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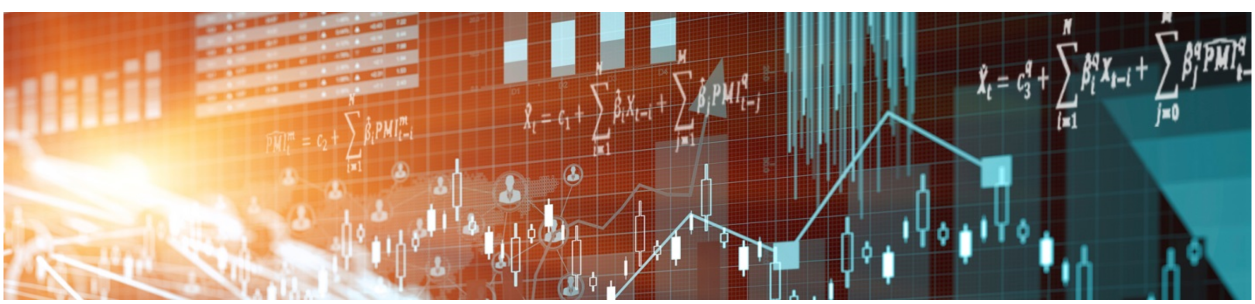

by Walter Steingress

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The Causal Impact of Migration on US Trade: Evidence from Political Refugees

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Abstract

Immigrants can increase international trade by shifting preferences towards the goods of their country of origin and by reducing bilateral transaction costs. Using geographical variation across U.S. states for the period 2008 to 2013, I estimate the respective causal impact of immigrants on U.S. exports and imports. I address endogeneity and reverse causality by exploiting the exogenous allocation of political refugees within the U.S. refugee resettlement program that prevents immigrants from choosing the destination location. I find that a 10 percent increase in recent immigrants to a U.S. state raises imports from those immigrants' country of origin by 1.2 percent and exports by 0.8 percent.

Bank topics: International topics; Regional economic developments
JEL codes: F14, F22, J61

Résumé

Les immigrants peuvent accroître les échanges internationaux en ce qu'ils accordent plus d'importance aux biens de leur pays d'origine et réduisent le coût des transactions bilatérales. Nous nous appuyons sur les variations géographiques entre les États américains de 2008 à 2013 pour estimer l'effet causal des immigrants sur les exportations et importations américaines. Nous contournons le problème de l'endogénéité et de la causalité inversée en nous servant de la répartition exogène des réfugiés politiques établie par le programme américain de réinstallation des réfugiés. Dans ce programme, les refugiés ne peuvent pas choisir leur destination d'accueil. Nous constatons qu'une augmentation de 10 % des nouveaux arrivants dans un État américain entraîne une hausse de 1,2% des importations provenant de leur pays d'origine et un accroissement de 0,8 % des exportations vers ce pays.

Sujets : Questions internationales; Évolution économique régionale
Codes JEL : F14, F22, J61

Non-technical summary

International migrants can influence international trade flows via two distinct mechanisms. First, migrants shift preferences towards the goods of their country of origin, thus generating demand for imports of those goods by their host country. Second, migrants reduce transaction costs between countries, either by holding information about relevant market characteristics or by attenuating frictions because of imperfect contract enforcement. While both mechanisms have trade-enhancing effects, only the cost-reducing channel is welfare improving in both countries.

In this paper, we estimate the impact of recent immigrants on U.S. imports and exports using geographical variations across U.S. states for the period 2008 to 2013. We follow a gravity approach and regress the log of bilateral imports and exports on the log of immigrants who entered the U.S. within the past 5 years. To address reverse causality, and more broadly endogeneity, we analyze the exogenous allocation of refugees within the U.S. refugee resettlement program. Within this program, the placement decision of refugees is taken by resettlement agencies, preventing refugees from choosing where to settle in the U.S. In a two-step Instrumental Variables (IV) estimation procedure, we first show that the number of resettled refugees is an exogenous predictor for the location decision of recent immigrants. In the second step, we use the predicted number of recent immigrants to causally estimate the impact of recent immigrants on international trade. This approach insulates our results from endogeneity concerns, where immigrants from a given country are likely to settle in states with the best trading opportunities to their country of origin. The resulting empirical estimates imply that a 10 percent increase in recent immigrants to a given U.S. state raises imports from those immigrants' country of origin by 1.1 percent and exports by 0.8 percent.

To shed light on the underlying mechanism at play, we borrow from the gravity literature and group immigrants according to common factors, i.e., sharing a common legal origin, having a common official language and sharing a common border. The idea is that migrants from countries that use the same language act as an information bridge because they can more easily understand the economic and cultural particularities of the related country. Immigrants who share a common legal origin may have knowledge about the legal framework and institutions and therefore help to overcome imperfect enforcement of contracts. Preferences for goods might be more similar across countries if the respective countries share a common border. Our empirical evidence supports these hypotheses. We find that immigrants from countries that share a common legal origin increase exports between their state of residence and the country-origin (transaction cost channel). On the other hand, sharing an official language and having a common border increases bilateral imports (preference channel).

Overall, these results suggest that immigrant networks play an important role in promoting trade across countries. By providing information on market conditions in both countries, the country of origin as well as the country of destination, they reduce transaction/trade costs for importers and exporters. As a result, consumers can purchase cheaper goods from abroad, and industries are more competitive in export markets.

1 Introduction

Work on individual attitudes shows that public opinion is not favorable to increases in the number of immigrants; see Mayda (2006) for further details. Yet increasing evidence in the literature shows a range of beneficial effects for the destination country. An important channel through which migration might increase the welfare of the host country is international trade. If migrants reduce transaction costs between countries, either by holding information about relevant market characteristics or by attenuating frictions because of imperfect contract enforcement, gains from trade are realized. Alternatively, if migrants shift preferences towards the goods of their country of origin, consumers in the host country may benefit from more consumption variety.[1]

This paper studies the impact of immigrants on imports and exports by exploiting geographical variation across U.S. states for the period 2008 to 2013. I follow a gravity approach and regress the log of exports and imports, respectively, on the log of recent immigrants. To estimate a causal relationship, I use variation in the number of immigrants driven by the exogenous allocation of political refugees within the U.S. refugee resettlement program. I find evidence of a significantly positive effect of immigrants on U.S. trade.

My analysis differs from previous studies in two respects. First, I focus on regional variation in immigration and trade *within* the United States over time. This approach minimizes the concern that the estimates are driven by the positive correlation between trade and migration policies, given that the latter are set at the national level. Second, and more importantly, I address endogeneity by focusing on exogenous shocks to immigration. Endogeneity arises because immigrants' decisions regarding settlement within the United States are likely to be correlated with several variables – such as income, employment opportunities and preferences – which in turn are correlated with trade (Borjas (1999)). An additional source of endogeneity arises in the form of reverse causality, i.e., immigrants from a given country are likely to settle in states that trade a lot with their country of origin. To address these issues, I estimate an IV specification, where I use the exogenous allocation of political refugees across states within the U.S. refugee resettlement program. The IV approach removes the endogenous component of migration decisions whereby individuals might move to those regions with the best trading opportunities. To establish the causal relationship, my analysis takes advantage of the fact that political refugees to the United States are exogenously allocated across locations once I control for time-varying state and time-varying country of origin fixed effects.[2] The main benefit of this approach compared with the existing literature is that it generates exogenous variation for many countries of origin. This variation is key in shedding new light on the channels through which migrants increase trade (transaction cost channel versus preference channel).

Starting with the pioneering work of Gould (1994), there exists ample empirical literature that argues that immigrants increase trade across international borders. Gould (1994) studies the effect of migration on aggregate U.S. exports and imports for the years 1970 to 1986. He estimates a gravity model of trade on migration and finds evidence of a strong positive relationship. Many authors follow Gould and study immigration into a single country; for example, Head and Ries (1998) examine Canada, and Girma and Yu (2002) examine the United Kingdom. As mentio-

[1] However, if imports crowd out domestic production, then welfare losses from trade may be realized.

[2] Other papers in the migration literature that use placement policies of refugees to obtain identification focus on labor market outcomes: see Beaman (2012) for evidence from the United States as well as Damm (2009) and Foged and Peri (2016) for evidence from Denmark.

ned above, a potential concern with aggregate-level analyses is the correlation of migration and trade policies. More recent studies exploit the regional distribution of immigrants and look at the bilateral trade relationship between U.S. regions and foreign countries, such as Bardhan and Guhathakurta (2004), Dunlevy (2006) and Parsons and Vézina (2016). While these studies focus exclusively on U.S. exports, this paper presents evidence on both U.S. exports and imports at the state level. More importantly, this paper puts greater emphasis on the relevance of the underlying channels and on the identification of causal effects, which are key for deriving meaningful policy implications.

In a recent literature review, Felbermayr et al. (2015) argue that the main concern for the identification of the causal effect of immigration is reverse causality. To deal with this issue, authors have adopted different approaches. Drawing upon the seminal work of Card (2001), several papers (see, for example Peri and Requena-Silvente (2010) and Bratti et al. (2014)) instrument changes in immigrants at the sub-national level based on the past distribution of immigrants across regions (Spain and Italy, respectively) by country of origin and on the growth in their aggregate inflows. Still, historical migrant stocks can have direct effects on trade even many years after their arrival, therefore violating the exclusion restriction.[3] Indeed, the estimated elasticity of immigrants with respect to trade flows using a Card instrument is significantly higher than the same elasticity using refugees.

There are two recent contributions to the literature, Parsons and Vézina (2016) and Cohen et al. (2017), which use a natural experiment for identification of a causal link between trade and migration. Both papers find a positive impact of migration on trade for a specific immigrant population. In Parsons and Vézina (2016) the identification is based on the location choice of the Vietnamese boat people across U.S. states and a concurrent trade embargo on Vietnam. Cohen et al. (2017) use the World War II Japanese internment camps to instrument for the location of the Japanese population in the United States. Instead, this paper uses the exogenous allocation of migrants from many countries of origin across different locations in the U.S. The resulting panel structure strengthens identification by controlling for time-varying origin and destination fixed effects. The results show that in the absence of these control variables, the estimated effects of immigrants are magnified. By borrowing from the gravity literature (e.g., Head and Mayer (2014)), the multi-country approach also generates new evidence on the underlying channels. Specifically, the results show that immigrants from countries that share a common legal origin reduce transaction costs for exporters, while sharing an official language and having a common border increases the demand for imports.

The rest of the paper is organized as follows. Section 2 covers the data and the summary statistics. Section 3 describes the identification strategy as well as the details of the political refugees program, which allow me to establish causality in the estimated impact of trade on migration. Section 4 discusses the OLS and IV results. Section 5 discusses the robustness of my findings and Section 6 sheds light on different channels through which immigrants can affect trade flows. Section 7 concludes.

[3] An interesting recent contribution that tackles endogeneity is Burchardi et al. (2016). The authors use the ethnic composition from the 19th century onward across U.S. states to predict the current immigrant population and their impact on foreign direct investment.

2 Data and summary statistics

My empirical analysis uses variation in exports, imports and the number of recent immigrants across U.S. states, countries of origin and time. Before presenting the estimating equation strategy in detail, we describe the data and the summary statistics.

2.1 Trade data

The trade data, which include all sectors of the economy, come from the U.S. Census Bureau and are based on the transaction level data set. Data on imports and exports are available for the years 2008 to 2013, while for earlier years (2000 to 2008) only export data exist. The data contain trade flows at the 3-digit NAICS industry level to 183 trading partners. The import value of shipments is defined as the net selling value exclusive of freight charges and excise taxes. The export value is the free-on-board value.

Note that, I focus on the manufacturing sector. The reason is that the state export data series uses the "Origin of Movement" definition, which implies that state export data do not necessarily reflect the state where the goods for export were produced, see Cassey (2009). The Census states that these limitations are particularly noticeable for agricultural and non-manufacturing shipments. On the import side, the state may not reflect the final location of consumption because the state, where the entry documentation was filed, may be a storage or distribution point. From there, shipments may later be distributed to another location in another state. To circumvent this problem I constructed in an earlier draft an import demand model and found similar results, see Steingress (2015). In addition, as a robustness check, I re-estimate the model excluding California and New York from the sample. These states contain the major ports in the United States and may therefore bias the estimates. However, the results remain unaffected by this change.

2.2 Trade cost data

To calculate the bilateral distances used in the trade cost function, I adopt the procedure used by Mayer and Zignago (2011). d_{ij} is the population-weighted distance between the state i and the country j measured in kilometers. Like Mayer and Zignago (2011), I calculate the geometric average of the population-weighted distance between the 15 most populated cities by country and by U.S. state. All data on population, latitudes and longitudes are from the free World Cities Database.[4]

2.3 Immigration data

The measures of the immigrant population are based on data from the American Community Survey (ACS) compiled by Ruggles et al. (2004) for the years 2008 to 2013 and include 1% of the population. The main explanatory variable in my regressions, i.e., recent immigrants, is defined as the number of immigrants who immigrated up to 5 years prior to the census year. I also control for pre-existing immigrants, which are all those immigrants who live in the respective state in the

[4]The database is freely available at https://www.maxmind.com/en/free-world-cities-database.

census year and immigrated more than five years ago to the United States. I follow Borjas (2003) and focus only on immigrants who are wage-earning civilian employees between the ages 18-64. I then aggregate the number of immigrants at the state level using the census sampling weight. Figure 2 plots the share of recent immigrants over the period 2008 to 2013 with respect to the state population in 2013.

In the identification strategy, I use political refugees as an instrument for immigration. Data on the number of refugees per U.S. state come from the Office of Refugee Resettlement (ORR). The ORR provides yearly refugee arrival data sorted by country of origin, state of initial resettlement and information on whether the refugee has family members or friends living in the United States. Each fiscal year, the U.S. government sets an overall refugee admissions limit based on regional allocations. The limit of refugee admissions varies from year to year depending on the Congress and the geopolitical situation.[5]

For the purpose of this study, we focus only on "no-U.S.-tie" refugees, i.e., refugees who did not have family members living in the United States prior to their arrival. Figure 1 plots the total number of "no-U.S.-tie" refugees for each state over the period 2003 to 2013. In order to make the refugee data compatible with the immigration data, I add up the refugee data per country of origin and state for all five years prior to the years 2008 and 2013. Table 2 contains the total number of "no-U.S.-tie" refugees per country of origin who arrived in the United States during the sample period.

2.4 Summary statistics

Table 1 presents the summary statistics for each of the sample years, 2008 and 2013. Rows 1 to 5 show the total number of immigrants, the number of newly arrived immigrants (in the past five years), their share in the immigrant population and the trade statistics for each census year. The total number of immigrants in the United States was 35.2 million in 2008 and 38.8 million in 2013. In terms of recent immigrants, immigration to the United States is very diverse. In the years between 2003 and 2008, 4.6 million migrants emigrated from 156 countries to the United States, which represents around 12% of the total population of immigrants in the United States. In the period from 2008 to 2013, the immigrant pattern changed only slightly with 4.4 million people with 159 different nationalities. During the same period, the total value of manufactured imported (exported) goods increased from USD 1.7 trillion (USD 1.2 trillion) in 2008 to USD 1.8 trillion (USD 1.5 trillion) in 2013.

Turning our attention to refugees, between 2004 and 2008, 98,000 "no-U.S.-tie" refugees from 73 countries settled across the United States (see lower part of Table 1). These numbers increase to 131,000 "no-U.S.-tie" refugees from 74 countries for the period between 2008 and 2013. The share of refugees within recent immigration to the United States was 2.1% in 2008 and 2.9% in 2013. In terms of export value, 24% of U.S. exports in 2013 were going to the refugees' countries of origin, while 22% of U.S. imports came from these countries.

[5]See U.S. Department of Homeland Security. 2005 Yearbook of Immigration Statistics. Washington, DC: U.S. Department of Homeland Security. Available at http://www.dhs.gov/xlibrary/assets/statistics/yearbook/2005/OIS_2005_Yearbook.pdf.

3 Identification strategy

Once migrants settle in a U.S. state, international trade between the state of residence and their home countries is likely to increase. This effect may take place through the following channels. First, migrants might provide information that reduces transaction costs. Second, trade might increase simply because migrants have a preference for the goods from their country of origin. Before discussing the channels in detail, I present the identification strategy.

3.1 Regression specification

To analyze the impact of immigration on trade flows, I follow the literature and employ the gravity equation. (See Gould (1994) for the seminal contribution.) The gravity equation relates trade flows between state i and country j to the relative sizes of the participating economies (which in my specification are captured by fixed effects). The log of the trade flow of state i from country j for the period t, X_{ijt}, is given by

$$\log(X_{ijt}) = \beta_1 \log(Imm_{ijt}) + \beta_2 \log d_{ij} + \beta_3 \log PImm_{ijt} + f_{jt} + f_{it} + \varepsilon_{ijt}. \tag{1}$$

The regressor Imm_{ijt} indicates the number of foreign-born residents in state i who immigrated from country j in any of the five years prior to time t. The other regressors are the log of the weighted distance, d_{ij}, between the capital of state i and the capital in country j, measured in kilometers. $PImm_{ijt}$ is the number of foreign-born residents in state i who immigrated from country j more than five years prior to time t.[6] In addition, I introduce time-varying state and country fixed effects, f_{it} and f_{jt} respectively. The coefficient of interest is β_1. If $\beta_1 > 0$, then the presence of recent immigration increases trade. Equation 1 will be my main regression specification.

In terms of identification of β_1, it is important to control for geography, since both migration and trade are correlated with distance; see Head and Mayer (2014) for further evidence. Specifically, distance is negatively correlated with both migration and trade, as states both import relatively more goods and receive relatively more migrants from neighboring countries. Neglecting these effects would introduce an omitted variable bias.

Note that, by analyzing the trade migration relationship across U.S. states, I directly address the criticism of Hanson (2010) with respect to the earlier literature. He argues that "it is difficult to draw causal inference from results based on international trading and migration patterns, since immigration may be correlated with unobserved factors that also affect trade, such as the trading partners' cultural similarity or bilateral economic policies (e.g., preferential trade policies or investment treaties that raise the return to both migration and trade)." Trade policies and investment treaties are negotiated at the federal level and are thus controlled for by fixed effects specific to the country of origin, f_{jt}. These fixed effects also control for any determinants of trade that are common to all U.S. states. For example, if a country of origin experiences a positive productivity shock, trade might increase since all U.S. states will face lower import prices from this country and emigration might decrease because of better employment opportunities.

A further concern for the identification of the parameter β_1 is the presence of time-varying

[6] Note that Imm_{ijt} and $PImm_{ijt}$ represent the change in the stock of immigrants in the last five years and the preceding period. Thus, my specification regresses trade flows on migration flows.

state-specific characteristics that may be correlated with trade flows as well as immigration. One such candidate is, for instance, economies of agglomeration, i.e., more immigrants are likely to settle in larger states and those states have higher demand for imported goods. For this reason, I include time-varying state-fixed effects, f_{it}, that control for any state-specific effects, such as local demand and income shocks, which are common to all migrants and vary over time.

By looking at regional variations and including state and country time-varying fixed effects, I follow the recent literature (see Bardhan and Guhathakurta (2004) and Dunlevy (2006)). The key difference of this paper with respect to the literature is that I follow a new approach to resolve endogeneity. Endogeneity arises because immigrants' decisions regarding settlement within the United States is likely to be correlated with several variables, such as income, employment opportunities and/or preferences, which in turn are correlated with trade (Borjas (1999)). An additional source of endogeneity arises in the form of reverse causality, i.e., immigrants from a given country of origin are likely to go to states that trade a lot with that country.

3.2 Refugees

To tackle reverse causality as well as endogeneity more generally, I focus on exogenous shocks to migration, i.e., refugees. Gould (1994) argues that immigration occurs before the onset of trade and is therefore predetermined. This is true if the migration decision is based on current or past levels of trade. However, if the migration decision is forward-looking and dependent on expected future trade (for example, people emigrate in order to take advantage of information arbitrage, which leads to trade), past immigration is endogenous. As a result, the number of immigrants and the level of trade are jointly determined. Generally, migration is endogenous due to omitted variables such as income, employment opportunities and/or preferences that are correlated with trade. Thus, the OLS estimates of both the number of recent and previous immigrants in equation 1 are likely to be inconsistent. To address this concern, we implement a two-stage least squares (2SLS) instrumental variable estimation strategy and instrument the number of *recent* immigrants by the number of refugee arrivals with no family members in the U.S. (the so-called "no-U.S.-tie" cases). We argue that the placement upon arrival of no-U.S.-tie cases is decided by resettlement agencies and not by the refugee. Also, the placement does not depend on the number of immigrants already in the state (I provide a formal test below). Thus, we can isolate the effect of recent immigrants from the effect of previous immigrants and exclude the latter from equation 1. Next, we describe the U.S. refugee program followed by the IV estimation strategy.

Refugees are people who have fled their home country and cannot return because they have a well-founded fear of persecution based on religion, race, nationality, political opinion or membership in a particular social group (Immigration and Nationality Act, Sect. 101[a][42]). Each fiscal year, the President of the United States sends a proposal to Congress for the maximum number of refugees to be admitted. After a congressional debate, the overall refugee admissions limit for the upcoming fiscal year is set. The limit varies from year to year. For example, over the period 2003 to 2013, 609,208 refugees were admitted to the United States, primarily from Myanmar (108,608), Iraq (93,514) and Bhutan (69,821).[7]

[7]The United States has a special concern for a designated group of refugees related to religious activists or minorities in certain countries. This group includes Jews and Christians in the former Soviet Union with close family ties in the United States, civil rights activists from Cuba, political refugees from Myanmar, Iranian members

One of the main endogeneity concerns in the migration-trade literature is that immigrants choose where to locate, and this decision might be correlated with trade. For refugees, this is not the case for the following reasons. In order to become a refugee, an individual presents his case before an Immigration and Naturalization Service officer in one of the U.S. refugee-processing centers around the world *outside* Union witthe United States.[8] Upon receiving the application, the Immigration officer reviews the case and decides whether the applicant fulfills the necessary conditions.[9] Within the application process, the applicants are asked to provide information on whether they have family and friends already living in the United States. If this is the case, the ORR tries to allocate them close to their family members. For this reason, this study focuses exclusively on the allocation of "no-U.S.-tie" refugee application cases, i.e., those where the political refugee has no family ties or friends in the United States. The placement of refugees without family ties is solely decided by the resettlement agency and not by the refugee. Table 2 shows the number of "no-U.S.-tie" refugees per country of origin.

A potential threat to the identification of a causal effect is strategic placement by resettlement agencies. For example, a given state may have greater opportunities for trade with a specific origin country, hence the resettlement agency may send refugees from that country to that city. The allocation of "no-U.S.-tie" refugees is handled by the Bureau of Population, Refugees and Migration (PRM), part of the State Department. The PRM takes care of the overseas processing and transportation to the United States. Upon the refugees' arrival in the United States, the PRM allocates the "no-U.S.-tie" refugees to voluntary resettlement agencies (Volag), who place them in one of their regional offices in the U.S. and provide social services that foster their integration. The decision on where to relocate the "no-U.S.-tie" refugee depends on the characteristics of each refugee, such as his or her medical condition and demographic information.[10] With this information, the main objective of the voluntary agencies is to place "no-U.S.-tie" refugees into locations where they can quickly integrate into American society. The aim of the PRM is to place "no-U.S.-tie" refugees not in states where there are people from the same country of origin to avoid the concentration of ethnic groups, as was the case for Cuban refugees in Florida in the 1960s and 1970s, see Kerwin (2012).

The PRM encourages Volags to help refugees to find employment quickly, as this reduces the economic costs for their social services (such as housing, furniture, access to health care as well as English-language courses) and helps refugees integrate faster into American society (David (2004)). Thus, the placement decision is correlated with economic opportunities in the state. For this reason, I include time-varying state fixed effects, which control for the potential correlation between a state's capacity to host refugees and the level of its income as well as trade. Figure 1 plots the resulting distribution of "no-U.S.-tie" refugees as a share of the local population across U.S. states over the whole sample period, i.e., 2008 to 2013. In terms of absolute numbers, the state that took in the most refugees was Texas with 35,301 refugees. New York ranked second with 18,691 refugees, followed by Arizona with 18,673 refugees.

of certain religious minorities and Sudanese Darfurians.

[8]Asylum seekers who claim refugee status within the United States are not included in the sample.

[9]The person must either be in imminent danger and identified as such by the United Nations High Commissioner for Refugees (UNHCR), a United States Embassy, or a designated non-governmental organization (NGO), or belong to a group of special humanitarian concern identified by the U.S. refugee program.

[10]For example, refugees with HIV are sent only to particular offices that specialize in such cases (see Beaman (2012)).

Turning to the empirical implementation, my identification is based on a two-stage least squares panel. In the first stage, I regress the log of the number of immigrants (Imm_{ijt}) who arrived in the five years prior to year t from country j and settled in state i on the log of the number of "no-U.S.-tie" refugees who arrived in the past five years (Ref_{ijt}). The second stage uses the predicted number of immigrants from the first stage as the identifying variable in equation 1. The resulting first-stage equation is given by

$$\log(Imm_{ijt}) = \alpha_0 + \alpha_1 \log Ref_{ijt} + \alpha_2 \log d_{ij} + f_{it} + f_{jt} + \epsilon_{ijt} \tag{2}$$

where f_{it} and f_{jt} are country of origin-year and state-year pair fixed effects. The state-year effects, f_{it}, control for any state-specific change in the allocation of refugees over time that is common to all countries of origin. Country-year fixed effects, f_{jt}, control for country of origin specific effects that are common to all states in the United States, such as the nature of the conflict that forced people to emigrate or any other macroeconomic condition in the country of origin. Note that in the robustness section, we address additional concerns on the validity of the instrument. In particular, we include bilateral (dyadic) fixed effects, f_{ij}, in equation 1 and 2. This more demanding specification addresses possible concerns that time-constant factors might allocate refugees to places that provide better trading opportunities with their country of origin.

The empirical model specified in equation 1 partitions the effect of immigrants on international trade into two categories: (1) recent immigrants and (2) past immigrants. However, in our 2SLS IV approach we use the number of "no-U.S.-tie" refugees to instrument *recent* immigrants and exclude the previous number of immigrants as an explanatory variable, see equation 2. The reason is that, based on similar arguments for recent immigrants, the location decision of past immigrants might be endogenous to trade flows. However, simply excluding the past number of immigrants is not sufficient because the allocation of refugees could depend on existing immigrant communities from the same country of origin. If this is the case, the refugee allocation mechanism would not be exogenous to trade flows because past immigrants are positively correlated with trade. In addition to past immigrants, we also check whether past bilateral trade is related to the allocation of future refugees. Since trade flows are persistent, a positive correlation between past trade flows and the allocation of refugees would suggest that the exclusion restriction of strict exogeneity does not hold. To shed further light on the validity of refugees as an instrument, we run the following regression:

$$\log(Ref_{ijt}) = \beta_0 + \beta_1 \log X_{ijt-1} + \beta_2 \log d_{ij} + \beta_3 \log PImm_{ijt} + f_{it} + f_{jt} + \epsilon_{ijt} \tag{3}$$

where the X_{ijt-1} represents exports or imports the year before the accumulated five-year refugee inflow in year t. Note that for exports, we do have two years of observation (2003 and 2008), while for imports the first year of observation is 2008, implying that, for imports, we can analyze only the refugee allocation in 2013. In addition to distance, trade flows and a set of fixed effects, I also include the number of past immigrants $PImm_{ijt}$ to test whether refugees are placed close to the existing communities from their country of origin. Table 3 shows that this is not the case.

The only remaining identification concern is that resettlement agencies base their allocation decisions on real-time information on trade opportunities between a state and the country of origin. This is unlikely to be the case. As Beaman (2012) notes, Volag employees in charge of placement have stated that the effectiveness of strategic decision-making is limited. Placement officers never know when a refugee who is assigned to the Volag by the State Department will actually be allowed to travel. For example, consider the refugee allocation in 2005. In some cases the individuals were granted refugee status in 2001, but arrived in 2005 because of delays associated with heightened post-September 11, 2001 security requirements. These significant time delays make exploiting placement with respect to country-specific trade shocks extremely difficult.

4 Results

This section shows empirically that the migration channel is important for increasing exports and imports across U.S. states. I start by a simple OLS regression of equation 1. The results are presented in Table 4 and show the estimation results for all countries that sent "no-U.S.-tie" refugees in the period 2003 to 2013 to the United States. Overall, I find a positive and significant migrant effect on trade across all specifications. The baseline OLS results in columns 1 and 4 suggest that a 10% increase in the number of recent migrants raises exports by 1.1% and imports by 1.4%.

Note that the magnitudes of the migration elasticities (0.11 for exports and 0.14 for imports) are significantly lower than the values found in the literature. Looking at the national level of U.S. imports for the period 1870 to 1910, Dunlevy and Hutchinson (1999) found an elasticity of 0.28. Similarly, Wagner et al. (2002) found an elasticity of 0.28 using a variation across Canadian provinces, and Bratti et al. (2014) estimated an elasticity of 0.32 for imports using Italian data. Briant et al. (2014) on the other hand found an elasticity of 0.12, which is very similar to our results, when looking at French import data. For exports, Bandyopadhyay et al. (2008) estimated an elasticity of 0.14 at the U.S. state level, slightly higher than 0.11 in Table 4. The main difference with respect to the high migration elasticities found in the literature is that column (1) and (4) focus on newly arrived immigrants, whereas the previous paper focused on the total stock of immigrants.[11]

Next, we estimate the full model specified in equation 1 and include the number of previous immigrants as an additional explanatory variable. I follow two distinct approaches. In columns (2) and (5), I add the number of immigrants who arrived six or more years ago and reported to be living in state i in the year t the Census was conducted. In columns (3) and (6) I use the number of immigrants before the first year of trade is observed. More precisely, I include the number of immigrants who arrived before 2003 and reported to be living in state i in 2008. Previous immigration will be correlated with actual immigration if recent immigrants prefer to settle in states where there is a large pre-existing community. However, the second specification is less demanding because it is based on the distribution of immigrants in the year 2003 and does not consider the year-to-year changes in the migration patterns of older immigrants.

Table 4 presents the results for the full sample. When I include pre-existing immigrants, the migration elasticity of recent immigrants decreases for both exports and imports. More specifically, the coefficient of exports decreases from 0.11 to 0.09 and for imports from 0.14 to 0.11.[12] As expected, omitting the number of previous immigrants increases the migration elasticity because

[11]One exception is Peri and Requena-Silvente (2010), who exploit the fact that the number of immigrants was very low in the first year of their study (Spain in 1993). They examine the quick, intense arrivals of immigrants in the next 15 years, similar to the definition of recent immigrants in this paper. Their estimated immigration elasticities range from 0.05 and 0.09 for exports and from 0.02 to 0.05 for imports.

[12]The results are similar if I use the immigrant distribution in the year 2003.

the immigration decision of recent immigrants is positively correlated with the presence of previous immigrants. Also note that the combined effect of previous and recent immigrants is now comparable in magnitude to values found in the literature. Thus, the positive correlation between trade and immigrants depends partly on the existing trade network established by previous immigrants.

As discussed, the OLS results might be biased: for example, if people immigrate in order to take advantage of trading opportunities, i.e., trade causes immigration. To infer a causal link between migration and trade, I instrument the number of recent immigrants by political refugees using a 2SLS approach. The first-stage results are presented in column (1) of Table 5. The number of refugees is positive and significantly correlated with the number of immigrants.[13] To confirm the validity of the IV regressions, I include the Kleibergen-Paap F statistics at the bottom of the tables. The Kleibergen-Paap F statistic provides an indication of the significance of the instrument. If the instrument is only weakly correlated with the endogenous regressor, the IV estimator is not valid. To assess the weakness of the instrument, I need to compare these F statistics with the Stock-Yogo critical values for the Cragg-Donald F-statistic with one endogenous regressor (Stock and Yogo (2002)). As a rule of thumb, an F-statistic above 10 indicates that the IV is acceptable.

Column (2) in Table 5 contains the second-stage IV results for exports and column (3) for imports. The coefficient on the number of recent immigrants is significantly positive, with an elasticity slightly lower than in the OLS regression for all specifications. However, performing a Hausman test does not reveal any significant difference in the coefficients. This suggests that sorting of immigrants towards locations that trade a lot with their country of origin plays only a negligible role. Overall, the results imply that a 10% increase in the number of immigrants in the past five years increases exports by 0.8% and imports by 1.0%.

5 Robustness

This section provides additional evidence that the positive effect of recent immigrants in international trade is robust. First, we compare our results with the literature and address potential biases introduced by studying just one country as in Parsons and Vézina (2016) or by following the Card approach as in Peri and Requena-Silvente (2010) and Bratti et al. (2014). Second, we account for issues related to zeros, i.e., the presence of trade flows but no immigrants and vice versa.

5.1 Comparison with alternative approaches in the literature

As mentioned in the Introduction, a related paper is Parsons and Vézina (2016). The authors base identification on the combination of an immigration shock – driven by the location of Vietnamese boat people across U.S. states – and a concurrent trade embargo. They use the cross-sectional variation in the share of Vietnamese immigrants and exports to Vietnam of the 50 U.S. states. They show that, after the end of the 1994 trade embargo, U.S. states with a higher share of Vietnamese immigrants exported significantly more to Vietnam. The key identification assumption is

[13]The estimated elasticity of 0.10 implies that doubling the refugee population increases the number of recent immigrants by 10%. To be more concrete, increasing the inflow of refugees in the five years between 2008 and 2013 from 150,000 to 300,000 would imply an "exogenous" increase of 200,000 new immigrants.

that the settlement choice of Vietnamese immigrants before the trade embargo is exogenous with respect to U.S. exports after the embargo. While Parsons and Vézina (2016) use Census data to identify Vietnamese refugees, my analysis is based on data from the Office of Refugee Resettlement (ORR) and considers only political refugees recognized by the State Department. Within this refugee resettlement program, immigrants cannot choose their settlement location; instead, the ORR allocates them across U.S. states.

The main differences with respect to Parsons and Vézina (2016) are the following. First, I focus on regional variation in both U.S. imports and exports, whereas they focus only on U.S. exports. Second, and more importantly, my analysis addresses endogeneity by exploiting the exogenous variation in migration not only across states but also over time and across countries of origin, i.e., in a *panel* structure. As a result, we observe migration flows from different countries and different time periods increasing the number of observations significantly compared with Parsons and Vezina's cross-section of 50 U.S. states. The resulting panel structure also allows us to include various fixed effects, like a state-year fixed effect that takes into account that California may experience an economic boom that draws in refugees and increase exports and imports versus all countries (i.e., an upward bias in the immigration elasticity).[14] Another important fixed effect is the country of origin-year fixed effect, which controls for size effects (i.e., more immigrants come from larger economies, which also have a higher trading volume with the United States) and potential bilateral economic policies that favor trade and migration between the two countries. Based on these arguments, we expect an upward bias in the immigration elasticity. To investigate the presence of these biases more formally, I re-estimate equation 1 (i) without any fixed effect, (ii) including a state-year fixed effect, (iii) including a country of origin-year fixed effect, (iv) including state-year and country of origin-year fixed effect (baseline specification) and, for completeness, (v) including all possible types of fixed effects. Note that I re-estimate the model using both OLS as well as 2SLS because the exclusion restriction of the IV specification requires the presence of state-year as well country of origin-year fixed effects. In the absence of these fixed effects, the direction of the bias is confounded by the refugee allocation mechanism, which complicates the analysis.

Table 6 shows the results. Consistent with our expectations, the presence of a state-year fixed effect or a country of origin-year fixed effect reduces the elasticity of recent immigrants with respect to trade, as seen by comparing columns (3) to (8) with (1) and (2). While columns (7) and (8) repeat the estimates of Tables 3 and 4, the results in columns (9) and (10) are based on an even more demanding specification, which includes dyadic (state-country of origin) fixed effects. These estimates are robust to any time-constant bilateral effects between a state and an immigrant's country of origin. The estimated coefficients are positive, significant and similar in magnitude to the coefficients obtained without dyadic fixed effects. However, there are signs of potential weak instruments (IV F-stat below 10), as the number of observations drops by half.

Overall, I see my approach as complementary to that of Parsons and Vézina (2016) and as a test of external validity in a multi-country, multi-period setting. Parsons and Vézina (2016) focus on a specific group of migrants at a given point in time. This paper uses data on refugees to the U.S. from all countries in the period of 2008 to 2013, leading to a more comprehensive sample in terms of countries of origin and allows us to investigate more deeply the underlying channels

[14] In Parsons and Vézina (2016), Vietnamese immigrants settled predominantly in coastal states in the western part of the U.S., which are naturally more open to trade and closer to Vietnam.

through which immigrants increase trade in Section 6.

A popular alternative approach to address endogeneity in the migration literature is based on Card's (2001) methodology. The idea is to instrument the current flow of recent immigrants by the share in the stock of past immigrants interacted with the aggregate growth of recent immigrants, see Peri and Requena-Silvente (2010) and Bratti et al. (2014), among others. However, historical migrant stocks could have established long-standing trade relationships, with direct effects on the current level of trade. Hence, Card-based instruments may violate the exclusion restriction and overestimate the effect of immigrants on trade. To assess whether there is a potential positive bias in comparison to using the allocation of refugees, I construct a Card instrument for recent immigrants as follows:

$$Imm_{ijt}^{Card} = sh_{ij2000} Imm_{jt} \tag{4}$$

where $sh_{ij2000} = Imm_{ij2000}/(\sum_i Imm_{ij2000})$ is the share of immigrants that arrived in the United States between 1996 and 2000 from country j and reported to live in state i in 2000. Imm_{jt} is the aggregate number of immigrants who arrived within five years prior to year t in the United States (note that t equals either 2008 or 2013). Next, we replace the number of refugees by the Card instrument in the first stage (equation 2) and re-estimate the model. The results in Table 7 show that the estimated immigration elasticities are significantly higher for both exports and imports, compared with the specification based on refugees in Table 5. Consistent with our prior, the results suggest that instruments based on Card can create an upward bias.

5.2 The presence of zeroes

An important issue is the presence of zeros. Given the fact that there are refugees from 80 different countries, the number of potential observations is 80x50x2, much higher than the actual 787 observations. As a first step, I count 5,813 observations with no trade flow and no immigrant flow in *both* sample periods. The bilateral (dyadic) fixed effects absorb these observations because there is no time-specific variation. With respect to observations that have no trade flow and no immigrant flow in one period and a positive trade or immigrant flow in the other period, I follow Peri and Requena-Silvente (2010) and add one so that the log of one equals zero. In the case of zero trade flows but positive immigration flows, I follow an alternative approach based on the Pseudo Poisson Maximum Likelihood (PPML) estimator of Silva and Tenreyro (2006). Columns (1) and (2) in Table 8 show the log plus one results, while columns (3) and (4) show the PPML ones. In both cases, the estimates imply that the positive effects of recent immigrants on trade are robust to the presence of zeroes.

Another concern relates to the fact that the countries of origin of refugees in the United States do not correspond to the major trading partners of the United States. As the summary statistics show, the combined value of exports and imports from the countries of origin of refugees represents only 24% of overall U.S. exports and 22% of overall U.S. imports in 2013. One paper that looks at cross-country differences in the effect of migration on trade is Egger et al. (2012). They show that the estimated coefficient of immigrants on trade flows decreases in the number of immigrants (i.e., smaller immigrant communities have larger effects on trade than larger ones). Since the average immigrant community from a refugee-sending country is smaller than the average immigrant communities across all countries of origin, I consider the estimated effect of recent immigrants in Table 5 as an upper bound.[15]

6 Channels

Note that in all regressions the effect of immigrants on imports is slightly higher than the effect on exports. The reason is that migrants affect exports and imports through different channels. In general, the literature distinguishes between two types of effects: the transaction cost channel and the preference channel. The transaction cost channel captures the idea that migrants either hold specific information about relevant market characteristics, attenuate frictions because of imperfect contract enforcement or reduce search costs. The preference channel implies that migrants have a preference for goods from their country of origin and demand those products in their host country.

In his seminal article, Rauch (1999) argued that the transmission of information through migrants' networks is particularly important for differentiated products since search costs are particularly high for this type of good. Immigrant networks can provide information and reduce these costs. On the other hand, homogeneous products are not subject to these information flows. For this reason, I separate exports and imports according to the Rauch classification and match each NAICS code to one of the three categories: differentiated products, reference priced products or organized exchange products. I find only a robust pro-trade effect for differentiated products (see Table 9) and suggest that both exports and imports are subject to transaction/search costs. These results are in line with the existing literature. The larger coefficient on imports suggests that the cost-reducing effect of immigrants is more pronounced for imports than for exports.

To shed further light on the underlying channels, I use proxies from the gravity literature (see Head and Mayer (2014)) and group immigrants according to common factors. For the transaction cost channel, I use the following variables: sharing a common legal origin and sharing a common official language. The idea is that migrants from countries that use the same language act as an information bridge because they can understand the economic and cultural particularities of the related country. Immigrants who share a common legal origin may have knowledge about the legal framework and institutions and therefore help to overcome imperfect enforcement of contracts. For the preference effect, I assume that preferences between countries are more similar to each other if the respective countries share a border. For example, the preferences for goods of French immigrants are more closely related to preferences of Italian immigrants than to those of Chinese immigrants. Of course, this is only an approximation. Sharing a border may also capture knowledge, as I expect that the French know more about the Italian economy than about the Chinese economy.

To examine whether a common factor (like sharing a language/border, etc.) increases trade flows, I include the number of related immigrants as an additional control variable. For example, suppose we want to explain trade between a U.S. state and France. In this case, the regression includes the trade of France with the respective U.S. state on the left-hand side and on the right-

[15]One way to support this argument is to compare the OLS estimates of equation 1 between a sample that includes all recent immigrants from all U.S. trading partners and a sample that includes only recent immigrants from countries that have refugees in the United States. The findings show that the estimated coefficients using the full sample are smaller than the estimated coefficients restricting to refugees' countries of origin. Detailed results are available upon request.

hand side the number of French immigrants and, as a separate variable, the number of other immigrants who also speak French (i.e., Belgian or Canadian immigrants). However, the number of immigrants who share the common factor may be endogenous. I address this endogeneity concern by first running the same first-stage regression as in equation 2. Then, I group the predicted number of recent immigrants $\widehat{M_{ijt}}$ from the first stage according to the common factor by summing over all countries except the one whose trade flows I want to explain. In particular, the number of immigrants that share a common official language with country j is calculated as

$$L\widehat{AM}_{ijt} = \sum_{l \neq j} \widehat{M_{ilt}} I_{ilt}$$

where I_{ijt} is an indicator function that equals 1 if the immigrants from l and j share a common language and are living in state i. Otherwise the indicator equals zero. I repeat the same calculation for each common factor. I then estimate the following second-stage regression:

$$\begin{aligned} \log(X_{ijt}) &= \beta_1 \log(\widehat{Imm}_{ijt}) + \beta_2 \log d_{ij} + \beta_3 b_{ij} + \beta_4 \log(B\widehat{OImm}_{-jit}) \\ &+ \beta_5 \log(L\widehat{AImm}_{-jit}) + \beta_6 \log(L\widehat{OImm}_{-jit}) + f_{jt} + f_{it} + \varepsilon_{ijt}. \end{aligned} \tag{5}$$

where the regressor $\widehat{Imm}_{ijt}$ indicates the predicted number of recent immigrants from country j, $B\widehat{OImm}_{-jit}$ the predicted number of recent immigrants who share a border with j, $L\widehat{AImm}_{-jit}$ the predicted number of recent immigrants who share a common language with j and $L\widehat{OImm}_{-jit}$ the predicted number of recent immigrants who share a common legal origin with j. Note that by all common factor variables, I exclude the immigrants from country j. In this way, I distinguish between the effects of recent immigrants on trade from the same country (β_1) and from related countries ($\beta_5 - \beta_7$).

Columns (1) and (2) in Table 10 show the results on the channels. Immigrants who share common legal origins increase only exports, whereas immigrants who share a common border or a common language increase imports of the related country. This evidence seems to suggest that immigrants increase the exports of a U.S. state by providing information about the legal system when contracting in the related country. Surprisingly, sharing a common language or a border does not increase exports, although it generates import demand. This finding is consistent with the idea that being close to each other and/or speaking the same language fosters cultural proximity and can manifest itself in similar preferences for goods.

Overall, the results in Table 10 imply that related immigrants of similar countries can act as complements to immigrants from the country of origin and increase trade flows. However, these immigrants cannot be arbitrarily related to the country under investigation. In a sensitivity analysis, I include all other immigrants living in the state as an additional regressor to the main specification in equation 1. Columns (3) and (4) in Table 10 show that other immigrants do not explain imports and exports.

7 Conclusion

Migrants carry information about the goods and market conditions of the country of origin as well as the country of destination. By providing this information, migrants reduce bilateral trade costs and increase trade between countries. As a result, consumers can purchase cheaper goods from abroad and industries are more competitive in export markets.

This paper focuses on the trade cost-reducing effect of migrants by looking at the relationship between immigration and imports as well as exports across U.S. states. Using the exogenous migration decision brought about by a quasi-natural experiment (political refugees), I establish the causal relationship between immigrants and trade. My results indicate a strong positive impact of migration on trade. I find that a 10% increase in immigrants raises exports by around 0.8% and imports by 1.1%.

Taking a broader perspective, immigrants may also have knowledge of production techniques used in their country of origin, which can increase the comparative advantage of industries in their country of destination; see Bahar and Rapoport (2016) for empirical evidence on this issue. All in all, these results suggest that the mobility of people between countries can serve as a key element in enhancing industrial productivity growth. However, more research is needed to assess the long-term impact of immigration on the economy.

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8 Figures

Figure 1: The share of refugees, who immigrated to the United States over the period from 2003 to 2013, with respect to the state population in 2013.

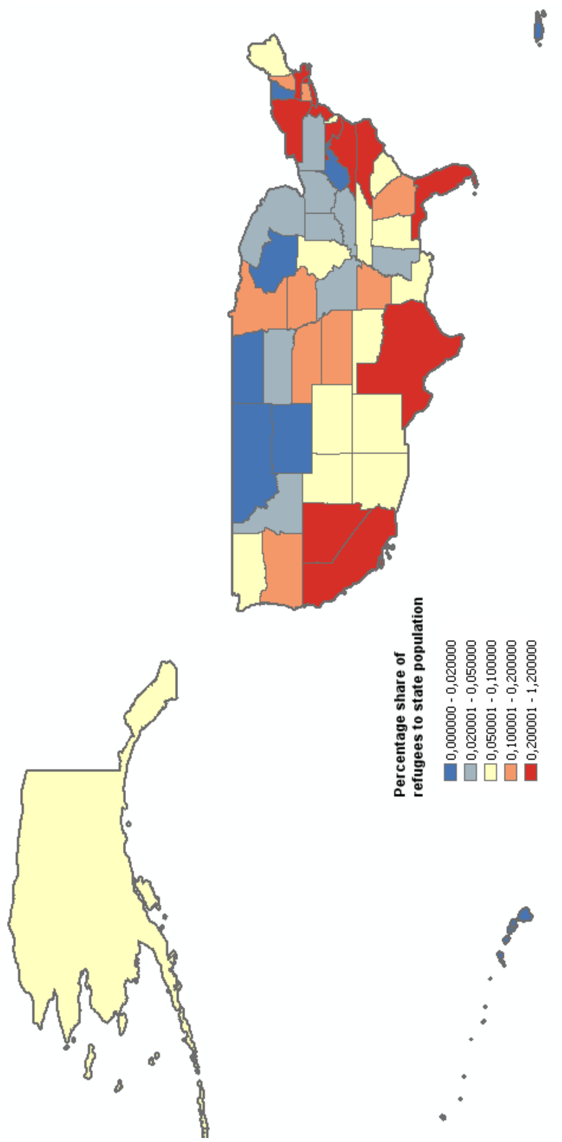
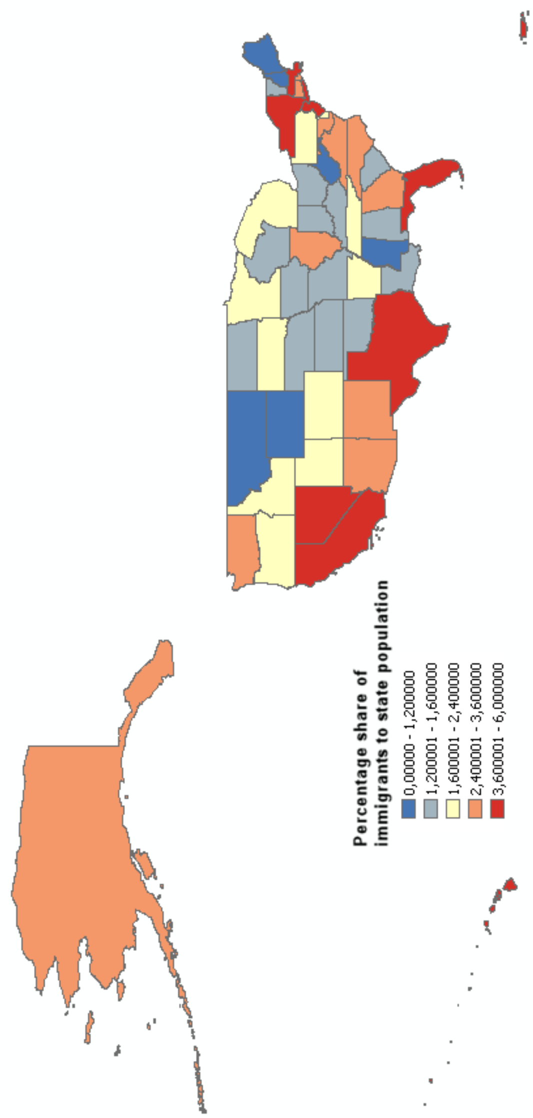


Figure 2: The share of recent immigrants, who migrated to the United States over the period 2003 to 2013, with respect to the state population in 2013.



9 Tables

Table 1: Summary Statistics

Year	2008	2013
Total		
No. of immigrants (thousands)	35200	38800
No. of immigrants last five years (thousands)	4663	4431
Share of immigrants (%)	13.2	11.4
Value of exports (Bil.$)	1211	1537
Value of imports (Bil.$)	1705	1809
Number nationalities among recent immigrants	156	159
Number of countries exporting to U.S.	152	154
Number of countries importing from U.S.	154	156
Refugees		
No. of "no-U.S.-tie" refugees last five years (thousands)	98	131
Share of no-U.S.-tie" refugees in immigrants last five years (%)	2.1	2.9
Value of exports (Bil.$) to countries of origin of "no-U.S.-tie" refugees	207	370
Value of imports (Bil.$) from countries of origin of "no-U.S.-tie" refugees	345	404
Share of U.S. exports going to countries of origin of "no-U.S.-tie" refugees	17.1	24.1
Share of U.S. imports from countries of origin of "no-U.S.-tie" refugees	20.2	22.3
Number nationalities among "no-U.S.-tie" refugees	73	74

Table 2: Total number of "no-U.S.-tie" refugees by country of origin for the years 2003 to 2013

Refugees	Country	Refugees	Country	Refugees	Country
63507	Myanmar	139	Laos	12	Georgia
28996	Somalia	128	Chad	11	Morocco
27614	Bhutan	128	Lebanon	10	Turkey
21618	Iraq	110	Jordan	10	Costa Rica
12951	Russia	107	Korea	9	Venezuela
9844	DR Congo	106	Ivory Coast	8	Philippines
9464	Burundi	91	Uganda	8	Thailand
8844	Liberia	90	Angola	6	Libya
8558	Cuba	76	Nigeria	6	Macedonia
7637	Sudan	62	Kuwait	5	Mozambique
7508	Eritrea	58	Cambodia	5	Guatemala
5013	Afghanistan	53	Zimbabwe	5	Algeria
3774	Ethiopia	48	Egypt	5	Zambia
2490	Viet Nam	47	Equatorial Guinea	4	Honduras
1470	Iran	44	Yemen	4	Tunisia
1238	Colombia	42	Cameroon	4	Bangladesh
1122	Rwanda	35	Ecuador	4	Namibia
1096	Israel	34	Gambia	3	Madagascar
936	Congo	30	Nepal	3	Mali
663	Yugoslavia	29	Moldova	2	Guinea-Bissau
660	Sierra Leone	25	Indonesia	2	Burkina Faso
643	Central African Republic	21	India	1	Panama
500	Pakistan	21	Kenya	1	Saudi Arabia
307	Togo	21	Gabon	1	Poland
262	Sri Lanka	18	Senegal	1	Antigua and Barbuda
215	Mauritania	15	Guinea	1	Oman
196	China	14	Haiti	1	Benin
161	Syria	13	Tanzania		

Table 3: Falsification test

Dependent Variable	log(No. "no-U.S.-tie" refugees five years)					
	(1)	(2)	(3)	(4)	(5)	(6)
log(Past Exports)	-0.00935	-0.00656	-0.000857			
	[0.0226]	[0.0247]	[0.0248]			
log(Past Imports)				0.00466	-0.0103	-0.0165
				[0.0201]	[0.0222]	[0.0234]
log(Distance)	-0.207	-0.0394	-0.107	-0.224	-0.126	-0.214
	[0.474]	[0.460]	[0.461]	[0.496]	[0.515]	[0.521]
log(No. Immigrants 6 or more years)		0.107			0.114	
		[0.321]			[0.422]	
log(No. Immigrants 2003)			0.0860			0.0924
			[0.317]			[0.433]
State-year FE	yes	yes	yes	yes	yes	yes
Country of origin-year FE	yes	yes	yes	yes	yes	yes
Observations	730	640	621	420	362	347
R-squared	0.831	0.856	0.856	0.823	0.843	0.845

Note: The dependent variable is the log of the number of refugees ("no-U.S.-tie" cases) who arrived in the past five years in state i from country j. Distances are weighted by population and in kilometers from the capital city in country i to the capital city in state j. All regressions include country of origin -as well as state-year fixed effects. Robust standard errors in parentheses (clustered by state): ***, **, * indicate the statistically significant difference from zero at the 1%, 5% and 10% levels respectively.

Table 4: Gravity equation with OLS estimates considering only immigrants from the same countries of origin as "no-U.S.-tie" refugees

Dependent variable	log(exports)			log(imports)		
	(1)	(2)	(3)	(4)	(5)	(6)
log(No. Immigrants five years)	0.108**	0.0931*	0.0953*	0.139*	0.111*	0.109**
	[0.0452]	[0.0498]	[0.0342]	[0.0774]	[0.0551]	[0.0517]
log(Distance)	-1.574***	-1.496***	-1.435***	-1.316	-1.231**	-1.217**
	[0.429]	[0.412]	[0.403]	[0.955]	[0.977]	[0.971]
log(No. Immigrants 6 or more years)		0.0500			0.134*	
		[0.0426]			[0.0671]	
log(No. Immigrants 2003)			0.129**			0.167**
			[0.0610]			[0.0920]
State-year FE	yes	yes	yes	yes	yes	yes
Country of origin-year FE	yes	yes	yes	yes	yes	yes
Observations	787	731	716	677	648	641
R-squared	0.82	0.83	0.83	0.66	0.67	0.68

Note: The dependent variables are the log of exports and the log of imports from country j to state i in period t. Distances are weighted by population and in kilometers from the capital city in country i to the capital city in state j. All regressions include country of origin -as well as state-year fixed effects. Robust standard errors in parentheses (clustered by state): ***, **, * indicate the statistically significant difference from zero at the 1%, 5% and 10% levels respectively.

Table 5: Gravity equation with IV estimates using “no-U.S.-tie” refugees as instrument

Dependent variable	log(No. Immigrants five years)	log(exports)	log(imports)
	1st stage	2nd stage	2nd stage
	(1)	(2)	(3)
log(No. Refugees five years)	0.153***		
	[0.0489]		
log(No. Immigrants five years)		0.0842**	0.103**
		[0.0386]	[0.0469]
log(Distance)	-2.078***	-2.325***	-1.450*
	[0.547]	[0.897]	[0.809]
State-year FE	yes	yes	yes
Country of origin-year FE	yes	yes	yes
Observations	789	787	677
R-squared	0.72	0.81	0.65
IV F-stat (Kleibergen-Paap)		15.44	14.21

Note: The number of immigrants is instrumented by the number of refugees (“no-U.S.-tie” cases) who arrived in the past five years in state i from country j. Distances are weighted by population and in kilometers from the capital city in country i to the capital city in state j. All regressions include country of origin -as well as state-year fixed effects. Robust standard errors in parentheses (clustered by state): ***, **, * indicate the statistically significant difference from zero at the 1%, 5% and 10% levels respectively.

Table 6: OLS and 2SLS estimates in various fixed effect specifications.

	log(Exp.)	log(Imp.)	log(Exp.)	log(Imp.)	log(Exp.)	log(Imp.)	log(Exp.)	log(Imp.)	log(Exp.)	log(Imp.)
OLS estimates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
log(No. Immigrants five years)	0.576***	0.585***	0.493***	0.624***	0.427***	0.332***	0.0842**	0.103**	0.0919*	0.123*
	[0.0442]	[0.0405]	[0.0416]	[0.0353]	[0.0563]	[0.0434]	[0.0386]	[0.0469]	[0.0526]	[0.0659]
log(Distance)	-1.029***	-0.343*	-1.320***	-0.378**	0.170	-0.694	-1.450*	-1.316		
	[0.175]	[0.170]	[0.156]	[0.181]	[0.995]	[0.716]	[0.809]	[0.955]		
State-year FE	no	no	yes	yes	no	no	yes	yes	yes	yes
Country of origin-year FE	no	no	no	no	yes	yes	yes	yes	yes	yes
State-country of origin FE	no	no	no	no	no	no	no	no	yes	yes
Observations	812	707	804	694	795	690	787	677	352	324
R-squared	0.22	0.27	0.38	0.34	0.69	0.59	0.82	0.66	0.99	0.99
2SLS estimates										
log(No. Immigrants five years)	5.797**	6.256*	4.102**	4.023**	1.114***	0.958***	0.0841**	0.117**	0.0801*	0.102*
	[2.475]	[3.532]	[2.082]	[2.264]	[0.409]	[0.319]	[0.0420]	[0.0502]	[0.0451]	[0.0517]
log(Distance)	-3.167**	-2.042	-3.492*	-3.016***	-0.241	-0.493	-2.325***	-1.450*		
	[1.739]	[1.791]	[1.821]	[1.138]	[1.041]	[1.210]	[0.897]	[0.809]		
State-year FE	no	no	yes	yes	no	no	yes	yes	yes	yes
Country of origin-year FE	no	no	no	no	yes	yes	yes	yes	yes	yes
State-country of origin FE	no	no	no	no	no	no	no	no	yes	yes
Observations	812	707	804	694	795	690	787	677	352	324
R-squared	0.21	0.26	0.30	0.33	0.67	0.54	0.81	0.65	0.98	0.97
IV F-stat (Kleibergen-Paap)	9.75	8.72	9.51	5.20	27.46	24.02	15.44	14.21	8.79	8.12

Note: The number of immigrants is instrumented by the number of refugees ("no-U.S.-tie" cases) who arrived in the past five years in state i from country j. Distances are weighted by population and in kilometers from the capital city in country i to the capital city in state j. Robust standard errors in parentheses (clustered by state): ***, **, * indicate the statistically significant difference from zero at the 1%, 5% and 10% levels respectively.

Table 7: Gravity equation with IV estimates based on Card instrument.

Dependent Variable	log(No. Immigrants five years)	log(Exports)	log(Imports)
	(1)	(2)	(3)
log(Card instrument)	0.511***		
	[0.0560]		
log(No. Immigrants five years)		0.181*	0.309*
		[0.0742]	[0.167]
log(Distance)	-0.665*	-1.376***	-1.030
	[0.373]	[0.437]	[1.138]
State-year FE	yes	yes	yes
Country of origin-year FE	yes	yes	yes
Observations	697	695	625
R-squared	0.74	0.81	0.65
IV F-stat (Kleibergen-Paap)		83.99	64.47

Note: The dependent variable in the first stage is the log of number of immigrants who arrived in the past five years in state i from country j. The dependent variables in the second stage are the log of exports and the log of imports from country j to state i in period t. Distances are weighted by population and in kilometers from the capital city in country i to the capital city in state j. All regressions include country of origin -as well as state-year fixed effects. Robust standard errors in parentheses (clustered by state): ***, **, * indicate the statistically significant difference from zero at the 1%, 5% and 10% levels respectively.

Table 8: Dealing with zero trade flows or zero immigrant flows: log+1 and IV estimates.

	Log+1		PPML	
Dependent Variable	log(Exports)	log(Imports)	log(Exports)	log(Imports)
	(1)	(2)	(3)	(4)
log(No. Immigrants five years)	0.128*	0.194*	0.0987*	0.128**
	[0.0654]	[0.107]	[0.0501]	[0.0618]
log(Distance)	-3.429***	-4.783*	-1.508***	-1.044
	[1.361]	[2.724]	[0.373]	[1.668]
State-year FE	yes	yes	yes	yes
Country of origin-year FE	yes	yes	yes	yes
Observations	3162	2187	814	809
R-squared	0.63	0.62	0.72	0.677
IV F-stat (Kleibergen-Paap)	10.44	10.47	12.79	11.65

Note: The number of immigrants is instrumented by the number of refugees ("no-U.S.-tie" cases) who arrived in the past five years in state i from country j. Distances are weighted by population and in kilometers from the capital city in country i to the capital city in state j. All regressions include country of origin -as well as state-year fixed effects. Robust standard errors in parentheses (clustered by state): ***, **, * indicate the statistically significant difference from zero at the 1%, 5% and 10% levels respectively.

Table 9: Gravity equation with IV estimates for exports and imports across Rauch product categories

	log(Differentiated goods)		log(Reference-priced goods)		log(Organized-exchange goods)	
Dependent Variable	log(Exports)	log(Imports)	log(Exports)2	log(Imports)	log(Exports)	log(Imports)
	(1)	(2)	(3)	(4)	(5)	(6)
log(No. Immigrants five years)	0.126**	0.162**	0.0430	0.172	-0.0429	-0.101
	[0.0525]	[0.0715]	[0.139]	[0.245]	[0.181]	[0.442]
log(Distance)	-1.429	-1.434**	-1.536	-0.603	-1.965***	-1.997*
	[1.116]	[1.251]	[2.823]	[5.550]	[0.664]	[1.132]
State-year FE	yes	yes	yes	yes	yes	yes
Country of origin - year FE	yes	yes	yes	yes	yes	yes
Observations	648	506	328	96	782	637
R-squared	0.66	0.59	0.54	0.72	0.79	0.61
IV F-stat (Kleibergen-Paap)	12.16	13.42	7.89	9.82	19.07	16.10

Note: The number of immigrants is instrumented by the number of refugees ("no-U.S.-tie" cases) who arrived in the past five years in state i from country j. Distances are weighted by population and in kilometers from the capital city in country i to the capital city in state j. All regressions include country of origin -as well as state-year fixed effects. Robust standard errors in parentheses (clustered by state): ***, **, * indicate the statistically significant difference from zero at the 1%, 5% and 10% levels respectively.

Table 10: Gravity equation IV estimates for channels effects using "no-U.S.-tie" refugees as instrument.

Dependent Variable	log(Exports)	log(Imports)	log(Exports)	log(Imports)
	(1)	(2)	(3)	(4)
log(Distance)	-2.720*	-2.618***	-3.039***	-2.386**
	[1.432]	[1.759]	[0.957]	[1.768]
log(No. Immigrants five years)	0.0745**	0.101*	0.0846**	0.127*
	[0.0381]	[0.0502]	[0.0429]	[0.0651]
log(No. Immigrants five years) border	0.144	0.0941*		
	[0.148]	[0.0515]		
log(No. Immigrants five years) common language	-0.106	0.0811**		
	[0.197]	[0.0413]		
log(No. Immigrants five years) common legal origin	0.0796**	0.0117		
	[0.0402]	[0.159]		
log(No. Immigrants five years) minus j			-0.431	-0.597
			[0.667]	[0.584]
State-year FE	yes	yes	yes	yes
Country of origin - year FE	yes	yes	yes	yes
Observations	728	641	779	668
R-squared	0.84	0.72	0.82	0.70
IV F-stat (Kleibergen-Paap)	9.51	8.42	11.29	10.48

Note: The number of immigrants is instrumented by the number of refugees ("no-U.S.-tie" cases) who arrived in the past five years in state i from country j. Distances are weighted by population and in kilometers from the capital city in country i to the capital city in state j. All regressions include country of origin -as well as state-year fixed effects. Robust standard errors in parentheses (clustered by state): ***, **, * indicate the statistically significant difference from zero at the 1%, 5% and 10% levels respectively.