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On Estimating The Effects of Legalization: Do Agricultural Workers Really Benefit?

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ON ESTIMATING THE EFFECTS OF LEGALIZATION: DO AGRICULTURAL WORKERS REALLY BENEFIT?

The question of whether legalization affects the economic returns of immigrants has been the focus of many empirical studies in the past two decades. Their results have consistently shown that there exists significant wage differences between legal and illegal workers. However, the validity of such findings have been questioned by many researchers, given the lack of good identification strategies to correctly account for omitted variables. In this article we move away from the methods previously used in the literature, which in most part rely on selection on observables, and propose to use recently developed techniques designed specifically to address the issue of selection into treatment based (in some degree) on unobservable variables. Our results highlight that measuring such effects is much more difficult, from an econometrics standpoint, than what previous analysis claim and suggest that lower skill levels and not discrimination explain differences in economic outcomes of immigrants.

Keywords: economic outcomes; undocumented workers; immigration; identification.

JEL Classification: J31, J32, J43, J71.

The United States has experienced a substantial increase in the number of undocumented immigrants entering and overstaying beyond their legally permitted time in the past decades (Passel 2005). According to estimates provided by Passel and Cohn (2009), the country is now home to approximately 12 million unauthorized immigrants. This phenomena caused dramatic changes in the agricultural workforce, with the ratio of undocumented and legal farm workers raising from only 16% in the years of 1989-92 to 36% in 1993-95 and 50% in 1998-2000 (Iway, Emerson, and Walters 2006). In a attempt to control this flow of illegal immigrants, the U.S. government proposed several changes regarding its immigration policy. It not only increased border security but introduced sanctions for U.S. employers who 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), allowing agricultural workers to acquire legal permanent residence status.

These important changes naturally lead researchers to question the potential effects of legalization on the economic returns of affected and unaffected workers, specially considering the impressive size of the undocumented population residing in the U.S. On 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 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 hypothesis 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 outcomes. The contrary will happen, however, if undocumented immigrants receive low wages because of discrimination.

Given this debate, several analysis devoted to the estimation of the legalization effect on economic outcomes have been carried out. Results have consistently shown that there exists a significant wage difference between legal and illegal workers, even when discounting for a whole set of demographic characteristics, which would support the hypothesis that illegal workers are discriminated against in the labor market. Differences are also observed when looking at several other variables such as, for example, the probability of receiving employer-sponsored health insurance and the probability of aid program participation.

Most of the papers in the literature, however, acknowledge the difficulty of measuring such effects, given workers are not randomly assigned to legal permanent resident and undocumented groups. That is, even accounting for important observable differences between individuals, such as income and education, it is still hard to argue that other unobserved variables affecting economic outcomes are uncorrelated with the likelihood of becoming a legal resident. Thus, researchers have tried in one way or another to circumvent such problems by using methodologies that in theory would minimize or even eliminate completely the omitted variable bias. These empirical strategies may be divided into two groups. The first uses cross-sectional data and compares 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 most two 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 and the probabilities of receiving employer-sponsored health insurance and/or monetary bonus and of participating in safety net programs. Their results imply significant positive effects of being legal in U.S. Agriculture. Looking specifically at wages, both papers

estimate that legal immigrants make on average 5-6% more in hourly wages.

In the second group, the most influential papers are Rivera-Batiz (1999), Kossoudji and Cobb-Clark (2002) and Kaushal (2006). The former two analyze the Immigration Reform and Control Act (IRCA) of 1986 and the latter the Nicaraguan Adjustment and Central American Relief Act (NACARA) of 1997. 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. Similarly 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), under the NACARA, found modest 3-4% gains.

A main 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 is not clear why it shouldn't in the current application, then ignoring selection on unobservables might lead to significantly biased estimates. That is, one might control for several characteristics, but it is not hard to argue that some unobserved variable, like perseverance, which is positively correlated to wages, is also likely to be correlated with the decision to enter the United States illegally and to the probability of becoming a legal permanent resident. Pena (2010) try to address this issue by using treatment effects regressions (which is analogous to a bivariate probability model with a continuous dependent variable) and use as exclusion restrictions the years of initial entry in the U.S. The main identifying assumption is that, conditional on a set of controls that include work experience and survey year, entry year should be uncorrelated with workers' outcomes but should affect signifi-

cantly the ability of immigrants to receive legal status. We believe such assumption to be very strong, given policy changes facilitating amnesty were also accompanied by changes toward undocumented immigrants currently living in the U.S. and also had a direct effect on newcomers to the U.S. Thus, it is not clear why the correlation between the error term of the equation of interest and dummy variables for the periods that had policy changes is zero.

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 is that the program not only facilitated amnesty but also changed significantly the policy toward undocumented immigrants currently living in the U.S. 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 more broader sense, the validity of such approaches depend mainly on how convincing are the comparison groups chosen as counterfactuals for the workers who were legalized under such programs, which has been questioned by many researchers.

The bottom line is that none of the studies discussed above are bullet proof, specially the ones relying on selection on observables for identification, which are mainly the ones devoted to analyze agricultural workers. In this article we propose the use of less restrictive approaches to address the issue of non-random selection into legal status. Firstly, following Altonji, Elder, and Taber (2005b, 2008), we evaluate how sensitive are estimates of the legalization effect when the degree of selection on unobservables increases relative to the case in which selection is completely driven by observables (as previous studies have relied on). Following their notion that the degree of selection on observed characteris-

tics 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 would be very useful, given these lower bounds need necessarily be larger than zero if causal effects were really robust (given previous results show positive benefits of becoming legal). Secondly, we use a recently developed technique proposed by Millimet and Tchernis (2010) which, under some assumptions, allows one to obtain estimates of the parameter of interest taking into account the bias arising from failure of the conditional independence assumption (CIA) required for consistency of propensity score estimators. We analyze not only the effect of legalization on wages, but we follow Kandilov and Kandilov (2010) and also analyze if 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 could also affect other forms of compensation, such as health insurance, which is of particular interest for policy makers given its low coverage rate among this 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 obtain 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. Thus, non-random selection appears to be an important issue in the present discussion. The results obtained by 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 become all statistically insignificant. Thus, our analysis point

in the direction 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 evidences provided so far, was already suggested by Borjas (1990) several years ago, when he wrote that "[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 analyze the robustness of the findings previously presented in the literature in regards to the effect of becoming a legal permanent resident in the U.S. Agricultural sector. Given previous papers rely on strong assumptions about selection into treatment (in this case, into legalization), we see our article as an important step into not only understanding the relationship between legal status and economic outcomes, but, more importantly, into highlighting that measuring such effects is much more difficult, from an econometrics standpoint, than what previous analysis claim. Hence, a main contribution of our article is to use the National Agricultural Workers Survey (NAWS), a nationally representative data set of employed U.S. farm workers and widely used to answer the question proposed in this article, to show that previous results are weak under slightly different (and weaker) assumptions.

After this introduction, 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 comes from the National Agricultural Workers Survey (NAWS), which is a nationally representative data set containing information on demographic, employment, and health characteristics of employed U.S. farm workers. Among a few advantages of this dataset, such as sample design that accounts for migratory behavior and 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), it contains information on legal status of respondents, allowing the researcher to identify legal permanent residents (for example, naturalized citizens, green cards holders, and other work authorizations) and undocumented workers.

We use NAWS samples for the years of 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 residents¹ or undocumented workers. Additionally, given most of the individuals surveyed in the NAWS are males, we exclude female agricultural workers, and focus only on unmarried males, since married males access to health insurance is facilitated through their wives employment. Finally, we consider only full-time agricultural workers (those who work at least 35 hours per week), given 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 the population of undocumented agricultural workers experience very low job mobility, given they are legally restricted from the formal economy. This implies that they have fewer jobs to choose from, which leads to lower wages on average. Hence, one may easily argue that an undocumented worker that is granted with amnesty will leave farm work and look for 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 both workers who were granted legal permanent resident status under IRCA and those who are undocumented workers. If this is the case, then 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 (about 7.21%). Not surprisingly, about 54.3% of legal permanent residents have wages that are larger than the average wage (this statistic is about 35.2% for undocumented workers). Looking at employer-sponsored health insurance and bonuses, 13.7% and 32.1% of legal workers receive such benefits, respectively, while these numbers are only 5.2% and 14.3% for undocumented workers.

Besides these significant differences in outcomes, legal and undocumented workers are also different 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, number of children, among others. Thus, these divergencies are important to be accounted for when comparing both types of workers. However, as emphasized below, many other unobserved factors not accounted for in previous studies might also differ significantly between both types of workers leading to biased estimates.

[Table 1 about here]

Methodology

In this section we begin by specifying the main equation of interest estimated in most previous 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) that allows one to look at how sensitive are estimates to assumptions about the amount of selection on unobservables and to obtain lower bound estimates of the legalization effect when variables unobserved to 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 failure of the conditional independence assumption (CIA) required for consistency of propensity score estimators.

Probit, OLS and Matching Estimates

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

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

where y is an outcome of interest, L is a dummy variable that assumes value equal to 1 when the worker is a legal permanent resident and 0 otherwise, \mathbf{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, that workers be randomly assigned legal permanent residents or assigned on the basis of 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 in many empirical applications² (ours included), finding a convincing IV is not always viable and one must rely on different identification strategies to infer about the parameter of interest.

Kandilov and Kandilov (2010), recognizing that by directly comparing legal permanent residents with 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 to estimate 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

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

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 on the average treatment effect on the treated (ATT) which are defined as

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

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

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

Hence, one must find different individuals (some treated and some not) such that after adjusting for differences in observed characteristics, or pretreatment variables, comparisons are allowed to be made (see Angrist and Pischke 2008). This is exactly the idea behind matching estimators which, under the conditional independence (CIA) or unconfoundedness assumption (see Rubin 1974 and 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

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

$$(6) \quad \beta_{ATT} = E[\beta_i|L = 1, p(\mathbf{X}_i)] = E[y_{1i} - y_{0i}|L = 1, p(\mathbf{X}_i)]$$

Conditioning on the propensity score basically implies that the distribution of covariates for the untreated workers are balanced in a way that it looks very similar to the distribution of covariates for the treated workers, which makes comparisons between outcomes more reasonable when compared to estimates obtained via equation 1. Hence, 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 due to the fact that selection into treatment is

based also on variables unobserved to the researcher but correlated with the outcome of interest (L) (that is, $COV(L, \varepsilon) \neq 0$ in equation 1, which also implies that CIA fails to hold). Hence, we use two techniques described below to analyze how robust are estimates previously obtained to selection on unobservables. The first, focuses on bounding a measure of the treatment effect (Altonji, Elder, and Taber 2005b, 2008) and the second 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 utilized by Altonji, Elder, and Taber (2005b, 2008) to assess how unobservables might affect the coefficient β obtained via equation 1. Secondly, 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

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

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

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

where L and \mathbf{X} are defined 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 it affects the outcome of interest. Similarly, a negative correlation ($\rho < 0$) implies that unobserved factors that affect positively the probability of being a legal permanent resident, affect negatively the outcome y .

The model presented in equations 7-9 is identified given the normality assumption even without an exclusion restriction (though, as pointed out by Altonji, Elder, and Taber (2005b), researchers take results from this model cautiously when there is no exclusion restriction), which would be required for semi-parametric identification. Hence, to assess how sensitive are estimates under some degree of selection on unobservables, Altonji, Elder, and Taber (2005b, 2008) propose to take this model as if it was underidentified by one parameter, namely ρ . Thus, their strategy is to constrain the model to certain values of ρ and to look at 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 if 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 exactly 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 whatsoever 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 the idea 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

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

Given $\phi_{\mathbf{X}'\delta}$ and ϕ_{ϑ} , which capture how L is dependent on observables ($\mathbf{X}'\delta$) and unobservables (ϑ), the idea 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 part of y^* that is captured by observables has same relationship with L^* as the part that is captured by unobservables. Note that by setting $\phi_{\vartheta} = 0$ is the same as setting $\rho = 0$ or estimating equation equation 1 directly.

The assumptions required to hold for the validity of this approach are precisely presented in Altonji, Elder, and Taber (2002). Following them, we take estimations based on $\phi_{\mathbf{X}'\delta} = \phi_{\vartheta}$ and on $\phi_{\vartheta} = 0$ as 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 to have the same impact as selection on the observables are negative and significant, which 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, i.e.,

$$(11) \quad 0 \leq \rho \leq \frac{COV(\mathbf{X}'\delta, \mathbf{X}'\lambda)}{Var(\mathbf{X}'\delta)}$$

Hence, the main 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

We have describe above the Altonji, Elder, and Taber (2005b, 2008) method that allows the researcher to look at how sensitive are estimates to assumptions about the amount of selection on unobservables and to obtain lower bound estimates of the legalization effect when variables unobserved to the econometrician are correlated with the outcome of interest. In this section we present a recently developed methodology proposed by Millimet and Tchernis (2010) which considers the bias arising from failure of the conditional independence assumption (CIA) required for consistency of propensity score estimators. We start by briefly describing the bias that arise when the CIA fails to hold and then present the minimum-biased and bias-corrected estimators.

To analyze the bias that arise when the CIA fails, consider the following two assumptions made by Millimet and Tchernis (2010) (and 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

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

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 captures unobserved individual gains from treatment.

The main idea behind the minimum-biased estimator is to choose 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 $p(X) = 0.5$. Hence, they recommend that the average treatment effect on the treated should be estimated using only observations in the neighborhood of $p(X) = 0.5$, such as 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 $\rho_{\delta u}\sigma_\delta$. They propose the following minimum biased estimator which derives from the normalized weighting estimator previously proposed by Hirano and Imbens (2001):

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

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

At this stage, the question that comes to mind is how to obtain estimates of p^* . For that, 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^* . For that, they invoke 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, given one knows $p^* = 0.5$ (by equation 17), an estimator along the lines of 19 is given by

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

Until now we have characterized the bias (under the assumptions described above) and how to get minimum-biased estimators for ATE and ATT when CIA fails to hold. However, given 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 get 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 start by describing the results obtained via probit, OLS and matching estimators. As specified in the data section, we use four outcomes: the *log* of hourly wages ($\ln(\text{Hourly Wage})$), a dummy that equals 1 if the worker wage is above the average wage of all workers in the sample and 0 otherwise ($\text{Wage} \geq \overline{\text{Wage}}$), 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 presents the estimates. As expected, given 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). We know, however, that one should interpret such coefficients very carefully, given that becoming a legal permanent resident are choices 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 conclusions reported in previous studies that the wage premium of becoming a legal permanent resident is of about 5.5%.⁴ As for the other outcomes, legalization seems to affect the probability of receiving employer-sponsored health insurance and monetary bonuses.

[Table 2 about here]

We now turn to the analysis of how robust are these results when unobservables are allowed to be correlated with the legalization variable. Let us first start by looking at how

sensitive are estimates of the legalization effect 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. Starting from the 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, given the existence of unobserved factors are assumed away. Imposing a correlation of only $\rho = 0.1$ is sufficient to make all coefficients statistically insignificant. For the wage dummy, for example, the marginal effect drops from .088 to only .038 while for the bonus dummy it 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 to be 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.

[Table 3 about here]

The sensitivity analysis presented above does point in the direction that the legalization effect found in previous studies is likely due to the omission of important variables that affect the outcome of interest and also the probability of becoming a legal permanent resident. However, one may not conclude that omitted variables are completely responsible for all the effect found if no information on the correct size of ρ is available. It might happen that the correlation is sufficiently close to zero that all the analysis conducted so far are approximately correct and there exists 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 dependent variable as well as estimates of θ and ρ when considering as dependent variable dummies for $Wage \geq \overline{Wage}$, health insurance, and bonus. 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.

Looking at the other outcomes in columns (2)-(4), similar conclusions are obtained. There is evidence of 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 concluding and strong evidences of positive effects when becoming a legal permanent resident on wages, employer-sponsored health insurance, and monetary bonuses. This is a results suggested by Lofstrom, Hill, and Hayes (2010), 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.

[Table 4 about here]

We have obtained lower bound estimates of the legalization effect when observables affecting the independent variable are assumed to have the same relationship as unobservables with the endogenous regressor. We now discuss the results obtained when using the

technique developed by Millimet and Tchernis (2010) to assess the bias arising from failure of the conditional independence assumption (CIA).

Table 5 presents minimum-biased (β_{MB}) and bias-corrected (β_{MB-BC}) estimates of the legalization effect for all of our four independent variables. In panel A we report the average treatment effect (ATE) and in panel B the average treatment effect on the treated (ATT) estimated via equations 19 and 24 for the minimum-biased estimates and 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 90% confidence intervals obtained using 200 bootstrap repetitions.

Looking first at the minimum-biased estimates, with the exception of the coefficients for $\ln(\text{Hourly Wage})$ 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 are looking at. The positive legalization effect found for log of hourly wages is, however, marginally significant as the 90% confidence level excludes zero by .006. These are, however, biased estimates given it is very unlikely that at p^* the bias turns out to be exactly zero. This becomes very evident once we look at the bias-corrected estimates, which are all smaller than the minimum-biased estimates. We can observe that all coefficients are not statistically 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.

[Table 5 about here]

The evidences provided above point in the direction that by not controlling for any selection on unobservables, previous evidences 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 hard to convince that some unobserved characteristic, like perseverance, for example, which is positively correlated to

wages, is also likely to be correlated with the decision to enter the United States illegally and to the probability of becoming a legal permanent resident. After controlling for some of this selection by estimating lower bounds and minimizing or removing the bias from the failure of the CIA, we obtain that becoming a legal permanent resident has no relation with wages, employer-sponsored health insurance or the gains of additional monetary bonuses.

Robustness Checks

In this section we check the robustness of the results obtained so far by estimating the legalization effect using (a) the IV estimator proposed by Klein and Vella (2009) and (b) considering the full sample of males (married and unmarried) and workers (full-time and part-time). Klein and Vella (2009)'s approach, which is also devoted 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. For that, one needs just to argue that the variables included in our model capture mostly differences in average characteristics which may vary considerably between individuals. Hence, the model, besides 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

$$\epsilon = S(X_{\varrho})\epsilon^*$$

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. The treatment probability conditional on X is

then calculated by

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

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

$$\ln L = \sum_i \left[\ln\left(\Phi\left(\frac{X\delta}{e^{X\Theta}}\right)\right) \right]^{T_i} \left[\ln\left(1 - \Phi\left(\frac{X\delta}{e^{X\Theta}}\right)\right) \right]^{1-T_i}$$

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

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

[Table 6 about here]

Now looking at the estimations for married and unmarried males and for full-time and part-time workers, we observe in table 7 that OLS, Probit, and Matching estimates considering to unmarried and married males are quite similar to the ones only censoring to unmarried males, although smaller when looking at the matching estimates. Our motivation for restricting the sample to unmarried males was to account for the fact 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 show up as significant and estimates are numerically small.

Looking at full-time and part-time workers, we observe that again results are smaller than the ones considering only full-time workers, however, still statistically significant for wages and bonuses under probit and OLS and for wages and health insurance under matching. We should emphasize that our motivation for censoring to full time workers is that they are the ones, in theory, more likely to be eligible for these benefits (health insurance and bonus). However, if legalization affects positively the probability of being a full-time worker, then estimations of the legalization effect based only on this subsample would be biased upward. Hence, we would expect to find small or no effect of legalization on benefits, which is the case given the numbers presented in the table. A regression looking at the correlation between being a full-time worker and the legalization dummy delivers a positive coefficient of .027 (.019), not statistically different from zero at any conventional statistical significance level.

[Table 7 about here]

In table 8 we look at how sensitive are estimates of the legalization effect to variation in the correlation between the error terms in the bivariate probit model 7-9 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 coefficients, but 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 a negative value and 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, still very small. Thus, results for our subsample of unmarried and full-time workers are qualitatively the same when including married and part-time workers.

[Table 8 about here]

Conclusions

The question of whether legalization affects the economic returns of immigrants has been the focus of many empirical studies in the past two decades. Their results have consistently shown that there exists significant wage differences between legal and illegal workers, even when controlling for several demographic characteristics. However, the validity of such results have been questioned by many researchers, given they lack of good identification strategies to correctly account for omitted variables.

In this article we move away from the methods previously used in the subject, which in most part rely on selection on observables, and propose to use recently developed techniques designed specifically to address the issue of selection into treatment based (in some degree) on unobservable variables. We start by evaluating how sensitive are estimates of the legalization effect 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). Given previous results show positive benefits of becoming legal, obtaining lower bound estimates of the parameter of interest under weaker selection assumptions is very intuitive and useful, given these number ought to be larger than zero if causal effects were really robust. Additionally, we use the method proposed by Millimet and Tchernis (2010) which allows one to obtain estimates of the parameter of interest taking into account the bias arising from failure of the conditional independence assumption (CIA) required for consistency of propensity score estimators.

Our results contradict what has consistently been reported in the literature that legalization does benefit workers by positively affecting their wages and many other important outcomes (although some studies point that these benefits might be small, they do find statistically significant positive effects). We show that a modest degree of selection on unobservables is sufficient to completely eliminate the positive effects previously obtained. Additionally, under the notion that selection on observables is the same as selection on unobservables, we obtain 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 Millimet and Tchernis (2010) technique, we arrive at the same conclusions, given all estimated coefficients are not statistically 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 sheds light on an important subject regarding the immigration policy of the United States, given we provide support to the theory that lower skill levels and not discrimination explain differences in economic outcomes of immigrants, what has been previously suggested by Borjas (1990).

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Notes

¹Foreign-born individuals legally authorized to work in the U. S.

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

³The propensity score is estimated using a logit model similar to that of Kandilov and Kandilov (2010). Given the focus of the article is 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.

⁴Note 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 1: Summary Statistics

Variables	Legal Permanent Residents	Undocumented Workers	Differences
Hourly wage (in 2006 dollars)	8.400 (1.922)	7.679 (1.604)	.721***
<i>ln</i> (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.

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

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

Note: 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. *** represents $p < 1\%$, ** represents $p < 5\%$ and * represents $p < 10\%$.

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

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

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. *** represents $p < 1\%$, ** represents $p < 5\%$ and * represents $p < 10\%$.

Table 4: Legalization Effects under Equality of Selection on Observables and on Unobservables

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

Note: (Bootstrapped) Standard errors are presented in parentheses and marginal effects in brackets. *** represents $p < 1\%$, ** represents $p < 5\%$ and * represents $p < 10\%$.

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

Coefficient	$\ln(\text{Hourly Wage})$	$\text{Wage} \geq \overline{\text{Wage}}$	Health Insurance	Bonus
	(1)	(2)	(3)	(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: 90% empirical confidence intervals obtained using 200 bootstrap repetitions presented in brackets.

Table 6: Legalization Effects Estimations Based on Klein and Vella (2009)

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

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

Table 7: Robustness Check: Estimations for Males (unmarried and married) and Part-time workers

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

Note: 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. *** represents $p < 1\%$, ** represents $p < 5\%$ and * represents $p < 10\%$.

Table 8: Robustness Check: Sensitivity of Legalization Effects for Males (unmarried and married) and Part-time workers

	Wage \geq \overline{Wage}	Health Insurance	Bonus
Males (Unmarried and Married)			
Panel A			
ρ	(1)	(2)	(3)
0.0	.251*** (.042) [.082]	.187*** (.055) [.029]	.175*** (.045) [.043]
0.1	.086** (.042) [.034]	.020 (.054) [.003]	.008 (.045) [.002]
0.2	-.081* (.042) [-.032]	-.148*** (.054) [-.019]	-.160*** (.045) [-.040]
0.3	-.249*** (.041) [-.097]	-.318*** (.053) [-.040]	-.329*** (.044) [-.081]
Panel B			
ρ	.152	.112	.105
Workers (Full-time and Part-time)			
Panel A			
ρ	(1)	(2)	(3)
0.0	.275*** (.076) [.106]	.032 (.102) [.003]	.149* (.084) [.028]
0.1	.103 (.075) [.039]	-.137 (.101) [-.012]	-.024 (.084) [-.004]
0.2	-.071 (.075) [-.026]	-.304*** (.100) [-.025]	-.196** (.083) [-.031]
0.3	-.246*** (.073) [-.089]	-.472*** (.098) [-.036]	-.368*** (.081) [-.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. *** represents $p < 1\%$, ** represents $p < 5\%$ and * represents $p < 10\%$.