CESifo WORKING PAPERS

10554
2023

July 2023

Identifying Partisan Gerrymandering and Its Consequences: Evidence from the 1990 US Census Redistricting

Navid Sabet, Noam Yuchtman

CESifo

Impressum:

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de
Editor: Clemens Fuest
https://www.cesifo.org/en/wp
An electronic version of the paper may be downloaded
· from the SSRN website: www.SSRN.com
· from the RePEc website: www.RePEc.org
· from the CESifo website: https://www.cesifo.org/en/wp

CESifo *Working Paper No. 10554*

Identifying Partisan Gerrymandering and Its Consequences: Evidence from the 1990 US Census Redistricting

Abstract

We empirically identify politically-motivated redistricting and its consequences, studying the effects of changed electorate composition on US congressional district boundaries and on political outcomes. We exploit the 1986 Immigration Reform and Control Act (IRCA), which legalized millions of immigrants, changing local *electorates* without changing *demographics* — legalized immigrants were already counted in the census. Where Democrats controlled the 1990 redistricting process, higher IRCA populations were associated with more spatially distorted districts. Consistent with theory, Democrats packed Hispanics (their ardent supporters) into majority-minority districts. House delegations had more Hispanics suggesting that partisan gerrymandering, in this case, served the historically disadvantaged.

JEL-Codes: D700, P000, J100.

Keywords: gerrymandering, minority political representation, immigrant legalization, state governance.

Navid Sabet
Faculty of Economics and Business
Goethe University Frankfurt / Germany
sabet@econ.uni-frankfurt.de

Noam Yuchtman
London School of Economics and Political Science / London / United Kingdom
yuchtman@lse.ac.uk

This Version: July 5, 2023
We thank Felix Bierbrauer, Davide Cantoni, Quoc-Anh Do, Guido Friebel, Claudia Goldin, Richard Holden, Larry Katz, Jed Stiglitz, Ebonya Washington and many seminar participants for very helpful comments.

1. Introduction

Congressional district boundaries in the United States are redrawn every ten years following decennial censuses. Such redistricting, carried out by state governments, aims at accommodating population changes so as to preserve the principle of "one person one vote". However, partisan control of the redistricting process has led to intense concern among policymakers and the general public regarding "gerrymandering": the drawing of district boundaries in a manner that confers an electoral advantage to a particular party, thus potentially undermining the democratic principle it is intended to uphold.[1] Of particular interest is the effect of redistricting on the representation of historically underrepresented minority groups (Cox and Holden 2011; Washington 2012).

Historical and contemporary examples of politically-distorted electoral districts abound (see Figure 1), and there exists a substantial literature formally analyzing and documenting the politically-motivated spatial distortion of electoral districts (Coate and Knight 2007; Friedman and Holden 2008; Gul and Pesendorfer 2010; Fryer Jr. and Holden 2011; Chen and Rodden 2013, 2015; Stephanopoulos and McGhee 2015; Coriale, Kaplan, and Kolliner 2020; Kolotilin and Wolitzky 2020; Bierbrauer and Polborn 2020). However, up to now there has been no empirical work identifying a key causal relationship at the heart of theories of gerrymandering: the effect of the electorate's political composition on the gerrymanderer's drawing of district boundaries, and on subsequent political outcomes. A fundamental challenge is to disentangle variation in political composition from concomitant local economic and demographic changes, which themselves may motivate adjustments to district boundaries. Resolving this challenge requires independent variation in the political composition of the electorate.

In this paper, we identify the effects of changes in the electorate's political composition on the shape of US congressional districts, and on political outcomes. We exploit variation arising from the legalization of millions of (primarily Hispanic) immigrants to the United States under the 1986 Immigration Reform and Control Act (IRCA). Crucially, the legalized immigrants were *already* resident in their states for years prior to legalization and were counted in previous censuses. By providing the legalized immigrants with a path to naturalization — and voting rights — the IRCA represented a shock to the composition of the *electorate*, without changing any other demographic dimension of the communities of the legalized immigrants. We examine the effects of this shock to the composition of the electorate on the boundaries of congressional districts drawn following the 1990 census (i.e., in the "1990 census redistricting"), as well as on political outcomes, including Hispanic substantive and descriptive representation.

We first show that the IRCA shock to the electorate was partisan: counties with more legalized IRCA immigrants (a higher "IRCA share" of the population) voted differentially more

1. The extent of public interest in gerrymandering can be seen in the recent development of an interactive *New York Times* online game, "Can you Gerrymander Your Party to Power?" See `https://www.nytimes.com/interactive/2022/01/27/us/politics/congressional-gerrymandering-redistricting-game-2022.html`, last accessed June 7, 2023.

for Democrats in presidential elections after legalized individuals earned the right to vote.[2] Importantly, we find no coincident sociodemographic changes associated with a county's share of legalized immigrants — whether examining biennial variation around the time of the 1990 census redistricting or in a "long difference" across the decade of the 1980s. The IRCA altered the political composition of the US population in a manner that was spatially-varying, partisan, and independent of other demographic changes.

We then explore how this partisan shock to the composition of the electorate affected the boundaries of congressional districts drawn in the 1990 census redistricting. One might expect that the sudden emergence of a large Democrat-leaning bulge in the electorate would induce a response by strategic gerrymanderers. Consider a state in which Democrats control the redistricting process.[3] Democrats might exploit the increase in the number of Hispanic (largely Democratic) voters to pursue one (or more) of several objectives. They may, for example, seek to increase the total number of Democrats in the House delegation; to support Hispanic representation in Congress through the creation of majority-Hispanic districts; or to create more left-leaning districts. To the extent that gerrymandering was concentrated in areas with larger changes in the electorate due to variation in the IRCA share, this would suggest within-state variation in the extent of redistricting. One would also expect that states with more legalized immigrants — offering more scope for strategic gerrymandering — would exhibit larger boundary changes. Of course, any such strategic gerrymandering would fundamentally depend on the control of the redistricting process: if no party had control of the process, one would expect much more modest redistricting.

To empirically examine the impact of the IRCA-induced change in the electorate, we compare the district boundaries for the 102^{nd} Congress (the last Congress elected before the 1990 census redistricting) and the 103^{rd} Congress (the first Congress elected after).[4] We exploit within-state variation in the IRCA share of counties' populations, and cross-state variation in the political control of the redistricting process, controlling for county fixed effects and state×Congress fixed effects. We find that in states where Democrats controlled the redistricting process, a higher IRCA share of a county's population is associated with a statistically significant increase in the spatial distortion (a reduction in several measures of "spatial compactness") of the associated congressional districts following the 1990 census redistricting.[5] Importantly, we find this spatial distortion associated with the IRCA share *only* in states where Democrats controlled the redistricting process: where control over redistricting was in the hands of non-partisan

2. By examining presidential election vote shares we consider an outcome that is not shaped by changes in US congressional district boundaries.

3. This was the case in 17 states during the 1990 redistricting cycle; by contrast Republicans controlled the process in only 2 states. No party held control of redistricting in 24 states and 7 states were single-district states. See Online Appendix Table A.1 for a list of states by partisan control of redistricting.

4. Members of the 102^{nd} Congress were elected in November, 1990, beginning their terms in January, 1991; members of the 103^{rd} Congress were elected in November, 1992, beginning their terms in January, 1993.

5. As discussed below, IRCA population shares are available at the county level, rather than the district level. We thus match counties to their associated congressional districts, assigning counties a weighted average of the district level outcome in case a county overlaps with more than one district.

commissions or neither party exercised control, we find no such distortion.[6] The greater spatial distortion in high IRCA counties in states with Democratic political control is *not* seen following the *1980* census redistricting (well before the IRCA), suggesting that substantial redistricting is not a generic characteristic of these counties, but is specific to redistricting following the legalization of immigrants in the 1980s.

An important question about our within-state estimates is whether they are biased by spillovers from districts exposed to the changed electorate to other districts (i.e., a SUTVA violation). To address this concern, we estimate the effect of *state*-level variation in IRCA exposure on district level spatial distortion. Reassuringly, we find that following the 1990 census redistricting, states with a higher IRCA share and Democratic control of redistricting experience a larger increase in the spatial distortion of their districts. Examining the distributions of district spatial compactness before and after the 1990 census redistricting, we find a striking shift to the left of the entire distribution (i.e., greater spatial distortion) specifically in states with above-median IRCA shares and Democratic control of redistricting.

Interpreting changes in districts as the outcome of strategic redistricting is made complicated by another source of institutional variation affecting the 1990 census redistricting: enforcement of the 1965 Voting Rights Act (VRA). In particular, Section 5 of the VRA required a subset of primarily Southern US states to seek pre-clearance from the Federal government prior to any change in voting rules. In addition, following the 1982 renewal of the VRA (and so affecting the 1990 census redistricting), Section 2 of the VRA exerted greater pressure to increase the number of minority-majority districts, especially in states covered by Section 5 (see Washington 2012, for a discussion). To the extent that high IRCA shares coincide with coverage under Section 5 of the VRA, this might create the appearance of gerrymandering, when in fact changed districts reflect an effort to create majority-minority districts to comply with the VRA. We address this concern in two ways: first, throughout our analysis, we allow counties covered under Section 5 of the VRA to exhibit their own post-redistricting changes in outcomes; including a control interacting a "Section 5 coverage" dummy with a post-redistricting dummy does not affect our findings. Second, we consider samples in which enforcement of Section 2 of the VRA is less likely to drive our results: we can drop all majority-minority districts and our findings remain unchanged.

As a second measure of gerrymandered political districts, we examine how the partisan shock to the political composition of the electorate affected the demographic composition of districts. In the context of the IRCA — which primarily legalized Hispanic immigrants[7] — and in light of traditional Democratic support for the descriptive representation of minority groups, it is of particular interest to study the effects of the 1990 census redistricting on the creation

6. Republicans controlled the 1990 census redistricting process in only 2 states, making statistical inference in the case of Republican control noisy (qualitatively, we generally find effects in the opposite direction where Republicans controlled redistricting).

7. Nearly 90% of the legalized immigrants were of Hispanic origin.

of majority-Hispanic districts.[8] This indicator also serves as a proxy for Democratic gerrymanderers' concentration of their party's ardent supporters (as predicted in, e.g., Friedman and Holden (2008) and Kolotilin and Wolitzky (2020)). We estimate the same specification used to study spatial distortion, exploiting within-state variation in the IRCA share of counties' populations and cross-state variation in political control of redistricting. We find that in states where Democrats controlled the redistricting process, a higher IRCA share of a county's population is associated with a statistically significant increase in the likelihood that the associated congressional district is majority-Hispanic following the 1990 census redistricting. Importantly, as was the case in our analysis of spatial distortion, this change in district demographic composition associated with the IRCA share occurs *only* in states where Democrats controlled the redistricting process: where control over redistricting was in the hands of non-partisan commissions or where neither party exercised control, we find no such demographic change. We again see no such patterns arising following the 1980 census redistricting. Moreover, these results do not appear to be driven by enforcement of the VRA: controlling for a "Section 5 coverage" dummy with a post-redistricting fixed effect does not affect our findings.[9]

What did these changed congressional districts imply for political outcomes, in particular the representation of Hispanics, a historically underrepresented group? To answer this question, we compare US House election outcomes for the 5 congressional elections conducted under the 1980 census district boundaries to those for the 5 congressional elections conducted under the 1990 census district boundaries. We find that, controlling for county and state×election fixed effects (and thus exploiting within state-by-election variation), in Democrat-controlled states, counties with a higher IRCA share are significantly more likely to have a Hispanic win their district's congressional seat following the 1990 census redistricting. That is, Democrat-controlled redistricting improved Hispanics' *descriptive* representation where there was a higher IRCA share. We next consider the *substantive* representation of Hispanics, examining both the intensive and extensive margins of Democratic Party outcomes.[10] On the intensive margin, districts with a higher IRCA share also see increased Democratic vote shares in states where Democrats controlled redistricting, but on the extensive margin, there is no increase in the likelihood of a Democratic representative.

We finally consider political outcomes at the state level. While this analysis must be interpreted with caution, owing to the limited cross-sectional variation, our findings suggest important state-level effects arising from politically-motivated redistricting. We find that in states

8. Guinier (1991), writes (in the context of African American representation), "For almost two decades, the conventional civil rights political empowerment agenda of black activists, lawyers, and scholars has focused on the election of black representatives." Though it is important to note that Democrats may have been more sensitive to tradeoffs between descriptive and substantive representation, making them less likely to concentrate minority voters in majority-minority districts (see Washington 2012).

9. While we cannot drop majority-minority districts (this would restrict our outcome to be 0), our results are robust to dropping the one state (New Mexico) that saw a case brought under Section 2 of the VRA.

10. Substantive representation promotes the political goals of a particular group (Washington 2012). We interpret Democratic Party political success as aligned with Hispanics' political aims in light of Democrats' historical support for underrepresented minority groups.

where redistricting was controlled by Democrats, with higher IRCA population shares, House delegations include more Hispanic members, suggesting that Democrat-controlled redistricting increased the descriptive representation of Hispanics at the state level. Examining substantive representation, we find patterns that match our county-level analysis. On the intensive margin, in states where redistricting was controlled by Democrats, with higher IRCA shares, we find a significant shift to the left in the ideology of the median member of their House delegation (as measured by DW nominate scores) after the 1990 census redistricting. However, while IRCA population shares generally predict more Democratic House members following immigrant legalization, there is no differential increase in states with Democratic control of redistricting. We do find that Democrats translate their votes into seats more efficiently at the state level, examining the efficiency gap measure of gerrymandering proposed by Stephanopoulos and McGhee (2015).

Our analysis contributes most directly to the growing empirical literature documenting and testing theories of gerrymandering (Gelman and King 1990; Coate and Knight 2007; Friedman and Holden 2009; Fryer Jr. and Holden 2011; Ansolabehere and Snyder Jr 2012; Chen and Rodden 2013, 2015; Stephanopoulos and McGhee 2015; Ansolabehere and Palmer 2016; Coriale, Kaplan, and Kolliner 2020; Jeong and Shenoy 2022). Closest to our analysis is work by Sabouni and Shelton (2022), which studies how the composition of the electorate (its competitiveness) interacts with sharp variation in political control in shaping the boundaries of state legislative districts. We provide the first direct evidence on politically-motivated redistricting in response to the composition of the electorate, a central element in theories of gerrymandering. Our findings suggest that political control of the redistricting process is used strategically, with our evidence consistent with theories of gerrymandering predicting the "packing" of ardent supporters (Friedman and Holden 2008; Kolotilin and Wolitzky 2020).[11]

Our finding that politically-motivated redistricting increased the substantive and descriptive representation of Hispanic Americans contributes to the empirical literature on the causes and consequences of political representation of underrepresented groups, both in the US and more generally (see, e.g., Pande 2003; Chattopadhyay and Duflo 2004; Iyer et al. 2012; Cascio and Washington 2014; Aneja and Avenancio-Leon 2019; Bernini, Facchini, and Testa 2022). We complement Washington (2012) in studying the political causes and consequences of the creation of majority-minority districts during the 1990 census redistricting. While Washington (2012) studies the effects of the VRA (exploiting variation across states), we study independent variation in both cross-state and within-state exposure to the IRCA, as well as political control of redistricting. Our findings corroborate those in Shotts (2003), Cox and Holden (2011), and Washington (2012): at least in this instance, the creation of majority-minority districts increased

11. Our findings of Hispanics (i.e., Democrats' supporters) "packed" into majority-minority districts is at odds with Gul and Pesendorfer (2010), whose model predicts the spreading of supporters across uniform districts. As discussed by Gul and Pesendorfer (2010) and Kolotilin and Wolitzky (2020), the different predictions in Gul and Pesendorfer (2010) and Friedman and Holden (2008) arise from the nature of gerrymanderer uncertainty regarding voter preferences in the models.

descriptive representation without sacrificing the substantive representation of underrepresented minorities.

In what follows, in Section 2, we discuss the institutional background for our study. In Section 3, we describe our data sources and in Section 4, we describe and evaluate our identification strategy. In Section 5, we present evidence on the impact of the IRCA legalization, and political control, on district shapes and composition; in Section 6, we present evidence on political outcomes. In Section 7, we discuss our findings and conclude.

2. Institutional background

2.1 Redistricting in the United States

Historical background: The practice of redistricting — that is, redrawing congressional district boundaries to accommodate population changes — has been a feature of American democracy from the birth of the Republic. In principle, redistricting aims to preserve the principle of "one person one vote," a notion enshrined in the Constitution. Although intended to equalize representation, the drawing of district boundaries has long been used by the politicians in charge of the process as a tool to serve political aims (Friedman and Holden 2008; Levitt 2008). The best-known example is the electoral district drawn in the shape of a salamander in *1812* by then-Governor of Massachusetts, Elbridge Gerry — hence the portmanteau "Gerry-mandering" — shown in Figure 1a.

Beginning in the 1960s, redistricting plans came under increasing legal scrutiny, with concern over possible violations of the Equal Protection Clause of the 14th Amendment to the US Constitution. Two binding legal criteria were used to evaluate district boundaries: (*i*) as near as possible to equal population across districts; and (*ii*) spatial contiguity.[12] In addition to these, the spatial *compactness* of congressional districts was increasingly emphasized by Supreme Court justices as a key factor in evaluating congressional district plans. Compactness is also required under the 1901 Apportionment Act and in many state Constitutions today (Fryer Jr. and Holden 2011). While widely viewed as a *prima facie* desirable feature of district boundaries, compactness is not precisely defined in the law, nor are there established benchmarks against which to compare district compactness. This means that there remains significant scope for variation in district compactness arising from politically-motivated redistricting.

Contemporary redistricting institutions: Redistricting today occurs in each US state with more than one political district (43 of the 50 states in 1990) following each decennial US census.[13] As discussed in Friedman and Holden (2008), redistricting typically follows a legislative

12. For a discussion, see Friedman and Holden (2008) or Fryer Jr. and Holden (2011).

13. While this is not always an explicit requirement of state law, the National Conference of State Legislatures writes that, "The standard practice in the states, over the last few decades, was to use the federal decennial census data." There exists some intercensal redistricting, but this is very rare. See `https://www.ncsl.org/research/redistricting/redistricting-and-use-of-census-data.aspx`, last accessed October 27, 2022.

process similar to that of other bills: new districts are proposed by legislators and require approval of the state's legislative and executive branches. Exceptions to the usual legislative process occur in states with redistricting commissions in place (in the 1990 redistricting cycle, four states had such commissions). Thus, in states with politically unified legislative and executive branches (and without independent commissions), one party has effective control over the redistricting process (subject to the constraints imposed by the judicial branch discussed above). In states with divided governments, political compromise may be required to redraw district boundaries, potentially restricting partisan redistricting.[14] In the 1990 census redistricting process, Democrats had control over the process in 17 states, Republicans had control in 2 states, and no party had control in 24 states, due either to divided governments or to the presence of redistricting commissions (see Online Appendix A).

2.2 The Immigration Reform and Control Act of 1986 (IRCA)

IRCA provisions: The Immigration Reform and Control Act (IRCA) of 1986 was the most comprehensive bill passed by the United States government to address the issue of unauthorized immigration. The bill, passed by the 99^{th} United States Congress and signed into law by President Reagan in November 1986, included three primary provisions: sanctions for employers who knowingly hired unauthorized immigrants; increased funding for border security; and, the flagship provision, an amnesty program that provided legal status and a pathway to citizenship to nearly all three million undocumented immigrants in the country at that time (Chishti and Kamasaki 2014; Sabet and Winter 2021).

At the time of the Act, there were around 3.5 million undocumented immigrants residing in the United States, corresponding to over 1 percent of the population (Rytina 2002). By 1990, approximately 3 million people applied for temporary resident status, of whom around 2.8 million (i.e., over 90 percent) were granted permanent residence.[15] On acceptance of their application, immigrants were given temporary legal status under the title of Temporary Resident Aliens, which could last for as long as 18 months. After this period, applicants were given permanent residency, provided they successfully completed a civics test and a language examination. Five years after permanent residency (i.e., from around 1992 on), those legalized under the IRCA were eligible for naturalization — and thus voting rights. Figure 2 illustrates the geographical distribution of IRCA immigrants across counties of the United States. While many immigrants are concentrated in states near the border, the figure also demonstrates that immigrants are spread across the country, with substantial variation within states as well. Finally,

14. It is possible for a single party to control redistricting even in states with divided governments, depending on the state's specific redistricting regulations (e.g., control may be assigned entirely to the legislative branch). See Coriale, Kaplan, and Kolliner (2020) for details. Of course, even in the absence of single-party political control, it is possible that politicians from different parties will collude in redistricting, e.g., to increase their likelihood of reelection.

15. We derive these figures from the Legalization Summary Tapes of the Immigration Naturalization Service (INS); see also Rytina (2002).

one can see that in many counties, the IRCA share of the population was over one percent of the population making the legalization of IRCA migrants a substantial political shock.

The IRCA as a partisan shock: The legalization of nearly 3 million illegal immigrants under the IRCA has two features that are important to our analysis. First, the IRCA was a shock that disproportionately affected Hispanics — the INS Tape Data indicate that 86 percent of the legalized IRCA immigrants were of Hispanic origin.[16] The differential impact on the voting rights of a historically underrepresented ethnic group suggests the second important feature: the possibility of a partisan bias in the expanded electorate, favoring the Democratic Party (Kuziemko and Washington 2018). We test for such a partisan bias, examining the effects of county IRCA population shares on county-level vote shares in US Presidential elections before and after legalized immigrants were able to attain voting rights.[17] One can see in Figure 3 the estimated impact of a one standard deviation greater county IRCA share on the Democratic Party vote share by Presidential election. We find that in the 1984, 1988, and 1992 elections (prior to substantial naturalization of the IRCA population), counties with a higher IRCA share displayed no differential voting behavior compared to counties with a lower share. In 1996 and 2000, however (after IRCA immigrants begin to be naturalized), the Democratic vote share is statistically significantly greater in counties with a higher IRCA share.[18] Thus, the IRCA shock to the electorate was *partisan*, differentially expanding the number of supporters of the Democratic Party.

Second, the IRCA granted legal status to immigrants who were already in the United States and were already included in census population counts (Passel 1986; Baker and Rytina 2013). The law thus shocked the electorate without altering underlying demographic or socioeconomic characteristics of the counties in which these immigrants lived. Crucially, the IRCA did *not* change population counts in the US census, which would directly have affected district boundaries to preserve the "one person one vote" principle. Thus, the IRCA legalization of immigrants generated partisan variation in the composition of the electorate, without concomitant changes that may themselves have affected district boundaries.

16. See Sabet and Winter (2021) for further details concerning the IRCA, including more information about the demographic background of the immigrants.

17. By examining county-level vote shares in Presidential elections, we are able to study partisan bias that is largely unaffected by any politically-motivated changes to congressional districts. We estimate a regression that includes county fixed effects, and state-by-congress fixed effects, and that controls for the time varying effect of the pre-IRCA share of the county population that is of Hispanic origin. The model also controls for the time-varying effect of counties with missing IRCA information.

18. In addition to the direct effect on the electorate through the naturalization of previously illegal immigrants, the IRCA likely also mobilized the broader Hispanic American community in locations with more legalized immigrants (Sabet and Winter 2021). The effects we observe combine the voting of the IRCA population and spillovers to the broader community.

2.3 The 1965 Voting Rights Act

The 1965 Voting Rights Act (VRA) was among the most comprehensive and impactful civil rights laws in American history (Cascio and Washington 2014; Aneja and Avenancio-Leon 2019; Bernini, Facchini, and Testa 2022). It prohibits racially discriminatory voting practices, which were adopted in many states of the US South following the end of Reconstruction (Naidu 2012). Although primarily intended to safeguard voting rights for African Americans, the Act was renewed and enhanced in 1970, 1975 and 1982 to help safeguard the voting rights of all minorities.[19]

Two provisions of the VRA that are especially relevant to the study of the 1990 census redistricting. First, Section 5 of the VRA required that jurisdictions (generally, states or counties within states) covered by it seek "pre-clearance" from the Department of Justice for any changes affecting voting, including redistricting.[20] This requirement could have shaped the 1990 census redistricting process differentially in states and counties with different numbers of IRCA migrants.

Second, Section 2 of the VRA more specifically regulated redistricting, prohibiting any redistricting plans intended to be racially discriminatory. Importantly, the 1982 amendment of the VRA relaxed the definition of discriminatory plans to include those that may not have had discriminatory intent, but which nonetheless had discriminatory results. As Washington (2012) notes, this stimulated the creation of majority-minority districts in the 1990 census redistricting, particularly where Section 5 of the VRA also required federal pre-screening of redistricting plans (where Section 5 did not apply, there was both less scope for the creation of new majority-minority districts and less federal scrutiny). This effect of Section 2 of the VRA, too, could have shaped the 1990 census redistricting process differentially in states and counties with different numbers of IRCA migrants.

We attempt to isolate the effect of variation in the IRCA share on the 1990 census redistricting from the effects of the VRA. In all of our empirical specifications we control for the time-varying effect of whether a jurisdiction was subject to the pre-clearance requirement of Section 5.[21] We also examine samples in which all Hispanic-majority and Black-majority districts are excluded, as these are locations plausibly most affected by Section 2 of the VRA.

19. A brief historical overview of the VRA is provided by the US Department of Justice online at: `https://bit.ly/3C1j1e1`, last accessed May 31, 2023.

20. The states of Alabama, Alaska, Arizona, Georgia, Louisiana, Mississippi, South Carolina, Texas, and Virginia were covered by Section 5 in their entirety. In addition, parts of California, Florida, New York, North Carolina, South Dakota, and Michigan were covered. See `https://bit.ly/3WwWdMg`, last accessed June 1, 2023. At the time of the 1990 census redistricting, jurisdictions were covered by Section 5 according to a coverage formula articulated in Section 4 of the VRA. This is no longer the case since the decision in *Shelby County v. Holder*, 570 U.S. 529 (2013).

21. Our results are also robust to excluding this control.

3. Data

To conduct our baseline analysis, we construct panel dataset at the county×Congress level: that is, the cross-sectional unit is the county, observed at each congressional session. In addition, we aggregate data to the state×Congress level as we discuss below. Throughout our analysis, we study the 43 US states with multiple congressional districts in 1990.[22] In this section we describe the sources and construction of the variables used in our analyses.

IRCA immigrants: The key explanatory variable in our study is the cumulative number of IRCA applicants per 1,000 county inhabitants in the United States measured in 1990. These data are taken from the Immigration and Naturalization Service (INS) Legalization Summary Public Use Tape data. An important limitation of this data is that there are no county identifiers for immigrants who applied from counties with populations less than 100,000 or with fewer than 25 applicants. We thus impute the number of IRCA immigrants in counties with missing IRCA information. To do so, we allocate a state's unassigned IRCA immigrants to counties with missing IRCA information according to the share of the total Hispanic population residing in such counties in 1990.[23]

We evaluate our imputed data first by conducting a robustness analysis excluding counties with missing IRCA information. Although this considerably shrinks the number of counties in our sample, our estimates are qualitatively unchanged (and quantitatively larger), suggesting that estimates based on imputed data are robust. In addition, we calculate the cumulative number of IRCA applicants per capita at the *state* level, which can be calculated for all states. We use state level IRCA shares in a district×Congress level panel, assigning a state's IRCA share to each district in that state.[24] Encouragingly, we find similar patterns exploiting the state level variation in a district×Congress level panel to those found in the county×Congress level panel.

When examining state level political outcomes — in a state×Congress level panel — we again use the aggregated state level IRCA shares as our explanatory variable of interest.

Legal control of redistricting: We identify which party (if any) exercised legal control of the 1990 census redistricting process, as well as which states employed independent or non-partisan redistricting commissions. Our coding of legal control has three categories: Democratic control, Republican control, and no legal control of redistricting (which includes the four states with redistricting commissions). This latter category serves as the omitted ref-

22. We thus drop from our analysis the 7 single district states of Alaska, Delaware, Montana, North Dakota, South Dakota, Vermont and Wyoming.

23. All IRCA immigrants have state identifiers, allowing us to identify the total number of a state's immigrants not assigned to identified counties. The Hispanic share of a county's population is available for all counties, even those missing the IRCA information, allowing us to calculate the share of Hispanics in each county among counties with missing IRCA information.

24. As a reminder, we cannot measure a district's IRCA share as the IRCA data are reported only at the county level.

erence group in our empirical analysis. We take this classification from Coriale, Kaplan, and Kolliner (2020). Because the institutional details determining legal control can be nuanced, as a robustness check, we use an alternative definition of legal control of redistricting based on *The American Redistricting Project*. We provide the codings of party control by state from both sources in Online Appendix Table A.1.

Compactness of congressional districts: We construct a panel of US congressional district boundaries from the 97^{th} through 107^{th} US Congresses. We obtain digital boundary definitions of congressional districts from Lewis et al. (2013). For each district, in each congressional session, we calculate four measures of district compactness using the Python package "geocompactness." Two of these measures — Polsby-Popper and Schwartzberg — capture *indentation* while the other two — Reock and Convex Hull — capture *dispersion*. All measures are on a scale from 0 to 1, which we multiply by 100 to ease interpretation. The lower the value, the less compact a district (i.e., the greater the spatial distortion) and *vice versa*.

Examples of these measures can be found in Figure 1. Figure 1b shows Nevada's 2^{nd} district during the 102^{nd} congress, the final election using districts from the 1980 census redistricting. The district, which has a very "regular" appearance, has a Polsby-Popper score of 73 and a Reock score of 59, indicating (consistent with one's visual impression) a compact district. In contrast, Figure 1c shows the boundaries of Florida's 3^{rd} district during the 103^{rd} congress, immediately following the 1990 census redistricting. What appear to be highly distorted boundaries of this district are reflected in its compactness scores: it has a Polsby-Popper score of 1 and a Reock score of 12, indicating that the district boundary is both highly indented and spatially dispersed.

The calculated district×Congress measures of spatial compactness are used directly in our district×Congress level analyses. As noted above, however, our IRCA data are most naturally used in a county×Congress panel. Moreover, because districts change in number over time, we cannot use district level compactness as an outcome in a panel dataset around the 1990 census redistricting. We thus must assign district spatial compactness outcomes to US counties (which are stable over time) by overlapping district and county boundaries and identifying in which congressional district a county lies. The vast majority of counties (around 85 percent) lie entirely within one district. In these cases, the county is simply assigned the compactness score of the district in which it lies. In those cases where a county overlaps with two or more districts (or when several districts lie within a county, as in Los Angeles County), we calculate a weighted sum of the compactness scores of the different districts that overlap with the county, where the weights correspond to the percentage of surface area of the county that overlaps with the district. In robustness exercises, we conduct our analyses in the sample of counties that lie within a single district to ensure that our method of weighting does not drive our results.

Majority-Hispanic districts: We calculate a district's Hispanic share by aggregating the

Hispanic and total populations in its constituent counties.[25] We then construct an indicator for a majority-Hispanic majority district and assign this to all counties associated with the district in our county×Congress panel.

Congressional election results: We collect election statistics for each US House election between the 98^{th} and 107^{th} congressional sessions from the website of the United States House of Representatives.[26] We use this information to construct several variables at the district×Congress level: an indicator for whether a Democrat or Hispanic candidate was victorious; and, the vote share for Democratic candidates.[27] As with our spatial compactness outcomes, we assign district election outcomes to US counties to construct a panel at the county×Congress level to match the level of variation in the IRCA shares.

We also aggregate the election district election data to the to the state×Congress level. We consider the count of US Representatives in a state's congressional delegation who are Democrats as well as the count of Hispanic Representatives. Using information on vote shares by party, we are able to calculate the efficiency gap (Stephanopoulos and McGhee 2015), which measures the efficiency with which Democrats are able to translate votes into US House seats in a given election.

Congressional delegation ideology: We measure the ideology of elected House Representatives using DW-NOMINATE scores, taken from Boche et al. (2018). We use the first dimension of the score, which is interpreted as a scale of liberalism (lower values) versus conservatism (higher values). We use the median DW-NOMINATE score in a state's congressional delegation to measure the ideological slant of a state's House Representatives by state×Congress.

County covariates: We gather data at the county×year level on a wide range of county characteristics from the US Census Bureau. We convert these data into a county×Congress level panel and use them to test for differential levels or trends of socioeconomic variables potentially associated with county IRCA shares around the time of the 1990 census redistricting. We also use these variables to test for differential "long differences" across the decade of the 1980s associated with a county's IRCA share. Finally, we control for the time-varying effects of these variables, measured in 1990.

25. When districts contain a fraction of a county we weight the populations accordingly.

26. See `https://history.house.gov/Institution/Election-Statistics/`, last accessed October 27, 2022.

27. We identify Hispanic representatives using data from the website of the United States House of Representatives. See `https://history.house.gov/Exhibitions-and-Publications/HAIC/Historical-Data/Hispanic-American-Representatives-and-Senators-by-State-and-Territory/`, last accessed October 27, 2022.

4. Identification strategy

We implement a difference-in-differences research design in order to identify partisan gerrymandering and its consequences. Specifically, we compare the spatial compactness and demographic composition of congressional districts in counties with greater and fewer IRCA applicants immediately before and after the 1990 redistricting cycle (i.e. using district boundary information from the 102^{nd} and 103^{rd} congressional sessions). We also compare political outcomes that might change as a result of changed district boundaries and composition in the five congressional elections leading up to the 1990 redistricting cycle and the five congressional elections that follow it. For both sets of outcomes, our identifying assumption is that a county's exposure to the IRCA changed the political incentive to redistrict locally *without* being associated with other changes in local socioeconomic variables that themselves may affect district shapes or political outcomes. In Section 2, we demonstrated that legalization under the IRCA was associated with increased Democratic vote shares, suggesting a local change in the electorate, and thus a local change in the political incentive to redistrict. In this section, we examine whether variation in the IRCA was associated with other relevant observable county characteristics.

4.1 The IRCA and trends in county observables

We begin by estimating the parameters of the following regression model 1:

$$y_{c,t} = \sum_{j=1982}^{2000} \beta_j [IRCA_{c,1990} \times D_j^t] + \sum_{j=1982}^{2000} \gamma_j [HISP_{c,1980} \times D_j^t] + \delta_c + \zeta_{st} + \alpha_{mt} + \epsilon_{c,t}, \quad (1)$$

where $y_{c,t}$ is a standardized measure of a given socioeconomic variable for county c in year t, and t is the election year for a congressional session (i.e., our panel is biennial). We consider the following county-level outcomes: population, income per capita, federal expenditures, public school enrollments, housing values, birth rates, social security payments, number of persons employed, the Hispanic population, and the number of violent crimes.

The explanatory variable of interest, $IRCA_{c,1990}$, is the standardized number of IRCA applicants per 1,000 capita in a county measured in 1990. We interact this with year dummies, denoted by D_j^t. The model controls for the time varying effect of the pre-IRCA share of the county population that is of Hispanic origin, $HISP_{c,1980} \times D_j^t$, and includes county and state×congress fixed effects, denoted by δ_c and ζ_{st}, respectively (this matches our baseline model, below, examining within state×Congress variation). The model also allows for time-varying shocks specific to counties with missing IRCA information, captured by α_{mt}. Standard errors are denoted by $\epsilon_{c,t}$ and are clustered at the county level, which is the level of variation in the explanatory variable of interest (the IRCA share).

Where data is available, we estimate the model from 1982, the first election after the 1980 redistricting cycle (and which determined the 98^{th} US Congress) until 2000, the last election based on the 1990 census redistricting (and which determined the 107^{th} US Congress). The reference category is 1992, the first election after the 1990 redistricting. The coefficient β_j thus captures the predicted change in a county's socioeconomic covariate associated with a one-standard-deviation increase in the county's IRCA share, in year t, compared to 1992. We plot β_j for each covariate in Figure 4. As can be seen, conditional on the fixed effects in the model (i.e., within a state×Congress), a higher IRCA share is not associated with significant differences in either levels or trends of a wide range of county covariates. This increases one's confidence that the IRCA provided a political shock without coincidental socioeconomic change.[28]

4.2 The IRCA and long differences

In addition to testing for differential trends in county socioeconomic characteristics around the time of the 1990 census redistricting, we check for differential changes over the decade leading up to the 1990 redistricting cycle. We estimate the same estimating model as shown in equation 1 for the same county covariates, but we restrict the sample to just two time periods: 1980 and 1990.[29] We plot the long difference coefficient for each covariate in Figure 5. As can be seen in the figure, there is only one significant difference in the 1980–1990 changes along these dimensions associated with a county's IRCA share (birth rates). This suggests that over the decade leading up to the 1990 census redistricting, counties' IRCA shares were generally not associated with differential changes in socioeconomic outcomes, further increasing confidence in the validity of our identifying assumption.

The analysis conducted above is *not* intended to suggest that IRCA shares are randomly assigned across counties. It is surely the case that locations closer to the Mexican border and locations with higher preexisting levels of Hispanic immigration would also have a higher share of legalized immigrants under the IRCA. Rather, the evidence above *does* suggest that conditional on state×Congress fixed effects, as well as on pre-IRCA Hispanic shares, IRCA shares are no longer significantly associated with county covariates in levels or trends.

5. Identifying gerrymandering: the IRCA and district compactness and composition

We begin our analysis by identifying the impact of immigrant legalization on the spatial distortion of US congressional district boundaries. We consider first the following baseline model,

28. It is worth noting that Sabet and Winter (2021) find that a higher IRCA share is associated with significantly greater fiscal transfers, but this effect arises only after the 1990 census redistricting.

29. The exceptions are: employed population, which is available only from 1990 (and thus excluded from our analysis), public school enrollment, available from 1988 (and thus excluded); federal expenditure, available from 1984 (so the long difference is 1984–1990) and violent crimes, available from 1982 (long difference from 1982–1990).

specified in equation 2, that abstracts from political control of the redistricting process:

$$\begin{aligned} COMPACT_{c,t} = \beta[IRCA_{c,1990} \times P_{102}] + \gamma[HISP_{c,1980} \times P_{102}] + \pi[VRA_c \times P_{102}] \\ + \theta[\mathbf{X_{c,1990}} \times P_{102}] + \delta_c + \zeta_{st} + \alpha_{mt} + \epsilon_{c,t}, \end{aligned} \tag{2}$$

where $COMPACT_{c,t}$ is the compactness score of the electoral district that overlaps with county c in time period (congressional session) t.[30] We compare district compactness using information from only two congressional sessions: the 102^{nd} congressional session (immediately before the 1990 redistricting) and the 103^{rd} congressional session (immediately after the 1990 redistricting). Including more congressional sessions in our panel would artificially increase the number of observations in our sample, because the district shapes hardly change between censuses.

The explanatory variable of interest, $IRCA_{c,1990}$, is the standardized number of IRCA applicants per 1,000 capita in a county measured in 1990. We interact this with the variable P_{102} — an indicator for a congressional session later than the 102^{nd}. In our analysis of spatial distortion, the variable is equal to 0 for the 102^{nd} congressional session and 1 for the 103^{rd} session.[31] This interaction captures the effect of the county IRCA share on district boundary changes after the 1990 census redistricting. We also add to this analysis a range of county level socioeconomic characteristics measured in 1990 and interacted with P_{102}, denoted by $\mathbf{X_{c,1990}} \times P_{102}$.[32] The model controls for the time-varying effect of the pre-IRCA share of the county population that is of Hispanic origin, $HISP_{c,1980} \times P_{102}$, and includes county and state×congress fixed effects, denoted by δ_c and ζ_{st}, respectively. The model also allows for time-varying shocks specific to counties with missing IRCA information, captured by α_{mt}. Importantly, the model captures the time-varying effect of whether a county was subject to the pre-clearance requirement of Section 5 of the VRA, $VRA_c \times P_{102}$. Standard errors are denoted by $\epsilon_{c,t}$ and are clustered at the county level, which is the level of variation in the explanatory variable of interest (the IRCA share). We weight the regressions by the number of districts that intersect a county.

In addition to this baseline model, we estimate a model that allows the effect of the IRCA share in a county to vary both across time (before and after the 1990 census redistricting) and depending on the political party in control of redistricting. Specifically, we add to the baseline model two triple interactions: $IRCA_{c,1990} \times P_{102} \times R$ and $IRCA_{c,1990} \times P_{102} \times D$ (as well as lower order terms). These interactions capture the differential effects of the county IRCA share

30. As noted above, for counties overlapping with more than one electoral district, we calculate the weighted sum of the compactness scores of the various electoral districts that overlap with a county in congressional session t.

31. Below, we consider more congressional sessions, but the P_{102} remains defined as an indicator for a congressional session later than the 102^{nd}.

32. These include county population, per capita income, share of the population over 18, unemployment rate, the percent with a Bachelor's degree or more, the birth rate, the share of the population that is white and the share of the population that is black.

on district boundaries after the 1990 census redistricting, depending on whether the process was controlled by Republicans (indicator $R = 1$) or Democrats (indicator $D = 1$), compared to the omitted category of no political control.[33]

Table 1 presents our baseline estimates as well as estimates of the model incorporating political control for each of the four measures of district compactness. In columns 1, 3, 5 and 7, we present estimates of β as specified in estimating equation 2. As can be seen, no clear pattern emerges: the Polsby-Popper index slightly increases with IRCA shares, while the other three measures fall. None of the estimates is statistically significant, and they are all quantitatively small, suggesting that significant redistricting is not an intrinsic characteristic of counties with a high IRCA share of the population.

In columns 2, 4, 6 and 8 of Table 1, we introduce legal control of redistricting into the analysis and across all four measures a clear pattern emerges: in states where Democrats control the 1990 redistricting process, counties with a higher share of IRCA immigrants are located in congressional districts that are significantly less compact immediately following redistricting. The magnitude of the coefficients suggest that a one standard deviation increase in the IRCA share of a county's population is associated with anywhere between a 2.5 percent to a 6 percent reduction in compactness relative to the pre-redistricting sample mean (or around 15 percent of the pre-redistricting sample standard deviation).

Importantly, where control of redistricting is in the hands of non-partisan commissions or where no party controlled redistricting, we find no such relationship between a county's IRCA share and its congressional district boundaries: the coefficients are quantitatively small and statistically insignificant even while they are estimated more precisely than effects where Democrats are in control.[34]

5.1 Robustness exercises

In Figure 6, we evaluate the robustness of our findings regarding the effects of the interaction of IRCA shares and political control on compactness using one indentation measure (Polsby-Popper) and one dispersion measure (Reock) as outcomes.[35] We plot only the coefficient on the triple interaction of interest, $IRCA_{c,1990} \times P_{102} \times D$, though we report the full set of results for all regressions plotted in the figure in Online Appendix Table B.1. We begin (model 1, top row) by reproducing our findings in Table 1, column 2 (Polsby-Popper) and column 6 (Reock), to provide a baseline comparison. Next, moving down the figure, we present estimates from a more

33. As noted above, Republicans controlled the 1990 census redistricting process in only 2 states, making statistical inference in the case of Republican control noisy. For this reason, we concentrate on differential effects in states with Democrat control and do not report the coefficient on $IRCA_{c,1990} \times P_{102} \times R$ although we do estimate it in our model.

34. In those states where Republicans controlled the 1990 redistricting process, we find the opposite effect: counties with higher IRCA shares exhibit increases in the compactness of their congressional district boundaries following the 1990 redistricting. However, because there are only two states where Republicans controlled redistricting, it is difficult to make reliable statistical inference, and we interpret these results with caution.

35. Results are very similar when using the other two measures presented in Table 1.

parsimonious model in which we drop all socioeconomic covariates that we use as controls; one can see that our results are nearly identical to the baseline (model 2). Next (model 3), we add to the baseline model additional socioeconomic controls measured in 1990 (allowing them to have time-varying effects).[36] Again, for both measures of spatial compactness, our results are nearly identical to the baseline. Next, to address the concern that our 1990 measures of county covariates are endogenous to the county IRCA share, we use instead baseline socioeconomic controls measured in 1980. One can see that our findings remain very similar (model 4). In model 5, we interact all county socioeconomic controls with both P_{102} *and* our measure of party control of redistricting, allowing county characteristics other than the IRCA share to differentially shape district boundaries depending on the party controlling redistricting. Again, this does not affect our conclusions. Next, we estimate our baseline specification, but using the alternative definition of legal control of redistricting taken from *The American Redistricting Project*; again our results are robust (model 7). Across all of these specifications, for both measures of spatial compactness, we thus find a significant reduction in spatial compactness in locations with a higher IRCA share and Democratic Party control of redistricting.

As mentioned above, an important limitation of our IRCA data from the INS is that it includes no county identifiers for immigrants residing counties with fewer than 100,000 inhabitants or fewer than 25 IRCA immigrants. In our baseline analysis, we thus impute the IRCA share of counties with missing IRCA information as described in Section 3. Importantly, all of our regressions allow for time varying effects of counties with missing IRCA information, so changes in districts associated with small counties or counties with few IRCA immigrants do not account our results. In models 7 and 8 of Figure 6, we undertake two additional exercises to assess the impact of our imputation method on our results. First, in model 7, we maintain our baseline sample of counties, but employ an alternative imputation method for counties with missing IRCA information, using the Hispanic share of the county's total population. Again, for both measures of spatial compactness, we find a significant reduction in spatial compactness associated with a higher IRCA share where Democrats controlled redistricting. In model 8, we restrict our analysis to counties for which the INS data provides information on IRCA immigrants — that is, counties with large populations and/or sufficient IRCA immigrants. Although this considerably shrinks the size of the sample (and changes the geographic composition of the analysis), we continue to see a reduction in spatial compactness associated with a higher IRCA share where Democrats controlled redistricting. Our estimate is statistically significant when considering the Polsby-Popper measure of compactness, and marginally statistically insignificant when using the Reock measure (but point estimates are very very similar to the baseline model). These results suggest that missing IRCA share data and our imputation method did not drive our baseline findings.

In model 9, we restrict the sample to counties that lie entirely within a single congressional

36. These include county poverty rates, the share of households with children, the number of social security recipients and the amount of social security payments, housing values and violent crimes.

district, avoiding the need to create a weighted sum compactness score (and any associated distortion). As can be seen in the figure, estimates are qualitatively and quantitatively similar to the baseline when restricting the analysis to this sample. Finally, in model 10, we omit counties located minority-majority districts (specifically, Hispanic-majority or Black-majority) to eliminate the potentially confounding influence of Section 2 of the VRA on the 1990 census redistricting process. Estimates for both measures of spatial compactness remain qualitatively and quantitatively unaffected, suggesting that the distortion of spatial compactness we observe in response to the IRCA is not driven by a subset of districts complying with the requirements of the VRA to safeguard the voting rights of minorities.

Placebo exercise In Figure 6, model 11, we estimate the effect of a county's IRCA share on district compactness, but now considering changes in districts around the time of the *1980* census redistricting, six years prior to the passage of the IRCA. We now consider the "pre" congressional session as the 97^{th} and the "post" session to be the 98^{th}. We also recode the legal control of redistricting variable reflecting the parties in control of redistricting in 1980. If the IRCA share only affected Democrat-controlled redistricting as a result of the changed electorate composition, one would expect to see no significant interaction between the IRCA share and Democrat control around the 1980 census redistricting. Indeed, as can be seen in model 11 of Figure 6, the IRCA is not predictive of changes in spatial compactness in the 1980 census redistricting, pre-legalization. This exercise provides further evidence that the significant redistricting of these counties during the 1990 census redistricting cycle is, in fact, due to the political shock to the electorate prompted by the IRCA.

5.2 State IRCA variation and district compactness

We next corroborate our county level analysis of spatial compactness by replicating our analysis, but now exploiting variation in the number of IRCA immigrants at the state level in a panel of US congressional districts. An analysis that exploits state level variation in the IRCA share is valuable for several reasons. First, because we observe each IRCA immigrant's state of residence, this analysis does not rely on imputed data. Second, by assigning the state IRCA share to each district in the state, we can directly study district level spatial distortion, rather than relying on our matching of districts to counties. Finally, this analysis can help address concerns regarding SUTVA violations. Consider a gerrymanderer who redraws the district boundaries in the districts with more IRCA immigrants, distorting districts to exploit the changed electorate. The same gerrymanderer may, either to minimize the appearance of distortions across the state or because of geographical constraints, *reduce* spatial distortions in districts that are less exposed to the IRCA. Such spillovers from districts with many IRCA immigrants to districts with fewer could bias our estimates away from zero. Alternatively, spillovers from districts with high IRCA shares that lead to *increased* distortion in districts with a low IRCA share may bias the analysis at the county level toward zero. Exploiting state level variation in the number of

IRCA immigrants to explain US congressional district compactness alleviates these concerns: every district within a state is now assigned to the same IRCA share. Of course, it is important to note that this exercise has drawbacks. It offers less statistical power than the corresponding county level analysis, it arguably exploits less "clean" variation than the within-state analysis considered above, and it introduces measurement error by assigning to each district the state's IRCA share.

In Table 2, we present regression results from a model analogous to those estimated in columns 2, 4, 6 and 8 of Table 1, but in which the cross-sectional unit of analysis is the congressional *district*. The explanatory variable is now the standardized number of IRCA immigrants per 1,000 *state* inhabitants measured in 1990 interacted with P_{102}, and with an indicator for the party holding legal control of redistricting, both of which are defined as in our baseline specification. The regressions include state fixed effects and congressional session fixed effects, and they control for the time-varying effect of the pre-IRCA share of the state population that is of Hispanic origin, as in our baseline model. Again we control for the time-varying effect of whether a state (or part of a state) was subject to the pre-clearance requirement of Section 5 of the VRA. We now cluster standard errors at the state level because this is the level at which IRCA shares vary in this model. Given the modest number of clusters (43), we also present p-values calculated using the wild cluster bootstrap (Cameron, Gelbach, and Miller 2008).

The results are reported in Table 2 and qualitatively confirm the patterns observed in our baseline estimates. Where Democrats control redistricting, states with high shares of IRCA immigrants have differentially less compact congressional districts across all measures of compactness. While the lack of statistical power (particularly evident when inference is based on the wild cluster bootstrap) makes these patterns only suggestive, it is reassuring that this alternative analysis broadly matches our baseline evidence.

In Figure 7, we employ a less parametric approach to studying changed district compactness depending on states' levels of IRCA immigrants and their political control of redistricting. We plot the distributions of district compactness, for every US district, for the 102^{nd} congressional session and the 103^{rd}, splitting the sample by state IRCA share (above/below median) and political control (Democrat control/other). As above, we consider one indentation measure (Polsby-Popper, in Panel (a)) and one dispersion measure (Reock, Panel (b)).

One can see in the figure a striking and consistent shift left in the Polsby-Popper distribution (i.e., toward lower compactness, or greater distortion) specifically in states with above median IRCA share and Democratic control of redistricting. Changes in the Reock distribution are less striking, but there is a visible movement of the distribution from right to left, again toward less compactness or greater distortion. These patterns suggest that districts in states with high IRCA immigrant populations and Democratic control of redistricting saw a consistent shift toward more distorted districts, and that our findings are not inflated by spillovers that significantly increased compactness (reduced distortion) in low IRCA counties located in high IRCA states.

5.3 Majority-Hispanic Districts

We next examine how the IRCA's shock to the electorate affected the demographic composition of US congressional districts, in particular, studying whether the IRCA led to the creation of majority-Hispanic districts. This outcome is of interest for several reasons. First, the establishment of majority-minority districts is of interest *per se*, to both policymakers and academics (Cameron, Epstein, and O'Halloran 1996; Shotts 2003; Cox and Holden 2011; Washington 2012). Second, Democrats have long supported the descriptive representation of minority groups, and they may have used their control of redistricting to produce more majority-Hispanic districts for ideological reasons. Finally, because Hispanics have long leaned strongly Democratic[37], a majority-Hispanic district also serves as a proxy for Democratic gerrymanderers' concentration of their party's ardent supporters (as predicted in, e.g., Friedman and Holden (2008) and Kolotilin and Wolitzky (2020)).

We estimate the same county×Congress level models as those presented in Figure 6, but now examining the outcome of whether a county overlaps with a majority-Hispanic congressional district. We plot the coefficient estimates of the triple interaction of interest, $IRCA_{c,1990} \times P_{102} \times D$, in Figure 8, and we report the full set of estimates for all 11 regression models in Online Appendix Table B.2.[38]

We plot estimates from our baseline model in Figure 8, model 1. We find that a one-standard deviation increase in the IRCA share, in states where Democrats controlled redistricting, is associated with a statistically significant 2.5 percentage point increase in the likelihood that a county overlaps with a majority-Hispanic congressional district. This is a substantial effect, just over one quarter of the pre-redistricting standard deviation. We find no significant effect of a higher IRCA share on the proportion of majority-Hispanic districts where Democrats did not control redistricting (reported in Online Appendix Table B.2).

In models 2–5, we estimate specifications including various combinations of controls. One can see that the estimated effect of a higher IRCA share in states where Democrats controlled redistricting is very stable across specifications. In model 6, we employ our alternative definition of legal control of redistricting, and again find results similar to the baseline. In models 7–8, we evaluate the impact of imputed IRCA shares on our estimates (i.e., due to missing county identifiers in the IRCA data). We undertake the same robustness exercises as in Figure 6: first, we impute IRCA shares in counties with missing data using Hispanic shares; second, we simply drop counties with missing IRCA data. One can see in Figure 8, models 7–8, that results are qualitatively identical to our baseline. When we restrict our analysis to counties for which the INS data provides information on IRCA immigrants — that is, counties with large populations

37. For data from the last two decades, see `https://www.pewresearch.org/hispanic/2016/10/11/latinos-and-the-political-parties/`, last accessed October 31, 2022.

38. We also present estimates examining the creation of majority-Hispanic districts, but considering only the voting age population (VAP), which is of particular interest in the redistricting process. Our findings are largely unaffected considering this alternative outcome, see Online Appendix Table C.1.

and/or sufficient IRCA immigrants — point estimates become larger, though estimates are not statistically significant due to expanded standard errors. In model 9, we restrict the sample to counties that lie entirely within a congressional district to mitigate potential distortions arising from weighting outcomes for counties that lie in more than one district. One can see that the results are very similar to the baseline.

In model 10, we address the concern that enforcement of Section 2 of the VRA may drive our findings. Because the outcome is an indicator for whether a county is in a Hispanic-majority district we cannot (as we did in Figure 6) simply drop all Hispanic-majority districts. Instead, we drop New Mexico, which was the one state the Department of Justice filed a complaint against for violating Section 2 of the VRA prior to the 1990 census redistricting — and also a state with a large IRCA share in several counties (see Figure 2).[39] One can see in Figure 8, model 10, that dropping New Mexico has very little effect on our estimates.

Finally, we conduct the placebo test considered above, examining the effect of a county's IRCA share and Democratic control of redistricting on the presence of majority-Hispanic districts, but now considering changes in districts around the time of the *1980* census redistricting. As can be seen in Figure 8, model 11, there is no relationship between the IRCA share, Democratic control, and the creation of Hispanic majority districts in the redistricting prior to the passage of the IRCA. This indicates the importance of the IRCA's political shock to the electorate in driving our results.

6. The IRCA and political outcomes

Thus far, we have documented the impact of the IRCA on the spatial distortion of US congressional district boundaries and the demographic composition of those districts. We now examine whether these changes affected political outcomes, particularly dimensions of minority (i.e., Hispanic) representation. To do so, we estimate models analogous to those used to study spatial distortion and the formation of majority-Hispanic districts. However, we now compare US House election outcomes across ten congressional elections: five elections conducted under the 1980 census district boundaries (i.e., from the 98^{th} to the 102^{nd} congressional session) and five congressional elections conducted under the 1990 census district boundaries (i.e., from the 103^{rd} to the 107^{th} congressional session).[40] In Figure 9 we present estimates from county level regressions, examining the impact of a county's IRCA share and Democratic control of redistricting on districts' congressional election outcomes matched to counties.[41] In Table 3, we present estimates from state level regressions, examining the impact of a state's IRCA share

39. The original dispute was United States v. New Mexico and Sandoval County (D.N.M. 1988), and involved the rights of Native Americans, rather than Hispanic immigrants (see `https://bit.ly/43KrEWb`, last accessed June 3, 2023). But the additional scrutiny may have affected redistricting considerations more broadly.

40. Each congressional election provides additional information, in contrast to the study of district boundaries, which were essentially fixed from the 98^{th} to the 102^{nd} congressional sessions, then fixed again from the 103^{rd} to the 107^{th} sessions.

41. All results presented in Figure 9 are reported in Online Appendix Table B.3.

and Democratic control of redistricting on the state's congressional delegation.

6.1 County level analysis

Descriptive representation: In panel (a) of Figure 9 we present estimates from a model in which the outcome is an indicator variable equal to one if a county overlaps a district represented by a Hispanic and zero otherwise. We find that where Democrats control redistricting, counties with a higher IRCA share are significantly more likely to be located in districts represented by a Hispanic following the 1990 census redistricting. While the absolute number of Hispanic representatives was not large, we find a large relative impact of the IRCA share and Democratic control of the 1990 census redistricting. In model 1, the baseline estimate, we find that a one standard deviation increase in the IRCA share increases the likelihood of a Hispanic representative by 6 percentage points where Democrats controlled redistricting. This is over half of the pre-redistricting standard deviation. In models 2–10, we undertake the same robustness exercises as those considered when examining changes in district composition (i.e., in Figure 8), and we find that the coefficient of interest remains stable across the various empirical specifications and sample restrictions.[42] A one standard deviation increase in the IRCA share increases the likelihood of a Hispanic representative by 5–10 percentage points where Democrats controlled redistricting. In model 11, we again conduct a placebo by testing for differential effects of IRCA shares and Democratic control of redistricting on the 1980 census redistricting (comparing the outcome of interest in the 5 congressional elections held under the 1970 redistricting plans to those in the 5 elections held under the 1980 plans), and find a null result. These patterns suggest that Democratic redistricting where there were more IRCA migrants increased the descriptive representation of Hispanic Americans.

Substantive representation: We next consider the substantive representation of Hispanics, examining both the intensive and extensive margins of Democratic Party outcomes. These outcomes would have been of interest to Democrats in charge of redistricting *per se*, reflecting partisan motives in addition to concern for the representation of Hispanics. In panel (b) of Figure 9, we repeat the analyses from panel (a), but now examining the Democratic candidate's vote share in the county's associated district. In our baseline estimate, Figure 9, panel (b), model 1, one can see that a one standard deviation increase in the IRCA share increases the Democratic vote share by over 3 percentage points where Democrats controlled redistricting. This is over 10 percent of the standard deviation of the outcome prior to redistricting. One can see in models 2–10 that the baseline estimate is largely robust to a wide range of alternative specifications. Only when we use our alternative imputation method for counties with missing

42. We follow Figure 8 and drop New Mexico to reduce the impact of Section 2 of the VRA on our estimates. If we instead drop all majority-minority districts, we find that the IRCA share and Democratic control are no longer associated with a higher likelihood of Hispanic representation. This suggests that the creation of majority-Hispanic districts (shown in Figure 8) played an important mediating role in increasing Hispanic descriptive representation.

IRCA information does our estimate meaningfully change (shrinking to zero). Still, the evidence generally points toward a higher Democratic vote share associated with a higher IRCA share and Democratic control of redistricting.

It is natural to wonder whether this finding arises mechanically due to the legalization and enfranchisement of largely Democratic Hispanic IRCA migrants. In fact, it is not mechanical. A generic rise in the number of Democratic voters in locations with more IRCA migrants is captured by the interaction term $IRCA_{c,1990} \times P_{102}$. Indeed, we estimate this generic effect to be (a statistically significant) 1–2 percentage points, as can be seen in Online Appendix Table B.3, Panel B. The triple interaction term we plot ($IRCA_{c,1990} \times P_{102} \times D$) captures the *differential* change in the Democratic vote share associated with a higher IRCA share specifically where Democrats controlled redistricting. In theory, this may have been *negative* if Democrats had tried to "spread" their voters across state. In practice, we find evidence of increased concentration of the Democratic party's ardent supporters (as predicted in, e.g., Friedman and Holden (2008) and Kolotilin and Wolitzky (2020)).

Finally, in panel (c) of Figure 9, we estimate a model in which the outcome is an indicator variable equal to one if a county overlaps a district represented by a Democrat and zero otherwise. Interestingly, there is no differential effect of a higher IRCA share in those states where Democrats had control of the redistricting process. This may reflect Democrats' multiple objectives in redistricting: if a higher IRCA share mechanically increased Democratic vote shares independent of gerrymandering, then Democrats may have strategically drawn districts aiming to achieve other objectives (e.g., increasing Hispanics' descriptive representation). Our findings corroborate those in Washington (2012): at least in this instance, the creation of majority-minority districts increased descriptive representation without sacrificing — but also without increasing — the substantive representation of underrepresented minorities. It is worth noting that our estimates of the generic effect of a higher IRCA share (captured by the interaction term $IRCA_{c,1990} \times P_{102}$) indicate a (positive and statistically significant) 3 percentage point increase in the probability of a Democratic representative. Even in the absence of strategic redistricting, the legalization of Democratic voters increased the probability of Democratic representative.

6.2 State level analysis

In Table 3, we study how the legalization of immigrants under the IRCA, along with the political control of redistricting, affected state level political outcomes. Our empirical specification remains the same, but we now aggregate the number of per capita IRCA immigrants to the state level.[43] While this analysis must be interpreted with caution due to the limited cross-sectional variation (the analysis only includes 43 clusters), the state-level findings broadly corroborate

43. We also replace county fixed effects with state fixed effects and state-by-congress fixed effects with congressional session fixed effects.

the results of our county-level analysis.[44]

Descriptive representation: In Table 3, column 1, we present estimates from a model in which the outcome is the count of the number of Hispanics in the state's House delegation. We find that following redistricting, in states where redistricting was controlled by Democrats, a one standard deviation greater IRCA share increases the number of Hispanic representatives by 0.4 — a large effect relative to the pre-redistricting standard deviation of 0.76. The effect is statistically significant (at the 95 percent level) when clustering standard errors at the state level, but it is not statistically significant at the usual standards when conducting statistical inference using the wild cluster bootstrap.

Substantive representation: In Table 3, column 2, we consider an "intensive margin" measure of substantive representation at the state level, examining the DW-Nominate score of the median member of the state House delegation (lower values indicate left-leaning voting behavior). One can see that following redistricting, in states where redistricting was controlled by Democrats, a greater state IRCA share is associated with a significant shift to the left of the median member of the state's delegation. In Table 3, column 3, we consider an "extensive margin" measure of substantive representation at the state level, examining the count of the number of Democrats in the state's House delegation. We find similar patters at the state level to those at the county level: a higher state IRCA share generally predicts an increase in the number of Democratic House members following redistricting, but there is no differential increase in those states where Democrats controlled the redistricting process.

Efficiency: In a final analysis of political outcomes at the state level, we present estimates from a model in which the outcome is the efficiency with which votes for a party were translated into congressional seats. Following Stephanopoulos and McGhee (2015), we calculate each party's "wasted" votes: votes for a party beyond those needed to win a seat in elections where a seat was won, and all votes cast for a party in elections where a seat was lost. We then construct our outcome as the difference between wasted Democratic votes — actual votes per district and not the vote share — and wasted Republican votes across all districts in a particular state in a particular congressional election. Hence, lower values of this efficiency gap outcome represent fewer wasted Democratic votes in a congressional election in a particular state. In Table 3, column 4, one can see that in states where redistricting was controlled by Democrats, a greater state IRCA share is associated with a reduction in Democrats' wasted votes (marginally statistically significant when clustering at the state level, but not significant when using the wild cluster bootstrap).[45] This suggests that Democrats' pursuit of multiple objectives

44. In Table 3, we present standard errors clustered at the state level, as well as wild cluster bootstrap p-values.

45. As suggested by Stephanopoulos and McGhee (2015), we impute congressional votes for uncontested districts using presidential voting results. Specifically, we obtain presidential voting results at the congressional district level from Kiernan Park-Egan (see `https://bit.ly/3OKD3B1`, last accessed May 31, 2023). We then predict the

in their partisan redistricting — for example, increasing Hispanics' descriptive representation without reducing their substantive representation — was accomplished while using votes more efficiently.

7. Conclusion

Research on gerrymandering has generated rich theoretical predictions that have been difficult to test due to the challenge of isolating variation in the composition of the electorate independent of other factors that may also affect the drawing of district boundaries. When millions of Hispanic Americans became legal residents (and later naturalized citizens) following the 1986 IRCA, they sharply changed the composition of local electorates without changing local demographics. We exploit this variation to test whether district boundaries changed in response to a changed electorate, as well as to examine the political consequences.

Consistent with theories of optimal gerrymandering, we find that Democrats in control of the redistricting process following the 1990 census significantly distorted political districts more affected by the change in the electorate. They "packed" their supporters into majority-Hispanic districts, creating districts more likely to elect Hispanics to Congress. State congressional delegations saw a shift to the political left and an increase in Hispanic members, though no change in the number of Democrats. These results inform our understanding of gerrymandering: they suggest that political control of the redistricting process is used strategically, with our evidence consistent with theories of optimal gerrymandering (Friedman and Holden 2008; Kolotilin and Wolitzky 2020).

The normative implications of the partisan redistricting we observe are ambiguous. On the one hand, Democrats appear to have exploited their control of redistricting to the benefit of their party: congressional delegations shifted to the political left and Democratic votes were more efficiently translated into seats in states with a larger IRCA shock and Democratic control of the redistricting process. In the current environment, with particular concern regarding *Republican* gerrymandering (Kolotilin and Wolitzky 2020), our results suggest that the phenomenon of politically-motivated redistricting does not belong to the domain of just one of the major political parties. On the other hand, Democratic redistricting achieved an increase in the descriptive and substantive representation of Hispanic Americans, a previously marginalized minority group. Partisan gerrymandering in this case served not only the interests of the political party in control, but also the historically disadvantaged.

Further academic work is needed to bring additional empirical evidence to bear on theories of strategic gerrymandering. This is particularly true in contexts in which politicians have multidimensional political objectives. In our setting, Democrats were largely successful

number of votes cast for Democratic or Republican congressional candidates in uncontested districts by using the estimated coefficients from a model that uses state and congressional session fixed effects as well as the number of presidential votes cast in a congressional district to predict the number of Democratic and Republican votes cast in contested districts. Using the number of presidential votes cast as a proxy for congressional votes or dropping uncontested districts produces similar results.

in pursuing multiple objectives, but this was likely aided by the expansion in the number of Democratic voters relaxing political constraints. How strategic gerrymanderers trade off competing objectives is an interesting area for future work.

References

Aneja, Abhay, and Carlos F Avenancio-Leon. 2019. "The effect of political power on labor market inequality: Evidence from the 1965 Voting Rights Act." *Working paper.*

Ansolabehere, Stephen, and Maxwell Palmer. 2016. "A two-hundred year statistical history of the gerrymander." *Ohio St. LJ* 77:741.

Ansolabehere, Stephen, and James M Snyder Jr. 2012. "The effects of redistricting on incumbents." *Election Law Journal* 11 (4): 490–502.

Baker, Bryan, and Nancy Rytina. 2013. *Estimates of the Unauthorized Immigrant Population Residing in the United States: January 2012.* Technical report. Department of Homeland Security, Office of Immigration Statistics, March.

Bernini, Andrea, Giovanni Facchini, and Cecilia Testa. 2022. "Race, representation and local governments in the US South: the effect of the Voting Rights Act." *Forthcoming, Journal of Political Economy.*

Bierbrauer, Felix, and Mattias Polborn. 2020. "Competitive gerrymandering and the popular vote." *CESifo Working Paper No. 8654.*

Boche, Adam, Jeffrey B Lewis, Aaron Rudkin, and Luke Sonnet. 2018. "The new Voteview. com: preserving and continuing Keith Poole's infrastructure for scholars, students and observers of Congress." *Public Choice* 176 (1): 17–32.

Cameron, A Colin, Jonah B Gelbach, and Douglas L Miller. 2008. "Bootstrap-based improvements for inference with clustered errors." *The Review of Economics and Statistics* 90 (3): 414–427.

Cameron, Charles, David Epstein, and Sharyn O'Halloran. 1996. "Do Majority-Minority Districts Maximize Substantive Black Representation in Congress?" *The American Political Science Review* 90 (4): 794–812.

Cascio, Elizabeth U, and Ebonya Washington. 2014. "Valuing the vote: The redistribution of voting rights and state funds following the voting rights act of 1965." *The Quarterly Journal of Economics* 129 (1): 379–433.

Chattopadhyay, Raghabendra, and Esther Duflo. 2004. "Women as policy makers: Evidence from a randomized policy experiment in India." *Econometrica* 72 (5): 1409–1443.

Chen, Jowei, and Jonathan Rodden. 2013. "Unintentional gerrymandering: Political geography and electoral bias in legislatures." *Quarterly Journal of Political Science* 8 (3): 239–269.

———. 2015. "Cutting through the thicket: Redistricting simulations and the detection of partisan gerrymanders." *Election Law Journal* 14 (4): 331–345.

Chishti, Muzaffar, and Charles Kamasaki. 2014. "IRCA in Retrospect: Guideposts for Immigration Reform." *Migration Policy Institute: Issue Brief No. 9.*

Coate, Stephen, and Brian Knight. 2007. "Socially optimal districting: a theoretical and empirical exploration." *The Quarterly Journal of Economics* 122 (4): 1409–1471.

Coriale, Kenneth, Ethan Kaplan, and Daniel Kolliner. 2020. *Political Control Over Redistricting and the Partisan Balance in Congress.* Technical report. Working Paper.[97].

Cox, Adam B, and Richard T Holden. 2011. "Reconsidering racial and partisan gerrymandering." *U. Chi. L. Rev.* 78:553.

Friedman, John N, and Richard T Holden. 2008. "Optimal gerrymandering: sometimes pack, but never crack." *American Economic Review* 98 (1): 113–44.

———. 2009. "The rising incumbent reelection rate: what's gerrymandering got to do with it?" *The Journal of Politics* 71 (2): 593–611.

Fryer Jr., Roland G, and Richard Holden. 2011. "Measuring the compactness of political districting plans." *The Journal of Law and Economics* 54 (3): 493–535.

Gelman, Andrew, and Gary King. 1990. "Estimating the electoral consequences of legislative redistricting." *Journal of the American statistical Association* 85 (410): 274–282.

Guinier, Lani. 1991. "The triumph of tokenism: the voting rights act and the theory of black electoral success." *Michigan Law Review* 89 (5): 1077–1154.

Gul, Faruk, and Wolfgang Pesendorfer. 2010. "Strategic redistricting." *American Economic Review* 100 (4): 1616–41.

Iyer, Lakshmi, Anandi Mani, Prachi Mishra, and Petia Topalova. 2012. "The power of political voice: women's political representation and crime in India." *American Economic Journal: Applied Economics* 4 (4): 165–93.

Jeong, Dahyeon, and Ajay Shenoy. 2022. "The Targeting and Impact of Partisan Gerrymandering: Evidence from a Legislative Discontinuity." *The Review of Economics and Statistics:* 1–47.

Kolotilin, Anton, and Alexander Wolitzky. 2020. "The Economics of Partisan Gerrymandering." *Working Paper.*

Kuziemko, Ilyana, and Ebonya Washington. 2018. "Why did the Democrats lose the South? Bringing new data to an old debate." *American Economic Review* 108 (10): 2830–67.

Levitt, Justin. 2008. "A citizen's guide to redistricting." *Available at SSRN 1647221.*

Lewis, Jeffrey B, Brandon DeVine, Lincoln Pitcher, and Kenneth C Martis. 2013. "Digital boundary definitions of united states congressional districts, 1789-2012." *Data file and code book.*

Naidu, Suresh. 2012. *Suffrage, schooling, and sorting in the post-bellum US South.* Technical report. National Bureau of Economic Research.

Pande, Rohini. 2003. "Can mandated political representation increase policy influence for disadvantaged minorities? Theory and evidence from India." *American economic review* 93 (4): 1132–1151.

Passel, Jeffrey S. 1986. "Estimating the number of undocumented aliens." *Monthly Labor Review* 109:33–34.

Rytina, Nancy. 2002. *IRCA Legalization Effects: Lawful Permanent Residence and Naturalization through 2001.* Technical report. Office of Policy and Planning, INS.

Sabet, Navid, and Christoph Winter. 2021. "Legal Status and Local Spending: The Distributional Consequences of the 1986 IRCA."

Sabouni, Hisam, and Cameron A. Shelton. 2022. "Gerrymandering in State Legislatures: Frictions from Axiomatic Bargaining." *American Economic Journal: Economic Policy* 14, no. 4 (November): 519–42.

Shotts, Kenneth W. 2003. "Does racial redistricting cause conservative policy outcomes? Policy preferences of southern representatives in the 1980s and 1990s." *The Journal of Politics* 65 (1): 216–226.

Stephanopoulos, Nicholas O, and Eric M McGhee. 2015. "Partisan gerrymandering and the efficiency gap." *U. Chi. L. Rev.* 82:831.

Washington, Ebonya. 2012. "Do majority-black districts limit blacks' representation? The case of the 1990 redistricting." *The Journal of Law and Economics* 55 (2): 251–274.

Figures


SALEM GAZETTE.

VOLUME XXVII.] SALEM, FRIDAY, APRIL 2, 1813.

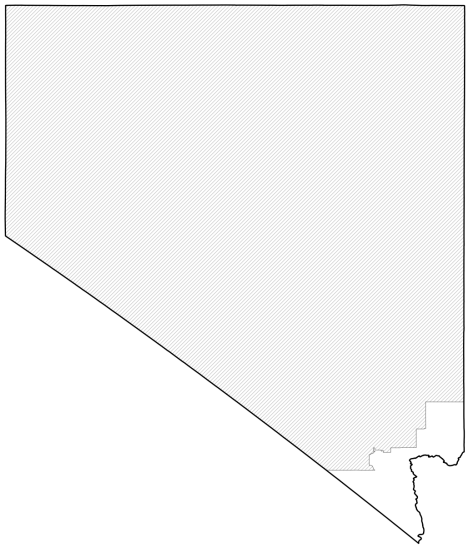
THE GERRY-MANDER,
OR
ESSEX SOUTH DISTRICT FORMED INTO A MONSTER!

requisition their utmoft e
means expreſsly to accomm
views and to promote th
ſhould ſtill be unwilling t
policy preſcribed by themſe
Such a man would have
lence and ſubordination m
to accompliſh what diſhonet
failed to ſecure. It is in fac
im of ſome, who, if the te
own profeſſions is to be cred
love for the people and a fa
their rights, to contemn all
which impede their ambitio
ever juſt and neceſſary to f
and order of ſociety; to e
whatever they are prohibited
and to aggrandize themſelve
to juſtice or decency. Upo
ciple can we account for t
activity of thoſe, who, at a
this town, ſcrupled not to br
not only alien reſidents, wh
never to have been naturaliz
certain clergyman (the learn
of Politics for this vicinity)
in common with the reſt of
qualified to vote in the choi
cers, and thus to involve hin
tude of their tranſgreſſions,
the cauſes of repentance, (un
lieves, "that the end wo
means," that there would be
tentie); but even to ſeduce ei
towns, to inveſt the minorit
which they would be ſure
who promiſes freedom to th
he increaſes the number and
chains, acts as conſiſtently,
declaims much and loud abo
of the people and the rights
when all his actions evince,
neither the one nor the oth
than he diſcovers a direct
his own intereſt. It is great
that *honeſt* men would fometi
ſelves to be influenced as wel
viſion, as *hearing*, and comp
profeſſions; and then let ju
upon the worth of character,
of repoſing confidence.

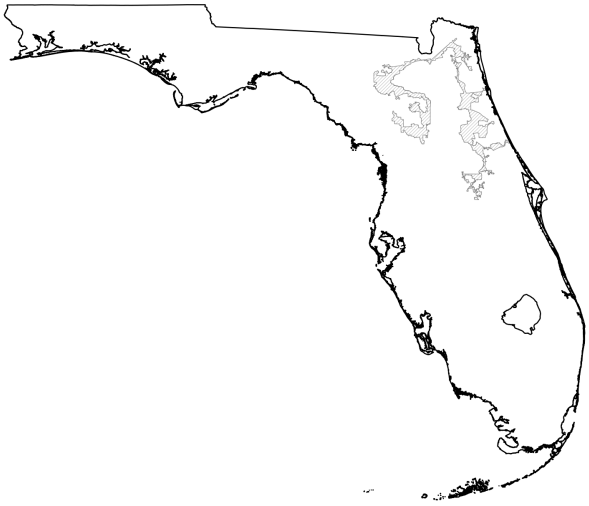
Mr. PRINTER,—I am a
Britain, and ſome years ago
at *Gottenburg*; but for ſevera

FEDERALISTS! FOLLOWERS OF WASHINGTON!

(a) The original gerrymander: South Essex district in 1812 Massachusetts



(b) Nevada's 2^{nd} District, 102^{nd} Congress



(c) Florida's 3^{rd} District, 103^{rd} Congress

Figure 1
Historical and Contemporary Gerrymandering

Notes: Panel (a): South Essex district, Massachusetts, in 1812. Panel (b): Nevada's 2^{nd} district, 102^{nd} Congress. Compactness scores: Polsby-Popper = 73; Schwartzberg = 85; Convex-Hull = 98; Reock = 59. Panel (c): Florida's 3^{rd} district, 103^{rd} Congress. Compactness scores: Polsby-Popper = 1; Schwartzberg = 10; Convex-Hull = 22; Reock = 12.

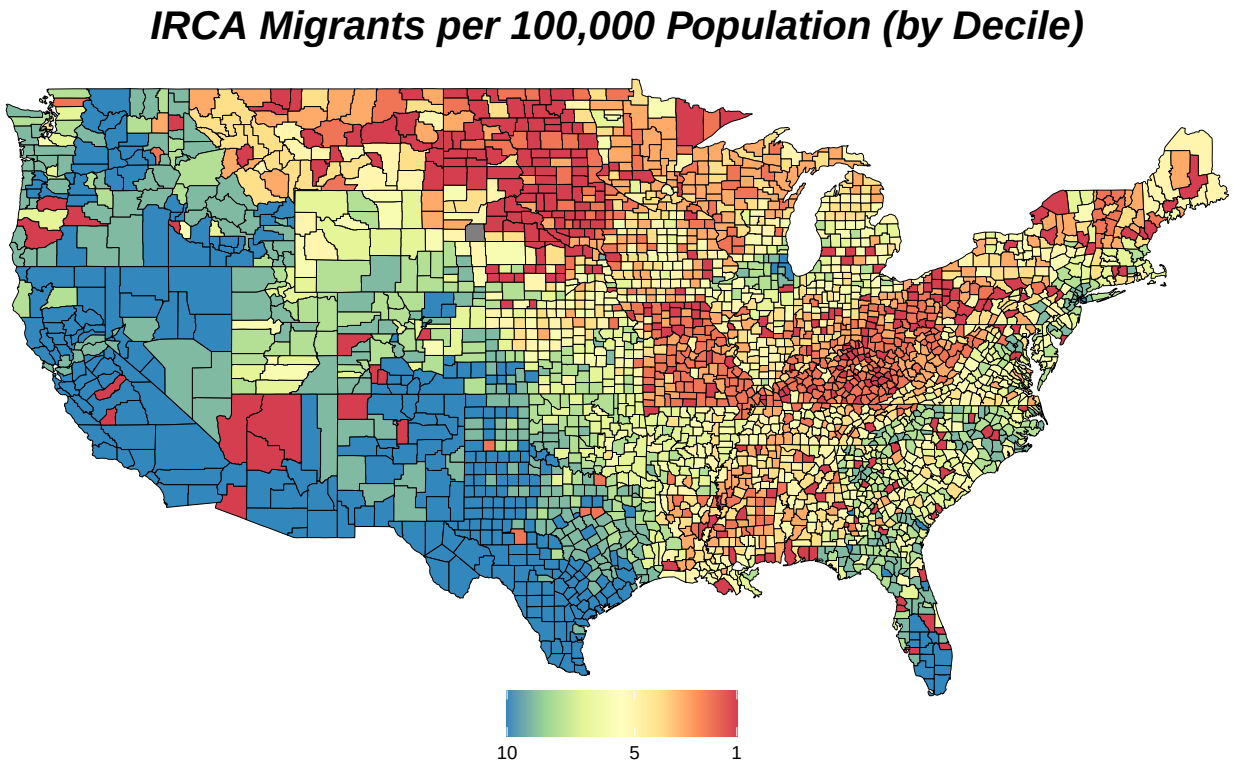


Figure 2
The Distribution of IRCA Immigrants Across the United States

Notes: This figure plots the geographical distribution, in deciles, of IRCA immigrants per 1,000 inhabitants across counties of the United States. Data come from the Immigration and Naturalization Service (INS) Legalization Summary Public Use Tape data (with imputations by the authors, as discussed in Section 3).

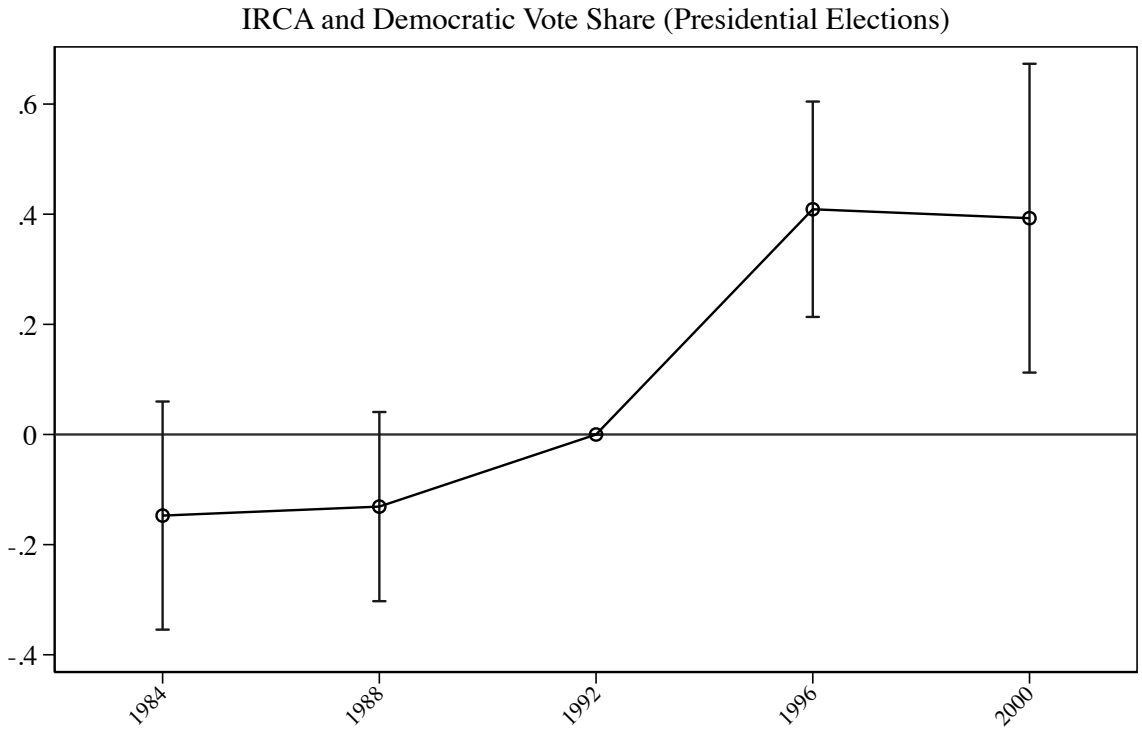


Figure 3
IRCA and Democratic Vote Share in Presidential Elections

Notes: This figure plots the time-varying effect of a one standard deviation increase in a county's IRCA share on the county's Democratic vote share in Presidential elections. The regression controls for county fixed effects, state×election fixed effects, the time-varying effect of the 1980 Hispanic share of the county population, and the time-varying effect of an indicator that a county has imputed IRCA data. Standard errors are clustered at the county level and 95% confidence intervals are shown.

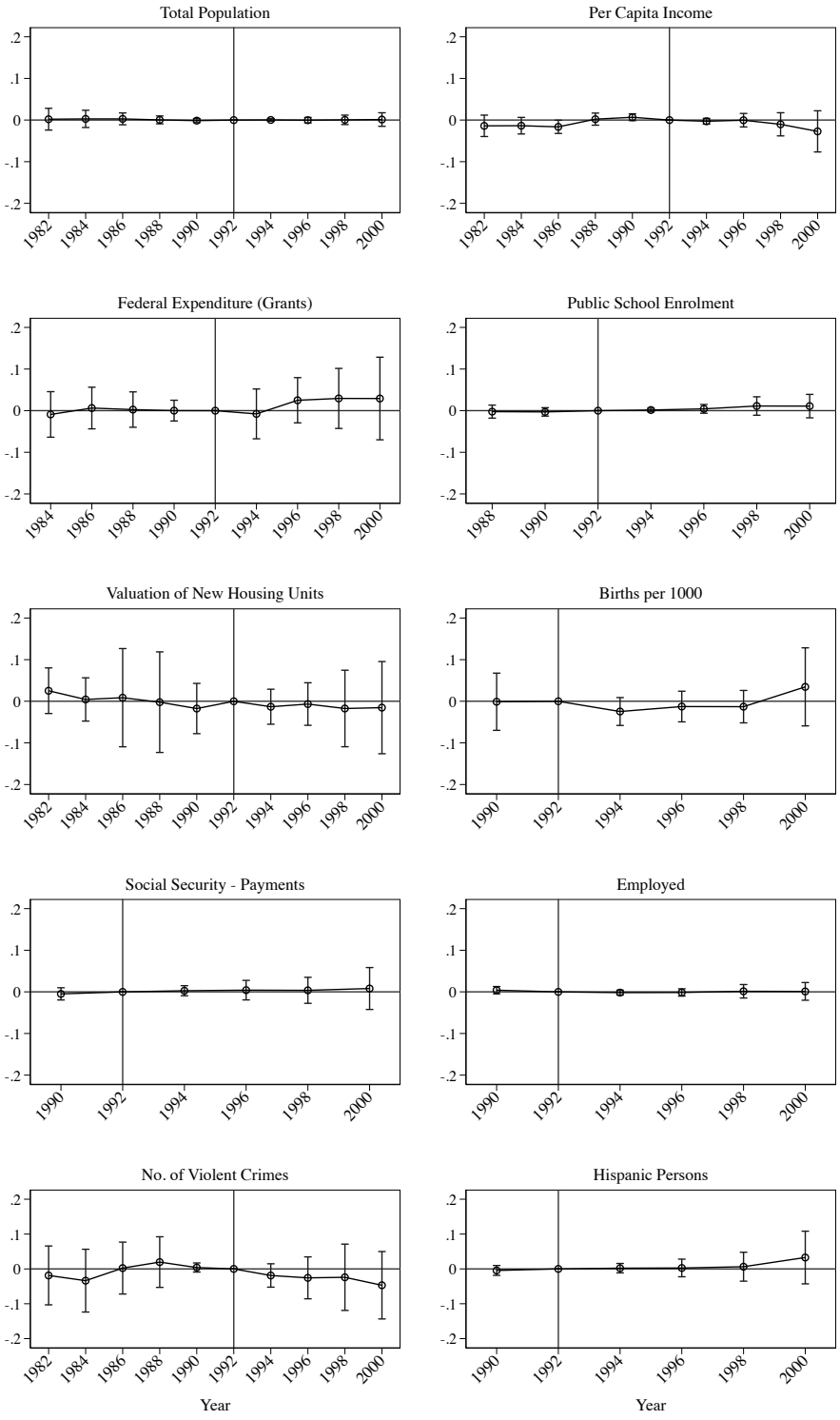


Figure 4
IRCA and County Covariates

Notes: This figure plots the time-varying effect of a one standard deviation increase in a county's IRCA share on the standardized outcomes indicated in the sub-figures. All regressions control for county fixed effects, state×year fixed effects, the time-varying effect of the 1980 Hispanic share of the county population, and the time-varying effect of an indicator that a county has imputed IRCA data. Standard errors are clustered at the county level and 95% confidence intervals are shown.

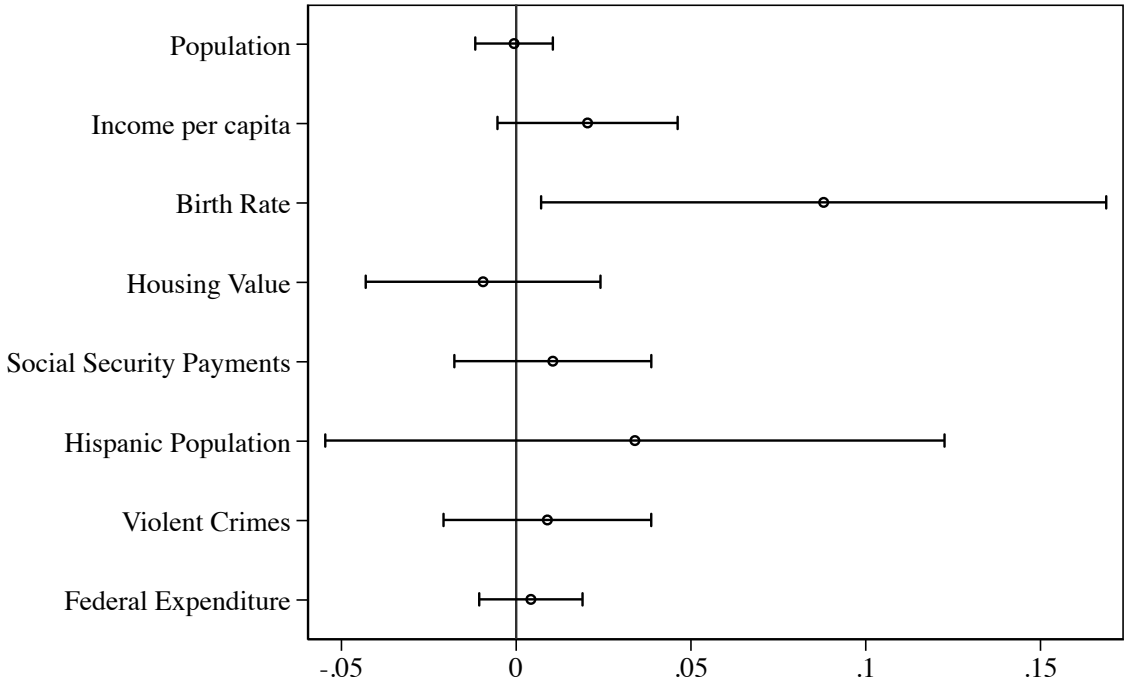


Figure 5
IRCA Share and Changes in County Covariates between 1980 and 1990

Notes: This figure plots the effect of a one standard deviation increase in a county's IRCA share on the county's change in the indicated standardized outcomes from 1980 to 1990 (for violent crimes, from 1982 to 1990; for federal expenditures, 1984–1990). Each outcome is considered in a separate regression that controls for county fixed effects, state×year fixed effects, the time-varying effect of the 1980 Hispanic share of the county population, and the time-varying effect of an indicator that a county has imputed IRCA data. Standard errors are clustered at the county level and 95% confidence intervals are shown.

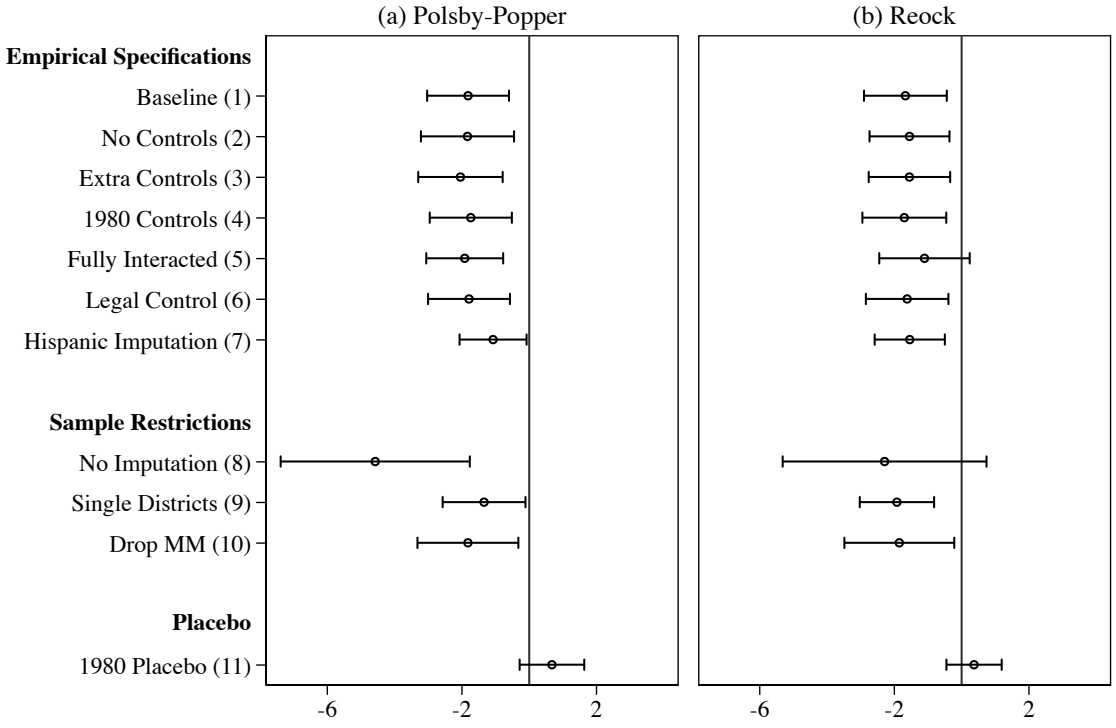
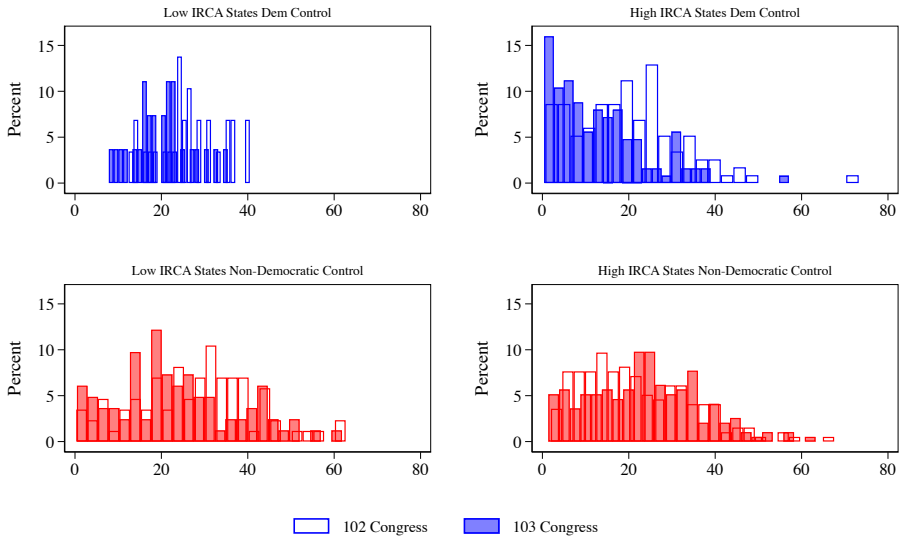
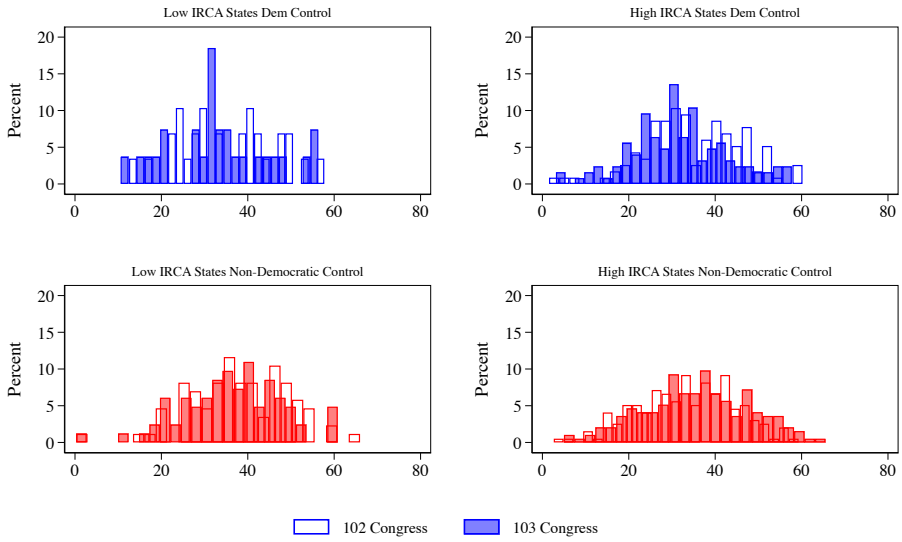


Figure 6
IRCA Share and District Compactness

Notes: This figure plots the coefficient on $\text{IRCA}_{90} \times \text{Post} \times \text{D}$ for different specifications, samples and placebo tests as explained in the text. IRCA_{90} is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990. Post is an indicator that is zero for the 102^{nd} congressional session and one for the 103^{rd} congressional session. For the placebo test it is zero for the 97^{th} congress and one for the 98^{th} congress. The outcome variable in both panels is the weighted sum of the compactness scores of the US congressional districts that overlap a county — Polsby-Popper in panel (a) and Reock in panel (b). All regressions control for county fixed effects, state×Congress fixed effects, the time-varying effect of the 1980 Hispanic share of the county population, and the time-varying effect of an indicator that a county has imputed IRCA data. They also control for the time-varying effect of whether a county was subject to the pre-clearance requirement of Section 5 of the VRA. All specifications include the triple interaction $IRCA_{c,1990} \times P_{102} \times R$ as well as lower order terms of the triple interactions for Democratic and Republican control, respectively. Standard errors are clustered at the county level and the regressions are all weighted by the number of districts that overlap with a county. Confidence intervals are drawn at 95%. Pre-redistricting mean [sd] dependent variable in baseline specification: 26.9 [12.9] for Polsby-Popper and 36.6 [10.3] for Reock. See Table B.1 for mean dependent variable of all other specifications.



(a) Distribution of Polsby-Popper Scores



(b) Distribution of Reock Scores

Figure 7
Changed Distributions of Spatial Compactness Scores depending on State IRCA Shares and Control of 1990 Census Redistricting

Notes: This figure plots the distributions of Polsby-Popper and Reock scores for every US district. We show distributions of district boundaries as defined during the 102^{nd} congress, immediately before the 1990 redistricting (shown as empty bars), and as defined during the 103^{rd} congress, immediately following redistricting (solid bars). We divide states depending on their IRCA share of the population (above/below median) and depending on the political control of the 1990 census redistricting (Democratic control/other).

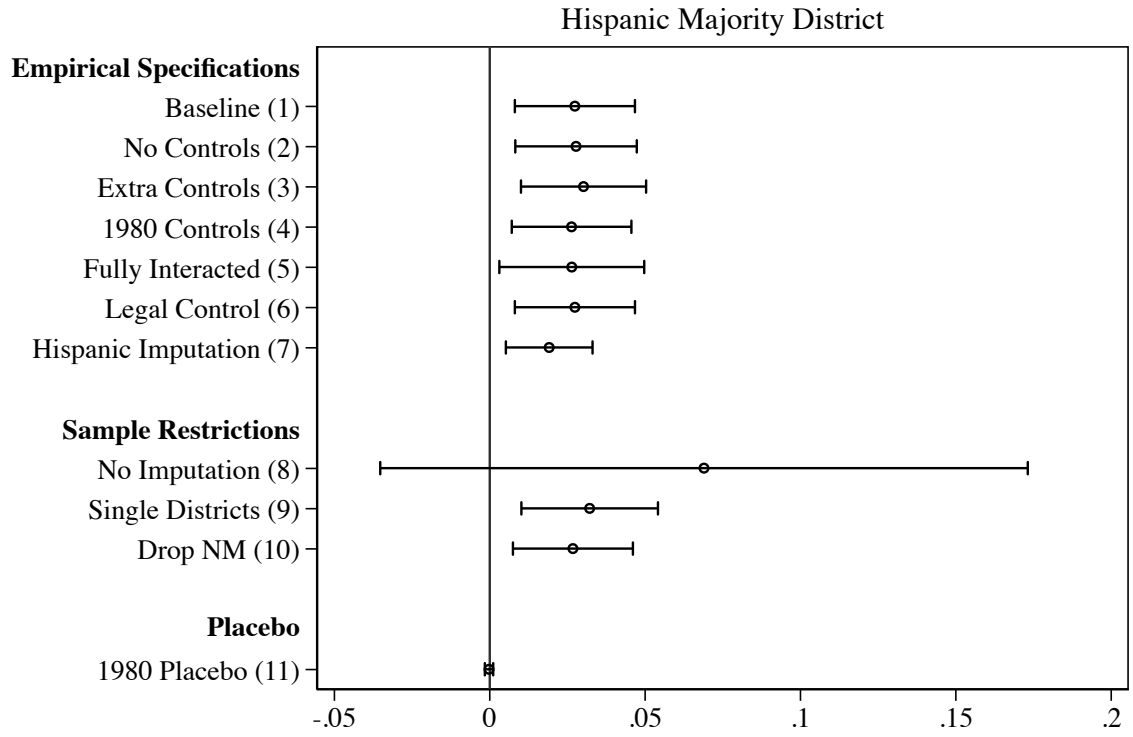


Figure 8
IRCA Share and Hispanic Majority Districts

Notes: This figure plots the coefficient on $\text{IRCA}_{90} \times \text{Post} \times \text{D}$ for different specifications, samples and placebo tests as explained in the text. IRCA_{90} is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990. Post is an indicator that is zero for the 102^{nd} congressional session and one for the 103^{rd} congressional session. For the placebo test it is zero for the 97^{th} congress and one for the 98^{th} congress. The outcome variable across all specifications is an indicator that is one if a county overlaps with, or is entirely within, a majority-Hispanic district and zero otherwise. All regressions control for county fixed effects, state×Congress fixed effects, the time-varying effect of the 1980 Hispanic share of the county population, and the time-varying effect of an indicator that a county has imputed IRCA data. They also control for the time-varying effect of whether a county was subject to the pre-clearance requirement of Section 5 of the VRA. All specifications include the triple interaction $IRCA_{c,1990} \times P_{102} \times R$ as well as lower order terms of the triple interactions for Democratic and Republican control, respectively. Standard errors are clustered at the county level and the regressions are all weighted by the number of districts that overlap with a county, except for the placebo test, which cannot be estimated with weights because of the sparsity of the outcome variable. Confidence intervals are drawn at 95%. Pre-redistricting mean [sd] dependent variable in baseline specification: .0126 [.111]. See Table B.2 for mean dependent variable of all other specifications.

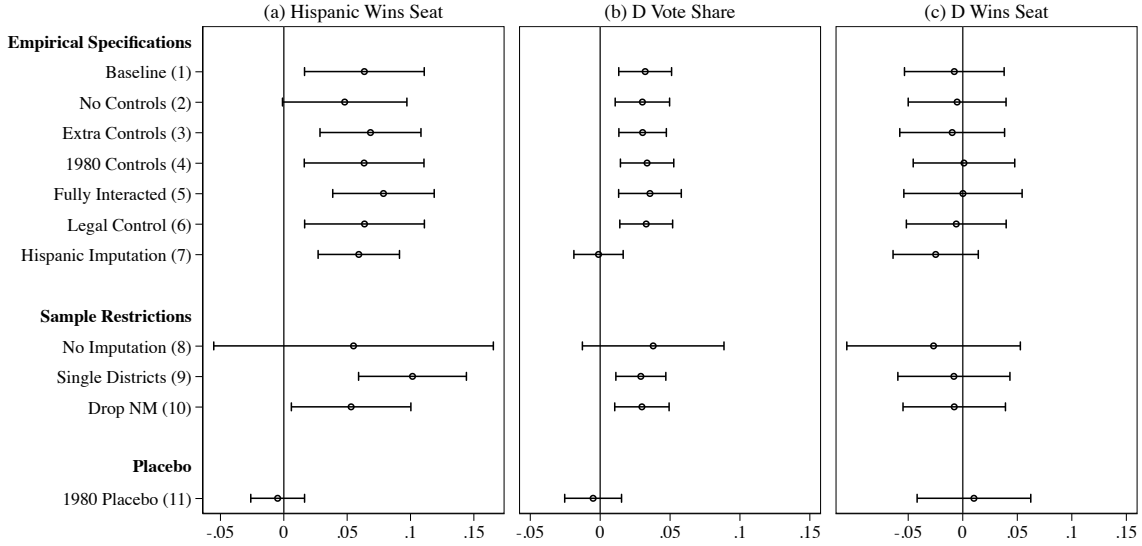


Figure 9
IRCA Share and District Political Outcomes

Notes: This figure plots the coefficient on $\text{IRCA}_{90} \times \text{Post} \times \text{D}$ for different specifications, samples and placebo tests as explained in the text. IRCA_{90} is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990. Post is zero for congressional elections held between the 98^{th} and 102^{nd} congressional sessions and one for congressional elections held between the 103^{rd} and 107^{th} congressional sessions. For the placebo test it is zero for congressional elections held between the 93^{rd} and 97^{th} congressional sessions and one for congressional elections held between the 98^{th} and 102^{nd} congressional sessions. The outcome variable in panel (a) is an indicator that is one if a county overlaps with, or is entirely within, a district won by a Hispanic representative and zero otherwise. The outcome in panel (b) is the Democratic vote share (in congressional elections) in the district with which a county overlaps, or is entirely within. The outcome in panel (c) is an indicator that is one if a county overlaps with, or is entirely within, a district won by a Democrat and zero otherwise. All regressions control for county fixed effects, state×Congress fixed effects, the time-varying effect of the 1980 Hispanic share of the county population, and the time-varying effect of an indicator that a county has imputed IRCA data. They also control for the time-varying effect of whether a county was subject to the pre-clearance requirement of Section 5 of the VRA. All specifications include the triple interaction $IRCA_{c,1990} \times P_{102} \times R$ as well as lower order terms of the triple interactions for Democratic and Republican control, respectively. Standard errors are clustered at the county level and the regressions are all weighted by the number of districts that overlap with a county. Confidence intervals are drawn at 95%. Pre-redistricting mean [sd] dependent variable in baseline specification: .0123 [.110] for panel (a); .536 [.260] for panel (b); and .579 [.494] for panel (c). See Table B.3 for mean dependent variable of all other specifications.

8. Tables

Table 1
IRCA Shares, Legal Control of Redistricting, and District Compactness

	Polsby-Popper		Schwartzberg		Reock		Convex-Hull	
	(1) Baseline Effect	(2) Party Effect	(3) Baseline Effect	(4) Party Effect	(5) Baseline Effect	(6) Party Effect	(7) Baseline Effect	(8) Party Effect
$IRCA_{90} \times P_{102}$	0.0589	0.227	-0.0451	0.141	-0.154	0.000584	-0.0355	0.151
	(0.373)	(0.340)	(0.439)	(0.401)	(0.377)	(0.398)	(0.461)	(0.446)
$IRCA_{90} \times P_{102} \times D$		-1.817***		-2.019***		-1.672***		-2.020***
		(0.620)		(0.771)		(0.628)		(0.775)
N	5,734	5,734	5,734	5,734	5,734	5,734	5,734	5,734
Clusters	2,867	2,867	2,867	2,867	2,867	2,867	2,867	2,867
$\bar{Y}_{102}$	26.9		49.7		36.6		73.1	
$[S.D]$	[12.9]		[14.3]		[10.3]		[11.4]	

Notes: $IRCA_{90}$ is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990. P_{102} is an indicator that is zero for the 102^{nd} congressional session and one for the 103^{rd} congressional session. "D" indicates that legal control of the 1990 redistricting process was held by the Democratic party; the base category includes states where neither party held legal control of redistricting; we do not report coefficients for the two states where Republicans held control of the process (as explained in the text), but we do estimate them. All regressions control for county fixed effects, state×Congress fixed effects, the time-varying effect of the 1980 Hispanic share of the county population, and the time-varying effect of an indicator that a county has imputed IRCA data. They also control for the time-varying effect of whether a county was subject to the pre-clearance requirement of Section 5 of the VRA. Furthermore, all specifications control for the time-varying effects of 1990 measures of county population, per capita income, share of the population over 18, unemployment rate, the percent with a Bachelor's degree or more, the birth rate, the share of the population that is white, and the share of the population that is black. The regressions in columns 2, 4, 6 and 8 include all lower order terms of the triple interactions. Standard errors (in parentheses) are clustered at the county level and the regressions are weighted by the number of districts that overlap with a county. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2
State Variation in the IRCA Share and District Compactness

	(1) Polsby-Popper	(2) Schwartzberg	(3) Reock	(4) Convex-Hull
State $IRCA_{90} \times P_{102}$	0.212	2.129	-0.618	2.437
	(2.108)	(2.256)	(1.471)	(3.179)
State $IRCA_{90} \times P_{102} \times D$	-3.588*	-5.449**	-3.676***	-3.532*
	(1.826)	(2.039)	(1.088)	(1.905)
N	855	855	855	855
Clusters	43	43	43	43
$\bar{Y}_{102}$	22.8	45.5	34.9	69.4
$[S.D.]$	[12.9]	[14.5]	[11.4]	[13.4]
Bootstrap p-values:				
$\beta_{IRCA_{90} \times P_{102}}$	.31	.328	.36	.395
$\beta_{IRCA_{90} \times P_{102} \times D}$	.227	.118	.027	.206

Notes: The outcome variable is the compactness score of a US congressional district. State $IRCA_{90}$ is the standardized number of IRCA immigrants per 1,000 state inhabitants measured in 1990. P_{102} is an indicator that is zero for the 102^{nd} congressional session and one for the 103^{rd} congressional session. "D" indicates that legal control of the 1990 redistricting process was held by the Democratic party; the base category includes states where neither party held legal control of redistricting; we do not report coefficients for the two states where Republicans held control of the process (as explained in the text), but we do estimate them. All regressions control for state fixed effects, congressional-session fixed effects, and the time-varying effect of the 1980 Hispanic share of the state population. They also control for the time-varying effect of whether a state (or part of a state) was subject to the pre-clearance requirement of Section 5 of the VRA. Furthermore, all specifications control for the time-varying effects of 1990 measures of state population, per capita income, share of the population over 18, unemployment rate, the percent with a Bachelor's degree or more, the birth rate, the share of the population that is white, and the share of the population that is black. All specifications include all lower order terms of the triple interactions. Regressions are weighted by the 1990 state population. Standard errors (in parentheses) are clustered at the state level and the reported bootstrap p-values are calculated using wild cluster bootstrap as described in Cameron, Gelbach, and Miller (2008). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3
IRCA Shares, Legal Control of Redistricting, and State Political Outcomes

	Descriptive Representation	Substantive Representation		Efficiency
	(1) Hispanics in Delegation	(2) Median DW-Nominate	(3) Democrats in Delegation	(4) E. Gap
State $IRCA_{90} \times P_{102}$	-0.123	0.0865	1.050*	0.0728
	(0.181)	(0.0952)	(0.531)	(0.0638)
State $IRCA_{90} \times P_{102} \times D$	0.398**	-0.120**	-0.481	-0.0560*
	(0.185)	(0.0505)	(0.329)	(0.0306)
N	430	430	430	430
Clusters	43	43	43	43
$\bar{Y}_{102}$	.23	-.056	6	.023
$[S.D.]$	[.76]	[.23]	[5.7]	[.12]
Bootstrap p-values:				
$\beta_{IRCA_{90} \times P_{102}}$	.53	.94	.18	.46
$\beta_{IRCA_{90} \times P_{102} \times D}$	.26	.091	.29	.21

Notes: The outcome in column 1 is the count of Hispanics in the state's House delegation. The outcome in column 2 is the DW-Nominate score of the median member of the state House delegation. The outcome in column 3 is the count of Democrats in the state's House delegation. The outcome in column 4 is a measure of wasted votes: the "efficiency gap," following Stephanopoulos and McGhee (2015). State $IRCA_{90}$ is the standardized number of IRCA immigrants per 1,000 state inhabitants measured in 1990. P_{102} is an indicator that is zero for congressional elections held between the 98^{th} and 102^{nd} congressional sessions and one for congressional elections held between the 103^{rd} and 107^{th} congressional sessions. "D" indicates that legal control of the 1990 redistricting process was held by the Democratic party; the base category includes states where neither party held legal control of redistricting; we do not report coefficients for the two states where Republicans held control of the process (as explained in the text), but we do estimate them. All regressions control for state fixed effects, congressional-session fixed effects, and the time-varying effect of the 1980 Hispanic share of the county population. They also control for the time-varying effect of whether a state (or part of a state) was subject to the pre-clearance requirement of Section 5 of the VRA. Furthermore, all specifications control for the time-varying effects of 1990 measures of state population, per capita income, share of the population over 18, unemployment rate, the percent with a Bachelor's degree or more, the birth rate, the share of the population that is white, and the share of the population that is black. All specifications include all lower order terms of the triple interactions. Regressions are weighted by the 1990 state population. Standard errors (in parentheses) are clustered at the state level and the reported bootstrap p-values are calculated using wild cluster bootstrap as described in Cameron, Gelbach, and Miller (2008). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Online Appendix for Paper:
Identifying Partisan Gerrymandering and its Consequences

A. List of States by Legal Control of Redistricting

In our paper, we exploit variation in the legalization of immigrants under the IRCA as well as in the political control over the 1990 census redistricting to identify partisan gerrymandering. We find that in states where Democrats controlled the redistricting process, a higher IRCA share of a county's population is associated with more spatially distorted district boundaries, changed district demographic composition, and changed political outcomes. Importantly, we find these patterns only where Democrats controlled the redistricting process: where control over redistricting was in the hands of non-partisan commissions or when neither party exercised control (i.e., due to divided government), we find no such distortion. We obtain information on which party held legal control of redistricting from Coriale, Kaplan, and Kolliner (2020) and in Table A.1 we provide a list of states by legal control of redistricting in the 1990 census redistricting cycle. In several robustness checks, we use an alternative definition of legal control of redistricting which we obtained from the American Redistricting Project (ARP) and which we also list in Table A.1.[46]

46. See this website for the definitions of legal control of redistricting from the ARP: https://thearp.org/blog/party-control/modern-congressional-history/. Accessed 25 April 2023.

Table A.1
List of States According to Political Control of Redistricting in 1990

Coriale et al. (2020)	No Control	Alabama, Arizona, California, Colorado, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Louisiana, Maine, Massachusetts, Michigan, Minnesota, Missouri, Nebraska, New York, Ohio, Oregon, Pennsylvania, South Carolina, Washington, Wisconsin
	Republican Control	New Hampshire, Utah
	Democrat Control	Arkansas, Connecticut, Florida, Georgia, Kentucky, Maryland, Mississippi, Nevada, New Jersey, New Mexico, North Carolina, Oklahoma, Rhode Island, Tennessee, Texas, Virginia, West Virginia
	Single District States	Alaska, Delaware, Montana, North Dakota, South Dakota, Vermont, Wyoming
ARP	No Control	Arizona, California, Colorado, Connecticut, Hawaii, Idaho, Illinois, Iowa, Kansas, Maine, Massachusetts, Michigan, Minnesota, Missouri, Nebraska, New York, Ohio, Oregon, Pennsylvania, South Carolina, Washington, Wisconsin
	Republican Control	New Hampshire, Utah
	Democrat Control	Alabama, Arkansas, Florida, Georgia, Indiana, Kentucky, Louisiana, Maryland, Mississippi, Nevada, New Jersey, New Mexico, North Carolina, Oklahoma, Rhode Island, Tennessee, Texas, Virginia, West Virginia
	Single District States	Alaska, Delaware, Montana, North Dakota, South Dakota, Vermont, Wyoming

B. Robustness Results in Table Form

In this Online Appendix, we present the results of our robustness exercises in table form. Table B.1 presents robustness when examining spatial distorition of district boundaries. In Table B.2 we present our full results when examining Hispanic-majority districts and in Table B.3, we present our findings when examining three different measures of county-level political outcomes.

Table B.1
Changes in District Compactness - Robustness Exercises

	Empirical Specifications							Sample Restrictions			Placebo
	(1) Baseline Model	(2) No Covariates	(3) Extra Covariates	(4) 1980 Covariates	(5) Interacted Observables	(6) Alternative Legal Control	(7) Hispanic Impute	(8) Drop Imputed	(9) Single-District Counties	(10) Drop MM Districts	(11) 1980 Placebo
Panel A. Polsby-Popper											
$IRCA_{90} \times Post$	0.227	0.520	0.119	0.232	0.417	0.224	1.616**	0.368	-0.00235	0.440	-0.447
	(0.340)	(0.469)	(0.333)	(0.342)	(0.344)	(0.340)	(0.697)	(0.681)	(0.445)	(0.366)	(0.476)
$IRCA_{90} \times Post \times D$	-1.817***	-1.835***	-2.044***	-1.736***	-1.917***	-1.790***	-1.075**	-4.577***	-1.342**	-1.822**	0.676
	(0.620)	(0.706)	(0.640)	(0.623)	(0.583)	(0.620)	(0.508)	(1.429)	(0.628)	(0.765)	(0.490)
$\bar{Y}_{Pre}$				26.87				23.05	27.94	28.23	27.05
$[S.D.]$				[12.89]				[12.84]	[13.11]	[12.73]	[13.36]
Panel B. Reock											
$IRCA_{90} \times Post$	0.000584	0.177	-0.100	-0.0495	0.0144	-0.00424	0.775	0.0599	0.726*	0.244	0.559
	(0.398)	(0.396)	(0.384)	(0.392)	(0.405)	(0.396)	(0.668)	(0.813)	(0.390)	(0.430)	(0.385)
$IRCA_{90} \times Post \times D$	-1.672***	-1.549**	-1.551**	-1.703***	-1.104	-1.619***	-1.542***	-2.289	-1.923***	-1.851**	0.369
	(0.628)	(0.606)	(0.618)	(0.636)	(0.686)	(0.626)	(0.532)	(1.541)	(0.563)	(0.833)	(0.419)
$\bar{Y}_{Pre}$				36.62				34.87	36.90	37.01	38.51
$[S.D.]$				[10.25]				[10.58]	[10.35]	[10.37]	10.12
N	5,734	5,734	5,734	5,734	5,734	5,734	5,734	728	4,620	4,712	5,874
Clusters	2,867	2,867	2,867	2,867	2,867	2,867	2,867	364	2,310	2,356	2,937

Notes: $IRCA_{90}$ is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990. In columns 1 to 10, Post is an indicator that is zero for the 102^{nd} congressional session and one for the 103^{rd} congressional session; in column 11, Post is zero for the 97^{th} congress and one for the 98^{th} congress. "D" indicates that legal control of the 1990 redistricting process was held by the Democratic party; the base category includes states where neither party held legal control of redistricting; we do not report coefficients for the two states where Republicans held control of the process (as explained in the text), but we do estimate them; in column 11 this variable is re-coded to measure party control of redistricting in 1980. All regressions control for county fixed effects, state×Congress fixed effects, the time-varying effect of the 1980 Hispanic share of the county population, and the time-varying effect of an indicator that a county has imputed IRCA data. They also control for the time-varying effect of whether a county was subject to the pre-clearance requirement of Section 5 of the VRA. Specifications in columns 1, 3, 6, 7, 8, 9 and 10 control for the time-varying effects of baseline county covariates measured in 1990, as in Table 1. Additional controls in column 3 include the time-varying effects of 1990 measures of county poverty, the share of households with children, the number of social security recipients, the amount of social security payments, housing values, and violent crime. Column 4 controls for the time-varying effects of the baseline covariates considered in Table 1, but measured in *1980*. The model in column 5 includes all baseline controls interacted with *Post* and the variable that indicates which party held legal control of redistricting; it also includes the interaction of whether a county is covered by Section 5 of the VRA with *Post* and legal control of redistricting. In column 6, we use an alternative definition of which party held legal control of redistricting. In column 7, we proxy the IRCA immigrant share in counties with no IRCA information using the Hispanic share of the population. In column 8 we restrict the sample to only those counties for which we observe IRCA immigrant information. In column 9 we restrict the sample to single-district counties only and in column 10 we drop counties in Hispanic-majority districts or in Black-majority districts. All specifications include the triple interaction $IRCA_{c,1990} \times P_{102} \times R$ as well as lower order terms of the triple interactions for Democratic and Republican control, respectively. Standard errors (in parentheses) are clustered at the county level and the regressions are weighted by the number of districts that overlap with a county. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.2
IRCA Shares, Legal Control of Redistricting, and Majority-Hispanic Districts

	Empirical Specifications							Sample Restrictions			Placebo
	(1) Baseline Model	(2) No Covariates	(3) Extra Covariates	(4) 1980 Covariates	(5) Interacted Observables	(6) Alternative Legal Control	(7) Hispanic Impute	(8) Drop Imputed	(9) Single-District Counties	(10) Drop New Mexico	(11) 1980 Placebo
$IRCA_{90} \times Post$	-0.00417	-0.00497	-0.00130	-0.00604	-0.00557	-0.00417	-0.0288	0.00144	0.000845	-0.00496	-0.000388
	(0.00439)	(0.00364)	(0.00209)	(0.00421)	(0.00454)	(0.00439)	(0.0253)	(0.00900)	(0.00208)	(0.00479)	(0.00103)
$IRCA_{90} \times Post \times D$	0.0274***	0.0278***	0.0302***	0.0263***	0.0264**	0.0274***	0.0191***	0.0690	0.0322***	0.0268***	-0.000239
	(0.00985)	(0.00997)	(0.0103)	(0.00982)	(0.0119)	(0.00986)	(0.00711)	(0.0530)	(0.0112)	(0.00984)	(0.000687)
N	5,734	5,734	5,734	5,734	5,734	5,734	5,734	728	4,620	5,670	5,908
Clusters	2,867	2,867	2,867	2,867	2,867	2,867	2,867	364	2,310	2,835	2,954
$\bar{Y}_{102}$				.0126				.0165	.0134	.0127	.003
$[S.D.]$				[.111]				[.128]	[.115]	[.112]	.052

Notes: The outcome variable across all specifications is an indicator that is one if a county overlaps with, or is entirely within, a majority-Hispanic district and zero otherwise. $IRCA_{90}$ is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990. In columns 1 to 10, Post is an indicator that is zero for the 102^{nd} congressional session and one for the 103^{rd} congressional session; in column 11, Post is zero for the 97^{th} congress and one for the 98^{th} congress. "D" indicates that legal control of the 1990 redistricting process was held by the Democratic party; the base category includes states where neither party held legal control of redistricting; we do not report coefficients for the two states where Republicans held control of the process (as explained in the text), but we do estimate them.; in column 11 this variable is re-coded to measure party control of redistricting in 1980. All regressions control for county fixed effects, state×Congress fixed effects, the time-varying effect of the 1980 Hispanic share of the county population, and the time-varying effect of an indicator that a county has imputed IRCA data. They also control for the time-varying effect of whether a county was subject to the pre-clearance requirement of Section 5 of the VRA. See table notes of Table B.1 for further details concerning each specification. In column 10 we drop New Mexico instead of Hispanic- and Black-majority districts as explained in the text. All specifications include the triple interaction $IRCA_{c,1990} \times P_{102} \times R$ as well as lower order terms of the triple interactions for Democratic and Republican control, respectively. Standard errors (in parentheses) are clustered at the county level and the regressions are all weighted by the number of districts that overlap with a county, except for column 11 which cannot be estimated with weights because of the sparsity of the outcome variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.3
IRCA Shares, Legal Control of Redistricting, and County Political Outcomes

	Empirical Specifications							Sample Restrictions			Placebo
	(1) Baseline Model	(2) No Covariates	(3) Extra Covariates	(4) 1980 Covariates	(5) Interacted Observables	(6) Alternative Legal Control	(7) Hispanic Impute	(8) Drop Imputed	(9) Single-District Counties	(10) Drop New Mexico	(11) 1980 Placebo
Panel A. Descriptive Representation: Hispanic Wins Seat											
$IRCA_{90} \times Post$	-0.0113	0.00259	-0.0161*	-0.0154	-0.00703	-0.0113	-0.0512*	-0.0272	-0.00221	-0.0188*	-0.00524
	(0.0100)	(0.0141)	(0.00951)	(0.0102)	(0.00979)	(0.0100)	(0.0291)	(0.0270)	(0.00672)	(0.0102)	(0.0116)
$IRCA_{90} \times Post \times D$	0.0636***	0.0480*	0.0684***	0.0634***	0.0787***	0.0637***	0.0592***	0.0550	0.102***	0.0531**	-0.00485
	(0.0241)	(0.0250)	(0.0203)	(0.0241)	(0.0204)	(0.0241)	(0.0164)	(0.0561)	(0.0217)	(0.0241)	(0.0108)
$\bar{Y}_{Pre}$				0.01				0.03	0.01	0.01	0.01
$[S.D.]$				[0.11]				[0.16]	[0.11]	[0.08]	[0.09]
Panel B. Substantive Representation: Democratic Vote Share											
$IRCA_{90} \times Post$	0.0179***	0.0215***	0.0186***	0.0192***	0.0138**	0.0178***	-0.00135	0.0170	0.0174***	0.0166***	-0.0156
	(0.00582)	(0.00588)	(0.00536)	(0.00581)	(0.00615)	(0.00582)	(0.0114)	(0.0136)	(0.00560)	(0.00596)	(0.00953)
$IRCA_{90} \times Post \times D$	0.0322***	0.0302***	0.0304***	0.0336***	0.0356***	0.0329***	-0.00118	0.0379	0.0291***	0.0299***	-0.00503
	(0.00963)	(0.00992)	(0.00866)	(0.00974)	(0.0114)	(0.00964)	(0.00899)	(0.0258)	(0.00911)	(0.00992)	(0.0104)
$\bar{Y}_{Pre}$				0.54				0.50	0.54	0.54	0.53
$[S.D.]$				[0.26]				[0.22]	[0.27]	[0.26]	[0.27]
Panel C. Substantive Representation: Democrat Wins Seat											
$IRCA_{90} \times Post$	0.0283***	0.0303***	0.0310***	0.0325***	0.0177	0.0281***	-0.0258	0.0105	0.0408***	0.0279**	-0.0341
	(0.0109)	(0.0103)	(0.0114)	(0.0110)	(0.0111)	(0.0109)	(0.0220)	(0.0271)	(0.0156)	(0.0112)	(0.0256)
$IRCA_{90} \times Post \times D$	-0.00773	-0.00515	-0.00968	0.00112	0.000207	-0.00595	-0.0248	-0.0268	-0.00811	-0.00781	0.0103
	(0.0233)	(0.0229)	(0.0245)	(0.0237)	(0.0276)	(0.0233)	(0.0199)	(0.0405)	(0.0262)	(0.0240)	(0.0266)
$\bar{Y}_{Pre}$				0.58				0.67	0.55	0.58	0.56
$[S.D.]$				[0.49]				[0.47]	[0.50]	[0.49]	[0.50]
N	28,844	28,844	28,844	28,844	28,844	28,844	28,844	3,693	25,277	28,524	20,553
Clusters	2,897	2,897	2,897	2,897	2,897	2,897	2,897	373	2,707	2,865	2,951

Notes: The outcome variable in Panel A is an indicator that is one if a county overlaps with, or is entirely within, a district won by a Hispanic representative and zero otherwise. The outcome in Panel B is the Democratic vote share (in congressional elections) in the district with which a county overlaps, or is entirely within. The outcome in Panel C is an indicator that is one if a county overlaps with, or is entirely within, a district won by a Democrat and zero otherwise. $IRCA_{90}$ is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990. In columns 1 to 10, Post is zero for congressional elections held between the 98^{th} and 102^{nd} congressional sessions and one for congressional elections held between the 103^{rd} and 107^{th} congressional sessions. In column 11 it is zero for congressional elections held between the 93^{rd} and 97^{th} congressional sessions and one for congressional elections held between the 98^{th} and 102^{nd} congressional sessions. "D" indicates that legal control of the 1990 redistricting process was held by the Democratic party; the base category includes states where neither party held legal control of redistricting; we do not report coefficients for the two states where Republicans held control of the process (as explained in the text), but we do estimate them; in column 11 this variable is re-coded to measure party control of redistricting in 1980. All regressions control for county fixed effects, state×Congress fixed effects, the time-varying effect of the 1980 Hispanic share of the county population, and the time-varying effect of an indicator that a county has imputed IRCA data. They also control for the time-varying effect of whether a county was subject to the pre-clearance requirement of Section 5 of the VRA. See table notes of Table B.1 for further details concerning each specification. In column 10 we drop New Mexico instead of Hispanic- and Black-majority districts as explained in the text. All specifications include the triple interaction $IRCA_{c,1990} \times P_{102} \times R$ as well as lower order terms of the triple interactions for Democratic and Republican control, respectively. Standard errors (in parentheses) are clustered at the county level and the regressions are all weighted by the number of districts that overlap with a county. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C. Hispanic VAP Majority Districts

Given Hispanics' low citizenship and turnout rates, one concern may be that our measurement of majority-Hispanic districts is incorrectly measured because a majority Hispanic population may not be relevant for Hispanic-preferred candidates. In this Online Appendix, rather than analyzing majority-Hispanic-population districts, we examine whether IRCA counties increased the liklihood of creating Hispanic majorities of among the voting age population (VAP). The outcome in Table C.1 is therefore 1 if a county overlaps with, or is entirely within, a district that is majority-Hispanic of Voting Age Population (VAP) and zero otherwise. As shown, the results are very similar to what is reported in the main body of the paper.

Table C.1
IRCA Shares, Legal Control of Redistricting, and Majority-Hispanic VAP Districts

	Empirical Specifications							Sample Restrictions			Placebo
	(1) Baseline Model	(2) No Covariates	(3) Extra Covariates	(4) 1980 Covariates	(5) Interacted Observables	(6) Alternative Legal Control	(7) Hispanic Impute	(8) Drop Imputed	(9) Single-District Counties	(10) Drop New Mexico	(11) 1980 Placebo
$IRCA_{90} \times Post$	0.000280	-0.00171	-0.000248	-0.00179	-0.000764	0.000279	-0.00149	0.00624	0.000845	-0.000111	-0.000388
	(0.00157)	(0.00190)	(0.00149)	(0.00160)	(0.00174)	(0.00157)	(0.00475)	(0.00515)	(0.00208)	(0.00176)	(0.00103)
$IRCA_{90} \times Post \times D$	0.0243***	0.0260***	0.0246***	0.0238***	0.0310***	0.0243***	0.0191***	0.0106	0.0322***	0.0242***	-0.000239
	(0.00867)	(0.00907)	(0.00833)	(0.00885)	(0.0102)	(0.00867)	(0.00689)	(0.0170)	(0.0112)	(0.00875)	(0.000687)
N	5,734	5,734	5,734	5,734	5,734	5,734	5,734	728	4,620	5,670	5,908
Clusters	2,867	2,867	2,867	2,867	2,867	2,867	2,867	364	2,310	2,835	2,954
$\bar{Y}_{102}$				.0126				.0165	.0134	.0131	.00271
$[S.D.]$				[.111]				[.128]	[.115]	[.114]	[.052]

Notes: The outcome variable across all specifications is an indicator that is one if a county overlaps with, or is entirely within, a district that is majority-Hispanic of Voting Age Population (VAP) and zero otherwise. $IRCA_{90}$ is the standardized number of IRCA immigrants per 1,000 county inhabitants measured in 1990. In columns 1 to 9, Post is an indicator that is zero for the 102^{nd} congressional session and one for the 103^{rd} congressional session; in column 10, Post is zero for the 97^{th} congress and one for the 98^{th} congress. "D" indicates that legal control of the 1990 redistricting process was held by the Democratic party (the base category includes states where neither party held legal control of redistricting and we do not report the results for the two states where Republicans held control of the process as explained in the text); in column 10 this variable is re-coded to measure party control of redistricting in 1980. All regressions control for county fixed effects, state×Congress fixed effects, the time-varying effect of the 1980 Hispanic share of the county population, and the time-varying effect of an indicator that a county has imputed IRCA data. They also control for the time-varying effect of whether a county was subject to the pre-clearance requirement of Section 5 of the VRA. See table notes of Table B.1 for further details concerning each specification. In column 10 we drop New Mexico instead of Hispanic- and Black-majority districts as explained in the text. All specifications include lower order terms of the triple interactions. Standard errors (in parentheses) are clustered at the county level and the regressions are all weighted by the number of districts that overlap with a county, except for column 11 which cannot be estimated with weights because of the sparsity of the outcome variable. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$