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## Legalization and the Economic Status of Immigrants

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# Legalization and the Economic Status of Immigrants

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## Abstract

This paper investigates the impact of legalization on the economic outcomes of the legalized population. It uses a natural experiment caused by the 1986 Immigration Reform and Control Act (IRCA) which gave amnesty for undocumented immigrants who could prove continuous residence in the U.S. after January 1, 1982. The arbitrary cutoff date on the eligibility criteria causes a discontinuity in the relationship between the year of immigration and the probability of being legal. This paper uses this discontinuity to identify the causal impacts of legalization on immigrants' outcomes. Regression discontinuity and difference-in-differences estimates show that immigrants eligible for the policy have a significantly higher probability of being naturalized citizens than those who were not. Legalization is also found to have a positive and significant effect on wages, a negative effect on the probability of working in a traditionally illegal occupation, and no significant effect on geographical mobility. The analysis for different demographic groups confirms such conclusions and shows that the estimated effects of legalization are larger for low-educated Latin American immigrants, the group that was disproportionately affected by the policy.

## 1 Introduction

Current proposals for U.S. immigration policy reform include a possible legalization program for undocumented migrants, a population estimated to be 11.9 million in 2008 (Passel and Cohn (2008)). Despite the implementation of a major amnesty for such workers in the late eighties, not enough is known about the economic effects of changing the legal status of this population. How legalization might impact immigrants' earnings, occupational choices, and where they choose to live all have critical implications for public policy. Illegal status might prevent immigrants from working on jobs that best match their skills, generating

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inefficiencies in the labor market. This paper will investigate how legalization affects the economic outcomes of the previously undocumented immigrants.

Economic theory predicts legalization can affect earnings, occupation and mobility of illegal immigrants. On the one hand, undocumented workers not only maximize earnings when making labor market decisions but also minimize the probability of detection by the immigration authorities. On the other hand, employers might fear the risk of being punished for hiring illegal immigrants, and this will change both the type of jobs that are available to undocumented workers and the treatment these workers get in these jobs. Illegal status generates barriers that constrain the choices of both workers and employers, and these constraints increase in severity the more human capital a worker has. In this sense, legalization could be interpreted as a removal of such barriers which could potentially improve the quality of job-worker matching and the overall efficiency of the labor market.

There are two main challenges to the empirical study of the effects of legalization on the economic status of immigrants. The first is the lack of information about legal status on major nationally representative household surveys. The second is an econometric issue. It is hard to determine if the observed differences in the outcomes of legal and illegal immigrants are due to legal status itself, or to an unobserved selection process that characterizes legal and illegal migration.

Here I propose an empirical strategy that overcomes both problems. I use the natural experiment created by the Immigration Reform and Control Act (IRCA) legalization program in 1986. Among other measures, the IRCA gave an amnesty for undocumented immigrants who could prove continuous residence in the U.S. after January 1, 1982. This rule causes a discontinuity on the relationship between the year of immigration and the probability of being legal in 1982. In other words, all else equal, an immigrant who arrived in the U.S. before 1982 should have, at any point in time, a higher probability of being legal than an immigrant who arrived after 1982.

I will use this discontinuity in two different ways to identify the causal impacts of legalization on immigrants' outcomes. First, I will use a regression discontinuity framework.

Except for the policy, there are no reasons to believe that there should be sharp differences in the observable or unobservable characteristics of immigrants who first arrived to the U.S. in the few years before or the few years after 1982. The regression discontinuity framework then implies that any estimated sharp differences in immigrants' outcomes in 1982 can be interpreted as causal effects of the policy. I use data from the March Current Population Survey (CPS), the American Community Survey (ACS) and the CENSUS from different survey years to estimate such discontinuities.

Second, I will use a difference-in-differences approach. In this strategy I assume that, in the absence of the policy, the relationship between years in the US and the outcome variables would be similar for immigrants observed in 1990 and 2000. Given this assumption, immigrants who have been in the US for less than 8 years are a good control group for immigrants who have been in the US for 8 to 18 years. Immigrants in the treatment group will be treated when observed in 1990 (since they arrived sometime between 1972 and 1981, being affected by the policy) but not when observed in 2000 (and therefore arrived after 1982 and where not eligible to legalization). The difference-in-differences analysis is performed using data from the 1990 and 2000 CENSUSES.

Previous studies that analyze the effects of IRCA's legalization programs on newly legalized immigrants use data collected by the amnesty program itself and focus on estimating the effects of legalization on wages. Borjas and Tienda (1993) use administrative files of information provided to the Immigration and Naturalization Service (INS) by the legalized immigrants upon applying for amnesty. They describe the demographic and labor market characteristics of this population and compare those with a sample of the foreign-born population from the CPS. The authors document how illegal immigrants have lower wages, wage growth, and returns to human capital than legal immigrants. However, their data does not allow them to investigate the reasons for these observed differences.

Singer (1994) uses a short panel dataset of immigrants who applied to the IRCA's general legalization program. The Legalized Population Survey (LPS) interviewed a random sample of amnesty applicants in 1988, two thirds of whom were re-interviewed in 1992. The LPS

sample is restricted to Mexican and Central American men who came to the U.S. to stay after 1975, who were born after 1944, and who worked prior to legalization. Singer (1994) estimates cross-sectional earnings equations for amnesty recipients before and after legalization and points to important changes in wage determinants after legalization. Using the panel aspect of the LPS, Rivera-Batiz (1999) investigates the impact of amnesty on the wages of formerly unauthorized workers and concludes that they enjoyed significant wage growth in the first 4 years after legalization.

A major drawback of the LPS data is the lack of a control group. Without such a group it is impossible to know if the gains reported by previous studies are due to the change in legal status or to other factors. Kassoudji and Cobb-Clark (2002) construct a comparison sample using the National Longitudinal Survey of Youth (NLSY). To make the group as comparable as possible to the sample of newly legalized immigrants, they restrict the sample to Latino men (natives and immigrants) who were not in the NLSY military sample and who were new labor market entrants. Using this comparison group, they conclude that the wage benefit of legalization under the IRCA was approximately 6 percent. However, as they point out, the lack of a control group prevents the interpretation of this finding as the causal effect of legalization on wages. Any unobserved differences between the sample of legalized immigrants and the NLSY constructed comparison group could bias their estimates in unpredictable directions.

This paper advances the literature in several dimensions, the most important being the construction of a credible control group. The use of the discontinuity caused by the IRCA in two different empirical strategies provides an improvement compared to existing approaches. Other advantages of this paper are related to the use of large, nationally representative samples of the population. Different from the LPS, the CENSUS, ACS and CPS samples are not restricted in any way, so that I can analyze the impact of the program for different subsamples of the population, including the ones for whom one would not expect a large impact of the program, for validation purposes. Furthermore, since these surveys are conducted periodically, I can compute the impacts of legalization at different points in time,

after different numbers of years since legalization. Finally, wages are only one dimension of the labor market outcomes of immigrants that we expect to be affected by legalization. The data used here allow the analysis of outcomes other than earnings, such as occupation and geographical mobility.<sup>1</sup>

One limitation of the present study is that legalization might also affect the composition of the sample, which could bias the estimate of its effect on immigrants' labor market outcomes. If legalization affects immigrants' decision to answer household surveys like the CPS or the CENSUS or to migrate back to their home country, the pre-82 immigrants (eligible for legalization) will be a selected sample when compared to the post-82 sample. The inspection of baseline covariates show that pre-82 immigrants are on average more likely to be low-educated Mexicans, which is consistent with the legalization program affecting survey response and /or return migration. Even though the composition effects are hard to disentangle from the true effects of legalization, the results presented are robust to the inclusion of individual level controls for sex, education, marital status, race, age and region of origin, which suggest that the bias does not affect the qualitative conclusions.

The results show that immigrants eligible for IRCA's legalization program have a significantly higher probability of being naturalized citizens than those who were not. The magnitude of this effect is estimated to be around 10% across different datasets and for the regression discontinuity and difference-in-differences estimator. Since legalization is a necessary step for naturalized citizenship, I interpret this as evidence that the program can be identified in the data. Legalization is estimated to have a positive and significant effect on wages, a negative effect on the probability of working on a traditionally illegal occupation, and no significant effect on geographical mobility. The estimated effects from the regression discontinuity and the difference-in-differences models are similar in signs, but the magnitudes implied by the difference-in-difference estimates are larger. Finally, the analysis for different demographic groups shows that the estimated effects of legalization are larger for

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<sup>1</sup>Kossoudji and Cobb-Clark (2000) use the two waves of the LPS to examine how occupational outcomes changed for a group of immigrants legalized under the IRCA. However, as discussed above, the LPS does not provide a control group which prevents the estimation of the causal impact of legalization.

low-educated Latin American immigrants, the group that was disproportionately affected by the policy.

The rest of the paper is organized as follows. After this brief introduction, a background section explains the details of legalization under the IRCA and the mechanisms through which legalization can affect immigrants' outcomes. Section III presents the data and is followed by the methodology section that discusses the regression discontinuity and difference-in-differences approaches. Section V reports the results from the two empirical strategies and section VI concludes.

## 2 Background

### 2.1 Immigration Control and Reform Act (IRCA)

The Immigration Control and Reform Act (IRCA) was passed in October 1986 with the main objective of combating undocumented immigration to the United States (Bean et. al (1989)).<sup>2</sup> The law sought to do this in three different ways. First, it imposed sanctions on employers who knowingly hired undocumented workers. Second, to deter further illegal immigration, it allocated new resources to expand Border Patrol. Finally, it authorized the legalization of undocumented immigrants under two different legalization programs. The first, focused on legalizing farm workers who could demonstrate they had worked in the United States for at least 90 days during the 12 months preceding May 1, 1986. The second program legalized immigrants who could prove continuous residence in the United States after January 1, 1982.

The two IRCA legalization programs generated a massive wave of legalization: by 1990, they had distributed documents to more than 3 million people. Of these, 1.7 million demonstrated long-term residence in the United States, and 1.3 million documented prior em-

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<sup>2</sup>Estimates of the size of the undocumented population seem to show that the IRCA was not successful on attaining its main goal. In 1980, the size of the illegal population was estimated to be between 2 and 4 million people (Bean, Edmonston and Passel (1990)), and to have grown in a range of 100,000 to 300,000 per year during 1980 to 1986 (Woodrow, Passel and Warren (1987)). In 2000, fourteen years after the passage of the IRCA, the estimated size of this population had more than doubled (at least), reaching 8.4 million (Passel and Cohn (2008)). See Donato, Durand and Massey (1992) for an assessment of IRCA's deterrent effect on the flow of unauthorized migrants.

ployment in U.S. agriculture. From the total number of people legalized, 2.3 million were Mexicans and 54 percent resided in California at the time of legalization (800,000 in the Los Angeles County alone) (Massey (2008)).

Many of those legalized under the IRCA were household heads, so due to family reunification, the IRCA became a trigger for additional migration. In 1992, 52,000 dependents of persons earlier legalized under the IRCA were granted permanent residence, followed by another 55,000 in 1993 and 34,000 in 1994. In addition to this increased legal migration, there was also an illegal movement triggered by the program estimated to average approximately 300,000 persons per year (Durand et. al (1999)).

This paper will analyze the impact of legalization on the outcomes of the immigrants who were legalized by proving continuous residence in the U.S. after 1982. The IRCA specified that the best way for immigrants to prove long-term residence was through past employment records. However, most illegal immigrants might have had difficulty getting such documentation, partly because of their prior efforts to remain undetected. Moreover, employers might have been reluctant to provide the necessary records because they may have revealed prior noncompliance with tax and labor laws. To mitigate these problems, the IRCA included confidentiality provisions specifying that “information submitted in applications for legalization is confidential and is not available to any division of the Department of Justice without the consent of the alien” [Marx (1987)]. Even with this provision, a large fraction of eligible undocumented immigrants might not have applied for legalization. Woodrow and Passel (1990) use CPS data and estimate that nearly one out of every three pre-1982 Mexican undocumented immigrants who were in the country in June 1988 did not apply for legalization. These individuals may have been unable to gather the necessary proof to apply for amnesty or may have been planning to reside in the United States for only a short while more, so that applying for legalization might not have been worth it. Of the two-thirds eligible immigrants who did apply, 90 percent were granted permanent residence (Rytina (2002)).

## 2.2 The Mechanisms Through which Legalization Affects Immigrant's Outcomes

There are several mechanisms through which economic theory predicts that legalization can affect earnings, occupation and mobility of illegal immigrants. Unauthorized workers are less likely to make labor market choices with the sole objective of maximizing earnings; they also have the competing aim to minimize the risk of apprehension by the Immigration and Naturalization Service (INS). On the other side of the market, employers might fear the risk of being punished for hiring illegal immigrants, which will change both the type of jobs that are available to undocumented workers and the treatment these workers get in these jobs.

A lack of legal status might alter workers' behavior. Illegality makes the process of searching for alternative wage offers risky and difficult, inducing illegal immigrant workers to lower their reservation wages to levels below those of legal workers (Bailey (1985)). Moreover, if undocumented workers rely on migrant networks to learn information about jobs, this will reduce their geographical and occupational mobility to areas where other illegal immigrants are concentrated and these networks are present (Massey et al. (1987); Kossoudji and Cobb-Clark (1996)). Finally, unauthorized workers might be less likely to know or exercise their labor rights due to the fear of being reported to the immigration authorities.

Illegal status might also allow employers to exert monopsonistic power over the workers, such as the case in immigrant smuggling networks (Rivera-Batiz (1999)). Another way to see it is that some employers might be willing to trade off the greater risk of employing illegal workers with the benefit of paying lower wages. Illegality might restrict the employment choices available to workers to few employers in marginal jobs where they can be sheltered from detection, usually in declining industries that offer comparatively low pay (Cornelius (1976); Gill and Long (1989); Taylor (1992)).

There is evidence that illegal immigrants from Mexico are drawn disproportionately from the middle of the country's schooling distribution (Hanson (2006)).<sup>3</sup> However, illegal status

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<sup>3</sup>The notion that immigrants are positively selected was challenged by Moraga (2008) who finds that Mexican immigrants earn lower wages and have less schooling years than individuals who remain in Mexico. However, his results are based on a much later flow of immigrants than the ones affected by the IRCA, when selectivity could have been different.

imposes barriers to these workers that prevent them from making job and location choices that maximize the returns on their human capital. Legalization can then be interpreted as the removal of such barriers. Once immigrants are legalized we expect them to have greater geographical mobility,<sup>4</sup> to be less likely to be employed in traditionally illegal occupations and to have higher wages. This process should improve the quality of job-worker matching and increase the efficiency of the job market as a whole.

### 3 Data

The data used in this project include the 5 percent samples of the 1990 and 2000 CENSUS, the 2005 ACS and the yearly March Current Population Survey (CPS) from 1994 to 2009.<sup>5</sup> The data were obtained from the Integrated Public Use Microdata Series (IPUMS) (King, Ruggles, Alexander, Leicach, Sobek (2009)).

The sample is restricted to immigrants, defined as respondents who are either non-citizens or naturalized citizens.<sup>6</sup> This paper investigates the effect of legalization on the mobility and labor market outcomes of immigrants, so I restrict the sample to working age immigrants, that is, immigrants 16 to 64 years old who are not currently enrolled in school.

Hourly wages are calculated by dividing annual earned income from wages, business and farm activities by the product of weeks worked during the year and usual hours worked per week. Wages are inflated to 2009 values using the Consumer Price Index (CPI).

The occupation variable is defined as the probability that the immigrant's primary occupation is one that traditionally hires many illegal immigrants. To classify what a traditionally illegal occupation is, I will follow Kossoudji and Cobb-Clark (2000) who used the LPS data to identify the ten most important (in terms of representation) occupations held by the re-

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<sup>4</sup>One exception is if the newly legalized immigrants decide to bring their families to the U.S. as a consequence of legalization. As explained above, legalization under the IRCA triggered the migration of many dependents of newly legalized workers. The presence of spouse and children might lead to decreased mobility instead of increased mobility.

<sup>5</sup>One option would be to use the monthly CPS data, which have larger sample sizes than the March supplements. However, the monthly CPS does not contain information about geographical mobility, so for consistency I use the March CPS for the analysis of all outcomes.

<sup>6</sup>Another way to define who is an immigrant is anyone who was born abroad. The main difference of this definition from the definition we are using are the respondents born abroad from American parents (who are U.S. citizens). Since this group is numerically small, the use of any of the two definitions should yield comparable results.

spondents at the time they filled the legalization application.<sup>7</sup> In 1990, approximately 27 percent of all immigrants held jobs in those 10 occupations (47 percent of immigrants with less than high school education) and 15 percent of total employment in those occupations was composed by immigrants.

Finally, the mobility outcome reflects the probability that the immigrant lives in a different state than the one he lived 5 years ago (or 1 year ago for CPS and ACS data). For immigrants who first arrived in the US sometime during the 5 years before the CENSUS survey year (or 1 year before, for the CPS and ACS) this variable will always be equal to 1 (since they lived abroad 5 years ago), so I drop them from the mobility analysis.<sup>8</sup> Note that this will make the samples from the CENSUS and CPS differ and this together with the difference in the question reference period will make the results for geographical mobility not comparable across surveys.

One limitation to the study of illegal immigration is the lack of information about legal status on the major household surveys such as the CENSUS and the CPS. Direct questions about legal status are highly sensitive and might increase survey non-response among the illegal population, posing threats to the aims of these surveys to be nationally representative. Given this lack of information, I will use the question on year of immigration to learn about legal status.

As explained above, under the IRCA, immigrants had to prove that they had been living continuously in the U.S. since 1982 in order to take advantage of the legalization program. So, if we compare immigrants with different years of arrival in the U.S., those who arrived before 1982 should have a higher probability of being legal than those who arrived afterwards. The CENSUS, the ACS, and the CPS contain information on the year of immigration, and I will use this information on the empirical analysis.<sup>9</sup>

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<sup>7</sup>These occupations include: farm workers, food counter occupations, groundskeepers/gardeners, janitors/cleaners, compact machine operators, constructions laborers, laborers (except construction), cooks, assemblers, and auto body repairers. According to the LPS, 45% of the sample of illegal immigrants was employed in one of these top ten occupations at the time they applied for legalization.

<sup>8</sup>Furthermore, I will not include the 2005 ACS or the 1995 CPS in the mobility analysis. The 2005 ACS contains 1-year migration variables only, which is not comparable with the 5-year migration variables from the CENSUS. Similarly, the 1995 CPS only contains 5-year migration variables, differently than the other CPS years.

<sup>9</sup>Redstone and Massey (2004) note that the wording of the year of arrival question could lead to different interpretations and answers, which could compromise the use of this question to derive a measure of U.S. experience. However, for the purpose of our exercise, the accuracy of the measure of U.S. experience derived from the year of immigration question is of secondary

One limitation of the available data on the date of immigration is that in most surveys the response to this question is coded in groups of years, varying from 2 to 5 years grouped together. This is particularly undesirable in the context of a regression discontinuity analysis, since misspecification of the functional form typically generates a bias in the treatment effect (Lee and Lemieux, 2009).<sup>10</sup> This paper uses several different surveys in the analysis but only the 2000 CENSUS and the 2005 ACS contain disaggregated data on the year of immigration.<sup>11</sup> I will use evidence on the shape of the relationships between the year of immigration and the outcomes of interest using these two surveys to argue for the functional form assumptions for other surveys. Besides that, I will also present estimates from a difference-in-differences framework, which are less vulnerable to the misspecification problem.

Table 1 shows some summary statistics of the CPS, CENSUS and ACS data. Statistics for three different subsamples are presented: all immigrants, immigrants less likely to be affected by the legalization policy and immigrants more likely to be affected by legalization policy. Administrative data on IRCA’s legalization program show that 2/3 of the legalized immigrants were Mexicans and the majority of those had less than high school. Based on this information, the subsample of non-Latin American immigrants with college degrees or higher defines the subsample of immigrants less likely to be affected by the policy while Mexican immigrants with less than high school are more likely to be affected.<sup>12</sup>

Comparing the means of the outcome measures, one can see that the subsample of highly educated non-Latin American immigrants is more likely to be naturalized citizens and to have moved between states, less likely to work on traditional illegal occupations, and earns higher wages than the subsamples of all immigrants and of low skill Mexican immigrants.

These differences are expected and are present both in the CPS and in the CENSUS/ACS

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importance. What is required is that the answers to the year of immigration question bear some relation to the year since the immigrant could prove continuous residence in the country. Any misclassification in this sense would cause downward biases in the estimates so that my results could be interpreted as a lower bound for the effects of legalization.

<sup>10</sup>The consequences of using an incorrect functional form are less serious in a regression framework, for example, when one runs a linear model where the true functional form is nonlinear, the estimated model can still be interpreted as a linear predictor that minimizes specification errors. But since specification errors are only minimized globally, large specification errors are still possible at specific points including the cutoff point, which could generate a large bias in RD estimates of the treatment effect.

<sup>11</sup>For the other surveys, the year of immigration variable was recoded to be equal to the mean of the group of years. For example, if the grouping of years include 1980 and 1981, the year of immigration would be recoded to be 1980.5.

<sup>12</sup>These two groups of immigrants probably had very different probabilities of being illegal to begin with, which explains the different impacts of the policy.

data.

## 4 Methodology

The principal goal of this paper is to estimate the impact of legalization on the earnings, occupation and geographical mobility of newly legalized immigrants. One equation one could think of estimating to answer this question is

$$Y_{it} = \beta_0 + \beta_1 L_{it} + W_{it}\theta + Yr_t + u_{it} \quad (1)$$

where  $Y_{it}$  is some measure of mobility or labor market outcome of immigrant  $i$  at year  $t$ ,  $L_{it}$  is a dummy for legal status,  $W_{it}$  is a vector of controls,  $Yr_t$  is a vector of time fixed effects and  $u_{it}$  is the error term. In this case,  $\beta_1$  would be the parameter of interest.

There are two main problems with estimating this equation and interpreting  $\beta_1$  as the causal impact of legalization on immigrant's outcomes. First, none of the large nationally representative surveys in the U.S. has information about  $L_{it}$ , the immigrant's legal status. Second, even if such information were available, the estimate of  $\beta_1$  would be biased in this setting because legal and illegal immigrants are different in terms of unobservables. For example, legal immigrants might earn higher wages not only because they are legal, but also because they may have a higher ability level than illegal immigrants. Since we cannot observe (and control for) ability this would generate a correlation between legal status and the error term, which would in turn bias the estimate of  $\beta_1$  (upwards, in the case of this example).

Here, I use a natural experiment to overcome these two problems. I focus on the wave of legalization that was authorized by the Immigration Reform and Control Act (IRCA) in 1986. Among other measures, the IRCA gave an amnesty for undocumented immigrants who could prove continuous residence in the United States after January 1, 1982. This rule should cause a discontinuity in 1982 in the relationship between the probability of being legal and the year of immigration. As discussed in the introduction, I use this discontinuity in two

different ways to identify the causal impacts of legalization on immigrants' outcomes: in a regression discontinuity and a difference-in-differences framework. The next two subsections will explain in detail the specifications used in these two empirical strategies.

One important first step for this analysis is to show that the IRCA increased the proportion of legal immigrants (by decreasing the proportion of illegal immigrants) present in the data. Since there is no information on legal status in these datasets, I will use information on citizenship as a proxy for legal status. Legalization is a necessary first step to naturalized citizenship and a fraction of those legalized under the IRCA eventually finished the naturalization process. According to the Department of Homeland Security, 40 percent of the general legalization applicants granted permanent residence had become naturalized citizens by 2001 (Rytina (2002)).

Figure 1 shows the fraction of naturalized citizens among all immigrants by year of immigration, for the 1990 and 2000 CENSUSES and the 2005 American Community Survey (ACS).<sup>13</sup> The first thing to notice in this graph is that year of immigration seems to be an important factor on whether the immigrant is a naturalized citizen - the longer the time the immigrant has spent in the US, the greater his probability of being a citizen. This can be seen both from the negative correlation between these two variables and from the fact that, given the year of arrival, an immigrant has a greater probability of being a citizen the later he is observed (i.e., the data points for the 1990 CENSUS lie below those for the 2000 CENSUS which in turn lie below those of the 2005 ACS).

The graph also presents lines fitted to the data by survey year, separately for immigrants who arrived before and after 1982.<sup>14</sup> There seems to be a discontinuity on the probability of being a citizen on 1982: controlling for year of arrival, the fraction of immigrants who are citizens is higher among those who arrived shortly before 1982 than among those who arrived on or right after 1982. Moreover, the slope of the relationship between year of arrival and citizenship status is smaller before 1982; possibly reflecting the fact that the policy

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<sup>13</sup>As explained in the previous section, for the 1990 CENSUS the year of arrival variable is grouped in groups of 2, 3, 4, or 5 years. This explains why there are fewer data points from this survey.

<sup>14</sup>The fitted lines are useful because they give a graphical representation of the estimates presented below using the regression discontinuity approach. But they can bias the perception of the graphs. For transparency, appendix figures 1 to 4 present the same graphs without the fitted lines.

offered the same opportunity of legalization to all who arrived before 1982, regardless of their specific year of arrival. Finally, note that for the three surveys, the discontinuity both in level and slope seems to be in 1982, offering suggestive evidence that the pattern is due to the legalization policy and not to non-linearities in the relation between years in the US and the probability of being a naturalized citizen.<sup>15</sup>

Figures 2, 3 and 4 present similar graphs for the immigrant outcomes that might be affected by the change in legal status. Figure 2 presents the evolution of log wages by year of immigration. As expected, the longer the time spent in the US, the higher the immigrant wage. This can be explained by traditional human capital theories, where the amount of US experience is important for labor market outcomes. The evidence of a discontinuity here is less clear than in the previous graph, but it does seem that immigrants who arrived before 1982 have slightly higher wages than those who arrived afterwards.

Figure 3 presents the graph for the fraction of immigrants employed in traditionally illegal occupations (as explained above, traditionally illegal occupations are defined to be the ten occupations that employ the most illegal immigrants according to the LPS). The right axis presents the scale for the 1990 CENSUS data, the left axis for the 2000 CENSUS and the 2005 ACS. The fraction of immigrants working on these occupations is much higher among newly arrived immigrants, and, as in the wage graph, the discontinuity is less clear.

Finally, figure 4 shows the trends for the geographical mobility variable. In the 2000 CENSUS this variable is defined as the probability that the immigrant does not live in the same state he lived 5 years ago (left axis); in the ACS the reference period is one year ago (right axis). Since for immigrants who first entered the US 5 years (CENSUS) or 1 year (ACS) before the survey this variable will be trivially equal to one, I drop them.<sup>16</sup> Figure 4 shows that there seems to be a discontinuity around 1982 in the 2000 CENSUS data, with pre-82 immigrants being less likely to have moved than post-82 immigrants. As explained above, legalization under the IRCA triggered the migration of many dependents of newly

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<sup>15</sup>In other words, since a given year of arrival means different number of years in the US depending on the survey year, any non-linearity in the relationship between years in the US and probability of naturalization would mean that the location of the break would vary with survey year. That is not what we observe in figure 1.

<sup>16</sup>This will leave only one data point left for the period after 1982 for the 1990 CENSUS, so this data is not shown in figure 4 nor will be used in the analysis.

legalized workers, which could explain the decreased mobility of this population.

The graphs presented suggest that IRCA’s legalization policy caused some discontinuities in the relationship between year of immigration and immigrant’s outcomes. However, in order to investigate whether these discontinuities are significant and what are the implied effects of legalization on immigrants’ outcomes, we need to move to a regression framework. The following two subsections explain in detail the empirical strategies used, namely the regression discontinuity and the difference-in-differences frameworks.

#### 4.1 Regression Discontinuity Framework

The idea behind the regression discontinuity framework is that, in the absence of the IRCA, legalization should be a continuous function of year of immigration to the US. However, due to the policy, legal status will now also depend on whether the immigrant arrived in the US before or after 1982 and possibly on the interaction between these two variables

$$L_i = \delta_0 + \delta_1 YrImmig_i + \delta_2 Pre82_i + \delta_3 Pre82 \times YrImmig_i + e_i \quad (2)$$

Therefore, the main estimated equation will come from substituting equation (2) on equation (1)

$$Y_{it} = \alpha_0 + \alpha_1 YrImmig_i + \alpha_2 Pre82_i + \alpha_3 Pre82 \times YrImmig_i + W_{it}\theta + Yr_t + \varepsilon_{it} \quad (3)$$

where  $Y_{it}$  is one of the outcomes variables of interest (citizenship, log wage, occupation and mobility) for immigrant  $i$  observed at year  $t$ ,  $YrImmig$  is the year of immigration,  $Pre82$  is a dummy equal to one if the year of immigration is before 1982,  $W_{it}$  is a vector of individual level controls, and  $Yr_t$  are survey year fixed effects. The interaction  $Pre82 \times YrImmig$  allows for the slope of the relationship between year of immigration and the outcome variable to be different for the immigrants who arrived before and after 1982, that is, for the group eligible and non-eligible for legalization under the IRCA. The parameter of interest,  $\alpha_2$ , estimates the discontinuity in 1982, controlling for different linear relations between the

outcome variable and year of immigration before and after 1982, year fixed effects and individual characteristics.

One difference from the basic regression discontinuity approach is that here several surveys are available, so that several discontinuities are used to estimate the effects. Since there is no reason to believe that the relation between year of immigration and the outcome variables is the same across all survey years, results from an alternative specification are also presented where interactions between (i) year of immigration and survey dummies, and (ii) year of immigration, survey dummies and pre-82 dummy are included in (2).

Another difference from the classic regression discontinuity specification is the inclusion of individual controls in equation (2). In a traditional regression discontinuity setting, the inclusion of such controls is not necessary, since the characteristics of the sample below and above the cutoff point are on average the same. In my context, there are two reasons why this might not be the case: legalization might affect the survey-response rate and return migration of immigrants. Both these effects will alter the composition of the sample, and might bias the estimates of the impacts of legalization. The inclusion of individual controls in this case will help minimizing these biases. In section 5.1, I present a discussion of such biases, as well as an inspection of the baseline covariates for their magnitude. Moreover, I will present the main results with and without controls for comparison.

## 4.2 Difference-in-Differences Framework

In the difference-in-differences approach I will use immigrants who have been in the US for 8 to 18 years as a treatment group and those who have been in the US for less than 8 years as a control group.<sup>17</sup> When observed in 1990, immigrants in the treatment group arrived in the US before 1982 and therefore were eligible for the legalization program. In contrast, immigrants observed in 2000 who have been in the US for 8 to 18 years arrived in the US sometime between 1982 and 1992, not being affected by the policy.<sup>18</sup> As usual in

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<sup>17</sup>In the analysis I drop immigrants who have been in the US for more than 18 years, since they will be treated regardless of when they are observed. Results that include those immigrants and add a dummy for more than 18 years in the US deliver the same qualitative conclusions.

<sup>18</sup>In principle several such comparisons could have been made using data from different CPS years. However, note that since the definition of treatment and control groups in terms of number of years in the US would vary depending on the survey year

a difference-in-differences strategy, the identifying assumption is that in the absence of the policy the relationship between the number of years in the US and the outcome variables would have been similar for immigrants observed in 1990 and 2000.

The estimated equation is

$$Y_{it} = \gamma_0 + \gamma_1 YrsUS_i + \gamma_2 D1990_t + \gamma_3 D8-18_i + \gamma_4 D8-18_i \times D1990_t + Z_{it}\Phi + \xi_{it} \quad (4)$$

where  $Y_{it}$  is one of the outcomes variables of interest (citizenship, log wage and occupation<sup>19</sup>) for immigrant  $i$  observed at year  $t$ ,  $YrsUS_i$  is the number of years the immigrant has been in the US,  $D1990_t$  is a dummy equal to one if the survey year is equal to 1990,  $D8-18_i$  is a dummy for immigrants who have been in the US for 8 to 18 years, and  $Z_{it}$  is a vector of individual level controls. Under the identifying assumption, the parameter of interest  $\gamma_4$  estimates the causal impact of legalization on immigrants' outcomes.

## 5 Results

### 5.1 Legalization and Sample Composition

The first step in the analysis is to examine the distribution of observable characteristics in the sample below and above the 1982 cutoff point. In the absence of the policy, there are no reasons why these characteristics would not be balanced across the two subsamples.<sup>20</sup> However, in my context, legalization might actually affect the composition of the sample, so that such imbalances could arise. Two possible reasons why legalization might alter the composition of the pre-82 sample is through its impacts in survey-response rate and return migration of immigrants. This change in the sample composition might, in turn, bias

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used, data from different CPS surveys would not be able to be pooled into a single specification. Given the need to run the regression with only two survey years, I decided to use CENSUS data due to the larger sample sizes.

<sup>19</sup>As explained in the data section, for the mobility analysis with the CENSUS I drop immigrants who arrived in the US sometime during the 5 years before the CENSUS, since all these immigrants would have the mobility dummy equal to 1. For the 1990 CENSUS this means that there is only one year in the control group (because in the 1990 CENSUS the year of immigration variable is only available grouped). Therefore, the data does not allow the analysis of the mobility outcome in a difference-in-differences framework.

<sup>20</sup>Business cycles fluctuations could in principle change the flows of immigrants to the US. In particular, the severe recession that happened from July 1981 to November 1982 could explain observed differences between the pre- and post-82 migrants. I cannot rule out this possibility, but the analysis in this section shows that the observed differences are also consistent with legalization affecting the behavior of immigrants eligible to the policy.

the estimates of the effects of legalization on immigrants' outcomes. In this subsection, I will show some evidence consistent with survey-non response and return migration being affected by legalization and discuss the consequences of these effects to the estimated impact of legalization on immigrants' labor market outcomes.

One explanation of why the illegal population might be undercounted in household surveys like the CENSUS and the CPS is that this population might have higher survey non-response rates. Undocumented immigrants spend a lot of effort to remain "in the shadows" and it is plausible that when prompted to answer voluntary surveys they will be more likely to refuse than other groups in the population. If legalization affects not only immigrants' outcomes but also their willingness to participate on the CENSUS or the CPS, this could pose threats to the validity of our estimate of the causal effects of legalization.

The differential non-response would bias our estimates if the group of immigrants who decide to participate in household surveys after legalization is a selected group compared to the group who usually participates regardless of legal status. For example, if after legalization, the less educated newly legalized immigrants decide to answer the surveys, this would cause the observed pre-82 immigrants to have on average lower wages than the observed post-82 immigrants, leading us to underestimate the effect of legalization on wages. Note that the inclusion of individual level controls in equations (2), such as education, would help in minimizing this bias (obviously, the inclusion of controls would not help in terms of bias caused by differences in unobservables).

Another possible behavior response that could cause a change in the composition of pre-82 immigrants and bias the results is return migration. Legalization could have an effect on immigrants' decision on whether to stay in the country or not. This bias would be potentially more severe the longer the time since legalization. In other words, the effect estimated using later surveys could be affected by changes in the sample due to return migration. The bias caused by such changes would depend on the specific characteristics of the subsample for whom legalization would change the decision about return migration.

The empirical literature on return migration provides some evidence on the size, mo-

tivation and personal characteristics of immigrants who leave the U.S. Estimates of the probability of outmigration to Mexico calculated by Durant et.al (1999) peaked in 1980, and then fell and remained low, ranging between 10 and 11 percent throughout the 1990's. Lindstrom (1996) investigates the relation between economic opportunities in Mexico and the likelihood of immigrant return migration from the U.S. He finds evidence that the economic characteristics of the origin area in Mexico influences the duration of immigrants' trips to the U.S. For the purposes of this paper, more important than the size and the motivation behind return migration, is to know if the group of immigrants who decide to leave is a selected one, that is, if they are different than the ones who stay. Borjas and Bratsberg (1996) investigate the characteristics of those who out-migrate from the U.S. They find that return migration intensifies the type of selection that generated the immigrant flow in the first place. In other words, if immigrants have above average skills, the return migrants will be the least skilled immigrants. In contrast, if the immigrant flow is negatively selected, the return migrants will be the most skilled immigrants.

Hanson (2006) provides evidence that illegal immigrants from Mexico are drawn disproportionately from the middle of the country's schooling distribution. So, according to Borjas and Bratsberg (1996) this would mean that normally illegal return migrants would be the least skilled among the illegal immigrants. So, if some of these return migrants change their decision due to legalization, the group of pre-82 immigrants should be on average less skilled than the group of post-82 immigrants. This selection effect would have a downward bias in the estimates of legalization.

One way to assess the effect of the program on the composition of the sample is to graph the histogram for year of arrival in the US. If after legalization immigrants are less likely to return to their country of origin or more likely to answer survey questions, one would find that there is a higher proportion of immigrants who arrived right before 1982 than who arrived right after in the data. Figure 5 plots the histogram of year of arrival using the 2000 CENSUS data. One can see that other than the significant spikes on years multiple of 10 (probably due to rounding in reported answers) there is no discernible evidence that there

is a higher proportion of immigrants in the pre-82 years.

Another way to investigate the extent of biases due to change in survey non-response behavior and return migration is to test for differences in personal characteristics of pre- and post-82 immigrants. Table 2 presents such tests for the probabilities of being Mexican, having less than high school, completed high school or college, and age using the CPS and the CENSUS/ACS samples. The coefficient on the pre-82 dummy is significant for the three first covariates, with immigrants who arrived before 82 being more likely to be Mexicans, have less than high school and less likely to be high school graduates.<sup>21</sup> The magnitudes of these effects vary between 5-10% of the average fraction of immigrants on these categories.

The results in table 2 points to differences in the composition of the pre- and post-82 samples that are consistent with legalization affecting survey response and/or return migration. IRCA's legalization program had a disproportional effect on low-educated Mexican immigrants, so changes in survey response and return migration would make eligible (pre-82) immigrants be more likely to have these characteristics.

To sum up, differential non-response and return migration are two factors that can explain the discontinuities presented in table 2. It is hard to disentangle and control for these differences, but they suggest the inclusion of the vectors of covariates  $W_{it}$  and  $Z_{it}$  to equations (3) and (4). The results below will be presented with and without the inclusion of controls for sex, marital status, race, age, education and country of origin. In most of the cases, the estimated effect of legalization on immigrant's outcomes is very similar if controls are included or not. This suggests that the composition effect due to changes in return migration and survey response does not play a major role in the relationship between legalization and immigrants' labor market outcomes, at least when it comes to biases due to observables. Biases due to unobservables are still possible and harder to deal with but I will show that once I separately estimate the effects of the program for different groups of immigrants the effects are largest exactly for the groups who were affected the most by the policy.

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<sup>21</sup>Tests based on Seemingly Unrelated Regressions (SUR) for the significance of the pre-82 dummy on all individual characteristics used as controls also reject the null hypothesis that these dummies are jointly equal to zero. For space reasons, here I chose to present only the variables that are most likely to characterize illegal immigrants.

## 5.2 Regression Discontinuity Results

Table 3 presents the main regression discontinuity results. It shows estimates of equation (3) for citizenship status, log wages, probability of being employed in a traditionally illegal occupation, and probability of having moved across states. The first 4 columns present the results for the 1994-2009 CPS, the last 4 for 1990 and 2000 CENSUS combined with 2005 ACS. The parameter of interest is the coefficient on the dummy that indicates whether the immigrant first arrived in the U.S. before 1982.<sup>22</sup>

One can see from panel A that for citizenship status this coefficient is positive and significant, showing that even after controlling for year of arrival, immigrants who arrived before 1982 had a chance of being naturalized citizens that was 3.6 percentage points higher. Given that the average probability of being a naturalized citizen in this sample is of 35 percent, this shows an effect of roughly 10 percent (13 percent for the ACS/CENSUS sample). Moreover, this estimate is robust to the inclusion of individual level controls, both in the CPS and the ACS/CENSUS samples, suggesting that the effect is not being driven by the differences in observables.

As mentioned in section 3.1 there is no reason to believe that the relation between year of immigration and the outcome variables is the same across all survey years. Columns (3) and (7) in panel A show the estimates of equation (3) including interactions between year of immigration and survey dummies, and year of immigration, survey dummies and pre-82 dummy. One can see that the estimate of the impact of legalization on citizenship status is robust to the inclusion of these fixed effects and is similar across both samples.

Panel A also presents results for an alternative specification in columns (4) and (8), where a second order polynomial on year of immigration is included separately for the pre and post 1982 period. In this specification the pre-82 dummy is no longer positive and significant. However, this might be because, except for the CENSUS 2000 and the ACS 2005, where yearly data is available, the other surveys only contain grouped data for the year of arrival

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<sup>22</sup>The immigration year is defined as a deviation from 1982, which is the cutoff year. As a consequence, the coefficient on the pre-82 dummy can be interpreted as the difference in the intercepts of the pre- and post-82 functions.

variable, so that there are not enough disaggregated data to estimate a nonlinear function. Moreover, figures 1-4 seem to suggest that, where yearly data is available, a linear function with different slopes before and after 1982 offers a good fit for the yearly data. Since the linear model with the extra interaction terms seems to offer the best fit for the data and allows the greatest flexibility in terms of differences across surveys, I will focus on this specification when analyzing the results below.

Panel A in table 3 offers evidence that the eligibility criteria under IRCA's legalization program did in fact cause a discontinuity on the relationship between year of immigration and legal status. The next step in the analysis is then to ask whether this change in legal status had any effect on the labor market outcomes of the legalized immigrants.

Panel B presents a similar set of results for log wages. They show that only for the ACS/CENSUS sample we see a small positive effect estimate of the pre-82 coefficient.<sup>23</sup>

The results for the probability of being employed in a traditionally illegal occupation show that legalization did not seem to have an impact on the types of jobs immigrants tend to work on. Panel C shows that once individual characteristics are controlled for, the pre-82 immigrants do not have a lower probability of being employed in such occupations.

Finally, panel D presents the estimates for the geographical mobility variable. As explained above, the results for the CPS and the CENSUS samples are not directly comparable here due to the difference in the questions asked (the CPS reference period is 1 year, while the CENSUS is 5 years) and sample restrictions imposed. For both samples, except for the specification where the second order polynomial is included, no significant difference is found on the migration patterns of the pre- and post-82 immigrants.<sup>24</sup>

To sum up, the results presented in table 3 show that legalization is associated with a significant increase in the probability of naturalized citizenship and a small increase in wages, but no significant effect is found on occupation or mobility variables. One possible

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<sup>23</sup>The effects of the covariates are as expected and consistent across surveys: male, married, older and more educated immigrants earn higher wages. One exception is the coefficient of the black dummy: among the subsample of immigrants, blacks earn higher wages, which is the opposite what one would find for the black coefficient on the overall population. Analyzing data from the 1980 CENSUS, Butcher (1994) presents evidence that immigrant black men have better labor market outcomes than native-born black men and attributes this difference to the selection process associated with migration.

<sup>24</sup>The mobility results in the CENSUS are calculated using only one survey year (2000) so there is no specification with survey year fixed effects (or interactions).

explanation for the lack of an estimated impact on some of the outcomes is that these effects are estimated off all immigrants as opposed to immigrants who are more likely to be affected by the policy. According to the administrative data on IRCA's program, low skilled Mexicans were the largest demographic group to take advantage of legalization. Given this information, it is expected that the estimated effects of the program will differ for immigrants with different personal characteristics. In table 4, I investigate such hypothesis. First, I divide the sample into nine different groups, depending on the immigrants' region of origin (Mexico, other Latin Americans, and non-Latin Americans) and education (less than high school, high school or some college, and college graduates). I then repeat estimation of the functional form in columns (3) and (7) of table 3 for each of these nine groups. For presentation purposes, the results for citizenship, wages and occupation are calculated pooling CPS, CENSUS and ACS data.<sup>25</sup> Due to the differences in the migration questions asked across surveys, the results for mobility are presented separately for CPS and CENSUS data.

Table 3 presents 4 figures for each group of immigrants: the coefficient estimate of the pre-82 dummy, its robust standard error (in parenthesis), the mean of the dependent variable {in curly brackets} and the number of observations in each regression [in brackets]. Panel A gives the results for citizenship: as expected, the effects as a fraction of the group's mean decrease as one moves towards more educated and non-Latin American immigrants. The effect is positive for all groups, largest for Mexican high school dropouts (38% increase in the probability of being a naturalized citizen) and smallest for non-Latin Americans with high school degree or some college (0.2%).

Panels B-E show similar results for wages, occupation and mobility outcomes. Consistent with the conclusions from table 3, for the majority of the groups no significant effects of legalization are found in any of the outcomes. Most surprisingly, except for a decrease in mobility found using the CENSUS sample, there are no significant effects for Mexican high school dropouts, a group who was very affected by the policy.

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<sup>25</sup>Results using the samples separately yield similar results and these results are available upon request.

Another dimension in which there could be heterogeneity on the impacts of legalization is time, as newly legalized immigrants might take a while to reoptimize their labor market decisions under their new legal status. Furthermore, as discussed above, biases due to return migration might also make the estimated effects of legalization vary over time, with the estimates from later surveys being less reliable than the estimates from earlier surveys. Even though it is not possible to distinguish between these two effects, it is important to know if the estimated effects of legalization are different at different points in time, i.e. after different numbers of years since legalization.

Table 5 shows the estimates of an equation where the pre-82 dummy is interacted with a survey year trend and its squared, allowing for heterogeneous effects for different survey years. The results show a positive and significant effect for citizenship which is increasing over time for the CPS sample. On the peak value, the effect corresponds to a 17 percent increase for the CPS sample and a 20 percent increase for the CENSUS sample. The results for wages are positive and significant in both samples. However, they decrease with time, so that the effect is only positive for the first few survey years and the maximum estimated increase in wages is only 4.8 percent (6.3 in the CENSUS sample). This could be attributed to heterogeneous effects of legalization over time but also to changes in return migration which will bias more severely the estimates that use more recent survey years.

The effects on the probability of being employed at a traditionally illegal occupation are negative but only significant for the CENSUS/ACS sample. Finally, estimates of the effect of legalization on geographical mobility are small and insignificant, and do not seem to vary with survey year.

Overall, the time analysis points to a similar conclusion as the previous tables: the effect for citizenship is significant and sizeable for the whole period covered, the one for wages is small and positive and restricted to the first years after legalization, and no significant effect is found for geographical mobility. One difference appears in the occupation analysis, where no effect on occupation is found in the CPS sample, while a negative effect is found in the CENSUS for the first few years after legalization.

### 5.3 Difference-in-Differences Results

Table 6 presents the main results for the difference-in-differences analysis. The coefficient of interest here is the interaction between the dummy for being 8 to 18 years in the US and the 1990 dummy. It gives the difference on outcomes of immigrants who were eligible for legalization under the IRCA (immigrants observed in 1990 who arrived sometime between 1972 and 1982) and those who were not (immigrants observed in 2000 who arrived sometime between 1982 and 1992), relative to immigrants who have been in the US for less than 8 years.

The first two columns show the effects for citizenship. The coefficient estimates are positive and significant, and do not vary with the inclusion of controls. Most importantly, they are remarkably similar to the estimates found using the regression discontinuity framework, implying that legalization increases the probability of naturalized citizenship by approximately 10%.

Columns 3-6 present similar results for the other outcome variables. I do not present the analysis of the mobility variable here, since the restriction of the sample to exclude immigrants who arrived in the 5 years previous to the survey means that for 1990 there is only one immigration year in the control group.

The estimated effect of legalization is positive and significant on wages (5.4%) and negative on the probability of being employed at an illegal occupation (between 2-3%). These effects are robust to the inclusion of controls for sex, marital status, race, age, education and region of origin.

Table 7 performs an analysis of the effects by the immigrants' region of origin and education level, being analogous to table 4. The estimated coefficient on the interaction between the dummy for being 8 to 18 years in the US and the 1990 dummy together with its standard error, the mean of the dependent variable and the sample size are shown for each education/origin group. Panel A presents the results for citizenship status, where Mexican high school dropouts have the largest effect (legalization increases citizenship probability on

17%).<sup>26</sup>

Panel B presents a similar set of results for log wages. The effects are positive and significant for all groups, varying from 3-13%. Finally, panel C shows results for the probability of working on a traditionally illegal occupation. Here, most of the effects are negative, but the magnitudes vary a lot, with incredibly large effects (relative to their very low means) observed for college graduates. Note again that, not surprisingly, the largest effects in panels B and C are observed for Mexican immigrants.

Overall, the results here paint a very similar picture as the ones in the previous section. Legalization has a strong effect on citizenship status and a positive and significant effect on wages. Results point to a decrease in the probability of working in illegal occupations, but the magnitude of the effect varies a lot depending on the demographic group one focus on.

## 6 Conclusions

This paper investigates the impact of legalization on economic outcomes of the legalized population using a natural experiment generated by the 1986 IRCA legalization program. The IRCA program was the only major legalization program ever implemented in the U.S. and the analysis of its consequences is of special interest now, as the country is in the process of discussing a new immigration policy reform.

The results show that immigrants eligible for IRCA's legalization program have a 10% higher probability of being naturalized citizens than those who were not. Legalization is also estimated to have a positive and significant effect on wages, a negative effect on the probability of working on a traditionally illegal occupation, and no significant effect on geographical mobility. Finally, the analysis for different demographic groups shows that the estimated effects of legalization are largest for low-educated Latin American immigrants, the group that was disproportionately affected by the policy.

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<sup>26</sup>This is much smaller than the almost 40% effect found for Mexican high school dropouts in table 4. Note, however, that here the treatment was observed in 1990, just a few years after legalization and probably before most immigrants were able to get naturalized citizenship. This could also account for the fact that we do not see a significant effect for other low-educated immigrants.

The estimated effects from the regression discontinuity and the difference-in-differences models are similar in signs, but the magnitudes implied by the difference-in-difference estimates are larger. Moreover, the negative effect of legalization on the probability of working on an illegal occupation estimated in the regression discontinuity framework is only significant for the first few years after legalization. The two estimates are based in different identifying assumptions and the question of which estimate is more credible depends on the relative credibility of these assumptions.

The difference-in-differences model assumes that, in the absence of the policy, the relation between the number of years in the US and the outcome variables would be similar for immigrants observed in 1990 and 2000. So, any changes in the returns to US experience between these two decades would result in biases to this estimate. More specifically, higher returns to US experience in terms of wages and occupational mobility in 1990 than in 2000 could generate the presented results. The regression discontinuity results, in turn, are based on the assumption that the post-82 immigrants are a good control group for the pre-82 immigrants. The effect of legalization on return migration and survey response are two reasons why this assumption might be violated. This paper presented some evidence that there are differences in the composition of the pre- and post-82 immigrant groups consistent with such effects, even though controlling for the observed composition differences does not affect the estimated effects of legalization on economic outcomes. Moreover, the analysis by education/region of origin groups – for which composition effects should in principle be less serious – points to the same conclusions as the overall analysis.

The effect of legalization on the composition of the sample can also be interpreted as an effect of legalization that is interesting on itself. The question of whether legalization affects return migration and what are the characteristics of the affected population can help improving the understanding of the motivations behind international migration flows. Further research should investigate this question, possibly combining US and Mexican migration data for a more complete picture of the large movements between these two countries.

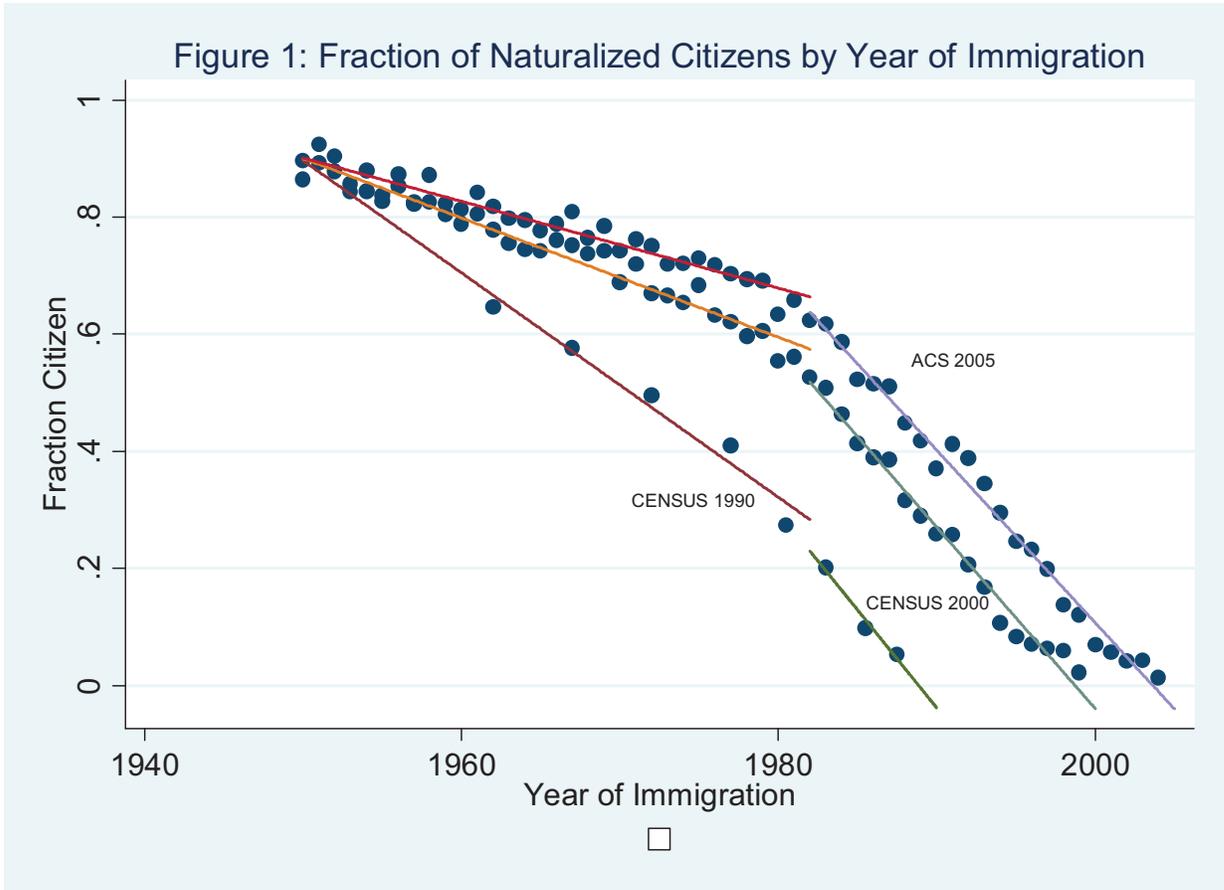
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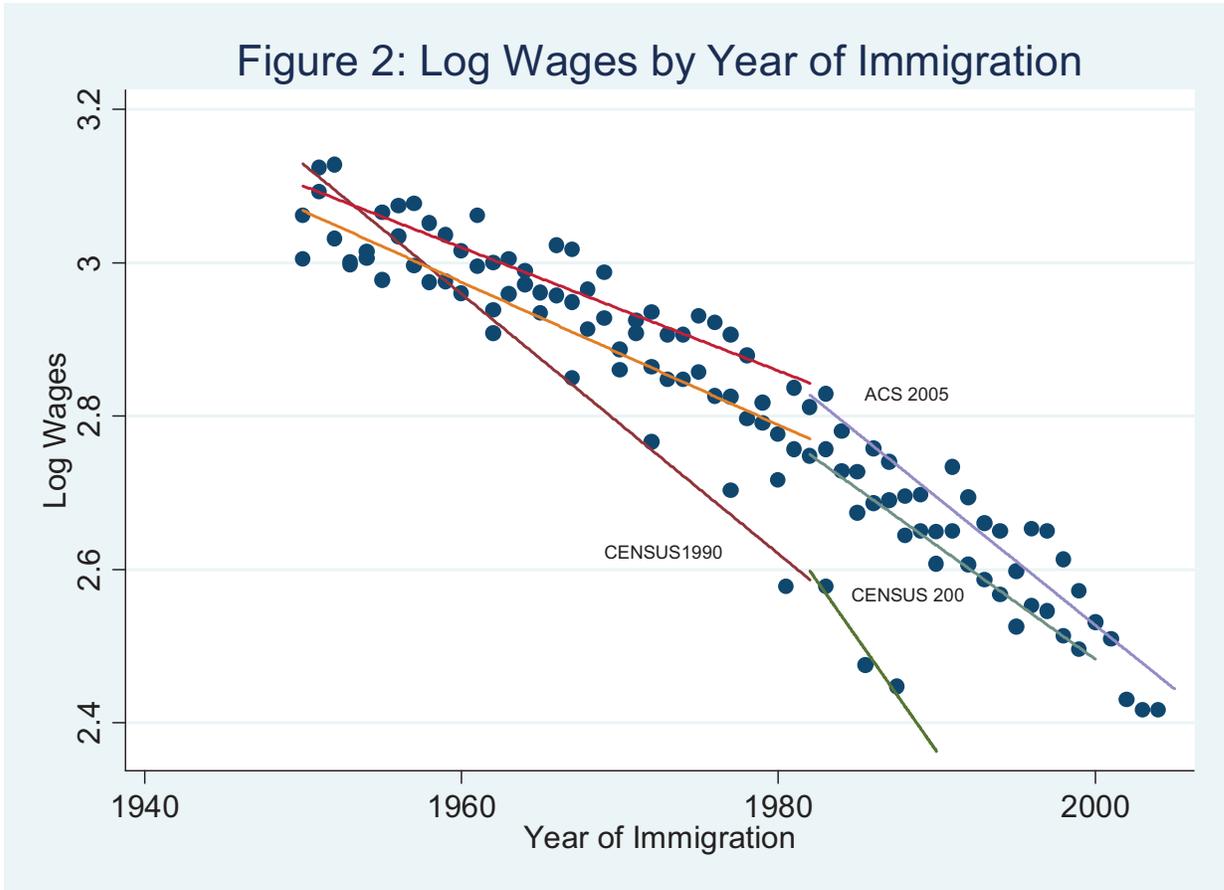
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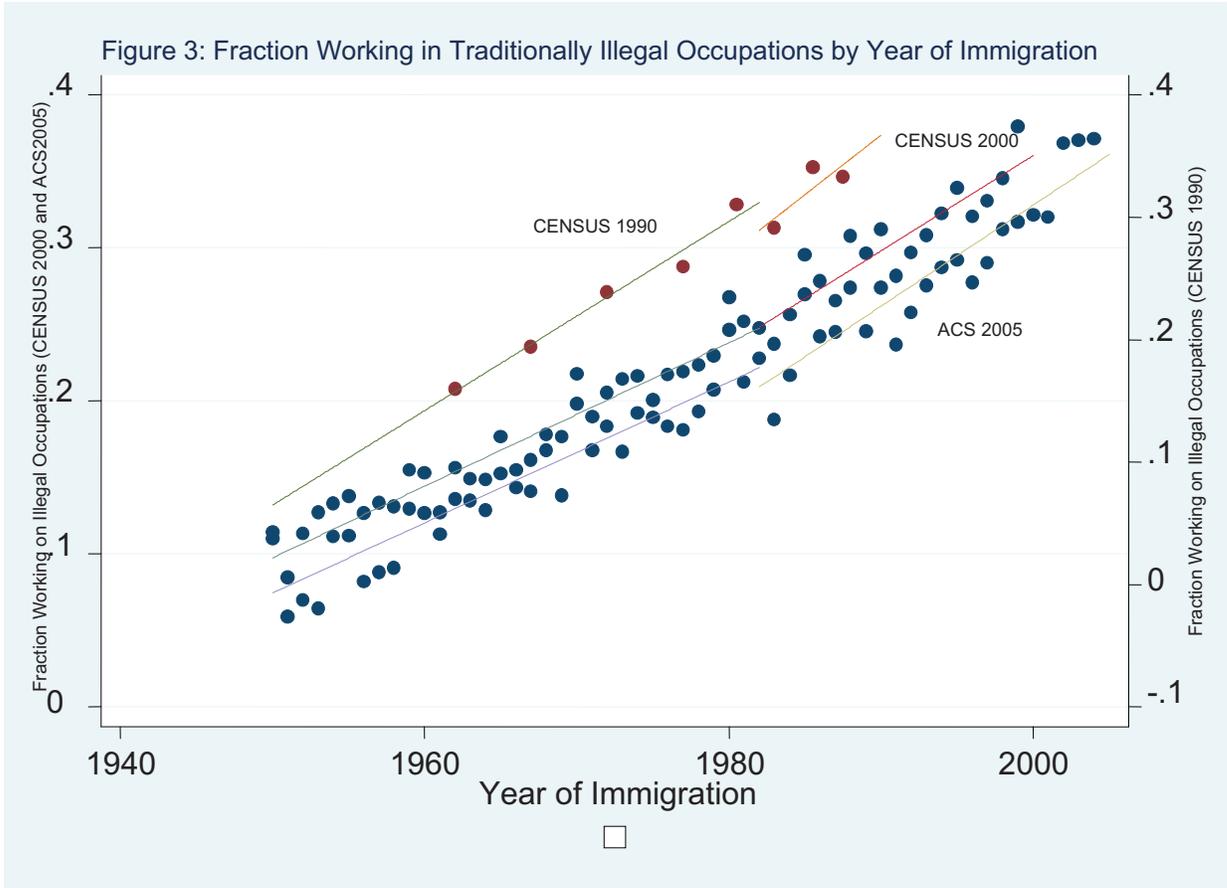
Urban Institute Press.



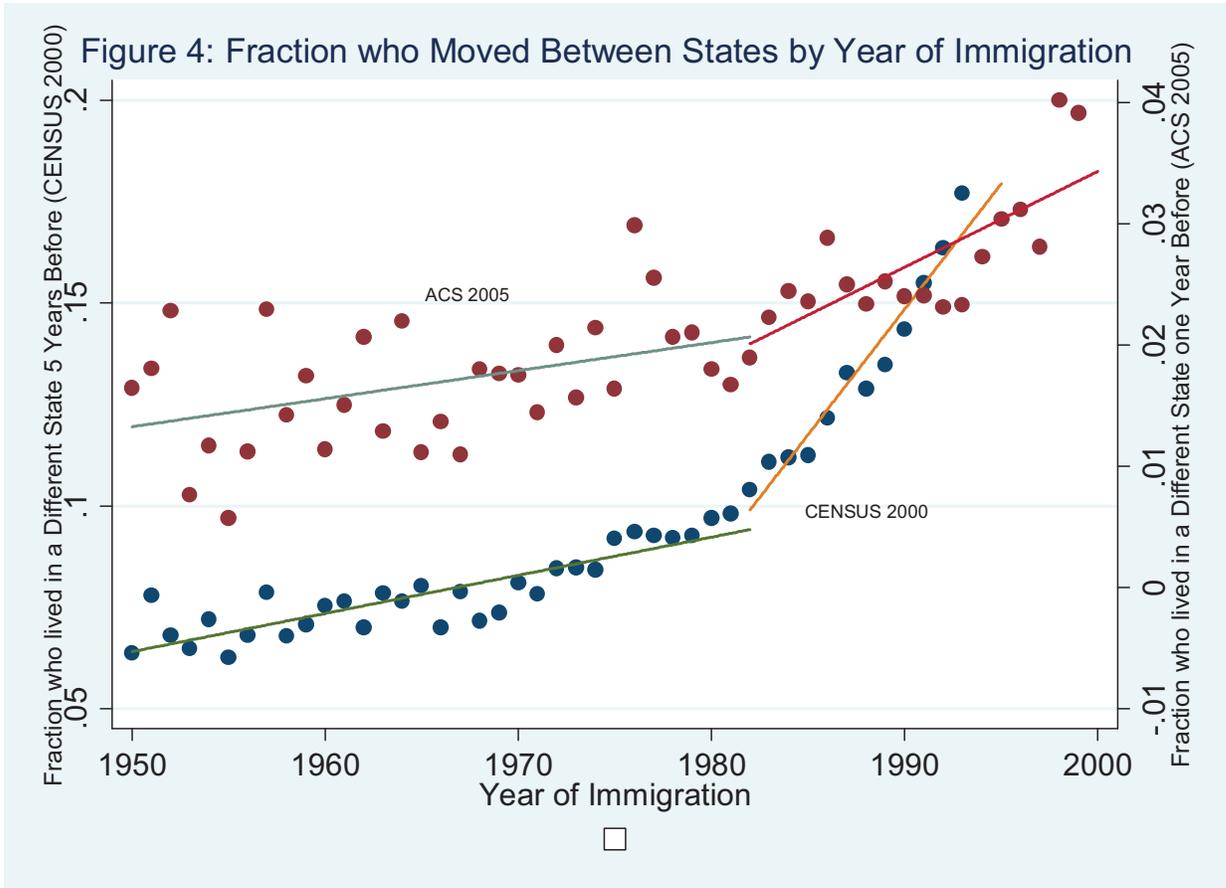
Notes: Weighted averages by year of immigration and survey year. The 1990 CENSUS has fewer data points due to the grouping of the year of immigration variable.



Notes: Weighted averages by year of immigration and survey year. The 1990 CENSUS has fewer data points due to the grouping of the year of immigration variable. Individual wages were inflated to 2009 dollar values using the CPI.

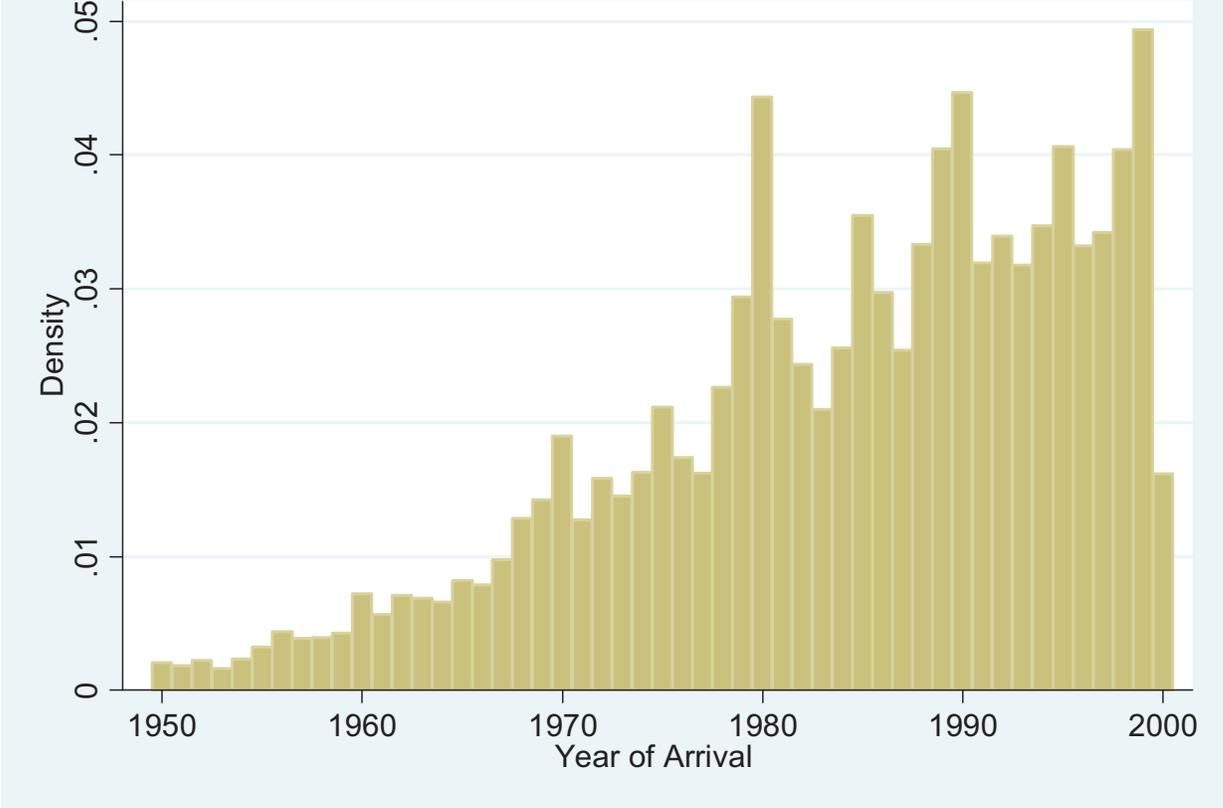


Notes: Weighted averages by year of immigration and survey year. The 1990 CENSUS has fewer data points due to the grouping of the year of immigration variable. For details on how a traditionally illegal occupation is defined see explanations section 3.2.



Notes: Weighted averages by year of immigration and survey year. In the 2000 CENSUS the mobility variable is defined as the probability that the immigrant does not live in the same state he lived 5 years ago (left axis); in the ACS the reference period is one year ago (right axis). Immigrants who arrived after 1994 (in the 2000 CENSUS) and after 2003 (in the 2005 ACS) are dropped from the analysis.

Figure 5: Histogram of Year of Arrival, CENSUS 2000



Notes: Distribution of year of arrival in the US for immigrants 16 to 64 years old who were not enrolled in school at the time of the interview.

TABLE 1: SUMMARY STATISTICS CPS, ACS/CENSUS

	CPS			ACS/CENSUS		
	All Immigrants	Non-Lat. Am., College Grads.	Mexicans, HS Dropouts	All Immigrants	Non-Lat. Am., College Grads.	Mexicans, HS Dropouts
Citizen	0.35 (0.48)	0.49 (0.50)	0.14 (0.35)	0.39 (0.49)	0.53 (0.50)	0.18 (0.38)
LogWage	2.65 (0.66)	3.05 (0.66)	2.29 (0.51)	2.70 (0.68)	3.09 (0.67)	2.35 (0.56)
Illegal Occupation	0.24 (0.42)	0.05 (0.22)	0.47 (0.50)	0.26 (0.44)	0.06 (0.24)	0.52 (0.50)
Move between states	0.03 (0.18)	0.04 (0.19)	0.03 (0.18)	0.11 (0.32)	0.15 (0.36)	0.10 (0.30)
Pre-82	0.31 (0.46)	0.33 (0.47)	0.28 (0.45)	0.38 (0.49)	0.42 (0.49)	0.33 (0.47)
Immigration Yr	4.49 (10.96)	4.31 (11.22)	5.63 (10.39)	2.73 (11.63)	1.81 (12.09)	4.68 (10.63)
Female	0.49 (0.50)	0.50 (0.50)	0.44 (0.50)	0.49 (0.50)	0.49 (0.50)	0.43 (0.49)
Married	0.66 (0.47)	0.72 (0.45)	0.67 (0.47)	0.67 (0.47)	0.74 (0.44)	0.67 (0.47)
Black	0.09 (0.28)	0.06 (0.24)	0.01 (0.09)	0.07 (0.26)	0.04 (0.20)	0.00 (0.06)
Age	38.94 (11.35)	40.33 (10.64)	36.53 (11.29)	39.19 (11.60)	41.29 (10.65)	35.96 (11.41)
High School Dropout	0.31 (0.46)			0.33 (0.47)		
High School Grad	0.28 (0.45)			0.27 (0.45)		
Some College and above	0.42 (0.49)			0.40 (0.49)		
North America	0.02 (0.13)			0.02 (0.15)		
Mexico	0.32 (0.47)			0.32 (0.47)		
Central America	0.17 (0.38)			0.17 (0.37)		
South America	0.06 (0.25)			0.06 (0.25)		
Europe	0.11 (0.31)			0.13 (0.34)		
Asia	0.26 (0.44)			0.26 (0.44)		
Africa	0.03 (0.16)			0.03 (0.16)		
Oceania	0.00 (0.07)			0.01 (0.07)		
Other	0.03 (0.16)			0.01 (0.09)		
Observations	231,108	58,847	47,863	1,716,173	475,509	352,863

Notes: weighted averages for the sample of immigrants ages 16-64 not enrolled in school, standard deviations in parentheses. The CPS averages are calculated over the 1994-2009 period, except for the geographical mobility variables, which exclude 1995. The ACS/CENSUS include the 1990 and 2000 CENSUSES and the 2005 ACS, except for the geographical mobility variable which is calculated only over the 2000 CENSUS. Wages are inflated to 2009 levels using the CPI. Geographical mobility corresponds to 1-year migration variables for the CPS sample and 5-year migration variables for the ACS/CENSUS sample. Immigration year is presented as a deviation from 1982.

TABLE 2: COVARIATE ANALYSIS

	CPS					ACS/CENSUS				
	mexico	lessHS	HS	college	age	mexico	lessHS	HS	college	age
Immigration Yr	0.002 (0.000)	0.002 (0.000)	-0.001 (0.000)	-0.001 (0.000)	-0.463 (0.005)	0.005 (0.000)	0.003 (0.000)	0.000 (0.000)	-0.003 (0.000)	-0.508 (0.003)
Pre-82	0.018 (0.004)	0.025 (0.004)	-0.029 (0.004)	0.004 (0.005)	0.113 (0.095)	0.033 (0.002)	0.028 (0.002)	-0.032 (0.002)	0.004 (0.002)	0.262 (0.044)
Pre-82xImmigration Yr	0.003 (0.000)	0.005 (0.000)	-0.001 (0.000)	-0.005 (0.000)	-0.134 (0.008)	0.002 (0.000)	0.005 (0.000)	-0.002 (0.000)	-0.003 (0.000)	-0.057 (0.004)
Observations	231,108	231,108	231,108	231,108	231,108	1,715,863	1,715,863	1,715,863	1,715,863	1,715,863
R-squared	0.005	0.008	0.001	0.006	0.234	0.016	0.014	0.001	0.012	0.27

Notes: All regressions include survey year fixed effects and are weighted using survey weights. Immigration year is calculated as a deviation from 1982. Robust standard errors in parentheses.

TABLE 3: EFFECTS OF LEGALIZATION, REGRESSION DISCONTINUITY ESTIMATES (CPS, ACS AND CENSUS)

	CPS				ACS/CENSUS			
<b>Panel A: Citizenship</b>								
Immigration Yr	-0.026 (0.000)	-0.025 (0.000)	-0.020 (0.001)	-0.027 (0.005)	-0.028 (0.000)	-0.026 (0.000)	-0.036 (0.000)	-0.051 (0.003)
Immigration Yr Sqrd				0.000 (0.000)				0.003 (0.000)
Pre-82	0.036 (0.004)	0.032 (0.004)	0.037 (0.004)	-0.014 (0.007)	0.053 (0.002)	0.051 (0.002)	0.035 (0.002)	0.003 (0.003)
Pre-82xImmigration Yr	0.012 (0.000)	0.012 (0.000)	0.000 (0.002)	0.001 (0.008)	0.016 (0.000)	0.015 (0.000)	0.016 (0.001)	0.019 (0.003)
Pre-82xImmigration Yr Sqrd				-0.001 (0.000)				-0.004 (0.000)
Controls?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Interaction FE?	No	No	Yes	Yes	No	No	Yes	Yes
Observations	231,108	231,108	231,108	231,108	1,715,863	1,715,863	1,715,863	1,715,863
R-squared	0.252	0.326	0.329	0.33	0.257	0.322	0.327	0.327
<b>Panel B: Log Wage</b>								
Immigration Yr	-0.012 (0.000)	-0.009 (0.000)	-0.018 (0.003)	-0.020 (0.010)	-0.015 (0.000)	-0.011 (0.000)	-0.022 (0.001)	-0.015 (0.004)
Immigration Yr Sqrd				0.000 (0.001)				-0.001 (0.001)
Pre-82	-0.001 (0.007)	0.006 (0.006)	0.002 (0.006)	-0.024 (0.010)	0.005 (0.003)	0.014 (0.003)	0.006 (0.003)	-0.012 (0.005)
Pre-82xImmigration Yr	-0.001 (0.001)	0.000 (0.001)	0.007 (0.004)	0.002 (0.013)	0.003 (0.000)	0.002 (0.000)	0.010 (0.001)	-0.008 (0.005)
Pre-82xImmigration Yr Sqrd				0.000 (0.001)				0.000 (0.001)
Controls?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Interaction FE?	No	No	Yes	Yes	No	No	Yes	Yes
Observations	173,089	173,089	173,089	173,089	1,267,601	1,267,601	1,267,601	1,267,601
R-squared	0.042	0.277	0.278	0.278	0.048	0.247	0.248	0.249
<b>Panel C: Illegal Occupation</b>								
Immigration Yr	0.005 (0.000)	0.005 (0.000)	0.006 (0.002)	0.002 (0.008)	0.006 (0.000)	0.005 (0.000)	0.005 (0.001)	0.012 (0.003)
Immigration Yr Sqrd				0.000 (0.001)				-0.001 (0.000)
Pre-82	0.014 (0.004)	0.006 (0.004)	0.006 (0.004)	0.001 (0.006)	0.011 (0.002)	0.002 (0.002)	0.003 (0.002)	0.008 (0.004)
Pre-82xImmigration Yr	0.000 (0.000)	-0.001 (0.000)	-0.001 (0.003)	0.002 (0.010)	0.000 (0.000)	-0.001 (0.000)	0.000 (0.001)	-0.005 (0.003)
Pre-82xImmigration Yr Sqrd				0.000 (0.001)				0.001 (0.000)
Controls?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Interaction FE?	No	No	Yes	Yes	No	No	Yes	Yes
Observations	169,582	169,582	169,582	169,582	1,226,215	1,226,215	1,226,215	1,226,215
R-squared	0.015	0.154	0.155	0.155	0.019	0.174	0.174	0.174
<b>Panel D: Mobility</b>								
Immigration Yr	0.002 (0.000)	0.001 (0.000)	0.002 (0.001)	0.004 (0.005)	0.006 (0.000)	0.005 (0.000)	-	0.001 (0.001)
Immigration Yr Sqrd				0.000 (0.001)			-	0.000 (0.000)
Pre-82	-0.001 (0.002)	-0.001 (0.002)	0.003 (0.002)	-0.006 (0.003)	-0.001 (0.001)	0.001 (0.001)	-	-0.006 (0.002)
Pre-82xImmigration Yr	-0.001 (0.000)	-0.001 (0.000)	-0.001 (0.001)	-0.003 (0.006)	-0.005 (0.000)	-0.005 (0.000)	-	-0.002 (0.001)
Pre-82xImmigration Yr Sqrd				0.000 (0.001)				0.000 (0.000)
Controls?	No	Yes	Yes	Yes	No	Yes		Yes
Interaction FE?	No	No	Yes	Yes	No	No		Yes
Observations	200,858	200,858	200,858	200,858	753,490	753,490		753,490
R-squared	0.008	0.013	0.014	0.015	0.009	0.025		0.025

Notes: All regressions are weighted using survey weights and include year fixed effects. Immigration year is calculated as a deviation from 1982. Controls include sex, marital status, race, age, age squared, region of origin and education. Interaction FE are immigration yrXsurvey yr and immigration yrXsurvey yrXPre-82 fixed effects. Robust standard errors in parentheses.

TABLE 4: EFFECTS OF LEGALIZATION BY REGION OF ORIGIN AND EDUCATION, R.D. ESTIMATES

Level of Education	Region of Origin					
	Mexican		Other Latin American		Non-Latin American	
<b>Panel A: Citizenship (CPS, CENSUS, ACS)</b>						
Less than High School	0.055 (0.007)	{0.145} [400593]	0.057 (0.013)	{0.193} [139217]	0.053 (0.017)	{0.367} [131747]
High School or Some College	0.081 (0.011)	{0.245} [196256]	0.072 (0.010)	{0.401} [234174]	0.001 (0.009)	{0.485} [418230]
College Graduates	0.048 (0.032)	{0.332} [20099]	0.017 (0.017)	{0.518} [63579]	0.020 (0.009)	{0.477} [343076]
<b>Panel B: Log Wage (CPS, CENSUS, ACS)</b>						
Less than High School	0.015 (0.011)	{2.297} [275799]	-0.018 (0.018)	{2.309} [96687]	-0.001 (0.026)	{2.417} [80664]
High School or Some College	0.002 (0.014)	{2.458} [149523]	-0.026 (0.013)	{2.578} [183571]	0.015 (0.013)	{2.68} [311850]
College Graduates	0.034 (0.044)	{2.81} [15765]	0.033 (0.025)	{2.99} [52648]	-0.014 (0.014)	{3.167} [274183]
<b>Panel C: Illegal Occupation (CPS, CENSUS, ACS)</b>						
Less than High School	0.017 (0.011)	{0.478} [272331]	-0.005 (0.017)	{0.425} [93515]	0.034 (0.023)	{0.359} [79163]
High School or Some College	-0.019 (0.013)	{0.334} [141748]	0.013 (0.010)	{0.212} [171031]	0.016 (0.008)	{0.171} [297143]
College Graduates	-0.054 (0.028)	{0.176} [15193]	0.000 (0.009)	{0.079} [51327]	-0.002 (0.004)	{0.03} [274346]
<b>Panel D: Mobility (CPS)</b>						
Less than High School	0.006 (0.004)	{0.032} [41283]	0.003 (0.005)	{0.027} [15566]	-0.005 (0.009)	{0.031} [8483]
High School or Some College	0.001 (0.005)	{0.028} [25409]	0.003 (0.005)	{0.029} [27669]	-0.002 (0.004)	{0.032} [35455]
College Graduates	-0.012 (0.014)	{0.031} [3118]	-0.002 (0.010)	{0.034} [8857]	0.008 (0.005)	{0.041} [35018]
<b>Panel E: Mobility (CENSUS)</b>						
Less than High School	-0.013 (0.003)	{0.095} [153523]	0.003 (0.005)	{0.094} [51448]	0.003 (0.006)	{0.084} [46557]
High School or Some College	-0.003 (0.004)	{0.092} [79013]	-0.004 (0.004)	{0.098} [95233]	0.001 (0.003)	{0.108} [166359]
College Graduates	0.018 (0.015)	{0.11} [7464]	0.012 (0.009)	{0.138} [24343]	0.010 (0.004)	{0.162} [129550]

Notes: Reported figures: coefficient on the pre-82 dummy, its robust standard error (in parenthesis), the mean of the dependent variable {in curly brackets} and the number of observations in each regression [in brackets]. All regressions are weighted using survey weights and contain controls for sex, marital status, race, age and age squared. Survey year FE, survey year by year of immigration, and survey year by year of immigration by pre-82 interactions are included. Immigration year is calculated as a deviation from 1982. Robust standard errors in parentheses.

TABLE 5: HETEROGENEOUS EFFECT BY SURVEY YEAR, R.D. ESTIMATES (CPS, ACS/CENSUS)

	CPS				ACS/CENSUS		
	Citizen	LogWage	Occupation	Move	Citizen	LogWage	Occupation
Immigration Yr	-0.024 (0.001)	-0.011 (0.001)	0.005 (0.001)	0.004 (0.000)	-0.030 (0.000)	-0.009 (0.001)	0.003 (0.001)
Pre-82	0.006 (0.009)	0.048 (0.014)	-0.011 (0.010)	-0.002 (0.005)	0.076 (0.002)	0.063 (0.004)	-0.010 (0.003)
Survey Year	0.033 (0.001)	0.038 (0.002)	-0.012 (0.001)	0.002 (0.001)	0.033 (0.000)	0.023 (0.001)	-0.003 (0.000)
Survey Year Sqrd	0.000 (0.000)	-0.001 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-0.001 (0.000)	0.000 (0.000)
Pre-82xImmigration Yr	0.005 (0.001)	-0.001 (0.001)	0.001 (0.001)	-0.004 (0.001)	0.012 (0.000)	-0.002 (0.001)	0.002 (0.001)
Pre-82 x Survey Year	0.015 (0.002)	-0.016 (0.003)	0.005 (0.002)	0.000 (0.001)	-0.001 (0.001)	-0.015 (0.001)	0.001 (0.001)
Pre-82 x Survey Year Sqrd	-0.001 (0.000)	0.001 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.001 (0.000)	0.000 (0.000)
Survey Year x Immigration Yr	0.000 (0.000)						
Pre-82 x Survey Year x Immigration Yr	0.001 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.001 (0.000)	0.001 (0.000)	0.000 (0.000)
Observations	231,108	173,089	169,582	200,858	1,715,863	1,267,601	1,226,215
R-squared	0.329	0.277	0.154	0.013	0.327	0.248	0.174

Notes: All regressions are weighted using survey weights and contain controls for sex, marital status, race, age, age squared, education and region of origin. Geographical mobility results for CENSUS only use 2000 CENSUS data, so it is not displayed here. Immigration year is calculated as a deviation from 1982. Robust standard errors in parentheses.

TABLE 6: EFFECTS OF LEGALIZATION, DIFFERENCE-IN-DIFFERENCES ESTIMATES (CENSUS)

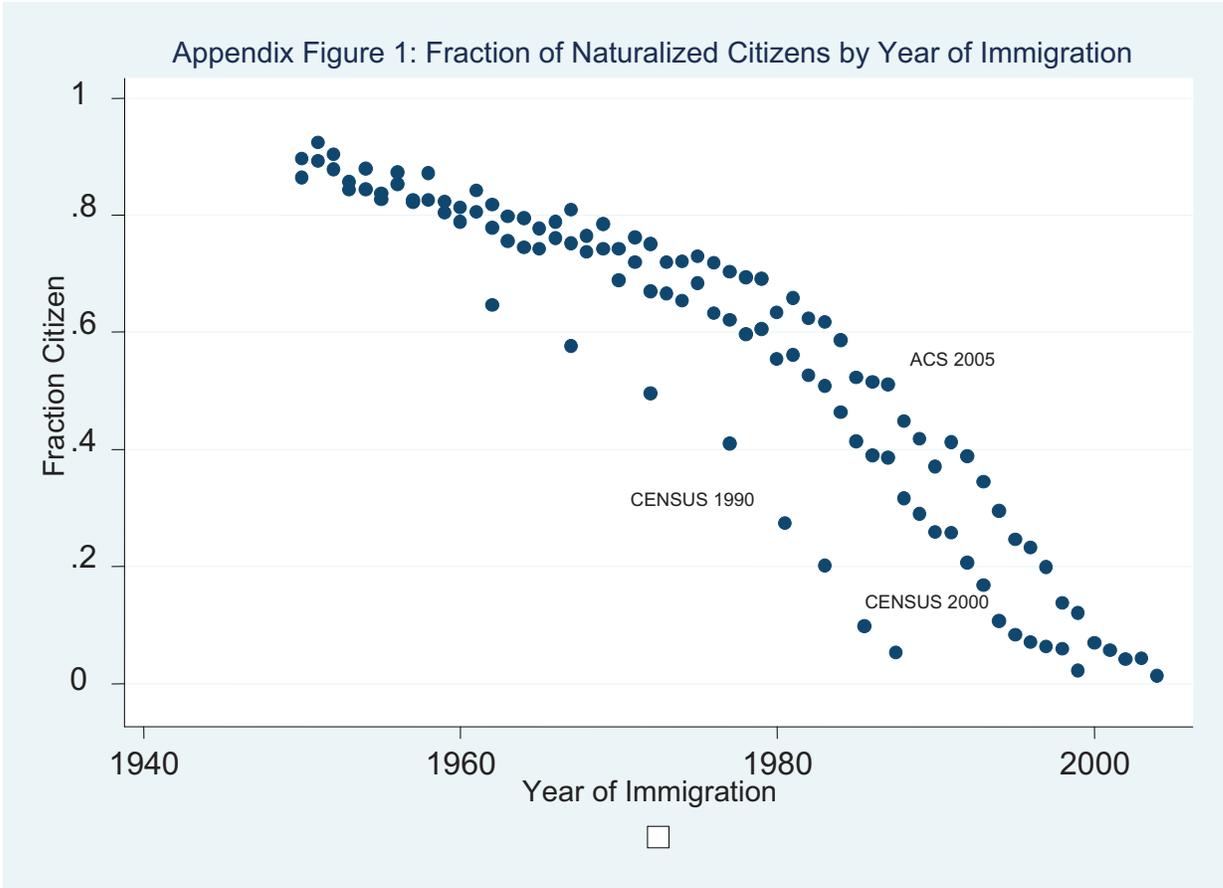
	Citizen		Log Wage		Illegal Occupation	
Years in the US	0.029 (0.000)	0.028 (0.000)	0.018 (0.000)	0.013 (0.000)	-0.009 (0.000)	-0.005 (0.000)
8-18 Yrs in the US	0.013 (0.001)	0.003 (0.001)	-0.052 (0.003)	-0.063 (0.002)	-0.010 (0.002)	0.000 (0.002)
Year = 1990	0.012 (0.002)	0.021 (0.002)	-0.031 (0.004)	-0.015 (0.004)	0.025 (0.003)	0.007 (0.003)
8-18 Yrs in the US x 1990	0.022 (0.002)	0.024 (0.002)	0.056 (0.004)	0.054 (0.003)	-0.005 (0.003)	-0.008 (0.002)
Controls?	no	yes	no	yes	no	yes
Mean of Y	0.216	0.216			0.307	0.307
Observations	919,406	919,406	660,702	660,702	645,230	645,230
R-squared	0.122	0.166	0.015	0.223	0.004	0.169

Notes: All regressions are weighted using survey weights. Controls include sex, marital status, race age, age squared, region of origin and education. Robust standard errors in parentheses.

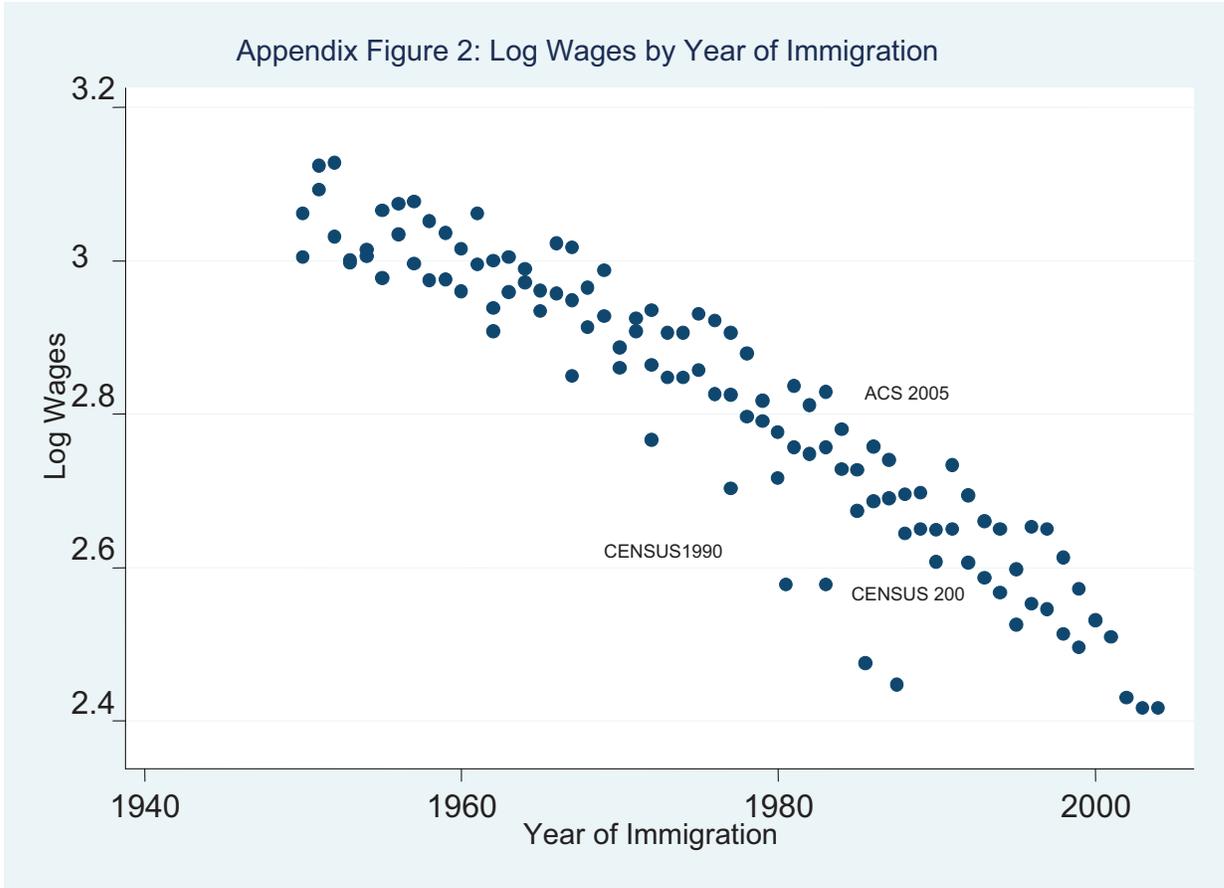
TABLE 7: EFFECT OF LEGALIZATION BY REGION OF ORIGIN AND EDUCATION, DIFF-DIFF ESTIMATES

Level of Education	Region of Origin					
	Mexican		Other Latin American		Non-Latin American	
<b>Panel A: Citizenship</b>						
Less than High School	0.019 (0.003)	{0.11} [214282]	0.003 (0.006)	{0.127} [76894]	-0.030 (0.007)	{0.228} [67121]
High School or Some College	0.004 (0.006)	{0.146} [91941]	-0.028 (0.006)	{0.236} [101858]	0.027 (0.004)	{0.32} [183854]
College Graduates	-0.034 (0.023)	{0.169} [8586]	-0.047 (0.013)	{0.262} [21980]	0.047 (0.005)	{0.293} [152890]
<b>Panel B: Wages</b>						
Less than High School	0.046 (0.007)	{2.287} [143696]	0.027 (0.011)	{2.355} [53452]	0.066 (0.013)	{2.428} [39462]
High School or Some College	0.074 (0.011)	{2.401} [66889]	0.042 (0.009)	{2.546} [77983]	0.030 (0.007)	{2.654} [133915]
College Graduates	0.137 (0.044)	{2.678} [6355]	0.056 (0.022)	{2.888} [17587]	0.074 (0.008)	{3.114} [121363]
<b>Panel C: Illegal Occupation</b>						
Less than High School	-0.017 (0.006)	{0.554} [145866]	-0.004 (0.010)	{0.47} [52347]	-0.019 (0.011)	{0.402} [39101]
High School or Some College	-0.033 (0.010)	{0.418} [64394]	-0.016 (0.007)	{0.262} [72451]	0.012 (0.005)	{0.208} [128294]
College Graduates	-0.099 (0.026)	{0.217} [6036]	-0.024 (0.011)	{0.1} [16740]	-0.011 (0.003)	{0.04} [120001]

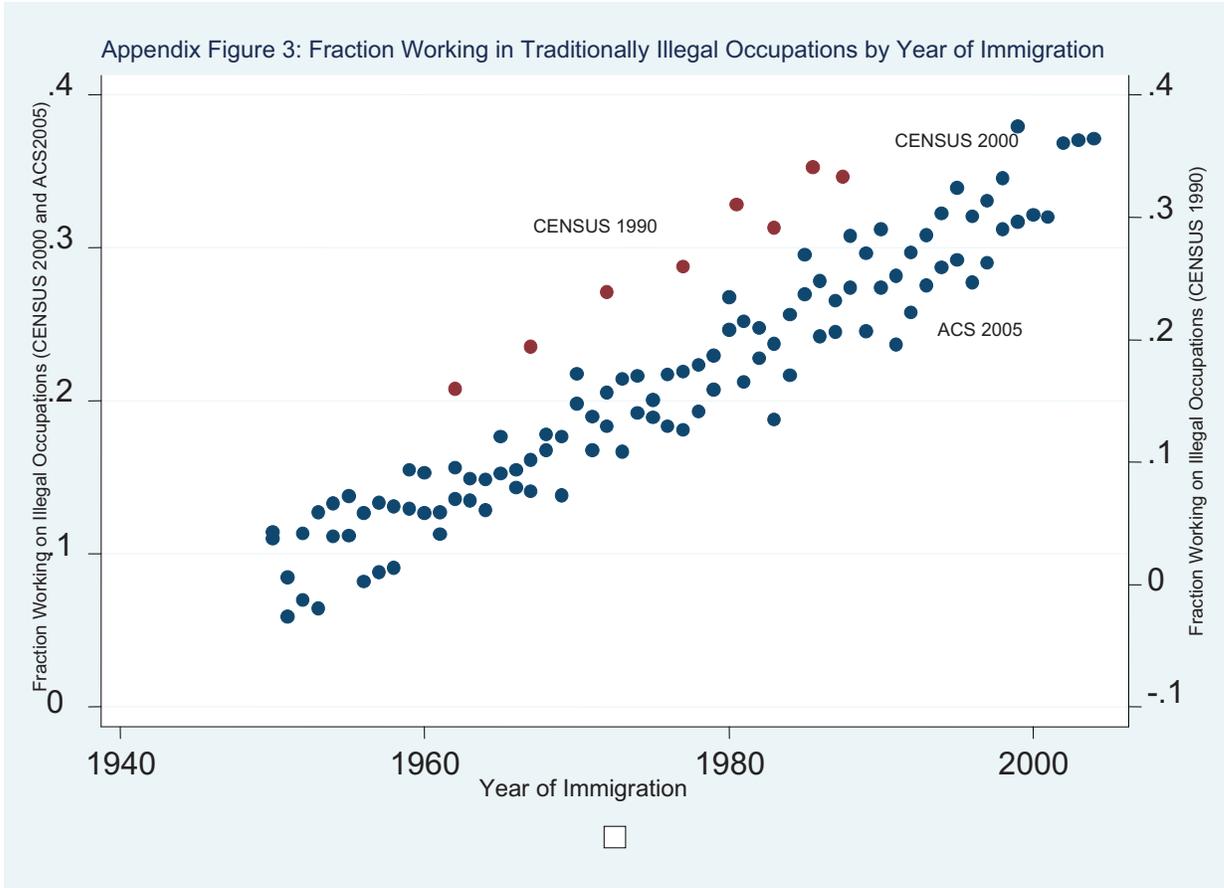
Notes: Reported figures: coefficient on the interaction between the 8 to 18 years in the US dummy and 1990 dummy, its robust standard error (in parenthesis), the mean of the dependent variable {in curly brackets} and the number of observations in each regression [in brackets]. All regressions are weighted using survey weights and contain controls for sex, marital status, race, age and age squared. Survey year FE, years in the US, and a dummy for 8 to 18 years in the US are included. Robust standard errors in parentheses.



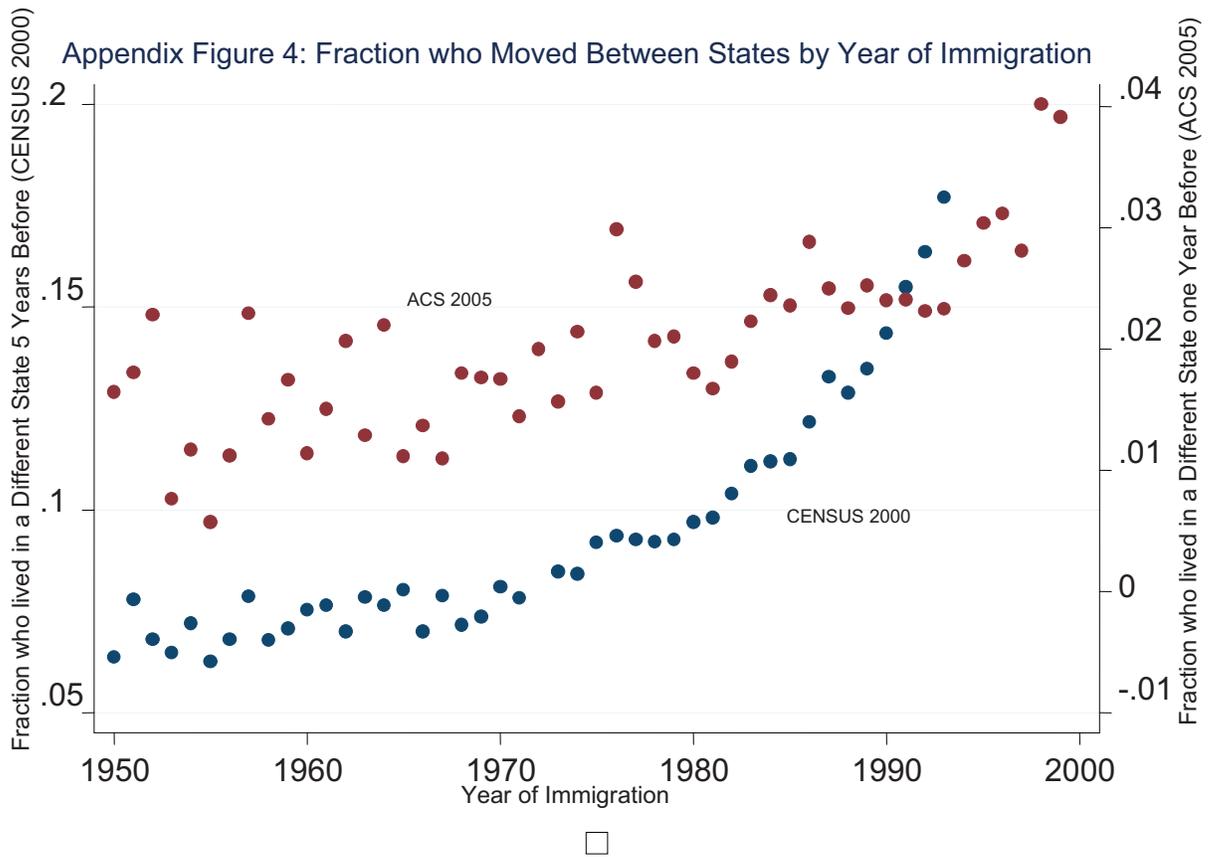
Notes: Weighted averages by year of immigration and survey year. The 1990 CENSUS has fewer data points due to the grouping of the year of immigration variable.



Notes: Weighted averages by year of immigration and survey year. The 1990 CENSUS has fewer data points due to the grouping of the year of immigration variable. Individual wages were inflated to 2009 dollar values using the CPI.



Notes: Weighted averages by year of immigration and survey year. The 1990 CENSUS has fewer data points due to the grouping of the year of immigration variable. For details on how a traditionally illegal occupation is defined see explanations section 3.2.



Notes: Weighted averages by year of immigration and survey year. In the 2000 CENSUS the mobility variable is defined as the probability that the immigrant does not live in the same state he lived 5 years ago (left axis); in the ACS the reference period is one year ago (right axis). Immigrants who arrived after 1994 (in the 2000 CENSUS) and after 2003 (in the 2005 ACS) are dropped from the analysis.