

**Rethinking the Refugee “Burden”:
The Impact of Refugee Inflows on U.S. Labor Market and
Education Outcomes**

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Abstract

I explore the impact of a negative shock to United States refugee admissions on U.S. labor market and educational outcomes. I use a temporary ban on refugee admissions following the September 11, 2001 attacks as a natural experiment, exploiting variation in the historical placement of refugees across the U.S. In historically high relative to low refugee receiving counties, the 43,000-refugee drop in refugee admissions from 2001 to 2002 was associated with a significant increase in unemployment rates, and a decrease in log median household income. The negative impact of the temporary ban differentially affected areas with a larger high-skilled workforce, as well as areas with a large foreign-born population. These margins of heterogeneity suggest complementarities between refugees and high-skilled or immigrant workers in the U.S. Despite significant labor market impacts, the 61% decrease in refugee inflows had no impact on enrollment or graduation rates at local 2-year postsecondary institutions. This study broadly contributes to the economics of migration literature and an understanding of the destination-country impacts of migration. My findings add to emerging research about the effect of refugee migrants on developed countries, and inform increasingly relevant policy debates about the refugee crisis.

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1 Introduction

As of 2021, there are over 26 million refugees across the globe – the largest refugee crisis since World War II. According to the 1952 Refugee Convention, refugees are individuals forced to flee their country of origin due to a well-founded fear of persecution based on factors such as race, religion, nationality, political opinion, or social group membership (UNHCR, n.d.). International powers like the United Nations and its member states have a humanitarian duty to ensure refugees' fundamental rights and safety. Although the international protection of refugees is a global effort, developing nations currently host 85% of the world's refugees, as neighboring states are often the first country of refuge. Refugee resettlement, identified by the United Nations High Commissioner for Refugees (UNHCR) as one of three durable solutions to the refugee crisis, is a way developed countries can help share the responsibility of supporting refugees. However, host communities often have perceptions that refugees may strain economic resources, threaten national security, and compete with host country workers for job opportunities.

Economists have widely debated the economic impact of migrants on destination countries. Migrant impacts on earnings largely depend on whether immigrants complement or substitute for receiving country workers. Canonical labor market theory assumes that immigrants perfectly substitute for domestic workers, suggesting that as migrant inflows expand the labor supply, they will compete with domestic workers for employment opportunities and drive down wages. However, empirical research has found that immigrants have positive or neutral effects on destination economies, supporting theories of complementarity (Peri and Sparber, 2009; Foged and Peri, 2016). More specifically, the literature on the effect of migrants on U.S. workers suggests that immigrants do not negatively impact the wages or employment of U.S.-born workers and may even complement them (Card, 1990; Peri and Yasenov, 2017; Clemens et al., 2018). This literature

on economic migrants is well documented, though still controversial. Refugees and economic migrants differ in why they migrate, where they migrate, and their circumstances upon arrival (Brell et al., 2020). Although some migration economists have researched refugees as a unique group, this literature is relatively new and limited.

This paper examines the impact of refugee inflows on destination countries. I explore the effect of a negative shock to U.S. refugee inflows on labor market outcomes for the U.S. workforce, and in turn, whether this affects U.S. postsecondary educational attainment. To do this, I use a policy change that temporarily banned refugee admissions as a natural experiment. After the September 11 (9/11) attacks, President George W. Bush implemented a temporary ban on refugee admissions, reducing refugee inflows by 61% from 2001 to 2002. Although this was a national policy, it affected areas differently based on whether they typically resettled a high or low number of refugees. I exploit plausibly exogenous variation in the timing of this policy and geographic variation in the historical exposure to resettled refugees to identify the causal effect of a negative shock to refugee inflows.

I compare differential changes in unemployment and income in historically high versus low refugee receiving counties during the years before and after the policy change using an event study methodology. I find significant labor market impacts for roughly three years after the policy change. The sudden contraction of refugee inflows significantly increased unemployment rates and lowered log median household incomes in high refugee receiving counties. The negative impacts of the temporary refugee ban suggest that at best, refugees can benefit the workforce in receiving communities, and at the very least, they cause no harm to local labor market conditions.

I explore heterogeneity in these impacts by the county skill composition and find that high refugee receiving areas with a large share of high-skilled workers experienced an additional

increase in unemployment rates relative those with less high-skilled workers. I also analyze heterogeneity by the county foreign-born population and find that high refugee receiving areas with a large foreign-born population share had a differential increase in unemployment compared to counties with a small foreign-born population share. These findings suggest that refugees may complement high-skilled and foreign-born workers.

Significant changes in labor market outcomes are a potential mechanism for making schooling decisions. If refugee inflows affect the earnings or employment opportunities of destination country workers, the changing costs and returns to education may affect their schooling decisions. To see whether these significant changes in unemployment and earnings influence human capital accumulation, I turn to a secondary analysis of education outcomes using an event study methodology. I compare differential changes in 12-month enrollment and graduation rates at 2-year postsecondary institutions in historically high relative to low refugee receiving states, in the years before and after the policy change. I find no statistically significant impact on enrollment or graduation rates.

I broadly contribute to the economics of migration literature by exploring the effect of migrants on destination country labor markets. However, these findings also contribute to the unique and emerging literature on refugee migrants. This literature finds that refugees substitute for destination country informal workers but positively impact firm production and local economic activity (Tumen, 2016; Altindag et al., 2020; Ruiz and Vargas-Silva, 2016; Alix-Garcia et al., 2018). In terms of human capital, some studies have found that refugee inflows negatively impact educational attainment and literacy (Baez, 2011), while others have found positive (Tumen, 2021) or null effects (Figlio and Ozek, 2019) on test scores. Since the developing world has hosted most

large refugee inflows, the literature is largely concentrated in this context.¹ Refugees may complement or substitute for destination country workers differently in developed countries than in the developing world. This study extends the refugee literature in a developed country context, by analyzing the effect of refugee inflows on workers across the entire U.S.

Exploring how refugee migrants impact labor outcomes and education in a developed country context yields valuable insights for policy debates about addressing the refugee crises. My results find that limiting resettlement has significant negative impacts on the labor market outcomes of refugee receiving communities. Assuming symmetric effects for changes in refugee admissions, this suggests that expanding resettlement may have positive or neutral impacts on economic outcomes in receiving communities. Resettlement is an effective and durable strategy that benefits both refugees and their receiving communities. As the refugee crisis has fallen on the developing world, developed countries can bear more responsibility by resettling refugees into safe and permanent communities.

2 Literature Review

2.1 A Review of Migrant Impacts on Receiving Countries

The economics of migration literature has extensively researched the impact of migrants on receiving countries by exploiting policies that create plausibly exogenous variation in the number or placement of immigrants. The Mariel Boatlift, a mass influx of Cuban exiles into Miami that increased the labor force by 8%, is a landmark natural experiment used in the migration literature to study the effect of low-skilled immigrants on the U.S. labor market.² Although Card (1990)

¹ With the exception of Figlio and Ozek (2019), which looks at Haitian earthquake-induced refugees in Florida.

² Even though Mariel migrants were exiles seeking asylum and refuge in the U.S., they were primarily low-skilled males of prime working age (Clemens, 2017). Their characteristics more closely resembled low-skilled migrant workers than refugees at the time, allowing these findings to shed light on the impact of low-skilled migrants on U.S. workers and contribute to the migration literature more broadly.

finds that the *Marielitos* had no effect on wages or employment in Miami, Borjas (2017) counters that there were negative impacts on U.S.-born high-school dropouts. Peri and Yasenov (2017) provide a final contribution to this debate, using synthetic controls to improve on Card's (1990) approach. They find no significant effects of the Mariel Boatlift, even when analyzing the limited sample of non-Cuban high school dropouts examined by Borjas (2017).

Some studies have found that low-skilled immigrants do not harm destination country workers, and moreover, may have direct benefits. Foged and Peri (2016) explore the impact of low-skilled migrants by exploiting variation in a random refugee dispersal policy in Denmark that influenced future migrant waves. Family reunification placed immigrants in the same areas refugees were randomly dispersed. Foged and Peri (2016) use exogenous variation in the placement of low-skilled migrants from refugee-sending countries to identify the causal impact of low-skilled migration on Danish workers. They find that low-skilled migrant inflows pushed Denmark's low-skilled workers into less manual labor. These occupational shifts are consistent with Peri and Sparber's (2009) theory that migrants can improve native specialization. While most of this research centers the impact of migrant inflows, Clemens et al. (2018) estimate the effect of the removal of the Bracero program, a migrant *exclusion* policy intended to improve the wages and employment of U.S.-born workers. Ultimately, Clemens et al. (2018) find this policy was unsuccessful, as wages increased at slower rates in areas with greater exposure to the policy. Broadly, this literature introduces empirical support that migrants do not harm the wages of U.S.-born workers, contrary to simple labor market theory.

2.2 Refugee Migrants and Economic Migrants

Refugees differ from economic migrants in several ways, which leads to differential economic and cultural integration in their destination countries. The core distinction between the two types of

migrants is the ability to decide when and where to migrate. Economic migrants make decisions about when and where to migrate based on economic "push" factors motivating them to emigrate from their home country and "pull" factors attracting them to immigrate to a specific host country. On the other hand, refugees must forcibly and unexpectedly migrate to flee violent conflict or persecution in their country of origin.

Due to sudden and forced migration, refugees arrive in receiving countries with less locally applicable human capital skills and more significant language barriers than economic migrants (Brell et al., 2020). Additionally, refugees often experience traumatic events, extended stays in asylum countries and refugee camps, and may develop mental and physical health issues, which could negatively affect their human capital skills (Brell et al., 2020). However, as most refugees cannot return home, they invest in country-specific human capital skills at higher rates (Cortes, 2004). Because non-economic factors like conflict and persecution drive refugee migration, refugee composition has less economic selectivity. Refugee populations include both low- and high-skilled workers, yet they are initially employed or compensated below their skill level because of barriers to economic integration. Even though refugees initially have lower wages and employment than economic migrants, their earnings increase at faster rates over time (Brell et al., 2020; Cortes, 2004).

2.3 Refugee Literature

The refugee literature has been largely concentrated in the context of the developing world. Tumen (2016) and Altindag et al. (2020) study the labor market impacts of Syrian refugee inflows in Turkey and find that refugees replace informal workers but increase firm production and formal employment. Similarly, Ruiz and Vargas-Silva (2016) find that refugee inflows to Tanzania increased the likelihood that destination country workers became self-employed while refugees

took up casual wage work. These findings suggest that refugees may complement formal workers with different skill profiles but substitute for informal workers, leading to unemployment or occupational shifts in low-skilled industries.

These studies explore the impacts of massive refugee inflows that enter host countries through temporary or informal processes. Alix-Garcia et al. (2018) explore the impact of long-term refugee camps in Kenya on the economic welfare of host communities. They find that refugee inflows increase economic activity and consumption in areas within proximity to the camps, as camps create employment opportunities and refugee demand raises agricultural prices. Bahar et al. (2021) extend the refugee literature and explore the effects of a formal refugee naturalization policy. The naturalization of Venezuelan refugees in Colombia had no negative impact on wages, employment, or labor force participation. Mayda et al. (2017) exploit the U.S. refugee placement process, and find compelling evidence that permanent refugee resettlement in the U.S. has no significantly negative long-term labor market impacts on U.S.-born workers.³

The effect of refugees on labor market outcomes is a potential mechanism by which changing returns to human capital accumulation impact educational decisions in receiving countries. Yet, the refugee literature has found conflicting evidence about refugee impacts on education. Some studies have found that refugees decrease years of schooling and literacy rates among local children (Baez, 2011), while others have found that refugees have positive or null impacts on test scores (Tumen, 2021; Filgio and Ozek, 2019). Nonetheless, the literature on how refugees impact human capital accumulation in host countries remains sparse, especially in the context of developed countries.

³ Mayda et al. (2017) use matching techniques to identify counterfactual observations for commuting zones that receive a high number of refugees relative to the population. Exploiting similar variation in the exogenous assignment of refugees, I identify counterfactual observations using a policy shock as a natural experiment.

3 Background

3.1 Refugee Resettlement

Refugee resettlement is when refugees are relocated from an asylum country to a third country that has agreed to grant them permanent residence status. Although the UNHCR identifies resettlement as one of three durable solutions to the refugee crisis, less than one percent of refugees are resettled each year (UNHCR, n.d.). There is a significant "resettlement gap" in the number of individuals the UNHCR recommends for resettlement and the number of actual resettlement admissions. The resettlement process begins with the UNHCR, which identifies refugees eligible for international protection. The UNHCR prioritizes the most vulnerable refugees for resettlement, including women and girls at risk, survivors of violence, children and adolescents at risk, and those with legal protection or medical needs. Next, refugee information is shared with one of 29 resettlement countries. As of 2021, the U.S., Canada, Sweden, Germany, and Norway resettle the highest number of refugees (UNHCR, 2021). Although refugees can ultimately decide whether to resettle, they have no say regarding their prospective country of resettlement.

3.1.1 U.S. Refugee Resettlement Process

Before 1980, the U.S. authorized refugee admissions on an ad-hoc basis. Refugee legislation was first implemented in 1948 for European refugees and later expanded to admit those fleeing communist regimes in Europe, Latin America, and Asia. The Refugee Act of 1980 was passed after the Vietnam War placed great demand on resettlement services. The act standardized the resettlement process by creating the Office of Refugee Resettlement (ORR) and U.S. Refugee Admissions Program (USRAP). Since the Refugee Act of 1980, the U.S. has resettled over 3 million refugees.

The number of refugees admitted to the U.S. is decided on an annual basis. The U.S. President sets a ceiling on the number of available refugee admissions each year, known as the Presidential Determination, which Congress then approves. All refugees referred to the U.S. by the UNHCR must undergo a selective vetting process that involves background checks, security screenings, and interviews. This rigorous process can take up to 2 years and is the most challenging way to migrate to the U.S. (International Rescue Committee, n.d.). Once approved, the Department of State assigns refugee cases to one of nine non-governmental organizations (NGOs) operating across the country. These NGOs connect refugees to voluntary resettlement agencies that resettle them in local communities.

The number of cases that voluntary resettlement agencies receive from their corresponding NGO depends on the affiliate agency's capacity. The agency's staffing and financial resources, local housing and employment availability, and past success with resettlement determine the capacity for resettlement (Catholic Charities Springfield, 2022). Once NGOs assign refugee cases to a local resettlement agency, several factors influence placement. Refugees have no say in where they resettle short of a U.S. tie. The U.S. prioritizes family reunification by placing refugees with existing relatives. However, less than one percent of refugees represent family reunification cases (Baugh, 2019). In the absence of a tie to the U.S., federal guidelines require refugees to be placed in housing within a 100-mile radius of a resettlement agency (Catholic Charities Springfield, 2022). Given that immigrants tend to migrate to certain areas, many voluntary resettlement agencies are in areas that have existing migrant communities from refugee countries or are welcoming and open to migrant populations. The goal of resettlement is to place refugees in an area with the best cultural and economic integration opportunities.

Upon resettlement, refugees receive direct assistance for 90 days through the USRAP. This includes housing services, enrollment in employment services, school registration for children, and access to social or language services (Catholic Charities Springfield, 2022; Beaman, 2012). Specifically, agencies assist in the housing search, lease negotiation process, and grant up to a year's worth of rental assistance. Employment services include assistance reaching out to employers, completing application documents, interview transportation, and translation services. Refugees are immediately granted employment authorization to encourage economic self-sufficiency as soon as possible.

3.1.2 U.S. Refugee Characteristics

Since 1975, refugees representing 127 different nationalities have entered the U.S., primarily from Southeast Asia and Eastern Europe. Table 1 shows that the former USSR, Yugoslavia, and Vietnam were the top refugee-sending countries in the years before and after the September 11 attacks.⁴ Where do these refugees resettle? As highlighted by Table 2 Panel A, a disproportionate number of refugees resettled in California, New York, and Florida during the sample period. At the city level, Table 2 Panel B shows that many refugees resettled in the Portland, Houston, and Phoenix from 1995 to 2005. Although these areas were major refugee destinations, when adjusting for the size of the labor force, South Dakota, North Dakota, and Washington resettled the highest number of refugees per 100,000 workers (Table 3).

⁴ The USSR includes modern day Russia, Georgia, Ukraine, Moldova, Belarus, Armenia, Azerbaijan, Kazakhstan, Uzbekistan, Turkmenistan, Kyrgyzstan, and Tajikistan. Yugoslavia includes modern day Bosnia and Herzegovina, Croatia, Macedonia, Montenegro, Serbia, Kosovo, and Slovenia. As the refugee data dates to 1975, the designation of these countries by their former names was likely to preserve consistency in the dataset.

Table 1: Top Refugee Sending Countries, 1995-2005

Country	Refugees
Cuba	1,584
Iran	2,754
Iraq	1,303
Liberia	1,453
Sierra Leone	968
Somalia	1,652
Sudan	1,464
Vietnam	3,980
Yugoslavia	5,111
USSR	7,085

Source: Dreher et al., (2020) and author's calculations

Table 2: Top U.S. Refugee Receiving Areas, 1995-2005

Panel A: Top Refugee Receiving States		Panel B: Top Refugee Receiving Cities	
State	Refugees	City	Refugees
1. California	4,406	1. Portland, OR	253
2. New York	2,428	2. Houston, TX	249
3. Florida	1,656	3. Phoenix, AZ	215
4. Texas	1,620	4. Rochester, VA	207
5. Minnesota	1,503	5. Kansas City, MO	203
6. New Jersey	1,491	6. Dallas, TX	202
7. Massachusetts	1,477	7. Arlington, VA	201
8. Illinois	1,457	8. Chicago, IL	187
9. Virginia	1,403	9. Denver, CO	179
10. Washington	1,400	10. Buffalo, NY	171

Source: Dreher et al., (2020)

Table 3: Top U.S. Refugee Receiving States 1995-2005, Adjusted to Labor Force Size

State	Refugees (per 100,000 workers)
1. South Dakota	2,033.12
2. North Dakota	1,783.72
3. Washington	1,573.80
4. Vermont	1,456.76
5. Idaho	1,430.79
6. Minnesota	1,402.97
7. Washington, D.C.	1,283.24
8. Oregon	1,116.92
9. Iowa	1,096.78
10. Utah	975.09

Source: Dreher et al., (2020), LAUS, and author's calculations

Additionally, the demographic characteristics of incoming refugees vary based on the nature of different refugee waves. Table 4 summarizes available individual-level data on the stock of refugees entering the U.S. from 1975 to 2008. Refugees represent children, young adults, and middle-aged individuals, in close relative proportions. 28% of refugees arrived between ages 5 and 17, 23% between ages 18 to 25, and 30% between ages 26 and 45. Slightly over half of refugees are male, representing a closely even gender distribution. While a fifth of refugees are heads of family, over half identify as family dependents. In terms of education, roughly 83% of refugees 18 years or older have completed a secondary education or less. Broadly, this limited sample of characteristics highlights the variety of individuals served by refugee resettlement.

Table 4: Individual Characteristics of U.S. Refugees 1975 - 2008

Age	%	Sex	%	Education	%	Family Status	%
Under 5	9.61	Female	42.6	None	8.57	Dependent	54.7
5-17	28.1	Male	57.4	Primary	35.9	Head of Family	21.7
18-25	22.9			Secondary	38.6	Spouse	7.43
26-45	30.4			Technical	3.66	Unaccompanied Minor	1.38
46-65	7.93			University	12.4	Unattached Individual	14.8
Above 65	1.08			Graduate	0.9		
N = 224,354		N = 224,353		N = 128,644		N = 224,196	

Notes: Number of observations changes across columns due to missing individual-data. I limit education summary statistics to adults 18 and older.

Source: Dreher et al., (2020) and author's calculations

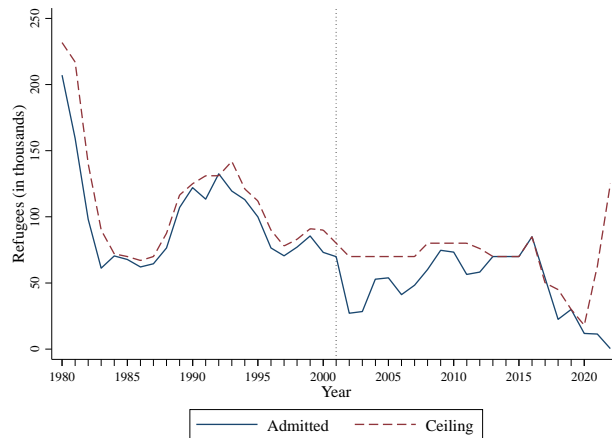
3.2 The 9/11 Moratorium on Refugee Admissions

Immediately following the September 11 attacks, President George W. Bush implemented a three-month ban on refugee admissions. President Bush cited national security concerns as the reason for this sudden ban, known as the 9/11 refugee moratorium. As shown in Figure 1, prior to 2001, the number of admitted refugees closely followed the annual refugee ceiling. However, the 9/11 moratorium created a 42,869-refugee gap between actual admissions and the admissions ceiling. Additionally, the reduction of nearly 43,000 admitted refugees from 2001 to 2002 is the largest year-to-year drop in admissions within the past three decades.

Although this ban was only in place for three months, it had lasting effects. The refugee moratorium occurred during the “fourth quarter bulge,” when as many refugees as possible are processed during the final month of the fiscal year (September), to fill any unused admissions spaces and meet the cap. Since the moratorium took place during a critical period for refugee admissions, the specific timing of the policy intensified the magnitude of the shock. This contraction in refugee admissions persisted for subsequent years, as backlogs in refugee admissions and increased security screening measures after the moratorium further slowed down the already lengthy bureaucratic process of refugee resettlement. From 2001 until the gap narrowed

in 2009, a total of nearly 188,000 admissions slots remained unused.⁵ At the time of the moratorium, roughly 23,000 refugees were already in the pipeline, while tens of thousands awaited the reopening of U.S. admissions (U.S. Committee for Refugees and Immigrants, 2001). Many refugees with cases approved for resettlement in 2001 did not arrive in the U.S. until 2005 (Beaman, 2012).

Figure 1: U.S. Refugee Admissions & Refugee Resettlement Ceilings



Notes: Includes FY 1980-2022 data (through February 2022)
 Source: Migration Policy Institute (MPI) Data Hub, 2021

4 Theoretical Framework

Labor market theory lies at the core of understanding how refugees impact economic outcomes in receiving country populations. According to canonical labor market theory, migrant inflows shift the labor supply curve outward and lower the equilibrium wage for all workers in a perfectly competitive market with perfect substitution. Since fewer U.S.-born workers are willing to work at the lower wage, migrants inflows will also increase unemployment. Broadly, this model suggests that refugees directly compete with U.S.-born workers for jobs and lower U.S.-born wages.

⁵ The gap between the refugee ceiling and actual admissions between 2001 and 2009 cannot be solely attributed to the 9/11 moratorium. However, the initial drop and stagnation from 2001-2003, and policy changes related to refugee screening likely had lasting impacts that affected the rate of increase in refugee admissions over time.

However, economists have proposed an alternative model to understand immigrant impacts on receiving country labor outcomes. The canonical model does not account for differences in the tasks performed by immigrants and destination country workers or heterogeneity in the skill level of workers. Relaxing the assumption that immigrants and receiving country workers are perfect substitutes, refugees migrants may complement U.S.-born workers (Peri and Sparber, 2009; Clemens et al., 2018). For instance, refugees may bring in specific skills or knowledge that increase the productivity of their employers. Similarly, as the labor supply increases, there may be a greater demand for low-skilled complementary workers. For example, as factories hire more refugees, they will need to hire more factory foremen. U.S.-born workers are more likely to fill these low-skilled complementary roles, as they have fewer barriers, such as language.

Under what I refer to as an “alternative” model, refugee inflows may have positive or null effects on the labor market outcomes of U.S.-born workers. Literature about the impact of economic migrants on U.S. earnings and employment provides empirical support for this model, which may also predict refugee impacts better than the canonical model (Card, 1990; Borjas, 2017; Peri and Yasenov, 2017; Clemens et al., 2018).

Labor market effects are a potential mechanism by which refugee inflows can affect educational outcomes in refugee host countries, as the costs and benefits to education influence schooling decisions. Under canonical labor market models, the influx of refugees lowers earnings, which reduces the opportunity cost and returns to education. Thus, the impact on educational attainment among U.S. workers would be ambiguous. Under an alternative labor market model, if refugees complement high-skilled workers and increase their earnings in the long run, the higher returns to skill may incentivize education for U.S.-born workers. If refugees substitute for low-skilled workers and drive down wages, the lower opportunity cost of schooling may also

incentivize increased educational attainment. Understanding the impact of refugee inflows on the labor market and educational outcomes using empirical data would shed light on which theoretical framework most appropriately explains the specific effect of refugees on destination country workers and students.

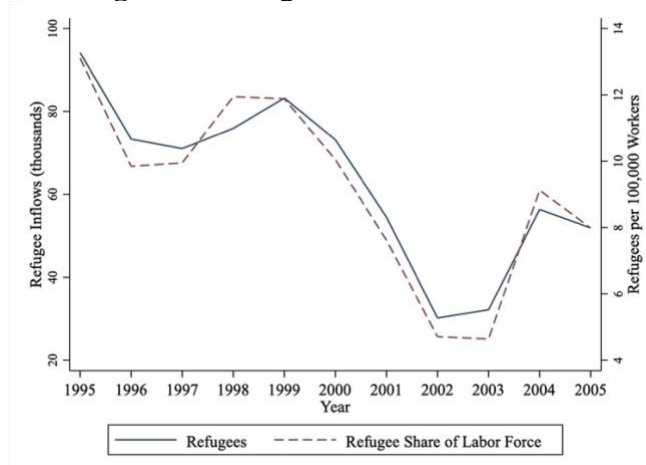
5 Data

5.1 Refugee Inflows

To identify refugee inflows over time, I use Dreher et al.'s (2020) Refugee Resettlement Data, which digitizes publicly held records from the Office of Refugee Resettlement (ORR) and Bureau for Population, Refugees, and Migration (PRM). PRM assists with the refugee screening and admissions process, and the ORR works with the refugee resettlement process and provision of resettlement services.

This geo-coded dataset includes country of origin, state and city of arrival, year of arrival, and destination county and state FIPS codes for all refugees entering the U.S. between 1975 and 2018. I use these data to aggregate refugee arrivals by county-year and calculate county-level refugee inflows from 1995 to 2005. Total refugee inflows over my sample period (1995-2005) are highlighted in Figure 2. Additionally, Figure 2 tracks the number of refugees per 100,000 U.S.-workers over time, which closely follows broader refugee patterns.

Figure 2: Refugee Inflows, 1995-2005



Source: Dreher et al., (2020), LAUS, and author's calculations

5.2 Labor Outcomes Data

I obtained labor force data from the U.S. Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS) program, which compiles monthly estimates of total employment and unemployment in 7,500 areas in the U.S. The data include county-level counts of the number of employed and unemployed workers, the size of the labor force, and the unemployment rate. I use these data to identify the unemployment rates and average size of the labor force in each U.S. County from 1995 to 2005.

I obtained income data from the U.S. Census Bureau's Small Area Income and Poverty Estimates (SAIPE) program, which produces annual counts of adults and children in poverty and includes median household income data for all U.S. states and counties. Using the SAIPE data, I estimate log median household incomes at the county-level from 1995 to 2005.⁶

5.3 Education Data

I obtained data on educational outcomes from the Integrated Postsecondary Education Data System (IPEDS). IPEDS data is submitted at the aggregate level from postsecondary institutions.

⁶ The 1996 SAIPE data is missing county-level estimates.

These data document institutional characteristics such as 12-month enrollment, admissions, completions, fall enrollment, finances, graduation rates, and financial aid information. From IPEDS data, I use variables on 12-month enrollment, the adjusted cohort of certificate-seeking students, and certificate seeking students who completed their degree in 150% of the typical time, for colleges reported between 1997 and 2005.⁷ I calculate the graduation rate for institutions in each year by taking the quotient of graduates within 150% of the typical completion time⁸ and the adjusted cohort of degree or certificate-seeking students. I report 12-month enrollment in thousands of students.

One concern is that aggregating institutional data at the county level will not accurately reflect changes in the enrollment and graduation patterns of county residents, due to migration out of counties for college. I address this by using state FIPS codes to aggregate 12-month enrollment and estimate mean graduation rates at the state level. A remaining concern is migration out of state for college, which I address by restricting my sample to 2-year institutions, since most 2-year college students are state residents.⁹ These concerns are further discussed in my empirical strategy section.

5.4 Descriptive Statistics

Table 5 provides summary statistics overall and by high and low refugee receiving areas. Panel A shows summary statistics at the county level. The average refugee receiving county admitted 67.8 refugees and resettled 38.9 refugees per 100,000 workers in the labor force from 1995 to 2000. The average unemployment rate is 4.8%, and log median household income is 10.62. Panel B

⁷ IPEDS data is missing for years 1995 and 1996, because postsecondary institutions did not record 12-month enrollment and graduation data in that year. IPEDS data from 1998 is dropped because the data was constructed in a way that is not comparable with the other years.

⁸ Completers within 150% of the normal time refers to students who graduated in 1.5 times the years it takes full-time students to complete a program. For instance, students graduating from a 2-year program within 3 years.

⁹ At two-year public colleges, the median distance from students' home to college is 8 miles, compared to 18 miles for those at 4-year public colleges (Wexler, 2016).

shows summary statistics at the state level. The average U.S. state resettled 33.1 refugees per year, and 17.25 refugees per 100,000 workers from 1995 to 2000. The mean 2-year postsecondary 12-month enrollment is 196,470 students, and the average graduation rate is 43.5%.

Table 5: Summary Statistics

	<u>Total</u>			<u>High Refugee Receiving</u>			<u>Low Refugee Receiving</u>		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Panel A: County Level Data									
Refugees	67.819	267.855	10,252	298.611	526.714	2,123	7.545	22.616	8,129
Pre-9/11 Refugee LF Share	38.389	95.486	10,252	152.356	165.404	2,123	8.625	8.825	8,129
Unemployment Rate	4.808	1.952	10,245	4.750	2.135	2,123	4.823	1.901	8,122
Log Median HH Income	10.616	0.231	9,116	10.643	0.218	1,874	10.609	0.233	7,242
Panel B: State Level Data									
Refugees	33.052	73.473	572	97.587	135.194	121	15.737	23.570	451
Pre-9/11 Refugee LF Share	17.225	28.439	572	52.589	46.238	121	7.737	5.445	451
12-Month Enrollment	196.472	348.979	408	338.369	650.844	88	157.450	181.037	320
Graduation Rate	0.435	0.133	407	0.468	0.140	87	0.426	0.130	320

Notes: Sample sizes differ across variables due to missing data.

Source: Dreher et al., (2020), IPEDS, LAUS, SAIPE, and author's calculations

6 Empirical Strategy

I use the 9/11 moratorium on refugee admissions as a natural experiment to identify the causal impact of restricted refugee inflows on U.S. labor market and education outcomes. Refugee inflows to the U.S. have fluctuated over time, as the resettlement ceiling has changed under different Presidential administrations. However, the 2001 refugee moratorium was a sudden negative shock to refugee inflows that decreased refugee admissions by 61% from one year to the next. Given the variation in where refugees are resettled across the U.S., counties that resettled a

higher number of refugees prior to the temporary ban were differentially impacted by this shock. This policy's exogenous timing and geographic impact allows me to exploit quasi-random variation and identify the effect of a sudden negative shock to refugee inflows.

I use a difference-in-difference (DD) methodology to isolate the impact of this sudden contraction of refugee inflows, drawing on methodologies used by Clemens et al. (2018) and Masterson and Yassenov (2021). Figure 3 shows significant geographic variation in where refugees resettle across the U.S. As highlighted in the background section, this geographic variation is related to the location of resettlement agencies, which tend to be in areas where refugees have a strong likelihood of successful cultural and economic integration. I exclude counties that did not resettle refugees prior to 2001 from my sample, as these counties may systematically differ from refugee receiving counties in factors beyond their resettlement capacity.

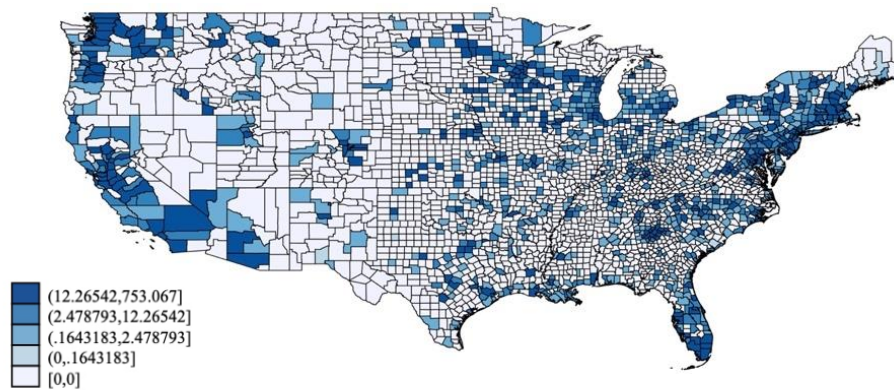
Using a sample of all counties that received refugees prior to the policy change, I exploit variation in whether counties are historically high or low refugee receiving areas. I identify historically high (low) refugee receiving counties as those with an above (below) mean number of refugees per 100,000 workers over the pre-policy period (1995-2000). Historically high refugee receiving areas usually remain high refugee receiving areas in the future. 62% of counties with above average refugee share in 1995 continued to resettle an above average refugee share until the policy change in 2001, and roughly 70% of counties had an above average refugee share¹⁰ for at least 4 years before the policy change.

Table 5 shows that on average, high refugee receiving counties resettle 298 refugees per year, compared to only 7 in low receiving counties. I use the pre-policy average (1995-2000) number of refugees per 100,000 people in the county labor force to measure the historical intensity

¹⁰ 398 counties had an above average number of refugees per 100,000 U.S. workers across the years before the policy change (1995-2000). Among these, 278 had a high share of refugees for at least 4 of those years.

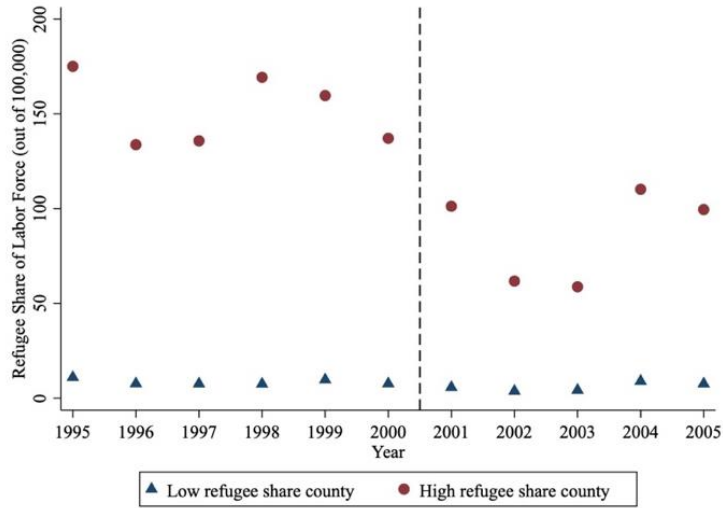
of refugee exposure. Although this was a nationwide policy change, it affected counties differently based on the previous intensity of refugee resettlement. I propose that historically high refugee receiving counties are differentially impacted by this policy, as they would have resettled a greater number of refugees in the absence of the policy. Historically low refugee receiving counties are essentially a control group, as they receive few refugees, irrespective of the policy. Thus, I exploit geographic variation in the historical intensity of refugee inflows to isolate the effect of the 9/11 refugee moratorium policy. Figure 4 shows that after the policy change, the refugee labor force share remained stable in low refugee receiving counties but dropped sharply in high refugee receiving counties. My identifying assumption is that in the absence of the temporary refugee ban, labor and education outcomes in high relative to low refugee receiving counties would trend in parallel, before compared to after the policy change.

Figure 3: Refugees (per 100,000 workers), by County



Notes: The map describes average number of refugees per 100,000 workers in the labor force from 1995-2000
Source: Dreher et al., (2020) and author's calculations

Figure 4: Refugee Share Over Time, by Historically High vs. Low Receiving Counties



Notes: The red circle markers show the mean number of admitted refugees per 100,000 U.S. workers in the labor force of historically high refugee receiving counties. The blue triangle markers show the mean number of admitted refugees per 100,000 workers in historically low refugee receiving counties. The vertical dashed line represents the passing of the temporary refugee admissions ban.

Source: Dreher et al., (2020), LAUS, and author’s calculations

Table 5 presents summary statistics by historically high and low refugee receiving counties. My county level sample includes 932 counties; 193 of them are classified as “historically high” refugee receiving areas. Unemployment rates in high refugee receiving counties are 0.073 percentage points lower than in low receiving counties. Log median household incomes are 0.3% higher in high refugee receiving counties, relative to low receiving counties. My state level sample includes 51 states, 11 of which are “historically high” refugee receiving areas.¹¹ 12-month enrollment in historically high refugee receiving states is nearly twice as high as in low refugee receiving states. Graduation rates in high refugee receiving states are roughly 4.2 percentage points higher than in low refugee receiving states. My analyses include county and state fixed effects to control for fixed differences between high and low refugee receive counties.

¹¹ My state level sample includes Washington, D.C.

6.1 Labor Outcomes Analysis

6.1.1. Difference-in-Difference

I estimate the effect of the temporary refugee ban on labor market outcomes using the following difference-in-difference equation:

$$Y_{ct} = \beta_0 + \beta_1 High_c * Post_t + X_{c0}\gamma_t + \alpha_c + \gamma_t + \varepsilon_{ct} \quad (1)$$

where Y_{ct} is a measure of the log median household income or unemployment rate in county c , year t . $High_c$ is a binary indicator equal to 1 for historically high refugee receiving counties, and 0 otherwise. $Post_t$ is a binary indicator equal to 1 for observations from 2001 to 2005, and 0 otherwise. To control for county-specific time-invariant characteristics, I include county fixed effects, α_c . Similarly, to control for national time-varying trends during my sample period I include year fixed effects, γ_t . I interact a vector of baseline controls, X_{c0} , with year fixed effects. These interactions capture the time-varying impact of baseline characteristics that are correlated with the variable $High$, and my outcomes of interest. To account for the fact that outcomes in counties may evolve differentially based on their pre-policy income or unemployment, I include 1995 county unemployment and log median household income as baseline controls. I cluster standard errors at the county level.

My coefficient of interest is β_1 , representing the differential change in income or unemployment in historically high compared to low refugee receiving counties, before relative to after the refugee moratorium. If refugees complement the U.S. labor force, I expect that the β_1 coefficient on income is negative, indicating that admitting fewer refugees negatively impacted log income in refugee receiving counties. I expect that the β_1 coefficient on unemployment is positive, suggesting that fewer refugees in the labor force also increases unemployment rates

among U.S. workers. However, if refugees substitute for U.S. workers, then both β_1 coefficients mentioned above would be signed in the opposite direction.

6.1.2. Event Study

I use an event study methodology to estimate the dynamic effects of year-to-year changes in refugee admissions in high relative to low refugee receiving counties. I estimate the following event study equation:

$$Y_{ct} = \sum_{\tau \neq 2000} \beta_{\tau} * High_c * D_t^{\tau} + X_{c0} \gamma_t + \alpha_c + \gamma_t + \varepsilon_{ct} \quad (2)$$

where Y_{ct} and $High_c$ are defined as in equation 1. D_t^{τ} is a binary indicator equal to 1 if the year of observation t equals the specific year, τ , and 0 otherwise. As in equation 1, $X_{c0} \gamma_t$ is a vector of baseline income and unemployment controls interacted with year fixed effects. In the robustness checks section, I discuss additional controls added to this specification. Again, I include county and year fixed effects, α_c and γ_t , respectively. Standard errors remain clustered at the county level. The coefficient β_{τ} on the interaction term $High_c * D_t^{\tau}$ represents the differential change in outcomes by year for historically high versus low refugee receiving counties relative to 2000, which is omitted to identify the model because it is the year before the policy change. Statistically insignificant β_{τ} coefficients prior to 2001 suggest that outcomes in high receiving counties were not trending differentially prior to the policy change and provide evidence that my identifying assumption for causal interpretation is valid.

6.2 Education Outcomes Analysis

If restricting refugees inflows significantly impacts the incomes and employment of the U.S. population, changing labor market outcomes are a potential mechanism for changes in postsecondary education attainment. I explore the impact of the temporary refugee ban on measures of postsecondary educational attainment such as enrollment and graduation.

A threat to identifying changes in educational attainment at the county level is migration out of county to attend college, as this would misrepresent the impact of residing in a high refugee receiving county. Instead, I turn to a state-level secondary analysis. Impacts on educational outcomes will reflect the impact of residing in a high refugee receiving state, thus accounting for within-state migration. During 2018, in over half of states (27 of 50), residents made up at least 75 percent of total fall student enrollment (NCES Blog, 2020). I also restrict my sample to 2-year postsecondary institutions. Refugees are more likely to substitute for low-skilled workers upon arrival (Cortes, 2004). These low-skilled workers planning to enter the workforce are more likely to be on the margin of enrolling in a 2-year institution than a 4-year institution. Additionally, 2-year colleges may have a higher proportion of in-state residents than 4-year colleges, which mitigates concerns about bias from out migration for education.

To estimate the effect of refugee inflows on education outcomes, I use equations 1 and 2, with state-year variables instead of county. My outcomes of interest, Y_{st} , are now measures of the total 12-month enrollment or graduation rate in state s , year t . The expected direction of β_1 coefficients on educational outcomes is ambiguous. Depending on the impact of the refugee ban on income and unemployment, the relative strength of changing opportunity costs and returns to education will determine the direction and magnitude of the effect.

I test the validity of my labor market and education results by using event study analyses to check for parallel trends and verify that my identifying assumptions hold. I also address threats to the validity of my estimates, such as out-migration, spillover effects, and the confounding impacts of the 9/11 attacks. I further detail these robustness checks and controls in section 8.

7 Results

7.1 Labor Outcomes Results

Table 6 summarizes the impact of the policy change on labor market outcomes. Columns 1 and 3 show the difference-in-difference results for log median household income and unemployment rates, respectively, estimated by equation 1. The sudden contraction in refugee inflows had a significant negative impact on log median household incomes, which were 0.9 percent lower in high relative to low refugee receiving counties after the policy. The differential decrease in median household income among high refugee receiving areas suggests that the presence of refugees help increase incomes in receiving communities. The coefficient on the unemployment rate is positive, suggesting that the negative shock to refugee inflows also increased unemployment in high receiving areas. However, this coefficient is not statistically significant.

Event study results show the dynamic effects of the policy change on income and unemployment. The insignificant coefficients for the pre-policy change years (1995-1999) in columns 2 and 4 of Table 6 support that labor outcomes in high and low refugee receiving counties were not trending differentially prior to the policy change. Given that the parallel trends assumption appears to hold, the post-policy changes can be causally interpreted. Consistent with the DD estimates, Panel A of Figure 5 shows that high refugee counties experienced a significant decrease in log median household income for three years after the policy. Although the DD coefficient on unemployment was insignificant, Figure 5 Panel B shows a significant increase in unemployment for two years after the policy, before leveling out. Since the DD coefficient averages the effect over post-policy years, it does not reflect the significant short-run impact of the temporary refugee ban on unemployment.

These findings suggest that a decrease in refugee admissions had significant negative impacts on labor market outcomes in receiving U.S. communities. The results provide interesting insights on the inverse scenario: a sudden increase in refugee admissions among high refugee receiving counties. Assuming symmetric effects for both increases and decreases in refugee admissions, these findings imply that an increase in refugee resettlement of roughly equal magnitude would positively impact the labor market outcomes of individuals in high refugee receiving areas across the U.S.

Table 6: Labor Market Outcome Results

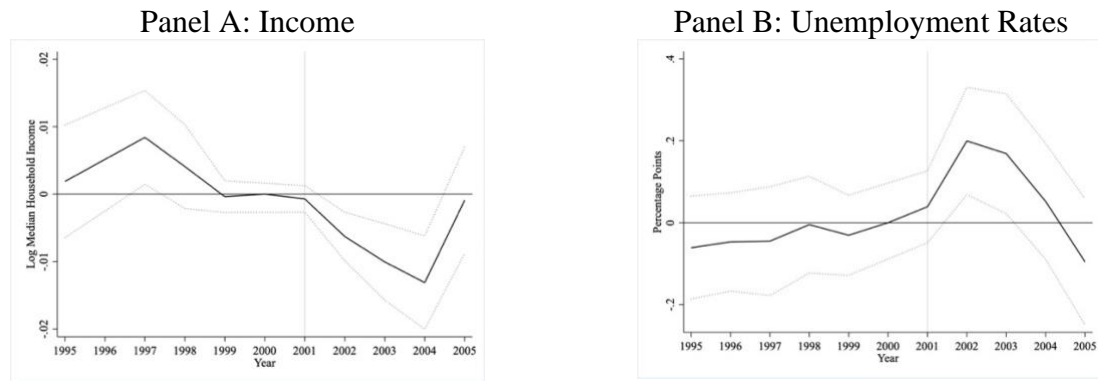
	Log Median HH Income		Unemployment Rate	
	DD (1)	Event Study (2)	DD (3)	Event Study (4)
High*Post	-0.009** (0.003)		0.104 (0.066)	
High*1995		0.002 (0.004)		-0.061 (0.064)
High*1996		0 (.)		-0.047 (0.061)
High*1997		0.008* (0.004)		-0.045 (0.068)
High*1998		0.004 (0.003)		-0.005 (0.060)
High*1999		0 (0.001)		-0.031 (0.050)
High*2001		-0.001 (0.001)		0.039 (0.045)
High*2002		-0.006*** (0.002)		0.200** (0.067)
High*2003		-0.010*** (0.003)		0.169* (0.075)
High*2004		-0.013*** (0.004)		0.052 (0.072)
High*2005		-0.001 (0.004)		-0.095 (0.079)
R-squared	0.853	0.853	0.624	0.625
N. of cases	9104	9104	10223	10223
Pre-period mean for high refugee counties	10.598	10.598	4.352	4.352

Notes: The table reports labor market estimates from basic DD and event study models. High is a binary variable equal to 1 for historically high refugee receiving counties in the U.S. Post is a binary variable equal to 1 if the year is 2001 or later. The dependent variable in Columns 1 and 2 is log median household income. The dependent variable in Columns 3 and 4 is unemployment rates. Columns 1 and 3 include baseline unemployment and income controls. Baseline controls are interacted with year dummies in Columns 2 and 4. County and year fixed effects are included in all specifications. Robust standard errors clustered at the county level are presented in parentheses. 1996 SAIPE income data is missing, hence the smaller sample size in Columns 1 and 2.

* p<0.05, ** p<0.01, *** p<0.001

Source: Dreher et al., (2020), LAUS, SAIPE, and author's calculations

Figure 5: Labor Outcomes Event Study Results



Notes: The solid lines represent coefficients from the event study model (equation 2). Interactions with a binary indicator for 2000 are omitted. The dotted lines show 95% confidence intervals. Specifications include county and year fixed effects, and baseline controls income interacted with year indicators. County-level baseline controls include 1995 unemployment rates, and 1995 log median household income. Standard errors are clustered at the county level. The light grey vertical line represents the temporary refugee ban in 2001. The p-values on the test of joint significance of the pre-period coefficients equal 0.0 (Panel A) and 0.8 (Panel B). Source: Dreher et al., (2020), LAUS, SAIPE, and author's calculations

7.2 Education Outcomes Results

Given the significant impact of refugees on U.S. workers' earnings and employment opportunities, I test whether shocks to refugee inflows also impact educational outcomes. The impact of the policy change on education outcomes is summarized in Table 7. Columns 1 and 3 show the difference-in-difference results. The DD coefficients on enrollment and graduation rates are not statistically significant, suggesting no differential change in educational outcomes in historically high relative to low refugee receiving states before and after the temporary ban.

The event study analysis of the refugee ban's impact on education outcomes sheds light on the dynamic effects. Like for labor market outcomes, the insignificant coefficients prior to 2001 suggest that we would not expect a differential change in educational outcomes for high refugee receiving states, in the absence of the policy. Again, estimates have causal interpretations because these parallel pre-trends hold. Consistent with the DD estimates, Panel C of Figure 6 shows no differential change in postsecondary enrollment at 2-year institutions for high refugee receiving

states. Similarly, Panel D of Figure 6 shows that there was also no significantly differential change in graduation rates for high refugee receiving states in the post-policy period.

The refugee ban's impact on educational outcomes may be diluted at the state level, potentially explaining the null effects of the policy change on enrollment and graduation at 2-year postsecondary institutions. I estimate the effects of the refugee shock on labor market outcomes at the state level in Figure 6. Panel A and B show that the policy change had no differential effect on the income or unemployment of historically high relative to low refugee receiving states. Since labor market effects are the proposed mechanism by which refugee inflows may affect education, the insignificant state level effects on income and unemployment may also fail to significantly impact state-level enrollment and graduation. Even though states like Texas have historically resettled many refugees, refugee inflows are concentrated to only a few counties (Figure 3). So, although the policy change may affect the educational outcomes of individuals residing in high refugee counties, these impacts are not reflected in the state-wide enrollment or graduation patterns that I am able to observe.

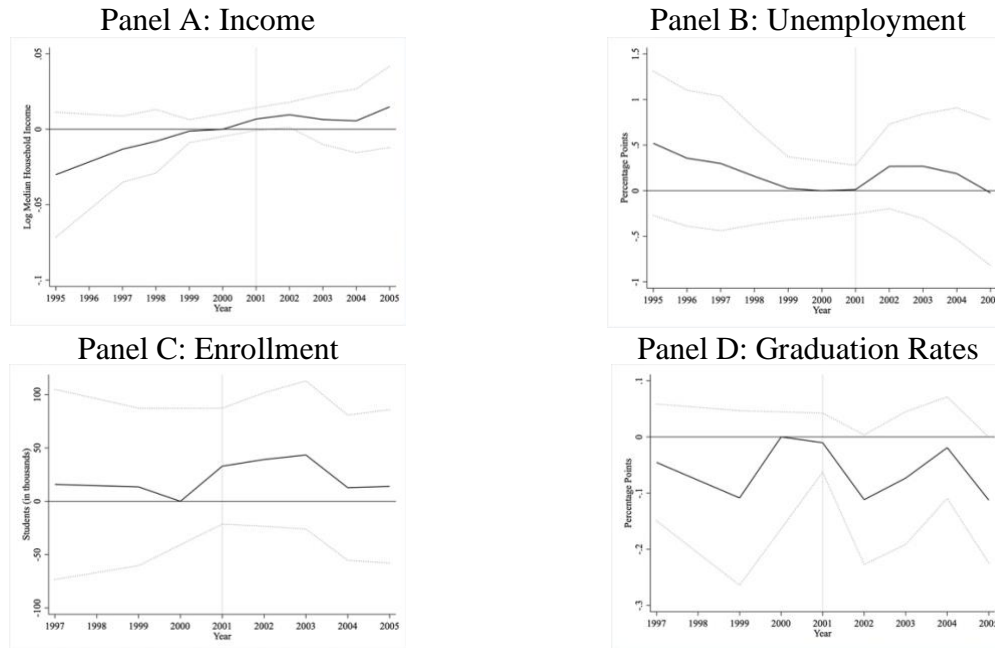
Table 7: Education Outcome Results

	Enrollment		Graduation Rate	
	DD (1)	Event Study (2)	DD (3)	Event Study (4)
High*Post	18.696 (21.757)		-0.014 (0.019)	
High*1997		15.855 (45.505)		-0.045 -0.053
High*1999		13.546 (37.650)		-0.109 -0.079
High*2001		32.993 (27.729)		-0.010 (0.027)
High*2002		39.241 (31.944)		-0.112 (0.059)
High*2003		43.451 (35.457)		-0.073 (0.060)
High*2004		12.758 (34.755)		-0.019 (0.046)
High*2005		14.038 (36.702)		-0.112 (0.057)
R-squared	0.226	0.990	0.130	0.822
N. of cases	408	408	407	407.000
Pre-period mean for high refugee states	312.448	312.448	0.470	0.470

Notes: The table reports education estimates from basic DD and event study models. High is a binary variable equal to 1 for historically high refugee receiving states in the U.S. Post is a binary variable equal to 1 if the year is 2001 or later. The dependent variable in Columns 1 and 2 is 12-month enrollment at 2-year postsecondary institutions, expressed in thousands of students. The dependent variable in Columns 3 and 4 is graduation rates at 2-year postsecondary institutions. Columns 1 and 3 include baseline unemployment and income controls. Baseline controls are interacted with year dummies in Columns 2 and 4. State and year fixed effects are included in all specifications. Robust standard errors clustered at the state level are presented in parentheses. IPEDS data from 1998 is dropped because the data was constructed in a way that is not comparable with the other years. * p<0.05, ** p<0.01, *** p<0.001

Source: Dreher et al., (2020), IPEDS, LAUS, SAIPE, and author's calculations

Figure 6: Education Outcomes Event Study Results



Notes: The solid lines represent coefficients from the event study model (equation 4). Interactions with a binary indicator for 2000 are omitted. The dotted lines show 95% confidence intervals. Specifications include state and year fixed effects, and baseline controls income interacted with year indicators. State-level baseline controls include 1995 unemployment rates, and 1995 log median household income. Standard errors are clustered at the state level. The p-values on the test of joint significance of the pre-period coefficients equal 0.7 (Panel A), 0.8 (Panel B), 0.9 (Panel C) and 0.4 (Panel D). The light grey vertical line represents the temporary refugee ban in 2001. Source: Dreher et al., (2020), IPEDS, LAUS, SAIPE, and author's calculations

7.3 Heterogeneity

The labor market effects of a contraction in refugee inflows provide insights on which theoretical models best explain the economic impact of refugee migrants, and migrants in general. The significant negative effect on the earnings and employment of U.S. workers suggests that refugees complement the U.S. workforce. This raises the question of which types of U.S. workers benefit from refugee labor. I estimate whether the contraction in refugee inflows had differential impacts based on the county skill composition. I also estimate whether the negative shock to refugee inflows had differential impacts on areas with a large foreign-born population.

7.3.1. Triple Difference

To parse out the impact of refugee inflows by the high-skilled (foreign-born) county composition, I use a triple difference (DDD) methodology to identify the differential impact of the policy change on areas with a large share of high-skilled workers (or large immigrant share of the population), in historically high versus low refugee receiving counties.¹² I designate counties with an above average share of employees in high-skilled industries during 1990 as a “high-skilled” county.¹³ Similarly, I designate counties with an above average 1990 foreign-born share of the population as a “high foreign” county. To isolate the differential impact of refugees by the skill composition or foreign-born share, I compare the double difference of the policy change in counties with a large high-skill share (high foreign-born share) to the same double difference in counties with a small high-skill share (low foreign-born share). The triple-difference estimating equation is:

$$Y_{ct} = \beta_0 + \beta_1 High_c * LargeShare_c * Post_t + \beta_2 High_c * Post_t + \beta_3 LargeShare_c * Post_t + X_{c0}\gamma_t + \alpha_c + \gamma_t + \varepsilon_{ct} \quad (3)$$

where Y_{ct} , $High_c$, $Post_t$, $X_{c0}\gamma_t$, α_c , and γ_t are defined as in equation 1. $Large Share_c$ is an indicator for counties with a large high-skilled or foreign-born share, equal to 1 for counties with an above average share of high-skilled workers or foreign-born individuals at baseline (1990), and 0 otherwise. The coefficient of interest, β_1 , captures the impact of the policy change when $High$,

¹² Ideally, I would use unemployment or income for high versus low-skilled workers as an outcome. However, since I do not have this data, I turn to using unemployment and income data for areas with a high versus low share of high-skilled workers.

¹³ I match educational attainment with occupational industry data from the 1990 Census to identify industries with above average years of education as “high-skilled” industries. Then, I use 1995 County Business Patterns (CBP) data on employment by SIC (Standard Industry Code) type to identify the share of county employees working in the industries I designated as high-skilled.

Large Share, and *Post* dummies all equal 1. β_2 and β_3 estimate double-interaction terms. Standard errors are clustered at the county level.

The identifying assumption is that in the absence of the policy change, the relative outcomes of workers in a more skilled or more foreign-born population in high versus low refugee receiving counties would trend in parallel to the relative outcomes of workers in a less skilled or foreign population, in high versus to low refugee receiving areas. Under this assumption, differences between high and low refugee receiving counties should not vary across areas with a large high-skilled or foreign-born share prior to the policy change.

7.1.1 Heterogeneity by Skill Level

A larger supply of low-skilled refugee workers may increase the demand for other complementary roles that low-skilled U.S.-workers could fill or may substitute for U.S.-workers. Similarly, high-skilled workers may become more productive as the supply of refugees with different and potentially complementary skills increases. I explore heterogeneity in refugee impacts by the receiving county skill composition, to identify whether areas with a larger portion of high-skilled workers are differentially affected by the policy change. Table 8 shows my triple-difference estimates. The impact of the policy change on income does not vary differentially by county skill level, as the triple difference coefficient is statistically insignificant. However, the refugee ban's impact on unemployment significantly varies by the county's skill composition. Workers in high-skilled, high refugee receiving counties experienced an additional 0.312 percentage point increase in unemployment rates after the policy, relative to those in less-skilled, high refugee receiving counties.

Table 8: Skill Heterogeneity in Labor Outcomes

	Income (1)	Unemployment (2)
High Skill * High Ref * Post	0.011 (0.007)	0.312* (0.157)
High Skill * Post	0.000 (0.003)	-0.296*** (0.064)
High Ref * Post	-0.017** (0.006)	-0.030 (0.136)
R-squared	0.853	0.628
N. of cases	9,094	10,212

Notes: The table reports labor outcome estimates from basic DDD model. High Skill is a binary variable equal to 1 for U.S. counties with a large high skill industry share. High Ref is a binary variable equal to 1 for historically high refugee receiving counties in the U.S. Post is a binary variable equal to 1 if the year is 2001 or later. The dependent variables are log median household income and unemployment rates. Baseline unemployment and income controls are interacted with year fixed effects. County and year fixed effects are included in all specifications. Robust standard errors clustered at the county level are presented in parentheses. 1996 SAIPE income data is missing, hence the smaller sample size in Column 1. * p<0.05, ** p<0.01, *** p<0.001

Source: CBP, U.S. Census, Dreher et al., (2020), LAUS, SAIPE, and author's calculations

7.1.2 Heterogeneity by Foreign-Born Share

Since immigrant workers may more closely substitute for refugee labor than U.S.-born workers, counties with a larger foreign-born population may respond differentially to this shock. I explore heterogeneity by the county immigrant population, using the 1990 Census¹⁴ to identify counties with an above average share of foreign-born residents. Table 9 shows the triple-difference estimates. The contraction of refugee inflows did not differentially impact incomes in counties with a historically large immigrant share. However, the refugee ban's impact on unemployment rates significantly differed by the county immigrant share. Unemployment rates for workers in high foreign-born, high refugee receiving counties were 0.5 percentage points higher than for those in low foreign-born, high refugee receiving counties. Given the disproportionately negative impact

¹⁴ I use a 5% Census sample. Since the sample does not have coverage of all U.S. counties, my data are restricted to 425 counties.

of the temporary ban on counties with a large foreign-born population, these findings suggest that refugee resettlement may improve employment opportunities in areas with large migrant communities.

Table 9: Heterogeneity in Labor Outcomes by Immigrant Share

	Income (1)	Unemployment (2)
High Foreign-Born * High Ref * Post	-0.003 (0.008)	0.500** (0.174)
High Foreign-Born * Post	0.019*** (0.005)	-0.398*** (0.112)
High Ref * Post	-0.007 (0.005)	0.030 (0.106)
R-squared	0.863	0.707
N. of cases	4,124	4,662

Notes: The table reports labor outcome estimates from basic DDD model. High Foreign-born is a binary variable equal to 1 for U.S. counties with a large foreign-born share of the population. High Ref is a binary variable equal to 1 for historically high refugee receiving counties in the U.S. Post is a binary variable equal to 1 if the year is 2001 or later. The dependent variables are log median household income and unemployment rates. Baseline unemployment and income controls are interacted with year fixed effects. County and year fixed effects are included in all specifications. Robust standard errors clustered at the county level are presented in parentheses. 1996 SAIPE income data is missing, hence the smaller sample size in Column 1. * p<0.05, ** p<0.01, *** p<0.001

Source: U.S. Census, Dreher et al., (2020), LAUS, SAIPE, and author's calculations

8 Robustness Checks

The two-way fixed effects used in my main specification control for many factors that may bias my estimates. County fixed effects control for any county-specific characteristics affecting labor market outcomes and refugee resettlement, which remain stable over time. Time fixed effects control for nationwide trends in labor outcomes, like a national economic recession. However, these fixed effects fail to control for factors that vary by county and time, besides the change in refugee inflows after the temporary ban. Thus, any factors correlated with historical refugee resettlement that are simultaneously occurring with the policy change will bias my estimates.

To address this concern, I interact 1995 unemployment rates and log median household income with year fixed effects. Interacting baseline labor outcomes with year fixed effects addresses concerns that locations with different labor market conditions before the policy change are trending differentially, thus leading to bias. Although the correlation between high refugee receiving counties and log median income or employment is small, areas with better or worse economic outcomes may evolve differentially even in the absence of the policy change. My results are robust to these controls, which I include in my main specifications. When estimating labor market impacts, these baseline controls from 1995 appear on both sides of equation 2. I omit these baseline controls and re-estimate equation 2 to check whether my results on income and unemployment are biased. My estimates are robust to the exclusion. With or without these restrictive baseline controls, I find that the temporary refugee ban significantly increased unemployment rates in historically high relative to low refugee receiving counties.

However, there may be differential trending by other baseline characteristics that could bias my results. I use a 5% sample of the 1990 Census to interact year fixed effects with a vector of baseline controls. These include within-state migration, out-of-state migration, the share of foreign-born population, and the share of the college-educated population. This dataset collects individual-level demographic survey data that provides a snapshot of the U.S. population. Since the 1990 Census does not have coverage of all U.S. counties, my sample is restricted to 425 counties. However, the ratio of high refugee receiving counties in the sample increases, and thus the relative size of the treatment group in the sample remains unaffected.¹⁵ Before adding controls, I estimate the event study results of this restricted sample using the preferred specifications in equation 2. As shown in Figure 7 Panel A, the results are consistent with my full sample findings.

¹⁵ The restricted sample includes 64 historically high refugee receiving counties and 443 historically low refugee receiving counties. Relative to the main sample, I lose 62 “treatment” counties and 443 “control” counties.

Using the restricted sample, I find that the temporary refugee moratorium significantly impacted income and unemployment, although the magnitude of the effect is smaller than in the full sample.

I discuss the intuition for each of my controls below.

One threat to the validity of my estimates is endogenous out-migration, that is, individuals may migrate in response to changes in immigration inflows. For example, destination country individuals may move out of their neighborhood or state as migrant populations grow in their neighborhoods. Conversely, the 9/11 moratorium may have decreased the outmigration of U.S. workers, as refugee inflows declined. Thus, endogenous moving patterns influenced by changes in refugee admissions may bias my estimates. Ideally, I would estimate whether out-of-state or within-state migration changes differentially because of the policy change. However, I cannot estimate county-level changes in outmigration over time because county FIPS code data is unavailable from 2000 to 2004. Instead, I interact year fixed effects with the baseline share of within-state and out-of-state migration. By controlling for whether certain areas have a higher or lower propensity to migrate in 1990 times year fixed effects, I allow states to evolve differently over time based on baseline out-migration patterns.

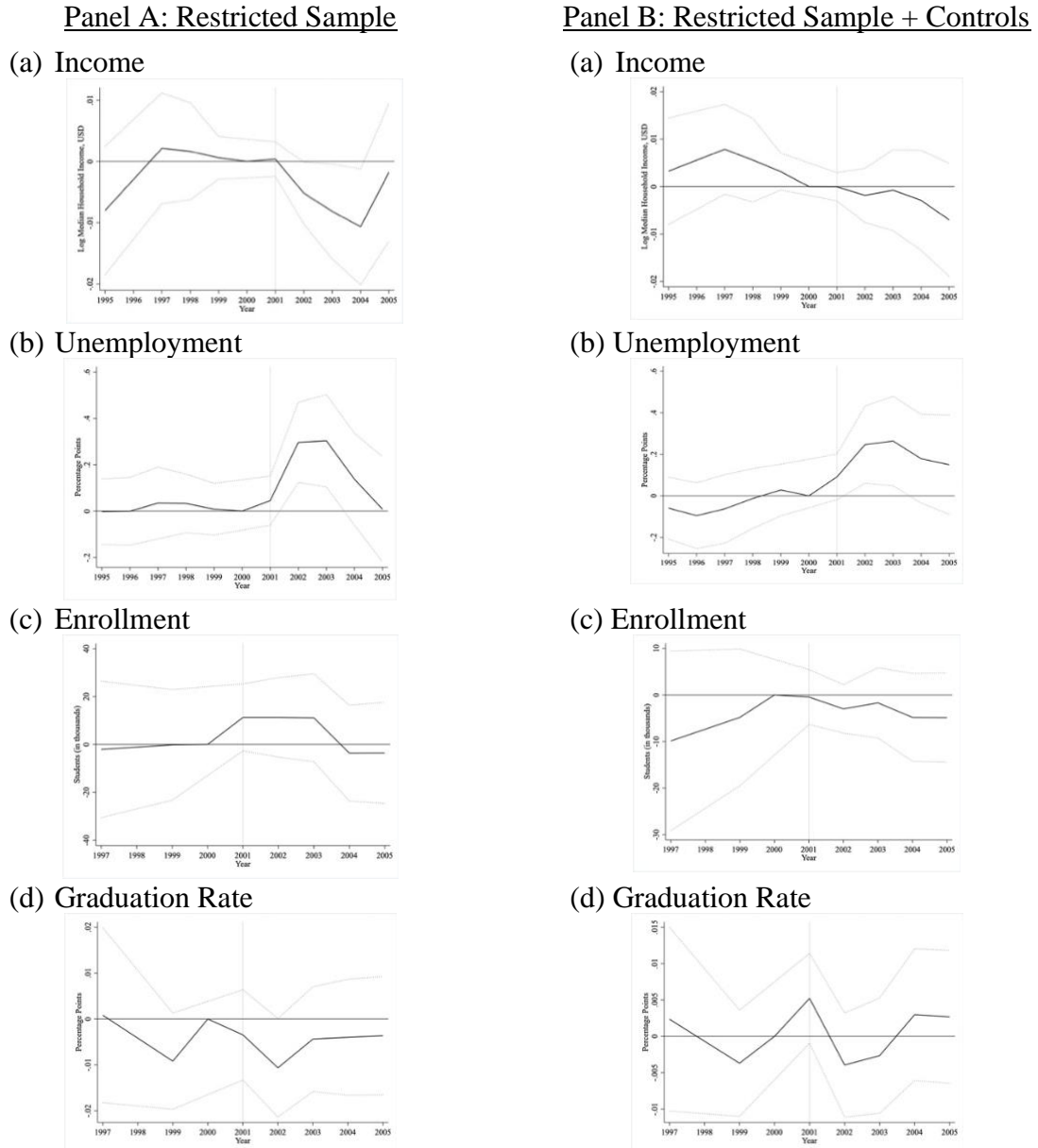
My results found that the policy change differentially impacted counties with a large share of high-skilled workers. I interact a baseline measure of college educational attainment with year fixed effects to control for the differential trending in outcomes for more highly educated individuals.¹⁶ The temporary ban also differentially impacted counties with a large share of immigrants. As with the county skill share, I interact a baseline measure of the foreign-born population with year fixed effects. This allows for differential trending of counties with a higher propensity to receive immigrants.

¹⁶ I measure the proportion of the county population with at least one year of college in 1990.

Figure 7 Panel B shows the event study results when controlling for a vector of these baseline characteristics, interacted with year fixed effects. The parallel pre-trends of my variables of interest are robust to the addition of these baseline controls, allowing for a causal interpretation of the effects while adjusting for potential threats. With added controls, the contraction in refugee resettlement does not significantly impact incomes in high relative to low refugee receiving counties. The effect of the policy change on unemployment remains significant, as unemployment rates differentially increased in high refugee receiving counties. However, the magnitude of this effect is smaller when controlling for baseline characteristics. There is no differential impact of the policy change on enrollment or graduation rates when adjusting for baseline characteristics.

I also report the labor market results using a continuous measure of mean refugees per 100,000 workers in the pre-policy period (1995-2000), instead of the binary variable *High*. Figure 8 shows event study results for income and unemployment. As expected, parallel trends hold when using a continuous measure. The effect of the policy change on income remains significant when using a continuous measure of refugees, yet the magnitude is much smaller. Results on unemployment are no longer significant when using the continuous share of refugees but follow the same pattern as earlier specifications.

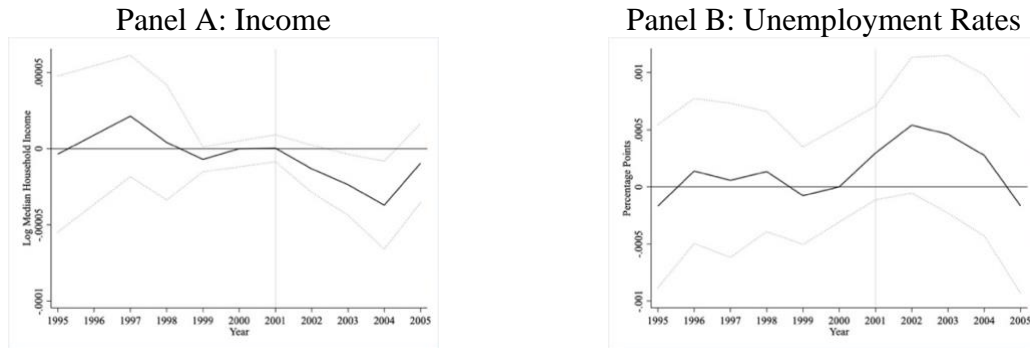
Figure 7: Event Study Results using Restricted Sample



Notes: The solid lines represent coefficients from event study models (equations 2 and 4). Interactions with a binary indicator for 2000 are omitted. The dotted lines show 95% confidence intervals. Specifications A and B include county and year fixed effects, specifications C and D include state and year fixed effects. All specifications include baseline controls income interacted with year indicators. Panel A baseline controls include unemployment rates and log median household income. Panel B baseline controls include: 1995 unemployment rates, 1995 log median household income, 1990 share in-state migration, 1990 share out-of-state migration, share of the foreign-born population, and share of college educated (at least one year). Panel B(c) does not control for the baseline share of college educated, due to high collinearity with enrollment. Standard errors are clustered at the county level in specifications A and B, and at the state level in specifications C and D. The p-values on the test of joint significance of the pre-period coefficients equal 0.01 (Aa), 0.9 (Ab), 0.9 (Ac), 0.1 (Ad), 0.1 (Ba), 0.7 (Bb), 0.5 (Bic), and 0.4 (Bd). The light grey vertical line represents the temporary refugee ban in 2001.

Source: U.S. Census, Dreher et al., (2020), IPEDS, LAUS, SAIPE, and author's calculations

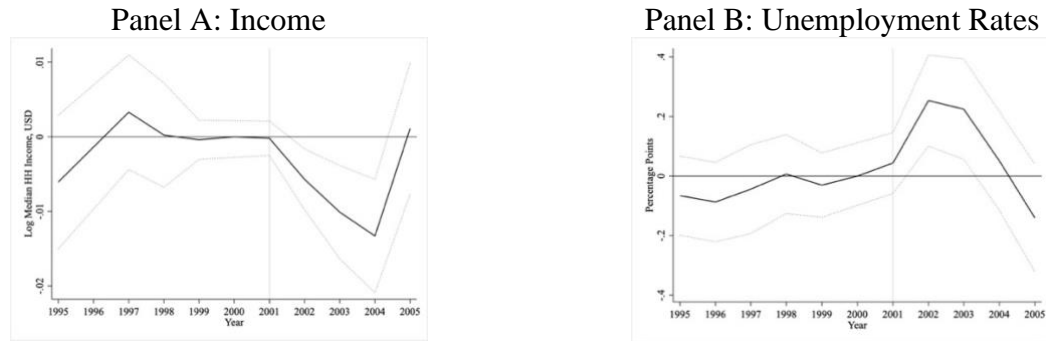
Figure 8: Event Study Results Using Continuous Share of Refugees (per 100,000 workers)



Notes: The solid lines represent coefficients from the event study model (equation 2), using a continuous measure of refugees per 100,000 in the labor force from 1995-2000. Interactions with a binary indicator for 2000 are omitted. The dotted lines show 95% confidence intervals. Specifications include county and year fixed effects, and baseline controls income interacted with year indicators. County-level baseline controls include 1995 unemployment rates, and 1995 log median household income. Standard errors are clustered at the county level. The light grey vertical line represents the temporary refugee ban in 2001. The p-values on the test of joint significance of the pre-period coefficients equal 0.0 (Panel A) and 0.4 (Panel B).

Spillover effects are a remaining threat to the validity of my estimates. Suppose county A is historically high refugee receiving, and neighboring county B is historically low refugee receiving. If workers in county A have higher unemployment after the policy change, they may look for jobs in county B instead. These spillover effects would underestimate the policy's true impact on labor market outcomes. To eliminate spillover effects, I identify all high-low bordering county pairs and drop the low “county B’s” from my sample. This exclusion ensures all high refugee receiving counties in the sample are not surrounded by any low refugee receiving counties. Shown in Figure 9, my results are robust to controlling for these spillover effects.

Figure 9: Event Study Results Excluding Low-Bordering Counties



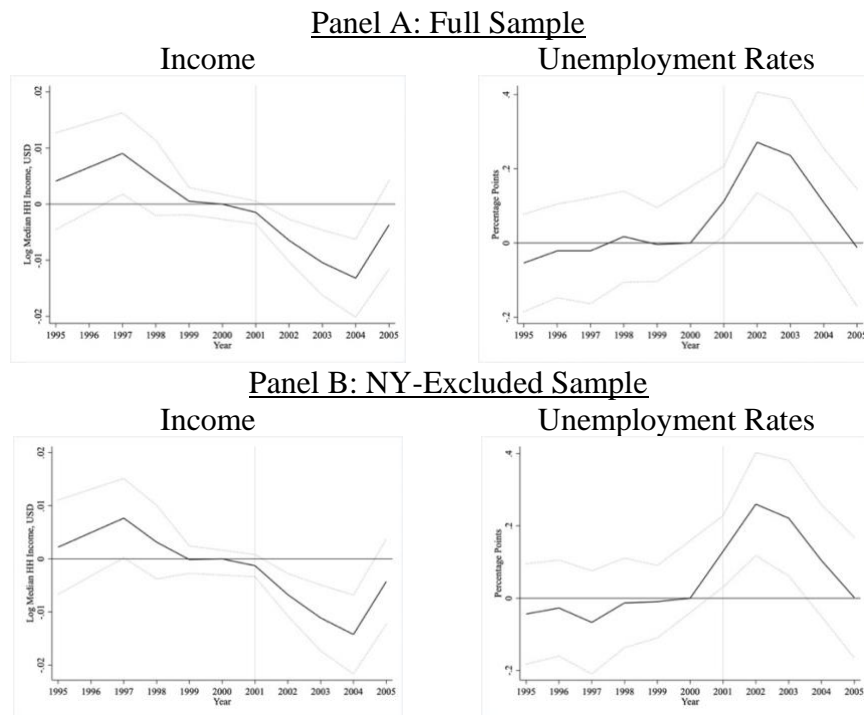
Notes: The solid lines represent coefficients from the event study model (equation 2). Interactions with a binary indicator for 2000 are omitted. The dotted lines show 95% confidence intervals. Specifications include county and year fixed effects, and baseline controls income interacted with year indicators. County-level baseline controls include 1995 unemployment rates, and 1995 log median household income. Standard errors are clustered at the county level. The p-values on the test of joint significance of the pre-period coefficients equal 0.0 (Panel A) and 0.6 (Panel B). The light grey vertical line represents the temporary refugee ban in 2001. The sample includes 601 counties, 193 counties are high refugee receiving.

Source: Dreher et al., (2020), LAUS, SAIPE, and author's calculations

The September 11 attacks had a resounding impact on the U.S., far beyond their implications for migration policies. After 2001, the U.S. experienced an economic downturn that disproportionately affected airlines, tourism, and insurance companies (Makinen, 2002). My income and unemployment estimates may be biased if areas that were differentially impacted by the post-9/11 economic downturn correlate with refugee placement, leading them to evolve differently over time. I account for the post-9/11 economic downturn by identifying counties that would have been highly affected by 9/11, using industry information from the 1995 Census' County Business Patterns (CBP) data. The dataset includes county-level employment counts by industry, which I use to calculate the county-level share of employees in transportation, utilities, finance, insurance, and real-estate industries. To control for the differential economic evolution of highly affected counties, I interact the baseline 9/11-affected industry share with year fixed effects. Figure 10 Panel A shows that my initial findings are robust to these controls, as parallel trends hold and changes in income and unemployment remain statistically significant.

To further control for the economic impacts of 9/11, I exclude the state of New York from my sample. The September 11 attacks had the largest impact on individuals in New York City, affecting earnings and unemployment beyond changes in refugee placement. Thus, areas in New York after the policy change may not be a good counterfactual. I test whether my estimates will be affected by excluding New York from my sample. However, Figure 10 Panel B shows my results are robust to omitting New York from the analysis, suggesting no significant bias.

Figure 10: Event Study Results, Controlling for Post-9/11 Affected Industry Shares



Notes: The solid lines represent coefficients from the event study model (equation 2). Interactions with a binary indicator for 2000 are omitted. The dotted lines show 95% confidence intervals. Specifications include county and year fixed effects, and baseline controls income interacted with year indicators. County-level baseline controls include 1995 unemployment rates, 1995 log median household income, and 1995 share of employees in 9/11-affected industries. The sample in Panel B excludes all observations from NY. Standard errors are clustered at the county level. The p-values on the test of joint significance of the pre-period coefficients in Panel A equal 0.0 and 0.8 for income and unemployment, respectively. The p-values in Panel B equal 0.0 and 0.8 for income and unemployment, respectively. The light grey vertical line represents the temporary refugee ban in 2001.

Source: CBP, Dreher et al., (2020), LAUS, SAIPE, and author's calculations

9 Conclusion

The impact of migrants on destination country workers has been at the forefront of research in the economics of migration. However, the literature on refugee migrants is small, especially in a developed country context. This paper broadly contributes to the migration literature and an understanding of how refugee migrants impact receiving country labor market and educational outcomes. I exploit variation in a temporary refugee ban implemented following the September 11 attacks, to isolate the causal impact of a sudden contraction in refugee admissions. Under my preferred specification, the policy change significantly decreased the log median household income in high refugee receiving counties. However, these results were sensitive to the addition of controls. I find that unemployment rates significantly increased in high relative to low refugee receiving counties after the negative shock to refugee inflows. This result is robust to controls for out-migration, the foreign-born share of the population, spillover effects, and the post-9/11 economic downturn. Furthermore, unemployment rates increased significantly more in counties with a larger share of high-skilled workers, and in counties with a larger immigrant population. The differential impact of a contraction in refugee inflows on high-skilled areas suggests that refugees may complement the high-skilled workforce in the U.S. Similarly, the differential impacts on areas with a large foreign-born population supports that there may be complementarities between refugee migrants and economic migrants. Although the contraction in refugee inflows significantly impacted local employment opportunities, there were no significant effects on educational outcomes.

Taken as a lower bound of the true effect, these findings suggest that refugee inflows have no negative impact on the economic outcomes of U.S. workers. Assuming these effects are symmetric, they imply that refugee inflows may even positively impact labor market outcomes,

especially for high-skilled or foreign-born workers. Refugees bring rich cultural diversity, boost innovation, and participate in local economies as consumers, workers, and taxpayers (Bahar et al., 2020; Moser et al., 2014). Informing global leaders about the true impact of refugees can motivate the implementation and expansion of resettlement services that provide necessary international protection for these involuntary migrants.

These findings also bring forth policy implications for the U.S., historically a leader in refugee resettlement. Since the Trump administration in 2016, refugee admissions have been at an all-time low. The negative economic impacts of limiting refugee inflows after the 9/11 moratorium shed light on the causal effects of Trump-era refugee policies. Given that only a three-month ban had lasting impacts, long-term policies that restrict refugee inflows could be harming receiving communities in unanticipated ways. Understanding how, if at all, resettled refugees impact the economic outcomes of U.S. individuals will help policymakers see resettlement as a viable solution to help thousands of refugees while increasing the cultural and economic richness of the U.S. If future administrations bring the U.S. to the forefront of refugee resettlement once again, perhaps global efforts will follow.

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