DISCUSSION PAPER SERIES

IZA DP No. 5492

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February 2011

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zur Zukunft der Arbeit
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ABSTRACT

The Labor Market Value to Legal Status*

We present estimates of the effect of legal immigration status on earnings of undocumented workers. Our contribution to the literature centers on a two-step procedure that allows us to first estimate the legal status of an immigrant and then estimate the effect of the Immigration Reform and Control Act (IRCA) on immigrants' labor market outcomes using a triple difference approach. From a sample of young to middle aged Mexican men, our results show that IRCA causes a 20 log point increase in labor market earnings of Mexican immigrants over the long run, and that nearly all of this increase is in the occupational wage. These results suggest that the primary disadvantage for undocumented workers is the type of jobs that they are able to obtain. We estimate the model for immigrants from other countries not benefiting from IRCA to the extent that Mexican immigrants did, and find no systematic bias towards positive and significant results.

JEL Classification: J08, F22

Keywords: immigration, legal status, immigration reform

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* This research was supported in part by a grant from the Ford Foundation to Research and Training Program on Poverty and Public Policy at the University of Michigan. We thank seminar participants at the CReAM at UCL, IZA Policy Evaluation Meetings, IZA Transatlantic Meetings, Lancaster University, SFI-Copenhagen, UC Davis, University of Arizona, University of Florida, and Western Michigan University. Alberto Davila, Arindrajit Dube, Christian Dustmann, and Mindy Marks provided useful comments and suggestions. Any ommission or errors are our own.

Estimates from the Pew Hispanic Center put the 2005 population of undocumented immigrants at around 11 million. A majority of these people are Mexican born (54%), and a majority of recent Mexican immigrants (80%) are undocumented (Passel 2005). Public opposition to immigration, particularly unauthorized immigration, has increased as unemployment rates have risen. However, there still remains some public support for a path to legal status, even in immigrant heavy states. A recent LA Times poll found that 68% of Californians supported allowing immigrants who have been working in the U.S illegally and who entered illegally, but have been employed for the last two years, to be allowed to stay in the U.S. Even when focusing on White Anglo Californians, the poll found that a majority (56%) still supports this policy (Decker 2010). Recent proposals for immigration reform have included some path to regularized status for the undocumented. Research that pins down the magnitude of the effect of legal status on earnings and sheds some light on the mechanisms through which this takes place could be very valuable to the current policy debate.

In this paper, we estimate the change in earnings for Mexican born workers of the last general amnesty for undocumented workers: the Immigration Control and Reform Act (IRCA) of 1986. Eligibility for IRCA required that immigrants had resided in the U.S. since prior to 1982. If legal status was observed in the Census, a simple difference-in-difference approach between the documented and undocumented who arrived before and after that date would be sufficient to identify the causal effect of obtaining legal status through the amnesty on labor market outcomes. As legal status is not observed in the Census, we turn to a survey in which it is observed: the Mexican Migration Project. Using this dataset, we estimate a logistic regression of documentation status on a number of demographic variables that are also observed in the Census. We use the parameters estimated in this model to predict the likelihood that an individual observed in the U.S. Census is undocumented. Following this, a difference-in-difference model is estimated to identify the causal effect of legal status obtained under IRCA on labor market outcomes of Mexican migrants. By focusing on both overall earnings and occupational wages, our findings also attempt to shed some light on how labor markets function for these workers.

Our study also includes a number of placebo tests. Using our sample of Mexican migrants, we run our model on a number of outcome variables for which we do not expect to find significant effects. We estimate the effect on overall earnings and occupational wages for groups for whom we would not expect to find an effect. Specifically, we measure the effect of IRCA eligibility on the labor market outcomes of U.S. born Latinos and non-Mexican immigrants (a group that, on average, is much less likely to be undocumented). Finally, we estimate our model separately for the immigrants from 55 countries that are the largest senders of migrants to the U.S. and examine the empirical distribution of T-statistics from these regressions to look for any systematic bias towards finding statisitcally significant positive effects.

1 Prior Evidence on Effect of Legal Status

There has been a number of previous studies which attempt to estimate the effect of legal status on labor market outcomes. Additionally, there are a number of relevant studies which suggest mechanisms through which this effect may take place. In other words, if legal status has effects on labor market outcomes, there should be both theoretical and empirical evidence for the mechanisms through which this takes place.

1.1 Evidence on Potential Mechanisms of Effect

Recently, economists have begun to study whether the low pay of undocumented workers is attributable to exploitation by employers. The underlying assumption is that legal status would improve bargaining power. Hotchkiss and Quispe-Agnoli (2009) test for this. Using the invalid Social Security numbers as a proxy for being undocumented, they find that documented workers are less attached to a particular firm than are undocumented workers. This difference in labor elasticity of supply to the firm results in equally productive documented and undocumented workers being paid different wages. This difference is likely a result of a lower arrival rate of job offers for undocumented workers, as some firms may not be willing to hire them. [1] Chen (2010) finds that Mexican born workers have much higher returns to being

[1] Hotchkiss and Quispe-Agnoli (2009) apply the model used by Ransom and Oaxaca (2010) and Ransom and Oaxaca (2005) to measure the contribution of differential market power over male and female workers to the male-female earnings gap.

in a union than natives and other immigrant groups. Both of these studies are consistent with employer market power over undocumented immigrants explaining their lower wages relative to natives.

Undocumented workers may also not be able to compete for the same set of jobs as the documented. Peri and Sparber (2009) show that low-skilled natives specialize in occupations requiring communication (as they are likely to have a better command of English), while the comparative advantage of low-skilled immigrants is to work in occupations that require more manual labor. A similar point is made by Ball, Dube and Sorensen (2010), who show that occupations heavy in communicative tasks are more likely to require an occupational license than are occupations that are more manual task intensive. Their paper also documents the fact that Mexican born workers, more likely to be undocumented, are less likely to be employed in licensed occupations.

Probably the most important single determinate of wages is human capital. Cortes (2004) finds that foreign born populations from refugee sending countries tend to invest less in human capital during their time in the United States than do immigrants from other countries. This is likely caused by refuggees' discounting the returns on these investments on account of a higher likelihood of return to their home country. The same may be said for undocumented immigrants, who, fearing deportation, do not find it profitable to undertake costly investments in formal education, language skills, or other forms of human capital that may pay a very low return should they have to return to their home country. This is also documented by Lozano (2010), Dustmann (1993) and Dustmann (2003).

1.2 Estimates on Magnitude of Effect

Below we give an overview of previous works that study the magnitude of changes in legal status on labor market outcomes. These include papers using variation from IRCA, focusing both on the effects on those directly treated by the program as well as second order effects, and papers using other sources of variation in legal status. We summarize the principle findings of these papers in Figure 1.

Papers studying the effect of IRCA on the labor market outcomes of those who benefited from the program include Cobb-Clark and Kossoudji (1995), Rivera-Batiz (1999), Kossoudji and Cobb-Clark (2002), Amuedo-Dorantes, Bansak and Raphael (2007), Hill, Lofstrom and Hayes (2010), Pastor, Scoggins, Tran and Ortiz (2010), Barcellos (2010) and Pan (2010). These studies all find positive effects of IRCA on earnings. Hill et al. (2010), using the Legalized Population Survey, finds the smallest of the effects and very little evidence of occupational mobility, at least in the short run.

Papers examining the indirect effects of IRCA include Phillips and Massey (1999), Orrenius and Zavodny (2004), and Hotchkiss and Quispe-Agnoli (2008), all of which estimate the effect of IRCA on workers who may be substitutes in the labor market for those treated under IRCA, and find small or no effects. A paper by Bean, Leach, Brown, Bachmeier and Hipp (2011) measures the effect of parents' legal status on educational outcomes of second generation children by exploiting the sharp design of IRCA.

Other studies on the effects of legal status include Bratsberg, Jr. and Nasir (2002), studying individuals naturalized for a number of different reasons, Kaushal (2006), studying the effect of the Hurricane Mitch response of the Nicaraguan Adjustment and Central American Relief Act (NACARA), Gass-Kandilov (2007), studying the effect of the movement from H1B visas to green cards for high-skilled workers, Chi and Drewianka (2010), studying the effect of changes in legal status through marriage, and Orrenius, Zavodny and Kerr (2011), studying amnesty granted to Chinese students in the wake of Tiananmen Square.

The key difference between our work and the above studies is the choice of a counterfactual group and the source of variation used to identify the causal effect of legal status on the labor market outcomes of undocumented workers. Nearly all previous work compares changes in outcomes of those receiving legal status to changes in outcomes for those *already having legal status*. A more natural counterfactual group would be those *who never have legal status*.

Another contribution of our paper is to exploit the 1982 cutoff in IRCA eligibility. To the best of our knowledge, only Bean et al. (2011), Pan (2010) and Barcellos (2010) do this. Bean et al. (2011)'s research question relates to the outcomes of the second generation and thus is not comparable. Both Barcellos (2010) and Pan (2010) make important contributions to the literature with their approach. Barcellos (2010)'s use of an RDD design is novel and is likely the cleanest use of the cutoff date. However, both of these papers do not make an explicit distinction between the documented and the undocumented. Thus, a share of their treatment group did not benefit from the treatment, as they were already documented. We believe this is the most likely cause of the smaller estimates that are obtained in both of these papers. Indeed, when we estimate a similar model here, we also find smaller numbers. In summary, we believe that our use of the 1982 cutoff data for IRCA eligibility, combined with our variable measuring the likelihood of being undocumented, provide the most credible estimates to date of the effect of legal status obtained through IRCA on labor market outcomes.

2 Identification Strategy and Data

In attempting to identify the impact of legal status on earnings, we face two major challenges in the data. First, there is no large data set that reports labor market outcomes and documentation status for both documented and undocumented immigrants. The Legalized Population Survey (LPS) provides data only for those who received legal status under IRCA, but not for those who did not.

The U.S. Census provides a large sample size, detailed, high quality data on labor market outcomes, and information on year of arrival in the United States. Changes in the population between 1990 and 2000 that cannot be explained by natural population growth or authorized immigration suggest that the Census captures a large share of the undocumented.

Were documentation status known in the Census, one could estimate a simple difference approach comparing the wages of the undocumented eligible for the program to wages of the undocumented ineligible for the program (where eligibility is determined by migration to the United States prior to 1982). To control for changes in the macro economy as a whole during this time period, one could estimate a difference-in-difference model that compared this change to a similar change for the documented. Finally, if one were worried that the characteristics of the undocumented changed (relative to the documented) during this period, one could estimate a difference-in-difference-in-difference model that controlled for changes in wages of the undocumented relative to the documented around some future cutoff year, for example 1992.

However, there is no question in the Census that asks about documentation status. This data is available in the Mexican Migration Project (MMP). Summary statistics for our samples from both of these datasets are reported in Table 1.

2.1 Estimating the Effect of IRCA with Mexican Migration Project Data

The Mexican Migration Project (MMP) is significantly smaller than the U.S. Census data, with the count of Mexican migrants differing by orders of magnitude in the two surveys. The MMP is a random sample of households in migrant-sending communities in Mexico. As the survey is at the household level, information is available on immigrants currently residing in the United States as long as some of their family members remain in Mexico. This is a situation that is quite common in Mexican migration, which typically involves sending a prime-aged male to the United States to work and send remittances back home.

The MMP is a retrospective survey, and thus recall error brings into question the quality of the labor market data. The MMP also collects detailed information on legal status and demographics, both of which are likely less subject to recall error than specific quantitative measures of labor market outcomes. The availability of these variables allows us to conduct a preliminary and very straight-forward approach to measure the effect of legal status: seeing how wages change as an individual's legal status changes.

$$\Delta ln(w_i) = \Delta doc_i\alpha + \Delta exp_i\alpha + \xi_{iMMP} \tag{1}$$

This fixed effects estimation serves to eliminate any time invariant unobserved heterogeneity that may affect both legal status and labor market outcomes. We also control for the number of years that have passed between the two observations to take account of changes in experience. Of course, the questionable quality of the wage data does imply that these results should be taken with a grain of salt.

2.2 Forecasting Legal Status with MMP Data

As discussed above, we generally regard the U.S. Census data as more appropriate for our study; its only major shortcoming is the lack of information on documentation status. We thus turn to an estimation approach that combines information from both of the surveys. Our primary use of the MMP data is to estimate the relationship between legal status and variables that are observed in both the MMP and the Census. This allows us to create an out of sample "forecast" of the likelihood that an individual in the Census arrived in the U.S. as an undocumented immigrant. This is similar to the methodology used by Arellano and Meghir (1992) who measure job search and labor supply using information from two different U.K. surveys.

We incorporate the information provided by the MMP by first running a logistic regression on an indicator that a migrant reported being undocumented in their first migration. We select a sample of individuals in the MMP who reported their first migration to the United States taking place between 1980 and 1984, inclusively. These sample dates reflect the policy enacted by IRCA, which we discuss at more length below. We further limit our sample to males in non-agricultural occupations who were between age 16 and 44 at the time of their first migration to the U.S. Individuals below age 16 are likely tied migrants moving to the U.S with their families and not economic migrants. As individuals over age 45 are a very small share of these migrants, we drop them from the sample to avoid our logistic results being driven by a small number of outliers. We also drop observations with missing values for the education and marriage variables. Individuals are considered "undocumented" if they report that as their documentation status on their first migration to the U.S.

The left hand side variable in our model is an indicator for an immigrant self-reporting being undocumented during their first migration. On the right hand side, we include age at first migration, its square, years of education, its square, indicators for marital status (divorced or widowed being the omitted categories), and all pairwise interactions of the variables. The education and marital status variables are measured at the time of the survey in both the Census and the MMP. There is simply no way for us to know the values of these variables at the time of migration to the U.S. and we believe that even ex-post values of these variables can tell us something about the *type* of migrant that we are dealing with, i.e. were they more or less likely to have initially migrated to the U.S. without documents [2]. Where or not these variables do indeed have predictive power is an empirical question that is addressed by the first stage results from our estimation. The latent variable in the logistic regression can then be represented as follows:

$$doc_i = x_{iMMP}\gamma + u_{iMMP} \tag{2}$$

After obtaining the Maximum Likelihood values of γ, we can then forecast out of sample the likelihood that an individual i is undocumented, provided

[2]In the MMP there is a variable that measures marital status at the time of first migration to the U.S.; however, a vast majority of our observations have a missing value for this variable.

that we have information on demographic characteristics x of this individual. The above described x characteristics are collected for both the MMP and Census. Turning to the Census data, we assign the following likelihood that an observed individual is undocumented. We forecast this likelihood based on the estimates above for undocumented migrants from the early 1980s, as even in the 2000 census we are trying to net out unobserved changes in these types of workers in order to control for things that may have been happening to them before and after 1982 relative to documented workers.

$$\hat{doc_i} = \frac{exp(\hat{\gamma} x_{iCensus})}{1 + exp(\hat{\gamma} x_{iCensus})} \tag{3}$$

2.3 Estimation with Census Data and Forecasted Documentation Status

Using this predicted documentation variable, one might be tempted to run an OLS regression of earnings on this variable to infer the labor market returns to legal status. However, this ignores another data issue in the Census: missing information on characteristics such as motivation, attachment to networks, and ability. These unobserved characteristics are likely to be correlated both with documentation status and (directly) with labor market outcomes. This calls for an approach that can control for unobserved individual heterogeneity.

It also seems appropriate in an OLS regression to control for observable characteristics that should affect labor market outcomes, such as years of education and age at arrival in the United States. If all of these variables are used in the first stage as well, separately identifying the direct effect of these variables and the effect of these variables through $\hat{doc}$ would rely solely on non-linearity. Economic identification would require an exclusion restriction: a demographic variable that affects $\hat{doc}$ but has no direct impact on labor market outcomes.

Luckily, IRCA provides just such variation. IRCA allowed individuals present in the U.S. since before January 1, 1982, to apply for permanent residency. In the 1990 Census, we only know year intervals of arrival to the United States. Luckily, one interval is the years 1980 and 1981, and another is 1982, 1983, and 1984. As there was no way to anticipate the enactment and timing of this policy in 1981/1982, one would expect that immigrants just before the end of 1981 and just after the beginning of 1982 to be very similar. We provide some evidence of this both in our summary statistics and in a set of figures in the next section.

The sharp design of IRCA and some measure of documentation status provide us with three possible difference-in-difference approaches.

2.3.1 DD1: Time Based Diff-in-Diff

One possible identification strategy is to compare the difference in outcomes for individuals in our sample who entered in 1980-1981 to the outcomes of individuals who entered in 1982-1984. Of course, this simple difference approach would confound differences in eligibility with differences in years of labor market experience in the U.S. This motivates a difference-in-difference approach where the above difference is compared to a similar difference, calculated using 2000 census data, for individuals who arrived in 1990-1991 versus those who arrived in 1992-1994. This approach would lump together the undocumented, who benefited from IRCA, and the documented, who did not directly benefit from IRCA. Thus one would expect that it may underestimate the true effect of the program. The estimating equation for this model is given below,

$$y_i = \beta_0^{DD1} + \beta_1^{DD1} 1(t_i = 90) + \beta_2^{DD1} 1(a_i < 2) + \beta_3^{DD1} 1(a_i < 2, t_i = 90) + \epsilon_{it}^1 \quad (4)$$

where y_{it} is a labor market outcome, $1(t = 90)$ is a binary variable indicating that the observation is from the 1990 census, $1(a < 2)$ is an indicator of year of migration being 1980, 1981, 1990, or 1991, and the β terms are parameters. The effect of IRCA is then identified off the interaction of the observation being from the 1990 census and for an individual who migrated "before the 2 year" (1980 or 1981).

2.3.2 DD2: Documentation Status Based Diff-in-Diff

A second possible identification strategy is to compare the difference in outcomes for individuals who are likely documented to those who are not, using only data from the 1990 census. This strategy more precisely models the effect of the program, but may be problematic if characteristics of the undocumented changed differently than characteristics of the documented between the years 1980-1981 and 1982-1984. The estimating equation for this model is given below,

$$y_{it} = \beta_0^{DD2} + \beta_1^{DD2} \hat{doc}_i + \beta_2^{DD2} 1(a_i < 2) + \beta_3^{DD2} 1(a_i < 2) \hat{doc}_i + \epsilon_{it}^2 \quad (5)$$

where $\hat{doc}_i$ is the fitted probability that individual i was undocumented on their arrival in the U.S. The effect of IRCA is then identified off the interaction of the fitted probability of the likelihood of being undocumented and the indicator that individual i migrated "before the 2 year" (1980 or 1981).

2.3.3 DDD: Tripple Difference Estimation

To address the concerns about each of the potential difference-in-difference estimators raised above, we run a triple difference estimator, described by the equation below.

$$
\begin{aligned}
y_{it} &= \beta_0^{DDD} + \beta_1^{DDD}\hat{doc}_i + \beta_2^{DDD}1(a_i < 2) + \beta_3^{DDD}1(t_i = 90) \qquad (6) \\
&+ \beta_4^{DDD}1(a_i < 2)\hat{doc}_i + \beta_5^{DDD}1(t_i = 90)\hat{doc}_i + \beta_6^{DDD}1(t_i = 90)1(a < 2) \\
&+ \beta_6^{DDD}1(t_i = 90)1(a < 2)\hat{doc}_i + \epsilon_{it}^2
\end{aligned}
$$

The identification of the effect of IRCA now comes off of the triple interaction between an observation being from the 1990 census, the likelihood of being undocumented, and the observation corresponding to an individual who migrated "before the 2 year" (1980 or 1981). Our key identifying assumption is that the characteristics, both observed and unobserved, of the undocumented did not change before or after the "2 year" at a different rate than they changed for the documented, in 1990 as compared to 2000. This identifying assumption is essentially untestable, as the only measure that we have of the likelihood of an individual being undocumented must include certain demographic characteristics that we must assume are not changing in the dimension above. However, we do provide some supporting evidence that our observations are essentially ”comparable”, the way that we need them to be in order to obtain a consistent estimate of the treatment effect.

2.3.4 Placebo Estimates

To further explore the validity of our design, we run a number of placebo estimates. Our first set of estimates will look for evidence that IRCA changed labor market earnings for Mexican migrants, as described above, in such a way as to bias us towards finding non-results when we estimate the effect of IRCA on labor market outcomes. To address this concern, we choose a number of outcome variables that we do not believe will be affected by IRCA and test for the presence of any estimated effect. We report the results of these estimations in our next section.

Another possible cause of false positive results would be a host country specific (positive) change that affects all immigrants, not only the undocumented. This would bias us towards finding positive results. Put another way, the triple difference that is designed to estimate the effect of IRCA should only affect immigrants from countries that are directly affected by IRCA.

To search for bias towards finding results for IRCA, we run our model on a total of 48 countries, and examine two things. First, we plot the empirical distribution of T-statistics obtained from the triple interaction term in the main regression. A skewed empirical distribution suggests a lower power of our estimate for Mexican migrants than a standard T-distribution would predict. Second, we present a scatter plot of the estimated T-statistics for each country in our placebo sample plotted against an estimate of the importance of IRCA to that community. Overall, we should expect more evidence of a significant effect of IRCA for individuals from countries that benefited greatly from IRCA, i.e. countries that had a large population of undocumented immigrants living in the United States in the early 1990s.

2.3.5 Obtaining Consistent Estimates of Standard Errors

Because our main regression model contains a variable constructed based on our first stage estimated parameters, the typical calculation of standard errors will not consistently estimate their true value. To address this problem, all such estimates are bootstrapped with 5000 replications. Our variance covariance matrix also accounts for robust standard errors and clusters at the state level.

3 Results

3.1 Results from MMP Data

Table 3 presents estimates of a fixed effects regression from the Mexican Migration Project data. In this estimation, we use all observations also included in our logistic regression on legal status that have complete labor market information for both their first and last migration to the U.S. (the only two migrations on which MMP collects information on labor market outcomes). Here, we use wages rather than annual earnings, as that is all that is available in the survey.

Our results are positive and significant at the 1% level. They indicate that there is approximately a 21 log point effect of legal status on earnings. This is after controlling for time invariant individual heterogeneity and changes in experience between the two observations.

Results from our first stage logit are presented in 2. The model has significant explanatory power, as we can reject a Wald-test of the joint insignificance of the parameters with greater than 99.9% confidence. However, the overall explanatory power of the model is relatively low, with a pseudo R-squared of around .10.

3.2 Results from Combined Census and MMP Data

The results of our main estimation are shown graphically in Figure 2 and Figure 3. Full results are presented in Table 4.

The first figure shows eight plots related to annual earnings. The left panel represents data from the 1990 census, while the right panel represents data from the 2000 census.

The non-linear series represent density plots of the estimated probability of being undocumented for both the group of migrants that moved to the U.S. in 1980-1981 (1990-1991), as well as the group that moved from 1982 through 1984 (1992 through 1994). The strong overlap between these two plots suggests that the observable characteristics of these two groups are indeed very similar: if they were in aggregate different from one another, we would expect to find different predictions of the likelihood that members of the two groups were undocumented according to the estimates obtained from our logit on MMP data.

We now turn our attention to the estimated treatment effect of IRCA represented by the two figures. It is important not to confuse our approach with a propensity score matching approach: we are not comparing differences between treatment and control group observations with a similar likelihood of treatment. In our case, *two* conditions must be met in order to receive treatment. First, an observation must come from the pre 1982 group. Second, the observation must represent an individual who is undocumented. Thus, we will not take the average in the differences in the outcome variable at each point in this plot. Rather, we compare changes in earnings for the two groups as we increase the likelihood that an individual is undocumented.

The steeper of the two lines in the left panel tells us that the labor market penalty for those who likely arrived as undocumented immigrants after 1982 was greater than those who entered beforehand. The difference in these two slope coefficients (in our regressions this is implicitly weighted by the densities at each point) is essentially a difference-in-difference estimate. In the right panel, we see dramatically less difference in the wage penalties between these two groups than we saw in the 1990 data. This is comforting, as no major policy change occurred around 1992. Differencing out this difference from the 1990 figure provides us with our triple difference estimator.

Both Figure 2 and Figure 3 show us that the penalty to being undocumented (i.e. how negative the slope of the line is) is higher for those who came just after 1982 compared to those who came before 1982, but no higher for those who came just after 1992 compared to those who came just before 1992. This is more evidence of an effect of IRCA on labor market outcomes, both on total earnings, as well as on the earnings potential in the occupation in which the immigrant worked [3].

The estimates from our triple difference model given in Equation 6 are presented in Table 4. The effects of a difference-in-difference estimation that does not use any information about likely legal status find very small esti-

[3]Our occupational earnings variable is computed by taking a mean of occupational earnings in the Census for an equivalent sample of natives.

mates of the effect of IRCA (Diff-in-Diff Time), on the order of 2 log points for both earnings and mean occupational earnings. Neither result is statistically significant.

As with the graphical evidence, there seems to be a positive effect of IRCA on both overall earnings and occupational earnings for our other two models. The 1990-only difference-in-difference (Diff-in-Diff Legal Status), as well as the triple difference, both show evidence of a much larger effect of 15 log points on overall earnings and 16 log points on occupational earnings for the diff-in-diff, and 20 and 19 log points for the triple difference, respectively. The estimated effects on overall earnings are not significant for the difference-in-difference model (p-value=.149), and marginally significant for the triple difference model (p-value=.105). Given that these are based on *estimated* legal status, that the first stage model had a pseudo R-squared of only around .10, and that there is generally a lot of unexplained variation in our very simple earnings model, relatively imprecise results shouldn't be surprising.

Our estimates of mean earnings at the occupational level are estimated much more precisely, also not surprising given how much less unexplained variation there tends to be in this outcome variable as compared to individual earnings (given that mean wages at the occupational level eliminates the effect of individual specific heterogeneity on earnings). Here, we find that our estimates in the difference-in-difference model are significant at the 1% level, and our estimates are significant at the 5% level [4].

[4]To provide a weak test of the validity of our identifying assumption, we also attempted

The large effects on occupational wages are suggestive that occupational mobility is one of the key benefits provided by legal status. However, it does not rule out differential monopsony power over undocumented immigrants, if *between occupation* differential rates of exploitation exist. In Figure 4 we present a locus of elasticities of labor supply to the firm for the documented and undocumented that would rationalize the 20 log point difference that we estimate. The Hotchkiss and Quispe-Agnoli (2009) estimates of these elasticities are close to being on this line.

to identify only off of the interaction variables in the first stage. In other words, we included in the second stage education, marital status, and age of arrival in the U.S., allowing these variables to directly affect the labor market outcome of the immigrant. Results from the tripple-difference estimation were around 6 log points smaller on both outcome variables, but still larger than many of the previous results in the literature at around 14 log points. The precision of these estimates was very similar.

3.3 Group Placebos

If our identifying assumption is valid, we would expect to only find estimates in Table 4 that are driven by IRCA. As most undocumented immigrants affected by IRCA were from Mexico and Central America, we should observe that estimates of change on income through IRCA of the triple differences for immigrants from other countries are close to zero [5].

In Table 5 and Table 6 we estimate our model for native born Hispanics and all immigrants who were not born in Mexico. We select a subsample of these workers with the same number of years of U.S. (total, in their case) labor market experience as the Mexican born in our earlier sample. Results are significant for only 2 of the 18 regressions, never significant and positive, and never significant in our triple difference. We did not expect to find significant results for either of these groups, especially U.S. born Hispanics; results that did find a treatment effect of IRCA for these untreated groups would call our design into serious question.

In Figures 5 and 6, we present the distribution of T-statistics obtained from estimating 48 triple difference estimators. The estimates for "incwage" are on annual earnings, and "occwage" are on the mean occupational earnings in the occupation in which the immigrant is employed. These figures were obtained from running a separate regression for each country with a migrant

[5] The above estimates rely on our estimates from the logistic regression on MMP data and also have explanatory power in predicting the likelihood that migrants from other countries are undocumented. To the extent that undocumented immigrants from all countries tend to arrive in the U.S. at younger ages and have lower levels of education, there is likely some predictive power of our estimates.

community numbering over 20,000 in both 1990 and 2000. We see that this distribution approximates the Students' T distribution for the appropriate degrees of freedom. In other words, there is no evidence of a contamination effect that would bias us towards finding positive results on all migrants. This indicates that the significant result for Mexican migrants was either a result of chance (standard Type 1 error), or that there is indeed a positive effect for Mexican migrants, more so than other groups.

Of course, if immigrants from some other countries than just Mexico were likely to be affected by IRCA, we would expect to find more significant results than random chance would predict (seen by the small amount of increased mass in the right tail of our empirical T-distributions). In Figures 7 and 8, we illustrate this point by presenting a scatter diagram of T-statistics obtained in the country-wise triple difference estimates, and an estimate of the percent of that country's immigrant population that was legalized under IRCA [6]. We see there there is some positive correlation between percent affected by the treatment and estimated significance of the treatment effect. This relationship is significant in a weighted regression, but not robust to the exclusion of Mexican migrants.

[6] This number was calculated through by comparing the total number of individuals from each country appearing in the 1990 census to the estimated number of people from each country obtaining legal status under IRCA according to the person weights in the Legalized Population Survey.

3.4 Outcome Placebos

In our main estimations, we also rely upon an assumption that the composition of documented and undocumented Mexican immigrants has not changed across time (before or after "2" years, in 2000 compared to 1990). To address this possibility we run a series of regressions whose outcomes we believe should not be affected by IRCA, but will be affected if immigrant characteristics (observed or unobserved) have changed. They are reported in Table 7 and the outcomes reflect characteristics of an immigrant dwelling and family. The results show that only one of the outcomes have is affected in the triple difference, and only two outcomes are changed in the difference-in-difference estimate that incorporates legal status.

4 Conclusion

In this paper we have presented estimates of the effect of IRCA on the labor market outcomes of immigrants. We have done this taking advantage of the policy design of IRCA and using other Mexican immigrants as a control group. We first directly estimate the effect of changes in legal status on changes in earnings with a fixed effects estimate using MMP data, finding a 21 log point effect of obtaining legal status. Next, we employ an identification strategy that is based on estimating undocumented status for Mexican immigrants in the Mexican Migration Project, and then recovering these estimates to estimate the probability that an observation in the Census is undocumented. Our results show that IRCA is associated with a marginally significant 20 log point increase in labor market earnings of Mexican immigrants, very similar to the estimates obtained with only MMP data. Estimates of the effect of IRCA on mean occupational wages are much more precisely estimated, statistically significant, and at 19 log points extremely close in magnitude to the total estimated effect of IRCA. As not all undocumented workers potentially eligible for IRCA received legal status under the program, conceivably the true effect of the program is somewhat higher than our estimates. Importantly, our estimates from the MMP data and Census are similar in magnitude.

These results are based on two identifying assumptions. First, we assume that our estimates are due to IRCA and not to a secular change in the labor market returns of all immigrants in the United States. Importantly, we find that estimates for immigrants from other countries, who did not benefit from IRCA to the extent that Mexican immigrants did, produce no systematic bias towards positive and significant results. Second, our results are not driven by changes in the observed or unobserved characteristics of undocumented immigrants across time. Similar estimates for outcomes that are not likely to be determined by IRCA find that our identification strategy fails to produce economically and statistically significant estimates as in the case of labor income.

In summary, we have obtained results that provide credible evidence that the effect of legal status gained through IRCA was larger than previously estimated. We believe that this results from our use of other undocumented workers as part of our counterfactual group, rather than a group such as native born Latinos. Additionally, our results suggest that nearly all of the effect of legal status on earnings comes through access to better paying occupations. Both of these findings should help inform policy makers about both the magnitude and the mechanism of changes in legal status on labor market outcomes.

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Table 1: Summary Statistics from MMP and Census by Year of Entry to US

	MMP	Census 1979-1981	Census 1982-1985	Census 1989 1991	Census 1992 1995
Undocumented	0.81	0.78	0.76	0.74	0.74
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Year in US	1982.03	1981.00	1984.00	1990.36	1993.08
	(0.04)	(0.00)	(0.00)	(0.00)	(0.01)
Age in US	22.57	23.05	22.66	22.73	22.88
	(0.17)	(0.08)	(0.08)	(0.06)	(0.06)
Age	35.08	32.05	28.66	32.37	29.80
	(0.23)	(0.08)	(0.08)	(0.06)	(0.06)
Years of Education	6.89	7.48	8.16	8.81	8.73
	(0.10)	(0.05)	(0.05)	(0.04)	(0.03)
Married	0.83	0.71	0.59	0.69	0.62
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Log Annual Income		9.32	9.22	9.73	9.65
		(0.01)	(0.01)	(0.01)	(0.01)
Log Occupation Income		9.70	9.66	10.09	10.07
		(0.01)	(0.01)	(0.00)	(0.00)
N	1266	6436	5953	9248	12232

Table 2: Logit on Legas Status: Migrants in MMP 1981-1985

Variable	**Coefficient**	**(Std. Err.)**
Age at First Migration to U.S.	-1.593	(1.146)
Age at First Migration to U.S. Squared	0.030	(0.021)
Years of Education	0.571	(1.546)
Years of Education Squared	-0.031	(0.083)
Indicator for Single	-32.458*	(17.006)
Indicator for Married	-26.583	(16.535)
Age/Education Interaction	-0.111	(0.088)
Age Squared/Education Interaction	0.002	(0.002)
Age/Education Squared Interaction	0.006	(0.005)
Age Squared/Education Squared Interaction	0.000	(0.000)
Age/Married Interaction	1.987*	(1.096)
Age/Single Interaction	2.512**	(1.154)
Age Squared/Married Interaction	-0.037**	(0.020)
Age Squared/Single Interaction	-0.050**	(0.021)
Education/Married Interaction	0.763	(0.983)
Education/Single Interaction	0.982	(1.020)
Education Squared/Married Interaction	-0.055	(0.050)
Education Squared/Single Interaction	-0.069	(0.052)
Intercept	24.102	(17.165)

N	1266
Log-likelihood	-554.642
$\chi^2_{(18)}$	106.546

* denotes signficance at 10% level, ** at 5% level, and *** at 1% level

Table 3: Fixed Effects Estimates from MMP: Migrants in MMP 1981-1985

Variable	Coefficient	(Std. Err.)
Change in Documentation Status	0.208***	(0.073)
Change in Years	0.017***	(0.006)
Intercept	0.112***	(0.033)
N	431	
R^2	0.071	
$F_{(2,428)}$	16.329	
* denotes signficance at 10% level, ** at 5% level, and *** at 1% level		

Table 4: Mexican Born Immigrants

	Diff-In-Diff (Time)	Diff-In-Diff (Legal Status)	Triple-Diff
Incwage Beta	0.02	0.15	0.20
	(0.02)	(0.10)	(0.13)
Occwage Beta	0.02**	0.16***	0.19**
	(0.01)	(0.06)	(0.08)
N	33860	12388	33860

Robust Standard Errors Clustered At State Level,
Procedure Bootstrapped with 5000 Reps
* denotes signficance at 10% level, ** at 5% level, and *** at 1% level

Table 5: Other Immigrants

	Diff-In-Diff (Time)	Diff-In-Diff (Legal Status)	Triple-Diff
Incwage Beta	0.02	0.05	0.02
	(0.02)	(0.06)	(0.09)
Occwage Beta	-0.03*	0.04	0.01
	(0.01)	(0.04)	(0.06)
N	48430	22044	48430

Robust Standard Errors Clustered At State Level,
Procedure Bootstrapped with 5000 Reps

Table 6: Native Born Hispanics

	Diff-In-Diff (Time)	Diff-In-Diff (Legal Status)	Triple-Diff
Incwage Beta	-0.02	-0.12*	-0.02
	(0.02)	(0.06)	(0.08)
Occwage Beta	-0.02	0.02	0.05
	(0.01)	(0.03)	(0.04)
N	45672	19170	45672

Robust Standard Errors Clustered At State Level,
Procedure Bootstrapped with 5000 Reps
* denotes signficance at 10% level, ** at 5% level, and *** at 1% level

Table 7: Placebo Outcomes for Mexican Migrants

	DD Time	DD Yhat	DDD
Old Children	0.41*	2.16*	0.46
	(0.22)	(1.31)	(1.65)
Young Children	0.11	1.16	0.72
	(0.17)	(0.98)	(1.32)
Transit Time	0.01	-0.05	-0.00
	(0.02)	(0.09)	(0.12)
Depart Time	-17.41*	-39.26	-4.28
	(10.27)	(48.15)	(63.08)
Home Ownership	-0.01	-0.07*	-0.06
	(0.01)	(0.04)	(0.05)
Cost Gas	0.09**	-0.11	-0.16
	(0.04)	(0.21)	(0.26)
Cost Water	-0.06	-0.55**	-0.63**
	(0.04)	(0.20)	(0.25)
Cost Electricity	0.08***	-0.15	-0.23
	(0.03)	(0.15)	(0.18)
Cost Fuel	0.02	0.01	-0.05
	(0.02)	(0.08)	(0.10)
Phone	0.02**	0.08	0.07
	(0.01)	(0.05)	(0.05)
Vehicle	0.12**	-0.28	-0.18
	(0.06)	(0.32)	(0.40)

* denotes signficance at 10% level, ** at 5% level, and *** at 1% level

Figure 1: Previous Estimates

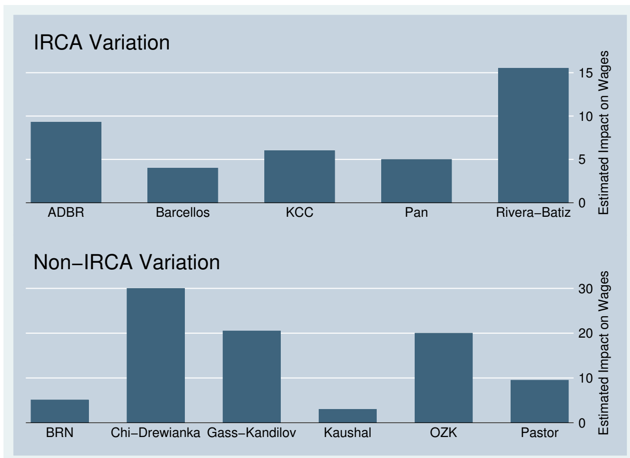

Figure 2: Earnings Differences and Distribution of Likelihoods

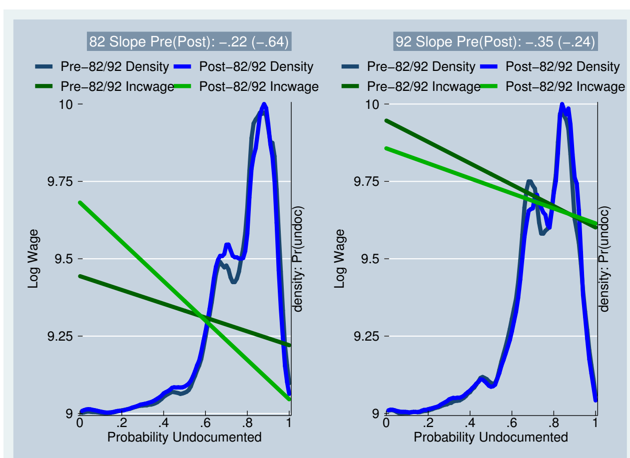

Figure 3: Occupational Earnings Differences and Distribution of Likelihoods

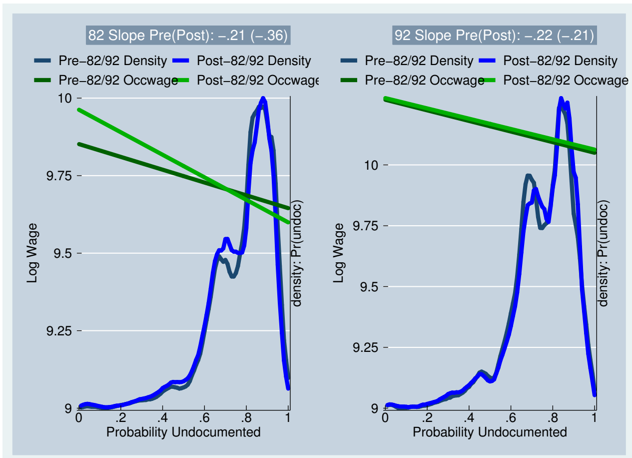

Figure 4: Elasticities of Labor Supply Rationalizing our Results

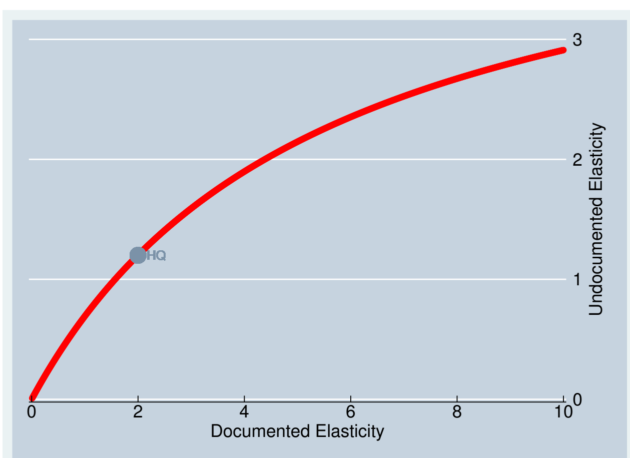

Figure 5: Empirical Density of Ts (For Earnings)

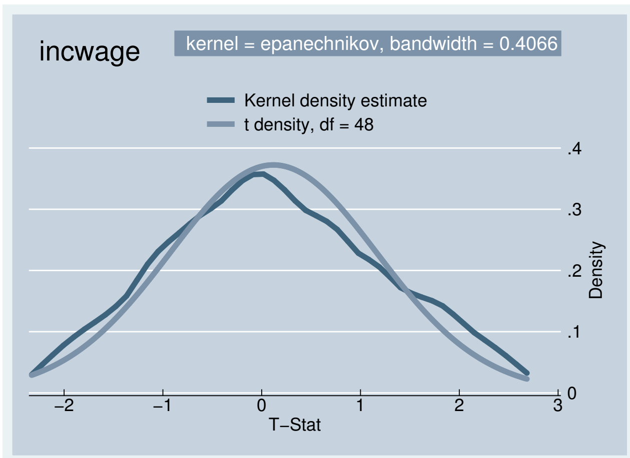

Figure 6: Empirical Density of Ts (For Occupational Earnings)

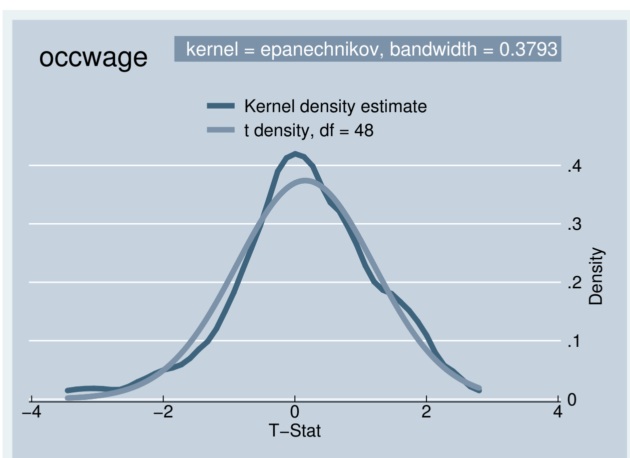

Figure 7: Relationship between T-statistic (For Earnings) and Share Legalized under IRCA

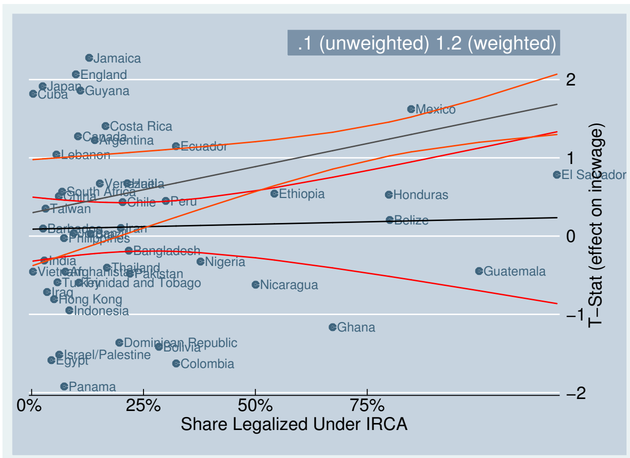

Figure 8: Relationship between T-statistic (For Occupational Earnings) and Share Legalized under IRCA