

Immigration, Employment Opportunities, and Criminal Behavior^{*}

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Abstract

There is little consensus on the effects of immigration on crime. One potential explanation for the conflicting evidence is heterogeneity across space and time in policies toward immigrants that affect their status in the community. In this paper, we take advantage of provisions of the Immigration Reform and Control Act of 1986 (IRCA), which granted legal resident status to long-time illegal residents but created new obstacles to employment for more recent immigrants, to explore how employment opportunities affect criminal behavior. Exploiting unique administrative data on the criminal justice involvement of individuals in San Antonio, Texas and using a difference-in-differences methodology, we find evidence of an increase in felony charges filed against Hispanic residents of San Antonio after the expiration of the IRCA amnesty deadline. This was concentrated in neighborhoods where recent immigrants are most likely to locate, suggesting a strong relationship between access to legal jobs and criminal behavior.

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1. Introduction

Immigration policy is one of the most hotly debated issues in United States today. Surveys 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% of Americans 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. Despite strong feelings on the subject, though, 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 employers 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, Bell et al. 2012, Bianchi et al. 2012). 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 unaffected by this major immigration reform and by those whose labor market opportunities were directly affected by the policies.

In this paper, we shed new light on the relationship between immigration, assimilation policies, 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 the estimated 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), and an estimated 3,000 to 5,000 immigrants arrive in the metropolitan area each year.¹ A key part of our identification strategy will be to exploit geographic variation in where newly arrived immigrants are most likely to reside.

To explore IRCA’s potentially varied impacts on criminal behavior, we use administrative records on felony charges filed in Bexar County between 1980 and 1994. The data include individual-level information on accused and convicted criminals’ local residence, ethnicity, and age. To motivate the analysis, in Figure 1 we plot the average number of alleged felonies committed by Bexar County residents across neighborhoods (specifically, census block groups) between 1980 and 1994, based on the month of the alleged offense and the ethnicity of the alleged felon. Prior to May of 1988 (the expiration of the first amnesty), there were roughly an equal number of felonies allegedly committed by Hispanics and non-Hispanics, with some increase in overall charging over time that is driven in part by changes in drug control policy. Once the primary amnesty expires, however, there is a clear and dramatic jump in alleged felonies committed by Hispanic residents relative to non-Hispanic residents. Relative to the pre-IRCA period, the number of alleged felonies committed by Hispanics increased by 59% in the months after amnesty expired.

After empirically establishing that the expiration of IRCA’s amnesty programs was associated with an increase in alleged felonies by Hispanic residents of Bexar County relative to their non-Hispanic neighbors, particularly for crimes with a clear economic motive, we further parse our data by the probability that the 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 neighborhood characteristics. We use these neighborhood characteristics to identify those accused and convicted criminals more or less likely to have been impacted by IRCA, and thus those whose legal status and employment opportunities changed differentially with the law’s passage.

We find that the relative increase in criminal activity, and in particular felony drug offenses, among Hispanics was largest in neighborhoods with higher poverty rates, higher concentrations of people who identify themselves as Mexican, larger households, and greater fractions of residents who speak Spanish at home. Based on demographic research and Census data, these are neighborhoods where Mexican immigrants are most likely to initially locate.² While our preferred specification uses a conservative set of

¹ These figures are based on data compiled by the Texas A&M Real Estate Center, which are available at <http://recenter.tamu.edu/data/pop/popc/cnty48029.asp>.

² Indeed, 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.

fixed effects, our within-group estimates are robust to very weak assumptions about unobserved determinants of crime. We also show that the effects are driven by real changes in criminal activity among Hispanics who are likely to be recent immigrants rather than by any relationship between other potentially correlated demographic characteristics and relative crime rates.

The empirical results are consistent with a basic 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 police increased their presence in immigrant or poor neighborhoods following IRCA or if newly legalized immigrants were more likely to report neighborhood crime 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 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).

We differentiate between the impact of immigration reform on the behavior of recent immigrants and the behavior of law enforcement in three ways. First, we verify that our findings are driven by Hispanics as opposed to other minority groups also 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. This “hit-rate” strategy is rooted in empirical methods to detect racial profiling in traffic stops developed by Knowles et al. (2001). We find some evidence that, after IRCA, felony charges filed against Hispanics were less likely to result in a conviction.

Finally, using separate data on police and sheriff activity between 1985 and 1992, we examine whether new-immigrant neighborhoods became more heavily policed during this time period. Our results suggest that changes in policing explain only a small portion of the observed changes in felonies in the wake of IRCA; while overall levels of policing in new-immigrant neighborhoods appear to have fallen as immigration reform unfolded, the prosecutorial acceptance rates for arrests increased. Further, Hispanic residents began to be arrested for felonies at relatively higher rates than their non-Hispanic neighbors after IRCA was initially enacted. To the extent that these statistical findings reflect a change in the treatment of Hispanic residents, we estimate that a change in the behavior of the criminal justice system, as opposed to an actual change in the criminal activity of Hispanics affected by IRCA, can account for at most 30% of the increase in alleged felonies committed by Hispanic residents.

The paper proceeds as follows. In Section 2, we describe the key institutional changes put in place by IRCA, highlighting the impact of the law on both legal labor market opportunities for immigrants and

incentives for local criminal justice agencies. We then summarize the existing research on how the passage and implementation of IRCA changed immigration, labor market opportunities, and crime rates in Section 3. In Section 4, we discuss the theoretical framework that guides our empirical analysis. Then, in Section 5, we describe our data set 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 6, and conclude with discussion in Section 7.

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

Confronted with a large and growing unauthorized population, Congress passed a comprehensive set of immigration reforms in 1986. IRCA aimed to reduce unauthorized immigration permanently by granting amnesty as well as putting in place policies to stem the future flow of unauthorized immigrants. The latter included increased enforcement measures at the border and the interior, with Congress doubling the budget and staff of the Immigration and Naturalization Service (INS) between 1986 and 1990 (Wells 2004). IRCA substantially reduced the unauthorized population in the U.S. from its then-peak of three million. However, 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. In both routes to legalization, applicants were excludable for criminal charges. In particular, a felony conviction or multiple misdemeanor convictions resulted in ineligibility for amnesty (Kerwin 2010).

A companion section of the IRCA legislation augmented enforcement measures aimed at stemming the future flow of unauthorized immigration. 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), made it illegal for employers to knowingly hire or recruit unauthorized immigrants, and set forth civil and criminal penalties for violation.³

Anecdotal reports and evidence on immigration patterns 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. 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.⁴

A comparison of Census and INS data also point to 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 people per year between 1982 and 1984. Immigration rates rose slightly in 1985 and 1986 before falling again later in the decade.

Meanwhile, 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 Figure 4 shows, fewer than 25% reported arriving in the fourth quarter of any other year between 1970 and 1988.

³ In part due to concerns that the potential sanctions against employers violating IRCA would result in 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).

⁴ Authors’ calculations from 1992 INS Legalization Summary Public Use Tape.

Taken together with the high application approval rates and anecdotal evidence on the low cost of obtaining false documentation of residency, these figures imply that there was a large amount of manipulation of entry dates by illegal immigrants who arrived in the U.S. after the LAW cutoff date of January 1, 1982. This suggests that, as long as they filed for amnesty before the INS offices closed, even technically ineligible illegal immigrants in Bexar County could have been granted legal status. Indeed, on the morning of May 4, 1988, the last day of LAW amnesty, over 500 people were lined up outside of the San Antonio INS office.⁵

3. Existing Research on the Economic and Social Impacts of IRCA

Research on the effects of IRCA generally concludes that newly legalized immigrants were conferred positive economic and social benefits as a result of amnesty. While there is little consensus on the impacts of the other provisions of IRCA, and in particular its employer sanctions, prior research largely points to negative effects for future unauthorized immigrants. We discuss both of these strands of research in this section.

There is broad agreement among researchers 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%, compared to penalty of 14-24% for being unauthorized. Rivera-Batiz (1999), Lozano and Sorensen (2011), and Pan (2012) also find positive impacts of legal status on immigrants' earnings after IRCA. Meanwhile, Amuedo-Dorantes et al. (2007) find evidence of increased wage growth and job mobility among newly legalized immigrants in IRCA's wake.⁶

While amnesty may have conferred economic gains to legalized immigrants, evidence suggests that IRCA's effects on unauthorized workers who failed to obtain amnesty were generally negative. First, unauthorized immigrants who did not gain legal status or came to the U.S. after IRCA faced increasingly limited labor market opportunities, likely a reflection of employer costs associated with sanctions or sanction avoidance (Phillips and Massey 1999, Kossudji and Cobb-Clark 2002). Indeed, a number of studies suggest that after IRCA's passage, unauthorized immigrants experienced a substantial reduction in wages as well as poorer working conditions (Donato et al. 1992, Donato and Massey 1993, Sorensen and Bean 1994, Bansak and Raphael 2001). Job search durations among unauthorized workers also increased

⁵ Report filed by Dan Chiszer, United Press International, Domestic News – AM Cycle, May 4, 1988.

⁶ 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.

after IRCA (Bach and Brill 1991). Taken together, these studies suggest that IRCA's employment measures restricted the labor market opportunities of unauthorized immigrants.⁷

Thus, given IRCA affected more and less recent immigrants to the U.S. differently, we might expect the impact of IRCA on incentives to engage in the criminal behavior of an immigrant to depend critically on the timing of that immigration. To the extent that legalized immigrants could earn higher wages in the formal labor market after IRCA, 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. Consistent with this, there is some evidence that aggregate crime rates fell in jurisdictions where more people were granted citizenship status through IRCA in the late 1980s and early 1990s (Baker 2013).

However, immigrants who arrived in the U.S. after IRCA faced barriers to work that their predecessors did not, plausibly increasing their relative return to crime.⁸ Those arriving in the U.S. after 1986, but before the expirations of LAW and SAW in 1988, could not work for employers who complied with federal law. Some could have plausibly acquired temporary resident status, which, with some additional paperwork, could provide access to legal jobs. However, immigrants who entered the U.S. after amnesty expired were at a significant disadvantage relative to earlier cohorts. Not only did they lack the required documentation to complete an I-9 form, but they could no longer apply to eventually receive citizenship through LAW and SAW. Under the assumption that Mexican citizens are frequently and regularly moving to San Antonio (an assumption supported by both past empirical research on immigration after IRCA and, as we discuss later in the paper, birth rates in Bexar County hospitals), IRCA's enactment and the expiration of amnesty progressively divided those immigrants into winners and losers of immigration reform.

In addition to affecting crime through its impacts on the employment opportunities of immigrants, IRCA could have affected observed criminal activity through several other channels. First, IRCA stipulated that temporary residency status granted under the amnesty could be voided if an applicant were convicted of a felony or three misdemeanors during an 18-month probationary period. Therefore, at least for a short period immediately after the amnesty, there was a stronger incentive among applicants to avoid

⁷ Another strand of research considers the impact of IRCA on competing legal workers, and 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 discrimination resulting from IRCA seems to be small (Lowell et al. 1995).

⁸ Illegal immigrants could file fraudulent paperwork in an attempt to circumvent the employment verification process instituted under IRCA. This would counteract the negative effects of the limitations on employment opportunities to some extent. At the same time, someone caught presenting false identification information could be charged with felony forgery under Texas state statute; we include forgery as an income generating offense in the empirical analysis.

engaging in any type of criminal activity. Second, legalized immigrants may have been more willing to contact the police about criminal activity in their communities. To the extent that immigrants are concentrated in certain areas within cities, this could drive up observed criminal charges among immigrant populations. Third, if policing increased in neighborhoods with relatively more immigrants following IRCA's passage, it could also contribute to higher reported criminal activity. Finally, to the extent that legalization allowed some immigrants to reconnect with their families (e.g., by bringing spouses and children across the border after probationary periods ended), it could reduce the amount of crime; past research suggests that the presence of family and marriage are negatively related to criminal activity (Sampson et al. 2006).

Under the first three of these four alternative explanations, we would expect charges for all types of crime (i.e., income generating as well as non-income generating crimes) to change proportionately after IRCA. In particular, we would expect an across-the-board decrease in crime if the IRCA provisions regarding revocation of temporary residency status were important, and an across-the-board increase in reported crime if IRCA changed the relationship between residents and police. Under the fourth explanation, we would expect IRCA to decrease crime, but perhaps relatively more so for certain aggressive crimes, such as assaults or rape. In contrast, if the effects are driven by changes in employment opportunities, we would expect differentially large effects on income generating crimes as compared to non-income generating crimes. We test for such a difference as well as explore variation in other outcomes, including conviction rates, that allow us to speak to the relative importance of alternative mechanisms behind changes in observed criminal activity.

4. Theoretical Framework

To help motivate the analysis that follows, 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 still 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 in the formal labor market as $w_t(s)$. Meanwhile, the total net return to crime

is denoted $r(k_t)$, where $r'(k_t) > 0$.⁹ 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 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

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

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.¹⁰

Taking the first-order condition with respect to k_t (and assuming an interior solution), we have

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

This condition yields several insights. First, note that the right-hand side of the expression is greater than or equal to zero.¹¹ Therefore, 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

⁹ 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 .

¹⁰ It would be possible to add to this model incarceration and/or deportation. Either might be expected to make punishment more costly, particularly for those with high wages.

¹¹ In the case in which there is no anticipated punishment, we arrive at $r'(k_t) = w_t(s)$, similar to Grogger (1998).

immigration policy), crime rates (or at least observed crime rates) may vary across groups. Skogan (1984) emphasizes the possibility that lower observed crime rates among immigrants could be partly attributable to lower reporting, although more recent work suggests differences in reporting patterns in the U.S. are not large (Davis and Henderson 2003). 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. These higher 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 (Butcher and Piehl 2007).¹²

Applied to our empirical setting, the model suggests ambiguous changes in crime following the enactment of IRCA. All else being equal, to the extent that amnesty under IRCA conferred wage benefits to those authorized to work in the formal market (Kossoudji and Cobb-Clark 2002, Amuedo-Dorantes et al. 2007), we would expect newly legalized immigrants to engage in less crime relative to before amnesty. 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. Meanwhile, for those who were not eligible for amnesty, we would expect relative declines in wages, leading to more criminal activity; it is less clear that actual or perceived punishments changed for this group after relative to before IRCA. Changes in the nature of immigrants' relationship with the police among members of each group could also influence criminal activity, although the observed effect on crime rates will depend on the elasticity of criminal activity with respect to the probability of arrest.

Thus, it is ultimately an empirical question how changes in legal status should affect observed criminal activity. 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. 2010), 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 (i.e., w). For example, taking advantage of exogenous variation in immigrants' legal status after a round of European Union enlargement, Mastrobuoni and Pinotti (2012) 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,

¹² Another explanation for the relatively low crime rates of immigrants is selection in who immigrates (Butcher and Piehl 2007).

suggesting that access to legal jobs drove the observed decline in immigrant recidivism rates. However, because Mastrobuoni and Pinotti (2012) 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 EU 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. (2012) identify substantial increases in property crime in British neighborhoods with large influxes of immigrants, but only if those immigrants were refugees legally prohibited from working. In another study closely related to ours, Baker (2013) finds that U.S. counties with more legalized immigrants had lower aggregate crime rates after IRCA's amnesty; he also attributes much of the drop to improved economic opportunities among those legalized under the law. Unlike Bell et al. (2012) and Baker (2013), our individual-level data allow us to distinguish between crimes committed by groups unaffected by immigration reform and by those whose labor market opportunities were directly affected by the policies. We can also better disentangle alternative mechanisms for the observed changes in criminal activity by exploiting detailed information on neighborhood characteristics and conviction rates.

5. Data and Empirical Strategy

5.1 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 2010.¹³ 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.¹⁴ We divided Texas statutes into 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 classify 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 exclude 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

¹³ Freedman and Owens (2012) use these felony charge data to examine the impact of localized economic development on crime in the 2000s.

¹⁴ The court records also include information on actual convictions, which we exploit in Section 6.6.1.

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.

After dividing charges into income and non-income generating offenses, we 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 devise 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. 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. Overall, out of 80,398 charges filed against Bexar County residents, we classify 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 exclude 12 Bexar County block groups with missing demographic information, and the median population of the remaining 1,001 block groups in the sample was 1,061 in 1990.¹⁵

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 very 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, the majority of people with Mexican ancestors are U.S. citizens; on average, 9% of block group residents were born outside of the

¹⁵ As we describe in more detail in Section 6.6.2, we also use arrest records compiled by the police in a complementary analysis aimed at understanding changes in police activity around the time of immigration reform.

U.S., and about 13% of immigrants entered after 1985. Non-citizens only constituted just over 6% of neighborhoods' populations on average in 1990.

5.2 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 unaffected by any changes in employment opportunities and/or police behavior associated with IRCA implementation.¹⁶

Recall that, as shown in Figure 1, roughly an equal number of felony charges were filed against Hispanics and non-Hispanics each month prior to May of 1988. However, after the primary LAW 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 the probability of punishment for immigrants, we would expect crime rates for Hispanic residents to fall relative to non-Hispanics. However, the second critical effect of IRCA was to limit labor market opportunities for new immigrants. Therefore, among Hispanic defendants, we would expect to see a relative increase in offenses that are substitutes for formal work.

In Figure 5, we divide felony charges into income and non-income generating felonies. While there is some evidence of an increase in alleged non-income generating felonies by Hispanic people, the differential increase in felony behavior is much sharper for income generating crimes. This pattern of change is consistent with the end of IRCA amnesty limiting legal work opportunities for Mexican immigrants, who could no longer apply for temporary legal resident status. It also runs counter to the idea that increased policing in immigrant-heavy neighborhoods is entirely responsible for the observed changes in crime, as increased policing would be expected to affect all types of crime equally.

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):

$$(1) \quad \ln(\text{Crime}_{bgt}) = \alpha_b + \gamma_t + \text{HISP}_g \theta_0 + (\text{Enact}_t \times \text{HISP}_g) \theta_1 + (\text{LAW}_t \times \text{HISP}_g) \theta_2 + (\text{SAW}_t \times \text{HISP}_g) \theta_3 + \varepsilon_{bgt}$$

¹⁶ 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>).

where $Crime_{bgt}$ is the estimated rate of criminal charges filed against residents of neighborhood (measured as a census block group) b , who are of ethnic group g , based on alleged crimes committed in month t .¹⁷

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.¹⁸ The dummy variables for IRCA's enactment ($Enact_t$) and the expiration of the two amnesty programs (LAW_t and SAW_t) are equal to one in every period beginning in November of 1986, May of 1988, and December of 1988, respectively.¹⁹ The estimated values of θ_2 and θ_3 therefore capture the extent of increases in criminal behavior by Hispanic residents after the end of IRCA amnesty that cannot be explained by any other economic or social policies that would have also affected the criminality of non-Hispanics or the relative birth rates of the two ethnic groups.²⁰

If IRCA allowed current undocumented immigrants to gain temporary resident status, 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 immigrants. 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 annual 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 1990.²¹ This linear interpolation measure is

¹⁷ We add 0.001 to the rate of criminal charges filed against residents so that the dependent variable is defined for all neighborhoods.

¹⁸ 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.

¹⁹ 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. This change in attitudes towards drugs is reflected in 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.

²⁰ As we discuss in Section 6.1, the results are robust to including 1990 block group characteristics instead of block group fixed effects.

²¹ 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.

conceptually straightforward, but the assumption that the Hispanic and non-Hispanic populations grow in a smooth way over time is probably incorrect. The enactment of IRCA almost certainly induced discrete changes in the immigrant population of Bexar County; indeed, based on data on the number of babies born to a Hispanic parent from the National Center for Health Statistics' Vital Statistics Database, population growth in the county was relatively fast in the mid-1980s as 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 upwards.

Because of this problem with a linearly interpolated denominator, we also construct a second measure of population change during IRCA. For each block group, we take our estimated 1980 and 1990 population measures and assume that the entire change in population between these two years 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 of the population change to occur at the start of the post-amnesty period, these estimates will be lower bounds on the true change in propensity for Hispanic and non-Hispanic residents of different block groups to commit crime in this period.²²

While the estimates in equation (1) quantify the differential change in the criminal behavior of Hispanic people around IRCA, they do not differentiate between the behavior of Hispanic U.S. citizens, the behavior of Hispanic immigrants who gained legal status through the amnesty, and the behavior of Hispanic immigrants who were unable to receive amnesty either because they immigrated after the deadline or because they were unable to provide documentation of previous U.S. residence. We try to isolate the behavior of the last group of people 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 here is to identify, in any given year, Hispanic residents who are more or less likely to be recent immigrants, and who were therefore differentially affected by the expiration of these programs. Intuitively, the expiration of LAW and SAW amnesty programs should not have directly affected employment opportunities for Hispanic U.S. citizens. However, Hispanic residents who entered the country illegally after IRCA 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.

²² When we do not scale 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.

5.3 Triple Differences Using Proxies for Immigrant Destinations

If the increase in felony behavior by Hispanic residents after IRCA amnesty is driven by gradually reduced employment opportunities after IRCA, and a discrete drop after amnesty expiration, we would expect any jump in criminal behavior to be largest in neighborhoods where more Mexican immigrants initially 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 established correlation with new immigrant destinations in the U.S. generally, and San Antonio specifically. There is strong evidence in demography and population research that immigrants tend to live in poorer neighborhoods before moving to “higher quality” neighborhoods over time, a process commonly referred to as spatial assimilation (Massey 1985, Alba and Logan 1993). Immigrants also tend to live in more crowded housing than natives (Krivo 1995, Standish et al. 2010). 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).

Immigrants who enter the U.S. illegally are more likely to live in poor neighborhoods, but it is also true that they are more likely to settle in ethnic enclaves; the presence of co-ethnics is an important determinant of new immigrant location choice (Bartel 1989). Therefore, we also identify areas where more people are affected by IRCA by using residents’ self-reported national origin. Notably, those of Mexican descent (which 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.²³ 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 may have poorer English language skills, neighborhoods where more people speak Spanish in casual conversation are likely to be more attractive. On average across block groups, 39% of the San Antonio population reported speaking Spanish at home in 1990.

In addition to being attracted to places where more people share their ethnicity or speak Spanish, recent immigrants may be more likely to settle in neighborhoods where more people were born outside of the country. Indeed, at the state level, the size of the foreign born population is one of the strongest

²³ At the same time, however, Duncan and Trejo (2011) present evidence that high income citizens of Mexican descent are less likely to identify their Mexican origin on Census forms than lower income people.

predictors of settlement patterns (Dunlevy 1991, Buckley 1996, 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 6-10, we present differences in criminal incidence by ethnicity and crime type across high and low poverty neighborhoods, high and low population density neighborhoods, 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 respective characteristic.

The top panel of Figure 6 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. In contrast, there is no clear change in the poverty-crime gradient for non-Hispanics over this time period. In the lower panel of Figure 6, which shows the same differentials for non-income generating 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 7, 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 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 clear in Figures 8 and 9, 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 10 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:

$$\begin{aligned}
 \ln(\text{Crime}_{bgt}) = & \lambda_b + \eta_t + \text{Hisp}_g \delta_0 + (\text{Enact}_t \times \text{Hisp}_g) \delta_1 + (\text{LAW}_t \times \text{Hisp}_g) \delta_2 \\
 & + (\text{SAW}_t \times \text{Hisp}_g) \delta_3 + (\text{Enact}_t \times \text{Demo}_b) \phi_1 + (\text{LAW}_t \times \text{Demo}_b) \phi_2 \\
 (2) \quad & + (\text{SAW}_t \times \text{Demo}_b) \phi_3 + (\text{Hisp}_g \times \text{Demo}_b) \mu_1 + (\text{Hisp}_g \times \text{Demo}_b) \mu_2 \\
 & + (\text{Hisp}_g \times \text{Demo}_b) \mu_3 + (\text{Enact}_t \times \text{Hisp}_g \times \text{Demo}_b) \beta_1 \\
 & + (\text{LAW}_t \times \text{Hisp}_g \times \text{Demo}_b) \beta_2 + (\text{SAW}_t \times \text{Hisp}_g \times \text{Demo}_b) \beta_3 + v_{bgt}
 \end{aligned}$$

where $Demo_b$ is, alternately, the poverty rate, the number of residents per housing unit (i.e., population density), the percent of residents who are of Mexican descent, the percent who speak Spanish at home, and the percent who are foreign born.²⁴ While any of these proxies may have direct effects on the levels of crime 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-Hispanics around IRCA's enactment operates only through the fact that these proxies are correlated with new immigrant location choice, and any variation in new immigrant 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 other words, the only reason that the poverty-crime gradient (i.e., the positive relationship between poverty rates and crime rates) became steeper for Hispanics after IRCA is because of the increased criminal behavior of new immigrants. Further, any change in the criminal behavior of new immigrants that does not map into the spatial distribution of poverty rates in 1990 is assumed to be uncorrelated with our other control variables.

Hence, in equation (2), β_1 , β_2 , and β_3 represent the differential increase in criminal behavior of Hispanic residents across different Bexar County neighborhoods at each stage of IRCA.²⁵ If the observed change in crime is driven by changing economic opportunities for immigrants, we would expect that any increase in criminal behavior would be greater in neighborhoods with larger immigrant populations (and in particular, populations of more recent immigrants).²⁶

²⁴ 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.

²⁵ We also estimate 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 not only because of fewer geographic observations, but also because we use 2000 census tract boundaries, which may not reflect homogenous neighborhoods in the late 1980s. However, the results are qualitatively similar to the block group level analysis. One benefit of the tract level analysis is that we can differentiate between people living in historically Mexican neighborhoods from those living in neighborhoods with growing Mexican populations. 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 qualitatively similar to those presented here. Consistent with that, in our tract-level analysis, we find that 1990 levels, rather than percentage point changes, are driving the observed differences in criminal behavior.

²⁶ Figures 9 and 10 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 potentially 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 6.3.

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 (e.g., β_1^{Poverty} , β_1^{Density} , or $\beta_1^{\text{Spanish Speaking}}$) is not meaningful. In lieu of this, or other proposed 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 interacted 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 people who work in agriculture, the percent of immigrants who moved into the neighborhood after 1985, and the fraction of housing units that are owner occupied.²⁷

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. 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 multiple 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 equally large increases in all crimes. Alternatively, if police simply became more aggressive in their monitoring of income generating crimes, we might 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.

²⁷ 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.

Additionally, we more explicitly test for changes in the behavior of the criminal justice system by examining conviction and arrest rates using the same analytic framework described above.

6. Results

6.1 *Difference-in-Differences Estimates*

In Table 2, we present our first set of results for felony charges based on estimating equation (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-Hispanics. Note that the expiration of the second amnesty (SAW) did not appear to affect this outcome. The estimates are all robust to the replacement of block group fixed effects with 1990 block group characteristics (second column). In the third and fourth columns, 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 roughly 4.6% 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. All the results are highly robust to the inclusion of demographic controls or block group fixed effects, so in the interest of space, we will only present estimates from regressions with block group fixed effects hereafter.

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, including increased policing in Hispanic neighborhoods and a greater willingness among legal immigrants to contact the police, would also increase reported criminal activity, but would not predict the differentially large effects for income generating crime. Meanwhile, the harsher penalties for amnesty applicants during probation and any effects of IRCA on family reunification would predict declines in crime.

6.2 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 were likely to live. 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 lived. In the interest of space, we present estimates of each triple-interaction coefficient, and leave estimates of the first and second order impacts of neighborhood characteristics and Hispanic identity to Appendix Table A1.

We start by examining charges filed against Hispanics and non-Hispanics in new immigrant neighborhoods in Table 3, taking into account time invariant differences in criminal behavior across neighborhoods and across ethnicities as well as arbitrary monthly shocks to criminal activity in the county. While not reported for the sake of space, we also include all lower level interactions of ethnicity, time, and demographics. 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 proxies.

It is clear that the impact of IRCA on Hispanic criminal behavior was not uniform across neighborhoods. Indeed, patterns of poverty, housing 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 amnesty, 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, 10 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 this individual demographic measure plus that of the unobserved correlated proxies, it is notable that all of the effects are greater than zero. Taken together, and including other potential neighborhood differences, 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.

If this 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 clearly 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 size of the increase in non-income generating offenses. When we include multiple proxies to identify where new immigrants are most likely to locate, we estimate that there is a 4% chance that Hispanic people living in new immigrant neighborhoods increased their involvement in non-income generating offenses after IRCA was enacted. Subsequent to IRCA's enactment, we find no evidence of any additional differential changes in Hispanic involvement in non-income generating offenses. 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 felony behavior.

6.3 Controlling for Other Unobserved Heterogeneity across Geography and Time

In Table 4, 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 move.

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 suggest that acquisitive crime by Hispanic residents increased after IRCA, and this behavioral

change was larger in neighborhoods that look like places a new immigrant would move. After new immigrants were excluded from legal labor market opportunities, the slope of the crime-demographic gradient essentially doubled. However, while a number of these individual proxies are statistically different from zero on their own, it is not clear that we can reject the null hypothesis that the observed change in income generating offenses could not be a statistical anomaly. When we include this full set of fixed effects, along with the other neighborhood characteristics, we estimate that there is a 13% chance that income generating crime among Hispanics in new immigrant destinations did not change when LAW amnesty expired. While not statistically significant at conventional levels, the pattern of statistical precision is noticeably different than the observed changes in non-acquisitive crime, which changed in a way that is essentially unrelated to neighborhood demographics after LAW.

6.4 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. Second, while burglary, robbery, and theft are certainly income generating offenses, involvement in drug selling shares even more characteristics with a typical legal job; individuals sell drugs explicitly to earn money rather than to also seek some sort of thrill (Reuter et al. 1990, Levitt and Venkatesh 2000, Venkatesh and Levitt 2000). Third, immigrants, and in particular recent immigrants with strong social ties in other countries, face lower transportation costs in illegal international trade, giving them a comparative advantage in selling drugs compared to, for example, stealing cars and selling them for scrap (Reuter 2004).

In Table 5, 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-Hispanics after the enactment of IRCA in November of 1986. There is an even larger increase in drug offending after new immigrants were no longer able to apply for temporary visitor status. Notably, the Hispanic drug crime gap actually narrows in new immigrant destinations after December of 1988, which could plausibly coincide with the September 1st enactment of the Texas Anti-Drug Act of 1989. The results suggest that the Texas Anti-Drug Act may have had a differentially negative effect on the crime rates of Hispanic people in new-immigrant neighborhoods. Even with our full set of fixed effects (column 2), the geographic heterogeneity in the impacts of amnesty are highly statistically significant.

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 (U.S. Sentencing Commission 2009, Kennedy 2011). 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 columns 3 and 4 of Table 5, 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.

6.5 Population Growth Patterns

As previously discussed, there is no good information on the actual population at risk of committing crimes during the enactment of the provisions of IRCA. Any smooth population growth rate will tend to bias our estimates of the expiration of amnesty upwards if new immigrants became disproportionately more likely to live in our new-immigrant neighborhoods immediately after LAW expired. In Table 6, we provide a lower bound 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. In other words, we calculate every crime rate between January of 1980 and April of 1988 using the actual number of alleged felonies in that month and the estimated 1980 ethnicity-specific population. For every crime rate between May of 1988 and December of 1994, we use ethnicity-specific population counts from 1990.

Even with this extreme assumption about population growth at the moment that access to legal employment is cut off, we still detect an increase in the rate of felony accusations against Hispanic residents in new-immigrant neighborhoods. In fact, our individual estimates of the geographic pattern of crime increases after LAW are only slightly smaller than when we assumed linear population growth. For example, each additional percentage point increase in the poverty rate is associated with a 0.54 percentage point relative increase in income generating crime by Hispanic residents after LAW if we assume linear population growth during this time period, compared to a 0.50 percentage point increase in income generating crime if all of the 1980 to 1990 population growth occurred in May of 1988.

Turning to drug offenses, regardless of our assumptions about population growth, the difference in the rate of alleged felonies committed by Hispanic and non-Hispanic residents increased by about two percentage points after amnesty for each percentage point increase in the fraction of residents who were

non-native. Similarly, when we exclude non-Hispanic whites from the control group, we estimate slightly less than a two percentage point increase in Hispanic felonies for each additional percentage point increase in the share non-native in a neighborhood whether we assume linear population growth or impose that all the population growth during the 1980s occurred in May of 1988.

Assuming a discontinuous jump in the population of Hispanic and non-Hispanic residents at the expiration of LAW only slightly reduces the precision of our estimates when we include multiple proxies for new immigrant destinations. While we are only able to bound the true change in the propensity of new immigrants to commit income generating crime after the closure of amnesty offices, it does not appear to be the case that all of the observed change in the number alleged felonies observed in Figure 1 can be explained by an increase in the number of Hispanic people living in San Antonio.

6.6 Criminal Justice Response

Our theoretical framework linking immigration reform to criminal activity includes an important role for the criminal justice system itself through changes in the probability of punishment associated with legal status. Our estimates of the impact of IRCA on crime would be biased upward if, in response to the passage of IRCA or the expiration of IRCA amnesty, police increased their patrol of 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 conviction rates and police arrest records.

6.6.1 Evidence from Conviction Rates

We provide some evidence on changes in the way in which the criminal justice system interacted with Hispanics after IRCA by examining 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 began to “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.²⁸

²⁸ 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), who themselves build on the Becker (1957) test for discrimination. 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 residents, Knowles et al. (2001) show that court agents will file felony charges against Hispanic and non-Hispanic residents in such a way that the fraction of cases resulting in conviction

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

We present our estimates of the change in conviction rates for Hispanics living in poorer neighborhoods in Table 7. 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 0.5 percentage points greater for each percentage point increase in the block group poverty rate. As the results in Table 7 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. Meanwhile, for other proxies, we tend to observe statistically insignificant reductions in the probability that felony charges against Hispanics in new-immigrant neighborhoods result in convictions after LAW. Overall, it is difficult to draw strong conclusions about conviction rates for income generating offenses for any particular proxy.

When we combine all of the demographic interactions in the same regression, no individual proxy is statistically significant on its own. While we can easily reject the null hypothesis that our key proxies for immigrant destination were unrelated to the incidence of alleged felonies by Hispanics after amnesty, we cannot reject the null that conviction rates were unrelated. There is at least a 30% probability that overall conviction rates did not differentially change for Hispanics in immigrant destinations over time, and almost a 32% chance that there was no change in the prosecution of non-income generating offenses. However, it is worth noting that for income generating offenses, there is a much 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. Further investigation of the changing relationship between Hispanic people and the criminal justice system during immigration reform is necessary to clarify this relationship.

6.6.2 Evidence from Police Records

Conviction rates provide some information about the composition of allegations that are being levied against Hispanic and non-Hispanic San Antonians over time, but an arguably more direct way of

are equal across ethnic groups. However, if prosecutors or police gained some additional utility from arresting and prosecuting immigrants after amnesty, 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.

identifying changes in the criminal justice system is to examine patterns of police activity. If immigration reform spurred a reallocation of police forces toward immigrant enclaves, and in particular Hispanic residents of those enclaves, then the differential increase in felony charging could simply be a function of law enforcement casting a wider net in these neighborhoods.

We were given access to digital records of all adult arrests made in Bexar County from June of 1986 to December of 1992. Of the 291,505 records initially provided to us by the Bexar County Department of Information Technology, we were able to identify the latitude and longitude of 202,115 arrests that occurred in public or private spaces, rather than in a police department or court house. Of these, 849 records were for arrests made before June of 1986, but a comparison of these data with FBI records of adult arrests made in Bexar County revealed clear undercounting in these months; between 1987 and 1992, the total number of arrests recorded by the IT department equaled 96% of the total reported to the FBI. Once we excluded arrests for DUIs, our final arrest data set contains 174,278 arrests, roughly 54% of the total number of adults arrests in Bexar County reported in the UCR.

Figure 11 displays the month-to-month variation in felony and misdemeanor arrests in our final sample, along with annual variation from the FBI. The most striking event is that, in contrast with the increase in felony charging, in May of 1988, just as LAW amnesty expired, there was a dramatic reduction in arrests.²⁹ In Figures 12 and 13, we divide these arrests based on whether they were for felonies or misdemeanors, and whether the arrestee was Hispanic.³⁰ The drop in policing is evident in both felony and misdemeanor arrests, and affects both Hispanic and non-Hispanic people.

There is less evidence that this reduction in policing reduced the probability that residents of new-immigrant destinations were arrested for a felony. In Table 8, we present estimates of how arrests changed across block groups over time. Unlike with our court data, here we do not focus on the ethnicity of the person being arrested, but rather examine whether there was more or less police activity, as measured by arrests, in poorer, denser neighborhoods where more people spoke Spanish, were of Mexican descent, or were born outside of the U.S. We do not find strong evidence that police arrested fewer people for felonies in new-immigrant destinations when IRCA was enacted or after LAW amnesty expired. It does appear to be the case that police arrested more felons in immigrant enclaves after SAW. While the majority of arrests take place during or immediately after the crime, an increase in arrests after December of 1988 is not inconsistent with the observed increase in criminal behavior that we found taking place after May of 1988.

²⁹ This reduction in 1988 occurred as law enforcement changed their strategies and in the midst of new police contract negotiations (Casey 2013). We are currently exploring archived newspaper articles from San Antonio to better understand the causes of the observed reduction in arrests.

³⁰ We define ethnicity using the same name-based methodology as in the court data.

Turning to the rate of misdemeanor arrests, there is strong evidence that police were less likely to arrest people for minor offenses in immigrant enclaves as immigration reform was enacted. We find sizable and statistically significant reductions in the misdemeanor arrest rate in immigrant destinations that coincide with IRCA's enactment and with the expiration of LAW. However, in December of 1988, misdemeanor arrest rates increase, counteracting the reduction observed after LAW. To the extent that misdemeanor arrests are a better reflection of general police activity than felony arrests, this suggests that the perceived probability of detection may have fallen in immigrant enclaves in May of 1988.

In the final column of Table 8, we conduct another "hit-rate" analysis with the arrest data. Specifically, not all arrests result in prosecution, but arrests that do result in formal felony or misdemeanor charges are assigned a court case number. Just as we treated conviction rates as a measure of the "quality" of felony charges being filed against individuals, we interpret prosecutorial acceptance rates as a measure of the "quality" of arrests made by police.

While our evidence that prosecutors were becoming more likely to file felony charges against Hispanics from immigrant enclaves after IRCA was merely suggestive, our hit-rate analysis of arrests is more conclusive. After the enactment of IRCA, we estimate small but highly precise increases in the rate at which prosecutors accept arrests from immigrant destinations for prosecution. After November of 1986, each percentage point increase in neighborhood poverty rate was associated with a 0.2 percentage point increase in the likelihood of formal charges being filed. Each additional person per housing unit was associated with a 2.3 percentage point increase in the probability that arrests resulted in charges, and block groups with more immigrants also saw substantial changes in the probability of being prosecuted conditional on arrest. After new immigrants were unable to apply for legal status through LAW, individual point estimates suggest that there was a further increase in the rate of charges being filed, and based on our joint estimates using all our proxies for new-immigrant destinations, there is only a 10.7% probability that there was no differential change in acceptance rates from new-immigrant neighborhoods after LAW expired.

The aggregate pattern of arrests in Bexar County strongly points to de-policing. In particular, police became much less likely to arrest people living in new-immigrant destinations for minor offenses. Consistent with this, the arrests made in those areas seem to be of higher quality in the sense that prosecutors believed that a larger fraction of them warranted formal prosecution. To the extent that this change in policing strategy reduced the perceived probability of apprehension for Bexar County residents, we would expect all crime rates to increase. However, we observe effects only for a subset of criminal behavior: income generating offenses, and specifically felony drug crimes, by Hispanic residents. Our interpretation of this as primarily a response to a change in employment opportunities, rather than the

change in apprehension probability, would be weaker if the reduction in the probability of arrest was larger for Hispanic residents of those new-immigrant destinations.

In Table 9, we add a third difference to our analysis of arrest patterns, and examine how the differential between Hispanic and non-Hispanic arrests changed in immigrant enclaves during immigration reform. While only one individual proxy is statistically significant on its own, we do find evidence of a change in the probability that Hispanic residents of new-immigrant destinations were arrested for felonies after the initial enactment of IRCA. These same residents appear to be increasingly subject to arrest after December of 1988, but we are unable to reject the null hypothesis that Hispanic and non-Hispanic residents were de-policed at the same rate after LAW expired. We also fail to find strong statistical evidence that there was a change in the probability that these arrests resulted in formal prosecution. While there was clearly de-policing in new-immigrant destinations during immigration reform, if anything, Hispanic people living in those areas were relatively more likely to be charged with serious offenses than their non-Hispanic neighbors. Because of this, we conclude that the reduced employment opportunities for immigrants without legal status were an important driver of the observed increase in crime.

7. Conclusion

Immigrants have long been associated with lawlessness and criminality in the public mind. However, there is very little consistent evidence that the arrival of new immigrants, legal or illegal, is correlated with an increase in crime rates. One potentially important explanation for the mixed results on the relationship between immigration and crime is that there is no first-order relationship; the propensity of a new immigrant 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 temporarily provided some undocumented immigrants with a pathway to legal status 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 amnesty, 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 determining the criminal behavior of new immigrants by exploiting the structure of IRCA together with a unique set of data on felony charges filed against residents of Bexar County, Texas, a county that 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 illegal 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 at home, and were born outside the U.S. This finding is particularly important today, as recent survey evidence from the U.S. suggests that employer sanctions are the most popular policy for controlling illegal immigration, and are considered by the public to be more effective than making it easier for immigrants to obtain legal status or reinforcing border controls (Transatlantic Trends 2011).

Our measure of criminal activity is based on felony charges filed in Bexar County's District Court, which reflect both criminal behavior and the propensity of police and prosecutors to arrest and file charges against Hispanic people. Comparing the change in alleged felonies to the change in felonies that result in a conviction implies that at most one-third of the observed change in felony charges could be driven by a change in prosecutorial attitudes towards Hispanic defendants. An additional analysis of police activity at the time of immigration reform further suggests changes in apprehension probability are unlikely to explain observed patterns of crime. Instead, our results provide strong support for the theoretical prediction that limiting job opportunities for immigrants leads to higher crime.

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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)			
Poverty Rate	1001	15.64	16.72
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
Concentration of New Immigrants [‡]	1001	12.76	16.00
Percent Non-Citizens	1001	6.40	6.13
Ln(Population)	1001	6.91	0.62
People per Housing Unit	1001	2.72	0.88
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

[‡] Percent of immigrants who moved to the U.S. after 1985.

Table 2: IRCA and Felony Charges, Linear Population Growth

	All Crimes		Income Generating		Non-Income Generating	
Hispanic Defendant	0.00943	0.00882	-0.0235	-0.0243	0.0400***	0.0403***
	[0.0205]	[0.0207]	[0.0165]	[0.0166]	[0.00806]	[0.00812]
Hispanic × IRCA	-0.0135	-0.00887	0.00504	0.0109	-0.0159	-0.0172
	[0.0240]	[0.0240]	[0.0210]	[0.0209]	[0.0131]	[0.0131]
Hispanic × LAW	0.108*	0.110*	0.0782+	0.0791*	0.0445*	0.0457*
Expiration	[0.0430]	[0.0432]	[0.0399]	[0.0401]	[0.0223]	[0.0225]
Hispanic × SAW	0.00102	-0.001	-0.00395	-0.00686	-0.00236	-0.00155
Expiration	[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

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. 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, Linear Population Growth and Baseline Fixed Effects

Results from Individual Proxy Regressions	All Crimes	Income Generating	Non-Income Generating
Hispanic × IRCA × Poverty Rate	0.00182 [0.00163]	0.00382* [0.00150]	-0.00177+ [0.000930]
Hispanic × LAW × Poverty Rate	0.00534+ [0.00273]	0.00542* [0.00262]	0.00176 [0.00150]
Hispanic × SAW × Poverty Rate	-0.0001 [0.00245]	-0.00158 [0.00239]	0.00131 [0.00123]
Hispanic × IRCA × People / Housing Units	0.00701 [0.0231]	0.0291 [0.0204]	-0.0213 [0.0139]
Hispanic × LAW × People / Housing Units	0.100+ [0.0547]	0.0996* [0.0494]	0.0364 [0.0306]
Hispanic × SAW × People / Housing Units	-0.0342 [0.0393]	-0.0461 [0.0351]	-0.00234 [0.0243]
Hispanic × IRCA × Percent Mexican	0.00161* [0.000821]	0.00222** [0.000711]	-0.0002 [0.000462]
Hispanic × LAW × Percent Mexican	0.00436** [0.00145]	0.00497*** [0.00133]	0.000305 [0.000777]
Hispanic × SAW × Percent Mexican	-0.00019 [0.00133]	-0.00133 [0.00126]	0.00119+ [0.000655]
Hispanic × IRCA × Percent Spanish at Home	0.00187+ [0.000973]	0.00278** [0.000854]	-0.00038 [0.000550]
Hispanic × LAW × Percent Spanish at Home	0.00558** [0.00173]	0.00608*** [0.00158]	0.000674 [0.000944]
Hispanic × SAW × Percent Spanish at Home	-0.00089 [0.00160]	-0.00209 [0.00150]	0.00107 [0.000786]
Hispanic × IRCA × Percent Immigrant	0.00338 [0.00362]	0.00766* [0.00349]	-0.00373 [0.00234]
Hispanic × LAW × Percent Immigrant	0.0126+ [0.00699]	0.0127+ [0.00699]	0.00358 [0.00383]
Hispanic × SAW × Percent Immigrant	-0.00105 [0.00621]	-0.00454 [0.00612]	0.00318 [0.00316]
Results from Multi-Proxy Regression			
	R ²	0.0985	0.0833
	<i>p(Enactment)=0</i>	0.4236	0.0834
	<i>p(exLAW)=0</i>	0.0498	0.0047
	<i>p(exSAW)=0</i>	0.6866	0.6338
			0.0332
			0.0350
			0.4120
			0.2637

Each regression includes 360,180 observations, 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.

Table 4: IRCA and Felony Charges, Linear Population Growth and Full Fixed Effects

Results from Individual Proxy Regressions	All Crimes	Income Generating	Non-Income Generating	
Hispanic × IRCA × Poverty Rate	0.00179 [0.00231]	0.00379+ [0.00212]	-0.00177 [0.00132]	
Hispanic × LAW × Poverty Rate	0.00539 [0.00387]	0.00546 [0.00371]	0.00176 [0.00212]	
Hispanic × SAW × Poverty Rate	-0.00011 [0.00347]	-0.0016 [0.00339]	0.00131 [0.00174]	
Hispanic × IRCA × People / Housing Units	0.00631 [0.0327]	0.0283 [0.0288]	-0.0212 [0.0197]	
Hispanic × LAW × People / Housing Units	0.101 [0.0777]	0.101 [0.0702]	0.0364 [0.0433]	
Hispanic × SAW × People / Housing Units	-0.0345 [0.0558]	-0.0465 [0.0498]	-0.00226 [0.0345]	
Hispanic × IRCA × Percent Mexican	0.00159 [0.00116]	0.00220* [0.00101]	-0.0002 [0.000655]	
Hispanic × LAW × Percent Mexican	0.00441* [0.00206]	0.00502** [0.00188]	0.000306 [0.00110]	
Hispanic × SAW × Percent Mexican	-0.0002 [0.00189]	-0.00135 [0.00179]	0.00119 [0.000929]	
Hispanic × IRCA × Percent Spanish at Home	0.00184 [0.00138]	0.00275* [0.00121]	-0.00038 [0.000779]	
Hispanic × LAW × Percent Spanish at Home	0.00563* [0.00245]	0.00612** [0.00225]	0.000675 [0.00134]	
Hispanic × SAW × Percent Spanish at Home	-0.00091 [0.00227]	-0.00211 [0.00213]	0.00108 [0.00111]	
Hispanic × IRCA × Percent Immigrant	0.00349 [0.00513]	0.00778 [0.00494]	-0.00373 [0.00332]	
Hispanic × LAW × Percent Immigrant	0.0124 [0.00991]	0.0125 [0.00991]	0.00358 [0.00543]	
Hispanic × SAW × Percent Immigrant	-0.001 [0.00881]	-0.00448 [0.00867]	0.00317 [0.00448]	
Results from Multi-Proxy Regression				
	R ²	0.560	0.551	0.522
	<i>p(Enactment)=0</i>	0.8025	0.4336	0.3089
	<i>p(exLAW)=0</i>	0.3438	0.1326	0.7751
	<i>p(exSAW)=0</i>	0.9093	0.8875	0.6660

Each regression includes 360,180 observations, 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.

Table 5: IRCA and Felony Drug Charges, Linear Population Growth

Results from Individual Proxy Regressions	Full Sample N=360,180		Non-Hispanic Whites Excluded N=349,200	
Hispanic × IRCA × Poverty Rate	0.00463*** [0.000868]	0.00462*** [0.00123]	0.00389*** [0.00101]	0.00397** [0.00145]
Hispanic × LAW × Poverty Rate	0.00810*** [0.00178]	0.00813** [0.00252]	0.00677** [0.00209]	0.00626* [0.00307]
Hispanic × SAW × Poverty Rate	-0.00520** [0.00171]	-0.00521* [0.00242]	-0.00495* [0.00206]	-0.00478 [0.00301]
Hispanic × IRCA × People / Housing Units	0.0365+ [0.0187]	0.0362 [0.0264]	0.0373* [0.0176]	0.0344 [0.0249]
Hispanic × LAW × People / Housing Units	0.116** [0.0373]	0.116* [0.0531]	0.0920** [0.0344]	0.0868+ [0.0489]
Hispanic × SAW × People / Housing Units	-0.0658* [0.0271]	-0.0660+ [0.0385]	-0.0567+ [0.0291]	-0.0578 [0.0423]
Hispanic × IRCA × Percent Mexican	0.00267*** [0.000457]	0.00267*** [0.000648]	0.00214*** [0.000539]	0.00218** [0.000777]
Hispanic × LAW × Percent Mexican	0.00572*** [0.000956]	0.00575*** [0.00136]	0.00471*** [0.00121]	0.00445* [0.00178]
Hispanic × SAW × Percent Mexican	-0.00246** [0.000861]	-0.00247* [0.00122]	-0.00223* [0.00111]	-0.00214 [0.00164]
Hispanic × IRCA × Percent Spanish at Home	0.00322*** [0.000541]	0.00321*** [0.000767]	0.00264*** [0.000629]	0.00269** [0.000908]
Hispanic × LAW × Percent Spanish at Home	0.00714*** [0.00113]	0.00717*** [0.00160]	0.00589*** [0.00141]	0.00564** [0.00208]
Hispanic × SAW × Percent Spanish at Home	-0.00343** [0.00104]	-0.00344* [0.00148]	-0.00321* [0.00133]	-0.00312 [0.00198]
Hispanic × IRCA × Percent Immigrant	0.00580** [0.00202]	0.00583* [0.00287]	0.00259 [0.00255]	0.0023 [0.00365]
Hispanic × LAW × Percent Immigrant	0.0197*** [0.00461]	0.0196** [0.00654]	0.0191*** [0.00525]	0.0181* [0.00769]
Hispanic × SAW × Percent Immigrant	-0.0101* [0.00428]	-0.0101+ [0.00607]	-0.0102* [0.00491]	-0.01 [0.00718]
Block Group × Month, Ethnicity × Month, and Block Group × Ethnicity FEs		Y		Y
Results from Multi-Proxy Regression	R ²			
<i>p(Enactment)=0</i>	0.0538	0.538	0.0456	0.545
<i>p(exLAW)=0</i>	0.0000	0.0029	0.0003	0.0372
<i>p(exSAW)=0</i>	0.0000	0.0005	0.0006	0.0862
<i>p(exSAW)=0</i>	0.0074	0.1608	0.0634	0.4087

Each regression includes 360,180 observations, 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.

Table 6: IRCA and Felony Charges, 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	0.00207 [0.00165]	0.00402** [0.00151]	-0.00168+ [0.000938]	0.00469*** [0.000867]	0.00383** [0.00120]
Hispanic × LAW × Poverty Rate	0.00482+ [0.00276]	0.00499+ [0.00263]	0.00161 [0.00151]	0.00797*** [0.00180]	0.00698*** [0.00208]
Hispanic × SAW × Poverty Rate	0.000144 [0.00245]	-0.00137 [0.00239]	0.00136 [0.00123]	-0.00510** [0.00171]	-0.00509* [0.00205]
Hispanic × IRCA × People / Housing Units	0.000548 [0.0254]	0.0242 [0.0218]	-0.024 [0.0150]	0.0334+ [0.0203]	0.0404+ [0.0227]
Hispanic × LAW × People / Housing Units	0.110+ [0.0588]	0.107* [0.0521]	0.0399 [0.0328]	0.119** [0.0388]	0.0847* [0.0366]
Hispanic × SAW × People / Housing Units	-0.0341 [0.0393]	-0.046 [0.0351]	-0.00193 [0.0243]	-0.0661* [0.0271]	-0.0552+ [0.0289]
Hispanic × IRCA × Percent Mexican	0.00188* [0.000836]	0.00244*** [0.000723]	-0.00014 [0.000468]	0.00276*** [0.000459]	0.00265*** [0.000638]
Hispanic × LAW × Percent Mexican	0.00388** [0.00147]	0.00458*** [0.00135]	0.00019 [0.000783]	0.00560*** [0.000964]	0.00401*** [0.00121]
Hispanic × SAW × Percent Mexican	-9.8E-06 [0.00134]	-0.00119 [0.00126]	0.00123+ [0.000655]	-0.00241** [0.000863]	-0.00216+ [0.00110]
Hispanic × IRCA × Percent Spanish at Home	0.00205* [0.000991]	0.00294*** [0.000868]	-0.00035 [0.000557]	0.00328*** [0.000545]	0.00314*** [0.000735]
Hispanic × LAW × Percent Spanish at Home	0.00526** [0.00176]	0.00580*** [0.00161]	0.000614 [0.000951]	0.00705*** [0.00114]	0.00521*** [0.00142]
Hispanic × SAW × Percent Spanish at Home	-0.00077 [0.00160]	-0.00199 [0.00151]	0.0011 [0.000786]	-0.00340** [0.00104]	-0.00315* [0.00133]
Hispanic × IRCA × Percent Immigrant	0.0035 [0.00367]	0.00769* [0.00350]	-0.00364 [0.00235]	0.00580** [0.00201]	0.00356 [0.00300]
Hispanic × LAW × Percent Immigrant	0.0125+ [0.00706]	0.0127+ [0.00703]	0.00355 [0.00385]	0.0198*** [0.00462]	0.0177*** [0.00536]
Hispanic × SAW × Percent Immigrant	-0.00109 [0.00621]	-0.00454 [0.00612]	0.00312 [0.00316]	-0.0101* [0.00428]	-0.0102* [0.00491]
Results from Multi-Proxy Regression					
R ²	0.0970	0.0821	0.0328		0.0438
<i>p</i> (Enactment)=0	0.2300	0.0574	0.0238	0.0000	0.0007
<i>p</i> (exLAW)=0	0.0578	0.0098	0.3004	0.0000	0.0071
<i>p</i> (exSAW)=0	0.5662	0.5758	0.2144	0.0066	0.0558

Regressions in columns 1 – 4 include 360,180 observations, and column 5 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.

Table 7: IRCA and Felony Conviction Rates

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 [0.0709]	-0.00882 [0.0867]	0.177 [0.176]	-0.145 [0.168]	0.100 [0.197]
Hispanic × LAW × Poverty Rate	-0.146 [0.121]	0.0333 [0.137]	-0.484+ [0.290]	0.116 [0.199]	0.0911 [0.231]
Hispanic × SAW × Poverty Rate	0.083 [0.109]	-0.0231 [0.113]	0.22 [0.256]	-0.0471 [0.167]	-0.106 [0.187]
Hispanic × IRCA × People / Housing Units	1.822 [1.319]	0.215 [1.458]	4.492+ [2.716]	-0.0718 [3.065]	7.492* [3.800]
Hispanic × LAW × People / Housing Units	-1.494 [1.944]	0.994 [2.417]	-5.943+ [3.201]	-2.938 [3.762]	-4.86 [3.659]
Hispanic × SAW × People / Housing Units	-0.0008 [1.722]	-0.555 [2.136]	0.465 [2.511]	2.859 [3.109]	2.885 [3.274]
Hispanic × IRCA × Percent Mexican	0.0832 [0.0512]	0.0493 [0.0589]	0.0723 [0.111]	-0.0446 [0.106]	0.0671 [0.125]
Hispanic × LAW × Percent Mexican	-0.170* [0.0757]	-0.117 [0.0848]	-0.185 [0.178]	-0.0591 [0.133]	-0.00652 [0.151]
Hispanic × SAW × Percent Mexican	0.0947 [0.0693]	0.0803 [0.0749]	0.044 [0.152]	0.0617 [0.113]	0.0707 [0.126]
Hispanic × IRCA × Percent Spanish at Home	0.107+ [0.0580]	0.0599 [0.0677]	0.143 [0.128]	-0.0476 [0.123]	0.132 [0.149]
Hispanic × LAW × Percent Spanish at Home	-0.191* [0.0870]	-0.125 [0.0974]	-0.262 [0.207]	-0.0507 [0.152]	-0.0148 [0.174]
Hispanic × SAW × Percent Spanish at Home	0.0981 [0.0801]	0.0856 [0.0861]	0.0468 [0.177]	0.0699 [0.130]	0.0702 [0.144]
Hispanic × IRCA × Percent Immigrant	0.249 [0.202]	0.217 [0.237]	-0.142 [0.408]	0.198 [0.438]	0.510 [0.524]
Hispanic × LAW × Percent Immigrant	-0.441 [0.290]	-0.382 [0.311]	-0.27 [0.794]	-0.258 [0.494]	-0.230 [0.537]
Hispanic × SAW × Percent Immigrant	0.284 [0.260]	0.234 [0.276]	0.361 [0.716]	0.199 [0.425]	0.270 [0.481]
Results from Multi-Proxy Regression					
R ²	0.0499	0.0628	0.0912	0.125	0.133
<i>p(Enactment)=0</i>	0.4182	0.5170	0.2422	0.8972	0.1434
<i>p(exLAW)=0</i>	0.3003	0.1601	0.3216	0.8125	0.4280
<i>p(exSAW)=0</i>	0.7691	0.5241	0.9410	0.9171	0.8288
Observations	55,418	43,172	16,514	16,231	12,920

Each regression includes 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.

Table 8: IRCA and Neighborhood Arrest Rates, June 1986 – December 1992, Linear Population Growth

Results from Individual Proxy Regressions	Felony Arrest Rate	Misdemeanor Arrest Rate	Prosecutorial Acceptance Rate
	<i>mean = 185</i>	<i>mean = 248</i>	<i>mean=80.0</i>
IRCA × Poverty Rate	0.00334 [0.00221]	-0.0171*** [0.00189]	0.200*** [0.0490]
LAW × Poverty Rate	-0.0005 [0.00156]	-0.0119*** [0.00156]	0.0416 [0.0360]
SAW × Poverty Rate	0.0131*** [0.00217]	0.0134*** [0.00215]	-0.00832 [0.0303]
IRCA × People / Housing Units	0.0335 [0.0388]	-0.122*** [0.0330]	2.330** [0.884]
LAW × People / Housing Units	-0.0433+ [0.0258]	-0.0307 [0.0386]	1.482* [0.673]
SAW × People / Housing Units	-0.0217 [0.0370]	0.0219 [0.0575]	-1.126* [0.570]
IRCA × Percent Mexican	0.00117 [0.00105]	-0.00978*** [0.00109]	0.149*** [0.0309]
LAW × Percent Mexican	-0.00133+ [0.000801]	-0.00551*** [0.000840]	0.0545* [0.0232]
SAW × Percent Mexican	0.00724*** [0.00115]	0.00604*** [0.00116]	-0.0215 [0.0194]
IRCA × Percent Spanish at Home	0.00104 [0.00128]	-0.0124*** [0.00127]	0.190*** [0.0353]
LAW × Percent Spanish at Home	-0.00134 [0.000972]	-0.00646*** [0.00101]	0.0645* [0.0266]
SAW × Percent Spanish at Home	0.00909*** [0.00138]	0.00709*** [0.00140]	-0.0172 [0.0221]
IRCA × Percent Immigrant	-0.00026 [0.00483]	-0.0401*** [0.00486]	0.571*** [0.111]
LAW × Percent Immigrant	-0.00326 [0.00416]	-0.0237*** [0.00425]	0.165+ [0.0952]
SAW × Percent Immigrant	0.0285*** [0.00581]	0.0245*** [0.00573]	-0.0115 [0.0841]
Results from Multi-Proxy Regression			
	R ²	0.275	0.334
	<i>p(Enactment)=0</i>	0.4021	0.0000
	<i>p(exLAW)=0</i>	0.1961	0.1067
	<i>p(exSAW)=0</i>	0.0000	0.3913

Regressions in columns 1 and 2 contain 78,078 observations, and regressions in column 3 contain 38,839 observations. All models include 78 month dummies and 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.

Table 9: IRCA and Ethnicity-Specific Arrest Rates, June 1986 – December 1992, Linear Population Growth

Results from Individual Proxy Regressions	Felony Arrest Rate <i>mean = 356</i>	Misdemeanor Arrest Rate <i>mean = 470</i>	Prosecutorial Acceptance Rate <i>mean=79.8</i>	
Hispanic × IRCA × Poverty Rate	0.00091 [0.00276]	-0.00491 [0.00311]	-1.404 [1.236]	
Hispanic × LAW × Poverty Rate	-0.00106 [0.00239]	-0.00186 [0.00228]	1.404 [1.150]	
Hispanic × SAW × Poverty Rate	0.0126*** [0.00255]	0.0129*** [0.00258]	-2.119* [0.968]	
Hispanic × IRCA × People / Housing Units	0.00549 [0.0460]	0.0343 [0.0727]	-0.164 [1.01]	
Hispanic × LAW × People / Housing Units	0.000349 [0.0305]	-0.0612+ [0.0354]	1.99 [2.51]	
Hispanic × SAW × People / Housing Units	0.100* [0.0433]	0.149** [0.0543]	-1.15 [2.22]	
Hispanic × IRCA × Percent Mexican	0.00106 [0.00131]	-0.00171 [0.00157]	-0.0955+ [0.0543]	
Hispanic × LAW × Percent Mexican	-0.00036 [0.00115]	-0.00185 [0.00123]	-0.0138 [0.0429]	
Hispanic × SAW × Percent Mexican	0.00823*** [0.00132]	0.00760*** [0.00133]	-0.0162 [0.0378]	
Hispanic × IRCA × Percent Spanish at Home	0.00215 [0.00158]	-0.00316** [0.00119]	-0.106+ [0.0630]	
Hispanic × LAW × Percent Spanish at Home	-0.00088 [0.00139]	0.00056 [0.000843]	-0.0296 [0.0494]	
Hispanic × SAW × Percent Spanish at Home	0.0106*** [0.00156]	0.00127 [0.000968]	-0.00978 [0.0433]	
Hispanic × IRCA × Percent Immigrant	0.0160** [0.00586]	-0.00393 [0.00849]	-0.0003 [0.190]	
Hispanic × LAW × Percent Immigrant	-0.00465 [0.00502]	-0.00913 [0.00635]	-0.0463 [0.171]	
Hispanic × SAW × Percent Immigrant	0.0263*** [0.00579]	0.0312*** [0.00658]	-0.066 [0.143]	
Results from Multi-Proxy Regression				
	R ²	0.198	0.271	0.183
	<i>p(Enactment)=0</i>	0.0165	0.5549	0.5357
	<i>p(exLAW)=0</i>	0.8385	0.3923	0.6283
	<i>p(exSAW)=0</i>	0.0000	0.0000	0.0487

Regressions in columns 1 and 2 contain 156,156 observations, and regressions in column 3 contain 51,257 observations. All models include 78 month dummies and 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.

Figure 1: Average Monthly Neighborhood Criminal Incidence by Ethnicity

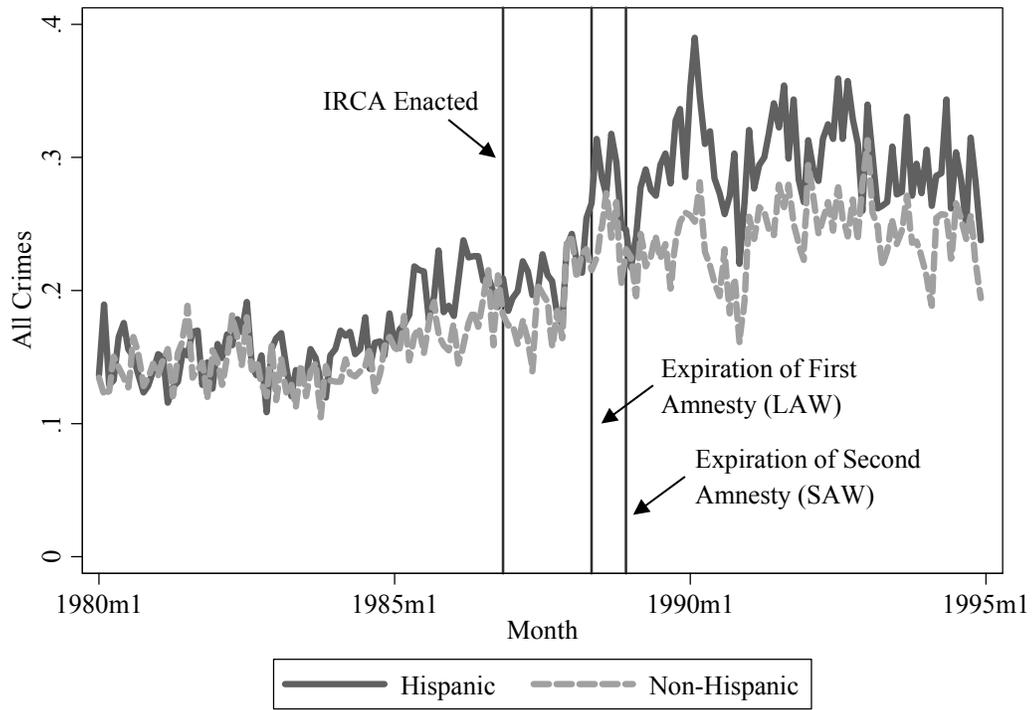


Figure 2: Immigration to Bexar County by Date of Entry, 1990 Census Data

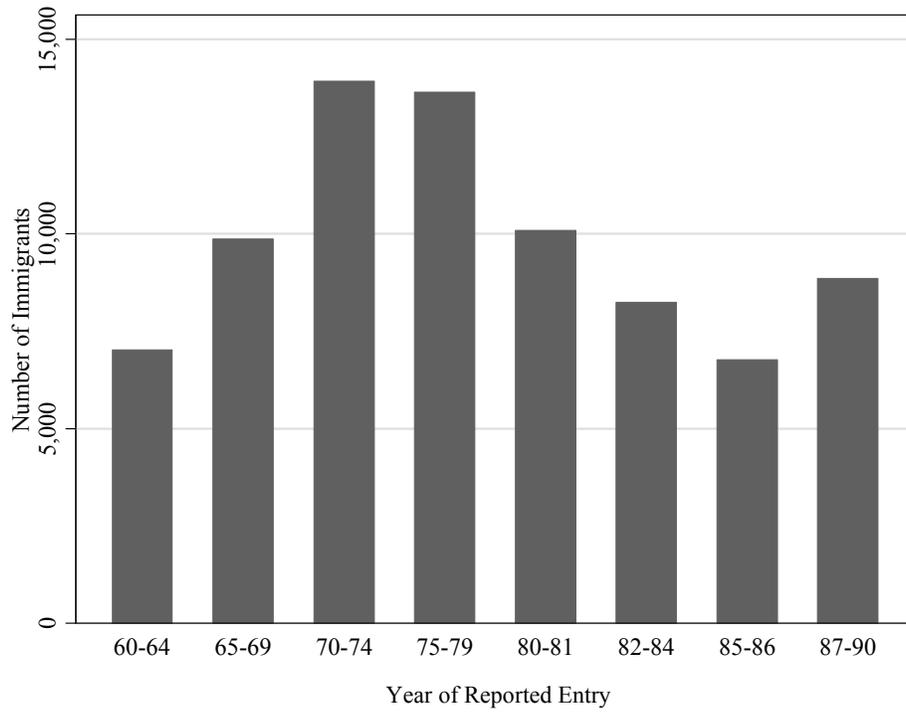


Figure 3: Immigration to Bexar County by Date of Entry, 1992 INS Legalization Summary Tape

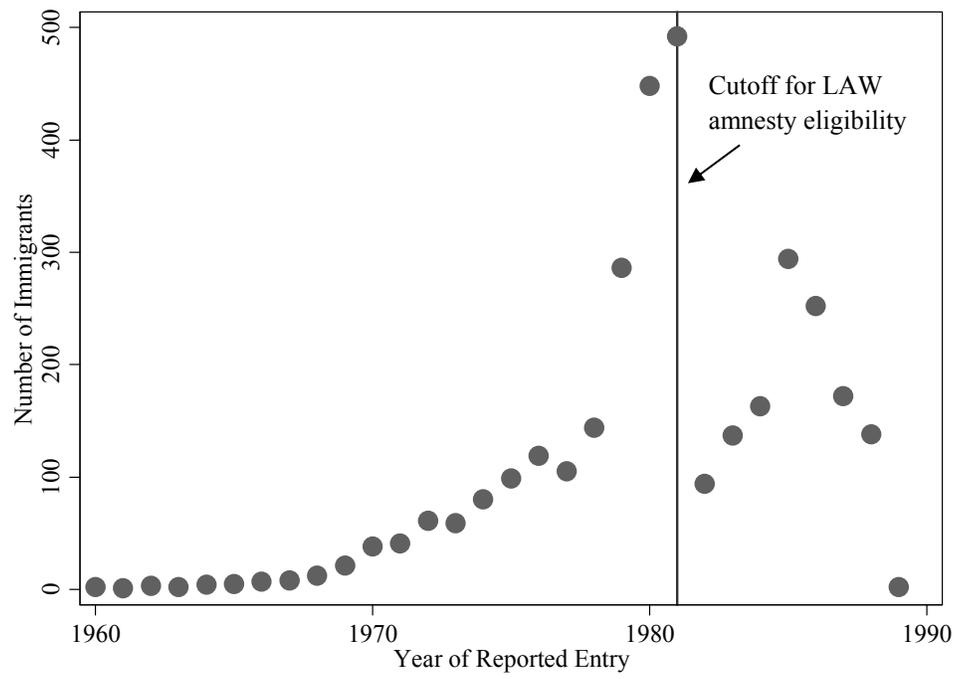


Figure 4: Share of Immigrants to Bexar County Arriving in Fourth Quarter, by Year of Entry, 1992 INS Legalization Summary Tape

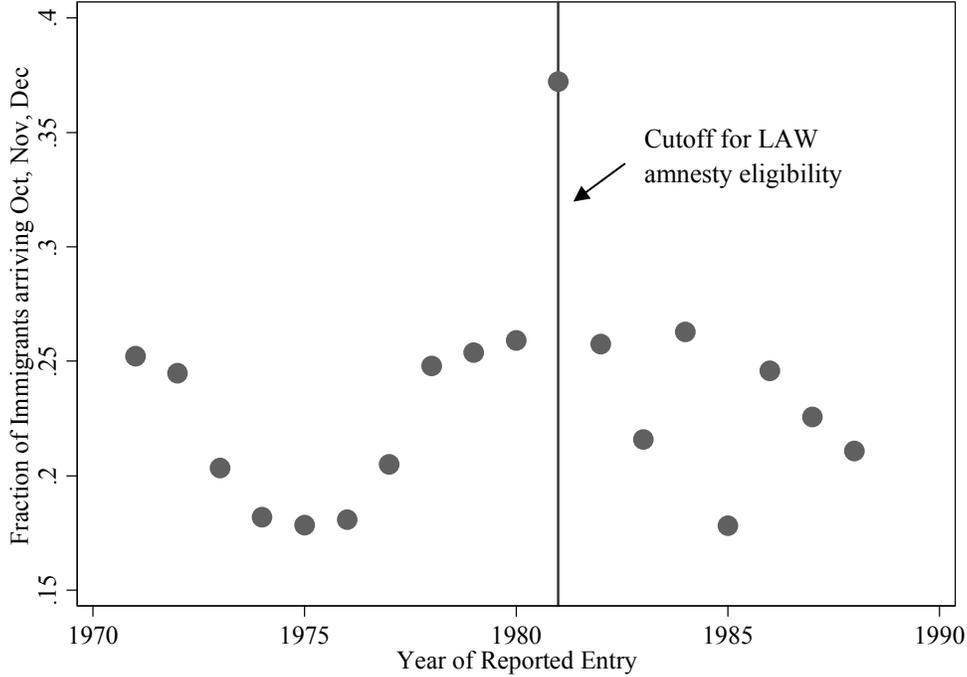


Figure 5: Average Monthly Neighborhood Criminal Incidence by Ethnicity and Crime Type

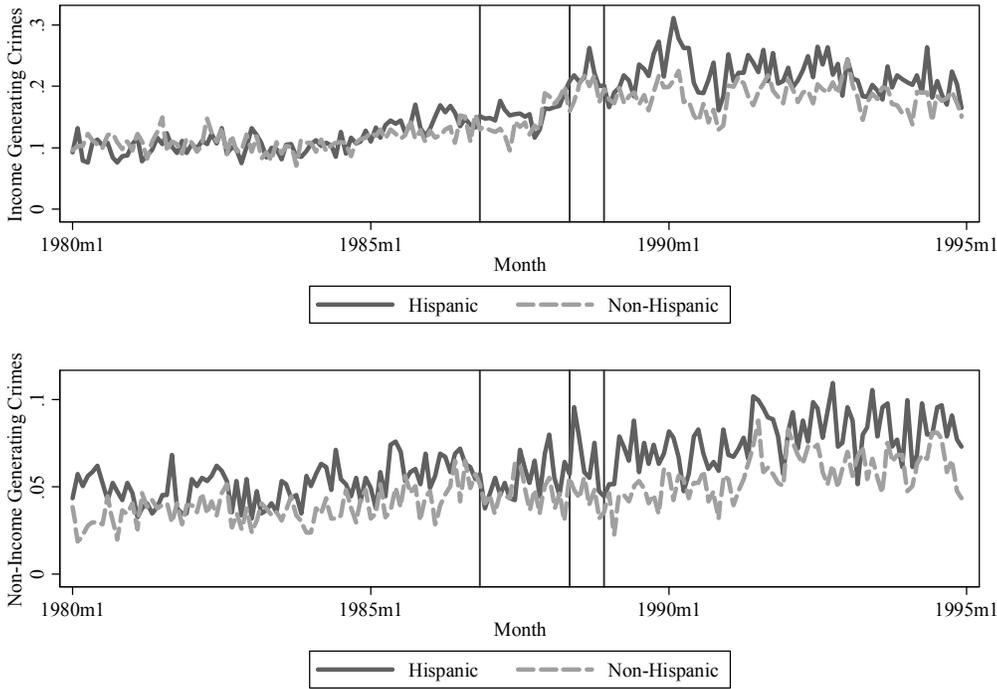
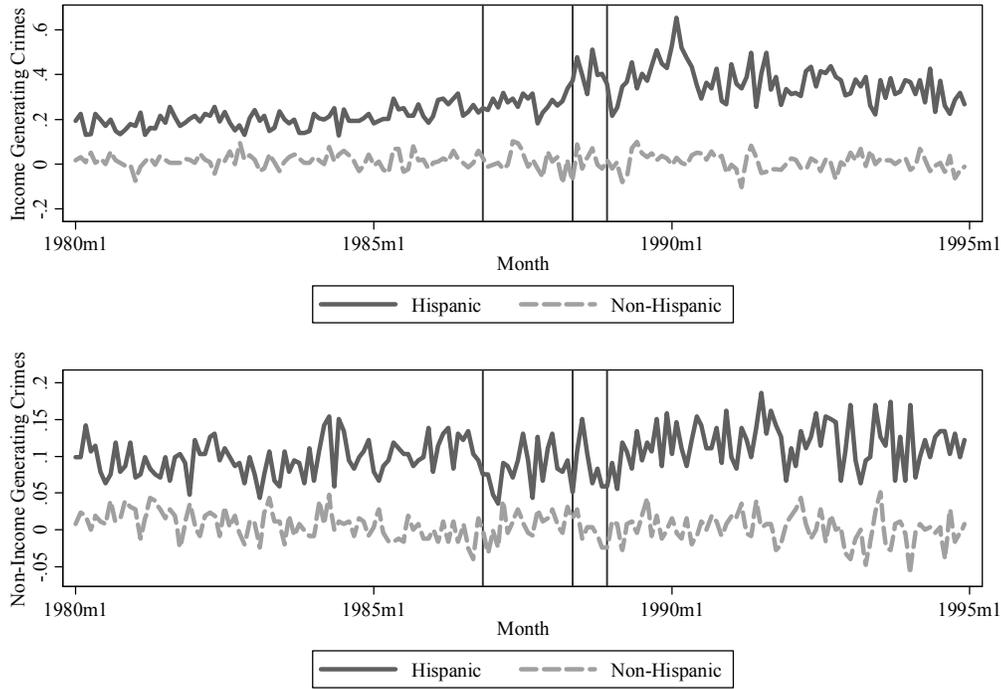
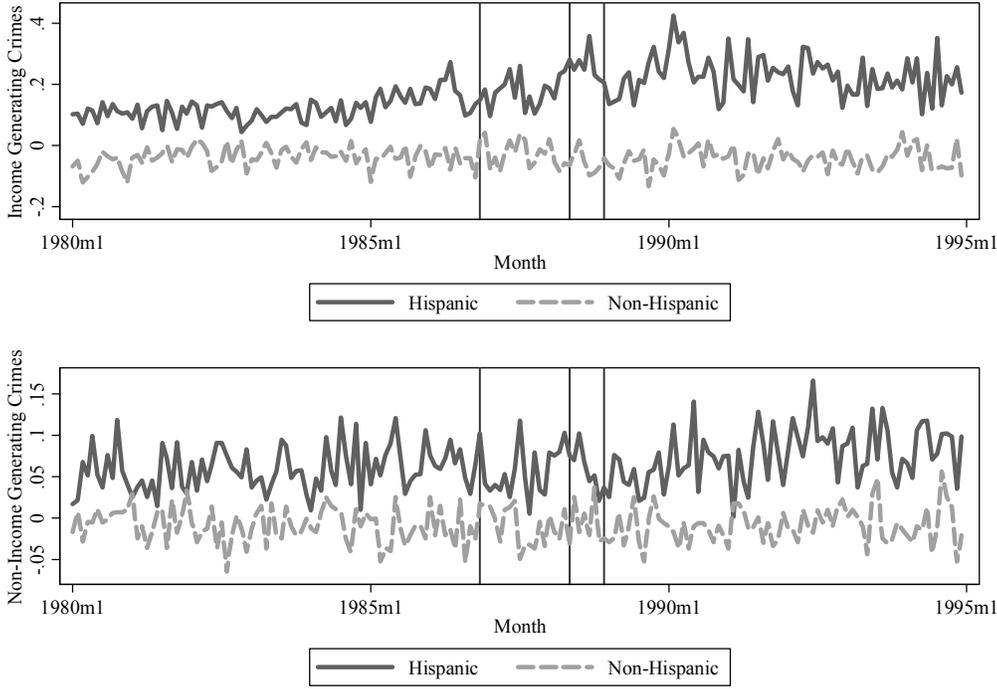


Figure 6: Difference in Criminal Incidence across High and Low Poverty Neighborhoods, by Ethnicity and Crime Type



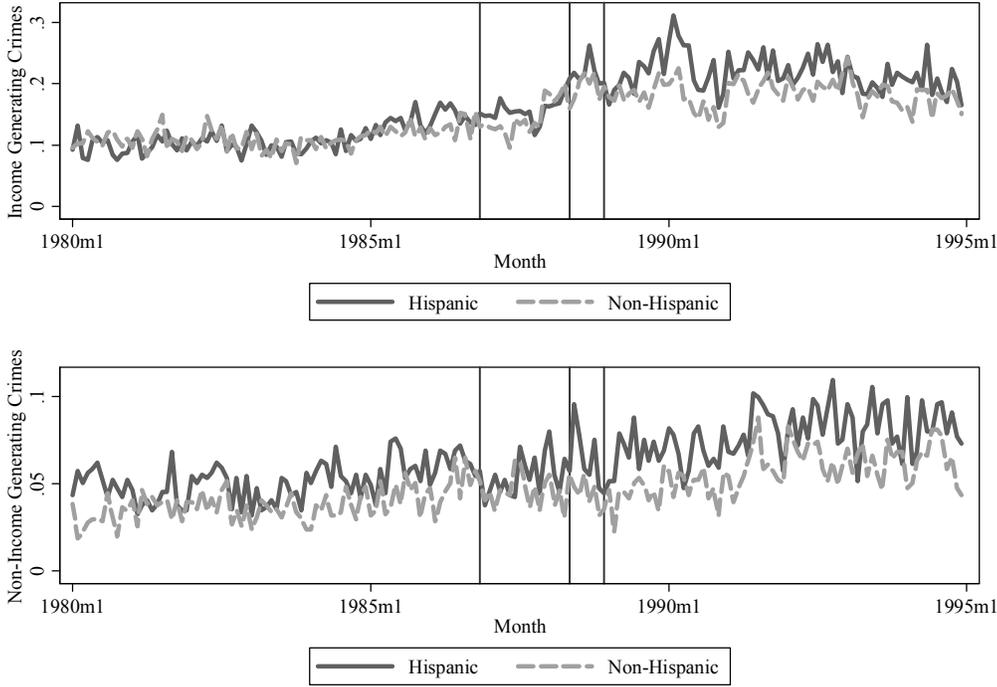
High and low poverty neighborhoods are block groups in the top quartile and bottom quartile of the poverty rate distribution in the 1990 Decennial Census.

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



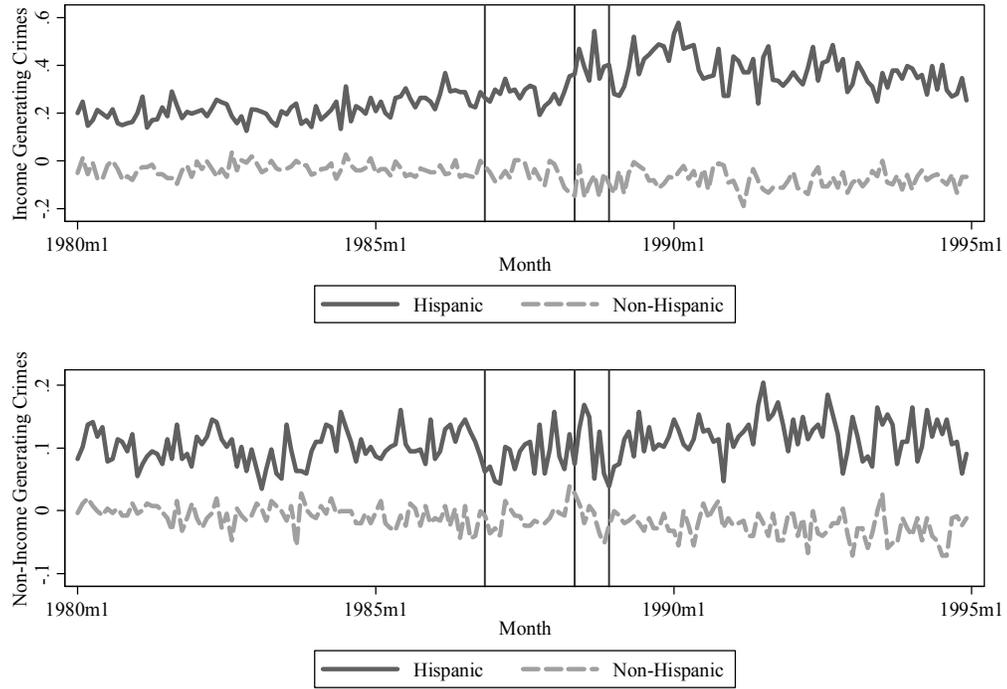
High and low residential density neighborhoods are block groups in the top quartile and bottom quartile of the residents per housing unit distribution in the 1990 Decennial Census.

Figure 8: Difference in Criminal Incidence across High and Low Percent Mexican Neighborhoods, by Ethnicity and Crime Type



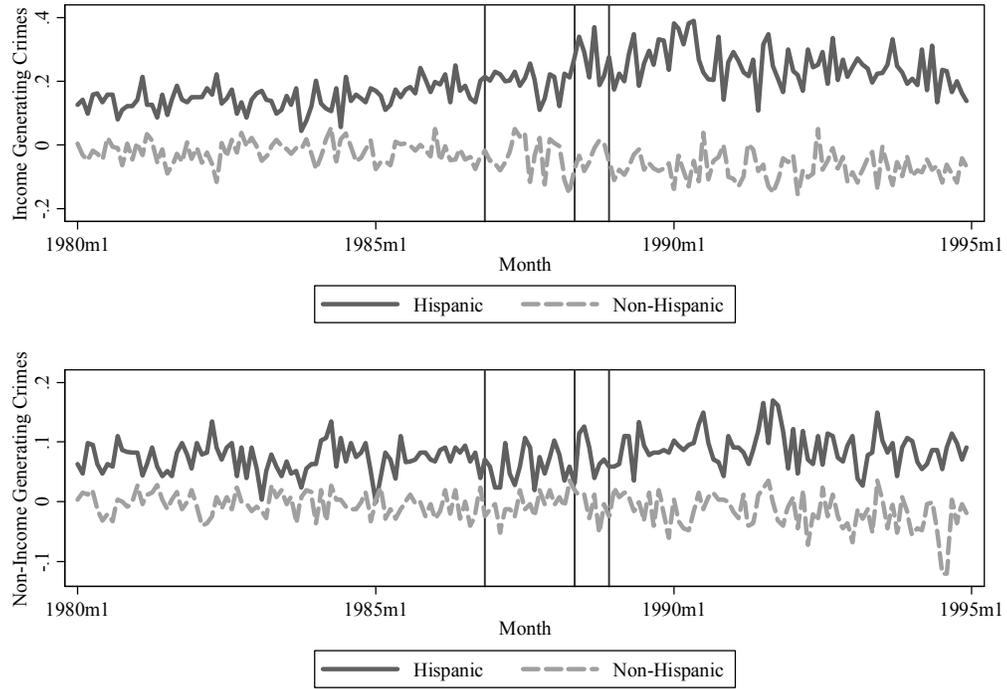
High and low percent Mexican neighborhoods are block groups in the top quartile and bottom quartile of the percent Mexican distribution in the 1990 Decennial Census.

Figure 9: Difference in Criminal Incidence across High and Low Spanish Speaking Neighborhoods, by Ethnicity and Crime Type



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.

Figure 10: Difference in Criminal Incidence across High and Low Immigrant Neighborhoods, by Ethnicity and Crime Type



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.

Figure 11: Monthly Sampled Arrests in Bexar County Compared to Annual UCR Totals

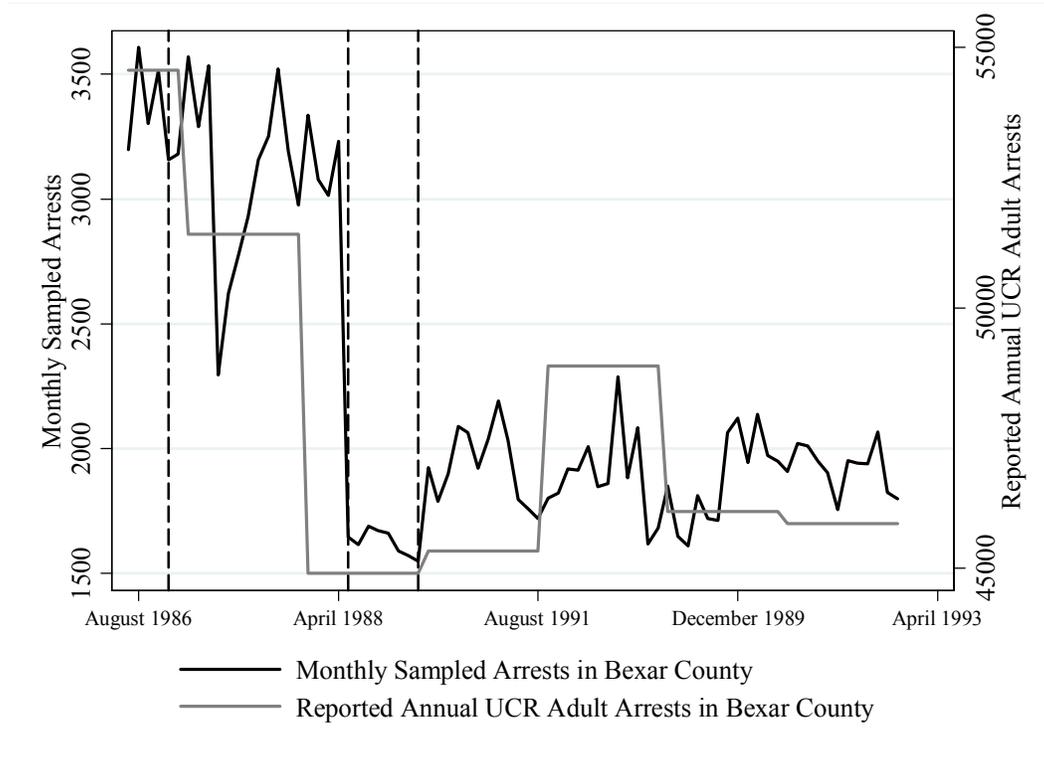


Figure 12: Misdemeanor Arrests of Hispanic and Non-Hispanic Individuals in Bexar County

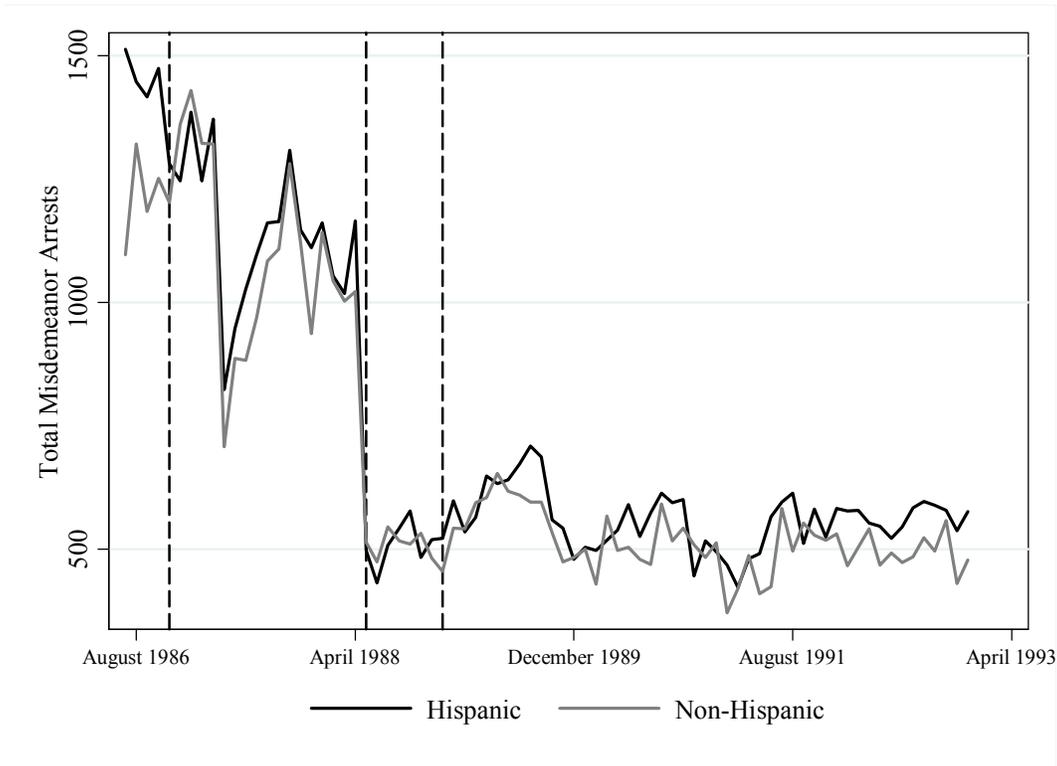
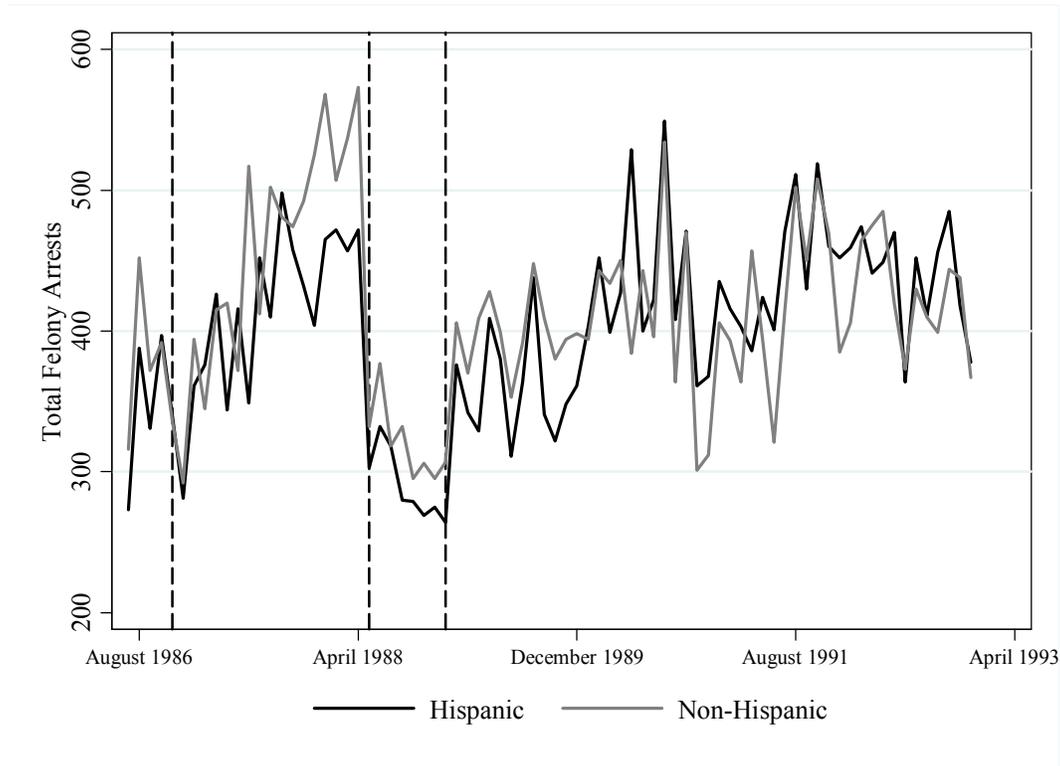


Figure 13: Felony Arrests of Hispanic and Non-Hispanic Individuals in Bexar County



Appendix

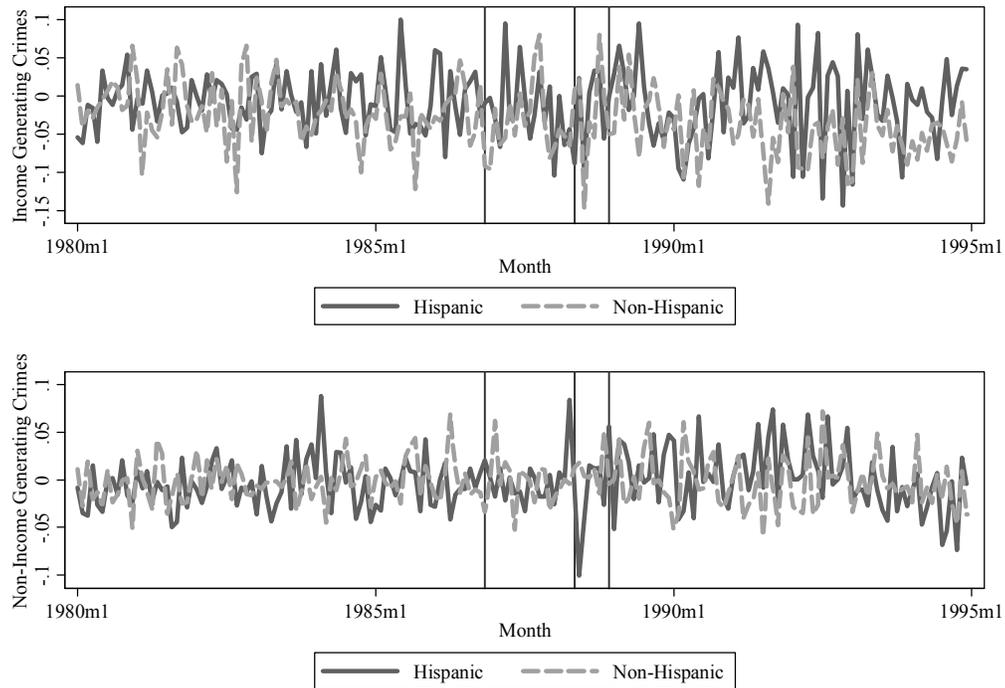
Table A1: First Order Impacts of IRCA on Hispanic Felonies, Linear Population Growth and Baseline Fixed Effects

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

See Tables 3 and 5 for notes. 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



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.