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Evaluating the Effects of Legalization on Farmworker Wages in the Crop Sector

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By

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Labor intensive sectors such as the specialty crop sector have historically had strong reliance on foreign labor, constituting roughly one-fifth of all U.S. farms while incurring roughly two-thirds of direct-hire expenses. It is estimated that more than half unauthorized of the foreign-born labor force in the specialty crop sector are unauthorized for US employment; however, adjusting current unauthorized farm workers to a legal status may be viewed as a ‘reward’ for violating federal immigration laws. Using data from the National Agricultural Workers Survey for 1989-2014, this study utilizes a treatment effects approach (via propensity score matching and minimum-biased estimation) to evaluate the farm wage implications of legalization of foreign-born specialty crop farm workers nationally, as well as specifically in California. Positive wage effects are estimated in nationally and in California, with higher magnitude effects observed in California.

DEDICATION

I would like to dedicate this thesis to my parents and sister. Without their constant love, support, and well-timed humor, these past two years in graduate school would have been much more difficult. I would also like to dedicate this thesis to my Starkville family–Dee Dee, Erick, Drake, and Johannah. The weekly dinner nights were bright spots every week. Several more people have been instrumental in supporting me throughout my graduate career at Mississippi State and I would like to take this opportunity to thank them all.

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CHAPTER I

INTRODUCTION

Foreign born workers have consistently comprised a significant proportion of the hired labor force in the U.S. agricultural industry over the past several decades. This has been most evident in the dairy, meatpacking and specialty crop sectors (Martin, 2017; Artz, Orazem, and Otto, 2007; Krumel, 2017). The specialty crop industry – specifically the fruit, vegetable and horticultural (FVH) sector – is unique in that it is the most labor intensive relative to all other agricultural inputs. In this sector, labor availability impacts the choice of production technologies, cropping patterns, and the competitiveness of U.S. producers relative to low-cost foreign producers (Boucher and Taylor, 2007).

According to the 2012 Census of Agriculture, FVH farms constituted 21% of total U.S. farms, however, they accounted for 63% of the $19 billion of direct-hire expenses and 80% of the $5.3 billion of contract crop labor expenses (Martin, 2014). FVH farms had the highest hired labor costs in 2016, with vegetable farms, fruit and nut farms, and greenhouse/nursery farms reporting an estimated $3.2 billion, $5.5 billion, and $4.7 billion in hired labor costs, respectively (Rural Migration News, 2017). Other crops such as soybeans, cotton, and corn, have been able to successfully implement mechanization and have experienced increases in productivity and decreases in labor costs.

In comparison, FVH farms tend to use labor intensively and have had fewer options for mechanization given the nature of their products, which are usually ill suited for such advancements. Many specialty crops, specifically fresh fruits and vegetables, are sold to a consumer base that demands produce with minimal damage, bruising, and blemishes. Further, these crops tend to be highly perishable and have short windows during which produce must be harvested. Mechanization adoption in the U.S. FVH subsector has hinged upon benefit-cost decisions in which the cost and availability of farm labor is taken into account. Implementing mechanization requires large capital commitments, changes in field configurations, and potentially an overall change in the farming operation (Huffman, 2012).

Over time, these factors appear to have disincentivized innovation in suitable mechanized options and encourage reliance on farm labor, despite much of the labor force lacking US employment authorization. The risk that this poses is not lost on producers, and it has sparked much concern among producers/producer groups to date. According to Krogstad, Passel, and Cohn (2017), undocumented immigrants are overrepresented in farming, constituting 26% of all farm workers, when compared to their estimated 5% share of the civilian labor force. In terms of hired labor on crop farms however, the representation is more pronounced. According to the 2013-2014 National Agricultural Workers Survey (NAWS) findings, 73% of all hired crop farm workers were foreign born, and only 53% of these workers were authorized to work in the United States (Hernandez, Gabbard, and Caroll, 2016).

In 2014, there was an estimated 43.2 million immigrants living the U.S., a fourfold increase since 1960 (Lopez and Radford, 2017). While the U.S. immigrant population has significantly increased, the undocumented immigrant population has stabilized and become more settled (Passel and Cohn 2016), with 66% having lived in the

U.S. for at least 10 years (Krogstad, Passel, and Cohn, 2017). An estimated 70% of the roughly 1.8 million crop-farm workers employed yearly were born in Mexico and it is estimated that half of all U.S. crop farm workers lack proper U.S. work authorization (Martin, 2017). Consequently, the decrease in migratory workers from Mexico to the U.S. since the 2008-2009 recession has contributed to some uncertainty for farms which rely on such labor to remain profitable (Villarreal, 2014). With the changes in immigration trends and the current political environment, farmers are understandably concerned that immigration reform, especially regarding the increased stringency of immigration enforcement advocated for by the Trump Administration, may disrupt labor supply and curb their access to workers. Fewer workers could lead to an increase in labor costs and adversely affect U.S. producers' competitiveness in the global market for FVH products (Calvin and Martin, 2010).

Given that a substantial proportion of the workforce is foreign born and unauthorized for US employment, it is important to consider the effects that changes in immigration policy may have, particularly in light of recent policy positions that have been proposed by the Trump Administration. On immigration policy, the Trump Administration has indicated a preference for increased internal and border enforcement and a tightening of rules that could directly impact how foreign-born individuals migrate to and how they may operate within the United States. Prior to the Administration's actions however, several states had already begun to enter into 287(g) agreements with U.S. Immigration and Customs Enforcement (ICE). These agreements allow a state or local law enforcement entities to enter into a partnership with ICE, under joint memoranda of agreement arrangements in order to receive delegated authority for immigration enforcement within their jurisdictions. Currently, twenty states have ongoing agreements with ICE (US-ICE, 2018).

Past immigration policy proposals have tended to reflect increased border and interior enforcement, increased severity of employer sanctions, and paths to legalization. Legalization remains a point of contention as there remains considerable disagreement over whether this would be appropriate or whether it would 'reward' undocumented workers for violating federal immigration laws. Farm worker advocates see it as a means for workers to acquire just compensation for their labor, as it has been shown that legal farmworkers earn more than their undocumented cohorts (Taylor, 1992; Isé and Perloff, 1995; Walters, Emerson, and Iwai, 2008). While not opposed to employing a legalized workforce, farm employers have expressed concern about immigration reform that may restrict their access to foreign workers, increase wage rates and stress profits in the short run; particularly in the absence of a more efficient guest worker program. The current H-2A temporary agricultural worker/guest worker program has often been criticized for being cumbersome and bureaucratic. It has been cited as a major obstacle in the timely employment of legal foreign workers, since the complexities of the program has occasionally resulted in delayed arrivals of workers on farms. Given the perishable nature of FVH crops and the time sensitive nature of harvests, such occurrences can be costly for agricultural producers.

This study uses a treatment effects framework to evaluate the farm wage implications of legalization, and to make comparisons between the national and regional level earnings outcomes. This study is similar to previous work (Walters, Emerson, and Iwai, 2008; Kandilov and Kandilov, 2010) that have argued that farm workers self-select into legal status, but goes further to assess the outcomes on national and regional levels. Specifically, this study (a) evaluates the impact of legal status on farm worker earnings outcomes for authorized and unauthorized workers in the U.S. crop farm workforce, and (b) assesses the differences in potential earnings outcomes at the national and regional levels.

Propensity score matching and bias minimizing treatment effects estimation techniques are used to estimate three population means: (1) the average treatment effect, (2) the average treatment effect on the untreated, and (3) the average treatment effect on the treated. These methods address the underlying self-selection process, whether arising from observed or unobserved sources. This study uses data for 1989-2014 from the National Agricultural Workers Survey (NAWS), which is a rich nationally representative data set. It is an employment-based, random-sample survey of U.S. crop workers that collects demographic, employment, and health data in face-to-face interviews. It also collects data on the legal status of survey respondents.

Following this introduction, section two reviews the relevant literature. Section three describes the methods and model specification, and section four discusses the results. The study concludes with a discussion of the findings and potential future research directions.

CHAPTER II

LITERATURE REVIEW

Studies that have analyzed wage effects of legalization have determined that obtaining legal status has a positive effect on the average earnings of foreign-born farm workers. They found that unauthorized immigrants experience an estimated 5 to 15% wage penalty when compared to their legalized counterparts (Taylor 1992; Cobb-Clark, Shiells, and Lowell, 1995; Rivera-Batiz, 1999; Kossoudji and Cobb-Clark, 2002; Iwai, Emerson and Walters, 2006; Walters, Emerson and Iwai, 2008; Kandilov and Kandilov, 2010; Nisbet and Rodgers III, 2013; Borjas, 2017).

In an influential article on legal status and immigrant farmworker wages, Taylor (1992) presented theoretical framework and empirical evidence of the wage implications of legal status for immigrant farmworkers. The study used data from a 1983 survey conducted by the University of California and the California Employment Development, and the Heckman two-step procedure was used to model the selection process into legal status and job type. He found that unauthorized immigrants select into low-skill, low-paying farm jobs and therefore experience lower earnings than their authorized counterparts. Alluding to the theory of human capital developed by Chiswick (1978), Taylor argued that the lack of legal status creates a barrier to higher earnings and job mobility for unauthorized immigrant farmworkers. He found that unauthorized workers earned less than authorized workers, and concluded that that lack of legal status serves as a barrier to job mobility, and adversely affects earnings potential.

In their study, Isé and Perloff (1995) separated legal status into five different categories and used a multinomial logit to explain the probability of a certain legal status as a function of demographic characteristics. Their analysis utilized National Agricultural Workers Survey data (1989-1991) and Lee's extension of Heckman's two-step procedure to mitigate selection bias and obtain consistent wage estimates. The authors found that authorized foreign-born workers earned 15% more, on average, increase and per week than unauthorized workers.

Rivera-Batiz (1999), using data drawn from the Legalized Population Survey for the years 1989-1992, analyzed the impact of legalization programs implemented through the Immigration Reform and Control Act of 1986, on previously unauthorized workers (in all sectors), and examined the earnings of unauthorized Mexican immigration in the U.S. labor market. The analysis yielded wage differentials of 41.8% between authorized and unauthorized Mexican immigrants. The study highlighted differences in human capital and demographic characteristics between the two groups, and how those differences alone do not account for the entirety of the wage gap between unauthorized and authorized Mexican immigrants. While Rivera-Batiz suggests that the difference in legal status provides positive wage effects for those who acquired it, it is unclear how much of the wage effect can be directly attributed to changes in legal status, that is, from an unauthorized to authorized status.

Using panel data from Legalized Population Surveys (1989 and 1992 survey rounds), Kossoudji and Cobb-Clark (2002) utilized a quasi-experimental method and the non-equivalent group technique to perform their analysis. The authors' results show that immigrant workers who became legalized gained more from investing in their own human capital. They asserted that post-legalization gains are mainly a product of changes to human capital (Kossoudji and Cobb-Clark, 2002). Interestingly, the authors found that the gains were most pronounced in California. The authors theorized that the relatively liberal labor market in California may have created an environment that eased the operation of legalization programs. Furthermore, the authors found that post-legalization wages for individuals who were previously unauthorized increased by approximately 6%. In addition, the authors estimated that entry wages for unauthorized workers would have been 14% higher had the workers been legal (Kossoudji and Cobb-Clark, 2002). Also, the authors found that "the changes occurring for legalized men did result from their new legal status rather than from macroeconomic conditions associated with the particular timing of the data" (p. 623).

Kossoudji and Cobb-Clark (2002) highlighted one way to deal with a specific difficulty regarding identification of the potential wage effects of legalization—the lack of observationally similar groups of unauthorized workers and groups of authorized workers which are necessary if one wishes to determine wage differences due specifically to legalization. Other studies (Ise and Perloff, 1995; Cobb-Clark, Shiells, and Lowell, 1995; Rivera-Batiz 1999; Kossoudji and Cobb-Clark, 2002; Kaushal, 2006) have also referenced this problem in their studies, but each utilized various methods to overcome this difficulty. Cobb-Clark, Shells, and Lowell (1995) studied the effects of employer sanctions and legalization on wages by deriving an equilibrium wage from separate labor and supply equations via a standard least-squares dummy variables estimation procedure and also a two-stage least squares procedure. Rivera-Batiz (1999) calculated human capital earnings functions from data gathered from a sample of unauthorized Mexican immigrant interviews and from legal Mexican immigrants counted in the U.S. Census. Kossoudji and Cobb-Clark (2002) utilized a quasi-experimental method and the non-equivalent group technique, and Kaushal (2006) utilized difference-in-difference methodology.

Iwai, Emerson and Walters (2006) used data from the National Agricultural Workers Survey for 1989-2004 to estimate U.S. farmworker wage differentials by legal status via a Heckman-type two-stage estimation method to control for selection bias issues, with an ordered probit model in the first stage and a wage equation model in the second stage. They determined that workers that were authorized for employment in US farm work had earned higher wages on average than those who lacked authorization for US employment.

Walters, Emerson, and Iwai (2008) used a treatment effects approach that utilized parametric and nonparametric methods to evaluate the wage implications of legal status for foreign born crop workers in the United States. In this context, legal status was used as the "treatment" to assess the potential earnings outcomes for authorized versus unauthorized foreign born farmworkers. The study used National Agricultural Workers Survey data for 1989 to 2006. The study findings indicated a positive wage effect due to legal status that was consistent with findings by previous research (Taylor, 1992; Isé and Perloff, 1995; Iwai, Emerson, and Walters, 2006).

Kandilov and Kandilov (2010) evaluated the effect of legal status on wages and health insurance of foreign-born farmworkers utilizing data for 2000-2006 from the

National Agricultural Workers Survey. The authors employed propensity score matching techniques and focused on a subset of single male farmworkers that were employed fulltime in US farm work. They reported a wage gain of roughly 5% in average wages of undocumented workers due to legalization, which is consistent with previous findings in the literature.

Nisbet and Rodgers III (2013) argued that the tweaks to U.S. immigration policy, social policy, and overall migration trends since the implementation of the Immigration Reform and Control Act of 1986 (IRCA) have shaped the farm labor market, specifically the structure of wages, for both authorized and unauthorized U.S. farm workers. The authors noted that U.S. policymakers have catered to and addressed the needs of employers within the agricultural sector through policy creation and assert that "policy can influence wage differentials by shifting labor supply and demand, workers characteristics, employment practices, or a changing balance of power between workers and employers" (p. 11), further highlighting the fact that immigration policy is highly complicated and contains far-reaching implications that are difficult to fully account for.

In their analysis, using National Agricultural Workers Survey (NAWS) data spanning from 1990-2009, the authors estimated a log wage function in order to estimate the unadjusted wage gap between the authorized and unauthorized farmworkers. The authors then identified the key predictors of the wage differential between authorized and unauthorized workers, by utilizing decomposition techniques[1]. Finally, the authors decomposed the changes in the wage gap across time. They found that between 1990 and

[1] Used in Oaxaca (1973).

1994, the unadjusted wage gap declined (from 8.0 to 3.4%) due to the impact of the IRCA on relative wages. They determined that by 2004, the wage gap increased to 14.6%, and attributed this to the immigration and immigration-related policy changes (the North American Free Trade Agreement, the Illegal Immigration Reform and Immigrant Responsibility Act, and the Patriot Act). The wage gap stabilized between 2004 and 2009 (between 13.1 to 15.2%), a finding which the authors did not expect but attributed perhaps to changing migration trends (Nisbet and Rodgers III, 2013). In conclusion, the authors argued that IRCA changed the nature of the labor market in the agricultural sector— wage differences grew and returns to measured characteristics differed[2] according to legal status (Nisbet and Rodgers III, 2013).

Using data from the Community Population Survey for years 1994-2014, Borjas (2017) analyzed wage differences between the legalized immigrants, unauthorized immigrants, and natives. He determined that unauthorized workers earned lower wages owing to their undocumented status, with a wage penalty ranging from 10 to 12 %. The wage penalty in Borjas' (2017) paper is defined as "the wage gap between observationally equivalent undocumented and legal immigrants" (Borjas, 2017, p. 27). These calculations revealed an interesting trend: between 2005 and 2014, the wage penalty for unauthorized men shrank from 9.1 to 3.4% (Borjas, 2017). Borjas concluded

[2] The authors used the Juhn Murphy Pierce (1991) decomposition. Measured characteristics included: years of schooling, job tenure, English speaking ability, age, reading ability, migration status, and demographic measures such as birthplace, race, gender, marital status, and number of children. Significant characteristics included: job tenure, English speaking ability, years of schooling and age.

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that the enactment of a regularization program may have only a modest effect on wages earned by undocumented workers.

In a discussion article on agricultural labor markets and immigration, Emerson (2007) outlined how wage rates could be affected under alternative immigration policy scenarios. He argued that if it were the case that all workers were authorized for US employment via full legalization and producers had access to guest workers, but that no changes in technology occurred (so that the structure of labor demand remained unchanged), then market-determined wage rates could be expected to remain at the levels observed for the legal workers. The only difference would be the absence of a wage penalty for the formerly undocumented workers and higher direct wage costs for employers (Emerson, 2007). Alternatively, if all undocumented workers were removed with limited or no replacement guest workers and borders were closed, there could be increased agricultural wages given fixed technology and product mix, and capital immobility, at least in the short term (Emerson, 2007). Alluding to the benefits of temporary or permanent immigrant labor that could boost the U.S. economy, Emerson argued that immigrant workers and complementary factors of production, including capital, land and complementary labor, would capture the gains, while losses would be absorbed by substitute labor.

While the previous studies have been focused at the national level, this study has an additional component that looks at the potential effects at the regional level, and it assesses these over a longer time frame. In terms of the overall methodological approach and assessing the treatment effects, it is similar to work by Walters, Emerson and Iwai (2008) and Kandilov and Kandilov (2010). However, Kandilov and Kandilov (2010) restricted their analysis to a six year sample of unmarried male farmworkers, and only used propensity score matching to analyze the wage effects of legalization, whereas this study does not. Finally, while this study uses propensity score matching (PSM), it also uses a minimum-biased estimation (MBE) method proposed by Millimet and Tchernis (2013) to check robustness of the PSM. Whereas PSM is appropriate for addressing selection bias from observed factors, the MBE is appropriate where selection bias arises from factors that are unobservable to researchers. In the context of this research, education and experience are examples of characteristics (factors) that would be observed, whereas an immigrant worker's motivation and reaction to news on immigration policies or laws are factors that are likely to be unobserved by researchers. The MBE method was not used in either of the previous studies, and to the best of my knowledge, it has not been applied in the context of farm labor and immigration policy research.

CHAPTER III

THEORETICAL FRAMEWORK

Mincer Earnings Functions

Earnings functions as developed by Mincer (1958 and 1974) provide a standard framework for characterizing individuals' earning profiles as a function of years of education (human capital stock) and experience (post-schooling investments). In the context of this study, the use of human capital earnings functions is attractive because it allows for the inclusion of other variables that could possibly affect individuals' earnings (Chiswick, 2003). The Mincer earnings functions assumes that human capital stocks are homogenous, that individuals would choose to maximize their present value earnings, and that individuals have the same rates of return on their investments in human capital. Log earnings is specified as:

$$\ln y = f(S,\ \mathrm{Exp},\ \mathrm{Exp}^2 \qquad (1)$$

where *y, S,* and *EXP* represent earnings, years of schooling (education) and post-schooling (on-the-job training) investments, respectively. In equation 1, observed earnings ($\ln y$) is expressed as a concave function dependent upon the individual's labor market experience and schooling. It is assumed that on-the-job training (post-schooling investment) declines over an individual's lifetime and schooling lasts *S* years (Polachek, 2007).

There are a few important empirical implications advanced by Mincer earnings function. The first being that individuals' earnings levels are related to human capital investments, implying that the higher the human capital investment, the higher the earnings. The second being that individuals' earnings functions exhibit concavity, meaning that earnings rise at a decreasing rate throughout an individual's life. This concavity was first identified by Mincer (1974), and this finding holds when adjustments for selectivity bias have been made (Hartog, Pfann, and Ridder, 1989; Kiker and Mendes de Oliveira, 1992; Baldwin, Zeager, and Flacco, 1994; Gibson and Fatai, 2006).

Thomas Lemieux (2006) and Das and Polachek (2017) discussed the practicality and popularity of the log earnings function in research focusing on various issues. By adding categorical dummy variables to equation 1, the basic function can be modified to produce estimates of differences in earnings across different subgroupings within a population (Das and Polachek, 2017). Of particular relevance regarding migration are papers produced by Chiswick (1978) and Borjas (1982, 1985, 1993). Other studies utilizing Mincer earnings functions to analyze differences in wages stemming from gender (Suter and Miller, 1973), college major (Webber, 2014), beauty (Hamermesh and Biddle, 1994; Scholz and Sicinski, 2015), bilingualism (Saiz and Zoido, 2005), and veteran status (Gabriel, 2016), and other issues have been performed, further highlighting the value and the adaptability of the Mincer earnings functions and the underlying theory.

Treatment Effects Approach

The treatment effects framework analyzes the outcomes of treatments, or interventions, on a designated treatment group against an un-treated group. It is important to note that "the term outcome refers to changes in economic status or environment on the economic outcomes of individuals" (Cameron and Trivedi, 2005, p. 860). In the realm of economics, this framework is particularly useful for analyzing policy interventions because treatments that lead to successful outcomes can relate to social program implementation or improvements to meet certain social policy objectives.

Inferring a causal connection between the treatment and the outcome is the standard problem of the treatment effects framework. The treatment in this study is defined as the legalization of unauthorized workers in the specialty crop sector. Because the treatment variable (legal status) is binary, making it impossible to observe the same individual in both states at the same time; causal connections must be inferred using counterfactuals. In the context of this study, the wage effect (outcome) of a change in legal status (treatment) is of primary interest. Since we are interested mainly in the wage effect caused by the 'switching' of the binary treatment variable, causation is identified *ceteris paribus,* signifying that all other independent variables are held constant. There is a participant group (D*=1) (treated) (authorized) and a non-participant group (D*=0) (untreated) (unauthorized), with Y_1 denoting the outcome for the treated group and Y_0 denoting the outcome for the untreated group. The treatment effect being is denoted as the causal effect of the treatment on the outcome (Long, 1997). The value in this framework is found in its evaluation of the change in a participating individual's average economic outcome against those individuals who choose not to participate (Cameron and Trivedi, 2005). Individuals included in the non-participant group are used to form a benchmark to estimate the changes brought about by the treatment.

The first step of the analysis utilizes a binary choice model to estimate the likelihood that the worker is either authorized or unauthorized. The wage effect of legal status is captured via the use of propensity score matching. Heckman, Tobias and Vytlacil (2001) and Blundell and Costa Dias (2002) use a latent variable framework and define the potential outcomes based on observable characteristics (*x)*, and the participation decision for a program as:

$$Y_1 = g_1(x) + u_1 = \beta'_1 X_i + u_1 \qquad \text{(treated)} \qquad (2)$$

$$Y_0 = g_0(x) + u_1 = \beta'_0 X_i + u_0 \qquad \text{(untreated)} \qquad (3)$$

$$D^* = \alpha' Z_i \qquad \text{(where: D=1 if D*≥0; D=0 otherwise)}$$

D=1 when treated
D=0 when untreated (4)

In equations (2) to (3) above, $g_0(x)$ and $g_1(x)$ denote the relationship between the observable characteristics and the potential outcomes (Y_1, Y_0). The decision to participate in treatment is denoted by the binary treatment variable D, which is equal to 1 if the individual chooses to select into legalization, whereas it is equal to zero if the individual chooses to refrain from legalization. Unobserved variables are denoted by $u_1,\ u_0$, and Z, while observed random variables are denoted by X. Errors are assumed to be independent of X and Z. The treatment effect is expressed by equation (5), and is equal to the difference between the potential outcomes, all else remaining the same:

$$\Delta_i = Y_{1i} - Y_{0i} \qquad \textit{(for i=1,..,N)} \qquad (5)$$

However, we are unable to observe that change because we would need data that captures the wages of a legalized foreign-born crop worker as well as the wages that the same foreign-born worker earned while working in an unauthorized state, highlighting the necessity of some type of matching methods.

We can rewrite (2) and (3) as follows:

$$Y_1 = E(Y_1|X_i) + u_1 = \mu_1(X_i) + u_1 \textit{(outcome for the treated)} \tag{6}$$

$$\mathrm{Y}_0 = \mathrm{E}(\mathrm{Y}_0|\mathrm{X}_\mathrm{i}) + \mathrm{u}_0 = \mu_0(\mathrm{X}_\mathrm{i}) + \mathrm{u}_0 \textit{(outcome for the untreated)} \tag{7}$$

This specification is akin to the switching regression type in that the treated and non-treated groups have different conditional mean functions, $\mu_0(X_i)$ and $\mu_1(X_i)$. It is assumed that $E(\mu_1|X_i) = E(\mu_0|X_i) = 0$, however the same equality is not assumed for $E(\mu_1|X_i, D)$ and $E(\mu_0|X_i, D)$. Thus, the observed outcome is expressed as:

(8) $\quad Y_i = D_i Y_{1i} + (1 - D_i)Y_{0i}$

in which the portion of the treatment effect which is unobservable is known as the counterfactual outcome. Thus, for individuals that are authorized, Y_0 would be the counterfactual outcome, whereas for individuals that are unauthorized, Y_1 would be the counterfactual outcome. Combining equations (6), (7), and (8), we get:

$$\begin{aligned} Y_i &= D_i(\mu_1(X_i)+u_1) + (1 - D_i)(\mu_0(X_i) + u_0) \\ &= \mu_0(X_i) + D_i(\mu_1(X_i) - \mu_0(X_i)+u_1 - u_0) + u_0 \end{aligned} \tag{9}$$

In equation (9), the second term in the equation 'switches' on and off because D_i is the binary treatment variable denoting legal status. The entire second term in (9) show the benefit of receiving the treatment, with $(\mu_1(X_i) - \mu_0(X_i))$ capturing the average gain for a participant and $(u_1 - u_0)$ demonstrating the specific benefit available only to the participant (Cameron and Trivedi, 2005). It is important to note that the treatment effect of each individual is independent of the treatment effect of others, meaning that an individual’s potential outcomes are a function of his participation decision only (Woolridge, 2002; Caliendo, 2006).

CHAPTER IV

METHODS

Treatment Effects via Propensity Score Matching

In non-experimental economic studies, when treatment participation is not random, and instead is reliant upon observed variables, propensity scores may apply (Cameron and Trivedi, 2005). Individuals in the authorized group are directly compared to matching individuals in an unauthorized group, which allows for the estimation of treatment effects as if a controlled experiment had been conducted. Unfortunately, exact matches do not exist for all authorized individuals.

To correct for these problems, Rosenbaum and Rubin (1983), proposed a propensity score that increases the balance (the similarity between the distribution of ***X*** in the control and treatment groups) by estimating a propensity score for each observation and then matching on that propensity score. The propensity score measures the likelihood that an individual selects into the treatment group. Most propensity scores are found using a logistic regression as expressed in equation (10):

$$\mathrm{P(D_i = 1|X_i)} = \frac{\mathrm{e}^{\mathrm{F(X_i)}}}{1-\mathrm{e}^{\mathrm{F(X_i)}}} \tag{10}$$

where the propensity score $P(D_i = 1|X_i)$ is the probability that D_i equals 1 given X_i and $F(X_i)$ is a function of the explanatory variables. The propensity score is used to create matches between authorized and unauthorized individuals to estimate the treatment effects. This study utilizes a logit regression to generate these propensity scores.

In treatment effects estimation, the Conditional Independence Assumption (CIA) is an important assumption. It states that conditional on observed variables X_i the potential outcomes are independent of treatment (Cameron and Trivedi, 2005):

$$(\mathrm{Y}_0, \mathrm{Y}_1 \perp \mathrm{D}_\mathrm{i})|\mathrm{X}_\mathrm{i} \tag{11}$$

If it holds, then the matching estimator should perform as expected (Rosenbaum and Rubin, 1983; Heckman, Ichimura, and Todd, 1997; Dehejia and Wahba, 2002; Smith and Todd, 2005). Presuming this assumption holds, it is understood that treatment status is random conditional upon the vector of observable variables, X_i. Thus, matching is a quasi-experimental technique used to replicate actual experimental conditions. It is satisfied as long as X_i includes all of the variables which affect the selection into treatment and the outcome (Kandilov and Kandilov, 2010).

Using data from the National Agricultural Workers Survey (NAWS), Kandilov and Kandilov (2010) employed propensity score matching techniques[3] to compare single male legal permanent residents employed in crop farm work to an appropriate control group of unauthorized workers, in order to analyze the effects of legalization on the wages and benefits. Other studies that have employed the propensity score matching techniques include Dehejia and Wahba (2002) to identify the wage effects of job training programs, and Yasar and Morrison Paul (2008) to analyze the impact of foreign technology transfer on plant productivity. Additionally, Liu and Lynch (2011) use

[3] As developed by Rosenbaum and Rubin (1983)

propensity score matching methods to examine the effects of agricultural preservation programs and changes in farmland loss.

Using PSM, we are able to estimate three population means:

1. **Average Treatment Effect (ATE):** The expected gain that a randomly selected individual accrues from participating in a program:

$$\alpha_{ATE} = E(\Delta) = E(Y_1) - E(Y_0) = E(Y_1|X_i, D = 1) - E(Y_0|X_i, D = 0) \qquad (12)$$

   In (12), $E(\Delta)$ denotes the expected difference between the two different outcomes (Y_1 and Y_0); $E(Y_1|X_i, D = 1)$ denotes the outcome (wage rate) for an individual conditional upon a vector of observed variables, X_i , and *D=1* (denoting that the observation has been treated (authorized)), and $E(Y_0|X_i, D = 0$ denotes the outcome for the individual conditional upon the same vector of observed variables, X_i, and *D=0* (denoting that the observations is untreated (unauthorized)).

2. **Average Treatment Effect on the Untreated (ATEU):** The effect for non-participants if they would have participated. The ATEU may be useful in future policy decisions regarding the extension of the treatment to excluded groups (Caliendo, 2006). In the context of this study, the ATEU would yield information related to the wage effect that an unauthorized worker could experience if the worker gained legal status. This would also yield information to the grower related to costs of labor if a large-scale legalization program was targeted at the agricultural sector. The ATEU is mathematically expressed as:

$$\alpha_{\text{ATEU}} = \text{E}(\Delta|\text{D} = 0) = \text{E}(\text{Y}_1|\text{D} = 0) - \text{E}(\text{Y}_0|\text{D} = 0) \tag{13}$$

Where $E(\Delta|D = 0)$ denotes the expected difference in the outcome (wage rate) given the individual is untreated (unauthorized); $E(Y_1|D = 0)$ denotes the expected outcome for the untreated individual if they had decided to participate in the treatment (authorization), and $E(Y_0|D = 0)$ denotes the outcome for the untreated individual that remained in an unauthorized status.

3. **Average Treatment Effect on the Treated (ATET):** The average gain from the treatment on those who opt into the treatment (Heckman, Tobias, and Vytlacil, 2001). That is, the gain in wage for those who decide to obtain legal status. The ATET is mathematically expressed as:

$$\alpha_{\text{ATET}} = \text{E}(\Delta|\text{D} = 1) = \text{E}(\text{Y}_1|\text{D} = 1) - \text{E}(\text{Y}_0|\text{D} = 1) \tag{14}$$

where $E(\Delta|D = 1)$ denotes the expected change in the outcome (wage rate) given that the individual is treated (authorized); $E(Y_1|D = 1)$ denotes the expected outcome for treated individual; and $E(Y_0|D = 1)$ denotes the expected outcome for the treated individual had they selected to refrain from gaining legal status.

Selection Bias

An issue that arises in the calculation for the ATET is that of selection bias. Selection bias poses a challenge and appears in the ATET, in that the ATET includes a hypothetical outcome of lack of treatment for the individuals who actually received the treatment (Caliendo, 2006). If using non-experimental data, as is the case with this study, this outcome will not be the same as the outcome for those individuals who did not receive treatment (i.e., gain legal status). Selection bias occurs when individuals elect whether to partake in the treatment (gain legal status), or to refrain from participating in the treatment. The decision to pursue legal status arguably involves weighing the costs and perceived benefits of obtaining legal status, and factors that may not be easily observed or measured by a researcher. Intuitively, individuals who would benefit most from gaining legal status, take deliberate actions to gain legal status; this therefore introduces positive selection-bias into the ATET. Proper randomization of the treatment (authorization) would circumvent self-selection issues. However, the deliberate selection into treatment makes proper randomization difficult to achieve, highlighting the need for estimation methods that take selection bias into account.

There are two general sources of selection issues – (a) selection on observables and (b) selection on unobservables. Selection on observables implies that we, as researchers, can observe all of the variables that impact an individual's decision to participate in treatment and therefore affect the outcome. Selection on unobservables implies that we cannot observe some of the variables that impact the individual's decision to participate in the treatment and the subsequent outcome. If an individual is selecting into treatment based on observables, it is assumed that treatment assignment is random conditional upon a specified vector of observable covariates, therefore providing a randomization of treatment and circumventing the selection issue. Selection on observables is known as the Conditional Independence Assumption (CIA) or *unconfoundedness*. The CIA is satisfied if the vector of observed variables includes *all* the variables which affect the participation and outcome. If selection bias occurs as a result of observed factors, linear regression techniques and propensity score matching are appropriate estimation methods.

When an individual selects on unobservables, randomization of treatment is difficult to achieve. Selecting on unobservables suggest that unobservable variables drive the individual's decision to participate in the treatment, and thus nonrandom assignment into treatment occurs and becomes a concern. Instrumental variables are one remedy when faced with issues stemming from selection on unobservables; however, in the context of this study, a valid instrument that is correlated with the decision and uncorrelated with the outcome is unavailable. For this reason, we employ a minimum bias estimation method in this study.

Bias-Minimizing Treatment Effects

To address bias that may occur due to unobserved variables, this study utilizes the minimum-biased estimator (MBE) proposed by Millimet and Tchernis (2013). This estimator allows for the bias to be minimized when the CIA fails – that is, when bias stemming from unobserved factors play a role. Thus, the motivation behind using the minimum-biased treatment effects estimation; it addresses issues regarding the efficacy of the CIA and subsequently the potential bias of the propensity score estimates, and deals directly with selection bias arising from unobserved variables.

Assuming that observations select on unobservables, the MBE trims the estimation sample to include only observations with propensity scores that lie within a certain interval. If the CIA holds, the MBE yields unbiased estimations, and works to minimize the bias when the CIA fails (Millimet and Tchernis, 2013). The MBE estimator draws heavily from Heckman's bivariate normal selection model to provide an estimate of the bias of estimators when the CIA fails.

Millimet and Tchernis (2013) propose that through the use of observations with propensity scores around the Bias-Minimizing Propensity Score (BMPS), $P*$, the bias from the failure of the CIA can be adequately minimized. The ATET, which is the estimator that is subject to selection bias, when estimated using the MBE is expressed as[4]:

$$\alpha_{ATE,MBE} = \left[\sum_{i\in\Omega} \frac{Y_i T_i}{\hat{P}(X_i)} \Big/ \sum_{i\in\Omega} \frac{T_i}{\hat{P}(X_i)} \right] - \left[\sum_{i\in\Omega} \frac{Y_i(1-T_i)}{1-\hat{P}(X_i)} \Big/ \sum_{i\in\Omega} \frac{1-T_i}{1-\hat{P}(X_i)} \right] \tag{15}$$

Where $\Omega = i|\hat{P}(X_i) \in C(P^*)$ and $C(P^*)$ symbolizes the neighborhood of propensity scores around $P*$ (Millimet and Tchernis, 2013). More specifically, $C(P^*)$is mathematically expressed as $C(P^*) = \left[\hat{P}(X_i)\middle|\hat{P}(X_i) \in (\overline{P}, \underline{P})\right]$, where $(\overline{P}, \underline{P})$ is the interval in which a certain percentage of the control and treatment groups' propensity scores must fall into P^* (Millimet and Tchernis, 2013). To estimate $P*$, Ω must be derived. Millimet and Tchernis propose using Heckman's bivariate normal selection model to estimate Ω.

The ATET using the MBE is expressed as:

$$\alpha_{ATET,MBE}(P *) = \sum_{i\in\Omega} Y_i T_i - \left[\sum_{i\in\Omega} \frac{Y_i(1-T_i)\hat{P}(X_i)}{1-\hat{P}(X_i)} \Big/ \sum_{i\in\Omega} \frac{(1-T_i)\hat{P}(X_i)}{1-\hat{P}(X_i)} \right] \tag{16}$$

The ATET can be calculated without estimating P^* (Millimet and Tchernis, 2013).

[4] See Millimet and Tchernis (2013) for detailed mathematical derivations.

Following Peel (2018), the MBE is used to check the robustness of the traditional propensity-score matching treatment effects estimates, thereby assessing the threat of unobserved selection bias. The CIA is strengthened if the MBE treatment effect estimate is similar and significant, while the presence of an unobserved correlated variable is detected if the MBE treatment effect estimate is still significant, but not similar to PSM estimates (Millimet and Tchernis, 2013; Peel, 2018).

Limitations and Other Considerations

A limitation posed PSM is that it typically must utilize a smaller sample size, which therefore reduces the power of the statistical tests conducted. However, unless the PSM technique is employed with an already small sample size, this should not cause any issues in terms of the validity of the PSM treatment effects estimates. In this analysis, authorized (treated) individuals are matched with unauthorized (untreated) individuals using the nearest-neighbor matching technique with replacement. The MBE further trims the sample to include only observations with a propensity score within a certain interval around the BMPS (Millimet and Tchernis, 2013; McCarthy, Millimet, and Tchernis, 2014; Peel, 2018).

Another limitation of PSM, and subsequently the MBE, occurs due to the fact that observations that are not on the common support are discarded. The observed difference in MBE and PSM treatment effects estimates may in part be due to heterogeneity that occurs from individuals' different responses to selection into treatment (Peel, 2018). However, according to Millimet and Tchernis (2013 p. 988), unobserved bias is still minimized using the MBE because it identifies the parameter that can be estimated with least amount of bias.

CHAPTER V

DATA

The data utilized in this study comes from the National Agricultural Workers Survey (NAWS) and spans from 1989 to 2014. The NAWS is an employment-based, random survey of U.S. crop workers. Demographic, employment, and health data are collected via face-to-face interviews conducted directly with hired crop farm workers. A multistage sampling process is used, and the survey is conducted in three different cycles per year to reflect the seasonal nature of agricultural employment and production (U.S.-DOL, 2018).

The NAWS is one of few nationally representative datasets where respondents are asked to self-report on legal status, focusing specifically on the agricultural sector. Interviewed workers include only those who are currently employed by an eligible establishment in crop and crop-related work. According to the NAWS, an eligible establishment is one that is classified within the North American Industrial Classification System as Crop Production (NAICS code 111) or Support Activities for Crop Production (NAICS code 11151) (Hernandez, Gabbard, and Carroll, 2016). Workers involved in pre-harvest, post-harvest, and harvesting tasks are eligible to be interviewed, as well as workers in supervisory positions and those who work as machinery operators. The NAWS dataset excludes H-2A workers as well as workers who have been out of agricultural work for over one year (Hernandez, Gabbard, and Carroll, 2016). Once respondents indicate legal status, or lack thereof, the respondents are then asked questions in order to cross-check the answer to help minimize potential reporting bias. Answers must be consistent and must conform with visa regulations (Hernandez, Gabbard, and Carroll, 2016).

Figure 1 depicts the NAWS Cropping Regions, which separates the country into six distinct crop regions (East, Southeast, Midwest, Southwest, Northwest, and California) for survey interviews. Since the first round of surveys began in 1989, over 61,000 crop workers have been interviewed in 612 counties and 46 states (Hernandez, Gabbard, and Carroll, 2016).

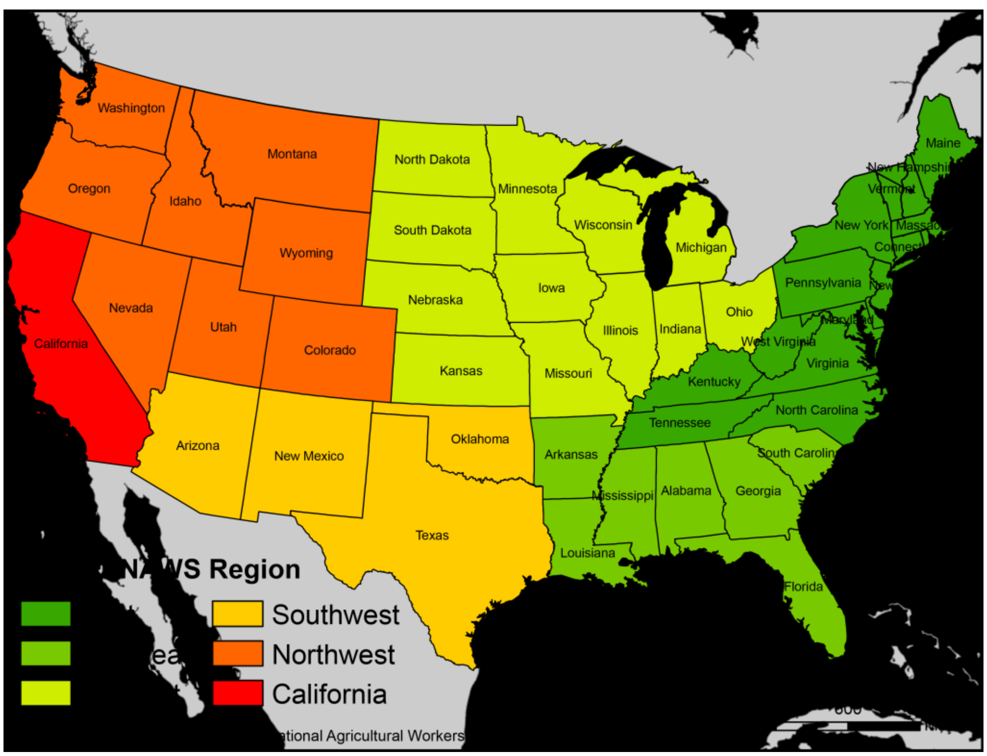

Figure 1 NAWS Crop Regions

Characterization of the Estimation Sample

The total estimation sample (N=11,958) was drawn from the National Agricultural Workers Survey included on foreign-born, FVH farmworkers from 1989-2014. Of the total sample, 7,851 observations lacked proper U.S. work authorized, while 4,107 observations had some type of legal status. Restricting the sample to only include California farmworkers (N=5,927), 2,207 observations had some type of legal status, while 3,720 observations did not. Grouping the other NAWS crop regions together, excluding California, yielded a total sample of 6,031 observations, of which, 1,900 observations had legal status and 4,131 observations did not.

Demographics

Tables 1 through 3 provide demographical summary statistics for the estimation sample at the national level (Table 1), for California (Table 2), and for all other regions grouped together (Table 3) for the years 1989-2014. Nationally, authorized foreign-born crop workers on average were older (45.5 years) than unauthorized workers (32.8 years). A higher percentage of authorized workers were female (23%) and a higher percentage of authorized workers were married (78%). In terms of educational attainment, 31% of authorized workers had attained some level of elementary education, 35% had attained at least some level of middle school education, 25% had attained at least some level of high school education, and 3% had received college-level schooling. For the unauthorized cohort, 23% had attained some level of elementary education, 38 % had attained some level of middle school education, 31% had obtained some level of high school education, and 2% had attended college. A higher percentage (40%) of authorized workers had attended adult-education classes; 22% of unauthorized workers had attended adult-education classes. A higher percentage of unauthorized workers were of Hispanic ethnicity (98%), with the vast majority of unauthorized workers having been born in Mexico (97%).

Regarding California's foreign-born crop farm labor force (Table 2), unauthorized workers in California were slightly younger (32.6 years) than the national average, and authorized workers were slightly older (46 years) than the national average. A lower percentage of both legal statuses were female, whereas a higher percentage of both legal statuses were married. Comparing educational attainment averages, the California averages are similar to the national averages in Table 1. Roughly the same percentage of authorized and unauthorized workers in California were of Hispanic ethnicity and were Mexican-born when compared to national averages.

The regions included in the grouping of the regions include the East, Southeast, Southwest, Midwest, and Northwest. Summary statistics characterizing the demographics of foreign-born farmworkers in the regions listed above are presented in Table 3. Authorized farmworkers were younger (44.8 years) whereas unauthorized farmworkers were older (32.9 years) than the national and California averages for the two groups. A higher percentage of both legal statuses were female and were married. Educational attainment for both groups were similar to California and national averages, as were the amount of workers in both statuses that were of Hispanic ethnicity and Mexican-born.

Employment Characteristics

Tables 4, 5, and 6 report summary statistics relating to various employment variables for authorized and unauthorized foreign-born farmworkers at the national level (Table 4), for California (Table 5), and for all other regions (Table 6) from 1989-2014. Nationally, foreign-born authorized farmworkers' inflation adjusted earnings are roughly $1.10 more, on average, than unauthorized farmworkers. Authorized workers reported being in the U.S. for roughly 25.18 years, whereas unauthorized workers reported being in the U.S. for roughly 10.62 years on average. Authorized workers have a greater amount of U.S. farmwork experience (22.11 years) when compared to their unauthorized counterparts (9.38 years). Unauthorized workers spent a greater number of weeks abroad, and a greater percentage of unauthorized workers were hired by a farm labor contractor as opposed to being hired directly by the grower. A higher percentage of authorized workers were able to speak English. In terms of type of work task (pre-harvest, harvest, post-harvest, semi-skilled, or supervisor), a larger percentage of unauthorized workers were employed to perform pre-harvest (66%), harvest (75%), post-harvest (64%), or semi-skilled (59%) tasks when compared to authorized workers, who were largely hired to perform supervisory tasks (86%).

In California (Table 5), wages for both legal status categories were higher than the national averages, as was the number of years since immigration. Authorized and unauthorized workers had more U.S. farmwork experience, and a higher percentage of both categories were employed by farm labor contractors. In terms of types of tasks, California farmworkers follow the same trend as shown in Table 4, with a higher percentage of unauthorized workers being hired to perform pre-harvest (64%), harvest

(75%), post-harvest (54%), or semi-skilled (57%) tasks, whereas a higher percentage of authorized workers had been employed to perform supervisory tasks (89%).

Table 1 Demographical Summary Statistics – National Level (1989-2014)

	Unauthorized		Authorized		All	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Age	32.79	10.11	45.50	11.74	37.15	12.28
Female	0.21	0.41	0.23	0.42	0.22	0.41
Married	0.59	0.49	0.78	0.41	0.66	0.47
No Educational Attainment	0.06	0.23	0.06	0.24	0.06	0.23
Elementary Education	0.23	0.42	0.31	0.46	0.26	0.44
Middle School Education	0.38	0.49	0.35	0.48	0.37	0.48
High School Education	0.31	0.46	0.25	0.43	0.29	0.45
College Education	0.02	0.14	0.03	0.17	0.02	0.15
Attended Adult-Ed Classes	0.22	0.41	0.40	0.49	0.28	0.45
Hispanic Ethnicity	0.98	0.15	0.90	0.30	0.95	0.22
Mexican-Born	0.97	0.16	0.89	0.31	0.95	0.23
N	7,851		4,107		11,958	

Table 2 Demographical Summary Statistics – California (1989-2014)

	Unauthorized		Authorized	
	Mean	Std. Dev.	Mean	Std. Dev.
Age	32.68	10.18	46.07	11.36
Female	0.19	0.40	0.22	0.41
Married	0.61	0.49	0.81	0.39
No Educational Attainment	0.05	0.23	0.06	0.23
Elementary Education	0.22	0.41	0.33	0.47
Middle School Education	0.39	0.49	0.36	0.48
High School Education	0.31	0.46	0.22	0.42
College Education	0.02	0.12	0.03	0.16
Attended Adult-Ed Classes	0.20	0.40	0.39	0.49
Hispanic Ethnicity	0.98	0.14	0.90	0.30
Mexican-Born	0.98	0.14	0.90	0.30
N	3,270		2,207	

Table 3 Demographical Summary Statistics – All Other Regions (1989-2014)

	Unauthorized		Authorized	
	Mean	Std. Dev.	Mean	Std. Dev.
Age	32.89	10.03	44.83	12.14
Female	0.23	0.42	0.25	0.43
Married	0.58	0.49	0.75	0.43
No Educational Attainment	0.06	0.23	0.07	0.25
Elementary Education	0.24	0.43	0.30	0.46
Middle School Education	0.37	0.48	0.33	0.47
High School Education	0.31	0.46	0.27	0.44
College Education	0.02	0.15	0.03	0.18
Hispanic Ethnicity	0.97	0.17	0.90	0.31
Attended Adult-Ed Classes	0.23	0.42	0.40	0.49
Mexican-Born	0.97	0.18	0.89	0.32
N	4,131		1,900	

Table 4 Employment Summary Statistics – National Level (1989-2014)

	Unauthorized		Authorized	
	Mean	Std. Dev.	Mean	Std. Dev.
Hourly Wage (2014 $)	9.51	2.51	10.66	3.36
Years Since Immigration	10.62	7.66	25.18	10.32
Seasonal Labor	0.42	0.49	0.44	0.50
Skilled Labor	0.31	0.46	0.40	0.49
U.S. Farmwork Experience	9.38	6.79	22.11	10.90
U.S. Farmwork Experience, sq.	134.11	188.86	607.70	503.35
Annual Farmwork Weeks	40.98	13.55	40.99	12.24
Annual Weeks Abroad	3.40	9.86	2.22	6.32
Annual Non-Farmwork Weeks	2.13	7.85	1.94	7.61
Employed by Farm Labor Contractor	0.17	0.38	0.12	0.33
English Speaking Ability	0.44	0.50	0.65	0.48
Pre-Harvest	0.66	0.47	0.34	0.47
Harvest	0.75	0.44	0.25	0.44
Post-Harvest	0.64	0.48	0.36	0.48
Semi-Skilled	0.59	0.49	0.41	0.49
Supervisor	0.14	0.36	0.86	0.36
N	7,851		4,107	

Table 5 Employment Summary Statistics – California (1989-2014)

	Unauthorized		Authorized	
	Mean	Std. Dev.	Mean	Std. Dev.
Hourly Wage (2014 $)	9.54	2.23	10.76	3.43
Years Since Immigration	10.63	7.91	26.11	9.99
Seasonal Labor	0.47	0.50	0.49	0.50
Skilled Labor	0.36	0.48	0.46	0.50
U.S. Farmwork Experience	9.59	6.99	23.52	10.68
U.S. Farmwork Experience, sq.	140.77	197.72	667.45	514.91
Annual Farmwork Weeks	40.13	13.44	41.06	11.75
Annual Weeks Abroad	3.99	10.80	1.60	5.13
Annual Non-Farmwork Weeks	1.62	6.89	1.53	6.84
Employed by Farm Labor Contractor	0.31	0.46	0.21	0.40
English Speaking Ability	0.36	0.48	0.61	0.49
Pre-Harvest	0.64	0.48	0.36	0.48
Harvest	0.75	0.43	0.25	0.43
Post-Harvest	0.54	0.50	0.46	0.50
Semi-Skilled	0.57	0.50	0.43	0.50
Supervisor	0.11	0.33	0.89	0.33
N	3,270		2,207	

Table 6 Employment Summary Statistics – All Other Regions (1989-2014)

	Unauthorized		Authorized	
	Mean	Std. Dev.	Mean	Std. Dev.
Hourly Wage (2014 $)	9.49	2.73	10.53	3.27
Years Since Immigration	10.62	7.43	24.10	10.60
Seasonal Labor	0.36	0.48	0.38	0.49
Skilled Labor	0.27	0.44	0.33	0.47
U.S. Farmwork Experience	9.19	6.61	20.47	10.92
U.S. Farmwork Experience, sq.	128.10	180.33	538.29	480.47
Annual Farmwork Weeks	41.75	13.60	40.92	12.79
Annual Weeks Abroad	2.87	8.90	2.94	7.40
Annual Non-Farmwork Weeks	2.58	8.59	2.41	8.39
Employed by Farm Labor Contractor	0.05	0.21	0.02	0.15
English Speaking Ability	0.51	0.50	0.70	0.46
Pre-Harvest	0.68	0.47	0.32	0.47
Harvest	0.74	0.44	0.26	0.44
Post-Harvest	0.70	0.46	0.30	0.46
Semi-Skilled	0.62	0.49	0.38	0.49
Supervisor	0.20	0.45	0.80	0.45
N	4,131		1,900	

CHAPTER VI

ESTIMATION RESULTS

Table 7 includes descriptions of the variables used in the logistic regression to determine the propensity scores. Variables included in the logit model to predict legal status include: age, experience, experience squared, English-speaking ability, whether the farmworker was hired by a farm labor contractor (FLC), whether the farmworker was employed seasonally, the annual number of farmwork weeks, the annual number of weeks spent abroad, the number of years since the farmworker immigrated to the U.S. for the first time, educational attainment, if the farmworker entered the U.S. after 1986, and if the farmworker entered the U.S. after 2001.

The selected variables have been used previously in the literature to determine legal status, and there are notable differences between the authorized and unauthorized in terms of means. The last two variables, after 1986 and after 2001, distinguish between two time periods. Following Walters, Emerson and Iwai (2008) the first indicates whether the farm worker first entered the United States to live or work, and is included to reflect legalization through the Special Agricultural Worker (SAW) program that those farmworkers who were already living in the United States and working prior to the 1986 Immigration Reform and Control Act (IRCA) being passed, and the relative difficulty of acquiring legal status since that time. The after 2001 variable reflects farmworker entry to the United States pre and post 2001; in the post-9/11 period following the terror attacks, immigration enforcement (border and interior) was significantly increased.

The results of the logit specification for the U.S., California, and all other regions are reported in Table 8. Most of the signs on the coefficients are as expected. An immigrant worker is more likely to be authorized to work in the U.S. if the worker is older in age, is proficient with the English language, and is employed seasonally. Additionally, the worker is more likely to be authorized if the worker has resided in the U.S. for a longer period (as captured by the 'years since immigration' variable), has more overall U.S. farmwork experience, and has more formal education. On the other hand, a worker is more likely to be unauthorized if the worker exhibits a higher number of annual farmwork weeks and is hired by a FLC (as opposed to being employed directly by the grower). This finding on annual farmwork weeks may reflect that many unauthorized immigrants work a greater number of hours in farm work weekly than authorized immigrant workers, and perhaps because they may not have the same degree of job mobility (in and out of farm work) as their authorized cohorts likely would. Additionally, the logit results indicate if the worker entered the U.S. for the first time to live or work after 1986 or 2001, they were more likely to be unauthorized. The results are broadly consistent with Walters, Emerson and Iwai (2008). On this particular finding, they concluded this reflected that indicated that immigrant farmworkers following these periods were more likely to be undocumented than otherwise, given the restrictions that were put in place after both of these periods, and heightened enforcement after 2001.

The logit model was used to predict the legal status of the worker based on calculated propensity scores to test its accuracy. The model correctly classified 83.93% of observations nationally, 82.83% of observations in California, and 84.84% of observations in all other regions. Figures 2-4 graphically display the distribution of propensity scores pre-matching and post-matching. Evidenced in figure 4, the distributions of propensity score for the matched sample of unauthorized workers is very similar to the distribution of propensity scores for authorized workers (see figure 3), implying that the two groups have similar characteristics.

Table 9 summarizes the bias present in mean differences before and after matching. There were statistically significant differences in the means of all the variables in the unmatched sample. After matching based on propensity scores generated by the logit specification, there were no significant differences in the means of all of the variables, and the average total bias decreased from 72.9% to 6.3%.

Table 10 presents the estimated propensity-score matching treatment effects parameters at the national level, for California only, and all other regions excluding California. The wage differences and percentage differences in each region (i.e. national, California, or all other regions) are differences relative to the average wage in each respective area. At the national level, the estimated average treatment on the treated, or the estimated benefit that workers who selected into treatment (gained authorization) would experience, on average, is $0.89. This means that the average benefit from selecting into treatment is associated with an $0.89 increase in the average wage rate. Translating the national wage difference into percentage terms, the average treatment on the treated yielded a 7% increase in the average national wage. The national ATET is broadly comparable to findings from Walters, Emerson, and Iwai (2008) and Kandilov and Kandilov (2010).

Additionally, overall, our findings are consistent with those of previous work that have assessed the wage implications of legal status for foreign-born farmwokers (Taylor, 1992; Isé and Perloff, 1995; Kossoudji and Cobb-Clark, 2002; Iwai, Emerson, and Walters, 2006). In California, the average treatment on the treated is higher than the national effect, with workers who selected into treatment experiencing a \$1.09 increase in average wages, which translates into a 9% increase in wage rates of California foreign-born farmworkers. Matching performed using observations from all other regions yielded an average treatment effect of \$0.65, or roughly a 5% increase in average wage rates of farmworkers found in regions other than California. The average treatment effect on the treated is significant at the national level, for workers in California, and for workers grouped in all other regions excluding California.

The average treatment effect on the untreated (the effect that untreated workers would experience if they sought treatment), is only significant at the national level. The average treatment effect on the untreated at a national level is an estimated \$0.29, or a 2%, increase in average wage rates. For untreated workers in California, there is an estimated \$0.49, or 5%, increase in average California wage rates. In the group encompassing workers in all other regions, there is an estimated \$0.02, or a 0.7%, increase in average wage rates.

The estimated average treatment effect (the wage effect that a worker selected at random to receive treatment would experience) is positive and significant nationally, as well as in California and for the group of all other regions. The national average treatment effect is estimated to be a \$0.49, or 4%, increase in wages. For California workers, the average treatment effect is estimated to be higher than the national effect, with the randomly selected worker experiencing a $0.71, or 6%, increase in wage rates due to receiving the treatment. In the all other regions grouping, the average worker could expect an estimated $0.22, or 2%, increase in wage rates.

Results from the minimum-biased estimation technique are reported and compared to the treatment effects estimates calculated from propensity score matching techniques in Table 11. The results are presented in terms of percentage differences between the wages of the treated (authorized) and untreated (unauthorized), and are interpreted as suggested by Peel (2018).

Comparison of the PSM and MBE methods indicate, in general, that immigrant workers who select into legal status for US farm work earn higher wages than the average immigrant worker who gains legal status, and more than the average undocumented immigrant worker. At the national level, the PSM reports that those immigrant farmworkers who selected into legal status ($ATET_{PSM}$) earn wages that are 3% higher than the average immigrant worker who randomly gained legal status (ATE_{PSM}), and 5% higher than those who remained undocumented (ATU_{PSM}). This gap is shown to narrow to 2% in both of the latter cases, when the MBE is used to account for selection bias that arises from unobserved sources – which may be peculiar characteristics about the workers or labor market conditions that are unknown to the researcher.

A similar direction of effect is apparent for California, and the magnitude of the gap is the same for the $ATET_{PSM}$ and the ATE_{PSM}. However, the gap between the estimates for the $ATET_{MBE}$ and the ATU_{MBE} is much lower (1%) than that the $ATET_{PSM}$ and ATU_{PSM} (4%). This suggests that the PSM would largely overstate the effect of legal status on the wages for authorized versus unauthorized workers. Also interesting, while the $ATET_{PSM}$ and ATE_{PSM} for the combined regions show the same magnitude of difference, the largest gap of the three regions modeled in the analysis is apparent: the $ATET_{PSM}$ is 4.3% higher than the ATU_{PSM}. However, this effect disappears when for the MB estimates of these population means. This appears to underscore the importance of recognizing that there are likely unobserved characteristics driving the selection bias, which would result in the wage implications of legal status being overestimated, if ignored.

Table 7 Explanatory Variables in Logit Specification

Variables	Definition
Age	Age of farm worker at time of interview
U.S. Farmwork Experience	Years of U.S. Farmwork
U.S. Farmwork Experience, sq.	Years of U.S Farmwork squared
English-Speaking Ability	=1 if worker has the ability to speak English =0 if worker does not
Employed by Farm Labor Contractor	=1 if worker is employed by FLC =0 if worker is not
Seasonal Labor	=1 if worker is classified as seasonal labor =0 if worker is not
Annual Farmwork Weeks	Number of weeks worked on-farm during the last year
Annual Weeks Spent Abroad	Number of weeks spent outside of the U.S. during the last year
Years Since Immigration	Number of years since worker entered the U.S. to live or work for the first time
Educational Attainment	Number of years of traditional schooling
Entered U.S. after 1986	=1 if worker entered the U.S. for the first time after 1986 =0 if entered before 1986
Entered U.S. after 2001	=1 if worker entered the U.S. for the first time after 2001 =0 if entered before 2001

Table 8 Propensity Score Logit Specification

Variables	Coefficients					
	National		California		All Other Regions	
Age	0.0207	***	0.0103	**	0.0282	***
	(0.0034)		(0.0051)		(0.0044)	
U.S. Farmwork Experience	0.0118		0.0370	**	-0.0163	
	(0.0127)		(0.0181)		(0.0174)	
U.S. Farmwork Experience, sq.	0.000853	**	0.0003		0.0013	***
	(0.000357)		(0.0004)		(0.0005)	
English-Speaking Ability	0.468	***	0.6510	***	0.4000	***
	(0.0584)		(0.0838)		(0.0807)	
Employed by FLC	-0.3240	***	-0.4070	***	-0.6690	***
	(0.0792)		(0.0915)		(0.227)	
Seasonal Labor	0.1560	***	0.1980	**	0.0078	
	(0.0577)		(0.0835)		(0.0790)	
Annual Farmwork Weeks	-0.0168	***	-0.0257	***	-0.0099	***
	(0.0024)		(0.0036)		(0.0032)	
Annual Weeks Spent Abroad	-0.0066		-0.0293	***	0.0110	
	(0.0041)		(0.0068)		(0.0052)	
Years Since Immigration	0.0561	***	0.0436	***	0.0752	***
	(0.0072)		(0.0105)		(0.0099)	
Educational Attainment	0.0755	***	0.0438	***	0.1060	***
	(0.0087)		(0.0116)		(0.0116)	
Entered U.S. after 1986	-1.525	***	-1.9230	***	-1.1130	***
	(0.0940)		(0.1360)		(0.1290)	
Entered U.S. after 2001	-0.3270	***	-0.5240	***	-0.1550	
	(0.1000)		(0.1520)		(0.1310)	
Constant	-1.7270	***	-0.4150		-2.9000	***
	(0.2360)		(0.3450)		(0.3200)	
Observations	11,956		5,927		6,031	

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.10

Table 9 Presence of Bias: Unmatched and Matched Samples

Variable	Unmatched			Matched			% of Bias Reduction
	Authorized	Unauthorized	Bias	Authorized	Unauthorized	Bias	
Age	45.53	32.89	116.5%***	45.53	45.20	2.8%	97.6%
U.S. Farmwork Experience	22.16	9.35	141.1%***	22.16	21.22	10.3%	92.7%
U.S. Farmwork Experience, sq.	608.65	134.23	125.0%***	608.65	573.78	9.2%	92.7%
English-Speaking Ability	0.64	0.44	42.9%***	0.64	0.61	6.3%	85.3%
Employed by FLC	0.12	0.17	-13.8%***	0.12	0.13	-2.2%	84.3%
Seasonal	0.44	0.41	5.4%***	0.44	0.45	-2.0%	63.3%
Annual Farmwork Weeks	41.02	40.95	0.5%	41.02	40.98	0.3%	43.3%
Annual Weeks Abroad	2.24	3.40	-14.1%***	2.24	2.28	-0.5%	96.9%
Years Since Immigration	25.18	10.62	160.3%***	25.18	25.07	1.2%	99.3%
Educational Attainment	6.15	6.59	-12.2%***	6.15	7.49	-37.2%	-204.8%
Entered U.S. after 1986	0.36	0.94	-153.8%***	0.36	0.37	-3.1%	98.0%
Entered U.S. after 2001	0.06	0.41	-89.7%***	0.06	0.06	-1.0%	98.9%
Mean Total Bias			*72.9%*			*6.3%*	

*** p<0.01, ** p<0.05, * p<0.10

Table 10 Propensity Score Matching Treatment Effects Estimation

	National		California		All Other Regions	
	Wage Differential	% Differential	Wage Differential	% Differential	Wage Differential	% Differential
ATET	$0.89***	7%***	$1.09***	9%***	$0.65***	5%***
ATU	$0.29**	2%**	$0.49*	5%*	$0.02	0.7%
ATE	$0.49***	4%***	$0.71***	6%***	$0.22*	2%*
----------------	---------------	-----------	---------------	-------------	--------------	-------------
Sorting Gains[1]	$0.40	3%	$0.39	3%	$.43	3%
Matching Distance[2]	0.0003		0.0009		0.0007	
N	11,958		5,927		6,031	

Asterisks denote significance - ***p<.01, **p<.05, *p<.10
[1]Sorting gains = ATET – ATE
[2]Matching distance =abs[propensity score – propensity score of nearest neighbor]

Table 11 Bias-Minimizing Treatment Effects Estimation

	National		California		All Other Regions	
	PSM	MBE	PSM	MBE	PSM	MBE
ATET	7%***	5%***	9%***	5%***	5%***	3%***
ATU	2%*	3%**	5%	4%***	0.7%	1%
ATE	4%***	3%***	6%***	3%***	2%*	3%***
N		11,958	5,927		6,031	

Asterisks denote significance - ***p<.01, **p<.05, *p<.10

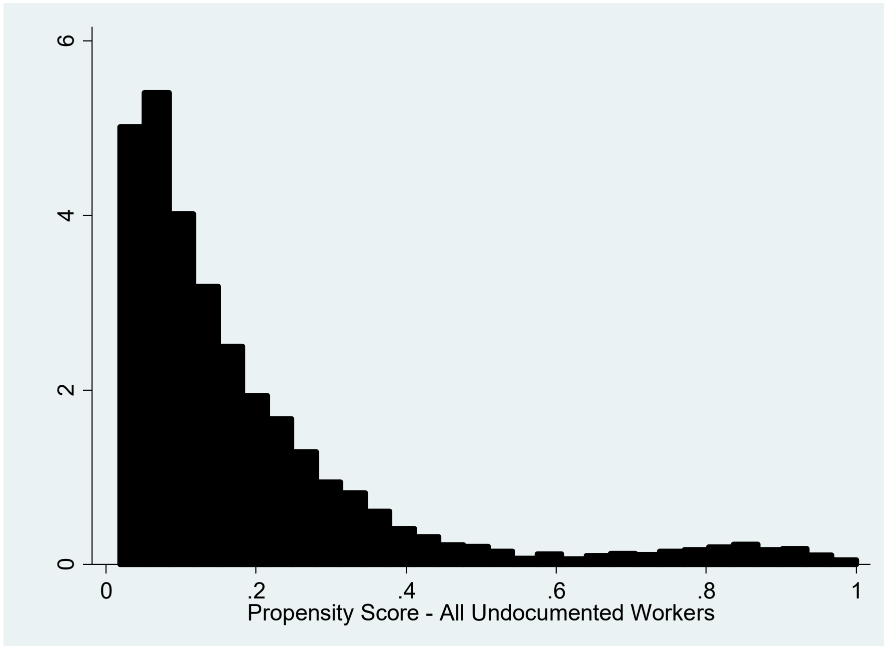

Figure 2 Distribution of Propensity Scores – All Undocumented Workers

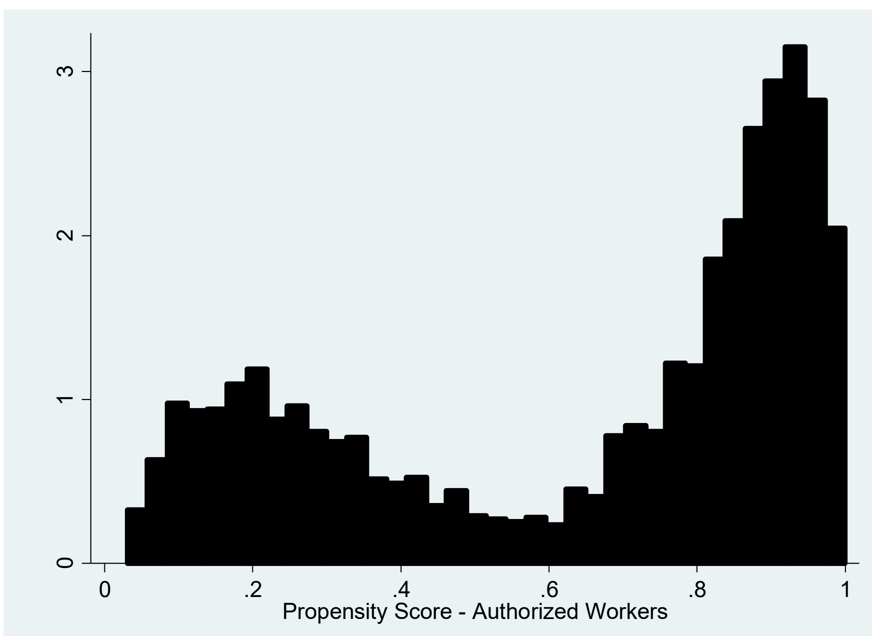

Figure 3 Distribution of Propensity Scores – All Authorized Workers

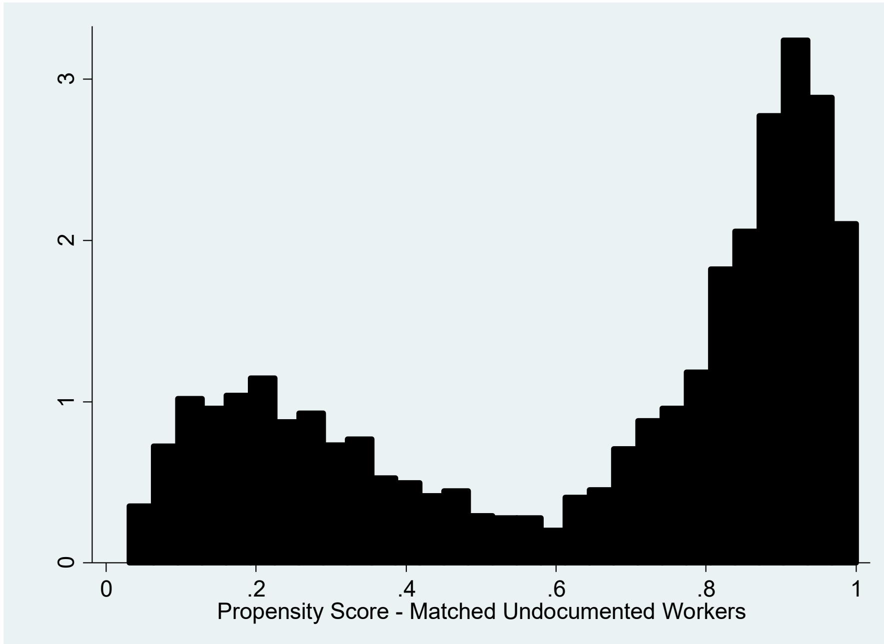

Figure 4 Propensity Score Distribution – Matched Undocumented Workers

CHAPTER VII

DISCUSSION AND CONCLUDING REMARKS

The purpose of this study was to analyze the wage implications of legalization on foreign-born U.S. crop farm worker wages at the national level, in California, and in a grouping of the other five NAWS crop regions. Using PSM and MBE, three population means were estimated: (1) the average treatment effect, (2) the average treatment effect on the treated, and (3) the average treatment effect on the untreated. This study contributes to the literature in two ways: first, it assesses the potential wage outcomes on the national relative to regional levels. This was not assessed previously by past studies. Second, this study uses the minimum-biased estimator (MBE) proposed by Millimet and Tchernis (2013) as a robustness check for unobserved selection-bias. To the authors' knowledge, the minimum-biased estimator has not be applied in the context of this particular research problem. Previous work has acknowledged selection bias that arises from observed sources, but has not explicitly addressed when it arises from unobserved sources.

Due to the non-randomization of the treatment (legal status), an individual either selects into treatment based on observable characteristics or selects into treatment based on unobservable characteristics or based on a mixture of both. This study employs treatment effects estimation methods under the assumption that farmworkers select into legalization based **both** on observables (via propensity score matching) and unobservables (via minimum-biased estimation). The strength of the estimates provided through propensity score matching rests on the conditional independence assumption, which signifies that selection into treatment is random conditional upon a specified vector of covariates. If the conditional independence assumption fails to hold, propensity score estimates become subject to selection bias. To detect the presence of selection on unobservables, and to provide the treatment effect estimate with the least bias (assuming the conditional independence assumption fails to hold), minimum-biased estimators are employed. Propensity scores were calculated via a logistical regression and authorized observations with were matched with unauthorized counterparts based on similar covariates.

Results from the matching show that there are statistically significant positive wage effects at the national level as well as for California and the rest of the United States. California is singled out for comparison as a major specialty crop producing state[5] with high labor intensity relative to the rest of the United States. California, as a single crop state/region in the NAWS data, has a large immigrant workforce, much of which is unauthorized for US employment. The average treatment effect on the treated is largest in California at \$1.09 more for authorized workers relative to unauthorized, while the wage effect for all other regions is \$0.65. The national wage effect of authorization is \$0.89. The average treatment effect (the wage effect that a randomly selected worker would experience if the work moved from unauthorized to authorized) is positive and significant nationally (\$0.49) as well as in California (\$0.71) and all other regions

[5] California is designated as a distinct crop region in the NAWS data.

($0.22). The average treatment effect on the untreated is significant at the 5% level nationally ($0.29) and at the 10% level for California ($0.49), but insignificant for all other regions. As discussed in Caliendo (2006), the average treatment effect on the untreated highlights the potential policy implications for the future. These results are a signal of what could happen if policies were put in place to make it easier for unauthorized workers to transition to authorized status or if another large-scale amnesty program was targeted toward the agricultural industry.

California tends to stand out relative to the rest of the country in terms of the impact of authorization on farm workers. The treatment effect is higher in California than the rest of the country for both the ATET and the ATU. This is important because 44% of all foreign farm workers are in California and California supplies roughly 50% of the nation's fresh fruits and vegetables (Martin 2014). The political climate is also different in California relative to much of the rest of the country. Immigration laws have been enforced more leniently in California and state-level laws tend to be more favorable for undocumented workers in California relative to much of the rest of the country. This is widely documented in various media reports (Ramakrishnan and Colbern, 2015; Garcias, 2017; FindLaw, 2018). Clearly, this may create a unique environment for farm workers in the state, in that it could potentially be easier to gain legal status. Furthermore, from an econometric standpoint, sample size considerations played a role in determining how to split the data for analysis. The sample size for each individual region in the NAWS dataset, with the exception of California, was too small for the minimum-biased estimation to properly run.

The order of the magnitude of the estimates (ATET>ATE>ATU) indicates positive sorting regarding the gains from obtaining legal status. Foreign-born farm workers who had taken deliberate actions to obtain legal status gained more from it than the average, randomly-selected worker, and more than those that were unauthorized. This suggests that farm workers may be weighing the benefits of obtaining work authorization with the costs of becoming authorized. Arguably, those who stand to gain the most may tend to become authorized while those who are likely to gain the least may opt to remain unauthorized. In the latter case, it is possible for there to be unique factors that may disqualify the immigrant worker, that are unobserved by the researcher. The process to become legalized is known to be time and effort intensive, as well as expensive and requires a knowledge of how to navigate the process of obtaining some type of legal status (i.e. green card, legal permanent resident, permanent U.S. citizen). For the average unskilled and undocumented farm worker, legal status may not be an option. Further, the average undocumented worker may also perceive it as risky, in that it could potentially increase detection and deportation risks. Clearly, these are factors that may not be easily documented on any survey or observed by researchers, although they could impact undocumented workers.

Focusing on the minimum-biased (MBE) estimates relative to the propensity score (PSM) estimates, PSM estimates were higher than MB estimates. This suggests the PSM method overstates the average treatment effect on the treated (ATET) (nationally, in California, and in all other regions), the average treatment effect on the untreated (California), and the average treatment effect (nationally and California), due to unobserved variables that are correlated with both the outcome and treatment. If the variable(s) could be included in the analysis, the PSM estimate would likely be lower.

The PSM estimate of the average treatment effect on to the untreated (ATU) (national), and the average treatment effect (all other regions) was lower than the MBE suggesting that the PSM estimate is understated due to unobserved correlated variable(s). In the case of a PSM overstatement of the effect, the unobserved variable is positively correlated with both the outcome variable and the treatment variable. Conversely, in the case of a PSM understatement of the effect, the unobserved variable is positively correlated with the outcome variable and negatively correlated with the treatment variable (Peel, 2018). The awareness of the presence of selection bias due to unobservable factors shows the importance of utilizing methods that correct for such bias.

Beginning with the average treatment effect on the treated, the MBE estimate is lower than the PSM in all three regional specifications. Like the $ATET_{PSM}$ estimate, the $ATET_{MBE}$ estimate is significant, signifying that the $ATET_{PSM}$ is overestimated due to the presence of an unobserved correlated variable. The minimum-biased estimate for the average treatment effect on the untreated is higher than the ATU_{PSM} estimates, and significant in California, unlike the ATU_{PSM} estimate for California. This implies that the ATU_{PSM} estimates are underestimated nationally and in the group containing all other regions, whereas ATU_{PSM} estimates are overestimated in California.

Regarding the average treatment effect, the ATE_{MBE} estimates are all significant, however, the ATE_{MBE} estimates are lower nationally and in California, implying that the ATE_{PSM} estimates are potentially overestimated due to the presence of an unobserved correlated variable. In the all other regions grouping, the ATE_{MBE} estimate is higher, suggesting that the ATE_{PSM} estimate could be underestimated due to an unobserved correlated variable. In sum, these results appear to suggest that studies that do not address this particular source of bias would overestimate that wage implications of legal status, and, by extension, the potential hired labor cost increases.

Evaluating the precise economic implications of legalization for foreign born workers in the US economy can be challenging considering the myriad data collection and estimation issues that arise, not to mention the dynamic political environment in which legislation on the state and national levels are made. Foreign-born individuals who seek employment would make such decisions based on a variety of factors, only some of which some are actually observable. There may be certain additional characteristics, behaviors and attitudes of immigrant workers, and/or conditions of the work environment, the labor market and the immigration policy environment that may need to be taken into account. The regional analysis conducted in this study was restricted to two regions – California and all other regions – due to sample size restrictions during the minimum-biased estimation. Future research could focus on addressing this issue and, if successful, analyze the effects across more regions, to ascertain how these may differ accordingly, particularly in light of the implementation of various immigration initiatives at the state level. Additionally, future research could attempt to assess the regional effects using other comprehensive data sets, if available. Although such analyses may be sensitive to the methods of estimation, they would contribute to greater understanding of potential impacts of immigration reform, especially if it were to affect the supply of workers and hired labor costs. This is crucial for labor intensive sectors in US agriculture, given the ramifications that it could have for agricultural investment and production decisions and producer livelihoods.

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APPENDIX A

ADDITIONAL TABLES

Table 12 Propensity Score Matching Treatment Estimation – National

Sample	Treated	Controls	Difference	S. E.	T-Stat	P-Value
Unmatched	10.66	9.51	1.14	0.05	21.03	0.0001
ATT	10.66	9.76	0.89	0.11	8.29	0.0001
ATU	9.51	9.79	0.28	0.13	2.26	0.0432
ATE			0.49	0.10	5.17	0.0002

Table 13 Propensity Score Matching Treatment Estimation – California

Sample	Treated	Controls	Difference	S. E.	T-Stat	P-Value
Unmatched	10.76	9.54	1.23	0.07	16.68	0.0001
ATT	10.76	9.67	1.09	0.14	7.85	0.0001
ATU	9.54	10.04	0.50	0.25	1.97	0.0724
ATE			0.72	0.17	4.16	0.0013

Table 14 Propensity Score Matching Treatment Effects Estimation – All Other Regions

Sample	Treated	Controls	Difference	S. E.	T-Stat	P-Value
Unmatched	10.53	9.49	1.04	0.08	12.93	0.0001
ATT	10.53	9.87	0.65	0.15	4.46	0.0008
ATU	9.49	9.51	0.02	0.13	0.19	0.8525
ATE			0.22	0.11	2.03	0.0651