On Estimating the Effects of Immigrant Legalization: Do U.S. Agricultural Workers Really Benefit?

Breno Sampaio, Gustavo Ramos Sampaio, and Yony Sampaio

The question of whether legalization affects immigrants' economic returns has been the focus of many empirical studies in recent decades. Results have consistently shown that there are significant wage differences between legal and illegal workers. However, the validity of such findings has been questioned, given the lack of good identification strategies to account for omitted variables. In this article we propose using techniques designed to address the issue of selection into treatment based (to some degree) on unobservables. Our results suggest that lower skill levels—not discrimination—explain differences in immigrants' economic outcomes.

Key words: economic outcomes, undocumented workers, immigration, identification.

JEL codes: J31, J32, J43, J71.

In recent decades the United States has experienced a substantial increase in the number of undocumented immigrants entering the country, as well as documented immigrants overstaying their legally permitted time (Passel 2005). According to estimates provided by Passel and Cohn (2009), the United States is now home to approximately 12 million unauthorized immigrants. This phenomenon has caused dramatic changes in the agricultural workforce, with the ratio between undocumented and legal farm workers rising from only 16% in the period 1989–92, to 36% in 1993–95, and 50% in 1998–2000 (Iway, Emerson, and Walters 2006). In an attempt to control this flow of illegal immigrants, the U.S. government proposed several changes to its immigration policy; it not only increased border security but also introduced sanctions for U.S. employers that hire unauthorized workers and launched several amnesty programs, such as the 1986 Immigration Reform and Control Act (IRCA) and the Comprehensive Immigration Reform Act of 2006 (CIRA), both of which allow agricultural workers to acquire legal permanent residence status.

These changes naturally led researchers to question the potential effects of legalization on the economic returns of affected and unaffected workers, especially considering the impressive size of the undocumented population residing in the United States. On the one hand, there is a view that illegal workers are discriminated against in the labor market, experience low job mobility (undocumented workers are restricted from entering the formal economy, which implies that they have fewer jobs to choose from), and are paid wages that are substantially lower than the values paid to legal workers with similar characteristics. On the other hand, some economists believe that instead of discrimination, the driving force behind the lower wages paid to undocumented workers is their skill levels. As noted by Kaushal (2006), the two hypotheses have very different implications. If the main cause of lower wages is fewer skills, then upon receiving amnesty, an undocumented immigrant should not observe any change in his labor market outcome. The opposite will occur, however, if undocumented immigrants receive low wages because of discrimination.

Given this debate, several analyses devoted to estimating the legalization effect on economic outcomes have been conducted. The results have consistently shown that there is a significant wage difference between legal and illegal workers, even when accounting for a full

Breno Sampaio, Gustavo Ramos Sampaio, and Yony Sampaio are professors in the Department of Economics, Universidade Federal de Pernambuco. Correspondence may be sent to: brenorsampaio@gmail.com.

Amer. J. Agr. Econ. 95(4): 932–948; doi: 10.1093/ajae/aat012
Published online June 2, 2013

set of demographic characteristics; this would support the hypothesis that illegal workers are discriminated against in the labor market. Differences are also observed when examining several other variables, such as the probability of receiving employer-sponsored health insurance and the probability of participating in aid programs.

Most of the literature, however, acknowledges the difficulty of measuring such effects, given that workers are not randomly assigned to the legal permanent resident and undocumented groups. That is, even accounting for important observable differences between individuals, such as income and education, it is still difficult to argue that other unobserved variables affecting economic outcomes are uncorrelated with the likelihood of becoming a legal resident. Thus, researchers have attempted, in one way or another, to circumvent such problems by using methodologies that in theory would minimize or even completely eliminate omitted variable bias. These empirical strategies may be divided into two groups. The first uses cross-sectional data and compares the labor market outcomes of legal and undocumented workers. The second uses panel data to investigate the effect of amnesty programs on future flows of undocumented workers.

In the first group, the use of propensity score matching techniques has prevailed as a strategy to control for the non-random assignment of legal status. The two most recent applications are Kandilov and Kandilov (2010) and Pena (2010), who use data from the National Agricultural Workers Survey (NAWS) to analyze the effect of legalization on wages, as well as the probabilities of receiving employer-sponsored health insurance and/or the monetary bonus of participating in safety net programs. These authors' results imply that having legal status confers significant positive effects in the U.S. agriculture sector. Examining wages in particular, both papers estimate that legal immigrants receive an average 5–6% more in hourly wages.

In the second group, the most influential papers were written by Rivera-Batiz (1999), Kossoudji and Cobb-Clark (2002), and Kaushal (2006). The former authors analyze the Immigration Reform and Control Act (IRCA) of 1986, while the latter examines the Nicaraguan Adjustment and Central American Relief Act (NACARA) of 1997. The results obtained by Rivera-Batiz (1999) showed that the average hourly wage rate of legal male Mexican immigrants was 41.8% higher than that of undocumented workers, and that only 48% of the log-wage gap was explained by differences in observed characteristics. Similar to the results obtained in the first group via propensity score matching, Kossoudji and Cobb-Clark (2002) find that the wage premium of legalization under IRCA was approximately 6%, and Kaushal (2006), analyzing the NACARA, found modest gains of 3–4%.

A central assumption required for identification in the articles of the first group is the validity of the conditional independence assumption (CIA), i.e., that the treatment assignment is independent of potential outcomes conditional on a set of covariates, or, as shown by Rosenbaum and Rubin (1983), on the propensity score. If this assumption fails, and it is unclear why it should be valid in the current application, then ignoring the selection of unobservables might lead to significantly biased estimates. That is, one might control for several characteristics, but it is not difficult to argue that some unobserved variable, such as perseverance, which is positively correlated with wages, is also likely to be correlated with the decision to enter the United States illegally and the probability of becoming a legal permanent resident. Pena (2010) attempts to address this issue using treatment effects regressions (which is analogous to a bivariate probability model with a continuous dependent variable) and uses the years of initial entry to the United States as exclusion restrictions. The main identifying assumption is that, conditional on a set of controls that include work experience and survey year, the entry year should be uncorrelated with workers' outcomes, but should significantly affect the ability of immigrants to receive legal status. We believe this assumption to be very strong, as the policy changes facilitating amnesty were also accompanied by changes directed toward undocumented immigrants currently living in the United States and had a direct effect on newcomers. Thus, it is unclear why the correlation is zero between the error term of the equation of interest and the dummy variables for the periods that had policy changes.

Moving to the papers that investigate the effect of amnesty programs on undocumented workers, two important concerns were raised by Kaushal (2006) on the articles analyzing the Immigration Regulation and Control Act (IRCA) of 1986. The first concern is that the program not only facilitated amnesty but also significantly changed the policy toward undocumented immigrants currently living in the United States by, for example, introducing sanctions for U.S. employers who hire unauthorized workers. The second is that the IRCA, by granting amnesty to approximately 2.8 million immigrants, might have impacted the overall supply of documented and undocumented immigrant workers in the United States. Thus, in a broader sense, the validity of such approaches primarily depends on how convincing the comparison groups selected as counterfactuals for the workers who were legalized under such programs are, which has been questioned by many researchers (Kaushal 2006).

Ultimately, none of the studies discussed above are unquestionable, especially those relying on the selection on observables for identification, which are primarily those devoted to analyzing agricultural workers. In this article, we propose the use of less restrictive approaches to address the issue of non-random selection into legal status. First, following Altonji, Elder, and Taber (2005b; 2008), we evaluate how sensitive estimates of the legalization effect are when the degree of selection on unobservables increases relative to the case in which selection is completely driven by observables. Following their notion that the degree of selection on observed characteristics is the same as the degree of selection on unobserved characteristics, we also obtain lower bound estimates for the parameters of interest. Obtaining lower bound estimates of the parameter of interest under weaker selection assumptions is very useful, as these lower bounds would necessarily be larger than zero if the causal effects were truly robust (as previous results reveal positive benefits of becoming legal). Second, we use a recently developed technique proposed by Millimet and Tchernis (2010) that, under certain assumptions, allows one to obtain estimates of the parameter of interest while accounting for the bias arising from failure of the CIA required for the consistency of propensity score estimators. We not only analyze the effect of legalization on wages, but also follow Kandilov and Kandilov (2010) and in addition analyze whether legal status affects other forms of labor compensation, such as employer-sponsored health insurance and employee bonuses. The underlying hypothesis is that legal status might not only affect wages directly but also could affect other forms of compensation, such as health insurance, which is of particular interest for policy-makers, given its low coverage rate among the immigrant population (McNamara and Ranney 2002).

Our results show that a modest degree of selection on unobservables is sufficient to completely eliminate the positive effects found in previous studies on wages, health insurance, and bonuses. Additionally, under the notion that selection on observables is the same as selection on unobservables, we find that the role of unobservables in determining wages would have to be more than .066 times the role of observables for the entire legalization effect to be explained away by the unobservables, which is very likely to be true. Thus, non-random selection appears to be an important issue in the present discussion. The results obtained from the Millimet and Tchernis (2010) technique are also in accordance with this statement. By accounting for the failure of the CIA and the influence of unobservables, our estimated coefficients all become statistically insignificant. Thus, our analysis indicates that becoming a legal permanent resident has no effect on wages or on the probability of receiving employer-sponsored health insurance or additional monetary bonuses. This result, contrary to most evidence provided thus far, was already suggested by Borjas (1990) several years ago when he wrote, "[i]llegal aliens in the United States have lower wages than legal immigrants not because they are illegal, but because they are less skilled. In other words, if one compares two persons who are demographically similar (in terms of education, age, English proficiency, years on the job, and so on), legal status has no direct impact on the wage rate" (Rivera-Batiz 1999).

As stated above, our article is devoted to analyzing the robustness of the findings previously presented in the literature regarding the effect that becoming a legal permanent resident has on the U.S. agricultural sector. As previous papers rely on strong assumptions regarding selection into treatment (in this case, into legalization), we see our article as an important step toward not only understanding the relationship between legal status and economic outcomes but also, more importantly, toward highlighting that measuring such effects is much more difficult, from an econometric standpoint, than what previous analyses claim. Therefore, a main contribution of our article is to use the National Agricultural Workers Survey, a nationally representative data set of employed U.S. farm workers which is widely used to answer the question proposed in this article, to show that prior results are weak under slightly different (and weaker) assumptions.

The remainder of this article is organized as follows. Section 2 describes the data used in the analysis. Section 3 presents the methods utilized throughout the article, and section 4 discusses the results and presents several robustness checks. Finally, conclusions are presented in section 5.

Data

The data used in this article come from the National Agricultural Workers Survey (NAWS), which is a nationally representative dataset containing information on the demographic, employment, and health characteristics of employed U.S. farm workers. Among the advantages of this dataset are a sample design that accounts for migratory behavior and the seasonality of agricultural production and employment (crop workers are surveyed in three cycles each year to account for the seasonality of agricultural production and employment). The NAWS also contains information on the legal status of respondents, allowing the researcher to identify legal permanent residents (for example, naturalized citizens, Green Card holders, and other work authorizations), as well as undocumented workers.

We use NAWS samples for the years 2000 through 2006. To estimate the legalization effect on wages, employer-sponsored health insurance, and bonuses, we restrict the data to individuals who are either legal permanent residents[1] or undocumented workers. Additionally, as most of the individuals surveyed in the NAWS are males, we exclude female agricultural workers and exclusively focus on unmarried males, as married males' access to health insurance is facilitated by their wives' employment. Finally, we only consider full-time agricultural workers (those who work at least 35 hours per week), as they are more likely to have access to benefits. We show, however, that our results are robust to the inclusion of married males and part-time workers.

We should emphasize that the population of undocumented agricultural workers experiences very low job mobility, as they are legally restricted from participating in the formal economy. This implies that they have fewer jobs to choose from, which leads to lower wages, on average. Therefore, one may easily argue that an undocumented worker who receives amnesty will leave farm work and seek higher wages outside this sector, leading to a negative selection in the population of agricultural workers who are legal residents. This would lead to bias when using NAWS to estimate the effects of legalization. Tran and Perloff (2002), however, using NAWS to investigate the probabilities of leaving farm work for those foreign-born workers who were granted amnesty and legal permanent residence following IRCA in 1986, showed that the probability of leaving the agricultural sector is similar for workers who were granted legal permanent resident status under IRCA, and those who are undocumented workers. This is a result also found in Lofstrom, Hill, and Hayes (2010, 49) using the New Immigrant Survey (NIS). If this is the case, our results are robust to any job mobility/selection effect between groups of workers.

Summary statistics by legal status are presented in table 1. Comparing the outcomes of interest between legal permanent residents and undocumented workers, we can first observe significant wage differences in favor of legal workers (approximately 7.21%). Unsurprisingly, approximately 54.3% of legal permanent residents have wages that are above the average wage (this value is approximately 35.2% for undocumented workers). Regarding employer-sponsored health insurance and bonuses, 13.7% and 32.1% of legal workers receive such benefits, respectively, while these figures are only 5.2% and 14.3% for undocumented workers.

In addition to these significant differences in outcomes, legal and undocumented workers also differ in several observable characteristics. Legal residents are older and more experienced than undocumented workers, but surprisingly, they are slightly less educated. Differences are also observed in English proficiency, migration, and number of children, among others. Thus, it is important to account for these divergences when comparing the two types of workers. However, as emphasized below, many other unobserved factors not accounted for in previous studies might also differ significantly between the types of workers, leading to biased estimates.

Methodology

In this section, we begin by specifying the main equation of interest estimated in most previous

[1] Foreign-born individuals legally authorized to work in the United States.

Table 1. Summary Statistics

Variables	Legal Permanent Residents		Undocumented Workers		Differences
Hourly wage (in 2006 U.S. $)	8.400	(1.922)	7.679	(1.604)	.721***
ln(Hourly wage)	2.105	(.209)	2.020	(.187)	.085***
Wages Larger than Average	.542	(.499)	.352	(.478)	19.051***
Employer-sponsored Health Insurance	.137	(.344)	.052	(.221)	.085***
Bonus	.321	(.467)	.143	(.350)	.178***
Age	38.755	(12.134)	25.214	(7.806)	13.541***
U.S. farm work experience (in years)	17.230	(8.884)	4.688	(4.321)	12.542***
U.S. farm work experience^2	375.674	(334.495)	40.643	(91.171)	335.031***
Years of Schooling	5.888	(3.523)	6.574	(2.889)	−.686***
English proficiency (speaking)	2.058	(.912)	1.434	(.641)	.624***
Migrant	.316	(.465)	.460	(.498)	−.144***
Employed by contractor	.177	(.382)	.222	(.416)	−.045**
Paid by the piece	.135	(.342)	.184	(.387)	−.049***
Children	.137	(.344)	.023	(.151)	.114***
Weekly hours	48.575	(9.663)	46.790	(8.927)	1.785***
Year					
2000	.209	(.407)	.214	(.410)	−.005
2001	.157	(.364)	.157	(.364)	.000
2002	.159	(.366)	.164	(.370)	−.005
2003	.167	(.373)	.152	(.359)	.015
2004	.144	(.351)	.148	(.355)	−.004
2005	.112	(.315)	.092	(.290)	.020
2006	.053	(.225)	.073	(.261)	−.020*
Region					
East	.055	(.228)	.129	(.335)	−.074***
Southeast	.095	(.294)	.164	(.371)	−.069***
Midwest	.119	(.324)	.098	(.298)	.021
Southwest	.093	(.291)	.041	(.198)	.052***
Northwest	.152	(.359)	.124	(.330)	.028*
California	.486	(.500)	.444	(.497)	.042*
Crop					
Field crops	.155	(.362)	.102	(.303)	.053***
Fruits and nuts	.407	(.492)	.411	(.492)	−.004
Horticulture	.187	(.390)	.214	(.410)	−.027
Vegetables	.190	(.393)	.218	(.413)	−.028
Miscellaneous/Multi-crop	.060	(.238)	.055	(.228)	.005
N	599		3,292		

Note: Standard errors are presented in parentheses.

studies, as well as the matching estimator used in Kandilov and Kandilov (2010) and Pena (2010). We proceed by describing the methodology developed by Altonji, Elder, and Taber (2005b; 2008), which allows one to examine how sensitive the estimates are to assumptions regarding the amount of selection on unobservables, and to obtain lower bound estimates of the legalization effect when variables unobserved by the econometrician are correlated with the outcome of interest. Finally, we present the methodology recently proposed by Millimet and Tchernis (2010), which considers the bias arising from a failure of the CIA that is required to ensure the consistency of propensity score estimators.

Probit, OLS and Matching Estimates

To estimate the effect of legalization on wages and benefits, consider the following model:

$$y = \alpha + \beta L + \mathbf{X}'\gamma + \varepsilon \quad (1)$$

where y is an outcome of interest, L is a dummy variable that takes a value equal to 1 when the worker is a legal permanent resident, and 0 otherwise, $\boldsymbol{X}$ is a vector of controls, and ε is an error term. The parameter of interest, β, represents the effect of legalization on a specific outcome, y.

As is well known, consistently estimating β via equation 1 requires the error term to be uncorrelated with the variable of interest (i.e., $Cov(L, \varepsilon) = 0$) or, in other words, workers must be randomly assigned legal permanent residence, or are assigned based on variables observed by the econometrician. If this assumption fails to hold and selection into treatment is based on variables unobserved to the researcher but correlated with the outcome of interest (L), then the researcher is left with the task of, for example, finding a valid instrumental variable (IV) to correctly estimate the causal effect of legalization. As is the case in many empirical applications[2] (ours included), finding a convincing IV is not always viable, and one must rely on different identification strategies to make inferences regarding the parameter of interest.

Kandilov and Kandilov (2010), recognizing that by directly comparing legal permanent residents to other undocumented workers, one "[w]ould ignore the selection issues that stem from the fact that entering the United States illegally and becoming a legal permanent resident are choices that can be affected by personal characteristics that also determine wages and benefits," and are unobserved to the econometrician, propose estimating the impact of legalization via a propensity score matching estimator. To briefly present the method (and the notation that will later be very useful), let there be two potential outcome variables for individual i (along the lines of Rubin (1974)) such that:

$$(2) \qquad y_i = \begin{cases} y_{1i}, & \text{if } L_i = 1 \\ y_{0i}, & \text{if } L_i = 0 \end{cases}$$

where y_{1i} is the outcome given legalization, and y_{0i} is the outcome without legalization. The causal effect of the treatment ($L_i = 1$) relative to the control ($L_i = 0$) is defined as the difference between the corresponding potential outcomes $\beta_i = y_{1i} - y_{0i}$.

Many population parameters might be of interest. Here, we focus on the average treatment effect (ATE) and the average treatment effect on the treated (ATT), which are defined as:

$$(3) \qquad \beta_{ATE} = E[\beta_i] = E[y_{1i} - y_{0i}]$$

$$(4) \qquad \beta_{ATT} = E[\beta_i | L = 1] = E[y_{1i} - y_{0i} | L = 1].$$

The problem that the researcher faces when estimating equations 3 and 4 arises from the fact that constructing comparisons of two outcomes for the same individual when exposed, and when not exposed, to the treatment is an unfeasible task, because the same worker can either be treated or not treated in the same time period (Imbens and Wooldridge 2009). That is, we only observe one of the two potential outcomes given treatment status, $y_i = y_{0i} - (y_{1i} - y_{0i})L_i$.

Therefore, one must find different individuals (some treated and some not) such that after adjusting for differences in observed characteristics, or pretreatment variables, comparisons can be made (see Angrist and Pischke 2008). This is precisely the intuition behind matching estimators that, under the CIA or unconfoundedness assumption (Rubin 1974; Heckman and Robb Jr. 1985), imply that treatment assignment is independent of potential outcomes conditional on a set of covariates $\mathbf{X}$ or, as shown by Rosenbaum and Rubin (1983), on the propensity score $p(\mathbf{X})$ defined as the conditional probability of being treated $Pr(L = 1|\mathbf{X})$. In this case, the ATE and ATT are obtained by:

$$(5) \qquad \beta_{ATE} = E[\beta_i] = E[y_{1i} - y_{0i} | p(\mathbf{X_i})]$$

$$(6) \qquad \beta_{ATT} = E[\beta_i | L = 1, p(\mathbf{X_i})] = E[y_{1i} - y_{0i} | \times L = 1, p(\mathbf{X_i})].$$

Conditioning on the propensity score essentially implies that the distribution of covariates for the untreated workers are balanced in a way that looks very similar to the distribution of covariates for the treated workers, which makes comparisons between outcomes more reasonable than estimates obtained via equation 1. Thus, the matching procedure, under CIA, eliminates any bias due to the non-random selection to treatment.

Similar to previous studies, we provide estimates for equations 1 and 6. However, we strongly believe such estimates are biased because selection into treatment is also based on variables unobserved by the researcher but correlated with the outcome of interest (L) (i.e., $Cov(L, \varepsilon) \neq 0$ in equation 1, which also implies that CIA fails to hold). Therefore, we use two techniques described below to analyze how robust the previously obtained estimates

[2] See, for example, Altonji, Elder, and Taber (2005a) for an extended critique on the IV strategies used to estimate the effects of Catholic schooling.

are to the selection on unobservables. The first technique focuses on bounding a measure of the treatment effect (Altonji, Elder, and Taber 2005b, 2008), and the second focuses on obtaining a bias-corrected estimate when the CIA is violated (Millimet and Tchernis 2010).

Using Selection on Observed Variables to Assess Bias from Unobservables

In this section, we first present the bivariate probit model employed by Altonji, Elder, and Taber (2005b, 2008) to assess how unobservables might affect the coefficient β obtained via equation 1. Second, we describe their procedure to obtain lower bound estimates when one assumes that the degree of selection on observed characteristics is the same as the degree of selection on unobserved characteristics. Recent applications of this method to very different contexts may be seen in Altonji, Elder, and Taber (2008), Bellows and Miguel (2009), and Cavalcanti, Guimaraes, and Sampaio (2010).

The Sensitivity to Correlation in Unobservables

Consider the following bivariate probit model:

$$(7) \qquad y = 1(y^* > 0) \equiv 1(\theta L + \mathbf{X}'\lambda + \vartheta > 0)$$

$$(8) \qquad L = 1(L^* > 0) \equiv 1(\mathbf{X}'\delta + \varepsilon > 0)$$

$$(9) \qquad \begin{bmatrix} \vartheta \\ \varepsilon \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix} \right)$$

where L and $\mathbf{X}$ are defined as above, and ϑ and ε are the error terms for the equation of interest and for the selection equation, respectively.

The parameter ρ represents the correlation between the error terms of equations 7 and 8, and captures how unobservables affect the outcome y and the probability of being a legal permanent resident, L. For example, if $\rho > 0$, then workers' unobserved characteristics affect the probability of being a legal permanent resident in the same way that they affect the outcome of interest. Similarly, a negative correlation ($\rho < 0$) implies that unobserved factors that positively affect the probability of being a legal permanent resident negatively affect the outcome, y.

The model presented in equations 7–9 is identified given the normality assumption even without an exclusion restriction (although, as noted by Altonji, Elder, and Taber (2005b), researchers should interpret results from this model with caution when there is no exclusion restriction), which would be required for semiparametric identification. Therefore, to assess how sensitive the estimates are under some degree of selection on unobservables, Altonji, Elder, and Taber (2005b; 2008) propose treating this model as if it were underidentified by one parameter, namely ρ. Thus, their strategy is to constrain the model to certain values of ρ and examine how θ behaves under these different levels of correlation in unobservables. We consider $\rho = 0.0$, 0.1, 0.2, and 0.3 (similar to the values considered in their article) and analyze whether the positive legalization effect found in previous studies still maintains its size and significance. Note that $\rho = 0.0$ implies that all selection comes from observables (which is precisely what is obtained when estimating equation 1).

Using Selection on Observables to Assess Selection Bias

Given the sensitivity analysis presented above, one might recognize that while $\rho > 0$ seems to provide a better description of how the selection into treatment occurred, it provides no information on what values of ρ are more reasonable. Should we consider higher degrees of selection on unobservables such as, for example, $\rho = 0.7$? As a guide to the magnitude of the effect of unobservables, Altonji, Elder, and Taber (2005b, 2008) propose that "selection on the unobservables is the same as selection on the observables." Formally, let the linear projection of L^* onto $\mathbf{X}'\lambda$ and ϑ (following equation 7), where ϑ captures unobserved factors that affect the outcome variable, be such that:

$$(10) \qquad Proj(L^*|\mathbf{X}'\lambda, \vartheta) = \phi + \phi_{\mathbf{X}'\lambda}\mathbf{X}'\lambda + \phi_\vartheta \vartheta.$$

Given $\phi_{\mathbf{X}'\delta}$ and ϕ_ϑ, which capture how L is dependent on observables ($\mathbf{X}'\delta$) and unobservables (ϑ), the notion that "selection on the unobservables is the same as selection on the observables" is formalized by imposing the condition that $\phi_{\mathbf{X}'\delta} = \phi_\vartheta$. This assumption implies that the component of y^* that is captured by observables has the same relationship with L^* as the component that is captured by unobservables. Note that setting $\phi_\vartheta = 0$ is the same as setting $\rho = 0$ or estimating 1 directly.

The assumptions required to ensure the validity of this approach are precisely presented in Altonji, Elder, and Taber (2002).

Following these authors, we take estimations based on $\phi_{\mathbf{X}'\delta} = \phi_{\vartheta}$ and $\phi_{\vartheta} = 0$ as the lower and upper bounds, respectively, for the parameter of interest. We should emphasize, however, that even if such conditions fail to hold, our estimates presented below when selection on the unobservables is imposed have the same impact as when selection on the observables are negative and significant; this implies that having a higher degree of selection on unobservables would deliver an even smaller coefficient.

In a bivariate probit model similar to equations 7–9, Altonji, Elder, and Taber (2005b; 2008) show that the correlation coefficient, when $\phi_{\mathbf{X}'\delta} = \phi_{\vartheta}$ holds, is equivalent to the condition that $Cov(\mathbf{X}'\delta, \mathbf{X}'\lambda)/Var(\mathbf{X}'\delta)$. This implies that the "true" correlation coefficient is bounded between the case when there is no selection on unobservables and the case when selection on the unobservables is the same as selection on the observables, that is:

$$(11) \quad 0 \le \rho \le \frac{Cov(\mathbf{X}'\delta, \mathbf{X}'\lambda)}{Var(\mathbf{X}'\delta)}.$$

Therefore, the primary strategy is to obtain lower bound estimates of the treatment effect by estimating the bivariate probit model with an additional constraint on ρ (namely its upper bound given in equation 11).

Minimum Bias and Bias-corrected Estimators Under Failure of the CIA

Above, we described the Altonji, Elder, and Taber (2005b; 2008) method that allows the researcher to examine how sensitive the estimates are to assumptions regarding the amount of selection on unobservables, and also allows the researcher to obtain lower bound estimates of the legalization effect when variables unobserved by the econometrician are correlated with the outcome of interest. In this section, we present a recently developed methodology proposed by Millimet and Tchernis (2010) that considers the bias arising from failure of the CIA, which is required to ensure the consistency of propensity score estimators. We begin by briefly describing the bias that arises when the CIA fails to hold and then present the minimum-biased and bias-corrected estimators.

To analyze the bias that arises when the CIA fails, consider the following two assumptions made by Millimet and Tchernis (2010), which are also present in Black and Smith (2004) and Heckman and Navarro-Lozano (2004):

- (A1) Potential outcomes and latent treatment assignment are additively separable in observables and unobservables:

$$(12) \quad y_{0i} = g_0(X) + \zeta_0$$

$$(13) \quad y_{1i} = g_1(X) + \zeta_1$$

$$(14) \quad L^* = h(X) - u$$

$$(15) \quad L = 1(L^* > 0)$$

- (A2) $\zeta_0, \zeta_1, u \sim N_3(0, \Sigma)$, where

$$(16) \quad \Sigma = \begin{bmatrix} \sigma_0^2 & \rho_{01} & \rho_{0u} \\ & \sigma_1^2 & \rho_{1u} \\ & & 1 \end{bmatrix}.$$

Under assumptions (A1) and (A2), Black and Smith (2004) and Heckman and Navarro-Lozano (2004) show that the bias when estimating the ATT given the failure of the CIA is given by:

$$(17) \quad B_{ATT}[p(X)] = -\rho_{0u}\sigma_0 \left[\frac{\phi(h(X))}{\Phi(h(X))[1 - \Phi(h(X))]} \right].$$

Similarly, Millimet and Tchernis (2010) (and equivalently Heckman and Navarro-Lozano (2004)) show that the bias when estimating the ATE is given by:

$$(18) \quad B_{ATE}[p(X)] = -\{\rho_{0u}\sigma_0 + [1 - p(X)]\rho_{\delta u}\sigma_\delta\} \times \left[\frac{\phi(h(X))}{\Phi(h(X))[1 - \Phi(h(X))]} \right]$$

where $\phi(\cdot)$ and $\Phi(\cdot)$ are, respectively, the standard normal and cumulative density functions, $p(X) = \Phi(h(X))$, and $\delta = \zeta_1 - \zeta_0$, which capture unobserved individual gains from treatment.

The primary rationale behind the minimum-biased estimator is to select an appropriate sample (based on $p(X)$) such that $B_{ATT}[p(X)]$ and $B_{ATE}[p(X)]$ are minimized. For the ATT, Black and Smith (2004) show that equation 17 is minimized when $h(X) = 0$ or, equivalently, when $p(X) = 0.5$. Therefore, Black and Smith recommend that the average treatment effect on the treated should be estimated using only observations in the neighborhood of $p(X) = 0.5$, for example observations in which $p(X) \in (0.5 - \nu, 0.5 + \nu), \nu > 0$. For the ATE, Millimet and Tchernis (2010) show that the bias is minimized when $p(X) = p^*$, which, differently from the ATT case, varies with $\rho_{0u}\sigma_0$ and $\rho_{\delta u}\sigma_\delta$. The authors propose the following minimum biased estimator that is derived from the normalized weighting estimator previously proposed by Hirano and Imbens (2001):

$$(19)\quad \widehat{\beta_{MB,ATE}}[p^*] = \left[\sum_{i\in\Omega} \frac{y_i L_i}{\hat{p}(X_i)} \Big/ \sum_{i\in\Omega} \frac{L_i}{\hat{p}(X_i)}\right] - \left[\sum_{i\in\Omega} \frac{y_i(1-L_i)}{1-\hat{p}(X_i)} \Big/ \sum_{i\in\Omega} \frac{(1-L_i)}{\hat{p}(X_i)}\right]$$

where $\Omega = \{i|\widehat{p}(X_i) \in C(p^*)\}$ and $C(p^*)$ denotes a neighborhood around p^* and is defined as $C(p^*) = \{\widehat{p}(X_i)|\widehat{p}(X_i) \in (\underline{p}, \overline{p})\}$. The lower and upper bounds of $\widehat{p}(X_i)$ are defined as $\underline{p} = max\{0.02, p^* - \alpha_\theta\}$ and $\overline{p} = min\{0.98, p^* + \alpha_\theta\}$, where $\alpha_\theta > 0$ selects at least θ% of both the treatment and control groups to be included in the set Ω, over which 19 will be summed.

At this stage, the question that comes to mind is how to obtain estimates of p^*. To do so, Millimet and Tchernis (2010) impose the following additional restrictions on the functional forms of the equations present in assumption (A1):

$$(20)\quad g_0(X) = X'\beta_0$$

$$(21)\quad g_1(X) = X'\beta_1$$

$$(22)\quad h(X) = X'\pi.$$

The main objective is to estimate $\rho_{0u}\sigma_0$ and $\rho_{\delta u}\sigma_\delta$ such that by minimizing equation 18, one would obtain exact values for p^*. To do so, the authors apply Heckman's bivariate normal (BVN) selection model by first estimating a probit model, and then estimating the following equation via OLS:

$$(23)\quad y_i = X_i'\beta_0 + X_i'L_i(\beta_1 - \beta_0) + \beta_{\lambda 0}(1-L_i)\left[\frac{\phi(X_i'\pi)}{1-\Phi(X_i'\pi)}\right] + \beta_{\lambda 1}L_i\left[\frac{-\phi(X_i'\pi)}{\Phi(X_i'\pi)}\right] + \eta_i$$

where $\beta_{\lambda 0}$ and $\beta_{\lambda 1}$ consistently estimate $\rho_{0u}\sigma_0$ and $\rho_{0u}\sigma_0 + \rho_{\delta u}\sigma_\delta$, respectively.

With respect to the ATT, as one knows $p^* = 0.5$ (by equation 17), an estimator along the lines of 19 is given by:

$$(24)\quad \widehat{\beta_{MB,ATT}}[p = 0.5] = \sum_{i\in\Omega} y_i L_i - \left[\sum_{i\in\Omega} \frac{y_i(1-L_i)\hat{p}(X_i)}{1-\hat{p}(X_i)} \Big/ \sum_{i\in\Omega} \frac{(1-L_i)\hat{p}(X_i)}{1-\hat{p}(X_i)}\right].$$

Thus far, we have characterized the bias (under the assumptions described above) and how to obtain minimum-biased estimators for ATE and ATT when CIA fails to hold. However, given the estimates of p^*, $\rho_{0u}\sigma_0$ and $\rho_{\delta u}\sigma_\delta$, a natural extension is to estimate the bias itself using equations 17 and 18. This would lead to the following:

$$(25)\quad \widehat{B_{ATE}}[p^*] = -\{\widehat{\rho_{0u}\sigma_0} + [1-p^*]\widehat{\rho_{\delta u}\sigma_\delta}\}\left[\frac{\phi(\Phi^{-1}(p^*))}{p^*[1-p^*]}\right]$$

$$(26)\quad \widehat{B_{ATT}}[p = 0.5] = -\widehat{\rho_{0u}\sigma_0}\left[\frac{\phi(\Phi^{-1}(0.5))}{0.5[1-0.5]}\right] \cong -1.6\widehat{\rho_{0u}\sigma_0}$$

which would then be used to obtain bias-corrected estimates (MB-BC) of both parameters:

$$(27)\quad \widehat{\beta_{MB-BC,ATE}}[p^*] = \widehat{\beta_{MB,ATE}}[p^*] - \widehat{B_{ATE}}[p^*]$$

$$(28)\quad \widehat{\beta_{MB-BC,ATT}}[p = 0.5] = \widehat{\beta_{MB,ATT}}[p = 0.5] - \widehat{B_{ATT}}[p = 0.5].$$

Results

We begin by describing the results obtained via the probit, OLS and matching estimators. As specified in the data section, we use four outcomes: the *log* of hourly wages (*ln*(Hourly Wage)); a dummy that equals 1 if the worker's wage is above the average wage of all workers in the sample, and 0 otherwise ($Wage \geq \overline{Wage}$); a dummy that equals 1 if the worker received employer-sponsored health insurance; and 0 otherwise (*Health Insurance*), and a dummy that equals 1 if the worker received any additional pay in the form of a bonus (*Bonus*).

Table 2. The Effect of Legal Permanent Resident Status on Wages, Health Insurance, and Bonuses

Estimation	*ln*(Hourly Wage) (1)	Wage ≥ $\overline{Wage}$ (2)	Health Insurance (3)	Bonus (4)
OLS	.033***	.094***	.025*	.052**
	(.011)	(.027)	(.014)	(.021)
Probit		.274***	.121	.192**
		(.080)	(.109)	(.089)
		[.088]	[.012]	[.037]
Nearest Neighbor Matching	.055**	.141**	.090***	.112**
	(.026)	(.061)	(.027)	(.054)

Notes: Robust standard errors are presented in parentheses (bootstrapped standard errors for matching estimates). Linear probability models are estimated for columns (2)–(4) under OLS. Marginal effects are presented in brackets for probit estimates. ***$p < 1\%$, **$p < 5\%$, and *$p < 10\%$.

Table 2 presents the estimates. As expected, given the results previously obtained in the literature, the coefficients for the probit and OLS estimations are all significant (with the exception of the coefficient for health insurance, which is positive but statistically insignificant). However, one should interpret such coefficients very carefully, given that becoming a legal permanent resident is a choice that can be affected by personal characteristics not controlled for in the analysis. Based on this conjecture, we improve upon simple probit and OLS estimates by using a propensity score matching to balance the distribution of covariates in the control and treatment groups. Table 2 contains the matching estimates of the legalization effect for all four outcomes.[3] As expected, all estimates are statistically significant. Thus, we arrive at the same conclusion reported in previous studies, namely, that the wage premium of becoming a legal permanent resident is approximately 5.5%.[4] Regarding the other outcomes, legalization seems to positively affect the probability of receiving employer-sponsored health insurance and monetary bonuses.

We now turn to the analysis of how robust these results are when unobservables are allowed to be correlated with the legalization variable. Let us begin by examining how sensitive the estimates of the legalization effect are to variation in the correlation between the error terms in the bivariate probit model presented in equations 7–9. Panel A of table 3 presents estimates of the parameter of interest in equation 7, θ, along with its marginal effects. From top to bottom, we impose different values for the correlation coefficient, ρ. When $\rho = 0$, we obtain the same results as the probit estimates presented in table 2, since the existence of unobserved factors is assumed away. Imposing a correlation of only $\rho = 0.1$ is sufficient to make all of the coefficients statistically insignificant. For the wage dummy, for example, the marginal effect drops from .088 to only .038,

Table 3. Sensitivity of Legalization Effects to Variation in the Correlation of Disturbances in Bivariate Probit Models

	Wage ≥ $\overline{Wage}$	Health Insurance	Bonus
Panel A			
ρ	(1)	(2)	(3)
0.0	.274***	.121	.192**
	(.080)	(.109)	(.089)
	[.088]	[.012]	[.037]
0.1	.100	−.048	.018
	(.080)	(.108)	(.088)
	[.038]	[−.005]	[.003]
0.2	−.074	−.217**	−.155*
	(.079)	(.107)	(.087)
	[−.027]	[−.018]	[−.026]
0.3	−.249***	−.386***	−.328***
	(.078)	(.105)	(.086)
	[−.090]	[−.030]	[−.052]
Panel B			
ρ	.162	.072	.112

Notes: Standard errors are presented in parentheses and marginal effects in brackets. For panel B, values of ρ are calculated such that the legalization effect becomes zero. *** represents $p < 1\%$, ** represents $p < 5\%$, and * represents $p < 10\%$.

[3] The propensity score is estimated using a logit model similar to that of Kandilov and Kandilov (2010). Given that the article focuses on how the parameter of interest may change when unobservables are accounted for, we do not report the results of this logistic regression. Results, however, are available upon request.

[4] Note that the matching procedure is very successful given there are no significant differences between the covariates of legal permanent residents and undocumented agricultural workers (see table 3 of Kandilov and Kandilov (2010)).

Table 4. Legalization Effects Under Equality of Selection on Observables and Unobservables

Estimation	*ln*(Hourly Wage) (1)	Wage ≥ $\overline{Wage}$ (2)	Health Insurance (3)	Bonus (4)
Bias	.514 (.071)			
α		−1.019*** (.070) [−.311]	−1.046*** (.084) [−.069]	−.973*** (.076) [−.124]
ρ		.727	.690	.672

Notes: (Bootstrapped) standard errors are presented in parentheses, and marginal effects in brackets. $^{***}p < 1\%$, $^{**}p < 5\%$, $^{*}p < 10\%$.

while for the bonus dummy this value drops from .037 to .003. The health insurance dummy surprisingly shifts its sign and becomes negative. Increasing ρ to 0.2 is sufficient to shift all coefficients to a negative value, and they become statistically significant when $\rho = 0.3$. In panel B of table 3 we calculate the values of ρ such that the legalization effect becomes zero ($\theta \approx 0$). As can be observed, a modest value of ρ could completely eliminate the positive effect of legalization on wages, health insurance, and bonuses.

The sensitivity analysis presented above indicates that the legalization effect found in previous studies is likely due to the omission of important variables that affect the outcome of interest and the probability of becoming a legal permanent resident. However, one may not conclude that omitted variables are completely responsible for all of the obtained effect if no information on the correct size of ρ is available. There is the possibility that this correlation is sufficiently close to zero, which would validate all analyses conducted thus far and assure the existence of a premium from becoming a legal permanent resident. To address this issue, we follow Altonji, Elder, and Taber (2005b, 2008) and take estimates based on the assumption that "selection on the unobservables is the same as selection on the observables" as lower bound estimates of the true parameter of interest.

In table 4 we present estimates of the bias when considering the log of wages as the dependent variable, and estimates of θ and ρ when employing the dummies for Wage ≥ $\overline{Wage}$, health insurance, and bonus receipt as dependent variables. In column 1, the estimated bias is approximately .5, which provides evidence of a potentially substantial bias in the OLS results presented in column 1 of table 2. The ratio between the estimated coefficient (.033) and the estimated bias (.5) measures the size of the shift in the distribution of the unobservables necessary to explain away the legalization effect. In this case, the ratio equals .066 and implies that the role of unobservables that determine wages would have to be more than .066 times the role of observables for the entire legalization effect to be explained away by the unobservables, which is very likely to be true.

Similar conclusions are reached by examining the other outcomes in columns (2)–(4). Evidence exists of a substantial selection on unobservables, given the high values of ρ calculated by $Cov(\mathbf{X}'\delta, \mathbf{X}'\lambda)/Var(\mathbf{X}'\delta)$. The lower bound estimates for all coefficients are negative and statistically significant. Thus, using the selection on the unobservables criteria proposed by Altonji, Elder, and Taber (2005b; 2008), we conclude that it is difficult to find conclusive and strong evidence of positive effects from becoming a legal permanent resident on wages, employer-sponsored health insurance, and monetary bonuses. This is the result suggested by Lofstrom, Hill, and Hayes (2010, 49), who used the New Immigrant Survey and found that "the data fail to reveal evidence of improved employment outcomes attributable to legal status," although they found positive effects for highly skilled unauthorized workers.

We obtained lower bound estimates of the legalization effect when observables affecting the independent variable are assumed to have the same relationship with the endogenous regressor as the unobservables. We now discuss the results obtained when using the technique developed by Millimet and Tchernis (2010) to assess the bias arising from the failure of the CIA.

Table 5 presents minimum-biased (β_{MB}) and bias-corrected (β_{MB-BC}) estimates of the legalization effect for all four independent

Table 5. Legalization Effects: Minimum-Biased and Bias-Corrected Estimations

Coefficient	*ln*(Hourly Wage) (1)	Wage ≥ $\overline{Wage}$ (2)	Health Insurance (3)	Bonus (4)
Panel A: ATE				
$\widehat{\beta_{MB\|\theta=0.05}}$	.062 [−.032,.095]	−.001 [−.058,.225]	.007 [−.046,.102]	.142 [−.081,.257]
$\widehat{\beta_{MB\|\theta=0.10}}$	.042 [−.008,.084]	.117 [−.001,.181]	.068 [−.026,.095]	.115 [−.043,.200]
$\widehat{\beta_{MB\|\theta=0.25}}$	.054 [.006,.068]	.130 [.032,.219]	.036 [−.025,.062]	.097 [.006,.166]
$\widehat{\beta_{MB-BC\|\theta=0.05}}$	.047 [−.054,.113]	−.131 [−.187,.157]	.008 [−.044,.125]	.097 [−.197,.194]
$\widehat{\beta_{MB-BC\|\theta=0.10}}$	.027 [−.036,.095]	−.013 [−.185,.148]	.068 [−.029,.134]	.069 [−.142,.164]
$\widehat{\beta_{MB-BC\|\theta=0.25}}$	.039 [−.029,.081]	−.016 [−.171,.142]	.036 [−.025,.088]	.051 [−.121,.130]
Panel B: ATT				
$\widehat{\beta_{MB\|\theta=0.05}}$	−.016 [−.047,.037]	.001 [−.050,.143]	.025 [−.028,.087]	−.017 [−.103,.079]
$\widehat{\beta_{MB\|\theta=0.10}}$	−.006 [−.038,.037]	.055 [−.015,.137]	.050 [−.020,.083]	.040 [−.049,.086]
$\widehat{\beta_{MB\|\theta=0.25}}$	.001 [−.029,.103]	.044 [−.035,.135]	.056 [−.001,.088]	.039 [−.036,.105]
$\widehat{\beta_{MB-BC\|\theta=0.05}}$	−.038 [−.107,.068]	−.115 [−.241,.108]	−.026 [−.130,.082]	−.197 [−.352,.020]
$\widehat{\beta_{MB-BC\|\theta=0.10}}$	−.028 [−.085,.067]	−.061 [−.220,.123]	−.001 [−.119,.091]	−.141 [−.317,.025]
$\widehat{\beta_{MB-BC\|\theta=0.25}}$	−.021 [−.075,.061]	−.072 [−.203,.100]	.005 [−.104,.085]	−.142 [−.285,.039]

Note: The 90% empirical confidence intervals were obtained using 200 bootstrap repetitions, presented in brackets.

variables. In panel A we report the ATE and in panel B the ATT estimated via equations 19 and 24 for the minimum-biased estimates, and equations 27 and 28 for the bias-corrected estimates, respectively. We consider three values of θ to select the size of the treatment and control groups to be included in the set Ω, $\theta = 0.05, 0.10$ and 0.25. In brackets, we present the 90% confidence intervals obtained using 200 bootstrap repetitions.

First, when examining the minimum-biased estimates, with the exception of the coefficients for *ln*(Hourly Wage) and the dummy for wages larger than average when $\theta = 0.25$, all other coefficients are statistically insignificant, regardless of what parameter (ATE or ATT) we consider. The positive legalization effect found for the log of hourly wages is, however, marginally significant, as the 90% confidence level excludes zero by .006. However, these are biased estimates, as it is very unlikely that at p^* the bias turns out to be exactly zero. This becomes evident once we examine the bias-corrected estimates, which are all smaller than the minimum-biased estimates. We can observe that none of the coefficients are significantly different from zero (for the ATE or ATT). Specifically for the ATT, with the exception of one parameter, all coefficients are negative and again, none are significant.

The evidence provided above indicates that by failing to control for any selection on unobservables, previous findings concluding that there are significant wage gains from becoming a legal permanent resident are severely biased. As we discussed above, one might control for several characteristics, but it is not difficult to conclude that some unobserved characteristic, for example perseverance, which is positively correlated with wages, is also likely to be correlated with the decision to enter the United States illegally and the probability of becoming a legal permanent resident. After controlling for some of this selection by estimating lower

Table 6. Legalization Effect Estimations Based on Klein and Vella (2009)

	ln(Hourly Wage) (1)	Wage $\geq \overline{Wage}$ (2)	Health Insurance (3)	Bonus (4)
Coefficient	−.021 [−.055, .034]	−.064 [−.159, .066]	.009 [−.054, .080]	−.043 [−.129, .066]

Note: The 90% empirical confidence intervals were obtained using 200 bootstrap repetitions presented in brackets.

bounds and minimizing or removing the bias from the failure of the CIA, we find that becoming a legal permanent resident has no relationship with wages, employer-sponsored health insurance, or additional monetary bonuses.

Robustness Checks

In this section, we check the robustness of the results obtained thus far by estimating the legalization effect using (a) the IV estimator proposed by Klein and Vella (2009), and (b) considering the full samples of males (married and unmarried) and workers (full-time and part-time). Klein and Vella's (2009) approach, which also aims to circumvent estimation in the absence of an exclusion restriction, relies on the presence of heteroscedasticity to identify the parameter of interest by first estimating the probability of treatment from a binary response model, and then using it as an instrument for the treatment variable. Before presenting the method, it is useful to first discuss why our model might be heteroscedastic. To do so, one simply needs to argue that the variables included in our model primarily capture differences in average characteristics that may vary considerably across individuals. Thus, the model, beyond accounting for mean differences, does not capture individual differences in the effect of the treatment.

Now consider the model presented in equations 7 and 8. Let the error term be characterized by:

$$(29)\qquad \varepsilon = S(X\rho)\varepsilon^{*}$$

where $S(\cdot)$ is an unknown function and ε^{*}, as in Millimet and Tchernis (2010), is assumed to be drawn from a standard normal density function. The treatment probability conditional on X is then calculated by:

$$(30)\qquad Prob(L=1|X) = \Phi\left(\frac{X}{S(X)}\delta\right).$$

If one assumes $S(X) = e^{X\Theta}$, then one can estimate the δ parameters by maximum likelihood estimation (MLE), where the log-likelihood function is given by:

$$(31)\qquad lnL = \sum_{i}\left[\ln\left(\Phi\left(\frac{X\delta}{e^{X\Theta}}\right)\right)\right]^{T_i} \times \left[\ln\left(1-\Phi\left(\frac{X\delta}{e^{X\Theta}}\right)\right)\right]^{1-T_i}.$$

The resulting estimates are then used to predict the probability of receiving the treatment and are considered valid instruments for the variable of interest.

The estimated coefficients are presented in table 6. As expected, they confirm the results previously reported in this article: becoming a legal permanent resident appears not to affect the labor market outcomes of previously undocumented agricultural workers, or at least support the claim that the previous results are not as consistent and conclusive as they appear to be, that is, under slightly weaker assumptions their results fail to hold.

Regarding the estimates for married and unmarried males and for full-time and part-time workers, in table 7 we observe that the OLS, Probit, and Matching estimates considering unmarried and married males are quite similar to those censoring the data to only include unmarried males, although these results are smaller in the matching estimates. Our motivation for restricting the sample to unmarried males was to account for the possibility that married males might have access to health insurance through their wives' employment. Surprisingly, the coefficient on health insurance was still positive and statistically significant for the matching estimates. Receiving bonuses, however, does not appear significant and the estimates are numerically small.

Regarding full-time and part-time workers, we again observe that the results are smaller than those considering only full-time workers; however, they are still statistically significant for wages and bonuses under probit and OLS, and for wages and health insurance under matching. We should emphasize that our

Table 7. Robustness Check: Estimations for Males (Unmarried and Married) and Part-time Workers

Estimation	*ln*(Hourly Wage) (1)	Wage $\geq \overline{Wage}$ (2)	Health Insurance (3)	Bonus (4)
Males (Unmarried and Married)				
OLS	.038***	.087***	.041***	.054***
	(.006)	(.014)	(.009)	(.012)
Probit		.251***	.187***	.175***
		(.042)	(.055)	(.045)
		[.082]	[.029]	[.043]
Nearest Neighbor Matching	.038*	.069**	.065***	.009
	(.020)	(.037)	(.021)	(.033)
Workers (Full-time and Part-time)				
OLS	.034***	.094***	.013	.044**
	(.010)	(.025)	(.013)	(.019)
Probit		.275***	.032	.149*
		(.076)	(.102)	(.084)
		[.106]	[.003]	[.028]
Nearest Neighbor Matching	.033	.114**	.060**	.039
	(.025)	(.056)	(.024)	(.049)

Notes: Robust standard errors are presented in parentheses (bootstrapped standard errors for matching estimates). Linear probability models are estimated for columns (2)–(4) under OLS. Marginal effects are presented in brackets for probit estimates. $^{***}p < 1\%$, $^{**}p < 5\%$, $^{*}p < 10\%$.

motivation for censoring to include only full-time workers is that they are, in theory, more likely to be eligible for these benefits (health insurance and bonus). However, if legalization positively affects the probability of being a full-time worker, then estimates of the legalization effect based on this subsample alone would be biased upward. Therefore, we would expect to find a small effect or no effect of legalization on benefits, which is the case given the numbers presented in the table. A regression considering the correlation between being a full-time worker and the legalization dummy delivers a positive coefficient of .027 (.019), which is not significantly different from zero at any conventional statistical significance level.

In table 8, we examine how sensitive estimates of the legalization effect are to variation in the correlation between the error terms in the bivariate probit model for the samples of married and unmarried males and full-time and part-time workers. Imposing a correlation of $\rho = 0.1$ is sufficient to make all of the coefficients, except for the dummy for wages when considering the sample of unmarried and married males, statistically insignificant. Again, increasing ρ to 0.2 is sufficient to shift all coefficients to negative values, and causes them to be statistically significant when $\rho = 0.3$. Compared to table 4, the values of ρ that are necessary to completely eliminate the positive effect of legalization on wages, health insurance, and bonuses are all smaller when considering the complete sample of males and the complete sample of workers. The only ρ that is larger is for health insurance, which shifts from .072 to .112, and is still very small. Thus, the results for our subsample of unmarried and full-time workers are qualitatively unchanged when including married and part-time workers.

Conclusions

The question of whether becoming a legal permanent resident affects the economic returns of immigrants has been the focus of many empirical studies over the past two decades. These studies' results have consistently shown that there are significant wage differences between legal and illegal workers, even when controlling for several demographic characteristics. However, the validity of such results has been questioned by many researchers, given their lack of solid identification strategies to correctly account for omitted variables.

In this article we move away from the methods previously used to study the subject, which for the most part rely on the selection on observables, and propose using recently-developed techniques designed specifically to address the issue of selection into treatment based (to some degree) on unobservable variables. We begin by evaluating how sensitive

Table 8. Robustness Check: Sensitivity of Legalization Effects for Males (Unmarried and Married) and Part-time Workers

	Wage ≥ $\underline{Wage}$	Health Insurance	Bonus
Males (Unmarried and Married)			
Panel A			
ρ	(1)	(2)	(3)
0.0	.251***	.187***	.175***
	(.042)	(.055)	(.045)
	[.082]	[.029]	[.043]
0.1	.086**	.020	.008
	(.042)	(.054)	(.045)
	[.034]	[.003]	[.002]
0.2	−.081*	−.148***	−.160***
	(.042)	(.054)	(.045)
	[−.032]	[−.019]	[−.040]
0.3	−.249***	−.318***	−.329***
	(.041)	(.053)	(.044)
	[−.097]	[−.040]	[−.081]
Panel B			
ρ	.152	.112	.105
Workers (Full-time and Part-time)			
Panel A			
ρ	(1)	(2)	(3)
0.0	.275***	.032	.149*
	(.076)	(.102)	(.084)
	[.106]	[.003]	[.028]
0.1	.103	−.137	−.024
	(.075)	(.101)	(.084)
	[.039]	[−.012]	[−.004]
0.2	−.071	−.304***	−.196**
	(.075)	(.100)	(.083)
	[−.026]	[−.025]	[−.031]
0.3	−.246***	−.472***	−.368***
	(.073)	(.098)	(.081)
	[−.089]	[−.036]	[−.055]
Panel B			
ρ	.159	.019	.086

Note: Standard errors are presented in parentheses and marginal effects in brackets. For panel B, values of ρ are calculated such that the legalization effect becomes zero. $^{***}p < 1\%$, $^{**}p < 5\%$, $^{*}p < 10\%$.

estimates of the legalization effect are when the degree of selection on unobservables increases relative to the case in which selection is completely driven by observables, which is what has been assumed in most previous studies. We then obtain lower bound estimates based on the notion that the degree of selection on observed characteristics is the same as the degree of selection on unobserved characteristics (Altonji, Elder, and Taber 2005b; 2008). As prior results indicate that becoming legal has positive benefits, obtaining lower bound estimates of the parameter of interest under weaker selection assumptions is very intuitive and useful, as these values ought to be larger than zero if the causal effects were truly robust. Additionally, we employ the method proposed by Millimet and Tchernis (2010) that allows one to obtain estimates of the parameter of interest while accounting for the bias arising from failure of the conditional independence assumption, which is required to ensure the consistency of propensity score estimators.

Our results contradict the finding that has consistently been reported in the literature, that obtaining legal residence benefits workers by positively affecting their wages and many other important outcomes (although some studies note that these benefits might be small, they find statistically significant positive effects; see, e.g., Kandilov and Kandilov 2010 and Pena 2010). We show that a modest degree of selection on unobservables is sufficient to completely eliminate the previously obtained positive effects. Additionally, under the notion that selection on observables is the same as selection on unobservables, we find that the role of unobservables that determine wages would have to be more than .066 times the role of observables for the entire legalization effect to be explained away by the unobservables, which is very likely to be true. Using the technique developed by Millimet and Tchernis (2010), we arrive at the same conclusions, because all of the estimated coefficients are not significantly different from zero. This is also obtained when using the IV estimator proposed by Klein and Vella (2009), and considering different samples (including married males and part-time workers). Thus, our results shed light on an important subject regarding the immigration policy of the United States; we provide support for the theory that lower skill levels—and not discrimination—explain differences in the economic outcomes of immigrants, which was previously suggested by Borjas (1990).

Acknowledgments

The authors thank Mary Arends-Kuenning, Monserrat Bustelo, Darren H. Lubotsky, Leonardo Lucchetti, Euler Mello, Elizabeth Powers, and Everardo Sampaio for valuable discussions and Amy Kandilov, Ivan Kandilov, and Daniel Millimet for sharing their Stata codes with us. The authors also thank editor J. Edward Taylor and three anonymous referees for their very helpful comments. Any remaining errors are the responsibility of the authors.

References

Altonji, J.G., T.E. Elder, and C.R. Taber. 2002. Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools. Manuscript (April), Department of Economics, Northwestern University.

Altonji, J.G., T.E. Elder, and C.R. Taber. 2005a. An Evaluation of Instrumental Variable Strategies for Estimating the Effects of Catholic Schooling. *Journal of Human Resources* 40 (4): 791–821.

———. 2005b. Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools. *Journal of Political Economy* 113 (1): 151–184.

———. 2008. Using Selection on Observed Variables to Assess Bias from Unobservables When Evaluating Swan-Ganz Catheterization. *The American Economic Review: Papers and Proceedings* 98 (2): 345–350.

Angrist, J., and J.S. Pischke. 2008. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton, NJ: Princeton University Press.

Bellows, J., and E. Miguel. 2009. War and local collective action in Sierra Leone. *Journal of Public Economics* 93 (11–12): 1144–1157.

Black, D.A., and J.A. Smith. 2004. How robust is the evidence on the effects of college quality? Evidence from matching. *Journal of Econometrics* 121 (1–2): 99–124.

Borjas, G.J. 1990. *Friends or Strangers: The Impact of Immigrants on the United States Economy*. New York: Basic Books.

Cavalcanti, T., J. Guimaraes, and B. Sampaio. 2010. Barriers to skill acquisition in Brazil: Public and private school students performance in a public university entrance exam. *Quarterly Review of Economics and Finance* 50 (4): 395–407.

Heckman, J., and S. Navarro-Lozano. 2004. Using Matching, Instrumental Variables, and Control Functions to Estimate Economic Choice Models. *Review of Economics and Statistics* 86 (1): 30–57.

Heckman, J.J., and R. Robb, Jr. 1985. Alternative methods for evaluating the impact of interventions: An overview. *Journal of Econometrics* 30 (1–2): 239–267.

Hirano, K., and G.W. Imbens. 2001. Estimation of Causal Effects using Propensity Score Weighting: An Application to Data on Right Heart Catheterization. *Health Services and Outcomes Research Methodology* 2 (3): 259–278.

Imbens, G., and J.M. Wooldridge. 2009. Recent Developments in the Econometrics of Program Evaluation. *Journal of Economic Literature* 47 (1): 5–86.

Iway, N., R.D. Emerson, and L.M. Walters. 2006. Legal Status and U.S. Farm Wages. Paper presented at Southern Agricultural Economics Association Annual Meeting, Orlando, Florida.

Kandilov, A.M.G., and I.T. Kandilov. 2010. The Effect of Legalization on Wages and Health Insurance: Evidence from the National Agricultural Workers Survey. *Applied Economic Perspectives and Policy* 32 (4): 604–623.

Kaushal, N. 2006. Amnesty Programs and the Labor Market Outcomes of Undocumented Workers. *Journal of Human Resources* 41 (3): 631–647.

Klein, R., and F. Vella. 2009. A semiparametric model for binary response and continuous outcomes under index heteroscedasticity. *Journal of Applied Econometrics* 24 (5): 735–762.

Kossoudji, S.A., and D.A. Cobb-Clark. 2002. Coming out of the Shadows: Learning about Legal Status and Wages from the Legalized Population. *Journal of Labor Economics* 20 (3): 598–628.

Lofstrom, M., L.E. Hill, and J.J. Hayes. 2010. Did Employer Sanctions Lose Their Bite? Labor Market Effects of Immigrant Legalization. IZA Discussion Papers 4972, Institute for the Study of Labor (IZA).

McNamara, P., and C. Ranney. 2002. Hired Farm Labour and Health Insurance Coverage. In *The Dynamics of Hired Farm Labor: Constraints and Community Responses*, ed. J. L. Findeis, Ann Vandeman, and J. Runyan, 219–232. New York, NY: CABI Publishing.

Millimet, D.L., and R. Tchernis. 2010. Estimation of Treatment Effects without an Exclusion Restriction: With an Application to the Analysis of the School Breakfast Program. Manuscript (March), Department of Economics, Southern Methodist University.

Passel, J. 2005. *Unauthorized Migrants Numbers and Characteristics*. Washington, DC: Pew Hispanic Center.

Passel, J., and D. Cohn. 2009. *A Portrait of Unauthorized Immigrants in the United States*. Washington, DC: Pew Hispanic Center.

Pena, A.A. 2010. Legalization and Immigrants in U.S. Agriculture. *B.E. Journal of Economic Analysis & Policy* 10 (1): 1–22.

Rivera-Batiz, F.L. 1999. Undocumented workers in the labor market: An analysis of the earnings of legal and illegal Mexican immigrants in the United States. *Journal of Population Economics* 12 (1): 91–116.

Rosenbaum, P.R., and D.B. Rubin. 1983. The central role of the propensity score in observational studies for causal effects. *Biometrika* 70 (1): 41–55.

Rubin, D.B. 1974. Estimating causal effects of treatments in randomized and nonrandomized studies. *Journal of Educational Psychology* 66 (5): 688–701.

Tran, L.H., and J.M. Perloff. 2002. Turnover in U.S. Agricultural Labor Markets. *American Journal of Agricultural Economics* 84 (2): 427–437.