expenditure. This allocation is, moreover, responsive to the electoral incentives of the governor, suggesting that it is politically motivated. Legalization also leads to significant increases in the high school completion rates of Hispanic youth, but not of Caucasian, African-American or Asian youth, lending credence to the view that governors targeted resources to Hispanic communities that were 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

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. In reality, however, less than 250 IRCA migrants had naturalized prior to 1992 (Rytina 2002).

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

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. In this respect, we find that IRCA-affected counties experience significant increases in voter turnout, an effect that is most pronounced in counties with larger shares of already legal Hispanic migrants in them. What is more, we find that legalization leads to a significant increase in the number of Hispanics elected to public office. This result holds for officials at all levels, but is strongest for those elected to local offices, in particular for school board officials and mayors. Together, our results uncover a novel political economy mechanism linking legal status to improvements in the socio-economic outcomes of individual migrants: Specifically, they demonstrate that immigrant legalization leads to a broad political transformation of local, Hispanic communities, a transformation marked not only by increases in political participation but also by significant changes to the ethnic composition of elected officials. Governors respond by targeting resources strategically in an effort to capture the political gains brought about by such a widespread mobilization of Hispanic interests.

Using a differences-in-differences regression framework, we compare the distribution of inter-governmental transfers[3] in counties affected by the IRCA with those unaffected by it, before and after 1986. Our baseline estimate suggests that treated counties receive 9.5 percent more per capita transfers than untreated counties, an effect driven by 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—in particular from sales tax—generated from the IRCA and not through decreases in funding to counties unaffected by the amnesty. 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.

In bringing significant numbers of migrants "out of the shadows", we argue that legalization, as distinct from enfranchisement, lifts deeply 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

results.

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

posit, therefore, that governors, who play an important role in formulating the state budget, allocate resources strategically to those counties most affected by the IRCA in order to respond to the newfound political voice of these previously marginalized communities. We substantiate this claim in three parts.

First, we provide evidence to show that legal status did indeed provide state governors with sufficient political incentives to respond differentially in terms of the allocation of public funds to counties affected by the IRCA. 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, we exploit variation in the political incentives and constraints facing state governors and analyze the sensitivity of our results to those incentives and constraints. If the transfers of the state government are reflective of mechanical forces outside the control of the governor, the results ought to be insensitive to the political context of the governor. If, on the other hand, the transfers are politically motivated, the results ought to exhibit heterogeneity with respect to the various incentives and constraints facing the incumbent. Consistent with this line of thinking, we find that counties affected by the IRCA receive more resources from the state when their governor is eligible for re-election, faces political competition or an upcoming election, enjoys line-item veto power over the budget 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.

Second, we show that the political incentive arose primarily in response to a broad political transformation that legal status engendered in communities of mixed legal status. To this end, we digitize a novel source of 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 repre-

sentation 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. Moreover, legalization's effect on Hispanic political selection is stronger in those counties with larger shares of Hispanic people in the population and is not explained by increases in the number of Hispanic candidates running for office.

Drawing on county and individual level voting data, we further underscore the political and electoral relevance of the IRCA. Counties affected by the IRCA experience significant increases in voter turnout in Presidential elections, an effect which is driven primarily by those counties with larger shares of already legal Hispanic migrants in them. 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. These results indicate that the IRCA led to meaningful Hispanic political mobilization without triggering nativist backlash.

Lastly, we focus the analysis on local expenditure. 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 the number of Hispanic individuals entering public office and in the amount of public investment in education. This finding is consistent with the idea that governors target resources to meet increased political demand resulting from more Hispanics serving as mayors and school board officials. It is also in line with the fact that schooling is first order concern for children of undocumented migrants (Amuedo-Dorantes and Lopez 2015; Brabeck and Xu 2010).

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 rich 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, including electing greater numbers of Hispanics to public office. Moreover, because of our rich data on state governors, we are able to shed light on the precise political mechanisms that motivate a governor's distributional decisions. We show, 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. Together, the results uncover the unique role of legal status, as distinct from naturalization and enfranchisement, in driving public resource allocation.

Finally, the paper adds to our understanding of the process of political selection. This scholarship began in earnest after Osborne and Slivinski (1996) and Besley and Coate (1997) articulated an alternative framework to that put forward by Downs (1957) for understanding policy credibility, convergence and candidate selection (Besley 2005). 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. 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. They find that stronger political representation of these groups leads to an allocation of public resources that positively affects those groups. 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 and on what grounds.

The rest of this paper proceeds as follows: Section 2 discusses the historical background of the IRCA as well as the demographic characteristics of its applicants. 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, 7 and 8, 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 passed through either the Senate or the House but none was able to win complete approval until the 99^{th} Congress passed the

4. By way of example, consider the results of the 2016 midterm elections: eight percent of that body comprised representatives of Hispanic origin even though Hispanics make up around 18 percent of the United States population (Manning 2018; *Facts for Features: Hispanic Heritage Month* 2017).

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 Legally Authorized Workers (LAWs, also knows as "pre-82s") 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)). LAW 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 LAW 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 were granted permanent residence.[7] Figure 1 shows

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).

the geographic distribution of IRCA applicants at the county level in 1992 for those counties for which data is available. As shown, the majority of the applicants come from the states of California (970,895), Texas (351,646), Illinois (125,399), Arizona (70,488) and New Jersey (29,012). Figure A.1 in the Online Appendix shows the time trend of the stock of IRCA migrants while Figure A.2 shows that undocumented migrants applied for legal status in approximately 330 counties.[8]

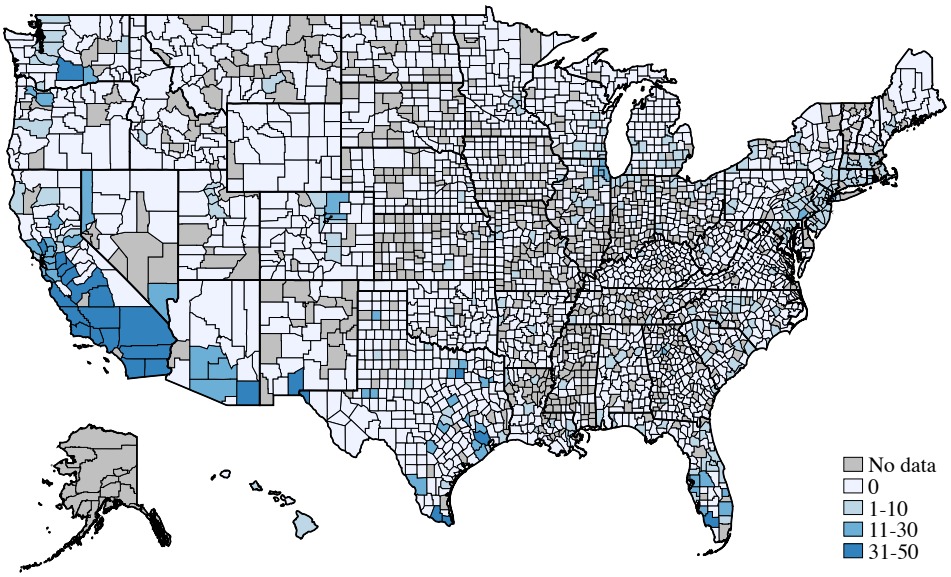

Figure 1
Number of legalized migrants per 1,000 county inhabitants in 1992

Figure A.3 in the Online Appendix presents 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 legalized migrants. 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).[9] In fact, median take-home

8. Of the 3,141 counties and statistical equivalent entities in the United States, our dataset includes information on 2,760 of them. Moreover, we do not observe every county in every year because the Annual Survey of Local Government Finances from which the inter-governmental revenue data is obtained does not survey every county in every year. This is the case for around 30 percent of the counties in the sample. For this reason, we linearly interpolate the revenue data. Restricting the analysis to only those counties that we observe throughout the entirety of the sample, however, makes no difference to the results as shown in Column 3 of Table A.3.

9. The National Longitudinal Survey of the U.S. Bureau of Labor Statistics suggests the poverty

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.

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 900,000, 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). 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

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.

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.

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 children, 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 family members, aside from spouse and children, who were citizens pre-IRCA.[10] 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

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

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:

> . . . [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.[11] In the treated counties (i.e. those counties that received at least 1 application for legal status), this value ranged from as little as .04 to as many as 50 applications per 1,000 inhabitants. By 1992, IRCA counties had, on average, 5,844 legalized migrants residing in them. We obtain this information from Baker (2015) who, in turn, takes it from the Immigration and Naturalization Service (INS).

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: Information on county revenues and expenditures are taken from the U.S. Census Bureau, Annual Survey of Local Government Finances and Census of Governments yearly series.[12] We use per capita inter-governmental revenues (IGR) from state governments to local governments (counties, cities, municipalities aggregated to the county) as our primary dependent variable.[13] 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."[14] Section A .1 in the Online Appendix details the budget

11. 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 Columns 5 and 6 of Table A.3 in the Online Appendix. As such, we use the term "applicants" and "legalized" interchangeably.

12. This database spans fiscal years 1957, 1962, 1967, and, on a yearly basis, from 1970 to 2006.

13. On average, counties in the sample receive USD 16 million in inter-governmental revenue per year, an amount which comprises approximately 30 percent of all local government revenue (shown in Figure A.4 in the Online Appendix). Moreover, Table A.7 in the Online Appendix details what is and what is not included in the inter-governmental revenue and gives an indication as to what types of local activity these revenues support.

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

making process in US states and brings to light the unique powers that governors have in formulating and allocating it.

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 governor veto power and the partisan make up of state legislatures which we digitized, respectively, from the historical archives of the Book of States and the National Conference of State Legislatures.

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 600 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 Inter-Governmental Revenue

5.1 The Evolution of IGR: Raw Data

Our aim is to understand the impact of documenting undocumented migrants on the distribution of inter-governmental revenue from state to local governments. The primary

identifying assumption of our econometric model is that inter-governmental 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 2 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.[15]

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

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

Where $y_{c,t}$ is per capita inter-governmental revenue from state to local governments (in 1999 USD) in county c in year t; T_c is a binary variable set to one if a county received one or more applications for legal status post-1986 and zero otherwise; 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 2 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.

15. Table A.1 in the Online Appendix shows summary statistics of the main variables in our study according to whether they are in treated or non-treated counties. As shown, the two county types are not the same, though when one conditions on state fixed effects, some differences disappear. Importantly, however, Figure A.5 in the Online Appendix demonstrates that, while level differences exist between the two county types, the IRCA did not lead to differential trends in such characteristics as population, income, unemployment, tax revenue or educational attainment. 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.

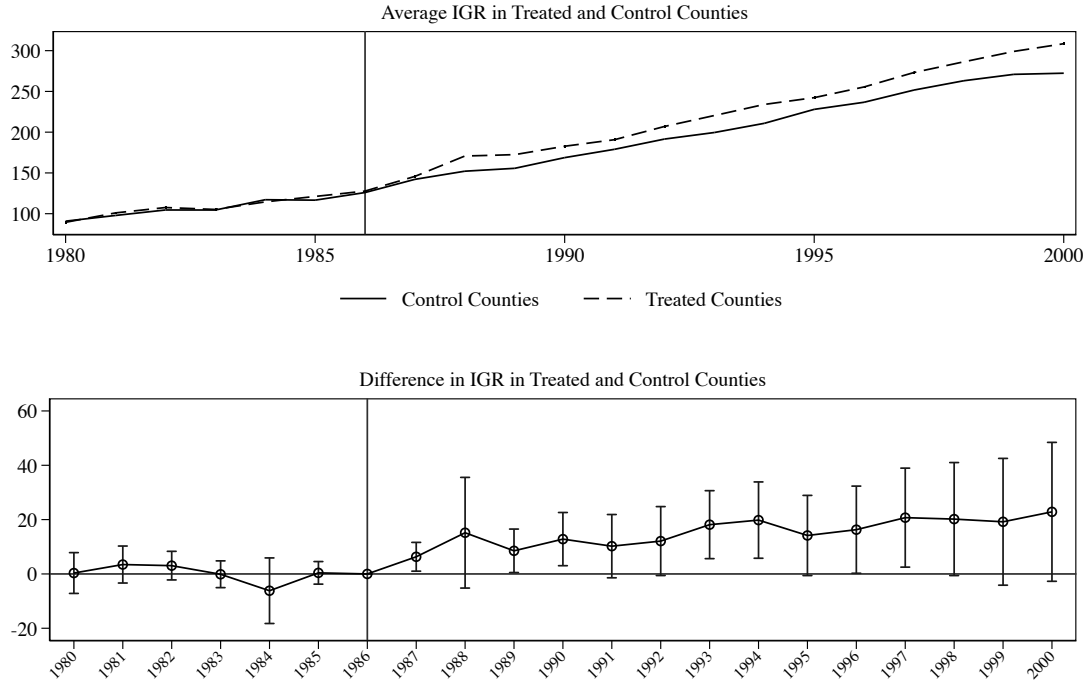

Figure 2
Parallel trends

Note: The top figure plots the mean value of per capita inter-governmental revenue (in USD1999) in treated and control counties. The bottom figure plots the regression coefficient on a treatment indicator 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 95 percent.

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.

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

Where $ln(y)_{c,t}$ is the natural log of per capita inter-governmental 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. The treatment indicator, T_c, 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 legalizations and transfers, including governor specific characteristics or other state-year-level political or economic shocks.[16] 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. 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 and population.[17]

The trends shown in the raw data are borne out in the regressions. Panel A of Table 1 shows our results across a number of variations of the model shown in equation 2 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 9.5 percent more in per capita transfers than other counties. IRCA counties have, on average, 50,605 Hispanics; a 9.5 percent increase in inter-governmental revenue as a result of legalization thus implies an increase in state aid by USD 1.53 million, or USD 30 per Hispanic per year.[18]

16. The Reagan administration passed a tax reform in 1986 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. Section A .4 in the Online Appendix provides evidence that SLIAG funds do not confound our results.

17. We cluster standard errors at the county because this is the level of treatment variation. The results, however, are robust to more conservative clustering as shown in Table A.8 in the Online Appendix.

18. If we assume the gains are spread evenly among the population in a county, a 9.5 percent increase translates into an increase of USD 17 per person per year. Assuming the gains are concentrated among the legalized migrants alone, the coefficient implies an increase of USD 267 per migrant per year.

Table 1
Inter-Governmental Revenue on IRCA Legalizations

	Log of Inter-governmental Revenue (per capita)						
	(1) Baseline Effect	(2) Drop Top 5 Treated States	(3) Drop 4 Border States	(4) Pop < 430,000	(5) Propensity Matching	(6) Linear Trends	(7) Differential P_{92} Effect
Panel A. Treatment Indicator							
Treatment × P_{86}	0.0956***	0.105***	0.108***	0.0791***	0.109***	0.0514*	0.0802***
	(0.0230)	(0.0258)	(0.0209)	(0.0244)	(0.0344)	(0.0277)	(0.0217)
Treatment × P_{92}							0.0297
							(0.0264)
Panel B. Treatment Intensity							
Log Legalized$_{92}$ × P_{86}	0.0421***	0.0675***	0.0576***	0.0304**	0.0532***	0.0293*	0.0335***
	(0.0140)	(0.0212)	(0.0139)	(0.0152)	(0.0206)	(0.0162)	(0.0124)
Log Legalized$_{92}$ × P_{92}							0.0165
							(0.0163)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	No	No	Yes	No	No
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	No	Yes	Yes
Log 1980 Hisp. Share × Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year Linear Trends	No	No	No	No	No	Yes	No
Observations	39,510	36,689	33,984	37,943	12,042	39,510	39,510
Number of Counties	2,053	1,898	1,784	1,984	604	2,053	2,053

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 log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) 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 poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In Panel B of Table 1, we estimate the same parameters but using a measure of treatment intensity as specified in equation 3. Here, $LLEGAL_{c,92}$ is the natural log of the cumulative number of IRCA applicants per 1,000 county inhabitants (plus one) in county c fixed to its 1992 value and this variable is interacted with P_{86} which indicates time periods before and after 1986. The parameter of interest, β, can be interpreted as the elasticity of state transfers with respect to the number of per 1,000 capita applicants. All other parameters are defined as before.

$$ln(y)_{c,t} = \beta_0 + \delta_c + \zeta_{st} + \sum_{j=1980}^{2000} \gamma_j [LHISP_{1980} \times D_j^t] + \beta [LLEGAL_{c,92} \times P_{86}] + \Theta \cdot \mathbf{X_{c,t}} + \epsilon_{c,t} \tag{3}$$

Using this measure of treatment intensity, the magnitude of the effect is comparable. The coefficient in Column 1 of Panel B implies that a one percent increase in the number of per capita legalizations in a county is associated with an increase in per capita transfers of approximately 0.04 percent. Within treated counties, this elasticity translates into an increase of approximately 6 percent in per capita inter-governmental revenue when moving from the 50^{th} to the 75^{th} percentile of treatment intensity, a move that amounts to USD 20 per Hispanic per year. Because treatment intensity is a more precise measure of treatment, equation 3 is our preferred specification and henceforth we will use the log of the cumulative number of per 1,000 capita IRCA applicants (plus one) 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. We begin by re-estimating the parameters of the model in a sample that omits the five most treated states which, in per capita terms, correspond to California, Arizona, Florida, New Jersey and Nevada. As shown in Column 2, the results hold. In Column 3, we run the model on a sample that excludes the four border states to ensure the results are not confounded by the IRCA's provision for additional funding for border security and, as shown, no such confound exists. 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 4, on a sample restricted only to those counties with populations less than average county population size and obtain a precise coefficient, albeit of slightly smaller magnitude, suggesting that the effect is not driven by very populated counties.

In Column 5 we use propensity score matching to generate a more comparable control group and running the model in this matched sample returns results very similar

to the baseline.[19] In Column 6, we rerun the baseline specification, adding to it county specific linear time trends. The idea here is to capture any differential trends with respect to the outcome variable that might arise over time for each county, 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 7, 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.[20] As shown, there is no significant increase to the baseline post-1992, consistent with the patterns shown in the raw data in Figure 2. 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 7 strongly suggest that legal status, as distinct from naturalization and enfranchisement, is a driving force in the distribution of state transfers.

Further Robustness: Tables A.2 and A.3 in the Online Appendix provide two further sets of robustness checks. In Table A.2, we probe the strength and nature of the relationship between legalization and transfers by including a long-difference estimation, using a county's 1980 population to carry out the per capita calculation of transfers and controlling for a range of additional county characteristics, including per capita tax revenue as a further measure of robustness against the 1986 tax reform and the share of the county population that is white, Black and Hispanic. We also investigate whether the relationship between legalization and transfers is linear or quadratic and find no evidence for a non-linear effect. In Table A.3, we undertake a number of checks concerning the data. These include replicating the baseline result using a $log - linear$ specification to demonstrate that the results are insensitive to the logarithmic

19. We used nearest neighbor matching and changing the number of neighbors up to 5 does not change the result. Figure A.6 in the Online Appendix shows the trends in inter-governmental revenue in treatment and control counties in the matched sample using nearest neighbor matching. The characteristics on which we generated the propensity score are county income, population, crime, tax revenue, poverty rate and unemployment in 1980. We drop state-year fixed effects to allow for the possibility that the best-matched control county for a given treated county may, in fact, lie in a different state.

20. 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.

transformation of the legalization data,[21] restricting the analysis to only those counties that we observe throughout the entirety of the sample, excluding very small or very large counties and reproducing the results using an alternative measure and source for legalization.[22] That the coefficient on legalization remains positive, precise and stable across all these specifications and subsamples indicates that the relationship between immigrant legalization and the distribution of state aid is in fact a robust one. Finally, we test for pre-trends using the continuous measure of treatment, both with and without the logarithmic transformation. The results are shown in Figure A.8 in the Online Appendix and confirm that differential pre-trends are not driving the results.

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.

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 A.5 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

21. We choose a $log - log$ specification because (a) the legalization variable is unevenly distributed and (b) an elasticity is easier for interpretation.

22. Specifically, we obtain information on the number of applicants as well as the number actually legalized under the IRCA from the Legalization Summary Tapes of the INS.

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.[23] 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 increase in transfers, we might expect the overall *level* of transfers to increase but there would be no reason, ex-ante, to expect any change in the amount of *per capita* transfers. That per capita transfers are a function of the number of legalizations in a county seems to suggest that the transfer activity we observe is more than a mechanical increase that might arise out of a transfer formula based on population considerations.

5.4 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.

First, there are four states in the sample that were unaffected by the IRCA. These are North and South Dakota, Vermont and Wyoming. As a first step, we run the baseline specification dropping state-year fixed effects (and include year fixed effects instead) so as to allow for comparisons of counties across state borders. This produces the results shown in Column 1 of Table 2. Next, we run the same specification but use only the treated counties from treated states and the control counties from the four control states. The idea here 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 no effect when we compare treatment and control counties from different states. That the coefficient on legalization in Column 2 of Table 2 is larger than that in Column 1 suggests that the treatment effect is not coming at the expense of control counties.

To understand 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

23. Later in the paper (in Figure 5), we utilize Census of Government expenditure data to better understand the impact of legal status on various categories of local expenditure and find that the IRCA does not have a significant impact on local welfare expenditure.

Table 2
SUTVA, the IRCA and Already Legal Migrants

	Log of Inter-governmental Revenue (per capita)		
	(1) Full Sample	(2) Control States	(3) Legalized v Already Legal Hispanics
Log Legalized$_{92}$ × P$_{86}$	0.0417***	0.0630**	-0.0161
	(0.0152)	(0.0271)	(0.0238)
Log Legalized$_{92}$ × P$_{86}$ × High Legal			0.0657*
			(0.0370)
Control Variables	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	No	No	Yes
Log 1980 Hisp. Share × Year Dummies	Yes	Yes	Yes
Observations	39,516	7,204	39,510
Number of Counties	2,053	365	2,053

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log Legalized$_{92}$ is the log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) fixed to its 1992 level. P_{86} is a binary variable that indicates time periods before and after 1986. In Column 1 we exploit the full sample. In Column 2 we use only treated counties from treated states and the control counties from the four control states in the sample. High Legal is an indicator that is 1 if the share of already legal Hispanic migrants in a county is greater than average and zero otherwise. Column 3 includes all lower order interactions. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

the number of per 1,000 capita legalized migrants at the state level. The results are presented in Table A.4 in the Online Appendix and indicate that state revenue from sales taxes significantly increase as a function of IRCA documented migrants in a state. This is consistent with the idea that legal status provides a boost to productivity, consumer spending and earnings (Lynch and Oakford 2013).

5.5 Legalized and Already Legal Hispanics

Finally, we investigate the relationship between legalized and already legal Hispanic migrants in driving targeted resource allocation. If, as we posit, the legalized attract resources on account of their ability to mobilize already legal Hispanic migrants, then we would expect legalization 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 is greater or less than average. In Column 3 of Table 2, we interact this indicator with the legalization variable and, in line with our thinking, we find that the effect of legalization on transfers is driven by those counties with larger shares of already legal Hispanic migrants, underscoring the capacity of legalized migrants to work through existing Hispanic networks to attract resources.

6. Political Economy Mechanisms: Discretionary Political Transfers

The analysis in the preceding section demonstrated that the relationship between immigrant legalization and the distribution of state finances is a robust one. In this section, we investigate to what extent the relationship 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 here 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 wide 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 by almost 80 percent when the governor is a Republican. In Column 2 we test whether state governors give more to counties that are politically aligned with them, in the sense that a given county's political leaning (measured by its Presidential election results) align with those of the party of the governor.[24] As shown, state aid increases to a county affected by the IRCA regardless of whether the county's political leaning is aligned with that of the governor. These results confirm that the distributional impact of the IRCA is driven more by political factors at the state level.

24. We use Presidential election data as a proxy for Gubernatorial electoral returns because the Gubernatorial election data is available only as of 1990, in the post-treatment period. A county's Presidential election outcomes do follow its Gubernatorial outcomes quite well as shown in Figure A.9.

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, 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 3 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 inter-governmental 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.

Table 3
Legalization and Political Heterogeneity

	Log of Inter-governmental Revenue (per capita)							
	(1) Governor Republican	(2) County Aligned	(3) Lame Duck	(4) Election Year	(5) Tightest 5%	(6) Governor Veto	(7) Legislative Alignment	(8) Party of Legislature
Log Legalized$_{92}$ × P$_{86}$	0.0686***	0.0378**	0.0439***	0.0800***	0.00406	-0.00532	0.0227*	0.0459***
	(0.0159)	(0.0159)	(0.0143)	(0.0214)	(0.0134)	(0.0236)	(0.0136)	(0.0146)
Log Legalized$_{92}$ × P$_{86}$ × ...	-0.0534***	0.00775	-0.0507**	0.0664**	0.0553***	0.0507*	0.0489***	-0.0416
	(0.0163)	(0.0139)	(0.0223)	(0.0259)	(0.0155)	(0.0275)	(0.0176)	(0.0258)
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Log 1980 Hisp. Share × Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39,119	39,078	39,510	36,937	39,510	31,778	39,510	30,390
Number of Counties	2,053	2,053	2,053	2,052	2,053	2,026	2,053	1,949

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log Legalized$_{92}$ is the log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) 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 8 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. County Aligned is 1 if the county's election results in the most recent Presidential election are aligned with the party of the governor and 0 if not. 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. Tightest 5% is 1 if the outcome of the Presidential election in a given state in a given election cycle was more competitive (defined as the absolute difference between votes for the Republican and Democratic candidate) than those in the top 5^{th} percentile of the competitiveness distribution and 0 if not. Governor Veto is 1 if the Governor enjoys line-item veto power over the budget and 0 if the governor has no veto or no line-item veto. Legislative Alignment is 1 when the party of the Governor is aligned with the partisan majority of the state legislature and 0 when it is not. Party of Legislature is a dummy when both houses of the state legislature have a Republican majority (0) or Democratic majority (1). All regressions include all lower order interactions. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

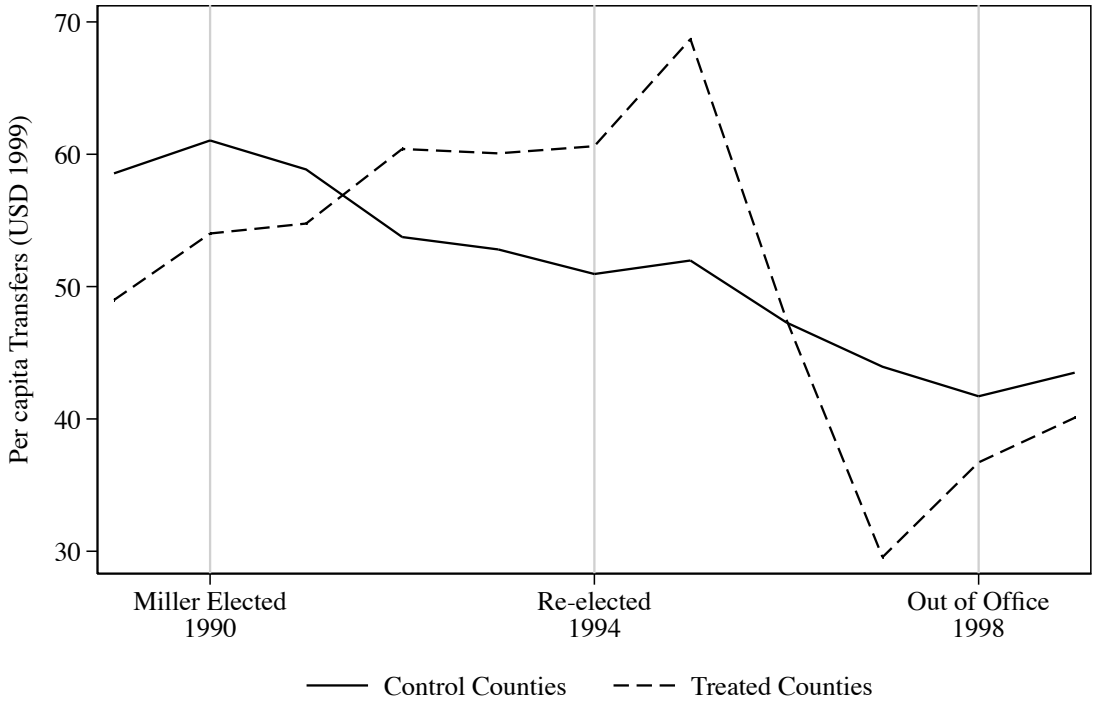

Figure 3
IGR in Georgia under Governor Zell Miller

Columns 3 and 4 in Table 3 investigate these questions. In Column 3, we interact the legalization variable 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 4, we analyze sensitivity to the gubernatorial election cycle. Here, the legalization 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 inter-governmental 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.

Electoral Competition: Next, we examine the sensitivity of transfers to electoral competition. The logic is similar to those of term limits. If the transfers we observe are discretionary, we would expect governors to allocate even more resources to counties affected by the IRCA in those states whose previous electoral races were more competitive. To test this hypothesis, we identify states with tight electoral races—as defined by win margins in Presidential elections that are tighter than the tightest 5 percent of the

distribution—and examine the effect of legalization in these states as compared to those that were less competitive.[25] As shown in Column 5 of Table 3, counties with a greater share of legalized migrants receive even more resources when their state experiences greater electoral competition. Table A.5 in the Online Appendix shows similar results for different measures of tightness and confirm a similar pattern.

Veto Power: Governors exercise various powers over the budget-making process in a given state. Here, we focus on one of these powers: the line-item veto, which provides governors with the power reduce or even nullify individual expenditure items within appropriations bills and, in some cases, within policy bills (Kousser and Phillips 2012). This accords with a range of theoretical and empirical literature that documents the growing importance of the state executive branch relative to the legislative branch in setting state priorities in general (Clych and Lauth 1991) and in shaping the state budget in particular (Kousser and Phillips 2012; Barrilleaux and Berkman 2003).

In Column 6 of Table 3, we compare the effect of legalization on state aid in those states where the governor enjoys line-item veto power over the state budget to those where he enjoys no such power. As shown, the effect of legalization on IGR is driven by counties in states where the governor has more control over the state budget.[26]

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 the sensitivity of transfers to the relationship between the state governor and the state legislature in Column 7 of Table 3. 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. The result indicates that the baseline effect of legal status on state transfers doubles when there is partisan alignment between the executive and legislative branches of state government, further underscoring the politically discretionary nature of these transfers.

In Column 8 we directly test the influence of the partisan majority of the state legislature on our results. To this purpose, we interact the legalization variable with a dummy that indicates whether both Houses of the state legislature enjoy a Republican majority (0) or a Democratic majority (1). As shown, the partisan composition of the

25. The win margin is calculated as the absolute value of the difference between votes for the Democratic and Republican candidate in each Presidential election from 1980 to 2000.

26. Specifically, we compare states where the governor has line-item veto power with two-thirds majority of the state legislature needed to override (the majority of cases) to those states where the governor has either a simple veto, but not line-item veto, or no veto power at all. Table A.5 in the Appendix investigates heterogeneities along different degrees of veto power override. The results are unchanged.

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, and Column 7, which illustrates the importance of partisan alignment between the state legislature and the state governor.

Together, the results in this section point to two noteworthy conclusions: First, in the context of immigrant legalization, we have shown that the state executive is more responsive to the incentives created by amnesty than the state legislature. Second, the various 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.[27]

7. Political Economy Mechanisms: Hispanic Political Empowerment

In this section, 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. To this end, 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.

7.1 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,

27. Table A.6 in the Online Appendix investigates the question of legalization and re-election. In this regard we find that legalization at the county level positively predicts the county vote share, in Presidential elections, going to the party of the incumbent state governor.

in Figure 4. The 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. Together, the results point to the role that immigrant legalization plays in stimulating meaningful political activity at the local level.

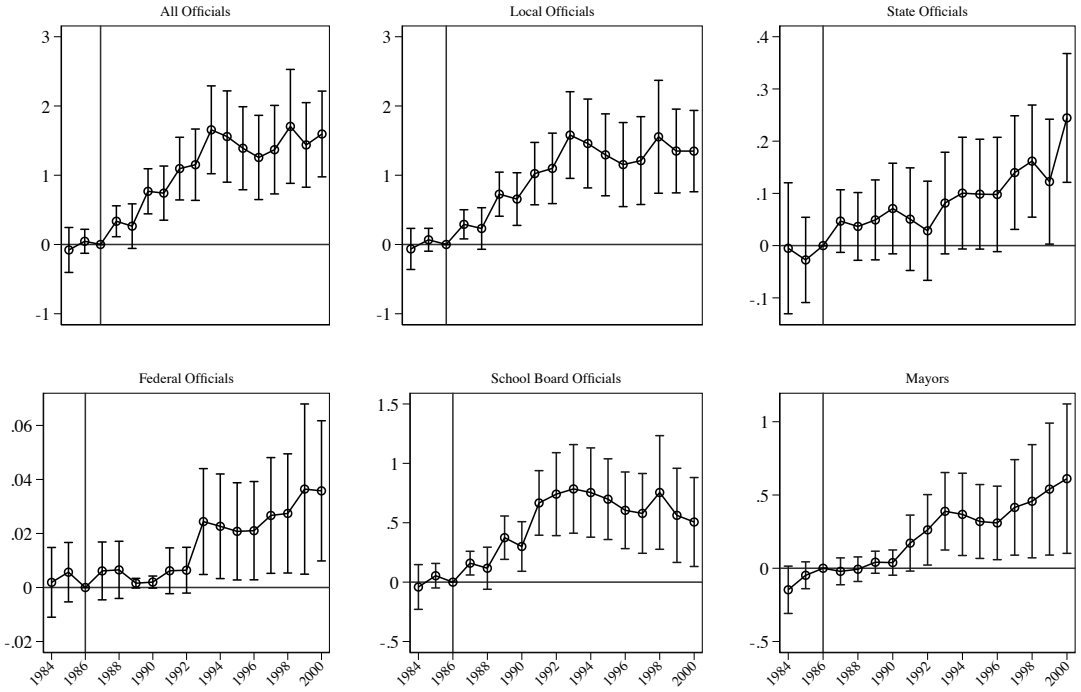

Figure 4
Legalization and Hispanic Public Officials

Note: This graph plots the regression coefficient on the log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) 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 poverty and unemployment rates, log of population and log of income as well as county and year fixed effects. Standard errors are clustered at the county level. Confidence intervals are drawn at 95 percent.

Sensitivity Analysis and Hispanic Networks: 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 legalizations. Column 1 is the baseline. It suggests that counties affected by the IRCA experience an increase of 2.4 Hispanics entering public office after 1986, a 27 percent increase on the sample mean. Around 40 percent of the Hispanic officials in the sample hail from counties in Texas alone. To ensure the results are due to immigrant legalization and the political empowerment it represents, as opposed to some other Texas-specific effect, we omit, in Column 2, Texas from the estimation. As shown, the results are very similar to the baseline. In Column 3, we restrict the sample to counties with less than average population size to again ensure that the results are not driven by a handful of migrant-friendly localities and in Column 4, we use propensity score matching in order to identify an even more comparable control group. As shown,

the results hold across these specifications. Column 5 includes county-specific linear trends. The positive and precisely estimated coefficient thus identifies the effect of legalization on Hispanic officials even if treatment and control counties followed non-parallel trends with respect to the evolution of Hispanic officials elected to office. Finally, in Column 6 we interact both measures of treatment with the pre-existing size of the Hispanic community. In both cases, the interaction is positively and precisely estimated, suggesting that the effect of immigrant legalization on Hispanic political selection is stronger in counties with larger Hispanic communities. This provides further evidence that legalization led to a flourishing of political activity within Hispanic communities of mixed legal status, political activity which state governors tried to capture via targeted resource allocation.

Table 4
Hispanic Officials and the IRCA

	Outcome: Number of Hispanic Elected Officials					
	(1) Baseline	(2) No TX	(3) Pop ≤ 430,000	(4) Matching	(5) Linear Trends	(6) Hispanic %
Panel A. Treatment Indicator						
Treatment × P_{86}	2.363***	2.284***	1.285***	2.451***	1.865***	5.839***
	(0.625)	(0.712)	(0.492)	(0.689)	(0.551)	(1.663)
Treatment × P_{86} × Log Hisp. Share 1980						1.595***
						(0.549)
Panel B. Treatment Intensity						
Log Legalized$_{92}$ × P_{86}	0.956***	0.968***	0.537**	0.913***	0.648***	2.099***
	(0.265)	(0.295)	(0.213)	(0.277)	(0.214)	(0.755)
Log Legalized$_{92}$ × P_{86} × Log Hisp. Share 1980						0.623**
						(0.315)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County-Year Trends	No	No	No	No	Yes	No
Log 1980 Hisp. Share × Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,532	1,912	3,568	3,100	4,532	4,532
Number of Counties	378	187	311	217	378	378
Mean of Dependent Variable	8.68	7.28	8.15	11.38	8.68	8.68

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 log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) fixed to its 1992 level. P_{86} is a binary variable that indicates time periods before and after 1986. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Candidate Entry and Political Mobility: Sections A .2 and A .3 in the Online Appendix provide additional evidence for the mechanisms that link legalization to Hispanic public officials. First, subsection A .2 addresses the question whether legalization led to increased Hispanic candidates running for office using a limited sample of mayoral candidate information obtained from the Local Elections America Project (LEAP). In this respect, we find that the share of Hispanic candidates entering mayoral races does not increase with legalization. In subsection A .3 we return to the NALEO data and test for a *mobility* effect. That is, 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. Our results suggest that legalization does lead to a significantly higher number of politically mobile officers being elected to office, reinforcing the idea that legalization was significant to Hispanic political mobilization.

7.2 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 purchased data on county level voter turnout in Presidential elections from David Leip's Atlas of US Presidential Elections and regress county level turnout on legalizations. The results are shown in Columns 1 and 2 of Table 5 and indicate that the IRCA not only exerts a positive and significant influence on turnout (Column 1) but that the effect is driven primarily by those counties with larger shares of already legal Hispanic migrants in them (Column 2). Importantly, the regressions include the share of the county population that is white, African-American and Hispanic, each interacted with a post-1986 indicator. Controlling for these shares isolates the effect of the IRCA on turnout from any potentially confounding influences that the IRCA might have had on the voting behavior of white nativist voters or of minority voters, regardless of the extent to which these communities had undocumented migrants in them. That the coefficients of interest are positive and precise support the idea that the IRCA was 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 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 5.4, the additional transfers that IRCA counties received did not come at the expense of non-IRCA counties but rather because of increased tax revenue at the state level generated by the newly legalized themselves. In this sense, the distributional effect did not produce "losers". 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.[28] 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.[29]

28. To identify individuals with negative attitudes towards undocumented migrants, we exploit the "illegal alien" thermometer of the ANES. This question asks people to gage 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 A.12 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".

29. In Section A .5 in the Online Appendix we provide additional evidence, using various other sources of data, that rule out the possibility that governors targeted resources to cater to anti-migrant sentiment.

Table 5
Legalization, Turnout and Anti-Migrant Voting

	County Data		Individual ANES Data	
	(1) Turnout	(2) Turnout	(3) Voted Pres	(4) Voted Gov
Log Legalized$_{92}$ × P$_{86}$	0.00293**	-0.00146	0.0195	0.000308
	(0.00115)	(0.00253)	(0.0172)	(0.0175)
Log Legalized$_{92}$ × P$_{86}$ × High Legal		0.00568*		
		(0.00290)		
Log Legalized$_{92}$ × P$_{86}$ × Anti-Migrant			0.0222	0.00852
			(0.0423)	(0.0274)
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
Log 1980 Hisp. Share × Year Dummies	Yes	Yes	Yes	Yes
Observations	11,538	11,538	4,081	4,460
Number of Counties	2,081	2,081	171	161

Notes: The dependent variable in Columns 1 and 2 is turnout in a given county in all Presidential elections from 1980 to 2000. This data is obtained from David Leip's Atlas of US Presidential Elections. 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. Log Legalized$_{92}$ is the log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) 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 is greater than average and zero otherwise. Anti-Migrant is an indicator that is 1 if a person has negative feelings towards undocumented migrants. The regressions in Columns 2 and 4 include all lower order interactions. County controls include poverty and unemployment rates, log of population and log of income as well as the share of the county that is white, Black and Hispanic, each interacted with P_{86}. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

8. Local Expenditure and Hispanic Outcomes

Finally, we turn our attention from county revenue to county expenditure in an effort to better understand in which areas and, potentially, on which constituents county revenue is spent. Given that the IRCA led to significant increases in Hispanics serving as mayors and school board officials, it is not unreasonable to expect increases in education expenditure at the local level. Moreover, given the demographic characteristics of

the legalized and the fact that the IRCA disallowed them from receiving benefits from programs of financial assistance, we would not expect to increases in welfare expenditure. To investigate the impact of the IRCA on local expenditure, Figure 5 plots regression coefficients on the interaction between legalization 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.

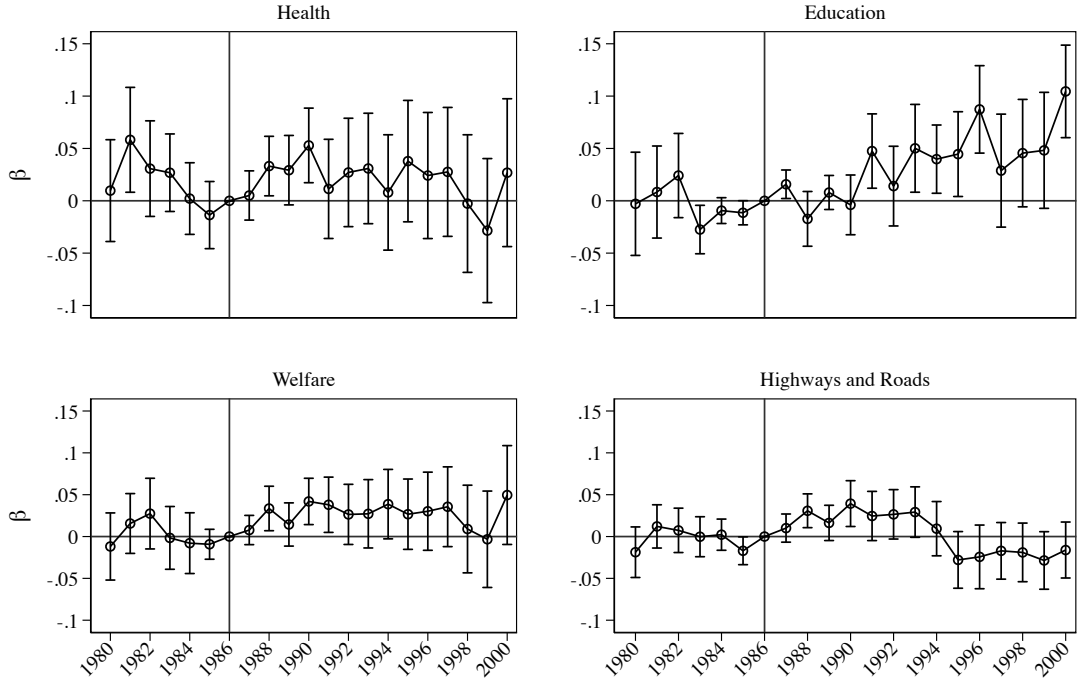

Figure 5
Event study estimates of local expenditure on legalization

Note: This graph plots the regression coefficient on the log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) 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 control for poverty and unemployment rates, log of population and log of income, all aggregated to the county level as well as county and year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.

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 4, 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.

$$H_{i,c,mse} = \delta_c + \psi_{mse} + \sum_{j=1}^{20} \beta_j \left[LLEGAL_{c,1992} \times D^j_{mse} \right] + \epsilon_{c,mse} \tag{4}$$

We run the specification on different samples according to a person's race and plot the corresponding coefficients, β_j, as shown in Figure 6. 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 and the much stronger political representation that these counties receive on account of that status.[30] For Caucasian, African American 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. These results lend further credence to our hypothesis that state politicians targeted resources to Hispanic communities affected by the IRCA in response to the heightened political

30. 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.

mobilization that legal status stimulated. They also underscore the role of local public spending and stronger political representation in linking legal status to improvements in various socio-economic outcomes, in this particular case education.

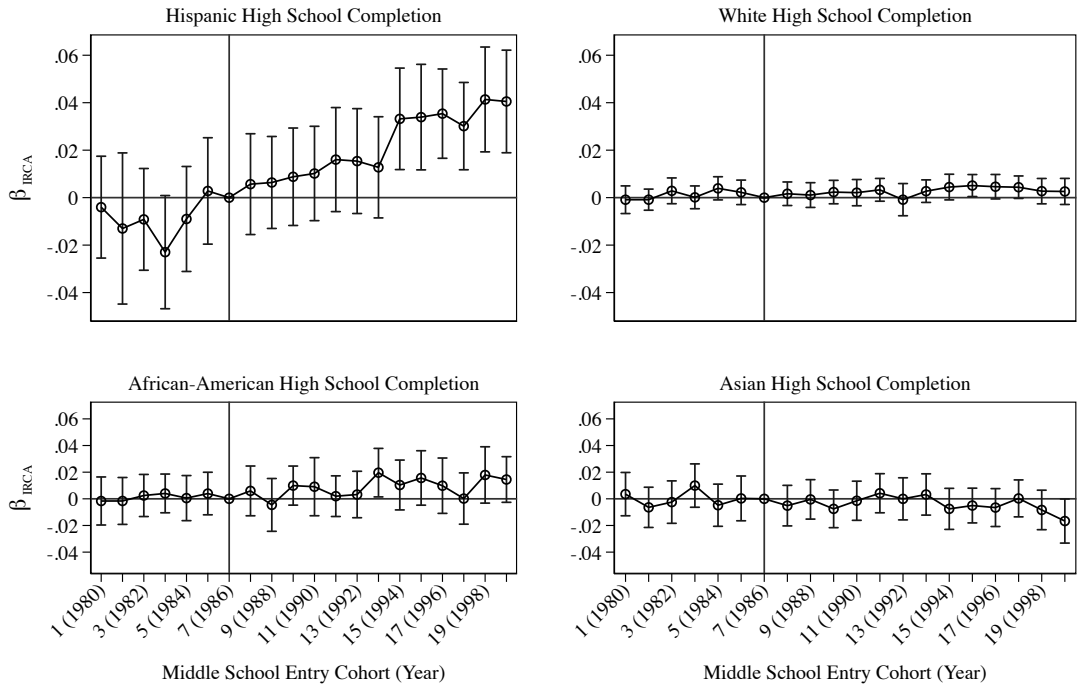

Figure 6
Event study estimates of high school completion on legalization

Note: This graph plots the regression coefficient on the log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) fixed to its 1992 level interacted with middle school entry cohort dummies as shown in equation 4. 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.

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 four main lessons which can inform current debates on immigration reform.

First, in documenting the distributional effect of immigrant legalization, we found that the transfers afforded to IRCA-affected counties are funded as a result of increases in revenue at the state level generated from new, legalized migrants in the economy. Contrary to some popular notions, we did not find that the funding increases came at the expense of counties unaffected by the amnesty. This is in line with other research that highlights the role of migrants, documented or not, as net contributors to the economy.

Second, in trying to understand why legal status affects the distribution of public finances, we uncovered novel political economy forces at work. On the one hand, we presented evidence that the distributional effect is responsive to the political context of the state governor. That is, governors transfer more resources to IRCA-affected counties when they are eligible for re-election, face political competition, have more control over the state budget and when they are politically aligned with the state legislature. These results are especially noteworthy because they demonstrate that the relationship between legal status and the distribution of public resources is one of discretionary political choice rather than one borne of welfare considerations. Indeed, we found no evidence of additional welfare expenditure as a result of the IRCA.

On the other hand, we argued that governors targeted resources to IRCA affected counties not so much to win the votes of the newly legalized but to capture political gains arising from entire communities that were politically transformed as a result of a landmark legalization policy. To substantiate this claim, we presented a number of pieces of evidence. First, 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. This indicates that the legalization of immigrants, as distinct from their enfranchisement, is a driving force in the distribution public resources. Second, 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. The effect of immigrant legalization on Hispanic selection to public office is augmented when it is

interacted with the size of the pre-existing local Hispanic community, pointing to the capacity of legal status to politically empower Hispanic communities of mixed legal status. These findings are of particular importance when one considers inequality in political representation among ethnic minorities that characterizes much of US politics.

Third, our analysis indicates that the IRCA led to significant increases in resource allocation and political participation without triggering anti-immigrant sentiment. Counties affected by the IRCA experience significant increases in voter turnout, an effect driven primarily by counties with larger shares of already legal Hispanic migrants in them. By contrast, we found no increases in turnout among individuals with negative views towards undocumented migrants, suggesting that nativist backlash played little to no role in the political and distributional response to immigrant legalization.

Fourth, we found that county expenditure in education increases significantly in IRCA-affected counties, leading to improvements in Hispanic high school completion. Importantly, these improvements were only discernible as of 1994, long after legal status was granted but soon after these counties experienced increases in the number of Hispanic individuals entering public office and in the amount of public investment in education. This finding underscores the important role that political representation and local public expenditure play in linking legal status to improvements in socio-economic outcomes of individual migrants.

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. Perhaps the most important contribution of the study, therefore, is that is throws light on the economics participation. Offering legal status not only leads to various 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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FOR ONLINE PUBLICATION

A.1 Additional Figures

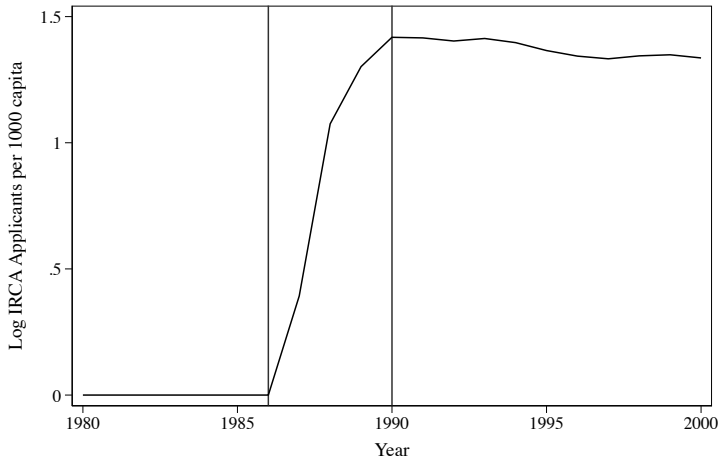

Figure A.1
Stock of IRCA applicants in treated counties on a log scale

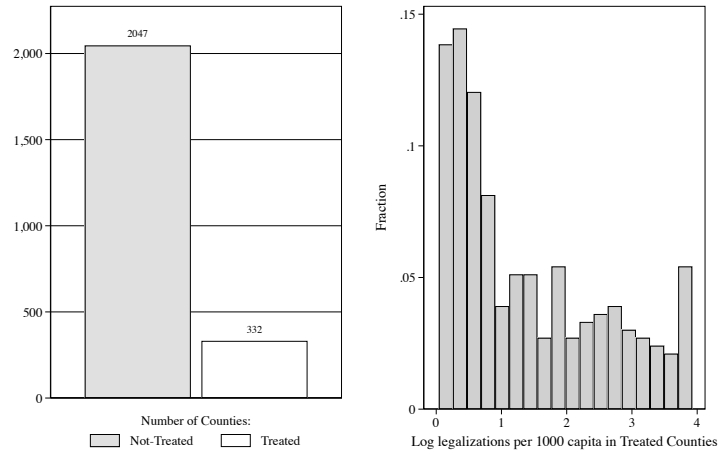

Figure A.2
Distribution of IRCA applicants

Note: This figure shows the distribution of IRCA applicants in 1992. The left panel indicates the number of treated (332) and control (2,047) counties whereas the panel on the right plots the distribution of treatment within treated counties. As of 1992, IRCA counties had 5,844 applicants residing in them.

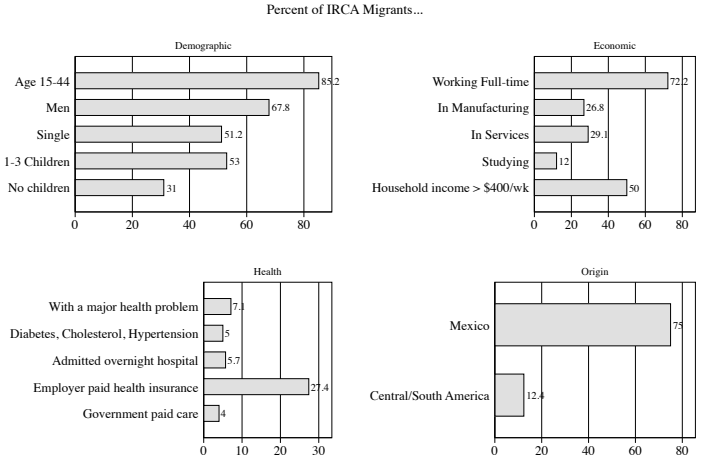

Figure A.3
Socio-economic characteristics of the IRCA applicants

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

A .1. The budget-making process

Our main contention is that state governors use their budgetary powers to allocate more resources to newly documented migrants in the hope of winning their future vote. 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.[31]

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.

Importantly, governors enjoy a number of powers over the budget-making process, including being able to spend unanticipated funds without legislative approval or to withhold appropriations from agencies within the executive, legislative or even judicial branches of government. Crucially, governors enjoy various forms of veto authority

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

over the state budget. Depending on the state, governors have the authority to either veto the entire budget or specific line-items of it, a power which gives them great leverage over the priorities of the budget. Later in this paper, we document heterogeneity in our results depending on the extent of veto power a governor enjoys.

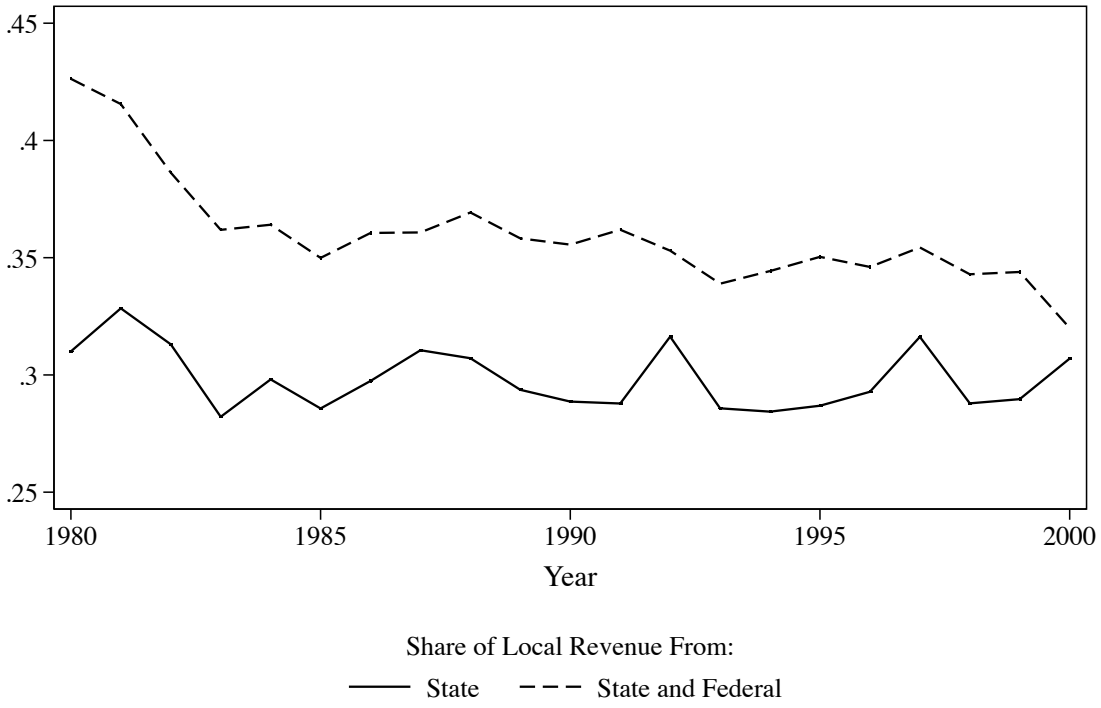

Figure A.4
Sources of local government revenue

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.

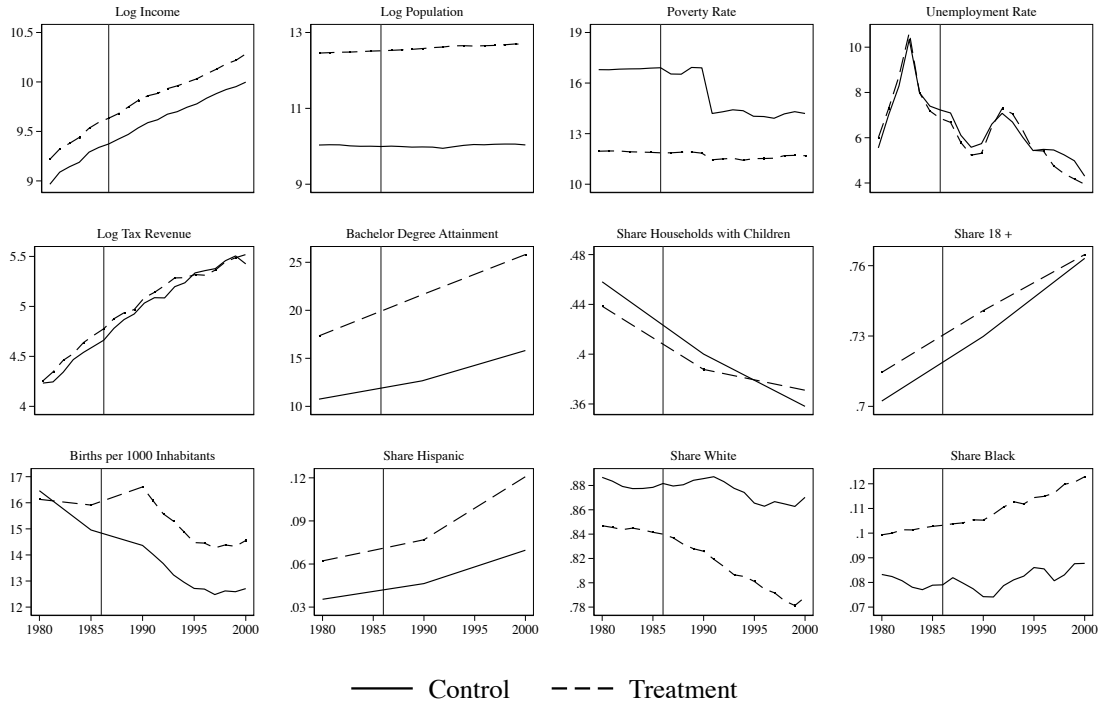

Figure A.5
Trends in county socio-economic characteristics

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.

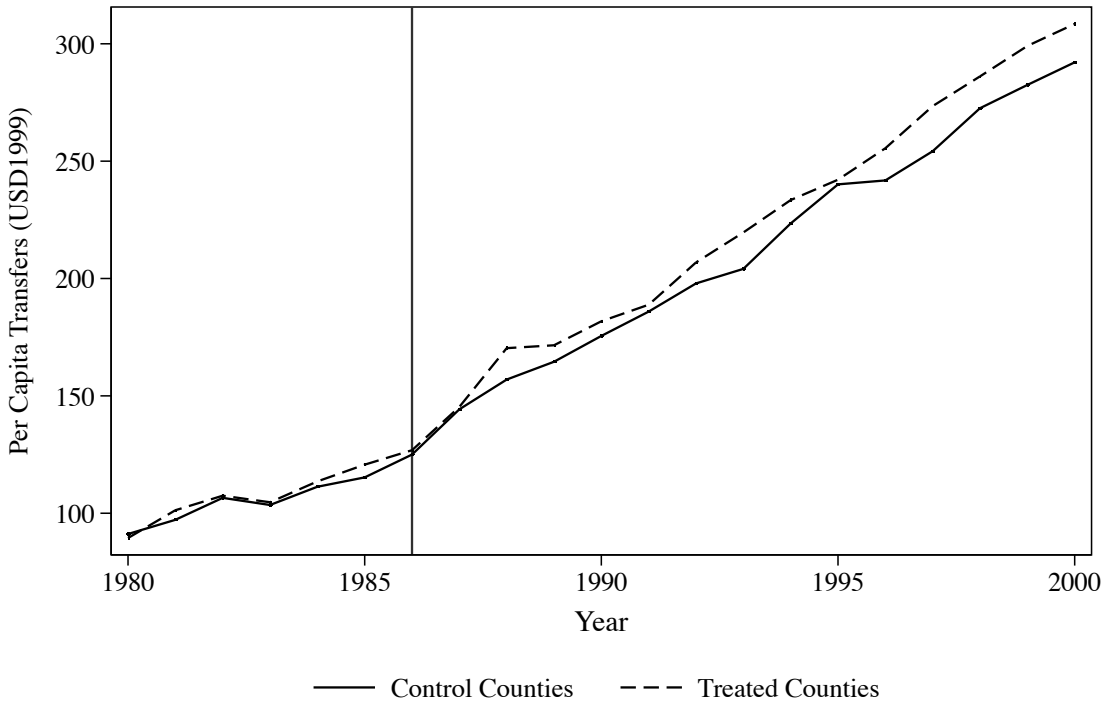

Figure A.6
Evolution of inter-governmental revenues in matched sample

Note: This graph compares per capita inter-governmental revenues (in 1999 USD) in those counties that never received applications for legal status (control) with those counties that did receive applications for legal status (treated) in a sample of treated and control counties matched on the basis of propensity scores using the nearest neighbor. The county characteristics on which we base the propensity score matching are county income, population, crime, tax revenue, poverty rate and unemployment in 1980.

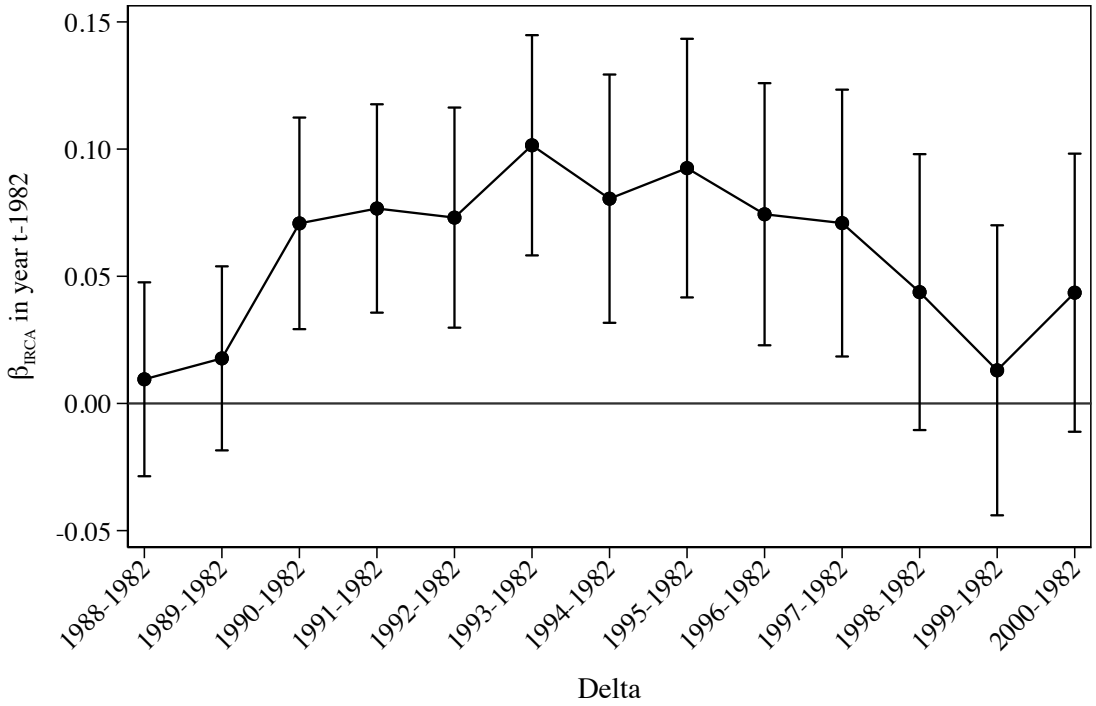

Figure A.7
First-difference coefficient estimates

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 natural log of the cumulative number of IRCA applicants per 1,000 county inhabitants (plus one) fixed to its 1992 level interacted with an indicator for time periods before and after 1986. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. 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.

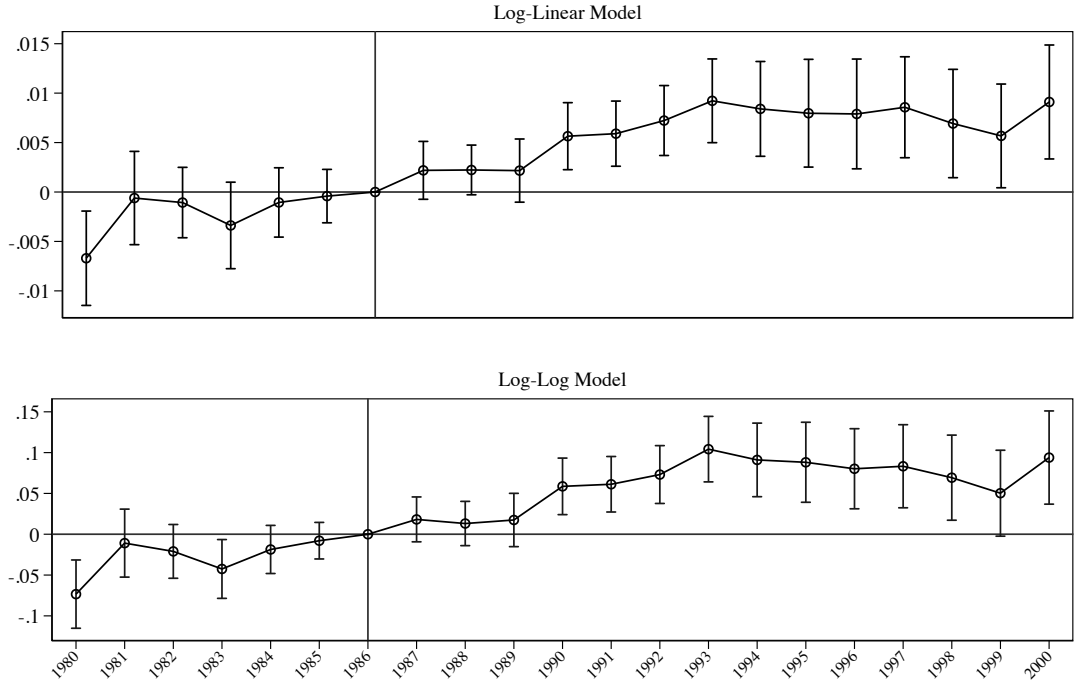

Figure A.8
Testing for Pre-trends

Note: This graph plots the the regression coefficient on legalizations per 1,000 capita in 1992 on a linear (top panel) and log (bottom panel) scale when it is interacted with year dummies. The dependent variable is the log of per capita transfers from state to local governments (in 1999 USD). County and year fixed effects are included in the estimations. Standard errors are clustered at the county level and confidence intervals are drawn at 99 percent.

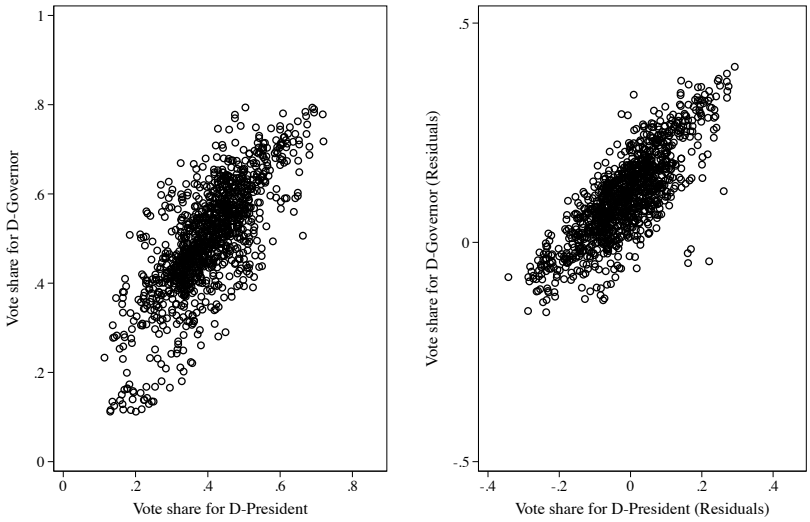

Figure A.9
Presidential and Gubernatorial election results

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 variables once state-year fixed effects and county fixed effects have been accounted for.

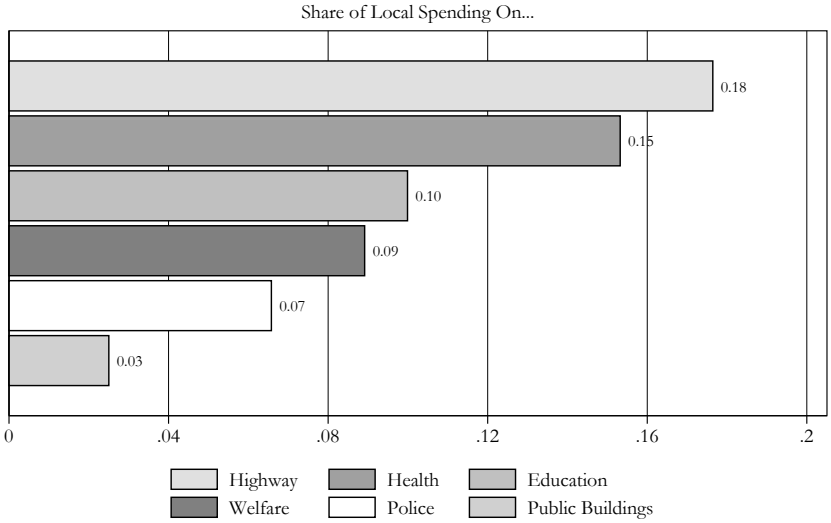

Figure A.10
Share of local expenditure on...

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

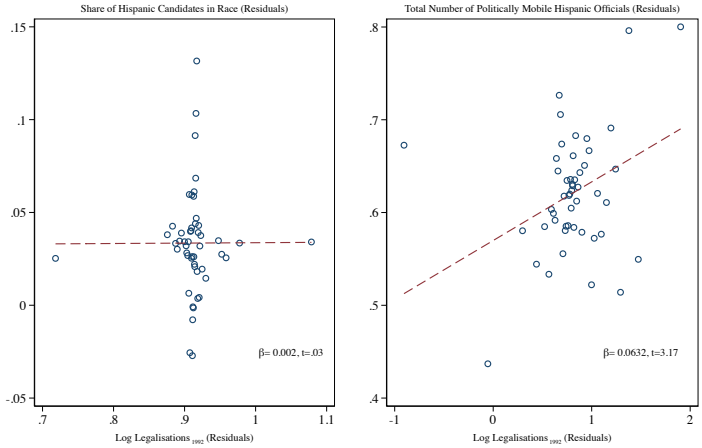

Figure A.11
Bin scatter plots of legalization, mayoral candidates and politically mobile officers

Note: This graph plots residuals of (a) the share of mayoral candidates of Hispanic origin (right panel) and (b) the total number of Hispanic public officials in a given county-year who moved up the political ladder (right panel) against residuals of the log of per capita legalization at the county level fixed to its 1992 level. The residuals were obtained after county fixed effects, state-by-year fixed effects and county level poverty and unemployment rates, log of population and log of income were accounted for. The figure on the left is generated by using data from LEAP as explained in subsection A .2 while the figure on the right is generated using information on Hispanic officials from the NALEO database.

A .2. Candidate Entry

Another potential mechanism that might explain the patterns in the data is that legalization leads to more Hispanic candidates running for office. Although this would not alter the essential argument of the paper—whether the legalization of migrants leads to increased political empowerment because it mobilizes others to run for office or because it simply encourages others to vote for existing Hispanic candidates, would arguably have the same effect on the governors incentive to allocate resources—in this subsection, we endeavor to distinguish to what extent candidate entry is a driving force of Hispanic selection to public office. Data limitations do not allow us to test for candidate entry across all potential offices. Instead, we focus only on elected mayors. To this purpose, we obtain data from from the Local Elections America Project (LEAP) which, at the time of writing, had collected data on some 28,000 candidates from 13,252 mayoral races across nine states from 1986 to 2017. After merging with my data on legalization and Hispanic officials, we are left with information on 3,265 candidates from 1,563 unique mayoral races. This data includes first and last names of all candidates. Accordingly, we match this information with Census Bureau data on surname frequency as well as basic demographic characteristics associated with surnames, including race. We are therefore able to identify Hispanic candidates in a given county in a given year and test whether more Hispanics ran for office after 1986 as compared to before in those counties more affected by the IRCA.

The results are shown in the left panel of Figure A.11, which plots residuals of the share of mayoral candidates of Hispanic origin against residuals of log legalization at the county fixed to its 1992 level level once county fixed effects, state-by-year fixed effects and county level controls, including population, unemployment, poverty and income, have been accounted for. As shown, the share of Hispanic candidates competing for office did not experience a corresponding increase in counties most affected by legalization after 1986. This suggests that Hispanic selection to mayorships was driven by more votes for existing Hispanic candidates rather than the entry of new ones into the race.

A .3. Political Mobility

In addition to the election effect of legalization on Hispanic public officials, we can test for a *mobility* effect. That is, 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 return to the NALEO data and classify each 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. We aggregate the total number of such officers in a given county in a given year and regress this outcome on the number of legalizations in that same county-year. The results are presented as a partial correlation plot in the right panel of Figure A.11. As shown, legalization leads to a significantly higher number of politically mobile officers at the county level, reinforcing the idea that legalization was significant to Hispanic political mobilization. This result may also explain why the effect of legalization on state and federal officers only becomes significant in the mid to late 1990s: starting mostly as municipal officers at the local level, the Hispanics who enter office as a result of the IRCA require more time to ascend up the political ladder.

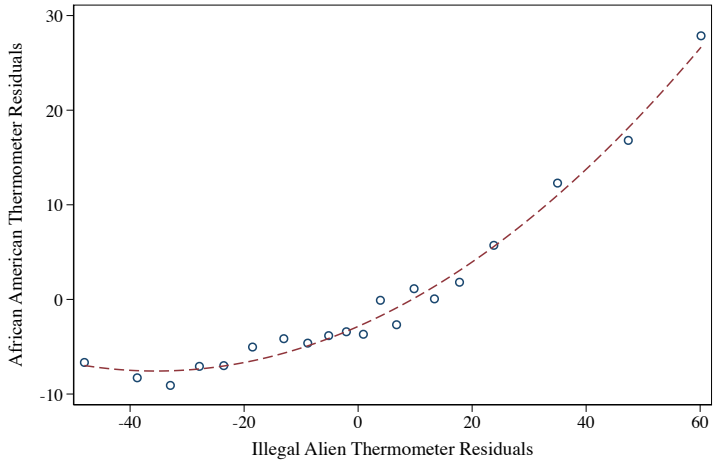

Figure A.12
Attitudes towards African Americans and Undocumented Migrants

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.

A.2 Additional Tables

Table A.1
Balance Table: Treated v Untreated Counties in 1980

	(1)	(2)	(3)	(4)	(5)	(6)
	Untreated		Treated		p-value	
	Mean	SD	Mean	SD	No State FE	State FE
County Characteristics:						
Log Transfers (Pc 1999)	4.347	[1.308]	4.268	[1.370]	0.333	0.002
Unemployment Rate	5.552	[1.444]	6.000	[1.240]	0.000	1.000
Poverty Rate	16.797	[7.515]	11.958	[5.888]	0.000	0.000
Population (1000)	32.446	[29.917]	392.692	[606.386]	0.000	0.000
Log County Income	8.966	[0.196]	9.219	[0.175]	0.000	0.000
Tax Revenue (Pc)	93.306	[80.508]	91.611	[76.027]	0.741	0.977
Share Hispanic	0.035	[0.100]	0.062	[0.113]	0.000	0.001
Share Black	0.083	[0.141]	0.099	[0.105]	0.051	0.015
Share White	0.887	[0.144]	0.847	[0.120]	0.000	0.001
Share Over 18	0.702	[0.037]	0.715	[0.035]	0.000	0.030
Households w/ Children	0.458	[0.049]	0.439	[0.061]	0.000	0.263
Births Per 1000	16.464	[3.429]	16.133	[3.313]	0.108	0.686
Bachelor Degree	10.752	[4.593]	17.340	[5.861]	0.000	0.000
Votes, D-President (%)	41.731	[12.666]	38.933	[8.732]	0.000	0.145
Governor Characteristics:						
Reelected	0.520	[0.500]	0.473	[0.500]	0.169	—
Republican	0.472	[0.499]	0.391	[0.489]	0.007	—
Lame Duck	0.238	[0.426]	0.182	[0.386]	0.027	—
Veto Power	0.961	[0.194]	0.951	[0.217]	0.399	—
Observations	1,762		325			

Notes: This table presents summary statistics for those counties that received applications for legal status (treated) and for those that did not (Untreated) in 1980. Columns 5 and 6 report the p-value from a regression coefficient when each covariate is regressed on a treatment indicator with (Column 6) and without (Column 5) state fixed effects included in the regression.

Table A.2
Robustness Checks

	Log of Inter-governmental Revenue (per capita)						
	(1) Long Difference $\Delta y_{1992\text{-}1982}$	(2) 1980 per capita	(3) IRCA^2	(4) Tax Control	(5) Demographic Controls	(6) Race Controls	(7) No Controls
Log Legalized$_{92}$ × P$_{86}$	0.0731***	0.0388**	0.0739**	0.0327**	0.0617***	0.0356**	0.0401***
	(0.0221)	(0.0154)	(0.0368)	(0.0130)	(0.0177)	(0.0143)	(0.0140)
Log Legalized$^2_{92}$ × P$_{86}$			-0.0108				
			(0.0107)				
Hispanic Share × P$_{86}$						0.502**	
						(0.206)	
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	No
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	No	No	No	No	Yes	No	No
Race Controls	No	No	No	No	No	Yes	No
Tax Controls	No	No	No	Yes	No	No	No
Log 1980 Hisp. Share × Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,936	39,337	39,510	31,871	22,490	39,510	39,510
Number of Counties	1,968	2,053	2,053	2,053	2,043	2,053	2,053

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log Legalized$_{92}$ is the log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) fixed to its 1992 level. P_{86} is a binary variable that indicates time periods before and after 1986. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Column 2 carries out the analysis using per capita transfers calculated with 1980 county population in the denominator. Column 4 includes per capita tax revenue at the county level as an additional control while the additional demographic controls in Column 5 include 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. Race controls in Column 6 include the share of the county population that is white, Black and Hispanic, each interacted with P_{86}. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.3
Additional Robustness Checks

	Log of Inter-governmental Revenue (per capita)					
	(1) Baseline Effect	(2) Linear Scale	(3) No Dropped Counties	(4) Pop > 100,000 < 430,000	(5) INS Applicants	(6) INS Legalized
Log $\text{Legalized}_{92} \times \text{P}_{86}$	0.0424***		0.0883***	0.0528*	0.0398***	0.0408***
	(0.0140)		(0.0277)	(0.0296)	(0.0141)	(0.0143)
$\text{Legalized}_{92} \times \text{P}_{86}$		0.00359**				
		(0.00141)				
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Log 1980 Hisp. Share × Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39,510	39,510	25,394	5,596	39,492	39,492
Number of Counties	2,053	2,053	1,218	338	2,051	2,051

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log Legalized_{92} is the log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) fixed to its 1992 level whereas Legalized_{92} is the same measure without the logarithmic transformation. P_{86} is a binary variable that indicates time periods before and after 1986. Column 3 restricts the analysis to only those counties that are observed in the Annual Survey of Local Government Finances and Census of Governments data throughout the entire duration of the sample. In Column 4 we exclude counties with populations less than 100,000 or greater than average (i.e. 430,000). Column 5 repeats the baseline using legalization application information derived directly from the INS Legalization Summary tape data while Column 6 uses actual legalizations (as opposed to applications) from the INS tape data. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.4
State Revenues and the IRCA

	Log of State Revenue From...		
	(1) Total Revenues	(2) Sales Tax	(3) Federal Gov't
Log Legalized State$_{92}$ × P$_{86}$	0.021***	0.025**	0.006
	(0.005)	(0.012)	(0.006)
Control Variables	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Log 1980 State Hisp. Share × Year Dummies	Yes	Yes	Yes
Observations	857	797	857
Number of States	42	39	42

Notes: The dependent variable is the log of state revenue from various sources, including intergovernmental revenue received by the state from the federal government (Column 3). Log Legalized State$_{92}$ is the log of the cumulative number of IRCA applications per 1,000 state inhabitants (plus one) fixed to its 1992 level. P_{86} is a binary variable that indicates time periods before and after 1986. Controls include state unemployment and poverty rates as well as log of population and log of income. Standard errors (shown in parentheses) are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.5
Legalization, Tightness-of-the-Race and Veto Power

	Log of Inter-governmental Revenue (per capita)		
	(1) Tightest 25%	(2) Tightest 10%	(3) Veto Power
Log Legalized$_{92}$ × P$_{86}$	0.0141	-0.00218	-0.00756
	(0.0145)	(0.0137)	(0.0232)
Log Legalized$_{92}$ × P$_{86}$ × Tight State 25	0.0398**		
	(0.0190)		
Log Legalized$_{92}$ × P$_{86}$ × Tight State 10		0.0703***	
		(0.0165)	
Log Legalized$_{92}$ × P$_{86}$ × Simple Majority			0.0929
			(0.128)
Log Legalized$_{92}$ × P$_{86}$ × 2/3 Majority			0.0528*
			(0.0273)
Log Legalized$_{92}$ × P$_{86}$ × 3/5 Majority			0.0128
			(0.0405)
Control Variables	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
Log 1980 Hisp. Share × Year Dummies	Yes	Yes	Yes
Observations	39,510	39,510	39,510
Number of Counties	2,053	2,053	2,053

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log Legalized$_{92}$ is the log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) fixed to its 1992 level. P_{86} is a binary variable that indicates time periods before and after 1986. Tight State 25 and 10 indicate, respectively, whether the outcome of the Presidential election in a given state in a given election cycle was more competitive (defined as the absolute difference between votes for the Republican and Democratic candidate) than those in the top 25^{th} and 10^{th} percentile of the competitiveness distribution. The interaction coefficients in Column 3 indicate what majority of a state legislature is required to override a governor's line-item veto. The omitted category is states where the governor has no veto power or no line-item veto power. The regressions include all baseline effects and lower order interactions. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.6
Legalization and Incumbent Vote Share

	Incumbent Vote Share
	(1)
Log $\text{Legalized}_{92} \times \text{P}_{86}$	1.007***
	(0.305)
Controls	Yes
County Fixed Effects	Yes
State-Year Fixed Effects	Yes
Log 1980 Hisp. Share × Year Dummies	Yes
Observations	8,003
Number of Counties	1,868

Notes: The dependent variable is the county level vote share (in percent) for the party of the incumbent Governor in Presidential elections. Log Legalized_{92} is the log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) fixed to its 1992 level. P_{86} is a binary variable that indicates time periods before and after 1986. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.7
Inter-governmental 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 inter-governmental 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`

Table A.8
Baseline Results with Alternative Clustering

	(1) Treatment × Post	(2) Legalisation Intensity
$\widehat{\beta}$	0.0956	0.0421
p-values:		
A. County clustering	0.000	0.003
B. State clustering	0.005	0.036
Observations	39,510	39,510
Number of Counties	2,053	2,053
Number of States	46	46

Notes: This table presents the baseline estimates (Column 1 of Table 1) clustering the standard errors at the county level and the state level.

A .4. 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)). It is unlikely that the SLIAG funds are confounding our results for the simple reason that SLIAG was administered through an entirely separate institutional set-up and is not part of the inter-governmental revenue outcome variable that we exploit.[32] Moreover, the main specification includes state-year fixed effects which could capture any state-specific time varying shocks to revenue arising out of the IRCA such as SLIAG. Nevertheless, we obtain the amount of funding states received from SLIAG and deduct it from our main outcome variable to create an 'inter-governmental revenue net of SLIAG' variable. Results are shown in Table A.9 and confirm that the resource allocation to affected counties is not being confounded by SLIAG funds.

32. 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).

Table A.9
Transfers on IRCA Legalizations net of SLIAG Funds

	(1) Net Transfers PC
Log Legalized$_{92}$ × P$_{86}$	0.0274* (0.0141)
Control Variables	Yes
County Fixed Effects	Yes
State-Year Fixed Effects	Yes
Log 1980 Hisp. Share × Year Dummies	Yes
Observations	39,501
Number of Counties	2,053

Notes: The dependent variable is the log of per capita transfers from state to county governments in 1999 USD net of SLIAG funds received from the federal government. Log Legalized$_{92}$ is the log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) fixed to its 1992 level. P_{86} is a binary variable that indicates time periods before and after 1986. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A .5. Attitudes Towards Migrants

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.[33] 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

33. 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.

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 first by quantifying the impact of Governor Wilson's term in office and of Proposition 187 on state aid. In Column 1 of Table A.10, therefore, we restrict the sample to consider only California during the eight years for which Governor Wilson was in power (1991 to 1998) and exploit variation in county level votes shares for Proposition 187, which varied from as little as 29 percent to as much as 77 percent. Perhaps unsurprisingly, counties more affected by the IRCA received less inter-governmental revenue during Governor Wilson's tenure. Interestingly, this result wiped away and made positive for counties with a Proposition 187 vote share of 49.5 percent or more, which might suggest that counties with high levels of opposition towards undocumented migrants actually attract more resources from the state when they are more affected by the IRCA. However, neither of the coefficients are precisely estimated, which suggests that the impact of immigrant legalization on state aid is not, in California at least, confounded by anti-migrant sentiment. In Column 2 we estimate the parameters of the baseline specification excluding California, the state with the strongest expression of anti-migrant sentiment at the time and the results hold. In the years following Proposition 187, ten other states passed ballot initiatives or laws similar to that of Proposition 187.[34] Dropping these states from the analysis—presumably the states where governors had the strongest incentives to cater to anti-migrant sentiment—does not alter the results in any meaningful way.

34. These are Arizona, Colorado, Florida, Georgia, Illinois, Nevada, New Mexico, New York, Oklahoma and Texas as reported by Richard Lacayo (December 19, 2004) in the following report: `https://ti.me/2PbD7YE`.

Table A.10
IRCA and Anti-Migrant Sentiment

	Log of Inter-governmental Revenue (per capita)		
	(1) Only Cali	(2) No Cali	(3) No Anti-Migrant States
Log Legalized$_{92}$ × P$_{86}$	0.0312	0.0625***	0.0530**
	(0.0536)	(0.0176)	(0.0215)
Log Legalized$_{92}$ × P$_{86}$ × Prop 187	-0.000721		
	(0.000808)		
Control Variables	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
Log 1980 Hisp. Share × Year Dummies	Yes	Yes	Yes
Observations	1,130	38,380	27,121
Number of Counties	54	1,999	1,404

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log Legalized$_{92}$ is the log of the cumulative number of IRCA applications per 1,000 county inhabitants (plus one) 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. This regression includes all baseline effects and lower order interactions. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Column 3 excludes the 10 states (plus California) that passed ballot initiatives or laws similar in spirit to those of Proposition 187. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

As a more general check, we merge the legalization variable with data from the General Social Survey (GSS), which includes a range of questions on attitudes towards migration. As indicated in the various columns of Table A.11, a higher share of legalized migrants in a county tends to improve, not worsen, attitudes towards documented and undocumented migrants. Individuals residing in such counties tend to think undocumented migrants work hard and deserve work permits. Such individuals are also of the opinion that documented migrants neither increase crime nor take jobs away from native citizens. Unfortunately, these questions are all posed in the post-treatment period and cannot be interpreted causally. Still, the correlations do suggest that anti-migrant sentiment was not an overwhelming concern in IRCA-affected counties.

Table A.11
The IRCA and Attitudes towards Migration (GSS Survey)

	Attitudes on Undocumented Migrants		Attitudes on Documented Migrants	
	(1) Given Work Permits	(2) Work Hard	(3) Increase Crime	(4) Take Jobs Away
Log legalizations	0.0175**	0.0281**	-0.0396**	-0.0415**
	(0.00868)	(0.0118)	(0.0198)	(0.0176)
Individual Controls	Yes	Yes	Yes	Yes
Observations	730	756	440	471
Number of Counties	157	157	135	137
Years in Sample	1994	1994	1996	1996

Notes: This table uses General Social Survey (GSS) data merged with the legalization data. Log legalizations is the log of the cumulative number of IRCA applications in a given county in a given year per 1,000 county inhabitants (plus one). The outcome variables are all binary indicators on various attitudes towards documented and undocumented migrants. Control variables include individual income, employment status, marital status, age, educational attainment and race. Standard errors (shown in parentheses) are clustered at the region level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$