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Authorizing the Unauthorized: Labor Market Consequences for Crop Farm Workers

1. Introduction and Policy Context

Hired farm work has historically been among the lowest paid occupations in the U.S., both due to relatively low hourly wage rates (averaging $9.30 per hour in FY 2012 for crop farm workers) and to the fact that most hired crop farm workers do not work a full year (the average was 33 weeks in 2012, up from 23-27 in the 1990s).[2] These low earnings are reflected in very high poverty rates in farm worker communities. Martin and Taylor (1998) report that seven of the twenty U.S. cities with the highest percentage of immigrants living in high-poverty census tracts (defined to include tracts with poverty rates above 40 percent) are located in California's San Joaquin Valley, which as a region had higher farm sales than any other *state* in the U.S. The town of Parlier, CA, in the heart of the San Joaquin, had a median family income of just $24,000 in 2000, and a poverty rate of 36 percent.[3] More than 90 percent of California's farm workers were born in Mexico, and roughly two-thirds of them, and half of all hired crop farmworkers in the country, are not legally authorized to live and work in the U.S.[2]

Advocates have long recognized two avenues to improving the lives of immigrant farm workers: one is to raise wages and hours of work in agriculture, and to improve working conditions, and the other is to help farm workers make the transition to better paid and more stable employment in other industries. One of the most important policy initiatives in pursuit of the former goal was the California Agricultural Labor Relations Act (ALRA) of 1975, which then-Governor Jerry Brown called his finest legislative achievement. A recent conference brought together many of the people who for the past 40 years have administered the Agricultural Labor Relations Board (ALRB), and litigated cases before it under provisions of the Act that defined the legal environment for labor union organizing on California's farms. The conference report reaches

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[2] Based on author's tabulations from the National Agricultural Workers' Survey (NAWS).

[3] http://en.wikipedia.org/wiki/Parlier,_California, reporting results from the 2000 Census.

the conclusion that the ALRA had many salutary effects on labor rights in agriculture, and generated a prodigious body of litigation, but did not succeed in bringing about the widespread direct representation of workers by unions. By one count the number of United Farm Workers (UFW) contracts peaked at 108 in 1978, and had fallen to 30 a decade later (Martin 2015). Between 2000 and 2012, fewer than one percent of California's crop farm workers report being covered by a union contract.[2] Union activity has thus had little contractual impact on farm wages in California, although the UFW has been more successful through legislative channels, being the driving force behind the passage of a bill that raised the state minimum wage from $8 to $9 as of July, 2014, and will raise it to $10 as of January, 2016, in addition to important legislation protecting farm workers from heat stress and pesticide exposure.

A second (national) legislative intervention came in 1986 with the passage of the Immigration Reform and Control Act (IRCA). The bill strengthened border enforcement and introduced penalties for employers who hired unauthorized workers, but also granted legal permanent resident status to some 2.7 million formerly unauthorized residents, including 1.1 million who were legalized via the Special Agricultural Worker (SAW) program (Rytina 2002). Advocates hoped that the granting of legal status and the removal of the threat of deportation would improve farm workers' bargaining power *vis a vis* farm employers, and also improve their sectoral mobility, permitting them to find better paid employment. Last minute lobbying by farm employers, who were concerned that this would result in an exodus of labor from agriculture, led to the addition of provisions that would have brought in large numbers of Replenishment Agricultural Workers (RAW) in the event that exits by newly-authorized workers led to farm labor shortages. In the end, neither the border control efforts nor the employer sanctions proved effective in stemming the inflow of unauthorized labor to agriculture, farm wages remained flat, and the RAW provisions were never invoked (Commission on Agricultural Workers, 1993).

The question of whether IRCA's legalizations accelerated the transition out of agriculture was extensively debated and researched, but not entirely resolved. The Commission on Agricultural Workers' final report states: "Different data sources provide conflicting answers to this question. A large proportion of SAWs who were identified as working within agriculture after obtaining legal status appear to continue to work in seasonal agricultural services." (Commission on Agricultural Workers, 1993, p. 53). Nonetheless, there is some empirical evidence, discussed below, that authorization did increase the rate of exit from farm employment, and one review of the IRCA-era literature concludes that the newly legalized (counting all occupations, not just farm workers) experienced wage gains of 10-15 percent within five years, primarily because the acquisition of legal status brought about greater occupational mobility, as anticipated (Martin 2014).

The current farm labor environment has been shaped by the slowdown of immigration from Mexico, due to demographic change and economic growth in that country, enhanced enforcement both at the border and internally, and the effects of the recession of 2007-09. Passel, Cohn, and Gonzalez-Barrera (2012) estimate that net migration from Mexico fell to zero, and may even have been negative, in 2012. This appears to have contributed to localized farm labor shortages (Hertz and Zahniser, 2013), a problem that anecdotal evidence suggests has since grown in scope. The fact that visa certifications for temporary non-immigrant farm workers requested by growers under the H-2A program have grown from roughly 50,000 positions in 2005-2006 to more than 100,000 in 2013-2014, despite the costs associated with this program, also suggests that farm labor markets are indeed tightening.[4]

The most recent attempt at comprehensive immigration reform passed the Senate in June of 2013 (S. 744), but was never taken up by the full Congress. Past USDA research has sought to quantify the macroeconomic impacts of various immigration reform scenarios (Zahniser *et al*, 2012), and two new USDA-funded research projects are underway that may shed light on the likely effects of various reforms that are currently being debated, using computable general equilibrium modeling.[5] Pending implementation of comprehensive reform, however, the most salient policy initiative is the President's November 2014 executive action that would expand the number of people eligible to obtain work authorization and administrative relief from the threat of deportation under the Deferred Action for Childhood Arrivals (DACA) program, and create a new program, known as Deferred Action for Parents of Americans and Lawful Permanent Residents (DAPA), that would extend similar rights to immigrants whose children were born in the U.S., and are thus citizens, or who have otherwise acquired lawful permanent resident status. It has been estimated that 45-50 percent of undocumented farm workers in the U.S. would be eligible for administrative relief under DAPA / DACA (Werner-Kohnstamm Family Fund, 2014; Martin, 2014). The program is currently suspended by virtue of a court injunction; oral arguments concerning the lifting of that injunction were heard by the 5th Circuit Court of Appeals on April 17th, and a decision is pending at the time of this writing.

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[4] Compiled from annual reports, and disclosure data from the Office of Foreign Labor Certification of the US Department of Labor; figures for 2014 are preliminary estimates. http://www.foreignlaborcert.doleta.gov/performancedata.cfm.

[5] These include a grant from the National Institute for Agriculture to Jeff Luckstead, a researcher at the University of Arkansas, and an agreement between the USDA's Office of the Chief Economist and Peter Dixon and Maureen Rimmer of Victoria University, in Melbourne, Australia.

2. Objectives, Outcomes & Methods

The aim of this paper is to estimate the effects of a change in legal status on wages, days of employment, farm earnings, the probability of nonfarm employment, and total farm and nonfarm earnings of currently unauthorized farm workers. The analysis draws on data from the National Agricultural Workers Survey (NAWS), which has surveyed a representative cross-section of crop farm workers annually since 1989, collecting data on wages, hours of work, and legal immigration status, along with many relevant covariates.[6] The study of wage differentials provides some information on the wage gains that might be experienced by newly authorized farmworkers who choose to remain in agriculture, but the effects of legal status on the number of days worked are equally important in determining the effect on overall farm earnings. The NAWS data also allow us to observe the incidence of simultaneous nonfarm employment among farm workers, and to test whether this is higher or lower for those with more secure legal status. While this does not directly measure the degree to which legal status facilitates the exit from farm work, it does provide a conditional measure of the ease with which members of the various legal status groups can, or choose to, obtain nonfarm employment. Last, I use the NAWS data to revisit the post-IRCA experience in an attempt to quantify the effects of legal status on the rate at which farm workers transition out of agriculture.

In these analyses we think of the unauthorized workforce as the "untreated" group, and seek to estimate the treatment effects of acquiring legal permanent residency (LPR, or green card status), or full citizenship via naturalization. We assume that the study of contemporaneous cross-sectional differences in labor market outcomes by legal status can tell us something about the first-order short-run benefits that would accrue to the unauthorized were they to receive work authorization, and thus also about the magnitude of the *ex ante* upward pressure on growers' labor costs. In fact, the relationship between these cross-sectional differences and the probable longitudinal effects of a hypothetical legalization program is quite complex, but studies that compare cross-sectional and longitudinal estimates in the wake of past legalization programs suggest they are often of the same order of magnitude. It should also be stressed that our estimates can only speak to the potential wage gains (from farm and nonfarm employment) for those workers

[6] *The National Agricultural Workers Survey*, Employment and Training Administration, U.S. Department of Labor, http://www.doleta.gov/agworker/naws.cfm. There is no comparable survey for livestock workers. It is often argued that the lack of legal immigration status will be under-reported in surveys, but the fact that 96 percent of Mexican-born workers with no more than one year of experience in U.S. agriculture who were interviewed on California vegetable farms reported being unauthorized suggests that the NAWS does a good job of eliciting this information.

that remain in agriculture to some extent; as such, they should be seen as a lower bound on the gains that might be achieved by workers who leave agriculture altogether for other sectors, a process that would likely be accelerated by their being granted legal work status and protection from deportation, as will be argued below.

The primary methodological challenge we face is to account for the fact that legal status is strongly correlated with many observable covariates that have clear effects on labor market outcomes. For example, the unauthorized are generally younger (with a mean age of 29 in our sample, versus 44 and 38 for green card holders and naturalized workers, see Appendix Table A), putting them very close to the age at which farm wages peak, all else being equal (about 30). On the other hand, the unauthorized are much less experienced (5 years versus 15 and 20) and have lower levels of English language proficiency, which depress their wages in relation to LPRs and naturalized citizens. Past research has consistently demonstrated, and we shall soon confirm, that the disadvantages associated with their lower levels of experience and English language capability systematically outweigh the advantages of youth, and can explain much of the observed wage differential between authorized and unauthorized workers. But the estimation of the share of the observed wage gap that can be explained by differences in worker and job characteristics versus the residual share that can be attributed to differences in legal status is a subtle matter, and methodological choices made by the researcher can have large effects on the results. An additional methodological challenge is the possibility that authorized and unauthorized workers may differ in *unobservable* ways; this problem, which is usually addressed by selection-bias-correction modeling, or by instrumental variables approaches, is discussed below and in my concluding comments.

The simplest way to calculate the treatment effect of legal status, controlling for differences in relevant covariates, is to estimate a model of log wages as a function of the chosen covariates using linear regression, pooling data from all survey years in order to obtain a large sample, and pooling workers of various legal statuses, using indicator variables to distinguish between status groups. The coefficients on these indicators then estimate the *ceteris paribus* effect of legal status. This approach, however, will obscure the fact that both the raw and the regression-adjusted wage gaps between legal status groups appear to have grown over time.[7] There are various ways the specification may be made less restrictive, to permit it to capture this time trend. One would be to

[7] This relation between the size of the legal status premium and the year of observation casts some doubt on the instrumental variables approach adopted by Alves Pena (2010). In an attempt to address the possibility that unauthorized immigrants differ in unobservable ways from their authorized counterparts, Alves Pena uses changes in the legal environment over time (captured by year-of-entry dummy variables) as instruments for legal status. But these instruments will be invalid if the legal status premium itself varies over time in a way that is strongly correlated with year of entry.

estimate separate models for various time periods; another is to continue to pool the data across years but to include interaction terms between the legal status indicators and indicators for various time periods. This latter approach is adopted here, because it has the advantage of permitting the estimation of the effects of age, experience, and year of arrival into the U.S. to be based on all observations for these variables, rather than the subsets that are found in each subperiod. In other words, we make the identifying assumption that age, experience, and cohort effects are constant over time, which results in much more efficient estimates of the effects of these important and highly collinear confounding variables. However, this efficiency comes at the cost of ignoring the possibility that the age and experience profiles may themselves have varied over time.

Another important finding from past research is that legal status differences in wages are larger for more skilled workers, both in cross-section and in longitudinal data. We thus relax our specification further to include an interaction between legal status and years of farm work experience, which consistently appears as the single most important source of wage differences between legal status groups. The resulting wage equation may then be written as follows:

[1] $$\ln(wage) = (S*P)\delta + (S*E)\beta + X\gamma + u\ ,$$

where *wage* is the real hourly wage; (*S*P*) includes the main effects of the two legal status categories (*S* = green-card holders or naturalized citizens, with the unauthorized being the reference category), the main effects of a set of indicators for the 8 three-year periods that span the sample (*P* = seven indicators for FY1992-95 through FY2010-12, with FY1989-91 being the reference category), and the interactions between these variables; (*S*E*) contains a cubic polynomial in years of experience in U.S. agriculture (*E*), along with interactions between legal status and the experience terms, while *X* includes various combinations of other covariates, described below, and *u* a well-behaved error term such that E(u|S,P,E,X)=0.

As soon as we permit the effect of legal status to vary across individuals, here by year of observation and level of experience, we must specify the manner in which we are estimating the marginal effects that we report. In the present context, the natural way to estimate the effect of a program that confers work authorization or other legal protections is to estimate the average effect of treatment (acquisition of legal status) on the presently untreated (the unauthorized). This is the statistic we report for all regression analyses. Because the treatment effect is smaller at lower levels of experience, we expect the average treatment effect across unauthorized (less experienced) workers to be lower than the corresponding average across authorized workers (this latter being the effect of “treatment on the treated.”)

Still less restrictive specifications may be achieved by interacting legal status with other covariates. In the limit, we may generated separate estimates for each legal status category, which permits all coefficients (those governing the age, cohort, and education profiles, in addition to the marginal effects of all other control variables) to vary by group, as in Isé and Perloff (1995) or Iwai, Emerson, and Walters (2006). Marginal effects are then estimated by comparing the predicted wages from each equation for various types of workers. This approach was considered but rejected for the current analysis because it led to an implausible degree of heterogeneity in treatment effects. In particular, these effects were estimated to be negative and statistically significant for large subsets of the unauthorized population, an outcome that was deemed implausible.[8]

In our basic model (Model 1), the covariates contained in *X* include a quartic polynomial in age; a cubic polynomial in years of education interacted with an indicator that flags those whose terminal year of schooling was completed in a foreign country (thereby permitting different returns to foreign versus U.S. schooling); and two measures of how well the worker speaks and writes English. These measures range from 1 = "Not at all" to 4 = "Well" and are entered as continuous variables to force their estimated impacts to be monotonic. The immigration literature has also emphasized that immigrant cohorts differ in their unobservable skill levels. We introduce a cubic polynomial in the year of entry into the U.S. to capture broad trends in the unobserved qualities of successive cohorts. Note that, in principle, the effects of year of observation, year of entry, years of U.S. farm experience, and age are all separately identifiable since none is a simple linear combination of the others. Together these variables form the basic controls for workers' potential labor market productivity, which are unlikely to change in the immediate aftermath of an authorization program. In the longer run, the granting of legal status may result in increased investments in education; any resulting gains in wages are not captured by our model.

In Model 2, we add a set of additional demographic controls, namely, indicators for gender, marital status. whether the worker is a parent, the number of children present in the household, four indicators for ethnicity, five indicators for race, and three indicators for country of birth. These should convey additional information about the workers' productivity and their degree of attachment to the labor force; they may also capture effects of employer discrimination by gender, race or ethnicity. These factors are also considered fixed in the short run, but are likely to change in

[8] This identifying assumption needs further justification. Alvarado, Riley and Mason (1996) report that some workers who gained legal status via IRCA moved to farm jobs that were less strenuous than their prior piece-rate tasks, but paid lower equivalent hourly wages. An improvement in legal status might also result in less effort on the job due to a reduced effective cost of job loss, lowering hourly equivalent pay for piece rate workers. We investigate this latter possibility in an alternative specification that restricts our sample only to workers paid on a strictly hourly wage basis, and find no qualitative difference in results.

the medium term. In particular, past legalizations have been shown to have been associated with increases in family size and a reduction in women's labor force participation rates (Kraly, Seltzer, and Powers, 2000; Amuedo-Dorantes, Bansak, and Raphael; 2007).

Finally, in Model 3 we add controls for six regions, six tasks, five crops, whether the worker was directly hired or employed through a farm labor contractor, whether the worker was paid by the hour, by the piece, by a combination of the two, or was salaried, the number of weeks of farm employment in the past year, and the number of weeks out of the year that the worker was out of the country. These variables measure job characteristics and provide further information about the degree of attachment to the U.S. workforce. If legal status permits greater mobility across regions, crops, and occupations, then we would expect to see our estimates of the effects of legal status diminish when we add these additional control variables, i.e. that legal status gaps between workers in the same areas, crops, and tasks would be smaller than gaps that are measured without taking these factors into account. In addition, the conferring of work authorization might change international migration patterns, altering weeks of labor supplied to agriculture; including these as control variables means that the effects of these behavioral responses to differences in legal status are not captured in the estimates for Model 3.

In the analysis of hourly wages, results are weighted by hours worked as is customary in the labor economics literature; this means that results apply not to the average worker, but to the average hour worked. In the linear regression analyses of days of work, farm earnings, total earnings from farm and nonfarm employment, and in the probit model of the probability of nonfarm employment, the variables measuring weeks of farm work and weeks outside the country are omitted, and person weights rather than hours weights are used. Heteroskedasticity-robust standard errors are reported for all equations. Design effects due to the multistage clustered sample design are not taken into account due to the lack of cluster identifiers in the public use dataset.[9]

The final analysis, of the rate of exit from agriculture, does not use regression methods. Instead, I employ a cohort analysis to attempt to determine if the workers authorized under the SAW program left agriculture any faster than their still-unauthorized counterparts. Although we lack panel data that tracks SAWs across industries, we do have access to many more years of data from the NAWS than were available to the Commission in 1992. These data confirm that unauthorized farm employment expanded steadily in the years following IRCA, as employment by

[9] The magnitude of these design effects can be estimated; this is work in progress.

both U.S. born and LPRs, including those obtaining LPR status under IRCA, fell (see Fig. 1).[10] Employment by SAWs fell most sharply between 1989 and 1999; thereafter the rate of decline slowed. Employment by the smaller groups of workers authorized via the pre-1982 arrival provisions of IRCA (hereafter, the Legalized Agricultural Workers, or LAWs), and by those authorized through non-IRCA channels, has been approximately constant since 1989. In the analysis that follows I group the LAWs and SAWs together, and designate them as IRCA-authorized workers (IRCAAWs).[11]

These data confirm that large numbers of IRCAAWs left agriculture, but the challenge is to determine how much of the observed decline in their employment is due to their having gained legal residency status, and how much instead reflects the expected decline in participation in agriculture by any fixed group of people as they grow older. To answer this question I first divide the IRCAAW sample into 5-year birth cohorts; the youngest of these cohorts was born between 1970 and 1974 (and so were 25 to 29 years old in 1989); the oldest includes all those born in 1954 or before. I then calculate the percentage change in IRCAAW employment relative to the 1989 base year, by birth cohort, after first smoothing the annual data as described below. The key next step is the construction of a counterfactual employment series for each cohort. This represents the level of employment by this fixed group of people that would have been observed had they not been legalized. Comparing the year-to-year rates of change in the actual and the counterfactual employment series for each birth cohort, and then summing across cohorts, yields an estimate of the effect of legalization.

The cohort-specific employment estimates display large fluctuations from year to year which reduce our confidence in the measurements of changes over time (see Figure 5, and note the suspiciously low employment totals for 1993). To address this, I first smoothed the annual data for each cohort by means of a regression of total employment for that cohort against a quartic

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[10] Employment was calculated by summing the survey sampling weights provided in the NAWS, which are adjusted for comparability over time, for sample design, and for nonresponse. I then scaled these estimates so that they match the trend in crop farm employment reported in the Farm Labor Survey (FLS). The FLS tracks employment by nonsupervisory directly hired workers in crop farming, and by agricultural service workers in both crops and livestock. I used data from the Quarterly Census of Employment and Wages (QCEW) on the share of agricultural service workers who are in crops versus livestock to assign a portion of the FLS's service workers to crop farming. Note that the FLS is the primary source of the sampling frame for NAWS. Note also that the NAWS weights in their unadjusted form are intended to track a slightly different measure of FLS crop farm employment. While the difficulty of measuring total employment in crop farming is well known, this modification of the weights has little impact on my results, because the same weights are used in estimating the overall trend in employment and the counterfactual that this trend is compared to.

[11] The reason for the difference in trends among the two types of IRCA legalizees is not clear, nor is it clear which of the two groups' behavior is the best predictor for the behavior of a future cohort of workers who are granted legal status by any of the mechanisms that have recently been proposed. Combining them seemed the safest strategy.

polynomial time trend. The fitted values from these regressions are used in place of the actual values in the analysis that follows. Similar regressions were used to smooth the data for the counterfactual employment series, whose construction we now describe.

To build a counterfactual employment series, we need to identify a control group whose employment trends (by birth cohort) could plausibly be said to represent the trends that would have been observed for the IRCAAWs had they remained unauthorized. The most obvious candidate for a control group is the workers who did in fact remain unauthorized, since prior to IRCA the IRCAAWs were themselves members of this group. The problem with this approach, however, is that this putative control group was also affected by the treatment: unauthorized employment increased in part *because* employment declined among the IRCA-authorized. If indeed legalization accelerated the departure of the IRCAAWs, then it should have contributed to the increase in unauthorized employment. This would inflate the difference between treatments and controls, leading us to overstate the effects of legalization.[12]

To solve this problem, I define the control group to include *both* the unauthorized and the IRCAAWs themselves. The assumption is that what legalization did was to create a differentiation between members of this group, causing some people to supply less labor to agriculture and others to supply correspondingly more; had the IRCAAW subset not been legalized, they would have continued to follow the overall average employment trends, by birth cohort, of the overall group. The identifying assumption is thus that the decrease in employment that resulted from the legalization of the IRCAAWs was matched by an increase in employment of the unauthorized. This implies that total employment for this combined group was not affected by the legalization of some of its members, but was instead determined by the normal process of transitioning out of farm work, the pace of which was also influenced by the rising demand for their services as U.S. born workers sharply decreased in number.

One final adjustment is made to render the counterfactual employment series more plausible, namely, I limit the unauthorized workers in the control group to those who, like the IRCAAWs, first entered the United States prior to 1989. This constraint prevents our counterfactual IRCAAW employment series from being inflated by new arrivals (who are concentrated in, but not

[12] Donato, Durand, and Massey (1992, p. 102) make the exact opposite argument, namely, that "Consistent with the reports of our informants, undocumented migrants appear to have been driven out of agriculture and replaced by Special Agricultural Workers." Their statistical evidence for this claim is that, following IRCA, SAWs had higher probabilities of employment in agriculture than did both other authorized workers and the still-undocumented, whereas prior to IRCA there was little difference in farm employment probabilities by legal status. But this seems a flawed argument, since SAW status was highly correlated with being a farm worker in the first place, even given the large number of fraudulent applications. Thus the SAW indicator is endogenous in an equation predicting farm employment.

exclusively located in, the younger birth cohorts), since the IRCAAWs themselves cannot be new (post-1988) arrivals.[13] Our identifying assumption thus becomes somewhat more restrictive: we now assume that the decrease in employment due to legalization of the IRCAAWs was matched by an increase in employment by unauthorized workers *who had also arrived prior to 1989*. While these assumptions cannot be tested with our data, I would argue they are plausible. They comport with the basic fact that unauthorized workers replaced legalized workers, whom they closely resemble and for whom they are close economic substitutes. Furthermore, by limiting the analysis to unauthorized workers who first arrived before 1989 we select more experienced workers: in 1989, the median number of years of prior work in U.S. agriculture was four for our unauthorized workers and five for IRCAAWs.

Note that the decline in total employment by the control group of pre-1989 arrivals may or may not have been accelerated by IRCA's various provisions: in particular, it remains a matter of some debate as to whether IRCA itself led to an increase in unauthorized immigration, which would have heightened competition for farm employment.[14] But the resolution of this debate is not critical to our estimates: if heightened competition from new entrants worked to increase exits from agriculture by previous entry cohorts, that effect is presumed to have been felt by both IRCAAWs and those who remained unauthorized.

3. Results

Figure 1 plots the share of employment by workers of each legal status category. The number of green-card holders (including the SAWs and LAWs authorized by IRCA but also others who obtained legal permanent residency through other means, primarily via family reunification) fell rapidly between 1989 and 1994, and has declined more slowly since. This decline, and the decline in the employment of US citizens, was made up for by an increase in the number of unauthorized workers, whose share has hovered around 50 percent for the last decade. The share of naturalized immigrants (including those born in Puerto Rico) has been fairly steady. The rise in the number of unauthorized workers may have contributed to depressing their wages in relation to their authorized counterparts, as argued by Donato and Massey (1993).

[13] Note that we are not assuming that either the unauthorized or the IRCAAWs remained in the U.S. continuously since they first entered: the pools of potential farm workers in each subgroup include those who have left the country, since they could re-enter.
[14] See Orrenius and Zavodny 2003.

Effect of Legal Status on Wages, Days, Farm and Nonfarm Earnings

Figure 2 plots the real hourly wages, at 2014 prices, for unauthorized farm workers in relation to each category of authorized worker, and to all authorized workers, prior to any adjustment for covariates. This authorized/unauthorized gap has averaged about $1.50 per hour since 2001, up from about 50 cents in the prior decade. The growth in the gap has been driven primarily by rising wages for U.S. born and naturalized immigrant workers. In Fig. 3, the observed (unadjusted) wage premia associated with green card status and citizenship are plotted in 3-year increments. Naturalized citizens enjoyed a premium of about 17 percent in FY 1989-2000 (see Table 1), rising to 23 percent in FY 2001-12. The green card premium rose from 8 to 10 percent over this interval.[15]

After adjusting for covariates, the green card premium falls to 2.3 to 4.1 percent for the most recent decade, depending on the model used (Table 1). Interestingly, the more restrictive Model 3, which controls for crop, task, and region, generates slightly *larger* estimates of the gap, suggesting that the lack of ability to move between crops, tasks and regions is not a reason that the unauthorized earn less. Wage premia for naturalized citizens are larger, at 6.8 to 7.6 percent, again looking at the figures for the second decade, FY 2001-12. Figure 4 plots these wage premia in three-year increments, along with their 95 percent confidence intervals. Note that the wage premia appear to have peaked in 2007-09, and fallen somewhat in the most recent 3-year period, 2010-12.

Table 2 presents several alternative specifications which serve as robustness checks. I first re-estimate Model 3 using quantile (median) regression; I then explore the effects of using person-weights instead of hours weights; I next consider the effects of including those born in Puerto Rico among the pool of naturalized citizens; and finally I restrict the sample to only those workers who are paid hourly. Results are qualitatively similar to the above, with the exception that the inclusion of Puerto Rican workers raises the naturalization premium considerably. This estimate is likely a less reliable guide to the prospects for the largely Mexican and Central and South American-born workforce that might benefit from legalization.

Table 3 summarizes the results of Model 3, applied to wages, days of work, total farm earnings, the probability of nonfarm employment, and total estimated farm and nonfarm earnings, still focusing on the more recent decade. (More detailed results for these outcomes appear in the

[15] These percentage differences are based on differences in means of logs, and will differ from arithmetic percentage differences both because the log approximation uses the logarithmic mean wage as its denominator and because the mean of logs corresponds to the geometric as opposed to the arithmetic mean wage. These subtleties can have a considerable impact on perceived wage gaps; further robustness checks are in preparation to assess the importance of the choice of the log functional form.

Appendix.) In the first panel we see that green-card holders worked 19 percent more days per year, and had 35 percent higher farm earnings, than unauthorized workers, but were no more likely to work off farm. However, once we adjust for covariates, the days-worked effect becomes negative (albeit insignificantly so) thereby eliminating the earnings gap, while the wage rate gap remains significant, but small, as already noted.

Naturalized citizens logged 35 percent more days, and had farm earnings that were 68 percent (in log percentage points) higher than the unauthorized. However, they were slightly less likely to also work off-farm, and hence enjoyed an overall earnings advantage on the order of 58 percent. Once we control for covariates, however (see final panel, Model 3), we see that the days premium has fallen by about 1/3rd, to 22 percent; the farm earnings premium thus falls to 28 percent. Interestingly, however, naturalized workers were, *ceteris paribus*, much less likely to work off farm, and this reduces their total farm plus nonfarm earnings to a level that was statistically indistinguishable from the unauthorized.

Effect of Legal Status on SAWs Exits from Agriculture

Figure 5 reports the weighted employment estimates for the IRCA-authorized workers (SAWs + LAWs), by birth cohort. We again see the fairly rapid decline between 1989 and 1994, and a slower rate of decline since then. Figure 6 displays the smoothed employment estimates, and the results of the cohort-based analysis of IRCAAW employment in the two decades following IRCA. In the base year (FY 1989), SAWs and LAWs together provided 272,000 FTEs of labor. Five years later (FY 1994) their employment had fallen to 146,000 (a decline of 126,000 or 46%). I estimate that roughly 70,000 workers (26%) were lost due to normal attrition of this cohort as they aged, while the remaining 56,000 workers (21%) were estimated to have departed in response to legalization. By about 2001, however, the legalization effect had dwindled to zero. Taken at face value these results suggest that the acquisition of legal status *did* encourage SAWs to leave the fields for other jobs, but that this only affected agricultural labor supply in the short to medium run.

4. Conclusions, Comparisons, and Caveats

Our results for the legal status effect on wages are slightly lower than those of Alves Pena (2010). Her estimates based on selection-bias-correction models were 4 percent for green card holders (our results are between 2 and 4 percent) and 12 percent for naturalized immigrants (compared to our range of 7-12). She also reports IV results, as noted above, which come in at 6 percent for both legal status categories. Finally, she reports results derived from propensity score matching, which were on the order of 7-12 percent, with the higher figure again corresponding to naturalized citizens.

Our results are considerably smaller, however, than other published estimates. Isé and Perloff (1996), for example, report an earnings estimate of 15 percent, driven largely by wage not hours effects. This is comparable to results found in Iwai, Emerson, and Walters (2006), while Walters, Emerson, and Iwai (2008) report results as high as 10-20 percent. The reasons for these differences in results are largely methodological. I have argued that Alves Pena's choice of instruments is problematic if the size of the treatment effect varies over time. Moreover, neither Alves Pena, nor Isé and Perloff, nor Iwai, Emerson, and Walters (2006) report the treatment effect for those who have not been treated, i.e. the unauthorized, which is systematically smaller than the estimated average treatment effect for all farmworkers, or than the effect on those who were in fact treated (the authorized).

Walters, Emerson, and Iwai (2008), however, do report estimates for the untreated only, and they remain much larger than mine. The difference here is their reliance on fully unrestricted equations by legal status (which I have argued yield implausible negative effects of more secure legal status in may cases) and, crucially, on the use of selection-bias-correction models. The risk associated with the use of Heckman-style sample selection methods is that they can have large and counter-intuitive effects when good instruments for the selection process are lacking, as they are in this case. In particular, it is important to ask whether the implied direction of the selection bias is as expected. If bias correction leads to larger estimates of the wage difference between unauthorized and authorized workers, it implies that unauthorized workers must have unobservable characteristics that cause them to earn higher wages, all else equal. In general, this is not what we expect, as the relevant unobservable traits (unmeasured skills, for example) typically covary positively with observable traits (measured experience), and we have seen that correcting for these observed traits reduces the wage gap considerably. One possible explanation for a positive effect of selection bias is that the unauthorized might exert greater effort on the job, as a rational response to their having a worse fall back position, i.e. a less desirable next best option should they lose their job due to poor performance. But if this is the explanation, it raises the question of whether these greater levels of effort would be maintained were the workers to obtain legal work authorization and protection from deportation. It would seem likely that these protections, by improving the workers' fall back position, would result in *lower* effort levels than when they were unauthorized, all else being equal. In other words, unobservable effort might itself be endogenous; if it is, then the not-selection-bias-corrected results would be the better estimate of the treatment effect.

If my lower results are accurate, they imply that that hourly wage gains to newly legalized farm workers who choose to remain in agriculture may be quite modest. Moreover, the wage premium appears to have fallen in the most recent 3-year period (2010-12). As noted, however, these estimates should be seen as lower bounds on the wage gains that might be obtained by those who leave agriculture. The positive effects of legalization on days worked and total earnings are larger, but are limited to fully naturalized citizens. For policy initiatives that do not immediately lead to citizenship, it seems more appropriate to rely on the results for green card holders, who appear to enjoy no significant farm earnings advantage due to their legal status.

The insignificant or even negative effects on the probability of nonfarm employment might appear to indicate that legalization does not increase occupational mobility, but this interpretation is not warranted. These results apply to those who currently working in agriculture, many of whom have likely determined that farm work is their best or preferred option. It is important to understand that while many workers transition through farm work to other sectors of the economy, they leave behind a core of workers who have no intention of working in other occupations. When asked "How long do you expect to continue doing farm work in the U.S.A.?" fully 80 percent of unauthorized workers in 2010-12 responded "Over five years / as long as I am able."

The effects of legalization on the occupational mobility of younger, less experienced workers, with less attachment to agriculture, are captured to an extent by our analysis of the patterns of SAW employment following IRCA. Our estimates may provide a rough guide to how farm labor supply might respond to the legalization of a portion of the currently unauthorized workforce. For example, if one-half of the current workforce is unauthorized, and supposing one-half of these are granted legal status, this might cause farm labor supply to decline by 0.5*0.5*0.21 = 5 percent over 5 years. Crops and regions with higher unauthorized shares might see proportionately larger effects. Actual outcomes, and the earnings these workers attain in other sectors, will also depend on the strength of the nonfarm economy in the years ahead.

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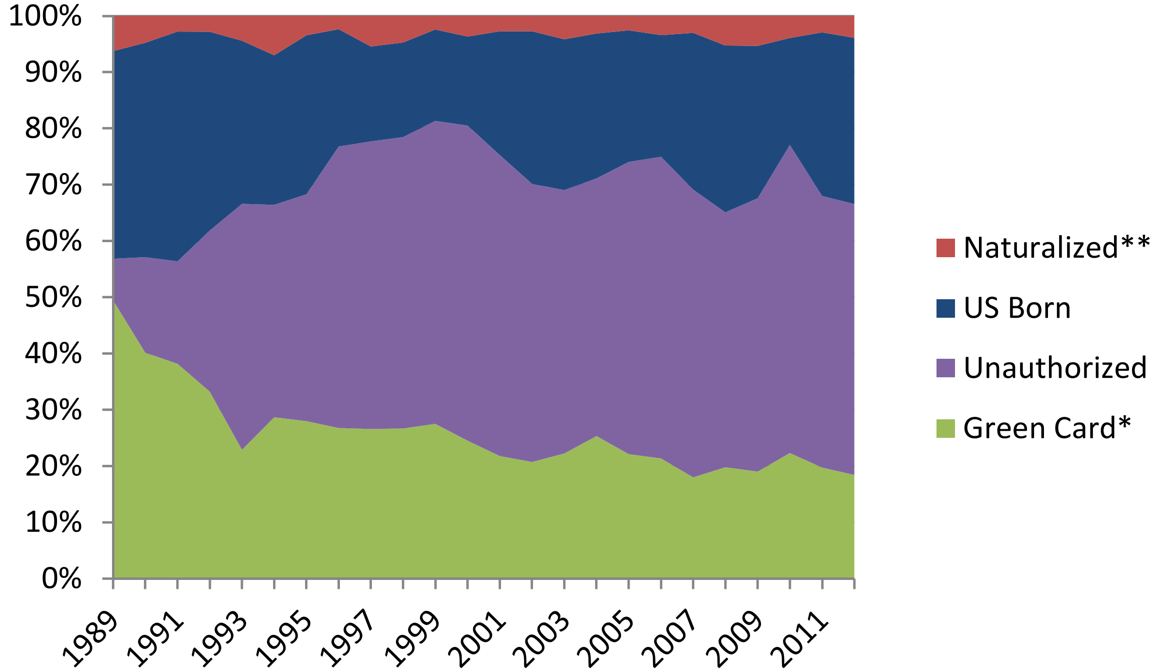
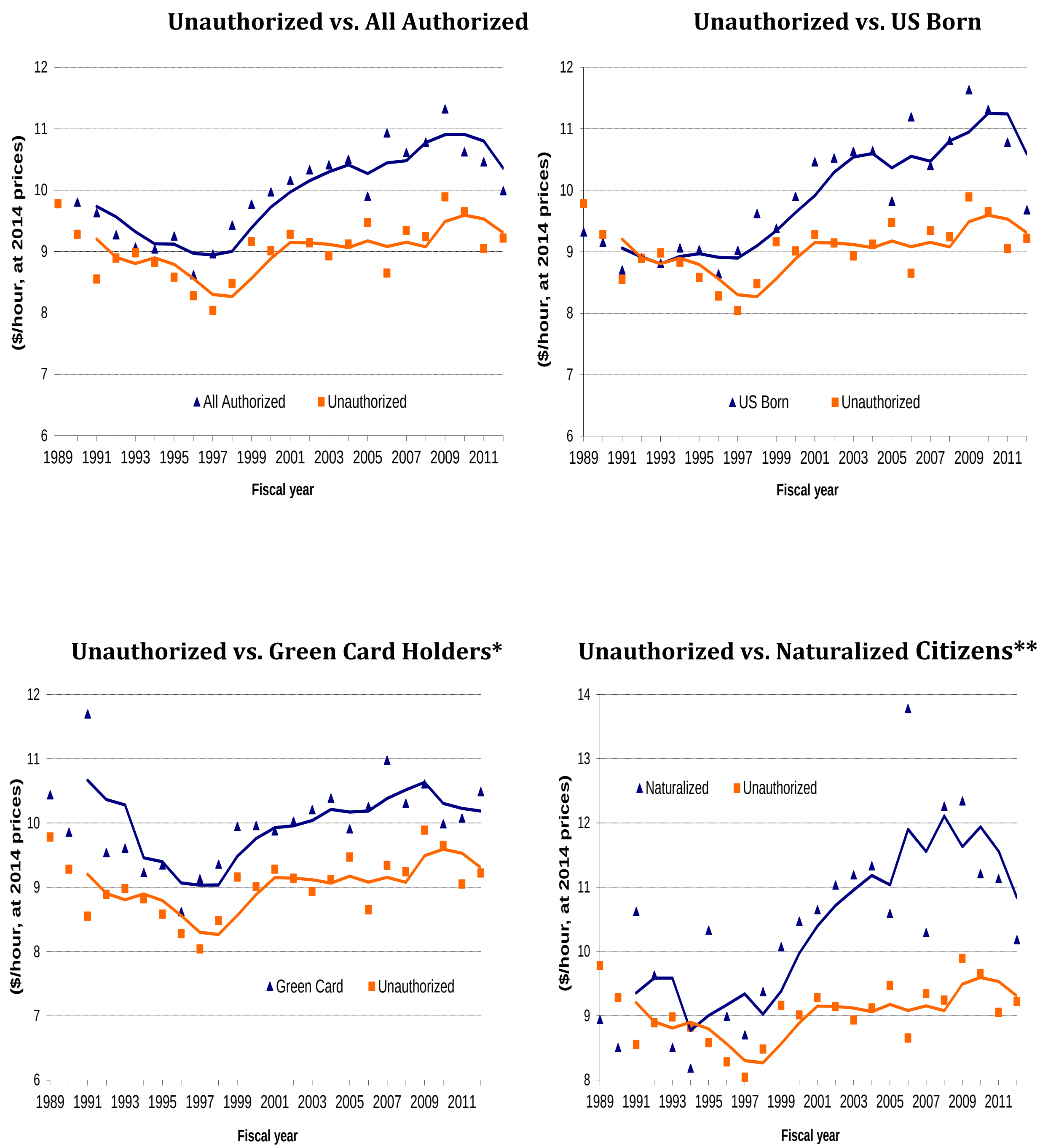
Fig. 1: Legal Immigration Status of Crop Farm Workers, FY1989 to FY2012



Source: Author's analysis of data from NAWS.
Notes: *The category "Green Card" includes the IRCA-authorized SAWs and LAWs, as well as those who obtained legal permanent residency status through programs other than IRCA. **Includes those born in Puerto Rico.

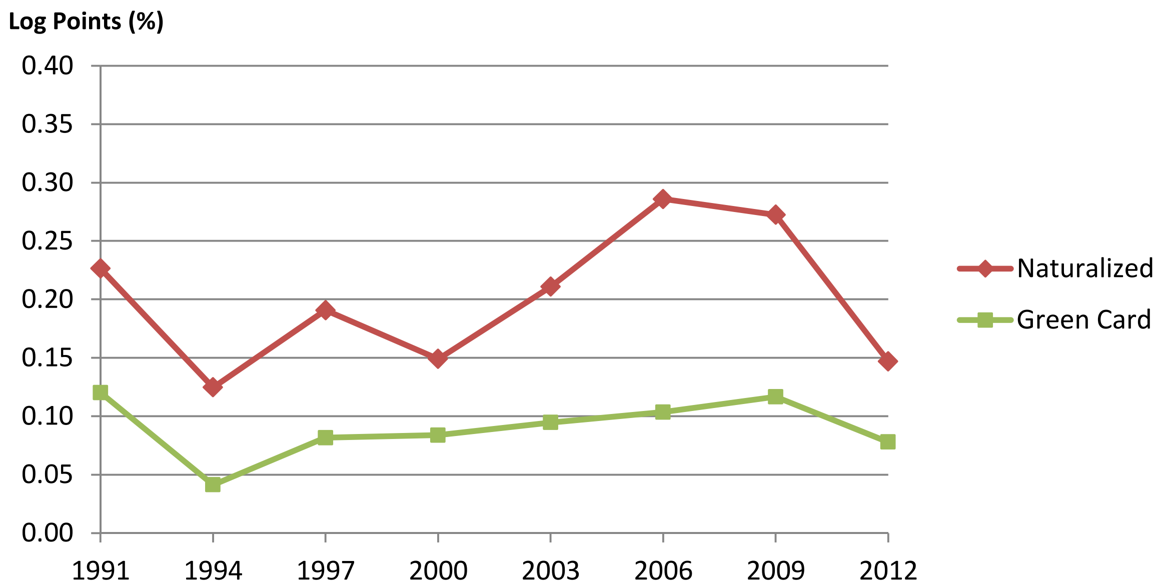
Fig. 2: Real Wages by Legal Status (Three-year Moving Averages)



Source: Author's analysis of data from NAWS.
*Note: Provisionally authorized SAWs, LAWs, and other non-IRCA authorized workers are included among green card holders. **Includes US citizens from Puerto Rico.

Fig. 3: Percent Real Wage Gaps: Green Card / Naturalized vs. Unauthorized

(As observed)

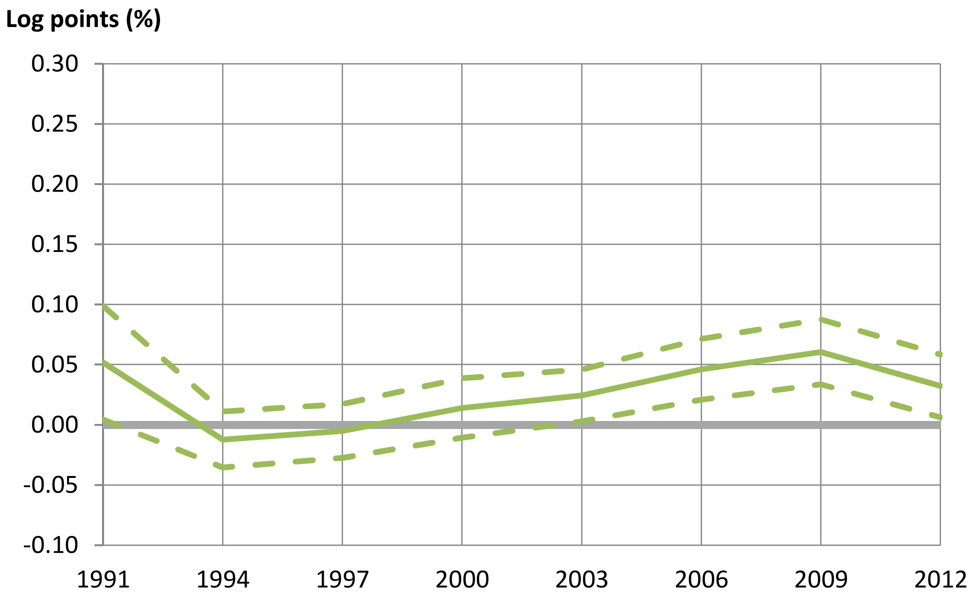


Source: Author's analysis of data from NAWS.
Note: Years on X-axis are averages of the three prior fiscal years, e.g. 1991 = Fiscal years 1989-1991. US citizens born in Puerto Rico are not included among Naturalized in this and subsequent analyses.

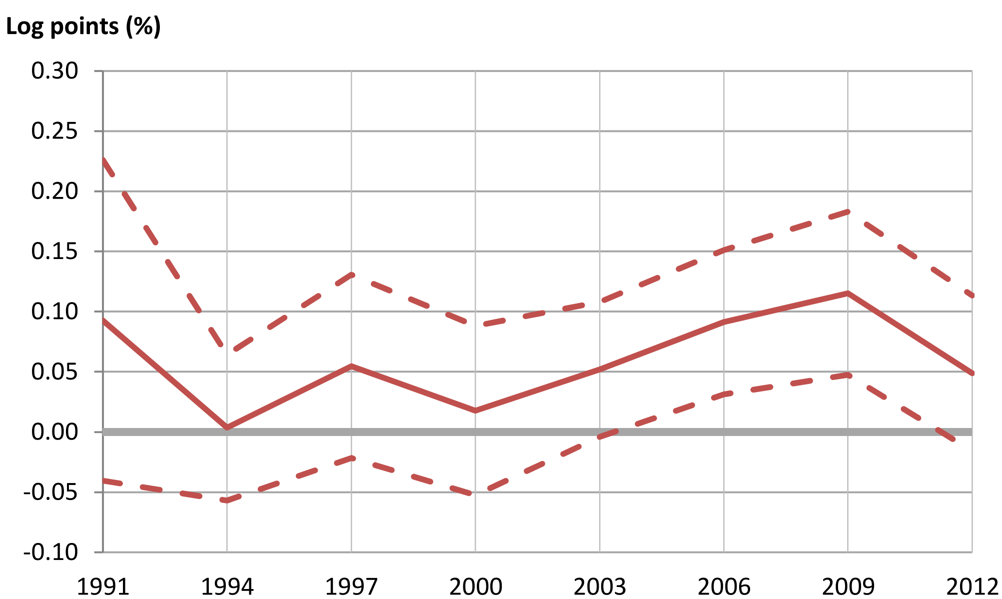
Fig. 4: Percent Real Wage Gaps: Green Card / Naturalized vs. Unauthorized

(Adjusted Estimates, See Model 3, below)

Green Card vs. Unauthorized



Naturalized vs. Unauthorized



Source: Author's analysis of data from NAWS.
Note: Dotted lines represent 95% confidence interval. Years on X-axis are averages of the three prior fiscal years, e.g. 1991 = Fiscal years 1989-1991. US citizens born in Puerto Rico are not included among Naturalized.

Table 1: Regression Models of Effects of Legal Status on Real Hourly Wage Rates

	Green Card vs. Unauthorized				Naturalized vs. Unauthorized			
As observed (N=40219)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.089	0.005	18.41	0.000	0.199	0.013	15.62	0.000
FY 1989-2000	0.079	0.007	12.00	0.000	0.168	0.020	8.41	0.000
FY 2001-2012	$\boxed{0.098}$	0.007	13.97	0.000	$\boxed{0.229}$	0.016	14.32	0.000
Model 1: Adjusted for age, experience, education, language skills, entry cohort, and year of interview (R^2=0.11) (N=40219)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.013	0.009	1.34	0.179	0.044	0.019	2.33	0.020
FY 1989-2000	-0.001	0.011	-0.06	0.948	0.014	0.024	0.59	0.558
FY 2001-2012	$\boxed{0.023}$	0.011	2.17	0.030	$\boxed{0.068}$	0.023	2.92	0.004
Model 2: Add controls for gender, marital status, parental status, number of children in h'hold, race, ethnicity, country of birth (R^2=0.12) (N=40219)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.018	0.009	1.87	0.062	0.051	0.019	2.68	0.007
FY 1989-2000	0.004	0.011	0.38	0.701	0.022	0.024	0.94	0.347
FY 2001-2012	$\boxed{0.028}$	0.011	2.61	0.009	$\boxed{0.073}$	0.023	3.15	0.002
Model 3: Add controls for region, job task, crop, pay mode, contract workers, weeks worked, weeks out of country (R^2=0.32) (N=40219)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.025	0.007	3.34	0.001	0.057	0.020	2.85	0.004
FY 1989-2000	0.005	0.010	0.55	0.585	0.033	0.026	1.28	0.200
FY 2001-2012	$\boxed{0.041}$	0.008	4.93	0.000	$\boxed{0.076}$	0.022	3.48	0.000

Source: Author's analysis of data from NAWS.
Notes: See text for methods. In all cases, increase in legal status effect between 1st and 2nd periods was statistically significant.

Table 2: Alternative Regression Models of Effects of Legal Status on Real Hourly Wage Rates

	Green Card vs. Unauthorized				Naturalized vs. Unauthorized			
Model 3a: Estimated via median regression (N=40219)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.019	0.005	3.78	0.000	0.057	0.015	3.76	0.000
FY 1989-2000	0.009	0.006	1.67	0.094	0.023	0.018	1.28	0.202
FY 2001-2012	$\boxed{0.026}$	0.006	4.05	0.000	$\boxed{0.084}$	0.018	4.77	0.000
Model 3b: Do not weight by hours worked (OLS) (R^2=0.29) (N=40962)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.023	0.010	2.35	0.019	0.043	0.022	1.99	0.047
FY 1989-2000	0.007	0.013	0.58	0.561	0.014	0.027	0.53	0.596
FY 2001-2012	$\boxed{0.036}$	0.009	3.97	0.000	$\boxed{0.069}$	0.023	3.03	0.002
Model 3c: Include those born in PR among naturalized (Weighted OLS) (R^2=0.32) (N=40895)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.026	0.007	3.45	0.001	0.093	0.015	6.37	0.000
FY 1989-2000	0.006	0.010	0.65	0.514	0.057	0.019	3.02	0.003
FY 2001-2012	$\boxed{0.041}$	0.008	5.01	0.000	$\boxed{0.122}$	0.016	7.43	0.000
Model 3d: Limit to workers paid hourly (Weighted OLS, exclude PR) (R^2=0.35) (N=31790)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.015	0.005	2.93	0.003	0.063	0.015	4.07	0.000
FY 1989-2000	-0.002	0.006	-0.41	0.684	0.032	0.020	1.62	0.106
FY 2001-2012	$\boxed{0.027}$	0.007	4.14	0.000	$\boxed{0.084}$	0.017	4.94	0.000

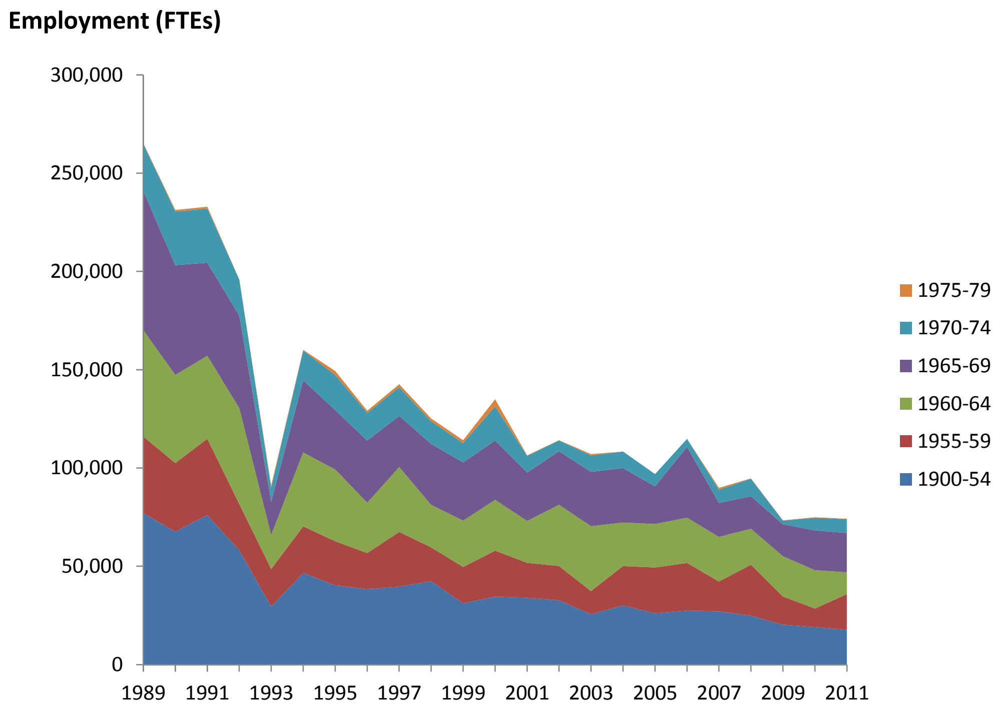
Source: Author's analysis of data from NAWS.
Notes: See text for methods. In all cases, increase in legal status effect between 1st and 2nd periods was statistically significant.

Table 3: Summary of Results: Real Wages, Days Worked, Farm Earnings, Nonfarm Employment, Total Earnings, FY 2001-2012

	Green Card vs. Unauthorized			Naturalized vs. Unauthorized		
	Estimate	P>|t|		Estimate	P>|t|	
As observed						
Hourly Wages (Logs)	0.098	0.00	*	0.229	0.00	*
Days Worked (Logs)	0.189	0.00	*	0.349	0.00	*
Total Farm Earnings (Logs)	0.345	0.00	*	0.676	0.00	*
Nonfarm Employment (Probability)	-0.011	0.43		-0.054	0.01	*
Total Farm+Nonfarm Earnings (Logs)	0.342	0.00	*	0.584	0.00	*
Model 1						
Hourly Wages (Logs)	0.023	0.03	*	0.068	0.00	*
Days Worked (Logs)	-0.058	0.19		0.207	0.02	*
Total Farm Earnings (Logs)	-0.036	0.48		0.246	0.03	*
Nonfarm Employment (Probability)	-0.012	0.52		-0.100	0.00	*
Total Farm+Nonfarm Earnings (Logs)	-0.029	0.53		-0.069	0.51	
Model 2						
Hourly Wages (Logs)	0.028	0.01	*	0.073	0.00	*
Days Worked (Logs)	-0.035	0.44		0.233	0.01	*
Total Farm Earnings (Logs)	0.009	0.87		0.290	0.01	*
Nonfarm Employment (Probability)	-0.003	0.88		-0.091	0.00	*
Total Farm+Nonfarm Earnings (Logs)	0.030	0.51		-0.001	1.00	
Model 3						
Hourly Wages (Logs)	0.041	0.00	*	0.076	0.00	*
Days Worked (Logs)	-0.030	0.51		0.222	0.01	*
Total Farm Earnings (Logs)	0.016	0.76		0.277	0.01	*
Nonfarm Employment (Probability)	-0.006	0.74		-0.091	0.00	*
Total Farm+Nonfarm Earnings (Logs)	0.036	0.41		-0.013	0.90	

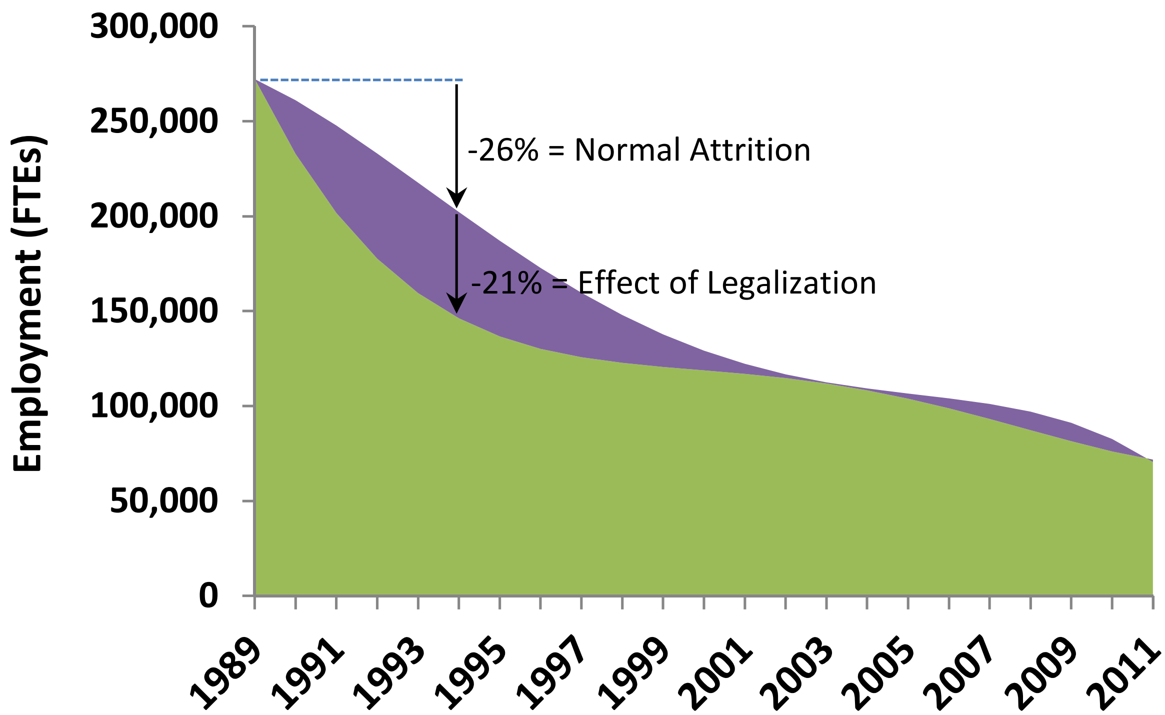
Source: Author's analysis of data from NAWS.
Notes: See text for methods. Note that percentage differences in farm earnings are approximately equal to the sum for real wages and days worked, but differ in that they are estimated in slightly different subsamples due to missing data.

Fig. 5: Employment of IRCA-Authorized Workers, By Birth Cohort, FY1989-2011



Source: Author's analysis of data from NAWS.
Notes: Weights scaled to reflect Farm Labor Survey employment totals for crop agriculture; estimated numbers of employees of farm labor contractors are included. Includes legalized agricultural workers (LAWs), who gained legal status through IRCA's 5-year-residency requirement program (pre-1982 arrivals) and those authorized under the Special Agricultural Workers program (SAWs).

Fig. 6: Estimated Effect of IRCA Legalization on SAWs+LAWs Working in Agriculture



APPENDIX

Table A: Means of Dependent and Independent Variables

	Unauthorized		Green Card		Naturalized	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Real Wage	8.96	2.57	9.83	3.61	11.08	4.42
Days Worked	160	101	187	88	205	89
Real Farm Earnings	11,117	9,094	14,709	10,485	19,684	14,456
Total Real Earnings	12,393	9,204	16,156	10,165	20,692	14,146
Share with Nonfarm Job	0.17	0.38	0.20	0.40	0.17	0.37
Model 1						
Years of farm experience	5.4	5.7	14.8	9.6	20.4	10.9
Age	28.9	10.0	38.4	11.8	44.3	12.7
Years of schooling	6.4	3.3	5.9	3.5	7.2	3.7
Share foreign educated	0.92	0.27	0.86	0.34	0.76	0.43
Speaks English (1-4)	1.5	0.7	1.9	0.9	2.5	1.0
Reads English (1-4)	1.4	0.7	1.7	0.9	2.4	1.0
Year of entry	1995	7.9	1982	9.2	1976	10.7
Added in Model 2						
Share female	0.18	0.38	0.22	0.42	0.27	0.44
Share married	0.51	0.50	0.75	0.43	0.80	0.40
Children in household	0.50	1.11	1.29	1.64	1.40	1.48
Worker is parent	0.47	0.50	0.66	0.47	0.64	0.48
Ethnicity (Ref: Mex.-American)						
Mexican	0.91	0.29	0.89	0.32	0.73	0.44
Other Hispanic	0.06	0.23	0.03	0.18	0.03	0.18
Non-Hispanic	0.01	0.10	0.03	0.16	0.06	0.24
Race (Ref: White)						
Black	0.01	0.09	0.01	0.09	0.01	0.08
Indigenous	0.09	0.29	0.05	0.22	0.03	0.17
Other	0.50	0.50	0.45	0.50	0.49	0.50
Missing	0.02	0.13	0.03	0.18	0.04	0.20

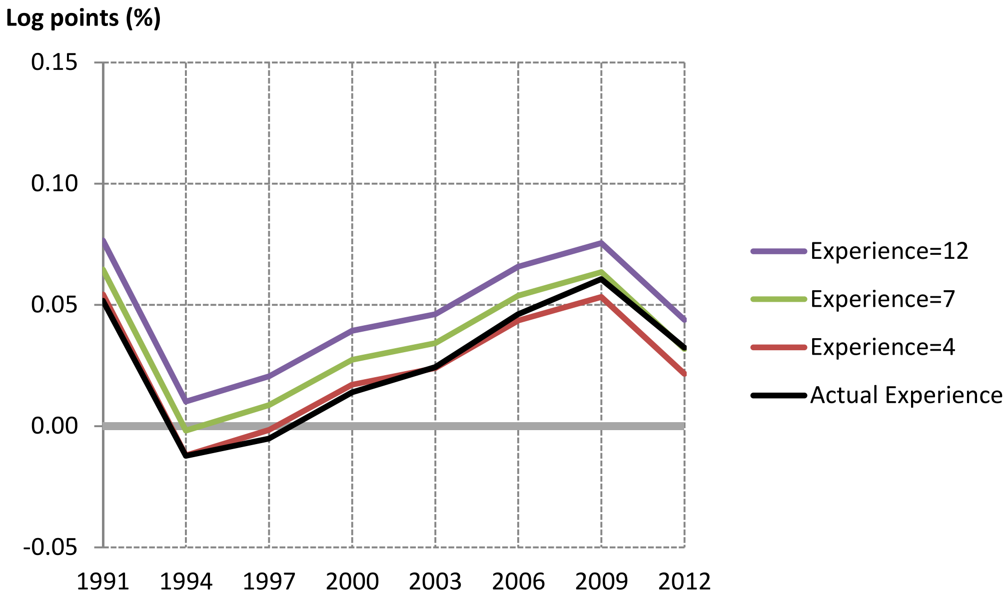
Table A, Continued: Means of Dependent and Independent Variables

Variable	Unauthorized		Green Card		Naturalized	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Added in Model 3						
Region (Ref: East)						
Southeast	0.15	0.35	0.09	0.29	0.06	0.23
Midwest	0.13	0.33	0.12	0.32	0.13	0.33
Southwest	0.05	0.21	0.11	0.32	0.14	0.35
Northwest	0.13	0.34	0.12	0.33	0.17	0.38
California	0.37	0.48	0.48	0.50	0.45	0.50
Task (Ref: Pre-harvest)						
Harvest	0.37	0.48	0.34	0.47	0.22	0.41
Postharvest	0.11	0.31	0.12	0.33	0.13	0.34
Semiskilled	0.18	0.39	0.26	0.44	0.30	0.46
Supervisor	0.00	0.02	0.00	0.07	0.01	0.10
Other	0.11	0.31	0.10	0.30	0.12	0.32
Crop (Ref: Field crops)						
Fruits & nuts	0.38	0.49	0.43	0.50	0.37	0.48
Horticulture	0.14	0.35	0.12	0.33	0.17	0.37
Vegetables	0.29	0.46	0.29	0.46	0.27	0.44
Miscellaneous & multiple	0.05	0.21	0.05	0.21	0.06	0.24
Share employed by contractor	0.25	0.43	0.21	0.40	0.16	0.36
Weeks of farm work last year	29	18	33	15	37	15
Weeks out of the country	13	17	5	10	2	7
Payment type (Ref: Hourly)						
Piece rate	0.20	0.40	0.19	0.39	0.10	0.31
Combination hourly & piece rate	0.02	0.14	0.03	0.18	0.03	0.16
Salaried	0.01	0.10	0.02	0.14	0.07	0.26
Sample size	23,089		15,519		1,611	

Fig. A: Wage Effects by Level of Experience:

Green Card / Naturalized vs. Unauthorized (Adjusted Estimates, Model 3)

Naturalized vs. Unauthorized



Green Card vs. Unauthorized

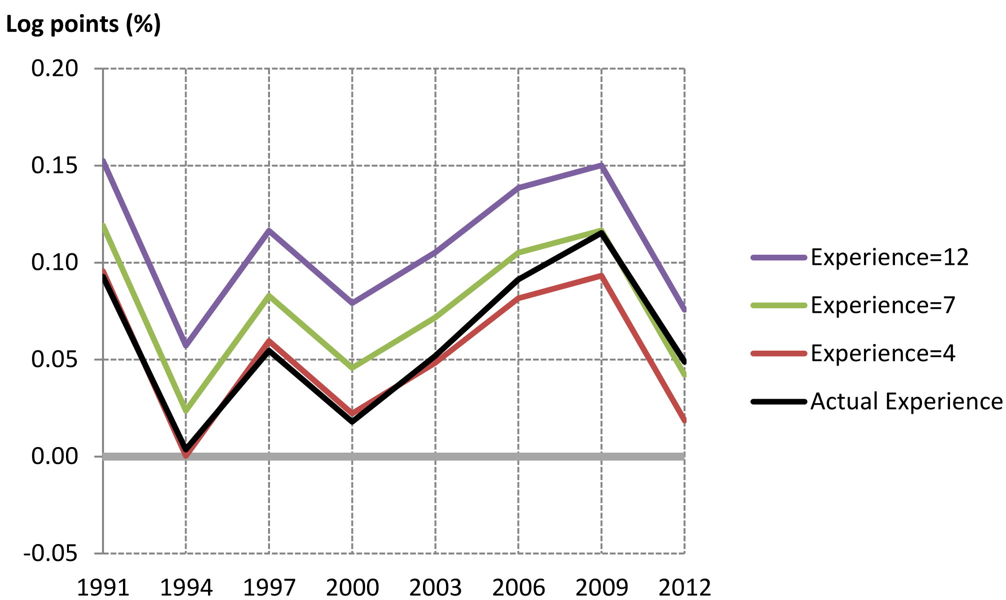
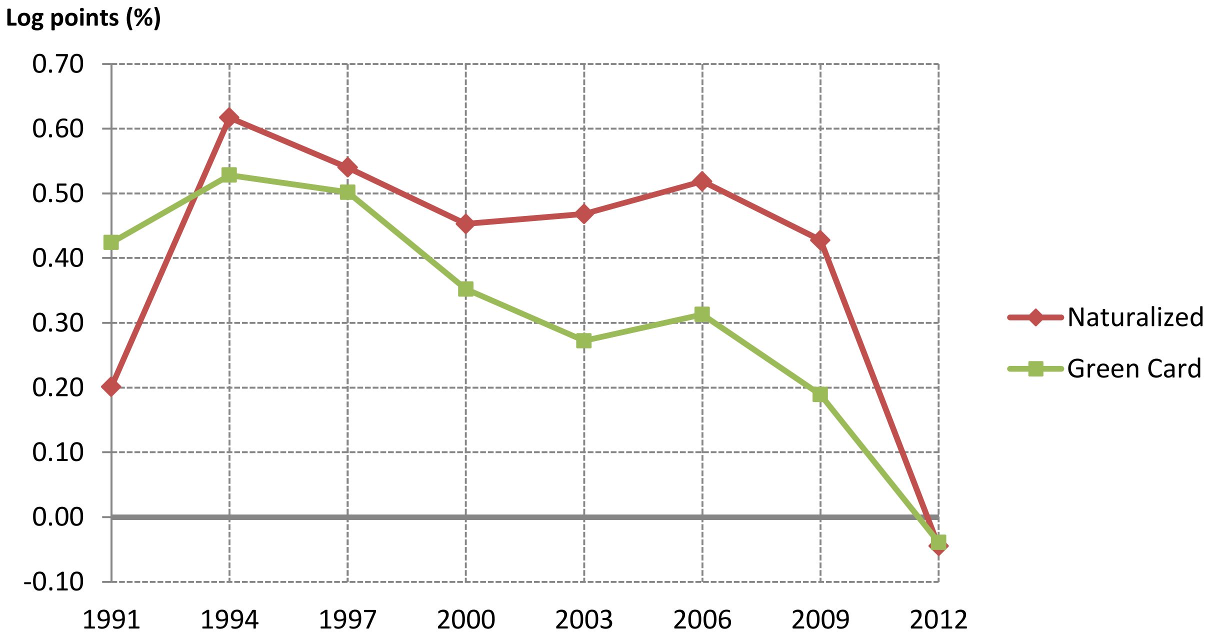


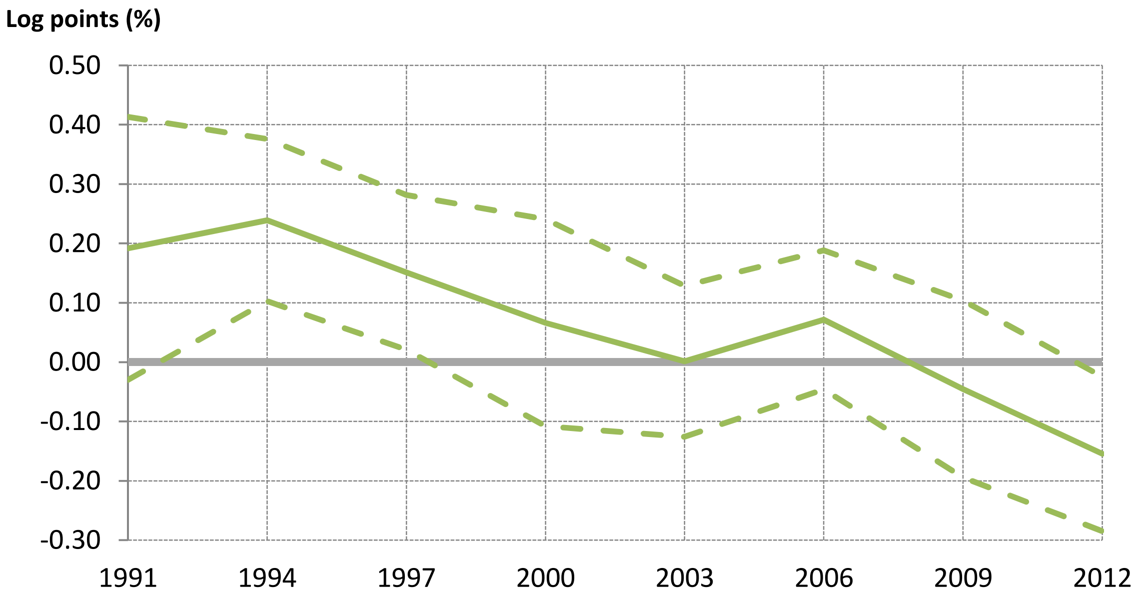
Fig. B: Pct. Difference in Days Worked: Green Card / Naturalized vs. Unauthorized (As observed)



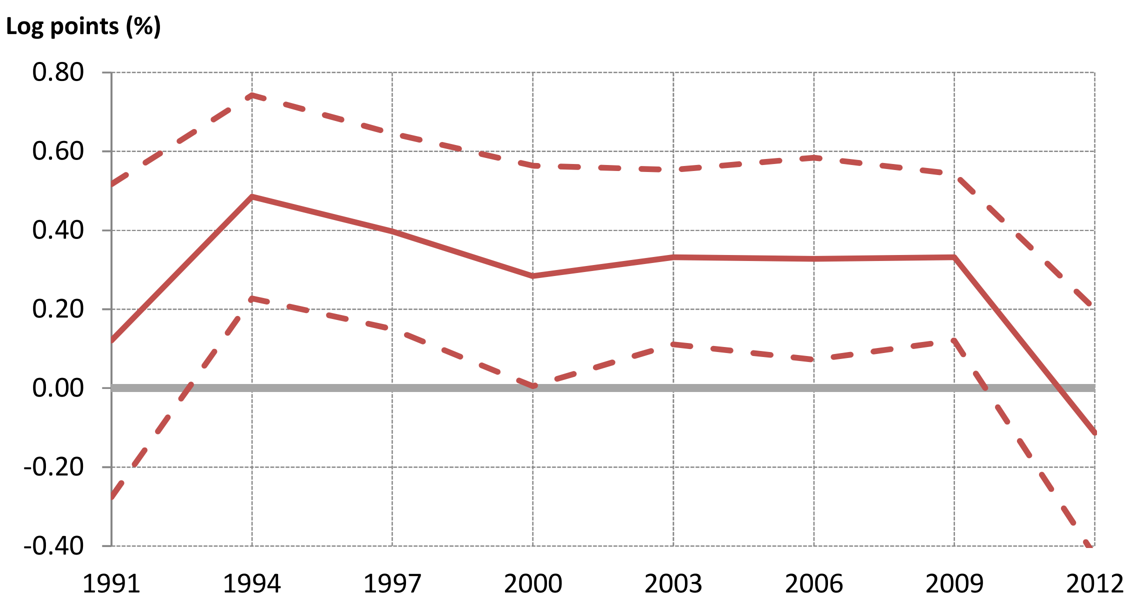
Source: Author's analysis of data from NAWS.
Note: Years on X-axis are averages of the three prior fiscal years, e.g. 1991 = Fiscal years 1989-1991. US citizens born in Puerto Rico are not included among Naturalized.

Fig. C: Pct. Differences in Days Worked: Green Card / Naturalized vs. Unauthorized (Adjusted Estimates, Model 3)

Green Card vs. Unauthorized



Naturalized vs. Unauthorized



Source: Author's analysis of data from NAWS.
Note: Dotted lines represent 95% confidence interval. Years on X-axis are averages of the three prior fiscal years, e.g. 1991 = Fiscal years 1989-1991.

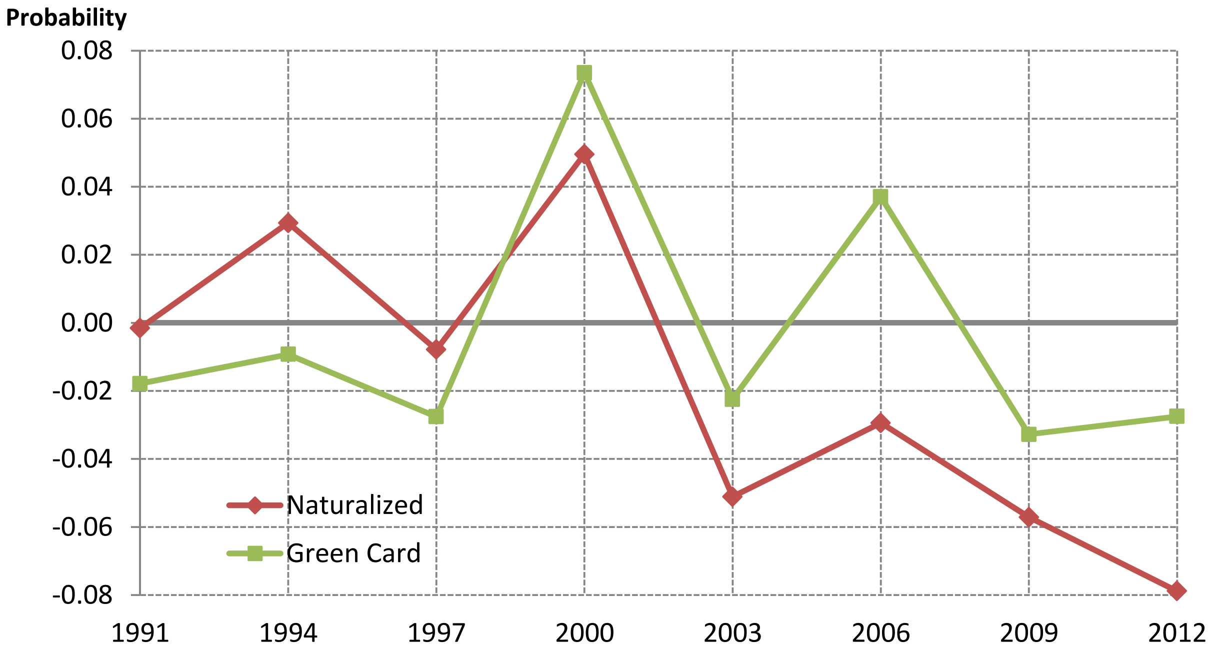
Table B: Regression Models of Effects of Legal Status on Days Worked

As observed (N=40219)	Green Card vs. Unauthorized Estimate	Std Err	t	P>|t|	Naturalized vs. Unauthorized Estimate	Std Err	t	P>|t|
All years	0.326	0.026	12.76	0.000	0.417	0.042	9.82	0.000
FY 1989-2000	0.450	0.039	11.50	0.000	0.478	0.062	7.74	0.000
FY 2001-2012	0.189	0.032	5.90	0.000	0.349	0.058	6.05	0.000
Model 1: Adjusted for age, experience, education, language skills, entry cohort, and year of interview (R^2=0.11) (N=40219)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.035	0.044	0.81	0.418	0.265	0.092	2.89	0.004
FY 1989-2000	0.137	0.057	2.43	0.015	0.327	0.108	3.04	0.002
FY 2001-2012	-0.058	0.044	-1.31	0.191	0.207	0.091	2.28	0.023
Model 2: Add controls for gender, marital status, parental status, number of children in h'hold, race, ethnicity, country of birth (R^2=0.12) (N=40219)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.057	0.045	1.25	0.210	0.295	0.091	3.23	0.001
FY 1989-2000	0.158	0.058	2.70	0.007	0.363	0.108	3.35	0.001
FY 2001-2012	-0.035	0.046	-0.77	0.442	0.233	0.090	2.58	0.010
Model 3: Add controls for region, job task, crop, pay mode, contract workers (R^2=0.22) (N=40219)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.053	0.044	1.21	0.228	0.286	0.089	3.21	0.001
FY 1989-2000	0.144	0.057	2.53	0.011	0.355	0.107	3.33	0.001
FY 2001-2012	-0.030	0.045	-0.66	0.509	0.222	0.088	2.53	0.011

Source: Author's analysis of data from NAWS.
Notes: See text for methods.

Fig. D: Difference in Probability of Nonfarm Employment:

Green Card / Naturalized vs. Unauthorized (As observed)



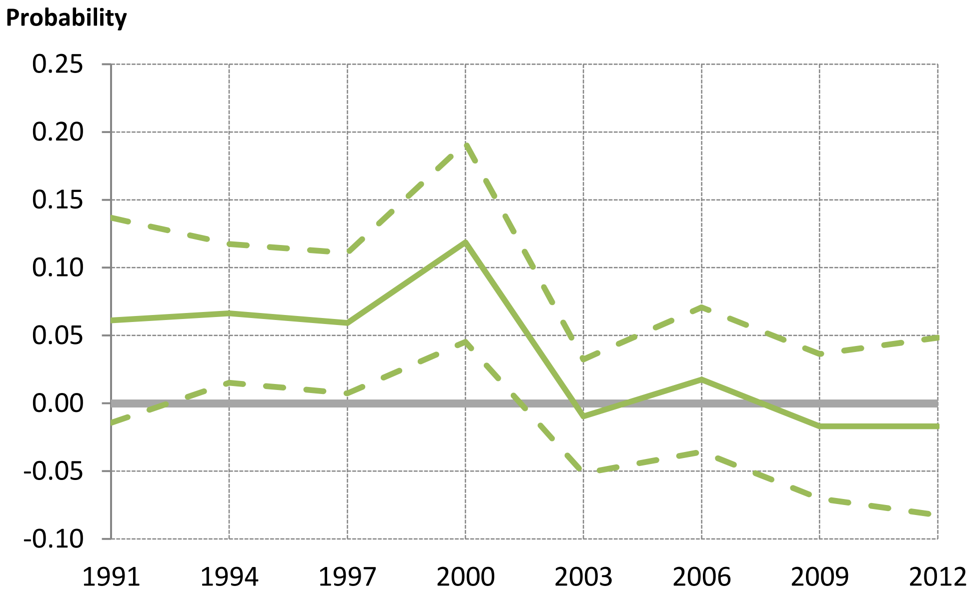
Source: Author's analysis of data from NAWS.
Note: Years on X-axis are averages of the three prior fiscal years, e.g. 1991 = Fiscal years 1989-1991. US citizens born in Puerto Rico are not included among Naturalized.

Fig. E: Differences in Probability of Nonfarm Employment:

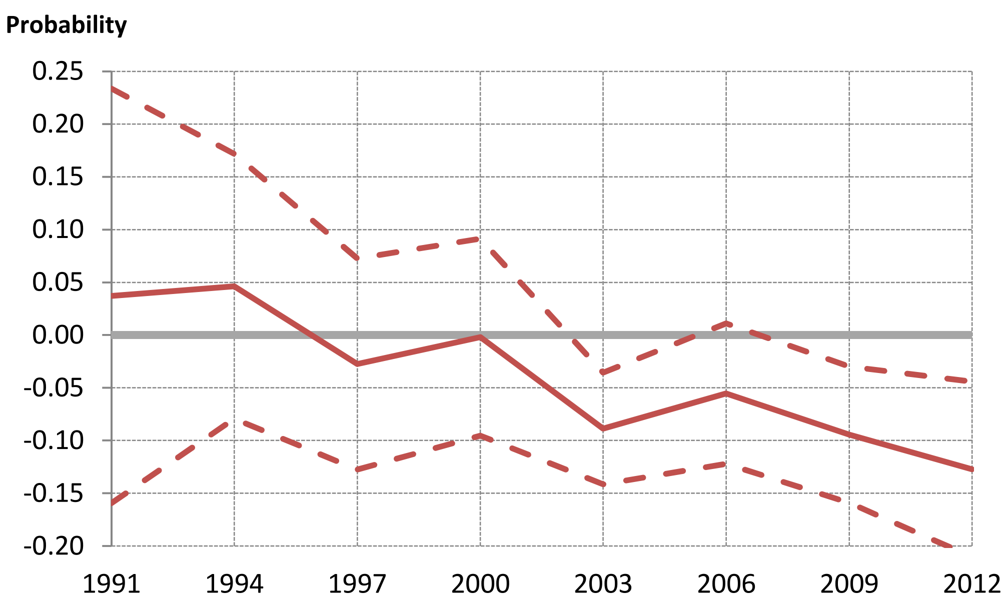
Green Card / Naturalized vs. Unauthorized

(Adjusted Estimates, Model 3)

Green Card vs. Unauthorized



Naturalized vs. Unauthorized



Source: Author's analysis of data from NAWS.
Note: Dotted lines represent 95% confidence interval.

Table C: Regression Models of Effects of Legal Status on Probability of Nonfarm Employment

	Green Card vs. Unauthorized				Naturalized vs. Unauthorized			
As observed	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.000	0.009	0.02	0.987	-0.015	0.017	-0.89	0.375
FY 1989-2000	0.010	0.012	0.80	0.425	0.020	0.025	0.79	0.428
FY 2001-2012	-0.011	0.013	-0.79	0.429	-0.054	0.022	-2.47	0.014
Model 1: Adjusted for age, experience, education, language skills, entry cohort, and year of interview (N=41941)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.014	0.018	0.78	0.436	-0.061	0.028	-2.23	0.026
FY 1989-2000	0.044	0.023	1.87	0.061	-0.020	0.044	-0.45	0.652
FY 2001-2012	-0.012	0.019	-0.64	0.520	-0.100	0.020	-5.06	0.000
Model 2: Add controls for gender, marital status, parental status, number of children in h'hold, race, ethnicity, country of birth (N=41941)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.030	0.020	1.51	0.130	-0.045	0.029	-1.56	0.120
FY 1989-2000	0.066	0.026	2.59	0.010	0.005	0.046	0.12	0.908
FY 2001-2012	-0.003	0.020	-0.15	0.879	-0.091	0.021	-4.33	0.000
Model 3: Add controls for region, job task, crop, pay mode, contract workers (N=41941)	**Estimate**	**Std Err**	**t**	**P>|t|**	**Estimate**	**Std Err**	**t**	**P>|t|**
All years	0.036	0.019	1.94	0.052	-0.045	0.027	-1.68	0.094
FY 1989-2000	0.083	0.024	3.39	0.001	0.004	0.042	0.10	0.921
FY 2001-2012	-0.006	0.019	-0.34	0.737	-0.091	0.021	-4.31	0.000