The Amnesty Effect: Evidence from the 1986 Immigration Reform and Control Act

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Abstract

The 1986 Immigration Reform and Control Act (IRCA) marked one of the biggest changes in the history of U.S. immigration policy. One of the main provisions in the legislation was a legalization program that granted amnesty to undocumented immigrants who could prove they had resided in the U.S. continuously since January 1, 1982. This study evaluates the impact of mass-legalization on the flow of undocumented immigrants across the U.S.-Mexican border by analyzing a monthly time series of Border Patrol apprehensions from January 1977 to December 2000 within the context of a multivariate regression model. In sharp contrast to previous studies that examine the immediate effects of IRCA on illegal immigration, this paper focuses on the long-term impact of the amnesty provision. Controlling for factors that influence the flow of illegal immigrants, including relative economic conditions in the U.S. and Mexico, the level of border enforcement, economic liberalization through trade, and seasonal fluctuations in the demand for agricultural labor in the U.S., I find that the IRCA amnesty program is associated with a decline in the number of border apprehensions. Although using apprehensions as a proxy for the flow of illegal immigrants precludes the analysis from estimating the exact magnitude of this effect, the findings refute unsubstantiated claims that the amnesty program encouraged further illegal immigration.

Introduction

In October 1986, the U.S. Congress passed the Immigration Reform and Control Act (IRCA), and President Reagan signed the legislation into law in the following month. The legislation aimed to curtail the flow of undocumented immigrants to the United States through three mechanisms: sanctions on employers who knowingly hired undocumented workers, increased border enforcement, and an amnesty program for undocumented immigrants already in the United States who met certain provisions. Approximately 3 million undocumented immigrants, including 2.3 million Mexicans, were granted legal permanent resident status under IRCA. It should be noted at the outset that I use the terms *illegal alien, undocumented immigrant, and illegal immigrant* interchangeably.

A considerable debate has arisen in the post-IRCA period over the extent to which IRCA has reduced the flow of undocumented immigrants across the U.S.-Mexican border. The flow of undocumented immigrants is expected to decline if these programs reduce the demand for illegal immigrant workers (and hence their wages) or if potential undocumented immigrants believe it would be more difficult to obtain a job in the United States.[1] Likewise, tighter border security is expected to deter illegal immigration by lowering the probability of a migrant successfully crossing the border. However, more at issue is whether the amnesty provision affected the flow of illegal immigrants in the long-term. This question is addressed by determining whether there were more or fewer illegal entrants in subsequent years than there would have been without the legislation. At the very least, the amnesty program reduced the potential population at-risk to migrate illegally, since nearly half of the newly-legalized immigrants were Mexican residents who traveled to the U.S. periodically in response to the seasonal demand for farm labor.

Opponents of the law argued that amnesty actually encourages illegal entry by setting a precedent for future legalization measures.[2] Furthermore, widespread amnesty creates a snowball effect because relatives attempt to join emigrants who gained legal status. For example, in response to a proposed amnesty bill in 2000, Congressman Lamar Smith urged his fellow lawmakers to heed the lesson of the 1986 IRCA. He claimed that "Amnesty actually precipitates even more illegal immigration, as individuals come to join their amnestied relatives or are encouraged in the belief that if they can just elude the Border Patrol and stay underground for a few years, they will eventually get amnesty themselves."[3] A more recent report on illegal immigration published by the Heritage Foundation echoed a similar sentiment: "As evident from the last amnesty in 1986, illegal aliens will rightfully view the federal government as a serial amnesty machine that will cave once again in 20 years when the illegal immigration population again swells in the United States." Although these claims may have merit, whether they are politically motivated or substantiated by empirical evidence is unclear.

1 Pia M. Orrenius and Madeline Zavodny, "Do Amnesty Programs Reduce Undocumented Immigration? Evidence from IRCA," *Demography 40*(3).

2 Annelise Anderson, "Illegal Aliens and Employer Sanctions: Solving the Wrong Problem," in *Hoover Essays in Public Policy* (Stanford, CA: Hoover Institution, 1986).

3 Lamar Smith, "Lamar Smith Press Conference Statement on Amnesty," Immigration Daily, http://www.ilw.com/.

The debate over amnesty has once again moved to the forefront of American politics, with the current administration continuing to push for immigration reform centered on mass-legalization of all illegal aliens currently in the U.S. Since IRCA is the only example of a large-scale amnesty program in U.S. history, understanding its impact on the long-term flows of undocumented immigrants will be extremely useful to policymakers and interest groups on both sides of the debate.

Previous Literature

Previous research on how IRCA affects the flow of undocumented immigrants has reached mixed conclusions. Controlling for various factors that are expected to affect the migration rate, including relative economic conditions and the level of border enforcement, Orrenius and Zavodny (2003) find that apprehensions of persons attempting to illegally cross the U.S.-Mexico border declined immediately following passage of the IRCA but returned to normal levels during the period when illegal immigrants could file for amnesty and the years thereafter. Their findings suggest that while the amnesty program may not have reduced undocumented immigration, it did not encourage it either. Using data collected from U.S. migrants interviewed in seven Mexican communities from 1986-1989, Donato, Durand, and Massey (1992) carried out a set of time-series experiments that examined changes in migrants' behavior before and after the passage of IRCA. They similarly find that there was no consistent change in either direction in the probability of making a first illegal trip to the United States. Woodrow and Passell (1990) use a residual methodology to estimate a post-IRCA measurement of the number of undocumented immigrants included in the 1988 Current Population Survey (CPS). After identifying the portion of the undocumented population who came after the IRCA was enacted, they conclude that the annual change in the number of undocumented immigrants was not significantly different from that prior to IRCA.

In contrast, several studies using border apprehensions data from the Immigration and Naturalization Services (INS) find that IRCA was successful in reducing the volume of illegal Mexican migration. Controlling for wages and unemployment levels in the United States as well as unemployment rates, income levels, and the size of the migrant-aged population in Mexico, Bean et al. (1990) conclude that in the three years after the IRCA amnesty program was passed, border apprehensions were 24 percent below the level anticipated in the absence of IRCA. Using a similar model, White, Bean, and Espenshade (1990) find that the legalization provision of IRCA contributed to a 17 percent decline in the flow of illegal immigrants during the 23-month period after it was enacted. Accordingly, all empirical studies that isolate the impact of amnesty on the flow of illegal immigrants find either a negligible effect or modest decline. A thorough review of the literature did not reveal any studies that indicate amnesty increased the flow of illegal immigrants across the U.S.-Mexico border.

Another shared characteristic of IRCA academic literature is that most studies only cover a short time period; analysis of the sample period rarely extends past 1989. This imposes several limitations on their findings. First, the application period for the legalization program did not end until April 1988. Detecting a decline in illegal immigration during this period cannot be attributed to the amnesty program because it may also be related to the unique

aspects of the application process. The decline may also reflect the immediate impact of reducing the population of potential illegal immigrants by removing those who apply for amnesty. If this impact dissipates in subsequent years, then short-term estimates do not capture the true affect of amnesty on the flow of undocumented immigrants. Moreover, restricting analysis to the years immediately following passage of the act fails to consider how mass-legalization might affect the flow of illegal immigrants in the long-term.

Explanation of Variables

In this paper, I develop a multivariate statistical model to estimate the long-term effect of the amnesty component of IRCA on the flow of undocumented immigrants, using data from 1977-2000. Following previous empirical studies on illegal immigration, I rely on INS apprehensions data as a proxy for inflows of undocumented immigrants into the United States. I also lean heavily on previous research to develop a set of control variables that are expected to affect migration rates. An additional variable for *trade openness* is introduced to capture the affect of globalization and trade liberalization on illegal immigration.

Why Analyze Apprehensions Data?

Research on illegal U.S. immigration is confounded by one unavoidable reality – the number of undocumented immigrants entering the United States is unobserved and therefore not precisely known.[4] This forces analysts to rely on proxy indicators, such as border apprehensions, and a variety of indirect estimates that try to measure the flow of undocumented immigrants into the U.S. However, White et al. (1990) contend that "Apprehensions data are relevant to the question of whether illegal immigration has risen or fallen over a given period of time because they may in some sense be an indicator of the number of border crossing attempts and successful entries into the country by illegal immigrants." In other words, border apprehensions are a suitable proxy because the volume of apprehensions is highly correlated with the total flow of undocumented immigrants. Espenshade (1995) examined the nature of this correlation and concluded that the simple correlation between the number of apprehensions and the volume of illegal immigration is about 0.90 and the size of the illegal immigrant flow is approximately 2.2 times the number of border apprehensions. This suggests that the signs of regression coefficients in models that regress apprehensions on a set of explanatory variables will capture the direction of the effects of changes in variables on flows of illegal immigrants, but the exact magnitude of these effects cannot be identified. Thus, a disadvantage to using apprehensions data as a proxy indicator for illegal immigration is that interpretation of regression results is severely limited.

The number of apprehensions at the U.S.-Mexico border is, of course, an imperfect proxy for the flow of illegal immigrants. First off, border apprehensions data do not account for illegal aliens who entered the U.S. legally and then overstayed their visas. Visa overstays are believed to account for about half of the unauthorized aliens present in the United States, although among Mexicans and Central Americans, the share is estimated to be between

4 Thomas J. Espenshade, "Using INS Border Apprehension Data to Measure the Flow of Undocumented Migrants Crossing the U.S.-Mexican Border," *International Migration Review, 20* (1994).

16% and 26%.[5] Additionally, because apprehensions data measure events and not people, it is possible to include repeat apprehensions for the same individual. This issue is problematic because undocumented immigrants are likely to keep trying to enter the United States until they succeed, no matter how many times they are apprehended.[6] Furthermore, the number of apprehensions is directly related to the level of Border Patrol effort. An increase in apprehensions may reflect stepped-up Border Patrol enforcement rather than a true increase in the flow of illegal immigrants. I control for this possibility by including a measure of Border Patrol person-hours in the set of explanatory variables. This also has the added benefit of isolating the impact of the IRCA amnesty program from provisions that increased enforcement activities.

Variables and Data

The variables used in the regression analysis and the corresponding data sources are listed in Table 1. The primary source of data is unpublished INS files on monthly Border Patrol activities. The INS reports statistics on two types of U.S. Border Patrol Activities: "linewatch" activities, which occur at international borders, and "non-linewatch" activities, which occur in the interior of the United States.[7] Linewatch apprehensions are more relevant to the study of illegal immigration because they reflect the moment in time that individuals attempt to cross the border. Therefore, I use monthly data on the number of individuals apprehended by U.S. Border Patrol officers on linewatch duty at the U.S.-Mexican border as my dependent variable. Since the level of enforcement, measured as the number of person-hours devoted to linewatch duty at the U.S. Mexican border, and the number of apprehensions which occur in the interior of the U.S are expected to affect the number of border apprehensions, they are modeled as control variables.

In addition, labor demand for low-wage farm-workers in California and other parts of the American Southwest is highly seasonal, increasing in summer months and falling during the winter, which is likely to create seasonal variations in the tendency to migrate illegally to the United States.[8] Apprehensions respond to seasonal fluctuations the same way, implying that the data do not exhibit a linear relationship. I correct for the non-linear nature of the data, by taking the natural log of each INS measure. A set of month dummy variables is also added to the model to control for seasonal patterns. Since the other variables in the model do not have a seasonal pattern, there is no reason for allowing the systematic seasonal component in apprehensions to be stochastic.[9]

The number of interior apprehensions lagged one time period is included as an

5 Orrenius and Zavodny, "Evidence from IRCA."
6 Barry Edmonston, Frank D. Bean, and Jeffrey S. Passel. "Perceptions and Estimates of Undocumented Migration to the United States," in *Undocumented Migration to the United States: IRCA and the Experiences of the 1980s*, ed. Barry Edmonston, Frank D. Bean, and Jeffrey S. Passel (Washington, DC: Urban Institute Press, 1990).
7 Gordon H. Hanson and Antonio Spilimbergo, "Illegal Immigration, Border Enforcement, and Relative Wages: Evidence from Apprehensions at the U.S.-Mexico Border," *American Economic Review,* 89 (1999).
8 Michael J. White, Frank D. Bean, and Thomas J. Espenshade, "The U.S. 1986 Immigration Reform and Control Act and Undocumented Immigration to the United States," *Population Research and Policy Review, 9* (1990).
9 Hanson and Spilimbergo, "Illegal Immigration."

explanatory variable instead of current interior apprehensions. The reason for this is that illegal aliens of Mexican descent that are apprehended in the interior of the United States get deported to the Mexican side of the border, where they form a ready pool of prospective migrants. It is expected that these individuals will then try to re-enter the country immediately. The lag is necessary to capture this effect because it accounts for the time it takes an apprehended illegal alien to be deported.

The size of the young-adult Mexican population is also included as an explanatory factor in the analysis since it is a proxy for the population at risk of migrating to the United States in undocumented status.[10] It is expected that the larger the size of this population, the greater the flow of undocumented immigrants across the border. Furthermore, White et al. (1990) suggest that singling out the Mexican population for the at-risk group is appropriate because INS statistics compiled between 1977 and 1988 indicate that more than 97 percent of all apprehensions at the U.S.-Mexico border were of persons born in Mexico. I take the natural log of this measure to reflect the non-linear trend of population growth.

Relative economic conditions in the United States and Mexico are also likely to affect the rate of undocumented migration across the border. Greater economic opportunities in the U.S. and/or worsening of circumstances in Mexico are expected to encourage more illegal immigration. This is reflected in the model by controlling for the relative wage ratio between the two countries, the U.S. unemployment rate, and the average monthly exchange rate (in Mexican pesos per U.S. dollar). The wage ratio is calculated as the U.S. hourly wage in U.S. dollars, deflated by the U.S. Consumer Price Index (CPI), divided by the Mexican hourly manufacturing wage in Mexican pesos, deflated by the Mexican CPI. However, there is an alternative measure of the U.S. real wage. If prospective migrants plan to support family members in Mexico by remitting a portion of their earnings, they may evaluate U.S. earnings in terms of its purchasing power in Mexico, rather than in terms of its purchasing power in the United States.[11] To control for this possibility, a second wage ratio is constructed that uses the real peso U.S. wage (U.S. nominal wage multiplied by the peso-dollar exchange rate, divided by the Mexican CPI) instead of the real U.S. wage. I test separate models for each wage ratio variable – Model 1 uses the original wage ratio, while Model 2 uses the second wage ratio just described. I am unable to create a ratio for levels of unemployment because reliable data are not available for the earlier parts of the sample period. The U.S. producer price index for crude oil is also included because of the significance of oil production in the Mexican economy. Most notably, a decline in oil prices in the 1980s triggered Mexico's worst economic crisis since the Mexican Revolution in 1910.[12]

Some combinations of the variables in this section were also included in the models of nearly all previous IRCA studies; however, a common omission is the degree of trade and investment liberalization. Economic theory suggests that trade and migration may be directly

10 Thomas J. Espenshade, "Does the Threat of Border Apprehension Deter Undocumented US Immigration?" *Population and Development Review, 20* (1994).
11 Hanson and Spilimbergo, "Illegal Immigration."
12 Richard C. Jones, "Immigration Reform and Migrant Flows: Compositional and Spatial Changes in Mexican Migration after the Immigration Reform Act of 1986," *Annals of the Association of American Geographers, 85* (1995).

related, though the direction of this relationship is unproven. According to the Mundell model, equalization of factor prices through international trade reduces the incentive for capital or labor to move across national boundaries; thus, international trade is considered a substitute for factor movements, including the movement of people.[13] Proponents of the 1993 North American Free Trade Agreement (NAFTA) based their arguments that the deal would raise Mexican living standard and wage levels, while also reducing incentives to migrate, on the economy theory underlying the Mundell model. However, the logic of the Mundell model is contingent on several important assumptions. Schiff (1996) demonstrated that if some of the assumptions about economies of scale are relaxed, trade and migration are more likely to be complements. Other assumptions of the Mundell model are undermined when there are sector-specific technological differences between trading partners and when potential migrants are relatively low-skilled and have low earning power. Consequently, increased trade liberalization, especially as brought on by NAFTA, may have encouraged additional illegal immigration. To control for either of these possibilities I include a proxy for trade openness in Mexico, defined as total trade (imports plus exports) divided by GDP.

Lastly, there is a time variable to capture any long terms trends and two IRCA dummy variables. The first dummy is for the application period, Nov. 1986 to April 1988, while the second dummy represents the entire post-IRCA period from Nov. 1986 onwards. It is important to control for the application period because there may have been different incentives for undocumented immigrants while they were still eligible to apply.

13 Andres Solimano, "International Migration and the Global Economic Order," *World Bank Working Paper* (2001).

Table 1. Variable names, definitions and sources of data.

Variable Name	Definition	Data Source
Dependent Variable		
LN_APPR	Natural log of monthly linewatch apprehensions along the U.S.-Mexican border	Unpublished field reports from the INS compiled by Professor Gordon Hanson of UCSD
Predictor Variable		
POSTIRCA	Effect of IRCA amnesty provision	Dummy variable (= 1 for Nov 1986 and all subsequent months)
Control Variables		
TREND	Effect of long term trends	Monthly (= 1 for Jan 1977 and increases to = 288 for Dec 2000)
IRCAFILING	Effect of IRCA amnesty provision during application period	Dummy variable (= 1 for Nov 1986 to April 1988)
LN_ENFHRS	Natural log of monthly U.S. Border Patrol hours devoted to linewatch activities	Unpublished field reports from the INS compiled by Professor Gordon Hanson of UCSD
WAGERATIO1	Ratio of hourly wage rate in U.S. non-agricultural sector (deflated by U.S. CPI) to hourly earnings in Mexican manufacturing sector (in Mexican pesos, deflated by Mexican CPI)	U.S. Bureau of Labor Statistics; International Labor Organization
WAGERATIO2	Ratio of hourly wage rate in U.S. non-agricultural sector (in Mexican pesos, deflated by Mexican CPI) to hourly earnings in Mexican manufacturing sector (in Mexican pesos, deflated by Mexican CPI)	U.S. Bureau of Labor Statistics; International Labor Organization
USUNEMPLOY	Total U.S. unemployment rate (percent)	U.S. Bureau of Labor Statistics
OIL	U.S. PPI for crude oil	U.S. Bureau of Labor Statistics
EXRT	Real exchange Rate between Mexico and the U.S. (pesos per $)	IMF International Financial Statistics
TRADE	The ratio of imports plus exports divided by GDP (measured annually and averaged over 12-month period)	IMF International Financial Statistics
LN_MEXPOP	Natural log of Mexican population 15-34 years of age (in millions, measured annually and averaged over 12-month period)	World Bank
LN_LAGINTAPPR	Natural log of monthly apprehensions of illegal aliens in the interior United States (lagged one month)	Unpublished field reports from the INS compiled by Professor Gordon Hanson of UCSD
JAN-NOV	Monthly dummy variables for season labor demand	Each monthly dummy = 1 for that month and =0 elsewhere

Method

I regress the natural log of border apprehensions on the predictor and explanatory variables listed in Table 1. Separate models are estimated for each wage ratio; the other variables do not change. The basic research design follows that of a single interrupted time series – measures of the outcome (dependent) variable, repeated for several time periods before a policy or program change, represent the counterfactual pretreatment status quo; measures of the same variable, repeated for several time periods after the change, are taken to reveal the impact of the change.[14] The specification I use does not include an interaction term for the predictor variable and time because there is no reason to expect the IRCA amnesty program would alter the trend of illegal immigration as time increases (i.e. there is an impact on the intercept but not on the slope). However, before an estimating equation can be derived, there are several issues that affect non-experimental designs that need to be addressed.

In order for regression estimates to be valid, the explanatory variables and error term must be independent of each other and there cannot be any reciprocal causation between the explanatory variables and the dependent variable. Violation of the former assumption reflects the presence of omitted variable bias (OVB), while violation of the latter indicates simultaneous equation bias. By definition, dual causality also implies that the explanatory variable(s) and error term will no longer be independent. Therefore, the presence of either OVB or SEB results in biased, internally invalid estimates of program impact. Valid estimation also requires that the dependent and explanatory variables be free of non-random measurement error (NRME). Removing the systematic component of error in INS data with dummy variables for each month satisfies this condition.

A central issue in this study is that border apprehensions are endogenous if increased flows of illegal immigrants cause a simultaneous increase in the level of border enforcement. Additionally, shocks to enforcement hours may be correlated with unobserved shocks to apprehensions, indicating that OVB may be a problem as well. Hanson and Spilimbergo (1999) describe a potential scenario where this occurs: "Suppose, for instance, that the INS acquires new enforcement technology - this could lead to a simultaneous increase in enforcement hours to implement the technology, and in apprehensions as the technology takes effect."

To correct for the effects of OVB and the endogeneity of enforcement hours, two separate equations are estimated using instrumental variables (IV) analysis. Following Orrenius and Zavodny (2003) and Hanson and Spilimbergo (1999), I instrument for border enforcement in the first stage equation with U.S. government expenditures on defense as the identifying variable. The defense budget is related to the number of enforcement hours because the budget allocates a set amount of fiscal resources to each agency, but this is not expected to directly influence the flow of undocumented immigrants. Although there is no consensus about what distinguishes whether the first stage IV equation is a "poor" fit to the data or a "good" one, the theoretical plausibility of the identifying variable and high R^2 (unreported) suggest that it is a suitable estimation. Thus, I use the following combined estimating equation:

14 Laura Langbein and Claire L. Felbinger, *Public Program Evaluation Guide: A Statistical Guide* (Armonk, New York: M.E. Sharpe Inc., 2006).

$$(1)\ \ln ENFHRS_t = a + b_1 POSTIRCA_t + b_2 TREND_t + b_k Zk_t + cW_t + e'_t$$

$$(2)\ln APPR_t = a + c_1 POSTIRCA + c_2 TREND + d\ln ENFHRS\hat{} + c_k Zk_t + e_t$$

$$where\ \ln ENFHRS\hat{} = a + b_1 POSTIRCA_t + b_2 TREND_t + b_k Zk_t + cW_t$$

Here, ln*ENFHRS*^ is the instrumental variable, W_t is the identifying variable (U.S. defense budget), Zk_t is a vector of all other explanatory variables from Table 1, and e_t is a stochastic error term.

In principle, equation (2) can be estimated by OLS; however, if there is serial correlation (autocorrelation) in the error term OLS will produce biased estimates of coefficients and standard errors. The presence of non-independent observations is particularly likely in time-series data, so the problem of autocorrelation is to be expected. Despite controlling for time with a trend variable, the Durbin-Watson test for OLS estimation indicated the presence of serial correlation. Therefore, I estimate the equation using a first-order autoregressive model; unreported Durbin-Watson test results confirm the AR(1) correction sufficiently captures the serial correlation. By lagging the dependent variable, the AR(1) model also embeds an additional correction for SEB and OVB in the design. Robust standard errors are used to correct for heteroskedasticity in the error term.

Descriptive Results

Apprehensions have clearly been on the rise during the 24-year sample period. The descriptive statistics in Table 2 indicate that there was an average of nearly 48,000 apprehensions per month in the 118 months (approximately 9 years) prior to IRCA, compared with almost 76,000 per month in the 152 months (12.5 years) after the application period for legalization under the IRCA ended. There was an average of 59,000 apprehensions per month during the 18-month application period. The number of Border Patrol hours devoted to linewatch activities follow a similar pattern, more than doubling in size from an average of 161,000 per month prior to IRCA to an average of 362,000 per month after the amnesty application deadline. This massive increase is not surprising because it reflects the provisions of the IRCA designed to increase border enforcement capabilities. Specifically, the number of Border Patrol personnel increased from 3,687 in fiscal year 1986 to 4,669 in fiscal year 1988.[15] On the other hand, the number of apprehensions in the interior of the United States drops from an average of 32,000 per month prior to IRCA, to around 28,000 per month in the post-IRCA period. Since raids on businesses comprise one of the larger components of interior apprehensions, the drop may signal that the employer sanctions provision of IRCA was not strictly enforced.

The statistics in Table 2 also indicate that U.S. and Mexican real wages, as well as the U.S. unemployment rate and price of crude oil, are declining over the sample period. The substantial drop in the Mexican real wage rate and the price of crude oil are particularly telling because they represent deteriorating economic conditions in Mexico. Coupled with a rising

15 White, Bean, and Espenshade, "1986 Immigration Reform."

exchange rate, this inflates the wage ratio when the constant peso U.S. wage is the numerator. Additionally, the "at-risk" Mexican population exhibits steady growth over the sample period, with an average of 23 million people before IRCA compared to an average of nearly 33 million people in the post-IRCA period. Likewise, the average trade ratio is twice as large in the post-IRCA period relative to the period before the legislation. This provides empirical evidence for using "trade openness" to represent the forces of globalization and the increase in economic liberalization induced by NAFTA in the early 1990s.

Table 2. Sample means

	Total Period 1/77-12/00	**Pre IRCA 1/77-10/86**	**IRCA Appl. Period 11/86-4/88**	**Post-IRCA 5/88-12/00**
N	288	118	18	152
Apprehensions (in thousands)	63.23 (28.78)	47.76 (16.98)	58.93 (14.12)	75.74 (31.34)
Enforcement (hours, in thousands)	269.63 (176.52)	161.24 (17.65)	200.22 (20.99)	361.99 (201.46)
U.S. real wage ($)	8.08 (0.38)	8.41 (0.35)	8.05 (0.07)	7.82 (0.18)
Mexican real wage (pesos)	25.42 (6.45)	31.14 (5.74)	23.93 (6.22)	21.16 (2.22)
U.S. Unemployment rate	6.39 (1.51)	7.49 (1.36)	6.16 (0.57)	5.56 (1.11)
Price of Crude Oil (per barrel)	63.35 (21.59)	73.60 (25.86)	52.54 (5.40)	56.68 (14.97)
Real Exchange Rate (pesos per $)	3.49 (3.34)	1.21 (0.61)	1.52 (0.51)	5.49 (2.86)
Trade Openness Ratio	0.38 (0.15)	0.25 (0.03)	0.34 (0.03)	0.49 (0.13)
Mexican Population (ages 15-34, in millions)	28.52 (5.17)	23.138 (2.08)	27.53 (0.50)	32.81 (2.47)
Interior Apprehensions (in thousands)	29.55 (10.25)	32.09 (12.46)	28.87 (8.59)	27.67 (7.91)

Note: Standard deviations are in parentheses.

Regression Results

It is important to recall that using apprehensions data as a proxy for the flow of undocu-

mented immigrants limits the interpretation of coefficients to inferences about directional impact only. As Table 3 indicates, the post-IRCA period is associated with a decline in the number of border apprehensions in both models. The dummy variable for the amnesty application period is also associated with a decrease in border apprehensions in both models. This suggests that the amnesty provision of IRCA may have actually been responsible for a reduction in illegal immigration when other factors that affect migration rates are controlled for. The relative decline in apprehensions during the application period is consistent with previous research (e.g. Bean et al., 1990 and White et al., 1990). However, the negative association between apprehensions and the long-term impact of the IRCA amnesty program is a new finding. Orrenius and Zavodny (2003) is the only other empirical analysis that tested for a long-term impact of IRCA, and they found that the post-IRCA period (5/88-12/96) to be associated with an increase in apprehensions, albeit the coefficient is not statistically significant and is actually very close to zero. On the contrary, I find that there are fewer apprehensions in the post-IRCA period (1986-2000) than there would have been without the amnesty program.

Inability to estimate the magnitude of IRCA's impact on the flow of undocumented immigrants makes interpreting this result inherently difficult and limits its substantive significance. This problem also makes it harder to theorize why amnesty may have had this type of effect. The most plausible explanation is that the legalization program removed a large number of individuals from the "at-risk" population. This is expected to reduce the number of apprehensions because the same individuals who would have been traveling between Mexico and the U.S. illegally are now able to cross the border freely in either direction. In this context, the amnesty provision did not alter the trend of illegal immigration; instead, it shifted the intercept of the trend line down, reflecting a smaller "at-risk" population.

The results also indicate that enforcement, instrumented with U.S. defense spending, is positively associated with apprehensions in both specifications of the model. Since both variables are measured in log form, the coefficient in Model 1 can be interpreted to mean that a 10 percent increase in Border Patrol hours is associated with a .57 percent increase in the number of border apprehensions. The effect in Model 2 is about the same. However, the coefficients are not statistically significant and the 95% confidence intervals cannot be used to infer a directional impact because the ranges span both positive and negative numbers. Another way to interpret this result is that it may imply enforcement is not an effective deterrent for illegal immigration. Espenshade (1994) also found that enforcement had a negligible impact on apprehensions and concluded that the threat of border apprehension is not likely to be a strong deterrent for migrants who have made it as far as the U.S.-Mexican border. Similarly, Donato et al. (1992) cite anecdotal evidence from case study interviews that suggests none of the interviewees were prevented from entering the United States when they wanted to go. Two possible conclusions can be drawn from the enforcement discussion – the theoretical relationship between the probability of apprehension and the flow of undocumented immigrants is flawed, and/or enforcement efforts are undermined by the ability of illegal immigrants to evade Border Patrol agents.

The lag of interior apprehensions is also positively associated with border apprehensions, with a 10 percent increase in interior apprehensions associated with a 1.65 percent increase in apprehensions in Model 1, and a 1.57 increase in apprehensions in Model 2. This result is consistent with theoretical expectation that illegal aliens of Mexican descent who are apprehended in the interior of the United States are likely to attempt re-entry as soon as they are deported. Results in Table 3 also indicate that the degree of trade openness is positively associated with apprehensions in both models. Although interesting, this finding does not come as a great surprise. A wide body of empirical evidence on how NAFTA encourages illegal immigration (e.g. Markusen and Zahniser, 1997) supports Schiff's (1996) theory that under certain conditions, trade and migration will act as complements.

The only difference between the two models is observed in the alternative wage ratio measures. Although the coefficient for the wage ratio (constant U.S. wage in the numerator) is much larger in Model 1, it is not statistically significant. On the other hand, the coefficient for the wage ratio in Model 2 (constant peso U.S. wage in the numerator) is positive and statistically significant at the 0.05 level. This can be interpreted to mean that an increase in the relative Mexican earning power of U.S. currency is associated with an increase in the flow of undocumented immigrants. Hanson and Spilimbergo (1999) also find that apprehensions are more sensitive to Mexican wages than U.S. wages and posit that this may be due to the relative volatility of the Mexican economy, giving Mexican wages greater weight in the illegal immigration decision. As expected, the exchange rate is negatively associated with apprehensions in both models, but it is only statistically significant in Model 2.

Though unreported, the month dummy variables are all statistically significant, which suggests they captured the seasonal variations in apprehensions. None of the other explanatory variables are statistically significant in either model.

Table 3. AR(1) Regression Results for the Natural Log of Monthly Apprehensions at the U.S. Mexico-Border, 1977-2000.

Model 1 – estimated using wageratio1			
Variable	**b**	**SE**	**95% CI**
POSTIRCA	-.318***	.090	(-.495 & -.140)
IRCAFILING	-.156**	.067	(-.288 & -.024)
TREND	.010	.009	(-.006 & .027)
LN_ENFHRS^	.057	.152	(-.241 & .356)
WAGERATIO	.410	.346	(-.269 & 1.088)
USUNEMPLOY	.005	.023	(-.040 & .049)
OIL	-.001	.001	(-.004 & .001)
EXRT	-.062	.041	(-.143 & .018)
TRADE	1.303**	.536	(.251 & 2.354)
LN_MEXPOP	-.251	.337	(-3.401 & 2.478)
LN_LAGINTAPPR	0.165***	.056	(.055 & .275)
Constant	26.708	33.834	(-39.604 & 93.021)
Adjusted R	0.891		
N	288		

Model 2 – estimated using wageratio2			
Variable	**b**	**SE**	**95% CI**
POSTIRCA	-.291***	.096	(-.480 & -.102)
IRCAFILING	-.168**	.070	(-.306 & 0.030)
TREND	.012	0.008	(-.003 & .029)
LN_ENFHRS^	.060	.147	(-.227 & .348)
WAGERATIO2	.023**	.012	(.001 & .046)
USUNEMPLOY	.002	.022	(-.040 & .045)
OIL	-.001	.001	(-.003 & .001)
EXRT	-.069*	.041	(-.149 & .011)
TRADE	1.141**	.569	(.024 & 2.258)
LN_MEXPOP	-.334	.321	(-.963 & .295)
LN_LAGINTAPPR	.157***	.055	(.049 & .264)
Constant	34.967	32.146	(-28.046 & 97.965)
Adjusted R	.922		
N	288		

*p<.10. **p<.05. ***p<.01

Note: Month dummy variables are included in both models but are not shown.

Discussion and Conclusion

This study has developed and estimated a statistical model to analyze the long-term effect of the IRCA amnesty program on border apprehensions over the period 1977 to 2000. The

analysis diverges from previous research on IRCA by examining a much longer post-IRCA period. The results show that the amnesty program is associated with a decline in the number of apprehensions at the U.S.-Mexican border in the fourteen years since it was enacted. The amnesty impact also does not appear to dissipate over time. The same two models were estimated in two-year intervals of the post-IRCA period (i.e., they were estimated for the time period 1/77-11/88, then for 1/77-11/90, 1/77-11/92, and so forth). Results are not reported, but the post-IRCA dummy, which captures the effect of the amnesty program, was statistically significant and associated with a decline in the number of apprehensions in every model. The empirical evidence from this analysis implies that amnesty programs do not encourage illegal immigration, contrary to the vigorous claims of some critics of amnesty programs. However, refuting this claim should not be misconstrued as tacit support for a widespread amnesty program. In no way have I endorsed mass-legalization as sound public policy. This analysis does not consider the potential costs and other financial distortions that are likely to accompany a massive amnesty plan. Nor does it consider how this might affect different sectors of the U.S. economy, not to the mention the strains it would place on Social Security. A true assessment of the overall impact of an amnesty program requires a highly detailed cost/benefit analysis.

Another major finding in this study is that the level of border enforcement does not affect the number of border apprehensions. From a public policy perspective, this suggests that allocating additional funds to border enforcement activities may not be an efficient use of public resources; rather, diverting funds to other types of deterrent strategies may have a greater impact on the flow of illegal immigrants. However, this should not be misinterpreted as a call to reduce Border Patrol funding, either. First off, border enforcement was not the main predictor variable being examined in this study and may require another model specification to fully capture its effect on apprehensions. Second, an increasingly large share of Border Patrol resources has been devoted to drug interdiction.[16] To the extent drug interdiction is a higher priority than apprehending illegal immigrants for certain Border Patrol officers, the increase in enforcement hours may not reflect an increase in the hours devoted to linewatch apprehension activities. This is one possible explanation for the non-finding on border enforcement.

The other significant finding from this study is that the constant peso U.S. wage ratio and trade liberalization are both associated with an increase in border apprehensions. The latter result reflects that NAFTA may have done more harm than good to the Mexican economy. Critics of NAFTA claim that the trade agreement created a need for intercontinental dependency in Mexico, which resulted in fewer job opportunities and lower wages. Consequently, there were higher levels of illegal immigration. The findings for the constant peso U.S. wage ratio are based on a similar concept: economic volatility in Mexico contributes to border apprehensions. Taken together, these results indicate that economic conditions in Mexico have the greatest impact on the flow of undocumented immigrants. Improving the Mexican economy may be the most effective and efficient deterrent strategy for illegal immigration.

16 Espenshade, "Threat of Border Apprehension."

Some limitations regarding my results warrant discussion, particularly in the research design. Although defense spending provided a rare opportunity to perform IV analysis, relying on an IV as a cure-all for OVB and SEB can be problematic. In a scenario where defense spending is actually somehow related to border apprehensions, the IV will have more random error relative to the original variable, thereby increasing rather than reducing threats to internal validity.

There is also some concern about the statistical procedures used to estimate the regression equation. The first-order autoregressive model reduces serial correlation; however it does not eliminate it completely. The Durbin Watson test statistic for this model was barely above the accepted threshold for evidence of no serial correlation, which suggests that the AR(1) term did not fully remove the serial correlation. As an alternative estimating procedure to correct for serial correlation, I could have used first-order differences. Transforming each variable into a difference reduces the threat of serial correlation because the differences are usually independent and also reduces the threat of OVB. The disadvantage, of course, is that the variables are no longer in their original firm and may be harder to interpret. A possible solution would be to estimate the equation using log-first differences so that all variables could be interpreted on a similar scale. Nevertheless, the debate on illegal immigration is not going anywhere and is sure to require further analysis on amnesty programs as well as other deterrent strategies for illegal immigration.

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