How Much is a Green Card Worth? Evidence from Mexican Men Who Marry Women Born in the U.S.

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Abstract: Many countries impose restrictions on some immigrants' job mobility, likely reducing their wages. We quantify such effects for Mexican-born men in the U.S. by recognizing that immigrants who marry U.S. natives receive expedited "green cards" (Permanent Residency). Robust IV estimates indicate intermarried Mexicans earn a 40 percent wage premium, and larger for the most mobile subgroups. Analogous premiums are statistically insignificant for men from Puerto Rico, who acquire no new rights because they are already U.S. citizens. Attributing the approximately 30 percent difference to green cards, we estimate eliminating wait times would increase Mexicans' mean earnings $120,000-$150,000 in present value.

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The worldwide acceleration of international migration in recent decades has not been matched by equal growth in the number of migrants granted permanent status, and the more temporary arrangements often favored by host governments typically restrict foreign workers to sectors in which there are skill shortages (United Nations 2013). While such rules are usually intended to promote labor market flexibility and to limit competition with native workers, those benefits come at a cost to both the foreign workers and the host economy. For example, workers allowed to work only in a specific occupation (or even for a particular employer) may be unable to secure the wage growth that often comes from actual or potential job mobility, and their inability to move may preclude the formation of more efficient employment matches.

This paper aims to illuminate such costs by estimating the wage gains earned by Mexican men working in the U.S. if they obtain permanent resident status (often called a "green card") faster than usual. Several considerations suggest this is a particularly relevant group to analyze. Mexicans living in the U.S. are the world's largest foreign-born population, and they tend to acquire permanent status quite slowly: the average Mexican who obtains a green card previously had a more temporary status for 16 years (Shear and Preston, 2013).[1] Many have low levels of skill, so they represent a group that host countries have often been reluctant to admit. Moreover, while immigrants from Mexico and elsewhere clearly view green cards as quite valuable, it is unclear whether this reflects potential gains from job mobility or simply the stability afforded by permanent status. Estimates from previous studies vary widely and thus offer limited guidance.

Since no sufficiently large data set reports both persons' visa status and wages, our analysis exploits a feature of U.S. immigration law that allows some people to obtain green cards faster than others. The law prioritizes applications from immediate family of U.S. citizens, so

[1] At 13 million, the population of Mexican natives in the U.S. exceeds the entire foreign-born population in every other country. It accounts for 28.4 percent of the U.S. foreign-born population and 5.6 percent of the world's (United Nations 2014). U.S. immigrants from most other countries typically wait six to eight years for a green card (queues are longer for those from common source countries), but even delays of that magnitude have recently prompted several proposed reforms to expedite the process.

immigrants who marry citizens (called "intermarriage") can become permanent residents in as little as six months. We thus use intermarriage as a proxy for a green card.

Our empirical strategy is then to measure the wage premium earned by intermarried men, beyond a general marriage premium. Since it would clearly be inappropriate to ascribe the full intermarriage premium to green cards if native wives offered benefits besides legal rights (e.g., knowledge of institutions or access to social networks), we compute the green card premium as a *difference* between intermarriage premiums of (a) men born in Mexico, and (b) control groups of immigrants who have permanent access to the U.S. labor market regardless of marital status. Our most common control group is men born in Puerto Rico (who are thus U.S. citizens),[2] but we also consider naturalized U.S. citizens born in Mexico and Mexicans eligible for green cards under the Immigration Reform and Control Act ("IRCA"), the 1986 law that granted amnesty to 2.7 million previously undocumented persons. However, the subtraction makes little difference in practice, as the estimated premiums are much larger for Mexicans than for the controls.

Another concern is that intermarried men may differ from others in unobserved ways that also affect their wages. A large literature shows that positive self-selection explains part of the raw earnings gap between married and unmarried men, and it is plausible that intermarried men may be even more positively selected – though previous work has found intermarried men in several nations are actually selected negatively on unobservables (Meng and Gregory 2005, Furtado and Theodoropoulos 2009, Meng and Meurs 2009).

For Mexicans, theory also provides a strong reason to expect *negative* selection into intermarriage on the basis of their employers' ability to pay immobile workers lower wages.[3] In short, immigrants whose wages have been most depressed by their inability to seek alternate employment have the strongest incentive to seek a (native) spouse who can help them obtain a green card.[4] Since their newfound mobility would then enable them to earn more competitive

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[2] For lack of a better word, throughout the paper we use "immigrant" to refer to any U.S. resident who was born outside the 50 states. While consistent with dictionary definitions, this usage is considerably broader than the terminology of U.S. immigration law, which excludes, e.g., Puerto Ricans and people without immigrant visas.

[3] For example, minimum wage laws or collective bargaining may prevent employers from paying immobile workers lower wages. Immobility penalties may also vary due to diversity across firms in the value of experienced workers.

[4] While fraudulent "green card marriages" are a common media trope and the subject of extensive scrutiny by U.S.

wages, we may observe intermarried men earning wages similar to (or even still a bit less than) those of comparable workers, yet that small gap in raw wages may mask a much larger initial wage gap and thus a large increase in wages made possible by their new green card.

Which of those biases dominates is an empirical question, but we are not surprised that our results imply the latter is much more relevant. Even if intermarried men were selected positively on their own skills, the estimated green card premium may not be seriously biased insofar as (a) we control for observed skills, (b) unobserved skills correlate with other controls (e.g., education and English fluency), or (c) remaining biases cancel when the groups' intermarriage premiums are differenced. In contrast, our control variables seem unlikely to correlate with firms' ability to pay lower wages to immobile men, and since only Mexicans could ever suffer the immobility penalty, differencing the intermarriage premiums would not remove the latter bias.

We address the potential bias by using instruments from previous work on intermarriage, most of which involve geographic variation in demographics. For example, the main variable we use to predict intermarriage is the local ratio of immigrants to natives among single Hispanic women, the idea being that men's choice between them is influenced by relative supply. This and several other—often weakly correlated—proposed instruments all yield very similar estimates.

Nonetheless, our estimates may be best interpreted as lower bounds on the true green card premium. One reason is that intermarriage is an imperfect proxy for green cards, so the resulting measurement error causes attenuation. Another is that our instruments identify the local average treatment effect (LATE) for men whose choice between a native or immigrant wife is sensitive to relative supply, which may not include men with the most to gain. We would also understate the gains for singles if joint labor supply decisions limit the mobility of married men (Mincer 1978).

Despite all those considerations, robust estimates imply that men with green cards receive a large wage premium, on the order of 30 percent. Since the estimated intermarriage premium for each control group is typically around 10 percent and always statistically insignificant (versus 40 percent for Mexicans), the green card premium would be grossly overestimated only if other, non-

immigration officials, note that selection may also arise from more mundane mechanisms like directed search for a (non-fraudulent) spouse who is a citizen or expedited marriages by couples who might otherwise (e.g.) cohabit.

green card benefits of intermarriage raised Mexicans' wages several times more than the controls'. Moreover, the modest estimates for control groups are consistent with Chi's (2013) recent claim that legal rights are a main source of the intermarriage wage premium.

1. Background

There are several reasons that immigrants with green cards may earn higher wages. First, green cards allow much greater mobility between jobs; this is a major source of early-career wage growth even for U.S. natives, resulting in a total gain of about 40 percent over the first decade of their careers (Topel and Ward 1992, Tchernis 2010). Just having the option to move provides leverage to negotiate for raises, as it reduces firms' monopsony power (Robinson 1933). Recent work suggests this is especially relevant for immigrants: Hotchkiss and Quispe-Agnoli (2012) estimate job mobility accounts for 30 percent of the wage gap between undocumented and other workers, Amuedo-Dorantes and Bansak (2011) argue it was a major cause of wage growth for those granted legal status under the IRCA, and Mukhopadhyay and Oxborrow (2012) cite mobility as the main source of wage raises when highly skilled immigrants get green cards.

A second advantage of green cards is their permanence. Foreigners authorized to work in the U.S. lose that right if they are laid off or if their employer does not petition for a green card before their visa expires (typically at most six years after issuance). Many firms do not sponsor visas or hire workers lacking green cards, and others may try to offer them lower wages. For undocumented workers, another benefit is protection against exploitation – e.g., legal status enables them to seek legal recourse against dishonest employers without risking deportation. Permanence also adds incentive to invest in country-specific human capital (Cortes 2004).

Most variation in immigrants' legal status stems from unobserved factors that affect wages directly (e.g., time since immigration, being related to a citizen, or having rare skills), so an exogenous source of variation in legal status is required to identify a causal effect. Most previous studies used apparently exogenous changes in immigration laws, most often the IRCA. Perhaps surprisingly, many found that green card recipients' wages rose less than 10 percent (Borjas and Tienda 1993, Cobb-Clark et al. 1995, Kossoudji and Cobb-Clark 2002, Barcellos 2010). Others

suggest IRCA-based estimates may greatly understate wage gains that would result from other reforms, however, either because the act caused a huge labor supply shock or because larger estimates (up to 25 percent) emerge from other natural experiments in which green cards went to more-skilled workers (Bailey 1985, Cobb-Clark et al. 1995, Kaushal 2006, Kandilov 2008, Barcellos 2010, Mukhopadhyay and Oxborrow 2012, Orrenius et al 2012). Lozano and Sorensen (2011) argue instead that earlier IRCA estimates were attenuated by measurement error in workers' legal status; when they address that bias they estimate the IRCA raised undocumented workers' wages 20 percent – mainly by enabling them to move to more lucrative occupations.

Our empirical strategy instead builds upon the fact that immigrants who marry U.S. citizens thereby acquire new rights, including expedited green cards. This is the single most common way green cards are obtained, accounting for 23.2 percent of all green cards issued in 2000. In the years preceding our study, Mexicans obtained 11.5 times as many green cards via marriage as via employment.[5] Since the law at the time placed few restrictions on spouse-based green card applications,[6] virtually all intermarried men would have had one. Conversely, while we lack direct data on men's immigration status, it is likely that relatively few non-intermarried men had acquired a green card another way. An estimated 56 percent of Mexican natives living in the U.S. were undocumented, young men are thought to be overrepresented in that group, and they were also among the leading recipients of nonimmigrant visas. Intermarriage thus seems a reasonable proxy for a green card (INS 1996-2000; DHS 2005, 2011; Hanson 2006).

One concern is that marriage itself may raise wages, regardless of wives' citizenship. Many studies have asked whether the marital wage premium owes to a causal mechanism like gains from specialization or discrimination, reverse causality (e.g., if women prefer high-wage men), or spurious correlation (e.g., if women and employers seek similar traits in men); see Maasoumi et al. (2009) for a thorough review. Premiums can also be negative, e.g., if co-location problems

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[5] Green cards were granted under at least 29 different provisions in 2000; the next most common category was spouses of alien residents (14.7 percent). In 1995-2000 Mexicans received 239,067 green cards via marriage to U.S. citizens and 20,709 via employment (INS 1996-2002, Tables 4 and 8).

[6] Since 2001, intermarriage no longer entitles undocumented immigrants to expedited consideration for a green card. See U.S. Citizenship and Immigration Services (http://www.uscis.gov) for a list of ways to obtain a green card.

inhibit married people from migrating to better jobs (Mincer 1978). Regardless, the effect does not involve green cards unless the men are intermarried, so our strategy is to estimate an *intermarriage* premium earned by intermarried men beyond the baseline marriage premium.

Only a few studies have measured a causal effect of intermarriage, mainly using instruments based on geographic variation in demographics. Meng and Gregory (2005) estimate intermarried Australian immigrants earn a 15-23 percent wage premium, and Meng and Meurs (2009) find a 25-35 percent premium for France. Kantarevic (2004) finds no intermarriage wage premium in the 1970 and 1980 U.S. Censuses, but intermarried U.S. immigrants are more likely to have a job (Furtado and Theodoropoulos 2009, 2010). Chi (2013) compares the intermarriage premiums of Mexicans who entered the U.S. before and after 1982, noting the former group would not have gained new legal rights since they were already eligible for them under the IRCA. She finds pre-1982 Mexican immigrants did not earn a statistically significant intermarriage premium in 1990, but post-1982 Mexicans and both pre- and post-1982 immigrants from South America (few of whom benefitted from the IRCA) earned intermarriage premiums of 30-60 percent.

Of course, an intermarriage premium need not derive from green cards—e.g., native wives may provide access to social networks that alert husbands to job openings. To isolate the green card effect, we compute the *difference* between Mexicans' intermarriage premium and those of control groups who do not gain new rights via intermarriage. While we also use two Mexican subgroups as control groups, we primarily present evidence based on men born in Puerto Rico.

As Furtado and Theodoropoulos (2010) explain, the motive for considering Puerto Ricans is twofold. First, all Puerto Ricans are U.S. citizens (since 1917) and thus have permanent access to the U.S. labor market.[7] Second, the economic conditions facing both groups are in many ways similar: emigrants from both places come mainly from the lower half of the earnings distribution, but not the very bottom (Borjas 2008); in 2000 their homelands had similar GDP/capita ($9,100 and $10,000 respectively for Mexico and Puerto Rico), Gini indices (55 and 56), life expectancy

[7] The U.S. annexed Puerto Rico after the 1898 Spanish-American War. It became an autonomous commonwealth in 1952, yet it remains a U.S. territory and periodically votes whether to petition for statehood or independence. Borjas (2008) covers the history, as well as migration patterns.

(72 and 76 years), and literacy (90 and 89 percent);[8] and once in the U.S. both face challenges common to Latin immigrants, including language barriers and discrimination.

Yet while such similarities suggest intermarriage *plausibly* offers similar non-legal benefits to Mexicans and Puerto Ricans, it remains possible that another mechanism (besides green cards) accounts for the difference in the groups' intermarriage premiums. For instance, having access to native spouses' social networks might ease some barrier to assimilation that confronts Mexicans more often than Puerto Ricans, yet which the econometrician cannot observe or control. Like green cards, such a mechanism would raise workers' wages by allowing them to consider more job opportunities, so most evidence consistent with one hypothesis would also support the other.

Nonetheless, some results relate directly to immigrants' legal status and thus lend support to the green card interpretation. For instance, we find no intermarriage premiums for Mexicans who had become naturalized U.S. citizens or for those eligible for green cards under the IRCA, versus large premiums for others who arrived just a few years later. Neither would be surprising if green cards caused the differences in the groups' intermarriage premiums, but it seems unlikely other benefits of intermarriage would end abruptly when Mexicans became citizens or permanent residents. We also find large premiums for men who entered the U.S. by age 6, even though we would not expect nearly life-long U.S. residents to benefit as much from spousal social networks. When we analyze a wider set of Latin source countries, we will find smaller, often insignificant estimates for nations from which green card recipients received them more quickly or had faced fewer prior legal vulnerabilities. Finally, Occam's Razor favors the green card interpretation, as it does not require Mexicans and Puerto Ricans to differ in unobservable ways. Taken together, we believe these points make a compelling case our estimates capture the effect of green cards.

2. Methodology

Our approach begins by estimating a model of log weekly wages Y_{ijg} earned by immigrant man i in metro area j from group g ($= M$ for Mexicans, P for controls):

[8] Data: U.S. Central Intelligence Agency (2002), Toro (2008). If the comparison is surprising, it may be because Puerto Rico had much faster economic growth since 2000, especially before 2008.

$$Y_{ijg} = \alpha_g + X_{ij}\beta_1 + Y_j\beta_2 + N_{ij}^g\gamma_g + D_{ij}^g\delta_g + \varepsilon_{ijg}\,. \tag{1}$$

The focal explanatory variable is N, a dummy variable that equals 1 if the man is intermarried. Men with immigrant wives serve as the baseline, so γ is the "intermarriage wage premium," reflecting benefits of intermarriage beyond any benefits of marriage itself. We aim to partition that premium into wage gains caused by green cards (Γ) and by all other ways that intermarriage affects wages (Ω_g). Assuming $E[\Omega_M|X,Y]=E[\Omega_P|X,Y]$, the "green card premium" is computed as the difference in intermarriage premiums: $\Gamma = \gamma_M - \gamma_P$.

Equation (1) also features a dummy variable D that equals 1 if the man is single; $(-\delta)$ is thus the baseline marriage premium. We cannot just exclude singles because the choice to marry may well depend on whether the wife is a native or an immigrant, and that dependence would likely differ between Mexicans and control groups. Specifically, if green cards offer large benefits, they would encourage Mexicans to intermarry (especially if their wages are depressed) and perhaps discourage them from marrying immigrants, but control groups would face no such incentives.

The rest of equation (1) are controls. Vector X includes variables common in immigration studies: full sets of dummies for ages, years since immigration, and levels of education; the local population share of Hispanic immigrants (to capture enclave effects); and dummies for English fluency (=1 if he speaks well or better), current enrollment, veteran and disability status, urban residence, and the nine Census divisions.[9] We add the mean log wages of local native men with the same education (Y_j) to control for intraregional price levels, which may correlate with intermarriage, e.g., if large cities had higher levels of both.[10] We experimented with allowing β_1 and β_2 to differ across groups, but the difference was statistically significant only for the schooling dummies (Puerto Ricans earn higher returns to postsecondary education), so we ultimately restricted the coefficients of the other controls to be equal across groups.

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[9] The disability dummy equals one for men with physical or mental health conditions that cause difficulty working. A binary fluency variable is standard in the literature, but see Table 5 for results using full sets of dummies.

[10] Some readers may be reassured to learn that this variable's influence is modest. Dropping it alters $\widehat{\Gamma}$ by less than 0.05 (in either direction), though it also becomes less precise—e.g., in the main specification, the exclusion changes the estimate from 0.315 (0.114) to 0.298 (0.124). If we instead use men's log wage relative to the average native's as the dependent variable (i.e., restrict β_2=1), the estimate is 0.380 (0.125).

2.1 Instrumental Variables Estimation

Since immigrants' marital status may be correlated with unobserved factors ε, we use an instrumental variables approach. As usual, this entails variables Z that predict variation in marital status but are otherwise unrelated to wages, but some problems arise from the discrete nature of N and D. While we could simply use Z as our instruments (i.e., a linear probability model in the first stage), in practice those first-stage regressions fit poorly and yield very imprecise estimates.

Instead we use a similar approach proposed by Heckman (1978) that is thought to be more efficient than standard two-stage least squares (2SLS) when endogenous variables are discrete. In short, the idea is to use Z not as instruments, but as excluded variables in a non-linear model of the endogenous discrete outcome, and then use the resulting fitted values as instruments for that discrete outcome in standard 2SLS.[11]

Formally, we begin by computing fitted values $\ddot{N}_{ij}^{g}$ and $\ddot{D}_{ij}^{g}$ from a multinomial logit of men's marital status S_{ijg} ($\equiv 0, n, d$ for married to an immigrant, intermarried, or single) on Z:

$$\Pr\left(S_{ijg} = q\right) = \frac{\exp(\theta_o^q + X_{ij}\theta_1^q + Y_j\theta_2^q + Z\theta_3^q)}{1+\sum_{s=n,d}\exp(\theta_o^s + X_{ij}\theta_1^s + Y_j\theta_2^s + Z\theta_3^s)} \qquad (q = n, d). \tag{2}$$

Call this step the "0th-stage" to avoid confusion with the usual 2SLS first-stage. $\ddot{N}_{ij}^{g}$ and $\ddot{D}_{ij}^{g}$ are then used to instrument for N_{ij}^{g} and D_{ij}^{g} in standard 2SLS. That is, our first-stage is

$$\begin{bmatrix} N_{ij}^{g} \\ D_{ij}^{g} \end{bmatrix} = \begin{bmatrix} \pi_{0N}^{g} \\ \pi_{0D}^{g} \end{bmatrix} + \begin{bmatrix} \pi_{1N}^{g}{}' \\ \pi_{1D}^{g}{}' \end{bmatrix} X_{ij} + \begin{bmatrix} \pi_{2N}^{g} \\ \pi_{2D}^{g} \end{bmatrix} Y_j + \begin{bmatrix} \pi_{3N}^{g} & \pi_{4N}^{g} \\ \pi_{3D}^{g} & \pi_{4D}^{g} \end{bmatrix} \begin{bmatrix} \ddot{N}_{ij}^{g} \\ \ddot{D}_{ij}^{g} \end{bmatrix} + \begin{bmatrix} v_N^g \\ v_D^g \end{bmatrix}, \tag{3}$$

and the second-stage is (1), but with the fitted values $(\widehat{N}_{ij}^{g}, \widehat{D}_{ij}^{g})$ from (3) in place of (N_{ij}^{g}, D_{ij}^{g}).

A few notes may be helpful for those unfamiliar with the approach. Most importantly, our estimator is consistent. It is well-known that estimates would be inconsistent if the second-stage used the non-linear 0th-stage estimates $(\ddot{N}, \ddot{D})$ in lieu of $(\widehat{N}, \widehat{D})$ ("the forbidden regression"), but our linear first-stage (3) precludes that problem. Second, since all controls are included in all three stages, γ and δ are identified from variation in Z, just as if Z were used as the instruments directly.

[11] The estimator is largely inspired by the insight of Kelejian (1971). See Wooldridge (2002, 230-237) or Angrist and Pischke (2009, 190-192) for introductory discussions, or Newey (1990) for a more general treatment. Dubin and McFadden (1984) were among the first to use this approach in an applied context.

The purpose of the intermediate step (2) is to create stronger instruments. Intuitively, $(\ddot{N}, \ddot{D})$ is just a more promising transformation of Z—the non-linearity of (2) curbs Z's influence in observations for which it predicts S poorly (i.e., the logit forces $(\ddot{N}, \ddot{D}) \in [0,1]^2$), so $(\ddot{N}, \ddot{D})$ predicts (N, D) better than Z would. Finally, since proper IV estimates account for the generated nature of first-stage fitted values, those generated in the 0th-stage require no further adjustments.

Note also that our non-linear 0th-stage in effect adds interactions between excluded variables (Z) and other controls W $(\equiv (X, Y))$: $\Pr(S = q)$ is proportional to $\exp(W\theta^q)\exp\left(Z\theta_3^q\right) \approx (a_0 + a_1 W)(b_0 + b_1 Z)$, where the last step substitutes Taylor approximations for e^x. Intuitively, this suggests model (2) is akin to a standard 2SLS approach in which both Z and $Z * W$ are used as instruments. In practice the most important interaction is between Z and the time since the man immigrated (call it τ). This strikes us as quite reasonable – indeed, we often expect the effect of a given condition (Z) to vary with the duration of one's exposure to it (τ). Accordingly, it may be most appropriate to compare results from our procedure with those of a standard 2SLS model that uses both Z and τZ as instruments and still includes τ as a control in the earnings equation.

As in previous work on intermarriage, all of our excluded variables (Z) involve local demographics (Kantarevic 2004, Meng and Gregory 2005, Furtado and Theodoropoulos 2009, Meng and Meurs 2009). Since we must predict N^g and D^g, we need two Z's.

Our preferred pair includes the ratio of immigrants to natives among the local population of unmarried Hispanic women (call it Z^1), and the share of local women who are unmarried (Z^2). Z^1 is meant to predict whether an immigrant man intermarries, as it reflects the relative supply of immigrant and native wives,[12] while Z^2 is intended to predict choices on the margin between being married or single because it reflects unobserved factors that make being single more attractive in some cities. Since the model also includes natives' mean wage (Y_j), it need not be problematic if those unobservables were correlated with overall wage levels; bias would result only if immigrants' wages were correlated with Z^2 beyond what can be predicted from natives' wages and the other controls.

One may also worry that Z^1 reflects men living in ethnic enclaves, an environment that could

[12] We interpret the relevant market to include all Hispanic women, not just those of the man's own nationality, based on Rosenfeld's (2001) evidence of a "pan-national Hispanic identity" in U.S. marriage markets.

affect their wages directly. Several points provide reassurance. First, both the numerator and denominator of Z^1 are counts of Hispanics, so it is unclear even whether their ratio would be larger or smaller in areas with many Hispanics. Second, while Z^1 *would* be larger in enclaves defined as areas with many Hispanic *immigrants*, there would be no bias in that case because we control directly for the population share of Hispanic immigrants. More to the point, that share is uncorrelated with Z^1 in our data ($\rho = 0.01$), so there is evidently little cause for concern anyhow.

While these two comprise our preferred set of Z's, we will show several alternate pairs yield similar estimates. Among those considered are analogues to instruments used by Meng and Gregory (2005) (the share of local single women who are Hispanic immigrants, and the sex ratio among single Hispanic immigrants[13]) and Kantarevic (2004) (the ratio of immigrants to natives among single women, divided by the same ratio over the whole U.S.), as well as the share of women who are Hispanic and the sex ratio among singles.

Our use of instruments based on local demographics likely leads to a conservative bias. Since IV measures the LATE for those whose propensity to be treated is sensitive to the instrument (Imbens and Angrist 1994), our approach identifies the intermarriage premium for men who become more apt to intermarry when living in a place with more native women – e.g., men whose choice among potential wives changes with shifts in relative supply. If men who would benefit greatly from a green card actively seek a U.S.-born wife even if they live among many immigrant women, our estimate would underweight a subgroup with a large potential benefit and thus understate the true average effect.[14] Further, since these incentives are only relevant for the Mexicans in our sample, there is no chance the conservative bias is reduced or reversed when we compute the difference in the groups' intermarriage premiums.

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[13] While akin to those used by Meng and Gregory, our variables refer to different ethnic groups due to the different sources of U.S. and Australian immigrants. While the analogue to their first variable is (unlike our Z^1) strongly correlated ($\rho = 0.98$) with the local share of Hispanic immigrants and thus might unintentionally reflect enclave effects, that concern is again greatly reduced by our inclusion of that share as a control variable.

[14] In principle results could be biased upwards if the marital decisions of men with the least to gain from a green card were insensitive to local demographics, but we find little evidence for this—e.g., while $\widehat{\Gamma}$ is small for high school dropouts and men working in occupations common among undocumented workers (Section 4.5), for those groups the correlations between marital status and Z^1 are quite similar to those of other men. In contrast, those correlations are notably smaller for college graduates, for whom $\widehat{\Gamma}$ is large.

3. Data and Descriptive Statistics

Our data come from the 5 percent sample of the 2000 U.S. Census. We restrict the sample to men aged 16 to 44 who were born in Mexico or Puerto Rico, had positive earnings in 1999, and did not reside in group quarters.[15] We drop men who (a) entered the U.S. after age 25, the mean age of men who married in 1990 in both Mexico and Puerto Rico (United Nations 2000); (b) are divorced, widowed, or separated; or (c) married non-Hispanic immigrants.[16] The impetus for restriction (a) is that the 2000 Census does not allow us to infer the men's age at marriage, so we cannot say whether they were already married when they immigrated and thus were ineligible to intermarry unless their first marriage ended (Meng and Gregory 2005). Of the Mexicans in our sample who entered the U.S. in 1999, 91 percent were either unmarried or married to a U.S. citizen in the 2000 Census. Our identification strategy motivates restriction (b): lacking information about previous wives, we cannot say whether the man may have obtained a green card via the earlier marriage. Restriction (c) addresses our fear that men married to non-Hispanic immigrants may differ considerably from men married to Hispanic immigrants. All that said, we will later show that relaxing these restrictions does not greatly alter our estimates.

We define intermarriage as a marriage between a man born in Mexico or Puerto Rico and a wife born in the 50 states or the District of Columbia.[17] Wives born in Puerto Rico are thus treated as immigrants, despite being U.S. citizens. If we instead classified them as natives, only 506 Puerto Rican men (7 percent) would have a foreign-born wife, and anyhow it would be odd to call a union of two Puerto Ricans "intermarriage." The cost is that 127 Mexicans with Puerto Rican wives are not called intermarried, even though they may have gained more access to the U.S. labor market via their marriages. However, this is just 0.1 percent of the sample, and estimates are nearly identical if we reclassify them as intermarried.

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[15] The data were obtained via the Integrated Public Use Microdata Series (Ruggles et al. 2010). We dropped men born in Mexico as U.S. citizens (due to parentage). Older men are excluded both to limit biases due to differences in divorce rates across the two types of marriage (e.g. if immigrants with low wages were more likely to divorce when intermarried) and to focus on ages at which inter-job mobility accounts for a large share of wage growth.

[16] We define such wives as women born abroad in non-Hispanic countries who also do not claim Hispanic ethnicity.

[17] Our definition of "intermarriage" is independent of ethnicities; a man from Mexico could be intermarried to a woman of Mexican descent born in, e.g., Texas.

The dependent variable is the natural log of individuals' pre-tax weekly wage and salary income, including all money income from employers (commissions, cash bonuses, tips, etc.). We use weekly rather than annual wages to avoid potential complications due to differences in employment rates; intermarriage is associated with higher rates of employment even for groups that do not obtain new legal rights (Furtado and Theodoropoulos 2010).

The resulting sample contains 101,203 men born in Mexico and 6,891 born in Puerto Rico. The top panel of Table 1 reports summary statistics. Similar shares of Puerto Ricans (52 percent) and Mexicans (56) are married, but the former are more apt to intermarry (42 percent versus 19). Intermarried men in both groups have more education, English fluency, and years in the U.S.

[Tables 1 and 2 about here.]

The lower panel of Table 1 describes their wives. Wives who are also immigrants differ in ways that parallel differences between their husbands. The more interesting comparison is among wives born in the U.S.: those married to Mexicans are younger, less educated, and earn less (they are both less likely to be employed and have lower wages conditional on employment). These differences are usually predictable from our controls, however. If regression (1) (or a similar probit) is run on wives' traits rather than husband's wages, we cannot reject the hypotheses that wives of comparable intermarried Mexicans and Puerto Ricans are equally likely to be employed (p=0.76), high school dropouts (0.38), college graduates (0.52), or veterans (0.16); or that they have equal annual earnings either unconditionally (0.09) or conditional on employment (0.18).

Table 2 reports summary statistics for the areas in which the men live, defined either as their metro area or as a separate area consisting of everyone in that state who lives outside any metro area. The first group of variables listed are controls (W in the notation above), and the rest are used to create marital status instruments (Z's). Perhaps the most notable feature is the wide variation in the immigrant/native ratio among local single Hispanic women (Z^1), which is reassuring considering that this variable plays the primary role in predicting intermarriage.

4. Empirical Results

Table 3 presents estimates of the intermarriage premium from our main sample. Ordinary

least-squares (OLS) estimates indicate intermarried Mexican men earn a statistically significant 6.3 percent more than comparable men married to immigrants (who earn 14.4 percent more than singles), but Puerto Ricans' 0.7 percent intermarriage premium is statistically insignificant. The OLS estimate of the green card premium Γ is thus 5.6 percent.

[Table 3 about here.]

The other columns of Table 3 report estimates from the IV method described above. The middle column uses instruments computed from a single multinomial logit on the pooled sample, while Column 3 – our preferred estimates – uses separate multinomial logits for Mexicans and Puerto Ricans. The two IV estimates of $\widehat{\Gamma}$ are fairly similar (40.2 and 31.5 percent), as are the $\hat{\gamma}$'s for Mexicans (42.5 and 40.3 percent) and Puerto Ricans (2.3 and 8.8 percent, neither statistically significant). The Mexican estimate is a bit larger than estimates for Australia and France (Meng and Gregory 2005, Meng and Meurs 2009), but similar to Chi's (2013) estimate from the IRCA.

4.1 Remarks on the Main Estimates

4.1.1. Selection. The estimates in Table 3 (and below) shed light on selection into marriage and intermarriage. If married men were positively selected on unobservables, OLS estimates of a group's marriage premium ($-\delta_g$) should exceed IV estimates, which is what we find for Mexicans. For Puerto Ricans there is little compelling evidence of selection into marriage, and while estimates of their intermarriage premium (γ_P) suggest negative selection, the differences are not large or statistically significant.[18]

Estimates for Mexicans strongly suggest negative selection into intermarriage, however. All 20 IV estimates of γ_M in Tables 3-5 are much larger than the OLS estimate. This should not be surprising, as negative selection has been found in previous studies of intermarriage in the U.S. and other nations (Meng and Gregory 2005, Furtado and Theodoropoulos 2009, Meng and Meurs 2009), and it is consistent with the hypothesis that Mexican men may become more eager to marry a native wife when a green card would allow them to escape a low-paying job.

4.1.2 Strength of excluded variables. The lower panel of Table 3 reports first-stage F-

[18] While our estimates of δ_P are toward the high end of previous estimates of the marital wage premium, they are in line with estimates at lower quantiles of the wage distribution (Maasoumi et al. 2009).

statistics. All are much larger than Stock and Yogo's (2002) guideline of 10, indicating the logit fitted values are strong instruments.

It is also instructive to compare our IV estimates to those from the standard IV procedure, i.e., using Z directly as instruments rather than excluded variables in a 0th-stage logit. $\widehat{\Gamma}$ is then 96.5 (SE=132.6) if the Z's are the only instruments, 43.7 (64.1) if we use $Z$and τZ, and 48.2 (21.8) if Z interacts with a full set of τ dummies. If anything this suggests our preferred estimate may be conservative, though we hesitate to put much faith in such imprecise results.[19]

4.1.3 Estimates without exclusions. One may also worry that the non-linearity of the 0th stage would permit estimation even if the specification lacked any exclusion restrictions. Of course, most scholars would dismiss such results as an artifact of functional form assumptions, so we seek reassurance that our estimates are driven by the intended sources of variation.

One way to investigate is simply to run the procedure described above, i.e., using the same explanatory variables in all three stages, with no exclusions. The estimates should not change much if the original model had mainly been identified from the 0th stage non-linearity, but we would expect them to be substantially different (likely smaller) and less precise if the Z's had been the main source of identification. Results strongly support the latter prediction: $\widehat{\Gamma}$ falls from 31.5 percent to 7.6, while the standard error rises from 11.4 to 19.9. $\widehat{\Gamma}$is also robust to both (a) adding quadratic terms in the non-dummy controls (31.3, SE=11.2) and (b) using a multinomial probit (not a logit) in the 0th stage (34.3, 11.7), again suggesting little cause for concern.

4.1.4 Shorter marriage specifications. To gain further insight into the sources of variation that determine our estimates, we have also run some modified specifications that maintain the same model of earnings from Table 3, but predict marital status in the 0th-stage using a sparse set of explanatory variables. If we use *only* the Z's and a battery of τ dummies to predict marital status, estimates of the green card premium become larger, though imprecise (58.1, SE=24.4), and they become similar to those in Table 3 (39.6, 13.9) if we add just a few more dummies for broad

[19] As expected, the imprecision arises because a linear probability model does not fit the first-stage very well. Compared to our preferred method, standard IV yields four times as many negative first-stage fitted values (13.2 percent versus 3.4), and 59 times as many fitted values less than -0.03 (5.9 percent versus 0.1). Even so, the first-stage F-statistics continue to indicate the instruments are strong, ranging from 33 to 156.

age and education categories.[20] It thus appears that our estimate results mainly from the intended source of variation, and the primary effect of including other controls in the 0th-stage is just to improve the model's predictive power and thereby strengthen the instruments $(\ddot{N}, \ddot{D})$.

4.2 Robustness

4.2.1 Alternate excluded variables. Given the potential controversy with any instrument, we next demonstrate that similar estimates emerge if we instead use several different pairs of Z's.

The first two columns of Table 4 address the fear that demographically-based Z's may be spuriously correlated with immigrants' wages because local economic shocks attract immigrants and thus raise Z^1.[21] Since Z^1 is an inverse predictor of intermarriage, this would mean that the same shock that raised immigrants' wages would also reduce their predicted probability of intermarriage—causing a downward bias in the intermarriage premiums γ.

[Table 4 about here.]

Column 1 addresses this possibility by recomputing our preferred Z's using metro areas' data from the 1990 Census ($\equiv Z_{90}$), which would resolve the issue if 1990 demographics were unrelated to local shocks that create a spurious correlation in 2000. Consistent with the proposed bias, point estimates of both intermarriage premiums are higher by 0.075 here than in Table 3, but $\widehat{\Gamma}$ is unaffected (since the change in $\hat{\gamma}$ is the same for both groups). Column 2 instead confronts the migration mechanism directly by using current (2000) data for metro areas where the men lived in 1995 ($\equiv Z_{95}$) and dropping men who lived abroad in 1995 (17 percent of our main sample). About 1/6 of those who remain moved between U.S. metro areas, so for them Z_{95} reflects demographics of a place where they no longer live. Nonetheless, the new estimates are very similar to those in Table 3.[22] It is also notable that Z^1 is more strongly correlated with

[20] This specification adds only dummies for 5-year age ranges and four education groups (college, some college, high school, and less than high school). Their inclusion nearly doubles the pseudo-R^2 of the Mexicans' logit (from 0.12 to 0.20), triples it for Puerto Ricans (0.05 to 0.15), and raises the first-stage F-statistics by 40 to 462 percent.

[21] The raw data lend some credence: for men who moved between U.S. metro areas during 1995-2000, the mean wage of men with the same level of education was 1.2 percent higher in the new area, and Z^1 was 48 percent higher.

[22] While the intermarriage premiums are larger in Table 3, this is mainly because the main sample includes recent immigrants, who earn higher premiums. If we re-estimate the model from Table 3 with the more restrictive sample used here, Mexicans' intermarriage premium is 0.341 (0.107) and the green card premium is 0.288 (0.117). Doing the same for the exercise in column 1 yields estimates of 0.420 (0.122) and 0.293 (0.117), respectively.

Z^1_{95} (ρ=0.81) than Z^1_{90} (ρ=0.48),[23] as it suggests variation in Z^1 owes less to selective internal migration (causing Z^1_{95} to differ from Z^1) than to intertemporal fluctuations (causing Z^1 to differ from Z^1_{90}). If so, this would seem to limit the relevance of the proposed mechanism anyhow.

The exercises in the rest of Table 4 are not motivated by a specific potential bias, but rather provide comparison with instruments from earlier work and illuminate our empirical strategy. Columns 3-5 report results from the three sets of alternate Z's described in Section 2.1, and column 6 uses all 12 Z's (including the main pair and those from columns 1-2). The alternate Z's are not highly correlated with our preferred set ($|\rho| \leq 0.31$ in each case), yet the $\hat{\gamma}$'s vary only modestly, those for Puerto Ricans remain statistically insignificant, and all $\hat{\Gamma}$'s are still near 0.30.[24] This should dispel any fear our findings rely heavily on choices between instruments.

4.2.2. Alternate samples. The top two panels of Table 5 consider alternate sample inclusion criteria. The first two columns in Panel A relax our restrictions on men's marital status: first by adding as a fourth marital group men dropped from the main sample because they were divorced, widowed, or separated; and then by adding men married to non-Hispanic immigrants. The next column uses only men who speak English well or better, and the last column drops men from the three largest cities, where more than 20 percent of our sample resides. Panel B then explores our restriction on the age at which the men immigrated. The first column includes men who entered the U.S. at any age (not just by 25), while the second retains only men who arrived by age 18 to reduce the chance they were already married. The third and fourth split the sample into those who arrived after- and by age 6 to explore the influence of nearly life-long residents.

Except for the very last case, the estimated green card premiums remain between 27 and 39 percent and statistically significant. Results are also similar if we pool previously-married men with never-married men or exclude only men who speak no English (few of whom intermarry).

[Table 5 about here.]

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[23] Z^2 is more autocorrelated (ρ=0.85), but note that the proposed bias is expected to arise through Z^1 anyhow.

[24] The tight range of estimates suggests an overidentification test is unnecessary here, which is fortunate because the non-linearity of the 0th-stage complicates such a test. The most complete test would involve constructing separate logit fitted values for each possible subset of Z's. We are using six Z's to predict each margin, so there are $4*[2^6 -1]^2$ =15,876 possible instruments for the four endogenous variables—more than twice the sample size of Puerto Ricans.

The estimates for men who entered the U.S. early in life also speak to the mechanisms at work. If the difference in the groups' intermarriage premiums arose because native wives' social networks were more valuable to Mexicans, we would expect smaller estimates for men who had many years to build their own networks. Yet Table 5 shows a *larger* $\widehat{\Gamma}$ for men who immigrated by age 6, and it is even larger (1.13, SE=0.58) for the subgroup who arrived after 1982 and thus did not benefit from the IRCA. This is not an age effect: $\widehat{\Gamma}$ is just 0.02 (0.55) for similarly-aged men (18-24) who arrived by age 6 *before* 1982. Thus, intermarriage is evidently quite valuable even for Mexicans who lived nearly their whole lives in the U.S., but not for Puerto Ricans or Mexicans known to have obtained green cards in another way. This seems difficult to reconcile with a network-based mechanism, but quite natural if the differences were due to green cards.

On a related note, some may be concerned about the inclusion of the youngest men in our sample. Such men are relatively unlikely either to participate in the labor force or to marry, so those who do may be strongly selected. Others may prefer part-time jobs, so their actual earnings understate their potential. There may also be some fear due to the fact that many young men have not completed their schooling, although note that we control for men's current level of schooling and that few men in our sample are currently enrolled, especially among those over 18.[25]

Table 6 should allay such fears. Each row reports $\widehat{\Gamma}$ from samples with the same minimum age, while each column uses a different maximum age. The right-hand panel repeats the exercise excluding men currently enrolled in school. All estimates are significant at the 5 percent level.

[Table 6 about here.]

We draw three main conclusions. First, the mild variation within each column shows that $\widehat{\Gamma}$ is not sensitive to minimum age we allow. Second, the fact that $\widehat{\Gamma}$ becomes smaller as we raise the maximum age suggests our preferred estimates are conservative and is consistent with the idea that green cards are more beneficial for younger men and/or those who arrived recently. Finally, $\widehat{\Gamma}$ is only a bit smaller when we exclude men enrolled in school, especially when the sample includes men over 35. Thus, while we prefer to include enrolled men because schooling is

[25] In our data, enrollment rates are respectively 39, 10, and 5 percent for men aged 16-18, 19-25, and 26-44.

a potential mechanism linking green cards and wages, our results are robust to that choice.

4.2.3. Expanded controls. The lower panel of Table 5 presents results from specifications with expanded sets of control variables. The first column adds dummies for the men's states, and the second adds ten variables detailing employment shares across local one-digit industries and local employment-population ratios for adults aged 16-65;[26] neither has much effect on $\widehat{\Gamma}$. The last two columns add dummies for workers' industries and each level of English fluency (not a binary variable). Although this in effect controls for ways in which green card may affect wages, $\widehat{\Gamma}$ declines only modestly (to 27 and 24 percent) and remains statistically significant, suggesting green cards have substantial benefits even beyond their effect on workers' language or industries.

In summary, Tables 4 through 6 confirm that the IV estimates from Table 3 are robust. Only one of the 28 other reported estimates of Γ for men aged up to 44 differs from our preferred estimate (0.315) by more than 25 percent (0.077), while 19 of those alternate estimates differ from it by less than nine percent (0.028), and the median difference is just 0.021.

4.3 Comparison with estimates for men from other Latin American countries

Insofar as there truly is a pan-Hispanic marriage market in the U.S., we can use the same method to estimate Γ for men from other Spanish-speaking Latin American nations. This serves as another check on our estimation strategy because there are good reasons to anticipate variation.

The right-hand columns of Table 7 documents such differences across several countries. The first five columns report characteristics of immigrants who received green cards in 2000.[27] The first indicates a striking difference: 69 percent of Cuban green card recipients previously had refugee or asylee status, but that fraction did not exceed 4 percent anywhere else. This is significant because refugees and asylees are allowed extensive access to the U.S. labor market, so a green card presumably has less incremental value for them.

[Table 7 about here]

[26] This addresses fears that local demographics and marital status may be correlated because both are affected by industrial composition, such as if many Mexicans lived in places with a relatively large agriculture sector, agriculture were in long-run decline, and this discouraged marriage and encouraged young people to move away.

[27] The first five measures are computed from data published by the INS (2002) and are based on all immigrants from those countries (including women, children, older men, etc.), except as indicated. The last measure is computed directly from the sample used to estimate the green card premiums.

Other factors vary more widely. The second column reports shares of new green card recipients who entered the U.S. within the previous five years, excluding those who arrived with one and assuming those with unknown entry dates had waited more than five years. There is nothing special about the five-year window – analogous measures for periods of different lengths are highly correlated, so this variable just summarizes the typical delay before acquiring a green card; presumably an expedited green card would be more beneficial to those who face longer waits. The third and fourth columns list shares of green cards granted via employment-based criteria and shares of employed green card recipients who already worked as professionals or executives.[28] Since they held those positions *before* getting a green card, we conjecture that the ability to move to new occupations is less valuable for them. The fifth column documents the share of green cards granted to persons who initially entered the U.S. without inspection;[29] larger values suggest more green cards go to persons with much to gain.[30] This last column lists shares of married male immigrants with non-Hispanic wives, an indication our empirical approach is less appropriate.

Perhaps surprisingly, these variables are highly correlated. Argentina, Chile, Panama, and Venezuela all rank in the top six countries (out of 15) on all five factors expected to yield smaller estimates of Γ. Cuba, Costa Rica, and Colombia are in the next stratum, while Mexico and most of Central America generally fall the other end of the spectrum. While these correlations preclude us from assessing factors' relative importance, they imply a clear hierarchy of expected premiums.

The left-hand columns of Table 7 evaluate those predictions by re-estimating Γ on a sample that now adds 34,379 men from other Spanish-speaking Latin American nations.[31] As predicted, estimates for men from El Salvador, Guatemala, Honduras, and Nicaragua are at least as large as Mexicans', those for the most dissimilar nations (Argentina, Chile, Cuba, Panama, Venezuela) are

[28] For example, some green cards are issued to workers transferred to U.S. offices of international companies.
[29] Most likely remained undocumented until they received the green card, but it is possible some may have acquired a legal visa in the meantime. Conversely, it is not uncommon for people to become undocumented after entering the U.S. legally (e.g., if they overstay a student or tourist visa), but such people would not be counted in this measure.
[30] This measure is only weakly correlated ($\rho = 0.22$) with shares of foreign-born populations that are undocumented (INS 2003b), but it is the more relevant concept here: estimates of Γ need not reflect potential benefits to workers who lack green cards, but should be larger when more green cards go to workers with large potential benefits.
[31] Bolivia, Paraguay, and Uruguay are excluded due to small sample sizes (483 observations in total). Results are very similar to those reported if those observations are instead combined with neighboring countries'.

smaller and statistically insignificant, and the expected group falls in the middle.[32] The final line shows that $\widehat{\Gamma}$ is highly correlated with most features, often with $\rho > 0.6$. For the refugee/asylee measure, the key point is that $\widehat{\Gamma}$ is statistically insignificant for Cuba.

We draw two main conclusions. First, while one might have feared that we had overstated Γ for Mexicans, these results indicate that our estimates are in line with those from the most similar source countries–perhaps even smaller than one might expect. Second, the fact that we find small, statistically insignificant estimates where they were expected demonstrates that our method is able to distinguish effects of varying magnitudes and that large estimates are not inevitable.

4.4 Evidence from the IRCA

It is also possible to use one subgroup of Mexicans as a control group for another. As Chi (2013) has noted, Mexicans who entered the U.S. before 1982 were eligible for green cards via the IRCA, so they would presumably have one regardless of whether they were intermarried. She thus estimates the effect of legal rights as a difference-in-difference-in-differences, comparing the intermarriage premiums of pre- and post-1982 Mexican immigrants. Using data from the 1990 Census, she finds the latter group earns an additional premium of 35 percent.

Table 8 presents comparable estimates obtained by applying Chi's strategy to our data from 2000. The samples also include Puerto Ricans, so we can compute $\widehat{\Gamma}$ using either the pre-1982 Mexicans or Puerto Ricans as the control. When estimated on the entire sample, $\widehat{\Gamma}$ is somewhat larger than before (46 and 53 percent), and $\widehat{\gamma}$ is again large and statistically significant for post-1982 Mexicans, but insignificant for both control groups.

[Table 8 about here.]

The other column of Table 8 addresses the potential objection that $\widehat{\Gamma}$ may be biased upwards if the true premium declines with immigrants' time in the U.S. (as Table 9 will suggest) by limiting the sample to men who entered the U.S. before 1990; this also fosters comparison with Chi's estimates using 1990 data since all such men were in the population from which her sample

[32] The estimate for Ecuador is admittedly larger than one might expect given the other variables. It is similarly anomalous even under OLS, however, so it does not seem to have been introduced by our estimation procedure.

was drawn.[33] The new $\widehat{\Gamma}$ is 40 percent using Puerto Ricans as the control group, or 28 percent using pre-1982 Mexicans – only a bit less than Chi's result from ten years earlier.

4.5 Estimates across Subgroups

Other interesting patterns emerge in the top panel of Table 9, where we partition the data by variables correlated with immigrants' legal status. The first comparison is a placebo test of our identifying assumption that, except for green cards, intermarriage provides the same benefits to Puerto Ricans and observably similar Mexicans. Since citizens have the same rights regardless of how citizenship was acquired, we would not expect naturalized U.S. citizens to earn a larger intermarriage premium than Puerto Ricans, and indeed $\widehat{\Gamma}$ is just 4.6 percent and statistically insignificant when our sample includes only naturalized Mexicans and Puerto Ricans. However, despite controlling for a potential mechanism (subsequent citizenship) by which green cards may raise wages, $\widehat{\Gamma}$ rises to 25.3 percent (p=0.04) when we instead exclude naturalized Mexicans.

[Table 9 about here.]

The next two rows explore the value of green cards for men who were previously undocumented. While a green card would provide them with a larger set of new rights, their wages may not rise much if they remain in occupations commonly held by undocumented workers, as those labor markets are often more competitive and feature less wage heterogeneity (Kossoudji and Cobb-Clark 2000; Bratsberg et al. 2002; Lozano and Sorensen 2011). To investigate, we isolate a set of large occupations often associated with undocumented workers, including jobs in agriculture, restaurants, and construction.[34] In our data each is held by at least 1,000 Mexicans, most are more common among Mexicans than Puerto Ricans (e.g., they are seven times as likely to be drywall installers or farm workers), and they employ 48 percent of Mexicans (62 percent of those who entered after 1995) but only 23 percent of Puerto Ricans.

Estimated green card premiums are 2.5 percent and statistically insignificant for men who work in the selected jobs, while others earn a 27 percent premium. Again, this does not mean

[33] Of course, our sample may differ from hers due to post-1990 mortality, migration, or changes in marital status.
[34] The occupations we consider are butchers and meat cutters; carpenters; construction laborers; cooks; drywall installers; farm workers; gardeners and groundskeepers; janitors; non-construction laborers; machine operators; masons, tilers, and carpet installers; food prep workers; painters; roofers and slaters; waiters; and waiters' assistants.

green cards are less valuable to undocumented workers (not even just in terms of wages), as this approach would miss the likely large wage gains caused by migration between the two sets of jobs. Rather, we conclude that green cards do not substantially raise the wages of previously undocumented workers who do not take advantage of their newfound mobility.

The next set of results is stratified by men's education. This is relevant in light of recent proposed reforms that aim to attract skilled immigrants; most debate presumes the increase in mobility that comes with green cards would be more valuable for college grads, as it is among U.S. natives (Tchernis 2010). Our results confirm that view: college grads earn larger premiums than high school dropouts (0.546 (0.239) versus 0.146 (0.182)), and given their higher mean earnings, this implies their annual earnings rise eight times as much: $23,590 versus $3,010.

4.6 Estimates by Time since Immigration and Potential U.S. Experience

The Panel B of Table 9 stratifies the sample by the time since the men entered the U.S. Men who arrived up to 8 years earlier receive very large green card premiums: point estimates imply premiums of 100 percent ($=e^{0.692}-1$) for those who arrived within the previous five years and 134 percent for those who arrived 5-8 years earlier. In contrast, estimates for men who arrived 9-12 years earlier are more typical in size and only marginally significant (p=0.08), and the estimate for those who arrived earlier yet (but still after 1982) is small and insignificant.

The last result may seem at odds with the large estimates in Section 4.2.2 for men who arrived by age 6 after 1982, but they can be reconciled by noting that here the sample also includes men who arrived *after* age 6, whose premiums are small and insignificant (0.041, SE=0.258). One might then wonder whether the pattern in Panel B arises because premiums accrue only to the youngest men, rather than those who arrived most recently, yet that conjecture is also problematic: $\widehat{\Gamma}$ is also large (0.81, 0.31) for the *oldest* men in our data (ages 23-29) who arrived after 1995, but small and statistically insignificant for others in that age range.

A more promising hypothesis may be that green cards become less valuable as workers' careers progress. Panel C thus reports $\widehat{\Gamma}$ stratified by "potential U.S. experience," defined as the minimum of (a) the usual measure of potential experience (age - years of schooling - 6), (b) the

number of years since the man entered the U.S. (since he may have arrived after completing his education), and (c) his age minus 16 (in case he left school at a young age). The results are stark: $\widehat{\Gamma}$ is very large (1.00, SE=0.32) over the first six years of men's U.S. careers, but zero thereafter.

There are at least two plausible explanations. First, since men often change jobs frequently early in their careers before settling into a more stable position (Topel and Ward 1992), green cards may become less valuable as workers' desired mobility falls. Alternatively, the decline may reflect non-intermarried men gradually acquiring green cards by other means. In that case the pattern would not reflect a decrease in the value of a green card, but rather our strategy's waning ability to measure it – although we could still appropriately interpret our estimates as the labor market value of receiving a green card *faster than usual*.[35]

Whichever interpretation one prefers, our results suggest that Mexicans' earnings would rise by $120,000-$150,000 in present value if they obtained a green card immediately upon arrival in the U.S. For instance, if we take the results in Panel B at face value, the last column shows they imply that men's annual earnings rise by $13,800 over the first four years and $22,500 over the next four. At a five percent discount rate, this amounts to $120,000 in present value, or $145,000 if we add the marginally significant estimate for the third four-year period. Likewise, the result from Panel C corresponds to a $130,000 gain in the present value of earnings over the first six years of a typical man's U.S. career (and none thereafter). If we accept that earnings premiums only persist for 8-12 years after men enter the U.S., our preferred estimate (Table 3) also leads to similar conclusions. It corresponds to a mean annual gain of $7,600 over all Mexicans, so if none of it accrued to men who arrived more than 8-12 year earlier (46-65 percent of our sample), the rest must receive $14,200–$22,000, which again amounts to $130,000-$150,000 in present value.

5. Summary and discussion

In summary, despite several conservative biases, we find consistent evidence that Mexican-born men with green cards earn about 30 percent more than they would without one. This finding

[35] One might also wonder whether the patterns in Panels B and C could reflect period effects associated with events like the 1994 Mexican peso crisis or the North American Free Trade Agreement. This seems unlikely, as (e.g.) the results in Panel B imply large effects even for men who immigrated in the late 1980s, well before those episodes.

proved robust to many alternate instruments, sample inclusion criteria, and control variables, and it is consistent with estimates from some (though not all) previous studies of green cards, studies of job mobility for U.S. natives, and studies of intermarriage premiums in other nations. As one might expect, we find the largest premiums accrue to groups for whom mobility is likely most valuable, including college graduates and those beginning their careers.

A more complete welfare analysis would consider whether the higher wages were paid from rents that currently accrue to the men's current employers or from efficiency gains. While the latter would be a pure welfare gain, the former may be more controversial because it represents a transfer from employers to the immigrants. Such a transfer would surely be opposed by those employers, and many U.S. lawmakers may find it unattractive as well.

The relative sizes of those components depend somewhat on whether an immigrant without a green card happens to work for the employer for whom his service is most valuable. If so, the green card premium would likely come mainly from reduced monopsony rents, though a portion could reflect efficiency gains caused by increased labor supply at the intensive margin.

Alternatively, if a green card enabled the worker to form a better employment match, there is an additional efficiency gain. Denote workers' original marginal product and wage respectively by μ and w, and use asterisks to represent those variables with the new employer. The green card premium ($w^* - w$) can be decomposed into rents lost by the old employer ($\mu - w$) and workers' share of efficiency gains ($w^* - \mu$). (The new employer may also receive a share of efficiency gains, $\mu^* - w^*$.) If the old employers match outside offers up to workers' marginal product, we can approximate ($w^* - \mu$) by wage increases that arrive when workers move to new jobs. Topel and Ward (1992) find that 40 percent of young native men's wage growth occurs at job changes, and if the same were true for immigrants, the smallest present value calculation above suggests a policy of granting green cards immediately upon arrival would increase efficiency by about $50,000 per Mexican man, with the remaining $70,000 or so coming from employer rents.

Although these are obviously imprecise, lower-bound estimates, they cast an interesting light on Gary Becker's (2011) proposal to replace the U.S.'s current quota-based immigration system

with a market-based plan in which green cards would be granted immediately to anyone (except, e.g., criminals or spies) who paid a fee of about $50,000.[36] We suspect many may view such a large fee as exploitative, yet the calculation above suggests it may be much less than the amount many immigrants currently lose while waiting for a green card. In that view, the plan would transfer to the government much of the rents employers receive as a result of the current policy, with immigrants retaining a portion of the rents and the increase in efficiency ($w^* - \mu$).

One fruitful question for future work is whether green cards raise employment. We have briefly explored the issue by repeating our analysis with measures of employment as dependent variables. Contrary to the results for wages, those estimates indicate that intermarried men from both Mexico and Puerto Rico are substantially more likely to be employed, echoing Furtado and Theodoropoulos's (2010) conclusion that this premium owes mainly to employment networks rather than legal rights. Still, it seems surprising for wages and employment to be affected by different mechanisms, and further evidence would help to confirm and/or resolve that paradox.

Finally, a word is in order about the generality of our results. Since they are based only on young men from Mexico and Latin America working in the U.S., and particularly since we found notable variation across source countries, it remains to be seen if older workers, women, or men born elsewhere receive similar gains. While some of our results suggest others may benefit more, especially those with more education, it seems likely that permanent status is less valuable to older workers considering that they are less likely to take advantage of the increased mobility.

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Table 1: Summary Statistics, Single and Married Immigrant Men from Mexico and Puerto Rico Aged 16-44

A. Men's Own Characteristics

	Annual Earnings		Log Weekly Earnings		Age		Years Since Migration		Age at Migration		Speaks English "well" or better		Educational Attainment				Number of Obs.
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	High Sch. Dropouts	High Sch. Graduates	Some College	College Graduates	
Mexican:																	
Single Men	16,184	18,121	5.75	0.65	24.78	5.80	9.13	7.26	15.65	5.83	0.47	0.50	67.0%	20.8%	9.6%	2.6%	44,537
All Married Men	24,693	23,618	6.11	0.67	32.24	6.19	15.54	7.61	16.70	5.40	0.59	0.49	68.6%	18.3%	10.1%	3.0%	56,666
Married to a Hispanic Immigrant	24,138	23,357	6.09	0.67	32.47	6.11	15.20	7.24	17.27	4.87	0.54	0.50	71.6%	17.3%	8.6%	2.5%	45,977 (81%)
Married to a Native	27,136	24,586	6.18	0.65	31.22	6.44	17.05	8.90	14.17	6.73	0.80	0.40	55.5%	22.8%	16.7%	5.0%	10,689 (19%)
Married to a Hispanic Native	26,015	23,068	6.15	0.65	31.00	6.45	17.12	8.83	13.88	6.73	0.77	0.42	59.1%	21.7%	15.3%	3.9%	8,177 (77%)
Married to a Non-Hispanic Native	30,744	28,648	6.26	0.63	31.93	6.34	16.85	9.13	15.08	6.65	0.93	0.26	44.0%	26.1%	21.5%	8.5%	2,512 (23%)
Puerto Rican:																	
Single Men	19,645	21,908	5.85	0.81	26.84	7.11	15.27	9.44	11.57	7.70	0.88	0.32	37.5%	27.6%	23.8%	11.1%	3,341
All Married Men	32,791	29,543	6.34	0.69	33.88	6.52	19.99	10.26	13.89	7.79	0.90	0.29	27.9%	30.2%	28.2%	13.6%	3,550
Married to a Hispanic Immigrant	31,299	27,703	6.32	0.71	33.92	6.58	18.58	9.97	15.33	7.43	0.87	0.34	30.5%	30.4%	26.0%	13.0%	2,056 (58%)
Married to a Native	34,847	31,799	6.37	0.66	33.82	6.46	21.91	10.33	11.91	7.85	0.95	0.21	24.3%	30.0%	31.3%	14.5%	1,494 (42%)
Married to a Hispanic Native	31,761	24,902	6.32	0.65	33.38	6.54	21.35	10.48	12.04	7.90	0.93	0.25	29.8%	33.2%	29.0%	8.0%	890 (60%)
Married to a Non-Hispanic Native	39,373	39,398	6.44	0.67	34.45	6.27	22.74	10.06	11.72	7.77	0.99	0.11	16.1%	25.3%	34.6%	24.0%	604 (40%)

B. Wives' Characteristics

	Annual Earnings		Percent employed		Annual Earnings, if positive		Age		Age difference (husband-wife)		Absolute age difference		Educational Attainment				Number of Obs.
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	High Sch. Dropouts	High Sch. Graduates	Some College	College Graduates	
US-born Wives Married to:																	
Mexicans	12,437	16,838	0.57	0.50	17,705	17,617	30.10	7.94	1.18	5.34	3.97	3.77	34.4%	31.0%	28.0%	6.6%	10,689
Puerto Ricans	18,269	22,674	0.66	0.47	23,092	23,206	32.61	7.08	1.30	4.64	3.49	3.32	16.1%	29.0%	38.0%	16.9%	1,494
Wives Born Abroad, Married to																	
Mexicans	6,503	14,067	0.35	0.48	13,751	17,853	30.83	6.74	1.73	4.62	3.66	3.31	70.4%	18.0%	8.7%	2.8%	45,977
Puerto Ricans	13,558	17,400	0.56	0.50	19,645	17,864	33.36	7.55	0.55	4.93	3.53	3.48	28.2%	28.4%	29.1%	14.4%	2,056

Notes: Data are from the 5% Public Use Microsample of the 2000 U.S. Census of Population. All calculations use person-level sample weights. The sample excludes men who are divorced, widowed, married to a non-Hispanic immigrant, or married to an absent spouse.

Table 2: Summary Statistics for Metro Areas

Variable		Mean	SD
Share of Hispanic immigrants in population		0.06	0.08
Employment-population ratio (ages 16-65)		0.69	0.05
Employment shares, by industry:			
Agriculture, forestry, fishing, and mining		0.03	0.02
Construction		0.07	0.02
Manufacturing		0.15	0.07
Transportation, communications, and public utilities		0.07	0.02
Wholesale and retail trade		0.21	0.02
Finance, insurance, and real estate		0.06	0.02
Business services		0.05	0.01
Personal services		0.03	0.01
Entertainment and recreation		0.02	0.01
Professional services		0.26	0.04
Public administration and military		0.06	0.05
Excluded exogenous variables (labelled by set):			
Main set	Immigrant/native ratio among single Hispanic women	0.74	0.67
	Share of women who are single	0.46	0.05
1990 Census	Immigrant/native ratio among single Hispanic women	0.58	0.84
	Share of women who are single	0.41	0.05
1995 metro	Immigrant/native ratio among single Hispanic women	0.74	0.67
	Share of women who are single	0.46	0.05
Meng and Gregory	Share of all single women who are Hispanic immigrants	0.04	0.06
	Sex ratio (women/men) among single Hispanic immigrants	0.62	0.50
Kantarevic	Hispanic immigrant/native ratio among single women / same ratio for US	0.77	1.33
	Sex ratio, including both single and married individuals	1.03	0.05
Other	Share of single U.S.-born women who are of Hispanic origin	0.08	0.14
	Sex ratio (women/men) among singles	0.93	0.08

Notes: Data are computed from the 5% Public Use Micro Sample of the 2000 U.S. Census, except for the two variables (labelled) computed from the 1990 Census. The sets of excluded exogenous variables are labelled in accordance with Table 6. Most statistics are very similar if weighted by the number of men in the sample, except that the mean immigrant shares rise (i.e., immigrant men tend to live in places with many immigrants).

Table 3: OLS and IV Estimates of the Wage Equation

	(1)			(2)			(3)		
Description:	**OLS**			**IV - instruments are fitted values from a single multinomial logit on the pooled sample**			**IV - instruments are fitted from separate multinomial logits for each group (preferred specification)**		
A. Estimates for the main equation (dependent variable is log weekly wages)									
Variable	**Est. (x100)**	**SE (x100)**	**P**	**Est. (x100)**	**SE (x100)**	**P**	**Est. (x100)**	**SE (x100)**	**P**
1. Intermarried dummy									
Men born in Mexico	6.3	0.9	0.00	42.5	11.5	0.00	40.3	9.9	0.00
Men born in Puerto Rico	0.7	2.3	0.76	2.3	20.2	0.91	8.8	12.8	0.49
Difference ("Green Card Premium")	5.6	2.6	0.03	40.2	15.1	0.01	31.5	11.4	0.01
2. Single dummy									
Men born in Mexico	-14.4	0.6	0.00	-0.8	13.8	0.95	-5.8	8.2	0.48
Men born in Puerto Rico	-25.9	2.1	0.00	-27.2	19.3	0.16	-29.3	9.7	0.00
R^2	0.18			0.16			0.16		
Observations	108,094			108,094			108,094		

B. Results from First Stage of IV Regressions

Endogenous variable	R^2	**F**	**P**	R^2	**F**	**P**
Intermarried dummy						
Mexicans	0.11	411.1	0.00	0.11	481.2	0.00
Puerto Ricans	0.29	196.6	0.00	0.31	208.9	0.00
Single dummy						
Mexicans	0.36	4,476.3	0.00	0.36	3,630.5	0.00
Puerto Ricans	0.60	681.6	0.00	0.61	754.5	0.00

Notes: The reported results are from regressions of men's log weekly wages on dummies for intermarriage and single status, as well as control variables listed in the main text. (The excluded category is thus men married to wives born abroad.) The main object of interest is the difference between Mexicans' and Puerto Ricans' intermarriage premiums, which we attribute to green cards. The first column reports OLS estimates, while the second and third report IV estimates, where there instruments are fitted values from 0th stage multinomial logits of men's marital statuses on the excluded variables and the other controls. Estimates in the second column use fitted values from a single logit on the full sample, while the last column (our preferred estimates) begins with separate logits for Mexicans and Puerto Ricans. In each case, the pseudo-R^2 of the logits are about 0.2. The excluded variables used to predict intermarriage and single status are (1) ratio of immigrants to natives among local unmarried Hispanic women, and (2) share of all local women aged 16-44 who are unmarried. All estimates use person-level sample weights, and standard errors correct for non-independence (clustering) of observations within metropolitan areas.

Table 4: Selected IV Estimates from the Earnings Equation When Using Alternate Excluded Exogenous Variables

	(1)			(2)			(3)			(4)			(5)			(6)		
Intermarriage Premium	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**
Men born in Mexico	47.8	10.9	0.00	36.1	10.4	0.00	46.6	11.1	0.00	44.8	11.0	0.00	38.8	11.0	0.00	31.8	10.1	0.00
Men born in Puerto Rico	16.3	13.8	0.24	4.9	13.2	0.71	13.8	13.6	0.31	11.8	13.6	0.39	7.6	13.4	0.57	4.0	12.5	0.75
Difference in Differences	31.6	11.6	0.01	31.2	11.7	0.01	33.0	11.8	0.01	33.0	11.8	0.01	31.2	11.6	0.01	27.8	11.4	0.02
R^2		0.15			0.14			0.15			0.15			0.16			0.15	
Observations		107,082			89,754			108,094			108,094			108,094			88,997	
First-stage IV regressions	R^2	F	P	R^2	F	P	R^2	F	P	R^2	F	P	R^2	F	P	R^2	F	P
Intermarriage - Mexicans	0.11	501.1	0.00	0.11	377.0	0.00	0.11	268.6	0.00	0.11	310.6	0.00	0.11	263.7	0.00	0.11	435.2	0.00
Non-intermarriage - Mexicans	0.36	3,814.3	0.00	0.30	3,851.3	0.00	0.36	4,107.8	0.00	0.36	3,996.9	0.00	0.36	4,154.4	0.00	0.30	3,939.7	0.00
Intermarriage - Puerto Ricans	0.31	210.7	0.00	0.32	195.0	0.00	0.31	199.7	0.00	0.31	202.9	0.00	0.31	205.5	0.00	0.32	198.6	0.00
Non-intermarriage - Puerto Ricans	0.61	755.7	0.00	0.61	591.8	0.00	0.61	676.1	0.00	0.61	698.7	0.00	0.61	698.4	0.00	0.61	646.8	0.00
Excluded variables for multinomial logit	**Baseline set, using location's 1990 data**			**Baseline set, using individual's location 5 years earlier**			**Meng and Gregory's (2005) instruments**			**Kantarevic's (2004) instrument plus sex ratio**			**(Two other candidate excluded variables)**			**All 12 candidates**		
	(1) Immigrant/native ratio among local unmarried Hispanic women			(same; sample excludes persons abroad 5 years ago)			(1) Share of local unmarried women who are Hispanic immigrants			(1) Ratio of Hispanic immigrants/natives among local unmarried women / same ratio for entire US			(1) Share of local native unmarried women who are of Hispanic descent					
	(2) Share of all local women aged 16-44 who are unmarried						(2) Sex ratio (women/men) among unmarried Hispanic immigrants			(2) Sex ratio (women/men) among all local people			(2) Sex ratio (women/men) among all unmarried people					

Notes: Apart from the difference in instruments created by the change in excluded exogenous variables, the specifications and techniques are the same as in Table 3. Estimates are comparable to those in the last column of that table. As in the other tables, estimates and standard errors are reported 100 times their actual value.

Table 5: Selected IV Estimates of the Earnings Equation using Alternate Samples and Expanded Sets of Controls

A. Alternate Sample Selection Criteria

Sample:	Include formerly married men (as a separate group)			Include men with non-Hispanic immigrant wives			Speaks English "well" or better			Exclude men from NYC, LA, Chicago		
Intermarriage Premium	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**
Men born in Mexico	37.5	11.1	0.00	38.5	9.8	0.00	37.4	11.27	0.00	36.6	10.52	0.00
Men born in Puerto Rico	1.6	16.2	0.92	11.4	11.7	0.33	8.5	13.52	0.53	7.8	12.77	0.54
Difference in Differences	35.8	13.3	0.01	27.1	10.9	0.01	28.9	11.3	0.01	28.8	12.0	0.02
R^2	0.14			0.16			0.19			0.16		
Observations	124,155			109,108			60,308			82,907		

B. More Sample Selection Criteria: Age at Arrival

Sample:	Men who arrived at any age (not just by age 25)			Exclude men older than 18 at arrival			Exclude men who arrived by age 6			Only men who arrived by age 6		
Intermarriage Premium	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**
Men born in Mexico	35.5	10.5	0.00	42.2	11.58	0.00	27.0	10.1	0.01	62.4	14.8	0.00
Men born in Puerto Rico	-3.4	11.6	0.77	4.9	14.57	0.74	-4.2	11.9	0.72	7.9	20.7	0.70
Difference in Differences	38.9	10.8	0.00	37.3	14.8	0.01	31.2	12.8	0.02	54.5	22.3	0.02
R^2	0.15			0.17			0.15			0.26		
Observations	123,306			68,472			97,542			10,552		

C. Additional Control Variables

Additional controls:	State dummies			Local emp/pop ratio & industry composition			Own-industry dummies			Detailed fluency dummies		
Intermarriage Premium	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**
Men born in Mexico	44.8	9.3	0.00	43.7	9.9	0.00	39.3	8.0	0.00	48.3	9.0	0.00
Men born in Puerto Rico	12.2	11.3	0.28	14.2	12.0	0.24	12.8	9.6	0.18	24.6	11.5	0.03
Difference in Differences	32.6	10.6	0.00	29.6	10.7	0.01	26.5	9.0	0.00	23.8	11.8	0.04
R^2	0.16			0.16			0.18			0.15		
Observations	108,094			108,094			108,094			108,094		

Notes: Apart from the additional controls, the estimates use the same specification and methods as in Table 3. As in the other tables, estimates and standard errors are reported 100 times their actual value.

Table 6: Comparison of Estimated Green Card Premiums by Age Range and School Enrollment Status

		Whole sample in age range			Excluding those enrolled in school		
		Upper age			Upper age		
		35	**40**	**44**	**35**	**40**	**44**
Lower age	**16**	47.7	40.8	31.5	32.3	37.8	28.8
	19	53.1	42.4	32.5	34.3	38.0	29.2
	22	49.9	40.2	30.6	31.9	35.9	27.3
	25	50.0	40.0	29.7	38.6	39.0	29.0
	28	48.6	42.7	31.2	36.2	42.9	30.2

Notes: Each number in the table is an estimate of the green card premium (x100, as in the other tables) from a sample of men at least as old as the "lower age" (the row) and no older than the "upper age" (the column). All are statistically significant at the 5 percent level. The samples in the left panel otherwise use the same restrictions as our main sample, and the right panel excludes men enrolled in school.

Table 7: Comparison of IV Estimates of Green Card Premiums across Spanish-Speaking Source Countries in Latin America

	Estimated Green Card Premiums (IV)				Percentage of green cards issued in 2000 to persons who:					
Birthplace	**N**	**Est**	**SE**	**P**	**Were already refugees or asylees**	**Arrived ≤ 5 years ago (w/o green card)**	**Applied for green card based on employment**	**Were already working as executives or professionals (if employed)**	**First entered US w/o inspection**	**Fraction of married men with non-Hispanic wives**
Ecuador	2,115	60.6	12.7	0.00	0	25	9	24	11	9
El Salvador	9,131	50.8	14.5	0.00	0	5	10	8	49	5
Nicaragua	1,722	50.2	22.7	0.03	1	11	1	12	61	9
Peru	1,523	39.7	16.5	0.02	3	21	9	29	11	19
Guatemala	5,267	37.7	14.6	0.01	1	7	8	18	29	6
Honduras	2,426	34.5	20.3	0.09	1	10	6	19	23	10
Mexico	101,203	31.8	10.4	0.00	0	5	2	7	40	4
Colombia	2,644	26.2	13.0	0.04	1	32	6	35	5	15
Costa Rica	471	25.8	15.6	0.10	1	40	9	43	2	24
Dominican Rep.	3,498	20.0	20.7	0.33	0	21	1	21	9	6
Cuba	3,721	17.9	13.3	0.18	69	40	0	24	2	20
Venezuela	509	13.3	18.4	0.47	4	36	17	62	1	27
Panama	415	13.1	15.6	0.40	1	35	12	33	1	36
Argentina	533	2.7	16.6	0.87	1	35	28	64	2	32
Chile	404	-13.2	17.5	0.45	1	33	14	48	2	29
	Correlation with point estimate:				-0.15	-0.62	-0.47	-0.70	0.65	-0.74

Notes: The sample for the regression (left columns) comes from the 2000 U.S. Census Micro Sample and satisfies the criteria for our main sample, but adds men from all Spanish-speaking Latin American except Bolivia, Uruguay, and Paraguay (483 observations in total). As in earlier tables, the regression also includes 6,891 men from Puerto Rico as the control group; the estimated intermarriage premium for them is 1.3 percent (SE=10.2). The regression uses the preferred specification from Table 3, and estimates and standard errors are again reported 100 times their actual value. The columns on the right are computed from data reported by the U.S. Immigration and Naturalization Service (2002) or from the estimation sample (last column). The share who arrived ≤ 5 years ago excludes people who arrived with green cards and assumes persons with "unknown entry date" do not qualify. Data on executive/professional occupations are percentages of persons already in the U.S. for whom the information is reported; other categories also include new entrants.

Table 8: IV Estimates When Pre- and Post-1982 Mexican Immigrants Are Treated As Separate Groups

Sample:	Whole Sample			Men in U.S. 10+ years		
Intermarriage Premium:	**Est. (x100)**	**SE (x100)**	**P**	**Est. (x100)**	**SE (x100)**	**P**
Mexicans who arrived after 1982	51.5	9.2	0.00	29.8	12.8	0.02
Mexicans who arrived before 1982	5.5	10.6	0.60	2.2	9.8	0.82
Puerto Ricans	-1.4	12.3	0.91	-9.9	13.5	0.47
Green card premiums for Post-82 Mexicans:						
Control group: Pre-82 Mexicans	46.0	11.4	0.00	27.6	11.9	0.02
Control group: Puerto Ricans	52.9	14.1	0.00	39.7	16.1	0.01
R^2	0.16			0.15		
Observations	108,094			62,367		

Notes: Estimates use the same specification and methods as in Table 3, except here they also incorporate Chi's (2013) strategy of treating pre- and post-1982 Mexican immigrants as different groups, reflecting their differential treatment under the Immigration Reform and Control Act of 1986.

Table 9: Estimated Intermarriage Premium and Green Card Premium, Estimated from Within-Group Variation

		Intermarriage premium, Mexicans			Intermarriage premium, Puerto Ricans			Difference = Green card premium			Implied avg. increase in Mexicans' ann. earnings	
A. By citizenship, education, and occupation	**Obs.**	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**	**Est.**	**SE**	**P**	**Intermarriage**	**Green card**
1. Placebo test - naturalization												
Sample includes only citizens (naturalized for Mexicans)	27,878	21.9	9.1	0.02	17.3	11.5	0.13	4.6	11.2	0.68	6,687	1,277
Sample excludes naturalized Mexicans	87,107	40.1	9.2	0.00	14.8	13.1	0.26	25.3	12.5	0.04	9,280	5,425
2. By occupation												
Common occupations of undocumented workers	50,201	4.0	12.9	0.76	1.4	12.9	0.91	2.5	14.4	0.86	722	458
Other occupations	57,893	45.0	9.9	0.00	17.8	12.7	0.16	27.2	13.2	0.04	13,089	7,209
3. By education												
College graduates	3,534	68.6	19.6	0.00	13.9	14.4	0.33	54.6	23.9	0.02	31,966	23,590
HS graduates, not college graduates	33,148	43.7	9.2	0.00	19.2	12.9	0.14	24.5	12.4	0.05	12,335	6,246
Less than HS education	71,412	-2.3	13.3	0.86	-16.9	15.3	0.27	14.6	18.2	0.42	-439	3,010
B. By time since immigration												
Arrived less than 5 years before survey	18,904	76.9	23.8	0.00	7.7	31.0	0.80	69.2	33.2	0.04	16,053	13,834
5 to 8 years	17,035	85.2	28.6	0.00	0.3	30.0	0.99	84.9	38.6	0.03	22,618	22,501
9 to 12 years	20,063	37.9	17.8	0.03	-5.7	20.6	0.78	43.5	24.8	0.08	9,144	10,834
13 to 18 years	22,349	19.5	19.0	0.31	15.1	17.7	0.40	4.5	25.4	0.86	4,839	1,028
C. By potential US experience												
0 to 5 years	33,213	111.0	21.7	0.00	10.7	27.4	0.70	100.3	31.6	0.00	28,785	24,444
6 to 9 years	16,191	1.0	28.5	0.97	2.6	22.5	0.91	-1.5	30.1	0.96	192	-292
10 to 18 years	28,947	-16.7	19.2	0.39	-15.1	15.4	0.33	-1.7	21.5	0.94	-3,552	-380

Notes: Estimates are calculated separately for all subsamples using the same specification and methods as in Table 3, and all estimates and standard errors are reported 100 times their actual value. We have taken the following to be common occupations of undocumented workers: Butchers and meat cutters; Carpenters; Construction Laborers; Cooks; Drywall installers; Farm workers; Gardeners and groundskeepers; Janitors; Laborers outside construction; Machine operators, not elsewhere classified; Masons, tilers, and carpet installers; Miscellaneous food prep workers; Painters, construction and maintenance; Roofers and slaters; Waiter/waitress, and Waiters' assistants. Potential U.S. experience is defined as the minimum of the man's (age-16), (age-years of schooling-6), and number of years since immigration. Estimates in the last two columns are computed using estimated premiums (b) and mean earnings (Y) of Mexican men in the desired subgroups who are not intermarried; the estimate is thus Y*[exp(b)-1].