



Short communication

# Immigrant legalization and the redistribution of state funds: Evidence from the 1986 IRCA

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## ABSTRACT

We study the impact of immigrant legalization on fiscal transfers from state to local governments in the United States, exploiting variation in legal status from the 1986 Immigration Reform and Control Act (IRCA). State governments allocate more resources to IRCA counties, an allocation that is responsive to the electoral incentives of the governor. Importantly, the effect emerges prior to the enfranchisement of the IRCA migrants and we argue it is driven by the IRCA's capacity to politically empower already legal Hispanic migrants in mixed legal status communities. The IRCA increases turnout in large Hispanic communities as well as Hispanic political engagement, without detectably triggering anti-migrant sentiment.

## 1. Introduction

Legal status has first-order economic consequences on the lives of individual migrants and on the communities in which they reside. Immigrant legalization has been shown, for example, to have significant consequences on labor market outcomes (Kossoudji and Cobb-Clark, 2002; Pan, 2012; Rivera-Batiz, 1999), educational outcomes (Cortes, 2013; Sabet, 2023), crime (Pinotti, 2017), safety net transfers (Cascio and Lewis, 2019) and congressional redistricting and Hispanic political representation (Sabet and Yuchtman, 2023). Less attention, however, has been paid to understanding the impact of legal status on fiscal transfers. This is particularly surprising given the rich literature that establishes the politically motivated fiscal responses to changes in political rights brought about by enfranchisement (Cascio and Washington, 2014; Miller, 2008).

In this paper, we aim to fill this gap by analyzing the impact of immigrant legalization on fiscal transfers from state to local governments in the United States. To do so, we exploit variation in legal status arising out of the historic 1986 Immigration Reform and Control Act (IRCA) which legalized millions of mostly Hispanic migrants across US counties. To identify our model, we rely on the fact undocumented migrants are already included in US census counts (Sabet and Yuchtman, 2023; Sabet, 2023). The IRCA thus shocked the legal status of millions of Hispanic migrants without triggering wider socio-economic change, enabling us to isolate the impact of immigrant legalization on fiscal transfers.<sup>1</sup>

Using a differences-in-differences regression framework, we compare the distribution of intergovernmental revenue (IGR) from state to local governments across US counties with differential exposure to the IRCA before and after 1989, the first year when IRCA applicants acquired legal status. Our baseline estimate suggests that a one standard deviation increase in the share of IRCA applicants in a county increases per capita IGR by 6 percent relative to the pre-legalization IGR mean. The result is robust to a wide range of alternative specifications and samples and is not reflective of differential pre-trends. What is more, we use per capita intergovernmental revenue from *federal* to local governments as a placebo outcome because state governments have no control over their distribution. By contrast to our main results, we find that the IRCA has no effect on the distribution of federal IGR to counties.

An important question is the extent to which the fiscal response reflects discretionary political choices as opposed to mechanical forces. To distinguish these explanations, we test for heterogeneous effects according to the political circumstances of the state governor and find a clear pattern: IRCA counties attract differentially more resources from the state when the governor is eligible for re-election. The effect vanishes when the governor is ineligible to run for office because of term limits. By contrast, partisan affiliation, of either the state governor or the state legislature, has no differential effect on state transfers. As before, we find no significant heterogeneous patterns with respect to

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E-mail address: [sabet@econ.uni-frankfurt.de](mailto:sabet@econ.uni-frankfurt.de) (N. Sabet).<sup>1</sup> Both Sabet and Yuchtman (2023) and Sabet (2023) demonstrate that the IRCA is not associated with significant changes, in levels or trends, in a wide range of county socio-economic covariates.

state-level political characteristics when using our placebo measure of fiscal transfers — per capita IGR from federal to local governments. These results suggest that the electoral incentives of state governors — rather than their political affiliation or mechanical forces — drive our results.

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

First, we find that the IRCA increases turnout significantly in communities with larger, *pre-existing* shares of legal Hispanic migrants. What is more, we split the sample according to the size of the pre-existing legal Hispanic population and find that the IRCA has a significant effect on fiscal transfers only in the sample of counties with above median populations of already legal Hispanic migrants. Second, relying on individual survey data, we find that the IRCA leads Hispanics to report significantly higher levels of political engagement and participation post-legalization, including a higher likelihood of volunteering for, donating to, and participating in political campaigns. Both whites and Hispanics residing in high-IRCA counties are significantly more likely to receive contact from a political party in the lead up to an election, suggesting that legalization is a relevant factor in the outreach efforts of political parties. We find no evidence of increased anti-migrant sentiment as a result of the IRCA. Together, these results suggest that the IRCA increased political mobilization without triggering nativist backlash.

Our work offers two main contributions. First, we add to the literature on the economics of legal status. This scholarship has documented the effect of legalization on a range of social and economic outcomes including education, earnings, employment, language skills, safety net transfers, and crime (Kossoudji and Cobb-Clark, 2002; Rivera-Batiz, 1999; Pan, 2012; Cascio and Lewis, 2019; Cortes, 2013; Sabet, 2023; Freedman et al., 2018; Pinotti, 2017). Other papers have examined the political consequences of immigrant legalization on such outcomes as congressional redistricting (Sabet and Yuchtman, 2023) and Hispanic representation on school boards (Sabet, 2023). We add to this literature by examining the impact of legalization on fiscal transfers. Additionally, by examining how political participation is affected by the IRCA, we are able to shed light on some of the important ways in which legalization spills over to affect the political participation of communities of mixed legal status.

Second, our paper contributes to the literature that examines the economic consequences of the expansion of voter franchise. This scholarship has examined the extension, or the de-facto extension, of voting rights to such groups as women (Miller, 2008), African Americans (Cascio and Washington, 2014), young people (Bertocchi et al., 2020) and lesser educated citizens (Fujiwara, 2015). While similar in spirit to these papers, our point of departure is to separate the effect of immigrant legalization from immigrant enfranchisement in explaining the distribution of resources. We argue that the effects of the former are more far-reaching than the latter because legal status lifts barriers of social exclusion not just for the undocumented but for their communities and family networks.

The rest of this paper proceeds as follows: Section 2 briefly discusses the historical background of the IRCA. Section 3 describes our data and their sources while our results are presented in Sections 4 to 6. Section 7 concludes.

## 2. The immigration reform and control act

The Immigration Reform and Control Act (IRCA) was signed into law in 1986 by the Reagan Administration. Its purpose was to restrict the flow of undocumented migrants into the United States and was built on three main pillars: an employer sanctions provision that made it unlawful to knowingly hire undocumented migrants; greater funding for border security; and an amnesty program aimed at documented undocumented migrants (Chishti and Kamasaki, 2014).

Some 3 million migrants applied for legal status under the IRCA and over 85 percent of them were of Hispanic origin (Sabet, 2023). The path to citizenship (and voting rights) occurred in stages. Once the initial application was accepted, migrants received temporary legal status which could last for up to 18 months. After this period migrants would, if they successfully completed a language and civics examination, receive permanent legal status. Five years after permanent residency, IRCA migrants were eligible to receive citizenship via naturalization.

As Rytina (2002) notes, “the impact of IRCA was much more concentrated with respect to legal immigration than naturalization.” This is borne out in the figures. In total, some 90 percent of all migrants received legal status under the IRCA (Sabet, 2023; Rytina, 2002). Of these, nearly 90 percent received permanent legal status by 1991 and the majority of the remaining migrants received it by 1994 (Sabet, 2023). By contrast, just over a third of IRCA migrants naturalized as US citizens by 2000 and the majority did so in the mid to late 1990s (Sabet, 2023). The institutional feature of the IRCA that mandated a five-year window between legal status and application for citizenship is what enables us to disentangle the effect of immigrant legalization from immigrant enfranchisement in our analysis.

As demonstrated in both Sabet (2023) and Sabet and Yuchtman (2023), the IRCA is not associated with differential changes, neither in levels nor in trends, with a wide range of county level socio-economic characteristics.<sup>2</sup> This is largely because undocumented migrants are already included in US census counts (Sabet and Yuchtman, 2023). This feature of the IRCA suggests that legalization shocked the legal status of a population without triggering wider socio-economic change. It also enables our regression framework to identify the effect of variation in legal status on intergovernmental revenues independently of other socio-demographic changes.

## 3. Data

In this section, we provide an overview of the main variables used in the study. In Online Appendix Table A.1 we provide summary statistics.

*IRCA Migrants*: The key explanatory variable in our study is the cumulative number of IRCA applicants per 1,000 county inhabitants measured in 1990, which we standardize.<sup>3</sup> We obtain this information from Sabet (2023) and Sabet and Yuchtman (2023) who, in turn, take it from the Immigration and Naturalization Service (INS) Legalization Summary Public Use Tape data. As explained in those papers, an important limitation of this data is that there are no county identifiers for migrants who applied from counties with populations less than 100,000 or with fewer than 25 applicants. The number of IRCA immigrants in counties with missing IRCA information is thus imputed by allocating a state’s unassigned IRCA immigrants (all migrants have state identifiers) to counties with missing IRCA information according to the share of

<sup>2</sup> Of course, this does not imply that IRCA migrants are randomly allocated across US counties. Instead, the findings in Sabet (2023) and Sabet and Yuchtman (2023) suggest that, conditional on exploiting variation within counties and within a state-year, the share of IRCA migrants does not lead to differential changes in county characteristics.

<sup>3</sup> We use applications per capita as opposed to legalized per capita to rule out any potential selection issues that might arise for those whose applications were actually accepted.

the total Hispanic population residing in such counties in 1990.<sup>4</sup> We also undertake three empirical exercises that demonstrate that the imputation method does not adversely affect the results or drive them. These include dropping all counties with imputed IRCA information, controlling for the time-varying effect of these counties; and an alternative imputation method that predicts the share of IRCA applicants in counties whose IRCA share is not known using the estimated coefficients from a model that uses a rich set of county characteristics to predict the migrant shares for the large counties for which the IRCA share is known (this method is described in more detail in Sabet (2023)). Across all approaches, we find very similar results suggesting that this data limitation is not an overwhelming concern.

**County finances:** We use intergovernmental revenues (IGR) per 1980 county population from state governments to local governments (counties, cities, municipalities aggregated to the county) as our primary dependent variable.<sup>5</sup> The Census Government Finance and Employment Classification Manual defines this variable as “state grants-in-aid” to local governments. Although it does include Federal pass-through money, 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.”<sup>6</sup>

We use intergovernmental revenue per 1980 county population from federal to local governments as a placebo measure of fiscal transfers. Like state-to-county IGR, these fiscal transfers are intended as budget support for local counties (i.e., they include “federal grants-in-aid”). Unlike state IGR, however, these transfers “include only direct aid from the Federal Government”. In other words, these transfers are not channeled through states, implying that state governments have no control over their distribution.

Our data on county revenues and expenditures are all taken from the US Census Bureau’s internal database on individual local government finances (“IndFin”). This database spans fiscal years 1957, 1962, 1967, and 1970 to 2006. The annual series begins in 1973.

**County covariates:** We collect data on county characteristics from the US Census Bureau to control for the time-varying effect of various socio-economic characteristics, measured in 1980, in our regression analyses.

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

**Governor elections:** We obtain county-level election returns for gubernatorial elections from two sources. For pre-1990 values, we obtain information from ICPSR study 13 (“General Election Data for the United States, 1950 – 1990”). For 1990 and beyond, we purchased data from Dave Leip’s Atlas of U.S. Presidential Elections (<http://uselectionatlas.org>).

**Political participation and engagement:** We test whether the IRCA leads to increases in political participation at the individual level for people with differential exposure to the IRCA. To conduct this exercise, we use restricted-use survey data from the American National Election Studies (ANES) with county identifiers which enable us to link IRCA information and other county covariates to this dataset.

<sup>4</sup> This imputation method is explained in more detail in Sabet and Yuchtman (2023).

<sup>5</sup> On average, counties in the sample receive USD 16 million in intergovernmental revenue per year, an amount which comprises approximately 30 percent of all local government revenue (figures derived from authors’ own calculations using our own data).

<sup>6</sup> Personal correspondence with Michael Fredericks of the Local Government Finance Statistics Branch of the Census Bureau on 26 November 2018.

## 4. Immigrant legalization and intergovernmental revenue

### 4.1. Event study estimates

We begin our analysis by examining trends in intergovernmental revenue from state to local governments. For each county  $c$  in time period  $t$ , we estimate the parameters of the following econometric model:

$$y_{c,t} = \sum_{j=1985, j \neq 1988}^{2000} \beta_j (IRCA_{c,1990} \times D_j^j) + \delta_c + \zeta_{st} + \theta(X_{c,1980} \times P_{88}) + \gamma(R_{c,1980} \times P_{88}) + \epsilon_{c,t} \quad (1)$$

Where  $y$  denotes total IGR from state to local governments per 1980 county population.  $IRCA_{c,1990}$  is a standardized measure of a county’s 1990 IRCA intensity and this term is interacted with year dummies, denoted by  $D_j^j$ . The model includes county fixed effects,  $\delta_c$  as well as state-by-year fixed effects,  $\zeta_{st}$ .  $X_{c,1980}$  is a vector of 1980 county level covariates which include log of county population, log of county income, the size of the population in poverty, and the size of the school age population (i.e., between 5 and 19), each interacted with  $P_{88}$ , an indicator that is zero for time periods on or before 1988 and 1 for periods after 1988.  $R_{c,1980}$  is a vector of 1980 race controls that includes the size of the county population that is white, black and Hispanic, each interacted with  $P_{88}$ . Standard errors are clustered at the county level, shown as  $\epsilon_{c,t}$ , and we weight all our regressions by the 1980 county population. Our sample begins in 1985, one year prior to the passage of the IRCA and extends until 2000.

The results are shown in Fig. 1 and demonstrate a positive post-1988 effect on transfers in counties with differential exposure to the IRCA. The pre-treatment coefficients display no trend and are both individually and jointly indistinguishable from zero (the  $p$ -value for a chi-squared test of joint significance of the three pre-period coefficients is 0.993). This increases confidence that the patterns are driven by legalization and not pre-existing linear trends in high-IRCA counties.

### 4.2. Baseline estimates

We test the strength of the relationship between legalization and fiscal transfers by estimating the parameters of the following specification:

$$y_{c,t} = \alpha_0 + \beta(IRCA_{c,1990} \times P_{88}) + \delta_c + \zeta_{st} + \theta(X_{c,1980} \times P_{88}) + \gamma(R_{c,1980} \times P_{88}) + \epsilon_{c,t} \quad (2)$$

In this model, we interact our standardized measure of IRCA migrants,  $IRCA_{c,1990}$ , with an indicator,  $P_{88}$ , that is 1 for time periods after 1988 and zero otherwise. All other terms are as previously defined. We report our estimates for  $\beta$  in Table 1.

Column 1 reports the baseline estimate. It suggests that a one standard deviation increase in IRCA intensity increases per capita IGR from state to local governments by around nine dollars. Relative to the pre-legalization sample mean, this represents a six percent increase in per capita IGR. In column 2, we present the results from a more parsimonious model that includes no controls (but does include county and state-by-year fixed effects) and in column 3 we add additional county level controls. These include the time-varying effect of: 1980 measures of population with tertiary education, 1982 measures of crime rates, and the 1999 share of public schools in the county covered by Title I of the Elementary and Secondary Education Act.<sup>7</sup> The results are not affected by the inclusion, or omission, of these controls.

In column 4, we do not weight the regression to demonstrate that our results are not sensitive to the choice of weights. In column 5, we

<sup>7</sup> We obtain the number of schools eligible for Title I funding from the Common Core of Data Public Schools data. Although this information is only available as of 1999, we assume that Title I eligibility is correlated over time.

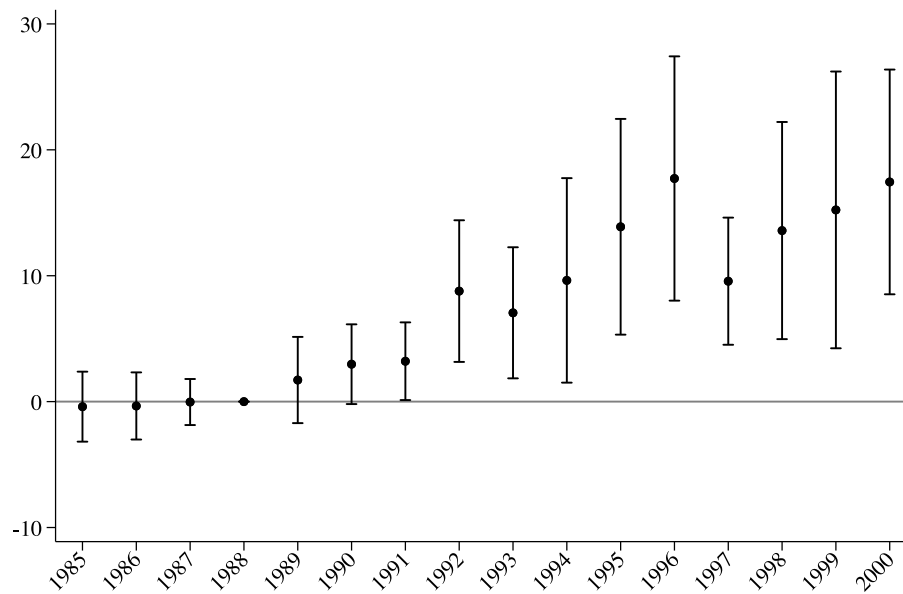


Fig. 1. IRCA and Trends in Total Per Capita State Intergovernmental Revenue.

Notes: This figure plots the coefficient on  $IRCA_{90}$ , the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990, interacted with time dummies. The outcome variable is total per 1980 capita intergovernmental revenue from state to local governments. The regressions include county fixed effects and state-by-year fixed effects. They also include 1980 measures of county population, income, school-aged population (i.e., between 5 and 19), population that is poor as well as the white, black and Hispanic population size, each interacted with  $P_{88}$  which is one for time periods after 1988 and zero before. Standard errors are clustered at the county level and confidence intervals are drawn at 90 percent. All regressions are weighted by the size of the 1980 county population. The  $p$ -value for a chi-squared test of joint significance of the three pre-period coefficients is 0.993.

Table 1  
IRCA and per capita intergovernmental revenue from state to local governments.

	Empirical specifications							Sample restrictions				Post-94	Placebo
	(1) Baseline covariates	(2) No covariates	(3) Additional covariates	(4) Unweighted	(5) Border State $\times$ Post	(6) Control imputed	(7) Alternative imputation	(8) Drop imputed	(9) Drop VRA 5	(10) Always in sample	(11) Population < 90th Pctl.	(12) $IRCA \times P_{94}$	(13) Federal IGR
$IRCA_{90} \times P_{88}$	8.743** (2.933)	8.912** (3.764)	6.056** (1.648)	7.925** (2.674)	8.743** (2.933)	9.084** (3.001)	5.352** (2.563)	10.20* (5.492)	9.960** (3.069)	13.22** (5.184)	4.411** (1.794)	4.939** (2.021)	0.836 (0.790)
$IRCA_{90} \times P_{94}$												8.373** (3.648)	
$N$	35,349	35,365	31,152	35,349	35,349	35,349	35,349	7,944	12,858	18,720	30,523	35,349	35,349
Clusters	3,024	3,025	3,020	3,024	3,024	3,024	3,024	505	2,176	1,170	2,721	3,024	3,024
$\bar{Y}_{P_{94}}$	147	147	146	147	147	147	147	154	143	174	145	147	22
[S.D.]	283	283	212	283	283	283	283	310	163	391	269	283	147

Notes: The outcome variable in columns 1 to 12 is per 1980 capita intergovernmental revenue (IGR) from state to local governments measured at the county level. In column 13, it is per 1980 capita intergovernmental revenue (IGR) from federal to local governments measured at the county level.  $IRCA_{90}$  is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990.  $P_{88}$  is an indicator that is zero for time periods on or before 1988 and 1 for periods after 1988. All regressions include county fixed effects and state-by-year fixed effects. They also include, except for column 2, 1980 measures of county population, income, school-aged population (i.e., between 5 and 19), population that is poor as well as the white, black and Hispanic population size, each interacted with  $P_{88}$ . Additional covariates in column 3 include the crime rate (1982 values), share of schools in the county covered under Title I (1999 values), and the share of the population with a bachelors degree (1980 values), each interacted with  $P_{88}$ . The specification in column 5 includes an interaction between  $P_{88}$  and an indicator for whether a county is located in a border state. Column 6 includes the time-varying effect of counties whose IRCA shares had to be imputed, column 7 employs an alternative imputation to calculate missing IRCA shares and column 8 drops counties whose IRCA shares had to be imputed. In column 9, counties covered by the pre-clearance requirement of Section 5 of the VRA are dropped. Column 10 restricts the sample to those counties whose IGR information is observed in each year of the sample. In column 11, counties with 1980 populations in the top 10 percentile are dropped. Column 12 includes an additional interaction of  $IRCA_{90}$  with  $P_{94}$ , an indicator that is 1 for periods on or after 1994 and zero otherwise. Standard errors (in parentheses) are clustered at the county. All regressions are weighted by the size of the 1980 county population, except for column 4 which has no weights.

\*  $p < 0.1$ .  
\*\*  $p < 0.05$ .  
\*\*\*  $p < 0.01$ .

control for the time-varying effect of being a border state. In columns 6 to 8, we undertake three approaches to ensure that our baseline method of imputing missing IRCA information does not adversely drive our results. First, in column 6, we control for the time-varying effect of counties with imputed IRCA shares; second, in column 7, we employ an alternative method of imputing the missing IRCA information; and third, in column 8, we drop counties with missing IRCA information altogether. In column 9, we drop counties covered by the pre-clearance requirement of Section 5 of the VRA and in column 10, we restrict the sample to those counties whose IGR information is observed every year in our sample.<sup>8</sup> In column 11, we drop counties with 1980 populations in the top 10 percentile to ensure the result is not driven by large urban

centers that may serve as sanctuary cities. Although estimated with precision, the magnitude of the coefficient drops by around 50 percent, suggesting that large population centers drive both IRCA intensity and fiscal transfers.

#### 4.3. Legalization v. Enfranchisement, placebo

In columns 12 and 13 of Table 1, we undertake two final exercises. First, in column 12, we test the extent to which immigrant legalization, as opposed to immigrant enfranchisement, drives our results. To this purpose, we include in our estimation the interaction of  $IRCA_{c,1990}$  with  $P_{94}$ , which is one for time periods on or after 1994, the year when

<sup>8</sup> As mentioned in Section 3, the data on county revenues comes from the US Census Bureau's internal database on individual local government finances.

Although it is a yearly series (as of 1973), not every county is surveyed in every year.



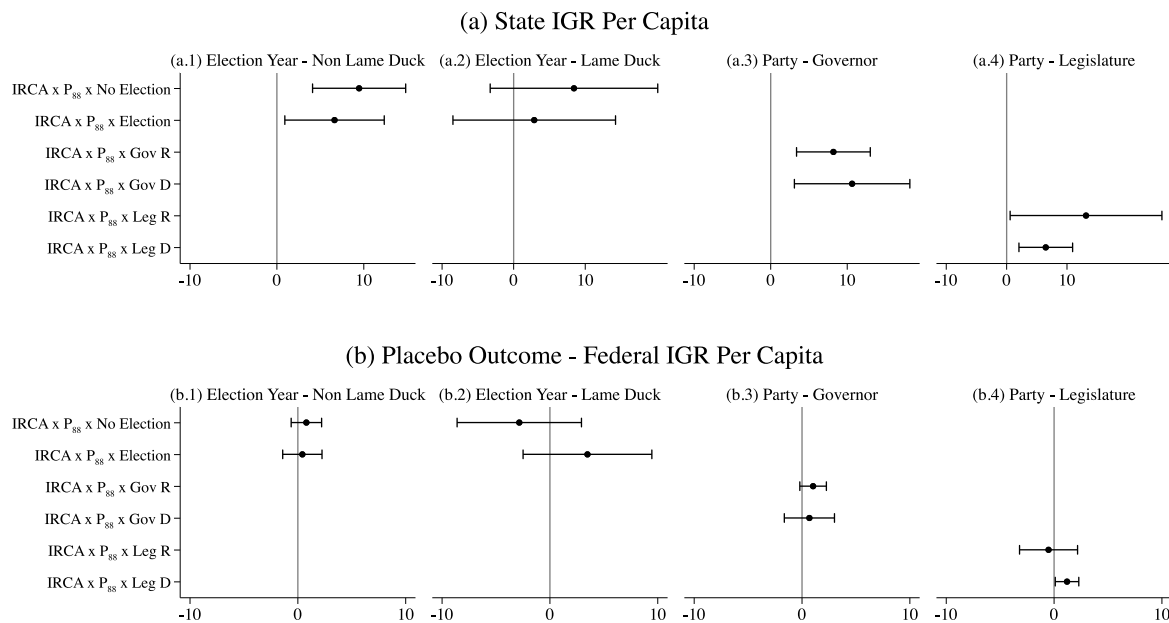


Fig. 2. IRCA, Per Capita IGR and Political Heterogeneity.

**Notes:** This figure plots the coefficient on  $IRCA_{90}$ , the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990, interacted with  $P_{88}$ , an indicator that is zero for time periods on or before 1988 and 1 for periods after 1988. The outcome in panel (a) is total per 1980 capita intergovernmental revenue from state to local governments and in panel (b) it is total per 1980 capita intergovernmental revenue from federal to local governments. The analysis in sub-panels (a.1) and (b.1) are restricted to governors who are not lame-ducks while the analysis in panels (a.2) and (b.2) are restricted to state's with a lame-duck governor (i.e, ineligible for re-election due to term limits). "Leg R" is one if the state legislature is Republican or split. The regressions include county fixed effects and state-by-year fixed effects. They also include 1980 measures of county population, income, school-aged population (i.e., between 5 and 19), population that is poor as well as the white, black and Hispanic population size, each interacted with  $P_{88}$ . Standard errors are clustered at the county level and confidence intervals are drawn at 90 percent. All regressions are weighted by the size of the 1980 county population.

the bulk of IRCA migrants naturalized, and zero otherwise. As shown, there is a significant post-1994 effect of the IRCA on the distribution of resources, highlighting the potential role of immigrant naturalization as a channel to explain our results. Importantly, however, the coefficient on  $IRCA_{c,1990} \times P_{88}$  remains positive and precisely estimated, suggesting that the legalization of immigrants, as distinct from their naturalization and enfranchisement, is a significant factor in the distribution of public resources.

Second, in column 13, we run our baseline estimating equation but use intergovernmental revenue from *federal* to local counties as the outcome. We use this measure as a placebo because, as explained earlier, these transfers include *only* direct aid from Federal to local governments, leaving no room for state governments to control their distribution. As shown in column 13, the IRCA has no influence on the distribution of these transfers.

In Online Appendix B, we investigate different categories of IGR revenue as well as different categories of county *expenditure* to discern for what purposes state IGR was targeted. We find that state IGR for education increases differentially in IRCA counties and that local spending follows similar patterns: counties with differential exposure to the IRCA spend more on education, a result driven by greater elementary and secondary spending. There are also some welfare effects. Both state IGR for welfare and local spending on welfare increase in IRCA counties but these effects are much shorter lived (appear in 1992/1993 and vanish by 1997) and display more pre-trends than the education results.

### 5. Political heterogeneities

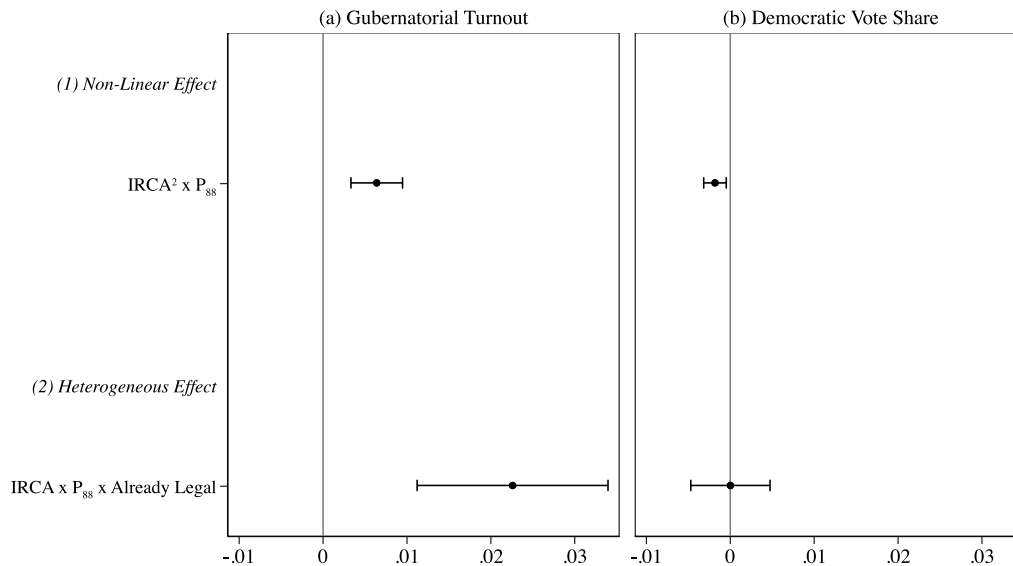
An important question concerns the extent to which our results reflect mechanical forces. Although each state determines “the nature, amount and distribution of state grants internally”, state IGR does include federal pass-through money. Moreover, one may be concerned that the fiscal response reflects a rules-based transfer formula based on the number of newly legalized migrants in a county. In this section, we distinguish the extent to which our results are driven by mechanical

forces as opposed to discretionary choices of state governors. To do so, we undertake two exercises. First, we leverage information on the political circumstances of state governors to test for heterogeneous effects of the IRCA with respect to such factors as term limits. Second, we test for these heterogeneous effects using our placebo measure of fiscal transfers, which state governments have no discretion over.

We present the results in Fig. 2. In panel (a), we use state IGR as the outcome. In panel (a.1), we find that when the governor is eligible for re-election, the IRCA has a positive and significant effect on fiscal transfers, in both election and non-election years. These patterns vanish in the sample of governors who are no longer eligible for re-election because of term limits, as shown in panel (a.2). Although the magnitude of the coefficients in panel (a.2) are similar to those in panel (a.1), they are indistinguishable from zero. In panel (a.3) and (a.4), we test for differences according to the partisan characteristics of state governors (panel (a.3)) and state legislatures (panel (a.4)). As shown, the effect of the IRCA on fiscal transfers remains positive and precisely estimated regardless of the partisan affiliation of the state’s executive and legislative branches. These results suggest that the electoral incentives of the state governor, rather than their political affiliation, drive the fiscal response of the IRCA.

In panel (b) we conduct the same exercises but use 1980 per capita federal IGR as the outcome. As shown, the IRCA loses all of its predictive power when using our placebo measure of fiscal transfers. The contrast in results when using state IGR compared to federal IGR suggest that, while there may be some mechanical or formula-based component to our main results, political considerations at the state level play a significant role in explaining the distribution of IGR from state to local governments.<sup>9</sup>

<sup>9</sup> In Online Appendix C, we repeat this exercise but include the interaction of  $P_{88}$  with an indicator variable that is 1 if a county has a population in the top 10 percentile of the population distribution and 0 otherwise as a control. As shown in Figure C.1, our results are robust to the inclusion of this



**Fig. 3.** IRCA and Individual Political Participation and Engagement.  
**Notes:** This figure plots the coefficient on  $IRCA_{90}$ , the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990, interacted with  $P_{88}$ , an indicator that is zero for time periods on or before 1988 and 1 for periods after 1988. The outcome variable in panel (a) is the standardized number of votes for governor per 1980 county population over the age of 18. In panel (b) it is the standardized vote share for the Democratic gubernatorial candidate. *Already Legal* is a standardized measure of the size of the 1980 Hispanic population that is already legal. All regressions include county fixed effects and state-by-year fixed effects. They also include 1980 measures of county population, income, school-aged population (i.e., between 5 and 19), population that is poor as well as the white, black and Hispanic population size, each interacted with  $P_{88}$ . Standard errors are clustered at the county level and confidence intervals are drawn at 90 percent. All regressions are weighted by the size of the 1980 county population. The sample includes all gubernatorial elections between 1980 and 2000.

### 6. IRCA and political mobilization

We argue that the differential allocation of transfers to counties affected by the IRCA was motivated not so much to win the political support of the newly legalized migrants — many of whom earned the right to vote in the mid to late 1990s — but rather that of entire Hispanic communities that were politically mobilized as a result of the IRCA. In this respect, both [Sabet \(2023\)](#) and [Sabet and Yuchtman \(2023\)](#) document significant political effects of the IRCA on such outcomes as congressional redistricting and Hispanic representation in Congress as well as on local school boards. [Sabet and Yuchtman \(2023\)](#), in particular, demonstrate that the IRCA legalized a politically cohesive group (i.e., Democratic-leaning) which increased the political relevance of the IRCA for state lawmakers.

In this section, we measure the impact of the IRCA on political mobilization at the local level using two additional measures: voter turnout in gubernatorial elections and political participation and engagement using individual survey data.

#### 6.1. Voter turnout

To test the electoral relevance of the IRCA at the state level, we estimate our baseline econometric specification using county level votes per 1980 capita in gubernatorial elections as the outcome. However, we test for two important heterogeneous effects: first, we test whether the relationship between the IRCA and turnout is non-linear, by including the term  $IRCA_{c,1990}^2 \times P_{88}$  in our estimation. Second, we examine whether the IRCA has a differentially stronger effect in Hispanic communities of mixed legal status. To this end, we include in our model the triple interaction  $IRCA_{c,1990} \times P_{88} \times \text{Already Legal}$  (all

important control, suggesting that very large counties are not confounding our heterogeneous effects.

lower order terms included) where “Already Legal” is the standardized measure of the 1980 share of the Hispanic migrant population that is already legal.<sup>10</sup>

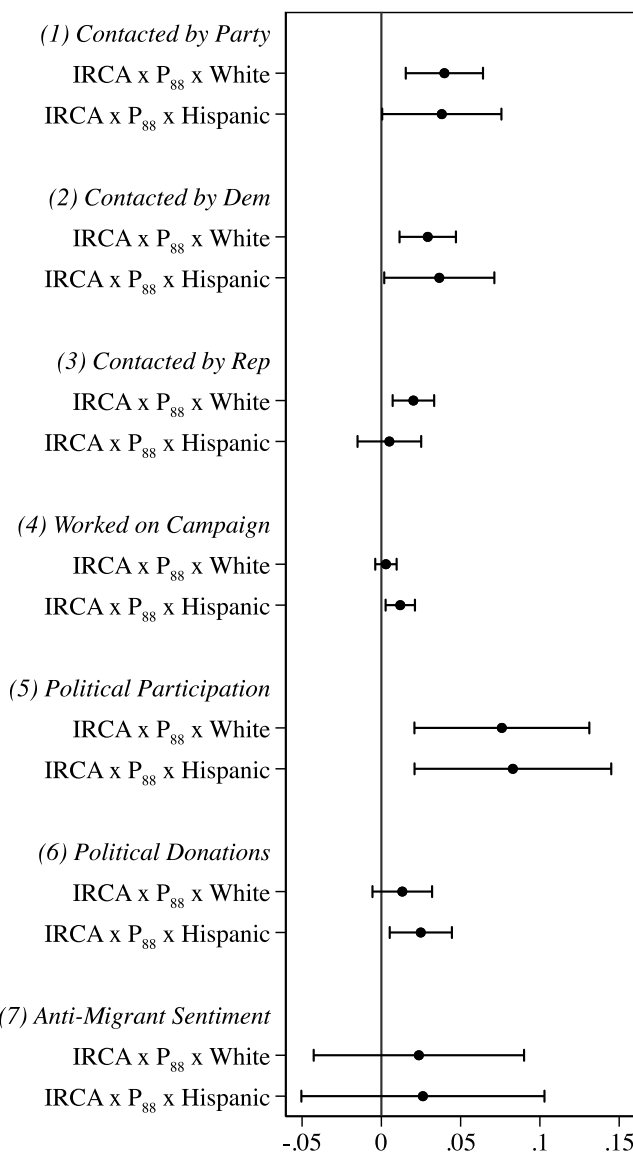
We report the coefficients on  $IRCA_{c,1990}^2 \times P_{88}$  and  $IRCA_{c,1990} \times P_{88} \times \text{Already Legal}$  in [Fig. 3](#). In panel (a) we study votes per 1980 capita in all governor elections from 1980 to 2000. In panel (b) we examine the vote share for the Democratic candidate in these same elections.

As shown in panel (a), the effect of the IRCA on turnout is non-linear. Communities with thick networks of newly documented migrants experience differentially greater levels of voter turnout. Moreover, the effect of the IRCA on turnout increases differentially as a function of the size of the pre-existing legal Hispanic population.<sup>11</sup>

In panel (b), we repeat the analysis but examine the Democratic vote share in gubernatorial elections. The IRCA does display a non-linear relationship: it is not the case that larger and larger undocumented communities vote increasingly Democratic. By contrast, however, the Democratic vote share of the gubernatorial candidate does not significantly differ according to the size of the pre-existing Hispanic legal population. This pattern suggests that the political impact of the IRCA on large, Hispanic communities of mixed legal status is to increase participation without affecting the partisan nature of that participation.

<sup>10</sup> We calculate this measure by deducting the total number of IRCA applicants in a county from its 1980 Hispanic population, arguing that this difference represents a measure of the size of the “already legal” Hispanic population.

<sup>11</sup> In Online Appendix D, we replicate our baseline results in a sample of counties with above and below median levels of already-legal Hispanic migrants. As shown, we find a positive and significant effect of the IRCA on fiscal transfers only in the sample of counties with above median populations of already-legal Hispanic migrants.



**Fig. 4.** IRCA and Individual Political Participation and Engagement.  
**Notes:** This figure plots the coefficient on  $IRCA_{90}$ , the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990, interacted with  $P_{88}$ , an indicator that is zero for time periods on or before 1988 and 1 for periods after 1988. All the outcomes (except for model 5) are indicator variables that are one (and zero otherwise) if a person reports: receiving contact by a political party (model 1), or contact from the Democratic or Republican party, models 2 and 3, respectively); working for a political campaign (model 4); donating money to a political campaign (model 6); or holding negative views towards undocumented migrants (model 7). In model 5, the outcome is a count of the number of campaign activities a person reports participating in. The regressions include county fixed effects and state-by-year fixed effects as well as individual measures of income, age and education. They also include 1980 measures of county population, income, school-aged population (i.e., between 5 and 19), population that is poor as well as the white, black and Hispanic population size, each interacted with  $P_{88}$ . Standard errors are clustered at the county level and confidence intervals are drawn at 90 percent. All regressions are weighted by the size of the 1980 county population.

### 6.2. Political participation and engagement

In this section, we analyze survey data from the American National Election Studies (ANES). For each person residing in a given county in the ANES, we link the 1990 number of per capita IRCA migrants in that county in order to test for the effect of the IRCA on various measures of political participation and engagement. We report our results in Fig. 4, where we plot the coefficient on  $IRCA_{c,1990} \times P_{88} \times White$ , on

$IRCA_{c,1990} \times P_{88} \times Hispanic$  (all lower order terms included) when they are regressed against a range of individual survey responses.<sup>12</sup>

In models 1 to 3, we test whether the IRCA increases the likelihood of receiving contact from a political party. As shown, both whites and Hispanics are more likely to receive contact from political parties post-legalization, highlighting the importance of the IRCA for political outreach. What is more, this outreach appears to have a partisan dimension: whereas both whites and Hispanics in high-IRCA counties are increasingly likely to receive contact from the Democratic party (model 2), the IRCA increases the likelihood of contact from the Republican party only for whites (model 3).

In model 4, we find that the IRCA increases the probability of Hispanics working for a political campaign. In model 5 and 6, we test for two additional measures of political participation: participation in campaign activities and political donations. In both cases, the IRCA increases the likelihood of Hispanic participation. Finally, in model 7, we test for anti-migrant sentiment using the “illegal alien” thermometer of the ANES.<sup>13</sup> As shown, there is no evidence that the IRCA increased anti-migrant sentiment. Together, these results suggest that the IRCA increased Hispanic political mobilization without triggering nativist backlash.

### 7. Conclusion

Undocumented migration is a contested issue in the United States where the number of such migrants has nearly quadrupled in the past thirty years. In this paper, we shed light on the impact of legal status on fiscal transfers. Counties with differential exposure to the IRCA receive significantly more per capita resources from state governments. This effect responds to the electoral incentives of the state governor, suggesting that it is politically motivated.

A key institutional feature of the IRCA was that it mandated a five-year window between legal status and citizenship. This enables us to disentangle the effect of immigrant legalization from immigrant enfranchisement in our analysis. Importantly, we found that immigrant legalization under the IRCA positively predicts transfers prior to enfranchisement. To understand why this might be the case, we studied the impact of the IRCA on political participation. We found that the IRCA increases turnout in state level elections in counties with large pre-existing Hispanic populations. It also increases various measures of Hispanic political engagement at the individual level without triggering anti-migrant backlash. These results point to important spillover effects that immigrant legalization has on the political participation and mobilization of communities of mixed legal status.

### Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

### Data availability

Data will be made available on request.

<sup>12</sup> The estimating model is the same as that presented in Eq. (2) but with two modifications: first, the outcome is measured at the level of people, living across US counties over time, and second, it includes an individual’s age, income and education.

<sup>13</sup> The thermometer is on a scale from 0 (cold feelings) to 100 (warm feelings). The variable is 1 if a person reports a score between 0 and 40 and zero if they report feelings of 41 and higher. Changing this to a 50-50 split does not change the results.

## Appendix A. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.jpubeco.2024.105155>.

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