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On the Persistence of the China Shock

ABSTRACT We evaluate the duration of the China trade shock and its impact on a wide range of outcomes over the period from 2000 to 2019. The shock plateaued in 2010, enabling analysis of its effects for nearly a decade past its culmination. Adverse impacts of import competition on manufacturing employment, overall employment-population ratios, and income per capita in more trade-exposed US commuting zones are present out to 2019. Over the full study period, greater import competition implies a reduction in the manufacturing employment-population ratio of 1.54 percentage points, which is 55 percent of the observed change in the value, and the absorption of 86 percent of this net job loss via a corresponding decrease in the overall employment rate. Reductions in population head counts, which indicate net out-migration, register only for foreign-born workers and the native born who are 25–39 years old, implying that exit from work is a primary means of adjustment to trade-induced contractions in labor demand. More negatively affected regions see modest increases in the uptake of government transfers, but these transfers primarily take the form of Social Security and Medicare benefits. 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

Conflict of Interest Disclosure: The authors did not receive financial support from any firm or person for this article or from any firm or person with a financial or political interest in this article. David Autor is on the Panel of Economic Advisers for the Congressional Budget Office, is a trustee of the Urban Institute, and is a member of the advisory boards of the Opportunity and Inclusive Growth Institute at the Federal Reserve Bank of Minneapolis, the Malcolm Wiener Center for Social Policy at the Harvard Kennedy School, and WorkRise at the Urban Institute. Other than the aforementioned, they are currently not officers, directors, or board members of any organization with an interest in this article.

the 1980s, indicating that the China trade shock holds lessons for other episodes of localized job loss. Import competition from China induced changes in 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 average real incomes.

Recent literature analyzing 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 the North Hickory, North Carolina, commuting zone (CZ) 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

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 to the Heckscher-Ohlin trade model, for example (Feenstra 2016).

3. The seminal work of Jacobson, LaLonde, and Sullivan (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.

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]

In this paper, we reexamine 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, for example, 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

4. Research addresses regional and industry employment (Autor, Dorn, and Hanson 2013a; Pierce and Schott 2016; Handley and Limão 2017); plant closure (Bernard, Jensen, and Schott 2006; Acemoglu and others 2016; Asquith and others 2019); labor earnings (Autor and others 2014; Chetverikov, Larsen, and Palmer 2016); government transfers (Autor, Dorn, and Hanson 2013a; Autor and others 2014); housing prices and tax revenues (Feler and Senses 2017); migration (Autor, Dorn, and Hanson 2013a; Monte, Redding, and Rossi-Hansberg 2018; Faber, Sarto, and Tabellini 2019; Greenland, Lopresti, and McHenry 2019); and marriage, fertility, and mortality (Autor, Dorn, and Hanson 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), US exports (Feenstra, Ma, and Xu 2019), domestic innovation (Hombert and Matray 2018; Autor and others 2020b), and schooling (Greenland and Lopresti 2016). Additional work studies how trade shock impacts are mediated by labor market conditions (Austin, Glaeser, and Summers 2018), industry product cycles (Eriksson and others 2019), labor regulations (Chan 2019), and trade deficits (Dix-Carneiro and Kovak 2017). Autor, Dorn, and Hanson (2016) and Redding (2020) review the literature; and Dorn and Levell (2021) provide a comparative European-US 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 head counts for less skilled workers (Bound and Holzer 2000; Diamond 2016; Notowidigdo 2020). Charles, Hurst, and Schwartz (2019) report comparable downward impacts on local employment rates from regional exposure to the national decline in manufacturing over 2000–2017.

in real income (Amior and Manning 2018; Galle, Rodríguez-Clare, and Yi 2017), 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. When examining the full study period, the midpoint for estimates using alternative data sources indicates that greater import competition implied a reduction in the manufacturing employment-population ratio of 1.54 percentage points, which is 55 percent of the observed change in the value, and that 86 percent of this manufacturing employment decline was absorbed by a decrease in the overall employment rate. Although the trade shock generated modest net out-migration, this occurred only among native-born adults age 25–39 and the foreign-born. Impacts of the trade shock thus appear to be long-lasting and to entail reduced participation in work, as 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 logical explanation is that the shock itself never stopped intensifying. The data do not support this conjecture, however. 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 roll back some reforms (Lardy 2019; Brandt and others 2020; Brandt and Lim 2020). Nor did export growth simply move en masse from China to nearby countries. Whether we look at the US 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 (Hanson 2020). Thus, the China trade shock did not unwind, but it did plateau, allowing us to 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, Scheinkman, and Shleifer 1995; Diamond 2016; Bloom and others 2019).

A second is that specialization in a narrow set of industries left these regions exposed to industry-specific shocks that, once industry decline was initiated, would set in motion a process that is self-reinforcing (Dix-Carneiro and Kovak 2017). We find some support for both the dearth of human capital hypothesis and the reverse agglomeration hypothesis. We also discuss other mechanisms that may be at work, though we do not provide a definitive, monocausal 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, Daniel, and Sanders 2002; Black, McKinnish, and Sanders 2005). Similarly, localities harder hit by the Great Recession had lower employment-population ratios ten 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—for example, whether manufacturing employment fell by more in more-trade-exposed locations—but not the absolute impact of the shock, for example, whether the trade shock reduced manufacturing employment nationally (Heckman, Lochner, and Taber 1998; Helpman 2018). Although relative impacts are informative about the distributional 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-form empirical results to discipline the calibration of quantitative trade models. These calibrations suggest that US aggregate welfare gains from China's market opening were positive but small and that some *relatively* adversely affected 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

6. This issue also arises in empirical work in macroeconomics (Chodorow-Reich 2019, 2020; Guren and others 2021; Wolf 2019).

7. See Caliendo, Dvorkin, and Parro (2019); Rodríguez-Clare, Ulate, and Vásquez (2020); Adão, Arkolakis, and Esposito (2019); Galle, Rodríguez-Clare, and Yi (2017); Kim and Vogel (2020).

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 US 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 United States and other countries. Few economists would interpret our empirical results as justifying greater trade protection. As expected, quantitative models indicate that US 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 shield workers from concentrated job loss.

I. The Timing of the China Shock

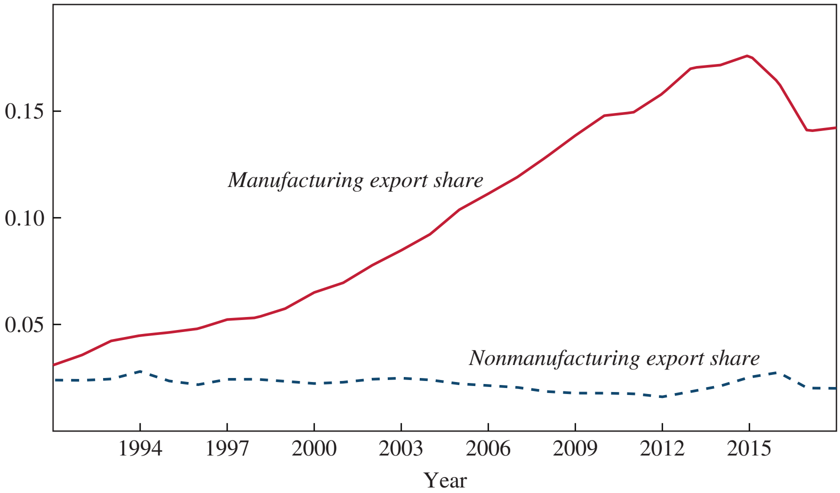
We begin by reviewing China's recent export performance. Enduring impacts of the China trade shock on US local labor markets could be the result of the shock continuing to build after the 2000s and into the 2010s. We find this not to be the case. China's export growth has progressed through well-defined periods of initiation, acceleration, and stabilization. The country's share of the US 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 percent in 1991 to 17.6 percent in 2015, before dipping to 14.2 percent in 2018, as documented in figure 1, panel A.[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 exports of nonmanufactured goods has changed little over time, averaging just 2.3 percent 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

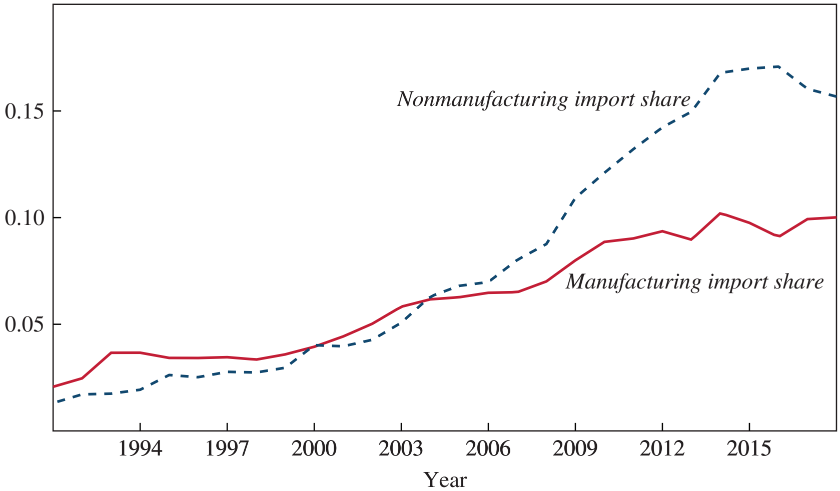
8. Online appendix A.2 provides details on the data we use in this section.
9. See, for example, *Economist* (2015).

Figure 1. China's Share of World Trade by Major Sector

A. China's share of world exports



B. China's share of world imports



Source: UN Comtrade, SITC Revision 2.

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 2006), 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 percent in 1991 to 15.7 percent in 2018 (figure 1, panel B).[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 2006; Vogel 2011).[11] China's market transition unleashed dramatic improvements in productivity resulting from spatial and sectoral reallocation of resources (Hsieh and Klenow 2009; Brandt, Tombe, and Zhu 2013), the expansion of the private sector at the expense of inefficient state-owned enterprises (Song, Storesletten, and Zilibotti 2011; Khandelwal, Schott, and Wei 2013; Hsieh and Song 2015; Bai, Krishna, and Ma 2017), inflows of foreign capital and imported intermediate inputs (Yu 2015; Brandt and Morrow 2017), a reorientation of manufacturing to export assembly plants producing at the behest of multinational firms (Feenstra and Hanson 2005; Brandt and others 2017), and the adoption of previously banned foreign technologies (Hu and Jefferson 2009; Wei, Xie, and Zhang 2017; Li 2020).[12] Over 1998–2007, output per worker in Chinese manufacturing grew at the stunning annual average rate of

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

11. China's manufacturing export growth in fact began in the 1980s (Brandt and others 2020). Given the country's very low initial export levels, however, this early growth 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.

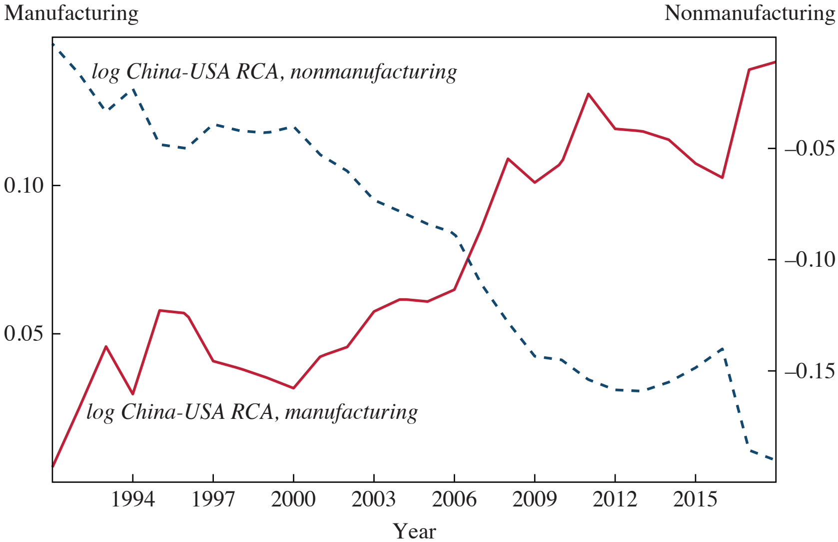
8.0 percent (Brandt, Van Biesebroeck, and Zhang 2012; Liu and Ma 2020). The 1992–2007 export surge thus appears to be an episode in which China's trade growth was driven largely by productivity gains resulting from its market opening.

Because much of China's post-Deng manufacturing growth was transitional, it was destined to be short-lived (Song, Storesletten, and Zilibotti 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 total factor productivity (TFP) growth in Chinese manufacturing was negative in eight of eleven 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 and others 2020). The end of the productivity growth miracle and the roll back 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 1991–2001 and 8.4 percentage points over 2001–2010, it was –0.4 percentage points over the 2010–2018 period.

China's post-2010 slowdown is also apparent when we compare its trade performance to the United States, which is helpful for defining the shock facing US 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 share of world exports of all goods.[13] In figure 2, we show the gap between the log China and log US RCA for manufacturing and nonmanufacturing. China began with a modest comparative advantage in manufacturing over the United States in 1991—as indicated by a China-US 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 United States had leaped to 13.1 percentage points, before falling and rising over the ensuing eight years. After 2010, the China-US relative RCA exhibits little trend.

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, Lind, and Muendler (2015).

Figure 2. China versus US Revealed Comparative Advantage



Source: UN Comtrade, SITC Revision 2.

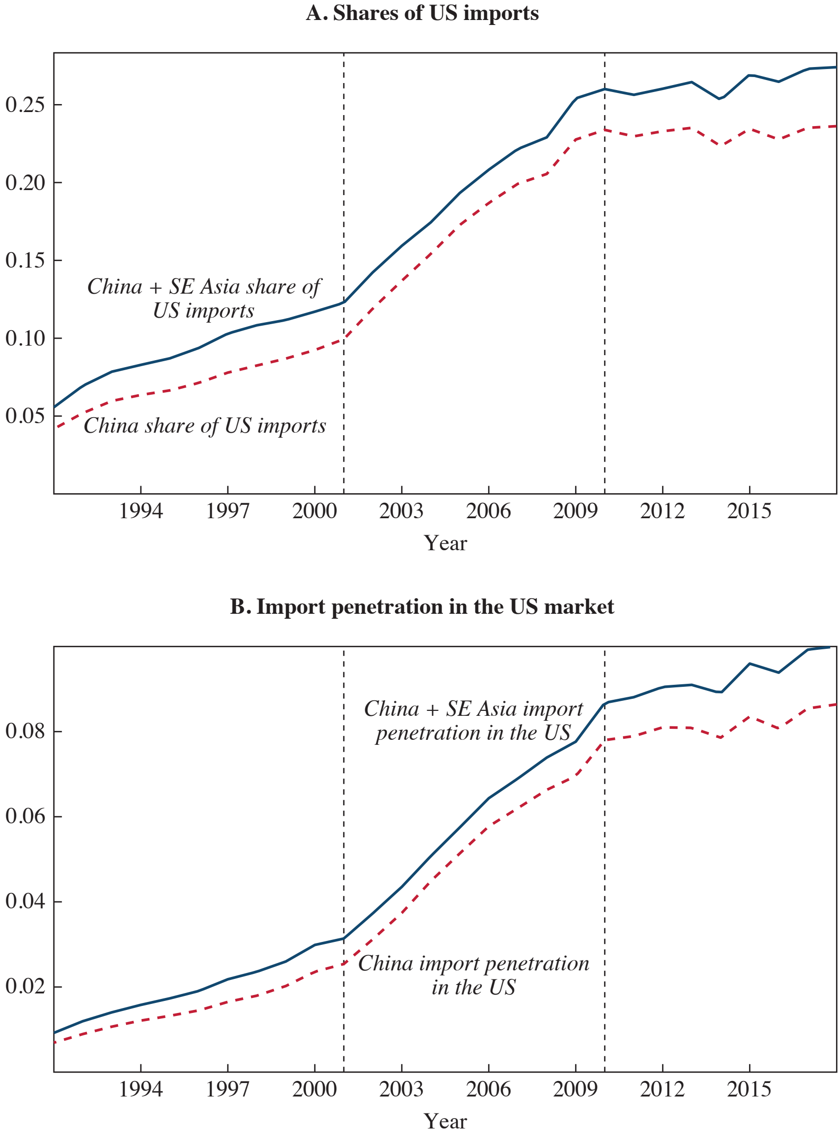
Note: Revealed comparative advantage (RCA) is a country's share of world exports in a sector relative to a country's share of world exports of all merchandise.

By construction, China's RCA in nonmanufacturing compared to the United States 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 nonmanufacturing relative to the United States likely reflects the success of US 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 US manufacturing import growth by offshoring production to other low-wage countries. Chinese firms have been active in building industrial parks for export production in Southeast Asia, and in Vietnam in particular.[14] In figure 3, we plot shares of US imports and import penetration in the US market for China alone and for China combined with low-income countries in Southeast Asia. We select these countries—Cambodia, Indonesia, Laos, Myanmar, Philippines,

14. See, for example, *Economist* (2016).

Figure 3. US Manufacturing Imports from China and Southeast Asia



Sources: UN Comtrade, SITC Revision 2; St. Louis Federal Reserve Bank; and authors' calculations.
Note: Import penetration in panel B is the ratio of US imports of manufactured goods to US domestic absorption (defined as gross output plus imports minus exports). All values exclude oil and gas.

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 United States dwarf those of Southeast Asia. In 2010, China accounted for 23.4 percent of US manufacturing imports, whereas the Southeast Asian nations accounted for just 2.6 percent. In 2018, these figures were only modestly changed, at 23.6 percent and 3.8 percent, respectively. Although Southeast Asian countries did gain a larger share of US imports, the increase over the 2010 to 2018 period was just 1.2 percentage points. Import penetration, shown in figure 3, panel B, tells a similar story. Whether we look at China alone or combined with low-income Southeast Asian nations, its US market presence largely stabilized after 2010.[16]

Taken together, these trade patterns allow us to mark three distinct periods of China's export 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. This slowdown may have been the result of China having completed its transitional period of post-reform growth, and possibly also 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.

II. 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, Dorn, and Hanson (2013a) and Acemoglu and others (2016). We aim to identify the causal effect of import competition from China on labor market outcomes for US CZs, which is 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.

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 $2,130 (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; World Bank, "DataBank: World Development Indicators," https://databank.worldbank.org/source/world-development-indicators.

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 2020).

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 United States	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

Sources: UN Comtrade, SITC Revision 2; NBER-CES Manufacturing Industry Database; BEA Regional Economic Information System; National Vital Statistics System; and authors' calculations.

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

II.A. Baseline Specification

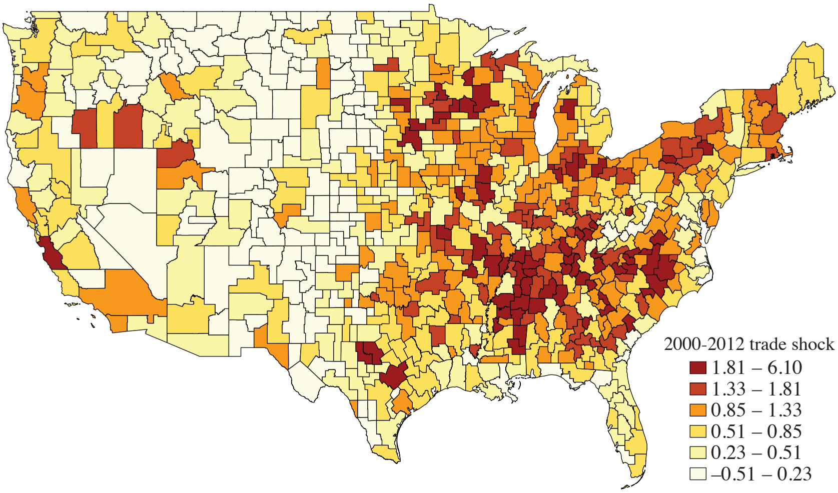
We examine exposure to import competition from China for the 722 CZs in the continental United States. As in Acemoglu and others (2016), our measure of trade exposure is the average change in Chinese import penetration across industries, weighted by industry shares in initial CZ employment:

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

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 US 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 the change in import penetration as the growth in US industry imports from China, $\Delta M^{cu}_{j\tau}$, divided by initial industry domestic absorption (US industry shipments plus net imports, $Y_{jt} + M_{jt} - X_{jt}$), as summarized in table 1.[17] Differences in $\Delta IP^{cu}_{i\tau}$ across CZs stem from variation in local industry

17. We measure imports using Harmonized System (HS) trade data from UN Comtrade, harmonized to four-digit Standard Industrial Classification (SIC) industries, and industry shipments using the National Bureau of Economic Research (NBER) manufacturing productivity database (Autor and others 2014).

Figure 4. Chinese Import Penetration (2000–2012) in US Commuting Zones



Sources: UN Comtrade, SITC Revision 2; NBER-CES Manufacturing Industry Database; BEA Regional Economic Information System; National Vital Statistics System; and authors' calculations.

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

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 equation (1) across CZs for the 2000–2012 time period. The most affected 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 large metropolitan areas). These CZs are where US manufacturing relocated as it moved out of major cities in the Northeast and northern Midwest in the middle of the twentieth century (Eriksson and others 2019).[18]

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,

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

18. Autor, Dorn, and Hanson (2013b) show that there is little correlation between a CZ's exposure to Chinese import competition and exposure to routine task-replacing technological change.

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 and 2000–2019. We focus on the period 2000–2019, 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 2000–2012, whose first year is one year prior to China's WTO entry and whose final year postdates the culmination of the trade shock in 2010 and the volatility in global trade following the 2008 global financial crisis.[19] For $h < 12$, we estimate shock impacts for outcome periods that are less than complete—that is, 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.

The control vector X'_{it} contains time trends for US Census divisions and start-of-period CZ-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 and non-white workers, the college-educated portion of 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 age 0–17, 18–39, and 40–64, which control for demographic factors that may affect labor force participation and eligibility for government transfers (see online appendix table A2). For all outcomes other than population head counts,

19. We show in online appendix figure A4 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 2000–2007 period. The 2000–2007 shock is 79.8 percent of the magnitude of the 2000–2012 shock and 88.8 percent 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.

regressions are weighted by the CZ working-age population (age 18–64) in the initial year; for head count regressions, weights are the total CZ population in the initial year. All specifications cluster standard errors at the state level.

We focus on US imports from China and not US 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 US export growth.[20] Autor, Dorn, and Hanson (2013a) find similar results when replacing growth in US imports with growth in US net imports. Consistent with these findings, Feenstra, Ma, and Xu (2019) detect no impact of growth in US exports to China on US manufacturing employment, although overall US export growth is positively associated with manufacturing activity.[21]

In related work, Eriksson and others (2019) study the Japan trade shock of 1975–1985 (during which Japan's exports of autos and other durables surged) and the East-Asian Tiger trade shock of 1975–1988 (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 US CZs, while Batistich and Bond (2019) find a similar overall null effect of the shock on CZ manufacturing employment, though a differentially negative effect on Black relative to white manufacturing workers. 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 China trade shock. The former group had relatively high levels of income, college attainment,

20. The 2000–2012 increase in US manufacturing imports from China ($292 billion) was 4.1 times the increase in US manufacturing exports to China ($71 billion), for values in 2015 US dollars.

21. Weak US manufacturing exports to China could be a result of US 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 qualitatively comparable to the US 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, Findeisen, and Suedekum 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 United States, where imports strongly dominated exports (Dorn and Levell 2021). Weak US export growth could also be attributable to the forces behind large trade surpluses in China and trade deficits in the United States (Autor, Dorn, and Hanson 2016; Dix-Carneiro and others 2021).

and patents per capita, whereas the latter had relatively low values of these indicators.[22]

Import penetration in equation (1) includes all types of shipments from China to the United States, whether they emanate from Chinese companies exporting final goods to the US 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, Flaaen, and Pandalai-Nayar (2020) find that much of the post-1990 employment decline in US manufacturing occurred in establishments owned by US 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 percent 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 percent by 2015 (Ma 2020). Unfortunately, our data do not allow us to separate US imports from China into those that came through global value chains versus other channels. In recent work, Aghion and others (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.[23]

II.B. Causal Identification

A challenge for identifying the causal impact of import exposure on labor-market outcomes in equation (2) is that US imports may change both because of shocks to US product demand and shocks to foreign product

22. Despite a null net impact of the Japan shock on overall employment levels, Borjas and Ramey (1995) find that US metropolitan areas more exposed to import competition during the East Asian shock period had large increases in college-noncollege 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.

23. In Aghion and others (2021), adverse impacts of import exposure are concentrated on low-productivity firms. In the US market, by contrast, Bernard, Jensen, and Schott (2006) and Autor and others (2014) find that trade-induced employment declines are concentrated in larger manufacturing establishments, consistent with the framework in Holmes and Stevens (2014).

supply, where the former may be correlated with the residual, ε_{it+h}. To identify the foreign supply–driven component of US imports from China, we follow Autor, Dorn, and Hanson (2013a) and Acemoglu and others (2016) in instrumenting US import exposure, $\Delta IP^{cu}_{i\tau}$, using non-US exposure, $\Delta IP^{co}_{i\tau}$, which we measure as the industry-level growth of Chinese exports to eight other developed countries:[24]

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

where $\Delta IP^{co}_{j\tau} = \Delta M^{co}_{j\tau}/(Y_{jt-3} + M_{jt-3} - X_{jt-3})$. This expression differs from equation (1) by using imports from China in other high-income markets ($\Delta M^{co}_{j\tau}$) in place of US imports ($\Delta M^{cu}_{j\tau}$), the three-year lag of industry absorption ($Y_{jt-3} + M_{jt-3} - X_{jt-3}$) in place of its base year t value, and the ten-year lag of CZ industry employment shares, $s_{ijt-10} \equiv L_{ijt-10}/L_{it-10}$, in place of year t values (see table 1).[25]

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 (Autor and others 2014; Autor and others 2020b). Recent literature formalizes the basis for identification and inference in such shift-share settings. Borusyak, Hull, and Jaravel (2022) treat identification as based on exogeneity of the shifts—that is, the industry-level 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.[26] Adão, Kolesár, and Morales (2019) 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

24. 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.

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

26. The assumption of exogenous shifts is evident in industry-level analyses of the China shock that instrument a US industry's import growth from China with the industry's Chinese imports in other countries (Autor and others 2014; Acemoglu and others 2016). Goldsmith-Pinkham, Sorkin, and Swift (2020) develop an alternative approach to shift-share analysis, which is based on exogeneity of the shares (i.e., preexisting industry structures). As we discuss in online appendix A.4, their approach appears to be less suitable for our application. See Ferman (2019) for further discussion.

statistical inference. In online 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.

III. Empirical Results

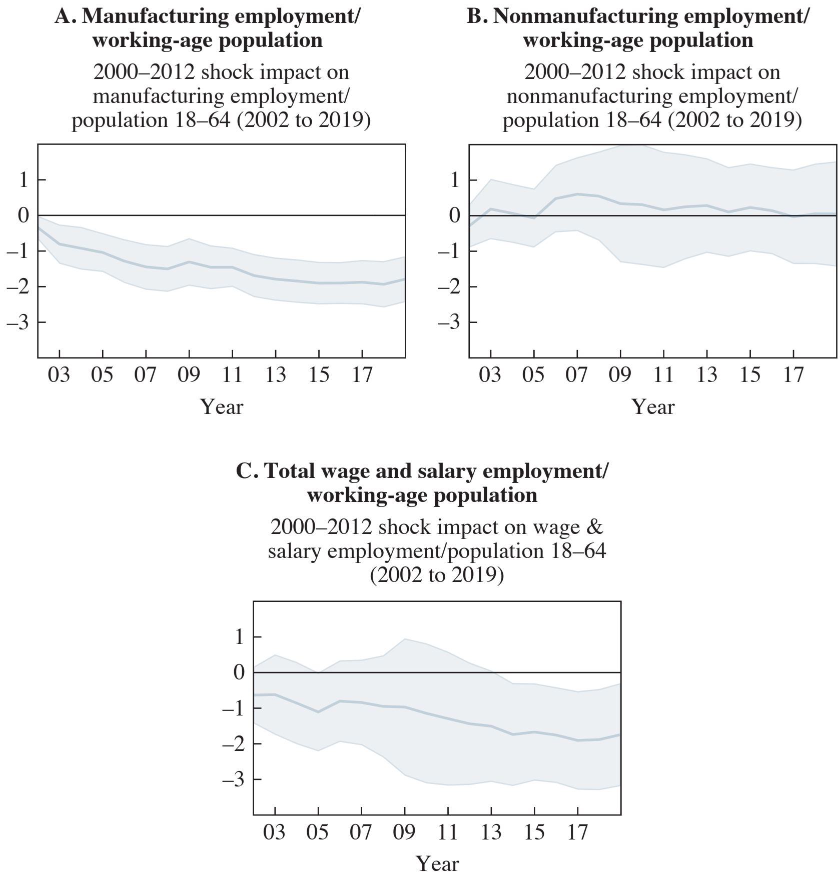
This section presents our main empirical results for the impact of trade exposure on US CZs. We use equation (2) to estimate how the 2000–2012 trade shock affected CZs over 2000–2019, focusing on three sets of outcomes. The first are ratios of manufacturing employment, nonmanufacturing employment, and total wage and salary employment to the working-age population (defined as individuals age 18–64). We measure employment using the Bureau of Economic Analysis (BEA) Regional Economic Information System (REIS) 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 nonemployment and other sectors. A second set of outcomes is population head counts, 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 is 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. We measure income, earnings, and government transfers using county-level data from the REIS. For most series, we use 2000 as the initial year.[27] Summary statistics on outcome and control variables are reported in the online appendix tables A1 and A2; online appendix A.2 contains further details on data sources and variable construction.

III.A. Employment Outcomes

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 nonmanufacturing (where the latter is defined as total wage and salary employment less manufacturing employment). Figure 5 displays two-stage least squares estimates of impact coefficients for trade exposure from the

27. Because of BEA changes in industry classification from the SIC to the North American Industry Classification System (NAICS) in 2001, our REIS series for manufacturing and nonmanufacturing employment begins in that year, rather than 2000.

Figure 5. Trade Shock Impact on Employment, 2001–2019



Source: Authors' calculations.

Note: Panels report two-stage least squares coefficient estimates for β_{1h} in equation (2) and 95 percent 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 equation (1) and instrumented by equation (3). Control variables include initial-period CZ employment composition (shares 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-white individuals, and those age 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.

time difference specification in equation (2) when using equation (3) to instrument for equation (1). Each panel shows results for eighteen separate regressions, where the horizontal axis organizes specifications according to the time difference being considered, beginning with 2001–2002 and extending out to 2001–2019. Vertical bars show 95 percent confidence intervals for the coefficient estimates. Structured in this manner, figure 5 shows the cumulative impact of trade exposure for progressively longer time differences (one to eighteen years).

The results for manufacturing employment in figure 5, panel A, show a negative effect of trade exposure that builds quickly and then stabilizes. The impact coefficient is −1.45 (t-value = −4.41) for 2001–2007, which is the end year of analysis in Autor, Dorn, and Hanson (2013a), reaches −1.90 (t-value = −6.24) for the 2001–2015 period, and then attenuates modestly to −1.79 (t-value = −5.42) for the 2001–2019 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 2000–2012 of 0.89 percentage points (see table 1), the implied reduction in the manufacturing employment share over 2001–2019 is −1.59 (= −1.79 × 0.89) percentage points, or 59.3 percent of the overall 2001–2019 change in the manufacturing employment share of −2.68 percentage points (online appendix table A1). Alternatively, when comparing CZs at the 25th and 75th percentiles of trade exposure, the latter would be predicted to have a reduction in its manufacturing employment share that is 1.18 (= −1.79 × [1.17 − .51]) percentage points larger than in the former over 2001–2019. This compares to the 25th–75th 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 CZs after 2000.[28]

Those losing jobs in manufacturing may move into other sectors or out of employment. Simultaneously, they may move to another CZ.[29] In figure 5,

28. These sustained negative impacts of trade exposure on local manufacturing employment for the United States 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 United States.

29. 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.

panel B, we consider the impact of exposure to import competition on the CZ share of the working-age population employed in nonmanufacturing 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 nonmanufacturing sectors absorbed local workers released from manufacturing due to the China trade shock.[30] The wide standard error bounds in figure 5, panel B, are suggestive of heterogeneity across CZs in how nonmanufacturing absorbs displaced manufacturing workers, a possibility we explore in section III.D.

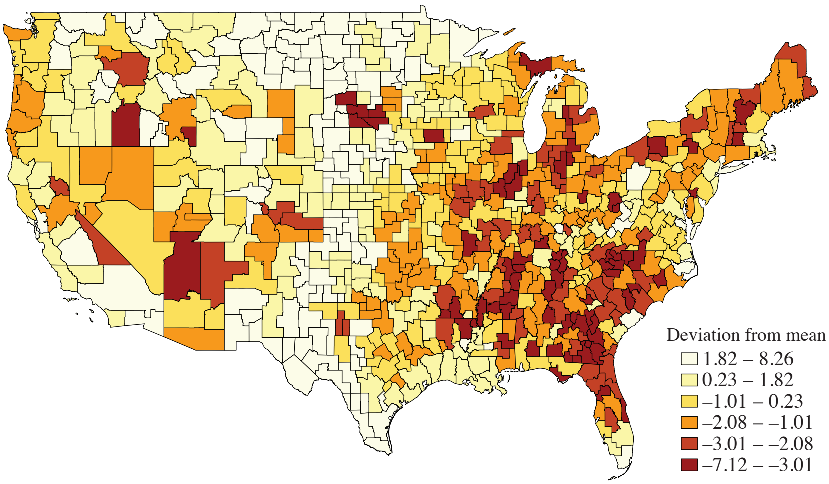
The fall in manufacturing employment and absence of offsetting job gains in nonmanufacturing imply a decline in total wage and salary employment in trade-exposed CZs, confirmed by figure 5, panel C. Decreases in the employment-population ratio are comparable in magnitude to those for manufacturing employment. The effect reaches its maximum value for the 2001–2017 period: the impact coefficient of −1.91 (t-value = −2.70) compares to −1.88 (t-value = −5.89) for manufacturing employment at the same horizon. The impact attenuates modestly late in the period, ending at −1.74 (t-value = −2.35) over 2001–2019, 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 nonemployment. 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 6, panel A, maps the actual change in wage and salary employment-population ratios across CZs for the 2001–2019 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 6, panel B, we map the implied impact of the China trade shock (as measured in equation (1) for 2000–2012) on changes in employment-population ratios for the same 2001–2019 time

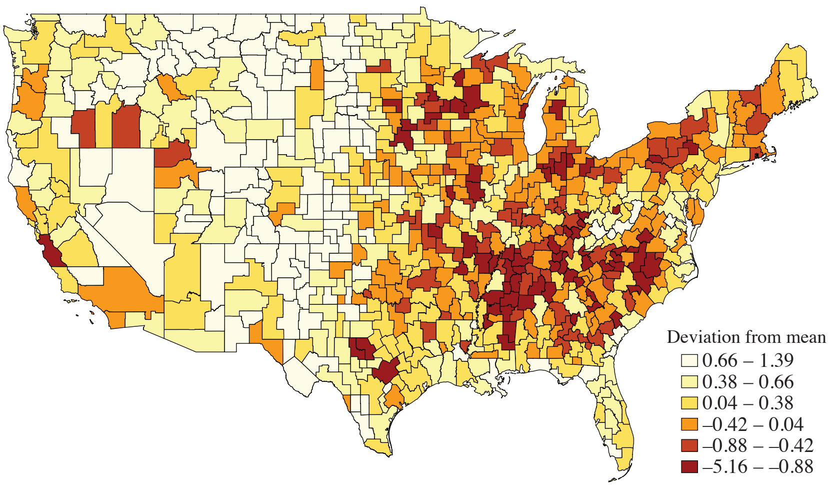
30. Bloom and others (2019) report evidence that CZs experiencing greater import competition do see larger increases in nonmanufacturing employment. We do not find this to be the case. We do, however, confirm their finding that employment losses spurred by the China trade shock are highly durable in CZs with a less-educated population, while they are ultimately more than fully offset in CZs with a more-educated population (see section III.D).

Figure 6. Implied Impact of Import Competition 2001–2019

A. Actual change in total employment-population ratio



B. Predicted change due to the China trade shock



Sources: UN Comtrade, SITC Revision 2; NBER-CES Manufacturing Industry Database; BEA Regional Economic Information System; National Vital Statistics System; and authors' calculations.

Note: Panel A plots the observed change in the ratio of wage and salary employment to working-age population over 2001–2019; panel B plots the implied change in this value due to the China trade shock based on our estimates (i.e., the product of the change in import penetration over 2000–2012 in equation (1) multiplied by the two-stage least squares coefficient estimate for this variable from the specification in equation (2) in which the dependent variable is the 2001–2019 change in the ratio of wage and salary employment to the working-age population, as shown in figure 4, panel C). Legends indicate values for the bottom four quintiles and the top two deciles of this value, arranged in order of shock intensity.

period. A visual comparison between the two panels in figure 6 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 US regions in the last two decades. The top 5 percent of CZs in terms of implied reductions in employment-population ratios, which are listed in online 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, thirty-three of the thirty-eight most exposed CZs had a manufacturing employment share above 25 percent, relative to the national population-weighted median of 15.4 percent, and thirty-three of thirty-eight had a college-educated share of the working-age population below 20 percent, relative to the national population-weighted median of 23.4 percent. The distinctiveness of trade-impacted CZs motivates the heterogeneity analysis that we undertake in section III.D.

To test robustness, online appendix figure A5 replicates the analysis in figure 5, now using US Census and American Community Survey (ACS) data to construct changes in employment-population ratios.[31] Census-ACS data have the advantage over the REIS of measuring employment as the number of individuals with a job rather than the total number of jobs in a CZ. A 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 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 percent sample) to 2007 (using pooled 1 percent annual samples from the 2006–2008 ACS files), 2012 (using pooled 1 percent annual samples from the 2011–2013 ACS files), and 2018 (using pooled 1 percent annual samples from the 2017–2019 files).[32] The 2000–2007 period matches the later time period studied in Autor, Dorn, and Hanson (2013a), the 2000–2012 period matches the duration of our trade shock measure, and 2000–2017/2019 is the longest post-2000 period that we can study using the census and ACS.

Focusing on results at the long horizon (i.e., 2000–2018 in census-ACS data, 2001–2019 in REIS data), we see that in figure A5 in the online

31. US Census Bureau, "American Community Survey (ACS)," https://www.census.gov/programs-surveys/acs.

32. See online appendix table A3 for summary statistics; other regression variables are the same as in figure 5.

appendix the impact coefficient for manufacturing employment-population of −1.67 (t-value = −7.00) compares to −1.79 (t-value = −5.42) in figure 5, panel A. Similarly, in figure A5 the impact coefficient for total employment-population of −1.23 (t-value = −3.32) compares to −1.74 (t-value = −2.35) in figure 5, panel C. These two sets of results appear quite comparable in light of the differences between the data sources, specifically: using census-ACS data-based estimates with 2000 as a start year (instead of 2001), combining 2017–2019 data for the 2019 observation, and measuring employment as persons employed instead of the total number of jobs. In census-ACS data, the implied trade-induced reduction in the manufacturing employment share is −1.49 (= −1.67 × 0.89) percentage points, or 50.9 percent of the overall 2000–2018 change in the manufacturing employment share of −2.92 percentage points (online appendix table A3); recall that when using REIS data, the corresponding figure is 59.3 percent. The two sets of results differ mildly in that the long-run impact of the China trade shock on the nonmanufacturing employment ratio is larger in figure A5, panel b (β = 0.43, t-value = 1.35) than in figure 5, panel B (β = 0.05, t-value = 0.06), though the former estimate is imprecise and too small to prevent a decline in the overall employment rate.[33] Thus, in census-ACS data the long-horizon, trade-induced decline in the overall employment-population ratio (given by the impact coefficient −1.23) absorbs 73.7 percent of the trade-induced decline in manufacturing employment (given by the impact coefficient −1.67), whereas in REIS data the corresponding figure is 97.2 percent (= 1.74 / 1.79).

One of the most surprising results reported by Autor, Dorn, and Hanson (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 infeasible to distinguish short-term from steady-state (or medium-run) impacts. The evidence 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, CZs that are more trade-exposed 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 nonemployment. The consequences of these disruptions went far beyond earnings and

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

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, Dorn, and Hanson 2019; Pierce and Schott 2020).

III.B. Spillovers across Regions and Industries

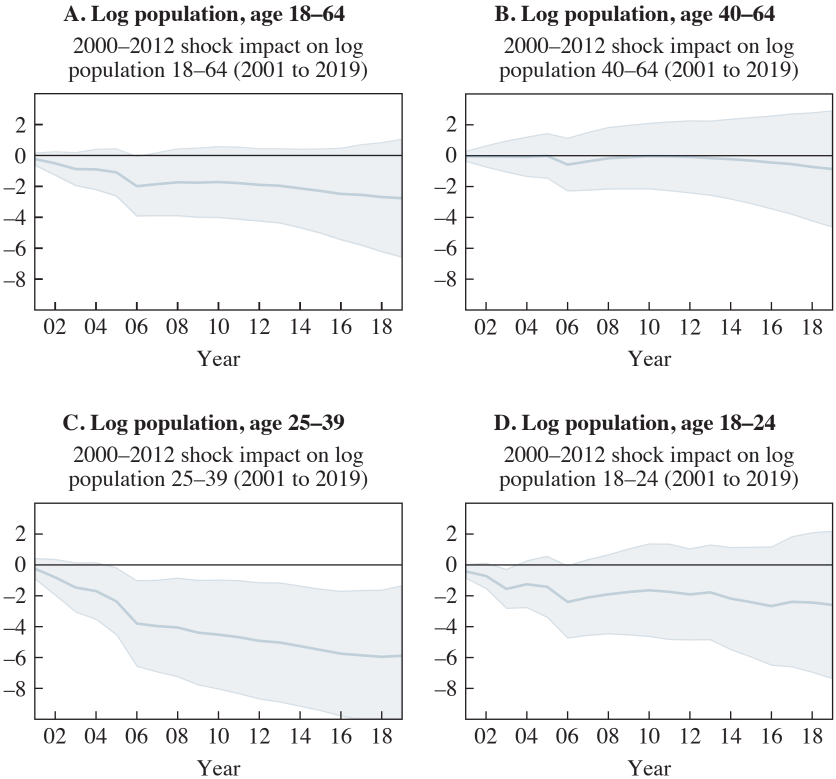
The empirical specification in equation (2) is consistent with a setting in which CZs are functionally small open economies. If changes in economic conditions in a given CZ do not materially affect outcomes in other CZs, we can examine each CZ on its own without modeling the transmission of shocks across locations. Of course, trade shocks having a direct impact on one CZ 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 head counts summarizes the net effect of trade shocks on the pool of both potential workers and nonworking residents (e.g., children, the elderly, and working-age nonparticipants). 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 head counts to greater import exposure separately for workers 18–24, 25–39, and 40–64 years old. This analysis is complicated by the fact that there are strong secular trends in population growth across US regions which began well before the China trade shock (Blanchard and Katz 1992). Greenland, Lopresti, and McHenry (2019) suggest that the approach used in Autor, Dorn, and Hanson (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.[34] Accordingly, when we estimate equation (2) for log population head counts, we additionally include as a control the log change in CZ population over 1970–1990 in order to capture long-standing trends in population growth.

Figure 7 reports estimation results for population head counts. In figure 7, panel A, we find negative but insignificant impacts of trade exposure on the size of the working-age population, though the point estimates grow as

34. 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.

Figure 7. Trade Shock Impact on Log Population Head Counts, 2000–2019



Source: Authors' calculations.

Note: Panels report two-stage least squares coefficient estimates for β_{1h} in equation (2) and 95 percent confidence intervals for these estimates. The dependent variable is the change in the specified log population head count 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 equation (1) and instrumented by equation (3); and control variables are the same as in figure 5 plus CZ population growth over 1970–1990. Regressions are weighted by the CZ total population in 2000; standard errors are clustered by state.

the time interval lengthens. The other three panels of the figure make clear that these negative effects are driven by one age group, those 25–39 years old. For adults age 18–24 and 40–64, the impact of greater import competition on population head counts is negative but small and imprecisely estimated for all time differences. The finding of no net impact of trade exposure on CZ populations is consistent with Faber, Sarto, and Tabellini (2019), while the greater responsiveness of younger adults echoes Bound and Holzer (2000), who show that less experienced workers are 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 25th and 75th percentiles of trade exposure, the more-exposed CZ would have a −3.89 (= −5.89 × [1.17 − .51]) percentage point larger decrease in head counts for this age group. For comparison, the 75th–25th percentile difference in population growth across CZs for individuals age 25–39 over 2000–2019 was 20.92 (= 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 foreign-born workers 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 head counts by nativity. Online appendix figure A6 summarizes regressions for two time differences: 2000 (using data from the 2000 census) to 2010 (using data from the combined 2006–2010 ACS 1 percent samples), and 2000 to 2019 (using data from the combined 2015–2019 1 percent samples).[35] 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 2000–2019 is −0.75 (*t*-value = −0.79). This contrasts with impacts on foreign-born workers, for whom greater import competition significantly reduces population head counts over 2000–2019 ($\beta = -6.79$, *t*-value = −2.05). When looking at age subgroups within these populations, we see negative significant impacts of import competition on native-born workers age 25–39 ($\beta = -5.03$, *t*-value = −2.29), which aligns with the results in figure 7, and on foreign-born workers age 40–64 ($\beta = -12.70$, *t*-value = −2.08). Whereas native-born labor-supply responses are strongest for younger workers, for foreign-born workers they are strongest for older workers.

GRAVITY-BASED TRADE SHOCKS To evaluate how changes in labor-market outcomes in one region affect outcomes in other regions, Adão, Arkolakis, and Esposito (2019) build a general equilibrium trade model that captures such spillovers explicitly and generates reduced-form equilibrium

35. Because of small population counts of foreign-born workers in many CZs in any individual year, we use combined five-year ACS samples rather than the combined three-year samples used in online appendix figure A5.

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 equation (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 for employment outcomes the larger and the closer these other markets are, as dictated by the gravity structure of trade. Adão, Arkolakis, and Esposito (2019) quantify this cross-region spillover by adding to the specification in equation (2) the gravity-weighted changes in import competition in 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. Online appendix A.5.3 provides details on the estimation and reports results. For all time differences, the impacts of trade shocks to a CZ's own industries are very similar in magnitude and significance to those in figure 4. 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 and others (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 and others (2016) and Pierce and Schott (2016) document spillovers that operate through input-output linkages in industry supply chains. Rising US furniture imports from China, for instance, may cause US factories in this downstream industry to reduce purchases of inputs from the upstream (i.e., supplier) sectors with which it is linked—for example, 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.

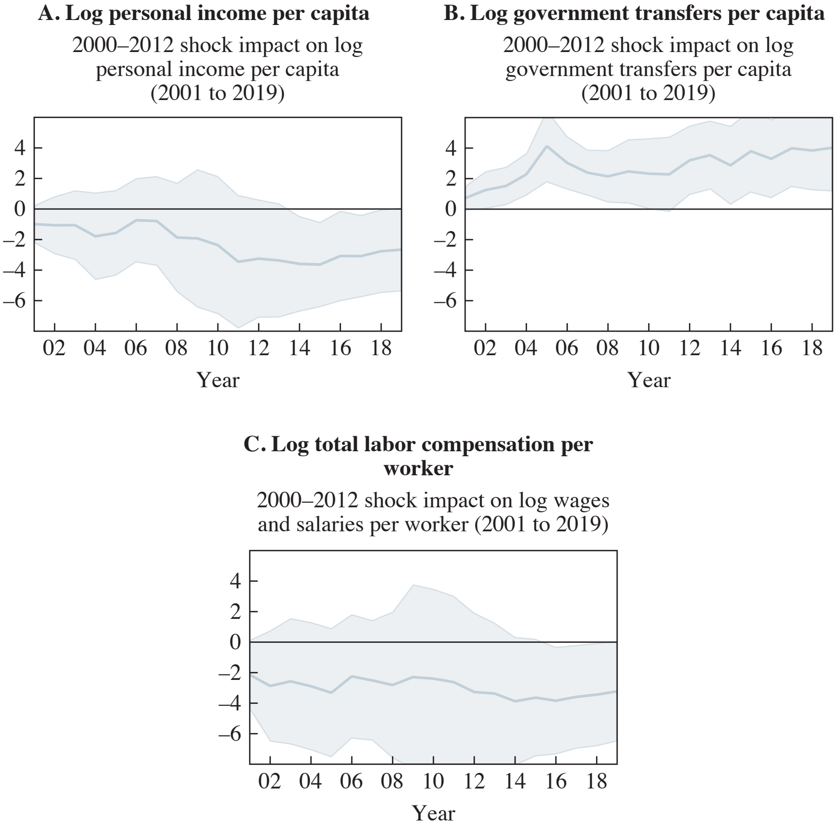
Using US input-output data to construct upstream (supplier) and downstream (customer) import exposure shocks for both manufacturing and nonmanufacturing industries, Acemoglu and others (2016) estimate negative employment effects in industries whose customer industries are directly trade-exposed. Conversely, 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.

III.C. 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, Larsen, and Palmer (2016) and Autor, Dorn, and Hanson (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 CZ 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 CZs that are more trade-exposed 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, Dorn, and Hanson (2013a) find that trade exposure caused an increase in government transfer receipts in adversely affected CZs, 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 labor compensation (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.

Figure 8. Trade Shock Impact on Income, 2000–2019



Source: Authors' calculations.

Note: Panels report two-stage least squares coefficient estimates for β_{1h} in equation (2) and 95 percent 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 equation (1) and instrumented by equation (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.

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.

Estimation results for these outcomes 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.[36] The impact of exposure to import competition on personal income per capita in figure 8, panel A, 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 2000–2015 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 2000–2019 time difference indicates that for CZs at the 25th and 75th percentiles of trade exposure, the more exposed CZ would have a 1.76 (= −2.66 × [1.17 − .51]) percentage point smaller increase in personal income per capita over 2000–2019, which compares to the 25th–75th percentile difference in the growth of personal income per capita over 2000–2019 of −12.2 (= 21.3 − 33.5) percentage points. Consistent with Autor, Dorn, and Hanson (2013a), we see in figure 8, panel B, that CZs that are more trade-exposed had larger increases in government transfer receipts per capita, which remain elevated through 2019. For the 2000–2019 time difference, the impact coefficient of 4.03 (t-value = 2.75) implies that at the 75th–25th percentile difference in trade exposure, the more exposed CZ would have a 1.81 (= 2.75 × [1.17 − .51]) percentage point larger increase in transfers per capita over the time period, relative to the 75th–25th percentile difference in the growth of transfers per capita of 10.90 (= 67.6 − 56.7) percentage points.

In online appendix A.5.4, we further explore the impact of trade shocks on government transfers by program type. Consistent with results in Autor, Dorn, and Hanson (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 reductions in incomes, means-tested government programs

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

meant to provide income assistance to poor households are instead largely unresponsive to greater import competition.

III.D. Heterogeneity in Impacts across Regions

We have so far focused on the average response of CZs 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 online appendix table A4). Consider, for instance, North Hickory (2000 population 378,000), and Raleigh-Cary (2000 population 1,420,000), both of which are located in North Carolina. Over 2000–2012, the two CZs were each above the 95th 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 CZs entered the 2000s with distinctly different economic structures. Hickory was a traditional factory town; in 2000, 43.0 percent of its working-age population had a manufacturing job and just 15.6 percent had a bachelor's degree. Raleigh-Cary, by contrast, was more educated and industrially diversified. In 2000, 34.2 percent of its working-age population had at least a college degree, and only 17.0 percent worked in manufacturing. These initial differences may have shaped how the two CZs responded to the large and sudden increase in import competition.

A substantial literature documents that US regions with fewer college-educated workers have grown less rapidly (Glaeser, Scheinkman, and Shleifer 1995; Behrens, Duranton, and Robert-Nicoud 2014; Diamond 2016) and have seen larger declines in employment-population rates (Austin, Glaeser, and Summers 2018). To explore the role that a dearth of human capital may play in the poor adjustment of local labor markets to the China shock, we separately examine CZs that initially had smaller versus larger supplies of college-educated workers.

Other work studies how greater specialization leaves regions more exposed to industry-specific shocks (Feyrer, Sacerdote, and Stern 2007; Michaels 2010). Many of the labor-intensive US industries exposed to trade with China were agglomerated in small and medium-sized cities in the Southeast and Midwest (Ellison, Glaeser, and Kerr 2010). Existing work documents that the impact of the China shock was greater in CZs that at the outset had lower employment rates (Austin, Glaeser, and Summers 2018) and were more specialized in mature industries positioned later in their product cycles (Eriksson and others 2019).[37] We explore the role that reverse agglomeration contributes to poor adjustment to the China shock, and we separately analyze CZs that initially were less versus more industrially specialized.

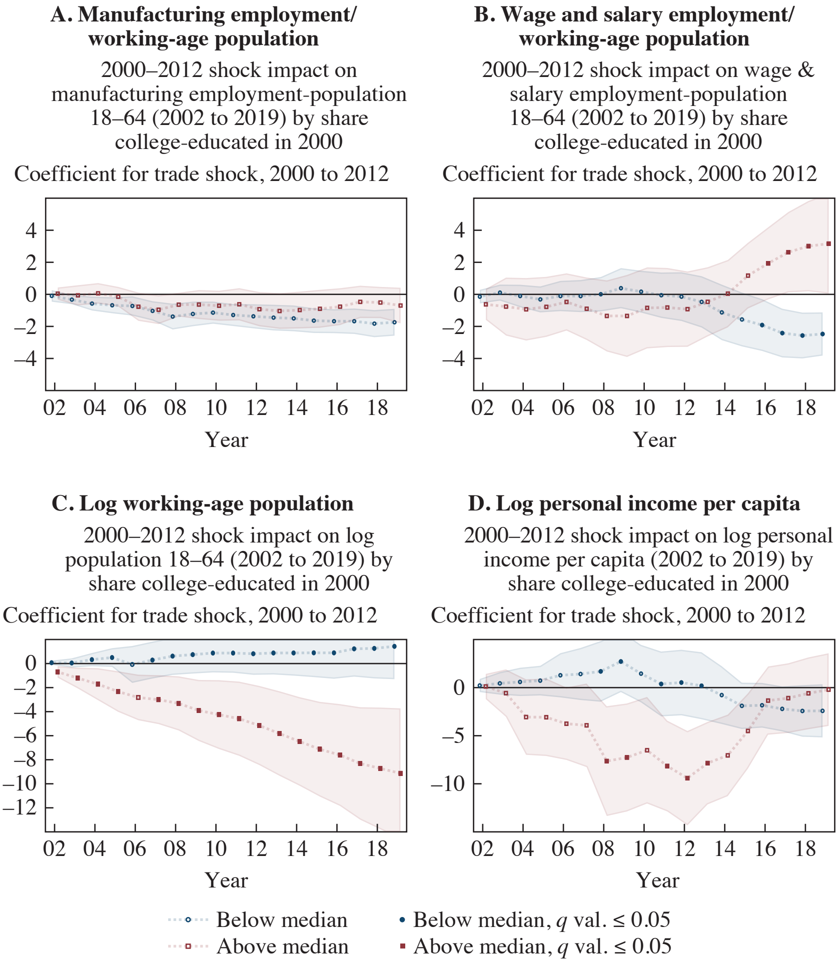
Figure 9 presents estimates in which we divide the sample of CZs into groups based on whether the share of college-educated workers in their working-age populations was above or below the population-weighted national median in 2000. There are 336 CZs in the former group and 386 in the latter group. We 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 constant across outcomes, we evaluate time differences from 2001–2002 to 2001–2019. To control the false discovery rate when evaluating differences in coefficient estimates across sample splits, we compute and display minimal Benjamini-Hochberg q-values based on the number of hypotheses being tested.[38]

Although CZs with higher-educated and lower-educated workers both 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 figure 9, panels A and B. These results are consistent with those in Bloom and others (2019), who study the 1992–2012 time period. For the 2001–2019 horizon, the first two figures show that in CZs with less-educated workers, a 1 percentage point increase in import penetration over 2000–2012 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-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

37. 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.

38. The number of hypotheses (144) is the product of the four outcomes, eighteen 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 8 and 9, 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



Source: Authors' calculations.

Note: Panels report two-stage least squares coefficient estimates for β_{1h} in equation (2) and 95 percent confidence intervals for these estimates. Coefficient estimates whose differences have a minimal Benjamini-Hochberg q-value of less than or equal to 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 college-educated workers 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 the year on the horizontal axis; the trade shock is the decadalized 2000–2012 change in CZ import exposure as defined in equation (1) and instrumented by equation (3); control variables are the same as in regressions reported in figure 4. 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.

that employment impacts on nonmanufacturing employment are null. For CZs with relatively few college-educated workers, the substantially larger impact on total employment than on manufacturing employment reveals a negative impact of trade exposure on nonmanufacturing employment, as shown in online appendix figure A9. Despite manufacturing job losses being compounded by nonmanufacturing losses in these CZs, there is no effect of trade exposure on the size of the log working-age population (figure 9, panel C). 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 9, panel D. Since economically motivated migration would tend to mitigate local employment-reducing 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 9, panel A) 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 CZs with more-educated versus less-educated workers. This trade-induced increase in the total employment-population ratio must imply a corresponding positive impact of trade exposure on the nonmanufacturing-population ratio, as seen in online appendix figure A9.

Just as critical, CZs with more-educated workers adjusted to adverse trade shocks in part through net out-migration. For all time differences except 2001–2006, we reject that impact coefficients on the working-age population are equal for CZs with more-educated versus less-educated workers. At the 2001–2019 time difference in CZs with more-educated workers, a 1 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 nonmanufacturing employment at the 2001–2019 time difference of 6.04 (t-value = 2.37, online appendix figure A9, panel b) implies that more of the increase in the nonmanufacturing employment population ratio occurred through the out-migration of labor rather than through greater job growth. Like CZs with less-educated workers, those with more-educated workers also see negative impacts of trade exposure on personal income per capita (figure 9, panel D). Distinct from CZs with less-educated workers, these impacts reach their maximum negative value for the 2001–2012 time difference and then diminish over time, becoming close to zero for the 2001–2016 horizon and beyond.[39]

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.[40] 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 CZs are less specialized by industry than smaller and medium-sized CZs. In figure 10, panels A and B, 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 CZs. 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 but not in the less specialized ones (figure 10, panel C), 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.

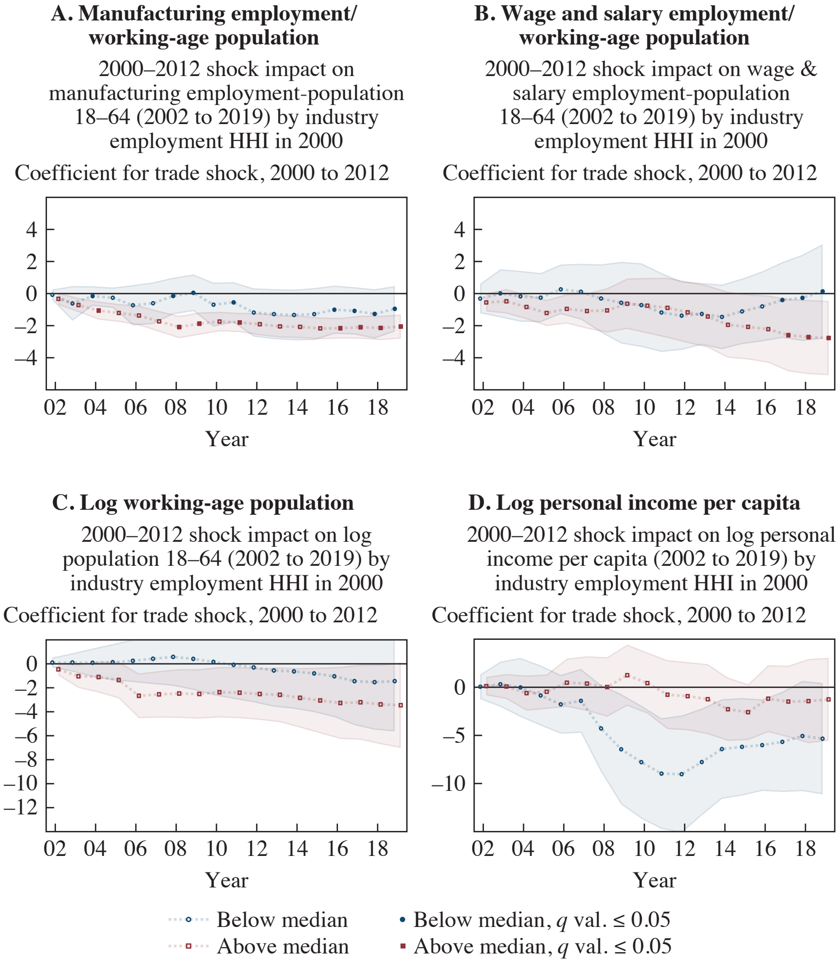
III.E. Putting the Pieces Together

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 nonmanufacturing or

39. Online appendix figure A9, panels c and d, show that in both CZs with more-educated and less-educated workers 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.

40. We construct these HHIs using ACS data for 2000 and the 1990 census industry classification code.

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



Source: Authors' calculations.

Note: Panels report two-stage least squares coefficient estimates for β_{1h} in equation (2) and 95 percent confidence intervals for these estimates. Coefficient estimates whose differences have a minimal Benjamini-Hochberg q-value of less than or equal to 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 the year on the horizontal axis; the trade shock is the decadalized 2000–2012 change in CZ import exposure as defined in equation (1) and instrumented by equation (3); control variables are the same as in regressions reported in figure 4. 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.

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. CZs that are more trade-exposed 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 hypothesis—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).[41] 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 that deserve mention. One is that, amid the regular ebb and flow of the US labor market, the China shock was no more consequential than a high tide. 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.[42] That is, import competition from low-wage countries other than China (Hanson 2020),

41. Housing market regulations in large US cities could also have impeded adjustment in trade-exposed regions. Inelastic housing supply in major cities, due in part to housing regulations (Glaeser, Gyourko, and Saks 2005), may hinder low-wage workers in the heartland from moving to expensive coastal cities (Hsieh and Moretti 2019).

42. 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, Dorn, and Hanson 2013b); a second is that there was no obvious abrupt acceleration in technological change after 2001 (indeed, productivity growth slowed after 2004).

rising capital intensity in manufacturing (Fort, Pierce, and Schott 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 overlook how concentrated the China trade shock was in place and time. The concentration in place was due to the relocation of manufacturing to smaller cities and towns over the twentieth century, made possible by the maturation of industrial production (Eriksson and others 2019) and improved transportation (Kim 1995; Michaels 2008). The concentration in time was due to the speed of China's rise and the immensity of its economy (Naughton 2006). The scarring effects of job loss in trade-exposed CZs were made more acute by this job loss occurring during trade-induced local economic recessions (Jacobson, LaLonde, and Sullivan 1993; Davis and von Wachter 2011; Huckfeldt 2021). Workers were not just losing their jobs, they were experiencing the dislocating effects of work disappearing in their communities (Wilson 1996; Autor, Dorn, and Hanson 2019; Charles, Hurst, and Schwartz 2019).

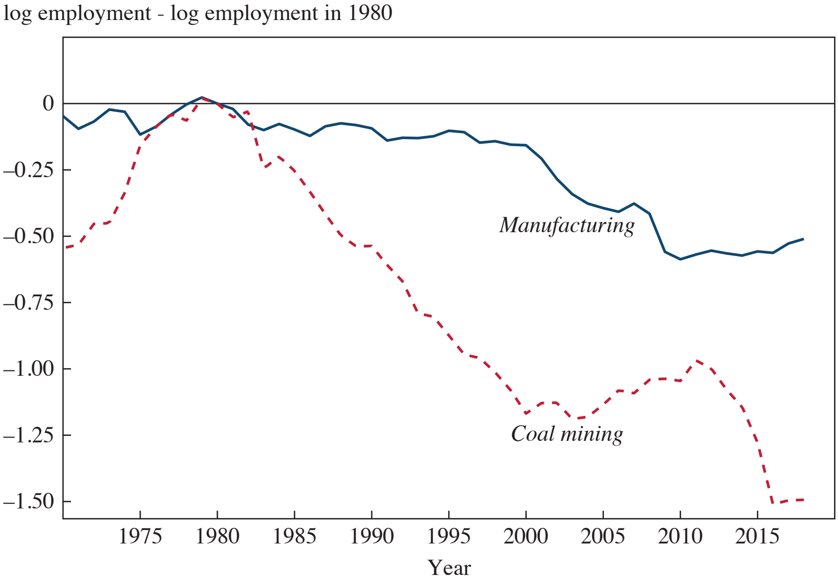
IV. Was the China Shock Unique?

A question arises as to 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, Hurst, and Schwartz (2019) document that US CZs 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 in production technology and energy demand (Black, Daniel, and Sanders 2002; Black, McKinnish, and Sanders 2005).[43] A second is the Great Recession, in which

43. On regional boom and bust cycles in energy production, see Michaels (2010), Jacobsen and Parker (2014), Allcott and Keniston (2017), Feyrer, Mansur, and Sacerdote (2017), Bartik and others (2019), Clay and Portnykh (2018), and Kearney and Wilson (2018).

Figure 11. Employment Decline in Coal Production versus Manufacturing



Sources: BEA Industry Economic Accounts and authors' calculations.

a housing collapse and subsequent credit freeze differentially affected US regions (Mian and Sufi 2014; Charles, Hurst, and Notowidigdo 2018; Beraja, Hurst, and Ospina 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.

IV.A. 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 China's rise. Figure 11 plots employment in US coal mining and, for reference, employment in US 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 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 (Black, McKinnish, and Sanders 2005). Given these patterns, we specify the coal shock as the change in coal employment over 1980–2000. Our analysis builds on Black, McKinnish, and Sanders (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 US CZs.[44]

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

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

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 $t = 1980$; ΔSS_i^{coal} is a shift-share variable that projects the coal shock onto CZ i; and X_{it} is a vector of controls. We specify the decadalized shift-share coal shock as,

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

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 1980–2000, outside of the state in which CZ i is located. To facilitate interpretation, in the regression analysis we multiply the shock in equation (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.[45] 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 college-educated workers in the CZ population, and the share of foreign-born workers in the CZ population. Because of the spatial concentration of coal deposits, the shock hit a relatively small number of CZs: just 258 CZs

44. Because CZs are aggregates of counties, our analysis may dampen the county-level effects in Black, McKinnish, and Sanders (2005).

45. 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 in equation (5) and the China shock shift-share variable in equation (2) is –0.08.

(35.7 percent) 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 90th and 95th percentiles were 0.19 and 0.83 percentage points, respectively.

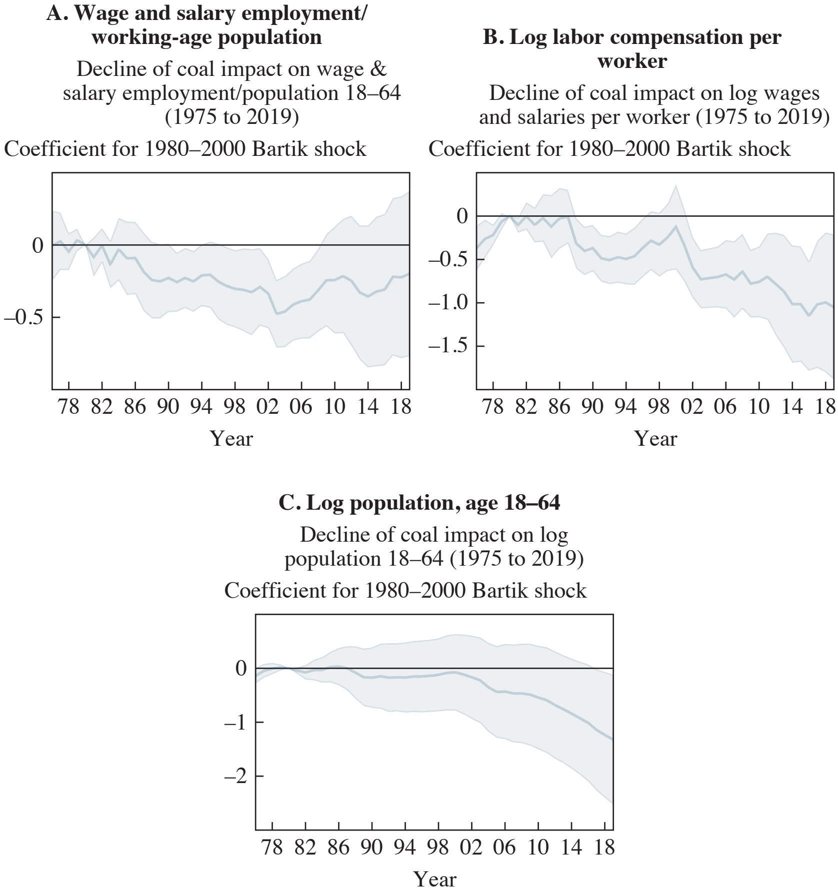
Estimation results appear in figure 12. The impact coefficients for the ratio of wage and salary employment to population (figure 12, panel A) become increasingly negative during the 1980–2000 shock. They are largest for the 1980–2003 horizon (–0.48, t-value = –3.98) and remain negative for all horizons out to 1980–2019, though the estimates for the longest time windows lack statistical precision. For the 1980–2006 difference—on the eve of the Great Recession and twenty-six 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 95th percentile of intensity would have experienced a 0.32 (= 0.39 × [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 1980–2006 the 5th–50th percentile difference in the change in employment-population was –9.4 percentage points.

In figure 12, panel B, 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, McKinnish, and Sanders (2005) and are precisely estimated at long horizons. Over 1980–2019 (β = –1.05, t-value = –2.46), a CZ subject to a coal shock at the 95th percentile of intensity would have seen a 0.88 (= –1.05 × [0.83 – .001]) log point greater decline in average labor income.

Given these lasting adverse labor market impacts of the coal shock, one might expect that heavily exposed CZs saw substantial population declines. However, figure 12, panel C, shows that the impact of exposure to the coal shock on population head counts 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 1980–2016. For the 1980–2019 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 × [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 twenty-five years after the shock arrived.

Qualitatively, figure 12 indicates that the reaction of employment, earnings, and population to the coal shock was similar to the response to the

Figure 12. Local Labor Adjustment to the Decline of Coal



Source: Authors' calculations.

Note: Panels report two-stage least squares coefficient estimates for β_{1h} in equation (4) and 95 percent confidence intervals for these estimates. The dependent variable is the change in the indicated measure between 1980 and the year on the horizontal axis; the coal shock is defined in equation (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.

China import shock (see figures 5, 7, and 8). 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 estimates in figure 5 suggest that the China shock induced a decline in the wage and salary employment-population ratio of −1.55 (= −1.74 × 0.89) percentage points between 2001 and 2019, the corresponding employment decline for the coal shock was only –0.04 (= –0.198 × 0.22) percentage points per decade from 1980 to 2019.[46] 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 95th percentile of exposure to the coal shock declined by 0.88 log points more than in a CZ at the 5th 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 75th and 25th (instead of 95th and 5th) percentile of exposure.

IV.B. 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 equation (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 $t = 2006$; ΔSS_i^{gr} is a shift-share variable that projects the Great Recession onto CZ i; and X_{it} is a vector of controls. The National Bureau of Economic Research (NBER) dates the Great Recession as beginning in December 2007 and ending in June 2009. Because US housing markets, which played a major role in the downturn, began contracting in 2006 (Charles, Hurst, and Notowidigdo 2016), we specify the shock as commencing in that year. We estimate equation (4) for twenty-one separate time periods: five periods before the shock begins (2001–2006 to 2005–2006) to check for pre-trends in outcomes; three shock periods

46. 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 (2001–2019 for the China shock, 1980–2019 for the coal shock) times the decadalized mean of the respective shock (0.89 for the China shock, 0.22 for the coal shock).

(2006–2007 to 2006–2009) to examine shock impacts as the shock intensifies; and thirteen periods after the recession had ended (2006–2007 to 2006–2019) to evaluate longer-run adjustment.

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

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

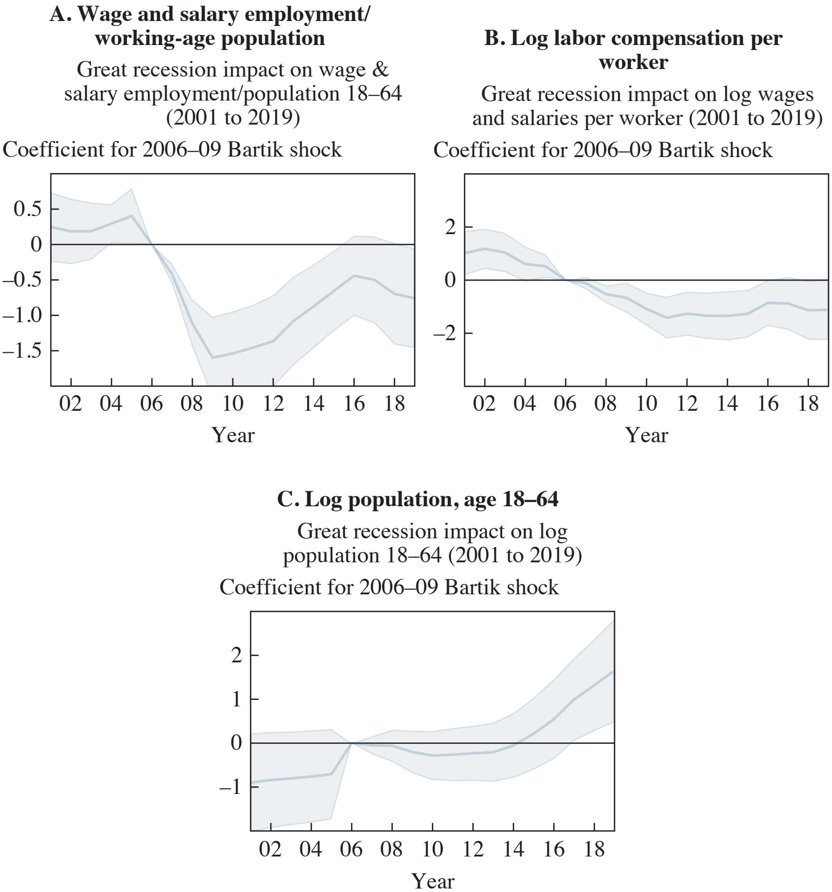
where L_{ij2000}/L_{i2000} is the share of industry j in employment of CZ in the year 2000, and the term in brackets is the log change in national employment in industry j over 2006–2009, outside of the state in which CZ i is located. To help interpret the results, we again multiply the shock in equation (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 equation (2).[47]

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, Hurst, and Notowidigdo (2016) document further that declines in employment rates were concentrated among less-educated workers. We extend Yagan's analysis out to 2019, using CZ-level REIS data, as compared to Yagan's 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 13, panel A). 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.

47. The correlation between the Great Recession shift-share variable in equation (6) (over 2006–2009) and the China shock shift-share variable in equation (2) (over 2000–2012) is 0.48. In unreported robustness exercises, 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 equation (3). Coefficient estimates and patterns of significance for the two sets of results are very similar.

Figure 13. Local Labor Market Adjustment to the Great Recession



Source: Authors' calculations.

Note: Panels report two-stage least squares coefficient estimates for β_{1h} in equation (4) and 95 percent confidence intervals for these estimates. The dependent variable is the change in the indicated measure between 2006 and the year on the horizontal axis; the Great Recession shock is defined in equation (6); control variables include initial period CZ employment composition (shares 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-white individuals, and those age 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.

This recovery appears to stall in 2016, however. For the 2006–2019 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 2006–2009 would have experienced a 1.66 (= 2.19 × 0.76) percentage point decline in its wage and salary employment-population ratio, relative to the mean change for the 2006–2019 period of +2.20 percentage points ($\sigma = 3.89$).

In figure 13, panel B, 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 CZ subject to the mean Great Recession shock would have experienced a 2.45 (= −2.19 × 1.12) percentage point decline in compensation per worker.

The enduring declines in employment ratio and labor income per capita over the period 2006–2019 could indicate persistent scarring effects of the Great Recession. However, figure 13, panels A and B, 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, Hurst, and Notowidigdo 2016), one might expect that these variables would not fully recover following the recession. Indeed, the estimates in figure 13, panel C, 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. CZs 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 United States, sets it apart from shocks such as the decline of the coal sector, the impact of which was more limited in scope. As the United States prepares for potentially more job loss due to ongoing energy transformation and expected changes in oil and gas production, the failure of local labor markets to adjust 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.

V. 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 uncontroversial to infer that trade-exposed CZs suffered losses in average economic well-being *relative to* 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.

V.A. Quantitative General Equilibrium Analysis of the China Trade Shock

For individuals living in a given US CZ, 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, Costinot, and Rodríguez-Clare 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 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 produce 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, Rodríguez-Clare, and Yi (2017).[48] They combine a model of heterogeneous firms from Eaton and Kortum (2002) with a model of heterogeneous workers from Roy (1951).[49] Workers are immobile across regions and partially mobile across sectors. They specify the China trade shock as innovations to China's industrial productivity, which they calibrate for the period 2000–2007 by backing out the implied productivity growth needed to generate the observed change in US manufacturing imports from China that is predicted by the first-stage regression of Autor, Dorn, and Hanson (2013a). Using these innovations, plus assumptions on other parameter values, they calculate that the average change in real income across CZs from trade with China is 0.22 percent, with a (unweighted) standard deviation across CZs of 0.31 percentage points.

The analysis in Caliendo, Dvorkin, and Parro (2019), also based on Eaton and Kortum (2002), allows labor to be mobile geographically in the longer run. Dynamic adjustment in labor allocations across US states and sectors 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 2000–2007, again using Autor, Dorn, and Hanson (2013a), and estimated model elasticities, they find that labor reallocation across sectors and regions is largely complete by thirteen years after shock initiation (i.e., 2013, which is well within our period of analysis). At this horizon, the average change in welfare is 0.20 percent, with a (unweighted) standard deviation across US states of 0.09 percentage points.[50]

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

49. Lagakos and Waugh (2013) and Hsieh and others (2019) were the first to combine the Eaton and Kortum and Roy models.

50. 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.

In both studies, because goods prices are assumed identical across regions, aggregate welfare impacts are similar.[51] Naturally, regional variation in welfare impacts is larger in Galle, Rodríguez-Clare, and Yi (2017), owing to geographic labor immobility. The two models also differ in how they model labor market frictions, which determine how trade shocks affect the overall employment rate.[52] In Caliendo, Dvorkin, and Parro (2019), nonemployment arises because of an option for home production; in an extended version of Galle, Rodríguez-Clare, and Yi (2017), nonemployment arises because of both a home production option and search and matching frictions in the labor market. Notably, 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 quantitative analyses of the China trade shock, we compare changes in the regional dispersion of income per capita in these analyses with reduced-form estimates of the trade-induced change in the variance of income across regions. Because 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, Larsen, and Palmer (2016) and Autor, Dorn, and Hanson (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

51. Kim and Vogel (2020) provide a related assessment of trade with China, which we discuss in online appendix A.6.

52. In related work, Adão, Arkolakis, and Esposito (2019) modify the baseline framework in Galle, Rodríguez-Clare, and Yi (2017) 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 percent, with a standard deviation across CZs of 0.56. In online appendix A.5.3, we find weak evidence of gravity-based spillovers of trade shocks across regions.

are flat across the income distribution, which implies that the purchasing power effects of freer trade are distributionally neutral. Relatedly, Bai and Stumpner (2019) and Hottman and Monarch (2020) find that the impact of trade with China on US 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 United States as the sum of a price-level effect, which is presumed homogeneous across regions, and an income effect, which varies across regions.[53]

V.B. Relative and Absolute Welfare Effects across Regions

Following Galle, Rodríguez-Clare, and Yi (2017), we can write the average change in welfare for a CZ due to the China trade shock as the induced change in real income,

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

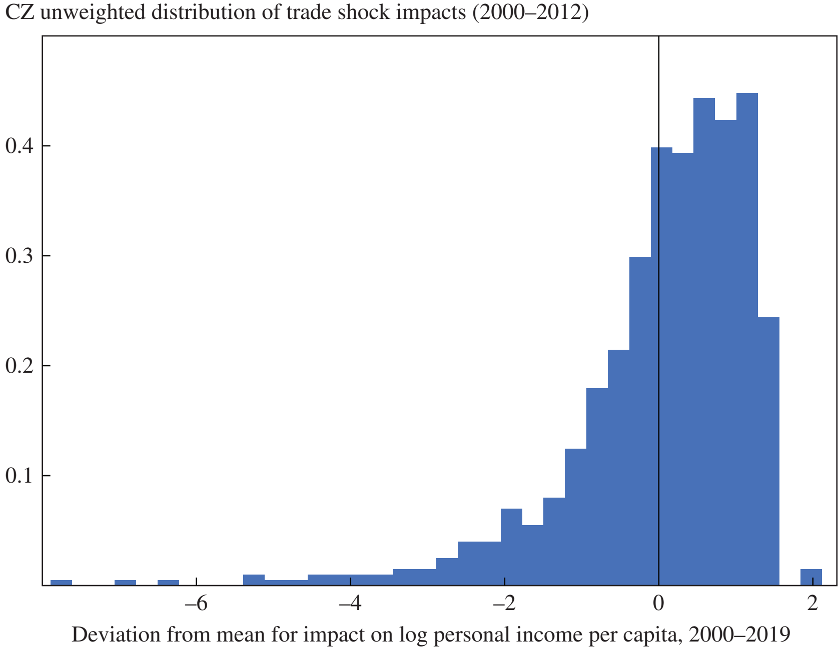
where $\hat{W}_i \equiv W_i'/W_i$ is the trade-induced change in income per capita (Y_{ci}/L_{ci}) in US region i, relative to the change in the national price index, which applies the β_j Cobb-Douglas expenditure shares for products in each sector j to induced changes in the price level P_j for each sector j. Because Galle, Rodríguez-Clare, and Yi (2017) 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 the impact of trade exposure on manufacturing employment over 2001–2007 ($\beta = -1.45$, t-value = –4.56) is approximately 80 percent of that over the 2001–2019 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 equation (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 in income per capita in each CZ relative to the national weighted average,

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

53. In online appendix A.6, we discuss results in Feler and Senses (2017) on how trade affects housing values.

Figure 14. Implied Variation in Changes in Personal Income per Capita, 2000–2019



Source: Authors' calculations.

Note: This figure shows a histogram for the welfare change in equation (8), evaluated for the 2000–2012 trade shock–induced change in personal income per capita (based on results in figure 8, panel A), 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.

where γ_l is the share of region l in the US population, β_1^y is the parameter estimate in equation (2) for the log change in personal income per capita, and $IP_{i\tau}^{cu}$ is the trade shock. This estimate of the relative impact leaves unmeasured the shock impact on the overall price level, which we treat as common across regions, as consistent with Caliendo, Dvorkin, and Parro (2019) and Galle, Rodríguez-Clare, and Yi (2017).

In figure 14, we show the estimation-implied variance in equation (8) induced by the 2000–2012 trade shock over the 2000–2019 horizon.[54]

54. The impact coefficient for 2000–2019 in figure 7, panel A, is marginally significant (t-value = –1.90). Impacts are statistically significant for all time differences from 2000–2014 to 2000–2018. Because these coefficients are larger in absolute value than the one for 2000–2019, using the long period difference can be seen as a conservative choice.

The (unweighted) standard deviation in trade shock impacts on personal income per capita across CZs over 2000–2019 is 1.22 percentage points, which far exceeds the cross-CZ income dispersion generated by quantitative models. Online appendix table A4 lists the CZs that are above the 95th 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, Dvorkin, and Parro (2019) and Galle, Rodríguez-Clare, and Yi (2017), gains from trade in the aggregate are 0.22 percent 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 percent of the US continental population in 2000. Even if we double these gains to 0.44 percent, 173 CZs, representing 15.9 percent of the US 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 on consumer prices in a reduced-form analysis. Using the 1991–2007 trade shock from Autor and others (2014), they find that a 1.0 percentage point larger increase in import penetration produces a decline in consumer prices of 1.4 percent.[55] Using the mean decadalized change in import penetration over 2000–2012 of 0.89 percentage points (table 1), the implied reduction in consumer prices based on Jaravel and Sager (2019) is 1.25 percent. 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 CZs suffering net losses from trade is smaller but still meaningful. In figure 14, there are 82 CZs, representing 6.3 percent of US population, with declines in personal income per capita of 1.25 percent or more. For these CZs, the net welfare effect of trade with China is negative, even when applying the sizable Jaravel and Sager (2019) 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 US CZs.

55. 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)

VI. 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 far too small to have reached most workers who lost their jobs because of import competition in the 1990s and 2000s. A further limitation of Trade Adjustment Assistance is that it conditions assistance on the cause of job loss, that is, 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 US and European nationalist-populists, for instance, have been greater in regions that have suffered larger trade-induced employment declines (Colantone and Stanig 2018a, 2018b; Autor and others 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 US-China trade war succeeded in elevating US product prices (Amiti, Redding, and Weinstein 2019; Fajgelbaum and others 2020; Cavallo and others 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.[56] Austin, Glaeser, and Summers (2018) find that employment impacts of labor demand shocks are larger in local markets in

56. On optimal spatial policies, see Fajgelbaum and Gaubert (2020) and Gaubert, Kline, and Yagan (2021).

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 globalization, one may expect continued popular support for political platforms that disparage foreign trade, despite the apparent inefficacy of these platforms to date.

Because of the distinctiveness of the China trade shock—in terms of its enduring magnitude and concentration in place and time—some see it as holding few lessons for policymakers. Such characterizations are mistaken. Large, localized contractions in labor demand—for example, trade and technology shocks to autos and steel in the 1970s, price shocks to coal in the 1980s, and ongoing automation and roboticization—are unfortunately common events. The global economy is now witnessing another round of localized shocks, arising from the energy transition. If economies succeed in sharply reducing reliance on fossil fuels, demand will plummet for the workers who mine coal, drill for oil and gas, refine petroleum, and transport and distribute these outputs (Popp and others 2021). Industries that are energy intensive in production and that located their operations close to carbon-based energy sources may also experience significant disruptions. Although increased demand for renewable energy will surely create new jobs in harnessing solar, wind, and hydro power, these jobs may be located far from existing carbon-based production facilities. New energy-intensive industries, such as data centers, may choose to locate near renewable energy providers, possibly further disadvantaging those working in the carbon-based economy. We are now armed with knowledge both of the consequences of localized job loss and the likely location of future worker displacement. This should enable policymakers to prepare for assisting workers, as carbon-based jobs disappear, and regions, as they reorient toward less carbon-intensive activities. Without such preparation, the energy transition may add to the painful history of regionalized job loss.

It is now clear that the China trade shock as we understand it appears to have stopped intensifying a decade ago. The China shock of the 1990s and 2000s was about China's onetime transition to a market economy. This transition, which affected US industries that were late in their economic life cycles, 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, for example, 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.

ACKNOWLEDGMENTS For helpful comments, we thank Loren Brandt, Lorenzo Caliendo, Federico Esposito, Bruno Ferman, Stephen Redding, Andrés Rodríguez-Clare, Katheryn Russ, Jonathan Vogel, and, especially, Marianne Bertrand, Edward Glaeser, and James Stock. Anne Beck, Peter Bruno, Brendan Chapuis, Rebecca Jackson, René Livas, Sophia Mo, and Savannah Noray provided excellent research assistance. Autor thanks the following for financial support: Smith Richardson Foundation (#20202252), Accenture LLP (#027843-0001), and Andrew Carnegie Fellowship (G-F-19-56882). Dorn acknowledges support by the University of Zurich's Research Priority Program Equality of Opportunity.

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Appendix

A.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.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 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).[55]

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 (A5). For that analysis, the employment rate is based on civilian working-age individuals

[55] 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 does not matter materially in the estimation (which we verify in unreported robustness exercises). 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
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 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
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

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.

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 A1, 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 A1a, 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 slight 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 A1c, and personal income per capita in Figure A1e. For non-manufacturing employment in Figure A1b, the log change in the working-age population in Figure A1d, and government transfers per capita in Figure A1f, 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).

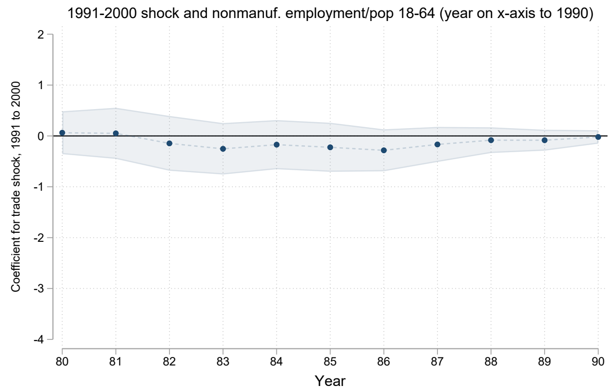
In Appendix Figure A2, we replicate the specifications in Figure 5, using the Borusyak et al. (2020) procedure for constructing standard errors. To aid in comparing the estimates, we report the Figure 5 estimates side-by-side with Borusyak et al. (2020) based estimates. (Here, we use the CZ manufacturing 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 A2a) and non-manufacturing employment (Figure A2b), 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.

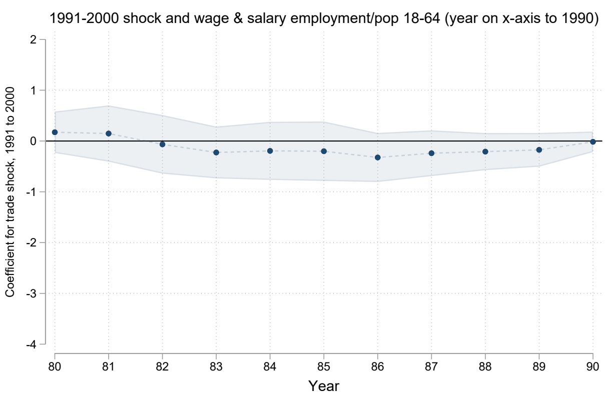
Figure A1: Analysis of Pre-Trends for 1991-2000 Trade Shock

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

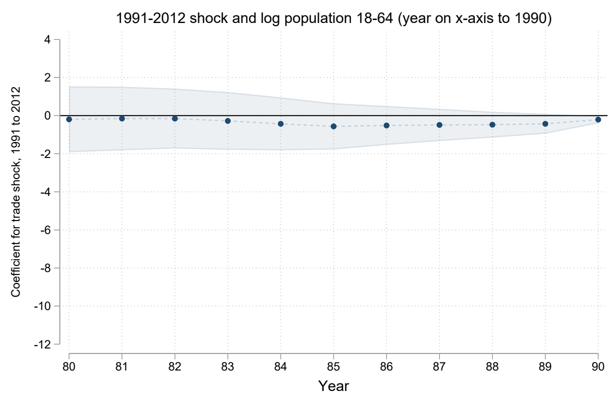
(b) Non-manuf. emp./Working-age pop.



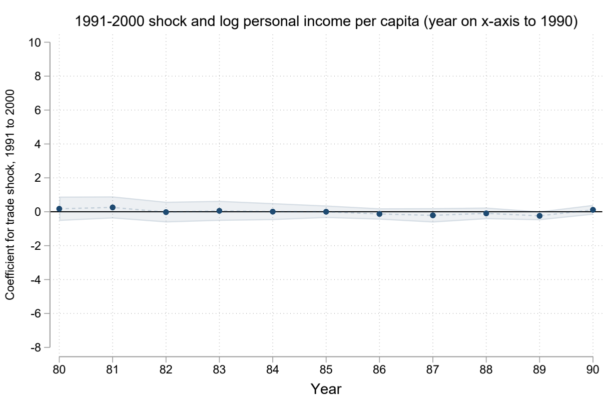
(c) Total wage and salary emp./Working-age pop.



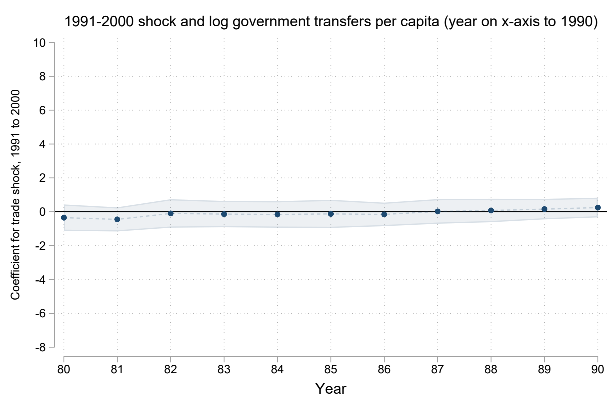
(d) Log working-age population



(e) Log personal income per capita



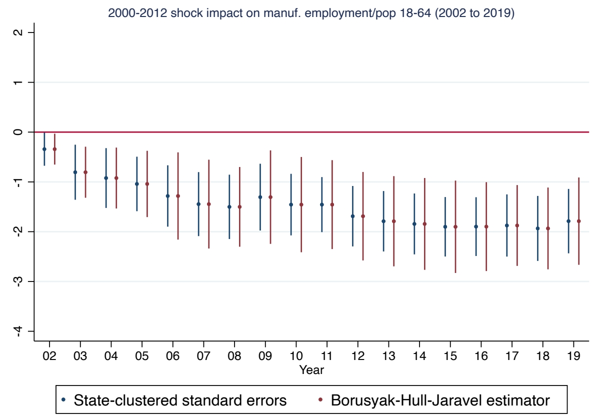
(f) Log government transfers per capita



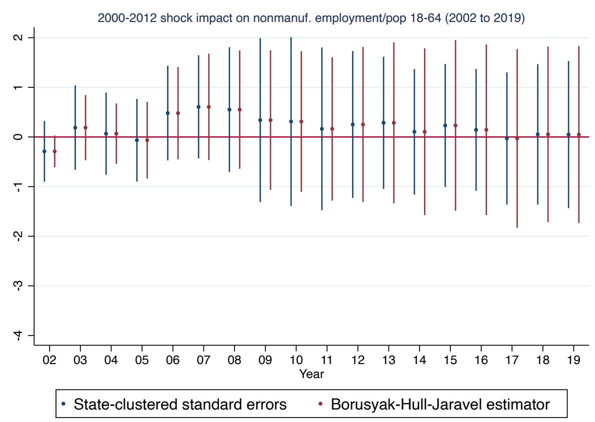
Note: Panels (a)-(f) report OLS regressions of the change in the indicated outcome between the year indicated on the horizontal axis and 1991 on the trade shock in (1) for the 1991-2000 period. Control variables include CZ Census region dummies, and the share of manufacturing in CZ employment in 1970 (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 A2: Estimation Results Based on Borusyak, Hull, and Jaravel (2020)

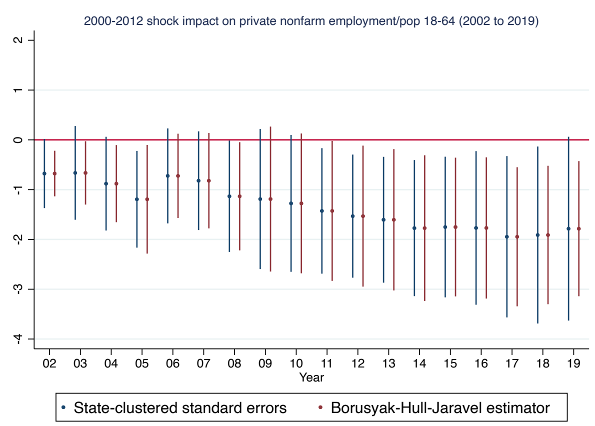
(a) Manufacturing employment/Working-age population



(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—i.e., 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.

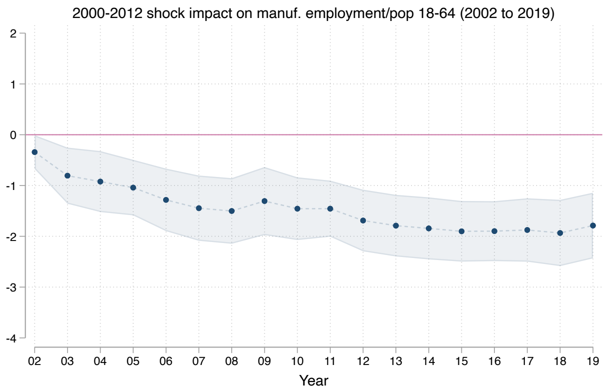
Appendix Figure A3 compares results for the change in manufacturing employment-population ratio over 2001 to 2019 when using our baseline 2000-2012 shock (Figure A3a), replacing this shock with that for 1991-2000 (Figure A3b), and including both shocks together (Figure A3c).[56] Because the trade shocks are highly correlated across decades, coefficient estimates in Figures A3a and A3b 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 A3c), coefficient estimates for the 2000-2012 trade shock become smaller and less precisely estimated.[57] 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.

[56] In Figure A3d, 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 A3a (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.

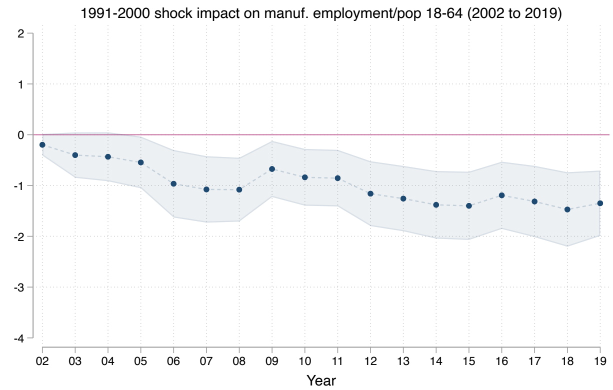
[57] In Figure A3c, unreported coefficients on the 1991-2000 trade shock are small and imprecisely estimated.

Figure A3: Combined Trade Shock Impacts on Manufacturing Employment

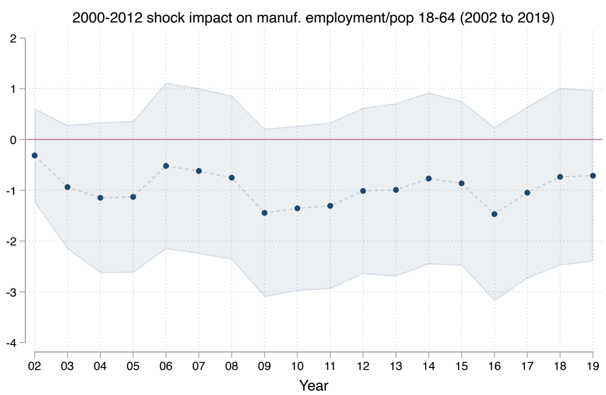
(a) 2000-2012 trade shock



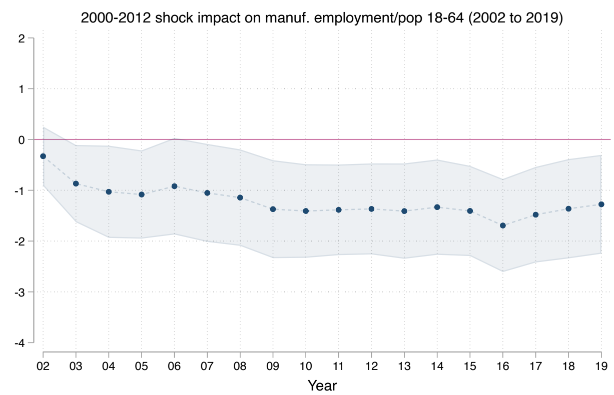
(b) 1991-2000 trade shock



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



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

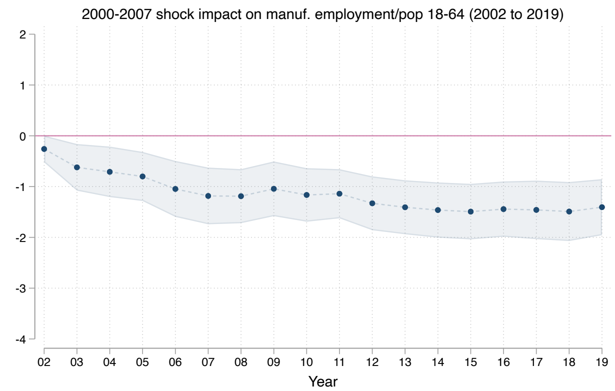


Note: Panels (a)-(d) report 2SLS estimates of (2). The dependent variable is the change in manufacturing employment as a share of the working-age population between 2000 and the year indicated on the horizontal axis. Panels (a) and (b) include the 2000-2012 and 1991-2012 trade shocks alone, respectively; 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.

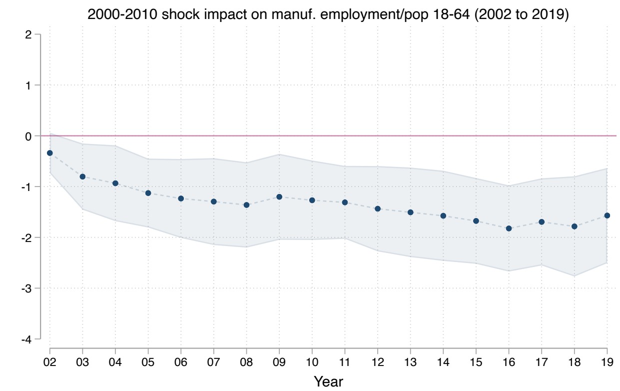
A.5.2 Employment and Population Headcounts

Figure A4: Trade Shock Impact on Manufacturing Employment, Varying Shock Periods

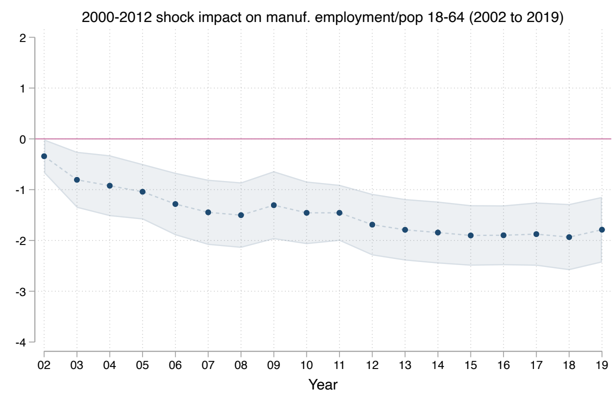
(a) Trade shock, 2000 to 2007



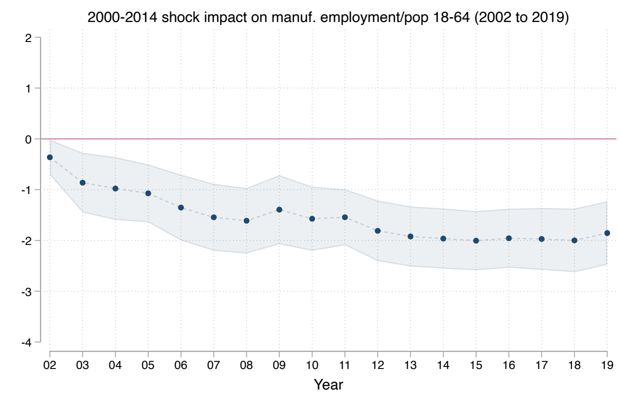
(b) Trade shock, 2000 to 2010



(c) Trade shock, 2000 to 2012



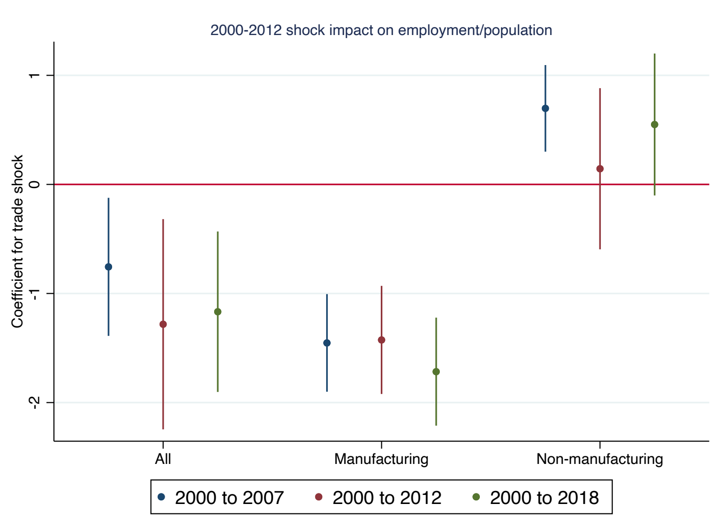
(d) Trade shock, 2000 to 2014



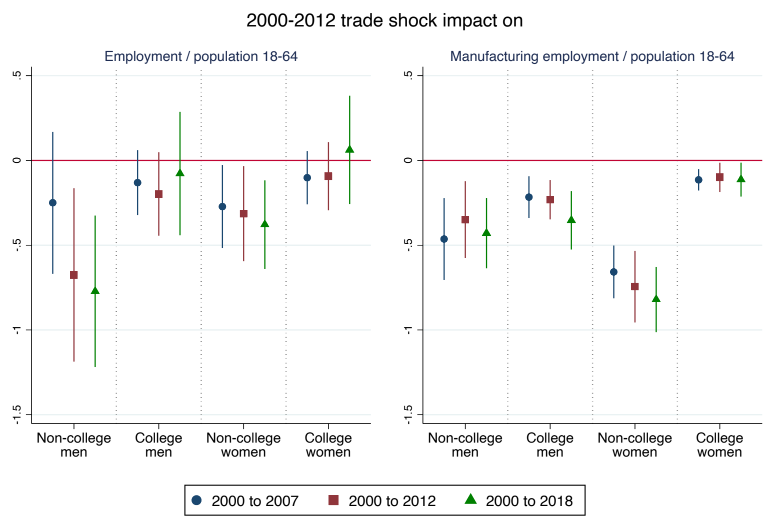
Note: Panels (a)-(d) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in manufacturing employment as a share of the working-age population between 2000 and the year indicated on the horizontal axis The trade shock 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 A5: 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



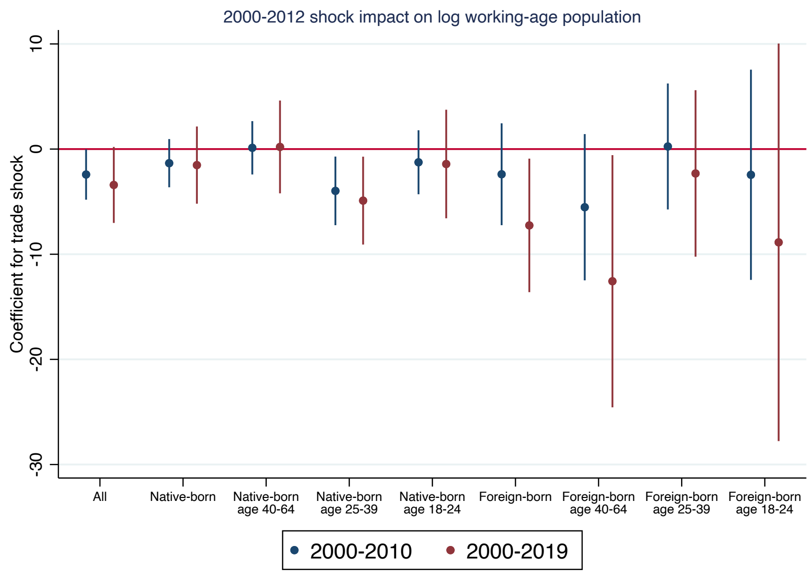
(b) Employment/Working-age population by sector and worker education



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 A6: Trade Shock Impact on Population Headcounts, Census-ACS data for 2000-2010 and 2000-2019

Log population by age cohort and nativity



Note: The figure 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).[58]

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.[59] Appendix Figure A7 reports the results.

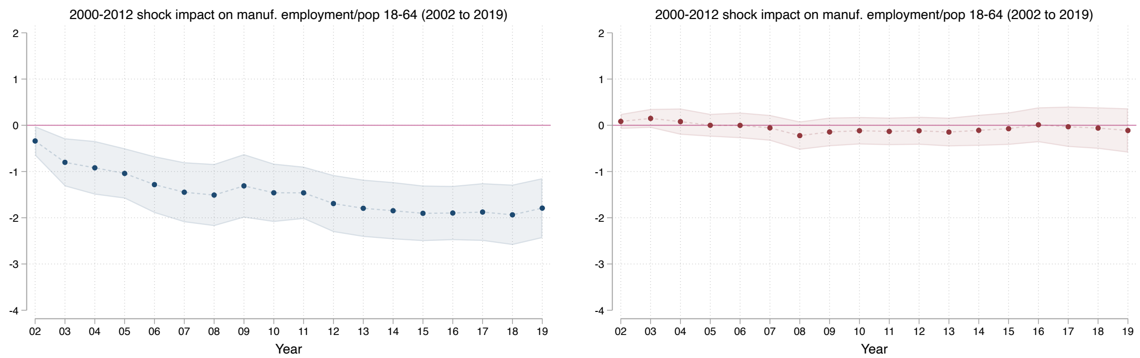
[58] A third channel through which national industry shocks affect local wages and employment is through changes in consumption costs. If greater import competition expands local consumption possibilities, real incomes and the demand for goods will increase. This will give rise to an own-region effect, 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).

[59] 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).

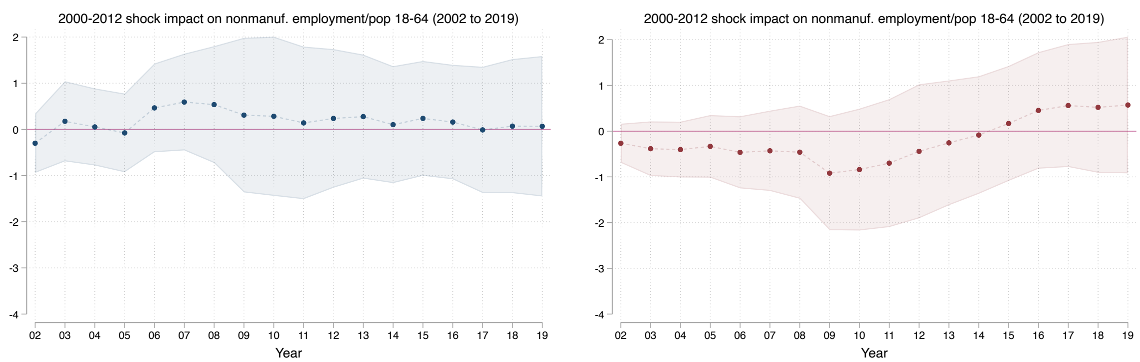
Figure A7: Impacts of Local vs. Gravity-Based Trade Shocks on Employment

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

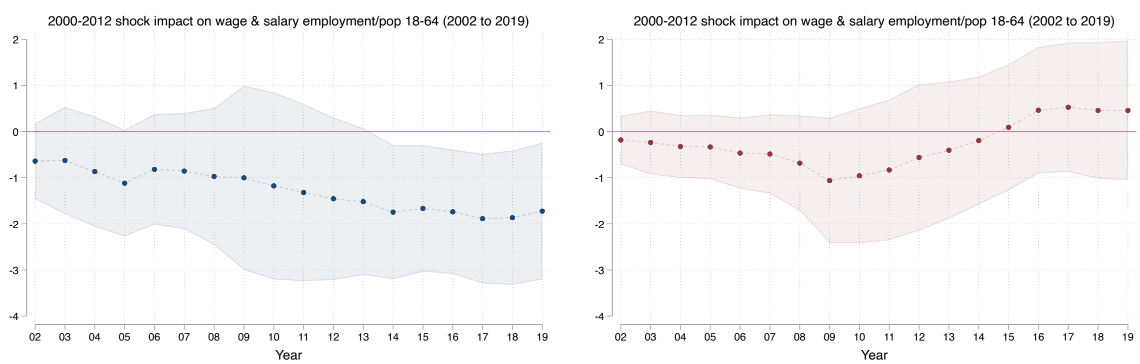
(a) Manufacturing employment/Working-age population



(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 (9) 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.

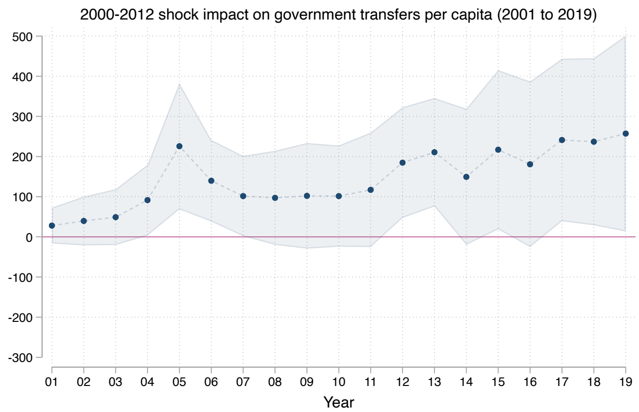
A.5.4 Personal Income and Government Transfers

In Figure A8, 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 A8b, 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 A8a. 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.

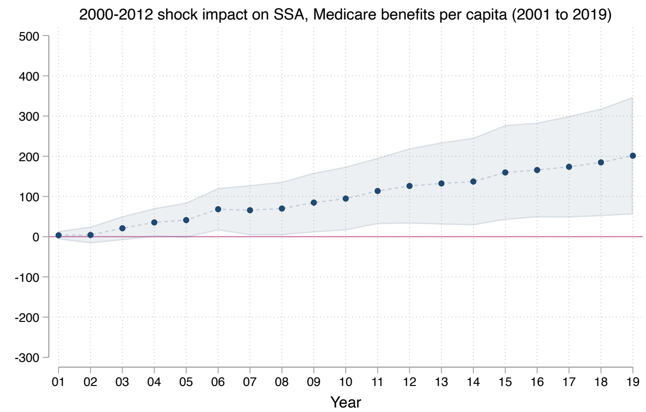
A second notable finding is that, despite trade-induced lower incomes, means-tested government programs meant to provide income assistance to poor households are largely unresponsive to greater import competition. We see this in the null or negative and insignificant responses of Medicaid and government income assistance to trade exposure in Figures A8c and A8d.

Figure A8: Trade Shock Impact on Government Transfers per Capita by Program Type

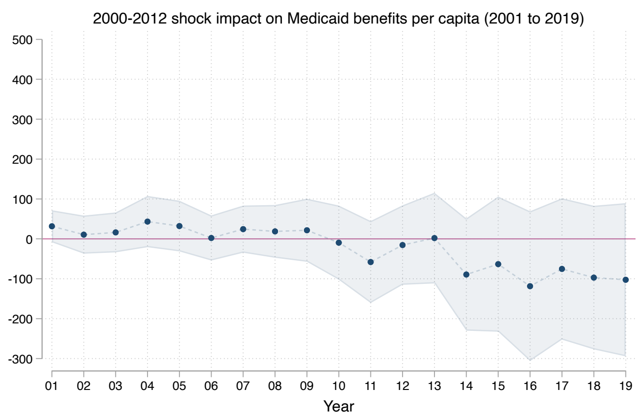
(a) Government transfers per capita



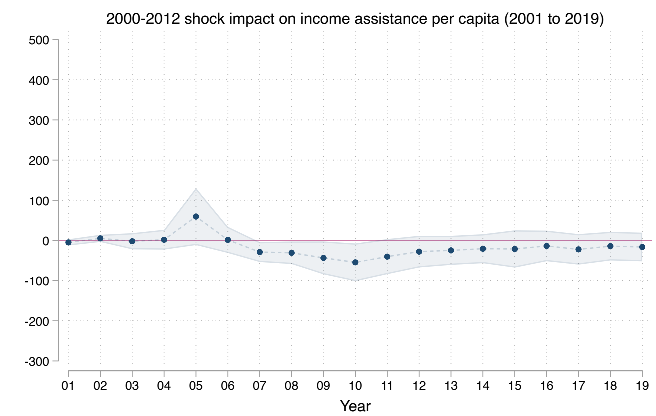
(b) SSA, SSDI, Medicare payments per capita



(c) Medicaid benefits per capita



(d) Income assistance per capita

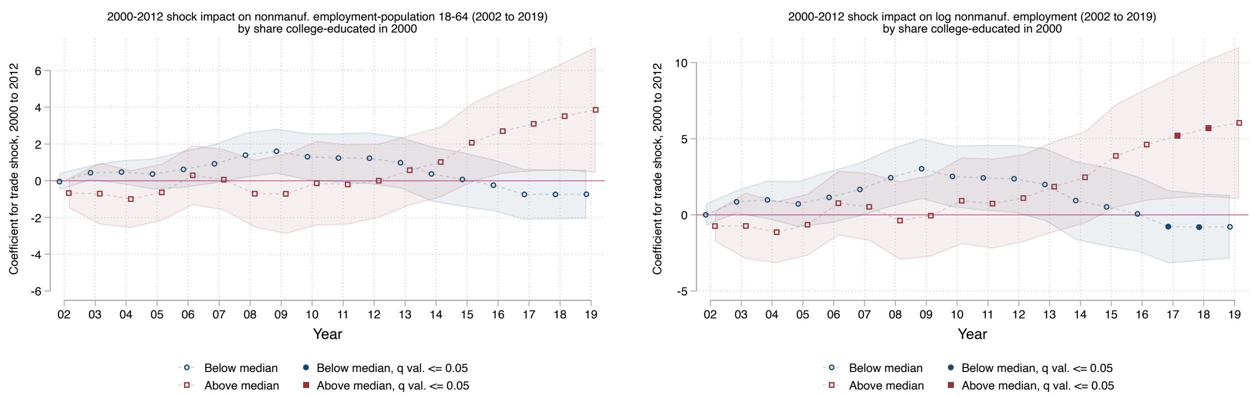


Note: Panels (a)-(d) report 2SLS coefficient estimates for β_{1h} in (2) and 95% confidence intervals for these estimates. The dependent variable is the change in the indicated value per capita between 2000 and the year indicated on the horizontal axis; the trade shock is the decadalized 2000-2012 change 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.

A.5.5 Heterogeneity in Impacts

Figure A9: Heterogeneity in Trade Shock Impacts by Initial College Educated Population: Additional Results

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



Note: Panels (a)and (b) 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 are weighted by the CZ working-age population in 2000.

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 may 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 related 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.