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

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Abstract

We examine the impact of immigrant legalization on the distribution of state and local spending by exploiting variation in legal status arising from the 1986 Immigration Reform and Control Act (IRCA), which legalized three million Hispanic migrants. We find that governors, irrespective of party, allocate more per capita to IRCA-affected counties. This allocation is sensitive to the governor's electoral incentives and leads to Hispanic educational improvements, suggesting that it is politically motivated and targeted. Overall, our work underscores the importance of public expenditure as a channel linking legal status to a range of socio-economic benefits documented by the literature. (*JEL*: J15, H72, P16)

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

1.   Introduction

Legal status has first order economic consequences on the lives of individual migrants and on the communities in which they reside. Various episodes of immigrant legalization have generated a rich scholarship that documents the effect of legal status, or the lack of it, on the individual migrant's labor market (Kossoudji and Cobb-Clark 2002; Pan 2012; Rivera-Batiz 1999), educational (Cortes 2013; Amuedo-Dorantes and Lopez 2015) and health (Baker 2010) outcomes as well as its impact on lowering crime (Baker 2015; Pinotti 2017). Surprisingly, less interest has been paid to understanding the political response to immigrant legalization and its subsequent impact on the distribution of state and local spending, even as the distributional effect of migration—and of undocumented migration in particular—remains a salient, if not polarizing, issue.[1] Indeed, insofar as the various socio-economic benefits of legal status are mediated by its impact on the allocation of public spending, understanding the distributional consequences of immigrant legalization is a question of vital economic importance.

To the extent that legalizing undocumented migrants leads to naturalization, standard models of electoral competition generally predict that the expansion of franchise to newly legalized migrants would incentivize politicians to allocate more resources towards them through a process of policy convergence (Downs 1957; Meltzer and Richard 1981; Roberts 1977; Romer 1975). But there is also theory and evidence to show that elected politicians, in fact, display ideological rigidity. In such instances, voters "elect" rather than "affect" policy outcomes (Lee, Moretti, and Butler 2004) and it is a politician's own ideology, rather than voter preferences, that acts as the primary determinant of his or her policy choices (Levitt 1996). In the event of an ideologically polarizing episode of voter enfranchisement, it is not clear to what extent incumbent politicians will allocate resources to capture the new "swing" vote or to further solidify a core constituency.

In this paper, we shed light on these issues by examining the impact of immigrant legalization on the distribution of public resources—specifically, per capita inter-governmental transfers from state to local governments—in the United States. Our analysis focuses on the

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[1]. A 2013 survey by the PEW Research Center, for example, found that 60 percent of Americans agreed that granting legal status to undocumented migrants would be a "drain on government resources" against 37 percent who did not. See here for more details: https://pewrsr.ch/33k1lqW Accessed 12 March 2020.

actions of incumbent state governors in order to better understand their response to the political incentives created through immigrant legalization. Do they allocate more resources to the areas affected by amnesty and if so, to what extent are these choices motivated in an effort to target the newly documented migrants—and win their political support—as opposed to that of other, ideologically competing, constituents?

The United States provides an ideal setting to study these questions. For one, it has a large and fast growing undocumented population—an estimated 10.5 million unauthorized migrants in 2017, up from 5.8 million in 1996.[2] For another, in 1986, the Reagan Administration passed into law the Immigration Reform and Control Act (IRCA) which legalized nearly 3 million undocumented migrants in the span of some three years and offered them a path to citizenship five years after legalization. In this study, we combine variation in legal status from the IRCA with administrative data from the Census of Governments in order to throw light on the ways in which documenting undocumented migrants affects the distribution of state and local finances. Our analysis is motivated by a distributive politics framework in which incumbent politicians react to changes in the electorate by adjusting their budget allocations to target the preferences of the newly enfranchised group so as to optimize (a) the welfare of the population and (b) their own re-election chances. Accordingly, we posit that governors, who play an important role in formulating the state budget, will allocate resources strategically to those counties most affected by the IRCA in an effort to win the political support of the newly documented migrants who were eligible to vote five years after legalization.

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 seven percent more per capita transfers than untreated counties, an amount that translates into USD 210 per legalized migrant per year. The transfers are funded as a result of increased revenue at the state level—in particular from sales and income tax—generated from the IRCA and not through decreases in funding to counties unaffected by the amnesty. Moreover, the result is not driven

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2. Pew Research Center, taken from http://pewrsr.ch/2oWlM93 Accessed 12 March 2020.

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 4.2.

by differences in county economic or demographic characteristics and is robust to alternative specifications and samples.

The identifying assumption of our model is that treated and control counties would have evolved on similar paths with respect to the distribution of state aid in the absence of amnesty. To lend credence to this assumption, and to overcome potential geographical endogeneity associated with where undocumented migrants settle, we follow three approaches: first, we test for parallel trends; second, we employ propensity score matching to identify a more comparable control group; and third, we use the share of a county's 1960 population that is foreign-born as an instrument for the number of documented migrants per county post-1986. All these tests confirm that the baseline result is not confounded by geographical factors.

We argue that the causal channel linking the IRCA to additional state spending is the political incentive to win the electoral support of the newly documented migrants. It is possible, however, that the distributional effect we uncover reflects purely mechanical or bureaucratic forces that oblige the state governor to better service the areas where the documented migrants reside. To rule out this competing explanation, we exploit variation in political constraints facing state governors and analyze the sensitivity of our results to those 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 political constraints faced by the governor. If, on the other hand, the transfers are politically motivated, the results ought to exhibit heterogeneity with respect to the various political 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: although both Republican and Democratic governors allocate significantly more resources to IRCA-affected counties, the effect increases by about half when the governor is a Democrat. We find no such relationship when examining the partisan composition of state legislatures, suggesting that the state executive responds more to the incentives created by amnesty. To this end, we find that a governor's likelihood for re-election increases significantly in the share of newly documented migrants in a state after

1992 when IRCA migrants first gained the right to vote. We also find that the re-election effect of the IRCA is amplified when there is a large Hispanic population residing in a given state, pointing to potential network effects.

Finally, undocumented migration was as polarizing an issue then as it is now. To distinguish to what extent a governor's distributional response to the IRCA was intended to target the newly legalized in an effort to win their vote as opposed to that of other, ideologically competing constituents in a county, we undertake a number of exercises. First, we exploit data on a prominent anti-migrant ballot measure in California and find no relationship between state transfers and the interaction between a county's share of legalized migrants and its support for the ballot measure. Second, we draw on individual level voting data from the American National Elections Studies (ANES) in order to better understand the relevance of the IRCA on voting behavior. We find that individuals residing in IRCA-affected counties have a significantly higher likelihood of voting in Presidential and Gubernatorial elections than similar individuals residing in non-affected counties and that this effect is not driven by an increase in turnout among individuals with negative feelings towards undocumented migrants. Third, we turn our attention to local expenditure and find that spending in education increases in the share of newly documented migrants in a county and that these increases are significantly different to zero as of 1993. Interestingly, Hispanic individuals, as opposed to Caucasian ones, residing in these counties experience significant improvements in the likelihood of completing high school, provided they entered middle school from 1994 onward. These results are especially noteworthy for two reasons. First, they suggest that the additional resources a county receives on account of the IRCA were in fact targeted to the newly legalized migrants. Second, they underscore the importance of targeted public expenditure in driving the socio-economic benefits of legal status, in this particular case education.

Our paper offers two 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. For example, Cortes (2013) shows that legal status helps migrants to obtain better educational outcomes whilst Kossoudji and Cobb-Clark (2002), Rivera-Batiz

(1999) and Pan (2012) use the IRCA to show that documenting undocumented migrants leads to an improvement in their wages, employment prospects and ability to speak English. 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) 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. More fundamentally, to the extent that these other social and economic improvements are consequences of the increases in public investment as a result of legal status, our paper sheds light on a key missing mechanism in the literature. Our work thus fills an important gap that helps understand the channel linking legal status to a wide range of socio-economic benefits which other studies have shown.

Second, we contribute to the literature that examines the distributional effects of the expansion of voter franchise. Cascio and Washington (2014), for example, study enfranchisement of African Americans in the United States through the Voting Rights Act of 1965. They find that counties that removed literacy tests in response to the law experienced greater turnout among black voters which, in turn, increased the share of public spending directed towards them. In a similar vein, Miller (2008) shows that the enfranchisement of women in the United States was followed by immediate changes in legislative behavior and substantial increases in public health spending at the local level. Examining the introduction of electronic voting technology in Brazil, Fujiwara (2015) finds that the de facto enfranchisement of lesser educated citizens led to increased health care spending on the poor. We extend this literature to consider the case of undocumented Hispanic migrants in the United States, one of the largest disenfranchised groups in the country. More importantly, because of our rich data on state governors, we are able to take this literature a step further by uncovering the precise political mechanisms that lead governors to allocate resources in light of a sudden shock to the electorate. We show, for example, that the distributional response is borne of discretionary political choice, politically rewarding in terms of re-election and targeted to capture the new swing vote rather than that of ideologically opposed groups. 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 incen-

tives created by amnesty. Together, our results indicate that the political reaction to immigrant legalization is indeed "affected" by those with new-found political voice.

The rest of this paper proceeds as follows: Section 2 contextualizes the study by discussing the historical background of the IRCA as well as the demographic characteristics of its applicants. In Section 3 we present a simple framework that guides our empirical analysis and the interpretation of our findings. Section 4 describes our data and explains institutional features associated with the budget-making process at the state level. Section 5 outlines our econometric methodology and, along with sections 6 and 7, presents our results. Section 8 concludes.

2. Background

2.1 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 99th Congress settled on and approved the IRCA on 17 October 1986 and which was signed into law on 6 November 1986.[4]

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

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4. The timing of the IRCA's passage in 1986 was indeed 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). See Table A.2 in the Online Appendix for details on how Congress voted to pass the bill.

illegal for employers to knowingly hire unauthorized workers; increased funding for border security to discourage new people from migrating illegally; and an amnesty program intended to legalize various unauthorized workers (Chishti and Kamasaki 2014).

While each of the components of the law was not without problems (in particular the employer sanction scheme which led to a great amount of fraud as well as workplace discrimination), 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).[5] 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 (2018)). 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 (2018)). 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 test and a civics test, applicants were given permanent resident status.

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). The law stipulated that both application periods (the 12 months for the LAW program and 18 months for the SAW program) were strictly enforced, which from an econometric point of view implies a relatively clean identification period. Indeed, by the end of the application period, roughly 3 million people applied for temporary resident status, of which 1.7 million comprised LAWs and 1.3 million comprised SAWs (DHHS, December 1991). By 1990, 94.6

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5. Baker (2014) and Baker (2015) cite a number of reasons why the IRCA "represented a near-universal legalization of immigrants in the United States."

percent and 58.7 percent, respectively, of LAW and SAW applications had received approval and for all intents and purposes, the legalization program of the IRCA ceased to generate newly legalized citizens after 1990 (DHHS, December 1991). Figure 1 shows the time trend of the stock of IRCA legalized migrants while Figure 2 shows the geographic distribution of IRCA applicants at the county level in 1992 for those counties for which data is available.[6] As shown, the majority of legalizations took place between 1986 and 1990 and in the states of California (970,895), Texas (351,646), Illinois (125,399), Arizona (70,488) and New Jersey (29,012). Figure A.2 in the Online Appendix shows that undocumented migrants applied for legal status in approximately 330 counties whereas the remaining counties received no such applications.

Figures 1 and 2 here.

2.2   Demographic Characteristics of the Legalized

Figure 3 presents data from the December 1991 report to Congress from the Department of Health and Human Services which documented the characteristics of the legalized population. 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).[7] In fact, median take-home pay for IRCA applicants stood at $400 per week. Median household income in the population in 1989 stood at $23,745, or $456 per week. The report also makes clear that no more than 5 percent of the migrants reported not being able to work in the month prior to the survey. Accordingly, IRCA applicants were, by and large, an economically active and self-reliant group earning somewhere between the poverty threshold and median income.

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6. Of the 3,142 counties in the United States, our dataset includes IRCA information on 2,760 of them (and from all states except Alaska and Delaware). However, we do not observe every county in every year because some of the counties drop out in the later stages of the sample. As such, the actual number of treated counties varies slightly in the sample from 276 counties in 1999 to 332 in 1991/92. Restricting the analysis to only those counties that we observe throughout the entirety of the sample makes no difference to the results.

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

Figure 3 here.

3. The Incentives of an Incumbent Politician

In this section, we present a simple framework to help fix ideas and guide our empirical analysis. In this framework, an incumbent politician controls the distribution of transfers flowing from the state budget to the various counties in the state. The politician is concerned both with the welfare of the population and 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 her re-election chances and the welfare of the population. In our setting, therefore, we expect that the legalization of a large group of mostly Hispanic people of lower socio-economic status will prompt politicians to offer policy over public resources so as to target the preferred outcome of the newly legalized group in the hope of winning their future political support.

Equation 1 captures how we think about the objective function of the incumbent politician, P.[8] For every county in the state, P transfers an amount of government assistance, $g > 0$, to the population at cost $C(g)$. As mentioned above, P is concerned with the utility of the residents, U, in every county as well as the utility derived from her expected vote share in elections, Ω.

Each county is composed of two types of inhabitants: already legal citizens, C, and newly legalized migrants, L. The population in each county is normalized to one such that the share of the population that is newly legalized is α and the share of already legal citizens is $1 - \alpha$. Only citizens can vote and voting decisions are based on local economic conditions, such as government expenditure on various public services. Voters base their voting decisions not just on local conditions immediately prior to the election but throughout P's term.

For every county in the state, P has a pay-off, Π, that is composed of three parts, shown in equation 1:

$$\Pi = \underbrace{(1 - \alpha)U_C(g) + \alpha U_L(g)}_{1} + \underbrace{E \cdot \Omega[\phi((1 - \alpha)U_C(g) + \alpha U_L(g))]}_{2} - \underbrace{C(g)}_{3} \qquad (1)$$

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8. This framework is an adaptation of that found in Englmaier and Stowasser (2017).

The politician is concerned with the well being of the population in each county. This is reflected in the first term of P's pay-off, $(1 - \alpha)U_C(g) + \alpha U_L(g)$. We assume utility functions are concave such that $U_i'(g) > 0$ and $U_i''(g) < 0 \; \forall \; i \in \{C, L\}$. The only way in which P can improve the utility of the population is through her allocation of g. Because the characteristics of the documented migrants presented earlier, we take it as given that $U_L'(g) > U_C'(g) \; \forall \; g$.

We assume there are gains to staying in office. Accordingly, the second term, $E \cdot \Omega[\phi((1 - \alpha)U_C(g) + \alpha U_L(g))]$, captures the pay-off P obtains from re-election. E is a binary variable that is one when P is eligible for re-election and zero when P is a lame duck. In every election for which P is eligible to run, ϕ captures her expected vote share in that election which is a function of the well being of the population. We assume that ϕ is a linear function bounded between zero and one. The utility P derives from this expected vote share is captured by Ω. It is assumed that $\Omega(\phi)$, a strictly increasing, non-linear function with a negative third order derivative. Figure A.1 in the Online Appendix provides an illustration of what such a function might look like. As shown, the marginal utility derived from the expected vote share is the greatest at the inflection point of $\Omega(\phi = \phi_T)$ which represents the winning threshold.

Finally, the last term of equation 1 indicates the costs, $C(g)$, to the incumbent associated with allocating g to a given county. These capture the opportunity costs associated with distributing g among the different counties in a given state so as to remain within the budget constraint. Costs are sufficiently convex such that $\frac{\partial^2 \Pi_P}{\partial g^2} < 0$.

A rational incumbent politician thus maximizes her expected pay-off over the allocation of state grants. That allocation is strongly affected by the share of newly legalized migrants, α, in a county. From this follows Predictions 1 to 3 which help guide our empirical analysis.

Prediction 1: The optimal allocation of state aid increases in the share of newly documented migrants in a county.

Prediction 2: The optimal allocation of state aid is larger when P is eligible for re-election and less when (s)he is a lame duck.

Prediction 3: The optimal allocation of state aid becomes larger the closer P's expected vote share is to the winning threshold.

Proofs can be found in the Online Appendix.

4.   Data and Institutional Context

4.1   Data

The key explanatory variable in our study is a measure of the number of IRCA applicants per 1,000 county inhabitants in the United States for the period between 1980 and 2000. 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 county inhabitants. As of 1992, counties affected by the IRCA had, on average, 5,410 legalized migrants residing in them, which corresponds to 11.75 migrants per 1,000 inhabitants. To carry out our analysis, we compiled a new dataset from a number of different administrative sources.[9]

Our measure of IRCA applications per county comes from Baker (2015) who, in turn, takes it from the Immigration and Naturalization Service (INS). 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.

We aim at understanding the impact of the IRCA on the distribution of state and local finances and the sensitivity of this impact to political constraints. We thus add data on state and local finances taken from the US Census of Governments and use per capita inter-governmental revenues from state governments to local governments (counties, cities, municipalities aggregated to the county) as our dependent variable. We also utilize a host of governor related data including party affiliation, his or her name, an indicator for whether (s)he is a lame duck and an indicator for whether (s)he enjoys line-item veto power in order to better understand the responsiveness of the governor to the IRCA. These data are obtained from the Codebook for State Elections. We apply an instrumental variables strategy to confirm our OLS estimates, using the share of a county's 1960 population that is foreign-born as an instrument for the number of documented migrants per county post-1986. This variable is taken from the County and City Data Book prepared by the US Department of Commerce and the Census Bureau and made available by ICSPR under Study No. 7736.

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9. 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.

4.2 Inter-governmental Revenue and The Budget-Making Process

Inter-governmental revenue (IGR)

The primary dependent variable is per capita inter-governmental revenue (IGR) received by local governments (counties, cities, municipalities, aggregated to the county) from state governments. 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."[10]

Local governments have few major sources of local revenue, most notably property taxes and, to a much lesser extent, sales taxes. Inter-governmental revenue, therefore, is a type of budget support for local governments. 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 as shown in Figure A.3. Importantly, the local governments that receive this aid have "no discretion as to its distribution". State and federal governments decide to what areas these revenues are directed. For example, state aid in education is intended to "support of local schools" but excludes "State grants for libraries". The variable at our disposal is aggregate inter-governmental revenue at the county level and so it lumps together revenue intended for such areas as health, highways, education and public welfare.[11]

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

11. Although we only observe aggregate revenue, Table A.3 in the Online Appendix details what is and what is not included in the inter-governmental revenue received from the state and gives an indication as to what types of local activity these revenues support.

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.[12]

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 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.

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

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 transfers from state to local governments. The primary identifying assumption of our econometric model is that no other shocks occurred around the same time as the passage of the IRCA that correlate either with the number of legalized migrants in a given county or with the amount of inter-governmental revenue it received from the state. 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. Figure 4 shows the trends in IGR as it appears in the raw data 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 and only after the passage of the law does one observe an appreciable difference between the two.[13]

Figure 4 here.

As a more rigorous test for pre-treatment differences, we plot the coefficients of an event study as specified in equation 2:

$$y_{c,t} = \delta_c + \alpha_t + \sum_{j=1980}^{2000} \beta_j \left[T_c \times D_t^j \right] + \epsilon_{c,t} \tag{2}$$

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 Figure 5 which indicate that the difference in transfers received between treated

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13. In Figure A.4 in the Online Appendix we show the evolution of a number of other county covariates that make clear that the IRCA is not associated with corresponding shocks in such county characteristics as population, income, tax revenue or public school enrollment. 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.

and non-treated counties shown in Figure 4 only becomes positive and significantly different to zero in the years after 1986 and not before, further increasing confidence in our identifying assumption.

Figure 5 here.

5.2   Baseline Estimates

We impose more structure on model 2 in order to estimate the parameters of a differences-in-differences regression specified in equation 3.

$$ln(y)_{c,t} = \beta_0 + \delta_c + \zeta_{st} + \beta_1 \cdot (T_c \times P_t) + \Theta \cdot \mathbf{X_{c,t}} + \epsilon_{c,t} \tag{3}$$

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 is defined as before. The treatment indicator, T_c, is now interacted with a binary variable P_t, that is one if $t > 1986$ and zero otherwise. In addition, we include 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.[14] We include a vector of county-level covariates, $\mathbf{X_{c,t}}$, that includes poverty and unemployment rates, income and population. As before, $\epsilon_{c,t}$ is an idiosyncratic disturbance term clustered at the county level.[15]

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 3 and we see precisely estimated coefficients of similar magnitude across a number of specifications. In Panel B, we estimate the same parameters but using a measure of treatment intensity as specified in equation 4. Here, $ln(IRCA + 1)_{c,t}$ is the natural log of the cumulative number of IRCA applicants per 1000 county inhabitants (plus one) in county c in year t. The parameter of

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14. 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-level tax revenue as a result of this reform which might potentially counfound a governor's decision to allocate revenue to IRCA-affected counties.

15. Because our unit of observation is the county and our treatment varies at this level, we cluster standard errors at the county. The results, however, are robust to clustering at higher levels, most notably the state. These results are captured in Table A.4 in the Online Appendix.

interest, β_1, can be interpreted as the elasticity of state transfers with respect to the cumulative number of per 1000 capita legalized applicants. All other parameters are defined as before.

$$ln(y)_{c,t} = \beta_0 + \delta_c + \alpha_t + \zeta_{st} + \beta_1 \cdot ln(IRCA + 1)_{c,t} + \Theta \cdot \mathbf{X_{c,t}} + \epsilon_{c,t} \qquad (4)$$

Table 1 here.

Column 1 is our baseline estimate and suggests that counties affected by the IRCA received, on average, seven percent more in per capita transfers than those that did not. By 1992, treated counties had, on average, 5,410 legalized migrants residing in them; a seven percent increase in inter-governmental revenue thus implies that 1 percent of the county population increases state aid by USD 1 million, or USD 210 per migrant per year. Using the measure of treatment intensity, the magnitude of the effect, while larger, is still reasonable. 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 0.06 percent. Within treated counties, this elasticity translates into an increase of approximately eight percent in per capita inter-governmental revenue when moving from the 50th to the 75th percentile of treatment intensity, a move that amounts to USD 535 per migrant per year. Because treatment intensity is a more precise measure of treatment, equation 4 is our preferred specification and henceforth we will use the log of the cumulative number of per 1000 capita IRCA applicants (plus one) as our main explanatory variable.

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.[16] As shown in Column 2, the results not only hold but become slightly larger. To alleviate concerns that the results are driven by very populous cities or counties—some of which may serve as so-called 'sanctuary cities'—we rerun the regression, in Column 3, on a sample restricted only to those counties with populations less than average county population size and obtain precisely estimated results, albeit of slightly

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16. Dropping the most treated states in terms of the absolute number of legalizations makes no difference to the results, nor does dropping the four states that border Mexico. These results are not reported.

smaller magnitude suggesting that the bulk of the effect is not driven by very populated counties.

As shown in Figure A.2, there are some 330 counties that received applications for legal status and just over 2,000 that did not. One might wonder, therefore, how comparable these two types of counties are. To address these concerns, we use propensity score matching to generate a more comparable control group. That is, for every county in the sample, we generate, on the basis of its observable characteristics, a propensity score that indicates a given county's likelihood to be treated. Then, for every treated county, we match the nearest neighbor from the untreated counties to generate a more comparable control group. In Column 4, we rerun the model in this matched sample and obtain results almost identical to those of the baseline.[17]

Finally, in Column 5, 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. That the result holds suggests that the relationship between immigrant legalization and the distribution of state aid is a robust one.

In Table A.5, we replicate the baseline estimates using a $log-linear$ specification to demonstrate that the results are insensitive to the logarithmic transformation of the legalization data. We choose a $log-log$ specification because (a) the legalization variable is unevenly distributed and (b) an elasticity is easier for interpretation.

5.3 Robustness Tests

In Table 2 we carry out a number of further empirical checks to test the strength of the relationship. In Column 1 we run a first-differences estimation using only two years in the sample: 1982 and 1992. The idea here is to skip intervening years to overcome issues with respect to timing of various sorts: different electoral cycles in different states, different budget response times and different IRCA application processing times. As shown, the legalization

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17. Changing the number of neighbors up to 5 does not change the result. Figure A.5 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. Matching within a state and leaving state-year fixed effects in the estimation does not change the results.

variable maintains its predictive power over per capita inter-governmental revenue.[18] In Column 2, we use a county's 1980 population to carry out all per capita calculations as another way of ensuring population changes are not driving the results. To understand whether the relationship between immigrant legalization and the distribution of state aid is linear or quadratic, we include a quadratic term of the key explanatory variable in Column 3. As shown, the linear variable retains its precision whereas the quadratic term enters imprecisely. In Column 4 we control for total per capita tax revenue at the county level as a further measure of robustness against the 1986 tax reform and in Column 5 we include additional county demographic controls, including the share of the population that is over 18 year of age, the share of the population that is Hispanic and the share of county households with children. In Column 6 we run the regression with no covariates so as to mitigate concerns related to potential "bad controls". As shown, the results across all these specifications are very similar to the baseline.

In Column 7 we undertake a final empirical to rule out geographical endogeneity. Specifically, we use the share of a county's foreign-born population in 1960 as an instrument for the number of IRCA applicants post-1986. In doing so, we follow a number of other studies (Hildebrandt et al. 2005; Woodruff and Zenteno 2007; McKenzie and Rapoport 2010) that utilize historical rates of migration as an instrument for present levels. Because the instrument in question is time-invariant we interact the 1960 foreign-born variable with year dummies in order to take advantage of the panel structure of our data. Doing so produces results that are positive and precisely estimated, though an order of magnitude larger than the baseline.[19] That the coefficient on legalization remains positive, precise and relatively stable across all these specifications suggests that the relationship between immigrant legalization and the distribution of state spending is a robust one.

Table 2 here.

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18. Figure A.6 plots the coefficients from a number of such regressions, each using a different time period for the difference estimation.

19. Another option is to run 13 cross-section regressions for each year between 1988 and 2000 where each variable is differenced from its 1982 value and the differenced variable is then instrumented in the cross-section. The regression coefficients are plotted in Figure A.7 and confirm, both in terms of precision and the timing of the effect, the baseline estimates shown in Figure A.6.

5.4   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 in some three years, 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.4 which shows population growth in treated and untreated counties. As illustrated, neither type of county experienced appreciable changes in population in the years before or after the passage of the IRCA. Accordingly, even if funds were transferred by formula on the basis of a county's population, the fact that there is no population growth associated with the IRCA alleviates our concern that mechanical population forces drive our results.

Second, a feature of the IRCA was that it "barred" the newly legalized "from participation in programs of financial assistance furnished under federal law on the basis of financial need for a period of five years from the effective date of each alien's lawful temporary resident status" (DHHS, December 1991). Moreover, given the demographic characteristics of the newly legalized discussed previously and that the children of undocumented migrants were already eligible for public services such as schooling pre-IRCA, we find it unlikely that our results are explained by mechanical increases as a result of social assistance eligibility criteria being satisfied at the state level.[20] 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*

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20. Later in the paper, 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 (as shown in Figure 7).

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.

5.5   Funding of Transfers

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

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 3. 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 3 is similar in size to that in Column 1 suggests that, by and large, the treatment effect is not coming at the expense of control counties.

Table 3 here.

To understand where the additional resources come from, we turn obtain state revenue data from the Census of Governments and regress various measures of state revenue on the number

of per 1,000 capita legalized migrants at the state level. As shown in Table 4, the coefficient on per capita legalizations suggests that state revenue, in particular from sales and income taxes, increases 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).[21]

Table 4 here.

6.   Political Economy Mechanisms

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 and to what extent it is merely reflective of mechanical, bureaucratic forces that oblige the governor to better serve the areas where the documented migrants reside. To shed light on these issues, and on the mechanisms that drive the result, we turn our attention to the political constraints on, and the political context of, the state governor. The contention here is simple. If, on the one hand, the increases in per capita transfers associated with the IRCA are the result of mechanical forces, the results ought to be entirely insensitive to political context or constraints. If, on the other hand, the transfers are the result of discretionary choices made by state governors in an effort to bolster political support, then it is not unreasonable to expect state aid to display some sensitivity to political context.

6.1   Political Party Heterogeneity

We begin by investigating the sensitivity of our results to the party affiliation of the governor. Column 1 of Table 5 indicates that the per capita transfers a county receives in response to the IRCA are positive and significant and that this amount increases by about half when the

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21. As shown in Column 3 of Table 4, legalization intensity in a state is also positively associated with the amount of inter-governmental aid that state receives from the federal government, although the magnitude of the coefficient is half the size of those in Columns 1 and 2. To alleviate concerns that the result is driven by increased inter-governmental revenue from the federal government which simply passing through the state, the Census Bureau explains that "federal aid that is given to the state to then be distributed to local governments is normally considered state aid because states usually have discretion over the distribution."

governor is a Democrat as compared to when he or she is a Republican.[22] 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.[23] 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]

Table 5 here.

6.2 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. From among the counties for which we have data, eight received legalized migrants as a result of the IRCA and 129 did not. Figure 6 shows the trends in transfers during Zell Miller's tenure as Governor. As shown, the counties that received no legalizations experienced a steady decline in the amount of per capita transfers received. The eight counties that received legalizations, by contrast, exhibit a great deal of variation. In Governor Miller's first term, transfers to these counties increase only to drop off drastically in his second term when he is no longer eligible for re-election. The question that arises, therefore, is to what

22. Figure A.9 and Table **??** show, perhaps unsurprisingly, that the Democratic vote share in Presidential elections increases as the share of IRCA migrants in a county increases.

23. 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.8.

24. In Table A.6 we explore whether the partisan composition of Congress has any explanatory power over the results. To this end, we interact the legalization variable with indicators for whether a given state's Members of Congress or Senators were majority Democrats or not. As shown, the party affiliation of a states federal representatives has no explanatory power on the overall manner in which the state budget is distributed in response to IRCA.

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.

Figure 6 here.

The regressions in Table 5 investigate these questions. In Column 3, we interact the legalization variable with a binary variable that is one when the governor is eligible for re-election and zero when (s)he is a lame duck owing to a term limit and the result indicates that the difference in transfers between lame duck and non-lame-duck governors is indeed positive and significant.[25] We carry out a similar analysis in Column 4, this time analyzing sensitivity to the gubernatorial election cycle. Here, 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 that counties affected by the IRCA receive about 35 percent more in inter-governmental revenue in the year prior to a gubernatorial election.

6.3 Electoral Competition

To shed further light on mechanisms, 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 with new potential voters 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 25 and 5 percent of the distribution— and examine the effect of legalization in these states as compared to those that were less competitive.[26] As shown in Columns 1 and 2 of Table 6, counties with a greater share of legalized migrants receive even more resources when their state experiences greater electoral

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25. One potential concern with this approach is that it may not estimate a governor's electoral incentive arising from the IRCA since the IRCA ceases to produce meaningful variation in the number of documented migrants after 1992. To address this, we re-estimate the parameter of interest, limiting the sample to the period only between 1989 and 1994 and compare governors who are lame ducks in this period with those who are not. Results are shown in Table A.7 and indicate that governors with an electoral incentive allocate significantly more resources than their lame duck counterparts as the share of documented migrants in a county increase.

26. 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.

competition. We then compare this baseline interaction across two time periods: before and after 1992, the first year (and Presidential election) in which IRCA migrants gained eligibility to vote. The results, shown in Columns 3 and 4, suggest that the impact of legalization on state-to-county transfers is particularly amplified in time periods after which IRCA migrants gained eligibility to vote.

Table 6 here.

6.4 Veto Power

As mentioned earlier in the paper, governors exercise strong influence over the budget-making process in a given state. In this subsection, we focus on one of these powers: the line-item veto. 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 1 of Table 7, 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.[27] As shown, the effect of legalization on inter-governmental revenue is entirely driven by counties in states where the governor has more influence over the state budget.

Table 7 here.

6.5 State Legislatures

Although governors do enjoy increasing power over the state budget, 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 2 of Table 7. 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.[28] The result indicates that,

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27. Specifically, we compare states where the governor has line-item veto power to states where the governor has either a simple veto, but not line-item veto, or no veto power at all.

28. This includes when the state legislature is split or has a majority of the opposite party to the governor.

although transfers increase as the share of IRCA applicants in a county increases when there is no political alignment between the legislative and executive branches of state government, the result increases by about 50 percent when there is political alignment, further underscoring the politically discretionary nature of these transfers.

In Column 3 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 are Republican or Democratic. The omitted category is when the Houses are split. As shown, the partisan composition of the state legislature plays no significant role in the distribution of state finances to counties affected by the IRCA. This is in contrast to the results of Column 1 of Table 5 which demonstrate a clear partisan pattern at the gubernatorial level. Together, these results seem to suggest that, in the context of immigrant legalization, the state executive is more responsive to the political incentives created by amnesty than the state legislature.

6.6   Re-election Considerations

How might these political economy results impact a governor's re-election chances? Our data includes an indicator for whether a particular governor who was eligible for re-election was, in fact, re-elected and we exploit this variable to understand whether the share of documented migrants in a state affects re-election likelihood in any way. Because this outcome varies at the state level over time, we aggregate our variables to the state level and only include state and year fixed effects separately, denoted by γ_s and α_t respectively, but not jointly. We fix the number of legalized migrants per capita at the state level to its 1992 value and compare its impact on a governor's re-election prospects before 1992, when IRCA migrants were ineligible to vote, and after 1992, when IRCA migrants earned the right to vote.[29] The specification is expressed in equation 5, where $R_{s,t}$ is an indicator for whether the governor in state s has been re-elected in year t and P_{1992} is an indicator that is one for $t \geq 1992$ and zero otherwise.

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29. An alternative way is to simply use the per capita legalized at the state level with a five or six year lag to account for the time between legalization and naturalization. Doing so produces results very similar in terms of magnitude and precision to the results reported when using an interaction.

$$R_{s,t} = \beta_0 + \gamma_s + \alpha_t + \beta_1 \cdot ln(IRCA + 1)_{s,1992} \times P_{1992} + \Theta \cdot \mathbf{X_{s,t}} + \epsilon_{s,t} \qquad (5)$$

The result is presented in Column 1 of Table 8. It suggests that the effect of legalization on a governor's chances for re-election are significantly greater after 1992 when undocumented migrants first gained the right to vote. This lends further credence to the political economy nature of our baseline results.[30] In Column 2 of Table 8, we test for an alternative channels through which legalization may influence an incumbent's re-election chances, namely, through networks of friends and families.[31] As such, we introduce the percentage of the state's population that is of Hispanic origin. The coefficient on the interaction between legalization at the state level and percent of the population of Hispanic origin is positive and significant, suggesting that immigrant legalization has stronger effects on re-election in those states with thicker Hispanic networks.

Table 8 here.

7. Capturing the Vote of the Newly Legalized

Thus far, we have demonstrated a robust relationship between immigrant legalization and the distribution of public resources. Governors allocate more resources to counties as the share of newly documented migrants in those counties increase, an allocation which is particularly sensitive to the political context of the incumbent. Until now, however, we have implicitly assumed that the inter-governmental revenue is intended to capture the vote of the migrants who obtained legal status through the IRCA. Of course, because the inter-governmental revenue we observe is aggregated at the county level, we are unable to verify with certainty which constituents these monies are actually intended to reach. In this section, therefore, we present a number of pieces of evidence that demonstrate that it is indeed the IRCA migrants, and not

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30. One question that might arise is how visible these transfers are in the sense of their ability to influence public opinion about the incumbent. In Table A.8 in the Online Appendix we investigate how the allocation on state aid affects local spending and the coefficient suggests that across all categories of local spending, the elasticity of such spending with respect to state aid is positive and precisely estimated, indicating that these transfers are visibly felt at the local level.

31. See, for example, McCann and Jones-Correa (2016) for a discussion on the political engagement of undocumented migrants through family and friend networks.

anti-migrant groups, who motivate the state governor.

7.1 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.[32] In his 1994 re-election campaign, Governor Wilson pinned his hopes onto Proposition 187, the "Save Our State" ballot initiative, and the Republican Party offered ide-ological and financial backing to see the proposition go through. Proposition 187 prohibited undocumented migrants from using non-emergency public services and required the providers of such services to immediately report undocumented migrants for deportation. It was passed by California's voters only to be struck down by a federal court. The proposition, and Wilson's campaign to support it, was highly controversial and left somewhat of an enduring legacy. Bowler, Nicholson, and Segura (2006), for example, find that racially charged ballot initiatives in California—and specifically Proposition 187—are significantly associated with a shift in political support away from the Republican party and towards the Democratic party on behalf of non-Hispanic white voters as well as Latino voters.

In light of this political context, it seems reasonable to ask to what degree our results are actually driven by governors catering to anti-migrant sentiment arising out of the IRCA rather than to the needs of the documented migrants themselves. We examine this question first by quantifying the impact of Governor Wilson's term in office and of Proposition 187 on state aid. In Column 1 of Table 9, 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

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

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.[33] 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]

Table 9 here.

7.2 The IRCA and Voting

The American National Elections Studies (ANES) provides individual level data from its own surveys on matters related to political participation and public opinion. In this subsection, we draw on the ANES data in order to better understand the electoral relevance of the IRCA. That is, we test whether individuals in counties affected by the IRCA voted more or less as compared to those not living in such counties. We also use this data to identify survey respondents who hold plausibly anti-migrant attitudes in order to examine whether the effect of the IRCA on individual voting propensity is affected in any way by the voting behavior of people with such sentiments.

To identify individuals with negative attitudes towards undocumented migrants, we exploit the "illegal alien" thermometer of the ANES. This question asks people to gauge their feelings towards undocumented migrants on a scale between 0 (very cold) and 100 (very warm).

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33. 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`.

34. 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.9, 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.

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.[35] We code individuals with thermometer scores less than 50 as "anti-migrant" and those with scores greater than 50 as "pro-migrant".

Table 10 contains the regression results. Columns 1 and 2 suggest that individuals in counties with a larger share of documented migrants tend to vote significantly more in Presidential and Gubernatorial elections than individuals in counties with less documented migrants. In Columns 3 and 4 we test whether these results are affected in any way by individuals with negative feelings towards undocumented migrants. In both cases, the introduction of such individuals does not affect the baseline effect of legalization on an individual's propensity to vote. If anything, such people tend to turnout less, suggesting that the positive impact of the IRCA on people's propensity to vote is not driven by an increase in turnout among those with negative feelings towards undocumented migrants.[36]

Table 10 here.

7.3 The IRCA, 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. Figure 7 plots event study estimates when the legalization intensity in 1992 is interacted with year dummies in regressions with various categories of local expenditure as the outcome. These figures suggest that the IRCA led to increases in local expenditure in the areas of health, education and welfare but that these increases are estimated with precision for education expenditure between 1991 and 1993.

Figure 7 here.

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35. Figure A.11 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.

36. As a further check of the IRCA's electoral relevance, we regress county level turnout in Presidential elections on legalization. The results are shown in Table A.10 in the Appendix and indicate a positive and significant relationship.

To understand whether these educational expenditures were intended to benefit the newly documented migrants and/or their families, we calculate race-specific high school completion rates to 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 6, 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_{mse}^j is an indicator that is one when $j = mse$ and zero otherwise $\forall$ $j \neq 7$. All other terms are defined as before.

$$H_{i,c,mse} = \delta_c + \psi_{mse} + \sum_{j=1}^{20} \beta_j \left[ln(IRCA + 1)_{c,1992} \times D_{mse}^j \right] + \epsilon_{c,mse} \tag{6}$$

We first run the specification on a sample of only Hispanic individuals and then on a sample of only Caucasian persons and plot the corresponding coefficients, β_j, as shown in Figure 8. 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 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. The fact that the effect only becomes precisely estimated as of 1994 suggests that the increased high school completion

rates among Hispanics arises not just from legal status but from additional resources that these counties receive for education on account of that status.[37] For Caucasians individuals, 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 newly documented migrants. They also underscore the role of the distribution of local public spending in linking legal status to improvements in various socio-economic outcomes, in this particular case education.

Figure 8 here.

8. Conclusion

Undocumented migration is a hotly contested issue in the United States where the number of undocumented 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 documenting such a large number of undocumented migrants remains an open and highly salient question. Our 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 per capita resources to counties affected by amnesty than counties unaffected by it. This central finding points to four main lessons which, we believe, 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 highlighting the fact that migrants, documented or not, are net contributors to the economy.

Second, in trying to understand why legal status affects the distribution of public finances,

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37. 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.

we uncovered political economy forces at work. We presented evidence that 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 of economic necessity or mechanical welfare increases. Indeed, we found no evidence of additional welfare expenditure as a result of the IRCA. We also found that individuals residing in IRCA-affected counties tend to vote more in gubernatorial elections and that a governor's chances for re-election increase as the share of newly legalized migrants in a state increases, suggesting that the strategic allocation of resources is electorally relevant for the governor.

Third, our analysis indicates that neither the distributional impact of immigrant legalization nor the increases in individual voter turnout it induced are driven by individuals with negative feelings towards undocumented migrants. Instead, we found that county expenditure in education increases significantly in IRCA-affected counties, leading to improvements in Hispanic high school completion. These findings indicate that nativist backlash plays little to no role in the political and distributional response to immigrant legalization.

Fourth, the improvements in Hispanic educational outcomes in IRCA-affected counties were discernible only as of 1994, long after legal status was granted but soon after these counties were targeted for additional investments in education. This finding demonstrates the important role that local public expenditure plays in linking legal status to improvements in socio-economic outcomes of individual migrants, in this particular case education.

On the whole, then, our work points to a significant political economy dimension to immigrant legalization. Offering legal status not only leads to various social and economic improvements at the local level but also provides politicians with strong electoral incentives to see that it does so.

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9. Figures

Figure 1
Stock of IRCA applicants in treated counties on a log scale
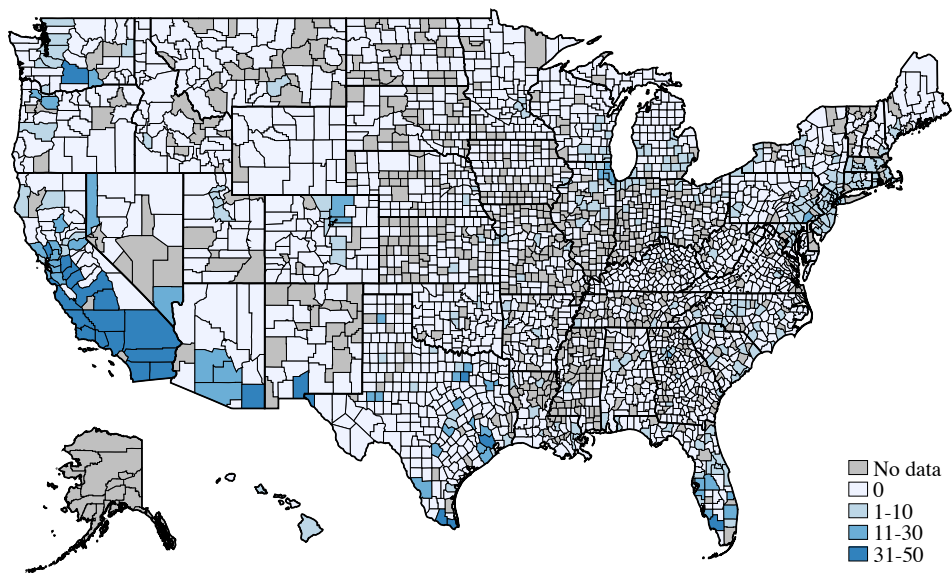
Figure 2
Number of legalized migrants per 1,000 county inhabitants in 1992

Percent of IRCA Migrants...
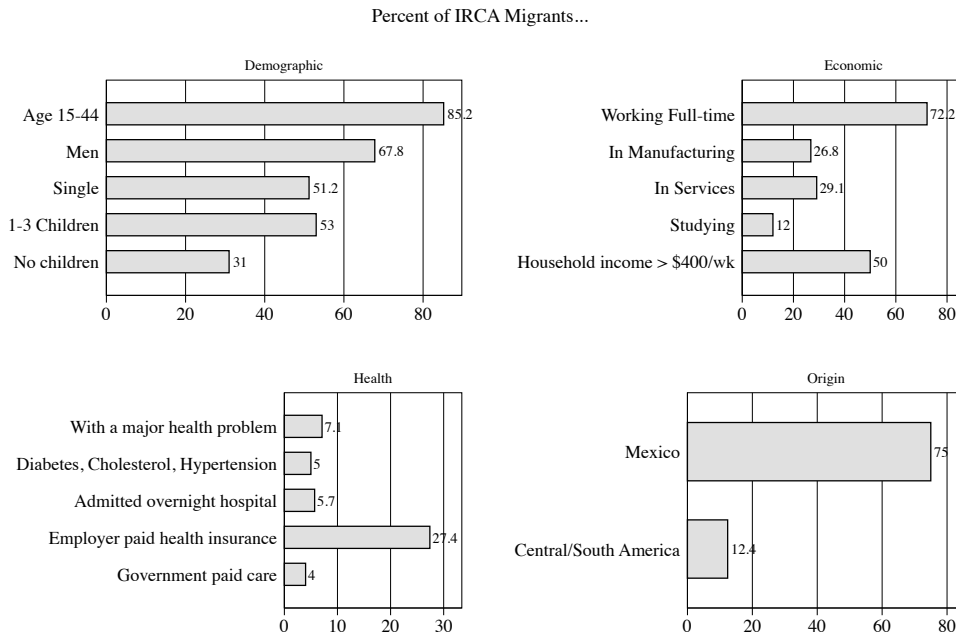
Figure 3
Socio-economic characteristics of the IRCA applicants

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

Figure 4
Evolution of IGR

Note: This figure compares per capita inter-governmental revenue received in those counties that never received applications for legal status (control) with those counties that did receive applications for legal status (treated) through the IRCA.
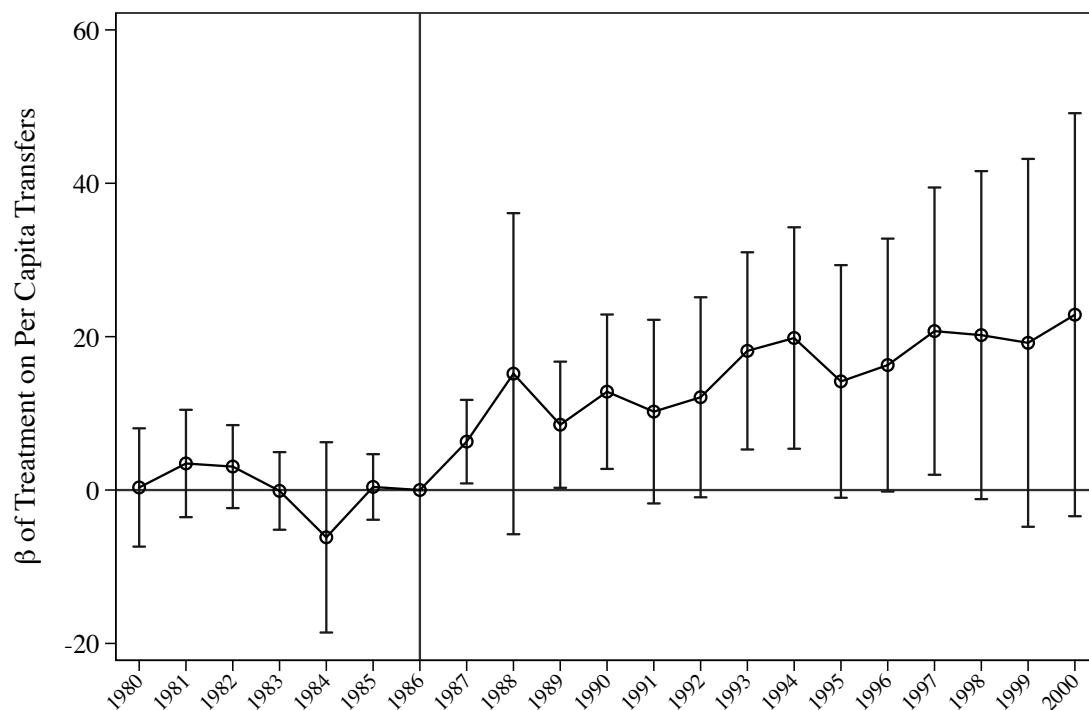
Figure 5
Treatment indicator interacted with year dummies

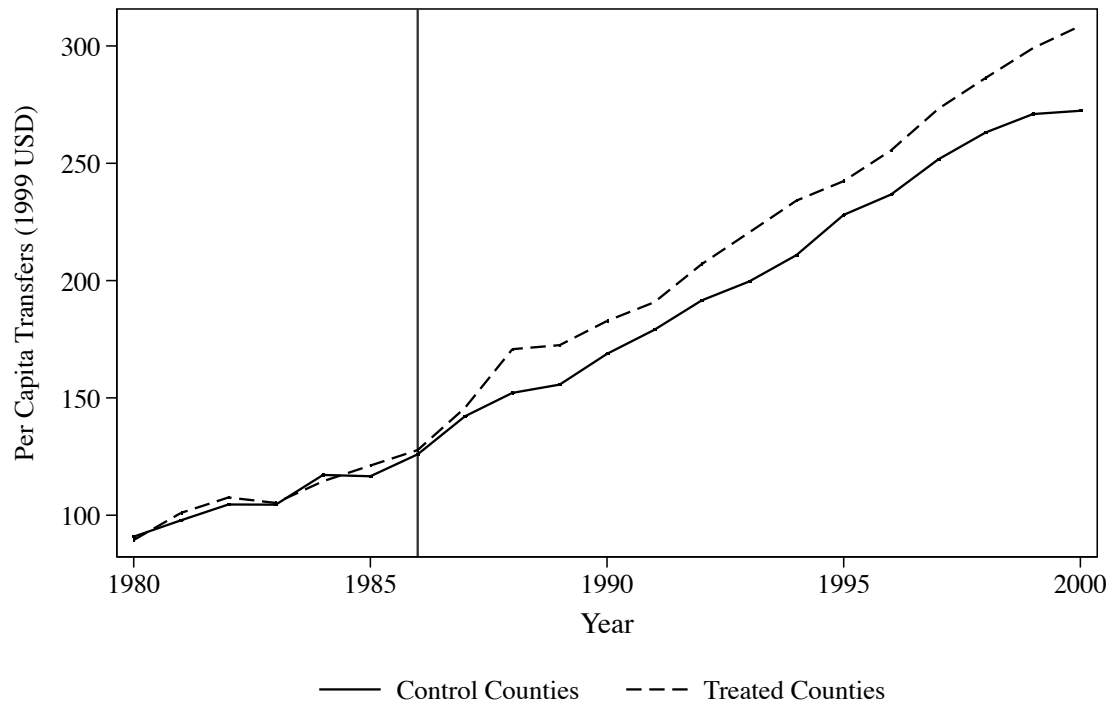**Note:** This figure plots the regression coefficient on a treatment indicator when it is interacted with year dummies as specified in equation 2. The regression only includes county and year fixed effects. The outcome, like Figure 4, is per capita inter-governmental revenue (in 1999 USD). Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. $N = 43,868$.
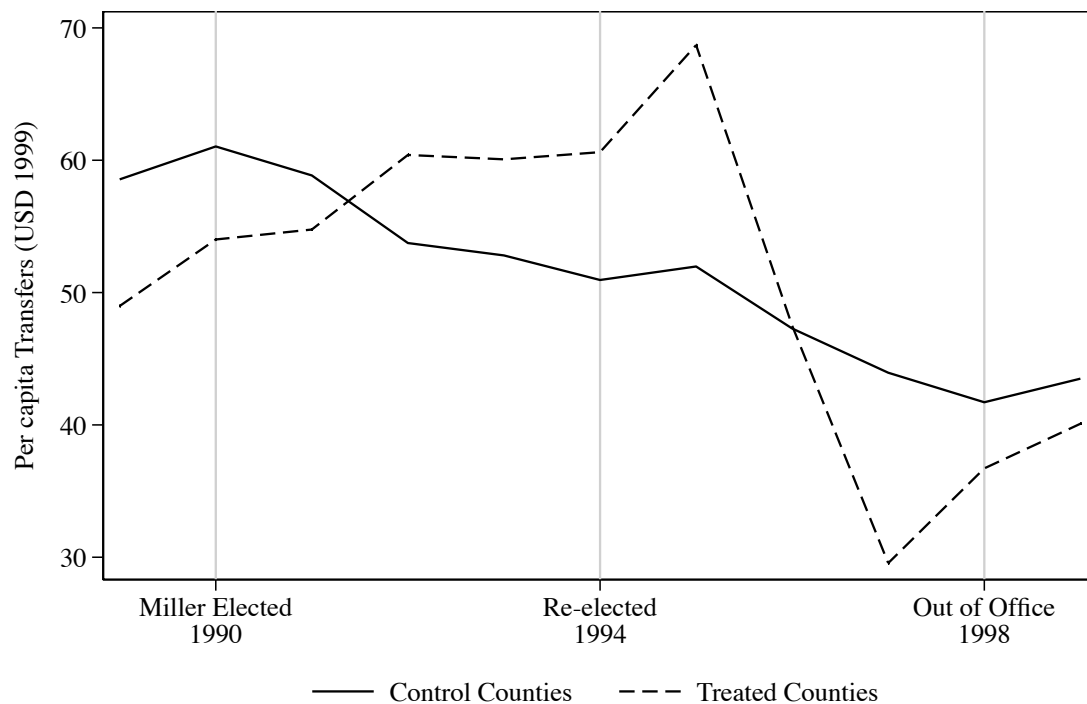
Figure 6
Transfers in Zell Miller's Georgia

Note: These are trends in inter-governmental transfers in 1999 USD in Georgia during Governor Zell Miller's time in office. Control counties are those that never received applications for legal status through the IRCA whereas treated counties did receive applications for legal status through the IRCA. Georgia comprises 157 counties of which we have data on 137: eight affected by the IRCA and 129 were not.
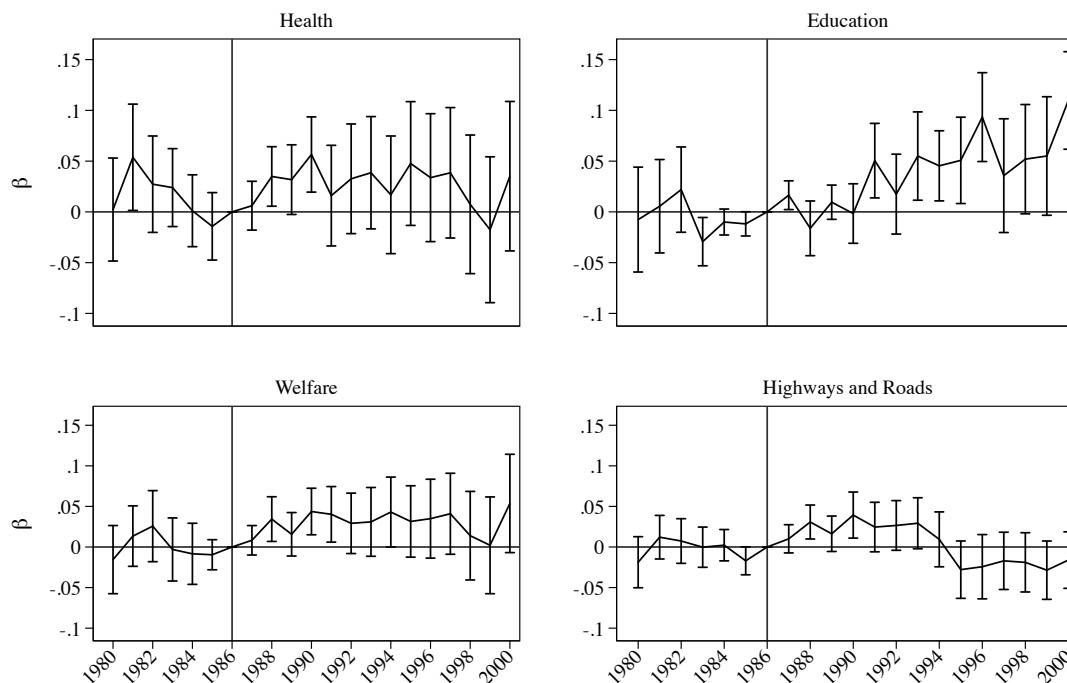
Figure 7
Event study estimates of local expenditure on legalization

Note: This graph plots the regression coefficient on the log of the cumulative number of IRCA applications in a given county per 1000 county inhabitants (plus one) in 1992 when it is 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 state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. $N = 34,840$ for all regressions.
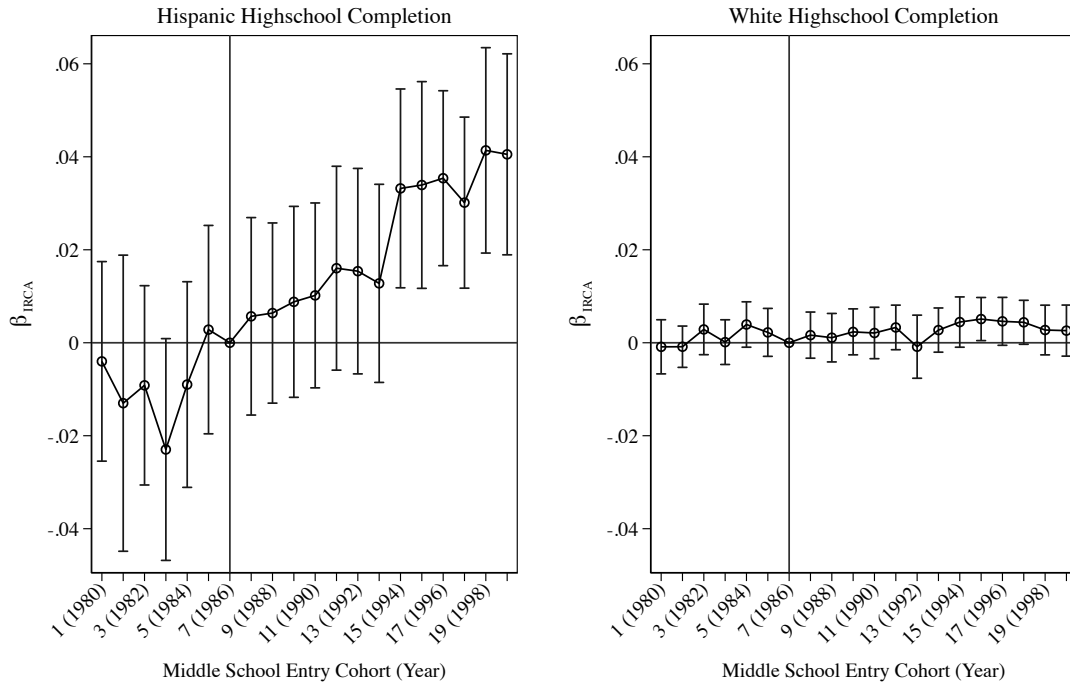
Figure 8
Event study estimates of high school completion on legalization

Note: This graph plots the regression coefficient on legalization intensity in 1992 when it is interacted with middle school entry cohort dummies as shown in equation 6. A person is placed in a middle school entry cohort depending on the year in which they were 12 years of age. 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. The regressions include county and cohort fixed effects. The panel on the left plots coefficients when the sample is restricted only to Hispanic individuals whereas the figure on the right estimates the coefficients on a sample of only Caucasian individuals. Standard errors are clustered at the county level and confidence intervals are drawn at 99 percent. For the Hispanic sample, $N = 81,111$ whereas for the Caucasian sample $N = 202,406$.

10. Tables

Table 1

Inter-Governmental Revenue on IRCA Legalizations

	Log of Inter-governmental Revenue (per capita)				
	(1) Baseline	(2) Drop Top 5	(3) Pop < 430,728	(4) Matching	(5) Linear Trends
Panel A. Treatment Indicator					
Treatment × Post	0.0709***	0.0725***	0.0498***	0.138***	0.0556**
	(0.0183)	(0.0199)	(0.0192)	(0.0314)	(0.0241)
Panel B. Treatment Intensity					
Log legalizations	0.0610***	0.0929***	0.0457***	0.0688***	0.0462***
	(0.0143)	(0.0217)	(0.0156)	(0.0157)	(0.0173)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	No	Yes	No
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	No	Yes
County-Year Linear Trends	No	No	No	No	Yes
Observations	46,820	43,952	45,220	12,042	46,820
Number of Counties	2,686	2,526	2,614	604	2,686

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 in a given county in a given year per 1000 county inhabitants (plus one). 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 2
Robustness Checks

	Log of Inter-governmental Revenue (per capita)						
	(1) $\Delta y_{1992-1982}$	(2) 1980 PC	(3) IRCA^2	(4) Tax Control	(5) Dem. Controls	(6) No Controls	(7) IV
Log legalizations	0.0777*** (0.0201)	0.0583*** (0.0140)	0.102*** (0.0363)	0.0468*** (0.0134)	0.0599*** (0.0151)	0.0565*** (0.0141)	0.199*** (0.0635)
Log legalizations^2			-0.0140 (0.0108)				
Control Variables	Yes	Yes	Yes	Yes	Yes	No	Yes
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
Tax Controls	No	No	No	Yes	No	No	No
Observations	4,208	41,349	46,820	36,869	46,820	46,820	46,810
Number of Counties	2,104	2,211	2,686	2,638	2,686	2,686	2,685

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log legalizations is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Column 1 is a first-difference estimation using values from 1992 and 1982. Column 2 carries out the analysis using per capita legalization and per capita transfers calculated with 1980 county population in the denominator. The specification in Column 4 includes per capita tax revenue at the county level as an additional control while additional controls in Column 5 include the share of county population that is over 18, the share of county population that is Hispanic and the share of county households with children. Column 7 uses the share of foreign-born people in a county in 1960 interacted with year dummies as an instrument for log legalizations. Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3
The IRCA and SUTVA

	Log of Inter-governmental Revenue (per capita)	
	(1) Full Sample	(2) Control States
Log legalizations	0.0988***	0.0782***
	(0.0143)	(0.0177)
Control Variables	Yes	Yes
Year Fixed Effects	Yes	Yes
County Fixed Effects	Yes	Yes
Observations	46,826	10,771
Number of Counties	2,686	749

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log legalizations is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). 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. 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 4
State Revenues and the IRCA

	Log of State Revenue From...		
	(1) Sales Tax	(2) Income Tax	(3) Federal Gov't
Log Legalizations, State	0.028** (0.013)	0.022* (0.011)	0.013* (0.007)
Control Variables	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Observations	853	853	916
Number of States	41	41	44

Notes: The dependent variable is the log of state revenue from various sources. Log legalizations, State is the log of the cumulative number of IRCA applications in a given state in a given year per 1000 state inhabitants (plus one). We control for state unemployment, population and income. Standard errors (shown in parentheses) are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5

Legalization and Political Heterogeneity

	Log of Inter-governmental Revenue (per capita)			
	(1) Party	(2) Aligned	(3) Incentive	(4) Election Cycle
Log legalizations	0.0516*** (0.0160)	0.0544*** (0.0144)	0.0420*** (0.0143)	0.0527*** (0.0157)
D-Governor × Log legalizations	0.0234* (0.0121)			
Aligned		0.000135 (0.00791)		
Aligned × Log legalizations		0.0127 (0.00941)		
Log legalizations × Incentive			0.0246** (0.0107)	
Log legalizations × Election Year				0.0188*** (0.00706)
County Controls	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	46,393	45,395	41,807	30,694
Number of Counties	2,686	2,639	2,677	2,381

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log legalizations is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). D-Governor is an indicator that is 1 if the party of the governor is Democratic and 0 if Republican. Aligned is an indicator that is 1 if the county's election results in the most recent Presidential election (Democrat or Republican) are aligned with the party of the Governor and 0 if not. Incentive is an indicator that is 1 if a governor is not a lame duck and 0 otherwise. Election Year is an indicator according to whether a governor is in an election year or not. The baseline effects of D-Governor, Incentive and Election Year are captured by state-year fixed effects and are thus unable to be estimated. The outcome variable in Column 4 is lagged by one year. 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 6
Legalization and Tightness-of-the-Race

	Log of Inter-governmental Revenue (per capita)			
	(1) Tightest 25%	(2) Tightest 5%	(3) Tightest 25%	(4) Tightest 5%
Log legalizations	0.0244* (0.0129)	0.0378*** (0.0142)	0.0370 (0.0227)	0.0833** (0.0378)
Log legalizations × Tight State 25	0.0550*** (0.0133)		0.0179 (0.0228)	
Log legalizations × Tight State 5		0.0261*** (0.00908)		-0.0349 (0.0359)
Log legalizations × Tight State 25 × Post$_{1992}$			0.0604** (0.0300)	
Log legalizations × Tight State 5 × Post$_{1992}$				0.0751** (0.0361)
Control Variables	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	46,820	46,820	46,820	46,820
Number of Counties	2,686	2,686	2,686	2,686

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log legalizations is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Tight State 25 and Tight State 5 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 25th and 5th percentile of the competitiveness distribution. Post$_{1992}$ is 1 for years 1992 and onward and 0 otherwise. The regressions in Columns 3 and 4 include all pairwise 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 7
Veto and State Legislatures

	(1) G with Veto	(2) Legislative Alignment	(3) Party of Legislature
	Log of Inter-governmental Revenue (per capita)		
Log legalizations	0.0153 (0.0332)	0.0535*** (0.0139)	0.0629*** (0.0234)
Log legalizations × G has veto	0.0530* (0.0287)		
Log legalizations × Alignment		0.0245** (0.0109)	
Log legalizations × Both Houses R			-0.0104 (0.0215)
Log legalizations × Both Houses D			-0.00220 (0.0202)
County controls	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
Observations	42,369	46,820	45,476
Number of Counties	2,682	2,686	2,605

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log legalizations is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Alignment is an indicator that is one when the party of the Governor is aligned with the partisan majority of the state legislature and 0 when it is not. Both Houses "R" or "D" means both Houses of the state legislature have a Republican or Democratic majority, respectively. The omitted category in Column 3 is when both Houses of the state legislature are split and have no partisan majority. 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 8
Legalization and Governor Re-election

	1 = Governor Re-elected	
	(1) Post$_{1992}$	(2) Hispanic Networks
Log state legalizations$_{1992}$ × Post$_{1992}$	0.241*** (0.0700)	
Percent Hispanic, State		-0.105** (0.0434)
Log state legalizations$_{1992}$ × % Hispanic, State		0.0779*** (0.0180)
State controls	Yes	Yes
Year Fixed Effects	Yes	Yes
State Fixed Effects	Yes	Yes
Observations	194	194
Number of States	45	45

Notes: The dependent variable is an indicator that is 1 when an incumbent governor eligible for re-election is re-elected and 0 otherwise. Log state legalizations$_{1992}$ is the log of the cumulative number of IRCA applications in a given state per 1000 state inhabitants (plus one) in 1992. Its baseline effect cannot be estimated as it is subsumed by state fixed effects. Post$_{1992}$ is an indicator that is 0 prior to 1992 and 1 otherwise. Percent Hispanic, State is the percent of the state's population that is of Hispanic origin. Control variables include party of the governor as well as poverty and unemployment rates, log of population and log of income, all aggregated to the state level. Standard errors (shown in parentheses) are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 9
IRCA and Anti-Migrant Sentiment

	Log of Inter-governmental Revenue (per capita)		
	(1) Wilson	(2) No Cali	(3) No Anti-Migrant States
Log legalizations	-1.844	0.0834***	0.0579**
	(1.601)	(0.0182)	(0.0238)
Log legalizations × Prop 187	0.0372		
	(0.0241)		
Control Variables	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No
State-Year Fixed Effects	No	Yes	Yes
Observations	428	45,690	32,232
Number of Counties	54	2,632	1,864

Notes: The dependent variable is the log of per capita transfers from state to local governments (aggregated to the county) in 1999 USD. Log legalizations is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Prop187 is the county vote share for Proposition 187, the baseline effect of which is subsumed by county fixed effects. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10
Individual Voting using ANES Data

	I voted in the following elections:			
	(1) Presidential	(2) Gubernatorial	(3) Presidential	(4) Gubernatorial
Log legalizations	0.0302***	0.0242*	0.0328***	0.0255**
	(0.00940)	(0.0123)	(0.0101)	(0.0124)
Anti-Migrant			0.000411	-0.0515**
			(0.0215)	(0.0253)
Log legalizations × Anti-Migrant			-0.0431*	0.000236
			(0.0237)	(0.0124)
Control Variables	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Income Dummies	Yes	Yes	Yes	Yes
Education Dummies	Yes	Yes	Yes	Yes
Observations	6,505	4,902	6,301	4,559
Number of Counties	194	180	193	167
Years in Sample	80, 84, 88, 92, 96	80, 82, 86, 90, 94, 98	80, 84, 88, 92, 96	80, 82, 86, 90, 94, 98

Notes: The dependent variable is an indicator for whether individual i in given county c voted in an election. Log legalizations is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Control variables include log of county population as well as individual controls for marital status, age, age^2 and indicators for being Hispanic, White or Black as well as for being male or not. Income dummies include indicators for five different percentiles of the income distribution (0-16th, 17th-33rd, 34th-67th, 68th-95th, and 95th-100th) while education dummies include indicators for four categories of educational attainment (grade school or less, high school, some college, college degree or more). Standard errors (shown in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

FOR ONLINE PUBLICATION

A.1 Analysis of the Framework

Our analysis begins by taking first order conditions of equation 1 with respect to g:

$$\frac{\partial \Pi}{\partial g}\bigg|_{g=g^*} = (1 - \alpha)U_C'(g^*) + \alpha U_L'(g^*) + E \cdot \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial \phi}{\partial g^*} - C'(g^*) \overset{!}{=} 0 \tag{7}$$

To understand how g^* responds to a sudden shock in legal status, α, we maximize 7 and this is implicitly given by the following:

$$\frac{\partial g^*}{\partial \alpha} = -\frac{U_L'(g^*) - U_C'(g^*) + E \cdot \frac{\partial \frac{\partial \Omega}{\partial \phi} \frac{\partial \phi}{\partial g}}{\partial \alpha}}{\frac{\partial^2 \Pi}{\partial g^2}\bigg|_{g=g^*}} \tag{8}$$

Because $\frac{\partial^2 \Pi}{\partial g^2} < 0$ the sign in front of equation 8 becomes positive. Moreover, we have assumed that $U_L'(g) > U_C'(g) \ \forall \, g$; accordingly, the first term in the numerator, $U_L'(g^*) - U_C'(g^*) > 0$. The overall sign of equation 8 thus hinges on the sign of the second term in the numerator which can be expressed as follows:

$$\frac{\partial \frac{\partial \Omega}{\partial \phi} \frac{\partial \phi}{\partial g}}{\partial \alpha} = \frac{\partial \frac{\partial \Omega}{\partial \phi}}{\partial \alpha} \cdot \frac{\partial \phi}{\partial g} + \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial \frac{\partial \phi}{\partial g}}{\partial \alpha} \tag{9}$$

Rewriting $\frac{\partial \frac{\partial \Omega}{\partial \phi}}{\partial \alpha} = \frac{\partial \frac{\partial \Omega}{\partial \phi}}{\partial \alpha} \cdot \frac{\partial \phi}{\partial \phi} = \frac{\partial^2 \Omega}{\partial \phi^2} \cdot \frac{\partial \phi}{\partial \alpha}$, and $\frac{\partial \frac{\partial \phi}{\partial g}}{\partial \alpha} = \frac{\partial \frac{\partial \phi}{\partial g}}{\partial \alpha} \cdot \frac{\partial g}{\partial g} = \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha}$ we can substitute these back into equation 9 to obtain:

$$= \frac{\partial^2 \Omega}{\partial \phi^2} \cdot \frac{\partial \phi}{\partial \alpha} \cdot \frac{\partial \phi}{\partial g} + \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha}$$

$$= \frac{\partial^2 \Omega}{\partial \phi^2} \cdot \frac{\partial \phi}{\partial \alpha} \cdot \frac{\partial \phi}{\partial g} \cdot \frac{\partial g}{\partial g} + \frac{\partial \Omega}{\partial \phi} \cdot \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha}$$

$$= \frac{\partial^2 \phi}{\partial g^2} \cdot \frac{\partial g}{\partial \alpha} \cdot \left(\frac{\partial^2 \Omega}{\partial \phi^2} + \frac{\partial \Omega}{\partial \phi} \right)$$

Under the assumption that $\frac{\partial \Omega}{\partial \phi} > |\frac{\partial^2 \Omega}{\partial \phi^2}|$, the overall sign of equation 9 is thus positive. This in turn allows us to state that $\frac{\partial g^*}{\partial \alpha} > 0$.[38]

Prediction 1: The optimal allocation of state aid increases in the share of newly documented migrants in a county.

Given that the second term in the numerator in equation 8 is positive, we can state that $\frac{\partial g^*}{\partial \alpha}\big|_{E=1} > \frac{\partial g^*}{\partial \alpha}\big|_{E=0}$.

Prediction 2: The optimal allocation of state aid is larger when P is eligible for re-election and less when (s)he is a lame duck.

Finally, the functional form of $\Omega(\phi)$ leads us to a final testable prediction. Because $\phi = \phi_T$ represents an inflection point (where ϕ_T represents the winning threshold of an election), it follows that $\frac{\partial \Omega}{\partial \phi}$ is maximized as $\phi \to \phi_T$.

Prediction 3: The optimal allocation of state aid becomes larger the closer the P's expected vote share is to the winning threshold.

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38. Although we have used a general functional form for Ω, for illustrative purposes, we set $\Omega(\phi) = \frac{1}{1+e^{-\phi}}$ and plot the various derivatives of $\Omega(\phi)$, shown in Figure A.1 in the Online Appendix, to provide some intuition behind this assumption.
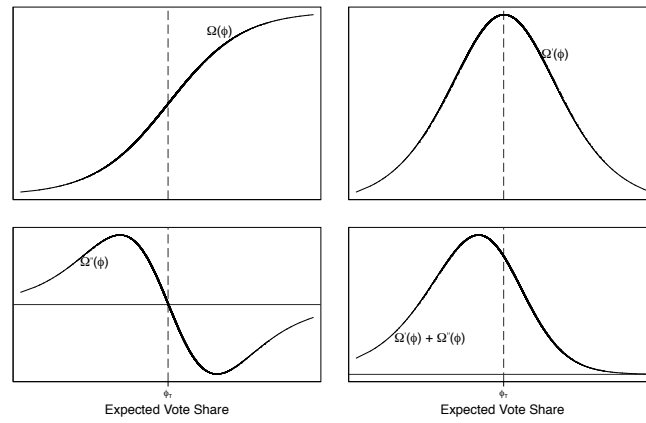
A.2 Additional Figures

Figure A.1
Functional form of $\Omega(\phi)$, its first and second derivative and their sum

Note: This graph plots, clockwise from top-left: $\Omega(\phi)$; $\Omega'(\phi)$; $\Omega''(\phi)$; and $\Omega'(\phi) + \Omega''(\phi)$
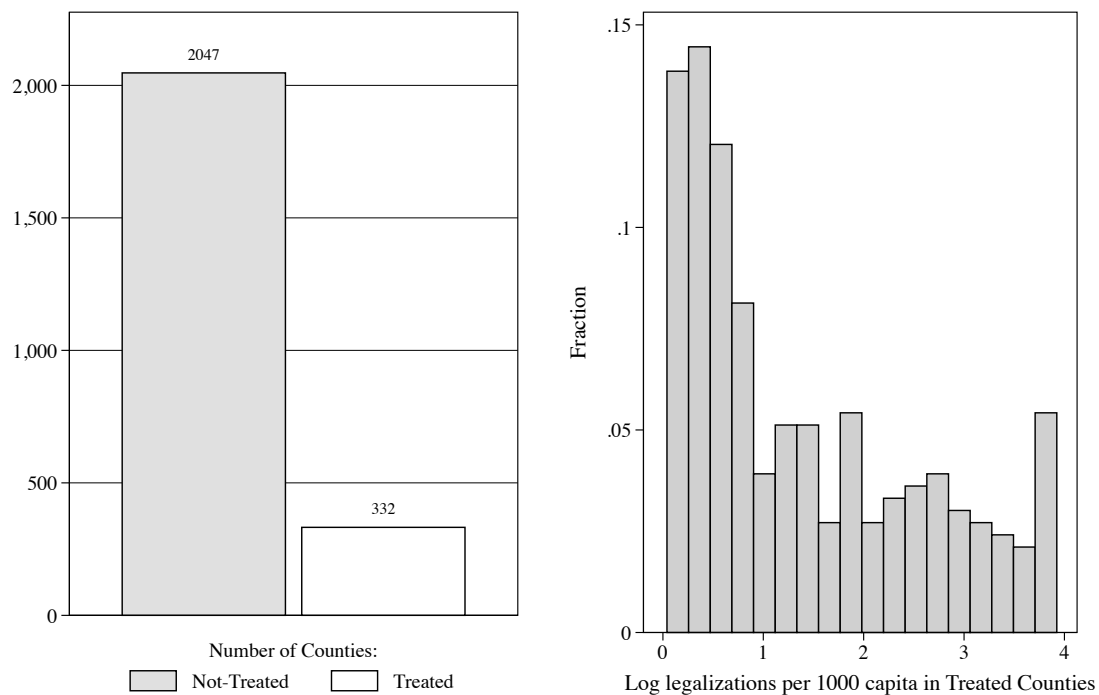
Figure A.2
Distribution of legalized migrants

Note: This figure shows the distribution of the legalized migrants 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, counties affected by the IRCA had, on average, 5,410 legalized migrants residing in them, which corresponds to 11.75 migrants per 1,000 inhabitants.
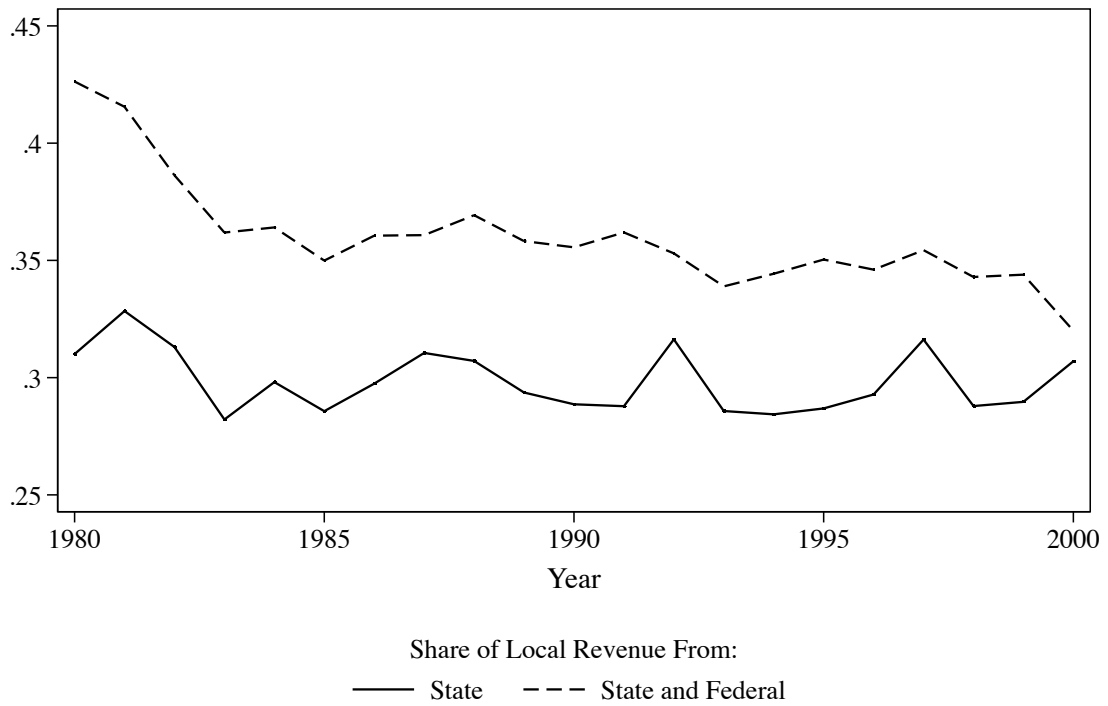
Figure A.3
Sources of local government revenue

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.
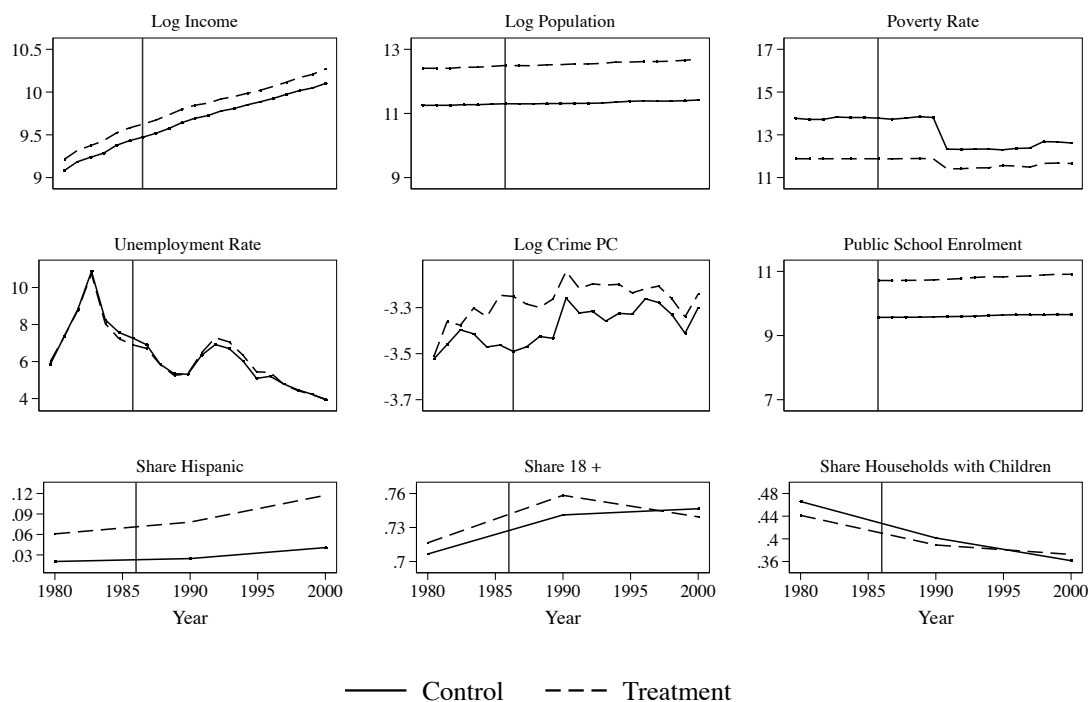
Figure A.4

Trends in county socio-economic characteristics

Note: This graph compares the evolution of various county characteristics in those counties that never received applications for legal status (control) with those counties that did receive applications for legal status (treated) through the IRCA.

Figure A.5

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 rece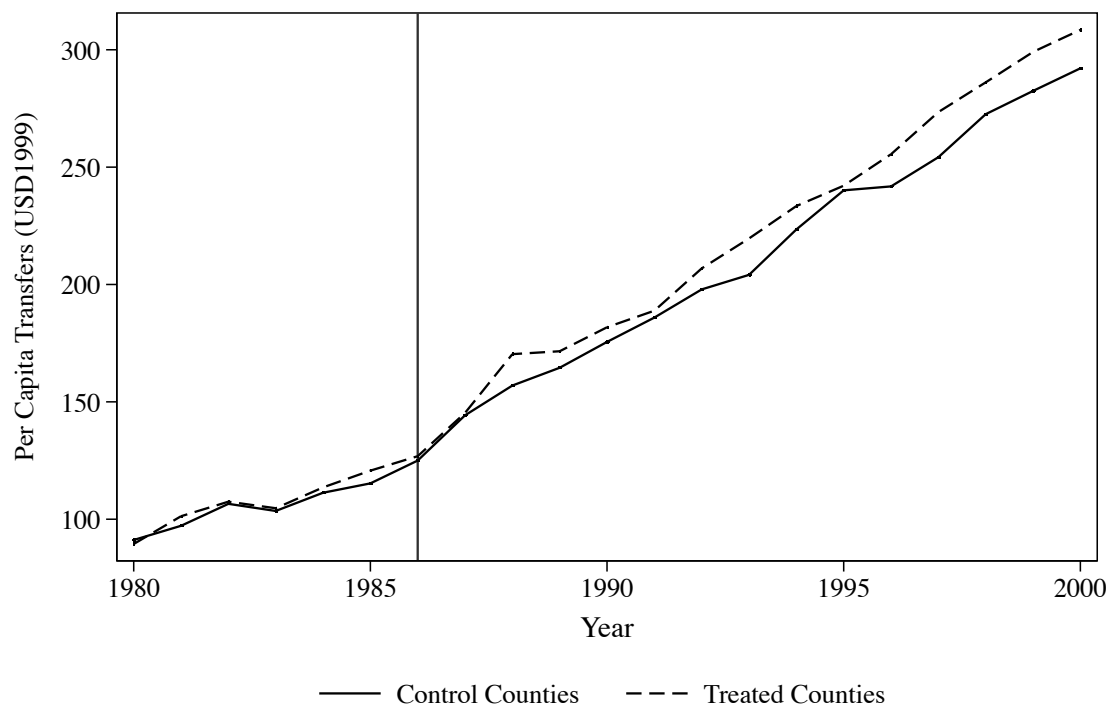ive applications for legal status (treated) through the IRCA in a sample of treated and control counties matched on the basis of propensity scores using the nearest neighbor. The county characteristics on which we base the propensity score matching are county income, population, crime, tax revenue, poverty rate and unemployment in 1980.
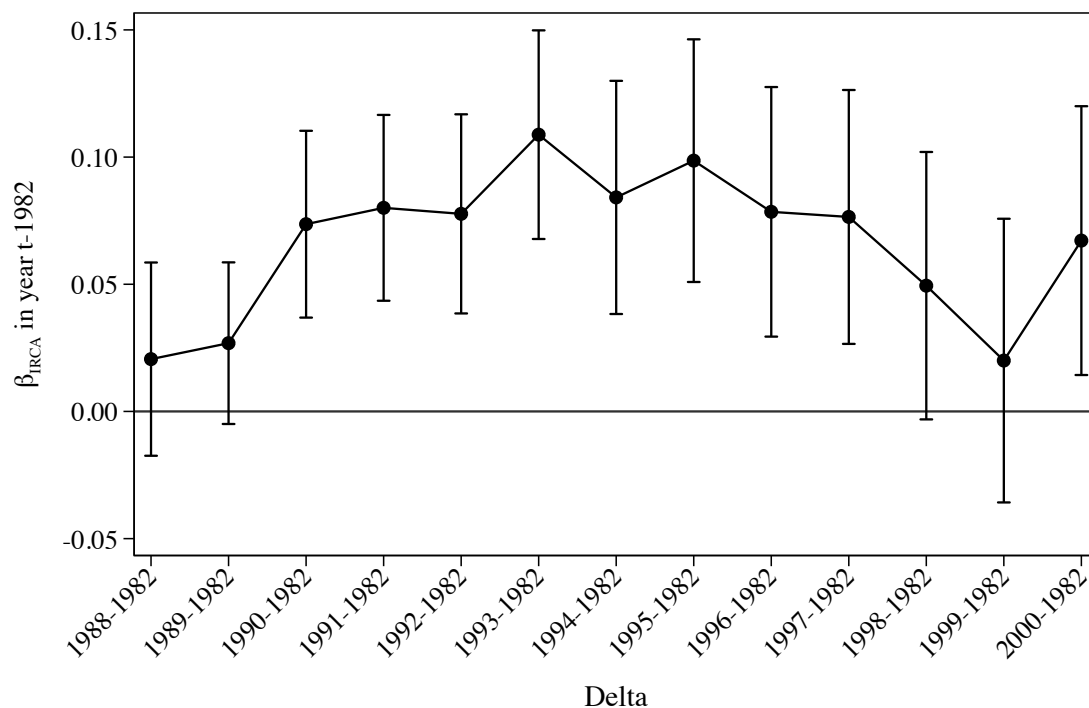
Figure A.6
First-difference coefficient estimates

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 1000 county inhabitants (plus one). 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 also included in the estimations. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.
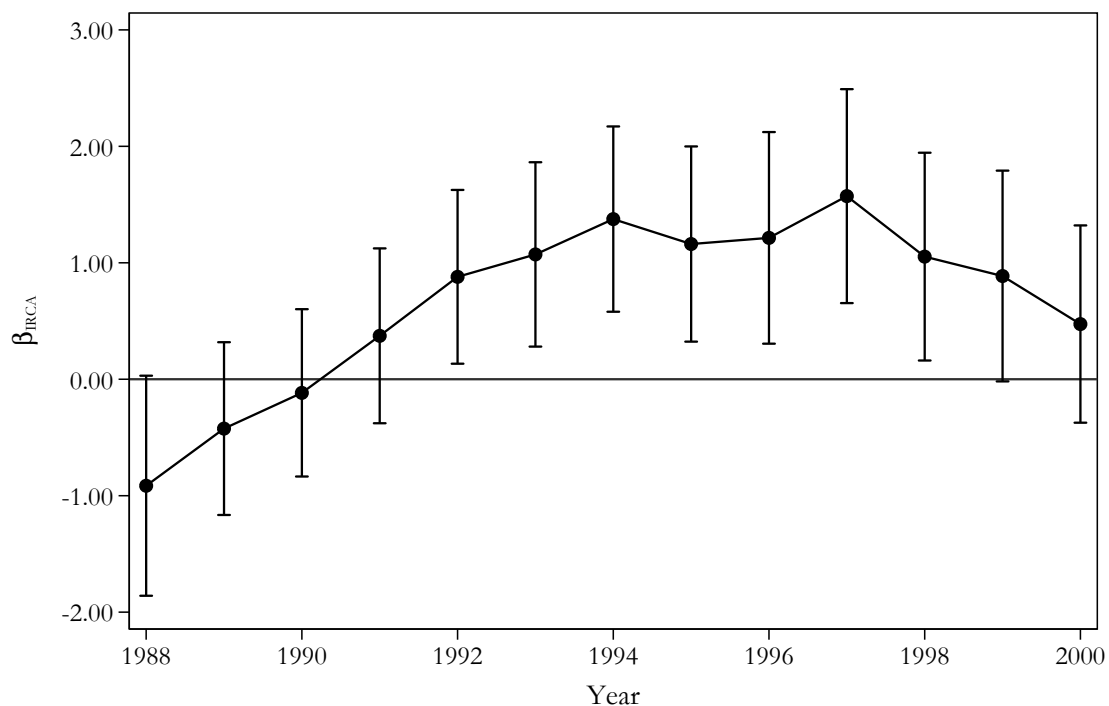
Figure A.7
Instrumental variables regression coefficients

Note: This graph plots β from 13 cross section regressions as specified in equation **??**, one regression each for the years between 1988 and 2000. For each year, the value of the covariates is differenced from their 1982 value. The dependent variable is the log of per capita transfers from state to county governments (in 1999 USD) and β is the coefficient on the natural log of the cumulative number of IRCA applicants per 1000 county inhabitants (plus one) when it is instrumented by the 1960 share of a county that is foreign-born. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent.
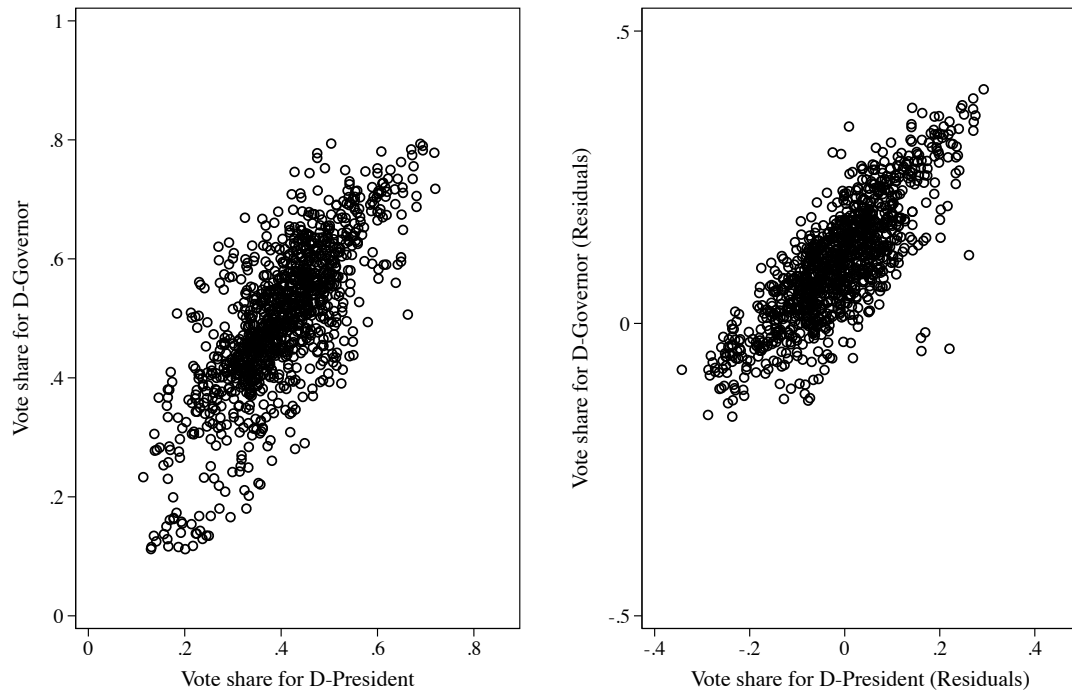
Figure A.8
Presidential and Gubernatorial election results

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.
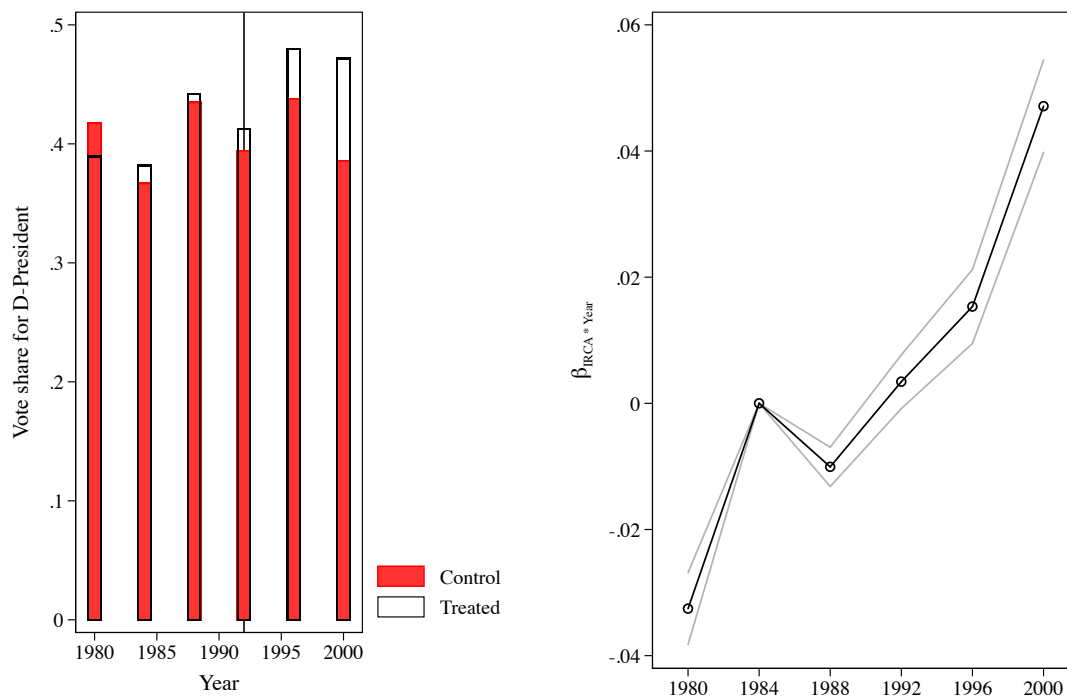
Figure A.9
The IRCA and the Democratic vote share

Note: The panel on left plots the Democratic vote share at the county level in Presidential elections in counties affected by the IRCA against those not affected by the IRCA. The panel on the right shows coefficients from a regression where Democratic vote share (in Presidential elections) is regressed on an interaction between a treatment indicator and year. Control variables include poverty and unemployment rates, log of population and log of income, all aggregated to the county level as well as county fixed effects and state-by-year fixed effects. Standard errors are clustered at the county level and confidence intervals are drawn at 95 percent. $N = 12,754$

Share of Local Spending On...
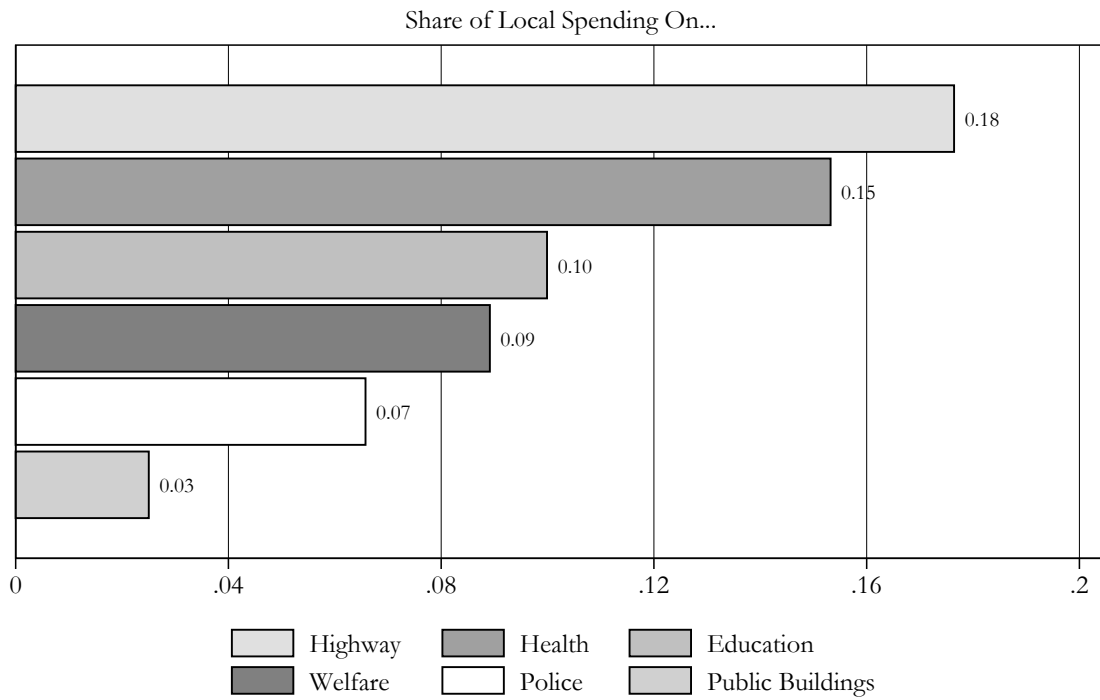
Figure A.10
Share of local expenditure on...

Note: This graph plots various categories of local government expenditure as a share of total local expenditure.
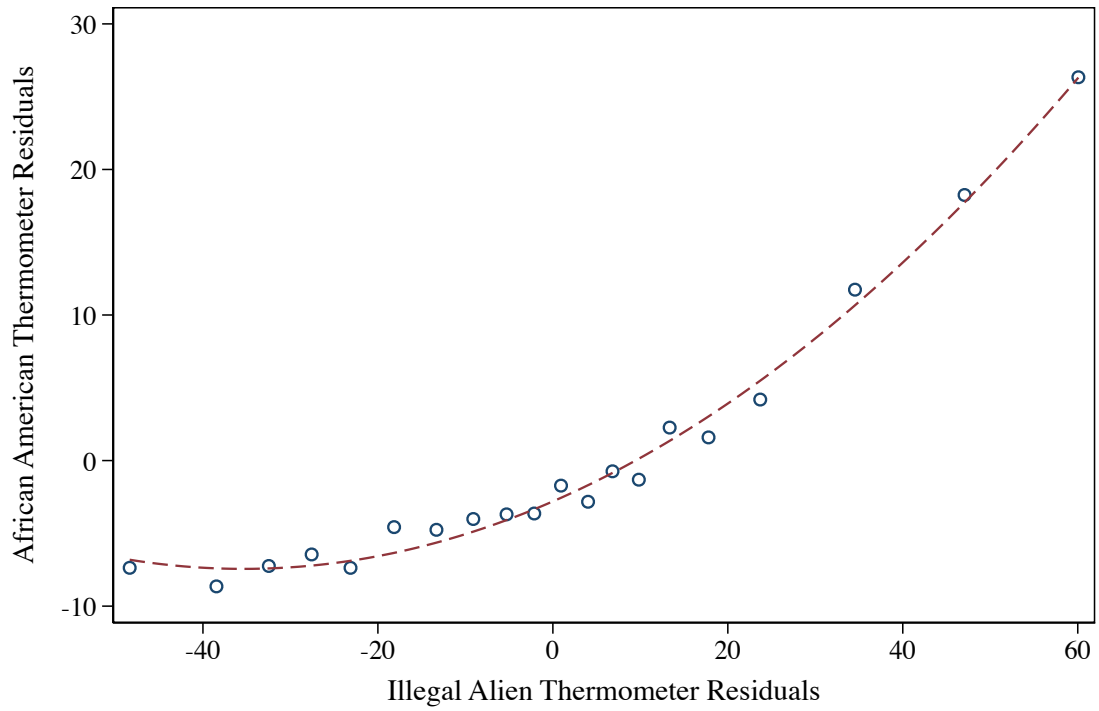
Figure A.11
Attitudes towards African Americans and Undocumented Migrants

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.3 Additional Tables

Balance Table: Treated v. Untreated Counties in 1984

|	Treated			Untreated			Difference	
	Mean	S.D	Counties	Mean	S.D	Counties	Mean	S.D
County Characteristics:								
Transfers (Pc 1999)	144.0	[150.4]	307	147.4	[344.9]	1886	-3.37	(20.0)
Log Of Transfers (Pc 1999)	4.33	[1.31]	307	4.30	[1.31]	1886	0.027	(0.081)
Unemployment Rate	7.98	[1.84]	328	7.98	[2.27]	1892	-0.0017	(0.13)
Poverty Rate	11.9	[5.85]	328	16.8	[7.48]	1892	-4.94***	(0.43)
Population (1000)	399.0	[617.1]	328	32.1	[30.0]	1892	366.8***	(14.3)
Log Of County Income	9.53	[0.19]	328	9.29	[0.18]	1892	0.24***	(0.011)
County Tax Revenue (Pc)	133.9	[116.1]	307	129.1	[130.3]	1838	4.81	(7.91)
Log Of Total County Crimes (Pc)	-3.36	[0.64]	328	-3.67	[0.73]	1892	0.31***	(0.043)
1960 Population Foreign Born (%)	4.62	[3.79]	328	1.77	[2.21]	1892	2.85***	(0.15)
Governor Characteristics:								
Lame-Duck Governor	0.32	[0.47]	328	0.39	[0.49]	1892	-0.071**	(0.029)
State Has Term Limits	0.50	[0.50]	328	0.56	[0.50]	1892	-0.065**	(0.030)
Share Democratic Governor	0.66	[0.48]	328	0.78	[0.41]	1892	-0.13***	(0.025)
Governor Reelected	0.43	[0.50]	30	0.65	[0.48]	209	-0.21**	(0.094)
Percent Votes Cast For D-President	38.2	[9.62]	328	36.7	[10.3]	1892	1.52**	(0.61)

Table A.2
Congressional Vote Record on the IRCA Bill

	House	Senate
Yes	274 (204-D; 70-R)	63 (34-D; 29-R)
No	132 (33-D; 99-R)	24 (5-D; 8-R)
Abstain	26	13

Notes: This table shows how the 99th Congress voted for the IRCA Bill on 17 October 1986.

Source: Congressional Votes Database accessed at `govtrack.us`

Table A.3

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.4
Baseline Results with Alternative Clustering and Inference

	(1) Treatment × Post	(2) Legalisation Intensity
$\widehat{\beta}$	0.0709	0.0610
p-values:		
A. Analytical values		
(clustered at the state level)	0.0325	0.0149
B. Wild Bootstrap values		
(clustered at the state level)	0.0521	0.0390
Observations	46,820	46,820
Number of States	46	46

Notes: This table presents the baseline estimates (Column 1 of Table 1) clustering the standard errors at the state level. *p*-values are derived both analytically, using Stata's conventional `vce(cluster state)` command as well as through Wild cluster bootstrapping generated using Roodman et al. (2018) `boottest` command.

Table A.5

Baseline Results using Linear Legalizations

	Log of Inter-governmental Revenue (per capita)				
	(1) Baseline	(2) Drop Top 5	(3) Pop < 430, 728	(4) Matching	(5) Linear Trends
Legalizations per 1,000 capita	0.00551*** (0.00149)	0.0129*** (0.00332)	0.00362*** (0.00136)	0.00557*** (0.00164)	0.00412*** (0.00158)
Control Variables	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	No	Yes	No
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	No	Yes
Linear Year Trends	No	No	No	No	Yes
Observations	46,820	43,952	45,186	12,042	46,820
Number of Counties	2,686	2,526	2,614	604	2,686

Notes: This table replicates the baseline estimation reported in Panel B of Table 1 but using the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants as the key independent variable without a log transformation. The dependent variable is the log of per capita transfers from state to local governments aggregated to the county in 1999 USD. 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 Dynamics of the 99th Congress

	Log of Inter-governmental Revenue (per capita)	
	(1)	(2)
Log legalizations	0.0442**	0.0444*
	(0.0221)	(0.0248)
Log legalizations × Majority D-Senators in State	0.0201	
	(0.0275)	
Log legalizations × Majority D-Members in State		0.0189
		(0.0293)
Control Variables	Yes	Yes
County Fixed Effects	Yes	Yes
State-Year Fixed Effects	Yes	Yes
Observations	46,820	46,820
Number of Counties	2,686	2,686

Notes: The dependent variable is the log of per capita transfers from state to county governments in 1999 USD. Log legalizations is the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). Majority D-Senators in State is an indicator that is 1 if both senators of a given state are Democrats and 0 otherwise. Majority D-Members in State is defined similarly: it is 1 if the majority of congress members from a given state were Democrats and 0 otherwise. 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
Legalization and Term limits

	Log of IGR (per capita)
	(1) Incentive
Log legalizations	0.254
	(0.197)
Log legalizations × Electoral Incentive	0.0923
	(0.0241)
Control Variables	Yes
County Fixed Effects	Yes
State-Year Fixed Effects	Yes
Observations	12,134
Number of Counties	2,384

Notes: The dependent variable is the log of per capita transfers from state to county governments in 1999 USD. Log legalizations is the log of the cumulative number of IRCA applications from a given county in a given year per 1000 county inhabitants (plus one). Electoral Incentive is an indicator that is 1 if a governor is not a lame duck in the period between 1989 and 1994 and zero otherwise. The baseline effect of Electoral Incentive is captured by state-year fixed effects and is thus unable to be estimated. 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.8
Local Spending and IGR

	Log of Per Capita Local Expenditure				
	(1) Total	(2) Health	(3) Education	(4) Welfare	(5) Highway
Log of Transfers	0.254***	0.285***	0.0435***	0.205***	0.245***
	(0.0184)	(0.0228)	(0.0113)	(0.0199)	(0.0193)
Control Variables	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	36,869	36,869	36,869	36,869	36,869
Number of Counties	2,638	2,638	2,638	2,638	2,638
p-values:					
A. Unadjusted	9.8e-42	8.0e-35	.00011	2.0e-24	6.8e-36
B. Westfall & Young Adjusted	0	0	0	0	0

Notes: This table presents regression results using various categories of per capita local government expenditure as the outcome variable. Log of Transfers is the per capita inter-governmental revenue from the state to local governments aggregated to the county in 1999 USD. The adjusted p-values in the last row are adjusted to take into account potential multiple hypothesis testing using Westfall and Young (1993) family-wise error rate procedure. 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.9

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** (0.00868)	0.0281** (0.0118)	-0.0396** (0.0198)	-0.0415** (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 1000 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.10

Legalization and Turnout in Presidential Elections

	Outcome: County Level Voter Turnout	
	(1) Turnout	(2) $Post_{1992}$
Log legalizations	0.00239** (0.00116)	
Treated × $Post_{1992}$		0.00606*** (0.00186)
County controls	Yes	Yes
County Fixed Effects	Yes	Yes
State-Year Fixed Effects	Yes	Yes
Observations	12,737	12,737
Number of Counties	2,370	2,370

Notes: The dependent variable is the total number of votes in a given county, c, divided by the number of people over the age of 18 in that same county in all Presidential election years from 1980 to 2000. Log legalizations is the log of the cumulative number of IRCA applications in a given county in a given year per 1000 county inhabitants (plus one). Treated is 1 if a county received 1 or more applications for legal status and 0 otherwise. Likewise, $Post_{1992}$ is 1 for time periods on or after 1992 and zero before. Control variables include poverty and unemployment rates, share of the population that is Hispanic, log of population and log of income, all aggregated to the county. Standard errors (in parentheses) are clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$