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How do low-education immigrants adjust to Chinese import shocks? Evidence using English language proficiency☆

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ABSTRACT

This paper examines the link between trade-induced changes in local labor market opportunities and English language fluency rates among low-education immigrants in the United States. The production-based manufacturing jobs lost due to Chinese import competition around the turn of the century did not require strong English-speaking skills while many of the jobs in expanding industries, mostly in the service sector, did. Consistent with responses to these changing labor market opportunities, we find that a $1,000 increase in import exposure per worker in a local area led to an increase in the share of low-education immigrants speaking English very well in that area by about half a percentage point. As evidence that at least part of this is a result of actual improvements in English language speaking abilities, we show that low-education immigrants in trade-impacted areas became especially likely to be enrolled in school compared to similarly low-education natives. While we find some evidence for domestic migration in response to trade shocks, we also show that our results are not likely to be driven by language-selective internal migration or initial settlement decisions.

1. Introduction

Most economists agree that while international trade results in aggregate welfare gains, these gains as well as accompanying losses are unevenly distributed. Suggestive of large welfare losses, Autor et al. (2013) find sizeable decreases in employment in the manufacturing sector in local areas specializing in industries competing with Chinese imports. The long-run welfare impacts of these job losses, however, depend on the ability of workers to move into expanding markets either by changing sectors of employment or geographic locations. Autor et al. (2013; 2021) find little evidence of such adjustments within the general population. We examine the impact of Chinese import competition on a population that may, on the one hand, be especially vulnerable to manufacturing job loss

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given their often-poor English-speaking skills, but on the other hand, may be more adaptable than the general population: low-education immigrants. Specifically, we examine whether, on average, the share of low-education immigrants speaking English very well increased in areas that were more affected by the large increases in Chinese import competition around the turn of the century. We also consider whether any such changes are likely to have been driven by actual improvements in English speaking abilities or selective migration into or out of local areas based on language abilities.

From a theoretical perspective, low-education immigrants may be especially susceptible to the negative consequences of trade shocks because their limited English-speaking abilities can make moving to growing sectors, many of which are in the service industry, especially difficult. The reason immigrants are highly represented in the manufacturing sector (Andersson et al. 2014) may be that production work does not require the English fluency of a native speaker. Production workers typically do not communicate with customers and clients, and given the repetitive nature of many manufacturing jobs, effective communication with managers and coworkers may not be very important. In fact, even rudimentary English-speaking abilities may not be necessary if immigrants are able to segregate into plants employing mainly speakers of their native language. While there is considerable evidence that immigrants who are better able to speak the host country's language earn higher wages (e.g., Bleakley and Chin 2004), the returns to host country language skills depend on a worker's occupation and in some occupations, can be close to zero (Berma et al. 2003).

On the other hand, low-education immigrants may be especially adaptable to changing labor market conditions. Many immigrants are not eligible for U.S. safety net programs, potentially making them more likely to accept less-than-ideal alternate jobs, even jobs requiring acquisition of new skills. In addition, many immigrants have family members in their home countries that rely on their remittances—again, providing further incentive to take on new jobs. Finally, immigrants in the U.S. are not a random sample of people from their home countries; they are likely positively selected in terms of willingness to work and adapt to very new environments (Cadena and Kovak 2016).

One way in which immigrants with limited English proficiency may respond to the loss of manufacturing jobs is by improving their English skills. Workers displaced from manufacturing jobs typically find reemployment in low wage service jobs (Autor and Dorn 2013; Goos et al. 2014) which often require the use of English on the job. Even the threat of job loss in the manufacturing sector may induce some immigrants to enroll in formal English classes or take other active steps to improve their English. Perhaps more importantly, when low-education immigrants lose their jobs in the manufacturing sector, they may take jobs in sectors requiring their use of English on-the-job. The increased exposure to the language is likely to increase fluency even without any active investments.

While immigrants with weak English-speaking skills may find it more difficult to move into sectors requiring English fluency within the same local geographic area, they may find it easier, compared to natives, to move to U.S. locations with less manufacturing job loss. Indeed, Cadena and Kovak (2016) show that low-education Mexico-born immigrants were more likely than low-education natives to migrate within the U.S. in response to Great Recession-induced labor market shocks. Autor et al. (2023) and Yu (2023) present some evidence of decreases in the population of immigrants in local areas with more exposure to the China shock. This may be because of internal migration responses to job loss but also because immigrants choose not to settle in affected areas when they first arrive in the United States or simply return to their home countries in response to poor local labor market conditions.

We start by examining empirically whether, on average, English language proficiency among immigrants remaining in areas most affected by Chinese import exposure improved relative to those in areas less hard hit. For information on English fluency as well as other local area characteristics, we use data from the 1990–2000 Censuses as well as the combined 2006–2008 American Community Surveys. Taken directly from Autor et al. (2013), our measure of import exposure makes use of initial industry composition in the local area together with industry-level Chinese import growth to the entire country. Our results suggest that for every $1,000 increase in import exposure per worker in a local area, the share of low-education immigrants speaking English very well in that area increased by about half of a percentage point, a magnitude comparable to the associated decline in manufacturing employment (Autor et al. 2013). Conducting a pre-shock placebo analysis, we show that this relationship is not likely to be coincidental. Large decreases in the share of employed low-education immigrants working in occupations not requiring English speaking skills point to labor demand shocks as drivers of our main findings. Our tests for heterogeneity reveal that our baseline results are mainly driven by white non-Hispanics. They also suggest that immigrants with a solid base level of English proficiency, immigrants with relatively higher educational attainment, and immigrants who have been in the U.S. for less than ten years respond more strongly to Chinese import competition than immigrants in other groups.

We then turn to examining whether the relationship between Chinese import exposure and English-speaking abilities of low-education immigrants is mainly a result of actual improvements in English language proficiency or selective migration. Because our data do not allow us to follow individuals over time, we cannot show conclusively that English-speaking abilities of individuals improved in response to changes in Chinese import competition. However, we do show that the low-education immigrants in our sample living in areas with more import competition are more likely to be enrolled in school than those in areas that are less affected. In fact, in terms of school enrollment, low-education immigrants are more responsive to import exposure than are low-education natives, despite the fact that on average, natives are more likely to be in school. These findings may reflect a mechanism through which Chinese imports increase English fluency immigrants; English skills are likely to improve regardless of course content. It is also possible, however, that in response to import shocks, immigrants learn enough English on their own to make U.S. schooling feasible. Either way, these results can be viewed as evidence of actual improvements in English as well as investments in other more general forms of human capital.

To explore how large a role selective migration may play in driving our baseline estimates, we start by simply looking at the impact of trade exposure on low-education immigrant population changes at the local level. Mirroring the results in Autor et al. (2013), we find no statistically significant populations shifts, a result that may mask language-based selective migration if the number of English-fluent in-migrants approximates the number of out-migrants with poor English skills. To address this issue, we explore the relationship between Chinese import exposure and changes in characteristics of people that are difficult or impossible to change. We do not find any statistically significant impacts in terms of educational attainment, age, and years in the U.S.—characteristics we would expect to change along with language fluency if our results were mostly driven by selective migration patterns.

Next, we examine internal migration decisions more directly by using answers to survey questions on residence five years prior to the survey. Keeping in our sample only immigrants who were living in the U.S. five years prior to the survey, we can link individuals either to trade exposure in their current residence or to trade exposure in their previous residence. Consistent with prior literature (Autor et al. 2023; Yu 2023), we find that immigrants who were living in areas harder hit by Chinese import exposure in the years prior to the survey were more likely to migrate out than those in less hard-hit areas. However, this relationship appears to be driven by the immigrants with better, not worse, English-speaking skills, making it more difficult for us to identify improvements in English fluency in our baseline model. When considering where internal migrants go, there is some evidence that they are less likely to move to areas with more trade exposure, but the estimated effect is statistically insignificant. Overall, internal migration patterns do not seem to be a first order driver of our language results.

We then explore whether our baseline findings may be driven by settlement choices of new immigrant arrivers by examining impacts separately by years in the U.S. Given that it takes time to become fluent in a language, a finding of strong effects of Chinese import exposure on language fluency among those who arrived in the U.S. within just a few years prior to the survey might be interpreted as evidence of selective migration. We find statistically insignificant and rather small impacts for the very recent arrivers suggesting that our results are not driven by initial settlement decisions. However, while estimates are strongest for those in the U.S. between four and eight years, there is also no evidence of English language improvements for immigrants who have lived in the U.S. for nine or more years. Finally, we show that when separating the sample based on where people were living five years prior to the survey, estimates are strongest among those who were previously living in the same house. Taken together, while our results might be consistent with selective settlement decisions based on immigrants' abilities to learn English upon arrival, they are not consistent with selective settlement based on language skills acquired before migration. Overall, our results are most suggestive of import-induced language acquisition.

The remainder of the paper is organized as follows. Section 2 presents background and motivation for the study. Section 3 describes the data and our measurement of key variables. Section 4 describes our empirical approach. Section 5 presents our baseline empirical results showing that in local areas with more exposure to the China shock, English language proficiency among low-education immigrants improved. This section also provides evidence that this result is both robust and interpretable in a causal way. Section 6 examines whether the increases in English fluency among low-education immigrants remaining in hard-hit areas is likely to be driven by actual improvements in English fluency or language-specific migration responses, and section 7 concludes.

2. Background

While in 1991, the share of total U.S. spending on Chinese goods was a little over half a percent, this figure rose more than seven-fold by the year 2007 (Autor et al. 2013) and has continued to grow since then (authors' own calculations)[1]. This has led to large reductions in U.S. manufacturing employment. More generally, because of input-output linkages as well as other local general equilibrium effects, there is evidence that Chinese imports have decreased overall U.S. job growth (Acemoğlu et al. 2016).[2] People living in areas with more Chinese-import induced job losses have received more transfer payments for unemployment, disability, retirement and healthcare (Autor et al. 2013), have experienced worsening physical and mental health (McManus and Schaur 2016; Adda and Fawaz 2020), and have higher mortality rates (Pierce and Schott 2020; Adda and Fawaz 2020). Other evidence also points to declines in marriage (Autor et al. 2019), increases in out of wedlock births (Autor et al. 2019), and increases in political polarization (Autor et al. 2020). While the China trade shock plateaued in 2010 or so, Autor et al. (2021) show that the decreased manufacturing employment, employment to population ratios, and income per capita in affected areas have persisted at least into 2019.

The degree to which employment losses in certain manufacturing industries result in overall declines in welfare depends on workers' abilities to reallocate themselves either to different jobs—jobs that often require acquiring new skills—or to different locations. Greenland and Lopresti (2016) find increases in U.S. high school graduation rates in local labor markets most negatively affected by import competition. This result is consistent with a model in which high school students compare opportunity costs of staying in school to the expected future benefits of a high school degree and make decisions accordingly; Chinese import competition decreases the opportunity costs and increases the expected benefits of a high school degree. We contribute to the literature on the impacts of Chinese import exposure by examining the impact of import exposure on the human capital investments of low-education immigrants, focusing on English fluency.[3]

Language skills are an important form of host country-specific human capital for immigrants. While previous studies have shown a positive association between language skills and earnings (e.g. Abramitzky et al. 2023; Angrist and Lavy 1997; Arendt et al. 2022;

[1] Data on U.S. imports from China by year are available on the U.S. Census website (https://www.census.gov/foreign-trade/balance/c5700.html). Deflating the 2018 values to 2007 dollars using the Personal Consumption Expenditures (PCE) price index, we calculate that net imports from China increased 38 percent between 2007 and 2018.

[2] In more recent research, Bloom et al. (2019) show that Chinese imports reallocated jobs from the manufacturing sector to the service sector resulting in a positive but statistically insignificant overall employment effect.

[3] There is a broader literature examining human capital investment responses to changes in labor market returns to skill. We provide an overview of this literature in Appendix 5.

Bleakley and Chin 2004; Dustmann and van Soest 2002; Dustmann and Fabbri 2003), there is considerable variation in the extent to which immigrants become fluent in the host country language, even after spending many years in the host country.[4] Much of the literature on language acquisition focuses on factors making learning a new language easier, for example, age at arrival (Bleakley and Chin 2004), the similarity between an immigrant's native language and the host country language (Adsera and Chiswick 2007), or the availability of high quality language courses (Arendt et al. 2022; Lochmann et al. 2019). Our paper considers whether increases in the labor market returns to learning the host country language influence immigrants' fluency in the host country language.[5]

We also explore whether low-education immigrants make migration decisions based on the returns to their skills in different locations. While in general there is evidence of little or no migration response to local Chinese import shocks (Autor et al. 2013; Faber et al. 2022), Greenland et al. (2019) show that the local labor markets most exposed to trade shocks experienced a relative reduction in population growth over the following decade.[6] Moreover, the low-education immigrants in our sample may be more mobile than the general population. Cadena and Kovak (2016) find that low-education Mexico-born immigrants were more responsive to Great Recession-induced local labor market shocks than were natives. Schündeln (2014) finds similar results using data on immigrants in Germany. Basso and Peri (2020) show that although the foreign-born in general tend to be less mobile than the native-born, immigrants who have been in the U.S. for less than ten years are more mobile across states and labor markets than natives.

Most closely related to our paper, Autor et al. (2023) and Yu (2023) present evidence that population headcounts in commuting zones with more exposure to the China shock decreased among the foreign born but not the native born. These population changes may result from migrants' internal migration decisions, initial settlement decisions of newly arriving immigrants, and return-migration decisions. Appendix 5 provides background information on each of these possible migration responses to changing economic conditions.

Migration responses to trade-induced shocks are not in themselves sufficient to generate increases in English fluency rates in trade-exposed areas. For this to happen, English-fluent workers must be relatively more likely to remain in (or more likely to migrate to) import-affected areas than workers with more limited English skill. In practice, however, it may be those who are most comfortable speaking English that are able to leave their families and ethnic communities in trade-impacted areas in search for better labor market opportunities elsewhere. After all, other evidence has shown that, in general, it is those with more education that are more mobile (Molloy et al. 2011; Notowidigdo 2020). Moreover, newly arrived immigrants with limited English proficiencies may be forced to live near family and friend within the U.S. despite better labor market opportunities elsewhere (see Munshi 2003).[7]

To conclude, it is possible for us to observe increases in language proficiency rates among low-education immigrants in areas more exposed to Chinese import competition even if individual immigrants do not become better English speakers. However, this would only happen if immigrants with limited English-speaking ability migrated out of or did not migrate to areas that were most affected by import shocks. In the mechanisms section of the paper, we explore whether migration patterns are likely to be important drivers of our results. We thus contribute to the migration-response literature generally, and more specifically to the part of the literature focusing on immigrant responses, by focusing on whether any immigrant migration responses are likely to be influenced by English speaking abilities.

3. Data

3.1. Main sample

Our data come from the five percent state samples of the 1980, 1990 and 2000 U.S. Censuses as well as the 3-year sample of the American Community Survey (ACS) data from 2006 to 2008 (later referred to as the 2007 sample) all drawn from the U.S. Census Integrated Public Use Micro Samples (IPUMS (Ruggles et al. 2018)); the 1980 data is used to construct our instrumental variable and to conduct our internal migration analysis. These data are particularly well suited for this study because they contain information on immigrants' English proficiency and the large sample sizes allow us to create accurate measures of demographic characteristics within

[4] About 9% of the U.S. population can be considered Limited English Proficient (LEP), and approximately 21% speak a language other than English at home (Zong and Batalova 2017). In the year 2012, more than 37 percent of immigrants living in the U.S. for 30 years or more were not able to speak English "very well" (Gambino et al. 2014).

[5] Despite widespread beliefs that government programs (such as language classes and transfer payments) explain why refugees tend to assimilate faster, Abramitzky et al. (2023) show using historical data that in fact refugees were better English speakers than economic migrants even before such refugee resettlement programs existed in the United States. They argue that because refugees are unlikely to return to their home countries, they have a greater incentive to learn English.

[6] Hakobyan and McLaren (2016) also find NAFTA-induced migration responses to labor market changes but only among those with less than a high school degree. Analyses of data from other countries also point to minimal migration responses to local trade shocks (see Erten et al. (2019) for evidence from South Africa; Topalova (2010) for evidence from India; and Dix-Carneiro and Kovak (2017; 2019) for evidence from Brazil).

[7] While in general, low-education workers are less likely to migrate domestically in response to economic opportunities than high-education workers (Molloy et al. 2011; Notowidigdo 2020), low-education immigrants are especially responsive (Cadena and Kovak 2016; Schündeln 2014). Notowidigdo (2020) suggests that eligibility for means tested public assistance programs during economic downturns is one important explanation for why low-education workers in general are less mobile than high skill-workers. Because the low-education immigrants in our sample are often not eligible for these programs, they may be more mobile than their native-born counterparts.

commuting zones for relatively small populations.[8]

Our main sample consists of immigrants between the ages of 18 and 65, from non-English speaking countries,[9] who arrived in the U.S. after turning 18 and who have completed at most a high school degree—those with any college attendance (even less than a year) are dropped from the sample. This last requirement is to ensure that we do not include in our sample international students who have not yet completed their college degrees. We also drop those immigrants who report speaking only English since for them, English is likely their native language despite their being born abroad. Our justification for dropping childhood immigrants is that they are typically fluent in English by the time they join the labor market (Bleakley and Chin 2004). We restrict our sample to low-education immigrants because they are more likely to work in production within the manufacturing sector. They are also less likely to have been fluent in English before coming to the United States making them more sensitive to changes in U.S. industrial structure. For constructing all of the variables in our analysis, we keep only individuals living in the mainland United States. Because our analysis relies on making comparisons across geographic areas, we drop individuals without detailed geographic information.

Our measure of English proficiency is based on answers to the survey question: “How well does this person speak English?” The question has four possible responses: “very well,” “well,” “not well”, and “not at all.” Following Bleakley and Chin (2004), we create an English fluency dummy variable equal to one for immigrants speaking English “very well” and zero otherwise.[10]

The primary goal of this paper is to study how immigrants with low levels of formal schooling respond to industrial structure changes in local labor markets. Following Autor et al. (2013), we measure labor markets using commuting zones (CZs)—areas resembling Metropolitan Statistical Areas (MSAs) in that they are created so that most inhabitants live and work within the same area but differing in that they cover the entire United States including rural areas. Using the method discussed in Autor and Dorn (2013), we map individuals in the Census and ACS samples to 1990 commuting zones based on their Public Use Microdata Areas (PUMAs).[11] By the 1990 definitions of commuting zones that we use, there are 741 commuting zones in the U.S. and 722 in the U.S. mainland. Just as in Autor et al. (2013), although we start with the individual-level data from the IPUMS, we eventually aggregate the data to the commuting-zone year level.[12]

3.2. Measuring Chinese import shocks to local areas

To measure Chinese import exposure in each CZ, we follow Autor et al. (2013) in interacting the change in U.S. imports from China in each industry with the share of workers in that commuting zone working in that industry and then summing this across all industries in the commuting zone. More specifically,

$$\Delta IPW_{zt}^{u} = \sum_{j} \frac{L_{zjt}}{L_{zt}} \frac{\Delta M_{ucjt}}{L_{ujt}} \tag{1}$$

where ΔM_{ucjt} is the change in the dollar value of imports in industry j to the U.S., u, from China, c, in a ten-year or equivalent period starting in year t, L_{ujt} is the start of period employment in industry j in the entire U.S., L_{zt} is the start of period employment in CZ z, and L_{zjt} is the start of period employment in industry j in CZ z. Thus, the variation in ΔIPW_{zt}^{u} stems from differences across commuting zones in start-of-period industrial structures.[13]

3.3. Descriptive statistics

Our data is aggregated to the CZ-year level. Our main variables are constructed as decadal differences, between 1990–2000 and

[8] We do not use more recent data given the findings that changes in Chinese import exposure do not appear to be influencing labor market outcomes very much after the late 2000s (Bloom et al. 2019; Autor et al. 2021).

[9] English speaking countries are defined as countries where more than half of the recent adult immigrants speak English at home (Bleakley and Chin 2004). Countries with English as an official language are also excluded from the sample. Puerto Rico is classified as a non-English speaking country. Details regarding the origin countries used in our analysis can be found in Appendix Table A.1.1.

[10] Because this information is self-reported (or reported by the household member filling out the survey), the English-speaking ability variable may suffer from measurement error. Different people might have different answers to the English-speaking ability question even holding constant actual English-speaking ability. We note, however, that while this type of error in a dependent variable will yield more imprecise estimates, there is little reason to be worried about severe bias. A more problematic concern is that people systematically report better English skills in areas with more import exposure. We expect, however, that when workers are expected to use more advanced-level English on the job, they will perceive their English skills to be less adequate. This would make it more difficult for us to detect improvements in English skills. Also placating concerns is a literature showing that self-assessed oral proficiency tends to be a reliable measure of actual oral skills (see Ma and Winke (2019) and references therein).

[11] It is difficult to match PUMAs to commuting zones in cases where PUMAs span multiple commuting zones. Following Autor et al. (2013), in these cases, we duplicate individual observations in the original data, one for each potential commuting zone a person in a PUMA may be in, and then we adjust the person weights based on the share of the population of the PUMA that resides in the different commuting zones.

[12] There are six small population CZs not included in our sample because there were no low-education immigrants sampled in these commuting zones. Of those six, four are in South Dakota, one is in Arkansas, and one is in Minnesota. We use 716 commuting zones in our baseline sample.

[13] We use the variable constructed in Autor et al. (2013) and made available on David Dorn’s website. To construct the variable, Autor et al. (2013) use data on imports by industry from the United Nations Commodity Trade Statistics.

2000–2007; again, the "2007" value is constructed from the 2006–2008 3-year ACS sample. Table 1 presents descriptive statistics of the 1432 cells in our data (716 commuting zones multiplied by the two decadal differences) separated by whether the changes in import exposure are above or below the median change in import exposure in the sample. As can be seen from the table, the share of low-education immigrants who speak English very well generally decreased by about four percentage points in this time period, but the decrease was smaller in magnitude in commuting zones with larger increases in Chinese import exposure. Table 1 also presents descriptive statistics on start of period commuting zone characteristics, again separated by whether the commuting zone experienced above or below median changes in Chinese import exposure in the ten years following that base period. As can be seen from the table, the low-education immigrants in high exposure commuting zones are less likely to have high school degrees, are more likely to be Hispanic, less likely to be Asian, and are significantly more likely to be employed in manufacturing. The table also presents start of period descriptive statistics for the general working age population of the commuting zones (not just the low-education immigrants in our sample). Relationships mirror those for the low-education immigrants: In CZs with larger increases in import exposure, the share of the working age population employed in manufacturing is higher. These commuting zones also have a slightly larger share of the population with college degrees.[14]

4. Empirical approach

To identify the impact of changes in industrial structure on the English language fluency of immigrants, we exploit variation across commuting zones in exposure to Chinese import competition. Following Autor et al. (2013), our baseline empirical specification is a stacked first difference model of the form,

$$\Delta ENG_{zt} = \alpha \Delta IPW^{u}_{zt} + W_{zt}\beta_1 + X_{zt}\beta_2 + \gamma_t + \varepsilon_{zt} \tag{2}$$

where the dependent variable, $\Delta \mathrm{ENG}_{zt}$ is the decadal change between year t and year $t+1$ in the share of low-education immigrants in CZ z that speak English very well. The right-hand side variable of interest, ΔIPW^{u}_{zt}, measures the change in Chinese import exposure in commuting zone z again between the year t and $t+1$.

The vector, W_{zt}, contains a set of demographic characteristics of commuting zone z measured at the start of decade. These include characteristics like share of the working-age population that is college educated and share female in the labor market, but also the share of the commuting zone workforce employed in manufacturing. The latter is an especially important control variable for our analysis because commuting zones may be strongly affected by Chinese imports both because they have more people employed in the manufacturing sector and because the particular manufacturing industries in those commuting zones are in direct competition with Chinese imports. Commuting zones with larger manufacturing sectors may be very different from commuting zones focusing on other industries and so the characteristics of people in the manufacturing-centric commuting zones may have evolved over time for reasons unrelated to Chinese import competition (for example, technological change). By controlling for base period share of the workforce employed in manufacturing, we are implicitly comparing the evolution of English-speaking abilities of immigrants residing in commuting zones with very similar initial industrial structures but with some facing more competition from imports from China than others. The vector, X_{zt}, controls for characteristics of the low-education immigrant population in the commuting zone, including the share with a high school degree, average age, average years in the U.S., share female, and the shares of different races.

Because the model is estimated in first differences, we have two observations for each commuting zone: one for the difference between 1990 and 2000 and the other for the difference between 2000 and 2007. Each observation is weighted by the start-of-period share of all low-education immigrants across the U.S. that reside in that CZ.[15] To allow for differences across decades in changes of English-speaking abilities of immigrants across the entire U.S., we include decade fixed effects, γ_t, which we estimate by including a dummy variable equal to one for commuting zone differences between 2000 and 2007. Finally, ε_{zt} is an error term. As in Autor et al. (2013), standard errors are clustered at the state level throughout. We note that the estimates from a first difference model of this type tend to be similar to those from a more traditional three period fixed effects model but with less restrictive assumptions made on the error term (see Autor et al. 2013, footnote 26).

If changes in our measure of import exposure across commuting zones arise mostly from supply shocks in China, then we might interpret our estimated α as the impact of imports-induced job losses in a commuting zone on the English language fluency of immigrants in that commuting zone. A potential concern with this estimation strategy is that the changes in CZ import competition are instead driven by U.S. demand shocks. For example, if people in the U.S. start demanding more smartphones, then China would export more smartphones to the U.S., but at the same time, U.S.-based smartphone manufacturers would also produce more smartphones. If this is true, commuting zones with more smartphone production may even have better labor market opportunities—and consequently less of an incentive for immigrants to learn English–than those specializing in other industries, despite the fact that they are exposed to more Chinese import competition. Thus, demand-induced changes in Chinese exports will attenuate our estimates of the impact of import exposure.

[14] While we constructed our own aggregate characteristics for the low-skilled immigrants in our baseline model, the data on working age population characteristics (namely, the share of the commuting zone workforce employed in manufacturing, the share of the working age (16-64) population that has a college education, and the share employed among working age female workers) were obtained from David Dorn's website.

[15] As a result, commuting zones with small low-education foreign-born populations cannot drive the estimates, even if in our sample these commuting zones include individuals who are outliers in terms of English fluency. We also re-ran our preferred specification using our immigrant-based weights multiplied by the Autor et al. (2013) whole population-based weights. As can be seen in Appendix Table A.1.2, results were robust.

Table 1
Descriptive statistics.

	Change in import exposure in CZ Below median (1)	At or above median (2)	Total sample (3)
Change imports from China to US / Worker	1.05	2.16	1.84
	(0.57)	(1.59)	(1.47)
Among low education immigrant population			
Change in percentage speaking English very well	-5.46	-3.77	-4.26
	(5.20)	(4.02)	(4.46)
Average age$_{-1}$	40.80	41.14	41.04
	(2.91)	(2.90)	(2.91)
Average years in the US$_{-1}$	12.64	13.04	12.92
	(2.45)	(2.22)	(2.29)
Percent female$_{-1}$	49.92	50.52	50.35
	(6.23)	(4.64)	(5.15)
Percent with high school degree$_{-1}$	30.57	27.84	28.63
	(11.06)	(8.13)	(9.15)
Percent non-Hispanic White$_{-1}$	14.01	14.59	14.42
	(12.75)	(12.79)	(12.78)
Percent non-Hispanic Black$_{-1}$	2.49	1.96	2.12
	(3.86)	(3.13)	(3.37)
Percent Asian$_{-1}$	17.22	11.92	13.45
	(12.91)	(8.41)	(10.20)
Percent Hispanic$_{-1}$	64.37	70.29	68.58
	(20.53)	(16.42)	(17.90)
Percent married$_{-1}$	62.51	61.70	61.93
	(6.93)	(5.08)	(5.68)
Percent of immigrants employed in manufacturing$_{-1}$	10.20	15.99	14.33
	(5.07)	(5.85)	(6.22)
Among whole commuting zone population			
Percent employed in manufacturing$_{-1}$	11.52	17.43	15.73
	(5.23)	(5.68)	(6.16)
Percent of women employed$_{-1}$	62.40	62.40	62.40
	(6.22)	(4.70)	(5.18)
Percent with college degree$_{-1}$	53.35	52.13	52.48
	(7.61)	(6.11)	(6.60)
Number of observations	716	716	1,432

Note: The sample of low education immigrants consists of immigrants between the ages of 18 and 65, from non-English speaking countries, who arrived in the US after turning age 18 and who have completed at most a high school degree. Statistics in column 1 are based on the 716 CZs that are below the median level of changes in import exposure per worker in each period. Statistics in column 2 are based on the 716 CZs that are above the median level of changes in import exposure per worker in each period. Percent and means are reported at the beginning of each period. All statistics are weighted by the start-of-period CZ share of the national sample immigrant population. Standard deviations are reported in parentheses.

To address this issue, we follow Autor et al. (2013) in instrumenting for Chinese import exposure with a variable constructed from changes in Chinese imports to other developed countries, namely, Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland:

$$\Delta IPW^{o}_{zt} = \sum_{j} \frac{L_{zjt-1}}{L_{ujt-1}} \frac{\Delta M_{ocjt}}{L_{zt-1}} \tag{3}$$

where ΔM_{ocjt} is the change in imports from China, c, to the eight other high-income countries, collectively denoted o. These changes are driven by Chinese supply-side factors as well as demand-side factors specific to those other countries. If the demand-side factors in these other developed countries are rather idiosyncratic, i.e., not correlated with U.S. demand, then the IV strategy will identify the impact of Chinese import exposure stemming solely from improvements in Chinese productivity and openness to trade. If this is the case, we expect our IV estimates to be larger in magnitude than our ordinary least squares (OLS) estimates.[16,17]

[16] For evidence that correlated import demand shocks are not important drivers of their results, Autor et al. (2013) replace their import-shock measure with a measure constructed from residuals from a gravity model run on bilateral trade data at the industry level. The importer fixed effects used in this model net out differences in demand from the importing country leaving only variation in China's exporting productivity and trade costs. We use the same residuals-based import measure in our model of immigrant language fluency, and as can be seen in column 2 of Appendix Table A.1.3, results are robust. In the same appendix table, we also show that our baseline results are also robust to using the other measures of import exposure explored in Autor et al. (2013).

[17] Another source of potential bias with our import exposure measure (Eq. 1) is that contemporaneous employment may be affected by anticipated trade exposure in the future. Following Autor et al. (2013), we address this issue by replacing the start-of-period employment levels in Eq. (2) with employment levels from the prior decade in Eq. (3). Using lagged employment decreases simultaneity bias.

Table 2
Baseline regressions.

Dependent variable:	Change in share speaking English very well					Change in imports from China to US / Worker
	OLS	OLS	OLS	OLS	IV	First Stage
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Imports from China to US / Worker	0.405***	0.446***	0.440***	0.373**	0.465**	
	(0.122)	(0.133)	(0.122)	(0.150)	(0.229)	
Δ Imports from China to other countries / Worker						0.547***
						(0.075)
Observations	1,432	1,432	1,432	1,432	1,432	1,432
R-squared	0.014	0.170	0.288	0.293	0.292	0.735
Low skilled immigrant controls	N	Y	Y	Y	Y	Y
Whole population controls	N	N	N	Y	Y	Y
State FE	N	N	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Dependent variable mean (levels, not changes)	17.13	17.13	17.13	17.13	17.13	1.839
F statistic first stage						53.72

Notes: N = 1,432 (716 × 2 time periods). The sample consists of immigrants between the ages of 18 and 65, from non-English speaking countries, who arrived in the US after turning age 18 and who have completed at most a high school degree. See Table 1 for a description of the control variables used in columns 2-6. The IV regression includes the full vector of controls from column (4). The year FE is a dummy for the 2000–2007 period. Robust standard errors are clustered at the state level. Stacked first difference models are weighted by the start-of-period CZ share of the national sample immigrant population. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A new literature has developed formalizing the basis for identification and inference in shift-share studies such as ours (Adão et al. 2019; Goldsmith-Pinkham et al. 2020; Borusyak et al. 2022). We view the Borusyak et al. (2022) framework, which assumes exogeneity of the shifts (change in imports, in our case) while allowing for endogeneity of the shares (shares of the commuting zone labor force working in the different import industries, in our case) as the most appropriate for our setting. Autor et al. (2021) provide justification for this assumption in their analysis of the long-term impacts of Chinese import exposure on employment outcomes (see their Appendix A.4), and the same reasoning can be used for our study. Nevertheless, in Appendix 3, we follow the recommendations of Borusyak et al. (2022) and show that our results are broadly robust.

5. Empirical results

5.1. Baseline findings

Table 2 displays our baseline results. Controlling only for period fixed effects, column 1 shows that the share of low-education immigrants speaking English very well increases in local areas with more exposure to Chinese import competition. In column 2, controls for base period low-education immigrant characteristics are added to the model, and this results in a slight increase in the magnitude of the estimate of interest. As can be seen in column 3, the magnitude of the estimated impact of import exposure remains almost the same when state fixed effects and base period manufacturing share are added to the model. Column 4 presents results when base period share of the commuting zone working age population that has a college degree and share female in the labor force are added as controls to the model. Our estimate of interest decreases again but not substantially.

Next, we turn to the IV analysis. Column 6 shows that the IV is positively associated with Chinese import exposure, and the F statistic of 53.7 points to a strong first stage. The two stage least squares estimate in column 5 suggests that for every $1,000 increase in import exposure per worker, the share of low-education immigrants in the commuting zone speaking English very well increased by 0.47 percentage points.[18] For comparison, this same increase in import exposure reduces manufacturing employment per working-age population by 0.60 percentage points (Autor et al. 2013).

As expected, the IV estimates are larger in magnitude than the corresponding OLS estimates in column 4 suggesting that U.S. demand shocks may be attenuating the OLS estimates, but the difference is not very large. We note, however, that it is certainly possible for demand shocks in other countries (used to create our IV) to be correlated with demand shocks in the U.S.; this would attenuate even our IV estimates.

A more worrisome issue arises if the IV is correlated with commuting zone level characteristics associated with improvements in English proficiency for reasons unrelated to Chinese import exposure or even industrial structure more broadly. To address this concern, we regress *past* changes in English proficiency of immigrants on *future* changes in Chinese import exposure. If, for example, Chinese import competition and English language fluency of immigrants in a commuting zone were both increasing over time, but Chinese import exposure was not causing the changes in English proficiency, then we would expect to estimate a positive coefficient on

[18] The half percentage point increase represents only about three percent (0.47/17.3) of the share of the sample that speaks English very well, a figure that may appear small. However, it is important to note that improving language fluency among adults is very difficult. Lochmann et al. (2019) show using a regression discontinuity design that 100 additional hours of a language-training course in France had no discernible impact on the ability of non-EU immigrants to speak French.

Table 3
Impact of Chinese imports on employment in different occupations (IV regressions).

Dependent variable:	Change in share employed in…			Change in share…	
	Manufacturing related occupations	Management and professional; technical, sales, and administrative; service occupations	Farming, forestry, and fishing occupations	Unemployed	Not in labor force
	(1)	(2)	(3)	(4)	(5)
Δ Imports from China to US / Worker	-0.937***	0.625**	0.311**	0.710***	0.313
	(0.309)	(0.297)	(0.158)	(0.174)	(0.388)
Observations	1,394	1,394	1,394	1,432	1,432
R-squared	0.325	0.344	0.180	0.212	0.899
Average Dependent Variable (Levels)	48.32	44.13	7.55	6.10	35.53
F	53.57	53.57	53.57	53.72	53.72

Notes: All samples are limited to immigrants between the ages of 18 and 65, from non-English speaking countries, who arrived in the US after turning age 18 and who have completed at most a high school degree. However, the samples used in columns (1)-(3) consist of only employed immigrants. Using the broad occupational categories within the OCC1990 variable, we classify "Precision Production, Craft, and Repairers" and "Operatives and Laborers" as manufacturing occupations. All IV regressions include the full vector of controls as well as the state and year fixed effects from Table 2 column (4). See Table 1 for description of the variables we use as controls. Robust standard errors are clustered at the state level. Stacked first difference models are weighted by the start-of-period CZ share of the national sample immigrant population. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

future changes in Chinese import exposure. If instead, our baseline estimates reflect causal relationships, we should see no statistically significant impacts. Results from this placebo regression are shown in Appendix Table A.1.4. The change in import exposure from 2000 to 2007 in a commuting zone has no statistically significant impact on the change in English fluency among immigrants from 1990 to 2000 in that commuting zone. In fact, the estimate has a negative sign.

Finally, for further evidence that the changes in English fluency we observe are driven by labor market shocks, we look at occupational changes among the employed low-education immigrants in our sample. Mirroring the results in Autor et al. (2013), the estimate in column 1 of Table 3 suggests that, just like the general population, low-education immigrants become less likely to work in manufacturing-related occupations (i.e., precision production, craft, and repairers as well as operatives and laborers) in places harder hit by Chinese import competition.[19] The estimate in column 2 shows that exposure to Chinese imports increases employment in occupations that are likely to require strong English skills. In particular, Chinese import exposure increases the share of low education immigrants employed in managerial and professional occupations; technical, sales, and administrative occupations; and service occupations. The estimate in column 3 suggests an increase in the share of the labor force working in farming, forestry, and fishing occupations as a result of Chinese import exposure. We do not expect these occupations to be more English-intensive than the manufacturing-related occupations, but we note that the increase in employment in these occupations are only about a third of the magnitude of the decrease in the manufacturing-related occupations. Columns 4 and 5 show that, consistent with the results in Autor et al. (2013) for the general population, import competition led to increases in the share of the low-education immigrant population that became unemployed (column 4) and out of the labor force (column 5), although the latter estimate is not statistically significant.

Next, we look more directly at whether Chinese import competition pushes low-education immigrants into jobs requiring more English skills and out of jobs that do not require much communication. We gather data from the U.S. Department of Labor's O*NET survey on the importance of oral abilities (specifically, oral expression and oral comprehension) in different occupations. Following Peri and Sparber's (2009) broad technique, we classify occupations based on the importance of oral abilities for success on the job.[20]

To determine whether the low-education immigrants in our sample are over-represented in occupations that O*NET lists as requiring strong oral skills, we classify all of the occupations in our sample into five groups based on how important oral communication is to success in that particular job. Pressing machine operators and textile sewing machine operators are examples of occupations in the bottom category of oral skill, while telephone operators and business or retail salesclerks are examples of occupations in the top category. Figure A.2.1 presents the distribution of low-education immigrants and low-education natives across these five different occupation types. As can be seen in the figure, immigrants are especially likely to work in occupations with weak communication requirements and unlikely to work in occupations with strong communication requirements. In contrast, their U.S. born counterparts are more evenly distributed across occupation types with most working in occupations–such as cashiers, barbers, and kitchen workers–with moderate oral skill requirements.

We then construct for each commuting zone-year cell in our sample, the share of employed low-skill immigrants who work in occupations that require different levels of oral skills grouped into quartiles ranking from the highest to the lowest. Finally, we estimate models of the form in Eq. (1) using the share of low-education immigrant workers in an occupation requiring different levels of spoken English skills as the dependent variables. The results are reported in Table 4.

The positive estimates in column 1 and 2 of Table 4 might suggest that Chinese import shocks pull low-education immigrants into occupations requiring very strong English skills, but the estimates are rather small in magnitude and not statistically significant. This is probably because the English skill requirements of those occupations are relatively high for most of the low-education immigrants in our sample. On average, only around 22 % of them work in those occupations. Nearly 78 % of them work in occupations with English skill requirements in the bottom quartiles. The results in columns 3 and 4 of Table 4 show that trade exposure pushes low-education immigrants out of occupations that require only very weak oral expression and comprehension skills and into occupations requiring better English skills; the estimated coefficient on trade exposure is positive in column 3 and negative in column 4, both statistically

[19] Appendix Table A.1.5 presents estimates of this model using different samples. In column 1, we start by using a sample of the entire working age population. Column 2 shows estimates constructed using a sample consisting of only the native born, column 3 uses a sample of low-education natives, and column 4 shows estimates constructed using a sample of low-education immigrants from non-English speaking countries. Panel A considers the impact of Chinese import exposure on the likelihood of working in manufacturing while Panel B shows the impact on working in any occupation that does not require strong oral abilities. The point estimates in both panels are consistently negative across the different samples. The estimate of the impact of Chinese import competition has the largest magnitude when constructed using low-skilled immigrants from non-English speaking countries.

[20] The O*NET dataset provides a measure of importance of different abilities for success in each occupation ranging from one to five (1 signifying "not important" and 5 signifying "extremely important"). We merge these occupation-specific numerical measures of the importance of oral abilities into a sample of workers drawn from a 2007-2011 ACS sample. Following Peri and Sparber (2009), we then create a variable measuring the relative importance of abilities. For example, an occupation with an oral ability score of 0.10 implies that only 10 percent of workers in 2007-2011 worked in an occupation where oral abilities were less important than in their occupation. We then merge these percentile scores to the low-skilled immigrant individuals in our 1990 U.S. Census, 2000 U.S. Census, and 2006 to 2008 ACS samples by occupation. This is complicated by the fact that O*NET contains 840 occupations classified using the standard occupation classification (SOC), while Census and ACS data classify only 324 possible occupations in the OCC1990 variable. To address this issue, we take the average score if multiple O*NET occupations matched with one OCC1990 occupation. We then take the average of the rescaled scores for the two oral abilities (expression and comprehension) as the oral score.

Table 4
Impact of Chinese imports on employment in occupations with different requirements for oral english abilities (IV regressions).

Dependent variable:	Change in share employed in corresponding occupations Top 25th percentile oral requirement (1)	75–50th percentile oral requirement (2)	50–25th percentile oral requirement (3)	Bottom 25th percentile oral requirement (4)
Δ Imports from China to US / Worker	0.041	0.151	0.626**	-0.818***
	(0.146)	(0.113)	(0.290)	(0.272)
Observations	1,394	1,394	1,394	1,394
R-squared	0.311	0.148	0.119	0.162
Average Dependent Variable (Levels)	12.79	9.55	31.09	46.57
F	53.57	53.57	53.57	53.57

Notes: The sample consists of only employed immigrants between the ages of 18 and 65, from non-English speaking countries, who arrived in the US after turning age 18 and who have completed at most a high school degree. These IV regressions include the full vector of controls as well as the state and year fixed effects from Table 2 column (4). See Table 1 for description of variables we use as controls. Oral English requirements for occupations are obtained from O*NET data. See footnote 20 in the text for more details. Robust standard errors are clustered at the state level. Stacked first difference models are weighted by the start-of-period CZ share of the national sample immigrant population. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

significant. These findings not only provide support for our hypothesis that the language fluency changes we estimated in our baseline regressions are driven by labor market changes, but they also alleviate any potential concerns about whether the self-reported measure of English fluency that we use is an appropriate measure of actual English fluency.[21]

5.2. *Who is most affected?*

As a first step towards understanding the main drivers of our baseline results, we examine which immigrants are most affected by changes in import competition. We start by testing for heterogeneity by level of English fluency in order to gain insight on the mechanisms through which immigrants might be improving English. For ease of comparison, in column 1 of Panel A in Table 5, we simply reproduce our baseline estimates again using changes in the share of immigrants speaking English very well as the dependent variable. In column 2 of the same panel, we replace the dependent variable with changes in the share of immigrants speaking English either well or very well, thereby decreasing the threshold for fluency. While 17 percent of our sample speaks English very well, 42 percent speak it well or very well. Chinese imports do not have a statistically significant impact on improvements in English fluency as measured by the share of speakers with abilities above this lower threshold. In the last column, results are shown for an even lower threshold and again, no statistically significant impacts. We conclude from this analysis that any language-based changes resulting from Chinese imports occur at the top of the English-speaking distribution. This is certainly consistent with average or above average English speakers who have lost their manufacturing jobs taking jobs in the service sector and then improving their English further with the extra practice, but these estimates alone cannot rule out selective migration at the top of the English-speaking distribution driving results.

Next, we consider heterogeneity by race and ethnicity in order to gain insight on whether discrimination or residential and occupational segregation influence the impact of Chinese import exposure. After all, previous research finds that non-Hispanic white immigrants are less segregated from US-born non-Hispanic whites compared to Asian, Hispanic, and non-Hispanic black immigrants (Iceland and Scopilliti 2008). In Panel B of Table 5, we conduct the analysis separately by race, and results suggest that our findings are driven by whites.[22,23]

Finally, we consider heterogeneity by educational attainment. As shown in Figure A.2.3, immigrants who do not have a high school degree are overrepresented in the manufacturing sector relative to those with a high school degree. This implies that on the one hand, they are more vulnerable to import-induced manufacturing shocks and so might be more motivated to improve their English. On the other hand, many of them might not have the basic skills necessary to quickly become proficient in English. To examine which effect is stronger empirically, we separate the sample by education and results are shown in Panel C of Table 5. To create the estimates in

[21] Also using O*NET data, Arendt et al. (2022) show that refugees to Denmark who completed more hours of language classes take jobs that require more language skills, pointing to a link between knowledge of the host country language and immigrants' abilities to take jobs requiring language skills.

[22] Approximately 70 percent of the non-Hispanic white group is from Europe. Poland, Italy, Germany, Portugal, Greece, and Russia are the top six source countries. Details can be found in Appendix Figure A.2.2.

[23] The number of observations in column 1 differs from the number in the baseline sample because there were 212 commuting zone-year cells with zero low-education white immigrants in them. Recall that our dependent variable is the decadal change in the language abilities of immigrants in the commuting zone. If there are no immigrants with a particular characteristic (like race) in the initial IPUMS sample in either the base year or the end year, we are not able to calculate a difference and so the commuting zone-year observation is dropped in our aggregate analysis. While this issue results in differences in the number of observations across groups, it is not likely to result in large differences in coefficient estimates because the dropped cells tend to represent very small populations, and we weight our observations (the commuting zone-year cells) by the relative size of the relevant population in the commuting zone in the start year. For example, in column 1, observations are weighted by the share of all white low-education immigrants in the country (in the base year) that resided in the particular commuting zone.

Table 5
Heterogeneity of impacts of Chinese import exposure (IV regressions).

Panel A: Heterogeneity by measure of English fluency				
Dependent variable:	Change in share speaking English …			
	Very well	Well or very well	(Regardless of how well)	
Δ Imports from China to US / Worker	0.465**	0.114	0.253	
	(0.229)	(0.421)	(0.184)	
Observations	1,432	1,432	1,432	
Average dependent variable (Levels)	17.13	42.18	78.69	
F	53.72	53.72	53.72	
Panel B: Heterogeneity by race				
Dependent variable:	Change in share speaking English very well			
Sample:	White	Black	Asian	Hispanic
Δ Imports from China to US / Worker	3.204***	0.280	0.543	0.331*
	(1.196)	(2.427)	(0.545)	(0.177)
Observations	1,220	158	1,242	1,288
R-squared	0.146	0.323	0.101	0.150
Average Dependent Variable (Levels)	32.80	29.20	12.79	14.02
F	42.17	20.77	124.7	39.81
Panel C: Heterogeneity by completed schooling				
Dependent variable:	Change in share speaking English very well			
Sample:	< High school	High school		
Δ Imports from China to US / Worker	0.191	1.142***		
	(0.198)	(0.432)		
Observations	1,384	1,384		
R-squared	0.258	0.240		
Average Dependent Variable (Levels)	13.07	27.33		
F	48.29	68.04		

Notes: The sample consists of immigrants between the ages of 18 and 65, from non-English speaking countries, who arrived in the US after turning age 18 and who have completed at most a high school degree. These IV regressions include the full vector of controls as well as the state and year fixed effects from Table 2 column (4). Robust standard errors are clustered at the state level. Stacked first difference models are weighted by the start-of-period CZ share of the national sample immigrant population. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

columns 1 and 2, we separate our baseline sample of immigrants with no more than a high school degree into a group with less than a high school degree (column 1) and another with a high school degree (column 2). Estimated impacts are stronger for those with a high school degree, perhaps because high school graduates are better equipped to learn a new language (Arendt et al. 2022) or because they are more likely to move in response to labor market opportunities elsewhere. In the following section, we examine these possibilities more carefully.

6. Mechanisms: English proficiency improvements vs. selective migration

In response to trade-induced manufacturing job losses, do the English-speaking abilities of immigrants actually improve or is it that the least English proficient immigrants leave (or do not move to) the hardest hit areas? We are not able to perfectly distinguish between these mechanisms, both of which may be occurring at the same time. Instead, we present evidence suggesting that English learning is driving at least part of our estimates and that selective migration does not seem to be playing an important role.

For evidence of actual learning, without actual measures of within-person changes in English proficiency, we look to other investments in human capital that are likely to be correlated with improved English skills. Specifically, we consider the impact of Chinese import exposure on the likelihood that the low-education immigrants in our sample (from non-English speaking countries, aged between 18 and 65, who arrived in the US after the age of 18, and who have never been enrolled in college) are enrolled in school. We note that the measure of school enrollment available in the Census and ACS only includes schooling which leads to a high school diploma or a college degree; English as a Second Language (ESL) classes would not be counted in this measure. Moreover, because we drop from the sample those who attended college for even less than a year, our enrollment measure is not picking up college attendance. Instead school enrolments are measuring participation in General Educational Development (GED) classes.[24] We believe that participation in GED classes improves English speaking abilities of immigrants regardless of the subjects being taught.

[24] Passing all four of the GED subject tests is meant to signal to employers and institutes of higher education that the test-taker has U.S. high school-level academic skills. GED classes are often offered free of charge (or for a nominal fee) at local community centers or other adult education centers. In our sample, almost half of the low-education immigrants enrolled in these courses already have a high school degree, presumably acquired in their home countries (authors' calculations). While individuals with a U.S. high school diploma are not eligible for a GED, individuals with high school diploma acquired abroad are eligible for the credential.

Table 6
Impact of Chinese imports on school enrollment of low skilled immigrants and natives (IV regressions).

Dependent variable:	Change in share enrolled in school	
Sample:	Low education immigrants	Low education natives
	(1)	(2)
Δ Imports from China to US / Worker	0.521***	0.191***
	(0.147)	(0.070)
Observations	1,432	1,444
R-squared	0.327	0.337
Average Dependent Variable (Levels)	5.826	7.033
F	53.72	39.88

Notes: In column (1), the sample consists of immigrants between the ages of 18 and 65, from non-English speaking countries, who arrived in the US after turning age 18 and who have completed at most a high school degree. In column (2), the sample consists of natives between the ages of 18 and 65 who have completed at most a high school degree. The IV regression in column (1) includes the full vector of controls as well as the state and year fixed effects from Table 2 column (4). In the IV regression of column (2), the same set of controls are included except the variable "Average years in the US. Robust standard errors are clustered at the state level. Stacked first difference models are weighted by the start-of-period CZ share of the national population of the sample listed in each column. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Column 1 of Table 6 shows that Chinese import exposure increases the likelihood that low-education immigrants in the commuting zone are enrolled in school. For comparison, we also consider the same model but run on a sample of low-education native-born individuals. Consistent with Greenland and Lopresti's (2016) findings that high school graduation rates increase in areas with more Chinese import exposure, we estimate a positive and statistically significant impact of import exposure on the likelihood that natives are enrolled in school. However, the magnitude of the impact is substantially smaller for natives than it is for immigrants, a result consistent with higher GED returns for immigrants than natives.[25] We cannot determine whether labor market shocks induce immigrants to take GED classes, and in doing so, they learn English or whether the labor market shocks induce English proficiency improvements which make GED classes feasible. However, regardless of the direction of causality, these results are certainly suggestive of actual English skill improvements.

Even if low-education immigrants do become more English-proficient if they happen to be in areas harder hit by Chinese import shocks, this does not rule out a role for selective migration. After all, it may be the immigrants who were planning to enroll in school that move to (or do not move out of) commuting zones with more import exposure. Following the literature, we start exploring this issue by examining whether Chinese import competition leads to changes in the number of low education immigrants in the commuting zone. Estimating our baseline model (Eq.1), including the controls for low-education immigrant characteristics as well as a control for lagged low-education immigrant population change $\Delta\ln(Population_{zt-1})$,[26] but substituting our language-based dependent variable with changes in the log of low-education immigrant population counts, we find no statistically significant evidence of changes in the low-education immigrant population in response to Chinese import competition (column 1, in Panel A of Table 7), and strangely the estimate is positive.

For an easier comparison with Autor et al. (2013) and Cadena and Kovak (2016), Column 1 in Panel B of Table 7 shows results from a model controlling for baseline characteristics of the entire commuting zone (that is, including natives as well as high-skilled immigrants, vector W in Eq. 2) but not including characteristics of the low-education immigrants in our sample. In line with Autor et al. (2013) but contrary to Autor et al. (2023) and Yu (2023), again we find no evidence of population changes.[27] The estimate has a positive sign but is very small and statistically insignificant. We note, however, that caution is necessary when interpreting this finding. As explained in Borusyak et al. (2022), close-to-zero estimates do not necessarily imply that workers are unwilling or unable to relocate. They may simply reflect the fact that the typical destination locations for individuals in hard-hit locations face similar economic shocks. Given that the purpose of our study is to examine the average English-speaking abilities of immigrants who remain (or end up) in import-affected commuting zones, the explanation for the lack of strong migration responses does not matter.

Regardless of the general migration responses, selective migration may still be behind our baseline results if the number of poor

[25] Clark and Jaeger (2006) show that while native-born GED holders earn less than high school graduates, foreign-born GED recipients earn more than immigrants with even high school degrees, but obtained abroad. As they explain, this may be either because immigrants actually learn U. S.-specific skills, such as English, while preparing for the tests or the GED allows them to credibly signal to employers that they already possess these skills.

[26] Following Monras (2020) and Greenland et al. (2019), we add a lagged population control to address the concern that population growth tends to be quite persistent over time.

[27] Autor et al. (2023) present new evidence that immigrant population headcounts decreased in response to the China shock. However, in many of the specifications presented in their paper, specifically those run using sample periods closer to ours, confidence intervals were quite large and point estimates were not statistically significant. Our overall assessment of this new literature (Autor et al. 2023; Yu 2023) is that immigrant location decisions are more sensitive to the China shock than the native-born, but even for the foreign-born, migration responses are not very robust, are often noisily estimated, and are not likely to be that large. In our particular sample period and with our preferred set of controls, there is no evidence of large population changes. This suggests that our English fluency results, estimated in the same sample period and practically the same specification, are not likely to be driven largely by population changes.

Table 7
Impact of Chinese imports on population (IV regressions).

Dependent variable:	Change in log of low-education immigrant population × 100			
Sample:	All	Do not speak English very well	Speak English very well	Immigrants from English-speaking countries
	(1)	(2)	(3)	(4)
Panel A: Full set of controls				
Δ Imports from China to US / Worker	0.472	-0.074	1.858	-2.054
	(0.979)	(0.947)	(2.019)	(2.323)
Observations	1,428	1,366	1,360	1,316
R-squared	0.670	0.665	0.465	0.410
Average Dependent Variable (Levels)	1199	1188	994.4	976.8
F	54.23	54.42	39.08	15.68
Panel B: Omitting controls for immigrant characteristics				
Δ Imports from China to US / Worker	0.028	-0.499	2.754	-0.304
	(1.807)	(2.032)	(2.153)	(2.427)
Observations	1,428	1,366	1,360	1,316
R-squared	0.652	0.644	0.453	0.389
Average Dependent Variable (Levels)	1199	1188	994.4	976.8
F	63.13	62.06	45.10	14.94

Notes In column (1), the sample consists of immigrants between the ages of 18 and 65, from non-English speaking countries, who arrived in the US after turning age 18 and who have completed at most a high school degree. In column (2), the sample only includes those from the sample in column (1) who cannot speak English very well. In column (3), the sample only includes those from the sample in column (1) who speak English very well. The column (4) sample includes immigrants aged 18–65, from English-speaking countries, who answered that they only speak English to the question regarding English speaking ability, who arrived in the US after turning age 18 and who have completed at most a high school degree. These immigrants are not included in the baseline sample. IV regressions in Panel A include the full vector of controls, the state and year fixed effects from Table 2 column (4), along with an additional control for lagged population growth. The immigrant-specific controls are not included in Panel B. Robust standard errors are clustered at the state level. Stacked first difference models are weighted by the start-of-period CZ share of the national population of the immigrant sample described in each column. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8
Impact of Chinese imports on changes in demographic composition (IV regressions).

Dependent variable:	Δ Share with high school degree	Δ Average years in the US	Δ Average age	Δ Share non-Hispanic black	Δ Share Asian	Δ Share Hispanic	Δ Share non-Hispanic white	Δ Share other races	Δ Share female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δ Imports from China to US / Worker	-0.054	-0.072	-0.099	-0.052	0.012	-0.452	0.280	0.212*	-0.383***
	(0.267)	(0.062)	(0.072)	(0.062)	(0.275)	(0.523)	(0.325)	(0.110)	(0.148)
Observations	1,432	1,432	1,432	1,432	1,432	1,432	1,432	1,432	1,432
R-squared	0.676	0.784	0.747	0.337	0.434	0.503	0.594	0.670	0.403
Average dependent variable (Levels)	28.63	12.92	41.04	2.117	13.45	68.58	14.42	1.434	50.35
F	54.13	53.72	54.56	52.86	52.86	52.86	52.86	52.86	51.93

Notes: The sample consists of immigrants between the ages of 18 and 65, from non-English speaking countries, who arrived in the US after turning age 18 and who have completed at most a high school degree. These IV regressions include the full vector of controls as well as the state and year fixed effects from Table 2 column (4). Robust standard errors are clustered at the state level. Stacked first difference models are weighted by the start-of-period CZ share of the national sample immigrant population. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

English speakers leaving areas with more trade exposure approximates the number of fluent English speakers coming to these areas.[28] In columns 2 and 3 of Table 7 (both panels), the point estimates reflect increases in the number of fluent English speakers and decreases in the number of poor English speakers, but the estimates are not statistically significant. Moreover, the coefficient signs may be explained by actual language improvements as opposed to migration. After all, when an immigrant becomes fluent in English, then

[28] This seems unlikely given previous research showing that Chinese import-exposed areas suffer job losses in all sectors, not just manufacturing, because of general equilibrium effects (Acemoğlu et al. 2016). On the other hand, Bloom et al. (2019) find little evidence that Chinese import competition led to net job losses, but instead show that it simply reallocated jobs from manufacturing to services. This is unlikely, however, to impact the low-education immigrants in our sample because the new service sector jobs were in research, management, and resale (i.e. high skill jobs) in high human capital areas (Bloom et al. 2019). Hakobyan and McLaren (2016) present evidence of decreased wage growth in areas affected by NAFTA even among service-sector workers.

even without anyone moving, the number of fluent English speakers increases by one and the number of poor English speakers decreases by one. To explore this further, in column 4, we show changes in the number of low-education immigrants from English speaking countries, a population not included in our baseline sample because they are likely to have arrived in the U.S. already proficient in English. The point estimates are negative in both panels. Even though they are not statistically significant, this result suggests that, if anything, those already proficient in English are relatively more likely to leave, not move to, commuting zones hard hit by trade shocks.

For further analysis of selective migration, in Table 8 we examine the relationship between import exposure and characteristics that are either impossible or at least difficult for a person to change. If the results in Table 2 are mostly driven by migration choices, then we would expect to see increases in the share of low-education immigrants with (difficult to change) characteristics that tend to be correlated with better English skills, for example, more schooling and years in the United States. On the other hand, if they are driven by actual improvements in English fluency as opposed to migration, we should see no impacts on these characteristics. Column 1 of Table 8 shows that there is no statistically significant relationship between Chinese import exposure and the share of low-education immigrants in the commuting zone with a high school degree. The estimate is small but is actually negative which is not what we would have expected if the more educated, and therefore more likely to be fluent, immigrants were moving to, or not moving out of, import-exposed commuting zones. Column 2 of Table 8 shows the same basic relationship when considering years in the U.S. instead of high school graduation. Similarly, there does not appear to be any relationship between trade exposure and the age distribution of low-education immigrants; the estimate in column 3 is negative, small in magnitude, and statistically insignificant.

Columns 4 through 8 consider the relationship between Chinese import competition and the racial composition of low-education immigrants in the commuting zone. Column 4 and 5 show that there are no statistically significant changes in the case of non-Hispanic blacks and Asians. The estimates are substantially larger but they remain statistically insignificant in the case of Hispanics (column 6) and non-Hispanic whites (column 7). Strangely, column 8 shows that the share of low-education immigrants who are “other races” increases and the estimate is statistically significant. Also, column 9 shows that the share of low-education immigrants in a commuting zone who are female decreases quite substantially in response to increased import exposure.[29] These findings may be interpreted as evidence that there are some population changes as a results of trade exposure, but given the statistically insignificant estimates of effects on the other composition measures, especially those that are likely to be correlated with English fluency, the overall results in Table 8 do not make a strong case for migration being a primary driver of English fluency improvements in commuting zones with more exposure to Chinese import competition.

Next, we consider internal migration more directly by limiting our baseline sample of low-education immigrants to those who were living in the U.S. years prior to the survey. This is not straightforward because the 1990 and 2000 Censuses asked respondents for place of residence five years prior to the survey while the ACS asks for place of residence one year prior to the survey. To address this issue, we follow Yu (2023) in dropping the ACS wave from our internal migration analysis while adding 1980 Census data. This allows for a consistent measure of previous residence (the 1980 Census also asks for place of residence five years prior to the survey) while at the same time allowing us to keep two differences per commuting zone. Also following Yu (2023), we set the import exposure change variable between 1980 and 1990 to zero in all commuting zones because China’s export growth during the 1980–1990 period was very small. We do the same in constructing the IV, and then confirm that our baseline language results are robust to adding the 1980 data in this way.

Using information provided by the Census on previous county group (in the 1980 Census) or PUMA (in the 1990 and 2000 Censuses), we can assign people to both their previous and current commuting zones.[30] We then label individuals as migrants if their current commuting zone is not the same as their previous commuting zone.[31] We start by examining whether the low-education immigrants in our sample of people already living in the U.S. in the year(s) prior to the survey are more likely to leave commuting zones hard hit by Chinese imports. To do this, we match our trade exposure and other commuting zone characteristics to individuals based on their previous commuting zone instead of their current commuting zone. We then estimate a model with the same right-hand side variables as Eq. 1 (using previous commuting zone characteristics) but with the change in the share of movers from that previous

[29] In Appendix Table A.1.6, we conduct the baseline analysis separately by gender. Although females are more likely to speak English very well than males, the estimated coefficient on trade is not statistically significant in the female sample. The point estimates are very similar to each other in the male and female samples.

[30] In the 1980 Census data, previous and current county group of residence have the same format. Using the county groups together with previous and current state codes, both can be easily matched with CZs. However, in the 1990 and 2000 Census data, previous PUMA is specified with a 3-digit code instead of the 5-digit code that is used in the PUMA to CZ cross-walk file. A detailed explanation of how we assigned previous PUMAs to commuting zones can be found in Online Appendix 4.

[31] As discussed previously, this process is rather complicated for those in county groups/PUMAs that span multiple commuting zones. We addressed this issue by duplicating individual observations in the original data, one for each potential commuting zone a person in a PUMA may be in, and then adjusting the person weights based on the probabilities that they live in each of the potential commuting zones. For our migration analysis, we duplicate observations again (even the already duplicated observations) when people’s previous PUMAs also span multiple commuting zones. Using this technique, an observation of an individual currently and previously living in a PUMA spanning, for example, two potential commuting zones would be duplicated to four observations (given the 2 x 2 = 4 possible combinations of commuting zones) with the weights on the four observations summing to one. This technique addresses the possibility that a person can remain in the same PUMA but still move between commuting zones, but it also overestimates the number of cross-commuting zone migrants. Because the duplicate observations receive little weight, they are unlikely to impact results. Instead of making duplicates, we also tried randomly allocating each individual to a CZ based on their PUMA. Results estimated using this technique, available upon request, are consistent with those presented in our tables.

Table 9
Impact of Chinese imports on migration between commuting zones (IV regressions).

Sample:	All (1)	Do not speak English very well (2)	Speak English very well (3)
Panel A: Dependent varable: Change in share migrated out of previous CZs			
Δ Imports from China to US / Worker	1.342*	1.287	1.542***
	(0.688)	(0.815)	(0.597)
Test for Equality of Estimates (p Value)		0.767	
Observations	1,370	1,216	1,260
R-squared	0.233	0.237	0.148
Average Dependent Variable (Levels)	9.301	8.992	10.33
F	324.1	354.3	177.9
Panel B: Dependent variable: Change in share migrated to current CZs			
Δ Imports from China to US / Worker	-1.678	-1.593	-1.587
	(1.081)	(1.159)	(1.147)
Test for Equality of Estimates (p Value)		0.993	
Observations	1,358	1,210	1,276
R-squared	0.244	0.222	0.200
Average Dependent Variable (Levels)	9.306	8.997	10.33
F	298.6	322.1	191.1

Notes: In column (1), the sample consists of immigrants between the ages of 18 and 65, from non-English speaking countries, who arrived in the US after turning age 18 and who have completed at most a high school degree. In column (2), the sample only include those who cannot speak English very well from the sample in column (1). In column (3), the sample only include those who can speak English very well from the sample in column (1). These IV regressions include the full vector of controls as well as the state and year fixed effects from Table 2 column (4). P values of tests for equality between estimates reported in columns 2 and 3 are reported. Robust standard errors are clustered at the state level. In Panel A, stacked first difference models are weighted by the prior CZ's start of period share of the national sample immigrant population. In Panel B, stacked first difference models are weighted by the current CZ's start-of-period CZ share of the national sample immigrant population. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

commuting zone as the dependent variable.[32]

Consistent with the findings in Autor et al. (2023) and Yu (2023), the estimate shown in column 1 of Panel A in Table 9 suggests that immigrants living in commuting zones with more exposure to Chinese import competition are more likely to migrate out of their commuting zones than those living in areas with less import exposure. This result, however, does not in itself imply that migration decisions drive our language results. For this to be the case, we would need immigrants with weaker English skills to be more likely to leave in response to shocks than immigrants with stronger English skills. To explore this possibility, in columns 2 and 3, we separate the sample based on English fluency. While the estimates in the two columns of Panel A are not statistically different from each other, the magnitude of estimate is in fact larger in the sample of immigrants who speak English very well. This is consistent with the idea that immigrants who are proficient in English (or are able to become proficient as a result of the shock) are better situated to take advantage of job opportunities elsewhere. They may also be better able to afford moving costs. In contrast, immigrants with limited English proficiency may find it difficult to leave their ethnic enclaves. These findings make it more challenging for us to detect improvements in English proficiency in import-affected areas even if they exist, lending more credence to our assessment that actual improvements in English skills are likely driving our main results.

Next, we consider where internal migrants go. Following Erten et al. (2019), we examine whether those currently living in commuting zones with more exposure to Chinese import competition are less likely to be recent arrivers. The point estimate in column 1 of Panel B in Table 9 suggests that immigrants are less likely to migrate to commuting zones with more trade exposure, but the estimate is not statistically significant. As shown in Columns 2 and 3 in Panel B, estimates remain negative and statistically insignificant when the analysis is done separately by English fluency. They are also of practically the same magnitude suggesting that immigrants in general are not moving to hardest hit commuting zones, regardless of English fluency.

Taken together, the results in Table 9 do not point to internal migration as a major driver of the positive relationship between trade exposure and English fluency rates. However, it is possible that initial settlement decisions of new arrivers to the U.S. can explain the pattern. To explore this possibility, we return to our full sample of low-education immigrants (including those who were not in the U.S. one or five years prior to the survey) observed in 1990, 2000, and 2006-2008. We then separate the sample based on years in the country. If our baseline results are mostly driven by how English fluency influences where immigrants choose to settle upon arrival, then we would expect a strong relationship between trade exposure and English fluency rates even among immigrants arriving in the previous year. On the other hand, if our results are driven mostly by people learning to speak better English, we would not expect to see impacts on the most recent arrivers. Presumably, they would not have had enough time to improve their English.

Results, shown in Table 10, show that there is no statistically significant impact of trade exposure on English fluency rates of

[32] This estimation procedure is not ideal because, for example, we are using changes in trade exposure in a commuting zone between 1990 and 2000 to explain the difference between the share of commuting zone leavers between 1985 and 1990 and the share of leavers between 1995 and 2000. If exposure to Chinese import competition changes gradually through time, our results are still likely to be informative about migration responses even though the timing does not line up perfectly. In any case, this issue is likely to attenuate our results.

Table 10
Impact of Chinese imports on immigrants with different years in the US (IV regressions).

Dependent variable:	Change in share speaking English very well				
Sample:	Years in the US, less than or equal to 3	Years in the US, 4-5	Years in the US, 6-8	Years in the US, 9-10	Years in the US, greater than 10
	(1)	(2)	(3)	(4)	(5)
Δ Imports from China to US / Worker	0.299	0.970**	1.071***	0.226	0.091
	(0.423)	(0.426)	(0.253)	(0.493)	(0.347)
Observations	1,168	994	998	1,008	1,396
R-squared	0.120	0.168	0.129	0.131	0.304
Average Dependent Variable (Levels)	12.02	13.18	14.83	14.39	20.98
F	27.42	35.59	43.85	67.61	82.01

Notes: Columns are created based on the "Years in the US" intervals reported in 1990. The sample consists of immigrants between the ages of 18 and 65, from non-English speaking countries, arrived in the US after turning age 18, have completed at most a high school degree and have spent certain years as specified in each column in the US. These IV regressions include the full vector of controls as well as the state and year fixed effects from Table 2 column (4). Robust standard errors are clustered at the state level. Stacked first difference models are weighted by the start-of-period CZ share of the national population of the immigrant sample described in each column. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11
Heterogeneity by recent migration history (IV regressions).

Dependent variable:	Change in share speaking English very well			
Sample:	Same house	Migrated within same state	Migrated from different state	Migrated from abroad
	(1)	(2)	(3)	(4)
Δ Imports from China to US / Worker	1.392*	0.767	-1.206	0.248
	(0.774)	(0.605)	(1.548)	(0.705)
Observations	1,266	1,020	902	1,110
R-squared	0.262	0.390	0.236	0.269
Average Dependent Variable (Levels)	24.08	18.14	25.57	11.49
F	207.4	355	185.7	270

Notes: The sample in each column consists of immigrants between the ages of 18 and 65, from non-English speaking countries, who arrived in the US after turning age 18, who have completed at most a high school degree and with certain migration history as specified in each column. These IV regressions include the full vector of controls as well as the state and year fixed effects from Table 2 column (4). Robust standard errors are clustered at the state level. Stacked first difference models are weighted by the start-of-period CZ share of the national population of the immigrant sample described in each column. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

immigrants who have been in the U.S. for three years or less, but there are strong and statistically significant impacts on those who have been in the U.S. between four and five years (column 2) or between six and eight years (column 3). Interestingly though, there is again no statistically significant impact of import exposure on English fluency rates of immigrants who have been in the U.S. for nine or ten years and the point estimate is practically zero for those who have been in the U.S. for more than ten years. Given that our baseline sample consists only of immigrants who arrived in the U.S. at age 18 or above, even the youngest immigrants in our sample of those who have been in the U.S. for more than ten years are 28 years old at the time of the survey. Our inability to detect impacts on this group may simply reflect how difficult it is for older people to learn a new language (Hartshorne et al. 2018).

It is also possible that our baseline results are driven by selective return migration, and immigrants in the U.S. for more than ten years are simply less likely to return to their home countries in response to poor labor market opportunities in their commuting zones. We do not believe that selective return migration is a major driver of our results in light of the extensive literature showing that immigrants did not return to their home countries during the Great Recession (Fix et al. 2009; Passel and Cohn 2009; Rendall et al. 2011). Unfortunately, however, it is difficult for us to test for return migration using our own data because we have only repeated cross-sections, and immigrants who return to their home countries simply do not appear in our data.

For further evidence that migration decisions may not be important drivers of our results, we also conduct the analysis (estimating Eq. 2) separately by where people were living five years prior to the survey: in the same house, in a different house but the same state, in a different state, or abroad. In constructing this table, we return to the Yu (2023) technique of replacing the 2006–2008 ACS wave with 1980 Census data (and assigning a zero to the change in Chinese import exposure between 1980 and 1990). As can be seen in Table 11, only for the low-education immigrants who stayed in the same house is the relationship between Chinese import exposure and the share of low-education immigrants speaking English very well statistically significant (see column 1). For those who migrated from different state and thus more likely to be from different commuting zone,[33] the estimate is actually negative. This is consistent with our prior finding English proficient speakers are more likely to leave heavily affected areas compared to those with more limited English skills. Taken together, these results suggest that the observed English improvement among immigrants in import affected areas

[33] Among all 722 commuting zones, only around 19% of them cross state boundaries.

is not likely to be a result of internal migration or initial settlement decisions.

7. Conclusion

This paper examines the relationship between Chinese import competition in an area and low-education immigrants' language skills in that area. Our results suggest that for every $1,000 increase in import exposure per worker, the share of low-education immigrants who speak English very well increases about half of a percentage point. This result is driven by whites, those with a high school degree, those who have been in the U.S. for four to eight years, and those who most likely start with a fairly advanced level of English proficiency.

Theoretically, these findings may be driven by actual improvements in immigrants' English-speaking abilities, either via active investments in English classes or more passive on-the-job learning, or by selective migration into or out of trade hit areas based on English proficiency. Because our data do not allow us to track individuals over time, we cannot perfectly distinguish between these two broad mechanisms. However, we do show that low-education immigrants in trade-impacted areas are more likely to be enrolled in school than their native-born counterparts providing some evidence of actual acquisition of new language skills. While we find some evidence showing immigrants are likely to leave import affected areas, there is no evidence implying that language-selective internal migration or language-selective initial settlement decisions drive our import-induced language improvement results.

Our results imply that even people who are the most negatively affected by trade can and often do make investments that may actually improve their long run outcomes. There is growing evidence that intense host country language classes increase employment prospects for refugees (Arendt et al. 2022; Lochmann et al. 2019). Our work implies that employment prospects themselves can further reinforce host country language proficiency leading to multiplier effects of these types of classes. In addition to any benefits to the immigrants themselves, improved language skills of immigrants can yield productive social interactions with natives potentially improving welfare overall. Perhaps instead of discouraging trade, policymakers may consider facilitating skill investments—both among immigrants and the native population.

Data availability

Furtado, Delia, and Kong, Haiyang. How Do Low-Education Immigrants Adjust to Chinese Import Shocks? Evidence using English Language Proficiency. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2024-01-13. https://doi.org/10.3886/E197401V1

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.euroecorev.2024.104681.

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