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Three Essays on Human Capital Outcomes of Immigrants in the United States

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Three Essays on Human Capital Outcomes of Immigrants in the United States

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B.A., St. Lawrence University, 2008

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Abstract

Three Essays on Human Capital Outcomes of Immigrants in the United States

The chapters in this dissertation confer attention to the labor- market wage and health insurance outcomes of two specific immigrant groups in the United States. Specifically, I distinguish between immigrants who arrive in the U.S. as refugees and all other documented immigrants. Economic studies using relatively large samples of refugees have been few and far between. In the following chapters, I use an innovative method to identify refugees in the U.S. Census data. This distinction in immigrant type is important as the two groups differ considerably in their pre-migration conditions and the manner in which the U.S. government treats them upon arrival.

The 1996 Welfare reforms in the U.S. brought some important changes to the welfare eligibility laws with respect to immigration status. All immigrants entering the country after August 1996 are barred from welfare until naturalization. The only exceptions to this rule are refugee immigrants. The chapters here exploit this difference in welfare eligibility for the two groups and the variability in welfare generosity between states to explain the differences in outcomes for refugees and non-refugee immigrants.

I find that among the most recent and youngest immigrants, wages for refugees increase at faster rates than those for non-refugee immigrants. This disparity is greater in the period when only refugees qualified for welfare. With respect to welfare participation itself, I find high rates of enrollment among refugees in programs like Medicaid. Participation is predominantly driven by local economic conditions. As such, during improved economic times, refugees are likely to opt out of Medicaid and enroll in private health insurance. In the final chapter, my co-author and I find that among recent immigrants, refugee wages tend to be higher in the lower tails and lower in the upper tails of the wage distributions. Favorable wage differentials for refugees in the lower tails, we find, arise from differences in returns to human capital characteristics for the two immigrant groups and not composition effects. Consistent with this finding, we also notice that the wage differentials are more favorable to refugees in states with more generous welfare programs and where per head expenditures on refugees are higher.

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CHAPTER ONE

FROM REFUGE TO RICHES? AN ANALYSIS OF REFUGEES' WAGE ASSIMILATION IN THE UNITED STATES

Abstract

Unlike non-refugee immigrants — given that refugees may be fleeing from political, social, racial, ethnic, or religious persecution — they are not expected to be economically independent upon arrival. Additionally, refugees are eligible for federal and state welfare programs. In this paper, I examine the extent to which differences between the two immigrant groups translate into differences in their wage assimilation over time. Using the decennial census and pooled American Community Survey (ACS), I analyze synthetic cohorts of immigrants during 1990 to 2000 and again from 2000 to 2010. Among the youngest and most recent immigrants, refugees earn lower wages on arrival but make larger gains over time. From 1990 to 2000, an increase in duration of 10 years increases wages for refugees and non-refugee immigrant cohorts by 15 and 7.7 percent respectively. In the period between 2000 and 2010, the gains for young and recent refugees and non-refugee immigrants are 22.7 and 5.7 percent respectively. In contrast, across both decades, duration effects for the oldest cohorts are negative among both immigrant groups — irrespective of their length of stay in the U.S. Differences in se- lection into the labor force explains a portion of the difference in assimilation rates between the two immigrant groups, but only for the 1990–2000 sample.

1 Introduction

Refugees comprise a distinct group of the U.S. population that has important differences from the larger non-refugee immigrant community. One of these differences is refugees' inability, unlike other documented immigrants, to return to their countries of origin.[1] Refugees also may not migrate to the United States by choice or with the same amount of preparation; ergo, they are not expected to be economically independent upon arrival. For these reasons, both the U.S. federal and state governments provide refugees with support that is not always available to the wider non-refugee immigrant population. Regarding federal support, refugees share the same eligibility criteria as natives with respect to welfare programs and, as such, benefit from state and federal resources. All other documented immigrants entering the U.S. after August 1996, (referred to in this paper as non-refugee immigrants), are effectively barred from welfare (Fix and Passel, 2002).[2] Whether refugees continue to depend on state and federal resources for extended periods of time or begin to assimilate and con- tribute to American society, by paying taxes for example, is a question that remains largely unanswered.

This paper contributes to the larger literature on immigrants' economic assimilation in the United States by distinguishing between changes in wages for refugee and non-refugee immigrants relative to natives. Wage assimilation, as defined here and in Borjas (1985 and 1995), Klopfenstein (1998), Friedberg (1992), and others, refers to whether or not immigrants with increasing duration in the U.S. reach wage parity with the average natives in the country. Others including, LaLonde and Topel (1992) and Tiagi (2012) differ in their definition of assimilation

[1]Refugee is defined as someone who "owing to a well-founded fear of being persecuted for reasons of race, religion, nationality, membership of a particular social group or political opinion, is outside the country of his nationality, and is unable to, or owing to such fear, is unwilling to avail himself of the protection of that country" (1951 Refugee Convention, UNHCR)

[2] Also referred to as the 1996 Personal Responsibility and Work Opportunity Reconciliation Act(PRWORA), the welfare reforms decentralized the former federal entitlement programs with Temporary Aid for Needy Families (TANF), a block grant distributed and regulated at the state level. Federal TANF regulations included new work requirements and time limits on cash assistance. The overarching nature of reforms under PRWORA included provisions for Supplemental Security Income (SSI) eligibility, child sup- port enforcement, child protection, childcare, marriage promotion, and abstinence education. (Takahashi, unpublished manuscript)

because of their choice of the base group to whom immigrants are compared. The latter cohort typically defines the native base as a specific sub-group of the U.S.-born population or those who share the same ethnic backgrounds as the immigrants.

While there is a plethora of existing research on the extent to which immigrants economically assimilate overtime (Chiswick, 1978 and 1982; Borjas 1985 and 1995; LaLonde and Topel, 1992; Tiagi, 2012 etc.), relatively less is known about the long term economic outcomes of refugee immigrants specifically, in the United States. Previous works focusing exclusively on refugees (Potocky-Tripodi 2001, 2004) have found that demographic characteristics such as, education, gender, and household composition are the most significant predictors of economic status. The handful of comparative studies on refugees and non-refugee immigrants suggest systematic differences between refugee and non-refugee immigrants' decisions with respect to human capital investments (Khan, 1997; Cortes, 2004), choice of location in the U.S. (Zavodny, 1999), and use of welfare (Bollinger and Hagstrom, 2008; Giri, 2013). Given these fundamental differences, it is only natural to question the presence of potential differences in wage assimilation between the two immigrant groups.

Using the decennial census from the years 1990 and 2000 and the three-year 2011 American Community Survey (ACS) sample, I construct synthetic cohorts of immigrants based on their age and timing of entry into the U.S. I analyze two different samples; the first, spanning the years 1990 to 2000, includes the pre-welfare reform period while the second sample spans the period from 2000 to 2010 and consists entirely of the post-reform period. Census data do not include an identifier for refugee immigrants; however, I accomplish the task of differentiating refugees from non-refugee immigrants by using an imputation method motivated by previous work from Bollinger and Hagstrom (2008 and 2011). The use of a double cohort analysis (age groups embedded within years of migration cohorts) allows for the identification of the effects of duration and an immigrant's age at arrival on his future wages. By comparing their respective duration effects relative to the same base native group, I attempt to determine which of the two immigrant groups assimilates more successfully. Additionally, I investigate selection into the labor force as a possible explanation for the differences in assimilation rates between the two immigrant groups.

Analyzing the wages of migration cohorts that entered the United States between 1950 and 2000 while controlling for local economic conditions, both within and across three census periods, I find that relative to non-refugee immigrants, the most recently-arrived refugees begin with lower wages on average but make larger gains over time. Refugees that enter the country at a younger age earn higher wages as they grow older and with increasing duration in the U.S. The experience of older refugees, however, is less favorable. From 1990 to 2000, a duration of ten years increases wages for the youngest and most recent (those arriving between 1985-1990) refugees and non-refugee immigrants by 15 and 7.7 percent respectively. Similarly, in the period between 2000 to 2010, duration effects account for an increase in wages among the youngest and most recent (those arriving between 1995-2000) refugees and non-refugee immigrants by 22.7 and 5.7 percent respectively. In contrast, duration effects reduce wages for the oldest among both immigrant groups irrespective of their length of stay in the U.S. These negative duration effects among older immigrants are larger in magnitude for refugees than non-refugee immigrants but only in the 1990-2000 sample.

I also investigate the extent to which the above deferential in assimilation rates between the two immigrant groups can be attributed to differences in their respective labor force participation decisions. Across both decades, refugees are more likely to participate in the labor force over time. The trend is opposite for non-refugee immigrants in the 1990-2000 time period, during which their labor force participation decreases over time. Results from employment correction models suggest that there is selection in the samples. For both immigrant groups, the sample of those participating in the labor force and hence earning wages differs in observable characteristics from those not in the labor force. The resulting bias from focusing only on those who participate in the labor force, however, does not offset the main results that young and recently-arrived refugees assimilate at higher rates than non-refugee immigrants. In the 1990-2000 sample, differences in selection into the labor force do explain a significant portion of the relative difference in assimilation rates between the youngest and most recent refugees and non-refugee immigrants. For the 2000-2010 sample, in which labor force participation for the two immigrant groups appears to be largely similar, a substantial gap in the assimilation rates of refugees and non-refugee immigrants persists. Given that I find a greater disparity in assimilation rates between the two immigrant groups in the post reform period, during which only refugees received welfare support, these results also provide suggestive evidence of the importance of welfare programs in the assimilation process of immigrants.

2 Motivation and Conceptual Framework

There are three broad reasons why an analysis of the wage assimilation of refugees is important and contributes to the literature. First, the study has both domestic and international policy implications. Second, the study of refugees' wages serves to fill a current void in the literature with respect to long-term economic outcomes of refugees and how and why these may vary from those of the non-refugee immigrants. Third, the analysis of refugees specifically overcomes one issue of selection bias that plagues most analyses of non-refugee immigrants.

Since 1975, according to the US Department of State, over 3 million refugees have been resettled in the US. This is more than the total number of refugees resettled in all other countries combined (Migration Policy Institute, 2004). The sheer number of refugees resettled in the U.S. makes their economic assimilation process, a matter of global interest. Refugees' assimilation or lack thereof, is of further interest given that the funds used for resettlement can be used to help many more refugees still in the source countries. Long-term economic outcomes of refugees in the U.S. helps to either validate or raise questions about the refugee resettlement program.

In the domestic context, even though refugees represent only about one percent of the total U.S. population, the question of their economic assimilation process is equally pressing. Among other reasons, the domestic concerns are likely to stem from possible effects on natives and the relative spending on this one percent of the country's population. In the 2009 fiscal year, the Office of Refugee Resettlement (ORR) received a total of $715.4 million to assist refugees (Report to Congress- Office of Refugee Resettlement, 2009). Additionally, the inflow of refugees into the local labor markets may strain state funds and also have impacts on the labor outcomes of natives. Given the large per-head spending on refugees in the U.S. and a continuing yearly influx of new refugees, a natural line of questioning refers to whether refugees continue to draw on federal and state resources over time or if they assimilate and contribute by earning higher wages and paying larger taxes. The refugee experience may have policy implications with respect to the assimilation of the larger non-refugee immigrant population in the U.S. More recently Tiagi (2012) and others before (Borjas 1985 & 1995; Singh and Augustine 1996) have found that, even after 20 years, wages of non-refugee immigrants in general and those of specific regions fail to catch up with earning levels of natives. Due to the fact that refugees receive federal assistance that non-refugee immigrants do not, with all other factors remaining constant, a different outcome for the former suggests similar policies might help with the assimilation process of non-refugee immigrants in the U.S. as well.

Explaining the second reason why a separate analysis of refugees' wage assimilation is warranted requires a brief review of the existing state of the literature. The large body of existing work on immigrants' wage assimilation focuses on immigrants in general or immigrants from specific countries and regions. The literature, although extensive, is far from reaching any consensus (Chiswick 1986; Duleep and Regets 2002; LaLonde and Topel 1992).

The majority of the papers, however, point to a lack of assimilation, especially among newer immigrants. The question of whether or not immigrants assimilate is also dependent on the base group to whom immigrants are compared; hence, results in a number of papers vary accordingly. Chiswick (1978) is consistently cited as the beginning of the debate on the extent to which immigrants successfully assimilate over time in the U.S. His cross-sectional analysis, based on the 1970 U.S. census, finds that immigrant wages are strongly and positively correlated with their increased duration in the U.S. and, posits that immigrants' wages rapidly increase with their time in the U.S. The cross-sectional regression analyses, however, suffer from a number of temporal biases. Immigration is a dynamic process; new immigrants at various ages continue to enter the country at different points in time and the ones already in the country get older and gain in experience. These temporal aspects are not captured separately in a cross-section.

By using two cross-sections to analyze the effect of duration in the U.S. on wages, Borjas (1985 & 1995) decomposes cross-sectional growth in wages into the sum of two parts. The first of these two parts is the actual assimilation or duration effect experienced by a given cohort (based on time of entry into the U.S.) over a period of time (within cohort). The second part is a temporal bias resulting from the wage growth between cohorts at two different periods in time but with the same amount of time spent in the U.S. (across cohort). The author provides evidence that the cross-sectional estimates are biased upward due to the increasing positive value of the second term in the decomposition. This implies a decreasing quality of more recent immigrants who, unlike previous cohorts of more skilled migrants, are unlikely to assimilate. More recent work by Borjas and Friedberg (2009), to the contrary, show a possible reversal in this trend of decreasing quality among immigrants arriving between 1995 and 2000.

While Cortes (2004) and Bollinger and Hagstrom (2011) analyze changes in refugees' wages and poverty rates respectively, they do not specifically tackle the question of refugees' wage assimilation relative to natives. Cortes (2004) compares changes in wages between refugees and non-refugee immigrants that entered in the period between 1975-80 and finds that refugees earn higher wages over time via greater investments in human capital. Similarly Bollinger and

Hagstrom (2011) find that poverty rates, particularly those for refugees, converge to that of natives over time. Both findings suggest that refugees have important differences from non-refugee immigrants and that the story of decreasing skills may not necessarily apply to or fully explain the experiences of refugees resettled in the U.S. Additionally, neither of the two aforementioned studies identifies the effects of duration or assimilation after accounting for the aging process. Other existing work on refugees (Potocky-Tripodi 2001, 2003) focus solely on determinants of their economic status. To my knowledge, no analyses have been conducted on refugees' wage assimilation specifically.

There are several reasons to expect different wage assimilation outcomes for refugees in comparison to non-refugee immigrants. Foremost, unlike non-refugee immigrants, refugees receive federal support during their early years in the U.S. This initial support structure may impact refugees' long-term economic outcomes. For instance, medical assistance in the short and long run can lead to better health outcomes which in turn may result in higher wages. Refugees also benefit from cash assistance and case workers who, among other things, help them with locating jobs. Non-refugee immigrants, in contrast, may rely on their social networks for initial support whereas it is unlikely that refugees have pre-existing networks when they first arrive in the country. The presence of networks for immigrants and their absence for refugees likely impacts their choice of location and subsequently their wages. Non-refugee immigrants may be more likely to locate themselves in their respective ethnic enclaves while refugees are likely to be placed in areas that maximize their chances of finding work. The mere fact that refugees rarely have the option of returning to their home countries, may also impact their choices of investments in human capital over time. These differences in human capital investment choices may influence wage outcomes for the two groups. Cortes (2004) provides evidence that the two immigrant groups differ in their investments in human capital over time and this accounts for their subsequently different changes in wages. There have been no studies, however, that look at the two immigrant groups' labor force participation decisions as additional reasons for the difference in wage outcomes.

Finally, a third reason for studying refugees specifically involves an issue of generalizability in the econometric sense. Most of the above studies, on long-term economic outcomes of immigrants, fail to take account of the emigration process. Non-refugee immigrants are free to migrate back to their countries of origin or to other destinations. If emigration patterns are random, then they pose no threat to the validity of results; there is, however, evidence that this may not be the case. Existing works on the topic conclude that emigration rates are lower for immigrant groups originating from countries that are poor, undemocratic, and located far from the U.S. (Jaso and Rose 1982; Borjas and Bratsberg 1996). Borjas and Brastberg (1996) also provide evidence that, given a positively selected inflow of immigrants, it is usually the less successful that are more likely to emigrate. Over time it is plausible that we witness the experience of only those immigrants who are relatively more successful, originate from certain specific regions, and choose to remain in the U.S.. As such, any statistical relationship between changes in wages over time is likely to be biased upwards with respect to the non-refugee immigrant population. The situation with refugees is very different as they — unlike non-refugee immigrants — may not want to or do not have the choice of returning to their countries of birth and therefore their sample at any given time is more likely to represent their population in the U.S.. For this reason, any statistical relationships between changes in wages over time are more readily generalizable to the refugee population in the U.S.

3 Data

3.1 Refugees in the U.S.

The ideal dataset to study the effects of aging and duration on refugees' wage assimilation would include individual observations of refugees and natives spanning multiple decades.

Unfortunately, no such panels exist. The New Immigrant Survey (NIS), which may be the closest in comparison to the ideal dataset, is a panel that includes only immigrants, of which refugees constitute a very small sample. Given that assimilation is a long-term phenomenon, the NIS panel also does not cover a substantial-enough time span to answer the question at hand. One of the key data challenges to determining the assimilation patterns of refugees, therefore, involves distinguishing refugees in large, national samples of immigrants and natives.

I use annual cross-sections, years 1972–2000, available through the Immigration and Naturalization Services (INS) to identify refugees from a sample of immigrants. I replicate the process outlined in Bollinger and Hagstrom (2008) to estimate the probability that a given immigrant in the census dataset is a refugee. I then use these probabilities to impute refugee status to immigrants in the dataset. Each one of the 27, annual INS datasets include immigrants admitted to the country in that year as well as those adjusting their status but who were admitted any time in the past. By combining the 27 different INS datasets, I construct a universe of all immigrants admitted to the U.S. for permanent residency by their year of entry. Next, for each year, gender, and country group with sufficient observations, I estimate individual probit regressions with refugee status as the dependent variable and age and square of age as the independent variables.[3] Out of a total of 15,507 country, time, and gender groups, 2,535 of them yield estimates for slopes and intercepts.[4] Excluding less than one percent of the outliers, the relationship between age and refugee status is typically negative. Groups that do not have a valid slope or intercept are because there are either no refugees from that country in the given period or because all of the immigrants are refugees. In order to account for countries from which all or too few immigrants came as refugees, I calculate — for each year, gender, and country grouping —

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[3] Probit regressions were estimated for country, year and gender groups with at least four refugees and four non-refugee immigrants.

[4] The number of country, time and gender groups differ from those in Bollinger and Hagstrom (2008 & 2011). This is because the Current Population Survey variable for year of immigration identifies 16 different periods of entry, prior to 1950, 1950-1959,1960-1964, 1965-1969, 1970-1974, 1975-1979, 1980-1981,
1982-1983,. . . 1996-1997, 1998-2001. The IPUMs data on the other hand provides exact year of entry.

individual ratios of refugees to total immigrants. There are also immigrants in both the INS and census datasets for whom country of origin is missing but the region is known. For these individuals, I calculate region- specific estimates. Finally, the slope, intercept, and refugees to immigrants ratios are merged with the census dataset by immigrants' gender, time period of entry, and country of origin. Estimates of age at entry for each immigrant in the census along with their corresponding slope and intercepts are used to calculate predicted probabilities of refugee status. For those immigrants belonging to year, gender, and country groups with all, none, or too few refugees, the ratio of refugees to immigrants for the group is used as the probability of being a refugee. I use these predicted probabilities to impute refugee status. Those immigrants with predicted probabilities equal to or greater than 0.5 are imputed as refugees.[5] The mean probability for those imputed as refugees with this imputation technique is 0.82.

The resulting sample of refugees in the census datasets looks vastly different from what previous methods of identifying refugees would have implied. Within the two samples, refugees have arrived from 46 different countries and in varying numbers across different migration cohorts (1960s-1990s). This exemplifies the larger measurement errors associated with previous methods of identifying refugees. Excluding the works of Bollinger and Hagstrom (2008 and 2011), previous related research has resorted to imputing refugee status to all immigrants from the most refugee-prone countries.[6] There are two types of measurement errors associated with any imputation method for identifying refugees; one, non-refugees may be incorrectly imputed as refugees and two, some refugees may be imputed as non-refugee immigrants. Both types of measurement errors are decreased in the imputation method used in the current analysis. Said imputation method identifies an additional 33 countries from which refugees have come.

[5] Giri (2013) also implements this method for defining a refugee.

[6] Borjas (2002) imputes all immigrants from the following countries regardless of their time of or age at entry as refugees: Afghanistan, Cuba, the Soviet Union, Ethiopia, Cambodia, Laos, Bulgaria, Czechoslovakia, Hungary, Poland, Romania, Thailand, and Vietnam. Cortes (2004) and Potocky-Tripodi (2001, 2004) impute all immigrants from a subset of the above countries as refugees.

Furthermore, this imputation method allows for distinguishing refugees from non-refugee immigrants originating from the same countries.

Table 1.1 and Table 1.2 list the number of refugees by country of origin and timing of arrival present in the two samples. The tables also include non-refugee immigrants who have arrived from the same countries. Refugees from Cuba, the former Soviet Union, Cambodia, Laos and Vietnam constitute the majority of the refugees in the sample. The sample also includes a sizeable refugee population from Poland, Romania, Ukraine, Bosnia, Afghanistan, Iraq, Ethiopia, and Somalia. Both tables also illustrate that many non-refugee immigrants have also arrived from the same refugee sending countries. In Table 1.1 for example, it is interesting to note that there are very few if any refugees from Thailand, which is considered (by previous imputation methods) to be a refugee prone country. There are, however, many more non-refugee significant immigrants from Thailand. Table 1.2, shows that it is only in the 1980s that there is a refugee population from Thailand. In fact, Table 1.1 and Table 1.2 indicate that, there have been large numbers of non-refugee immigrants originating from all of the 13 refugee-prone countries identified by earlier papers. Given that refugees and non-refugees have arrived from the same countries, imputing all immigrants from a selection of countries as refugees and none from others, highlights the potentials for serious measurement errors. Additionally, having both immigrant types originating from the same countries helps strengthen the argument that the reported differences are refugee and non-refugee effects and not country specific effects.

The breakdown of the refugee sample by decade long migration cohort is also consistent with the history of refugee resettlement and related legislation in the U.S. Prior to the Refugee Act of 1980, the U.S.'s approach to refugee resettlement was largely ad hoc and on an as-needed basis. Refugees during this time were exclusively defined in an anticommunist context (Huyck and Bouvier, 1983). The earliest refugees admitted to the U.S., as such, were those displaced by World War II and others fleeing communist regimes. In the 1960s, this consisted of great numbers fleeing from Fidel Castro’s Cuba (Zucker, 1983). Subsequently in both the samples, refugees entering in the 1960s consist almost entirely of Cubans and a handful of those from the former USSR. Refugees from these regions continued to be resettled in the following decades but were accompanied by refugees from additional countries. The 1970’s saw boats full of Indo-Chinese refugees from Vietnam, Cambodia and Laos entering the U.S., indiscreetly earning this group the name "boat people" (Huyck and Bouvier, 1983). The refugee samples in Table 1.1 and Table 1.2, from the 1970s are also consistent with this historical trend.

It was only after the 1980 Refugee Act that the U.S. revised its definition of refugees and adopted the formal definition of refugees as commissioned by the UNHCR. The 1980’s act has made it possible for refugees worldwide to apply for resettlement in the U.S.. Refugee resettlement programs have since been consolidated and made formally available to all incoming refugees (Kennedy, 1981). The samples of refugees entering in the 1980’s and 90’s reflects the changing demographics. New refugees have arrived from additional regions, including Africa and other parts of Asia and Europe. Imputing refugee status by country of origin alone is even more restrictive in the post 1980 period. In the following section, I describe how the sample of refugees compare to non-refugee immigrants and natives in the dataset.

3.2 Data Description

The empirical analysis in this paper is based on data from the decennial census, years 1990 and 2000, and the pooled three-year 2011 ACS sample (which from here onwards is referred to as the 2010 sample). I use the Integrated Public Use Microdata (IPUM), five percent samples, for the two decennial census years. Using data from the three time periods, I construct two samples: one from 1990 to 2000 and a second from 2000 to 2010. In the present analysis I focus only on males who are 18 and above and below 58 years of age, in the labor force, work for wages, and for whom hourly wages can be calculated.[7] Focusing only on males helps to avoid the complexities resulting from unobserved factors that may be involved in women's labor force participation decisions. A relatively much larger percentage of males participate in the labor force, and as such most of the previous works (Borjas 1985 and others) have also focused solely on male wage outcomes. Results are not sensitive to outliers in estimated hourly wages, so I use the entire distribution of wages in the analysis below.[8]

The 1990-2000 sample includes 4,081,658 observations, of these observations, 23,206 are refugees, 334,718 are non-refugee immigrants, and the remaining 3,723,734 are natives. Similarly, the 2000-2010 sample includes 34,887 refugees, 424,817 non-refugee immigrants, and 2,877,521 natives. For the purpose of this study, I define natives as those who are born in the U.S. or born in a foreign country but to American parents. Immigrants include both refugees and non-refugee immigrants and are defined as those born in foreign countries. Refugees are a distinct subset of immigrants and mutually exclusive from non-refugee immigrants.

I report summary statistics by immigration status on the key characteristics analyzed in the paper in Table 1.3 and Table 1.4. On average and across both samples, natives earn the highest wages, followed by refugees and non-refugee immigrants. Mean wages are lower in the 2000-2010 sample compared to the 1990-2000 sample for all three groups. Similar to the ranking in hourly wages across both samples, homeownership is highest among natives, followed by refugees and then non-refugee immigrants. Homeownership is considered an important symbol of

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[7] In the paper, I follow three age cohorts as they age by 10 years across two time periods. These age cohorts include 18 to 27, 28 to 37 and 38 to 47 years of age in the first time period. In the second time period the same age cohorts are then, 28 to 37, 38 to 47 and 48 to 57 years of age respectively. I use 18 years as the lower bound to restrict the analysis to adults. The upper bound of 57 ensures that the sample of older cohorts is not confounded by the absence of retirees.
Estimates for hourly wages are calculated by dividing yearly earnings by the product of total weeks and usual hours of work per week

[8] In line with previous work on wage gaps, such as Lemieux (2006) and Hotchkiss and Shiferaw (2011), after converting nominal wages to real (year 2000) dollars, I check the sensitivity of results by dropping hourly wages below one and above a 1,000 dollars. This results in the removal of less than one percent on either side of the tail ends of the hourly wage distribution.

residential assimilation (Myers et al. 1999), and these statistics suggest that refugees are closer to ownership rates of natives, than non-refugee immigrants. Proficiency in English is considered an important determinant of wages (Cortes, 2004), and its variation may help to explain any wage differentials between refugee and non-refugee immigrants. In comparison to refugees, more non-refugee immigrants have low proficiency in English. The sample means suggest that between 1990 and 2000 non-refugee immigrants entered the U.S. at slightly younger ages than refugees, but the trend is reversed in the years between 2000 and 2010. Non-refugee immigrants are also most likely to have less than a high school level of education. Refugees, although less educated than natives on average, tend to be more educated than non-refugee immigrants.

The summary statistics also include four different characteristics of local labor markets. Following Autor and Dorn (2008) and Hotchkiss and Shiferaw (2011), local labor markets in the 1990-2000 sample are defined by commuting zones (CZ). These are 741 clusters of counties with strong commuting ties and were originally constructed by Tolbert and Sizer (1996) using the 1990 Census data on journey-to-work county commuting flows. As pointed out by the previous authors, CZs are preferred over Metropolitan Statistical Areas (MSA) and counties as the former excludes those not living in MSAs, and the latter reflect artificial geographic boundaries. CZs are constructed from the 1990 and 2000 IPUMs five percent sample, including only 18 to 64 year olds.[9] Each individual in the sample used for the present analysis is assigned one of these 741 CZs. The four labor market variables are then constructed from individual data over each CZ. The first of the three CZ variables is the local unemployment rate which is simply the mean of the individual indicator variable for unemployment over a given CZ. Local female labor force participation is the ratio of females in a given CZ who participate in the labor force and the total number of 16-24 year old females in the CZ. Similarly, local immigrant population measures the percent of the CZ population that consists of immigrants and while local mobility of population

[9] Details on the construction of CZs are explained in Hotchkiss and Shiferaw (2011).

captures the percent of the CZ population that reported having moved from a different state or country in the last five years. These CZ level variables are expected to capture variations in local labor conditions that most likely also impact local wages. Since CZs are not identified in the ACS sample, for the 2000-2010 sample I use counties to approximate local economic conditions. I construct four variables analogous to the above CZ variables for the 2000-2010 sample using counties.

In Figures 1-5, I graph mean wages by migration cohorts and age groups. These raw statistics on their own reveal the importance of immigrants' age at entry and duration of stay in the U.S.. The graphs also reveal the obvious differences in levels and changes in wages between refugees and non-refugee immigrants. The graphs for refugees show a discernible pattern; excluding the most recent migration cohorts in the two samples (the first graph in Figures 3 and 5), the youngest refugee cohorts, regardless of their time of entry into the U.S., earn equal or higher wages than natives. As shown in the second and third graphs in Figure 1.1 among the middle and oldest age cohort, only those refugees who entered the country before 1980 earn wages equal to or above those of natives. In Figure 1.1, mean wages among the youngest non-refugee immigrants that arrived before 1980 are greater than those for natives. For all other non-refugee immigrants, irrespective of their age or duration in the U.S., mean wages in the Figures 2-5 are lower than those for natives. In comparing the two immigrant groups, I find that among the youngest refugees consistently outperform non-refugee immigrants. Across Figures 1-5 with the exception of the refugees that arrived before 1980, older non-refugees outperform their refugee counterparts.

It should be noted that Figures 1-5 refer to unadjusted means and do not control for any of the relevant characteristics. Furthermore, the source of change in mean wages for immigrants from 1990 to 2010 cannot be inferred from these raw statistics alone. Across both samples, all the birth cohorts aged by 10 years, lived in the U.S. for an additional duration of 10 years, and there also could have been structural changes from 1990 to 2010. In other words, there are three distinct factors that could potentially have affected immigrants' wages across the three periods: aging, duration and period effects. The duration effect net of aging and period effect is, in fact, the degree of assimilation achieved by refugees and/or non-refugee immigrants. These three factors, however, are linear combinations of each other and perfectly collinear, making their identification a tricky ordeal. Assumptions are made in the methods section of the paper to identify duration and, hence, assimilation, net of aging, and period effects.

4 Methods

In the absence of panel data at the individual level, previous works on related topic (Borjas 1985 and 1995, Klopfenstein 1998, Cortes 2004, Tiagi 2012 and others) have typically resorted to analyzing synthetic cohorts over time. With the exception of Myers et al. (1998) and Klopfenstein (1998), all the above studies construct cohorts based solely on the year of entry into the U.S. In their analysis of assimilation based on rates of homeownership, Myers et al. (1998) innovate the double cohort method, which involves embedding age cohorts within migration cohorts and allows for the identification of both duration and aging effects. The estimation of natives' and immigrants' wage function differ in an important way in that the latter includes an assimilation or duration factor absent in the former. On the one hand, within time related factors, wages for immigrants can be understood as a function of aging, duration, and period effects. Natives, on the other hand, are affected by aging and period effects but not by duration. The three time related factors are linear combinations of each other which means, without further assumptions, they cannot be individually identified in a model. As pointed out by Klopfenstein (1998), the double cohort method recognizes the above dynamics and makes two important assumptions:

$$Age = Period\ Birth\ Cohort$$

$$Duration = Period\ Migration\ Cohort$$

For identification, the aging effects for natives are set equivalent to that of immigrants. These assumptions are not unreasonably stringent given that we accept natives as the control group towards whom the immigrants assimilate. Following the double cohort method outlined in Myers et al. (1998), I estimate the following regression equation separately for both samples:[10]

$$\ln(w_i) = \beta_0 + \beta_m MC_{mi} + \alpha_b BC_{bi} + \gamma_{yr} Year + \gamma_{byr} BC_{bi} * Year + \gamma_{myr} MC_{mi} * Year + \gamma_{bmyr} BC_{bi} * MC_{mi} * Year + \boldsymbol{X\gamma} + \varepsilon_i \quad (1)$$

where $\ln(w_i)$ is the logged wage of individual i. In the 1990-2000 sample, BC indexed by the letter b stands for a specific birth cohort, coded in 1990 as 18-27, 28-37 and 38-47 year-olds. In 2000, each cohort is then 10 years older. Similarly, in the 2000-2010 sample, 18-27, 28-37 and 38-47 year-olds are first identified in the year 2000 and followed in 2010 at which time each cohort has aged by 10 years.[11] M C indexed by the letter m represents natives and specific migration cohorts of immigrants based on their time of entry into the U.S. With natives as the base group (MC has value 0 for natives), the remaining values of the M C variable indicate immigrants that arrived in the U.S. before 1980, those arriving between 1980 and 1984, 1985-1990, 1991-94, and 1995-2000. The first sample includes only the first three migration cohorts while the second sample includes all five.[12] This ensures that each cohort in each sample is observed twice. In equation 1 above, Year is an indicator variable for the year 2000 in the first sample and 2010 in the second. $\boldsymbol{X}$ is a vector of controls including highest level of education completed, married with spouse present, interactions of immigrant status and region of origin and

[10] The models presented here and in Myers et al. (1998) are both unsaturated. The key difference, however, is that here the models include the high level interaction M C* BC* Year and not the M C*BC term. Myers et al. (1998) do the opposite. Their omission criterion is based on goodness of model Fit. The omission of the M C*BC term here follows directly from the assumption that aging effects are assumed to be the same for immigrants and natives.

[11] Alternatively, one could also pool all three years into a single sample and analyze the migration and birth cohorts for 10 and 20 years. Given the double cohort methodology used here, I would only be able to study the first three migration cohorts. Also, with the oldest age cohorts, a duration of twenty years would include potential retirees. Finally, the two sample approach present here, allows me to examine the two immigrant groups, once when both are eligible for welfare and again when only refugees are eligible.

[12] For this reason, the sample sizes of natives, refugees, and non-refugee immigrants analyzed, both in the descriptive and regression sections, are lower than their respective totals in all the Census years.

local CZ or county level variables. For each sample, the regression equation above is estimated twice, once with natives and refugees and a second time with non-refugee immigrants and natives. Given that wages of both immigrant groups are compared to that of the same natives, the coefficient estimates across the two regression equations can also be compared directly to assess the relative performances of the two different immigrant groups. For all the regressions in the 1990-2000 and 2000-2010 samples, I cluster standard errors at the CZ and county levels, respectively.

In general, the estimates from equation 1 can be understood as the relative difference in mean wages between immigrants with varying duration of stay in the U.S. and age at entry relative to a base native group. Estimates of coefficients on the interactions between birth cohort and year and migration cohort and year represent aging and duration effects respectively. Estimate $\hat{\beta}_m$ is the effect of belonging to migration cohort m for the base year relative to natives. $\hat{\alpha}_b$ is the age effect for the estimated percent difference in wages between the youngest and older birth cohort b. $\hat{\gamma}_{yr}$ represents the period effect and captures structural changes between the two Census periods; subscript yr refers to the year 2000 and 2010 in the 1990-2000 and 2000-2010 samples respectively. $\hat{\gamma}_{byr}$ is the aging effect for birth cohort b, for natives and immigrants alike. $\hat{\gamma}_{myr}$ represents duration effects for the youngest migration cohorts. A positive and statistically-significant duration effect denotes assimilation for the given immigrant cohort. Finally, $\hat{\gamma}_{bmyr}$ is the additional duration effect for older immigrants above that of the youngest immigrants. The sum of $\hat{\gamma}_{myr}$ and $\hat{\gamma}_{bmyr}$ is the total duration effect for the older migration cohorts. Again, a positive and significant sum denotes assimilation for the given older migration cohorts. Conversely, a negative and statistically significant total duration effect indicates a lack of assimilation. The interpretation of the coefficients remains the same across both samples with the exception of the reference to years.

5. Results

I report regression estimates: $\hat{\beta}_m$, $\hat{\alpha}_b$, $\hat{\gamma}_{yr}$, $\hat{\gamma}_{byr}$, $\hat{\gamma}_{myr}$, and $\hat{\gamma}_{bmyr}$ and additional total duration effects from four separate regressions in Table 1.5 - Table 1.8. Table 1.5 and Table 1.6 pertain to the 1990-2000 sample and the estimates in Table 1.7 and Table 1.8 are from the 2000-2010 sample. In this section, I focus on the main results from models 1A and 1C. These are Ordinary Least Square (OLS) estimates from equation 1. The first column of estimates in Table 1.5 is from using only natives and non-refugee immigrants; the second column of estimates is obtained from using the samples of natives and refugees alone. The remaining covariates from the models for both regressions and samples are reported in table A1 and A2 in the appendix. The inclusion of these additional controls, although important, are not the focus of the current analysis and therefore not discussed in detail.

For both non-refugee immigrants and refugees, the estimates of m, the entry cohort effects, indicate that immigrants, in general, earn lower wages than natives in the base year. This is consistent across both samples in Tables 5 and 7. The point estimates suggest that relative to non-refugee immigrants, refugees start with a larger wage disadvantage. It should be noted that for only the most recent migration cohorts the estimates for the two immigrant groups are statistically different. These estimates highlight the incomplete picture portrayed by the unadjusted mean wages reported in Figures 1-5. Earlier cohorts of immigrants, on average, might earn higher wages than natives, but once we compare immigrants to similarly aged and educated natives, we find a significant wage gap between the two groups. The most recent cohorts of refugees and non-refugee immigrants earned in 1990, on average, 34.4 percent and 26.5 percent lower wages than natives, respectively. In 2000, refugees and non-refugee immigrants that arrived between 1995 and 2000 earned, on average, 38.2 percent and 18.7 percent lower wages than natives, respectively.

Age effects, $\hat{\alpha}_b$, which are constrained to be equivalent for natives and immigrants, show that older individuals earn higher wages. The magnitudes are greater in the second columns for Tables 5 and 7, implying older refugees earn higher wages than comparable non-refugee immigrants. This can be inferred because the same natives are present in both regressions, and the only source of variation for estimates in the regressions is from the different immigrant groups. Aging effects, $\hat{\gamma}_{b00}$ and $\hat{\gamma}_{b10}$, are negative for both regressions in Tables 5 and 7 suggesting that as individuals get older their wages increase at a decreasing rate.

Duration effects, the main foci of this paper, are reported separately for the two samples and two immigrant groups in Table 1.6 and Table 1.8. Duration effects indicate assimilation, or lack thereof, and are extremely revealing in terms of the differences in experiences of refugees and non-refugee immigrants. Among the youngest cohorts, duration effects of 10 additional years are positive for non-refugee immigrants and refugees across both samples. Estimates from the 1990-2000 sample in Table 1.6 indicate that among the youngest immigrants duration effects for refugees and non-refugee immigrants are only statistically different for the most recent cohorts. The most recent and youngest of refugees experienced a 15 percent increase in wages in the 10 years, compared to 7.7 percent for non-refugee immigrants. In the 2000-2010 sample, the duration effects among the youngest immigrants are largely in favor of refugees. Estimates in Table 1.8 indicate that among the earliest migration cohorts duration effects for young refugees and non-refugee immigrants are not statistically different. For all remaining migration cohorts, however, the youngest refugees show larger assimilation rates than non-refugee immigrants. Among immigrants arriving between 1995 and 2000, the youngest refugees and non-refugee immigrants have assimilation rates of 5.7 percent and 22.7 percent respectively. In general, the results for the youngest cohorts, and more specifically those for refugees, are consistent with the earlier findings in Duleep and Regets (2002) that immigrants with lower initial wages also make larger percentage gains over time.

Across both samples, the middle age cohorts (28-37 years) of refugees have negative and, generally, statistically insignificant duration effects. The related estimates do not suggest that these refugees are catching up with their native counterparts. Among the non-refugee immigrants belonging to this age cohort, duration effects are positive for those arriving before 1980. Although the assimilation effect is positive in both of the samples, it is only statistically significant in the 2000-2010 sample. For all other migration cohorts in both the samples, the middle age cohorts of non-refugee immigrants have negative duration effects.

Duration effects for the oldest cohorts are negative and significant for refugees and non-refugee immigrants. The estimates in Tables 6 and 8 indicate a very interesting reversal in trend. For the first three migration cohorts, the negative assimilation effects are significantly larger in magnitude for refugees than non-refugee immigrants. This is consistent across both samples. With the two most recent cohorts of immigrants (those arriving between 1991 and 2000), however, non-refugee immigrants have significantly more negative assimilation effects than refugees. Taking the 2000-2010 sample estimates, for example, from Table 1.8 indicate that the oldest refugees and non-refugee immigrants arriving between 1985 and 1990 have assimilation rates of -18.7 percent and -11.9 percent respectively. Among immigrants who arrived between 1995 and 2000, however, refugees and non-refugee immigrants have assimilation rates of -12.6 percent and -20.6 percent respectively.

The above results suggest higher and quicker rates of wage assimilation among the most recent refugees in the U.S. relative to that of non-refugee immigrants.[13] I find that older non-

[13] The overall results and patterns in this section are consistent across a host of alternative samples and model. I separately re-estimate equation 1 using only white natives as the comparison group, removing potentially undocumented immigrants, and dropping students from the dataset. Additionally, in an alternative model, I adjust for potential matching bias resulting from using imputed wage values. Nearly 20 percent of the wage values in both the samples used in the analysis are not those directly reported by surveyed individuals. Instead the Census uses an unspecified method to impute wages for missing values. Hirsch and Schumacher (2004) note that this can lead to a sizeable downward bias when analyzing independent variables that are not in the list of variables that the Census uses to match when imputing missing wages. It is unlikely that the imputed wage values are adjusted for refugee status or even

refugee immigrants and refugees both experience lower levels of wage assimilation. Nonetheless, the above results provide evidence that the decreasing skills argument does not readily apply to refugees and that young refugees resettled in the U.S. show positive assimilation in wages over time. Results for the older refugees, however, are less favorable.

6 Why Refugee Assimilation Rates Differ

The presence of systematic differences in the wage assimilation rates of refugees and non-refugee immigrants established in the above half of the paper raises important questions. Why do most recent younger refugees seem to assimilate more successfully than their non-refugee counterparts? And at the same time, why do older refugees lag behind? While the first half of the paper is devoted to identifying these differences, in the current section, I investigate potential differences in labor force participation rates among refugees and non-refugee immigrants as a possible explanation for why these differences may occur.

6.1 Labor Force Participation

Wages are outcomes of individuals' decisions to participate in the labor force, making it a natural starting point for investigating why wages may differ between certain groups. Labor force participation rates may systematically differ between natives and immigrants in general. Some immigrants may need more time to acclimate to the change in culture and space and/or to invest in U.S. specific human capital before joining the labor force. All else the same, natives at any given point in time may be more likely to participate in the labor force. If immigrants of specific characteristics are delaying their participation in the labor force, then it implies that the sample of immigrants is not representative of their population. If this is especially the case with recent immigrants, then the assimilation effects may be driven by selection into the labor force.

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immigrant status in general. The 1990-2000 sample regression estimates to these models are reported in the Appendix.

Subsequently, the differences in assimilation rates across the two immigrant groups may be a product of how the immigrants are selecting into the labor force.

Work opportunities in the U.S. are strong pull factors for non-refugee immigrants and, as such, it is likely that they may participate in the labor market more readily and at higher rates than refugees. Lack of formal support from state or federal governments at arrival and the potential need to remit back to their countries of origin might render very little time for non-refugee immigrants to settle in and find work. For refugees, migration to the U.S. is a matter of resettlement and less of a choice. Resettlement programs for refugees ensure that their most basic needs are met at arrival and while economic self-sufficiency is strongly encouraged, there is a grace period during which case workers help refugees find suitable jobs and provide financial support in the meantime (Migration Policy Institute, 2004). For refugees who meet the income requirements, enrollment into anti-poverty programs like Aid to Families with Dependent Children (AFDC), before 1996, and Temporary Assistance to Needy Families (TANF), after 1996, may also affect labor force participation decisions. If enrollments into anti-poverty programs benefit the enrollees by allowing them more time and resources to invest in their human capital, one could expect lower labor force participation initially but higher levels in the future. Conversely, if anti-poverty programs discourage people from seeking work altogether, then one could expect an overall negative effect on labor force participation.

In Tables 1.9 and 1.10, I report labor force participation rates from the two samples for natives and for the two immigrant groups by their age and migration cohorts. Labor force participation rates reported in these tables show a clear difference in the participation rates among the three groups. Overall, refugees have the lowest participation rates in the base years in both samples. The most recent and youngest refugees in the 1990-2000 sample begin with participation rates of 59 percent compared to 80 percent and 84 percent among the non-refugee immigrant and native counterparts respectively. By 2000, participation rates for these refugees and non-refugees are identical but still well below the levels for natives. Table 1.9 shows a unique trend among the most recent cohort of refugees, whereas participation rates among the older cohorts drop over the 10 year period for both natives and non-refugee immigrants, labor force participation rates continue to increase for refugees regardless of age. These differences and the initially lower participation levels among refugees suggest that in the 1990-2000 sample selection into the labor force may account for a portion of the higher duration effects reported earlier in the paper.

Labor force participation trends in the 2000-2010 sample, reported in Table 1.10, show some interesting differences from the previous sample. On the one hand, although refugee participation rates are still the lowest at start, they are larger in magnitude and closer to the non-refugee immigrant rates. Even the most recent cohorts of refugees begin with relatively higher participation rates than those seen in the previous sample. On the other hand, compared to their labor force participation rates in the previous sample, non-refugee immigrants' participation rates are lower in the 2000-2010 sample. The change in refugees' labor force participation in the two samples may be linked to the welfare reforms of 1996 that incentivized higher participation in the labor force. Since then, the reforms largely excluded new non-refugee immigrants, we do not see a similar increase in their participation rates. Nonetheless, it would seem that labor force participation rates vary considerably across the three groups and should be accounted for in the analysis of wages. In the next section, I describe the Heckman selection model that accounts for the possibility of selection into the labor force.

6.2 Employment Correction Model

Here, I briefly explain the selection problem at hand and the proposed solution in the form of a Heckman selection model. For the purposes of this paper, I am interested in estimating the effect of a 10 year duration on the wages of immigrants (refugees and non-refugee immigrants). Although in the analysis I estimate duration effects for age and migration cohorts, for notational simplicity I proceed by explaining the problem at the individual level. The missing data problem here is that we witness the wages of only those who participate in the labor force and those participating in the labor force may differ in unmeasured ways from those who do not work. For example, young, skilled and motivated refugees may be more likely to delay participating in the labor force when they first arrive in the country and join at a later time. As such, the initial sample of young refugees may be negatively selected and the later sample more positively selected. The sample selection for non-refugee immigrants may be similar but further complicated due to return migration. Over time, the sample of non-refugee immigrants may be limited to those who choose not to migrate back to their home countries or elsewhere. These factors could account for the differences in duration effects found in the results section above. Because the selected samples may systematically differ from the population, the above results may not be generalizable.

In the current empirical setup, we witness the wages of each cohort twice. If refugees, assisted by state and federal support, delay their labor force participation to acquire the necessary human capital skills, then their sample in the initial time period is likely to be negatively selected. The same cohort in ten years is likely to include those who have since acquired the necessary human capital skills. All else the same, not accounting for the negative selection in the first time period and positive selection in the second would result in a larger increase in wages for refugees. The selection model below attempts to correct this bias.

I begin the setup for Heckman selection model with a basic selection into the labor force equation:

$$z_i^* = w_i\delta + u_i \tag{2}$$

$$z_i = \begin{cases} 1 \ if \ z_i^* > 0 \\ 0 \ if \ z_i^* > 0 \end{cases}$$

z_i^* represents the latent, unobserved utility from participating in the labor force which depends on variable w_i. What we observe is z_i the participation or non-participation of individual i: The subsequent wage outcome equation can then be presented as:

$$y_i = \begin{cases} x_i\beta + \epsilon_i \; if \; z_i^* > 0 \\ - \; if \; z_i^* > 0 \end{cases}$$

where x_i represents the measure of duration. Given this structure, the problem in estimating the duration effect β stems from the possible correlation between u_i and ϵ_i. To account for this the Heckman selection model typically makes the following additional assumption:

$$u_i \sim N(0,1)$$
$$\epsilon_i \sim N(0,\sigma^2)$$
$$corr(u_i,\epsilon_i) = \rho$$

The conditional mean wage given participation in the labor force can then be shown as:[14]

$$\begin{aligned} E(y_i|y_i \; is \; observed) &= x_i\beta + \rho\sigma_\epsilon \left[\frac{\varphi\left(\frac{w_i\delta}{\sigma_u}\right)}{\emptyset\left(\frac{w_i\delta}{\sigma_u}\right)} \right] \\ &= x_i\beta + \beta_\lambda\lambda(\alpha_u) \end{aligned}$$

The OLS regression equation estimated in the previous result section does not include the second term above (the inverse Mills ratio), and thus may suffer from omitted variable bias. The Heckman Selection Model allows for the estimation of $\lambda(\alpha_u)$ and subsequently $\hat{\beta}_\lambda$, $\hat{\rho}$ and $\hat{\sigma}_\epsilon$. The presence and size of the bias in the OLS results depends of the statistical and quantitative significance of these three estimates. On a technical note, even though the model is fully identified based on the non-linearity of the selection equation, it is conventional to have at least one instrument present in the selection equation that is not present in the outcome equation. In the Heckman estimation that follows, although I do not have a clear instrument, the list of

[14] A more thorough explanation can be found in Greene (2003, pp. 782-787)

independent variables in the two equations differ slightly. In the main outcome equation where I seek to identify duration effects separately from aging effects, I use a partially specified model which excludes the interaction between age and migration cohorts. In the selection equation where I have no need for the separate identification of duration and aging effects, I simply include individual measures of age up to a cubic function and migration cohorts separately.[15]

I report results from the selection model in the second set of columns in Tables 1.5-1.8. Similar to the OLS regressions, I estimate separate selection models for refugees and non-refugee immigrants for both the 1990-2000 and 2000-2010 samples. Estimates of $\lambda(\alpha_u)$ and ρ reported for each selection model at the bottom of Table 1.5 and Table 1.7, are statistically significant. This suggests that there is, in fact, selection into the labor force.

When we compare the duration effects from the OLS and selection models, however, the overall story remains unchanged. There are, however, some changes, especially in the 1990-2000 sample, that are worth noting. Duration effects for the 1990-2000 sample in Table 1.6 indicate that not accounting for selection into the labor force causes an upward bias for refugees and a downward bias for non-refugee immigrants. Focusing on the most recent immigrant cohort, controlling for selection narrows the assimilation gap between young refugees and non-refugee immigrants. Although assimilation rates are still higher for young refugees, the estimate drops from 15 percent to 11 percent while those for non-refugees increases from 7.7 percent to 8.3 percent. Among older cohorts, the gap in duration effects between the two immigrant groups seems to widen once we account for selection. Older refugees appear to fare even worse than initially indicated. These estimates are consistent with the theory that the sample of recent and young refugees in 1990 is negatively selected and that 10 years later in 2000 this same cohort is positively selected.

[15] The estimates from the first stage selection regression are not included in the paper. These estimates are available upon request.

Although there is still evidence of selection in the 2000-2010 sample, the subsequent effect on the duration effects for refugees and non-refugees does nothing to explain the large gap between the two immigrant groups. Duration estimates for the youngest and most recent refugees drop from 22.7 percent to 19.7 percent while those for non-refugee immigrants drop from 5.7 percent to 3.3 percent. In general, estimates from the selection models suggest an upward bias in duration effects for both immigrant types. This makes sense given that labor participation rates in the second sample are fairly similar for refugees and non-refugee immigrants. Estimates from the selection model also preserve the reversal in trend among the most recent and oldest immigrants found in the main section.

7 Concluding Remarks

The analysis reveals mixed results in response to the question raised in the introduction of the paper. Long term economic analysis of refugees suggests that those who enter the country at a younger age assimilate most successfully. Older refugees on the other hand are likely to need extended support over time. It is, however, worth noting again that among older immigrants arriving after 1990, non-refugee immigrants have relatively lower rates of assimilation than refugees. The changes in welfare programs introduced in the mid-nineties, which among other things encouraged higher rates of labor force participation, could explain this reversal in trend.

The question as to why younger immigrants, in general, assimilate faster and more successfully than older immigrants is, perhaps, relatively easier to explain. Immigrants that arrive at a young age go through many of the same experiences as similarly aged natives. This may include going to school, college, and other educational institutions. Additionally, younger immigrants are also likely to be culturally more adaptive which may, in turn, lead to higher wages in the future. Older immigrants, in comparison, may be more set in their ways and less culturally adaptive. This limitation in cultural assimilation is likely to spill over into their wage assimilation as well. Another and more economic line of reasoning is that along acquisition of country-specific human capital. Immigrants arriving in the country at a younger age, over time, acquire U.S. specific human capital which may be more generously rewarded in the labor market. Older immigrants who acquire a significant portion of their human capital in their source countries are likely to earn relatively lower wages for the same amount of human capital (same education levels for example).

The harder and, perhaps, more relevant question is why younger refugees seem to assimilate more successfully than non-refugee immigrants. There is much that remains to be explored and explained along these lines. The present analysis takes an important step to- ward answering this question. Although selection into the labor force cannot explain the entire difference in relative assimilation rates, it reveals some interesting differences between the two immigrant groups especially before and after the welfare reform period. Differences in labor force participation explain a larger portion of the difference in assimilation rates prior to the welfare reforms compared to the period after. In the post reform period, in which only refugees remained eligible for welfare and non-refugee immigrants were effectively barred from all welfare programs, I find larger assimilation rates among refugees. The analysis suggests that support for recent immigrants' matters and those who are younger seem most capable of utilizing existing support structures. Furthermore, the lack of progress seen among older immigrants, in general, and specifically with refugees suggests that there may be a void with respect to programs that are meant specifically for older immigrants.

There are additional reasons for differences between the two immigrants groups which I have not been explored in this paper. Other examples, such as differences in motivation among refugees and non-refugee immigrants is harder to measure. There are reasons why motivation may be an important distinguishing factor; unlike non-refugee immigrants, refugees seldom have the choice to migrate back to their source country. Refugees may view their stay in the U.S. as more permanent, whereas non-refugee immigrants may still harbor intentions of returning to their countries of origin. This differing approach to their stay in the country is likely to affect their motivation for assimilation as well. Motivation, however, is much harder to measure. The differences in wages may also arise from the choice of location for residence. Non-refugee immigrants are more likely to have larger networks and as such likely to locate themselves in close proximity to enclaves of other similar immigrants. It is unclear whether enclaves lead to higher or lower wages for immigrants over time. Refugee immigrants, conversely, may not have a pre-existing network, and as such, they may settle in areas where they are more likely to find work.

There are at least two policy implications of the above results. First, non-refugee immigrants' wage assimilation may be aided through stronger support structures, at least, during their early years in the country and second, more attention is needed for the older refugees who seem to be falling through possible cracks in the resettlement programs. Future work in this area of research may focus on identifying additional reasons for the difference in experience between the two immigrant groups. Future collection and availability of data on refugees and non-refugee immigrants and their use of different support structures, personal networks and federal and state programs would be essential to make further progress in this area of work.

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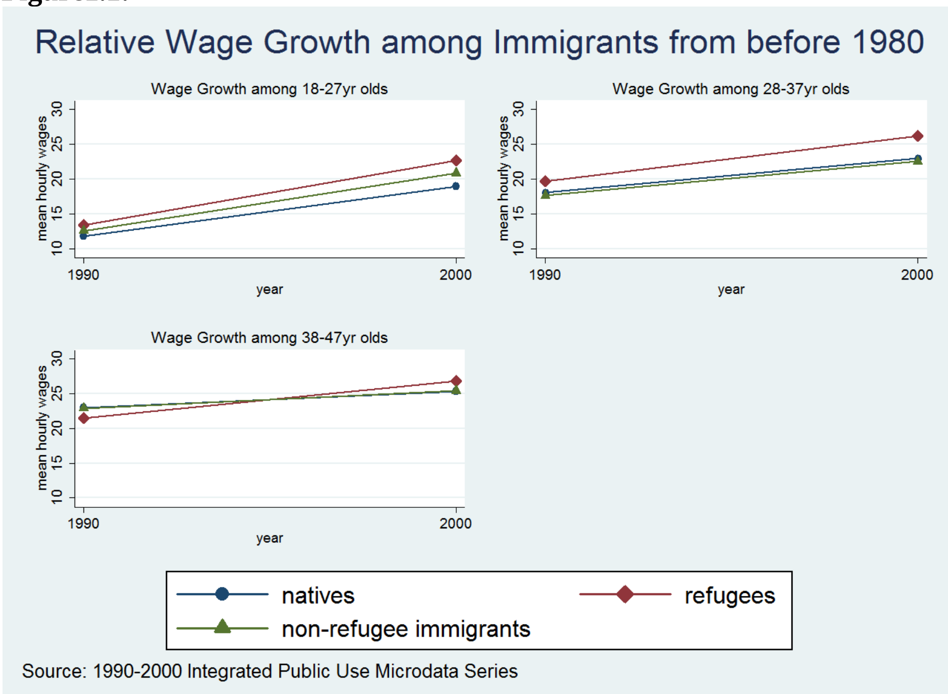
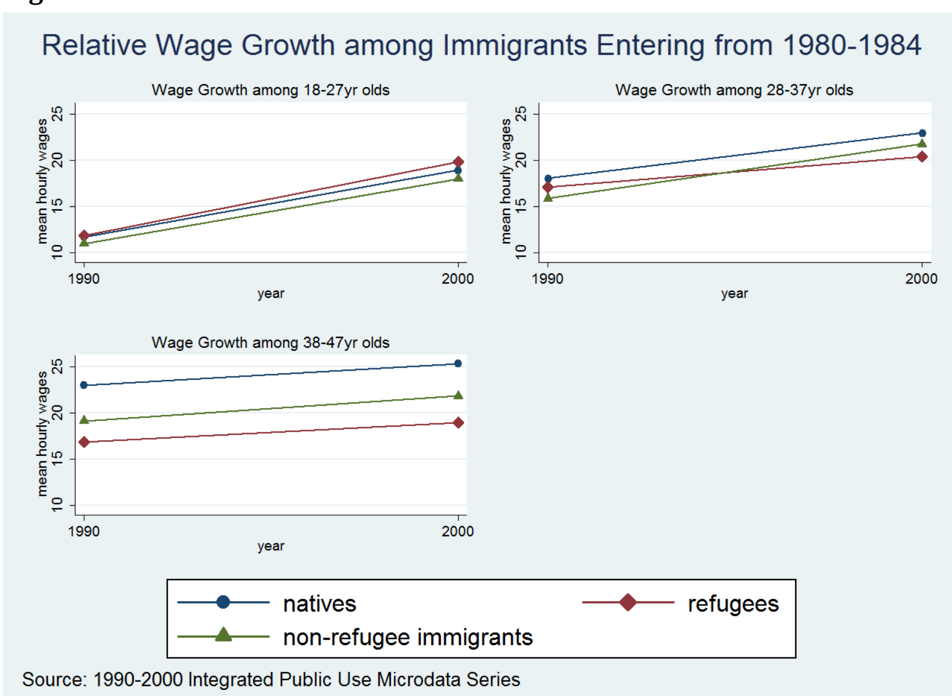
Figure1.1:



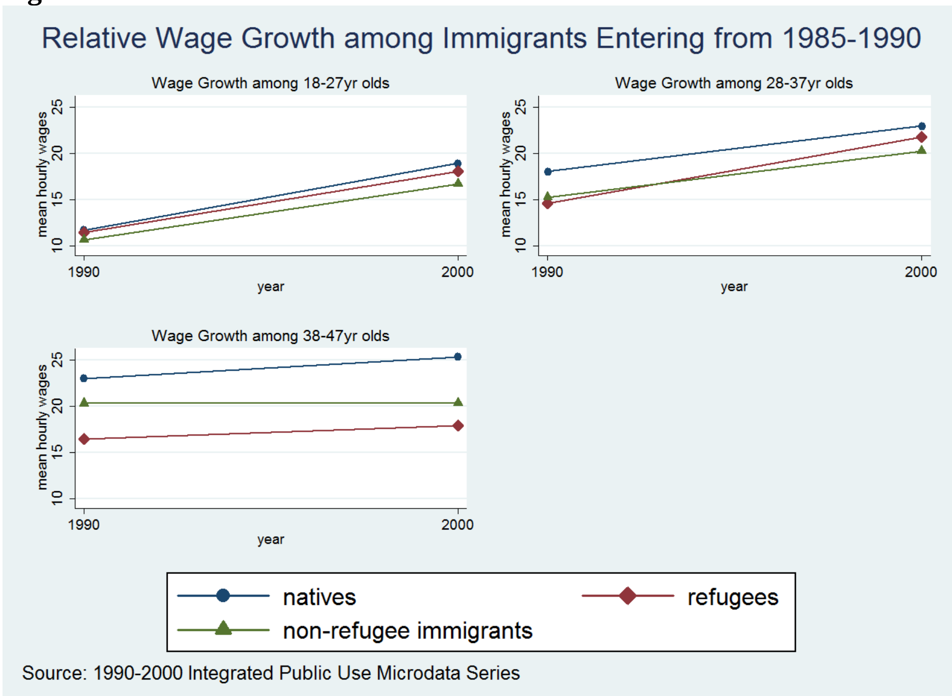
Notes: The above graphs compare unadjusted mean wages of refugees and non-refugee immigrants, who arrived in the U.S. before 1980, with natives.

Figure 1.2:



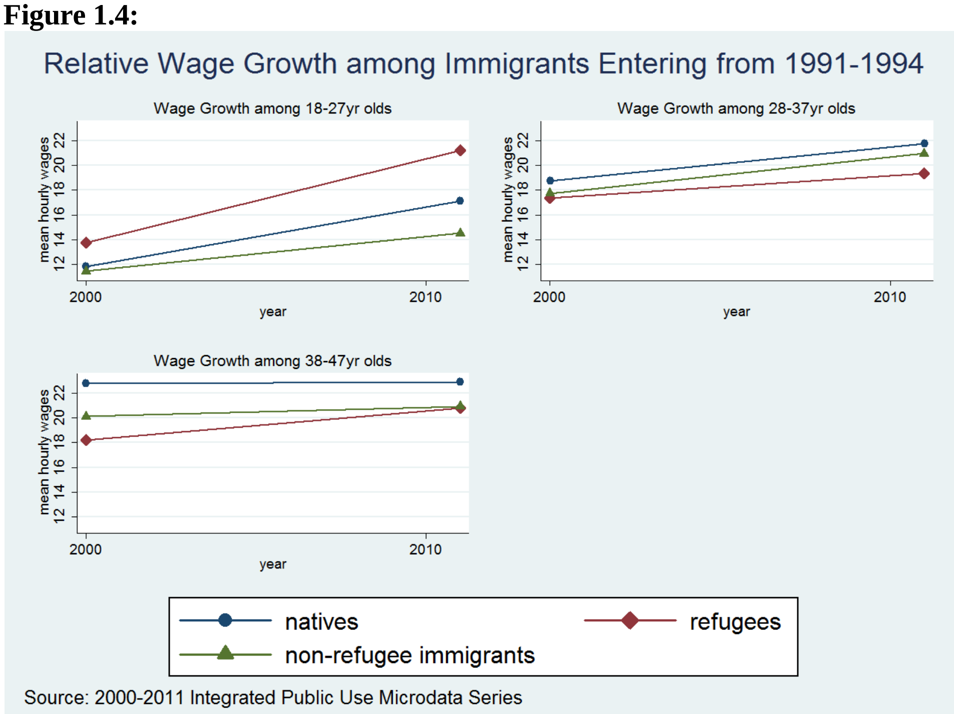
Notes: The above graphs compare unadjusted mean wages of refugees and non-refugee immigrants, who arrived in the U.S. between 1980 and 1984, with natives

Figure 1.3:



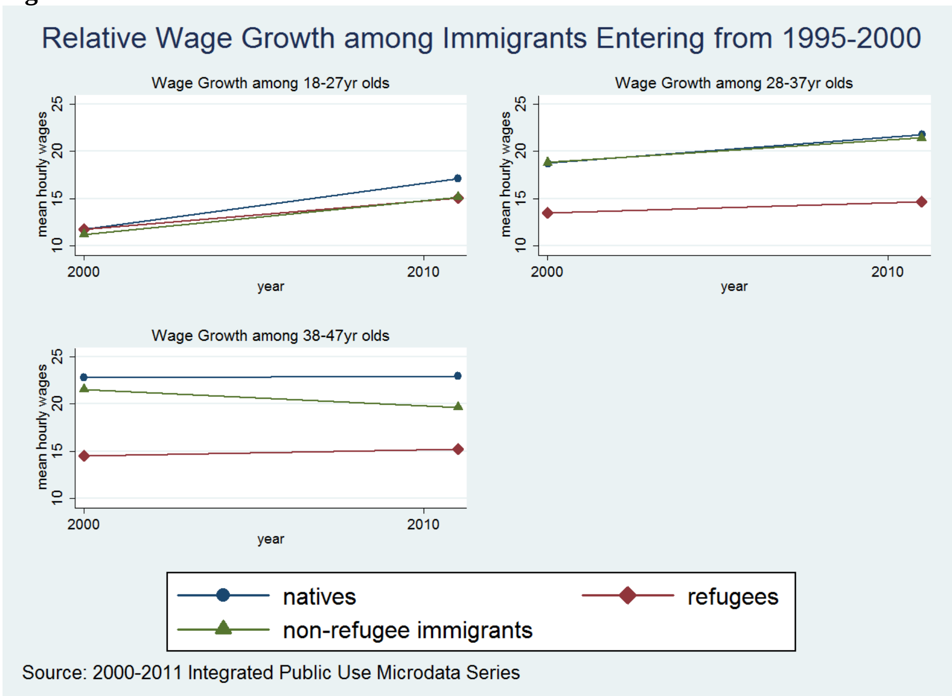
Notes: The above graphs compare unadjusted mean wages of refugees and non-refugee immigrants, who arrived in the U.S. between 1985 and 1990, with natives.

Figure 1.4:



Notes: The above graphs compare unadjusted mean wages from 2000 to 2010 for refugees and non-refugee immigrants, who arrived in the U.S. between 1991 and 1994, with natives.

Figure 1.5:



Notes: The above graphs compare unadjusted mean wages from 2000 to 2010 for refugees and non-refugee immigrants, who arrived in the U.S. between 1995 and 2000, with natives.

Table 1.1: Refugees by Country of Origin and Migration Cohorts (1990-2000)

Country of birth	Migration Cohorts			
	1960s	1970s	1980s	Total
Afghanistan	0	0	377	377
Albania	2	0	22	24
Armenia	0	48	75	123
Azerbaijan	0	2	7	9
Bosnia	2	5	0	7
Bulgaria	3	0	46	49
Byelorussia	1	9	30	40
Cambodia	2	308	1,440	1,750
Cuba	1,137	395	460	1,992
Czechoslovakia	0	0	166	166
Estonia	0	0	1	1
Ethiopia	0	0	486	486
Haiti	13	0	0	13
Hungary	0	0	187	187
Iran	0	0	125	125
Iraq	0	0	75	75
Laos	1	792	1,979	2,772
Latvia	0	0	5	5
Macedonia	0	12	0	12
Moldavia	0	7	7	14
Namibia	0	0	1	1
Other USSR/Russia	27	513	383	923
Poland	0	0	832	832
Republic of Georgia	0	3	1	4
Romania	0	0	593	593
Thailand	0	2	4	6
Ukraine	8	136	183	327
USSR, ns	1	42	45	88
Uzbekistan	0	0	6	6
Vietnam	1	3007	6,721	9,729
Total	1,198	5,281	14,257	20,736

Notes: The above table tabulates refugees identified in the IPUMs sample by their timing of entry in to the U.S. and country of origin.
Source: 1990-2000 Integrated Public Use Micro-data

Table 1.2: Refugees by Country of Origin and Migration Cohorts (2000-2010)

Country of birth	Migration Cohorts				
	1960s	1970s	1980s	1990s	Total
Afghanistan	0	0	354	70	424
Albania	3	0	20	61	84
Armenia	0	76	185	274	535
Austria	0	0	1	5	6
Azerbaijan	0	1	9	88	98
Bosnia	1	4	0	1,433	1438
Bulgaria	1	0	27	0	28
Byelorussia	0	15	58	192	265
Cambodia	0	145	1,540	43	1728
Croatia	0	0	0	54	54
Cuba	2,298	1,036	1,392	3,179	7905
Czechoslovakia	0	0	54	0	54
Eritrea	0	0	0	19	19
Estonia	0	0	2	1	3
Ethiopia	0	0	406	213	619
Guinea	0	0	0	4	4
Haiti	6	0	0	0	6
Hungary	0	0	163	0	163
Indonesia	0	1	26	0	27
Iran	0	0	276	0	276
Iraq	0	0	106	429	535
Kazakhstan	0	2	0	18	20
Kosovo	0	0	0	2	2
Laos	0	597	2,074	446	3117
Latvia	0	0	11	46	57
Liberia	0	0	0	22	22
Lithuania	0	0	6	3	9
Macedonia	0	12	0	0	12
Malaysia	0	6	13	0	19
Moldavia	0	13	18	118	149
Oceania	0	0	2	0	2
Other USSR/Russia	2	182	315	1,320	1819
Poland	0	0	539	0	539
Republic of Georgia	0	4	4	42	50
Romania	0	3	451	194	648
Serbia	0	0	0	3	3
Somalia	0	0	0	334	334
Sudan	0	0	4	66	70
Thailand	0	48	379	52	479
Ukraine	7	150	272	1,449	1878
USSR, ns	0	46	69	227	342
Uzbekistan	0	2	8	190	200
Vietnam	1	1,219	6,740	2,723	10683
Yugoslavia	0	0	0	22	22
Total	2,319	3,562	15,526	13,342	34749

Notes**:** The above table tabulates refugees identified in the IPUMs sample by their timing of entry in to the U.S. and country of origin. *Source:* 2000 Integrated Public Use Micro-data, 2011 ACS (3-year sample)

Table 1.3: Summary of Statistics by Immigration Status (1990-2000)

	Refugees		**Non-refugee Immigrants**		**Natives**	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Hourly wages	18.81	28.98	18.80	50.17	19.89	39.42
Home Ownership	0.58	0.49	0.52	0.50	0.72	0.45
Not Proficient in English	0.19	0.39	0.24	0.43	--	--
Age	36.57	8.78	36.85	9.05	37.01	9.27
Married with spouse present	0.62	0.49	0.63	0.48	0.63	0.48
Age at entry into the US	22.74	9.03	20.54	9.04	--	--
Percentage with less than High school education	17.77	-	33.97	--	8.79	--
Percentage with High school degree	29.83	--	24.40	--	37.79	--
Percentage with some years of college	27.07	--	16.81	--	26.84	--
Percentage with college degree or higher	25.33	--	24.82	--	26.59	--
Local unemployment rate (%)	4.12	--	4.42	--	4.13	--
Local female labor force participation rate (%)	50.98	--	51.07	--	51.37	--
Local immigrant population (%)	21.05	--	23.19	--	9.54	--
Local mobility of population (%)[§§§§]	51.37	--	50.94	--	47.79	--
Number of Cases	23,206		334,718		3,723,734	

Notes: The above sample includes only males, who are at the time of the interview between 18 to 57 years of age. The sample includes only immigrant cohorts who entered between 1950 and 1990 and are seen twice in the dataset. Local area above refers to commuting zones identified only in the 1990 and 2000 sample. Source: 1990-2000 Integrated Public Use Micro-data

[§§§§] Local mobility refers to the percentage of people in the area that reported having moved from a different state or country to their current location in the last five years.

Table 1.4: Summary of Statistics by Immigration Status (2000-2010)

	Refugees		Non-refugee Immigrants		Natives	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Hourly wages	18.49	19.09	17.86	65.90	19.27	27.83
Home Ownership	0.63	0.48	0.50	0.50	0.72	0.45
Not Proficient in English	0.18	0.39	0.28	0.45	--	--
Age	37.80	9.11	36.43	9.24	37.10	9.62
Married with spouse present	0.61	0.49	0.58	0.49	0.58	0.49
Age at entry into the US	19.15	10.16	19.84	9.09	--	--
Percentage with less than High school education	13.66	-	31.89	--	7.33	--
Percentage with High school degree	33.94	--	27.98	--	41.47	--
Percentage with some years of college	23.17	--	14.15	--	24.14	--
Percentage with college degree or higher	29.24	--	25.98	--	27.06	--
Local unemployment rate (%)	5.58	--	5.60	--	5.09	--
Local female labor force participation rate (%)	35.43	--	34.95		35.71	
Local immigrant population (%)	25.04	--	25.38	--	11.59	--
Local mobility of population (%)[*****]	31.56	--	32.36	--	30.33	--
Number of Cases	34,887		424,818		2,877,521	

Notes: The above sample includes only males, who are at the time of the interview between 18 to 57 years of age. The sample includes only immigrant cohorts who entered between 1950 and 2000 and are seen twice in the dataset. Local area above refers to counties since commuting zones are not identified in the ACS sample. Source: 2000 Integrated Public Use Micro-data and 2011 ACS (3-year sample)

[*****] Local mobility refers to the percentage of people in the area that reported having moved from a different state or country to their current location in the last five years.

Table 1.5: Coefficient Estimates for Male Log Wages (1990-2000)

	Model 1A		**Model 1B (Heckman Selection)**	
	Non-refugee Immigrants	**Refugees**	**Non-refugee Immigrants**	**Refugees**
Constant	1.563*** (0.445)	1.442*** (0.445)	1.554*** (0.445)	1.437*** (0.450)
Year (2000 = 1) ($\hat{\gamma}_{00}$)	0.220*** (0.028)	0. 226*** (0.029)	0.224*** (0.012)	0.230*** (0.029)
Effect of Being an Immigrants in 1990 ($\hat{\beta}_m$)				
Immigrated before 1980 (MC 1)	-0.107*** (0.018)	-0.146*** (0.039)	-0.107*** (0.018)	-0.145*** (0.038)
Immigrated between 1980-84 (MC 2)	-0.178*** (0.019)	-0.209*** (0.034)	-0.182*** (0.019)	-0.201*** (0.034)
Immigrated between 1985-90 (MC 3)	-0.245*** (0.022)	-0.347*** (0.033)	-0.235*** (0.023)	-0.304*** (0.035)
Birth Cohort Effect ($\hat{\alpha}_b$)				
Age 28	0.258*** (0.034)	0.267*** (0.036)	0.248*** (0.034)	0.257*** (0.037)
Age 38	0.469*** (0.031)	0.482*** (0.033)	0.461*** (0.031)	0.475*** (0.032)
Aging Effect ($\hat{\gamma}_{b00}$)				
BC 2 * Year(2000 = 1)	-0.114*** (0.033)	-0.122*** (0.036)	-0.099*** (0.033)	-0.108*** (0.036)
BC 3 * Year(2000 = 1)	-0.277*** (0.030)	-0.290*** (0.032)	-0.250*** (0.030)	-0.262*** (0.031)
Duration Effect ($\hat{\gamma}_{m00}$)				
MC 1 * Year(2000 = 1)	0.039* (0.017)	0.012 (0.029)	0.047** (0.017)	0.008 (0.029)
MC 2 * Year(2000 = 1)	0.075*** (0.020)	0.075*** (0.025)	0.094*** (0.019)	0.067** (0.026)
MC 3 * Year(2000 = 1)	0.077*** (0.025)	0.149*** (0.025)	0.083*** (0.024)	0.110*** (0.027)
Additional Duration Effect of Belonging to older Birth Cohorts ($\hat{\gamma}_{bm00}$)				
MC 1 * BC2 * Year(2000 = 1)	-0.073*** (0.011)	-0.032 (0.020)	-0.072*** (0.011)	-0.031 (0.020)
MC 1 * BC3 * Year(2000 = 1)	-0.092*** (0.012)	-0.088** (0.033)	-0.089*** (0.012)	-0.082** (0.033)

MC 2 * BC2 * Year(2000 = 1)	-0.113*** (0.010)	-0.142*** (0.020)	-0.113*** (0.009)	-0.137*** (0.021)
MC 2 * BC3 * Year(2000 = 1)	-0.197*** (0.015)	-0.277*** (0.026)	-0.191*** (0.015)	-0.263*** (0.026)
MC 3 * BC2 * Year(2000 = 1)	-0.119*** (0.009)	-0.139*** (0.022)	-0.119*** (0.009)	-0.137*** (0.022)
MC 3 * BC3 * Year(2000 = 1)	-0.204*** (0.012)	-0.310*** (0.029)	-0.196*** (0.012)	-0.300*** (0.030)
Rho	--	--	-0.254*** (0.039)	-0.255*** (0.043)
Lambda	--	--	-0.148*** (0.024)	-0.147*** (0.026)
Observations	4,057,149	3,745,791	4,585,304	4,217,911

Notes: The table above presents coefficient estimates for parameters of interest from regression equation 1 in the paper. The regression equation is estimated once for refugee and native sample and second time with economic migrants and natives. Standard errors in parentheses * p < 0.05, ** p < 0.01, *** p < 0.001
Source: 1990-2000 Integrated Public Use Micro-data

Table 1.6: Duration Effects by Migration and Birth Cohorts (1990-2000)

	Duration effects(Model 1A) $(\hat{\gamma}_{m00} + \hat{\gamma}_{bm00})$		**Duration effects (Model 1B, Heckman Selection Model)** $(\hat{\gamma}_{m00} + \hat{\gamma}_{bm00})$	
	Non-refugee Immigrants	Refugees	Non-refugee Immigrants	Refugees
Immigrated before 1980 (MC 1)				
Age 18-27 in 1990 (BC 1)	0.039* (0.017)	0.012 (0.029)	0.047** (0.017)	0.008 (0.029)
Age 28-37 in 1990 (BC 2)	0.034 (0.018)	-0.020 (0.027)	0.026 (0.018)	-0.023 (0.027)
Age 38-47 in 1990 (BC 3)	-0.053** (0.020)	-0.076 (0.040)	-0.043* (0.019)	-0.074 (0.039)
Immigrated between 1980-84 (MC 2)				
Age 18-27 in 1990 (BC 1)	0.075*** (0.020)	0.075*** (0.025)	0.094*** (0.019)	0.067** (0.026)
Age 28-37 in 1990 (BC 2)	-0.038 (0.022)	-0.067** (0.025)	-0.019 (0.022)	-0.069** (0.026)
Age 38-47 in 1990 (BC 3)	-0.122*** (0.024)	-0.202*** (0.026)	-0.098*** (0.025)	-0.196*** (0.026)
Immigrated between 1985-90 (MC 3)				
Age 18-27 in 1990 (BC 1)	0.077*** (0.025)	0.149*** (0.025)	0.083*** (0.024)	0.110*** (0.027)
Age 28-37 in 1990 (BC 2)	-0.042 (0.026)	-0.010 (0.032)	-0.036 (0.026)	-0.027 (0.033)
Age 38-47 in 1990 (BC 3)	-0.126*** (0.012)	-0.161*** (0.033)	-0.112*** (0.029)	-0.190*** (0.033)

Notes: The estimates above are derived from linear combinations of specific estimates in Table 1.5. The duration effects are indicative of positive or negative wage assimilation for each combination of migration and birth cohorts. The wage of refugees and non-refugee immigrants in 2000 are relative to natives and results of ten year increase in duration. Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: 1990-2000 Integrated Public Use Micro-data

Table 1.7: Coefficient Estimates for Male Log Wages (2000-2010)

	Model 1C		Model 1D (Heckman Selection)	
	Non-refugee Immigrants	**Refugees**	**Non-refugee Immigrants**	**Refugees**
Constant	1.326*** (0.085)	1.324*** (0.084)	1.416*** (0.084)	1.420*** (0.084)
Year (2010 = 1) ($\hat{\gamma}_{10}$)	0.061 (0.041)	0.031 (0.038)	0.047 (0.040)	0.016 (0.038)
Effect of Being an Immigrants in 2000 ($\hat{\beta}_m$)				
Immigrated before 1980 (MC 1)	-0. 092*** (0.015)	-0.050 (0.038)	-0. 082*** (0.016)	-0.046 (0.041)
Immigrated between 1980-84 (MC 2)	-0.133*** (0.014)	-0.139*** (0.028)	-0.118*** (0.014)	-0.130*** (0.028)
Immigrated between 1985-90 (MC 3)	-0.165*** (0.014)	-0.181*** (0.032)	-0.149*** (0.014)	-0.170*** (0.031)
Immigrated between 1991-94 (MC 4)	-0.163*** (0.015)	-0.254*** (0.029)	-0.151*** (0.015)	-0.241*** (0.029)
Immigrated between 1995-00 (MC 5)	-0.187*** (0.015)	-0.382*** (0.027)	-0.171*** (0.015)	-0.361*** (0.029)
Birth Cohort Effect ($\hat{\alpha}_b$)				
Age 28	0.273*** (0.003)	0.290*** (0.004)	0.265*** (0.003)	0.282*** (0.003)
Age 38	0.410*** (0.004)	0.433*** (0.005)	0.405*** (0.003)	0.430*** (0.004)
Aging Effect ($\hat{\gamma}_{b00}$)				
BC 2 * Year(2010 = 1)	-0.078*** (0.004)	-0.095*** (0.004)	-0.065*** (0.005)	-0.082*** (0.004)
BC 3 * Year(2010 = 1)	-0.175*** (0.004)	-0.199*** (0.004)	-0.156*** (0.005)	-0.180*** (0.004)
Duration Effect ($\hat{\gamma}_{b10}$)				
MC 1 * Year(2010 = 1)	0.141*** (0.014)	0.127** (0.049)	0.129*** (0.015)	0.117* (0.049)
MC 2 * Year(2010 = 1)	0.069*** (0.012)	0.122*** (0.018)	0.049*** (0.012)	0.103*** (0.018)
MC 3 * Year(2010 = 1)	0.078*** (0.010)	0.102*** (0.023)	0.056*** (0.010)	0.084*** (0.024)
MC 4 * Year(2010 = 1)	0.058*** (0.009)	0.176*** (0.032)	0.039*** (0.009)	0.158*** (0.031)
MC 5 * Year(2010 = 1)	0.057*** (0.009)	0.227*** (0.033)	0.033*** (0.009)	0.197*** (0.031)

Additional Duration Effect of Belonging to older Birth Cohorts ($\widehat{\gamma}_{bm10}$)				
MC 1 * BC2 * Year(2010 = 1)	-0.122*** (0.014)	-0.123*** (0.042)	-0.123*** (0.014)	-0.123*** (0.043)
MC 1 * BC3 * Year(2010 = 1)	-0.200*** (0.014)	-0.139*** (0.048)	-0.203*** (0.015)	-0.141* (0.048)
MC 2 * BC2 * Year(2010 = 1)	-0.074*** (0.017)	-0.146*** (0.024)	-0.076*** (0.017)	-0.146** (0.024)
MC 2 * BC3 * Year(2010 = 1)	-0.166*** (0.016)	-0.256*** (0.025)	-0.171*** (0.016)	-0.255*** (0.025)
MC 3 * BC2 * Year(2010 = 1)	-0.113*** (0.014)	-0.141*** (0.024)	-0.114*** (0.014)	-0.141*** (0.024)
MC 3 * BC3 * Year(2010 = 1)	-0.198*** (0.016)	-0.289*** (0.031)	-0.202*** (0.016)	-0.288*** (0.031)
MC 4 * BC2 * Year(2010 = 1)	-0.106*** (0.015)	-0.213*** (0.034)	-0.106*** (0.015)	-0.213*** (0.034)
MC 4 * BC3 * Year(2010 = 1)	-0.230*** (0.015)	-0.300*** (0.035)	-0.233*** (0.015)	-0.301*** (0.035)
MC 5 * BC2 * Year(2010 = 1)	-0.116*** (0.008)	-0.244*** (0.028)	-0.116*** (0.008)	-0.245*** (0.028)
MC 5 * BC3 * Year(2010 = 1)	-0.264*** (0.014)	-0.353*** (0.031)	-0.267*** (0.014)	-0.353*** (0.031)
Rho	--	--	-0.178*** (0.015)	-0.182*** (0.017)
Lambda	--	--	-0.107*** (0.010)	-0.109*** (0.011)
Observations	3,302,339	2,912,408	3,829,884	3,358,840

Notes: The table above presents coefficient estimates for parameters of interest from regression equation 1 in the paper. The regression equation is estimated once for refugee and native sample and second time with economic migrants and natives.
Standard errors in parentheses * p < 0.05, ** p < 0.01, *** p < 0.001
Source: 2000 Integrated Public Use Micro-data, 2011 ACS (3-year sample)

Table 1.8: Duration Effects by Migration and Birth Cohorts (2000-2010)

	Duration effects(Model 1C) $(\hat{\gamma}_{b10} + \hat{\gamma}_{bm10})$		Duration effects (Model 1D, Heckman Selection Model) $(\hat{\gamma}_{b10} + \hat{\gamma}_{bm10})$	
	Non-refugee Immigrants	Refugees	Non-refugee Immigrants	Refugees
Immigrated before 1980 (MC 1)				
Age 18-27 in 1990 (BC 1)	0.141*** (0.014)	0.127** (0.049)	0.129*** (0.015)	0.117* (0.049)
Age 28-37 in 1990 (BC 2)	0.019* (0.008)	0.004 (0.029)	0.006 (0.008)	-0.006 (0.029)
Age 38-47 in 1990 (BC 3)	-0.059*** (0.009)	-0.011 (0.019)	-0.074*** (0.010)	-0.024 (0.019)
Immigrated between 1980-84 (MC 2)				
Age 18-27 in 1990 (BC 1)	0.069*** (0.012)	0.122*** (0.018)	0.049*** (0.012)	0.103*** (0.018)
Age 28-37 in 1990 (BC 2)	-0.005 (0.011)	-0.024 (0.021)	-0.027* (0.012)	-0.042* (0.021)
Age 38-47 in 1990 (BC 3)	-0.097*** (0.011)	-0.134*** (0.019)	-0.121*** (0.012)	-0.152*** (0.019)
Immigrated between 1985-90 (MC 3)				
Age 18-27 in 1990 (BC 1)	0.078*** (0.010)	0.102*** (0.023)	0.056*** (0.010)	0.084*** (0.024)
Age 28-37 in 1990 (BC 2)	-0.035*** (0.010)	-0.039* (0.019)	-0.059*** (0.011)	-0.057*** (0.019)
Age 38-47 in 1990 (BC 3)	-0.119*** (0.012)	-0.187*** (0.025)	-0.146*** (0.013)	-0.204*** (0.026)
Immigrated between 1991-94 (MC 4)				
Age 18-27 in 1990 (BC 1)	0.058*** (0.009)	0.176*** (0.032)	0.039*** (0.009)	0.158*** (0.031)

Age 28-37 in 1990 (BC 2)	-0.048*** (0.013)	-0.037 (0.026)	-0.067*** (0.013)	-0.055*** (0.019)
Age 38-47 in 1990 (BC 3)	-0.173*** (0.013)	-0.124*** (0.030)	-0.194*** (0.014)	-0.143*** (0.019)
Immigrated between 1995-2000 (MC5)				
Age 18-27 in 1990 (BC 1)	0.057*** (0.009)	0.227*** (0.033)	0.033*** (0.009)	0.197*** (0.031)
Age 28-37 in 1990 (BC 2)	-0.059*** (0.012)	-0.018 (0.022)	-0.082*** (0.012)	-0.047*** (0.022)
Age 38-47 in 1990 (BC 3)	-0.206*** (0.013)	-0.126*** (0.031)	-0.233*** (0.014)	-0.155*** (0.030)

Notes: The estimates above are derived from linear combinations of specific estimates in Table 1.7. The duration effects are indicative of positive or negative wage assimilation for each combination of migration and birth cohorts. The wage of refugees and non-refugee immigrants in 2000 are relative to natives and results of ten year increase in duration. Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: 2000 Integrated Public Use Micro-data, 2011 ACS (3-year sample)

Table 1.9: Labor Force Participation (%) by Migration and Birth Cohorts (1990-2000)

	18-27 year-olds (BC1)		28-37 year-olds (BC2)		38-47 year-olds (BC3)	
Refugees	**Year (1990)**	**Year (2000)**	**Year (1990)**	**Year (2000)**	**Year (1990)**	**Year (2000)**
Immigrated before 1980 (MC 1)	68.05	85.74	92.13	84.82	90.85	80.02
Immigrated between 1980-84 (MC 2)	68.66	81.87	85.76	78.59	81.17	71.75
Immigrated between 1985-90 (MC 3)	59.04	80.73	71.23	78.28	66.31	72.02
Non-refugee Immigrants						
Immigrated before 1980 (MC 1)	82.21	85.2	94.75	82.51	94.59	79.78
Immigrated between 1980-84 (MC 2)	85.89	79.07	94.35	81.07	94.34	79.22
Immigrated between 1985-90 (MC 3)	80.41	79.21	87.9	81.48	90.29	79.1
Natives	83.81	90.43	95.1	88.74	94.38	82.08

Notes: The above table provides rates of labor force participation among natives and the two immigrant groups by their age and migration cohorts. All natives are grouped into migration cohort zero. The table uses data from the 1990-2000 sample.

Table 1.10: Labor Force Participation (%) by Migration and Birth Cohorts (2000-2010)

	18-27 year-olds (BC1)		**28-37 year-olds (BC2)**		**38-47 year-olds (BC3)**	
Refugees	**Year (2000)**	**Year (2010)**	**Year (2000)**	**Year (2010)**	**Year (2000)**	**Year (2010)**
Immigrated before 1980 (MC 1)	75.99	92.16	85.18	90.56	84.93	87.09
Immigrated between 1980-84 (MC 2)	71.68	90.99	81.56	91.51	77.78	84.31
Immigrated between 1985-90 (MC 3)	69.45	89.05	80.35	90.95	77.95	85.14
Immigrated between 1991-94 (MC 4)	70.39	91.19	78.87	91.42	80.47	88.57
Immigrated between 1995-00 (MC 5)	69.97	94.41	75.16	91.9	73.84	87.78
Non-refugee Immigrants						
Immigrated before 1980 (MC 1)	80.3	94.04	85.19	93.22	82.49	87.72
Immigrated between 1980-84 (MC 2)	73.47	93.03	79.1	93.74	80.95	90.66
Immigrated between 1985-90 (MC 3)	72.92	94.09	79.17	94.73	81.33	91.75
Immigrated between 1991-94 (MC 4)	75.04	94.05	81.99	95.24	82.66	91.64
Immigrated between 1995-00 (MC 5)	73.95	95.57	80.38	95.64	80.75	92.19
Natives	80.55	91.82	90.41	90.36	88.72	83.65

Notes: The above table provides rates of labor force participation among natives and the two immigrant groups by their age and migration cohorts. All natives are grouped into migration cohort zero. The table uses data from the 2000-2010 sample.

Table 1.A1: Regression Coefficients for Control Variables

VARIABLES	Non-refugee Immigrants	Refugees
Africa and Immigrant Interaction	0.0166	
	(0.0298)	
Asia and Immigrant Interaction	0.0514*	0.0131
	(0.0272)	(0.0332)
Europe and Immigrant Interaction	0.238***	0.0887**
	(0.0240)	(0.0376)
North America and Immigrant Interaction	0.302***	
	(0.0161)	
South America and Immigrant Interaction	-0.0152	
	(0.0302)	
Oceania and Immigrant Interaction	0.186***	
	(0.0238)	
Caribbean and Immigrant Interaction	-0.0358	0.0187
	(0.0414)	(0.0677)
Central America and Immigrant Interaction	-0.0940***	
	(0.0143)	
Middle East and Immigrant Interaction	0.0908***	0.0321
	(0.0156)	(0.0306)
High school graduate	0.201***	0.210***
	(0.00420)	(0.00308)
Some college	0.321***	0.326***
	(0.00481)	(0.00452)
College or higher	0.639***	0.638***
	(0.00752)	(0.00745)
Married with spouse present	0.230***	0.235***
	(0.00267)	(0.00300)
CZ unemployment rate	-3.441***	-3.270***
	(0.864)	(0.837)
CZ immigrant share of population	1.016***	1.093***
	(0.112)	(0.121)
CZ level of mobility	-0.375***	-0.390***
	(0.104)	(0.104)
CZ unemployment rate and year 2000 interaction	-0.934	-0.764

	(0.637)	(0.638)
CZ immigrant share of population and year 2000 interaction	-0.116	-0.0938
	(0.0812)	(0.0766)
CZ level of mobility and year 2000 interaction	0.151***	0.112**
	(0.0559)	(0.0550)
Constant	2.176***	2.152***
	(0.0696)	(0.0679)
Observations	4,100,927	3,714,274
R-squared	0.278	0.274

Notes: Above coefficients are those for the control variables in regression equation 1. The two sets of estimates pertain to the two separate regressions for refugees and economic migrants. The 1990-2000 sample is used for all the tables in the appendix. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 1.A2: Coefficient Estimates from Alternate Model 1

	Non-refugee Immigrants	**Refugees**
Year (2000 = 1) ($\hat{\gamma}_{00}$)	0.218*** (0.043)	0.235*** (0.042)
Effect of Being an Immigrants in 1990 ($\hat{\beta}_m$)		
Immigrated before 1980 (MC 1)	-0.149*** (0.016)	-0.133*** (0.028)
Immigrated between 1980-84 (MC 2)	-0.235*** (0.020)	-0.188*** (0.026)
Immigrated between 1985-90 (MC 3)	-0.290*** (0.026)	-0.364*** (0.028)
Immigrated between 1991-94 (MC 4)	-0.312*** (0.032)	-0.425*** (0.029)
Immigrated between 1995-00 (MC 5)	-0.334*** (0.027)	-0.505*** (0.031)
Birth Cohort Effect ($\hat{\alpha}_b$)		
Age 28	0.310*** (0.005)	0.326*** (0.004)
Age 38	0.472*** (0.007)	0.492*** (0.005)
Aging Effect ($\hat{\gamma}_{b00}$)		
BC 2 * Year(2000 = 1)	-0.166*** (0.005)	-0.177*** (0.004)
BC 3 * Year(2000 = 1)	-0.288*** (0.006)	-0.302*** (0.004)
Duration Effect ($\hat{\gamma}_{m00}$)		
MC 1 * Year(2000 = 1)	0.036*** 0.008)	0.045 (0.034)
MC 2 * Year(2000 = 1)	0.084*** (0.009)	0.067*** (0.020)
MC 3 * Year(2000 = 1)	0.076*** (0.012)	0.174*** (0.029)
Additional Duration Effect of Belonging to older Birth Cohorts ($\hat{\gamma}_{bm00}$)		
MC 1 * BC2 * Year(2000 = 1)	-0.073*** (0.009)	-0.011 (0.023)
MC 1 * BC3 * Year(2000 = 1)	-0.087*** (0.010)	-0.071* (0.034)
MC 2 * BC2 * Year(2000 = 1)	-0.108*** (0.008)	-0.149*** (0.021)

MC 2 * BC3 * Year(2000 = 1)	-0.185^{***} (0.014)	-0.265^{***} (0.029)
MC 3 * BC2 * Year(2000 = 1)	-0.116^{***} (0.008)	-0.144^{***} (0.021)
MC 3 * BC3 * Year(2000 = 1)	-0.197^{***} (0.010)	-0.303^{***} (0.029)
Observations	3625778	3239125

Notes: The table above presents coefficient estimates for parameters of interest, from an alternate model 1, where I change the base native group to only white natives. Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A3: Coefficient Estimates from Alternate Model 2

	Non-refugee Immigrants	**Refugees**
Year (2000 = 1) ($\hat{\gamma}_{00}$)	0.224*** (0.042)	0.239*** (0.042
Effect of Being an Immigrants in 1990 ($\hat{\beta}_m$)		
Immigrated before 1980 (MC 1)	-0.119*** (0.015)	-0.115*** (0.028)
Immigrated between 1980-84 (MC 2)	-0.214*** (0.019)	-0.170*** (0.026)
Immigrated between 1985-90 (MC 3)	-0.268*** (0.026)	-0.344*** (0.028)
Immigrated between 1991-94 (MC 4)	-0.278*** (0.030)	-0.399*** (0.028)
Immigrated between 1995-00 (MC 5)	-0.302*** (0.025)	-0.483*** (0.030)
Birth Cohort Effect ($\hat{\alpha}_b$)		
Age 28	0.304*** (0.004)	0.316*** (0.004)
Age 38	0.467*** (0.006)	0.492*** (0.005)
Aging Effect ($\hat{\gamma}_{b00}$)		
BC 2 * Year(2000 = 1)	-0.164*** (0.004)	-0.171*** (0.004)
BC 3 * Year(2000 = 1)	-0.280*** (0.006)	-0.290*** (0.004)
Duration Effect ($\hat{\gamma}_{m00}$)		
MC 1 * Year(2000 = 1)	0.041*** (0.009)	0.053 (0.031)
MC 2 * Year(2000 = 1)	0.100*** (0.008)	0.075*** (0.021)
MC 3 * Year(2000 = 1)	0.090*** (0.011)	0.181*** (0.028)
Additional Duration Effect of Belonging to older Birth Cohorts ($\hat{\gamma}_{bm00}$)		
MC 1 * BC2 * Year(2000 = 1)	-0.068*** (0.010)	-0.010 (0.022)
MC 1 * BC3 * Year(2000 = 1)	-0.090*** (0.011)	-0.076* (0.033)
MC 2 * BC2 * Year(2000 = 1)	-0.108*** (0.009)	-0.144*** (0.021)

MC 2 * BC3 * Year(2000 = 1)	-0.190*** (0.014)	-0.265*** (0.029)
MC 3 * BC2 * Year(2000 = 1)	-0.114*** (0.008)	-0.141*** (0.021)
MC 3 * BC3 * Year(2000 = 1)	-0.200*** (0.010)	-0.305*** (0.029)
Observations	4080944	3714274

Notes: The table above presents coefficient estimates for parameters of interest, from alternate model 3, where I excludes potentially illegal immigrants from the sample while. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 1.A4: Regression Coefficient Estimates from Alternate Model 3

	Non-refugee Immigrants	**Refugees**
Year (2000 = 1) ($\hat{\gamma}_{00}$)	0.197*** (0.042)	0.213*** (0.041)
Effect of Being an Immigrants in 1990 ($\hat{\beta}_m$)		
Immigrated before 1980 (MC 1)	-0.137*** (0.015)	-0.089** (0.031)
Immigrated between 1980-84 (MC 2)	-0.229*** (0.019)	-0.155*** (0.027)
Immigrated between 1985-90 (MC 3)	-0.283*** (0.027)	-0.337*** (0.037)
Immigrated between 1991-94 (MC 4)	-0.289*** (0.029)	-0.375*** (0.039)
Immigrated between 1995-00 (MC 5)	-0.308*** (0.024)	-0.454*** (0.038)
Birth Cohort Effect ($\hat{\alpha}_b$)		
Age 28	0.267*** (0.004)	0.279*** (0.004)
Age 38	0.428*** (0.006)	0.443*** (0.005)
Aging Effect ($\hat{\gamma}_{b00}$)		
BC 2 * Year(2000 = 1)	-0.131*** (0.004)	-0.138*** (0.004)
BC 3 * Year(2000 = 1)	-0.246*** (0.006)	-0.256*** (0.004)
Duration Effect ($\hat{\gamma}_{m00}$)		
MC 1 * Year(2000 = 1)	0.043*** (0.010)	0.054 (0.034)
MC 2 * Year(2000 = 1)	0.099*** (0.008)	0.083*** (0.024)
MC 3 * Year(2000 = 1)	0.092*** (0.010)	0.206*** (0.031)
Additional Duration Effect of Belonging to older Birth Cohorts ($\hat{\gamma}_{bm00}$)		
MC 1 * BC2 * Year(2000 = 1)	-0.067*** (0.009)	-0.009 (0.026)
MC 1 * BC3 * Year(2000 = 1)	-0.089*** (0.010)	-0.074* (0.037)
MC 2 * BC2 * Year(2000 = 1)	-0.102*** (0.009)	-0.146*** (0.024)

MC 2 * BC3 * Year(2000 = 1)	-0.188*** (0.013)	-0.254*** (0.027)
MC 3 * BC2 * Year(2000 = 1)	-0.114*** (0.008)	-0.147*** (0.023)
MC 3 * BC3 * Year(2000 = 1)	-0.198*** (0.010)	-0.314*** (0.031)
Observations	3761448	3413593

Notes: The table above presents coefficient estimates for parameters of interest, from alternate model 3, where I drop all individuals in the labor force who are students. Standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

Table 1.A5: Regression Coefficient Estimates from Alternate Model 4

	Non-refugee Immigrants	**Refugees**
Year (2000 = 1) ($\hat{\gamma}_{00}$)	0.211^{***} (0.044)	0.234^{***} (0.042)
Effect of Being an Immigrants in 1990 ($\hat{\beta}_m$)		
Immigrated before 1980 (MC 1)	-0.116^{***} (0.016)	-0.075^{**} (0.028)
Immigrated between 1980-84 (MC 2)	-0.193^{***} (0.019)	-0.124^{***} (0.027)
Immigrated between 1985-90 (MC 3)	-0.235^{***} (0.027)	-0.295^{***} (0.035)
Immigrated between 1991-94 (MC 4)	-0.271^{***} (0.028)	-0.350^{***} (0.037)
Immigrated between 1995-00 (MC 5)	-0.295^{***} (0.025)	-0.426^{***} (0.036)
Birth Cohort Effect ($\hat{\alpha}_b$)		
Age 28	0.300^{***} (0.005)	0.316^{***} (0.004)
Age 38	0.462^{***} (0.007)	0.481^{***} (0.005)
Aging Effect ($\hat{\gamma}_{b00}$)		
BC 2 * Year(2000 = 1)	-0.163^{***} (0.005)	-0.175^{***} (0.004)
BC 3 * Year(2000 = 1)	-0.278^{***} (0.006)	-0.292^{***} (0.004)
Duration Effect ($\hat{\gamma}_{m00}$)		
MC 1 * Year(2000 = 1)	0.045^{***} (0.009)	0.052 (0.032)
MC 2 * Year(2000 = 1)	0.086^{***} (0.009)	0.067^{**} (0.021)
MC 3 * Year(2000 = 1)	0.065^{***} (0.011)	0.174^{***} (0.029)
Additional Duration Effect of Belonging to older Birth Cohorts ($\hat{\gamma}_{bm00}$)		
MC 1 * BC2 * Year(2000 = 1)	-0.067^{***} (0.009)	-0.007 (0.022)
MC 1 * BC3 * Year(2000 = 1)	-0.091^{***} (0.010)	-0.075^{*} (0.033)
MC 2 * BC2 * Year(2000 = 1)	-0.105^{***} (0.008)	-0.140^{***} (0.022)

MC 2 * BC3 * Year(2000 = 1)	-0.185*** (0.013)	-0.260*** (0.029)
MC 3 * BC2 * Year(2000 = 1)	-0.111*** (0.008)	-0.138*** (0.021)
MC 3 * BC3 * Year(2000 = 1)	-0.193*** (0.010)	-0.299*** (0.029)
Observations	4100927	3714274

Notes: The table above presents coefficient estimates for parameters of interest, from alternate model 4, where I adjust for possible matching bias resulting from imputed wage values. The samples used for the above regressions are the same as those for the main models in the paper. Standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

CHAPTER TWO

THE CURIOUS CASE OF REFUGEES: WHY DID MEDICAID PARTICIPATION FALL FOLLOWING THE 1996 WELFARE REFORMS?

Abstract

Title IV of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) revoked welfare eligibility for all immigrants entering the U.S. after August 1996; however, refugees were the exception to this rule, remaining eligible before and after the reform. In spite of this, welfare participation among refugees fell dramatically in the post reform period. This paper examines the curious fall in refugees' Medicaid participation rates following the 1996 welfare reforms. Using repeated cross-sections of the March supplement to the Current Population Survey, years 1994 to 2001, I attempt to determine as to whether or not title IV of PRWORA "chilled" potentially eligible refugees into non-participation. My findings are twofold. First, in states that did not opt for welfare waivers, I find no evidence that refugees were chilled by Title IV. Conversely, in states that did implement welfare waivers, I am unable to reject the possibility of chilling, given that waiver-state refugees exhibited an unusual 16 to 12 percentage point drop in Medicaid participation in the post reform period. Additionally, the increase in private health insurance participation, among refugees in waiver states, is equal in magnitude to the fall in Medicaid. This suggests that although Title IV of PRWORA may have unintentionally reduced refugees' participation in Medicaid, it did not necessarily increase the likelihood that they would be without insurance coverage.

1 Introduction

Over two and half million refugees, roughly 15 percent of the U.S. immigrant population, have been resettled in the U.S. in the period between 1980 and 2010 (INS, 2010). Refugees are distinct from other non-refugee immigrants because refugees either cannot or do not want to return to their home countries for fear of persecution (Cortes, 2004).[18] Given the difficult circumstances under which refugees migrate to the U.S., it is not surprising that relative to other immigrants, refugees are poorer and more welfare dependent on arrival. A comparison of recently arrived refugees and non-refugee immigrants in *Table 2.1*, shows that refugees who have been in the U.S. for 7 or fewer years are more likely to be older, on welfare, and have lower family earnings than their non-refugee counterparts. Perhaps it is in recognition of this vulnerability that even the 1996 welfare reforms, which brought significant changes to the welfare eligibility of most documented immigrants, made exceptions for refugees.

Title IV of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) is of special relevance when analyzing the reform's impact across groups with different immigration status.[19] In accordance with title IV post-reform, all documented immigrants entering the country after August 1996 were made ineligible for welfare until naturalization. For most documented immigrants this requires five years. This rule is commonly referred to as the five-year bar and even after this period documented immigrants' use of welfare

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[18]Refugee is defined as someone who "owing to a well-founded fear of being persecuted for reasons of race, religion, nationality, membership of a particular social group or political opinion, is outside the country of his nationality, and is unable to, or owing to such fear, is unwilling to avail himself of the protection of that country" (1951 Refugee Convention, UNHCR)

[19]The 1996 welfare reforms decentralized the former federal entitlement programs with Temporary Aid for Needy Families (TANF), a block grant distributed and regulated at the state level. Federal TANF regulations included new work requirements and time limits on cash assistance. The overarching nature of reforms under PRWORA included provisions for Supplemental Security Income (SSI) eligibility, child support enforcement, child protection, childcare, marriage promotion, and abstinence education. (Takahashi, unpublished manuscript)

is still subject to deeming and public charge restrictions (Fix and Passel, 1999).[20] Refugees and asylees were the exceptions to this rule and thus remain eligible for welfare programs regardless of their time of entry. Curiously, however, in the period between 1994 to 1999 Medicaid participation rates among low-income, working age, adult refugees fell by 58 percent. In comparison, participation among the native and non-refugee immigrant counterparts fell by 8 percent and 23 percent, respectively (Fix and Passel, 2002). Amidst the more usual concerns of moral hazard and welfare dependence, the decrease in program participation among potentially eligible groups is somewhat puzzling at the outset. Furthermore, falling participation rates among a known economically vulnerable group like the refugees, may raise questions about their economic assimilation in the U.S.

The assimilation process of refugees in the United States is of particular interest, given that the country has historically accepted more refugees for resettlement than all others combined (Migration Policy Institute, 2004). This underscores the importance of factors that contribute to the assimilation of refugees over time. Public assistance programs in this context may serve a dual purpose; increased levels of program participation by itself can be seen as assimilation (Klopfenstein, 1998), or participation may serve as means to assimilate refugees. In either one of these two instances, changes to levels of program participation can have conflicting underlying reasons. A fall in participation can be the result of improved economic conditions or it might reflect increased barriers to accessing welfare. The experience of refugees in the post-1996 welfare reform period mirrors exactly such a situation. In this paper I endeavor to determine why Medicaid participation fell among refugees and whether this is the result of an improved economy, an unintended effect of the welfare reform, or some other third factor.

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[20]Deeming attempts to shift the financial responsibility of immigrants from the federal government to immigrants' sponsors and subsequently discourage poor migrants, with the potential to become "public charge", from entering the U.S. (Takahashi unpublished manuscript).

The current literature provides no consensus and little guidance to the potential causes behind the fall in refugees' Medicaid participation in the post-reform period. Fix and Passel (1999) document the fall and point to new time limits on eligibility set by the reform for non-naturalized refugees as potential source. They conclude that behavioral changes brought by the reform as opposed to changes in family structure and demographics are responsible for the fall in refugees' participation in Medicaid. Bollinger and Hagstrom (2008), in an analysis of refugees' participation in the Food Stamp program, argue that improvements in local employment opportunities are primarily responsible for lowering Food Stamp participation rates. Another potential cause suggested by Fix and Passel (2002) includes "chilling," which refers to the non-participation of eligible groups due to fear generated either directly or indirectly by an icy policy climate. Even if refugees, unlike other documented immigrants, were exempt from the changes related to Title IV of the PRWORA, it is possible that they were misinformed or unclear about their eligibility in the post-reform period. While there is a sizeable literature focusing on chilling among immigrants in general, (Borjas, 1994; Borjas and Hilton, 1996; Loftstrom and Bean, 2002; Mazzaolari and Gordon, 2004; Watson, 2010; etc.) only one paper (Tripodi, 2004) investigates chilling as a possible cause for the drop in welfare usage and specifically among refugees. Using panel data on refugees residing in the U.S. for less than five years, and thus eligible before and after the reforms, Tripodi (2004) finds no evidence of chilling among refugees on grounds that their fall in welfare usage precedes the 1996 welfare reforms.

In this paper I analyze the fall in Medicaid participation among refugees, relative to non-refugee immigrants and natives. Specifically, I attempt to answer the question as to whether or not the fall in participation was due to the reforms, particularly Title IV of PRWORA, as it passed in August of 1996. In addition to variation in local economic conditions already explored in previous literature, I analyze yearly state level variations in federal expenditures on refugees and availability of state waivers prior to the 1996 reforms as additional reasons that may explain the refugee experience. States proposing experimental changes that furthered the goals of the AFDC system were granted welfare waivers that allowed some federal requirements to be waived.

I use the Immigration and Naturalization Service (INS) data available from 1972 until 2000 to impute refugee status for immigrants identified in the March supplement of the CPS for years 1994 to 2001. The strategy for identifying refugees draws on the approach in Bollinger and Hagstrom (2008 and 2011). My findings suggest a two-part story in the case of refugees' Medicaid participation before and after the welfare reform. In states that did not opt for welfare waivers, I find no evidence that refugees were "chilled" by title IV of PRWORA. In states that implemented welfare waivers however, I am unable to reject the possibility of "chilling." Refugees in these states experienced an unexplained 16 to 12 percentage point drop in Medicaid participation in the post reform period. There is, however, an added caveat to the refugee experience in the waiver states. Private health insurance participation among refugees in the waiver states probabilistically increased by a magnitude almost equal to that of the fall in Medicaid. This suggests that although Title IV of PRWORA may have unintentionally reduced refugees' participation in Medicaid, it did not necessarily increase the likelihood of refugees ending up with no health insurance.

2 Motivation

Most comparative studies between the native and immigrant experiences of the welfare reforms fail to distinguish between refugee and non-refugee immigrants (e.g., Mazzaolari and Gordon 2004, Watson 2010, Borjas and Hilton 1996). This failure to distinguish between the two immigrant types can have strong implications. For instance, the pattern of welfare usage between the two groups may differ systematically due to differences in their observable characteristics and access to welfare. The need to separate the two immigrant groups is even greater given the historical backdrop of the welfare reforms. Borjas (2002) argues that Title IV was a response to the high and growing rates of welfare participation among new immigrants. However, when we look at the participation rates of the two immigrant groups separately, a different picture emerges.

For example, in *Figure 2.1*, Medicaid participation rates are lowest for non-refugee immigrants before and after the welfare reforms. It is entirely possible that higher welfare participation among immigrants in general is due to refugees in the mix. Ironically, refugees who were exempt under Title IV of PRWORA still witnessed the largest fall in participation compared to non-refugee immigrants and natives. In fact, the graphs in *Figure 2.1* suggest that the fall in immigrants' Medicaid participation in the post-reform period is driven almost entirely by refugees.

The topic at hand relates to three broad strands of existing literature; the first two, including the impact of the welfare reforms on immigrants' welfare participation (Borjas, 1994; Borjas and Hilton, 1996; Loftstrom and Bean, 2002; Mazzaolari and Gordon, 2004) and determinants of take-ups of means tested programs have both been widely researched. Mazzaolari and Gordon (2004) find that in the post-reform period a sizable difference in the relative drop in welfare participation between citizens and eligible non-citizens remains even after controlling for various demographic and economic factors. This unexplained drop for non-citizens is attributed to a chilling effect. An example of a chilling effect among immigrants is the misguided fear of becoming a public charge and being denied citizenship for using federal welfare. Failure to understand the complicated eligibility requirements could also act as deterrence for certain eligible groups. In attributing the residual drop in participation to chilling, Mazzaolari and Gordon (2004), however, are silent in regards to the source of the chilling.

More recently, Watson (2010) identifies federal immigration enforcement as a possible source for this chilling effect. In looking at Medicaid participation among children, Watson finds that participation among children with non-native parents is highly sensitive to enforcement levels. She argues that changes in enforcement levels and not those in welfare laws are responsible for chilling eligible population from participating in Medicaid. The impact of Federal immigrant enforcements on refugees *a priori* is unclear. On the one hand, enforcement levels should not be a source of chilling for refugees who do not face the same naturalization process as other legal non-citizens. If, however, federal immigrant enforcement levels are driven by an underlying overall anti-migrant sentiment, then it is quite possible that refugees maybe deterred from participating as well.

The modeling of take-up of welfare benefits goes back to Moffit (1983). In his model, an individual maximizes her utility, a function of hours of work (a bad), private income sources, and welfare benefits that are subject to a budget constraint. The model also includes disutility from participating in a welfare program which can be thought of as transaction costs and or any stigma associated with being a welfare recipient. Consequently, individuals participate if the increase in utility derived from welfare benefits outweighs the related costs of participation. Currie (2004) provides a comprehensive survey on the determinants while Stuber and Kronebush (2004) focuses specifically on take up of TANF and Medicaid. Currie (2004) identifies social stigma associated with participating in welfare programs and transaction costs in the form of time, effort, and energy spent on acquiring knowledge and applying for such programs as being the main predictors of enrollment, or lack thereof. Given the focus on stigma and transaction cost, the refugee sub-population is of special interest as refugees arrive in the U.S. from a variety of disadvantaged backgrounds for the sole purpose of resettlement. At least initially, it seems plausible that stigma may not be a large deterrent to participation in welfare programs. Over time, however, successful assimilation may, in part, rely on refugees reducing participation in welfare programs and garnering respect in society. Stigma, as such, may play an increasingly larger role in deterring participation over time. Refugees also benefit from the assistance of local community organizations when they first arrive in the country.[21] These organizations help refugees in their

[21]In addition to federal agencies, ten regional offices across the country and state partners the U.S. Office of Refugee Resettlement (ORR) includes nine voluntary organizations that work directly with refugees at the local level. Per the ORR webpage these local community organizations include: Church World Service, Ethiopian Community Development Council, Episcopal Migration Ministries, Hebrew Immigrant Aid Society, International Rescue Community, U.S. Committee for Refugees and Immigrants, Lutheran Immigration and Refugee Services, United States Conference of Catholic Bishops and World Relief Corporation

relocation and also help with their enrollment in related welfare programs, in effect lowering any transaction cost associated with program participation. All other factors being equal, given the lower transaction costs and arguably lower importance of stigma, one can expect higher take-up rates among refugees during their early years in the country. Welfare participation rates among refugees can also be linked to Cortes (2004), in which the author provides both theoretical and empirical evidence that investments in human capital are inversely related to the probability of an immigrant returning to her country of origin (close to zero for refugees). To the extent that welfare participation may complement investment in human capital, results from Cortes (2004) would support higher levels of welfare participation among refugees.

Finally, the third stream of related literature pertains to the assimilation experience of refugee immigrants in the United States. The lack of identifiers for refugees as a distinct subset of immigrants in national census datasets (CPS, SIPP, ACS and others) makes this area of research relatively scant. Previous work on refugees has involved classifying all migrants from a list of refugee prone countries as refugees (Potocky-Tripodi, 2001; Borjas, 2002; Cortes, 2004). Such *ad hoc* measures fail to take into account that refugee and non-refugee immigrants can have the same country of origin and that over time refugees have come from many different countries. Measurement errors resulting from miss-classifying certain refugees as non-refugees and vice versa render the slope estimates from regression analysis unreliable. Addressing this measurement issue, Bollinger and Hagstrom (2008 & 2011) use a statistical matching technique to assign probabilities among immigrants for being a refugee. This probability by construction is directly related to an immigrant's year of entry into the U.S., country of origin, gender and age. Bollinger and Hagstrom (2008) argue that this approach minimizes the measurement errors and allows for the specific analysis of refugees. using this technique, the authors analyze refugees' participation in the Food Stamp program. They find refugees' participation to be three times more responsive to local employment conditions than those of non-refugee migrants. Furthermore, they find no evidence of refugees being chilled during the 1996 welfare reforms.

The methodology and identification strategy in the present study borrows from the approach in Bollinger and Hagstrom (2008). The obvious difference in the current paper and that of Bollinger and Hagstrom (2008) is that the previous authors look at Food Stamp participation at the household level while I analyze Medicaid participation at the individual level. Although both are welfare programs, they differ in eligibility requirements and the manner in which they are utilized within a household. Food stamps, though provided individually, are likely to be more fungible in that access to food stamps among any family members will likely spill over to others in the family. Furthermore, looking at participation at the household level fails to capture any fall in participation among members within the household. Medicaid, conversely, is individual-specific and non-transferable within a family. An individual level study for the latter is consequently more appropriate.

There are additional reasons for thinking that Medicaid participants may differ from Food Stamp participants. Multiple changes have occurred in Medicaid eligibility rules since the mid-1980s which means that eligibility is not strictly based on income levels and assets. Medicaid has grown continuously through the period in question, mainly due to growth in the price of medical care, extensions of program eligibility, and other reforms, resulting in caseload growth (Gruber, 2003). Consequently, Medicaid participants include even those above the official 185 percent of poverty level cutoff mark. These include children, pregnant mothers, and individuals above the poverty cutoff point but with exceptionally large medical expenses. Compared to Food Stamp participants, those belonging to the Medicaid program are likely to be more heterogeneous.

Furthermore, Stuber and Kronebush (2004) estimate that among non-participants, almost 70 percent believe there is a negative perception of individuals enrolled in welfare (including the Food Stamp program) whereas only 33 percent of non-participants believe the same about Medicaid. Confusion about eligibility is a larger concern with Medicaid, suggesting that the factors determining enrollment for Food Stamp and Medicaid may differ systematically. It is therefore plausible that the impact of the welfare reforms in 1996 may have been different for the two programs.

Also important are the policy changes that have occurred since the 1996 welfare reforms, mainly the 1998 Agriculture Research Extension and Education Reform Act and the 2002 Farm Bill. The former restored Food Stamp benefits for selected immigrants, including pre-enactment children, elders, and the disabled. The Farm Bill added low-income immigrant children, disabled legal immigrants who arrived after August 1996, and legal immigrants with five years of residency to the list of those eligible (Capps et al, 2004). In contrast, no such policies have been enacted with regards to Medicaid eligibility which necessitates a separate analysis for the impact on Medicaid participation among refugees and non-refugee immigrants before and after the welfare reforms. Finally, a side by side comparison of time series plots of Food Stamp and Medicaid participation rates in *Figure 2.2* provides further impetus for the analysis at hand. Both overall and refugee Food Stamp participation rates show a clear downward trend that precedes the welfare reforms of 1996. This calls into question any causal link between the welfare reforms and refugees' Food Stamp participation rates. Medicaid participation rates, in contrast, were rising just before the welfare reforms and fall right after. In this regard the present Medicaid analysis maybe more fitting to the difference in difference framework utilized here and in Bollinger and Hagstrom (2008).

3 Data

The empirical findings of the paper are based on data gathered from multiple sources. I use publicly available individual level data from the March supplement to the Current Population Survey (CPS, 1994- 2001) for information on Medicaid enrollment, related demographics, and economic characteristics. Immigration and Naturalization Services (INS) data titled "Immigrants Admitted to the United States," which is available for years 1972 through 2000, is used for identifying refugees in the CPS. I use information from a technical report by the Council of Economic Advisors (CEA) titled "Explaining The Decline in Welfare Receipts, 1993-1996" to identify if and when states opted for federal welfare waivers. Local unadjusted unemployment rates for years 1993 to 2000 at the MSA level are extracted from the Bureau of Labor Statistics (BLS) website. To account for individuals living outside of MSAs, I include unemployment rates at the state level. Lower unemployment rates proxy for better economic conditions and vice versa. The INS statistical year books are used to compare different imputation techniques for assigning refugee status to immigrants. The INS statistical year books are also used to calculate the state by year flow of refugees into the U.S. Finally, I use the Consolidated Federal Funds Report (CFFR) to approximate year by state expenditures on refugees in the U.S.

The choice of years in the CPS is constrained by two factors. Immigrants and their country of origin are not identified in the CPS prior to 1994, and refugee status for immigrants can only be estimated between 1950 and 2001 using the INS data.[22] Beginning in 1994, the CPS asks respondents about the country they were born in and the year they came to the U.S.[23] The CPS data consists of nationally representative repeated cross-sections corresponding to the years 1993 to 2000. The dataset includes a wide range of demographic and welfare related individual level information. The dependent variable analyzed here is Medicaid participation, which is an indicator variable that equals one if an individual is enrolled in Medicaid and zero otherwise. A complete list of variables analyzed in the paper can be found in the appendix section, *Table 2.A1*.

For the purpose of the study at hand, immigrants are defined as those admitted to the U.S. for permanent residence. To avoid the endogeniety issue of immigrants naturalizing in order to

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[22]Even though the INS dataset only span from 1972 to 2000, these datasets contain information of immigrants who arrived anytime during or before these periods.

[23]The peinusyr variable in the CPS identifies 16 different periods of entry, prior to 1950, 1950-1959, 1960-1964, 1965-1969, 1970-1974, 1975-1979, 1980-1981, 1982-1983,1996-1997, 1998-2001

receive welfare benefits, those who have naturalized since coming to the U.S. are still considered immigrants. Owing to the paucity of data and given the length of stay in the country, those who came to the U.S. before 1950 are considered natives. Refugees are a distinct subset of immigrants and identified using imputation techniques explained below. This includes those who naturalized after 1950. Finally, natives are those born in the U.S. or a U.S. territory and immigrants who arrived before 1950. The CPS may include immigrants who are not admitted for permanent residence, such as foreign students, guest workers, or even undocumented immigrants. The inclusion of these immigrants who are categorically ineligible for Medicaid likely lowers the participation rates of immigrants. Following common practice in the related literature, I take some measures to exclude potentially undocumented immigrants in the sample section. Nonetheless, it is important to keep in mind that, to some extent, the lower participation rates of immigrants is owing to the inclusion of the aforementioned non-resident immigrants. This is especially apparent when comparing the different imputation techniques used for identifying refugees.

The CFFR lists yearly federal expenditures by programs; I use data from 1993 to 2000 to approximate all year and state specific federal expenditures on refugees. From the INS statistical year books, I calculate total year and state specific flow of refugees for the years 1993 to 2000.[24] I then divide the federal expenditure variable by the refugee flow variable to construct a state and year specific per head expenditure on refugees. This variable proxies for federal resources available to refugees and is adjusted for the varying number of refugees in each state.

3.1 Identifying Refugees

I replicate the process outlined in Bollinger and Hagstrom (2008) to estimate the probability that a given immigrant in the CPS dataset is a refugee. I then use these probabilities to impute

[24]Given that the CPS March supplement is carried out earlier in the year, I use the annual 1993 refugee flow data and the annual federal expenditure data for the cps year 1994.

refugee status to immigrants in the CPS. There are two types of immigrants captured in each one of the 27 INS datasets. The 1972 INS dataset, for instance, contains all immigrants that arrived in the year 1971 and 1972 and applied for permanent residency upon entry. The same dataset also contains other immigrants who arrived at various years before 1971 and for some reason waited until 1972 to adjust their status to permanent residents. For both types of immigrants, the INS dataset provides information on the year and immigration status at initial entry into the U.S. Immigration status at entry can be used to identify refugees in the datasets. The 27 INS datasets also include useful demographic data on country of origin, year of entry into the U.S., age at entry, and gender.

Combining the 27 different INS datasets, I construct a universe of all immigrants admitted to the U.S., by their year of entry. Next, for each CPS time period, gender, and country group with sufficient observations, I estimate individual probit regressions with refugee status as the dependent variable and age and square of age as the independent variables.[25] Out of a total of 6162 country, time, and gender groups, 1541 of them yield valid slopes and intercepts. Excluding less than 1 percent of the outliers, the relationship between age and refugee status is typically negative. Groups without a valid slope or intercept are either because there are no refugees from that country in the given period or because all the immigrants are refugees. In order to accommodate for countries where all or too few immigrants came as refugees, I also calculate, for each time period, gender, and country group, individual ratios of refugees to total immigrants. Finally, I merge the slope, intercept, and refugees to immigrants ratios with the CPS dataset by immigrants' gender, time period of entry, and country of origin. I use the estimated age at entry for immigrants in the CPS along with their corresponding slope and intercepts to calculate

[25]Probit regressions were estimated for country, year and gender groups with at least 4 refugees and 4 non-refugee immigrants.

predicted probabilities of refugee status.[26] For those immigrants belonging to time period, gender, and country groups with all, none, or too few refugees, I use the ratio of refugees to immigrants as the probability of refugee status. I then use these predicted probabilities to impute refugee status. Those immigrants with predicted probabilities equal to or greater than 0.5 are imputed as refugees.[27] The mean probability for those imputed as refugees with this imputation technique is 0.83. For the remainder of the paper, this imputation type is referred to as the BH method.

For comparative purposes, I also use an alternative method of imputing refugee status. Following Borjas (2002) and Cortes (2004), this imputation method is based on the 13 main refugee sending countries (henceforth referred to as the Borjas method). All immigrants from the following countries, regardless of their time of or age at entry, are imputed as refugees: Afghanistan, Cuba, the Soviet Union, Ethiopia, Cambodia, Laos, Bulgaria, Czechoslovakia, Hungary, Poland, Romania, Thailand, and Vietnam. The relative performance of the two imputation strategies is discussed in Appendix B.

3.2 Welfare Waivers

Before the 1996 welfare reforms, the majority of Medicaid participants were automatically enrolled for the program if they were already enrolled in Aid to Families with Dependent Children (AFDC). With the introduction of Temporary Assistance to Needy Families (TANF), the welfare reform decoupled Medicaid and cash assistance (Medicaid Section 1931). In the pre-reform period, while AFDC was still in effect, the program was administered by states but was subject to federal regulations. Since 1962, a waiver systems existed through which some federal

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[26]Since the CPS identifies periods of entry rather than exact dates I take the mid-point of the time period as the year of entry.

[27]While Imputations based on more stringent (>=0.7) and generous (>=0.3) thresholds were also experimented with the resulting measurement error was the least for the >=0.5 threshold reported in the analysis. Measurement errors, between the imputations differing in the probability thresholds, were compared in the same way as the BH and Borjas imputation methods detailed in the appendix.

requirements could be waived for states proposing experimental changes that furthered the goals of the AFDC system. These waivers became increasingly common after 1993 up until the welfare reform in 1996 (CEA report, 1997). An appendix table in the 1997 CEA report identifies any one of six major waivers used by states along with the waivers' starting dates. These six waivers include: termination and work-requirement time limits, reduced JOBS (Job Opportunities and Basic Skills) exemptions, increased JOBS sanctions, family caps, and increased earnings disregards. Each one of these waivers were designed to provide welfare recipients with incentives to work and not remain dependent on welfare. Not surprisingly, use of these waivers also resulted in reduction of welfare caseload. Because Medicaid was coupled with AFDC, it could be expected that Medicaid caseloads may have also been affected by the use of waivers.

I use information provided in the appendix table of the 1997 CEA report to identify states that opted for any one of the six state-wide waivers along with the year of waiver implementation prior to the 1996 reform date. Following Moffit (1999), I use information on the waivers to code a dummy variable indicating whether the state had any one of the six waivers in effect in the year in question. On the year of waiver implementation, the dummy variable goes from 0 to 1 and remains at 1 until the end of the study period.[28] A total of 35 states implemented at least one of the six major waivers in the period before the 1996 welfare reform. Because waiver-states were already making adjustments to their AFDC programs, the changes brought by the welfare reforms may not have been as unfamiliar as those witnessed by non-waiver states. While the post-reform outcomes in non-waiver states can be seen as resulting from a mixture of changes to AFDC and Title IV, those in waiver states are more likely to have been dominated by the latter.

3.3 Data Description

In this paper, I focus on children and working age adults below the age of 65 and from

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[28] Again, following Moffit (1999), in the year in which the waiver first appears, the variable is coded as the fraction of the year that the waiver was in effect.

households below 200 percent of the poverty line.[29] The resulting sample size consists of 283,602 individuals. Immigrants as defined in this paper make up 8 percent of the sample. Based on the BH (Borjas) imputation method refugees represent 1.2 percent (1.5 percent) of the total sample. Unless stated otherwise, statistics on refugees hereafter are based on the BH imputation method. I calculate overall Medicaid participation rates as the ratio of those enrolled in Medicaid divided by total population. An implication of limiting the sample to potentially eligible families is that the Medicaid participation rate is likely to be an underestimate of the actual take-up rate. For example, although I only consider here those who are at 200% or below the poverty level, Medicaid participants also include those who are above the threshold. About 31 percent of the sample report being enrolled in Medicaid. That participation rates are the highest among refugees (38 percent) is consistent with the aforementioned theory that refugees face lower transaction costs in participating in means-tested programs. Just over 64 percent of the entire sample reported having some form of private insurance. Slightly more than half the sample consists of females; the majority of individuals have less than a high-school level of education and almost three fourth of the sample reside in metropolitan areas. Given that almost 40 percent of the sample is children, it is not surprising to find that about 63 percent of the sample consists of individuals who have never been married. Those who are married make 24 percent of the sample. Race is dominated by whites (75 percent), followed by blacks at 19 percent and Asians at 3.5 percent. Finally, 22 percent of the sample reported having Hispanic roots.

Table 2.2 provides a summary of the key variables in the pre (1993-96) and post-reform period (1997-2000). A comparison of statistics on relevant variables in the two periods is a good starting point in unraveling the curious case of refugees' fall in Medicaid participation. The total number of immigrants in the post-reform period is 0.3 percentage points higher than in the pre-

[29]Those above sixty five qualify for Medicare and may also be enrolled in Medicaid as part of their Medicare coverage. The sample is chosen to avoid any confounding effects of the two programs.

reform period. The refugee population remains constant in the two periods. Overall Medicaid participation fell by 1.3 percentage points while private insurance increased by 0.8 percentage points in the post-reform period. This drop in Medicaid participation following PRWORA, although small, is consistent with the findings of existing literature, e.g., Fix and Passel (2002), Mazzolari and Gordon (2004).

Changes in overall participation rates are miniscule and not very revealing of the heterogeneous impact the reforms may have had on various groups. *Table 2.3* provides a summary of changes in Medicaid participation in the two periods for different groups based on immigration status. Participation rates for refugees fell by about fifteen percentage points following the reforms of 1996. This stands out especially when compared to the 2 and 1 percentage point drop among non-refugee immigrants and natives respectively. In comparing the mean participation rates among those potentially eligible, it seems as though in comparison to the general population refugees disproportionately failed to take up Medicaid in the post-reform period.

Differences in Medicaid participation rates by immigration status is further elucidated by the graph on the left in *Figure 2.1* which plots the yearly mean Medicaid participation rates for natives, refugees, and non-refugee immigrants. Also superimposed on the graph are the yearly mean unemployment rates for each state. The unemployment rates fall continuously throughout the period of study which is consistent with the improving economy. It is interesting to note from looking at the graph that the overall participation rate and those for natives and non-refugee immigrants seem to be more or less static through the period and most likely uncorrelated with unemployment rates. The participation rates for refugees, however, are far more dynamic and the subsequent fall in these rates are most likely correlated with falling unemployment rates.[30] In the

[30]The graph on the right in figure 1 shows the same plot but using the Borjas imputation method. The participation level and the changes in these levels are both dwarfed in comparison to the earlier graph. This is not surprising, as using the Borjas method classifies more non-refugee immigrants as refugees and the former have the lowest participation rates among the three groups analyzed here.

following sections, I attempt to disentangle the relative effects of various factors—including those from chilling—that may have led to the fall in refugees' Medicaid participation.

4 Methods

The sections above suggest a number of important factors relating to the refugees' Medicaid participation. The analysis, however, does not answer the question as to whether or not Title IV of PRWORA resulted in chilling among refugees. Addressing these questions requires a regression-based analysis where one can tease out marginal effects of individual variables while holding other relevant factors constant. There are a few obstacles in resorting to a regression model that need to be addressed.

Foremost, Medicaid eligibility rules regarding immigrants vary considerably across clusters of states and may not be successfully captured by state fixed effects.[31] Post-reform Medicaid eligibility for even natives varies considerably across groups of states. Inclusion of undocumented immigrants in the CPS who are categorically ineligible for Medicaid further complicates the situation. It is important to purge the sample of any undocumented immigrants. Finally, the lack of refugee identifiers is another confounding factor. Identification of refugees in the paper is based on using country of origin, gender, and age at entry as instruments which yields probabilities for immigrants being refugees. As pointed out by Bollinger and Hagstrom (2008), these probabilities should not be directly used as regressors, and as such, the model for Medicaid participation needs to be adjusted appropriately. Each one of these three issues is addressed

[31]In summary of State Programs for Immigrants; 50 states offer federally funded coverage for pre 8/22/96 qualified immigrants and 42 states offer the same for post 8/22/96 qualified immigrants after the five-year bar. During the five-year bar 22 states offer state-funded program for immigrants. Only 19 states offer state-funded programs for children during the five-year bar through Medicaid, SCHIP or both. Families, seniors and people with disabilities, during the five-year bar, are supported by state-funded programs in 14 states. Nineteen states offer state-only funded program for pregnant women during the five-year bar. Finally,13 states offer state-funded programs to all documented immigrants ineligible for Medicaid or SCHIP during the five-year bar.

below.

Part of the concern with varying eligibility for Medicaid across states is that the base group of natives, to which refugees and non-refugee immigrants are compared, may differ in more ways than those identified in the data. To address this concern, I further stratify the model by limiting the analysis, first, to only women and children and then only to children. Eligibility requirements are more homogeneous across states for women and children. The conclusions based on these more homogenous sample are not as robust, but remain consistent with the results presented here. These results are further discussed in the appendix section. Here, the sample includes those at 200 percent and below the poverty level. This again helps to restrict the sample to those potentially eligible. To overcome potential complications that may result from undocumented workers in the CPS, I exclude Central American immigrants and those from Mexico who are below forty years old and have less than a high-school education. Regarded as standard in previous literature, the above step attempts to purge the sample of illegal immigrants based on undocumented immigrant profiles.

Finally, as in the descriptive section, I use the predicted probabilities to impute refugee status. Those immigrants with predicted probabilities equal to or greater than 0.5 are imputed as refugees.[32]

4.1 Theoretical Framework and Model Specification

I estimate a reduced form of the model proposed in Moffit (1983) where labor supply, income, and participation are functions of demographic characteristics and are also potentially related to policy changes. The reduced form of the model is further modified to accommodate for the choices individuals have with respect to health insurance types. At the outset, it is unclear

[32]For comparative purposes I also estimate the samples using refugee identifiers based on the Borjas imputation method. The qualitative results remain the same but coefficient estimates on refugee related variables are biased downwards. These results are not presented here but are available upon request.

whether a fall in Medicaid participation is a good or bad thing. On the one hand, more people acquiring private health insurance may be evidence of a positive outcome. A drop in Medicaid participation followed by an increase in the uninsured population, on the other hand, implies the opposite. Therefore, instead of modeling Medicaid participation as a binary outcome, I model health insurance as a choice between private, Medicaid and none. I estimate the following multinomial logit model for health insurance type:

$$\Pr\{y_i = Medicaid \mid \mathbf{W}_i\} = \frac{\exp(\mathbf{W}_i \boldsymbol{\beta}_{medicaid})}{1 + \exp(\mathbf{W}_i \boldsymbol{\beta}_{private})} \tag{1}$$

where on the left-hand side of the equation is the probability of an individual participating in Medicaid. The matrix $\mathbf{W}_i\boldsymbol{\beta}$ on the right includes the following set of variables and related coefficients:

$$\mathbf{W}_i\boldsymbol{\beta} = \gamma I_i + \delta R_i + \lambda reform + \alpha(R_i * reform) + \theta(I_i * reform) + \mathbf{X}_i \tag{2}$$

I_i is a dummy variable indicating a non-refugee immigrant, and R_i is another indicator variable for refugee status. The variable $reform$ is an indicator for the post-reform period followed by the interaction term between refugee status and the reform variable. The vector $\mathbf{X}_i$ includes a list of control variables. The model allows for the estimation of two sets of coefficients, one for Medicaid participation and another set for private health insurance with both relative to no insurance. The above model represent a difference-in-difference (DD) specification, where I examine the Medicaid participation among refugees in the periods before and after the 1996 welfare reform, relative to natives. Since the coefficient estimates from a multinomial logit model are not marginal effects, I estimate marginal effects at the mean, separately for Medicaid and private insurance. In all the estimations above and those to follow, I cluster standard errors at the state level and use the BH imputation method to identify refugees.

Analyzing the behavior of refugees relative to a base of native workers helps to overcome any biases that may result from the effects of temporal changes in aggregate labor market conditions.

The use of a comparison group helps to net out any such aggregate effects. Natives, more so than non-refugee immigrants, make a good comparison group because natives and refugees face similar eligibility requirements before and after the reforms. One key assumption in the above specification is that after controlling for observable characteristics, natives make a valid comparison group.

First, I estimate marginal effects for a pair of base models, one with no control variables and another with only individual demographic characteristics and state fixed effects as control variables. In the next set of models, I add potentially relevant variables to the list of controls one at a time. The final model is the full difference-in-difference model which includes all the variables explored.

In the base model, absence of variables that could impact Medicaid participation and also be correlated with the independent variables of interest is also likely to bias the corresponding estimates. The addition of omitted variables in the form of controls and relevant interaction terms hopefully remedies this bias. In the full model, I include individual and family level demographic variables, local MSA or PMSA level unemployment rates, state fixed effects, and federal expenditures on refugees. Identification here relies on the assumption that once the appropriate demographic and economic variables are controlled for, the parameter estimates of $\gamma, \delta, \lambda, \theta$ and α are unbiased and close to the true parameters. If the estimates of λ and α are no longer significant after the inclusion of controls in the second set of models, it can be concluded that any difference in Medicaid participation between refugees and other groups can be explained using individual, family-level, and state-level characteristics. If, however, these parameters are persistently negative and statistically and quantitatively significant, even after the inclusion of additional controls, then we cannot readily reject the hypothesis that refugees may have been chilled into non-participation by Title IV of the PRWORA. In this case, the related coefficient estimates from the private insurance regression are also of interest. If there is a comparable unexplained increase in take up of private insurance, relative to no insurance, then it may be the case that refugees are leaving Medicaid to take up private health insurance. Although the reason behind leaving Medicaid may still be a case of chilling, the end outcome is not necessarily unfavorable. The absence of an increase in private insurance and a persistent drop in Medicaid, however, indicates a much worse outcome from the potential chilling effect.

The above DD models do not incorporate the potentially important effects of the welfare waivers. In order to exploit potential effects of the timing and take-up of welfare waivers, in a separate set of regressions, I modify the matrix $\mathbf{W}_i\boldsymbol{\beta}$ in equation 1 such that:

$$\begin{aligned}\mathbf{W}_i\boldsymbol{\beta} = \gamma I_i + \delta R_i + \lambda reform + \alpha\left(R_i * reform\right) + \eta_1 waiver_{st} + \eta_2(waiver_{st} * R_i) + \\ \eta_3(waiver_{st} * I_i) + \eta_4(waiver_{st} * R_i * reform) + \eta_5(waiver_{st} * I_i * reform) + \mathbf{X}_i\end{aligned} \tag{3}$$

where $waiver_{st}$ indicates whether or not a given state s has implemented a waiver in year t . The above modification results in a triple difference model where I compare the change in Medicaid or private health insurance (relative to natives) before and after the reforms and between states with and without welfare waivers. In this triple difference model, estimates of α and η_4 now represent the post reform change for refugees in states that did not opt for waivers and those states that did, respectively. Marginal effects for the triple difference model are again estimated separately for Medicaid and private health insurance.

5 Results

I begin with the results from the DD model explained in equation 2. I report marginal effects for the probability of participating in Medicaid and private health insurance rather than having no insurance in *Table 2.4* and *Table 2.5* respectively. In both the tables, the first two models are the base models with none and only individual demographics and state fixed effects as controls, respectively. Models 3 and 4 are extensions of the second base model which include separate, additional demographic and economic variables. In Model 3, I include local unemployment rates and related interaction terms. Model 4 includes an interaction term for refugee and annual state federal expenditure per refugee. Model 5 is the full model with all the variables. I only report here parameter estimates of primary concern.[33]

For all regression estimates in the interaction terms of refugee status with time spent in the United States and local unemployment rates, the local unemployment variable is a deviation from the mean unemployment rate while the duration in the U.S. is zero for all natives.[34] I also use deviations from the mean annual per head federal expenditures by states in the related interaction term with refugee status. The re-parameterizations allow for easier interpretations of the refugee indicator variable. The interaction terms are included in the model because it is suspected that time spent in the U.S., federal expenditures on refugees, and local unemployment rates affect Medicaid participation differently for refugees than for non-refugee immigrants and natives respectively. The coefficient on the refugee indicator variable after the re-parameterization is a measure of the relative propensity to participate for a refugee compared to a native at the mean unemployment rate and mean state specific per head federal expenditures on refugees.

In *Table 2.4* for models 3 and 5, it is interesting to note that on average unemployment rates are quantitatively insignificant predictors of Medicaid participation. This is consistent with the finding that unlike Food stamp participation rates, those for Medicaid (on average) are not cyclic with respect to economic conditions. Refugees' Medicaid participation, however, remains fairly responsive to unemployment rates. The estimates corresponding to the refugee status variable from model 3 in *Table 2.4* suggest that given mean unemployment rates and all else the same, newly arrived refugees are about 25 percentage points more likely to participate in Medicaid than

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[33]The same models are also estimated using the Borjas imputation method but not included in the paper. In general the coefficient estimates on refugee related variables obtained using the Borjas imputation method are smaller in magnitude compared to the BH method.

[34]As such the variable for duration in the country should be understood as an interaction between an immigrant (both refugee and non-refugee) dummy and time in the US.

natives. As for per head federal expenditures on refugees, the corresponding estimate in model 4 shows that at the mean state level, per head refugee expenditures refugees are 35 percentage points more likely to participate in Medicaid than natives. The coefficient estimates on the interaction between refugee and duration in the U.S. suggest that refugees in comparison to non-refugee immigrants are more likely to stop participating in Medicaid over time. Every additional year in the country reduces refugee participation rates by 0.01 percentage points.

I now turn to the main variables of interest in models 1 through 5, reported in *Table 2.4*. Marginal effects from the first base model points to a 10.5 percentage point raw difference in the fall in Medicaid participation rates between refugees and natives in the post-reform period. In the second model, even after controlling for demographic variables and state fixed effects, a post-reform fall of 8.7 percentage points remains among refugees' Medicaid participation. Controlling for the observed differences, estimate from model 2 suggests that refugees are on average and relative to having no health insurance 37 percentage points more likely to participate in Medicaid than natives. In both the base models, there is a one percentage point fall in overall participation in the post-reform period and large negative and significant coefficients on the interaction term between the post-reform period and refugee status. These estimates, however, are likely to be biased, and this is confirmed when looking at the models 3 through 5.

After including demographic characteristics, local unemployment rates, state level variables, and relevant interaction terms, the coefficient estimate $\widehat{\alpha}$ significantly decreases in magnitude. In fact, including just the unemployment rates in model 3 results in the loss of both magnitude and statistical significance of $\widehat{\alpha}$ which suggests that the difference in Medicaid participation among refugees in the periods before and after the reform arose from individual, household, demographic, and state-level observables and local economic conditions. In model 4, where I include an interaction between refugee status and mean, yearly per-head state expenditure on refugees, the estimate $\widehat{\alpha}$ is smaller in magnitude but still quantitatively and statistically significant. The model suggests that while variations in the per-head spending on refugees do matter, they do not fully explain the differential fall in refugees' Medicaid participation in the post-reform period.

Estimates of marginal effects on the probability of participating in private health insurance relative to having no insurance reported in *Table 2.5* almost mirror the effects found in *Table 2.4*. Estimates for models 1 and 2 suggest that in the post-reform period refugees' private health insurance participation increased by six percentage points more than that for natives during the same period. In models 3 and 5 in which I control for local economic conditions, the related estimates are no longer significant. In general, participation rates for refugees decrease with their time in the U.S. and increase during instances of high unemployment rates. The DD estimates from *Tables 4* and *5* collectively indicate that title IV of PRWORA may not have been responsible for chilling refugees into non-participation. As explained in the previous section, the DD estimates, however, do not incorporate the potential effects of the welfare waiver.

Next, I turn to estimates from the triple difference models reported in *Table 2.6*. Here, I report marginal effects for the multinomial logit model in equation 1, modified per equation 3. *Table 2.6* includes marginal effects for two sets of models for each of the two outcomes, Medicaid and private health insurance. For both outcomes, model 6 excludes local unemployment, state expenditure on refugee variables, and related interaction terms. The second set of models (model 7) are the full models that include all variables explored in the present analysis. For all the models in Table 2.6, $\widehat{\alpha}$ is now the estimated differential change (relative to natives) in Medicaid participation in the post-reform period among refugees living in non-waiver states. Estimate $\widehat{\alpha}$ from the first column in Table 2.6, suggests that in the post-reform period Medicaid participation rates among refugees living in non-waiver states show insignificant changes from the pre-reform period. The full model in the second column indicates a significant and positive relative change of 11 percentage points in Medicaid participation for refugees in non-waiver states. Once more, columns 3 and 4 pertaining to private health insurance, reflect effects of equal magnitude, but in the opposite direction of the Medicaid estimates. Estimates from these two columns suggest a post-reform, relative fall in private health insurance coverage for refugees residing in non-waiver states. Collectively, these estimates suggest, not only was there no chilling among refugees, there was in fact, a post-reform increase in Medicaid participation among refugees living in the non-waiver states.

The story of refugees' Medicaid participation, in states that opted for waivers, stands in stark contrast to that in non-waiver states. In fact, the health insurance coverage patterns for refugees and non-refugee immigrants are very similar in the waiver states. The take up of at least one major waiver before the 1996s reforms is associated with a sizeable increase in the relative Medicaid participation of refugees and non-refugee immigrants. The estimates, however, are precisely estimated only for the latter. Based on estimates from columns 1 and 2 of *Table 2.6*, take up of waiver is associated with a 10 to 15 percentage point increase in refugees' Medicaid participation. The estimates also point to a post-reform, 12 to 16 percentage point drop in refugees' Medicaid participation in waiver states. The post reform, relative drop in refugees' Medicaid participation remains even after accounting for local economic conditions and state expenditures on refugees. The related coefficients for non-refugee immigrants, which tell the same story, are more precisely estimated.

Based on the above estimates alone, I am unable reject the possibility of chilling as a potential explanation for the post-reform, refugee experience in welfare waiver states. There is however, an interesting caveat that suggests the non-participation of refugees in the Medicaid program might not have necessarily been a bad outcome for refugees. This is where the marginal effects, from the private insurance outcome, help to complete the story. Estimates from the third and fourth columns of Table 2.6 suggest that refugees and non-refugee immigrants both witnessed a relative drop in private health insurance coverage (comparable to the corresponding increase in Medicaid) following the take-up of waivers. Both immigrant groups in the waiver states, also witnessed a sizeable increase in private health insurance coverage (again comparable to the corresponding drop in Medicaid) in the post-reform period. When the Medicaid and private health insurance results are viewed together in the post-reform period and among waiver states, the probability of refugees opting out of Medicaid is not significantly different from the probability of refugees opting into private health insurance. While decreasing probability of Medicaid participation coupled with an increase in probability of having no health insurance might be deemed an unfavorable outcome, a decrease in the former and an increase in the probability of private insurance is hardly the same.

6 Concluding Remarks

The findings in this paper point to a number of important details regarding both the refugee Medicaid experience in the pre- and post-1996 reform period and also refugees' Medicaid take up in general. In contrast to what initially may have seemed like an unusual case of all eligible refugees being chilled from participation, the findings here suggest a two-part story. In states that did not implement any welfare waivers, the fall in refugees' Medicaid participation is explained away by observed individual, local and state level difference. In these states, I find evidence against the chilling hypothesis. In states that did implement welfare waivers, however, there remains an unexplained fall in Medicaid participation even after accounting for improved economic conditions and other related variables. Among refugees living in these waiver states, it is possible that title IV of PRWORA may have unintentionally resulted in the decline in refugees' Medicaid participation rates. The eventual outcome of refugees in these waiver states however, is not necessarily unfavorable. The same process may have also led refugees in these waiver states to increase participation in private health insurance.

Why chilling effects are only present in the waiver states remains to be explained. It is possible, and also mentioned in Moffit (1999), that the implementations of the welfare waivers may have been endogenous. Participation rates in those states that opted for waivers might have fallen even in the absence of waivers or that both the implementation of the waivers and fall in refugee and non-refugee immigrants' Medicaid participation was driven by a third factor. Anti-immigrant sentiment, not rare during the period, is one example of a possible third factor. These sentiments, if present, could have led eligible non-refugee immigrants and even refugees to not participate in Medicaid and other welfare programs. It also remains to be explained why the take up of waivers initially led to sizeable increases in Medicaid participation among refugees and non-refugee immigrants.

With respect to refugee Medicaid participation in general, I find evidence supporting the theory that owing to lower transaction costs of participating and lower importance of stigma when refugees first arrive in the country; participation rates among refugees are much higher than non-refugee immigrants and natives. Furthermore, to the extent that participation in welfare programs like Medicaid can be seen as means to assimilating into life in the United States, the findings also support the claim that, on average, refugees assimilate to life in the U.S. more successfully than non-refugee immigrants. It is also likely that assimilation to a degree necessitates the importance of stigma associated with being on welfare over time. Consistent with this idea, I find that unlike non-refugee immigrants, refugees tend to wean themselves off Medicaid over time.

These results add to the earlier findings in Bollinger and Hagstrom (2008) regarding the refugee experience with Food Stamp. Similar to findings of the previous authors, I find local economic conditions to be strong determinants of refugees' Medicaid participation. Local economic conditions, however, do not tell the whole story about refugees' Medicaid participation. Welfare waivers are equally, if not more, important components in the analysis of the effects of the 1996 Welfare reforms on refugees.

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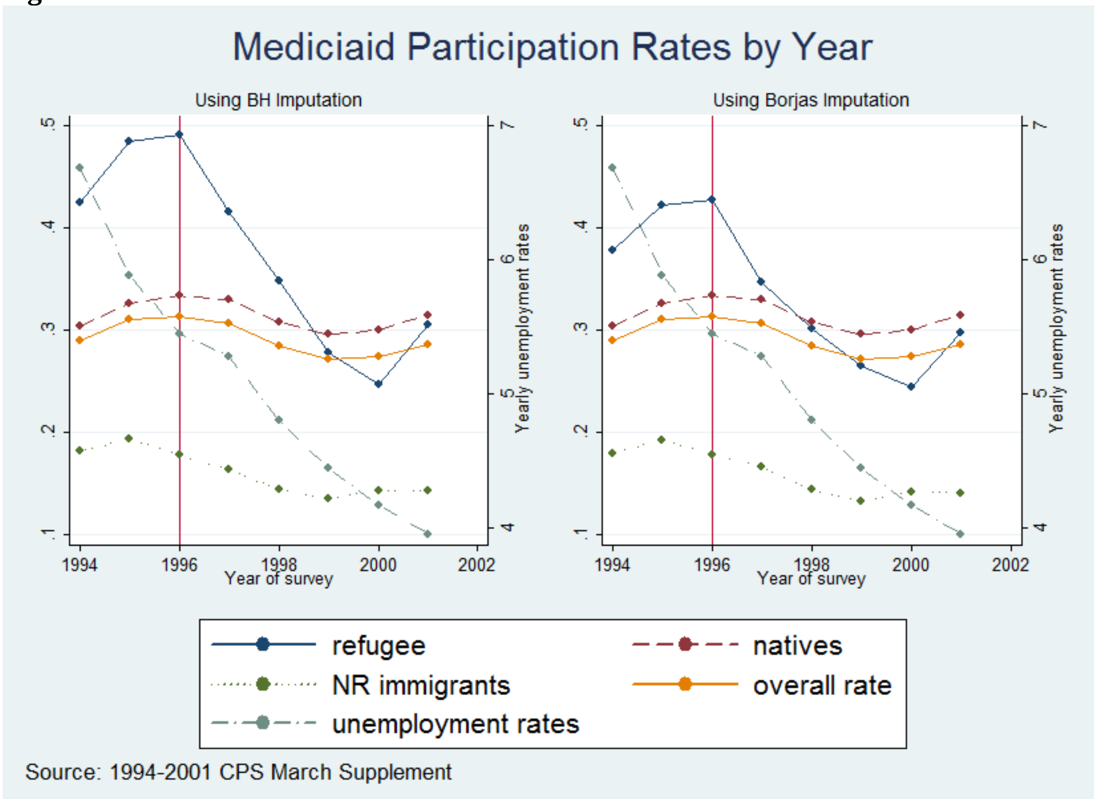
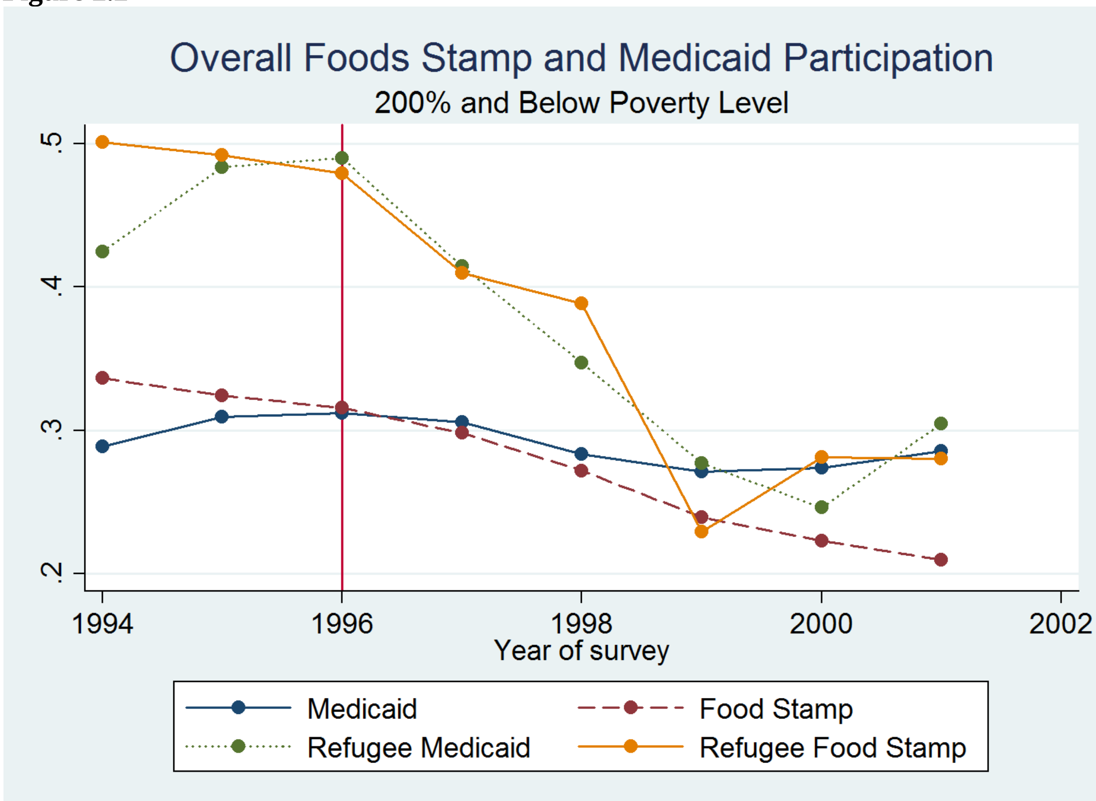
Figure 2.1



Notes: Unemployment rates plotted in the above graph are yearly averages for Metropolitan Statistical Areas (MSA), when identified and states otherwise. The other two graphs represent yearly participation rates for Medicaid separated by refugee, non-refugee and native status of individuals. The sample includes those at 200% and below the poverty line.

Figure 2.2



Notes: The two sets graphs represent yearly overall participation rates for Medicaid and Food Stamp programs and participation rates specific to refugees.

Source: Current Population Survey- March Supplement, Bureau of Labor Statistics

Table 2.1: A Comparison of recent refugees and non-refugee immigrants

Variable	Recent Refugees		Recent Non-refugee Immigrants	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**
Age	29.45	16.35	27.79	14.03
Proportion enrolled in Medicaid	0.35	0.48	0.10	0.29
Proportion enrolled in private health insurance	0.54	0.50	0.62	0.49
Proportion enrolled in Food Stamp	0.35	0.48	0.08	0.26
Proportion at 200% or below poverty line	0.67	0.47	0.49	0.50
Family earnings (1000s)	24.64	34.90	36.87	46.78
Proportion living in MSA	0.98	0.15	0.94	0.23
Observations	1712		11250	

Notes: The above table compares refugees and non-refugee immigrants that have been in the U.S. for seven or fewer number of years.

Source: Current Population Survey- March Supplement

Table 2.2 Summary Statistics on Key Variables Pre- and Post-Reform

Variables	Pre-reform (1993-1996)	Post-reform (1997-2000)	Total
	Col %	Col %	Col %
Medicaid			
Not enrolled in Medicaid	68.50%	69.80%	69.30%
Enrolled in Medicaid	31.50%	30.20%	30.70%
Private Health Insurance			
No private insurance	36.20%	35.40%	35.80%
Private insurance	63.80%	64.60%	64.20%
Gender			
Male	45.70%	45.80%	45.70%
Female	54.30%	54.20%	54.30%
Marital status			
Married	24.70%	22.80%	23.60%
Widowed	2.00%	2.00%	2.00%
Divorced	8.00%	8.60%	8.40%
Separated	3.10%	2.80%	2.90%
Never Married	62.20%	63.80%	63.10%
Education			
Less than high-school	59.40%	58.80%	59.10%
High-school graduate	22.00%	21.80%	21.90%
Some college	10.90%	10.90%	10.90%
Associate degree or higher	7.80%	8.40%	8.10%
Residential Location			
Not MSA	29.70%	27.20%	28.20%
MSA	70.30%	72.80%	71.80%
Race			
White	73.50%	75.30%	74.50%
Black	18.90%	18.40%	18.60%
American Indian	2.30%	2.70%	2.60%
Asian	3.40%	3.60%	3.50%
Other	1.90%	0.00%	0.80%
Hispanic			
Not of Hispanic origin	80.20%	76.20%	77.90%
Of Hispanic origin	19.80%	23.80%	22.10%

Table 2.2 (Continued)

Variables	Pre-reform (1993-1996)	Post-reform (1997-2000)	Total
	Col %	Col %	Col %
Immigration Status			
Refugee	1.20%	1.20%	1.20%
Non-refugee Immigrant	6.20%	6.50%	6.40%
Native	92.60%	92.40%	92.50%
Total (n= 283,602)			

Notes: The above table summarizes key variables before and after the 1996 welfare reforms. The samples used here are the same as those used in the regressions and include individuals within 200% of the poverty line.

Source: Current Population Survey- March Supplement

Table 2.3 Summary Statistics Before and After Welfare Reforms by Immigration Status

	Percent Participating in Medicaid		**Percent Covered By Private Insurance**		**Households Living Below 200% of Poverty Level**		**Full Labor Force Participation Among Households Below 200% Of Poverty Level**	
Immigration Status	pre-reform (1993-1996)	post-reform (1997-2000)	pre-reform (1993-1996)	post-reform (1997-2000)	pre-reform (1993-1996)	post-reform (1997-2000)	pre-reform (1993-1996)	post-reform (1997-2000)
Refugee	0.47	0.32	0.40	0.42	0.51	0.41	0.30	0.39
Non-refugee Immigrant	0.19	0.17	0.45	0.43	0.37	0.33	0.39	0.43
Native	0.32	0.31	0.65	0.66	0.34	0.31	0.25	0.25
Total (n= 283,602)	0.31	0.30	0.64	0.65	0.34	0.31	0.26	0.26

Notes: The table shows proportionate changes in some key variables in the period before and after the welfare reforms of 1996. The sample used in the table above is the same as Table 2.2**.**

Source: Current Population Survey- March Supplement

Table 2.4. Marginal Effects at the Mean for Medicaid Participation from Multinomial Logit Models

	Base Model 1	Base Model 2	Model 3	Model 4	Model 5
Non-Refugee(NR) Immigrants	-0.125*** (0.0168)	-0.103*** (0.0248)	-0.125*** (0.0201)	-0.102*** (0.0248)	-0.124*** (0.0201)
Refugees	0.141 (0.0994)	0.374*** (0.0799)	0.245*** (0.0669)	0.346*** (0.0803)	0.578*** (0.138)
Post Welfare Reform Period	-0.0102* (0.00490)	-0.0116* (0.00575)	-0.000362 (0.00670)	-0.0110 (0.00579)	0.000277 (0.00673)
Post Reform and Refugee Interaction	-0.105** (0.0367)	-0.0865*** (0.0261)	0.0273 (0.0354)	-0.0632* (0.0306)	0.0353 (0.0326)
Post Reform and NR Immigrants Interaction	-0.0136 (0.0150)	-0.00595 (0.0159)	0.0252 (0.0153)	-0.00524 (0.0158)	0.0268 (0.0151)
Refugee and Duration in the US Interaction		-0.0114*** (0.00149)	-0.0114*** (0.00130)	-0.0114*** (0.00146)	-0.0113*** (0.00132)
Local Unemployment			0.00764*** (0.00227)		0.00762*** (0.00227)
Refugee and Unemployment Interaction			0.0594*** (0.0111)		0.0559*** (0.0114)
NR Immigrants and Unemployment Interaction			0.0153** (0.00554)		0.0157** (0.00546)
Refugee and State Per Head Spending Interaction				-0.0809* (0.0370)	-0.0464 (0.0340)
Demographic Variables	NO	YES	YES	YES	YES
State Fixed Effects	NO	YES	YES	YES	YES
Observations	283602	283602	283602	281806	281806

Notes: The marginal effects reported above are in reference to the probability of being enrolled in Medicaid, relative to no insurance. Demographic variables in the control include age, number of children and indicators for residence in metropolitan area, marital status, education levels, veteran status, race, Hispanic origin and gender. The samples in models 1,2 and 3 exclude all individuals living in the state of Wyoming, and immigrants from Central America and Mexico who are below 40 years of age and have less than high school level of education. The samples in models 4 and 5, in addition to the above exclusions, also exclude observations in the state of Alaska which along with Wyoming has no data on state level expenditures on refugees. Standard errors are reported directly below in parenthesis and clustered at the state level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.5. Marginal Effects at the Mean for Private Insurance Participation from Multinomial Logit Models

	Base Model 1	Base Model 2	Model 3	Model 4	Model 5
Non-Refugee(NR) Immigrants	-0.0638** (0.0234)	-0.00667 (0.0290)	0.0290 (0.0204)	-0.00779 (0.0289)	0.0285 (0.0205)
Refugees	-0.211*** (0.0488)	-0.348*** (0.0576)	-0.249*** (0.0510)	-0.330*** (0.0590)	-0.465*** (0.125)
Post Welfare Reform Period	0.00688 (0.00425)	0.0109* (0.00495)	-0.00216 (0.00555)	0.0108* (0.00499)	-0.00218 (0.00560)
Post Reform and Refugee Interaction	0.0663** (0.0214)	0.0590** (0.0198)	-0.0527 (0.0320)	0.0422 (0.0276)	-0.0553 (0.0299)
Post Reform and NR Immigrants Interaction	-0.00995 (0.0170)	-0.0125 (0.0207)	-0.0532* (0.0207)	-0.0135 (0.0206)	-0.0552** (0.0205)
Refugee and Duration in the US Interaction		0.0101*** (0.00122)	0.0100*** (0.00112)	0.0101*** (0.00123)	0.00998*** (0.00115)
Local Unemployment			-0.00895** (0.00283)		-0.00890** (0.00283)
Refugee and Unemployment Interaction			-0.0584*** (0.0123)		-0.0560*** (0.0131)
NR Immigrants and Unemployment Interaction			-0.0197** (0.00660)		-0.0202** (0.00653)
Refugee and State Per Head Spending Interaction				0.0653 (0.0419)	0.0306 (0.0387)
Demographic Variables	NO	YES	YES	YES	YES
State Fixed Effects	NO	YES	YES	YES	YES
Observations	283602	283602	283602	281806	281806

Notes: The marginal effects reported above are in reference to the probability of being enrolled in private insurance, relative to no insurance. Standard errors in parentheses are clustered at the state level * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.6 Marginal Effects for Medicaid and Private Insurance Participation from Multinomial Logit Models

	Model 6 (Medicaid)	Model 7 (Medicaid)	Model 6 (Private Insurance)	Model 7 (Private Insurance)
Non-Refugee(NR) Immigrants	-0.131*** (0.0132)	-0.151*** (0.0123)	0.0194 (0.0264)	0.0521** (0.0182)
Refugees	0.291*** (0.0732)	0.185** (0.0588)	-0.302*** (0.0574)	-0.219*** (0.0529)
Post Welfare Reform Period	-0.0151* (0.00697)	-0.00365 (0.00803)	0.0129* (0.00545)	0.000734 (0.00619)
Post Reform and Refugee Interaction	0.0138 (0.0500)	0.112* (0.0578)	-0.0288 (0.0583)	-0.116 (0.0605)
Post Reform and NR Immigrants Interaction	0.0368* (0.0168)	0.0654*** (0.0168)	-0.0705** (0.0224)	-0.107*** (0.0219)
Refugee and Duration in the US Interaction	-0.0112*** (0.00167)	-0.0111*** (0.00150)	0.00991*** (0.00136)	0.00982*** (0.00127)
Local Unemployment		0.00780*** (0.00226)		-0.00899** (0.00284)
Refugee and Unemployment Interaction		0.0531*** (0.0117)		-0.0547*** (0.0127)
NR Immigrants and Unemployment Interaction		0.0150*** (0.00430)		-0.0193*** (0.00566)
Refugee and State Per Head Spending Interaction		-0.0435 (0.0335)		0.0273 (0.0383)
State Opted for At Least One Major Waiver Before Reform	0.0122 (0.0107)	0.0144 (0.0110)	-0.00724 (0.00899)	-0.0106 (0.00896)
Refugee and State Waiver Interaction	0.156 (0.120)	0.104 (0.0882)	-0.0981 (0.118)	-0.0514 (0.0849)
Immigrant and State Waiver Interaction	0.0898* (0.0416)	0.0908** (0.0341)	-0.0808 (0.0503)	-0.0802* (0.0390)
Refugee, State Waiver and Post Reform Interaction	-0.162*** (0.0447)	-0.124* (0.0518)	0.124 (0.0673)	0.0797 (0.0688)
Immigrant, State Waiver and Post Reform Interaction	-0.0878*** (0.0230)	-0.0825*** (0.0236)	0.108*** (0.0307)	0.101*** (0.0293)
Demographic Variables	YES	YES	YES	YES

State Fixed Effects	YES	YES	YES	YES
Observations	283602	281806	283602	281806

Notes: The marginal effects reported above are in reference to the probability of being enrolled in Medicaid (columns 2 and 3) and private insurance, (columns 4 and 5) relative to no insurance. Standard errors in parentheses are clustered at the state level * $p < 0.05$, ** $p < 0.01$, *** $p < 0.0$

Appendix 2.A: Robustness of Results

Empirical Strategy

The robustness of the above results are examined across multiple dimensions including, functional forms and varying samples. A possible area of concern with all the above regression analysis is that one of the main right hand side variables of interest is an imputed variable (refugee status). While I argue that the identification technique used in this paper is a novel addition to the literature, the ramifications of using an imputed variable is still a concern. For completeness, I re-estimate the models for both the samples using the full maximum likelihood specification outlined in Bollinger and Hagstrom (2008). In their model for Food Stamp participation the authors modify a probit specification to make use of the refugee probabilities estimated from the INS dataset itself. As such the regression analysis is free of any imputation. The new estimated model is given by:

$$\Pr\{Medicaid = 1\} = F(D_i\beta + \gamma A_i + \delta)\Pr\{R_i = 1\} + F(D_i\beta + \gamma A_i)\Pr\{R_i = 0\}$$

Where F() is the cdf of a normal and Pr(Ri=1) is the probability of an immigrant being a refugee calculated from the INS dataset. The above model can be estimated using maximum likelihood methodology. Estimates from the model are reported in *Table 2. A2*. The coefficients from the above regression are not marginal effects and thus cannot be directly compared to the marginal effects from the multinomial logit models from the main results. For the purpose of checking the robustness of the results in the earlier section, however, the signs and magnitude of the coefficients in *Table 2.A2* may be used as guides. The coefficients confirm the same overall findings from the earlier models. The raw difference in refugee Medicaid participation pre- and post-welfare reforms is completely explained by individual, household, local, and state-level observables for those states that did not use welfare waivers prior to the 1996 welfare reforms. For states that did use welfare reforms, an unexplained fall in the relative Medicaid participation rates among refugees remains in the post reform period.

The difference-in-difference and triple difference estimates from the main analysis are based on the assumption that natives and non-refugee immigrants make valid comparison groups for refugees. The violation of this assumption is another concern in the current analysis. Medicaid eligibility rules vary from state to state and limiting the analysis to those at 200% and below the poverty line does not ensure that everyone in the sample is eligible for Medicaid. Furthermore, inclusion of non-residential immigrants that are systematically ineligible for Medicaid also adds to the concerns about the comparison groups. I attempt to mitigate these concerns by looking at sub-samples of the population for whom Medicaid eligibility is more homogenous across all the states. This involves limiting the sample to only women and children. In *Table 2. A3*, I report marginal effects from three versions of the full model in *Table2.6* of the main analysis, by limiting the sample to only women, children and both. Limiting the sample in this way, greatly reduces the already small number of refugees in the dataset. The small sample size for refugees manifest in the lack of precision with which the marginal effects are estimated. The estimates themselves however, with the exception of those for only children, reflect the same patterns from the main model.

Appendix 2.B: Comparing the Imputation Methods

The BH and Borjas imputation methods classify 7705 and 10604 immigrants from the dataset as refugees, respectively. A total of 342 refugees, identified using the BH method, come from countries other than the 13 used to impute refugee status in the Borjas method. Finally 3241 of the refugees identified by the Borjas method are classified as non-refugees using the BH method.

In imputing refugee status, there are two types of possible measurement errors; one could impute a non-refugee immigrant as a refugee (henceforth referred to as type 1) or incorrectly classify a refugee as a non-refugee immigrant (type 2). In the following exercise, I demonstrate that both types of errors are lower for the BH imputation method compared to the Borjas method. *Table 2.A4* shows a list of countries that are considered to be refugee-sending countries based on one or both the imputation methods for the time period 1986-87. The countries in bold text are those identified as one of the 13 refugee prone countries by the Borjas method. The second and third columns list the number of immigrants that are imputed as refugees using the BH and Borjas methods respectively. The fourth column lists the country-specific means of predicted probabilities for immigrants being refugees. Poland, for instance, was the source country for the 78 immigrants who came to the US in 1986-87 and were interviewed in the CPS. Based on column four, roughly 40% of the immigrants from Poland in the time-period arrived as refugees. Using the Borjas method all 78 immigrants are classified as refugees even though more than half were likely to be non-refugee immigrants. The BH imputation method, which takes into account the year, age at entry, and gender classifies only 15 of the immigrants from Poland as refugees, thus reducing the type 1 error. Again, based on column four, almost forty percent of the immigrants from Iran were refugees. The Borjas method fails to identify any one of sixty-two Iranian immigrants in the CPS as refugees. The BH method on the other hand identifies seven refugees from the country which suggests that compared to the BH method there is relatively greater type 2 errors in the Borjas method. The impacts of the differences in relative measurement errors between the two imputation methods are evident in the descriptive analysis. In general, Medicaid participation rates for refugees under the Borjas imputation method are much lower than those for the BH method.

Table 2.A1: List of Variables

Variable	Definition
Individual Level observables	
Medicaid	Dummy variable equals 1 if individual is enrolled in Medicaid
Private insurance	Dummy variable equals 1 if individual is enrolled in some Private Insurance
Age	Age of the individual
Square of age	Square value of the individual's age
Marital Status	Categorical variable indicating marital status
Education	Categorical variable indicating an individual's completion of less than high-school, high-school, some college and associate college or higher level of education
Army veteran	Dummy variable equals 1 if individual is an Army veteran
Race	Categorical variable indicating White, Black, American Indian, Asian or some other race
Of Hispanic origin	Dummy variable equals 1 if individual identifies being of Hispanic origins.
MSA	Dummy variable equals 1 if individual leas in an MSA
length of stay in the US	Continuous variable derived the CPS, estimates the number of years since immigration for international immigrants.
Female	Dummy variable equals one if the individuals is a female
Probability of being a refugee	The estimated probability that a given immigrant in the March CPS is a refugee
Refugee probability greater than or equal to 0.5	Dummy variable equals 1 if a given immigrants is considered a refugee using the BH imputation method.
Refugee status by Borjas Method	Dummy variable equals 1 if a given immigrants is considered a refugee using the Borjas imputation method.
Immigrant	Dummy variable equals 1 if the individual arrived in the US form a foreign country of birth after year 1950.
Non-refugee immigrant (BH method)	Dummy variable equals 1 if individual is an immigrants but not a refugee per the BH method

Non-refugee immigrant (Borjas method)	Dummy variable equals 1 if individual is an immigrants but not a refugee per the Borjas method
Family level variables	
Total number of children under six	Continuous variable measuring number of children in the family.
Total number of children between 7 and 18	Continuous variable measuring number of children in the family between the ages of 7 and 18
Below 200% of poverty level	Dummy variable equals 1 if the family income levels place the family 20% below the poverty level
Family income level	Categorical variable with 16 ranges of family Income values
State and local level variables	
Local unemployment levels	Yearly Unemployment rates at the local Metropolitan Statistical Area level
Yearly State per head expenditures on refugees	Ratio of all yearly refugee related state expenditures and yearly flow of refugees into the state.
State waiver	Variable indicates whether the state had any waiver in effect in the year in question and In the year in which the waiver first appears, the variable is coded as the fraction of the year that the waiver was in effect.

Table 2.A2: Regression Results from the MLE Model

	Base Model 1	Base Model 2	Model 3	Model 4	Model 5	Model 6
Immigrants	-0.423***	-0.321***	-0.351***	-0.318***	-0.419***	-0.452***
	(0.079)	(0.057)	(0.052)	(0.057)	(0.038)	(0.046)
Refugees	0.947**	1.494***	1.147***	1.414***	1.340***	1.077***
	(0.321)	(0.289)	(0.221)	(0.284)	(0.234)	(0.218)
Post welfare reform period	-0.029*	-0.029	0.001	-0.027	-0.038*	-0.007
	(0.014)	(0.016)	(0.018)	(0.016)	(0.019)	(0.022)
Post reform and refugee Interaction	-0.341*	-0.232*	0.088	-0.156	0.116	0.367*
	(0.16)	(0.11)	(0.121)	(0.113)	(0.174)	(0.186)
Refugee and duration in the US interaction		-0.036***	-0.034***	-0.036***	-0.035***	-0.033***
		(0.006)	(0.005)	(0.006)	(0.006)	(0.005)
Deviations from mean Unemployment rate			0.021***			0.021***
			(0.006)			(0.006)
Refugee and unemployment interaction			0.156***			0.136***
			(0.039)			(0.036)
Immigrants and unemployment interaction			0.014			0.014
			(0.014)			(0.009)
Refugee and deviation from mean state per head spending interaction				-0.246**		-0.123
				(0.095)		(0.098)
State opted for at least one major waiver before reform					0.032	0.038
					(0.029)	(0.03)
Refugee and state waiver interaction					0.297	0.126
					(0.244)	(0.202)
Immigrant and state waiver interaction					0.240**	0.244**
					(0.087)	(0.077)

Refugee, state waiver and post reform interaction					-0.587**	-0.443
					(0.225)	(0.234)
Immigrant, state waiver and post reform interaction					-0.195**	-0.191**
					(0.06)	(0.063)
Demographic variables	NO	YES	YES	YES	YES	YES
State fixed effects	NO	YES	YES	YES	YES	YES
Observations	283602	283602	283602	281806	283602	281806

Notes: The regression table above reports coefficient estimates from an alternate MLE specification that utilizes the refugee probabilities. Coefficient estimates above cannot be interpreted as marginal effects. These estimates are used only for a comparative purpose. The magnitude, sign and statistical significance and their respective changes between the base and full models are consistent with the multinomial logit models used in the main results. Standard errors in parentheses are clustered at the state level * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A3. Marginal Effects at the Mean for Medicaid Participation from Multinomial Logit Models

	Only Women	Only Children	Women & Children
Non-Refugee(NR) Immigrants	-0.154*** (0.0150)	-0.230*** (0.0257)	-0.168*** (0.0173)
Refugees	0.233*** (0.0585)	0.209* (0.0840)	0.224*** (0.0545)
Post welfare reform period	-0.0150 (0.00849)	0.00542 (0.0100)	-0.00521 (0.00899)
Post reform and refugee Interaction	0.0992 (0.0632)	-0.00860 (0.114)	0.116 (0.0706)
Refugee and duration in the US interaction	0.00765*** (0.00217)	0.0108** (0.00368)	0.00885*** (0.00263)
Deviations from mean Unemployment rate	0.0494*** (0.0129)	0.0632** (0.0242)	0.0531*** (0.0131)
Refugee and unemployment interaction	0.0153** (0.00518)	0.0192 (0.0113)	0.0204*** (0.00568)
NR Immigrants and unemployment interaction	-0.0132*** (0.00188)	-0.00591 (0.00846)	-0.0129*** (0.00164)
Refugee and deviation from mean state per head spending interaction	-0.0500 (0.0368)	-0.0157 (0.0691)	-0.0429 (0.0404)
State opted for at least one major waiver before reform	0.0186 (0.0122)	0.0239 (0.0146)	0.0155 (0.0122)
Refugee and state waiver interaction	0.0330 (0.0935)	-0.0411 (0.146)	0.0610 (0.102)
Immigrant and state waiver interaction	0.0781* (0.0312)	0.141** (0.0507)	0.0837* (0.0347)
Refugee, state waiver and post reform interaction	-0.0779 (0.0672)	0.0376 (0.148)	-0.120 (0.0734)
Immigrant, state waiver and post reform interaction	-0.0796** (0.0246)	-0.149** (0.0544)	-0.0880** (0.0302)
Observations	152938	128565	217561

Notes: The marginal effects reported above are in reference to the probability of being enrolled in Medicaid, relative to no insurance. The estimates correspond to model 6 in the main analysis in *Table 2.6*. In the three columns of estimates above, the samples includes only women, only children and women and children respectively.

Table 2.A4: Comparing Imputation Methods

Immigrants to the U.S. (1986-1987)				
Country of Birth	No. of Refugees by BH Imputation	No. of Refugees by Borjas Imputation	Mean Refugee Probability	Total Number of Immigrants
Hungary	8	8	0.69	8
Poland	15	78	0.38	78
Romania	25	25	0.68	25
Soviet Union	44	44	0.8	44
Armenia	3	-	0.53	5
Cambodia	16	16	0.92	16
Laos	44	44	0.97	44
Afghanistan	1	1	0.88	1
Iran	7	-	0.39	62
Thailand	8	23	0.35	23
Vietnam	95	95	0.81	95
Malaysia	1	-	0.11	6
Ethiopia	5	7	0.59	7
Pacific Islands	2	-	0.17	15
Cuba	22	55	0.51	55
Total	296	396		

Notes: The above table uses a sample of immigrants that entered in the period between 1986 and 1987 to compare two methods of identifying refugees in the dataset. Type 1 refers to the main imputation method used and recommended in the paper which utilizes the refugee probabilities estimated using INS data. Type 2 refers to the imputation method that assigns all immigrants from select 13 countries as refugees.

CHAPTER THREE

THE REFUGEE WAGE-GAP AND STATE ASSISTANCE

Abstract

We utilize a metric entropy-based measure to analyze the wage distributions of refugees and non-refugee immigrants in the American Community Survey data. Our findings indicate that among recent immigrants, refugee wages tend to be higher in the lower portions and lower in the upper portions of the wage distributions. We find that wage differentials in the lower tails arise from differences in returns to human capital characteristics for the two immigrant groups. Furthermore, if refugees were to have the same human capital characteristics as non-refugee immigrants, the wage differential would be even more in favor of refugees. Consistent with these findings, we also notice that the wage differentials are more favorable to refugees in states with more generous welfare programs and where per head expenditures on refugees are higher. These findings question the conventional belief in the existence of a refugee wage gap and highlight the importance of looking at the entire distribution of wages instead of just specific moments.

1 Introduction

The U.S. Department of State estimates that since 1975 over three million refugees have been resettled in the United States. Despite these substantial numbers, studies on the economic outcomes of resettled refugees have been too few and far between. The importance of refugees, in fact, goes beyond their numbers. Since refugees are often fleeing from various forms of persecution they can be viewed as forced migrants, relative to those who migrate in search of better economic or educational opportunities. Consequently, refugees are also likely to be less prepared when they first arrive in the U.S. Given the nature and cause for migration, it is not surprising that there is a prevailing notion of the existence of a wage gap between refugees and non-refugee immigrants (Connors, 2010). Connors finds that even after controlling for observable human capital characteristics, refugees wages are about 10 percent lower than those for non-refugee immigrants. Evidence of the refugee hardship and wage gap also stems from ethnographic studies (Papadopoulos et al., 2004) that focus on specific refugee groups. And yet, according to the United Nations High Commissioner for Refugees (UNHCR), less than one percent of refugees globally are selected for resettlement in a third country like the U.S. While the UNHCR encourages refugee selection to be based on security and safety concerns and not the refugees' potential for successful integration, the final selection decision remains in the hands of the countries accepting refugees for resettlement. If the potential for integration is an additional criterion for selection then refugees maybe positively selected and there is reason enough to question the assumed existence of a wage gap. Also, it is possible that more motivated refugees self-select into the resettlement process.

In this paper, we extend the wage analysis of the two immigrant groups in three important ways. First, unlike previous studies that have relied on specific refugee groups or relatively small samples of refugees, the present analysis will use significantly larger samples from the U.S. census. The larger and arguably more representative sample allows us to investigate whether or not a refugee wage gap actually exists.

Second, we move beyond the traditional comparison of conditional moments of the wage distribution to analyze the entire distribution of immigrant wages. We utilize a metric-entropy based measure of the distance between two whole distributions, as introduced by Granger, Maasoumi, and Racine (2004). For social welfare comparisons, we rely on stochastic dominance tests to rank the distributions. We demonstrate how limiting the analysis to only mean wages can make the wage gap study both incomplete and misleading.

Finally, we attempt to decompose the wage differential for the two immigrant groups through a counterfactual analysis based on a propensity score reweighting method. Specifically, we utilize the aforementioned dominance tests and reweighting method to rank the wage distributions of refugees and two counterfactual refugee wage distributions. The first of these are the counterfactual wages of refugees if they had the same returns to human capital as non-refugee immigrants. The second counterfactual analysis entails a wage distribution of refugees if they had the same human capital characteristics as non-refugee immigrants. We further exploit three interesting facts about refugees and non-refugee immigrants in the U.S. to explain the wage differential. Among recent immigrants, only refugees qualify for federal and state-level assistance. The generosity of this assistance varies considerably from state to state; and finally, refugees have little control over their initial placement in the U.S.

We are not the first to explore potential differences in trends at various percentiles of the wage distribution. Albrecht et al. (2003) uses data from Sweden to demonstrate a glass ceiling effect — the process whereby differences in gender wage gap increase throughout the wage distribution and maximize at the upper tail. Chiswick et al. (2006) show that while minimum wages in the U.S. compress the immigrant-native wage gap at the lower tail, the wage gap widens significantly at the higher deciles. After distinguishing between refugees and non-refugee immigrants, Wahlberg (2008) finds that in Sweden the refugee-native wage gap increases across the distribution, whereas the non-refugee-native wage gap decreases across the distribution. There are, however, no studies to date that examine the refugee-non-refugee wage gap across the wage distribution. Given that there are state and federal resources devoted to the economic assimilation of refugees — resources that are not always available for non-refugee immigrants — we believe that the above wage gap analysis will help in understanding the efficacies of such programs.

The data for the present analysis comes from the American Community Survey (ACS, 2000-2011). The census dataset does not identify refugees from the immigrant population. We accomplish this task by using an imputation method based on an immigrant's country of origin, gender, year, and age of arrival in the U.S. We utilize the Immigration and Naturalization Services (INS) datasets along with the yearbooks of immigration statistics for imputing refugee status to the immigrants in the ACS. We derive a measure of state welfare generosity by using data from the University of Kentucky Center for Poverty Research and the Consolidated Federal Funds Report (CFFR).

Contrary to conventional belief, our findings suggest that there is no refugee wage gap. Analyzing the raw wages of all immigrants entering the U.S. after 1980, we find that refugees in fact earn higher wages than non-refugee immigrants across all percentiles. Limiting the analysis to more recent immigrants — those in the U.S. for ten years or fewer — we find that refugee wages are higher in the lower tail, but generally lower in the upper tail of the wage distribution. Results from the stochastic dominance tests suggest that the wage distributions of refugee and non-refugee immigrants are generally nonrankable in most years except in 2003, 2004, and 2005. In these three years, we find the wage distribution of refugee immigrants to empirically dominate, in a second-order sense, the wage distribution of non-refugee immigrants. The second-order dominance relation is statistically significant at the 10% level or higher in 2003 and 2004. This means that an immigrant with a social welfare function that is increasing and concave in wages would prefer the refugee distribution to the non-refugee wage distribution in 2003 and 2004. Second-order dominance indicates that at lower wage percentiles, refugee immigrants are better paid than non-refugee immigrants. Decomposing the wage gap among recent immigrants, we find that for 2003 and 2004, where the distributions can be ranked, the wage differential is explained by better returns to human capital characteristics for refugees. Furthermore, if refugees were to have the same human capital characteristics as non-refugee immigrants, the wage differential would be even more in favor of refugees. We also find suggestive evidence that shows that states with generous welfare programs and high per-head state expenditure on refugees, refugee wages exceed non-refugee wages by larger magnitudes across the entire wage distribution.

2 Motivation and Conceptual Framework

The 2012 UNHCR statistical yearbook estimates that out of the 45.2 million people who are currently forcibly displaced worldwide, 15.4 million are refugees. While a majority of the refugee burden is borne by neighboring countries — very often who are themselves developing countries — less than one percent of the total displaced refugees are selected for resettlement in a third country. The number of refugees resettled in the U.S. is more than the total number of refugees resettled in all other countries combined (Migration Policy Institute, 2004). Yet, the majority of the economic studies conducted on resettled refugees originate from countries other than the U.S.

Several papers have provided assimilation information in a number of countries: Cobb-Clark (2006) for Australia; Aydemir (2011) for Canada; and Bevelander and Pendakur (2009), Hartog and Zorlu (2009), Albrecht et al. (2003), and Wahlberg (2008) for Europe. These studies collectively suggest that refugees face an uphill battle when it comes to economic assimilation. They endure lower employment rates, rely more heavily on welfare, and take several years to catch up with other non-refugee immigrants. The handful of papers on the economic assimilation of refugee in the U.S., however, paints a less dire picture. Akresh (2008) finds that while a refugee's first job placement in the U.S. is on average lower than those for a non-refugee immigrant, over time the former makes steeper gains in occupational trajectory. Cortes (2005) and Giri (2013) also find that refugee earnings increase at a faster rate than those for non-refugee immigrants. The only U.S. study specifically on refugee wage gap (Connors, 2010) finds that, conditional on human capital characteristics refugees on average face lower wages and occupational attainments compared to non-refugee immigrants. In the present analysis, we revisit the refugee wage gap analysis using a larger dataset and focus on the entire distribution of wages.

The present analysis focuses on refugees and non-refugee immigrants that entered the U.S. after the Refugee Act of 1980. Prior to 1980 in the U.S., refugees were exclusively defined in an anticommunist context (Huyck and Bouvier, 1983). The earliest refugees admitted to the U.S., as such, were those displaced by World War II and others fleeing communist regimes. Refugees from these regions continued to be resettled in the following decades, but there was also an influx of refugees from additional countries. It was only after the 1980 Refugee Act that the U.S. revised its definition of what it meant to be a refugee along the lines of that commissioned by the UNHCR. The 1951 Refugee Convention defines a refugee as one who "owing to a well-founded fear of being persecuted for reasons of race, religion, nationality, membership of a particular social group or political opinion, is outside the country of his nationality, and is unable to, or owing to such fear, is unwilling to avail himself of the protection of that country." The1980s act has made it possible for refugees worldwide — all those fitting the above description — to apply for resettlement. The refugee resettlement programs have since been consolidated and made formally available to all incoming refugees (Kennedy, 1981). Next, I briefly explain the refugee reception and placement program in the U.S. and why it may be important in the current wage gap analysis.

2.1 Refugee Reception and Placement in the U.S.

Unlike non-refugee immigrants, refugees do not pick their initial location in the U.S. Every week, the Department of State and nine domestic resettlement agencies meet to review each refugee's biographical information and determine where he or she will be resettled in the United States. The agencies attempt to match the particular needs of each incoming refugee with available resources. Except for those refugees with relatives in the United States, the resettlement agencies decide on the best match between a community's resources and the refugee's needs. The reception process begins from the moment refugees arrive at the airport, where they are received by sponsoring resettlement affiliates. Upon arrival, refugees are provided with furnished apartments, some clothing, and food. Assigned caseworkers then help the refugees apply for Social Security cards, register children in schools, learn how to access shopping facilities, arrange medical appointments, and connect with language services. The Department of State's Reception and Placement program provides refugees with assistance for the first three months after arrival. Longer-term cash and medical assistance, along with language and job search services, are then provided via contracted nongovernmental agencies through support from the Department of Health and Human Services' Office of Refugee Resettlement. Guaranteed short term assistance might help refugees widen their job search and allow them to find better matching and higher paying work opportunities. Availability of basic necessities might also mean that refugees are less likely to be trapped in jobs with little or no upward mobility. In fact, Akresh (2008) finds that relative to non-refugee immigrants, refugees make steeper gains in occupational trajectory over time.

The aforementioned resettlement process for refugees differs considerably from the typical experience for non-refugee immigrants when they first arrive in the country. We surmise that these differences are important sources for any subsequent differences in wages for the two immigrant groups. Also, among recent immigrants, only refugees qualify for federal and state-level assistance. The amount of cash assistance received by refugees, for example, is based on Temporary Assistance for Needy Families (TANF) rates, which vary by state and county (Bruno, 2011). Brick and Krill (2010), as well as Bruno (2011), argue that the current setup of the refugee resettlement process creates a “lottery effect” by which refugees placed in more generous states are better off than those placed in less generous states. In this paper, we explore whether there exists a refugee non-refugee wage gap and if differences in the wages of the two groups can be decomposed into structural and/ or composition effects. We consider the possibility that state welfare assistance, especially medical assistance, might lead to improved health and subsequently higher wages for refugees. Additionally, varying state expenditures on refugee programs might also proxy for states' attitudes toward refugees. More generous states may share a more positive outlook on refugees. Given that refugees and poorer natives compete for some of the same resources, anti-refugee sentiments and discrimination, especially in less generous states, is a distinct possibility. Next, we outline the methodology used for establishing whether or not there exists a wage gap, identifying structural and composition effects, and directly test for any lottery effect.

3 Methodology

The first question we attempt to answer in this paper is if in fact there exists a refugee wage-gap. Quantitative studies on refugees using U.S. data are few in number and utilize relatively small samples. Additionally, even studies from outside the U.S. have focused only on specific conditional moments of the wage distribution — mostly at the mean or specific quantiles. None of the studies on refugee wages have analyzed the entire wage distribution. The drawbacks of using only the conditional mean or median to generalize about the population wage gap have been well articulated with examples in Maasoumi and Whang (2005). The disadvantage of relying only on conditional moments can be summarized as follows: if wage differentials of the two groups being analyzed are not consistent or even in the same direction throughout the wage distribution, then the conditional average or median is not representative of the population. For example, one group's wages may be higher at the lower tail and the other group's is higher at the upper tail.

To overcome the shortcomings of studying wage gaps at the sample mean level, we use more general entropy measures that summarize information of the entire wage distribution. Shannon's entropy and Kullback-Leibler divergence measure are commonly used in the literature. However, those entropy measures are known to be non-metric as they violate the symmetry rule; i.e., they take different values when the basis distribution switches. Hence they are not properly defined measures of distance. This paper uses a metric entropy measure S_ρ proposed by Granger et al.

(2004), which is a normalization of the Bhattacharya-Matusita-Hellinger measure of distance. The measure is defined as

$$S_\rho = \frac{1}{2}\int_{-\infty}^{\infty}\left(f_1^{\frac{1}{2}} - f_0^{\frac{1}{2}}\right)^2 dy$$

This measure has several desirable properties: 1) It is well defined for both continuous and discrete variables[35] ; 2) It is normalized to be between 0 and 1 and it takes a 0 value if two distributions are equal; 3) It satisfies all properties that define a metric and hence is a true distance function; and 4) It is invariant under continuous and strictly increasing transformation on the underlying variables. The natural log transformation widely used in the literature is a strictly increasing function, thus the findings are invariant whether we use the raw wages or the log form. Following Granger et al. (2004) and Maasoumi and Racine (2002), we consider a kernel-based nonparametric implementation of the metric entropy measure shown in the above equation.

3.1 Stochastic Dominance

Using S_ρ as the distributional distance measure, we can estimate the distance between the refugee wage distribution and the non-refugee wage distribution. However, in order to compare the social welfare associated with different wage distributions, we need to apply stochastic dominance tests. The stochastic dominance test approach is contingent upon the choice of social welfare functions. The first order stochastic dominance test corresponds to a class (denoted as U_1) of all (increasing) von Neumann-Morgenstern types of social welfare functions u such that welfare is increasing in wages (i.e., $u^{'} > 0$), and the second order stochastic dominance test corresponds to the class of social welfare functions in U such that $u^{''} \leq 0$ (i.e., concavity), denoted as U_2. Concavity implies an aversion to higher dispersion (or inequality) of wages across workers. In this paper, we only focus on the one-dimensional social welfare function of

[35]For discrete variables, $S_\rho = \frac{1}{2}\sum\left(p_1^{\frac{1}{2}} - p_0^{\frac{1}{2}}\right)$

earnings.

Case 1. *First Order Dominance: Non-refugee immigrants' wages first order stochastically dominates refugee immigrants' wages* [denoted as $\ln(w^0)$ *FSD* $\ln(w^1)$] *if and only if*

1) $E[u(\ln(w^0))] \geq E[u(\ln(w^1))]$ for all u in U_1 with strict inequality for some u ;

2) Or $F_0(y) \leq F_1(y)$ for all y with strict inequality for some y

Case 2. *Second Order Dominance: Non-refugee immigrants' wages second order stochastically dominates refugee immigrant' wages* [denoted as $\ln(w^0)$ *SSD* $\ln(w^1)$] *if and only if*

1) $E[u(\ln(w^0))] \geq E[u(\ln(w^1))]$for all u in U_2 with strict inequality for some u;

2) Or $\int_{-\infty}^{y} F_0(t)dt \leq \int_{-\infty}^{y} F_1(t)dt$ for all y with strict inequality for some y .

In this paper, we apply a generalized Kolmogorov-Smirnov test to detect stochastic dominance relations as discussed in Linton et al. (2005). The test statistics for FSD and SSD are given respectively by:

$$d = \sqrt{\frac{N_0 N_1}{N_0 + N_1}} \min\{\sup[F_1(y) - F_0(y)], \sup[F_0(y) - F_1(y)]\}$$

$$s = \sqrt{\frac{N_0 N_1}{N_0 + N_1}} \min\{\sup \int_{-\infty}^{y} [F_1(t) - F_0(t)]dt, \sup \int_{-\infty}^{y} [F_0(t) - F_1(t)]dt\}$$

When estimating the test statistics, the cumulative distributions are replaced with empirical CDFs, which are given by

$$\widehat{F_d(y)} = \frac{1}{N_d} \sum_{i=1}^{N_d} I\left[ln\left(w_i^d\right) \leq y\right], d = 0,1$$

where $I(\cdot)$ is an indicator function. The underlying distributions of the test statistics are generally unknown and are dependent on the data. Following Maasoumi and Heshmati (2000), a simple bootstrap technique based on 99 replications are used to obtain confidence intervals for the test statistics.

3.2 The Decomposition Problem

Considering the entire distribution, if wage differentials exist between refugees and non-refugee immigrants, then we would like to know the origin of these differences. Wage decompositions using the Blinder-Oaxaca method or variants of the method are common in the literature.[36] These methods attempt to trace the raw differences at specific moments of the wage distributions to differences in the distribution of covariates and differences in estimated coefficients. Because we are interested in the entire wage distribution, we take a slightly different approach to this counterfactual analysis.

We consider the following scenario: holding the refugee immigrants' human capital characteristics constant, if we change their wage structure to that of non-refugee immigrants would the counterfactual wage distribution be different from the original refugee wage distribution? Also, would the counterfactual wage distribution stochastically dominate the original wage distribution in terms of welfare? Evidence of dominance at the first or second order would indicate that the wage structure for non-refugee immigrants is better. Similarly, in a second scenario, we hold the refugee wage structure constant but change the refugee human capital characteristics to those of non-refugee immigrants and then test for stochastic dominance.

We generate two wage distributions to conduct the above two counter factual analyses using the propensity score reweighting methods introduced in Firpo, Fortin, and Lemieux (2007) and also used in Maasoumi et al. (unpublished manuscript). As with the earlier tests using actual distributions, simple bootstrapping with replacement is applied to obtain the statistical significance for the dominance tests for the refugee and counterfactual wages. We identify the

[36]For example, Melly (2006) introduces a version of the decomposition to handle estimates from quantile regressions. Wahlberg (2008) uses this method to decompose the refugee-native wage gap at various quantiles of the wage distribution.

distributions of the following two counterfactual outcomes:

$$\ln\left(w_i^{c1}\right) = g_0\left(X_i, \varepsilon_i\right) \mid D = 1 \text{ (counter - factual outcome \#1)}$$

$$\ln\left(w_i^{c2}\right) = g_1\left(X_i, \varepsilon_i\right) \mid D = 0 \text{ (counter - factual outcome \#2)}$$

The original outcome is the conditional wage distribution of refugees represented as $\ln(w_i^1) = g_1(X_{i1}; \varepsilon_{i1})$. Identification assumptions for the above quantiles are detailed in Firpo (2007). In summary, these assumptions imply that, conditional on the observable human capital characteristics X, the distribution of wage outcome or the error term ε is independent of whether one is a refugee or a non-refugee immigrant. The assumptions also rule out the possibility that some specific x belongs only to either one of the two immigrant groups and hence such x can predict the probability of being treated perfectly. For this second reason, we do not include state generosity of welfare programs and state spending on refugees as covariates when generating the counterfactual distributions. We test the efficacies of these factors separately.

Following the propensity score reweighting methods outlined in Firpo (2007), the counter-factual distribution of $\ln(w_i^{c1})$ is estimated by:

$$F_{c1} = E[w_{c1}(D_1, X) \cdot I[(\ln(w_i) \leq y)]$$

where $w_{c1}(D_1, X) = \left(\frac{p_1(x)}{1-p_1(x)}\right)\left(\frac{1-D_1}{p_1}\right)$, D_1 is a dummy variable for refugees, $p_1(x) = \Pr\left[D_1 = 1 \mid X = x\right]$ is the propensity score, and $p_1 = \Pr\left[D_1 = 1\right] = E[p_1(X)]$ is the marginal probability of being a refugee. We estimate the propensity score using a logit model. We derive the first counter-factual distribution of wages $\ln(w_i^{c1})$ by applying the weights $w_{c1}(D_1, X)$ to the CDF of log refugee and non-refugee wages. For the second counterfactual wage distribution $\ln(w_i^{c2})$, we use the non-refugee immigrants as the treated group D_2.

$$F_{c2} = E[w_{c2}(D_2, X) \cdot I[(\ln(w_i) \leq y)]$$

Applying the weights $w_{c2}(D_2, X)$ to the log wages of refugees and non-refugee immigrants gives us our second counterfactual wage distribution. Once we have the two counterfactual wage distributions, we can compare these to the original refugee wage distribution using stochastic dominance tests.

4 Data

We use repeated cross-sections of the American Community Survey (ACS) from 2001 to 2011 for data on immigrants' wages and human capital characteristics. We restrict the ACS sample to working age individuals between the ages of 18 and 64 who were born outside of the U.S. or U.S. territories. We further restrict the sample to individuals living outside group quarters, claimed to be in the labor force, worked for wages, and for whom hourly wages could be calculated. We use yearly wage income, weeks worked in the previous year, and average hours worked in a week to estimate hourly wages. In line with previous work on wage gaps, such as Lemieux (2006) and Hotchkiss and Shiferaw (2011), after converting nominal wages to real (year 2000) dollars, we exclude individuals with hourly wages below one and above a thousand dollars. In the counterfactual analysis, we use variables measuring or indicating marital status, education, potential experience, duration in the U.S., age, region of origin, region of residence in the U.S., and industry of work. The ACS, like other demographic Census datasets, does not include an identifier for refugees. We accomplish this task with the help of INS data.

We use a combination of two methods to identify refugees in the dataset. For refugees entering the U.S. in 2000 or before, we make use of INS data available from years 1972 to 2000. We replicate the process outlined in Giri (2013) to impute refugee status based on estimated probabilities that a given immigrant in the ACS is a refugee. All immigrants with a probability of 0.5 or greater are imputed as refugees. The probabilities themselves are calculated using the process outlined in Bollinger and Hagstrom (2008). These probabilities are based on an immigrant's country of origin, year of entry in the U.S., gender, and approximate age at entry.

Details of the procedure can be found in Giri (2013). For refugees entering the country after 2000, we utilize refugee flow data available from U.S. immigration statistical yearbooks. We impute refugee status to all immigrants from countries where, in a given year, over half of the immigrants were refugees. Our final sample includes 136,818 non-refugee immigrants and 15,700 refugees. When focusing on recent immigrants, the sample sizes for refugees and non-refugee immigrants reduce to 3,153 and 45,269, respectively. We provide summaries of the key variables used in the analysis in Table 3.1, separately for refugees and non-refugee immigrants.

We are interested in the potential effects of federal and state-level support for refugees and its absence for non-refugee immigrants on their respective wages. A measure of state welfare generosity is derived from data available through the University of Kentucky Center for Poverty Research. The data enumerates, for each state in a given year, the AFDC/TANF dollar amount by family size (of two, three, and four people). For each year and each family size, we first create an indicator for states with assistance levels above the yearly sample average. We consider a state's welfare assistance as generous if, for a given year, its welfare assistance is greater than the yearly average for any two of the three family sizes.

Refugees also benefit from state and federal spending other than those related to welfare. To include this assistance, we utilize the Consolidated Federal Funds Report (CFFR) data. The CFFR lists yearly federal expenditures by programs. We use data from 2000 to 2010 to approximate year and state-specific federal expenditures on refugees. From the INS statistical yearbooks we calculate the total year and state-specific flow of refugees for the years 2000 to 2010.[37] Next, we divide the federal expenditure variable by the refugee flow variable to construct a state and year specific per head expenditure on refugees. This variable proxies for federal resources available to refugees and is adjusted for the varying number of refugees in each state. Finally, those states

[37]Given that the wages reported in the ACS refer to the previous year's wage income, we use the annual 2000 refugee flow data and the annual federal expenditure data for the ACS year 2001 and so on.

with above average per-head spending on refugees and ones that are considered to have generous welfare assistance are categorized as being overall generous states.

5 Results

In Figure 3.1 below, we present pooled cumulative density plots for two different samples. The first sample includes all immigrants in the ACS (2000 to 2011) that reported being on the labor force and working for wages, and who entered the U.S. after 1980. The second sample includes the same criteria but is limited to immigrants who have been in the country for ten or fewer years. In the first sample, we find the CDF plots for refugees to the right of that for non-refugee immigrants, implying a negative refugee-non-refugee wage gap. In the second plot, which includes more recent immigrants — those in the U.S. for ten or fewer years — we find that the wage gap transitions from negative to positive as we go from the lower to upper tail of the wage distribution. This is suggestive evidence that there might be a glass ceiling for recent refugees.

Table 3.2 and 3.3 report yearly log wage differences between non-refugee immigrants and refugees $[\ln\left(w^0\right) - \ln(w^1)]$ for the post-1980 immigrant and recent immigrant samples, respectively. In the second column, we report the preferred wage gap measure S_ρ multiplied by one hundred. The remaining columns report more frequently used differences at specific points of the wage distribution. Among the larger immigrant sample in Table 3.2, the trend in wage gap shows a negative refugee wage gap. Refugee wages are higher than non-refugee wages for almost all of the years and at all quantiles.

In Table 3.3, where we focus on more recent immigrants, it is harder to find a discernible trend. Yearly differences in mean wages fluctuate from negative to positive in select years and tell us very little about the wage differentials. Even the differences in wage quantiles are not entirely conclusive, although we do see that refugee wages tend to be higher in the lower quantiles and vice versa. Our preferred measure S_ρ in both the tables shows that there is in fact a wage differential. The metric entropy measure, however, does not say anything about how the CDFs for the two immigrants groups are ranked. For this, we must look to results from the stochastic dominance tests. In the following results sections, we limit our focus to recent immigrants, for it is here we believe most of the interesting results can be found.

5.1 Stochastic Dominance Tests Results

In Table 3.4, we find that the wage distributions of refugee and non-refugee immigrants are generally non-rankable in most years, except in 2003 to 2005. In those three years, we find the wage distribution of refugee immigrants to empirically dominate in a second order sense the wage distribution of non-refugee immigrants. The second order dominance relation is statistically significant at a 10% level or higher in the years 2003 and 2004. This means that an immigrant with a social welfare function in the class U_2 (increasing and concave in wage) would prefer the refugee distribution to the non-refugee wage distribution for 2003 and 2004.

Second order dominance indicates that at lower wage percentiles refugee immigrants are better paid than non-refugee immigrants, which may be due to government subsidies. It also suggests that at the far right tail of the wage distribution (i.e., higher wage percentiles), non-refugee immigrants are paid better than refugee immigrants. To further investigate this finding, we divide the sample into high and low wage subgroups using a weighted median wage. The division point is $9.50 per hour.

Table 3.5 reports dominance test results for the higher wage subgroup. In 2008 to 2011, we find that non-refugee wage distribution in the first or second order dominate refugee wage distribution, but such dominance relations are not statistically significant in any year. Although not statistically significant, the direction of the dominance relations are what we expected. More interesting findings are in Table 3.6. For the lower wage subgroup, we find that the wage distribution of refugee immigrants empirically dominates, in a second order sense, the wage distribution of non-refugee immigrants in 2003 and 2004. The dominance relation is statistically significant at the 10% level. This leads us to conclude that, among lower wage immigrants, refugee immigrants are generally better off than non-refugee immigrants. We suspect the advantages are mainly due to state support programs.

Before moving on to decomposing the wage differential, we conduct a stochastic dominance test for wage differentials in generous and non-generous states. This allows us to directly test whether or not state and federal support for refugees impacts wage differentials. We expect higher wage differentials in favor of refugees in generous states more so than in non-generous states. The results in this section are in a way analogous to difference-in-difference estimates. Owing to the paucity of data, we conduct the analysis for all the years pooled into one sample. Table 3.7, clearly shows higher wage differentials in favor of refugees in the generous states. Even the S_{ρ} measure shows no difference in wages in the non-generous states, but does show a wage gap in the generous states. Table 3.8 provides the two related stochastic dominance test results. Although not statistically significant, we find that refugee wages stochastically dominate non-refugee wages in the second order.

5.2 Counterfactual Analysis

Table 3.9 reports the stochastic dominance test results for the structural wage effect for each year in the analysis. These are tests for wage gaps between refugees and counterfactual wages generated using refugee human capital characteristics with a non-refugee wage structure. In most years, we find that the two wage distributions are non-rankable. Given that we found the same with refugee non-refugee wage distributions earlier, we do not find this surprising. Recall, however, that we did find second order dominance for refugees and non-refugee immigrants for 2003 and 2004. For these same years, we find that refugee wages dominate the counterfactual wages in the second order. The results, however, are not statistically significant at the standard 10 percent or higher levels. These direction of the results suggest that the dominance relationships found between refugee and non-refugee wages may stem from larger structural effects.

In Table 3.10, we report the stochastic dominance test results for the composition wage effect, again for each year in the analysis. These are tests for wage gaps between refugees and the counterfactual wages generated using non-refugee human capital characteristics and refugee wage structure. The results show that in most years the two distributions are non-rankable. In years where we do find second order dominance, it is the counterfactual wages that dominate. Although these second order dominance results are not statistically significant, the direction of the tests suggest that the counterfactual wage distribution would be more preferable than the actual refugee wage distribution.

The counterfactual analysis collectively suggests that wage structure and composition of the two immigrant groups have opposing impacts on their existing wage differential. The positive wage differentials between refugees and non-refugee immigrants arise from larger and positive structural effects in favor of refugees. Composition effects, on the other hand, are more favorable for non-refugee immigrants. The magnitudes of the composition effects, however, are not large enough to offset the effects of the wage structure.

6 Conclusion

In this paper, we utilized a metric entropy-based measure to analyze the wage distributions of refugees and non-refugee immigrants in the U.S. Analyzing the distributions in their entirety, we established that, among immigrants that have been in the U.S. since 1980, there exists no wage gap between refugees and non-refugee immigrants. Among recent immigrants, the situation is more complex. We find evidence that refugee wages tend to be higher in the lower tails and lower in the upper tails of the immigrant wage distributions. Wage differentials in the lower tails, we find, arise from differences in returns to human capital characteristics for the two immigrant groups. Furthermore, if refugees had the human capital characteristics of non-refugee immigrants, the wage differential would be even more in favor of refugees. Consistent with this finding, we also notice that the wage differentials are more favorable to refugees in states with more generous welfare programs and where per-head expenditures on refugees are higher.

Our results differ from those based on existing works on refugee wages in the U.S. in part because we look at the entire distribution of wages and not just the mean or other specific moments of the distribution. The results here suggest that refugees are resilient and, despite their difficult pre-migration experiences, are still able to make the most of the opportunities available to them. From a policy standpoint, the results here point to the success of policies aimed at improving the wage structure for refugees. Furthermore, the negative composition effects for refugees suggests that more policies aimed at improving the human capital characteristics of refugees can result in even more favorable wage outcomes. It remains to explored, however, why refugee wages are lower in the upper tails of the wage distributions.

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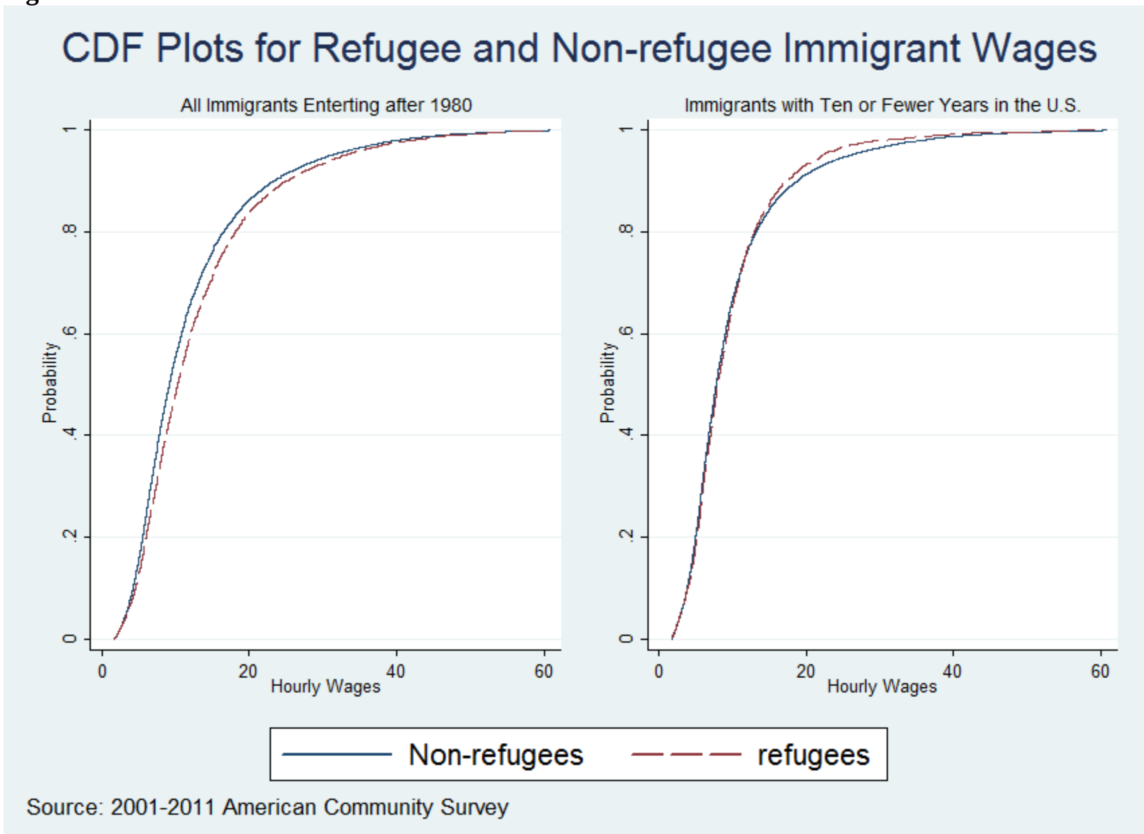
Figure 3.1



Note: In constructing the above wage plots, I exclude observations from the top and bottom one percent of the hourly wage distribution.

Table 3.1: Refugee and Non-refugee Immigrants' Means/ Proportions of Key Variables

	Non-refugee Immigrants		Refugees	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Presently or previously married	0.23	0.42	0.25	0.43
Education (proportions**)**				
Less than Highschool	0.16	0.36	0.09	0.29
Highschool **degree**	0.35	0.48	0.35	0.48
Some college	0.24	0.43	0.28	0.45
College degree or higher	0.25	0.43	0.28	0.45
Potential experience	13.71	10.90	14.90	11.49
Duration in **the U.S.**	15.99	9.79	18.40	9.61
Age	31.40	10.78	33.01	11.15
U.S. Region of Residence (proportions)				
Northeast	0.23	0.42	0.15	0.36
Midwest	0.09	0.28	0.11	0.31
South	0.27	0.44	0.36	0.48
West	0.41	0.49	0.38	0.48
Immigrant region of origin (proportions)				
Africa	0.03	0.17	0.03	0.16
Asia	0.31	0.46	0.45	0.50
Europe	0.07	0.26	0.24	0.43
North America	0.01	0.10	0.00	0.00
South America	0.09	0.28	0.00	0.00
Oceania	0.01	0.07	0.00	0.00
Caribbean	0.09	0.29	0.24	0.43
Central America	0.37	0.48	0.00	0.00
The Middle East	0.02	0.16	0.05	0.21
Industry of work (proportions)				
Mining	0.01	0.09	0.01	0.10
Construction	0.06	0.24	0.03	0.18
Manufacturing	0.12	0.32	0.16	0.37
Wholesale trade	0.03	0.18	0.03	0.17
Retail trade	0.14	0.35	0.14	0.35
Transportation	0.04	0.20	0.04	0.21
Utilities	0.01	0.11	0.01	0.12
Information and communication	0.02	0.15	0.02	0.15
Finance, insurance, real	0.07	0.25	0.08	0.28

estate				
Professional services	0.10	0.30	0.10	0.30
Educational, health, and social services	0.19	0.39	0.19	0.39
Arts and entertainment	0.14	0.34	0.10	0.30
Other industries	0.04	0.20	0.06	0.23
Number of Observations	136818		15700	

Notes: The sample includes immigrants who entered the U.S. in 1980 or later, participated in the labor force, worked for wages, and were between 18 and 64 years of age.

Table 3.2: Measures of Differences between Non-Refugee and Refugee Immigrants entering US after 1980

Year	Srho*100	Mean	10th	25th	50th	75th	90th
2001	0.69	-0.10	-0.15	-0.18	-0.15	-0.06	-0.03
2002	0.76	-0.08	-0.11	-0.12	-0.14	-0.10	-0.04
2003	0.82	-0.13	-0.10	-0.16	-0.16	-0.16	0.01
2004	1.12	-0.19	-0.13	-0.13	-0.22	-0.21	-0.18
2005	0.48	-0.12	-0.11	-0.09	-0.16	-0.13	-0.07
2006	0.60	-0.09	0.00	-0.06	-0.13	-0.15	-0.09
2007	0.37	-0.09	-0.08	-0.13	-0.08	-0.11	-0.07
2008	0.37	-0.08	-0.12	-0.10	-0.08	-0.07	-0.06
2009	0.57	-0.13	-0.09	-0.12	-0.15	-0.16	-0.15
2010	0.28	-0.08	-0.11	-0.05	-0.08	-0.07	-0.19
2011	0.32	-0.08	0.01	-0.04	-0.06	-0.10	-0.09

Notes: The same sample from Table 3.1 is used in the above table. Column (2) reports the overall wage gap (×100) between non-refugee immigrants and refugees as measured by s-rho of the distributions of log wages. Columns (3)-(8) report conventional measures based on difference in parts of the wage distributions between non-refugees and refugees.

Table 3.3: Measures of Differences between Recent Non-Refugee and Refugee Immigrants

Year	Srho*100	Mean	10th	25th	50th	75th	90th
2001	0.63	-0.09	-0.12	-0.11	-0.15	-0.09	-0.06
2002	1.03	-0.05	0.11	-0.10	-0.11	-0.07	0.00
2003	1.51	-0.12	-0.13	-0.16	-0.12	-0.12	-0.07
2004	0.68	-0.10	-0.11	-0.11	-0.08	0.01	-0.20
2005	0.45	-0.05	-0.13	-0.05	-0.05	-0.08	-0.03
2006	0.66	0.00	0.15	0.03	-0.02	-0.05	-0.08
2007	1.35	0.04	0.06	-0.05	0.01	0.04	0.17
2008	0.35	-0.03	-0.04	-0.07	-0.03	0.00	0.03
2009	0.98	0.04	0.00	-0.03	0.00	0.05	0.20
2010	0.56	0.10	0.08	0.04	0.04	0.10	0.21
2011	0.46	-0.00	-0.02	-0.05	0.00	0.03	0.17

Notes: Column (2) reports the overall wage gap (×100) between recent non-refugee immigrants and refugees as measured by s-rho of the distributions of log wages. Columns (3)-(8) report conventional measures based on difference in parts of the wage distributions between recent non-refugees and refugees. Recent refers to immigrants that have been in the U.S. for 10 years or fewer.

Table 3.4: Stochastic Dominance Test Results for Recent Immigrants

Year	Obrank	d1,max	d2,max	d	Pr(d<=0)	s1,max	s2,max	s	Pr(s<=0)
2001	3	1.90	0.32	0.32	0.00	40.57	0.48	0.48	0.24
2002	3	1.17	0.27	0.27	0.00	16.45	0.19	0.19	0.29
2003	2	1.41	0.34	0.34	0.00	24.28	-0.02	-0.02	1.00
2004	2	1.11	0.14	0.14	0.00	17.84	-0.01	-0.01	0.93
2005	2	1.08	0.46	0.46	0.01	15.96	-0.01	-0.01	0.54
2006	3	0.23	0.49	0.23	0.00	0.35	8.05	0.35	0.12
2007	3	0.76	0.80	0.76	0.00	6.23	8.78	6.23	0.19
2008	3	0.47	0.54	0.47	0.00	2.17	4.93	2.17	0.13
2009	3	0.04	1.08	0.04	0.00	0.15	30.06	0.15	0.01
2010	3	0.03	1.27	0.03	0.05	0.05	34.92	0.05	0.16
2011	3	0.48	1.03	0.48	0.00	3.86	10.37	3.86	0.14

Notes: For FSD — denoted by obrank 1 — we first check to see if d is negative, then if d1 is negative it means non-refugee wages dominate and vice versa. Similarly for SSD — denoted by obrank 2 — we first check for a negative s, then if s1 is negative it means non-refugee wages dominate and vice versa. Pr(d<=0)>= 90 or Pr(s<=0)>= 90 is considered statistically significant. Obrank 3 denotes that distributions are non-rankable.

Table 3.5: Stochastic Dominance Test Results for Recent Immigrants-High Wage Subgroup

Year	Obrank	d1,max	d2,max	d	Pr(d<=0)	s1,max	s2,max	s	Pr(s<=0)
2001	3	0.66	0.77	0.66	0.00	2.82	12.28	2.82	0.12
2002	3	0.03	0.79	0.03	0.04	0.01	19.56	0.01	0.18
2003	3	0.58	0.87	0.58	0.01	1.28	13.20	1.28	0.10
2004	3	0.25	0.43	0.25	0.02	0.17	2.07	0.17	0.31
2005	3	0.51	0.96	0.51	0.00	1.46	14.17	1.46	0.14
2006	3	0.30	0.76	0.30	0.02	0.61	9.79	0.61	0.23
2007	3	0.15	1.39	0.15	0.08	0.20	38.88	0.20	0.19
2008	2	0.09	0.99	0.09	0.08	-0.26	17.70	-0.26	0.78
2009	1	-0.01	1.49	-0.01	0.61	-0.20	45.58	-0.20	0.68
2010	1	-0.02	1.68	-0.02	0.46	-0.24	44.05	-0.24	0.79
2011	2	0.25	1.37	0.25	0.01	-0.09	16.60	-0.09	0.16

Table 3.6: Stochastic Dominance Test Results for Recent Immigrants-Low wage subgroup

Year	Obrank	d1,max	d2,max	d	Pr(d<=0)	s1,max	s2,max	s	Pr(s<=0)
2001	3	1.44	0.14	0.14	0.01	38.78	1.79	1.79	0.10
2002	3	0.39	0.46	0.39	0.00	0.05	6.88	0.05	0.13
2003	2	0.74	0.19	0.19	0.00	36.58	-0.02	-0.02	0.99
2004	2	0.76	0.06	0.06	0.06	27.91	-0.02	-0.02	0.91
2005	2	0.85	0.11	0.11	0.06	24.03	-0.02	-0.02	0.55
2006	3	0.13	0.68	0.13	0.00	1.16	20.04	1.16	0.08
2007	2	0.87	0.23	0.23	0.03	21.31	-0.02	-0.02	0.41
2008	3	0.71	0.36	0.36	0.03	6.89	3.54	3.54	0.27
2009	3	0.56	0.48	0.48	0.00	0.67	11.36	0.67	0.03
2010	3	0.35	0.68	0.35	0.00	0.24	17.05	0.24	0.06
2011	3	0.79	0.60	0.60	0.00	18.60	0.66	0.66	0.33

Notes: The sample is divided into high and low wage subgroups using weighted median wage. The division point is $9.5 per hour.

Table 3.7: Recent Non-Refugee and Refugee Wage-gap by State Generosity

State Type	Srho*100	mean	10th	25th	50th	75th	90th
Non-generous	0	-0.03	-0.02	-0.05	-0.05	-0.02	0.05
Generous	1	-0.12	-0.17	-0.13	-0.12	-0.09	-0.13

Notes: States with welfare assistance levels and per head spending on refugees above the national average are considered to be generous.

Table 3.8: Stochastic Dominance Test Results for Wage-gap by State Generosity

State Type	Obrank	d1,max	d2,max	d	Pr(d<=0)	s1,max	s2,max	S	Pr(s<=0)
Non-generous	2	1	1.23	1.23	0.00	17.35	-0.06	-0.06	0.28
Generous	2	1	0.08	0.08	0.01	17.10	-0.03	-0.03	0.83

Table 3.9: Stochastic Dominance Test Results, Counterfactual #1

Year	Obrank	d1,max	d2,max	d	Pr(d<=0)	s1,max	s2,max	s	Pr(s<=0)
2001	3	0.80	2.87	0.80	0.00	2.77	52.78	2.77	0.06
2002	3	0.64	1.72	0.64	0.01	3.68	12.90	3.68	0.31
2003	2	0.92	2.75	0.92	0.00	-0.11	42.28	-0.11	0.50
2004	2	0.84	1.69	0.84	0.01	-0.10	25.90	-0.10	0.88
2005	3	2.46	1.32	1.32	0.00	19.73	10.91	10.91	0.24
2006	3	3.21	0.29	0.29	0.01	60.25	0.29	0.29	0.13
2007	3	5.14	1.18	1.18	0.00	115.31	4.75	4.75	0.15
2008	3	4.16	1.60	1.60	0.00	65.70	4.71	4.71	0.23
2009	3	5.89	0.14	0.14	0.01	148.8	0.27	0.27	0.41
2010	1	5.05	-0.07	-0.07	0.24	139.35	-0.10	-0.10	0.51
2011	3	3.78	1.31	1.31	0.00	40.24	11.63	11.63	0.19

Notes: Counterfactual 1 refers to generated wages that have refugee human capital characteristics with non-refuge wage structure.

Table 3.10: Stochastic Dominance Test Results, Counterfactual #2

Year	Obrank	d1,max	d2,max	d	Pr(d<=0)	s1,max	s2,max	s	Pr(s<=0)
2001	2	2.36	0.02	0.02	0.04	49.49	-0.06	-0.06	0.48
2002	3	0.98	1.62	0.98	0.01	1.30	18.19	1.30	0.34
2003	3	0.83	0.65	0.65	0.02	5.75	1.76	1.76	0.32
2004	2	2.02	0.09	0.09	0.04	29.21	-0.07	-0.07	0.40
2005	3	3.12	0.85	0.85	0.01	25.58	6.90	6.90	0.30
2006	2	2.04	0.55	0.55	0.01	29.20	-0.07	-0.07	0.40
2007	3	0.50	7.54	0.50	0.02	2.56	108.25	2.56	0.24
2008	3	2.67	1.41	1.41	0.01	1.06	6.97	1.06	0.23
2009	3	0.26	2.16	0.26	0.01	0.84	39.10	0.84	0.24
2010	3	1.37	1.19	1.19	0.00	1.50	9.02	1.50	0.23
2011	3	2.67	2.09	2.09	0.02	1.94	26.42	1.94	0.29

Notes: Counterfactual 2 refers to generated wages that have non-refugee human capital characteristics with refuge wage structures.