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Why are immigrants' incarceration rates so low?: Evidence on selective immigration, deterrence, and deportation

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*Why Are Immigrants' Incarceration Rates So Low?
Evidence on Selective Immigration, Deterrence, and Deportation*

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*Why Are Immigrants' Incarceration Rates So Low?
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Immigrants to the United States tend to have characteristics in common with native-born populations that are disproportionately incarcerated. The perception that immigration adversely affects crime rates led to legislation in the 1990s that particularly increased punishment of criminal aliens. In fact, immigrants have much lower institutionalization (incarceration) rates than the native born—on the order of one-fifth the rate of natives. More recently arrived immigrants have the lowest relative incarceration rates, and this difference increased from 1980 to 2000.

We present a model of immigrant self-selection that suggests why, despite poor labor market outcomes, immigrants may have better incarceration outcomes than the native born. We examine whether the improvement in immigrants' relative incarceration rates over the last three decades is linked to increased deportation, immigrant self-selection, or deterrence. Our evidence suggests that deportation is not driving the results. Rather, the process of migration selects individuals who are more responsive to deterrent effects than the average native. Immigrants who were already in the country reduced their relative institutionalization probability over the decades; and the newly arrived immigrants in the 1980s and 1990s seem to be particularly unlikely to be involved in criminal activity, consistent with increasingly positive selection along this dimension.

I. Introduction

Much of the concern that immigration to the United States adversely affects crime derives from the fact that immigrants tend to have characteristics in common with native-born populations that are disproportionately incarcerated. That is, immigrants have low average levels of education and very low average wages, and many are young, male, and Hispanic. For similar reasons, there are general concerns that immigration adds to the “underclass” in the United States by increasing dependence on cash assistance and subsidized medical care, decreasing homeownership, and creating pockets of entrenched poverty with adverse social outcomes.¹ During the 1990s, when immigration rates were high and crime rates were high and rising, observers feared a link between immigration and crime, and several significant pieces of federal legislation increased criminal penalties for noncitizens.

Economic theories tend to support the concern about a link between immigration and crime. The economic model of crime (Becker 1968), for example, posits that those who have poor labor market outcomes—and thus low opportunity costs from giving up activities in the legal sector—will be more likely to engage in criminal activity. Many studies have documented immigrants’ poor labor market outcomes (see, for example, Borjas 2004). These outcomes are due in part to the low skills that many immigrants bring with them, and in part to immigrants’ loss of other elements of human capital (such as language and social networks) that enable individuals to make full use of their skills. A one-dimensional model of skills would lead one to expect that a population with poor labor market outcomes would also have poor outcomes in other arenas such as crime, health, and family life.

¹ Research on the criminal-justice outcomes of immigrants is limited (Mears 2002). However, research on other outcomes shows that immigrants are less likely to use welfare than similar natives (Butcher and Hu 2000). Immigrants are less likely to own homes than the native-born, and this gap widened between 1980 and 2000. However, this gap is mainly driven by location choice and immigrants’ country of origin. Future increases in immigrant enclaves may be expected to increase demand for owner-occupied housing (Borjas 2002). Other research has examined the participation of immigrants in mainstream financial institutions. Recent evidence suggests that immigrants are less likely to participate in financial markets,

In this paper, we examine immigrants' institutionalization rates as a proxy for incarceration, and thus their involvement in criminal activity. Contrary to what one might expect from labor market studies, immigrants have very low rates of institutionalization compared with the native born. What's more, immigrants' relative rates of institutionalization have fallen over the last three decades. More recent cohorts also have better criminal-justice outcomes than earlier cohorts, and synthetic cohort analyses show that immigrants' relative rates of institutionalization tend to decline with time in the country. If one assumed that the relationship between "skills" and outcomes is the same among immigrants and the native born, this is precisely the opposite of what one would have predicted from most synthetic cohort analyses of immigrants' labor market outcomes.

We begin with a model of immigrant selectivity. This model shows how migration may peel apart different dimensions of "skills," such that these skills enable immigrants with poor labor market outcomes to achieve good outcomes in other arenas. Next we document immigrants' low institutionalization rates, the cohort patterns in institutionalization, and how these have changed over time. Then we investigate potential reasons for these changes. Important laws enacted in the 1990s increased penalties for criminal noncitizens by broadening the crimes for which they could be deported. Did this mechanically lower immigrants' institutionalization rates by ensuring that criminal aliens were deported? Or did the greater punishment change immigrants' criminal activity in the U.S.? Beyond laws specifically increasing punishments for criminal aliens, the 1980s and 1990s saw increased punishment for crimes more generally. Did these changes affect immigrants' behavior more than the native born? Or did increased punishment for criminal activity combined with welfare reform—which

that these differences tend to persist, and that they may be driven by immigrants' experience with financial institutions in their countries of origin (Osili and Paulson 2004a, b).

decreased immigrants' access to social welfare programs—change the type of person who self-selects to immigrate to the United States?

We present a variety of tests of these potential explanations. We rule out deportation as an important mechanical factor for the observed differences in institutionalization. Instead, the evidence is consistent with a model of multiple dimensions regarding who self-selects to immigrate to the United States. That is, the type of person who chose to immigrate to the United States in the 1990s shifted toward those who are less criminally active. The results also suggest that immigrants are more responsive to criminal punishment than the average native, and decreased their criminal activity in response to harsher criminal penalties imposed during the 1980s and 1990s.

II. Immigrant Self-Selection

The last four decades have seen striking changes in immigration to the United States. Borjas (2004) provides a thorough accounting of the experience of immigrants in the U.S. labor market. Male immigrants have slightly lower employment rates but wage rates that are substantially below those of the native born. While in 1960 immigrants' wages were 6.5% above those of natives, by 2000 they were 19% lower. Those who have been in the U.S. for the shortest period have larger deficits: in 1960 those who had arrived recently earned 9% less than natives, but that gap expanded to 38% in 1990. Because they begin at lower earnings rates, immigrant cohorts arriving after 1970 are not expected to fully assimilate to higher native earnings rates. A one-dimensional model of skills, then, obviously predicts that more recent immigrants would be increasingly likely to have poor social outcomes as well.

Borjas (1987, 1994) provides a framework for understanding these changes in immigrants' labor market outcomes over time. He adapts a version of the Roy (1951) model to the problem of immigrant self-selection. Suppose that each country has a distribution of skill

that is transferable across country boundaries. Skill is translated into earnings in different ways in each country, and the distribution of earnings is more unequal in some countries than in others. Thus, low-skilled individuals may have very different earnings rates in one country versus another. Immigrants will choose to move to a country if their earnings—given their skill set—will be higher than in their country of origin.

Migration is then a function of wages in both the source and the host country, as well as migration costs, as in the following equation (Borjas 1994):

$$(1) \quad I = (\mu_1 - \mu_0 - \pi) + (\varepsilon_1 - \varepsilon_0)$$

where μ_1 is the mean log earnings (of immigrants) in the host country, μ_0 is the mean log earnings of these potential immigrants in the source country, π is the cost of migration divided by the wage in the source country (which Borjas calls the “time cost” of migration), and ε_1 and ε_0 are the deviations in earnings in the two countries. When $I > 0$, the individual migrates; when $I < 0$, the individual stays. Consider the case where the first term in equation (1) is zero. Then $I > 0$ if $(\varepsilon_1 - \varepsilon_0) > 0$. For individuals with high skill, moving to a country where the earnings distribution is more dispersed will generate a benefit from migration ($I > 0$). For individuals with low skill, moving to a country with a more compressed earnings distribution will generate a benefit.

This model gives insight into the change in immigrants’ earnings in the U.S. over the last four decades.² As the sending countries changed from predominantly European countries to predominantly Latin American and Asian countries the skills of immigrants coming to the U.S. also shifted. That’s because European countries tend to have earnings distributions that are more compressed than those in the United States, while Latin American countries tend to have distributions that are more dispersed. Thus, the model predicts a shift in the skill distribution of

those who immigrate to the U.S.³ Those from Sweden, for example, who find their “offer” from the U.S. earnings distribution higher than that from their own country would tend to be of high skill. On the other hand, immigrants from Mexico would tend to come from the lower end of the wage distribution, as those with high levels of skill would prefer the high wages from the relatively unequal wage distribution in Mexico.⁴ This model shows why the United States drew immigrants who were predominantly low-skilled in recent years.

Borjas analyzes the case where migration costs are constant in the population (and thus proportional to wages). However, suppose migration costs vary with the quality of an individual’s social network, such that migration costs are lower when one has strong connections to those who are succeeding in the new country (Chiquiar and Hanson 2005; Hanson forthcoming). In this case, the Roy model implies that those with productive social networks will require a lower wage premium to reach the migration threshold.

Consider also what happens when policies that affect immigrants change in the U.S., as occurred in the 1980s and 1990s, when criminal penalties in general rose, criminal penalties for noncitizens, in particular, rose dramatically, and immigrant eligibility for welfare declined. These changes would reduce the benefits to migration, as the index in (1) is now a function of these other attributes of the package associated with living in the U.S.:

² See Borjas (1985, 1995) empirical evidence on changes in immigrants’ earnings over time.

³ The model also suggests that as the U.S. wage distribution became more unequal, we should have seen a change in the skills of immigrants coming to the United States. Interestingly, the most recent cohort in 2000 appeared to have higher relative wages than the cohort in 1980 and 1990, a fact that can be attributed to an influx of engineers and computer scientists (Borjas and Friedberg 2004).

⁴ Chiquiar and Hanson (2005) examine the question of immigrant self-selection from Mexico using Mexican and U.S. Census data from 1990 and 2000. Contrary to what one might expect from the Borjas-Roy model, they find that Mexican immigrants in the U.S. tend to be selected from the middle to upper part of the observable skill distribution compared with Mexicans who remain in Mexico. Ibarra and Lubotsky (forthcoming), on the other hand, find that households that report having members who have emigrated to the U.S. tend to be selected from the lower part of the observable skill distribution. The difference between these likely arises because Ibarra and Lubotsky’s methodology should pick up those individuals who are undercounted in the U.S. Census—young, low-skilled, single men. We will

$$(2) \quad I = f(\mu_1, \mu_0, \pi, \varepsilon, \varepsilon_0, \delta)$$

where I is positively related to δ , the expected policy environment (defined as favorable to immigrants). A downward shift in δ would affect the migration decision, reinforcing other mechanisms that select immigrants with better social outcomes, including lower criminal propensities.

If immigrants with different social networks face different migration costs, then the process of migration may peel apart different dimensions of skill and selection. Because the costs of migration select who chooses to move, immigrants with poor wage outcomes may nonetheless have relatively good social outcomes. This model of selection implies that the correlation between wages and other outcomes at the country level may not be as strong as in individual-level data for the native born. As a brief illustration, we show the relationship in the U.S. between mean real wages and two other outcomes—average institutionalization and marriage rates⁵—for the 20 countries with the largest immigrant populations in the U.S.⁶ In Figures 1 and 2, the lines are country-level linear regression, weighted by the size of the immigrant population in the United States. We also plot the analogous information for the U.S. native born.

While the relationship between real wages and institutionalization is negative, as predicted by a one-dimensional model of skills, it is only marginally statistically significant ($t=$ 1.7). The relationship between real wages and marriage rates is quite flat and statistically insignificant. For both of these social outcomes, there is a great deal of variation across countries, and there are many countries whose people have very low wages in the United States, but also have lower rates of institutionalization and higher rates of marriage than expected given

address the undercount issue in the context of institutionalization below. Ibarra and Lubotsky also suggest that education among Mexican immigrants in the U.S. is likely to be misreported.

⁵ “Married” includes those whose spouses are present or absent and those who are separated.

⁶ The data are for men aged 18-40 in the 2000 Census. The data are described later in the text. The top immigrant countries were selected based on the number of men in this age group in the 1990 Census.

their wages. Immigrants may have poor real wage outcomes, but relatively good outcomes in social domains, suggesting selection along more than one dimension of “skill.”

III. Comparison of Immigrant and Native born Institutionalization Rates across Three Decades

A. Descriptive Statistics

We use data from the 5% Public Use Microsamples of the U.S. Census in 1980, 1990, and 2000 to examine institutionalization rates for men ages 18–40. Butcher and Piehl (1998b) shows that for this population, institutionalization closely approximates incarceration.⁷ Tables 1 and 2 present summary statistics for demographic characteristics and institutionalization rates, respectively, by native-born and foreign-born status.⁸

Before we proceed, a note about our definition of “immigrant.” In many contexts, the law distinguishes between the foreign born who intend to become permanent residents and those who are more transient. For example, permanent resident aliens typically have the right to work in the U.S., while those on a student visa do not. In these data, however, we do not have information on visa status, and “immigrant” is equivalent to “foreign born.”⁹ The important distinction in terms of the legal treatment of criminal aliens is between immigrants who have naturalized and those who have not, since the latter are subject to deportation. Later in the study we will pay close attention to citizenship status and how this has changed over time.

The immigrant population in the United States rose substantially between 1980 and 2000. Figure 3 shows the fraction immigrant inside and outside institutions for 18–40-year-old men for all three censuses. The fraction immigrant outside institutions nearly tripled over this period, rising from around 6% to 17% of the population. The fraction immigrant inside institutions, on

⁷ The 1980 Census identifies the incarcerated among the institutionalized. For men aged 18–40, at least 70% of the institutionalized are incarcerated. In addition, Butcher and Piehl (1998b) demonstrates that limiting the 1980 analysis to only those who are incarcerated does not substantively change the results.

⁸ Throughout the paper, we reported estimates using the person weight reported by the Census (there are no weights in 1980).

the other hand, actually fell between 1990 and 2000, despite the increase in the overall immigrant population.

As well-documented elsewhere, immigrant and native-born populations differ greatly in education and race/ethnicity distributions. Between 1980 and 2000, the fraction of the native-born population with some college education or more rose 25 percent, while among immigrants this group held steady. Similarly, the fraction of the immigrant population that is Hispanic rose substantially over this period; by 2000, nearly 60 percent of all immigrants describe themselves as Hispanic.

Because among the native born, those with low levels of education and Hispanics are more likely to be institutionalized than the average person, one might expect high institutionalization rates among immigrants. Figure 4 reports the institutionalization rates we expect to see among immigrants, based on the institutionalization propensities of the native born.¹⁰ This exercise reveals just how low the observed rates of institutionalization for immigrants are, considering their lower educational attainment and other characteristics.

Simply predicting institutionalization for immigrants based on their ages and native-born institutionalization propensities in 1980 gives an average predicted institutionalization rate of 0.013—up from their actual rate of 0.004, and equal to the native born. The effect for 1990 and 2000 is similar: predictions based on age-institutionalization relationship give immigrants institutionalization rates similar to those of natives. The second bar in Figure 4 for each year represents predictions based on age, education, race, and ethnicity. In 2000, this model predicts an institutionalization rate for immigrants of 0.073 -- double that of the native born, and 10 times

⁹ We omit those born in outlying areas of the United States and those born abroad to U.S. citizens.

¹⁰ These calculations come from running logits on a sample of the native born only and then using the estimated coefficients to predict institutionalization for immigrants.

the actual rate for immigrants. Clearly, immigrants have characteristics that in the native-born population are highly correlated with institutionalization.

Figure 5 shows the relationship between age and institutionalization for the native born and the most recent immigrants for each of the three Census years. For native-born Americans, the age-institutionalization curve peaks in the early twenties and gradually falls off (in a pattern well-known to criminologists). Institutionalization rates rose each decade for all age groups. In contrast, a dramatic feature of the graph is the relatively low rate for recent immigrants (those who arrived within the last five years in each Census). One possible explanation is that immigrants are not likely to be institutionalized before several years of exposure to the U.S. criminal justice system, and recent immigrants have not accumulated enough experience (to begin criminal enterprises, to be caught by law enforcement, or to have cases processed through the system). This explanation may also explain the relatively linear relationship between age and institutionalization among immigrants. If we set aside this “exposure time” hypothesis (which we explore in a later section), it is clear that recent immigrants have not seen increases in institutionalization comparable to those of natives, and, in fact, appear to have reduced institutionalization from 1990 to 2000.

The main result from these tables and figures is that immigrants have very low institutionalization rates, despite characteristics that in the native population are highly correlated with institutionalization. Furthermore, their institutionalization rates relative to the native born fell between 1990 and 2000. In the next section, we discuss the reliability of using these results to draw inferences about how institutionalization and criminal propensities have changed over time.

B. Enumeration Issues and Corroborating Evidence

Our institutionalization rates are measures of the number of individuals in institutions divided by the number of individuals overall. Mismeasurement of either the numerator or the denominator

would result in poor estimates of institutionalization rates. For the analyses we will conduct, we are particularly concerned with whether any mismeasurement differs systematically between the native-born and immigrant populations, and whether mismeasurement of the institutionalized population changes over time.

i. The Numerator

The institutionalized population is a subset of the “special populations” category in the U.S. Census. The Census has separate questionnaires and procedures for those housed in group quarters, including institutions. Many of those living in institutions, including prisons and jails, are deemed unable to fill out their own questionnaires, in which case Census enumerators fill out these forms over several weeks using administrative data. (See the data appendix for a more detailed description of Census enumeration procedures in special populations.) Thus, the Census records for the incarcerated population should be as good as the administrative data on which they are based. Administrators of prisons and jails have large incentives to accurately count their inmates, as keeping track of inmates is their foremost responsibility. And administrative data are verified during the processing of criminal cases. Thus, we would expect this population to be accurately counted relative to the rest of the population.

The next question is whether the counting of immigrants and the native born differs systematically, and whether any differences changed over time. While all the foreign born may not be properly identified, criminal justice administrators have incentives to identify them, particularly those who are not citizens. The incentives to identify noncitizen aliens increased over this period, as the federal government became more active in managing criminal aliens (Butcher and Piehl 2000; Legomsky 1999). Thus, if anything, we would expect any undercount of institutionalized immigrants to decrease relative to the native born over the time period. Such a change in mismeasurement would be expected to increase the measured institutionalization rate of immigrants relative to the native born.

We have no reason to believe that the mismeasurement in the institutionalized population changed over the period considered in this study. Differences in enumeration practice are more likely to have occurred for the noninstitutionalized than for the institutionalized. We turn to that population now.

ii. The Denominator

A second source of mismeasurement comes from the “undercount” of the overall population. The undercount arises when the Census does not enumerate some individuals. This is thought to be more likely in certain populations, particularly those that are more likely to be transient. The 2000 Census is widely reputed to have improved the undercount problem relative to the 1990 Census. We are not necessarily concerned if the undercount improved for all populations in the same way. However, if immigrants are more likely to be undercounted than the general population, and the undercount improved for them, we could find a spurious decrease in the institutionalization rates of immigrants between 1990 and 2000 relative to the native born.

The data appendix provides more details on the potential impact of changes in the undercount on our estimates of immigrant institutionalization rates. Our simulations show that our estimates are unlikely to be driven purely by changes in the undercount. Suppose that the institutionalization rate for immigrants in 1990 and 2000 was actually the same, but the 1990 Census differentially miscounted immigrants in the civilian population. For changes in the undercount to generate the estimates of institutionalization observed in the data (see Table 2), the undercount of immigrants relative to the native born would have to be 37:1 (that is, the Census missed 37 immigrants for every 1 missed native) in the 1990 Census.

iii. Incarceration and Its Relationship to Crime Commission

A second important question is whether we can use institutionalization rates to make inferences about immigrants’ commission of crime in the United States. If Census measures of institutionalization are poor measures of true incarceration rates, then these measures would not

tell us much about how immigration affects public safety. Alternatively, if immigrants are less likely than the native born to be caught for a given criminal act (for example, because they evade police by slipping across the border), immigrants will have lower institutionalization rates.

However, areas with large immigrant populations will have higher crime rates.¹¹

We can use information on Metropolitan Area (MA) crime rates and immigrant density to provide some evidence on the relationship between immigration and crime.¹² Figure 6 shows the change in MA crime rates graphed against the change in fraction immigrant for 1990 to 2000 for the 20 largest MAs.¹³ Areas with the largest increases in their fraction immigrant had the largest decreases in their crime rates. This confirms earlier results in Butcher and Piehl (1998a), which analyzed changes in metropolitan crime rates and immigration patterns between 1980 and 1990.¹⁴ This analysis at the MA level also corroborates the results from the individual-level Census data reported here: immigrants' criminality improved relative that of to the native born between 1990 and 2000.

IV. What Explains the Low Institutionalization Rates of Immigrants?

Having established that immigrants have low institutionalization rates relative to the native born in the U.S., we now turn to the potential causes. We begin this section with a baseline

¹¹ Some might argue that immigrants are less likely to report crimes. The nationally representative study of criminal victimization does not allow analysis of reporting rates by immigrant status. However, there is no consistent pattern between Hispanics and non-Hispanics regarding the propensity to report crimes to police (Catalano 2005).

¹² There is a sizeable literature on the labor market impacts of immigration that uses cross-MA variation to examine the issue. Many of the concerns around interpretation there also pertain for the impact of immigration on crime rates. See Card (2001) and Borjas (2003) for examples and discussion.

¹³ Data are from the Uniform Crime Reports (see table notes for details). Four of the 24 largest MAs had to be dropped because data were unavailable.

¹⁴ Butcher and Piehl (1998a) also used individual-level data on criminal activity, and found that immigrant youth are much less likely to report involvement in crime.

multivariate model. Then we discuss the competing hypotheses and our empirical strategy for testing them.

A. Baseline Specification

In Table 3 we present the results of logit models to explain institutionalization, with all three Censuses stacked together.¹⁵ Each specification includes a full set of age dummies, year dummies, and controls for the length of time in the U.S. for immigrants, as in equation (1) below:

$$(3) \quad I_i = \alpha + \beta_1 (c_{96-00}) + \beta_2 (c_{91-95}) + \beta_3 (c_{85-90}) + \dots + \beta_{10} (c_{40-50}) \\ + \delta_1 (1980) + \delta_2 (1990) + \gamma_1 (yearsUS) + \gamma_2 (yearsUS^2) + \varphi(X) + \varepsilon$$

The table reports the marginal effects evaluated at the sample means, not the logit coefficients.

The first model in Table 3 shows that immigrants have a 2.5-percentage-point lower probability of being institutionalized, controlling for year, age, and time in the U.S. As the length of time immigrants are in the U.S. grows, institutionalization rates fall—the opposite of assimilation. Column (1) shows that the immigrant effect varies by cohort, with more recent cohorts having lower institutionalization rates (relative to natives) than earlier cohorts. Note that some cohorts appeared in just one or two of the Census years, and that time in the U.S. is highly correlated with cohort, especially because of the age limitations on our sample.¹⁶ In later specifications, we free up the coefficients to vary by Census year. In those specifications, the meaning of a particular cohort is clearer. For now, it is enough to note these issues affect interpretation of the estimates.

¹⁵ To estimate this model, we randomly dropped 75% of the native born and adjusted the weights accordingly.

¹⁶ Age-at-arrival will also systematically vary across cohorts since the people who arrived in earlier cohorts would on average have had to arrive younger in order to still be under 40 and in our sample. Friedberg (1992) shows the importance of age-at-arrival for labor market outcomes. Here, we find substantively similar results if we split the sample between those who arrived as young children (under 12) and those who arrived at older ages.

Model (2) adds education, model (3) further adds race and ethnicity, and model (4) adds an indicator of U.S. citizenship to the earlier controls. All these variables are strongly statistically significant predictors of institutionalization. In particular, the addition of education attenuates all the measured effects in the earlier models, including the cohort indicators and year dummies. The main qualitative description of the cohort pattern persists, though with a more modest quantitative difference between immigrants and the native born.

Lubotsky (2000) points out that the Census may misclassify reentering immigrants as recent arrivals. Indeed, he finds that many of the studies focusing on immigrant wage assimilation overstate the secular decline in earnings across immigrant cohorts, because these mostly low-wage entrants are misclassified as “recent immigrants.” It is less clear how this misclassification may affect our results. If some in the “recent immigrant” category are these reentrants with very low skills, then we might expect, as a corollary to the wage studies, to find this group more likely to be incarcerated. This would work against our finding that recent immigrants have particularly low incarceration rates. On the other hand, the fact that they are reentrants may suggest a certain fluidity of movement that allows them to escape detection, and thus to have lower institutionalization rates for a given level of criminal activity. However, if this were the case, we would expect that an increase in the immigrant population would increase crime rates, and Figure 6 shows the opposite.

Another source of misclassification may come from efforts by the Census Bureau to fill in missing information. If the Bureau is more likely to allocate incorrect data to immigrants than to the native born, that might affect our results. There is evidence that immigrants are more likely to have allocated education data than are the native born, for example (Ibarraran and Lubotsky forthcoming). In our sample, immigrants are one and a half times as likely as natives to have any of our variables of interest allocated (22% versus 14%). Of particular concern is that 14% of our sample immigrants in 2000 have their year of arrival allocated (the rate is under 6%

in 1990 and 1980). We have reestimated our model after dropping all observations with any allocated variables and find qualitatively similar results, though the estimated cohort effects are less negative.¹⁷ From here forward we rely on the full sample for analysis.

Before turning to potential explanations for our estimates, we examine how sensitive the results are to the choice of where to evaluate the marginal effects. The non-linear nature of the logit means that the marginal effects may differ, depending on where along the function they are calculated. Appendix Table 2 presents estimates analogous to those in Table 3 evaluated for a 25-year-old Hispanic with a high school degree. The estimates are qualitatively similar to those in Table 3—namely, that the more recent cohorts have lower institutionalization rates than earlier cohorts, and that there is no evidence that immigrants are assimilating toward the higher institutionalization rates of the native born with time in the country. However, the marginal effects are nearly twice as large, suggesting even lower institutionalization rates for immigrants. Note that when we evaluate at the mean of the sample, we are comparing immigrants to the average native, who has an ever-higher education level. When we evaluate the results for a 25-year-old Hispanic with a high school degree, we are comparing institutionalization rates for immigrants and natives with those characteristics. We continue with the more conservative estimate, evaluating the logits at the sample mean, but we note that our comparisons are much larger when we evaluate at the average characteristics for immigrants.

B. Three Hypotheses

We hypothesize three types of causes of the institutionalization patterns documented above: deportation, deterrence, and selection. Changes in the policies toward criminals during the 1980s and 1990s, toward criminal noncitizens in particular in the 1990s, and toward those who were likely to be less successful in the labor market in the 1980s and 1990s may have operated

¹⁷ Results available upon request.

through all three mechanisms to dramatically reduce incarceration rates among immigrants by 2000. Penalties for criminal activity tightened in the 1980s and 1990s (Travis 2005). The Violent Crime Act of 1994 and the Anti-Terrorism and Effective Death Penalty Act of 1996 expanded the list of crimes for which noncitizen immigrants can be deported (Legomsky 1999), and applied this change retroactively to noncitizen immigrants. This greater emphasis on deportation may have mechanically decreased the probability that a criminal alien would be incarcerated. Because noncitizens must complete their sentences before being deported, the higher probability of deportation acts as an additional punishment. The increase in punishment may have deterred some immigrants from committing crimes in the U.S.

The Personal Responsibility and Work Reconciliation Act of 1996 (welfare reform) also made it more difficult for noncitizens to receive welfare benefits.¹⁸ This, combined with the added punishment for criminal noncitizens, may have given immigrants with poor social outcomes an incentive to naturalize to obtain the protection citizenship provides.¹⁹ Anecdotes at the time suggested that immigrants were lining up to apply for citizenship as the atmosphere changed in the mid-1990s.

Alternatively, these policy changes, coupled with economic conditions attractive to high-skilled immigrants, may have undercut the attraction of the U.S. to potential immigrants likely to interact with the criminal justice or welfare systems. Thus, these policy changes may have had a deterrent effect on the intensive margin, changing the behavior of immigrants already in the U.S.,

¹⁸ As originally passed, the welfare reform bill barred non-naturalized immigrants from receipt of most forms of welfare; as revised, only immigrants who arrived after the law are subject to the ban. States had the option to use state funds to extend benefits to immigrants left out of the federal statute. Many, especially those with large immigrant populations chose to do so.

¹⁹ In general, the better-off immigrants become citizens. Fix et al. (2003) reports that immigrants with the least English language proficiency, lower education, and lower incomes are less likely to become naturalized citizens.

and on the extensive margin, changing the pool of self-selected immigrants to a less criminally active one.

Changes in policies toward criminals, immigrants, and criminal immigrants during the 1980s and 1990s may have operated through all three of these mechanisms: deportation, deterrence, and selection. Although the mechanisms are not mutually exclusive, they generate testable implications that allow distinctions among them.

V. Results

To test these hypotheses, we need estimates of institutionalization of cohorts of immigrants over time, allowing estimated cohort effects to vary by period. For the remainder of the analyses, therefore, we run separate logits for each Census year, as shown in equations (4) and (5) for 1990 and 2000, respectively:

$$(4) \quad I_i = a^{90} + b_3^{90} (c_{85-90})_i + \dots + b_9^{90} (c_{50-59})_i + \varphi^{90} (X)_i + \varepsilon_i^{90}$$

$$(5) \quad I_i = a^{00} + b_1^{00} (c_{96-00})_i + b_2^{00} (c_{91-95})_i + b_3^{00} (c_{85-90})_i + \dots + b_8^{00} (c_{60-64})_i + \varphi^{00} (X)_i + \varepsilon_i^{00}$$

Because each equation is limited to a single cross section, we can no longer identify the effect of time in the country separately from cohort of entry. Another difference from the stacked regressions shown earlier is that all coefficients vary freely by year. Appendix Table 3 reports marginal effects of the year-by-year results for 1990 and 2000 for easy comparison to the stacked regressions reported in Table 3. The dominant features from the earlier approach—that immigrants have lower institutionalization rates, and more recent cohorts have lower rates than earlier cohorts—carry through to the new modeling approach.

The top panel of Table 4 reports estimates of how the institutionalization of immigrant cohorts changed from one Census to the next. For example, column (1) shows the difference between the marginal effect of b_3^{00} and b_3^{90} , with a negative number indicating that the (negative)

gap between that immigrant cohort and the native born widened. That is, the first entry in the table means that the gap between the institutionalization of the cohort arriving between 1985 and 1990 and natives grew more negative by 0.0074 between 1990 and 2000. This represents an enormous shift compared with the base institutionalization rate among natives in 1990 (0.022), or among that immigrant cohort in 1990 (0.007).

The rest of the columns in the table contain the results of adding further control variables. As in the earlier table, column (1) controls only for age, column (2) adds education controls, column (3) adds race and ethnicity, and column (4) adds citizenship to all the previous controls. Between 1990 and 2000, all the cohorts decreased their relative institutionalization rates, regardless of which controls are included. For example, these estimates suggest a 0.36–0.86-percentage-point decline in relative institutionalization for the 1985–1990 and 1980–1984 cohorts between 1990 and 2000. Between 1980 and 1990, the 1975–1979 and 1970–1974 cohorts decreased their relative institutionalization rates once education is included in the controls. Immigrants appear to have improved relative to natives with time in the country, and this improvement was greater from 1990 to 2000 than it was from 1980 to 1990.

These results are consistent with a mechanical decrease in the institutionalized population of immigrants due to deportation, which would have had a bigger effect from 1990 to 2000 than from 1980 to 1990. However, the results are also consistent with a deterrent effect due to increased penalties for criminal activity, to which immigrants are more responsive than the average native. We examine each of these explanations for the cohort pattern in our results in turn.

A. Deportation

First, we consider the mechanical effect of deportation on incarceration. The number of immigrants deported (both voluntary departures and formal removals) rose over the three decades we examine. From 1971 to 1980, about 7.5 million immigrants were expelled (Yearbook

of Immigration Statistics 2002); from 1981 to 1990, about 10.2 million immigrants were expelled; and from 1991 to 2000, about 14.5 million immigrants were expelled (Immigration Statistics Reports 2002).²⁰ Among those deported, not simply excluded, the most common administrative reasons given during the 1990s were “attempted entry without proper documents” (35%) and “criminal activity” (31%).

This increase in deportation might be expected to decrease immigrants’ relative institutionalization rates.²¹ However, the countervailing effects of this policy may increase the probability that an immigrant will be institutionalized. First, the Anti-Terrorism and Effective Death Penalty Act increased the list of criminal acts for which noncitizens must be detained. Legomsky (1999) reports that “mandatory detention now applies to almost all noncitizens who are inadmissible or deportable on crime-related grounds—not just to those convicted of aggravated felonies (p. 532).” Second, the speed of removal of deportable aliens may critically affect immigrants’ institutionalization rates. Shuck and Williams (1999) estimate that the Immigration and Naturalization Service (INS) had removed “fewer than twenty percent of the nearly 300,000 criminal aliens estimated to be already under law enforcement supervision.” Butcher and Piehl (2000) showed that immigrants under a deportation order spent more time incarcerated for a given sentence than similar natives—perhaps because backlogs in the system meant that immigrants awaiting deportation waited in prison until the INS could clear their cases. These effects would tend to inflate immigrants’ relative incarceration rates.

²⁰ The vast majority of these expulsions are voluntary departures. For example, from 1991 to 2000, only 939,749 of the expulsions were formal removals.

²¹ Note that because immigrants serve their sentences and then are deported, deportation should not decrease incarceration probabilities for a given offence, but only for subsequent offences, since immigrants would no longer be in the country to commit these offences. However, even that may be in doubt. A recent *New York Times* investigation reported that city sanctuary policies, such as the one in Los Angeles that prohibits police from inquiring about immigration status unless there is a formal charge of a crime, mean that those who have been deported can frequently return to the U.S. and resume their lives (LeDuff 2005).

We would like to know the extent to which the added emphasis on deportation in the 1990s influenced estimated institutionalization rates of immigrants. Secondary sources on the number of immigrants deported are of little use because although deportation increased, so did the population of immigrants. Thus, we take a different approach, beginning with the fact that naturalized citizens are not subject to deportation. If growing deportation of criminal aliens represents an important risk to immigrants, we would expect to see differences in the institutionalization of citizen and noncitizen immigrants in the 2000 Census, as the mechanical impact of increasing deportation must occur for all the noncitizens. We use the sample of citizens (native born and naturalized) to examine how institutionalization patterns changed over time for immigrants who are not subject to the greater threat of deportation from legislation enacted in the 1990s.

The second panel in Table 4 shows the results of this exercise. Here we see that the decline in relative institutionalization rates is at least as large for immigrants who cannot be deported as for immigrants overall. Thus, the decline in the relative institutionalization rates in the top panel of Table 4 cannot all be driven by deportation.

This test is predicated on the assumption that a great shift in naturalization over this period did not occur. If immigrants who were criminally active became more likely to naturalize, then this could inflate the relative incarceration rates of citizen immigrants. Because the relative incarceration rates for naturalized citizens fell by more than for immigrants as a whole, this seems unlikely. However, we examined rates of naturalization directly to see whether the policy changes in the 1990s that would seem to give immigrants an incentive to naturalize had a detectable effect. Table 1 shows the fraction of immigrants who are citizens in each cohort. Not surprisingly, the fraction citizen is higher in earlier cohorts. However, the probability of naturalizing for a cohort with a given number of years in the U.S. did not rise between 1990 and 2000. For example, 7% of immigrants who had been in the country for less than 5 years in 1990

were citizens; in 2000, 4% of recent arrivals were citizens. This pattern holds up when we run logit models that control for changes in immigrant characteristics as well.²²

B. Deterrence

The 1980s and 1990s saw the adoption of many policies that increased criminal penalties and thus had the potential to deter criminal activity of both immigrants and the native born. If migration selects individuals who are particularly responsive to incentives, we might see more of a deterrent effect for this group than for the native born, on average. If immigrants are responding to this general deterrence, we would expect to see naturalized citizens reducing their incarceration probabilities as well, not simply noncitizens who face the increased threat of deportation. As we saw in the second panel of Table 4, naturalized citizens reduced their incarceration rates relative to the native born. The effect is larger than for immigrants overall, suggesting immigrants are responding to a deterrent effect from the general increase in punishment, rather than to the specific threat of deportation.

If migration selects individuals who are particularly responsive to incentives, we would further expect that, just like immigrants, native-born migrants -- defined as those who live outside their state of birth²³ -- would have reduced their incarceration probabilities over time as well.²⁴ This is, in fact, what happened. In 1980, native-born movers were 0.04 percent less

²² Available upon request.

²³ This measure is somewhat problematic since one reason a person may live outside his state of birth is if he is sent to a federal prison in another state. This would tend to increase the institutionalization rates of native born movers. This bias is likely to be small, however, as federal prisoners are a small fraction of all prisoners.

²⁴ Butcher (1994) compared labor market outcomes for immigrant and native-born blacks, finding that immigrant blacks had better labor market outcomes than the native-born. However, when the native-born who had moved from their state of birth were used as the comparison group, outcomes were very similar, suggesting that movers, whether native-born or immigrant, are similar.

likely than nonmovers to be institutionalized. By 1990, this difference had risen to 0.2%, and by 2000 to 0.3%.²⁵

Native-born movers and immigrants appear to have responded to the harsher criminal penalties in the 1980s and 1990s. The bottom panel of Table 4 compares immigrants to native-born movers, and shows that the decline in immigrants' relative institutionalization rates is much smaller here. This suggests a general deterrence effect, with both immigrants and native-born movers responding to the incentives of new policies toward criminals in similar ways.

C. Changes in Immigrant Selection

Changes in the legal, economic, and social environment during the 1980s and 1990s may have affected the type of immigrant who self-selects to come to the United States.²⁶ As the Roy model outlined above indicated, these changes would be expected to improve the social outcomes of immigrants to the U.S., even if these immigrants had poor labor market outcomes. To see whether selection has changed over time, we compare the cohorts who arrived just before the 2000 Census to the outcomes of cohorts who had been in the country a comparable amount of time in previous Censuses. With this strategy, we will see whether the most recent immigrants in 2000 appear more positively selected (conditional on various sets of controls) relative to natives than earlier cohorts, holding constant the time exposed to the U.S. criminal justice system.

The laws increasing penalties for criminal aliens passed in 1994 and 1996, so we would expect the biggest impact for the cohort arriving between 1996 and 2000. Table 5 shows the change in relative institutionalization rates, holding time in the U.S. constant, for the two most recent arrival cohorts. The numbers in the top panel of Table 5 are calculated by subtracting the

²⁵ These results come from logits controlling for a full set of age dummies, available on request.

²⁶ Welfare reform and changes in the labor market in the 1990s may also have contributed to the changes in the type of immigrant who came to the U.S. in the 1990s.

marginal effects in equations 4 and 5, $b_1^{00} - b_3^{90}$, for example.²⁷ This comparison limits the bias that may arise from selective return migration—the most recently arrived cohorts will not have lost as many return migrants as those who have been in the U.S. longer. The results show that the most recently arrived cohort in 2000 was 1.1 percentage points less likely to be institutionalized than the most recently arrived cohort was in 1990 (column 1, top panel). The fact that the results are similar when we limit the sample to citizens again suggests that increased deportation is not mechanically driving this result.

Although the evidence in Table 5 suggests that the type of immigrant who came to the U.S. was changing over these decades toward those who were less crime-prone, there is less evidence for a specific effect of the legislation passed in the mid 1990s. For example, the decrease in the relative incarceration rate is as big for the cohort arriving 5–10 years ago as for those with fewer than 5 years in the U.S.²⁸

The comparison to native-born movers corroborates the evidence that immigrants who came later are more positively selected along the dimension of crime commission. Once again we see that the magnitudes of the changes are smaller, because native-born movers have lower incarceration rates than the rest of the native born. Again, there does not appear to be a particularly large effect for the cohort of immigrants whose decision should have been most affected by the harsher environment toward noncitizens in the U.S. beginning in the mid-1990s.

VI. Conclusion

Using the 1980, 1990, and 2000 Censuses, we show that 18-40 year-old male immigrants have lower institutionalization rates than the native born in each year. The gap in these

²⁷ See Appendix Table 3 for the marginal effects from which these numbers are calculated.

²⁸ The change in the gap is similar for those arriving 10 to 15 years before each Census. The comparison is not shown here, but may be calculated from Appendix Table 3.

institutionalization rates widens over the decades, and by 2000 immigrants have institutionalization rates that are one-fifth those of the native born.

Legislation passed in the 1980s and 1990s raised penalties for criminal activity; and legislation passed in the 1990s increased penalties for criminal noncitizens in particular. We present a version of the Roy model that shows that these types of policy changes may lead individuals to immigrate to the United States who are positively selected along dimensions of “skill” associated with good social outcomes, even if they have poor labor market outcomes.

The fact that immigrants have lower relative incarceration rates in 2000 than in earlier years may be due to several factors. The legislation passed in the 1990s may have increased the probability that immigrants are deported for their crimes, thus mechanically lowering their incarceration rates. Alternatively, immigrants’ criminal behavior may have changed in response to the general deterrence from increased criminal penalties. Or, the type of person who chose to immigrate to the U.S. may have changed in response to these general increases in penalties for criminal activity, or in response to the specific penalties (deportation after serving one’s sentence) that apply to noncitizens. We exploit the fact that some of these effects will pertain for some groups and not others to distinguish among these explanations.

We find that deportation is not driving the decline in relative institutionalization rates of immigrants, because naturalized citizens, who are not subject to deportation, also reduced their institutionalization rates. There is evidence that the process of migration selects individuals who are more responsive to deterrent effects than the average native, since the native born who live outside their state of birth also reduced their relative institutionalization rates over time. Immigrants who were already in the country reduced their relative institutionalization probability over the decades; and the newly arrived immigrants in the 1980s and 1990s seem to be particularly unlikely to be involved in criminal activity, consistent with increasingly positive

selection along this dimension. However, we find little evidence of a dramatic change in the 1996–2000 cohort that should have been most affected by increased penalties for criminal aliens.

These results suggest there are multiple dimensions along which immigrant selection occurs. A model of selection with multiple dimensions can explain why we observe some groups outperforming natives in the criminal justice realm while underperforming in the labor market. Interestingly, our results suggest immigrants are more responsive to incentives than the average native, a fact, which if it is found to hold in other areas as well, may be important for policy as well as for economic models.

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Figure 1: Institutionalization and Real Hourly Wages, 2000, by Country

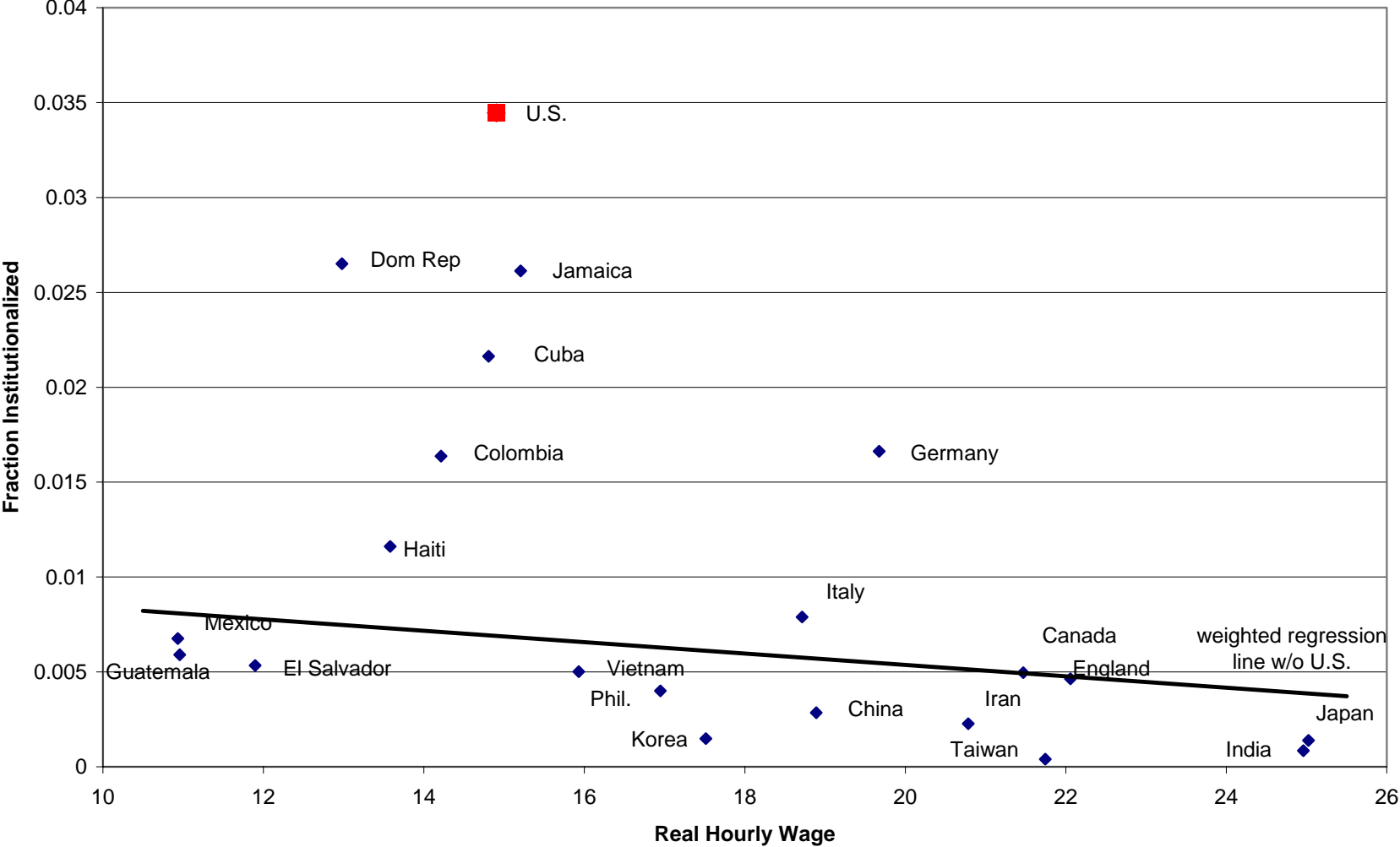


Figure 2: Fraction Married and Real Hourly Wages, 2000, by Country

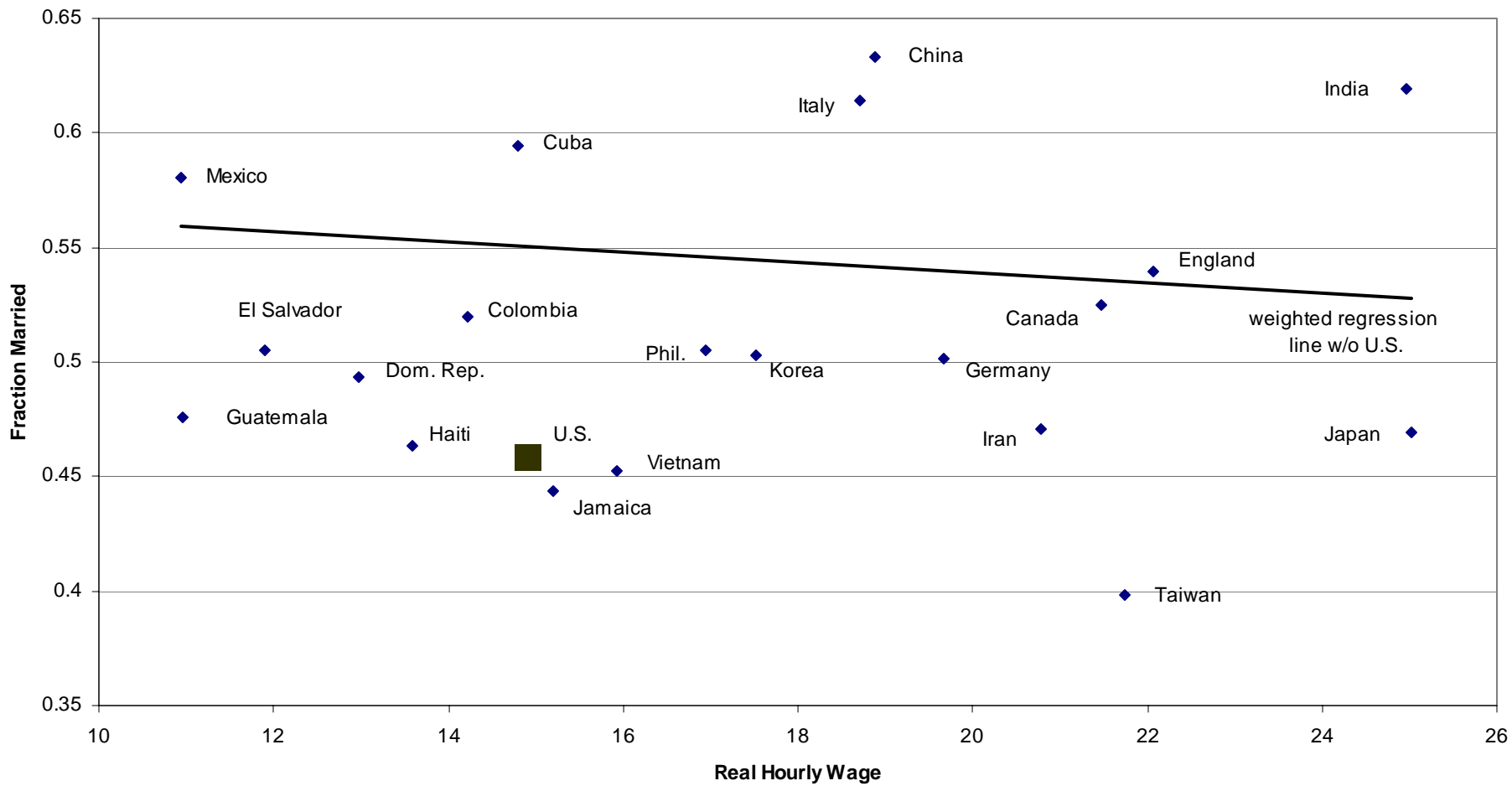


Figure 3. Fraction Immigrant Inside and Outside Institutions

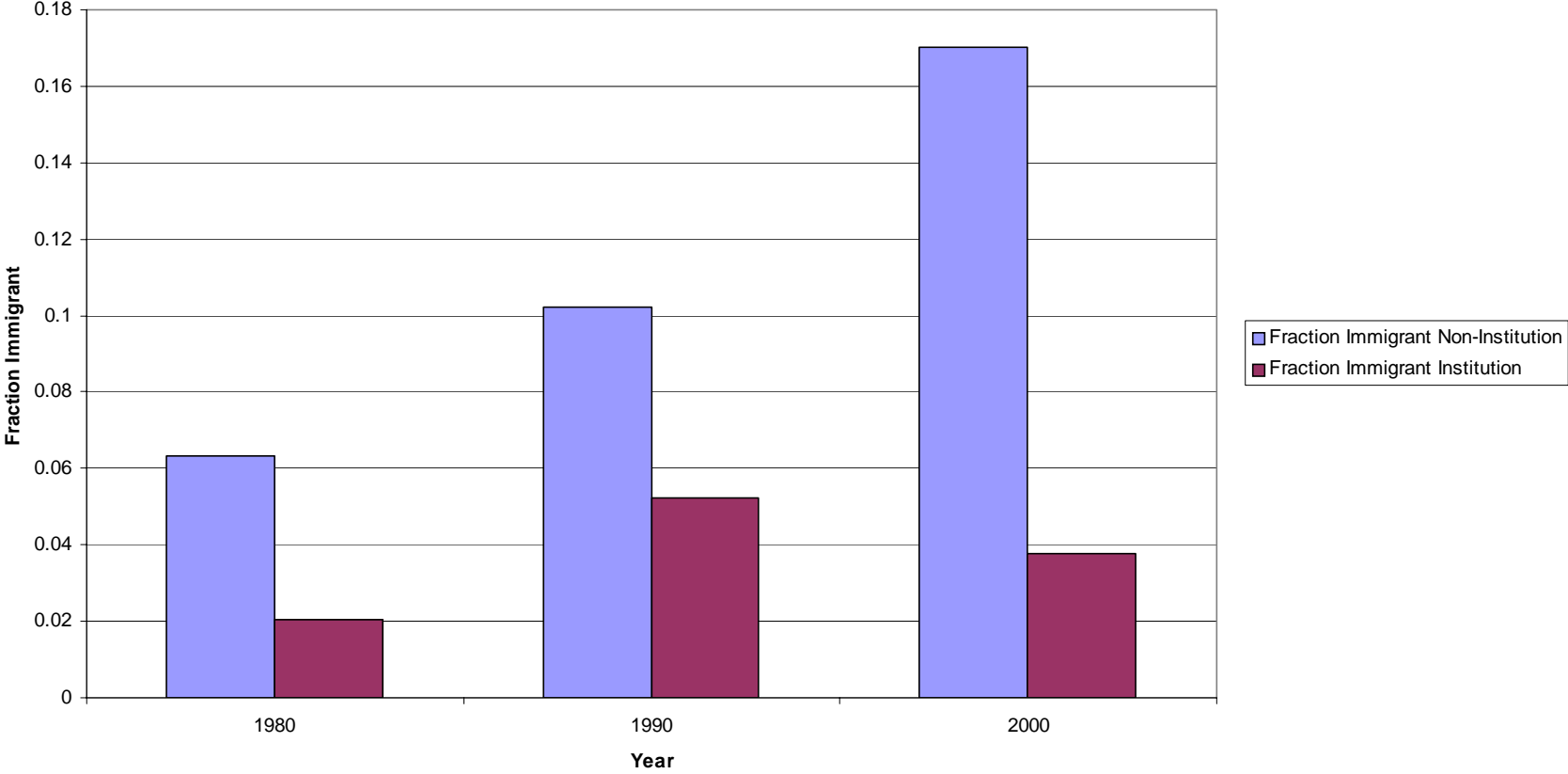
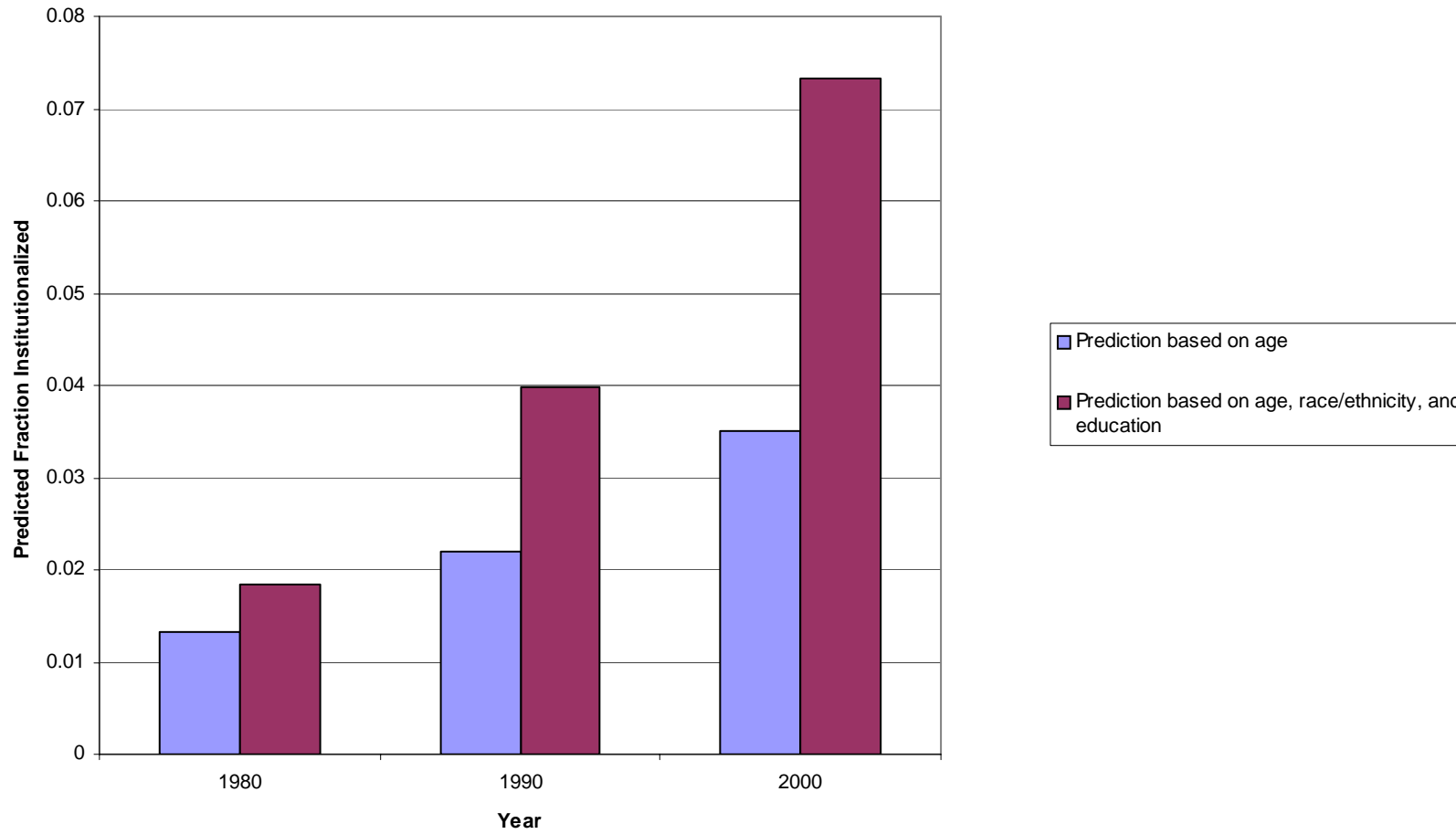


Figure 4. Predicted Institutionalization Rates For Immigrants



Notes: These numbers are calculated from logit regressions using the 5% Public Use Microdata Samples of the U.S. Census. Predictions are created by running the logits for natives alone and predicting immigrant institutionalization rates using these coefficients and the characteristics of immigrants. Controls include a full set of age dummies and dichotomous variables for black, Asian, other race, Hispanic origin, high school dropout, high school degree, and some college.

Figure 5. Institutionalization by Age
Native-born and Most Recent Immigrant Cohort
1980, 1990, and 2000 Census

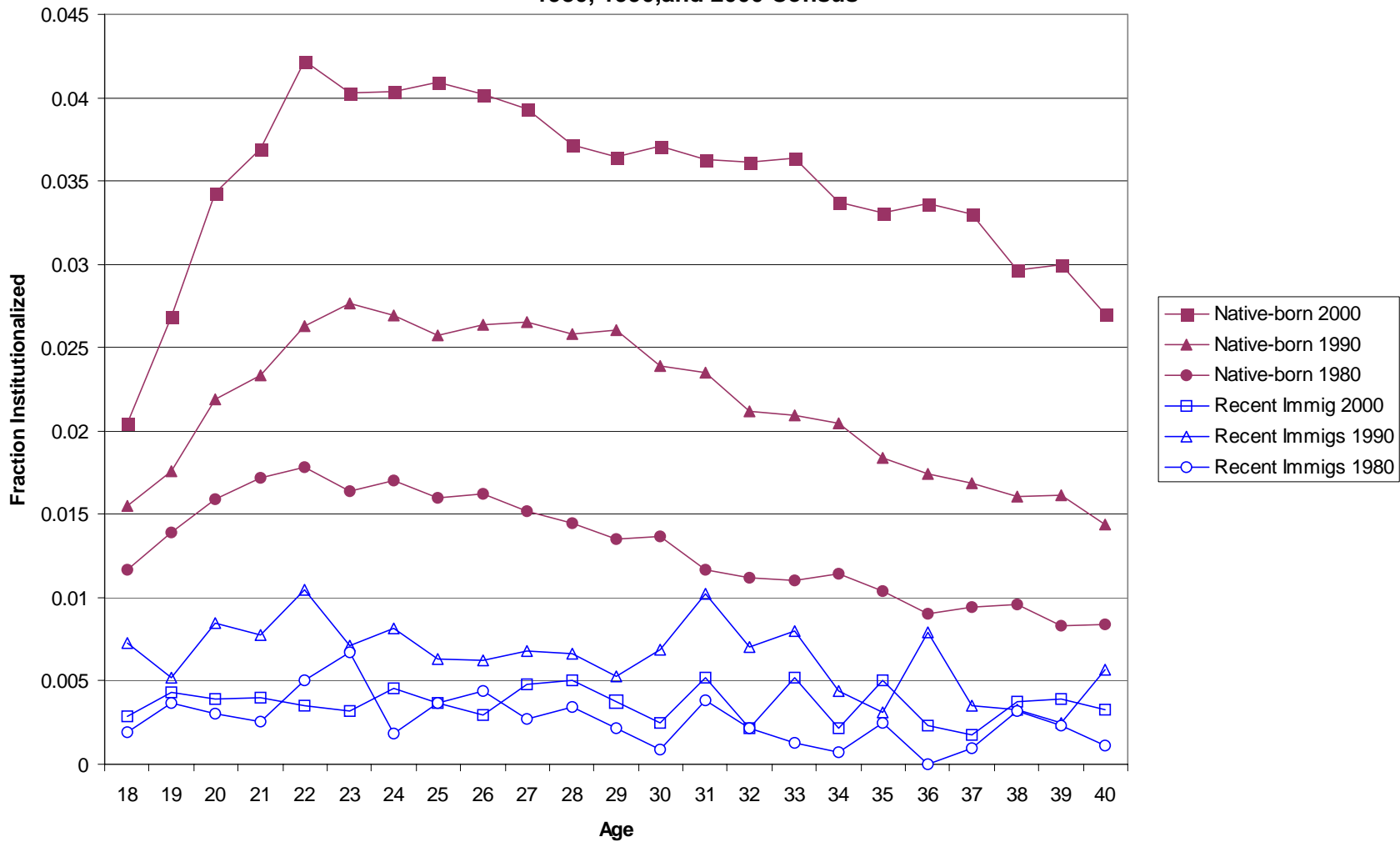
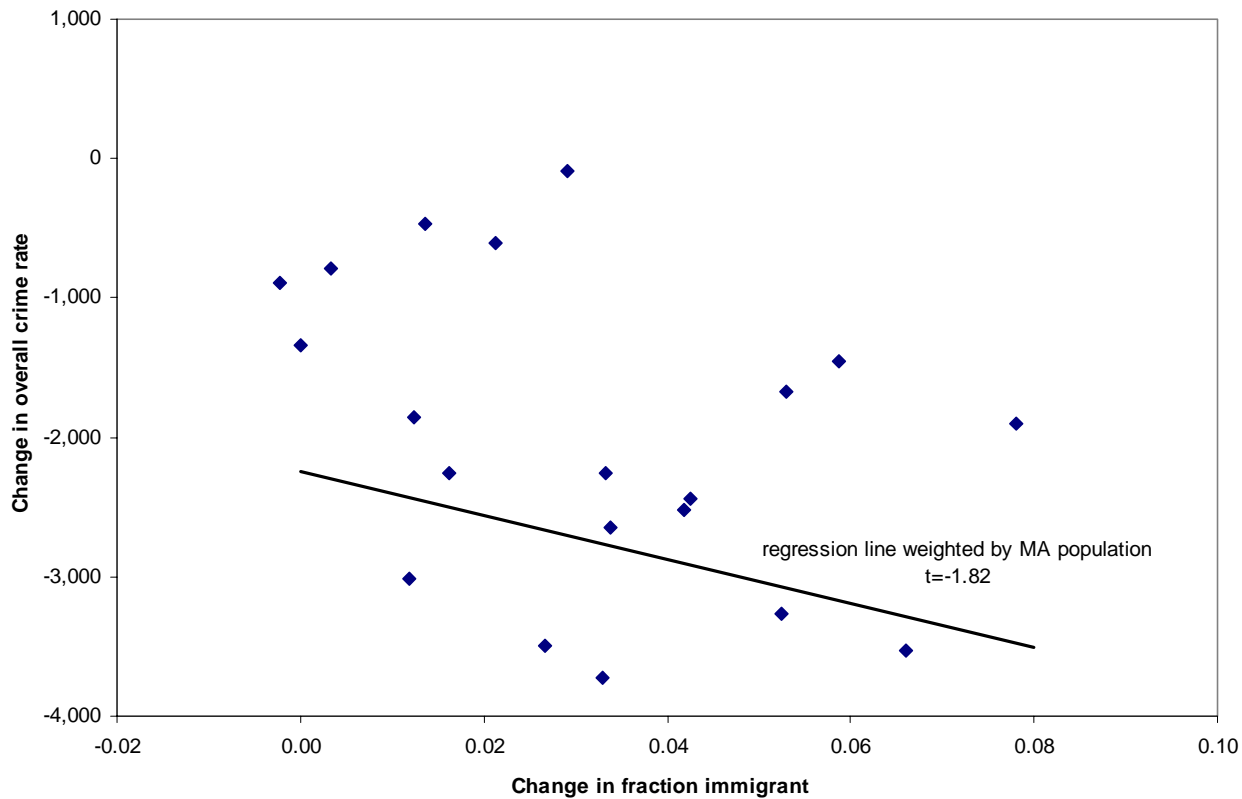


Figure 6. Changes in Metropolitan Area (MA) Crime Rates
by Changes in Fraction Immigrant
1990 to 2000



**Table 1. Summary Statistics:
Characteristics of Immigrants and Natives in 1980, 1990 and 2000**
(Standard Errors in Parentheses)

<i>Characteristic</i>	<i>1980</i>		<i>1990</i>		<i>2000</i>	
	<i>Native born</i>	<i>Immigrants</i>	<i>Native born</i>	<i>Immigrants</i>	<i>Native born</i>	<i>Immigrants</i>
Age	27.834 (0.0047)	28.793 (0.0178)	29.085 (0.0046)	29.280 (0.0137)	29.321 (0.0050)	29.671 (0.0107)
< H.S.Degree	0.1925 (0.0003)	0.3449 (0.0013)	0.1268 (0.0002)	0.3258 (0.0010)	0.1241 (0.0002)	0.3396 (0.0008)
H.S.Degree	0.3909 (0.0004)	0.2365 (0.0012)	0.3545 (0.0003)	0.2470 (0.0009)	0.3506 (0.0003)	0.2693 (0.0007)
Some College	0.2285 (0.0003)	0.2029 (0.0011)	0.3222 (0.0003)	0.2228 (0.0009)	0.3256 (0.0003)	0.1889 (0.0007)
College Degree	0.1880 (0.0003)	0.2157 (0.0011)	0.1964 (0.0003)	0.2043 (0.0009)	0.1997 (0.0003)	0.2023 (0.0007)
Black	0.1143 (0.0002)	0.0682 (0.0007)	0.1243 (0.0002)	0.0807 (0.0006)	0.1401 (0.0003)	0.0719 (0.0004)
White Non-Hispanic	0.8330 (0.0003)	0.3421 (0.0013)	0.8084 (0.0003)	0.1994 (0.0009)	0.7631 (0.0003)	0.1547 (0.0006)
Asian or Pacific	0.0060 (0.0001)	0.1957 (0.0011)	0.0082 (0.0001)	0.2347 (0.0009)	0.0169 (0.0001)	0.2198 (0.0007)
Other Race	0.0024 (0.0000)	0.0270 (0.0005)	0.0005 (0.0000)	0.0034 (0.0001)	0.0405 (0.0001)	0.3400 (0.0008)
Hispanic	0.0405 (0.0001)	0.3975 (0.0014)	0.0519 (0.0002)	0.4977 (0.0011)	0.0784 (0.0002)	0.5671 (0.0008)
U.S. Citizen	1	0.3306 (0.0013)	1	0.2903 (0.0010)	1	0.2667 (0.0007)
Citizen: 96-00						0.0445 (0.0007)
Citizen: 91-95						0.1392 (0.0012)
Citizen: 85-90				0.0674 (0.0010)		0.2991 (0.0015)
Citizen: 80-84				0.2388 (0.0018)		0.4863 (0.0022)
Citizen: 75-79		0.0730 (0.0012)		0.3973 (0.0025)		0.5874 (0.0031)
Citizen: 70-74		0.2604 (0.0025)		0.4771 (0.0032)		0.6671 (0.0043)
Citizen: 65-69		0.4345 (0.0034)		0.5839 (0.0044)		0.7292 (0.0057)
Citizen: 60-64		0.5875 (0.0041)		0.6809 (0.0054)		0.7667 (0.0100)
Citizen: 50-59		0.7890 (0.0034)		0.7699 (0.0057)		
Citizen: 40-49		0.8965 (0.0057)				
No. Obs	1,900,112	127,392	1,984,069	209,878	1,875,961	352,534

Notes: These data are from the 1980, 1990, and 2000 Integrated Public Use Microdata Series (IPUMS) of the U.S. Census. The data include men aged 18–40 inclusive. Those born in U.S. outlying areas, born abroad of American parents, or born at sea are excluded from the sample. All means are weighted to reflect sampling.

Table 2. Fraction of the Population Institutionalized in 1980, 1990 and 2000
(Standard Errors in Parentheses; Sample Size in Square Brackets)

<i>Group</i>	<i>1980</i>		<i>1990</i>		<i>2000</i>	
	All					
Full Sample	0.0129 (0.00008) [2,027,504]		0.0206 (0.00010) [2,193,947]		0.0299 (0.00011) [2,228,495]	
	By Immigrant Status					
	<i>1980</i>		<i>1990</i>		<i>2000</i>	
	<i>Native born</i>	<i>Immigrants</i>	<i>Native born</i>	<i>Immigrants</i>	<i>Native born</i>	<i>Immigrants</i>
All	0.0135 (0.00008) [1,900,111]	0.0042 (0.00018) [127,392]	0.0217 (0.00010) [1,984,069]	0.0107 (0.00022) [209,878]	0.0345 (0.00013) [1,875,961]	0.0068 (0.00014) [352,534]
< H.S Degree	0.0389 (0.00032)	0.0076 (0.0041)	0.0673 (0.00049)	0.0167 (0.0048)	0.1104 (0.00064)	0.0101 (0.0028)
H.S. Degree	0.0101 (0.00011)	0.0041 (0.00037)	0.0229 (0.00018)	0.0119 (0.00048)	0.0412 (0.00024)	0.0082 (0.00024)
Some College	0.0069 (0.00013)	0.0024 (0.00030)	0.0143 (0.00015)	0.0082 (0.00042)	0.0171 (0.00017)	0.0047 (0.00027)
Black	0.0445 (0.0004)	0.0078 (0.00095)	0.0811 (0.00060)	0.0289 (0.00142)	0.1132 (0.00065)	0.0179 (0.00087)
White Non-Hispanic	0.0088 (0.00007)	0.0040 (0.00030)	0.0116 (0.00008)	0.0052 (0.00035)	0.0170 (0.00011)	0.0039 (0.00027)
Asian or Pacific	0.0086 (0.00087)	0.0011 (0.00021)	0.0130 (0.00090)	0.0024 (0.00022)	0.0253 (0.00090)	0.0037 (0.00022)
Hispanic	0.0210 (0.00052)	0.0054 (0.00032)	0.0396 (0.00062)	0.0152 (0.00037)	0.0659 (0.00066)	0.0079 (0.00020)
U.S. Citizen		0.0055 (0.00036)		0.0097 (0.00040)		0.0051 (0.00023)
<i>Immigrant Cohorts</i>						
1996-2000						0.0037 (0.00020)
1991-1995						0.0050 (0.00025)
1985-1990				0.0068 (0.00032)		0.0072 (0.00028)
1980-1984				0.0117 (0.00046)		0.0106 (0.00046)
1975-1979		0.0029 (0.00025)		0.0117 (0.00055)		0.0096 (0.00061)
1970-1974		0.0036 (0.00034)		0.0128 (0.00072)		0.0141 (0.00108)
1965-1969		0.0039 (0.00043)		0.0172 (0.00115)		0.0098 (0.00127)
1960-1964		0.0067 (0.00068)		0.0163 (0.00147)		0.0183 (0.00309)
1950-1959		0.0065 (0.00068)		0.0090 (0.00128)		
1940-1949		0.0089 (0.0018)				

Notes: These data are from the 1980, 1990, and 2000 Integrated Public Use Microdata Series (IPUMS) of the U.S. Census. The data include men aged 18–40 inclusive. All means are weighted to reflect sampling.

Table 3. Marginal Effects for Logit Estimates of Institutionalization
(Evaluated at Sample Mean, Robust Standard Errors in Parentheses)

	(0)	(1)	(2)	(3)	(4)
Immigrant	-0.0251 (0.0005)				
1996-2000		-0.0208 (0.0003)	-0.0137 (0.0002)	-0.0116 (0.0002)	-0.0117 (0.0002)
1991-1995		-0.0192 (0.0004)	-0.0130 (0.0002)	-0.0111 (0.0002)	-0.0113 (0.0002)
1985-1990		-0.0162 (0.0006)	-0.0118 (0.0003)	-0.0105 (0.0002)	-0.0108 (0.0002)
1980-1984		-0.0094 (0.0011)	-0.0083 (0.0005)	-0.0083 (0.0004)	-0.0089 (0.0004)
1975-1979		-0.0082 (0.0013)	-0.0073 (0.0007)	-0.0075 (0.0005)	-0.0082 (0.0004)
1970-1974		-0.0012 (0.0022)	-0.0031 (0.0011)	-0.0052 (0.0007)	-0.0062 (0.0007)
1965-1969		0.0047 (0.0003)	0.0022 (0.0019)	-0.0018 (0.0012)	-0.0032 (0.0011)
1960-1964		0.0132 (0.0044)	0.0102 (0.0031)	0.0040 (0.0022)	0.0019 (0.0020)
1950-1959		0.0054 (0.0050)	0.0055 (0.0036)	0.0028 (0.0028)	0.0009 (0.0025)
1940-1950		0.0229 (0.0098)	0.0151 (0.0065)	0.0089 (0.0048)	0.0067 (0.0044)
Less than H.S.			0.2227 (0.0068)	0.1428 (0.0050)	0.1424 (0.0050)
H.S. Degree			0.0594 (0.0019)	0.0398 (0.0014)	0.0397 (0.0014)
Some College			0.0354 (0.0016)	0.0246 (0.0012)	0.0246 (0.0012)
Black				0.0397 (0.0008)	0.0396 (0.0008)
American Indian				0.0088 (0.0011)	0.0088 (0.0011)
Asian or Pacific				0.0065 (0.0011)	0.0066 (0.0011)
Other Race				-0.0008 (0.0005)	-0.0008 (0.0005)
Hispanic				0.0135 (0.0007)	0.0134 (0.0007)
U.S. Citizen					-0.0023 (0.0006)
Years in US	-0.0016 (0.0002)	-0.0004 (0.0002)	-0.0007 (0.0001)	-0.0005 (0.0001)	-0.0005 (0.0001)
Years in US ²	-0.00002 (0.00001)	-0.000006 (0.00001)	-0.000009 (0.00001)	0.000008 (0.00000)	0.000008 (0.00000)
1980	-0.0143 (0.0002)	-0.0146 (0.0002)	-0.0103 (0.0002)	-0.0081 (0.0001)	-0.0081 (0.0001)
1990	-0.0101 (0.0004)	-0.0108 (0.0004)	-0.0075 (0.0003)	-0.0055 (0.0002)	-0.0055 (0.0002)
Age Dummies	Yes	Yes	Yes	Yes	Yes
Pseudo R-square	0.0193	0.0204	0.1039	0.1614	0.1614

Notes: The marginal effects are calculated at the sample means. Number of observations for 1980 is 2,027,504. Number of observations for 1990 is 2,193,947. Number of observations for 2000 is 2,228,495. All specifications include a full set of age dummies. Controls are: (1) age dummies; (2) age, education; (3) age, education, race/ethnicity; (4) age, race, ethnicity, education, and U.S. citizen.

Table 4. Change in the Relative Institutionalization Rate for Immigrant Arrival Cohorts Compared with the Native Born in 1980, 1990, and 2000 Between Census Estimates^a
(Standard Errors in Parentheses)

	1990 to 2000				1980 to 1990			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
All								
<i>1985-1990 Cohort</i>	-0.0074 (0.0005)	-0.0043 (0.0003)	-0.0039 (0.0002)	-0.0043 (0.0003)				
<i>1980-1984 Cohort</i>	-0.0086 (0.0006)	-0.0042 (0.0003)	-0.0036 (0.0002)	-0.0041 (0.0003)				
<i>1975-1979 Cohort</i>	-0.0098 (0.0009)	-0.0042 (0.0004)	-0.0036 (0.0003)	-0.0040 (0.0003)	0.0009 (0.0008)	-0.0016 (0.0003)	-0.0012 (0.0003)	-0.0018 (0.0004)
<i>1970-1974 Cohort</i>	-0.0075 (0.0014)	-0.0025 (0.0006)	-0.0025 (0.0004)	-0.0031 (0.0005)	0.0014 (0.0011)	-0.0011 (0.0005)	-0.0009 (0.0004)	-0.0014 (0.0004)
Native Born and Naturalized Citizens Only								
<i>1985-1990 Cohort</i>	-0.0137 (0.0014)	-0.0065 (0.0006)	-0.0054 (0.0004)					
<i>1980-1984 Cohort</i>	-0.0140 (0.0012)	-0.0072 (0.0006)	-0.0060 (0.0004)					
<i>1975-1979 Cohort</i>	-0.0137 (0.0012)	-0.0072 (0.0007)	-0.0060 (0.0005)		-0.0055 (0.0016)	-0.0030 (0.0008)	-0.0022 (0.0006)	
<i>1970-1974 Cohort</i>	-0.0144 (0.0017)	-0.0072 (0.0010)	-0.0061 (0.0007)		-0.0021 (0.0017)	-0.0015 (0.0009)	-0.0012 (0.0007)	
Immigrants Compared with the Native Born Who Live Outside Their State of Birth								
<i>1985-1990 Cohort</i>	-0.0030 (0.0005)	-0.0013 (0.0003)	-0.0015 (0.0003)	-0.0019 (0.0003)				
<i>1980-1984 Cohort</i>	-0.0044 (0.0006)	-0.0011 (0.0003)	-0.0011 (0.0002)	-0.0014 (0.0003)				
<i>1975-1979 Cohort</i>	-0.0051 (0.0008)	-0.0010 (0.0003)	-0.0010 (0.0003)	-0.0012 (0.0003)	0.0017 (0.0007)	-0.0010 (0.0003)	-0.0008 (0.0003)	-0.0013 (0.0004)
<i>1970-1974 Cohort</i>	-0.0034 (0.0012)	0.0000 (0.0005)	-0.0003 (0.0004)	-0.0007 (0.0042)	0.0019 (0.0011)	-0.0007 (0.0004)	-0.0006 (0.0004)	-0.0010 (0.0042)

Notes: These numbers are calculated using the marginal effects calculated from logit estimates run separately for each year; see Appendix Table 3 for the marginal effects from which the top panel numbers are calculated. All specifications include a full set of age dummies. Controls are: (1) age dummies; (2) age, education; (3) age, education, race/ethnicity; (4) age, race, ethnicity, education, and U.S. citizen. Standard errors are calculated as for the difference between two means.

^aBetween-Census estimates are calculated by subtracting the probability for a given cohort in the two different Censuses (probability in later census minus probability in earlier census) from logit regressions run separately for each year.

Table 5. Change in the Relative Institutionalization Rate for Immigrant Arrival Cohorts Compared with the Native Born in 1980, 1990, and 2000
Constant Exposure Time Estimates^a
(Standard Errors in Parentheses)

	1990 to 2000				1980 to 1990			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
All								
Fewer than 5	-0.0110 (0.0005)	-0.0054 (0.0003)	-0.0047 (0.0002)	-0.0051 (0.0003)	-0.0044 (0.0005)	-0.0039 (0.0002)	-0.0029 (0.0002)	-0.0034 (0.0003)
Between 5 and 10	-0.0142 (0.0006)	-0.0067 (0.0003)	-0.0054 (0.0002)	-0.0058 (0.0003)	-0.0007 (0.0007)	-0.0024 (0.0003)	-0.0019 (0.0002)	-0.0024 (0.0003)
Native Born and Naturalized Citizens Only								
Fewer than 5	-0.0124 (0.0016)	-0.0059 (0.0007)	-0.0049 (0.0004)		-0.0066 (0.0018)	-0.0048 (0.0007)	-0.0037 (0.0005)	
Between 5 and 10	-0.0163 (0.0011)	-0.0082 (0.0006)	-0.0065 (0.0004)		-0.0040 (0.0013)	-0.0034 (0.0006)	-0.0027 (0.0005)	
Immigrants Compared with the Native Born Who Live Outside Their State of Birth								
Fewer than 5	-0.0065 (0.0005)	-0.0025 (0.0003)	-0.0025 (0.0003)	-0.0029 (0.0003)	-0.0036 (0.0005)	-0.0032 (0.0003)	-0.0026 (0.0003)	-0.0031 (0.0003)
Between 5 and 10	-0.0095 (0.0026)	0.0069 (0.0003)	-0.0031 (0.0003)	-0.0034 (0.0003)	0.0001 (0.0006)	-0.0019 (0.0003)	-0.0016 (0.0003)	-0.0021 (0.0004)

Notes: These numbers are calculated using the marginal effects calculated from logit estimates run separately for each year; see Appendix Table 3 for the marginal effects from which the top panel numbers are calculated. All specifications include a full set of age dummies. Controls are: (1) age dummies; (2) age, education; (3) age, education, race/ethnicity; (4) age, race, ethnicity, education, and U.S. citizen. Standard errors are calculated as for the difference between two means.

^aThese numbers are calculated by subtracting the relative institutionalization rate in the earlier Census from the relative institutionalization rate in the later Census for cohorts that had been in the U.S. for the same length of time (e.g., marginal effect for 1996–2000 cohort in 2000 minus marginal effect for 1985–1990 cohort in 1990).

Appendix

Two potential problems with Census data could affect our results. The first is the “undercount”—that is, failing to enumerate individuals, a problem typically considered more serious in poor and minority communities. The second potential problem concerns how the Census counts special populations, such as those in institutions. For example, if a high fraction of those under correctional supervision are in transition (being transferred from one place to another), the population count may miss them. Thus, it is worth understanding how the Census collects data for special populations.

1) Data Collection in Special Populations

Data collection in special populations—like that in the institutionalized population—may present particular challenges. For example, many people in institutions may be unable or unwilling to fill out Census forms. In the case of prisoners, people may also frequently be moved between institutions, creating difficulty in counting them.

The Census has developed a different form for people living in group quarters, and additional forms for those in military quarters and onboard ships. As mentioned in the data section, some types of group quarters are designated as “institutions.” Jails and prisons fall into this category. Although the PUMS data does not separately identify this population, we can infer that a very high fraction of the institutionalized population is in correctional facilities by limiting our sample to men aged 18-40 (confirmed by comparisons to the 1980 Census, where type of institution is identifiable).

In the 2000 Census, about half of those living in group quarters were unable to fill out Census forms (personal correspondence with Karen Humes, Special Populations Division, U.S. Census Bureau). A disproportionate share of these individuals is in an institution (jail, prison, or mental institution, for example). In this case, Census enumerators fill out the forms using the institutions’ administrative records.

The enumeration procedure for group quarters occurs over several weeks. Census enumerators ask where an inmate was on April 1. If inmates who are in transit on April 1 reach their final destination that day, they are counted at the final destination. If they are in transit, they are counted at their originating location.

Under these circumstances, then, the institutionalized population is likely to be well-counted, as the institutions are likely to keep accurate administrative records that document the number of inmates. Thus, the “undercount” of the institutionalized population is likely much less severe than that of the overall population. The demographic information on inmates of correctional institutions is also likely to be about as good as the administrative records themselves. Since there was more pressure and more incentive for correctional institutions to identify (non-naturalized) immigrants in their inmate populations in 2000 than in 1990, we would expect that a higher fraction of immigrants would be identified in 2000 than in 1990. Thus, any “undercount” of institutionalized immigrants would likely be higher in 1990 than in 2000.

2) The Undercount

The 1990 Census is widely viewed to have missed a substantial number of people. This problem is thought to be particularly severe in the case of poor and minority communities. The undercount does not present a problem for our analysis per se, if all populations are miscounted to the same degree in all years. There may be a problem for our analysis of changes in institutionalization between years, however, if the undercount changes across the years, or is different for different populations.

Consider, for example, the change in the institutionalization rates of immigrants between 1990 and 2000. Table 2 shows that the fraction institutionalized for immigrants in 1990 was 0.0107 and fell to 0.0068 in 2000. Our interpretation is that immigrants were less likely to be institutionalized in 2000 than in 1990. However, this change could occur mechanically if the undercount of minority communities was less severe in 2000 than in 1990. Our interpretation of this decline in institutionalization as signaling something about the behavior of immigrants in the U.S. would be flawed, if the decline really occurs because of better data collection.

Robinson et al. (2002) uses demographic analysis to estimate the undercount in 1990 and 2000. They estimate that the net undercount in 1990 was 1.65%, and in 2000 was a much smaller 0.12%. We can use these estimates to do some “back-of-the-envelope” calculations as to how the undercount of the immigrant population might affect our estimate of the fraction of immigrants who are institutionalized. Appendix Table 1 shows how our estimate of the fraction of immigrants institutionalized would change under different assumptions about the undercount of immigrants in 1990 and 2000.

Appendix Table 1: Estimates of How the Undercount Might Affect Fraction of Immigrants Institutionalized

Undercount Ratio Immigrants : Native-Born	Fraction Institutionalized	
	1990	2000
1:1	0.0105	0.00679
2:1	0.0104	0.00678
3:1	0.0102	0.006776
37:1	0.0067	0.0065

These calculations are based on the numbers reported in Tables 1 and 2. For example, in 1990, our sample included 209,878 immigrants. The fraction institutionalized was 0.0107, implying 2245.7 institutionalized immigrants in 1990. If we assume that the undercount estimate applies to the non-institutionalized population, then we need to subtract the number of institutionalized immigrants from the full sample, multiply this number by the fraction “missing,” and then add this number back onto the estimate of the total number of immigrants: $(209878 - 2245.7) * 0.0165 = 3425.9$. Thus, the fraction institutionalized among the immigrants, assuming a 1.65% undercount, would be: $2245.7 / (209878 + 3425.9) = 0.0105$.

Using this formulation, we can examine the impact on the estimate of the fraction of immigrants institutionalized, given different assumptions about the severity of the undercount in the non-institutionalized immigrant population.

Assuming that the undercount is three times larger for immigrants than for the overall population (that is, there are three “missing” immigrants for every “missing” person overall), we would still find that the fraction institutionalized among immigrants was over 1.5 times higher in 1990 than in 2000. For the undercount to be the only reason for the decline in the fraction institutionalized among immigrants between 1990 and 2000, we would have to think that the undercount was about 37 times larger for immigrants than for the population overall.

In sum, neither the improvement in the undercount of the overall population between 1990 and 2000, nor undercount problems that pertain to the institutionalized population, would be likely to mechanically generate our finding that the fraction of immigrants institutionalized between 1990 and 2000 substantially declined.

Appendix Table 2. Marginal Effects for Logit Estimates of Institutionalization
(Evaluated at Constant Profile, Robust Standard Errors in Parentheses)

	(0)	(1)	(2)	(3)	(4)
Immigrant	-0.0348 (0.0014)				
1996-2000		-0.0363 (0.0014)	-0.0455 (0.0018)	-0.0552 (0.0026)	-0.0665 (0.0044)
1991-1995		-0.0341 (0.0014)	-0.0437 (0.0018)	-0.0535 (0.0026)	-0.0648 (0.0043)
1985-1990		-0.0284 (0.0014)	-0.0390 (0.0017)	-0.0493 (0.0025)	-0.0606 (0.0043)
1980-1984		-0.0168 (0.0021)	-0.0283 (0.0021)	-0.0402 (0.0027)	-0.0513 (0.0043)
1975-1979		-0.0148 (0.0024)	-0.0248 (0.0024)	-0.0369 (0.0029)	-0.0478 (0.0044)
1970-1974		-0.0021 (0.0039)	-0.0107 (0.0040)	-0.0258 (0.0040)	-0.0364 (0.0052)
1965-1969		0.0084 (0.0055)	0.0074 (0.0064)	-0.0089 (0.0061)	-0.0188 (0.0070)
1960-1964		0.0235 (0.0078)	0.0337 (0.0102)	0.0191 (0.0102)	0.0107 (0.0110)
1950-1959		0.0096 (0.0090)	0.0182 (0.0121)	0.0132 (0.0133)	0.0050 (0.0141)
1940-1950		0.0405 (0.0172)	0.0493 (0.0206)	0.0415 (0.0216)	0.0369 (0.0230)
Less than H.S.			0.6068 (0.0144)	0.5473 (0.0160)	0.5795 (0.0166)
H.S. Degree			0.0446 (0.0018)	0.0515 (0.0025)	0.0613 (0.0039)
Some College			0.1693 (0.0109)	0.1588 (0.0112)	0.1811 (0.0135)
Black				0.1947 (0.0074)	0.2206 (0.0102)
American Indian				0.0412 (0.0050)	0.0481 (0.0061)
Asian or Pacific				0.0311 (0.0053)	0.0372 (0.0065)
Other Race				-0.0039 (0.0025)	-0.0046 (0.0029)
Hispanic				0.0326 (0.0020)	0.0386 (0.0029)
U.S. Citizen					-0.0114 (0.0032)
Years in U.S.	0.0028 (0.0003)	-0.0008 (0.0003)	-0.0024 (0.0004)	-0.0025 (0.0005)	-0.0028 (0.0006)
Years in U.S. ²	-0.00004 (0.00001)	-0.00001 (0.00001)	0.00003 (0.00001)	0.00004 (0.00001)	0.00004 (0.00002)
1980	-0.0250 (0.0010)	-0.0262 (0.0010)	-0.0348 (0.0014)	-0.0395 (0.0019)	-0.0470 (0.0030)
1990	-0.0145 (0.0007)	-0.0156 (0.0008)	-0.0200 (0.0010)	-0.0216 (0.0013)	-0.0256 (0.0018)
Age Dummies	Yes	Yes	Yes	Yes	Yes
Pseudo R-square	0.0193	0.0204	0.1039	0.1614	0.1614

Notes: The marginal effects are calculated for a 25-year-old Hispanic male with a high school degree. Number of observations for 1980 is 2,027,504. Number of observations for 1990 is 2,193,947. Number of observations for 2000 is 2,228,495. All specifications include a full set of age dummies. Controls are: (1) age dummies; (2) age, education; (3) age, education, race/ethnicity; (4) age, race, ethnicity, education, and U.S. citizen.

Appendix Table 3. Marginal Effects for Logit Estimates of Institutionalization
(Evaluated at Sample Mean, Robust Standard Errors in Parentheses)

	1990				2000							
Immigrant	-0.0110 (0.0003)				-0.0276 (0.0002)							
1996-2000					-0.0254	-0.0166	-0.0142	-0.0146	(0.0002)	(0.0002)	(0.0001)	(0.0002)
1991-1995					-0.0239	-0.0160	-0.0137	-0.0141	(0.0003)	(0.0002)	(0.0001)	(0.0001)
1985-1990	-0.0144	-0.0112	-0.0095	-0.0095	-0.0219	-0.0155	-0.0134	-0.0138	(0.0004)	(0.0002)	(0.0002)	(0.0002)
1980-1984	-0.0097	-0.0094	-0.0083	-0.0083	-0.0183	-0.0136	-0.0119	-0.0123	(0.0006)	(0.0003)	(0.0002)	(0.0003)
1975-1979	-0.0091	-0.0090	-0.0079	-0.0079	-0.0189	-0.0131	-0.0115	-0.0118	(0.0007)	(0.0003)	(0.0002)	(0.0003)
1970-1974	-0.0076	-0.0081	-0.0073	-0.0073	-0.0151	-0.0106	-0.0098	-0.0104	(0.0011)	(0.0005)	(0.0003)	(0.0004)
1965-1969	-0.0043	-0.0049	-0.0049	-0.0049	-0.0180	-0.0111	-0.0100	-0.0104	(0.0014)	(0.0008)	(0.0006)	(0.0006)
1960-1964	-0.0044	-0.0031	-0.0031	-0.0031	-0.0097	-0.0045	-0.0048	-0.0058	(0.0019)	(0.0013)	(0.0010)	(0.0010)
1950-1959	-0.0096	-0.0060	-0.0041	-0.0041	-0.0096	-0.0060	-0.0041	-0.0041	(0.0017)	(0.0012)	(0.0012)	(0.0012)
1940-1950												
Less than H.S.	0.1920 0.1153 0.1153 (0.0054) (0.0038) (0.0038)				0.2468 0.1630 0.1621 (0.0046) (0.0035) (0.0035)							
H.S. Degree	0.0491 0.0315 0.0315 (0.0014) (0.0010) (0.0010)				0.0689 0.0469 0.0466 (0.0014) (0.0010) (0.0010)							
Some College	0.0356 0.0243 0.0243 (0.0013) (0.0010) (0.0010)				0.0349 0.0243 0.0242 (0.0011) (0.0008) (0.0008)							
Black	0.0393 0.0393 (0.0007) (0.0007)				0.0432 0.0430 (0.0006) (0.0006)							
American Indian	0.0173 0.0173 (0.0013) (0.0013)				0.0043 0.0043 (0.0006) (0.0006)							
Asian or Pacific	0.0013 0.0013 (0.0009) (0.0009)				0.0081 0.0083 (0.0008) (0.0008)							
Other Race	0.0314 0.0314 (0.0071) (0.0071)				-0.0010 -0.0010 (0.0003) (0.0003)							
Hispanic	0.0119 0.0119 (0.0005) (0.0005)				0.0165 0.0164 (0.0005) (0.0005)							
U.S. Citizen	0.0000 (0.0008)				-0.0053 (0.0010)							
Age Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Pseudo R-square	0.0072	0.0077	0.0779	0.1379	0.1379	0.0213	0.0221	0.1166	0.1739	0.1739	0.1739	

Notes: The marginal effects are calculated at the sample means. Number of observations for 1990 is 2,193,947. Number of observations for 2000 is 2,228,495. All specifications include a full set of age dummies. Controls are: (1) age dummies; (2) age, education; (3) age, education, race/ethnicity; (4) age, race, ethnicity, education, and U.S. citizen.