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Local Unemployment and the Relative Wages of Immigrants: Evidence from the Current Population Surveys

By
Erling Barth, Bernt Bratsberg and Oddbjørn Raaum

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Department of Economics
University of Oslo

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University of Oslo
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P. O.Box 1095 Blindern
N-0317 OSLO Norway
Telephone: + 47 22855127
Fax: + 47 22855035
Internet: http://www.oekonomi.uio.no/
e-mail: econdep@econ.uio.no

In co-operation with
The Frisch Centre for Economic Research

Gaustadalleén 21
N-0371 OSLO Norway
Telephone: +47 22 95 88 20
Fax: +47 22 95 88 25
Internet: http://www.frisch.uio.no/
e-mail: frisch@frisch.uio.no

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Local Unemployment and the Relative Wages of Immigrants: Evidence from the Current Population Surveys*

Erling Barth
Institute for Social Research

Bernt Bratsberg
The Ragnar Frisch Centre for Economic Research and Kansas State University

Oddbjørn Raaum
The Ragnar Frisch Centre for Economic Research

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Abstract

We provide evidence on wage profiles of immigrants using CPS data from 1979 to 2001, taking into account that changes in labor market conditions impact natives and immigrants differently. High rates of immigrant wage assimilation in general, and relatively high wages of immigrant cohorts that arrived during the 1990s in particular, can largely be explained by a negative trend in unemployment in the data. Relating immigrant and native period effects to local labor market unemployment, we find that wage assimilation among lesser-educated immigrants is negligible and that the immigrant-native wage gap is strongly increasing in unemployment. For highly educated immigrants, rates of wage assimilation during early years in the United States are higher the lower is unemployment.

Introduction

Immigrants typically earn lower wages than comparable native-born workers during the first years after arrival in the host country. The extent to which immigrants experience faster wage growth than natives, and, perhaps, close the wage gap with time in their new country, forms a central topic in the economics of immigration (Chiswick, 1978; Borjas, 1994; 1999). Wage assimilation of immigrants is also of major interest for public policy concerning immigration, poverty, and human capital accumulation. An important challenge to the empirical literature has been to consistently estimate wage profiles of immigrants in the presence of unobserved heterogeneity. Borjas (1985) demonstrates that a decline in unobserved earnings capacity ("cohort quality") across immigrant cohorts in the United States leads to upward bias in estimates of assimilation effects based on cross-sectional data, as such data cannot separate the wage effects associated with time since immigration and arrival cohort. To overcome this problem, recent empirical studies of immigrant assimilation rely on the *synthetic panel methodology*, in which one combines multiple cross-sections and tracks the wages of immigrant arrival cohorts over time (Borjas, 1999).

Because of inherent problems of untangling the three effects of aging, cohort, and period on immigrant wages, the synthetic panel approach requires that the researcher make some identifying assumption. In order to identify the remaining two effects, the common empirical strategy is to impose the restriction that period effects for immigrants are identical to those of natives. In the present paper, we use data from the Current Population Surveys (CPS) from 1979 through 2001 and demonstrate that changes in labor market conditions affect wages of natives and immigrants differently. Consequently, the equal-period effects assumption is unlikely to hold in data that cover periods of changing macroeconomic conditions and synthetic-panel based estimates of assimilation effects may contain severe

bias when such estimates ignore the effects of macroeconomic conditions on the wages of immigrant and native workers.[1]

Although prior studies suggest that immigrants and natives are affected differently by changes in economic conditions, such linkages are largely ignored in the empirical literature on immigrant labor market assimilation. For example, Chiswick et al. (1997) report tentative evidence that employment of U.S. immigrants is more adversely affected by macroeconomic downturns than is employment of natives. Similarly, McDonald and Worswick (1997) find that the unemployment incidence of immigrant men in Canada increases more during a recession than that of natives.[2] Further, studies of empirical wage curves, linking earnings of individuals to unemployment in their local labor market, show that wages of less-established workers tend to be more responsive to changes in local labor market conditions than are wages of established workers (Blanchflower and Oswald, 1994; Card, 1995; Barth et al., 2002a). A central hypothesis of the present paper is that such differences also characterize the local labor market responsiveness of wages of immigrants and natives. Indeed, two recent studies of immigrants to Norway conclude that annual earnings of immigrants are more sensitive to local unemployment than are earnings of natives (Longva and Raaum, 2002; Barth et al., 2002b).

The basic premise behind our empirical strategy is to augment the synthetic panel methodology with wage curve effects and, thus, link period effects to conditions in the local labor market. By allowing the association between individual wages and local unemployment

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[1] LaLonde and Topel (1992), Borjas (1995), and Lubotsky (2001) discuss a related source of bias that results from changes in skill prices. Because immigrants on average earn less than natives, widening wage inequality over the sample period can lead to understatement of the relative growth in immigrant wages over time. Given the rise in returns to skill in the United States during the 1980s, skill-price bias may affect estimates of assimilation effects in studies based on data from the 1980 and 1990 decennial censuses. Such bias is, however, less likely to impact results of the present study that in main draws on data from 1994 to 2001—a period characterized by stability of wage inequality (Card and DiNardo, 2002). In fact, when we restrict the empirical analysis to the 1994-2001 period, estimates are very similar to those presented in the paper.

[2] Both the Chiswick et al. and the McDonald and Worswick studies link employment experiences of immigrants to the national unemployment rate. One problem affecting the statistical evidence of these studies is that of short

to differ for immigrant and native workers, we estimate assimilation effects on immigrant wages accounting for differential responses to local labor market conditions. In result, the augmented framework relaxes the equal-period effect assumption. In an extended empirical specification, we also permit the rate of wage assimilation to depend on conditions in the labor market.

The next section outlines a simple theoretical framework that clarifies the relationship between local labor market conditions and the evolution of immigrant wages, taking into account that local unemployment affects immigrant wages both through the wage-bargaining process and the accumulation of country-specific human capital. Section 3 presents the empirical strategy and includes a discussion of scenarios under which changes in labor market conditions give rise to biased estimates of wage assimilation and immigrant cohort differentials within a standard synthetic panel framework. The section also introduces an augmented methodology that conditions period effects on local unemployment and allows effects to differ for natives and immigrants. After a description of the CPS data samples and our measure of local unemployment rates, section 5 presents the empirical results of the study. The empirical evidence confirms the prediction from the theoretical model that immigrant wages are more sensitive to changes in local unemployment than are wages of native workers. We also find that failure to consider such differences leads to serious bias in estimates of immigrant wage assimilation and cohort effects. Accounting for differential immigrant and native responsiveness to changes in economic conditions, we uncover evidence that, for lesser-educated immigrants, the decline in wages across successive immigrant cohorts continued into the 1990s and then stalled. Only for highly educated male immigrants is there support for the hypothesis that the added emphasis of U.S. policy since

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time series. In fact, the U.S. study is based on only four and the Canadian study on eleven unemployment observations.

1990 on skilled immigration has resulted in higher earnings capacity of recent immigrant arrivals.

2. Theoretical Framework

In order to sort out the various mechanisms behind the relationship between local labor market conditions and immigrant pay, we begin the analysis by sketching a simple theoretical framework. The framework holds that business cycles influence wages of immigrants in two important ways, as employment opportunities affect both the *accumulation of human capital specific to the host country* and the relative *bargaining position* of immigrants. Thus, both immigrants' productivity on the job and their ability to extract pay for their productive contribution will depend on conditions in the labor market.

To begin, we assume that the employment probability of an immigrant is given by $\pi = 1 - u\varphi$, where u is the unemployment rate in the local labor market and $\varphi \geq 1$ is a factor measuring an immigrant's relative disadvantage in obtaining a job in the host country. At the time of entry, immigrants often lack the language skills, informal networks, and knowledge of the functioning of the labor market necessary for successful job search. Such disadvantages diminish as the immigrant spends time in the host country.[3] We therefore assume that φ is a declining function in years since migration and approaches unity as the immigrant assimilates into the labor market, i.e., $\varphi' \leq 0$ and $\varphi'' \geq 0$. For natives, the employment probability equals (1-u).

The wage rate, W, is given by

$$W = BP, \tag{1}$$

[3] See Funkhouser (2000) for recent evidence that immigrants face significant employment disadvantage for the first 6-10 years following entry into the United States.

where $B \in (0,1]$ is the fraction of productivity that accrues to the worker—or the worker's bargained share—and P denotes individual productivity. We proceed by separately discussing the effects of local unemployment on each of the two factors, P and B.

2.1. Unemployment and the accumulation of country-specific human capital

We adopt a "learning by doing" approach. Through work, an immigrant acquires skills and human capital that enhance productivity in the new country. To simplify the exposition, assume for now that unemployment has been at its steady state level since the immigrant's date of arrival. Total work experience in the new country is then given by:

$$E = \int_0^{YSM} [1 - u\varphi(t)]dt \,,$$

where *YSM* denotes years since migration. Work experience is increasing in *YSM*, and its growth rate equals:

$$\partial E / \partial YSM = 1 - u\varphi = \pi > 0,$$

which is the immigrant's expected work experience in the current period. Accumulated experience is declining in u, as

$$\partial E / \partial u = -\int_0^{YSM} \varphi(t)dt \leq 0.$$

In words, a higher level of unemployment results in a lower employment probability for each year in the host country and, thus, less accumulated experience.

In equation (1), the factor P denotes the productivity level of the individual. We assume that the productivity level of an immigrant relates to that of a native through the following expression:

$$\ln P = p^I = p^N + \kappa(E) \,, \;\; \kappa(E) \leq 0 \qquad (2)$$

where p^N is the log of the productivity level of a native-born worker with identical formal qualifications (e.g., age, gender, educational attainment) as the immigrant. The function $\kappa(E)$ can be thought of as a learning function that captures the gap between the productivity levels of an immigrant and a native.[4] The function thus describes the accumulation of country-specific human capital over time, with $\kappa(0)$ reflecting the "cultural distance" between the home and host countries. Because immigrants accumulate skills with work experience in the new country, we interpret the derivative, $\partial\kappa/\partial E \geq 0$, as the learning effect of work experience on relative immigrant wages. We assume that κ is concave (i.e., $\partial^2\kappa/\partial E^2 \leq 0$) and that, eventually, κ approaches zero as the immigrant closes the cultural gap.

Consider the following specific form of the learning function:

$$\kappa(E) = -ke^{-\lambda E}$$

where k captures cultural distance and λ is a proportional skills-improvement factor. The rate of relative productivity growth of an immigrant is given by $\partial\kappa/\partial E = -\lambda\kappa \geq 0$, and the annual growth rate of country-specific human capital by $\partial\kappa/\partial YSM = -\lambda\kappa(1-u\varphi) \geq 0$.

One important concern is how the rate of human capital accumulation is affected by the unemployment rate. Taking the derivative of $\partial\kappa/\partial YSM$ with respect to unemployment yields:

$$\partial^2\kappa/\partial YSM\,\partial u = \varphi\lambda\kappa - (1-u\varphi)\lambda^2\kappa\int_0^{YSM}\varphi(t)dt\,.$$

The first term of the cross-partial derivative is negative, reflecting that a higher level of unemployment reduces immigrants' employment experiences and accumulated learning. The second term, however, is positive, arising from the concavity of the learning function and the fact that less accumulated learning renders the immigrant with a lower κ and, consequently, a

[4] Note that the set-up allows for human-capital accumulation of natives and improvements in p^N with

higher learning potential. With the two opposing terms, the sign of the cross-partial derivative is indeterminate. Plugging in *YSM*=0, it is easy to see, however, that the sign initially is negative. As prior accumulation of human capital gains weight with higher *YSM*, the sign will eventually turn positive with the turning point, *YSM**, implicitly defined by:[5]

$$\int_0^{YSM^*} \varphi(t)dt = \varphi(YSM^*)/\{[1-u\varphi(YSM^*)]\lambda\}. \qquad (3)$$

For recently arrived immigrants with *YSM* less than YSM^*, higher unemployment reduces the rate of human capital accumulation. Such reduction during early years leads to postponement of acquisition of country-specific human capital and, thus, a positive effect of unemployment on the rate of human capital accumulation for established immigrants with *YSM* greater than YSM^*.

2.2. A simple bargaining model of wage determination

Consider next the worker's share factor *B*. Assume that wages are determined as the outcome of an asymmetric Nash bargaining process (Binmore et al., 1986), in which the worker's objective is to maximize the difference between the wage and the expected alternative pay, and the firm seeks to maximize profits. If disagreement payoffs are zero for both parties, we have

$$W = \arg\max\,[(W-A)^{\beta}(P-W)^{1-\beta}] = \beta P + (1-\beta)A, \qquad (4)$$

where $\beta \in (0,1]$ is an underlying bargaining-power parameter and *A* is the worker's alternative wage. Let the alternative wage be given be the expected wage from employment outside the firm; that is, $A = (1-u\varphi)\bar{W}$, where $\bar{W}$ is the average wage for similar workers with productivity *P* in the labor market, and $(1-u\varphi)$ is again the probability of obtaining a

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experience, but that $\kappa(E)$ again captures the native-immigrant productivity differential given *E*.

job at this wage. Assuming that workers with the same characteristics (including *YSM*) and productivity are paid the same wage, the market equilibrium is given by $W^* = \overline{W}$. Inserting the expression for A into (4) yields the equilibrium wage $W^* = B^* P$, where

$$0 < B^* = \frac{\beta}{1-(1-u\varphi)(1-\beta)} \leq 1. \qquad (5)$$

Measured in logs, $b^* = ln(B^*)$, and we have

$$\frac{\partial b^*}{\partial u} = -\frac{(1-\beta)}{\beta}\varphi B^* \leq 0, \ \frac{\partial b^*}{\partial YSM} = -\frac{(1-\beta)}{\beta}u\varphi' B^* \geq 0, \ \frac{\partial^2 b^*}{\partial u \partial YSM} = -\frac{(1-\beta)}{\beta}\varphi' B^{*2} \geq 0.$$

The outcome of the bargaining process depends on the unemployment rate, with the share of productivity going to the worker in form of pay declining with higher unemployment. This holds for both natives and immigrants. For immigrants, the bargaining outcome additionally depends on years since migration because the expected alternative wage increases with years in the host country. As the relative employment disadvantage declines over time, the immigrant share factor rises and approaches that of natives (i.e., $B^{*I} \rightarrow B^{*N} = \beta/[\beta + (1-\beta)u]$). The result is an indirect assimilation effect on wages, operating through improvements in the bargaining outcome of the immigrant.

Note also that the cross-partial derivative is positive—the adverse effect of rising unemployment on immigrant wages lessens with years in the host country. Because of their poorer outside employment prospects, the bargaining position of recently arrived immigrants is more responsive to changes in labor market conditions than is the position of established immigrants.

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[5] To see that *YSM** is unique, observe that the left-hand side of equation (3) is zero when *YSM*=0 and is strictly growing in *YSM*, while the right-hand side equals $\varphi(0)/\{[1-u\varphi(0)]\lambda\} > 1$ when *YSM*=0 and is falling in *YSM*.

2.3. The overall effect of unemployment on immigrant wage profiles

Accounting for both the bargaining process and human capital accumulation, the total effect of unemployment on immigrant (ln) wages is given by:

$$\frac{\partial w}{\partial u} = -\frac{(1-\beta)}{\beta}\varphi B^{*} + \lambda\kappa \int_{0}^{YSM} \varphi(t)dt \leq 0 .$$

In words, an increase in unemployment depresses wages of immigrants relative to natives through a lower bargained share. Next, higher unemployment reduces accumulated learning for each year in the host country.

The rate of immigrant wage assimilation is given by:

$$\frac{\partial w}{\partial YSM} = -\frac{1-\beta}{\beta}u\varphi' B^{*} - \lambda\kappa(1-u\varphi) \geq 0 . \qquad (6)$$

An additional year in the host country raises the immigrant's employment probability and outside opportunity wage and, thus, her bargaining outcome. Moreover, productivity from country-specific human capital improves as immigrants acquire work experience in the host country.

Consider next the influence of unemployment on the rate of wage assimilation, given by the derivative of equation (6) with respect to unemployment:

$$\frac{\partial^2 w}{\partial YSM \partial u} = -\frac{(1-\beta)}{\beta}\varphi' B^{*2} + \varphi\lambda\kappa - (1-u\varphi)\lambda^2\kappa \int_{0}^{YSM} \varphi(t)dt . \qquad (7)$$

The sign of this cross derivative is indeterminate. The first term represents the bargaining effect, which is positive because the impact of unemployment on the bargained share is less negative the more established is the immigrant in the host country. The second term, the initial productivity effect, pulls in the other direction, however, as accumulation of human capital through work experience initially is slower when unemployment is high. The final term is positive, reflecting that a higher unemployment rate implies lower levels of accumulated experience and thus a stronger learning effect at the margin.

The predictions from the theoretical framework can be summarized as follows. First, the pay gap between immigrants and natives is larger the higher is unemployment. Less favorable job opportunities affect immigrants more severely than natives, having a stronger effect on immigrants' outside opportunity wage and, thus, their bargained wage. Moreover, the relative productivity of immigrants is lower during periods of high unemployment because their accumulated human capital through work experience is hampered.

In addition to the direct impact on wages, unemployment also affects the rate of wage assimilation, or the slope of the wage profile, of immigrants. On the one hand, because bargaining outcomes of recently arrived immigrants are more sensitive to labor market conditions than are those of established immigrants, an increase in the unemployment rate reduces wages more for recently arrived immigrants than older immigrants—which in turn results in a steeper wage profile. On the other hand, the impact of an increase in unemployment on human capital accumulation is, at least initially, a flatter wage profile because of reduced learning effects. After some years in the host country, however, the effect of unemployment on learning switches from negative to positive, implying a steeper profile in high unemployment regimes. Whether increases in unemployment raise or flatten the slope of the immigrant wage profile at low *YSM* depends of which of the two mechanisms—bargaining or human capital accumulation—dominates. Further, any negative impact of unemployment on the slope of the wage profile should be observed only during the early years in the host country.

3. Empirical Methodology

3.1. Augmenting the synthetic panel model

The empirical model builds on the synthetic panel framework of Borjas (1985; 1995). Suppose the wage equation of immigrants observed in calendar year *t* is given by[6]

$$y_{jt} = X_{jt}\phi^I + \delta^I A_{jt} + \alpha YSM_{jt} + \sum_m \beta_m C_{jm} + \sum_s \gamma_s^I \Pi_{js} + \varepsilon_{jt} \quad (8)$$

and the wage equation of natives by

$$y_{jt} = X_{jt}\phi^N + \delta^N A_{jt} + \sum_s \gamma_s^N \Pi_{js} + \varepsilon_{jt}, \quad (9)$$

where y_{jt} is the log wage of person *j* in year *t*; *X* is a vector of socio-economic characteristics such as schooling and marital status; *A* gives the age of the individual at the time of observation; C_{jm} is an indicator variable for the calendar year in which the immigrant arrived in the host country; YSM_{jt} is the number of years the immigrant has resided in the host country; and $\Pi_{j.}$ denotes a set of indicator variables set to unity if the observation is made in calendar year *t*.

In equations (8)-(9), the β-vector captures any time-invariant differences in wages across immigrant arrival cohorts and the vectors γ^I and γ^N the period effects, i.e., the impact of macroeconomic conditions, on immigrant and native wages. The coefficient of *YSM*, α, which measures the additional wage growth associated with spending time in the host country, forms the key parameter of interest in studies of immigrant wage assimilation.[7] Unfortunately, because of collinearity between year of arrival, *YSM*, and year of observation, the coefficients α, β, and γ^I are not separately identified in the immigrant wage equation. Following Borjas (1985; 1991), the common strategy around the identification problem is to

[6] To simplify the notation, higher-order terms of age and YSM are omitted from the discussion of the empirical specification.

[7] Note, however, that for wage growth of immigrants to exceed that of natives, the sum of α and δ^I must be greater than δ^N. See also Borjas (1999).

impose the restriction that $\gamma^I = \gamma^N$. That is, in the standard synthetic panel framework, trends and transitory changes in aggregate macroeconomic and labor market conditions are assumed to have the same relative impact on native and immigrant wages. In effect, the restriction eliminates the immigrant period effect from the empirical model and computation of the coefficient of *YSM* and the cohort effects uses the estimated effect of macroeconomic conditions on the wages of the native-born comparison group. As we argued in the previous section, changes in macroeconomic conditions likely affect the wages of natives and immigrants differently. Accordingly, the "equal period effects" assumption is unlikely to hold when the sample period covers years with fluctuating macroeconomic conditions.

In this paper, we relax the restriction imposed by the equal-period effect assumption and allow for native-immigrant differences in responsiveness to local labor market conditions. To account for such differences, we extend the empirical framework, drawing on the wage-curve literature (Blanchflower and Oswald, 1994; Card, 1995). In that literature, transitory regional effects on wages have been shown to vary systematically (and inversely) with the unemployment rate in the local labor market. Thus, we model the period effect as proportional to the natural logarithm of the local unemployment rate (u_{rt}) and allow for separate transitory wage effects for immigrants and natives:

$$\gamma^I_{rt} = \gamma^0_t + \eta^I \ln u_{rt}, \text{ and} \tag{10}$$

$$\gamma^N_{rt} = \gamma^0_t + \eta^N \ln u_{rt}, \tag{11}$$

where the coefficients η^I and η^N denote the wage-curve elasticities of immigrants and natives, respectively.[8] A consequence of equations (10) and (11) is that estimated period effects differ for immigrants and natives if (i) local labor market conditions indeed have different effects

[8] Blanchflower and Oswald show that proper identification of the wage-curve elasticity requires inclusion of a fixed regional effect in the wage equation. The full empirical specification therefore includes a set of regional indicator variables. Also, to capture macroeconomic conditions common to all regions, the empirical specification contains indicator variables for year of observation, giving rise to γ^0_t of equations (10) and (11).

on immigrant and native wages (i.e., $\eta^I \neq \eta^N$) and (ii) the sample period covers years of varying unemployment.

Equation (10) is restrictive in the sense that the impact of local labor market conditions on the immigrant wage is independent of years of residence in the host country. According to the theoretical discussion of the previous section, this restriction is not likely to be valid. As immigrants accumulate human capital such as work experience, seniority, union membership, and interpersonal networks in the host country, we expect the influence of local labor market conditions on immigrant wages to become more similar to that of natives. In other words, η^I is expected to depend on time spent in the host country and may perhaps eventually approach η^N. Furthermore, the process of accumulation of human capital may itself be influenced by the unemployment rate. We therefore extend the empirical specification and let the effect of local unemployment interact with years since migration. This allows us to discuss the impact of local labor market conditions on both the relative level of wages as well as on the assimilation rate of immigrants.

3.2. Biased estimates of immigrant assimilation and cohort effects?

Before we proceed to the empirical analysis, we briefly discuss the conditions under which failure to account for differential responsiveness of immigrant and native wages to changes in local unemployment will lead to bias in the standard synthetic panel methodology. Consider first the coefficient of *YSM*, α, in equation (8). Let $\widehat{\alpha}$ be the OLS estimator, based on the assumption of equal period effects and estimated *without* local unemployment among the right-hand side variables. Standard omitted variable discussion yields the following expression for the bias in $\widehat{\alpha}$:

$$E(\widehat{\alpha}) - \alpha = v\eta, \qquad (12)$$

where ν is the coefficient of *YSM* from a multiple regression in which the local unemployment rate is regressed on *YSM* and the other right-hand side variables of the model, and η is the difference between the immigrant and native wage-curve elasticities in equations (10) and (11). Because the standard framework through the inclusion of period effects captures average sensitivity of native wages to changes in unemployment, bias in $\hat{\alpha}$ will arise only if η^I differs from η^N.

As equation (12) reveals, the sign and size of the bias depend on two factors. The first factor relates to the conditional covariance between unemployment and *YSM* in the data at hand. Recall that the empirical specification conditions on the year of immigration, so, within immigrant cohorts, *YSM* is perfectly correlated with calendar time. This implies that if there is *a trend in unemployment* during the period of observation, ν will be significant and failure to account for unemployment effects may lead to biased estimates of assimilation rates. On the other hand, if there is no trend in unemployment over the period of observation, excluding unemployment from the empirical model does not introduce any bias in the estimated effect of years since migration.

The theoretical model in section 2 suggests that immigrant wages on average are more responsive to changes in unemployment than are native wages. Accordingly, the sign of the second factor, η, is expected to be negative. Thus, if there is a negative trend in unemployment over the period of observation, estimated assimilation rates will be contaminated by an upward bias. Conversely, if the trend is positive, estimated assimilation rates based on the standard empirical framework will be downward biased.

Consider next cohort effects. The omitted variable bias formula is similar to that in equation (12), with α interchanged with β, and where ν now reflects on the conditional covariance between year of immigration and the unemployment rate. If all immigrant cohorts are observed in equal proportions each sample year, there will be no correlation between the

(contemporary) unemployment rate and immigrant cohort in the data. Entry and exit of cohorts over time will, however, introduce covariance between calendar time and cohorts in the data, resulting in biased coefficient estimates if unemployment is rising or falling over the sample period.

In sum, if immigrant and native earnings respond differently to changes in unemployment and if there is a trend in unemployment over the sample period, the coefficient of *YSM* will be biased when the empirical model fails to account for unemployment effects on wages. Similarly, if immigrant cohorts are observed with varying proportions over the sample period, trends in unemployment may induce bias in estimated cohort effects on wages when estimates are based on the standard synthetic panel framework.

4. Data

To study the empirical linkages between local unemployment and wages of immigrants, it is desirable that the data contains sufficient time-series variation in local unemployment.[9] To provide background on recent trends in U.S. unemployment, Figure 1 plots the time series of the national unemployment rate between 1958 and 2002. The figure hints that census data, which form the basis for major studies of immigrant assimilation using the synthetic panel approach, are unlikely to contain much time-series variation in the unemployment rate, as the past four decennial census years all lie at the tail end of periods characterized by sustained economic expansion.[10] In light of the bias discussion of the preceding section, an implication of this observation is that estimates of immigrant earnings assimilation based on census data are unlikely to be contaminated by bias from failure to account for differential immigrant and native responsiveness to changes in unemployment. In

[9] Because the empirical model conditions on a fixed regional effect, estimation is based on variation in unemployment within regions.
[10] Recall that earnings questions in census data refer to the year prior to the census.

other words, because of the stability of macroeconomic conditions across census years, the assumption of equal period effects for immigrants and natives appears reasonable in census data. The native-immigrant wage gap, however, is likely to be extraordinary low in census data simply because evaluation is based on observation years with low rates of unemployment.

Both to obtain variation in the data and longer time series of local unemployment, in the empirical analyses we rely instead on data drawn from the Current Population Survey (CPS).[11] The CPS is a monthly survey covering about 60,000 households. Households are typically included in the survey for four consecutive months, out of the survey for the next eight months, and then back in the survey for another four months. Each month, one-quarter of those surveyed (i.e., the outgoing rotation groups) are asked detailed questions about labor earnings. Beginning in January 1994, questions relating to immigration have been part of the basic monthly questionnaire, and prior to that date supplemental questionnaires covering immigration topics were administered to all households participating in the survey in November 1979, April 1983, June 1986, June 1988, and June 1991. In the present study, analysis samples consist of all immigrants included in the 1994-2001 outgoing rotations and the earlier immigrant supplements. To optimize sample sizes, we merge immigration-related information for the individual from the pre-1994 supplements into the outgoing rotations data of the concurrent and following three surveys.[12]

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[11] Another important advantage of CPS data is that earnings information pertains more directly to hourly wages than in census data, where hourly wages must be computed by combining information on reported annual salary, weeks worked, and usual hours worked per week during the preceding year. If there is measurement error in computed annual hours, census-based estimates of immigrant wage assimilation will in part capture changes in hours worked as immigrants adjust to the U.S. labor market.

[12] Because every household that participated in, say, the June 1986 survey received the supplemental immigration questionnaire, earnings data are available for one-quarter of those households (i.e., the households that became outgoing rotations) in July 1986, and so forth. The merge algorithm uses CPS rotation, household id, gender, and age, and allows for the possibility of a birthday between the months of the supplement and the outgoing rotation when these are not the same. Funkhouser and Trejo (1995) employ a similar strategy for the CPS surveys from the 1980s. See also the discussion in Duleep and Regets (1997).

From the CPS outgoing rotations data, we keep every observation of foreign-borns of non-U.S. parents and a 20 percent random sample extract of natives. Because date of entry to the United States has not been asked consistently of individuals born in outlying areas (e.g., Puerto Rico), such observations are dropped. We further restrict regression samples to those aged 22 to 64 who are not enrolled in school and who usually work at least one hour per week at the time of the survey. The dependent variable of the empirical analyses is the natural logarithm of the hourly wage, with the hourly wage measured as the rate of pay for hourly employees and as weekly earnings divided by usual hours worked per week for salaried workers.[13] Individuals reporting earning less than $1.00 per hour (constant 1982-1984 dollars) are excluded from the samples.

The sample restrictions leave total samples of 367,764 observations (of whom 194,362 are males and 131,720 are immigrants) covering the 1979-2001 period. We merge into the micro samples monthly data on unemployment in the state of residence, defining the unemployment rate most relevant to the prevailing labor contract as the average state unemployment rate over the 12 months prior to the wage observation. The monthly unemployment rates are collected from the Local Area Unemployment Statistics (LAUS) program of the Bureau of Labor Statistics.[14] In total, the samples contain 5,916 observations of local unemployment (116 months times 51 states including District of Columbia). To avoid downward bias in standard errors caused by unobserved, common components of variance for individuals in the same labor market (Moulton, 1986), we calculate standard errors in all regression analyses using state-by-month clustering of observations. Sample

[13] We adjusted top-coded weekly earnings so as to obtain consistency across sample years. The adjustment first identified the real dollar value of the strictest top-coded value in the data and then replaced the weekly earnings of individuals earning more than this limit by 1.5 times the limit. The conclusions of the empirical analysis are, however, robust to whether or not we implement this adjustment.

[14] In the LAUS program, monthly estimates of state unemployment combine data from the CPS, the Current Employment Statistics (CES) program, and state unemployment insurance systems. For certain states, the monthly estimate is based on relatively small samples and may therefore contain measurement error. Our procedure of averaging state unemployment over 12-month windows will reduce such noise in the data.

descriptive statistics are presented in appendix tables A-1 and A-2. (As will be motivated in the next section, samples are split according to educational attainment with the high-education group consisting of those with educational attainment beyond a high-school diploma.)

An important concern for the empirical analysis is whether or not there is a trend in unemployment in the sample. With more than 80 percent of the sample points observed during the January 1994-December 2001 period, Figure 1 suggests that any such trend be negative. In fact, when we, based on the sample, regress the natural logarithm of our unemployment measure on a simple time trend (i.e., the year of observation), the coefficient estimate is -.0295 (s.e.=.0001). With a significant negative trend in the unemployment rate in the data, estimation results based on the synthetic panel model might be expected to be highly sensitive to treatment of period effects.

5. Empirical Analyses

5.1. Immigrant and native wage-curve responses

A central prediction from the theoretical framework of section 2 is that immigrant wages are more sensitive to changes in local unemployment than are wages of natives. To test this proposition, we begin the empirical analysis by applying the synthetic panel methodology (equations 8-9) augmented with simple wage-curve effects (equations 10-11), to the CPS samples. Equations are estimated separately for male and female workers; estimates of the wage-curve elasticities—the coefficients η^I and η^N of equations (10) and (11)—appear in Table 1.

As the table reveals, wages of immigrants do indeed exhibit greater responsiveness to changes in local unemployment than do wages of natives. According to the estimates in the first table row, an increase in local unemployment has, on average, a seven times greater

impact on the wages of immigrant men than of native men. A ten percent (not percentage points) increase in the unemployment rate reduces wages of immigrant men by 1.4 percent and wages of native men by .2 percent. Similarly, a ten percent increase in local unemployment is estimated to reduce wages of immigrant women by .9 percent while leaving the wages of native women basically unchanged. For both genders, the difference between immigrant and native wage-curve responses is highly significant. The evidence therefore confirms the prediction that immigrants are more adversely affected by economic downturns—and, conversely, benefit more from economic expansions—than natives.

The table also indicates that the magnitude of the wage-curve response depends on the educational attainment of the worker. Regardless of nativity or gender, the estimated wage-curve elasticity of high-school dropouts is more negative than that of better-educated workers. The finding is consistent with Card's (1995) suggestion that, because they tend to have greater levels of firm-specific human capital, better-educated workers experience a "smoothing" of their wage over the business cycle. Perhaps as important for the present study, however, is that the estimated wage-curve elasticity remains more negative for immigrants than for native workers even when we account for differences by educational attainment.[15]

The earnings profile, i.e., the relationship between experience and pay, depends on the educational attainment of the worker. A stylised fact of U.S. wage structures is that wages of better-educated workers are higher and continue to rise for a longer period than for lesser-educated workers. Such differences may be even more pronounced for immigrants. Educational skills acquired abroad and host-country specific skills such as language proficiency are likely to be complementary (Berman et al., 2000; Chiswick and Miller, 2002), with productivity of foreign skills expected to be low when immigrants do not master the

host-country language. Moreover, development of interpersonal networks and knowledge of social institutions may have a greater effect on the wages of highly educated immigrants, partly because they improve the precision of signals immigrants provide potential employers. As a result, returns to skills acquired abroad, such as educational attainment, are likely to increase as immigrants spend time in the host country. The ability to accumulate country-specific human capital may also depend on educational attainment, giving rise to different rates of wage assimilation for highly and lesser-educated immigrants. For such reasons, and because recent empirical evidence in Schoeni (1997), Betts and Lofstrom (2000), and Borjas (2000) indicate that the earnings assimilation process and earnings growth of U.S. immigrants is linked to educational attainment, in the following sections we study wage profiles of immigrants and natives separately for workers with low (high school or less) and high (at least some college) educational attainment.

5.2. Treatment of period effects and estimates of immigrant wage assimilation

The combination of greater wage-curve responsiveness of immigrants and a trend in unemployment will—according to the bias discussion of section 3—make estimates of immigrant wage assimilation sensitive to treatment of period effects. To investigate this issue, we estimate the synthetic panel model using three alternative specifications of the period effect (complete regression results are reported in appendix tables A-3 and A-4). In the first specification (cols. 1 and 4), we follow the standard approach and impose the restriction that period effects of immigrants are identical to those of natives. The second specification (cols. 2 and 5) adds simple wage curve effects but allows for differential responses of immigrants and natives; and the third specification (cols. 3 and 6) permits immigrant wage-curve responses to depend on years since migration by including interaction terms between the log

[15] Of the eight within-education cell comparisons in Table 1, in only one case (females with some college) is the

unemployment rate and the quartic polynomial of *YSM*. Because such interaction effects are statistically significant for all groups considered (see the last row of Tables A-3 and A-4), we proceed by contrasting results from the first ("standard methodology") and third ("augmented methodology") specifications of the period effect.[16] Besides the quadratic polynomials of age and years since migration and indicator variables for immigrant cohort, the set of control variables in the wage regressions includes marital status and educational attainment (interacted with immigrant status) as well as indicator variables for state of residence, year of observation, and country of origin.

Based on the augmented methodology, Figure 2 plots predicted wage paths (with 95 percent confidence intervals) between the ages of 25 and 50 of immigrants and a native comparison group for each of four gender-education groups. The immigrant profile describes the wage path of someone who arrives in the United States at age 25 and is evaluated at the weighted mean cohort and country of origin effects of the respective group. Both immigrant and native intercepts are evaluated at immigrant means of explanatory variables such as educational attainment, state of residence, and year of observation. Moreover, all profiles hold the state unemployment rate constant at 5.4 percent (the median unemployment rate in the immigrant sample).

As expected, the figure illustrates that wage profiles differ by educational attainment, with profiles of the low-education groups generally exhibiting less wage growth than those of the high-education groups. And although immigrant wage profiles initially are steeper than native profiles for all groups considered, only for the high-education groups are there visible

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difference not statistically significant at the one percent level. Complete test results are available upon request.

[16] Nakamura and Nakamura (1992) and Chiswick and Miller (2002) report evidence, based on cross-sectional census data, that current earnings of immigrants are affected by (national) unemployment at the time of entry into the United States. This finding suggests an alternative specification of the relationship between earnings and economic conditions than that used in the present study. When we include both the current unemployment rate (i.e., the average over the prior 12 months) and that at the time of entry in the empirical model, results support use of the current unemployment rate-specification. We reach the same conclusion when we include both unemployment measures in earnings regressions based on census data.

assimilation effects on immigrant wages. In fact, for lesser-educated immigrants wage growth of both men and women appears to stall approximately 10 to 15 years after arrival. Because the profiles of the native low-education comparison groups indicate continued, albeit moderate, wage growth, the result is that the wage gap between lesser-educated immigrants and natives actually widens after 20 years in the United States. Overall, the figure reveals sizeable wage gaps between immigrants and the native comparison groups without absolute wage convergence for any of the gender-education groups considered.

In Table 2, columns 2 and 3, we list the predicted log wage differentials between immigrants and natives, based on both standard and augmented methodologies. Columns 5 and 6 report the implied assimilation effects, computed as the difference in log wage growth between the ages of 25 and 35 (10-year growth) or 45 (20-year growth) for immigrants and natives. The table documents important differences in the patterns of wage gaps and wage growth from the two sets of estimates. For all four groups, the standard methodology indicates a substantial reduction of the wage gap with years in the United States. In other words, the standard methodology points to significant assimilation effects on immigrant wages, with estimated wage growth of immigrants after 20 years exceeding wage growth of natives by 16.3 and 19.4 percentage points for highly educated males and females and 9.9 and 4.8 points for the low-education groups. In comparison, the augmented methodology shows much smaller assimilation effects for higher-educated immigrants (after 20 years, 7.9 percentage points for males and 12.2 points for females) and, as was evident in Figure 1, zero or even negative assimilation effects for lesser-educated immigrants.[17]

[17] Based on census data and using a slightly different model specification and pooling low and high-education groups, Borjas (1999) computes an assimilation effect of 10.0 percentage points after 20 years for male immigrants. When we apply our specification and sample restrictions to samples drawn from 1970, 1980, and 1990 census data, we find greater assimilation effects (estimates ranging from 12 to 20 percentage points depending on group considered) than those reported in Table 2. We speculate that differences between CPS and census-based estimates in part are due to census estimates, because of measurement issues, being influenced by changes in hours worked. This issue warrants future consideration. As expected (because of the stability of

The finding that the standard methodology yields stronger assimilation effects on immigrant wages as compared to the augmented methodology is precisely as predicted by the bias discussion of section 3. Because wages of immigrants are more responsive to changes in economic conditions than are wages of natives, the relative immigrant wage improved as a result of the sustained economic expansion during the 1990s. When the empirical methodology fails to consider the differential effects of unemployment on immigrant and native wages, such favorable economic trends will be attributed to years since migration and estimates of assimilation effects will be upwardly biased. Put differently, the standard methodology overstates the wage gap at the time of entry and understates the wage gap for established immigrants. As shown in the fourth column of Table 2, the bias in estimated entry wages is between 5.3 and 9.1 percent of the native wage depending on gender and educational group, while at 20 years since migration the standard methodology understates wage gaps by 1.9 to 3.6 percentage points. As a result, the standard methodology overstates the gain in immigrant wages relative to native wages over the 20 years by 7.2 to 12.0 percentage points depending on the group considered (see Table 2, col. 7). What these results demonstrate is that, because immigrant wages are more sensitive to changes in economic conditions than are native wages, and because the unemployment rate trended downward over the sample period, estimates of assimilation effects are upwardly biased when the empirical model assumes that period effects are equal for immigrants and natives.

5.3. Local unemployment and the immigrant-native wage gap

With immigrant wages exhibiting greater sensitivity to economic conditions, the level of unemployment might be expected to influence the wage assimilation process. To shed light on this issue, in Figure 3 we plot the predicted wage gap between immigrants and natives for

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economic conditions across census years), census data yield only minor differences between estimates based on

three different levels of unemployment.[18] Interestingly, within each gender-education group the gap profiles roughly converge toward some common level irrespective of unemployment regime. After 25-30 years in the United States, the wage gap for lesser-educated male immigrants tends toward approximately 24 percent; for higher-educated males 17 percent; for lesser-educated females 19 percent; and for higher-educated females approximately 15 percent.

The path of the wage gap depends, however, importantly on the level of unemployment. At the time of entry, the wage gap for lesser-educated immigrants is approximately twice as large during the high-unemployment regime as compared to the low-unemployment regime (-.26 vs. -.15 log point for males; -.13 vs. -.06 log point for females). Put differently, lesser-educated immigrants benefit greatly from favorable economic conditions at the time of entry. For higher-educated immigrants, entry wages are less sensitive to economic conditions, but the rate of change of wages depends on the unemployment regime. Under favorable conditions, the immigrant-native wage gap of the highly educated groups reaches its long-term level after only ten years. Under less favorable economic conditions, the process takes 25 to 30 years.

In Figure 4, we focus specifically on the effect of local unemployment on the rate of change in the immigrant wage, and plot estimates of this effect against years since immigration for each of the four gender-education groups. The effect, which captures the cross-partial derivative of the log immigrant wage with respect to years since migration and the log unemployment rate ($\partial^2 w / \partial YSM \partial u$), plays an interesting role in the theoretical framework of section 2. On the one hand, from a bargaining perspective the effect describes the change in the wage-curve elasticity as the immigrant adapts to the new country. Because

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the standard and augmented methodologies.

[18] The unemployment rates are chosen to correspond to the 10th, 50th, and 90th percentiles in the immigrant sample.

the immigrant's bargaining outcome improves with years in the host country, this effect is positive. On the other hand, the cross-partial derivative also describes how wage growth from accumulation of country-specific human capital depends on economic conditions. According to theory, this relationship is negative at low *YSM* as increases in the unemployment rate slow the human capital acquisition of immigrants. Of course, by Young's theorem, $\partial^2 w / \partial u \partial YSM = \partial^2 w / \partial YSM \partial u$. Whether the cross-partial derivative at low *YSM* is negative or positive, therefore, depends on which of the two processes, bargaining or human capital accumulation, dominates the unemployment-wage relationship of immigrants. At high *YSM*, both effects are positive and pull in the same direction.

It follows that a prediction of the theoretical framework is that the cross-partial derivative is negative only at low *YSM* (if at all) and positive at high *YSM*. The plots in Figure 4 confirm this prediction. For all four groups, the estimate of the cross-partial derivative starts out negative (although statistically significant only for two of the four groups—see the parameter estimates listed in Tables A-3 and A-4). Consider, for example, the estimate for highly educated males. At the time of arrival, the cross effect is -.026, indicating that accumulation of U.S.-specific human capital dominates the bargaining process (the estimate is negative) and that a ten percent increase in unemployment lowers wage growth during the initial year in the United States by one-quarter percentage point. Moreover, as predicted by theory, at high values of *YSM* (empirically, 5-10 years) each estimate turns positive. These patterns are consistent with the dichotomous theoretical framework that holds that local unemployment affects the relative wages of immigrants both through their bargaining position and through their acquisition of country-specific human capital. Moreover, the finding that the cross-partial derivative of wages with respect to local unemployment and *YSM* is negative at the time of entry suggests that accumulation of country-specific human capital is particularly important for early wage growth of U.S. immigrants.

5.4. Immigrant cohort differentials

The final issue to consider is whether or not accounting for local unemployment impacts estimates of wage differentials across immigrant cohorts. A central theme of recent research has been the decline in wages across successive immigrant cohorts. Borjas (1995), for example, points to a secular decline in cohort effects and concludes that "(t)he relative entry wage of successive immigrant cohorts declined by 9% in the 1970s and by an additional 6% in the 1980s" (p. 201). Interestingly, the conclusion of Borjas (and of other studies that use census data) that entry wages continued to fall in the 1980s is contradicted by prior studies based on CPS data. Two recent studies to draw on the immigrant supplements to the CPS report evidence that the negative trend in cohort effects turned around with the immigrant cohorts of the late 1980s (Sorensen and Enchautegui, 1994; Funkhouser and Trejo, 1995). Both studies cite changes in U.S. immigration policy during the 1980s as a plausible explanation for such a turnaround. With enactment of the Immigration Act of 1990, U.S. policy has further strengthened its emphasis on skilled immigration, and it is of particular interest to assess whether or not such policy changes has resulted in higher entry wages of immigrants that arrived during the 1990s.

In Figure 5, we plot trends in estimated cohort differentials for each of the four gender-education groups based both on the standard and augmented methodologies. (To facilitate comparisons with the immigrant-native wage gaps discussed in the two preceding subsections, each displayed differential is computed as the deviation from the weighted mean cohort effect of the respective gender-education group.) Perhaps the most striking feature of the figure is the systematic differences between estimates from the two methodologies. As predicted by the bias discussion of section 3, the standard methodology overstates wage effects for recent immigrant cohorts. With a negative trend in unemployment over the sample

period, recent arrival cohorts are, on average, observed during more favorable economic times than are the older cohorts. When the empirical methodology assumes equal period effects for immigrants and natives, and therefore fails to consider the gain in relative immigrant wages caused by the economic upturn of the 1990s, estimated cohort effects for recent arrivals contain a positive bias. As the figure reveals, for the 1996-99 arrivals the bias in estimates based on the standard methodology is 9 to11 percentage points for the low-education groups and 6 to7 points for highly educated immigrants.

An important consequence of such bias is that the standard methodology understates the decline in earnings capacity across successive immigrant arrival cohorts. Consider, for example, lesser-educated male immigrants. According to the augmented methodology, wages of immigrants that arrived during the 1990s (i.e., the three most recent cohorts of the figure) were 17.1 percent below the wages of immigrants that arrived before 1970—a decline that is consistent with the census-based estimates of Borjas cited above. In comparison, the standard methodology places the decline at only 6.6 percent.

A third implication of Figure 5 is that the trend toward declining cohort effects has been stronger for lesser educated than for highly educated immigrants, but that, within education cells, there are small differences by gender. Again comparing the immigrant cohorts of the 1990s with those that arrived before 1970, and accounting for differential immigrant and native period effects, the decline in earnings capacity is estimated to be 17.1 and 17.5 percent for lesser-educated male and female immigrants compared to 2.3 and 3.7 percent for higher-educated male and female immigrants.

Finally, as the figure reveals, accounting for differential immigrant-native sensitivity to local unemployment has important consequences for conclusions regarding trends in cohort effects. When the methodology imposes equal period effects for immigrants and natives, estimates suggest a definite turnaround with significant positive trends in the cohort

effects of recent immigrant arrivals for all four gender-education groups. Consistent with the results of Funkhouser and Trejo (1995), who studied male immigrants, earnings capacity appears to improve in the late 1980s for both low and high education males. When the methodology allows for differential immigrant and native period effects, however, the positive trend among recent arrival cohorts disappears for three of the four gender-education groups considered. Instead, estimates from the augmented approach show that the negative trend in cohort effects continued through the 1980s. For these groups, the steady decline appears to have stalled and entry wages have stabilized with the arrival cohorts of the 1990s. Earnings capacity of recent immigrants remains low by historical standards, however, and is, as previously cited, as much as 17 percent below that of immigrants who arrived 30 years earlier. For the fourth group considered, that of highly educated male immigrants, the positive trend persists even though accounting for unemployment effects reduces its magnitude. The empirical evidence therefore supports the notion that the added emphasis of U.S. policy on skilled immigration during the 1990s has resulted in improved wages for highly educated male immigrants but not for other groups of immigrants.

6. Summary and Conclusion

This paper uses CPS data from 1979-2001 to examine the relationships between local labor market unemployment rate and immigrant and native wages. A principal finding of the study is that immigrant wages are more responsive than native wages to changes in local labor market conditions. As a result, the native-immigrant wage gap widens during economic downturns and contracts when labor markets strengthen. The empirical evidence reveals certain differences by educational attainment: For lesser-educated immigrants, local unemployment primarily affects the level of wages and, in particular, wages at the time of entry, while for higher-educated immigrants there is a larger effect of local unemployment on

wage growth during the early years in the United States. These results are consistent with our theoretical framework in which wages of immigrants are affected by local labor market conditions both through immigrants' bargaining outcomes and the accumulation of host-country specific human capital.

An important implication of these findings is that empirical studies of the labor market performance of immigrants must take into account trends in macroeconomic conditions in the data. Based on the CPS samples, we show that the standard synthetic panel methodology—which assumes that changes in aggregate macroeconomic and labor market conditions have the same relative impact on native and immigrant wages—yields upwardly biased estimates of immigrant wage growth. The positive bias arises because the methodology attributes wage effects of a negative trend in unemployment in the data to immigrant wage assimilation. The negative trend in unemployment also induces a positive bias in estimated cohort effects of recent immigrant arrivals when estimates are based on the standard methodology.

Augmenting the synthetic panel methodology with wage-curve effects, and allowing the elasticity of wages with respect to local unemployment to differ for immigrants and natives, we relax the equal-period effects assumption and account for differential responsiveness of immigrant and native wages to changes in economic conditions. According to the empirical analysis, the standard methodology overstates wage assimilation effects after 20 years in the United States by 7 to 12 percentage points depending on gender and educational attainment of the immigrant. Similarly, the positive bias in wages of immigrant cohorts that arrived during the late 1990s is estimated to be between 6 and 11 percent. When we control for local labor market conditions, we find that wages of low-education immigrants continued to decline into the 1990s but stabilized during that decade. Only for highly

educated male immigrants is there evidence that earnings capacity trended upward during the 1990s.

Interestingly, the patterns of bias in results based on the standard methodology and the CPS samples are exactly opposite of those we uncover in a companion study of immigrants to Norway (Barth et al, 2002b). But, importantly, the trend in macroeconomic conditions in the Norwegian samples is also opposite of that in the CPS data. Like many other European countries, Norway experienced a dramatic rise in unemployment during the 1980s and early 1990s, and this shift induced a positive trend in unemployment in the Norwegian data that cover the period 1980-96. In the Norwegian study, the positive trend in unemployment is shown to lead to severe negative bias in estimates of assimilation rates and understatement of earnings capacity of recent immigrant arrival cohorts from non-OECD countries. Taken together, the two studies from different continents offer reinforcing evidence that immigrants and natives are not equally affected by changes in macroeconomic conditions and that failure to consider such differences may seriously bias assessment of the economic progress of immigrants.

In a recent study, Lubotsky (2001) shows that measurement of native-immigrant earnings gaps depends on skill prices during the period of observation. Specifically, Lubotsky demonstrates that the earnings gap between natives and immigrants that arrived in the United States during the early 1990s is reduced by one quarter when evaluated using 1980 skill prices rather than those that prevailed during the 1990s. As such, assessments of native-immigrant wage differentials in the present study would have been smaller had we used 1980 rather than late-1990s skill prices. A closely related, and important, implication of the finding of the present study, that wages of immigrants are more sensitive to unemployment than are wages of natives, is that measurement of the relative economic performance of immigrants also depends on the economic conditions underlying the data at hand. In the United States,

the major studies of immigrant wage assimilation, such as Chiswick (1978), Borjas (1985; 1995), LaLonde and Topel (1992), and Schoeni (1997), are based on data from one, two, or all of the 1970, 1980, and 1990 censuses, and existing evidence is therefore conditional on the strength of the U.S. economy during the year preceding past census years. As Figure 1 revealed, each of the three censuses followed periods of significant economic expansion. In fact, the average national unemployment rate for 1969, 1979, and 1989 was 4.9 percent, while the average for the three decades of the 1970s, 1980s, and 1990s was 6.4 percent. According to our estimates, the difference implies that relative wages of lesser-educated male immigrants were 3.1 percentage points, and those of highly educated 2.3 points, higher in census data than under "normal" economic conditions. Thus, because of the favorable economic conditions during census years, past use of census data has lead to overstatement of the economic assimilation of U.S. immigrants. Unfortunately, such overstatement will likely only be exacerbated when researchers start making use of data from the 2000 census.

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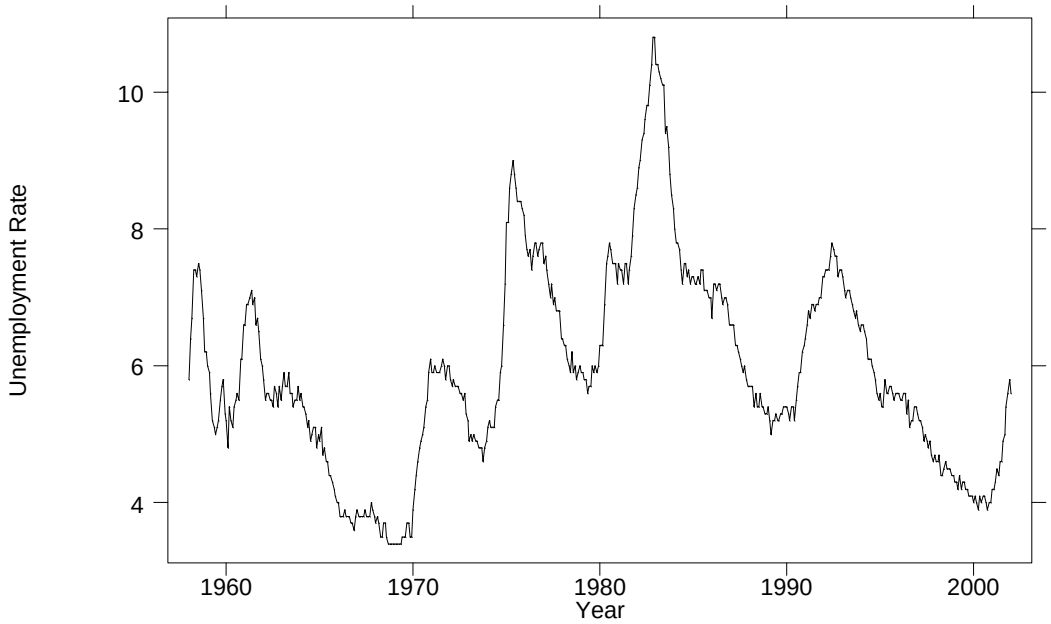

Fig. 1: Unemployment in the United States, 1958-2001

Source: Bureau of Labor Statistics (www.bls.gov).

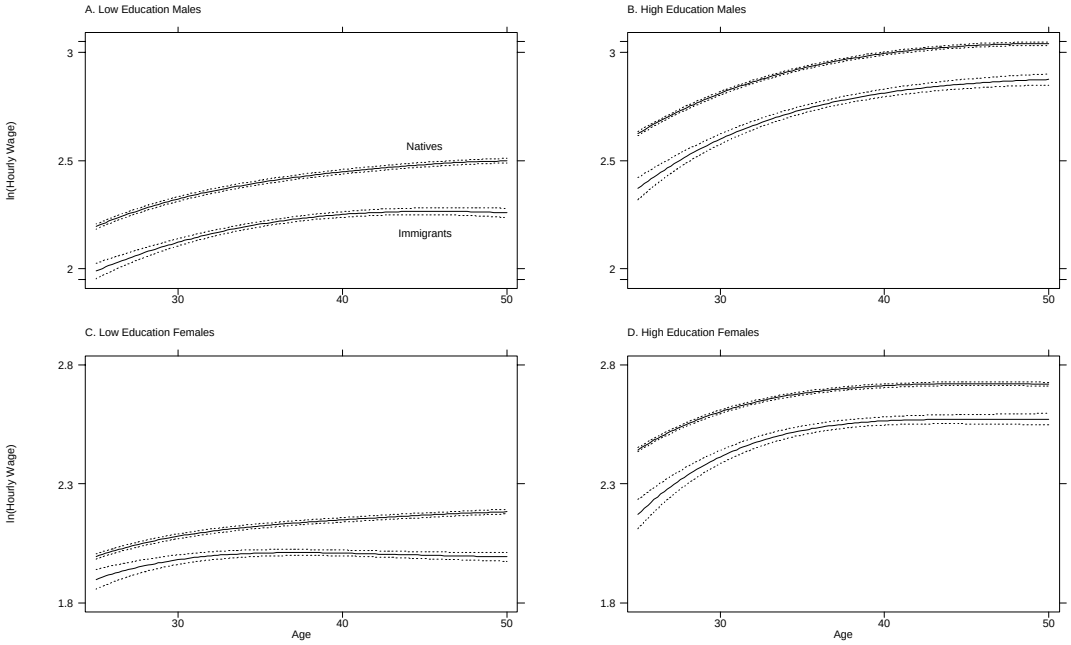

Fig. 2: Predicted Wage Profiles

NOTE: Profiles illustrate predicted wage paths with 95 percent confidence intervals for immigrants and a native comparison group. Predictions are based on coefficient estimates listed in tables A-3 and A-4, cols. 3 and 6, and are evaluated at the median unemployment rate in the immigrant sample (5.4 percent). Immigrant profiles are drawn for someone who is 25 years of age at the time of arrival and use the weighted average cohort and country-of-birth effects. Native and immigrant intercepts are both evaluated at mean characteristics (education, marital status, year of observation, and state of residence) of the respective immigrant sample.

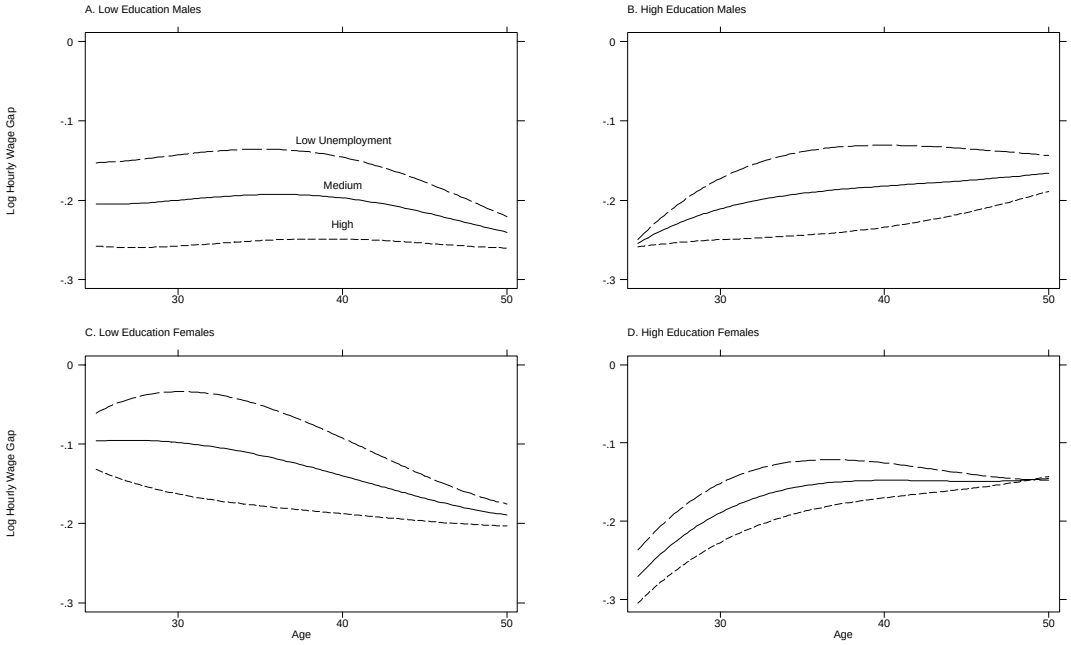

Fig. 3: Immigrant-Native Wage Differentials by Level of Unemployment

NOTE: Differentials are based on coefficient estimates listed in tables A-3 and A-4, cols. 3 and 6. Figures are evaluated at local unemployment rates of 3.8 percent (long dashes), 5.4 percent (solid), and 7.7 percent (short dashes), corresponding to the 10^{th}, 50^{th}, and 90^{th} percentiles of the immigrant sample. The underlying profiles are otherwise evaluated as in Figure 2.

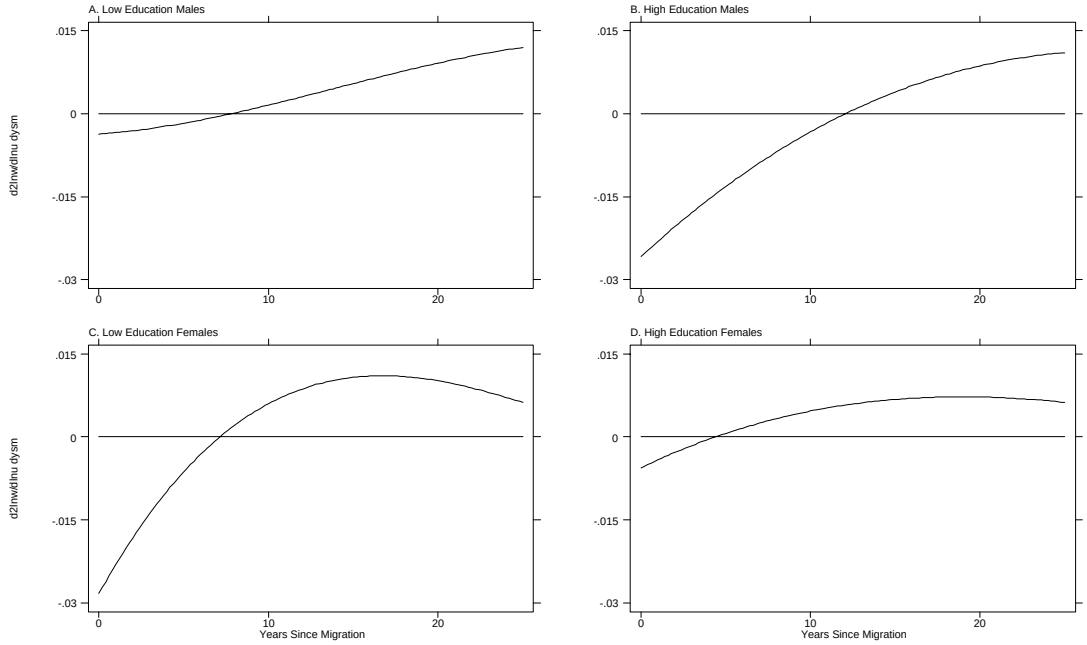

Fig. 4: Evolution of Cross-partial Derivative

NOTE: Figures illustrate the cross-partial derivate of the log immigrant wage with respect to log unemployment and years since migration and are based on coefficient estimates listed in tables A-3 and A-4, cols. 3 and 6.

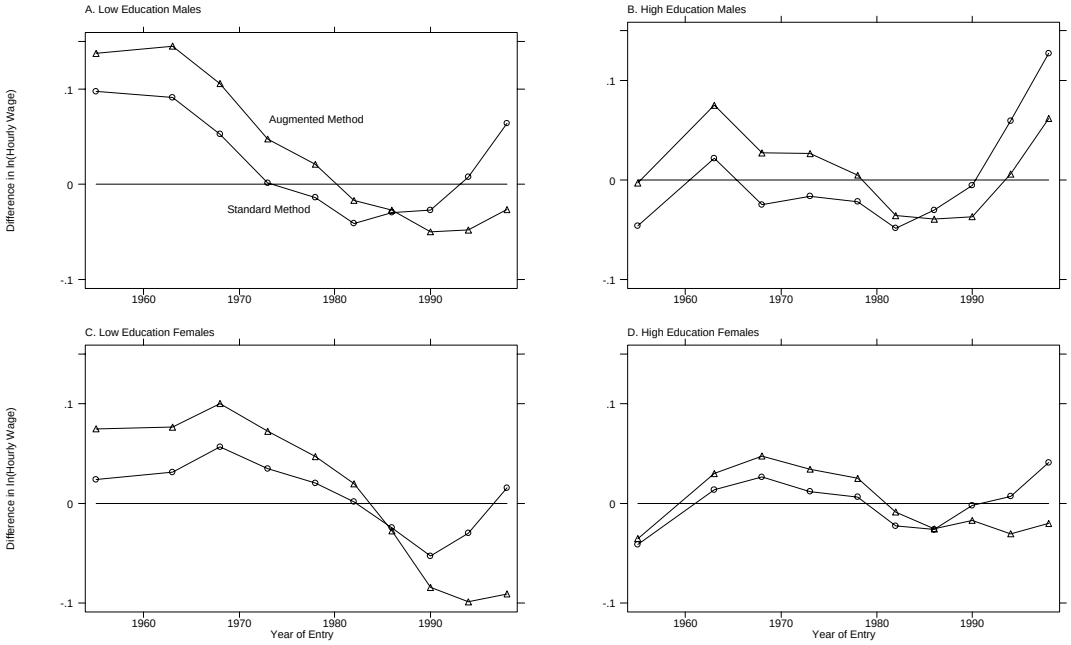

Fig. 5: Estimated Cohort Wage Differentials

NOTE: Arrival cohort wage differentials are based on coefficient estimates reported in tables A-3 and A-4, cols. 1 and 4 (standard method) and cols. 3 and 6 (augmented method). Displayed differentials are computed as deviations from the weighted mean coefficient estimate,

$$\hat{\beta}_i^* = \hat{\beta}_i - \sum_m w_m \hat{\beta}_m ,$$

where $\hat{\beta}_i^*$ is the displayed differential for immigrant cohort *i*, $\hat{\beta}_i$ is the coefficient estimate listed in the appendix table, and w_m is the proportion of gender-education cell belonging to arrival cohort *m* (i.e., the averages reported in table A-1).

Table 1: Wage-Curve Elasticities by Gender, Nativity, and Educational Attainment

	Males		Females	
	Immigrants	Natives	Immigrants	Natives
All Education Levels	-.1432	-.0207	-.0876	.0031
	(.0134)	(.0106)	(.0123)	(.0098)
Educational Attainment				
Less Than 12th Grade	-.1812	-.0573	-.1414	-.0456
	(.0283)	(.0277)	(.0288)	(.0294)
Completed High School	-.0988	-.0365	-.0773	-.0026
	(.0215)	(.0158)	(.0195)	(.0149)
Some College	-.1022	-.0383	-.0378	-.0087
	(.0257)	(.0198)	(.0250)	(.0190)
Bachelor's or Post-Graduate Degree	-.1398	-.0068	-.0682	.0166
	(.0250)	(.0206)	(.0258)	(.0199)

NOTE: Standard errors are reported in parentheses and are computed using state-by-month clustering of observations. Estimates are based on regression specifications similar to those listed in Tables A-3 and A-4, cols. 2 and 5.

Table 2: Predicted Immigrant-Native Log Wage Differentials and Immigrant Wage Assimilation

	Predicted Wage Differential			Estimated Wage Assimilation		
Years Since Migration	Standard Methodology	Augmented Methodology	Bias	Standard Methodology	Augmented Methodology	Bias
			A. Low Education Males			
0	-.2776	-.2050	-.0726			
	(.0184)	(.0192)				
10	-.2017	-.1929	-.0088	.0759	.0121	.0638
	(.0077)	(.0081)				
20	-.1791	-.2153	.0362	.0985	-.0103	.1088
	(.0094)	(.0098)				
			B. High Education Males			
0	-.3096	-.2540	-.0556			
	(.0248)	(.0265)				
10	-.2079	-.1912	-.0167	.1017	.0628	.0389
	(.0090)	(.0096)				
20	-.1469	-.1750	.0280	.1627	.0790	.0836
	(.0110)	(.0117)				
			C. Low Education Females			
0	-.1867	-.0962	-.0905			
	(.0209)	(.0217)				
10	-.1292	-.1140	-.0152	.0575	-.0178	.0753
	(.0080)	(.0085)				
20	-.1390	-.1681	.0291	.0477	-.0712	.1196
	(.0084)	(.0089)				
			D. High Education Females			
0	-.3240	-.2710	-.0530			
	(.0292)	(.0313)				
10	-.1619	-.1555	-.0064	.1621	.1155	.0466
	(.0098)	(.0102)				
20	-.1306	-.1491	.0185	.1934	.1219	.0715
	(.0102)	(.0107)				

NOTE: Standard errors are reported in parentheses and are computed using state-by-month clustering of observations. The immigrant-native wage differentials are based on coefficient estimates listed in Tables A-3 and A-4, cols. 1 and 4 (standard methodology) and cols. 3 and 6 (augmented methodology). The differential is computed for an immigrant who arrives in the United States at the age of 25. Estimates from the augmented methodology are evaluated at the median local unemployment rate in the immigrant sample (5.4 percent). Cumulative wage assimilation rates are calculated as the difference in predicted wage growth of immigrants and natives between the ages of 25 and 35/45.

Table A-1: Descriptive Statistics, Immigrant Samples

	Low Education Males		High Education Males		Low Education Females		High Education Females	
Variable	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
ln(Hourly Wage)	2.1802	0.4843	2.7392	0.6433	1.9902	0.4467	2.5153	0.5949
ln(Unemployment Rate)	1.6911	0.2749	1.6718	0.2840	1.6922	0.2779	1.6790	0.2751
Age	37.8009	10.7778	39.3363	10.1447	40.6687	10.7113	39.2623	10.1997
Married	0.6411	0.4797	0.6825	0.4655	0.6207	0.4852	0.6203	0.4853
EDUC2	0.1292	0.3354	0.3549	0.4785	0.1100	0.3130	0.3906	0.4879
EDUC3	0.4244	0.4943	0.2850	0.4514	0.5286	0.4992	0.1860	0.3891
Years Since Migration	14.4329	9.9753	15.9064	11.3775	16.4284	10.7527	17.2752	11.3353
Immigrant Cohort:								
Arrived Before 1960	0.0403	0.1968	0.0623	0.2416	0.0616	0.2405	0.0628	0.2426
1960-64	0.0305	0.1719	0.0481	0.2139	0.0496	0.2172	0.0559	0.2297
1965-69	0.0561	0.2302	0.0719	0.2583	0.0766	0.2659	0.0860	0.2803
1970-74	0.0956	0.2940	0.0985	0.2980	0.1118	0.3152	0.1159	0.3201
1975-75	0.1280	0.3341	0.1267	0.3326	0.1337	0.3403	0.1385	0.3455
1980-83	0.1221	0.3274	0.1129	0.3165	0.1215	0.3267	0.1126	0.3161
1984-87	0.1333	0.3399	0.1123	0.3157	0.1162	0.3205	0.1132	0.3169
1988-91 (omitted)	0.1644	0.3706	0.1339	0.3405	0.1441	0.3512	0.1283	0.3345
1992-95	0.1237	0.3292	0.1284	0.3346	0.1034	0.3045	0.1080	0.3104
1996-99	0.0775	0.2674	0.0785	0.2689	0.0550	0.2279	0.0581	0.2339
Country of Birth:								
Central America	0.6477	0.4777	0.1995	0.3996	0.5110	0.4999	0.2157	0.4113
South America	0.0540	0.2259	0.0639	0.2446	0.0709	0.2567	0.0682	0.2521
Asia	0.1342	0.3409	0.4074	0.4914	0.1989	0.3992	0.3941	0.4887
Africa	0.0088	0.0937	0.0401	0.1961	0.0087	0.0927	0.0239	0.1526
Country N/A	0.0349	0.1835	0.0591	0.2358	0.0386	0.1926	0.0473	0.2122
Canada,UK,Australia,NZ	0.0248	0.1554	0.0767	0.2661	0.0438	0.2046	0.0895	0.2854
Europe (omitted)	0.0957	0.2942	0.1533	0.3603	0.1282	0.3344	0.1614	0.3679
Observations	41,921		32,348		29,742		27,709	

NOTE: EDUC2 denotes grades 10 and 11 in the low-education samples and Bachelor's degree in the high-education samples; EDUC3 denotes completed high school in the low-education samples and post-graduate degree in the high-education samples.

Table A-2: Descriptive Statistics, Native Samples

	Low Education Males		High Education Males		Low Education Females		High Education Females	
Variable	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
ln(Hourly Wage)	2.3986	0.5112	2.7676	0.5891	2.0880	0.4889	2.5029	0.5631
ln(Unemployment Rate)	1.6415	0.3147	1.6169	0.3074	1.6393	0.3108	1.6045	0.3025
Age	39.9628	11.1430	39.8056	10.3694	41.1283	11.0986	39.1041	10.2875
Married	0.6610	0.4734	0.6828	0.4654	0.6064	0.4886	0.5965	0.4906
EDUC2	0.1257	0.3315	0.3436	0.4749	0.1063	0.3082	0.3291	0.4699
EDUC3	0.7915	0.4062	0.1747	0.3797	0.8403	0.3663	0.1534	0.3603
Observations	53,375		66,718		49,414		66,537	

NOTE: EDUC2 denotes grades 10 and 11 in the low-education samples and Bachelor's degree in the high-education samples; EDUC3 denotes completed high school in the low-education samples and post-graduate degree in the high-education samples

Table A-3: Log Wage Regressions, Low Education Samples

	Males			Females		
ln(Unempl Rate)		-.0374	-.0378		-.0072	-.0074
		(.0143)	(.0143)		(.0135)	(.0135)
Immigrant*		-.1172	-.1483		-.1134	-.1004
ln(Unempl R)		(.0149)	(.0402)		(.0144)	(.0415)
YSM*			-.0036			-.0283
ln(Unempl R)			(.0104)			(.0101)
(YSM^2/10)*			.0010			.0275
ln(Unempl R)			(.0084)			(.0080)
(YSM^3/100)*			.0014			-.0079
ln(Unempl R)			(.0025)			(.0023)
(YSM^4/1000)*			-.0003			.0007
ln(Unempl R)			(.0002)			(.0002)
YSM	.0233	.0168	.0167	.0224	.0150	.0137
	(.0040)	(.0040)	(.0040)	(.0042)	(.0042)	(.0044)
YSM^2/10	-.0053	-.0030	-.0053	-.0122	-.0092	-.0112
	(.0032)	(.0032)	(.0032)	(.0031)	(.0031)	(.0034)
YSM^3/100	.0002	-.0005	.0006	.0031	.0023	.0034
	(.0010)	(.0010)	(.0010)	(.0009)	(.0009)	(.0010)
YSM^4/1000	.0000	.0001	.0000	-.0003	-.0002	-.0003
	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)
Arrived Before	.1246	.2317	.1873	.0771	.1829	.1596
1960	(.0249)	(.0298)	(.0302)	(.0247)	(.0285)	(.0288)
1960-64	.1181	.2018	.1950	.0842	.1667	.1612
	(.0221)	(.0251)	(.0255)	(.0211)	(.0237)	(.0241)
1965-69	.0796	.1486	.1557	.1099	.1773	.1850
	(.0180)	(.0204)	(.0209)	(.0178)	(.0196)	(.0201)
1970-74	.0284	.0828	.0974	.0876	.1412	.1568
	(.0141)	(.0158)	(.0165)	(.0147)	(.0160)	(.0166)
1975-79	.0134	.0534	.0706	.0732	.1115	.1316
	(.0120)	(.0126)	(.0132)	(.0119)	(.0125)	(.0131)
1980-84	-.0141	.0168	.0331	.0548	.0836	.1042
	(.0110)	(.0112)	(.0118)	(.0112)	(.0112)	(.0117)
1984-87	-.0025	.0140	.0231	.0286	.0443	.0571
	(.0085)	(.0086)	(.0090)	(.0093)	(.0093)	(.0095)
1992-95	.0348	.0124	.0020	.0229	.0007	-.0146
	(.0093)	(.0093)	(.0096)	(.0097)	(.0098)	(.0103)
1996-99	.0912	.0424	.0234	.0683	.0179	-.0062
	(.0132)	(.0131)	(.0151)	(.0150)	(.0151)	(.0180)
Immigrant	1.9499	1.9731	2.013	1.3598	1.3852	1.4301
	(.5154)	(.5144)	(.5145)	(.5319)	(.5313)	(.5300)
Central America	-.1904	-.1852	-.1899	-.0940	-.0867	-.0892
	(.0092)	(.0092)	(.0092)	(.0086)	(.0085)	(.0085)
South America	-.1335	-.1306	-.1346	-.0577	-.0527	-.0553
	(.0126)	(.0126)	(.0126)	(.0119)	(.0119)	(.0119)
Asia	-.1497	-.1458	-.1505	-.0261	-.0222	-.0245
	(.0112)	(.0113)	(.0112)	(.0094)	(.0095)	(.0095)
Africa	-.1068	-.1091	-.1155	.0227	.0221	.0187
	(.0261)	(.0260)	(.0261)	(.0318)	(.0316)	(.0314)
Country N/A	-.0613	-.0577	-.0636	.0246	.0305	.0267
	(.0141)	(.0142)	(.0142)	(.0139)	(.0140)	(.0140)
Anglo	.0974	.0950	.0965	.0999	.1002	.1006
	(.0171)	(.0171)	(.0171)	(.0139)	(.0140)	(.0140)
Age	.2636	.2627	.2628	.2349	.2345	.2346
	(.0367)	(.0367)	(.0367)	(.0350)	(.0350)	(.0350)

$Age^2/10$	-.0848	-.0845	-.0845	-.0790	-.0789	-.0789
	(.0141)	(.0141)	(.0141)	(.0132)	(.0132)	(.0132)
$Age^3/1000$	.1280	.1275	.1275	.1209	.1207	.1207
	(.0231)	(.0231)	(.0231)	(.0215)	(.0215)	(.0215)
$Age^4/100000$	-.0747	-.0744	-.0745	-.0700	-.0699	-.0699
	(.0138)	(.0138)	(.0138)	(.0127)	(.0127)	(.0127)
Immigrant*	-.1933	-.1922	-.1946	-.1457	-.1443	-.1467
Age	(.0548)	(.0547)	(.0547)	(.0556)	(.0556)	(.0555)
Immigrant*	.0689	.0685	.0694	.0518	.0512	.0520
$Age^2/10$	(.0211)	(.0210)	(.0210)	(.0210)	(.0210)	(.0210)
Immigrant*	-.1129	-.1123	-.1136	-.0838	-.0827	-.0840
$Age^3/1000$	(.0348)	(.0347)	(.0347)	(.0342)	(.0342)	(.0341)
Immigrant*	.0691	.0687	.0695	.0504	.0497	.0504
$Age^4/100000$	(.0208)	(.0208)	(.0208)	(.0202)	(.0202)	(.0202)
Grades 10-11	.0994	.0990	.0990	.0615	.0613	.0613
	(.0086)	(.0086)	(.0086)	(.0095)	(.0095)	(.0095)
High School	.2725	.2723	.2723	.2707	.2705	.2705
	(.0074)	(.0074)	(.0074)	(.0082)	(.0082)	(.0082)
Immigrant*	-.0297	-.0296	-.0303	-.0009	-.0016	-.0019
Grades 10-11	(.0107)	(.0107)	(.0107)	(.0122)	(.0122)	(.0122)
Immigrant*	-.0712	-.0720	-.0727	-.0608	-.0623	-.0625
High School	(.0091)	(.0091)	(.0091)	(.0098)	(.0098)	(.0098)
Married	.1474	.1477	.1477	.0127	.0129	.0129
	(.0044)	(.0044)	(.0044)	(.0040)	(.0040)	(.0040)
Immigrant*	-.0742	-.0738	-.0743	.0115	.0099	.0097
Married	(.0063)	(.0063)	(.0063)	(.0062)	(.0062)	(.0062)
1983	.1775	.2027	.2029	.2179	.2257	.2256
	(.0134)	(.0157)	(.0157)	(.0122)	(.0142)	(.0142)
1986	.2580	.2673	.2672	.3202	.3233	.3232
	(.0131)	(.0132)	(.0132)	(.0130)	(.0130)	(.0130)
1988	.3106	.3114	.3112	.3838	.3845	.3844
	(.0131)	(.0129)	(.0129)	(.0125)	(.0123)	(.0123)
1991	.3892	.3961	.3959	.5369	.5402	.5401
	(.0132)	(.0132)	(.0132)	(.0123)	(.0122)	(.0122)
1994	.4491	.4590	.4586	.5773	.5827	.5823
	(.0111)	(.0112)	(.0112)	(.0107)	(.0107)	(.0107)
1995	.4775	.4814	.4810	.6003	.6038	.6035
	(.0109)	(.0108)	(.0108)	(.0106)	(.0105)	(.0105)
1996	.4982	.5004	.4999	.6344	.6373	.6369
	(.0110)	(.0110)	(.0110)	(.0107)	(.0106)	(.0106)
1997	.5326	.5317	.5312	.6696	.6717	.6713
	(.0111)	(.0112)	(.0112)	(.0106)	(.0106)	(.0106)
1998	.5758	.5704	.5701	.7051	.7057	.7054
	(.0113)	(.0116)	(.0116)	(.0107)	(.0111)	(.0111)
1999	.6152	.6068	.6066	.7423	.7417	.7415
	(.0111)	(.0117)	(.0117)	(.0107)	(.0113)	(.0113)
2000	.6416	.6290	.6289	.7905	.7884	.7883
	(.0112)	(.0121)	(.0121)	(.0107)	(.0116)	(.0116)
2001	.6757	.6654	.6656	.8352	.8340	.8340
	(.0113)	(.0120)	(.0120)	(.0107)	(.0115)	(.0115)
AK	.3710	.3823	.3824	.3861	.3903	.3903
	(.0253)	(.0253)	(.0253)	(.0249)	(.0251)	(.0251)
AZ	.0524	.0450	.0450	.1327	.1299	.1300
	(.0210)	(.0212)	(.0212)	(.0198)	(.0199)	(.0199)
AR	-.1174	-.1194	-.1194	-.0237	-.0241	-.0241
	(.0210)	(.0210)	(.0210)	(.0178)	(.0179)	(.0179)
CA	.1343	.1504	.1511	.2303	.2396	.2401
	(.0165)	(.0167)	(.0167)	(.0153)	(.0154)	(.0154)
CO	.1468	.1268	.1260	.2011	.1949	.1946
	(.0222)	(.0228)	(.0228)	(.0222)	(.0227)	(.0227)

CT	.1829	.1681	.1685	.2701	.2641	.2644
	(.0208)	(.0215)	(.0215)	(.0208)	(.0211)	(.0211)
DE	.1169	.1059	.1057	.1701	.1672	.1671
	(.0213)	(.0217)	(.0217)	(.0237)	(.0240)	(.0240)
DC	.0580	.0762	.0776	.2638	.2711	.2721
	(.0258)	(.0261)	(.0261)	(.0212)	(.0214)	(.0214)
FL	.0199	.0123	.0123	.1030	.0999	.0998
	(.0167)	(.0168)	(.0168)	(.0159)	(.0160)	(.0160)
GA	.0119	.0032	.0029	.0988	.0964	.0963
	(.0201)	(.0203)	(.0203)	(.0192)	(.0195)	(.0195)
HI	.1541	.1510	.1508	.2358	.2353	.2350
	(.0239)	(.0240)	(.0239)	(.0206)	(.0207)	(.0207)
ID	.0172	.0157	.0157	.0335	.0334	.0334
	(.0211)	(.0212)	(.0212)	(.0187)	(.0188)	(.0188)
IL	.1708	.1672	.1670	.1759	.1750	.1749
	(.0170)	(.0171)	(.0171)	(.0159)	(.0159)	(.0159)
IN	.0901	.0789	.0789	.1000	.0977	.0976
	(.0186)	(.0192)	(.0192)	(.0178)	(.0182)	(.0182)
IA	.0129	-.0083	-.0090	.0454	.0405	.0401
	(.0195)	(.0207)	(.0207)	(.0193)	(.0205)	(.0205)
KS	.0038	-.0097	-.0100	.0911	.0874	.0874
	(.0224)	(.0229)	(.0229)	(.0205)	(.0211)	(.0211)
KY	.0162	.0142	.0142	.0294	.0289	.0289
	(.0202)	(.0203)	(.0203)	(.0187)	(.0187)	(.0187)
LA	-.0088	-.0029	-.0029	-.0380	-.0364	-.0365
	(.0223)	(.0224)	(.0224)	(.0200)	(.0201)	(.0201)
ME	-.0108	-.0152	-.0152	.0698	.0690	.0689
	(.0195)	(.0196)	(.0196)	(.0188)	(.0189)	(.0189)
MD	.1516	.1427	.1425	.2632	.2603	.2602
	(.0219)	(.0220)	(.0220)	(.0208)	(.0210)	(.0210)
MA	.1573	.1424	.1424	.2487	.2427	.2426
	(.0186)	(.0196)	(.0196)	(.0169)	(.0175)	(.0175)
MI	.1580	.1545	.1544	.1424	.1415	.1413
	(.0176)	(.0177)	(.0177)	(.0157)	(.0157)	(.0157)
MN	.1187	.1003	.0998	.1717	.1667	.1665
	(.0208)	(.0218)	(.0218)	(.0215)	(.0223)	(.0223)
MS	-.0642	-.0612	-.0612	-.0438	-.0434	-.0434
	(.0217)	(.0216)	(.0216)	(.0205)	(.0205)	(.0205)
MO	.0586	.0499	.0497	.0898	.0877	.0876
	(.0212)	(.0215)	(.0215)	(.0190)	(.0193)	(.0193)
MT	-.0197	-.0213	-.0213	-.0130	-.0132	-.0133
	(.0223)	(.0223)	(.0223)	(.0207)	(.0208)	(.0208)
NE	.0037	-.0251	-.0258	.0458	.0378	.0373
	(.0212)	(.0232)	(.0232)	(.0196)	(.0216)	(.0216)
NV	.1303	.1242	.1240	.2308	.2285	.2285
	(.0198)	(.0200)	(.0200)	(.0182)	(.0183)	(.0183)
NH	.1269	.1068	.1072	.1734	.1685	.1687
	(.0211)	(.0223)	(.0223)	(.0213)	(.0222)	(.0222)
NJ	.2129	.2099	.2096	.2623	.2615	.2613
	(.0175)	(.0177)	(.0177)	(.0161)	(.0162)	(.0162)
NM	-.0076	-.0016	-.0016	.0206	.0228	.0226
	(.0225)	(.0225)	(.0225)	(.0233)	(.0233)	(.0233)
NY	.1381	.1427	.1427	.2170	.2201	.2203
	(.0166)	(.0167)	(.0167)	(.0152)	(.0152)	(.0152)
NC	-.0030	-.0179	-.0185	.0912	.0874	.0872
	(.0170)	(.0178)	(.0178)	(.0160)	(.0168)	(.0168)
ND	-.0389	-.0589	-.0589	-.0147	-.0192	-.0194
	(.0228)	(.0240)	(.0240)	(.0193)	(.0208)	(.0208)
OH	.0929	.0880	.0881	.1199	.1187	.1186
	(.0164)	(.0165)	(.0165)	(.0154)	(.0156)	(.0156)

OK	-.0339	-.0443	-.0444	.0574	.0546	.0545
	(.0199)	(.0204)	(.0204)	(.0190)	(.0194)	(.0194)
OR	.1337	.1359	.1360	.1410	.1420	.1420
	(.0206)	(.0206)	(.0206)	(.0215)	(.0214)	(.0215)
PA	.0809	.0788	.0787	.1305	.1300	.1300
	(.0167)	(.0168)	(.0168)	(.0158)	(.0159)	(.0159)
RI	.1232	.1209	.1209	.1701	.1700	.1700
	(.0219)	(.0221)	(.0221)	(.0196)	(.0196)	(.0197)
SC	-.0094	-.0139	-.0139	.0090	.0080	.0080
	(.0213)	(.0215)	(.0215)	(.0196)	(.0197)	(.0197)
SD	-.0508	-.0745	-.0751	-.0112	-.0169	-.0171
	(.0203)	(.0222)	(.0222)	(.0204)	(.0221)	(.0221)
TN	-.0208	-.0262	-.0263	.0674	.0662	.0662
	(.0205)	(.0206)	(.0206)	(.0181)	(.0182)	(.0182)
TX	.0051	.0030	.0031	.0617	.0611	.0611
	(.0164)	(.0164)	(.0164)	(.0152)	(.0152)	(.0152)
UT	.0924	.0724	.0716	.1132	.1074	.1071
	(.0223)	(.0231)	(.0231)	(.0194)	(.0202)	(.0202)
VT	.0141	-.0003	-.0001	.0984	.0949	.0950
	(.0216)	(.0224)	(.0224)	(.0218)	(.0225)	(.0225)
VA	.0709	.0532	.0525	.1543	.1484	.1479
	(.0204)	(.0213)	(.0213)	(.0190)	(.0198)	(.0198)
WA	.1682	.1699	.1700	.1975	.1988	.1989
	(.0218)	(.0219)	(.0219)	(.0222)	(.0222)	(.0222)
WV	-.0192	-.0089	-.0088	-.0417	-.0398	-.0397
	(.0220)	(.0222)	(.0222)	(.0193)	(.0197)	(.0197)
WI	.1116	.0979	.0979	.1119	.1085	.1084
	(.0192)	(.0200)	(.0200)	(.0177)	(.0184)	(.0184)
WY	.0853	.0789	.0789	.0308	.0290	.0290
	(.0244)	(.0244)	(.0244)	(.0238)	(.0238)	(.0239)
Constant	-1.6680	-1.6559	-1.6562	-1.5523	-1.5498	-1.5499
	(.3478)	(.3477)	(.3477)	(.3346)	(.3347)	(.3347)
R^2	.2765	.2770	.2771	.2803	.2806	.2807
Observations		95,296			79,156	
p-value,YSM*lnu Interactions			.0000			.0000

NOTE: Standard errors are reported in parentheses and are computed using state-by-month clustering of observations. In columns (3) and (6), YSM effects are evaluated at the median unemployment rate in the immigrant sample (5.4 percent). Omitted immigrant cohort is 1988-91 arrivals; omitted region of birth is Europe; omitted period is November 1979-February 1980; and omitted state is Alabama.

Table A-4: Log Wage Regressions, High Education Samples

	Males			Females		
ln(Unempl Rate)		-.0202	-.0202		.0045	.0043
		(.0145)	(.0145)		(.0135)	(.0135)
Immigrant*		-.0858	-.0130		-.0459	-.0954
ln(Unempl R)		(.0155)	(.0565)		(.0152)	(.0650)
YSM*			-.0258			-.0056
ln(Unempl R)			(.0151)			(.0159)
(YSM^2/10)*			.0141			.0074
ln(Unempl R)			(.0121)			(.0119)
(YSM^3/100)*			-.0020			-.0016
ln(Unempl R)			(.0036)			(.0033)
(YSM^4/1000)*			.0001			.0001
ln(Unempl R)			(.0003)			(.0003)
YSM	.0276	.0225	.0252	.0377	.0350	.0323
	(.0056)	(.0056)	(.0058)	(.0061)	(.0061)	(.0063)
YSM^2/10	-.0045	-.0028	-.0072	-.0132	-.0123	-.0127
	(.0044)	(.0044)	(.0046)	(.0045)	(.0045)	(.0046)
YSM^3/100	.0000	-.0005	.0011	.0024	.0021	.0027
	(.0013)	(.0013)	(.0014)	(.0013)	(.0013)	(.0013)
YSM^4/1000	.0000	.0001	-.0001	-.0002	-.0001	-.0002
	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)
Arrived Before	-.0405	.0487	.0338	-.0391	.0097	-.0180
1960	(.0365)	(.0408)	(.0414)	(.0352)	(.0380)	(.0382)
1960-64	.0269	.0969	.1117	.0161	.0547	.0472
	(.0268)	(.0298)	(.0303)	(.0285)	(.0306)	(.0307)
1965-69	-.0196	.0371	.0637	.0286	.0603	.0648
	(.0222)	(.0240)	(.0245)	(.0240)	(.0255)	(.0260)
1970-74	-.0107	.0336	.0634	.0141	.0389	.0519
	(.0187)	(.0202)	(.0211)	(.0204)	(.0215)	(.0223)
1975-79	-.0167	.0150	.0415	.0084	.0266	.0427
	(.0160)	(.0170)	(.0179)	(.0175)	(.0182)	(.0190)
1980-83	-.0430	-.0198	.0010	-.0201	-.0070	.0088
	(.0148)	(.0153)	(.0162)	(.0156)	(.0159)	(.0166)
1984-87	-.0247	-.0126	-.0025	-.0241	-.0178	-.0082
	(.0129)	(.0130)	(.0133)	(.0135)	(.0135)	(.0139)
1992-95	.0644	.0477	.0426	.0095	.0004	-.0130
	(.0129)	(.0130)	(.0135)	(.0144)	(.0147)	(.0152)
1996-99	.1327	.0948	.0983	.0432	.0240	-.0029
	(.0189)	(.0195)	(.0222)	(.0207)	(.0214)	(.0241)
Immigrant	-.2127	-.2260	-.2569	-.3408	-.3407	-.2742
	(.7478)	(.7470)	(.7467)	(.6834)	(.6833)	(.6839)
Central America	-.2252	-.2224	-.2234	-.1045	-.1029	-.1043
	(.0109)	(.0109)	(.0109)	(.0111)	(.0111)	(.0111)
South America	-.1265	-.1258	-.1271	-.0719	-.0713	-.0732
	(.0145)	(.0145)	(.0145)	(.0140)	(.0140)	(.0140)
Asia	-.0420	-.0391	-.0399	.0369	.0388	.0381
	(.0096)	(.0096)	(.0096)	(.0102)	(.0102)	(.0102)
Africa	-.2025	-.2046	-.2066	-.0541	-.0544	-.0570
	(.0183)	(.0183)	(.0184)	(.0221)	(.0221)	(.0220)
Country N/A	-.0884	-.0827	-.0852	.0057	.0086	.0064
	(.0147)	(.0147)	(.0147)	(.0161)	(.0161)	(.0161)
Anglo	.1856	.1838	.1836	.1329	.1319	.1319
	(.0143)	(.0144)	(.0143)	(.0137)	(.0137)	(.0137)
Age	.2511	.2511	.2510	.3505	.3503	.3502
	(.0401)	(.0401)	(.0402)	(.0368)	(.0368)	(.0368)

$Age^2/10$	-.0684	-.0684	-.0684	-.1079	-.1078	-.1078
	(.0153)	(.0153)	(.0153)	(.0141)	(.0141)	(.0141)
$Age^3/1000$	.0881	.0882	.0882	.1500	.1499	.1499
	(.0252)	(.0252)	(.0252)	(.0233)	(.0233)	(.0233)
$Age^4/100000$	-.0457	-.0457	-.0457	-.0798	-.0797	-.0797
	(.0151)	(.0151)	(.0151)	(.0140)	(.0140)	(.0140)
Immigrant*	.0260	.0296	.0328	.0183	.0198	.0152
Age	(.0784)	(.0784)	(.0784)	(.0716)	(.0716)	(.0716)
Immigrant*	-.0181	-.0192	-.0205	-.0100	-.0106	-.0088
$Age^2/10$	(.0299)	(.0299)	(.0299)	(.0273)	(.0273)	(.0273)
Immigrant*	.0343	.0356	.0380	.0132	.0141	.0111
$Age^3/1000$	(.0491)	(.0490)	(.0490)	(.0447)	(.0447)	(.0447)
Immigrant*	-.0212	-.0219	-.0233	-.0043	-.0049	-.0030
$Age^4/100000$	(.0293)	(.0293)	(.0293)	(.0267)	(.0267)	(.0267)
Bachelor's	.2591	.2592	.2592	.2832	.2832	.2832
Degree	(.0043)	(.0043)	(.0043)	(.0042)	(.0042)	(.0042)
Post-Graduate	.3789	.3788	.3788	.4739	.4739	.4739
Degree	(.0059)	(.0059)	(.0059)	(.0058)	(.0058)	(.0058)
Immigrant*	.0050	.0042	.0042	.0020	.0020	.0021
Bachelor's	(.0085)	(.0085)	(.0085)	(.0083)	(.0083)	(.0083)
Immigrant*	.1191	.1161	.1154	.0410	.0398	.0392
Post-Grad	(.0099)	(.0099)	(.0099)	(.0107)	(.0107)	(.0107)
Married	.1601	.1603	.1603	.0239	.0239	.0240
	(.0045)	(.0045)	(.0045)	(.0038)	(.0038)	(.0038)
Immigrant*	-.0351	-.0366	-.0370	-.0022	-.0030	-.0030
Married	(.0088)	(.0088)	(.0088)	(.0075)	(.0075)	(.0075)
1983	.2149	.2290	.2292	.2277	.2268	.2271
	(.0152)	(.0173)	(.0173)	(.0165)	(.0185)	(.0185)
1986	.3439	.3492	.3492	.3931	.3932	.3931
	(.0153)	(.0154)	(.0154)	(.0160)	(.0162)	(.0162)
1988	.3687	.3688	.3688	.4424	.4432	.4429
	(.0148)	(.0147)	(.0147)	(.0156)	(.0157)	(.0157)
1991	.5280	.5323	.5325	.6228	.6236	.6233
	(.0150)	(.0150)	(.0150)	(.0152)	(.0153)	(.0153)
1994	.5770	.5826	.5826	.6993	.7005	.7000
	(.0126)	(.0128)	(.0128)	(.0137)	(.0139)	(.0139)
1995	.5936	.5962	.5962	.7199	.7215	.7210
	(.0126)	(.0126)	(.0126)	(.0134)	(.0135)	(.0135)
1996	.6255	.6272	.6273	.7350	.7367	.7362
	(.0128)	(.0128)	(.0128)	(.0136)	(.0137)	(.0137)
1997	.6650	.6651	.6652	.7759	.7776	.7772
	(.0127)	(.0127)	(.0127)	(.0135)	(.0136)	(.0136)
1998	.6930	.6908	.6909	.8251	.8271	.8267
	(.0127)	(.0130)	(.0130)	(.0135)	(.0138)	(.0138)
1999	.7422	.7386	.7387	.8638	.8659	.8655
	(.0125)	(.0130)	(.0130)	(.0136)	(.0140)	(.0140)
2000	.7895	.7837	.7839	.9055	.9077	.9074
	(.0127)	(.0135)	(.0135)	(.0135)	(.0142)	(.0142)
2001	.8188	.8143	.8145	.9435	.9457	.9455
	(.0127)	(.0133)	(.0133)	(.0136)	(.0140)	(.0141)
AK	.1960	.2024	.2024	.3016	.3013	.3013
	(.0252)	(.0254)	(.0254)	(.0234)	(.0237)	(.0237)
AZ	.0282	.0254	.0254	.1127	.1129	.1128
	(.0215)	(.0215)	(.0215)	(.0215)	(.0216)	(.0216)
AR	-.1549	-.1558	-.1558	-.0067	-.0065	-.0065
	(.0233)	(.0233)	(.0233)	(.0226)	(.0226)	(.0226)
CA	.1845	.1912	.1916	.2848	.2859	.2860
	(.0166)	(.0167)	(.0167)	(.0167)	(.0169)	(.0169)
CO	.1032	.0945	.0945	.1616	.1625	.1624
	(.0205)	(.0212)	(.0212)	(.0208)	(.0213)	(.0213)

CT	.2056	.1976	.1976	.2795	.2799	.2799
	(.0224)	(.0227)	(.0227)	(.0221)	(.0225)	(.0225)
DE	.0927	.0867	.0866	.1901	.1908	.1907
	(.0237)	(.0239)	(.0240)	(.0212)	(.0214)	(.0214)
DC	.1450	.1552	.1555	.3333	.3336	.3342
	(.0251)	(.0253)	(.0253)	(.0227)	(.0230)	(.0230)
FL	-.0074	-.0111	-.0111	.1337	.1335	.1336
	(.0190)	(.0191)	(.0191)	(.0173)	(.0174)	(.0174)
GA	.0492	.0445	.0445	.1171	.1175	.1175
	(.0220)	(.0221)	(.0221)	(.0203)	(.0204)	(.0204)
HI	.0661	.0652	.0652	.1284	.1286	.1286
	(.0228)	(.0228)	(.0228)	(.0217)	(.0217)	(.0217)
ID	-.0675	-.0680	-.0680	.0056	.0056	.0056
	(.0218)	(.0218)	(.0218)	(.0221)	(.0221)	(.0221)
IL	.1422	.1407	.1406	.1932	.1932	.1932
	(.0178)	(.0178)	(.0178)	(.0176)	(.0176)	(.0176)
IN	.0206	.0140	.0142	.0872	.0882	.0882
	(.0228)	(.0232)	(.0232)	(.0223)	(.0226)	(.0226)
IA	-.0580	-.0693	-.0692	.0397	.0415	.0411
	(.0228)	(.0238)	(.0238)	(.0211)	(.0220)	(.0220)
KS	-.0351	-.0420	-.0419	.0119	.0127	.0125
	(.0244)	(.0248)	(.0248)	(.0218)	(.0221)	(.0221)
KY	-.0482	-.0492	-.0492	.0318	.0319	.0319
	(.0235)	(.0235)	(.0235)	(.0229)	(.0229)	(.0229)
LA	-.0237	-.0197	-.0197	.0309	.0303	.0303
	(.0260)	(.0262)	(.0262)	(.0202)	(.0204)	(.0204)
ME	-.1067	-.1088	-.1087	.0458	.0461	.0461
	(.0231)	(.0232)	(.0232)	(.0232)	(.0232)	(.0232)
MD	.1584	.1536	.1537	.2526	.2528	.2526
	(.0213)	(.0215)	(.0215)	(.0205)	(.0206)	(.0206)
MA	.1209	.1136	.1135	.2357	.2360	.2358
	(.0181)	(.0183)	(.0183)	(.0182)	(.0185)	(.0185)
MI	.1385	.1362	.1363	.1904	.1906	.1905
	(.0177)	(.0177)	(.0177)	(.0183)	(.0183)	(.0183)
MN	.0869	.0768	.0768	.1659	.1672	.1670
	(.0209)	(.0218)	(.0218)	(.0195)	(.0202)	(.0202)
MS	-.1268	-.1250	-.1250	-.0410	-.0414	-.0414
	(.0245)	(.0245)	(.0245)	(.0221)	(.0221)	(.0221)
MO	-.0332	-.0379	-.0378	.0642	.0650	.0650
	(.0224)	(.0226)	(.0226)	(.0211)	(.0212)	(.0212)
MT	-.2212	-.2216	-.2216	-.1204	-.1204	-.1204
	(.0227)	(.0227)	(.0227)	(.0213)	(.0213)	(.0213)
NE	-.0798	-.0943	-.0943	-.0143	-.0120	-.0123
	(.0210)	(.0229)	(.0229)	(.0207)	(.0226)	(.0226)
NV	.0238	.0214	.0213	.1402	.1402	.1403
	(.0209)	(.0210)	(.0210)	(.0213)	(.0213)	(.0213)
NH	.0668	.0561	.0565	.1399	.1412	.1413
	(.0234)	(.0241)	(.0241)	(.0232)	(.0239)	(.0239)
NJ	.2460	.2450	.2449	.3053	.3054	.3054
	(.0183)	(.0184)	(.0184)	(.0184)	(.0184)	(.0184)
NM	-.0371	-.0338	-.0338	.0013	.0009	.0009
	(.0234)	(.0235)	(.0235)	(.0219)	(.0220)	(.0220)
NY	.1539	.1566	.1566	.2531	.2536	.2536
	(.0171)	(.0171)	(.0171)	(.0171)	(.0172)	(.0172)
NC	-.0002	-.0078	-.0078	.1034	.1045	.1044
	(.0191)	(.0197)	(.0197)	(.0181)	(.0186)	(.0186)
ND	-.1696	-.1804	-.1804	-.0740	-.0721	-.0723
	(.0218)	(.0230)	(.0230)	(.0213)	(.0223)	(.0223)
OH	.0328	.0302	.0302	.1268	.1272	.1271
	(.0180)	(.0181)	(.0181)	(.0178)	(.0178)	(.0178)

OK	-.0721	-.0774	-.0773	-.0093	-.0086	-.0086
	(.0227)	(.0230)	(.0230)	(.0228)	(.0230)	(.0230)
OR	.0048	.0058	.0058	.0970	.0969	.0969
	(.0214)	(.0214)	(.0214)	(.0214)	(.0214)	(.0214)
PA	.0826	.0816	.0816	.1599	.1600	.1600
	(.0182)	(.0182)	(.0182)	(.0181)	(.0181)	(.0181)
RI	.0608	.0600	.0599	.1967	.1969	.1968
	(.0230)	(.0230)	(.0230)	(.0230)	(.0230)	(.0230)
SC	-.0487	-.0507	-.0506	.0672	.0675	.0675
	(.0242)	(.0242)	(.0242)	(.0215)	(.0216)	(.0216)
SD	-.1822	-.1947	-.1946	-.0286	-.0263	-.0265
	(.0233)	(.0248)	(.0248)	(.0210)	(.0225)	(.0225)
TN	-.0520	-.0547	-.0547	.0574	.0578	.0578
	(.0218)	(.0219)	(.0219)	(.0210)	(.0211)	(.0211)
TX	.0464	.0459	.0459	.1224	.1225	.1225
	(.0178)	(.0178)	(.0178)	(.0175)	(.0175)	(.0175)
UT	-.0124	-.0220	-.0218	.0565	.0574	.0573
	(.0201)	(.0209)	(.0209)	(.0210)	(.0216)	(.0216)
VT	-.0377	-.0461	-.0458	.0443	.0454	.0453
	(.0248)	(.0253)	(.0253)	(.0225)	(.0230)	(.0230)
VA	.1220	.1110	.1108	.1655	.1660	.1658
	(.0209)	(.0216)	(.0216)	(.0215)	(.0221)	(.0221)
WA	.0669	.0683	.0682	.1495	.1495	.1494
	(.0211)	(.0212)	(.0211)	(.0213)	(.0213)	(.0213)
WV	-.0784	-.0723	-.0723	-.0117	-.0129	-.0128
	(.0246)	(.0250)	(.0250)	(.0223)	(.0227)	(.0227)
WI	.0498	.0423	.0424	.0962	.0973	.0973
	(.0210)	(.0216)	(.0216)	(.0201)	(.0206)	(.0206)
WY	-.0728	-.0757	-.0757	-.0629	-.0624	-.0625
	(.0228)	(.0229)	(.0229)	(.0214)	(.0215)	(.0215)
Constant	-1.6665	-1.6654	-1.6651	-2.8146	-2.8144	-2.8138
	(.3815)	(.3815)	(.3815)	(.3475)	(.3475)	(.3475)
R^2	.3133	.3135	.3135	.3062	.3062	.3062
Observations		99,066			94,246	
p-value,YSM*lnu Interactions			.0000			.0026

NOTE: Standard errors are reported in parentheses and are computed using state-by-month clustering of observations. In columns (3) and (6), YSM effects are evaluated at the median unemployment rate in the immigrant sample (5.4 percent). Omitted immigrant cohort is 1988-91 arrivals; omitted region of birth is Europe; omitted period is November 1979-February 1980; and omitted state is Alabama.