LABOR MARKET OUTCOMES OF IMMIGRANTS IN THE UNITED STATES

by

Amy Melissa Gass Kandilov

A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Economics)
in The University of Michigan
2008

Doctoral Committee:

Professor John Bound, Chair
Professor John E. DiNardo
Associate Professor Sherrie A. Kossoudji
Research Assistant Professor Helen G. Levy

To God be the glory

Acknowledgements

Ivan, how can I ever thank you for all you have done for me through the last seven years? When I thought this was out of reach, you're the reason I still tried. You always believed in me. I know I never would have made it this far without you. I love you.

Mom and Dad, thank you for encouraging me and my praying for me. Ellen, thank you for being my comic relief, and for reminding me of the world outside of graduate school.

I also am grateful to John Bound, John DiNardo, Sherrie Kossoudji, and Helen Levy for their suggestions, advice, and encouragement.

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Chapter 1

Introduction

This dissertation focuses on immigrants living in the United States and their experience in the U.S. labor market. In Chapter 2, I use the policy variation provided by the welfare reform of 1996 to examine how the most recent immigrant cohorts (those arriving after 1996) adjust their health insurance status and labor supply in response to the eligibility requirements of the reform. I examine the wages of foreign-born workers whose employers sponsor them for green cards, measuring the magnitude of the wage premium associated with receiving a green card in Chapter 3. The effect of job displacement on immigrants is the focus of Chapter 4, with the duration of unemployment and the re-employment wages as the outcomes of interest. In the final chapter, I briefly summarize the main findings of the dissertation.

Chapter 2

Health Insurance and Labor Supply among Recent Immigrants following the 1996 Welfare Reform: Examining the Effect of the Five-Year Residence Requirement

1 Introduction

The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA or welfare reform) of 1996 significantly altered the relationship between the welfare system and non-citizen immigrants, particularly non-citizen immigrants who arrived in the United States after the passage of the bill. In addition to other eligibility requirements, immigrants who arrived after PRWORA (post-enactment immigrants) had to reside in the U.S. for five years before they could receive almost all types of federally-funded welfare benefits.[1] Some states used their own funds to provide welfare benefits for these immigrants during the first five years of residence; but other states did not offer such replacement programs.

Consider Medicaid, a welfare program that provides health insurance primarily to three categories of low-income individuals – families with dependent children, the elderly, and the disabled. Twelve states and the District of Columbia continued to offer Medicaid benefits to post-enactment non-citizen adult immigrants who otherwise met the Medicaid eligibility requirements but who had not yet lived in the U.S. for five years (Chin, Dean, and Patchan, 2002). I refer to these twelve states and D.C. as "more generous" states, while the

[1] Undocumented immigrants are not eligible for any federally-funded welfare benefits, with the exception of emergency Medicaid benefits.

remaining thirty-eight states, those that did not provide Medicaid benefits to post-enactment non-citizen adult immigrants, I refer to as "less generous" states. Table 2.1 lists the more and less generous states. Note that, of the six traditional gateway states that are home to the majority of immigrants in the U.S., two are classified as more generous – California and New Jersey – and four are classified as less generous – Florida, Illinois, New York, and Texas.

Using the variation across states in the provision of Medicaid for post-enactment non-citizen adult immigrants, I estimate the effects of the five-year residence requirement on Medicaid coverage, private health insurance coverage and labor supply among recent immigrants. I use nine March supplements to the Current Population Survey (CPS), from 1998 through 2006, and a difference-in-differences framework to assess the change in the trends of health insurance coverage and labor supply among the immigrant cohorts who arrived after the 1996 welfare reform.

The majority of the research on the impacts of the welfare reform on the immigrant population has focused on the time period directly after the passage of PRWORA. In studies that compare welfare use among immigrants before and after the 1996 welfare reform, the samples consist mainly of immigrants who were already living in the U.S. when PRWORA was enacted (pre-enactment immigrants) (see, for example, Borjas, 2003). Certain provisions of the welfare reform – work requirements and time limits for receiving benefits – affected all residents of the U.S. However, pre-enactment immigrants were not subject to the five-year residence requirement that affected post-enactment immigrants. In this paper, I focus on immigrants who arrived in the U.S. after the passage of PRWORA.

In the first few years after arriving in the U.S., immigrants experience fairly rapid growth in wages (LaLonde and Topel, 1992). The longer immigrants live in the U.S., the more they learn about the U.S. labor market and the more they are able to move to better and higher-paying jobs. With improving labor market outcomes, immigrants are more likely to have access to health insurance through their employers, or to be able to afford private health insurance on their own. For example, in my analysis I document that each additional year of residence in the U.S. increases the probability that an immigrant will have employer-sponsored or other private health insurance.

Each additional year in the U.S. also exposes immigrants to information about available welfare benefits. As new immigrants interact with previous cohorts of immigrants, they learn more about the availability and eligibility requirements of safety-net programs such as Medicaid. Borjas and Hilton (1996) use panel data from the Survey of Income and Program Participation to show that immigrants increase their use of welfare programs the longer they live in the United States. They also provide evidence that an immigrant's country of origin is correlated with welfare use, as recent immigrants are more likely to use the particular welfare programs that are most common among existing populations from the same country of origin.

The introduction of the five-year residence requirement in PRWORA has the potential to affect the trends in Medicaid coverage for immigrants, particularly among those living in less generous states. This, in turn, could affect their labor supply and private health insurance coverage. Borjas (2003) used the implementation of PRWORA in 1996 as a policy experiment to examine health insurance and labor supply outcomes for a sample of predominantly pre-enactment immigrants. Using CPS data from before and after 1996, he finds that the passage of the welfare reform led to a decrease in Medicaid coverage among immigrants living in less generous states, relative to immigrants in more generous states.

Since pre-enactment immigrants did not face any actual restrictions from Medicaid following PRWORA, Borjas (2003) attributes this decrease in Medicaid to the "chilling effect" of the 1996 welfare reform where, despite being eligible, immigrants are less likely to seek out welfare benefits due to concerns that receiving benefits could affect their eligibility to stay in the United States (Fix and Passel, 1999). Borjas (2003) also shows that, during the same time period, the decrease in Medicaid coverage among immigrants in less generous states was offset by increases in their private health insurance coverage and labor supply.

This paper contributes to the literature by taking advantage of the policy experiment provided by the five-year residence requirement for non-citizen immigrants who arrived after the passage of PRWORA. Because immigrants are much more likely to be uninsured than native U.S. citizens, this study informs the policy debate over the question of how to increase health insurance coverage in the U.S. population. Camarota and Edwards (2000) estimate that immigrants who arrived in the U.S. in the 1990s were responsible for more than half of the growth in the uninsured population over that same time period. If the results in Borjas (2003) hold for post-enactment immigrants, then expansions in Medicaid may not be an effective means of increasing the proportion of immigrants with health insurance.

I use the cross-state variation in Medicaid eligibility requirements for post-enactment immigrants to identify the effects of the five-year residence requirement of PRWORA on the trends in non-citizen immigrants' Medicaid and private health insurance coverage. Consistent with the five-year residence requirement imposed by the 1996 welfare reform, I find no increase in Medicaid coverage among recent immigrants in less generous states during their first five years in the U.S. While post-enactment immigrants in more generous states experience a rise in the probability of Medicaid coverage for each additional year in the U.S., those in less generous states have no growth in Medicaid coverage. Unlike Borjas (2003), I do not find evidence of increased private health insurance coverage or increased labor supply among immigrants who were restricted from Medicaid. If immigrants in less generous states were responding to the Medicaid restrictions by obtaining private health insurance and working more, we would expect to find higher trends in these outcomes for immigrants in those states. However, the growth in private health insurance and in labor supply is not higher among immigrants in less generous states. The five-year residence requirement does not appear to affect the labor supply and private health insurance coverage of immigrants who arrived in the U.S. after PRWORA.

I use a different population of immigrants (post-enactment immigrants as opposed to pre-enactment immigrants) from Borjas (2003), and also a different policy experiment, each of which contributes to the differences in the results. However, the findings in Borjas (2003) are largely due to a composition effect (see Christian, 2004). As I show in my empirical analysis, private health insurance coverage and labor supply among non-citizen immigrants increase the longer that they live in the U.S., particularly in the first five years. After those first few years of growth, private health insurance and labor supply for these immigrants do not increase significantly from year to year. In Borjas' (2003) sample, the immigrant population in less generous states contains a much larger fraction of individuals with less than five years of U.S. residence, compared to the population in more generous states. Since there are more immigrants in less generous states who are experiencing the steep growth associated with the first five years of U.S. residence, Borjas (2003) finds increases in labor supply and private health insurance for immigrants in less generous states relative to those in more generous states. As Borjas (2003) himself acknowledges, his findings are largely driven by the immigrants who have lived in the U.S. for less than ten years.[2] However, Borjas (2003) would not have been able to do a more focused analysis on the most recent cohorts of immigrants given the data available to him at the time. Only now, with many more cohorts of recent immigrants followed in the CPS, is this analysis possible.

To account for the fact that recent immigrants are such a dynamic population, in my analysis, I focus on the trends in health insurance coverage and labor supply before and after these immigrants reach five years of residence. Using the cross-state variation in the provision of Medicaid as a result of the five year residence requirement of PRWORA, I then compare the trends in Medicaid coverage, private health insurance coverage, and labor supply between immigrants in more and less generous states.

The remainder of the paper is organized as follows. I next present the theoretical framework to guide intuition. In Section 3, I describe the data I employ in my empirical analysis. The details of the econometric strategy follow in Section 4, and the results are presented in Section 5. I further provide evidence in Section 6 that cross-state migration is not likely to account for the differences in the trends in Medicaid coverage which I document. The last section concludes.

2 Framework

Cutler and Gruber (1996), among others, document how the government provision of health insurance (i.e. Medicaid) can reduce the purchase of health insurance through an employer or in the private insurance market, a phenomenon called crowd-out. To graphically illustrate this framework, consider individual i who maximizes his utility over two goods, c and h, where c is a consumption good and h is health insurance. The price of h is denoted by

[2] See discussion on page 948 of Borjas (2003).

p, and the price of c is normalized to 1. Individuals maximize their utility subject to the budget constraint $c + ph = m$, where m is total income. If an individual buys only the consumption good c, then $c = m$; if an individual buys only health insurance h, then $h = m/p$. As depicted by the indifference curves in Figure 2.1, some individuals choose to spend most of their income on c (see indifference curve U_1), consuming a bundle such as (c_{high}, h_{low}), while others choose much higher levels of h (see indifference curve U_2), consuming a bundle such as (c_{low}, h_{high}).

The budget constraint changes when the government offers free health insurance in the amount of $h = h_g$, illustrated by point A in Figure 2.1. Individuals can now devote all of their income to the consumption good and still have health insurance, locating at point A. Note that if individuals desire $h > h_g$, they are not allowed to receive h_g from the government, but must purchase the entire amount of desired h in the private market.[3] Those who would have originally purchased little or no health insurance in the private market consume the bundle (m, h_g) once the government offers free health insurance h_g. This is an example of crowd-out – individuals who were purchasing some health insurance in the private market choose instead to receive the government insurance (compare indifference curves U_1 and U_1^* in Figure 2.1).

In contrast to this typical crowd-out model, I use a simple framework to illustrate why the introduction of a government health insurance program might not lead to a significant amount of crowd-out of private insurance, or, conversely, why the discontinuation of a government health insurance program might not lead to an equivalent increase in the

[3] This is an accurate description of the Medicaid program. Unlike Medicare, where various "Medi-Gap" insurance policies can be purchased to supplement the coverage of Medicare, Medicaid coverage is restricted to those who have no other source of private health insurance coverage.

consumption of health insurance in the private market, as my empirical results imply.[4] The central assumption in this model is that health insurance in the private market is not a continuous good, but rather, can only be purchased at discrete levels. This is a realistic assumption – among employers, who are the primary source of private health insurance, 98 percent of those who offer private health insurance coverage offer only one or two health insurance plans to their employees (Kaiser Family Foundation, 2007).

For simplicity, assume that individuals have only one option for health insurance, h_p, which comes at a price of p. The budget constraint is no longer a line but two discrete points (see points B and C in Figure 2.2). Individuals can choose to buy health insurance at price p, and spend the remainder of their income ($m - p$) on the consumption good; or they can choose not to have health insurance and spend their entire income m on the consumption good. Each individual i chooses the greater of $u_i(m - p, h_p)$ and $u_i(m, 0)$.

Assume also that there are two types of individuals, H and L. Type H has a stronger preference for health insurance, while type L has a weaker preference for health insurance. As depicted in Figure 2.2, type H consumes the bundle $(m - p, h_p)$ and type L consumes the bundle $(m, 0)$, implying that type L is uninsured.

When the government introduces a free health insurance plan h_g, where $h_p > h_g > 0$, the budget constraint becomes points B $(m - p, h_p)$ and D (m, h_g) in Figure 2.2. Type L will locate at point D (moving from indifference curve U_L to indifference curve U_L^*), consuming health insurance h_g. Because h_g is significantly smaller that h_p, type H will prefer to remain at budget point B, consuming the bundle $(m - p, h_p)$. Hence, in this framework, government provision of health insurance does not crowd-out private consumption. This occurs because

[4] In Borjas (2003), the results indicate that the decrease in Medicaid is completely offset by an equivalent increase in private health insurance.

the price of health insurance in the private market is relatively high, so type L does not purchase it, and because the plan provided by the government h_g is less desirable to type H than the plan h_p that type H buys in the private market.

3 Data

For my empirical analysis, I use data from nine of the March supplements to the CPS, from 1998 through 2006. I focus on adult non-citizen immigrants who arrived in the U.S. after the passage of the 1996 welfare reform. The immigration questions in the CPS were first introduced in 1994, and they provide information on citizenship status (native, naturalized, or non-citizen), country of birth, mother's country of birth, father's country of birth, and year of arrival into the U.S. I use only the non-citizen immigrants, excluding those immigrants who have become naturalized citizens, because the five-year residence requirement of PRWORA only applies to non-citizen immigrants.[5] All U.S. citizens, regardless of whether they are native citizens or naturalized citizens, face the same eligibility requirements for welfare benefits; only non-citizen immigrants must reside in the U.S. for five years to be eligible for welfare.

Ideally the data would include only legal non-citizen immigrants.[6] All other categories of non-citizens, such as international students, foreign workers with temporary

[5] A foreign-born individual must accumulate five years as a permanent resident (green card holder) before being eligible to apply for U.S. citizenship, unless that individual is married to a U.S. citizen (three years) or serving in the Armed Forces (one year). Therefore, it is unlikely that an immigrant could evade the five-year residence requirement by becoming a U.S. citizen. For 1998-2006 CPS, only 4.8 percent of the foreign-born individuals who have been in the U.S. for less than 5 years are citizens, whereas the proportion is 12.5 percent for those who have been in the U.S. for 5 years or more.

[6] Technically, the term "immigrants" is reserved for those who are legal permanent residents of the U.S. (green card holders). All other foreign-born residents of the U.S. are considered by the Bureau of Citizenship and Immigration Services (BCIS) to be temporary aliens.

work visas, and undocumented aliens, are ineligible to receive all but emergency Medicaid benefits, regardless of their length of residence in the U.S. and regardless of their eligibility under other criteria. Unfortunately, the CPS does not ask its respondents about their visa status, so I am unable to limit my sample only to legal permanent residents who could be eligible for Medicaid if they meet the categorical and income requirements.

The state generosity measure I create incorporates the replacement of Medicaid benefits to adult non-citizen immigrants, and so I limit the sample to those non-citizen immigrants who were at least 15 years old at the time of the CPS survey. I use adults only because many states that did not replace the missing Medicaid benefits for adults did in fact have replacement programs for immigrant children.

In my analysis, in addition to the CPS data, I also use data on state unemployment rates from the Bureau of Labor Statistics (BLS). I match these unemployment rates to each individual based on their state of residence and the year of the CPS survey.

Summary statistics for non-citizen immigrants age 15 and older who arrived in the U.S. in 1996 or subsequent years are provided in Table 2.2. The summary statistics are reported separately for non-citizens who reside in more and less generous states. The observable characteristics for the two sub-samples of non-citizens are very similar. The two exceptions are residence in a metropolitan area and Medicaid coverage. Almost all immigrants in more generous states live in metropolitan areas (97 percent); while for less generous states, the proportion of immigrants in metropolitan areas is somewhat lower at 89 percent.

The other noticeable difference between these two groups of non-citizen immigrants is in their Medicaid coverage. In the more generous states, 11 percent of adult non-citizens report Medicaid coverage, whereas in less generous states, that figure is only 6 percent. This difference between more and less generous states is not surprising, considering the residence requirement of PRWORA that restricts Medicaid coverage among recent immigrants in less generous states.

Two exceptions to the five-year residence requirement likely account for the majority of the 6 percent of Medicaid coverage seen in the less generous states. First, Medicaid covers emergency health care (including childbirth) in all states, regardless of an immigrants' length of residence in the U.S. Second, refugees and those granted asylum in the U.S. are not required to meet the five year residence requirement before receiving Medicaid and other welfare benefits. They are also more likely to seek out such benefits, compared to other categories of immigrants (Borjas and Hilton, 1996).

I limit my sample to those immigrants who arrived in the U.S. in 1996 or later because I want to focus my analysis on the welfare reform's impact on immigrants who were restricted by the five-year residence requirement. However, I cannot eliminate from my sample a very small fraction of pre-enactment immigrants (approximately 8 percent of the sample) who arrived between in 1996 but before the month of August, when PRWORA became law, because the year-of-entry information in the CPS is reported in two-year intervals. The presence of these pre-enactment immigrants might inflate the size of the estimated trend in Medicaid coverage in less generous states. This would reduce the estimated difference in trends in Medicaid coverage between the more and less generous states for immigrants with less than five years of U.S. residence. Thus, my estimate of this difference in trends should be considered a lower bound of the true effect (see the Appendix for more details).

Trends in health insurance coverage for non-citizen immigrants in more and less generous sates who arrived after the passage of PRWORA are illustrated in Figures 2.3 through 2.5. Medicaid coverage is increasing for immigrants in more generous states in their first five years of residence, but for immigrants in less generous states, Medicaid coverage is fairly flat throughout the first five years of U.S. residence (Figure 2.3). This pattern is consistent with the five-year residence requirement of the 1996 welfare reform, which restricts non-citizen immigrants in less generous states from Medicaid until they have lived in the U.S. for five years. Private health insurance coverage increases for immigrants in both more and less generous states the longer that they live in the U.S. (Figure 2.4). Not surprisingly, Figure 2.5 documents that the rise in overall health insurance coverage is higher for immigrants in more generous states, particularly in the first five years of U.S. residence, because both their Medicaid coverage and their private health insurance coverage are increasing.

Trends in labor supply are shown in Figure 2.6 (labor force participation), Figure 2.7 (employment rates among those who are in the labor force), and Figure 2.8 (full-time work among those who are employed). For all three outcomes, the patterns among non-citizen immigrants in more and less generous states are very similar. Labor force participation among post-enactment immigrants grows over time, particularly in the first few years. Employment and full-time work also increase the longer that non-citizen immigrants reside in the U.S.

In my empirical analysis, I use the post-enactment immigrants in more generous states as a control group for the post-enactment immigrants in less generous states. Another useful comparison group might be non-citizen immigrants (in less generous states) who arrived in the U.S. prior to the 1996 welfare reform. Unfortunately, 1994 was the first year in which the country-of-birth and citizenship questions were included in the CPS questionnaire. The amount of data for non-citizen immigrants who are in their first few years of U.S. residence prior to the 1996 welfare reform is thus greatly limited. Additionally, for those cohorts of immigrants who entered the U.S. in the early 1990s, the 1996 welfare reform interrupts their trends in Medicaid coverage. Although I cannot use these immigrants as a control group in my empirical strategy, they are useful in the cross-section to illustrate the trends that existed in Medicaid coverage, private health insurance, and labor supply before PRWORA.

The health insurance and labor supply patterns of pre-enactment adult non-citizen immigrants are illustrated in Figures 2.9 through 2.14. In contrast to the graphs for the post-enactment immigrants, these figures do not show the same cohorts over time, but rather a cross-section of cohorts from the 1994-1996 CPS. Figure 2.9 illustrates that Medicaid coverage is consistently higher among non-citizen immigrants in more generous states, but the coverage rates follow similar trends over time. Medicaid coverage increases for immigrants in both more and less generous states in their first few years of residence, unlike the case after the 1996 welfare reform, when Medicaid increases only for immigrants in more generous states (see Figure 2.3).

Private health insurance coverage is higher for immigrants in less generous states, but grows for all, as seen in Figure 2.10. Overall health insurance coverage grows for non-citizen immigrants in both types of states (see Figure 2.11), but is higher for those in less generous states because they have higher private health insurance coverage, which more than compensates for their lower levels of Medicaid coverage. This again is in contrast to the pattern among post-enactment immigrants, where those in less generous states have lower overall insurance coverage due to the restrictions on Medicaid in those states. Trends in labor supply are similar for non-citizen immigrants in more and less generous states (see Figures 2.12, 2.13, and 2.14) prior to the 1996 welfare reform.

4 Methodology

Using the following difference-in-differences-in-trends linear probability model, I identify how an additional year of residence in the U.S. affects Medicaid coverage for non-citizen immigrants:

$$(1)\quad P_{istj} = \gamma_1 Y_{istj} + \gamma_2 LG_s + \gamma_3 R_{istj} + \gamma_4 (Y_{istj} * LG_s) + \gamma_5 (Y_{istj} * R_{istj}) + \\ + \gamma_6 (LG_s * R_{istj}) + \gamma_7 (Y_{istj} * LG_s * R_{istj}) + \mathbf{X_{istj}} \boldsymbol{\beta} + \kappa_s + \tau_t + \eta_j + \varepsilon_{istj},$$

where P_{istj} is an indicator variable equal to unity if non-citizen immigrant i, living in state s, surveyed in year t, born in country j, reported having Medicaid coverage; Y_{istj} is the number of years the immigrant has lived in the United States; LG_s is an indicator variable equal to unity if individual i lives in a less generous state that does not provide Medicaid benefits to its post-enactment adult non-citizen immigrants with less than five years of residence; and R_{istj} is an indicator equal to one if individual i has met the five-year residence requirement (see the Appendix for more information on P_{istj}, Y_{istj}, and R_{istj}).

The interaction terms are the variables of interest. They capture the difference, if any, in the patterns of Medicaid coverage between immigrants in more and less generous states, before and after they have reached the five-year residence requirement. The coefficient γ_1

captures the effect of an additional year of residence in the U.S. on the probability of having Medicaid coverage for immigrants in more generous states who have lived in the U.S. for less than five years. The sum of γ_1 and γ_4 captures the effect for immigrants in less generous states with less than five years of residence. If there is no difference in the effects of an additional year of residence between more and less generous states, then γ_4 will be zero.

For immigrants who have been in the United States for five years, long enough to meet the residence requirement of PRWORA, there may be different patterns of Medicaid coverage. The sum of γ_1 and γ_5 gives the effect of an additional year of residence on the coverage of Medicaid for immigrants in more generous states who have at least five years of residence. For immigrants in less generous states with at least five years of residence, the effect of an additional year of residence is given by the sum of γ_1, γ_4, γ_5, and γ_7.

The matrix $\boldsymbol{X_{istj}}$ contains socio-demographic characteristics including age, age squared, gender, marital status, educational attainment categories (no high school, high school drop-out, some college education, bachelor's degree, and advanced degree, where high school graduate is the omitted category), metropolitan area resident status, and finally the state unemployment rate, which is intended to absorb cyclical state-wide shocks.

I include fixed effects for state of residence, year of the survey, and country of birth – κ_s, τ_t, and η_j respectively. State fixed effects control for any differences across states that would affect access to Medicaid (i.e., the number of locations in a state where you can apply for Medicaid, the income ceiling for benefit eligibility, etc.). Year of the survey fixed effects absorb aggregate economy-wide shocks that affect Medicaid coverage. Additionally, starting in the year 2000, the CPS adjusted the health insurance questions, thereby increasing the reported percentages of all types of health insurance (Nelson and Mills, 2001). Year of the survey fixed effects control for that survey change and ensure that the results are not driven by changes in the CPS questionnaire.

Country-of-birth fixed effects, η_j, are included for three reasons. First, immigrants from some countries may be more likely to seek out welfare benefits such as Medicaid. Borjas and Hilton (1996) use SIPP data to demonstrate that immigrants who have recently arrived in the U.S. are more likely to enroll in the particular types of welfare programs that are more common among previous cohorts of immigrants from the same country of origin. Second, immigrants who are refugees are exempt from the provisions of PRWORA that limit welfare use in the first five years of residence in the U.S. Since the CPS does not report immigrants' refugee status and refugees tend to emigrate from certain countries, country-of-birth fixed effects also serve to effectively control for refugee status. Lastly, undocumented aliens are likely present in the CPS sample, but they are not eligible to receive anything except emergency Medicaid benefits.[7] Similar to refugees, undocumented immigrants tend to emigrate from certain countries – mainly Mexico and Central American nations. Passel (2006) estimates that more than 80 percent of the Mexican immigrants who entered the U.S. between 1995 and 2005 were undocumented. Hence, country-of-birth effects help to control for the presence of undocumented immigrants in the CPS sample. Note that the use of country-of-birth effects precludes the use of race and ethnicity indicators as controls in the matrix $\boldsymbol{X}_{istj}$. Race and ethnicity indicators, if included, would be largely collinear with the country-of-birth fixed effects.

[7] Undocumented aliens are significantly under-represented in the CPS. See, for example, Findeis et al. (2002) for a comparison of the foreign-born agricultural workers in the CPS and those in the National Agricultural Workers Survey (NAWS).

The error term ε_{istj} is assumed to have mean zero, and I calculate heteroskedasticity robust standard errors. Individuals interviewed in the CPS have the potential to appear in two consecutive March supplements, due to the sampling design. It is important, therefore, to control for the potential serial correlation associated with multiple observations of the same individual. Because individuals in the CPS do not have a unique identifier, I create one by matching individuals based on their household identifier, as well as their state of residence, gender, race, ethnicity, age, education, and country of birth. See the Appendix for further details on the matching methodology. Creating the unique identifier allows me to cluster the standard errors by individual.[8]

I estimate linear probability models similar to equation (1) with private health insurance coverage and overall health insurance coverage as the dependent variables. Additionally, I examine labor supply for this population, using the linear probability model in equation (1) for the dependent variables of labor force participation, employment status, and full-time work.

The choice of an estimating equation that is linear in the years of U.S. residence variable, Y_{istj}, is supported by the data. I have estimated specifications which include quadratic terms in Y_{istj} and the relevant interactions, as well as both quadratic and cubic terms in Y_{istj} and the relevant interactions. I tested if higher-order these terms are jointly significant for all six health insurance and labor supply outcomes. Using both a robust Wald test and a robust Lagrange multiplier test, I cannot reject (at the 5 percent confidence level) the null hypothesis that the coefficients on these higher order terms are equal to zero.

[8] An alternative choice of standard errors would be using the hc2 option. However, clustering the standard errors is the more conservative approach, so the reported standard errors are clustered.

When using a linear probability model, it is important to keep in mind that extreme values of the covariates may cause the fitted values from the OLS regression to fall below zero or to be greater than one. Therefore, I compare the results of the linear probability model to the results from nonlinear models, whose fitted values are always between zero and one. To this end, I estimate both probit and logit specifications similar to equation (1); the results of these regressions are reported in the Appendix, Tables 2.A1 through 2.A6. The trends in the health insurance and labor supply outcomes are remarkably similar across specifications. For better comparisons to Borjas (2003), who employs the linear probability model in his analysis, and for simplicity, the linear probability model remains the preferred specification, and the results reported are all estimated using the linear model.

5 Results

I estimate equation (1) using the entire population of post-enactment non-citizen immigrants 15 years of age and older. Table 2.3 presents the results from this first regression with Medicaid coverage as the dependent variable. In the first specification (2.3.1), I include only the difference-in-differences-in-trends variables. In 2.3.2, I add the covariates in $\boldsymbol{X_{istj}}$ to the regression.[9] Finally, in 2.3.3, I report the coefficients from the full model, which includes the covariates as well as country-of-birth, state-of-residence, and year-of-the-survey fixed effects. Across all three specifications, the coefficients on the trends of interest (in bold) remain remarkably similar.

Individuals are coded as having Medicaid coverage if they reported only Medicaid coverage and no other type of health insurance. Both Cutler and Gruber (1996) and Borjas

[9] For robustness, I have also estimated specifications which include an interaction between female and married, and specifications which include an indicator for whether or not the individual has children. The inclusion of these variables does not affect the estimated coefficients of the trends in Medicaid coverage.

(2003) use a different measure, where those who reported Medicaid and also other types of health insurance coverage are counted as having Medicaid coverage. My primary motivation for using the "only Medicaid" measure as opposed to the "any Medicaid" measure found in previous studies is that the "only Medicaid" measure allows me to see more clearly demonstrate how the availability of Medicaid affects overall insured/uninsured rates. In my analysis using the "only Medicaid" measure, an increase in the percentage of the population with Medicaid translates into a same-size decrease in the percentage who are uninsured. Using an "any Medicaid" measure, the magnitude of changes in Medicaid coverage would be larger than the magnitude of changes in overall insurance rates, since some of the Medicaid population also report other health insurance coverage. For robustness, I also use the "any Medicaid" measure found in the previous literature (results not shown). The patterns in Medicaid coverage between immigrants in more and less generous states are very similar, regardless of the measure chosen.

In Table 2.4, I present the estimated coefficients from a regression with private health insurance coverage as the dependent variable. Overall health insurance coverage is the dependent variable in Table 2.5. In both of these tables, I report results from the three specifications – with no covariates, with covariates but no fixed effects, and with both covariates and fixed effects. The coefficients on the trends of interest are very similar across the three specifications.

I am primarily interested in the effect of an additional year of residence on immigrants' health insurance coverage. Thus, I use the estimates in Tables 2.3, 2.4, and 2.5 to compute the trends for immigrants in more and less generous states before and after they have spent five years in the U.S. Table 2.6 summarizes these trends for the four groups of non-citizen immigrants in Medicaid coverage, private health insurance coverage, and overall health insurance coverage.

The first column of Table 2.6 reports the coefficient on Y_{istj}., which is the number of years an immigrant has spent in the U.S.; this trend represents the effect of an additional year of residence for immigrants living in more generous states who have not yet met the five-year residence requirement. For the estimates in column 2.6.2, the reported trend is the sum of the coefficient on Y_{istj} and the coefficient on $Y_{istj} * LG_s$, the interaction between the number of years lived in the U.S. and the indicator for living in a less-generous state. These values in the second column represent the trends in health insurance for immigrants in less generous states with less than five years of U.S. residence. To calculate the trend for immigrants living in more generous states who have five years or more of U.S. residence, I add the coefficients on Y_{istj} and $Y_{istj} * R_{istj}$, which is the interaction between the number of years lived in the U.S. and the indicator for having reached the five-year residence requirement; these trends are reported in column 2.6.3. In the final column of Table 2.6, I sum the coefficients on Y_{istj}, $Y_{istj} * LG_s$, $Y_{istj} * R_{istj}$, and $Y_{istj} * LG_s * R_{istj}$ in order to generate the trends for immigrants in less generous states with at least five years of U.S. residence.

For immigrants in more generous states, each additional year of residence in the first five years results in a 0.74 (std. dev. 0.27) percentage points increase in Medicaid coverage, which is a seven percent increase at the mean. This estimate is similar to previous research showing that the longer immigrants live in the U.S., the more likely they are to participate in welfare programs (see, for example, Borjas and Hilton, 1996).[10] However, for immigrants in less generous states, there is no increase in Medicaid coverage in the first five years of

[10] This pattern is also consistent with the cross-sectional patterns of Medicaid coverage among pre-enactment non-citizen immigrants; see Figure 9 for comparison.

residence; instead there is a 0.41 (std. dev. 0.15) annual percentage points decrease. The restrictions of PRWORA are effective in eliminating growth in Medicaid coverage among non-citizen immigrants in less generous states. The estimates in 2.6.3 and 2.6.4 show that there is no significant trend in Medicaid coverage for immigrants in either type of states once they have reached the five-year residence requirement. In addition, there is no discrete jump in Medicaid coverage when immigrants in less generous states reach five years of U.S. residence, even though they have become eligible for it (see coefficients on R_{istj} and on LG_s* R_{istj} in Table 2.3). These results may be evidence of a lack of information among recent immigrants about their eligibility, or to a continued "chilling effect" of the welfare reform (Fix and Passel, 1999). Although they may now qualify for Medicaid, these immigrants in less generous states may be concerned that receiving welfare benefits such as Medicaid could negatively affect their eligibility to live and work in the U.S.

The results indicate that private health insurance coverage increases significantly for immigrants in both more and less generous states in their first five years of U.S. residence. In more generous states, each additional year of residence in the first five years leads to a 3.15 (std. dev. 0.42) percentage point increase in private health insurance coverage, and in less generous states, the increase is 2.43 (std. dev. 0.33) percentage points per year. Though these estimates are not statistically significantly different from one another, it is interesting to note that the trend for immigrants in less generous states is slightly smaller in magnitude than that for immigrants in more generous states. If immigrants in less generous states seek out private health insurance because of their ineligibility to receive Medicaid (see Borjas, 2003), the results should have shown a significantly larger increase in private health insurance for immigrants in less generous states compared to immigrants in more generous states. These estimates confirm that the basic trends in private health insurance coverage are similar for non-citizens in more and less generous states, just as they appeared to be in Figure 2.4. After five years of U.S. residence, an additional year in the U.S. does not affect private health insurance coverage for immigrants in either more or less generous states.

While all immigrants experience an increase in the probability of having some type of health insurance with each additional year in the U.S., those who live in more generous states have a significantly larger increase in overall health insurance coverage – 4.18 (std. dev. 0.43) percentage points compared to 1.95 (std. dev. 0.34) for those in less generous states in the first five years of U.S. residence. By limiting immigrants' access to Medicaid in less generous states, PRWORA also effectively limited their growth in overall health insurance coverage. Again, after five years of U.S. residence, there is no significant increase in the probability of having overall insurance associated with an additional year of living in the U.S., and there is no difference between immigrants in more and less generous states.

Lack of access to Medicaid (and other welfare) benefits could motivate new immigrants to increase their labor supply (Borjas 2003), which could enable them to afford private health insurance or to access private health insurance through their employers. Overall, my results for the labor supply of post-enactment immigrants do not support that hypothesis. Table 2.7 presents the results for equation (1) using labor force participation as the dependent variable.[11] Estimates for the probability of being employed are reported in Table 2.8, and Table 2.9 reports the results for full-time work. As with the health insurance outcome variables, the results for each of these three dependent variables are reported with

[11] Typically, labor force participation equations are estimated separately for males and for females. F-tests indicate that there are no significant differences between the coefficients on the covariates of interest (the years in the U.S. variable and interaction terms) between males and females in the labor force participation equation. In fact, there are no significant differences between males and females in these coefficients for any of the insurance or labor supply outcome variables. Therefore, I keep the males and females pooled in the analysis.

no covariates, with covariates but no fixed effects, and with both covariates and fixed effects. Table 2.10 summarizes the labor supply results and translates the coefficients from Tables 2.7, 2.8, and 2.9 into the trends for immigrants in more and less generous states before and after they reach five years of U.S. residence.

Overall, two thirds of the immigrants in the sample report being in the labor force at the time of the survey (Table 2.2). Labor force participation increases significantly with each additional year of residence in the U.S. in the first five years, for immigrants in both more and less generous states. Column 2.10.1 reports that in more generous states, the probability of being in the labor force increases by 2.12 (std. dev. 0.41) percentage points for each additional year of U.S. residence; in less generous states, the increase is 1.30 (std. dev. 0.32) percentage points (column 2.10.2). These two trends are not statistically different from one another. If post-enactment immigrants were more likely to enter the labor force in response to the restrictions of PRWORA, we would expect to find larger growth in labor force participation among immigrants in less generous states. In fact I find no significant difference between the trends for immigrants in more and less generous states. Once immigrants have lived in the U.S. for at least five years (columns 2.10.3 and 2.10.4), there is no longer a significant change in their labor force participation associated with an additional year of U.S. residence for immigrants in both more and less generous states.

For post-enactment immigrants who are in the labor force, the likelihood of employment increases by 1.31 (std. dev. 0.33) percentage points annually in the first five years for those in more generous states. This effect is very similar for immigrants in less generous states (column 2.10.2) – their employment increases by 1.03 (std. dev. 0.23) percentage points per year. Once immigrants in both more and less generous states have lived in the U.S. for at least five years, there is no significant change in the probability of being employed with an additional year of U.S. residence (see columns 2.10.3 and 2.10.4).

Only in the probability of working full-time do I find weak evidence that post-enactment immigrants in less generous states might have increased their labor supply more than those in more generous states. However, these differences are not statistically significant. For each additional year in the U.S. during the first five years of residence, immigrants in less generous states have increases in full-time work by 0.77 (std. dev. 0.35) percentage points, compared to 0.30 (std. dev. 0.49) percentage points for immigrants in more generous states (columns 2.10.1 and 2.10.2). The two coefficients, though, are not significantly different from each another.

To check the robustness of the results, I narrow the focus of the analysis to the foreign-born population that is more likely to be living legally in the U.S. and therefore eligible for welfare benefits such as Medicaid. I estimate equation (1) for the health insurance and labor supply outcomes of this ‘likely legal’ population. The simplest and most straightforward way to do this is to eliminate from the analysis all of the persons who report Mexico as their country of birth. Passel (2006) estimates that 80 of foreign-born population from Mexico who entered the U.S. between 1995 and 2005 were undocumented and also that the majority of the undocumented population living in the U.S in 2005 came from Mexico.

Excluding the foreign-born from Mexico greatly reduces the sample size of the post-enactment population, from 35,158 to 21,907. However, the reduction of the number of observations is the largest change that is observed. Table 2.11 presents the trends in health insurance and labor supply for the post-enactment non-citizen population from everywhere else but Mexico. Compared with the results in Tables 2.6 and 2.10, the majority of the trends for this smaller population are of similar sign and magnitude to those for the entire population. Medicaid growth is still significant in the more generous states in the first five years; it is also larger in magnitude (0.91 percentage points vs. 0.74 percentage points), which is consistent with the fact that the sample now has fewer individuals who would not be eligible for Medicaid due to their legal status. There is still no growth in Medicaid coverage among immigrants in less generous states in their first five years of U.S. residence. Interestingly, the main difference when the Mexican-born population is excluded is that immigrants in less generous states now experience statistically significant growth in Medicaid once they have reached the five-year residence requirement.

The increase in private health insurance is still significant in the first five years in both more and less generous states, and the lack of access to Medicaid coverage results in significantly lower growth in overall health insurance coverage for immigrants in less generous states. The labor supply outcomes are fairly similar to those that include the population from Mexico; none of these trends are significantly different between immigrants in more and less generous states.

As described previously, Medicaid is a means-tested program that provides health insurance to particularly disadvantaged populations such as single mothers with dependent children. To see if the trends in health insurance and labor supply I find for the entire population of non-citizens are similar for the population more likely to be eligible for Medicaid, I estimate equation (1) only for females with lower levels of education (those who have no more than a high school diploma). These women are more likely to have low earnings and thus meet the means test for Medicaid and other welfare programs, and so they are much more likely to qualify for Medicaid coverage than women with higher education or men (see Table 2.13).

The results for the less-educated female non-citizens are reported in Table 2.12. Given the greatly reduced sample size, none of the trends for Medicaid coverage are precisely estimated, though the signs and magnitudes are consistent with the findings for the entire population. Private health insurance coverage increases significantly for less-educated females in both more (2.15 percentage points per year) and less (2.49 percentage points per year) generous states during the first five years of residence (see columns 2.12.1 and 2.12.2); these trends are not significantly different from one another.

For less-educated females in more generous states, overall health insurance grows by 2.99 percentage points each year in the first five years; but in less generous states, that growth is only 1.89 percentage points. Though these trends are not significantly different from one another, at least in magnitude they are consistent with the overall finding that the five year residence requirement leads to lower growth in overall health insurance for non-citizens in less generous states. After five years of U.S. residence, there is no significant growth in overall health insurance coverage for less-educated females in either more or less generous states.

For the labor supply outcomes, almost all of the trends for less-educated females are not statistically significant. However, the estimate in column 2.12.1 shows that labor force participation increases by 1.96 percentage points per year in the first five years for less-educated females in more generous states. There is no significant growth in labor force participation for the less-educated females in less generous states. If the lack of access to Medicaid was stimulating non-citizens to participate more in the labor force, we would expect to see the opposite, that labor force participation grew more in the less generous states. This is consistent with the finding for the non-citizen population as a whole – there is little evidence that the five year residence requirement had any effect on labor supply.

The majority of immigrants must be legal residents of the U.S. for at least five years before they are eligible to naturalize as citizens.[12] Therefore, the CPS sample of non-citizen immigrants who have not yet reached the five year residency requirement is representative of the total population of immigrants with less than five years of residence. However, after these immigrants have lived in the U.S. for five years, not only are they eligible for Medicaid and other welfare benefits, but they can also begin the process of becoming U.S. citizens. The population of non-citizens with more than five years of U.S. residents will no longer be representative of the entire immigrant population with more than five years of residence. In generalizing these results, it is important to remember that the trends for those with less than five years of residence represent the trends for the entire immigrant population with less than five years of residence, while the trends for those with more than five years of residence represent the trends only for the segment of the immigrant population who do not become U.S. citizens as soon as they are eligible to do so.

6 Discussion

The five year residence requirement of PRWORA might influence the initial location decisions of immigrants coming to the U.S. after 1996. Therefore, my findings – that non-citizens in more generous states experience increasing Medicaid coverage in their first five years of U.S. residence but those in less generous states have no growth in Medicaid – could

[12] The main exceptions are those who serve in the military and those who are married to U.S. citizens. The minimum waiting times for these groups are one and three years, respectively.

be a result of initial self-selection of immigrants into more generous states. Immigrants who know they are at greater risk of needing Medicaid and other benefits could locate in more generous states instead of less generous states when they first come to the U.S.

Kaushal (2005) investigated whether or not the passage of PRWORA affected the distribution of new permanent residents across states, using data from the Immigration and Naturalization Service from before and after the 1996 welfare reform. She found that immigrants who were more likely to qualify for means-tested benefits (unmarried women in low-skilled occupations) were no more likely to immigrate into more generous states after the welfare reform than were immigrants who were less likely to qualify for benefits (high-skilled and/or married women). The availability of welfare benefits does not appear to influence the initial state of residence for these new immigrants. Lack of information about U.S. welfare programs and how they vary across states is one possible reason Kaushal used to explain her findings. Also, she notes that family-sponsored immigrants tend to locate near their families when they first arrive in the U.S., and employer-sponsored immigrants have jobs that determine their initial locations; these two factors play a much more important role in determining initial location for new immigrants than does the relative generosity of state safety net programs. This evidence supports the idea that the trends I find in Medicaid coverage are likely not due to initial self-selection of immigrants into more generous states.

Additionally, non-citizens already living in less generous states after 1996 might have an incentive to move to more generous states where welfare benefits such as Medicaid are not restricted to those who have lived in the U.S. for at least five years. An out-migration of Medicaid-seeking immigrants from less generous to more generous states could potentially account for some of the differences in the trends in Medicaid coverage that I find. While I cannot directly address this concern using the CPS data, since CPS does not follow individuals but samples from the same physical residence even when households move, I can provide some evidence that those who would be more likely to receive Medicaid coverage in more generous states are not leaving the less generous states.

First, I show that less-educated female immigrants would have a larger incentive to move from less to more generous states in order to receive Medicaid and other benefits, compared to more-educated females and compared to both less- and more-educated males. I define immigrants as "less-educated" if they have no education beyond a high school diploma. More than two thirds of the less-educated non-citizens are not high school graduates (see Table 2.2). "More-educated" describes immigrants who have at least some college education or a higher degree. Table 2.13 shows the participation rates in three means-tested government welfare programs for four separate groups of immigrants – less-educated females, less-educated males, more-educated females, and more-educated males. As these means-tested programs primarily serve female-headed households with dependent children, it is not surprising that the participation rates in Medicaid, Food Stamps, and Temporary Aid to Needy Families (TANF) for less-educated female immigrants are higher than the participation rates for the other groups of immigrants. If the five-year residence requirement provides incentives for immigrants in need of welfare benefits to move from less generous to more generous states, we would expect this effect to be most prominent among the population at greatest risk for receiving Medicaid benefits – less-educated females.

Next, I present evidence that less-educated female immigrants are less likely to move from less to more generous states, compared to the other education-gender groups. Table 2.14 displays the percentage of each education-gender group that lives in less generous states, both when these cohorts of immigrants initially arrived in the U.S., and about five years later. If Medicaid availability in more generous states is prompting relocation from less to more generous states, then we would expect to see the proportion of less-educated females living in less generous states to fall over time. While there is a slight decrease in the proportion of less-educated females living in less generous states, from 64.9 percent to 64.1 percent, that this drop is much smaller than the decrease for less-educated males, and for more-educated males and females, none of whom have a great incentive to relocate to more generous states.[13] The relatively stable proportion of less-educated females living in less-generous states indicates that out-migration due to PRWORA is not a significant concern.

7 Conclusion

The 1996 welfare reform was successful in preventing the post-enactment cohorts of immigrants living in less generous states from gaining access to Medicaid during their first five years of residence in the U.S. This in turn meant that post-enactment immigrants in less generous states experienced lower growth in overall health insurance coverage when compared to immigrants in more generous states. Using the passage of PRWORA as the policy experiment, Borjas (2003) finds that immigrants in less generous states experience a decrease in Medicaid coverage but an increase in private health insurance coverage. However, when I compare post-enactment immigrants in more and less generous states, I do not find differences in private health insurance coverage.

Because recent immigrants are a dynamic population, I focus on the trends in health insurance and labor supply among immigrants who arrived in the U.S. after the passage of

[13] These percentages reflect net movements of immigrants across states and out of the country, and these different effects cannot be disentangled with the CPS data.

PRWORA in 1996. I find that Medicaid coverage increases among immigrants living in more generous states with each additional year of residence, but for immigrants living in less generous states, there is no growth in Medicaid coverage in the first five years. Post-enactment immigrants in both more and less generous states experience an increase in their private health insurance coverage the longer they live in the U.S., and this upward trend is not higher for the immigrants in less generous states who were denied access to Medicaid benefits in their first five years of U.S. residence. Hence, the five-year residence requirement does not increase the growth of private health insurance for immigrants in less generous states. As a result of the federal restrictions, non-citizen immigrants in less generous states do not gain health insurance coverage as quickly as do those in more generous states.

Upon reaching the five-year residence requirement, immigrants living in less generous states do not significantly increase their Medicaid coverage, even though they no longer face the residence restrictions imposed by PRWORA. This could point to a lack of information among recent immigrants about their eligibility, or to a continued "chilling effect" of the welfare reform (Fix and Passel, 1999). Although they may now qualify for Medicaid, these immigrants in less generous states may be concerned that receiving welfare benefits such as Medicaid could negatively affect their eligibility to live and work in the U.S.

The five year residence requirement does not increase the labor supply of immigrants in less generous states. For immigrants in all states, the first five years of U.S. residence are a period of significant growth in labor supply, as immigrants learn about the labor market in the U.S. These trends were present before the passage of PRWORA in 1996, and they do not appear to have been affected by the provisions of that legislation.

APPENDIX

The March supplements to the Current Population Survey used in this paper are available from the Inter-University Consortium for Political and Social Research, which is located online at http://www.icpsr.umich.edu.

Year of entry variable

In the March supplement to the CPS, the respondents who report that they were not born in the United States are asked in what year they came to the U.S. to stay. Using the matching criteria described in the next section of this Appendix to match individuals with two observations, I test whether or not the immigrants in the CPS have consistent replies to the question about their year of entry. For all immigrants in the CPS (1998-2006) who have two observations within those survey years, over 95 percent give the same response for their year of entry both times they are asked the question. The CPS grouping of the responses to the year of entry question changes slightly from year to year; immigrants who entered the U.S. in 1998 would be grouped with the 1996 and 1997 entry cohorts in the 1999 CPS, but these same immigrants would be grouped with the 1999 entry cohort in the 2000 CPS. Thus the responses for the year of entry question are likely to be even more than 95 percent consistent.

I use the responses to the year of entry question to determine which immigrants arrived after the passage of PRWORA, as so to limit my sample to the post-enactment immigrants. I construct the variable Y_{istj} – the number of years that immigrant i, living in state s, surveyed in year t, and born in country j, has lived in the U.S. – by taking the difference between the year of the CPS survey and the year of entry. The variable Y_{istj} is then used to construct the indicator variable R_{istj}. I assign an immigrant $R_{istj} = 1$ if Y_{istj} is greater than or equal to 5, to indicate which immigrants have reached the five-year residence requirement. The responses in the CPS data for the relevant population (those who arrived to stay in the U.S. in 1996 or later) are grouped together in two-year intervals.

The first problem caused by this grouping of the data is that there is a small fraction of pre-enactment immigrants in my sample. These pre-enactment immigrants were not required to live in the U.S. for five years before they could be eligible for welfare benefits; only the post-enactment immigrants faced this residence requirement. Post-enactment immigrants are those who arrived after the passage of PRWORA in August of 1996. Since those who arrived in 1996 are grouped with those who arrived in 1997 in the CPS data, to use the observations of the post-enactment immigrants who arrived in late 1996 and in 1997, I must also include those who arrived before August of 1996. Because the CPS does not ask about the month of arrival in the U.S., I cannot remove these pre-enactment immigrants from my data without also removing true post-enactment immigrants who arrived later in 1996 and in 1997, which would reduce my sample by more than one quarter (9,576 observations).

The presence of these pre-enactment immigrants in my sample may inflate the magnitude of the trend in Medicaid coverage for immigrants in less generous states, since pre-enactment immigrants are not restricted by the five-year residence requirement. This might reduce the estimated difference between the trends in Medicaid coverage for immigrants in more and less generous states in the first five years of U.S. residence. Thus, my estimate of this difference in trends should be considered a lower bound of the true effect. If I assume that immigrants arrive uniformly throughout the two-year interval, we would expect that about 29 percent (7 months between January and July divided by 24 months in two years) of the 1996-97 entry cohort are actually pre-enactment immigrants. With 9,576 observations in the 1996-97 cohort, roughly 2,793 of those observations are likely to be pre-enactment immigrants who were not subject to the five-year residence requirement. As that number is only about 8 percent of my entire sample of 35,158 immigrants, any bias should be fairly small. When I remove the 1996-97 cohort from my dataset and estimate equation (1) for all of the dependent variables (Medicaid, private health insurance, overall health insurance, labor force participation, being employed, and working full-time), the results are not significantly different from those including the 1996-97 cohort (not shown).

Another issue arises in determining which cohorts of immigrants have indeed reached the five-year residence requirement, thus making them eligible for welfare benefits such as Medicaid. I assign the value of Y_{istj} based on the difference between the year of the survey and the year of arrival. As the arrival years are grouped in two-year intervals, I use the second year of the grouping as the arrival year. For example, in the 2004 survey, immigrants who report arriving in the U.S. in 1998 or 1999 (grouped together in the CPS data) are assigned a value of Y_{istj} equal to 5 (2004 - 1999 = 5). Assuming a uniform distribution of arrival times across the two-year interval, at the time of the survey in March 2004, roughly half of the 1998-99 cohort will have lived in the U.S. for more than 5 years (but less than 6 years), and the other half will have lived in the U.S. for less than 5 years (but more than 4 years), which means that half of the cohort have reached the five-year residence requirement and could be eligible for Medicaid benefits, and half have not reached the residence requirement. In the regression results reported in this paper, I treat those cohorts of immigrants who have 5 years of residence as though everyone in the cohort has reached the residence requirement (even though some have not). To check for the robustness of my results, I remove the cohorts with Y_{istj} equal to 5 from the data, and use this smaller dataset to estimate equation (1) for all of the reported outcomes (results not shown). Removing these cohorts does not affect the magnitude of any of the results; the coefficients using the reduced sample are not significantly different from those that use the entire sample, and similar trends emerge.

Medicaid coverage

The health insurance questions in the CPS ask the survey respondents to report their health insurance coverage for the previous year. However, when comparing the CPS responses to other surveys, some researchers conclude that the CPS responses are actually more similar to point-in-time estimates (see, for example, Swartz, 1986). The estimations in this paper also assume that the CPS responses are point-in-time estimates, but robustness checks assuming that the CPS responses refer to coverage in the previous year yield similar coefficients (results not shown).

For individuals who do not respond to the questions about Medicaid coverage in the CPS, their Medicaid coverage is imputed using responses to other questions such as TANF receipt. To determine how my results could be affected by the imputed information, I eliminate the 9 percent of the sample (3,190 non-citizens) whose Medicaid coverage is imputed rather than reported. Among those with imputed Medicaid information, 5.2 percent have Medicaid coverage as compared to 6.4 percent among those who answered the Medicaid question.

Eliminating those with imputed Medicaid coverage does not significantly affect the results for the trends in Medicaid coverage among non-citizens (results not shown). For those in more generous states, each additional year of U.S. residence results in a statistically significant 0.91 percentage point increase in Medicaid coverage, which is comparable to the 0.74 percentage point increase for the entire population from Table 2.6. For non-citizens in less generous states, there is no growth in Medicaid coverage in the first five years, the trend is -0.30 (comparable to -0.44 in Table 2.6). As with the entire sample, there is no significant growth in Medicaid coverage once non-citizens have live in the U.S. for five years when I remove those with imputed Medicaid information. Given the similarity between the results whether or not those with imputed Medicaid coverage are included, it does not appear that the data imputation is biasing my main results.

Matching methodology

In the CPS survey methodology, a housing unit is included in the sampling frame for four consecutive months, excluded for the following eight months, and then included again for four additional months. Individuals interviewed in the CPS then have the potential to appear in two consecutive March supplements, due to the sampling design. Because individuals in the CPS do not have a unique identifier, I create one by matching individuals based on household and person identifiers, as well as state of residence, gender, race, ethnicity, age, education, and country of birth.

I eliminate any matches where the reported age or education status decreased or grew by an amount more than could be expected with the passage of one year. About 40 percent of the sample have two observations. Given that the CPS follows physical residences as opposed to individuals or households, and given the high mobility of recent immigrants, this low rate of matching is not surprising.

Specification Robustness Checks

The Appendix Tables 2.A1 through 2.A6 compare the results of the probit and logit specifications side-by-side with the results from the linear probability model for all six health insurance and labor supply outcomes. The coefficients for the difference-in-differences-in-trends variables are remarkably similar across all three specifications. The results therefore do not appear to be driven by the functional form of the econometric specification. For clarity and for ease of comparison to the results in Borjas (2003), the main results reported are obtained using the linear probability model.

APPENDIX TABLES

Table 2.A1 Trends in Medicaid coverage comparing linear probability, probit, and logit specifications

Variable \ Model	Linear prob.	Probit	Logit
Y_{istj} (Years in the U.S.)	0.007*	0.004*	0.005*
	(0.003)	(0.002)	(0.002)
LG_s (Less generous state)	0.011	-0.026	-0.033
	(0.063)	(0.044)	(0.049)
R_{istj} (Resident for five years)	0.016	0.006	0.007
	(0.022)	(0.013)	(0.015)
$Y_{istj} * LG_s$	-0.012*	-0.008*	-0.010*
	(0.003)	(0.002)	(0.003)
$Y_{istj} * R_{istj}$	-0.006	-0.003	-0.003
	(0.004)	(0.002)	(0.003)
$LG_s * R_{istj}$	-0.054*	-0.034*	-0.061*
	(0.025)	(0.011)	(0.024)
$Y_{istj} * LG_s * R_{istj}$	0.014*	0.011*	0.015*
	(0.005)	(0.004)	(0.005)
Age	-0.001*	-0.001*	-0.001*
	(0.001)	(0.000)	(0.001)
Age squared	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Female	0.027*	0.023*	0.030*
	(0.003)	(0.002)	(0.003)
Married	0.010*	0.008*	0.011*
	(0.003)	(0.002)	(0.003)
No high school	0.022*	0.017*	0.021*
	(0.004)	(0.004)	(0.004)
High school drop out	0.026*	0.019*	0.022*
	(0.004)	(0.004)	(0.004)
Some college	-0.013*	-0.008*	-0.012*
	(0.005)	(0.003)	(0.005)
College degree	-0.029*	-0.022*	-0.037*
	(0.004)	(0.003)	(0.006)
Advanced degree	-0.034*	-0.027*	-0.050*
	(0.005)	(0.003)	(0.008)
Metropolitan area	-0.001	0.000	-0.001
	(0.005)	(0.004)	(0.006)
State U^{RATE}	0.002	0.003	0.002
	(0.003)	(0.003)	(0.004)
State, year, and country effects	Yes	Yes	Yes
No. observations	35,158	34,666	34,666
R^2	0.0579	-	-
Log pseudolikelihood	-	-6,895.8	-6,878.2

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient. Data from the March supplement to the CPS, 1998-2006. Probit coefficients represent the marginal effects at the mean of the independent variables. Logit coefficients are evaluated at the population mean of the dependent variable (p=0.079). * indicates statistical significance at 5 percent.

Table 2.A2 Trends in private health insurance coverage comparing linear probability, probit, and logit specifications

Variable \ Model	Linear prob.	Probit	Logit
Y_{istj} (Years in the U.S.)	0.032*	0.041*	0.044*
	(0.004)	(0.005)	(0.006)
LG_s (Less generous state)	0.153	0.154	0.166
	(0.114)	(0.117)	(0.133)
R_{istj} (Resident for five years)	0.116*	0.145*	0.158*
	(0.034)	(0.042)	(0.045)
$Y_{istj} * LG_s$	-0.007	-0.010	-0.010
	(0.005)	(0.007)	(0.007)
$Y_{istj} * R_{istj}$	-0.028*	-0.035*	-0.038*
	(0.007)	(0.008)	(0.009)
$LG_s * R_{istj}$	-0.016	-0.019	-0.022
	(0.044)	(0.053)	(0.058)
$Y_{istj} * LG_s * R_{istj}$	0.006	0.008	0.008
	(0.008)	(0.010)	(0.011)
Age	0.004*	0.005*	0.005*
	(0.001)	(0.001)	(0.001)
Age squared	0.000*	0.000*	0.000*
	(0.000)	(0.000)	(0.000)
Female	-0.018*	-0.023*	-0.024*
	(0.005)	(0.006)	(0.007)
Married	0.100*	0.122*	0.133*
	(0.005)	(0.007)	(0.007)
No high school	-0.065*	-0.082*	-0.092*
	(0.007)	(0.009)	(0.010)
High school drop out	-0.015	-0.015	-0.016
	(0.008)	(0.009)	(0.010)
Some college	0.074*	0.081*	0.084*
	(0.009)	(0.011)	(0.011)
College degree	0.181*	0.197*	0.204*
	(0.009)	(0.011)	(0.011)
Advanced degree	0.272*	0.316*	0.341*
	(0.011)	(0.012)	(0.015)
Metropolitan area	-0.039*	-0.050*	-0.053*
	(0.010)	(0.013)	(0.014)
State U^{RATE}	-0.008	-0.010	-0.011
	(0.006)	(0.007)	(0.008)
State, year, and country effects	Yes	Yes	Yes
No. observations	35,158	35,157	35,157
R^2	0.2429	-	-
Log pseudolikelihood	-	-19,244	-19,245

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient. Data from the March supplement to the CPS, 1998-2006. Probit coefficients represent the marginal effects at the mean of the independent variables. Logit coefficients are evaluated at the population mean of the dependent variable (p=0.416). * indicates statistical significance at 5 percent.

Table 2.A3 Trends in overall health insurance coverage comparing linear probability, probit, and logit specifications

Variable \ Model	Linear prob.	Probit	Logit
Y_{istj} (Years in the U.S.)	0.042*	0.051*	0.054*
	(0.004)	(0.005)	(0.006)
LG_s (Less generous state)	0.229*	0.061	0.051
	(0.004)	(0.127)	(0.136)
R_{istj} (Resident for five years)	0.128*	0.148*	0.159*
	(0.035)	(0.042)	(0.045)
$Y_{istj} * LG_s$	-0.022*	-0.027*	-0.028*
	(0.005)	(0.007)	(0.007)
$Y_{istj} * R_{istj}$	-0.034*	-0.040*	-0.043*
	(0.007)	(0.008)	(0.009)
$LG_s * R_{istj}$	-0.068	-0.076	-0.080
	(0.045)	(0.054)	(0.057)
$Y_{istj} * LG_s * R_{istj}$	0.021*	0.024*	0.025*
	(0.009)	(0.011)	(0.011)
Age	-0.012*	-0.015*	-0.016*
	(0.001)	(0.001)	(0.001)
Age squared	0.000*	0.000*	0.000*
	(0.000)	(0.000)	(0.000)
Female	0.015*	0.017*	0.018*
	(0.005)	(0.006)	(0.006)
Married	0.123*	0.147*	0.155*
	(0.006)	(0.007)	(0.007)
No high school	-0.041*	-0.047*	-0.049*
	(0.008)	(0.009)	(0.010)
High school drop out	0.003	0.005	0.005
	(0.008)	(0.009)	(0.010)
Some college	0.062*	0.068*	0.071*
	(0.009)	(0.010)	(0.011)
College degree	0.151*	0.172*	0.183*
	(0.009)	(0.010)	(0.012)
Advanced degree	0.234*	0.280*	0.325*
	(0.010)	(0.012)	(0.016)
Metropolitan area	-0.036*	-0.041*	-0.046*
	(0.010)	(0.012)	(0.013)
State U^{RATE}	-0.008	-0.009	-0.010
	(0.006)	(0.007)	(0.008)
State, year, and country Effects	Yes	Yes	Yes
No. observations	35,158	35,157	35,157
R^2	0.2245	-	-
Log pseudolikelihood	-	-20,038	-20,034

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient. Data from the March supplement to the CPS, 1998-2006. Probit coefficients represent the marginal effects at the mean of the independent variables. Logit coefficients are evaluated at the population mean of the dependent variable (p=0.497). * indicates statistical significance at 5 percent.

Table 2.A4 Trends in labor force participation comparing linear probability, probit, and logit specifications

Variable \ Model	Linear prob.	Probit	Logit
Y_{istj} (Years in the U.S.)	0.021*	0.026*	0.028*
	(0.004)	(0.005)	(0.005)
LG_s (Less generous state)	-0.216*	-0.127	-0.143
	(0.089)	(0.104)	(0.122)
R_{istj} (Resident for five years)	0.072*	0.101*	0.111*
	(0.033)	(0.038)	(0.045)
$Y_{istj} * LG_s$	-0.008	-0.010	-0.011
	(0.005)	(0.006)	(0.007)
$Y_{istj} * R_{istj}$	-0.020*	-0.028*	-0.029*
	(0.006)	(0.008)	(0.009)
$LG_s * R_{istj}$	-0.010	-0.028	-0.025
	(0.042)	(0.054)	(0.058)
$Y_{istj} * LG_s * R_{istj}$	0.007	0.011	0.011
	(0.008)	(0.010)	(0.011)
Age	0.048*	0.060*	0.068*
	(0.001)	(0.002)	(0.002)
Age squared	-0.001*	-0.001*	-0.001*
	(0.000)	(0.000)	(0.000)
Female	-0.326*	-0.365*	-0.408*
	(0.005)	(0.005)	(0.007)
Married	-0.086*	-0.098*	-0.116*
	(0.005)	(0.007)	(0.007)
No high school	-0.092*	-0.114*	-0.120*
	(0.007)	(0.010)	(0.010)
High school drop out	-0.109*	-0.127*	-0.133*
	(0.008)	(0.010)	(0.010)
Some college	-0.026*	-0.036*	-0.038*
	(0.009)	(0.011)	(0.012)
College degree	0.019*	0.016	0.017
	(0.009)	(0.011)	(0.012)
Advanced degree	0.050*	0.050*	0.057*
	(0.010)	(0.013)	(0.015)
Metropolitan area	0.004	0.005	0.005
	(0.009)	(0.012)	(0.013)
State U^{RATE}	-0.012*	-0.017*	-0.018*
	(0.006)	(0.007)	(0.008)
State, year, and country Effects	Yes	Yes	Yes
No. observations	35,139	35,138	35,138
R^2	0.2478	-	-
Log pseudolikelihood	-	-17,626	-17,607

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient. Data from the March supplement to the CPS, 1998-2006. Probit coefficients represent the marginal effects at the mean of the independent variables. Logit coefficients are evaluated at the population mean of the dependent variable (p=0.661). * indicates statistical significance at 5 percent.

Table 2.A5 Trends in being employed (if in labor force) comparing linear probability, probit, and logit specifications

Variable \ Model	Linear prob.	Probit	Logit
Y_{istj} (Years in the U.S.)	0.013*	0.011*	0.012*
	(0.003)	(0.003)	(0.003)
LG_s (Less generous state)	0.044	0.072	0.054
	(0.095)	(0.074)	(0.067)
R_{istj} (Resident for five years)	0.039	0.028	0.034
	(0.024)	(0.020)	(0.026)
$Y_{istj} * LG_s$	-0.003	-0.001	-0.001
	(0.004)	(0.003)	(0.004)
$Y_{istj} * R_{istj}$	-0.012*	-0.009*	-0.010*
	(0.005)	(0.004)	(0.005)
$LG_s * R_{istj}$	-0.012	-0.012	-0.012
	(0.029)	(0.032)	(0.034)
$Y_{istj} * LG_s * R_{istj}$	0.003	0.002	0.002
	(0.006)	(0.006)	(0.006)
Age	0.006*	0.004*	0.005*
	(0.001)	(0.001)	(0.001)
Age squared	0.000*	0.000*	0.000*
	(0.000)	(0.000)	(0.000)
Female	-0.033*	-0.032*	-0.032*
	(0.004)	(0.004)	(0.004)
Married	-0.006	-0.004	-0.005
	(0.004)	(0.003)	(0.004)
No high school	-0.022*	-0.020*	-0.020*
	(0.006)	(0.005)	(0.005)
High school drop out	-0.025*	-0.023*	-0.023*
	(0.006)	(0.006)	(0.005)
Some college	0.011	0.009	0.011
	(0.006)	(0.005)	(0.006)
College degree	0.017*	0.016*	0.020*
	(0.006)	(0.005)	(0.007)
Advanced degree	0.021*	0.023*	0.031*
	(0.006)	(0.006)	(0.009)
Metropolitan area	-0.002	-0.005	-0.004
	(0.006)	(0.006)	(0.007)
State U^{RATE}	-0.011*	-0.010*	-0.011*
	(0.004)	(0.004)	(0.004)
State, year, and country Effects	Yes	Yes	Yes
No. observations	23,243	23,008	23,008
R^2	0.0291	-	-
Log pseudolikelihood	-	-5,403	-5,405

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient. Data from the March supplement to the CPS, 1998-2006. Probit coefficients represent the marginal effects at the mean of the independent variables. Logit coefficients are evaluated at the population mean of the dependent variable (p=0.933). * indicates statistical significance at 5 percent.

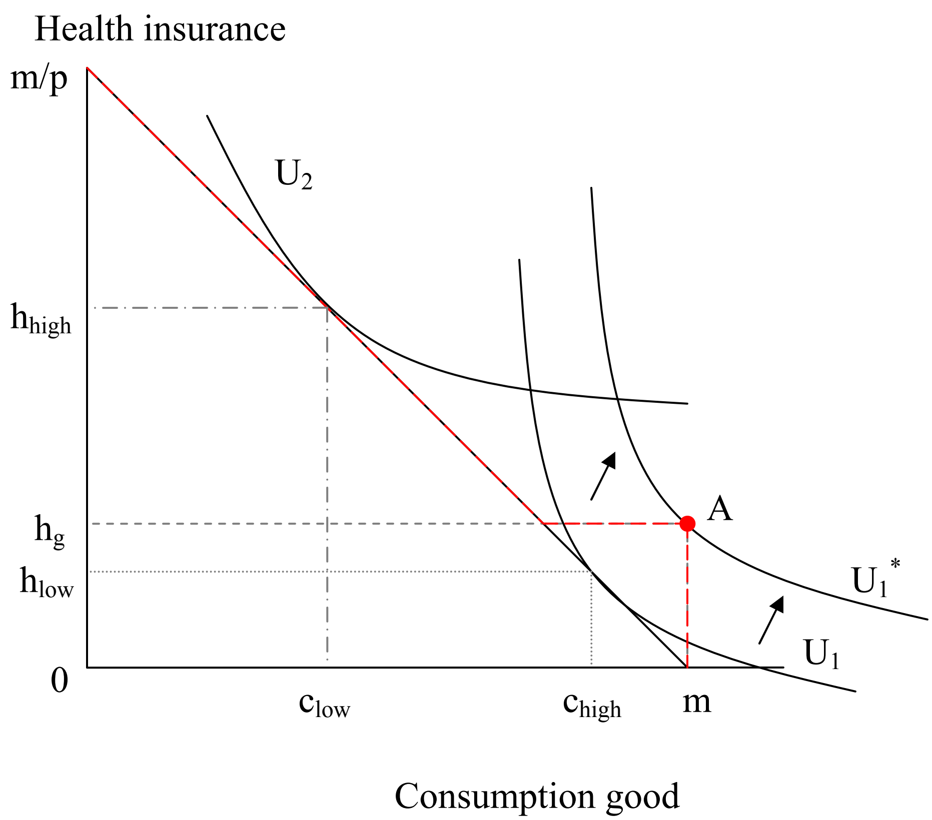
Table 2.A6 Trends in full-time work (if employed) comparing linear probability, probit, and logit specifications

Variable \ Model	Linear Prob.	Probit	Logit
Y_{istj} (Years in the U.S.)	0.003	0.003	0.003
	(0.005)	(0.005)	(0.005)
LG_s (Less generous state)	0.288*	0.168*	0.164
	(0.086)	(0.099)	(0.093)
R_{istj} (Resident for five years)	-0.032	-0.039	-0.045
	(0.036)	(0.040)	(0.042)
$Y_{istj} * LG_s$	0.005	0.006	0.005
	(0.006)	(0.006)	(0.006)
$Y_{istj} * R_{istj}$	0.008	0.009	0.010
	(0.007)	(0.007)	(0.008)
$LG_s * R_{istj}$	0.052	0.048	0.059
	(0.045)	(0.042)	(0.053)
$Y_{istj} * LG_s * R_{istj}$	-0.013	-0.013	-0.014
	(0.009)	(0.009)	(0.010)
Age	0.028*	0.025*	0.027*
	(0.002)	(0.001)	(0.002)
Age squared	0.000*	0.000*	0.000*
	(0.000)	(0.000)	(0.000)
Female	-0.149*	-0.155*	-0.152*
	(0.006)	(0.006)	(0.006)
Married	0.009	0.010	0.011
	(0.006)	(0.006)	(0.006)
No high school	-0.036*	-0.037*	-0.041*
	(0.008)	(0.009)	(0.009)
High school drop out	-0.049*	-0.046*	-0.050*
	(0.009)	(0.009)	(0.009)
Some college	-0.066*	-0.064*	-0.064*
	(0.010)	(0.010)	(0.010)
College degree	0.000	-0.001	-0.002
	(0.009)	(0.010)	(0.011)
Advanced degree	0.041*	0.043*	0.051*
	(0.011)	(0.010)	(0.014)
Metropolitan area	0.002	0.001	0.001
	(0.011)	(0.010)	(0.011)
State U^{RATE}	-0.015*	-0.015*	-0.017*
	(0.006)	(0.006)	(0.007)
State, year, and country Effects	Yes	Yes	Yes
No. observations	21,675	21,582	21,582
R^2	0.0931	-	-
Log pseudolikelihood	-	-9,135	-9,130

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient. Data from the March supplement to the CPS, 1998-2006. Probit coefficients represent the marginal effects at the mean of the independent variables. Logit coefficients are evaluated at the population mean of the dependent variable (p=0.823). * indicates statistical significance at 5 percent.

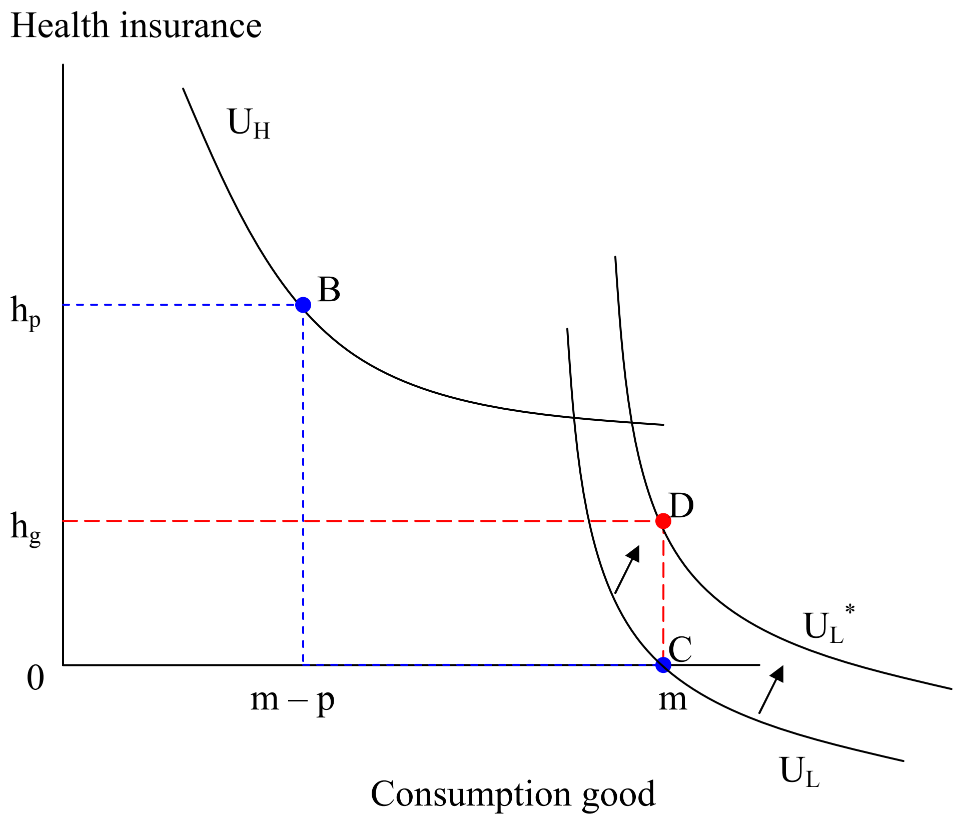
FIGURES

Figure 2.1 Standard crowd-out model with government health insurance



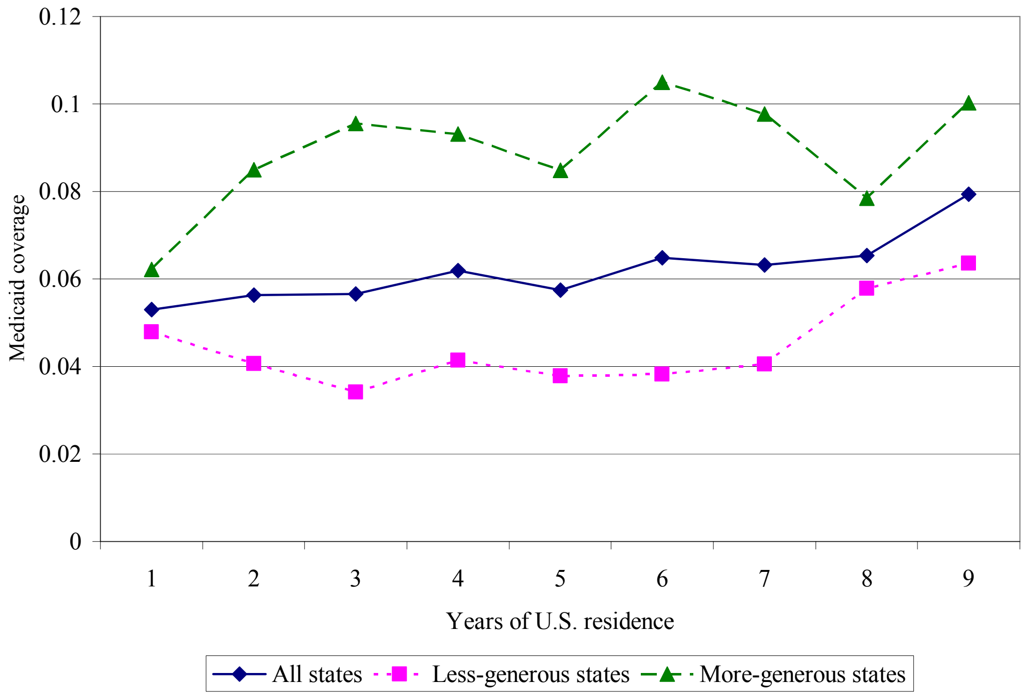
Notes: Individual *i* maximizes his utility over two goods, *c* and *h*, where *c* is a consumption good and *h* is health insurance. The price of *h* is *p*, and the price of *c* is normalized to 1. Utility is maximized subject to $c + ph = m$, where *m* is total income. U_1 and U_2 are two representative indifference curves that correspond to this initial budget constraint (solid black line).
The government offers health insurance h_g, (illustrated by point A). The new budget constraint is the red dashed line. Those who would have originally purchased little or no health insurance in the private market consume the bundle (m, h_g), and locate on the higher indifference curve U_1^*.

Figure 2.2 Model with government health insurance, where health insurance is modeled as discrete instead of continuous



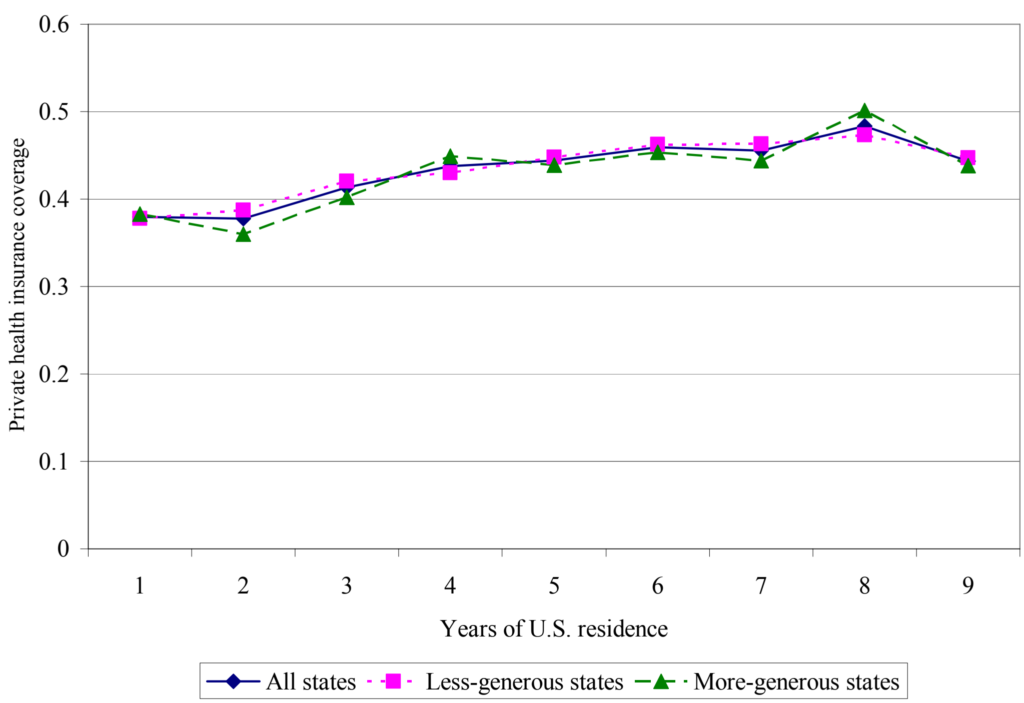
Notes: Health insurance is a discrete good, with only one private option, h_p, with price of p. Points B and C are the budget constraint. Each individual i chooses the greater of $u_i(m - p, h_p)$ and $u_i(m, 0)$. There are two types of individuals. Type H has a stronger preference for health insurance, while type L has a weaker preference for health insurance. The government introduces a free health insurance plan h_g, where $h_p > h_g > 0$; then the budget constraint becomes points B and D. Type L locates at point D (on the higher indifference curve U_L^*), but type H will remain at B.

Figure 2.3 Medicaid coverage among post-PRWORA non-citizen immigrants by years of U.S. residence



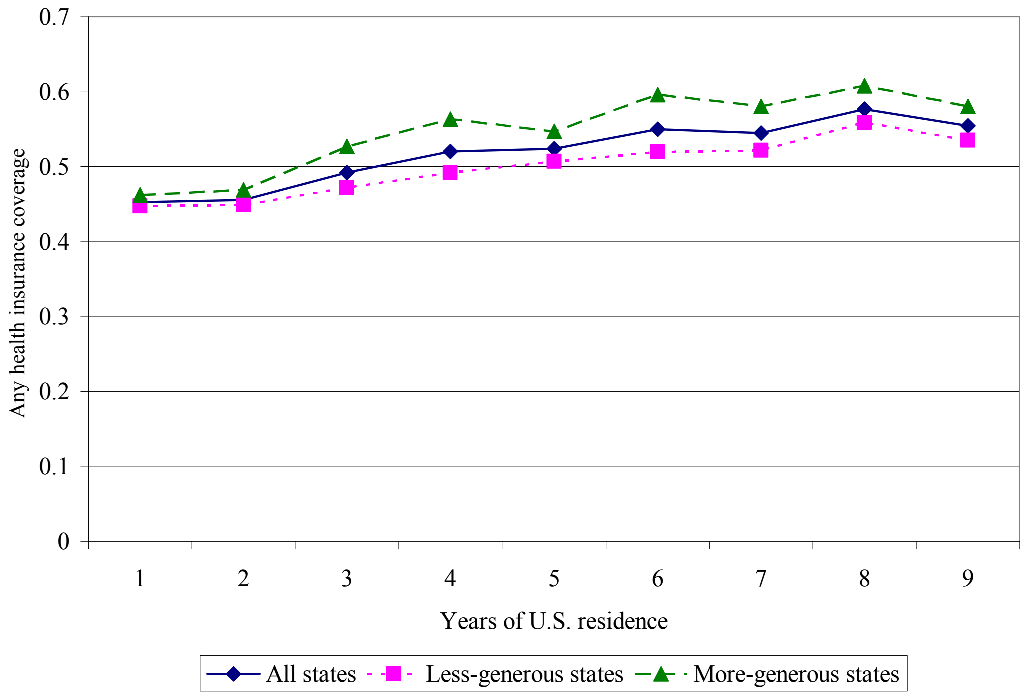
Non-citizen immigrants who are 15 years old or older, from CPS 1998-2006.

Figure 2.4 Private health insurance coverage among post-PRWORA non-citizen immigrants by years of U.S. residence



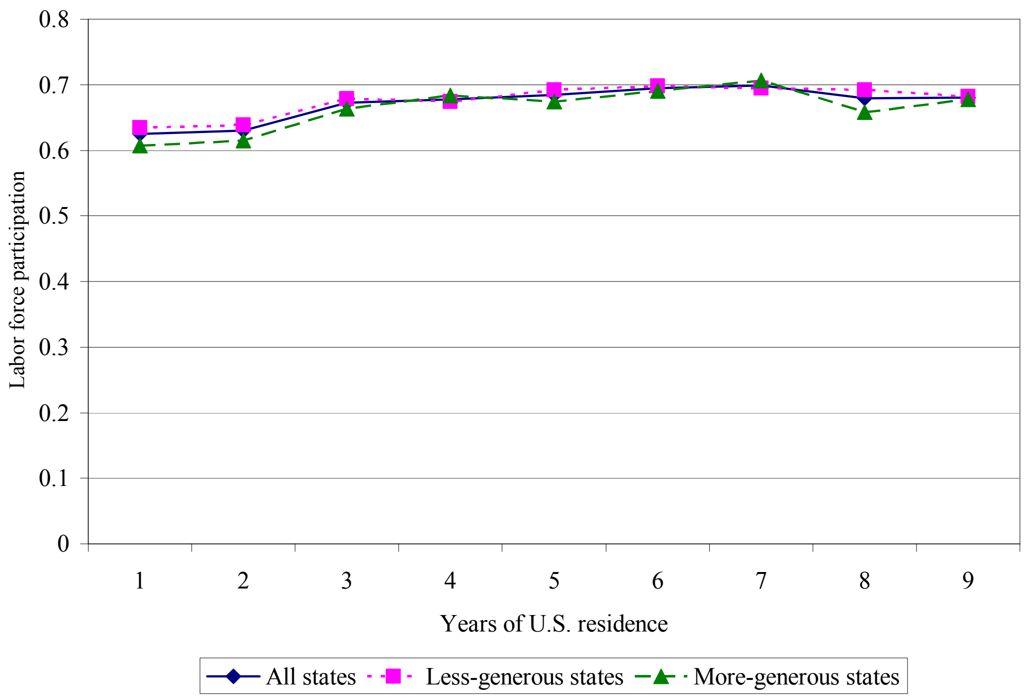
Non-citizen immigrants who are 15 years old or older, from CPS 1998-2006.

Figure 2.5 Overall health insurance coverage among post-PRWORA non-citizen immigrants by years of U.S. residence



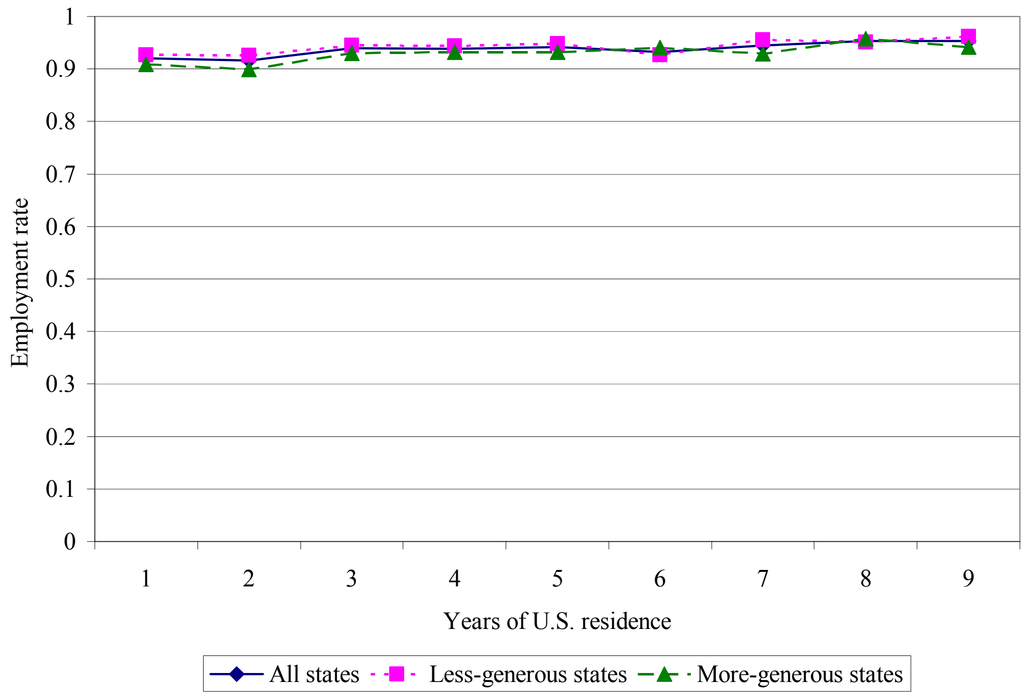
Non-citizen immigrants who are 15 years old or older, from CPS 1998-2006.

Figure 2.6 Labor force participation among post-PRWORA non-citizen immigrants by years of U.S. residence



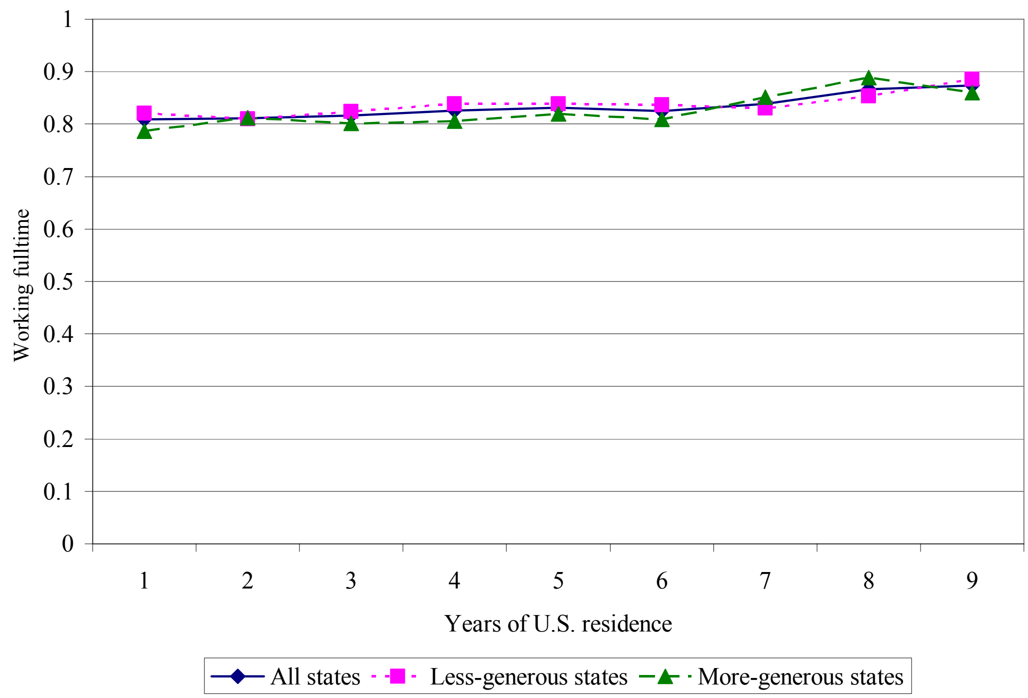
Non-citizen immigrants who are 15 years old or older, from CPS 1998-2006.

Figure 2.7 Employment among post-PRWORA non-citizen immigrants by years of U.S. residence (if in the labor force)



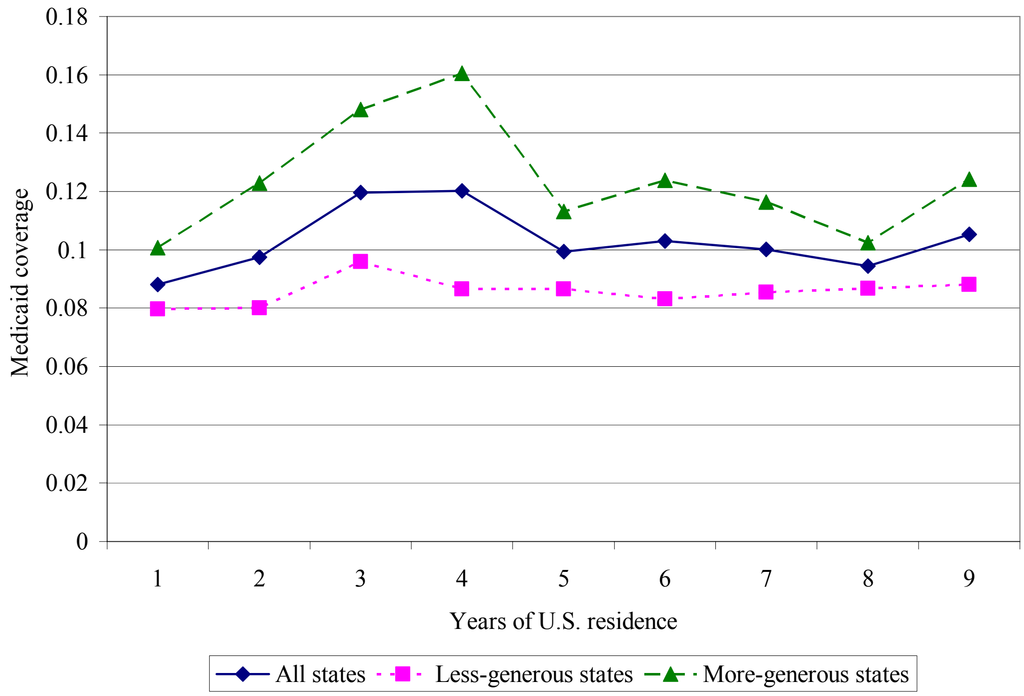
Non-citizen immigrants who are 15 years old or older, from CPS 1998-2006.

Figure 2.8 Full-time work (>35 hours per week) among post-PRWORA non-citizen immigrants by years of U.S. residence (if employed)



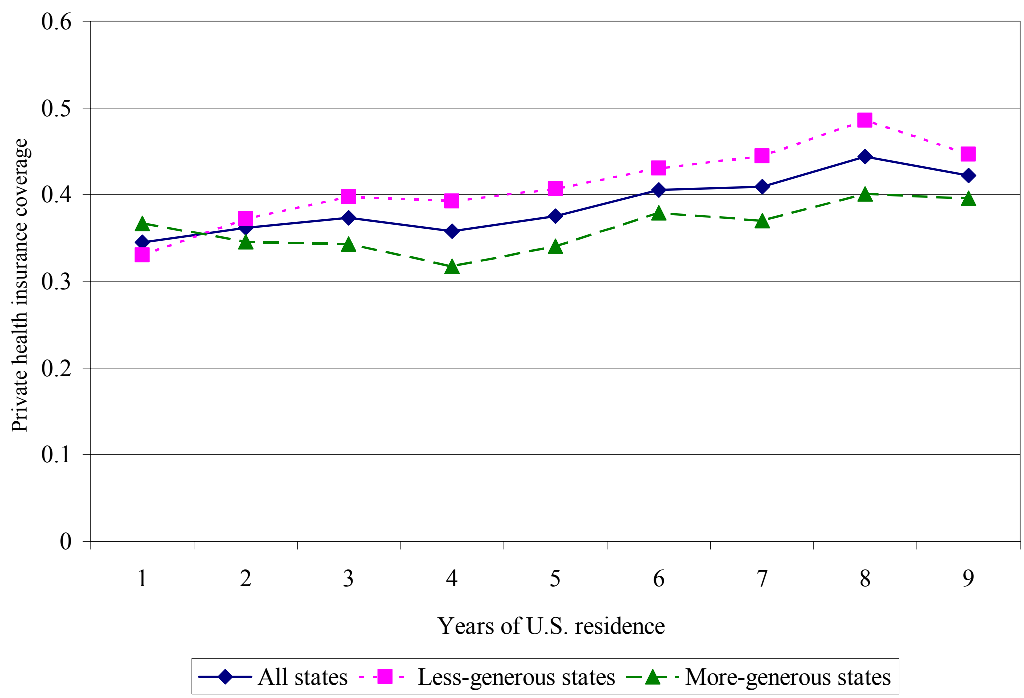
Non-citizen immigrants who are 15 years old or older, from CPS 1998-2006.

Figure 2.9 Medicaid coverage among pre-PRWORA non-citizen immigrants by years of U.S. residence



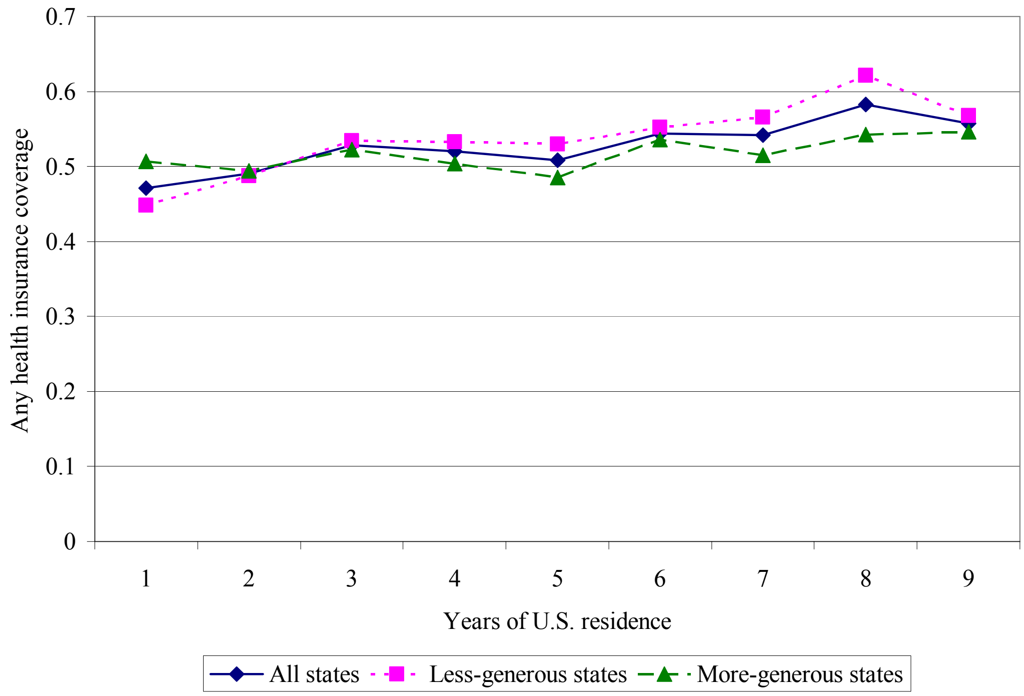
Non-citizen immigrants who are 15 years old or older, from CPS 1994-1996.

Figure 2.10 Private health insurance coverage among pre-PRWORA non-citizen immigrants by years of U.S. residence



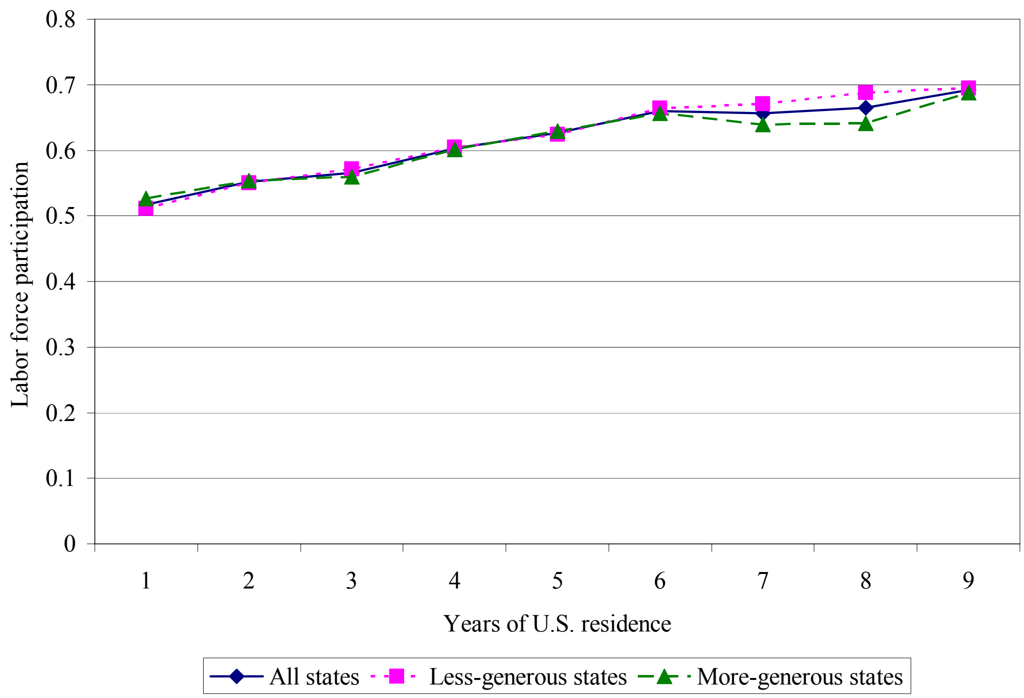
Non-citizen immigrants who are 15 years old or older, from CPS 1994-1996.

Figure 2.11 Overall health insurance coverage among pre-PRWORA non-citizen immigrants by years of U.S. residence



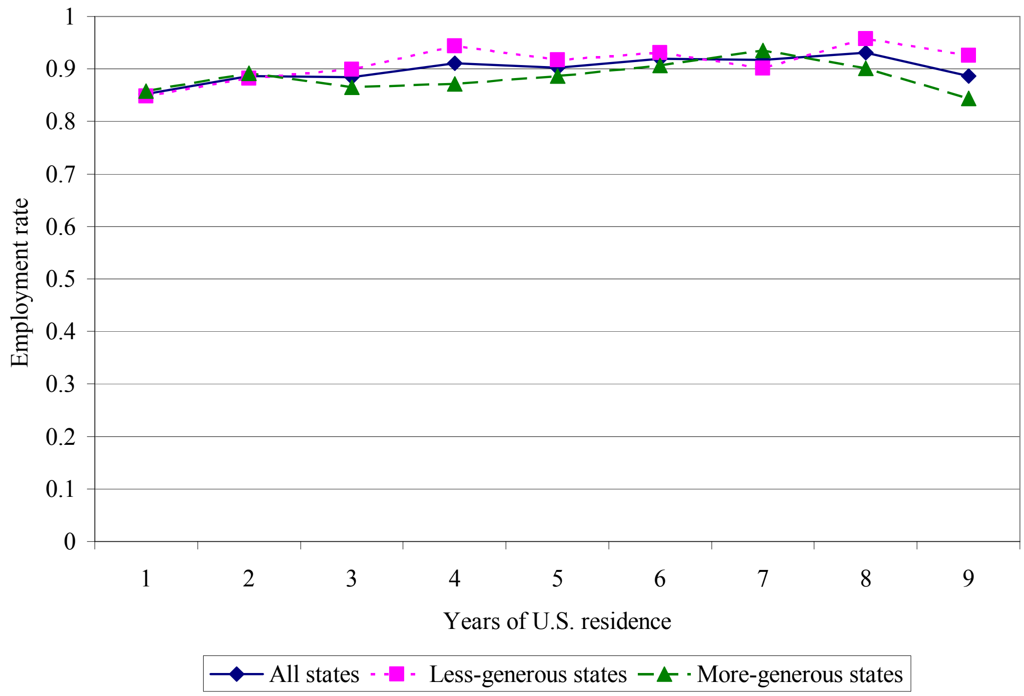
Non-citizen immigrants who are 15 years old or older, from CPS 1994-1996.

Figure 2.12 Labor force participation among pre-PRWORA non-citizen immigrants by years of U.S. residence



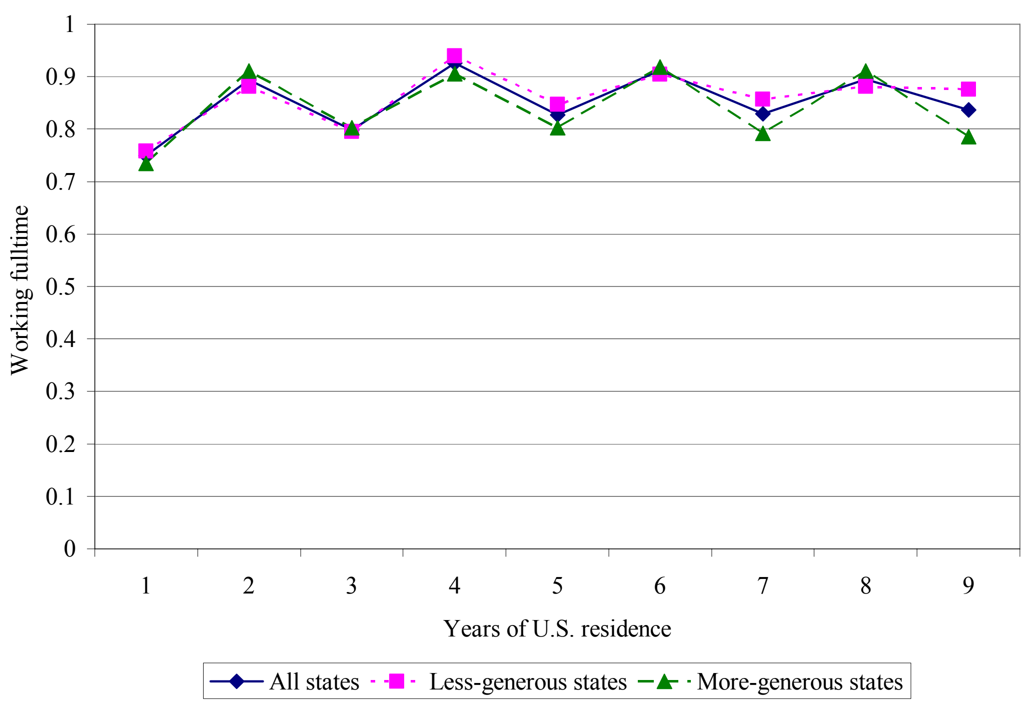
Non-citizen immigrants who are 15 years old or older, from CPS 1994-1996.

Figure 2.13 Employment among pre-PRWORA non-citizen immigrants by years of U.S. residence (if in the labor force)



Non-citizen immigrants who are 15 years old or older and report being in the labor force, from CPS 1994-1996.

Figure 2.14 Full-time work (>35 hours per week) among pre-PRWORA non-citizen immigrants by years of U.S. residence (if employed)



Non-citizen immigrants who are 15 years old or older who reported being employed, from CPS 1994-1996.

TABLES

Table 2.1 More generous states provide Medicaid coverage to otherwise eligible adult non-citizen immigrants who arrived after the 1996 welfare reform and who have been living in the U.S. for less than five years. Less generous states do not provide this coverage.

More Generous	
California	Minnesota
Connecticut	Nebraska
Delaware	New Jersey
District of Columbia	Pennsylvania
Indiana	Rhode Island
Maine	Washington
Massachusetts	

Less Generous	
Alabama	Nevada
Alaska	New Hampshire
Arizona	New Mexico
Arkansas	New York*
Colorado	North Carolina
Florida	North Dakota
Georgia	Ohio
Hawaii	Oklahoma
Idaho	Oregon
Illinois	South Carolina
Iowa	South Dakota
Kansas	Tennessee
Kentucky	Texas
Louisiana	Utah
Maryland	Vermont
Michigan	Virginia
Mississippi	West Virginia
Missouri	Wisconsin
Montana	Wyoming

*Immigrants arriving in the U.S. after PRWORA were initially ineligible to receive Medicaid in the state of New York, until a court ruling in 2001 ended that practice.

Source: Chin K, Dean S, Patchan K. 2002. How Have States Responded to the Eligibility Restrictions on Legal Immigrants in Medicaid and SCHIP? Kaiser Commission on Medicaid and the Uninsured, Washington, DC.

Table 2.2 Variable means for the sample of non-citizen immigrants who arrived in the U.S. after the passage of PRWORA

	Less-Generous States		More Generous States	
Variable	Mean	(Std. Dev.)	Mean	(Std. Dev.)
Years in the U.S.	3.45	(2.22)	3.63	(2.24)
Less Generous State	1.00	(0.00)	0.00	(0.00)
Resident for Five Years	0.32	(0.46)	0.35	(0.48)
Age	32.1	(12.3)	32.7	(12.8)
Female	0.48	(0.50)	0.50	(0.50)
Married	0.49	(0.50)	0.48	(0.50)
No High School	0.24	(0.42)	0.22	(0.41)
High School Drop Out	0.19	(0.39)	0.18	(0.38)
High School Graduate	0.23	(0.42)	0.22	(0.41)
Some College	0.12	(0.33)	0.13	(0.34)
College	0.15	(0.35)	0.15	(0.36)
Advanced Degree	0.08	(0.27)	0.10	(0.30)
Metropolitan Status	0.89	(0.32)	0.97	(0.18)
State Unemployment Rate	4.96	(1.07)	5.34	(0.97)
Labor Force Participation	0.67	(0.47)	0.65	(0.48)
Employed (if in the labor force)	0.94	(0.24)	0.92	(0.27)
Full-Time Work (if employed)	0.83	(0.38)	0.81	(0.39)
Medicaid	0.06	(0.23)	0.11	(0.32)
Private Health Insurance	0.42	(0.49)	0.41	(0.49)
Any Health Insurance	0.48	(0.50)	0.53	(0.50)
AFDC/TANF	0.01	(0.08)	0.01	(0.10)
Food Stamps	0.06	(0.24)	0.06	(0.23)
No. of Observations	**21,834**		**13,324**	

Note: This includes all non-citizen immigrants 15 years old or older, who reported arriving in the US in 1996 or later, from CPS 1998-2006. For all immigrants, 23,243 report being in the labor force, and 21,675 report being employed. For less (more) generous states, 14,529 (8,714) report being in the labor force, and 13,627 (8,048) report being employed.

Table 2.3 Linear probability model for trends in Medicaid coverage

Variable	2.3.1	2.3.2	2.3.3
Y_{istj} (Years in the U.S.)	**0.009***	**0.009***	**0.007***
	(0.003)	**(0.003)**	**(0.003)**
LG_s (Less generous state)	-0.016*	-0.014*	0.011
	(0.007)	(0.007)	(0.063)
R_{istj} (Resident for five years)	0.015	0.009	0.016
	(0.022)	(0.022)	(0.022)
Y_{istj}* LG_s	**-0.012***	**-0.012***	**-0.012***
	(0.003)	**(0.003)**	**(0.003)**
Y_{istj}* R_{istj}	**-0.007**	**-0.005**	**-0.006**
	(0.004)	**(0.004)**	**(0.004)**
LG_s* R_{istj}	-0.055*	-0.054*	-0.054*
	(0.026)	(0.026)	(0.025)
Y_{istj} * LG_s* R_{istj}	**0.015***	**0.015***	**0.014***
	(0.005)	**(0.005)**	**(0.005)**
Age	-	-0.002*	-0.001*
		(0.001)	(0.001)
Age squared	-	0.000*	0.000
		(0.000)	(0.000)
Female	-	0.030*	0.027*
		(0.003)	(0.003)
Married	-	0.010*	0.010*
		(0.003)	(0.003)
No high school	-	0.010*	0.022*
		(0.004)	(0.004)
High school drop out	-	0.020*	0.026*
		(0.004)	(0.004)
Some college	-	-0.011*	-0.013*
		(0.005)	(0.005)
College degree	-	-0.032*	-0.029*
		(0.004)	(0.004)
Advanced degree	-	-0.039*	-0.034*
		(0.004)	(0.005)
Metropolitan area	-	0.008	-0.001
		(0.004)	(0.005)
State U^{RATE}	-	0.002*	0.002
		(0.001)	(0.003)
Fixed effects for state, year, and country of birth	No	No	Yes
R^2	0.0096	0.0228	0.0579
No. observations	35,158	35,158	35,158

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient. Data from the March supplement to the CPS, 1998-2006. * indicates statistical significance at 5 percent.

Table 2.4 Linear probability model for trends in private health insurance coverage

Variable	2.4.1	2.4.2	2.4.3
Y_{istj} (Years in the U.S.)	**0.024***	**0.024***	**0.032***
	(0.005)	**(0.004)**	**(0.004)**
LG_s (Less generous state)	0.020	0.015	0.153
	(0.015)	(0.013)	(0.114)
R_{istj} (Resident for five years)	0.074	0.119*	0.116*
	(0.039)	(0.035)	(0.034)
Y_{istj}* LG_s	**-0.006**	**-0.003**	**-0.007**
	(0.006)	**(0.005)**	**(0.005)**
Y_{istj}* R_{istj}	**-0.019***	**-0.027***	**-0.028***
	(0.008)	**(0.007)**	**(0.007)**
LG_s* R_{istj}	-0.001	-0.021	-0.016
	(0.051)	(0.046)	(0.044)
Y_{istj} * LG_s* R_{istj}	**0.004**	**0.004**	**0.006**
	(0.010)	**(0.009)**	**(0.008)**
Age	-	0.001	0.004*
		(0.001)	(0.001)
Age squared	-	0.000	0.000*
		(0.000)	(0.000)
Female	-	-0.008	-0.018*
		(0.005)	(0.005)
Married	-	0.102*	0.100*
		(0.006)	(0.005)
No high school	-	-0.148*	-0.065*
		(0.007)	(0.007)
High school drop out	-	-0.059*	-0.015
		(0.008)	(0.008)
Some college	-	0.138*	0.074*
		(0.010)	(0.009)
College degree	-	0.302*	0.181*
		(0.009)	(0.009)
Advanced degree	-	0.420*	0.272*
		(0.010)	(0.011)
Metropolitan area	-	-0.044*	-0.039*
		(0.009)	(0.010)
State U^{RATE}	-	-0.022*	-0.008
		(0.002)	(0.006)
Fixed effects for state, year, and country of birth	No	No	Yes
R^2	0.0044	0.173	0.2429
No. observations	35,158	35,158	35,158

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient. Data from the March supplement to the CPS, 1998-2006. * indicates statistical significance at 5 percent.

Table 2.5 Linear probability model for trends in overall health insurance coverage

Variable	2.5.1	2.5.2	2.5.3
Y_{istj} (Years in the U.S.)	**0.035***	**0.036***	**0.042***
	(0.005)	**(0.004)**	**(0.004)**
LG_s (Less generous state)	0.007	0.009	0.229*
	(0.015)	(0.014)	(0.102)
R_{istj} (Resident for five years)	0.075	0.125*	0.128*
	(0.039)	(0.036)	(0.035)
Y_{istj}* LG_s	**-0.020***	**-0.019***	**-0.022***
	(0.006)	**(0.006)**	**(0.005)**
Y_{istj}* R_{istj}	**-0.023***	**-0.032***	**-0.034***
	(0.008)	**(0.007)**	**(0.007)**
LG_s* R_{istj}	-0.046	-0.076	-0.068
	(0.050)	(0.047)	(0.045)
Y_{istj} * LG_s* R_{istj}	**0.018**	**0.021***	**0.021***
	(0.010)	**(0.009)**	**(0.009)**
Age	-	-0.015*	-0.012*
		(0.001)	(0.001)
Age squared	-	0.000*	0.000*
		(0.000)	(0.000)
Female	-	0.028*	0.015*
		(0.005)	(0.005)
Married	-	0.125*	0.123*
		(0.006)	(0.006)
No high school	-	-0.138*	-0.041*
		(0.008)	(0.008)
High school drop out	-	-0.047*	0.003
		(0.009)	(0.008)
Some college	-	0.130*	0.062*
		(0.010)	(0.009)
College degree	-	0.270*	0.151*
		(0.009)	(0.009)
Advanced degree	-	0.377*	0.234*
		(0.009)	(0.010)
Metropolitan area	-	-0.037*	-0.036*
		(0.010)	(0.010)
State U^{RATE}	-	-0.019*	-0.008
		(0.002)	(0.006)
Fixed effects for state, year, and country of birth	No	No	Yes
R^2	0.0079	0.149	0.2245
No. observations	35,158	35,158	35,158

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient. Data from the March supplement to the CPS, 1998-2006. * indicates statistical significance at 5 percent.

Table 2.6 The effect of an additional year in the U.S. on Medicaid, private health insurance, and overall health insurance coverage – coefficients calculated from the estimates in Tables 2.3-2.5

	Less than five years residence		More than five years residence	
	More generous state	Less generous state	More generous state	Less generous state
	2.6.1	2.6.2	2.6.3	2.6.4
Medicaid (Table 2.3.3)	0.0074* (0.0027)	-0.0041* (0.0015)	0.0012 (0.0033)	0.0039 (0.0022)
Private insurance (Table 2.4.3)	0.0315* (0.0042)	0.0243* (0.0033)	0.0038 (0.0051)	0.0025 (0.0044)
Overall insurance (Table 2.5.3)	0.0418* (0.0043)	0.0195* (0.0034)	0.0076 (0.0052)	0.0061 (0.0044)

Note: Robust standard errors, clustered by individual, are reported in parentheses below the trends.
*indicates statistically significantly different from zero at the 5 percent level.
In the column 2.6.1, the reported trend is the coefficient on $\boldsymbol{Y_{istj}}$, the number of years spent in the U.S.
In column 2.6.2, the reported trend is the sum of the coefficient on $\boldsymbol{Y_{istj}}$ and the coefficient on $\boldsymbol{Y_{istj} * LG_s}$, the interaction between the number of years in the U.S. and the indicator for living in a less-generous state.
In column 2.6.3, the reported trend is the sum of the coefficient on $\boldsymbol{Y_{istj}}$ and the coefficient on $\boldsymbol{Y_{istj} * R_{istj}}$, the interaction between the number of years in the U.S. and the indicator for having reached the five-year residence requirement.
In column 2.6.4, the reported trend is the sum of the coefficient on $\boldsymbol{Y_{istj}}$, the coefficient on $\boldsymbol{Y_{istj} * LG_s}$, the coefficient on $\boldsymbol{Y_{istj} * R_{istj}}$, and the coefficient on $\boldsymbol{Y_{istj} * LG_s * R_{istj}}$.
The coefficients in columns 2.6.1 and 2.6.2 are statistically significantly different from one another for Medicaid and Overall insurance. The coefficients in 2.6.3 and 2.6.4 are not significantly different from one another.

Table 2.7 Linear probability model for trends in labor force participation

Variable	2.7.1	2.7.2	2.7.3
Y_{istj} (Years in the U.S.)	**0.027***	**0.023***	**0.021***
	(0.005)	**(0.004)**	**(0.004)**
LG_s (Less generous state)	0.040*	0.017	-0.216*
	(0.015)	(0.013)	(0.089)
R_{istj} (Resident for five years)	0.092*	0.079*	0.072*
	(0.037)	(0.032)	(0.033)
Y_{istj}* LG_s	**-0.011**	**-0.006**	**-0.008**
	(0.006)	**(0.005)**	**(0.005)**
Y_{istj}* R_{istj}	**-0.025***	**-0.023***	**-0.020***
	(0.007)	**(0.006)**	**(0.006)**
LG_s* R_{istj}	-0.008	0.022	-0.010
	(0.048)	(0.042)	(0.042)
Y_{istj} * LG_s* R_{istj}	**0.007**	**0.001**	**0.007**
	(0.009)	**(0.008)**	**(0.008)**
Age	-	0.049*	0.048*
		(0.001)	(0.001)
Age squared	-	-0.001*	-0.001*
		(0.000)	(0.000)
Female	-	-0.332*	-0.326*
		(0.005)	(0.005)
Married	-	-0.091*	-0.086*
		(0.005)	(0.005)
No high school	-	-0.062*	-0.092*
		(0.007)	(0.007)
High school drop out	-	-0.096*	-0.109*
		(0.008)	(0.008)
Some college	-	-0.056*	-0.026*
		(0.009)	(0.009)
College degree	-	-0.036*	0.019*
		(0.008)	(0.009)
Advanced degree	-	-0.019*	0.050*
		(0.009)	(0.010)
Metropolitan area	-	-0.002	0.004
		(0.009)	(0.009)
State U^{RATE}	-	-0.010*	-0.012*
		(0.002)	(0.006)
Fixed effects for state, year, and country of birth	No	No	Yes
R^2	0.0036	0.2226	0.2478
No. observations	35,139	35,139	35,139

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient. Data from the March supplement to the CPS, 1998-2006. * indicates statistical significance at 5 percent.

Table 2.8 Linear probability model for trends in employment (if in labor force)

Variable	2.8.1	2.8.2	2.8.3
Y_{istj} (Years in the U.S.)	**0.011***	**0.011***	**0.013***
	(0.003)	**(0.003)**	**(0.003)**
LG_s (Less generous state)	0.025*	0.021*	0.044
	(0.010)	(0.010)	(0.095)
R_{istj} (Resident for five years)	0.026	0.036	0.039
	(0.023)	(0.023)	(0.024)
Y_{istj}* LG_s	**-0.003**	**-0.003**	**-0.003**
	(0.004)	**(0.004)**	**(0.004)**
Y_{istj}* R_{istj}	**-0.008**	**-0.010***	**-0.012***
	(0.005)	**(0.005)**	**(0.005)**
LG_s* R_{istj}	-0.020	-0.017	-0.012
	(0.029)	(0.029)	(0.029)
Y_{istj} * LG_s* R_{istj}	**0.004**	**0.004**	**0.003**
	(0.006)	**(0.006)**	**(0.006)**
Age	-	0.006*	0.006*
		(0.001)	(0.001)
Age squared	-	0.000*	0.000*
		(0.000)	(0.000)
Female	-	-0.034*	-0.033*
		(0.004)	(0.004)
Married	-	-0.005	-0.006
		(0.004)	(0.004)
No high school	-	-0.020*	-0.022*
		(0.005)	(0.006)
High school drop out	-	-0.023*	-0.025*
		(0.006)	(0.006)
Some college	-	0.007	0.011
		(0.006)	(0.006)
College degree	-	0.016*	0.017*
		(0.005)	(0.006)
Advanced degree	-	0.025*	0.021*
		(0.005)	(0.006)
Metropolitan area	-	-0.008	-0.002
		(0.006)	(0.006)
State U^{RATE}	-	-0.010*	-0.011*
		(0.002)	(0.004)
Fixed effects for state, year, and country of birth	No	No	Yes
R^2	0.0027	0.0148	0.0291
No. observations	23,243	23,243	23,243

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient. Data from the March supplement to the CPS, 1998-2006. * indicates statistical significance at 5 percent.

Table 2.9 Linear probability model for trends in full-time work (if employed)

Variable	2.9.1	2.9.2	2.9.3
Y_{istj} (Years in the U.S.)	**0.003**	**0.000**	**0.003**
	(0.005)	**(0.005)**	**(0.005)**
LG_s (Less generous state)	0.012	0.005	0.288*
	(0.016)	(0.015)	(0.086)
R_{istj} (Resident for five years)	-0.056	-0.051	-0.032
	(0.036)	(0.035)	(0.036)
Y_{istj}* LG_s	**0.004**	**0.005**	**0.005**
	(0.006)	**(0.006)**	**(0.006)**
Y_{istj}* R_{istj}	**0.012**	**0.011**	**0.008**
	(0.007)	**(0.007)**	**(0.007)**
LG_s* R_{istj}	0.050	0.068	0.052
	(0.046)	(0.044)	(0.045)
Y_{istj} * LG_s* R_{istj}	**-0.013**	**-0.016**	**-0.013**
	(0.009)	**(0.009)**	**(0.009)**
Age	-	0.028*	0.028*
		(0.002)	(0.002)
Age squared	-	0.000*	0.000*
		(0.000)	(0.000)
Female	-	-0.155*	-0.149*
		(0.006)	(0.006)
Married	-	0.002	0.009
		(0.006)	(0.006)
No high school	-	-0.018*	-0.036*
		(0.007)	(0.008)
High school drop out	-	-0.038*	-0.049*
		(0.008)	(0.009)
Some college	-	-0.080*	-0.066*
		(0.010)	(0.010)
College degree	-	-0.028*	0.000
		(0.009)	(0.009)
Advanced degree	-	-0.008	0.041*
		(0.009)	(0.011)
Metropolitan area	-	0.019	0.002
		(0.010)	(0.011)
State U^{RATE}	-	-0.004	-0.015*
		(0.003)	(0.006)
Fixed effects for state, year, and country of birth	No	No	Yes
R^2	0.0019	0.0663	0.0931
No. observations	21,675	21,675	21,675

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient. Data from the March supplement to the CPS, 1998-2006. * indicates statistical significance at 5 percent.

Table 2.10 The effect of an additional year in the U.S. on labor force participation, being employed, and full-time work – coefficients calculated from the estimates in Tables 2.7-2.9

	Less than five years residence		More than five years residence	
	More generous state	Less generous state	More generous state	Less generous state
	2.10.1	2.10.2	2.10.3	2.10.4
Labor force participation (Table 2.7.3)	0.0212* (0.0041)	0.0130* (0.0032)	0.0007 (0.0048)	-0.0003 (0.0041)
Employed (Table 2.8.3)	0.0131* (0.0033)	0.0103* (0.0023)	0.0013 (0.0034)	0.0012 (0.0025)
Full-time Work (Table 2.9.3)	0.0030 (0.0049)	0.0077* (0.0035)	0.0108* (0.0052)	0.0029 (0.0042)

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient.
*indicates statistically significantly different from zero at the 5 percent level.
In the column 2.10.1, the reported trend is the coefficient on $\boldsymbol{Y_{istj}}$, the number of years spent in the U.S.
In column 2.10.2, the reported trend is the sum of the coefficient on $\boldsymbol{Y_{istj}}$ and the coefficient on $\boldsymbol{Y_{istj} * LG_s}$, the interaction between the number of years in the U.S. and the indicator for living in a less-generous state.
In column 2.10.3, the reported trend is the sum of the coefficient on $\boldsymbol{Y_{istj}}$ and the coefficient on $\boldsymbol{Y_{istj} * R_{istj}}$, the interaction between the number of years in the U.S. and the indicator for having reached the five-year residence requirement.
In column 2.10.4, the reported trend is the sum of the coefficient on $\boldsymbol{Y_{istj}}$, the coefficient on $\boldsymbol{Y_{istj} * LG_s}$, the coefficient on $\boldsymbol{Y_{istj} * R_{istj}}$, and the coefficient on $\boldsymbol{Y_{istj} * LG_s * R_{istj}}$.
The coefficients in column 2.10.1 are not statistically significantly different from those in 2.10.2, and the coefficients in 2.10.3 are not significantly different from those in 2.10.4.

Table 2.11 The effect of an additional year in the U.S. on Medicaid, private health insurance, and overall health insurance coverage – excluding those born in Mexico

	Less than five years residence		More than five years residence	
	More generous state	Less generous state	More generous state	Less generous state
	2.11.1	2.11.2	2.11.3	2.11.4
Medicaid (N = 21,907)	0.0091* (0.0031)	-0.0077* (0.0020)	-0.0037 (0.0037)	0.0066* (0.0029)
Private insurance (N = 21,907)	0.0340* (0.054)	0.0373* (0.0045)	0.0165* (0.066)	0.0057 (0.0060)
Overall insurance (N = 21,907)	0.0463* (0.0053)	0.0294* (0.0044)	0.0151* (0.0064)	0.0116* (0.0058)
Labor force participation (N = 21,889)	0.0353* (0.0051)	0.0273* (0.0043)	0.0016 (0.0061)	0.0006 (0.0056)
Employed (N = 14,281)	0.0068 (0.0038)	0.0115* (0.0030)	0.0039 (0.0044)	0.0046 (0032)
Full-time work (N = 13,365)	-0.0040 (0.0060)	0.0079 (0.0048)	0.0124 (0.0064)	0.0057 (0.0056)

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient.
*indicates statistically significantly different from zero at the 5 percent level.
Sample is foreign-born non-citizens who arrived in or after 1996, excluding those born in Mexico.
In the column 2.11.1, the reported trend is the coefficient on $\boldsymbol{Y_{istj}}$, the number of years spent in the U.S.
In column 2.11.2, the reported trend is the sum of the coefficient on $\boldsymbol{Y_{istj}}$ and the coefficient on $\boldsymbol{Y_{istj} * LG_s}$, the interaction between the number of years in the U.S. and the indicator for living in a less-generous state.
In column 2.11.3, the reported trend is the sum of the coefficient on $\boldsymbol{Y_{istj}}$ and the coefficient on $\boldsymbol{Y_{istj} * R_{istj}}$, the interaction between the number of years in the U.S. and the indicator for having reached the five-year residence requirement.
In column 2.11.4, the reported trend is the sum of the coefficient on $\boldsymbol{Y_{istj}}$, the coefficient on $\boldsymbol{Y_{istj} * LG_s}$, the coefficient on $\boldsymbol{Y_{istj} * R_{istj}}$, and the coefficient on $\boldsymbol{Y_{istj} * LG_s * R_{istj}}$.
The coefficients in columns 2.11.1 and 2.11.2 are only statistically significantly different from one another for Medicaid and Overall insurance. The coefficients in 2.11.3 and 2.11.4 are only significantly different from one another for Medicaid.

Table 2.12 The effect of an additional year in the U.S. on Medicaid, private health insurance, and overall health insurance coverage – less-educated females

	Less than five years residence		More than five years residence	
	More generous state	Less generous state	More generous state	Less generous state
	2.12.1	2.12.2	2.12.3	2.12.4
Medicaid (N = 10,535)	0.0074 (0.0061)	-0.0020 (0.0035)	0.0026 (0.0073)	0.0060 (0.0047)
Private insurance (N = 10,535)	0.0215* (0.0076)	0.0249* (0.0060)	-0.0070 (0.0093)	0.0002 (0.0081)
Overall insurance (N = 10,535)	0.0299* (0.0084)	0.0189* (0.0064)	0.0002 (0.0102)	0.0021 (0.0085)
Labor force participation (N = 10,532)	0.0196* (0.0088)	0.0066 (0.0068)	-0.0095 (0.0104)	0.0099 (0.0088)
Employed (N = 4,648)	0.0026 (0.0086)	0.0048 (0.0064)	-0.0086 (0.0103)	-0.0010 (0.0078)
Full-time work (N = 4,171)	0.0135 (0.0136)	-0.0002 (0.0098)	0.0094 (0.0147)	-0.0086 (0.00122)

Note: Robust standard errors, clustered by individual, are reported in parentheses below the coefficient.
*indicates statistically significantly different from zero at the 5 percent level.
Sample is foreign-born female non-citizens who have no more than a high school diploma.
In the column 2.12.1, the reported trend is the coefficient on $\boldsymbol{Y_{istj}}$, the number of years spent in the U.S.
In column 2.12.2, the reported trend is the sum of the coefficient on $\boldsymbol{Y_{istj}}$ and the coefficient on $\boldsymbol{Y_{istj} * LG_s}$, the interaction between the number of years in the U.S. and the indicator for living in a less-generous state.
In column 2.12.3, the reported trend is the sum of the coefficient on $\boldsymbol{Y_{istj}}$ and the coefficient on $\boldsymbol{Y_{istj} * R_{istj}}$, the interaction between the number of years in the U.S. and the indicator for having reached the five-year residence requirement.
In column 2.12.4, the reported trend is the sum of the coefficient on $\boldsymbol{Y_{istj}}$, the coefficient on $\boldsymbol{Y_{istj} * LG_s}$, the coefficient on $\boldsymbol{Y_{istj} * R_{istj}}$, and the coefficient on $\boldsymbol{Y_{istj} * LG_s * R_{istj}}$.
The coefficients in columns 2.12.1 and 2.12.2 are not statistically significantly different from one another at the five percent level. The coefficients in 2.12.3 and 2.12.4 are not significantly different from one another at the five percent level.

Table 2.13 Participation rates in means-tested government welfare programs for newly arrived immigrants in the U.S.

	Percent Participating in Means-Tested Programs			
	Medicaid	Food Stamps	TANF	No. obs.
Less-educated females	11.0	9.7	2.2	1,566
Less-educated males	4.2	6.8	0.7	1,873
More-educated females	2.5	3.0	0.7	1,019
More-educated males	2.5	2.4	0.6	970

Note: Data from the March supplement to the CPS, 1998-2000. Sample includes all foreign-born, both citizens and non-citizens, ages 15 or older, who entered the U.S. in 1996-99 and are in their first or second year of U.S. residence. Less-educated refers to those with at most a high school degree. More-educated refers to those with at least some college education. The Food Stamp variable is a family-based variable.

Table 2.14 Percentage of different immigrant populations residing in less generous states

	Percent Living in Less Generous States	
	1-2 Years of U.S. Residence	6-7 Years of U.S. Residence
Less-educated females	64.9	64.1
Less-educated males	68.2	64.5
More-educated females	65.0	61.3
More-educated males	66.8	58.0

Note: Data from the March supplement to the CPS, 1998-2006. Sample includes all foreign-born, both citizens and non-citizens, ages 15 or older, who entered the U.S. in 1996-99. Less-educated refers to those with at most a high school degree. More-educated refers to those with at least some college education. The fractions in this table are calculated as, for example, the number of less-educated females living in less generous states divided by the number of less-educated females living in all states.

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Chapter 3

The Value of a Green Card: Immigrant Wage Increases following Adjustment to U.S. Permanent Residence

1 Introduction

Highly-skilled foreign-born workers are in great demand in the U.S. labor market. In each of the last five fiscal years, the Congressionally-mandated quota of H-1B visas (a temporary work visa for foreign workers with specialized skills) has been exhausted, typically within days after the visas become available (National Foundation for American Policy 2007b). In addition to visa limits, employers looking for skilled foreign workers face significant financial costs. Hiring foreign-born workers can be much more expensive than hiring native workers. The National Foundation for American Policy (2007b) estimates that employers pay close to $6,000 in legal and processing fees for each foreign national they hire on a temporary employment-based visa (such as an H-1B visa). In addition, there are likely to be many other costs associated with recruiting foreign-born workers, such as international travel for interviews, international relocation costs, etc. Given these higher costs, we would expect to find that employers offer foreign-born workers lower wages than similarly skilled native workers, in order to offset the additional costs they incur to hire the foreign-born workers.

On the labor supply side, high-skilled foreign born workers who desire to live and work permanently in the U.S., and who apply to become permanent residents of the U.S., may be willing to receive lower wages than similar natives due to the process they must undergo to become immigrants.[1] There are at least two potential sources of job market friction faced by foreign-born workers who desire to be legal permanent residents. The first comes from the aforementioned costs that employers must pay to hire foreign-born workers, limiting these workers to finding employment with companies who are willing to undergo those additional costs.

The second source of job friction comes from the process by which a foreign-born worker becomes a permanent resident. Most employment-based immigrants wait at least five years from the time they apply to become permanent residents to the time they receive their green cards (National Foundation for American Policy 2007a), but there are often time limits on temporary employment visas. For one common example, the time limit on an H-1B visa is six years.[2] Though the H-1B visa can be transferred from company to company (the six year limit follows the worker, not the position), green card applications cannot be transferred to another employer. To change jobs after they have applied for permanent residence but before they have received their green cards means that employment-based immigrants must restart the entire process. Foreign-born workers risk losing their place in the green card queue, or even not being able to get a green card, if they change jobs before they have completed the process to become legal permanent residents.

[1] Technically, an "immigrant" is defined by the U.S. Citizenship and Immigration Service as someone who holds a permanent resident visa (green card). All other non-citizens in the U.S., such as tourists, international students, and those with work visas, are considered to be "temporary aliens."
[2] The initial period for the H-1B visa is three years, and it can be renewed once for three additional years. Holders of H-1B visas are not required to return to their country of origin before adjusting to permanent residence. This is not true of some other temporary visas, such as J visas for visiting scholars.

The receipt of a green card enables a foreign national to work for any employer without the need for that employer to apply for a visa or a green card on his behalf. Thus, these immigrants can more easily move from job to job. Sponsoring employers who want to retain the new permanent resident workers may need to compete with other employers and potentially increase the wages they offer to their immigrant workers.

To estimate the wage premium associated with the increase in job mobility when an employer-sponsored immigrant becomes a legal permanent resident, I use the recently released New Immigrant Survey (Jasso, Massey, Rosenzweig, and Smith 2006), which provides wage observations for employer-sponsored immigrants both before and after they have become legal permanent residents. Once employer-sponsored immigrants have received their green cards, they have are just as unrestricted in the labor market as are native U.S. workers. I construct a control group of otherwise similar native workers using the Merged Outgoing Rotation Groups of the Current Population Survey. To determine the effect of receiving a green card on the wages of employer-sponsored immigrants, I employ a difference-in-differences matching estimator (Blundell and Costa Dias 2002).

The following section presents a theoretical framework where the costs born by employers on behalf of their foreign-born workers drive the initial wage differences between immigrants and natives. Additionally, I discuss the significant differences between the immigrant and native populations that motivate the use of the matching strategy. In the third section, I describe the propensity score matching estimator that I use to construct an appropriate control group for the employer-sponsored immigrants from among native workers and the difference-in-differences strategy I implement to compare their wages. Section 4 describes both the New Immigrant Survey and the Merged Outgoing Rotation

Groups of the Current Population Survey that I use for the analysis. In the fifth section, I present the empirical results, which show that employer-sponsored immigrants experience at least a 13.5 percent wage increase (over and above the wage increase of similar native workers) when they receive their green cards. I also demonstrate that employer-sponsored immigrants who change jobs experience larger wage increases than those who stay in the same job, supporting the hypothesis that the wage increases are due to greater job mobility. In Section 6, I present evidence that the wage increases experienced by employer-sponsored immigrants are not likely driven by the time spent in the U.S., since their post-green card wages are similar to those of employer-sponsored immigrants who arrived in the U.S. with green cards. The final section concludes.

2 Theoretical Framework

Consider two identical (in terms of skills or marginal product of labor) workers ($i = N, F$), where N is a native-born worker, and F is a foreign-born worker, employed in two time periods ($t = 0, 1$). In the first period ($t = 0$), both workers receive the same total compensation $\overline{C_0}$ in exchange for their labor. This compensation involves two components:

$$\overline{C_0} = w_{i0} + b_{i0},$$

where w_{i0} is the wage and b_{i0} is a sponsorship benefit for individual i. The sponsorship benefit b_{i0} represents the costs that an employer undertakes in order to hire a foreign-born worker and sponsor that worker for a legal permanent resident visa. Worker N has no need of the sponsorship benefit, and so prefers $b_{N0} = 0$, while the worker F would like to be able to continue to live and work in the United States, and so prefers $b_{F0} > 0$. Since the two workers receive the same compensation, $\overline{C_0}$, N will have higher wages than F:

$$\overline{C_0} = w_{N0} + b_{N0} = w_{F0} + b_{F0}$$

$$b_{N0} = 0, \ \ b_{F0} > 0, \ \Rightarrow$$

$$w_{N0} > w_{F0}.$$

In the second period (t = 1), F has become a legal permanent resident, and no longer desires the sponsorship benefit. Now both N and F prefer $b_1 = 0$. The two workers still receive the same total compensation, which now consists only of wages for both workers:

$$\overline{C_1} = w_{N1} + b_{N1} = w_{F1} + b_{F1},$$

$$b_{N1}, b_{F1} = 0,$$

$$\overline{C_1} = w_{N1} = w_{F1}.$$

Because F prefers $b_{F0} > 0$ in the first period (t = 0) but then prefers $b_{F0} = 0$ in the second period (t = 1), the wage change experienced by F across the two time periods will be larger than the wage change experienced by N:

$$(w_{F1} - w_{F0}) > (w_{N1} - w_{N0}).$$

To estimate the wage premium associated with receiving a green card for immigrants who are sponsored by their employers, I calculate the difference between the wage change for the native worker and the wage change for the foreign-born worker:

$$GreenCardValue = (w_{F1} - w_{F0}) - (w_{N1} - w_{N0})$$

which can be rearrange and written as:

(1) $$GreenCardValue = (w_{F1} - w_{N1}) - (w_{F0} - w_{N0}).$$

The primary complication associated with estimating this value of a green card is finding an appropriate group of native workers that are otherwise identical to the employer-sponsored immigrants. On average, employer-sponsored immigrants are quite dissimilar from the native population in the U.S. in ways that would be expected to affects wages. However, to calculate the wage premium of a green card requires that the two types of workers be viewed as substitutes by employers, such that they would receive identical compensation.

A comparison of employer-sponsored immigrants from the New Immigrant Survey and native workers from the Merged Outgoing Rotation Groups of the Current Population Survey highlights the differences between these two populations (see Table 3.1). For examples, less than a quarter of employer-sponsored immigrants are female, while over half of native workers are female. The contrasts in the education profiles of these two groups are particularly striking. Forty percent of employer-sponsored immigrants have a bachelor's degree, which is more than twice the percentage among native workers (18.9 percent). Another forty percent of the employer-sponsored immigrants have obtained graduate degrees, compared to less than ten percent of native workers. Naturally, differences in schooling lead to significant differences between the wage profiles of the two groups.

On average, the native workers are older than the employer-sponsored immigrants. The median age for the employer-sponsored immigrants is between 35 and 39 in 2004, compared to a median age between 50 and 54 for the native workers. This would tend to narrow the wage gap between the two groups, as native workers will have more experience, which is rewarded in the labor market. Employer-sponsored immigrants are more geographically concentrated than natives, with 47.5 percent of them located in the six traditional gateway states (California, Florida, Illinois, New Jersey, New York, and Texas); only 29.5 percent of native workers live in those six states. The top industries where employer-sponsored immigrants work are also different from the top industries where natives are employed. Table 3.2A lists the top ten industries that for employer-sponsored immigrants, and Table 3.2B lists the top ten for native workers. Over one fifth of the immigrants are employed in computer system design, while the largest industry among natives is elementary and secondary schools.[3] Consistent with their higher levels of education, employer-sponsored immigrants are concentrated in higher paying industries.

Along all of these dimensions, employer-sponsored immigrants are different from native workers in ways that we would expect to affect wages. Therefore, native workers in general would not make a good control group for employer-sponsored immigrants. To address the differences between these two populations, and to generate an appropriate control group for the employer-sponsored immigrants from among the native workers, I implement a matching strategy as described in the following section.

3 Econometric Specification

To estimate the impact of legal permanent residence and the associated increase in job mobility on the wages of employer-sponsored immigrants, I use the difference-in-differences strategy already outlined in equation (1) in the previous section:

$$GreenCardValue = (w_{F1} - w_{N1}) - (w_{F0} - w_{N0}). \tag{1}$$

[3] See the Appendix (Table 3A.2 in particular) for a similar discussion of family-sponsored immigrants as compared to employer-sponsored immigrants.

Instead of a dollar amount for the *GreenCardValue*, I calculate the percentage wage premium associated with receiving legal permanent residence. To this end, I use the log of wages instead of the levels and compute the expectation to obtain:

$$(2) \qquad GreenCardValuePercent = \{E(\ln w_{F1i}) - E(\ln w_{N1i})\} - \{E(\ln w_{F0i}) - E(\ln w_{N0i})\}.$$

where *i* is an index for all individuals, $i = (1,2,3,...,I)$. Using data from the NIS, I can calculate the means of both the first wages, $E(\ln w_{F0i})$, and the current wages, $E(\ln w_{F1i})$, for the employer-sponsored immigrants. As discussed in the previous section, using the means of the wages for the entire population of natives from the MORG of the CPS would not lead to accurate estimates of $E(\ln w_{N0i})$ and $E(\ln w_{N1i})$ because the natives and the employer-sponsored immigrants differ along a number of demographic characteristics that affect their wages. To calculate appropriate values for $E(\ln w_{N0i})$ and $E(\ln w_{N1i})$ to use in computing the difference-in-differences equation (2), I implement a matching strategy that selects a control group of native workers who are otherwise similar to the employer-sponsored immigrants.

To estimate the wage premium associated with receiving a green card for employer-sponsored immigrants, I implement a difference-in-differences propensity score matching estimator as originally developed by Rosenbaum and Rubin (1983). The difference-in-differences matching technique is further discussed in Blundell and Costa Dias (2002). Because there is no longitudinal data set containing native workers that is comparable to the NIS, I need to perform two separate matching procedures, one for the employer-sponsored immigrants' first wage, and another for their current wage.

In a standard difference-in-difference set-up, you have two observations of an outcome for the 'treated' group – one observation before the treatment takes place and the other observation after the treatment. You also have two observations for the 'control' group to align with the observations for the treatment group. The control group does not receive any treatment. In the present analysis, the treatment is the increase in job mobility that comes with legal permanent residence for employer-sponsored immigrants, which is the treatment group. The NIS provides wage observations for these immigrants both before and after they receive their green cards. What is different in this set-up is that the control group of native workers is essentially treated in both time periods.[4] Native workers do not need to receive green cards; they always have the job mobility that foreign-born workers sponsored by their employers only gain when they become legal permanent residents. Still, throughout the analysis I refer to the employer-sponsored immigrants as the 'treatment' group and the native workers as the 'control' group.

The purpose of matching is to ensure that the distribution of covariates (that are contained in a vector $\boldsymbol{Z}$) which could affect the outcome of interest (weekly wages) are the same in both the treatment group (of employer-sponsored immigrants) and the selected control group (of native workers). Matching essentially randomizes the treatment by selecting controls with similar distributions of covariates as the treated. Though the matching procedure can be done using the entire vector of covariates, Rosenbaum and Rubin (1983) show that the dimensionality problem of matching on a large number of covariates can be avoided by matching on a single function of the covariates $\boldsymbol{Z}$. This function, $P(Z)$,

[4] Another potential method to measure the value of a green card would be to compare the wages of employer-sponsored immigrants before and after green card receipt to the wages of foreign-born workers who did not receive green cards. This would fit more closely with a typical difference-in-differences framework, since the control group would not receive the treatment. However, I am not aware of any comparable dataset that would allow me to identify foreign-born workers who did not receive green cards.

called the propensity score, is simply the conditional probability of being in the treatment, in this case, the probability of being an employer-sponsored immigrant. Let $EmployerSponsored_i$ be an indicator variable equal to 1 if individual *i* is a member of the treatment group, i.e., an employer-sponsored immigrant, and equal to 0 if *i* is a member the control group, i.e., a native worker. The propensity score I estimate using a logit specification is given by the following equation:

$$(3) \qquad P_i(Z_i) = \Pr(EmployerSponsored_i = 1 \mid Z_i) = E[EmployerSponsored_i = 1 \mid Z_i].$$

Because there is no longitudinal data set containing native workers that is comparable to the NIS, I need to perform two separate matching procedures. I estimate the logit propensity score twice, once for the first observation of the employer-sponsored immigrants, and then again for the immigrants' current (post-green card) observation. The fitted values from the propensity score estimations, called p-scores, give the probability that an individual is a member of the treatment group. The next step in the matching strategy is to use the p-scores to choose a control group of native workers who are otherwise similar to the employer-sponsored immigrants. With nearest neighbor matching, each member of the treatment group (employer-sponsored immigrants) is paired with one (or more) members from the entire control group with the closest p-score value. These neighbors form the matched control group that is then used to calculate the value of a green card from the following equation:

$$\begin{aligned}(4)\ GreenCardValuePercent = \{&E[\ln w_{F1i} \mid EmployerSponsored_i = 1] - \\ &- E[\ln w_{N1i} \mid EmployerSponsored_i = 0, (P_i(Z_i)]\} - \{E[\ln w_{F0i} \mid EmployerSponsored_i = 1] - \\ &- E[\ln w_{N0i} \mid EmployerSponsored_i = 0, P_i(Z_i)]\}.\end{aligned}$$

Note the similarity between this and the original equation (1) from the Theoretical Framework. The main difference is that I have now conditioned the values of $E(\ln w_{N0i})$ and $E(\ln w_{N10i})$ on the p-score estimated in equation (3).

4 Data

For information on employer-sponsored immigrants, I use newly available data from the 2003 New Immigrant Survey (NIS). This survey is a nationally representative sample of foreign-born individuals who became legal permanent residents of the United States between May and November of 2003.[5] In the first round of this (future) panel survey, new legal permanent residents were interviewed between June 2003 and June 2004, after they had received their green cards. Out of the sampling frame of 12,500 immigrants, 8,573 completed the initial interview, resulting in an overall response rate of 68.6 percent. The NIS is unique in the wealth of information that it provides about new legal permanent residents. The respondents answer questions about their work experience and wages before they came to the United States, as well as their work experience and wages in the U.S., both before and after receiving green cards.

From the surveyed population of new immigrants, I limit my sample to those who report having an employer sponsor, who are principal immigrants (that is, those whose own employers are the sponsor, as opposed to the employers of their spouses or parents), and who adjusted their status to legal permanent resident (that is, they were already living in the U.S. on another type of visa when they applied for green cards, as opposed to those who applied for and received their green cards while living in another country). Of the 8,573 new

[5] NIS data is available at http://nis.princeton.edu/.

permanent residents in the NIS, 491 individuals met all of these criteria, and additionally worked for pay in the U.S. both before and after they received their green cards, and reported the wages for the jobs they held.

The descriptive statistics for this population of employer-sponsored immigrants are reported in the first column of Table 3.1. Note that three quarters of these immigrants are male, and 80 percent have at least a bachelor's degree. The educational attainment of these immigrants is much higher than that of the immigrant population as a whole, or of the native U.S. population. Seventy-five percent were between the ages of 29 and 43 when they received their green cards in 2003, and almost two thirds arrived in the U.S. between 1996 and 2000. The top ten industry categories in which these immigrants worked when they first arrived in the U.S. are listed in Table 3.2A. One fifth of the employer-sponsored immigrants were employed in "computer system design and related services" before receiving their green cards, and "colleges and universities, including junior colleges" were the next largest employer for this population when they first arrived in the United States.

The ideal dataset to use in examining the effects of increased mobility on wages would have information on immigrants' U.S. wages just before and just after they received their green cards. However, rather than asking about the immigrants' U.S. wages just before receiving green cards, the NIS has detailed information on the first U.S. job, as well as information on the current (post-green card) U.S. job. Survey respondents also answer basic demographic questions and provide information on their green card sponsor.

To construct an appropriate control group for the employer-sponsored immigrants in the NIS, I use the Merged Outgoing Rotation Groups (MORG) from the Current Population Survey (CPS). This survey also contains basic demographic questions and information on wages and industry of employment for U.S. workers. I use only observations of native-born citizens in the CPS to construct a control group for the employer-sponsored immigrants, since I do not know which foreign-born workers in the CPS have green cards and which do not. About twenty-five percent of the natives in the CPS have imputed wage data, which could lead to biased estimates of the wages of the control group (see Hirsch and Schumacher, 2003). Therefore, I exclude those with imputed wage information in the MORG, and only use those observations with reported wages.[6]

Given that I have longitudinal data for the employer-sponsored immigrants from the New Immigrant Survey, an ideal dataset from which to draw the control group of natives would also be longitudinal. However, existing longitudinal datasets do not properly align with the age cohorts of the NIS or do not include enough wage information. For example, the National Longitudinal Survey of Youth 1979 (NLSY79) surveyed individuals born between 1957 and 1964, meaning that the NLSY79 population overlaps with less than thirty percent of the employer-sponsored immigrants in the NIS (see Table 1). Likewise, the National Longitudinal Survey of Youth 1997 (NLSY97) surveyed young men and women born between 1980 and 1984. This NLSY97 population then is younger than all but 0.4 percent of the employer-sponsored immigrants in the NIS (see Table 1).

The Survey of Income and Program Participation (SIPP) is another longitudinal dataset that could possibly be used to construct a control group of natives to compare to the employer-sponsored immigrants in the NIS. While the 2004 SIPP contains an Employment History topical module with questions about the earliest work experiences of the survey participants, the module does not include information on wages, and so cannot be used to compare wage changes between natives and employer-sponsored immigrants.

[6] See the Appendix for details on how the CPS and the NIS data are coded so as to be comparable.

Additionally, the population of employer-sponsored immigrants in the NIS is highly educated, with more than 80 percent holding a bachelor's degree or more, and more than 40 holding a graduate degree. Even the larger longitudinal datasets are unlikely to contain enough individuals with advanced levels of education from which to select a matched control group which is sufficiently similar to the employer-sponsored immigrants in the NIS to make the propensity score matching procedure a viable option. For these reasons, the MORG of the CPS represents the best available option for constructing an appropriate control group for the employer-sponsored immigrants in the NIS.

In the empirical implementation of the estimator from equation (4), I divide the NIS data into two separate datasets, one that contains the current wage observation and all the covariates, and another that contains the first wage observations and all the covariates. The immigrants in the NIS are only asked about their current educational attainment, not the educational attainment they had at the time of their first job in the U.S., so current educational attainment is used to proxy for educational attainment at the time of the first wage. Gender and year of birth are assumed to be the same over time. Industry is reported for both the first wage and the current wage. Region for the current wage observation is the current state or region of residence, while region for the first wage is assigned the state or region to which the green card was mailed.

I combine the data on the employer-sponsored immigrants' first wages from the NIS with the data on native workers in the MORG (1983-2002) and estimate a logit regression, with the indicator for being an employer-sponsored immigrant (as opposed to a native worker) as the dependent variable. The fitted value from this logit regression is the propensity score – the probability of being in the treatment group, i.e., the probability of being an employer-sponsored immigrant. Similarly, I combine the data on the same immigrants' current wage observations from the NIS with 2003-2004 MORG to estimate a logit propensity score. The vector of covariates used to estimate the propensity score, $\boldsymbol{Z}$, includes variables that affect wages, and also variables that could predict whether or not an individual in the sample is an employer-sponsored immigrant. Since the employer-sponsored immigrants are highly educated, indicators for educational attainment are included in $\boldsymbol{Z}$. Immigrants have a different geographical distribution in the U.S than natives, and so indicators for state/region belong in $\boldsymbol{Z}$. Gender, year-of-birth, industry, and year of the survey dummies are also included in $\boldsymbol{Z}$.[7]

To construct appropriate control groups to compare to the employer-sponsored immigrants, I use nearest-neighbor matching. For each employer-sponsored immigrant, nearest-neighbor matching selects one (or more) native workers with the closest propensity score to that of the immigrant. The matching is done with replacement, and ties are equally weighted. Also, native workers chosen as the nearest neighbor for more than one of the employer-sponsored immigrants are assigned weights to reflect the frequency with which they are matched to observations in the treatment group. For robustness, I vary the number of nearest neighbors to use in the control groups, choosing one, five, and ten nearest neighbors. I calculate the difference between the (unweighted) log weekly wages of the employer-sponsored immigrants and the log weekly wages of the matched neighboring natives, weighted by the number of times they are used as nearest neighbor (since the matching is done with replacement) for the first observation, and for the current observation. The green card value is then the difference between these two wage differences.

[7] See the Appendix for more details about these covariates.

With nearest-neighbor matching estimation, the estimated standard errors do not take into account the fact that the propensity score is estimated. To correct for this, I perform a bootstrap procedure, which estimates the distribution of the wage difference coefficient and which provides a better standard error for the coefficient. For the difference in the current wages between the employer-sponsored immigrants and the native controls, I sample with replacement from the native population in the MORG to generate a sample of natives with the same number of individuals. I combine this sample of natives with all of the employer-sponsored immigrants in the NIS and run the logit propensity score estimation, which is then used to select the matched control group of natives who have the closest propensity scores to those of the employer-sponsored immigrants. The wage difference is then the difference between the wages of the employer-sponsored immigrants and the matched natives. I repeat this two hundred times, each time creating a new sample of natives by sampling with replacement from the natives in the MORG, which gives me a distribution and a standard error for the wage difference. I follow this same procedure to generate a distribution and standard error for the first wage difference.

5 Results

I use matching techniques to compare the wages of employer-sponsored immigrants to those of a group of otherwise similar native workers. First, I specify the vector of covariates $\boldsymbol{Z}$ used in the estimation of the propensity score, i.e. the probability of being an employer-sponsored immigrant. The vector of controls $\boldsymbol{Z}$ should include all characteristics that may affect both treatment (having an employer sponsor) and outcome (wages). I include the standard Mincerian covariates such as the education level of the individual and age (from a series of indicators for the year-of-birth cohort). Additional controls included in $\boldsymbol{Z}$ are indicator variables for gender, industry of employment, region of residence, and year of the survey.[8]

The results of the propensity score estimation are reported in Table 3.3. Because there is no longitudinal data for natives that is comparable to the NIS (see discussion in Data section), I have to perform two matching procedures. First, I need to find a group of otherwise similar natives for the first observation of the employer-sponsored immigrants, and then do the same for their second (current) observation. The first column of Table 3.3 presents the estimates that correspond to the first observation, and in the second column are the estimates corresponding to the current observation, both estimated using a logit model. As described in the Empirical Strategy section, the dependent variable for this logit estimation is an indicator equal to one for employer-sponsored immigrants and zero for native workers. As expected, the estimated coefficients confirm that higher levels of education significantly increase the likelihood that an individual is a member of the treatment group of employer-sponsored immigrants. The overall fit of the logit propensity score model, both for the first wage observation and the current wage observation, is fairly good, with pseudo-R square measures of 0.32. The fitted values from these logit regressions are the propensity scores used to select a control group from the native citizens similar to the treatment group of employer-sponsored immigrants.

The left side of Table 3.4A presents the differences in covariates for the first observation between the employer-sponsored immigrants from the NIS and the native workers from the MORG of the CPS in the original unmatched sample. As previously

[8] See Table 3.A1 in the Appendix for details about the regional categories used in the NIS. Also, see the Appendix for a description of the industry variable.

discussed, the two groups of workers have quite dissimilar personal characteristics. On average, employer-sponsored immigrants were born more recently and have more education. Almost all of the differences between these two populations are statistically significant at the 5 percent level, indicating that direct comparison of wages between the two groups without correcting for their characteristics would be inappropriate.

Based on this evidence, the goal of the matching procedure is to select an appropriate control group of untreated individuals (native workers). The matching procedure is successful if members in the selected control group have similar observable characteristics to the members of the treatment group (employer-sponsored immigrants). The right side of Table 3.4A formally verifies that there are no significant differences in covariates left between employer-sponsored immigrants and native workers in the single nearest-neighbor matched sample. For example, the difference between employer-sponsored immigrants and native workers in the proportion who are female is only 1 percentage point (22.6 percent among employer-sponsored immigrants and 21.6 percent among natives). The difference in the proportion of females between the two groups in the original unmatched sample, on the other hand, is 29.2 percentage points. After matching, the difference in the proportion of females between the two groups is small and no longer statistically significant. Similarly, the differences in the rest of the covariates in $\boldsymbol{Z}$ in the original sample disappear in the matched sample. Also, the average propensity score difference between the two groups (employer-sponsored immigrants and native workers) in the matched sample is very small at 0.00065, providing evidence that the balancing property of the propensity score is ensured.

In a similar manner, Table 3.4B demonstrates how the propensity-score matching for the current observations results in a control group of native citizens that have characteristics similar to those of the employer-sponsored immigrants. On the left side of Table 3.4B, note that there are significant differences between natives and immigrants in their gender composition, educational attainment, and year-of-birth cohort. As reported on the right side of the table, the single nearest-neighbor propensity score matching results in a control group that closely resembles the treatment group of employer-sponsored immigrants. After the matching procedure, no significant differences remain between the matched natives and the immigrants.

In addition to using the single nearest neighbors to construct an appropriate control group for the employer-sponsored immigrants, I also create larger control groups of native workers by choosing the five nearest and ten nearest neighbors for each employer-sponsored immigrant. Choosing a greater number of neighbors may not reduce the differences between the employer-sponsored immigrants and the natives as much as choosing just a single neighbor, since each additional native neighbor is farther away from the treated immigrant in terms of the propensity score. However, both when the five nearest neighbors are chosen and when the ten nearest neighbors are chosen, there are no significant differences between the treated immigrants and the matched control natives along any of the covariates used in the propensity score logit regression (results not shown). The large sample of natives in MORG allows me to select a larger number of neighbors for the control group and still have a control group that is comparable to the treatment group.

Based on the evidence presented so far, the matching procedure appears to be effective in eliminating the selection bias that may affect the naïve estimator of the differences in wages between the treated (employer-sponsored immigrants) and the controls (native workers). I next turn to the matching estimator of the differences in wages between the two groups and present the estimates of the effect of legalization and the associated increase in job mobility on weekly wages using difference-in-differences nearest-neighbor matching.

The three panels of Table 3.5 present the difference-in-difference wage results from matching the employer-sponsored immigrants in the NIS to a single nearest neighbor (3.5A), five nearest neighbors (3.5B), and ten nearest neighbors (3.5C) among native U.S. citizens in the Merged Outgoing Rotation Groups of the CPS. All matching is done with replacement, and observations from the control group with the same propensity scores are equally weighted.

In all three specifications, employer-sponsored immigrants experience an increase in their wages following the receipt of a green card, and in two of the three specifications, the increase is statistically significant at the 5 percent level.[9] The estimates range from a weekly wage increase of 13.5 percent (Table 3.5A) to an increase of 16.5 percent (Table 3.5B). Note that there is a trade-off between variance and bias. The more neighbors chosen for the control group, the lower the standard error, but the greater potential bias, since the counterfactual is being constructed using observations that are less and less like the treated observation. As the sign and magnitude of the estimate is not particularly sensitive to the number of nearest neighbors chosen, bias does not appear to be a major concern.

The first U.S. wages of employer-sponsored immigrants are between 3.7 and 6.7 percent lower in magnitude than the counterfactual wages constructed using the nearest neighbor matching, but these differences are not statistically significant at the 5 percent level. The lower initial wages for the employer-sponsored immigrants are consistent with the hypothesis that foreign-born workers who want to become legal permanent residents of the

[9] All difference-in-difference estimates are statistically significant at the 10 percent level.

United States are willing to accept lower wages in exchange for the benefit of having their employers sponsor them for green cards. For the current wages, employer-sponsored immigrants have 6.8 to 11.8 percent higher wages than their native counterparts, and these differences are statistically significant at the 5 percent level for two out of the three specifications.

For robustness, I implement the propensity score matching estimator with the additional restriction of an exact match on the year of the survey. For each employer-sponsored immigrant in the NIS, the nearest neighbor is now chosen only from among the natives in the MORG of the CPS whose wage observations occur in the same year as the wage observations of the employer-sponsored immigrant.[10] The results of this estimation are presented in Tables 3.6A through 3.6C. Overall, these results confirm the previous findings. Employer-sponsored immigrants initially have lower wages than similar natives, but the immigrants have higher wages after they receive their green cards. In this specification, the receipt of a green card is accompanied by a 16.1 to 18.2 percent wage increase for employer-sponsored immigrants.

Employer-sponsored immigrants experience a significant increase in their wages following adjustment to permanent residence. Their lack of job mobility while they are waiting to receive their green cards limits them to wages that are lower in magnitude than those of comparable native workers, but once these employer-sponsored immigrants become permanent residents, their wages are (significantly) higher than those of comparable natives because they are able to search for the highest paying employment for their skills.

[10] Due to the very small number of employer-sponsored immigrants who first worked in the U.S. in 1983 and 1984, these immigrants are restricted to match with natives from the pooled 1983 and 1984 CPS MORG data.

Wage changes following adjustment to legal permanent residence could happen in two ways. Immigrants released from their ties to the sponsoring employers could accept jobs with other employers at higher wages, or the threat of outside opportunities could be enough to induce the sponsoring employers to offer higher wages in order to retain their immigrant employees. In Table 3.7, I examine the average log weekly wages (inflated to constant 2006 dollars) for employer-sponsored immigrants both before and after they received their green cards, separating those who remained in the same job after becoming legal permanent residents from those who changed jobs. The average current log weekly wages are the same (7.092) for employer-sponsored immigrants who changed jobs and for those who stayed in the same job. However, the immigrants who changed jobs had much lower weekly wages than those who stayed in the same job when they first arrived in the U.S. (6.270 vs. 6.726). Job changers have much larger growth in wages following their adjustment to legal permanent resident status. This difference suggests that the wage change experienced by employer-sponsored immigrants after they receive their green cards is due to largely to those who change jobs, and thus that the green card wage premium is due to increased job mobility.

To further explore the role that changing jobs plays in the wage value of a green card, I estimate the following difference-in-differences linear regression on the log of weekly wages, comparing the wages of those who stayed in the same job to those who changed jobs before and after becoming legal permanent residents:

$$(5) \qquad LnWeeklyWages_{it} = \delta_0 + \delta_1 Time_t + \delta_2 ChangedJob_i + \delta_3 Time_t * ChangedJob_i + W_{it}\beta + \nu_{it}$$

where $LnWeeklyWages_{it}$ is the log of the weekly wage for employer-sponsored immigrant i at time $t \in (0,1)$; $Time_t$ is an indicator equal to 1 for the current (post-green card) wage observation and 0 for the first wage observation; and δ_3 is the coefficient on the interaction between those two variables, the difference-in-differences coefficient. The matrix W_{it} holds other covariates that affect the wage including gender, education, year-of-birth cohort, and region of residence as previously described. The coefficients from regression (5) are presented in Table 3.8.

As expected, the first wages are significantly lower than the current wages, by 30.1%. When controlling for the socio-demographic characteristics, the wage change for employer-sponsored immigrants who change jobs after receiving their green cards is 49.7% larger than for those who stay in the same job. The regression results confirm the findings in Table 3.7, that the wage increase experienced by employer-sponsored immigrants is driven largely by those who change jobs once they have the job mobility provided by legal permanent residence.

6 Discussion

An alternative explanation for these findings is that the wages of employer-sponsored immigrants increase more than those of similar natives over time because the education and skills that the immigrants obtained in their native countries are not completely transferable to the U.S. labor market upon their initial arrival. The wage increases for employer-sponsored immigrants could be due to increasing skill transferability the longer they live in the U.S., instead of being the result of the greater job mobility that accompanies permanent resident status. Jasso, Rosenzweig, and Smith (2002) use the NIS-P (the pilot survey of the NIS) to show that the skill transferability of immigrants is initially low, but that it increases with greater exposure to the U.S. labor market. They find that skill transferability is greater for immigrants who are younger and who are male, compared to those who are older and who are female.

To test whether or not increasing skill transferability among the sample of employer-sponsored immigrants may be causing the immigrant wage increases following green card receipt, I compare the wages of (principal) employer-sponsored immigrants in the NIS who arrived in the U.S. with green cards (new arrivals) to my sample of (principal) employer-sponsored immigrants who adjusted to permanent resident status after living in the U.S for a number of years (adjustees). If increasing skill transferability is driving the wage increases for immigrants, then I would expect to find that new arrivals have significantly lower current wages than adjustees, who have already lived and worked in the U.S. for years (about half of adjustees arrived in the U.S. in 1997 or earlier). Using OLS, I estimate the following equation:

(6) $$LnWeeklyWages_i = \alpha_0 + \alpha_1 NewArrival_i + \mathbf{X}_i \beta + \varepsilon_i$$

where $LnWeeklyWages$ is the log of the weekly wage for the current job at the time of the survey, either in 2003 or 2004.[11] The variable $NewArrival$ is an indicator equal to 1 if the employer-sponsored immigrant arrived in the U.S. with a green card, and equal to 0 if the employer-sponsored immigrant was already living in the U.S. when he or she received a green card.

The vector $\mathbf{X}$ contains other characteristics which affect wages and which differ between the two populations of principal employer-sponsored immigrants. For example, there are a greater proportion of women among new arrivals compared to adjustees, so the gender of the immigrant is one of the variables in $\mathbf{X}$. Also, although more than three

[11] See the Appendix for more information about the coding of the wages.

quarters of both new arrivals and adjustees have attained at least a college degree, the proportion of adjustees with advanced degrees is higher than the proportion of new arrivals with advanced degrees (39% vs. 22%). Educational attainment is also included in $\mathbf{X}$, using indicators for having less than a high school degree, having some college education or an associates degree, having a bachelors degree, and having an advanced degree (having a high school diploma is the omitted category). New arrivals are younger on average than adjustees, which would result in their earning lower wages because they have fewer years of experience working. Indicators for year-of-birth cohort are included in $\mathbf{X}$ to control for the effect of age (as a proxy for labor market experience). Regional wage differences persist in the United States, and new arrivals and adjustees differ in their geographic distribution in the U.S. A greater fraction of new arrivals than adjustees live in Texas, while a greater fraction of adjustees live in New England. This geographical pattern would also tend to affect wages, so regional indicators are included in $\mathbf{X}$.[12]

The result for the estimation of equation (6), without the inclusion of any covariates, is presented in Table 3.9, column 3.9.1. As expected given their characteristics, new arrivals have significantly lower weekly wages than do adjustees. However, once the above-mentioned covariates are included in the regression, the wage difference between new arrivals and adjustees falls greatly in magnitude, and I can no longer reject the null hypothesis that there is no difference between the wages of newly arrived employer-sponsored immigrants and those employer-sponsored immigrants who have lived in the U.S. for a number of years (column 3.9.2). Further controlling for the industries in which the immigrants work also results in a coefficient which is very small (-0.019) and not significantly different from zero (column 3.9.3).

[12] See Table 3.A1 in the Appendix for the regional categories used in the NIS.

As new arrivals do not have significantly lower wages than adjustees, it appears that skill transferability and length of time in the U.S. have little effect on the wages of employer-sponsored immigrants. This is not surprising, given that employer-sponsored immigrants are a highly educated population, with skills valued in many different countries. These results provide further support that the wage increase experienced by employer-sponsored immigrants following their adjustment to permanent residence is due to the increased job mobility that accompanies the receipt of a green card.

In all specifications of the nearest neighbor propensity score matching, the employer-sponsored immigrants have significantly higher wages than the matched natives, who have a similar distribution of covariates. One possible explanation is that employer-sponsored immigrants may be more willing to locate in areas where natives prefer not to live (such as rural areas, or urban city centers). Employers in these less desirable locations may offer higher wages for high-skilled jobs than employers in more desirable locations, to compensate their workers for the lack of amenities in the area. For example, foreign-born physicians are more likely than native physicians to locate their practices in rural areas (Brooks, Marden, and Clawson 2003). If this is true for other high-skilled professionals, that may be part of the reason why the employer-sponsored immigrants have higher wages than comparable natives after they receive their green cards.

7 Conclusion

Using a difference-in-differences nearest-neighbor propensity score matching estimator and data from both the NIS and the MORG of the CPS, I estimate the wage premium associated with the increase in job mobility that accompanies the arrival of a green card for employer-sponsored immigrants. These immigrants may receive lower wages prior to adjusting to legal permanent residence due to the costs that employers must undergo to sponsor their visas and due to their inability to change jobs while their green card applications are in process, which takes an average of five years. For foreign-born workers, becoming legal permanent residents eliminates any additional costs that employers must pay for their visas and also allows the workers to change jobs freely without affecting their ability to live and work in the U.S.

In my analysis, employer-sponsored principal immigrants who adjust to legal permanent resident status experience large and significant wage increases following the receipt of their green cards. The estimates of the wage increases are between 13.5% and 16.5%. Immigrants who change jobs experience larger wage increases than those who remain in the same job from the time they first work in the U.S. until after they become legal permanent residents. This implies that the wage premium associated with the receipt of a green card is due to greater job mobility. Additionally, current wages for employer-sponsored immigrants who arrive in the U.S. with green cards are not significantly lower than the wages of green card holders who have lived in the U.S. for a number of years before adjusting their status to legal permanent resident. Thus, increasing skill transferability with time spent in the U.S. does not appear to be the cause for the wage increases experienced by this highly-skilled population of employer-sponsored immigrants.

APPENDIX

Merged Outgoing Rotation Groups of the CPS and the New Immigrant Survey

Many data issues arose in formatting the data in the MORG of the CPS and in the NIS in such a way that the data were directly comparable. The following details the creation of the dependent and independent variables from both the CPS and the NIS, which are then used in my analysis.

Year-of-birth

In the CPS, survey respondents are asked about their current age, as of their most recent birthday. In the NIS, survey respondents are asked to report their year of birth, and the year-of-birth responses are aggregated into ten categories, beginning with those who were born before 1940, and combining the remainder together in five year intervals (e.g., 1940-1944, 1945-1949, 1950-1954, …), ending with the 1980-1984 interval.

There were two options to consider in formatting this data. The first was to convert the NIS year-of-birth intervals into ages, and the second was to convert CPS ages into year-of-birth intervals. In the first option – assigning each NIS respondent the central age that would be associated with the reported year-of-birth interval – nearly all of the NIS respondents would suffer from measurement error in their constructed ages, as the constructed age could be as much as two and a half years more or less than the actual age. Instead, I chose to convert the CPS ages into year-of-birth intervals comparable to those in the NIS, so as to reduce the amount of measurement error in this explanatory variable.

For the CPS outgoing rotation group interviewed in December, the year of birth can be fairly accurately calculated as the difference between the current year and the reported age. By December, the majority of the survey respondents have passed their birthdays for that year (assuming a fairly uniform distribution of birthdays across the year, at least 11/12 of the respondents have their birthdays before they are surveyed in December). However, for the outgoing rotation group interviewed in January, the majority of respondents have not yet passed their birthdays for that year, and the difference between their current age and the survey year will be one year later than the actual birth year of most of the respondents (again, about 11/12 of the respondents have birthdays after January). To account for this misalignment between ages and birth years, I assigned a birth year that was equal to the difference between the survey year and the respondent's age advanced by one year (birth year = survey year – [age +1]) for all of the observations that were recorded in the first half of the calendar year (January through June). In the last half of the calendar year (July through December), I assigned the respondents a birth year that was the difference between the survey year and the reported age (birth year = survey year – age), to account for the fact that the respondents are more likely to have passed their birthdays in the second half of the year than in the first half of the year.

While there is still some measurement error involved in this assignment strategy, it is largely mitigated by the aggregation of the birth year information into five year intervals. For example, an individual who reports being 30 years old in the 2004 CPS could have been born in 1974 (if the survey is taken after her birthday in the survey year), or she could have been born in 1973 (if the survey is taken before her birthday in the survey year). The month of the outgoing rotation group will determine which of these birth years will be assigned to the respondent. However, whether or not the assigned birth year is the true birth year is largely irrelevant, since both birth years are aggregated together in the 1970-1974 year-of-birth interval. The respondent will be correctly assigned to the appropriate year-of-birth interval.

With this assignment strategy, miscoding only occurs when the two possible birth years belong to two different year-of-birth intervals. For example, an individual who reports being 30 years old in the 2000 CPS could have been born in 1970 or in 1969, depending on his month of birth and the month in which the survey is administered. These two years belong to different year-of-birth intervals, so there is a possibility that the respondent will be assigned the wrong birth year and thus be placed in the wrong year-of-birth interval. Assuming a uniform distribution of ages of the respondents in CPS (which is fairly true across the ages of the population of interest), about twenty percent of the respondents will be assigned a birth year such that their other likely birth year is in a different year-of-birth interval. Not all of these assignments will be wrong; I would expect roughly half of these respondents to be assigned to the wrong year-of-birth interval, which means that around ten percent of the CPS sample is classified into a year-of-birth interval which does not correspond to their actual year of birth. This misclassification would be more likely to occur among respondents surveyed in the middle of the year, as opposed to the beginning or the end of the year, when the assignment strategy is likely to be more accurate.

State/region

The geographical information attached to the current wage observations for NIS respondents is the state of residence at the time of the survey. For their first U.S. wage, the new immigrants are assigned the state to which their green cards were mailed. Less than five percent of sample had moved to a different state/region in between when their green cards were mailed and when they were surveyed.

The state-level geographical information is aggregated to the nine Census divisions in the public use NIS dataset, unless the state of residence was one of the six traditional gateway states (California, Florida, Illinois, New Jersey, New York, and Texas), which are home to the majority of immigrants living in the U.S. These six states were coded as individual states, not as a part of the larger Census divisions. See Appendix Table 3.A1 for a list of the states which were included in each division. The state-level geographical information in the CPS was aggregated to align with the state and division categories in the NIS.

Education

For CPS data in 1992 and later years, aligning the educational attainment responses with the nine education categories in the NIS was fairly straightforward. Those who reported their highest grade attended as 6^{th} grade or lower in the CPS were considered to have no education. Those who reported attending 7^{th} or 8^{th} grade were classified as having finished elementary school. Those who reported attending 9^{th} through 12^{th} grade but not having a high school diploma were classified as having finished middle school. Among all employer-sponsored immigrants in the NIS, less than 2 percent reported educational attainment less than a high school diploma. High school graduates and those with some college but no degree in the CPS were classified as high school graduates. Both types of Associates degrees in the CPS were combined into one Associates degree category as they are in the NIS. All other degree categories in the CPS had a one-to-one correspondence with degree categories in the NIS.

Before 1992 in the CPS, survey respondents were asked the highest grade that they attended, and in a separate question, they were asked whether or not they completed that grade. The responses are truncated at 18 years, so it is not possible from the responses to these questions to separate those who received Masters degrees from those who received PhDs or MDs. Only 12 percent of employer-sponsored immigrants in the NIS report their first wage occurred before 1992, so few of these pre-1992 respondents in the CPS are used in the control group.

For the CPS surveys conducted before 1992, respondents who did not finish 5^{th} grade were classified as having no education. Those who finished 5^{th} grade but did not finish 8^{th} grade were considered to have finished elementary school. Those who finished 8^{th} grade but did not finish 12^{th} grade were considered middle school graduates. Those who reported finishing 12^{th} grade but who did not finish at least two years of college were considered to be high school graduates. Finishing at least two years of college, but not four years, puts the respondent in the Associates degree category. Respondents were coded as having a Bachelors degree if they had completed at least four years of college but had not completed six or more. Those who had completed six or more years of college were assigned to the Masters degree category.

This same assignment system was also used to assign a highest degree completed to the NIS respondents who reported years of schooling but did not report a highest degree completed (about 15 percent of the sample).

Industry

The NIS uses an industry classification system based on the 2002 North American Industry Classification (NAICS). This same classification is also used in the MORG of the CPS from 2000 through 2004. However, the industry classification in earlier years of the CPS is based on the Standard Industry Classification (SIC). There is no one-to-one correspondence between these two systems. However, in the 2000, 2001, and 2002 CPS data, each respondent has a value for both the NAICS-based industry code and the SIC-based industry code. For each SIC value in the 2000-2002 CPS data, I determined the unique NAICS value into which it was most likely to map. All of those SIC values in the earlier data were then assigned to the most common NAICS value. On average in the 2000-2002 CPS data, over two thirds of an SIC value mapped into the NAICS value it was assigned. CPS data before 1983 used an earlier version of the SIC to categorize the respondents' industries, so CPS data before 1983 was not used.

Wages

The CPS contains information on the hourly and weekly earnings of U.S. workers. Hourly workers are asked to report their hourly wages and the usual number of hours they work. Weekly earnings are calculated for hourly workers by multiplying the hourly wage by the usual number of hours worked. Non-hourly workers are asked to report their weekly wages. Thus either hourly wage or weekly wage could be the dependent variable in my regressions. Given that the employer-sponsored immigrants, with their higher-than-U.S.-average levels of education, are more likely to have salaried jobs than hourly jobs, it is likely that natives with high propensity scores will also be salaried and thus be reporting their earnings as the amount they earn in a week. Weekly earnings, then, are the more appropriate measure to use as the dependent variable.

In the NIS, for both the first U.S. wage and the current wage, respondents reported their earnings in a variety of ways – by the hour, by the day, by the week, by two-week pay periods, by the month, and by the year. To construct weekly wages for each individual, I multiplied hourly wage data by the reported usual number of hours worked. I assumed a five-day workweek and multiplied the daily wage data by five. Weekly wages remained as they were reported. Bi-weekly wages were divided by two, and monthly wages are divided by four. Annual earnings were divided by the reported usual number of weeks worked in a year.

In the MORG, the weekly earnings data are top-coded, while the earnings data in the NIS are not top-coded. This difference could bias the results, making it appear that the employer-sponsored immigrants have higher wages than the natives whose wages are top-coded. For better comparisons between the two wage distributions, I top-coded the NIS wage data following the top-coding scheme of the MORG. For wage reported in 1983-1988, weekly wages were top-coded at $999. For wages in 1989-1997, the highest value was $1923. And for 1998-2004, wages were truncated at $2884. This top-coding was binding for about 5% of wage observations in the NIS.

For equation (5) and the corresponding Table 3.9, all of the wage observations are from the NIS, which is not top-coding. Therefore, top-coding is unnecessary, and the weekly wages are used as they are reported (or constructed as described above).

Restricting the CPS sample

To generate a control group that was comparable to employer-sponsored immigrants in the NIS, I limited the CPS sample to those who reported being in the labor force, who were not attending school, either full-time or part-time. I further limited the sample by excluding the CPS respondents who reported being self-employed, because they did not have wage observations. Among those with wage observations, I limited my control group to those who reported their wages, excluding those with allocated wages, since the presence of allocated wages has the potential to bias the results (Hirsch and Schumacher, 2003). Only native U.S. citizens, born in the U.S. (but not its territories) were considered. It was necessary that the control group not face any of job mobility limitations associated with being an employer-sponsored immigrant. Since the CPS does not ask the foreign-born about their visa status, I could not be sure that there were no employer-sponsored immigrants in the control group unless I removed all foreign-born respondents. However, the CPS only introduced the questions regarding country of birth and citizenship status in 1994, so I am unable to remove the foreign-born from the 1983-1993 CPS data. Lastly, I limited the industries in the CPS to the industries in the NIS that employed immigrants who were sponsored by their employers. This step was unnecessary, as these observations would not have been included in the propensity score calculation anyway (since they would perfectly predict not being an employer-sponsored immigrants), but excluding them helped to reduce the CPS data to a more manageable size.

Weights in the New Immigrant Survey

Since certain sub-populations of legal permanent residents were oversampled in the NIS to allow for better analysis, sampling weights were created to allow analysts to produce a representative sample of the entire population of new legal permanent residents. The sub-population of employer-sponsored immigrants were over-sampled relative to their representation among the entire population of new legal permanent residents. (Employer-sponsored immigrants constitute 16.5 percent of the NIS sample, but they are less than ten percent of the legal permanent resident population.) However, there is no oversampling in the NIS from the employer-sponsored immigrant sub-population. The sample of employer-sponsored immigrants is therefore representative of the population of employer-sponsored immigrants. Since my analysis focuses only on employer-sponsored immigrants and does seek to generalize to the entire population of new legal permanent residents, it is not necessary for me to use the NIS sampling weights. (See Jasso, Massey, Rosenzweig, and Smith 2004 for more details about the sampling frame of the NIS).

Family sponsored immigrants in the New Immigrant Survey

The initial choice of control group for the employer-sponsored immigrants in the NIS was the sample of family-sponsored immigrants in the NIS. Since family-sponsored immigrants would not be dependent on their employers for their adjustment to legal permanent residence, they would be free to move from job to job even before receiving their green cards (for those who had authorization to work in the U.S.). However, family-sponsored immigrants are very different demographically from employer-sponsored immigrants. Appendix Table 3.A2 presents the means of a selection of demographic variables for the employer-sponsored immigrants and the family-sponsored immigrants. Note that the average weekly wages for the employer-sponsored immigrants are more than twice that of the family-sponsored immigrants. There is a smaller proportion of women among the employer-sponsored immigrants. Only one third of those with a family sponsor have a bachelor's degree or higher, compared to over seventy percent for the employer-sponsored immigrants. The employer-sponsored immigrants on average have been working in the U.S. for longer than the family-sponsored immigrants. The geographical distributions of these two types of immigrants are also dissimilar, as the family-sponsored immigrants are more likely to live in California or Texas, and less likely to live in New England or the South Atlantic states.

While the characteristics of the native citizens in the CPS are also very different from those of the employer-sponsored immigrants, the CPS has a much larger population from which to draw an appropriate control group. Among the treatment group, there are 41 employer-sponsored immigrants who hold PhDs. Only 1.4% of the native citizens in the CPS have a PhD. (see Table 3.3B), but that translates into about 1800 individuals to use in selecting a control group, compared to only 15 family-sponsored immigrants in the NIS with a PhD. Applying the nearest-neighbor propensity score to the sample of family-sponsored immigrants results in a control group that is still significantly different from the treatment group of employer-sponsored immigrants along many important dimensions (results not shown). In contrast, the control group selected from the CPS using the propensity score is not significantly different from the employer-sponsored immigrants along any of the covariates (see Tables 3A and 3B). Therefore, the sample of natives from the CPS is used in the main analysis.

Pooling male and female wages

While it is standard practice in labor economics to separate males and females when considering wages, due to the distinctly different patterns of labor force participation exhibited by the two groups, I have pooled the males and females together, controlling for any effect of gender with an indicator variable in the logit propensity score regressions. There are two main motivations behind the choice to pool the wages for male and female employer-sponsored. First, it is reasonable to assume that the labor force participation patterns of female employer-sponsored immigrants are fairly similar to those of male employer-sponsored immigrants, given that the application for a green card is dependent on the immigrant having an employer-sponsor (and so necessarily being employed). Recall that the analysis focuses on principal employer-sponsored immigrants, meaning that the female immigrants that appear in the sample are adjusting to legal permanent residence through an application by their own employers, and not the employers of their spouses.

Second, an examination of the distribution of industries of male and female employer-sponsored immigrants supports the pooling of the wages. In Tables 3.A3 and 3.A4 I present the top ten industries of the first U.S. job for male and female employer-sponsored immigrants, respectively. The largest industry of employment for both male and female employer-sponsored immigrants is Computer System Design and Related Services. Male and female employer-sponsored immigrants share three of their top four industries and five of their top ten industries (see industry descriptions in italics). These similar industry patterns further validate the assumption that the employment patterns of male and female employer-sponsored immigrants are similar enough to pool their observations for the analysis.

Inverse probability weighting

As an additional robustness check, I also estimate the wage value of a green card using an inverse probability weighting strategy instead of a nearest-neighbor propensity score matching strategy. The main difference between these two methods is that inverse probability weighting uses information from all of the native citizen controls, while matching uses only those controls who most closely resemble the employer-sponsored immigrants.

For inverse probability weighting, as with the propensity score matching, I first pool the observations of the employer-sponsored immigrants with the natives from the MORG of the CPS from 1983 through 2002. I then estimate a logit propensity score with the indicator for employer-sponsored immigrants as the dependent variable. The employer-sponsored immigrants are then pooled with the natives from the MORG of the CPS from 2003 and 2004, and I estimate another logit propensity score predicting the probability of being an employer-sponsored immigrants. The results from these propensity score estimations are presented in Table 3.3 – these estimations are the same as in the first stage of the nearest-neighbor propensity score matching strategy.

The observations for the native citizens are weighted by the normalized inverse of the propensity scores – the fitted values from the logit regressions. Those native citizens who most closely resemble the employer-sponsored immigrants will have larger weights, while those natives who are not similar to the immigrants will have smaller weights. Using these inverse probability weights, I compare the wages for employer-sponsored immigrants to those of native citizens, both before and after the immigrants become legal permanent residents. The results are presented in Table 3.A5.

As with the findings using the nearest-neighbor propensity score matching estimation, the first wages of employer-sponsored immigrants are slightly lower than those of the weighted native citizens. Following the receipt of a green card, the wages of the immigrants are 13 percent higher than those of the weighted natives. Overall, receiving a green card is accompanied by a 16.2 percent wage increase for employer-sponsored immigrants, a result which is of similar magnitude to those found using the matching strategy (see Tables 3.5A through 3.5C for comparison).

APPENDIX TABLES

Table 3.A1 States and divisions in the NIS

GATEWAY STATES	
California	New Jersey
Florida	New York
Illinois	Texas

CENSUS DIVISIONS	
NEW ENGLAND	WEST NORTH CENTRAL
Connecticut	Iowa
Maine	Kansas
Massachusetts	Minnesota
New Hampshire	Missouri
Rhode Island	Nebraska
Vermont	North Dakota
	South Dakota
MIDDLE ATLANTIC	
Pennsylvania	WEST SOUTH CENTRAL
	Arkansas
SOUTH ATLANTIC	Louisiana
Delaware	Oklahoma
District of Columbia	
Georgia	MOUNTAIN
Maryland	Arizona
North Carolina	Colorado
South Carolina	Idaho
Virginia	Montana
West Virginia	Nevada
	New Mexico
EAST SOUTH CENTRAL	Utah
Alabama	Wyoming
Kentucky	
Mississippi	PACIFIC
Tennessee	Alaska
	Hawaii
EAST NORTH CENTRAL	Oregon
Indiana	Washington
Michigan	
Ohio	
Wisconsin	

Note: The six gateway states are identified individually in the 2003 New Immigrant Survey. The remaining states are not identified individually, but instead are grouped by their Census Division.

Table 3.A2 Variable means for employer-sponsored immigrants and for family-sponsored immigrants in the NIS

Variable	Employer Sponsored Immigrants	Family Sponsored Immigrants
Weekly wages (2003-04)	$1079.11	$440.54
Female	0.226	0.474
Education		
Less high school	0.063	0.238
High school diploma	0.077	0.317
Associates degree	0.043	0.111
College degree	0.401	0.213
Masters degree	0.301	0.081
PhD	0.100	0.026
MD/JD	0.014	0.014
Year of birth		
Born before 1940	0.006	0.009
Born 1940-1944	0.016	0.019
Born 1945-1949	0.022	0.032
Born 1950-1954	0.059	0.048
Born 1955-1959	0.094	0.063
Born 1960-1964	0.196	0.099
Born 1965-1969	0.228	0.155
Born 1970-1974	0.316	0.227
Born 1975-1979	0.059	0.243
Born in 1980 or later	0.004	0.106
First worked in the U.S.		
1983-1990	0.123	0.104
1991-1995	0.201	0.181
1996-2000	0.629	0.354
after 2000	0.047	0.361
State/region		
California	0.165	0.241
Florida	0.024	0.070
Illinois	0.065	0.055
New Jersey	0.094	0.046
New York	0.084	0.106
Texas	0.043	0.114
New England	0.145	0.058
Middle Atlantic	0.090	0.039
South Atlantic	0.102	0.025
East South Central	0.004	0.011
East North Central	0.092	0.021
West North Central	0.045	0.042
West South Central	0.000	0.005
Mountain	0.018	0.107
Pacific	0.031	0.060
No. observations	491	568

Note: Author's calculations from the2003 New Immigrant Survey.

Table 3.A3 Top ten industries for first U.S. job among male employer-sponsored immigrants in the NIS

2002 Census Code	Industry	Percent
7380	*Computer system design and related services*	25.0%
7870	*Colleges and universities, including junior colleges*	8.3%
8680	*Restaurants and other food services*	8.2%
770	Construction	4.0%
9160	*Religious organizations*	3.9%
7390	Management, scientific, and technical consulting services	3.4%
6970	Securities, commodities, funds, trusts, and other financial investments	3.1%
6680	Wired telecommunications carriers	2.9%
7860	*Elementary and secondary schools*	1.9%
7290	Architectural, engineering, and related services	1.7%

Author's calculations from the 2003 New Immigrant Survey.
Male immigrants who adjusted their status to lawful permanent residence with an employer sponsor.

Table 3.A4 Top ten industries for first U.S. job among female employer-sponsored immigrants in the NIS

2002 Census Code	Industry	Percent
7380	*Computer system design and related services*	10.5%
8680	*Restaurants and other food services*	8.3%
9290	Private households	7.0%
7870	*Colleges and universities, including junior colleges*	5.2%
8190	Hospitals	5.2%
9160	*Religious organizations*	5.2%
7860	*Elementary and secondary schools*	3.9%
7460	Scientific research and development services	3.5%
8180	Other health care services	3.5%
7690	Services to buildings and dwellings	3.1%

Author's calculations from the 2003 New Immigrant Survey.
Female immigrants who adjusted their status to lawful permanent residence with an employer sponsor.

Table 3.A5 Log weekly wages for employer-sponsored immigrants and all native citizen controls, weighted by the inverse of the normalized propensity score

	Employer Sponsored Immigrants (Treatment)	Native Citizens (Controls)	Difference
First wage	6.414 (0.872)	6.446 (0.834)	-0.032 (0.040)
Current wage	6.984 (0.663)	6.854 (0.847)	0.130* (0.033)
Difference-in-differences			0.162* (0.052)

Notes for Table 3.A4:
*Statistically significant with $p<0.05$.
Robust standard errors
Native Citizens controls from the Merged Outgoing Rotation Groups of the CPS, 1983-2004, are weighted with the normalized inverse of the propensity score, which is estimated using a logit specification.
Employer Sponsored Immigrants are principal immigrants with employer sponsors, who adjusted their status to lawful permanent residence, surveyed in the 2003 New Immigrant Survey.

TABLES

Table 3.1 Variables means for employer-sponsored immigrants in the NIS

Variable	Employer Sponsored Immigrants	Native Citizens
Female	0.226	0.518
Education		
Less high school	0.063	0.114
High school diploma	0.077	0.488
Associates degree	0.043	0.113
College degree	0.401	0.189
Masters degree	0.301	0.082
PhD	0.100	0.007
MD/JD	0.014	0.007
Year of birth		
Born before 1940	0.006	0.158
Born 1940-1944	0.016	0.088
Born 1945-1949	0.022	0.122
Born 1950-1954	0.059	0.143
Born 1955-1959	0.094	0.156
Born 1960-1964	0.196	0.147
Born 1965-1969	0.228	0.101
Born 1970-1974	0.316	0.056
Born 1975-1979	0.059	0.023
Born in 1980 or later	0.004	0.006
First worked in the U.S.		
1983-1990	0.123	-
1991-1995	0.201	-
1996-2000	0.629	-
after 2000	0.047	-
State/region		
California	0.165	0.071
Florida	0.024	0.041
Illinois	0.065	0.040
New Jersey	0.094	0.037
New York	0.084	0.059
Texas	0.043	0.047
New England	0.145	0.091
Middle Atlantic	0.090	0.044
South Atlantic	0.102	0.123
East South Central	0.004	0.050
East North Central	0.092	0.116
West North Central	0.045	0.101
West South Central	0.000	0.035
Mountain	0.018	0.100
Pacific	0.031	0.047
No. observations	491	1,503,397

Note: Employer Sponsored Immigrants from the 2003 NIS.
Native Citizens from the 1983-2002 MORG of the CPS.

Table 3.2A Top ten industries for first U.S. job among employer-sponsored immigrants in the NIS

2002 Census Code	Industry	Percent
7380	Computer system design and related services	21.9%
7870	Colleges and universities, including junior colleges	8.7%
8680	Restaurants and other food services	7.7%
9160	Religious organizations	3.8%
0770	Construction	3.5%
7860	Elementary and secondary schools	3.3%
7390	Management, scientific, and technical consulting services	3.0%
6970	Securities, commodities, funds, trusts, and other financial investments	2.8%
6680	Wired telecommunications carriers	2.6%
8190	Hospitals	2.5%

Author's calculations from the 2003 New Immigrant Survey.
Among immigrants who adjusted their status to lawful permanent residence with an employer sponsor.

Table 3.2B Top ten industries for native citizens in the MORG of the CPS

2002 Census Code	Industry	Percent
7860	Elementary and secondary schools	9.6%
0770	Construction	8.4%
8190	Hospitals	7.3%
8680	Restaurants and other food services	6.6%
7870	Colleges and universities, including junior colleges	3.6%
4970	Grocery stores	3.5%
6990	Insurance	3.3%
6870	Banking and related activities	3.0%
8270	Nursing care facilities	2.5%
6170	Truck transportation	2.3%

Author's calculations from the 1983-2002 MORG of the CPS.
Among native U.S. citizens.

Table 3.3 Logit propensity score models: dependent variable – employer-sponsored immigrant indicator

Variables	**Employer Sponsored Immigrant**	
	First Observation	**Current Observation**
Female	-1.043*	-0.977*
	(0.116)	(0.119)
Elementary school	-0.005	-0.176
	(0.513)	(0.543)
Middle school	-2.114*	-2.372*
	(0.490)	(0.504)
High school diploma	-2.698*	-3.664*
	(0.441)	(0.457)
Associates degree	-1.393*	-2.708*
	(0.467)	(0.484)
Bachelors degree	-0.127	-1.443*
	(0.424)	(0.442)
Masters degree	1.158*	-0.453
	(0.431)	(0.448)
PhD	2.307*	0.693
	(0.458)	(0.476)
MD/JD	0.306	-1.387*
	(0.579)	(0.591)
Born 1940-1944	1.246*	0.762
	(0.679)	(0.687)
Born 1945-1949	1.287*	0.551
	(0.653)	(0.661)
Born 1950-1954	2.142*	1.255*
	(0.609)	(0.618)
Born 1955-1959	2.612*	1.629*
	(0.599)	(0.608)
Born 1960-1964	3.430*	2.380*
	(0.591)	(0.599)
Born 1965-1969	3.854*	2.594*
	(0.591)	(0.599)
Born 1970-1974	4.751*	2.959*
	(0.592)	(0.597)
Born 1975-1979	4.495*	1.586*
	(0.621)	(0.620)
Born in 1980 or later	3.690*	-0.063
	(0.940)	(0.924)
Industry, Region, and Year indicators	Yes	Yes
Pseudo R^2	0.321	0.318
Log Likelihood	-3008.13	-2203.78
Observations	1,503,888	130,251

Note: * indicates statistical significance at p<0.05. The omitted education variable is "No schooling completed." The omitted birth cohort is those born before 1940.

The first observation combines data on employer-sponsored immigrants from the 2003 NIS with native citizens from the MORG of the CPS, 1983-2002. The current observation combines data from the 2003 NIS with data from the MORG of the CPS, 2003-2004.

Table 3.4A Comparisons between employer-sponsored immigrants and native citizens in the original (unmatched) and the (single nearest-neighbor) matched sample, for the characteristics corresponding to the first weekly wages

Variable	Original Sample			Matched Sample		
	Employer Sponsored Immigrants	Native Citizens	t-test*	Employer Sponsored Immigrants	Native Citizens	t-test*
Female	0.226	0.518	-12.95	0.226	0.216	0.38
No schooling completed	0.014	0.008	1.64	0.014	0.022	-0.95
Elementary school	0.020	0.014	1.11	0.020	0.018	0.23
Middle school	0.029	0.092	-4.88	0.029	0.041	-1.05
High school diploma	0.077	0.488	-18.21	0.077	0.079	-0.12
Associates degree	0.043	0.113	-4.9	0.043	0.037	0.49
Bachelors degree	0.401	0.189	12	0.401	0.428	-0.84
Masters degree	0.301	0.082	17.77	0.301	0.257	1.57
PhD	0.100	0.007	25.04	0.100	0.108	-0.42
MD/JD	0.014	0.007	1.91	0.014	0.010	0.58
Born before 1940	0.006	0.158	-9.23	0.006	0.006	0
Born 1940-1944	0.016	0.088	-5.62	0.016	0.014	0.26
Born 1945-1949	0.022	0.122	-6.73	0.022	0.033	-0.98
Born 1950-1954	0.059	0.143	-5.29	0.059	0.081	-1.37
Born 1955-1959	0.094	0.156	-3.8	0.094	0.079	0.79
Born 1960-1964	0.196	0.147	3.07	0.196	0.204	-0.32
Born 1965-1969	0.228	0.101	9.31	0.228	0.214	0.54
Born 1970-1974	0.316	0.056	25.07	0.316	0.312	0.14
Born 1975-1979	0.059	0.023	5.21	0.059	0.053	0.42
Born 1980 or later	0.004	0.006	-0.65	0.004	0.004	0
Average P-Score diff		-			0.00065	
Observations	491	1,503,397		491	1,543	

Note: *t-tests for differences between Employer-Sponsored Immigrants and Native Citizens.
Employer Sponsored Immigrants from the 2003 NIS; Native Citizens from the MORG of the CPS, 1983-2002.

Table 3.4B Comparisons between employer-sponsored immigrants and native citizens in the original (unmatched) and the (single nearest-neighbor) matched sample, for the characteristics corresponding to the current weekly wages

Variable	**Original Sample**			**Matched Sample**		
	Employer Sponsored Immigrants	Native Citizens	t-test*	Employer Sponsored Immigrants	Native Citizens	t-test*
Female	0.226	0.541	-14.01	0.226	0.212	0.54
No schooling completed	0.014	0.003	2.63	0.014	0.008	0.91
Elementary school	0.020	0.005	4.44	0.020	0.014	0.73
Middle school	0.029	0.048	-2.05	0.029	0.024	0.4
High school diploma	0.077	0.487	-18.13	0.077	0.084	-0.35
Associates degree	0.043	0.111	-4.81	0.043	0.043	0
Bachelors degree	0.401	0.226	9.25	0.401	0.420	-0.58
Masters degree	0.301	0.091	16.14	0.301	0.310	-0.28
PhD	0.100	0.014	15.63	0.100	0.094	0.32
MD/JD	0.014	0.015	-0.18	0.014	0.004	1.68
Born before 1940	0.006	0.001	2.09	0.006	0.002	1.00
Born 1940-1944	0.016	0.050	-3.4	0.016	0.012	0.54
Born 1945-1949	0.022	0.091	-5.31	0.022	0.016	0.69
Born 1950-1954	0.059	0.123	-4.28	0.059	0.051	0.56
Born 1955-1959	0.094	0.139	-2.88	0.094	0.092	0.11
Born 1960-1964	0.196	0.138	3.65	0.196	0.206	-0.4
Born 1965-1969	0.228	0.124	6.94	0.228	0.196	1.25
Born 1970-1974	0.316	0.121	13.12	0.316	0.342	-0.88
Born 1975-1979	0.059	0.111	-3.68	0.059	0.079	-1.26
Born 1980 or later	0.004	0.069	-5.7	0.004	0.004	0
Average P-Score diff		-			0.00027	
Observations	491	129,760		491	1,255	

Note: *t-tests for differences between Employer-Sponsored Immigrants and Native Citizens.
Employer Sponsored Immigrants from the 2003 NIS; Native Citizens from the MORG of the CPS, 2003-2004.

Table 3.5A Log weekly wages for employer-sponsored immigrants and the matched control group of native citizens – nearest single neighbor matched with replacement

	Employer Sponsored Immigrants (Treatment)	Native Citizens (Controls)	Difference
First wage	6.414 (0.872)	6.481 (0.777)	-0.067 (0.055)
Current wage	6.984 (0.663)	6.916 (0.745)	0.068 (0.049)
Difference-in-differences			0.135 (0.074)

Table 3.5B Log weekly wages for employer-sponsored immigrants and the matched control group of native citizens – nearest five neighbors matched with replacement

	Employer Sponsored Immigrants (Treatment)	Native Citizens (Controls)	Difference
First wage	6.414 (0.872)	6.461 (0.818)	-0.047 (0.044)
Current wage	6.984 (0.663)	6.867 (0.833)	0.118* (0.040)
Difference-in-differences			0.165* (0.059)

Table 3.5C Log weekly wages for employer-sponsored immigrants and the matched control group of native citizens – nearest ten neighbors matched with replacement

	Employer Sponsored Immigrants (Treatment)	Native Citizens (Controls)	Difference
First wage	6.414 (0.872)	6.451 (0.813)	-0.037 (0.041)
Current wage	6.984 (0.663)	6.878 (0.823)	0.108* (0.034)
Difference-in-differences			0.145* (0.053)

Notes for Tables 3.5A-3.5C:
*Statistically significant with $p<0.05$.
Native Citizens controls are chosen using a nearest-neighbor propensity score matching strategy. Propensity score is estimated using a logit specification. Ties are equally weighted.
Standard errors on the differences are bootstrapped to account for the estimated propensity score.
Employer Sponsored Immigrants are principal immigrants with employer sponsors, who adjusted their status to lawful permanent residence, surveyed in the 2003 New Immigrant Survey.
Native Citizens are from the Merged Outgoing Rotation Groups of the CPS, 1983-2004.

Table 3.6A Log weekly wages for employer-sponsored immigrants and the matched control group of native citizens – nearest single neighbor, exact match on year of the survey

	Employer Sponsored Immigrants (Treatment)	Native Citizens (Controls)	Difference
First wage	6.414 (0.872)	6.508 (0.819)	-0.094 (0.054)
Current wage	6.984 (0.663)	6.896 (0.779)	0.088* (0.044)
Difference-in-differences			0.182* (0.070)

Table 3.6B Log weekly wages for employer-sponsored immigrants and the matched control group of native citizens – nearest five neighbors, exact match on year of the survey

	Employer Sponsored Immigrants (Treatment)	Native Citizens (Controls)	Difference
First wage	6.414 (0.872)	6.481 (0.823)	-0.067 (0.045)
Current wage	6.984 (0.663)	6.888 (0.797)	0.096* (0.038)
Difference-in-differences			0.164* (0.059)

Table 3.6C Log weekly wages for employer-sponsored immigrants and the matched control group of native citizens – nearest ten neighbors, exact match on year of the survey

	Employer Sponsored Immigrants (Treatment)	Native Citizens (Controls)	Difference
First wage	6.414 (0.872)	6.465 (0.838)	-0.051 (0.043)
Current wage	6.984 (0.663)	6.874 (0.805)	0.110* (0.036)
Difference-in-differences			0.161* (0.035)

Notes for Tables 3.6A-3.6C:
*Statistically significant with $p<0.05$.
Native Citizens controls are chosen using a nearest-neighbor propensity score matching strategy. Propensity score is estimated using a logit specification. Ties are equally weighted. Exact match on the year of the survey. Standard errors on the differences are bootstrapped to account for the estimated propensity score.
Employer Sponsored Immigrants are principal immigrants with employer sponsors, who adjusted their status to lawful permanent residence, surveyed in the 2003 New Immigrant Survey.
Native Citizens are from the Merged Outgoing Rotation Groups of the CPS, 1983-2004.

Table 3.7 Log weekly wage changes for employer-sponsored immigrants, comparing those who were still in their first job after becoming legal permanent residents to those who had changed jobs

	Kept the same job	Changed jobs
First wage	6.726	6.270
	(0.759)	(0.939)
Current wage	7.092	7.092
	(0.838)	(0.810)
Change in wages	36.6%	82.2%
No. observations	193	231

Weekly wages are inflated to constant 2006 dollars before taking the natural logarithm.

Table 3.8 Regression on log weekly wages for employer-sponsored immigrants, difference-in-difference results comparing those who changed jobs to those who stayed in the same job

Variable	
Changed jobs	0.301*
	(0.060)
Time (=1 for post green card wage)	-0.462*
	(0.069)
Changed jobs*Time	0.497*
	(0.085)
Female	-0.346*
	(0.058)
Elementary school	0.287
	(0.153)
Middle school	0.557*
	(0.169)
High school diploma	0.504*
	(0.147)
Associates degree	0.649*
	(0.164)
Bachelors degree	1.179*
	(0.132)
Masters degree	1.244*
	(0.133)
PhD	1.287*
	(0.146)
MD/JD	1.274*
	(0.227)
Born 1940-1944	0.43
	(0.293)
Born 1945-1949	0.477
	(0.262)
Born 1950-1954	0.610*
	(0.239)
Born 1955-1959	0.556*
	(0.239)
Born 1960-1964	0.524*
	(0.228)
Born 1965-1969	0.429
	(0.227)
Born 1970-1974	0.449*
	(0.226)
Born 1975-1979	0.262
	(0.244)
Born in 1980 or later	0.309
	(0.326)

Table 3.8 (continued)

Florida	0.115
	(0.149)
Illinois	0.251*
	(0.112)
New Jersey	0.360*
	(0.080)
New York	0.0464
	(0.100)
Texas	0.167
	(0.128)
New England	0.228*
	(0.088)
Middle Atlantic	0.175
	(0.093)
South Atlantic	0.107
	(0.095)
East South Central	0.202*
	(0.091)
East North Central	0.199*
	(0.088)
West North Central	0.320*
	(0.128)
Mountain	0.351*
	(0.133)
Pacific	0.225
	(0.175)
R-squared	982
No. observations	0.357

Note: *p<0.05, standard errors are robust to heteroskedasticity. Employer-sponsored immigrants from the 2003 NIS. No schooling completed is the omitted schooling variable; born before 1940 is the omitted birth cohort variable. California is the omitted region; West South Central also omitted due to no positive observations.

Table 3.9 Log weekly wages for principal employer-sponsored immigrants in the NIS, comparing those who arrived in the U.S. with a green card to those who were already living in the U.S. when they received their green cards

	7.1	7.2	7.3
New arrival with a green card	-0.211*	-0.050	-0.019
	(0.058)	(0.053)	(0.054)
Female	-	-0.300*	-0.219*
		(0.056)	(0.060)
Less than high school degree	-	-0.263*	-0.009
		(0.095)	(0.098)
Some college	-	0.024	-0.068
		(0.128)	(0.128)
College degree	-	0.652*	0.200*
		(0.082)	(0.107)
Advanced degree	-	0.865*	0.393*
		(0.083)	(0.112)
Born before 1940	-	0.023	0.135
		(0.372)	(0.356)
Born 1940-1944	-	0.457	0.376
		(0.388)	(0.305)
Born 1945-1949	-	0.518*	0.491*
		(0.270)	(0.274)
Born 1950-1954	-	0.579*	0.539*
		(0.278)	(0.271)
Born 1955-1959	-	0.579*	0.503*
		(0.251)	(0.256)
Born 1960-1964	-	0.598*	0.492*
		(0.246)	(0.254)
Born 1965-1969	-	0.482*	0.364
		(0.241)	(0.246)
Born 1970-1974	-	0.407*	0.232
		(0.240)	(0.249)
Born 1975-1979	-	0.315	0.220
		(0.243)	(0.253)
Year dummies	No	Yes	Yes
State/region dummies	No	Yes	Yes
Industry dummies	No	No	Yes
R-squared	0.0143	0.2876	0.6334
No. observations	914	914	914

*Statistically significant with p<0.10.
High school degree is the omitted education category.
Born 1980 or later is the omitted birth cohort.

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Chapter 4

The Effects of Job Displacement on Immigrant Workers

1 Introduction

As immigration to the United States has grown over the last few decades, so has the importance of immigrants in the U.S. workforce. A large proportion of workers in some industries – agriculture, construction, domestic services – and in some education classes – those with no high school education and those with PhDs in some disciplines – are now persons who were not born in the U.S. Immigrants to the United States constitute a sizable and growing fraction of the workforce, but their experiences with job displacement have not been extensively explored. In this paper, I investigate how job displacement affects workers differently based on their immigrant and citizenship status. I focus particularly on two post-displacement outcomes – the duration of unemployment and the re-employment wage.

There are many reasons why displacement outcomes might differ between immigrants and natives. Displaced workers who are willing and able to relocate geographically in order to find a new job often fare better than those who are geographically constrained – movers would likely have shorter jobless spells and higher re-employment wages because there are more jobs available to them. Immigrants and natives may differ in this geographic mobility. On one side, displaced immigrants, particularly recent immigrants, may have less attachment to a particular region and may be more willing than natives to relocate within the U.S. to find new employment (Chiswick 2000). On the other side, immigrants may be more dependent on the social capital in ethnic enclaves, and thus may be less mobile than natives (Boman 2006). These differences in geographic mobility may lead to differences in post-displacement outcomes.

Another mobility that may affect post-displacement outcomes is occupational mobility. Research by Green (1999) indicates that immigrants have more occupational mobility than natives. This greater flexibility in the labor market may help to alleviate some of the negative effects of job displacement.

Another possibility is that immigrants, particularly those who are undocumented or have only temporary legal status, might remain in jobs that are relatively poor matches if they do not want to draw attention to themselves by searching for better jobs. Displacement might be less harmful for these immigrants because it might allow them to find jobs that are relatively better fits, and pay relatively higher wages.[1]

In the following section, I outline both a job search model and a returns-to-human-capital wage model that will help to motivate the differences in the post-displacement outcomes of immigrants (both non-citizens and naturalized citizens) and native workers. I describe the Displaced Workers Survey data that I use in this analysis in section 3. In the two sections following the data description, I outline the empirical strategies that I use to compare the re-employment wages and the duration of unemployment among natives, naturalized citizens, and non-citizens. In section 6, I present and discuss the main results of my estimation. I consider how the minimum wage and the reasons for displacement may be

[1] The author would like to thank Dr. Sherrie Kossoudji for suggesting this possible scenario.

affecting the differences between natives and immigrants in section 7. The final section summarizes the main findings and concludes.

2 Theoretical Framework

In my analysis, I focus on the two central aspects of job displacement – the unemployment duration following displacement and the re-employment wage. I consider a simple version of the standard job search model.[2] I assume job offers arrive to a searching unemployed worker at random intervals according to a Poisson process with offer arrival rate π. Workers are assumed to maximize the expected present value of income over an infinite time horizon at a known and constant discount rate r. The net income flow (unemployment benefit) for an unemployed worker is b and is time-invariant throughout any given spell of unemployment. The optimal policy in this model is a constant reservation wage.

A job offer is summarized by a wage rate w; when a job is accepted it lasts forever. Successive job offers are independent realizations from a known wage offer distribution with a finite mean, μ, variance, σ, cumulative distribution $F(w)$, and density $f(w)$. There is no recall allowed. The following Bellman equation defines the optimal policy, a reservation wage w^r

$$w^r = b + \frac{\pi}{r}\int_{w^r}^{\infty}(w - w^r)\,dF(w).$$

This equilibrium condition allows me to investigate the consequences of exogenous changes in the wage offer distribution, and the offer arrival rate, π, on the expected re-

[2] See, for example, Devine and Kiefer (1991), as well as Burdett and Ondrich (1985).

employment wage, $E_w[w \mid w \geq w^r]$, and the expected jobless spell, $E[T] = \frac{1}{\tau}$, where

$$\tau = \pi \int_{w^r}^{\infty} f(w)dw = \pi(1 - F(w^r)).$$

While the job search model is particularly helpful in framing the discussion of the duration of unemployment, perhaps a more appropriate model to consider with respect to the re-employment wages of immigrants and natives would be a human capital model, where wages are a function of multiple types of human capital, such as education, work experience, firm-specific human capital, and industry-specific human capital. Following Neal (1995), let the wages on the pre-displacement job be given by

$$w_1 = \alpha * WorkExperience + \theta * IndustryTenure + \gamma * FirmTenure + X\beta + \varepsilon_1$$

and the wages on the post-displacement job by

$$w_2 = \alpha * WorkExperience + \theta * IndustryTenure + X\beta + \varepsilon_2$$

if the worker remains in the same industry and

$$w_3 = \alpha * WorkExperience + X\beta + \varepsilon_3$$

if the worker changes industry between the pre-displacement and the post-displacement job. The vector X contains worker characteristics that affect wages, such as educational attainment.

Both of these models help to illustrate why we might expect to see differences between immigrants and natives (and between non-citizens and naturalized citizens) in the duration of unemployment and the re-employment wages. In the human capital model, immigrants, particularly those who have not lived in the United States for very long, are likely to have much lower levels of firm-specific and industry-specific human capital. Thus the differences between the post-displacement wages (w_2, w_3) and pre-displacement wages (w_1) of immigrants is likely to be much lower than that difference for native workers. The lack of industry- and firm-specific tenure among the immigrant workers may mean that their wages do not fall as much as the wages of native workers.

In the standard job search model, lower unemployment benefits (b) will depress the reservation wage and thus the re-employment wage. In general, immigrants, particularly non-citizens, may have less information about their eligibility for unemployment benefits, and they may also not meet job tenure or other legal qualifications to receive them. Therefore, lower access to unemployment benefits would tend to reduce immigrants' re-employment wages as compared to natives. Additionally, immigrant workers, particularly those who have not lived in the U.S. for very long, may lack the necessary information about the U.S. labor market to have a job offer arrival rate (π) that is similar to that of native workers. This would tend to both lower the re-employment wage and lengthen the jobless spell.

Workers with less skill and education have lower probabilities of leaving unemployment, i.e. longer unemployment duration, and also lower re-employment wages (Farber 2005; Addison and Portugal 1989). On average, displaced immigrants have lower levels of education (see Table 4.1) and English language ability than do displaced natives. One might then expect to see worse post-displacement outcomes for immigrant workers as compared to native workers.

However, foreign-born workers in the U.S. tend to have a much stronger relationship with the labor market than natives; their labor force participation rates are much higher and they are more likely to work multiple jobs (LaLonde and Topel 1992). Higher search intensity on the part of the immigrants could increase the arrival rate of job offers (π), which would lead to shorter jobless spells and higher re-employment wages. Networks of immigrants from the same country may serve as a source of job offers for displaced immigrants, which could also increase the offer arrival rate and result in higher re-employment wages and less time spent unemployed.

Naturalized citizens and non-citizens may also have very different post-displacement outcomes from each other. Grouping the two together could potentially mask differences within the immigrant population and between natives and these subgroups of immigrants. Naturalized citizens, who have lived longer in the U.S. and are likely to have better English language skills as well as other human capital valued in the labor market, might be more similar to native workers more in their post-displacement outcomes. Non-citizens may be more disadvantaged compared to naturalized citizens, and they are more likely to differ from the native population.

3 Data

To investigate the differences in the effects of job displacement between foreign-born and native workers, I use data from the only large-scale and nationally representative survey of displaced workers – the Displaced Workers' Survey (DWS), a biennial supplement to the January or February Current Population Survey (CPS) conducted by the U.S. Bureau of

Labor Statistics (BLS).[3] The first DWS was instituted in January of 1984, but workers in the survey were not identified by their citizenship status and country of birth until February of 1994. Therefore, I limit my analysis to the years following 1994.

The surveys conducted in and before 2002 relied upon the Standard Industry Classification system for industry and occupation categories, while the surveys after 2002 utilized the North American Industry Classification System for industry and occupation categories. As these two systems cannot be correlated, my analysis must trade-off between a larger sample size and consistent controls for industry and occupation. I complete the primary analysis using all available data from 1994 through 2006 (seven DWS cross-sections), which contains information about individuals displaced between the years of 1991 and 2005. I also include robustness checks using only the data from 1994 through 2002 but controlling for displaced workers' former industries and occupations.

In addition to personal characteristics found in the regular monthly CPS, the DWS collects information on both old and new employment for displaced workers – previous and current wages, hours, current industry, industry of displacement, reason for displacement, occupation, and duration of unemployment. I use data on workers who were between the ages of 20 and 65 at the time of the survey, displaced from a full-time job but still in the labor force at the time of the survey.[4] I supplement the DWS with data on the annual unemployment rate in each state, to better control for the labor market conditions in the local economy; this data is available from the Bureau of Labor Statistics (BLS). Consumer Price

[3] DWS data used in this paper is available through the Inter-University Consortium for Political and Social Research.

[4] Also, as suggested by Angrist and Krueger (1999), I "winsorized" displaced workers' pre-displacement and re-employment wages at the tails, replacing values in the lower or upper 1 percent tails with values at the 1st and 99th percentiles, respectively.

Index (CPI) data from the BLS are also used to adjust the wage amounts from all years of the survey into 2006 dollars.

Summary statistics are presented in Table 4.1, for the sample of individuals between the ages of 20 and 65 who were displaced from full-time employment within the three years prior to being surveyed. This sample is split into displaced male and displaced female workers, and further sub-divided into natives and immigrants, as the comparison of post-displacement outcomes between natives and immigrants is the focus of this paper. Note that immigrants constitute roughly 10 percent of both the male and the female samples; immigrants are defined as individuals who were not born in the United States and who were not born U.S. citizens. Some of the immigrants, however, have become naturalized citizens. About one third of male immigrants in the DWS sample are naturalized citizens, and 45 percent of the female immigrants have naturalized. The remaining immigrants are non-citizens.

In this sample, immigrants overall have noticeably different patterns of educational attainment when compared to natives. About 30 percent of immigrant workers displaced from full-time employment, both male and female, have less than a high school degree; while the same proportion for natives is less than 10 percent. At the other end of the education spectrum, a higher proportion of immigrants have obtained advanced degrees when compared with natives (10.9 percent versus 6.9 for males, 10.5 percent versus 5.8 percent for females). Higher proportions of immigrants are married and live in metropolitan areas than natives. On average, immigrants have fewer years of tenure on their pre-displacement jobs, and longer jobless spell durations than natives.

Next, Table 4.2 provides summary statistics for workers who were displaced from full-time employment and re-employed full-time by the time of the survey. For both male and female displaced workers, the weekly wages from their post-displacement jobs are lower than the weekly wages from their pre-displacement jobs (reported in 2006 dollars); this wage decrease is roughly \$94 per week for displaced native male workers and \$73 per week for displaced native female workers. Notice, however, that immigrants who are re-employed experience much smaller wage losses after displacement. Average weekly wages for displaced immigrant workers decrease by \$39 for men and by \$37 for women. Both male and female natives experience on average a 10 percent wage drop following displacement and re-employment; but for immigrants, both male and female, the average wage drop following displacement and re-employment is only 5 percent.

4 Empirical Framework: Re-employment Wages

The unit of analysis is a displaced worker in the DWS. Regression equation (1) below relates the first outcome of interest, the logarithm of the weekly re-employment wage for an individual *i*, currently employed in year of the survey *k*, displaced from previous employment in year *t*, and residing in state *s*, to a host of personal characteristics as well as an indicator for being a non-citizen immigrant and another indicator for being a foreign-born naturalized citizen (with natives being the excluded category):

$$(1) \qquad \ln(w_{ikts}^{re-employment}) = \beta_0 + \mathbf{X}_{ikts}\boldsymbol{\beta}_1' + \beta_2 NonCitizen + \beta_3 NativeCitizen + \delta_k + \tau_t + \sigma_s + \varepsilon_{ikts}.$$

In equation (1), $\mathbf{X}_{ikts}$ is a vector of personal characteristics, containing the typical covariates used in the re-employment wage literature (see, for example, Addison and Portugal, 1989).

Included in $\mathbf{X}_{ikts}$ are the standard Mincerian controls for age and age squared (to proxy for experience) and education (indicators for no high school, high school dropout, some college, college graduate, and advanced degree, with high school graduates as the omitted category). I control for race and marital status – demographic characteristics that affect wages. The vector $\mathbf{X}_{ikts}$ contains an indicator for living in a metropolitan area, since wages in metropolitan areas tend to be higher than in rural areas, and also an indicator for union status. I include tenure on the lost job among the covariates; this controls for the loss of job-specific human capital when workers are displaced. I will estimate the wage equation both with and without tenure as a covariate, to determine how controlling for tenure may mediate some of the differences between immigrants and natives. Farber (2005) and others find a strong negative relationship between the length of tenure on the lost job and the change in earnings from the pre-displacement job to the re-employment job. The final covariate is state unemployment rate, which helps to control for the local labor market conditions the displaced worker faced. Higher state unemployment rates would likely lower the re-employment wage by reducing the frequency with which job offers arrive. Regression (1) is run separately for male and for female workers.

To control for time-invariant state of residence characteristics (such as the generosity of unemployment benefits, which in the model tend to increase re-employment wages), I include state of residence fixed effects – σ_s. Year of displacement and year of the survey fixed effects, τ_t and δ_k, are added to absorb annual economy-wide shocks in the year of displacement and year of the survey. The individual specific error term, ε_{ikts}, is assumed to be have mean zero. I estimate equation (1) by Ordinary Least Squares (OLS).

Since not every displaced worker is re-employed by the date of the survey, I do not have information on the re-employment wages for those who are still unemployed at the date of the interview. A selection problem may arise, since those who were most recently displaced have had little time to find new jobs. As the re-employment wage regression (1) is linear, and as it includes controls for both year of displacement and year of the survey, it also effectively controls for the number of years since displacement, thus mitigating the potential selection bias.[5]

I also employ a conventional two-step selection adjustment procedure to control for the potential selection bias (see Heckman, 1979). Because they are intrinsically associated with the re-employment censoring mechanism, following Addison and Portugal (1989), the year of displacement and the year of the survey dummies are excluded from the wage equation (1) and only enter the re-employment (selection) probit equation. Additionally, I include reason for displacement dummies in the selection equation, as they might affect the probability of re-employment.

5 Empirical Framework: Unemployment Duration

The jobless spell durations in the DWS are recorded in weeks. Following McCall (1996), I group the durations into two-week intervals, to reduce the possible bias from piling the reported unemployment durations at even weeks as evident from inspection of the data. Since the unemployment duration data are discrete, again following McCall (1996) I take a grouped data approach (see Kiefer, 1988; Han and Hausman, 1990; Meyer, 1990; Lancaster, 1990; and Wooldridge, 2002).

[5] Number of years since displacement is a linear combination of the dummies for the survey year and the dummies for the year of displacement.

First, I convert the unit of analysis from a displaced worker to a jobless spell interval (two-week period) at risk of leaving the unemployment pool. I divide the time line into 81 intervals, [0,2), [2, 4), …, [160, ∞) as there are no observed durations greater than 160 weeks. Following Wooldridge (2002), for a displaced worker *i*, I define $c_{i,m}$ to be a binary censoring indicator equal to unity if the duration is right-censored in the interval *m*, *m* = 1, 2, …, 81, and zero otherwise. Note that $c_{i,m}=1$ implies that $c_{i,m+1}=1$, as well. There are two potential sources of right-censoring in the data. First, durations in the DWS were top-coded at 168 weeks. The longest reported duration for this sample of displaced workers was 160 weeks, so this top-coding is not binding. Second, some workers were still unemployed at the date of the survey; this is the only source of right-censoring in this population. I define $y_{i,m}$ to be a binary indicator equal to unity if displaced worker's *i* unemployment duration ends in the m^{th} interval and zero otherwise. Hence, $y_{i,m}=1$ implies that $y_{i,m+1}=1$. If duration is censored in the m^{th} interval ($c_{i,m}=1$), I set $y_{i,m}\equiv 1$. For each displaced worker *i*, I observe ($y_{i,m}$, $c_{i,m}$).

Given a hazard function $\phi(t;\mathbf{Z}_i,\boldsymbol{\eta})$, where $\boldsymbol{\eta}$ is the vector of parameters to be estimated, and $\boldsymbol{Z}_i$ the matrix of personal and industry characteristics, I can now calculate all the probabilities that $y_{i,m}$ takes on a value of zero or one given ($y_{i,m-1}$, …, $y_{i,1}$), ($c_{i,m}$, …, $c_{i,1}$), and $\boldsymbol{Z}_i$.[6] Wooldridge (2002) calculates the only two such probabilities that are not identically zero or one: $\mathrm{P}(y_{i,m}=1\,|\,y_{i,m-1}=0,\mathbf{Z}_i,c_{i,m}=0)=1-\alpha_m(\mathbf{Z}_i,\boldsymbol{\eta})$, and $\mathrm{P}(y_{i,m}=0\,|\,y_{i,m-1}=0,\mathbf{Z}_i,c_{i,m}=0)=\alpha_m(\mathbf{Z}_i,\boldsymbol{\eta})$, for *m* = 1, 2, …, 81, where

$$\alpha_m(\mathbf{Z},\boldsymbol{\eta})\equiv\exp\left[-\int_{a_{m-1}}^{a_m}\phi(t;\mathbf{Z},\boldsymbol{\eta})ds\right].$$

[6] Note that by definition, these probabilities only depend on $y_{i,m-1}$, $c_{i,m}$, and $\mathbf{Z}_i$.

I can now specify the log-likelihood function to be maximized as

(2) $$\log L_1 = \sum_{i=1}^{\mathrm{N}} \sum_{h=1}^{m_i-1} \log[\alpha_h(\mathbf{Z_i}, \boldsymbol{\eta})] + d_i \log[1 - \alpha_{m_i}(\mathbf{Z_i}, \boldsymbol{\eta})],$$

where d_i is a censoring indicator equal to unity if duration of displaced worker i is uncensored, and N is the number of displaced workers included in the analysis.

Before I can implement conditional MLE, I need to specify the hazard function, $\phi(t; \mathbf{Z}_i, \boldsymbol{\eta})$. The following Weibull hazard function captures a monotonically increasing or monotonically decreasing hazard:

(3) $$\phi(t; \mathbf{Z}_i, \boldsymbol{\eta}) = \exp(\mathbf{Z}_i \boldsymbol{\eta}) \varphi t^{\varphi-1}.$$

If $\varphi > 1$, the hazard exhibits positive duration dependence, and if $\varphi > 1$, it exhibits negative duration dependence.[7] For further computational simplicity, one can assume that the grouped data is continuous instead of discrete and estimate the Weibull model maximizing the following log-likelihood function:

(4) $$\log L_2 = \sum_{i=1}^{\mathrm{N}} \{d_i \log[f(t_i \mid \mathbf{Z}_i, \boldsymbol{\eta})] + (1 - d_i) \log[1 - F(t_i \mid \mathbf{Z}_i, \boldsymbol{\eta})]\},$$

where the Weibull distribution with covariates has the following conditional density

$$f(t_i \mid \mathbf{Z}_i; \boldsymbol{\eta}) = \exp(\mathbf{Z}_i \boldsymbol{\eta}) \varphi t^{\varphi-1} \exp[-\exp(\mathbf{Z}_i \boldsymbol{\eta}) t^{\varphi}].$$

The choice of the Weibull model is appealing because it has an accelerated failure time (AFT) representation. The estimated coefficients in the AFT representation can be interpreted as semi-elasticities of the expected unemployment duration with respect to a

[7] If $\varphi = 1$, the Weibull hazard reduces to an exponential one and has no duration dependence.

given covariate. This is useful as I am primarily interested in how the observed covariates, in particular the immigrant and citizenship status, affect the jobless spell duration.

Unlike the re-employment wage regressions, the jobless spell duration regressions avoid potential selection issues by incorporating duration information from both re-employed workers and workers who are still unemployed at the time of the survey (but who report being in the labor force). For the latter group, I only observe interrupted (right-censored) spells, which were accommodated in the likelihood function.

The matrix $\mathbf{Z}_i$ includes a vector of personal characteristics, which were described in the previous section. They include education, current age, current age squared, tenure on the lost job, state unemployment rate in the year of displacement, and dummies for race, marital status, and metropolitan area residence status. In addition, $\boldsymbol{Z}_i$ includes state of residence fixed effects (σ_s), and year of displacement and year of the survey fixed effects (τ_t , δ_k). Finally, $\boldsymbol{Z}_i$ includes indicators for non-citizens and naturalized citizens, which are the covariates of interest. I estimate (4) separately for male and female displaced workers.

6 Results

To translate the coefficients from the Weibull hazard specification described in the previous section into AFT coefficients, it is necessary to divide the hazard coefficients by the negative of the duration dependence parameter α. The advantage of considering the estimates in the AFT models is that the coefficients are easily interpreted as semi-elasticities. The following results on the jobless spell duration are reported as AFT coefficients.

Without controlling for any covariates other than the immigrant and citizenship status of the worker, displaced male naturalized citizens are estimated to experience 31.9 percent longer jobless spell durations than displaced male native workers (Table 4.3, column 4.3.1). However, there is no significant difference between the duration of unemployment between native workers and non-citizens. Among displaced female workers, both non-citizens and naturalized citizens have significantly longer unemployment durations than native workers when no other covariates are included in the estimation, 29.5 percent longer for non-citizens and 31.9 percent longer for naturalized citizens (Table 4.4, column 4.4.1).

The importance of immigrant and citizenship status to jobless spell duration falls when educational attainment variables are included as controls (Table 4.3, column 4.3.2 for males, and Table 4.4, column 4.4.7 for females). When controls for education are added, the coefficients for being a non-citizen fall much more than the ones for being a naturalized citizen, reflecting that naturalized citizens have a more similar education profile to natives than non-citizens do. Including the state unemployment rate in the year of displacement, as well as other personal characteristics, and the tenure on the previous job further reduces the importance of immigrant and citizenship status on the jobless spell duration for both male and female displaced workers (third columns of Tables 4.3 and 4.4). With these controls included in the regression specification, only displaced male naturalized citizens have significantly longer durations of unemployment than native males. Further controlling for state, year of displacement, and year of the survey effects (fourth columns of Tables 4.3 and 4.4), reduces the coefficients for both male and female naturalized citizens and non-citizens. Still, for displaced male workers, naturalized citizens have significantly longer jobless spells than native workers. These immigrants experience a 17.6 percent longer duration of unemployment, which at the mean, translates into about 5.5 addition weeks of unemployment.

As an additional robustness check, I limit the sample to those who were surveyed in 2002 or earlier, so that I can include consistent industry and occupation controls in the regressions. Neither naturalized citizens nor non-citizens have significantly different jobless spells when compared to natives when industry and occupation are included in the regression, but, at least for the displaced male workers, this is partly due to the loss in sample size (from 10,096 to 6,585) and the corresponding increase in the standard errors when 2004 and 2006 data is removed.

While it is only (male) naturalized citizens that experience longer jobless spell duration than their native counterparts, the significant differences when considering re-employment wages are between native citizens and non-citizens. Tables 4.5 and 4.6 report the results from the OLS estimation of equation (1). In these specifications, only workers who were displaced from full-time jobs and re-employed in full-time jobs are included.[8] Displaced non-citizen males experience re-employment wages that are on average 23.6 percent lower than those of otherwise similar displaced native males (column 4.5.1). This re-employment wage gap is similar to the white-black re-employment wage gap, which for males is 20.2 percent. For naturalized citizens, however, the re-employment wage gap is only about half the size of the gap for non-citizens, 11.0 percent versus 23.6 percent. This likely reflects the fact that naturalized citizens have lived in the U.S. longer and are more familiar with the U.S. labor market.

A similar pattern emerges for displaced female workers. Female non-citizens have re-employment wages that are 23.4 percent lower that those of similar natives, and this wage

[8] For robustness, I include also the displaced workers who were re-employed part-time, while controlling for hours worked in the regression (results not shown).   The re-employment wage gap between displaced immigrant and native workers, both for males and for females, is not significantly different from the gap reported in Tables 4.5 and 4.6 when these displaced workers are excluded.

gap is larger in magnitude than the white-black re-employment wage gap. There is no difference in the re-employment wages of naturalized citizens and native workers who are female.

Compare the wage re-employment wage gaps in the first columns of Tables 4.5 and 4.6 to the pre-displacement wage gaps in the second columns of those same tables (where the dependent variable is the natural log of the pre-displacement weekly wages). The re-employment wage gap between displaced native foreign-born workers (both non-citizens and naturalized citizens) is smaller in magnitude than the wage gap that existed prior to displacement. This is not the case for black workers – for males the black-white wage gap is almost the same for both re-employment and pre-displacement wages, and for females, the re-employment black-white wage gap is actually larger. Prior to displacement, non-citizen males were earning 29.5 percent less than their native counterparts, but that falls to 23.6 percent less after displacement and re-employment. For displaced non-citizen females, the former gap was 30.4 percent, but it falls to 23.4 percent when they are re-employed. Displacement in some way appears to reduce the wage gap between native and foreign-born workers, particularly for the non-citizens.

To further explore the idea that displaced and re-employed non-citizens experience relative wage gains in comparison to native workers, I modify equation (1) such that the dependent variable is the natural log of the ratio of the weekly re-employment wages to the weekly pre-displacement wages. This wage ratio is significantly higher for non-citizens than it is for natives. From the third column of Table 4.5 (males) and Table 4.6 (females), non-citizens experience wage gains relative to natives following displacement and re-employment. For males, their re-employment wages relative to their pre-displacement wages are 5.9 percent higher than the ratio for native wages; and for females, the difference is 7.0 percent. This is consistent with my earlier findings that the wage gap between non-citizens and natives is higher in the pre-displacement wage than in the re-employment wage. Naturalized citizens, on the other hand, do not experience these relative wage gains following displacement and re-employment. For displaced black workers as well, there is no relative wage gain.

I do not have re-employment wages for every displaced worker due to the fact that many of them have not been re-employed at the time of the survey. To correct for the potential selection into re-employment, I use a two-step Heckman procedure. These results are presented in Table 4.7 for males and Table 4.8 for females. The first columns of these tables present the results for the probit regression. Here, I use all of the displaced workers, those who have been re-employed and those who have not. The dependent variable is an indicator equal to 1 if the individual is re-employed at the time of the survey. In addition to the other control variables included in equation (1), I use the reasons for displacement as well as the year of displacement and year of the survey dummies to identify the probability of re-employment.[9] Note that year of displacement and the year of the survey are intimately related to the re-employment probability since these determine the time since displacement, a strong predictor of re-employment at the time of the survey.

In column 4.7.2 (males) and in column 4.8.2 (females), the inverse Mills ratio calculated from the selection regression is included among the controls. In the end, correcting for selection does not significantly change the re-employment wage gap between displaced non-citizen and native males, which remains around 23 percent. For displaced

[9] I do not include the year of displacement and year of the survey dummies in the re-employment wage regressions in Tables 4.7 and 4.8. These controls only enter into the selection regression.

female workers as well (column 4.8.2), correcting for selection does not affect the magnitude of the re-employment wage gap between non-citizens and natives, which is also estimated around 23 percent. When I include the inverse Mills ratio as a covariate, both male and female displaced non-citizen workers still have statistically significantly higher ratios of re-employment wages to pre-displacement wages than natives do (columns 4.7.3 and 4.8.3).

7 Discussion

As discussed in the Theoretical Framework, greater tenure on the pre-displacement job leads to lower re-employment wages (and thus a lower re-employment to pre-displacement wage ratio), due to the loss of firm-specific or industry-specific human capital. For foreign-born workers, particularly non-citizens who have only lived in the U.S. for a few years and have little tenure on their pre-displacement jobs, this lack of tenure may partially explain why their wages do not fall as much as those of natives following displacement. In Table 4.9, I compare the differences in the re-employment to pre-displacement wage ratio between non-citizens, naturalized citizens, and natives when tenure on the previous job is excluded as a covariate to the results when tenure is included. The relative wage gain experienced by non-citizen workers following displacement is about ten percent larger in magnitude when tenure is excluded as an explanatory variable, for both males and females. The lack of tenure on the previous job for non-citizens then accounts for some of the difference in the wage ratio between non-citizens and natives, but even when controlling for tenure, a significant difference remains between the two populations.

Another possible explanation for the finding that foreign-born workers, particularly non-citizens, experience relative (in comparison to natives) wage gains following displacement and re-employment involves the minimum wage. In this sample, immigrants have lower levels of education than native workers, and so would be more likely to be re-employed at minimum wage jobs. It is possible that, in the absence of the minimum wage, immigrant workers would experience a proportionally similar decrease in their wages to that of native workers following displacement and re-employment.

To examine the potential effect of the minimum wage on the re-employment wages, I first compare the minimum wage in the state of residence in the year of the survey to the re-employment wage.[10] Though the re-employment wages used in the analysis are weekly wages, for this comparison I use hourly wages. For those individuals who do not report hourly wages, I divide their weekly wages by the number of hours they report working in the previous week.

Overall, a very small percentage of the displaced workers have re-employment wages that are constrained by the minimum wage, only 3.3 percent for males and 5.0 percent for females (see Table 4.10). However, non-citizen immigrants are much more likely to be working at the minimum wage than natives or naturalized citizens, with 8.2 percent of non-citizen males and 13.3 percent of non-citizen females employed at minimum wage jobs. For robustness, I replicate the regressions from Tables 4.5 and 4.6 without those individuals who receive the minimum wage (or lower). There are very few changes when the minimum wage earners are excluded (results not shown). For non-citizen males, the relative wage gain is 5.5 percent (st. dev. 2.3), which is comparable to the 5.9 percent (from Table 4.5.3) that resulted when minimum wage earners were included. Eliminating those at the minimum wage

[10] State and federal minimum wage data available for even years from 1994-2006 from the U.S. Department of Labor at: http://www.dol.gov/esa/programs/whd/state/stateMinWageHis.htm. Where state and federal law set different minimum wage rates, the higher standard applies. In about 80 percent of the states across the seven years, the federal minimum wage is the higher standard.

actually increases the magnitude of the relative wage gain among non-citizen females, from 7.0 percent in Table 4.6.3 to 9.1 percent (st. dev. 3.0). Thus, the lower bound of the minimum wage does not appear to be driving the relative wage gain experienced by non-citizens following displacement and re-employment.

The differences between natives and immigrants in their reasons for displacement could potentially be driving some of the differences in their duration of unemployment and re-employment wages. From Table 4.1, notice that, compared to native workers, both male and female immigrants are more likely to be displaced due to a plant closing or insufficient work, and less likely to be displaced due to a shift or position being abolished. Further sub-dividing the immigrants into non-citizens and naturalized citizens shows that for males, non-citizens are more likely to have been displaced due to insufficient work (49.7% vs. 38.6% for the naturalized citizens), and naturalized citizens are more likely to have been displaced due to plant closing (43.3% vs. 39.1% for the non-citizens) or having their position or shift abolished (25.6% vs. 18.1% for the non-citizens). Among displaced female immigrants, reasons for displacement are fairly similar between naturalized citizens and non-citizens. These differences in the reasons for displacement, particularly for the displaced males, could help explain why naturalized citizens have longer jobless spells.

To examine how the reasons for displacement might be affecting the results for the duration of unemployment, I include indicators for these reasons in the vector of covariates $\boldsymbol{Z}_i$ and maximize the likelihood function in equation (4). These results, for both male and female displaced workers, are presented in Table 4.11, side-by-side with the coefficients from Tables 4.3 and 4.4 where the reasons for displacement are not included. In both column 4.11.1 and column 4.11.3, we see that the reasons for displacement are significant covariates affecting the duration of unemployment. Displacement that is due to having a shift or position abolished increases the duration of unemployment more than displacement due to insufficient work or plant closing or relocating. However, including the reasons for displacement in the duration regressions does not change the differences between immigrants and natives. Displaced males who are naturalized citizens still have jobless spells that significantly longer than those of natives (see column 4.11.1).

Including the reasons for displacement in the estimation of the re-employment wage and the ratio of the re-employment wage to the pre-displacement wage does not affect the coefficients for either the non-citizens or the naturalized citizens (not shown). In contrast to the unemployment duration, where the reasons for displacement themselves have statistically significant coefficients, these reasons do not significantly affect the re-employment wage for males. This further validates the use of the reasons for displacement as instruments in the Heckman selection regression, since the reasons affect the duration of unemployment (which is related to the probability of leaving unemployment) but not the re-employment wages.

Another possibility is that the wages of non-citizens do not fall as much as those of native workers following displacement due to the lack of job mobility that results from the lack of citizenship. Non-citizens, particularly those who are undocumented or who have only temporary legal status, may prefer not to draw attention to themselves by searching for better and higher-paying jobs. They might remain in jobs that are relatively poor matches (compared to the match between native workers and their jobs) if they believe that course of action will help them to stay in the U.S. longer. Also, foreign-born workers who want their employers to sponsor them for legal permanent residence may be willing to forego wages in return for the benefit of receiving a green card (Kandilov 2008). Involuntary job loss may allow both these groups of non-citizens to find jobs that are relatively better fits, and pay relatively higher wages, compared to the pre-displacement and re-employment jobs of native workers.

8 Conclusions

When naturalized citizens and non-citizens experience job displacement, their post-displacement labor market outcomes do differ from those of native workers. On average, displaced male naturalized citizens have longer jobless spell duration than displaced native workers, and this effect is both economically and statistically significant. At the mean, a difference in duration of unemployment of 17.6 percent corresponds to 5.5 additional weeks of unemployment for the displaced naturalized citizens.

For re-employment wages, it is non-citizen immigrants that have significant differences from the native population. Both male and female non-citizens have lower re-employment wages when compared to native workers, but the re-employment wage gap is smaller than the wage gap prior to displacement. Following displacement and re-employment, non-citizens experience a 5.9 percent (for males) and 7.0 percent (for females) wage increase relative to native workers. Displacement and subsequent re-employment seems to narrow the wage gap between immigrants and natives, as non-citizens do not experience as great of a drop in wages post-displacement as natives do. This is not the case for other potentially disadvantaged workers (such as black workers), and these results do not appear to be driven by the minimum wage acting as a lower bound for the re-employment wages of non-citizens.

This paper examines not only the differences between immigrants and natives, but also the differences within subgroups of the immigrant population (specifically, between non-citizens and naturalized citizens) in post-displacement outcomes. In doing so, it sheds further light on how the citizenship status of immigrants affects their relationship with the U.S. labor market. In this case, the differences between naturalized citizens and non-citizens are likely due to the amount of time that they have lived in the U.S. On average, naturalized citizens in this sample have 11 more years of U.S. residence than non-citizens (21 years vs. 10 years). The longer jobless spells experienced by male naturalized citizens may reflect that they have greater access to unemployment benefits than non-citizens do. Additionally, they may have better credit or stronger social support as a result of being more established as U.S. citizens.

For non-citizens, there are a variety of factors that could lead to the smaller wage decreases following displacement. Their shorter tenure in the U.S. may indicate that they are still adapting to the U.S. labor market. LaLonde and Topel (1992) show that in the first few years after arriving in the U.S., immigrants experience fairly rapid growth in wages. The longer immigrants live in the U.S., the more they learn about the U.S. labor market and the more they are able to move to better and higher-paying jobs. Non-citizens on average have less tenure on their pre-displacement jobs, which helps to decrease the wage gap between pre-displacement and re-employment wages. As I show in Table 4.9, though, this only explains a small portion of the differences between displaced non-citizens and natives. Higher re-employment wages for non-citizen immigrants are consistent with the idea that these non-citizens are more intense in their job searches following displacement. Finally, displaced non-citizens may experience relative wage gains following displacement if they had previously avoided job search due to undocumented or temporary status, and involuntary job loss forces them to find employment that is a relatively better fit.

APPENDIX

Piecewise-constant proportional hazard

Another more flexible choice for the hazard function, $\phi(t;\mathbf{Z}_i,\boldsymbol{\eta})$, in the duration of unemployment specification is a piecewise-constant proportional hazard

$$\phi(t;\mathbf{Z}_i,\boldsymbol{\eta}) = \exp(\mathbf{Z}_i\boldsymbol{\eta})\phi_m, \tag{5}$$

for m =1, 2, …, 81, and $m\text{-}1 \leq t < m$. To check for robustness, I also use this piecewise-constant proportional hazard function and compare the results to the ones found using the Weibull hazard. For identification, I estimate interval-specific baseline hazard rate, ϕ_m, for all intervals in which there is at least one exit from the unemployment pool, and I suppress the constant in $\boldsymbol{Z}_i$. With the hazard rate assumptions in place,

$$\alpha_m(\mathbf{Z}_i,\boldsymbol{\eta}) \equiv \exp[-\exp(\mathbf{Z}_i\boldsymbol{\eta})\phi_m],$$

for m =1, 2, …, 81, and I use conditional maximum likelihood to estimate (2), where $\boldsymbol{\eta}$, and ϕ_m are the parameters to be estimated. The matrix $\boldsymbol{Z}_i$ includes the same covariates described in the Empirical Framework section of the paper. I estimate (2) separately for male and female displaced workers.

In the first two columns of Table 4.A1, I report the results for the hazard of leaving unemployment, using the flexible specification (5) in the log-likelihood function (2), separating the sample into male (column 4.A1.1) and female (column 4.A1.2) workers. Consistent with the findings in the main analysis, the likelihood of leaving the unemployment pool is significantly lower for displaced male naturalized citizens compared to displaced male native workers. For female naturalized citizens and for both male and female non-citizens, the hazard of leaving unemployment is not significantly different from that of natives.

For comparison, the next two columns (4.A1.3 for males and 4.A1.4 for females) display the results for the hazard of leaving unemployment using the Weibull hazard model from equation (3). Note that the coefficients from the Weibull specification are very similar in sign and magnitude to the coefficients that result from the more flexible proportional rate hazard model. Thus the restricting assumption that the hazard rate of leaving unemployment is monotonically decreasing does not seem to bias the estimated coefficients on the covariates, since using the more flexible piecewise-constant proportional hazard results comparable coefficients.

The final two columns of Table 4.A1 repeat the AFT representations of the Weibull hazard model that were presented in the fourth columns of Tables 4.3 and 4.4. To translate the coefficients from the Weibull hazard specification into AFT coefficients, it is necessary to divide the hazard coefficients by the negative of the duration dependence parameter α. As discussed in the main analysis, the advantage of considering the estimates in the AFT models is that the coefficients are easily interpreted as semi-elasticities.

Controlling for pre-displacement wages in the re-employment wage regressions

In the third columns of Tables 4.5 through 4.7, I report the results from estimating equation (1) on the dependent variable of the natural log of the ratio of the weekly re-employment wage to the weekly pre-displacement wage. This is mathematically equivalent to including the (log of the) pre-displacement wage as an independent variable in a regression where the re-employment wage is the dependent variable, and restricting the coefficient on the pre-displacement wage to equal 1. However, that restriction may not be supported by the data (see Addison and Portugal 1989). As a robustness check, I estimate equation (1) with the pre-displacement wage included in the covariates, without restricting the value of the coefficient. The results are presented in Table 4.A2, and include also specifications where I correct for selection.

For both male and female displaced workers, the wages they were receiving before displacement are highly correlated with their re-employment wages, but the coefficients are not equal to 1. The re-employment wage gap between non-citizens and natives is now 6.8 percent for males and 7.5 percent for females. These are similar in magnitude to the black-white wage gaps of 8.2 percent for both males and females.

APPENDIX TABLES

Table 4.A1 Hazard of leaving unemployment and duration of unemployment

	Hazard Rate (flexible specification)		Hazard Rate (Weibull)		Duration (Weibull AFT)	
	4.A1.2	4.A1.2	4.A1.3	4.A1.4	4.A1.5	4.A1.6
Variable	Male	Female	Male	Female	Male	Female
Non-citizen	-0.041	-0.103	-0.031	-0.126	0.034	0.136
	(0.049)	(0.069)	(0.060)	(0.073)	(0.064)	(0.079)
Naturalized citizen	-0.130*	-0.043	-0.163*	-0.071	0.176*	0.077
	(0.060)	(0.073)	(0.067)	(0.074)	(0.073)	(0.080)
No High School	-0.140	-0.308*	-0.127	-0.323*	0.137	0.350*
	(0.074)	(0.114)	(0.092)	(0.111)	(0.099)	(0.120)
High School Drop	-0.226*	-0.223*	-0.213*	-0.180*	0.230*	0.195*
Out	(0.044)	(0.064)	(0.050)	(0.069)	(0.054)	(0.075)
Some College	0.027	0.026	0.030	0.016	-0.032	-0.017
	(0.028)	(0.034)	(0.031)	(0.038)	(0.034)	(0.041)
College Degree	-0.025	0.107*	-0.030	0.109*	0.032	-0.118*
	(0.034)	(0.041)	(0.036)	(0.043)	(0.039)	(0.047)
Advanced Degree	0.008	0.344*	-0.004	0.342*	0.005	-0.371*
	(0.046)	(0.059)	(0.052)	(0.063)	(0.056)	(0.068)
Age	-0.066*	-0.066*	-0.038*	-0.030*	0.041*	0.032*
	(0.006)	(0.008)	(0.008)	(0.010)	(0.009)	(0.011)
Age Squared	0.001*	0.001*	0.000*	0.000	0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure	-0.005*	-0.005*	-0.007*	-0.007*	0.007*	0.007*
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
Black	-0.282*	-0.275*	-0.309*	-0.289*	0.334*	0.313*
	(0.046)	(0.048)	(0.054)	(0.049)	(0.058)	(0.053)
Married	0.301*	-0.008	0.305*	-0.027	-0.329*	0.030
	(0.025)	(0.029)	(0.028)	(0.031)	(0.030)	(0.034)
Metropolitan Area	0.029	0.048	0.046	0.061	-0.050	-0.066
	(0.031)	(0.041)	(0.035)	(0.043)	(0.038)	(0.046)
Union member	-0.089*	-0.015	-0.094*	-0.038	0.101*	0.041
	(0.035)	(0.055)	(0.039)	(0.054)	(0.042)	(0.059)
State U^{RATE}	-0.220*	-0.176*	-0.143*	-0.076*	0.154*	0.082*
	(0.017)	(0.022)	(0.022)	(0.028)	(0.024)	(0.030)
Degrees of freedom	78,063	55,949	10,010	6,602	10,010	6,602
α	-	-	0.927	0.923	0.927	0.923
Log pseudolikelihood	-23,534.3	-15,939.1	-15,152.1	-9,934.8	-15,152.1	-9,934.8
No. observations	78,212	56,094	10,096	6,688	10,096	6,688

Note: Robust standard errors are reported. State-year unemployment rates are from the BLS; all other variables from the DWS, 1994 - 2006. All specifications include fixed effects for state of residence, year of the survey (DWS), and year of displacement.* indicates statistical significance at 5 percent.

Table 4.A2 Re-employment wages controlling for pre-displacement wages

	Re-employment wages		Heckman selection correction for re-employment wages	
	Male	Female	Male	Female
Variable	4.A2.1	4.A2.2	4.A2.3	4.A2.4
Non-citizen	-0.068*	-0.075*	-0.068*	-0.076*
	(0.022)	(0.031)	(0.021)	(0.031)
Naturalized citizen	-0.023	0.000	-0.023	0.000
	(0.033)	(0.031)	(0.033)	(0.031)
Log of pre-	0.570*	0.523*	0.569*	0.519*
displacement wages	(0.015)	(0.020)	(0.015)	(0.019)
No high school	-0.119*	-0.226*	-0.115*	-0.221*
	(0.030)	(0.057)	(0.030)	(0.057)
High school drop out	-0.043*	-0.121*	-0.036	-0.118*
	(0.020)	(0.031)	(0.020)	(0.031)
Some college	0.035*	0.069*	0.032*	0.067*
	(0.014)	(0.015)	(0.014)	(0.015)
College degree	0.182*	0.203*	0.176*	0.198*
	(0.018)	(0.020)	(0.018)	(0.020)
Advanced degree	0.277*	0.298*	0.270*	0.295*
	(0.028)	(0.036)	(0.028)	(0.036)
Age	0.015*	0.017*	0.015*	0.017*
	(0.004)	(0.005)	(0.004)	(0.005)*
Age squared	0.000*	0.000*	0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)*
Tenure	-0.004*	-0.002	-0.004*	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Black	-0.082*	-0.082*	-0.072*	-0.080*
	(0.023)	(0.022)	(0.023)	(0.022)
Married	0.060*	0.004	0.054*	0.004
	(0.012)	(0.013)	(0.013)	(0.013)
Metropolitan area	0.045*	0.123*	0.041*	0.125*
	(0.016)	(0.020)	(0.016)	(0.020)
Union member	0.047*	-0.027	0.052*	-0.024
	(0.017)	(0.026)	(0.018)	(0.026)
State U^{RATE}	0.005	-0.012	-0.017*	-0.026*
	(0.009)	(0.011)	(0.005)	(0.006)
Inverse Mills ratio	-	-	-0.069*	-0.028
			(0.031)	(0.037)
Degrees of freedom	6,445	3,967	6,464	3,986
R^2	0.482	0.496	0.477	0.492
No. observations	6,532	4,054	6,532	4,054

Note: Robust standard errors are reported. Wages are adjusted using CPI data from the BLS to reflect 2006 dollar amounts. State-year unemployment rates are from the BLS. All other variables are from the DWS, 1994 - 2006. All specifications include fixed effects for state of residence, survey year, and displacement year. For workers who were displaced from full-time work and re-employed full-time.

* indicates statistical significance at 5 percent.

TABLES

Table 4.1 Variable means for workers displaced from full-time employment

Variable	Male Displaced Workers All	Male Displaced Workers Native	Male Displaced Workers Imm.	Female Displaced Workers All	Female Displaced Workers Native	Female Displaced Workers Imm.
Immigrant	0.112	0.000	1.000	0.097	0.000	1.000
Citizen	0.929	1.000	0.362	0.946	1.000	0.439
No High School	0.031	0.013	0.171	0.022	0.006	0.172
High School Dropout	0.090	0.083	0.139	0.065	0.059	0.117
High School Diploma	0.334	0.347	0.238	0.331	0.339	0.255
Some College	0.290	0.305	0.172	0.333	0.349	0.185
College Degree	0.182	0.183	0.172	0.188	0.190	0.166
Advanced Degree	0.073	0.069	0.109	0.062	0.058	0.105
Black	0.078	0.079	0.069	0.124	0.128	0.086
Married	0.623	0.613	0.705	0.490	0.477	0.606
Metropolitan Area Resident	0.793	0.774	0.942	0.808	0.791	0.969
Employed in Manufacturing	0.187	0.186	0.192	0.149	0.137	0.259
Duration of unemployment (# of weeks)	15.5 (19.6)	15.3 (19.3)	17.0 (21.8)	16.8 (20.3)	16.5 (20.1)	19.7 (21.7)
Age	39.3 (11.0)	39.3 (11.0)	38.9 (10.8)	39.6 (10.8)	39.5 (10.9)	40.6 (10.1)
Years of Tenure	5.0 (6.8)	5.1 (6.9)	3.9 (5.1)	4.9 (6.2)	5.0 (6.3)	4.1 (4.7)
State Unemployment Rate	0.053 (0.013)	0.052 (0.013)	0.057 (0.014)	0.052 (0.013)	0.052 (0.013)	0.057 (0.014)
Reason: Plant Closed Down or Moved	0.363	0.358	0.406	0.417	0.412	0.463
Reason: Insufficient Work	0.394	0.386	0.456	0.269	0.260	0.359
Reason: Position or Shift Abolished	0.243	0.256	0.137	0.314	0.328	0.179
No. observations	10,096	8,965	1,131	6,688	6,039	649

Note: State-year unemployment rates are from the BLS; all other variables from the DWS, 1994 - 2006.

Table 4.2 Variable means for workers displaced from full-time work and re-employed full-time

	Male Displaced Workers			Female Displaced Workers		
Variable	All	Native	Imm.	All	Native	Imm
Immigrant	0.106	0.000	1.000	0.087	0.000	1.000
Citizen	0.929	1.000	0.332	0.954	1.000	0.469
No High School	0.026	0.011	0.154	0.016	0.004	0.135
High School Dropout	0.077	0.068	0.147	0.044	0.040	0.084
High School Diploma	0.329	0.339	0.241	0.316	0.324	0.242
Some College	0.298	0.313	0.174	0.353	0.366	0.225
College Degree	0.194	0.196	0.175	0.205	0.206	0.188
Advanced Degree	0.077	0.073	0.110	0.066	0.060	0.126
Black	0.064	0.064	0.064	0.102	0.104	0.082
Married	0.665	0.657	0.728	0.493	0.484	0.579
Metropolitan Area Resident	0.800	0.784	0.936	0.813	0.798	0.969
Employed in Manufacturing	0.201	0.201	0.199	0.155	0.145	0.261
Old Weekly Wages ($)	914 (595)	931 (593)	768 (590)	700 (459)	704 (453)	656 (512)
New Weekly Wages ($)	826 (547)	837 (543)	729 (563)	630 (403)	631 (398)	619 (446)
Age	38.6 (10.4)	38.8 (10.5)	37.7 (10.3)	39.1 (10.3)	39.0 (10.4)	39.5 (9.8)
Years of Tenure	5.0 (6.5)	5.2 (6.6)	3.7 (4.7)	5.0 (6.1)	5.1 (6.2)	4.0 (4.6)
State Unemployment Rate	0.054 (0.015)	0.054 (0.015)	0.059 (0.015)	0.053 (0.014)	0.053 (0.014)	0.058 (0.015)
Reason: Plant Closed Down or Moved	0.372	0.368	0.407	0.418	0.412	0.472
Reason: Insufficient Work	0.376	0.368	0.443	0.243	0.235	0.326
Reason: Position or Shift Abolished	0.251	0.264	0.150	0.339	0.352	0.202
No. observations	6,532	5,837	695	4,054	3,701	353

Note: Wages are adjusted using CPI data from the BLS to reflect 2006 dollar amounts. State-year unemployment rates are from the BLS; all other variables from the DWS, 1994 - 2006.

Table 4.3 Duration of unemployment (Weibull AFT), male

Variable	4.3.1	4.3.2	4.3.3	4.3.4	4.3.5
Non-citizen	0.065	-0.012	0.047	0.034	0.107
	(0.059)	(0.063)	(0.064)	(0.064)	(0.079)
Naturalized citizen	0.319*	0.296*	0.185*	0.176*	0.120
	(0.075)	(0.076)	(0.072)	(0.073)	(0.092)
No high school	-	0.257*	0.059	0.137	0.262*
		(0.099)	(0.097)	(0.099)	(0.118)
High school drop out	-	0.226*	0.230*	0.230*	0.228*
		(0.056)	(0.054)	(0.054)	(0.065)
Some college	-	-0.044	-0.047	-0.032	-0.105*
		(0.037)	(0.035)	(0.034)	(0.042)
College degree	-	0.063	0.054	0.032	-0.107
		(0.042)	(0.041)	(0.039)	(0.055)
Advanced degree	-	0.030	-0.031	0.005	-0.048
		(0.057)	(0.057)	(0.056)	(0.081)
Age	-	-	0.039*	0.041*	0.043*
			(0.009)	(0.009)	(0.011)
Age squared	-	-	0.000	0.000*	0.000
			(0.000)	(0.000)	(0.000)
Tenure	-	-	0.009*	0.007*	0.004
			(0.002)	(0.002)	(0.003)
Black	-	-	0.295*	0.334*	0.326*
			(0.056)	(0.058)	(0.077)
Married	-	-	-0.354*	-0.329*	-0.344*
			(0.031)	(0.030)	(0.036)
Metropolitan area	-	-	0.002	-0.050	-0.097*
			(0.036)	(0.038)	(0.047)
Union member	-	-	0.147*	0.101*	0.131*
			(0.042)	(0.042)	(0.052)
State U^{RATE}	-	-	0.199*	0.154*	0.130*
			(0.011)	(0.024)	(0.034)
Controls for state, year of survey, and year of displacement	No	No	No	Yes	Yes
Controls for industry and occupation of displacement	No	No	No	No	Yes
Degrees of freedom	10,388	10,383	10,103	10,010	6,236
α	0.850	0.851	0.901	0.927	0.966
Log pseudolikelihood	-16,530.7	-16,510.5	-15,576.0	-15,152.1	-9,555.4
No. observations	10,391	10,391	10,119	10,096	6,585

Note: Robust standard errors are reported. State-year unemployment rates are from the BLS; all other variables from the DWS, 1994 - 2006. All specifications include fixed effects for state of residence, year of the survey (DWS), and year of displacement.
* indicates statistical significance at 5 percent.

Table 4.4 Duration of unemployment (Weibull AFT), female

Variable	4.4.1	4.4.2	4.4.3	4.4.4	4.4.5
Non-citizen	0.295*	0.199*	0.152	0.136	-0.027
	(0.074)	(0.079)	(0.080)	(0.079)	(0.097)
Naturalized citizen	0.319*	0.298*	0.151	0.077	-0.012
	(0.084)	(0.082)	(0.082)	(0.080)	(0.106)
No high school	-	0.277*	0.304*	0.350*	0.185
		(0.124)	(0.124)	(0.120)	(0.153)
High school drop out	-	0.175*	0.209*	0.195*	0.171
		(0.075)	(0.074)	(0.075)	(0.091)
Some college	-	-0.086*	-0.036	-0.017	-0.013
		(0.042)	(0.042)	(0.041)	(0.052)
College degree	-	-0.212*	-0.133*	-0.118*	-0.093
		(0.048)	(0.048)	(0.047)	(0.066)
Advanced degree	-	-0.360*	-0.387*	-0.371*	-0.390*
		(0.073)	(0.070)	(0.068)	(0.099)
Age	-	-	0.030*	0.032*	0.020
			(0.011)	(0.011)	(0.015)
Age squared	-	-	0.000	0.000	0.000
			(0.000)	(0.000)	(0.000)
Tenure	-	-	0.008*	0.007*	0.012*
			(0.003)	(0.003)	(0.004)
Black	-	-	0.292*	0.313*	0.311*
			(0.050)	(0.053)	(0.065)
Married	-	-	0.025	0.030	0.037
			(0.035)	(0.034)	(0.042)
Metropolitan area	-	-	-0.015	-0.066	-0.064
			(0.043)	(0.046)	(0.061)
Union member	-	-	0.039	0.041	-0.007
			(0.059)	(0.059)	(0.080)
State U^{RATE}	-	-	0.176*	0.082*	0.030
			(0.013)	(0.030)	(0.046)
Controls for state, year of survey, and year of displacement	No	No	No	Yes	Yes
Controls for industry and occupation of displacement	No	No	No	No	Yes
Degrees of freedom	6,809	6,804	6,687	6,602	4,038
α	0.867	0.870	0.897	0.923	0.974
Log pseudolikelihood	-10,681.0	-10,646.7	-10,262.9	-9,934.8	-6,215.0
No. observations	6,812	6,812	6,703	6,688	4,370

Note: Robust standard errors are reported. State-year unemployment rates are from the BLS; all other variables from the DWS, 1994 - 2006. All specifications include fixed effects for state of residence, year of the survey (DWS), and year of displacement.

* indicates statistical significance at 5 percent.

Table 4.5 Re-employment and pre-displacement wages, male

Variable	Re-employment wages 4.5.1	Pre-displacement wages 4.5.2	Ratio of re-employment wages to pre-displacement wages 4.5.3
Non-citizen	-0.236*	-0.295*	0.059*
	(0.028)	(0.028)	(0.024)
Naturalized citizen	-0.110*	-0.153*	0.043
	(0.038)	(0.036)	(0.037)
No high school	-0.250*	-0.229*	-0.021
	(0.037)	(0.038)	(0.033)
High school drop out	-0.113*	-0.122*	0.009
	(0.022)	(0.023)	(0.024)
Some college	0.102*	0.117*	-0.015
	(0.016)	(0.015)	(0.016)
College degree	0.413*	0.404*	0.009
	(0.020)	(0.018)	(0.018)
Advanced degree	0.630*	0.619*	0.011
	(0.029)	(0.026)	(0.028)
Age	0.047*	0.056*	-0.009*
	(0.005)	(0.004)	(0.004)
Age squared	-0.001*	-0.001*	0.000
	(0.000)	(0.000)	(0.000)
Tenure	0.001	0.009*	-0.008*
	(0.001)	(0.001)	(0.001)
Black	-0.202*	-0.211*	0.009
	(0.025)	(0.024)	(0.026)
Married	0.137*	0.135*	0.002
	(0.015)	(0.014)	(0.014)
Metropolitan area	0.119*	0.130*	-0.011
	(0.017)	(0.016)	(0.017)
Union member	0.148*	0.178*	-0.030
	(0.020)	(0.018)	(0.018)
State U^{RATE}	-0.004	-0.017	0.013
	(0.011)	(0.010)	(0.010)
Degrees of freedom	6,446	6,446	6,446
R^2	0.289	0.379	0.041
No. observations	6,532	6,532	6,532

Note: Robust standard errors are reported. Wages are adjusted using CPI data from the BLS to reflect 2006 dollar amounts. State-year unemployment rates are from the BLS. All other variables are from the DWS, 1994 - 2006. All specifications include fixed effects for state of residence, survey year, and displacement year. For workers who were displaced from full-time work and re-employed full-time.

* indicates statistical significance at 5 percent.

Table 4.6 Re-employment and pre-displacement wages, female

Variable	Re-employment wages 4.6.1	Pre-displacement wages 4.6.2	Ratio of re-employment wages to pre-displacement wages 4.6.3
Non-citizen	-0.234*	-0.304*	0.070*
	(0.035)	(0.032)	(0.033)
Naturalized citizen	-0.036	-0.069	0.033
	(0.041)	(0.040)	(0.033)
No high school	-0.387*	-0.307*	-0.080
	(0.061)	(0.047)	(0.062)
High school drop out	-0.217*	-0.183*	-0.034
	(0.034)	(0.033)	(0.035)
Some college	0.155*	0.165*	-0.010
	(0.017)	(0.017)	(0.017)
College degree	0.445*	0.463*	-0.018
	(0.023)	(0.021)	(0.022)
Advanced degree	0.656*	0.683*	-0.028
	(0.039)	(0.033)	(0.038)
Age	0.046*	0.056*	-0.010
	(0.005)	(0.005)	(0.005)
Age squared	-0.001*	-0.001*	0.000
	(0.000)	(0.000)	(0.000)
Tenure	0.006*	0.016*	-0.010*
	(0.002)	(0.001)	(0.002)
Black	-0.147*	-0.125*	-0.022
	(0.025)	(0.025)	(0.025)
Married	0.001	-0.006	0.007
	(0.015)	(0.014)	(0.015)
Metropolitan area	0.208*	0.161*	0.047*
	(0.021)	(0.020)	(0.022)
Union member	0.004	0.060*	-0.055
	(0.029)	(0.027)	(0.029)
State U^{RATE}	-0.017	-0.009	-0.008
	(0.013)	(0.012)	(0.012)
Degrees of freedom	3,968	3,968	3,968
R^2	0.331	0.391	0.068
No. observations	4,054	4,054	4,054

Note: Robust standard errors are reported. Wages are adjusted using CPI data from the BLS to reflect 2006 dollar amounts. State-year unemployment rates are from the BLS. All other variables are from the DWS, 1994 - 2006. All specifications include fixed effects for state of residence, survey year, and displacement year. For workers who were displaced from full-time work and re-employed full-time.
* indicates statistical significance at 5 percent.

Table 4.7 Heckman (probit) selection (into re-employment) equation, re-employment wages, re-employment/pre-displacement wage difference, male

Variable	Probit: re-employment 4.7.1	Re-employment wages 4.7.2	Wage ratio 4.7.3
Non-citizen	-0.027	-0.228*	0.054*
	(0.058)	(0.028)	(0.024)
Naturalized citizen	-0.074	-0.108*	0.041
	(0.071)	(0.038)	(0.037)
No high school	-0.245*	-0.248*	-0.014
	(0.079)	(0.038)	(0.034)
High school drop out	-0.297*	-0.107*	0.017
	(0.048)	(0.023)	(0.024)
Some college	0.140*	0.101*	-0.020
	(0.035)	(0.016)	(0.016)
College degree	0.194*	0.408*	0.000
	(0.043)	(0.020)	(0.018)
Advanced degree	0.240*	0.626*	0.001
	(0.062)	(0.029)	(0.028)
Age	0.006	0.047*	-0.010*
	(0.009)	(0.005)	(0.004)
Age squared	0.000*	-0.001*	0.000
	(0.000)	(0.000)	(0.000)
Tenure	0.005*	0.000	-0.008*
	(0.002)	(0.001)	(0.001)
Black	-0.356*	-0.189*	0.016
	(0.050)	(0.026)	(0.026)
Married	0.310*	0.129*	-0.003
	(0.030)	(0.016)	(0.015)
Metropolitan area	0.130*	0.119*	-0.018
	(0.039)	(0.018)	(0.017)
Union member	-0.169*	0.152*	-0.023
	(0.040)	(0.020)	(0.019)
State U^{RATE}	-0.095*	-0.030*	-0.007
	(0.022)	(0.006)	(0.005)
Inverse Mills ratio	-	-0.068	-0.070*
		(0.037)	(0.035)
Reason: Insufficient Work	-0.166*	-	-
	(0.032)		
Reason: Position or Shift Abolished	-0.023	-	-
	(0.038)		
Degrees of freedom	12,219	6,465	6,465
R^2 or Pseudo R^2	0.141	0.282	0.037
No. observations	12,307	6,532	6,532

Note: Omitted Reason: Plant or company closed down or moved. Robust standard errors are reported. Wages are adjusted using CPI data from the BLS to reflect 2006 dollar amounts. State-year unemployment rates are from the BLS; all other variables from the DWS, 1994 - 2006. All specifications include fixed effects for state of residence. Only the probit in 4.7.1 includes controls for the year of displacement and the year of the survey.
* indicates statistical significance at 5 percent.

Table 4.8 Heckman (probit) selection (into re-employment) equation, re-employment wages, re-employment/pre-displacement wage difference, female

Variable	Probit: re-employment 4.7.1	Re-employment wages 4.7.2	Wage ratio 4.7.3
Non-citizen	-0.052	-0.232*	0.069*
	(0.079)	(0.035)	(0.034)
Naturalized citizen	-0.054	-0.034	0.032
	(0.089)	(0.041)	(0.033)
No high school	-0.291*	-0.390*	-0.065
	(0.113)	(0.060)	(0.063)
High school drop out	-0.279*	-0.216*	-0.027
	(0.069)	(0.034)	(0.036)
Some college	0.129*	0.153*	-0.012
	(0.042)	(0.017)	(0.017)
College degree	0.209*	0.444*	-0.028
	(0.052)	(0.023)	(0.022)
Advanced degree	0.485*	0.655*	-0.038
	(0.087)	(0.040)	(0.038)
Age	0.022	0.046*	-0.010
	(0.011)	(0.005)	(0.005)
Age squared	0.000*	-0.001*	0.000
	(0.000)	(0.000)	(0.000)
Tenure	0.003	0.006*	-0.010*
	(0.003)	(0.002)	(0.002)
Black	-0.389*	-0.145*	-0.020
	(0.052)	(0.025)	(0.025)
Married	0.027	-0.001	0.008
	(0.035)	(0.015)	(0.015)
Metropolitan area	0.011	0.210*	0.046*
	(0.050)	(0.021)	(0.022)
Union member	0.052	0.006	-0.051
	(0.066)	(0.029)	(0.029)
State U^{RATE}	-0.058*	-0.039*	-0.013*
	(0.028)	(0.007)	(0.007)
Inverse Mills ratio	-	-0.025	-0.030
		(0.042)	(0.041)
Reason: Insufficient Work	-0.233*	-	-
	(0.042)		
Reason: Position or Shift Abolished	-0.064	-	-
	(0.043)		
Degrees of freedom	7,848	3,987	3,987
R^2 or Pseudo R^2	0.148	0.327	0.055
No. observations	7,936	4,054	4,054

Note: Omitted Reason: Plant or company closed down or moved. Robust standard errors are reported. Wages are adjusted using CPI data from the BLS to reflect 2006 dollar amounts. State-year unemployment rates are from the BLS; all other variables from the DWS, 1994 - 2006. All specifications include fixed effects for state of residence. Only the probit in 4.7.1 includes controls for the year of displacement and the year of the survey.
* indicates statistical significance at 5 percent.

Table 4.9 Ratio of re-employment wages to pre-displacement wages, with and without controlling for tenure on the previous job

	4.9.1	4.9.2	4.9.3	4.9.4
Variable	Male	Male	Female	Female
Non-citizen	0.065*	0.059*	0.079*	0.070*
	(0.024)	(0.024)	(0.034)	(0.033)
Naturalized citizen	0.053	0.043	0.034	0.033
	(0.037)	(0.037)	(0.034)	(0.033)
No high school	-0.022	-0.021	-0.058	-0.080
	(0.033)	(0.033)	(0.062)	(0.062)
High school drop	0.016	0.009	-0.018	-0.034
out	(0.024)	(0.024)	(0.035)	(0.035)
Some college	-0.011	-0.015	-0.008	-0.010
	(0.016)	(0.016)	(0.017)	(0.017)
College degree	0.013	0.009	-0.010	-0.018
	(0.018)	(0.018)	(0.022)	(0.022)
Advanced degree	0.015	0.011	-0.017	-0.028
	(0.027)	(0.028)	(0.038)	(0.038)
Age	-0.011*	-0.009*	-0.014*	-0.010
	(0.004)	(0.004)	(0.005)	(0.005)
Age squared	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Tenure	-	-0.008*	-	-0.010*
		(0.001)		(0.002)
Black	0.007	0.009	-0.031	-0.022
	(0.026)	(0.026)	(0.025)	(0.025)
Married	-0.004	0.002	0.001	0.007
	(0.014)	(0.014)	(0.015)	(0.015)
Metropolitan area	-0.010	-0.011	0.044*	0.047*
	(0.017)	(0.017)	(0.022)	(0.022)
Union member	-0.044*	-0.030	-0.078*	-0.055
	(0.018)	(0.018)	(0.029)	(0.029)
State U^{RATE}	0.013	0.013	-0.007	-0.008
	(0.010)	(0.010)	(0.012)	(0.012)
Degrees of freedom	6,479	6,446	3,977	3,968
R^2	0.040	0.041	0.055	0.068
No. observations	6,564	6,532	4,062	4,054

Note: Robust standard errors are reported. Wages are adjusted using CPI data from the BLS to reflect 2006 dollar amounts. State-year unemployment rates are from the BLS. All other variables are from the DWS, 1994 - 2006. All specifications include fixed effects for state of residence, survey year, and displacement year. For workers who were displaced from full-time work and re-employed full-time.

* indicates statistical significance at 5 percent.

Table 4.10 Percentage of re-employment wages that are less than or equal to the state minimum wage in the year of the survey

	% with hourly re-employment wages at or below the state minimum wage
MALES	
All displaced workers	3.3
Natives	2.9
Non-citizens	8.2
Naturalized citizens	3.9
Blacks	3.8
FEMALES	
All displaced workers	5.0
Natives	4.7
Non-citizens	13.3
Naturalized citizens	3.6
Blacks	7.3

Note: Unadjusted wage data from the DWS, 1994-2006. State minimum wages from the U.S. Department of Labor.

Table 4.11 Duration of unemployment (Weibull AFT), reasons for displacement

	Displaced Males		Displaced Females	
Variable	4.11.1	4.11.2	4.11.3	4.11.4
Non-citizen	0.046	0.034	0.149	0.136
	(0.063)	(0.064)	(0.079)	(0.079)
Naturalized citizen	0.193*	0.176*	0.089	0.077
	(0.071)	(0.073)	(0.080)	(0.080)
Reason: Insufficient Work	0.091*	-	0.123*	-
	(0.042)		(0.041)	
Reason: Position or Shift Abolished	0.221*	-	0.142*	-
	(0.035)		(0.039)	
No high school	0.145	0.137	0.359*	0.350*
	(0.098)	(0.099)	(0.120)	(0.120)
High school drop out	0.232*	0.230*	0.196*	0.195*
	(0.053)	(0.054)	(0.075)	(0.075)
Some college	-0.044	-0.032	-0.024	-0.017
	(0.034)	(0.034)	(0.0413)	(0.041)
College degree	-0.004	0.032	-0.134*	-0.118*
	(0.040)	(0.039)	(0.047)	(0.047)
Advanced degree	-0.039	0.005	-0.396*	-0.371*
	(0.056)	(0.056)	(0.068)	(0.068)
Age	0.040*	0.041*	0.030*	0.032*
	(0.009)	(0.009)	(0.011)	(0.011)
Age squared	-0.000	0.000*	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Tenure	0.008	0.007*	0.009*	0.007*
	(0.002)	(0.002)	(0.002)	(0.003)
Black	0.337*	0.334*	0.315*	0.313*
	(0.058)	(0.058)	(0.053)	(0.053)
Married	-0.335*	-0.329*	0.030	0.030
	(0.030)	(0.030)	(0.034)	(0.034)
Metropolitan area	-0.062	-0.050	-0.070	-0.066
	(0.038)	(0.038)	(0.047)	(0.046)
Union member	0.110*	0.101*	0.043	0.041
	(0.042)	(0.042)	(0.059)	(0.059)
State U^{RATE}	0.156*	0.154*	0.077*	0.082*
	(0.024)	(0.024)	(0.030)	(0.030)
Controls for state, year of survey, and year of displacement	Yes	Yes	Yes	Yes
Degrees of freedom	10,008	10,010	6,600	6,602
α	0.929	0.927	0.924	0.923
Log pseudolikelihood	-15,127.6	-15,152.1	-9,925.1	-9,934.8
No. observations	10,096	10,096	6,688	6,688

Note: Omitted Reason: Plant or company closed down or moved. Robust standard errors are reported. Robust standard errors are reported. State-year unemployment rates are from the BLS; all other variables from the DWS, 1994 - 2006. * indicates statistical significance at 5 percent.

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Chapter 5

Conclusion

In this conclusion, I highlight the main findings of the previous three chapters. While the 1996 welfare reform was successful in reducing Medicaid coverage among recent cohorts of immigrants to the United States, it has not affected the private health insurance coverage or labor supply of these immigrants. Non-citizen immigrants who lacked access to Medicaid experience half of the growth in overall health insurance coverage compared to those who were eligible for Medicaid. For employer-sponsored immigrants, receiving a green card is accompanied by a wage increase of at least 13 percent, which is consistent with the idea that these immigrants have limited job mobility prior to becoming legal permanent residents of the U.S. Finally, for non-citizen immigrants who are displaced from their job and re-employed, their wages do not fall as much as the wages of similar displaced and re-employed native workers. Naturalized citizens experience longer duration of unemployment when compared to native workers.