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DISTRIBUTING THE GREEN (CARDS):
PERMANENT RESIDENCY AND THE INCOME TAX AFTER THE
IMMIGRATION REFORM AND CONTROL ACT OF 1986

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ABSTRACT

We explore how permanent residency affects the income tax using variation from the Immigration Reform and Control Act of 1986 (IRCA), which authorized the largest U.S. amnesty to date. We exploit the timing and geographic unevenness of IRCA's legalization programs alongside newly digitized data on the income tax in California, home to the majority of applicants. Green Cards induced the previously unauthorized to file state income tax returns at rates comparable to other California residents. While the new returns generated little additional revenue through the end of the 1990s, they did raise the earnings of families with children through new claims of the federal Earned Income Tax Credit.

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1. Introduction

An estimated 11.1 million unauthorized immigrants, many from Mexico or Central America, live in the U.S. today (Pew Research Center, 2016). As ranks of the unauthorized have swelled, policymakers have implemented only temporary fixes, such as Deferred Action for Childhood Arrivals (DACA). In recent years, many legal immigrants from Central America and the Caribbean have also been eligible only for Temporary Protected Status (TPS).¹ Some argue that offering such immigrants a more permanent place would set a dangerous precedent and condone, if not encourage, illegal border-crossing. Others say that doing so is not only morally just but could also have broader social benefits by incentivizing immigrants to “come out of the shadows” – to work in the formal sector and more generally, join mainstream American society.

Existing research typically finds that extending legalization opportunities to unauthorized immigrants improves their earnings prospects.² However, we know less about the downstream societal impacts of legalization. Kuka, Shenhav, and Shih (2018) argue that, by increasing the return to education, DACA has raised educational attainment and lowered teenage fertility among affected immigrants. Other recent studies show that large-scale legalization both in the U.S. (Baker, 2015) and Europe (Bell, Fasani, and Machin, 2013; Mastrobuoni and Pinotti, 2015; Pinotti, 2017) has reduced crime, possibly because legal status increases its opportunity cost.

We build on this nascent literature by estimating the impacts of receiving a Green Card, or lawful permanent resident (LPR) status, on the income tax system. Green Card holders have

¹ DACA provides work authorization and deferral from deportation for childhood arrivals prior to 2007. TPS provides work (and travel) authorization and deportation deferrals in response to temporary home country conditions, such as civil wars and natural disasters. The Trump administration recently terminated the TPS designation for Haitians and Hondurans, suspended new applications for temporary status under DACA, and is currently accepting applications for DACA renewals only under federal court order.

² Some studies exploit variation from the one of IRCA’s two legalization programs that targeted the long-time resident unauthorized (Kassoudji and Cobb-Clark, 2002, Amuedo-Dorantes, Bansak, and Raphael, 2007; Lozano and Sorensen, 2011; Pan, 2012; Steigleder and Sparber, 2017). Others consider the 1997 Nicaraguan Adjustment and Central American Relief Act (Kaushal, 2006) and DACA (Pope, 2016; Amuedo-Dorantes and Antman, 2017).

an incentive to file income tax returns to protect their status. But there may also be personal financial gain in filing, since LPRs qualify for the federal Earned Income Tax Credit (EITC). A refundable tax credit that began as a payroll tax offset for lower-earning families, the EITC is now a central federal anti-poverty program, providing cash transfers to 27 million families of up to 40% of annual earnings. Given current tax law, conferring LPR status on unauthorized parents is necessary for millions of citizen children to access these transfers, and any benefits from childhood exposure to them.³ State income tax participation among the previously unauthorized may also help to fund local public goods, such as education, that immigrants routinely use.^{4, 5}

We estimate the effects of Green Card distribution on the state and federal income tax using variation from the Immigration Reform and Control Act of 1986 (IRCA). The last major comprehensive immigration reform, IRCA provided a path to a Green Card for over three million immigrants, mostly low-wage Mexicans, living unlawfully in the U.S. at the time. About 88% of applicants were successful, driving an unprecedented short-lived spike in the flow of new LPRs starting in the late 1980s, shown in Figure 1. Applicants were also unevenly distributed across the country as well as across California, which was the intended home of the majority. Combined with the state's income tax statistics, which provide unparalleled insights into income tax participation for this period, this spatial variation makes California the ideal setting for this study.

³ Over 2009 to 2013, an estimated 4 million U.S. citizens lived with at least one unauthorized parent (Capps, Fix, and Zong, 2016). The additional family income from the EITC lifts large numbers of children out of poverty every year (Short, 2014). Previous research has also found that it improves health at birth (Hoynes, Miller, and Simon, 2015) and the test scores (Dahl and Lochner, 2012), college enrollment rates (Manoli and Turner, 2018), and educational attainment and earnings (Bastian and Micheltore, 2017) of older children.

⁴ Most of the “fiscal burden” of immigration is state and local, due largely to enrollment of immigrants in public schools (National Academy of Sciences, 2016). In some states with large immigrant populations, like California, education is also partially funded through the state income tax. See Brunner and Sonstelie (2006) for an overview of California's school finance system.

⁵ Our investigation is complementary to that in Monras, Vázquez-Grenno, and Elias (2018), who estimate the payroll tax impacts of a recent legalization episode in Spain.

Our baseline empirical strategy takes advantage of this variation and the timing of Green Card distribution under IRCA’s legalization programs. Intuitively, we ask whether California metro areas where higher shares of the working-age population applied for temporary legal status – and were eventually eligible for Green Cards – experienced larger changes in aggregate tax outcomes starting in the late 1980s as those Green Cards were issued (the “short term”), then entered a new equilibrium through the end of the 1990s (the “medium term”).⁶ We apply this strategy to newly-digitized, richly detailed, county-level California state income tax statistics spanning the 1980s and 1990s, as well as county-level panel data on EITC transfers. To validate our EITC findings, we also exploit variation in applicant share across and within other states.

Our estimates imply that distributing Green Cards to an estimated 1.33 million unauthorized in California led to a sustained increase of around 700,000 state income tax returns annually, accounting for a million adults. While there is suggestion of upward income mobility and increasing tax contributions over the medium term, in the short term Green Cards generated little in the way of additional state income tax revenue given the relatively large number of dependents claimed by new LPRs and California’s generous tax treatment of children in low-income families. At the federal level, annual EITC transfers to state residents rose, initially by about \$500 million and by \$1 billion in the years after the 1994 EITC expansion.

Additional evidence supports a causal interpretation. Filing increases in areas with higher applicant shares were well-timed with Green Card distribution to IRCA applicants and were initially concentrated at the bottom of the income distribution, consistent with their economic

⁶ Though we could extend the analysis out further into the future to obtain “long term” estimates, potential residential mobility of IRCA applicants from their place of application becomes an ever-increasing concern, given the spread of Mexicans from California that began in the 1990s (Card and Lewis, 2007). Reassuringly, the relationship between applications per capita and Green Cards issued to IRCA applicants per capita at the state level is very close to what would have been expected in the absence of internal migration (Appendix A), but no data exist on the location decisions of applicants after they became permanent residents.

profile. Effects of the EITC expansions over our analysis period on the non-applicant populations of areas with higher applicant shares also cannot explain our EITC estimates.⁷ In addition, application of the baseline research design within several other states suggests that the California findings are not idiosyncratic; variation across states also shows the rise in the number of EITC claims – not just transfer amounts – fueled by new Green Cards. Further, consideration of an alternative research design – one which exploits the fact that California’s Hispanics generated more applicants per capita than did New York’s but may have been equally subject to IRCA’s other, enforcement provisions – yields similar inferences for the EITC.

Our findings thus arguably represent the reduced-form income tax impacts of the largest legalization episode in the history of the U.S., and possibly the world. If the non-applicant local population was unaffected, however, they also uncover the person-level impacts of obtaining a Green Card. Under this interpretation, our estimates imply that new LPRs were represented on state income tax returns at about the same rate (75 to 80%) as other California residents were prior to IRCA. The implied EITC effects of obtaining a Green Card also align well with predictions of EITC receipt based on applicants’ earnings and fertility and these tax-filing rates. By the end of the 1990s, IRCA’s Green Card distribution was generating around \$700 in EITC transfers per new LPR, amounting to about \$1,400 per new (state) income tax return.

Our findings speak to current conversations over immigration reform. Large-scale distribution of Green Cards would arguably have similar effects today: the EITC remains a central transfer program, and though demographically similar as in the past, the unauthorized are now less concentrated in states with income taxes (Hoefer, Rytina, and Baker, 2013). That said, our estimates are context-specific, dependent on the population at hand and the tax institutions in

⁷ The EITC was expanded three times over our analysis period: first by the Tax Reform Act of 1986 and later by the Omnibus Budget Reconciliation Acts of 1990 and 1993.

place in the 1980s and 1990s. Individual tax identifier numbers (ITINs) enable federal income tax compliance (though still not EITC access) in this population today. IRCA's size and accompanying enforcement efforts may have also affected economic opportunity – and tax liability – for this cohort of legalized immigrants. We thus urge some caution in generalization.

2. The Immigration Reform and Control Act

2.1. IRCA's Legalization Programs

Public Law 99-603 – familiarly known as IRCA – was largely unanticipated when signed into law by President Reagan on November 6, 1986, having passed due only to a fragile coalition rapidly assembled at the end of the year after 15 years of failed attempts at immigration reform (Baker, 1990). Then, like now, opposition to amnesty was fierce among a significant share of politicians, and the increased enforcement measures that opponents demanded in exchange for support were anathema to existing supporters.

IRCA had two legalization programs. The general legalization program, sometimes referred to as the “Legally Authorized Worker” (LAW) program, allowed immigrants who could document continuous residence in the U.S. since before January 1, 1982 to apply for temporary legal status, which consisted of work and travel authorization, between May 1987 and May 1988. The “Special Agricultural Worker” (SAW) program accepted applications for temporary status between May 1987 and November 1988 from those who could demonstrate 90 days of work on certain USDA-defined seasonal crops in the year ending May 1, 1986, with no additional residency requirement. 40% of all applications came through the SAW program.

Both programs charted a path to permanent residency. LAW applicants were able to apply for LPR status 18 months after approval of temporary status, contingent on learning English and passing a civics test by the time of application. SAW applicants, by contrast, were

granted LPR status almost automatically one or two years after gaining temporary admission.⁸ 88% of all 3.04 million applicants – nearly all of the 90% who achieved temporary status – eventually became LPRs, and transition rates to permanent residency were similar across programs, at 90.5% for LAWs and 85.5% for SAWs (Rytina, 2002). LPRs through either program could go on to apply for U.S. citizenship, but residency for income tax purposes – and qualification for the EITC – was established by a Green Card alone.⁹

Table 1 Panel A gives characteristics of the 1.62 million resident Californians who applied for legal status, based on anonymized data on the universe of applications from the Legalization Applications Processing System (LAPS). (See Appendix A for a complete description of the data.) Nearly all applicants were working age – between the ages of 15 and 64 – and only a third were women (column 1). The applicant pool was also overwhelmingly Hispanic, with 83% from Mexico alone. Similar to national averages, 41% of California applicants were SAWs. LAW and SAW applicants differed: SAWs were more likely to be working age, male, and Mexican (columns 2 and 3). Survey data from the Legalized Population Survey (LPS) and the 1989 National Agricultural Workers Survey (NAWS), summarized in Panel B, also show that working-age SAWs had lower hourly wages, less completed education, and fewer children than LAWs. However, the two groups were more similar to each other than they were to the rest of California’s population – low wage, high employment, and of the age to have children, suggesting high EITC eligibility and limited liability under the state’s income tax, at least at the time of application.

⁸ The first 350,000 applicants who could demonstrate having worked on farms with qualifying crops for each of the three years ending May 1, 1986 were on the faster track (one year) to receive permanent residency.

⁹ A law change put in effect in 1995 added a full (calendar) year residency requirement to qualify for the EITC. This should not affect our analysis since almost all Green Cards for IRCA legalization applicants were issued by 1993.

2.2. *IRCA's Timing and Regional Variation*

IRCA presents an intuitive strategy for identifying the effects of permanent residency in geographic panel data: if such effects exist, areas with more applicants in the working-age population should have begun to experience relatively large changes in outcomes when the typical applicant received a Green Card.¹⁰

Figure 2 considers timing, tracing the status of applications over time from the LAW and SAW programs combined based on the application-level data from the LAPS and statistics on LPR transitions and naturalizations from Rytina (2002).¹¹ As expected, all applications for temporary status were submitted by late 1988. Half were accepted by the end of 1988, but acceptances continued through 1992. These trends were similar in California and the nation, supporting our application of the nationwide timing of LPR transitions to the California analysis. These transitions occurred mainly between the 1989 and 1992 fiscal years, spanning October 1, 1988 to September 30, 1992. Since an immigrant is a U.S. resident for tax purposes if an LPR for any of a calendar year, Figure 2 thus suggests that the filing effects of LPR status should have first appeared in 1988 and continued to rise through 1992. But there could have been filing lags for bureaucratic reasons (e.g., delays in obtaining a social security number), making when growth in new filing ends – and our “medium term” analysis begins – an empirical question.

California was the intended home of 53% of working-age applicants nationally, and applicants made up a large share – 8.2% – of California's working-age population when IRCA was passed in 1986. Texas was a distant second, with 415,000 working-age applicants (14.6% of the national total), accounting for 3.8% of the state's working-age population. The population percentages are examples of our measure of policy intensity going forward: the ratio of working-

¹⁰ LPR share could be viewed as endogenous to tax outcomes and is also difficult to measure precisely.

¹¹ Appendix Figure A1 shows cumulative application status trends separately for LAW and SAW applicants.

age applicants (based on LAPS data) to total working-age population as of 1986 (based on Census Bureau estimates) times 100, which we also refer to as the “applicant share,” or A_c .¹²

Our empirical approach works because the state most affected and the subject of our analysis – California – had an uneven spatial distribution of applicants. Table 2 Panel A summarizes the variation in applicant share across California metropolitan statistical areas (MSAs, or metro areas), which will be our primary cross-sectional unit of analysis.¹³ There was significant variation in A_c in the state: the standard deviation was 4.3 percentage points (column 1). In addition, metro areas with above versus below (unweighted) median A_c for the state on average had a 7.4 percentage point higher applicant share (column 4).¹⁴ As shown in Figure 3, the highest applicant share MSAs were in southern California (Los Angeles) and the Central Valley, but MSAs with above-median applicant shares were distributed throughout the state.

2.3. *Green Cards or Enforcement?*

As noted, the compromise that made IRCA possible countered the “immigration-loosening” legalization programs with new “immigration-tightening” enforcement measures – specifically, increased funding for border security and new sanctions for employers who knowingly hired unauthorized workers. This raises the concern that our estimates may not isolate the effect of Green Cards *per se*.

These enforcement measures were not directly related to tax outcomes, but might have had indirect effects by affecting earnings. Even in the aggregate, however, these effects could be opposite-signed. On one hand, Bansak and Raphael (2001) find that IRCA’s employer sanctions

¹² Arizona, New Mexico, and Nevada rounded out the top five states in terms of applicant shares. See Appendix Table A1 and Appendix Figure A2.

¹³ Statistics are weighted by 1986 population for consistency with the empirical analysis to follow.

¹⁴ While they do not move in lockstep, areas with higher values of A_c on average have both higher LAW applicant shares and higher SAW applicant shares: the LAW applicant share was 5.3 percentage points higher and the SAW share 2.1 percentage points share in above-median versus below-median A_c California MSAs. We choose not to investigate these programs separately because of limited and not fully credible identifying variation from each.

increased wage discrimination against Hispanic workers relative to non-Hispanic workers, regardless of legal status.¹⁵ Given the high Hispanic share among amnesty applicants, any such discrimination should have lowered earnings among legalized workers relative to a counterfactual without employer sanctions, potentially increasing the likelihood that they had earnings in the EITC eligibility range, for example. On the other hand, if effective, heightened border security might have increased wage growth of newly legalized workers by reducing the flow of competing Mexican and Central American migrants, lowering their EITC eligibility.¹⁶

Our baseline empirical approach accounts for such aggregate impacts, as we ask whether trends in outcomes varied systematically with MSA applicant share. That is, our baseline approach will yield biased estimates only if any direct effects of enforcement on tax outcomes were not completely offsetting *and* were proportional to applicant share. An alternative approach using an area with a high density of Hispanics but a low density of applicants (New York) as a comparison group for California also allows for time-varying direct effects of baseline MSA Hispanic share, potentially differencing out (net) enforcement effects.

3. Data and Key Variables

3.1. Income Tax Outcomes

Our analysis relies on a trove of detailed annual state income tax statistics published in the California Franchise Tax Board (CAFTB) *Annual Reports*.¹⁷ These reports give, separately by county and for narrow bins of adjusted gross income (AGI), the number of state income tax returns filed (total and joint returns), the number of dependents claimed, aggregate AGI, and tax

¹⁵ Other theoretical and empirical work also suggests that increased interior enforcement likely lowers wages (Cobb-Clark, Shiells, and Lowell, 1995; Chassambouli and Peri, 2015).

¹⁶ Time-series analysis suggests that IRCA had little impact on flows from Mexico (Passel and Woodrow, 1990; Orrenius and Zavodny, 2003), and there is little evidence to suggest border security affects the rate of border crossing (Gathmann, 2008). However, border security may raise apprehensions (Hanson and Spilimbergo, 1999).

¹⁷ All data sources introduced in this section are described in detail in Appendix A.

assessed. We initially focus on outcomes for low-AGI (“low-income”) filers, where short-term effects of Green Cards via IRCA were likely concentrated (Table 1). We consider the number of low-income state returns, as well as total AGI, number of dependents, estimated number of adults represented, and state income tax assessed on those returns. We also use data on EITC transfers to counties, reported in the BEA’s Local Area Personal Income Accounts. These latter data are available for states outside of California as well, which we consider in robustness checks. All monetary values are converted to real 2014 dollars using the CPI-U.

We apply a fixed definition across years of a low-income state return based on quintiles of California’s 1979 AGI distribution.¹⁸ More specifically, we define the bottom quintile – AGI below \$13,150 (2014 dollars) – as low-income. By a rough approximation – assuming applicants occupied the same ranks in the AGI and hourly wage distributions (see Appendix Figure A3) – the bottom quintile would have been relevant for 96% of California SAWs and 51% of California LAWs at the time of application, or nearly 71% of all California applicants.¹⁹

While we focus on the bottom quintile in our event-study estimates, we show results for higher quintiles for our preferred long-difference specification. We anticipate finding declining short-term estimates across quintiles; in fact, the AGI minimum for the third quintile (\$28,450) exceeds expectations for 90% of applicants at the time of application, suggesting that short-term estimates for the third quintile and above provide useful falsification checks. Over the medium

¹⁸ Using the 1979 distribution allows us to present results for the narrowest possible income ranges given rising inequality over the 1980s. Note that we must trim the 1979 AGI distribution at both the bottom (\$1) and top (\$130,433 in 2014 dollars, the 93rd percentile of the 1979 distribution) to apply a fixed definition of a low-income return across years; the quintiles pertain to this trimmed distribution. See Appendix A.

¹⁹ We benchmark the quintile thresholds of the overall 1979 California AGI distribution to the California wage distribution (based on the 1987 and 1988 CPS-MORGs) and determine the percentiles of the California LAW and California SAW wage distributions at the time of application (based on data from the LPS and 1989 NAWS) that correspond to the resulting wage values. This procedure implies that the second quintile will capture the remaining 4% of SAW applicants and 31% of LAW applicants (19% of all applicants), the third quintile 10% of LAW applicants (5.6% of all applicants), the fourth quintile 5% of LAW applicants (2.8% of all applicants), and the fifth quintile the remaining 3% of LAW applicants (1.7% of all applicants).

term, however, this need no longer need be true, as the IRCA cohort ages and earns more. Even so, EITC eligibility would have arguably remained high through the end of the sample period.²⁰

We aggregate the CAFTB state tax outcomes and BEA EITC transfers to the MSA level, due to limitations on geographic detail in several key controls introduced below, and normalize the aggregate MSA figures by annual estimates of MSA working age population (15- to 64-year-olds), based on county-level data published by the Census Bureau. Our analysis then focuses on an annual, MSA-level panel for California for the 21 years spanning 1979 through 1999.²¹

Table 2 Panel B summarizes pre-IRCA (1986) levels of key outcome measures; the CAFTB count measures are multiplied by 100 to be on a comparable scale with A_c . For every 100 working-age people, between 10 and 11 California state returns were filed in 1986 and about 12 adults and 4 dependents were represented in the bottom quintile of the state's 1979 AGI distribution. The average working age person also received about \$23.50 in EITC transfers. While there are some significant differences in 1986 average tax characteristics across MSAs with above- and below-median applicant shares (column 4), our estimation strategy exploits the entire continuum of A_c and, to account for the “tech boom,” allows for different unrestricted outcome trends in the Bay Area and the rest of the state.²² Column 5 shows that conditional on a Bay Area fixed effect to match this specification, the predictive power of A_c for 1986 tax outcomes is a bit weakened statistically. Even so, our preferred estimates allow for time-varying effects of tax characteristics where the coefficient on A_c is significant, marked with a “†”.

²⁰ During the period of Green Card distribution under IRCA (1988 to 1992), EITC eligibility was limited to families with children and phased out completely at around \$37,000 in annual family earnings. In 1994, following the 1993 Omnibus Budget Reconciliation Act, the maximum earnings to qualify for the EITC rose, particularly for families with two or more children, and an EITC for childless adults was introduced. See Appendix Figure A4.

²¹ Using metro areas instead of counties also may help limit attenuation due to internal migration of the applicants. The choice of a 1999 end date is arbitrary, but also motivated by this concern.

²² As shown in Figure 3, the MSAs of the Bay Area – Oakland, San Francisco, San Jose, Santa Cruz, Santa Rosa, and Vallejo – on average had lower values of A_c .

Unfortunately, we cannot study impacts on the same outcomes for the federal income tax due to the lack of sufficiently detailed statistics on federal income tax returns for counties or MSAs. We instead provide evidence of increased federal filing among the low-income using state-level variation in A_c (Appendix Figure A2) and published state-level data on the number of EITC claims from IRS Statistics of Income (SOI) from 1983 forward. The *SOI Bulletins* also give total EITC amounts claimed (including refunds and reductions in positive tax liability), providing a point of comparison to estimates based on the BEA reports of EITC transfers. Again, we normalize these figures by annual estimates of working-age population and focus on annual panel data through 1999. Since it is an outlier in terms of applicant share, we exclude California from the state-level analysis.

3.2. *Data on Potential Confounders*

Because our empirical strategy relies on variation across areas over time, our estimates are at risk of contamination by other local area shocks to income tax outcomes for lower-income populations. We attempt to mitigate this risk by including control variables in our models.

One worry is that metro areas with higher A_c experienced relative increases in tax-filing rates among low earners or in EITC transfers because those areas gained relatively more low earners over time. Our models therefore allow for time-varying effects of pre-IRCA area characteristics that correlate with trends in local wage structures over the period of interest. Aside from the Bay Area dummy, 1980 educational composition of the population is a useful predictor of changes in the local wage structure: metro areas with higher education levels experienced relatively large gains in the returns to education over the 1980s (Beaudry, Doms, and Lewis, 2010). We gather data on the educational attainment of adults from published tabulations from the 1980 Census (Minnesota Population Center, 2011). California MSAs with

higher applicant shares did indeed have higher high school dropout rates, though not different college equivalent rates (Table 2 Panel C); we allow for time-varying effects of both.

Most of our remaining controls are motivated by the need to account for other determinants of the EITC over the period of interest. First, holding the EITC schedule constant, EITC transfers to an area will depend not only on the local wage distribution, but also on the presence of children in tax-filing units with low earnings, the employment rate among low earners, and how informed the local population is about the EITC. We cannot observe these factors directly, but we can use employment to working-age population ratios (from County Business Patterns) as a proxy for local economic activity and the ratio of kids to the total working-age population as a measure of the presence of children.²³ By allowing for time-varying effects of 1986 per-capita EITC transfers, we may also account for different metro area trends in EITC knowledge.²⁴

Second, the EITC schedule expanded three times over the sample period – in 1987, 1991, and 1994 (Appendix Figure A4) – contributing to mechanical increases in EITC transfers holding constant program knowledge and the demographic and economic composition of the MSA population. Our estimates would be unaffected if A_c were orthogonal to MSA-level correlates of these expansions. However, metro areas with relatively high applicant shares may have been more likely to have non-applicant populations with earnings in EITC expansion ranges. We gather information on the local share of likely non-applicants who stood to gain from the EITC schedule changes, given their earnings and presence of children, from the 1980 Census

²³ For the state-level analysis, we use the state unemployment rate as a control.

²⁴ Chetty, Friedman, and Saez (2013) use the degree of “sharp bunching” around the first kink in the EITC schedule as a proxy for how informed a local population is about the EITC. For tax year 1996 (the first for which our data overlap), per-capita state EITC transfers are positively correlated with their state-level sharp bunching measure, suggesting that per-capita EITC transfers may also reflect knowledge about the program.

Public Use Microdata Samples (PUMS; Ruggles et al., 2015).²⁵ We also consolidate information on entire earned income distributions of likely non-applicants by family type in 1980 into an “expected EITC” for non-applicants, which applies later EITC schedules to each area’s non-applicant population as of the 1980 Census.

There is little preliminary evidence that changes in the EITC schedule confound our inferences about EITC transfers. As shown in Table 2 Panel D, few predictors of future EITC transfers to the non-applicant population vary significantly with A_c . In addition, Figure 4 Panel A shows that both levels and trends in the expected non-applicant EITC are virtually identical across California MSAs with above versus below median applicant shares, with gains in expected EITC transfers arising as anticipated when the EITC expanded in 1987, 1991, and especially in 1994.²⁶ Yet, California MSAs experienced relatively large gains in actual EITC transfers well before this, and particularly between 1990 and 1991, when many IRCA applicants got Green Cards (Figures 1 and 2). To keep our specifications parsimonious, we allow for time-varying effects only of the two non-applicant shares that coincide roughly with newly eligible groups – those corresponding to the highest income ranges shown, by family type.

4. Permanent Residency and Tax Outcomes

Our first empirical model is an event-study one in the spirit of Figure 4 Panel A, preserving year-to-year variation in the relationship of A_c with outcomes and so not imposing the timing of LPR transitions. However, it uses all of the variation in A_c across metro areas, more readily enabling a person-level interpretation of the estimates (see Appendix B). It also allows us to test whether differences in outcomes with A_c change in a statistically meaningful way over time and to regression adjust these differences for other metro area-by-time varying factors. We

²⁵ Likely non-applicants include citizens and non-citizens not from Mexico or Central America.

²⁶ A similar result holds if we apply the EITC schedule to an MSA’s likely non-applicant population in 1990. We prefer to construct expected EITC based on 1980 characteristics because they were predetermined as of IRCA.

then summarize the content of these event-study estimates and probe the robustness of our findings with a more restrictive long-differences model that takes the timing of Green Card distribution explicitly into account.

4.1. Event-study estimates

Our event-study specification models outcome y_{ct} in MSA c in year t as

$$(1) \quad y_{ct} = \sum_{\tau \neq 87} \theta_{\tau} D_t^{\tau} \times A_c + \sum_{\tau \neq 87} \beta_{\tau} D_t^{\tau} \times X_c + X'_{ct} \kappa + \gamma_c + \alpha_t + \varepsilon_{ct} ,$$

where D_t^{τ} is a year dummy, set to one if t is equal to τ , zero otherwise (or $D_t^{\tau} = 1[t = \tau]$). We interact the year dummies with applicant share, A_c , as well as with X_c – the other pre-IRCA MSA characteristics described in Section 3.2 and marked with a “†” in Table 2, as well as a Bay Area dummy. We also control for the ratios of employment and children to the working-age population, X_{ct} . The MSA fixed effects, γ_c , then absorb all sources of potential bias, observed and unobserved, that are fixed within a metro area over time. The year fixed effects, α_t , account for shocks to outcomes shared by all California MSAs at a given point in time. ε_{ct} represents unobserved determinants of outcomes.

The θ_{τ} ’s are the parameters of interest, capturing the precise timing of differential changes in outcomes for areas with relatively high values of A_c relative to the base year 1987 – the year before the LPR transitions for IRCA amnesty applicants began. If A_c were a dummy for being above the median applicant share for the state, like in Figure 4 Panel A, and if there were no controls beyond the MSA and year fixed effects, θ_{τ} would give a difference-in-differences in mean outcomes between above- and below-median MSAs shown in Figure 4 Panel A, in year τ versus 1987. Instead of splitting A_c in two groups, model 1 uses the entire continuum of A_c , so θ_{τ} becomes a difference in slope coefficient estimates on A_c in τ versus 1987, and controls for other metro-area characteristics.

Figure 4 Panel B shows estimates of model 1 for EITC transfers per capita and for the expected non-applicant EITC. The capped vertical lines around the estimates represent their 95% confidence intervals; inference accounts for clustering on metro area, and regression estimates are weighted by 1986 MSA population.²⁷ For actual EITC transfers, estimates of the interaction coefficients θ_t are first statistically significant in 1991 and grow in magnitude in the years to follow. Also consistent with the trends shown in Figure 4 Panel A, event-study coefficient estimates for the expected EITC for non-applicants are relatively small and never significant.

Figure 5 turns to estimates of model 1 for the California state income tax measures, starting with low-income state filing rates in Panel A. MSAs with higher applicant shares experienced relatively large increases in bottom-quintile state returns filed per working age person starting in 1988. Estimates of the interaction coefficients θ_t are statistically significant by 1990 and continue to grow in magnitude through 1993; stabilization in 1992 or shortly thereafter is expected if Green Cards are the causal mechanism at work, given the timing of their distribution, repeated from Figure 2 with dashed green line (right axis) in the figure. Declines are possible across the late 1990s if the new LPRs moved up the income distribution with age.²⁸ Effects on real AGI per working age person (Panel B) follow a similar pattern.

As seen in Panel C, however, effects on state income taxes assessed on low-income filers do not, despite the increase in real AGI, suggesting limited state income tax liability among most new LPRs. State income taxes assessed are a function not only of AGI, but also of any deductions that lower taxable income, tax rates, and credits that reduce tax liability. Application of California's non-refundable child tax credit to the new filers can alone explain the null (to

²⁷ Given the potentially small number of clusters (23), our inference throughout the paper assumes degrees of freedom equal to the number of clusters less two (e.g., 21 degrees of freedom in the California analysis).

Simulations by Cameron, Gelbach, and Miller (2008) imply that this approach generates tests of about correct size.

²⁸ That EITC transfers per capita continue to rise (Figure 4) is a function of changes in the EITC schedule benefiting new LPRs, as discussed in more detail below.

negative) findings for taxes assessed on low-income filers. Consistent with the IRCA amnesty applicants having children or being of prime age to do so (Table 1), many of the marginal low-income filers had dependents: the number of dependents claimed among low-income filers in relatively high A_c MSAs changes roughly one-for-one with returns filed after 1987 (Panel A). Adding back our estimate of this credit to taxes assessed, in Panel C, generates a positive impact on “counterfactual” taxes, assuming this credit did not exist.

The key identifying assumption in model 1 is that unobserved determinants of outcomes were not trending differently across metro areas with higher applicant shares, particularly after 1987. Put differently, in the absence of IRCA’s legalization provisions, we assume that areas with higher applicant shares would not have experienced different trends in outcomes after Green Cards were issued. Our choice of controls was intended to help satisfy this assumption, but we cannot test it directly. If it were violated, though, metro areas with higher values of A_c may have already been experiencing changes in outcomes in the pre-IRCA period. We test for pre-trends formally below but note now that the event-study estimates for $\tau < 1987$ are consistently statistically insignificant and small relative to those for $\tau > 1987$.

4.2. *Long-difference estimates*

The graphical evidence in Figures 4 and 5, based on estimation of model 1, suggests that the distribution of Green Cards through IRCA brought the previously unauthorized “out of the shadows” and onto income tax rolls in California. It also led to an increase in EITC transfers to California residents – and potentially to the residents of other states with amnesty applicants. But the focus there was only on low-income returns; the event-study model itself also does not provide a simple take-away message about magnitudes or a ready way of assessing robustness.

To address these limitations, we turn to a long-differences specification. We restrict attention to data from key years: 1981 (pre-IRCA), 1987 (pre-transition to permanent residency), 1993 (end of filing response to permanent residency), and 1999 (end of analysis period) and take 6-year long differences over 1981 to 1987, 1987 to 1993 (short term), and 1993 to 1999 (continuing to medium term). Stacking these differences, we then estimate the model:

$$(2) \quad \Delta y_{ct} = \alpha_t + \theta_t A_c + \beta_t X_c + \Delta X'_{ct} \kappa + \Delta \varepsilon_{ct} ,$$

where Δ represents a 6-year difference ($\Delta y_{ct} = y_{ct} - y_{ct-6}$) and all other variables are as already defined. Thus, as in model 1, we force the effects of the time-varying covariates to be the same over time but allow effects of fixed MSA characteristics (and the intercept) to vary.

The θ_t ($t = 1987, 1993$, and 1999) are now the parameters of interest. If IRCA's legalization were driving our findings, estimates of θ_{93} should be sizable and statistically significant for low-income tax outcomes, whereas estimates of θ_{87} should not be. Interpretation of estimates of θ_{99} depends on the outcome at hand, even among the low-income; as discussed, effects could be seen as the legalized population ages, or as the tax policy that applies to new LPRs (e.g., the EITC schedule or California's child tax credit) changes. We designate estimates of θ_{93} as short-term impacts and the sum of estimates of θ_{93} and θ_{99} as medium-term impacts, allowing effects to accumulate over time. The sources of identification in short- and medium-term impacts in model 2 are relatively transparent: these coefficients will allow us to identify the effects of Green Cards distributed under IRCA only if the unobservable shocks to outcomes captured in $\Delta \varepsilon_{ct}$ – including anything from the intensity of IRCA's enforcement and the EITC expansions to changes in local wage structures – are not correlated with A_c .

Table 3 shows the long-difference estimates for the outcomes presented in Figures 4 and 5, again weighting by 1986 population and clustering standard errors on metro area. Column 1

gives estimates for state filing in the bottom quintile. These estimates follow the basic pattern posited above. There was no differential growth in state filing in higher A_c areas before IRCA and a significant decline in bottom-quintile filing between 1993 and 1999, consistent with Figure 5 Panel A. But during the short-term, each additional percentage point increase in applicant share yielded on average 4 more bottom-quintile returns per every 1000 working age people. For the average California MSA, which had an applicant share of 7.1% and a 1986 working age population of 770,000, the estimates suggest that IRCA's legalization programs generated an additional 23,070 low-income tax returns by 1993.

There is another way to contemplate magnitudes. In particular, if the estimates not only identify the effect of legalization but non-applicants were also completely unaffected, the effect of moving from 0% to 100% on A_c is equivalent to the person-level impact of applying for amnesty (see Appendix B). Under these assumptions, our estimates imply that being an amnesty applicant increased the short-term probability of filing a bottom-quintile state income tax return by 0.422. Scaling up this estimate to reflect the 88% rate of Green Card receipt among applicants by 1993, we arrive at the implied short-term effect of LPR status on the likelihood of filing a return and having AGI in the lowest quintile – a 48 percentage point ($0.422/0.88 \times 100$) increase.

Of course, the implied effect of a Green Card on the likelihood of filing a return *at all* needs to add up effects across quintiles of the AGI distribution, just like the *total* number additional returns filed due to the amnesty needs to account for higher-income returns. The solid lines in Figure 6 Panel A connect the short-term long-difference estimates for the state filing rate for all quintiles through the fourth, where our ability to reliably detect effects dies out along with their likelihood of occurring.²⁹ The short-term impact, shown in the first row of Table 4, is then

²⁹ Appendix Figure 1 plots the pre-trends (long-difference estimates for 1981 to 1987) by quintile for all of the same outcomes as in Figure 6.

the implied total effect of having a Green Card at the person level, aggregating across the first four quintiles and normalizing by the 88% Green Card issuance rate.³⁰ Becoming an LPR increased an applicant's short-term chances of filing a state income tax return by 61.8 percentage points. This corresponds to an increase in the total number of returns filed in California between 1987 and 1993 of around 820,000 annually.³¹ By 1999, these figures decline to 53.2 percentage points and about 708,000 annual returns.

Such figures might seem too low to be consistent with a high degree of tax compliance among IRCA Green Card holders. But in cases where these new LPRs filed *joint* returns (i.e., if they were married to one another), only one new tax return would have been filed for every two applicants. Figure 6 Panel B gives long-difference estimates by quintile for the number of *adults* represented on returns ($2 \times$ number of joint returns + number of single returns), a concise way of summarizing this information; see also column 2 of Tables 3 and 4. These estimates follow the same pattern for filing overall but yield a higher short-term impact, of 83.7%, corresponding to 1.11 million adults. For the medium term, these figures fall slightly to 75.5% and one million, respectively. The implied filing rate is very similar to the non-applicant filing rate of 76.4%, estimated using the number of adults represented on California state income tax returns as a fraction of the 1986 working-age population, net of applicants.

The quintile-specific estimates also provide insight into income mobility among the applicants. The short-term filing estimates shown in Panels A and B of Figure 6 are roughly proportional to the share of applicants that we anticipated each quintile would represent: effects

³⁰ Adding in the fifth quintile generates enough noise to swamp the point estimate, particularly for continuous outcomes such as real AGI and taxes assessed, where outlying values can be influential on the estimates. For the count outcomes, estimates are not statistically different with the fifth quintile included.

³¹ We use the total number of working-age Californian applicants ($1,511,772 = 1,622,074 \times 0.932$; see Table 1) in this calculation and all subsequent calculations like it. We also continue to apply the 88% transition rate to LPR status. Thus, we estimate that $1,330,360 (= 0.88 \times 1,511,772)$ working age California applicants obtained Green Cards.

are largest for the bottom quintile and fade out by the third. But there is suggestion of upward mobility over the medium term, as shown in the dashed line: over 1993 to 1999, declines in representation in the bottom quintile are statistically significant and increases in the third are almost so. There is also evidence of increases in real AGI and state income tax contributions in the third quintile later in the 1990s (Panels D and E), and of relative declines in the number of dependents claimed (Panel C), which would be expected as the IRCA cohort ages.³² However, any conclusions about income mobility must necessarily be relatively speculative given the increasing possibility of geographic mobility among applicants over time.

The last two columns of Tables 3 and 4 present long-difference estimates and implied short- and medium-term impacts for actual EITC transfers and expected non-applicant EITC transfers. Even with potential income mobility in this population, it is not surprising to see a positive medium-term impact that is significantly larger than the short-term estimate: the expansion of the EITC in 1994 would have yielded large benefits for new LPRs with two or more children already filing a tax return and claiming the EITC. The estimates imply that IRCA's legalization provisions generated nearly \$494 million in EITC transfers for California residents by 1993. By 1999, this figure rose to \$988 million. Reassuringly, we find no impact on the expected non-applicant EITC and no significant pre-IRCA trend in EITC transfers to higher A_c areas.

Returning to the question of interpretation, the implied person-level EITC effects of having a Green Card can be fruitfully compared to mechanical EITC eligibility impacts – what we anticipate EITC transfers to applicants to have been based on their characteristics (Table 1 and associated data) if there had been no behavioral labor supply response to the EITC and an

³² We do not show tax revenues gross of the California child tax credit above the third quintile (panel F) because for much of the 1990s the credit was unavailable to higher income taxpayers.

effect on federal tax filing equal to what we see at the state level.³³ Assuming complete take-up of the EITC among new tax filers, mechanical EITC impacts are very similar to what we find over the medium term – \$697 versus \$743 (Table 4, column 7).³⁴ Combining the actual estimate with its counterpart for overall state filing rate – and assuming that increases in state filing match those for federal filing – generates around \$1,400 ($\$743/0.532$) in EITC transfers on the marginal return. In the short-term, the mechanical amounts (\$598) are larger than the estimates (\$371), suggesting that take-up of the EITC among new filers may have been incomplete initially.

5. Robustness

Our approach yields fairly compelling evidence that IRCA Green Card holders filed California state tax returns at high rates and received sizable federal EITC transfers. But questions remain: Are these findings somehow idiosyncratic to California? Applicant share is also not randomly assigned, so despite our judicious choice of controls and the robustness checks shown thus far, could our estimates be capturing something beyond the impacts of IRCA’s legalization? We address these concerns in this section, largely by estimation of EITC impacts using other geographies.

5.1. *Application of the Long-Difference Model to Other Geographies*

³³ To project “mechanical” EITC eligibility, we map family income-by-fertility bins in the LPS and NAWS onto formulaic 1993 and 1999 EITC amounts using Mexican and Central American immigrants in California in the 1980 Census PUMS. We use family income from baseline and fertility as of 1992 in the LPS (sample of LAW applicants). In the NAWS (sample of SAW applicants), we use family income at baseline but project fertility forward using a model estimated on the two years of LPS data. Mechanical EITC amounts would be larger if fertility is higher than we predict but smaller if LPRs experienced family earnings growth after 1992.

³⁴ If we allow for the more realistic scenario of incomplete take-up – specifically, an 80% take-up rate, consistent with existing IRS tabulations (<https://www.eitc.irs.gov/eitc-central/participation-rate/eitc-participation-rate-by-states>, accessed July 3, 2018) – our estimates are larger than would be expected on the basis of mechanical effects alone, leaving some scope for a behavioral response (e.g., \$568 by 1999). Such behavioral labor supply responses have been found to be empirically important (e.g., Eissa and Liebman, 1996; Eissa and Hoynes, 2004). Unfortunately, we cannot estimate behavioral labor supply responses to qualifying for the EITC using our research design, because it at best identifies the reduced-form impacts of obtaining a Green Card; the Current Population survey is also too small and identifies too few metro areas to estimate even reduced-form labor supply responses. However, estimates of the impact of the EITC on the income distribution suggest that mechanical contributions outweigh behavioral ones (Chetty, Friedman, and Saez, 2013).

In the spirit of the last column of Table 2, the first two columns of Table 5 give within-state slopes on A_c for key baseline MSA characteristics for California (column 1, but now without Bay Area fixed effects) and for Florida and Texas (column 2, controlling for state fixed effects), which offer enough within-state variation in applicant share to support identification (Appendix Table A1). Column 3 does a similar exercise for state-level characteristics, using the state-level variation in A_c (Appendix Table A2) but excluding California. There may be slightly more balance on education composition in Florida and Texas than in California, but the estimates are imprecise. At the state level, A_c is positively correlated with college equivalent share, but negatively correlated with dropout share – the opposite of what is true across California MSAs.

The first two columns of Table 6 give EITC transfer and expected non-applicant EITC transfer estimates for California MSAs using a specification that is estimable for these other geographies.³⁵ The short- and medium-term estimates are larger than what we find at baseline, suggesting the importance for magnitudes of the additional controls. Even so, the consistent pattern of effects supports consideration of the pared-down specification for Florida and Texas. As shown in columns 3 and 4, estimates for Florida and Texas MSAs (including state-by-year fixed effects) are similar to the California estimates. At the state level, the short-term impacts are also very similar to what we found for California: LPR status on average yields about a \$520 annual EITC transfer (column 5), compared to a \$480 transfer in California (column 1). Medium-term impacts are not as large, but they are imprecise. The expected non-applicant EITC findings (column 6) also suggest that the medium-term estimates at the state level may be biased downward by non-applicant populations in states with higher applicant shares benefiting less from the 1994 EITC expansion.

³⁵ From the vector X_c , we thus exclude the Bay Area fixed effect and the 1986 levels of state income tax outcomes.

Despite this caveat, the remaining columns of Table 6 show estimates for two outcomes that can only be observed at the state level: total EITC amounts (which incorporate reductions in tax liability; column 7) and federal returns claiming the EITC (x 100; column 8), both normalized by working age population. The identifying variation in the long-difference model yields increases in EITC transfers on the extensive margin, just as we would expect if Green Cards were the causal mechanism. Taken literally, the estimates imply that becoming an LPR increases the likelihood of claiming the EITC by 38 percentage points in the short term and by 27 percentage points over the medium run. These figures understate the number of adult LPRs on a return claiming the EITC, however, due to instances of joint filing.

5.2. *Alternative Research Design*

As a final check on our findings, we consider an alternative research design, motivated in part by concerns that IRCA's stepped-up enforcement measures may have affected Hispanics broadly (e.g., through employer discrimination or labor market competition). If enforcement fell differentially on high Hispanic density areas, our estimates so far may capture the joint effects of legalization and enforcement.³⁶ Adding New York as a comparison group allows us to control for time-varying effects of Hispanic share, since it also had a high Hispanic share in 1980 (11%, versus 19% in California in 1980), but New York's Hispanics were weighted toward groups that were already citizens or authorized. In addition, anecdotal evidence suggests that community organizations were particularly mobilized and effective in helping unauthorized immigrants apply for amnesty in California. On the other hand, New York had fewer applicants than expected, potentially due to less in the way of such grassroots efforts (Baker, 1990).

³⁶ Such enforcement efforts may have also differentially affected areas with high foreign-born share. We have therefore also done the exercise described below using foreign-born share in place of Hispanic share, with very similar results. As explained earlier, though, it is not *a priori* clear what impact IRCA's combination of border enforcement and employer sanctions might have had on *local* tax outcomes, if any.

The historical record thus suggests that the ratio of applicants to Hispanics should have been much higher in California than in New York. If application (and ultimately Green Cards) affected income tax outcomes, the same Hispanic share should have then translated into a larger change in outcomes in California than in New York. This idea is captured in the model:

$$(3) \quad \Delta y_{cst} = \alpha_{st} + \theta_{st} H_c + \beta_{st} X_c + \Delta \varepsilon_{cst} ,$$

where H_c is now the 1980 Hispanic population share in MSA c in state s , estimated from Census tabulations (Minnesota Population Center, 2011). Except for the use of H_c in place of A_c , model 3 is very similar to model 2, where parameters had implicit s subscripts in being estimated for California only. But the parameters of interest in model 3 are *differences* in the coefficients on θ_{st} across California and New York (i.e., $\theta_{CA,t} - \theta_{NY,t}$). Thus, adding New York allows us to difference out time-varying direct effects of Hispanic share – something that would have been difficult to do in the California-only analysis given its high correlation with applicant share.

The first column and panel of Table 7 shows this correlation, regression adjusting for the more limited set of controls available for MSAs in all states that was used in Table 6. In California, every percentage point increase in Hispanic share was associated with a 0.597 percentage point increase in applicant share. The corresponding estimate in New York (column 2) is much smaller, and the 0.423 percentage point difference in coefficients on Hispanic share is highly statistically significant (column 3).

The remainder of Table 7 presents the estimates of $\theta_{CA,t}$ and $\theta_{NY,t}$ for EITC transfers, the only outcome for which we can implement this approach, along with $\theta_{CA,t} - \theta_{NY,t}$ (without and then with time-varying MSA controls), for each of the three periods considered in our long-difference analysis. The California-specific estimates (column 1) suggest an effect: the coefficient on H_c is much more positive and statistically significant after IRCA. And the New

York-specific estimates (column 2) provide little reason to think that the California estimates are biased, at least through the short-term. However, the New York coefficient on Hispanic share is negative though not strongly significant for the 1993 to 1999 difference. Areas with higher Hispanic shares thus appear to have had lower growth in EITC transfers after 1993; this might reflect impacts of IRCA's increased border security (e.g., increased earnings from reduced labor market competition for applicants), but it could well have other causes.

Regardless, differences in the California and New York coefficients for the short- and medium-term, shown in column 3, are positive and statistically significant. Implied effects are larger than our initial long-difference estimates for California only, but they also have large standard errors.³⁷ Adding time-varying controls, in column 4, produces implied effects that are somewhat smaller. Consistent with the effects being driven by new LPRs, we find no relationship with expected EITC transfers to the non-applicant population (column 5).

6. Conclusion

In this paper, we have explored how the income tax evolved after three million low-wage unauthorized immigrants, mainly from Mexico and Central America, were given the opportunity to apply for Green Cards starting in the late 1980s. Exploiting the timing and geographic intensity of IRCA's legalization programs and unique tax statistics from California, among other data sources, we find that becoming an LPR dramatically increased the chances these immigrants both filed income tax returns and, potentially via new filing, received the EITC.

The new state returns generated little in the way of new tax payments through the end of the 1990s. However, tentative evidence of rising incomes among the new LPRs is consistent with

³⁷ To obtain these impacts, we estimated model 3 on the combined California-New York data using instrumental variables, where the instruments were (year-specific) interactions between 1980 Hispanic share and a dummy for California (conditional on year-specific direct effect of Hispanic share). We then scaled the IV estimates by multiplying by 100 and dividing by the estimated LPR transition rate, 0.88, as in earlier tables.

existing research findings (Kassoudji and Cobb-Clark, 2002; Amuedo-Dorantes, Bansak, and Raphael, 2007) and suggests that an alternative approach following people rather than areas over time (to account for residential mobility of applicants) would reveal long-term positive impacts of legalization on state income tax revenues. Unfortunately, such person-level data are not currently available, nor are published statistics granular enough to examine how this historical episode affected payroll tax contributions. If Green Cards induced shifts in employment to the covered sector, the payroll tax effects would have been sizable.³⁸

By the late 1990s, however, the maximum EITC would have more than just offset payroll tax contributions; it would have elevated the incomes of immigrant families, and potentially dramatically so. On the one hand, there would have been deadweight loss involved in this redistribution. On the other, mounting evidence suggests that the social benefits from childhood exposure to the EITC – manifested in improved health at birth (Hoynes, Miller, and Simon, 2015), higher test scores (Dahl and Lochner, 2012), and higher levels of educational attainment (Bastian and Micheltore, 2017; Manoli and Turner, 2018) – would have been more than enough to compensate. And indeed, recent work suggests that childhood exposure to other social programs, such as food stamps (East, 2014) and public health insurance (Bronchetti, 2014), is particularly beneficial for immigrants.³⁹ Overall, our findings are thus consistent with other studies (e.g., Baker, 2015) that point to social benefits from a permanent solution to the challenge of a large unauthorized population.

³⁸ There was likely a positive impact on sales tax revenues proportional to applicant share as well. For example, the combined state and local California rate of sales tax was 7.25% (starting in 1991); Florida and Texas had sales tax rates of 6% and 6.25%. So a significant share of legalized immigrants' income and wage gains were likely returned to treasuries in these states. On top of this, IRCA's legalization appears to have induced Mexican arrivals to decrease their remittances (Amuedo-Dorantes and Mazzolari, 2010) and so may have increased their consumption holding constant income, as has been found in European amnesties (Dustmann, Fasani, and Speciale, 2017).

³⁹ See Bitler and Hoynes (2013) for a discussion of the evidence on immigrants and social programs broadly. Revoking legal status is not necessary to deter take-up of social programs by immigrants; research suggests that aggressive interior enforcement is enough (Alsan and Yang, 2018; Watson, 2014).

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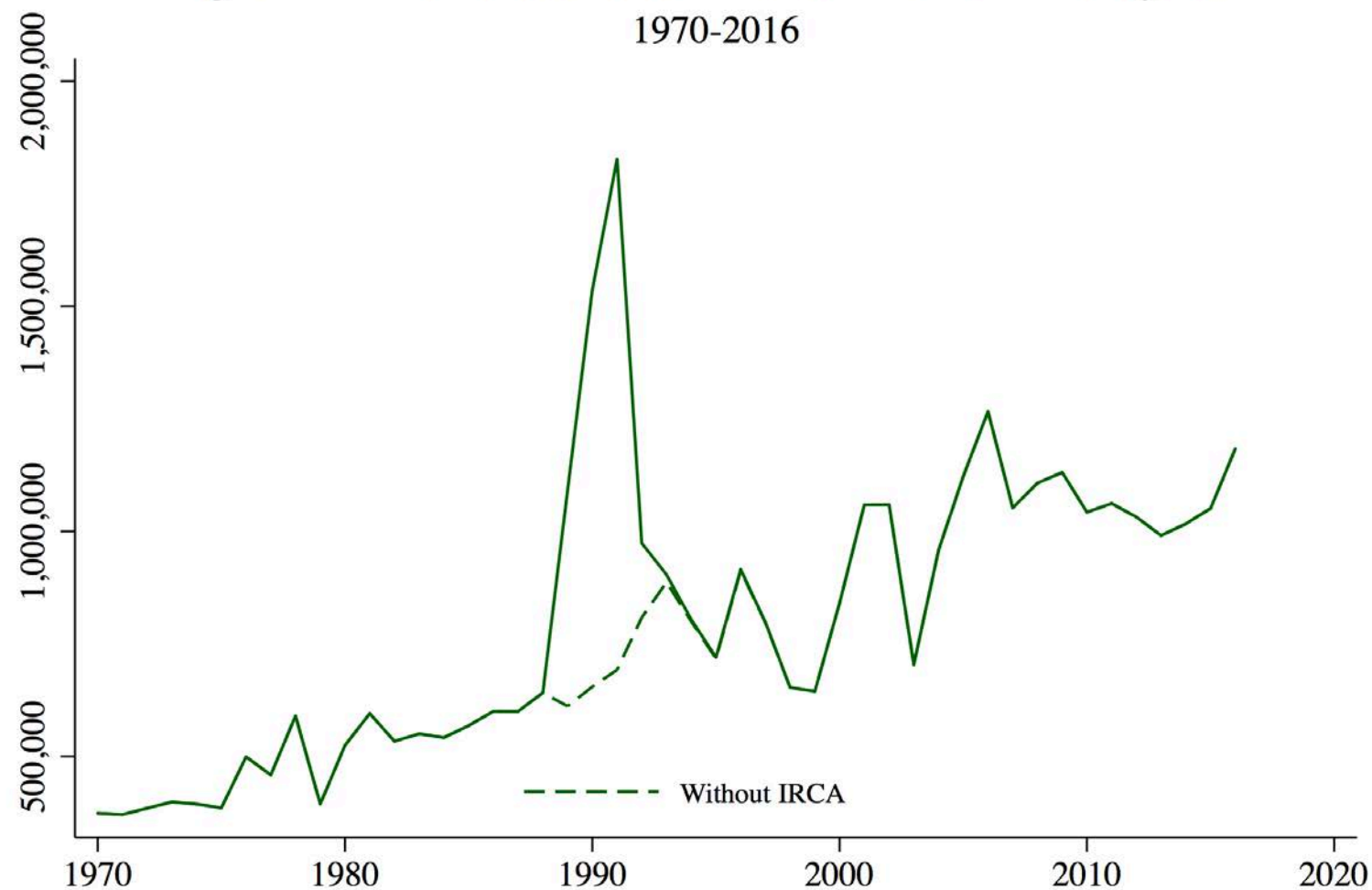
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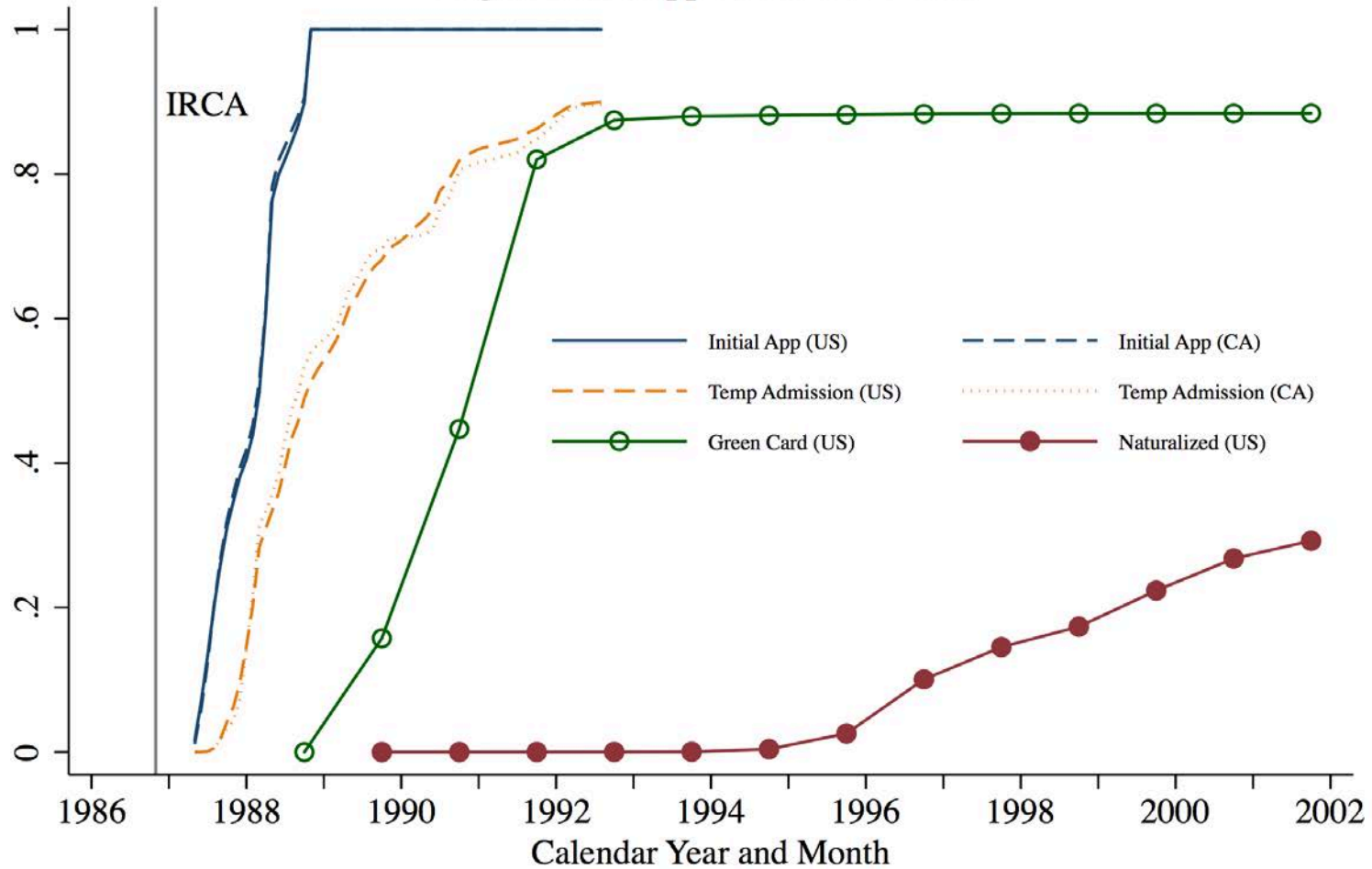
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Figure 1. U.S. Admissions: Green Cards Issued by Year
1970-2016



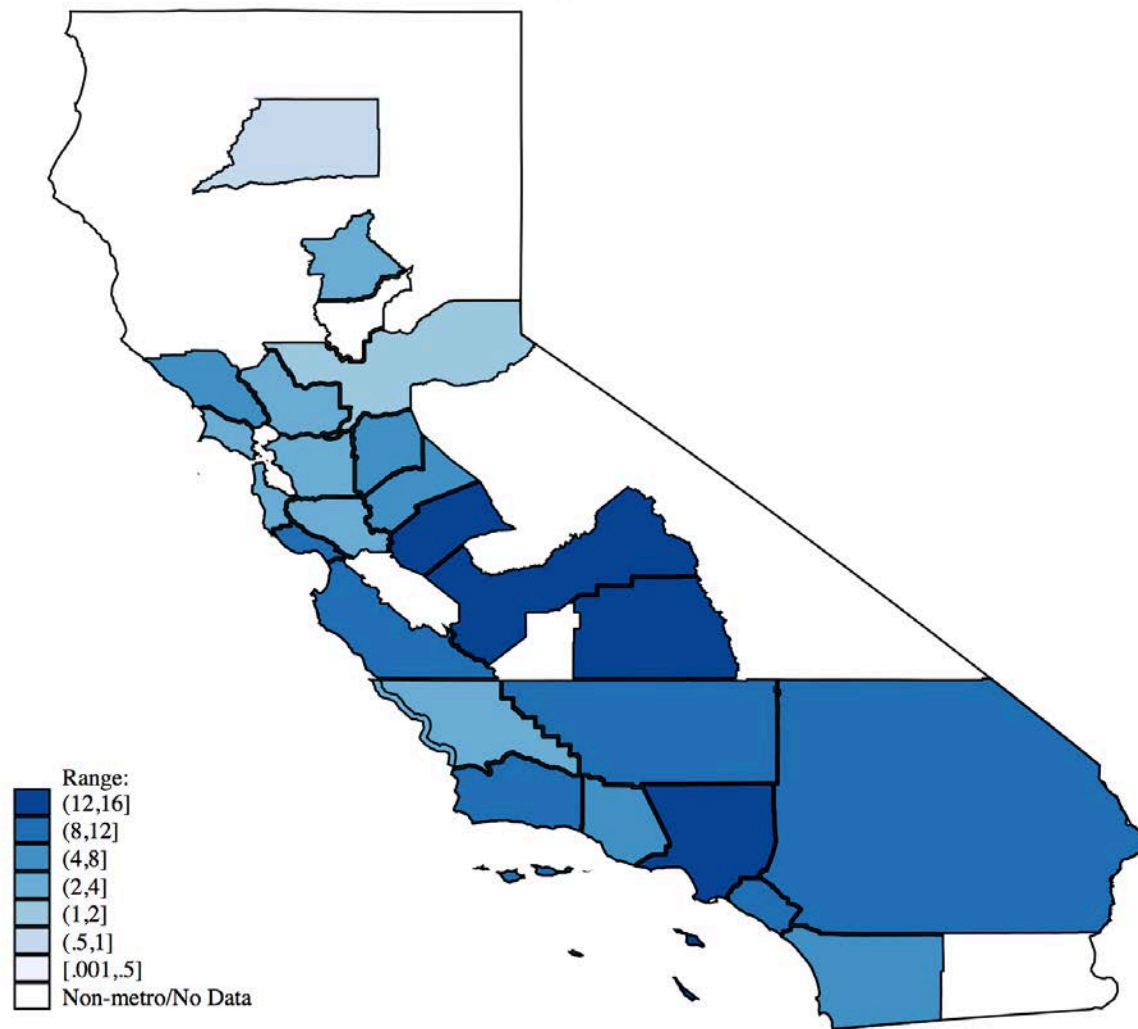
Notes: Data sources: United States Department of Homeland Security (2017), Rytina (2002), and authors' calculations.

Figure 2. Cumulative Legal Status of IRCA
Legalization Applicants Over Time



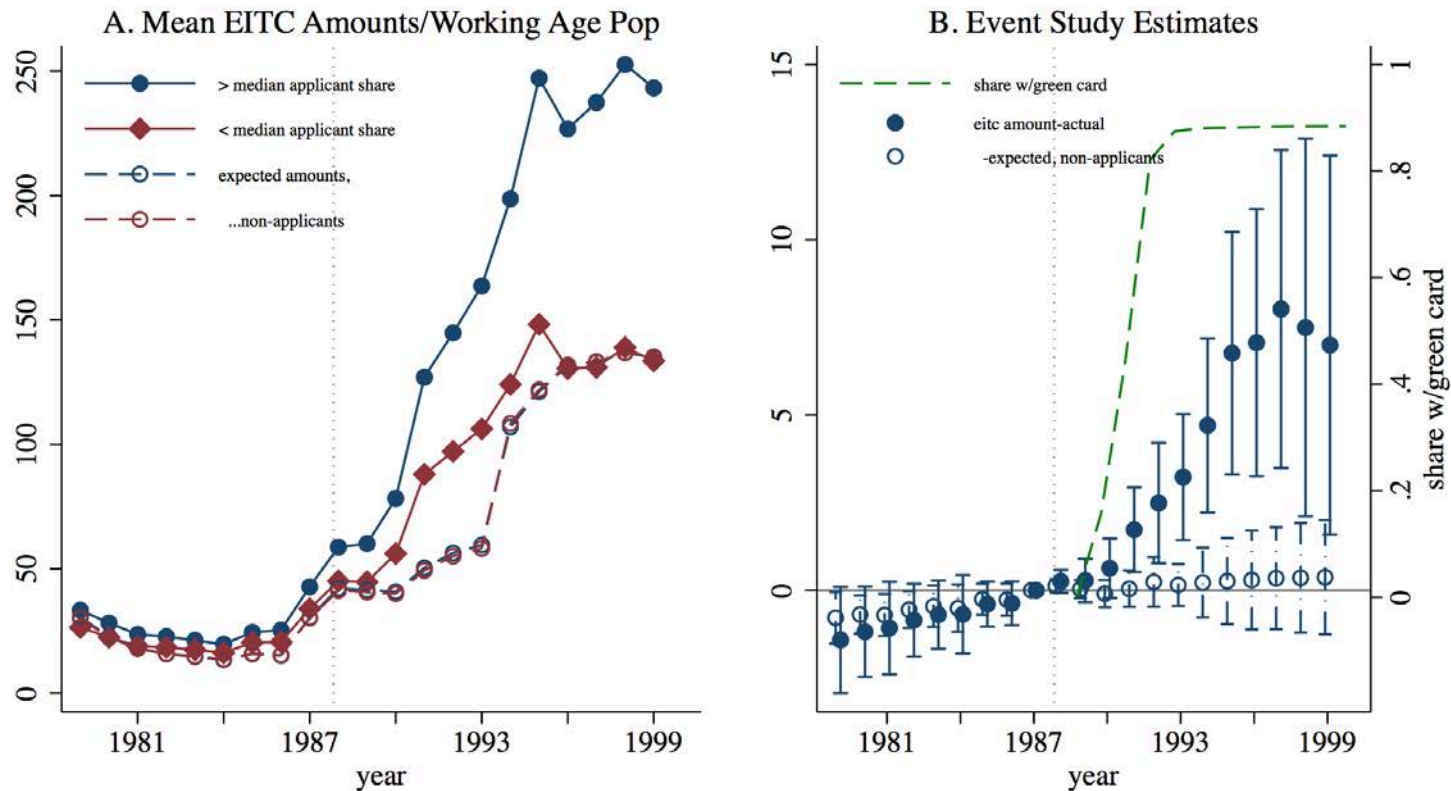
Notes: Numbers of initial applicants and temporary admissions were calculated by the authors at the year and month level from the LAPS data, excluding the 2% of applicants whose application date was not provided. Numbers of permanent residents and naturalized citizens by fiscal year (ending October of calendar year) are from Rytina (2002). To calculate cumulative transitions to permanent residency and citizenship, we divide the published fiscal year figures by the total number of applicants as reported in the LAPS data.

Figure 3. IRCA Legalization Applications, Age 15-64, by California MSA
 % of 1986 Population Aged 15-64



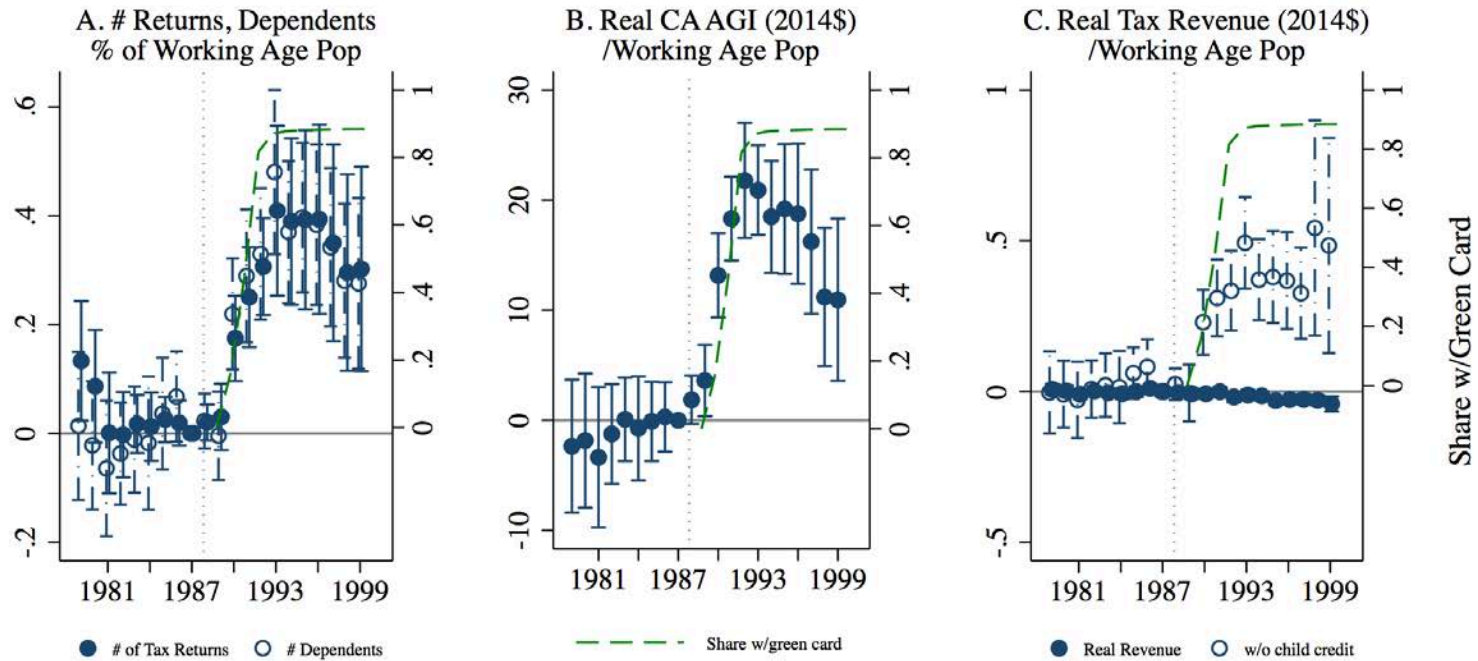
Notes: Map plots the percent of a metropolitan area's 1986 working age population that applied for legal status under IRCA. The number of working-age applicants for legal status was calculated by the authors from the LAPS data, and the 1986 working age population was estimated by the Census Bureau. Working age is defined as ages 15-64 for consistency across the two data sets.

Figure 4. EITC Transfers/Working Age by Applicant Share



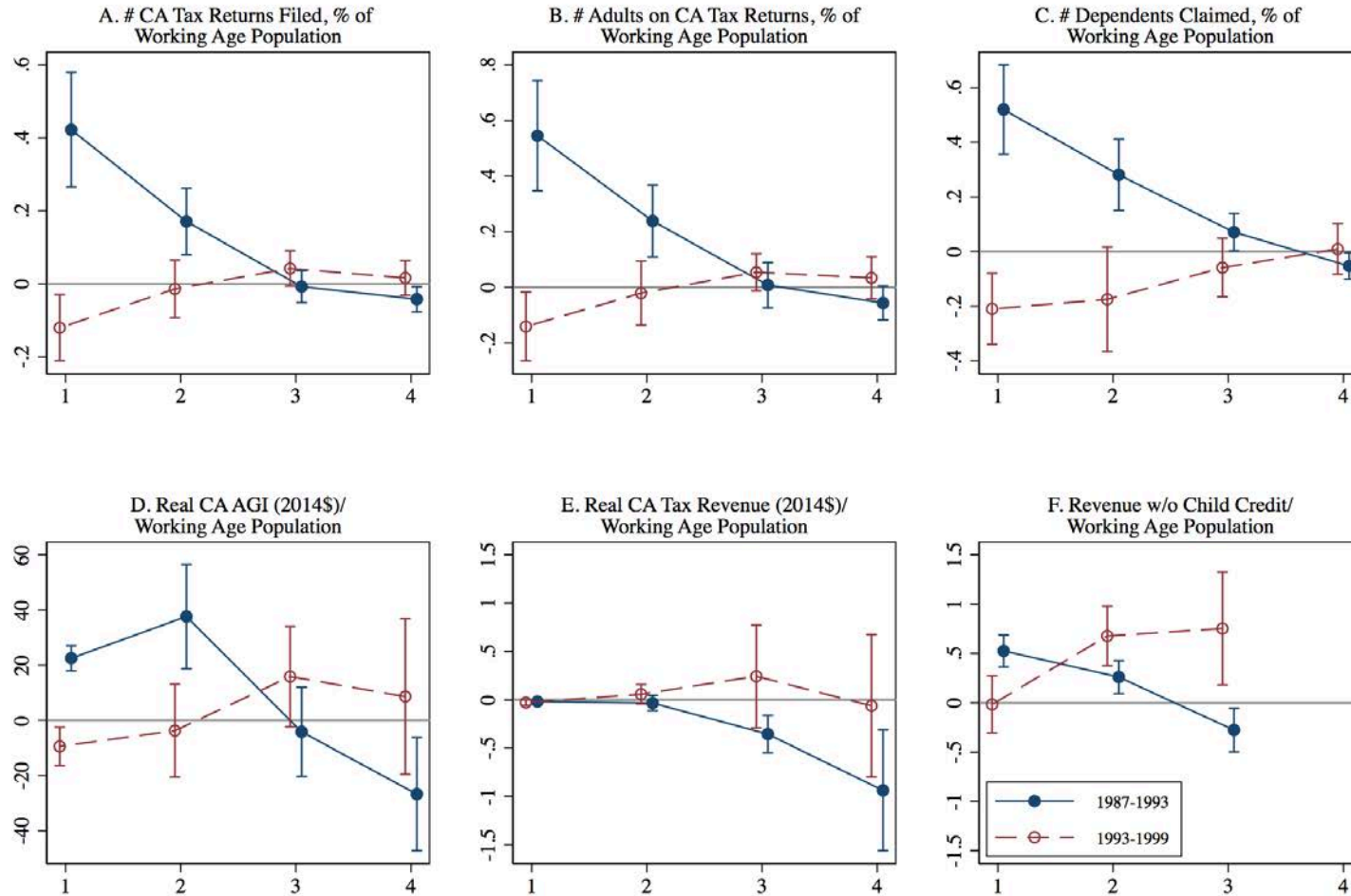
Notes: Panel B shows coefficients (95% confidence intervals) on interactions between applicant share (A_c) and year dummies. All specifications include MSA fixed effects, year fixed effects, employment-to-(working age) population, the ratio of children to (working age) population, and time-varying effects of a Bay Area dummy and the pre-existing MSA characteristics marked with a “†” in Table 2. Interactions with the indicator for 1987 are omitted to identify the model. Specifications are weighted by 1986 working age population, and standard errors are clustered on metro area. Vertical dotted lines at the end of fiscal 1987.

Figure 5. Personal Tax Outcomes Bottom Quintile of 1979 CA AGI



Notes: Figures show coefficients (95% confidence intervals) on interactions between applicant share (A_c) and year dummies. All specifications include MSA fixed effects, year fixed effects, employment-to-(working age) population, the ratio of children to (working age) population, and time-varying effects of a Bay Area dummy and the pre-existing MSA characteristics marked with a “†” in Table 2. Interactions with the indicator for 1987 are omitted to identify the model. Specifications are weighted by 1986 working age population, and standard errors are clustered on metro area. Vertical dotted lines at the end of fiscal 1987.

Figure 6. Impact on CA Income Tax Outcomes, by Quintile of 1979 CA AGI and Year



Notes: Solid lines show long-difference estimates separately by quintile of the 1979 CA AGI distribution of the outcome shown on applicant share (A_c). All specifications include year fixed effects, long differences in employment-to-(working age) population and the ratio of children to (working age) population, and time-varying effects of interactions between the year indicators and each of a Bay Area dummy and the pre-existing MSA characteristics marked with a “†” in Table 2. Specifications are weighted by 1986 working age population, and standard errors are clustered on metro area. Appendix Figure 1 plots pre-trends that are jointly estimated with these coefficients.

Table 1. Demographic and Economic Characteristics of IRCA Applicants for Legal Status in California

Applicant type:	LAW & SAW (1)	LAW (2)	SAW (3)
A. Universe Data (LAPS), at Time of Application			
Age (%):			
Working Age (15-64)	93.2	88.8	99.6
Parent Age (20-44)	74.4	71.6	78.4
Female (%):	33.8	45.0	17.9
Mexican (%)	83.3	77.8	91.3
Hispanic (%)	93.8	93.3	94.6
Number of Applications	1,622,074	956,302	665,772
Number of Working-Age Applications, Analysis Sample	1,448,891	838,516	610,375
B. Survey Data (LPS & NAWS)			
Female (%)		43.3	17.9
Mexican (%)		77.4	99.0
Hourly wage (\$2014), median *		11.4	9.5
Percentile, from CPS**		18.0	11.1
Employed (%) *		79.5	
Hours per Week Employed *		41.2	
Paid Hourly (v. Piece Rate) (%) *			81.3
Hours per Week Paid Hourly *			47.5
High school dropout (%)		74.5	93.8
Years of Schooling		7.8	4.9
Married (%)***		68.1	64.8
Any Children (%)***		82.0	45.9
Two or More Children (%)***		66.9	32.6
Number of Observations		3,146	390

Notes: LAW=Legally Authorized Workers. SAW=Seasonal Agricultural Workers. LAPS=Legalization Applications Processing System. LPS=Legalized Population Survey. NAWS=National Agricultural Workers Survey (FY89). Calculations from the LAPS are based on the universe of applicants who intended to reside in California at the time of application. Calculations from the LPS are based on the 3,146 survey respondents who were age 15-64 and intended to reside in California at the time of application for amnesty; note that the LPS only surveyed LAW applicants who were still in the U.S. and at least age 18 in 1989. Calculations from the NAWS are based on the 390 respondents aged 15-64 who were residing in California and had a pending application for legalization (assumed under the SAW program) at the time of the FY89 survey. * Measured at time of application for legal status in the LPS and at the time of the survey (FY89) in the NAWS. ** Percentile of the California wage distribution in the 1987-88 Merged Outgoing Rotation Group (MORG) files. *** In 1992 for LAW and 1989 for SAW.

Table 2. Pre-IRCA Area Characteristics and their Relationship with Applicant Share, California MSAs

	Means (sd)	Means (sd) by A_c :		Difference	Linear
	All CA MSAs	Above Median	Below Median	(3)-(2) (se)	Slope on A_c (se)*
	(1)	(2)	(3)	(4)	(5)
<i>A. Treatment Variable: Applications/Working Age Population</i>					
All Applications (LAW + SAW), A_c	8.16 (4.29)	11.16 (2.34)	3.72 (1.90)	7.44 (1.32)	
<i>B. 1986 Levels of Key Outcomes: Full Population</i>					
EITC Transfers per working age person (2014\$) [†]	23.5 (5.9)	25.4 (6.3)	20.7 (4.3)	4.7 (2.7)	0.7 (0.3)
California Tax Returns in the Bottom Quintile of the 1979 CA AGI Distribution/Total Working Age Pop:					
Number of Returns x 100	10.5 (1.3)	10.6 (1.2)	10.2 (1.4)	0.4 (0.5)	0.1 (0.1)
Number of Adults on Returns x 100 = (2 x No. of Joint + No. of Single) x 100	12.0 (1.8)	12.3 (1.7)	11.6 (1.9)	0.7 (0.6)	0.1 (0.1)
Number of Dependents x 100 [†]	4.2 (2.0)	5.2 (1.8)	2.7 (1.0)	2.5 (0.5)	0.3 (0.1)
Real Adjusted Gross Income (2014\$) [†]	631.9 (58.5)	650.2 (54.7)	604.7 (55.8)	45.5 (17.6)	6.0 (3.1)
Tax Payments (2014\$)	0.2 (0.1)	0.2 (0.1)	0.2 (0.1)	0.0 (0.0)	0.0 (0.0)
<i>C. % <u>all</u> of Persons Ages Over Age 25 (1980 Census):</i>					
College Equivalents [†]	40.9 (7.4)	38.2 (6.0)	44.8 (7.7)	-6.6 (2.9)	-0.3 (0.2)
No HS Degree [†]	26.4 (5.7)	28.8 (5.4)	22.9 (4.3)	5.9 (2.0)	0.8 (0.2)
<i>D. 1980 Characteristics of Non-Applicants</i>					
1986 Expected EITC Transfers to Non-Applicants (2014\$) /Total Working Age Population	15.3 (3.3)	15.2 (3.3)	15.4 (3.5)	-0.2 (1.5)	-0.2 (0.2)
Number of Families as a % of Total Working Age Pop					
w/ Kids and No Earned Income	1.71 (0.45)	1.77 (0.47)	1.60 (0.43)	0.17 (0.23)	0.03 (0.03)
w/ Kids and Earning \$1-\$4,999	1.52 (0.34)	1.50 (0.34)	1.55 (0.37)	-0.05 (0.15)	-0.03 (0.02)
w/ Kids and Earning \$5,000-\$9,999	1.97 (0.44)	2.01 (0.44)	1.91 (0.46)	0.10 (0.21)	-0.02 (0.02)
w/ Kids and Earning \$10,000-\$14,999 [†]	24.22 (4.44)	24.71 (4.49)	23.51 (4.53)	1.20 (2.10)	-0.22 (0.24)
w/o Kids and No Earned Income	13.56 (2.25)	13.20 (2.00)	14.09 (2.58)	-0.89 (1.09)	-0.06 (0.09)
w/o Kids and Earning \$1-\$4,999 [†]	7.78 (1.68)	7.22 (1.35)	8.61 (1.84)	-1.38 (0.47)	-0.15 (0.09)
# of Metro Areas	23	11	12	23	23
Control for Bay Area Dummy?	No	No	No	No	Yes

Notes: Authors' calculations from the Legalization Application Processing System and Census estimated population (Panel A); the BEA's Local Area Personal Current Transfer Receipts (+1986 Census population estimates) and CAFTB statistics (Panel B); the 1980 Census tabulations (Panel C, from NHGIS), and 1980 Census PUMS (Ruggles et al., 2015; Panel D). Means and regressions are weighted by Census estimates of 1986 area working-age population. Dollar figures in the 1980 Census are nominal (1979\$). The median California MSA has an applicant share of 6.98. Standard errors in columns 4 and 5 are heteroskedasticity-robust. [†]Time-varying effects of these variables are included in subsequent models. *Slope on working age applications/working age population (A_c) in a linear regression of each characteristic on A_c and a dummy for Bay Area = Oakland, San Francisco, San Jose, Santa Cruz, Santa Rosa and Vallejo.

Table 3. Baseline Long-Difference Estimates for Low-Income Tax Outcomes: California MSAs

	Dependent variable is long difference in:							
	Count per total working age person (x 100)			Amount per working age person (\$2014)			Amount per working age person (\$2014)	
	In First Quintile of 1979 CA AGI Distribution						EITC Transfers	
	#CA Tax Returns	#Adults on CA Tax Returns ⁺	#of Dependents	CA Adjusted Gross Income	CA Income Tax Assessed	CA Tax Gross of Child Credit	Actual	Expected, Non-Applicants ⁺⁺
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. Pre-IRCA (1981 to 1987)							
Applicant Share (A _c)	-0.018 (0.064)	-0.027 (0.084)	0.062 (0.091)	4.3 (4.0)	0.009 (0.011)	0.021 (0.085)	0.360 (0.554)	0.400 (0.264)
	B. Short Term (1987 to 1993)							
Applicant Share (A _c)	0.422 (0.075)	0.545 (0.095)	0.52 (0.079)	22.5 (2.2)	-0.018 (0.010)	0.526 (0.078)	3.266 (0.733)	0.146 (0.186)
	C. Medium Term (1993 to 1999)							
Applicant Share (A _c)	-0.120 (0.043)	-0.140 (0.059)	-0.210 (0.063)	-9.4 (3.3)	-0.028 (0.015)	-0.016 (0.139)	3.271 (1.884)	0.011 (0.500)
N (metro area x year)	69	69	69	69	68	69	69	69
P-values on F-tests								
1981-87 = 1987-93	0.000	0.000	0.000	0.000	0.027	0.000	0.000	0.253
1987-93 = 1993-99	0.000	0.000	0.000	0.000	0.638	0.000	0.997	0.696

Notes: All regressions are estimated on stacked long-difference data (for 1981-87, 1987-93, and 1993-99), and coefficients shown are those on interactions between MSA applicant share and year dummies. All regressions include year fixed effects, long differences in the ratios of total employment and number of children to the working age population, and time-varying effects of being in the Bay Area and the pre-IRCA MSA characteristics marked with a “†” in Table 2. Standard errors are clustered on metro area, and regressions are weighted by 1986 working-age population. +Number of joint returns + 2 x number of single returns. ++Non-applicants defined as citizens and non-citizens not from Mexico or Central America. Expected EITC created by applying year-specific EITC program rules to non-applicants in the 1980 Census PUMS (Ruggles et al., 2015).

Table 4. Implied Effect of Permanent Residency on Tax Outcomes

	CA Tax Outcomes, Combining AGI Quintiles 1-4						EITC Transfers	
	Number per working age person			Amounts per working age person, 2014\$				
	#of Tax Returns	#of Adults ⁺	#of Dependents	AGI	Income Tax	Tax Gross of Child Credit	Actual	Expected, Non-Applicants ⁺⁺
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Short-Term (-1993)	0.618	0.837	0.931	\$3,328.01	-\$152.83	\$58.11	\$371.15	\$16.58
	(0.120)	(0.169)	(0.190)	(2323.24)	(36.02)	(20.97)	(83.32)	(21.11)
Medium-Term (-1999)	0.532	0.755	0.438	\$4,618.10	-\$129.01	\$218.88	\$742.85	\$17.87
	(0.192)	(0.297)	(0.299)	(4207.64)	(44.67)	(44.96)	(288.32)	(76.50)

Notes: In columns 1-6, the short-term effect is the sum of 1987 to 1993 coefficients from quintiles 1-4, shown in the corresponding panel of Figure 6, divided by 0.88 (reflecting the LPR transition rate). Medium-term effect is the sum of the 1987 to 1993 and 1993 to 1999 coefficients from Figure 6, also divided by 0.88. Short-term effects in columns 7 and 8 are the 1987 to 1993 coefficient in columns 7 and 8, respectively, of Table 4, and medium-term effects are the sum of the 1987 to 1993 and 1993 to 1999 coefficients, again divided by 0.88. All dollar outcomes are all also multiplied by 100. ⁺Number of joint returns + 2 x number of single returns. ⁺⁺Non-applicants defined as citizens and non-citizens not from Mexico or Central America. Expected EITC created by applying year-specific EITC program rules to non-applicants in the 1980 Census PUMS (Ruggles et al., 2015).

Table 5. Linear Relationship Between Pre-Existing Area Characteristics and Policy Intensity, by Geography

geographic variation:	Slope on A_c		
	CA MSAs (1)	FL+TX MSAs (2)	State-Level, no CA (3)
<i>A. 1986 Levels of Key Outcomes: Full Population</i>			
EITC Transfers per working age person (2014\$) [†]	0.9 (0.2)	1.6 (1.0)	1.7 (0.8)
<i>B. % <u>all</u> of Persons Ages Over Age 25 (1980 Census):</i>			
College Equivalents [†]	-0.8 (0.3)	-0.2 (0.6)	1.7 (0.5)
No HS Degree [†]	0.9 (0.1)	1.0 (0.6)	-0.5 (0.9)
<i>C. 1980 Characteristics of Non-Applicants</i>			
1986 Expected EITC Transfers to Non-Applicants (2014\$)	0.0	0.4	0.0
/Total Working Age Population	(0.2)	(0.7)	(0.6)
Number of Families as a % of Total Working Age Pop			
w/ Kids and No Earned Income	0.04 (0.02)	0.04 (0.04)	-0.10 (0.06)
w/ Kids and Earning \$1-\$4,999	0.00 (0.02)	0.03 (0.06)	-0.02 (0.05)
w/ Kids and Earning \$5,000-\$9,999	0.02 (0.03)	0.05 (0.10)	0.06 (0.09)
w/ Kids and Earning \$10,000-\$14,999 [†]	0.18 (0.26)	-0.33 (0.49)	-0.11 (0.71)
w/o Kids and No Earned Income	-0.05 (0.09)	-0.21 (0.31)	-0.32 (0.53)
w/o Kids and Earning \$1-\$4,999 [†]	-0.12 (0.04)	-0.54 (0.16)	-0.18 (0.09)
State Fixed Effects?	No	Yes	No
Bay Area Fixed Effect?	No	No	No
#of Areas	23	43	49

Notes. Table shows slope from a regression of the specified characteristic on applicant share at the level of geography given: California metro areas (column 1), Florida and Texas metro areas (column 2), and U.S states except California (column 3). Heteroskedasticity-robust standard errors in parentheses. Time-varying effects of characteristics marked with a dagger are included in Table 6.

Table 6. Long-Difference Estimates of Impact on EITC/Working Age Population: Other Geographies and Controls

	MSAs in California		MSAs in Florida and Texas		U.S. States, Excluding California			
	Expected, Non-		Expected, Non-		Expected, Non-		Returns	
	Amount	Applicants ⁺⁺	Amount	Applicants ⁺⁺	Transfers	Applicants ⁺⁺	Total Amount	Claiming x 100
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Pre-IRCA (1981 to 1987)								
Applicant Share (A_c)	0.496 (0.276)	-0.157 (0.075)	1.036 (0.345)	0.009 (0.190)	1.457 (1.049)	-0.142 (0.166)	0.688 (0.953)	0.072 (0.074)
B. Short Term (1987 to 1993)								
Applicant Share (A_c)	4.248 (0.376)	-0.067 (0.109)	3.439 (1.423)	0.710 (0.468)	4.572 (1.151)	-0.325 (0.357)	6.629 (1.356)	0.331 (0.060)
C. Medium Term (1993 to 1999)								
Applicant Share (A_c)	4.009 (0.844)	-0.420 (0.293)	6.678 (4.305)	0.249 (0.928)	-0.802 (2.760)	-1.147 (0.514)	0.509 (3.149)	-0.124 (0.121)
N (metro area x year)	69	69	129	129	147	147	147	147
P-values on F-tests								
1981-87 = 1987-93	0.000	0.439	0.059	0.086	0.049	0.430	0.000	0.000
1987-93 = 1993-99	0.706	0.102	0.282	0.481	0.067	0.041	0.068	0.006
D. Implied Effects of Permanent Residency								
Short term (-1993)	\$482.73 (42.73)	-\$7.61 (12.39)	\$390.84 (161.67)	\$80.70 (53.15)	\$519.49 (130.76)	-\$36.90 (40.61)	\$753.32 (154.05)	0.38 (0.07)
Medium term (-1999)	\$938.28 (130.41)	-\$55.29 (44.35)	\$1,149.66 (645.72)	\$109.02 (149.81)	\$428.34 (353.22)	-\$167.29 (90.24)	\$811.17 (405.96)	0.24 (0.12)

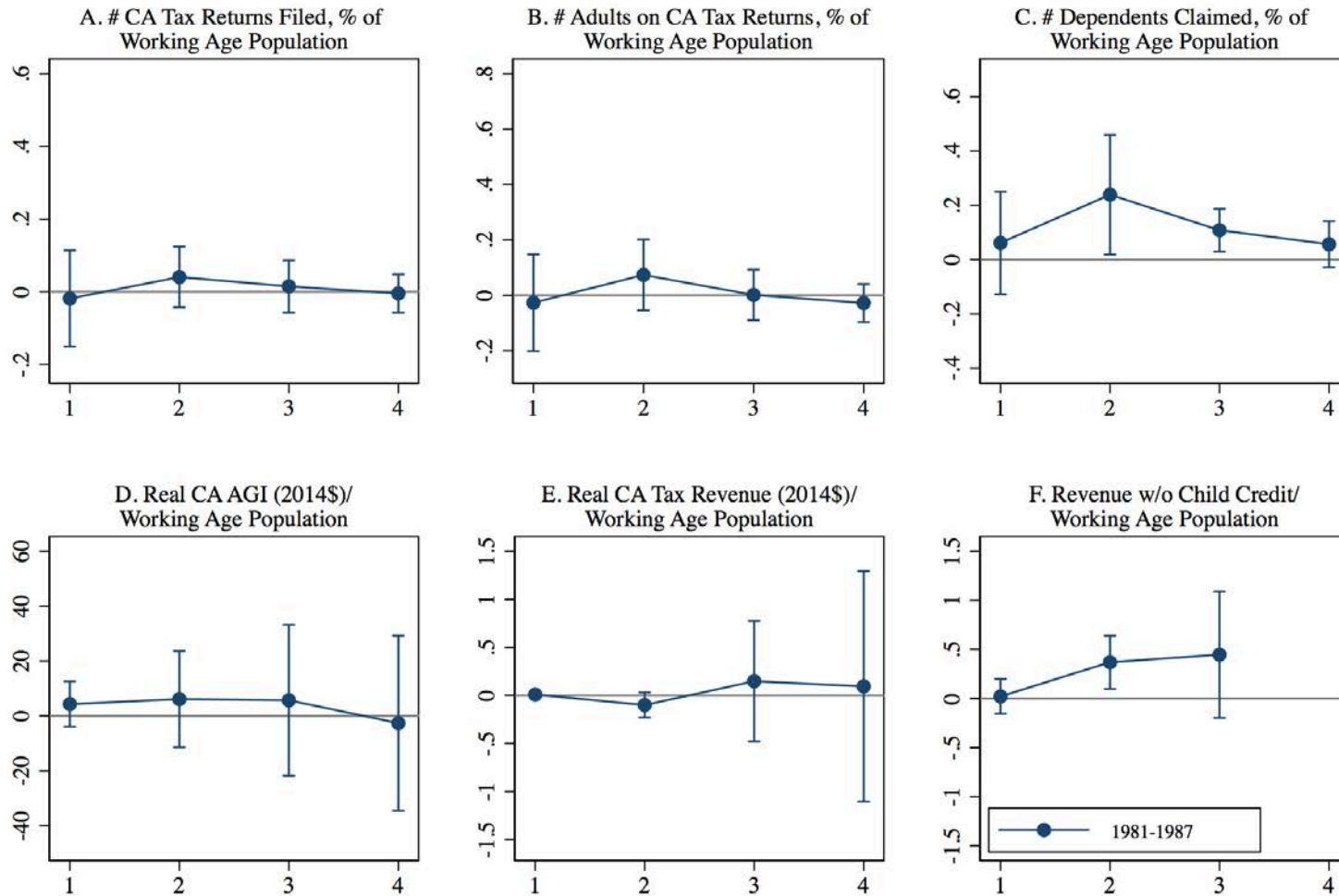
Notes: All regressions are estimated on stacked long-difference data (for 1981-87, 1987-93, and 1993-99), and coefficients shown are those on interactions between MSA applicant share and year dummies. All regressions include year fixed effects, long differences in the ratios of number of children, and total employment to the working age population (in columns 4-6 this is replaced with the unemployment rate), and time-varying effects of pre-IRCA MSA characteristics marked with a “+” in Table 5. Columns 3-4 also unrestricted state x year effects. Standard errors are clustered on metro area (columns 1-4) or state (columns 5-8), and regressions are weighted by 1986 working-age population. . ++Non-applicants defined as citizens and non-citizens not from Mexico or Central America. Expected EITC created by applying year-specific EITC program rules to non-applicants in the 1980 Census PUMS (Ruggles et al., 2015).

Table 7. Alternative Approach: Using New York Metro Areas to Account for Direct Effects of Hispanic Share

	Fully Interacted Fixed Controls Only*			With Time-Varying Controls	
	CA	NY	Difference	Actual EITC	Expected EITC, Non-Applicants ⁺
	(1)	(2)	(3)	(4)	(5)
A. Dep. Var.: Applicant Share (A_c)					
Hispanic Share (H_c)	0.597 (0.051)	0.174 (0.007)	0.423 (0.052)	**	**
B. Dep. Var.: Δ EITC Transfers/Working Age					
1. Pre-IRCA (1981 to 1987)					
Hispanic Share (H_c)	0.173 (0.054)	-0.070 (0.133)	0.244 (0.117)	-0.168 (0.403)	0.045 (0.081)
2. Short-term (1987 to 1993)					
Hispanic Share (H_c)	2.521 (0.272)	-0.100 (0.338)	2.621 (0.392)	3.116 (0.908)	-0.034 (0.242)
3. Medium-term (1987 to 1993)					
Hispanic Share (H_c)	2.768 (0.468)	-1.573 (0.815)	4.342 (0.806)	4.516 (0.963)	0.088 (0.484)
N (metro area x year)	69	33	102	102	102
P-values on F-tests					
1981-87 = 1987-93	0.000	0.913	0.000	0.006	0.738
1987-93 = 1993-99	0.411	0.033	0.004	0.050	0.755
C. Implied Effects of Permanent Residency					
Short term (-1993)	-	-	\$703.53 (107.11)	\$448.76 (199.84)	-\$12.96 (47.27)
Medium term (-1999)	-	-	\$1,868.85 (330.70)	\$1,526.69 (329.85)	\$9.27 (129.00)

Notes: *Fixed controls are those marked with a “†” in Table 5. ** With the inclusion of time-varying controls (employment/population and children/working age population), we stack all of the differences and estimate the model jointly, and so the relationship between Hispanic share and applicant share in California relative to New York becomes year-varying. Coefficient estimates between year-specific applicant share and year-specific Hispanic share in California are in the 0.4-0.5 range. To obtain implied effects of permanent residency, we estimated model 3 on the combined California-New York data using instrumental variables, where the instruments were (year-specific) interactions between 1980 Hispanic share and a dummy for California (conditional on direct, time-varying effect of Hispanic share). We then scaled the IV estimates by multiplying by 100 and dividing by the estimated LPR transition rate, 0.88, as in earlier tables. +Non-applicants defined as citizens and non-citizens not from Mexico or Central America. Expected EITC created by applying year-specific EITC program rules to non-applicants in the 1980 Census PUMS (Ruggles et al., 2015).

Appendix Figure 1. Impact on 1981-87 CA Income Tax Outcomes



Notes: Figures show pre-trends (1981-87 effects) for model jointly estimated with coefficients shown in Figure 6. Solid lines show long-difference estimates separately by quintile of the 1979 CA AGI distribution of the outcome shown on applicant share (A_c). All specifications include year fixed effects, long differences in employment-to-(working age) population and the ratio of children to (working age) population, and time-varying effects of interactions between the year indicators and each of a Bay Area dummy and the pre-existing MSA characteristics marked with a “†” in Table 2. Specifications are weighted by 1986 working age population, and standard errors are clustered on metro area.

On-line Appendices for

**Distributing the Green (Cards):
Permanent Residency and the Income Tax after
the Immigration Reform and Control Act of 1986**

by

Elizabeth U. Cascio
Ethan G. Lewis

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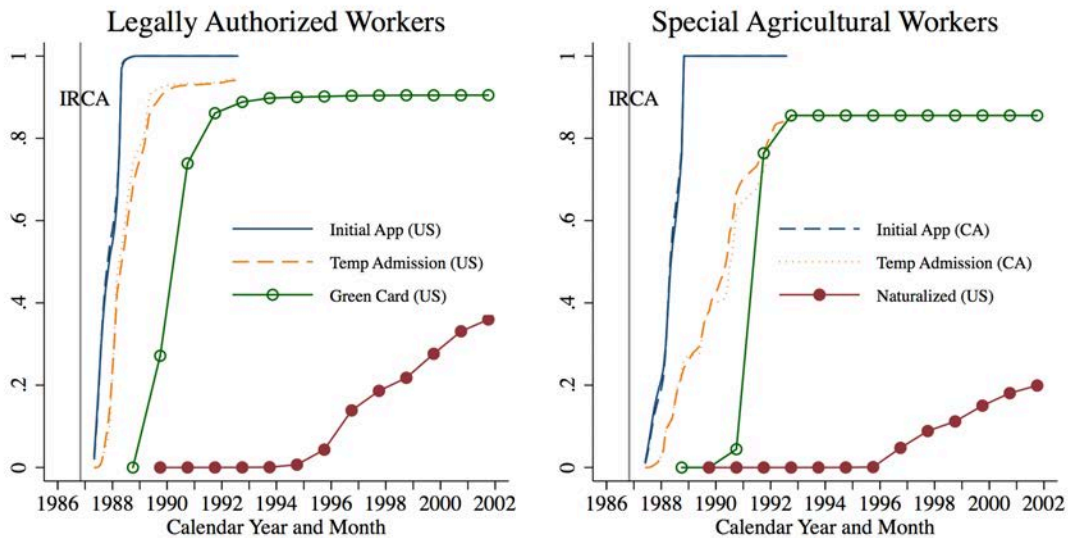
(Not for journal publication)

Appendix A: Data Sources (For On-line Publication Only)

1. Legalization Applications Processing System (LAPS)

Our main measure of policy intensity and Figures 2 and 3 and Table 1 use, in whole or in part, statistics compiled from the Legalization Applications Processing System (LAPS), available from the National Archives. These public-use microdata consist of selected fields from anonymized records from all forms I-687 (application for temporary legal status under IRCA's LAW program) and forms I-700 (application for temporary legal status under IRCA's SAW program) received by the Immigration and Naturalization Service, consisting of 3,040,948 records in total. These fields describe some outcomes of the application process, including the month and year of application for temporary status and of decision on temporary status, through the end of fiscal year 1992; Figure A1 shows trends in application status for LAWs and SAWs separately based on this information and statistics from Rytina (2002).

Figure A1. Cumulative Legal Status of IRCA
Legalization Applicants Over Time, by Application Type



LAPS data identify the state and county of “intended residence” (current U.S. address) of the applicant at the time of application, imputed from the zip code of intended residence. County is suppressed for applicants in counties with under 100,000 population (as of the 1990 census) or with fewer than 25 applications.

Table A1. States with Highest Population Shares of Working-Age LAW and SAW Applicants

	Applicants/ Pop Aged	% of All US LAW & SAW	<u>% of States' Applicants:</u>			<u>Metro Areas</u>	
State	15-64	Applicants	LAW	In Identified County	In Identified Metro Area	Number ID'd in State	Applicants/Pop Std. Dev.
California	8.21	53.1	56.2	98.1	95.8	23	4.29
Texas	3.78	14.6	66.6	82.2	82.2	24	2.43
Arizona	3.62	2.75	31.8	92.0	85.2	3	4.48
Nevada	2.82	0.67	52.1	83.7	83.7	2	0.07
New Mexico	2.67	0.89	54.0	54.1	54.1	2	3.46
Illinois	1.99	5.25	73.9	98.3	98.1	11	1.33
Florida	1.94	5.07	30.4	88.7	88.3	19	1.94
Idaho	1.57	0.34	18.6	1.7	1.7	1	0.00
Oregon	1.51	0.92	13.3	64.0	64.0	4	1.06
New York	1.39	5.90	67.0	99.1	98.9	11	1.21
Washington	1.20	1.26	22.7	57.9	57.9	9	2.48

Notes: Authors' calculations from the LAPS data and Census population estimates for 1986.

Table A1 shows some statistics on our measure of policy intensity – the ratio of working age applicants to 1986 working age population, times 100 (“applicant share”) – for the 11 top applicant-generating states; Figure A2 shows applicant share for all states graphically. Each of the 11 top states had an applicant share of over 1 percent, and together they represented nearly 91% of all applications. California, location of the majority (53.1%) of applicants and the focal state of our analysis, had the highest applicant density, with applicants representing 8.21% of its working-age population. Texas was a distant second, home to 14.6% of working-age applicants representing 3.78% of the state’s working-age population. In both states, as in the country as a whole, a majority of applications came through the LAW program. Applicants in Florida, the second state to Texas in our supplemental within-state analysis, were more skewed towards farmworker applications (SAWs), at only 30% LAW.

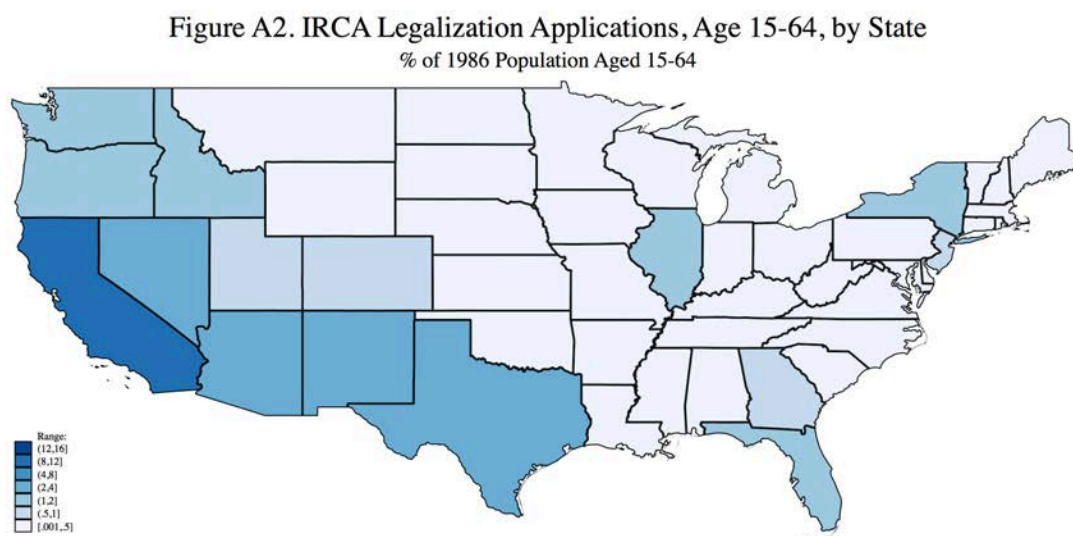
Our main cross-sectional unit of analysis is a metropolitan area (metro area, or MSA), which is a collection of counties. We relied on the 1990 definition of metropolitan areas with one exception.¹ Geographic suppression in the LAPS means that we lose a handful of smaller metropolitan areas,² but columns 4 and 5 of Table A1 show that the vast majority of applications that are in identified counties are also in identified metropolitan areas. In California, 95.8% of applications are in a metropolitan area we can identify in the LAPS, out of the 98.1% of applications in identified counties in the state. Thus, suppressed counties not only contain a small share of applications, but are also rural and so would not be in our analysis anyway.

The final two columns of Table A1 show the variation in applications per capita within each state. California, Texas, and Florida are particularly well-suited for within-state analysis, in that they each contain a large number of identifiable metropolitan areas: 23 in California, 24 in Texas, and 19 in Florida. These states also have considerable variation in applicant density across their metropolitan areas. We focus on California because of the quality of the state’s

¹ See <http://www.census.gov/population/metro/files/lists/historical/90mfips.txt>. In California we added San Luis Obispo, which attained status as a metropolitan area shortly after the 1990 Census.

² These include Yuba City (California); San Angelo, Sherman-Denison, and Victoria (Texas), and Fort Walton Beach (Florida).

income tax statistics and a desire to offer as complete an analysis as possible of how large-scale distribution of Green Cards impacts the income tax system.

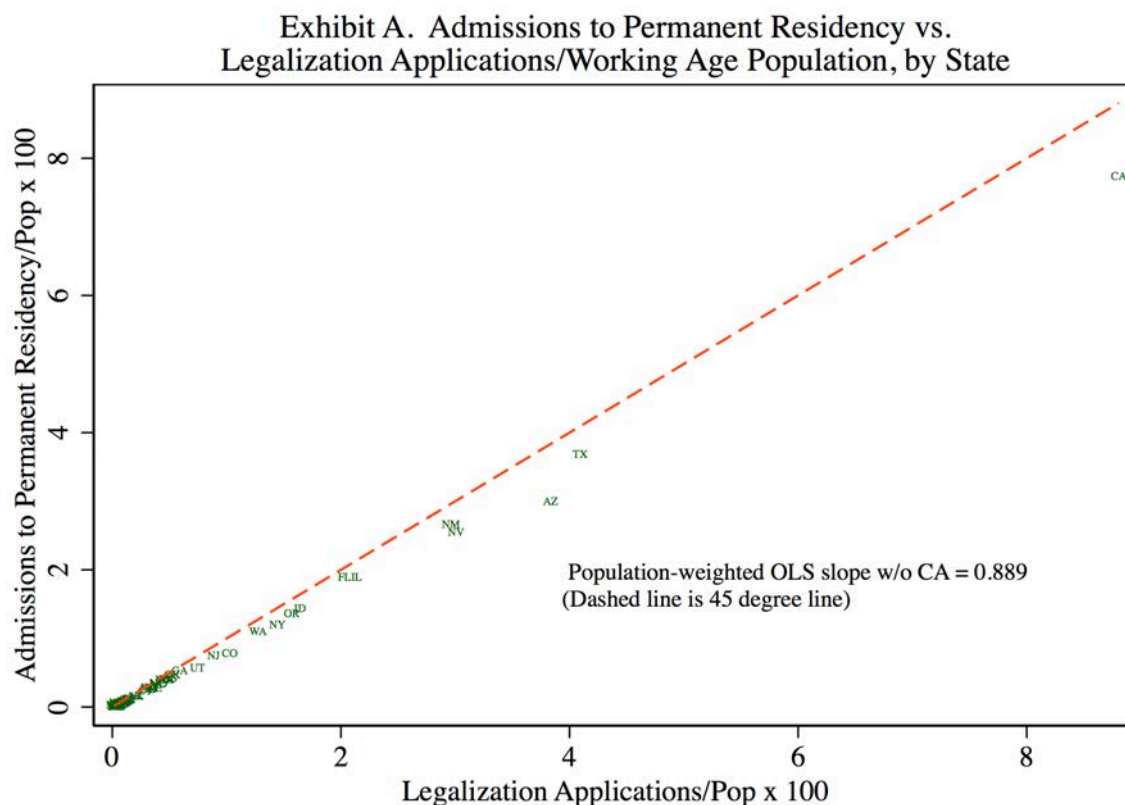


Regarding geography in the LAPS, it is important to note that the relevant county for later phases of the application process is simply not available. This is the main reason that we focus on applicants, rather than legal permanent residents (LPRs), in constructing our policy intensity measure; the other is that using LPRs would more likely lead to an endogenous “treatment.” Under this framing, our econometric model is the reduced-form for a two-stage model, whose unobserved first stage coefficient on applicant share (in predicting LPR share) is less than one since not all applicants become LPRs. To obtain an instrumental variables-like estimate in the paper, we thus divide our reduced-form estimate by 0.88, the fraction of applicants becoming LPRs by 1993. But migration within the U.S., migration out of the U.S., or mortality may make this the wrong scaling factor.

Public-use statistics fortunately allow us to measure state at admission to permanent residency, and thus can provide insight into the appropriateness of dividing by 0.88 to scale our estimates.³ Exhibit A plots LPR admissions per capita (i.e., per 1986 working age population, times 100) against applicant share by state, with a 45° line shown for reference. The points are below the 45° line, as expected since not all applicants become LPRs, but not far below. Indeed, the weighted regression line (weighted by 1986 working age population, and excluding California and the District of Columbia, as in our state-level analysis) gives a slope of 0.889, which is very close to just being the aggregate rate (88.3 percent) at which applicants became LPRs (through

³ In particular, the annual *Statistical Yearbook of the Immigration and Naturalization Service* tabulates the total number of admissions to permanent residency each year by state (U.S. Department of Justice, Immigration and Naturalization Service, 1990, 1991, 1992, 1993b, 1994, 1995, 1995, 1996, 1997). These tables include those who gained legal status and those who came to the U.S. legally through other channels. Another INS data source, *Immigrants Admitted to the United States* (ibid, 1993a, 1993c, 1993d, 2007, 2010a, 2010b) is microdata on all of the individual cases of new admission to permanent residency each fiscal year excluding those who legalized under IRCA. Thus, by tabulating the latter sources to counts of admissions at the area level and subtracting them from the total counts in the former sources, we can obtain counts of legalized immigrants admitted to permanent residency by state. Unlike in the main analysis, though, it is not possible to limit this to the working age population, since the tabulated data are not stratified by age. However, recall that the vast majority of applicants were working age.

1996, the final year we tracked it using the sources listed in the prior footnote).⁴ This suggests very little net reshuffling of the legalized immigrant populations across states through the mid-1990s and that dividing our estimates by 0.88, as we do in the paper, should provide a good approximation of the person-level of impact of obtaining a Green Card.



2. Legalized Population Survey and the National Agricultural Workers Survey

Two other sources are used to construct the descriptive statistics on the legalized population in Table 1 and to calculate “mechanical” EITC transfers for applicants: the Legalized Population Survey (LPS) and the 1989 wave of the National Agricultural Workers Survey (NAWS).⁵ The LPS was a small random sample (N=6,193), initially taken in 1989, of those aged 18 or older who applied for legal status under the LAW program and were still in the U.S., which essentially excludes anyone whose application for temporary legal status was denied by that date. The LPS includes some information directly from respondents’ applications (some the same information that is reported in LAPS), retrospective questions about wages and employment at the time of application, and questions about education and family composition. There was also a follow-up

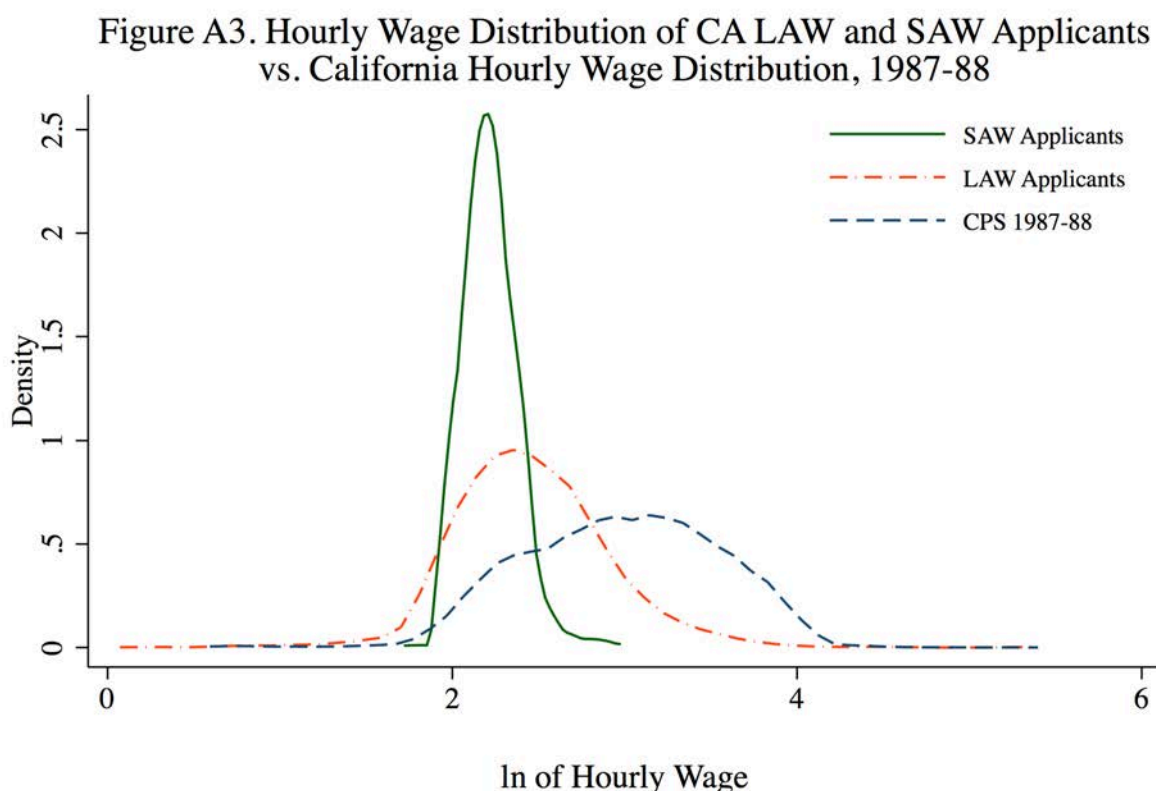
⁴ Although admissions to permanent residency among the IRCA-legalized population continue to trickle in after 1996, the numbers were trivial as a share of the total. In fact, the vast majority of legalization applicants who would ever receive permanent residency had done so by fiscal year 1992 (see, e.g., Figure 2). Specifically, though, Exhibit A excludes the 2,548 applicants who were admitted to permanent residency in fiscal year 1997, and the fewer than 1,000 applicants who were admitted each year after that (United States Department of Homeland Security, 2003).

⁵ The LPS is available on the web at <http://mmp.opr.princeton.edu/LPS/LPSpage.htm>. The NAWS are available at <https://www.doleta.gov/agworker/naws.cfm>.

survey in 1992 that asked many of the same questions about applicants' current labor force status, income, and family. The target population for this follow-up was limited to those applicants who had been admitted as LPRs, dropping the sample size down to 4,012. The survey documentation also says refusals to be surveyed were rare (2% of attempted interviews in the 1992 wave), and the main reason people were not interviewed was because they could not be located (14% of attempted interviews in the 1992 wave). People who had left the state from which they applied could thus be underrepresented in these data.

The LPS does not cover those who applied for legalization under the SAW program. For a sample of SAW applicants, we turned to the (fiscal year) 1989 NAWS. The NAWS is a random sample of workers employed by farms. The NAWS does not ask about IRCA directly, so to proxy for those attempting to legalize under IRCA, we identified the 970 NAWS respondents who still had a pending application for legalization (and who were working age). Note that this excludes the SAW applicants whose legal status had been decided on before fiscal year 1989 (which, according to the LAPS, was about 21% of applicants; another 18% were ruled on by the end of fiscal 1989) or who had left the U.S. farm sector by that time.

Figure A3 shows the hourly wage distributions of respondents applying from California (in the case of the LPS) or residing in California (in the case of the NAWS). The hourly wage distribution of California residents from the Merged Outgoing Rotation Group files of the 1987 and 1998 Current Population Survey (available at NBER) are shown for comparison.



3. California State Income Tax Data

We draw our state income tax outcomes from *Annual Reports* of the California Franchise Tax Board.⁶ We digitized Table 7 of the reports for tax years 1979 to 1999 for this project. This table consistently gives, by county, the number of returns (all, joint), the number of dependents, adjusted gross income (AGI; in thousands), and tax assessed (in thousands) within many mutually-exclusive and exhaustive narrow ranges of AGI. Our main outcomes from these data derive from quintiles of California’s “trimmed” 1979 AGI distribution; we designate the bottom quintile as “low-income,” and so apply a consistent definition of a low-income return over time.

More specifically, we estimate percentile thresholds assuming a uniform distribution of returns within each narrow income bin (e.g., \$1000 ranges at the bottom of the 1979 distribution).⁷ We then calculate aggregate outcomes (e.g., the total number of state returns) by 1979 percentile for later years by first inflating the percentile thresholds and later income ranges to real 2014 dollars, then by assuming a uniform distribution of returns within these ranges. Because the AGI bins have the same nominal values for many years and having distinct minimum and maximum values on each relevant bin is a requirement of this approach, we trim the 1979 AGI distribution at a minimum of \$1 and a maximum of \$40,000 (nominal dollars) to implement it. The maximum amounts to \$130,433 in 2014 dollars, representing the 93rd percentile of the 1979 AGI distribution.

One of the outcomes – “counterfactual” state income tax assessed – requires us to know the formula for California’s child tax credit. This formula is provided for all years in Table 2 of the 2000 *Annual Report*.

4. Other Data Sources

Bureau of Economic Analysis EITC data: Our main measure of EITC transfers is taken directly from Table CA35 (“Personal Current Transfer Receipts”) of the Local Area Personal Income Accounts, published by the Bureau of Economic Analysis.⁸

California Department of Finance Population Estimates: Annual data on the age composition of the population for California metropolitan areas were aggregated from July population estimates detailed by age x race x county in State of California (1998, 2009).⁹ Unless otherwise noted, we use California’s population estimates to construct the MSA-by-time varying controls (the ratio of employment to (working age) population and the ratio of children to (working age) adults).

Census Bureau Population Estimates: Throughout the paper, the denominator of applicant share is MSA population aged 15 to 64 in 1986, computed using the Census Bureau’s county age-sex-race files (U.S. Dept. of Commerce, Bureau of the Census, 1992; available as ICPSR study

⁶ Scans of these reports are available at https://www.ftb.ca.gov/Archive/aboutFTB/annrpt/archive_index.shtml. The reports give statistics with a one-year lag, so we rely on the 1980 through 2000 *Annual Reports*.

⁷ This seems a reasonable enough assumption. For example, the mean and median AGI per return are very similar to each other for all bins in all county-years, consistent with expectations under a uniform distribution.

⁸ Go to <https://www.bea.gov/iTable/iTable.cfm?reqid=70&step=1&isuri=1&acrdn=7#reqid=70&step=1&isuri=1>.

⁹ These annual intercensal population estimates, and some information on how they were estimated, can be found at <http://www.dof.ca.gov/Forecasting/Demographics/Estimates/>.

6031). We also use these population estimates to construct employment-to-(working age) population ratios and the ratio of children to (working-age) adults for all MSAs and for states in Table 6.

Census Public-Use Microdata: We use the 1980 Census 5% PUMS (Ruggles, et al., 2015) to construct shares of “likely non-applicant” families with and without children with family earnings in various ranges (Table 2) and to construct the expected EITC for non-applicants. We use all adults in these calculations, and non-applicants consist of citizens and non-citizens not from Mexico or Central America. We also use a sample of “likely applicants” from the 1980 Census 5% PUMS (i.e., non-citizens from Mexico and Central America) to refine our family earnings estimates for the calculation of “mechanical” EITC transfers, in Section 4 of the paper.

Census Tabulations: We calculate the 1980 MSA-level Hispanic population shares using STF-1 (100%) counts of Spanish origin and total population at the county level, published by the National Historical Geographic Information System (NHGIS) (Minnesota Population Center, 2011). For the education composition variables (share high school dropout and share college equivalent; Table 2), we use the STF-3 long-form sample counts of persons ages 25 and over by years of school completed at the county level, also published by NHGIS.¹⁰ The number of college equivalents is calculated as the sum of people with (at least) four years of college education, plus one half of the number of people with 1-3 years of college education.

County Business Patterns: The numerator of the MSA employment-to-(working age) population ratio is derived from County Business Patterns data spanning 1979 to 1999 and available on ICPSR or (in the case of 1997 and 2000) the Census Bureau’s website.¹¹ Our employment measure is County Business Patterns total employment.

Earned Income Tax Credit Schedules: EITC schedules are from the Tax Policy Center (<http://www.taxpolicycenter.org/taxfacts/displayafact.cfm?DocID=36&Topic2id=40&Topic3id=42>). The evolution of key EITC parameters over our sample period is shown in Figure A4.

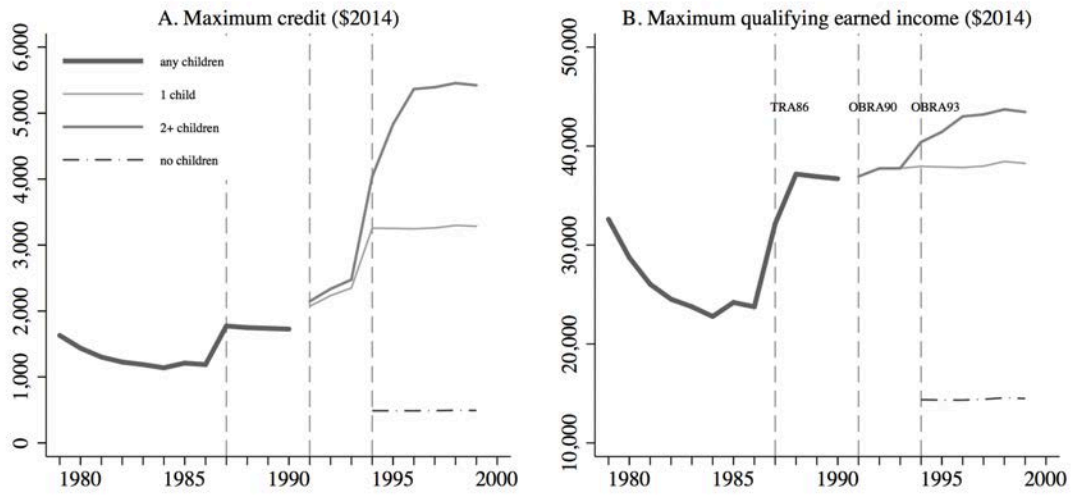
IRS Statistics of Income: We draw two state-level EITC measures – the number of EITC claims and the total EITC amounts (including refunds and reductions in positive tax liability) – from published data from IRS Statistics of Income. We digitized this information from Table 2 of the *SOI Bulletin*, which provides it for 1983 forward with a two-year lag in fall issues through 1995 (for 1993) and in the spring 1996 and 1997 issues (for 1994 and 1995) (U.S. Department of the Treasury, various).¹² For tax years 1996 and later, the tables are available for direct download (in Excel format from 1997 forward) at <https://www.irs.gov/statistics/soi-tax-stats-historic-table-2>.

¹⁰ The NHGIS identifiers for these tables are ds104 and NT8 (Spanish origin) and ds107 and NT48A (education).

¹¹ There are various ICPSR study numbers: 8442 (1979), 8142 (1980), 8348 (1981), 8360 (1982), 8433 (1983), 8665 (1984), 8883 (1985), 9198 (1986), 9381 (1987), 9711 (1988), 9740 (1989), 6030 (1990), 6382 (1991), 6488 (1992), 2343 (1993), 2280 (1994), 2406 (1995), 2701 (1996), 3087 (1998), and 3439 (1999). The data for 1997 and 2000 were downloaded from <https://www.census.gov/programs-surveys/cbp/data/tables.All.html>.

¹² Scans of *SOI Bulletins* are available at <https://www.irs.gov/statistics/soi-tax-stats-soi-bulletins>.

Figure A4. Changes in EITC Parameters Over Time



Appendix B: Supporting the Person-Level Interpretation of our Estimates (For On-Line Publication Only)

One interpretation given to the slope estimate on A_c in the long-difference model is that of a person-level causal impact of becoming a permanent resident. This interpretation relies on several assumptions: (1) Our regressions aggregate a person-level regression model; (2) There is no effect of Green Cards distributed in an area on non-applicants that is proportional to applicant share; and (3) Increases in A_c translate into increases in the LPR share at a rate equal to the national average. We addressed (3) in Appendix A. In this Appendix, we demonstrate (1) and consider the theoretical and empirical evidence in support of (2).

1. Derivation of the Estimating Equation from an Individual-Level Model

A randomized controlled trial (RCT) would be the ideal way to estimate the effects of obtaining a Green Card. In an RCT, Green Cards would be randomly assigned among unauthorized individuals who apply for them. The model of interest would be given by:

$$(B1) \quad y_{ict} = \alpha_t + \theta_t G_i + \varepsilon_{ict},$$

where y_{ict} represents an outcome of interest (e.g., EITC transfers) for working age person i in MSA c in year t , G_i is an indicator for whether i is randomly assigned a Green Card in t^* , and ε_{ict} captures unobservables. With the study sample limited to unauthorized immigrants and Green Cards randomly assigned, it should be the case that $\theta_t = 0$ for all years $t < t^*$. That is, on average, there should be no difference in outcomes between the treatments and controls before Green Cards are distributed. After legalization, or for $t \geq t^*$, θ_t then captures the causal impacts of having a Green Card.

The identification strategy afforded by variation from IRCA is difference-in-differences, which can be represented by the model:

$$(B2) \quad \Delta y_{ic} = \tilde{\alpha} + \tilde{\theta} G_i + \Delta \varepsilon_{ic},$$

The coefficient of interest is now $\tilde{\theta}$, which captures differential trends in outcomes between those who received Green Cards versus those who did not. Unlike model B1, identification in model B2 does not rely on an assumption of identical expected outcomes. Instead, estimates will be unbiased if the trend in outcomes for the comparison group, represented by $\tilde{\alpha}$, is an accurate representation of what would have happened for the legalized population absent the status change.¹³

In our context, a lack of existing microdata that identify both whether an immigrant has a Green Card and the outcomes of interest precludes estimation of model B2. Taking averages of model B2 at the area-by-year level (across *all* working age people) then differencing over time yields a model closer to our long-difference specification:

¹³ This approach has been taken in past studies of the wage and employment impacts of IRCA's legalization, but for LAW applicants only and using native-born Hispanics in the NLSY79 as the comparison group (Kossoudji and Cobb-Clark, 2002; Amuedo-Dorantes, Bansak, and Raphael, 2007).

$$(B3) \quad \Delta y_c = \tilde{\alpha} + \tilde{\theta} G_c + \Delta \varepsilon_c,$$

where Δy_c represents the change in average outcomes over time in MSA c , and G_c represents the fraction of the area's working age population receiving Green Cards. If model B2 is correctly specified, the difference-in-differences coefficient in model B3, estimated using aggregate data, is thus equivalent to the effect of having a Green Card at the person level from model B2 using data from the same population (in this case, all MSA residents, so the comparison group consists of all non-applicants in the MSAs under study). This is intuitive: if a Green Card increased the likelihood that an immigrant received EITC income, for instance, areas with higher Green Card shares should have experienced larger increases from before to after Green Card distribution in EITC transfers per capita.

But clearly, model B3 could also have been derived from a person-level model with an effect of G_c on both applicants and non-applicants, such as

$$(B4) \quad \Delta y_{ic} = \tilde{\alpha} + \tilde{\theta}_1 G_i + \tilde{\theta}_2 G_c + \Delta \varepsilon_{ic}.$$

In this case, $\tilde{\theta} = \tilde{\theta}_1 + \tilde{\theta}_2$; that is, the impact of Green Card distribution as estimated on aggregate data (model B3) captures both the direct effect for Green Card holders and an indirect, aggregate effect on each member of the MSA population.

2. Potential Indirect Impacts of IRCA

Could IRCA's Green Card distribution have had broader impacts beyond the person-level ones we hope to identify? We are mainly interested in impacts on non-applicants that could be proportional to applicant share, our measure of policy intensity. Given our focus on the income tax, these could operate through impacts of IRCA on the labor market. In theory, large-scale distribution of Green Cards could have had aggregate impacts on the labor market by changing workers' bargaining position, time horizon, or remittances, but the available evidence suggests that these impacts would be orders of magnitude smaller than the direct effect for applicants.

First, theoretical work on amnesty suggests that unauthorized immigrants affect the labor market because they have a higher search cost (or worse outside option) than authorized immigrants (or natives) in a wage bargaining model (Chassamboulli and Peri, 2015). Giving a large share of the population Green Cards thus can push up immigrants' – and so similarly skilled natives' – wages, though at the expense of employment due to higher labor costs and lower profits. The available calibrations however suggest the impacts on natives are miniscule.¹⁴

¹⁴ Chassamboulli and Peri (2015)'s calibrations suggest that increasing the rate at which undocumented Mexican workers are legalized would likely increase both native-born earnings and employment. This is not an ideal model of IRCA, however, which was a one-time amnesty. Indeed, their findings appear to partly – perhaps mainly – derive from the greatly increased flows of immigrants that result from the higher legalization rate. Although not explicitly about unauthorized immigration, comparisons in Chassamboulli and Palivos (2014) appear closer in spirit to IRCA: they compare labor market impacts of an immigration-induced skill mix shock when immigrants have, alternatively, higher or the same search costs as natives. Such a shift in theory raises the expected costs of hiring workers of their skill level, and so induces lower job entry and leads to lower employment. Despite the fact that immigrants make up

Second, by increasing immigrants' time horizon, legal status may over time induce immigrants to acquire U.S.-specific skills (like English), potentially making them more substitutable for natives. However, the elasticity of substitution between immigrants and natives is already so large (e.g., Card, 2009; Ottaviano and Peri, 2012) that this change would also likely have a small effect on natives' wages.

Third, immigrants legalized under IRCA reduced their remittances (Amuedo-Dorantes and Mazzolari, 2010), which could theoretically lead to positive effects on earnings through higher U.S. consumer spending. However, estimates in Olney (2015) suggest the resulting wage increase would be less than one percent.

Consistent with this, the only study that has examined the broader labor market impacts of amnesty found that its impact on aggregate wages was negligible (Cobb-Clark, Shiells, and Lowell, 1995).¹⁵

over 10 percent of the labor market in their simulations, the effects derived from changes in search costs are generally below 0.1% (comparing columns of their table 2).

¹⁵ The authors examined a very crude outcome, production worker wages (not broken out by nativity), which shows a negligibly larger increase after IRCA in areas with more LAW applicants compared to areas with fewer.

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