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IZA DP No. 11525

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ABSTRACT

Comparing Wage Gains from Different Immigrant Legalization Programs

We use data from the New Immigrant Survey (NIS) for data on immigrants who were legalized based on family ties or small-scale legalization programs, and the Legalized Population Survey (LPS) for data on immigrants legalized by the IRCA. Estimates suggest that the increase in wage after legalization is about 22% higher for male immigrants who were legalized based on family ties, or smaller scale legalization programs, compared to IRCA beneficiaries. Difference-in-Difference-Difference regressions with National Longitudinal Survey of Youth (1979 cohort and 1997 cohort) comparison groups suggest similar results. A large part of that higher legalization premium can be explained by higher undocumented status penalty brought on by the employer sanctions instituted by the IRCA. The rest of the differential may be explained by changes in the ratio of documented and undocumented workers (supply shock) brought on by large-scale amnesty programs.

JEL Classification: J3, J6

Keywords: immigration, illegal immigration, legalization, undocumented immigrants, wage

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1. Introduction

Estimates suggest that there are about 11.0 million undocumented immigrants in the U.S. (Passel and Cohn, 2017). A number of previous studies show that legalization of undocumented workers leads to increase in wages. Most of these studies use the variation brought on by the 1986 Immigration Reform and Control Act (IRCA) to estimate the wage returns to legalization. The 1986 IRCA legalized 1.6 million undocumented immigrants through a general amnesty[2].The interest in IRCA is understandable given that it is the only general legalization program in the modern history of the U.S. However, family-based legalization and other targeted legalization programs in the U.S. have a long history. Between 1986 and 2009, more than 1.3 million previously undocumented immigrants have been legalized thorough registry, or population-specific legalization programs (Kerwin, 2010). Examples of small-scale[3] population-specific legalization programs in the 1990s include the Nicaraguan Adjustment and Central American Relief Act (NACARA), the Chinese Student Protection Act (CSPA), the Cuban Adjustment Program, as well as legalization programs for Employees of Hong Kong Businesses, Denied Parolees from the former Soviet Union, Vietnam, Laos, Cambodia, Poland, Hungary, American Indians Born in Canada, and Employees of the Panama Canal Company or Government, etc. These population-specific legalization programs have received relatively less attention (Kaushal, 2006; Lofstrom, Hill, and Hayes, 2010; Orrenius, Kerr, and Zavodny, 2012 are some of the exceptions).

While many of these programs have been studied individually, there has not been any attempt to formally compare the wage gains from the IRCA to the post-IRCA legalization programs—which is the primary purpose of this paper. The IRCA also instituted employer sanctions that may have increased undocumented status penalty. Phillips and Massey (2002) find no penalty for the pre-IRCA penalty (Massey, 1987 report the same result) but estimate that illegal immigrants earn about 25% less than comparable legal immigrants in the post-IRCA period. Rivera-Batiz (1999) estimates that legal Mexican immigrants earn about 20% more than undocumented immigrants, after controlling for observable differences. Thus, those who were legalized in the post-IRCA period may receive a larger wage gain. In addition, large-scale legalization programs (IRCA) may bring labor supply shocks (increasing

[2] Another 1.1 million undocumented immigrants were also legalized through Special Agricultural Workers (SAW) program, which is a population-specific program.
[3] For implementation purpose we treat the IRCA as the only large-scale legalization program. All other legalization programs (between 1987 and 2002) are treated as small-scale legalization programs.

the supply of documented workers coupled with a decrease in supply of undocumented workers), which may restrict the gains from legalization, at least in the short run.

Studies that use the variation brought on by the 1986 IRCA, to estimate the wage returns to legalization include Cobb-Clark, Shiells, and Lowell (1995) who found a small, but statistically significant, wage increase after legalization. Kossoudji and Cobb-Clark (2002) (KCC from now on) report that the wage gains from legalization are around 6%. Amuedo-Dorantes, et al. (2007) estimate that legalization increased wages of immigrant men by 9.3%, and immigrant women by 21%. Amuedo-Dorantes and Bansak (2011) report similar results. Pan (2010) estimates that the 1986 IRCA increased wages of legalized immigrants by about 5%. Barcellos (2010) finds only a small, but positive, effect of legalization on wages. Lozano and Sorensen (2011) aim to identify the *long-term* effects of legalization, estimating that legalization increased wages by 20%.

A few papers have focused on population specific smaller-scale legalization programs. Kaushal (2006) uses the Current Population Survey (CPS), to estimate the gains from legalization for individuals legalized under 1997 Nicaraguan Adjustment and Central American Relief Act (NACARA). However, she notes that her treatment group includes legal permanent residents (LPRs), and therefore she estimates an intent-to-treat (ITT) effect, and not a treatment effect. The estimated ITT is between 4.3% and 6.4% for individuals legalized under section 202 (Nicaraguans and Cubans). She notes (*pp-637*) that since 76% of her target population was eligible for amnesty, and take-up rate was 37% the implied treatment on the treated (TT) is ITT/(0.37*0.76) is between 15.3% and 22.8%. Orrenius, Kerr, and Zavodny (2012) find that the 1992 Chinese Student Protection Act (CSPA) increased the wages of Chinese immigrants by almost 24%. Lofstrom, Hill, and Hayes (2010) on the other hand do not find any effect of legalization on wages. Orrenius, and Zavodny (2015) found that temporary protected status (TPS) from deportation increases the wage of non-college educated men by 13.1%.

In this paper, we estimate the difference in the wage gains from legalization of previously undocumented immigrants, who were legalized through family ties, or population-specific legalization programs, and those who were legalized by the 1986 IRCA. We use the New Immigrant Survey (NIS, 2003) for data on the former group and the Legalized Population Survey (LPS, 1992) for data on the latter group. The NIS interviewed a nationally representative sample of immigrants who achieved legal permanent resident (LPR) status in 2002 or 2003. Some of the new LPRs were previously undocumented. Jasso, Massey, Rosenzweig, and Smith (2008), show that

administrative record (available as part of the NIS data) and survey questions can be used to identify the previous legal status of NIS respondents.

The LPS (1992) on the other hand surveyed only previously undocumented immigrants, who were legalized by the 1986 IRCA[4]. The LPS collected wages of respondents when they started working in the U.S., at the time of application for legalization, and at the time of interview (which was up to three years after receiving LPR status). However, the NIS did not collect information on wage of respondents at the time of application for legalization. The NIS only contains information on wages when the immigrants first started working in the U.S. (i.e. when they were staying and working as undocumented workers), and in their post LPR jobs. Therefore, we compare the change in wages (between their first U.S. job and post LPR job) of previously undocumented NIS respondents to the corresponding change in wages of LPS respondents.

One potential problem with difference-in-difference analysis (as described above) is that post-LPR interviews were conducted in 1992 for the LPS respondents and in 2003 for the NIS respondents. The macroeconomic conditions prevalent in 2003 may be different from those prevalent in 1992. For example, the national unemployment rate was 7.5% in 1992, but it was 6.0% in 2003. In addition, the rate of growth in wages of undocumented workers may have been different in 1980s and 1990s. The above analysis cannot separately identify the time effect from cohort size effect. To isolate the cohort size effect we need to control for time effect. We use the data on the Hispanic sample of the National Longitudinal Survey of Youth (NLSY), 1979 cohort and 1997 cohort. This is similar (in spirit) to a difference in difference in difference (DDD) regression which can separately identify cohort effect from time effect.

The Hispanic sample of NLSY79 has been previously used by a number of papers, including KCC (2002), as a comparison group to identify the effects of legalization in the LPS sample. There are observable (and possibly unobservable) differences between NLSY79 respondents and LPS respondents. As KCC (2002) point out the Hispanic sample of NLSY79 is not a control group but a comparison group. They argue that since the objective is to difference out the macroeconomic trends that would have affected the unskilled labor market, a comparison group consisting of the Hispanic sample of NLSY79 is sufficient.

The problem in this paper is somewhat different from that addressed in KCC (2002) and other papers that use the NLSY79 data to estimate the gains from legalization in the population that were legalized by the 1986

[4] The LPS did not interview immigrants who were legalized through the SAW program.

IRCA. The median LPS immigrants started working in 1980, and median NLSY79 respondent started working in 1980 as well. This makes the NLSY79 Hispanic sample a good comparison group for the LPS immigrants since both groups consist of labor market entrants i.e. workers without previous U.S. work experience (KCC, 2002). The median NIS immigrant on the other hand started working in 1991. An ideal comparison group for this group is not available. One option is to continue with NLSY79 Hispanic sample (i.e. use the change in wage between 1992 and 2004 for this group). However, the problem is that the median NIS immigrant started working in 1991 while median NLSY79 respondent started working in 1980. The other option is for a comparison group for NIS group is the NLSY97 Hispanic sample[5]. The advantage of the NLSY97 sample is that it consists of labor market entrants (i.e. no previous U.S. work experience). The disadvantage is that the median NLSY97 respondent started working in 1999 (compared to 1991 for median NIS worker). In our analysis, we use Hispanic respondents from both NLSY79 and NLSY97 as comparison groups for the NIS immigrants. Then we show robustness of our results with respect to changes in the composition of comparison group.

In our analysis, we focus only on male respondents to abstract from labor force participation decision. Passel (2006) estimates that the labor force participation rate of male undocumented immigrants is 94%, but the labor force participation rate of female undocumented immigrants is only 54%. Previous papers suggest that legalization does not lead to significant changes in employment probabilities. While some papers find a small decline in employment rate (Amuedo-Dorantes, Bansak, and Raphael 2007; Amuedo-Dorantes and Bansak 2011), others find a small increase in employment rate especially among women (Pan, 2010). Given the relatively low labor force participation among undocumented immigrant women modeling the participation decision is necessary to address sample selection bias. Since we do not have an appropriate instrument to model the employment decision, we focus only on male respondents.

Our DD results suggest that the increase in wages following legalization was about 22% more for male NIS respondents compared to the male LPS respondents. This estimate is robust to reasonable changes in specification and sample criteria. Our DDD results are also consistent with DD results. DDD results suggest that the increase in wages following legalization was about 18% to 26% more for male NIS respondents compared to the male LPS respondents. Then we explore the reasons behind the differential in wage gains: we compare the starting wages (i.e.

[5] Other potential datasets such as Panel Study of Income Dynamics (PSID) or National Educational Longitudinal Survey (NELS) does not meet the requirements either.

in their first U.S. job) of undocumented immigrants from NIS, and LPS samples. Our estimates suggest that the starting wages (wage in first U.S. job) of the NIS respondents were about 15% lower compared to comparable LPS respondents. Moreover, this wage differential disappears once we control for year of first U.S. job, suggesting employer sanctions depressed wages of undocumented workers in the post IRCA era. This is consistent with Phillips and Massey (2002).

Thus, we show that 1) immigrants who were legalized through family ties, or through small-scale legalization programs in the post-IRCA era received higher legalization premium and 2) a large part of that higher premium can be explained by higher undocumented status penalty brought on by the employer sanctions instituted by the IRCA. The rest of the differential may be explained by changes in the ratio of documented and undocumented workers (supply shock) brought on by a large scale amnesty program such as the IRCA. While we cannot prove that supply shock is a factor, given that a number of other studies have documented that certain occupations have a high density of undocumented immigrants (Gill and Long 1989; Taylor 1992; Massey et al. 1987; Kossoudji and Cobb-Clark 1996), it is plausible that supply shock brought on by mass legalization can restrict wage gain.

Rest of the paper is organized in the following way. Section 2 describes the NIS, LPS, NLSY79, and NLSY97 data. Section 3.1 presents results using Difference-in-Difference (DD) method. Section 3.2 adds the NLSY data to estimate the returns to legalization using Difference-in-Difference-in-Difference (DDD) method. Section 4 concludes.

2. Data

We use data from four different surveys: the New Immigrant Survey (NIS 2003), the Legalized Population Survey (LPS 1992), and the National Longitudinal Survey of Youth (1979 cohort and 1997 cohort or NLSY79 and NLSY97 respectively)[6]. The NIS (2003) is a nationally representative sample of 8,573 new LPRs. In the NIS, respondents were interviewed between June 2003, and June 2004, after getting LPR status in the previous year. Among them 4,402 (51.4%) were adjustees (those who were already in the U.S. on a non-immigrant visa, and changed their status to LPR) and 4,171 (48.6%) were new arrivees (those who arrived with a green card). Some of the adjustees who received green cards were previously undocumented. They were legalized through family ties, or

[6] All empirical analyses were conducted in STATA (version 14.0). Codes for creating regression samples and results are available from the corresponding author on request.

small-scale legalization programs[7]. The NIS data does not allow us to identify how each individual immigrant was legalized. To identify previously undocumented workers in the NIS (2003), we follow the algorithm suggested by Jasso, Massey, Rosenzwig and Smith (2008). We distinguish between two types of undocumented status: those who entered the country without documents (Entry without Inspection or EWI immigrants) and those who overstayed their visa (Over-stayers or OS immigrants). To identify EWI immigrants, we start with a survey question asking the respondents whether they entered the U.S. without inspection. Then, we use administrative data[8] to identify OS immigrants. We use two sources of information in the administrative data: temporary visa status, and class of admission. Temporary visa status is not available for all immigrants. Jasso et al. (2008) suggest that this could be due either to the USCIS not knowing full histories, or statuses not being recorded in their computer system. Some of these immigrants have an "unknown (UU)" code, which Jasso et al. (2008) calls an "*euphemism for illegal status*". Class of admission is another variable in the administrative data that can be used to identify previously undocumented immigrants. Class of admission "Z" refers to immigrants who were admitted through legalization. Following Jasso et. al. (2008), we assume an immigrant to be previously undocumented if either the temporary visa status, or class of admission, suggests they were previously undocumented. Among those that were identified as previously undocumented from administrative sources, some are EWI immigrants, as indicated by the survey question described above. Undocumented immigrants, who are not EWI immigrants, are classified as OS immigrants.

Table 1 presents the sample selection criteria and shows how each criterion changes the sample size for all the data sources. Using this algorithm described above, we find that 1717 NIS respondents were previously undocumented[9]. Out of 1717 previously undocumented immigrants, 872 are male. Among them, 315 have a valid pre-legalization and post-legalization wage observation. We exclude another 24 observations because one of the covariates used in regression analysis is not available, leaving us with 291 observations from the NIS. Out of them 208 (71.5%) have 12 or less years of education.

[7] Since the year of Entry into the U.S. is after 1980 for all but 3 individuals in our sample we know they were not eligible for the 1986 IRCA.
[8] The publicly available NIS data includes some variables from USCIS administrative data. Please see Jasso et al. (2008) for details.
[9] Among them 1387 are EWI and 330 are OS immigrants.

The second data source used in this paper is the Legalized Population Survey. The LPS (1992) surveyed only previously undocumented immigrants, who were legalized by the 1986 IRCA. The logic of Jasso et. al. (2008) can be used in LPS data as well to identify EWI or OS status of LPS respondents. We impose the same sample restrictions as above to the LPS data[10]. After imposing sample restrictions, we end up with 1690 observations from the LPS. Out of them 1396 (82.6%) have 12 or less years of education.

The sample sizes NLSY respondents are shown in columns 3 (NLSY79) and 4 (NLSY97 of Table 1. Following previous research in this area, we focus on Hispanic respondents of NLSY. All NLSY respondents are legal residents of the U.S. (i.e. our comparison group). There are 1000 (930) male Hispanic respondents in NLSY79 (NLSY97). Out of them 659 (652) have a valid before and after wage observation.

Table 2 presents the summary statistics. Panel A shows the summary statistics for all men and Panel B shows the summary statistics for men with less than 12 or less years of education. In our robustness analysis, we restrict our attention to respondents with 12 or less years of education to make the NLSY respondents comparable to the NIS and LPS respondents. We start by comparing the average real hourly wages for different groups, both at their first U.S. job, and after receiving LPR status. All wages are in 2003 prices, and winsorized at 1%.[11] The NIS collected information on the wages of immigrants: when they started working in the U.S., and after they received LPR status. One potential problem with starting wages is recall bias. However, as long as recall bias is not different for the NIS respondents from the LPS respondents, our estimates should still be unbiased.

The starting wages for the NIS (LPS) immigrants refer to wages they earned when they were staying and working in the U.S. as undocumented workers. It is important to note, that while this assumption is clearly valid for EWI immigrants, it may or may not be valid for OS immigrants. For OS immigrants, it is not possible to determine when their visa expired. In other words, OS immigrants entered the U.S. legally, and then at some point during their stay, lost their legal status, when their temporary visas expired. At some time after that, they were legalized, and received LPR status. However, it is not possible to determine if they were staying in the U.S. legally at the time of their first U.S. job. At the time of first U.S. job, the legal status can be accurately determined for the NLSY respondents (citizens), and EWI group (undocumented). However, for the OS group legal status cannot be

[10] All respondents in the LPS were previously undocumented who adjusted their status.
[11] Winsorization allows us reduce the effect of outliers without dropping them from sample. In other words wage below 1st percentile (or above 99th percentile) of wage distribution were replaced by wage at the 1st percentile (99th percentile). If we do not Winsorize the wages, the qualitative results do not change.

accurately determined. In baseline results, we combine EWI and OS immigrants into one category (undocumented). As a robustness check, we report results that include only the EWI immigrants as the treated group (i.e. by dropping OS immigrants from sample).

Column 1 of Table 2 presents summary statistics of LPS respondents; column 2 presents summary statistics of NIS respondents, and columns 3 and 4 presents the summary statistics for NLSY79 and NLSY97 respondents respectively. All wages are in 2003 prices. At their first U.S. job (the "before" wage), the average hourly wage for LPS respondents is $9.90, and for NIS respondents it is $9.05. In their post-LPR job, LPS respondents earned $12.58 (in 1992) and NIS respondents earned $13.68 (in 2003-2004).

At their first U.S. job, average age of LPS respondents is 23.78, and for NIS respondents it is 24.59. Median year of observation for wage at first U.S. job is 1980 for LPS respondents and 1992 for NIS respondents. LPS respondents have 8.86 years of education on an average and NIS respondents have 10.31 years of education. It is possible to determine the number of years of U.S. education in the NIS data, but not in the LPS data. Therefore, we control for whether the immigrants has any U.S. education, which can be determined in both data sets. 18% of LPS respondents and 33% of NIS respondents have some U.S. education. Average U.S. work experience (defined as the number of years between the first job in the U.S. and the current job) is 13.09 years for LPS respondents and 11.51 years for NIS respondents.

English proficiency was evaluated in NIS using a five-point scale (very good, good, fair, poor, and no English ability) and in LPS using a four-point scale. However, in NLSY samples English ability is a binary variable (English deficiency or not). Therefore, we created binary measures (English deficiency or not) for NIS and LPS samples as well. It takes value one is English is average or below and zero otherwise. About 55% of LPS respondents and about 57% of NIS respondents have English deficiency. About 92.2% of LPS respondents and 86.5% of NIS respondents in our sample are from Latin America.

Summary statistics for the NLSY79 and NLSY97 respondents are shown in columns 3 and 4 of Table 2. As column 3 shows, average starting wage for NLSY79 male Hispanic respondents was $8.27 and by 1992 it has increased to $15.10. The average education is this group is 12.13 years. Column 4 shows that the NLSY97 respondents had starting wage of $7.35, and by 2003, average wage for NLSY97 sample has increased to $11.44.

Panel B presents the summary statistics for respondents with 12 or less years of education. Imposing this restriction may reduce heterogeneity and presumably makes NLSY respondents more comparable (in terms of

educational attainment) to NIS and LPS respondents. In the results section below we check the robustness of our baseline results in this subsample.

3. Results

3.1 Comparing the legalization premium for NIS and LPS respondents

First, we test whether the gains from legalization are different for immigrants legalized through family ties, or small-scale legalization programs (NIS immigrants) from those legalized through IRCA (LPS immigrants).

Most papers that use IRCA variation to estimate gains from legalization rely on the change in wage between the time of application[12], and at the time of interview (which was up to three years after receiving LPR status) for identification. However, the NIS did not collect information on wage of respondents at the time of application for legalization. The NIS only contains information on wages when the immigrants first started working in the U.S., and in their post LPR jobs. Therefore, to implement a difference-in-difference regression, we compare the change in wages (of previously undocumented NIS respondents between their first U.S. job, and post LPR job) to the corresponding change in wage of those who were legalized through the 1986 IRCA. We estimate the following regression equation

$$\Delta lnW_i = \varphi_1 \Delta NIS_i + \theta \Delta Y_i + \Delta \varepsilon_i \qquad (2)$$

In the above equation, ΔlnW_i represents the change in (log of) real hourly wage, between the first U.S. job, and post LPR job, of previously undocumented immigrants. ΔY_i represents changes in the control variables. Control variables include individual level controls (such as changes in U.S. work experience or U.S. education) as well as macroeconomic changes (change in the national unemployment rate: between first U.S. job year and post-LPR interview year). The Change in unemployment rate is defined as (unemployment rate at time of post LPR interview) – (unemployment rate at time of first U.S. job). The post-LPR interview year is 1992 for LPS sample and 2003 or 2004 for NIS sample. It varies between -6.0 and 2.7 percentage points. The mean of this variable is -1.68 percentage points. The treatment group is NIS immigrants. LPS immigrants who were legalized through IRCA constitute the control group.

The regression results are presented in Table 3. Columns one to three show the results where the treatment group is all undocumented immigrants (EWI and OS immigrants and irrespective of their country of origin). Column 1 shows the results without any control variables and for maximum possible sample size. In other words we only

[12] KCC (2002) use the beginning wage, wage at the time of application, and wage after legalization in their analysis.

impose the restriction that both pre and post legalization wages are observed. The increase in the wage for NIS immigrants after legalization is 16.3% higher compared to LPS immigrants. In column two, we add the restriction that all covariates must be available. However, we do not control for any covariates. The sample size drop from 2036 to 1981. In this sample, the increase in the wage for NIS immigrants after legalization is 17.4% higher compared to LPS immigrants. In column 3, we control for a number of time varying control variables. The control variables have the expected signs: U.S. work experience increases wage in a concave way. Five years of U.S. work experience increases wage by 14.3% and 10 years of U.S. work experience increases wage by 21.2%. Immigrants who have obtained any education in the U.S. earn 10.7% more than immigrants who do not have any U.S. education. An increase in the difference in unemployment rate between the first U.S. job year, and post LPR interview year, reduces wage growth. Estimates suggest that for a one percentage point increase in the change in unemployment rate (between first U.S. job year, and post LPR interview year) reduces wages by 3.38%. The coefficient of NIS suggest that the increase in wages following legalization was 22.6% more for NIS respondents compared to the LPS respondents. In column 4, we restrict our attention to individuals with 12 or less years of education. The sample size falls from 1981 to 1588. The coefficient of NIS suggest that the increase in wages following legalization was 21.7% more for NIS respondents compared to the LPS respondents. In columns five and six, we further restrict our attention only to the EWI immigrants (i.e. drop OS immigrants from regression sample), and the sample size falls from 1588 to 1519. Coefficient of NIS-EWI remains similar to the one reported in previous columns. In column 6, we further restrict the sample to EWI immigrants from Latin America (and 12 years or less education). The results remain similar.

Results from Table 3 suggest that legalization premium for male NIS immigrants is significantly more compared to male LPS immigrants. Next, we check the robustness of these results. First, we use un-Winsorized wages instead of Winsorized (at 1%) wages. The results (in Appendix, Table 9) suggest that the results are not sensitive to this change. In the rest of the paper, we use Winsorized wages as it reduces the effect of outliers.

Next, we add time-constant regressors in the regressions. The effects of time constant controls should be differenced out in a first difference regression. However, one rationale for including them may be that immigrants with different characteristics (such as English ability, country of origin) may have different baseline wage-growth rates. Table 4 presents the results. In the first column, in addition to the time varying controls mentioned in Table 3, we include controls for total education, dummy for English deficiency, and age at the time of first U.S. job (and its

square). We also add source country/region dummies to account for possible country specific trends in wage growth[13]. Estimate in column one suggests that the wage growth is 13.0% lower for immigrants with English deficiency compared to immigrants who are not English deficient. Estimates also suggest that immigrants with more education have a higher wage growth. However, the estimated coefficient of NIS dummy is robust to these additional controls. Estimated increase in wages among NIS immigrants is 25.7% more compared to LPS immigrants, which is similar in magnitude to the coefficient reported in column three of Table 3. In second column, instead of controlling for U.S. work experience, we include a set of dummies for the year of first job in U.S. Note that once we have dummies for first year of U.S. job return to work experience is not identified anymore. All other controls are same as column one. Result from this specification suggests that the increase in wages among NIS immigrants was 21.3% more compared to LPS immigrants. Columns three and four re-estimate the specifications mentioned in columns one and two, but restricted to immigrants with 12 years or less education. The estimates are similar to those reported in columns one and two. Therefore, the estimates suggest that the result that the gain from legalization is larger for those who were legalized through small-scale legalization programs compared to gains from legalization from the IRCA.

In Table 5, Panel A, we restrict our treatment group to EWI immigrants only (i.e. we drop OS immigrants from regression sample). First two columns are for without any educational restriction and last two columns are for individuals with 12 years or less education (same structure as Table 4). Here we only report the coefficient of the NIS dummy. In Panel B, we further restrict our treatment group to EWI immigrants from Latin America. The coefficient of interest is similar to those discussed in Table 4.

3.2 Difference in Difference in Difference regression.

The analysis above requires a critical identifying assumption. The post-LPR interviews were conducted in 1992 for LPS respondents, and 2003-2004 for NIS respondents. The macroeconomic conditions prevalent in 2003-2004, were different from those prevalent in 1992. In addition, rate of growth in wage in 1980s may be different compared to the growth rate in wage during the 1990s[14]. The above specification attempts to control for the changes by adding the change in unemployment rate but it cannot separately identify the 'time effect' from the 'cohort size effect' – which is our primary parameter of interest.

[13] In the public version of the NIS data, source country is identified for about 88% of the NIS-EWI immigrants. For the rest only the source region is identified.
[14] There is some evidence that skill prices in the U.S. may have changed between 1980 and 2000 (Lubotsky, 2009).

In order separately identify the time effect from the cohort effect; we implement a difference in difference in difference (DDD) analysis with the Hispanic respondents of NLSY79 and NLSY97 as comparison groups. In the Introduction Section, we discussed the relative advantage and disadvantage of NLSY79 and NLSY97 respondents as comparison group. As we have discussed there, in our baseline analysis, we include all data from Hispanic respondents from NLSY79 and NLSY97 cohorts. Each NLSY79 respondent has two ΔlnW_i observations: one representing the change between first job and 1992 and second once representing change between 1992 and 2004. Respondents from all the other groups (NLSY97, LPS, and NIS) has one ΔlnW_i observation. We estimate the following regression equation:

$$\Delta lnW_i = \varphi_1 \Delta LPS_i + \varphi_2 \Delta NIS_i + \theta \Delta Y_i + \gamma(After) + \Delta\varepsilon_i$$

In this equation, φ_1 represents the effect of legalization through IRCA (LPS respondents) and φ_2 represents the effect of legalization through small-scale legalization programs or thorough family ties (NIS respondents), given the comparison group (the NLSY respondents). γ represents the coefficient of $After$, which is one for NIS respondents, NLSY97 respondents, and the 1992-2004 change in wage observations for NLSY79 respondents. We check the robustness to changes in composition of comparison group using two strategies. First, we keep the observations from NLSY97 but exclude the observations from the second half of NLSY79 cohort (i.e. change in the wage between 1992 and 2004 for NLSY79 respondents). Second, we exclude the observations from NLSY97 but keep the observations from the second half of NLSY79 cohort (i.e. change in the wage between 1992 and 2004 for NLSY79 respondents).

In regressions, we allow returns to human capital acquired in the U.S. to depend on legal status. For example, the return to U.S. education and work-experience for undocumented immigrants may be different from NSLY Hispanic respondents, who are U.S. citizens. Table 6 presents the estimation results. Columns 1 and 2 include all previously undocumented (EWI and OS) immigrants along with NLSY79 and NLSY97 respondents. Estimates show that returns to U.S. work experience is indeed lower for undocumented immigrants compared to U.S. citizens. For NLSY respondents, 5 (10) years of U.S. work experience increases wage by 30.8% (54.9%). On the other hand, for undocumented immigrants, 5 (10) years of U.S. work experience increases wage by 16.1% (23.5%). This result is consistent with KCC (2002) who found that returns to experience of previously undocumented immigrants in LPS was lower than that of NLSY respondents. Furthermore, EWI or OS immigrants who have any U.S. education earn 11.0% more than EWI or OS immigrants without any U.S. education do. NLSY

respondents who have had any education in between their two wage observations earn 14.2% more than those who did not get any additional education.

Inclusion of NLSY comparison groups allows us to identify a separate time effect (γ). The coefficient "period 2" estimates whether the average change in wages in the before period (when LPS respondents were legalized) are different from the average change in wage in the after period (when NIS respondents were legalized). Regression estimates suggest that γ is insignificant. Estimates show that the NIS immigrants received 25.7% increase in wage after legalization and the LPS immigrants received 7.5% increase in wage. The coefficient estimate is significant for NIS respondents but not for LPS respondents. Moreover, the difference between the coefficients estimates (18.3%) is statistically significant at 5% level of significance (shown in the last row). Second column adds time constant regressors (education, English ability, and age at the time of first U.S. job and its square) to the set of controls included in the first column. Estimates in column two suggest the difference between the coefficient estimates (21.4%) is statistically significant at 5% level of significance. In columns three and four the sample is restricted to immigrants and citizens with 12 years or less education. They produce results similar to columns 1 and 2.

Next, we compare our estimates to those reported in the literature. Our estimates suggest that the benefits from legalization for LPS respondents is between 7.5% and 21.3%. However, none of them is statistically significant. In comparison, previous papers (discussed in the introduction section) have estimated the gains from legalization for IRCA beneficiaries between 5% and 20%. On the other hand, previous paper have reported that gains from legalization (implied TT estimates as discussed in details in the introduction section) from various small-scale legalization program varies between 15% and 48%. Our estimates suggest a gain between 25.7% and 40.8%. Therefore, our estimates are comparable to those reported in the literature before. Furthermore, our DDD estimates suggest that the wage gain from legalization among NIS respondents is about 20 percentage points more than the gain experienced by LPS respondents. This DDD estimate is comparable to DD estimates presented in Table 3 and 4 of this paper.

Table 7 presents the results of additional robustness checks. In Table 7, we only present the difference between the coefficients of NIS and LPS. First two columns of Panel A of Table 7 reproduces the results from columns 1 and 2 of Table 6. In columns 3 and 4, we restrict our treatment groups to only EWI immigrants (along with NLSY respondents as comparison group). Estimates remain similar to columns 1 and 2. In columns 5 and 6 we

further restrict our treatment groups to only EWI immigrants from Latin America (along with NLSY respondents as comparison group). Estimates are robust to these sample restrictions.

Next, we keep the observations from NLSY97 but exclude the observations from the second half of NLSY79 cohort (i.e. change in the wage between 1992 and 2004 for NLSY79 respondents). Results from these regressions are presented in Panel B of Table 7. Estimates suggest that the wage gain from legalization among NIS respondents is between 21.2 percentage and 28.4 percentage points more than the gain experienced by LPS respondents. Therefore, the results are similar to those reported in Panel A.

Next, we exclude the observations from NLSY97 but keep the observations from the second half of NLSY79 cohort (i.e. change in the wage between 1992 and 2004 for NLSY79 respondents). Results from these regressions are presented in Panel C of Table 7. Estimates suggest that the wage gain from legalization among NIS respondents is between 30.9 percentage and 42.4 percentage points more than the gain experienced by LPS respondents. These estimates are somewhat higher than those reported in Panels A and B. However, overall the results from DDD estimates (Table 7) suggest that the wage gain among NIS respondents were higher than the wage gain among LPS respondents. This is consistent DD results (Tables 3 and 4).

3.3 Why did the legalization premium change?

Thus, the results suggest a difference in the legalization premium: between those legalized through small-scale programs, or family ties (i.e. NIS immigrants) and those who were legalized by IRCA (LPS immigrants). In this section, we explore the potential reasons behind this difference in legalization premium. One reason behind the increased legalization premium may be an increased undocumented status penalty when the NIS respondents started their first U.S. job. For example, if the employer sanction provisions in the 1986 IRCA increased the undocumented status penalty, then the NIS respondents will have a lower wage in their first U.S. job. This in turn may explain why they have a bigger legalization premium. Thus, we compare the starting wages (i.e. wages on their first U.S. job) of undocumented immigrants from NIS and LPS samples. The regression equation is given by

$$lnW_i = \mu_1 NIS + \rho Y_i + \delta_j + \varepsilon_i \qquad (3)$$

Where W_i is the real hourly wage, in the first U.S. job. Y_i represents a vector of control variables: age at the time of first job, total education (in years), and English ability. We also include source country dummies.

Panel A of Table 8 presents the results from estimating equation (3). Estimates in column 1 suggest that NIS respondents earned 14.5% less than comparable LPS respondents. In column 2, we restrict the sample to EWI immigrants only. The differential remains 14.4%. In column 3, we further restrict the sample to EWI immigrants from Latin America only. The differential now is 15.9%. In all cases, the coefficient is significant at 1% level of significance. Column three to six presents the results for sample restricted to individuals with 12 years of less education. The result remains unchanged.

In Panel B, we add dummies for year of first U.S. job, in addition to the control variables included in Panel A. Now there are no differences between the starting wages of NIS immigrants, and LPS respondents. This may suggest that the starting wages of the NIS respondents were depressed by the employer sanctions instituted by the IRCA. Once it is controlled for, the differential disappears[15]. Thus, these results suggest that a substantial part of higher increase in wages received by NIS immigrants (legalized through family ties, or small-scale legalization programs) can be explained by employer sanctions brought on by the IRCA. The remaining part may be explained by supply shock. Large scale amnesty programs increase in supply of documented workers and reduce the supply of undocumented workers, in the short run (KCC 2002). A number of other studies have documented that certain occupations have a high density of undocumented immigrants (Gill and Long 1989; Taylor 1992; Massey et al. 1987; Kossoudji and Cobb-Clark 1996) possibly due to network effects (Patel and Vella 2013). Passel (2009) estimates that even though undocumented immigrants constituted only about 4.9% of the total civilian labor force; 40% of all brick-masons, block-masons, and stone-masons are undocumented immigrants. Similar concentrations can be found in other occupations, such as drywall installers, ceiling tile installers and tapers (37%), roofers (31%), construction helpers (28%), and construction laborers (27%) (Passel, 2009). These occupations are male dominated. In general, more than 10% of all workers employed in farming, cleaning, construction, and food preparation industries are undocumented immigrants.

Therefore, large-scale legalizations increase the ratio of documented and undocumented workers in occupations frequented by male undocumented immigrants. This may imply that undocumented immigrants, who

[15] It is worth noting that the results in Tables 3 to 7 are about comparing the *change* between first U.S. wage, and post-LPR wage. Thus, time-constant individual level unobserved heterogeneity was differenced out. In Table 8, the comparison is at the *level* of wages, thus individual specific unobserved heterogeneity may be a concern. In other words, μ_1 represent effects of changes in the undocumented status penalty after the 1986 IRCA, and unobserved difference between NIS, and LPS respondents. In this case, these two effects cannot be separately identified, in the absence of a suitable instrument.

are legalized through a large-scale legalization program, may not get the full benefit of legalization in the short run, since the increase in labor supply would lead to downward pressure on wages, especially for low-skilled workers (Mansour, 2010 for Palestinian workers).

4. **Conclusion**

Camarota (2004) estimated that in 2002, the net cost[16] of providing services to undocumented immigrants was about $10.4 billion. He further estimated that legalizing all undocumented immigrants would increase the net cost to about $29 billion. While "amnesty" type programs have generated large opposition, the cost of deporting undocumented immigrants is likely to be even higher. An estimate from Immigration and Customs Enforcement, suggests that cost of detaining, and deporting, an undocumented immigrant is $12,500[17] — or about $137 billion to deport all undocumented immigrants.

We estimate whether the gains from legalization are different for NIS immigrants, compared to those who were legalized through the 1986 IRCA (LPS immigrants). We show that 1) NIS immigrants, who were legalized through family ties, or through small-scale legalization programs in the post-IRCA era, received higher legalization premium and 2) a large part of that higher premium can be explained by higher undocumented status penalty brought on by the employer sanctions instituted by the IRCA. The rest of the differential may be explained by supply shock brought on large-scale amnesty programs through changes in the ratio of documented and undocumented workers. While we cannot prove that supply shock is a factor, if it is, then that may imply that previously undocumented immigrants, who are legalized through a large-scale legalization program, may not get the full benefit of legalization in the short run.

[16] Net cost is the cost of all services provided to illegal immigrants, minus all federal and state taxes paid by illegal immigrants.

[17] http://blog.chron.com/immigration/2011/01/ice-reveals-cost-for-deporting-each-illegal-immigrant/

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Table 1: Sample criteria and size

	NIS	LPS	NLSY79	NLSY97
Total	8573	6159	2002	1811
Adjustee	4402	6159	0	0
Previously undocumented	1717	6159	0	0
Male	872	3444	1000	930
Has pre and post-legalization U.S. wage	315	1721	661	652
& all other covariates available	291	1690	659	652
& education <=12 years	208	1396	450	402

Table 2: Summary statistics

	LPS	NIS	NLSY79	NLSY97
Panel A: All respondents				
Period covered	First job to 1992	First job to 2003	First job to 1992	First job to 2003
"Before" Wage	9.90	9.05	8.27	7.35
	(8.38)	(6.97)	(8.10)	(5.63)
"After" Wage	12.58	13.68	15.10	11.44
	(6.82)	(8.03)	(9.51)	(7.81)
Age at first U.S. job	23.78	24.59	19.10	16.79
	(7.76)	(7.73)	(1.62)	(1.36)
Year of first U.S. job	1978.91	1991.85	1980.59	1998.80
	(3.76)	(5.78)	(1.89)	(1.53)
English deficiency	0.55	0.57	0.14	0.05
	(0.50)	(0.50)	(0.35)	(0.22)
Years of education	8.86	10.31	12.13	12.22
	(4.44)	(4.59)	(2.59)	(2.03)
U.S. experience	13.09	11.51	11.76	5.20
	(3.76)	(5.81)	(1.91)	(1.53)
Any U.S. educ.	0.18	0.33	0.51	0.83
	(0.38)	(0.47)	(0.50)	(0.38)
From Latin America	0.85	0.79	-	-
	(0.36)	(0.41)	-	-
N	1690	291	659	652
Panel B: Respondents with 12 years or less education				
"Before" Wage	9.45	7.91	8.22	7.39
	(7.98)	(4.47)	(7.86)	(6.16)
"After" Wage	11.69	12.01	14.17	11.09
	(5.77)	(6.19)	(8.95)	(7.49)
Age at first U.S. job	23.29	23.03	19.10	16.80
	(7.71)	(6.89)	(1.65)	(1.35)
Year of first U.S. job	1978.76	1990.99	1980.62	1998.79
	(3.88)	(5.40)	(1.99)	(1.54)
English deficiency	0.64	0.72	0.16	0.06
	(0.48)	(0.45)	(0.37)	(0.25)
Years of education	7.47	8.02	10.91	11.06
	(3.56)	(3.46)	(1.89)	(1.54)
U.S. experience	13.24	12.38	11.74	5.21
	(3.88)	(5.41)	(2.00)	(1.54)
Any U.S. educ.	0.12	0.26	0.33	0.73
	(0.32)	(0.44)	(0.47)	(0.44)
From Latin America	0.93	0.90	-	-
	(0.26)	(0.31)	-	-
N	1387	201	450	402

Note: NLSY79 and NLSY97 sample includes only Hispanic male respondents in those surveys.

Table 3: DD regressions (comparing wage growth among the NIS respondents with the LPS respondents)

	No restriction on education			Respondents with 12 years or less education		
	All	All	All	All	EWI	EWI from Lat. Am.
NIS	0.163***	0.174***	0.226***	0.217***	0.232***	0.257***
	(4.215)	(4.473)	(5.204)	(4.678)	(4.830)	(5.115)
U.S. exp.			0.0357***	0.0362***	0.0298*	0.0236
			(3.071)	(2.597)	(1.929)	(1.351)
U.S. exp. sq.			-0.00145***	-0.00145***	-0.00121**	-0.00101*
			(-3.502)	(-3.061)	(-2.302)	(-1.706)
Any U.S. edu.			0.107***	0.0995**	0.106**	0.0912*
			(2.757)	(2.015)	(2.113)	(1.734)
Change in UR			-0.0338***	-0.0291***	-0.0296***	-0.0290***
			(-3.789)	(-3.001)	(-2.962)	(-2.802)
Constant	0.293***	0.293***	0.0124	0.00600	0.0451	0.0822
	(19.15)	(19.08)	(0.146)	(0.0579)	(0.394)	(0.635)
Observations	2,036	1,981	1,981	1,588	1,519	1,420

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Robustness of DD results: adding time constant regressors in regressions

	No restriction on education		Respondents with 12 years or less education	
	(1)	(2)	(3)	(4)
NIS	0.257***	0.213**	0.241***	0.225**
	(5.958)	(2.462)	(5.096)	(2.527)
U.S. exp.	0.0292**		0.0392***	
	(2.411)		(2.672)	
U.S. exp. Sq.	-0.00118***		-0.00145***	
	(-2.808)		(-2.973)	
Any U.S. educ	-0.0439	-0.0424	-0.0396	-0.0252
	(-0.975)	(-0.916)	(-0.763)	(-0.470)
Change in UR	-0.0310***	0.255	-0.0286***	0.0401
	(-3.471)	(1.021)	(-2.936)	(0.161)
Total education	0.0109**	0.0101**	0.0123**	0.0107**
	(2.453)	(2.236)	(2.415)	(2.077)
Age at first U.S. job	0.00545	0.00884	0.00973	0.0122
	(0.546)	(0.876)	(0.882)	(1.095)
Age at first U.S. job sq.	-0.000240	-0.000289*	-0.000314*	-0.000352*
	(-1.491)	(-1.774)	(-1.766)	(-1.944)
English deficient	-0.144***	-0.154***	-0.162***	-0.172***
	(-3.858)	(-4.072)	(-4.109)	(-4.258)
Constant	-0.292	-0.647**	-0.283	-0.598*
	(-1.346)	(-2.255)	(-1.013)	(-1.752)
Source Country FE	Yes	Yes	Yes	Yes
Year of first job FE	No	Yes	No	Yes
Observations	1,981	1,981	1,588	1,588

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Robustness of DD results: changing the definition of treatment group

	No restriction on education		Respondents with 12 years or less education	
	(1)	(2)	(3)	(4)
Panel A: EWI immigrants only				
NIS	0.254***	0.200**	0.251***	0.221**
	(5.493)	(2.113)	(5.148)	(2.378)
Time varying regressors	Yes	Yes	Yes	Yes
Time constant regressors	Yes	Yes	Yes	Yes
Source Country FE	Yes	Yes	Yes	Yes
Year of first job FE	No	Yes	No	Yes
Observations	1,824	1,824	1,519	1,519
Panel B: EWI immigrants from Latin America only				
NIS	0.288***	0.222**	0.281***	0.235**
	(6.047)	(2.321)	(5.772)	(2.506)
Time varying regressors	Yes	Yes	Yes	Yes
Time constant regressors	Yes	Yes	Yes	Yes
Source Country FE	No	Yes	No	Yes
Year of first job FE	No	Yes	No	Yes
Observations	1,577	1,577	1,420	1,420

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Controls in columns 1 and 3 time include following varying regressors: U.S. experience (and square), Any U.S. education, change in UR. Columns 2 and 4 do not control for U.S. experience, instead it adds year of first U.S. job dummies as controls. Time constant regressors included in all columns (1-4) include years of education, age at first U.S. job (and square), and indicator for English deficiency, and source country/region fixed effects.

Table 6: DDD regressions: Using NLSY79 and NLSY97 to separately identify time effect and cohort effect

	No restriction on education		Respondents with 12 years or less education	
	(1)	(2)	(3)	(4)
LPS	0.0747	0.157	0.132	0.213
	(0.446)	(0.940)	(0.636)	(1.033)
NIS	0.257**	0.371***	0.291**	0.408***
	(2.388)	(3.407)	(2.110)	(2.976)
“Second period” dummy	0.00491	-0.0156	0.0292	0.0101
	(0.0609)	(-0.193)	(0.281)	(0.0970)
U.S. exp.	0.0681***	0.0667***	0.0723***	0.0709***
	(4.591)	(4.517)	(3.918)	(3.862)
U.S. exp. Sq.	-0.00133***	-0.00132***	-0.00155***	-0.00150***
	(-4.671)	(-4.641)	(-4.381)	(-4.226)
U.S. exp. * Undocumented	0.0409***	0.0432***	0.0366***	0.0419***
	(3.490)	(3.742)	(2.580)	(2.997)
U.S. exp. Sq.* Undocumented	-0.00174***	-0.00173***	-0.00159***	-0.00167***
	(-4.196)	(-4.272)	(-3.322)	(-3.571)
Any U.S. educ.	0.142***	0.0951**	0.118**	0.0962*
	(3.574)	(2.353)	(2.399)	(1.942)
Any U.S. educ.* Undocumented	0.110***	-0.0324	0.102**	-0.0276
	(2.818)	(-0.775)	(2.041)	(-0.535)
Years of educ.		0.0169***		0.0147***
		(4.604)		(3.136)
Age at first U.S. job		-0.00291		0.00177
		(-0.315)		(0.175)
Age at first U.S. job sq.		-0.000136		-0.000196
		(-0.892)		(-1.187)
English deficient		-0.0654**		-0.0991***
		(-2.140)		(-2.979)
Constant	-0.0144	-0.0642	-0.0517	-0.141
	(-0.100)	(-0.330)	(-0.290)	(-0.608)
Observations	3,762	3,762	2,751	2,751

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Robustness of DDD results: changes in the definition on treatment and control groups

	Treatment: All undoc.		Treatment: EWI		Treatment: EWI from Lat. Am.	
Panel A: control group includes NLSY79 (both observations) and NLSY97						
	(1)	(2)	(3)	(4)	(5)	(6)
NIS-LPS	0.183**	0.214**	0.188**	0.214**	0.241**	0.262***
	(2.012)	(2.351)	(2.034)	(2.309)	(2.565)	(2.785)
Time varying controls	Yes	Yes	Yes	Yes	Yes	Yes
Time constant controls	No	Yes	No	Yes	No	Yes
Observations	3,762	3,762	3,605	3,605	3,358	3,358
Panel B: control group includes NLSY79 (first observation only) and NLSY97						
	(1)	(2)	(3)	(4)	(5)	(6)
NIS-LPS	0.212**	0.237**	0.218**	0.234**	0.271**	0.284***
	(2.043)	(2.289)	(2.066)	(2.232)	(2.538)	(2.673)
Time varying controls	Yes	Yes	Yes	Yes	Yes	Yes
Time constant controls	No	Yes	No	Yes	No	Yes
Observations	3,292	3,292	3,135	3,135	2,888	2,888
Panel C: control group includes NLSY79 (both observations)						
	(1)	(2)	(3)	(4)	(5)	(6)
NIS-LPS	0.309	0.359*	0.314	0.368*	0.368	0.420*
	(1.386)	(1.662)	(1.404)	(1.697)	(1.638)	(1.926)
Time varying controls	Yes	Yes	Yes	Yes	Yes	Yes
Time constant controls	No	Yes	No	Yes	No	Yes
Observations	3,110	3,110	2,953	2,953	2,706	2,706

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Set of time varying controls in columns 1, 3, 5 include U.S. experience (and squared) and any U.S. education. All time varying controls are interacted with undocumented status. Columns 2, 4, and 6 includes time constant regressors (total education, age at first U.S. job, and English deficiency), in addition to the time varying regressors mentioned above.

Table 8: First U.S. job wage regressions (comparing NIS and LPS respondents)

	No restriction on education			Respondents with 12 years or less education		
	All	EWI	EWI from Lat. Am.	All	EWI	EWI from Lat. Am.
Panel A: without year of first job dummies						
	-0.145***	-0.144***	-0.159***	-0.141***	-0.160***	-0.146***
	(-4.204)	(-3.894)	(-4.206)	(-3.582)	(-3.971)	(-3.530)
Year of first job FE	No	No	No	No	No	No
Observations	1,981	1,824	1,577	1,588	1,519	1,420
Panel B: with year of first job dummies						
NIS	-0.0169	-0.0305	-0.0558	-0.0198	-0.0236	-0.0455
	(-0.271)	(-0.453)	(-0.827)	(-0.279)	(-0.323)	(-0.624)
Year of first job FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,981	1,824	1,577	1,588	1,519	1,420

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Appendix

Table 9: DD regressions (comparing wage growth among the NIS respondents with the LPS respondents based on un-winsorized wages)

	No restriction on education			Respondents with 12 years or less education		
	All	All	All	All	EWI	EWI from Lat. Am.
NIS	0.195***	0.208***	0.267***	0.271***	0.285***	0.303***
	(4.296)	(4.588)	(5.228)	(4.873)	(4.944)	(5.073)
U.S. exp.			0.0471***	0.0503**	0.0426*	0.0187
			(2.683)	(2.314)	(1.845)	(0.951)
U.S. exp. sq.			-0.00195***	-0.00205***	-0.00174**	-0.000880
			(-2.985)	(-2.639)	(-2.124)	(-1.370)
Any U.S. edu.			0.0978**	0.0677	0.0861	0.0576
			(2.007)	(0.948)	(1.236)	(0.781)
Change in UR			-0.0307***	-0.0263**	-0.0276**	-0.0279**
			(-2.899)	(-2.270)	(-2.336)	(-2.284)
Constant	0.272***	0.273***	-0.0562	-0.0797	-0.0396	0.110
	(14.58)	(14.65)	(-0.472)	(-0.537)	(-0.250)	(0.742)
Observations	2,036	1,981	1,981	1,588	1,519	1,420

Robust t-statistics in parentheses
*** p<0.01, ** p<0.05, * p<0.1