

EFFECTS OF IMMIGRANT LEGALIZATION ON CRIME: THE 1986 IMMIGRATION REFORM AND CONTROL ACT*

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Abstract

I examine the effects that the 1986 Immigration Reform and Control Act (IRCA), which legalized almost 3 million immigrants, had on crime in the United States. I exploit the IRCAs quasi-random timing as well as geographic variation in the intensity of treatment to isolate causal impacts. I find decreases in crime of 2%-6%, primarily due to decline in property crimes, equivalent to 80,000-240,000 fewer violent and property crimes committed each year due to legalization. I calibrate a labor market model of crime, finding that much of the drop in crime can be explained by greater labor market opportunities among applicants.

JEL classification: F22, J22, J61, K37, K42

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1 Introduction

In the late 1970's, rates of undocumented immigration into the United States began to increase dramatically. As with more recent debates over undocumented immigration, many thought that this increase was hurting the job prospects of natives and legal immigrants, as well as having other undesirable social effects, leading politicians to seek a method to deal with this influx. The result was the 1986 Immigration Reform and Control Act (IRCA), which imposed harsh penalties on the hiring of undocumented immigrants, increased border security, and provided a near-universal amnesty for undocumented immigrants currently in the United States. The present state of the immigration debate has much in common with the early 1980's, with many politicians seeking to find some way of dealing with the current stock of over 12 million undocumented immigrants and the hundreds of thousands of new arrivals every year. Similarly to the 1986 IRCA, the current debate focuses on labor market effects as well as other social effects; of these, the most prominent is that of crime.

This paper examines the effects that the IRCA, which legalized close to 3 million undocumented immigrants, had on the commission of crime in the United States using administrative data from the IRCA application process. Using a variety of empirical estimation strategies, I provide estimates of the total effect of this legalization and of the potential effects on crime of a new amnesty bill. With these specifications, I exploit the large amount of variation in both the geographical distribution and quasi-random timing of the legalizations. I find persistent decreases in crime of approximately 2%-6% associated with one percent of the population being legalized, primarily driven by a drop in property crimes. This fall in crime is equivalent to 80,000-240,000 fewer violent and property crimes committed each year across the nation due to legalization. While there were drastic changes in crime during the 1980s and 1990s, the declines in crime that I find are linked to legalization cannot be explained by existing trends, economic conditions, declines in drug crimes, changes to police forces or prison populations, or other common explanations of changes in crime rates during this period. Given that proposals for legalization similar to the 1986 IRCA have been debated in the United States and around the world, this analysis can provide some estimates of potential effects on crime stemming from similar amnesty programs and immigration reforms in general.

Looking more deeply into prior literature and examining surveys conducted on the legalized IRCA applicants, I find strong evidence for enhanced levels of human capital and greater labor

market opportunities resulting from becoming legal residents. To this end, I also provide theoretical evidence that these increases in labor market opportunities and shifts from crime to legal work could be the primary mechanism that drove down crime. I present a model of job search and crime, calibrating this model to real-world data and matching its predicted outcomes against county-level empirical crime data, finding a close match.

1.1 Literature

Both before and after the IRCA program, the large presence of both legal and undocumented immigrants in the United States has given rise to a large body of literature devoted to studying them. Of most relevance to this paper are strands of research examining interactions of immigration, legal status, crime, and labor market access.

Much of the existing literature on undocumented immigration highlights the changes in behavior and labor market access produced by various legalization programs. For example, Orrenius and Zavodny (2003) examine whether the amnesty portion of the IRCA reduced undocumented immigration at a national level. A wide range of others such as Bratsberg et. al. (2002), Amuedo-Dorantes et. al. (2007), Kossoudji and Cobb-Clark (2002), Kaushal (2006), and Lozano and Sorenson (2011) examine the value of legal status in terms of labor market access and income. Their results consistently find large effects of obtaining legal status on income among previously undocumented immigrants, speaking to better labor market access, skill acquisition, and more efficient bargaining.

Outside the literature explicitly dealing with only undocumented immigrants, a primary focus of immigration work has been on the effects of immigrants on labor markets. While innumerable labor market studies have been conducted, (eg. Dolado et al (1997), Friedburg and Hunt (1995), and George Borjas (2003, 2005, 2006), and Passel (2006)), two which are methodologically relevant are those by Bailey (2002), and Card (1990). Bailey utilizes the 1986 IRCA data to extensively examine labor market effects of legalization. Bailey uses the IRCA data primarily for identifying applicants by location, as I do, in order to match local labor market outcomes. Bailey finds little effect of the IRCA on natives' labor market outcomes. Such estimates are important to the assumptions in my labor market model of crime, which assumes no negative effect of the IRCA on natives, only positive effects among IRCA applicants. David Card (1990) analyzes the Mariel Boatlift, exploiting the massive influx of Cubans into the Miami area as a natural experiment, examining labor market

outcomes among native groups.

While most immigration literature deals with effects on the labor force, there is also a large literature on the effects of immigration on crime. Much of this immigrant-crime research focuses on the common belief that immigrants are much more prone to commit crimes than natives. For instance, Martens (1997), working with data from Sweden, found that both first and second generation immigrants have higher rates of crime than natives, but that second generation immigrants have lower rates than first generation immigrants do. Butcher and Phiel (1998a, 1998b, 2007) provide several studies of the United States which reach a different conclusion. They find that cities which have high concentrations of immigrants also have relatively high rates of crime but after controlling for demographic characteristics of cities and of groups of immigrants and natives, they find that recent immigrants have equal or slightly less chance of committing crimes than natives. Hagan and Polloni (1999) also find that the number of crimes committed by Hispanic immigrants are inflated when compared to the number committed by natives, since most recent immigrants are young males, who are more disposed to crime than the average person.

A number of papers since Becker's (1968) seminal work have linked crime rates, especially those for economically-driven crimes, to economic conditions. Others such as Meyers (1983) and Gould et. al. (2002) draw tighter links between the two. One recent study by Borjas, Grogger, and Hanson (2009) find that an increase in low-skilled workers from immigration, representing a drop in wages and the per capita labor market opportunity for similarly low-skilled blacks, manifested itself partly in an increase in incarceration rates for the affected group of competing natives.

Finally, several researchers have turned to various immigration and legalization programs in Europe to examine the relationship between legalization and crime. For instance, Bell, Machin, and Fasani (2013) present evidence on the impact on crime of large-scale immigration into the United Kingdom. They examine two large waves of immigration in the 1990s and 2000s, one composed of asylum-seekers who were legally prevented from finding work while their applications were being considered, and one composed largely of workers from newly admitted EU countries. They find evidence for increases in property crime associated with the first group and none with the second. They also find no increase in violent crime associated with either group. They conclude that the observed increases in crime derived primarily from lack of attachment to the labor force. Freedman, Owens, and Bohn (2013) concur with this assessment, finding some evidence of decreases in crime associated with the ability of undocumented immigrants to obtain legal employment.

Finally, of particular interest is recent work by Mastrobuoni and Pinotti (2014) and Pinotti (2014). Mastrobuoni and Pinotti examine the causal effects of the legal status of immigrants in the European Union on criminal behavior. They utilize variation in the timing of changes to migration restrictions among different EU nationalities, finding lower recidivism among economically-motivated immigrants with legal status than those without legal status. Such findings mirror many of those found in this paper, with legal status decreasing crime rates, especially property (economic) crimes, and possibly functioning primarily through the labor market. While Mastrobuoni and Pinotti are able to exploit a quasi-experimental identification strategy, they are able to apply this to only a small population (<2,000) of released inmates, potentially a non-representative group when compared to the typical population of legalized individuals, and for only a relatively short period after the policy change. In Pinotti (2014), the author utilizes a regression discontinuity design to examine how having an employer barely miss or barely miss the threshold for sponsoring a legal immigrant affects their individual probability to commit crime, finding a significant negative effect. Pinotti estimates the number of serious crimes being committed by legalized individuals falls from almost 3 per 100 to 1.2 per 100 applicants in the following year. Moreover, consistent with my findings, Pinotti finds a much larger effect on economic crimes and in regions with better economic opportunities to legal immigrants.

2 The 1986 Immigration Reform and Control Act

Woodrow and Passel (1990), US Census Bureau demographers, found that, immediately prior to the Immigration Reform and Control Act of 1986, there were approximately 3.2 million undocumented immigrants living in the United States. Combined with data regarding all 3.04 million applicants to the 1986 IRCA, this suggests that almost all undocumented immigrants present in the United States at the time applied to the program. The 1986 IRCA had the effect of legalizing over 2.8 million previously undocumented immigrants between the years of 1987 and 1990, with the majority of legalizations occurring in 1988 and 1989. A cumulative number of national legalizations can be seen in Figure I. This legalization was a substantial shift in the lives of these immigrants, with the potential to produce large changes in behavior — in labor market outcomes, family life, and interaction with the government and community — in a large group of individuals comprising approximately 1.1% of the total population of the United States.

The 1986 Immigration Reform and Control Act (IRCA) was a bipartisan effort to strengthen the nation's controls on undocumented immigrants. The primary purpose of the bill was to enhance the controls on the hiring of undocumented immigrants, as it was theorized that such financial penalties would reduce employment opportunities for undocumented immigrants and thus decrease the flow of undocumented immigrants into the United States. Prior to the bill's passage, there were essentially no federal laws regulating the ability of employers to knowingly hire undocumented immigrants, though there were a number of state laws which did just this. The bill made it illegal to knowingly hire or recruit undocumented immigrants and also required employers to at least give a cursory investigation into their immigration status, as long as the business employed at least three employees. The bill also, and most importantly for the purposes of this study, granted amnesty to certain groups of undocumented immigrants who had entered the United States prior to 1982 and lived here continuously, as well as to many agricultural workers.

The IRCA was subject to much debate in Congress and the media, beginning with its inception in the early 1980s. The first criticisms came from human rights groups and Hispanic groups who railed against the bill's labor market provisions. They feared that employers would become unwilling to hire Hispanic workers for fear of their being undocumented and thus the bill would greatly worsen labor market discrimination against Hispanics, who would be the group most affected by the bill. Additionally, farmers and growers also strongly opposed the bill, fearing an end to their usage of undocumented immigrants as temporary agricultural workers, and the Chamber of Commerce opposed any financial sanctions on businesses. Over the following years, the furor over employer sanctions relented to some degree as requirements that employers diligently verify employment status were dropped and passages were added which banned racial discrimination in hiring. After this change, attention began to focus more intently on a compromise dealing with agricultural workers and the legalization provisions. Finally, in 1986, the bill was passed in its final form. While the bill seemed likely to pass at some point in some form, its passage in this particular year and with the final provisions in the state that they were was by no means certain.

In its final form, the IRCA provided paths to citizenship for two groups of immigrants. The first were immigrants who had resided in the United States for a relatively uninterrupted period since January 1st, 1982 and applied between May 5, 1987 and May 4, 1988. The second were Special Agricultural Workers (SAW), those who had worked with certain types of crops in the United States for 90 days or more in the 1984, 1985, or 1986 and applied between June 1, 1987 and

November 30, 1988. Both types of applicants would be disqualified if they had committed three misdemeanors or a felony in the United States prior to application. After the acceptance of their application, all applicants were given the status of 'Temporary Resident Aliens', a step towards green card status, lasting 18 months. After this period, upon completing a proof of English test and civics test, they were given permanent resident status. During their temporary residency, if they committed a felony or three misdemeanors, they would be removed from the program. In addition, during this temporary residency, their access to government benefits programs was limited and they could not yet sponsor family members as additional immigrants. However, they could now legally enter and leave the United States through ports of entry.

One important factor in determining the population that could become legalized was the extent to which fraud played a role in admittance to the program. Examining the reports from a variety of sources, there seemed to exist a large amount of fraud in the application process, especially among the Special Agricultural Worker cohort. For example, the number of SAW applicants in California was far greater than any government estimate of the entire agricultural worker population in the state, not just the estimated number of undocumented agricultural workers. Furthermore, a number of reports from front-line interviewers suggested that they believed a high percentage of the SAW applicants were fraudulent, as well. One interviewer constructed a book of pictures of various crop types and would ask applicants to point to a picture of the crop they had claimed to work on; few could answer correctly. Finally, a number of sources, such as North (2010), within the program noted the amount of political pressure placed on them to approve as many applications as possible. All of this suggests the near-universal availability of legalization for virtually every undocumented immigrant within the country. Moreover, the INS was also tasked with spreading awareness about the program to all potential applicants. To this end, the INS funded a large number of Qualified Designated Entities (QDEs), community organizations which both spread awareness and helped applicants with some their paperwork. These QDEs were often Hispanic or other ethnic organizations around the country, as well as traditional refugee- and immigrant-serving agencies. These agencies went to great lengths to publicize the availability of the program and assist with the application process.

There were a small number of rejections of applicants (under 5%). These applications were kept on file, but were not forwarded to law enforcement and no punitive action was taken against those who were rejected from the program (leading to, for example, the expulsion of undocumented

immigrants who had committed felonies and were therefore ineligible for the program).

The application review process was one of the largest bureaucratic undertakings ever attempted by the INS up to that time. The INS consulted with the IRS for information about handling millions of sets of paperwork in a short period of time and set up a new series of 107 ‘legalization offices’ throughout the country. In conjunction with these offices, the INS greatly increased its ‘remoting’ practices, which would see paperwork sent around the country in order to distribute work to officers who had more spare time. For example, paperwork would be sent to border crossing facilities to be worked on during the middle of the night when there was little border crossing activity. The 107 ‘legalization offices’ generally conducted the initial applicant interviews and then sent the paperwork along to one of 4 larger INS offices that would then make the decisions about legalization.

The entire INS operation was intended to be standardized across the country, with no strong regional variation in the decisions regarding similar legalization applications as had been seen in decades past with naturalization decisions. The practice of ‘remoting’ and the unfamiliarity of the INS with the massive undertaking, as well as the underestimate of the number of applicants, meant the INS was overwhelmed and the application approval process took much longer for some applicants than others. In essence, it meant that if two identical IRCA applicants both applied in mid-1987 in the same county, one might be legalized by the end of 1987 and the other remaining without legal status for up to 3 additional years. To quantify this point, the application date, county of application, and individual characteristics like age, race, sex, and marital status only explain about 60% of the variation in the legalization period, with the remainder largely driven by the randomness of the application processing time.

2.1 Effects of Bill

Much of the literature on undocumented immigrants spanning the 1970’s, 1980’s, and 1990’s highlights the lower wages that they received, relative to legal immigrants. This gap remained after controlling for observable demographic characteristics and levels of education. One such survey of nearly 800 undocumented immigrants by North and Houstoun (1975) finds 37% lower hourly wages among undocumented immigrants compared to similar workers in their same industry. A similar ‘premium’ for legal immigrants is found by Douglas Massey in a 1987 study involving only Mexican immigrants. Finally, Rivera-Batiz (1999) finds nearly identical discrepancies in wages, with legal male Mexican immigrants earning approximately 42% more than undocumented workers and legal

female Mexican immigrants earned nearly 41% more. Overall, the undocumented immigrants were lower educated and had a lesser grasp of English, but these observable factors could explain less than half of the gap in wages. Such evidence points to a premium paid for the legal status of legal workers, beyond differences in skills and education.

There is also evidence of much greater incidence of part time and seasonal work among undocumented immigrants, as noted by Orrenius and Zavodny (2003). Much of this was due to the seasonal demands of agricultural work, where immigrants would often be unable to find employment for a number of months during any given year. However, voluntary ‘job hopping’ or other relatively rapid changes in employment locations and status were a common occurrence among non-agricultural undocumented immigrants. The primary reasons behind such changes stemmed from a desire to elude deportation through frequent changes in employers as well as the insecure nature of their jobs in general. Employers were able to more easily fire undocumented workers, or simply stop paying them, as they had essentially no legal recourse available to them. Such frequent shifts in employment most likely hindered undocumented workers’ ability to acquire job-specific capital and decreased their average productivity as they were not changing jobs in the face of better job opportunities, but to avoid detection. Prior to the IRCA, a number of the largest destinations for immigrants, including California and Florida, already had various types of employer sanctions as existing laws. In total, 11 states had such laws on the books before the IRCA was passed.¹ So, in many cases, the IRCA did not create new penalties for employers or provide additional disincentives for employers to hire undocumented immigrants, causing the only meaningful effect of the bill to be the legalization provisions.

In the years following the passage of the IRCA, a number of surveys have pointed to increases in both language skills and education levels, as well as higher marriage rates, among IRCA applicants. Such increases spoke to increases in levels of general skills and productivity, coinciding with increases in wages of 15%-25% among this group. Though skill increases did account for much of the higher wages following legalization, there remains much evidence for a legal immigrant ‘premium’ unrelated to observables. Rivera-Batiz (1999) finds large levels of wage growth in the four years following the passage of the 1986 IRCA. However, he finds that such gains are due primarily to the actual change

¹The states already possessing employer sanction laws were California, Connecticut, Delaware, Florida, Kansas, Maine, Massachusetts, Montana, New Hampshire, Vermont, and Virginia. Chau, Nancy. Strategic Amnesty and Credible Immigration Reform. *Journal of Labor Economics*, 2001.

in legal status, not to increases in education, language skills, or other observables. In other work, Kossoudji and Cobb-Clark (2000) find significant evidence for increased job mobility and upwards earnings trajectories for newly-legalized IRCA applicants, stating that “Relative to pre-legalization mobility, few characteristics surpass in importance the now common experience of having legal papers.”

However, despite these gains due to a shift to legal status, they were generally not caused by increases in wages induced by the need to adhere to minimum wages for legal workers. Most evidence points to undocumented immigrants earning more than the minimum wage in most occupations. Rivera-Batiz (1999) finds average male undocumented immigrant wages of approximately \$6.75 in 1987 and 1988, when the federal minimum wage was only \$3.35. Self-reported wages from the data utilized in this analysis (over all IRCA applicants during 1986) point to average hourly wages of \$5.75 and average annual wages of \$12,028, equivalent to approximately \$6.00 over a 2,000 hour working year.

In addition to rising incomes from better labor market outcomes, the 1986 IRCA, combined with the Federal Omnibus Budget Reconciliation Act of 1986 and other state-level bills, extended the coverage of some benefit programs to newly legalized IRCA applicants. For instance, in California, IRCA applicants were now able to get a wide variety of medical services under expanded Medical and Medicaid programs even before the conclusion of their 18-month temporary residency period. However, most federal benefit programs remained off-limits to IRCA applicants during their temporary residency period, and were fully accessible only following a period of 5 years after legalization.² Such programs had the effect of boosting effective pay and benefits and gave IRCA applicants some additional social safety nets during the times in which they were unemployed or received other negative income shocks. These types of benefits, in combination with rising labor incomes, further diminish the economic motives for criminal behavior.

One important conclusion from this literature, combined with results of Bailey’s work, is that

²The text of the 1986 IRCA mandated a period of 5 years after legalization before most federal benefit programs would be available to IRCA applicants. Included in these off-limits programs were food stamps, Medicaid, and most other financial assistance programs based on financial need. There were a variety of exceptions for medical care for pregnant women and children, as well as for the disabled and emergency services. Various other services such as discounted school lunches, Head Start, child nutrition programs, and job training were available to legalized IRCA applicants without the 5 year waiting period. During this time, states were also allowed to institute their own laws regarding the accessibility of state-programs for legalized IRCA applicants.

the passage of the IRCA seems to represent a net gain for IRCA applicants in terms of the labor market and no significant loss for competing workers. While there is evidence of minor economic harm among low-skilled natives as a consequence of the arrival of new immigrants, the legalization of already-present undocumented workers seems to have not had similar negative impacts, as the IRCA applicants had already been present in the country and in competition with low-skilled natives for a number of years. The IRCA helped to usher in higher levels of education, skill acquisition, and English language proficiency, as well as diminished reasons for frequent job-hopping to evade government detection, among IRCA applicants. Thus the IRCA, in terms of labor market outcomes, represents a net gain in productivity and output on a national level, not just a reallocation of wages or jobs from native workers to newly-legalized IRCA applicants. Further evidence to this effect can be found in Appendix 3.

3 Data

There are two principle sources of data used in this paper. The first is the 1990 Legalization Summary Tapes created by the Immigration and Naturalization Service (now US Citizenship and Immigration Service). The Legalization Summary Tapes are a set of large databases of comprehensive demographic information regarding every immigrant admitted or legalized in each fiscal year from 1972-1996.

The primary purpose of this dataset is to quantify the number of IRCA applicants in each county in the United States. Over 95% of the applicants to the 1986 IRCA were accepted into the program, so this list of applicants is a good measure of the number of undocumented immigrants that were legalized in each of these counties and the year in which they were legalized. Due to privacy concerns, counties where fewer than 25 applicants resided are not listed, and only the state of residence is given. However, the IRCA applicants in these counties compose a small percentage of applicants, so the county level data are still relatively comprehensive. This dataset is useful in providing an accurate demographic and geographic portrayal of the undocumented immigrants in question.

Also taken from the 1990 Legalization Summary Tapes are other demographic information used for weighting of crime outcomes. Since the demographic composition of IRCA applicants differed greatly from county to county, it would be inappropriate to use only the level of immigrants in a

county as my dependent variable, as propensity to commit or be a victim of crime is, in general, highly dependent on demographic characteristics. Overall, the mean ratio of IRCA applicants to county population, displayed in Figure II, is approximately 0.8%, with individual county values ranging from 0% to over 20%. The data also shows the applicant population is overwhelmingly male, at approximately 73% of total applicant population. In addition, the applicants, with a median age of 27 years old, are younger than the national average of 35. The average annual wage, when reported, is much lower than that of the average American. Almost 80% of applicants do report some information on earnings; they report earning approximately \$12,000 a year when reporting an annual wage, or \$6 an hour when reporting an hourly wage. All of the demographic differences will influence the weighting of crime outcomes in each county. The differences in rates of crime between males and females is large, as seen in Figure III. That is, a county where the entire population of IRCA applicants is young and male will naturally be predicted to have a greater change in crime upon legalization than one in which the entire population of applicants is elderly and female given the greater predisposition to crime of the treated population. Comparisons of demographic statistics between the IRCA applicant population and national averages can be found in Table VI.

I employ a weighting constructed using the FBI's demographic statistics on violent and property crimes per capita. Using these numbers, I create a cumulative index, by county and year, which gives the predicted number of crimes per capita of IRCA applicants given their age and sex. The weight is then given by this index divided by the predicted number of crimes per capita of the entire population of IRCA applicants. Thus, if the population of IRCA applicants in county A is expected to commit 100 crimes per 1000 while the population of IRCA applicants county B is expected to commit 50 crimes per 1000, the weight given to the legalizations in county B will be equal to 2. Compared to the native population, the average IRCA applicant, based on age and sex characteristics, would be expected to commit about 2 times as many crimes as the average native.³

One limitation of the IRCA data is that the locational data that is used is the answer to a question of 'County of Intended Residency' subsequent to the IRCA. If a large amount of applicants changed locations during the legalization period, the comparisons of pre- and post-IRCA crime

³Over all counties and based solely on age and sex distributions, the average IRCA applicant would be expected to commit 1.98 times more crime overall, 1.86 times more for solely property crimes, and 2.28 times more for violent crimes.

rates in counties would not correspond exactly to the desired measure. However, as previously noted, surveys of IRCA applicants conducted after legalization showed no large trends in mobility. Furthermore, there is evidence from the United States Immigration and Naturalization Service that these applicants did not exhibit large amounts of long-range mobility in search of new labor market opportunities. Due to such evidence, I can be relatively confident in the necessary assumption that the ‘County of Intended Residency’ represents both the county of residency prior to and following the IRCA for the vast majority of IRCA applicants.

The second primary data source for this paper is the set of Uniform Crime Reports, data which are collected by the Federal Bureau of Investigation. These data are collected annually using a standardized methodology across the country, thereby providing an even-handed look across all counties. The data is collected directly from law enforcement agencies or from state reporting agencies. The data is checked for errors and for consistency by the FBI, sometimes necessitating further contact with the reporting agency and corrections to the initial reports. For the purposes of this paper, data on arrests from the annual Uniform Crime Reports (UCR) from 1980-2000, publicly available from the FBI website, are used. The arrests data contains information on what offense the perpetrator was arrested for and the race of the perpetrator. The mean value of arrests per capita by county is approximately 0.03-0.04 for all years. This data contains county level data for each year used in the categories of homicide, forcible rape, robbery, assault, burglary, larceny, motor vehicle theft, and arson. The first four of these types are classified as violent crimes and the last four as property crimes. In addition, crimes such as alcohol, gambling, and ‘other’ crimes are recorded. Using this dataset does pose a problem if crimes that are not measured by it, such as vandalism or fraud, are more often committed by undocumented immigrants or by natives. Despite some shortcomings, these measures of crimes still provide the most comprehensive national measure possible, as they are generally the most serious crimes and much of interest in and of themselves.

In addition to these primary sources, I utilize a number of other datasets to provide additional control variables for my analysis. Data on numbers of police officers or police department employees is taken from the Uniform Crime Reporting Program Data: Police Employee (LEOKA) Data. This data was obtained by an annual Law Enforcement Employees Report which was sent to police agencies throughout the country by the FBI. This gives a count of the number of full-time law enforcement employees, both officers (that is, those who are sworn, full-time law enforcement personnel with full arrest powers) and civilians, at each agency. These agencies are coded at a

Standard Metropolitan Statistical Area level, and I aggregate these numbers to the county level. This data allows me to control for the number of police officers and police employees at a county-year level. In addition, I use Bureau of Justice Statistics data regarding the number of prisoners under federal or state jurisdiction, by year and state.

Finally, other economic controls, namely county level unemployment rates and poverty rates, are used. Unemployment rates are taken from the Bureau of Labor Statistics Local Area Unemployment Statistics database. I report these unemployment rates at the county-year level when possible and at the state-year level when county level numbers are not available. Poverty rates are taken from 1990 and 2000 Census data, compiled by the Economic Research Service of the United States Department of Agriculture. These datasets give county level poverty statistics for 1989 and 1999, allowing limited controls for poverty by county and decade.

4 Results

4.1 OLS Results

My primary empirical strategy is a panel OLS measure of the impact of the IRCA, measured by the number of legalized IRCA applicants per capita, on the amount of crime by county and year.

$$CrimePerCap_{it} = \beta_0 + \beta_1 IRCA_{it} + \beta_2 W_{it} + \beta_3 Year_{1980} + \dots + \beta_{22} Year_{1999} + \beta_{23} County_1 + \dots + \beta_{3161} County_{3139} + u_{it}$$

The measure of the county level impact of the IRCA is a yearly weighted measure of IRCA applicants per capita. The weighting is done to control for the different age and sex composition of the IRCA applicants across different counties. The weighted measure describes a cumulative percentage of each county which has been legalized by the IRCA. From 1980 until 1986, this measure is 0 for all counties, as no immigrant has yet been legalized. From 1987-1990, the measure increases for counties which have IRCA applicants living in them. Finally, after the period of legalization is over, the measure remains relatively constant, only changing due to fluctuations in population a given county. Figure IV gives an example of this variable for Santa Clara County over time.

Using a cumulative measure of the legalized population has distinct advantages over using a simpler difference-in-differences or regression discontinuity design. It allows me to exploit some of

the quasi-randomness in timing of legalization to more precisely estimate effects that may occur upon legalization. As noted in Section 3, the new INS procedures caused some applications to be sent to different locations and experience wide variation in the time to approval for similar applications filed at the same time. For this reason, any effect of legalization on crime should be seen to arrive more quickly in counties which had more applications approved in 1987 relative to another county with a majority of applications not approved until 1990.

The level of crime is measured using the number of arrests per capita by year and county for each of the county-year observations. The primary specification utilizes data from crimes of all types except for drug crimes (approximately 6% of the total sample), though my results are robust to restricting the analysis to various sub-samples of crime, such as solely property or violent crimes, or to the inclusion of drug crimes.

As there have been large national shifts in the amount of crime per capita over the past decades, yearly indicators are added in order to control for nation-wide shifts in crime. In addition, county indicators are added, as the incidence of crime varies greatly across counties, from fewer than 5 crimes per year per 1000 residents to more than 100 per year per 1000 residents. The national average was approximately 25-30 crimes per year per 1000 residents. Standard errors are clustered by county.

Table I shows results from OLS regressions of the log of total arrests per capita on the cumulative amount of IRCA applicants per capita who had been legalized. Column (1) displays this regression including other county level controls as well as county and year fixed effects. I find an increase of one percentage point in the number of legalized IRCA applicants per capita (e.g. 1 legalized IRCA applicant per 100 individuals in a county) drives a fall in overall crime of 4.4%. Column (2) reports results from the same regressions with a sample restricted to counties with non-zero numbers of IRCA applications. I find a similar drop in crime, of approximately 3.9%, within this group.

Column (3) gives results without economic controls, of unemployment and poverty rates, which leaves the effect of IRCA legalizations relatively unchanged. I see a loss of significance in column (4), which does not include any year or county dummies. These results show the importance of controlling for both county-level fixed effects, and for strong time trends at the national level, which obscure a more accurate view of the effect of IRCA legalizations.

Columns (5) and (6) report results for differing subsections of arrests. Column (5) restricts its dependent variable to be a measure of arrests for solely violent crimes, while column (6) does the

same but for property crimes. Both find declines in crime, with a fall in violent crime of 2.9% and a fall in property crime of just under 5.3%.

Figure V also shows plotted coefficients from a variant of these OLS specifications. Here I regress crime per capita on economic controls as well as year dummies interacted with the total number of IRCA applicants each county saw over the entire application period. Thus, these coefficients represent the contribution of the IRCA applicant population to crime in a given county over time. If there is truly a causal effect of the legalizations, the coefficients on the number of IRCA applicants will show a distinct jump (either up or down), during and following the period of legalizations. I find a significant decline in crime associated with this population over the years 1980-1999. In the years leading up to legalization, I find evidence that the size of this population of future IRCA applicants was associated with higher amounts of crime in a county. Following the first year of legalizations in 1987, there is a dramatic drop in crime ending in 1993. After 1993, the relative contribution of this population to crime per capita in a county is indistinguishable from zero with stable point estimates near zero.

This time-profile is consistent with a causal negative impact of legalization on crime and also with the gradual transition of IRCA applicants into the labor force, substituting away from criminal activities. If the legalizations have no effect, we would expect to see coefficients which are not significantly different year to year, but cannot make a prediction on the sign or magnitude of these coefficients.

4.2 Differenced Measure and Placebo Tests

I also examine a differenced measure of crime that uses only the county variation in total number of IRCA applicants and disregard the variation stemming from the different years of legalization. This gives a measure of the change in crime between various periods as a function of the total number of IRCA applicants, legalized or not, living in each county.

$$\begin{aligned}
 & (CrimePerCap_{i1991} - CrimePerCap_{i1986}) = \\
 & \beta_0 + \beta_1(IRCA_{i1991} - IRCA_{i1986}) + \beta_2(W_{i1991} - W_{i1986}) + u_i
 \end{aligned}$$

One benefit of this specification is that it allows for placebo tests on either side of the period of legalization. I run this specification for differences in crime between 1981 and 1986, prior to the legalization, where we would expect no effect to be evident. I use the same independent variable,

the total number of eventual IRCA applicants per capita in a county. This provides for a test of long-run trends in crime by county which are correlated with the total number of IRCA applicants per capita in each county. If there were exogenous trends towards lower or higher crime in counties with high levels of IRCA applicants per capita, these placebo tests will return a non-zero result.

Columns 1-3 of Table II present results from my regressions which use only a measure of the change in crime between 1986 and 1991. These measures of change in crime are regressed on the total number of IRCA applicants per capita in each county, representing the total population which was legalized over this period. Column (1) gives results from this regression on all crime, finding a drop of approximately 1.7% in crime. Columns (2) and (3) present regressions using the differenced amount of violent and property crime during this period, respectively. While I find no significant effect for of violent crime during the period surrounding the legalizations, I do find a significant fall in property crime arrests of approximately 1.8%.

Columns 4-6 display results for placebo tests with differenced measures of changes in crime. In contrast with Columns 1-3, these regressions use differences in crime between 1981 and 1986, which saw no change in number of legalized applicants. I find no significant effects of the presence of IRCA applicants in the 1981-1986 period for either violent crime or property crime. This null result gives more confidence that there were not county-specific trends towards lower crime which were correlated with the amount of IRCA applicants in a county.

4.3 Robustness Results

In Table III, I also present a number of specifications which control for additional variables and further robustness checks. Column (1) gives results with the inclusion of urban-year dummies, finding little effect as the inclusion heightens the drop in crime per capita to 4.6%. Column (2) displays results for using a population-weighted least squares approach across counties with little effect on my results. Column (3) shows results where I restrict my sample to the years of application and legalization, 1986-1990. This method exploits more heavily the variation in timing of legalization across counties and yields a somewhat smaller estimate of the effect of legalization. This is consistent with the findings in Table II where a continued drop in crime was seen in the years following legalization. Column (4) gives results for a measure of crime inclusive of drug crimes, finding a somewhat smaller but similar and still significant result.

Column (5) adds the logged number of prisoners in a state as well as the number of police per

capita in a given county. Column (6) includes a ‘crack index’ designed to alleviate some concerns regarding correlation with the crack ‘epidemic’ during the late 1980’s. As this boom and fall in drug-related crime occurred during much the same period as the IRCA legalizations, there is a worry that the incidence of usage could be correlated with the amount of IRCA legalizations and thus bias my results. To this end, I utilize a “crack index” constructed by Fryer, et al (2006) to control for the effect of the boom and decline of the crack cocaine epidemic. This index is constructed as an annual state-level weighted set of proxies for crack cocaine usage, with the weights given by the squares of the loadings on each proxy. Proxies include things such as cocaine arrests, cocaine-related emergency room visits, crack cocaine mentions in newspapers, and DEA drug busts. The index has a strong correlation with a variety of social indicators such as homicide victimization rates among African Americans, low birth weight babies, child mortality, and overall crime rates. Additionally, I add a lagged measure of abortions by state in Column (7), as Levitt (2004) asserted this to be one potential driver of a fall in crime.

Table IV explicitly leverages the variation in timing of legalization within counties to estimate the impact of the IRCA. Given the fact that the INS was overwhelmed with applicants, some applications took much longer to process than others, even those from the same country and filed at the same time. I construct a ‘predicted’ number of IRCA legalizations per capita by year derived by regressing the legalization month on the month and county of filing. That is, I construct the predicted number of IRCA applicants that would have been legalized in a given county if each applicant experienced the average processing time to legalization, conditional on when and where they filed their application. The results of this regression can be seen in Column (1), with a point estimate nearly identical to the baseline estimation, as would be expected due to the fact that the actual IRCA per capita values are essentially random noise around this series. I then employ my baseline specification including both the predicted cumulative number of legalized immigrants per capita as well as the residual number of applicants per capita. In columns (2) and (3) I add in this residual IRCA per capita series. Here I also see a negative and significant coefficient that is statistically indistinguishable from the baseline estimate and from the predicted IRCA per capita coefficient. This results yield additional confirmation that it was the legalizations that drove crime down and not other trends or unobservable characteristics in the applicants the caused the declines since both the predictable and and ‘unpredictable’ pace of legalization led to similar declines in crime.

4.4 Instrumental Variables Results

Finally, I also perform an instrumental variables analysis to aid in addressing any omitted variables problems causing spurious correlations between IRCA applicants and crime.

I take direction from Ottaviano and Peri (2005) and Peri (2009), utilizing the distance from major sources of immigration as an instrument for the number of IRCA applicants per capita in a county. Being geographically based, there is a strong cause for exogeneity of the instrument as well as a high correlation of the instrument with the amount of IRCA applicants per capita in a county. The measure of crime does not include drug-related crimes, which may be correlated with distance to borders or ports. Distance from major ports of entry increases moving costs, inducing higher levels of immigrants to settle near these entry points. As the sources of immigration, I use the three largest ports of immigration, Los Angeles, New York, and Miami, as well as the border between Mexico and the United States.

Using GIS, I calculate the population-weighted geographical center of each county in the United States. With this data, for each county I determine the distance between its population-weighted center and the closest major immigrant port city (Los Angeles, New York, Miami) as well as to the nearest part of the Mexican-United States border (smallest distance to one of 12 segments of the border) in terms of geodesic distance, yielding two distance-based measures per county.

The second instrument consists of predicted shares of immigrants by state, based on the population of immigrants already present in states in 1950. Using nationality of origin and population data from the Census, I construct a snapshot of the population of immigrants in 1950. I then calculate the national rate of growth of each nationality and apply this growth rate to the initial population of immigrants in 1950. I thereby arrive at a predicted number and share of immigrants in each state in 1990.

For both of these instruments, I interact with year indicators in order to capture the dynamics of legalization by county over time, as each instrument is static over time at a county level.

First Stage:

$$IRCA_{it} = \gamma_0 + \gamma_1 Distances_{it} + \gamma_2 ExpectedImmShare_{st} + \gamma_3 W_{it} + YearDummies + e_{it}$$

Second Stage:

$$CrimePerCap_{it} = \beta_0 + \beta_1 \widetilde{IRCA}_{it} + \beta_2 W_{it} + YearDummies + u_{it}$$

Columns (1) and (2) of Table V report OLS results for my primary set of results as well as results restricting to solely those counties that reported a non-zero number of IRCA applicants. Column (3) reports IV results over all counties and including all instruments. Here I find a significantly higher effect of legalization, of almost 19%. Columns (4) - (6) display results restricting to counties reporting a non-zero number of IRCA applicants, as this greatly increases the power of the first stage and this sample is less prone to measurement error (counties with under 25 reported IRCA applicants were reported as having 0 applicants to preserve anonymity). Column (4) utilizes all instruments while Columns (5) and (6) use only the distance-based instruments or the predicted immigrant share instruments. Here I find results which are broadly in line with my OLS estimates, with a significant decrease in crime of over 5% due to a 1 percentage point increase in the number of legalized IRCA applicants in a county.

5 Labor Market Model

As a primary explanation of the observed decline in crime following the 1986 IRCA, I propose a formal labor market model which relates shifts in labor market outcomes due to legalization with changes in rates of crime based on evidence that crime is related to both levels of income and alternate uses of time. Thus, the additional labor market opportunities, in the form of new jobs and higher wages, available to legalized IRCA applicants, would have a significant affect on crime rates among this group by increasing income and participation rates in the legal labor force.

The model I propose is a partial equilibrium model of the labor market and crime. In the model, an agent allocates his time between four activities: formal sector employment (f), informal sector employment (i), a crime sector (r), and a job search sector (s). Participation in full-time employment is stochastic and driven by unmodeled macroeconomic trends, but is also influenced by an agent's job search effort while unemployed as well as by his choice of time spent in the crime sector. The agent gains utility from log consumption and has a quadratic utility cost of participating in the crime sector, reflecting an innate distaste for crime. Finally, the agent has a probability of being caught when committing crime that is increasing in the amount of time he allocates to the crime sector. If caught, he receives only \bar{c} consumption in the current period, and is disqualified from full-time sector employment in the following period, representing time spent in jail.

The agent maximizes:

$$(1) V = g(r_t)\log\bar{c} + (1 - g(r_t))\log c_t - \theta r_t^2 + \beta(V')$$

$$c_t \leq w_f f_t + w_r r_t + w_i i_t$$

$$w_f > w_r > w_i$$

Where w_i is the exogenously determined wage in sector $i \in$ (formal, crime, informal). By maintaining this condition on relative wages, I restrict the analysis to only cases where it is optimal to desire to work in the formal sector and where the optimal amount of time spent in the crime sector is not 0.

$$s_t + r_t + i_t = h - f_t$$

$$f_t \in (0; \bar{h})$$

As seen here, if employed in the full-time sector, this employment consumes \bar{h} of the agent's available time and the remaining $(h - \bar{h})$ of time is allocated among the other sectors. I set $g(r) = \frac{r^2}{h^2}$ such that crime increases the probability of being caught at an exponential rate, and if all available time is spent in the crime sector, the probability of being caught is 1.

The dynamics of the full-time job transition are shown here, where the top left value represents moving from formal sector work to formal sector work, the middle left represents moving from informal sector work to formal sector work, the top middle represents moving from informal sector work to formal sector work, the center shows the probability of remaining in informal sector work, while the right-most column and bottom row show probabilities of entering and exiting prison:

$$\begin{pmatrix} (1 - \gamma)(1 - g(r_t)) & \lambda(1 - g(r_t))h(s_t) & 0 \\ 1 - ((1 - \gamma)(1 - g(r_t))) & 1 - (\lambda(1 - g(r_t))h(s_t)) & 1 \\ g(r_t) & g(r_t) & 0 \end{pmatrix} \begin{pmatrix} I_{f_t} \\ 1 - I_{f_t} - I_{Prison} \\ I_{Prison} \end{pmatrix}$$

$$= (\text{Pr(Employed)} \text{Pr(Unemployed)} \text{Pr(Prison)})$$

$$g(r), g'(r), g''(r) \geq 0$$

$$h(s), h'(s) \geq 0, h''(s) \leq 0$$

γ =exogenous gross rate of formal sector separation

λ =exogenous gross rate of formal sector hiring

$g(r_t)$ =probability of being apprehended as an increasing function of time spent in the crime sector

$h(s_t)$ =multiplier which increases the probability of formal sector hiring as a function of time allocated to job search

I solve the model separately for IRCA applicants and legal residents (hereafter referred to as natives for simplicity). IRCA applicants and natives are differentiated in the model by differing access to the formal employment sector. Prior to legalization, IRCA applicants are not able to access the formal employment sector, and must divide their time between only the informal or part-time employment sector and the crime sector (having no use for the job search activity). Following legalization, they also have access to the full-time employment sector. However, after the 1986 IRCA, all IRCA applicants begin as ‘unemployed’ (participants in only the part-time/informal sector), and only gain employment over time through their own job-search efforts and natural churn in the labor market. Thus, they do not reach full steady state employment for a number of years following their legalization.

In this model, the level of crime in a year is given by $(\delta * r_a + (1 - \delta) * r_n)$ where δ is the fraction of IRCA applicants in the population and r_a and r_n are the optimal amounts of time IRCA applicants and natives/legal residents, respectively, allocate to the crime sector. Thus, the level of crime is equal to the total proportion of time allocated to the crime sector throughout the economy relative to the total amount of time available.

5.1 Model Results

To obtain a complete set of results from this model, I must solve it for a number of groups: for employed natives, unemployed natives, and for IRCA applicants prior to amnesty. Each solves:

$$V = g(r) \log \bar{c} + (1 - g(r)) \log c - \theta r^2 + \beta(V')$$

$$c_t \leq w_f f + w_r r + w_i i$$

$$V' = Pr(V_E | s, r) V_E + Pr(V_U | s, r) V_U$$

That is, utility is equal to current period utility plus the discounted utility from next period, which is equal to the weighted sum of employed utility and unemployed utility. I numerically solve for r^* and s^* , which are optimal levels of crime and job search, for Unemployed Natives, Employed Natives, and Unlegalized IRCA Applicants.

5.2 Parameterization

I calibrate parameters γ, λ, w_f , and w_i to correspond to real-world values of rates of job losing, job finding, full-time and part-time sector pay. For γ and λ , I use data from the BLS and the Global Financial Database to construct average rates of job losing and job finding. λ and γ , are both calibrated so that the net rates of job finding and job losing are equal to their true averages. That is, rates of job finding and job losing include equilibrium search time and equilibrium rates of crime in their construction, as well as the ‘gross’ rates characterized by γ and λ .

For natives, full-time pay, w_f , is taken from US Census data regarding median earnings of the full national population of adults aged 25-64 engaged in full-time work. Part-time pay, w_i is calculated from the median income of adults aged 25-64 who were not engaged in full-time work but reported non-zero income. Both values are equivalent to pay for eight-hour blocks of time. Pay for crime, w_r is set at the midpoint between these two values. For legalized IRCA applicants, full and part-time wage data is taken from US Census data on median earnings of Hispanic adults. For pre-legalization IRCA applicants, part-time wages are derived from self-reported earnings data taken from the applications.

β is the discount rate for future utility and is set at 0.95. \bar{c} is consumption while in prison, and is set at half of the consumption resulting from working for 8 hours a day in the part-time sector. θ is a parameter relating to the intrinsic dislike of spending time working in the crime sector, and is set to 0.1. I find a great deal of robustness to a range of values for these uncalibrated parameters.

A summary of all parameter values can be found in Table VIII.

5.3 Comparative Statics

For a high-level overview of the model’s implications, I compute the comparative statics of a number of variables. These results are shown in Table IX. First examining comparative statics for unlegalized IRCA applicants, I find the expected results: that the time spent in the crime sector rises as consumption subsequent to being caught rises and falls when innate distaste for crime rises.

Furthermore, I find that time in the crime sector increases when wages from crime increase, and falls when wages from the informal or part-time sector increase.

Turning to the native population, I again find similar results by numerically solving for optimal levels of time spent in the crime, job search, and informal or part-time sector. I can see that the native population responds to wage changes or parameter shifts in much the same way as does the IRCA Applicant population. In addition, both employed and unemployed natives decrease the amount of time spent in the crime sector as formal/full-time sector wages increase, as they seek to maximize the chance they keep, or find, a full-time job.

However, employed and unemployed natives diverge in their responses to changes in job losing rates and job search parameters. Whereas employed workers increase time in the crime sector if the gross job losing rate increases, due to the lessened marginal impact of crime on the net job losing rate, unemployed workers decrease time in the crime sector. Furthermore, while employed workers decrease time spent in the crime sector when γ increases, reflecting the desire to avoid a worsened spell of unemployment, unemployed workers do the opposite, increasing time spent on crime.

5.4 Estimation Results

Estimation results can be found in Table X. I give results both for the period prior to the 1986 IRCA as well as for the steady state achieved afterwards. As shown by the values for r^* in the pre- and post-IRCA periods, the overall levels of crime drop by approximately 1.75% due to the effects that legalization had on the labor market for IRCA applicants. As their wage and access to full-time employment increased, time spent searching for full-time employment rose (from 0 to 0.33) while time spent in the crime sector fell (from 0.255 to 0.06 or 0.14, depending on employment status). This led to reductions in crime in the total population. I find immediate drops in crime as all IRCA applicants are initially legalized and shift towards job search from the crime sector. After these initial drops in the year that they are legalized, there are then a further gradual declines in crime over approximately 8 years as IRCA applicants find full-time jobs and shift even further away from the crime sector.

Figure VI presents a comparison of my model's predicted changes in crime due to 1% of the population being legalized and the empirically estimated declines following 1% of a population being legalized. I find that the actual and predicted declines match fairly well, with steady crime prior to the IRCA, steep declines during the years of legalization and the years immediately following,

and relatively steady rates afterwards.

6 Conclusion

Undocumented immigration is, and will remain, an important topic in American politics and around the globe. While there have been a large number of policies aimed at lessening or dealing with the flow of undocumented immigrants, one policy that has been implemented in several instances is a general ‘amnesty’ programs. These programs give a route to legal status for large numbers of undocumented immigrants. The 1986 IRCA was one such program, eventually providing a path to legal residency in the United States for approximately 3 million people.

I find that the implications of this amnesty program on the commission of crime are large. Having one percent of a county composed of IRCA applicants who are legalized lowers crime approximately 2%-6%. This decline is higher for property crime than for violent crime, suggesting more effect on crimes with an economic motive and is robust to controls such as economic indicators, police strength, and and demographic. The fall in crime is economically significant, representing 80,000-240,000 fewer crimes committed each year due to legalization. If similar results hold true for a proposed amnesty for current undocumented immigrants, the effects would be much larger owing to the larger numbers of undocumented immigrants currently residing in the United States. Finally, I provide some theoretical guidance with a labor market model of crime, finding that this model fits the data well and that much of the drop in crime could be attributed to greater job market opportunities among IRCA applicants.

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Table I. Panel OLS

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	All Crime	Non-Zero IRCA	All Crime	All Crime	Violent Crime	Property Crime
IRCA Per Capita	-4.400*** (1.152)	-3.922*** (1.325)	-5.052*** (1.158)	1.670 (1.782)		
IRCA Per Cap, Viol. Weight					-1.443** (0.581)	
IRCA Per Cap, Prop. Weight						-2.670*** (0.811)
Observations	52,722	7,398	52,722	52,722	52,722	52,722
R ²	0.593	0.595	0.592	0.075	0.651	0.581
Year FE	YES	YES	YES	NO	YES	YES
County FE	YES	YES	YES	NO	YES	YES
Economic Controls	YES	YES	NO	YES	YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Dependent variable is log of crime per capita. IRCA Per Cap refers to the cumulative legalized weighted IRCA applicants per capita by county. Population is the logged population of each county. The economic controls are a vector composed of unemployment rates, poverty rates, county income levels and county employment levels. The unemployment rates are given by the BLS provided annual county-level unemployment rate. Poverty rate gives the official county poverty rate according to the federal definition, with the 1980s represented by the 1989 poverty rate and the 1990s by the 1999 poverty rate. County level income and employment levels are taken from Census data and are represented by the logged mean income or logged employment level in each county, by year. IRCA Per Cap Viol. or Prop. Weights are similar to the base IRCA Per Cap but utilize demographic weightings that examine solely violent or property crimes, respectively. Column (2) restricts sample to counties with non-zero numbers of IRCA applicants.

Table II. Differenced and Placebo Tests

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	86-91 - All	86-91 - Vio	86-91 - Prop	81-86 - All	81-86 - Vio	81-86 - Prop
Total IRCA Apps Per Cap	-1.669*** (0.637)	-0.916 (0.638)	-1.779*** (0.685)	0.542 (0.735)	0.618 (0.845)	0.506 (0.625)
Observations	2,418	2,418	2,418	2,356	2,356	2,356
R^2	0.009	0.013	0.008	0.005	0.005	0.008
Economic Controls	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Dependent variable is differenced log of crime per capita. Total IRCA Per Cap is the weighted total number of legalized IRCA applicants by county (that is, the cumulative total of applicants legalized in 1987-1990). Also included is log of county-level population. The economic controls are a vector composed of unemployment rates, poverty rates, county income levels and county employment levels. The unemployment rates are given by the BLS provided annual county-level unemployment rate. Poverty rate gives the official county poverty rate according to the federal definition, with the 1980s represented by the 1989 poverty rate and the 1990s by the 1999 poverty rate. County level income and employment levels are taken from Census data and are represented by the logged mean income or logged employment level in each county, by year. First 3 columns give results regarding the 1986-1991 period while Columns 4-6 give results from the period prior to legalization (1981-1986).

Table III. Robustness

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All Crime	Pop. Weight.	1986-1990	Inc. Drug Crime	All Crime	All Crime	All Crime
IRCA Per Capita	-4.697*** (1.244)	-4.945** (2.372)	-3.524*** (0.969)	-3.607*** (1.159)	-4.001*** (1.252)	-4.008*** (1.251)	-4.068*** (1.263)
Police Per Capita					6.275 (6.561)	6.534 (6.665)	6.535 (6.665)
Prisoners					-0.0744*** (0.0267)	-0.0710*** (0.0270)	-0.0735*** (0.0279)
Crack Index						-0.00872 (0.00688)	-0.00977 (0.00768)
Lagged Abortions							1.646 (4.429)
Observations	40,221	40,221	9,958	40,221	40,221	40,221	40,221
R ²	0.578	0.644	0.762	0.594	0.578	0.578	0.578
Year FE	YES	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES	YES
Urban-Year FE	YES	NO	NO	NO	NO	NO	NO
Economic Controls	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Dependent variable is log of crime per capita. IRCA Per Cap refers to the cumulative legalized weighted IRCA applicants per capita by county. Also included is log of county-level population. The economic controls are a vector composed of unemployment rates, poverty rates, county income levels and county employment levels. The unemployment rates are given by the BLS provided annual county-level unemployment rate. Poverty rate gives the official county poverty rate according to the federal definition, with the 1980s represented by the 1989 poverty rate and the 1990s by the 1999 poverty rate. County level income and employment levels are taken from Census data and are represented by the logged mean income or logged employment level in each county, by year. Police Per Capita is the number of Police Officers (non-civilian employees) by county-year, per capita. Prisoners gives the logged value of the annual number of prisoners in state or federal custody, by state. Crack index is a weighted measure of crack-related indicators by state, as constructed by Fryer, et al. (2004). Lagged abortions are given by the 14-year lagged number of abortions per capita by state (current or 7-year lagged abortions do not change results).

Table IV. Randomness of Legalization

VARIABLES	(1) All Crime	(2) All Crime	(3) All Crime
Predicted IRCA Per Cap	-4.113*** (1.316)	-4.603*** (1.403)	-4.755*** (1.415)
Residual IRCA Per Cap		-3.753*** (1.431)	-3.941*** (1.438)
Observations	49,067	49,067	49,067
R^2	0.618	0.618	0.618
Year FE	YES	YES	YES
County FE	YES	YES	YES
Economic Controls	YES	YES	YES
Other Crime Controls	NO	NO	YES

Robust standard errors in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Dependent variable is log of crime per capita. Predicted IRCA Per Cap refers to the cumulative legalized weighted IRCA applicants per capita by county predicted by regressing legalization date on filing date, county of filing, and all individual observables such as age, sex, race, and marital status. Residual IRCA Per Cap refers to the difference between the Predicted IRCA Per Cap and the actual IRCA Per Cap by county-year. Also included is log of county-level population. The economic controls are a vector composed of unemployment rates, poverty rates, county income levels and county employment levels. The unemployment rates are given by the BLS provided annual county-level unemployment rate. Poverty rate gives the official county poverty rate according to the federal definition, with the 1980s represented by the 1989 poverty rate and the 1990s by the 1999 poverty rate. County level income and employment levels are taken from Census data and are represented by the logged mean income or logged employment level in each county, by year. Other Crime Controls refer to the additional controls found in Table III.

Table V. Instrumental Variables

VARIABLES	(1) OLS - All Cty	(2) OLS - Imm Cty	(3) IV - All Cty	(4) IV - Imm Cty	(5) IV - Imm Cty	(6) IV - Imm Cty
IRCA Per Capita	-4.367*** (1.153)	-3.765*** (1.327)	-18.92*** (2.120)	-6.948*** (1.407)	-8.404*** (1.520)	-5.558*** (1.584)
Observations	52,628	7,356	52,628	7,356	7,356	7,356
Year FE	YES	YES	YES	YES	YES	YES
Economic Controls	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Dependent variable is log of crime per capita. IRCA Per Cap refers to the cumulative legalized weighted IRCA applicants per capita by county. Also included is log of county-level population. The economic controls are a vector composed of unemployment rates, poverty rates, county income levels and county employment levels. The unemployment rates are given by the BLS provided annual county-level unemployment rate. Poverty rate gives the official county poverty rate according to the federal definition, with the 1980s represented by the 1989 poverty rate and the 1990s by the 1999 poverty rate. County level income and employment levels are taken from Census data and are represented by the logged mean income or logged employment level in each county, by year. Column (2) restricts to counties with a non-zero number of IRCA applicants. Columns (3) and (4) instrument for IRCA Per Capita with distances to port cities and the Mexican border as well as predicted shares of immigrants by county based on historical immigration patterns. Column (5) uses only the distance instruments while Column (6) uses only the estimated immigrant share instruments.

Table VI: Summary Statistics, 1986

	Applicants	United States
Male	0.663	0.487
Female	0.337	0.512
Median Age	27.5	32.7
1-5 years	0.0002	0.090
6-10 years	0.025	0.072
11-15 years	0.048	0.067
16-20 years	0.138	0.075
21-25 years	0.226	0.077
26-30 years	0.200	0.087
31-35 years	0.144	0.087
36-40 years	0.088	0.078
41-45 years	0.053	0.067
46-50 years	0.033	0.053
51-55 years	0.021	0.044
56-60 years	0.011	0.042
61-65 years	0.005	0.043
66+ years	0.006	0.116

Applicant data taken from full sample of IRCA applicants during application phase in 1987. United States data taken from 1986 Census Bureau tables.

Table VII: Summary Statistics, Applications

	Number of Applications	Number Rejected
SAW Applicants	1,276,743	49,128
Non-SAW Applicants	1,762,495	96,842
Total	3,039,238	145,970

Data taken from administrative IRCA logs. SAW refers to the IRCA's Special Agricultural Worker program which provided legal status to certain types of agricultural workers present in the United States.

Table VIII: Parameter Calibration

Parameter	Native Value	Pre-IRCA Value	Post-IRCA Value
γ	.04	.04	.04
λ	.8	.8	.8
θ	.1	.1	.1
β	.95	.95	.95
\bar{c}	30	30	30
w_f	158	–	109
w_r	117	82	82
w_i	77	54	65
\bar{h}	2	2	2

Calibration methodology detailed in Section 8.3

Table IX: Comparative Statics

Parameter	Employed	Unemployed	IRCA
$\frac{r^*}{d\bar{c}}$	≥ 0	≥ 0	≥ 0
$\frac{r^*}{dw_r}$	≥ 0	≥ 0	≥ 0
$\frac{r^*}{dw_i}$	≤ 0	≤ 0	≤ 0
$\frac{r^*}{dw_f}$	≤ 0	≤ 0	--
$\frac{r^*}{d\theta}$	≤ 0	≤ 0	≤ 0
$\frac{r^*}{d\gamma}$	≥ 0	≤ 0	--
$\frac{r^*}{d\lambda}$	≥ 0	≥ 0	--

r^* is the optimal level of crime for either employed or unemployed natives. w_f , w_i , and w_r refer to the wages of formal sector, informal sector, and crime sector workers. \bar{c} refers to consumption in prison. θ is a parameter reflecting a distaste for crime. γ and λ refer to the rates of separation and hiring.

Table X: Model Results - Time Allocations

Type	s^*	r^*	i^*	f^*
Native Employed	0	.05	.95	1
Native Unemployed	.43	.13	1.44	0
IRCA Applicant Pre-IRCA	0	.255	1.745	0
Total Population	.03	.057	.99	.921
IRCA Employed Post-IRCA	0	.06	.94	1
IRCA Unemployed Post-IRCA	.33	.14	1.53	0
Total Population Post-IRCA	.03	.056	.984	.93

Total Population represents a mix of 99% Native and 1% IRCA Applicants. The Native population consists of 93% employed and 7% unemployed. Post-IRCA numbers are taken as the long term steady state values achieved after several periods of job finding by IRCA Applicants.

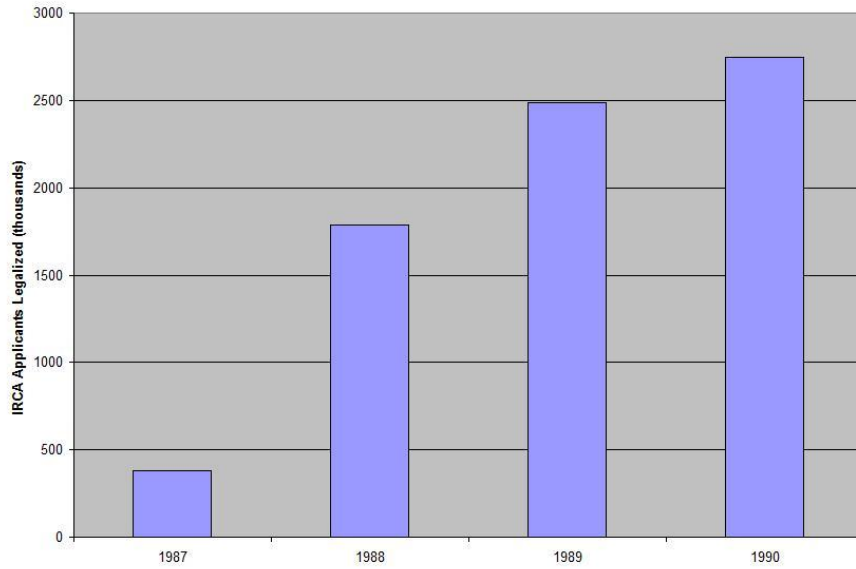


Figure I: Cumulative Number of IRCA Applicants Legalized

Bars represent the total number of immigrants legalized through the IRCA program in the United States over time. The program ceased accepting applications in 1988 but did not legalize the final applicants until 1990.

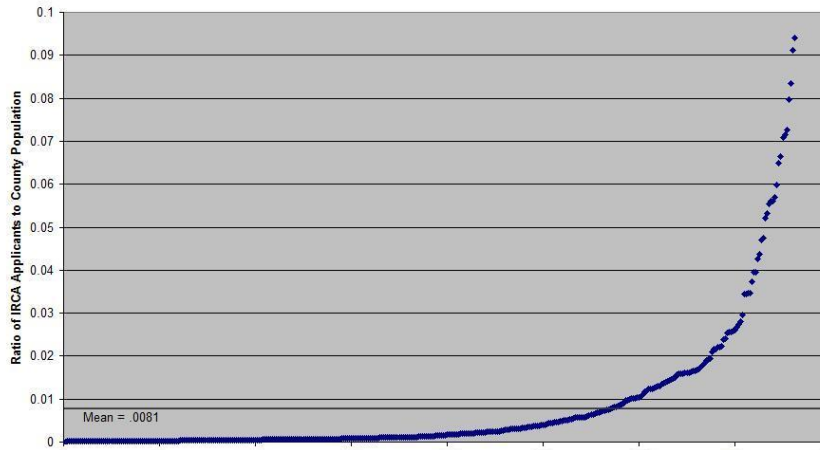


Figure II: Number of IRCA Applicants Per Capita

Only shown are counties with positive numbers of IRCA applicants. These counties represent 12% of US counties and 70% of the total US population.

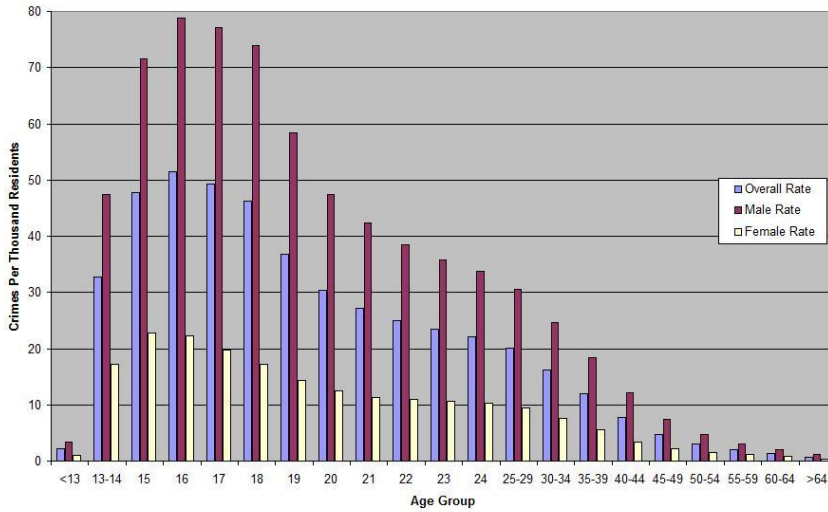


Figure III: Rates of Crime by Age and Sex

Data taken from FBI Uniform Crime Reports data on arrest records. Numbers represent national aggregates by age and sex.

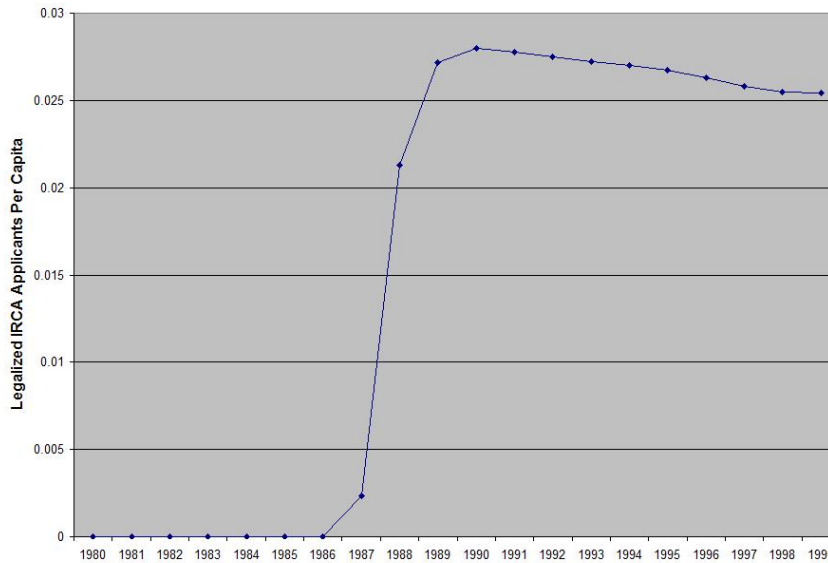


Figure IV: Legalized IRCA Applicants Per Capita, Santa Clara County

Example of the cumulative legalized IRCA applicants per capita metric used. Metric is zero prior to the IRCA, quickly rising as IRCA applicants are legalized during 1987-1990, and then slowly declining after 1990 as the population grows but no new undocumented immigrants are legalized.

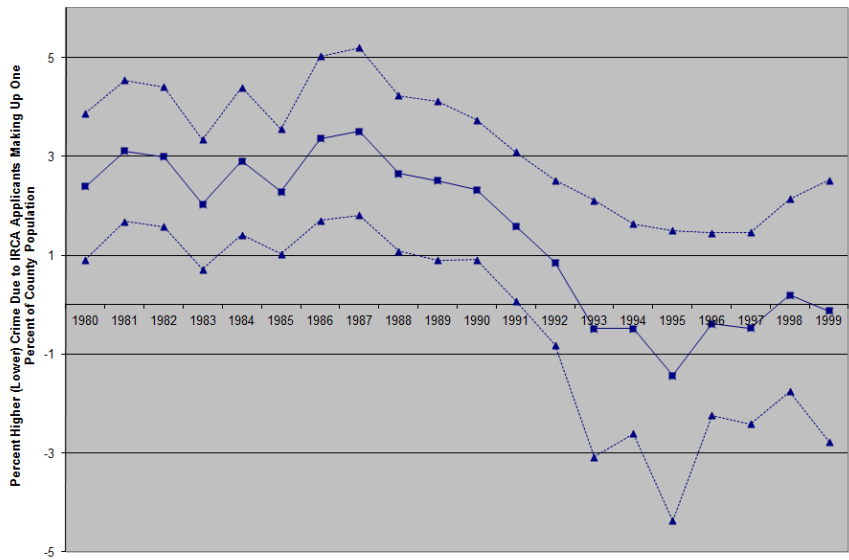


Figure V: Effect of IRCA on Crime Per Capita

Graph displays coefficients from a regression of the total weighted number of IRCA applicants per capita on crime per capita, interacted with yearly dummies. Dotted lines represent 95% confidence bands (1.96 SE bands).

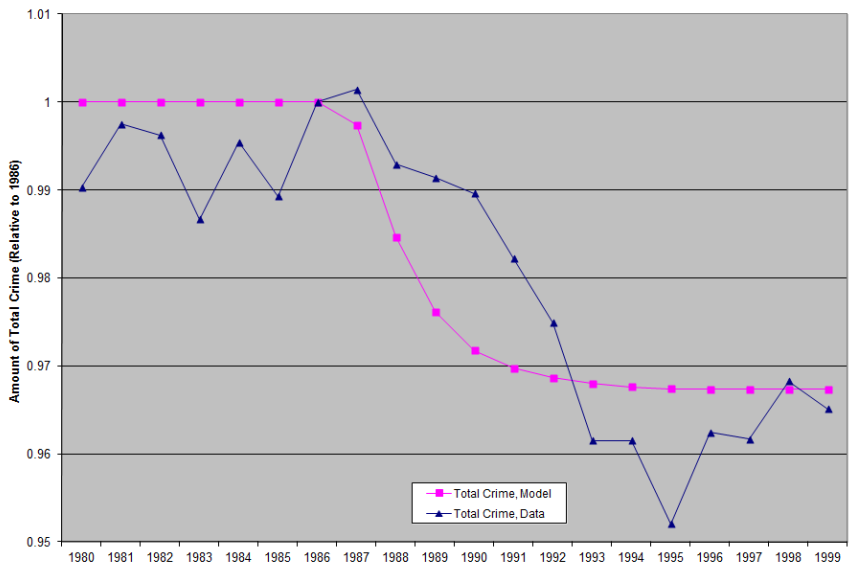


Figure VI: Modeled and Actual Change in Crime

The blue 'Data' line denotes coefficients from a regression of the all-time weighted number of IRCA applicants per capita on crime per capita, interacted with yearly dummies. The pink 'Model' line denotes the path of crime from the calibrated model described in Section 5. Crime is measured relative to 1986, before the IRCA took effect.

A Model Appendix

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For an employed native ($f_t = 1$), the value function to be solved is:

$$\begin{aligned}
 V_E &= g(r)\bar{c} + (1 - g(r))(w_f + w_r r + w_i(1 - r - s)) - \theta r^2 + \beta(V') \\
 &= g(r)\bar{c} + (1 - g(r))(w_f + w_r r + w_i(1 - r - s)) - \theta r^2 + \beta(\text{Prob}(V_E|s, r)V_E + \text{Prob}(V_U|s, r)V_U) \\
 &= g(r)\bar{c} + (1 - g(r))(w_f + w_r r + w_i(1 - r - s)) - \theta r^2 + \beta((1 - \gamma)(1 - g(r))V_E + (\gamma + g(r) - g(r)\gamma)V_U) \\
 &= \frac{g(r)\bar{c} + (1 - g(r))(w_f + w_r r + w_i(1 - r - s)) - \theta r^2 + \beta((\gamma + g(r) - g(r)\gamma)V_U)}{(1 - \beta * (1 - \gamma) * (1 - g(r)))}
 \end{aligned}$$

For an unemployed native ($f_t = 0$), it is:

$$\begin{aligned}
 V_U &= g(r)\bar{c} + (1 - g(r))(w_f + w_r r + w_i(1 - r - s)) - \theta r^2 + \beta(\text{Prob}(V_E|s, r)V_E + \text{Prob}(V_U|s, r)V_U) \\
 &= g(r)\bar{c} + (1 - g(r))(w_f + w_r r + w_i(1 - r - s)) - \theta r^2 + \beta(\lambda(1 - g(r))h(s)V_E + (1 - \lambda(1 - g(r))h(s))V_U) \\
 &= \frac{g(r)\bar{c} + (1 - g(r))(w_f + w_r r + w_i(1 - r - s)) - \theta r^2 + \beta(\lambda(1 - g(r))h(s)V_E)}{1 - \beta(1 - (\lambda(1 - g(r))h(s)))}
 \end{aligned}$$

For an IRCA Applicant (always ‘unemployed’), it is:

$$\begin{aligned}
 V_I &= g(r)\bar{c} + (1 - g(r))(w_f + w_r r + w_i(1 - r)) - \theta r^2 + \beta(V_I) \\
 &= \frac{g(r)\bar{c} + (1 - g(r))(w_r r + w_i(1 - r)) - \theta r^2}{1 - \beta}
 \end{aligned}$$

For an IRCA Applicant, there exists no ability to work at a full-time, formal job and thus the applicant has no use for the job search sector, supplying time to only the crime and informal or part-time sector. For analytical simplicity, I take $g(r) = \frac{r}{2}$, such that crime increases your probability of being caught and if you spend all of your time in the crime sector, you have a probability one of being caught. I assume parameters are such that I do not have corner solutions. These simplifications can be considerably relaxed without greatly affecting the computational results. Computational

results are also computed using log utility and more complex functions of $g(r)$ and $h(s)$, as long as the functional conditions described above hold.

With this simplified case, I can solve analytically for r^* , differentiating V_I with respect to r and finding the optimal allocation of time to the crime sector:

$$\begin{aligned}\frac{dV_I}{dr} &= \frac{\frac{\bar{c}}{2} + (w_r - w_i) - rw_r - \frac{w_i}{2} + rw_i - 2\theta r}{1 - \beta} = 0 \\ \rightarrow \frac{\frac{\bar{c}}{2} + (w_r - \frac{3w_i}{2})}{w_r - w_i + 2\theta} &= r_I^*\end{aligned}$$

As an additional example, I analytically compute comparative statics for unlegalized IRCA Applicants. See Table IX for all comparative statics results.

$$\begin{aligned}\frac{r_I^*}{d\bar{c}} &= \frac{1}{2(w_r - w_i + 2\theta)} && \geq 0 \\ \frac{r_I^*}{dw_r} &= \frac{1}{w_r - w_i + 2\theta} - \frac{\frac{\bar{c}}{2} + w_r - \frac{3w_i}{2}}{(w_r - w_i + 2\theta)^2} && \geq 0 \\ \frac{r_I^*}{dw_i} &= \frac{-2}{3(w_r - w_i + 2\theta)} + \frac{\frac{\bar{c}}{2} + w_r - \frac{3w_i}{2}}{(w_r - w_i + 2\theta)^2} && \leq 0 \\ \frac{r_I^*}{d\theta} &= \frac{-2(\frac{\bar{c}}{2} + (w_r - \frac{3w_i}{2}))}{(w_r - w_i + 2\theta)^2} && \leq 0\end{aligned}$$

A.1 Model Variants

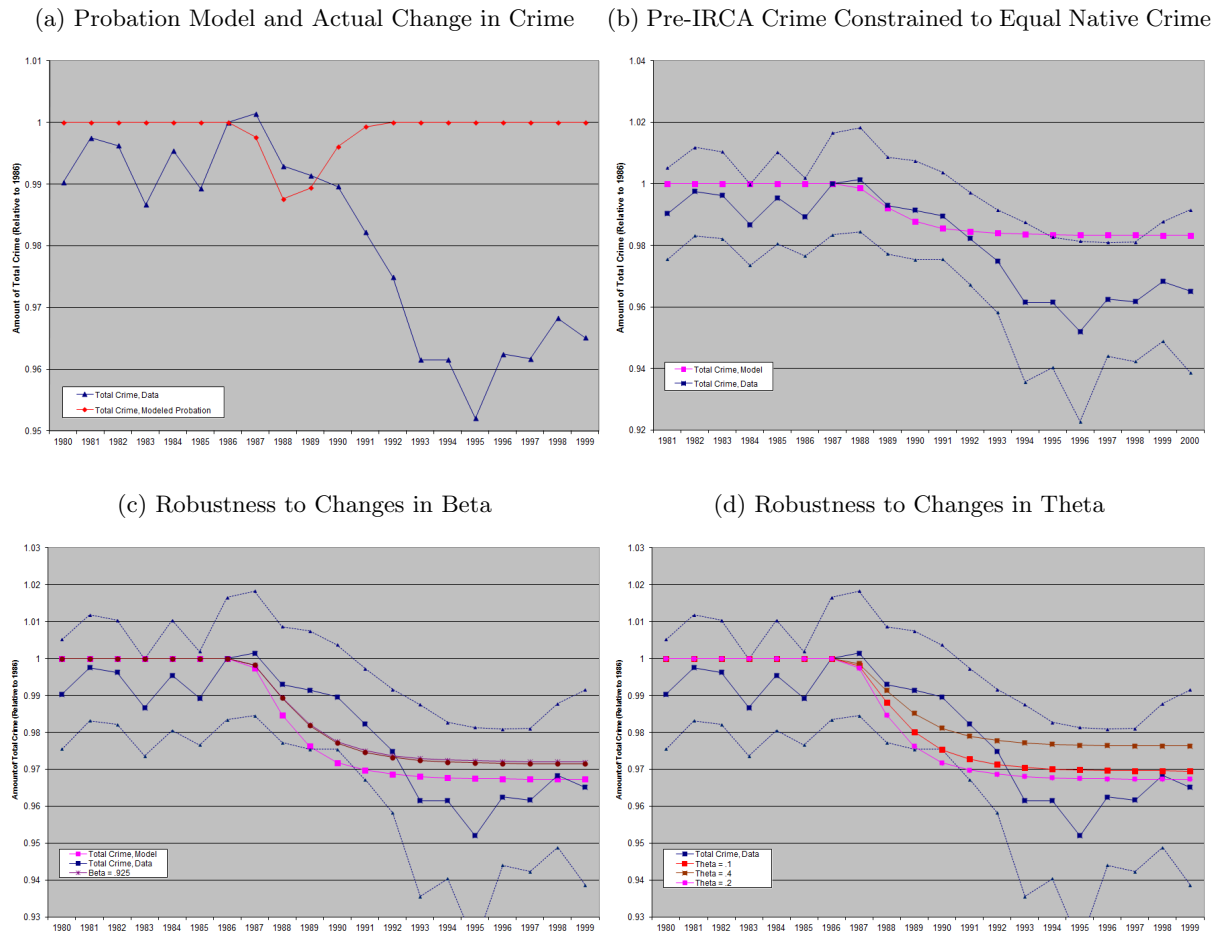
One alternate channel which can be investigated is that of a temporary decrease in crime due to the desire not to be caught committing a crime during the temporary residency period. During this period, if caught committing three misdemeanors or a single felony, the IRCA applicant's temporary residency would be cancelled and their opportunity to become a legal resident would end. In Figure VIIa, I display results testing this explanation. Here, crime among IRCA applicants declines only in the 18 months following their legalization and returns to normal thereafter. I find that this explanation does not match the observed decrease in crime due following legalization. Thus, it is unlikely that this channel alone is responsible for the decline in crime. However, the decline in crime could have been partially driven by this channel, with probation driving individuals to temporarily leave the crime sector and enter the newly available formal labor sector which they then remained in even after the probationary period expired. This would argue for any future legalization program being paired with a similar probationary period to achieve the same ends, but

the results suggest that a probationary period alone could not account for the persistent decline in crime observed through the late 1990s.

Figure VIIb gives numerical results from a modified model in which pre-legalization crime among IRCA applicants is constrained to be equal to that of natives. We find a similar pattern as in the data, but a decline in crime which is smaller in magnitude than the decline we observe in the data.

Figures VIIc and VIId display results of utilizing alternate parameters for both β and θ . I find similar results for a range of alternate values.

Figure VII: Model Variants



A Labor Market and Migration Effects: Evidence from LPS

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Examining the twin Legalized Population Surveys (LPS1 and LPS2) gives some concrete ideas of the effects of legalization on this cohort of IRCA applicants. The surveys were conducted in 1989 and 1992 and covered approximately 6000 and 4000 respondents, respectively. The respondents were taken solely from the pool of non-SAW applicants and covered 44 of 50 states and the District of Columbia (there were no respondents from Maine, Montana, North Dakota, New Hampshire, South Carolina, or South Dakota). The surveys asked a wide variety of questions about education, employment, language proficiency, family, and health in the period just prior to application, the period around the first survey in 1989, and the period around the second survey in 1992.

In Table XI, I see responses to questions regarding migration of respondents after legalization. I see that the vast majority of respondents either did not move or moved only within a ZIP code, thereby remaining in the same county. A smaller number moved across ZIP codes but within the same state. However, many or even most of these moves are likely within the same county as well, as there are approximately 40,000 ZIP codes in the United States, whereas there are just over 3,000 counties. This is especially true in some of the counties where a large number of IRCA applicants lived (e.g. Los Angeles County has 522 ZIP codes). Only a small fraction of respondents definitely changed counties, moving across state lines. With these data in mind, I can be more confident in assigning IRCA applicants to certain counties and not worrying about drastic changes in migration patterns following their legalization.

Table XII presents some evidence of the strong effects of legalization on labor market outcomes among IRCA applicants. Fully 75% of respondents reported that having legal status made it ‘Somewhat’ or ‘Much’ easier to find work. In addition, about 60% reported that legal status made it ‘Somewhat’ or ‘Much’ easier to advance in their current job. Only a few percent reported legal status hurting them in either of these categories. Such stark results speak to the importance of legal status in the labor market, and that the transition to being a legal resident could help labor market outcomes in and of itself.

In conjunction with these self reports, Table XIII gives an idea of the magnitude of the changes in income following legalization. I find weekly wages in comparable age groups are, in general, 30-40% higher in the years following legalization. Moreover, hourly wages increased by even more

than weekly wages, well over 40%, showing that IRCA applicants, after legalization, were able to both boost earnings and leisure time.

The entirety of these increases in income cannot be attributed solely to the act of legalization. In the years after the IRCA, many respondents also attended additional classes, increasing English skills or continuing their educational attainment. Table XIV gives some statistics regarding additional education IRCA applicants undertook following their legalization. I find that approximately one third of respondents took additional classes in English while over one seventh pursued additional academic education. In addition, other IRCA survey data shows that the average years of education increased by over one year in the five years following legalization, with this increase holding true even for those far past secondary education age. Overall, this speaks to the increases in skills seen by IRCA applicants in the years after their legalization.

In summary, these survey results provide much evidence for increases in human capital and labor market opportunities among IRCA applicants following legalization. Such experiences could potentially drive large changes in the likelihood of engaging in criminal behavior among these individuals, as I explore in Section 5. Income effects and ‘incapacitation’ due to time constraints imposed by full time work could both act to discourage crime and other anti-social behavior. There is evidence that undocumented immigrants worked for significantly fewer hours than did legal immigrants with similar levels of education and skills, presumably due to lower labor mobility and a weaker bargaining position. Furthermore, these undocumented immigrants were unable to obtain any formal employment with a wide array of government agencies as well as private companies which did conduct screenings for legal status. Since IRCA applicants were not able to take full advantage of the labor market opportunities available to legal immigrants and citizens, some criminal activity may be more enticing and relatively more profitable for them to engage in. Moreover, they were unable to receive federal and state unemployment, medical, and other benefits which they gained access to in the years following legalization. This large expansion in their labor market opportunities in the legal sector could most likely be expected to exert downward pressure on crime among this group.

Table XI: Post-Legalization Migration

	1989 Survey	1992 Survey
Did Not Move	4740	1720
Moved Within ZIP	575	855
Moved Within State	798	1335
Moved Outside State	80	102
Total Respondents	6193	4012

Answers taken from LPS1 and LPS2 surveys conducted on IRCA applicants after their legalization in 1989 and 1992. The third category denotes number who changed ZIP codes but not States. For many, this could constitute a move within the same county but across ZIP codes.

Table XII: Self-Reported Effects of Legalization

	On Ability to Find Work	On Ability to Advance in Job
Made it Much Easier	1098	1380
Made it Somewhat Easier	510	885
No Effect	418	1355
Made it Somewhat Harder	52	37
Made it Much Harder	18	13
Total Respondents	2096	3670

Answers taken from LPS2 survey conducted on IRCA applicants in 1992, subsequent to their legalization. First column denotes response to question “How has receiving legal status effected your ability to advance at work?” Second column denotes response to question “How has receiving legal status affected your ability to get work?”

Table XIII: IRCA Applicants' Weekly Wages and Education Levels

Age	Weekly Wages, 1987	Weekly Wages, 1992	Years of Education, 1987	Years of Education, 1992
<20	177.41		10.41	
20-25	219.91	301.51	8.70	12.05
25-30	258.60	345.39	8.81	9.59
30-35	283.72	356.59	8.68	9.51
35-40	293.89	383.26	8.69	9.48
40-50	301.19	383.86	7.83	9.08
50-60	246.20	322.59	6.40	7.52
60+	206.10	250.73	4.73	6.38

Answers taken from LPS1 and LPS2 surveys conducted on IRCA applicants after their legalization in 1989 and 1992. 1987 wages denote weekly wages reported for the week of application, actual date varies somewhat. 1992 wages denote weekly wages reported at the time of 1992 LPS2 interview. Education question asks for number of years of education. Maximum in 1987 was 18, maximum in 1992 is 20 years. Individuals with reported level of education in 1992 less than that in 1987 are removed from the sample.

Table XIV: IRCA Applicants' Additional Classes by 1992

	Additional Education?	Additional English Classes?
Yes	552	1262
No	3326	2746

Answers taken from LPS2 survey conducted on IRCA applicants in 1992. Questions ask if respondents have taken any classes which could be credited towards a degree or diploma and if the respondent has taken any English classes in excess of the required classes.