IMPORT COMPETITION AND INTERNAL MIGRATION

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Abstract—We examine the U.S. internal migration response to increased import competition following the granting of Permanent Normal Trade Relations to China in 2001. Using a variety of data sets and empirical approaches, we find that local labor markets most exposed to the policy change experienced a relative reduction in population growth over the following decade. The majority of the effect occurs at a lag of seven to ten years and is most pronounced among young individuals and low-education groups. Such population adjustments should influence the interpretation of evidence in the growing literature on import competition and local labor markets.

I. Introduction

A TREMENDOUS body of theoretical and empirical work underscores the fact that trade disrupts labor markets. In the face of increased import competition, workers in the most exposed sectors and locations bear a disproportionate share of the costs of trade in the form of reduced earnings and employment. From a welfare perspective, the crucial question then becomes: How do such workers and communities adjust? The long-run welfare gains predicted by theory depend on the ability of workers to reallocate themselves from negatively affected sectors and locations toward comparative advantage industries and expanding markets.

Migration, as a primary means of adjustment to such labor demand shocks, has been shown to affect the returns to both labor (Blanchard & Katz, 1992; Bound & Holzer, 2000) and other factors of production in the local economy (Moretti, 2011; Notowidigdo, 2011) in a way that equilibrates returns across local labor markets (Cadena & Kovak, 2016). Yet in response to one of the defining economic phenomena of the past quarter-century—the rise of China and the surge in import competition that accompanied it—no such migratory response has been documented. For example, Autor, Dorn, and Hanson (2013) find that rising imports per worker due to China's emergence were not clearly associated with population growth at the local level. This lack of a migratory response is particularly surprising given the fact that many studies have subsequently documented a broad range of consequences of the "China shock" for workers and residents of affected local labor markets.[1]

In this paper, we confront this puzzling result. Across a range of data sets and empirical approaches, we document evidence that the local labor markets most exposed to Chinese import competition experienced reduced population growth. Furthermore, we find considerable heterogeneity in the effect across population groups: the migratory response is primarily driven by the young, males, and low-education groups. To the best of our knowledge, we are the first to document a U.S. internal migration response to rising import competition from China at the level of the local labor market. We confirm this result using four distinct data sets at both the individual and aggregate levels. Our results are robust to controlling for a wide range of labor market characteristics that may have been correlated with coincident changes in labor demand not driven by import competition. We are also able to reconcile our results with the existing null results in the literature; we find that both preexisting population trends and a lagged response to import competition help to explain the lack of previous evidence on a migratory response to trade.

These findings have important implications for estimates of other labor market effects of rising import competition from China. In particular, selective migration in response to trade liberalization alters the composition of local labor markets and thus may confound estimates of the direct impact of import exposure on local economic conditions. Estimated changes in average economic outcomes may reflect both the direct treatment effect as well as the change in outcomes driven by changes in labor market composition. Future work on the local labor market effects of the China shock should account for such composition effects. Our results also matter for the welfare effects of rising Chinese import competition. As Caliendo, Dvorkin, and Parro (2017) showed in the context of a dynamic spatial model of trade, costly migration prevents complete labor market adjustment in the short run, leading to reduced wages in the most affected labor markets. This suggests that absent the kind of population adjustments we document in this paper, the labor market effects of import competition might have been even more severe.

Our primary empirical approach follows that of Pierce and Schott (2016a, 2016b), who analyze U.S. labor markets following the granting of Permanent Normal Trade Relations (PNTR) to China in 2001 and the surge in U.S. imports of Chinese goods that accompanied the policy change. We

[1] Such detrimental effects include increased unemployment, reduced labor force participation, and lower wages, particularly among those employed in manufacturing (Autor et al., 2013, 2014; Pierce & Schott, 2016a), those at the low end of the income distribution (Chetverikov, Larsen, & Palmer, 2015), and those with less education (Greenland & Lopresti, 2016). These groups have also seen reductions in both physical and mental health (McManus & Schaur, 2016) and increased mortality (Pierce & Schott, 2016b). In addition, Feler and Senses (2017) document reductions in the tax base, thereby reducing governmental budgets and provision of public services.

begin our analysis by documenting a robust negative relationship between PNTR exposure and ten-year changes in U.S. Census population counts among local labor markets that experienced varying exposure to the policy change. Using alternative Census samples from IPUMS (Ruggles et al., 2017), we explore heterogeneity in this response. The population of men, young adults, and low-education groups were particularly responsive to the granting of PNTR. This response is consistent with findings elsewhere in the literature that men and low-education groups were most directly affected by Chinese import competition (Autor et al., 2013; Greenland & Lopresti, 2016).

To complement these aggregate results, we examine the effect of PNTR on the propensity of young adults to move in response to the shock using longitudinal microdata from the Department of Education. These data allow us to track migration behavior among young adults while controlling for a broad range of characteristics at the individual level. This reduces concerns that our aggregate results may be driven by unobservable factors at the local level. This also allows us to observe out-migration separately from other adjustments to net population: in-migration, births, and deaths. Consistent with the results from the Census data, we find that individuals living in labor markets more exposed to the policy change were more likely to move out of their local labor market between tenth grade and age 26. We also confirm that movers in the microdata were less likely to choose higher-exposure locations as their destinations.

To examine the dynamics of the migration response over time, we also employ a distributed lag model using annual data on county-to-county population flows from the Internal Revenue Service (IRS). Notably, we find that the bulk of this effect occurred at a lag of seven years or more. Across multiple specifications, we find that at most, one-third of the population change had taken place five years after the shock. Such a finding is consistent with the findings of dynamic structural approaches aimed at estimating the costs of labor adjustment (Artuc, Chaudhuri, & McLaren, 2010; Dix-Carneiro, 2014; Monras, 2018; Caliendo et al., 2017) that typically find substantial impediments to mobility across sectors or markets, implying that reallocation will be sluggish. This suggests that analyses examining contemporaneous responses or ones over short time periods may understate overall population adjustments.[2]

Finally, we turn to an alternative measure of the China shock developed by Autor et al. (2013). While the Pierce and Schott (2016a, 2016b) approach exploits a policy shock

to measure regional variation in exposure to trade, the Autor et al. (2013) approach uses measured changes in U.S. imports per worker from China between 1991 and 2007 and has been the workhorse approach in this literature. Here too we find evidence of population adjustments to import competition. Crucially, this finding depends on accounting for preexisting trends in population growth at the local level. When we extend the analysis to 2010 to match our Census sample, the results become stronger. As when using the Pierce and Schott (2016a, 2016b), identification strategy, we observe population adjustments among working-age adults, young adults, and those without a college education.

The similarity in results across data sets and empirical approaches in the paper is striking. Our baseline results using decadal Census counts suggest that an interquartile increase in import competition exposure would have reduced local working-age population growth by approximately 0.015 log points, and 15- to 34-year-old population growth by double that amount. Exploring heterogeneity in this effect across different educational groups using IPUMS microdata, we find that the working-age interquartile effect in the Census data is bounded by the estimated effects among those with less than a high school diploma (an interquartile effect of 0.025 log points) and those with a high school diploma or some college (an interquartile effect of 0.013 log points). Employing an entirely distinct econometric approach to examine the dynamic response in IRS data on tax exemptions, we find a ten-year interquartile effect between 0.017 and 0.023 log points. Even using the alternative identification strategy of Autor et al. (2013), we find an interquartile effect of 0.013 log points for the entire working-age population and 0.026 log points for the 15 to 34 age group. Documenting such a robust population adjustment across multiple data sets and identification strategies is one of the primary contributions of this paper.

A number of papers estimate migration responses to trade and general local labor market shocks in contexts other than our own. Analyzing Indian trade reforms in the 1990s, Topalova (2010) finds little migratory response to trade-associated changes in poverty. In Vietnam, McCaig (2011) finds modest increases in net migration flows toward provinces positively affected by the 2001 U.S.–Vietnam Bilateral Trade Agreement. Focusing on the U.S. labor market response to NAFTA, Hakobyan and McLaren (2016) find some evidence of out-migration of low-skilled workers—a group that saw reduced wage growth following its implementation. Dix-Carneiro and Kovak (2017) find that the detrimental effect of the 1990s Brazilian trade liberalization on wages grew over time, a feature that is consistent with imperfect labor mobility across regions. Among more general labor demand shocks, this literature largely finds evidence that workers leave or avoid declining areas and move toward areas with more job opportunities (Bartik, 1991; Blanchard & Katz, 1992; Carrington, 1996; Black et al., 2005; Foote, Grosz, & Stevens, 2019).

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[2] Much of the existing literature on the migratory response to labor demand shocks relies on just such an approach, examining contemporaneous population adjustments to Bartik-style measures of changes to labor demand over a period of years (Bartik, 1991; Bound & Holzer, 2000; Wozniak, 2010). A related literature assesses dynamic local labor market responses, including migration, to discrete local shocks: the construction of the trans-Alaska pipeline system (Carrington, 1996), the coal boom and bust (Black, McKinnish, & Sanders, 2005), and the Bakken oil production boom (Vachon, 2015).

The remainder of the paper proceeds as follows. Section II describes the empirical methodology based on the Pierce and Schott (2016a) treatment of the China shock. In section III, we report results on the aggregate population response to import competition using decennial U.S. Census counts data and heterogeneity by demographics using Census samples from IPUMS. In section IV, we use microlevel data from the Department of Education to examine the responsiveness of young adults to the trade shock. Section V examines the dynamics of the migration response using annual migration data from the IRS. Section VI explores the robustness of our finding using the Autor et al. (2013) treatment of the China shock. Section VII includes a brief discussion and concludes.

II.   Empirical Methodology

Our primary empirical approach uses the insight from Pierce and Schott (2016a) that import competition for United States industries changed dramatically when the U.S. granted Permanent Normal Trade Relations (PNTR) to China in 2001. Prior to 2001, Chinese firms received Normal Trade Relations (NTR) tariff rates on exports to the United States—the tariff rates given to all World Trade Organization (WTO) members. However, provision of these rates to Chinese exporters was subject to annual renewal by the U.S. Congress. Renewal was politically contentious, with a great deal of uncertainty surrounding the annual vote. Had renewal of the NTR tariff rates failed to pass, Chinese exports would have faced non-NTR, or "column 2," rates. These rates, determined by the Smoot Hawley Tariff Act of 1930, represented an average increase of approximately 30 percentage points on the NTR rates. Thus, Chinese firms considering establishing a foothold in the U.S. market faced considerable uncertainty regarding the future costs of exporting.

This uncertainty was eliminated in 2001 when, following China's accession to the WTO, Congress voted to grant China NTR rates on a permanent basis. While observed tariff rates were unchanged by the policy shift, the accompanying reduction in uncertainty led to a substantial increase in Chinese exports to the United States (Handley & Limão, 2017; Pierce & Schott, 2016a). Considerable variation existed across industries in the extent of the potential increase prior to 2001, with the most at-risk industries facing tariff hikes of an order of magnitude or more, while other industries faced no increase at all. Pierce and Schott (2016a) take a difference-in-differences approach to examine the effects of the policy change, exploiting the variation in the potential tariff increase across industries before and after the granting of PNTR in 2001. In addition to increased import competition, they show that the industries most exposed to the policy change experienced a reduction in employment in the years after 2001.

We build on this approach to examine population adjustments to the policy shift and the attendant increase in import competition across local labor markets that varied in their preexisting industrial structure. Pierce and Schott (2016a)

define the difference between the observed NTR tariff rates and the potential non-NTR rates in industry j as the "NTR gap" facing the industry:

$$NTR\ Gap_j = Non\ NTR\ Tariff_j - NTR\ Tariff_j. \qquad (1)$$

As our outcome of interest is inherently geographical, we require a geographically based measure of import competition. To obtain this, we create a commuting zone (henceforth CZ) level measure of the NTR gap. CZs, a concept pioneered by Tolbert and Sizer (1996) and employed recently in a trade context by Autor et al. (2013), are approximations of local labor markets, created using journey-to-work data such that individuals living in a given CZ are highly likely to work in the same CZ. There are 722 CZs in our data, spanning the entire continental United States. We create a CZ-level measure of the NTR gap by weighting industry NTR gaps by the share of employment in CZ c accounted for by each industry j. Specifically, the NTR gap in CZ c is defined as

$$NTR\ Gap_c = \sum_j \frac{L_{cj}}{L_c} \times NTR\ Gap_j, \qquad (2)$$

where L_{cj} represents the employment in industry j in CZ c and L_c represents total employment in CZ c. Data on the NTR gap at the industry level are provided by Pierce and Schott (2016a). We map this to the CZ level using data on industry-level employment by county taken from the Census County Business Patterns data, which we aggregate to the CZ level. This also follows a similar approach in Pierce and Schott (2016b), who construct a county-specific NTR gap in order to explore the effects of import competition on local mortality.[3]

To avoid concerns regarding reverse causality, in our baseline specification we use labor shares as of 1990—the beginning of our sample. Similarly, we follow Pierce and Schott (2016a) and use industry NTR gaps in 1999, immediately preceding the policy change.[4] The distribution of the resulting CZ-level NTR gap is displayed in appendix figure A1. The median CZ faced an average potential increase of 0.053 prior to granting of PNTR—that is, a 5.3 percentage point increase in ad valorem tariff rates. However, there is substantial variation in the extent of the potential increase across CZs, with an interquartile range of 5.88 percentage points. In appendix figure A2, we show the geographic distribution of $NTR\ Gap_c$ values. While many areas of the country experienced some change in import competition, the shock

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[3] For our main results, we include all industries when calculating employment share weights for $NTR\ Gap_c$, as do Pierce and Schott (2016b). An alternative approach associated with the framework in Kovak (2013) would include only traded-goods industries. Because there is some disagreement in the literature on how this should be handled, appendix section 9.1 provides a detailed analysis of the choice of industries to include and how it applies in our context. We report results for our baseline approach using this alternative approach in table A1.

[4] Our results are robust to this choice of year. Specifically, outcomes are qualitatively unchanged by using industry-level NTR gaps in 1990.

was clearly clustered geographically, with the largest effects on the Southeast and Midwest.

We exploit this variation in the NTR gap in a difference-in-differences identification strategy that compares CZs facing more and less import competition before and after China's accession to the WTO in 2001. We take two primary empirical approaches, one employing aggregate data and one at the microlevel. In our aggregate approach, we employ decadal U.S. Census data on CZ population counts and consider the following CZ population growth equation:

$$\Delta ln(population_{ct}) = \beta_0 + \beta_1 NTR\ Gap_c \times Post2001_t$$
$$+ \beta_2 X_c \times Post2001_t$$
$$+ \beta_3 \Delta ln(population_{ct-10})$$
$$+ \delta_{rt} + \epsilon_{ct}, \qquad (3)$$

where $\Delta ln(population_{ct})$ represents the change in CZ c's log population between years $t - 10$ and t. We consider as outcomes changes in the log CZ population from 1990 to 2000 and from 2000 to 2010. $NTR\ Gap_c$ represents our measure of import competition exposure: the CZ average tariff gap as defined above. We allow this term to affect CZ populations differentially in years after 2001. That is, the $NTR\ Gap_c \times Post2001_t$ term will be 0 for all CZs in the 1990–2000 period and will be equal to the level of the NTR gap in the 2000–2010 period. This is a difference-in-differences specification that controls for preshock trends.[5]

We include a control for lagged population growth, $\Delta ln(population_{ct-10})$ to account for the possibility that more- and less-shocked areas experienced differential growth on average throughout this period, an important consideration since local population growth is persistent over time (Monras, 2018).[6] The lag variable measures CZ log population change between 1980 and 1990 (associated with 1990–2000 outcome variable observations) and between 1990 and 2000 (associated with 2000–2010 outcome variable observations).[7] In addition, we control for

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[5] Note that time-invariant CZ traits that influence population levels are differenced out when considering population growth as in equation (3).

[6] See appendix section 9.2 for details about the relationship between local import competition measures and population growth in prior periods. There, we show that import competition measures in the literature are correlated with prior population growth. An identification strategy that ignores prior population growth rates may mistakenly attribute effects of prior growth to the import competition shock. Our direct controls for prior CZ population growth and our identification strategy (Pierce & Schott, 2016b) that relies on changes in growth around 2001 (rather than levels of growth) should mitigate concerns that such incidental correlations explain our findings.

[7] An alternative identification strategy would include CZ fixed effects rather than lagged population growth. This triple-difference strategy would implicitly assume $\beta_3 = 1$ in equation (3). We have also estimated CZ fixed-effects models and find results similar to those reported below, though parameters are somewhat less precisely estimated. As our estimates strongly reject a coefficient of $\beta_3 = 1$, we focus on the more general specification described above. Finally, in appendix table A4, we employ an alternative control for population pretrends. Specifically, we control for the change in the log population from 1980 to 1990, interacted with period indicators for 1980 to 1990 and 1990 to 2000, respectively. Our results are robust to this alternative approach.

other time-invariant CZ characteristics, X_c, that are potentially correlated with $NTR\ Gap_c$ and may have led to differential population changes in the years following the policy change. As with $NTR\ Gap_c$, we interact these controls with a post-2001 indicator so that our specification allows X_c to influence population growth differentially in the 2000–2010 period. Finally, Census-division-by-decade fixed effects δ_{rt} control for time-varying fluctuations at the region level. We weight the population regressions by the CZ's share in the 1990 national population. In section V, we also estimate a distributed-lag version of equation (3) with annual IRS population data to investigate dynamics more explicitly.

Our second approach employs data on mobility at the individual level. We make use of the restricted access version of the National Education Longitudinal Study of 1988 (NELS:88) and the Education Longitudinal Study of 2002 (ELS:2002), both provided by the Department of Education. These data sets longitudinally track individuals in two waves. First, the NELS:88 includes a nationally representative sample of tenth graders in 1990, providing information on location, demographics, and educational outcomes. A follow-up survey reports student locations ten years later when they were mostly 26 years old. The ELS:2002 was conducted similarly, following a group of young adults between tenth grade and age 26 beginning in 2002. We employ these data to examine the effect of a change in the NTR gap within an individual's CZ at tenth grade on the probability that the student lived in a different CZ at age 26—that is, that the student migrated. We specify the following equation:

$$Migrate_{ict} = \alpha_0 + \alpha_1 NTR\ Gap_c \times Post2001_t$$
$$+ \alpha_2 X_c \times Post2001_t + \alpha_3 Y_{it} + \nu_c + \sigma_t$$
$$+ \gamma_{rt} + \epsilon_{ict}. \qquad (4)$$

The dependent variable $Migrate_{ict}$ is a dummy variable equal to 1 if individual i from cohort t (NELS:88 or ELS:2002) and originating in CZ c was living in a different CZ at age 26. The primary covariate of interest, $NTR\ Gap_c \times Post2001_t$, is equal to 0 for members of the NELS:88 cohort and equal to the CZ NTR gap for the ELS:2002 cohort. As already discussed, a primary benefit of this approach is that it allows us to control for a vector of individual-level controls Y_{it} that are unobservable in the more aggregate Census data. In this specification, σ_t is an indicator variable for membership in the ELS:2002 cohort (rather than the NELS:88), ν_c is a CZ fixed effect, and γ_{rt} is a Census-division-by-year fixed effect. As above, we also control for time-invariant CZ characteristics, X_c, interacted with $Post2001_t$.

III. Aggregate Population Effects

We begin by exploring the effect of granting PNTR on aggregate population movements at the CZ level. Specifically, we analyze changes in CZ working-age log populations between 1990 and 2000 and between 2000 and 2010, employing equation (3). We examine this specification using

TABLE 1.—IMPORT COMPETITION AND TEN-YEAR CHANGES IN LOG CZ POPULATION, CENSUS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Change in Log CZ Population: Ages 15–64				Change in Log CZ Population: Ages 15–34			
NTR Gap	−0.253***	−0.307***	−0.283**	−0.264**	−0.634***	−0.731***	−0.543***	−0.503***
× Post 2001	(0.079)	(0.070)	(0.117)	(0.114)	(0.108)	(0.096)	(0.174)	(0.170)
$\Delta\ln(population_{t-10})$	0.565***	0.558***	0.541***	0.529***	0.368***	0.360***	0.349***	0.338***
	(0.032)	(0.031)	(0.033)	(0.034)	(0.044)	(0.044)	(0.048)	(0.047)
Demographic controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Local economy controls	No	No	Yes	Yes	No	No	Yes	Yes
Local finance controls	No	No	No	Yes	No	No	No	Yes
Observations	1,444	1,444	1,444	1,322	1,444	1,444	1,444	1,322
R^2	0.714	0.725	0.739	0.743	0.621	0.630	0.643	0.653

$N = 1,444$ (722 CZs in two panels). Dependent variable is change in log CZ population of persons ages 15–64 in columns 1–4 and persons ages 15–34 in columns 5–8 from 1990–2000 and 2000–2010. Data taken from the U.S. Census. All specifications also include region-time fixed effects. NTR gap defined in equation (2). Regressions weighted by 1990 share of national population of relevant group. Standard errors clustered at the CZ and indicated in parentheses. Significant at ***1%, **5%, and *10%.

county-level Census population data, which we aggregate to the CZ level.[8] In addition to total population counts, we use this approach to examine changes in the populations of various demographic groups over time. We discuss this in further detail below.

We report our baseline estimates in columns 1 through 4 of table 1.[9] In column 1, we regress ten-year changes in the log CZ population of 15- to 64-year-olds on the interaction of the NTR gap with a post-2001 dummy variable, as well as region-by-decade fixed effects and ten-year lagged population changes. As described above, our inference exploits changes in log population growth within CZs before and after the granting of PNTR to China by comparing locations differentially exposed to the policy, while controlling for preexisting population growth. Thus, all cross-CZ variation in the NTR gap comes from the second ten-year change, as $NTR\ Gap_c \times Post2001_t$ is 0 for the first ten-year difference.

The estimates in column 1 suggest a reduction in working-age population growth among CZs with a higher average NTR gap—those more exposed to import competition. Specifically, the point estimate implies that an interquartile increase in the NTR gap would have reduced a CZ's population growth by 0.015 log points (approximately 1.5 percentage points). This estimate is statistically significant at the 1% level.[10]

In addition to trends in CZ population growth, we seek to control for other CZ characteristics that might be jointly correlated with industrial composition and population growth in the years following PNTR. In column 2 of table 1, we thus introduce the following demographic controls measured in 1990 and interacted with $Post2001_t$: the share of the population under the age of 25 and the shares of the population accounted for by black, Asian, American Indian, and Hispanic individuals, respectively. To the extent that these demographic characteristics are correlated with employment in industries most exposed to the policy change—largely manufacturing—as well as with population growth, omitting them will bias our estimates. Inclusion of these controls increases the magnitude of the point estimate of interest slightly. The results in column 2 imply an interquartile effect of a 1.8% reduction in CZ working-age population growth. This result is significant at the 1% level.

In column 3 of the table, we control for a number of additional 1990 CZ labor market characteristics that may have differentially affected labor markets in the years after PNTR. Specifically, we control for the 1990 share of the CZ labor force accounted for by women, the log of the 1990 average capital-to-labor ratio in the CZ, the 1990 average skill intensity of CZ industries, and the share of the population holding a college degree as of 1990.[11] We also include the 1990 share of employment in a CZ accounted for by routine occupations, which is intended to capture the extent to which occupations are likely to be more susceptible to automation, and the 1990 average offshorability index, which measures the feasibility of completing tasks without face-to-face interactions. These forces have played a substantial role in labor market turmoil in the past two decades (Autor & Dorn, 2013; Autor & Handel, 2013) and are potentially correlated with a CZ's industrial structure. Each of these measures is taken from Autor et al. (2013).

As a final control in column 3, we include a measure of the exposure of surrounding CZs to import competition. The magnitude of the NTR gap exhibits strong geographical clustering, as shown in figure A2. CZs that were highly exposed to the policy were likely to be proximate to CZs that were also exposed. Our specifications in columns 1 and 2, while controlling for shocks to the labor market opportunities within a given CZ, omit changing labor market opportunities in neighboring CZs. If the costs of moving increase with distance, omitting neighboring CZ shocks may understate the own CZ effect of PNTR, as we will have confounded the

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[8] The county-level data are concorded to the CZ level using code provided by David Dorn at his website: http://www.ddorn.net/data.htm. County-level employment data, taken from the County Business Patterns data set, are similarly aggregated.

[9] Appendix table A5 reproduces table 1 with point estimates from all controls included.

[10] As noted above, the interquartile range is approximately 0.059. Multiplying this value by −0.253 yields the above interquartile effect.

[11] Capital-to-labor ratios and skill intensity measures are calculated using the Becker, Gray, and Markov (2016) NBER-CES Manufacturing Industry Database. Industry-level capital-to-labor ratios and the share of employment accounted for by nonproduction workers in an industry are aggregated to the CZ level using industry employment shares.

potential migration-increasing effects of increased import competition in a given labor market with the migration-reducing effect of shocks in surrounding markets. To address this concern, for each CZ c, we calculate the weighted average of the NTR gap in all CZs $c' \neq c$, where the weight is proportional to the squared inverse distance between c and c'. Each of the above measures is interacted with an indicator variable equal to 1 during the 2000–2010 period, thus allowing for differential trends in migration following China's WTO entry. Inclusion of these controls reduces the magnitude of the effect of PNTR slightly, from -0.307 to -0.283, but the result remains economically meaningful and statistically significant.[12]

Finally, a primary potential confounding factor during our sample period is the financial crisis of 2007. To the extent that the severity of the crisis in a given labor market was correlated with the market's industrial composition, exclusion of controls for the crisis may bias our results. In column 4, we address this concern by introducing two controls for the severity of the crisis at the CZ level. The first, from Mian, Rao, and Sufi (2013), is the 2001 CZ debt-to-income ratio, interacted with a post-2001 dummy variable. This controls for the possibility that more indebted CZs experienced deeper declines in labor market conditions following the financial crisis (Mian et al., 2013; Mian & Sufi, 2014). Second, we include a measure of the magnitude of the housing bubble prior to 2007. A number of papers have argued that the housing market influenced labor market outcomes during this period (Mian & Sufi, 2014; Charles, Hurst, & Notowidigdo, forthcoming). To the extent that the housing bubble was correlated with the NTR gap through employment composition, omitting it is problematic. To control for the extent of the bubble, we follow Charles, Hurst, and Notowidigdo (2018) and estimate structural breaks in CZ housing prices between 2001 and 2006. As by Charles et al. (2018) argued, fundamental determinants of housing prices are likely to be incorporated into prices smoothly, while breaks in the price trend are more likely to be representative of a housing bubble. Using data from the Federal Housing Finance Agency as employed in Feenstra, Ma, and Xu (2017), we estimate a structural break on annual house prices at the CZ level between 2000 and 2006 and control for the magnitude of the break interacted with a post-2001 dummy in column 4.[13]

Inclusion of these controls reduces the magnitude of the NTR gap point estimate slightly, but leaves the broader message unchanged. An interquartile increase in the NTR gap reduced CZ working-age population growth by slightly more than 1.5 percentage points.[14]

A. Heterogeneity by Demographic Group

Such adjustments among the working-age population corroborate the view that granting China PNTR did affect migratory behavior. Yet one may wonder the extent to which this effect was driven by particular subpopulations. The answer to this question is important for two reasons. First, welfare effects at the local level depend on the migration response of different subpopulations. Differential changes in population by education group, for instance, will alter the income distribution of a labor market. This has implications for the local tax base, productivity, and amenity levels, among other things. Changes in the age distribution of a labor market will also have considerable effects on long-run economic and population growth rates.

Second, compositional shifts in CZ populations driven by selective migration will confound causal identification of the effects of import competition on economic outcomes, as estimated changes in average labor market outcomes will reflect both the treatment effect and the effects of a shifting composition of local workers or residents. Failing to account for compositional changes in the labor force may thus result in biased estimates of the effects of trade on average CZ-level outcomes such as wages, unemployment rates, or health.

As a first step in exploring heterogeneity across subpopulations, in columns 5 to 8 of table 1, we repeat the specifications from columns 1 to 4, considering instead changes in the log population of those 15 to 34 years old.[15] The costs of mobility are likely lower among the young due to fewer local ties, such as children in school and mortgaged houses. Furthermore, younger individuals have more years over which to amortize any costs of migration. Thus, we would expect more recent entrants into the labor market to be more responsive to shocks than older individuals. As can be seen from columns 5 to 8, this is in fact the case. Across all specifications, the magnitude of the effect of the NTR gap on CZ log population changes is approximately twice as large for those 15 to 34 years old as it was for the entire working-age population. The final column suggests that an interquartile increase in the CZ NTR gap would have reduced population growth by approximately 0.03 log points. Given that the interquartile range of the distribution of changes in log 15- to 34-year-old populations during our sample is 0.14 log points, this is a considerable effect.

To explore differential effects on population growth by education and other demographics, we turn to IPUMS USA. We collect the decennial census long-form samples from 1980, 1990, and 2000 and pool the American Community Surveys from 2009 through 2011 to measure outcomes near 2010. We collect data on respondents' race, ethnicity, sex, age, education, location, and industry of employment. As in the Census, we restrict our attention to persons ages 15 to 64 and aggregate to the CZ.[16]

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[12] The PNTR effect estimates to which we refer when controlling for "neighbor NTR gap" (columns 3, 4, 7, and 8) capture the partial effect of the NTR Gap on a CZ's own population. The coefficient on Neighbor NTR Gap captures another channel of the effect.

[13] We discuss the estimation of the structural break in greater detail in appendix section 9.5.

[14] In unreported results, we further find that our estimates are robust to ending the sample in 2007, prior to the financial crisis. These results are available on request.

[15] Here we control for the lag change in the population of 15- to 34-year-olds, rather than 15- to 64-year-olds. All other controls are identical.

[16] Full discussion of IPUMS samples and aggregation can be found in appendix section 9.6.

TABLE 2.—IMPORT COMPETITION AND 10-YEAR CHANGES IN LOG CZ POPULATION BY DEMOGRAPHIC GROUP, IPUMS

	(1) Male	(2) Female	(3) Hispanic	(4) Black	(5) White	(6) Asian	(7) Less Than High School	(8) High School– Some College	(9) College Graduate
NTR Gap	−0.329**	−0.141	−0.431	0.0534	−0.166	−1.495***	−0.422***	−0.223**	−0.164
× Post 2001	(0.128)	(0.114)	(0.316)	(0.319)	(0.121)	(0.437)	(0.155)	(0.0911)	(0.190)
$\Delta \ln(population_{t-10})$	0.433***	0.581***	0.393***	0.340***	0.506***	0.120**	0.553***	0.504***	0.322***
	(0.0363)	(0.0297)	(0.0574)	(0.0616)	(0.0296)	(0.0501)	(0.0418)	(0.0363)	(0.0348)
Observations	1,444	1,444	1,444	1,426	1,441	1,444	1,444	1,444	1,444
R^2	0.6675	0.7591	0.4586	0.4207	0.3068	0.4659	0.7069	0.7314	0.5186

$N = 1,444$ (722 CZs in two panels). Dependent variable is change in log CZ population of particular demographic group from 1990–2000 and 2000–2010. Data taken from IPUMS (Ruggles et al., 2017). All specifications also include region-time fixed effects and all controls from column 3 of table 1. To avoid mechanical correlation, initial share of the demographic group under consideration is omitted from the regression. NTR gap defined in equation (2). Regressions weighted by 1990 share of national population of relevant group. Standard errors clustered at the CZ and indicated in parentheses. Significant at ***1%, **5%, and *10%.

In table 2 we repeat the specification from column 3 of table 1 for various subpopulations. In columns 1 to 6, we explore changes in the log populations of the following demographic groups: male, female, Hispanic, black, white, and Asian. In columns 7 to 9, we explore variation across education groups. The dependent variable in the three columns is the change in log population of those without a high school diploma (or equivalent), those with at least a high school diploma but no college diploma, and those with at least a college degree, respectively.[17]

Column 1 in table 2 implies significant reductions in the male population growth in response to PNTR exposure. The point estimate for men is slightly larger than the estimate for the working-age population as a whole and is statistically significant at the 5% level. The point estimate for women is roughly half that for the working-age population and is statistically insignificant. This is consistent with the fact that men are disproportionately likely to be employed in manufacturing industries, which were most directly exposed to increased import competition.

Hispanic, black, and white groups do not exhibit statistically significant population reductions. While we do observe a large effect for the Asian population, we note that the average share of the population in any CZ with Asian individuals is 0.7%, making small population adjustments relatively large in log changes. Furthermore, the Asian population is highly clustered in relatively few CZs, making these estimates highly dependent on a small number of observations.

The heterogeneous response across educational groups is striking. The population changes are significantly negative for the lower two education groups—those without a college degree. Column 8 implies that an interquartile increase in the NTR gap would have reduced population growth for those with a high school diploma but no college degree by 0.013 log points. This estimate is statistically significant at the 5% level. The interquartile effect of PNTR on population changes among those without a high school diploma

is nearly twice that and is statistically significant at the 1% level. The coefficient on PNTR for those without a high school diploma implies that an interquartile movement in PNTR exposure would result in a 0.025 log point reduction in growth among this population. This effect accounts for over 10% of the interquartile change in CZ log populations for those without a high school diploma from 2000 to 2010.

These repeated cross-section data cannot allow us to test directly whether the observed population adjustments occurred among individuals displaced from their jobs by PNTR exposure. However, given that PNTR operated primarily through the manufacturing sector, the results do suggest that the population adjustments were strongest among populations directly affected by it. Indeed, among the 15- to 64-year-old population, those without a college degree and men accounted for 81% and 65% of manufacturing employment, respectively, prior to the granting of PNTR in 2000.[18]

In table 3, we further investigate heterogeneity in population adjustments to PNTR exposure by both age and education. Column 1 displays four sets of estimates of equation (3), where log population changes refer to individuals with less than a high school diploma and each row corresponds to a distinct age group. Columns 2 and 3 similarly report results by age group for high school graduates, including those with some college attendance, and bachelor's degree holders, respectively.

Our results in table 2 suggest that local population growth among young adults is particularly responsive to import competition. In table 3, we find that this result was driven by individuals at all education levels. In the first row of the table, we see that population adjustments among 25- to 34-year-olds were, if anything, stronger among college graduates than among their less-educated counterparts. Across all educational groups, we find economically meaningful and statistically significant reductions in population growth in this age group. The table also suggests that the population adjustment among the least-educated individuals was not driven solely by the youngest individuals. In column

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[17] In columns 7 to 9, we further restrict our sample to those ages 25 to 64 to avoid conflating migration with human capital adjustments (e.g., increased high school graduation) such as those documented by McHenry (2015), Greenland and Lopresti (2016), and Feler and Senses (2017).

[18] Manufacturing is defined as Census IND1990 codes 100–399. These shares refer to manufacturing employment among currently employed, noninstitutionalized persons ages 15 to 64.

TABLE 3.—IMPORT COMPETITION AND TEN-YEAR CHANGES IN LOG CZ
POPULATION BY AGE AND EDUCATION

	(1) Less Than High School	(2) High School– Some College	(3) College Graduate
All persons ages 25–34			
NTR Gap	−0.926***	−0.681***	−1.032***
× Post 2001	(0.274)	(0.156)	(0.340)
R^2	0.4439	0.7381	0.3276
All persons ages 35–44			
NTR Gap	−0.778***	−0.117	−0.156
× Post 2001	(0.260)	(0.133)	(0.184)
R^2	0.8417	0.9017	0.2852
All persons ages 45–54			
NTR Gap	−0.327	−0.0225	0.177
× Post 2001	(0.204)	(0.149)	(0.202)
R^2	0.6179	0.7328	0.8764
All persons ages 55–64			
NTR Gap	0.00765	−0.271**	−0.411**
× Post 2001	(0.217)	(0.114)	(0.177)
R^2	0.6604	0.7981	0.6095

All specifications include all controls from column 3 of table 1. Regressions weighted by 1990 share of national population of relevant group. Standard errors clustered at the CZ and indicated in parentheses. Significant at ***1%, **5%, and *10%.

1, we observe large and statistically significant reductions in the population growth of adults without a high school diploma for the 25–34 and 35–44 age groups. The corresponding point estimate for the 45–54 age groups is also sizable, although the confidence interval includes 0.

Finally, we also document significant population reductions among the 55–64 age group for those with at least a high school diploma. It is possible that low-education individuals among younger age groups were directly affected by import competition, while more educated individuals in the 55–64 age group responded to the indirect effects on local conditions—crime, public services, and so on. The latter result is consistent with a literature finding that more-educated individuals are more geographically mobile (Bound & Holzer, 2000; Wozniak, 2010). Furthermore, we note that population adjustments for higher age-education cells may feature individuals with substantially different jobs from those of recent college graduates, as individuals in this category have labor market experience dating back to the 1970s.

Insofar as young adults and those with low levels of education are likely to have lower-than-average wages, the selective population adjustment we document here suggests that prior studies may have understated the effect of import exposure on average CZ wages. That is, had low-wage workers not left or avoided affected CZs, the measured CZ wage declines may have been even larger in magnitude. We note that while in the case of wages, it seems possible that the compositional shift in the labor force may have attenuated estimates of the effect of import exposure, this need not be the case for other outcomes of interest. Researchers should take care in considering how such selective migration may affect estimates in their own context.

IV. Individual-Level Migration Responses

Thus, there is considerable evidence that local populations responded to the rising import competition that followed the granting of PNTR to China. In this section, we turn to evidence on migration patterns at the level of the individual. Specifically, we estimate the effect of increased import competition on migration among young adults using individual-level microdata from two data sets provided by the Department of Education: the National Education Longitudinal Study of 1988 (NELS:88) and the Education Longitudinal Study of 2002 (ELS:2002). By using these two sequential microlevel longitudinal data sets, we are able to account for individual-level factors that are unobservable in more aggregate data and may confound our ability to estimate migratory patterns. These data follow individuals between high school and age 26. As shown in the previous section and has been documented elsewhere in the literature (McHenry, 2014), young adults are particularly responsive to local economic conditions, making these data especially useful for our application. Because of the sequential nature of the panels, we are also able to include labor market and time period fixed effects, exploiting variation in out-migration over time within CZs. Specifically, our empirical specifications explore whether the latter cohort (ELS:2002) became more likely to move away from their origin CZ than the earlier cohort (NELS:88) in the same CZ as the extent of exposure to trade, as measured by the NTR gap, rose in the CZ.

The NELS:88 began with a nationally representative sample of eighth graders in U.S. schools in 1988. A follow-up survey was fielded in 1990 to a freshened sample that allows us to use a representative sample of tenth graders in U.S. schools in 1990. The final follow-up survey was fielded in 2000, when respondents were approximately 26 years old.[19] Using school and residence zip codes in the restricted-access version of the NELS:88, we assign to each sample member the CZ where she attended the tenth grade when respondents were approximately 16 years old. We also employ a residence zip code variable to identify the CZ in which each respondent resided in 2000, at age 26. Our main migration variable is an indicator for living in different CZs in tenth grade and at age 26.

The ELS:2002 is the next secondary school cohort study in the series of Department of Education data sets. It began with a nationally representative sample of tenth graders in U.S. schools in 2002. The third follow-up survey was fielded in 2012 when respondents were approximately 26 years old. As with the NELS:88, we use the restricted access version of the ELS:2002 in order to identify students' residence CZs in tenth grade and at age 26.[20] And as in the NELS:88, we define migration as an indicator for living in different CZs in tenth grade and at age 26. Therefore, we have two

[19] For details regarding the sampling procedure, see Curtin et al. (2002).
[20] For details regarding the sampling procedure, see Ingels et al. (2014).

longitudinal data sets following students at the same life cycle period, conducted twelve years apart. Importantly for our empirical strategy, the NELS:88 sample was concluded prior to 2001. Thus, tenth graders in the NELS:88 sample were entirely unexposed to PNTR. However, tenth graders in the ELS:2002 sample were affected by rising import competition from China, as PNTR was granted immediately prior to this sample period.[21]

Our focus here on migration of relatively young adults reduces the generalizability of results somewhat. We will not observe midcareer migration in response to trade-associated job loss, for example. Rather, the migrants in our NELS:88 and ELS:2002 samples will be young adults who anticipate location-specific trends in career and other opportunities and migrate accordingly. Further, the NELS:88 and ELS:2002 sampling frames do not include students originating in all CZs, so we are not able to use these data to estimate total population adjustments for all locations as in the prior section. While the NELS:88 and ELS:2002 are designed to yield nationally representative samples, the samples are not necessarily representative at the CZ level.[22]

However, while we observe NELS:88 and ELS:2002 respondents around the life-cycle time of college attendance choices, our results are not likely to be driven by college attendance. An individual is reported as having migrated only if she was in a different CZ at age 26 than in tenth grade. Individuals who attended college in a different CZ and returned to their tenth-grade CZs by age 26 are thus not counted as having migrated. By age 26, a large majority of individuals appear to have completed postsecondary education. Only 24.5% of respondents were enrolled in any postsecondary education program in the ELS:2002 2012 survey, and only 15.4% were enrolled in programs at four-year colleges (where attendance is most likely to influence geographic mobility). Thus, to the extent that the decision to attend college affects our results in the NELS:88 and ELS:2002 data, it is largely through individuals attending college and permanently leaving their home CZ. This type of migration would play an important role in shaping the skill and age distribution of local labor markets. Finally, as discussed in section IIIA, among young adults in the IPUMS data, we observe substantial migration at all education levels, suggesting that the choice to attend college is not the primary driver of our results.

Our NELS:88 and ELS:2002 samples include approximately 9,990 and 12,280 respondents, respectively.[23] Approximately half of the respondents are male, and the race

and ethnicity proportions are similar to population measures. We have access to test scores in reading and mathematics from standardized tests taken in tenth grade. We convert the scores into percentiles within each sample distribution (separately for NELS:88 and ELS:2002). We also have detailed controls for family background, including parents' education and family income. The original measures of family income are in bins, and we impute bin midpoints to dollar values and convert them to 2002 dollars for both surveys. We also observe the foreign-born status of respondents and their parents. As noted above, the NTR gap is 0 by definition for all NELS:88 respondents, as the final wave of the survey was conducted prior to the granting of PNTR. The average CZ NTR gap measure is 0.06 for the (post-PNTR) ELS:2002 respondents.

Table 4 displays our baseline results from this approach. Each column includes results from a separate regression for which the dependent variable is an indicator for the respondent living in a different CZ from her tenth-grade location at age 26. All specifications include CZ fixed effects, so we identify import competition effects from changes over time within origin locations—that is, between the NELS:88 cohort starting in 1990 and the ELS:2002 cohort starting in 2002. We also control for time fixed effects by including a dummy variable equal to 1 during the ELS:2002 sample and include the interactions of this time dummy with Census division of origin (nine categories). Additionally, in each specification we control for basic individual-level demographic variables, including sex, race, and ethnicity. Standard errors are clustered at the tenth-grade CZ level.

In each column in table 4, we include different individual-level controls for academic ability and family background. It is possible that import competition was more pronounced in places with young people who were naturally more geographically mobile due to their family backgrounds, including whether their parents were foreign born. Again, microdata allow us to observe migration choices coincident with trade shocks while controlling for just such factors. The column 1 specification controls for reading and math test scores in the tenth grade. Conditional on test scores, the estimated effect of NTR gap on migration is large and implies a sizable out-migration effect of local import competition: an interquartile increase of the NTR gap predicts an increased migration likelihood of 2.7 percentage points.[24] Column 2 includes controls for family background, including parents' education (indicators for high school, some college, bachelor's degree, and master's degree or more, defined by the higher of the parents' completed schooling) and family income. Children of more-educated and higher-income parents are relatively geographically mobile, and these controls if anything increase the relationship between

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[21] Due to data constraints, we measure ELS:2002 origin locations in 2002, one year after the beginning of the PNTR episode. To the extent that students' families move away from shocked areas immediately, our estimates will understate out-migration effects.

[22] We have estimated versions of our main specifications that weight the regressions by the number of NELS:88 or ELS:2002 respondents in a CZ to confirm that results are not driven by small cells. The results of those specifications are similar to those reported below.

[23] Table A6 in the appendix displays the sample sizes and means of main variables for both surveys. Note that sample sizes are rounded to the nearest 10 due to confidentiality restrictions.

[24] The NTR gap variable in the ELS:2002 sample has a mean of 0.06 and an interquartile range of 0.033. Note that the interquartile range is different from the one described in section III due to the fact that we have incomplete CZ coverage in the NELS/ELS data. In particular, in the ELS:2002, only about 300 CZs contain at least one respondent.

TABLE 4.—IMPORT COMPETITION AND MIGRATION BETWEEN GRADE 10 AND AGE 26

	(1) Migrate	(2) Migrate	(3) Migrate	(4) Migrate	(5) Migrate
NTR Gap	0.8262***	1.034***	1.338***	1.053***	0.8185***
× Post 2001	(0.2689)	(0.2659)	(0.2997)	(0.2764)	(0.2673)
ELS:2002	−0.0871*	−0.1026*	−0.1287*	−0.1082*	−0.0901
	(0.0528)	(0.055)	(0.0703)	(0.0631)	(0.0555)
Test score controls	Yes	No	No	No	Yes
Parent education and income controls	No	Yes	No	Yes	Yes
Foreign-born controls	No	No	Yes	Yes	Yes
Observations	21,810	21,000	19,470	19,160	18,830
R^2	0.0627	0.0488	0.0108	0.0514	0.0802

Data from NELS:88 and ELS:2002. Dependent variable in all specifications is an indicator for living at age 26 in a different CZ from tenth-grade residence. All specifications include CZ fixed effects, Census division (nine categories) by year effects, and controls for male, Hispanic, black, and Asian. Standard errors clustered at tenth-grade CZ level and indicated in parentheses. Significant at ***1%, **5%, and *10%.

local import competition and out-migration. Immigrants are a relatively mobile population (Cadena & Kovak, 2016), so we control for foreign-born status of parents (separately for mother and father) and child in column 3. As in previous columns, the positive relationship between the NTR gap and out-migration is robust to these individual controls. In columns 4 and 5, we show that the effect is robust to various combinations of these controls. In particular, in column 5, we include all of the individual-level controls from columns 1 through 3 and find a large positive and statistically significant effect of import competition on migration.[25]

The above results describe out-migration and suggest that young people tended to move away from trade-shocked locations. However, this does not shed any light on the ultimate destination of these individuals and whether out-migrants settled in locations that were less affected by trade liberalization. In order to explore this issue more fully, in appendix section 9.3, we describe results from conditional logit models of location choice. These models demonstrate that in choosing destinations, ELS:2002 sample members also avoided locations with higher values of NTR gap. That is, not only did young people move away from trade-shocked areas, but they avoided settling in them as well.

As with the Census results, we are interested in whether various groups responded differently to import competition shocks. With this in mind, table 5 reports coefficients on the NTR gap in regressions of tenth grade to age 26 CZ migration among different subsamples. Each specification includes time and CZ fixed effects; Census division-by-year fixed effects; and respondents' sex, race, and ethnicity. As the table shows, the mobility response among men was larger than among women. This is consistent with the Census results in the previous section and the larger concentration of men within the manufacturing industry, which faced the greatest increase in import competition. The subsample sizes appear to be underpowered to infer differential effects by race and ethnicity, but the clearest effect is among non-Hispanic white respondents.

TABLE 5.—IMPORT COMPETITION AND MIGRATION BY RESPONDENTS' TRAITS

	Coefficient	SE	Number
Male	1.559***	(0.4098)	10,470
Female	0.8708**	(0.3583)	11,780
Black, non-Hispanic	0.2564	(0.6212)	2,480
Hispanic	1.158	(1.109)	2,890
White, non-Hispanic	1.396***	(0.3862)	15,100
Foreign born	−0.9326	(1.44)	1,620
Foreign-born parent	−0.0011	(0.7871)	3,910
Parents no postsecondary education	1.068***	(0.3733)	5,810
Parents some postsecondary education	0.4915	(0.3466)	8,010
Parents B.A.	1.54***	(0.5703)	8,190
Family income less than $40,000 (in 2002 dollars)	1.091**	(0.479)	6,800
Family income more than $40,000 (in 2002 dollars)	1.152***	(0.3454)	14,210
Below median reading test	0.564*	(0.3196)	9,660
Above median reading test	1.335***	(0.3719)	12,180
Below median math test	0.6493**	(0.3057)	9,460
Above median math test	1.341***	(0.4235)	12,370

Data from NELS:88 and ELS:2002. Dependent variable is an indicator for living at age 26 in a different CZ from tenth-grade residence. Table shows only the coefficient on CZ NTR Gap interacted with an indicator for ELS:2002 respondents. All models include a constant, indicators for being in the ELS:2002 (rather than NELS:88), sex, race/ethnicity, CZ fixed effects, and Census division (nine categories) by year effects. Standard errors clustered at tenth-grade CZ level and indicated in parentheses. Significant at ***1%, **5%, and *10%.

The third panel of table 5 displays estimates with subsamples of foreign-born respondents and those with foreign-born parents. Results in Cadena and Kovak (2016), suggest that foreign-born children may be particularly geographically mobile in response to local economic shocks. Estimates with those subsamples are imprecise; again, insufficient statistical power may be an issue. The fourth and fifth panels of table 5 examine the relationship between family background and mobility responses to trade. Results by parents' education and family income here are somewhat mixed. The strongest out-migration responses are among respondents with either the least-educated or the most-educated parents. This could reflect two competing factors related to the positive correlation between parents' and children's schooling. First, students with less-educated parents are more likely to attain less education themselves and thereby face more competition for jobs due to import competition from China. Second, students with more-educated parents are more likely to attain more education themselves and thereby become

[25] Appendix table A7 shows the specifications in table 4 with their full sets of controls. In appendix table A8, we also show that the out-migration result is robust to the inclusion of time-varying controls for CZ characteristics.

more geographically mobile and sensitive to local labor demand shocks overall (Wozniak, 2010). We see similarly sized migration responses to trade among those with more and less family income.

Finally, consistent with greater sensitivity to local conditions among the more educated, the last panel of table 5 shows that students with higher tenth-grade standardized test scores, in math and reading, were more likely to move away from trade-shocked areas. While this finding contrasts the larger general population adjustments among less educated persons documented in table 2, it is consistent with the strong migratory reaction observed for those under age 35 with a college education.

V.   Dynamic Population Effects

Our results thus far suggest that over the course of a decade, individuals responded to the increased import competition caused by the granting of PNTR to China, shifting populations away from the most exposed locations. However, these results do not shed light on the dynamics of the adjustment process, in particular the speed at which it occurred. As noted above, existing research suggests that the response to trade shocks may be sluggish. From a welfare perspective, the time it takes a labor market to adjust to economic shocks matters considerably. Furthermore, the timing of adjustment is important for estimation. To the extent that much of the response to such shocks takes place at a lag, approaches considering only contemporaneous or short-run responses to trade may understate the true effect. To examine this issue, we turn to annual data on county-to-county migration, taken from the IRS. These data afford us the opportunity to track changes in CZ populations over time, something that was not possible in either the decennial Census or NELS/ELS data.

For each year between 1990 and 2013, the IRS provides information about 1040 filings in each county. The data include the number of returns filed and the number of exemptions claimed in each county in the United States. Returns measure the number of households, while exemptions serve as a measure of the population. We observe the number of exemptions and returns at the county level in each year in the sample, except when suppressed to ensure the anonymity of filers.[26]

By way of comparison with our previous results, in appendix table A12 we consider ten-year changes in log CZ exemptions and returns as a function of the NTR gap. Specifically, we estimate equation (3) with IRS data and include the same controls as column 4 of table 1; we find similar

results in the IRS data to those obtained using Census population counts. However, our primary interest using the IRS migration data is to examine the dynamics of the population response to import competition over time rather than simply the long difference. To do so, we specify a distributed lag model in which we allow the effect of the NTR gap to vary over time. Specifically, we estimate the following equation:

$$
\Delta \ln(population_{ct})
$$
$$
= \eta_0 + \sum_{l=0}^{T} \tau_l \Delta_{t-l}(NTR\ Gap_c \times Post2001_t)
$$
$$
+ \eta_1 X_c \times Post2001_t + \eta_2 \Delta \ln(population_{ct-1})
$$
$$
+ \delta_{rt} + \omega_{ct}. \tag{5}
$$

That is, we allow the effect of $\Delta(NTR\ Gap_c \times Post2001_t)$ to persist for up to T years after 2001. In results that follow, we set T equal to 12, implying that full adjustment to the shock may take as many as thirteen years (the contemporaneous effect plus twelve lags). As above, we additionally control for a vector X_c of CZ variables as of 1990 interacted with $Post2001_t$. That is, we allow for post-2001 deviations from CZ trends as a function of preexisting variation in CZ controls, which include the 1990 values of the following: CZ capital-labor ratio, skill intensity, the female share of the labor force, the share of the population with a college degree, average task routineness, and offshorability. We additionally control for 2001 CZ debt-to-income ratios and the magnitude of the structural break in CZ housing prices, as already described. As before, we control for CZ trends and macroeconomic shocks through (one-year) lagged population changes and region-year fixed effects.

Figure 1 displays the results of this regression for returns and exemptions separately. The two upper panels display the lag distribution—that is, the marginal effect of the NTR gap in each year, from contemporaneous to twelve years after the shock. For each possible lag, the point estimate in these panels corresponds to its τ_l in equation (5). The lower panels depict the long-run cumulative effects—the sum of the marginal effects.
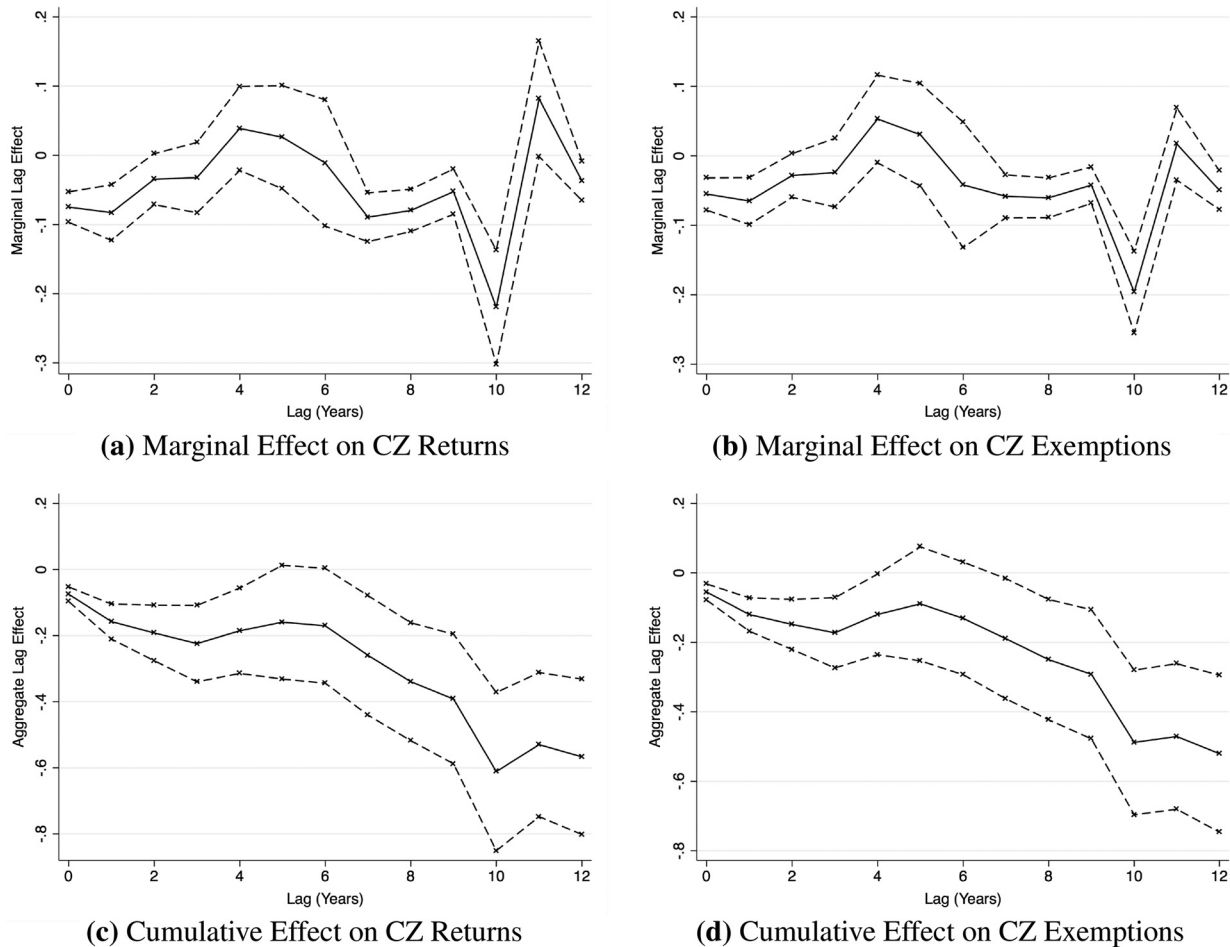
As is clear from the figure, the IRS data reveal a cumulative effect very similar to the one found in the Census and IPUMS data sets. The thirteen-year (contemporaneous plus twelve lags) cumulative point estimate using exemptions is -0.52. Recalling that the interquartile value of the NTR gap is approximately 0.059, this suggests that CZs experiencing an interquartile increase in the NTR gap would have seen population growth reduced by approximately 0.031 log points twelve years later. The corresponding value using data on returns is approximately 0.033 log points.[27] Each of these results is statistically significant at the 1% level.[28]

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[26] Data, available at https://www.irs.gov/uac/soi-tax-stats-migration-data are suppressed when the flows are fewer than ten between any county-county pair for 1990–2012 and when flows are fewer than twenty between any county-county pair in 2013. In such cases, data are aggregated and provided by the IRS as returns filed in nonspecific "other regions." In such cases, we assume that returns filed in other regions were filed in separate CZs.

[27] The analogous results for ten-year changes, including the contemporaneous response and nine lags, imply a reduction in population growth of 0.017 log points for exemptions and 0.023 log points for returns.

[28] The specification described in equation (5) involves estimating a large number of parameters. In appendix section 9.4.2, we describe how an alternative specification with a more constrained lag structure (and thereby fewer

FIGURE 1.—UNCONSTRAINED DISTRIBUTED LAG MODEL OF NTR GAP AND LOG POPULATION

(a) Marginal Effect on CZ Returns

(b) Marginal Effect on CZ Exemptions

(c) Cumulative Effect on CZ Returns

(d) Cumulative Effect on CZ Exemptions

The figure displays coefficients from estimation of the distributed lag log population growth model, as described in equation (5). The top row corresponds to estimated marginal effects and the bottom to aggregate effects. Data are taken from the IRS annual county-to-county migration files. Returns proxy for households, and exemptions proxy for population counts.

The process of adjustment depicted in figure 1 is note-worthy. There is a negative and statistically significant but relatively muted response to PNTR contemporaneously or at an immediate lag. The majority of the effect is felt at a lag of seven or more years after the policy shift. This is consistent with previous work on labor market adjustments to trade shocks (Artuc et al., 2010; Dix-Carneiro, 2014) that finds that full reallocation takes time—up to a decade or more. However, the majority of this literature finds a deceleration in the adjustment process over time, while we document an increased response at a lag of several years. This is related to the findings of Dix-Carneiro and Kovak (2017), who document an accelerating effect of trade liberalization on wages in Brazil over a period of two decades. However, the mechanism in their paper is quite different. They argue that sluggish labor mobility in the face of a labor demand shock, coupled with slow capital adjustment, leads to an acceleration

in wage declines over time. Separately, figure 4 in Pierce and Schott (2016a) shows a growing effect of PNTR with China on manufacturing employment in the United States over time, which is also consistent with lagged population adjustments if households move after job loss. The details of the dynamic response to PNTR, and to trade shocks more broadly, remain an interesting and important topic for future research.

The seven- to ten-year lag after PNTR with China corresponds with the Great Recession. Internal migration is historically procyclical in the United States (Saks & Wozniak, 2011), and the Great Recession clearly influenced migration behavior. In response to the potential concern that the Great Recession, rather than import competition, drove the population adjustments we observe, we make the following observations. First, our specification controls for local factors that may explain differential effects of the Great Recession, such as CZ debt-to-income ratios and the magnitude of the structural break in CZ housing prices. In addition, our results come from comparisons between CZ trends, so for the Great Recession to influence our results it would

need to influence CZs differentially. Finally, as noted above, our baseline Census estimates are robust to excluding years after 2007 from our analysis, so we do observe significant population adjustments to PNTR that predated the Great Recession.[29] Even so, we do acknowledge that it is possible that the Great Recession shock played some role in accelerating preexisting tendencies for population adjustments away from already-trade-shocked local labor markets.

In addition to confirming our earlier results regarding the aggregate migration response to PNTR, these results suggest that timing matters a great deal for estimation. Approaches that consider only contemporaneous or short-term responses to trade shocks are likely to underestimate the aggregate effect. In a setting in which the barriers to mobility are considerable, much of the readjustment occurs at a considerable lag. We have also noted that selective migration should influence the interpretation of empirical results in this literature. Our results in this section imply that this bias is likely to be more acute over longer time horizons. Inferences using short-run changes are less likely to be biased by selective migration, but estimated local labor market effects over seven or more years may reflect compositional changes, since the local population generating data is likely to have changed.

VI.   Comparison with Existing Literature

Finally, we compare our results to those obtained following the approach of Autor et al. (2013), who similarly examine the effect of increased import competition from China on local U.S. labor market outcomes. Notably, Autor et al. (2013) do not find any evidence of population adjustment in response to rising import competition. We seek here to understand the distinction between our results and theirs. Autor et al. (2013) measure imports per worker at the CZ level as follows:[30]

$$\Delta IPW_{uit} = \sum_j \frac{L_{ijt}}{L_{it}} \frac{\Delta M_{ucjt}}{L_{ujt}}. \tag{6}$$

Changes in imports per worker in CZ i at time t, ΔIPW_{uit}, are calculated as the weighted average of changes in imports per worker at the national level across all industries j, $\frac{\Delta M_{ucjt}}{L_{ujt}}$, where each industry's weight is equal to its labor share in CZ i, $\frac{L_{ijt}}{L_{it}}$.

It is possible that import exposure was correlated with local economic conditions that would also affect migration. For example, if industrial composition—and therefore import exposure—was correlated with recent changes in

amenities, OLS results would be biased. In order to isolate the trade shock driven by supply-side shifts, Autor et al. (2013) create an instrument for changes in import competition, employing changes in Chinese imports in eight other high-income countries, ΔM_{ocjt}.[31] In addition to using imports in countries other than the United States, Autor et al. (2013) lag industry-specific labor shares by ten years to mitigate concerns that labor markets anticipated rising trade. This yields the following instrument:

$$\Delta IPW_{oit} = \sum_j \frac{L_{ijt-10}}{L_{it-10}} \frac{\Delta M_{ocjt}}{L_{ujt-10}}. \tag{7}$$

The authors then examine the CZ population response to import competition in the following specification:[32]

$$\Delta L_{it} = \gamma_t + \kappa_r + \beta_1 \Delta IPW_{uit} + X'_{it}\beta_2 + e_{ct}. \tag{8}$$

The paper examines changes between 1990 and 2000 and between 2000 and 2007 in the log counts of the following population groups: ages 16 to 64, 16 to 34, 35 to 49, and 50 to 64 years old; individuals without a college degree; and individuals with a college degree. The specification controls for time and Census division fixed effects, the share of employment accounted for by manufacturing, the share of the population with a college degree, the foreign-born share of the population, the female labor force share, the routine task share, and the offshorability index. Standard errors are clustered at the state level.

We report results from estimating equation (8) in row 1 of table 6 below. This row replicates results in panel C from table 4 of Autor et al. (2013). They do not find evidence of population adjustment in response to rising import competition. While the identification strategies in their paper and ours are distinct, both attempt to capture the effects of rising import competition on labor market outcomes. Thus, the lack of a migratory response in their approach is surprising, given our results already described. To explore this further, we consider several differences between their approach and ours.

First, we note that Autor et al. (2013) do not account for preexisting trends in CZ populations, while we do. In the second row of table 6, we repeat their specification but introduce ten-year lagged changes in the log population for the relevant group.[33] As lagged population changes directly capture trends at the CZ level, we drop region fixed effects from these specifications.[34] As is clear from the row, con-

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[29] We also find evidence in the ELS:2002 cohort of migration between tenth and twelfth grades away from trade-shocked CZs (table A9 in the Appendix). This is consistent with parents moving their families away from import competition, and those moves occurred between 2002 and 2004, prior to the Great Recession.

[30] The u subscript refers to the United States, and the c subscript refers to China, so M_{ucjt} is U.S. imports from China in industry j at time t.

[31] The subscript o refers to "other." The eight other countries are Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland.

[32] We reuse β here to align the notation with Autor et al. (2013), but β_1 in equation (8) is not the same parameter as in equation (3).

[33] The lag variable measures CZ log population change between 1980 and 1990 (associated with 1990–2000 outcome variable observations) and between 1990 and 2000 (associated with 2000–2007 outcome variable observations). We explore the relationship between preexisting population growth and exposure to Chinese imports in appendix 9.2.

[34] Region fixed effects in a growth equation like equation (8) capture population trends over the sample period that are specific to regions. Lagged

TABLE 6.—IMPORT COMPETITION AND CHANGES IN LOG CZ POPULATION, AUTOR ET AL. (2013) METHOD

	(1) 16–64	(2) College	(3) No College	(4) 16–34	(5) 35–49	(6) 50–64
1990–2007 Autor et al. (2013)						
ΔIPW_{uit}	−0.050	−0.026	−0.048	−0.138	0.367	−0.138
	(0.746)	(0.685)	(0.823)	(1.190)	(0.560)	(0.651)
1990–2007 Autor et al. (2013) with $\Delta\ln(population_{t-10})$						
ΔIPW_{uit}	−0.709	−0.592	−0.710	−1.039	−0.194	−0.530
	(0.485)	(0.582)	(0.483)	(1.037)	(0.329)	(0.527)
1990–2007 Census data with $\Delta\ln(population_{t-10})$						
ΔIPW_{uit}	−0.632	–	–	−1.283**	0.023	−0.175
	(0.407)			(0.601)	(0.405)	(0.473)
1990–2010 Autor et al. (2013)						
ΔIPW_{uit}	−0.153	0.0394	−0.383	−0.854	0.702	0.114
	(0.776)	(0.676)	(0.902)	(0.942)	(0.748)	(0.798)
1990–2010 Autor et al. (2013) with $\Delta\ln(population_{t-10})$						
ΔIPW_{uit}	−0.806*	−0.558	−1.041**	−1.491**	−0.060	−0.475
	(0.426)	(0.551)	(0.453)	(0.731)	(0.443)	(0.611)
1990–2010 Census data with $\Delta\ln(population_{t-10})$						
ΔIPW_{uit}	−0.701*	–	–	−1.353**	−0.023	−0.345
	(0.390)			(0.646)	(0.399)	(0.506)

$N = 1,444$ (722 CZs in two panels). Dependent variable is change in log CZ population. All specifications include controls from Autor et al. (2013). Population counts for rows 1, 2, 4, and 5 come from IPUMS USA. Population counts for rows 3 and 6 come from the Census Bureau intercensal estimates, which also include 15-year-olds. All specifications weighted by start-of-period CZ population shares. Standard errors clustered at the state and indicated in parentheses. Significant at ***1%, **5%, and *10%.

trolling for population trends alters the results substantially. To the extent that CZs previously experiencing greater population growth were more exposed to import competition, failing to account for such pretrends will bias estimates of the effect of import competition on population growth upward. As suggested by the second row and as we further discuss in appendix 9.2, this seems to have been the case. While the results are not statistically significant at conventional levels, the point estimates are consistently negative and are an order of magnitude larger than in the first row.

Because a primary focus of Autor et al. (2013) is on employment and wage outcomes, they do not use Census population data, which do not include employment and wage variables. Autor et al. (2013) instead use IPUMS data, which include information on labor market outcomes for nationally representative samples of individuals. However, IPUMS data are samples of the population, rather than the complete population and do not always allow the identification of a respondent's precise CZ of residence.[35] Both features imply that measures of CZ populations may be noisier in the IPUMS data than in Census population counts. To explore this possibility, in the third row, we repeat the specification from the second row, using data from intercensal estimates from the complete Census rather than IPUMS.

These data are not available by education group, so we are able to explore variation only by age category. In addition, we explore changes in the 15–64 and 15–34 age groups rather than 16 to 64 and 16 to 34. As is clear from this row, point estimates employing Census data are similar to those obtained using IPUMS data. However, standard errors for the 15–64 and 15–34 age groups are reduced, and we find statistically significant evidence of a reduction in the population of 15- to 34-year-olds. These results are much more similar to the results we report in this paper. The interquartile range of ten-year equivalent changes in CZ import competition between 2000 and 2007 in thousands of dollars per worker is approximately 2.56.[36] Thus, the point estimates suggest that an interquartile increase in import competition would have reduced population growth among 15- to 34-year-olds by approximately 0.033 log points. This estimate is quite similar to the one obtained in our baseline specifications.

In addition to controlling for population trends and employing Census count data, a potentially important distinction between our paper and Autor et al. (2013) is the fact that their paper examines labor market and population adjustments through 2007, while we extend the analysis to 2010. This is especially relevant given that our results in section V suggest that much of the population adjustment to rising import competition occurred at a considerable lag. To explore the importance of including additional years, we re-create the Autor et al. (2013) measure to include changes in imports per worker between 2000 and 2010 rather than between 2000 and 2007. We then reestimate the first three rows of table 6, exploring population changes between 1990 and 2000 and between 2000 and 2010. As can be seen in row 4 (the first row of the second panel), changing the years alone does not meaningfully alter the Autor et al. (2013) conclusion. Repeating their specification exactly, altering only the years considered leaves their result largely unchanged: there is no statistically significant evidence of population adjustment in response to rising import competition. However, when we introduce controls for population trends, we find results that are very similar to ours. In rows 5 and 6 (with both IPUMS data and Census count data), we find statistically significant reductions in population growth of working-age individuals (column 1) and of the young (column 4), with an effect among the younger group that is approximately twice as large as the total working-age population effect. In addition, in column 3, we find evidence of a reduction in the population growth of individuals without a college degree.

Thus, when controlling for population trends following the Autor et al. (2013) approach, we find evidence of population adjustments among the young. When additionally extending the analysis through 2010, we find evidence of reductions in growth among the aggregate working-age

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CZ population changes also capture population trends and do so more flexibly (i.e., allowing a variety of trends within regions). Therefore, we prefer controls for lagged CZ population changes rather than region fixed effects. We also estimated versions of equation (8) that are similar to those in table 6 but include region fixed effects or region-by-decade fixed effects; the results are somewhat smaller and noisier than those in table 6 but still consistent with population reductions associated with import competition.

[35] IPUMS data identify the PUMA of residence, and PUMAs sometimes overlap CZ boundaries.

[36] This is the unweighted interquartile range. Weighting by CZ population share, the analogous value is 1.51, implying a seven-year interquartile range of 1.06, as in Autor et al. (2013).

population and individuals with low levels of education. The interquartile range of changes in import competition between 2000 and 2010 in thousands of dollars per worker is approximately 1.92.[37] The point estimates in the final row of the table suggest that an interquartile increase would have reduced CZ population growth by 0.013 log points for the entire working-age population and by 0.026 log points for the 15–34 age group. Again, these estimates are strikingly similar to those we report in our main results. This suggests that the primary distinction between the Autor et al. (2013) population adjustment results and ours is the inclusion of lagged population growth in our approach and, to a lesser extent, the inclusion of data through 2010. Our use of complete Census count data as opposed to samples from IPUMS also seems to play a small role.

VII.   Conclusion

Recent evidence regarding the costs of Chinese export growth has highlighted the fact that for a sizable portion of the population, the losses from trade may be substantial. In response to such losses, the most exposed workers and labor markets must adjust. This paper sheds light on this adjustment by examining the change in migration patterns across U.S. local labor markets in response to the granting of Permanent Normal Trade Relations to China in 2001. We exploit variation in exposure to the policy change using data on preexisting industrial structure at the commuting zone level. Using a variety of data sets on population changes and migration flows, we find evidence of substantial internal migration responses to the shock.

Specifically, using population data from the Census, we find that as import exposure rose, population growth fell. This suggests that individuals moved away from or avoided more exposed areas. This effect was most pronounced among the young, men, and low-education groups. We validate these findings in individual data from two longitudinal studies from the Department of Education in which we observe that among young adults, there is an increase in out-migration and avoidance of the most exposed CZs. We then assess the speed of adjustment by employing a distributed lag model to examine mobility across CZs using IRS tax filings between 1990 and 2013. We find that the majority of the change in population occurred at a lag of seven to ten years. Finally, we contrast our results with those in the existing literature on the effects of Chinese import competition and find that controlling for trends in population growth is important for identifying population adjustments. Taken as a whole, our results provide positive evidence of labor market adjustments in response to the recent surge in Chinese exports to the United States. In the face of large and geographically concentrated costs of trade, workers seek out markets that are less negatively affected by import competition, in a manner consistent with theory.

These results have implications for both the welfare of affected communities and for researchers. Due to the compositional changes in the labor force of exposed markets, attempts to identify causal effects of import competition on labor market outcomes may be confounded by simultaneous changes in outcomes due to demographic shifts. Consider Autor et al. (2013), who find evidence that import exposure led to reductions in average CZ household income from 1990 to 2007. Our findings indicate that the population of younger and less educated residents, who tend to have lower incomes, disproportionately fell in more exposed locations at the same time. The population adjustment would have, by itself, increased average household income and biased Autor et al. (2013) against finding an effect of import exposure on income. This suggests that the trade-related household income effect may have been even larger than they originally documented.

While the previous example pertains to average household income, all studies analyzing the effect of changes in import competition on changes in average local population characteristics (e.g., wages, employment, health, and education) are susceptible to the same type of bias. Researchers should be aware of the effects that such selective migration—based on age, gender, and education—may have on the interpretation of estimates in their particular application. In some applications, the migratory responses we document here may lead researchers to overestimate treatment effects. Our findings also imply that such a bias will increase with the time window used in estimation.

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[37] Again, this is the unweighted interquartile range. Weighting by CZ population share yields a value of 1.18.

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