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On the Persistence of the China Shock

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On the Persistence of the China Shock*

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Abstract

We evaluate the duration of the China trade shock and its impact on a wide range of outcomes, building on analyses in Autor et al. (2013a) and Acemoglu et al. (2016). This shock plateaued in 2010, enabling study of its effects for nearly a decade past its culmination. Adverse impacts of import competition on manufacturing employment, employment-population ratios, and income per capita in more trade-exposed U.S. commuting zones are present out to 2019. Reductions in population headcounts, which indicate net out-migration, register only for foreign-born workers and the native-born 25-39 years old, implying that exit from work is a primary means of adjustment. More negatively affected regions see larger increases in the uptake of government transfers, but these transfers primarily take the form of Social Security and Medicare benefits and replace only a small fraction of lost personal income. Adverse outcomes are more acute in regions that initially had fewer college-educated workers and were more industrially specialized. Impacts are qualitatively—but not quantitatively—similar to those caused by the decline of employment in coal production since the 1980s, indicating that the China trade shock holds lessons for other episodes of localized job loss. Import competition from China induced shocks to income per capita across local labor markets that are much larger than the spatial heterogeneity of income effects predicted by standard quantitative trade models. Even using higher-end estimates of the consumer benefits of rising trade with China, a substantial fraction of commuting zones appears to have suffered absolute declines in real average incomes.

Keywords: import competition, China trade, local labor markets, manufacturing decline, job loss.

JEL Classifications: E24, F14, F16, J23, J31, L60, O47, R12, R23.

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1 Introduction

Recent literature on how import competition affects local labor markets has transformed our understanding of economic adjustment to globalization. Since at least David Ricardo, economists have appreciated that freer trade creates both winners and losers.[1] Yet, until recently, the prevailing view among scholars was that losses from trade were likely to be diffuse. The logic behind this view is simple and appealing: if labor is mobile across industries and regions, then the effects of greater import competition on, say, a labor-intensive and spatially agglomerated industry such as furniture making would be transmitted across all industries and regions employing less-educated labor.[2] Even though furniture factories in North Hickory, North Carolina, may shut their doors, the pain felt by former furniture workers in the region would be little different from the losses suffered by those in equivalently skilled service-sector jobs in Houston or Miami. We have since learned that losses from trade are often highly regionally concentrated.[3]

Much of the research that elucidates how local economies adjust to globalization is based on the China trade shock. In the early 1990s, China had both a large economy, and, for its size, a narrow comparative advantage in manufacturing. Its rapid opening to trade and investment disrupted global markets. Economies producing the labor-intensive goods that China began exporting experienced a massive increase in global competitive supply, while those producing the raw materials and other inputs demanded by China felt a commensurately large increase in global demand. Recent work exploits the speed, scale, and concentrated impacts of China's reform-driven export growth to analyze the labor market consequences of globalization. Labor markets more exposed to import competition from China experienced more plant closures; larger declines in manufacturing employment, employment-population ratios, earnings for low-wage workers, housing prices, and tax revenues; and larger increases in childhood and adult poverty, single-parenthood, and mortality related to drug and alcohol abuse, as well as greater uptake of government transfers.[4]

[1] That Ricardo recognized winners and losers from trade is evident in his opposition to the British Corn Laws (Ricardo, 1815), which he saw as redistributing income to landlords from workers (Maneschi, 2008).

[2] Labor mobility across industries and regions is core, e.g., to the Heckscher-Ohlin trade model (Feenstra, 2015).

[3] The seminal work of Jacobson et al. (1993) found scarring effects from job loss due to mass layoffs in the form of lower long-run earnings, which are more severe when the job loss occurs during a recession (Davis and von Wachter, 2011). Although earlier work in trade found that exposure to imports can induce job loss (Grossman, 1982; Kletzer, 2001), the literature has only recently connected trade shocks to scarring effects from worker displacement.

[4] Research addresses regional and industry employment (Autor et al., 2013a; Pierce and Schott, 2016; Handley and Limao, 2017); plant closure (Bernard et al., 2006; Acemoglu et al., 2016; Asquith et al., 2019); labor earnings (Autor et al., 2014; Chetverikov et al., 2016); government transfers (Autor et al., 2013a; Autor et al., 2014); housing prices and tax revenues (Feler and Senses, 2017); migration (Autor et al., 2013a; Monte et al., 2018; Faber et al., 2019;

In this paper, we re-examine the local labor market impacts of exposure to Chinese import competition in order to evaluate which market adjustment mechanisms are operative and over what time horizons, how long adverse impacts endure, and how adjustment to trade compares to adjustment to other localized shocks. Among the most striking findings in recent work is that regions experiencing larger trade-induced reductions in manufacturing employment saw neither differential reductions in labor supply—due, e.g., to out–migration—nor greater absorption of workers by non-manufacturing sectors. Manufacturing job loss translated nearly one for one into declines in the employment-population ratio.[5] Because in equilibrium changes in the employment rate are approximately proportional to changes in real income (Amior and Manning 2018; Galle et al. 2020), trade-induced increases in joblessness may indicate adverse changes in welfare. Motivated by this concern, we explore the duration of shock impacts through 2019.

In more trade-exposed regions, we find that negative effects on manufacturing employment continue to build well beyond the culmination of the trade shock itself. Adverse impacts persist out to 2019, nearly a decade after the shock reached peak intensity. Although the trade shock did generate modest net out-migration, this occurred only among the foreign-born and native-born adults ages 25 to 39. Impacts of the trade shock thus appear to be long-lasting and to entail suppressed participation in work, which is evident in declines in wages and salaries and personal income in trade-exposed regions. Income losses are only minimally offset by the increased uptake of government transfers, which also remain elevated to the end of the analytic window.

Why has the China trade shock had such enduring consequences? One explanation is that the shock itself never stopped intensifying. We find little support for this idea. China's spectacular manufacturing export growth slowed dramatically in the last decade, by which point China's reform-driven boom had run its course and the government had begun to rollback reforms (Lardy, 2019; Brandt et al., 2020; Brandt and Lim, 2020). Export growth also did not simply move en masse from

Greenland et al., 2019); and marriage, fertility, and mortality (Autor et al., 2019; Pierce and Schott, 2020). Other work explores impacts on consumer prices (Bai and Stumpner, 2019; Jaravel and Sager, 2019; Hottman and Monarch, 2020; Borusyak and Jaravel, 2021), U.S. exports (Feenstra et al., 2019), domestic innovation (Hombert and Matray, 2018; Autor et al., 2020b), and schooling (Greenland and Lopresti, 2016). Additional work studies how trade-shock impacts are mediated by labor market conditions (Austin et al., 2016), industry product cycles (Eriksson et al., 2019), labor regulations (Chan, 2019), and trade deficits (Dix-Carneiro and Kovak, 2017). Autor et al. (2016) and Redding (2020) review the literature; and Dorn and Levell (2021) provide a comparative European-U.S. perspective.

[5] The absence of a local labor supply response to the China trade shock is consistent with findings on the null impact of generic Bartik (1991) labor demand shocks on local population headcounts for less-skilled workers (e.g., Bound and Holzer, 2000; Diamond, 2016; Notowidigdo, 2020). Charles et al. (2019) report comparable downward impacts on local employment rates from regional exposure to the national decline in manufacturing over 2000 to 2017.

China to other nearby countries. Whether we look at the U.S. market presence of China alone or combined with Vietnam and other Southeast Asian nations, to which China has begun to offshore some labor-intensive activities, the trade shock reached peak intensity around 2010 and stabilized thereafter. Although the China trade shock did not unwind, it did plateau, letting us examine its consequences for nearly a decade beyond its full expression.

We conduct preliminary analyses of two other explanations for why trade-exposed labor markets suffered long-lasting hardship. One hypothesis is that many traditional manufacturing regions were poorly positioned to recover from job loss because of a dearth of college-educated workers, who are in high demand by sectors that are expanding nationally (Glaeser et al., 1995; Diamond, 2016; Bloom et al., 2019). A second is that specialization in a narrow set of industries left these regions exposed to industry-specific shocks that, once industry decline initiated, would set in motion a process that is self-reinforcing (e.g., Dix-Carneiro and Kovak, 2017). We find some support for both the dearth-of-human-capital and reverse-agglomeration hypotheses. We also discuss other mechanisms that may be at work, though we do not provide a definitive, mono-causal explanation

To put the China trade shock in context, we examine adjustment to the secular decline of the coal industry and the impact of the Great Recession, the intensity of each of which varied sharply across regions. Regions more specialized in coal saw larger declines in employment rates, which remained suppressed more than a quarter century after the industry downturn began in 1980 (Black et al., 2002, 2005). Similarly, localities harder hit by the Great Recession had lower employment-population ratios 10 years after the recession had officially ended, extending the findings in Yagan (2019). Although the long-run impacts of import competition from China do not appear to be unique qualitatively, the quantitative magnitudes of these impacts stand out relative to other shocks.

Much of the empirical trade literature estimates trade-shock impacts by comparing changes in outcomes in more and less trade-exposed regions. This strategy identifies the *relative* impact of the trade shock—e.g., whether manufacturing employment fell by more in more-trade-exposed locations—but not the *absolute* impact of the shock, e.g., whether the trade shock reduced manufacturing employment nationally (Heckman et al., 1998; Helpman, 2018). Although relative impacts are informative about the distributive consequences of trade, they may not be indicative of changes in aggregate outcomes.[6] To evaluate the welfare impacts of trade, recent literature uses reduced-

[6] This issue arises more broadly in empirical work in macroeconomics (Chodorow-Reich, 2019, 2020; Guren et al., 2020).

form empirical results to discipline the calibration of quantitative trade models. These calibrations suggest that U.S. aggregate welfare gains from China's market opening were positive but small, and that some *relatively* adversely effected regions may have suffered *absolute* welfare declines.[7] To investigate these effects further, we compare regional variation in real-income changes in quantitative analyses with that implied by our reduced-form work, which imposes no model structure. Standard quantitative models generate regional variation in income changes that is only one-quarter of what we find in reduced-form estimation. Even higher-end estimates of the cost-reducing impact of trade with China on U.S. consumer prices (Jaravel and Sager, 2019; Borusyak and Jaravel, 2021) imply that a substantial number of regional economies suffered absolute declines in real incomes.

A final consideration regards the normative implications of adverse impacts of the China trade shock on labor markets in the U.S. and other countries. Few economists would interpret our empirical results as justifying greater trade protection. As expected, quantitative models indicate that U.S. aggregate gains from trade with China are positive. Yet, the fact that the losses from trade are regionally concentrated and long lasting suggests that existing policies failed to insulate workers from the disruptive impacts of globalization. We close the paper by discussing what we know, and do not know, about policies that help insulate workers from concentrated job loss.

2 The Timing of the China Shock

We first review China's recent export performance. Enduring impacts of the China trade shock on U.S. local labor markets could be the result of the shock continuing to build after the 2000s and into the 2010s. We find this not to be the case. China's export growth has progressed through well-defined periods of initiation, acceleration, and stabilization. The country's share of the U.S. market approached peak intensity around 2010 and plateaued thereafter.

That China's export growth has been explosive is universally understood. Its share of world manufacturing exports rose from 3.1% in 1991 to 17.6% in 2015, before dipping to 14.2% in 2018, as documented in Figure 1.[8] This export boom has been concentrated in manufacturing and is recognized as transforming China into the world's factory.[9] Conversely, China's share of world

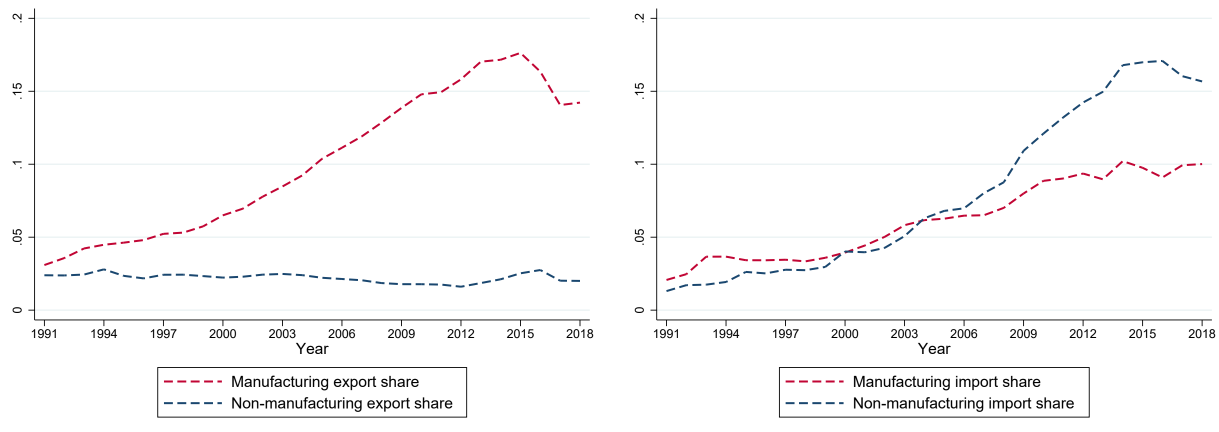
[7] See, in particular, Caliendo et al. (2019); Rodríguez-Clare et al. (2019); Adao et al. (2019a); Galle et al. (2020); Kim and Vogel (2020).

[8] Appendix A.2 provides details on the data we use in this section.

[9] See, e.g., "Global Manufacturing: Made in China," *The Economist*, March 12, 2015.

Figure 1: China Share of World Trade by Major Sector

(a) China share of world exports (b) China share of world imports



Note: Data are from UN Comtrade, SITC Revision 2, and cover all merchandise trade.

exports of non-manufactured goods has changed little over time, averaging just 2.3% between 1991 and 2018 and showing no discernible trend. This divergence in export growth between major sectors reflects China's powerful comparative advantage in manufacturing—particularly in labor-intensive manufacturing—which derived from the country's initial abundant supply of labor relative to its supplies of agricultural land, natural resources, and physical capital (Wood, 1995, 2018). Once China began to relax restrictions on foreign trade and investment, first through the expansion of special enterprise zones initiated by Deng Xiaoping (Naughton, 2007), and later through an economy-wide liberalization tied to the country's accession to the World Trade Organization (WTO) in 2001 (Lardy, 2019), it was able to realize this latent prowess and integrate into global value chains. The surge in manufacturing exports generated a corresponding increase in the import of raw materials used in industrial production, which mushroomed China's share of world non-manufacturing imports from 1.2% in 1991 to 15.7% in 2018 (Figure 1b).[10]

Our concept of the China shock is the period spanning the country's reform-driven transition from an inward-oriented and heavily centrally planned economy to a comparatively open and market-oriented one in which the majority of production occurred in privately owned firms. We conventionally date the China trade shock as commencing in 1992, when Deng expanded his "reform and opening" agenda to include foreign trade and investment (Naughton, 2007; Vogel, 2011).[11] China's

[10]China's share of world manufacturing imports also rose over time, from 2.1% in 1991 to 10.0% in 2018, an increase driven in part by the expansion of global value chains whose assembly operations were anchored in China (Koopman et al., 2012; Kee and Tang, 2016).

[11]China's manufacturing export growth in fact began in the 1980s (Brandt et al., 2020). Given the country's very

market transition unleashed dramatic improvements in productivity resulting from a spatial and sectoral reallocation of resources (Hsieh and Klenow, 2009; Brandt et al., 2013), the expansion of the private sector at the expense of inefficient state-owned enterprises (Song et al., 2011; Khandelwal et al., 2013; Hsieh and Song, 2015; Bai et al., 2017), inflows of foreign capital and imported intermediate inputs (Yu, 2010; Brandt and Morrow, 2017), a reorientation of manufacturing to export assembly plants producing at the behest of multinational firms (Feenstra and Hanson, 2005; Brandt et al., 2017), and the adoption of previously banned foreign technologies (Hu and Jefferson, 2009; Wei et al., 2017; Li, 2020).[12] Over 1998 to 2007, output per worker in Chinese manufacturing grew at the stunning annual average rate of 8.0% (Brandt et al., 2012; Liu and Ma, 2020). The 1992 to 2007 export surge thus appears to be an episode in which China's trade growth was driven largely by productivity gains resulting from its marketing opening.

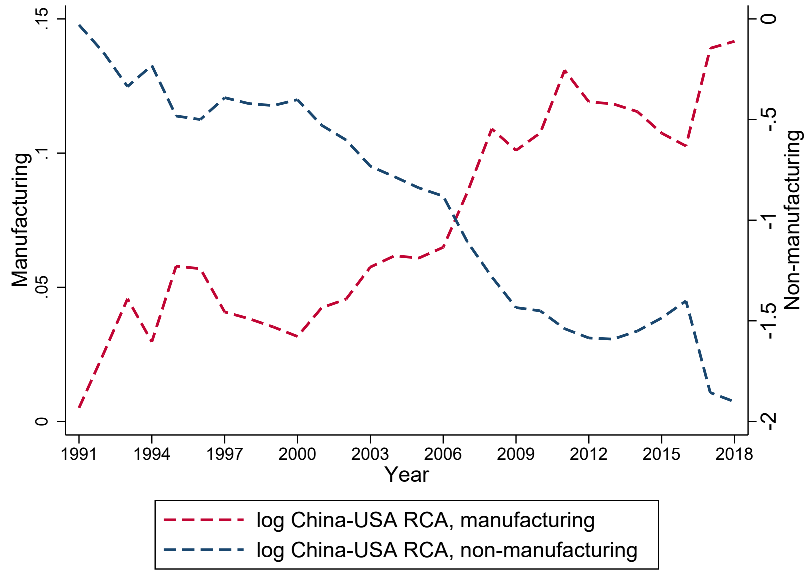
Because much of China's post-Deng manufacturing growth was transitional, it was destined to be finite (Song et al., 2011). Once a substantial share of the labor force had moved from the countryside to cities and from state-owned to private firms, easy growth may have been less attainable. Between 2010 and 2013, Brandt and Lim (2020) estimate that TFP growth in Chinese manufacturing was negative in 8 of 11 subsectors, in stark contrast to earlier years. Changes in the political economy of reform may have further hampered manufacturing. In 2008, President Hu Jintao gave (low productivity) state-owned enterprises renewed prominence in industrial planning (Naughton, 2016), a shift strongly reinforced by President Xi Jinping in 2012 (Lardy, 2019). After 2007, entry of private and foreign-owned manufacturing firms fell sharply (Brandt et al., 2020). The end of the productivity-growth miracle and the rollback of pro-market reforms slowed manufacturing growth after 2010. Whereas the differential in annual manufacturing export growth between China and the rest of the world was 8.2 percentage points over 1991 to 2001 and 8.4 percentage points over 2001 to 2010, it was -0.4 percentage points over the 2010 to 2018 period.

China's post-2010 slowdown is also apparent when we compare its trade performance to the U.S., which is helpful for defining the shock facing U.S. manufacturing industries. A simple method to evaluate China's relative export success is to use the Balassa (1965) measure of revealed comparative advantage (RCA), which is the ratio of a country's share of world exports in a particular sector to its

low initial export levels, however, this early grow did little to change China's share of global exports.

[12] The undervaluing of China's yuan-dollar exchange rate in the early 2000s may have also contributed to the country's export boom (Bergsten and Gagnon, 2017), though without creating an industry bias in this growth.

Figure 2: China vs. U.S. Revealed Comparative Advantage



Note: Data are from UN Comtrade, SITC Revision 2. Revealed Comparative Advantage (RCA) is a country's share of world exports in a sector relative to a country's share of world exports of all merchandise.

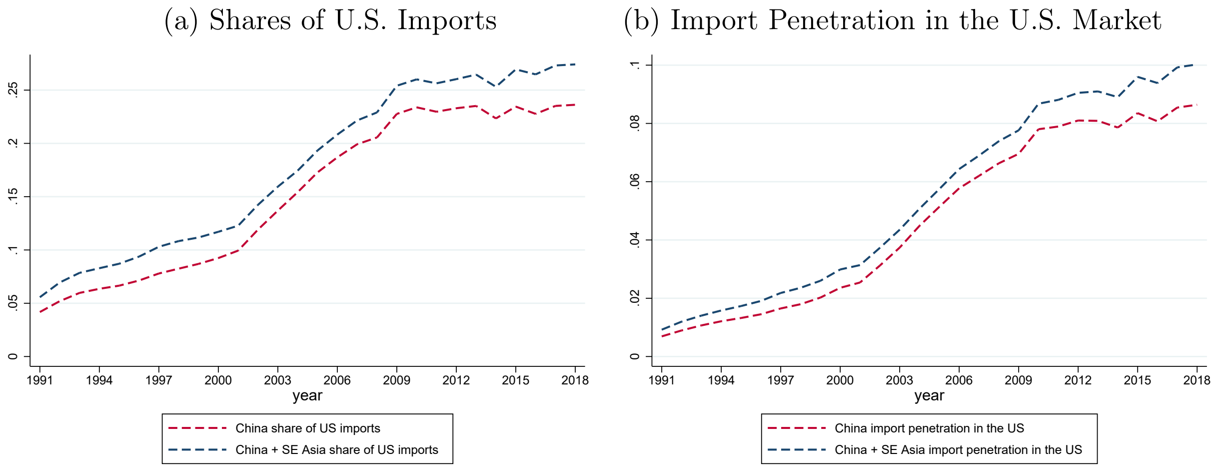
share of world exports of all goods.[13] In Figure 2, we show the gap between the log China and log U.S. RCA for manufacturing and non-manufacturing. China began with a modest comparative advantage in manufacturing over the U.S. in 1991—as indicated by a China-U.S. manufacturing versus all goods export differential of just 2.0 percentage points in that year. By 2011, its manufacturing-versus-all-goods export differential over the U.S. had leaped to 13.1 percentage points, before falling and rising over the ensuing eight years. After 2010, the China-U.S. relative RCA exhibits little trend.

By construction, China's RCA in non-manufacturing compared to the U.S. exhibits approximately the inverse pattern, dropping steadily through the 1990s and 2000s and then remaining at negative levels after 2011. China's comparative disadvantage in non-manufacturing relative to the U.S. reflects the success of U.S. firms in exporting agricultural products, certain mineral products, and oil and gas, to the rest of the world, as well as market forces in China that have concentrated factors of production in manufacturing at the expense of other sectors.

Even though China's export growth in manufacturing slowed after 2010, it still could have contributed to overall U.S. manufacturing import growth by offshoring production to other low-wage countries. Chinese firms have been active in building industrial parks for export production

[13] In the Eaton and Kortum (2002) model of Ricardian comparative advantage, RCA summarizes differences in effective productivity between a country and the rest of the world. See also Hanson et al. (2015).

Figure 3: U.S. Manufacturing Imports from China and Southeast Asia



Note: Shares of U.S. manufacturing imports in (a) are from UN Comtrade, SITC Revision 2; import penetration in (b) is the ratio of U.S. imports of manufactured goods to U.S. domestic absorption (defined as gross output plus imports minus exports). All values exclude oil and gas. Gross output is from the St. Louis Federal Reserve Bank.

in Southeast Asia, and in Vietnam in particular.[14] To investigate this possibility, in Figure 3 we plot shares of U.S. imports and import penetration in the U.S. market for China alone and for China combined with low-income countries in Southeast Asia. We select these countries—Cambodia, Indonesia, Laos, Myanmar, Philippines, Vietnam—based on their per capita GDP in 2010 being less than that of China.[15] We also add neighboring Bangladesh to this group because in recent decades multinational companies from Asia have expanded apparel factories in the country (Heath and Mobarak, 2015). Figure 3 underscores that China's exports to the U.S. dwarf those of Southeast Asia. In 2010, China accounted for 23.4% of U.S. manufacturing imports, whereas the Southeast Asian nations accounted for just 2.6%. In 2018, these figures were only modestly changed, at 23.6% and 3.8%, respectively. Although Southeast Asian countries did gain a larger share of U.S. imports, the increase over the 2010 to 2018 period was just 1.2 percentage points. Import penetration, shown in Figure 3b, tells a similar story. Whether we look at China alone or combined with low-income Southeast Asian nations, its U.S. market presence largely stabilized after 2010.[16]

Taken together, these trade patterns allow us to mark three distinct periods of China's export

[14]See, e.g., "Good Afternoon, Vietnam," *The Economist*, August 6, 2016.

[15]In 2010, World Development Indicators show per capita GDP (at 2010 prices) of $4,560 in China and a range of $988 (Myanmar) to a $2130 (Philippines) in our Southeast Asian nations. The excluded, higher-income countries in the region are Brunei, Malaysia, Singapore, and Thailand (whose per capita GDPs range from $5,075 to $46,570).

[16]Results are qualitatively similar when we include additional low-income countries in South Asia (India, Pakistan, Sri Lanka), whose shares of global manufacturing exports have remained flat in recent years (Hanson, 2000).

performance: the gradual initiation of China's export boom in the early 1990s; the dramatic acceleration of China's export growth following its WTO accession in 2001; and China's export plateau after 2010, which coincided with slowing manufacturing productivity growth.[17] This slowdown may have been the result of China having completed its transitional period of post-reform growth, and possibly too of the partial reversal of reforms by Presidents Hu and Xi. Whatever the cause, the China trade shock appears to have stopped intensifying after 2010.

3 Estimating Labor Market Adjustment to Trade Shocks

This section presents our measure of the China trade shock and main empirical specification, which builds on Autor et al. (2013a) and Acemoglu et al. (2016). We aim to identify the causal effect of import competition from China on labor market outcomes for U.S. commuting zones (CZs), our measure of local labor markets (Tolbert and Sizer, 1996; Dorn, 2009), and evaluate impacts as the shock intensifies in the early 2000s, reaches peak intensity around 2010, and stabilizes thereafter.

3.1 Baseline Specification

We examine exposure to import competition from China for the 722 CZs in the continental United States. As in Acemoglu et al. (2016), our measure of trade exposure is the average change in Chinese import penetration across industries, weighted by industry shares in initial CZ employment:

$$\Delta IP^{cu}_{i\tau} = 100 \times \sum_j s_{ijt} \Delta IP^{cu}_{j\tau}, \tag{1}$$

where $\Delta IP^{cu}_{j\tau} = \Delta M^{cu}_{j\tau}/(Y_{jt} + M_{jt} - X_{jt})$ is the growth of Chinese import penetration for U.S. manufacturing industry j over time interval τ (2000 to 2012 in most cases), t is the base period (2000 in most cases), and $s_{ijt} \equiv L_{ijt}/L_{it}$ is the share of industry j in CZ i's total employment in the base year. We compute import penetration as the growth in U.S. industry imports from China, $\Delta M^{cu}_{j\tau}$, divided by initial industry domestic absorption (U.S. industry shipments plus net

[17] In Appendix A.1, we study China's sectoral export performance in detail. Most of the goods in which China had an initial or eventual strong revealed comparative advantage relative to the U.S. were ones in which it captured a large share of world exports. The corresponding U.S. sectors were highly exposed to the China trade shock. In some goods, many of which appear to be relatively less sophisticated, China's advantage manifested itself early. In others, China's export strength came late. This group includes both more sophisticated products, such as computers and cellphones, and less sophisticated ones, such as textile fabrics. For most products, the trade shock was enduring. Once China acquired a large share of world exports, it tended to hold onto it, at least through the late 2010s.

Table 1: Summary Statistics for CZ Import Penetration

	Change in CZ import penetration	Mean (ppts)	Standard deviation	25th percentile	50th percentile	75th percentile
Imports by US	1991 to 2000	0.94	0.60	0.52	0.88	1.22
	2000 to 2007	1.35	0.84	0.84	1.15	1.62
	2000 to 2010	0.85	0.59	0.50	0.69	1.07
	2000 to 2012	0.89	0.59	0.51	0.75	1.17
	2000 to 2014	0.85	0.56	0.47	0.73	1.10
Imports by other high-income countries	1991 to 2000	0.70	0.48	0.40	0.63	0.87
	2000 to 2007	1.38	0.79	0.91	1.31	1.61
	2000 to 2010	1.27	0.82	0.82	1.13	1.47
	2000 to 2012	1.22	0.69	0.79	1.15	1.44
	2000 to 2014	1.01	0.58	0.66	0.96	1.21

Notes: This table shows the change in import penetration (100 x CZ weighted-average change in imports from China/domestic industry absorption) for the 722 commuting zones in the continental US and the instrumental variable for this value at given time horizons. Values are decadalized (x 10/length of time period) and weighted by the CZ working-age population in the initial year.

imports, $Y_{jt} + M_{jt} - X_{jt}$), as summarized in Table 1.[18] Differences in $\Delta IP^{cu}_{i\tau}$ across CZs stem from variation in local industry employment in the base year, which arises from differential specialization in manufacturing and in import-intensive industries within manufacturing.

In Figure 4, we map the China trade shock in (1) across commuting zones for the 2000 to 2012 time period. The most impacted CZs, shown as those in the top two deciles of increased import penetration, are concentrated in the eastern half of the United States, and especially in the Southeast (north of the Deep South) and Midwest (outside of the largest metropolitan areas). These CZs are where U.S. manufacturing relocated as it moved out of large cities in the Northeast and northern Midwest in the middle of the 20^{th}century (Eriksson et al., 2019).[19]

To quantify the evolving impact of the China trade shock on labor market outcomes, we estimate first-difference models using successively longer time differences. Our regressions have the form,

$$\Delta Y_{it+h} = \alpha_t + \beta_{1h} \Delta IP^{cu}_{i\tau} + \mathbf{X}'_{it} \beta_2 + \varepsilon_{it+h}, \tag{2}$$

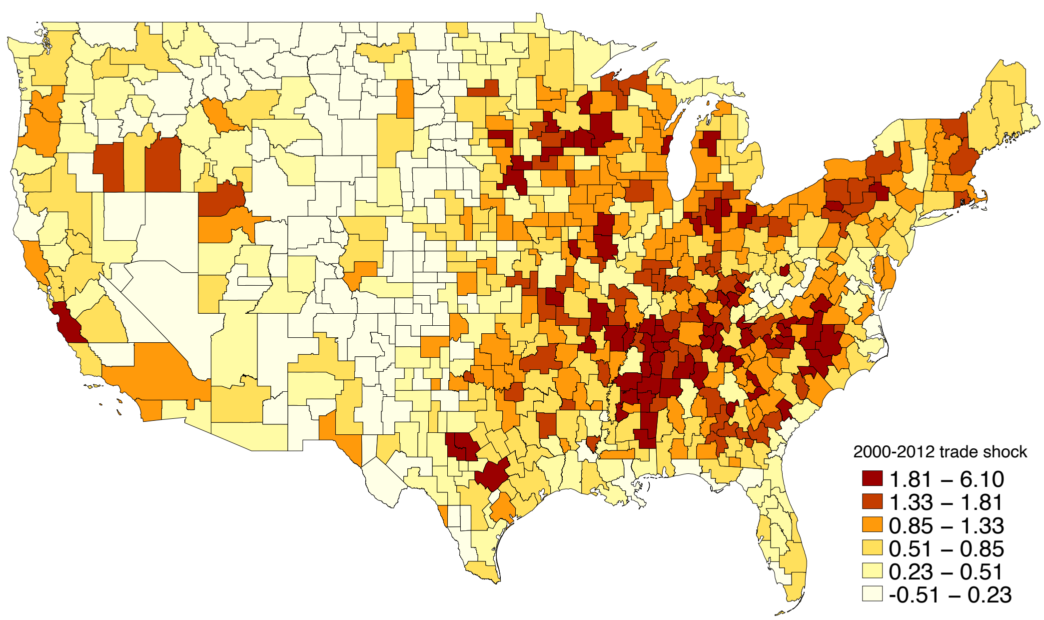
where ΔY_{it+h} is the change in an outcome for CZ i between the initial year t and later year $t+h$ for $h = 1, \ldots, T$, such that we estimate separate regressions for each time difference between 2000-2001

[18] We measure imports using HS trade data from UN Comtrade, harmonized to 4-digit SIC industries, and industry shipments using the NBER manufacturing productivity database (Autor et al., 2014).

[19] Appendix Figure A5 maps the China trade shock for the 1991-2012 and 2000-2007 time periods, which exhibit very similar patterns. Autor et al. (2013b) show that there is little correlation between a CZ's exposure to Chinese import competition and exposure to routine task-replacing technological change.

to 2000-2019. We focus on the period 2000 to 2019, which comprises the decade of China's most intense export growth, and the subsequent decade when China's export growth leveled off, up to the year before the onset of the Covid-19 recession in 2020. Our baseline definition of the trade shock is the period 2000 to 2012, whose first year is one year prior to China's WTO entry and whose final year post-dates the culmination of the trade shock in 2010 and the volatility in global trade following the 2008 global financial crisis.[20] For $h < 12$, we estimate shock impacts for outcome periods that are less than complete—i.e., whose length is less than that of the trade shock itself—which reveals how long it takes for the full impact of the shock to manifest. For $h > 12$, we estimate shock impacts on outcome periods that extend beyond the culmination of China's export boom, which reveals whether shock impacts attenuate over longer time horizons. If, for instance, it takes time for workers displaced from manufacturing to find jobs in other sectors or to migrate elsewhere, we may not see full adjustment along these margins until well after the shock reaches full intensity.

Figure 4: Chinese Import Penetration (2000-2012) in U.S. Commuting Zones



Note: This figure shows the change in import penetration from China in (1) over 2000-2012. The legend indicates values for the bottom four quintiles and the top two deciles.

The control vector $\mathbf{X}'_{it}$ contains time trends for U.S. Census Divisions and start-of-period CZ-

[20] We show in Appendix Figure A9 that we obtain nearly identical coefficient estimates when using trade shocks constructed for the 2000-2007, 2000-2010, 2000-2012, and 2000-2014 time periods. This similarity in results is a consequence of the post-2000 trade shock being front loaded on the 2000 to 2007 period. The 2000-2007 shock is 79.8% of the magnitude of the 2000-2012 shock and 88.8% of the magnitude of the 2000-2014 shock. The CZ-level pairwise correlations of the 2000-2007, 2000-2010, 2000-2012, and 2000-2014 trade shocks range from 0.93 to 0.96.

level covariates: the manufacturing share of employment, which allows us to focus on trade exposure arising from the within-manufacturing industry mix; specialization in occupations according to their routine-task intensity and offshorability (based on Autor and Dorn, 2013), thus accounting for exposure to automation and non-China-specific globalization; the fractions of foreign-born, non-whites, and the college educated in the population, and the fraction of working-age women who are employed, which absorbs variation in outcomes related to labor-force composition; and the population shares of residents ages 0 to 17, 18 to 39, and 40 to 64, which control for demographic factors that may affect labor-force participation and eligibility for government transfers (see Appendix Table A2). For all outcomes other than population headcounts, regressions are weighted by the CZ working-age population (ages 18 to 64) in the initial year; for headcount regressions, weights are the total CZ population in the initial year. All specifications cluster standard errors at the state level.

We focus on U.S. imports from China and not U.S exports to China because the former dwarf the latter and because our instrumentation strategy (see below) is less well-suited to isolating exogenous variation in U.S. export growth.[21] Autor et al. (2013a) find similar results when replacing growth in U.S. imports with growth in U.S. net imports. Consistent with these findings, Feenstra et al. (2019) detect no impact of growth in U.S. exports to China on U.S. manufacturing employment, although overall U.S. export growth is positively associated with manufacturing activity.[22]

In related work, Eriksson et al. (2019) study the "Japan trade shock" of 1975 to 1985 (during which Japan's exports of autos and other durables surged) and the "East-Asian Tiger trade shock" of 1975 to 1988 (during which Hong Kong, Korea, Singapore, and Taiwan had manufacturing export booms), which occurred a dozen years before China's WTO accession. They detect no impact of the Japan shock on employment rates in U.S. CZs, while Batistich and Bond (2019) find a similar overall null effect of the shock on CZ manufacturing employment. It is further the case that the CZs exposed to the earlier East Asian export surges had limited overlap with those exposed to the later

[21] The 2000-2012 increase in U.S. manufacturing imports from China ($292bn) was 4.1 times the increase in U.S. manufacturing exports to China ($71bn), for values in 2015 USD.

[22] Weak U.S. manufacturing exports to China could be a result of U.S. comparative advantage lying elsewhere, such as in agriculture, oil and gas, and intellectual property. Dorn and Levell (2021) show that most major European economies experienced a combination of rapidly increasing Chinese import penetration and modestly growing exports to China that was quantitatively comparable to the U.S. experience, with the notable exception of Germany where manufacturing exports (dominated by machine tools and cars) grew in parallel to imports (dominated by labor-intensive products). In Germany, job loss in import-exposed regions counteracted job growth in export-oriented ones (Dauth et al., 2014). OECD countries in which exports to China grew to a similar extent as imports from China experienced smaller declines in manufacturing employment than countries like the U.S., where imports strongly dominated exports (Dorn and Levell, 2021). Weak U.S. export growth could also be attributable to the forces behind large trade surpluses in China and trade deficits in the U.S. (Autor et al., 2016; Dix-Carneiro et al., 2021).

China trade shock. The former group had relatively had high levels of income, college attainment, and patents per capita, whereas the latter had relatively low values of these indicators.[23]

Import penetration in (1) includes all types of shipments from China to the United States, whether they emanate from Chinese companies exporting final goods to the U.S. market on their own accord or export processing plants—such as Foxconn, which assembles Apple iPhones from imported parts and components—that are part of global value chains controlled by multinational enterprises. Boehm et al. (2020) find that much of the post-1990 employment decline in U.S. manufacturing occurred in establishments owned by U.S. multinationals that were simultaneously expanding operations in their foreign subsidiaries. Between 1995 and 2005, export processing plants contracting with multinationals accounted for a hefty 55% of China's manufacturing exports (Feenstra and Hanson, 2005). Over time, however, Chinese firms have expanded input production in China, which reduced the share of processing plants in China's exports to 35% by 2015 (Ma, 2020). Unfortunately, our data do not allow us to separate U.S. imports from China into those that came through global value chains versus other channels. In recent work, Aghion et al. (2021) use firm-level data for France to separate imports from China into "horizontal" products—imports in product categories similar to a firm's exports—and "vertical" products—imports in product categories similar to a firm's imported inputs. Whereas exposure to the first type of imports predicts employment declines, exposure to the second type of imports does not. These results suggest that imports that intensify head-to-head competition with domestic firms spur contraction of domestic manufacturing.[24]

3.2 Causal Identification

A challenge for identifying the causal impact of import exposure on labor-market outcomes in (2) is that U.S. imports may change both because of shocks to U.S. product demand and shocks to foreign product supply, where the former may be correlated with the residual, ε_{it+h}. To identify the foreign-supply-driven component of U.S. imports from China, we follow Autor et al. (2013a) and Acemoglu et al. (2016) in instrumenting U.S. import exposure, $\Delta IP^{cu}_{i\tau}$, using non-U.S. exposure,

[23] Despite a null net impact of the Japan shock on overall employment levels, Borjas and Ramey (1995) find that U.S. metropolitan areas more exposed to import competition during the East Asian shock period had large increases in college-non-college wage differentials. Batistich and Bond (2019) find further that the Japan shock caused changes in composition of employment in exposed CZs, with employment falling for Black workers and rising for white workers.

[24] In Aghion et al. (2021), adverse impacts of import exposure are concentrated on low-productivity firms. In the U.S. market, by contrast, Bernard et al. (2006) and Autor et al. (2014) find that trade-induced employment declines are concentrated in larger manufacturing establishments, consistent with the framework in Holmes and Stevens (2014).

$\Delta IP^{co}_{i\tau}$, which we measure as the industry-level growth of Chinese exports to eight other developed countries:[25]

$$\Delta IP^{co}_{i\tau} = \sum_j s_{ijt-10} \Delta IP^{co}_{j\tau}. \tag{3}$$

where $\Delta IP^{co}_{j\tau} = \Delta M^{co}_{j\tau}/(Y_{jt-3} + M_{jt-3} - X_{jt-3})$. This expression differs from (1) by using imports from China in other high-income markets ($\Delta M^{co}_{j\tau}$) in place of U.S. imports ($\Delta M^{cu}_{j\tau}$), the 3-year lag of industry absorption ($Y_{jt-3} + M_{jt-3} - X_{jt-3}$) in place of its base-year t value, and the 10-year lag of CZ industry employment shares, $s_{ijt-10} \equiv L_{ijt-10}/L_{it-10}$, in place year t values (see Table 1).[26]

Analyses of the China trade shock have used $\Delta IP^{co}_{i\tau}$ as a shift-share instrument in local labor market regressions and have directly applied the industry-level shocks, $\Delta IP^{co}_{j\tau}$, as instruments for the growth in industry-level import penetration (e.g., Autor et al., 2014; Autor et al., 2020b). Recent literature formalizes the basis for identification and inference in such shift-share settings. Borusyak et al. (2020) treat identification as based on exogeneity of the shifts—i.e., the industry-levels changes in import penetration—which clarifies the identifying assumptions in the analysis and provides an asymptotic rationale for using the shift-share structure to construct standard errors.[27] Adao et al. (2019b) present a related method for estimating standard errors. Although these refinements of the shift-share approach do not change the fundamental conclusions about the impact of the China trade shock on regional employment, they do formalize exclusion restrictions and advance methods for statistical inference. In Appendix A.4, we draw on this recent literature to discuss the basis for econometric identification, present results using alternative methods for constructing standard errors, and provide analysis of pre-trends in the data.

4 Empirical Results

This section presents our main empirical results for the impact of trade exposure on U.S. commuting zones. We use equation (2) to estimate how the 2000-2012 trade shock affected CZs over 2000 to 2019, focusing on three sets of outcomes. The first are ratios of manufacturing employ-

[25]The eight comparison countries—determined by the availability of comparable HS trade data for the full sample period—are Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland.

[26]The use of lagged values helps reduce both the role of simultaneity and the influence of measurement error.

[27]The assumption of exogenous shifts is evident in industry-level analyses of the China shock that instrument a U.S. industry's import growth from China with these industry's Chinese imports in other countries (e.g., Autor et al. (2014); Acemoglu et al. (2016)). Goldsmith-Pinkham et al. (2020) develop an alternative approach to shift-share analysis, which is based on exogeneity of the shares (i.e., pre-existing industry structures). As we discuss in Appendix A.4, their approach appears to be less suitable for our application. See Ferman (2019) for further discussion.

ment, non-manufacturing employment, total wage and salary employment, and unemployment to the working-age population (defined as individuals ages 18 to 64 years old). We measure employment using the Bureau of Economic Analysis' (BEA) Regional Economic Information System (REIS), unemployment using Local Area Unemployment Statistics (LAUS), and population using the National Vital Statistics System, where we aggregate data from the county to the CZ level. Trade shock impacts on these outcomes reveal the direct consequences on manufacturing employment, and the magnitude of labor reallocations to unemployment, non-employment, and other sectors. A second set of outcomes are population headcounts, overall and by nativity and age-based subgroups, which reveal whether exposure to import competition led to net reductions in the resident population (e.g., due to out-migration). A third set of outcomes are per capita personal income, labor compensation, and government transfers, overall and by program type, which reveal the impact of trade shocks on average income and its components, and allow us to quantify the degree to which government transfers offset income losses caused by trade exposure. We measure income, earnings, and government transfers using county-level data from the REIS. For most series, we use 2000 as the initial year.[28] Summary statistics on outcome and control variables are reported in Appendix Tables A1 and A2; Appendix A.2 contains further details on data sources and variable construction.

4.1 Employment Outcomes

4.1.1 Manufacturing and Non-manufacturing Employment

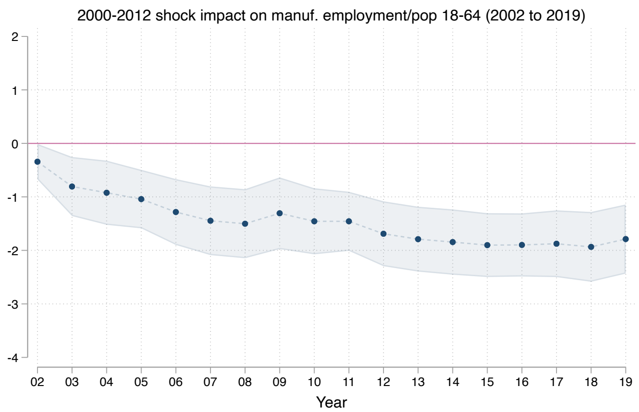
We begin with the impact of the China trade shock on the CZ employment-population ratio, where we measure employment either as the total number of wage and salary jobs in a CZ, or by splitting these jobs into manufacturing and non-manufacturing (where the latter is defined as total wage and salary employment less manufacturing employment). Figure 5 displays 2SLS estimates of impact coefficients for trade exposure from the time-difference specification in (2) when using (3) to instrument for (1). Each panel shows results for 18 separate regressions, where the horizontal axis organizes specifications according to the time difference being considered, beginning with 2001 to 2002 and extending out to 2001 to 2019. Vertical bars show 95% confidence intervals for the coefficient estimates. Structured in this manner, Figure 5 shows the cumulative impact of trade exposure

[28] Because of BEA changes in industry classification from the SIC to the NAICS in 2001, our REIS series for manufacturing and non-manufacturing employment begin in that year, rather than 2000.

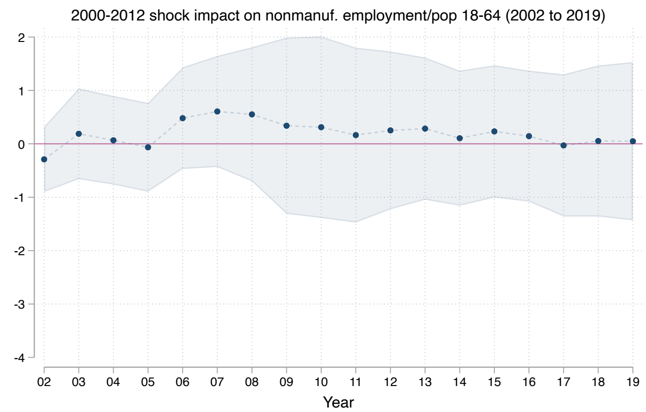
for progressively longer time differences (1 to 18 years).

Figure 5: Trade Shock Impact on Employment, 2001-2019

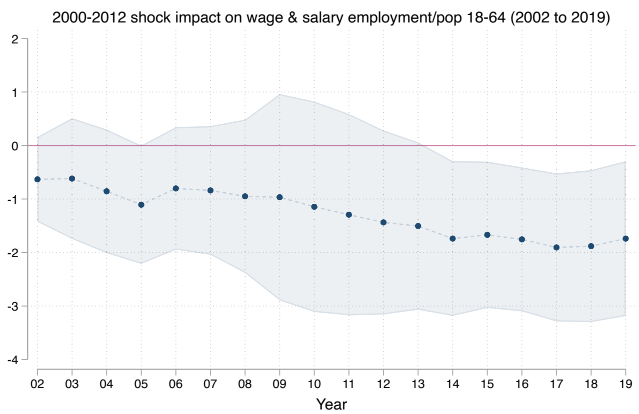
(a) Manuf. employment/Working-age pop.



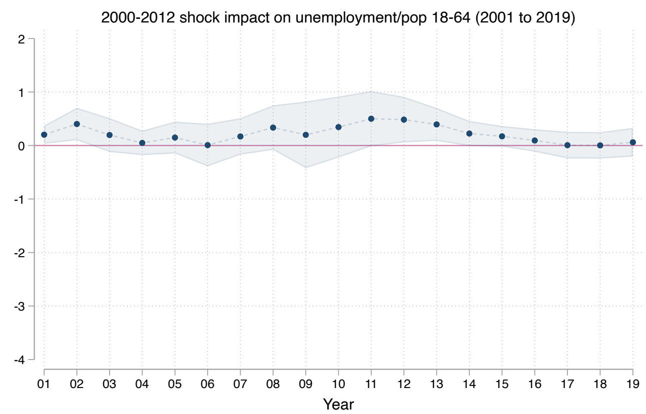
(b) Non-manuf. employment/Working-age pop.



(c) Total wage & salary employment/Working-age pop.



(d) Unemployed persons/Working-age pop.



Note: Panels (a)-(d) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in the specified outcome between 2001 and the year indicated on the horizontal axis; the trade shock is the decadalized 2000-2012 change in CZ import exposure, as defined in (1) and instrumented by (3). Control variables include initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies. Regressions are weighted by the CZ working-age population in 2000; standard errors are clustered by state.

The results for manufacturing employment in Figure 5a show a negative effect of trade exposure that builds quickly and then stabilizes. The impact coefficient is -1.45 (t-value$= -4.41$) for 2001 to 2007, which is the end year of analysis in Autor et al. (2013a), reaches -1.90 (t-value$= -6.24$) for the 2001 to 2015 period, and then attenuates modestly to -1.79 (t-value$= -5.42$) for the 2001 to 2019 period. As one moves past 2012, the negative effect of greater import competition on manufacturing employment continues to grow, even though the trade shock itself no longer appears to be intensifying. Given a decadalized increase in import penetration over 2000 to 2012 of 0.89 percentage points (Table 1), the implied reduction in the manufacturing employment share over 2001 to 2019 is -1.59 ($= -1.79 \times 0.89$) percentage points, relative to the overall 2001-2019 change in the manufacturing employment share of -2.68 percentage points (Appendix Table A1). Alternatively, when comparing CZs at the 25^{th} and 75^{th} percentiles of trade exposure, the latter would be predicted to have a reduction in its manufacturing employment share that is 1.18 ($= -1.79 \times [1.17 - .51]$) percentage points larger than in the former over 2001 to 2019. This compares to the 25^{th}–75^{th} percentile differential change in the manufacturing-employment-share of -2.17 ($= -3.79 + 1.62$) percentage points for the same period. Exposure to import competition from China thus appears to account for a large share of net manufacturing job loss in US commuting zones after 2000.[29]

Those losing jobs in manufacturing may move into other sectors, into unemployment, or out of the labor force. Simultaneously, they may move to another commuting zone.[30] In Figure 5b, we consider the impact of exposure to import competition on the CZ share of the working-age population employed in non-manufacturing industries. Impact coefficient estimates are close to zero at all time intervals and are imprecisely estimated (e.g., the impact coefficient for the 2001-2019 period is 0.05, t-value= 0.06). We see no evidence that on net non-manufacturing sectors absorbed local workers released from manufacturing due to the China trade shock.[31] The wide standard error bounds in Figure 5b are suggestive of heterogeneity across CZs in how non-manufacturing absorbs displaced manufacturing workers, a possibility we explore in section 4.4.

The fall in manufacturing employment and absence of offsetting job gains in non-manufacturing imply a decline in total wage and salary employment in trade-exposed CZs, confirmed by Figure 5c. Decreases in the employment-population ratio are comparable in magnitude to those for manufacturing employment. The effect reaches its maximum value for the 2001 to 2017 period: the impact coefficient of -1.91 (t-value= -2.70) compares to -1.88 (t-value= -5.89) for manufacturing em-

[29] These sustained negative impacts of trade exposure on local manufacturing employment for the U.S. are consistent with an analysis by Dix-Carneiro and Kovak (2017) for Brazil where trade liberalization in the 1990s persistently reduced employment in exposed regions, with even larger magnitudes than China trade impacts for the U.S.

[30] Workers exiting manufacturing employment may also end up incarcerated, enlisted into military service, homeless, or deceased. We observe these outcomes imperfectly in the data, but they are likely to be small in aggregate relative to the margins that we observe. Workers exiting the labor force may be of course replaced by new entrants, offsetting these losses. Our analysis captures net effects of the trade shock on employment.

[31] Bloom et al. (2019) report evidence that CZs experiencing greater import competition do see larger increases in non-manufacturing employment. We do not find this to be the case. We do, however, confirm their finding that employment losses spurred by the China trade shock are highly durable in less-educated commuting zones while they are ultimately more than fully offset in more-educated commuting zones (see section 4.4).

ployment at the same horizon. The impact attenuates modestly late in the period, ending at -1.74 (t-value$= -2.35$) over 2001 to 2019, which is close to the manufacturing impact at this horizon. At long time horizons, much of the net absorption of manufacturing job loss is through increases in non-employment. Amior and Manning (2018) argue that changes in the local employment-population ratio summarize changes in local average real income. By this logic, the China trade shock would have substantially altered the distribution of well-being across CZs, a possibility we explore in more detail below through our analysis of personal income per capita.

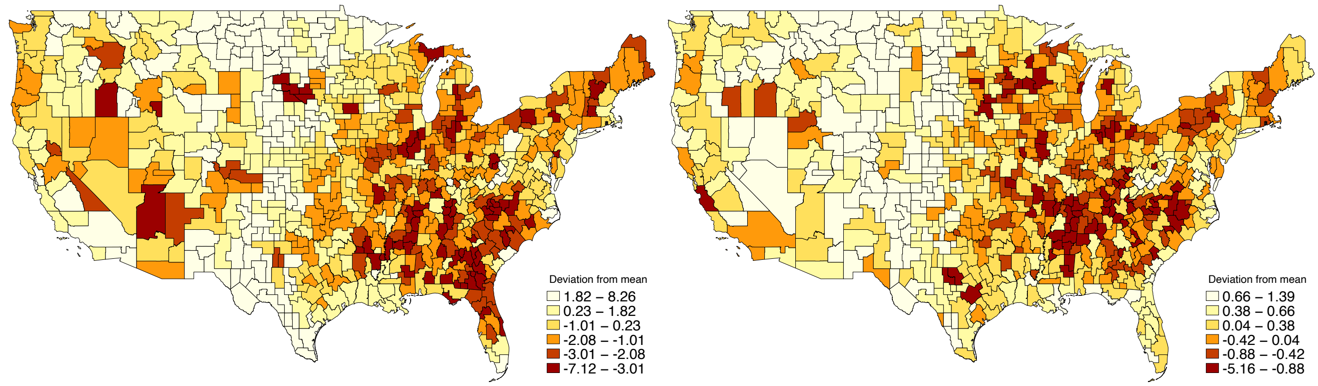
Figure 6a maps the actual change in wage and salary employment-population ratios across CZs for the 2001 to 2019 time period. While the national employment rate barely changed over this period ($+0.19$ percentage points), the map reveals considerable variation across space, with lower employment growth in parts of the South, Midwest and Northeast, and faster employment growth in the Great Plains and some of the Western and Northeastern coastal areas. In Figure 6b, we map the implied impact of the China trade shock (as measured in (1) for 2000 to 2012) on changes in employment-population ratios for the same 2001 to 2019 time period. A visual comparison between Figure 6's two panels shows a striking correlation, as many of the CZs that lost employment overall (in panel a) were also more adversely affected by the trade shock (in panel b). This correlation suggests that the China shock had an important influence on the differential employment growth across U.S. regions in the last two decades. The top 5% of CZs in terms of implied reductions in employment-population ratios, which are listed in Appendix Table A4, include a preponderance of locations that in 2000 were relatively highly specialized in manufacturing and had relatively few college-educated workers. In 2000, 33 of the 38 most exposed CZs had a manufacturing employment share above 25 percent, relative to the population-weighted median of 15.4%, and 33 of 38 had a college-educated share of the working-age population below 20 percent, relative to the national population-weighted median of 23.4%. The distinctiveness of trade-impacted commuting zones motivates the heterogeneity analysis we undertake in section 4.4.

To test robustness, Appendix Figure A10 replicates the analysis in Figure 5, now using Census and ACS data to construct changes in employment-population ratios. Census-ACS data have the advantage over the REIS of measuring employment as individuals with a job rather than total number of jobs in a CZ; the disadvantage of these household-survey-based data is that they offer comparatively small sample sizes in annual samples after 2000, requiring us to use combined ACS

Figure 6: Implied Impact of Import Competition on Employment-Population over 2001 to 2019

Change in total employment-population ratio

(a) Actual change (b) Predicted change due to China trade shock



Note: Panel (a) plots the observed change in the wage and salary employment-working-age population ratio over 2001 to 2019; panel (b) plots the implied change in this value based due to the China trade shock based on our estimates (i.e., the product of the change in import penetration over 2000 2012 in (1) multiplied by the 2SLS coefficient estimate for this variable from the specification in (2) in which the dependent variable is the 2001 to 2019 change in the ratio of wage and salary employment to the working-age population, as shown in Figure 5c). Legends indicate values for the bottom four quintles and the top two deciles of this value, arranged in order of shock intensity.

annual surveys. We estimate the impact of the China trade shock on employment-population ratios for three time differences: 2000 (using data from the 2000 Census 5% sample) to 2007 (using pooled 1% annual samples from the 2006-2008 ACS files), 2012 (using pooled 1% annual samples from the 2011-2013 ACS files), and 2018 (using pooled 1% annual samples from the 2017-2019 files).[32] The 2000-2007 period matches the later time period studied in Autor et al. (2013a), the 2000-2012 period matches the duration of our trade shock measure, and 2000 to 2017/2019 is the longest post-2000 period we can study using the Census-ACS. We see that for the 2000-2018 time difference, the impact coefficient for total employment-population ratio of -1.23 (t-value $= -3.32$) in Figure A10 compares to that in Figure 5c of -1.74 (t-value$= -2.35$) for 2001 to 2019. Similarly, the 2000-2018 impact coefficient for manufacturing employment-population of -1.67 (t-value $= -7.00$) in Figure A10 compares to that in Figure 5a of -1.74 (t-value$= -2.35$). These two results are similar, in light of the former estimates using 2000 as a start year (instead of 2001), combining 2017-2019 data for the 2019 observation, and measuring employment as persons employed instead of total jobs. One mild difference between the two sets of results is that the long-run (2000 to 2018) impact of the China trade shock on the non-manufacturing employment ratio is somewhat larger in Figure A10b ($\beta = 0.43$, t-value $= 1.35$) than in Figure 5b ($\beta = 0.05$, t-value $= 0.06$), though the estimate is

[32] See Appendix Table A3 for summary statistics; other regressions variables are the same as in Figure 5.

imprecise and far too small to prevent a decline in the overall employment rate.[33]

4.1.2 Unemployment

A decrease in the employment-population ratio potentially combines increased exits from the labor force with an increased number of workers who are jobless but searching for new employment. In Figure 5d, we examine the impact of trade exposure on the unemployment-to-population ratio, defined as the share of those unemployed in the working-age population, for time differences from 2000 to 2001 to 2000 to 2019.[34] Impact coefficients are positive, indicating that CZs more exposed to the China trade shock experienced larger increases in unemployment. These effects are statistically significant in just four of the 19 time periods, however, reaching reach their peak in 2011 with a coefficient of 0.50 (t-value= 1.90), indicating perhaps that the impact of the China trade shock was amplified by the Great Recession. When comparing CZs at the 25^{th} and 75^{th} percentiles of trade exposure, the more-exposed CZ would have a 0.33 ($= 0.50 \times [1.17 - .51]$) percentage-point larger increase in the share of the working-age population that is unemployed over 2000 to 2011. The positive impacts of greater import competition attenuate over time, dropping close to zero in 2017 and later years. Unsurprisingly, movements into unemployment play little role in absorbing the fall in employment over the long run (Blanchard and Katz, 1992). Any increase in unemployment, however, should have resulted in increased uptake of UI benefits, evidence of which we see in Appendix Figure A11a. There is a positive impact of trade exposure on UI benefits per working-age person over the first half of the 2000 to 2019 period, which becomes negative and imprecisely estimated after 2013.[35]

One of the most surprising results reported by Autor et al. (2013a) is that the adverse impacts of the China Shock on manufacturing and total employment-to-population persisted over at least a decade. At the time that paper was written, the China trade shock was ongoing, making it impossible to distinguish short-term from steady-state (or "medium run") impacts. The evidence

[33]Because the China trade shock began in the 1990s, one may view the specification in (2) as incomplete in that it does not control for the previous decade's trade shock. Appendix section A.5.1 explores these adjustment dynamics.

[34]Conventionally, the unemployment rate is defined the fraction of those in the labor force who are unable to find work. We use the unemployment-to-population ratio because the manufacturing, non-manufacturing, non-participation, and unemployment to population rates sum to one (meaning that a fall in any one quantity must be offset by an increase in the sum of the others). In practice, LAUS unemployment numbers differ from REIS employment numbers in concept and measurement. Whereas REIS measures the number of jobs based on detailed employment data, LAUS provides a model-based estimate of the number of local individuals who are unemployed.

[35]Whereas our analysis of unemployment is for 2000 to 2019, the analysis of manufacturing and non-manufacturing employment in Figure 5 covers the slightly shorter time interval of 2001 to 2019, owing to changes in REIS industry classifications from SIC to NAICS. In Figure A11b we report results for changes in the wage and salary employment-population ratio over 2000 to 2019, which are very similar to those in Figure 5c.

presented above makes clear that even in 2019, nine years after the plateau of the China trade shock, there is no recovery of manufacturing or total employment-to-population rates in trade-exposed CZs. To a first approximation, more-trade-exposed CZs suffered a durable reduction in the size of their manufacturing labor force, with the bulk of this decline translating into a long-run increase in non-employment. The consequences of these disruptions went far beyond earnings and employment. Job loss engendered social dislocation, in the form of lower marriage rates, increased single parenthood, a higher incidence of children living in poverty, and increased mortality from drug and alcohol abuse, especially among young males (Autor et al., 2019; Pierce and Schott, 2020).

4.2 Spillovers across Regions and Industries

The empirical specification in (2) is consistent with a setting in which commuting zones are functionally small open economies. If changes in economic conditions in a given CZ do not materially affect outcomes in other CZs, we can examine each commuting zone on its own without modeling the transmission of shocks across locations. Of course, trade shocks directly impacting one commuting zone may be transmitted to other CZs via changes in regional flows of goods and production factors. We next evaluate evidence of cross-region and cross-industry spillovers of trade shocks.

Changes in Local Labor Supply The impact of import competition on CZ population headcounts summarizes the net effect of trade shocks on the pool of both potential workers and non-working residents (e.g., children, elderly, and working-age non-participants). Labor supply responses to negative labor demand shocks may in turn differ by worker age. Younger workers, in particular, are likely to be relatively mobile (Bound and Holzer, 2000). We examine the responsiveness of population headcounts to greater import exposure separately for workers 18 to 24, 25 to 39, and 40 to 64 years old. This analysis is complicated by the fact that there are strong secular trends in population growth across U.S. regions, which began well before the China trade shock (Blanchard and Katz, 1992). Greenland et al. (2019) suggest that the approach used in Autor et al. (2013a) for evaluating the impact of trade shocks on labor supply, which conditions on a control vector similar to that in equation (2), does not account for such dynamics.[36] Accordingly, when we estimate (2) for log population headcounts, we additionally include as a control the log change in CZ population

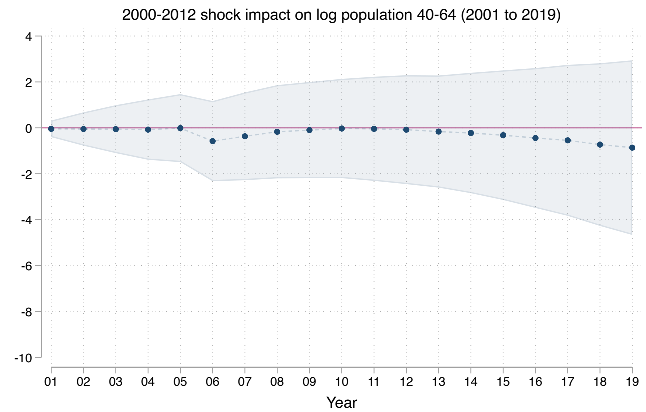
[36] Estimation results for the employment-population ratio are not subject to this critique because using the ratio of employment to population effectively differences out population growth trends.

over 1970 to 1990 in order to capture longstanding trends in population growth.

Figure 7: Trade Shock Impact on Log Population Head Counts, 2000-2019

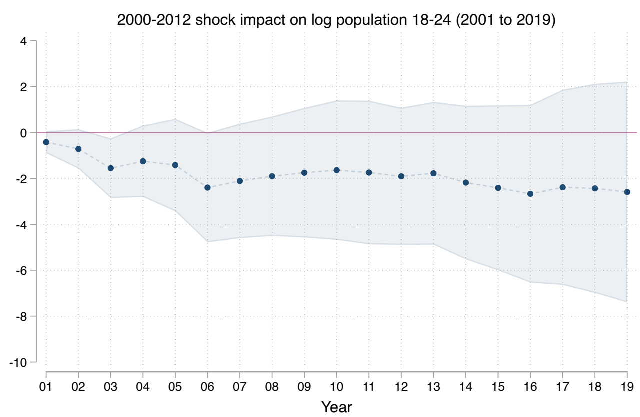
(a) Log population, ages 18-64

(b) Log population, ages 40-64



(c) Log population, ages 25-39

(d) Log population, ages 18-24



Note: Panels (a)-(d) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in the specified log population headcount between 2000 and the year indicated on the horizontal axis; the trade shock is the decadalized 2000-2012 change in CZ import exposure, as defined in (1) and instrumented by (3); and control variables are the same as in Figure 5 plus CZ population growth over 1970 to 1990. Regressions are weighted by the CZ total population in 2000; standard errors are clustered by state.

Figure 7 reports estimation results for population headcounts. In Figure 7a, we find negative but insignificant impacts of trade exposure on the size of the working-age population, though the point estimates grow as the time interval lengthens. The next three panels of the figure make clear that these negative effects are driven by one age group, those 25 to 39 years old. For adults ages 18 to 24 and 40 to 64, the impact of greater import competition on population headcounts is negative but small and highly imprecisely estimated for all time differences. The finding of no net impact of trade exposure on CZ populations is consistent with Faber et al. (2019), while the greater responsiveness of younger adults echoes Bound and Holzer (2000), who show that less-experienced workers are more relatively more mobile in the face of adverse shocks. For the 2000-2019 time difference, the coefficient estimate of -5.89 (t-value= -2.52) indicates that when comparing CZs at the 25^{th} and 75^{th} percentiles of trade exposure, the more-exposed CZ would have a -3.89 ($-5.89 \times [1.17 - .51]$) percentage-point larger decrease in headcounts for this age group. For comparison, the 75^{th}– 25^{th} percentile difference in population growth across CZs for individuals ages 25 to 39 over 2000 to 2019 was 20.96 ($= 16.08 + 4.84$) percentage points. Cross-CZ spillovers transmitted via net migration appear to be modest and concentrated on a narrow age group.

The greater mobility of the foreign-born may contribute to the labor-supply responsiveness to the China trade shock seen in Figure 7 (Cadena and Kovak, 2016). To explore this channel, we use Census and ACS data to evaluate the impact of trade exposure on population headcounts by nativity. Appendix Figure A12 summarizes regressions for two time differences: 2000 (using data from the 2000 Census) to 2010 (using data from the combined 2006-2010 ACS 1% samples), and 2000 to 2019 (using data from the combined 2015–2019 1% samples).[37] All other variables are the same as in the regressions in Figure 7. The impact of trade exposure on the total working-age population is negative for both the 2000-2010 ($\beta = -2.42$, t-value $= -1.92$) and 2000-2019 ($\beta = -3.47$, t-value $= -1.84$) time differences, and marginally significant in each case. When we examine the impact on the native-born population, we find a smaller and much less precisely estimated effect: the impact coefficient for 2000 to 2019 is -0.75 (t-value $= -0.79$). This contrasts with impacts on the foreign-born, for which greater import competition significantly reduces population headcounts over 2000 to 2019 ($\beta = -6.79$, t-value $= -2.05$). When looking at age subgroups within these populations, we see negative significant impacts of import competition on native-born workers ages 25 to 39 ($\beta = -5.03$, t-value $= -2.29$), which aligns with the results in Figure 7, and on foreign-born workers ages 40 to 64 ($\beta = -12.70$, t-value $= -2.08$). Whereas native-born labor-supply responses are strongest for younger workers, for the foreign-born they are strongest for older workers.[38]

[37]Because of small population counts of the foreign-born in many CZs in any individual year., we use combined five-year ACS samples rather than the combined three-year samples used in Appendix Figure A10.

[38]Bound and Holzer (2000) and Notowidigdo (2020) document that non-college adults are less mobile geographically than the college-educated in response to adverse shocks. In Appendix Figure A12, we use the 2000-2010 and 2000-2019 Census-ACS samples to evaluate the impact of trade exposure on population headcounts for the native and foreign-born of working age, separated into non-college and college adults. For the native-born over the 2000 to 2019 horizon, trade exposure does have larger negative impacts on headcounts for college than non-college adults, but neither impact is close to precisely estimated. For the foreign-born, negative trade impacts on headcounts are substantially larger overall, though they are greater in magnitude for non-college adults, and precisely estimated only for this group.

Gravity Based Trade Shocks To evaluate how changes in labor-market outcomes in one region affect outcomes in other regions, Adao et al. (2019a) build a general equilibrium trade model that captures such spillovers explicitly and generates reduced-form equilibrium conditions that have a shift-share structure. If national industries are subject to exogenous shocks, such as reform-induced growth in Chinese manufacturing, then employment and wages in regional economies will be affected through two channels. One is through changes in local industry revenue, which in the case of greater import competition will place downward pressures on local wages and employment, as captured by changes in import penetration in (2). Second, in the presence of cross-region spillovers, wages and employment in one region will also be affected by localized changes in import penetration in other regions, whether through cross-CZ trade or migration. For a given CZ, shocks to other CZs will matter more the larger and the closer are these other markets, as dictated by the gravity structure of trade. Adao et al. (2019a) quantify this cross-region spillover by adding to the specification in equation (2) the gravity-weighted changes in import competition all other regions (i.e., the sum of the trade shock in each region weighted by the size of and the distance to that region).

We incorporate their approach by estimating an extended version of equation (2) that includes a gravity-based measure of trade exposure in other CZs. Appendix A.5.3 provides details on the estimation and reports results. For all time differences, the impacts of own-CZ trade shocks are very similar in magnitude and significance to those in Figure 5. The impacts of gravity-based trade shocks are small and imprecisely estimated, both for manufacturing employment and non-manufacturing employment-population ratios. Evidence of gravity-based spillovers appears to be weak.

Cross-Industry Spillovers The literature also investigates spillovers in trade shocks between industries, which may operate both within and across regions. On within region transmission, Autor et al. (2014) find that workers' earnings are adversely affected both by shocks to their own industry of employment and by shocks to the industries of other workers in the same CZ (as captured by the average trade shock for these workers' industries). Acemoglu et al. (2016) and Pierce and Schott (2016) document spillovers that operate through input-output linkages in industry supply chains. Rising U.S. furniture imports from China, for instance, may cause U.S. factories in this downstream industry to reduce purchases of inputs from the upstream sectors with which it is linked—planed lumber, plywood, woodworking machinery, textiles, screws, and adhesives. Because buyers and suppliers often locate near one another, much of the impact of increased trade exposure in downstream industries may transmit to suppliers in the same regional market. Using US input-output data to construct upstream (supplier) and downstream (customer) import exposure shocks for both manufacturing and non-manufacturing industries, Acemoglu et al. (2016) estimate negative employment effects in industries whose customer industries are directly trade-exposed. Conversely, they find little evidence for differential employment changes in industries whose suppliers are directly trade-exposed. Trade exposure thus appears to primarily propagate upstream in the supply chain—that is, from trade-exposed customers to their suppliers.

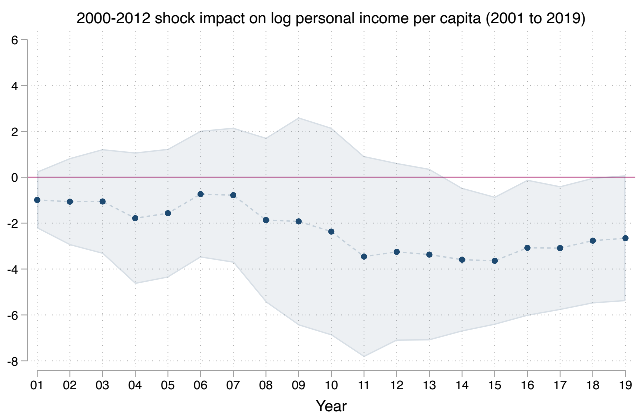
4.3 Personal Income, Labor Compensation, and Government Transfers

Adverse impacts of trade shocks on local labor demand are likely to reduce labor income, especially among low–wage workers. Chetverikov et al. (2016) and Autor et al. (2019) find that greater exposure to import competition from China caused larger relative reductions in earnings in the bottom four deciles of earnings distributions within CZs, where declines were larger for men than for women. Yet, impacts of trade exposure on labor earnings provide only a partial sense of the consequences for economic activity. Reductions in the employment-population ratio in a commuting zone may dampen demand for local goods and services and housing, possibly reducing revenues flowing to local business owners and landlords. Feler and Senses (2017) document negative impacts of the China trade shock on CZ housing values and property tax revenues, though they do not find evidence that more trade-exposed commuting zones saw larger declines in the number of business establishments. Declines in local income may in turn trigger greater uptake of government transfers, as more individuals qualify for means-tested government assistance or elect to retire and begin to receive Social Security income. Autor et al. (2013a) find that trade exposure caused an increase in government transfer receipts in adversely affected commuting zones, with most of the uptake accounted for by retirement, disability, and medical benefits. These prior analyses extend only through 2007, however, so they do not illuminate the long-run impact of the China trade shock on local area income.

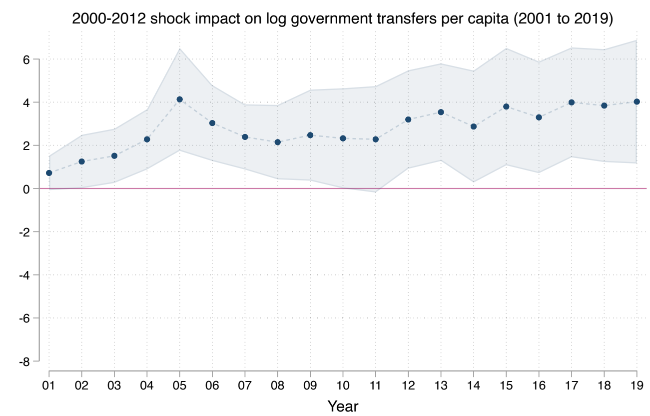
To fill in this picture, we examine how trade exposure affects the components of personal income per capita. BEA estimates of local area personal income include wages, salaries, bonuses, employer contributions to health and pension plans, proprietor income (income of sole proprietorships, partnerships, tax-exempt cooperatives), financial returns (rent, interest, dividends, realized capital gains), government transfers (both cash and in-kind), and adjustments to capture income by place of residence. By including as many sources of income as possible, and by seeking to assign income according to the place of residence of recipients, BEA personal income per capita approximates aggregate local income per capita and is therefore suitable for evaluating the distributional consequences of trade exposure across CZs.

Figure 8: Trade Shock Impact on Income, 2000 to 2019

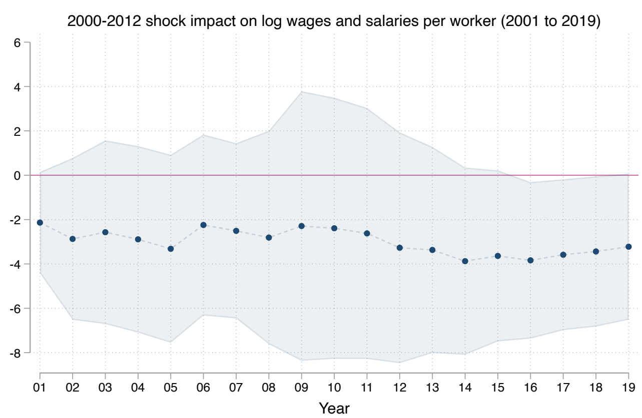
(a) Log personal income per capita



(b) Log government transfers per capita



(c) Log total labor compensation per worker



Note: Panels (a)-(c) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in the specified outcome between 2000 and the year indicated on the horizontal axis; the trade shock is the decadalized 2000-2012 change in CZ import exposure, as defined in (1) and instrumented by (3); and control variables are the same as in Figure 5. Regressions are weighted by the CZ population in 2000; standard errors are clustered by state.

Estimation results appear in Figure 8, where outcomes are in terms of log income and transfers relative to the total population of a CZ, except for total labor compensation, which is relative to wage and salary employment.[39] The impact of exposure to import competition on personal income per capita in Figure 8a is negative at all time horizons and precisely estimated for most time differences from 2000 to 2014 forward. Negative effects reach their peak for the 2000 to 2015 time difference, for which the impact coefficient is -3.64 (t-value $= -2.55$). The moderately attenuated coefficient estimate of -2.66 (t-value $= -1.90$) for the 2000 to 2019 time difference indicates that for CZs at the 25^{th} and 75^{th} percentiles of trade exposure, the more-exposed CZ would have a 1.76 ($-2.66 \times [1.17 - .51]$) percentage point smaller increase in personal income per capita over 2000 to 2019, which compares to the 25^{th} – 75^{th} percentile difference in the growth of personal income per capita over 2000 to 2019 of -12.2 ($= 21.3 - 33.5$) percentage points. Consistent with Autor et al. (2013a), we see in Figure 8b that more trade-exposed CZs had larger increases in government transfer receipts per capita, which remain elevated through 2019. For the 2000 to 2019 time difference, the impact coefficient of 4.03 (t-value $=$ 2.75) implies that at the 75^{th}- 25^{th} percentile difference in trade exposure, the more exposed CZ would have a 1.81 ($2.75 \times [1.17 - .51]$) percentage point larger increase in transfers per capita over the time period, relative to the 75^{th} – 25^{th} percentile difference in the growth of transfers per capita of 10.90 ($= 67.6 - 56.7$) percentage points.

In Appendix A.5.4, we further explore the impact of trade shocks on government transfers by program type. Consistent with results in Autor et al. (2013a) for earlier time periods, adjustments in Social Security and Medicare benefits account for most of the responsiveness in government transfers induced by greater import competition, where the magnitude of these benefit gains expands as the time horizon lengthens. To receive these benefits, an individual must have left the labor force, either through retirement or by being declared medically unable to hold a job. The primary means through which government transfers replace labor income lost due to import competition is thus by accommodating an exit from paid work, which may help account for the long-run negative effects of trade exposure on employment-population ratios we see in Figure 5. Despite trade-induced lower incomes, means-tested government programs meant to provide income assistance to poor households are instead largely unresponsive to greater import competition.

The negative overall impact of trade exposure on personal income implies that uptake of government transfers, though clearly responsive to greater import competition, did not fully offset income losses from other sources. To probe the origin of these losses, we examine how trade exposure affected

[39] We normalize personal income by the total (rather than working-age) population, given that non-working-age individuals may earn income from non-labor sources and receive government transfers of various kinds.

the non-transfer components of personal income: total labor compensation (wages, salaries, bonuses, employer benefits, shown in Figure 8c), financial returns, and proprietor income (shown in Appendix Figure A14).[40] CZs facing greater trade exposure had smaller increases in all income sources. Aggregating across these sources, for the 2000 to 2019 time difference, the impact coefficient on all personal income except government transfers is -3.99 (t-value $= -2.40$) (Figure A14a). At this horizon, the 25^{th}- 75^{th} percentile difference in trade-exposure impact of -2.64 ($-3.99 \times [1.17 - .51]$) percentage points compares to the 25^{th} – 75^{th} percentile difference in non-transfer income growth of -13.1 ($= 14.5 - 27.6$) percentage points. From equation (2), the trade-induced log change in personal income per capita over 2000 to 2019 equals the sum of the product of the change in import penetration over 2000 to 2012, the impact coefficient for each component of personal income, and the initial share of each component in personal income. The 2000-2019 trade-induced change in government transfers of 0.53 ($= 0.89 \times 4.0 \times .15$) percentage points would have only offset 16% of the 3.01 ($= -0.89 \times 3.99 \times .85$) percentage point trade-induced decline in other income sources.

4.4 Heterogeneity in Impacts across Regions

We have so far focused on the average response of commuting zones to import competition, which provides insight into the overall implications of the China trade shock. These average impacts mask heterogeneity in how CZs adjust to adverse shocks. Among the most trade-impacted CZs, there is wide variation in initial industrial specialization and labor-force composition (see Appendix Table A4). Consider, for instance, North Hickory (2000 population $378k$), and Raleigh-Cary (2000 population $1,420k$), both of which are located in North Carolina. Over 2000 to 2012, the two CZs were each above the 95^{th} percentile of trade exposure, with increases in import penetration of 4.4 percentage points for the industries in North Hickory and 3.4 percentage points for those in Raleigh-Cary. However, these towns entered the 2000s with distinctly different economic structures. North Hickory was a traditional factory town. In 2000, 43.0% of its working-age population had a manufacturing job and just 15.6% had a bachelor's degree. Raleigh-Cary, by contrast, was more educated and industrially diversified. In 2000, 34.2% of its working-age population had at least a college degree, and only 17.0% worked in manufacturing. These initial differences may have shaped

[40] Over the 2000 to 2019 period, the share of labor compensation in CZ personal income fell from 66.6% to 60.6%, while the income share of financial returns rose from 18.5% to 19.6%, the income share of government transfers rose from 12.6% to 17.9%, and the income share of proprietor income was unchanged at 8.7%.

how the two commuting zones responded to the large and sudden increase in import competition.

A substantial literature documents that U.S. regions with fewer college-educated workers have grown less rapidly (e.g., Glaeser et al., 1995; Behrens et al., 2014; Diamond, 2016) and have seen larger declines in employment rates (Austin et al., 2016). To explore the role that a "dearth of human capital" contributes to poor adjustment to the China shock, we separately examine CZs that initially had smaller versus larger supplies of college workers.

Other work studies how greater specialization leaves regions more exposed to industry-specific shocks (e.g., Feyrer et al., 2007; Michaels, 2011). Many of the labor-intensive U.S. industries exposed to trade with China were agglomerated in small and medium-sized cities in the U.S. Southeast and Midwest (Ellison et al., 2010). Existing work documents that the impact of the China shock was greater in CZs that at the outset had lower employment rates (Austin et al., 2016) and were more specialized in mature industries positioned later in their product cycles (Eriksson et al., 2019).[41] We explore the role that "reverse agglomeration" contributes to poor adjustment to the China shock, we separately analyze CZs that initially were less versus more industrially specialized.

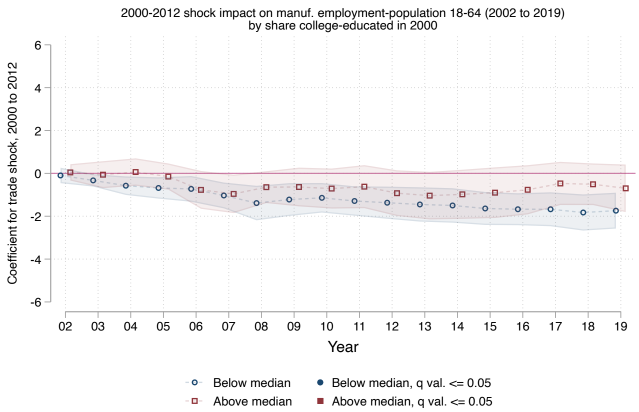
Figure 9 presents estimates in which we divide the sample of CZs into groups based on whether the share of the college-educated in their working-age populations was above or below the population-weighted national median in 2000. There are 336 CZs in the former group and 386 in the latter group. We consider four outcomes: the manufacturing employment-population ratio, the total wage and salary employment-population ratio, the log working-age population, and log personal income per capita. To keep the time horizon the same across outcomes, we evaluate time differences from 2001 to 2002 to 2001 to 2019. To control the false discovery rate when evaluating differences in coefficient estimates across sample splits, we compute and display minimal Benjamin-Hochberg q-values based on the number hypotheses being tested.[42]

[41] In Brazil, Dix-Carneiro and Kovak (2017) document that regional manufacturing continued to decline well after trade reform had permitted greater import competition, in a manner consistent with agglomeration economies.

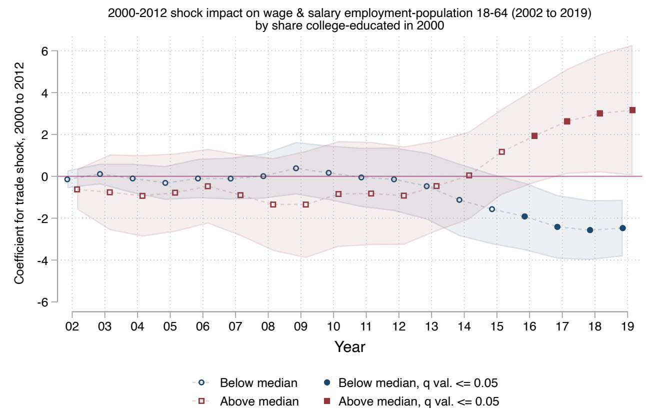
[42] The number of hypotheses (144) is the product of the four outcomes, 18 years in the sample, and two sample splits. In the standard Benjamini-Hochberg procedure, hypotheses are ranked according to their unadjusted p-value. For a fixed significance level q, the researcher rejects all hypotheses that satisfy $p < qr/M$, where p is the p-value, r is the rank of the p-value, and M is the number of hypotheses being tested. Following the step-up method in Anderson (2008), we find the smallest q at which each null hypothesis can be rejected. In Figures 9 and 10, we show the coefficients whose differences across sample splits have a minimal q-value of less than 0.05 with solid markers, and use hollow markers everywhere else.

Figure 9: Heterogeneity in Trade Shock Impacts: Initial College Educated Population

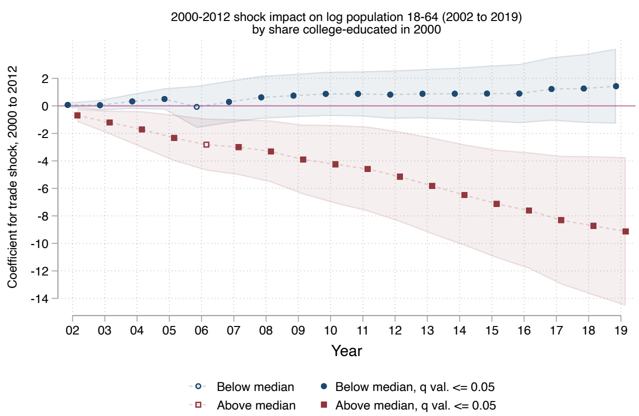
(a) Manuf. employment/Working-age pop.



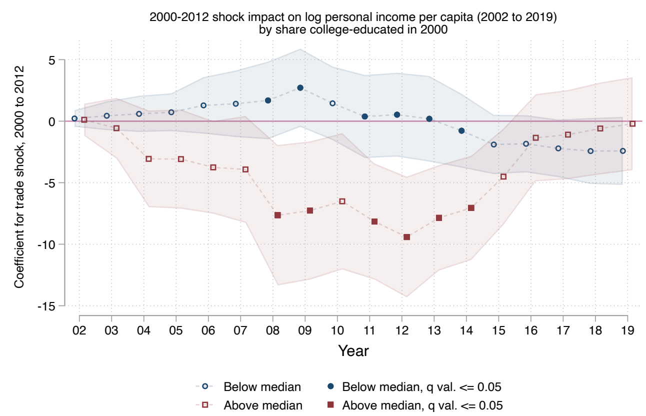
(b) Wage & salary employment/Working-age pop.



(c) Log working-age population



(d) Log personal income per capita



Note: Panels (a)-(d) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. Coefficient estimates whose differences have a minimal Benjamini-Hochberg q-value of less than 0.05 are shown with solid markers (with hollow markers for other estimates). Estimates are reported for two samples: the 386 CZs whose share of the college educated in the working-age population was below the population-weighted national median in 2000, and the complementary set of 336 CZs. The dependent variable is the change in the indicated measure between 2001 and and the year on the horizontal axis; the trade shock is the decadalized 2000-2012 change in CZ import exposure as defined in (1) and instrumented by (3); control variables are the same as in regressions reported in Figure 5. Regressions in panels (a)-(c) are weighted by the CZ working-age population in 2000; regressions in panel (d) are weighted by the CZ total population in 2000. Standard errors are clustered by state.

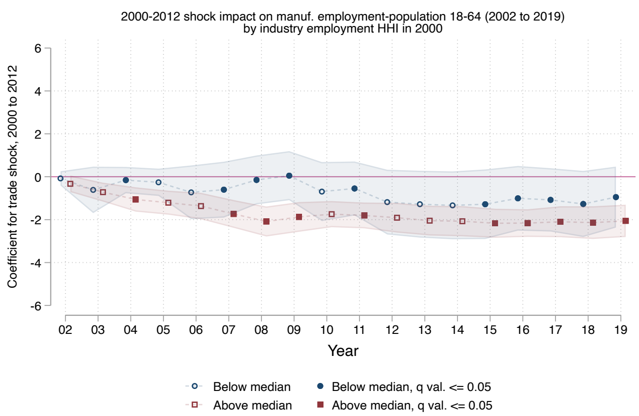
Although both higher and lower-educated CZs experienced declines in manufacturing employment, the negative impacts of trade exposure on overall employment in Figure 5 are concentrated in CZs with relatively few college-educated workers, as shown in Figures 9a and 9b. These results are consistent with those in Bloom et al. (2019), who study the 1992 to 2012 time period. For the 2001 to 2019 horizon, the first two figures show that in less-educated CZs, a one percentage point increase in import penetration over 2000 to 2012 predicts a 1.74 (t-value $= -4.07$) percentage point decrease in the manufacturing employment-population ratio and a 2.47 (t-value = −3.59) percentage point decrease in the total wage and salary employment-to-population ratio. Across all CZs (see Figure 5), trade-induced changes in manufacturing and total employment-population ratios are very similar at long horizons, indicating that employment impacts on non-manufacturing employment are null. For CZs with relatively few college workers, the substantially larger impact on total employment than on manufacturing employment reveals a negative impact of trade exposure on non-manufacturing employment, as shown in Appendix Figure A17. Despite manufacturing job losses being compounded by non-manufacturing losses in these CZs, there is no effect of trade exposure on the log working-age population (Figure 9c). It thus appears that more trade-impacted CZs with fewer college-educated workers did not experience differential out-migration, though they did experience larger declines in personal income per capita, as shown in Figure 9d. Since economically-motivated migration would tend to mitigate the local disemployment effects of trade exposure, one interpretation of these results is that the *lack* of migration is both symptom and cause of the slow adjustment process.

In CZs with more-educated working-age populations, the pattern of adjustment is qualitatively different. Impacts of trade exposure on manufacturing employment (Figure 9a) are negative, but somewhat smaller and less precisely estimated. Impacts on the total wage and salary employment-population ratio are small and imprecise for short time differences and then become large, positive, and marginally significant for long time differences. For the 2001-2016 time difference and beyond, we easily reject that impact coefficients for wage and salary employment-population are the same in more- versus less-educated CZs. This trade-induced increase in the total employment-population ratio must imply a corresponding positive impact of trade exposure on the non-manufacturing-population ratio, as seen in Appendix Figure A17.

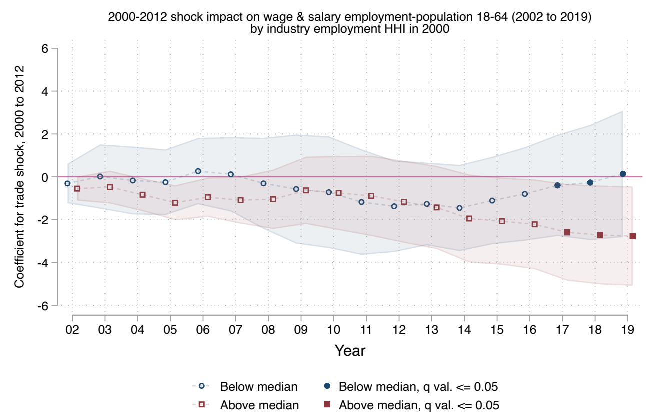
As critically, more-educated CZs adjusted to adverse trade shocks in part through net out-migration. For all time differences except 2001 to 2006, we reject that impact coefficients on the working-age population are equal for more versus less-educated CZs. At the 2001 to 2019 time difference in more-educated CZs, a one percentage point increase in import penetration is predicted to cause a 9.13 (t-value = −3.31) percentage point decrease in the working-age population, or an annual population decline of approximately one-half a percentage point. The impact coefficient on log non-manufacturing employment at the 2001 to 2019 time difference of 6.04 (t-value = 2.37, Appendix Figure A17b) implies that more of the increase in the non-manufacturing employment population ratio occurred through the out-migration of labor rather than through greater job growth. Like less-educated CZs, more-educated CZs also see negative impacts of trade exposure on personal income per capita (Figure 9d). Distinct from less-educated CZs, these impacts reach their maximum negative value for the 2001 to 2012 time difference and then diminish over time, becoming close to zero for the 2001 to 2016 horizon and beyond.[43]

Figure 10: Heterogeneity in Trade Shock Impacts: Initial Industry Specialization

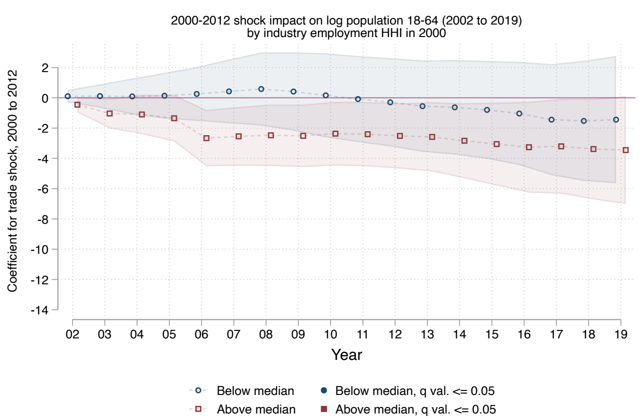
(a) Manuf. employment/Working-age pop.



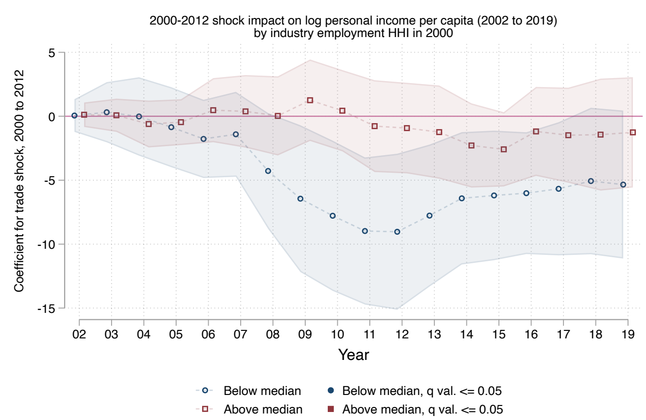
(b) Wage & salary employment/Working-age pop.



(c) Log working-age population



(d) Log personal income per capita



Note: Panels (a)-(d) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. Coefficient estimates whose differences have a minimal Benjamini-Hochberg q-value of less than 0.05 are shown with solid markers (with hollow markers for other estimates). Estimates are for two samples: the 619 CZs whose Hirschman Herfindahl Index of industry specialization was above the population-weighted national median in 2000, and the complementary set of 103 CZs. The dependent variable is the change in the indicated measure between 2001 and and the year on the horizontal axis; the trade shock is the decadalized 2000-2012 change in CZ import exposure as defined in (1) and instrumented by (3); control variables are the same as in regressions reported in Figure 5. Regressions in panels (a)-(c) are weighted by the CZ working-age population in 2000; regressions in panel (d) are weighted by the CZ total population in 2000. Standard errors are clustered by state.

[43]Appendix Figures A17c and A17d show that in both more and less-educated CZs trade exposure induced an increase in government transfers per capita, with the majority of this increase accounted for by payments related to Social Security and Medicare.

Next, we consider a second dimension of heterogeneity in adjustment to the China trade shock. In Figure 10, we split the sample of CZs by their industrial specialization in 2000, which we measure using a Hirschman-Herfindahl Index (HHI) equal to the sum of the squared shares of CZ employment in each industry.[44] There are 103 CZs with HHIs above the population-weighted median, and 619 CZs with HHIs below it. The uneven split in CZs across the two groups reveals, unsurprisingly, that large commuting zones are less specialized by industry than smaller and medium-sized CZs. In Figures 10a and 10b, it appears that the negative impacts of trade exposure on manufacturing and wage and salary employment across all CZs are driven predominantly by smaller, more industrially-specialized commuting zones. For these CZs, there are negative and significant impacts of greater import competition on manufacturing employment and total wage and salary-employment-population ratios. For less specialized CZs, impacts on manufacturing employment are small, negative and imprecise, and impacts on total employment are null. In the more-specialized CZs (Figure 10c), but not in the less-specialized ones (Figure 10c), trade exposure induces larger decreases in the working-age population. Despite this, employment-population ratios decline by relatively more in more trade-exposed and more specialized CZs. Our conclusions about differences in adjustment between more and less-specialized CZs are tentative, however, given that for most time horizons and outcomes we cannot reject that impact coefficients for the two groups of CZs are the same.

4.5 Putting the Pieces Together

The first wave of China shock research found that greater import competition caused localized job loss in manufacturing, declines in earnings for low-wage workers, and greater economic distress across a wide range of outcomes. The primary mechanism of adjustment to trade exposure was exit from work, rather than increased employment in non-manufacturing or migration to other regions. The impacts of rising import competition, originally documented for 1991 through 2007, are manifest out to 2019, nearly two decades after China's accession to the WTO in 2001 and nine years after the plateau of China's export surge. More trade-impacted CZs suffered durable increases in joblessness and decreases in personal income per capita that are not close to being offset by government transfers. The resulting economic distress appears to be most acute in local labor markets that lacked abundant supplies of college-educated workers—consistent with the dearth of human capital hy-

[44]We construct these HHIs using ACS data for 2000 and the 1990 Census industry classification code.

pothesis—and that were narrowly specialized in labor-intensive manufacturing—consistent with the reverse agglomeration hypothesis. For CZs such as North Hickory, North Carolina, the consequences of the China trade shock have been profound and long-lasting.

There are other plausible explanations for the lack of an out-migration response to the China trade shock. These include downward pressure on housing values in contracting CZs (Feler and Senses, 2017), which for renters may have diminished the pressure to leave and for homeowners may have complicated selling their homes (Glaeser and Gyourko, 2005; Notowidigdo, 2020). Lower housing values may have further blocked local recovery by slowing the formation of new businesses, which are often financed using home equity (Davis and Haltiwanger, 2019).[45] It does not appear, however, that regional variation in labor market regulations account for differential adjustment to the China shock. The impacts of trade exposure were no less acute in CZs located in right-to-work states or states with lower minimum wages (Chan, 2019).

There are alternative characterizations of the China trade shock, which deserve mention. One is that the U.S. labor market is too dynamic for the China shock to have mattered much. If every year millions of jobs are created and millions of jobs are destroyed, how could job loss in a collection of factory towns be important? A second is that manufacturing job loss would have happened anyway.[46] That is, import competition from low-wage countries *other than* China (Hanson, 2000), rising capital intensity in manufacturing (Fort et al., 2018), and the availability of industrial robots (Acemoglu and Restrepo, 2020) would ultimately have closed factories and displaced workers, irrespective of China's rise. Both lines of thought ignore how the China trade shock was highly concentrated in place and time. The concentration in place was due to the relocation of manufacturing to smaller cities and towns over the 20^{th}century, made possible by the maturation of industrial production (Eriksson et al., 2019), and improved transportation (Kim, 1995; Michaels, 2008). The concentration in time was due the speed of China's rise and the immensity of its economy (Naughton, 2007). The scarring effects of job loss in trade-exposed CZs was made more acute by this job loss occurring during trade-induced local economic recessions (Jacobson et al., 1993; Davis and von Wachter, 2011). Workers

[45] Housing market regulations in large U.S. cities could also have impeded adjustment in trade-exposed regions. Inelastic housing supply in major cities, due in part to housing regulations (Glaeser et al., 2005), may hinder low-wage workers in the Heartland from moving to expensive coastal cities (Hsieh and Moretti, 2019).

[46] A related argument, that it was technological change and not import competition that caused manufacturing job loss in trade-exposed CZs, has two weaknesses. One is that the cross-CZ correlation between exposure to automation and exposure to import competition is very low (Autor et al., 2013b); a second is that there was no obvious abrupt acceleration in technological change after 2001 (indeed, productivity growth slowed after 2004).

were not just losing their jobs, they were experiencing the dislocating effects work disappearing in their communities (Wilson, 2011; Autor et al., 2019; Charles et al., 2019).

5 Was the China Shock Unique?

A question arises whether the China trade shock was a singular event with limited relevance for other shocks, or whether it holds general lessons about concentrated job loss. The regional consequences of trade-induced employment contractions in the United States mirror those of the broader decline of manufacturing. Charles et al. (2019) document that U.S. commuting zones suffering greater job loss in manufacturing after 2000—whether because of import competition, automation, or other forces—had larger declines in employment rates and wages out to 2017. We do not know, however, whether this pattern of adjustment is closely comparable to shocks that are not specific to manufacturing or that occurred at other moments in time.

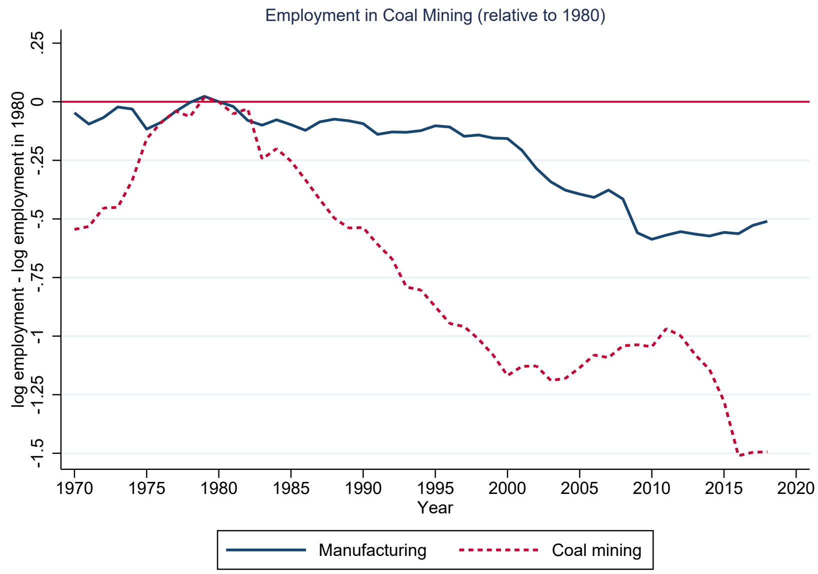
To provide a benchmark against which to compare the China trade shock, we examine outcomes associated with two other major events whose impacts were also highly localized. One is the decline of the coal industry, which after 1980 suffered two decades of contraction due to changes production technology and energy demand (Black et al., 2005; Jacobsen and Parker, 2016).[47] A second is the Great Recession, in which a housing collapse and subsequent credit freeze differentially affected U.S. regions (Mian and Sufi, 2014; Charles et al., 2018; Beraja et al., 2019; Yagan, 2019). The first episode lets us evaluate long-run adjustment to a sectoral shock during an earlier period, while the second involves adjustment to an unusually deep cyclical shock.

5.1 Labor Market Adjustment to the Decline of Coal

The secular decline of employment in the coal industry is a case of a spatially concentrated shock that precedes the China's rise. Figure 11 plots employment in U.S. coal mining and, for reference, employment in U.S. manufacturing, each expressed as the log for a given year relative to the log value in 1980. There was a boom in coal production during the 1970s due to a spike in energy prices caused by the decade's two major oil price shocks. Employment in coal rose from 140,600 workers in 1969 to 263,600 workers in 1979. After 1980, coal prices began an extended decline. Employment in

[47] On regional boom and bust cycles in energy production, see Michaels (2011), Allcott and Keniston (2017), Feyrer et al. (2017), Bartik et al. (2019), Clay and Portnykh (2018) and Kearney and Wilson (2018).

Figure 11: Employment Decline in Coal Production vs. Manufacturing



Note: Data are from the BEA National Industry Product Accounts.

coal followed suit, falling to 151,200 workers in 1990 and to 80,400 workers in 2000. After rebounding modestly in the 2000s, coal employment plummeted after 2011. Given these patterns, we specify the coal shock as the change in coal employment over 1980 to 2000. Our analysis builds on Black et al. (2005), who find that during the 1983-1993 coal bust, counties more specialized in coal (as indicated by initial coal reserves) in Kentucky, Ohio, Pennsylvania, and West Virginia had larger decreases in employment rates, total earnings, and earnings per worker. We expand their analysis forward in time from 1993 to 2019 and include all U.S. commuting zones.[48]

To evaluate local labor market adjustment to the decline of coal, we adapt the specification in equation (2) to the following:

$$\Delta Y_{it+h} = \alpha_t + \beta_{1h} \Delta SS_i^{coal} + \mathbf{X}_{it}' \beta_2 + \varepsilon_{it+h}, \tag{4}$$

where ΔY_{it+h} is the change in an outcome for CZ i between year $t+h$ for $h = -5, \ldots, 39$, and the base year $t = 1980$; and ΔSS_i^{coal} is a shift-share variable that projects the coal shock onto CZ i, and $\mathbf{X}_{it}$ is a vector of controls. We specify the decadalized shift-share coal shock as,

$$\Delta SS_i^{coal} = -\left(\frac{100}{20}\right) \times \frac{L_{ic1980}}{L_{i1980}} \left[\ln L_{coal,2000}^{-i} - \ln L_{coal,1980}^{-i}\right], \tag{5}$$

[48]Because CZs are aggregates of counties, our analysis may dampen the county-level effects in Black et al. (2005).

where L_{ic1980}/L_{i1980} is the share of coal production in the employment of CZ i in 1980, and the term in brackets is the log change in national employment in coal production over 1980 to 2000, outside of the state in which CZ i is located. To facilitate interpretation, in the regression analysis we multiply the shock in (5) by -1, such that a higher value of the shift-share variable indicates a larger negative change. The population-weighted mean of this projected employment change is 0.22 ($\sigma = 1.25$) percentage points.[49] Control variables in the regression include a dummy for the CZ having positive coal employment in 1980, time trends for Census region divisions, and parallel to above, values in 1980 for the share of CZ employment in manufacturing, the share of women in CZ employment, the share of the college educated in the CZ population, and the share of the foreign born in the CZ population. Because of the spatial concentration of coal deposits, the shock hit a relatively small number of commuting zones: just 258 (35.7%) of CZs had positive coal employment in 1980, and the population-weighted median value of the shock is only 0.001 percentage points. By contrast, the shocks at the 90^{th} and 95^{th} percentiles were 0.19 and 0.83 percentage points, respectively.

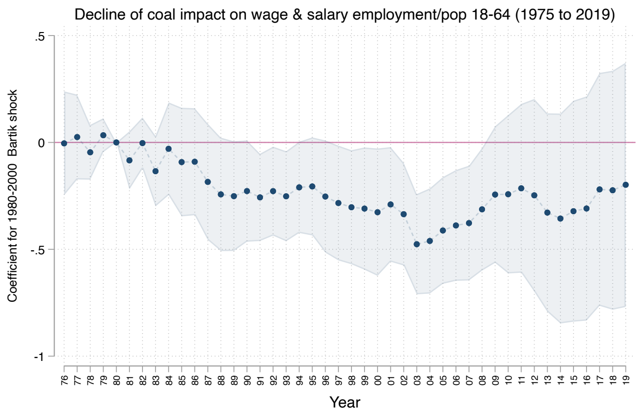
Estimation results appear in Figure 12. The impact coefficients for the ratio of wage and salary employment to population (Figure 12a) become increasingly negative during the 1980-2000 shock. They are largest for the 1980-2003 horizon (-0.48, t-value$= -3.98$), and remain negative for all horizons out to 1980 to 2019, though the estimates for the longest time windows lack statistical precision. For the 1980 to 2006 difference—on the eve of the Great Recession and 26 years after coal's decline had begun—the impact coefficient of -0.39 (t-value$= -2.94$) indicates that a CZ subject to a 1980-2000 coal shock at the 95^{th} percentile of intensity would have experienced at 0.32 ($= 0.39 \times [0.83 - .001]$) percentage point larger decline in its wage and salary employment-population ratio than a CZ at the median of shock intensity. For context, over 1980 to 2006 the $5^{th} - 50^{th}$ percentile difference in the change in employment-population was -9.4 percentage points.

In Figure 12b, we see that CZs more exposed to the decline of coal also had larger reductions in total labor compensation per worker, which persist well beyond the 1983-1993 period studied in Black et al. (2005) and are precisely estimated at long horizons. Over the 1980 to 2019 ($\beta = -1.05$, t-value$= -2.46$), a CZ subject to a coal shock at the 95^{th} percentile of intensity would have seen a 0.88 ($= -1.05 \times [0.83 - .001]$) log point greater decline in average labor income.

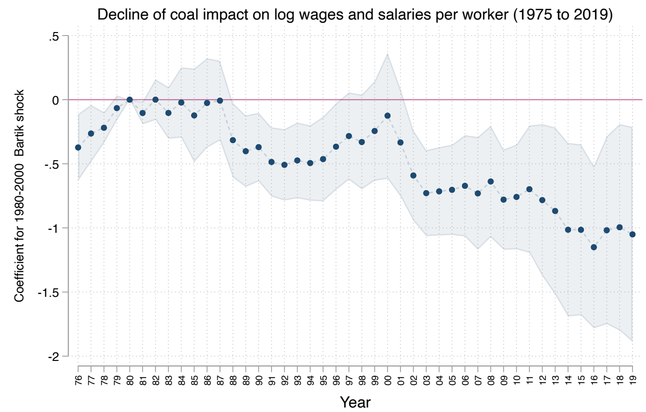
[49] Given our shock definition, this indicates that the average change in coal employment across CZs was negative. The correlation between the coal shift-share variable (5) and the China shock shift-share variable (2) is -0.08.

Figure 12: Local Labor Adjustment to the Decline of Coal

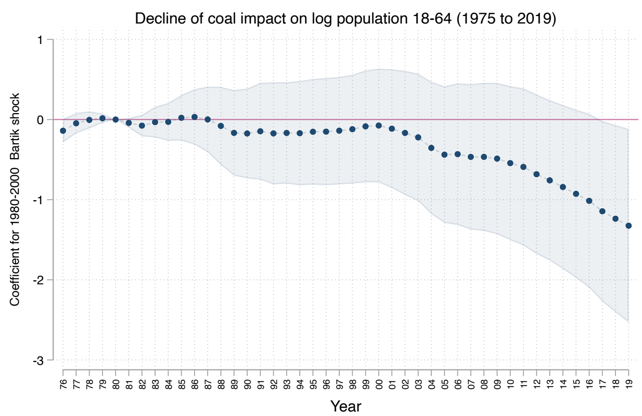
(a) Wage & salary employment/Working-age pop.



(b) Log labor compensation per worker



(c) Log population, ages 18-64



Note: Panels (a)-(c) report 2SLS coefficient estimates for β_{1h} in (4) and 95% confidence intervals for these estimates. The dependent variable is the change in the indicated measure between 1980 and and the year on the horizontal axis; the coal shock is defined in (5); control variables in the regression include a dummy for the CZ having positive coal employment in 1980, time trends for Census region divisions, and values in 1980 for the share of CZ employment in manufacturing, the share of women in CZ employment, the share of the college educated in the CZ population, and the share of the foreign born in the CZ population. Regressions are weighted by the CZ working-age population in 1980; standard errors are clustered by state.

Given these lasting adverse labor market impacts of the coal shock, one might expect that heavily exposed CZs saw substantial population declines. However, Figure 12c shows that the impact of exposure to the coal shock on population headcounts is small and imprecisely estimated for the first two decades after the coal contraction began. Larger negative population responses appear only well after 2000, and are statistically significant only for time periods beyond 1980 to 2016. For the 1980 to 2019 difference, the impact coefficient of -1.33 (t-value= -2.15) indicates that a CZ subject to a coal shock at the 95th percentile of intensity would have had a 1.22 ($= -2.23 \times [0.83 - .001]$) log point larger decline in its working-age population when compared to a CZ at the median of shock intensity. These impacts are small and do not appear until 25 years after the shock initiated.

Qualitatively, Figure 12 indicates that the reaction of employment, earnings, and population to the coal shock was similar to the response to the China import shock (see Figures 5, 7 and A14). In both cases, CZs with greater exposure to the adverse sectoral shocks suffered declines in employment and earnings that persisted well beyond the period reaching peak intensity. These local labor markets eventually saw a decline in population but that decline was neither immediate nor large. Despite these *qualitative* similarities between the coal shock and the China trade shock, there is a substantial *quantitative* difference between the two. Whereas the Figure 5 estimates suggest that the China shock induced a decline in the wage and salary employment-population ratio of -1.55 ($= -1.74 \times 0.89$) percentage points between 2001 and 2019, the corresponding employment decline for the coal shock was only -0.04 ($= -0.198 \times 0.22$) percentage points per decade from 1980 to 2019.[50] The much smaller average impact of the coal shock may be unsurprising given that only a subset of CZs produce coal. Nor did the coal shock generate the extremes of variation in labor market outcomes across CZs that we observe for the China trade shock. Although wage and salary income per worker in a CZ at the 95^{th} percentile of exposure to the coal shock declined by 0.88 log points more than in a CZ at the 5^{th} percentile over the 1980-2019 period, the differential impact of the China shock on wages from 2000 to 2019 was three times as large when comparing CZs at the 75^{th} and 25^{th} (instead of 95^{th} and 5^{th}) percentile of exposure.

5.2 Labor Market Adjustment to the Great Recession

As a second point of comparison, we evaluate local labor market adjustment to the Great Recession. We adapt the specification in (4) where ΔY_{it+h} becomes the change in an outcome for CZ i between year $t+h$ for $h = -5, \ldots, 13$, and base year $t = 2006$; and ΔSS_i^{gr} is a shift-share variable that projects the Great Recession onto CZ i, and $\mathbf{X}_{it}$ is a vector of controls. The NBER dates the Great Recession as beginning in December 2007, and ending in June 2009. Because U.S. housing markets, which played a major role in the downturn, began contracting in 2006 (Charles et al., 2016), we specify the shock as commencing in that year. We estimate (4) for 19 separate time periods: five

[50]We obtain these estimated impacts by multiplying the impact coefficient for the wage and salary employment/working-age population ratio (-1.74 for the China shock, -0.19 for the coal shock) that corresponds to the longest time difference (2001 to 2019 for the China shock, 1980 to 2019 for the coal shock) times the decadalized mean of the respective shock (0.89 for the China shock, 0.22 for the coal shock).

periods before the shock begins (2001-2006 to 2005-2006), to check for pre-trends in outcomes; three shock periods (2006-2007 to 2006-2009), to examine shock impacts as the shock intensifies; and 12 periods after the recession had ended (2006-2007 to 2006 to 2019), to evaluate longer-run adjustment.

Similar to (5), we specify the annualized Great Recession shock in shift-share form as,

$$\Delta SS_i^{gr} = -\left(\frac{100}{3}\right) \times \sum_j \frac{L_{ij2000}}{L_{i2000}} \left[\ln L_{j,2009}^{-i} - \ln L_{j,2006}^{-i}\right], \tag{6}$$

where L_{ij2000}/L_{i2000} is the share of industry j in employment of CZ in the year 2000, and the term in brackets is the log change in national employment in industry j over 2006 to 2009, outside of the state in which CZ i is located. To help interpret the results, we again multiply the shock in (6) by -1, such that a higher value indicates a larger negative projected change in employment. The population-weighted mean of the shift-share variable is 2.19 ($\sigma = 0.73$) percentage points—on average, CZs faced strongly contractionary forces during the recession period. The control variables included in the regressions are the same as those in the China shock regressions in (2).[51]

Yagan (2019) documents that the recovery in unemployment rates after the Great Recession was not matched by a recovery in employment rates. In CZs more adversely affected by the downturn, employment rates were suppressed out to 2015. Charles et al. (2016) document further that declines in employment rates were concentrated among the less-educated. We extend Yagan's analysis out to 2019, using CZ-level REIS data, as compared to his use of federal income tax records.

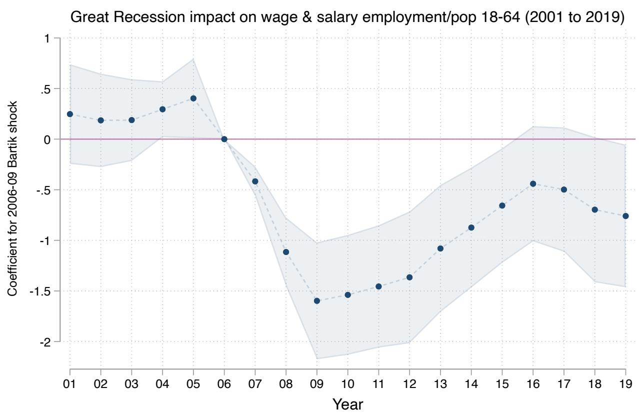
Estimation results appear in Figure 13. CZs more exposed to contracting industries during the Great Recession saw larger declines in their employment rates (Figure 13a). Impact coefficients for the wage and salary employment-population ratio become rapidly more negative as the recession intensified, reaching their peak value for the 2006-2009 difference ($\beta = -1.60$, t-value$= -5.42$). As the post-2009 expansion began, CZs began to recover from earlier declines in employment; the impact coefficient for the 2006-2016 period is less than one third the size of the 2006-2009 effect. This recovery appears to stall in 2016, however. For the 2006 to 2019 long difference, CZs that were more exposed to the Great Recession still register net declines in their employment-population ratios. The impact coefficient of -0.76 (t-value$= -2.11$) at that horizon indicates that a CZ subject to the mean Great Recession shock over 2006 to 2009 would have experienced a 1.66 ($= 2.19 \times 0.76$)

[51]The correlation between the Great Recession shift-share variable in (6) (over 2006 to 2009) and the China Shock shift-share variable in (2) (over 2000 to 2012) is 0.48. For robustness, in Appendix Figure A18 we replicate the results in Figure 13 in which we include as a control variable the instrument for the China trade shock, as defined in (3). Coefficient estimates and patterns of significance for the two sets of results are very similar.

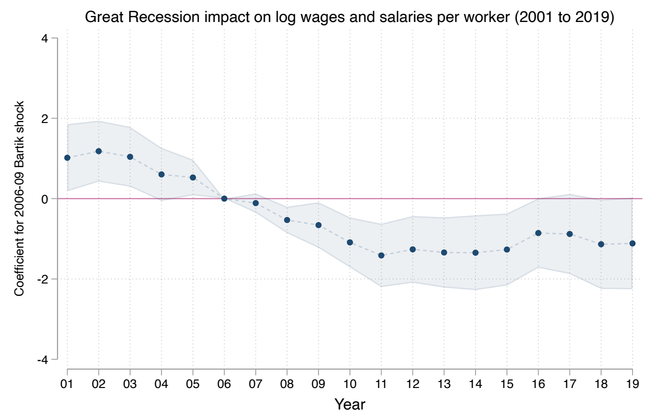
percentage point decline in its wage and salary employment-population ratio, relative to the mean change for the 2006 to 2019 period of +2.20 percentage points ($\sigma = 3.89$).

Figure 13: Local Labor Market Adjustment to the Great Recession

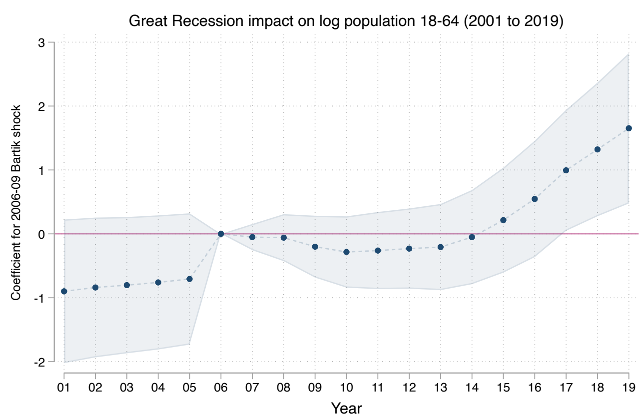
(a) Wage & salary employment/Working-age pop.



(b) Log labor compensation per worker



(c) Log population, ages 18-64



Note: Panels (a)-(c) report 2SLS coefficient estimates for β_{1h} in (4) and 95% confidence intervals for these estimates. The dependent variable is the change in the indicated measure between 2006 and and the year on the horizontal axis; the Great Recession shock is defined in (6); control variables include initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies. Regressions are weighted by the CZ working-age population in 2001; standard errors are clustered by state.

In Figure 13b, we see that CZs more exposed to the Great Recession had larger declines in wage and salary income. The impact reaches its maximum for the 2006-2011 time difference ($\beta = -1.41$, t-value$= -3.52$), then attenuates modestly through 2016, after which recovery stops. Over the 2006-2019 long difference, a commuting zone subject to the mean Great Recession shock would have experienced a 2.45 ($= -2.19 \times 1.12$) percentage point decline in compensation per worker.

The enduring declines in employment ratio and labor income per capita over the period 2006 to 2019 could indicate persistent scarring effects of the Great Recession. However, Figure 13a and 13b also show that CZs with greater exposure to the Great Recession had differentially faster growth in employment and labor income leading up to the recession. To the extent that this faster growth was driven by a temporary boom in construction that expanded employment and earnings to unsustainable levels (Charles et al., 2016), one might expect that these variables would not fully recover following the recession. Indeed, the Figure 13c estimates for population belie the expectation of a lasting downturn in the CZs with greater exposure to the Great Recession. These CZs experienced only modest and imprecisely estimated population declines during the recession, and from 2006 to 2016 onward, had *faster* relative population growth. This rapid growth raises the denominators for both the employment-population ratio and average wage and salary income, thus helping to explain why the recovery of those outcomes stalls in exposed CZs after 2015. These CZs were rapidly gaining population, which is likely a positive economic development.

In summary, we observe that Chinese import competition, the fall in demand for coal, and the Great Recession each reduced employment and labor incomes as these shocks unfolded. CZs with greater shock exposure did not experience large declines in population in any of these cases, suggesting that migration did not disperse the local impacts of these shocks. CZ more exposed to the Great Recession eventually experienced a substantial recovery of their employment rates, and faster population growth. Those with greater exposure to the China or coal shocks instead endured persistently depressed employment rates and labor income levels, combined with a slow decline in population. The comparison with the coal shock in particular indicates that the China shock's long-lasting impact on CZ labor market conditions and the sluggish population response to depressed local labor market conditions was not without precedent. However, the large magnitude of the China shock, which had a sizable impact on many local labor markets in the US, sets it apart from shocks such as the decline of the coal sector whose impact was more limited in scope. As the United States prepares for potentially more job loss due to the ongoing energy transformation and expected changes in oil and gas production, the failure of local labor markets to adjustment successfully to the coal and China trade shocks reminds us that the adjustment process is typically slow and sclerotic, unlike the textbook model of frictionless labor market adjustment.

6 The Distributional Consequences of the China Trade Shock

Local labor markets more exposed to import competition from China suffered larger declines in manufacturing jobs, employment-population ratios, and personal income per capita. These effects persist for nearly two decades beyond the intensification of the trade shock after 2001, and almost a decade beyond the shock reaching peak intensity. The absence of a substantial out-migration response to the shock indicates that resident populations in exposed CZs bore the brunt of local job loss and its repercussions. While it is not controversial to infer that trade-exposed commuting zones suffered relatively larger losses on average in economic well-being than did less trade-exposed CZs, these results do not reveal which if any CZs experienced *absolute* declines in average welfare or by how much. Computing absolute effects requires combining reduced-form empirical analysis, which estimates relative effects, with a general equilibrium model, which quantifies aggregate gains.

6.1 Quantitative General Equilibrium Analyses of the China Trade Shock

For individuals living in a given U.S. commuting zone, trade with China affects well-being by changing earnings and consumption possibilities. If economic reform in China raises its productivity in labor-intensive manufacturing, economies with a comparative advantage in these industries would see demand for their factor services contract, thus diminishing their consumption possibilities, while economies whose comparative advantage lies elsewhere would see their factor demand and consumption possibilities expand (Arkolakis et al., 2012). If labor is fully mobile across regions and sectors within a country, as has traditionally been assumed in modeling trade shocks, then the change in welfare would be common across regions. The evidence above clearly contradicts such baseline assumptions; to a startling degree, trade shocks appear to have an enduring impact on the locations in which their immediate impact is felt. To interpret cross-region differences in welfare impacts emanating from trade shocks, theoretical models must incorporate frictions—normally labor market frictions—that can give rise to the concentrated geographic impacts that we observe in the data.

As a benchmark for how labor immobility affects the welfare impact of the China trade shock, consider the analysis in Galle et al. (2020).[52] They combine an Eaton and Kortum (2002) model of heterogeneous firms with a Roy (1951) model of heterogeneous workers (Lagakos and Waugh, 2013; Hsieh et al., 2019). Workers are immobile across regions and partially mobile across sectors.

[52] For an early welfare analysis of China's trade expansion, see Hsieh and Ossa (2016).

They specify the China trade shock as innovations to China's industrial productivity, which they calibrate for the period 2000 to 2007 by backing out the implied productivity growth needed to generate the observed change in U.S. manufacturing imports from China that is predicted by the first-stage regression of Autor et al. (2013a). Using these innovations, and making assumptions on other parameter values, they calculate that the average change in real income across CZs from trade with China is 0.22%, with a (unweighted) standard deviation across CZs of 0.31.

The analysis in Caliendo et al. (2019), also based on an Eaton and Kortum (2002) model, allows for labor mobility in the longer run. They introduce dynamic adjustment in labor allocations across U.S. states and sectors, which arises from workers facing a fixed cost in moving from one region-industry to another and preference shocks in region-industry choice. Applying a calibrated China trade shock over 2000 to 2007 (again using Autor et al., 2013a), and estimated model elasticities, they find that labor reallocation across sectors and regions is complete by about 13 years after shock initiation (i.e., 2013, which is well within the period of our analysis). At this horizon, the average change in welfare is 0.20%, with a (unweighted) standard deviation across U.S. states of 0.09.[53]

In both studies, because goods prices are assumed identical across regions, *aggregate* welfare impacts are similar.[54] Naturally, regional variation in welfare impacts is larger in Galle et al. (2020), owing to geographic labor immobility. The two models also differ in how they model labor market frictions, which determines how trade shocks affect the overall employment rate.[55] In Caliendo et al. (2019), non-employment may arise because of an option to engage in home production; in an extended version of Galle et al. (2020), non-employment arises because of the home-production option and search and matching frictions in the labor market. It is notable that in the latter analysis, eliminating search and matching frictions and home production yields little change in the mean and variance of income changes across CZs. Wherever regional variance in income changes is coming from, it does not appear to be labor-market frictions within regions as modeled.

To evaluate such quantitative analyses of the China trade shock, we next compare changes in the

[53]We thank Lorenzo Caliendo for providing this estimate. Regional labor reallocation does not eliminate variation in welfare impacts across regions because migration costs and region-specific preferences create real adjustment costs.

[54]Kim and Vogel (2020) provide a related assessment of trade with China, which we discuss in Appendix A.6.

[55]In related work, Adao et al. (2019a) modify the baseline framework in Galle et al. (2020) by making trade between CZs costly and production subject to external economies of scale. If some trade-exposed CZs are proximate to other trade-exposed CZs, the impact of adverse trade shocks will be magnified via localized changes in trade flows, where scale economies may intensify such effects. Their counterfactual analysis implies that the China trade shock *reduced* average real income by 0.35%, with a standard deviation across CZs of 0.56. In our own analysis in Appendix A.5.3, we find only weak evidence of gravity-based spillovers of trade shocks across regions.

regional dispersion of income per capita in these analyses with reduced-form estimates of the trade-induced change in income variance. Because the quantitative approaches target the average impact of trade exposure on manufacturing employment in their calibrations, untargeted moments, such as regional dispersion in income changes, are useful for assessing modeling approaches. In considering regional variation in trade impacts, we abstract from within-region variation in the welfare impacts of trade shocks, which would arise from differential effects of trade exposure on earnings and price levels across households within a CZ. On earnings, Chetverikov et al. (2016) and Autor et al. (2019) find that lower-wage workers suffered larger percentage declines in earnings in CZs harder hit by the China trade shock. Our focus on personal income per capita aggregates over such worker-level variation. On prices, although the China trade shock appears to have raised consumer purchasing power, Borusyak and Jaravel (2021) document that shares of imports in consumer expenditures are flat across the income distribution, which implies that the purchasing-power effects of freer trade are neutral distributionally. Relatedly, Bai and Stumpner (2019) and Hottman and Monarch (2020) find that the impact of trade with China on U.S. product prices was similar across income groups (and geographic regions). To a first approximation, we can analyze the impact of the China shock on welfare in the U.S. as the sum of a price level effect, which is presumed homogeneous across regions, and an income effect, which varies across regions.[56]

6.2 Relative and Absolute Welfare Effects across Regions

Following Galle et al. (2020), we can write the average change in welfare for a commuting zone due to the China trade shock as the induced change in real income,

$$\hat{W}_i = \frac{\hat{Y}_i}{\hat{L}_i} \prod_j \hat{P}_j^{-\beta_j} \tag{7}$$

where $\hat{W}_i \equiv W_i'/W_i$ is the trade-induced change in income per capita (Y_{ci}/L_{ci}) in U.S. region i, relative to the change in the national price index, which applies the β_j Cobb-Douglas expenditure shares for products in each sector j to induced changes in the price level P_j for each sector j. Because Galle et al. (2020) calibrate their model to the 2000-2007 trade shock, they implicitly assume that the long run impacts of the shock had been realized by 2007. For context, we see in Figure 5 that impact of trade exposure on manufacturing employment over 2001 to 2007 ($\beta = -1.45$, t-value

[56] In Appendix A.6, we discuss results in Feler and Senses (2017) on how trade affects housing values.

$= -4.56$) is approximately 80 percent of that over the 2001 to 2019 long difference ($\beta = -1.79$, t-value $= -5.42$). It thus seems reasonable to compare their simulated standard deviation in income changes (0.31) to that estimated for a long-run time change.

As a reduced-form application of the welfare formula in (7), we estimate the impact of trade shocks on the variation in average income, Y_{ci}/L_{ci}. Specifically, we evaluate the deviation in changes income per capita in each CZ relative to the national weighted average,

$$\ln \frac{\hat{Y}_i}{\hat{L}_i} - \sum_l \gamma_l \ln \frac{\hat{Y}_l}{\hat{L}_l} = \beta_1^y \Delta IP_{i\tau}^{cu} - \sum_l \gamma_l \beta_1^y \Delta IP_l^{cu}, \tag{8}$$

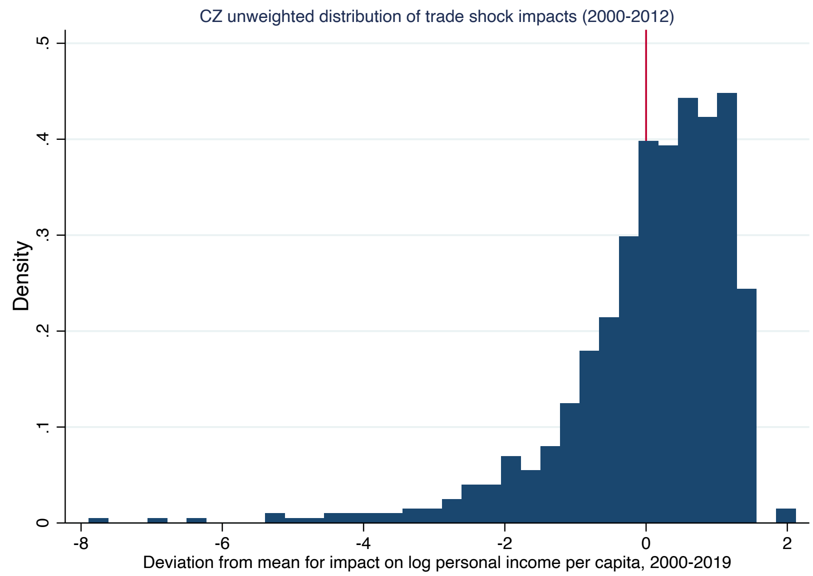
where γ_l is the share of region l in the U.S. population, β_1^y is the parameter estimate in (2) for the log change in personal income per capita, and $IP_{i\tau}^{cu}$ is the trade shock. This relative impact estimates leaves unmeasured the shock impact on the overall price level, which we treat as common across regions, as consistent with Caliendo et al. (2019) and Galle et al. (2020).

In Figure 14, we show the estimation-implied variance in (8) induced by the 2000-2012 trade shock over the 2000 to 2019 horizon.[57] The (unweighted) standard deviation in trade shock impacts on personal income per capita across commuting zones over 2000 to 2019 is 1.22 percentage points, and thus far exceeds the cross-CZ income dispersion generated by quantitative models. Appendix Table A4 lists the CZs that are above the 95^{th} percentile of *gross* income losses—not accounting for any offsetting gains from trade—which is 2.23 percentage points. The number of CZs that suffered *net* declines in welfare depends, of course, on the magnitude of the gains from trade that potentially offset these gross losses. The high variance of impacts across CZs, relative to apparently small average gains from trade in quantitative analyses, raises the possibility that many CZs suffered net welfare losses. In Caliendo et al. (2019) and Galle et al. (2020), gains from trade in the aggregate are 0.22% or less. When adding this value to the gross losses calculated above, the implied number of CZs experiencing net losses from trade is 223, representing 32.8% of the U.S. continental population in 2000. Even if we double these gains to 0.44%, 173 commuting zones, representing 15.9% of the U.S. population, would be estimated to have suffered net absolute welfare losses.

The translation of the CZ-level relative effects into absolute level effects hinges on the impact of trade on consumer prices. Jaravel and Sager (2019) estimate the impact of the China trade shock

[57] The impact coefficient for 2000 to 2019 in Figure 8a is marginally significant (t-value $= -1.90$). Impacts are statistically significant for all time differences from 2000 to 2014 to 2000 to 2018. Because these coefficients are larger in absolute value than the one for 2000 to 2019, using the long difference is a conservative choice.

Figure 14: Implied Variation in Changes in Personal Income per Capita, 2000 to 2019



Note: This figure shows a histogram for the welfare change in (8), evaluated for the 2000-2012 trade-shock induced change in personal income per capita (based on results in Figure 8a), expressed as the deviation in shock impacts from the population-weighted national mean. The impact coefficient used is that for the 2000-2019 time difference ($\beta = -2.66$, t-value $= -1.90$). The standard deviation in the implied shock impact is 1.22 percentage points.

on consumer prices in a reduced-form analysis. Using the 1991-2007 trade shock from Autor et al. (2014), they find that a 1.0 percentage point larger increase in import penetration produces a decline in consumer prices of 1.4%.[58] Using the mean decadalized change in import penetration over 2000 to 2012 of 0.89 percentage points (Table 1), the implied reduction in consumer prices based on Jaravel and Sager (2019) is 1.25%. If the positive impact of trade exposure on consumer prices is indeed this large—over five times the gains from the China trade shock based on quantitative analyses—the number of commuting zones suffering net losses from trade is smaller but still meaningful. In Figure 14, there are 82 CZs representing 6.3% of U.S. population with declines in personal income per capita of 1.25% or more. For these CZs, the net effect welfare effect of trade with China is negative, even when applying the sizable Jaravel-Sager price index adjustment. Given the large dispersion of per capita incomes across CZs in Figure 14, there would have to be enormous consumer price benefits from trade with China in order to generate net average gains for *all* U.S. commuting zones.

[58] Dorn and Levell (2021) analyze the impact of trade with China on consumer prices in the United Kingdom and obtain treatment effects that are about half as large as those in Jaravel and Sager (2019).

7 Concluding Discussion

Economists have long known that individuals are scarred by job loss. Displaced workers earn significantly less than similar workers who are not displaced, even years after displacement. The China trade shock caused locally concentrated job loss, which led to lasting declines in employment rates and income levels in the most exposed communities. Despite these now well-documented adverse labor market impacts of globalization, there is no consensus about how to remediate such injuries.

The United States does little to protect workers from mass-layoff events such as the China trade shock. Although Trade Adjustment Assistance delivers benefits to some workers who have been displaced by trade shocks (Hyman, 2018), the program is too small to have mattered in the aggregate for workers who lost their jobs because of import competition in the 1990s and 2000s. A further limitation of TAA is that it conditions assistance on the cause of job loss, i.e. trade. Presumably, job loss is equally scarring, no matter whether the underlying cause of displacement is import competition, technological change (Autor and Dorn, 2013; Acemoglu and Restrepo, 2020), government regulation (Walker, 2013), or some other factor. Policy failures in this domain are far from innocuous. The political gains of U.S. and European nationalist-populists, for instance, have been greater in regions that have suffered larger trade-induced employment declines (Colantone and Stanig, 2018a,b; Autor et al., 2020a; Rodrik, 2020).

The favored solution of populist politicians to regional distress is to raise import barriers and block immigration. Indeed, the Donald J. Trump Administration cited the adverse impacts of the China trade shock to justify taking aggressive trade action against China (Redding, 2020). The subsequent U.S. China trade war succeeded in elevating U.S. product prices (Amiti et al., 2019; Fajgelbaum et al., 2020; Cavallo et al., 2021) but not in expanding employment in import-protected sectors (Flaaen and Pierce, 2019). We are aware of no research that would justify ex-post protectionist trade measures as a means of helping workers hurt by past import competition.

Recent literature suggests that fostering employment growth in regions beset by chronic joblessness may help workers hurt by persistent negative local labor demand shocks.[59] Austin et al. (2016) find that employment impacts of labor demand shocks are larger in local markets in which joblessness was initially high. Bartik (2020) argues further that job growth in distressed regions is especially beneficial for low-wage workers. Absent success in helping regions left behind by globaliza-

[59] On optimal spatial policies, see Fajgelbaum and Gaubert (2020) and Gaubert et al. (2020).

tion and other disruptive shocks, one may expect continued popular support for political platforms that disparage foreign trade, despite the apparent inefficacy of these platforms to date.

It is now clear that the China trade shock as we understand it appears to have stopped intensifying a decade ago. The 1992 to 2012 China Shock was about China's one-time transition to a market economy. This transition, which affected U.S. industries that were late in their economic lifecycles, was in some sense forecastable based on market fundamentals, both in China and the United States. The next China shock may be more likely to be triggered by industrial policy, e.g., if China makes good on its promise to support advanced technology industries. Such a shock may depend less on market fundamentals and more on China's future interventionist policy choices—and the reactions of the United States and other countries to these choices—making its dimensions difficult to foresee. Because of China's immense scale and willingness to enact sweeping policy changes in short order, future 'China shocks' may again have profound albeit unpredictable consequences.

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Appendix

A.1 Sectoral Trade Patterns for China

A.1.1 Trade Data

The trade data we use to examine revealed comparative advantage are from UN Comtrade and based on SITC Rev. 2 product codes (https://comtrade.un.org). We thank Robert Feenstra and Mingzhi Xu for access to a cleaned version of these data. We use SITC product codes instead of HS product codes in order to calculate world exports by product in all years. HS codes, which we use to construct the China trade shock in the empirical estimation so as to be able to concord trade values to SIC industries, were adopted after 1991 by many countries. Changes in SITC data in 2017 and 2018 appear to have moved trade in some of China's manufactured goods into SITC 931 (special transactions and commodities not classified according to kind, which is a category for anomalies and errors in trade flows). The share of SITC 93 in China's merchandise exports rose from 1.3% in 2000 to 4.2% in 2016 and then to 13.7% in 2017, before dropping slightly to 13.4% in 2018.

We classify as manufacturing SITC one-digit categories 5 (chemicals), 6 (manufactured goods classified by material, excluding SITC 68), 7 (machinery and transport equipment) and 8 (miscellaneous manufactures), and select two-digit (09, 11, 25, 41, 42, 43), three-digit (012, 014, 023, 024, 035, 037, 046, 047, 048, 056, 058, 062, 071, 072, 073, 091, 098, 111, 112, 122, 233, 246, 248, 251, 266, 267, 334, 335, 411, 423, 424, 431), and four-digit (0224, 0612, 0615, 0619, 3413) categories from other one-digit sectors. Because other definitions of manufacturing trade (e.g., World Development Indicators) exclude SITC 0 (food), 1 (beverages, tobacco), 2 (crude materials), 3 (mineral fuels), and 4 (animal, vegetable oils) from manufacturing in their entirety—despite the presence of manufactured products within these categories—they may generate values for China's share of world trade that are somewhat lower than those we report here.

A.1.2 Cross-Sectoral Variation in China's Export Performance

Movements in China's revealed comparative advantage for overall manufacturing, as seen in Figure 2, obscure sectoral variation in export performance, which we explore in Appendix Figures A1 to A4. These figures show China-U.S. relative log RCA values in the left column and China's share of world exports in the right column. They cover the 54 three-digit SITC Rev. 2 products for which China's log RCA equals or exceeds 0.5 at some point during the 1991 to 2018 period, meaning that China's share of world exports of that product was at least 1.65 ($= \exp(0.5)$) times China's share of world exports of all goods. There are 237 three-digit SITC Rev. 2 products in total and 160 products that are manufactures. We exclude a handful of products in which China briefly had a high RCA and then moved into comparative disadvantage territory: SITC Rev. 2 products 014 (meat products), 111 (non-alcoholic beverages), 585 (resins and other plastic materials), 786 (trailers and other non-motorized vehicles), and 897 (jewelry made from precious materials). By choosing sectors based on China's RCA *relative to the world as a whole* while presenting China's RCA *relative to the U.S.*, we display the relative productivity advantage of China over the U.S. in the goods for which China's comparative advantage is global, rather than U.S. specific. These strong comparative advantage products accounted for of 82.7% of China's manufacturing exports over the period; they comprised 41.4% and 29.5% of world and U.S manufacturing exports, respectively.

We organize products into four groups according to when they reach their maximum RCA relative to the U.S. In a first group of products, shown in Figure A1, China achieves its maximum RCA over the U.S. before 1997. These products include labor-intensive apparel and textiles (e.g., women's outerwear, underwear, handbags, leather goods, carpets) in Figures A1a and A1b, and simple manufactures (e.g., dried foods, explosives and fireworks, radios, assorted plastic goods) in Figures A1c and A1d. In all cases, these are goods in which China already had a strong comparative in 1992. In handbags, underwear, women's outwear, woven fabric, and yarn, the China-U.S. log relative RCA exceeded a value of 3 in the early 1990s, indicating an initial differential of China-U.S. exports in the product relative to China-U.S. exports of all manufactures of over 300 log points. The China-U.S. relative RCA in these goods is either stable or declines modestly over time. Yet, because China's overall export capacity was expanding, its share of world exports in these products continued to grow, at least until around 2015. For each of the six just-mentioned apparel and textile products, China's world export share exceeded 35% by 2006, before stabilizing over the following 12 years. For simple manufactures, shown in Figures A1c and A1d, the China-U.S. relative RCA declines from high initial values over time, while China's shares of world exports in these goods are roughly stable (except for pig iron). As China diversified its exports away from the simple products shown in Figure A1, its revealed comparative advantage in these products diminished, even as it maintained its share of world exports.

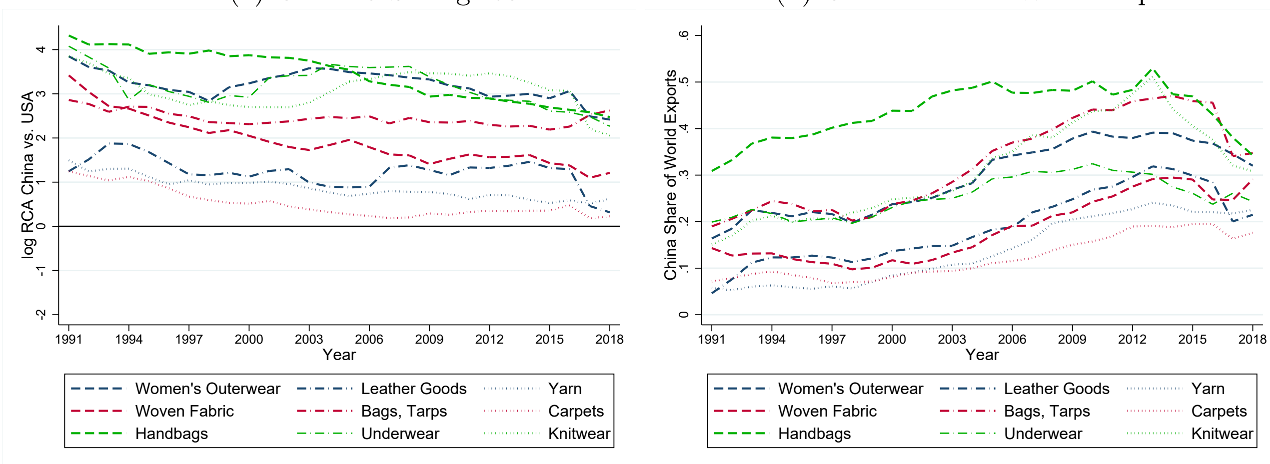
A second group of products includes those in which China reached its peak RCA between 1997 and 2002, shown in Figure A2. This group is comprised of moderately more sophisticated goods than those shown in Figure A1, including electrical power machinery, heating, lighting and plumbing fixtures, bicycles, motorcycles and scooters, and cutlery. These products also register high initial RCAs relative to the U.S. in the early 1990s, which rise into the late 1990s before plateauing and then declining modestly in ensuing years. Again, even as the RCAs for these products stabilized or diminished, China's expanding overall export capacity meant that its shares of world exports in these products continued to grow until finally stabilizing after 2015. In footwear, household equipment, and lighting and plumbing fixtures, China's share of world exports reached or exceeded 40%, in toys and games it exceeded 50%, and in pottery it exceeded 60%.A third group of products, shown in Figure A3, includes more sophisticated exports, such as computers, computer equipment, recording equipment, and TVs. China's RCA in these goods peaked between 2002 and 2007. For computers and computer equipment, China began with a strong comparative disadvantage, as indicated by large negative relative RCAs in the early 1990s. Its RCA for these goods rose sharply over time, and its share of world exports ultimately exceeded 50% for computers and 30% for computer equipment.

Although the general pattern is for China to achieve its peak RCA early for less-sophisticated products (e.g., women's outerwear) and later for more-sophisticated products (e.g., computers), there are many exceptions to this rule. China did not achieve its peak RCA until after 2002 in men's outwear, knitted underwear, and embroidered items, all labor-intensive products. Once China achieved comparative advantage in a sector, its rapid overall economic growth ensured that it maintained a dominant position in world exports for the good, even if its revealed comparative advantage diminished. In just two cases in Figures A1-A4, pig iron and fur skins, do we see goods in which China gained and then lost a substantial share of world exports in goods in which it registered a strong RCA sometime after 1991. For most products, once the China shock commenced it did not subside.

A final group consists of products for which China's RCA peaks after 2007. As with the products in which China's RCA peaked after 2002, this group contains both relatively sophisticated products, such as telecommunications equipment (e.g., cellphones) and optical instruments, and less-sophisticated products, such as clay and cotton and man-made fabrics. Because China's share of world exports for these products peaked late in the time period, the impact of the China trade shock on exposed industries and regions would have occurred late, as well.

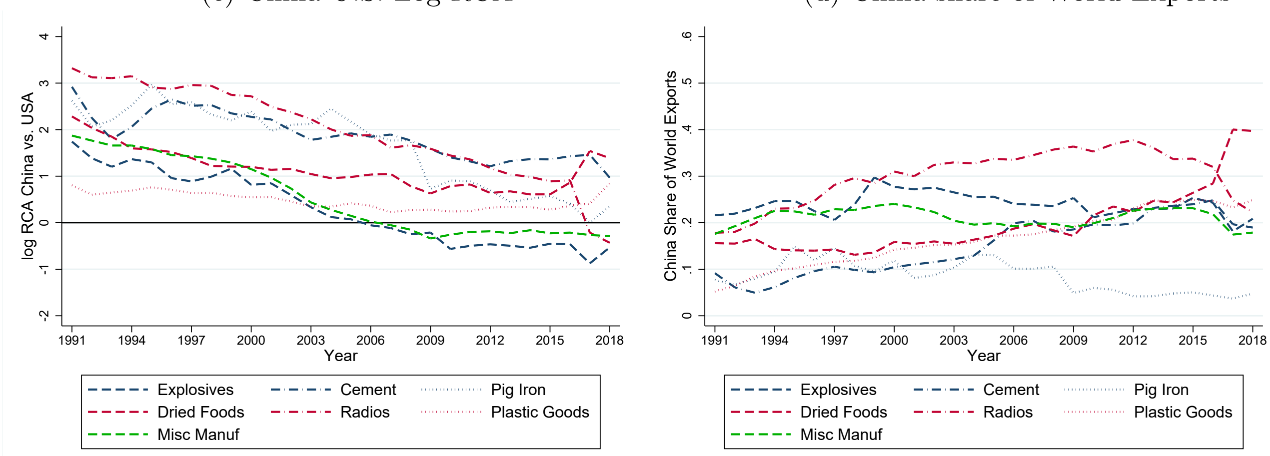
Figure A1: China vs. U.S. Revealed Comparative Advantage
(products where China's RCA peaks before 1997)

(a) China-U.S. Log RCA (b) China share of World Exports



Note: SITC Rev. 2 three-digit categories 612 (leather goods), 651 (yarn), 654 (woven fabrics), 658 (bags, tarps), 659 (carpets), 831 (handbags), 843 (women's and girl's outerwear), 844 (undergarments), and 845 (knitwear).

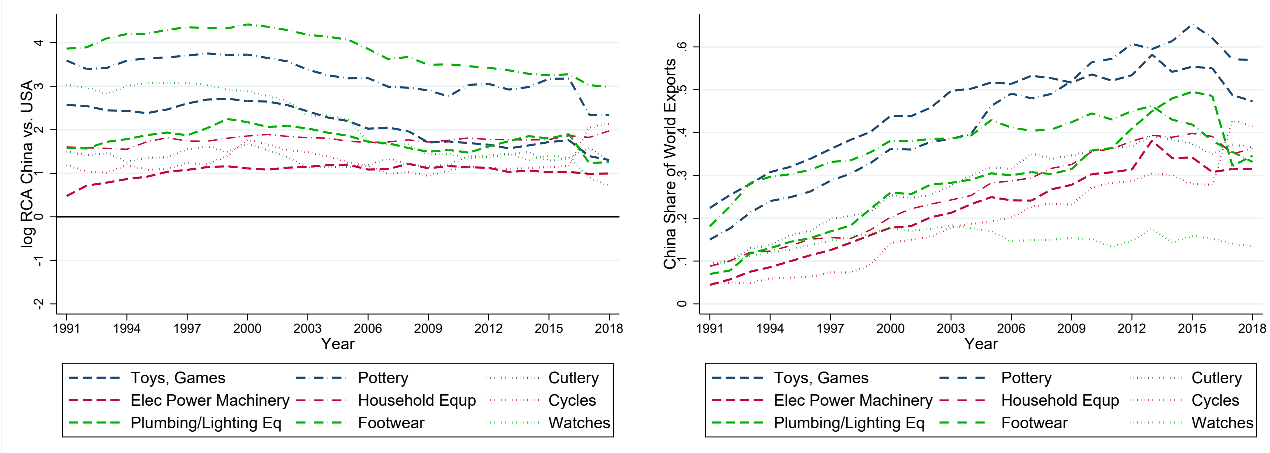
(c) China-U.S. Log RCA (d) China share of World Exports



Note: SITC Rev. 2 three-digit categories 056 (dried foods), 572 (explosives and fireworks), 661 (cement, lime), 671 (pig iron), 762 (radios), 893 (assorted plastic goods), 899 (miscellaneous manufactured goods). Revealed Comparative Advantage (RCA) is a country's share of world exports in a sector relative to a country's share of world exports of all merchandise.

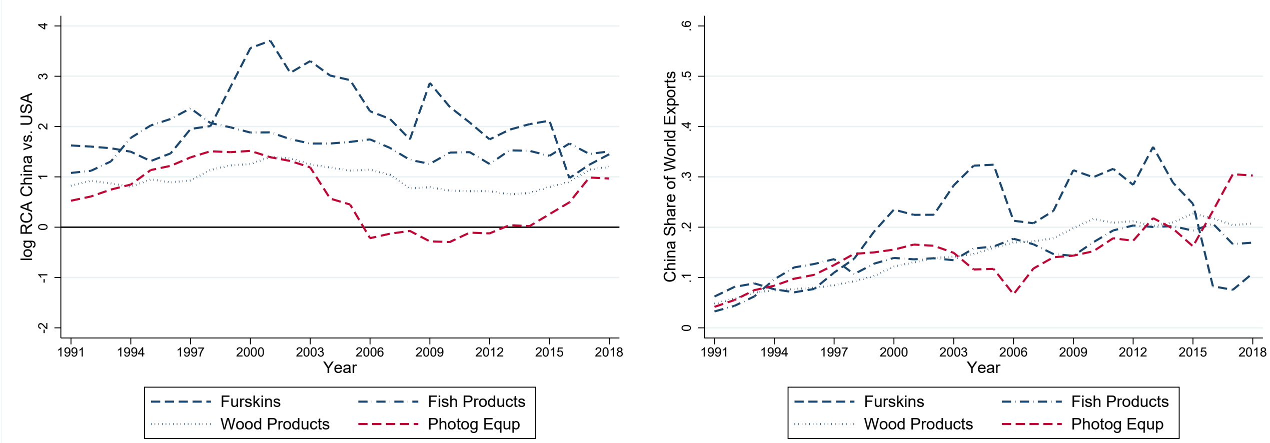
Figure A2: China-U.S. Revealed Comparative Advantage
(products where China's RCA peaks during 1997-2002)

(a) China-U.S. Log RCA (b) China share of World Exports



Note: SITC Rev. 2 three-digit categories 666 (pottery), 696 (cutlery), 771 (electrical power machinery), 775 (household type equipment, NES), 785 (bicycles, motorcycles, scooters), 812 (sanitary, plumbing, heating, lighting fixtures), 851 (footwear), 885 (watches and clocks), and 894 (toys, games, sporting equipment, baby carriages).

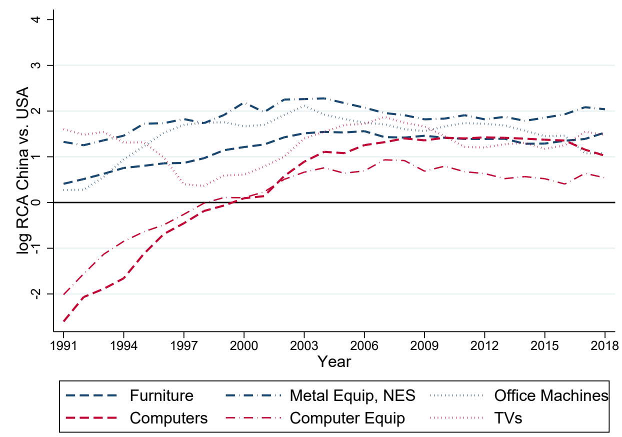
(c) China-U.S. Log RCA (d) China share of World Exports



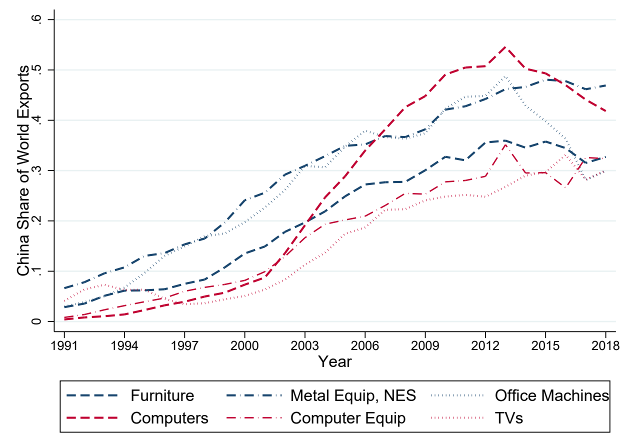
Note: SITC Rev. 2 three-digit categories 037 (prepared or preserved fish, crustaceans, and mollusks), 613 (tanned or dressed furskins), 635 (wood products, NES), and 881 (photographic equipment). Revealed Comparative Advantage (RCA) is a country's share of world exports in a sector relative to a country's share of world exports of all merchandise.

Figure A3: China vs. U.S. Revealed Comparative Advantage
(products where China's RCA peaks during 2002-2007)

(a) China-U.S. Log RCA

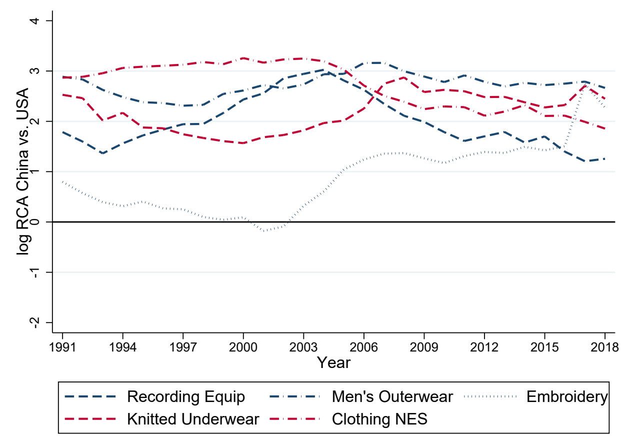


(b) China share of World Exports

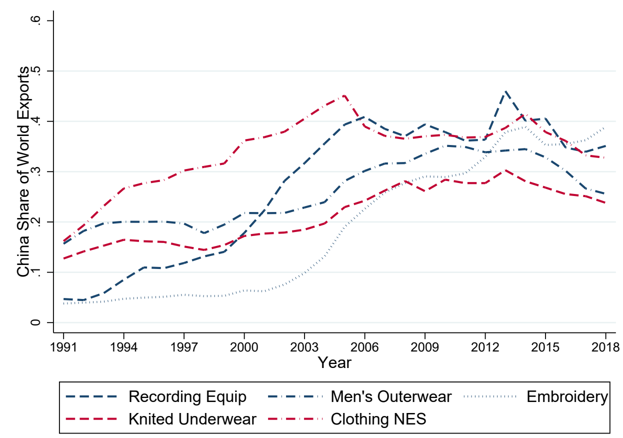


Note: SITC Rev. 2 three-digit categories 697 (metal equipment, NES), 751 (office machines), 752 (computers, laptops), 759 (computer parts and peripheral devices), 761 (televisions), and 821 (furniture).

(c) China-U.S. Log RCA

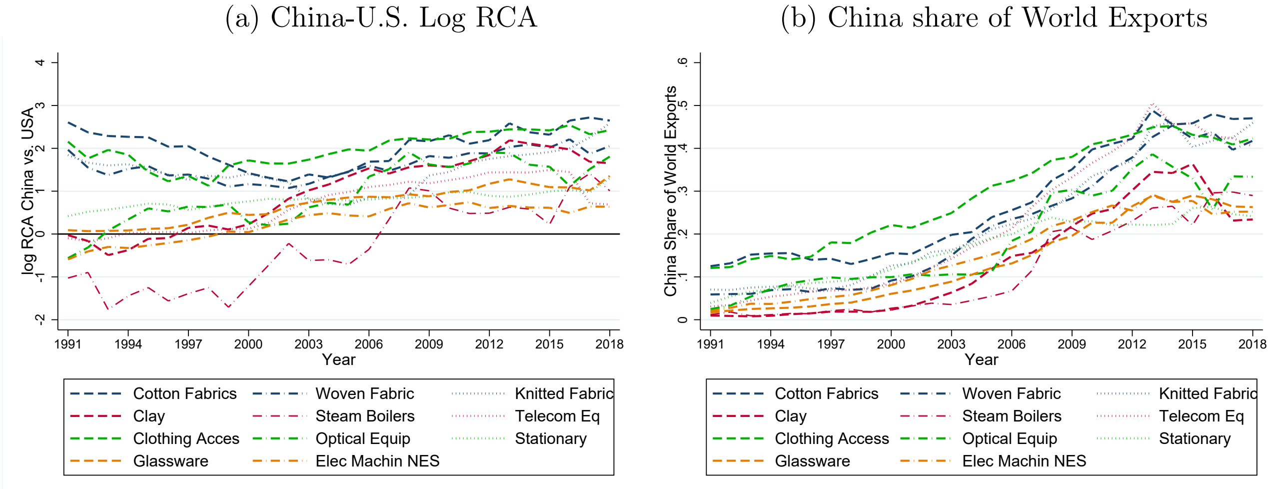


(d) China share of World Exports



Note: SITC Rev. 2 three-digit categories 656 (embroidery, lace, ribbons), 763 (sound recording devices), 842 (men's and boy's outerwear), 846 (knitted undergarments), 848 (articles of clothing, NES). Revealed Comparative Advantage (RCA) is a country's share of world exports in a sector relative to a country's share of world exports of all merchandise.

Figure A4: China vs. U.S. Revealed Comparative Advantage
(products where China's RCA peaks after 2007)



Note: SITC Rev. 2 three-digit categories 652 (cotton fabrics), 653 (woven fabrics, man-made fibers), 655 (knitted fabrics), 662 (clay and refractory construction materials), 665 (glassware), 711 (steam boilers), 764 (telecommunications equipment), 778 (electrical machinery, NES), 847 (clothing accessories), 871 (optical instruments), and 895 (office and stationary supplies). Revealed Comparative Advantage (RCA) is a country's share of world exports in a sector relative to a country's share of world exports of all merchandise.

A.2 Employment and Earnings Data

A.2.1 REIS vs. QCEW, CBP, Census/ACS and NIPAs

In much of our analysis, we evaluate employment and earnings outcomes based on data from the Bureau of Economic Analysis (BEA) Regional Economic Information System (REIS). REIS data on employment at the county level are primarily based on the comprehensive quarterly tabulations of unemployment insurance contribution reports that the Bureau of Labor Statistics uses to construct the Quarterly Census of Employment and Wages (QCEW). The BEA also uses supplementary data sources to additionally account for employment in industries that are not or not fully covered by unemployment insurance, and thus achieves slightly more comprehensive coverage of employment than the QCEW. The REIS data is also more comprehensive than employment counts from the County Business Patterns (CBP), which is an annual extension of the Census Bureau's quinquennial economic censuses that covers the private non-farm sector (see https://www.bea.gov/help/faq/104).[60]

The REIS, QCEW and CBP data all report the number of wage and salary jobs in a CZ. The REIS-based employment-population ratios used here, and the CBP-based employment-population ratios studied in Acemoglu et al. (2016)), thus indicate the number of jobs in a CZ divided by CZ working-age population. These employment-population ratios correspond to the employment rate among the working-age population under the simplifying assumption that all jobs are held by working-age individuals who have at most one job; they provide a proxy for that employment rate otherwise.

Autor et al. (2013a) instead measure changes in employment rates based on data from the decennial population Census and the American Community Survey (ACS). These household survey-based data enumerate the total number of individuals who have a job rather than the total number of jobs. Since Census data are available only at decennial frequency while annual ACS samples are relatively small, the Census/ACS data are not well suited for the data analysis at annual frequency that we conduct here, although we show Census/ACS-based results for selected time periods in Figure (A10). For that analysis, the employment rate is based on civilian working-age individuals

[60] The REIS data allow us to distinguish between employment in the manufacturing and non-manufacturing sectors, but do not provide a detailed industry breakdown of employment at the county or CZ level. In order to construct CZ-level trade exposure according to equation (1) and (3), we draw on CBP data on employment by country and 4-digit SIC or 6-digit NAICS industry. Because the CBP suppresses data in county-industry cells with few establishments, we imputed values for these cases using the fixed-point algorithm of Autor et al., 2013a.

(who do not reside in group quarters) who have a job and are not self-employed.

BEA estimates of personal income in the REIS differ slightly from personal income in the National Income and Product Accounts (NIPA). Whereas the NIPA includes some income earned abroad by U.S. residents, the REIS excludes such income. Similarly, where the NIPA excludes income earned by foreign nationals residing in the U.S. for less than a year, the REIS includes these earnings. REIS personal income is a more expansive measure than adjusted gross income reports (AGI) reported by the Internal Revenue Service. Unlike AGI, REIS personal income includes the income of non-profit institutions serving individuals, private non-insured welfare funds, and private trust funds; all government transfer receipts; and imputed income from in-kind current transfer receipts (such as Medicaid and Medicare) and employer contributions to health and retirement programs. (See https://apps.bea.gov/regional/definitions/.)

A.2.2 SIC vs. NAICS Industries in Employment and Trade Data

As in previous work (Autor et al., 2013a), we use SIC industries to define the aggregate trade shock facing CZs in equation (1). By construction, only SIC manufacturing industries are exposed to competition though imports of Chinese manufactures. Our outcome measure for aggregate manufacturing employment based on REIS data is instead based on NAICS industries. Because the transition from the SIC to the NAICS moved just five of 459 SIC industries from manufacturing to non-manufacturing, this difference is unlikely to matter materially in the estimation. The affected industries are SIC 2411 (logging), SIC 2711 (newspapers), SIC 2721 (periodicals), SIC 2731 (book publishing), and SIC 2741 (miscellaneous publishing). None of these are ones in which import competition from China grew substantially.

A.3 Summary Statistics on Outcomes and Controls

Table A1: Summary Statistics for CZ Outcome Variables

Outcome variable	Mean	Standard deviation	25th percentile	50th percentile	75th percentile
2001 to 2019 change in:					
Manufacturing employment/population 18-64	-2.68	1.76	-3.79	-2.66	-1.62
Non-manufacturing employment/population 18-64	2.87	4.17	0.37	2.86	5.49
Wage and salary employment/population 18-64	0.19	4.29	-2.81	0.27	1.79
2000 to 2019 change in:					
Wage and salary employment/population 18-64	-1.00	4.67	-4.21	-1.01	1.27
Unemployed persons/population 18-64	-0.26	0.57	-0.67	-0.27	0.13
Log population 18-64	12.92	13.98	3.45	10.24	21.33
Log population 18-24	9.05	14.08	0.04	6.28	18.98
Log population 25-39	6.41	15.48	-4.84	4.91	16.07
Log population 40-64	18.73	15.25	9.79	16.10	26.85
Log personal income/total population	26.95	8.07	21.31	26.09	33.49
Log personal income - government transfers/total population	20.55	10.22	14.47	19.81	27.55
Log government transfers/total population	61.85	8.70	56.70	62.67	67.59
Log wages, salaries, benefits/total population	17.38	10.40	10.42	17.02	24.09
Log dividends, interest, rent/total population	28.81	15.88	17.48	25.51	40.82
Log proprietor income/total population	25.85	24.67	13.41	24.34	37.51
Government transfers/total population	4,071.74	760.65	3,530.81	4,039.76	4,451.07
Social Security benefits/total population	1,112.62	309.34	886.72	1,056.72	1,310.32
Medicare benefits/total population	1,259.07	276.95	1,086.54	1,204.63	1,425.66
Medicaid benefits/total population	903.43	450.41	534.54	883.83	1,222.22
Income assistance/total population	266.73	102.74	210.35	258.72	332.41
SNAP benefits/total population	21.48	29.95	2.52	24.76	42.08
Earned Income Tax Credits/total population	54.50	24.21	35.55	56.10	65.68
SSI benefits/total population	21.48	29.95	2.52	24.76	42.08
Other income assistance/total population	100.09	72.07	54.52	90.08	117.38
Education, training assistance/total population	98.51	47.93	61.97	91.27	133.06
TAA-related unemployment compensation/total population	-1.23	4.12	-1.91	-0.90	0.00

Notes: This table shows changes in outcome variables (x 100 for shares and logs) over the indicated time period across the 722 commuting zones in the continental US. Values are weighted by the CZ working-age or total population in 2000.

Table A2: Summary Statistics for CZ Control Variables

Control variable	Mean	Standard deviation	25th percentile	50th percentile	75th percentile
Manufacturing employment/total employment	16.18	7.46	11.28	15.27	19.62
Female employment/total employment	64.38	5.51	60.46	64.69	68.11
Routine occupation employment/total employment	31.90	2.37	30.54	32.22	33.81
Offshorability index	0.00	0.51	-0.37	0.13	0.35
College educated/total population	53.61	7.47	50.34	53.91	57.97
Foreign born/total population	14.76	12.81	4.81	9.19	22.75
Non-white population/total population	18.18	10.95	9.41	17.66	24.98
Population 65+/total population	12.37	2.91	10.62	12.04	13.80
Population 40-64/total population	30.10	1.87	29.15	30.32	31.33
Population 0-17/total population	25.63	2.22	24.52	25.33	26.80
Δ log population 1970-1990/2	12.31	12.25	2.17	10.22	19.23

Notes: This table shows control variables (x 100 for shares and logs) for the indicated time period across the 722 commuting zones in the continental US. Values are weighted by the CZ working-age or total population in 2000.

Table A3: Summary Statistics for Outcome Variables Based on the Census and ACS

Outcome variable	Mean	Standard deviation	25th percentile	50th percentile	75th percentile
2000 to 2019 change in:					
Total employment/population 18-64	1.55	2.14	0.09	1.37	2.95
Manufacturing employment/population 18-64	-2.92	1.58	-3.42	-2.84	-2.00
Non-manufacturing employment/population 18-64	4.46	2.03	3.15	4.20	5.65

Notes: This table shows changes in employment-population ratios (x 100) over 2000 to 2019 calculate using the 2000 Census and the 2015-2019 ACS. Values are weighted by the CZ working-age or total population in 2000.

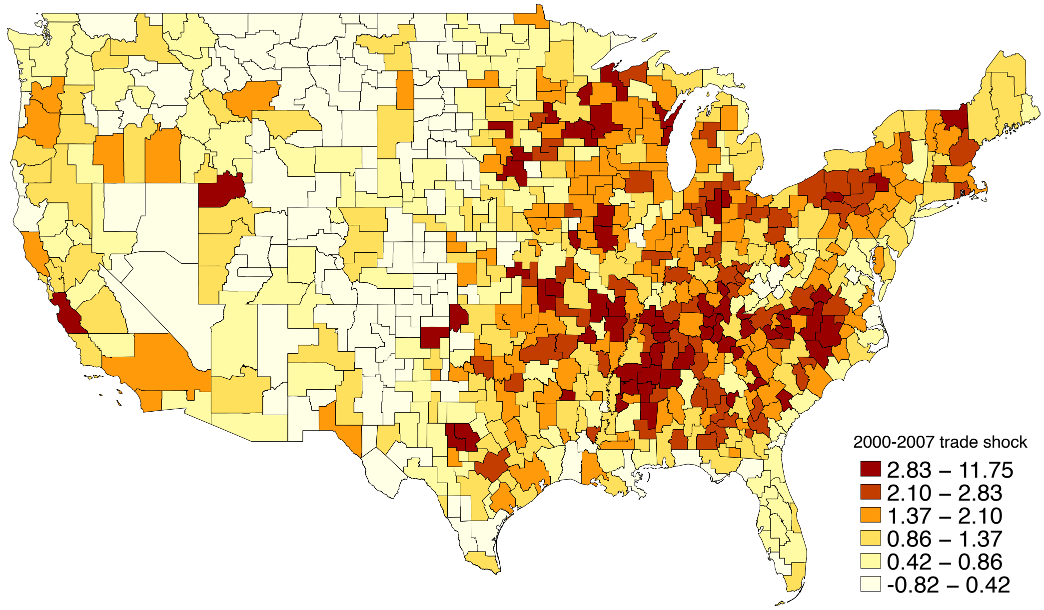
Table A4: Initial Conditions and Trade Shocks in the Most Trade Impacted CZs

	Values in 2000			Trade Shock	
Commuting Zone	Population (000s)	Manuf. share of employment (%)	BA degree share of pop. 18-64 (%)	Change in import penetration (ppt), 2000-2012	Impact on log personal income per capita, 2000-2019
Sioux City, IA-NE-SD	187.6	27.0	18.8	6.10	-7.89
Union County, MS	54.4	50.1	15.2	5.41	-6.84
Meridian, MS	156.9	26.5	13.3	5.09	-6.37
Hutchinson, MN	73.0	41.5	16.2	4.43	-5.36
North Hickory, NC	377.5	43.0	15.6	4.40	-5.32
Tupelo, MS	198.1	43.7	14.4	4.18	-4.99
Martinsville, VA	19.4	47.4	11.6	3.94	-4.62
Carroll County, VA	27.5	45.1	10.4	3.80	-4.40
Lynchburg, VA	112.4	26.9	18.5	3.74	-4.32
West Hickory, NC	165.1	49.9	12.9	3.70	-4.25
Henderson County, TN	44.9	45.9	9.7	3.58	-4.07
Crossville, TN	104.5	35.6	11.5	3.45	-3.88
Raleigh-Cary, NC	1420.0	17.0	34.2	3.42	-3.84
Cleveland, TN	203.7	39.9	12.4	3.20	-3.50
McMinnville, TN	84.5	48.9	10.4	3.19	-3.48
Faribault-Northfield, MN	110.1	32.9	20.2	3.16	-3.43
St. Marys, PA	41.0	54.7	13.2	3.13	-3.40
Danville, KY	86.7	38.3	16.6	3.01	-3.21
Quincy, IL-MO	152.3	23.8	16.1	2.97	-3.15
Greene County, GA	35.5	41.1	13.4	2.84	-2.96
Fort Wayne, IN	558.4	29.2	18.4	2.83	-2.94
Huntsville, AL	521.4	25.5	24.6	2.75	-2.82
Cherokee County, NC	59.9	30.8	14.9	2.71	-2.76
Fairmont, MN	48.9	28.0	17.2	2.69	-2.73
San Jose-Sunnyvale, CA	2397.6	20.8	34.9	2.67	-2.69
Starkville, MS	105.3	36.9	17.5	2.67	-2.69
Cleburne County, AR	51.8	30.2	11.8	2.58	-2.55
Brownwood, TX	58.2	24.4	16.3	2.57	-2.55
Union City, TN-KY	117.4	38.6	13.6	2.57	-2.55
Vernon County, MO	54.6	29.5	11.6	2.55	-2.51
Middlesborough, KY	66.7	29.4	8.2	2.52	-2.46
Runnels County, TX	23.6	33.0	13.4	2.50	-2.43
Austin-Round Rock, TX	1313.4	15.6	33.0	2.48	-2.41
Corinth, MS	129.7	37.4	9.9	2.45	-2.36
North Wilkesboro, NC	90.3	38.9	12.8	2.42	-2.32
Jonesboro, AR	199.1	34.4	14.6	2.37	-2.24
Toccoa, GA	89.3	41.3	11.6	2.36	-2.23
Richmond, KY	116.7	28.7	13.3	2.36	-2.23

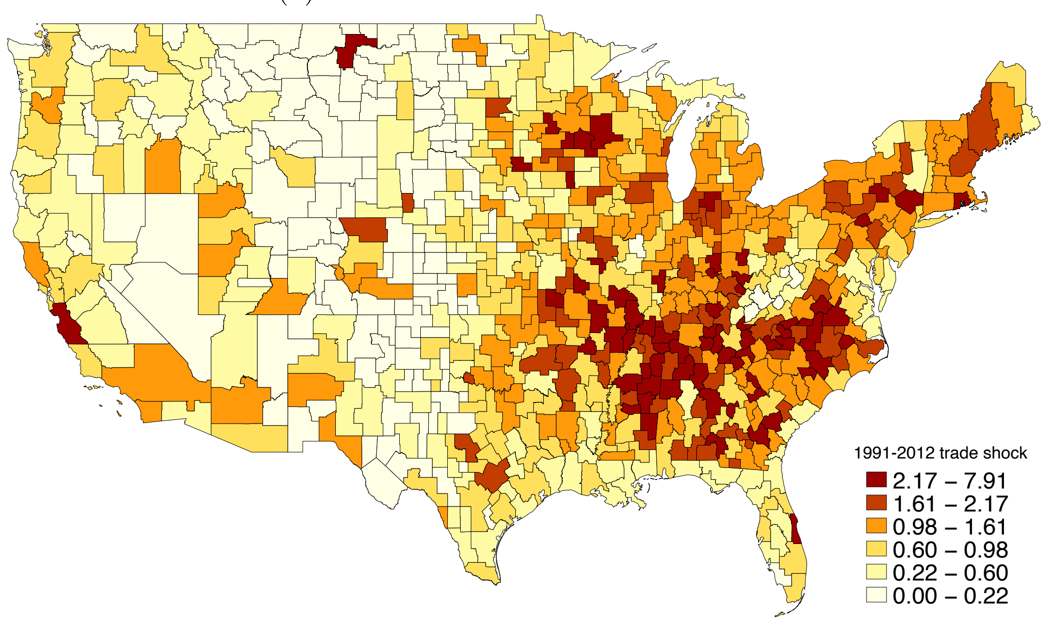
Notes: This table summarizes initial conditions in 2000 (total population, share of employment in manufacturing, share of working-age population with a BA degree or higher) and the China trade shock (decadalized change in import penetration over 2000 to 2012, implied impact on log personal income per capita over 2000 to 2019) in commuting zones above the 95th percentile of the change in import penetration over 2000 to 2012.

Figure A5: Change in Import Penetration for U.S. Commuting Zones

(a) China trade shock for 2000 to 2007



(b) China trade shock for 1991 to 2012



Note: These figures show the change in import penetration from China in (1) over 2000 to 2007, in panel (a), and 1991 to 2012, in panel (b), for CZs in the continental United States. The legend indicates values for the bottom four quintiles and the top two deciles.

A.4 Causal Identification and Inference

In this section, we discuss recent literature on identification and inference when using shift-share instruments, evaluate evidence of pre-trends in the data, and present results using alternative methods for constructing standard errors in the estimation of (2), when using (3) to instrument for (1).

In their evaluation of shift-share IV estimators, Borusyak et al. (2020) derive sufficient conditions for causal identification in empirical setups such as those in Autor et al. (2013a), Acemoglu et al. (2016), and related contexts. Applying their framework here, for the instrument, $\Delta IP^{co}_{i\tau}$, to be orthogonal to the residual, ε_{it+h}, in (3), the following must hold: $\mathbb{E}\left[\sum_j s_j \Delta IP^{co}_{j\tau}\bar{\varepsilon}_j\right] = 0$, where s_j is the national employment share of industry j and $\bar{\varepsilon}_j \equiv \sum_i s_{ijt-10}\varepsilon_{it+h}/\sum_i s_{ijt-10}$ is the exposure-weighted average of unobserved shocks for industry j. This orthogonality condition is satisfied if either the large-sample covariance between the industry-level instrument $\Delta IP^{co}_{i\tau}$ and unobserved shocks $\bar{\varepsilon}_j$ is zero, or if the employment shares s_{ijt-10} are exogenous and uncorrelated with these shocks. Because of the shift-share structure—shocks originate at the industry level and are transmitted to the region level via CZ industry employment shares—orthogonality is defined for the sample of industries, rather than for the sample of regions.

As detailed in Borusyak et al. (2020), identification in shift-share analyses, such as in equation (2), requires exogenous industry shocks (AKA, shifts)—identified by 2SLS in Autor et al. (2014); Autor et al. (2020b)—while industry shares are taken as given. Conversely, Goldsmith-Pinkham et al. (2020) study an alternative setting where industry shifts are taken as given while industry employment shares are assumed to be exogenous. Borusyak et al. (2020) show that the orthogonality condition is satisfied under the assumptions that industry shocks are as-good-as-randomly assigned conditional on industry-level unobservables and industry weights, ($\mathbb{E}\left[\Delta IP^{co}_{i\tau}|\bar{e}_j, s_j\right] = \mu$ for all j), where μ is a constant, and that there are many industry shocks ($\mathbb{E}[\sum_j s_j^2] \to 0$) which themselves are uncorrelated given unobservables and industry weights ($Cov\left[\Delta IP^{co}_{j\tau}, \Delta IP^{co}_{k\tau}|\bar{\varepsilon}_j, \bar{\varepsilon}_k, s_j, s_k\right] = 0$ for all industries j and $k \neq j$). In regressions with covariates, shock expectations can depend on the observables and must be as-good-as-random conditional on controls.

Our discussion of the substantial industry-level variation in the timing and intensity of the China trade shock highlighted in section 2 suggests that our approach is more consistent with assuming shift exogeneity than share exogeneity. To check for industry-level orthogonality, Borusyak et al.

(2020) recommend regressing current shocks on past outcomes, which are likely correlated with current residuals. Autor et al. (2013a) and Acemoglu et al. (2016) perform such validation exercises for CZs and industries and fail to reject industry-level orthogonality. Validating these earlier results, Borusyak et al. (2018) fail to reject the null of industry-level orthogonality in the Autor et al. (2013a) estimation for 10 of 12 falsification tests at conventional significance levels, which they interpret as evidence consistent with the analysis succeeding in leveraging exogenous variation in the estimation.

In Appendix Figure A6, we report falsification tests in which we regress the changes in outcomes whose end year is 1991, just prior to the onset of the China trade shock, and whose initial year ranges from 1975 to 1990 on the trade shock in (1), defined over the period 1991 to 2000. (Results are very similar when we use the 1991 to 2012 trade shock, instead.) This allows us to examine whether future trade shocks are correlated with pre-China shock changes in labor-market conditions. Control variables include Census region dummies and the CZ share of employment in manufacturing in 1970. For the manufacturing employment-working-age population ratio in Figure A6a, there is near zero correlation with the trade shock for changes in outcomes over the 1985 to 1991 horizon. As we extend the outcome period further back in time, a positive correlation emerges between the future trade shock and past changes in manufacturing employment, similar to that reported in Autor et al. (2013a). This indicates that CZs subject to larger increases in import competition after 1991 had *faster* manufacturing employment growth in preceding decades. There is no evidence of negative pre-trends in manufacturing employment growth in more trade-exposed CZs. A similar pattern emerges when we examine the overall employment-to-population ratio in Figure A6c, and personal income per capita in Figure A6e. For non-manufacturing employment in Figure A6b, the log change in the working-age population in Figure A6d, and government transfers per capita in Figure A6f, there is near zero correlation between the future trade shock and past changes at all time horizons. (The regression for the log change in population head counts includes lagged CZ population growth as a control.) We interpret these results as evidence against the existence of negative pre-trends in labor-market conditions for commuting zones subject to the China trade shock.

Borusyak et al. (2018) show that the impact coefficient in a regional shift-share regression is identified by regressing the industry level outcome on the industry level shift and using weights that are a function of regional industry employment shares. Related work by Adao et al. (2019b) evaluates confidence intervals for shift-share estimators where the residual has a shift-share structure (e.g., where unobserved industry shocks are transmitted to CZs via industry employment weights). Their corrected shift-share IV standard errors, when applied to Autor et al. (2013a), widen confidence intervals asymmetrically to include substantially more-negative impacts of trade shocks on manufacturing employment (with no change in statistical significance). The coefficient estimate for the trade shock impact on US manufacturing employment in Autor et al. (2013a) (Table 3, column 6) is -0.60. Autor et al. (2013a) compute a 95% confidence interval based on standard errors clustered at the state level whose lower bound is -0.79, while Adao et al. (2019b) and Borusyak et al. (2020)'s modified version of Adao et al. (2019b) yield much lower bounds of -1.01 and -1.06. The upper bound of the confidence interval instead has similar values of -0.40 in Autor et al. (2013a), -0.36 in Adao et al. (2019b), and -0.40 in Borusyak et al. (2020).

In Appendix Figure A7, we replicate the specifications in Figure 5, using the Borusyak et al. (2020) procedure for constructing standard errors. To aid in comparing the estimates, we report the Figure 5 estimates side-by-side with Borusyak et al. (2020) based estimates. (Here, we use the CZ manufacturing employment share in 2000 as a control. Results are similar when using the CZ manufacturing employment share in 1990, as suggested by Borusyak et al. (2020).) By construction, the two methods yield identical coefficient estimates and only differ in how they calculate standard errors. For manufacturing employment (Figure A7a) and non-manufacturing employment (Figure A7b), standard errors are slightly larger when using the Borusyak et al. (2020) method; for wage and salary employment, standard errors are modestly small when applying Borusyak et al. (2020). In the results that follow, we continue to use standard errors clustered by state.

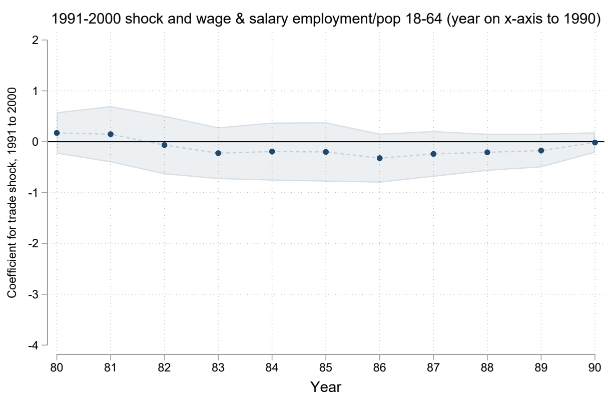
In finite samples, a question arises whether approaches based on asymptotic theory, such Borusyak et al. (2018) and Adao et al. (2019b), yield results that are more reliable than a simple cluster robust variance estimator. Ferman (2019) develops a simulation approach to assess this question, which he applies to Autor et al. (2013a) and other shift-share analyses, as well as other estimation frameworks. His results suggest that in the specific context of the China trade shock, there is little gain to applying these alternative methods for estimating standard errors. Stated differently, by clustering standard errors at the state level, our approach is consistent with Adao et al. (2019b), as long as common specialization patterns across CZs within states are the source of correlated errors. This assumption is more restrictive than that in Adao et al. (2019b). However, because our confidence intervals exclude the larger negative impacts spanned by the Adao et al. (2019b) confidence intervals, in this instance our clustering approach would appear to be more conservative in terms of ruling out very large negative impacts of the China shock.

Figure A6: Analysis of Pre-Trends for 1991-2000 Trade Shock

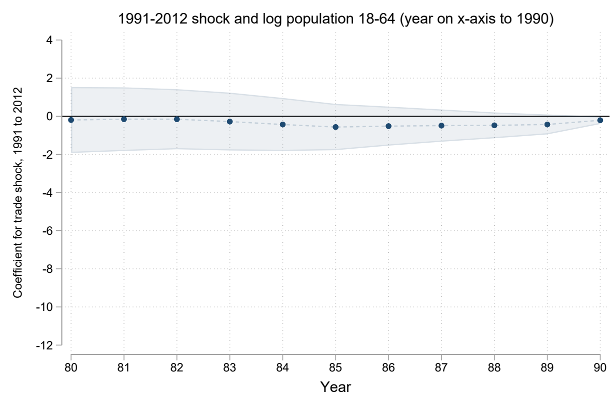
(a) Manuf. emp./Working-age pop.

(b) Non-manuf. emp./Working-age pop.

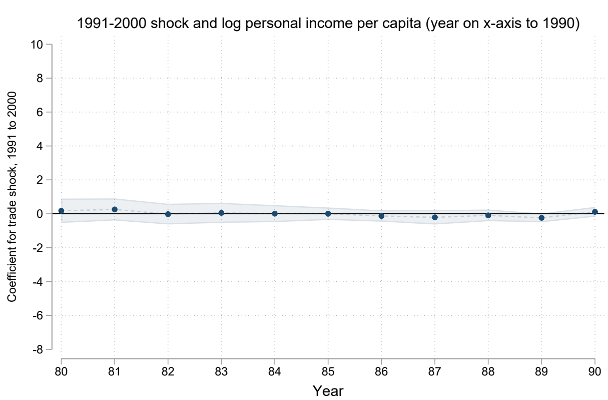
(c) Total wage and salary emp./Working-age pop.



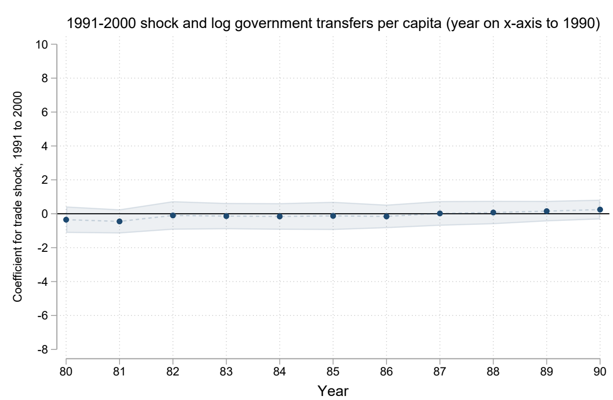
(d) Log working-age population



(e) Log personal income per capita



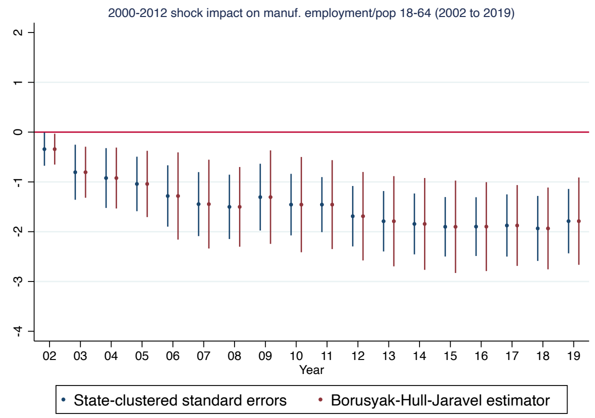
(f) Log government transfers per capita



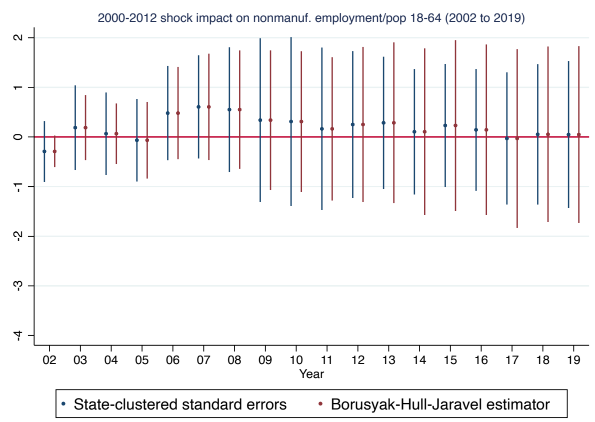
Note: Panels (a)-(f) report OLS regressions of the change in the indicated outcome between the year indicated on the horizontal axis and 1991 on the trade shock in (1) for the 1991-2000 period. Control variables include CZ Census region dummies, and the share of manufacturing in CZ employment in 1970 (except for panel (d) which also includes the log change in CZ population growth over 1970 to 1975 as a control). Regressions are weighted by the CZ share of the U.S. mainland population in 1991; standard errors are clustered by state.

Figure A7: Estimation Results Based on Borusyak, Hull, and Jaravel (2020)

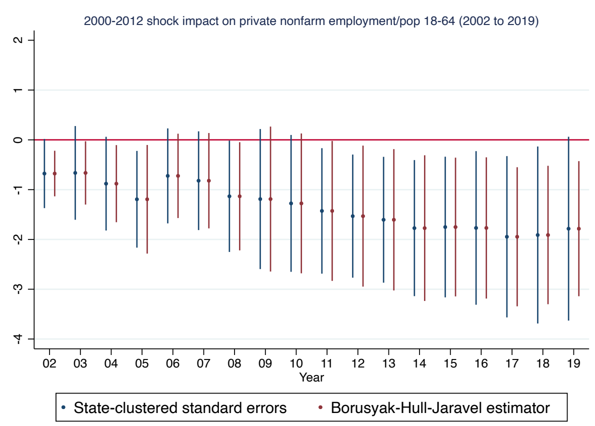
(a) Manufacturing employment/Working-age population



(b) Non-manufacturing employment/Working-age population



(c) Wage and salary employment/Working-age population



Note: Panels (a)-(c) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in the specified outcome between 2001 and the year indicated on the horizontal axis; the trade shock is the decadalized 2000-2012 change in CZ import exposure, as defined in (1) and instrumented by (3). Control variables include initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies. Regressions are weighted by the CZ working-age population in 2000. Estimates in blue are the same as those in Figure 5; estimates in red, calculate standard errors based on the method in Borusyak et al. (2020).

A.5 Supplementary Figures

A.5.1 Dynamic Adjustment to the China Trade Shock

Because the China trade shock began in the 1990s, one may view the specification in (2) as incomplete in that it does not control for the previous decade's trade shock—so, the results for 2001 through 2019 could in part reflect ongoing labor market adjustment from the prior decade. To allow for dynamic adjustment, we could in theory add the lagged trade shock to (2). Complicating this approach is the fact that the 1991-2000 and 2000-2012 shocks are highly correlated ($\rho = 0.57$). As noted in section 3.1, most of the China trade shock occurred after 2000: the average values of the (undecadalized) change in import penetration in (1) are 0.72 percentage points for 1991-2000 and 2.33 percentage points for 1991-2012, indicating that 69.5% of the shock occurs over the 2000 to 2012 period. Accordingly, we capture the bulk of the China trade shock by studying the post 2000 period.

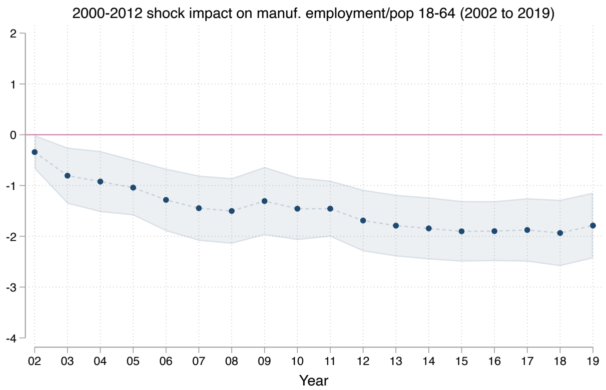
Appendix Figure A8 compares results for the change in manufacturing employment-population ratio over 2001 to 2019 when using our baseline 2000-2012 shock (Figure A8a), replacing this shock with that for 1991-2000 (Figure A8b), and including both shocks together (Figure A8c).[61] Because the trade shocks are highly correlated across decades, coefficient estimates in Figures A8a and A8b are very similar. Impact magnitudes are naturally larger for the 2000-2012 trade shock, which contains more information about shock impacts in the 2000s. The high correlation of the trade shocks means that when including both shocks together in the same regression (Figure A8c), coefficient estimates for the 2000-2012 trade shock become smaller and less precisely estimated.[62] We conclude that we cannot separately identify impacts of the 2000-2012 trade shock and continued adjustment to the 1991-2000 trade shock on outcomes in the 2000s. The estimated impact of the 2000-2012 trade shock is therefore a composite of these two effects.

[61] In Figure A8d, we include an orthogonalized version of the 1991-2000 trade shock (i.e., the residuals from a regression of the 1991-2000 shock on the 2000-2012 shock). The resulting coefficient estimates for the 2000-2012 shock are very close to those in Figure A8a (but not identical, due to covariance between the residual and the controls). Adding the residualized pre-2000 trade shock would thus have little impact on results for the post-2000 trade shock.

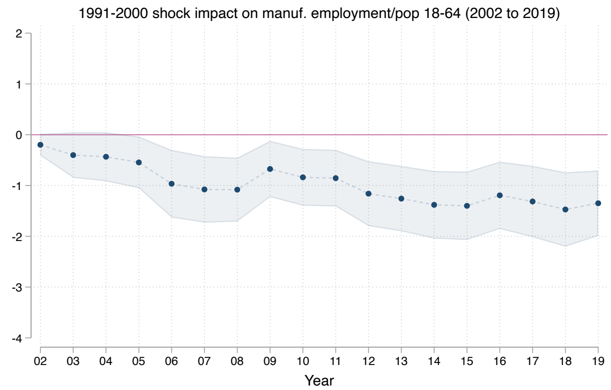
[62] In Figure A8c, unreported coefficients on the 1991-2000 trade shock are small and imprecisely estimated.

Figure A8: Combined Trade Shock Impacts on Manufacturing Employment

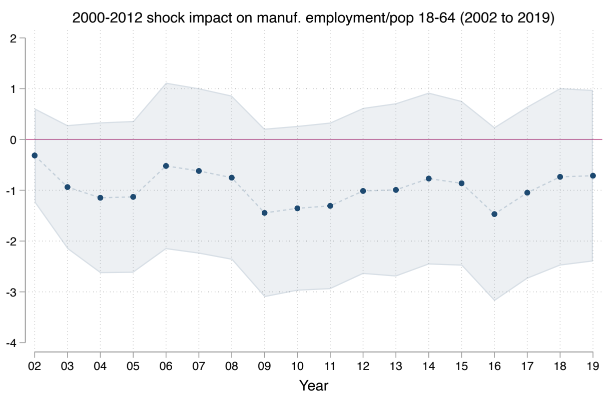
(a) 2000-2012 trade shock



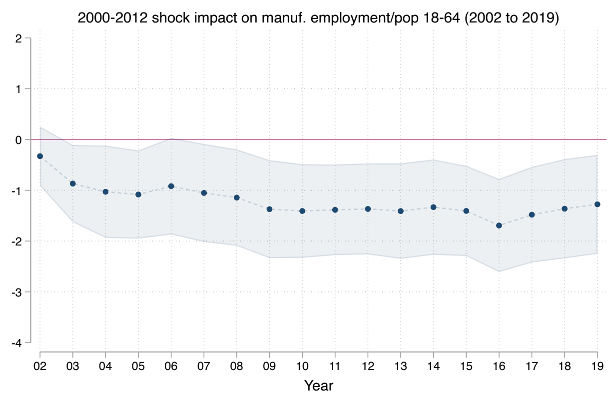
(b) 1991-2000 trade shock



(c) 2000-2012 shock (with 1991-2000 shock)



(d) 2000-2012 shock (with residual 1991-2000 shock)

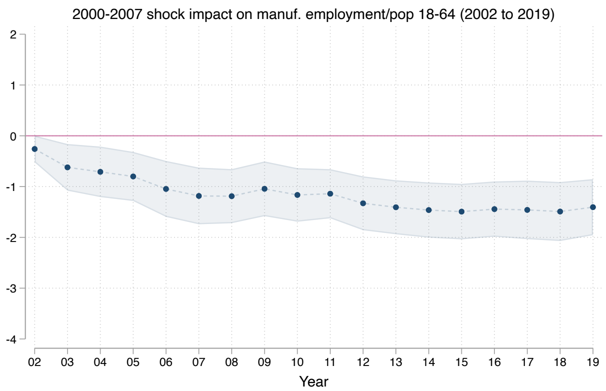


Note: Panels (a)-(d) report 2SLS estimates of (2). The dependent variable is the change in manufacturing employment as a share of the working-age population between 2000 and the year indicated on the horizontal axis. Panels (a) and (b) include the 2000-2012 and 1991-2012 trade shocks alone, respectively; panel (c) includes the two shocks together; panel (d) includes the 2000-2012 shock and the residualized 1991-2000 shock. The 1991-2000 instrument is used for the 1991-2000 trade shock, and the 2000-2012 instrument for the 2000-2012 shock. Control variables include initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies. Regressions are weighted by CZ working-age population in 2000; standard errors are clustered by state.

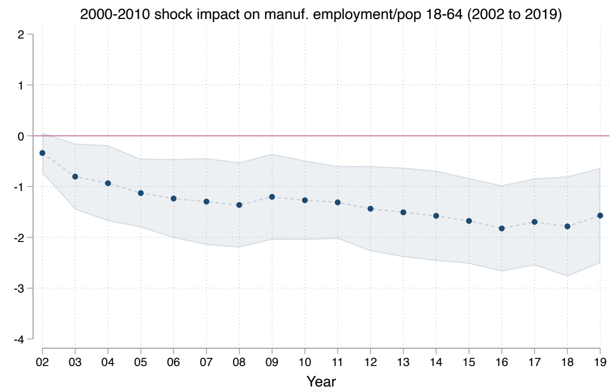
A.5.2 Employment, Unemployment and Population Headcounts

Figure A9: Trade Shock Impact on Manufacturing Employment, Varying Shock Periods

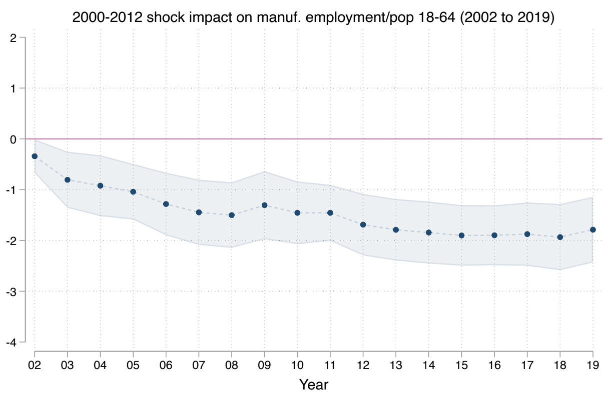
(a) Trade shock, 2000 to 2007



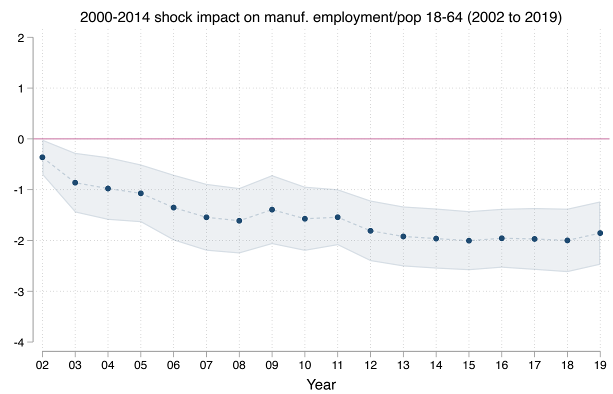
(b) Trade shock, 2000 to 2010



(c) Trade shock, 2000 to 2012



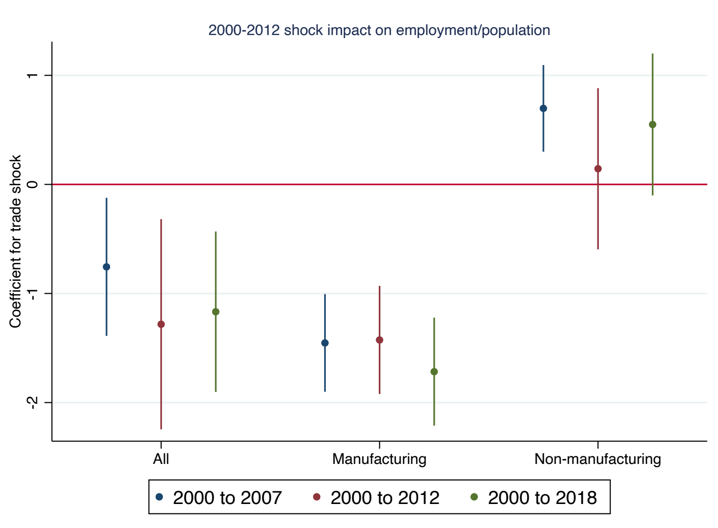
(d) Trade shock, 2000 to 2014



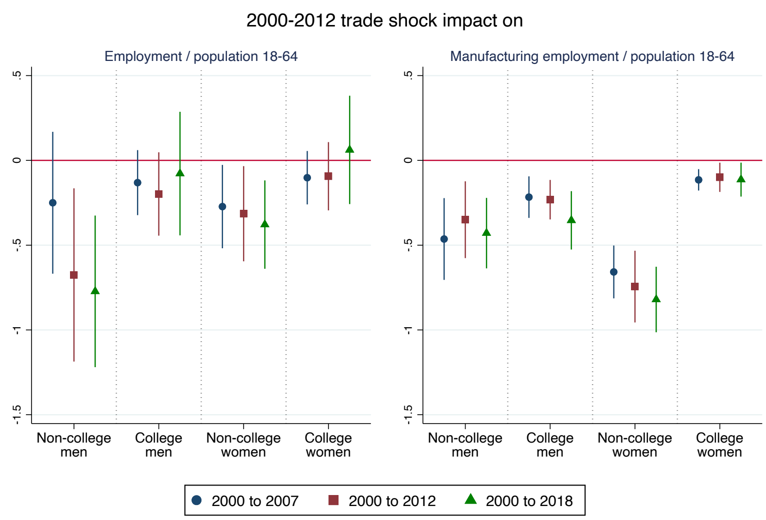
Note: Panels (a)-(d) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in manufacturing employment as a share of the working-age population between 2000 and the year indicated on the horizontal axis The trade shock is the decadalized change in CZ import exposure for the indicated time period, as defined in (1) and instrumented by (3). Control variables include initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies. Regressions are weighted by the CZ working-age population in 2000; standard errors are clustered by state.

Figure A10: Trade Shock Impact on Employment-Population Ratios, Census-ACS data for 2000-2010, 2000-2014, and 2000-2019

(a) Employment/Working-age population by sector



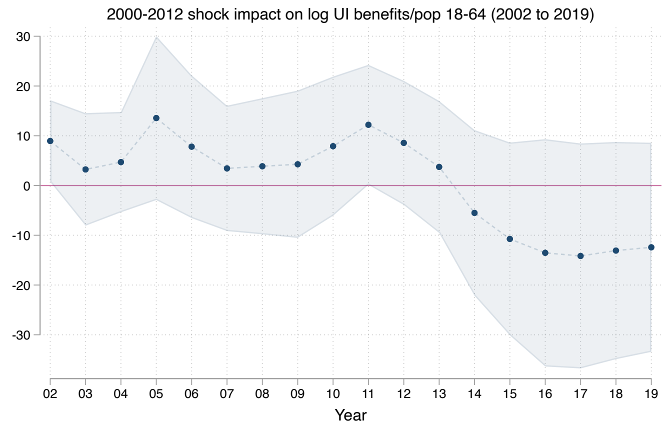
(b) Employment/Working-age population by sector and worker education



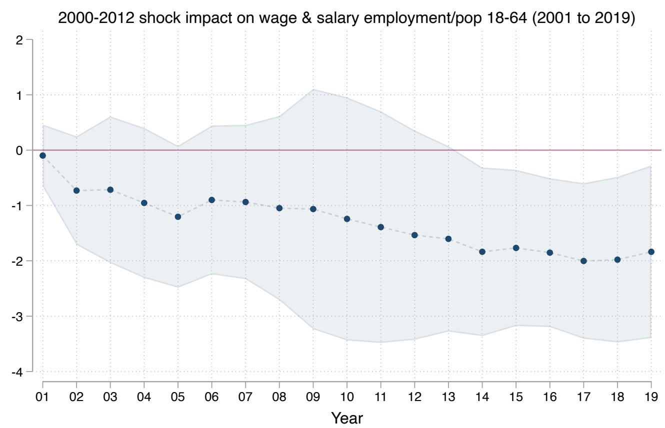
Note: The figures report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in the employment-population ratio for the indicated group (all workers, manufacturing workers, non-manufacturing workers), for workers of a given education level (all, bachelor's degree or high, no bachelor's degree), and over the time period indicated on the legend (data for 2000 are from the Census, for 2007 are from the combined 2006-2008 ACS samples, for 2012 are from the combined 2011-2013 ACS samples, and for 2018 are from the combined 2017-2019 ACS samples); the trade shock is the decadalized 2000-2012 change in CZ import exposure, as defined in (1) and instrumented by (3). Control variables include initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies. Regressions are weighted by the CZ working-age population in 2000; standard errors are clustered by state.

Figure A11: Trade Shock Impact on Unemployment, 2000-2019

(a) Log unemployment insurance compensation/Working-age population



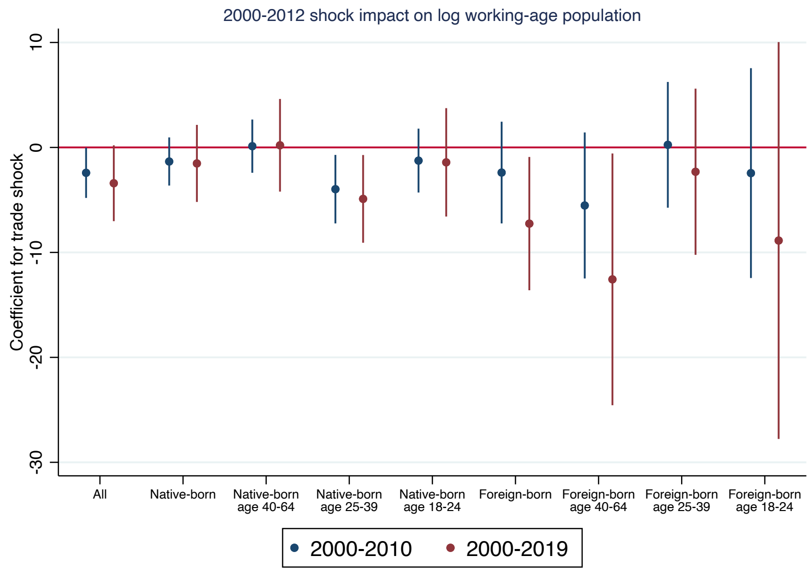
(b) Wage and salary employment/Working-age population



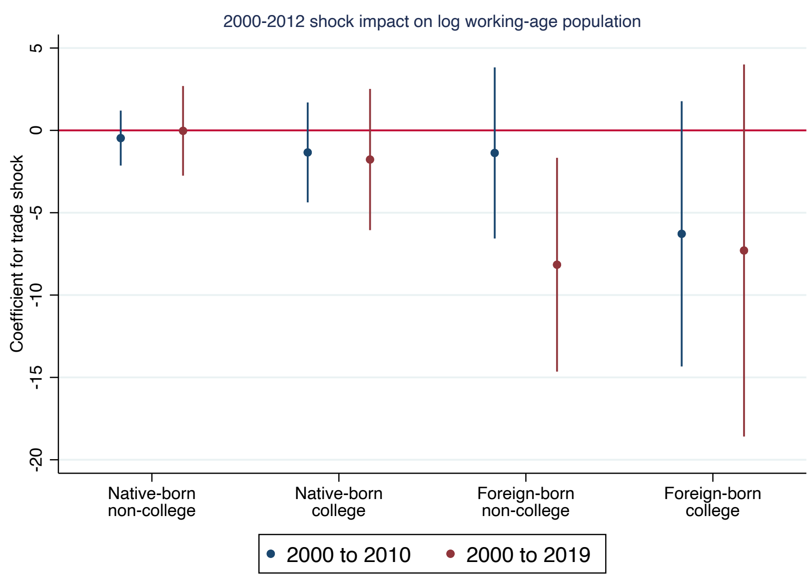
Note: Panels (a)-(b) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in the specified outcome between 2000 and the year indicated on the horizontal axis; the trade shock is the decadalized 2000-2012 change in CZ import exposure, as defined in (1) and instrumented by (3). Control variables include initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies. Regressions are weighted by the CZ working-age population in 2000; standard errors are clustered by state.

Figure A12: Trade Shock Impact on Population Headcounts, Census-ACS data for 2000-2010 and 2000-2019

(a) Log population by age cohort and nativity



(b) Log population by educational attainment and nativity



Note: The figures reports 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in the log working-age population of the indicated nativity and age group over the time period indicated on the legend (population for 2000 is from the Census, population for 2010 is from the 2006-2010 ACS five-year sample, and population for 2019 is from the 2015-2019 ACS five-year sample); the trade shock is the decadalized 2000-2012 change in CZ import exposure, as defined in (1) and instrumented by (3). Control variables include initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies, and the change in CZ log total population between 1970 and 1990. Regressions are weighted by the CZ population in 2000. Standard errors are clustered by state.

A.5.3 Gravity-Based Spillovers

Adao et al. (2019a) build a general equilibrium trade model that captures spillovers between regions and generates reduced-form equilibrium conditions that have a shift-share structure. If national industries are subject to exogenous shocks, then employment and wages in regional economies will be affected through two channels. One is through changes in local industry revenue, which in the case of greater import competition will place downward pressures on local wages and employment. This is captured by changes in import penetration in (2). Second, in the presence of cross-region spillovers, wages and employment in one region will also be affected by localized changes in import penetration in other regions. For a given CZ, shocks to other CZs will matter more the larger and the closer are these other markets, as dictated by the gravity structure of trade. Adao et al. (2019a) quantify this spillover by adding to the specification in (2) the gravity-weighted changes in import competition all other regions (i.e., the sum of the trade shock in each region weighted by the size of and the distance to that region).[63]

We incorporate their approach by estimating the following extended version of equation (2):

$$\Delta Y_{it+h} = \alpha_t + \beta_{1h}\Delta IP^{cu}_{i\tau} + \beta_{2h}\sum_k z_{ikt}\Delta IP^{cu}_{k\tau} + \mathbf{X}'_{it}\beta_2 + e_{it+h}. \tag{9}$$

where the added variable, $\sum_k z_{ik}\Delta IP^{cu}_{k\tau}$, is the sum of trade shocks in other commuting zones, weighted by the gravity-model-implied linkage between CZs, z_{ik}, where

$$z_{ikt} \equiv \frac{L_{kt}D_{ik}^{-\delta}}{\sum_h L_{ht}D_{ih}^{-\delta}}, \tag{10}$$

and L_{kt} is the initial-period population of CZ k, D_{ik} is the distance between CZs i and k, and δ is the trade-cost elasticity, which following Adao et al. (2019a) we set equal to 5.[64] Appendix Figure A13 reports the results.

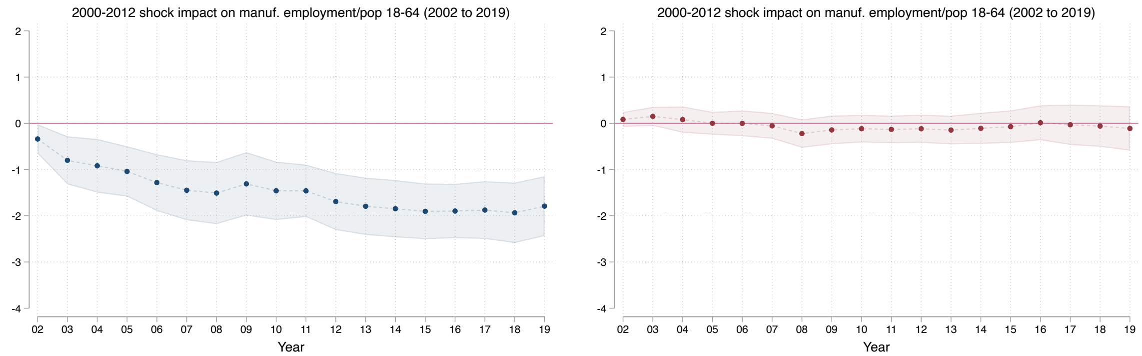
[63] A third channel through which national industry shocks affect local wages and employment is through changes in consumption costs. If greater import competition expands local consumption possibilities, real incomes and the demand for goods will increase. This will give rise to an own-region effect, in which local demand for goods expands, and a cross-region effect, coming from gravity-weighted changes in demand in other regions. To calculate the change in consumption possibilities, Adao et al. (2019a) modify (1) by adding the CZ industry consumption share (i.e., the share of consumption spending a CZ devotes to a good), which they construct based on CZ industry composition and national input-output tables. The consumption channel introduces two additional terms to (2), one for own-region consumption effects and a second for gravity-weighted consumption effects in other regions. Their estimated coefficients on these consumption terms are quantitatively small and imprecisely estimated. These null effects may indicate that consumption channel effects are common across regions and therefore absorbed into the constant term and does not necessarily imply that they are small in aggregate (see, e.g., Jaravel and Sager, 2019).

[64] We instrument for $\sum_k z_{ikt}\Delta IP^{cu}_{k\tau}$ by applying the gravity weights in (10) to the trade shock instrument in (3).

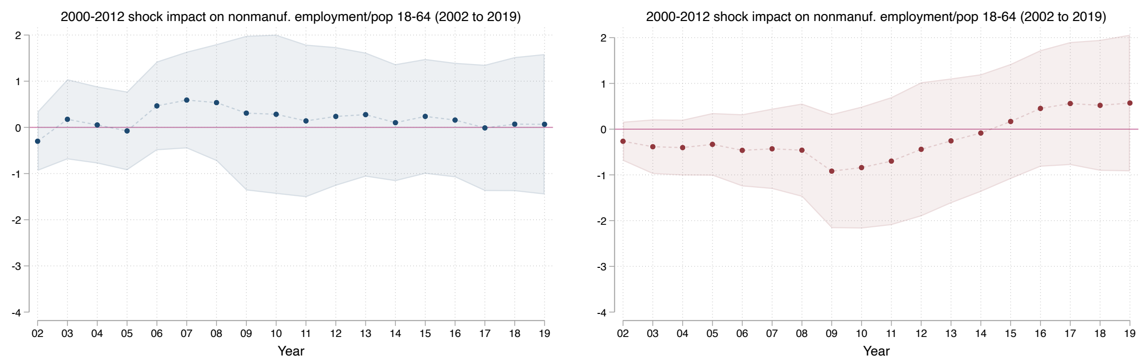
Figure A13: Impacts of Local vs. Gravity-Based Trade Shocks on Employment

I. Impact of Local Trade Shock | II. Impact of Gravity-Based Trade Shock

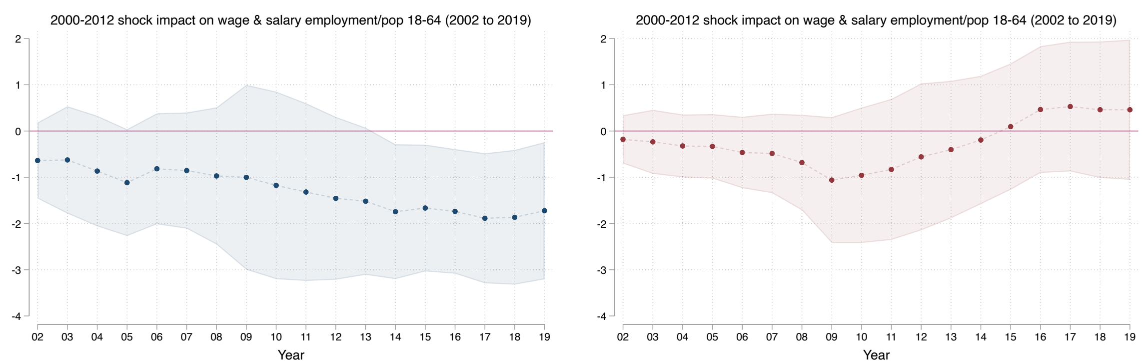
(a) Manufacturing employment/Working-age population



(b) Non-manufacturing employment/Working-age population



(c) Wage and salary employment/Working-age population



Note: Panels (a) and (b) report 2SLS coefficient estimates for β_{1h} and β_{2h} in (**??**) and 95% confidence intervals for these estimates. The dependent variable is the change in employment for the indicated measure between 2001 and the year on the horizontal axis; the trade shock is the decadalized 2000-2012 change in CZ import exposure, for the CZ itself, coefficients on which appear in the first column, and for a gravity-based version of import exposure in other CZs, coefficients on which appear in the second column); and control variables include initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies. Regressions are weighted by the CZ working-age population in 2000; standard errors are clustered by state.

A.5.4 Personal Income and Government Transfers

In Figure A15, we explore the impact of trade shocks on government transfers by program type. To easily compare impact magnitudes across programs, we express them in terms of dollars per capita (rather than in logs). Two results stand out. First, consistent with results in Autor et al. (2013a) for earlier time periods, adjustments in Social Security and Medicare benefits, shown in Figure A15b, account for most of the responsiveness in government transfers induced by greater import competition, where the magnitude of these benefit gains expands as the time horizon lengthens. For the 2000 to 2019 time difference, the impact coefficient for Social Security and Medicare benefits per capita of \$201 (t-value = 2.46) is 78.2% (= 191/257) of that for total transfers per capita of \$257 (t-value = 2.07), shown in Figure A15a. Social Security payments include retirement benefits, from the Social Security Administration pension system, and disability benefits, from Social Security Disability Insurance. To receive these benefits, an individual must have left the labor force, either through retirement or by being declared medically unable to hold a job. The primary means through which government transfers replace labor income lost due to import competition is thus by accommodating an exit from paid work. The fact that the preponderance of transfer benefit payments accrue to non-workers may help account for the long-run negative effects of trade exposure on employment-population ratios see in Figure 5.

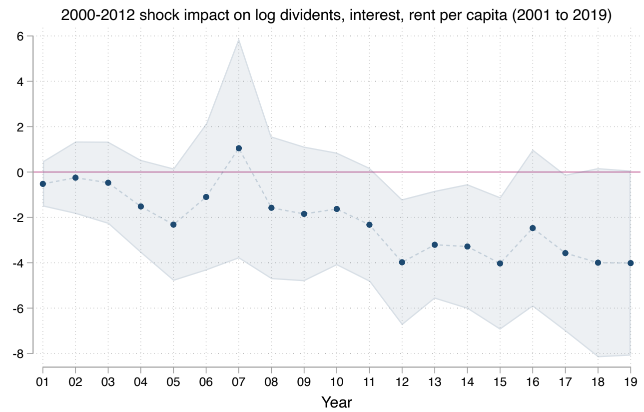
A second notable finding is that, despite trade-induced lower incomes, means-tested government programs meant to provide income assistance to poor households are largely unresponsive to greater import competition. We see this in the null or negative and insignificant responses of Medicaid and government income assistance to trade exposure in Figures A15c and A15d. In Appendix Figure A16, we explore the non-response of specific income assistance programs, including Supplemental Security Income, Earned Income Tax Credits, and Supplemental Nutritional Assistance (food stamps), and non-state administered unemployment compensation, which includes Trade Adjustment Assistance.[65] Programs designed to encourage participation in work are largely unresponsive to trade shocks: trade exposure induces negligible changes in EITC payments and TAA-related unemployment compensation. The lack of a positive impact of trade shocks on EITC payments may be due to the fact that the EITC functions as a wage subsidy for those employed, whereas the primary

[65] In the REIS, the BEA reports Trade Adjustment Assistance (TAA) payments in a category that also includes unemployment compensation for federal employees, railroad employees, and veterans.

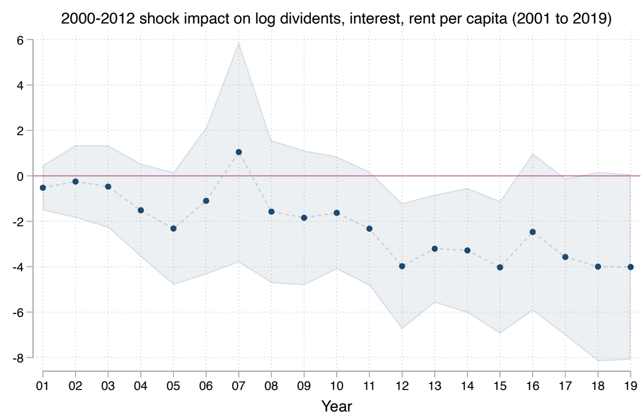
labor-market impact of greater trade exposure appears to be enduring joblessness. The response of TAA-related unemployment benefits to trade exposure is positive over shorter time horizons, with the impact coefficient reaching its maximum value of \$0.83 (t-value = 1.23) per capita over the 2000 to 2006 time difference. For the mean 2000-2012 increase in import penetration of 0.89 percentage points, the implied increase in TAA-related unemployment benefits would be just \$0.74 per capita. For the 2000 to 2011 time difference and longer horizons, the impact of trade exposure on TAA-related benefits is negative but imprecisely estimated in most instances. This negative impact may arise from CZs that experience greater trade-related job loss having fewer remaining workers eligible to receive TAA. Among work-oriented government programs, only the small category of education and training assistance has a consistently positive response to greater import competition.

Figure A14: Trade Shock Impact on Components of Personal Income, 2000-2019

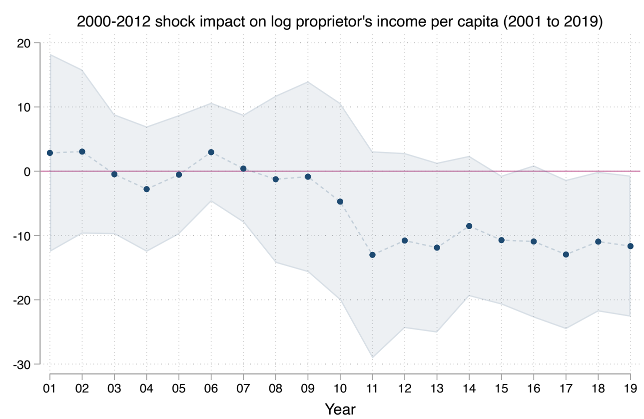
(a) Personal income less government transfers per capita



(b) Dividends, interest and rent per capita



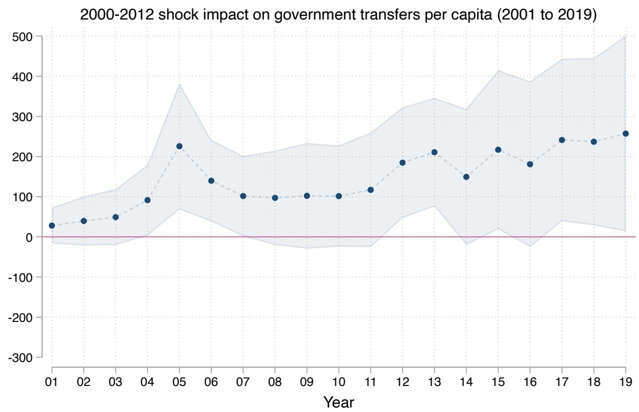
(c) Proprietor's income per capita



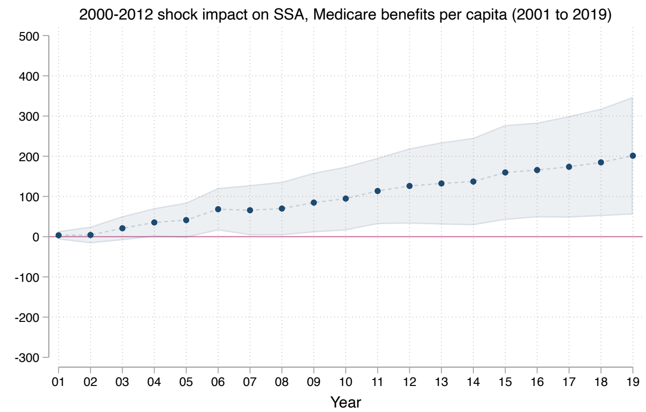
Note: Panels (a)-(d) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in the specified outcome between 2000 and the year indicated on the horizontal axis; the trade shock is the decadalized 2000-2012 change in CZ import exposure, as defined in (1) and instrumented by (3); and control variables include initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies. Regressions are weighted by the CZ population in 2000; standard errors are clustered by state. The working-age population is individuals ages 18 to 64 years old.

Figure A15: Trade Shock Impact on Government Transfers per Capita by Program Type

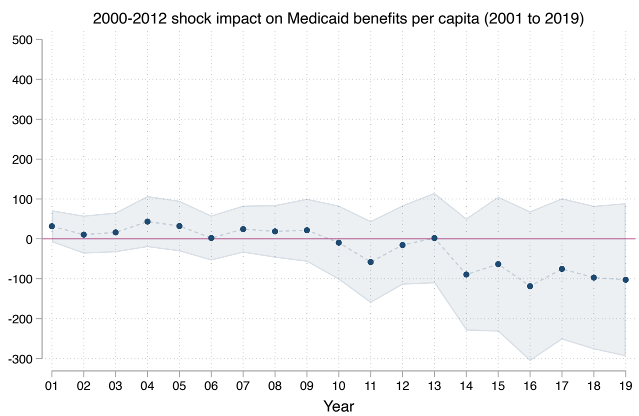
(a) Government transfers per capita



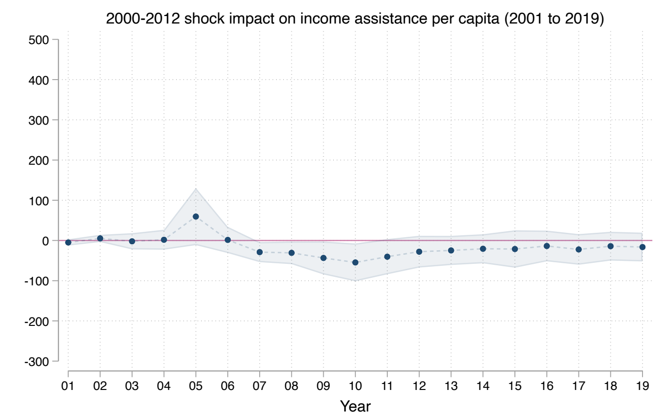
(b) SSA, SSDI, Medicare payments per capita



(c) Medicaid benefits per capita



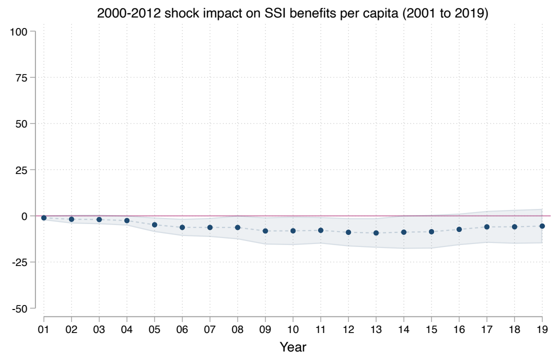
(d) Income assistance per capita



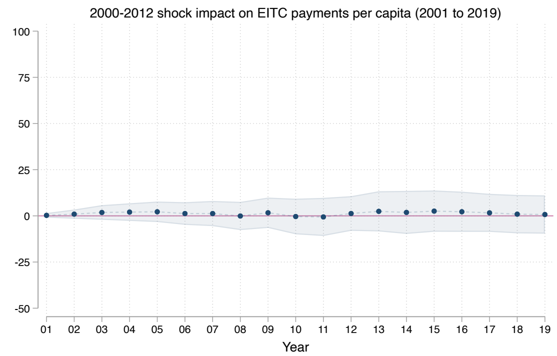
Note: Panels (a)-(d) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in the indicated value per capita between 2000 and the year indicated on the horizontal axis; the trade shock is the decadalized 2000-2012 change in CZ import exposure, as defined in (1) and instrumented by (3); and control variables are the same as in Figure 5. Regressions are weighted by the CZ population in 2000; standard errors are clustered by state.

Figure A16: Trade Shock Impact on Government Transfers per Capita by Detailed Program

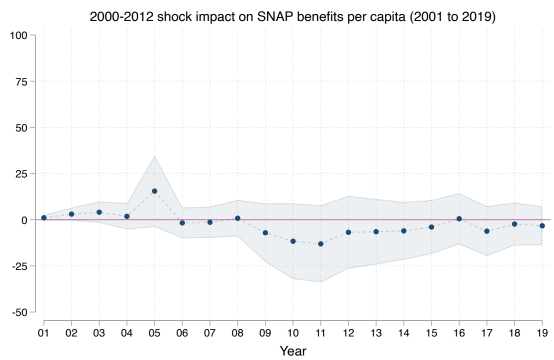
(a) Supplemental Security Income



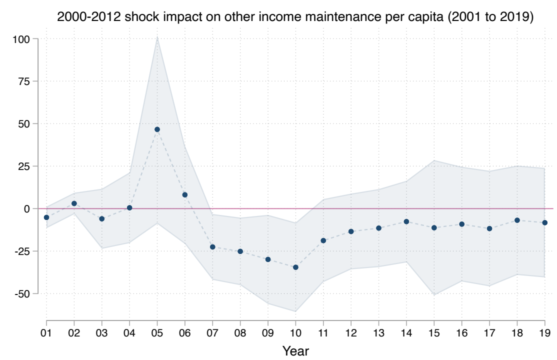
(b) Earned Income Tax Credits



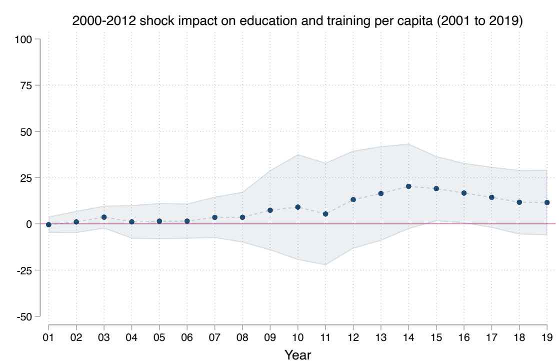
(c) Supplemental Nutrition Assistance



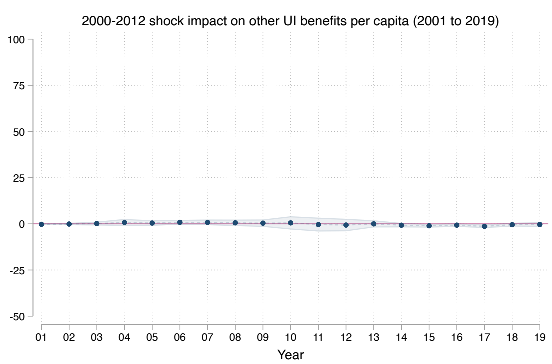
(d) Other income assistance



(e) Education and training assistance



(f) Other unemployment benefits (including TAA)

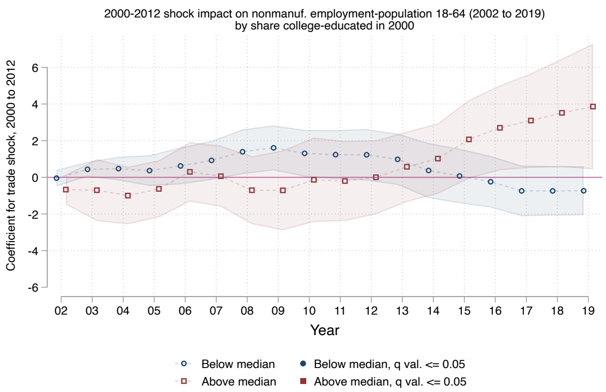


Note: Panels (a)-(f) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in the indicated value per capita between 2000 and the year indicated on the horizontal axis; the trade shock is the decadalized 2000-2012 change in CZ import exposure, as defined in (1) and instrumented by (3); and control variables include initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies. Regressions are weighted by the CZ population in 2000; standard errors are clustered by state.

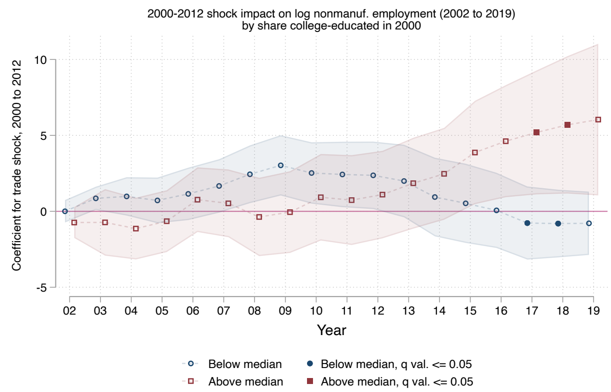
A.5.5 Heterogeneity in Impacts

Figure A17: Heterogeneity in Trade Shock Impacts by Initial College Educated Population: Additional Results

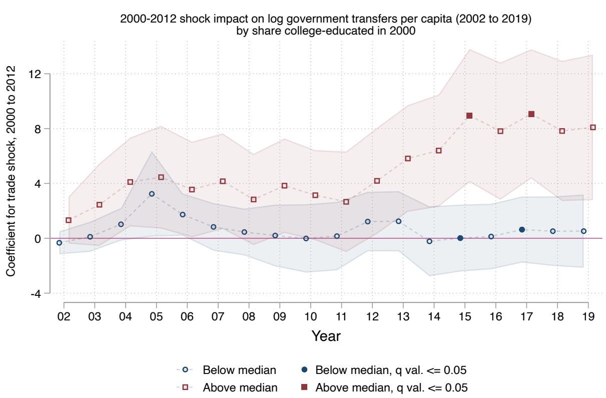
(a) Non-manuf. employment/Working-age pop.



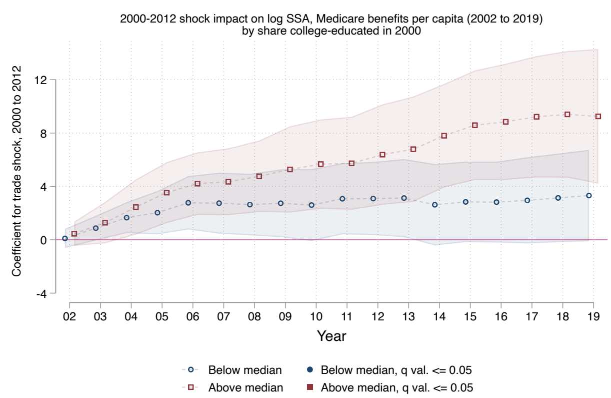
(b) Log non-manuf. employment



(c) Log government transfers per capita



(d) Log Soc. Sec., Medicare benefits per capita

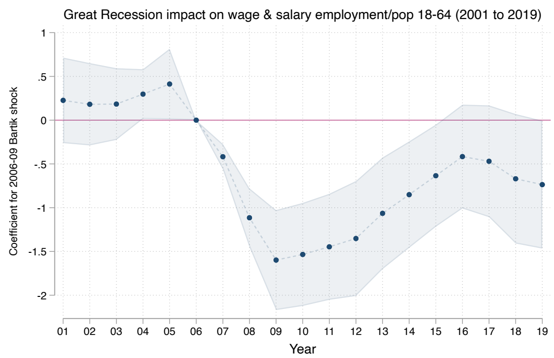


Note: Panels (a)-(d) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. Coefficient estimates whose differences have a minimal Benjamini-Hochberg q-value of less than 0.05 are shown with solid markers (with hollow markers for other estimates). Estimates are reported for two samples: the 386 CZs whose share of the college educated in the working-age population was below the population-weighted national median in 2000, and the complementary the set of 336 CZs. The dependent variable is the change in the indicated measure between 2001 and and the year on the horizontal axis; the trade shock is the decadalized 2000-2012 change in CZ import exposure as defined in (1) and instrumented by (3); control variables include initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies. Regressions in panels (a)-(c) are weighted by the CZ working-age population in 2000; regressions in panel (d) are weighted by the CZ total population in 2000. Standard errors are clustered by state.

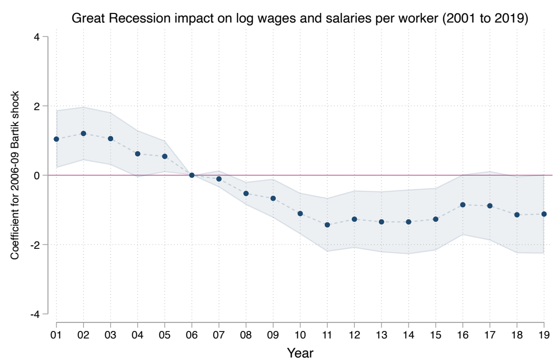
A.5.6 Labor Market Adjustment to the Great Recession

Figure A18: Local Labor Market Adjustment to the Great Recession, Controlling for the China Trade Shock

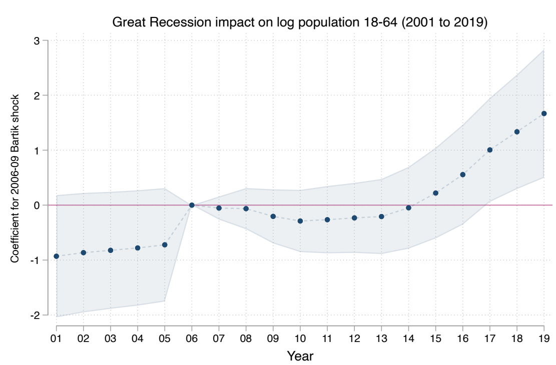
(a) Wage and salary employment/Working-age pop.



(d) Log labor compensation per worker



(c) Log population, ages 18-64



Note: Panels (a)-(c) report 2SLS coefficient estimates for β_{1h} in (4) and 95% confidence intervals for these estimates. The dependent variable is the change in the indicated measure between 2006 and and the year on the horizontal axis; the Great Recession shock is defined in (6); control variables include the instrument for the China trade shock over 2000 to 2012 defined in (3), initial-period CZ employment composition (share of employment in manufacturing, routine-task-intensive occupations, and offshorable occupations, as well as employment share among women), initial-period CZ demographic conditions (shares of the college educated, the foreign born, non-whites, and those ages 0-17, 18-39, and 40-64 in the population), and Census region dummies. Regressions are weighted by the CZ working-age population in 2001; standard errors are clustered by state.

A.6 Welfare Analysis of the China Trade Shock

A.6.1 Impacts of Import Competition on Housing Values

Quantitative assessments of the China trade shock consider changes in prices of traded goods, and abstract away from non-traded goods, the most important of which is housing services. Feler and Senses (2017) document that median contract rents fell by more in commuting zones more exposed to trade with China. Using data for 1990 to 2007, they estimate that a one-standard deviation difference in trade shocks between two CZs over 2000 to 2007 would imply a 4.96 percentage point differential change in contract rents (on a decadalized basis). (This calculation uses the coefficient estimate in Feler and Senses (2017) Table 5 of -2.47 and multiplies it by the standard deviation of the 2000-2007 China trade shock (-2.01).) Using this estimate, each 1.0 percentage point increase in import penetration over 2000 2012 (on a decadalized basis) implies a 2.78 ($= 4.96/\,[1.00/0.56]$) percentage-point decrease in contract rents (0.56 is the standard deviation of the 2000-2012 trade shock in Table 1). If we (unrealistically) treat all CZ residents as renters and use a share of housing in the consumer price index of 32.8% (Moretti, 2013), then each 1.0 percentage point increase in the trade shock would reduce the Consumer Price Index by 0.91 ($= 2.78 \times 0.33$) percentage points over a decade. Because many residents of trade-exposed CZs are homeowners rather than renters, this estimate will however substantially overstate how trade exposure affects the cost of living through the price of housing. See Notowidigdo (2020) on how negative impacts of adverse labor demand shocks on housing prices can reduce incentives for out-migration.

A.6.2 Other Quantitative Analyses of the China Trade Shock

Kim and Vogel (2020), in a fourth general equilibrium assessment of trade with China, allow for search and matching frictions in the labor market, which generate unemployment; distinct from the other approaches, they allow for amenities in employment, such that non-employed workers suffer an additional non-pecuniary loss. Applying their framework empirically and quantitatively, they find that trade exposure causes substantial variation in changes in welfare across commuting zones: the CZ at the 90^{th} percentile of exposure has a change in welfare that is 3.1 percentage points lower than in a CZ at the 10^{th} percentile of exposure. Because they measure trade exposure using U.S. tariffs (the threat of which was greatly diminished by China's entry into the WTO), as in Pierce and

Schott (2016), and not using import penetration, as in Autor et al. (2014), it is difficult to translate the dispersion in income effects that they find with our analysis.