

**The Effect of Tougher Enforcement on the
Job Prospects of Recent Latin American Immigrants***

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Abstract

Attempts to enforce immigration laws in the U.S. interior have proliferated in recent years, yet the effects of these laws on immigrants are largely unknown. This paper examines whether increases in immigration-related law enforcement since 2001 have adversely affected the labor market outcomes of low-education male immigrants from Latin America, a group that composes the bulk of undocumented workers in the U.S. The crackdown on use of fraudulent Social Security numbers, increased requirements for government-issued identification, and other changes associated with greater focus on national security likely lowered the demand for undocumented foreign-born workers in the years following the 9/11 terrorist attacks. Using Current Population Survey data and a difference-in-differences estimation technique, we find strong evidence of worse labor market outcomes among recent Latin American immigrants in the post-9/11 period relative to natives and prior Latin American immigrants. The results indicate a decline in employment, hours worked, and earnings among recent male Latin American immigrants relative to similarly low-skilled black and Hispanic natives and vis-à-vis Latin American immigrants who have been in the U.S. longer. Our findings are consistent with firms increasingly substituting legal workers for undocumented labor in the years following 9/11.

Keywords September 11, immigrants, labor market outcomes

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Illegal immigration has traditionally been fought on the U.S. borders, particularly along the U.S.-Mexico border, but in recent years the crackdown has become more pervasive. The tragic events of 9/11 were a catalyst for many federal and state law changes that made national security a priority in an attempt to protect the U.S. from foreign enemies both within the nation and across the world. The large illegal immigrant population—estimated at around 8 million¹ at that time—came under heightened scrutiny as observers began to worry that terrorists could be hiding within it.

A number of government actions taken after 9/11 affected undocumented immigrants. Laws passed in the aftermath of 9/11 include the USA Patriot Act, Enhanced Border Security Act, Homeland Security Act, Aviation Transportation and Security Act, and Real ID Act. The Patriot Act gave greater powers to immigration authorities, curbed the rights of non-citizens who were detained, and required more record keeping and reporting by financial institutions. The Enhanced Border Security Act required enhanced background checks for visa applicants both at home and abroad. The Homeland Security Act divided the INS into three separate entities, with the service side separated from enforcement functions. The Aviation Transportation and Security Act made airport security personnel federal employees and required them to be U.S. citizens. The Real ID Act imposed national standards on state identification cards and required proof of legal residency from non-citizen applicants. After 9/11 but prior to the Real ID Act, a number of states had already begun requiring proof of legal residence for issuance of state id's

¹ Estimates of the size of the undocumented population in mid-2001 range from about 5.9 million to 9.9 million (Bean, Van Hook, and Woodrow-Lafeld, 2002).

and driver's licenses.² Background and security checks proliferated in the private sector, and many office buildings began requiring workers and visitors to present identification to gain entry.

One of the most far-reaching actions was the Social Security Administration's (SSA) "no-match" letter program. These letters notify employers of workers who have Social Security numbers (SSN) that are invalid (have never been issued) or do not match the name on SSA's records. This is typically an indicator that someone is working illegally. In 2002, the SSA sent no-match letters to approximately 950,000 employers, each listing up to 500 Social Security numbers that did not match SSA's records (National Immigration Law Center 2003).³ Even though the no-match program did not involve financial penalties or other sanctions at that time, employers fired thousands of Hispanic and other workers identified in no-match letters. Up to 100,000 workers—both authorized and unauthorized—are believed to have lost jobs as a result of letters during the first seven months of 2002 (Sheridan 2002). In addition, many undocumented workers identified in the letters quit their jobs out of concern that immigration authorities would follow up on the letters by raiding their workplace (Mehta, Nik, and Hincapie 2003). The SSA also made nonwork SSNs much harder to get.⁴ This action likely increased the

² Virginia, West Virginia, and Florida are among the states that began requiring proof of legal residence for issuance of state id's and driver's licenses after 9/11 and before the Real ID Act. At the end of 2004, only 11 states did not require proof of lawful presence for a driver's license. In 2004 alone, at least 19 states considered legislation related to lawful presence requirements and licenses for noncitizens (National Conference of State Legislatures 2005).

³ Authorized workers may also generate no-match letters because a simple clerical error, the misspelling of a name, or other such mistakes can create a mismatch between a SSN and the corresponding name. However, the corrections rate following the no-match letter process is low, with fewer than 2 percent of records corrected, suggesting that the majority of no-match letters concern unauthorized workers.

⁴ SSA announced on March 1, 2002, that the agency would no longer assign nonwork SSNs (numbers that are not valid for employment) to persons legally present in the U.S. who needed them solely to secure a state driver's license (National Immigration Law Center, 2002). The number of nonwork SSNs issued fell dramatically. In 1998, for example, SSA issued 128,000 nonwork SSNs; by fiscal year 2005, the number was below 15,000 (Bovbjerg, 2006). Prior to the 2002 change, a number of immigrants who entered the country legally but became illegal when they overstayed their visas obtained valid SSNs by applying for nonwork SSNs in states that required them for driver's licenses. The no-match letters did not identify workers using SSNs that were not valid for employment, only workers whose names and SSNs did not match SSA records.

number of unauthorized immigrants using fraudulent Social Security numbers, making the no-match letter campaign even bigger.

Immigration proponents and Hispanic interest groups claimed that rather than catch terrorists, these changes harmed Latino workers (Waslin, 2003). For example, the federal crackdown on airport security hurt immigrant workers, many of them Hispanic. In 2002, the INS launched “Operation Tarmac,” a series of large-scale raids at airports that resulted in about 1,200 undocumented workers being arrested, most of them Hispanic (Ashcroft, 2003; Fazlollah, 2003). The airport crackdowns rooted out over 4,000 undocumented workers (Porter, 2003). Many more undocumented airport workers likely quit on their own initiative for fear of being discovered and deported. The massive push in the Social Security Administration’s (SSA) “no-match” letter program described above had similar adverse effects on undocumented immigrants’ job prospects.

In this paper, we investigate whether post-9/11 initiatives such as those listed above resulted in worse labor market outcomes for a group that contains many undocumented immigrants: low-education males who are recent immigrants from Latin America. Previous research has found a link between enforcement and worse job outcomes for illegal immigrants from Mexico and for Latino workers overall. Using Mexican data on legal and illegal U.S. migrants, Phillips and Massey (1997) found that pay and working conditions fell substantially for undocumented Mexican workers in the U.S. following passage of the 1986 Immigration Reform and Control Act (IRCA). Dávila and Pagán (1997) find that the adverse effects were larger in sectors with stricter enforcement although the relative earnings of Mexican and Latino workers

as a whole also fell after IRCA's passage.⁵ This could have occurred because of the large number of undocumented workers in these groups or because employers were concerned that Latino workers might be illegal and treated them as such regardless of their actual status (Bansak, 2005; Bansak and Raphael, 2001; Dávila, Pagán, and Grau, 1998; Pagán and Dávila, 1996).

Like these earlier studies, we cannot directly identify illegal immigrants in the data. Hence we focus on recent immigrants from Latin America.⁶ The vast majority of the undocumented are from Latin America; these countries accounted for at least 80 percent of undocumented immigrants present in the U.S. in 2000 (INS, 2003; Passel, Capps, and Fix, 2004). Estimates indicate that, as of 2000, Mexico accounted for about 69 percent of the foreign-born population illegally present in the United States, with El Salvador, Guatemala, Colombia, and Honduras rounding out the top five source countries. Further, over half of Mexican immigrants present in the U.S. were undocumented as of 2004, including at least 80 percent of those who entered during the 1990s and early 2000s (Passel, 2004, 2005a).

A few studies have examined the economic effect of 9/11 on other minority groups, although not in the context of immigration enforcement. Kaushal, Kaestner, and Reimers (2007) argue that discrimination likely caused a statistically significant decline in the earnings of first- and second-generation Arab and Muslim men but had no impact on their employment compared with other non-Latin American immigrants and U.S. natives. Dávila and Mora (2005) also find evidence of a post-9/11 decline in earnings among Middle Eastern Arab men in the U.S. and

⁵ This adverse effect occurred despite the fact that the employer sanctions in IRCA were rarely invoked. Employers did not know that enforcement would be slack when IRCA was passed, however, which apparently led to adverse effects in the period soon after its passage.

⁶ Kaushal (2006) adopts a similar research strategy in using the Current Population Survey (CPS) to look at the effect of legalization on illegal immigrants from Central America.

attribute the drop to increased discrimination. Lauderdale (2006) reports that poor birth outcomes increased among Arabic-named women compared with other women in California after 9/11.

We use a similar approach to these studies in examining the impact of the post-9/11 changes in enforcement programs on Latin American immigrants. We compare employment, hours worked, and earnings of low-skilled recent immigrants from Latin America to several groups of low-skilled natives and to less recent Latin American immigrants. In order to minimize the impact of the 2001 recession on our analysis and to better capture the effects of enforcement changes, we define our pre-9/11 period as January 1999 to August 2001 and the post-9/11 period as January 2003 to August 2005. This allows us to focus on more general relative changes among Latin American immigrants during the post-9/11 era rather than on the immediate economic aftermath of the attacks and the then-ongoing recession. To preview our results, we find that low-education male Latin American immigrants who had been in the U.S. for less than 5 years experienced substantial declines in employment, hours worked, and earnings in the post-9/11 period relative to Hispanic and black natives and relative to Latin American immigrants who had been in the U.S. longer. The results here suggest that the heightened national security environment and stricter immigration enforcement worsened relative labor market outcomes among illegal immigrants.

Post-9/11 changes in the market for undocumented workers

The 9/11 terrorist attacks were followed by a series of changes that likely affected both the demand for and supply of undocumented labor. We expect that the demand for undocumented workers fell after 9/11 because of the initiatives discussed above, but the net

change in supply is less certain. We discuss here the theoretical effects of the potential shifts in supply and demand for assistance in interpreting the empirical results shown later in the paper.

The post-9/11 national security initiatives discussed above almost certainly made employers less willing to hire undocumented workers. In isolation, this decline in demand should have reduced employment rates, hours, and earnings among undocumented immigrants while boosting labor market outcomes in groups that are close substitutes, such as low-skilled natives and legal immigrants.

Knowingly hiring undocumented workers has been illegal and punishable by civil and criminal penalties since IRCA passed in 1986. Although this “employer sanctions” provision of IRCA had rarely been enforced, 9/11 likely heightened employers’ concerns that the government would launch a crackdown. Reinforcing this belief was the dramatic upswing in the SSA’s no-match program beginning in 2002 discussed above, an upswing that eventually culminated in new, tougher no-match rules in September 2007.⁷ In addition, well-reported cases of INS lawsuits and raids (which primarily captured undocumented Hispanic workers) occurred at Tyson Chicken and Wal-Mart in late 2001 and 2003, respectively.⁸ Media reports highlighted many instances of undocumented workers, most of them from Latin America, being fired after 9/11 and replaced by workers with legal status.

Such reports and raids continue. U.S. Department of Homeland Security (DHS) data reveal that administrative arrests at worksites jumped in 2006 and 2007 (from 1116 arrests in 2005 to 3667 in 2006 and 4077 in 2007) (DHS, 2007). We limit our post-9/11 analysis to a

⁷ The DHS safe harbor guidelines, which took effect on September 14, 2007, but were enjoined shortly thereafter, require that employers fire workers with unresolved no-matches within 90 days. Employers who fail to act in accordance with the guidelines will be assumed to have had ‘constructive knowledge’ that they were employing illegal immigrants should they be prosecuted (Migration Policy Institute, 2007).

⁸ Despite these efforts to crack down on demand for undocumented workers, the employer sanctions caseload actually fell between 2001 and 2003 as law enforcement was directed at more specific targets, such as interviewing thousands of Muslim and Arab men and detaining and deporting hundreds of them.

period following the terrorist attacks but that does not include the immediate aftermath during which the recession was still ongoing and disruptions and uncertainty were widespread. We also do not include 2006 and 2007 in our analysis—a period of perhaps even greater enforcement—because those years included a tremendous surge and then collapse in residential construction activity as well as a post-Katrina construction boom along the Gulf coast. Given the large number of Hispanic workers in the construction industry, it would be difficult to disentangle the effects of a decrease in demand due to enforcement versus an increase in demand due to low interest rates and hurricane recovery.

The supply of new immigrant labor—especially undocumented—also may have fallen after 9/11 if fewer migrants entered the U.S. Scrutiny of visa applications and border-crossers increased soon after 9/11, perhaps making it more difficult for Latinos to enter and then illegally overstay a visa. DHS data on apprehensions of illegal aliens along the Southwest border show a decline in apprehensions that actually predates both the recession (which officially began in March 2001) and 9/11. The number of apprehensions before and after 9/11 does not show a clear pattern suggestive of smaller inflows of illegal immigrants after 9/11, but the numbers are so volatile that they are difficult to interpret.⁹ Passel (2005b) estimates that the annual number of net unauthorized entries may have slowed somewhat in the post-2000 period but remained high (within 50,000 per year of pre-2000 estimates).

The changes in border enforcement may also have affected the supply of undocumented labor among illegal aliens already present in the U.S. Some migrants may have fled the U.S. after the attacks. The Mexican government estimates that about 350,000 Mexicans returned home in the two months after 9/11 (Robertson, 2002). However, increased enforcement

⁹ We therefore do not show data here on the number of apprehensions along the U.S.-Mexico border, but we are happy to share those data with interested readers on request.

theoretically could have deterred some illegal immigrants from leaving the U.S. because of concerns they might not be able to reenter (Hill 1987) and thereby increased the supply of undocumented labor relative to the counterfactual. Research suggests that increases in border enforcement are associated with less circular migration and increased duration of stay (Angelucci 2005; Reyes 2004). It is thus theoretically conceivable that undocumented labor supply increased in the short to intermediate run after 9/11, but it is more likely that it decreased.

Taking together these effects on demand and supply, it is difficult to predict the net impact on undocumented immigrants' labor market outcomes. By itself, the decrease in demand predicts a drop in employment rates, hours, and earnings. If supply fell as well, then these effects become ambiguous; if supply increased, then employment rates, hours, and earnings all should have fallen as more workers crowded into a market with lower demand.¹⁰ This theoretical ambiguity makes the effects of enforcement programs in the post-9/11 era an empirical question.

Business cycle effects

Changes in the demand and supply of undocumented workers following 9/11 were likely confounded by the effects of the recession that was already underway. According to the NBER Business Cycle Dating Committee, the recession started in March 2001 and ended in November of that year. Despite the November end date, the job market remained sluggish for at least another year. Studies suggest that the uncertainty associated with the political and macroeconomic environment after 9/11 may have kept employers from hiring, contributing to a

¹⁰ The predicted effects are ambiguous if both demand and supply fall because we look at employment rates and at hours conditional on employment, not at employment levels and aggregate hours. A fall in both demand and supply would predict a decline in employment levels and total hours, but not necessarily in employment rates and conditional average hours.

jobless recovery in 2002 (e.g., Groshen and Potter, 2003). The events of 9/11 had a particularly adverse immediate effect on hospitality and travel-related industries, which may have led to a disproportionately negative impact on Latino immigrants given their overrepresentation in those industries (Dillon, 2001). However, once hiring in the general economy picked up, job growth recovered faster among foreign-born workers than among natives, suggesting that macroeconomic conditions did not disproportionately harm immigrant workers as a whole (Sum, Fogg, and Khatiwada, 2004).

Nevertheless, the lack of job opportunities in the initial aftermath almost certainly affected the supply of undocumented workers. Our analysis takes this into account in two ways. We control for economic conditions explicitly by including two state-level indicators of economic activity similar to the national ones that the NBER Business Cycle Dating Committee uses. We also compare the change in labor market outcomes for recent Latin American immigrants to several control groups.¹¹

Other workers besides the undocumented may have been affected not only by the recession but by post-9/11 changes in the labor market. In particular, employers may have turned to legal immigrants or the native-born as a substitute for undocumented workers. There is no reason to think that labor supply changed among these groups in response to 9/11, so legal immigrants and natives—particularly those who are close substitutes for undocumented workers, such as those with little education—may have experienced an increase in employment and earnings. This would magnify any negative effect on employment and earnings among undocumented immigrants in our relative comparison below.

¹¹ There is some evidence that the effect of the business cycle differs across racial and ethnic groups (e.g., DeFreitas, 1991; Reimers, 2000) and nativity (e.g., Haider et al., 2004). We control for differential effects of the business cycle by interacting state-level controls with the treatment dummy, as explained in the text below.

Data

The data we use are from the Current Population Survey (CPS) monthly outgoing rotation group files. As noted above, we compare January 1999-August 2001 with January 2003-August 2005. We exclude the immediate period after 9/11—when most of the enforcement programs were passed and implemented—in order to capture the longer-term effects of the post-9/11 changes instead of any temporary, short-term effects due to the drop in the tourism and hospitality industry right after 9/11. The sample used here includes only males aged 18-39 who have not completed high school.¹² We do not include older individuals since the majority of undocumented immigrants are under age 40 (Passel, 2005a). We include individuals as young as age 18 because most immigrants from Latin America this age are in the labor force; only 9 percent of 18-24 year-old male Latin American immigrants are enrolled in school full time and not working, according to the CPS.¹³

Our sample only includes men who have not completed high school because most undocumented Latin American immigrants have relatively little education. About 56 percent of all male immigrants from Latin America aged 18-39 report not having completed high school. This fraction is likely higher among those who are undocumented. In addition, the majority of less-educated Hispanic immigrant men, particularly recent arrivals, are likely to be illegal immigrants. Among Mexican migrants in the U.S. less than 10 years, 80 to 85 percent are undocumented (Passel, 2006).

Since 1994, the monthly CPS has asked about place of birth, citizenship status, and the year that foreign-born individuals came to the U.S. Only individuals who report they were born in a Latin American country and were not U.S. citizens at birth (because of parental citizenship)

¹² We do not include individuals who report having a GED in our sample.

¹³ We therefore did not use current enrollment status to select the sample.

are included as Latin American immigrants here. The great majority (78 percent) of the Latin American sample is from Mexico, followed by 8 percent from El Salvador.

Part of the analysis compares immigrants who are relatively recent arrivals in the U.S. with those who have been in the U.S. longer. We divide immigrants into those present about 4 or fewer years in the U.S., those present approximately 5-10 years, and those in the U.S. for more than 10 years.¹⁴ It should be noted that the number of years in the U.S. is based on reported year of entry, but it is not clear whether immigrants reported the year they entered the U.S. to stay or when they first came to the U.S. The data may therefore misestimate total years of residence in the U.S. The analysis compares Latin American immigrants across the three arrival groups and also compares them to three groups of natives: non-Hispanic whites, non-Hispanic blacks, and Hispanics (who can be of any race).¹⁵ The samples of natives are also men aged 18-39 who have not completed high school.

The hypothesis underlying our comparison of immigrants by years of U.S. residence is that, in our sample of low-education Latino immigrants, length of U.S. residence is related to legal status. The CPS does not ask about visa status or whether the foreign-born are legally present in the U.S., just whether immigrants are naturalized citizens or not.¹⁶ We assume that more recent immigrants are more likely to lack legal documents whereas those present in the U.S. for a longer time are more likely to be legal permanent residents or U.S. citizens.

Unpublished estimates by Jeffrey Passel suggest that about 62 percent of the undocumented

¹⁴ Because the CPS reports year of entry in intervals, these categories are not exact; for some survey years, the most recent immigrants arrived within the last 3 years and the least recent over 11 years ago.

¹⁵ Hispanic immigrants can also be of any race. We do not use respondents' answer to the Hispanic ethnicity question to determine whether they are Hispanic, only their country of birth. About 5 percent of immigrants from Latin America do not report being of Hispanic origin. The results are robust to dropping those immigrants from the sample.

¹⁶ As one would expect, virtually no recent immigrant in our sample reports being a citizen (those who do make up less than 2 percent of the sample). The proportion is higher among those present about 5-10 years (4 percent) and even higher among those present more than 10 years (16 percent).

population present in the U.S. in March 2003 entered the U.S. between 1995 and 2003; 22 percent during the period 1990-1994; and about 15 percent in 1989 or earlier.¹⁷ As discussed above, we posit that illegal immigrants—and therefore more recent immigrants since a larger fraction of them are illegal—were more adversely affected by post-9/11 enforcement measures than other workers.

An important issue for this study is whether the CPS includes undocumented immigrants. Illegal aliens may be reluctant to complete a survey conducted by the U.S. government even though the survey can be administered in Spanish and they are told that the answers are confidential. But, in the *Binational Study on Migration between Mexico and the United States*, Bean et al. (1998) indicate that the CPS includes illegal immigrants. Hanson (2006) also notes that there is “now abundant evidence” that illegal immigrants are represented in the CPS. The number of foreign-born individuals enumerated in the household survey (and in the decennial Census, upon which the CPS weights are based) far exceeds estimates of the number of the foreign-born legally present in the U.S., particularly in certain groups consistent with the profile of undocumented immigrants (e.g., young adult males from Latin American countries; see Passel, Van Hook, and Bean, 2004). As a result, researchers use the CPS to estimate the number of undocumented immigrants; they first calculate the total number of foreign born (based on estimates from the CPS and Census), subtract naturalized citizens and the estimated number of immigrants and non-immigrants who are legally present (based in part on other data sources such as the Department of Homeland Security), and adjust for undercounting (e.g., Passel, 2004, 2005a,b; Passel et al., 2004). This methodology suggests that the CPS does contain illegal

¹⁷ Passel (2005a,b) provides similar statistics for March 2004.

aliens, although they are almost certainly underrepresented in the survey.¹⁸ Research using the CPS (and Census) data further shows that over time, the shares of the foreign-born who are undocumented and who are in the U.S. temporarily (nonimmigrants) have risen relative to the shares composed by legal immigrants and naturalized citizens (Massey and Bartley, 2005).

We examine four measures of labor market outcomes: employment, hours worked last week given employment (i.e., not including individuals with zero hours), and the natural log of real hourly and weekly earnings. For salaried workers, hourly earnings are calculated by dividing weekly earnings by usual weekly hours. Both measures of earnings are deflated using the monthly consumer price index for urban workers (CPI-W, with December 2005=100).¹⁹

Summary statistics

Latin American immigrants who lack a high school degree are much more likely to be employed than similarly educated natives but tend to have lower earnings. As the sample means in the first row of Table 1 show, Latin American immigrants had higher employment rates and worked more hours than white and black non-Hispanic natives and Hispanic natives both before and after the post-9/11 period. Despite this, Latin American immigrants had statistically significantly lower hourly earnings than white natives in both the pre and post period.²⁰ They also had significantly lower earnings than Hispanic natives in the post period, while pre-9/11

¹⁸ Estimates of the CPS undercount change over time. In the late 1990s, for example, the CPS probably missed between one-quarter and one-third of illegal immigrants (Passel and Fix, 2001). Post-2000, the undercount is generally believed to be much improved, with the CPS covering about the same share of the illegal immigrant population—about 90 percent—as the 2000 Census (Passel, Van Hook, and Bean, 2004). On the other hand, the national security initiatives discussed here may have worsened the undercount by making illegal aliens more unwilling to complete the survey. We are not aware of any evidence on this issue, however.

¹⁹ We only include individuals with real hourly earnings above \$1 per hour and below \$100 per hour. The results are qualitatively similar (with larger coefficients in absolute magnitude) if we instead drop observations in the top and bottom 5 percent of the hourly earnings distribution.

²⁰ Unless otherwise indicated, we refer to differences that are statistically different from 0 with greater than 90 percent confidence ($p < 0.1$) as significant here.

differences between Latino immigrants and Hispanic natives were not statistically significant below the 10 percent level. Latino immigrants had significantly higher hourly earnings than black natives in the pre and post periods, although the gap narrowed after 9/11.

Employment rates were significantly lower in the post-9/11 period for all groups shown in Table 1, and hours worked were significantly lower for Latino immigrants and white natives. Real hourly earnings were significantly higher in the post-9/11 period for all groups except white non-Hispanic natives. White non-Hispanic men experienced a slight (and statistically insignificant) decline in real earnings. This is not surprising given that our sample consists of men who do not have a high school degree, a group whose real earnings have been trending down since the 1970s.

Among Latin American immigrants, earnings clearly increase with years of U.S. residence, as the sample means in Table 2 indicate. Before 9/11, there was little difference in employment and hours by length of U.S. residence. Average hours were significantly lower among all three duration groups in the post-9/11 period, but the drop was greatest among the most recent immigrants. The most recent immigrants experienced the only significant drop in employment rates. While average hourly earnings rose in the post-9/11 period for all 3 Latino immigrant groups by length of residence, the gain was the smallest among the most recent immigrants. The sample means thus suggest that 9/11 and the period thereafter may have adversely affected employment and earnings among recent Latino immigrants relative to their more established counterparts.

The next section describes the econometric methodology we use to examine how Latino immigrants' job prospects changed relative to natives and previous Latino immigrants after 9/11 when controlling for observable characteristics and the business cycle. It is important to also

control for characteristics such as age and other demographics in case the composition of the flow of immigrants changed after 9/11.

Methods

We use a difference-in-differences (DD) method to estimate the effect of institutional changes after 9/11 on Latin American immigrants' relative labor market outcomes. The method compares the change in Latin American immigrants' outcomes before and after the period of September 2001-December 2002 with the change in outcomes for a comparison group. As noted above, we use three native-born comparison groups: white, non-Hispanics; Hispanics; and black, non-Hispanics. We also do comparisons among Latin American immigrants by length of residence in the U.S. because proportion of illegal immigrants is almost certainly highest among the most recent Latin American immigrants.

The DD method requires relatively few identifying assumptions. The methodology is essentially a before and after comparison of an affected, or "treatment," group's outcomes with the change in outcomes among an unaffected control group. The key identifying assumption is that there is no factor other than the events under investigation—in this study, tougher immigration enforcement enacted after 9/11—that causes the treatment group's outcomes to change differently than the control group's outcomes. The main other factor likely to influence labor market outcomes is the recession that occurred from March until November 2001, which motivates our inclusion of controls for the business cycle.

The basic ordinary least squares regression model we estimate for all of the outcomes (employment, hours worked, hourly, and weekly earnings) is

$$Y_{ist} = \alpha + \beta_1 \text{Sept}11_t + \beta_2 \text{Immig}_i + \beta_3 \text{Sept}11_t * \text{Immig}_i + \gamma \text{Demog}_i + \delta_1 \text{BusCycle}_{st} + \delta_2 \text{Immig}_i * \text{BusCycle}_{st} + \sigma S_s + \tau T_t + \varepsilon_{ist},$$

where Y_{ist} is a measure of labor market outcomes for individual i , who lives in state s and is surveyed at time t . The dummy variable $\text{Sept}11_t$ is equal to one if an observation is in the post-9/11 period, which is January 2003-August 2005. Immig_i is a dummy variable equal to one for an immigrant in the treatment group. The coefficient on the interaction of the two dummy variables, β_3 , measures the average change in labor market outcomes for the treatment group relative to the control group; this is the coefficient of interest and is presented in the tables that report regression results.²¹

We control for several measures of individuals' demographic characteristics in the vector Demog_i . The regressions include linear variables for age and its square and an indicator variable for being married. They also include an indicator variable for being a U.S. citizen and a linear variable measuring years of residence in the U.S. (calculated using the midpoint of the interval for year of arrival). These variables are equal to one and zero, respectively, for natives.²²

The regressions control for business cycle conditions with state-level measures of real personal income per capita and the employment rate (among persons age 16 and older). Personal income (quarterly) is reported by the Bureau of Economic Analysis while the employment data are reported monthly by the Bureau of Labor Statistics.²³ We use the income and employment variables as proxies for the two primary variables used by the NBER to determine expansions

²¹ All other regression results are available from the authors on request.

²² Coding these two variables as constants for natives (and including the foreign-born dummy variable) results in their coefficients being solely identified from variation among the foreign-born.

²³ We used annual population estimates from the Census Bureau to create these variables. We deflated personal income using the personal consumption expenditures (PCE) deflator.

and contractions: payroll employment and real personal income.²⁴ The business cycle variables are interacted with the treatment group dummy variable to control for the possibility that cyclical factors affect that group differently than the comparison group. All regressions also include state and survey date (month/year) fixed effects, S_s and T_t , respectively. Standard errors are White-Huber corrected for heteroscedasticity and clustered on treatment group status in the pre- and post-9/11 periods.

Results

Effects of post-9/11 changes

The regression results, shown in Table 3, indicate that most labor market outcomes were significantly lower among low-education Latino immigrants compared with native-born minorities in the post-9/11 period. While only hours worked declined relative to white natives (panel 1, column 1, row 2), Latino immigrants lost ground vis-à-vis Hispanic and black natives along almost all dimensions, including employment, hours worked, and weekly earnings (panels 2 and 3). Declines in hours worked drove down weekly earnings, which fell 3 percent and 6.2 percent relative to Hispanic and black natives, respectively (panels 2 and 3, column 1, row 4). Hourly earnings fell 2.8 percent compared with native blacks (panel 3, column 1, row 3). As noted above, Latino immigrants did not post losses relative to whites except in hours worked; in fact, the group as a whole posted employment and hourly earnings *gains* vis-à-vis similarly low-education native whites (panel 1, column 1, rows 1 and 3). The results thus indicate a worsening of employment and hourly earnings for low-education white native men, consistent with the general decline in their labor market prospects since the 1970s.

²⁴ We do not use unemployment rates because they are a lagging indicator of the business cycle during downturns.

The experiences of recent Latino immigrants, our group of interest, stand out in Table 3. Column 2 reports results for the most recent arrivals. For relative labor market outcomes where Latino immigrants posted losses, these losses were the most severe for the recently arrived, and where Latino immigrants posted gains overall, recent arrivals did not. There were no significant gains among recent arrivals relative to white natives, for example. Recent arrivals experienced sizable employment declines of 5.7 percent relative to Hispanic and black natives. Their weekly earnings losses were 4.1 and 8.3 percent relative to Hispanic and black natives, respectively. Relative losses in hours worked were larger among recent arrivals than among the other two groups of immigrants for all 3 control groups. Recent immigrants also experienced a 4.2 percent drop in hourly earnings relative to black natives.

Taken together, the results suggest that native-born minority workers may have benefited at the expense of Latino immigrants, particularly recently arrived immigrants. Relative to native whites and Hispanics, more experienced Latino immigrants (present in the U.S. for at least 5 years) had a mixed experience in the post-9/11 period. In contrast, new arrivals had a decidedly negative experience in relative terms. This suggests that employers may have shifted towards hiring Hispanic or black natives rather than Latino immigrants. In addition, when choosing among immigrants, employers may also have substituted earlier immigrants, more likely to be legally present in the U.S., for more recent immigrants.

It is worth noting that the regression results in Table 3 tend to show larger and more significant relative declines (or smaller relative gains) than the summary statistics in Table 1 suggest.²⁵ Much of the difference between the DDs implied by the sample means and those from

²⁵ In results not shown here, the coefficients on the demographic characteristics are similar to those found in other studies. The age coefficients show the usual concavity while married men tend to have better labor market outcomes than separated/widowed/divorced/never married men. Labor market outcomes tend to be better among

the regressions is due to the inclusion of 2 measures of state-level economic conditions (interacted with a treatment group dummy variable) in the regressions. Those economic controls are typically, although not always, jointly statistically significantly different from 0.

To further investigate differences among Latin American immigrants by duration of U.S. residence, we estimated the DD regressions using only the sample of Latin American immigrants and compare immigrants by length of residence in the U.S. The results are shown in Table 4. The column headings indicate first the treatment group and second the comparison group; for example, the first column reports results for immigrants who arrived within the last 5 years relative to immigrants who arrived over 10 years ago.

The comparisons across immigrants by duration of residence in the U.S. confirm that the most recent immigrants experienced adverse effects in the post-9/11 period. Employment, hours worked, and earnings declined significantly among immigrants who arrived in the U.S. within the last 5 years relative to immigrants present in the U.S. longer. For example, recent immigrants had employment probabilities that were 3.5 to 4.0 percentage points lower than more experienced immigrants (row 1, columns 1 and 2). Recent immigrants also experienced a decline in hourly earnings of 2.8 percent after 9/11 relative to immigrants who had been in the U.S. for over 10 years and of 3.0 percent compared with immigrants who had been in the U.S. for about 5-10 years (row 3, columns 1 and 2).

The regression results for the other coefficients in the DD regressions are as expected. In results not shown here, Latino immigrants are much more likely to be employed, work about the same number of hours as natives but have lower average earnings than native whites. Among immigrants, employment is negatively associated with length of residence in the U.S. and U.S.

immigrants who have naturalized than those who have not. Years of U.S. residence is negatively associated with employment and hours and positively associated with hourly earnings.

citizenship, while hourly and weekly earnings are positively associated with years of U.S. residence and with U.S. citizenship. Employment, hours, and earnings all increase with age but at a decreasing rate. Men who are single have worse labor market outcomes than married men. The business cycle controls and their interactions with the treatment indicator variable are jointly significantly different from zero below the 10 percent level in most of the regressions.

Robustness checks

A primary concern about the above results is whether they reflect a causal impact of events after 9/11 or simply an underlying trend of worsening labor market outcomes among recent Hispanic immigrants. To investigate this possibility, we conducted a “pseudo experiment” on a hypothetical break in the data in 1997-1998. We ran all of the above regressions with January 1996 to August 1997 as the “before” period and January 1999 to August 2000 as the “after” period. All of this period is during an economic expansion, so the pseudo experiment avoids concerns about capturing the effects of a recession. This is also a period when enforcement efforts were fairly stable.

The results do not suggest that a general trend of worsening labor market outcomes among recent Hispanic immigrants underlies our results, with the possible exception of the decline in hours worked. Table 5 reports the pseudo-experiment regression results. Hispanic immigrants as a whole had larger gains in hourly earnings than white, non-Hispanic and Hispanic natives (panels 1 and 2, column 1). Relative to native blacks, however, Latino immigrants posted relative declines in hours worked and weekly earnings, much like we see in the post-9/11 period. In contrast to the post-9/11 period, however, hourly earnings among recent immigrants rose relative to blacks in the pseudo-post period, and there were no significant

employment losses relative to any native control group. Since the treatment period in the pseudo experiment coincides with the passage of federal welfare reform, it is possible that work requirements contributed to some of the relative improvement in black job market outcomes in the pseudo-post period.

Comparing Hispanic immigrants by period of entry, as in Table 6, also does not reveal a pattern similar to our 9/11 results with the exception of the decline in hours worked. With regard to employment and earnings in our pseudo-post period, recent Hispanic immigrants did not experience any adverse outcomes relative to immigrants who had been in the U.S. longer; in fact, their hourly earnings actually rose by about 8.5 percent.

The main results discussed above are robust to several additional checks. The results in Tables 3 and 4 include all immigrants from Latin America, not just those from Mexico, which is the main source of Latin American immigrants and of undocumented immigrants. When the regressions are estimated using just low-education male immigrants born in Mexico, the results follow a similar pattern but gains tend to be smaller and losses larger. This is consistent with what we would expect when looking at a group with a larger proportion of illegal immigrants.

Restricting the sample to states with large numbers of unauthorized immigrants (according to the DHS) generally results in more negative estimates in both sets of comparisons, particularly among recent immigrants. *Vis-à-vis* natives, recent Latin American immigrants in traditional gateway states had greater employment, hours worked and earnings losses in the post-9/11 period than they did in the full sample. Established immigrants meanwhile did slightly better in the traditional migration states than in the full sample. Again, the results indicate that more recent Latin American immigrants experienced relative losses after 9/11.

Results for women

The above analysis focused on men since adult males compose about half of illegal immigrants (Passel, 2006). If we instead examine female immigrants from Latin America aged 18-39 who do not have a high school degree, we generally obtain results similar to those for men. However, the DD coefficient is typically less adverse. The most striking differences are when we compare Hispanic immigrants with Hispanic natives and when we compare immigrants by duration of residence. We find that, unlike the case for men, female Hispanic immigrants experienced earnings gains in the post-9/11 period relative to female Hispanic natives. We do not find a clear pattern of post-9/11 declines in labor markets outcomes for the most recent female immigrants relative to their more established counterparts except for employment. Potential reasons for sex differences in the results include the fact that a lower proportion of female migrants may be coming illegally and that many undocumented women work in sectors that are relatively shielded from enforcement measures, such as private household services.

Discussion

This paper used CPS data to examine whether post-9/11 immigration enforcement measures created an environment that discouraged employers from hiring undocumented workers and led to worse labor market outcomes for those workers. After 9/11, the federal government extended immigration law enforcement powers to some state and local police departments; increased identification requirements for new banking customers; resurrected the address change notification rule for non-citizens; and gave immigration authorities greater powers to detain non-citizens without judicial review. It also beefed up border security and began notifying employers when workers' SSNs were invalid. Because we cannot directly

observe illegal immigrants in the CPS data, we use recent male immigrants from Latin America aged 18-39 who lack a high school degree as a proxy for this population. We use three groups of natives—non-Hispanic whites, Hispanics, and non-Hispanic blacks—to control for confounding changes in the demand and supply of labor during this time. We also compare recent to earlier Latin American immigrants since a larger proportion of recent immigrants should be undocumented.

The empirical evidence suggests that recent low-education male Latin American immigrants experienced significant relative declines in employment, hours, and earnings as a result of 9/11-related changes. Latin American immigrants with longer residence in the U.S. also experienced employment, hours, and earnings declines compared with native-born minorities, but the adverse effects were largest among recent immigrants. The evidence thus suggests the post-9/11 changes in the labor market and, more broadly, the national security environment, adversely affected the job prospects of undocumented immigrants from Latin America. The results are consistent with a decrease in the demand for undocumented immigrants that does not appear to be driven by the business cycle or outweighed by any decreases in the supply of undocumented immigrants.

Policy Implications

Worse job prospects for Latin American immigrants have several important implications that should be taken into consideration as harsh enforcement measures continue to be enacted and enforced. There may be a positive impact on low-education native workers and more experienced immigrants, groups that compete with recent immigrants in the labor market. However, with fewer job opportunities and lower earnings, recent immigrants' relative earnings

growth and, hence, their rate of economic assimilation will slow. Moreover, income gaps between legal and illegal immigrants will increase.

It is possible that increased enforcement and worse labor market outcomes among illegal immigrants could lead to larger expenditures on social programs and lower tax revenues. Greater economic need among undocumented immigrants might necessitate higher spending on public programs that serve the families of undocumented workers, particularly their U.S. citizen children. For example, fewer undocumented immigrants might be able to afford private health insurance or pay out of pocket for health care, resulting in higher Medicaid costs, particularly for their U.S. citizen children who are eligible for such programs. Slower economic assimilation by illegal immigrants also might lead to worse outcomes for those children in the years ahead. At the same time, tougher no-match rules and more worksite raids may drive undocumented workers off official payrolls and into the informal economy, worsening the fiscal impact via lower tax revenues.

An additional implication of worse outcomes for new immigrants is a decline in the immigrant inflow. A worsening of relative labor market outcomes among recent male immigrants from Latin America—if sustained over time and holding all other factors constant—should lead to a decline in the probability of migrating to the U.S. in the future. Of course, this may or may not actually translate into fewer Latin American immigrants, illegal or otherwise, depending on how other push and pull factors play out. If economic conditions worsen in source countries, then a slight worsening of relative labor market outcomes for Latin American immigrants in the U.S. will not have a big negative impact on migrant inflows. Indeed, there appears to have been only a small reduction in the flow of undocumented immigrants in the post-

2000 period (Passel, 2005b) even as their relative labor market outcomes appear to have worsened.

It is important to note that real average hourly earnings among low-education male immigrants from Latin America were higher in the period when post-9/11 stricter enforcement measures were coming into place than they were before 9/11. Immigration inflows are more likely to be affected by potential earnings in the U.S. relative to opportunities in the home country, not relative to other groups in the U.S. A decline in relative outcomes might not lead to appreciably smaller immigration inflows whereas a decline in the level of earnings might. Other changes that have occurred since 9/11, such as discussion of an amnesty or guestworker program and the dramatic slowdown in residential construction beginning in 2007, will likely have a bigger impact on the size and well-being of this population than the post-9/11 national security initiatives discussed here.

Finally, increased enforcement and worse labor market outcomes are likely to affect the number of current illegal immigrants who remain and their duration of stay. Increased enforcement conceivably could dissuade illegal immigrants from leaving and lengthen their duration of stay because it raises the difficulty of reentering the U.S. However, increased enforcement combined with worse labor market outcomes seems more likely to push some illegal immigrants to leave and others to leave sooner than they otherwise would. This mitigates any adverse fiscal impact while boosting labor market outcomes among competing groups of workers. Such possibilities may provide some of the motivation for the enforcement programs the government has implemented since 2001.

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Table 1 Descriptive statistics for immigrants and natives, pre- and post-9/11

	Latin American Immigrants		White, Non-Hispanic Natives		Hispanic Natives		Black, Non-Hispanic Natives	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Employment	0.89 (0.32) [5887]	0.88 (0.33) [7262]	0.70 (0.46) [10098]	0.62 (0.48) [10446]	0.66 (0.47) [2194]	0.62 (0.49) [2373]	0.41 (0.49) [2152]	0.39 (0.49) [2106]
Hours worked	40.5 (9.2) [5126]	39.2 (8.8) [6231]	37.3 (14.1) [6854]	36.9 (14.1) [6278]	38.4 (11.0) [1393]	38.3 (10.5) [1408]	35.9 (12.3) [846]	35.6 (11.8) [765]
Hourly earnings	10.03 (4.47) [4934]	10.42 (4.53) [5932]	11.17 (5.70) [6334]	11.08 (5.32) [5728]	10.24 (4.18) [1358]	10.89 (5.32) [1347]	9.24 (4.69) [834]	10.09 (6.04) [723]
Weekly earnings	417.24 (212.23) [4934]	424.08 (209.70) [5932]	448.14 (307.15) [6334]	438.48 (289.81) [5728]	417.41 (238.98) [1358]	444.74 (268.94) [1347]	358.81 (243.70) [834]	376.73 (255.76) [723]

Note: Shown are weighted sample means, with standard deviations in parentheses and sample sizes in brackets. Hourly and weekly earnings are deflated using the monthly CPI-W and expressed in 2005 dollars. Data are from the CPS monthly outgoing rotation groups and only include men who have not completed high school and are aged 18-39. The pre period is January 1999-August 2001 and the post period is January 2003-August 2005.

Table 2 Descriptive statistics for immigrants by length of residence in U.S., pre- and post-9/11

	<5 Years		5-10 Years		>10 Years	
	Pre	Post	Pre	Post	Pre	Post
Employment	0.88 (0.33) [1141]	0.85 (0.36) [1302]	0.89 (0.31) [1956]	0.89 (0.31) [2801]	0.89 (0.32) [2790]	0.87 (0.33) [3159]
Hours worked	40.2 (8.9) [993]	38.4 (9.3) [1076]	40.2 (9.5) [1708]	38.9 (8.4) [2458]	40.8 (9.1) [2425]	39.9 (8.9) [2697]
Hourly earnings	9.01 (3.25) [975]	9.33 (3.88) [1045]	9.39 (3.73) [1654]	9.81 (3.58) [2361]	10.92 (5.17) [2305]	11.42 (5.29) [2526]
Weekly earnings	366.66 (177.98) [975]	375.16 (190.95) [1045]	386.03 (180.60) [1654]	394.21 (164.66) [2361]	460.59 (236.44) [2305]	470.90 (241.95) [2526]

Note: Shown are weighted sample means, with standard deviations in parentheses and sample sizes in brackets. Hourly and weekly earnings are deflated using the monthly CPI-W and expressed in 2005 dollars. Data are from the CPS monthly outgoing rotation groups and only include men born in Latin America (and not citizens at birth) who have not completed high school and are aged 18-39. The pre period is January 1999-August 2001 and the post period is January 2003-August 2005.

Table 3 Difference-in-differences estimates of the effect of 9/11 on Latin American immigrants compared with natives

Outcome	Length of Residence in U.S.			
	All	<5 Years	5-10 Years	>10 Years
<u>Compared with White, Non-Hispanic Natives</u>				
Employment	0.036** (0.005)	-0.016 (0.007)	0.042** (0.005)	0.040** (0.007)
Hours worked	-1.029* (0.212)	-2.201* (0.624)	-1.342* (0.254)	-0.551 (0.280)
Hourly earnings	0.058** (0.009)	0.036 (0.017)	0.060* (0.011)	0.073** (0.010)
Weekly earnings	0.039 (0.017)	0.016 (0.017)	0.033 (0.020)	0.052 [†] (0.018)
<u>Compared with Hispanic Natives</u>				
Employment	-0.011* (0.003)	-0.057** (0.005)	-0.010* (0.002)	-0.003 (0.005)
Hours worked	-1.211** (0.181)	-2.412* (0.503)	-1.211** (0.098)	-0.758 [†] (0.262)
Hourly earnings	0.019 [†] (0.006)	0.000 (0.012)	0.024* (0.007)	0.030** (0.004)
Weekly earnings	-0.030* (0.008)	-0.041** (0.006)	-0.020 (0.009)	-0.021 [†] (0.008)
<u>Compared with Black, Non-Hispanic Natives</u>				
Employment	-0.006 (0.006)	-0.057* (0.012)	-0.002 (0.009)	-0.012 [†] (0.005)
Hours worked	-1.408** (0.075)	-3.007** (0.349)	-1.683** (0.155)	-0.995** (0.048)
Hourly earnings	-0.028** (0.003)	-0.042* (0.008)	-0.022** (0.003)	-0.017** (0.003)
Weekly earnings	-0.062** (0.004)	-0.083** (0.010)	-0.062** (0.008)	-0.056** (0.003)

** p<0.01; * p<0.05; [†] p<0.1

Note: Shown are OLS estimates of the change in immigrants' labor outcomes relative to the indicated comparison group of natives from January 1999-August 2001 to January 2003-August 2005. Each coefficient is from a separate regression. All regressions also control for demographic characteristics, state-level business cycle conditions, state and month fixed effects (see text for details). Hourly and weekly earnings are measured as the log of real earnings. Robust clustered standard errors are in parentheses.

Table 4 Difference-in-differences estimates of the effect of 9/11 within Latin American immigrants, by years of U.S. residence

Outcome	<5 vs >10	<5 vs 5-10	5-10 vs >10
Employment	-0.035** (0.004)	-0.040** (0.002)	0.008 (0.004)
Hours worked	-1.273* (0.327)	-0.787 [†] (0.265)	-0.351** (0.053)
Hourly earnings	-0.028** (0.005)	-0.030* (0.006)	-0.004 (0.006)
Weekly earnings	-0.004 (0.007)	-0.026** (0.003)	0.007 (0.007)

** p<0.01; * p<0.05; [†] p<0.1

Note: Shown are OLS estimates of the change in the first indicated group of immigrants' labor outcomes relative to the second indicated group of immigrants from January 1999-August 2001 to January 2003-August 2005. Each coefficient is from a separate regression. All regressions also control for demographic characteristics, state-level business cycle conditions, state and month fixed effects (see text for details). Hourly and weekly earnings are measured as the log of real earnings. Robust clustered standard errors are in parentheses.

Table 5 Pseudo experiment: Difference-in-differences estimates of the effect of 9/11 on Latin American immigrants compared with natives

Outcome	Length of Residence in U.S.			
	All	<5 Years	5-10 Years	>10 Years
<u>Compared with White, Non-Hispanic Natives</u>				
Employment	0.003 (0.010)	0.021 (0.021)	0.007 (0.008)	-0.018 (0.011)
Hours worked	-0.261 (0.296)	-2.487* (0.458)	-0.292 (0.367)	-0.244 (0.194)
Hourly earnings	0.038* (0.007)	0.105* (0.024)	0.052** (0.008)	0.003 (0.012)
Weekly earnings	0.023 (0.010)	-0.006 (0.035)	0.035** (0.003)	-0.002 (0.017)
<u>Compared with Hispanic Natives</u>				
Employment	-0.006 (0.009)	-0.002 (0.016)	-0.001 (0.015)	-0.004 (0.011)
Hours worked	-0.254 (0.428)	-1.622* (0.366)	-0.381 (0.504)	0.258 (0.471)
Hourly earnings	0.049 [†] (0.021)	0.143* (0.045)	0.064 [†] (0.024)	0.015 (0.023)
Weekly earnings	0.016 (0.012)	0.047 (0.035)	0.028 (0.018)	0.011 (0.014)
<u>Compared with Black, Non-Hispanic Natives</u>				
Employment	-0.018 (0.009)	-0.034 (0.017)	-0.016 (0.009)	-0.030 (0.017)
Hours worked	-1.855* (0.351)	-4.785* (0.804)	-1.639* (0.408)	-1.699* (0.469)
Hourly earnings	0.026 (0.018)	0.129 [†] (0.045)	0.042 (0.025)	0.003 (0.022)
Weekly earnings	-0.066* (0.018)	-0.098 [†] (0.039)	-0.067* (0.016)	-0.073 [†] (0.025)

** p<0.01; * p<0.05; [†] p<0.1

Note: Shown are OLS estimates of the change in immigrants' labor outcomes relative to the indicated comparison group of natives from January 1996-August 1997 to January 1999-August 2000. Each coefficient is from a separate regression. All regressions also control for demographic characteristics, state-level business cycle conditions, state and month fixed effects (see text for details). Hourly and weekly earnings are measured as the log of real earnings. Robust clustered standard errors are in parentheses.

Table 6 Pseudo experiment: Difference-in-differences estimates of the effect of 9/11 within Latin American immigrants, by years of U.S. residence

Outcome	<5 vs >10	<5 vs 5-10	5-10 vs >10
Employment	0.002 (0.010)	0.005 (0.009)	0.024* (0.005)
Hours worked	-1.495* (0.339)	-1.084* (0.237)	-0.867* (0.190)
Hourly earnings	0.085* (0.025)	0.086 [†] (0.036)	0.054** (0.009)
Weekly earnings	-0.020 (0.021)	0.019 (0.028)	0.022 (0.015)

** p<0.01; * p<0.05; [†] p<0.1

Note: Shown are OLS estimates of the change in the first indicated group of immigrants' labor outcomes relative to the second indicated group of immigrants from January 1996-August 1997 to January 1999-August 2000. Each coefficient is from a separate regression. All regressions also control for demographic characteristics, state-level business cycle conditions, state and month fixed effects (see text for details). Hourly and weekly earnings are measured as the log of real earnings. Robust clustered standard errors are in parentheses.