Essays on the Long-Term Consequences from Entering the Labor Market During a Recession

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THESIS
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For my wife, Priya, who supported me through every step of this journey.

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PREFACE

This dissertation primarily focuses on the topic of wage scarring. Wage scarring is the long-term negative disparity in wages that results from starting a career during a recession. Oreopoulus et al. (2012) and Kahn (2010) show that this disparity can last for up to 10 years and 20 years, respectively. I first became interested in this topic after witnessing the job search struggles many of my friends had after graduating college in 2008. I was lucky. I graduated in 2006. A tight labor market at the start of my career meant that I had relative ease finding a good paying job commensurate with my degree. Two years after I graduated, however, and the US economy was ravaged by the housing market crash. My friends starting their careers during that time were not as lucky. Without an abundance of available jobs reflecting what they learned in school, many were forced to take jobs to make ends meet instead. Through no fault of their own, the investment they made in their education had a significantly lower return than mine.

I first study this topic in the context of migration. While it has been shown in the literature that starting a career during a recession can harm wages for college graduates, there is not much literature on the severity of this effect for other groups. In Chapter 1, I ask what happens when you migrate to the US during a recession. Although this question seems fairly straightforward, it is difficult to answer because of selective migration. As economic conditions worsen, potential migrants may become less likely to move. To sidestep this selection problem, I use refugees as a my primary population of interest. Due to unique restrictions related to the US Refugee Resettlement program, refugees are not able to choose when they can migrate to the US. By exploiting the timing of these refugee arrivals, I am able to measure how much of an effect local economic conditions can have on employment and wage outcomes for refugees. I show that migration during a recession also creates a long-term negative wage disparity, especially for the most vulnerable migrant groups.

I also study the mechanisms behind wage scarring. In particular, I want to understand why this effect persists. It makes sense why wage offers for inexperienced workers might be depressed during a recession. With less jobs available and more people applying, inexperienced workers face much higher competitive pressure. However, even after the economy recovers and these competitive pressures dissipate, the negative wage disparity observed with this cohort still persists. In the literature (Oreopoulus et al., 2012), job mobility is suggested as the principal cause. If scarred workers are not switching jobs once the economy recovers, they will not see increases in their wages. As these workers get older, the costs involved with switching jobs becomes greater. In Chapter 2, I show this disparity also persists because employers use prior wages to screen job applicants. This is a notable finding because it means that scarred workers may not see the same level of wage growth from switching jobs as non-scarred workers. As a result, it may take even more job mobility for scarred workers to reach parity with non-scarred workers.

As recessions continue to disrupt the long-term career plans of young graduates and new labor market entrants, it is important to understand both the severity of this effect and the root causes of why it persists. This dissertation offers insight in both areas. I show that wage scarring can be especially severe for vulnerable immigrant groups. I also show that this effect persists partially because employers ask job applicants about their current and past salary. These contributions help to underline the importance of studying wage scarring and also offer insight into ways to potentially reverse this effect through policy.

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AFDC	Aid to Families with Dependent Children
ASR	Annual Survey of Refugees
BLS	Bureau of Labor Statistics
C&S	Callaway and Sant'Anna (2020)
CEPR	Center for Economic and Policy Research
CPS	Current Population Survey
CPS-ORG	Current Population Survey - Outgoing Rotational Group
DDD	Difference-in-differences-in-differences empirical strategy
DID	Difference-in-differences empirical strategy
IPUMS	Integrated Public Use Microdata Series
J2J	Job-to-job transition
NGO	Non-governmental organization
OLS	Ordinary Least Squares

LIST OF ABBREVIATIONS (continued)

ORR	Office of Refugee Resettlement
PRWORA	Personal Responsibility and Work Opportunity Reconciliation Act of 1996
PUA	Pandemic Unemployment Assistance
QMLE	Quasi-maximum likelihood
SHB	Salary History Ban laws
SNAP	Supplemental Nutrition Assistance Program
TANF	Temporary Assistance for Needy Families
TWFE	Two-way fixed effects model
U2E	Unemployment-to-employment transition
UNHCR	United Nations High Commission for Refugees
VOLAG	Non-profit volunteer resettlement agency

SUMMARY

Chapter 1: 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.

Chapter 2: Salary History Bans and Healing Scars from Past Recessions

Abstract: In a recession, increased competition forces inexperienced job market entrants to accept lower wages than those who start their careers during an economic boom. Yet despite years of improvement in labor market conditions following a recession, a wage disparity, known as scarring, persists between these cohorts. I use Salary History Ban laws (SHBs) to test whether job mobility for scarred workers is constrained because employers screen on prior compensation. For scarred workers who began their careers during a moderate-to-severe recession, or a 5 percentage point higher state unemployment rate, I find SHBs increase job mobility by 0.6%, hourly wages by 3.4%, and weekly earnings by 5.45% relative to workers who graduated in baseline labor market conditions. These estimates represent a substantial reduction in the original scarring effect and provide evidence this effect partially persists due to salary disclosure.

1 CONSEQUENCES OF IMMIGRATING DURING A RECESSION: EVIDENCE FROM THE US REFUGEE RESETTLEMENT PROGRAM

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1.1 Introduction

The timing of labor market entry matters. Several studies (Oyer, 2006; Oyer, 2008; Kahn, 2010; Oreopoulus 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

[1] A copy of the open access licensing agreement from the *IZA Journal of Development and Migration* granting permission to reprint this material is available in the Appendix on page 116.

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,[2] easily migrate between countries,[3] or choose where they are eventually resettled.[4] If selected to resettle in the US, they must also undergo 18–24 months of screening before arrival.[5] 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.

[2] https://www.unrefugees.org/refugee-facts/what-is-a-refugee/

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

[4] https://www.pbs.org/newshour/world/asked-refugees-referred-live-u-s

[5] https://refugees.org/explore-the-issues/our-work-with-refugees/security-screening/

(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 a various geographic settings at different points of time. They find that poor initial economic conditions can decrease wages for refugees for 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.[6] The US also has more geographic variation and ethnic diversity, providing more variation in potential outcomes for refugees. The US Refugee 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 out-

[6]https://www.unhcr.org/statistical-yearbooks.html

comes 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.[7]

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.

[7] https://www.state.gov/j/prm/ra/receptionplacement/

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.

1.2 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."[8] The UNHCR makes a decision on where to send these individuals based on 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.[9] In rare cases, officials expedite this process deliberately because of an emergency; even in such instances, the minimum wait time is still 6 months.[10]

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

[8]http://www.unhcr.org/en-us/information-on-unhcr-resettlement.html

[9]https://www.state.gov/refugee-admissions/u-s-refugee-admissions-program-access-categories/

[10]https://www.unhcr.org/3c5e5a764.html

Figure 1: Resettlement Sites by Volunteer Agency.

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

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.[11]

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

[11]https://www.acf.hhs.gov/orr/resource/the-us-refugee-resettlement-program-an-overview

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.[12]

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.

[12] https://www.state.gov/j/prm/ra/receptionplacement/

1.3 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.

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.

1.4 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.[13] 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 sur-

[13]https://archive.acf.hhs.gov/programs/orr/data/04arc8.htm8

veys 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.[14] The household survey asks about household participation in social welfare programs like the Supplemental Nutrition Assistance Program (SNAP) and the Aid to Families with Dependent Children/Temporary Assistance for Needy Families program (AFDC/TANF).[15]

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.[16] The parameters used in compiling ASR data do not distinguish whether Cubans and Haitians

[14]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.

[15]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.

[16]https://www.acf.hhs.gov/orr/resource/who-we-serve-cuban-haitian-entrants

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,[17] 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 individuals[18] 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.

[17]https://www.acf.hhs.gov/orr/resource/annual-orr-reports-to-congress-2005-iii-the-lost-boys-of-sudan

[18]The original individual indicator variable in the data (f1ID) 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.

TABLE I: 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**

Despite big differences in origin-region composition, the composition of refugees by 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 1.5.4 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–

TABLE II: YEARS SINCE MIGRATION BY PANEL ITERATION

	Panel iteration					
Years since Migration	**1**	**2**	**3**	**4**	**5**	**Total**
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**

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 1.5. 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 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 1.7.1, I assessed whether the underlying composition of these groups is changing and formally test how these underlying differences might bias my estimates.

1.5 Empirical Strategy

1.5.1 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.

1.5.2 Base specification

The base specification is

$$y_{it} = \alpha + \beta ue_i + \delta X_i + \varphi_i^m + \varphi_i^c + \varphi_t + \delta ue_{it}^s + \varphi_{it}^k + \varepsilon_{it}$$

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.[19]

Calendar-month-of-arrival fixed effects, φ_i^m, are used to control for seasonal variation in the monthly unemployment rate.[20] 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 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.[21] 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.

[19]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 on Tables 33 and 34 in Appendix provide evidence that these endogeneity concerns do not seem to drive results.

[20]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 1.8, 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.

[21]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.

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.

1.5.3 Preferred specification

To measure how this effect might vary over time, I borrowed from Godøy (2017)[22] and used an interaction between arrival unemployment rates and years since migration. My preferred specification is

[22] 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.

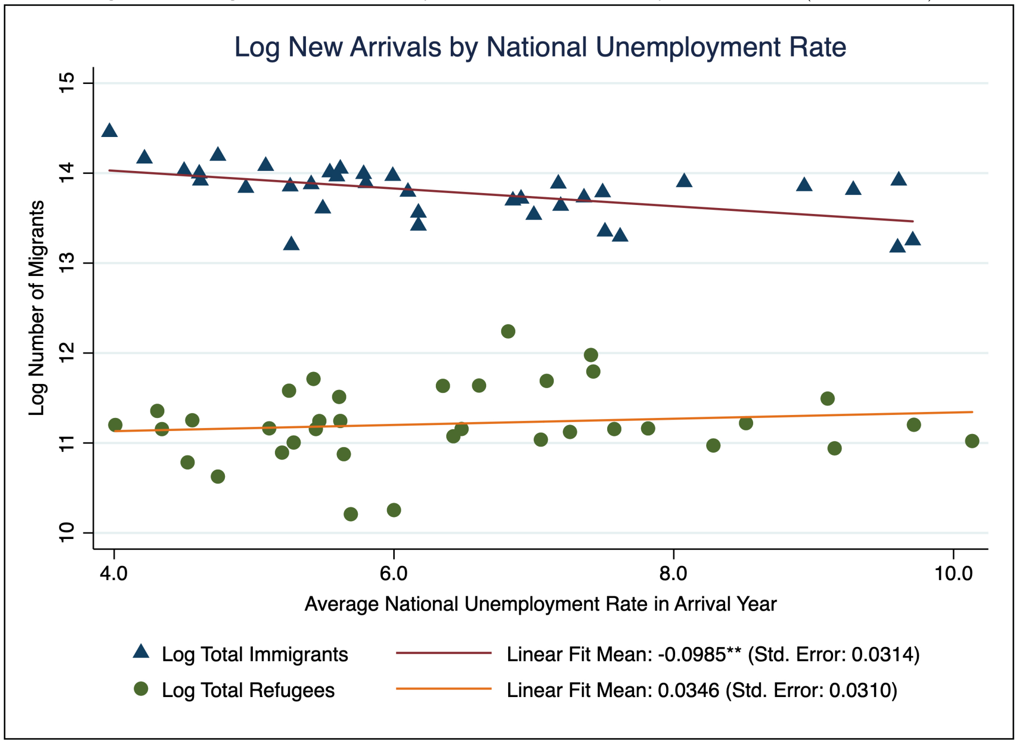
$$y_{it} = \alpha + \beta_k(ue_i \times \varphi^k_{it}) + \delta X_i + \varphi^m_i + \varphi^c_i + \varphi_t + \delta ue^s_{it} + \varphi^k_{it} + \varepsilon_{it}$$

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.

1.5.4 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 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

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.

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.

1.5.5 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,[23] partially determine how many refugees are allowed to enter the 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, $ue_i = \alpha + \delta X_i + \varphi_i^c + \varepsilon_i$. 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

[23] https://www.state.gov/j/prm/releases/docsforcongress/261956.htm

TABLE III: TEST OF BALANCE FOR CONTINUOUS TREATMENTS

	(1) Arrival National Unemp. rate	(2) Arrival State Unemp. rate
Age	-0.0012	-0.0008
	(0.0011)	(0.0014)
English fluency	0.0056	0.0224
	(0.0396)	(0.0455)
Years of education	0.0065+	0.0050
	(0.0035)	(0.0053)
Gender	0.0107	-0.0094
	(0.0115)	(0.0174)
Disability	0.0150	0.1023
	(0.0431)	(0.0686)
Married	0.0049	0.0955*
	(0.0309)	(0.0410)
Any Children	0.0240	0.0057
	(0.0347)	(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.

alternative treatment, the state unemployment rate at arrival, ue_i^s. The specification for this column is $ue_i^s = \alpha + \delta X_i + \varphi_i^c + \varphi_i^0 + \varepsilon_i$. This specification controls for both country of origin fixed effects, φ_i^c, and date-of-arrival fixed effects, φ_i^0. 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, I am confident that there is a minimal compositional change within nationality groups across arrival years.

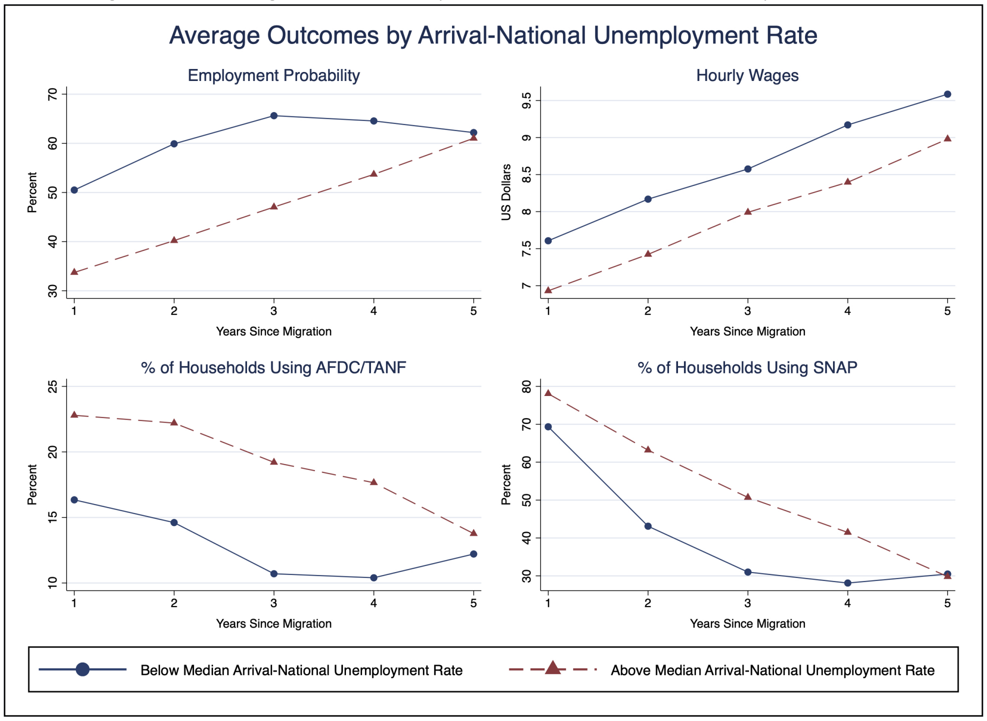
However, in column 2 of Table 3, I found that marriage is strongly correlated with the arrival placement-state unemployment rate. It means that states with worse economic conditions than the rest of the country receive more married individuals. This could potentially bias estimates using the placement-state 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.

1.6 Results

1.6.1 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 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.

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.

1.6.2 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

TABLE IV: MAIN RESULTS

	(1) Employment	(2) Employment	(3) Log Wages	(4) Log Wages
ue_i	-0.0157**		-0.0198**	
	(0.0055)		(0.0061)	
1 year, ue_i		0.0168		-0.0153+
		(0.0104)		(0.0087)
2 years, ue_i		-0.0113		-0.0240**
		(0.0073)		(0.0079)
3 years, ue_i		-0.0225*		-0.0088
		(0.0089)		(0.0075)
4 years, ue_i		-0.0360***		-0.0211*
		(0.0081)		(0.0091)
5 years, ue_i		-0.0208*		-0.0251**
		(0.0085)		(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 33 and 34 in Appendix, respectively.

conditions. The regression performed is outlined in Sections 1.5.2. 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 individuals[24] 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 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.

[24] 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.

1.6.3 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.

TABLE V: MAIN RESULTS BY GENDER

	(1) Male Employment	(2) Male Employment	(3) Male Log Wages	(4) Male Log Wages	(5) Female Employment	(6) Female Employment	(7) Female Log Wages	(8) Female Log Wages
ue_i	-0.0170*		-0.0201**		-0.0143*		-0.0198**	
	(0.0068)		(0.0075)		(0.0070)		(0.0071)	
1 year, ue_i		0.0044		-0.0161		0.0293*		-0.0153
		(0.0132)		(0.0114)		(0.0119)		(0.0100)
2 years, ue_i		-0.0153		-0.0270**		-0.0072		-0.0216*
		(0.0099)		(0.0104)		(0.0089)		(0.0101)
3 years, ue_i		-0.0258*		-0.0030		-0.0190+		-0.0171*
		(0.0104)		(0.0100)		(0.0113)		(0.0087)
4 years, ue_i		-0.0293**		-0.0198+		-0.0429***		-0.0218+
		(0.0097)		(0.0107)		(0.0117)		(0.0112)
5 years, ue_i		-0.0169		-0.0285*		-0.0240*		-0.0208+
		(0.0112)		(0.0111)		(0.0112)		(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.

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.

1.6.4 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.[25] 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, differential guide-

[25]https://aspe.hhs.gov/basic-report/overview-immigrants-eligibility-snap-tanf-medicaid-and-chip

TABLE VI: WELFARE UTILIZATION

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

lines across states, unobserved income, fear of stigmatization, or other unobserved factors. Refugees might have more trouble in getting approved to receive AFDC/TANF[26] and SNAP[27] 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.[28] 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)

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

[27] 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

[28] https://www.acf.hhs.gov/orr/state-programs-annual-overview

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 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.

1.6.5 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 1.6.3, 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

TABLE VII: HETEROGENEITY WITHIN EMPLOYMENT ESTIMATES

	(1) No HS Males	(2) HS Males	(3) College Males	(4) No HS Females	(5) HS Females	(6) College Females
ue_i	-0.0149	-0.0190+	-0.0101	0.0032	-0.0352**	-0.0423+
	(0.0094)	(0.0099)	(0.0231)	(0.0100)	(0.0107)	(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

	(1) No HS Males	(2) HS Males	(3) College Males	(4) No HS Females	(5) HS Females	(6) College Females
1 year, ue_i	0.0130	0.0143	-0.0632	0.0627***	0.0093	-0.0856+
	(0.0206)	(0.0169)	(0.0433)	(0.0161)	(0.0198)	(0.0478)
2 years, ue_i	-0.0078	-0.0131	-0.0373	0.0081	-0.0224	-0.0118
	(0.0148)	(0.0143)	(0.0330)	(0.0128)	(0.0148)	(0.0353)
3 years, ue_i	-0.0339*	-0.0246	0.0050	-0.0031	-0.0347*	-0.0582+
	(0.0151)	(0.0151)	(0.0303)	(0.0155)	(0.0158)	(0.0307)
4 years, ue_i	-0.0226	-0.0463***	0.0400	-0.0382*	-0.0596***	-0.0317
	(0.0143)	(0.0127)	(0.0324)	(0.0160)	(0.0158)	(0.0385)
5 years, ue_i	-0.0210	-0.0202	0.0101	-0.0054	-0.0549***	-0.0357
	(0.0175)	(0.0148)	(0.0367)	(0.0162)	(0.0149)	(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 VIII: HETEROGENEITY WITHIN LOG WAGE ESTIMATES

	(1) No HS Males	(2) HS Males	(3) College Males	(4) No HS Females	(5) HS Females	(6) College Females
ue_i	-0.0070	-0.0282**	-0.0668**	-0.0191*	-0.0082	-0.0799*
	(0.0094)	(0.0098)	(0.0251)	(0.0076)	(0.0097)	(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

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

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.”

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.

1.7 Additional Checks on Interval Validity

1.7.1 Testing for changes in composition

In Section 1.4, 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

TABLE IX: 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**

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.

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, in order to provide a means of comparison with my main results found in Table 4.

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

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

As explained in Section 1.5, ue_i is the monthly national-unemployment-rate each refugee faces at arrival. φ_i^m, φ_i^c, φ_t, 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 1.5.3, without covariates that include an interaction between the arrival national unemployment rate and years-since-migration.

Step 2a: $\hat{y}_{it} = \alpha + \beta_k(ue_i \times \varphi^k_{it}) + \varphi^m_i + \varphi^c_i + \varphi_t + \delta ue^s_{it} + \varphi^k_{it} + \varepsilon_{it}$

The regressions in Steps 2 and Step 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 are opposite to the coefficients laid down in Table 4. This means that composition changes are actually attenuating my results, not driving them.

TABLE X: TEST FOR CHANGES IN COMPOSITION

	(1) Employment	(2) Employment	(3) Log Wages	(4) 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.

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.

1.7.2 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 a regression on a sub-sample of known non-movers using the national-unemployment-rate-at-arrival treatment. The estimates in Table 11 are larger in magnitude than the main estimates in Table 4, suggesting that post-arrival movement is likely attenuating my national unemployment rate estimates. In Section 1.7.1, 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.

1.7.3 Testing for robustness

As a robustness check for my preferred estimates in Table 4, I have also included estimates from alternate specifications in Tables 33 and 34 in Appendix. Column 1 of Tables 33 and 34 in Appendix shows results without any of the following covariates: years of education at arrival, gender, age,

TABLE XI: MAIN RESULTS FOR NON-MOVERS

	(1) Employment	(2) Employment	(3) Log Wages	(4) Log Wages
ue_i	-0.0227**		-0.0263***	
	(0.0071)		(0.0078)	
1 year, ue_i		0.0213		-0.0275*
		(0.0145)		(0.0128)
2 years, ue_i		-0.0012		-0.0311**
		(0.0095)		(0.0106)
3 years, ue_i		-0.0380**		-0.0135
		(0.0130)		(0.0111)
4 years, ue_i		-0.0538***		-0.0330**
		(0.0120)		(0.0115)
5 years, ue_i		-0.0345**		-0.0229*
		(0.0114)		(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.

English fluency at arrival, marital status at arrival, disability status at arrival, and parental status at arrival. Columns 2–8 of Tables 33 and 34 in Appendix show how these estimates change as each covariate is added, with column 8 being the preferred specification.

1.8 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.[29] 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

$$y_{it} = \alpha + \beta ue_i^s + \delta X_i + \varphi_i^c + \varphi_t + \delta ue_{it}^s + \varphi_{it}^k + \varphi_i^0 + \varphi_i^s + \varepsilon_{it}$$

[29] https://www.state.gov/refugee-admissions/reception-and-placement/

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 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

$$y_{it} = \alpha + \beta_k(ue_i^s \times \varphi_{it}^k) + \delta X_i + \varphi_i^c + \varphi_t + \delta ue_{it}^s + \varphi_{it}^k + \varphi_i^0 + \varphi_i^s + \varepsilon_{it}$$

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,[30] so an unknown portion of my sample is not being placed 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.[31] A balance test outlined in Section 1.5.5 suggests that marital status at arrival might be systematically related to local state economic conditions. In Section 1.8.2, I attempt to reduce this potential bias by restricting my sample to refugees less likely to have family already living in the US.

1.8.1 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

[30] 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.

[31] https://www.state.gov/refugee-admissions/u-s-refugee-admissions-program-access-categories/

TABLE XII: STATE UNEMPLOYMENT ESTIMATES

	(1) Employment	(2) Employment	(3) Log Wages	(4) Log Wages
ue_{si}	0.0049		-0.0140*	
	(0.0067)		(0.0060)	
1 year, ue_{si}		0.0017		-0.0249***
		(0.0085)		(0.0071)
2 years, ue_{si}		-0.0080		-0.0238***
		(0.0075)		(0.0071)
3 years, ue_{si}		0.0046		-0.0153*
		(0.0076)		(0.0068)
4 years, ue_{si}		0.0071		-0.0117+
		(0.0076)		(0.0066)
5 years, ue_{si}		0.0137+		-0.0067
		(0.0072)		(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.

decrease in wage scarring or if they are the result of non-random placement in areas with better economic conditions.

1.8.2 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 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 Reports[32] 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

[32] https://www.acf.hhs.gov/orr/resource/annual-orr-reports-to-congress

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 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.

1.9 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.

TABLE XIII: STATE UNEMPLOYMENT ESTIMATES USING NATIONALITY-BY-STATE PIONEERS

	Nationality-by-state Pioneers	
	(1)	(2)
	Employment	Log Wages
ue_{si}	-0.0048	-0.0359+
	(0.0155)	(0.0183)
Observations	4,800	2,169
Adj. R^2	0.292	0.332

	(1)	(2)
	Employment	Log Wages
1 year, ue_{si}	0.0148	-0.0579*
	(0.0237)	(0.0242)
2 years, ue_{si}	-0.0114	-0.0408+
	(0.0181)	(0.0225)
3 years, ue_{si}	-0.0006	-0.0368+
	(0.0174)	(0.0204)
4 years, ue_{si}	-0.0026	-0.0325
	(0.0177)	(0.0200)
5 years, ue_{si}	-0.0143	-0.0323+
	(0.0170)	(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.

TABLE XIV: STATE UNEMPLOYMENT ESTIMATES USING PIONEERS

	Nationality Pioneers	
	(3)	(4)
	Employment	Log Wages
ue_{si}	-0.0314	-0.1250**
	(0.0259)	(0.0419)
Observations	1,739	681
Adj. R^2	0.298	0.347

	(3)	(4)
	Employment	Log Wages
1 year, ue_{si}	0.0028	-0.1322+
	(0.0440)	(0.0713)
2 years, ue_{si}	-0.0621*	-0.1058*
	(0.0312)	(0.0513)
3 years, ue_{si}	-0.0305	-0.1321**
	(0.0289)	(0.0461)
4 years, ue_{si}	-0.0241	-0.1387**
	(0.0313)	(0.0419)
5 years, ue_{si}	-0.0582+	-0.1245**
	(0.0306)	(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.

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.

2 SALARY HISTORY BANS AND HEALING SCARS FROM PAST RECESSIONS

2.1 Introduction

Empirical literature has shown that entering the labor force during a recession results in a negative wage disparity, called wage scarring, that can last decades (Kahn, 2010; Oreopoulous et al., 2012). Under adverse labor market conditions, inexperienced job-seekers have more difficulty finding employment. With fewer outside options, salary negotiations strongly favor employers. As a result, these individuals accept lower initial wage offers. However, it unclear why this wage disparity persists years after economic conditions have improved. Two prevailing theories posit differences in human capital accumulation or job search friction as potential mechanisms. Focusing on the latter, I use Salary History Ban laws (SHBs) to test whether job mobility for scarred workers is constrained due to employers screening job-seekers on prior wages. I find SHBs increase job mobility for scarred workers relative to non-scarred workers and reduce the gap in hourly wages and weekly earnings between these workers. This finding contributes to the wage scarring literature because it represents the first evidence, to my knowledge, that wage scarring is partially caused by job search friction related to salary disclosure.

Several US states began passing SHBs in 2017 in an effort to eliminate gender and racial wage disparities. These laws explicitly bar employers from asking job applicants about prior or current compensation during the hiring process. Given that some job applicants may still volunteer this information, there exists a possibility for adverse selection (Agan et al., 2020). Despite these adverse selection concerns, an emerging empirical literature has shown that SHBs reduce gender and racial wage gaps (Hansen and McNichols, 2020; Sinha, 2019; Bessen et al., 2020). One potential explanation is that employers have increasingly responded to SHBs by including target pay information in job postings (Ibid; Sran et al., 2020). Increased pay transparency increases information for job-seekers and arguably eliminates the need to discuss salary history in the first place.

To estimate the overall effect of SHBs, I use a difference-in-differences (DiD) empirical strategy that exploits state-by-year variation in SHB enactment on a sample of working-age adults from the Current Population Survey (CPS). I then extend this analysis to account for wage scarring by splitting the sample between "scarred" and "non-scarred" workers using a method proposed by Schwandt and Von Wachter (2019). In DiD event studies with each sample, I find SHBs raise hourly wages and weekly earnings for scarred workers but have no statistically significant effect on compensation for non-scarred workers. I also find SHBs increase job mobility for scarred workers but reduce job mobility for non-scarred workers. This reduction in job mobility may be the result of higher-paid individuals' inability to signal higher wages after SHBs are enacted (Meli and Spindler, 2019). These event studies also confirm parallel pre-trends in compensation and job mobility between scarred and non-scarred workers.

I then directly estimate how the effect of SHBs varies for workers affected by early-career job-market conditions using a difference-in-differences-in-differences (DDD) empirical strategy. This model fully interacts the state-by-entry-year unemployment rate[33] with the original DiD model. For workers who started their careers during a moderate recession, or a 5 percentage point higher state unemployment rate, I find SHBs increase job mobility by 0.6%, hourly wages by 3.4%, and weekly earnings by 5.45% relative to workers who graduated in baseline labor market conditions. Additionally, I show these estimates represent a 90% reduction in the original scarring effect for workers with one to five years of experience.

Given that SHBs originated with gender and racial wage disparities in mind,[34] I also test the effect of SHBs on wage scarring between men and women and between whites and non-whites. This represents another contribution to the wage scarring literature as the scarring effect has been found to differ across demographics (Schwandt and Von Wachter, 2019). After split-

[33] This variable, first proposed by Schwandt and Von Wachter (2019), merges state-of-residence, current year, and potential experience in the Current Population Survey with historic state unemployment data to approximate the state-by-entry-year unemployment rate for each person in the sample. This is discussed in detail in Section 2.4.2.

[34] https://www.nytimes.com/2019/10/22/us/dont-ask-me-about-my-salary-history.html

ting the sample between male and females, I use the aforementioned DDD estimation strategy and find that SHBs increase job mobility, hourly wages, and weekly earnings for scarred men and women relative to non-scarred men and women, with a smaller effect observed for men. In a separate analysis, I split the sample between whites and non-whites and find that SHBs have a small effect on wage scarring for whites but substantially raise hourly wages and weekly earnings for scarred non-whites. Additionally, I find that scarred whites and non-whites increase job mobility relative to non-scarred whites and non-whites. Finally, I observe increased unemployment-to-employment transitions for scarred whites relative to non-whites.

This study is also the first to my knowledge to explore a potential policy intervention for wage scarring. Traditionally, wage scarring has been thought of as a catch-up scenario. In a perfectly competitive labor market with perfect information, scarred workers transition to higher-paid positions as the economy improves. Policies that target increased job switching therefore might prove useful (Oreopoulos et al., 2012). However, I provide evidence that an additional channel inhibits this process. Although banning salary history disclosure increases job mobility for scarred workers relative to non-scarred workers, much larger relative effects are found with wages and earnings. If compensation growth is partially path dependent through disclosure, then it may not matter as much how often scarred workers switch jobs. Each subsequent job switch would yield a lower increase in wages than their non-scarred counterparts. This suggests that compensation parity between these cohorts may require even more job switching by scarred workers.

Finally, despite my finding that SHBs improve employment and wage outcomes for scarred workers in general, policymakers should also consider the unintended consequences. While SHBs increase job mobility for scarred workers, I also find they decrease job mobility for non-scarred workers. This could be the result of higher paid individuals (non-scarred workers) being unable to signal market-perceived quality to employers through salary history disclosure (Meli and Spindler, 2019). If the benefits of SHBs are partially explained through increased pay transparency, then pay transparency laws may achieve the same desirable effects without harming job mobility for these individuals.

2.2 Conceptual Framework

2.2.1 Scarring

Negative consequences associated with entering the labor market during a recession have been well documented. Kahn (2010) shows that US college graduates entering the job market during a recession experience a wage decline that lasts 20 years. A number of other studies have shown similar findings in other countries (Oreopoulus et al., 2012; Liu and Chen, 2014;

Kondo, 2007; Choi et al., 2020; Brunner and Kuhn, 2014). The literature also shows substantial heterogeneity within this effect across race and educational attainment. Notably, non-whites and high school graduates exhibit much larger scarring effects (Schwandt and Von Wachter, 2019; Hershbein, 2012; Altonji et al., 2016). Inexperience in a job search is also more costly in a recession. Forsythe (2016) shows firms are less likely to hire inexperienced workers during recessions.

However, it is less clear why this disparity persists once labor market conditions improve. The literature focuses on two theoretical themes to explain the long-run nature of wage scarring: human capital accumulation and job search friction. The first and most studied theme, human capital accumulation, posits that individuals entering the labor market during a recession match poorly with their first jobs. With less available jobs and more competition, they are forced to take positions that do not directly involve tasks related to their training. As a result, they accumulate less industry-specific human capital than they would otherwise, resulting in long-term productivity disparities (Kahn, 2010). Liu et al. (2016) find the quality of one's first employer can be a major contributor to the wage scarring gap. Arellano-Bover (2020) shows initial career firm size affects job skill growth and that inexperienced workers match more with smaller firms during economic downturns. In Hagedorn and Manovskii (2013), they "...develop a method to measure match qualities and show empirically that various variables summarizing past aggregate labor market conditions have explanatory power for current wages only because they are correlated with match qualities" (771).

The second theoretical theme, job search friction, is based on contract theory work by Beaudry and Dinardo (1991) and posits that workers experience long-run scarring effects if subsequent job mobility is constrained. Workers that enter employment during a recession take employment contracts that pay lower wages than employment contracts offered during economic boom periods. As the economy improves, this disparity disappears as scarred workers leave lower-paying employment contracts for better-paying ones. However, if these workers cannot change jobs due to search costs, lack of information, or other labor market friction, they will remain in these contracts. Oreopoulous et al. (2012) theorizes this search friction is related to age. With incomplete information, searching for a new job takes time. These costs increase with age and scarred workers may stop changing jobs long before non-scarred workers. Forsythe (2019) shows that within-firm mobility also declines with age. Kwon et al. (2010) finds that individuals who graduate during a recession are also promoted less frequently.

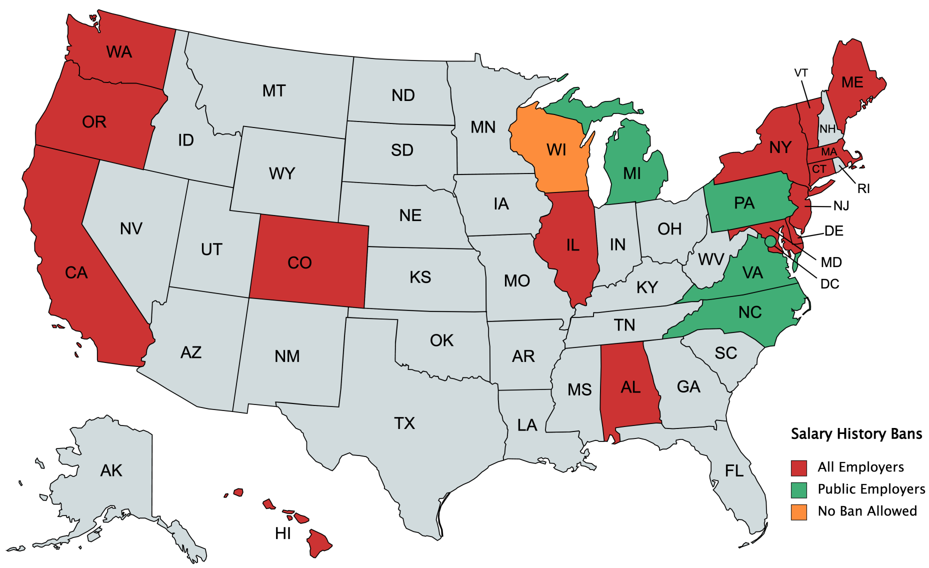
2.2.2 Salary History Bans

This paper focuses on the second theoretical theme, job search friction, by testing a mechanism not previously considered: employer screening through salary history disclosure. There are many reasons why an employer would want to know a job-seeker's salary history. According to Barach and Horton (2021), "in a competitive labor market, a very recent wage in a similar job is approximately the worker's marginal productivity–precisely what a would-be employer is interested in learning (Kotlikoff and Gokhale 1992; Altonji and Pierret 2001; Lange 2007; Oyer and Schaefer 2011; Kahn and Lange 2014)" (194). Employers also gain an advantage in salary negotiations from this information as they learn more about the job-seeker's reservation wage while the job-seeker remains unaware of the employer's offer expectations.

Survey evidence confirms that many employers do ask job-seekers about their prior salary prior to making an offer. Hall and Krueger (2012) find 47% of respondents in a national representative survey had been asked about past wages at some point in their career. Payscale (2017) also finds that 43% of respondents had been asked about salary history in the past year. If workers accept lower wages during recessions and then disclose this lower wage to potential employers in a subsequent job search, the potential employer might infer they are less productive and not hire them. Therefore, salary history disclosure could plausibly perpetuate scarring effects.

To test how salary history disclosure affects compensation and job mobility for scarred individuals, I exploit variation in the passage of SHB laws. As of June 1, 2021, 15 states have passed SHBs for all employers and 4 states have passed SHBs for public employers. Figure 4 shows the states that have

Figure 4: States that Have Passed Salary History Bans (SHBs)



Data from: `https://www.hrdive.com/news/salary-history-ban-states-list/516662/`

passed SHB laws. States highlighted in green ban state public sector employers and contractors from discussing salary history prior to a job offer, while states highlighted in red ban all employers. Curiously, Wisconsin, highlighted in orange, took the opposite approach and passed a law that prevents local municipalities from passing local SHB laws.[35]

[35]There have also been efforts to ban salary history disclosure on the national level. The Paycheck Fairness Act, which included a provision to ban salary history questions nationwide, was introduced in Congress in January 2019 but failed to pass in both chambers. https://www.shrm.org/resourcesandtools/legal-and-compliance/employment-law/pages/congress-considers-nationwide-ban-on-salary-history-inquiries.aspx

Advocates of SHBs believe salary history disclosure requirements during hiring are discriminatory and perpetuate existing gender and racial wage gaps. They argue that banning the practice would force employers to offer market wages instead of wages tied to prior discrimination.[36] In an experiment using an online job market, Barach and Horton (2021) show that removing salary history information during hiring results in employers evaluating 7% more applicants and hiring workers with 13% lower average past wages. However, given that these findings are derived in a controlled setting, it is still unclear how this law might work in practice.

An emerging theoretical literature offers several predictions on expected outcomes of SHB enactment in a broader setting. One prediction is that if job-seekers typically lie about their salary history when asked to disclose, then this information is not valuable and there would be no effect from banning the practice of salary disclosure (Khanna, 2020). Another prediction is that if employers adhere to the law but some job-seekers with higher prior wages continue to volunteer this information, then banning the practice can result in a temporary effect. In this setting, employers infer that any job-seeker who does not disclose salary history is of lower quality. Job-seekers with marginal salary histories may initially refuse to disclose salary history, but they would be increasingly incentivized to do so to avoid discrimination. As more and more job-seekers disclose, the initial effect of the law would

[36]https://www.washingtonpost.com/news/on-leadership/wp/2015/04/14/the-worst-question-you-could-ask-women-in-a-job-interview/

unravel (Agan et al., 2020). Finally, if employers and job-seekers both stop discussing salary history after SHBs become law, then the law could work similarly to the controlled experiment in Barach and Horton (2021). However, this could also result in unintended consequences as job-seekers with higher salary histories can no longer signal this information to employers (Meli and Spindler, 2019).

Early empirical evidence shows SHBs reduce gender and racial wage gaps (Hansen and McNichols, 2020; Sinha, 2019; Bessen et al., 2020). These results align closest with the prediction that both employers and job-seekers no longer discuss salary history. However, it is unclear how policymakers could prevent job-seekers from volunteering this information. Changes in employer behavior may offer a more plausible explanation. Sran et al. (2020) and Bessen et al. (2020) provide evidence that after SHB enactment, bargaining and screening become too costly and employers post more jobs with target salary information. This provides more information to job-seekers and makes salary history discussions less relevant as employers no longer enjoy an advantage in salary negotiations.

If wage scarring is solely caused by human capital accumulation, then I do not expect a differential effect between scarred and non-scarred workers with either mechanism of strict adherence or changes in job posting behavior. However, both mechanisms would yield differential effects between scarred and non-scarred workers if wage scarring is partially caused by job search friction. With strict adherence to the law, employers lose information on scarred workers' salary history and expand their applicant pool. With changes in job posting behavior, scarred workers gain information and become more likely to apply to new jobs.

2.3 Data

My primary data source is the January 2013 to May 2021 CPS (Flood et al., 2020). This survey samples roughly 60,000 households each month. Each household is surveyed for four consecutive months, followed by an eight-month period of no surveying. After eight months, surveying resumes and each household is surveyed for an additional four months. The monthly data measure employment outcomes and hours worked. The survey also provides basic demographic information like state, gender, race, and educational attainment. Potential experience is calculated as age minus years of education minus 6. Graduation year (or job-market-entry year) is approximated by subtracting potential experience from the current year. Information on job-to-job transitions is provided for survey periods 2–4 and 6–8. Additionally, unemployment-to-employment transitions can be inferred by observing month-to-month changes in the employment variable.

The Outgoing Rotational Group (CPS-ORG) survey is a supplemental survey conducted in the fourth and eighth survey periods. Respondents are asked whether they are paid hourly or salary, and those paid hourly provide information on hourly wage and weekly earnings. Respondents who are paid salary provide information on weekly earnings. Following the methodology used by the Center For Economic and Policy Research (CEPR)[37] and Schmitt (2003), I create a consistent hourly wage series by dividing weekly earnings for all workers by their average number of hours worked. If hourly wage workers report a higher hourly wage than the wage computed from dividing weekly earnings by average number of hours worked, then the original hourly wage is used. Weekly earnings and hourly wages are also normalized to 2018 dollars using the Consumer Price Index for All Urban Consumers[38] and subsequently converted to logs.

Similar to prior scarring literature (Oreopoulos et al., 2012; Schwandt and von Wachter, 2019), I use a cell-based model by aggregating outcomes at the level of current state of residence, job-market-entry year, gender, race, and educational attainment. Cell-level data allow me to work closer to level of variation of my treatment, the staggered state-by-state implementation of SHB laws. Additionally, cells are reweighted based on weights provided by the CPS and CPS-ORG data to reflect population-level estimates. Following Schwandt and von Wachter (2019), I also merge the historical state unemployment rate for each state-by-entry-year group in order to approximate economic conditions at job market entry.

[37] http://ceprdata.org/cps-uniform-data-extracts/cps-outgoing-rotation-group/cps-org-faq/

[38] https://fred.stlouisfed.org/series/CPIAUCSL

To measure the effects of SHBs on the prime-age working population, I restrict the sample to individuals between the ages of 18 and 45 with at least 1 to 20 years of potential experience. I also drop public sector state workers since these individuals could be affected by public sector SHBs not included in my treatment variable.[39] Table 15 provides a summary table of the sample used in my empirical analysis. The average respondent is 30 years old with nearly 10 years of experience. Twenty-eight percent of the sample possess a high school diploma, while another 36% possess a college degree, and 78% of the sample are white.

TABLE XV: CPS SAMPLE SUMMARY TABLE

	Mean
Age	30.06
% Female	51.20
Potential Experience	9.94
% High School Graduates	27.98
% College Graduates	35.52
% Caucasian	77.93
Observations	3,021,637

[39]Self-employed workers are also dropped. In Tables 35 and 36 in Appendix, I show that results are robust to including both self-employed workers and public sector state workers.

In Table 16, I divide the sample by states that implemented an SHB and states that did not. States that pass SHB laws are more educated and more racially diverse. I use a DiD and DDD empirical strategy, so fixed demographic differences across states are less of a concern than how these states trended prior to policy implementation. In Section 2.5, I provide evidence that trends between SHB and non-SHB states are parallel prior to policy implementation. I also provide evidence of the validity of my DDD design by confirming that separate samples of scarred and non-scarred workers also have parallel trends prior to SHB enactment.

TABLE XVI: SUMMARY TABLE BY TREATMENT

	No Ban States	Ban States
	Mean	Mean
Age	29.99	30.20
% Female	51.24	51.13
Potential Experience	9.96	9.91
% High School Graduates	28.76	26.27
% College Graduates	33.75	39.42
% Caucasian	79.81	73.81
Observations	2,075,989	945,648

2.4 Empirical Strategy

2.4.1 SHB Effect

My empirical strategy relies on the staggered implementation of SHB laws across states to measure the differential effect of SHBs across scarred and non-scarred workers. To first assess how SHB laws affect employment and wage outcomes in general, I employ a two-way fixed effects or DiD model. This specification is similar to other papers in the SHB literature (Hansen and McNichols, 2020; Sinha, 2019; Bessen et al., 2020) and is meant to provide the reader with a baseline estimate of general effect of SHB enactment using the specific sample data described in Section 2.3.

Specification 1: $\bar{y}_{g,s,t,e,k} = \alpha + \beta D_{s,t} + \Phi_s + \Phi_t + \Phi_e + \Phi_k + \Phi'_g + \varepsilon_{g,s,t,e,k}$.

Employment or compensation outcomes, $\bar{y}_{g,s,t,e,k}$, are regressed on an indicator variable, $D_{s,t}$, that takes a value of one if a respondent lives in an SHB state after the ban goes into law and zero otherwise. Table 17 provides an overview of the treated states and the number of observations comprising each pre- and post-period. Treatment starts in the year that each state passes an SHB law,[40] with New York State as the only exception. While a

[40] https://www.hrdive.com/news/salary-history-ban-states-list/516662/

TABLE XVII: TOTAL OBSERVATIONS BY PERIOD FOR TREATED STATES ONLY

	Period									
State	<=t-4	t-3	t-2	t-1	**t**	**t+1**	**t+2**	**t+3**	**t+4**	Total
Alabama	15,640	7,093	7,166	7,033	6,798	5,772	2,458	0	0	51,960
California	72,884	35,314	35,086	34,113	32,933	32,172	27,322	11,319	0	281,143
Colorado	33,386	4,932	4,469	4,062	1,639	0	0	0	0	48,488
Connecticut	18,214	3,459	3,496	3,233	3,219	2,813	1,131	0	0	35,565
Delaware	5,268	5,024	4,259	3,961	3,644	3,583	3,127	2,672	1,159	32,697
Hawaii	17,373	5,430	5,311	4,839	4,507	3,866	1,449	0	0	42,775
Illinois	36,042	11,506	10,896	10,216	9,654	8,720	3,240	0	0	90,274
Maine	13,035	2,648	2,725	2,560	2,315	1,972	838	0	0	26,093
Maryland	28,139	4,953	4,841	4,347	3,827	1,621	0	0	0	47,728
Massachusetts	11,387	7,053	8,335	8,294	8,016	7,811	7,320	2,805	0	61,021
New Jersey	30,557	7,160	6,709	6,465	5,675	2,217	0	0	0	58,783
New York	16,640	16,334	15,764	15,743	15,436	14,819	14,711	12,246	4,476	126,169
Oregon	5,425	5,504	5,832	5,764	5,556	5,454	5,524	5,115	2,438	46,612
Vermont	8,907	4,314	4,216	4,041	4,012	3,918	3,331	1,397	0	34,136
Washington	21,068	6,828	7,325	7,094	6,974	6,458	2,673	0	0	58,420
Total	333,965	127,552	126,430	121,765	114,205	101,196	73,124	35,554	8,073	1,041,864

statewide private-sector SHB did not become law in New York State until 2020, New York City and Albany County independently passed a ban in 2017.[41] Given that at least 40% of New York state's population was treated in 2017, I code the state as passing the ban in 2017.[42]

Year and state fixed effects, Φ_t and Φ_s, respectively, are included so that β represents the difference in outcomes in SHB states before and after the ban, after accounting for national trends. Potential experience fixed effects, Φ_e, and predicted year of job-market-entry fixed effects, Φ_k, are included to ensure workers with similar tenure in the labor force are being compared to one another. I also include a vector of demographic-group-level fixed effects, Φ'_g, for gender, race,[43] and educational attainment to ensure that individuals with similar demographic characteristics and labor market experience are being compared to one another.

To assess the validity of my DiD empirical strategy, I use an event study to visually inspect whether there are pre-existing trends between treated and non-treated states that could be driving my results. This estimation is an indirect test of the common trends assumption.

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[41] https://legistar.council.nyc.gov/LegislationDetail.aspx

[42] In Tables 37 and 38 in Appendix, I show main results are robust to the exclusion of New York.

[43] Race is a dummy variable equal to one if an individual identifies as white and is zero otherwise.

Specification 1a:

$$\bar{y}_{g,s,t,e,k} = \alpha + \sum_{j=-4}^{-2} \beta_j D_{s,t+j} + \sum_{j=0}^{4} \beta_j D_{s,t+j} + \Phi_s + \Phi_t + \Phi_k + \Phi_e + \Phi'_g + \varepsilon_{g,s,t,e,k}.$$

In this estimation, seven DiD estimates (β_j) are estimated for each year relative to the year prior to SHB enactment, t–1. If years t–4, t–3, and t–2 show no statistically significant difference from year t–1, then this provides suggestive evidence that differential pre-existing trends do not exist between these states prior to SHB enactment.

2.4.2 SHB Effect by Scarring

The second part of my empirical strategy approximates job market entry economic conditions for each respondent. Since the CPS does not contain information on the exact year a person enters the labor market, I use a method proposed by Schwandt and von Wachter (2019). The state-of-residence is combined with the predicted year of job market entry, based on potential experience and the current year, to create a variable called state-by-entry-year. Scarring is then measured using the historical unemployment rate[44] observed for each state-by-entry-year combination. Given this measure doesn't account for individuals who take longer to graduate or move states

[44] http://download.bls.gov/pub/time.series/la/la.data.3.AllStatesS.txt

after they graduate, there are obviously selection concerns with this method. However, the authors test these concerns using an alternate double-weighted estimator that incorporates trends in graduate rates and geographic mobility and confirm these selection concerns have a negligible impact on estimates when using the CPS, the dataset used in this analysis.

Figure 5 shows a graph of the range of business cycle activity individuals in the sample faced when they first entered the labor market. Given that the sample is restricted to people with 1–20 years of experience, 1993 would be the earliest year an individual in the data would have entered the labor force. As the figure shows, this range of potential labor market entry years allows me to estimate scarring from peak to trough in the unemployment rate for three recessions: the early 90s recession, the dot-com bubble, and the Great Recession.[45]

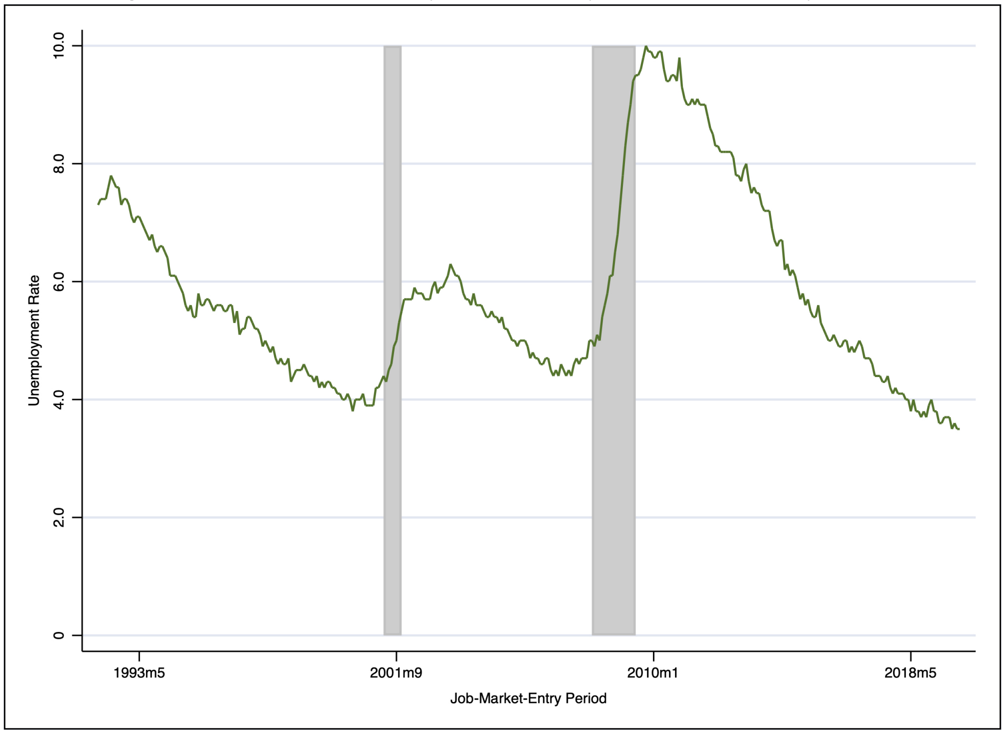
The specification for the second part of my empirical strategy is as follows:

Specification 2:

$$\bar{y}_{g,s,t,e,k} = \alpha + \eta_{s,t,k}(D_{s,t} * ue_{s,k}) + \delta D_{s,t} + \Phi_s + \Phi_t + \Phi_e + \Phi_k + \Phi'_g + \Phi_{s,k} + \Phi_{t,k} + \varepsilon_{g,s,t,e,k}.$$

[45] I limit my analysis to individuals who began their careers between 1993 and 2019. Although my results are robust to the inclusion of the 2020 cohort, there is an ongoing debate in the literature over how much unemployment rates reported by the Bureau of Labor Statistics (BLS) for early-to-mid-2020 reflect true unemployment versus furloughed workers who received Pandemic Unemployment Assistance (PUA).

Figure 5: National Unemployment Rate by Job-Market-Entry Period



Data retrieved from https://www.bls.gov/ces/. The range of the x-axis, 1992 to 2019, represents all potential job-market-entry periods covered in the analysis.

In this specification, the state unemployment rate in each respondent's job market entry year, $ue_{s,k}$, is interacted with the SHB indicator variable, $D_{s,t}$. I also include state fixed effects (Φ_s), year fixed effects (Φ_t), potential experience fixed effects (Φ_e), job-market-entry-year fixed effects (Φ_k), a vector of group fixed effects (Φ'_g),[46] state-by-entry-year fixed effects ($\Phi_{s,k}$), and year-by-entry-year fixed effects ($\Phi_{t,k}$).[47] Therefore, the interaction coefficient, $\eta_{s,t,k}$, is a DDD estimate of the SHB laws effect on scarred workers relative to non-scarred workers. δ is also reported as a measure of the baseline SHB effect (similar coefficient to Specification 1). For every percentage point increase in the state unemployment rate in a respondent's job market entry year, $\eta_{s,t,k}$ represents the increase in log wages or log weekly earnings from the passage of SHB laws relative to individuals who started their careers in baseline labor market conditions.

2.4.3 General Scarring Effect

To understand how my DDD estimates in Specification 2 compare to general estimates of wage scarring, I use the following specification:[48]

Specification 3: $\bar{y}_{g,s,t,e,k} = \alpha + \lambda ue_{s,k} + \Phi_s + \Phi_t + \Phi_e + \Phi_k + \Phi'_g + \varepsilon_{g,s,t,e,k}.$

This specification estimates the average scarring effect over 20 experience years for workers in the sample. Similar to Specification 1, I control

[46] Specification 2 uses the same vector of group fixed effects as Specification 1.

[47] Results are robust to using individual interaction terms between the entry-year state unemployment rate and each control. I use group interaction terms as this method absorbs additional variation unrelated to changes in SHB laws and scarring.

[48] This specification is similar to the method proposed in Schwandt and Von Wachter (2019). I additionally add controls for gender and race to be consistent with other specifications in this paper.

for state fixed effects, year fixed effects, job-market-entry-year fixed effects, potential experience, and group-level fixed effects.[49] λ measures the average scarring effect for every percentage point change in $ue_{s,k}$ for workers with 1–20 years of experience.

I also stratify the scarring effect by experience to assess how the effect is distributed across years of experience. Scarring has an initial effect that slowly decays as the person gains experience, so it is important to understand how experience affects average measures of wage scarring.[50] I use the following specification:

Specification 3a:

$$\bar{y}_{g,s,t,e,k} = \alpha + \sum_{j=1}^{20} \lambda_j (\Phi_e * ue_{s,k}) + \Phi_s + \Phi_t + \Phi_e + \Phi_k + \Phi'_g + \varepsilon_{g,s,t,e,k}.$$

Specification 3a is similar to Specification 3 except that the coefficient for the treatment effect, λ_j, is now stratified across 1 to 20 years of experience. For every percentage point increase in $ue_{s,k}$, λ_1 represents wage

[49] Specification 3 also uses the same vector of group fixed effects as Specification 1.

[50] Ideally, I would also want to stratify my DDD estimates by experience to understand how this reversal is distributed across years of experience, but I unfortunately lack statistical power to stratify them in this manner. However, I do have enough power if I divide experience into groups of five years or more. In Table 25, I divide the sample into five-year experience groups and find most of the DDD estimate comes from earlier experience workers. This finding is consistent with estimates in Table 24 that show most of the average scarring effect is also concentrated in this group.

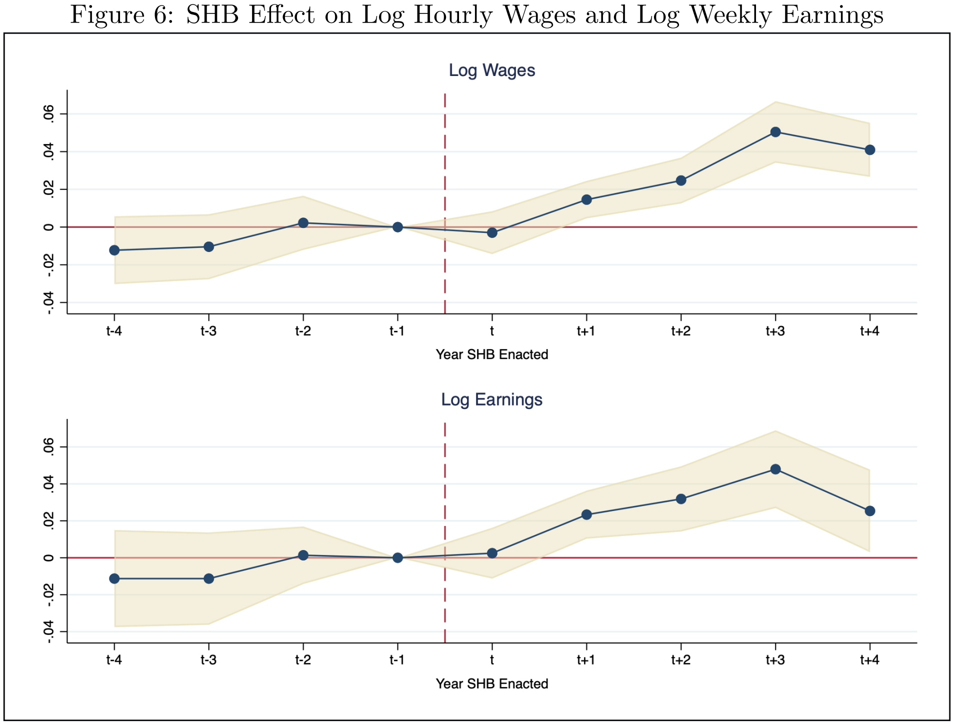
losses for workers with 1 year of experience, λ_2 represents wage losses for workers with 2 years of experience, and so on.

2.5 Results

2.5.1 Assessing Pre-Trends

Figure 6 tests for parallel pre-trends in the general SHB effect and shows they are parallel for log hourly wages and log weekly earnings. Both graphs in Figure 6 confirm pre-trends are parallel in both log hourly wage and log

Figure 6: SHB Effect on Log Hourly Wages and Log Weekly Earnings

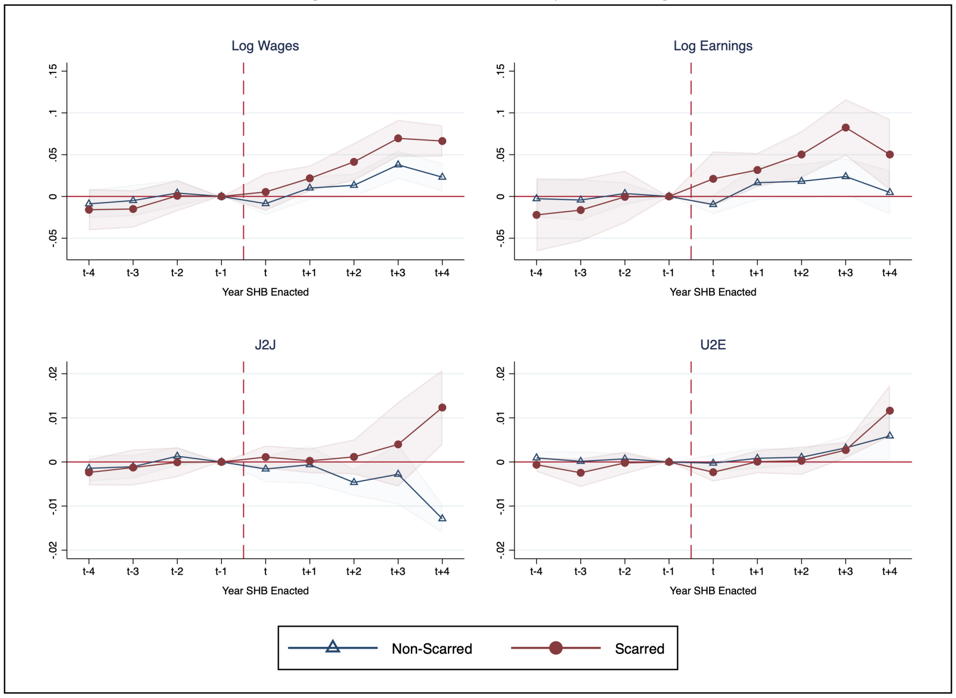


weekly earnings prior to the policy change. After SHBs are enacted, between periods t and t+4, I observe an increase in both log hourly wages and log weekly earnings for the treated states.

Figure 7 shows that there are also no pre-trends in the outcome variables for my DDD strategy.[51] I find no evidence of pre-trends for log hourly wages, log weekly earnings, or job-to-job transitions for either sample. By overlaying the event studies, I also show there are no pre-existing trends between the samples. Scarred individuals enjoy higher log hourly wage and log weekly earnings gains from the law change than non-scarred individuals. I also observe an increase in job-to-job transitions for scarred individuals and a decrease in job-to-job transitions for non-scarred individuals. This result is possible if non-scarred individuals, who are higher paid on average, are unable to signal their higher wage to employers after SHBs are enacted (Meli and Spindler, 2019).

[51]The sample is first divided into two groups, scarred workers and non-scarred workers. Individuals with state-by-entry-year unemployment rates above the long-run median for that particular state are categorized as scarred, and those with a below-median state-by-entry-year unemployment rate are categorized as non-scarred. I then plot event studies for both non-scarred and scarred groups and overlay them for comparison.

Figure 7: SHB Effect by Scarring



Using potential experience and state combined with historical state unemployment data, I impute the state unemployment rate at job-market-entry for each CPS respondent. Workers who have a state-by-entry-year unemployment rate that is above the long-run median for each state are classified as "scarred." Workers that are below the long-run median for each state are classified as "non-scarred." J2J represents job-to-job transitions and U2E represents unemployment-to-employment transitions.

2.5.2 SHB Effect and Wage Scarring

General SHB Effect (DiD Estimate) In Table 18, column 1, I estimate the general effect of SHB enactment using a DiD strategy. As noted in Section 2.4, these results use similar methods to other papers in the SHB

TABLE XVIII: OVERALL SHB EFFECT ON COMPENSATION (DID ESTIMATE)

	(1) Log Hourly Wages	(2) Log Weekly Earnings
SHB	0.0176***	0.0236**
	(0.0047)	(0.0082)
Observations	101,284	102,990
Adjusted R^2	0.724	0.718

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

literature (Hansen and McNichols, 2020; Sinha, 2019; Bessen et al., 2020). I provide these results to give the reader a baseline estimate of general effect of SHB enactment. Similar to aforementioned SHB literature, I find the law change induces a positive and statistically significant increase of 1.76% on log hourly wages. SHBs also increase weekly earnings by 2.36% (column 2), meaning workers in SHB states are enjoying higher hourly wages and weekly earnings in general after the bans are enacted. This is plausible as salary negotiations now favor job-seekers.

In Table 19, I show SHBs have no general effect on job-to-job transitions and unemployment-to-employment transitions. While SHBs may have spillover effects that affect workers who do not switch jobs after the law changes, I would expect the effects to be concentrated among those who switch jobs or gain employment after the law changes. Table 20, columns

TABLE XIX: OVERALL SHB EFFECT ON JOB TRANSITIONS (DID ESTIMATE)

	(1) J2J Changes	(2) U2E Changes
SHB	-0.0002	0.0002
	(0.0007)	(0.0006)
Observations	108,007	121,046
Adjusted R^2	0.023	0.049

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

1 and 2 restricts the sample to observations from individuals who switched jobs during the survey period and shows that most of the effect from SHBs comes from these individuals. In Table 20, columns 3 and 4, I restrict the sample to observations from individuals who went from unemployment-to-employment, and I do not observe any statistically significant effect for this group.

TABLE XX: OVERALL SHB EFFECT ON J2J AND U2E COMPENSATION

		J2J		**U2E**
	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) Log Hourly Wages	(4) Log Weekly Earnings
SHB	0.0521***	0.0525**	0.0102	0.0059
	(0.0130)	(0.0177)	(0.0177)	(0.0356)
Observations	22,046	23,217	14,365	15,739
Adjusted R^2	0.351	0.342	0.264	0.228

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

SHB Effect by Scarring (DDD Estimate) Table 21 shows the results from the DDD strategy. These estimates represent the differential effect of SHB enactment on individuals who started their careers in a recession and represent the main results of this study. Columns 1 and 2, row 2 show that for every percentage point increase in state-by-entry-year unemployment rate, log hourly wages increase by 0.68% and log weekly earnings increase by 1.09% relative to the baseline SHB effect in row 1.

TABLE XXI: SHB EFFECT ON COMPENSATION BY SCARRING (DDD ESTIMATE)

	(1) Log Hourly Wages	(2) Log Weekly Earnings
SHB	0.0126*	0.0158+
	(0.0055)	(0.0086)
SHB × ue_{sk}	0.0068***	0.0109***
	(0.0019)	(0.0023)
Observations	101,284	102,990
Adjusted R^2	0.726	0.719

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

In Table 22, column 1, I show that SHBs differentially increase job-to-job transitions by 0.12% for every percentage point increase in the state-by-entry

TABLE XXII: SHB EFFECT ON JOB TRANSITIONS BY SCARRING (DDD ESTIMATE)

	(1) J2J Changes	(2) U2E Changes
SHB	-0.0011	-0.0006
	(0.0009)	(0.0007)
SHB × ue_{sk}	0.0012***	0.0004*
	(0.0003)	(0.0002)
Observations	108,007	121,046
Adjusted R^2	0.022	0.047

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

-year unemployment rate over workers who started their careers in baseline economic conditions. In Table 22, column 2, I show that SHBs differentially increase unemployment-to-employment transitions by 0.04% for every percentage point increase in the state-by-entry-year unemployment rate over baseline estimates. For workers who entered the labor market in a moderate-to-severe recession (5 percentage point increase in the state unemployment rate), this represents differential increases from SHB enactment of 3.4% for hourly wages, 5.45% for weekly earnings, 0.6% for job-to-job transitions, and 0.2% for unemployment-to-employment transitions relative to workers who entered the labor market during baseline labor market conditions.

2.5.3 Comparison to General Scarring Effect

Tables 21 and 22 show a DDD estimate of the differential effect of SHBs on scarred workers. However, I also want to understand how this estimate compares to general scarring estimates to assess how much SHBs reverse the original scarring effect. In the literature, the initial wage loss from scarring ranges from 1%–2% for every percentage point increase in the job market entry unemployment rate and decays to zero after a period of 15 to 20 years (Kahn, 2010; Oreopoulus et al., 2012; Schwandt and Von Wachter, 2019; Mask, 2020). If SHBs differentially advantage scarred workers, then I expect my DDD estimates in Table 21, columns 1 and 2 will be smaller in magnitude and the opposite sign of general scarring estimates.

To provide an understanding of baseline scarring effects in the absence of SHBs, in Table 23, columns 1 and 3, I show that the average scarring effect for workers with 1–20 years of experience is –0.52% for log hourly wages and –0.88% and log weekly earnings. In comparison to the DDD estimates for log hourly wages and log weekly earnings found in Table 21, columns 1 and 2 respectively, SHB laws appear to completely reverse the average scarring effect and even increase wages for scarred workers. However, these two estimates are likely not directly comparable if the differential effect from SHB laws is concentrated in less experienced workers. The scarring effect is largest in earlier years and decays over time as the worker gains experience.

TABLE XXIII: SCARRING EFFECT

	(1) Log Hourly Wages	(2) Log Hourly Wages	(3) Log Weekly Earnings	(4) Log Weekly Earnings
ue_{sk}	-0.0052***		-0.0088***	
	(0.0012)		(0.0016)	
ue_{sk} × Experience = 1		-0.0114***		-0.0255***
		(0.0034)		(0.0066)
ue_{sk} × Experience = 2		-0.0102***		-0.0209***
		(0.0026)		(0.0041)
ue_{sk} × Experience = 3		-0.0070**		-0.0174***
		(0.0026)		(0.0037)
ue_{sk} × Experience = 4		-0.0081**		-0.0141***
		(0.0027)		(0.0033)
ue_{sk} × Experience = 5		-0.0051*		-0.0073**
		(0.0020)		(0.0026)
ue_{sk} × Experience = 6		-0.0024		-0.0057*
		(0.0018)		(0.0023)
ue_{sk} × Experience = 7		-0.0043*		-0.0057*
		(0.0017)		(0.0024)
ue_{sk} × Experience = 8		-0.0032+		-0.0037+
		(0.0016)		(0.0021)
ue_{sk} × Experience = 9		-0.0054**		-0.0073***
		(0.0017)		(0.0022)
ue_{sk} × Experience = 10		-0.0032		-0.0059+
		(0.0025)		(0.0032)
ue_{sk} × Experience = 11		-0.0092**		-0.0116***
		(0.0030)		(0.0035)
ue_{sk} × Experience = 12		-0.0037		-0.0066+
		(0.0029)		(0.0036)
ue_{sk} × Experience = 13		-0.0052		-0.0058
		(0.0037)		(0.0044)
ue_{sk} × Experience = 14		-0.0097**		-0.0143**
		(0.0035)		(0.0045)
ue_{sk} × Experience = 15		-0.0074+		-0.0118*
		(0.0042)		(0.0052)
ue_{sk} × Experience = 16		-0.0029		-0.0057
		(0.0041)		(0.0049)
ue_{sk} × Experience = 17		-0.0067		-0.0060
		(0.0042)		(0.0051)
ue_{sk} × Experience = 18		-0.0033		-0.0007
		(0.0033)		(0.0044)
ue_{sk} × Experience = 19		-0.0007		-0.0009
		(0.0037)		(0.0042)
ue_{sk} × Experience = 20		0.0003		0.0022
		(0.0038)		(0.0041)
Observations	101,284	101,284	102,990	102,990
Adjusted R^2	0.724	0.724	0.718	0.718

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state-by-job-market-entry-year level.

Table 23, columns 2 and 4 uses Specification 3a from Section 2.4.3 to show the scarring effect stratified by experience, which for the first experience year is –1.14% for log hourly wages and –2.55% for log weekly earnings for every percentage point increase in the state-by-entry-year state unemployment rate. These estimates are noisier but consistent with initial wage scarring estimates found in Schwandt and Von Wachter (2019).[52] Similar to their findings, I observe that this scarring effect decays with experience and is undetectable after 15 years: the average scarring effect found in columns 1 and 3 over 20 years is much smaller than the initial scarring effect found in columns 2 and 4, row 2.

Given that experience is an important factor in assessing wage scarring, I would ideally like to also stratify my DDD estimates by individual years of experience. However, I lack statistical power to do so using the CPS. As an alternative, I divide the sample into two experience groups: workers with 1–5 years of experience and workers with 5–20 years. Looking again at scarring averages using Specification 3 from Section 2.4.3, I find that the average estimated scarring effect is concentrated in workers with 1–5 years of experience and is undetectable in workers with 6–20 years of experience (Table 24, columns 1 and 2).

[52] Schwandt and Von Wachter (2019) use 30 years of data to assess scarring so that they can detect the effect beyond 10 years. I use a smaller sample of years (2013–2021) because I am directly testing a policy that started being implemented in 2017.

TABLE XXIV: AVERAGE SCARRING EFFECT BY EXPERIENCE YEAR

	1 to 5 Years of Experience		**5 to 20 Years of Experience**	
	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) Log Hourly Wages	(4) Log Weekly Earnings
ue_{sk}	-0.0150***	-0.0232***	-0.0015	-0.0017
	(0.0026)	(0.0035)	(0.0014)	(0.0016)
Observations	25,117	25,592	76,167	77,398
Adjusted R^2	0.692	0.701	0.686	0.647

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state-by-job-market-entry-year level.

This is because the scarring effect becomes smaller and smaller with every experience year (Table 23, columns 2 and 4), so an average effect between experience years 5–20 would be small and underpowered. Table 25 estimates the DDD empirical strategy on a sample of workers with 1–5 years of experience and a sample of workers with 6–20 years of experience. I confirm the DDD estimate is concentrated within less experienced workers. Comparing the estimates in Table 24, columns 1 and 2 to Table 25, columns 1 and 2, respectively, I find that DDD estimates for 1–5 years of experience group represent a reversal of 90% of the original scarring effect.

I also estimate the general scarring effect for job-to-job transitions and unemployment transitions and find no statistically significant effects.[53] However-

[53]This separate analysis is omitted for brevity as no effects are reported.

TABLE XXV: DDD ESTIMATE BY EXPERIENCE YEAR

	1 to 5 Years of Experience		**5 to 20 Years of Experience**	
	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) Log Hourly Wages	(4) Log Weekly Earnings
SHB × ue_{sk}	0.0137**	0.0219*	0.0019	0.0038
	(0.0040)	(0.0087)	(0.0025)	(0.0037)
Observations	25,117	25,592	76,167	77,398
Adjusted R^2	0.693	0.701	0.686	0.647

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

ever, it is not necessary that a negative job mobility gap exist for SHBs to induce scarred workers to switch jobs more. If markets are perfectly competitive with full information, then scarred workers should switch jobs more relative to non-scarred workers because they are underpaid. The absence of a positive job mobility scarring gap but the presence of a negative compensation scarring gap suggests that scarred workers are not doing this prior to SHBs.

2.5.4 Heterogeneity within DDD Estimate

Many SHB laws were written with gender and racial wage disparities in mind. Similarly, Schwandt and von Wachter (2019) show that wage scarring is particularly damaging for non-whites. Therefore, it is important to understand how this law affects gender and race groups differently in the context of scarring.

Gender The event studies in Figure 8 show no evidence of pre-trends in SHB treatment for males in either log hourly wage or log weekly earnings. Similarly, with the overlay, there are also no differences in pre-trends between scarred and non-scarred men either. I observe a statistically significant increase in log hourly wages and log weekly earnings for scarred men from SHB enactment but do not observe this for non-scarred men.

Figure 8: SHB Effect by Scarring and Gender

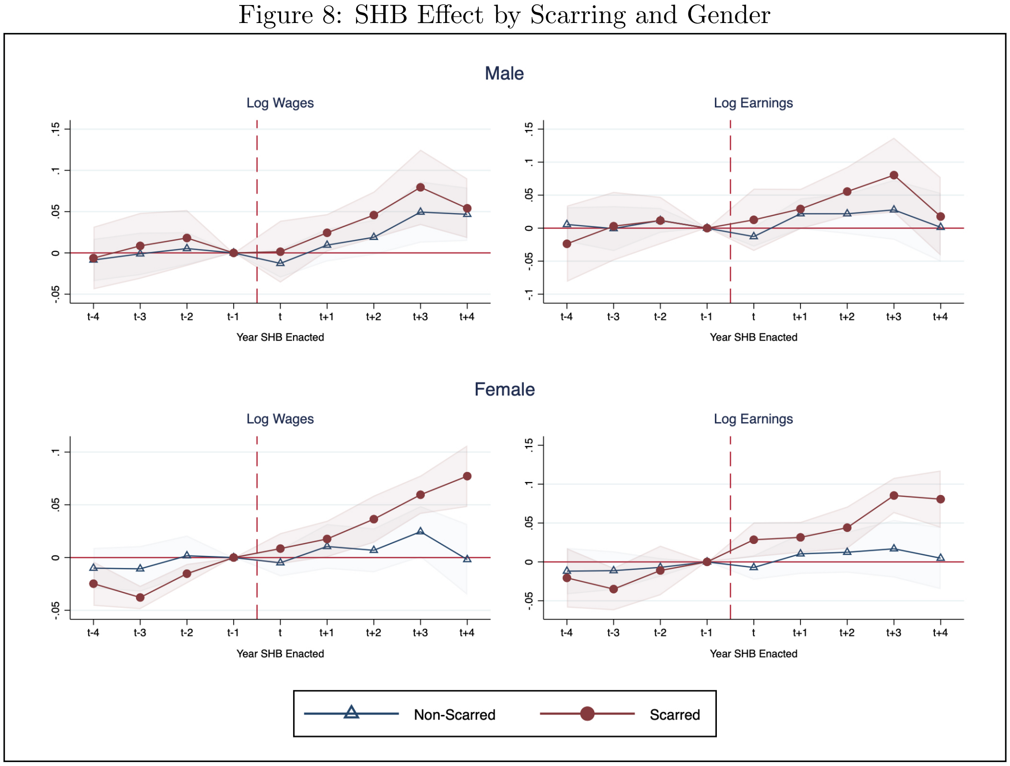


Table 26, columns 1 and 2 show that scarred men are advantaged by the law by 0.51% for log hourly wages and 0.74% for log weekly earnings relative to non-scarred men for every percentage point increase in the state-by-entry-year unemployment rate. In Table 26, columns 3 and 4, I observe an increase of 0.1% for job-to-job transitions but no statistically significant effect for unemployment-to-employment transitions for scarred males.

TABLE XXVI: SHB EFFECT BY SCARRING FOR MALES

	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) J2J Changes	(4) U2E Changes
SHB	0.0057	0.0112	-0.0003	-0.0013
	(0.0076)	(0.0098)	(0.0009)	(0.0011)
SHB × ue_{sk}	0.0051*	0.0074**	0.0010*	0.0006
	(0.0021)	(0.0026)	(0.0004)	(0.0004)
Observations	51,681	52,551	54,995	61,487
Adjusted R^2	0.722	0.728	0.025	0.056

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

I do find some evidence that scarred women were trending lower than non-scarred women in the pre-period (under Female in Figure 8). Table 27, columns 1 and 2 show that scarred women are advantaged by the law by 0.78% for log wages and 1.35% for log earnings relative to non-scarred women for every percentage point increase in the state-by-entry-year unemployment rate. I also observe evidence of an increase of 0.15% in job-to-job transitions for scarred women relative to non-scarred women as a result of the law change. In terms of magnitude, my estimates show that SHB laws advantage scarred women more than it advantaged scarred men.

TABLE XXVII: SHB EFFECT BY SCARRING FOR FEMALES

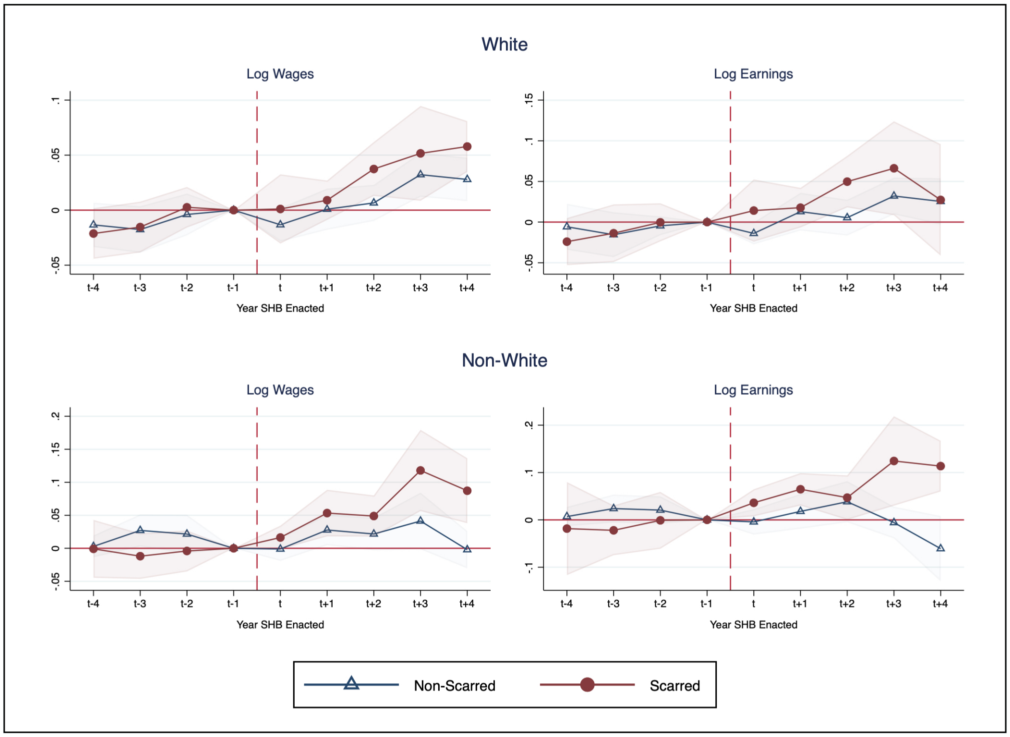
	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) J2J Changes	(4) U2E Changes
SHB	0.0194***	0.0208*	-0.0020	0.0002
	(0.0052)	(0.0100)	(0.0012)	(0.0007)
SHB × ue_{sk}	0.0078***	0.0135***	0.0015***	0.0002
	(0.0020)	(0.0025)	(0.0004)	(0.0002)
Observations	49,603	50,439	53,012	59,559
Adjusted R^2	0.730	0.705	0.017	0.033

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

Race In a separate heterogeneity analysis, I split the sample between whites and non-whites, and I find no evidence of pre-trends between scarred workers and non-scarred workers across race (Figure 9).

Figure 9: SHB Effect by Scarring and Race



In Table 28, I find no statistically significant difference for scarred whites for log hourly wages, but I do observe an increase of 0.79% for log weekly earnings, 0.12% for job-to-job transitions, and 0.05% for unemployment-to-employment transitions. Non-whites increase log wages by 1.34%, log earnings by 1.85%, and job-to-job transitions by 0.13% for every percentage point increase in the state-by-entry-year unemployment rate (Table 29). Schwandt and Von Wachter (2019) show that general scarring effects are much larger in magnitude for non-white workers. Therefore it is plausible that DDD

TABLE XXVIII: SHB EFFECT BY SCARRING FOR WHITES

	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) J2J Changes	(4) U2E Changes
SHB	0.0124*	0.0153*	-0.0021+	-0.0011+
	(0.0052)	(0.0076)	(0.0011)	(0.0006)
SHB × ue_{sk}	0.0041	0.0079**	0.0012***	0.0005**
	(0.0024)	(0.0027)	(0.0003)	(0.0002)
Observations	61,775	62,449	63,950	67,983
Adjusted R^2	0.774	0.768	0.037	0.065

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

TABLE XXIX: SHB EFFECT BY SCARRING FOR NON-WHITES

	(1) Log Hourly Wages	(2) Log Weekly Earnings	(3) J2J Changes	(4) U2E Changes
SHB	0.0094	0.0123	0.0011	0.0012
	(0.0094)	(0.0144)	(0.0018)	(0.0015)
SHB × ue_{sk}	0.0134***	0.0185***	0.0013+	-0.0001
	(0.0033)	(0.0044)	(0.0008)	(0.0004)
Observations	39,509	40,541	44,057	53,063
Adjusted R^2	0.640	0.622	0.015	0.030

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

estimates are much larger for non-whites because of a larger initial scarring effect.

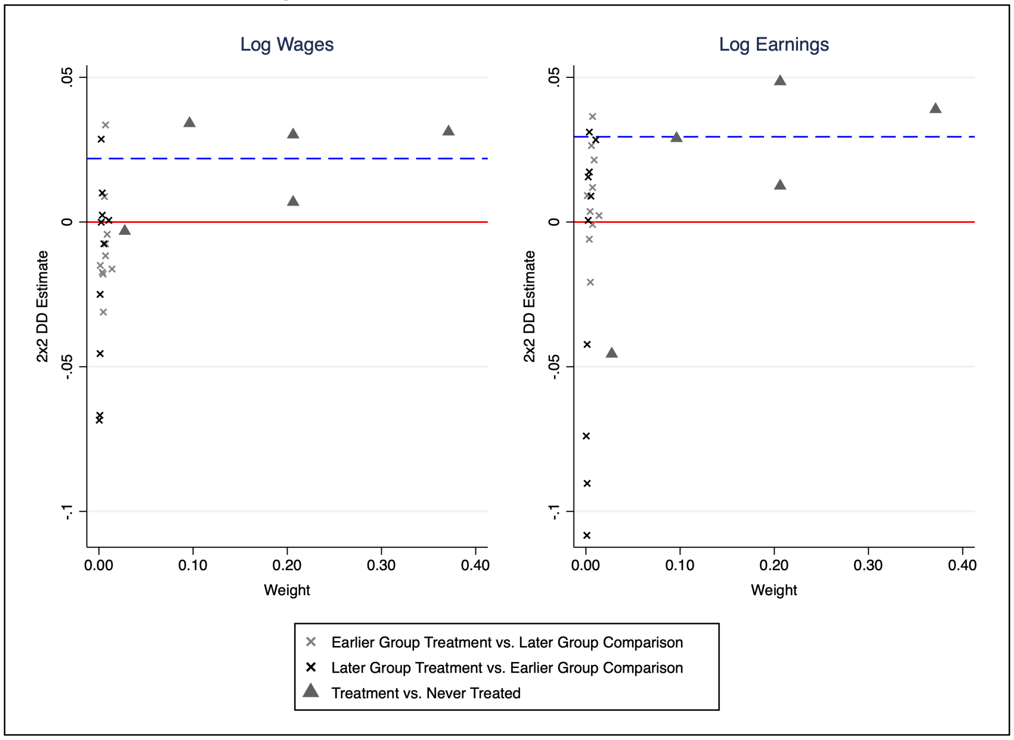
2.6 Additional Checks on Internal Validity

2.6.1 Goodman-Bacon (2021)

Estimates from DiD empirical models that exploit policy changes over multiple periods, often called two-way fixed effects (TWFE) models, can be biased if treatment effects are not homogeneous over time. In the context of this study, the principle concern is whether SHB estimates are biased because earlier adopters of SHBs are being compared to late adopters of SHBs. If treatment effects are homogeneous, then these comparisons are still valid. However, if treatment effects are increasing or decreasing over time, then these comparisons can attenuate or exacerbate estimates, respectively.

Goodman-Bacon (2021) proposes a decomposition method and shows that TWFE estimates are a variance-weighted average of each individual difference-in-difference comparisons between treated and non-treated states. Figure 10 illustrates this method by plotting individual treatment and non-treatment group DiD estimates by their magnitude and weighted contribution to the aggregate TWFE estimate. I observe that the aggregate estimate (denoted by the blue line) is mostly driven by treatment versus never treated

Figure 10: Goodman-Bacon Decomposition



This figure uses a decomposition method proposed in Goodman-Bacon (2021). The empirical strategy used in this paper to measure the effect of SHB laws represents a weighted average of individual difference-in-difference (DID) comparisons across all treated states and time periods used in the analysis. The y-axis the magnitude of each DiD estimate, and the x-axis represents the weight each estimate contributes to the composite effect. Each triangle represents a DiD comparison between treated states and non-treated states. The light grey x's represent DiD comparisons between states that enacted SHB laws earlier to the pre-SHB period in states that eventually pass SHBs. The black x's represent DiD comparisons between states that pass SHBs later and the pre- and post-SHB periods in states that pass SHBs earlier. These later group treatment vs. earlier group comparisons are potentially biased. The estimates using the Callaway-Sant'Anna (2020) method in Table 32, Columns 2 and 4, show the composite DiD effect of SHBs excluding these potentially biased terms.

comparisons. There are a few earlier group treatment versus later group comparisons that yield negative coefficients in magnitude, but they also have very low weights. Conversely, I observe a few later group treatment versus earlier group comparisons that are positive but also have very low weights.

Table 30 shows that most of the aggregate TWFE estimate for log wages comes from a positive average difference-in-difference estimate of 2.5% from comparing treated states to non-treated states ("T vs. Never treated") as the weight on this estimate is 0.907. Both comparisons between groups with different treatment periods show negative coefficients. When earlier treated states post-treatment are compared to later states post-treatment ("Earlier T vs. Later C) and later treated states post-treatment are compared to earlier treated states post-treatment ("Later T vs. Earlier C"), they both show a negative effect on average.

TABLE XXX: GOODMAN-BACON DECOMPOSITION FOR LOG WAGES

	(1)	(2)
DiD Comparison	Weight	Average DiD Estimate
Earlier T vs. Later C	0.062	-0.007
Later T vs. Earlier C	0.030	-0.002
T vs. Never treated	0.907	0.025

For log weekly earnings, Table 31 shows that most of the effect measured in the TWFE estimates comes from comparing treated states to non-stated treated states. However, the average DiD estimate for comparisons between

TABLE XXXI: GOODMAN-BACON DECOMPOSITION FOR LOG WEEKLY EARNINGS

DiD Comparison	(1) Weight	(2) Average DiD Estimate
Earlier T vs. Later C	0.062	0.010
Later T vs. Earlier C	0.030	0.009
T vs. Never treated	0.907	0.031

earlier and later treated states are both positive. I can see in Figure 10 that these terms do not contribute much to my overall estimate. Nevertheless, given that they are not approximately 0, I am concerned about the potential for bias and want to test for it. In Section 2.6.2, I use a new estimator by Callaway and Sant'Anna (2020) to assess the magnitude of this bias.

2.6.2 Callaway and Sant'Anna (2020)

In their paper, Callaway and Sant'Anna (2020) propose a new estimator when using the staggered treatment in a difference-in-difference framework. They propose an estimator that uses group-time average treatment effects. For example, Oregon, NY,[54] and Delaware all passed SHB laws in 2017. Together these states constitute the 2017 treatment group. Comparisons are then made between this group and non-ban states and pre-treatment SHB states. Under a parallel trends assumption, these estimates would be valid as

[54] As noted in Section 2.4.1, I treat NY as passing an SHB in 2017 because of bans passed in NYC and Albany County in 2017.

no other states were treated when they states were treated. The 2018 group, California, Massachusetts, and Vermont, is compared to non-ban states and pre-treatment SHB states. However, the states treated in 2017 are excluded as these comparisons might yield bias. This same process is repeated for 2019 and 2020 groups, and four estimates are derived. These estimates are then aggregated to a main parameter of interest, the overall effect of participating in the treatment across all groups that have ever participated in the treatment.

Table 32, columns 1 and 3 provides baseline OLS estimates of the SHB laws passing.[55] These estimates are similar to results found in Table 18 with the exception that only state and year fixed effects are included in this specification. Columns 2 and 4 show estimates of SHB laws passing using the estimator proposed by Callaway and Sant'Anna (2020). Since this estimator does not include comparisons between earlier and later treated SHB states, these estimates provide a perfect gauge to assess potential bias. Given that columns 2 and 4 are similar in magnitude and more precise than columns 1 and 3, respectively, it is likely that bias from comparing earlier and later treated SHB states do not change the general conclusions from my OLS estimates.

[55]These estimates are similar to DiD estimates in Table 18. However, I exclude covariates at this time as the R package used for Callaway and Sant'Anna (2020) estimates, did, requires data aggregated at the state-level instead of the individual-level. However, excluding covariates yields similar results to my general DiD estimates in Table 18 that do use covariates (1.76% versus 2.19% for log wages and 2.36% versus 2.95% for log weekly earnings).

TABLE XXXII: TWO-WAY FIXED EFFECTS VERSUS CALLAWAY AND SANT'ANNA (2020)

	Log Wages		Log Weekly Earnings	
	(1)	(2)	(3)	(4)
	TWFE	C&S	TWFE	C&S
SHB	0.0219**	0.0245***	0.0295**	0.0284**
	(0.0079)	(0.0059)	(0.0093)	(0.0133)
Observations	459	459	459	459
Adjusted R^2	0.946		0.926	

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level. Table 32, Columns 1 and 3 are analogous to Table 18 estimates. I estimate the general SHB effect on the dataset without including additional fixed effects because I want my estimates to serve as a valid comparison to the the Callaway and Sant'Anna (2020) method. The estimation package in R that uses this new estimator, did, requires data to be aggregated at the state level, not the individual level. Table 32, Columns 2 and 4 show the general DiD effect using this new estimation package.

2.7 Conclusion

In this paper, I show that SHBs increase job mobility for scarred workers relative to non-scarred workers and reduce the gap in hourly wage and weekly earnings between these workers. However, my analysis also suggests this policy carries unintended consequences. I find, in absolute terms, that job mobility for non-scarred workers is lower after SHBs are enacted. This finding is consistent with Meli and Spindler (2019) that higher-paid workers may be disadvantaged by these laws because they can no longer signal higher perceived productivity from their higher wages. It may be a better policy to simply encourage employers to post salary information without limiting job applicants from volunteering it. Roussille (2020) shows that providing this information removes disparities in initial salary bids between men and women, as women increase their bids after learning what the employer intends to pay. Scarred workers appear to react similarly to these gains in information, and such a policy would not limit non-scarred workers from volunteering salary information.

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APPENDIX

TABLE XXXIII: 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_i	0.0078	0.0035	0.0041	0.0057	0.0073	0.0105	0.0151	0.0168
	(0.0107)	(0.0106)	(0.0106)	(0.0106)	(0.0105)	(0.0106)	(0.0104)	(0.0104)
2 years, ue_i	-0.0087	-0.0149+	-0.0145+	-0.0139+	-0.0141+	-0.0142+	-0.0117	-0.0113
	(0.0080)	(0.0077)	(0.0076)	(0.0076)	(0.0076)	(0.0074)	(0.0074)	(0.0073)
3 years, ue_i	-0.0183*	-0.0234*	-0.0232*	-0.0230*	-0.0224*	-0.0249**	-0.0228*	-0.0225*
	(0.0091)	(0.0091)	(0.0091)	(0.0091)	(0.0090)	(0.0091)	(0.0090)	(0.0089)
4 years, ue_i	-0.0285***	-0.0331***	-0.0322***	-0.0332***	-0.0326***	-0.0366***	-0.0359***	-0.0360***
	(0.0083)	(0.0083)	(0.0082)	(0.0082)	(0.0082)	(0.0082)	(0.0082)	(0.0081)
5 years, ue_i	-0.0055	-0.0129	-0.0129	-0.0144+	-0.0148+	-0.0210*	-0.0212*	-0.0208*
	(0.0091)	(0.0089)	(0.0088)	(0.0087)	(0.0086)	(0.0087)	(0.0085)	(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***	-0.0539***	-0.0540***	-0.0535***	-0.0536***	-0.0518***	-0.0530***	-0.0529***
	(0.0039)	(0.0037)	(0.0037)	(0.0037)	(0.0037)	(0.0038)	(0.0039)	(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 XXXIV: 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*	-0.0221*	-0.0200*	-0.0199*	-0.0182*	-0.0181*	-0.0153+	-0.0153+
	(0.0095)	(0.0088)	(0.0087)	(0.0088)	(0.0087)	(0.0087)	(0.0087)	(0.0087)
2 years, ue_{si}	-0.0234**	-0.0251**	-0.0243**	-0.0246**	-0.0252**	-0.0253**	-0.0240**	-0.0240**
	(0.0087)	(0.0080)	(0.0080)	(0.0081)	(0.0080)	(0.0080)	(0.0079)	(0.0079)
3 years, ue_{si}	-0.0089	-0.0099	-0.0097	-0.0098	-0.0092	-0.0092	-0.0088	-0.0088
	(0.0083)	(0.0075)	(0.0076)	(0.0076)	(0.0076)	(0.0076)	(0.0075)	(0.0075)
4 years, ue_{si}	-0.0218*	-0.0199*	-0.0201*	-0.0207*	-0.0203*	-0.0205*	-0.0211*	-0.0211*
	(0.0097)	(0.0091)	(0.0091)	(0.0091)	(0.0091)	(0.0091)	(0.0091)	(0.0091)
5 years, ue_{si}	-0.0181+	-0.0205*	-0.0222*	-0.0227*	-0.0237**	-0.0239**	-0.0251**	-0.0251**
	(0.0095)	(0.0092)	(0.0090)	(0.0090)	(0.0089)	(0.0090)	(0.0090)	(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***	-0.0190***	-0.0200***	-0.0204***	-0.0205***	-0.0205***	-0.0204***	-0.0204***
	(0.0036)	(0.0036)	(0.0035)	(0.0035)	(0.0035)	(0.0035)	(0.0035)	(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.

TABLE XXXV: SHB EFFECT ON COMPENSATION (INCLUDING STATE WORKERS)

	(1) Log Hourly Wages	(2) Log Weekly Earnings
SHB	0.0168***	0.0225**
	(0.0043)	(0.0079)
Observations	102,452	104,131
Adjusted R^2	0.730	0.723

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

TABLE XXXVI: SHB EFFECT ON COMPENSATION BY SCARRING (INCLUDING STATE WORKERS)

	(1) Log Hourly Wages	(2) Log Weekly Earnings
SHB	0.0125*	0.0144
	(0.0054)	(0.0086)
SHB $\times$ ue_{sk}	0.0065***	0.0110***
	(0.0019)	(0.0023)
Observations	102,452	104,131
Adjusted R^2	0.731	0.725

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

TABLE XXXVII: SHB EFFECT ON COMPENSATION (EXCLUDING NY STATE)

	(1) Log Hourly Wages	(2) Log Weekly Earnings
SHB	0.0194***	0.0261**
	(0.0043)	(0.0079)
Observations	99,947	101,603
Adjusted R^2	0.725	0.719

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

TABLE XXXVIII: SHB EFFECT ON COMPENSATION BY SCARRING (EXCLUDING NY STATE)

	(1) Log Hourly Wages	(2) Log Weekly Earnings
SHB	0.0166**	0.0212**
	(0.0049)	(0.0073)
SHB $\times$ ue_{sk}	0.0052**	0.0093***
	(0.0016)	(0.0020)
Observations	99,947	101,603
Adjusted R^2	0.727	0.720

+ 0.1, * 0.05, ** 0.01, *** 0.001

Note: Standard errors clustered at the state level.

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