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Immigration, Employment Opportunities, and Criminal Behavior

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Abstract

We take advantage of provisions of the Immigration Reform and Control Act of 1986 (IRCA), which granted legal resident status to long-time unauthorized residents but created new obstacles to employment for more recent immigrants, to explore how employment opportunities affect criminal behavior. Exploiting administrative data on the criminal justice involvement of individuals in San Antonio, Texas and using a difference-in-differences strategy, we find evidence of an increase in felony charges filed against residents most likely to be affected by IRCA's employment regulations. Our results suggest a strong relationship between access to legal jobs and criminal behavior.

JEL Codes: F22, J15, J18, J61, K42, R23
Keywords: Crime, Immigration, Employment Regulations

1. Introduction

Immigration policy is one of the most hotly debated issues in the United States today. Public opinion polls suggest that 89% of Americans currently believe that immigrants are hard workers and that 60% believe that immigrants enhance American culture. At the same time, 40% view immigrants as a drain on social services and large shares believe that immigrants in general (32%), and immigrants who entered the country illegally in particular (58%), increase local crime (Bell and Machin 2013). These public divisions over immigration are played out on the political stage, where there are sharply contrasting views on the extent to which people living in the U.S. illegally should have access to employment opportunities and public services. However, despite strong feelings on the subject, there is little empirical research on the social implications of barring immigrants from access to employment or social support programs.

In the late 1980s, approximately 2.7 million people in the U.S. were granted legal resident status through the Immigration Reform and Control Act of 1986 (IRCA). Under the provisions of IRCA, any non-citizen who could document living in the U.S. for a substantial period of time could apply to be a permanent legal resident of the U.S. until May 4, 1988. Agricultural workers who were not citizens could apply for amnesty through November 30, 1988.

At the same time that IRCA created a pathway to legal status for previously undocumented immigrants, it shut off access to legal employment for people who could not satisfy IRCA's amnesty requirements. Specifically, IRCA required that employers attest to their employees' immigration status and made it illegal for firms to knowingly hire unauthorized immigrants. Consequently, as of 1988, individuals living in the U.S. without proper documentation were barred from the formal labor market.

The passage and implementation of IRCA provides an opportunity to explore how variation in policies toward immigrants, and specifically policies that affect immigrants' ability to find gainful employment, influence their propensities to engage in criminal behavior. Differences in immigration policies could help to explain the often conflicting findings on the effects of immigration on crime across countries and over time (e.g., Butcher and Piehl 1998, Reid et al. 2005, Moehling and Piehl 2007, Bianchi et al. 2012, Bell et al. 2013). While several studies examine the impact of IRCA's provisions on aggregate crime rates, no study has been able to distinguish between crimes committed by groups whose labor market opportunities were directly

affected by the reform from responses of natives, in large part because the immigration status of people who violate state laws is generally not collected by local authorities.

In this paper, we shed new light on the relationship between immigration, employment regulations, and crime by examining the criminal justice involvement of individuals in Bexar County, Texas. Bexar County is a roughly two-hour drive from Mexico and is home to a large Mexican immigrant population. According to INS records, 28,891 people in Bexar County were "legalized" under IRCA, about 2.2 times Census estimates of the number of undocumented immigrants in the county and 2.5% of the estimated county population at the time. The largest city in Bexar County, San Antonio, has been a "minor-continuous" immigrant gateway since 1900 (Hall et al. 2011); during the 1980s, an estimated 2,500 to 5,000 generally low-skilled immigrants arrived in the metropolitan area each year, driven largely by economic conditions in Mexico as opposed to U.S. immigration policy (Donato et al. 1992b).[1]

To explore IRCA's potentially varied impacts on criminal behavior, we use administrative records detailing every felony charge filed in Bexar County between 1980 and 1994. To motivate the analysis, in Figure 1, we plot the total number of alleged felonies committed by Bexar County residents between 1980 and 1994, based on the year of the alleged offense and the ethnicity of the alleged felon. Prior to the expiration of IRCA's amnesty programs in 1988, there were roughly an equal number of felonies allegedly committed by Hispanics and non-Hispanics each year. Once the amnesty expires, however, there is a clear and dramatic jump in alleged felonies committed by Hispanic residents relative to non-Hispanic residents. Compared to the pre-IRCA period, the number of alleged felonies committed by Hispanics increased by 59% in the years after amnesty expired. We show in the empirical analysis that this increase was driven not by violent offenses, but instead by felony drug offenses – income generating crimes that are a close substitute for formal work (Reuter et al. 1990, Levitt and Venkatesh 2000).

Of course, only a fraction of the Hispanic residents of Bexar County were directly impacted by IRCA. After empirically establishing that the expiration of IRCA's amnesty programs was associated with an increase in alleged felonies by Hispanic residents relative to non-Hispanic residents, particularly for drug-related crimes that have a clear economic motive, we further

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[1] On May 5, 1988, Carlos Salinas, then a candidate for president of Mexico, stated in a public address that the expiration of amnesty and increased border enforcement "cannot defeat the economic reality that is experienced in the U.S., where Mexican workers continue being needed" (Associated Press 1988).

parse our data by the probability that the accused Hispanic residents were, in fact, recent immigrants who faced increased barriers to employment. To do so, we draw on the literature on immigrant location decisions and combine our administrative data on crimes with finely detailed information on characteristics of the neighborhoods in which people were living when they committed the alleged offense. We use these neighborhood characteristics to identify residents more or less likely to have been impacted by IRCA, and thus those whose legal status and employment opportunities changed differentially as the law's provisions went into effect.

We find that the relative increase in felony offenses, and in particular felony drug offenses, among Hispanics was largest in neighborhoods with higher poverty rates, larger households, higher concentrations of people who identify themselves as Mexican, greater fractions of residents who speak Spanish at home, and larger shares of individuals who are foreign born. Based on demographic research and Census data, these are neighborhoods where Mexican immigrants are most likely to initially locate. We find no evidence that other correlates of neighborhood disadvantage that have not been identified as predictors of initial immigrant settlement patterns in Texas (e.g., local industry composition or home ownership rates) explain geographic variation in Hispanic crime rates. Our within-group estimates are robust to very weak assumptions about unobserved determinants of crime, alternative functional forms, and extreme assumptions about the growth of the (authorized and unauthorized) immigrant population.

The empirical results are consistent with a simple economic model of rational criminal behavior and also have strong implications for the relationship between immigration and crime. In particular, policies governing access to formal employment for immigrants may have unintended effects on their subsequent criminal activity. However, another possible mechanism linking immigration reform to our measure of crime is a change in the propensity of Hispanics to have felony charges filed against them. For example, if the police's treatment of Hispanics (and in particular Hispanics in immigrant neighborhoods) changed following IRCA or if newly legalized immigrants were more likely to report neighborhood crime by Hispanics to the police, we could observe more charges even in the absence of any increase in underlying criminal behavior. This is of particular concern for drug offenses, as new anti-drug policies enacted during this time period are widely thought to have contributed to heightened racial disparities in incarceration (U.S. Sentencing Commission 2009, Kennedy 2011, Neal and Rick 2014).

We differentiate between the impact of immigration reform on the behavior of recent immigrants and the behavior of law enforcement in two ways. First, we verify that our findings are driven by Hispanics as opposed to other minority groups, and in particular other minority groups also differentially affected by stricter drug policy enforcement. Second, we test more rigorously for a change in the relationship between Hispanics and the criminal justice system by examining patterns of conviction rates across ethnic groups over the same time period. We find some suggestive evidence that, after IRCA, felony charges filed against Hispanics were less likely to result in a conviction, but on average it appears that Hispanic people from immigrant enclaves were convicted at similar rates as the general population. Overall, the results are consistent with existing research on police behavior during IRCA (Bohn et al. 2015) as well as anecdotal evidence from local news articles from the period, which highlight the difficulties faced by new immigrants lacking legal documentation, but limited if any effects of the law on other populations or on police behavior.[2] Instead, the results suggest that limiting immigrants' access to legal employment increases crime, and in particular crime that is a close substitute for formal work.

The paper proceeds as follows. In Section 2, we describe the key institutional changes put in place by IRCA and summarize the existing research on the law's economic impacts. In Section 3, we discuss the theoretical framework that guides our empirical analysis. Then, in Section 4, we describe our dataset in detail and discuss how it allows us to empirically disentangle general changes in crime from those driven by immigration reform. We present our results in Section 5, and conclude with discussion in Section 6.

2. The Immigration Reform and Control Act of 1986 (IRCA)

A. Background

Confronted with a large and growing unauthorized population, Congress passed a comprehensive set of immigration reforms in 1986. Enacted on November 6, IRCA aimed both to reduce the unauthorized population by granting amnesty to resident non-citizens and to stem the future flow of unauthorized immigrants through enforcement policy at the border and in the

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[2] Specifically, as part of our analysis, we identified all articles published in the *San Antonio Express-News*, the major local newspaper, between January of 1986 and December of 1988 that referenced criminal justice policy, immigration, and local public finance. As we later discuss, the results of this search corroborate our empirical results and help us to rule out many otherwise plausible alternate explanations for our findings, such as changes in the criminal behavior of Black residents, changes in policing or the criminal justice system, or spillovers from the Mexican drug war, that would typically be mentioned in local news reports.

interior. Despite an initial decline following enactment, the unauthorized population soon resumed its upward trend and grew to over ten million by 2010 (Passel and Cohn 2011), suggesting that IRCA did not change long-term patterns of undocumented immigration (Orrenius and Zavodny 2003).

Amnesty under IRCA conferred temporary, then permanent, legal status (if applied for) for immigrants under two primary programs: a general legalization program and a program specific to seasonal agricultural workers. Nationwide, these two programs legalized roughly 1.1 and 1.6 million immigrants, respectively (Kerwin 2010). The general legalization program (LAW) required continuous residence in the U.S. since before January 1, 1982. The Seasonal Agricultural Workers legalization program (SAW) allowed flexibility on year of arrival (which could be after 1982) and length of stay (which need not be continuous) for agricultural workers meeting certain work requirements.

A companion section of the IRCA legislation augmented border and interior enforcement measures aimed at stemming the future flow of unauthorized immigration.[3] First, funds were directed to increasing infrastructure at the border in order to deter illegal crossing. Second, a set of interior measures were aimed at discouraging illegal immigration by diminishing employment opportunities for unauthorized individuals. These measures were targeted at employers. Specifically, IRCA required employers to verify the legal status of workers (by completing I-9 forms for all employees) and set forth civil and criminal penalties for knowingly hiring or recruiting unauthorized immigrants. After a large-scale campaign to educate employers about the new rules, a survey of a random sample of employers by the Government Accountability Office revealed a compliance rate with I-9 requirements of 65% nationwide, and 75% in Texas specifically, in 1989 (U.S. GAO 1990). The wage differentials that emerged between documented and undocumented workers (discussed in the next subsection) corroborate the segmentation in the labor market that arose due to the law.[4]

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[3] Passed shortly after IRCA, the Immigration Marriage Fraud Amendments of 1986 also made it more difficult to obtain citizenship through marriage by instituting a new "conditional" permanent resident status for alien spouses and children. It also made marriage fraud a crime.

[4] In part due to concerns that the potential sanctions against employers violating IRCA would lead to discrimination against some groups of authorized workers, the law also prohibited employers with four or more employees from discriminating against authorized workers on the basis of citizenship or national origin (U.S. GAO 1999). Illegal immigrants could file fraudulent paperwork to employers in an attempt to circumvent the employment verification process. However, someone caught presenting false identification could be charged with felony forgery under Texas state statute; we include forgery as an income generating offense in the empirical analysis.

Under both LAW and SAW amnesties, an applicant who could provide prima facie evidence that he or she qualified for amnesty was issued a U.S. work authorization card when he or she left the legalization office; this authorization was immediately effective and renewable until the INS made a final determination on that individual's case (Baker 1990, Hagan and Baker 1993). Additionally, after IRCA was enacted but before December of 1987, employers could legally hire individuals if they were prima facie eligible for amnesty; after December of 1987, however, only those who filed for legalization and received work authorization from the INS were legally employable (Garza v. INS 1987).

Evidence on immigration patterns as well as anecdotal reports strongly suggest that the residency requirements of both LAW and SAW programs were widely flouted. Based on surveys conducted in Mexico, Donato and Carter (1999) concluded that over 70% of LAW applications and 40% of SAW applications were likely fraudulent. A black market emerged for the documents needed to "prove" the date of entry into the U.S.; as one federal employee in California recounted, "rent receipts, food receipts… anything needed was for sale on Los Angeles streets… there were document vendors all over the place and fraud was rampant" (Oltman 2011). Further, in order to reduce the administrative burden, initial amnesty applications could be submitted by mail as well as by local community groups (such as Catholic Charities and public notaries); the latter were paid $15 per application forwarded to the legalization office and, as Baker (1990) noted, would generally accept "anything with '1981' in the file" as sufficient evidence of LAW amnesty eligibility, particularly as the deadlines neared. Despite the ease with which ineligible immigrants could collect documentation to demonstrate long-term residency and submit amnesty applications, almost all applicants were granted some form of legal status. As of 1992, only 4.5% of amnesty applications filed in Bexar County had been denied by the INS.[5]

A comparison of Census and INS data highlights the degree of systematic misrepresentation of immigrants' date of entry into the U.S on their amnesty applications. Figure 2 uses the 1990 Decennial Census to estimate the size of immigrant cohorts, legal and illegal, by year of entry. The Census data suggest that roughly 2,000 people per year moved to Bexar County permanently from outside the country in the second half of the 1960s. That number increased to about 2,700 per year in the 1970s. Annual immigration rates rose to about 5,000 in the first two years of the 1980s before falling back to roughly 2,700 between 1982 and 1984. Immigration rates rose

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[5] Authors' calculations from 1992 INS Legalization Summary Public Use Tape.

slightly in 1985 and 1986 before falling again later in the decade.

Meanwhile, Panel A of Figure 3 shows the year of entry stated on applications for amnesty under IRCA based on the 1992 INS Legalization Summary File Public Use Tape. In contrast to the Census data, which suggest that annual immigration less than doubled in the first two years of the 1980s, the INS data point to a 300% increase during that period. Further, instead of falling by half after 1981, the INS records suggest that immigration fell by 70%.

Not only is there significant bunching in self-reported, retrospective year of entry in the INS records, but almost 40% of Bexar County residents who told the INS that they arrived in 1981 reported arriving in the last three months of the year. As Panel B of Figure 3 shows, fewer than 25% reported arriving in the fourth quarter of any other year between 1970 and 1988.

The large amount of manipulation of entry dates by amnesty applicants that these figures imply, together with the low cost of obtaining false documentation of residency and lax standards for approving applications, suggest that many even technically ineligible immigrants in Bexar County (i.e., those who had only recently arrived in the country) could have been granted work authorization. Indeed, on the morning of the last day of LAW amnesty, over 500 people were lined up outside of the San Antonio INS office (Ramirez and Crouse 1988).

However, the unauthorized population of Bexar County did not fall to zero in 1988; many unauthorized immigrants, including some who were eligible, did not apply because they believed they could not prove their cases, were misinformed about requirements for amnesty, or were fearful of deportation (Ramirez 1988, Baker 1990). In addition, accounts of immigration into the U.S. during this time period suggest little, if any, reduction in the arrival rate of immigrants from Mexico in the months after the expiration of amnesty (Associated Press 1988).[6]

B. Economic Impacts of IRCA

There is broad agreement that IRCA improved the labor market opportunities of newly legalized immigrants. Kossoudji and Cobb-Clark (2002) find a wage benefit of legalization under IRCA of approximately 6%. Rivera-Batiz (1999) and Lozano and Sorensen (2011) also find positive impacts of legal status on immigrants' earnings after IRCA. Meanwhile, Amuedo-

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[6] Bustamante (1990) presents monthly survey data from immigrants entering the U.S. at Nuevo Laredo, the closest major border town to San Antonio, between November of 1987 and November of 1988. He finds that the average cost of entering the country each month over this time period was quite stable at roughly $80,000, which is not consistent with a drop in demand.

Dorantes et al. (2007) find evidence of increased wage growth and job mobility among newly legalized immigrants following IRCA.[7]

While amnesty conferred economic gains to legalized immigrants, IRCA's effects on unauthorized workers who failed to obtain amnesty were generally negative. Unauthorized immigrants who did not gain legal status or came to the U.S. after IRCA faced more limited labor market opportunities, likely a reflection of employer costs associated with sanctions or sanction avoidance (Phillips and Massey 1999, Kossoudji and Cobb-Clark 2002). A number of studies suggest that after IRCA's passage, unauthorized immigrants experienced a substantial reduction in wages, on the order of 14-24%, as well as poorer working conditions (Donato et al. 1992a, Donato and Massey 1993, Sorensen and Bean 1994, Bansak and Raphael 2001, Kossoudji and Cobb-Clark 2002). Job search durations among unauthorized workers also increased after IRCA (Bach and Brill 1991). Indeed, ethnographic research suggests that many applicants for legalization were compelled primarily out of fear of job loss (Hagan and Baker 1993). Taken together, these studies suggest that IRCA's employment measures restricted the labor market opportunities of unauthorized immigrants.[8]

3. Legal Status and Criminal Activity

To help motivate the empirical analysis that follows, we outline in this section a theoretical framework relating work, crime, and legal status. We relegate the formal model to the appendix, but discuss the intuition and several key implications here.

There are three primary channels through which legal residency status could affect decisions to engage in crime. First, legal residency status could affect wages; higher wages will tend to reduce time devoted to criminal activity. Second, legal status could affect the probability of being caught committing crime; if the propensity to report crimes differs across groups or police treat groups differently (potentially due to changes in immigration policy), crime rates (or at least

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[7] Notably, some states immediately extended coverage of various benefit programs to immigrants legalized under IRCA. Meanwhile, many federal programs, such as food stamps and Medicaid, were available to legalized immigrants five years after legalization. Any social assistance available to newly legalized immigrants will only amplify the differences in conditions faced by legal and illegal immigrants.

[8] Some studies have found that Hispanic legal workers may have faced discrimination and wage declines as a result of IRCA's employer sanctions (Bansak and Raphael 2001). However, the extent of such discrimination resulting from IRCA seems to be small (Lowell et al. 1995).

observed crime rates) may vary across groups.[9] Third, legal residency status could affect punishment if caught engaging in criminal activity. For example, immigrants in the country illegally may be deported for committing a felony; if deportation is perceived as harsher than imprisonment, it might differentially deter crime among illegal immigrants.[10]

Applied to our empirical setting, to the extent that amnesty under IRCA conferred wage benefits to those newly authorized to work in the formal market, the law should have lowered the incentive for this group to engage in illegal behavior, and in particular income generating illegal behavior such as car theft, burglary, larceny, drug sales, and prostitution. Provisions under IRCA that increased criminal penalties for newly legalized immigrants (at least during the probationary period) would tend to further dampen incentives to engage in criminal activity through the punishment channel. Over the longer run, though, perceived punishments could be lower since deportation is no longer a threat once citizenship was conferred.

At the same time, unauthorized immigrants who arrived in the U.S. after amnesty offices closed in 1988 faced barriers to work that their predecessors did not, plausibly increasing their relative return to crime. It is less clear that actual or perceived punishments immediately changed for those who arrived after relative to those who arrived before IRCA expired, as a felony conviction voided the amnesty process. Changes in the treatment of newly legalized and illegal immigrants by the criminal justice system could also influence criminal activity, although the observed effect on the crime rate for each group will depend on the elasticity of criminal activity with respect to the probability of arrest as well as law enforcement's ability to determine a suspect's legal status.

While the relationship between immigration and crime has been the topic of a number of studies (e.g., Butcher and Piehl 1998, Moehling and Piehl 2007, Bianchi et al. 2012), researchers have only recently begun to explore the crucial link between legal status and criminal activity. As highlighted in a recent review by Bell and Machin (2013), the little work that exists points to an important role for changes in economic opportunities. For example, taking advantage of exogenous variation in immigrants' legal status after a round of European Union enlargement,

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[9] Skogan (1984) hypothesizes that lower observed crime rates among immigrants could be partly attributable to lower reporting, although more recent work suggests that such differences in reporting patterns in the U.S. are not large (Davis and Henderson 2003).

[10] Greater expected punishments are one plausible explanation for the fact that Hispanic immigrants tend to commit fewer crimes on average than other groups in the U.S. with similar economic circumstances. Another explanation for the relatively low crime rates of immigrants is selection in who immigrates to the U.S. (Butcher and Piehl 2007).

Mastrobuoni and Pinotti (2015) find that obtaining legal status lowered recidivism among Italian immigrants. The reductions were relatively large among legalized immigrants in Italian regions where the informal economy was small, suggesting that access to legal jobs drove the observed decline in immigrant recidivism rates. However, because Mastrobuoni and Pinotti (2015) measure recidivism as re-incarceration in Italy, they are limited in their ability to distinguish the effects of actual declines in criminal behavior from the effects of increased mobility and resettlement of the new European Union residents. Since the policy change we examine plausibly increased, rather than decreased, criminal behavior, any differential change in mobility will lead us to understate, rather than overstate, the impact of job access on crime.

Meanwhile, Bell et al. (2013) identify substantial increases in aggregate property crime in British neighborhoods with large influxes of immigrants, but only if those immigrants were refugees legally prohibited from working. In another study on IRCA's effects, Baker (2015) finds that U.S. counties with more legalized immigrants after IRCA had lower aggregate crime rates in the 1990s. Unlike Bell et al. (2013) and Baker (2015), our individual-level data identifying both crime type and residence of the alleged offender allow us to distinguish between crimes committed by groups unaffected by immigration reform and by those directly affected by the policies. They also allow us to isolate the specific effects of restrictions on labor market opportunities, which in the case of IRCA were immediately binding for those who had not submitted required paperwork by the amnesty deadlines; though citizenship itself was not granted upon filing, work authorization was almost invariably conferred. We can also better disentangle alternative mechanisms for the observed changes in criminal activity by exploiting detailed information on neighborhood characteristics and on the treatment of individuals by the criminal justice system, an issue unexplored in previous work.

4. Data and Empirical Strategy

A. Data

The data used in this study come from several sources. First, we obtained data on felony charges filed in Bexar County District Court between 1976 and 2010. Using information on initially filed charges, we identified individuals who were accused of committing a crime that occurred between January 1, 1980 and December 31, 1994, a wide window around the date IRCA went into effect and the dates of its amnesty expirations.[11] We divided Texas statutes into

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[11] The court records also include information on actual convictions, which we exploit in Section 5.E.

two categories based on the strength of the financial incentive to commit the crime. Income generating offenses include robbery, burglary, car theft, larceny, fraud, forgery, gambling, any felony drug charge, and prostitution. Crimes that we classified as non-income generating are murder, manslaughter, assault, arson, offenses against children, kidnapping, destruction of property, sexual assault, weapons violations, trespassing, evasion of arrest, corruption, conspiracy, and public order offenses. We excluded all DUI charges, as repeat DUIs were officially classified as felonies for the first time in the late 1980s. Across our measure of neighborhoods (census block groups), there is on average one person charged with a felony every five months, and roughly three times as many income generating offenses as non-income generating crimes. This low incidence of offenses will be important to keep in mind in interpreting our results.

We next classified each defendant as either Hispanic or non-Hispanic. The court data contain a race variable that identifies defendants as Latino/Latina, White, Black, Asian, or of unknown race. However, because reported race may be endogenous, particularly when the policy we are evaluating directly affects the standing of Hispanics in the community, we devised our own objective, time-invariant measure of Hispanic origin based on last name. We first identified defendants as Hispanic if their last name was one of the 639 most frequently occurring heavily Hispanic surnames identified in Word and Perkins (1996). The origins of all surnames in the court data that were not on the Word and Perkins (1996) list were verified using Ancestry.com, and we classified anyone with a last name originating in Central or South America, Spain, or Portugal as Hispanic.[12] Overall, out of 80,398 charges filed against Bexar County residents, we classified roughly half of the accused criminals as Hispanic. Men make up 85% of our alleged felons, and 72% of charges are filed against someone between the ages of 18 and 35.

We then used mapping software to locate the census block groups in Bexar County where individuals in the data lived at the time that charges were filed against them. Census block groups are the second smallest geographic unit identified by the Census Bureau and represent the smallest areas for which the Census Bureau publishes sample data (i.e., data collected in the long-form Decennial Census, such as income information). We excluded 12 Bexar County block groups with missing demographic information, and the median population of the remaining

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[12] We identified as Hispanic 85% of people identified in the court data as Latino/Latina, 20% of people identified as White, 2% of people identified as Black, 5% of people identified as Asian, and 10% of people of unknown race.

1,001 block groups in the sample was 1,061 in 1990.

Table 1 presents descriptive statistics for our sample. San Antonio is a relatively middle-to-lower income city; on average, 16% of block group residents lived at or below the poverty line in 1990, and there were roughly six jobs for every ten adults. About 38% of housing units were rented as opposed to owner-occupied, and there are about 2.7 people per housing unit. Not surprisingly given its proximity to the U.S.-Mexico border, there is a large Hispanic population in Bexar County; in 1990, just under half of neighborhood residents identified themselves as being of Mexican descent, and almost 40% of people reported that they spoke Spanish at home. At the same time, however, only 9% of block group residents were born outside the U.S., and non-citizens constituted only 6% of neighborhoods' populations on average in 1990.

B. Difference-in-Differences Strategy

The passage of IRCA and the timing of the amnesty expiration create four natural comparison groups that allow us to isolate the impact of the law's provisions on criminal activity. First, because the majority (76%) of the foreign born population of San Antonio in 1990 was from Latin America, we assume that people who are not identified as Hispanic are not directly affected by any changes in employment opportunities and/or police behavior associated with IRCA implementation. This link between ethnicity and immigrant status is a relatively distinctive feature of southern Texas.[13]

Recall that, as shown in Figure 1, roughly an equal number of felony charges were filed against Hispanics and non-Hispanics each year prior to 1988. However, after amnesty expired, there was a clear and dramatic jump in alleged felonies committed by Hispanic people relative to non-Hispanics. To the extent that IRCA increased wages for amnesty applicants, we would expect crime rates for Hispanic residents to fall relative to non-Hispanics after 1986. However, the second critical effect of IRCA was to limit labor market opportunities for new immigrants, particularly after 1988. Therefore, among Hispanic defendants, we would expect to see a relative increase in offenses that are substitutes for formal work after amnesty offices closed.

In Figure 4, we plot the average number of felony charges filed by month in each Bexar County neighborhood, dividing charges into income and non-income generating felonies for

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[13] By comparison, 72% of the foreign born population of Texas and 44% of the U.S. foreign born population in 1990 was from Latin America (Texas State Data Center, "Number and Percent of Foreign Born Population by Region of Birth with Numeric and Percent Change, 1990 and 2000," http://txsdc.utsa.edu/reports/subject/ForeignBorn.aspx).

Hispanic and non-Hispanic defendants. Even at this level of disaggregation, there is some indication of a relative increase in income generating offenses by Hispanic residents of Bexar County after 1989. Given the month-to month noise in the graph, however, it is difficult to say whether or not this increase is statistically meaningful.[14]

We formalize this graphical analysis in a difference-in-differences framework in which we compare changes in the criminal behavior of Hispanic Bexar County residents before and after IRCA with the change in criminal behavior of non-Hispanic residents over the same time period. Our most basic formulation is equation (1):

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\begin{aligned}
\ln(Crime_{bgt}) = \alpha_b + \gamma_t + HISP_g\theta_0 + (Enact_t \times HISP_g)\theta_1 + (LAW_t \times HISP_g)\theta_2 \\
+ (SAW_t \times HISP_g)\theta_3 + \varepsilon_{bgt}
\end{aligned}
$$

(1)

where $Crime_{bgt}$ is the estimated rate of criminal charges filed against residents of census block group b, who are of ethnic group g, based on alleged crimes committed in month t.[15] We allow for time invariant differences in criminal behavior across block groups (α_b) and ethnic groups ($HISP_g$), and include a set of monthly fixed effects γ_t that allow for seasonality as well as long run trends in crime.[16] The dummy variables for IRCA enactment ($Enact_t$) and the expiration of the two amnesty programs (LAW_t and SAW_t) are equal to one in every month beginning in November of 1986, May of 1988, and December of 1988, respectively.[17]

The estimated values of θ_1, θ_2, and θ_3 capture the relative changes in criminal behavior by Hispanic residents following IRCA enactment and amnesty deadlines after conditioning out ethnicity, neighborhood, and time effects. If IRCA allowed current undocumented immigrants to obtain work authorization, and the expiration of IRCA amnesty prevented new immigrants from accessing the formal labor market, then we would expect $\theta_1<0$, but $\theta_2>0$ and $\theta_3>0$, corresponding with an opening, and then elimination, of legal labor market opportunities for undocumented

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[14] Further, Figure 4 abstracts away from any variation across neighborhoods that might affect criminal behavior, a point to which we return in Section 4.C.

[15] We add 0.001 to the rate of criminal charges filed against residents so that the dependent variable is defined for all neighborhoods. Linear probability models where the dependent variable is whether or not any resident is accused of a felony generate results of the same sign and statistical significance.

[16] The monthly fixed effects include 180 dummies, one for each month in each year in our sample (12 × 15). These subsume the IRCA enactment and amnesty date dummies.

[17] Recall that IRCA was enacted on November 6, 1986, the LAW amnesty expired on May 4, 1988, and the SAW amnesty expired on November 30, 1988. Using the exact timing of these changes, particularly the LAW expiration, is critical for our analysis. At the federal level, the Anti-Drug Abuse Act of 1986, which established mandatory minimum sentences for federal drug offenses, was enacted on October 27, 1986 and led to a sharp increase in all felony drug charges in 1986 and 1987. Texas revamped its drug policy on September 1, 1989 with the passage of the Texas Controlled Substances Act.

immigrants. The timing of the amnesty rollout is central to our identification; our estimate of θ_2, for example, is driven by alleged felonies occurring in the seven months between May of 1988 and November of 1988. We allow for arbitrary correlation in crime rates within neighborhoods over time by clustering our standard errors by census block group.

Estimating the size of the population at risk of engaging in crime is complicated by the absence of high-frequency data on Hispanic and non-Hispanic populations at fine levels of geographic resolution. In our baseline specification, we construct an estimate of the Hispanic and non-Hispanic populations of census block groups each year by linearly interpolating the ethnicity-specific population between the 1980 and 1990 censuses, and extrapolating population growth after 1990.[18] However, while existing evidence suggests that the flow of people into Bexar County changed little, the stock of new immigrants in the county likely increased in a discrete way around IRCA. Durand et al. (1999) argue that IRCA curtailed return migration to Mexico among Mexicans living in the U.S, increasing the stock of new immigrants. Consistent with this, data on the number of babies born to a Hispanic parent from the National Center for Health Statistics' Vital Statistics Database suggest that Hispanic population growth in Bexar County was relatively fast as IRCA rolled out compared to the early or late 1980s. Failure to account for these nonlinear changes in the Hispanic population over time will bias our crime rate estimates upward.

Therefore, we construct a second measure of neighborhood population change during our sample period. For this alternative measure, we assume that the entire change in each block group's population between 1980 and 1990 occurred in May of 1988, which corresponds to the expiration of the first major amnesty program and therefore the period in which we would expect the largest increase in crime. Obviously, this population growth path is also incorrect; county-level data on Hispanic births suggest that the biggest population increase occurred between the enactment of IRCA in 1986 and the expiration of amnesty. However, by forcing all the population change to occur at the start of the post-amnesty period, these estimates will be lower

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[18] Census geographies are inconsistent over time. Constructing estimates of the 1980 populations of 1990 block groups involved a number of steps. First, we mapped the 1990 block groups (our geographic unit of analysis) onto 1980 census tracts (for which we have population data). This gives us the ethnicity-specific counts of people in the 1990 block group-grouping in 1980. We then allocated the 1980 tract populations across 1990 block groups in proportion to 1990 population shares. We are forced to exclude 1.4% of our total ethnicity-block group observations because there are no people of that specific ethnicity in that 1990 block group-grouping. In later robustness tests, we compare Hispanic residents to non-Hispanic Black residents (about 7% of the Bexar County population in 1990), in which case we are forced to exclude 3% of our ethnicity-block group observations.

bounds on the true change in the propensity for Hispanic and non-Hispanic residents of different block groups to commit crime in this period.

C. Triple Differences Using Proxies for Immigrant Destinations

While we can use equation (1) to quantify the differential change in overall criminal behavior as well as in income and non-income generating offenses by Hispanic people around IRCA, it does not differentiate between the criminal activity of Hispanic U.S. citizens, the criminal activity of Hispanic immigrants who gained legal status through the amnesty, and the criminal activity of Hispanic immigrants who did not receive amnesty because they immigrated after the deadline or because they failed to provide documentation of previous U.S. residence. Unlike in many other countries, offenders' nativity is not formally collected by most criminal justice agencies in the U.S., as immigration violations are federal offenses, and most crimes are state offenses. This difference in jurisdiction complicates any effort to differentiate local crime by immigration status. We attempt to isolate the behavior of Hispanic immigrants who did not receive amnesty by allowing for heterogeneity in θ_1, θ_2, and θ_3 across neighborhoods where recent immigrants are more likely to live, based on demographic characteristics recorded in the 1990 Decennial Census.

The goal is to identify, at any given time, Hispanic residents who were more or less likely to be recent immigrants, and who were therefore differentially affected by the expiration of IRCA's amnesty programs. Intuitively, the expiration of LAW and SAW should not have directly affected employment opportunities for Hispanic U.S. citizens. However, Hispanic individuals who entered the country illegally after IRCA's amnesty deadlines were suddenly unable to legally acquire the documentation necessary to find a job in the formal sector, which in turn sharply limited their employment options. If the increase in felony behavior by Hispanic residents observed in Figure 1 is driven by reduced employment opportunities after IRCA, we would expect any jump in criminal behavior among Hispanics relative to non-Hispanics to be largest in neighborhoods where more Mexican immigrants first settled. We identify these neighborhoods as places with higher poverty rates, more residents per housing unit, more people of Mexican descent, a higher fraction of adults who speak Spanish at home, and a higher fraction of foreign born residents.

We chose these particular demographic variables because of their well-established correlation with new-immigrant destinations in the U.S. generally, and in San Antonio

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specifically. There is strong evidence in demography and population research that immigrants tend to live in poorer neighborhoods before moving to "higher quality" neighborhoods, a process commonly referred to as spatial assimilation (Massey 1985). Immigrants also tend to live in more crowded housing than natives (Krivo 1995). For example, in 2005, roughly 15% of foreign born, non-U.S. citizens lived in housing with more than one person per room, compared with 1% of people born in the U.S. (Blake et al. 2007). Mexican immigrants in San Antonio in particular tend to live in urban areas (Telles and Ortiz 2008).

In addition to living in poorer neighborhoods, immigrants who enter the U.S. illegally are also more likely to settle in ethnic enclaves (Bartel 1989).[19] Therefore, we also identify areas where more people are likely affected by IRCA by using residents' self-reported national origin. Notably, those of Mexican descent (who constitute roughly half of San Antonians) include both immigrants and U.S. citizens, and plausibly many high socio-economic status San Antonians who are unlikely to live near recent illegal immigrants.[20] Therefore, we refine our proxy for ethnic enclaves that might be attractive to new immigrants by identifying neighborhoods in which more people speak Spanish. To the extent that recent immigrants have poorer English language skills, neighborhoods where more people speak Spanish in casual conversation are likely to be more attractive. Finally, recent immigrants may be more likely to settle in neighborhoods where more people were born outside the country. Indeed, at the state level, the size of the foreign born population is one of the strongest predictors of settlement patterns (Dunlevy 1991, Zavodny 1999). Therefore, we also use the fraction of residents that are foreign born as a final measure of the location of recent immigrants.

In Figures 5-9, we present differences in criminal incidence by ethnicity and crime type across neighborhoods with high and low poverty, high and low population density, high and low fractions of the population of Mexican descent, high and low fractions of the population that speak Spanish, and high and low fraction immigrants. In each case, "high" and "low" are defined as block groups in the top quartile and bottom quartile of all block groups in 1990 of the

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[19] As Bell and Machin (2013) note, the historical concentration of co-ethnics and immigrants are frequently used as instruments for the location decisions of new immigrants in quasi-experimental research.
[20] At the same time, Duncan and Trejo (2011) present evidence that more educated citizens of Mexican descent are less likely to identify their Mexican origin on Census forms than less educated citizens of Mexican descent.

respective characteristic.[21]

Notably, if police merely began targeting Hispanics more after IRCA, we would not expect to see differential trends in offenses across neighborhoods that likely had more or fewer new immigrants based on our proxies, nor would we expect to see large differences across income and non-income generating crimes among Hispanics. Further, if police merely increased their presence in higher poverty or immigrant neighborhoods around the time of IRCA, we would not expect to see differential trends in offenses across Hispanics and non-Hispanics in each type of neighborhood, nor would we expect to see marked differences across income and non-income generating offenses in each type of neighborhood. Similarly, if there were changes in criminal opportunities generated by the increased earning power of IRCA beneficiaries after the reform, we would expect property crime to increase, and increase by potentially more in immigrant enclaves, but this increase should be driven by all neighborhood residents, not just Hispanic residents. Finally, if police merely began targeting income generating offenses more around this time period (possibly because of changes in drug policy), we would not expect to see differential trends in these income generating offenses among Hispanics and non-Hispanics, nor would we expect to see differences in trends in income generating offenses across neighborhoods with more or fewer immigrants according to our proxies. Changes in employment opportunities due to IRCA would be expected to generate a relative increase specifically in income generating offenses among Hispanics as compared to non-Hispanics that is concentrated in neighborhoods that are likely destinations for new immigrants.

The top panel of Figure 5 clearly shows a relative increase in income generating criminal behavior among Hispanic people living in poorer neighborhoods in May of 1988, the month when LAW expired. Meanwhile, there is no clear change in the poverty-crime gradient (i.e., the relationship between poverty rates and crime rates) for non-Hispanics over this time period. In the lower panel of Figure 5, which shows the same differentials for non-income generating

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[21] Our proxies for new-immigrant destinations are also plausibly correlated with the Hispanic and non-Hispanic population. Importantly, however, there are Hispanic and non-Hispanic people living in the neighborhoods that are both most and least likely to be enclaves. The high poverty neighborhoods have, on average, 898 Hispanic residents and 130 non-Hispanic residents, while the low poverty neighborhoods have 233 Hispanics and 977 non-Hispanics. For density, the corresponding numbers are 838 Hispanics and 454 non-Hispanics (high) and 356 Hispanics and 679 non-Hispanics (low). For Mexican descent, the numbers are 988 Hispanics and 84 non-Hispanics (high) and 163 Hispanics and 991 non-Hispanics (low). For Spanish-speaking, the numbers are 961 Hispanic residents and 88 non-Hispanics (high) and 187 Hispanics and 1,051 non-Hispanics (low). For immigrants, the numbers are 782 Hispanics and 221 non-Hispanics (high) and 317 Hispanics and 835 non-Hispanics (low).

criminal behavior, there is perhaps a widening of the poverty-crime gradient for Hispanic and non-Hispanic residents, but it is much noisier and less dramatic than that for crimes with a clear financial motive. This pattern is repeated when we look across high and low density neighborhoods in Figure 6, based on people per housing unit. There may be a slight pre-trend in the density-crime gradient for Hispanics, but there is clearly a large divergence in the relative income generating criminal behavior of Hispanic people living in more crowded neighborhoods around the expiration of the amnesty programs. We do not observe the same divergence for crimes that are not obvious substitutes for work. As is apparent in Figures 7 and 8, Hispanic residents of heavily Mexican and Spanish speaking neighborhoods also engage in relatively more income generating crimes after IRCA's amnesty expiration. This is in contrast to the trends for non-income generating crimes, which are similar throughout the period for Hispanic and non-Hispanic residents in heavily Mexican and Spanish-speaking neighborhoods. As Figure 9 shows, the same general findings hold for the fraction of the population that is foreign born, although the differences are not as stark.

These patterns across neighborhoods suggest that an extension of our difference-in-differences strategy can better isolate changes in criminal behavior attributable to the IRCA policy changes. In particular, we can exploit a triple-differences approach as follows:

$$
\begin{aligned}
\ln(Crime_{bgt}) = {} & \lambda_b + \eta_t + Hisp_g\delta_0 + (Enact_t \times Hisp_g)\delta_1 + (LAW_t \times Hisp_g)\delta_2 \\
& + (SAW_t \times Hisp_g)\delta_3 + (Enact_t \times Demo_b)\phi_1 + (LAW_t \times Demo_b)\phi_2 \\
& + (SAW_t \times Demo_b)\phi_3 + (Hisp_g \times Demo_b)\mu_1 + (Hisp_g \times Demo_b)\mu_2 \\
& + (Hisp_g \times Demo_b)\mu_3 + (Enact_t \times Hisp_g \times Demo_b)\beta_1 \\
& + (LAW_t \times Hisp_g \times Demo_b)\beta_2 + (SAW_t \times Hisp_g \times Demo_b)\beta_3 + v_{bgt}
\end{aligned}
$$

(2)

where $Demo_b$ is, alternately, the poverty rate, the number of residents per housing unit, the percent of residents who are of Mexican descent, the percent who speak Spanish at home, and the percent who are foreign born.[22] While any of these proxies may have direct effects on crime levels in a given neighborhood, in this triple-differences framework, our identifying assumption is that any correlation between these proxies and the change in the criminal behavior of Hispanic residents relative to non-Hispanic residents around IRCA's enactment operates only through the fact that these proxies are correlated with new immigrant location choice, and that any variation

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[22] Note that the block group effects subsume the first-order effects of the demographic variables and that the monthly time dummies subsume the IRCA enactment and amnesty date dummies.

in new immigrant location choice and the change in the criminal behavior that is not correlated with these proxies is uncorrelated with any of our other control variables. In that case, β_1, β_2, and β_3 represent the differential change in criminal behavior at each stage of IRCA among Hispanic people who, based on their neighborhood of residence in Bexar County, were most likely to be affected by the policy's restrictions on employment.[23] If the observed change in crime is driven by changing economic opportunities for new arrivals, we would expect that any increase in criminal behavior among Hispanics would be greater in neighborhoods with larger populations of more recent immigrants.[24]

Each of our demographic measures is essentially a proxy for the probability that a new Mexican immigrant would choose to move into that neighborhood. Since these measures are also highly correlated with each other, the estimated values of β_1, β_2, and β_3 include both the actual change in, for example, the observed poverty-crime gradient for Hispanic residents in that neighborhood over time as well as the unmeasured change in the housing density-crime gradient and Spanish-speaking-crime gradient. At the same time, if all proxies are entered into equation (2) simultaneously, the marginal impact of any one measure is not particularly meaningful. In lieu of this, or other possible measures based on normalization, we will present estimates of β_1, β_2, and β_3 derived from equation (2) where each proxy is entered separately in order to establish the sign of the differential change in criminal behavior in new-immigrant destinations. We will also present F tests of the joint significance of triple-interaction terms when all proxies are simultaneously included in one regression. In these "multiple proxy" regressions, we will include

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[23] We have also estimated equation (2) at the census tract level, incorporating measures of change in neighborhood characteristics (from the 1980 to 1990 Census) as well as the level values. This tract-level analysis has the drawback of lower precision because of fewer geographic observations. Nonetheless, the tract-level results are qualitatively similar to the block group-level results. We have also replicated our analysis using 1980 census block group characteristics, with felony defendants assigned to 1980 block groups. Results using 1980 measures are also qualitatively similar to those presented here. Consistent with that, in our tract-level analysis, we find that 1990 levels of the neighborhood characteristics, rather than percentage point changes in those characteristics between 1980 and 1990, are driving the observed differences in criminal behavior.

[24] Figures 8 and 9 introduce a potential concern about our identification strategy. While we see the same large jumps in the Spanish-crime and immigrant-crime gradients for Hispanics at the time of the policy change, there is also a slight decrease in the crime gradients for non-Hispanics; non-Hispanics living in neighborhoods with many Spanish speakers or immigrants appear to commit fewer income generating crimes relative to non-Hispanics in neighborhoods with fewer Spanish speakers or immigrants. To the extent that this drop is not differenced out by any of our fixed effects or population changes, our triple-differences approach that focuses on Spanish-speaking and immigrant concentration would overstate the impact of IRCA on Hispanic crime. We must therefore be careful to confirm that our results are driven by a change in the behavior of Hispanics, rather than two simultaneous changes in the behavior of both ethnic groups that varied across place. We address this issue in Section 5.B.

interacted neighborhood-level demographic measures that are plausibly related to both poverty and crime, but are not identified in the demographic literature as being determinants of new-immigrant destinations: the natural log of population, the percent of workers in agriculture, the percent of immigrants who moved to the U.S. "to stay" after 1985, and the fraction of housing units that are owner occupied.[25]

One potential concern is that any observed change in crimes in Hispanic neighborhoods is driven not by a change in actual criminal activity, but instead by a change in the behavior of the criminal justice system (Bohn et al. 2015). Recent reviews of the literature emphasize the role of the criminal justice system, and sentencing policy in particular, in driving the explosive growth in incarceration in the 1980s and 1990s (Neal and Rick 2014). Police and initial prosecutors are unlikely to have information about someone's legal status, but can plausibly observe whether or not someone is Hispanic and may have responded to immigration reform by changing their propensity to arrest and file charges against Hispanic residents.

In our empirical analysis, we address this concern in several ways. First, we explore whether the change in felony charging is due to a change in individual behavior or a change in the criminal justice system by re-estimating equations (1) and (2) for income generating and non-income generating crimes separately. If police responded to IRCA by patrolling Hispanic neighborhoods more heavily, or if newly legalized immigrants were more likely to contact the police, we would expect to see increases in all crimes. Alternatively, if police simply became more aggressive in their monitoring of income generating crimes, we would expect Hispanic and non-Hispanic crimes in neighborhoods to increase in proportion to the fraction of people in that neighborhood who are Hispanic or non-Hispanic. Finally, as described further in Section 5.E., we more explicitly test for changes in the behavior of the criminal justice system by examining conviction rates using the same analytic framework described above.

5. Results

A. *Difference-in-Differences Estimates*

In Table 2, we present our first set of results for felony charges based on estimating equation

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[25] The percent of the population working in agriculture is a positive predictor of the Hispanic crime gap after the expiration of SAW. However, an examination of the time pattern of criminal behavior across this margin (see Appendix Figure A1) reveals that this gap appears after 1990, suggesting that this is unlikely to be a direct impact of immigration reform. This is not surprising given the small fraction of Bexar County workers employed in agriculture at the time (less than 1%).

(1), which is aimed at establishing the differential change in the criminal behavior of Hispanic residents around IRCA. In subsequent sections, we exploit a triple-differences approach to further differentiate between the behavior of Hispanic residents more and less likely to be affected by immigration reform.

Consistent with Figure 1, we estimate that the expiration of the first IRCA amnesty (LAW) was associated with an approximately 11% increase in the incidence of felony charges filed against Hispanic residents relative to non-Hispanic residents, or roughly 20 additional charges per month countywide (see the first column in Table 2). The expiration of the second amnesty (SAW) did not appear to affect this outcome, which is not surprising given the relatively small share of workers in agriculture in Bexar County (less than 1%).[26] In the second column, we focus only on income generating offenses, and estimate that there was an 8% increase in the propensity of Hispanic residents to be charged with these crimes relative to their non-Hispanic neighbors after LAW amnesty expired. Notably, we find a much smaller increase in offenses for which there is no clear economic motive – a 4-5% increase in charges filed against Hispanics for non-income generating crimes after the expiration of LAW. Neither type of crime appears to change differentially for Hispanics after the expiration of SAW.

Overall, these results indicate that the expiration of IRCA amnesty, which cut off access to formal work for later immigrants, was associated with a disproportionate increase in the rate of felony charges being filed against people of Hispanic descent, and in particular charges for income generating crimes. This is consistent with employer sanctions for hiring illegal immigrants put in place under IRCA limiting employment opportunities and thereby increasing the relative return to crime for later immigrants. Other plausible channels through which IRCA would affect crime, such as increased policing in immigrant neighborhoods, a greater willingness among legal immigrants to contact the police, or more attractive criminal opportunities, would also increase reported criminal activity, but would not predict the differentially large effects for income generating crime committed by Hispanic residents.[27] Meanwhile, the harsher penalties

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[26] As shown in Appendix Table A1, the estimates are all highly robust to the replacement of block group fixed effects with 1990 block group characteristics.

[27] There is very little convincing evidence on the extent to which crime reporting rates among immigrants differ from other groups, much less whether there are differences across authorized and unauthorized immigrants or across different types of crimes (Bell and Machin 2013). This problem affects all studies on the relationship between immigration and crime. However, as we discuss in the next section, we find that income and non-income generating crimes actually move in different directions after IRCA in new-immigrant neighborhoods, which is stronger

for amnesty applicants during probation and any effects of IRCA on family reunification would predict not only declines in crime among Hispanic residents, but declines that predate the expiration of amnesty.

B. Triple-Differences Estimates Using Proxies for Immigrant Destinations

We now exploit the geographic information in our court data and estimate equation (2) to determine if these changes were larger in neighborhoods where more people affected by IRCA plausibly lived. After amnesty expired, recent immigrants no longer had a way to obtain legal jobs. We use neighborhood characteristics in the 1990 Decennial Census to attempt to estimate where new immigrants were more likely to have settled.

In Table 3, we begin by examining charges filed against Hispanics and non-Hispanics in new-immigrant neighborhoods, taking into account time invariant differences in criminal behavior across ethnicities as well as across neighborhoods and allowing for arbitrary monthly shocks to criminal activity in the county. In the interest of space, we present estimates of only the triple-interaction coefficients, and leave estimates of lower level interactions of ethnicity, time, and neighborhood demographics to Appendix Table A2. For each type of crime (all crimes, income generating crimes, and non-income generating crimes), each of the five top panels in Table 3 reports the results from a single regression in which we include one of the proxies for new-immigrant destinations. The bottom panel contains p-values of the joint significance of the triple interaction terms on all of the new-immigrant destination proxies.

It is clear that the impact of IRCA on Hispanic criminal behavior was not uniform across neighborhoods. Indeed, as the results in the first column of Table 3 show, patterns of poverty, population density, and ethnic composition help to explain the increase in overall felony charges filed against Hispanics in the wake of IRCA. After the expiration of LAW, the increase in Hispanic felonies was a statistically significant 0.5 percentage points greater for each percentage point increase in the block group poverty rate, ten percentage points larger for each additional person per housing unit, 0.4 percentage points higher for each additional percent of residents of Mexican descent, 0.6 percentage points higher for each percentage point increase in residents speaking Spanish at home, and 1.3 percentage points higher for each additional percent of the population that is foreign born. While each particular estimate reflects both the impact of the

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evidence that the observed patterns are not driven by changes in reporting behavior (which we would at least expect to lead to movements in different types of crime in the same direction).

individual demographic measure plus that of the unobserved correlated proxies, it is notable that all of the effects are positive. Further, while each is likely biased upward, the individual point estimates are of plausible size if immigrants arriving after May of 1988 faced a 24% wage penalty (Rivera-Batiz 1999, Kossoudji and Cobb-Clark 2002) and the wage elasticity of crime is roughly negative one (Grogger 1998). For example, in a neighborhood where 65% of the population spoke Spanish at home (one standard deviation above the mean), the data imply that crime by Hispanic residents increased by roughly 26% relative to non-Hispanic residents.[28]

Based on results from a single regression in which we include all the demographic interactions, there is only a 5% probability that the expiration of amnesty in May of 1988 did not differentially affect the criminal behavior of Hispanic people living in new-immigrant destinations. Notably, we observe that a few proxies for new-immigrant destinations, particularly primary language and presence of self-identified people of Mexican descent, also correspond with an increase in Hispanic felonies after the enactment of IRCA in November of 1986. However, we fail to reject the null hypothesis that our full set of proxies cannot explain any post-enactment behavioral change.

It is important to emphasize that these results do not simply reflect changes in the poverty-crime gradient over time, as the observed increases in offending are occurring specifically among Hispanic (but not non-Hispanic) residents of these neighborhoods. To explain the results, there must have been a shock that not only differentially affected criminal activity in neighborhoods where, according to our proxies, new immigrants were more likely to have settled, but that also increased the propensity of Hispanics to commit crimes relative to non-Hispanics.[29] IRCA is the most plausible candidate given its timing and the particular populations it affected.

It is also worth noting that the observed effects are driven by increases in criminal activity among Hispanic residents of immigrant neighborhoods as opposed to decreases in criminal activity among control populations. In unreported regressions, we estimate the effects of IRCA on criminal activity among Hispanics and non-Hispanics in immigrant neighborhoods separately. While we find negative effects of IRCA on non-Hispanic crime in immigrant neighborhoods, the effects are not only statistically insignificant, but they are also generally an order of magnitude

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[28] This estimate is based on a model without block group fixed effects, which allows us to identify the coefficient on the second-order interaction of LAW × Hispanic Defendant (-0.104; see Appendix Table A2).

[29] This shock must have also not been covered in the local media, as our review of all issues of the *San Antonio Express-News* published during this time period yielded no other plausible explanation for this pattern of results.

smaller than the positive and statistically significant effects we find for Hispanic crime in immigrant neighborhoods. This is true for all of our proxies for new immigrant neighborhoods.

Disaggregating the results by crime type provides further corroborating evidence that immigration reform, and the restrictions on employment for illegal immigrants that came with it, are responsible for the observed increases in offenses among Hispanics in immigrant neighborhoods. If the increase in crime was driven by a reduction in expected wages for new immigrants after amnesty expired, then we would expect to see a larger reduction for crimes that are substitutes for work. In the second and third columns of Table 3, we show that this is the case. While we found that, on average, the enactment of IRCA and the introduction of the I-9 form was not statistically associated with a change in income generating felonies by Hispanics on average, we find some evidence that as we focus on neighborhoods where people were less likely to qualify for amnesty, there is more likely to be an increase in income generating crime. The gap between felony behavior by Hispanics and non-Hispanics widens further after the expiration of LAW closed off legal employment opportunities for new immigrants. When all proxies are taken into account, we estimate that there is an 8% chance that Hispanic crime in new-immigrant destinations did not increase after the enactment of IRCA, but there is only a 0.5% chance that the expiration of amnesty did not increase income generating felony behavior of the people most likely to be affected by the law.

Not only is the impact of amnesty expiration on income generating crimes more precisely estimated than the impact on more violent offenses, but the estimated percentage point increase in income generating crimes is typically an order of magnitude larger than the increase in non-income generating offenses. Notably, though, when we include multiple proxies to identify where new immigrants are most likely to locate, we estimate that there is a 4% chance that there was no change in the involvement of Hispanic people living in new immigrant neighborhoods in non-income generating offenses after IRCA was enacted. However, looking at each proxy individually, it appears that if anything, non-income generating crime among Hispanic residents of Bexar County declined after the enactment of IRCA. After the expiration of LAW, we find no evidence of any additional differential changes in Hispanic involvement in non-income generating offenses. This is again inconsistent with the results being driven by changes in police behavior or crime reporting patterns, which we would expect to lead to movements in these different types of crime at least in the same direction. Meanwhile, the expiration of SAW, the

agricultural amnesty program that was less important in San Antonio, does not appear to be associated with any further change in income or non-income generating felony behavior that is correlated with the spatial distribution of new-immigrant enclaves.

C. Robustness

The results are robust to a variety of alternative specifications. For example, in Appendix Table A3, we take advantage of the high frequency, spatially disaggregated nature of our data to include a larger set of fixed effects. Specifically, we allow for arbitrary, time invariant differences in the Hispanic and non-Hispanic crime rates that are unique to each neighborhood, fully flexible neighborhood-specific crime trends in each neighborhood, and general, undefined, month-to-month shocks to the crime rates for Hispanics relative to non-Hispanics. With this comprehensive set of fixed effects, any other plausible explanation for the observed change in the behavior of Hispanic people must not only occur at the same time as the key months of immigration reform, but also only affect the Hispanic residents of the specific neighborhoods of San Antonio where new immigrants are most likely to settle. The introduction of all of these undefined variables increases our standard errors by about 30%, but the magnitudes of the observed single-proxy effects are essentially unchanged. Overall, the pattern of coefficients suggests that income generating crime by Hispanic residents increased after IRCA, and that this increase was larger in immigrant destinations.

In an additional test presented in Appendix Table A4, we provide lower bounds on our estimates by assuming that the entirety of the change in population in each neighborhood between 1980 and 1990 occurred in May of 1988 (instead of assuming a linear population growth rate). Even with this extreme assumption about population growth at the moment that access to legal employment is cut off, our individual estimates of the geographic pattern of crime increases after LAW are only slightly smaller than those from regressions in which we assumed linear population growth.

Notably, when we do not scale the dependent variable by estimated population, our estimates of the differential change in Hispanic felonies are qualitatively similar and estimated with equal precision. This is true for the natural log of crimes, the number of crimes, and a linear probability model for any criminal behavior by a block group resident.[30] The consistency of the results across alternative measures of criminal activity mitigates concerns about our measures of

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[30] These results are available upon request.

population failing to capture patterns of immigration over the 1980s accurately, as well as concerns about systematic undercounting of immigrant populations in the Decennial Censuses (Costanzo et al. 2001).

D. Drug Offenses

Roughly one third of our income generating offenses are drug felonies. These income generating crimes are of particular interest for a number of reasons. First, while not directly on the Mexican border, Bexar County is generally considered to be a hub for cross-border drug activity, and has been designated a High Intensity Drug Trafficking Area since the U.S. Office of National Drug Control Policy was created in 1990. Notably, however, while precise information on the origin and evolution of Mexican drug cartels is scarce, major events in Mexican drug policy bracket, rather than coincide with, the rollout of IRCA.[31] More so than drug trafficking or violence, the main driver of Mexican emigration in the 1980s was deteriorating economic conditions in the country (Donato et al. 1992b).

Second, while burglary, robbery, and theft are income generating offenses, involvement in drug selling shares even more characteristics with a typical legal job; individuals plausibly sell drugs explicitly to earn money rather than to also seek some sort of thrill (Reuter et al. 1990, Levitt and Venkatesh 2000). In that sense, it is conceptually closer to a substitute for legal work.

Third, to the extent that immigrants who obtained work authorization through amnesty were able to earn higher wages, and those immigrants also lived in new immigrant destinations, some of our results could be explained by an increase in criminal opportunities rather than reduced wages (Freedman and Owens 2015). As previously mentioned, it is not obvious that an increase in criminal opportunities would have differentially affected Hispanic people in new immigrant destinations. However, differentiating between property crimes, for which opportunities may have increased, and people charged with trying to earn money through drug sales provides additional evidence on this issue.

Finally, immigrants, and in particular recent immigrants with strong social ties in other countries, face lower transportation costs in illegal international trade. This may give them a comparative advantage in selling drugs relative to, for example, stealing cars and selling them for

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[31] Mexican government expenditure of crop eradication increased dramatically in 1985, in part in response to the murder of undercover DEA agent Enrique Camarena by the Gulf Cartel (Astorga 1999). In 1989, the Mexican police arrested the head of the head of the Sinaloa Cartel, increasing the market share of the Gulf Cartel, to which newly elected (in December of 1988) Mexican President Carlos Salinas was later allegedly connected (Grillo 2011).

scrap (Reuter 2004).

In Table 4, we focus only on the incidence of alleged drug felonies, which are clearly driving the relationship between income generating crimes and immigration policy. Entered individually, each of our proxies for new-immigrant destinations is positively related to the increase in Hispanic offending compared to non-Hispanic offending after the enactment of IRCA. There is an even larger increase in drug offending after new immigrants were no longer able to apply for amnesty, an effect that remains highly statistically significant even when we include our full set of fixed effects (see Appendix Table A5).[32]

One important caveat in interpreting the increase in alleged drug felonies as an increase in income generating crime is the well-established fact that the wave of drug laws passed in the 1980s and early 1990s had a disproportionate impact on the incarceration rates of minorities (Neal and Rick 2014). While our finding that Hispanic drug offending differentially increased in new-immigrant destinations is robust to the inclusion of neighborhood and Hispanic-specific monthly fixed effects, it is still possible that our estimates are picking up some as-of-yet uncontrolled for change in the policing and prosecution of minorities. In the second column of Table 4, we eliminate all drug felonies allegedly committed by non-Hispanic white residents from our sample (roughly 4,000 of the 9,400 non-Hispanic drug defendants in our sample). While this exclusion reduces our point estimates slightly, it remains clear that Hispanic people became disproportionately more likely to be accused of felony drug offenses relative to other minority groups after IRCA closed off access to legal work, first by introducing I-9 forms and then by cutting off access to documentation necessary to complete these forms, and that this effect was concentrated in new-immigrant destinations.[33]

The fact that the estimates do not get larger with the exclusion of non-Hispanic white residents also suggests that the results are unlikely to be driven by substitution between Black and Hispanic residents in the labor market, which could lead to less criminal activity in the former group (as in Borjas et al. (2010)). In unreported regressions, we also find that crime

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[32] Notably, the Hispanic drug crime gap actually narrows in new-immigrant destinations after November of 1988, which could plausibly coincide with the September 1 enactment of the Texas Controlled Substances Act of 1989. The results suggest that the Texas Controlled Substances Act may have had a differentially negative effect on the crime rates of Hispanic people in new-immigrant neighborhoods.

[33] This also helps to address concerns that the effects are driven by changes in gang-related activity, which was documented among Hispanics as well as other minority groups. Gangs were also not as prevalent around the time of IRCA as in subsequent years; the San Antonio Police Department formed its gang unit in 1991 (Duff 1994).

among Hispanic residents of immigrant neighborhoods rises in absolute terms at the IRCA dates, which further implies that the results are not driven by changes in criminal activity in the control populations.

E. Criminal Justice System Response

It is plausible that the treatment of certain groups by the criminal justice system changed in response to immigration reform in 1986. Our estimates of the impact of IRCA on crime could be biased upward if, in response to the passage of IRCA or the expiration of IRCA amnesty, police focused more of their attention on Hispanics living in immigrant communities or prosecutors became more likely to file charges against immigrants. In order to shed light on the potential importance of changes in the criminal justice system, we examine how conviction rates vary around the time of immigration reform. To the extent that criminal justice system behavior is one of the mechanisms driving the observed increase in felonies among Hispanics, then the marginal Hispanic resident accused of a felony after IRCA should, all else being equal, be less criminal and thus less likely to be convicted than the marginal resident charged prior to IRCA. The intuition behind this idea is that if police and prosecutors "cast a wider net" in the immigrant community after IRCA, we would observe more Hispanics charged with felonies, but in the absence of an increase in the underlying criminality of Hispanic residents, fewer of these accused felons should be convicted.[34]

We implement this by estimating a modified version of equation (2) in which we replace the dependent variable with the fraction of charges brought against residents living in block group *b* of ethnicity *g* for crimes committed in month *t* that result in conviction. Note that the number of observations will vary across crime type, as the conviction rate is undefined in block groups and time periods in which no alleged crimes occurred.[35]

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[34] Using variation in conviction rates to test for variation in charging practices is an extension of the hit rate test for racial profiling proposed in Knowles et al. (2001). Suppose that police and prosecutors maximize the number of successful felony prosecutions, subject to the cost of obtaining evidence, negotiating a plea agreement, and prosecuting a case at trial. Even if there is variation in the actual underlying criminal culpability of defendants across ethnic groups, as long as it is equally costly to bring charges against all Bexar County residents, court agents will file felony charges against Hispanic and non-Hispanic residents in such a way that the fraction of cases resulting in conviction are equal across ethnic groups. However, if prosecutors or police gained some additional utility from arresting and prosecuting immigrants after immigration reform, then we would see the fraction of charges that result in convictions among probable new immigrants fall over time, as criminal justice agents gave up some of the gain from conviction in exchange for this discrimination-based utility gain.

[35] Alternately, we could replace the dependent variable with the number of people convicted per 1,000 residents. The triple-difference estimates using this conviction rate are very similar to our main results.

We present our estimates of the change in conviction rates for Hispanics living in immigrant destinations in Table 5. Notably, because many of the estimated coefficients are very small, we multiply the dependent variable by 100, putting it on a different scale than the charge rates in previous regressions. Based on the results in Table 3, after the expiration of LAW amnesty, the increase in income generating felony charges against Hispanics was 0.54 percentage points greater for each percentage point increase in the block group poverty rate. As the results in Table 5 show, at the same time that charges increased, there was a simultaneous, very imprecisely estimated 0.03 percentage point increase in the probability that those charges resulted in conviction. For other proxies, we observe statistically insignificant reductions in the probability that felony charges for income generating offenses against Hispanics in new-immigrant neighborhoods result in convictions after LAW. When we combine all of the demographic interactions in the same regression, no individual proxy is statistically significant on its own.

While we could easily reject the null hypothesis that our proxies for immigrant destination were unrelated to the incidence of alleged felonies by Hispanics after amnesty in Table 3, here we cannot reject the null that conviction rates were unrelated. There is at least a 30% probability that overall conviction rates did not change differentially for Hispanics in immigrant destinations after LAW expiration, and almost a 32% chance that there was no change in the prosecution of non-income generating offenses after LAW expiration. However, it is worth noting that for income generating offenses, there is a lower probability of a true null effect. Specifically, there is a 16% probability that there was no differential change in the conviction rates of Hispanics living in immigrant destinations after the expiration of the primary amnesty program.

Overall, though, conviction rates were not changing in way that would suggest that variation in criminal activity as measured by charges filed is attributable to changes in the treatment of Hispanic residents, and in particular Hispanic residents in immigrant communities, following IRCA. Consistent with this interpretation, using entirely different data (from police as opposed to court records) on all adult arrests made in Bexar County from June of 1986 to December of 1992, Bohn et al. (2015) also find that policing did not change systematically across ethnicities or neighborhoods in the wake of immigration reform in the 1980s. When we include their ethnicity-specific arrest rates by neighborhood as additional controls in our preferred specification for felony charge rates (see Appendix Table A6), our main results from Table 3 are essentially unchanged, consistent with police activity not varying in a way that would induce

differential observed criminal activity across ethnic groups in a particular neighborhood. Given this, we conclude that the reduced employment opportunities for immigrants without legal status were an important driver of the observed increase in felonies after IRCA's amnesties expired.

6. Conclusion

Despite public perceptions to the contrary, there is very little consistent evidence that the arrival of new immigrants, legal or illegal, is associated with an increase in crime. The empirical evidence that exists points to important differences in the effects of immigration on crime across countries and over time. One potential explanation for the mixed results on the relationship between immigration and crime is that there is heterogeneity in policies that might be driving any relationship. In general, the propensity of any individual to engage in criminal behavior is a function of his or her ability to access jobs, housing, and other social services as well as his or her expected returns to and costs of committing crime.

In the U.S., the most significant recent change in immigration policy took place in 1986, when the Immigration Reform and Control Act (IRCA) mandated that employers verify the legal status of their employees. IRCA also provided some undocumented immigrants with work authorization through the LAW and SAW amnesty programs, but in May and November of 1988, these programs expired. The enactment of IRCA, along with the subsequent expiration of LAW and SAW amnesties, constituted large and discrete shocks to the employment opportunities for new immigrants to the U.S.

In this paper, we provide new evidence on the importance of immigration policy in influencing the criminal behavior of new immigrants by exploiting the structure of IRCA together with unique data on felony charges filed against residents of Texas' Bexar County, which is two hours from Mexico and receives regular and steady flows of Hispanic immigrants. Using a triple-differences framework, we find that federal policies limiting employment opportunities for undocumented immigrants are associated with a robust increase in the incidence of alleged felonies committed by Hispanic people living in poorer neighborhoods where more people are of Mexican descent, speak Spanish, and were born outside the U.S.

While we find that the employment restrictions put in place by IRCA had a non-trivial impact on criminal activity, our results do not imply changes in criminality out of line with other high-risk groups, even if we assume that IRCA legalized every immigrant who arrived in Bexar County before 1988. Our point estimates suggest that, after the LAW amnesty expired, an

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additional 225 people in Bexar County had felony charges filed against them each year, and 119 of those charged were convicted.[36] Based on the low end of the estimated annual immigrant arrival rate in Hall et al. (2011), this suggests that between 4 and 7.5 additional crimes were committed for every 100 immigrants who arrived in Bexar County after IRCA limited employment opportunities for unauthorized workers. The actual flow of unauthorized immigrants into Bexar County during the IRCA period is unknown. However, immigration rates immediately after IRCA could have fallen by two-thirds relative to average arrival rates prior to the reform, more than twice the largest estimated reductions and almost five times the most commonly cited estimates of the impact of IRCA on immigration flows, and the implied criminal behavior of new immigrants would still be roughly equivalent to that of other high-risk groups in the U.S (Heller et al. 2013).[37]

Immigration policy remains a pressing issue in many countries, and numerous measures have been proposed to address perceived problems arising from the flow of undocumented individuals across borders. Recent surveys from the U.S. suggest that employer sanctions are the most popular policy for controlling unauthorized immigration, and are considered by the public to be more effective than making it easier for immigrants to obtain legal status or stepping up border controls (Transatlantic Trends 2011). Our results, however, suggest that limiting job opportunities for immigrants could have the unintended consequence of increasing crime, and in particular crime that is a close substitute for formal work.

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[36] These numbers are based on multiplying the coefficient on Hispanic defendant × LAW expiration in the first column of Table 2 (along with the analogous coefficient from a model where convictions per capita is the dependent variable, 0.0832) by the average number of felony charges filed against (or felony convictions of) Hispanic people in each block group in each month between January 1980 and April 1988. This corresponds to an estimated 0.0188 charges (0.0099 convictions) per block group per month, or roughly 225 charges (119 convictions) per year. By comparison, the annual arrest rate for Chicago Public School students living in high-crime neighborhoods is 20 per 100 people (Heller et al. 2013).

[37] Orrenius and Zavodny (2003) estimate that immigration across the Mexican border fell by 13% in the months between the enactment of IRCA and the opening of the LAW program. After IRCA expired, they estimate that monthly immigration was between 0.7% higher and 1% lower than it had been in the 1977-1985 period. We thank Jacob Vigdor for suggesting this back-of-the-envelope calculation.

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Table 1: Summary Statistics

	Unique Observations	Mean	Standard Deviation
Felony Charges (1980-1994)			
All Charges	360,180	0.212	0.585
Income Generating Charges	360,180	0.157	0.488
Drug Charges	360,180	0.055	0.280
Non-Income Generating Charges	360,180	0.055	0.280
Block Group Characteristics (1990)			
Ln(Population)	1001	6.91	0.62
Poverty Rate	1001	15.64	16.72
People per Housing Unit	1001	2.72	0.88
Percent Mexican Descent	1001	48.01	30.50
Percent Speaking Spanish at Home	1001	38.94	25.98
Percent Immigrant	1001	9.04	6.76
Percent Non-Citizens	1001	6.40	6.13
Percent Working in Agriculture	1001	0.83	3.37
Jobs per Adult	1001	59.18	13.40
Percent of Housing Stock in Rental Market	1001	38.39	24.32

Note: Figures derived from Bexar County District Court felony charge records and 1990 Decennial Census data.

Table 2: IRCA and Felony Charges, Difference-in-Differences Estimates

	All Crimes	Income Generating	Non-Income Generating
Hispanic Defendant	0.0094	-0.0235	0.0400***
	[0.0205]	[0.0165]	[0.0081]
Hispanic × IRCA	-0.0135	0.0050	-0.0159
	[0.0240]	[0.0210]	[0.0131]
Hispanic × LAW Expiration	0.108*	0.0782+	0.0445*
	[0.0430]	[0.0399]	[0.0223]
Hispanic × SAW Expiration	0.0010	-0.0040	-0.0024
	[0.0387]	[0.0363]	[0.0203]
R^2	0.036	0.029	0.012
Observations	360,180	360,180	360,180

Note: Each regression includes block group fixed effects and 180 month dummies. Standard errors in brackets allow for arbitrary correlation in crime measure within block group. Significant at the + 10% level, * 5% level, ** 1% level, and *** 0.1% level.

Table 3: IRCA and Felony Charges, Triple-Differences Estimates

Results from Individual Proxy Regressions	All Crimes	Income Generating	Non-Income Generating
Hispanic × IRCA × Poverty Rate	0.0018	0.0038*	-0.0018+
	[0.0016]	[0.0015]	[0.0009]
Hispanic × LAW × Poverty Rate	0.0053+	0.0054*	0.0018
	[0.0027]	[0.0026]	[0.0015]
Hispanic × SAW × Poverty Rate	-0.0001	-0.0016	0.0013
	[0.0025]	[0.0024]	[0.0012]
Hispanic × IRCA × People / Housing Units	0.0070	0.0291	-0.0213
	[0.0231]	[0.0204]	[0.0139]
Hispanic × LAW × People / Housing Units	0.100+	0.0996*	0.0364
	[0.0547]	[0.0494]	[0.0306]
Hispanic × SAW × People / Housing Units	-0.0342	-0.0461	-0.0023
	[0.0393]	[0.0351]	[0.0243]
Hispanic × IRCA × Percent Mexican	0.0016*	0.0022**	-0.0002
	[0.0008]	[0.0007]	[0.0005]
Hispanic × LAW × Percent Mexican	0.0044**	0.0050***	0.0003
	[0.0015]	[0.0013]	[0.0008]
Hispanic × SAW × Percent Mexican	-0.0002	-0.0013	0.0012+
	[0.0013]	[0.0013]	[0.0007]
Hispanic × IRCA × Percent Spanish at Home	0.0019+	0.0028**	-0.0004
	[0.0010]	[0.0009]	[0.0006]
Hispanic × LAW × Percent Spanish at Home	0.0056**	0.0061***	0.0007
	[0.0017]	[0.0016]	[0.0009]
Hispanic × SAW × Percent Spanish at Home	-0.0009	-0.0021	0.0011
	[0.0016]	[0.0015]	[0.0008]
Hispanic × IRCA × Percent Immigrant	0.0034	0.0077*	-0.0037
	[0.0036]	[0.0035]	[0.0023]
Hispanic × LAW × Percent Immigrant	0.0126+	0.0127+	0.0036
	[0.0070]	[0.0070]	[0.0038]
Hispanic × SAW × Percent Immigrant	-0.0011	-0.0045	0.0032
	[0.0062]	[0.0061]	[0.0032]
Results from Multi-Proxy Regression			
R^2	0.0985	0.0833	0.0332
p(Enactment)=0	*0.4236*	*0.0834*	*0.0350*
p(LAW)=0	*0.0498*	*0.0047*	*0.4120*
p(SAW)=0	*0.6866*	*0.6338*	*0.2637*

Note: Each regression contains 360,180 observations and includes month dummies and block group fixed effects as well as full Hispanic, demographic, and immigration reform interactions. Multi-proxy regressions also include interactions between immigration reform and log 1990 population, percent working in agriculture, the percent of immigrants who moved to the U.S. after 1985, and the fraction of housing units that are owner occupied. F tests report joint significant of triple difference coefficients for each reported immigration reform measure. Standard errors in brackets allow for arbitrary correlation in crime measure within block group. Significant at the + 10% level, * 5% level, ** 1% level, and *** 0.1% level.

Table 4: IRCA and Felony Drug Charges, Triple-Differences Estimates

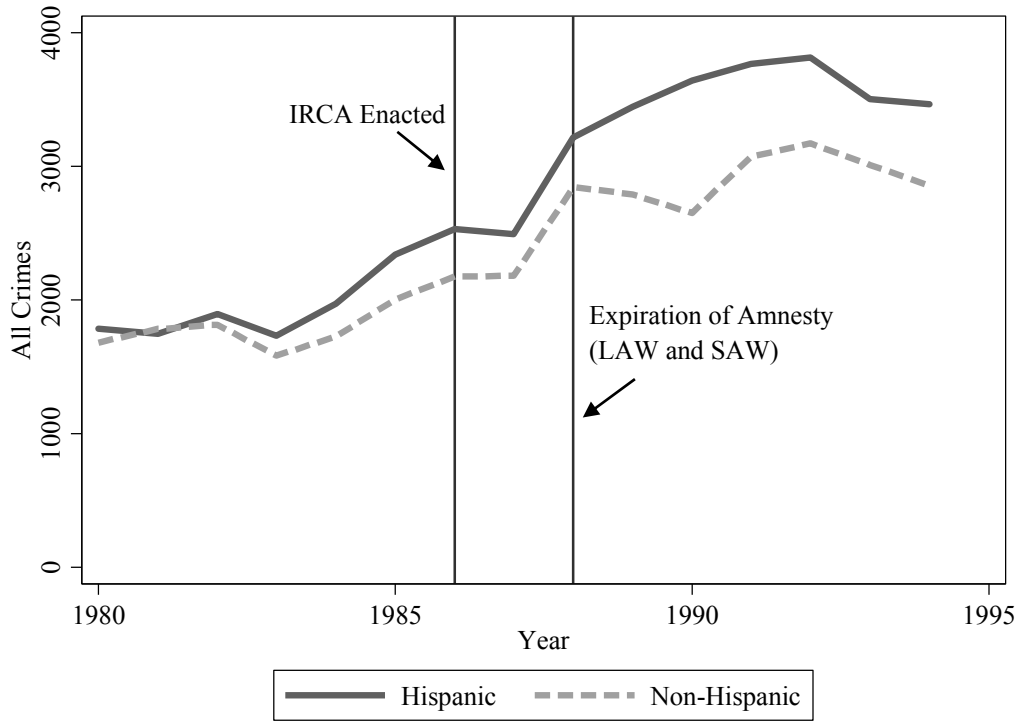| Results from Individual Proxy Regressions	Full Sample	Non-Hispanic Whites Excluded
Hispanic × IRCA × Poverty Rate	0.0046***	0.0039***
	[0.0009]	[0.0010]
Hispanic × LAW × Poverty Rate	0.0081***	0.0068**
	[0.0018]	[0.0021]
Hispanic × SAW × Poverty Rate	-0.0052**	-0.0050*
	[0.0017]	[0.0021]
Hispanic × IRCA × People / Housing Units	0.0365+	0.0373*
	[0.0187]	[0.0176]
Hispanic × LAW × People / Housing Units	0.116**	0.0920**
	[0.0373]	[0.0344]
Hispanic × SAW × People / Housing Units	-0.0658*	-0.0567+
	[0.0271]	[0.0291]
Hispanic × IRCA × Percent Mexican	0.0027***	0.0021***
	[0.0005]	[0.0005]
Hispanic × LAW × Percent Mexican	0.0057***	0.0047***
	[0.0010]	[0.0012]
Hispanic × SAW × Percent Mexican	-0.0025**	-0.0022*
	[0.0009]	[0.0011]
Hispanic × IRCA × Percent Spanish at Home	0.0032***	0.0026***
	[0.0005]	[0.0006]
Hispanic × LAW × Percent Spanish at Home	0.0071***	0.0059***
	[0.0011]	[0.0014]
Hispanic × SAW × Percent Spanish at Home	-0.0034**	-0.0032*
	[0.0010]	[0.0013]
Hispanic × IRCA × Percent Immigrant	0.0058**	0.0026
	[0.0020]	[0.0026]
Hispanic × LAW × Percent Immigrant	0.0197***	0.0191***
	[0.0046]	[0.0053]
Hispanic × SAW × Percent Immigrant	-0.0101*	-0.0102*
	[0.0043]	[0.0049]
Results from Multi-Proxy Regression		
R^2	0.0538	0.0456
p(Enactment)=0	*0.0000*	*0.0003*
p(LAW)=0	*0.0000*	*0.0006*
p(SAW)=0	*0.0074*	*0.0634*
Observations	360,180	349,200

Note: See Table 3 for notes. Significant at the + 10% level, * 5% level, ** 1% level, and *** 0.1% level.

Table 5: IRCA and Felony Conviction Rates, Triple-Differences Estimates

Results from Individual Proxy Regressions	All Crimes	Income Generating	Non-Income Generating	Drug Crimes	Drug Crimes, Non-Hispanic Whites Excluded
Hispanic × IRCA × Poverty Rate	0.0595	-0.0088	0.177	-0.145	0.100
	[0.0709]	[0.0867]	[0.176]	[0.168]	[0.197]
Hispanic × LAW × Poverty Rate	-0.146	0.0333	-0.484+	0.116	0.0911
	[0.121]	[0.137]	[0.290]	[0.199]	[0.231]
Hispanic × SAW × Poverty Rate	0.083	-0.0231	0.22	-0.0471	-0.106
	[0.109]	[0.113]	[0.256]	[0.167]	[0.187]
Hispanic × IRCA × People / Housing Units	1.822	0.215	4.492+	-0.0718	7.492*
	[1.319]	[1.458]	[2.716]	[3.065]	[3.800]
Hispanic × LAW × People / Housing Units	-1.494	0.994	-5.943+	-2.938	-4.86
	[1.944]	[2.417]	[3.201]	[3.762]	[3.659]
Hispanic × SAW × People / Housing Units	-0.0008	-0.555	0.465	2.859	2.885
	[1.722]	[2.136]	[2.511]	[3.109]	[3.274]
Hispanic × IRCA × Percent Mexican	0.0832	0.0493	0.0723	-0.0446	0.0671
	[0.0512]	[0.0589]	[0.111]	[0.106]	[0.125]
Hispanic × LAW × Percent Mexican	-0.170*	-0.117	-0.185	-0.0591	-0.00652
	[0.0757]	[0.0848]	[0.178]	[0.133]	[0.151]
Hispanic × SAW × Percent Mexican	0.0947	0.0803	0.044	0.0617	0.0707
	[0.0693]	[0.0749]	[0.152]	[0.113]	[0.126]
Hispanic × IRCA × Percent Spanish at Home	0.107+	0.0599	0.143	-0.0476	0.132
	[0.0580]	[0.0677]	[0.128]	[0.123]	[0.149]
Hispanic × LAW × Percent Spanish at Home	-0.191*	-0.125	-0.262	-0.0507	-0.0148
	[0.0870]	[0.0974]	[0.207]	[0.152]	[0.174]
Hispanic × SAW × Percent Spanish at Home	0.0981	0.0856	0.0468	0.0699	0.0702
	[0.0801]	[0.0861]	[0.177]	[0.130]	[0.144]
Hispanic × IRCA × Percent Immigrant	0.249	0.217	-0.142	0.198	0.510
	[0.202]	[0.237]	[0.408]	[0.438]	[0.524]
Hispanic × LAW × Percent Immigrant	-0.441	-0.382	-0.27	-0.258	-0.230
	[0.290]	[0.311]	[0.794]	[0.494]	[0.537]
Hispanic × SAW × Percent Immigrant	0.284	0.234	0.361	0.199	0.270
	[0.260]	[0.276]	[0.716]	[0.425]	[0.481]
Results from Multi-Proxy Regression					
R²	0.0499	0.0628	0.0912	0.125	0.133
p(Enactment)=0	*0.4182*	*0.5170*	*0.2422*	*0.8972*	*0.1434*
p(LAW)=0	*0.3003*	*0.1601*	*0.3216*	*0.8125*	*0.4280*
p(SAW)=0	*0.7691*	*0.5241*	*0.9410*	*0.9171*	*0.8288*
Observations	55,418	43,172	16,514	16,231	12,920

Note: See Table 3 for notes. Significant at the + 10% level, * 5% level, ** 1% level, and *** 0.1% level Significant at the + 10% level, * 5% level, ** 1% level, and *** 0.1% level.

Figure 1: Average Annual Neighborhood Criminal Incidence by Ethnicity

IRCA Enacted

Expiration of Amnesty
(LAW and SAW)

All Crimes

1980   1985   1990   1995
Year

Hispanic   Non-Hispanic

Note: Crimes include all alleged felonies. Authors' calculations based on Bexar County Court records.

Figure 2: Immigration to Bexar County by Date of Entry, 1990 Census Data

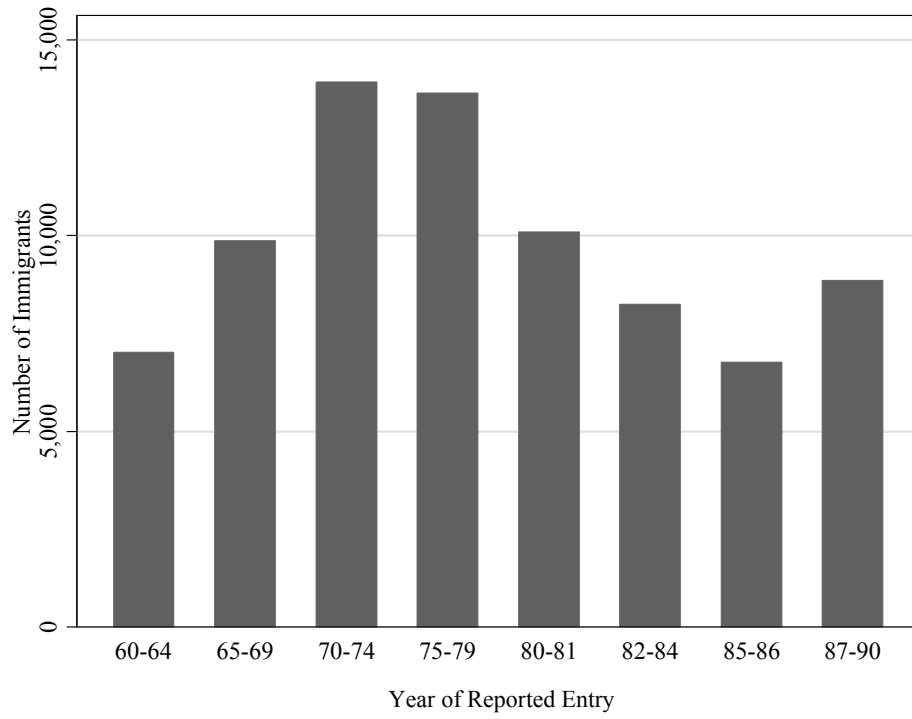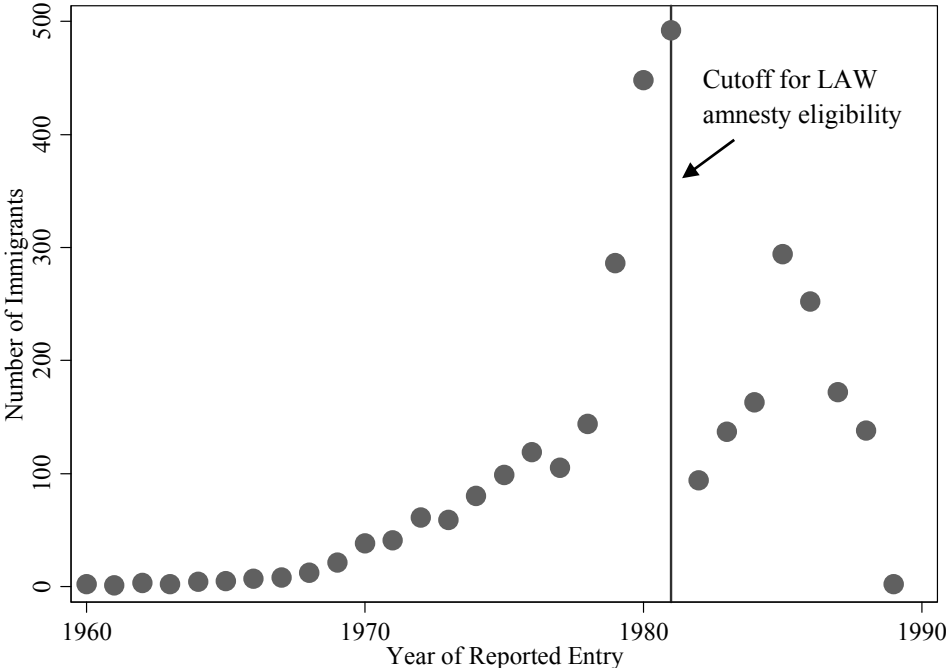Figure 3: Immigration to Bexar County, 1992 INS Legalization Summary Tape

A. Immigration by Date of Entry

B. Share of Immigrants Arriving in the Fourth Quarter by Year of Entry
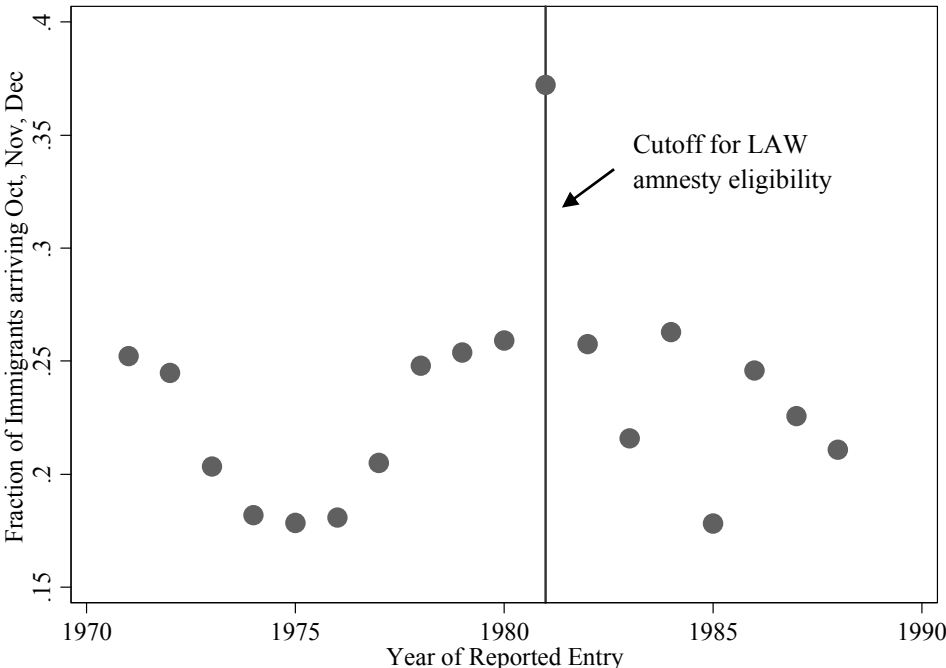
Figure 4: Average Monthly Neighborhood Criminal Incidence by Ethnicity and Crime Type
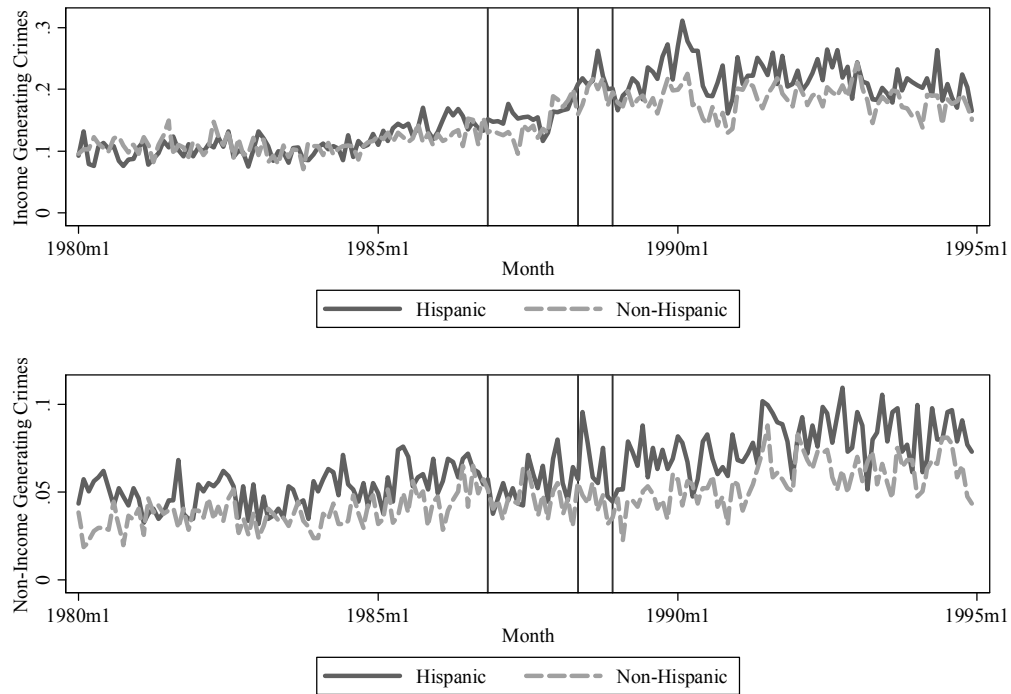
Note: Vertical lines represent the months of IRCA enactment (November 1986), LAW amnesty expiration (May 1988), and SAW amnesty expiration (December 1988).

Figure 5: Difference in Criminal Incidence across High and Low Poverty Neighborhoods, by Ethnicity and Crime Type
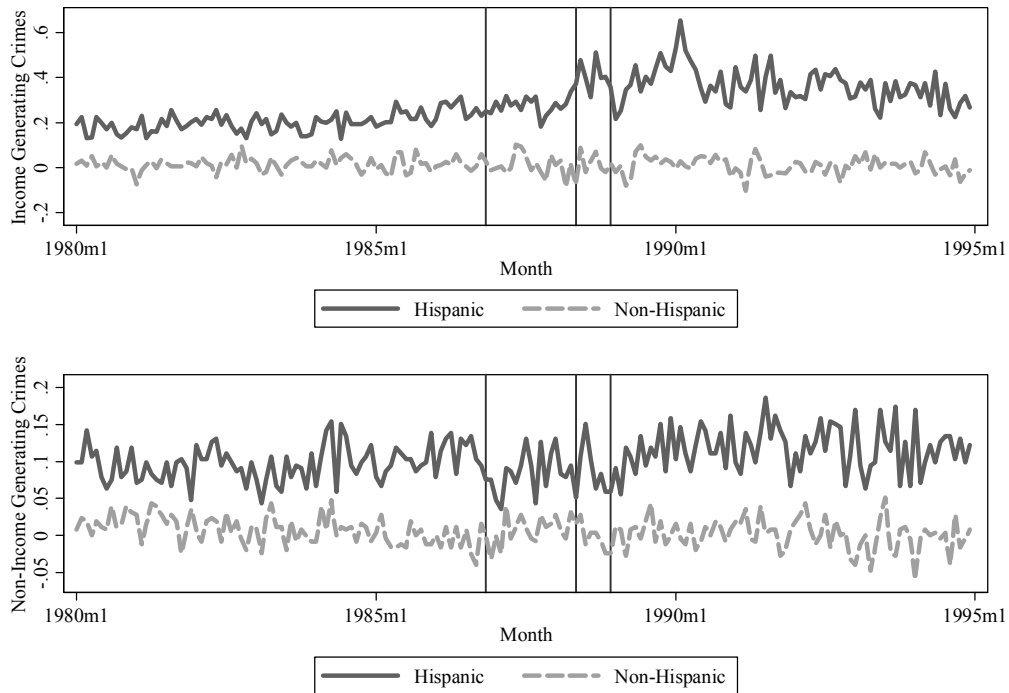
Note: High and low poverty neighborhoods are block groups in the top quartile and bottom quartile of the poverty rate distribution in the 1990 Decennial Census. Vertical lines represent the months of IRCA enactment (November 1986), LAW amnesty expiration (May 1988), and SAW amnesty expiration (December 1988).

Figure 6: Difference in Criminal Incidence across High and Low Population Density Neighborhoods, by Ethnicity and Crime Type

Note: High and low population density neighborhoods are block groups in the top quartile and bottom quartile of the residents per housing unit distribution in the 1990 Decennial Census. Vertical lines represent the months of IRCA enactment (November 1986), LAW amnesty expiration (May 1988), and SAW amnesty expiration (December 1988).

Figure 7: Difference in Criminal Incidence across High and Low Percent Mexican Neighborhoods, by Ethnicity and Crime Type

Note: High and low percent Mexican neighborhoods are block groups in the top quartile and bottom quartile of the percent Mexican distribution in the 1990 Decennial Census. Vertical lines represent the months of IRCA enactment (November 1986), LAW amnesty expiration (May 1988), and SAW amnesty expiration (December 1988).

Figure 8: Difference in Criminal Incidence across High and Low Spanish Speaking Neighborhoods, by Ethnicity and Crime Type
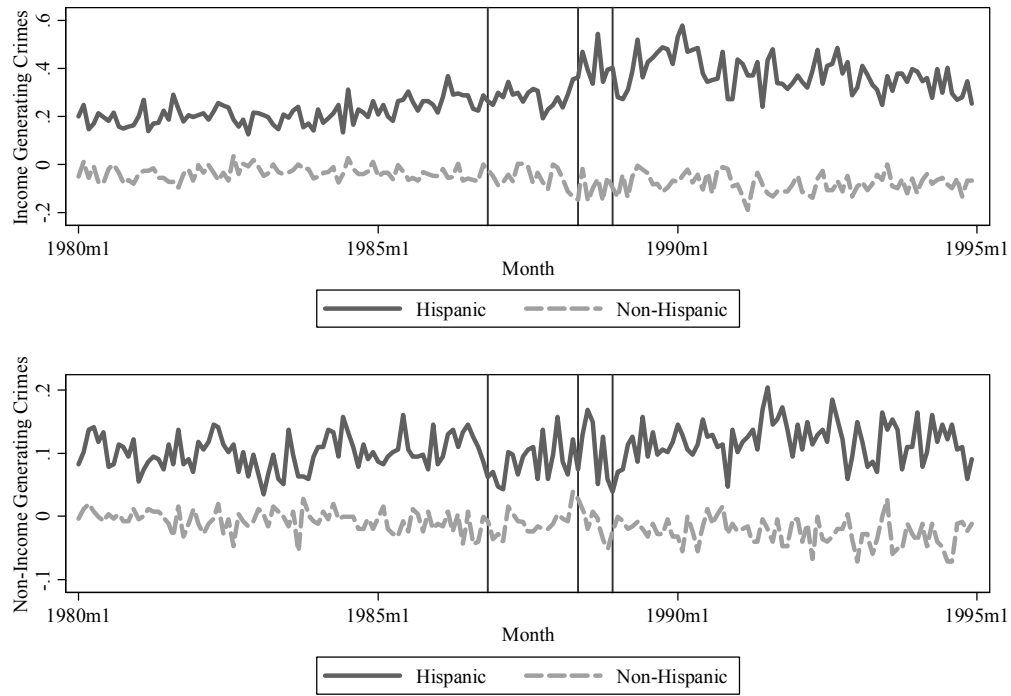
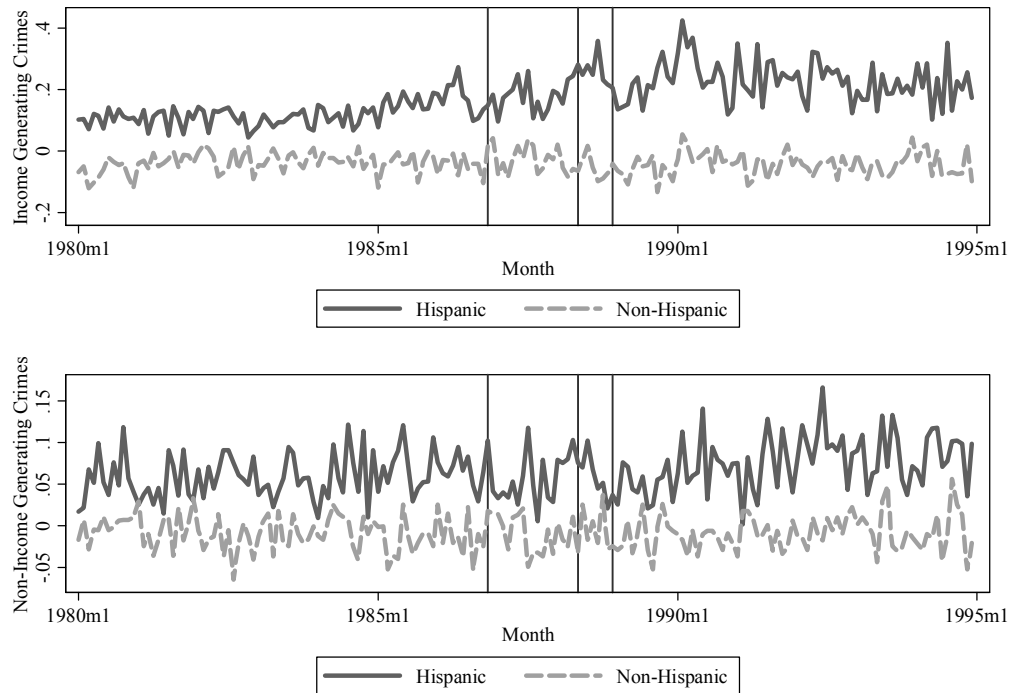Note: High and low Spanish speaking neighborhoods are block groups in the top quartile and bottom quartile of the percent Spanish speaking distribution in the 1990 Decennial Census. Vertical lines represent the months of IRCA enactment (November 1986), LAW amnesty expiration (May 1988), and SAW amnesty expiration (December 1988).

Figure 9: Difference in Criminal Incidence across High and Low Immigrant Neighborhoods,
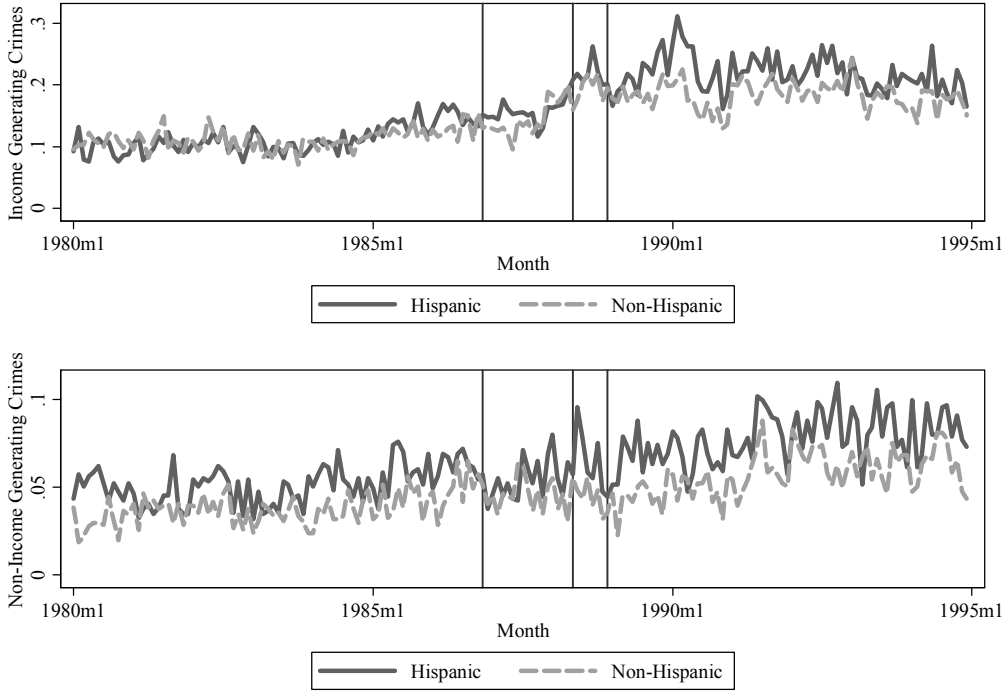by Ethnicity and Crime Type
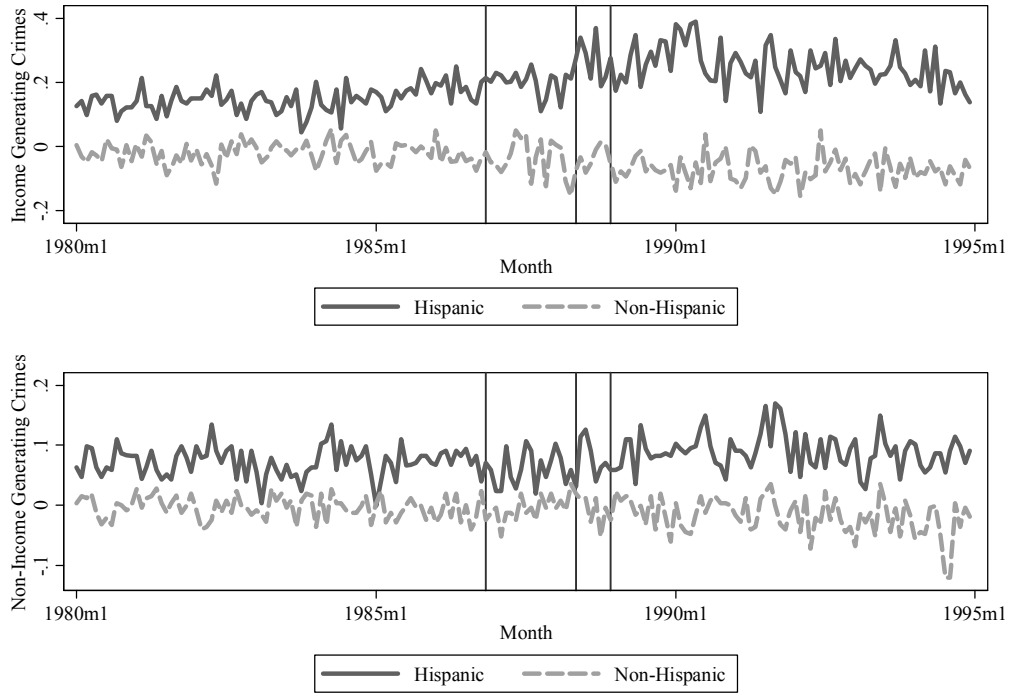
Note: High and low immigrant neighborhoods are block groups in the top quartile and bottom quartile of
the percent foreign born distribution in the 1990 Decennial Census. Vertical lines represent the months of
IRCA enactment (November 1986), LAW amnesty expiration (May 1988), and SAW amnesty expiration
(December 1988).

Immigration, Employment Opportunities, and Criminal Behavior
Appendix

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Appendix

A. Theoretical Framework

To motivate the empirical analysis in Freedman et al. (2015), we develop a simple model relating work, crime, and legal status. The model is not intended to capture all possible channels through which legal status could affect decisions to engage in crime, but rather is intended to highlight the key mechanisms we explore in the empirical analysis. The model builds on Lochner and Moretti (2004), who consider how schooling interacts with decisions to work and engage in crime.

Letting s denote legal residency status, individuals in the model can be native citizens or immigrants; after an amnesty (as under IRCA), the latter group is separated into newly legalized residents and illegal residents unauthorized to work in the formal market (perhaps because they failed to meet amnesty eligibility requirements). We will consider decisions of individuals in each group regarding how to allocate their time between formal market work and crime, where k_t denotes the fraction of time engaged in crime at age t. We assume that individuals are homogeneous except with respect to their legal status s, and thus denote the wage rate at age t in the formal labor market as $w_t(s)$. Meanwhile, the net return to crime is denoted $r(k_t)$, where $r'(k_t) > 0$.[1] Let $\pi(k_t, s)$ be the probability of being caught and punished for committing a crime, which is increasing in k_t and also allowed to vary with legal residency status due to potential differences in reporting patterns or police treatment. We assume that the punishment if caught, $p(s)$, is also a function of legal status; for simplicity, we assume that $p(s)$ is measured in terms of utility. As we discuss further below, how $\pi(k_t, s)$ and $p(s)$ vary with s has implications for the likely impact of legal employment opportunities on criminal activity.

In each time period, an individual consumes the income generated through formal work and by engaging in criminal activity, which is $y_t = w_t(s)(1 - k_t) + r(k_t)$. By consuming this income, the individual receives utility $u(y_t)$, where $u'(y_t) > 0$ and $u''(y_t) \leq 0$. Therefore, we can write an individual's maximization problem for a given legal status s as

$$V(s) = \max_{\{k_t\}_{t=0}^{T}} \left\{ \sum_{t=0}^{T} \beta^t [u(w_t(s)(1 - k_t) + r(k_t)) - \pi(k_t, s)p(s)] \right\}.$$

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[1] We could allow $r(k_t)$ to also be a function of s; Lochner and Moretti (2004), for example, allow the net return to crime to vary both with time spent engaging in crime and on educational attainment. While punishment might be expected to vary with s (which we allow for in the model), there is no reason to think that the net return to crime would vary with s.

Appendix

Here, $\beta \in [0,1]$ is the individual's discount factor, and T denotes the total amount of time he or she has to work or engage in crime. Thus, having chosen the optimal amount of time to allocate to legal work and criminal activity, $V(s)$ is the lifetime value associated with a particular legal residency status s, where s includes native citizens, newly legalized residents, and illegal residents.

Assuming an interior solution, the first-order condition with respect to k_t is

$$r'(k_t) - w_t(s) = \frac{\partial \pi(k_t, s)}{\partial k_t} \frac{p(s)}{u'(y_t)}.$$

This condition yields several insights. First, note that the right-hand side of the expression is greater than or equal to zero.[2] Assuming there is some punishment if caught committing a crime ($p(s) > 0$), the marginal return to criminal activity must be greater than the wage to compensate individuals for the risk of being caught and punished. The compensating differential must be greater the faster the probability of being caught increases with additional criminal activity.

More important for our empirical analysis, the first-order condition highlights several important channels through which legal residency status could affect decisions to engage in crime. First, legal residency status could affect wages; higher wages will tend to reduce time devoted to criminal activity. Second, legal status could affect the probability of being caught committing crime. If the propensity to report crimes differs across groups or police treat groups differently (potentially due to changes in immigration policy), crime rates (or at least observed crime rates) may vary across groups. Third, legal residency status could affect punishment if caught engaging in criminal activity. For example, if immigrants who are in the country illegally are deported for committing a felony, whereas native citizens are only imprisoned, p might be perceived as higher for a given crime among illegal immigrants.

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[2] In the case in which there is no anticipated punishment, we arrive at $r'(k_t) = w_t(s)$, similar to Grogger (1998).

Appendix

B. Supplementary Tables and Figures

Table A1: IRCA and Felony Charges, Difference-in-Differences Estimates, Including Estimates with
Block Group Demographic Controls

	All Crimes		Income Generating		Non-Income Generating	
Hispanic Defendant	0.0094	0.0088	-0.0235	-0.0243	0.0400***	0.0403***
	[0.0205]	[0.0207]	[0.0165]	[0.0166]	[0.0081]	[0.0081]
Hispanic × IRCA	-0.0135	-0.0089	0.0050	0.0109	-0.0159	-0.0172
	[0.0240]	[0.0240]	[0.0210]	[0.0209]	[0.0131]	[0.0131]
Hispanic × LAW Expiration	0.108*	0.110*	0.0782+	0.0791*	0.0445*	0.0457*
	[0.0430]	[0.0432]	[0.0399]	[0.0401]	[0.0223]	[0.0225]
Hispanic × SAW Expiration	0.0010	-0.001	-0.0040	-0.0069	-0.0024	-0.0016
	[0.0387]	[0.0390]	[0.0363]	[0.0366]	[0.0203]	[0.0204]
Block Group FE	Y		Y		Y	
Demographic Controls		Y		Y		Y
R^2	0.036	0.085	0.029	0.069	0.012	0.028
Observations	360,180	360,180	360,180	360,180	360,180	360,180

Note: Each regression includes 180 month dummies. Demographic controls include block level poverty rate, percent Mexican descent, percent speaking Spanish at home, percent immigrant, the percent of immigrants who moved to the U.S. after 1985, the percent of the population that is a U.S. citizen, the number of people per housing unit, jobs per adult, the fraction of the housing stock that is for rent, and the natural log of population in 1990. Standard errors in brackets allow for arbitrary correlation in crime measure within block group. Significant at the + 10% level, * 5% level, ** 1% level, and *** 0.1% level.

Appendix

Table A2: IRCA and Felony Charges, First-Order Impacts from Baseline Triple-Differences Regressions

Results from Individual Proxy Regressions	All Crimes	Income Generating	Non-Income Generating	Drug Crimes	Drug Crimes, Non-Hispanic Whites Excluded
Poverty Rate					
Hispanic × IRCA	-0.0377	-0.0437+	0.00476	-0.0492**	-0.0473**
	[0.0292]	[0.0254]	[0.0155]	[0.0156]	[0.0171]
Hispanic × LAW	0.0266	-0.0104	0.0231	-0.103**	-0.0813*
	[0.0521]	[0.0477]	[0.0266]	[0.0338]	[0.0369]
Hispanic × SAW	0.000566	0.0179	-0.022	0.0367	-0.00761
	[0.0477]	[0.0440]	[0.0248]	[0.0309]	[0.0349]
People / Housing Units					
Hispanic × IRCA	-0.0464	-0.0716	0.0179	-0.0866+	-0.0969*
	[0.0636]	[0.0565]	[0.0367]	[0.0461]	[0.0463]
Hispanic × LAW	-0.163	-0.209	-0.0291	-0.283**	-0.217*
	[0.148]	[0.136]	[0.0835]	[0.102]	[0.0947]
Hispanic × SAW	0.12	0.151	-7.6E-05	0.134+	0.0696
	[0.108]	[0.0963]	[0.0696]	[0.0763]	[0.0807]
Percent Mexican					
Hispanic × IRCA	-0.0925*	-0.0971**	-0.0119	-0.105***	-0.0874***
	[0.0390]	[0.0332]	[0.0212]	[0.0214]	[0.0229]
Hispanic × LAW	-0.0951	-0.160*	0.0343	-0.252***	-0.201***
	[0.0716]	[0.0641]	[0.0358]	[0.0471]	[0.0540]
Hispanic × SAW	0.00808	0.057	-0.0586+	0.0735+	0.0217
	[0.0662]	[0.0604]	[0.0337]	[0.0423]	[0.0494]
Percent Spanish at Home					
Hispanic × IRCA	-0.0867*	-0.0969**	-0.00745	-0.102***	-0.0878***
	[0.0379]	[0.0324]	[0.0206]	[0.0208]	[0.0223]
Hispanic × LAW	-0.104	-0.159*	0.0236	-0.255***	-0.205***
	[0.0698]	[0.0625]	[0.0357]	[0.0452]	[0.0512]
Hispanic × SAW	0.0337	0.0747	-0.0433	0.0888*	0.0389
	[0.0647]	[0.0588]	[0.0335]	[0.0412]	[0.0478]
Percent Immigrant					
Hispanic × IRCA	-0.0438	-0.0602+	0.0134	-0.0307	-0.00378
	[0.0370]	[0.0334]	[0.0218]	[0.0208]	[0.0244]
Hispanic × LAW	-0.00035	-0.0347	0.016	-0.154**	-0.151**
	[0.0720]	[0.0683]	[0.0363]	[0.0478]	[0.0533]
Hispanic × SAW	0.00849	0.0341	-0.0303	0.0464	0.00756
	[0.0647]	[0.0613]	[0.0329]	[0.0434]	[0.0486]

Note: Each regression includes 360,180 observations, 180 month dummies and block group fixed effects, as well as full Hispanic, demographic, and immigration reform interactions. Standard errors in brackets allow for arbitrary correlation in crime measure within block group. Significant at the + 10% level, * 5% level, ** 1% level, and *** 0.1% level.

Appendix

Table A3: IRCA and Felony Charges, Triple-Differences Estimates with Full Fixed Effects

Results from Individual Proxy Regressions	All Crimes	Income Generating	Non-Income Generating
Hispanic × IRCA × Poverty Rate	0.0018	0.0038+	-0.0018
	[0.0023]	[0.0021]	[0.0013]
Hispanic × LAW × Poverty Rate	0.0054	0.0055	0.0018
	[0.0039]	[0.0037]	[0.0021]
Hispanic × SAW × Poverty Rate	-0.0001	-0.0016	0.0013
	[0.0035]	[0.0034]	[0.0017]
Hispanic × IRCA × People / Housing Units	0.0063	0.0283	-0.0212
	[0.0327]	[0.0288]	[0.0197]
Hispanic × LAW × People / Housing Units	0.101	0.101	0.0364
	[0.0777]	[0.0702]	[0.0433]
Hispanic × SAW × People / Housing Units	-0.0345	-0.0465	-0.0023
	[0.0558]	[0.0498]	[0.0345]
Hispanic × IRCA × Percent Mexican	0.0016	0.0022*	-0.0002
	[0.0012]	[0.0010]	[0.0007]
Hispanic × LAW × Percent Mexican	0.0044*	0.0050**	0.0003
	[0.0021]	[0.0019]	[0.0011]
Hispanic × SAW × Percent Mexican	-0.0002	-0.0014	0.0012
	[0.0019]	[0.0018]	[0.0009]
Hispanic × IRCA × Percent Spanish at Home	0.0018	0.0028*	-0.0004
	[0.0014]	[0.0012]	[0.0008]
Hispanic × LAW × Percent Spanish at Home	0.0056*	0.0061**	0.0007
	[0.0025]	[0.0023]	[0.0013]
Hispanic × SAW × Percent Spanish at Home	-0.0009	-0.0021	0.0011
	[0.0023]	[0.0021]	[0.0011]
Hispanic × IRCA × Percent Immigrant	0.0035	0.0078	-0.0037
	[0.0051]	[0.0049]	[0.0033]
Hispanic × LAW × Percent Immigrant	0.0124	0.0125	0.0036
	[0.0099]	[0.0099]	[0.0054]
Hispanic × SAW × Percent Immigrant	-0.001	-0.0045	0.0032
	[0.0088]	[0.0087]	[0.0045]
Results from Multi-Proxy Regression			
R^2	0.560	0.551	0.522
p(Enactment)=0	*0.8025*	*0.4336*	*0.3089*
p(LAW)=0	*0.3438*	*0.1326*	*0.7751*
p(SAW)=0	*0.9093*	*0.8875*	*0.6660*

Note: Each regression contains 360,180 observations and includes ethnicity by block group fixed effects, month by block group fixed effects, and month by ethnicity fixed effects. Multi-proxy regressions also include interactions between immigration reform and log 1990 population, percent working in agriculture, the percent of immigrants who moved to the U.S. after 1985, and the fraction of housing units that are owner occupied. F tests report joint significant of triple difference coefficients for each reported immigration reform measure. Standard errors in brackets allow for arbitrary correlation in crime measure within block group. Significant at the + 10% level, * 5% level, ** 1% level, and *** 0.1% level.

Appendix

Table A4: IRCA and Felony Charges, Triple-Differences Estimates with 1980-1990 Population Growth in May of 1988

Results from Individual Proxy Regressions	All Crimes	Income Generating	Non-Income Generating	Drug Crimes	Drug Crimes, Non-Hispanic Whites Excluded
Hispanic × IRCA × Poverty Rate	0.0021	0.0040**	-0.0017+	0.0047***	0.0038**
	[0.0017]	[0.0015]	[0.0009]	[0.0009]	[0.0012]
Hispanic × LAW × Poverty Rate	0.0048+	0.0050+	0.0016	0.0080***	0.0070***
	[0.0028]	[0.0026]	[0.0015]	[0.0018]	[0.0021]
Hispanic × SAW × Poverty Rate	0.0001	-0.0014	0.0014	-0.0051**	-0.0051*
	[0.0025]	[0.0024]	[0.0012]	[0.0017]	[0.0021]
Hispanic × IRCA × People / Housing Units	0.0005	0.0242	-0.024	0.0334+	0.0404+
	[0.0254]	[0.0218]	[0.0150]	[0.0203]	[0.0227]
Hispanic × LAW × People / Housing Units	0.110+	0.107*	0.0399	0.119**	0.0847*
	[0.0588]	[0.0521]	[0.0328]	[0.0388]	[0.0366]
Hispanic × SAW × People / Housing Units	-0.0341	-0.046	-0.0019	-0.0661*	-0.0552+
	[0.0393]	[0.0351]	[0.0243]	[0.0271]	[0.0289]
Hispanic × IRCA × Percent Mexican	0.0019*	0.0024***	-0.0001	0.0028***	0.0027***
	[0.0008]	[0.0007]	[0.0005]	[0.0005]	[0.0006]
Hispanic × LAW × Percent Mexican	0.0039**	0.0046***	0.0002	0.0056***	0.0040***
	[0.0015]	[0.0014]	[0.0008]	[0.0010]	[0.0012]
Hispanic × SAW × Percent Mexican	-9.8E-06	-0.0012	0.0012+	-0.0024**	-0.0022+
	[0.0013]	[0.0013]	[0.0007]	[0.0009]	[0.0011]
Hispanic × IRCA × Percent Spanish at Home	0.0021*	0.0029***	-0.0004	0.00323***	0.0031***
	[0.0010]	[0.0009]	[0.0006]	[0.0005]	[0.0007]
Hispanic × LAW × Percent Spanish at Home	0.0053**	0.0058***	0.0006	0.0071***	0.0052***
	[0.0018]	[0.0016]	[0.0010]	[0.0011]	[0.0014]
Hispanic × SAW × Percent Spanish at Home	-0.0008	-0.0020	0.0011	-0.0034**	-0.0032*
	[0.0016]	[0.0015]	[0.0008]	[0.0010]	[0.0013]
Hispanic × IRCA × Percent Immigrant	0.0035	0.0077*	-0.0036	0.0058**	0.0036
	[0.0037]	[0.0035]	[0.0023]	[0.0020]	[0.0030]
Hispanic × LAW × Percent Immigrant	0.0125+	0.0127+	0.0036	0.0198***	0.0177***
	[0.0071]	[0.0070]	[0.0039]	[0.0046]	[0.0054]
Hispanic × SAW × Percent Immigrant	-0.0011	-0.0045	0.0031	-0.0101*	-0.0102*
	[0.0062]	[0.0061]	[0.0032]	[0.0043]	[0.0049]
Results from Multi-Proxy Regression					
R²	0.0970	0.0821	0.0328	0.0531	0.0438
p(Enactment)=0	*0.2300*	*0.0574*	*0.0238*	*0.0000*	*0.0007*
p(LAW)=0	*0.0578*	*0.0098*	*0.3004*	*0.0000*	*0.0071*
p(SAW)=0	*0.5662*	*0.5758*	*0.2144*	*0.0066*	*0.0558*

Note: Each regression in the first four columns contains 360,180 observations; regression in the fifth column contains 349,200 observations. All regressions include 180 month dummies and block group fixed effects as well as full Hispanic, demographic, and immigration reform interactions. Multi-proxy regressions also include interactions between immigration reform and log 1990 population, percent working in agriculture, the percent of immigrants who moved to the U.S. after 1985, and the fraction of housing units that are owner occupied. F tests report joint significant of triple difference coefficients for each reported immigration reform measure. Standard errors in brackets allow for arbitrary correlation in crime measure within block group. Significant at the + 10% level, * 5% level, ** 1% level, and *** 0.1% level.

Appendix

Table A5: IRCA and Felony Drug Charges, Triple-Differences Estimates

Results from Individual Proxy Regressions	Full Sample N=360,180		Non-Hispanic Whites Excluded N=349,200	
Hispanic × IRCA × Poverty Rate	0.0046***	0.0046***	0.0039***	0.0040**
	[0.0009]	[0.0012]	[0.0010]	[0.0015]
Hispanic × LAW × Poverty Rate	0.0081***	0.0081**	0.0068**	0.0063*
	[0.0018]	[0.0025]	[0.0021]	[0.0031]
Hispanic × SAW × Poverty Rate	-0.0052**	-0.0052*	-0.0050*	-0.0048
	[0.0017]	[0.0024]	[0.0021]	[0.0030]
Hispanic × IRCA × People / Housing Units	0.0365+	0.0362	0.0373*	0.0344
	[0.0187]	[0.0264]	[0.0176]	[0.0249]
Hispanic × LAW × People / Housing Units	0.116**	0.116*	0.0920**	0.0868+
	[0.0373]	[0.0531]	[0.0344]	[0.0489]
Hispanic × SAW × People / Housing Units	-0.0658*	-0.0660+	-0.0567+	-0.0578
	[0.0271]	[0.0385]	[0.0291]	[0.0423]
Hispanic × IRCA × Percent Mexican	0.0027***	0.0027***	0.0021***	0.0022**
	[0.0005]	[0.0006]	[0.0005]	[0.0008]
Hispanic × LAW × Percent Mexican	0.0057***	0.0058***	0.0047***	0.0045*
	[0.0010]	[0.0014]	[0.0012]	[0.0018]
Hispanic × SAW × Percent Mexican	-0.0025**	-0.0025*	-0.0022*	-0.0021
	[0.0009]	[0.0012]	[0.0011]	[0.0016]
Hispanic × IRCA × Percent Spanish at Home	0.0032***	0.0032***	0.0026***	0.0027**
	[0.0005]	[0.0008]	[0.0006]	[0.0009]
Hispanic × LAW × Percent Spanish at Home	0.0071***	0.0072***	0.0059***	0.0056**
	[0.0011]	[0.0016]	[0.0014]	[0.0021]
Hispanic × SAW × Percent Spanish at Home	-0.0034**	-0.0034*	-0.0032*	-0.0031
	[0.0010]	[0.0015]	[0.0013]	[0.0020]
Hispanic × IRCA × Percent Immigrant	0.0058**	0.0058*	0.0026	0.0023
	[0.0020]	[0.0029]	[0.0026]	[0.0037]
Hispanic × LAW × Percent Immigrant	0.0197***	0.0196**	0.0191***	0.0181*
	[0.0046]	[0.0065]	[0.0053]	[0.0077]
Hispanic × SAW × Percent Immigrant	-0.0101*	-0.0101+	-0.0102*	-0.0100
	[0.0043]	[0.0061]	[0.0049]	[0.0072]
Block Group × Month, Ethnicity × Month, and Block Group × Ethnicity FEs	Y		Y	
Results from Multi-Proxy Regression				
R^2	0.0538	0.538	0.0456	0.545
p(Enactment)=0	*0.0000*	*0.0029*	*0.0003*	*0.0372*
p(LAW)=0	*0.0000*	*0.0005*	*0.0006*	*0.0862*
p(SAW)=0	*0.0074*	*0.1608*	*0.0634*	*0.4087*

Note: See Table 3 for notes. Significant at the + 10% level, * 5% level, ** 1% level, and *** 0.1% level.
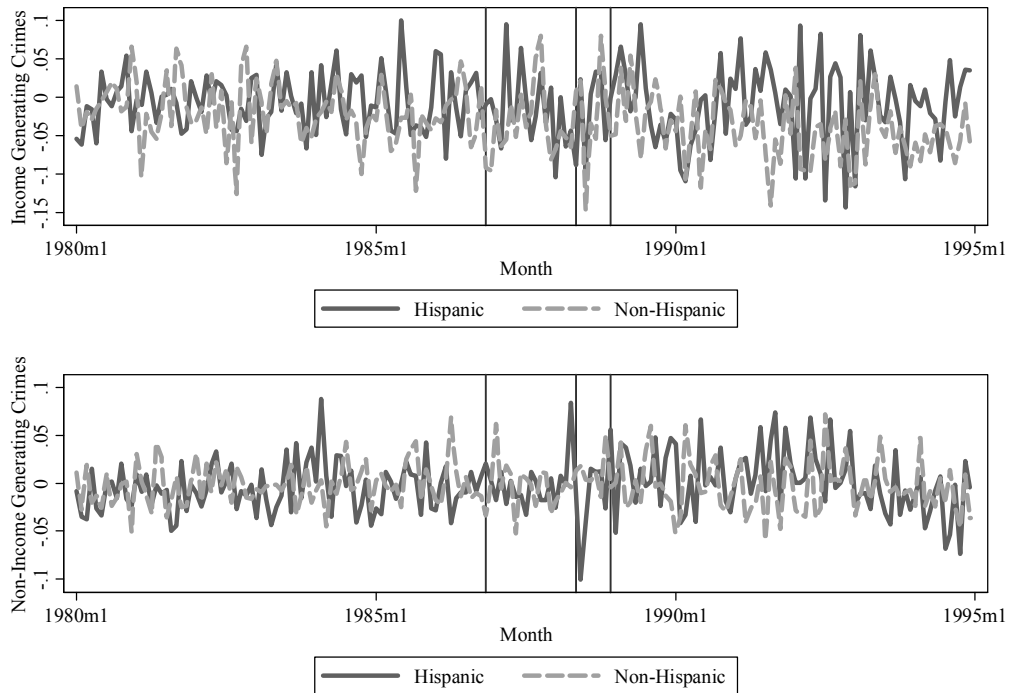
Appendix

Table A6: IRCA and Felony Charges, Triple-Differences Estimates Controlling for Ethnicity-Specific Arrest Rates

Results from Individual Proxy Regressions	All Crimes	Income Generating	Non-Income Generating	Drug Crimes
Hispanic × IRCA × Poverty Rate	-0.0019	0.00112	-0.00335**	0.00185
	[0.00216]	[0.00205]	[0.00116]	[0.00122]
Hispanic × LAW × Poverty Rate	0.00543*	0.00550*	0.00177	0.00815***
	[0.00274]	[0.00262]	[0.00150]	[0.00179]
Hispanic × SAW × Poverty Rate	0.00116	-0.00017	0.00139	-0.00443*
	[0.00248]	[0.00244]	[0.00125]	[0.00175]
Hispanic × IRCA × People / Housing Units	-0.0623+	-0.0341	-0.0451*	-0.0133
	[0.0353]	[0.0302]	[0.0202]	[0.0238]
Hispanic × LAW × People / Housing Units	0.102+	0.101*	0.0365	0.117**
	[0.0551]	[0.0497]	[0.0307]	[0.0376]
Hispanic × SAW × People / Housing Units	-0.0277	-0.039	-0.00187	-0.0646*
	[0.0398]	[0.0359]	[0.0232]	[0.0271]
Hispanic × IRCA × Percent Mexican	-0.00139	4.55E-05	-0.00150*	0.000841
	[0.00105]	[0.000931]	[0.000591]	[0.000594]
Hispanic × LAW × Percent Mexican	0.00444**	0.00505***	0.000313	0.00577***
	[0.00146]	[0.00133]	[0.000779]	[0.000960]
Hispanic × SAW × Percent Mexican	0.000441	-0.00061	0.00117+	-0.00213*
	[0.00136]	[0.00130]	[0.000666]	[0.000895]
Hispanic × IRCA × Percent Spanish at Home	-0.00172	4.98E-05	-0.00184*	0.00106
	[0.00126]	[0.00113]	[0.000716]	[0.000704]
Hispanic × LAW × Percent Spanish at Home	0.00569**	0.00617***	0.000688	0.00720***
	[0.00174]	[0.00159]	[0.000946]	[0.00114]
Hispanic × SAW × Percent Spanish at Home	-5.5E-05	-0.00118	0.00111	-0.00298**
	[0.00164]	[0.00155]	[0.000798]	[0.00108]
Hispanic × IRCA × Percent Immigrant	-0.00108	0.0035	-0.00511	0.00121
	[0.00501]	[0.00472]	[0.00326]	[0.00267]
Hispanic × LAW × Percent Immigrant	0.0127+	0.0128+	0.00366	0.0198***
	[0.00699]	[0.00699]	[0.00384]	[0.00462]
Hispanic × SAW × Percent Immigrant	0.00072	-0.00218	0.00304	-0.00889*
	[0.00646]	[0.00631]	[0.00328]	[0.00436]
Results from Multi-Proxy Regression				
R^2	0.108	0.095	0.036	0.062
p(Enactment)=0	*0.6231*	*0.9348*	*0.0508*	*0.4768*
p(LAW)=0	*0.0439*	*0.0044*	*0.4096*	*0.0000*
p(SAW)=0	*0.8082*	*0.8458*	*0.3893*	*0.0226*

Note: Each regressions contains 178,000 observations and includes month dummies and block group fixed effects as well as full Hispanic, demographic, and immigration reform interactions. Multi-proxy regressions also include interactions between immigration reform and log 1990 population, percent working in agriculture, the percent of immigrants who moved to the U.S. after 1985, and the fraction of housing units that are owner occupied. Arrest data from Bohn et al. (2015). F tests report joint significant of triple difference coefficients for each reported immigration reform measure. Standard errors in brackets allow for arbitrary correlation in crime measure within block group. Significant at the + 10% level, * 5% level, ** 1% level, and *** 0.1% level.

Appendix

Figure A1: Difference in Criminal Incidence across High and Low Agriculture Neighborhoods, by Ethnicity and Crime Type

Note: High and low agriculture neighborhoods are block groups in the top quartile and bottom quartile of the distribution of the percent working in the agriculture industry in the 1990 Decennial Census.

Appendix

C. Appendix References

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