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Joshua Mask*

Consequences of immigrating during a recession: Evidence from the US Refugee Resettlement program

Abstract

Are there long-term labor consequences in migrating to the US during a recession? For most immigrants, credibly estimating this effect is difficult because of selective migration. Some immigrants may not move if economic conditions are not favorable. However, identification is possible for refugees as their arrival dates are exogenously determined through the US Refugee Resettlement program. A one percentage point increase in the arrival national unemployment rate reduces refugee wages by 1.98% and employment probability by 1.57 percentage points after 5 years.

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1 Introduction

The timing of labor market entry matters. Several studies (Oyer, 2006; Oyer, 2008; Kahn, 2010; Oreopoulos et al., 2012) have shown that poor business cycle conditions at labor market entry can have a detrimental effect on long-term employment and wage outcomes for both college graduates and post-graduates. I provide evidence that this phenomenon, known as “scarring,” is also observed among US-resettled refugees. Exploiting plausible exogeneity in refugee arrival dates, I estimate that a one percentage point increase in the arrival national unemployment rate reduces refugee wages by 1.98% and employment probability by 1.57 percentage points after 5 years on average. For most immigrants, credibly estimating this effect is difficult because individuals may selectively delay or forgo migration when economic conditions become unfavorable. Refugees, however, do not have this choice. They are unable to stay in their country of origin,¹ easily migrate between countries,² or choose where they are eventually resettled.³ If selected to resettle in the US, they must also undergo 18–24 months of screening before arrival.⁴ Arrival dates for US-resettled refugees are therefore not endogenous to US economic conditions.

A key feature of this study is the use of a novel, longitudinal, government-administered dataset called the Annual Survey of Refugees (ASR). The ASR is a household survey of US-resettled refugees conducted annually for 5 years post-arrival. These data have only appeared in a limited capacity in previous research (Beaman, 2012; Arafah, 2016). This study provides a breakthrough opportunity for research on the US Refugee Resettlement program because the ASR is the only dataset to my knowledge that identifies US-resettled refugees for >90 days post-arrival (Capps et al., 2015; Evans and Fitzgerald, 2017).

Previous work on immigrant wage and employment scarring has studied both immigrants in the US and refugees in Scandinavia. Chiswick et al. (1997) have examined immigrant employment outcomes in the US and found no evidence of a long-term scarring effect. Chiswick and Miller (2002) found some evidence of wage scarring for immigrants in the US. However, these studies do not account for selective migration based on economic conditions during arrival. Given this concern, Åslund and Rooth (2007) have used refugees in Sweden to measure this effect. Similar to the US context, refugees in Sweden in the early 1990s were exogenously placed in various geographic settings at different points of time. They find that poor initial economic conditions can decrease wages for refugees up to 10 years after migration. Godøy (2017) also examined refugees in Norway and found no evidence of a long-lasting wage scarring effect.

To the best of my knowledge, this study is the first to examine employment and wage scarring effects for US-resettled refugees. There are several reasons why the US setting provides a valuable contribution. Traditionally, roughly half of the refugees who resettled in a third country are resettled in the US.⁵ The US also has more geographic variation and ethnic diversity, providing more variation in potential outcomes for refugees. The US Refugee

1 <https://www.unrefugees.org/refugee-facts/what-is-a-refugee/>

2 <https://www.unhcr.org/en-us/research/working/57ee60d57/rights-risk-thematic-investigation-states-restrict-freedom-movement-refugees.html>

3 <https://www.pbs.org/newshour/world/asked-refugees-referred-live-u-s>

4 <https://refugees.org/explore-the-issues/our-work-with-refugees/security-screening/>

5 <https://www.unhcr.org/statistical-yearbooks.html>

Resettlement program has also enjoyed relative stability since its inception in 1980. Åslund and Rooth (2007) noted that the refugee resettlement program in Sweden was suspended in the early 1990s as resources were diverted, which limited their analysis to only one period of economic decline. The long-term stability of the US Refugee Resettlement program allows me to observe outcomes for refugees resettled over multiple business cycles. Finally, estimates found in other countries may not be applicable to the US setting. For example, refugees in Sweden are encouraged to defer entry into the labor market for up to 18 months post-arrival (Ibid.). In the US, refugees are encouraged to find work and become self-sufficient as soon as possible.⁶

This study also contributes to the literature that examines the heterogeneity of scarring effects within the population. Differences have been found between education groups based on the field of study (Altonji et al., 2016) and across male workers based on their different years of education (Speer, 2016). Schwandt and von Wachter (2019) found larger effects for disadvantaged workers, particularly non-whites and high school dropouts. In a separate analysis, I divide my sample across gender and educational attainment. One key advantage of this study is that educational attainment is not endogenous to US economic conditions as refugees report their education-level prior to arrival. Curiously, in terms of magnitudes, I find college-educated refugees are far less likely to find employment in their early years than less-educated refugee groups. I also find that wage scarring effects are much greater for college-educated refugees, with particularly severe effects for college-educated female refugees. However, persistent measures of these effects at statistically significant levels are observed mostly for less-educated groups only.

Finally, this study also contributes to the economics of migration literature. Migration economists have long analyzed whether immigrant earnings differ from natives, why they differ, and how that gap changes over time (Chiswick, 1978; Borjas, 1985; LaLonde and Tobel, 1992; Friedberg, 1993; Borjas, 1995; Hu, 2000; Card, 2005; Lubotsky, 2007; Lubotsky, 2011; Kim, 2012; Abramitzky et al., 2014). Events like the Mariel Boatlift, a mass emigration event of Cubans to the US between April and October 1980, have also been used to examine whether immigration hurts native wages and labor supply (Card, 1990; Bodvarsson et al., 2008; Peri and Yasenov, 2015; Borjas, 2017; Borjas and Monras, 2017; Clemens and Hunt, 2017). However, little is known about how changes in native labor supply might affect immigrants themselves. By providing evidence that arrival economic conditions can adversely affect refugee employment and wages, this study also provides a plausible mechanism for aggregate wage differentials found between various immigrant groups and natives, *ceteris paribus*. The timing of migration also matters.

2 US Refugee Resettlement Program

In most circumstances, individuals or families seeking to resettle in the US as refugees at first approach the United Nations High Commission for Refugees (UNHCR). The UNHCR determines the need for permanent resettlement based on seven criteria: “legal and/or physical protection needs, survivors of torture and/or violence, medical needs, women and girls at risk, family reunification, children and adolescents at risk, and lack of foreseeable alternative duration solutions.”⁷ The UNHCR makes a decision on where to send these individuals based on

6 <https://www.state.gov/j/prm/ra/receptionplacement/>

7 <http://www.unhcr.org/en-us/information-on-unhcr-resettlement.html>

country refugee acceptance quotas, family presence, and cultural affinities. If the individual or family is referred by the UNHCR to resettle in the US, they must undergo a screening process of the US Department of Homeland Security. This screening process involves multiple interviews, submission of biometric information, and background checks. On average, applicants must wait 18–24 months before being granted admission to the US. All refugees must undergo this waiting period, regardless of family ties to the US.⁸ In rare cases, officials expedite this process deliberately because of an emergency; even in such instances, the minimum wait time is still 6 months.⁹

The State Department partners with nine non-profit voluntary resettlement agencies (VOLAGs) to determine the placement once a refugee or family has been granted admission to the US. These organizations have 315 affiliates in 180 communities throughout the US. In Figure 1, each affiliate’s office is mapped by its corresponding VOLAG. The State Department meets with these organizations collectively to review information on incoming refugees and assign them to a particular organization. If an individual or family has family currently living in the US, every effort is made to resettle them with or near their family. Otherwise, a resettlement agency agrees to sponsor an individual or family based on available resources.¹⁰

Figure 1 Resettlement sites by volunteer agency.



Source: <https://www.wrapsnet.org/documents/PRM-RPP+Affilaite+Sites+2014.jpg>

8 <https://www.state.gov/refugee-admissions/u-s-refugee-admissions-program-access-categories/>

9 <https://www.unhcr.org/3c5e5a764.html>

10 <https://www.acf.hhs.gov/orr/resource/the-us-refugee-resettlement-program-an-overview>

The nine VOLAGs are responsible for providing welcome and necessary services for refugees during their first 90 days after arrival, including providing safe and affordable housing, furnishings, and services to acclimate them to their new environment. After 90 days, the Office of Refugee Resettlement works with individual states and non-governmental organizations (NGOs) to provide longer-term services such as medical assistance and social welfare benefits. Refugees are allowed freedom of movement and are therefore not bound to stay in the state where they were initially resettled. However, their financial assistance may get jeopardized if they move to a state that does not offer the same benefits as their initial state of resettlement.¹¹

There are some exceptions to this resettlement process. Some individuals who eventually resettle in the US as refugees are referred through a US embassy or a human rights group. Nevertheless, these individuals must still undergo the same screening process as refugees referred by UNHCR. Some individuals may also request asylum at the US border, or cross the border through illegal means and request asylum afterward. The asylum process is significantly different than the formal refugee resettlement process. These individuals must undergo court proceedings to gain asylum and they are not afforded the same benefits and support. For this study, the term “refugee” will refer to individuals who undergo the formalized refugee resettlement process. This distinction is important because my identification strategy will rely on the assumption that refugees who undergo this formalized process cannot choose when they arrive in the US.

3 Theory on Employment and Wage Scarring

The term “scarring” was first coined by Ellwood (1982) to describe the long-term negative consequences of entering the job market in a bad economy that persist well beyond the transitory period. This phenomenon has been observed primarily with college graduates. Oreopoulos et al. (2012) and Kahn (2010) have found that large and persistent negative wage effects have lasted for 10 years and 20 years for college graduates, respectively. It has also been observed with individuals re-entering the job market after displacement. Ruhm (1991) has found that such displaced workers experienced a 10–13% drop in wages in <5 years after displacement.

One potential theoretical explanation for this phenomenon is labor market friction. If employment and wages are determined by labor market conditions in a spot labor market, where wages are determined by current supply and demand, then we will not expect to observe any differences between similar individuals who enter the economy during different business cycle conditions once economic conditions become normalized. This is because productivity between these individuals should not differ apart from slight experience disparities. If the relationship between current employment and wages is influenced by labor market conditions in a contract model, where future wages are pre-determined based on agreements with employers made in prior periods, then the persistence of depressed wages and employment could be explained by mobility. An individual who cannot easily move between firms once labor market conditions improve could see persistent effects. Beaudry and DiNardo (1991) have examined how wages are affected by market conditions and find that a contract model with costless mobility fits this relationship better than a traditional spot labor market.

¹¹ <https://www.state.gov/j/prm/ra/receptionplacement/>

Scarring may also reflect a worker's inability to develop human capital. If an individual enters the job market when opportunities are scarce, he might be forced to spend more time in a job which is not suited to his competencies. As noted in Kahn (2010), if human capital accumulation is important, particularly in the first few years of an individual's career, then an individual's inability to switch jobs and find a compatible or suitable job could yield persistent, long-term detrimental outcomes. As the labor market improves, individuals can switch jobs and gain human capital but they would have lost the opportunity in earlier years. Therefore, controlling for experience, there would be a disparity in human capital between individuals who entered the labor market under different economic conditions. In the context of migration, human capital accumulation and initial job placement could also be affected by the refugee's choice in social networks. Wang (2019) showed that immigrants are more likely to assimilate with natives than fellow migrants if initial economic conditions are unfavorable. Assimilation with natives could be favorable for human capital accumulation in the long-run, but Beaman (2012) showed that recently arrived refugees established a cordial contact with refugees who migrated in previous years, benefitting substantially in terms of employment probability and initial wages.

4 Data

The dataset I used in my analysis is the Annual Survey of Refugees (ASR). The ASR was started in 1975 as a mechanism through which refugee resettlement groups could assess assimilation outcomes for Asian refugees, particularly those from Vietnam. In 1980, with the passage of the Refugee Act, the survey became an important tool for the newly created Office of Refugee Resettlement (ORR). In 1993, the survey was expanded to include all refugee groups.¹² I used the available data from 1993 to 2004 to conduct my analysis. These data were previously used by Beaman (2012) to provide intuition on the magnitude of her results derived from another data set. More recent versions of the ASR data were provided by ORR through Freedom of Information Act requests (Arafah, 2016), but unfortunately, it did not contain information on the initial state of resettlement or country of origin for individuals in the data. Without this information, I am unable to extend my analysis beyond the 1993–2004 survey period.

The ASR samples 1,000–2,000 refugee households each arrival year and surveys them 6–18 months after their initial resettlement. Follow-up surveys are then conducted annually for four more years. Households who have resided in the US for >5.5 years are no longer surveyed. For each survey period, an individual survey is given to all individuals in the household over the age of 16, and a household survey is given to the head of household. The individual survey asks basic demographic information like gender, age, years of education prior to arrival, disability, fluency in English upon arrival, marital status, parental status, country of origin, month and year of entry, original state of resettlement, employment, and hourly wages.¹³ The household survey asks about household participation in social welfare programs like the

¹² <https://archive.acf.hhs.gov/programs/orr/data/04arc8.htm>

¹³ The survey is conducted between September and November of each year. In my analysis, wages are assumed to be in nominal October dollars for each survey year. Wages are then inflation-adjusted to constant 2000 US dollars to allow for comparison across years.

Supplemental Nutrition Assistance Program (SNAP) and the Aid to Families with Dependent Children/Temporary Assistance for Needy Families program (AFDC/TANF).¹⁴

To create a sample that is best suited for my analysis, I first ensured that the sample is restricted to individuals who go through the formalized refugee resettlement process. The ORR is required to collect survey information for both Cuban and Haitian asylees and refugees.¹⁵ The parameters used in compiling ASR data do not distinguish whether Cubans and Haitians forming part of the data records are asylees or refugees; consequently, I have excluded these individuals. I have further excluded Sudanese refugees who arrived after the year 2000 as the ORR began oversampling a specific group of mostly male Sudanese refugees starting in 2001,¹⁶ but provided no weights in the data to distinguish between this oversampled group and other Sudanese refugees. I also dropped individuals who did not arrive in the US during the target period of 6 months to 5.5 years prior to being surveyed. Since the survey participants are determined on a household basis instead of an individual basis, some individuals appear in the data who did not arrive during the target period. Finally, I limit the sample to individuals between the ages of 16–65 to focus on the working-age population. The final sample used in my analysis contains 38,075 observations of 17,771 individuals¹⁷ who resettled in the US between May 1988 and May 2004.

Table 1 contains summary statistics of the sample broken down by intervals of the year of arrival. As expected, the composition of refugees by region of origin changes over time. In the late-1980s and early-1990s, a large portion of resettled refugees came from Asia. After the mid-1990s, following the breakup of Yugoslavia, a larger portion of refugees came from Europe. Despite big differences in origin-region composition, the composition of refugees by

Table 1 Summary statistics by year of arrival

Demographics	1988–1991	1992–1995	1996–1999	2000–2004	All years
Years of education	10.19	10.38	10.74	10.08	10.36
% Female	50.62	51.38	49.76	51.52	50.98
Age at arrival	31.47	32.92	32.09	32.12	32.38
% Fluent in English	9.09	7.33	10.71	14.29	9.18
% Disabled	10.73	13.13	8.19	8.97	11.15
% Married	61.01	54.09	58.33	57.64	56.67
% Have children	54.88	56.44	61.32	67.96	58.84
% From Africa	1.64	5.23	10.26	13.80	6.80
% From Asia	90.91	86.12	50.51	53.87	75.70
% From Europe	7.45	8.66	39.10	31.76	17.38
% From South America	0	0	0.13	0.56	0.11
Individuals	3,289	8,573	3,069	2,840	17,771

¹⁴ The Temporary Assistance for Needy Families (TANF) program replaced the Aid to Families with Dependent Children (AFDC) program following the passage of the Personal Responsibility and Work Opportunity Reconciliation (PRWORA) Act in 1996. The data make no distinction between the two programs.

¹⁵ <https://www.acf.hhs.gov/orr/resource/who-we-serve-cuban-haitian-entrants>

¹⁶ <https://www.acf.hhs.gov/orr/resource/annual-orr-reports-to-congress-2005-iii-the-lost-boys-of-sudan>

¹⁷ The original individual indicator variable in the data (FLID) has inconsistencies in terms of gender, country of origin, and date of birth. This is likely because numbers are recycled after an individual's 5-year-survey period ends. I construct a new individual indicator variable that groups individual records by the dataset's original indicator variable and fixed demographic characteristics to account for this problem.

other demographic characteristics appears to be fairly consistent. The most noticeable difference is that refugees in the early-2000s are much more fluent in English than in previous years. However, a balance test outlined in Section 5.4 suggests that these differences do not correlate much with the timing of arrival once I relate them to the country of origin as a control variable.

Table 2 provides an overview of the observed panel structure of the data. Unfortunately, there is no variable in the ASR that tells me whether an observation is in the first, second, third, fourth, or fifth iteration of the panel. However, panel IDs are unique and consistent across survey years, so I tracked these panel IDs across surveys to create my own panel iteration variable. For the first survey year, if a particular panel ID appears in the data, I assigned a value of 1 for panel iteration. If the same panel ID appears in the next survey year, I assigned a value of 2 for panel iteration. I repeated this process for all panel years and found (the bottom row of Table 2) that only 2,398 of the original 17,771 individuals are observed 5 years later.

However, some of the refugees I observed for the first time may actually be in the second, third, fourth, or fifth iterations of their panel. This is because the data I obtained starts from 1993, thus only capturing portions of previous panel waves. If a refugee was first surveyed in 1989, he would only appear once in my sample as I do not have data for survey years 1989–1992. Using the previous method, I would assign these refugees a panel iteration value of 1 even though they may actually be in their fifth year of the panel. As I do have information on the period of arrival of refugees as well as their survey-period, I have used this information to construct a variable called years-since-migration that is used throughout the manuscript to measure duration in the US. I discuss this variable in detail in Section 5.2.

Table 2 shows that out of 17,771 unique individuals observed for the first time in my data, 7,442 are in their first year, 3,619 are in their second year, 2,639 are in their third year, etc. Therefore, the best way to understand attrition in the survey is to examine the diagonals in this table. For example, in panel iteration one, 7,442 individuals are surveyed in their first year. In panel iteration two, 4,202 individuals are surveyed in their second year. In panel iteration three, 3,596 individuals are surveyed in their third year. In panel iteration four, 3,089 individuals are surveyed in their fourth year. In panel iteration five, we observe that 2,398 people remained after 5 years from an original sample of 7,442 individuals.

While it is clear that some attrition is occurring in the ASR data, this is not necessarily problematic for my empirical strategy discussed in Section 5. This is because my treatment variable never changes for the individual, so my empirical strategy consists of carrying out a comparison between groups of individuals over time, not between the same individual

Table 2 Years since migration by panel iteration

Years since Migration	Panel iteration					Total
	1	2	3	4	5	
1 Year	7,442	0	0	0	0	7,442
2 Years	3,619	4,202	0	0	0	7,821
3 Years	2,639	1,546	3,596	0	0	7,781
4 Years	2,147	1,225	1,247	3,089	0	7,708
5 Years	1,924	907	976	1,118	2,398	7,323
Total	17,771	7,880	5,819	4,207	2,398	38,075

over time. Therefore, the principal concern with attrition in the context of my empirical strategy is not whether an individual appears in each year of the survey, but whether the underlying composition of the groups I am comparing is changing over time. In Section 7.1, I assessed whether the underlying composition of these groups is changing and formally test how these underlying differences might bias my estimates.

5 Empirical Strategy

5.1 Overview

My primary empirical strategy is based on the assumption that the month and year of arrival for refugees is plausibly exogenous. I used the monthly seasonally-adjusted civilian national unemployment rate each refugee faced at arrival to proxy for initial economic conditions. Since refugees cannot choose to selectively migrate to the US based on economic conditions, percentage point changes in the arrival national unemployment rate measure the changes in outcomes for refugees arriving under different economic conditions. A rich set of controls are also used to ensure demographic characteristics, duration in the US, and contemporaneous economic conditions do not drive my results.

5.2 Base specification

The base specification is

$$y_{it} = \alpha + \beta ue_i + \delta X_i + \phi_i^m + \phi_i^c + \phi_t + \delta ue_{it}^s + \phi_{it}^k + \varepsilon_{it}$$

y_{it} is either the employment status or log wages for each refugee i in survey year t . ue_i is the monthly seasonally adjusted national unemployment rate that corresponds to the date-of-arrival of each refugee. The arrival unemployment rate never varies for a refugee, so it is not possible to measure a scarring effect by comparing an individual refugee to himself across time. Therefore, I control for individual characteristics to create comparisons between individuals with similar characteristics. X_i contains a vector of controls which includes years of education prior to arrival, gender, age, English ability at arrival, disability status, marital status, and parental status. Disability status, marital status, and parental status are questioned in the context of the period being surveyed, so I used only the initial answer given when the refugee first appears in the dataset.¹⁸

Calendar-month-of-arrival fixed effects, ϕ_i^m , are used to control for seasonal variation in the monthly unemployment rate.¹⁹ Country of origin fixed effects, ϕ_i^c , are used to ensure that only individuals from the same country are being compared with one another. Considering that push factors (where a conflict starts) and pull factors (possible discrimination on who is

¹⁸ Given that refugees are not surveyed until at least 6 months after entry, these controls could still be endogenous. However, differences between columns 5 through 8 in Tables A1–A2 in Appendix provide evidence that these endogeneity concerns do not seem to drive results.

¹⁹ I do not control for date-of-arrival as the national unemployment rate does not vary within a particular arrival month and year. In Section 8, I alternatively use the arrival placement-state unemployment rate because this provides variation in treatment for a particular arrival date, allowing me to control for both arrival month and year. However, this state treatment specification is inferior because the geographical placement is somewhat endogenous, whereas the timing of resettlement is not.

admitted based on country of origin) can determine the origin-county composition of refugees in a particular year, it is especially imperative to include this control. To account for the persistence of economic conditions, I controlled for contemporaneous year fixed effects, ϕ_t , and the contemporaneous placement-state unemployment rate, ue_{it}^s .²⁰ It is expected that poor initial economic conditions would persist for the next few years as the economy is recovering. I wanted to measure the effect of initial economic conditions that is unexplained by the economic recovery.

Finally, the years-since-migration fixed effect variable, ϕ_{it}^k , divides the number of days since each refugee arrived (calculated using the contemporaneous survey date and the documented arrival date) into intervals of 1–5. The earliest a refugee appears in the Annual Survey of Refugees data is 6 months post-arrival. Therefore, a value of 1 for k would represent a refugee who has been in the US between 6 months and 18 months. A value of 5 for k represents a refugee who has been in the US between 4.5 years and 2,175 days, the longest-tenured refugee in the sample. This control ensures that only refugees with the same number of years in the country are being compared with one another. My coefficient of interest, β , therefore measures the average effect of initial economic conditions on subsequent assimilation outcomes that is unexplained by post-arrival economic conditions for refugees of the same nationality, demographic characteristics, and years living in the US.

5.3 Preferred specification

To measure how this effect might vary over time, I borrowed from Godøy (2017)²¹ and used an interaction between arrival unemployment rates and years since migration. My preferred specification is

$$y_{it} = \alpha + \beta_k(ue_{it} \times \phi_{it}^k) + \delta X_i + \phi_i^m + \phi_i^c + \phi_i + \delta ue_{it}^s + \phi_{it}^k + \varepsilon_{it}$$

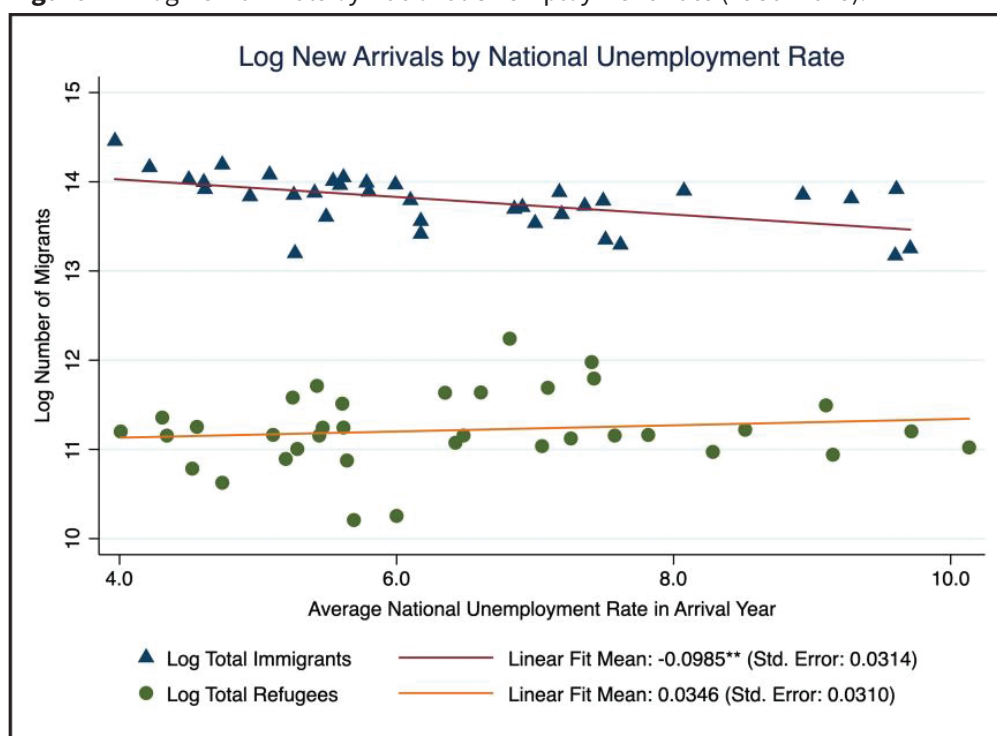
This specification is similar to the base specification, but the coefficient of interest, β_k , stratifies the average effect found in my base specification across years since migration. This specification also allows for full flexibility since I do not make any linearity assumptions regarding the interaction between years since migration and the initial unemployment rate.

5.4 Testing for exogeneity in treatment

In Figure 2, I provided evidence that total refugee immigration is not systematically related to national economic conditions. I used fiscal year refugee arrival totals found in Zong et al. (2017) for the period 1980–2015. These data cover the entire period of the refugee resettlement program. I compared these data with annual new immigrant arrival totals calculated using IPUMS American Community Survey data (Ruggles et al., 2017) for the period 1980–2015. I converted both sets of totals to logs to ease interpretation (immigrant totals are in millions

²⁰ Ideally I would like to control for the unemployment rate of the state that the refugee is currently residing. Unfortunately, this information is not available.

²¹ Godøy (2017) used immigrant employment rates instead of unemployment rates because Norway measures unemployment based on the number of registered jobseekers. Refugees in Norway have little incentive to register as jobseekers. This is not a concern in the US context because unemployment rates are derived from the randomized sampling of the entire population.

Figure 2 Log new arrivals by National Unemployment Rate (1980–2015).

Log total immigrants are based on author estimates of total immigration by year using IPUMS American Community Survey data for 2011–2016 (Ruggles et al., 2017). Log total refugees are based on estimates of total refugee migration by fiscal year from the Migration Policy Institute (Zong et al., 2017). Regressions are estimates of log totals for each population regressed on annual national unemployment rates for arrival years. Standard errors are clustered at the year-of-arrival level.

while refugee totals are in tens of thousands) and plotted them across average national unemployment rates for the time periods for which the totals were reported. The graph shows that while total immigration falls as national economic conditions worsen, refugee immigration appears unaffected, or counter-cyclical. For better precision, I regressed both sets of totals on the arrival annual national unemployment rate. I found that total immigration decreases at a statistically significant rate of 9.85% for every one percentage point increase in the national unemployment rate. Total refugee migration, however, shows no statistically significant response to changes in the national unemployment rate.

5.5 Testing for balance in treatment

As I am working with only a sample of refugees, I also need to assess whether the arrival national unemployment rate and arrival placement-state unemployment rate are not systematically related to any of my covariates. It is understood that country of origin will be systematically related to the timing of arrival for refugees because of both push and pull factors. Push factors, including the break out of conflict in a particular country at a particular time, partially determine the number of refugees who are applying to the UNHCR and US Refugee Resettlement program from that particular country. Pull factors, including differential arrival quotas of refugees by region,²² partially determine how many refugees are allowed to enter the

²² <https://www.state.gov/j/prm/releases/docsforcongress/261956.htm>

US at a particular time from a particular country. Therefore I controlled for country-of-origin to account for this.

In column 1 of Table 3, I tested whether any other covariates might be systematically related to the arrival national unemployment rate after controlling for country of origin. I used the following specification, $ue_i = \alpha + \delta X_i + \phi_i^c + \varepsilon_i$. This regression tests whether any of the covariates, X_i , are related to the arrival national unemployment rate, ue_i , after controlling for country of origin fixed effects, ϕ_i^c . For comparison purposes, in column 2 of Table 3, I also tested whether any of the covariates, X_i , are related to an alternative treatment, the state unemployment rate at arrival, ue_i^s . The specification for this column is $ue_i^s = \alpha + \delta X_i + \phi_i^c + \phi_i^o + \varepsilon_i$. This specification controls for both country of origin fixed effects, ϕ_i^c , and date-of-arrival fixed effects, ϕ_i^o . Date-of-arrival fixed effects are used to demean state unemployment rates from national economic conditions so that I can test whether covariates are related to states with better or worse economic conditions.

Column 1 of Table 3 shows that the number of years of education has a slight positive relationship with the national unemployment rate. This could provide some indication that more educated refugees are arriving in the US in worse economic conditions. However, the coefficient is very small and only marginally significant. Given that my covariates do not appear to correlate in general with the national unemployment after controlling for country of origin,

Table 3 Test of balance for continuous treatments

	(1)	(2)
	Arrival	Arrival
	National	State
	Unemp. rate	Unemp. rate
Age	-0.0012 (0.0011)	-0.0008 (0.0014)
English fluency	0.0056 (0.0396)	0.0224 (0.0455)
Years of education	0.0065+ (0.0035)	0.0050 (0.0053)
Gender	0.0107 (0.0115)	-0.0094 (0.0174)
Disability	0.0150 (0.0431)	0.1023 (0.0686)
Married	0.0049 (0.0309)	0.0955* (0.0410)
Any Children	0.0240 (0.0347)	0.0057 (0.0494)
Country-of-Origin FE	*	*
Date-of-Arrival FE		*
Observations	31,969	31,969
Adj. R^2	0.213	0.509

+0.1; *0.05; **0.01; ***0.001.

Note: Standard errors are clustered at the date-of-arrival level for national unemployment rate estimates. Standard errors are clustered at the state-of-placement-by-date-of-arrival level for state unemployment rate estimates.

I am confident that there is a minimal compositional change within nationality groups across arrival years.

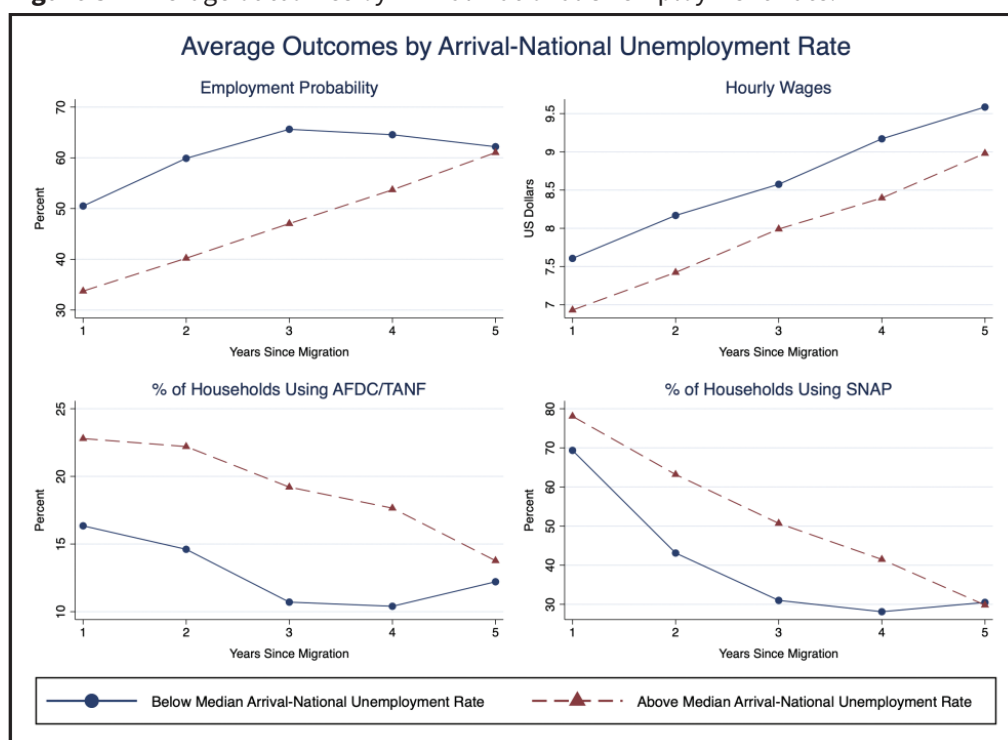
However, in column 2 of Table 3, I found that marriage is strongly correlated with the arrival placement-state unemployment rate. It means that states with worse economic conditions than the rest of the country receive more married individuals. This could potentially bias estimates using the placement-state unemployment rate downward as marriage is linked to better labor outcomes and those individuals are placed in states with worse initial economic conditions. Refugees are placed semi-randomly geographically if they do not have family already in the US. Unfortunately, the ASR data do not provide any information on refugees who are placed with family members. Therefore, family placement could be driving estimates using the placement-state unemployment rate treatment. For this reason, I rely *solely* on the national unemployment rate treatment to provide unbiased estimates of scarring for refugees.

6 Results

6.1 Overview

Figure 3 provides a naive comparison of outcomes for refugees arriving under different economic conditions, which will guide the reader on my empirical results. I first divided my

Figure 3 Average outcomes by Arrival-National Unemployment Rate.



Source: I estimated using the Annual Survey of Refugees data. The median arrival-national unemployment rate is 5.9. The gap in the average arrival-national unemployment rate between the aggregate above and below-median groups is roughly two percentage points. Employment is based on a binary variable for employment status. Hourly wages are conditional on employment and measured in real 2000 US dollars. The percentage of households is based on a binary variable defining whether or not at least one member of a household collected a particular benefit (AFDC/TANF, SNAP) in the previous year.

sample across the median arrival-national unemployment rate. I then plotted average outcomes across employment, hourly wages, and household utilization of social welfare benefits for the above and below-median groups over the 5 years sampling period. I found that refugees who arrive during an above-median arrival-national unemployment rate (bad economy) on average experience a persistent lower probability of employment, lower hourly wages conditional on employment, and an increased household usage of social welfare programs. The goal of my empirical strategy is to identify the portion of this effect that cannot be explained by demographics or subsequent economic conditions.

6.2 Base specification for employment and wages

In columns 1 and 3 of Table 4, I tested whether initial economic conditions have a general effect on employment and log wages, respectively, after accounting for demographics, duration in the US, and subsequent economic conditions. The regression performed is outlined in Sections 5.2. Employment represents employment status at the time the refugee was surveyed and should be interpreted as percentage point changes in the probability of a refugee being employed. Log wages represent a log transformation of hourly wages of employed individuals²³ and should be interpreted as (approximate) percent changes.

In column 1 of Table 4, I observed that refugees after 5 years in the US, on average, experience a 1.57 percentage point decrease in the probability of current employment for every one percentage point increase in the arrival national unemployment rate. Considering that I control

Table 4 Main results

	(1)	(2)	(3)	(4)
	Employment	Employment	Log wages	Log wages
ue_i	-0.0157** (0.0055)		-0.0198** (0.0061)	
1 year, ue_i		0.0168 (0.0104)		-0.0153+ (0.0087)
2 years, ue_i		-0.0113 (0.0073)		-0.0240** (0.0079)
3 years, ue_i		-0.0225* (0.0089)		-0.0088 (0.0075)
4 years, ue_i		-0.0360*** (0.0081)		-0.0211* (0.0091)
5 years, ue_i		-0.0208* (0.0085)		-0.0251** (0.0090)
Observations	31,815	31,815	13,772	13,772
Adj. R ²	0.202	0.203	0.251	0.251

+0.1; *0.05; **0.01; ***0.001.

Note: Standard errors are clustered at the date-of-arrival level. Robustness tables for columns 2 and 4 can be found in Tables A1 and A2 in Appendix, respectively.

²³ The log wage estimates are based only on those individuals who are employed at the time they are surveyed. This is a classic selection bias issue. To verify results, I estimate the effect of initial economic conditions on hourly wages (with those currently unemployed reporting zero dollars in wages) using a Poisson QMLE model and find results that have the same sign but are larger in magnitude, as expected.

for the contemporaneous economic conditions and years since migration, these estimates represent the effect of labor market conditions at arrival that is unexplained by the persistence of economic conditions or experience. Standard errors are clustered at the date-of-arrival level and statistically significant at the 5% level. In column 3 of Table 4, I found that refugees experience a 1.98% decrease on average in wages for every one percentage point increase in the arrival national unemployment rate. Standard errors are also clustered at the date-of-arrival level and statistically significant at the 1% level.

6.3 Preferred specification for employment and wages

To get a better understanding of how this effect might vary over time, as presented in columns 2 and 4 of Table 4, I analyzed the results found in columns 1 and 3 of Table 4, respectively, with the years since migration fixed effect. A value of “1 year, ue_0 ” represents the interaction between the arrival national unemployment rate and refugees who have been in the US between 6 months (the earliest a refugee appears in the data) and 18 months. A value of “5 years, ue_0 ” represents the interaction between the arrival national unemployment rate and refugees who have been in the US between 4.5 years and 2,175 days, the longest-tenured refugee in the sample.

Curiously, in column 2 of Table 4, I observed a *positive* relationship between employment probability and the arrival national unemployment for refugees who have been in the US between 6 months and 18 months (1 year). In Table 5, I split the sample by gender and found in column 6 that this initial increase in employment probability is owing to female refugees mostly. This might be related to the family income. In Table 6, I observed a negative relationship between welfare utilization and the arrival national unemployment rate during the first year, providing some evidence that income may be more constrained for refugees in their first year if initial economic conditions are unfavorable. Regardless, this increase in labor force attachment disappears by the second year and turns negative between the third year and fifth year, suggesting these were likely bad matches. In column 4 of Table 4, I observed a wage scarring effect that mostly persists at statistically significant levels for the entire 5 year period. In columns 4 and 8 of Table 5, I showed that this wage scarring effect is observed for both males and females at similar levels over the entire period.

6.4 Welfare utilization

In Table 6, I showed how the arrival national unemployment rate affects utilization of means-tested social welfare programs for refugees. Unlike most immigrants, refugees are an exempt group that is allowed to participate in means-tested social welfare programs during their first 5 years in the country.²⁴ This is an important outcome which ought to be investigated, owing to the fact that empirical evidence has shown that increasing access to welfare programs for refugees can lead to increases in wages (LoPalo, 2019).

In Table 6, row “1 year, ue_0 ”, I observed that refugees 1 year post-arrival show large decreases in the utilization of each program in response to worsening arrival economic conditions. It’s unclear why this is the case, but it could be related to pro-cyclical delays in scaling services,

²⁴ <https://aspe.hhs.gov/basic-report/overview-immigrants-eligibility-snap-tanf-medicaid-and-chip>

Table 5 Main results by gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male	Male	Male	Male	Female	Female	Female	Female
	Employment	Employment	Log wages	Log wages	Employment	Employment	Log wages	Log wages
ue_i	-0.0170* (0.0068)		-0.0201** (0.0075)		-0.0143* (0.0070)		-0.0198** (0.0071)	
1 year, ue_i		0.0044 (0.0132)		-0.0161 (0.0114)		0.0293* (0.0119)		-0.0153 (0.0100)
2 years, ue_i		-0.0153 (0.0099)		-0.0270** (0.0104)		-0.0072 (0.0089)		-0.0216* (0.0101)
3 years, ue_i		-0.0258* (0.0104)		-0.0030 (0.0100)		-0.0190+ (0.0113)		-0.0171* (0.0087)
4 years, ue_i		-0.0293** (0.0097)		-0.0198+ (0.0107)		-0.0429*** (0.0117)		-0.0218+ (0.0112)
5 years, ue_i		-0.0169 (0.0112)		-0.0285* (0.0111)		-0.0240* (0.0112)		-0.0208+ (0.0115)
Observations	15,748	15,748	7,504	7,504	16,067	16,067	6,268	6,268
Adj. R ²	0.203	0.203	0.248	0.249	0.191	0.192	0.232	0.232

+0.1; *0.05; **0.01; ***0.001.

Note: Standard errors are clustered at the date-of-arrival level.

Table 6 Welfare utilization

	(1)	(2)	(3)	(4)
	AFDC/TANF	AFDC/TANF	SNAP	SNAP
ue_i	-0.0019 (0.0067)		0.0094 (0.0080)	
1 year, ue_i		-0.0286* (0.0126)		-0.0416** (0.0157)
2 years, ue_i		0.0098 (0.0111)		0.0133 (0.0140)
3 years, ue_i		-0.0026 (0.0095)		0.0199+ (0.0119)
4 years, ue_i		0.0109 (0.0085)		0.0372** (0.0129)
5 years, ue_i		-0.0052 (0.0096)		0.0101 (0.0133)
Observations	31,751	31,751	31,780	31,780
Adj. R ²	0.186	0.187	0.281	0.283

+0.1; *0.05; **0.01; ***0.001.

Note: Standard errors are clustered at the date-of-arrival level.

differential guidelines across states, unobserved income, fear of stigmatization, or other unobserved factors. Refugees might have more trouble in getting approved to receive AFDC/TANF²⁵ and SNAP²⁶ benefits if these services are pro-cyclical in nature and do not scale during downturns to meet demand. Bitler and Hoynes (2016) find that TANF did not respond to the Great Recession and so extreme poverty became more cyclical as a result.

The other possible explanation is that individual states have a fair amount of latitude in how these benefits are approved and dispersed. For programs like TANF, states set income and work requirements that might make it more difficult for refugees to get approved (LaPalo, 2019). If states react to deteriorating economic conditions by limiting access to these programs, refugees would have a harder time for getting approved. Another unobserved factor is outside income. In addition to VOLAGs, refugees also work with the local community- and religious-based organizations.²⁷ If these benefits and services are counter-cyclical in nature, then refugees might enjoy increased assistance from these groups even if they arrive during a recession.

Finally, chilling, or the inhibition to exercise legitimate rights because of fear of stigmatization, might also be a contributing factor. In 1997, the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) denied eligibility to most welfare programs for immigrants who had been in the country for <5 years. Despite refugees being exempt from this policy change, utilization of these programs by refugees dropped 37% after the law was passed (Fix and Passel, 1999).

Regardless, welfare utilization levels for refugees who arrived during bust periods are roughly the same as refugees who arrived during boom periods after the first year. There is

²⁵ AFDC/TANF is a cash grant program for families with children, <https://www.cbpp.org/research/policy-basics-an-introduction-to-tanf>

²⁶ SNAP is a food nutrition program that provides vouchers and/or debit cards to purchase food, <https://www.fns.usda.gov/snap/supplemental-nutrition-assistance-program-snap>

²⁷ <https://www.acf.hhs.gov/orr/state-programs-annual-overview>

also some evidence of an increase in the utilization of SNAP benefits after the first year, but a statistically significant effect is only observed in the fourth year post-arrival. On average for the entire 5 years period, I observed no statistically significant change in welfare utilization.

6.5 Heterogeneity within employment and wage estimates

In addition to looking at the entire sample population, I also assessed whether scarring might differ across gender and origin-country educational attainment. As stated in Section 6.3, I showed in Table 5 that male and female refugees have different employment probabilities in the first year, but experience similar employment scarring effects in later years. Wage scarring persists for both male and female refugees throughout the entire 5 years period. In Tables 7 and 8, I further split the sample based on educational attainment. Educational attainment is classified as “No High School” for refugees with <12 years of education in their country of origin. I classified refugees who report between 12 years and 15 years of education in their country of origin as “High School.” Finally, I classified refugees who completed >16 years of education in their country of origin as “College.”

Table 7 Heterogeneity within employment estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	No HS	HS	College	No HS	HS	College
	Males	Males	Males	Females	Females	Females
ue_i	-0.0149 (0.0094)	-0.0190+ (0.0099)	-0.0101 (0.0231)	0.0032 (0.0100)	-0.0352** (0.0107)	-0.0423+ (0.0246)
Observations	6,596	7,762	1,390	7,980	6,984	1,103
Adj. R ²	0.177	0.187	0.270	0.165	0.185	0.224

+0.1; *0.05; **0.01; ***0.001.

	(1)	(2)	(3)	(4)	(5)	(6)
	No HS	HS	College	No HS	HS	College
	Males	Males	Males	Females	Females	Females
1 year, ue_i	0.0130 (0.0206)	0.0143 (0.0169)	-0.0632 (0.0433)	0.0627*** (0.0161)	0.0093 (0.0198)	-0.0856+ (0.0478)
2 years, ue_i	-0.0078 (0.0148)	-0.0131 (0.0143)	-0.0373 (0.0330)	0.0081 (0.0128)	-0.0224 (0.0148)	-0.0118 (0.0353)
3 years, ue_i	-0.0339* (0.0151)	-0.0246 (0.0151)	0.0050 (0.0303)	-0.0031 (0.0155)	-0.0347* (0.0158)	-0.0582+ (0.0307)
4 years, ue_i	-0.0226 (0.0143)	-0.0463*** (0.0127)	0.0400 (0.0324)	-0.0382* (0.0160)	-0.0596*** (0.0158)	-0.0317 (0.0385)
5 years, ue_i	-0.0210 (0.0175)	-0.0202 (0.0148)	0.0101 (0.0367)	-0.0054 (0.0162)	-0.0549*** (0.0149)	-0.0357 (0.0472)
Observations	6,596	7,762	1,390	7,980	6,984	1,103
Adj. R ²	0.178	0.187	0.271	0.168	0.186	0.223

+0.1; *0.05; **0.01; ***0.001.

Note: Standard errors are clustered at the date-of-arrival level.

Table 8 Heterogeneity within log wage estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	No HS	HS	College	No HS	HS	College
	Males	Males	Males	Females	Females	Females
ue_i	-0.0070 (0.0094)	-0.0282** (0.0098)	-0.0668** (0.0251)	-0.0191* (0.0076)	-0.0082 (0.0097)	-0.0799* (0.0321)
Observations	2,634	4,224	646	2,641	3,194	433
Adj. R ²	0.200	0.276	0.236	0.196	0.223	0.205

+0.1; *0.05; **0.01; ***0.001.

	(1)	(2)	(3)	(4)	(5)	(6)
	No HS	HS	College	No HS	HS	College
	Males	Males	Males	Females	Females	Females
1 year, ue_i	-0.0011 (0.0161)	-0.0150 (0.0164)	-0.0967** (0.0340)	-0.0071 (0.0123)	0.0026 (0.0146)	-0.1520** (0.0510)
2 years, ue_i	-0.0001 (0.0149)	-0.0385** (0.0136)	-0.0641+ (0.0373)	-0.0191+ (0.0105)	-0.0096 (0.0149)	-0.0455 (0.0465)
3 years, ue_i	-0.0007 (0.0130)	-0.0067 (0.0129)	-0.0480 (0.0352)	-0.0368*** (0.0109)	0.0064 (0.0112)	-0.0746+ (0.0443)
4 years, ue_i	-0.0097 (0.0143)	-0.0251+ (0.0133)	-0.0466 (0.0466)	-0.0214+ (0.0119)	-0.0197 (0.0154)	-0.0116 (0.0506)
5 years, ue_i	-0.0190 (0.0167)	-0.0420** (0.0136)	-0.0741 (0.0475)	-0.0111 (0.0146)	-0.0123 (0.0149)	-0.1264* (0.0572)
Observations	2,634	4,224	646	2,641	3,194	433
Adj. R ²	0.199	0.276	0.232	0.197	0.223	0.211

+0.1; *0.05; **0.01; ***0.001.

Note: Standard errors are clustered at the date-of-arrival level.

Tables 7 and 8 are divided into two parts. The first part shows the average effect of the arrival national unemployment rate, similar to columns 1 and 3 of Table 4. The second part shows the results of interaction between the arrival national unemployment rate and years since migration, similar to columns 2 and 4 of Table 4. Broadly, it appears that college-educated refugees experience poorer outcomes from entering the US during a recession than less-educated refugees. In column 4 of Table 7, I found that non-high-school-educated female refugees are the primary group driving the initial increase in employment probability. In columns 3 and 6 of Table 7, I found that college-educated male and female refugees are much less likely to enter the job market during the first year if arrival economic conditions are unfavorable. I also observed poorer employment probabilities for less-educated refugees in later periods.

In Table 8, I found strong evidence leading to the conclusion that college-educated male and female refugees experience poorer wage outcomes than their less-educated peers as a result of poor initial economic conditions. This is probably because college-educated refugees have a better chance of finding a job commiserate with their skill level if initial economic conditions are favorable. Non-high-school-educated male refugees have the best outcomes of any gender-education group, but all groups suffer some degree of persistent wage-scarring. Unfortunately, statistical power is not available to make a precise determination.

7 Additional Checks on Interval Validity

7.1 Testing for changes in composition

In Section 4, I provided an overview of the ASR data and described potential attrition issues with the panel data. Since my treatment variable never varies for the individual refugee, I am not comparing individual refugees to themselves over time. I am comparing individuals to similar individuals over time. Therefore, the principal concern with attrition is not the number of panels a particular person appears, but whether there are differences in the underlying composition of individuals over time, as measured by years-since-migration. Composition changes across years-since-migration can create a bias if the trajectory of these changes differs between those who entered the US when there were conditions of high and low unemployment prevailing, respectively. In Table 9, I provided descriptive summary statistics across years-since-migration between those entering the country during bust periods and boom periods, respectively. This table splits the sample by the median monthly national-unemployment-rate-at-arrival (the same methodology used to construct Figure 3). For the above-median group, or those who enter during a busting economy, I found some evidence of composition changes between 1 year and 5 years post-migration. Refugees who enter the US during bust periods and observed 5 years thereafter are more likely to be male, younger, and less likely to be educated, fluent in English at arrival, married at arrival, or have children when they arrive. Refugees who enter the US during boom periods (the below-median group) show fewer changes in education and gender, but I do observe several similarities with the above-median group in regards to the trajectory of these composition changes. Refugees who enter the US during boom periods and observed 5 years thereafter are also younger, less fluent in English at arrival, less likely to be married, and less likely to have children. However, refugees who are disabled at arrival are less likely to appear in later years if they migrate during boom periods.

Table 9 Summary statistics by years since migration

Above median	1	2	3	4	5	All
Years of education	10.80	10.84	10.73	10.34	10.04	10.58
% Female	51.33	51.24	50.96	49.41	50.89	50.79
Age at arrival	34.29	33.74	32.94	32.00	31.56	33.01
% Fluent in English	9.16	6.86	8.50	8.39	6.63	7.97
% Disabled	10.85	11.55	10.88	10.43	11.17	10.98
% Married	62.61	61.99	61.92	60.01	60.96	61.55
% Have children	63.50	62.18	62.76	61.64	60.35	62.17
Below median	1	2	3	4	5	All
Years of education	10.34	10.83	10.84	10.87	10.63	10.72
% Female	50.28	50.62	51.32	50.39	50.62	50.67
Age at arrival	34.78	34.53	33.94	33.47	32.23	33.71
% Fluent in English	8.71	8.33	6.74	5.29	4.94	6.66
% Disabled	14.00	13.70	12.32	11.81	10.06	12.25
% Married	56.30	58.45	58.74	57.19	55.33	57.22
% Have children	57.51	54.33	54.40	54.33	56.37	55.31

To test formally how these composition changes might bias my estimates, I used a predicted outcomes test. I first regressed employment probability and log wages, y_{it} , on all the covariates, X_i : years of education, gender, age, English fluency at arrival, disability at arrival, marriage at arrival, and whether or not you are a parent at arrival. I then regressed the predicted outcomes from the first regression on my original specification, without covariates, to provide a means of comparison with my main results found in Table 4.

$$\text{Step 1: } y_{it} = \alpha + \gamma X_i + \varepsilon_{it}$$

$$\text{Step 2: } \hat{y}_{it} = \alpha + \beta ue_i + \phi_i^m + \phi_i^c + \phi_i + \delta ue_{it}^s + \phi_{it}^k + \varepsilon_{it}$$

As explained in Section 5, ue_i is the monthly national-unemployment-rate each refugee faces at arrival. ϕ_i^m , ϕ_i^c , ϕ_i , and ϕ_{it}^k correspond to calendar-month-of-arrival, country-of-origin, contemporaneous year, and years-since-migration fixed effects, respectively. ue_{it}^s is the contemporaneous state-of-placement unemployment rate. In addition, I have also regressed the predicted outcomes from the regression in Step 1 on my preferred specification outlined in Section 5.3, without covariates that include an interaction between the arrival national unemployment rate and years-since-migration.

$$\text{Step 2a: } \hat{y}_{it} = \alpha + \beta_k (ue_i \times \phi_{it}^k) + \phi_i^m + \phi_i^c + \phi_i + \delta ue_{it}^s + \phi_{it}^k + \varepsilon_{it}$$

The regressions in Steps 2 and 2a are measuring the portion of my estimated scarring effect that is predicted by changes in composition. If there are no differential changes in composition over time between cohorts which arrived during boom periods and bust periods, respectively, I should observe a zero effect. If I observe a non-zero effect, the sign and magnitude provide an estimate of how much of the observed scarring effect is driven or attenuated by composition changes.

Table 10 shows the results of this analysis and is analogous to Table 4. There is some evidence that composition changes do affect my employment estimates in periods 1, 4, and 5, and my wage estimates in period 5. However, when I compared the estimates found in Table 10 with my main estimates in Table 4, I saw that all the signs of the significant coefficients in Table 10

Table 10 Test for changes in composition

	(1)	(2)	(3)	(4)
	Employment	Employment	Log wages	Log wages
ue_i	0.0052*		0.0010	
	(0.0023)		(0.0015)	
1 year, ue_i		-0.0092*		-0.0003
		(0.0044)		(0.0026)
2 years, ue_i		0.0026		0.0014
		(0.0027)		(0.0017)
3 years, ue_i		0.0045		0.0003
		(0.0028)		(0.0019)
4 years, ue_i		0.0079*		-0.0009
		(0.0034)		(0.0021)
5 years, ue_i		0.0155***		0.0035+
		(0.0038)		(0.0020)
Observations	31,974	31,974	31,974	31,974
Adj. R ²	0.077	0.078	0.197	0.197

+0.1; *0.05; **0.01; ***0.001.

Note: Standard errors are clustered at the date-of-arrival level.

are opposite to the coefficients laid down in Table 4. This means that composition changes are actually attenuating my results, not driving them. For example, in Table 4, I estimated that refugees entering the US face a 2.08 percentage point reduction in their employment probability for every percentage point increase in the arrival unemployment rate after 5 years. However, the estimate in column 2 of Table 10, row “5 years, ue_0 ” is positive. This suggests that composition changes are responsible for a 1.55 percentage point *increase* in this estimate. Therefore the true effect for employment scarring in the fifth year might be closer to a drop of 3.64 percentage points in magnitude. Conversely, the coefficient in row “1 year, ue_0 ” in Table 10 is negative, while the coefficient in my main results table, Table 4, is a positive estimate of 1.68 percentage points. This suggests that the increase in employment probability in the first period is likely closer to 2.6 percentage points. Overall, the composite effect of 0.52 percentage points found in column 1 of Table 10 suggests that the composite estimate for employment scarring in column 1 of Table 4 is likely closer to a drop of 2.09 percentage points.

7.2 Mobility

Post-arrival interstate mobility is another important outcome that could be affected by initial economic conditions. A refugee placed in a state with poorer economic conditions than neighboring states could move and potentially experience better outcomes. Wozniak (2010) has made the observation that economic improvement in states can drive relocation for highly educated workers. Unfortunately, the Annual Survey of Refugees data do not offer a credible way to test actual mobility. There is no information on a refugee’s current state of residence. There is a question about whether a refugee lived in the same state in the previous year, but a large portion (>40%) of the observations is missing. However, I can use the remaining sample of observed non-movers to gain a better understanding of how post-arrival mobility might affect my estimates. In Table 11, I showed the results of regression on a sub-sample of known non-movers using the national-unemployment-rate-at-arrival treatment. The estimates in

Table 11 Main results for non-movers

	(1)	(2)	(3)	(4)
	Employment	Employment	Log wages	Log wages
ue_i	-0.0227** (0.0071)		-0.0263*** (0.0078)	
1 year, ue_i		0.0213 (0.0145)		-0.0275* (0.0128)
2 years, ue_i		-0.0012 (0.0095)		-0.0311** (0.0106)
3 years, ue_i		-0.0380** (0.0130)		-0.0135 (0.0111)
4 years, ue_i		-0.0538*** (0.0120)		-0.0330** (0.0115)
5 years, ue_i		-0.0345** (0.0114)		-0.0229* (0.0115)
Observations	18,289	18,289	8,190	8,190
Adj. R^2	0.192	0.193	0.246	0.246

+0.1; *0.05; **0.01; ***0.001.

Note: Standard errors are clustered at the date-of-arrival level.

Table 11 are larger in magnitude than the main estimates in Table 4, suggesting that post-arrival movement is likely attenuating national unemployment rate estimates. In Section 7.1, I have shown that composition changes are also likely attenuating my estimates. The post-arrival movement will be a driver of changes in composition over time if it is assumed that refugees who move are also less likely to participate in future surveys.

7.3 Testing for robustness

As a robustness check for my preferred estimates in Table 4, I have also included estimates from alternate specifications in Tables A1–A2 in Appendix. Column 1 of Tables A1 and A2 in Appendix shows results without any of the following covariates: years of education at arrival, gender, age, English fluency at arrival, marital status at arrival, disability status at arrival, and parental status at arrival. Columns 2–8 of Tables A1 and A2 in Appendix show how these estimates change as each covariate is added, with column 8 being the preferred specification.

8 State Unemployment Rate Treatment

In a separate regression, I have used the arrival placement-state unemployment rate to test another plausibly exogenous feature of the US Refugee Resettlement program. Refugees who do not have family living in the US are also placed semi-randomly geographically.²⁸ If refugees are also unable to migrate selectively to a particular state based on economic conditions, then percentage point differences in the arrival placement-state unemployment rate, after controlling for national economic conditions, could provide a better estimate of scarring effects because variation in this treatment allows me to also control for date-of-arrival.

The base specification using the arrival placement-state unemployment rate treatment is

$$y_{it} = \alpha + \beta ue_i^s + \delta X_i + \phi_i^c + \phi_i + \delta ue_{it}^s + \phi_{it}^k + \phi_i^0 + \phi_i^s + \varepsilon_{it}.$$

This specification is similar to the specification using the arrival national unemployment rate, with two extra controls the two controls. Date-of-arrival fixed effects, ϕ_i^0 , are used instead of calendar-month-of-arrival fixed effects since this treatment variable has a state-level variation for the date-of-arrival. The date-of-arrival fixed effect controls for national economic trends at the time of arrival. State fixed effects, ϕ_i^s , control for general differences between states. With these controls, the coefficient of interest, β , should be interpreted as the effect of initial state labor market conditions deviating from the national average that is unexplained by the persistence of economic conditions, experience, or idiosyncratic differences between states.

The preferred specification using the arrival placement-state unemployment rate is

$$y_{it} = \alpha + \beta_k (ue_i^s \times \phi_{it}^k) + \delta X_i + \phi_i^c + \phi_i + \delta ue_{it}^s + \phi_{it}^k + \phi_i^0 + \phi_i^s + \varepsilon_{it}.$$

The preferred specification for this treatment also relies on an interaction between the arrival placement-state unemployment rate and years since migration to stratify the effect across years since migration.

Unfortunately, there is no information in the data regarding whether a refugee already has family living in the country,²⁹ so an unknown portion of my sample is not being placed

²⁸ <https://www.state.gov/refugee-admissions/reception-and-placement/>

²⁹ Unfortunately, there is no published information on how many of these individuals have family already living in the country. My discussions with former employees of various VOLAGS suggest it could be as high as 50% of refugees.

semi-randomly geographically. This is not a concern with national estimates as having a family in the US prior to arrival does not affect the timing of arrival, as all refugees are subject to 18–24 months of pre-arrival screening.³⁰ A balance test outlined in Section 5.5 suggests that marital status at arrival might be systematically related to local state economic conditions. In Section 8.2, I attempt to reduce this potential bias by restricting my sample to refugees less likely to have family already living in the US.

8.1 Employment probability and wages

In Table 12, I showed the results of the arrival placement-state unemployment rate treatment on employment probability and log wages. The estimates suggest that refugees experience a slight increase in employment probability in their fifth year, while wage scarring decreases each year. However, I do not have the means to differentiate between refugees who are, respectively, placed with family and placed randomly geographically. Therefore, it is not possible to determine whether these estimates reflect a true decrease in wage scarring or if they are the result of non-random placement in areas with better economic conditions.

8.2 Testing placement—state treatment on a restricted sample

Further, to overcome this selection bias problem in my arrival state-placement estimates, I limited my analysis to refugees who are less likely to have family already living in the US. If a refugee is one of the first to be resettled from their home country, it is less likely they have family already living here. To achieve this, I create two different groups of pioneers. The first group, nationality-by-state pioneers, represents refugees who are resettled in a particular state

Table 12 State unemployment estimates

	(1)	(2)	(3)	(4)
	Employment	Employment	Log wages	Log wages
ue_si	0.0049 (0.0067)		-0.0140* (0.0060)	
1 year, ue_si		0.0017 (0.0085)		-0.0249*** (0.0071)
2 years, ue_si		-0.0080 (0.0075)		-0.0238*** (0.0071)
3 years, ue_si		0.0046 (0.0076)		-0.0153* (0.0068)
4 years, ue_si		0.0071 (0.0076)		-0.0117+ (0.0066)
5 years, ue_si		0.0137+ (0.0072)		-0.0067 (0.0063)
Observations	31,815	31,815	13,772	13,772
Adj. R ²	0.221	0.221	0.278	0.278

+0.1; *0.05; **0.01; ***0.001.

Note: Standard errors are clustered at the state-of-placement-by-date-of-arrival level.

³⁰ <https://www.state.gov/refugee-admissions/u-s-refugee-admissions-program-access-categories/>

within 2 years of the nationality's first appearance in that state. I use both the Annual Survey of Refugees data and previous ORR Annual Reports³¹ to assess whether a refugee of a particular nationality has been placed in a state before. The second group, nationality pioneers, represents refugees who are resettled within 2 years of their nationality's first appearance in the US. The second method is more restrictive in terms of likely pioneers, so comparing the two restricted samples should provide some understanding of the direction of this potential bias. In addition, I also drop refugees from both groups that come from countries that constitute >0.1% of their placement state's population in the month and year they immigrate. These shares are calculated using population weights, state of residence, and country-of-origin variables in the US Current Population Survey (Ruggles et al., 2017). These country-of-origin shares of state population estimates are then merged with my original ASR data by date-of-arrival and placement state.

In Tables 13 and 14, I showed the estimates of the effect of the arrival placement-state unemployment rate on these two groups of pioneers. In columns 1 and 2 of Table 13, a statistically significant wage scarring effect is observed in the first year for nationality-by-state pioneers. However, the magnitudes of the estimates in columns 1 and 2 of Table 13 are similar

Table 13 State unemployment estimates using pioneers

	Nationality-by-state pioneers	
	(1)	(2)
	Employment	Log wages
ue_si	-0.0048 (0.0155)	-0.0359+ (0.0183)
Observations	4,800	2,169
Adj. R ²	0.292	0.332
1 year, ue_si	0.0148 (0.0237)	-0.0579* (0.0242)
2 years, ue_si	-0.0114 (0.0181)	-0.0408+ (0.0225)
3 years, ue_si	-0.0006 (0.0174)	-0.0368+ (0.0204)
4 years, ue_si	-0.0026 (0.0177)	-0.0325 (0.0200)
5 years, ue_si	-0.0143 (0.0170)	-0.0323+ (0.0187)
Observations	4,800	2,169
Adj. R ²	0.293	0.332

+0.1; *0.05; **0.01; ***0.001.

Note: Nationality-by-state Pioneers are refugees who are resettled in a particular state within 2 years of their nationality's first appearance in that state either in the data or in previous ORR Annual reports.

Standard errors are clustered at the state-of-placement-by-date-of-arrival level.

³¹ <https://www.acf.hhs.gov/orr/resource/annual-orr-reports-to-congress>

Table 14 State unemployment estimates using pioneers

	Nationality pioneers	
	(3)	(4)
	Employment	Log wages
ue_si	-0.0314 (0.0259)	-0.1250** (0.0419)
Observations	1,739	681
Adj. R^2	0.298	0.347
1 year, ue_si	0.0028 (0.0440)	-0.1322+ (0.0713)
2 years, ue_si	-0.0621* (0.0312)	-0.1058* (0.0513)
3 years, ue_si	-0.0305 (0.0289)	-0.1321** (0.0461)
4 years, ue_si	-0.0241 (0.0313)	-0.1387** (0.0419)
5 years, ue_si	-0.0582+ (0.0306)	-0.1245** (0.0445)
Observations	1,739	681
Adj. R^2	0.300	0.343

+0.1; *0.05; **0.01; ***0.001.

Note: Nationality Pioneers are refugees who are resettled within 2 years of their nationality's first appearance in the US in general, either in the data or in previous ORR Annual reports. Standard errors are clustered at the state-of-placement-by-date-of-arrival level.

to the results found in Table 4 using the arrival national unemployment rate treatment. In Table 14, I gain more precision and find that estimates are larger in magnitude than Table 4, but also follow a similar pattern. This provides evidence that my original arrival placement-state unemployment rate estimates are likely biased toward positive outcomes by an unknown number of sample respondents being placed near family.

9 Conclusion

This study provides evidence of both wage and employment scarring among refugees who migrate to the US. A one percentage point increase in the arrival national unemployment rate reduces refugee wages by 1.98% and their probability of employment by 1.57 percentage points after 5 years. I also find evidence that welfare access and utilization can affect the labor supply decisions for female refugees. Unfortunately, this increase in labor supply does not appear to be persistent suggesting that these are likely bad matches. On the other hand, wage scarring is unaffected by labor supply decisions and persists for 5 years.

I also attempt to understand how interstate migration might help mitigate these effects. Using the placement-state unemployment rate at arrival, I find no evidence of employment

scarring effect and a less-persistent wage scarring effect. However, empirical tests show that estimates using the arrival placement-state unemployment rate may be biased downward due to an unknown number of refugees being placed near their families. Therefore, I rely on the arrival national unemployment rate treatment to provide unbiased estimates of employment and wage scarring for refugees. To account for potential bias in my estimates using the placement-state unemployment rate as treatment, I limit my sample to two sets of pioneers. One group is defined as refugees who were among the first of a certain nationality to resettle in a particular state. The second group is defined as refugees who were among the first of a certain nationality to resettle in the US in general. Comparisons between estimates obtained using these two sample groups suggest that the state unemployment rate estimates are probably positively biased and that the true employment and wage scarring effects are probably more severe than estimates that do not account for differences which arise from family placement.

Declarations

Availability of data and materials

The datasets used and/or analyzed during the current study are available from the corresponding author on reasonable request.

Competing Interests

None.

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Appendix

Table A1 Employment estimates—national unemployment rate treatment (robustness table for results found in Column 2 of Table 3)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 year, ue_j	0.0078 (0.0107)	0.0035 (0.0106)	0.0041 (0.0106)	0.0057 (0.0106)	0.0073 (0.0105)	0.0105 (0.0106)	0.0151 (0.0104)	0.0168 (0.0104)
2 years, ue_j	-0.0087 (0.0080)	-0.0149+ (0.0077)	-0.0145+ (0.0076)	-0.0139+ (0.0076)	-0.0141+ (0.0076)	-0.0142+ (0.0074)	-0.0117 (0.0074)	-0.0113 (0.0073)
3 years, ue_j	-0.0183* (0.0091)	-0.0234* (0.0091)	-0.0232* (0.0091)	-0.0230* (0.0091)	-0.0224* (0.0090)	-0.0249** (0.0091)	-0.0228* (0.0090)	-0.0225* (0.0089)
4 years, ue_j	-0.0285*** (0.0083)	-0.0331*** (0.0083)	-0.0322*** (0.0082)	-0.0332*** (0.0082)	-0.0326*** (0.0082)	-0.0366*** (0.0082)	-0.0359*** (0.0082)	-0.0360*** (0.0081)
5 years, ue_j	-0.0055 (0.0091)	-0.0129 (0.0089)	-0.0129 (0.0088)	-0.0144+ (0.0087)	-0.0148+ (0.0086)	-0.0210* (0.0087)	-0.0212* (0.0085)	-0.0208* (0.0085)
Country of origin	*	*	*	*	*	*	*	*
Years of education	*	*	*	*	*	*	*	*
Gender			*	*	*	*	*	*
Age				*	*	*	*	*
English at arrival					*	*	*	*
Disabled at arrival					*	*	*	*
Married at arrival					*	*	*	*
Any children at arrival					*	*	*	*
Years since migration FE	*	*	*	*	*	*	*	*
Contemp. year FE	*	*	*	*	*	*	*	*
Month of arrival FE	*	*	*	*	*	*	*	*
Contemp. state UR	-0.0541*** (0.0039)	-0.0539*** (0.0037)	-0.0540*** (0.0037)	-0.0535*** (0.0037)	-0.0536*** (0.0037)	-0.0518*** (0.0038)	-0.0530*** (0.0039)	-0.0529*** (0.0039)
Observations	31,815	31,815	31,815	31,815	31,815	31,815	31,815	31,815
Adj. R ²	0.116	0.161	0.170	0.174	0.175	0.195	0.201	0.203

+0.1; *0.05; **0.01; ***0.001.

Note: Standard errors are clustered at the date-of-arrival level.

Table A2 Log wage estimates—national unemployment rate treatment (robustness table for results found in column 4 of Table 3)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 year, ue_si	-0.0215* (0.0095)	-0.0221* (0.0088)	-0.0200* (0.0087)	-0.0199* (0.0088)	-0.0182* (0.0087)	-0.0181* (0.0087)	-0.0153+ (0.0087)	-0.0153+ (0.0087)
2 years, ue_si	-0.0234** (0.0087)	-0.0251** (0.0080)	-0.0243** (0.0080)	-0.0246** (0.0081)	-0.0252** (0.0080)	-0.0253** (0.0080)	-0.0240** (0.0079)	-0.0240** (0.0079)
3 years, ue_si	-0.0089 (0.0083)	-0.0099 (0.0075)	-0.0097 (0.0076)	-0.0098 (0.0076)	-0.0092 (0.0076)	-0.0092 (0.0076)	-0.0088 (0.0075)	-0.0088 (0.0075)
4 years, ue_si	-0.0218* (0.0097)	-0.0199* (0.0091)	-0.0201* (0.0091)	-0.0207* (0.0091)	-0.0203* (0.0091)	-0.0205* (0.0091)	-0.0211* (0.0091)	-0.0211* (0.0091)
5 years, ue_si	-0.0181+ (0.0095)	-0.0205* (0.0092)	-0.0222* (0.0090)	-0.0227* (0.0090)	-0.0237** (0.0089)	-0.0239** (0.0090)	-0.0251** (0.0090)	-0.0251** (0.0090)
Country of origin	*	*	*	*	*	*	*	*
Years of education								
Gender								
Age								
English at arrival								
Disabled at arrival								
Married at arrival								
Any children at arrival								
Years since migration FE	*	*	*	*	*	*	*	*
Contemp. year FE	*	*	*	*	*	*	*	*
Month of arrival FE	*	*	*	*	*	*	*	*
Contemp. state UR	-0.0183*** (0.0036)	-0.0190*** (0.0036)	-0.0200*** (0.0035)	-0.0204*** (0.0035)	-0.0205*** (0.0035)	-0.0205*** (0.0035)	-0.0204*** (0.0035)	-0.0204*** (0.0035)
Observations	13,772	13,772	13,772	13,772	13,772	13,772	13,772	13,772
Adj. R ²	0.178	0.221	0.243	0.244	0.247	0.247	0.251	0.251

+0.1; *0.05; **0.01; ***0.001.

Note: Standard errors are clustered at the date-of-arrival level.