

VERY PRELIMINARY AND INCOMPLETE

**The Tax Consequences of Amnesty:
Evidence from the Immigration Reform and Control Act of 1986**

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Abstract. The potential consequences of granting amnesty to some or all of the estimated 11.5 million undocumented immigrants living in the U.S. today are a source of great controversy. We attempt to inform this debate by estimating the impacts of the last major U.S. amnesty – through the Immigration Reform and Control Act of 1986 (IRCA) – on income tax contributions and participation. Our empirical approach exploits both the timing of IRCA and the geographic unevenness of applications for temporary legal status under the law, and our analysis focuses on California, which was home to a majority of applicants. Using a newly constructed county-level panel of highly-detailed income tax statistics, we find that counties with more amnesty applicants per capita saw relatively large increases in per-capita income tax returns filed starting in 1986. Consistent with applicant demographics, the increases in filing rates were concentrated among low-income filers and were accompanied by increased claims of California’s renter’s tax credit and increases in per-capita federal transfers under the EITC.

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I. Introduction

How to deal with the estimated 11.5 million unauthorized immigrants in the U.S. today – up from 8.5 million in 2000 (Hoefler, Rytina, and Baker, 2012) – has been an unresolved source of policy debate for almost two decades.¹ One stumbling block to policy consensus may be the lack of empirical evidence on the consequences of a key policy option: legalization or “amnesty.” Existing papers on amnesty’s impacts focus on its consequences for the labor market outcomes of the newly legalized immigrants. However, animosity to immigration appears to stem more from its (presumed) impact on public finances. For example, “fiscal burden” is typically among the top concerns listed by opponents of immigration in opinion surveys,² and efforts to limit even legal immigrants’ access to public programs, such as the Personal Responsibility and Work Opportunity Reconciliation Act and California’s Proposition 187, have been successful.³

Public opinion aside, we do not actually know whether giving legal status to the undocumented would generate a fiscal burden on the U.S. It is certainly possible that giving undocumented immigrants legal access to public programs would induce them to use such programs heavily, as the undocumented tend to be low-income. Yet, existing research suggests that legalization raises immigrants’ earnings, which may instead increase tax payments and reduce demand for public programs.⁴ Legalization may also increase tax compliance, both by the

¹ The last sizable U.S. amnesty was the Nicaraguan Adjustment and Central American Relief Act of 1997. Several pieces of legislation that would have given legal status to certain immigrants have been introduced since that time, but none have made it through Congress.

² For example, over 60% of respondents to recent (2006, 2008, 2010) Gallup surveys agree that “illegal immigrants cost the taxpayers too much by using government services like public education and medical services” over “illegal immigrants in the long run become productive citizens and pay their fair share of taxes,” whereas fewer than 20% agree that “illegal immigrants mostly take jobs that American workers want,” over “mostly take low-paying jobs Americans don’t want” (see <http://www.gallup.com/poll/1660/immigration.aspx>).

³ Indeed, it is common to model the key downside of amnesty as the increased cost of providing public services (Chau, 2001; Casarico, Fachini, and Frattini, 2012). Consistent with this, amnesty seems to be less common at times and places where public programs are more generous (Casarico, Fachini, and Frattini, 2012).

⁴ The most credible estimates, which include a comparison group, for which legal status is observed, and which include data from both before and after amnesty, range from a 6% earnings boost from legalization for men (Kossoudji and Cobb-Clark, 2002) to up to a 20% gain for women (Amuedo-Dorantes, Bansak, and Raphael, 2007).

immigrants themselves and their employers. Further, by changing immigrants' prospective time in the U.S., legal status may encourage the formation of families in the U.S., placing new burdens on public services, but also support for investment in U.S.-specific human capital (Cortes, 2004) and public goods.

We attempt to gain insight into this question by investigating the some of the immediate state and federal fiscal impacts of the largest U.S. amnesty to date, which occurred through the Immigration Reform and Control Act of 1986 (IRCA). Among other provisions, IRCA gave legal status to a large swath of the illegal population estimated to have been in the U.S. at the time – more than 2.7 million immigrants, the vast majority of whom, like now, were from Mexico. For reasons discussed in more detail below, we focus on the impacts of this amnesty on the extent of participation in and redistribution through the income tax system, namely via the federal Earned Income Tax Credit (EITC).⁶

A key challenge facing this and past studies of IRCA's impacts is the difficulty – often impossibility – of obtaining data on undocumented immigrants from both before and after the law. Even with such data, an additional challenge is to identify a comparison group that credibly captures what would have happened absent the law.⁷ We address these issues by taking a different approach: instead of focusing on IRCA's impacts at the level of the individual, we will examine its impacts at the county level, exploiting the timing of the law alongside variation across counties in the degree to which their populations applied for amnesty. An advantage of

⁶ In the future, we will also investigate its impacts on local fiscal outcomes related to public education provision.

⁷ Likewise, the traditional “accounting” approach for studying the fiscal impact of immigration is both infeasible and potentially confounded by non-random selection into legal status. This traditional accounting approach sums up the tax payments and subtracts off the expenditures implied by the program use of immigrants relative to natives (e.g., Smith and Edmonston, eds., 1997; Hinte and Zimmerman, 2014). In this case, the comparison would be between legal immigrants and illegal immigrants, and fiscal variables are not available by legal status.

this approach over even a perfectly-executed individual-level analysis is that it will capture some of the spillovers of amnesty to other populations.

More specifically, our empirical model tests for sharp, differential changes in outcomes starting around 1986 in counties where the share of the population that applied for temporary legal status was large compared to counties where it was small. While a handful of previous studies have approached estimation of this amnesty's impacts with the same idea in mind (Cobb-Clark et al., 1995; Barreto et al., 2005; Baker, 2014), none have examined fiscal outcomes, and all impose restrictions on the estimation – such as assuming no differential trends in outcomes prior to IRCA – that are sometimes rejected in the data. Our models are also made more robust by inclusion of unrestricted trends in outcomes by other county characteristics that reflect of other, concurrent policy changes, most notably the Tax Reform Act of 1986. We further reduce the possibility that spatial comparisons might be confounded by unobservables by looking across counties *within* the same state. In particular, we focus on the one state – California – that was home to a majority of the amnesty applicants under IRCA. Figure 1 shows quite a bit of variation across metropolitan California counties in the share of the population applying for legal status. An additional benefit of focusing on California is that the state offers rich county-level panel data on taxation that is not available in any other state with a sizeable undocumented population.¹⁰

Our findings are consistent with a sizeable positive impact of amnesty on tax compliance among immigrants: the amnesty was associated with a reversal of what had been a steeper downward trend in per-capita income tax returns filed in California counties with higher ratios of

¹⁰ Five states contained 84 percent of applicants for legal status: California, Texas, Illinois, New York and Florida. The state of New York offers data which are almost as detailed as California, but for fewer years; we may add these to the analysis in the future. Florida and Texas do not have state income taxes. Illinois does not appear to have historical income tax statistics available. Regular publication of Federal income tax statistics at the county level – beyond numbers of filers – does not appear to begin until 1989.

amnesty applicants to population. By 1992 – six years after the amnesty – the share of a county’s population filing income tax returns (state or federal) was 1 percentage point higher for each additional 3 percentage greater share of the population that applied for amnesty.¹¹ This is roughly equal to average returns filed per capita in California in the early 1980s, suggesting that amnesty resulted in nearly all newly legalized immigrants becoming part of a filing unit for the first time.

We also find that, consistent with applicant demographics, the increases in filing rates were concentrated among the lowest-income filers and were accompanied by increases in both per-capita claims of California’s renter’s credit and per-capita federal transfers under the Earned Income Tax Credit. The increase in EITC expenditures is substantial, implying a near doubling of EITC expenditures statewide (from around \$15 to \$29 per capita in 2013 dollars). While these findings imply that the newly legalized immigrants were net beneficiaries of the state and federal income tax systems, we find little evidence that amnesty increased transfers under other federal income maintenance programs, such as food stamps, by 1992.

II. Background and Significance

A. Description of IRCA

Legal Provisions

IRCA was largely unanticipated when it was signed into law on November 6, 1986. 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 those who already supported amnesty. IRCA also passed only after 15 years of failed attempts to pass legislation to address illegal immigration. Indeed, bills similar to IRCA

¹¹ Three percentage points is roughly the standard deviation in the applicant share of the county population.

had been introduced in the two prior Congresses and did not pass; the ultimate passage came from a fragile coalition rapidly assembled at the end of 1986.¹²

IRCA had two major legalization provisions. The first allowed workers who could document continuous residence in the U.S. since before January 1, 1982 (among other criteria) to apply for temporary legal status, which consisted of work and travel authorization. This is known as the “Legally Authorized Workers” (LAW) program. The application period ran from May 1987 to May 1988. After meeting some additional provisions (learning English and passing a civics test), successful LAW applicants could then apply to become permanent residents 18 months later. The second, “Special Agricultural Workers” (SAW) program granted the same status to 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), during the application period May 1987 to November 1988. They, too, were allowed to apply for permanent residency one or two years after receiving temporary status.¹³ Of all applications, about 40% came through the SAW program, and 90% were successful in achieving temporary legal status.

Figure 2 traces the status of applications under both programs over time, based on the public-use version of the Legalization Applications Processing System (LAPS), which provides anonymized longitudinal data on all 3.04 million LAW and SAW applications through 1992. The figure confirms that all applications were submitted by late 1988. It also shows that over half of the adjustment to temporary legal status had happened by the end of 1988, though it continued through 1992. Transitions to permanent residency occurred mainly in 1989 and 1990, consistent with the provisions laid out above. By 1992, 51.2% of applicants for temporary legal status had become permanent residents. Naturalization did not begin until 1994 (Rytina, 2002).

¹² See Baker (1990) for more.

¹³ The first 350,000 applicants who could also 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.

IRCA had other provisions that are relevant for our analysis. In particular, the law stipulated that amnesty applicants would not be eligible for Aid to Families with Dependent Children (AFDC) until five years after being granted temporary legal status. LAW applicants in particular were also more broadly ineligible for “any program of assistance furnished under Federal law on the basis of need,” for five years, although several exceptions were laid out for educational and child welfare programs, the disabled, pregnant women, and children. Based on the trends shown in Figure 2, successful applicants under either program would thus have been ineligible for AFDC until 1993, and only SAW applicants may have had earlier eligibility to other federal programs, such as food stamps. However, successful applicants under either program would have been theoretically eligible for the EITC once authorized to work. On this front, another IRCA provision granted all workers likely to have successful applications immediate work authorization and a stay from deportation just after the law’s passage.

Thus, because our analysis must end in 1992 due to data limitations,¹⁴ the income tax system is a good focal point, as it is here that the state and federal fiscal impacts of the amnesty were likely to have been immediately felt. While we also estimate impacts on transfers under other federal income maintenance programs, such as food stamps, the expectation is that any impacts we find here should be weaker than what we see for the EITC. However, a consequence is that we may understate the impacts of the amnesty on participation in these other programs over the longer term, since applicants would not have been eligible for them until 1993.

Applicant Characteristics

Table 1 provides details on the distribution of LAW and SAW applicants across states and within states, across counties. Here we also rely on the LAPS data, which provide intended

¹⁴ Focusing on 1992 and prior also increases the chances that the identifying assumptions of our empirical models are satisfied. We describe this issue in more detail below.

counties of residence with more than 100,000 people (as of the 1990 Census) or more than 25 applications. The table lists states in descending order of the proportion of applicants nationwide they represent. To keep the table concise, we restrict attention to the five top applicant-generating states, which also happen to be the most populous states and which together represented 84.1% of all applications. As shown, more than half (53.3%) of all applicants nationwide listed California – the focal state of our analysis – as their intended residence. Texas was a distant second, with about 450,000 applicants (14.8% of the total). New York, Illinois, and Florida each represented 5% to 6% of applicants.¹⁵

The impacts of IRCA’s legalization provisions on the tax system will depend in part on the characteristics of applicants themselves. In Table 2, we summarize some basic characteristics provided in the public-use LAPS data for California, for the top five states listed in Table 1, and for the U.S. as a whole. The table shows that applicants for temporary legal status under IRCA were overwhelmingly Hispanic, male, and of working age. In the country overall, for example, nearly 87% of all applicants were Hispanic, only 32% were female, and fewer than 1% were aged 65 or older. They therefore overlap demographically with the undocumented population today (Hoefer, Rytina, and Baker, 2012).¹⁶ Being working age and male, they were also likely to be net contributors to the Social Security system for the near term, particularly if amnesty encouraged transitions to covered employment.¹⁷ On the other hand, as expected, applicants were on average low-earning. LAW applicants, for whom wages are reported, earned on average only

¹⁵ The next five states of intended residence were (in descending order of the percent of national applications) Arizona (2.7%), New Jersey (1.5%), Washington (1.2%), New Mexico (0.9%), and Oregon (0.9%).

¹⁶ Today’s undocumented immigrants are estimated to have a lower Mexican share (59%) than in 1986, made up for by the rising share from other Central American countries and several Asian countries. Women also appear to be a larger share of the recent undocumented (47%). The recent undocumented are also less concentrated in California, part of the broader pattern of immigrants, especially Mexicans, spreading to other parts of the U.S. (Card and Lewis, 2007). These shifts had already largely occurred by 2000. See Hoefer, Rytina, and Baker (2012).

¹⁷ We hope to explore impacts on payroll tax contributions in future work.

about \$9 an hour (in 2013 dollars) at the time of application. Working in agriculture, SAW applicants were likely even lower paid.

Table 2 also shows that, with the exception of having a relatively high Hispanic share – presumably due to relatively high rates of Mexican immigration – California looks similar in terms of applicant characteristics to the top five applicant-generating states and the U.S. overall. This similarity provides a useful basis for generalizing from California’s experience, as California offers richer data than that available for the other states.

B. Other Policy Changes at the Time of IRCA’s Passage

Several other policy changes coincided in time with the passage of IRCA, or were passed shortly thereafter. First, in order to build the consensus to include the amnesty provisions, IRCA also included new enforcement provisions, including increased funding for border security and new employer sanctions for knowingly hiring illegal immigrants. Though increased border security raises the costs of migrating from Central America, there is little evidence that it affects the rate of border crossing (Gathmann, 2008), though increased border enforcement does seem to raise apprehensions (Hanson and Spilimbergo, 1999). Time-series analysis suggests that IRCA, in particular, had little impact on flows from Mexico (Passel and Woodrow, 1990; Orrenius and Zavodny, 2003). However, Bansak and Raphael (2001) find that IRCA’s sanctions provisions increased wage discrimination against Hispanic workers relative to non-Hispanic workers.¹⁸ As Hispanic share is otherwise larger in counties with more IRCA legalization applicants, any such discrimination would lower taxable income by relatively more in these counties.¹⁹

¹⁸ Bansak and Raphael’s approach exploited the brief period in which sanctions applied only to non-agricultural employers; Hispanic workers’ non-agricultural wages fell relative to agricultural wages relative to the same double difference for non-Hispanics during this period. Other theoretical and empirical work also suggests that increased interior enforcement likely lowers wages (Cobb-Clark, Shiells, and Lowell, 1995; Chassambouli and Peri, 2014).

¹⁹ This could be thought of as a downward bias our estimates of the net fiscal benefits of amnesty. However, as any future amnesty law would likely include similar provisions, these effects might be alternatively thought of as part of the reduced form effect of immigration reform.

Second, the Tax Reform Act of 1986 (TRA86) became law two weeks before IRCA. This was a complex package of tax changes, but broadly speaking, it increased the standard deduction and personal exemption and lowered top marginal tax rates while broadening the tax base in several ways (e.g., by phasing out the ability to deduct interest on consumer debt and sales tax). The first set of changes lowered the share of low-income individuals who were subject to taxation. The second set appears to have increased the taxable income reported by top earners (Feldstein, 1995).

Another way that low-income individuals benefited from TRA86 was through the law's expansion of the EITC, a refundable tax credit available to low-income individuals with positive earnings. The EITC expansion authorized by TRA86, which went into effect during the 1987 tax year, led to modest increases in the phase-in rate and maximum credit, and applied only to families with children. The later expansion of the EITC during the period of interest, through the Omnibus Budget Reconciliation Acts of 1990 (OBRA90) was larger in scope, introducing a more generous credit for families with two or more children than for families with only one child.

These other changes to the policy environment offer important context for interpretation of our estimates. For example, that amnesty applicants under IRCA were mostly low-income (Table 2) means that the EITC expansions would be more strongly felt in areas with higher densities of applicants. This is part of the reduced-form effect of legalization. On the other hand, these concurrent policy changes also have the potential to bias our estimates. For example, if applicant-dense areas also had high shares of citizens and legal immigrants eligible for an expanded EITC, our estimates would also pick up the impacts of the EITC expansion on these other populations. As detailed below, we will attempt to account for the impacts of concurrent

policy changes in general by allowing for differential trends in outcomes by relevant county characteristics measured prior to IRCA.

III. Data

As described above, the data used to construct the numerator of our key treatment variable – the mean amnesty applicant to population (in 1980) ratio – are drawn from the public-use version of the federal administrative LAPS data, available through the National Archives.²⁰ Recall that the LAPS data give longitudinal information on all individual applications for temporary legal status following IRCA under both the LAW and SAW programs. To ensure confidentiality, the public-use LAPS data suppress applicant names and as already described, do not identify intended counties of residence with fewer than 100,000 people (as of the 1990 Census) or fewer than 25 applications. Because of this, we focus on California’s 31 metropolitan counties.

Our main source of data on income tax returns comes from the *Annual Report of the State of California Franchise Tax Board*.²¹ The *Annual Reports* offer rich county-level statistics on a large number of variables related to the California state income tax. Our main outcome of interest in these data is the ratio of state income tax returns filed to county population. However, we can also observe the number of returns claiming California’s renter’s credit, as well as per-capita returns filed in narrow bins of distribution of adjusted gross income (AGI).²² Here, we parse out county-level returns into ten categories based on deciles of California’s 1979 AGI; dividing by population, the resulting variables sum up to our primary outcome of interest. These

²⁰ The denominator for this measure (1980 population) comes from county-level tabulations from the 1980 Census, which we obtained from the National Historical Geographic Information System (NHGIS).

²¹ The *Annual Reports* are not in electronic form, but scans are available on the website of the California Franchise Tax Board. For the purposes of this paper, we converted the 1980 through 1993 *Annual Reports* (corresponding to the 1979 through 1992 tax years) to electronic format.

²² For example, the bins are in \$2,000 increments in the 1986 *Annual Report*.

decile-by-decile estimates, as well as estimates for per-capita claims of California renter's credit, are a helpful check on the internal validity of our research design: if our model identifies the effects of legalization, we should expect to see the greatest impacts on tax filing rates in the bottom tail of the income distribution and large take-up of the renter's credit, based on applicant characteristics (Table 2).

A limitation of these California-specific data is they do not provide any information on the content of *federal* income tax returns. We attempt to address this limitation in several ways. First, using the Internal Revenue Service (IRS) Statistics of Income (SOI) County-to-County Flows data, we can estimate the number of federal tax returns filed at the county level for most years in the period of interest (1979 through 1992 tax years).²³ In addition to the fact that some years are missing, another important caveat on these data is that an individual filer has to have filed in two consecutive years to be included, but only in 1983 and later.²⁴ Further, in no publicly-available data does the IRS provide counts of federal returns filed at the county level by bin of AGI, or any information about the number of returns claiming the EITC.

Unfortunately, information on the former does not seem to be available. To explore the latter, however, we turn to a different data source: the Bureau of Economic Analysis (BEA) Local Area Personal Income Accounts. These BEA data report per-capita transfers to counties under not only the EITC, but also other federal income maintenance programs that operate outside of the tax system, such as food stamps (later the Supplemental Nutrition Assistance Program, or SNAP) and Supplemental Security Income (SSI).²⁸

IV. Amnesty and Tax Participation

²³ These data are available from the National Archives and ICPSR (prior to 1989) and on the IRS website (for 1989 and later). They are annual back to 1983, with data missing for 1979 and 1981.

²⁴ Data for 1980 and 1982 pertain to all filers.

²⁸ Unfortunately, cash welfare transfers are not separately reported in the BEA data. In the next draft of this paper, we hope to add this information using a different data source.

Our identification strategy takes advantage of the timing of IRCA alongside variation across counties in the expected “intensity” of the law’s impacts. Our measure of treatment intensity is the number of total applicants for temporary legal status under both the LAW and SAW programs in county c expressed as a percentage of county c ’s 1980 population, $pcamn_c$. If IRCA had an impact on tax filing, we would expect to find that tax-filing rates would have increased more after IRCA was passed in counties where $pcamn$ was higher.

To formalize this intuition, suppose we were to estimate, separately by year t , a series of bivariate regressions of county-level tax-filing rates, y_{ct} , on $pcamn_c$:

$$(1) \quad y_{ct} = \alpha_t + \beta_t pcamn_c + \varepsilon_{ct}.$$

The coefficient β_t would then give the amount by which tax-filing rates on average differed in year t between two counties with every one percentage point difference in their amnestied population shares. Prior to the amnesty itself, we would expect these coefficients to be negative: counties with more amnesty applications relative to their 1980 populations probably had fewer tax returns filed per capita. After the amnesty, however, this gap in filing rates should have been diminished, as illegal immigrants came “out of the shadows.” Put differently, it should be the case that the *change* in β between some post-amnesty year and, say, 1986, is positive.

Our objective is then to test whether the gradient of per-capita tax returns filed in $pcamn_c$ significantly changed in post-amnesty relative to pre-amnesty years. This test is implicit in the “event-study” model:

$$(2) \quad y_{ct} = \sum_{\tau \neq 1986} \theta_{\tau} D_{\tau}^{\tau} \times pcamn_c + \gamma_c + \delta_t + \varepsilon_{ct},$$

where y_{ct} remains tax returns filed per capita in county c in year t ; D_{τ}^{τ} is a dummy variable set to one if the year of observation t is equal to τ , zero otherwise; and γ_c and δ_t represent vectors of

county and year fixed effects, respectively.³⁰ The coefficients θ_τ then capture the parameter of interest – the *difference* in the slope coefficient on $pcamn$ between year τ and the omitted year, 1986. We believe that using 1986 as a reference point is conservative, as not even applications for amnesty were accepted until May 1987 (Figure 2). Note that by incorporating data from prior to 1986 as well, model (2) also allows us to test whether the tax-filing rates were already trending differentially in counties with higher amnestied population shares in the pre-amnesty period.

Figure 3 provides estimates of the θ_τ for per-capita California state income tax returns filed (Panel A) and per-capita federal income tax returns filed (Panel B) (both are expressed in percent terms)..Due to the constraints on the LAPS data described above, the underlying data incorporates only the 31 metropolitan California counties. The capped vertical lines around the θ_τ estimates represent their 95% confidence intervals. Inference accounts for heteroscedasticity and autocorrelation of the error terms within counties over time, and we weight the regression estimates by 1980 county population.³²

Figure 3 shows that the counties more intensely “treated” by IRCA were experiencing relative declines in tax-filing rates prior to the law’s passage, possibly because they were experiencing faster growth in their undocumented populations over this period. Suggestive of a positive impact of legalization, however, these trends appear to have reversed after 1986. Usefully, estimates of the θ_τ for tax filing rates appear fairly similar for state and federal returns, though the individual point estimates are more precisely estimated for the latter.

We attempt to address the relative lack of precision in the state data by turning to several more parametric approaches. Given what appear to be (and are) significant differences in pre-

³⁰ Note that the county fixed effects absorb the direct effects of $pcamn_c$ in 1986.

³² We use a critical value from a t-distribution with 29 degrees of freedom to account for the potentially small number of clusters.

initiative trends by treatment status in Figure 3, a difference-in-differences model would clearly be inappropriate for summarizing these patterns. Indeed, such a specification would essentially take the difference in the average value of the post- (1987 to 1992) and pre- (1980 to 1986) coefficients, implying a null if not negative effect of legalization on tax filing rates. If the trend of declining tax filing rates in counties with higher values of $pcamn_c$ were to have continued in the absence of IRCA, the true treatment effect is arguably larger than what would come from the simple difference-in-differences specification.

This idea forms the basis of our first more parametric specification (which we will call a “restricted event study” model):

$$(2') \quad y_{cst} = \lambda t \times pcamn_c + \sum_{\tau > 1986} \lambda_{\tau} D_t^{\tau} \times pcamn_c + \tilde{\gamma}_c + \tilde{\delta}_t + v_{ct} .$$

The difference between this specification and (2) is that, rather than varying in an unrestricted fashion, differences in pre-IRCA trends in outcomes across counties with higher and lower treatment intensity are modeled linearly. By including the full set of interactions $D_t^{\tau} \times pcamn_c$ for the post-IRCA period ($\tau > 1986$), the model ensures that identification of the linear trend, λ , derives only from the pre-IRCA period. The coefficients of interest are now the λ_{τ} , which give the difference between what actually happened, on average, in post-IRCA year τ and what would have been predicted to have happened based on these linear pre-IRCA trends. For example, λ_{1992} gives the treatment effect under the assumption that linear pre-IRCA trends would have continued through 1992. The strength of this assumptions is one of the reasons we choose to end the analysis in 1992.

An alternative, even more restrictive specification (the “comparative interrupted time series” (CITS) model) is given by:

$$(3) \quad y_{ct} = \mu t \times pcamn_c + \mu_1 D_t \times pcamn_c + \mu_2 D_t \times (t - 1986) \times pcamn_c + \tilde{\gamma}_c + \omega_{ct} ,$$

where D_t is set to one if $t > 1986$. Relative to the restricted event-study model in (3), model (4) captures IRCA's effects in an intercept shift (μ_1) and a change in the slope on $pcamn_c$ (μ_2) in 1986. Thus, the impact of IRCA as of 1992 would be given by $\mu_1 + \mu_2 \cdot 6$. While this model has the drawback of being more restrictive than (3), it can potentially help us to achieve more statistical power.³³

Estimates of the restricted event study and CITS models are shown in Panels B and C, respectively, in Table 3, with the coefficient estimates for the same two outcomes as in Figure 3 shown in the first two columns. For reference, Panel A of the table gives the mean of the dependent variable in the benchmark year, 1986; for both outcomes, this is about 35%, implying that there is about one return filed per every three people. Estimates are shown without and with additional controls. Here, to account for the potential confounding effects of TRA86, we control for the percentages of the 1980 county population with and without children and with incomes less than \$15,000 (in 1980), interacted with the same time variables with which $pcamn_c$ is interacted in the relevant model.

Consider first the estimates without additional controls, which correspond with the event-study estimates shown in Figure 3. In both panels (or for both models), the row labeled $pcamn \times year$ shows estimated pre-IRCA trends, which are (unsurprisingly from Figure 3) significantly negative. Relative to this existing trend, however, per-capita returns filed rose significantly after 1986. To make the estimates across the two models comparable, Panel C also shows the estimated effect in 1992 from the CITS model.³⁴ There is decent agreement across the two data sources and models, with CITS estimates a bit larger. In particular, in the average metropolitan

³³ See Dee and Jacob (2011) for an example of use of the CITS model in a different context. The CITS model is similar to the more familiar trend break specification, but it is less restrictive. In particular, it allows for a change in intercept in addition to a change in slope at 1986; that is, the coefficient on $D_t^t \times pcamn_c$ is not forced to zero.

³⁴ As described above, this is the sum of post-1986 intercept shift and change in trend evaluated in 1992, or $\mu_1 + \mu_2 \cdot 6$. Standard errors are calculated using the Delta method.

California county, where 6.8% of the 1980 population applied for temporary legal status under IRCA (Table 1), the restricted event-study estimates imply that tax-filing rates in 1992 were 2.10 (0.309×6.8) to 2.34 (0.344×6.8) percentage points higher than they would have otherwise been as a result of IRCA, depending on whether state or federal returns are the outcome of interest. Thus, IRCA appears to have increased tax-filing by on average 5.3 to 6.7 percent, given the baseline (1986) tax-filing rates of between 35 and 40 percent reported in Panel A of Table 3.

The rightmost two columns of Table 3 examine the same outcomes but add controls for the share of the counties families earning under \$15,000 (nominal dollars) in 1979, separately by the presence of a child.³⁵ Families with children might have been induced by the EITC expansion of the late 1980s to file taxes at greater rates if their incomes were in this range. If areas with more amnesty applicants had a greater share of families with incomes in this range for other reasons, this might confound our estimates. The controls are entered with same year interactions as the model for *pcamn*. Consistent with the concern that the EITC expansion confounds our analysis of Federal tax returns, the estimates of the increase in the number of Federal tax returns is reduced by about 8 percentage points (for example, from 0.48 to 0.39 in the CITS model). Reassuringly, however, this control has little effect when the outcome is California, rather than Federal returns. From this point forward these controls will always be included.

V. Amnesty and Income Tax Contributions

As discussed above, applicant demographics (per Table 2) give us reason to believe that immigrants legalized under IRCA newly filing taxes probably would have been low-income filers. Demonstrating that the impacts of amnesty are concentrated in bottom tail of the income

³⁵ From tabulations of the 1980 census of population. \$15,000 in 1979 dollars corresponds roughly to the median of the California income distribution.

distribution by our empirical methodology would therefore be helpful in demonstrating its internal validity. But such estimates would also serve another purpose, helping us to quantify the extent to which newly legalized immigrants received income support through the income tax system, particularly via the EITC.

Figure 4 gives event-study (model 2) estimates, but now for per-capita state returns filed in each of 10 bins defined by deciles of the California's 1979 distribution of AGI; the capped vertical lines again represent 95% confidence intervals on the estimates.³⁶ Figure 5 summarizes these estimates by plotting the CITS (model 4) estimates of the impact of amnesty as of 1992 for each of the ten bins, depicting them by the AGI that they represent (expressed in real 2013 dollars). Figure 5 also gives the CITS estimates for 1992 from a model with the additional controls for TRA86 described above. Table 4 provides these CITS estimates (Panel C), along with estimates of the restricted event study model (model 3) with additional controls.

Figure 4 provides strong evidence for the impacts of amnesty in the lowest decile of the AGI distribution (below \$5,371). Though the effects do not appear to be immediate, by 1990 there is evidence of a significantly greater increase over time in per-capita state returns filed in counties where a higher percentage of the 1980 population applied for amnesty. By 1992, a one percentage point increase in amnesty applicants is associated with 0.13 more very low income tax returns per capita; applied at the statewide average of 6.8 amnesty applicants per capita, this might have increased the number of these very low-income tax returns by more than 25 percent ($= 6.8 * 0.13 / 3.46$, where 3.46 is the pre-amnesty rate in panel A). What is more, there is no evidence of a pre-trend here (a finding confirmed by the regression estimates in Table 4), suggesting that conclusions about amnesty's impact can be drawn under weaker assumptions.

³⁶ As was the case above, in calculating the confidence intervals, we cluster the standard errors on county, and with 31 counties in our data, assume a t-distribution with 29 degrees of freedom.

This result only strengthens with the addition of controls (Figure 5; see also Table 4). Though conclusions do rest more on the assumption of linearity implicit in models 3 and 4, there is also evidence of economically and statistically significant impacts of amnesty on the number of returns filed between the 10th and 40th percentiles of California's 1979 income distribution (\$5,371 to \$29,902 in real 2013 dollars). By contrast, estimates for the number of per-capita returns higher up in the income distribution are smaller in magnitude and generally not statistically significant, particularly with controls.

Consistent with these findings, amnesty also appears to have increased the share of state returns claiming the Renter's Credit and per-capita federal transfers under the EITC (expressed in real 2013 dollars). We show this in Panels A and B, respectively, in Figure 6 (for the event-study model) and in the first two columns of Table 5 (for the restrict event-study and CITS models); each of the specifications includes the additional 1980 county characteristics interacted with the full vector of time variables relevant to the model. It is perhaps noteworthy that the coefficient on the renter's credit is only slightly smaller than the one for California returns overall, that is, one additional return for each 3 amnesty applications. This is consistent with most of the amnesty applicants being eligible for the renter's credit, which seems plausible. As was the case for returns filed in the lowest decile, there is essentially no evidence of a pre-trending here, lending credibility to the findings.

Recall that the applicants for temporary legal status under IRCA would have been eligible for the EITC in the year after applying, as IRCA granted preliminary work authorization. In contrast, SSI was not available to anyone legalized under IRCA in the period covered by the estimates, while SNAP is in between, with eligibility only for those who had achieved temporary legal status under the SAW program. The estimates are largely consistent with these eligibility

patterns. For example, there appears to be a significant positive impact of IRCA only on per-capita EITC receipt through 1992, and not on SNAP or SSI expenditures per capita. The latter is shown in Figure 7 and the two rightmost columns of Table 5.

These estimates provide suggestive evidence that, at least in the short term, the legalization provisions of IRCA increased both tax compliance and federal outlays for EITC. In terms of magnitudes, the impacts are fairly significant. Applied to the statewide amnesty applicant share of 6.8%, the estimates in Panel B imply that IRCA increased EITC outlays per capita by on average \$13.8 – nearly doubling baseline per-capita outlays (Panel A).

VI. Conclusions

This paper provides the first ever evidence on the fiscal impacts of amnesty. Nevertheless, our analysis remains a long way from a full “fiscal accounting” of the sort carried out in Smith and Edmonston, eds. (1997). A key insight of that analysis was that the main fiscal “costs” of immigration were not derived from immigrants’ use of transfer programs, but rather in their greater use of public schools due to their high fertility. Whether amnesty induced immigrants to have more U.S. children is something we may analyze in the future. Other fiscal outcomes missing from our analysis include contribution to payroll taxes, as well as expenditures on AFDC/TANF, which the newly legalized immigrants would have potentially become eligible before just after the period of our analysis.

While the focus of our analysis is on a limited number of primarily tax variables, the pattern of effects is consistent with provisions of the IRCA law as well as the observed demographics of the amnesty applicants. In particular, we find that amnesty under IRCA was associated with an increase in the number of tax returns roughly in line with what you would expect if granting amnesty induced the newly legalized workers to become a part of a US tax

filing unit for the first time. In addition, the added returns were concentrated in the lower tail of the California income distribution, consistent with their demographics. The amnesty was also associated with an increased proportion of California taxpayer's claiming the renter's tax credit, and a growth in EITC claims on Federal tax returns, but not with increases in expenditures on SSI or SNAP, which the newly legalized would have had little eligibility for. This helps to demonstrate the credibility of our approach and its potential value in for future research analyzing additional fiscal outcomes.

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Figure 1. IRCA Legalization Applications by Intended Residence

% of 1980 Population

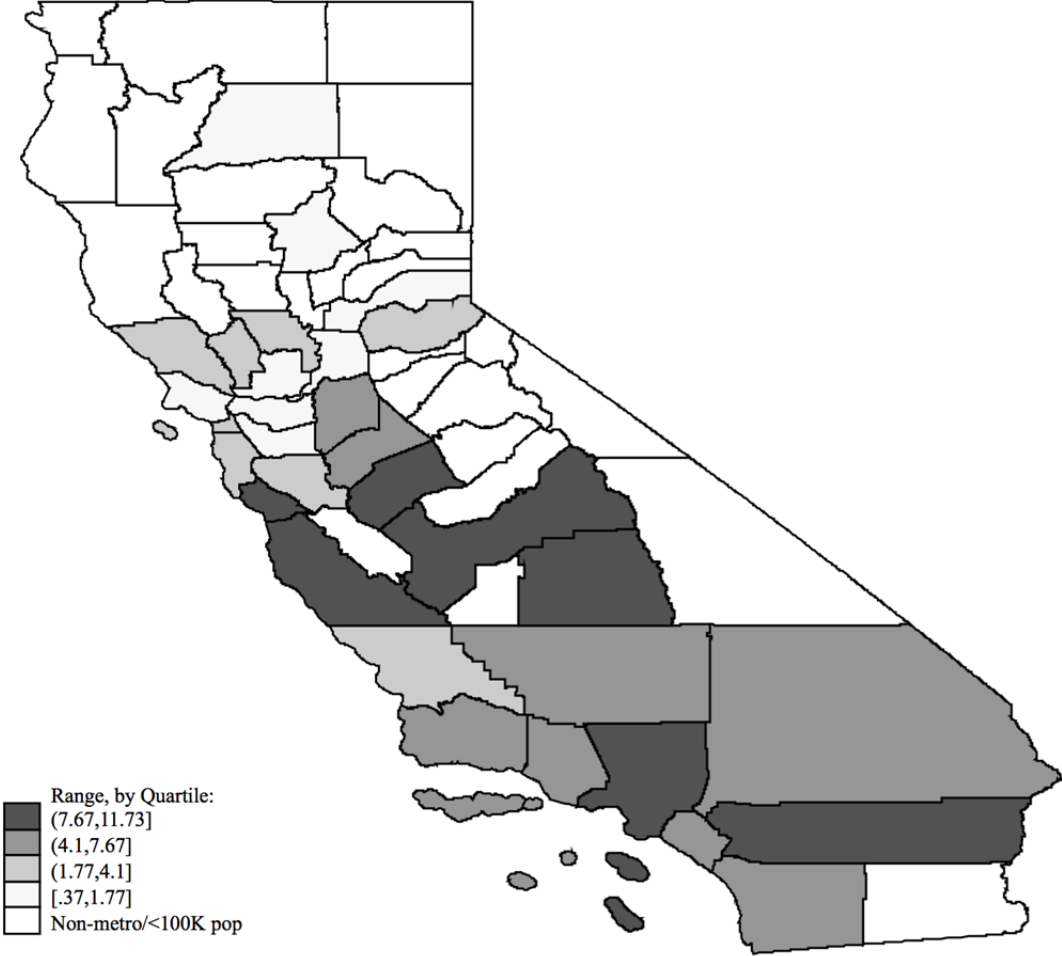


Figure 2. Cumulative Legal Status of IRCA (LAW+SAW) Applicants Over Time

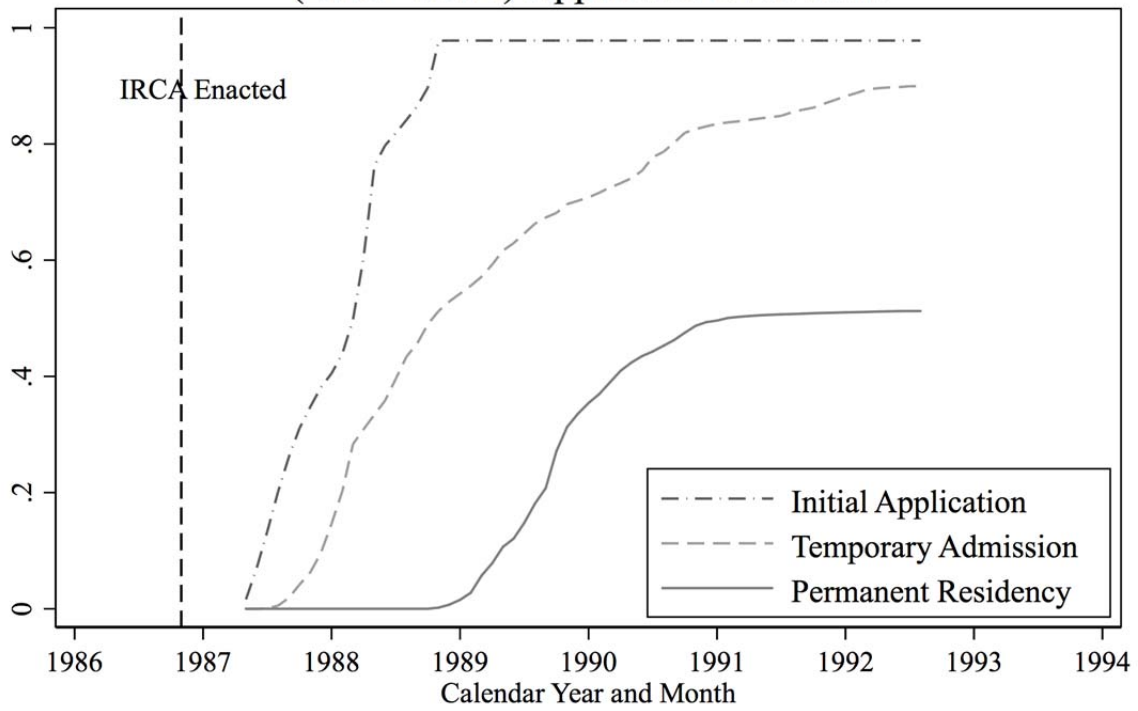
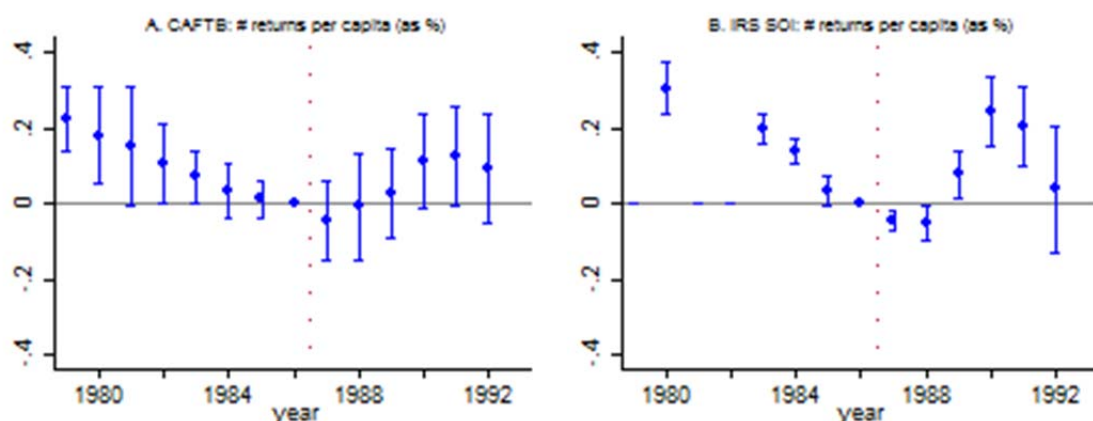
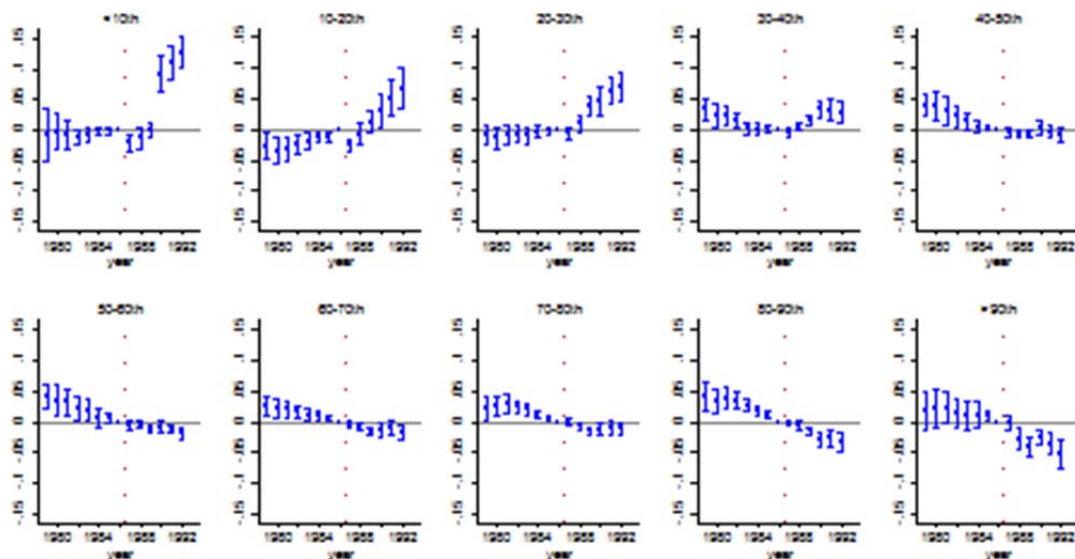


Figure 3. Event-Study Estimates: Tax Filing Rates
 Treatment=% 1980 Population Applying for Amnesty. Sample: Metropolitan CA



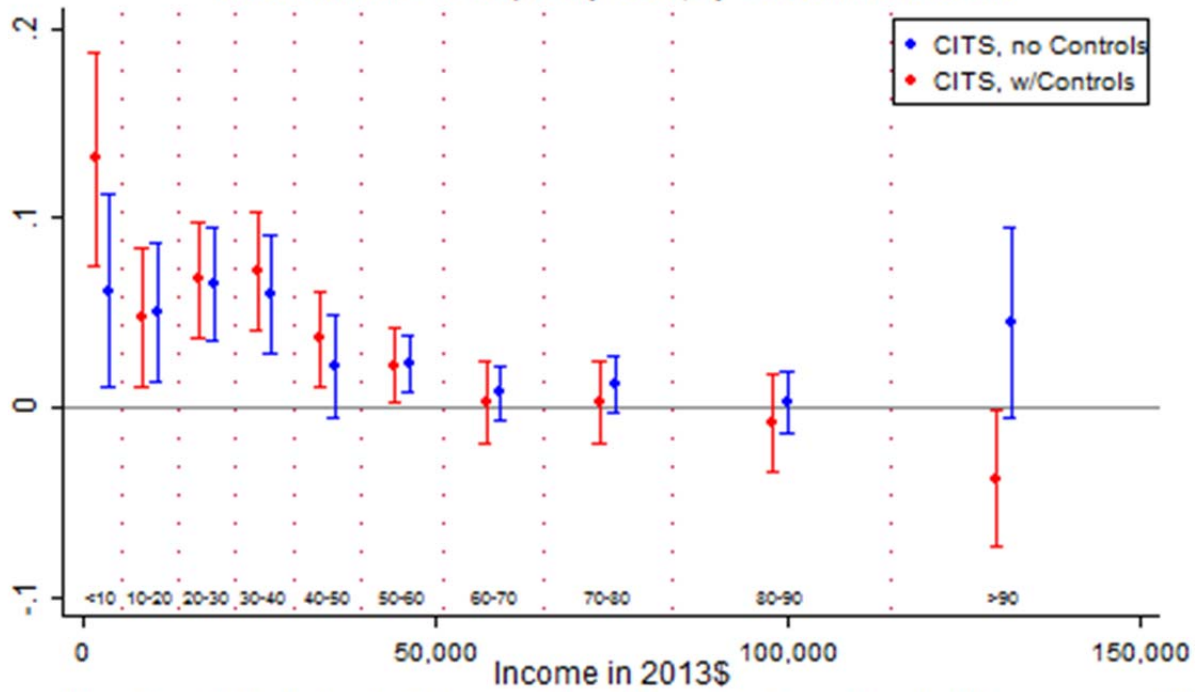
Note: Monetary values in real 2013 dollars. Sample limited to the 31 metropolitan CA counties. Capped vertical lines represent 95% confidence intervals. Standard errors clustered on county. Estimates weighted by 1980 population.

Figure 4. Event-Study Estimates: CA Returns per Capita by 1979 CA AGI Decile
 Treatment=% 1980 Population Applying for Amnesty. Sample: Metropolitan CA



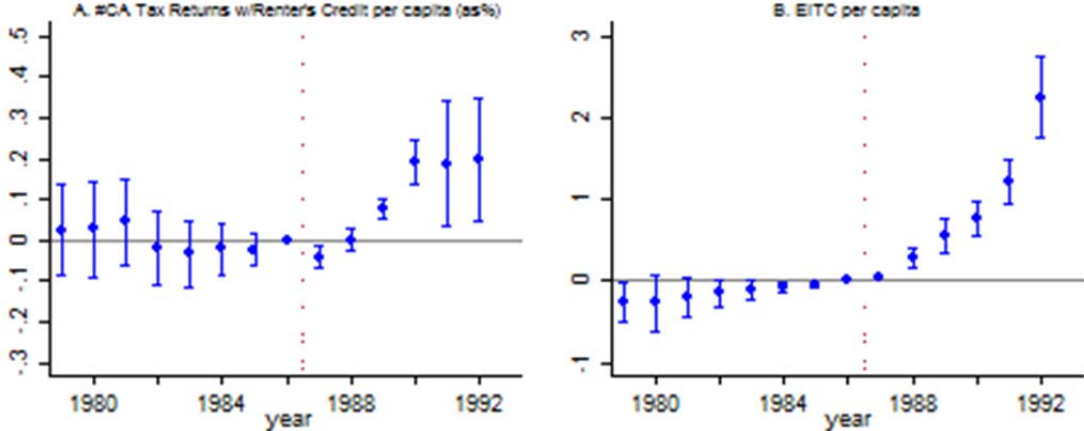
Note: Sample limited to the 31 metropolitan CA counties. Capped vertical lines represent 95% confidence intervals. Standard errors clustered on county. Estimates weighted by 1980 population. Adjusted, by year, for share of families with and without children earning <\$15K in 1979.

Figure 5. Impact of % 1980 Population Applying for Amnesty on CA returns Per Capita by 1992, by 1979 CA AGI Decile



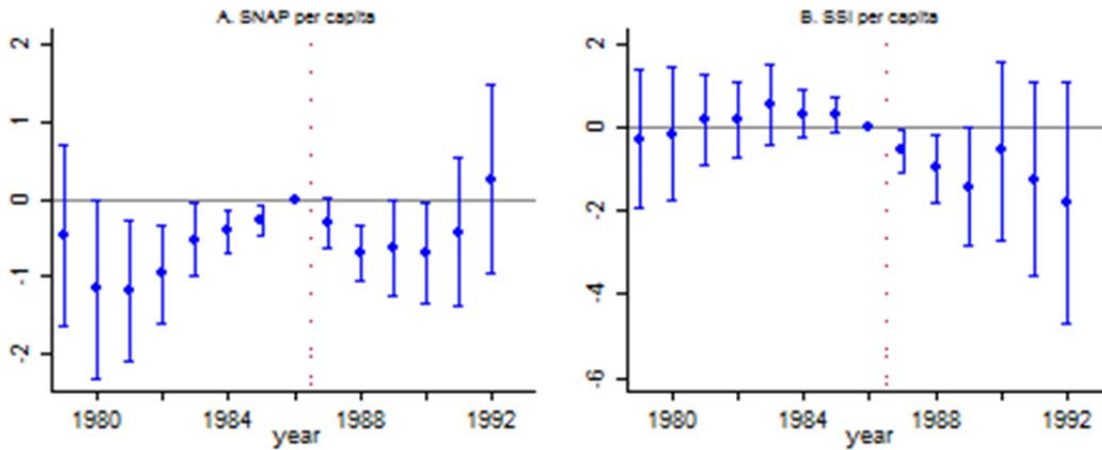
Note: Sample limited to the 31 metropolitan CA counties. Capped vertical lines represent 95% confidence intervals. Standard errors clustered on county. Estimates weighted by 1980 popula

Figure 6. #CA Tax Returns w/Renter's Credit and Federal EITC per capita
 Treatment=% 1980 Population Applying for Amnesty. Sample: Metropolitan CA



Note: Monetary values in real 2013 dollars. Sample limited to the 31 metropolitan CA counties. Capped vertical lines represent 95% confidence intervals. Standard errors clustered on county. Estimates weighted by 1980 population and adjusted, by year, for share of families with and without children earning <\$15K in 1979.

Figure 7. Federal Transfers: SNAP and SSI per capita
 Treatment=% 1980 Population Applying for Amnesty. Sample: Metropolitan CA



Note: Monetary values in real 2013 dollars. Sample limited to the 31 metropolitan CA counties. Capped vertical lines represent 95% confidence intervals. Standard errors clustered on county. Estimates weighted by 1980 population and adjusted, by year, for share of families with and without children earning <\$15K in 1979.

Table 1. Top IRCA Legalization Application Receiving States

| <u>State</u> | <u>Applications</u> | | <u>#of Counties</u> | | | <u>Applicants/1980 Pop²</u> | |
|--------------|---------------------|-------------|---------------------|-------------------------|-----------------|--|-------------|
| | <u>%of US Tot</u> | <u>%LAW</u> | <u>Total</u> | <u>ID'd¹</u> | <u>in Metro</u> | <u>Std Dev</u> | <u>Mean</u> |
| California | 53.3% | 59.0% | 58 | 34 | 31 | 0.036 | 0.068 |
| Texas | 14.8% | 69.0% | 254 | 29 | 29 | 0.021 | 0.036 |
| New York | 5.7% | 68.1% | 62 | 21 | 20 | 0.010 | 0.012 |
| Illinois | 5.3% | 75.6% | 102 | 15 | 14 | 0.010 | 0.018 |
| Florida | 5.0% | 33.2% | 67 | 28 | 26 | 0.015 | 0.015 |

Note: ¹Counties identified in public-use LAPS data. ²Mean and standard deviation across identified metropolitan counties weighted by 1980 population.

Table 2. Selected Characteristics of IRCA Applicants

| <u>Location</u> | <u>Age</u> | | <u>Female</u> | <u>Hispanic</u> | <u>Country of Citizenship</u> | | | | <u>Mean Ln</u> | <u>LAW</u> |
|-----------------|---------------|---------------|---------------|-----------------|-------------------------------|----------------|---------------|--------------|----------------|------------|
| | <u><16</u> | <u>>65</u> | | | <u>Mexico</u> | <u>El Sal.</u> | <u>Guate.</u> | <u>Haiti</u> | <u>Hourly</u> | |
| California | 7.1% | 0.8% | 33.8% | 93.8% | 83.3% | 6.5% | 3.0% | 0.0% | 2.21 | 59.0% |
| Top 5 States | 6.8% | 0.9% | 33.3% | 87.8% | 76.0% | 5.8% | 2.5% | 2.0% | 2.19 | 60.9% |
| Whole US | 6.5% | 0.9% | 32.2% | 86.7% | 74.7% | 5.5% | 2.3% | 2.0% | 2.19 | 58.0% |

Note: ¹LAW applicants only in 2013 \$; corresponds to roughly \$9/hour.

Table 3. Estimates of the Restricted Event Study and CITS Specifications for Selected Tax and Transfer Outcomes. Sample: Metropolitan CA Counties, 1979-92

| | Source: CAFTB | IRS/SOI | CAFTB | IRS/SOI |
|---|-----------------------------------|-----------------------------------|-----------------------------------|-----------------------------------|
| Dependent variable: | Tax Returns Filed p.c. (%) | Tax Returns Filed p.c. (%) | Tax Returns Filed p.c. (%) | Tax Returns Filed p.c. (%) |
| <i>A. 1986 Mean of Dependent Variable</i> | | | | |
| | 39.99 | 35.05 | 39.99 | 35.05 |
| <i>B. Restricted Event-Study Estimates (Model 2)</i> | | | | |
| $pcamn_c \times \text{year}=1987$ | 0.007 (0.054) | -0.002 (0.008) | -0.050 (0.029) | -0.009 (0.011) |
| $pcamn_c \times \text{year}=1988$ | 0.077 (0.068) | 0.041*** (0.012) | -0.014 (0.029) | 0.0359** (0.015) |
| $pcamn_c \times \text{year}=1989$ | 0.144*** (0.052) | 0.225*** (0.022) | 0.0420* (0.021) | 0.200*** (0.026) |
| $pcamn_c \times \text{year}=1990$ | 0.262*** (0.052) | 0.443*** (0.040) | 0.223*** (0.053) | 0.376*** (0.048) |
| $pcamn_c \times \text{year}=1991$ | 0.310*** (0.054) | 0.455*** (0.048) | 0.290*** (0.064) | 0.377*** (0.058) |
| $pcamn_c \times \text{year}=1992$ | 0.309*** (0.065) | 0.344*** (0.071) | 0.309*** (0.074) | 0.264*** (0.083) |
| $pcamn_c \times \text{year}$ | -0.033*** (0.008) | -0.0522*** (0.007) | -0.0262*** (0.006) | -0.0479*** (0.006) |
| <i>C. CITS Estimates (Model 3)</i> | | | | |
| $pcamn_c \times \text{after 1986}$ $\times (\text{year}-1986)$ | 0.066*** (0.020) | 0.0911*** (0.014) | 0.0823*** (0.021) | 0.0732*** (0.016) |
| $pcamn_c \times \text{after 1986}$ | -0.033*** (0.008) | -0.052*** (0.006) | -0.155*** (0.053) | -0.0489* (0.026) |
| $pcamn_c \times \text{year}$ | -0.047 (0.083) | -0.068*** (0.021) | -0.0262*** (0.006) | -0.0479*** (0.006) |
| <i>Implied effect by 1992</i> <i>(standard error)</i> | 0.351*** (0.066) | 0.479*** (0.065) | 0.339*** (0.077) | 0.390*** (0.076) |
| Other Controls? ^a | No | No | Yes | Yes |
| N (county-year obs.) | 434 | 341 | 434 | 341 |

Notes: All models include county and year fixed effects in addition to the variables listed. Regressions are weighted by 1980 county population. ^aOther controls are the share of families with and without kids earning less than \$15,000 (in 1979, nominal dollars) each interacted with linear year and 1987-1992 year dummies (in panel A) or year, post-86, and year x post86 (in panel B)

Table 4. Restricted Event Study and CITS Specifications for CA Tax Returns Per Capita, by Decile of 1979 CA AGI. Sample: Metropolitan CA Counties, 1979-92.

| Percentile Bin: | <10th | 10-20th | 20-30th | 30-40th | 40-50th | 50-60th | 60-70th | 70-80th | 80-90th | >90th |
|--|-----------------|-----------------|------------------|------------------|------------------|-----------------|----------------|----------------|----------------|------------------|
| <i>A. 1986 Mean of Dependent Variable</i> | | | | | | | | | | |
| | 3.46 | 4.22 | 4.09 | 3.82 | 3.83 | 4.01 | 3.88 | 3.88 | 3.84 | 4.95 |
| <i>B. Restricted Event-Study Estimates (Model 2)</i> | | | | | | | | | | |
| <i>pcamn_c</i> x year=1987 | -0.0196* | -0.0249*** | -0.00655 | 0.00488** | 0.00491 | 0.00213 | -0.003 | -0.002 | -0.001 | -0.004 |
| | (0.010) | (0.005) | (0.0) | (0.002) | (0.003) | (0.004) | (0.003) | (0.005) | (0.006) | (0.012) |
| <i>pcamn_c</i> x year=1988 | -0.0136 | -0.010 | 0.00801 | 0.0192*** | 0.00885** | 0.00909*** | -0.003 | -0.005 | -0.00128 | -0.0261* |
| | (0.013) | (0.009) | (0.0) | (0.004) | (0.003) | (0.003) | (0.004) | (0.006) | (0.007) | (0.014) |
| <i>pcamn_c</i> x year=1989 | -0.001 | 0.005 | 0.0343*** | 0.0361*** | 0.0152*** | 0.00817** | -0.006 | -0.009 | -0.00367 | -0.0373** |
| | (0.013) | (0.009) | (0.0) | (0.006) | (0.005) | (0.003) | (0.005) | (0.006) | (0.008) | (0.014) |
| <i>pcamn_c</i> x year=1990 | 0.0902*** | 0.0173 | 0.0417*** | 0.0581*** | 0.0306*** | 0.0185*** | 0.000 | -0.003 | -0.012 | -0.018 |
| | (0.023) | (0.014) | (0.0) | (0.010) | (0.010) | (0.006) | (0.007) | (0.008) | (0.009) | (0.016) |
| <i>pcamn_c</i> x year=1991 | 0.107*** | 0.0351** | 0.0566*** | 0.0620*** | 0.0319*** | 0.0204** | 0.006 | 0.003 | -0.005 | -0.0268 |
| | (0.025) | (0.015) | (0.0) | (0.013) | (0.011) | (0.008) | (0.009) | (0.010) | (0.012) | (0.016) |
| <i>pcamn_c</i> x year=1992 | 0.124*** | 0.0470** | 0.0612*** | 0.0630*** | 0.0321*** | 0.0199** | 0.002 | 0.006 | -0.006 | -0.0407** |
| | (0.027) | (0.018) | (0.0) | (0.015) | (0.012) | (0.009) | (0.010) | (0.011) | (0.012) | (0.017) |
| <i>pcamn_c</i> x year | 0.001 | 0.00428*** | 0.002 | -0.00499*** | -0.00630*** | -0.00620*** | -0.00361*** | -0.00363** | -0.00557*** | -0.00274 |
| | (0.003) | (0.001) | (0.0) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) | (0.002) |
| <i>C. CITS Estimates (Model 3)</i> | | | | | | | | | | |
| <i>pcamn_c</i> x after 1986 | 0.0334*** | 0.0145*** | 0.0141*** | 0.0126*** | 0.00631** | 0.004 | 0.002 | 0.002 | -0.001 | -0.00475 |
| x (year-1986) | (0.004) | (0.004) | (0.0) | (0.003) | (0.003) | (0.002) | (0.002) | (0.002) | (0.002) | (0.004) |
| <i>pcamn_c</i> x after 1986 | -0.0691*** | -0.0390*** | -0.0167*** | -0.004 | -0.001 | 0.000 | -0.006 | -0.009 | -0.000644 | -0.009 |
| | (0.009) | (0.010) | (0.0) | (0.005) | (0.004) | (0.006) | (0.005) | (0.005) | (0.006) | (0.016) |
| <i>pcamn_c</i> x year | 0.000941 | 0.00428*** | 0.002 | -0.00499*** | -0.00630*** | -0.00620*** | -0.00361*** | -0.00363** | -0.00557*** | -0.00274 |
| | (0.003) | (0.001) | (0.0) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) | (0.002) |
| <i>Implied effect by 1992</i> | 0.131*** | 0.048** | 0.068*** | 0.072*** | 0.036*** | 0.023** | 0.003 | 0.003 | -0.008 | -0.037** |
| <i>(standard error)</i> | (0.028) | (0.018) | (0.015) | (0.015) | (0.012) | (0.009) | (0.011) | (0.011) | (0.013) | (0.018) |
| Other Controls? ^a | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| N (county-year obs.) | 434 | 434 | 434 | 434 | 434 | 434 | 434 | 434 | 434 | 434 |

Notes: All models include county and year fixed effects in addition to the variables listed. Regressions are weighted by 1980 county population. ^aOther controls are the share of families with and without kids earning less than \$15,000 (in 1979, nominal dollars) each interacted with linear year and 1987-1992 year dummies (in panel A) or year, post-86, and year x post86 (in panel B).

Table 5. Estimates of the Restricted Event Study and CITS Specifications for Selected Tax and Transfer Outcomes. Sample: Metropolitan CA Counties, 1979-92

| Source: | CAFTB | BEA | BEA | BEA |
|--|-----------------------------------|-----------------------------------|---------------------------------|---------------------------------|
| Dependent variable: | #CA Returns w/Renter's p.c. | EITC p.c. (2013\$) | SNAP p.c. (2013\$) | SSI p.c. (2013\$) |
| <i>A. 1986 Mean of Dependent Variable</i> | | | | |
| | 15.89 | 14.64 | 47.52 | 216.2 |
| <i>B. Restricted Event-Study Estimates (Model 2)</i> | | | | |
| $pcamn_c \times year=1987$ | -0.006 (0.011) | 0.015 (0.036) | -0.245 (0.343) | -0.974* (0.572) |
| $pcamn_c \times year=1988$ | 0.0419*** (0.012) | 0.202** (0.095) | -0.763* (0.426) | -1.456* (0.721) |
| $pcamn_c \times year=1989$ | 0.127*** (0.014) | 0.441*** (0.150) | -0.799 (0.552) | -1.949* (1.031) |
| $pcamn_c \times year=1990$ | 0.252*** (0.036) | 0.617*** (0.166) | -0.997 (0.612) | -1.154 (1.451) |
| $pcamn_c \times year=1991$ | 0.253*** (0.064) | 1.034*** (0.215) | -0.854 (0.735) | -1.905 (1.550) |
| $pcamn_c \times year=1992$ | 0.272*** (0.062) | 2.028*** (0.322) | -0.294 (0.709) | -2.506 (1.946) |
| $pcamn_c \times year$ | -0.00766 (0.008) | 0.0381* (0.022) | 0.123 (0.092) | 0.060 (0.135) |
| <i>C. CITS Estimates (Model 3)</i> | | | | |
| $pcamn_c \times$ after 1986 x (year-1986) | 0.0614*** (0.012) | 0.364*** (0.051) | -0.0205 (0.120) | -0.235 (0.285) |
| $pcamn_c \times$ after 1986 | -0.0584** (0.025) | -0.551*** (0.027) | -0.587 (0.408) | -0.836** (0.323) |
| $pcamn_c \times$ year | -0.00766 (0.008) | 0.0381* (0.022) | 0.123 (0.090) | 0.0597 (0.133) |
| <i>Implied effect by 1992</i> (standard error) | 0.310*** (0.052) | 1.633*** (0.288) | -0.710 (0.752) | -2.244 (1.880) |
| Other Controls? ^a | Yes | Yes | Yes | Yes |
| N (county-year obs.) | 434 | 434 | 434 | 434 |

Notes: All models include county and year fixed effects in addition to the variables listed. Regressions are weighted by 1980 county population. ^aOther controls are the share of families with and without kids earning less than \$15,000 (in 1979, nominal dollars) each interacted with linear year and 1987-1992 year dummies (in panel A) or year, post-86, and year x post86 (in panel B)