The Value of an Employment Based Green Card

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Employment of foreign-born workers is a fiercely debated issue centering on the beneficial aspects of additional labor within the United States, balanced against its potential adverse effects on native workers. To work in the U.S., foreign-born workers must have either a valid employment visa or become a legal permanent resident (i.e. a green card holder). A legal permanent resident can easily change jobs and can work within the U.S. indefinitely. According to the Department of Homeland Security (DHS), during the period 1999-2008 the number of green cards approved averaged at about one million per year. About 15% of all green cards approved were employment based (EB), about 21% were family based, about 43% were relatives of U.S. citizens with the rest made up of diversity visas, refugees, and asylees (about 16%).

Temporary visa holders, on the other hand, face more restrictions on the duration of their employment in the U.S. (maximum is six years for H-1B visa holders). They also face restricted mobility across jobs (especially if they have already applied for a green card). Two types of visas that are commonly used to bring skilled foreign workers to the U.S. include the H-1B and the L-1 visas. These “Dual intent” immigration programs permits an immigrant to enter into the United States with a temporary visa, but with an intention for permanent migration. Such programs have been at the center of the controversy since this program was introduced by the U.S. Congress in 1990.[1]

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[1] In our analysis we do not distinguish between H-1B and L-1 temporary workers because our data do not have that information. While the use of L-1 visa has increased since the late 1990s the number of L-1 visas issued is still small compared to the H-1B program even though the number of L-1 visas is not capped (Kirkegaard 2005). Given the time frame of our sample we do

The H-1B visa allows U.S. employers to temporarily employ foreign workers in "specialty" occupations for up to 6 years (NFAP 2009). Proponents of this program argue that it gives employers access to the skilled foreign workers they require in order to satisfy their staffing needs; however, opponents argue that the system is misused by the employers (Hira 2007; Miano 2007). The L-1 visa on the other hand is used for intra-company transfers. Although the effects of the use of temporary workers has been debated in the media and in popular press for years (Wayne 2001; Hafner and Preysman 2003; Hamm and Herbst 2009 among others) but careful empirical analysis of this topic is rare. There are two very much interrelated, nonetheless distinct, questions involved in this debate. First, do temporary workers work for less than the comparable natives (or legal permanent residents) and second, if so, does that adversely affect the wage of domestic workers? Even if the answer to the first question is yes (i.e. temporary workers get paid less than the natives) that does not necessarily imply a lower wage for the natives if the supply of foreign-born workers is restricted to a number that is not significant enough to affect the market for domestic workers at the aggregate level. Furthermore, if there are some "localized" effects they may not be evident in the aggregate data.

On the former question, Kirkegaard (2005) reports *"aggressive wage-cost cutting"* and paying H-1B workers only the legally mandated wage (which is 95% of "prevailing wage") but he did not find any evidence of systematic abuse. Hamm and Herbst (2009) on the other hand presents evidence of abuse by a section of employers. On the later question, National Research Council (2001), Lowell (2001), as well as Zavodny (2003) found no evidence of adverse effects

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not expect the share of L-1 immigrants to be particularly large in our sample. We further discuss this issue in the data section.

of H-1B program on wages. All these studies use aggregate data. Unlike the papers discussed above, Kandilov (2007) uses individual level data and found that immigrants experience a wage gain of 18-25 percent after they receive their permanent resident status. Her analysis however suffers from several shortcomings which we discuss in the next section.

In this paper we estimate whether immigrants with a temporary visa are paid less than permanent residents. Therefore the primary contribution of this paper is in the area of first question discussed above. We do not compare the wages of temporary workers to any artificial benchmark like "prevailing wage" because that approach has certain limitations. For example some employers formally process the paperwork for employees for one market (presumably a low cost of living market) but then send the employees to a high cost of living market (Hamm and Herbst, 2009). Hamm and Herbst (2009) also report a plethora of frequent exploitations by employers; paying immigrants less than the promised wage, withholding healthcare benefits, the charging of excessive fees, and the practice of "benching," where the worker is not paid between successive projects. Therefore, we simply ask the question, do the temporary workers (i.e. the ones without a green card) earn less than comparable immigrants with a green card? In the absence of any employment friction, any temporary immigrant worker should receive the same wage as a comparable immigrant with green card (or a native worker). We use a propensity score matching model and individual level data from the New Immigrant Survey (2003). Our structure is most closely related to Kandilov (2007). We focus only on monetary (wage) value employment based (EB) green cards[2].We use immigrants who first arrived without a green card

[2] Green cards (especially Family based green cards, political asylums, or green cards through marriage) may have a lot of non-monetary value (like utility from having relatives close-by; peace of mind from persecution) but our paper does not address those issues.

as control group and immigrants who first arrived with a green card as our treatment group. Our results show that an Employment based green card leads to a wage gain of around $11,680 per year.

Background

In the absence of any employment friction, any temporary immigrant worker should receive the same wage as an equivalent (i.e. other things being equal) native worker. If the wage offered by current employer is below his/her marginal productivity then another employer can potentially 'poach' the worker and still make profit. The structure of immigration rules creates friction in the labor market that may prevent temporary workers from earning the same wage as natives and permanent residents. One such friction comes from the direct cost of legal and processing fees which is about $6,000 (National Foundation for American Policy 2006).
The primary source of this friction is, however, the restricted mobility among immigrants during the period between applying for permanent residency and receiving permanent residency (i.e. green card). Since the number of temporary worker visas is far greater than number of permanent visas issued each year, a large number of temporary workers have to wait a substantial length of time, roughly 6 to 10 years (NFAP 2009) for their permanent residence request to process[3]. While most H-1B holders would like to have a green card (Wayne 2001) it is not possible to figure out exactly how many H-1B holders eventually get a green card because the I.N.S. keeps no statistics on the number of H-1B's who eventually get green cards (Wayne 2001). The percentage of successful transfers from a temporary H-1B worker to a legal permanent resident

[3] For example a *New York Times* article in 2001 describes the plight of temporary immigrant IT workers dealing with the uncertainty of imminent deportation dependent upon their reception of a green card (Wayne 2001).

remained undocumented by the I.N.S. even until their dissolution in 2003 (Wayne 2001). However using data from U.S. Citizenship and Immigration Services (USCIS) and Department of Homeland Security (DHS) we can get a somewhat rough estimate on the upper bound. For example, column 1 of table 1 shows the number of H-1B petitions approved during the years 2000-2008. The average number of H-1B petitions approved during this period is 265,217 per year. Column 2 shows the number of employment based (EB) legal permanent residency (LPR)[4] awarded to adjustees during the same period with the average being 118,565. Given the delay in processing LPR s these averages are not directly comparable. However, the average of the number of H-1B petitions approved during the years 2000-2003 is 262,128. On the other hand number of EB green cards awarded to adjustees in 2008 was 149,542. Thus the ratio between number of EB LPRs granted to adjustees and H-1B petitions approved is about 56%. We must note that this is an approximate upper bound and the true estimate of the share of H-1B workers that end up with a green card is even lower that 56% because employment based LPR numbers includes spouses of the principal applicants but the H-1B number is for principal applicant only. Furthermore, some of the immigrants who were awarded employment based green cards may have entered the country on L-1 or other visas.

The visa application process is very restrictive towards the applicant and while the immigrants wait to become permanent residents, they are unable to change jobs without losing their position in the visa queue or even accept a promotion in many cases (since green card applications cannot be transferred). The restrictions of the application procedure allow employers to underpay their foreign workers because of the immigrant's highly limited mobility. While

[4] We use the terms "green card", "legal permanent residency (LPR)", and "permanent residency" interchangeably in this paper.

employers are legally required to pay the “prevailing wage” to H-1B employees, opponents of such programs argue that existing loopholes allow employers to underpay their immigrant workforce (Matloff 2004; Hamm and Herbst 2009). A study conducted by the GAO in 2003 reported that “*Although some employers acknowledged that H-1B workers might work for lower wages than their U.S. counterparts, the extent to which wage is a factor in employment decisions is unknown*.” Furthermore, L-1 visas do not even require employers to pay prevailing wage to workers (Zavodny 2003).

Kandilov (2007) is the only paper (that we are aware of) that uses individual level data to estimate whether the wages of temporary immigrants increases after they receive their permanent resident status. She uses the data from the NIS 2003. She uses the initial wages of the immigrants after they first arrived in the U.S. and their current wages (after receiving permanent residency) and then applies difference in difference matching using native workers as her control group. She finds that immigrants experience a wage gain of 18-25 percent between their first job in the U.S. and their current job after receiving a green card. She attributes the wage gain experienced by immigrants to the acquisition of a green card.

Her approach however has several problems. The first problem comes from using natives as the control group. Assimilation literature argues that new immigrants lack host country specific capital and, as they remain in the host country, they accumulate the specific human capital needed, thereby narrowing the wage gap between natives and immigrants. This means that, for any given level of human capital, the initial difference between natives and immigrants will be larger and that difference will narrow over time as the immigrants accumulate host country specific human capital. If this trend occurs, then this narrowing will be interpreted as the green card effect. Kandilov (2007) estimates an OLS regression with a binary dummy equaling one if the immigrant is a new arrivee. Based on this regression, she reports that newly arrived immigrants do not have lower wages than comparable natives once she controls for other covariates. She interprets this as a sign that assimilation and the accumulation of host country specific capital does not impact the wage of this group of high skilled immigrants. However, in her regressions, she does not allow the new arrivals to be different from the adjustees in any unobservable way. If their unobservable differences are not identical, for example if new arrivals are more able than adjustees, then such a difference would make the new arrival dummy endogenous.

Another bias, going in the opposite direction, may come from the fact that the respondents were interviewed immediately, in most cases within six months, after becoming permanent resident. In some cases, adjustees may not have had the time to find and relocate to a new job. In other words, the full impact of the treatment may not have been realized by the time they were interviewed. If this is the case then estimates from Kandilov (2007) may suffer from downward bias. On the other hand, for both our treatment and control groups, we are using wage data from the first job after coming to the U.S. Thus our estimates are less likely to face this problem

Finally Heckman, Ichimura, and Todd (HIT) (1997), Heckman, Ichimura, Smith, and Todd (HIST) (1996), and HIST (1998) found that one of the most important criterion under which matching estimators perform well is when the treatment and control group data come from the same sources. This allows earnings and other characteristics to be measured in an analogous way. Kandilov (2007) does not conform to this last condition and therefore could be further biasing her results.

In this paper we solve these problems by estimating a matching model using the data from immigrants only. We exploit the fact that some of the NIS 2003 interviewees were adjustees (i.e. individuals who were already living in the U.S.) when they applied for green cards and the others new arrivals (i.e. they arrived with green card). Because the newly arrived immigrants came to the U.S. with a green card, they do not face the frictions that may reduce the wages of temporary immigrants. In other words, the newly arrived immigrants received the treatment, which is the reception of permanent residency in this context. On the other hand, the adjustees, when they first arrived in the U.S., did not have green cards and therefore become susceptible, during their first job in the U.S., to the labor market frictions outlined above. We use the adjustees as our control group. For both groups we use the wages of immigrants right after they migrated to the U.S., which means they have very minimal U.S. specific experiences, thus making the matching more effective (Smith and Todd 2005; Heckman, Ichimura, Smith, and Todd 1996; Heckman, Ichimura, Smith, and Todd 1998). Nevertheless, our argument above suggests that matching on observable characteristics may not be enough, in this case, as there might be time invariant unobservable differences.

HIT (1997), HIST (1998), Smith and Todd (2005) concluded that usually difference in difference matching estimators produces results closest to experimental outcomes, because the time invariant characteristics, like ability, between the treatment and control groups are differenced out. Since we suspect that is to be the case in this application, we implement difference in difference matching estimation. In the NIS, interviewees were asked about their wages of their last job in source countries before they migrated to the U.S. We use the purchasing power parity adjusted source country wages for both the treatment and the control groups as the before treatment wages in order to implement a difference in difference matching estimator.

Note that our "before income" refers to their income right before they migrated to the U.S. and our "after income" refers to their income right after they migrated to the U.S. Since different individuals came to the U.S. at different points of time, especially our control group, the before-after reference used here may not always correspond to calendar time. For example, an adjustee (our control group) who arrived in the U.S. in 1996 has both a before and after wage during their immigration year of 1996, while a new arrivee (our treatment group) who arrived in the U.S. in 2003, has a before and after wage from their immigration year of 2003. All wages are at 2003 prices. For source country we use the purchasing power parity (PPP) adjusted data from NIS. The New Immigrant Survey conducts a purchasing power parity adjustment of the wages of the immigrant from their host country into U.S. dollars based on purchasing power estimates derived from the Penn International Comparisons Project (Jasso, 2000; Jasso and Rosenzweig 2008). The estimates were developed by using a comparison of the costs of living and exchange rate fluctuations between the immigrant's source country and the United States, allowing an exact comparison of the earnings of different immigrant workers from a variety of host countries (Jasso 2000; Jasso and Rosenzweig 2008). To see further details regarding this approach please see Summers and Heston (1991).

Before we proceed further, it may be worthwhile to note some of the problems that might be present with our current approach. We noted previously that a share of H-1B workers do not ultimately get (or apply for) a green card. But in our sampling framework we only observe the H-1B worker who successfully applied for a green card. However, if the group that ends up with a green card is either positively or negatively selected then our estimates would be biased.

However, existing literature suggests that selection may not be a significant problem in this context. For example Chiswick (1980), and Reagan and Olsen (2000) found no selectivity among return migrants. Some authors do report evidence of selection among emigrants but they do not agree whether the selection is positive or negative. For example Jasso and Rosenzweig (1988) report evidence of positive selection among return migrants but Borjas (1987), and Massey (1989) report evidence of negative selection among return migrants. Finally, Constant and Massey (2003) report that *"(I)n congruence with previous studies, we find that cross sectional earnings results are not substantively distorted by selection biases due to emigration."* In light of the above evidence we feel that using adjustees as control group is not a significant problem in the present context.

Also, if there is discrimination against immigrants, and that discrimination is changing over time, then we might create a bias in our estimates. For example, if discrimination against immigrants is decreasing over time, then we may overestimate the green card effect. However, given the relatively short timeframe, we do not feel this to be a problem. Also, if the wage in the U.S. is growing at a faster rate compared to the source countries of the immigrants, then also we will overestimate the green card effect. But since most of the source countries wage grew faster than that in the U.S., if anything, we might underestimate the green card effect.

Data

Data used in this paper comes from the New Immigrant Survey which provides extensive information on nationally representative new lawful immigrants.[5] We use the adult sample i.e.,

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[5]According to USCIS an immigrant is someone who has a green card (permanent residence). All other foreigners in the U.S. are not considered immigrant.

individuals who were over the age of 18 (at the time of the interview), was the principal applicant, and became permanent residents between May and November of 2003. A total of 8,573 adult immigrants were interviewed between June 2003 and June 2004, after they achieved permanent resident status.

(Table 2 about here)

Our baseline sample consists of 333 individuals on whom we have data on all variables. This sample size is considerably smaller than original sample size. This is because only 15 percent (1,389) of all principal applicants are employer sponsored. Among those employer sponsored individuals, the source country wage is available only for 434 individuals. Out of those 434 individuals, we drop a further 101 from the sample, because some of the other variables are missing observations, leaving us with 333 individuals. First four columns of table 2 presents the summary statistics for the variables used for the treatment (those who arrive with a green card) and the control group (adjustees)[6]. Most of the control group (about 75%) first entered the U.S. on a temporary worker visa[7]. The control group (adjustees) came earlier between 1987 and 2003 with about 85% coming between 1995 and 2000. The immigrants in the treatment group arrived in the U.S. during 2002 and 2003. The treatment and control group are not too dissimilar given that this is a non-experimental sample. The variable age refers to the current age for the

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[6] We also implemented a cross-sectional matching on a larger sample of 863 immigrants. The summary statistics for this group are in the last four columns of table 2.

[7] In the NIS visa category is identified only as "temporary worker" and does not provide additional information like whether it was H-1B or L-1. However the period during which most the control immigrants arrived L-1 was not particularly large program.

treatment group and age at the time of first U.S. job for the control group[8]. Note that we are using their wage in their first U.S. job as our control hence that is the appropriate way to define the age variable. The average age of the control group is a little higher (about 39 years) compared to the treatment group (about 36 years). We use education obtained in their source countries for matching between the treatment and the control group. In the NIS, respondents were asked about their total years of education at the time of the interview and they were also asked how many years of U.S. education they have. We calculated source-country education as the difference between these two variables. The average education level in the treatment group is about 15.4 years and in the control group is 16.5 years.

In the NIS, respondents were asked about their entry into the labor force in their source countries and about their jobs right before they left their source countries. We used this information to construct their source country work experience. For some of the immigrants who have worked on more than one job in their source countries, exact source-country work experience cannot be determined. The reason is that there is a gap between the time they left their first source-country job and started their last source-country job. Questions about the intervening jobs were not asked in the survey. In these cases we have used potential work experience which is the difference between the first year in which they worked in the source country and the last year in which they worked in the source country. In our sample the average work experience in the treatment group is 12.9 years and in the control group is about 11.7 years.

[8] Alternatively we could have matched of the year of birth. That would not require this adjustment. But interpretations of coefficients of higher order terms (like age squared) are problematic. Using year of birth yields same results.

We also use their occupations (which are reported using census four digit categories) in their source countries for matching. We group the occupations in five categories: professional and managerial (codes 10-2960), health (codes 3000-3650), services (codes 3700-4650), sales and administrative (codes 4700-5930), and production (codes 6000-9750). In the data we have the information on the occupation about the occupation of the immigrants in the U.S. However we do not use that information in propensity score matching primarily because having a green card (or lack of it) expands (shrinks) the choices of immigrants in terms of their job search and therefore the occupational status is affected by the treatment and hence it should not used for matching (See Todd 2008 for a detailed discussion). We also match the treatment and the control group on their source continent, religion, gender, and marital status.

In the NIS respondents were asked about their relative family income when they were 16 years old. The exact question was *"Now I'd like to ask you some questions about when you were a child. Thinking about the time when you were 16 years old, compared with families in the country where you grew up, would you say your family income during that time was far below average, below average, average, above average, or far above average?"* About 21% of the treatment group is from families that had above average or far above average income while 31% of the control group had above average or far above average income.

Wages

We use the wages of the respondents at two different points of time. The before wage is the respondent's wage from their last job in his source country just before he migrated to the U.S., while the after wage is his wage right after his migration to the U.S. For individuals who arrived with a green card, the second wage reflects the treatment (i.e., green card) effect and the re-pricing of their skills in the U.S. labor market, while for adjustees the after wage (i.e. their first wage after they migrated to the U.S.) captures the re-valuation of their skills at the U.S.

prices. The differential of these wages would allow us to identify the green card effect. We use hourly wages in 2003 prices for all wage observations. Source country wages are also adjusted for purchasing power parity. In the questionnaire, individuals were asked about payment and pay periods. For salaried individuals, hours of work per pay period are available. We calculate hourly wage rate by dividing total payment in any particular pay period with hours worked.

(Table 3 about here)

Table 3 shows the different wages for the treatment and the control group. For the treatment group, their average before (source country) wage is $18.13 and it increases to $29.67 after they migrate to the U.S. For the control group, their average before (source country) wage is $16.38 and it increases to $23.44 after they move to the U.S. Therefore, after moving to the U.S., the treatment group experiences a wage increase of $11.54 per hour while the control group experiences an increase of $7.06 per hour. Thus the simple mean difference in difference estimate of the acquisition of a green card leads to an increase of $4.48 an hour or $8,960 per year (based on 2000 hours per year). This estimate does not take into account the differences between the treatment and control groups that exist in non-experimental data. To account for such differences, we implement matching estimators. We provide an abbreviated and rather informal discussion on matching estimators based on Smith and Todd (2005) below. Please see HIT (1997) and HIST (1998) for more detailed discussion about the properties of these estimators.

Methods

Let R=1 denote the treatment of receiving a green card and R=0 indicate not receiving a treatment. Also let Y_1 denote the wage of the treated group with green card and Y_0 be what the wage would have been of the treated group without a green card. The cross sectional estimate of the value of green card is therefore given by

$$\text{TT}= E(Y_1 \mid R=1) - E(Y_0 \mid R=1)$$

For the treated group, we observe the mean wage after they receive the green card $E(Y_1 \mid R=1)$ but the counterfactual $E(Y_0 \mid R=1)$ is not observable. A matching estimator allows us to construct the unobservable counterfactual $E(Y_0 \mid R=1)$ using data from the control group. Matching estimators assume that Y_0 is independent of getting the treatment conditional on a set of observable characteristics (Z). Since we are interested only in the treatment effect on the treated (TT) we require a much weaker assumption, namely conditional mean independence (Smith and Todd 2005)

$$E(Y_0 \mid Z, R=1) = E(Y_0 \mid Z, R=0)$$

Rosenbaum and Rubin (1983) showed that when outcomes are independent of program participation conditional on Z , they are also independent of participation conditional on propensity score $P(R=1 \mid Z)$. We also require that $P(R=1 \mid Z) < 1$, i.e. for each participant there is a non-participant analogue. Then TT can be expressed in the following way:

$$\begin{aligned} TT &= E(Y_1 \mid R=1) - E(Y_0 \mid R=1) \\ &= E(Y_1 \mid R=1) - E_{Z|R=1}\{E_Y(Y_0 \mid R=1, Z)\} \\ &= E(Y_1 \mid R=1) - E_{Z|R=1}\{E_Y(Y_0 \mid R=0, Z)\}. \end{aligned}$$

However, as argued above, cross sectional matching estimators perform poorly if there are permanent unobserved differences between groups so a difference in difference matching strategy is more appropriate in the current context. This type of estimator is analogous to difference indifference regression but it does not impose some of the restrictions like linearity. We implement a panel data version of difference in difference matching estimator.

$$\alpha_{DDM} = E(Y_{1t} - Y_{0t'} \mid X, R=1) - E(Y_{0t} - Y_{0t'} \mid X, R=1)$$

It uses both pre and post treatment (t' and t respectively) data. The first term is the difference between the U.S. wage (after receiving the green card) and the source country wage for the treated group. The second term is what the difference would have been between the U.S. wage (without green card) and the source country wage for the treatment group. Just like before the second term (i.e., the counterfactual) is not observable. Again matching allows us to construct the unobservable counterfactual $E(Y_{0t} - Y_{0t'} \mid X, R=1)$ using data from the control group. In practice the following can be implemented

$$\alpha_{DDM} = \frac{1}{n_1} \sum_{i \in I_1 \cap S_P} \{(Y_{1ti} - Y_{0t'i}) - \sum_{j \in I_0 \cap S_P} W(i,j)(Y_{0tj} - Y_{0t'j})\}$$

where i denotes the individual, $W(i,j)$ are the weights which depend on the distance between the propensity scores P_i and P_j. I_1 denotes the set of participants and I_0 the set of non-participants. S_P is the region of common support and n_1 is the number of persons in the set $I_1 \cap S_P$. We implement two types of matching nearest neighbor and kernel matching.

Let $C(P_i)$ be a neighborhood for each i in the participant sample. Neighbors are non-participants $j \in I_0$ for whom $P_j \in C(P_i)$. In traditional pair wise matching $C(P_i) = \min_j \|P_i - P_j\|, j \in I_0$. That is, the non-participant with the value of P_j that is closest to P_i is selected as the match. In our empirical implementation, we first use a single nearest neighbor followed then by a five and ten nearest neighbor implementation. We also implement a kernel matching estimation procedure. In this case a match for each participant is constructed using a kernel weighted average over multiple persons in the comparison group. We use normal distribution as kernel in our empirical implementation.

Results

Column 1 of table 4 presents the parameter estimates for the propensity score equation. We used a probit model to estimate the coefficients. Estimates suggest that women are more likely to receive the treatment. Immigrants with more source country experience are also more likely to get the treatment. We use indicators for bachelor's degree (more than 16 years but less than 18 years of education) and graduate degree (more than 18 years of education) in our baseline specification[9],[10]. Somewhat surprisingly there is a negative (and statistically significant) correlation between obtaining a graduate degree in source country and probability of getting the treatment. However this is due to the fact that we are also controlling for source country wage as well as source country occupation, both of which is correlated with education. In fact if we do not control for source country wage (as we do later for cross sectional matching) the correlation becomes insignificant (both in statistical and in economic sense). As expected, a higher source-

[9] We did not use years of education in our baseline specification because that variable did not satisfy the balancing criterion in cross-sectional matching sample (discussed below). However years of education does satisfy the balancing criterion in this sample and including it in the propensity score specification does not change our estimates.

[10] We used the –pscore- command written by Becker and Ichino (2002) to check the balancing criterion. This command performs a t-test to check that means of covariates do not differ between the treatment and the control groups within propensity score blocks. See Becker and Ichino (2002) for further details.

country wage increases the probability of getting the treatment. Being in health care related occupation increases the chance of getting the treatment. This probably reflects the shortage of workers in the healthcare field in the U.S. Being Catholic reduces the chance of getting the treatment. Europeans have a higher chance of getting the treatment.

Our results are not sensitive to reasonable changes in the propensity score estimation specification. We checked a few other specification including controlling for years of education in addition to degree dummies, adding higher order terms for experience. Again our estimates do not change in any substantive way as a result of such changes.

Table 5 presents results from the difference in difference estimates using nearest neighbor matching and kernel matching[11],[12]. The first column reproduces the mean difference in difference estimate from table 3. Then we present estimate from nearest neighbor matching using one, five, and ten nearest neighbors. In all matching estimators we impose the common support condition. Bootstrap standard errors based on 200 replications are in parenthesis. Nearest neighbor matching estimates show that employer-sponsored immigrants experience significant wage gain. This wage increase is $5.90 per hour using single nearest neighbor matching. We also implemented matching based on five and ten nearest neighbors. As we increase the number of neighbors used, the variance decreases but the average of quality of matches also decreases. This is the well known trade-off between bias and variance of nearest neighbor matching estimates. Using 5 nearest neighbor our estimate suggest a wage gain of $6.42 per hour but using 10 nearest neighbor the wage gain increases to $9.67 per hour. Given our relatively modest sample size,

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[11] The Details of difference in difference matching tables are in tables A1 and A2 in Appendix A.

[12] We used –psmatch2- command written by Leuven and Sianesi (2003) to get matching estimates.

using a large number of neighbors may quickly lead to bad matches and therefore an increased bias. The nearest neighbor matching estimates are not statistically significant. It could be due to our relatively small sample size but it is important to note that bootstrap standard errors may not be accurate because of the lack of smoothness in nearest neighbor matching (Abadie and Imbens 2008). Also, the literature suggests that kernel matching performs better than one to one matching.

(Table 5 about here)

Therefore, we implement kernel matching next. We use normal kernel. For the bandwidth parameter we use Silverman's (1986) rule of thumb method as suggested by Smith and Todd (2005). The Silverman rule suggests: $bw = (n)^{-\frac{1}{5}} * 1.06 * \sigma$, where σ is the standard deviation of the outcome variable (i.e. hourly wages) and n is the number of observations. Silverman's rule suggests a bandwidth around 6.87. In kernel matching there is also a trade-off between the bias and the variance; the lower the bandwidth, the lower the bias, but the higher the variance. We show robustness to change in bandwidth parameter. Along with our baseline estimate we also present estimates for a bandwidth of 1.0 and bandwidth of 15.0.

We report bootstrap standard errors. Using 6.87 as the bandwidth parameter, matching estimates show that the hourly wage gain from receiving a green card is about $5.93. If we use 1.0 as our bandwidth then the estimate changes to $6.23 per hour and it is $5.93 per hour when using 15.0 as our bandwidth parameter. All estimates are statistically significant, and are not sensitive to changes in bandwidth parameter in any substantive way. For reason discussed above we prefer our kernel matching estimates (although they are very close to one to one matching estimates in the present context). If we assume that an immigrant is working full time (2000 hours per year), then an hourly wage gain $5.93 implies a wage increase of $11,860 per year.

Next we report the estimates from cross-sectional matching for comparison purpose. As discussed above cross sectional matching may suffer from unobserved heterogeneity but it may be informative to compare to the estimates from cross sectional matching to the difference-in-difference matching reported above. Another reason behind this exercise is that in the data section we discussed that we lost a large part of our sample (about 60%) because we do not have their source-country wages which is necessary to implement difference-in-difference matching. Smith and Todd (2005) showed that matching estimates can be sensitive to sample choice; for example they showed that some of the results of Dehejia and Wabha (2002) were driven by their '*ad-hoc'* sample selection criteria. Even though we do not impose any 'ad-hoc' restrictions it still is informative to check the robustness of estimates to different samples. In a cross sectional matching we do not need the information on source country wages and hence we can have a bigger sample of 863 observations[13].

To that end we re-estimate the propensity scores using a slightly different specification (specification B) compared to the specification discussed above. In the new specification we drop log of source country wage (to increase our sample size as mentioned above) and the indicator for whether family of the respondent had more than average income (because this variable was violating the balancing condition in this larger sample) as regressors. The parameter estimates from the probit model for propensity score estimation are in column 2 of table 4.

Table 6 compares the Kernel matching estimates based on two samples. Panel A shows the estimates based on our baseline sample (333 observations) and panel B shows the estimates based on the expanded sample (863 observations). Cross sectional estimates can be obtained for both sample but the difference in difference matching estimates can only be obtained for the

[13] The summary statistic for this sample is presented in the last four columns of table 2.

baseline sample. Matching estimates are obtained for specification A (column 1 of table 4) and for specification B (column 2 of table 4). Cell 1 (row1, column1) presents the difference-in-difference estimate of hourly wage gain using specification A ($5.93) for comparison purpose. Cell 2 (row1, column2) shows the cross-sectional estimate of hourly wage gain associated with an employment based green card ($7.68). Cell 3 (row2, column1) presents the difference-in-difference estimate of hourly wage gain using specification B ($5.37). Cell 4 (row2, column2) shows the cross-sectional estimate of hourly wage gain using specification B ($6.97).

Since we are keeping the propensity score specification and the sample exactly same this exercise (row 1 vs. row 2) allows us to compare the estimates from the cross sectional matching to difference-in-difference matching. In both cases difference-in-difference estimates are smaller than cross-sectional estimates (though they are not statistically different from each other) suggesting that treatment group, perhaps not surprisingly, is 'more able' than the control group in unobservable ways. Comparing column (1) to column (2) shows the sensitivity of our estimates to changes in propensity score specification. Again, estimates are not very sensitive to such changes.

Panel B shows the cross-sectional estimate of hourly wage gain associated with an employment based green card using specification B for the larger sample (863 observations). Specification A cannot be used for this larger sample as it controls for source country wages. Estimates show that green card leads to an hourly wage gain of $8.32. The corresponding estimate is about $6.97 for the smaller sample (333 observations). Both of these two estimates are statistically significant but the larger sample size leads to a big reduction in standard error. Furthermore, we cannot reject the hypothesis that the estimated effects are same in two samples. This result gives us confidence that our relatively small sample size may not be an issue.

Finally, to asses whether the conditional independence assumption is satisfied in our sample (given our controls) we perform a specification (sometimes also referred to as falsification test) test as implemented by Smith and Todd (2005). In the present context this implies that we estimate the "green card effect" on the source country wages of the immigrants. If the conditional independence is satisfied then our estimated effect should be close to zero. Note that we can only implement a cross-sectional matching (as in Smith and Todd, 2005) because we observe one pre-treatment wage only once. Estimates show that the effect of green card on pre-treatment (i.e. source country) wage is $1.75 per hour (with a standard error of 2.93). Therefore the estimated effect is not statistically different from zero, as expected. The positive point estimate is most likely due to the unobserved heterogeneity (i.e. the treatment group is more able than the control group in unobservable ways). This evidence suggests that matching is valid is the present context and would produce unbiased estimates.

The various estimates using different types of matching as well as using different specification within a particular type of matching are all close to each other. The robustness checks increase our confidence in our estimates. Finally, our preferred estimate, using difference-in-difference kernel matching, gives us an hourly wage gain of $5.93, which translates into a wage gain of 25.4%, is very close to the estimate reported by Knadilov (2007) who reported a wage gain of 24.7% following the receipt of a green card.

Conclusion

In this paper we use data from New Immigrant Survey and implement matching estimators to estimate the wage gain experienced by immigrants after they become permanent resident. Estimates show that permanent residency leads to a gain of $11,860 per year. This result confirms the popular belief held by both the proponents and opponents of high skill temporary worker programs that H- 1B workers are paid less than native workers. In addition this paper quantifies the size of the gain.

This result shows that the current process of acquiring green card gives too much power to employers and hinders job mobility among high skill immigrants. One way to resolve this would be to increase the quota of green cards awarded to highly skilled immigrants. The current waiting period of 6 to 10 years is too long and increasing the quota would ensure greater mobility there-by applying upward pressure to the wages of H -1B workers. It is important to note that an increasing in the number of H-1B visa (like the one approved by the Congress in 2000) may not increase the mobility of temporary workers because even though the H-1B visa can be transferred from employer to employer (as long as the new employer files a petition for the immigrant) the green card application *cannot* be transferred. If an immigrant changes jobs after applying for a green card (but have not yet received the green card) then he must restart the entire process. Therefore increased cap of the H-1B quota does not really increase the mobility of H-1B workers who have applied for a green card. Such a policy, on the other hand, may be counter-productive. If employers pay temporary workers less and the supplies of such workers are increased then it might create additional downward pressure on the wages of comparable natives. On the other hand increasing the number of permanent residents would mean that employers have to pay immigrants similar to natives. This would also ensure that employers do not have any incentive to hire H- 1B workers if a suitable native worker is available for that job.

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Table 1. Number of H-1B petitions approved and number of employment based legal permanent residency awarded to temporary workers (FY 2000 – FY 2008).

Year	Number of H-1B Petitions Approved	Number of EB permanent residency awarded to adjustees.
2000	257,640	84,594
2001	331,206	137,318
2002	197,537	133,783
2003	217,340	52,151
2004	287,418	128,238
2005	267,131	219,999
2006	270,981	121,587
2007	281,444	133,099
2008	276,252	149,542

Source: U.S. Citizenship and Immigration Services (Annual report: H-1B Petitions for FY 2007 and FY 2008) and Department of Homeland Security and http://www.dhs.gov/files/statistics/publications/LPR08.shtm (Table 6); Accessed on 04/03/2010

Table 2. Summary statistics

Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Age	36.0	6.88	39.1	8.65	35.6	7.29	37.5	8.34
Family income far below avg.	0.019	0.138	0.013	0.113	0.012	0.109	0.029	0.168
Family income below avg.	0.145	0.354	0.130	0.337	0.120	0.325	0.132	0.338
Family income above avg.	0.203	0.404	0.278	0.449	0.224	0.417	0.265	0.442
Family income far above avg.	0.010	0.098	0.034	0.183	0.012	0.109	0.044	0.205
Female	0.553	0.499	0.200	0.400	0.524	0.500	0.252	0.435
Source country education	15.4	2.89	16.5	2.58	15.4	3.01	15.5	3.74
Source country college degree	0.349	0.479	0.426	0.495	0.336	0.473	0.371	0.483
Source country masters (or higher) degree	0.155	0.363	0.334	0.472	0.188	0.391	0.278	0.448
Source country experience	12.89	7.68	11.70	8.10	8.24	8.47	6.70	7.81
Professional job	0.417	0.495	0.752	0.432	0.204	0.403	0.327	0.469
Health job	0.398	0.491	0.047	0.213	0.188	0.391	0.022	0.149
Service job	0.048	0.215	0.047	0.213	0.02	0.140	0.017	0.132
Administrative job	0.097	0.297	0.086	0.282	0.052	0.222	0.047	0.212
Catholic	0.456	0.500	0.173	0.379	0.444	0.497	0.216	0.412
Orthodox Christian	0.019	0.138	0.069	0.254	0.04	0.196	0.057	0.232
Protestant	0.262	0.441	0.200	0.400	0.212	0.409	0.189	0.392
Jewish	0 .029	0.168	0.017	0.131	0.012	0.109	0.013	0.113
Buddhists	0.038	0.194	0.030	0.172	0.032	0.176	0.022	0.149
Hindu	0.058	0.235	0.286	0.453	0.096	0.295	0.267	0.443
No religion	0.116	0.322	0.156	0.364	0.140	0.347	0.158	0.365
Other religion	0.019	0.138	0.065	0.247	0.024	0.153	0.075	0.263
Source country hourly wage	18.13	21.67	16.19	27.21	-	-	-	-
Latin America	0.087	0.283	0.095	0.294	0.084	0.277	0.140	0.347
Europe	0.145	0.354	0.160	0.368	0.108	0.311	0.117	0.322
Africa	0.038	0.194	0.048	0.213	0.032	0.176	0.045	0.208
Married	0.708	0.456	0.808	0.394	0.66	0.474	0.773	0.419
Number of Observations	103		230		250		613	

Table 3. Estimate Using Difference in Difference in Means

	Source Country Wage (Before)	U.S. Wage (After)	Difference	Difference in Difference
Treated Group (New Arrivees)	18.13	29.67	11.54	4.48
Control Group (Adjustees)	16.38	23.44	7.06	

Table 4. Propensity score estimation results.

	DDM	CSM
Age	0.029	0.021
	(0.26)	(0.41)
Age sq.	-0.002	-0.001
	(1.22)	(1.00)
Family income far below avg.	1.267	-0.527
	(1.63)	(1.36)
Family income below avg.	-0.027	-0.138
	(0.10)	(0.92)
Family income above avg.	-0.332	-
	(1.49)	-
Family income far above avg.	-0.922	-0.550
	(1.45)	(1.76)
Female	0.761	0.356
	(3.39)**	(3.11)**
Source country college degree	-0.300	-0.004
	(1.26)	(0.03)
Source country masters (or higher)	-0.582	-0.083
	(2.18)*	(0.61)
Source country experience	0.081	0.036
	(3.69)**	(4.07)**
profjob1 Professional job	0.171	-0.414
	(0.39)	(2.92)**
healthjob1 Health job	1.334	0.711
	(2.63)**	(3.17)**
servicejob1 Service job	0.421	-0.095
	(0.73)	(0.26)
adminjob1 Administrative job	0.135	-0.327
	(0.27)	(1.37)
Catholic	0.395	0.841
	(0.79)	(3.09)**
Orthodox Christian	-0.989	0.294
	(1.61)	(0.90)
Protestant	0.072	0.557
	(0.15)	(2.03)*
Buddhists	0.332	0.561
	(0.51)	(1.47)
Hindu	-0.507	-0.056
	(0.97)	(0.20)
No religion	-0.153	0.363
	(0.30)	(1.29)
Other religion	-0.453	-0.045
	(0.63)	(0.10)
Log of source country wage	0.311	-
	(3.10)**	-
Lam	-0.102	-0.602
	(0.30)	(3.52)**
Europe	0.460	-0.205
	(1.67)	(1.27)
Africa	-0.246	-0.155
	(0.53)	(0.59)
Married	0.177	-0.139
	(0.73)	(1.16)
Constant	-2.016	-1.283
	(0.86)	(1.27)
Observations	333	863

Note: t-stat in parenthesis. * significant at 5%; ** significant at 1%

Table 5. Estimates using Difference-In-Difference Matching

	No Matching	Nearest Neighbor Matching			Kernel Matching		
	Mean	1	5	10	bw=1.0	bw=6.87	bw=15.0
Difference in Difference	4.48	5.90	6.42	9.67	6.23	5.93	5.93
	(3.73)	(5.11)	(6.22)	(5.96)	(3.26)*	(3.41)*	(3.16)*

Note: standard error in parenthesis. * significant at 10%

Table 6. Kernel Matching Estimates using Difference-In-Difference and Cross-Sectional Matching

Panel A		
	Specification A1	Specification A2
Diff-in-diff matching	5.93 (3.11)*	5.37 (2.97)*
Cross sectional matching	7.68 (3.87)**	6.97 (3.74)*
Sample size	333	333
Panel B		
Cross sectional matching	-	8.32 (1.68)***
Sample size	863	863

Note: standard error in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%

Appendix A

Table A1. Difference-in-Difference Nearest Neighbor Matching

	Single Nearest Neighbors		
	Before	After	Diff-in Diff
Treatment	17.96	30.90	
Control	16.99	24.03	
Diff	0.97	6.87	5.90
	Five Nearest Neighbors		
Treatment	17.96	30.90	
Control	21.22	27.75	
Diff	-3.27	3.15	6.42
	Ten Nearest Neighbors		
Treatment	17.96	30.90	
Control	22.47	25.73	
Diff	-4.51	5.16	9.67

Table A2. Difference in Difference Kernel Matching

	Before	After	Diff-in Diff
	bw=1.0		
Treatment	17.96	30.90	
Control	16.51	23.22	
Diff	1.45	7.68	6.23
	bw = 6.87		
Treatment	17.96	30.90	
Control	16.20	23.21	
Diff	1.75	7.68	5.93
	bw = 15.0		
Treatment	17.96	30.90	
Control	16.20	23.21	
Diff	1.75	7.68	5.93