

# Immigration, Employment Opportunities, and Criminal Behavior<sup>†</sup>

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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 others, 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 at both the state and national levels. Surveys suggest that 89% of Americans currently believe that immigrants are hard workers and that 60% believe 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 about 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<sup>th</sup>, 1988. Agricultural workers who were not citizens could apply for amnesty through November 30<sup>th</sup> 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.<sup>1</sup> 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 January 1, 1980 and December 31, 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

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<sup>1</sup> 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>.

immigrants are most likely to initially locate.<sup>2</sup> While our preferred specification uses a conservative set of 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 a mechanical relationship between 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 perverse 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.

In addition to verifying that our findings are driven by Hispanics as opposed to other minority groups also affected by stricter drug policy enforcement, 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. To the extent that this reflects 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% percent of the increase in alleged felonies committed by Hispanic residents. Thus, while changes in relationship between Hispanics and the criminal justice system explain a small portion of the observed changes in felonies, it also appears that some recent immigrants may have been induced to substitute illegal activity for formal employment in face of new barriers to legal work.

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

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<sup>2</sup> Indeed, Bell and Machin (2013) point out that the historical concentration of co-ethnics and immigrants are frequently used as instruments for the location decisions of new immigrants in quasi-experimental research.

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 set out increased 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.<sup>3</sup>

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.<sup>4</sup>

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.

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<sup>3</sup> 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).

<sup>4</sup> Authors’ calculations from 1992 INS Legalization Summary Public Use Tape.

Taken together with the anecdotal evidence on the low cost of obtaining false documentation of residency and the high application approval rates, 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.<sup>5</sup>

### **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.<sup>6</sup>

While amnesty may have conferred economic gains to legalized immigrants, evidence suggests that IRCA's effects on unauthorized workers who failed to qualify for 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

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<sup>5</sup> Report filed by Dan Chiszer, United Press International, AM Cycle, May 4, 1988.

<sup>6</sup> Notably, some states immediately extended coverage of various benefit programs to immigrants legalized under IRCA. However, many federal programs, such as food stamps and Medicaid, were available to legalized immigrants only five years after legalization. Nonetheless, 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.<sup>7</sup>

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 2012).

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.<sup>8</sup> 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. Yet they could have plausibly acquired temporary resident status, which, with some additional paperwork, could open 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 the 18 month probationary period. Therefore, at least for a short period immediately after the amnesty, there was a stronger incentive among applicants to avoid

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<sup>7</sup> Another strand of research considers the impact of IRCA on competing legal workers, and some studies have found that especially 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).

<sup>8</sup> 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  $r'(k_t) > 0$ .<sup>9</sup> 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 activity 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.<sup>10</sup>

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.<sup>11</sup> 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

<sup>9</sup> 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  $s$ .

<sup>10</sup> 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.

<sup>11</sup> 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).<sup>12</sup>

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,

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<sup>12</sup> 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 (2012) 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 (2012), 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.<sup>13</sup> Using information on initially filed charges, we identified individuals who were accused of committing a crime that occurred between 1980 and 1994, a wide window around the date IRCA went into effect and the dates of its amnesty expirations. 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 times as many income generating

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

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 1,050 in 1990.

Table 1 presents descriptive statistics for our sample. San Antonio is a relatively middle-to-lower income city; on average, 15% of block group residents lived at or below the poverty line in 1990, and there was roughly one job for every two 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 U.S., and about 13% of immigrants entered after 1985. Non-citizens only constituted 6% of neighborhoods' populations on average in 1990.

## 5.2 Empirical 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.<sup>14</sup>

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):

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

where  $\text{Crime}_{bgt}$  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  $t$ .<sup>15</sup>

<sup>14</sup> 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>).

<sup>15</sup> We add 0.001 to the rate of criminal charges filed against residents so that the dependent variable is defined for all neighborhoods.

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. The typical solution to this problem is to linearly interpolate the block group population between decadal censuses, but this method would artificially smooth any differential shocks to the Hispanic and non-Hispanic populations in the middle of the 1980s.

We therefore construct a rough proxy for population that captures both geographic variation in the size of ethnic groups, as well as changes in the growth rate of each population over time, using on the number of babies born to Hispanic and non-Hispanic fathers in Bexar County each year. These data are derived from the National Center for Health Statistics' Vital Statistics Database. Patterns of births to Hispanic and non-Hispanic fathers appear in Figure 6; not only is there seasonal variation (which we address with month dummies), but it is also clear that births, and by implication the number of adult males, were not growing linearly during the decade, particularly for Hispanics.<sup>16</sup> We "assign" births to block groups based on the percentage of Hispanic (or non-Hispanic) Bexar County residents who lived in each block group in 1990. While the number of felonies committed by Hispanic people per 1,000 births to Hispanic fathers is not an intrinsically meaningful value, we will be (1) emphasizing crimes by members of an ethnic group that is not very prevalent in a block group, and (2) discounting any increases in crimes committed by members of an ethnic group that plausibly experienced a sudden jump in the population of young men.<sup>17</sup>

We allow for time invariant differences in criminal behavior across block groups ( $\alpha_b$ ) and ethnic groups ( $HISP_g$ ), and include a set of monthly fixed effects  $\gamma_t$  that allow for seasonality as well as long run trends in crime.<sup>18</sup> 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.<sup>19</sup> The estimated values of  $\theta_2$  and  $\theta_3$  therefore capture the extent of increases in criminal behavior by Hispanic residents after the end of IRCA amnesty that

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<sup>16</sup> Notably, the time trend in births to Hispanic fathers suggests that, to the extent that IRCA had an effect on the overall flow of immigrants to Bexar County, it was small and short-lived.

<sup>17</sup> When we do not scale by estimated population, our estimates of the differential change in Hispanic felonies are roughly half the size, but are estimated with equal precision. This is true for the natural log of crimes, the number of crimes, and a linear probability model for any crime occurring.

<sup>18</sup> The monthly fixed effects include 180 dummies, one for each month in each year in our sample ( $12 \times 15$ ). These subsume the IRCA enactment and amnesty date dummies.

<sup>19</sup> 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.

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.

If IRCA allowed current undocumented immigrants to gain temporary resident status, and the expiration of IRCA amnesty precluded new immigrants from 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.

While the estimates in equation (1) quantify the differential change in the criminal behavior of Hispanic people around IRCA, it does 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  $\theta_1$ ,  $\theta_2$ , and  $\theta_3$  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.

If the increase in felony behavior by Hispanic residents after IRCA amnesty is driven by gradual 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,<sup>20</sup> more people of Mexican descent, a higher fraction of adults who speak Spanish at home, and a higher fraction of foreign born residents.

In Figures 7-11, 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

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<sup>20</sup> 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).



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 7 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 7, 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 the crimes with a clear financial motive. This pattern is repeated when we look across high and low density neighborhoods in Figure 8, 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 9 and 10, 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 11 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  $\text{Demo}_b$  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.<sup>21</sup> In this triple-differences framework,  $\beta_1$ ,  $\beta_2$ , and  $\beta_3$  represent the differential increase in criminal behavior of Hispanic residents across different Bexar County

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<sup>21</sup> 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.

neighborhoods at each stage of IRCA.<sup>22</sup> 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).<sup>23</sup>

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 or not 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 to see roughly equivalent increases in Hispanic and non-Hispanic crimes in neighborhoods in proportion to the fraction of people in that neighborhood who are Hispanic or non-Hispanic. Additionally, we more explicitly test for changes in the behavior of the criminal justice system by examining conviction rates using the same analytic framework described

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<sup>22</sup> 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. The 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.

<sup>23</sup> Figures 10 and 11, where we compare crime across neighborhoods in the top and bottom quartiles of percent Spanish speaking and immigrant concentration, introduce a potential concern about our identification strategy. Specifically, 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 immigrants or Spanish speakers appear to potentially commit fewer income generating crimes relative to non-Hispanics in neighborhoods with fewer immigrants or Spanish speakers. 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 immigrant and Spanish-speaking 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 6.7.

above. As we discuss further in Section 6.9, differential changes in conviction rates across ethnic groups could point to a change in the policing or prosecution of Hispanics in the wake of IRCA.

## 6. Results

In Table 2, we present our first set of results for felony charges based on estimating equation (1). Consistent with Figure 1, we estimate that the expiration of the first IRCA amnesty (LAW) was associated with a 33% increase in the incidence of felony charges filed against Hispanic residents relative to non-Hispanics.<sup>24</sup> 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 1990 block group characteristics with block group fixed effects (second column). In the third and fourth columns, we focus only on income generating offenses, and estimate that there was a 24% 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 12% increase in charges filed against Hispanics after the expiration of LAW, which becomes insignificantly larger after the expiration of the SAW amnesty. Our 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.

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 will use neighborhood characteristics in the 1990 Decennial Census to attempt to estimate where new immigrants were more likely to have lived.

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<sup>24</sup> The percentage change in this case is calculated as  $(\exp(0.282)-1)\times 100$ .

### *6.1 Poverty Rates*

We start by examining charges filed against Hispanics and non-Hispanics in poorer neighborhoods in Table 3. It is clear that the impact of IRCA on Hispanic criminal behavior was not uniform across neighborhoods. Indeed, patterns of poverty help to explain the increase in overall charges filed against Hispanics in the wake of IRCA. After the expiration of amnesty, the increase in Hispanic felonies was a statistically significant 1.5 percentage points greater for each percentage point increase in the block group poverty rate. In the second column of Table 3, 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 rate in 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 poor neighborhoods of San Antonio. The introduction of all of these undefined variables increases our standard errors by about 30%, but the magnitudes of the observed effects are essentially unchanged and the coefficient on the interaction between Hispanic, LAW, and the neighborhood poverty rate remains statistically significant at the 10% level.

Hispanic residents in poor neighborhoods became slightly more likely to be accused of income generating felonies after IRCA was enacted, but the largest increase in crime was associated with the closing of amnesty offices in May of 1988. While our preferred specification assumes a constant difference in income-generating crime across neighborhoods, across ethnicities, and one common but arbitrary trend in income-generating crime across the county, relaxing these assumptions does not affect our points estimates at all. Further, the increase in statistical precision for these offenses relative to total crimes suggests that financially motivated crimes are the most sensitive to changing labor market opportunities, and that including other crimes in our outcome is simply introducing noise. Consistent with this, we find little evidence that poverty rates predict the geographic patterns of the differential increase in non-income generating felonies committed by Hispanic relative to non-Hispanic people, with or without a full set of fixed effects. To the extent that Hispanic people living in poorer neighborhoods were more likely to be ineligible for legal work after each successive law change, these results suggest that access to jobs is an important determinant of criminal behavior.

### *6.2 Residents per Housing Unit*

There is strong evidence in demography and population research that immigrants tend to live in more crowded housing than natives (Kriwo 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). We therefore use the number of people per housing unit as second non-ethnically based measure of plausible immigrant location decisions.

The results of this version of equation (2) are presented in Table 4. Consistent with our poverty rate regressions, we estimate a positive, but substantively small and statistically imprecise, increase in the number of crimes committed by Hispanic people after the enactment of IRCA, but only in neighborhoods where there were, on average, more people per housing unit. After the first expiration of amnesty in May 1988, the increase in Hispanic felonies was a statistically significant 20 percentage points greater for each additional person per housing unit (the mean is 2.73). We find no additional change after the agricultural amnesty expired. However, unlike with poverty, when we include our full set of fixed effects, we are no longer able to statistically distinguish the effect for LAW from zero.

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 third through sixth columns of Table 4, we show that this is the case. 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 over three times the size of the increase in non-income generating offenses. Overall, however, poverty rates appear to be a better proxy for the impact of IRCA on crime; the inclusion of our most comprehensive set of fixed effects reduces our ability to draw strong statistical conclusions from these results.

### *6.3 Percent of Population of Mexican Descent*

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). Our next method of identifying areas where more people are affected by IRCA is to use self-reported national origin. Of course, roughly half of San Antonians identified at least one Mexican ancestor in 1990. This group therefore includes both immigrants and U.S. citizens, and plausibly many high socio-economic status San Antonians who are unlikely to live near recent illegal immigrants. 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.

As can be seen in Table 5, as with patterns of poverty, the increased felony differential between Hispanics and non-Hispanics was larger in Mexican enclaves, an increase that is statistically significant even allowing for an extremely extensive set of fixed effects. After IRCA required employees to document their immigration status, the increase in Hispanic felonies was a half a percentage point greater

for each additional percentage point of the neighborhood population that was of Mexican descent. After immigrants were ineligible for temporary resident status, the Hispanic felony gap jumped by an additional percentage point for each percentage point increase in the percent of residents who identified themselves as of Mexican descent. This is driven by income generating crimes, although we do observe a small (0.3 percentage point) increase in non-income generating offenses after the SAW expiration. However, the statistical precision of this result is significantly dampened by a more extensive set of fixed effects.

#### *6.4 Percent of Population that Speaks Spanish at Home*

Since 48% of San Antonians are of Mexican descent, we try to 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.

As the results in Table 6 show, language spoken at home is a strong predictor of the increase in the Hispanic gap in income-generating crimes after amnesty. Meanwhile, similar to neighborhoods with a larger fraction of residents who are of Mexican descent, neighborhoods with a greater share of residents who speak Spanish at home do not have differential changes in the incidence of crimes that are less likely to be substitutes for work (i.e., non-income generating crimes).

#### *6.5 Percent of Population that Is Foreign Born*

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 predictors of settlement patterns (Dunlevy 1991, Buckley 1996, Zavodny 1999).

To the extent that this is true at the neighborhood level as well, the geographic patterns in where felons live suggest that new immigrants were much more likely to engage in income generating crime after immigration reform. As Table 7 shows, after the expiration of LAW amnesty, the increase in Hispanic felonies was 3 percentage points greater for each additional percentage point of the neighborhood population that was born outside of the U.S. Again, consistent with an economic model of crime, this is driven by offenses with a clear financial motive; the enactment of labor restrictions under IRCA was associated with an increase in economically motivated felonies among Hispanics of 2 percentage points for each percentage point increase in the fraction of the population that was foreign born. After the expiration of SAW amnesty, the cumulative increase in income generating felonies was over 5 percentage points for each additional percentage point of the local population that was born outside

the U.S. We find no statistically or economically meaningful differential increase in non-income generating offenses among Hispanic people living in neighborhoods where more people were born outside of the U.S.<sup>25</sup> Perhaps surprisingly given the strong predictive power of this variable in state level analyses of immigrants' location decisions, once we include our most comprehensive set of fixed effects, we cannot reject the null hypothesis that, at the neighborhood level, the observed post-amnesty increase in criminal behavior was not related to the concentration of other immigrants.

### *6.6 Multiple Proxies for Location Choice*

Finally, in Table 8, we combine all our plausible measures of location choice in regressions for each crime type. We also include a full set of interactions between other neighborhood characteristics, immigration reform, and ethnicity that could plausibly be correlated with crime or immigration patterns, but are not established determinants of the location decisions of new immigrants. These include the natural log of population in 1990, the fraction of adults working in agriculture, the percent of housing that is owner-occupied, and the percent of immigrants who report entering the U.S. after 1985.<sup>26</sup>

Because all our proxies for new immigrant locations are highly correlated with each other, no one factor emerges as statistically significant. Interesting exceptions are the fraction of the population that is of Mexican descent and the fraction of the population that speaks Spanish at home, which are moderately significant predictors of the change in Hispanic crime gap, but of roughly equal and opposite signs.

Since all our measures are proxies for an unobserved variable, the location choice of new immigrants, we report p-values for an F test of the joint significance of our triple-differences estimates in the bottom rows of Table 8. Conditional on block group fixed effects, monthly fixed effects, and other basic demographics, we can reject the null hypothesis that the enactment of IRCA and the expiration of LAW amnesty were unrelated to the relative criminal behavior of Hispanic San Antonians at the 90 and 99% confidence levels, respectively. As with the individual proxies, this is clearly driven by income generating offenses, where our proxies are significant at any standard level of confidence.<sup>27</sup> We find less evidence

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<sup>25</sup> In results not shown, we also find no statistically significant relationship between the fraction of new immigrants to the U.S. (those arriving after 1985) and the Hispanic crime gap. Our measure of the number of new immigrants is problematic, however, due to cell suppression of that variable by the Census Bureau to protect confidentiality.

<sup>26</sup> The percent of the population working in agriculture is, on its own and in this combined model, a positive predictor of the Hispanic crime gap after the expiration of SAW amnesty (see Appendix Table A1). 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 potentially an artifact of our identification strategy rather than a true impact of immigration reform.

<sup>27</sup> If we include ethnicity and block group-specific monthly fixed effects along with block group-specific level differences for Hispanics and non-Hispanics, the probabilities that our measures of immigrant location decisions are unrelated to income generating crime are 13% after IRCA's enactment, 6% after LAW expiration, and 84% after

that non-income generating criminal behavior changes for Hispanics relative to their non-Hispanic neighbors in immigrant destinations after IRCA. There is less than a 5% chance that Hispanics living in likely immigrant neighborhoods did not change their non-income generating felony behavior after IRCA was enacted; however, recall that the individual impact of each proxy in previous regressions was negative as often as it was positive for non-income generating offenses among Hispanics, so it is not clear that the direction of this relationship can be signed. In contrast, every individual proxy for immigrant location in the previous regressions was associated with an increase in income generating offenses among Hispanics.

### *6.7 Results by Ethnicity*

While our graphical analysis generally supported the validity of our difference-in-differences strategy, two proxies, specifically the percent of the neighborhood that speaks Spanish and the percent of the neighborhood that was born outside of the country, may have been negatively associated with the incidence of crime by non-Hispanics. If this apparent drop at the date of amnesty expiration was statistically significant and substantively meaningful, our previous estimates would overstate the impact of job opportunities on crime.

In Table 9, we address this directly by dividing our sample into Hispanic and non-Hispanic groups and separately estimating equation (2) for each new-immigrant destination proxy. For the sake of space, we present only the interaction terms of interest. Clearly, our triple-differences estimates are driven by a change in the behavior of Hispanic residents; the increase in crime across neighborhoods is positive and significant for Hispanics for all proxies, while we do not observe any statistically significant change in the behavior of non-Hispanic residents after any of the amnesty expiration dates. Meanwhile, while we do observe a decrease in most crime gradients for non-Hispanics, the substantive impact is quite small – never more than 20% of the impact on Hispanics, and almost always an order of magnitude smaller.

### *6.8 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 1990. Second, while burglary, robbery, and theft are certainly income generating offenses, involvement in drug selling shares even more characteristics with a

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SAW expiration. Impacts on total crimes are not statistically significant at conventional levels. The corresponding probabilities are 22%, 5%, and 69% if we restrict our sample to alleged felonies committed by men.



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 10, 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 1<sup>st</sup> 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.

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 (Kennedy 2011, U.S. Sentencing Commission 2009). While our finding that Hispanic drug offending differentially increase in new-immigrant destinations is robust to the inclusion of neighborhood and Hispanic-specific monthly fixed effects, it is still possible that our estimates are picking up some as of yet uncontrolled for change in the policing and prosecution of minorities. In the remaining columns of Table 10, 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, it is clear that Hispanic people became disproportionately more likely to be accused of felony drug offenses relative to other minority groups as IRCA closed off access to legal work, and this effect was concentrated in new-immigrant destinations.

### *6.9 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 upwards 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.

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.

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 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. 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 11. Based on the results in Table 3, after the expiration of LAW amnesty, the increase in income generating felony charges against Hispanics was 1.6 percentage points greater for each percentage point increase in the block group poverty rate. As the results in Table 11 show, at the same time that charges increased, there was a simultaneous half a percentage point reduction in the probability that those charges resulted in conviction, an effect that is marginally significant. We observe no other large or statistically significant changes in conviction rates at other points in time or for other crime types.

To give these changes some context, we provide a back of the envelope calculation of how much of the increase in felony charges can be attributed to a change in the criminal justice system. This is based on

a separate estimation of equation (2), where we recalculate the crime “rate” using only crimes that resulted in a conviction. There is a 6.3% smaller increase in the rate of successful income-generating felony charges filed against Hispanics than there is in overall income-generating felony charges after LAW, and so we conclude that roughly 6.3% of the changes observed in Table 3 were due to an increase probability of prosecution.<sup>28</sup>

We find similar imprecise estimates of the relationship between people per housing unit and conviction rates. As Table 12 shows, for income generating offenses, the standard errors are larger than the point estimates, and the sign of the change in conviction rates flips back and forth as IRCA and the subsequent amnesties roll out. The imprecision of these estimates is further evident in the back-of-the-envelope decompositions. When time invariant and geographically stable differences in felony and successful felony charges are not constrained to be equal, the signs of the estimated difference in charges brought versus charges convicted flips. For example, after LAW expired, we estimate a 1.2 percentage point relative increase in Hispanic conviction rates with each additional person per housing unit, but at the same time the (marginally significant) increase in overall felony charges is roughly 10% larger than the (marginally significant) increase in successful felony charges.

In Table 13, we turn to the concentration of neighborhood residents of Mexican descent, which was also associated with increased charges filed after IRCA and LAW for both all felony charges and income-generating ones. After IRCA’s enactment, it appears as if police and prosecutors were able to bring stronger cases against Hispanic residents charged with felonies in these neighborhoods; Hispanics were roughly one tenth of a percentage point more likely to be convicted after IRCA’s enactment for each additional percent of the block group’s population that was of Mexican descent. After the first expiration of amnesty, however, conviction rates fell by one-sixth of a percentage point per percent of residents that were of Mexican descent, a reduction that is statistically significant at the 5% level. To the extent that this decline in conviction rates reflects an increase in taste for prosecuting Hispanics, a change in the propensity to file felony charges can explain roughly 30% of each percentage point increase in felony charges per percent of residents that were of Mexican descent.

The small spatial variation in households speaking Spanish relative to percent Mexican appears to have important implications for differentiating individual versus criminal justice system behavior; while

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<sup>28</sup> For example, if we estimated that there was a 0.12 log point increase in felony charges filed per percentage in poverty, and a 0.11 log point increase in successful felony charges, we would attribute  $100 \times 0.01 / 0.12$  percent of the 0.12 log point change to the justice system. Note that these percentages are not bounded by zero or one, as the number of successful felony charges could increase by a larger percent than the percentage change in charges, implying that Hispanic residents are becoming easier to prosecute (explaining a negative percent of the change in felony charges), or the number of successful charges could actually fall in percentage terms, implying that the criminal justice system is becoming much more aggressive in its treatment of Hispanics (explaining more than 100% of the change in felony charges).

we observed roughly the same relationship between ancestral origin and primary language with respect to felony charges, we observe a much weaker relationship between language patterns and conviction rates. Indeed, as can be seen in Table 14, we can only explain 10% of the change in felony charges in Spanish-speaking neighborhoods as resulting from a change in criminal justice system behavior.

Consistent with demographic literature suggesting new immigrants tend to settle in areas with more immigrants, we see in Table 15 that after LAW amnesty expired, for each additional percent of neighborhood residents who were foreign born, felony charges filed against Hispanic neighbors increased by a statistically significant three percentage points and the fraction of charges resulting in a conviction fell by a statistically insignificant 0.4 percentage points. While imprecise, based on the magnitude of the change in successful prosecutions, a change in the criminal justice system can explain 24-30% of the observed change in felonies, which is a non-trivial amount.

When we combine all of the demographic interactions in the same regression (Table 16), no individual proxy is statistically significant on its own, as in the previous regressions. While we could 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 not affected. Overall, 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 40% 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 15% 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.<sup>29</sup>

## **7. Conclusion**

Immigrants have long been associated with lawlessness and criminality in the public mind. In spite of this perception, 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

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<sup>29</sup> We have recently obtained access to arrest records for law enforcement agencies in Bexar County from 1985 to 1992 and are in the process of assigning the location of each arrest to block groups. This new data set will allow us to test for a change in police activity in recent immigrant destinations.

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 allowed some undocumented immigrants 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, constitute 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 using 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. We find mixed evidence that the observed increase in felonies is driven by an increase in the probability that Hispanic people are charged with crimes. While there is a relatively consistent negative relationship between conviction rates and restrictions on access to legal work, many of the estimates are statistically imprecise. Further, comparing the change in alleged felonies to the change in felonies that result in a conviction suggests that at most one-third of the observed change in felony charges could be driven by a change in prosecutorial attitudes towards Hispanic defendants. Instead, we find 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
All Charges	360360	0.212	0.584
Income-Generating Charges	360360	0.157	0.488
Drug Charges	360360	0.055	0.280
Non-Income Generating Charges	360360	0.055	0.279
<u>Block Group Characteristics (1990)</u>			
Poverty Rate	1001	15.63	16.72
Percent Mexican Descent	1001	47.96	30.52
Percent Speaking Spanish at Home	1001	38.90	25.99
Percent Immigrant	1001	9.04	6.76
Concentration of New Immigrants*	1001	12.77	15.99
Percent Non-Citizens	1001	6.40	6.12
Ln(Population in 1990)	1001	6.91	0.63
People per Housing Unit	1001	2.73	0.12
Percent Working in Agriculture	1001	0.83	3.37
Jobs per Adult	1001	59.19	13.39
Percent of Housing Stock in Rental Market	1001	38.39	24.36

\* Percent of immigrants who moved to the U.S. after 1985.

Table 2: IRCA and Felony Charges

	All Crimes		Income Generating		Non-Income Generating	
Hispanic Defendant	0.101*	0.101*	0.00831	0.00831	0.118***	0.118***
	[0.0501]	[0.0502]	[0.0400]	[0.0401]	[0.0195]	[0.0195]
Hispanic x IRCA	0.0106	0.0106	0.0533	0.0533	-0.0371	-0.0371
	[0.0509]	[0.0510]	[0.0446]	[0.0446]	[0.0280]	[0.0281]
Hispanic x LAW	0.282**	0.282**	0.216*	0.216*	0.110*	0.110*
Expiration	[0.0922]	[0.0923]	[0.0864]	[0.0865]	[0.0485]	[0.0486]
Hispanic x SAW	0.0486	0.0486	0.0156	0.0156	0.0194	0.0194
Expiration	[0.0832]	[0.0833]	[0.0787]	[0.0788]	[0.0445]	[0.0446]
Demographic Controls	Y		Y		Y	
Block Group FE		Y		Y		Y
R <sup>2</sup>	0.040	0.084	0.032	0.071	0.014	0.030
N	360360	360360	360360	360360	360360	360360

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, Interactions with Neighborhood Poverty Rates

	All Crimes		Income Generating		Non-Income Generating	
Hispanic x IRCA x Poverty Rate	0.00523 [0.00341]	0.00523 [0.00483]	0.00925** [0.00315]	0.00925* [0.00447]	-0.00337+ [0.00195]	-0.00337 [0.00276]
Hispanic x LAW x Poverty Rate	0.0150* [0.00587]	0.0150+ [0.00831]	0.0159** [0.00565]	0.0159* [0.00800]	0.00385 [0.00321]	0.00385 [0.00454]
Hispanic x SAW x Poverty Rate	0.0000324 [0.00514]	3.24E-05 [0.00728]	-0.00377 [0.00508]	-0.00377 [0.00719]	0.00381 [0.00272]	0.00381 [0.00385]
Hispanic Defendant	-0.714*** [0.0500]		-0.602*** [0.0417]		-0.172*** [0.0198]	
Hispanic x IRCA	-0.0711 [0.0625]		-0.0912+ [0.0551]		0.0155 [0.0337]	
Hispanic x LAW Expiration	0.048 [0.112]		-0.0323 [0.103]		0.0496 [0.0578]	
Hispanic x SAW Expiration	0.0481 [0.102]		0.0746 [0.0948]		-0.0401 [0.0542]	
Hispanic Defendant x Poverty Rate	0.0522*** [0.00270]		0.0390*** [0.00216]		0.0186*** [0.00119]	
IRCA x Poverty Rate	0.000489 [0.00238]		0.000916 [0.00212]		-0.00032 [0.00130]	
LAW x Poverty Rate	-0.00027 [0.00364]		0.00171 [0.00344]		-0.00285 [0.00192]	
SAW x Poverty Rate	-0.00131 [0.00343]		-0.00223 [0.00318]		0.000907 [0.00157]	
Neighborhood x Hispanic		Y		Y		Y
Neighborhood x Month		Y		Y		Y
Hispanic x Month		Y		Y		Y
R <sup>2</sup>	0.099	0.565	0.083	0.555	0.035	0.525
N	360360		360360		360360	

Each regression includes 180 month dummies and block group fixed effects. 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, Interactions with Neighborhood Residents per Housing Unit

	All Crimes		Income Generating		Non-Income Generating	
Hispanic x IRCA x People / Housing Units	0.0349 [0.0502]	0.0349 [0.0711]	0.0732 [0.0449]	0.0732 [0.0636]	-0.0365 [0.0309]	-0.0365 [0.0438]
Hispanic x LAW x People / Housing Units	0.218+ [0.115]	0.218 [0.163]	0.246* [0.109]	0.246 [0.155]	0.0705 [0.0700]	0.0705 [0.0992]
Hispanic x SAW x People / Housing Units	-0.0646 [0.0829]	-0.0646 [0.117]	-0.110 [0.0749]	-0.110 [0.106]	0.00571 [0.0570]	0.00571 [0.0807]
Hispanic Defendant	-1.453*** [0.423]		-1.180*** [0.319]		-0.410** [0.153]	
Hispanic x IRCA	-0.0845 [0.140]		-0.146 [0.124]		0.0622 [0.0849]	
Hispanic x LAW Expiration	-0.311 [0.317]		-0.453 [0.302]		-0.0821 [0.189]	
Hispanic x SAW Expiration	0.224 [0.233]		0.314 [0.211]		0.00388 [0.159]	
Hispanic Defendant x People / Housing Units	0.571*** [0.159]		0.437*** [0.120]		0.194*** [0.0575]	
IRCA x People / Housing Units	0.0436 [0.0454]		0.0398 [0.0405]		0.00669 [0.0190]	
LAW x People / Housing Units	-0.0177 [0.0619]		-0.0309 [0.0583]		0.00432 [0.0332]	
SAW x People / Housing Units	-0.0318 [0.0584]		-0.0104 [0.0523]		-0.0268 [0.0306]	
Neighborhood x Hispanic		Y		Y		Y
Neighborhood x Time		Y		Y		Y
Hispanic x Month		Y		Y		Y
R <sup>2</sup>	0.089	0.565	0.075	0.554	0.031	0.524
N		360360		360360		360360

Each regression includes 180 month dummies and block group fixed effects. 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 Charges, Interactions with Neighborhood Percent Mexican

	All Crimes		Income Generating		Non-Income Generating	
Hispanic x IRCA x Percent Mexican	0.00480** [0.00171]	0.00480* [0.00242]	0.0061*** [0.00148]	0.00609** [0.00209]	-0.000249 [0.000976]	-0.00025 [0.00138]
Hispanic x LAW x Percent Mexican	0.0110*** [0.00307]	0.0110* [0.00435]	0.0123*** [0.00284]	0.0123** [0.00402]	0.00104 [0.00166]	0.00104 [0.00235]
Hispanic x SAW x Percent Mexican	0.0000142 [0.00281]	0.0000142 [0.00398]	-0.00271 [0.00269]	-0.00271 [0.00380]	0.00300* [0.00143]	0.00300 [0.00203]
Hispanic Defendant	-1.562*** [0.0718]		-1.246*** [0.0610]		-0.455*** [0.0276]	
Hispanic x IRCA	-0.220** [0.0830]		-0.239*** [0.0712]		-0.0252 [0.0457]	
Hispanic x LAW expiration	-0.244 [0.152]		-0.376** [0.137]		0.0599 [0.0772]	
Hispanic x SAW expiration	0.0479 [0.140]		0.145 [0.129]		-0.125+ [0.0729]	
Hispanic Defendant x Percent Mexican	0.0347*** [0.00128]		0.0261*** [0.00106]		0.0120*** [0.000559]	
IRCA x Percent Mexican	-0.000129 [0.00120]		-0.0000405 [0.00107]		-0.000521 [0.000601]	
LAW x Percent Mexican	-0.00293 [0.00200]		-0.00257 [0.00182]		-0.000551 [0.00102]	
SAW x Percent Mexican	0.000217 [0.00182]		0.000582 [0.00166]		-0.00087 [0.000917]	
Neighborhood x Hispanic		Y		Y		Y
Neighborhood x Time		Y		Y		Y
Hispanic x Time		Y		Y		Y
R <sup>2</sup>	0.107	0.566	0.090	0.556	0.037	0.525
N	360360		360360		360360	

Each regression includes 180 month dummies and block group fixed effects. 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, Interactions with Neighborhood Percent that Speaks Spanish at Home

	All Crimes		Income Generating		Non-Income Generating	
Hispanic x IRCA x Percent Spanish at Home	0.00539**	0.00539+	0.00734***	0.00734**	-0.00062	-0.00062
	[0.00202]	[0.00286]	[0.00176]	[0.00249]	[0.00116]	[0.00164]
Hispanic x LAW x Percent Spanish at Home	0.0138***	0.0138**	0.0150***	0.0150**	0.00187	0.00187
	[0.00366]	[0.00519]	[0.00339]	[0.00480]	[0.00201]	[0.00285]
Hispanic x SAW x Percent Spanish at Home	-0.00158	-0.00158	-0.00447	-0.00447	0.00273	0.00273
	[0.00337]	[0.00477]	[0.00321]	[0.00455]	[0.00171]	[0.00243]
Hispanic Defendant	-1.509***		-1.206***		-0.435***	
	[0.0675]		[0.0573]		[0.0263]	
Hispanic x IRCA	-0.199*		-0.232***		-0.0131	
	[0.0809]		[0.0696]		[0.0445]	
Hispanic x LAW Expiration	-0.255+		-0.368**		0.0368	
	[0.148]		[0.134]		[0.0769]	
Hispanic x SAW Expiration	0.11		0.19		-0.0869	
	[0.138]		[0.126]		[0.0724]	
Hispanic Defendant x Percent Spanish at Home	0.0414***		0.0312***		0.0142***	
	[0.00148]		[0.00122]		[0.000666]	
IRCA x Percent Spanish at Home	-0.00042		-0.00018		-0.0008	
	[0.00139]		[0.00125]		[0.000706]	
LAW x Percent Spanish at Home	-0.00404+		-0.00343		-0.00092	
	[0.00233]		[0.00212]		[0.00120]	
SAW x Percent Spanish at Home	0.00104		0.00112		-0.00051	
	[0.00215]		[0.00193]		[0.00107]	
Neighborhood x Hispanic		Y		Y		Y
Neighborhood x Time		Y		Y		Y
Hispanic x Month		Y		Y		Y
R <sup>2</sup>	0.108	0.565	0.090	0.555	0.037	0.524
N	360360		360360		360360	

Each regression includes 180 month dummies and block group fixed effects. 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 Charges, Interactions with Neighborhood Percent Foreign Born

	All Crimes		Income Generating		Non-Income Generating	
Hispanic x IRCA x Percent Immigrant	0.00882 [0.00753]	0.00882 [0.0107]	0.0190** [0.00721]	0.0190+ [0.0102]	-0.00873+ [0.00474]	-0.00873 [0.00671]
Hispanic x LAW x Percent Immigrant	0.0306* [0.0149]	0.0306 [0.0211]	0.0313* [0.0149]	0.0313 [0.0212]	0.00842 [0.00791]	0.00842 [0.0112]
Hispanic x SAW x Percent Immigrant	-0.00148 [0.0132]	-0.00148 [0.0187]	-0.0103 [0.0132]	-0.0103 [0.0187]	0.00882 [0.00688]	0.00882 [0.00974]
Hispanic Defendant	-0.738*** [0.0718]		-0.604*** [0.0595]		-0.194*** [0.0284]	
Hispanic x IRCA	-0.0693 [0.0793]		-0.118 [0.0722]		0.0419 [0.0469]	
Hispanic x LAW Expiration	0.0052 [0.154]		-0.067 [0.147]		0.0335 [0.0780]	
Hispanic x SAW Expiration	0.0619 [0.138]		0.109 [0.132]		-0.0604 [0.0713]	
Hispanic x Percent Immigrant	0.0928*** [0.00703]		0.0677*** [0.00566]		0.0346*** [0.00296]	
IRCA x Percent Immigrant	-0.00827 [0.00568]		-0.00724 [0.00515]		-0.00254 [0.00339]	
LAW x Percent Immigrant	-0.00768 [0.00870]		-0.00525 [0.00823]		-0.00476 [0.00478]	
SAW x Percent Immigrant	-0.00069 [0.00785]		-0.00089 [0.00728]		0.000338 [0.00398]	
Neighborhood x Hispanic	Y		Y		Y	
Neighborhood x Time	Y		Y		Y	
Hispanic x Month	Y		Y		Y	
R <sup>2</sup>	0.047	0.565	0.077	0.555	0.033	0.525
N	360360		360360		360360	

Each regression includes 180 month dummies and block group fixed effects. 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 Felony Charges, Multiple Proxies for Location Choice

	All Crimes	Income Generating	Non-Income Generating
Hispanic x IRCA x Poverty Rate	-0.00521 [0.00582]	-0.00055 [0.00556]	-0.00660* [0.00330]
Hispanic x LAW x Poverty Rate	-0.00138 [0.00973]	-0.00344 [0.00933]	0.00427 [0.00589]
Hispanic x SAW x Poverty Rate	0.00126 [0.00887]	0.000994 [0.00856]	5.98E-05 [0.00521]
Hispanic x IRCA x People / Housing Units	0.0187 [0.0524]	0.0352 [0.0420]	-0.0375 [0.0370]
Hispanic x LAW x People / Housing Units	0.0358 [0.123]	0.0527 [0.110]	0.0629 [0.0817]
Hispanic x SAW x People / Housing Units	-0.0616 [0.0911]	-0.0745 [0.0794]	-0.0249 [0.0635]
Hispanic x IRCA x Percent Mexican	0.0084 [0.00726]	0.00261 [0.00650]	0.00478 [0.00371]
Hispanic x LAW x Percent Mexican	-0.00983 [0.0132]	-0.00343 [0.0121]	-0.00956 [0.00675]
Hispanic x SAW x Percent Mexican	0.0198+ [0.0115]	0.014 [0.0107]	0.0135* [0.00626]
Hispanic x IRCA x Percent Spanish at Home	-0.00127 [0.00916]	0.00362 [0.00811]	-0.00078 [0.00488]
Hispanic x LAW x Percent Spanish at Home	0.0258 [0.0171]	0.0206 [0.0157]	0.00946 [0.00897]
Hispanic x SAW x Percent Spanish at Home	-0.0245+ [0.0149]	-0.0196 [0.0137]	-0.0136+ [0.00818]
Hispanic x IRCA x Percent Immigrant	-0.00435 [0.00992]	0.00299 [0.00960]	-0.00973 [0.00614]
Hispanic x LAW x Percent Immigrant	-0.00144 [0.0205]	-0.00115 [0.0206]	0.00282 [0.0101]
Hispanic x SAW x Percent Immigrant	0.00571 [0.0184]	-0.00087 [0.0184]	0.00781 [0.00892]
R <sup>2</sup>	0.11	0.092	0.038
N	360360	360360	360360
<i>p(Enactment)=0</i>	0.0741	0.0043	0.0305
<i>p(exLAW)=0</i>	0.0129	0.0008	0.5497
<i>p(exSAW)=0</i>	0.6104	0.5280	0.1439

Each regression includes 180 month dummies and block group fixed effects, as well as full Hispanic, demographic, and immigration reform interactions. 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 Felony Charges, by Ethnicity and Crime Type

	All Crimes		Income Generating		Non-Income Generating	
	Hispanic	Non-Hispanic	Hispanic	Non-Hispanic	Hispanic	Non-Hispanic
IRCA x Poverty Rate	<b>0.00572*</b> [0.00244]	0.000489 [0.00238]	<b>0.0102***</b> [0.00234]	0.000916 [0.00212]	<b>-0.00369*</b> [0.00146]	-0.00032 [0.00130]
LAW x Poverty Rate	<b>0.0147**</b> [0.00452]	-0.00027 [0.00365]	<b>0.0176***</b> [0.00434]	0.00171 [0.00344]	<b>0.000996</b> [0.00256]	-0.00285 [0.00192]
SAW x Poverty Rate	<b>-0.00128</b> [0.00385]	-0.00131 [0.00343]	<b>-0.00601</b> [0.00387]	-0.00223 [0.00319]	<b>0.00472*</b> [0.00229]	0.000907 [0.00157]
IRCA x People / Housing Units	<b>0.0786+</b> [0.0413]	0.0436 [0.0455]	<b>0.113*</b> [0.0460]	0.0398 [0.0406]	<b>-0.0298</b> [0.0222]	0.00669 [0.0190]
LAW x People / Housing Units	<b>0.200*</b> [0.0989]	-0.0177 [0.0620]	<b>0.215*</b> [0.0908]	-0.0309 [0.0584]	<b>0.0748</b> [0.0598]	0.00432 [0.0332]
SAW x People / Housing Units	<b>-0.0964</b> [0.0712]	-0.0318 [0.0585]	<b>-0.120*</b> [0.0602]	-0.0104 [0.0524]	<b>-0.0211</b> [0.0490]	-0.0268 [0.0307]
IRCA x Percent Mexican	<b>0.00467***</b> [0.00126]	-0.00013 [0.00120]	<b>0.00605***</b> [0.00112]	-4.1E-05 [0.00107]	<b>-0.00077</b> [0.000764]	-0.00052 [0.000602]
LAW x Percent Mexican	<b>0.00804***</b> [0.00233]	-0.00293 [0.00200]	<b>0.00977***</b> [0.00218]	-0.00257 [0.00183]	<b>0.000486</b> [0.00129]	-0.00055 [0.00102]
SAW x Percent Mexican	<b>0.000231</b> [0.00208]	0.000217 [0.00182]	<b>-0.00212</b> [0.00199]	0.000582 [0.00166]	<b>0.00213+</b> [0.00114]	-0.00087 [0.000918]
IRCA x Percent Spanish at Home	<b>0.00497**</b> [0.00155]	-0.00042 [0.00139]	<b>0.00715***</b> [0.00138]	-0.00018 [0.00126]	<b>-0.00142</b> [0.000915]	-0.0008 [0.000707]
LAW x Percent Spanish at Home	<b>0.00976***</b> [0.00283]	-0.00404+ [0.00233]	<b>0.0116***</b> [0.00266]	-0.00343 [0.00212]	<b>0.000949</b> [0.00157]	-0.00092 [0.00120]
SAW x Percent Spanish at Home	<b>-0.00054</b> [0.00251]	0.00104 [0.00215]	<b>-0.00335</b> [0.00243]	0.00112 [0.00193]	<b>0.00222</b> [0.00139]	-0.00051 [0.00107]
IRCA x Percent Immigrant	<b>0.000556</b> [0.00622]	-0.00827 [0.00570]	<b>0.0117*</b> [0.00585]	-0.00724 [0.00516]	<b>-0.0113***</b> [0.00340]	-0.00254 [0.00339]
LAW x Percent Immigrant	<b>0.0229+</b> [0.0120]	-0.00768 [0.00873]	<b>0.0260*</b> [0.0119]	-0.00525 [0.00824]	<b>0.00366</b> [0.00606]	-0.00476 [0.00479]
SAW x Percent Immigrant	<b>-0.00217</b> [0.0104]	-0.00069 [0.00787]	<b>-0.0112</b> [0.0103]	-0.00089 [0.00729]	<b>0.00916</b> [0.00590]	0.000338 [0.00399]

Each panel reports coefficients from a set of regressions of criminal behavior on one neighborhood characteristic. Each regression contains 180,180 observations and includes 180 month dummies and block group fixed effects. 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 10: IRCA and Felony Drug Charges

	All Felony Drug Charges		Non-Hispanic Whites Excluded	
IRCA x Poverty Rate	0.0111*** [0.00189]	0.0111*** [0.00268]	0.0105*** [0.00176]	0.0105*** [0.00249]
LAW x Poverty Rate	0.0213*** [0.00395]	0.0213*** [0.00560]	0.0190*** [0.00376]	0.0190*** [0.00533]
SAW x Poverty Rate	-0.0115** [0.00376]	-0.0115* [0.00533]	-0.00940** [0.00364]	-0.00940+ [0.00516]
IRCA x People / Housing Units	0.0982* [0.0400]	0.0982+ [0.0566]	0.102** [0.0377]	0.102+ [0.0534]
LAW x People / Housing Units	0.270** [0.0840]	0.270* [0.119]	0.200** [0.0688]	0.200* [0.0974]
SAW x People / Housing Units	-0.147* [0.0589]	-0.147+ [0.0835]	-0.0985+ [0.0515]	-0.0985 [0.0729]
IRCA x Percent Mexican	0.00670*** [0.000968]	0.00670*** [0.00137]	0.00649*** [0.000868]	0.00649*** [0.00123]
LAW x Percent Mexican	0.0142*** [0.00209]	0.0142*** [0.00296]	0.0122*** [0.00199]	0.0122*** [0.00282]
SAW x Percent Mexican	-0.00542** [0.00188]	-0.00542* [0.00266]	-0.00382* [0.00179]	-0.00382 [0.00254]
IRCA x Percent Spanish at Home	0.00804*** [0.00115]	0.00804*** [0.00163]	0.00768*** [0.00105]	0.00768*** [0.00149]
LAW x Percent Spanish at Home	0.0177*** [0.00249]	0.0177*** [0.00353]	0.0153*** [0.00239]	0.0153*** [0.00339]
SAW x Percent Spanish at Home	-0.00759*** [0.00228]	-0.00759* [0.00323]	-0.00572** [0.00219]	-0.00572+ [0.00311]
IRCA x Percent Immigrant	0.0144** [0.00437]	0.0144* [0.00619]	0.0117** [0.00408]	0.0117* [0.00577]
LAW x Percent Immigrant	0.0482*** [0.0103]	0.0482*** [0.0146]	0.0438*** [0.00970]	0.0438** [0.0137]
SAW x Percent Immigrant	-0.0224* [0.00940]	-0.0224+ [0.0133]	-0.0180* [0.00893]	-0.018 [0.0126]
Neighborhood x Hispanic		Y		Y
Neighborhood x Month		Y		Y
Hispanic x Month		Y		Y
$p(Enactment)=0$	0.000	0.000	0.000	0.000
$p(exLAW)=0$	0.000	0.000	0.000	0.0001
$p(exSAW)=0$	0.006	0.1475	0.003	0.3005

Each panel reports coefficients from a set of regressions of criminal behavior on one neighborhood characteristic. Each regression contains 360,360 observations and includes 180 month dummies and block group fixed effects. Standard errors in brackets allow for arbitrary correlation in crime measure within block group. P values reflect the probability that all interactions are simultaneously equal to zero, in a regression that also includes controls for the log 1990 block group 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, all fully interacted the immigration reform dummies. Significant at the + 10% level, \* 5% level, \* 1% level, and \*\*\* 0.1% level.

Table 11: IRCA and Convictions per Charge Filed, Interactions with Neighborhood Poverty Rates

	All Crimes	Income Generating	Non-Income Generating
Hispanic x IRCA x Poverty Rate	0.0635 [0.0705]	0.174 [0.174]	-0.0105 [0.0847]
Hispanic x LAW x Poverty Rate	-0.148 [0.122]	-0.477+ [0.286]	0.0347 [0.139]
Hispanic x SAW x Poverty Rate	0.083 [0.109]	0.219 [0.256]	-0.023 [0.113]
Hispanic Defendant	3.433** [1.136]	1.351 [2.216]	4.089** [1.294]
Hispanic x IRCA	-2.42 [2.050]	-4.95 [4.568]	-1.065 [2.386]
Hispanic x LAW expiration	2.547 [3.332]	18.59** [6.802]	-3.717 [3.724]
Hispanic x SAW expiration	-2.325 [2.893]	-12.17* [5.974]	1.813 [3.162]
Hispanic Defendant x Poverty Rate	0.0042 [0.0410]	0.0441 [0.0794]	0.00262 [0.0476]
IRCA x Poverty Rate	-0.0598 [0.0578]	-0.0616 [0.132]	-0.0447 [0.0679]
LAW x Poverty Rate	0.183* [0.0865]	0.174 [0.236]	0.126 [0.0998]
SAW x Poverty Rate	-0.0539 [0.0777]	-0.0004 [0.214]	-0.0287 [0.0828]
R <sup>2</sup>	0.049	0.0873	0.0612
N	55418	16514	43172

Each regression includes 180 month dummies and block group fixed effects. 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 12: IRCA and Convictions per Charge Filed, Interactions with Neighborhood Residents Per Housing Unit

	All Crimes	Income Generating	Non-Income Generating
Hispanic x IRCA x People / Housing Units	1.434 [1.335]	-0.154 [1.486]	4.315+ [2.617]
Hispanic x LAW x People / Housing Units	-1.17 [1.942]	1.28 [2.445]	-5.794 [3.149]
Hispanic x SAW x People / Housing Units	0.0016 [1.723]	-0.556 [2.136]	0.465 [2.508]
Hispanic Defendant	5.197* [2.216]	5.257* [2.520]	3.182 [3.542]
Hispanic x IRCA	-5.474 [4.095]	-1.355 [4.477]	-13.79+ [8.310]
Hispanic x LAW Expiration	4.371 [5.938]	-4.582 [7.225]	25.19* [10.55]
Hispanic x SAW Expiration	-1.114 [5.050]	1.94 [6.235]	-7.842 [8.593]
Hispanic Defendant x People / Housing Units	-0.755 [0.743]	-0.511 [0.857]	-0.477 [1.096]
IRCA x People / Housing Units	-0.59 [0.892]	-0.0155 [1.321]	-0.658 [1.980]
LAW x People / Housing Units	-0.663 [1.503]	-1.929 [1.759]	1.883 [2.617]
SAW x People / Housing Units	1.001 [1.474]	1.788 [1.631]	-1.579 [2.046]
R <sup>2</sup>	0.0488	0.0611	0.0872
N	55418	43172	16514

Each regression includes 180 month dummies and block group fixed effects. 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 13: IRCA and Convictions per Charge Filed, Interactions with Neighborhood Percent Mexican

	All Crimes	Income Generating	Non-Income Generating
Hispanic x IRCA x Percent Mexican	0.0828+ [0.0492]	0.0831+ [0.0504]	0.0385 [0.0579]
Hispanic x LAW x Percent Mexican	-0.165* [0.0752]	-0.169* [0.0756]	-0.109 [0.0848]
Hispanic x SAW x Percent Mexican	0.0954 [0.0689]	0.0947 [0.0693]	0.0803 [0.0749]
Hispanic Defendant	4.918** [1.706]	5.000** [1.796]	6.573** [2.022]
Hispanic x IRCA	-5.958+ [3.215]	-5.687+ [3.294]	-2.883 [3.762]
Hispanic x LAW Expiration	8.979+ [4.892]	9.378+ [4.911]	3.029 [5.431]
Hispanic x SAW Expiration	-6.315 [4.350]	-6.203 [4.347]	-3.541 [4.774]
Hispanic Defendant x Percent Mexican	-0.0279 [0.0264]	-0.0295 [0.0280]	-0.0464 [0.0320]
IRCA x Percent Mexican	-0.0528 [0.0359]	-0.0592 [0.0366]	-0.0652 [0.0409]
LAW x Percent Mexican	0.101+ [0.0544]	0.114* [0.0545]	0.118* [0.0595]
SAW x Percent Mexican	-0.0338 [0.0509]	-0.039 [0.0506]	-0.0407 [0.0529]
R <sup>2</sup>	0.0209	0.049	0.0613
N	55418	55418	43172

Each regression includes 180 month dummies and block group fixed effects. 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 14: IRCA and Convictions per Charge Filed, Interactions with Neighborhood Percent that Speaks Spanish at Home

	All Crimes	Income Generating	Non-Income Generating
Hispanic x IRCA x Percent Spanish at Home	0.103+ [0.0566]	0.0466 [0.0656]	0.139 [0.127]
Hispanic x LAW x Percent Spanish at Home	-0.186* [0.0871]	-0.114 [0.0974]	-0.256 [0.206]
Hispanic x SAW x Percent Spanish at Home	0.0981 [0.0801]	0.0857 [0.0861]	0.046 [0.177]
Hispanic Defendant	4.952** [1.692]	6.157** [1.923]	-0.0277 [3.181]
Hispanic x IRCA	-5.946* [3.021]	-3.195 [3.468]	-8.675 [6.570]
Hispanic x LAW Expiration	8.371+ [4.633]	2.435 [5.088]	21.40* [9.800]
Hispanic x SAW Expiration	-5.364 [4.106]	-3.151 [4.492]	-8.893 [8.469]
Hispanic Defendant x Percent Spanish at Home	-0.0316 [0.0323]	-0.0438 [0.0370]	0.0417 [0.0605]
IRCA x Percent Spanish at Home	-0.0613 [0.0436]	-0.0609 [0.0498]	-0.0123 [0.0956]
LAW x Percent Spanish at Home	0.128* [0.0652]	0.119+ [0.0718]	0.0667 [0.159]
SAW x Percent Spanish at Home	-0.0398 [0.0606]	-0.0342 [0.0638]	-0.0244 [0.144]
R <sup>2</sup>	0.049	0.0612	0.0872
N	55418	43172	16514

Each regression includes 180 month dummies and block group fixed effects. 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 15: IRCA and Convictions per Charge Filed, Interactions with Neighborhood Percent Foreign Born

	All Crimes	Income Generating	Non-Income Generating
Hispanic x IRCA x Percent Immigrant	0.285 [0.200]	0.228 [0.228]	-0.0862 [0.408]
Hispanic x LAW x Percent Immigrant	-0.466 [0.290]	-0.388 [0.309]	-0.309 [0.795]
Hispanic x SAW x Percent Immigrant	0.284 [0.260]	0.234 [0.276]	0.359 [0.716]
Hispanic Defendant	3.856** [1.301]	3.953** [1.494]	2.992 [2.410]
Hispanic x IRCA	-4.444+ [2.384]	-4.271 [2.706]	-0.236 [4.969]
Hispanic x LAW Expiration	4.683 [3.654]	1.594 [4.033]	11.18 [8.438]
Hispanic x SAW Expiration	-3.221 [3.250]	-0.803 [3.559]	-10.53 [7.690]
Hispanic x Percent Immigrant	-0.0434 [0.105]	0.0152 [0.119]	-0.084 [0.185]
IRCA x Percent Immigrant	-0.0918 [0.157]	-0.0654 [0.178]	-0.00113 [0.314]
LAW x Percent Immigrant	0.443+ [0.227]	0.457+ [0.243]	0.238 [0.652]
SAW x Percent Immigrant	-0.266 [0.203]	-0.307 [0.204]	-0.114 [0.611]
R <sup>2</sup>	0.049	0.0612	0.087
N	55418	43172	16514

Each regression includes 180 month dummies and block group fixed effects. 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 16: IRCA and Convictions per Charge Filed, Multiple Proxies for Location Choice

	Crimes	Income Generating	Non-Income Generating
Hispanic x IRCA x Poverty Rate	-0.12 [0.115]	-0.214 [0.139]	0.182 [0.289]
Hispanic x LAW x Poverty Rate	0.173 [0.193]	0.411+ [0.222]	-0.515 [0.470]
Hispanic x SAW x Poverty Rate	-0.09 [0.173]	-0.248 [0.183]	0.318 [0.421]
Hispanic x IRCA x People / Housing Units	0.622 [1.551]	-0.753 [1.856]	2.74 [2.830]
Hispanic x LAW x People / Housing Units	-0.495 [2.083]	2.894 [2.552]	-4.808 [3.701]
Hispanic x SAW x People / Housing Units	0.0405 [1.907]	-1.877 [2.254]	2.273 [2.993]
Hispanic x IRCA x Percent Mexican	0.025 [0.189]	0.0951 [0.222]	-0.487 [0.396]
Hispanic x LAW x Percent Mexican	-0.312 [0.295]	-0.359 [0.329]	0.373 [0.649]
Hispanic x SAW x Percent Mexican	0.28 [0.267]	0.216 [0.284]	0.178 [0.598]
Hispanic x IRCA x Percent Spanish at Home	0.0944 [0.227]	0.013 [0.269]	0.63 [0.494]
Hispanic x LAW x Percent Spanish at Home	0.116 [0.354]	0.114 [0.406]	-0.477 [0.788]
Hispanic x SAW x Percent Spanish at Home	-0.213 [0.324]	-0.0509 [0.351]	-0.369 [0.725]
Hispanic x IRCA x Percent Immigrant	0.184 [0.272]	0.316 [0.307]	-0.737 [0.550]
Hispanic x LAW x Percent Immigrant	-0.195 [0.383]	-0.48 [0.425]	0.801 [1.018]
Hispanic x SAW x Percent Immigrant	0.202 [0.337]	0.285 [0.362]	0.139 [0.941]
R <sup>2</sup>	0.0499	0.0628	0.0912
N	55418	43172	16514
<i>p(Enactment)=0</i>	0.5318	0.5597	0.4470
<i>p(exLAW)=0</i>	0.3250	0.1579	0.3744
<i>p(exSAW)=0</i>	0.7697	0.5245	0.9404

Each regression includes 180 month dummies and block group fixed effects, as well as full Hispanic, demographic, and immigration reform interactions. 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

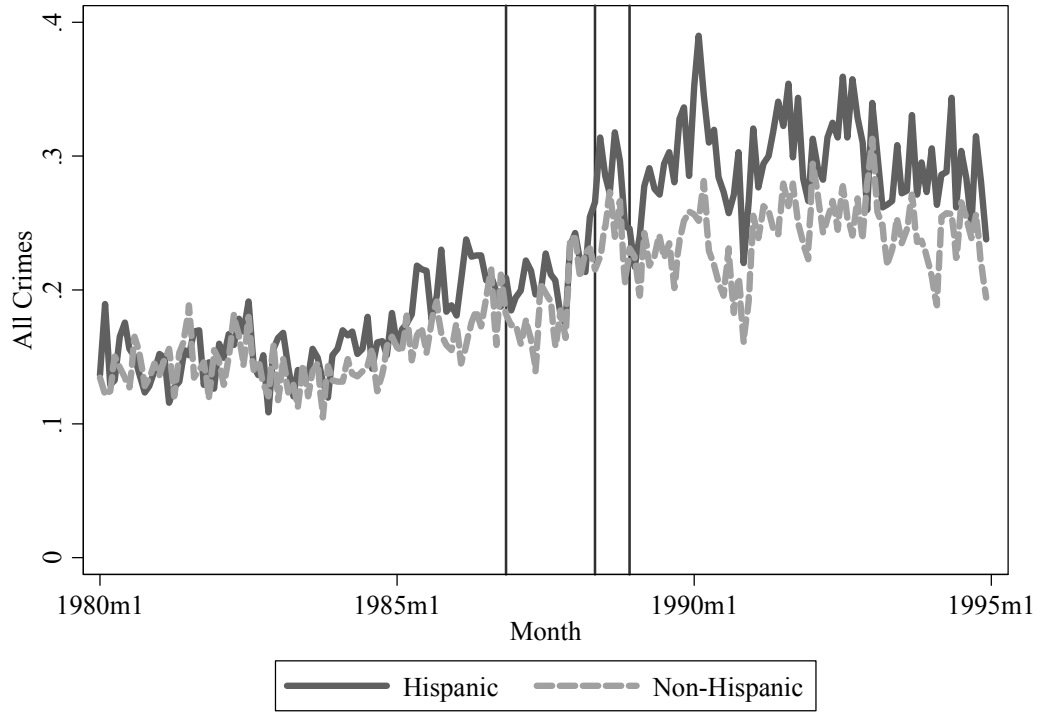


Figure 2: Immigration to Bexar County by Date of Entry, 1990 Census Data

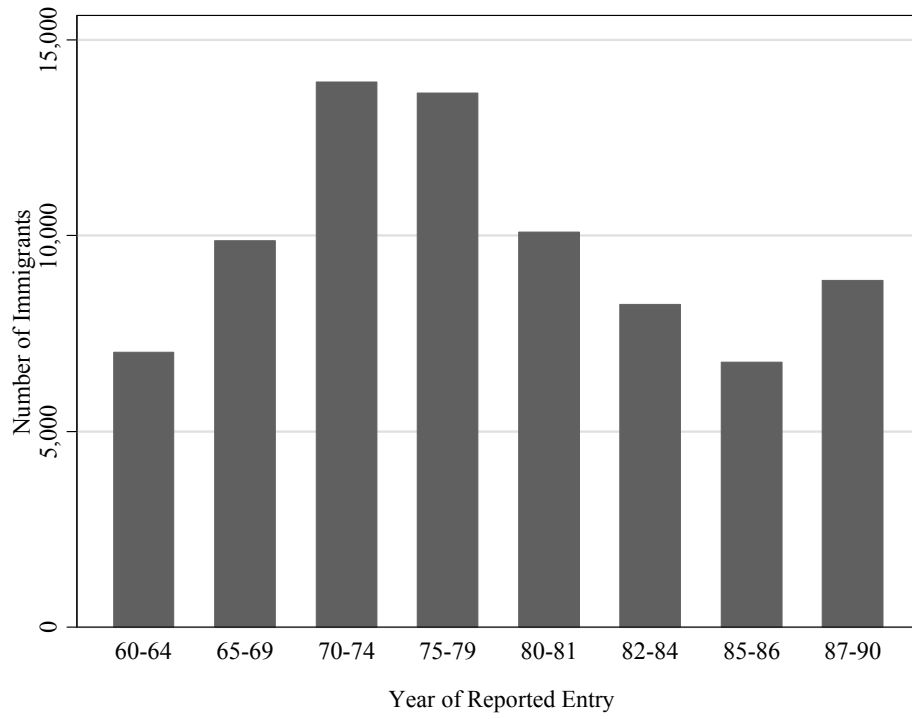


Figure 3: Immigration to Bexar County by Date of Entry, 1992 INS Legalization Summary Tape

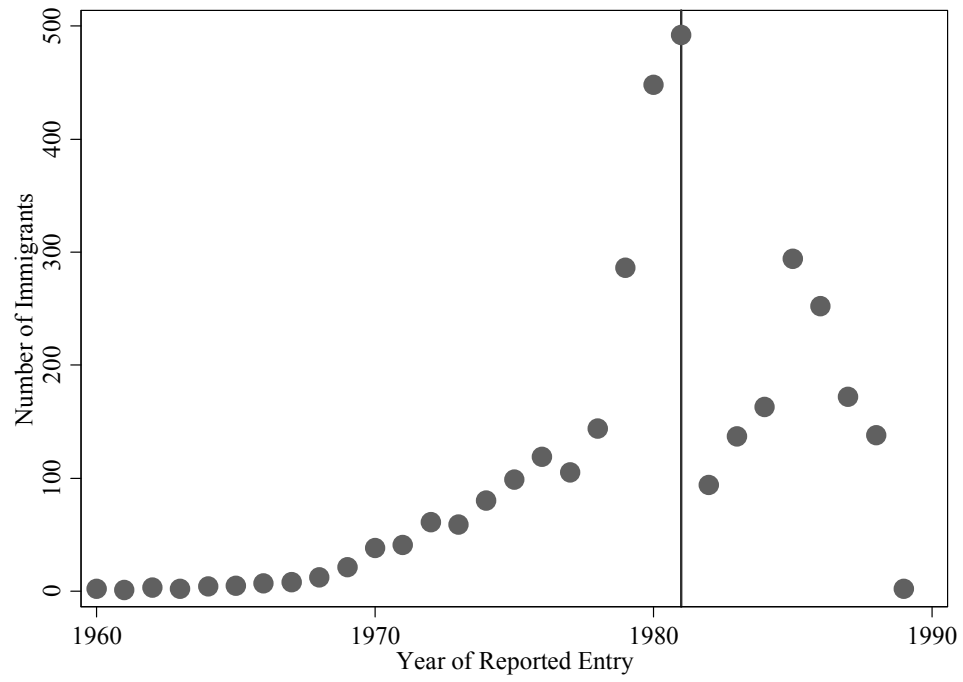


Figure 4: Share of Immigrants to Bexar County Arriving in Fourth Quarter, by Year of Entry, 1992 INS Legalization Summary Tape

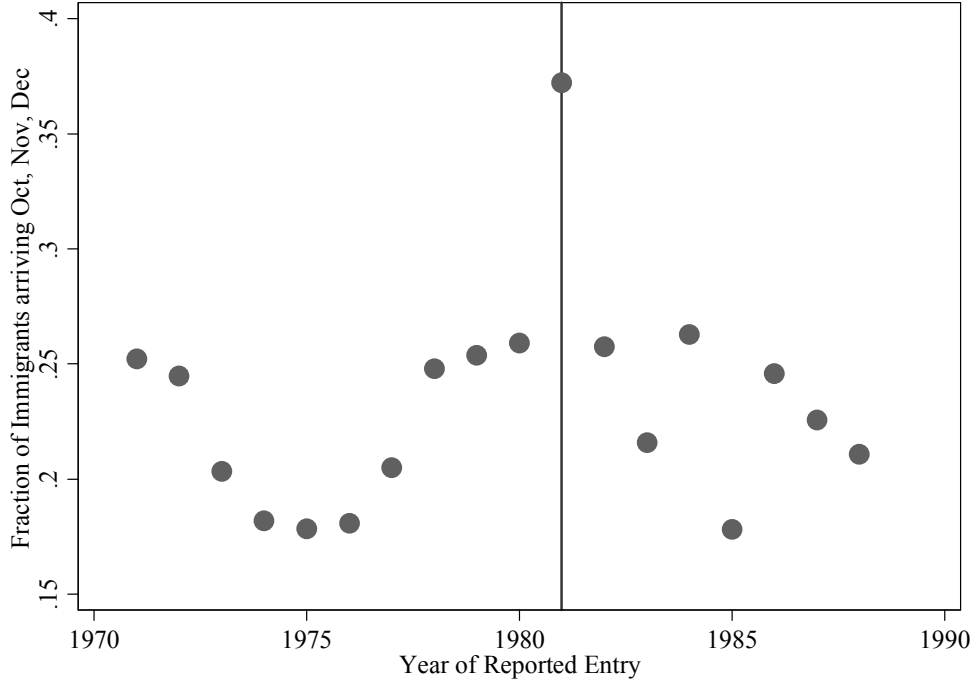


Figure 5: Average Monthly Neighborhood Criminal Incidence by Ethnicity and Crime Type

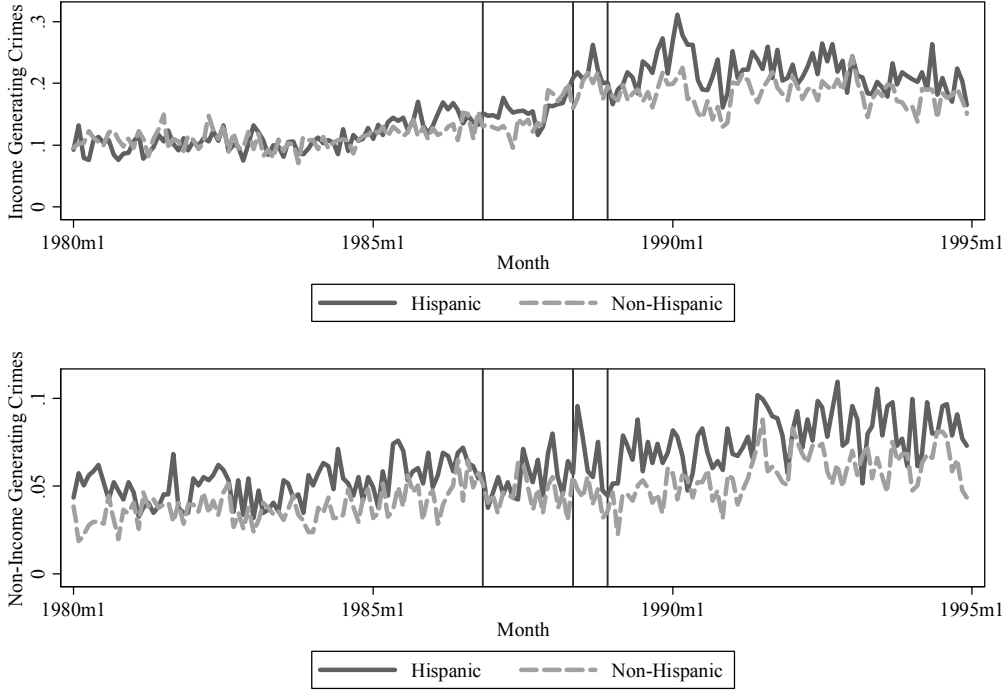
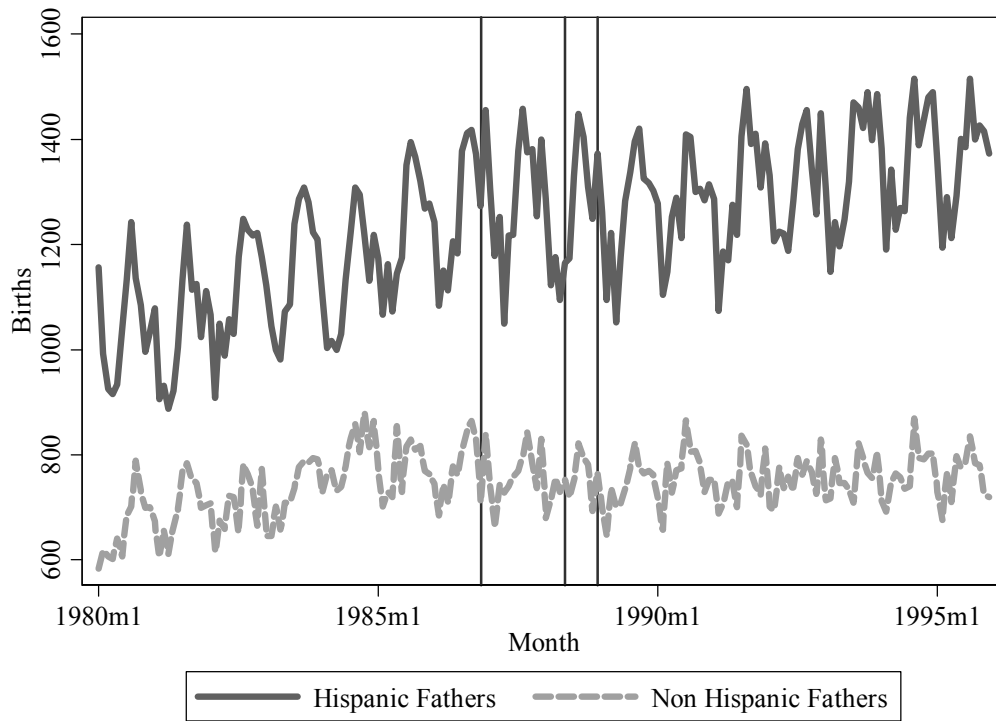
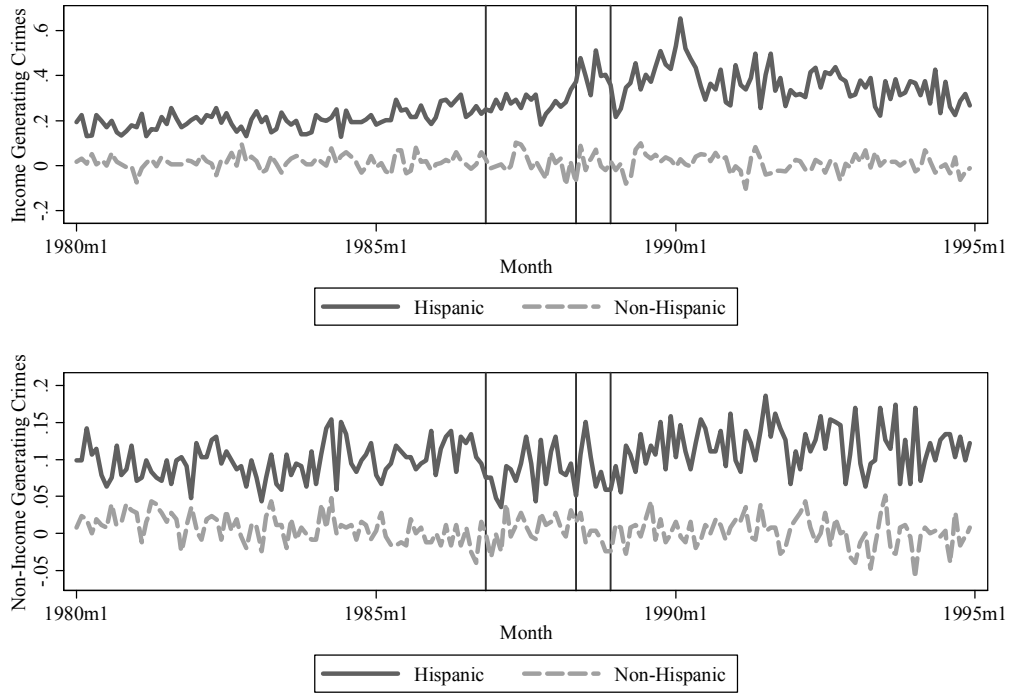


Figure 6: Births among Hispanic and Non-Hispanic Fathers in Bexar County



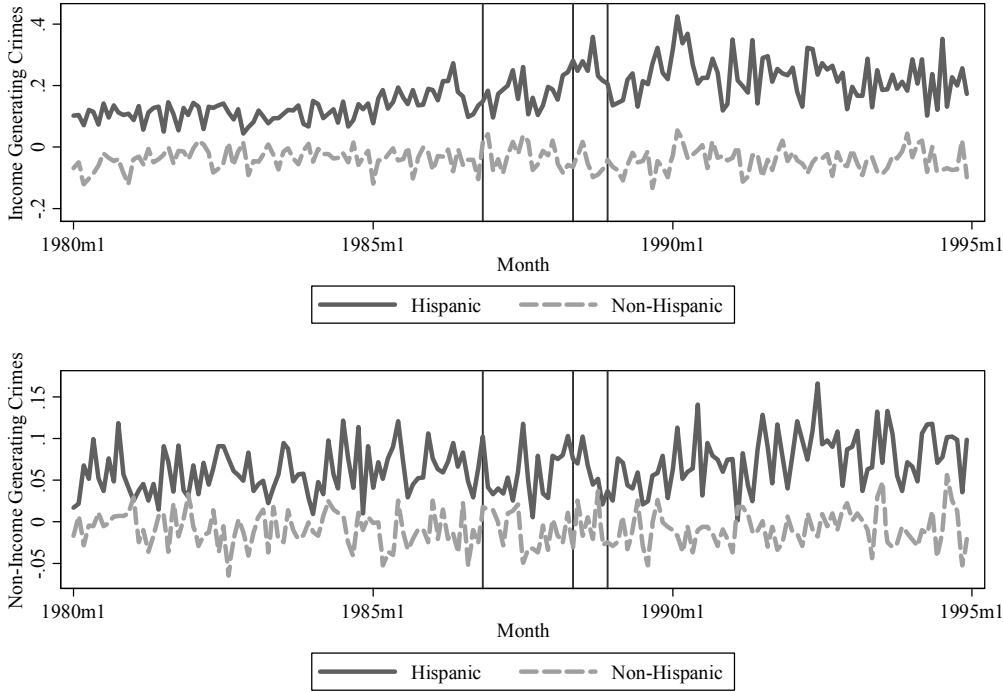
Source: Vital Statistics Database, National Center for Health Statistics.

Figure 7: 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.

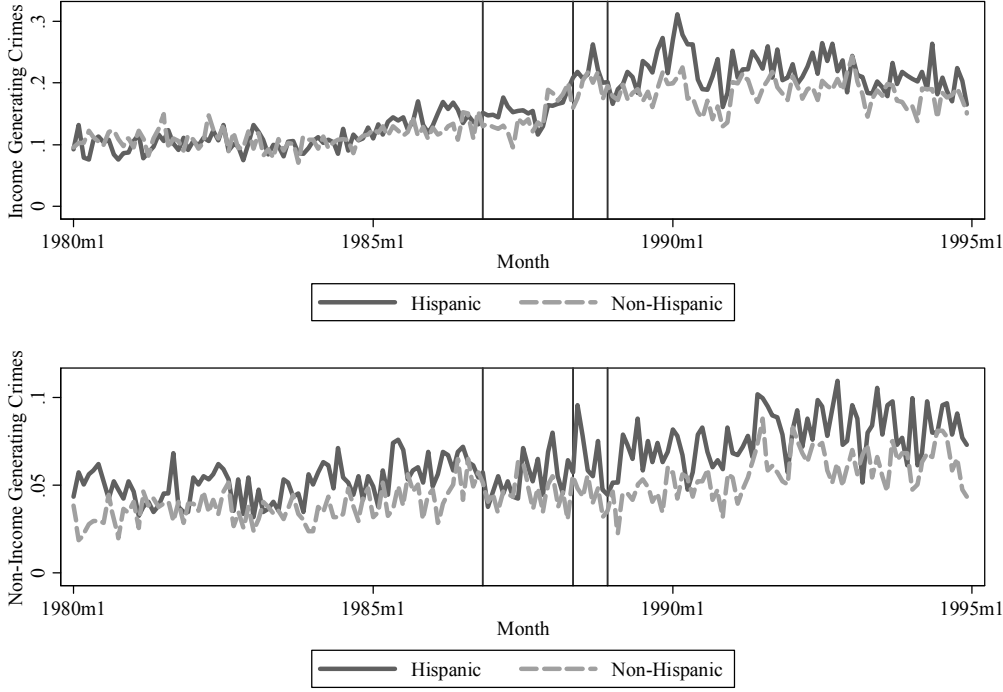
Figure 8: Difference in Criminal Incidence across High and Low Residential Density Neighborhoods, by Ethnicity and Crime Type



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.

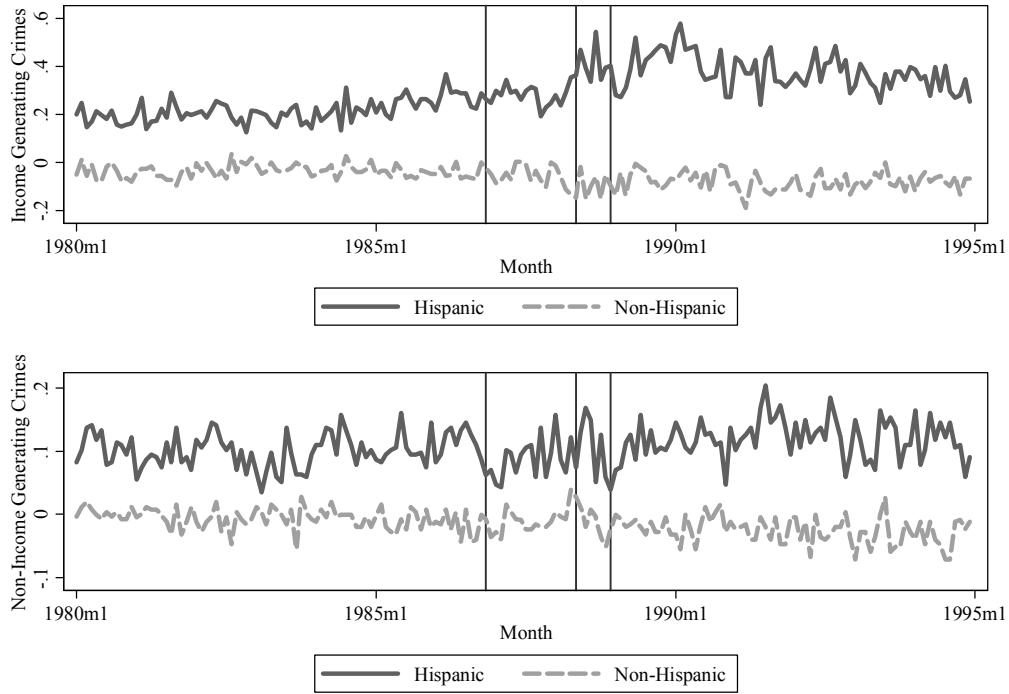


Figure 9: Difference in Criminal Incidence across High and Low Percent Mexican Neighborhoods, by Ethnicity and Crime Type



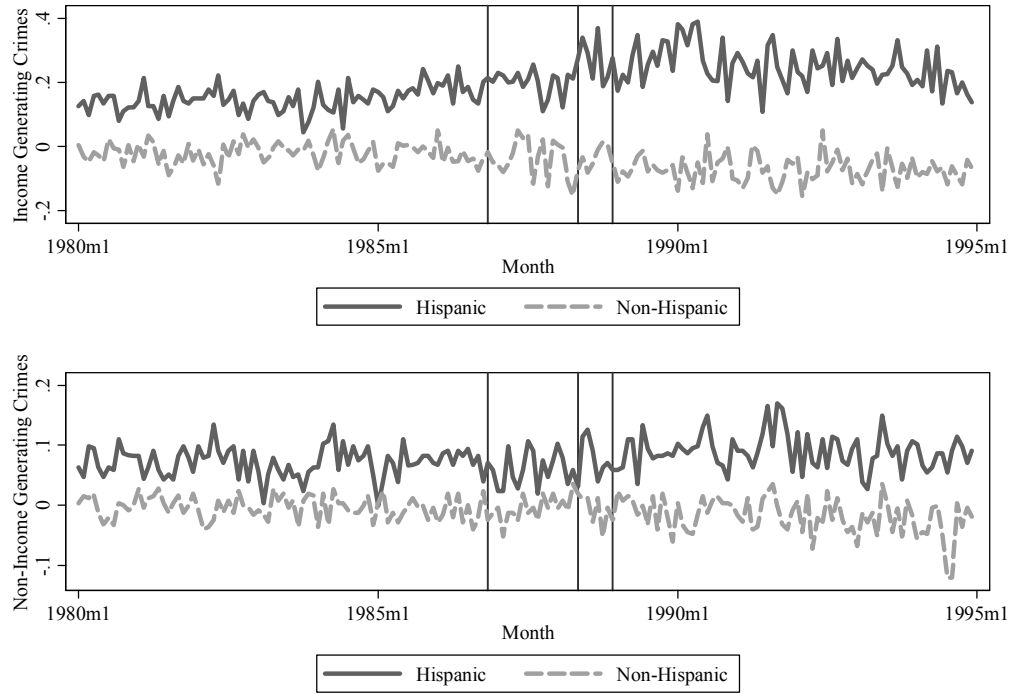
High and low percent Mexican neighborhoods are block groups in the top quartile and bottom quartile of the percent Mexican distribution in the 1990 Decennial Census.

Figure 10: 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 11: 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.

## Appendix

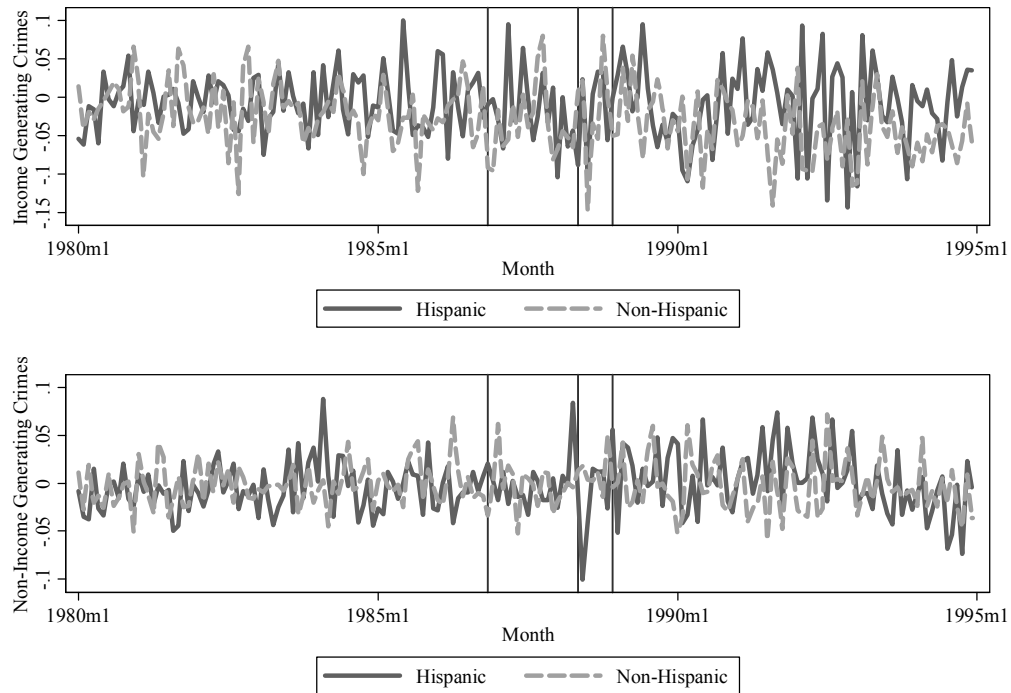
Table A1: IRCA and Felony Charges, Interactions with Neighborhood Percent Working In Agriculture

	Crimes	Income Generating	Non-Income Generating
Hispanic x IRCA x Percent Working in Agriculture	0.0123* [0.00524]	0.00683 [0.00427]	0.00538* [0.00262]
Hispanic x LAW x Percent Working in Agriculture	-0.0476** [0.0147]	-0.0429*** [0.0118]	-0.00751 [0.00705]
Hispanic x SAW x Percent Working in Agriculture	0.0290+ [0.0153]	0.0289* [0.0126]	0.0038 [0.00747]
Hispanic Defendant	0.0875+ [0.0509]	-0.0103 [0.0406]	0.124*** [0.0198]
Hispanic x IRCA	0.00035 [0.0518]	0.0477 [0.0454]	-0.0416 [0.0287]
Hispanic x LAW Expiration	0.322*** [0.0937]	0.252** [0.0877]	0.116* [0.0494]
Hispanic x SAW Expiration	0.0245 [0.0849]	-0.00832 [0.0801]	0.0163 [0.0454]
Hispanic Defendant x Percent Working in Agriculture	0.0166** [0.00569]	0.0225*** [0.00429]	-0.00625** [0.00228]
IRCA x Percent Working in Agriculture	-0.0125** [0.00457]	-0.00945+ [0.00497]	-0.00414* [0.00194]
LAW x Percent Working in Agriculture	0.00645 [0.0106]	0.00479 [0.00940]	0.00341 [0.00448]
SAW x Percent Working in Agriculture	0.00941 [0.00974]	0.00159 [0.00788]	0.00588 [0.00494]
R <sup>2</sup>	0.0844	0.0708	0.03
N	360360	360360	360360

Each regression includes 180 month dummies and block group fixed effects. 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.