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

by

Scott R. Baker

Stanford Institute for Economic Policy Research
Stanford University
Stanford, CA 94305
(650) 725-1874

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

Scott R. Baker
Economics Department
Stanford University

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Abstract

In the late 1970's, rates of undocumented immigration into the United States increased dramatically. This increase led to pressure on the federal government to find some way of dealing with the immigrants, culminating in the 1986 Immigration Reform and Control Act (IRCA). This paper seeks to examine the effects that the 1986 IRCA, which legalized over 2.5 million undocumented immigrants, had on the commission of crime in the United States. Using administrative data from the IRCA application process, I find evidence that IRCA applicants are associated with higher crime rates prior to legalization and that, subsequent to legalization, this association disappears. I find national decreases in crime of approximately 2%-5% associated with one percent of the population being legalized, primarily due to a drop in property crimes. This fall in crime is equivalent to 160,000-400,000 fewer crimes committed each year due to legalization. Finally, I calibrate a labor market model of crime using empirical wage and employment data and find that much of the drop in crime could be explained by greater job market opportunities among those legalized by the IRCA.

JEL classification: F22, J22, K42

*Contact: Scott R. Baker, Stanford Economics Department, 579 Serra Mall, 415-244-8274, srbaker@stanford.edu. Thanks to Linda Baily for generously sharing the IRCA data and to Ronald Lee, Stefano DellaVigna, Nick Bloom, Caroline Hoxby, Ran Abramitzky, David Card, David Romer, and seminar participants from UC Berkeley, Stanford Economics, and Stanford Law School for their comments

1 Motivation

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. I provide estimates of the total effect of this legalization and provide some credible idea of the potential effects on crime of a new amnesty bill through a variety of specifications, exploiting variation in both the geographical distribution and quasi-random timing of the legalizations. While I cannot disentangle all channels through which this change took effect, I give theoretical evidence for one major channel. I present a model of job search which links labor market opportunities and crime, calibrating this model and matching its predicted outcomes against county-level empirical crime data. Despite an inability for precise quantification of which channels the decrease in crime takes effect through, the reduced form impact is still of great interest. 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 within a country stemming from similar amnesty programs.

Section 2 discusses some of the previous literature regarding undocumented immigration and crime and related fields. Section 3 gives background information on the 1986 IRCA and details the components of the act and its effects on the applicant population in terms of crime and the broader economic and social developments surrounding it. Section 4 describes the data and Section 5 outlines my empirical strategy and primary empirical specifications. Section 6 provides an overview of a number of robustness tests undertaken to strengthen my main findings while Section 7 presents my empirical results. Section 8 outlines a labor market model and its solution as well as comparing the calibrated results to those from the empirical analysis. Section 9 concludes.

2 Literature

Both before and after this 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. Innumerable labor market studies have been conducted, a few of which are Dolado et al (1997), Djajic Slobodan (1997), Friedburg and Hunt (1995), and George Borjas (2003, 2005, 2006), and Passel (2006).

Two such labor market papers 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. She uses a standard difference-in-difference approach across metropolitan statistical areas to judge various labor market effects such as income, hours, and employment. In addition to its relevance in terms of methodology, Bailey's paper also gives estimates of the impact of the IRCA on the labor market, finding little effect 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.

Also instructive is David Card's (1990) paper on the Mariel Boatlift, which exploits the massive influx of Cubans into the Miami area as a natural experiment, examining labor market outcomes among native groups. However, due to problems of identification and variation, Card cannot identify the exact locations of Cuban immigrants within the city or in the larger area. He can only provide statistical comparisons between demographic groups within the city and between selected 'comparison' cities, a problem which my dataset solves through more precise location data.

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. He theorized that this is due to the social welfare system of Sweden, which is more generous towards natives and non-first generation immigrants, perhaps helping to alleviate some economic causes of crime. 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.

Additionally, Bell, Machin, and Fasani (forthcoming) 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.

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. This demonstrates the possibility that in some instances, negative effects on labor market outcomes will occur for groups competing with increased levels of immigrants.

Finally, of particular interest is recent work by Mastrobuoni and Pinotti (2012), who 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. They find that recidivism is lower among economically-motivated immigrants with legal status than those without legal status when living in areas with better labor market access to immigrants with 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. This paper is able to take advantage of a national program to examine the effects of a legalization on a much larger scale and scope. In addition, in contrast to their static labor market model, I employ a dynamic labor market model to explain some of the gradual sorting out of the crime sector over time.

This paper represents the first analysis of the effects on criminal behavior of a large, national legalization program using administrative data from the IRCA applications to provide a firm picture of the individual undocumented immigrants in question. The data collected from the applications to the 1986 IRCA provide county-level locations for almost 90% of the applicants. These data was obtained by the federal government during the implementation of the IRCA from the information contained in millions of applications submitted by undocumented immigrants desiring legalization of status. This paper uses these comprehensive data on location and other demographic characteristics of the IRCA applicants to study the changes in patterns of crime during the years preceding and those following the passage of the IRCA.

3 Background

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 2.7 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.

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 so it does not seem likely that any 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 at 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 entire INS operation was intended to be standardized across the country, with no strong regional variation in the decisions regarding similar legalization applications. The practice of 'remoting' and the unfamiliarity of the INS with the massive undertaking meant the application approval process took much longer for some applicants than others. In essence, it meant that of two IRCA applicants who both applied in mid-1987, one might be legalized by the end of 1987 and the other remaining without legal status for up to 3 additional years.

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 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.

In addition to the amnesty provisions, the bill made it illegal for an employer to knowingly hire an undocumented immigrant. However, in order to combat discrimination arising from this requirement, it also made it illegal to discriminate in hiring based on country of origin of an employee. In practice, both of these requirements were not well enforced and did not greatly constrain employers. Finally, the bill also provided for a large (over 50%) rise in Border Patrol

spending in an effort to better restrict the flow of undocumented immigrants.

3.1 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 [2004]. 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 leaving to find 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 meaningful effects of the bill to be concentrated on the legalization provisions and increased Border Patrol spending.

In the years following the passage of the IRCA, a number of surveys have pointed to increases in

⁵The 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.

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 in legal status, not to increases in education, language skills, or other observables. In other work, Kossoudji and Cobb-Clark [200] 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 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. Such evidences points to newly invigorated competition for IRCA applicants’ labor and the ability to voluntarily shift jobs, not minimum wages, which pushed up wages.

One important conclusion from this literature, combined with results of Bailey’s work, is that 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 no 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.

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 Medi-Cal 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.

3.2 Evidence from LPS

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 XIV, 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 XV 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

⁸The 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.

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 XVI 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. Tables XVII and XVIII give 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. Indeed, I find 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.

3.3 Possible Effects on Crime

There are a number of channels through which the 1986 IRCA could have affected crime. While I cannot disentangle all possible channels, I provide some evidence regarding a number of them.

One possible channel, which I explore later in more detail, is that of the labor market. 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.

Another channel through which crime could be affected is through changes in family structures of IRCA applicants following legalization. Legalization preceded the reconnection of families, as men brought their wives into the United States after their temporary probationary period ended and they were able to sponsor family members as legal residents. Laura Hill and Hans Johnson describe

a miniature baby boom in California caused by this influx of wives of newly legalized residents. Hill and Johnson [2002] note that “research indicates that many of those granted amnesty were joined later by spouses and relatives in the United States. As a result, many young adult Hispanic women came to California during the late 1980s.” In addition to the arrival of wives, they describe the hurried formation of families: “because many of those granted amnesty and their spouses had been apart for some time, their reunion in California prompted a ‘catch-up’ effect in the timing of births.” These changes could have prompted declines in crime among IRCA applicants, as research has generally shown declines in criminal activity upon marriage.

Changes in the relationship with police could also have affected the amount of crime committed by and against IRCA applicants. Greater trust of police after legalization of their status may have caused IRCA applicants to report more of the crimes committed against them. This would drive a positive relationship between higher levels of IRCA applicants and higher reports of crimes in that county. However, greater cooperation with police by IRCA applicants, following legalization, may have increased police effectiveness and thereby decreased crime within and against the legalized community. This change in relationship with local police forces could be part of a change in the attitudes of the IRCA applicants, who may now feel more at home in a community and less likely to engage in anti-social activity.

Finally, the provisions of the IRCA may have had a direct influence on crime rates among applicants. The Act stipulates that the temporary residency status may be terminated if an applicant is convicted of any felony or any three misdemeanors within the United States. Due to this qualification, the 18-month long temporary residency period could be expected to produce less crime among applicants as they sought to avoid deportation and the loss of potential permanent residency. The loss of potential permanent residency may have acted as a stimulus to avoid criminal behavior for at least 18 months and led to the applicant seeking gainful employment for at least this period and possibly beyond.

4 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. Well over 90% 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. While there have been many estimates of the size and geographic distribution of the undocumented immigrant population, Robinson (1980) and Hanson (2006), this paper sidesteps the need for such estimations as it uses the 1990 Legalization Summary Tapes, and can therefore accurately count every undocumented immigrant who applied to the 1986 IRCA.

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 differs greatly from that of the general population, 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 highly dependent on age, sex, marital status, and income. In a brief examination of my applicant data, I find that the mean ratio of IRCA applicants to county population, displayed in Figure II, is approximately .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 country average, which had a median age 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 vast, as seen in Figure III. For example, a county which had an applicant population of solely elderly women would not be expected to see large changes in crime following legalization. Comparisons of demographic statistics between the IRCA applicant population and national averages can be found in Figure IV and Table IX. Information on the weighting procedure is described in more detail in Section 6.

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 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.³ 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 standard 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, annual Uniform Crime Reports (UCR) from 1980-1999, publicly available from the FBI website, are used. I will use data on the total number of arrests by county as well as Total Crime Reports by county. 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 .03-.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. A summary of the numbers of crimes of each type is found in Figure V. 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. Unfortunately, the data on Total Crimes Reported does not contain information regarding drug crimes as the UCR arrest data does. 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.

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.

5 Empirical Strategy

My primary empirical strategy is an OLS measure of the impact of the IRCA on the amount of crime by county and year.

$$CrimePerCap_{it} =$$

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

$IRCA$ = Number of IRCA applicants per capita legalized by year and county

$Year_j = 1$ if $t = j$

$County_j = 1$ if $i = j$

$i = 1, 2, \dots, 3139$

$t = 1981, 1982 \dots, 2000$

W_{it} = Demographic and economic county-level controls

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. This weighting is described in more detail in the following section. 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 VI 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 60,000 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, year indicators are added in order to control for nation-wide shifts in crime. In addition, county indicators are added in order to control for county specific levels of crime. 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. Finally, I cluster standard errors by county to aid in accounting for measurement error arising at the county-reporting level.

6 Robustness Tests

Given a lack of ideal individual data on criminal behavior and immigration status, I present a number of robustness tests and alternate specifications in an effort establish the effect of legalization on crime is a causal one.

6.1 Demographic Weighting

One important component of the measure of IRCA applicants per capita is a weighting utilizing the age and sex of each county's IRCA applicants. This allows for the control of the differential potential to commit crimes by residents of varying age and sex. 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. This weighting allows for a more accurate comparison of the groups of applicants across counties and across time.

The weighting is 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.⁴ ver all counties and based on age and sex characteristics, 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.

¹⁴O

6.2 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, measured in levels or logs of levels, as a function of the total number of IRCA applicants living in each county.

$$\begin{aligned} & (\textit{CrimePerCap}_{i1991} - \textit{CrimePerCap}_{i1986}) = \\ & \beta_0 + \beta_1(\textit{IRCA}_{i1991} - \textit{IRCA}_{i1986}) + \beta_2(W_{i1991} - W_{i1986}) + u_i \end{aligned}$$

\textit{IRCA}_{it} = Number of IRCA applicants per capita legalized by year and county

$i = 1, 2, \dots, 3139$

W_i = Other demographic and economic county-level controls

The cumulative number of legalized applicants in 1991 is the entire population of IRCA applicants in a county, since all legalizations took place between 1987 and 1990. The number of legalized applicants in 1986 is uniformly 0 for all counties. Thus, the differenced measure of legalized applicants is equal to the total number of eventual IRCA applicants per capita in a given county.

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, as well as between 1991 and 1996, after all applicants had already been legalized. For each, I use the same independent variable, the total number of 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. Zero results suggest that it was the legalizations which had an impact on crime and not simply the presence of IRCA applicants or other county-specific trends correlated with their presence. I present the results of this specification in Table II.

6.3 Various Measures of Crime

Another important test of robustness is to restrict my measure of crime in various ways. It is important to separately consider different types and measures of crime, such as solely violent crime, property crime, or drug crime. Such differing measures could vary significantly in their response to the IRCA legalizations. If crime among undocumented immigrants was concentrated within a certain category, or if the legalization affected crime primarily through a channel which affects a single category, one would expect to find disparate results for different categories of crime.

For example, if the majority of crime among undocumented immigrants was property crime such as car thefts and robberies due to poorer economic circumstances, a positive shift in the labor market for IRCA applicants may have the effect of lowering property crime rates while leaving violent crime rates unchanged. In contrast, if there was an outsized level of violent crimes against undocumented immigrants, legalization may bring decreases in violent crime but not property crime.

6.4 Yearly Regressions

I will also run tests on single year subsets of my data with the independent variable the total number of IRCA applicants (not just the subset legalized in that year) per capita who reside in a county and as the dependent variable, the level of crime per capita in the county. This specification returns the correlation between IRCA applicants per capita and crime per capita, by county, for each year. 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 the period of legalizations. This is due to the fact that the same number of IRCA applicants per capita represents a group which is being legalized.

For instance, with the hypothesis that legalization will decrease crime to the level among natives from a higher initial level, one would expect to see significantly positive coefficients on the number of IRCA applicants per capita in the pre-legislation period, followed by a sharp decrease during 1987-1990, and then coefficients statistically indistinguishable from zero in the post-legalization period. If the legalizations have no effect, I 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.

6.5 Police and Prison Data

One additional regressor which I add is the number of police officers and number of police department employees by county. These are obtained from the Law Enforcement Officers Killed and Assaulted (LEOKA) data, which are collected by the FBI and compiled by the United States Department of Justice. All jurisdictions are subject to some mandatory disclosure programs and report numbers of officers employed as well as various statistics about officers killed or assaulted in the line of duty. I utilize this data to control for any change in police levels which is correlated with the number of IRCA legalizations in a county. If the number of police officers decreases as a result of predictions of less crime due to legalizations, I may see no change in crime. Or, a decrease (increase) in crime after the IRCA passes may be due to increased (decreased) police presence in neighborhoods populated by IRCA applicants.

I also utilize state prison data in order to control for the effects of incarceration on crime. Other studies have found that greater levels of incarceration have led to decreases in crime (Donohue and

Levitt, 2001), and may be correlated to numbers of IRCA applicants in state. I use Bureau of Justice Statistics data regarding the number of prisoners under federal or state jurisdiction, by year and state, in order to control for these effects.

6.6 Drug Crime

I also utilize various specifications in order to alleviate some concerns regarding correlation with the crack ‘epidemic’ during the late 1980’s. As this boom and fall in crack-related crime occurred during much the same period as the IRCA legalizations, there is a worry that the incidence of crack usage could be correlated with the amount of IRCA legalizations and thus bias my results. While my main specification already reports results which omit all drug crimes from my measure of the dependent variable, I also wish to perform alternate robustness tests in order to strip out more of this influence. I present results controlling for the percentage of each county that identified as African-American, as this was the demographic which was primarily involved in this activity.

Additionally, I utilize a “crack index” constructed in Fryer, et al (2006) to control for the effect of the boom and decline of the crack epidemic. This crack index is constructed as an annual state-level weighted set of proxies for crack 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 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. I use this index to control for the crack-cocaine epidemic across the nation.

Finally, I present two modified specifications to assess my results’ sensitivity to various treatments of drug crime. First, I present a specification in which my crime variable includes all drug crimes. Secondly, I use a specification which includes drug crime as a right hand side variable to see if the increase in crime could possibly just be a correlation with contemporaneous increases in drug crime.

6.7 Abortions

Another notable potential cause of a change in crime rates is the dramatic rise in abortion rates in the years before the 1986 IRCA. As asserted by Donohue and Levitt (2001), the increase in the abortion rate was one factor in the decline in crime seen in the 1990s. I wish to control for this alternate factor, and include robustness tests with current, 7-year lagged, and 14-year lagged state abortion rates. my abortion data comes from the Guttmacher Institute, which has collected annual, state-level data on number of abortions since 1974.

6.8 Urban-Rural Trends

Differential trends across counties which are correlated with the numbers of IRCA applicants could also cause biases in my estimation. I note that one trend throughout much of the period I examine was a sizeable decline in crime in urban areas. In light of this possible trend, I examine a specification which includes a county urban indicator which is interacted with year dummies.

6.9 Restriction of Years of Analysis

One way to minimize the effect of the noise of alternate causes relative to the signal of changing numbers of legalized immigrants, I also obtain results restricting my analysis to the years of application and legalization, 1986 through 1990. This allows for the measuring of the full change in the number of legalized immigrants while including the shortest amount of time, potentially minimizing the size of changes in other variables.

6.10 Instrumental Variables

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

In addition, 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.^{5 6} 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. 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 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. I then use either this set of distances or the minimum of these distances, as the national source flow of immigrants shifted over the years. This set of distance terms makes up my first instrument.

In addition, I also include an instrument based on the date of application (as opposed to the date of legalization) for each population of IRCA applicants. This measures the cumulative proportion of the population which has applied to the IRCA program. Application date is highly and positively correlated with the date of legalization, explaining much of the variation in legalization date, and was driven in large part by the rollout of information and application-assistance programs across the United States.

First Stage:

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

Second Stage:

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

$IRCA_{it}$ = Number of IRCA applicants per capita legalized by year and county

\widetilde{IRCA}_{it} = Fitted values from first stage

Distance = Miles to nearest major port (Miami, New York, or Los Angeles) or to Mexican-American border

CumulativeNumberofAppsFiled = Cumulative number of IRCA applicants filed as a fraction of the population, by county

$Year_j = 1$ if $t = j$

$County_j = 1$ if $i = j$

$s = 1, 2, \dots, 51$

$i = 1, 2, \dots, 3139$

$t = 1981, 1982, \dots, 2000$

W_{it} = Other demographic and economic county-level controls

7 Results

7.1 Main Results

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) is associated with 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 report drops in crime correlated with greater numbers of IRCA legalizations, with a fall in violent crime of 2.9% and a fall in property crime of just under 5.3%.

7.2 Differenced Measure and Placebo Tests

Table II presents results from my regressions which use only a measure of the change in crime between 1986 and 1991. As can be seen from the number of observations, these regressions do not utilize panel data, having only a single value for each county. 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, finding a drop of approximately 1.7% in crime. Columns (2) and (3) present regressions using the differenced amount of violent and property crime, 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%.

Table III displays results for placebo tests with differenced measures of changes in crime. In contrast with Table II, this table uses differences in crime between 1981 and 1986 as well as the difference between 1991 and 1996, which saw no change in number of legalized applicants. I predict differences in the change in crime to be uncorrelated with the presence of IRCA applicants prior to legalization, controlling for county and year effects. In keeping with this prediction, I find no significant effects of the presence of IRCA applicants in the 1981-1986 period for all crime, solely violent crime, or solely 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.

Columns (4) to (6) show results from the period following legalization. Here I do find negative and significant results, with all crime, violent crime, and property crime all falling around 3%. This suggests that counties with large numbers of IRCA legalizations continued to experience declines in crime after the years of legalization. I suggest that this is consistent with a gradual assimilation of the newly legalized immigrants into the labor force.

7.3 Yearly Regressions

Table IV reports results from yearly regressions of the effect of the total IRCA applicant population per capita (without regard to if they had yet been legalized) on crime by county. The results are graphed in Figure VII, displaying the effect of a one percentage point increase in the IRCA population in a county (from 1% of a county to 2%, for example) on the amount of crime per capita. The graph shows that the presence of IRCA applicants was, prior to the late 1980's, associated with

higher levels of crime per capita. During the late 1980's and early 1990's, the period coinciding with the 1986 IRCA legalizations, there is a marked drop in the strength of this relationship. After the period of legalizations concludes, the relationship also stabilizes at approximately 0; that is, the presence of IRCA applicants (now legalized) is not correlated with differing levels of crime. This suggests a that the presence of IRCA applicants prior to their legalization is associated with a higher initial level of crime while, subsequent to legalization, this association disappears, their presence no longer an indicator for higher crime.

7.4 Robustness Results

In Tables VII and VIII, I also present a number of additional specifications which control for additional variables and further robustness checks for my OLS regressions. In Table VII, Column (1) gives results with the inclusion of urban-year dummies. This allows for separate rural and urban trends over my sample period. I find 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 III where a continued drop in crime was seen in the years following legalization. Columns (5) and (6) give results for a measure of crime inclusive of drug crime and one in which drug crimes are included as a separate variable. The magnitude of the estimate for the effect of IRCA legalizations remains roughly the same.

Finally, I also include specifications regressing crime on the cumulative number of IRCA applications/filings per capita. This is to determine if it was the application or the legalization which had more of an impact on crime. I find that IRCA filings has a negative correlation with crime. However, once IRCA legalizations are included in the final column, I find no significant effect of filings and a still negative and significant effect of legalization. This suggests that it was not just the prospect of legalization upon applying to the program which had an impact, but the actual legalization itself.

Table VIII gives an additional set of robustness tests, adding a variety of control variables which may account for some of the changes in crime during my sample period. Firstly, I add the logged number of prisoners in a state, as increasing severities of sentencing meant criminals being locked up for longer periods and, by some accounts, lowering crime rates. Secondly, I add in a 'Crack Index', from Fryer et al. (2004). This aids in controlling for the 'crack epidemic' which swept the country in the late 1980's and 1990's. To the extent that this epidemic was correlated with the population of IRCA applicants, I would be worried that this could produce a spurious fall in crime as the epidemic ebbed. I find this not to be the case. Additionally, I add a lagged

measure of abortions by state, as Levitt (2004), showed this to be one potential driver of a fall in crime. Finally, I include the number of police per capita, as the number of police has the potential to greatly impact the amount of crime in a county. I find that none of these factors can explain the significant negative effect of legalized IRCA applicants on crime, though I do find decreases in magnitude of the effect by about 0.5%.

7.5 Instrumental Variables Results

Table IX reports results from my instrumental variables regressions. I utilize both instruments in an attempt to isolate the causal effect of legalization of IRCA applicants on crime. Firstly, I utilize the cumulative number of IRCA applications processed by county as an instrument for the cumulative number of legalizations. This measure is highly correlated with the number of legalizations, as those who applied earlier tended to be legalized relatively earlier. Secondly, I use the distance to the nearest major port city (Miami, New York, and Los Angeles) or the Mexican border. Finally, I use the predicted percentage of each state's population who are immigrants, based on pre-1950 statistics.

Column (1) in Table IX gives results using only the first of these instruments, the cumulative number of IRCA filings. Column (2) uses both cumulative filings as well as minimum distance to a port or border. I find that, for all both specifications, the effect of IRCA legalizations on crime remains negative and significant and of approximately the same magnitude. In the specification for Column (2), I leverage the multiple instruments and run a Sargan overidentification test, finding a p-value of 0.31 and allaying some fears of invalid instruments. Column (3) presents results using a fixed effects instrumental variables approach, thus necessitating dropping the second, distance-based, instrument. Again I find a significant negative effect of legalizations. Moreover, for all instrumental variable specifications' first stages, I find highly significant F-tests of over 50.

7.6 Effects of Unemployment

Finally, I give one brief result that drives some intuition for the labor market model in the following section. Table X briefly displays a common finding among my empirical results. I find that, while unemployment rate is not significantly correlated with the violent crime rate, it does significantly increase the rates of property crime and total crime. I see increases of property crime rates on the order of 0.2% corresponding with an increase in the unemployment rate of 1%. This suggests that there are some effects of unemployment on crime, especially crimes of a more economic nature (property as opposed to violent crime). Such a finding is consistent with a model of economic motives for crime and of incapacitation from full-time work hindering the ability to engage in criminal activities.

8 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. As a simple test, Table X shows that the unemployment rate is positively correlated with property crime but negatively correlated with violent crime, conditional on time and local fixed effects. This seems intuitive as property crimes are most likely due to monetary incentives to a greater degree than violent crime or other types of crime.

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 $g(r) = \frac{r^2}{h}$ 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:

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

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

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

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

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). 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.

8.1 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.

8.2 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 XII. 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 α increases, reflecting the desire to avoid a worsened spell of unemployment, unemployed workers do the opposite, increasing time spent on crime.

8.3 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 pay. For γ and λ , I use data from the Bureau of Labor Statistics 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 year-round 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, which represents a lower value than that of the population as a whole. For pre-legalization IRCA applicants, part-time wages are derived from self-reported earnings data taken from the applications themselves.

β is the discount rate for future utility and is set at .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 .1. However, 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 XI.

8.4 Estimation Results

Estimation results can be found in Table XIII. 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 .33) while time spent in the crime sector fell (from .255 to .06 or .14, depending on employment status). This led to reductions in crime in the total population.

Figure VIII shows a graph of the national impact of legalization on crime over time as well as the impact within the IRCA applicant sub-group. 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 IX presents a comparison of my model's predicted changes in crime due to 1% of the population being legalized and the empirically estimated declines associated with 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.

Figure X 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 XI and XII display results of utilizing alternate parameters for both β and θ . I find similar results for a range of alternate values.

8.5 Alternate Channels

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 XIII, 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 associated with IRCA applicants' presence in each county. Thus, it is unlikely that this channel alone is responsible for the decline in crime.

9 Conclusion

Undocumented immigration is, and most likely 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 large-scale policy has been implemented a number of times in a multitude of countries, is that of general 'amnesty' programs. These programs give a route to legal status for undocumented immigrants, conditional on the fact that various requirements are met. 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%-5%. This decline is higher for property crime than for violent crime, suggesting more effect on crimes with an economic motive. It is robust to the inclusion of a number of controls

such as economic indicators, police strength, and and demographic. I find that placebo tests of the years prior to the IRCA, reassuringly, yield no significant effect. This fall in crime is economically significant, representing 160,000-400,000 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 proportion of undocumented immigrants currently residing in the United States. With estimates of this population of close to 12,000,000, almost 4% of the population, crime could be predicted to fall by even larger amounts due to such an amnesty program.

While I cannot decisively choose between a number of competing channels through which this effect could take place, I attempt to provide some theoretical guidance through a labor market model of crime. Applying the results of the model to the numbers legalized over time, I find that this model fits the data well using a range of plausible parameters, wages, and employment data and that much of the drop in crime could be attributed to greater job market opportunities among IRCA applicants.

A Appendix

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 XII for all comparative statics results.

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

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Table 1. Arrests - 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					-2.886** (0.581)	
IRCA Per Cap, Prop. Weight						-5.340*** (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

IRCA Per Cap refers to 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.

Table 2. Arrests - Differenced Values

VARIABLES	(1)	(2)	(3)
	All Crime	Violent Crime	Property Crime
Total IRCA Apps Per Cap	-1.669*** (0.637)	-0.916 (0.638)	-1.779*** (0.685)
Observations	2,418	2,418	2,418
R^2	0.009	0.013	0.008
Economic Controls	YES	YES	YES

Robust standard errors in parentheses

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

Total IRCA Per Cap is the weighted total number of 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.

Table 3. Arrests - Placebo Tests

VARIABLES	(1) 81-86 - All	(2) 81-86 - Violent	(3) 81-86 - Property	(4) 91-96 - All	(5) 91-96 - Violent	(6) 91-96 - Property
Total IRCA Apps Per Cap	0.922 (1.250)	0.618 (0.845)	0.506 (0.625)	-2.964* (1.631)	-3.650*** (0.900)	-2.981*** (1.008)
Observations	2,356	2,356	2,356	2,112	2,112	2,112
R^2	0.005	0.005	0.008	0.009	0.003	0.008
Economic Controls	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

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

Total IRCA Per Cap is the weighted total number of 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 1981-1986 period while second 3 give results regarding 1991-1996 period.

Table 4. Arrests - Yearly Series

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	1980 Crime	1981 Crime	1982 Crime	1983 Crime	1984 Crime	1985 Crime
Total IRCA Apps Per Cap	3.114*** (0.731)	2.992*** (0.721)	2.032*** (0.669)	2.909*** (0.761)	2.291*** (0.646)	3.364*** (0.846)
Observations	2,583	2,661	2,722	2,752	2,759	2,718
R^2	0.079	0.113	0.117	0.108	0.132	0.112
Economic Controls	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses

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

Total IRCA Per Cap is the weighted total number of 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. These results display the reduced form effect of the total population of IRCA applicants on crime in a given year.

Table 4. Arrests - Yearly Series
Continued

VARIABLES	(1) 1986 Crime	(2) 1987 Crime	(3) 1988 Crime	(4) 1989 Crime	(5) 1990 Crime	(6) 1991 Crime	(7) 1992 Crime
Total IRCA Apps Per Cap	3.505*** (0.864)	2.659*** (0.801)	2.508*** (0.819)	2.323*** (0.722)	1.586** (0.768)	0.853 (0.852)	-0.484 (1.325)
Observations	2,614	2,570	2,711	2,701	2,666	2,691	2,569
R ²	0.126	0.117	0.117	0.107	0.093	0.094	0.088
Economic Controls	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses

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

Total IRCA Per Cap is the weighted total number of 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. These results display the reduced form effect of the total population of IRCA applicants on crime in a given year.

Table 4. Arrests - Yearly Series
Continued

VARIABLES	(1) 1993 Crime	(2) 1994 Crime	(3) 1995 Crime	(4) 1996 Crime	(5) 1997 Crime	(6) 1998 Crime	(7) 1999 Crime
Total IRCA Apps Per Cap	-0.485 (1.081)	-1.434 (1.496)	-0.390 (0.940)	-0.469 (0.988)	0.193 (0.993)	-0.127 (1.348)	-1.871 (1.232)
Observations	2,532	2,438	2,362	2,332	2,300	2,391	2,560
R^2	0.088	0.082	0.067	0.073	0.059	0.069	0.078
Economic Controls	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses

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

Total IRCA Per Cap is the weighted total number of 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. These results display the reduced form effect of the total population of IRCA applicants on crime in a given year.

Table 5a. Arrests - 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	-4.638*** (1.166)	-4.826** (2.049)	-3.333*** (0.918)	-3.567*** (1.089)	-5.745*** (1.100)	-4.399*** (1.349)	-5.414*** (1.927)
Drug Crimes					0.400*** (0.00874)		
IRCA Filings Per Cap						-4.399*** (1.349)	1.482 (2.013)
Observations	44,573	44,573	11,023	44,573	44,573	44,573	44,573
R ²	0.570	0.635	0.758	0.586	0.691	0.569	0.569
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

IRCA Per Cap refers to 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. AfrAmer Per Capita is the number of self-reported African Americans per capita by year, according to Census data. Unweighted IRCA Per Cap is the unweighted cumulative number of IRCA applicants per capita. Inc. Drug Crime uses a logged, per-capita measure of all crime, including drug sales and drug possession crime. Drug crimes are the logged per-capita measure of drug sales and drug possession crimes. Column (6) restricts my sample to years of application and legalization, 1986-1990.

VARIABLES	(1) All Crime	(2) All Crime	(3) All Crime	(4) All Crime
IRCA Per Capita	-3.700*** (1.364)	-3.698*** (1.363)	-3.942*** (1.366)	-3.928*** (1.367)
Prisoners	-0.0780*** (0.0298)	-0.0757** (0.0299)	-0.0852*** (0.0312)	-0.0854*** (0.0312)
Crack Index		-0.00485 (0.00771)	-0.00883 (0.00867)	-0.00939 (0.00867)
Lagged Abortions			6.349 (4.952)	6.310 (4.949)
Police Per Capita			8.367** (4.151)	
Observations	49,067	49,067	49,067	49,067
R^2	0.618	0.618	0.618	0.618
Year FE	YES	YES	YES	YES
County FE	YES	YES	YES	YES
Economic Controls	YES	YES	YES	YES

Robust standard errors in parentheses

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

IRCA Per Cap refers to 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. AfrAmer Per Capita is the number of self-reported African Americans per capita by year, according to Census data. 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). Crack index is a weighted measure of crack-related indicators by state, as constructed by Fryer, et al. (2004). Prisoners gives the logged value of the annual

Table 7. Arrests - Instrumental Variables

VARIABLES	(1)	(2)	(3)
	Change from 1980	Change from 1980	All Crime
IRCA Per Capita	-3.536*** (1.107)	-3.549*** (1.107)	-3.520*** (1.145)
Observations	48,472	48,472	48,472
Year FE	YES	YES	YES
Economic Controls	YES	YES	YES
County FE	NO	NO	YES

Standard errors in parentheses

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

IRCA Per Cap refers to 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. Total IRCA Per Cap is the weighted total number of IRCA applicants by county (that is, the cumulative total of applicants legalized in 1987-1990).

Table 8. Crime Reports - Unemployment Rate Effects

VARIABLES	(1)	(2)	(3)
	All Crime	Property Crime	Violent Crime
Unemp. Rate	-0.00418* (0.00247)	0.00557* (0.00291)	-0.0131*** (0.00337)
Observations	52,739	52,739	52,739
Year FE	YES	YES	YES
County FE	YES	YES	YES
Economic Controls	YES	YES	YES

Robust standard errors in parentheses

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

IRCA Per Cap refers to weighted IRCA applicants per capita by county. Unemployment Rate is given by the county specific annual unemployment rate as calculated by the BLS. The economic controls are a vector composed of poverty rates, county income levels and county employment levels. 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. All crime refers to annual logged, per-capita non-drug arrests by county. Property crime refers to the same measure for solely those crimes designated as property crimes by the FBI's Uniform Crime Reports. Violent crime refers to the same measure for solely those crimes designated as violent crimes by the FBI's Uniform Crime Reports.

Table IX: Summary Statistics, 1986

	Applicants	United States
Male	.663	.487
Female	.337	.512
Median Age	27.5	32.7
1-5 years	.0002	.090
6-10 years	.025	.072
11-15 years	.048	.067
16-20 years	.138	.075
21-25 years	.226	.077
26-30 years	.200	.087
31-35 years	.144	.087
36-40 years	.088	.078
41-45 years	.053	.067
46-50 years	.033	.053
51-55 years	.021	.044
56-60 years	.011	.042
61-65 years	.005	.043
66+ years	.006	.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 X: 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

Table XI: 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 XII: 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
$\frac{r^*}{d\alpha}$	≤ 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.

Table XIII: 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.

Table XIV: Post-Legalization Migration

	1989	1992
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 question 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 XV: 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 XVI: IRCA Applicants' Weekly Wages

Age	Weekly Wages, 1987	Weekly Wages, 1992
<20	177.41	
20-25	219.91	301.51
25-30	258.60	345.39
30-35	283.72	356.59
35-40	293.89	383.26
40-50	301.19	383.86
50-60	246.20	322.59
60+	206.10	250.73

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.

Table XVII: 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.

Table XVIII: IRCA Applicants' Education Levels in 1987 and 1992

Age	Avg. Years of Education, 1987	Avg. Years of Education, 1992
<20	10.41	
20-25	8.70	12.05
25-30	8.81	9.59
30-35	8.68	9.51
35-40	8.69	9.48
40-50	7.83	9.08
50-60	6.40	7.52
60+	4.73	6.38
Overall	8.50	9.28
Avg Individ. Additional Years		1.05

Answers taken from LPS1 and LPS2 surveys conducted on IRCA applicants after their legalization in 1989 and 1992. Questions ask 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.

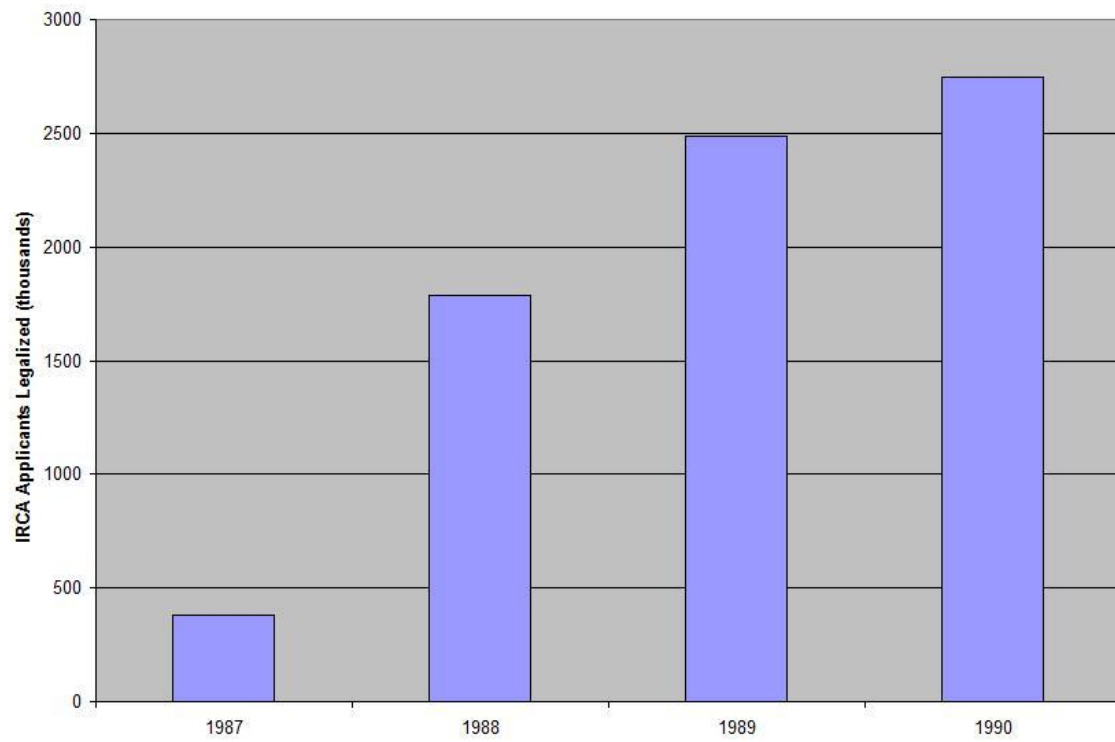


Figure I: Cumulative Number of IRCA Applicants Legalized

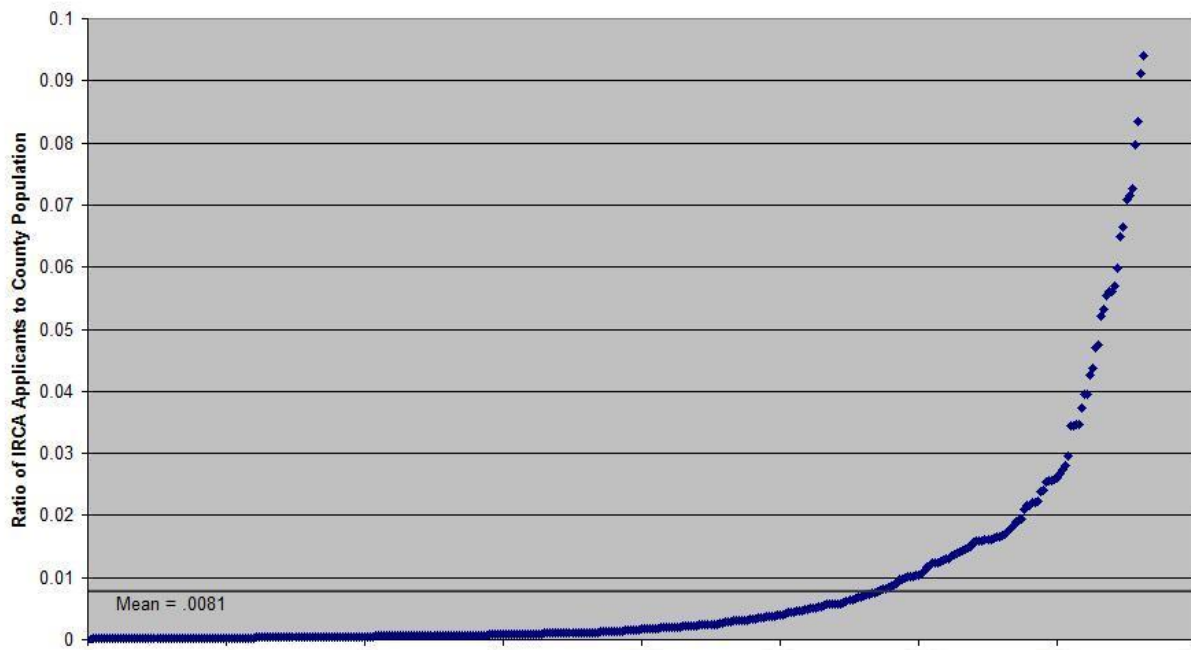


Figure II: Number of IRCA Applicants Per Capita for Counties with Positive Number of IRCA Applicants

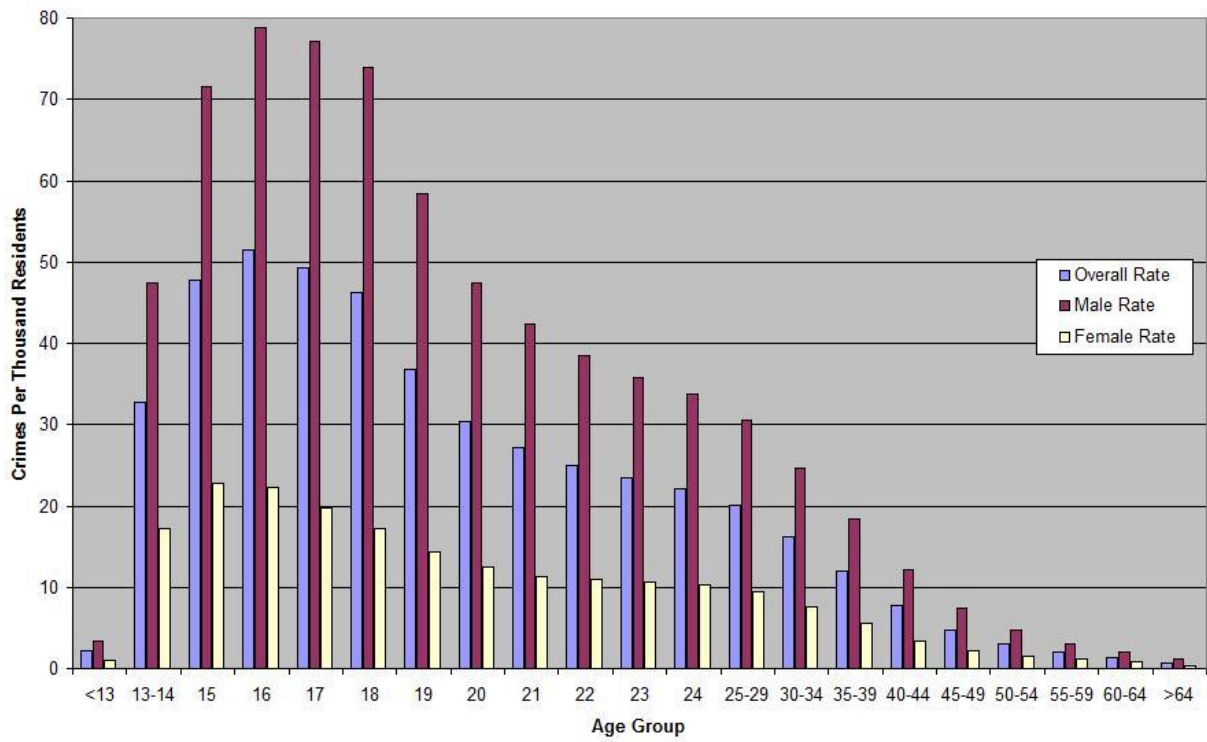


Figure III: Rates of Crime by Age and Sex

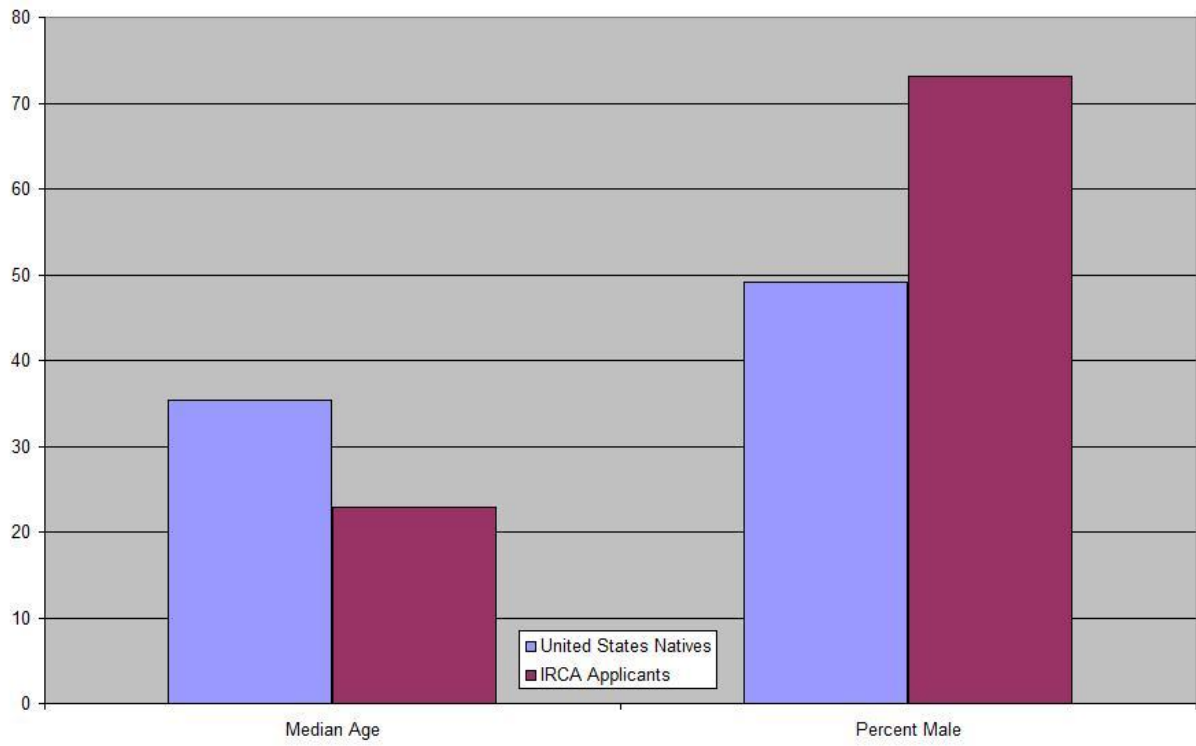


Figure IV: Average Age and Percentage Males Among IRCA Applicants and the United States

Appendix Figure 1. Number of Crimes by Type and Year

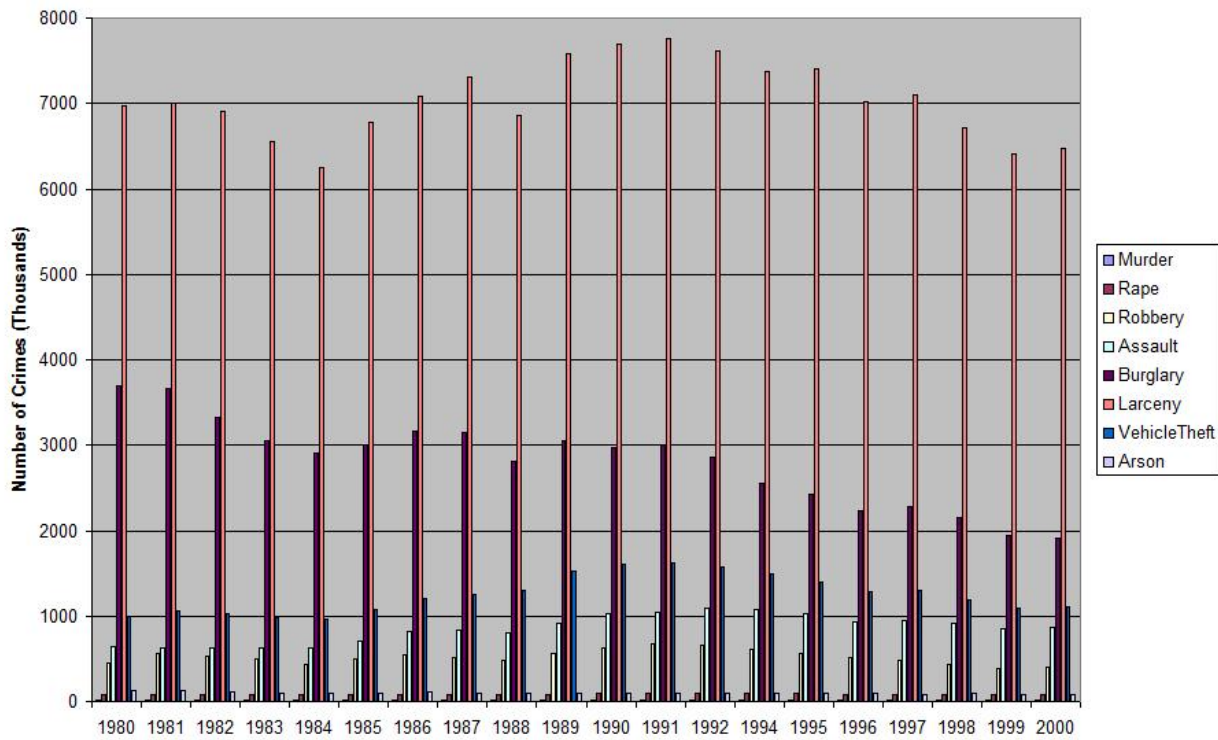


Figure V: National Number of Crime Reports, by Crime Type

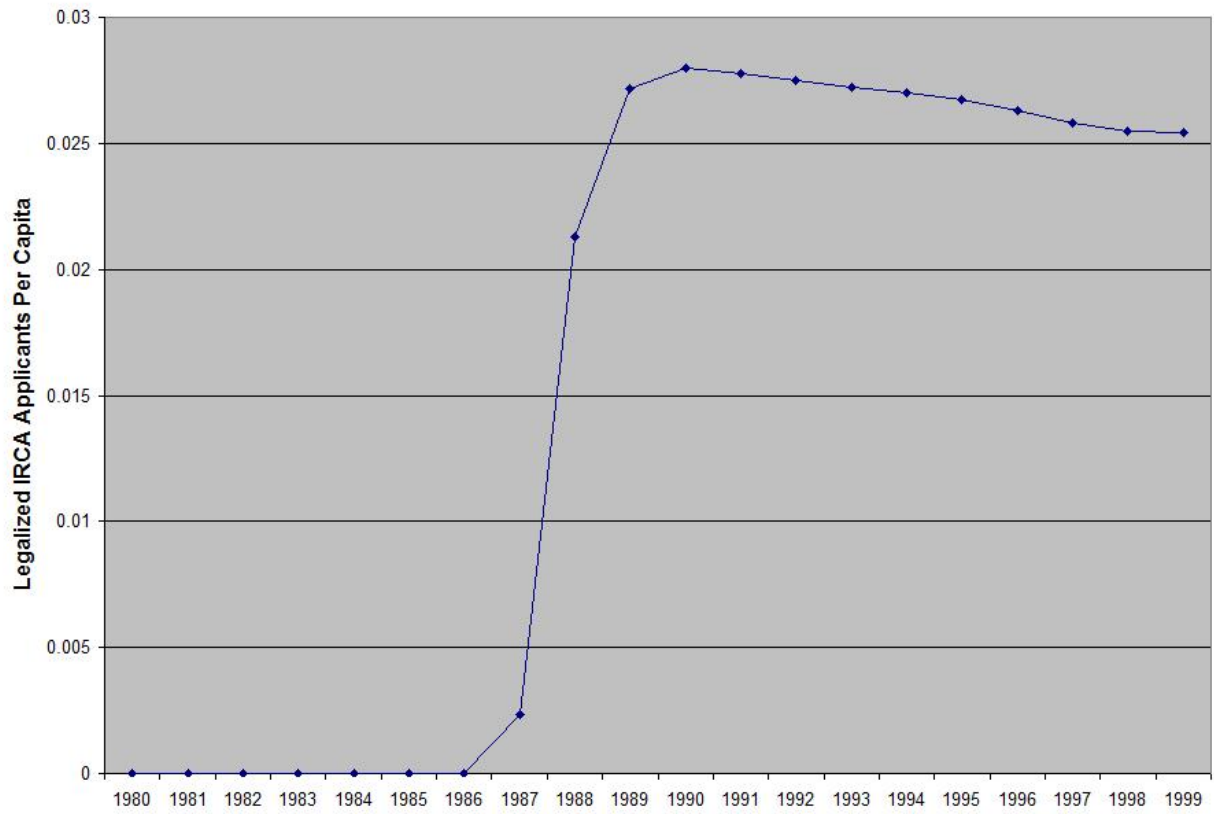


Figure VI: Legalized IRCA Applicants Per Capita, Santa Clara County

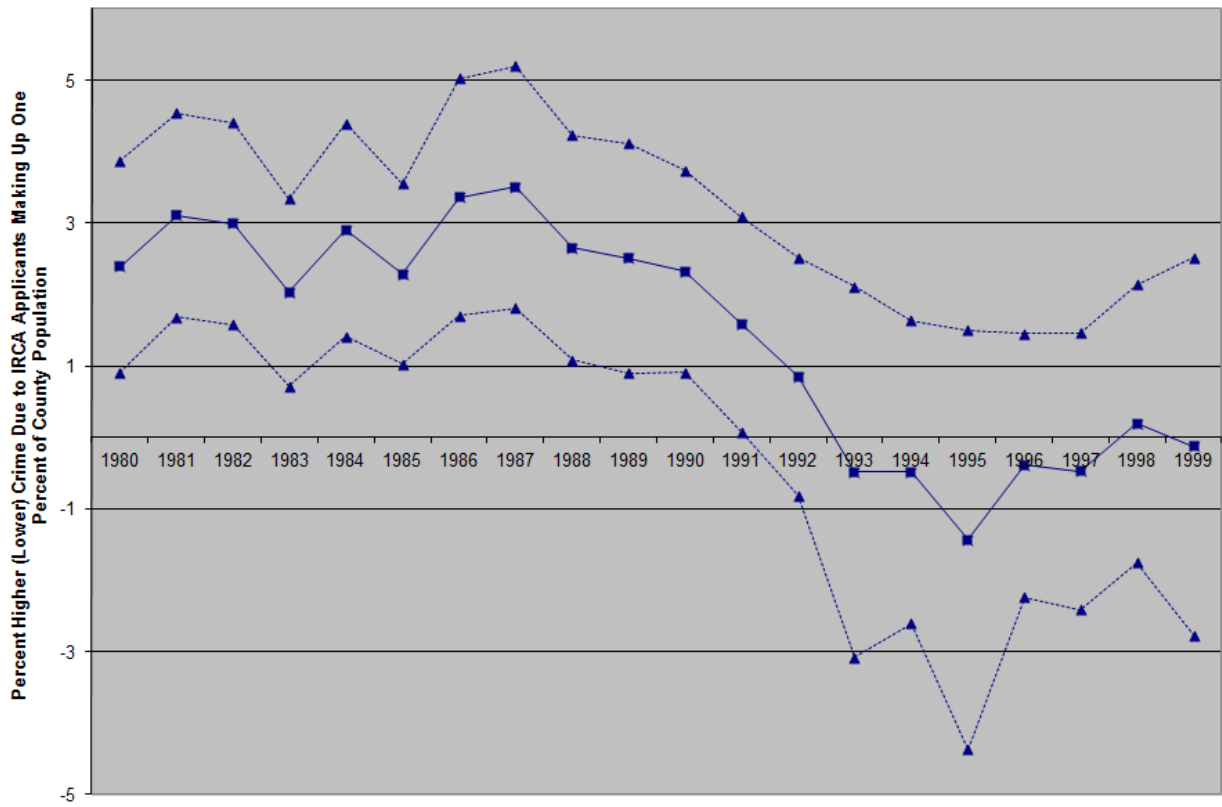


Figure VII: Effect of IRCA on Crime Per Capita

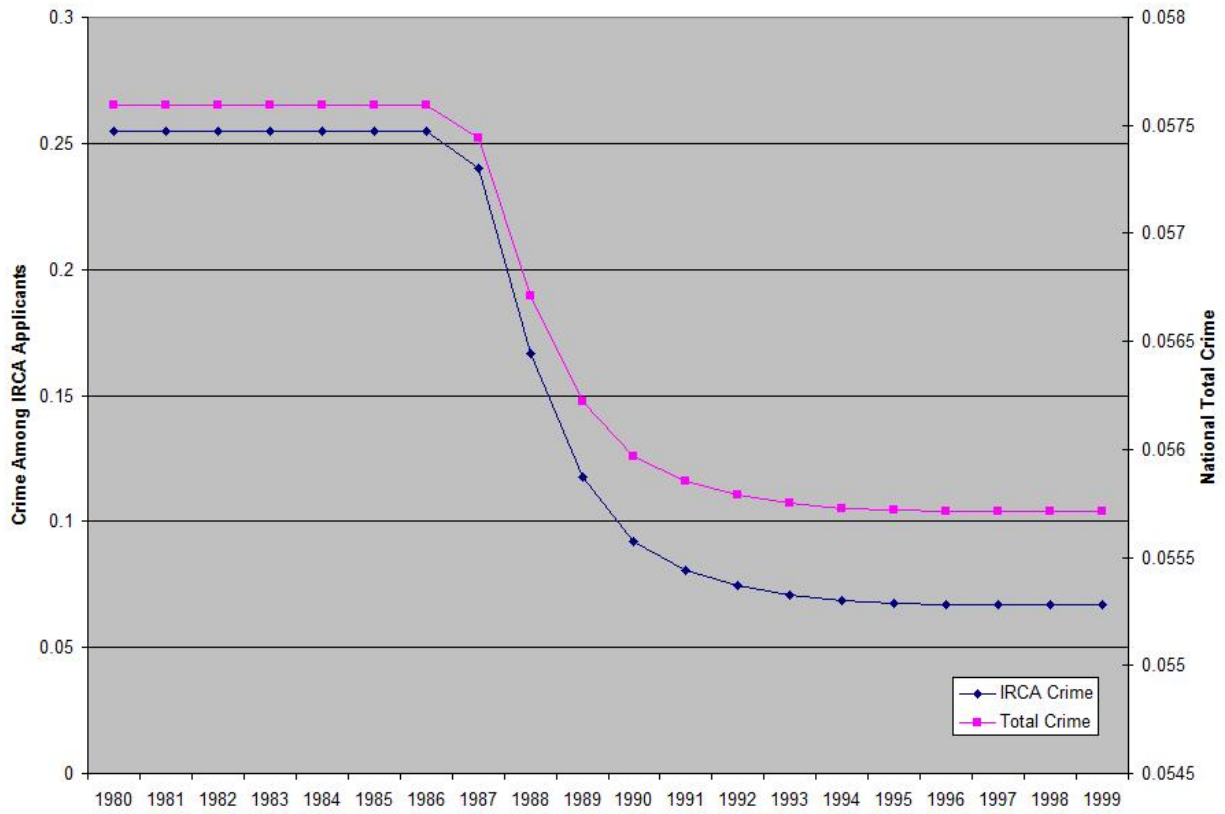


Figure VIII: Changes in Crime, Nationally and Among IRCA Applicants

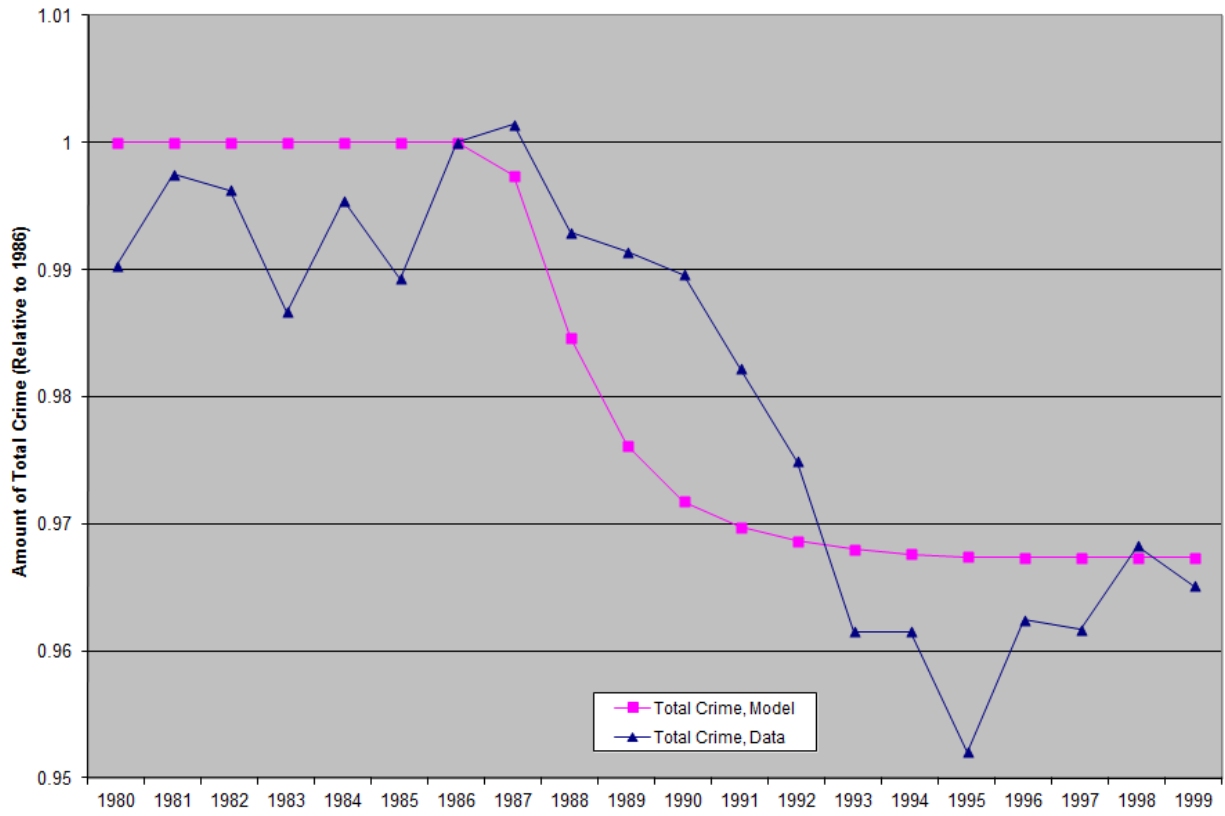


Figure IX: Predicted and Actual Change in Crime

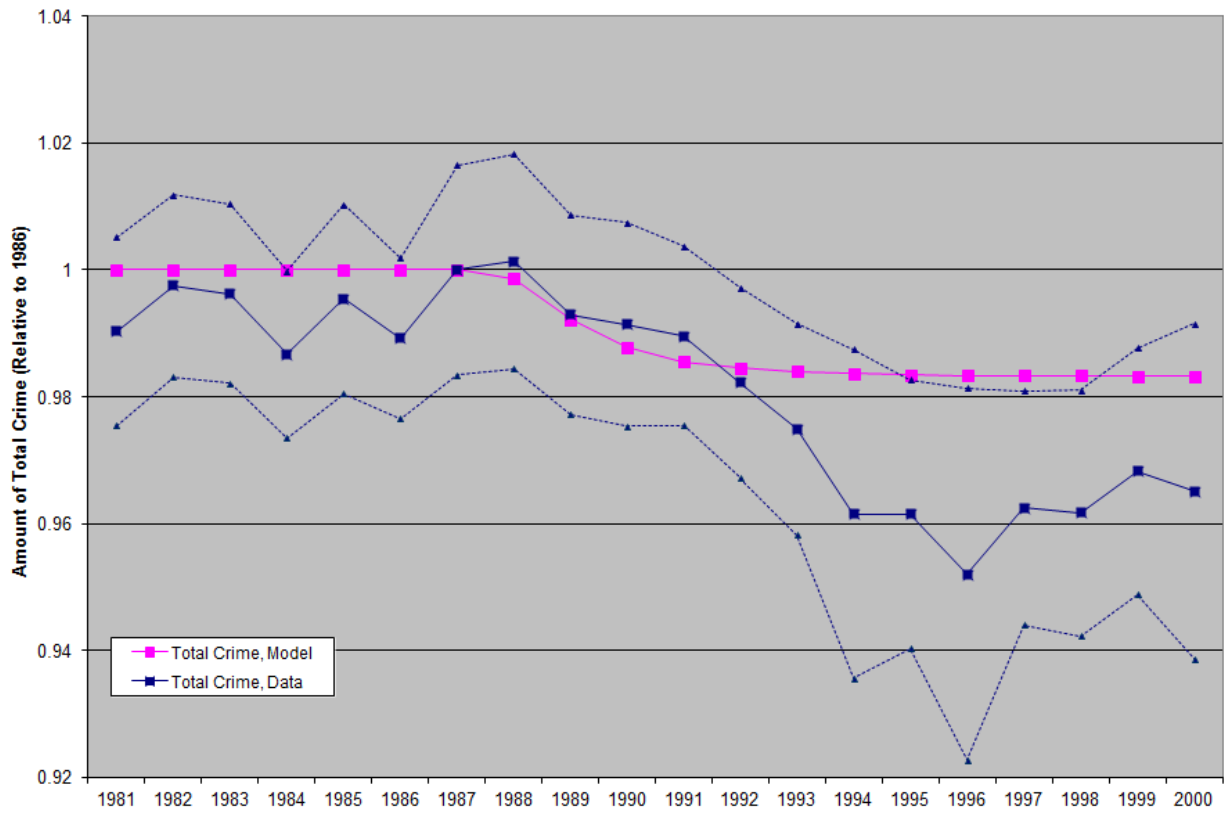


Figure X: Pre-Legalization IRCA Crime Level Constrained to Equal Native Crime Level

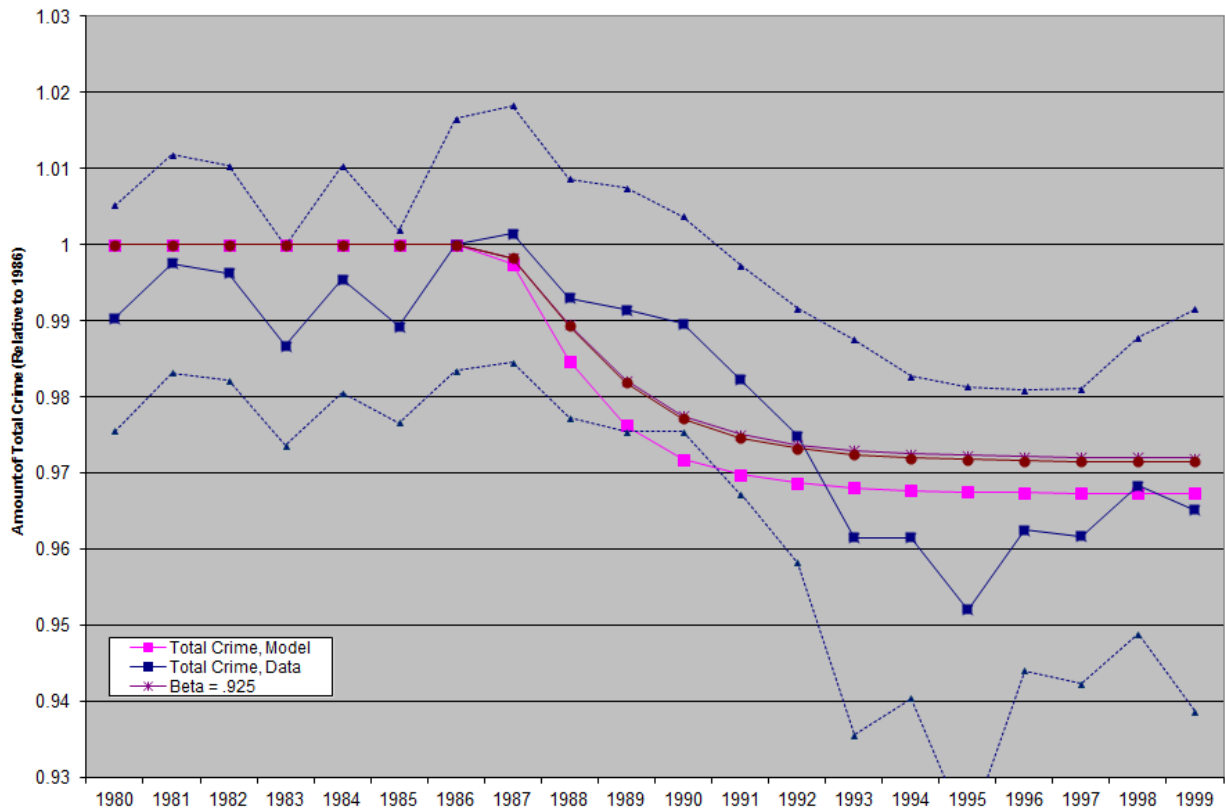


Figure XI: Robustness to Changes in Beta

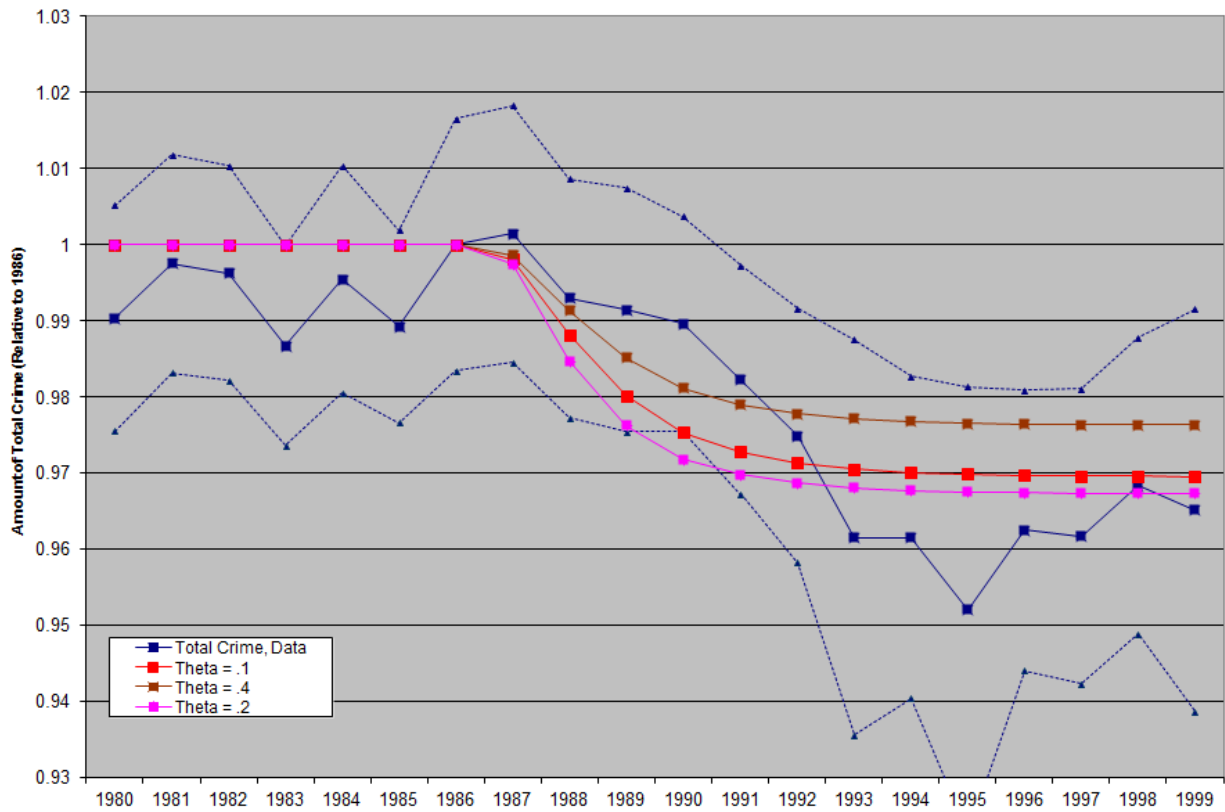


Figure XII: Robustness to Changes in Theta

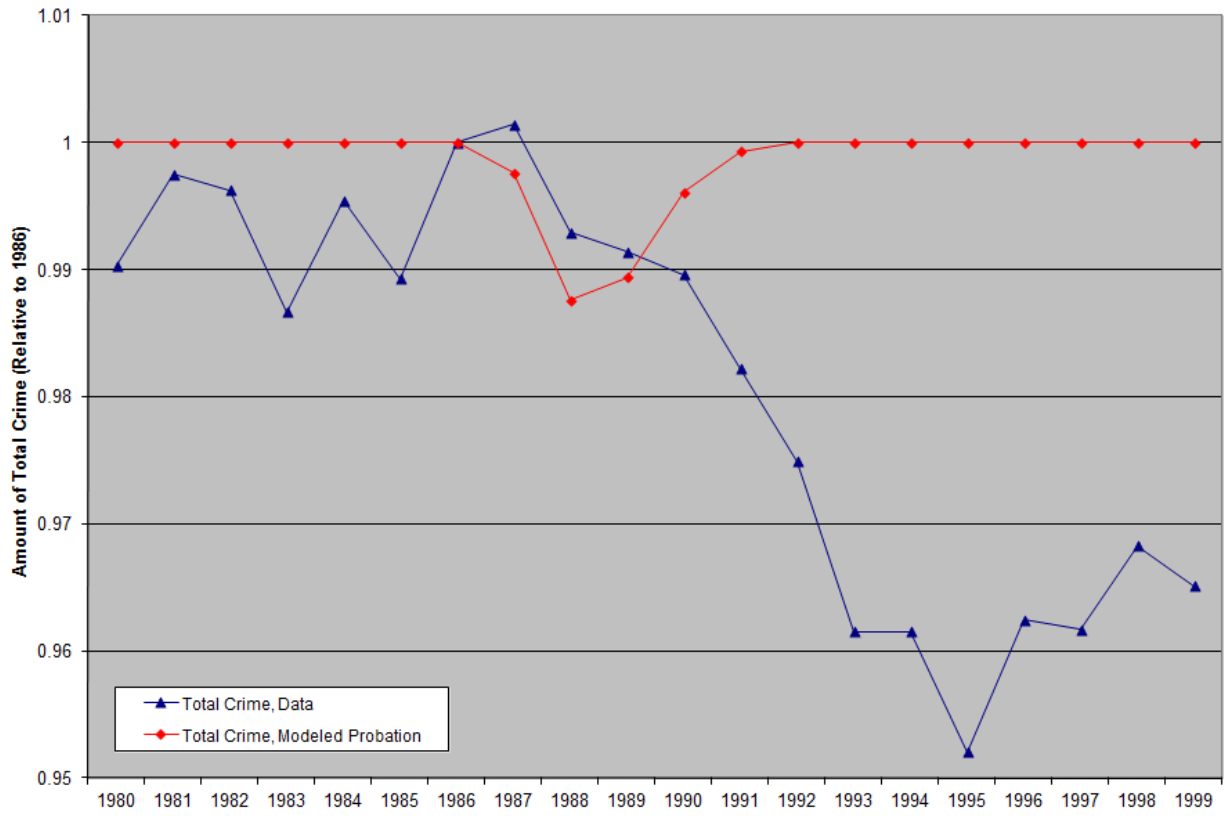


Figure XIII: Predicted and Actual Change in Crime Using Solely a Probation Model